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[779] Common Sense, History, and the Theory of Relativity[1]

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Abstract. The relationship between modern physics and common sense is discussed by investigating whether there is any substantial role left for the latter in the scientific activity. It is shown that ordinary judgement is needed in some of the most crucial decisions that a researcher has to take in his or her work. The history of a notoriously counter-intuitive physical theory (i. e. relativity) is quoted to provide examples in support of this claim, and some little-known aspects of this theory's reception by the scientific community are discussed. A simple model of collective behaviour leading to consensus formation is briefly described, as an example of a nontechnical argument which is nonetheless useful in making sense of the scientific activity. Some indications for the teaching of science arising from this approach are sketched in the final section.
Key-words: theory of relativity, history of physics, physics teaching.

Resumo. A relação entre a física moderna e o senso comum é discutida investigando se há qualquer papel significativo deixado posteriormente na atividade científica. Para isto é mostrado que de julgamento ordinário é necessário em algumas das decisões mais cruciais que um investigador tem que levar dentro o de seu trabalho. A história de uma teoria física notoriamente contra-intuitiva (i. e. relatividade) é citada para fornecer exemplos em defesa desta reivindicação, e são discutidos alguns aspectos pouco conhecidos da recepção desta teoria pela comunidade científica. É descrito brevemente um modelo simples de comportamento coletivo que conduz a formação de consenso, como um exemplo de um argumento não técnico que é, no entanto, útil, dando sentido à atividade científica. São esboçadas, na seção final, algumas indicações para o ensino de ciência que surge desta investigação.
Palavras-chave: teoria da relatividade, história da física, ensino de física.

I. Prologue

The relationship between science and common sense can be envisaged in basically two ways. It may be seen as essentially conflictual, with common sense being constantly in need to be tutored and reformed by science; or it may be seen as cooperative, with common sense lending the ground on which science can grow and prosper, and science offering specialized information to fill the gaps in common knowledge. For some disciplines (e. g. the social sciences) the cooperative approach is quite widespread and prima facie reasonable: indeed, one may be sometimes at a loss in

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[1] This paper is a revised and expanded version of a talk delivered at the international conference "Science as Culture" (Como [Italy], 15-19 September 1999).

detecting a real (that is, not just verbal) distinction between their doctrines and ordinary, common sense views.[2]

However, with respect to the natural sciences there has been more emphasis on the ways in which science drastically diverges from common sense and follows a path which may be at times strongly counter-intuitive and upsetting for the person-in-the-street. Physics is presented very often as a chief **[780]** instance of this phenomenon; as such, it is sometimes employed to disparage the ambition of the non-experts to say something relevant about the concerns of scientists in general. Indeed, it is mainly physical science that has been qualified as "uncommon sense", and chosen to show the "heretical nature of science".[3] The Galileo's affair has come to be presented ever more often as the chief, highly symbolic instance of the conflict between science and common sense (represented by the not too clever character of Simplicio in Galileo's *Dialogo*), than of the opposition between free inquiry and academic and priestly authority. It is clear that opinions held about this issue are intimately connected with views of the nature and historical development of science, and have wide-ranging consequences on teaching.

As everybody knows, from the XVII century onward physics has grown more and more mathematical, and experiments have lost increasingly contact with anything a non-scientist - or even a scientist working in isolation - can ever hope to ascertain directly. But does this mean that the layperson has nothing to contribute except for epistemological self-restraint and faith in what he or she is told by scientists? Has common sense got at least a residual part to play in the understanding of the physical world?

For a number of reasons this is a question which is today rarely addressed as if it were genuinely open. In fact, in most authors' opinion, the success of science should have convinced by now all thoughtful people to throw away - in Wittgensteinian fashion -[4] the ladder of common sense on which mankind climbed to reach the qualitatively different level of scientific knowledge. Today this insistence is particularly aggressive from the quarters of cognitive psychologists, who write books on human "irrationality"

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[2] Compare Giddens 1989: "Common-sense ideas often provide sources of insight about social behaviour" (p. 15). In my opinion this is quite an understatement. It is interesting to remark that, at least as far as I can see, *none* of the 7 opinions stated in the previous page - to show that, on the other hand, sociological findings "*disturb*" our "common-sense beliefs about ourselves and others" - would qualify as a "common-sense belief". (An extreme case is the opinion [p. 14] that "How long people live is dependent upon their biological make-up and cannot be strongly influenced by social differences", which is countered by the argument that "The poor are less healthy on average than the rich, for example, because they usually have worse diets, live a more physically demanding existence, and have access to inferior medical facilities" (p. 14): I can only say that I am glad that even sociologists have succeeded in convincing themselves of what no sane person ever dared to doubt). This does not mean that sociologists cannot contribute to knowledge, only that their conceptual contributions (as distinct from the rationally organized gathering of empirical data about particular societies) are best viewed as articulating and refining common sense intuitions, rather than subverting them.

[3] "With each new freshman class, I again must face the fact that the human mind wasn't designed to study physics" (Cromer 1993, p. 23).

[4] "Er muss sozusagen die Leiter wegwerfen, nachdem er auf ihr hinaufgestiegen ist" (Wittgenstein 1921, 6.54). Wittgenstein is here giving instructions as to the use the reader has to make of the *Tractatus*'s propositions.

(for instance, in taking decisions in ordinary situations), and attack what they call "folk psychology", which they claim should be substituted by the much more 'scientific' views on human thinking and agency these scholars support.[5] If all this were to be accepted, the only resource being left to laypeople in dealing with their own vital (private and public) interests would be to refer to some scientific oracle for enlightenment and guidance.

For my part, I do not think that things are so simple - nor the future of non-scientists so dark. In particular, I shall argue in this paper that common sense, as the intellectual faculty by which we interpret and assess historical claims (in the widest sense), is essential to the making of science, for *in order to contribute to science one has first to learn about it, and learning science is indissolubly meshed with the acceptance of historical narratives*.

I shall illustrate this view by concentrating on a very representative example. In fact, from the point of view of the conflict between physics and common sense, the XX century opened ominously with what can be regarded as the paradigm of counter-intuitive science: special relativity (1905).[6] Accounts of the origin and development of this theory, up to the 1915-1916 synthesis, can be found with strong similarities and often almost the same words in innumerable articles and books.[7] One should not hastily assume that historical detail is introduced only in elementary textbooks and popularizations in order to spice with 'the human element' a matter which would otherwise appear as too dry and technical. On the contrary, historical narratives, if sometimes very short and implicit, form an important component also of the original technical articles, and cooperate with other textual elements in their rhetorical strategies.[8] My aim is to discuss some aspects of it by stressing particularly the *non-technical assumptions needed in order for the standard expositions to work as persuasive accounts*. I shall divide the subject-matter by dealing in sequence with three topics: experimental support; revolutionary concepts; mathematical proofs. These three aspects joined a winning alliance in the affirmation of the theory of relativity (as in other revolutions in physics, earlier and afterwards). We shall try to indicate how much there is to learn from careful consideration of each of them from the viewpoint just sketched.

[781] II. Experimental support

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[5] For instance, according to one of them, common sense psychology "suffers explanatory failures on an epic scale, [...] has been stagnant for at least twenty-five centuries, and [...] its categories appear to be (so far) incommensurable with or orthogonal to the categories of *the background physical science whose long-term claim to explain human behaviour seems undeniable*. Any theory that meets this description must be allowed a *serious candidate for outright elimination*" (P. M. Churchland in 1981, quoted in Glover 1988, p. 112; italics added).

[6] In fact the XIX century closed with the introduction (in 1900) of the quantum hypothesis by M. Planck, the bearing of which on common sense notions was not, however, obvious. Quantum mechanics, which was developed in the Twenties, is a different matter.

[7] To save space I shall keep the references to the relativity literature to a minimum. In Mamone Capria 1999, ch. 3 (pp. 265-416), one can find the story told from the viewpoint defendend in the present paper.

[8] A recent article on this topic is Staley 1998.

The special theory of relativity was essentially an outgrowth of a debate turning around a number of experiments meant “to discover any motion of the earth with respect to the light medium” (Einstein *et al.* 1923, p. 37). The emphasis on the experimental basis of the theory was as strong as widespread. For instance, in 1908 Hermann Minkowski, himself a mathematician, opened his famous lecture on “Space and Time” by saying:

> The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. (Einstein *et al.* 1923, p. 75)

The most important and celebrated experiment was performed by A. Michelson in 1881, and then by himself and E. Morley in 1887 - exactly two centuries after the publication of Isaac Newton's *Naturalis Philosophiae Principia Mathematica*, where the distinction between relative and absolute motion had been memorably drawn. The importance of the Michelson and Morley trial as a motivation for Einstein's 1905 paper has been strongly - in my opinion, *too* strongly - questioned in the last few decades by some historians of science.[9] However, it is simply a fact that the experiment was referred to by virtually *all* physicists who took a stand with respect to relativity, including Einstein (for the first time in print in 1907). Virtually all physicists regarded it as the empirical rock on which the theory rested, at least in the ‘context of the justification’, and textbook authors devoted as a rule one or two pages to a description of its main features.

Now a search through the literature[10] shows that on the interpretation of the Michelson-Morley experiment controversy raged for over three decades. There was hardly a circumstance, theoretical or experimental, which was immune from criticism. Very soon, however, in works intended to provide an overall account of the theory the descriptions of the experiment lost much of their specificity. Already in his famous 1918 report (for physicists and mathematicians), Arthur S. Eddington used, to describe the essence of the Michelson-Morley experiment, the simile of “a swimmer in a river”, which in fact obfuscates to a very large extent the optical intricacies of the question.[11]

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[9] Many historians of relativity seem to have fallen in love with the romantic idea of the lonesome genius, and they have tried to deny - as much as allowed by gaps in the surviving documentation - any resemblances between the road taken by Einstein to special relativity and the one that led others (Lorentz and, most impressively, Poincaré) to very similar discoveries. However, I would like to remind that in a letter of 28(?) September 1899, Einstein referred to a survey published in 1898 by the renowned physicist Wilhelm Wien in *Annalen der Physik*, “On the problems connected to the translatory motion of the luminiferous aether” (as the title reads in English), in which the Michelson-Morley and other experiments performed to discover the influence of the aether on terrestrial phenomena were listed and discussed (the letter can be found as Document 57 of Vol. I of the ongoing Princeton edition of *The Collected Papers of Albert Einstein*, first published in 1987). Of course also history of science, as most other academic subject, suffers from - sometimes very obstinated - fashions.

[10] Most references can be found in Swenson 1972.

[11] Eddington 1918, p. 1; cf. also Eddington 1920, ch. I, Bondi 1980, ch. VI, and Russell 1985, pp. 27-8. For a recent re-examination of the optics of the experiment see Mamone Capria & Pambianco 1994.

So what about *different interpretations*? In the same book, Eddington limits himself to stating that he will not discuss "the unsuccessful attempts at alternative explanations" (Eddington 1918, p. 3), and most other authors followed suit. Thus, less than fifteen years after Einstein's paper, alternative explanations are no more discussed, but just dismissed, at least in textbooks (they were still somewhat debated in specialized journals). So the confidence of the readers in the relativistic interpretation comes to rest crucially on how much they feel they can rely on Eddington's and other authors' fairness.

Another question that one is justified in asking about an experiment, and particularly about a controversial one, is *how many times* it has been repeated. It is important to remark that for anyone who has not him or herself performed the experiment - that is, for almost everybody -, to know about the number of repetitions and the number of teams of experimenters involved is a piece of information crucial to the evaluation of the empirical claim. The common sense principle here at work is, roughly speaking, that the more the teams and the repetitions with similar outcomes, the more likely it is that the effect is genuine. The authors of popularizations of course are aware of this circumstance: so one of them says that "careful repetitions made doubt impossible" (Russell 1985, p. 28), another is more dramatic and talks about "thousands of repetitions" (Gardner 1957, p. 85).[12] The historical record, however, tells a different story: the experiment of Michelson-Morley has been repeated (with the same kind of apparatus, though not always with the same accuracy) only a dozen **[782]** times,[13] and the controversy has never really come to an end.[14]

That difficult experiments are not so frequently repeated as could be desired is a fact that has come to occur quite ordinarily in modern physics, and for good practical reasons: experiments are expensive, and no ambitious scientist is likely to waste his or her own limited financial and human resources just to *confirm* somebody else's achievement.[15] The times are gone when René Descartes could write with his usual self-confidence, in a letter to Mersenne of 1640, that "In fact I doubt all experiments which I have not watched myself".[16] This statement may be taken to typify a 'heroic' age of science which has very little in common with our age.

Similar considerations apply to the observational tests of general relativity. Today we know that both the bending of light and the gravitational redshift were advertised as spectacular confirmations of Einstein's theory many years before there could be any reasonable certainty about the data obtained. And things were not quite better for Mercury's perihelion.[17] However, even at the time it was clear that the main ground for believing in those claims was one's degree of confidence in the word of a

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[12] Martin Gardner, the well-known science writer, does not refrain from insulting the memory of the respected physicist D. C. Miller, who had found in the Twenties contrary results, by commenting that "'Repeatability' is a matter of degree. It is always possible to find *someone* unable to perform an experiment" [p. 332]. What is more interesting is that Gardner - as is the case for so many popularizers of physics - is *not* himself a physicist!

[13] Cf. Swenson 1972, p. 242, Table 3.

[14] For instance, see the recent theoretical vindication of Miller's experimental findings by a well-known physicist in Vigier 1997, and the positive reappraisal in Allais 1997.

[15] For related remarks, see Broad & Wade 1982, pp. 76-9.

[16] "Nam vehementer dubito de experimentis omnibus, quae egomet non viderim".

[17] See Will 1986 (ch. 3-5) and Hetherington 1988 (ch. 6).

few (*very* few, incidentally) eminent scientists. So no lesser a physicist than the president of the Royal Society, J. J. Thomson, in the famous meeting of November 6, 1919 in which Einstein was "canonized",[18] had to say:

> It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Prof. Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value for the displacement. (Cit. in Earman, Glymour 1980, p. 77)

What these examples show is that *the assessment of the experimental support of a theory involves in a crucial way common sense criteria.* The fact that basic information concerning the management of experimental data is normally missing both in specialized expositions and textbooks contributes to the wrong belief that the whole process of theory selection has very little to do with ordinary life (e. g. the purchase of a house, or the choice of a job).

III. Revolutionary concepts

The special and general theories of relativity introduced quite a few departures from classical notions: the relativity of simultaneity, length contraction, time dilation, 'twin paradox' effects, the curvature of space-time, the 'closure' of the world, and so on. It is understandable that many physicists felt they had to do something to 'defend' the classical notions, and so a number of 'confutations' of relativity appeared in the press, particularly in the Twenties and the Thirties. Today, the idea that relativity in the Einsteinian version may be internally inconsistent has been mostly abandoned, but opposition to the theory on other grounds has by no means ceased, though it does not make the headlines as it used to do.[19]

It is worth remembering that criticism of relativity was often promoted in the name of classical physics *and* common sense alike. Now, that the concepts of classical physics can be classified as 'commonsensical' is to some extent unwarranted. Moreover, when things come to the production of technical concepts which is typical of scientific theorizing, it would seem as if common sense had no right to interfere. 'Length contraction', 'mass-energy equivalence', or what else, have just to be taken for what they are - their inconsistency with ordinary notions is simply no objection.[20]

However, it must be stressed that insistence on conformity to ordinary notions from students and laypersons (and 'heretical' scientists as well) cannot be dismissed too

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[18] See Pais 1982, p. 305, for the parallel with the ecclesiastical procedure.

[19] As a starting point respectable journals like *Foundations of Physics* and *Physics Essays* can be usefully consulted. Every second year a very interesting conference is organized in London on "Physical Interpretations of Relativity Theory" (the next one, the 7th edition, will be in 2000), where, among other things, a fair number of argued critical opinions on the theory are usually presented.

[20] One of the "philosophic influences" of relativity is, according to the famous physicist Richard P. Feynman, that "if we have a set of 'strange' ideas, such as that time goes slower when one moves, whether we *like* them or do *not* like them is an irrelevant question. The only relevant question is whether the ideas are consistent with what is found experimentally" (Feynman *et al.* 1963, p. 16-3).

fast. The relationship between theory and experience is far too complex for an upholder of relativity to be entitled to say that experiments *forced on us* those strange concepts. At most one can say that some physicists - but not others - found easier and/or more fruitful to depart from, than to stick to, previous basic notions. For instance, perhaps one day scientific progress will take **[783]** us back to some kind of absolute time.[21] In fact, the history of science presents us with several instances of comebacks of supposedly 'dead' theories.

Furthermore, what can a non-expert make of a *disagreement between experts*? In 1928 Bertrand Russell soberly suggested that the right approach was "that when they [the experts] are not agreed, no opinion can be regarded as certain by a non-expert" (Russell 1928, p. 12).[22] However this seems too austere to most authors, who use to refer to the verdict of the 'scientific community' as a remedy against the paralyzing doubts which might arise in themselves or in their readers. But what can be concluded from such frequent statements on what the 'community' or the 'majority' of physicists believes?

One point should be clear: these are *sociological* statements, and must be evaluated as such. Thus, prior to an inquiry into the mechanisms which make a certain disciplinary community hang together, no reliable conclusions can be reached. As we stressed in § 2 for the Michelson-Morley experiment, detailed accounts of controversy are virtually absent in textbooks, and from this omission one might wrongly be led to think that differences of opinion about the validity of a theory are short-lived within science. But the history of science and any practitioner's experience tell that things are otherwise.[23] Once aware of this, one soon realizes that the formation of 'consensus' in the scientific community is a quite intriguing and multi-faceted social phenomenon, which raises many difficult questions. For instance, how much does the scientific community behave in this respect like an "open society" in Karl Popper's sense, where "personal decisions" grounded on "rational reflection" prevail, rather than "the rigidity of tribalism" (Popper 1966, vol. I, pp. 171-3)?[24] Be that as it may, when in a book or

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[21] In a sense it already has, with cosmological time in standard evolutionary cosmology.

[22] In view of what has been said in §2, this should be contrasted with the quotation from Russell 1980 above.

[23] Failure to realize this represents, in my opinion, the most serious defect in Kuhn's brilliant account of scientific research. (For instance he wrote: "There are schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints. But they are *far rarer there than in other fields*; they are always in competition; and *their competition is usually quickly ended*" [Kuhn 1970, p. 177, italics added]). The strong conservative tone of Kuhn's writings is in fact curiously at odds with the celebrity that his talk about changes of paradigms and revolutions in science earned him with the general public. No wonder that he is reported not to have enjoyed very much this sort of fame (cf. Weinberg 1998 for some references).

[24] One should not think that only outsiders are tempted to be sceptical about the perfect rationality and independence of thought in the scientific community. For instance, the well-known cosmologist G. F. R. Ellis had this to say in a historical survey of cosmology: "On various occasions in the history of cosmology the subject has been dominated by the bandwagon effect, that is, strongly held beliefs have been widely held because they were unquestioned or fashionable, rather than because they were supported by evidence. As a result, particular theories have sometimes dominated the discussion while more convincing

article a theory is selected - and a number of barely mentioned alternatives rejected - on the ground that 'most' physicists think it is the best one, *we must be aware that the authors are no more talking about the 'physical world'*. Instead they are asking us to commit ourselves to some, more or less idealized, image of the physical community, and *it is up to our ordinary judgement to decide in any given circumstance whether this is the reasonable thing to do*.

IV. Mathematical Proofs

The theory of relativity is partly a mathematical theory. It contains statements which can be proved as much as any matematical theorem can be.[25] However, quite often, the physical situation one wants to analyze with the help of the theory is too complicated for rigorous mathematical treatment. So one resorts to simplified arguments that do not really prove the final statements, but that are deemed by the author to be intuitively plausible or otherwise acceptable. Now as these statements are used again and again, an increasing number of researchers comes to believe *that they are logical consequences of the theory* - though in fact no one has ever proved them.

Thus many so-called 'theorems' in physics are simply *folklore* - that is, more or less plausible and widely accepted but *unproven* generalizations from strictly circumscribed statements which *can* be proved.[26] There is nothing really strange in this. Apart from trying to construct a proof by oneself - an uncertain task which may take from weeks to centuries! -, even a scientist is bound to evaluate - on any but `mathematical' grounds! - *how reliable are the authors who make those assertions*. This piece of information is sometimes essential also in order to devise empirical tests with a reasonable hope of doing something meaningful.

A good example of this is the 'relativistic' explanation of the advance of Mercury's perihelion. In fact it depended essentially on the unproven assumption that one could use, *on one hand,* taking into account the influence of the other planets of the Solar system, Newtonian physics for the *n*-body perturbative explanation of 531 seconds of degree of the observed secular advance; and, *on the other hand*, **[784]** the relativistic one-body spherically symmetric Schwarzschild solution for the residual 43 seconds! It is hardly surprising that this argument has been termed "intellectually repellent" by a respected physicist, J. L. Synge.[27]

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explanations were missed or neglected for a substantial time, even though the basis for their understanding was already present" (Ellis 1989, p. 367).

[25] For instance: under smoothness conditions on the metric, the Einstein equation implies that the energy tensor has vanishing divergence.

[26] See Ehlers 1987 for a discussion as regards general relativity.

[27] Synge 1971, pp. 296-7. As explained in this reference (p. 296, note 4; see also Misner *et al.* 1973, p. 1113, Box 40.3), things are made even more confusing by the circumstance that the *total* observed secular advance is $5599''.74 \pm 0.41$, most of which is accounted for in terms of inertial effects (i. e., the Earth not being an inertial frame). Also the philosopher of science Paul Feyerabend discussed the issue of Mercury's perihelion in similar terms in his controversial book *Against Method*, first published in 1975. (In fact Feyerabend was rather well-read in physics and knew personally a number of more or less famous physicists; for this reason his remarks on special theories and people in physics are often interesting and to the point). However, in the 1988 edition, he added a footnote at p. 49, in which he acknowledged

One might object that, though the procedures followed by Einstein and others may have been questionable, still the stunning agreement between the final formula and the astronomical observations more than justified a suspension of the strictest mathematical rigour. However, in order to see trough the question one has to get some essential information concerning the mathematics involved. In fact, working in the 1-body approximation, the 43 seconds come from correcting the Newtonian potential

$$U = GM/r,$$

where in our case M is the mass of the Sun, by adding a term proportional to the inverse of the third power of the distance, so that the gravitational potential becomes:

$$U^* = GM/r + A/r^3,$$

where $A = (GM)^2 a(1-e^2)/c^2$, c is the speed of light, and e is the excentricity of the ideal Keplerian orbit of the planet. This new potential leads to the following formula for the perihelion advance:

$$\Delta\theta = 24\pi^3 a^2 / [T^2 c^2 (1-e^2)] \qquad (*)$$

where a is the semi-major axis and T is the period of the Keplerian orbit. This is the formula obtained by Einstein in 1915.

Now, the transition from U to U^* can be justified in more than one way. It is certainly remarkable that a German high-school teacher, Paul Gerber (1854-?), had proposed in a renowned physics magazine in 1898[28] a different potential for the gravitational interaction which gave, in approximation, precisely the "r^{-3}" correction (with the right coefficient A), and provided a striking connection between the gravitational interaction and the speed of light.[29] In fact Gerber derived formula (*) 17 years before Einstein! This result did not pass unnoticed at the time, notwithstanding the obscurity of the author, as is proved by the fact that Ernst Mach mentioned it in his *Die Mechanik*, for the first time in 1901, in the fourth edition.[30] So if one is willing to

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that "Today the so-called parametrized post-Newtonian formalism satisfies most of the desiderata outlined in the text (details in C. M. Will, *Theory* [that is, Will 1981]). My point is that this was a later achievement whose absence did not prevent scientists from arguing, *and arguing well*, about the new ideas. Theories are not only used as premises for derivations; they are even more frequently used as a general background for novel guesses whose formal relation to the basic assumptions is difficult to ascertain" (italics in the original). Much as I am in agreement with the third statement, I think that even today the PPN formalism cannot be considered a satisfactory answer to the mathematical difficulties of the *n*-body problem in general relativity; and I would not be entirely happy with saying that relativists were "arguing well" when they were claiming for their theory more than at the time (and, probably, even today) could be fairly established.

28 In their very useful compilation, from which I shall quote below, K. and A. Hentschel [1996, p. 3, fn. 14] miss this article, and only refer to another paper by Gerber published in 1902 in an "obscure" (as they correctly say) magazine.

29 Details can be found in Roseveare 1982, pp. 137-144 and Beckmann 1987, pp. 170-175.

30 "Only Paul Gerber [and here reference is given to Gerber 1898], studying the motion of Mercury's perihelion, which is 41 [sic] seconds of arc per century, did find that the speed of

lend the perihelion result such a great weight, it is Gerber's work that should be acclaimed.

But are our present standards of rigour appropriate for analyzing the historical problem at hand? Perhaps what may leave perplexed some of us today in the relativistic derivation was completely acceptable at the time. The answer is simple: doubts as to the correctness of the relativistic derivations were already voiced by contemporaries, like the mathematicians C. Burali-Forti, S. Zaremba, C. L. Poor, between 1922 and 1923.[31] But it is even more interesting that, when his originality in the derivation of Mercury's anomalous advance was challenged, Albert Einstein himself replied as follows:

> Or else you base yourself on a paper by Gerber in which he had anticipated my correct formula for Mercury's perihelion motion. But specialists in the field agree not only that *Gerber's derivation is thoroughly incorrect, but that the formula cannot even be obtained as a consequence of Gerber's leading assumptions*. Mr. Gerber's paper is therefore *utterly worthless*, a failed and irreparable attempt at a theory. I establish that the general theory of relativity has provided the first **[785]** realistic explanation for the motion of Mercury's perihelion.[32]

So for Einstein the fact that the right formula (*) had been derived from Gerber's theory could not count in favour of that theory as long as the derivation was logically objectionable: indeed it was not enough to give that theory even a modicum of respectability (cf. "utterly worthless").[33] This shows once more that it is not legitimate to dismiss the whole question as arising from a misplaced request of logical perfection.[34]

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propagation of gravitation is the same as the speed of light. This speaks in favour of the aether as the medium of gravity" (Mach 1901, p. 199).

31 References are given in Mamone Capria 1999, pp. 332-3.

32 Hentschel & Hentschel 1997, p. 3, italics added. The passage continues in a way that throws some light on Einstein's citation habits, and should be meditated by the several historians of science that have inferred his ignorance of somebody's work from the mere absence of explicit references to that work in his papers: "I did not mention Gerber's article originally, because I did not know about it when I wrote my paper on the motion of Mercury's perihelion; *but even if I had been acquainted with it, I would have had no cause to mention it*" (Hentschel & Hentschel 1996, pp. 3-4, italics added). Incidentally, I do not think it unlikely that Einstein might have been informed of Gerber's work through Mach's passage quoted above, though this is not very relevant in the context of his rejoinder: Einstein's general relativity is indeed a very different theory, both mathematically and philosophically, from Gerber's and similar proposals (cf. Roseveare 1982, chapter 6). However, one may be *inspired* and *helped* in making precise one's aims even by contributions of people with a different frame of mind. (Earman and Janssen [1993, p. 156] suggest a link between Einstein and Gerber's work in the person of the astronomer Erwin Freundlich, who was a friend of Einstein's).

33 In fact in 1917, when Gerber's 1902 paper was republished, this time on *Annalen der Physik,* Hugo von Seeliger (a famous German astronomer, who had advanced in 1906 the accepted Newtonian explanation of Mercury's anomalous advance) had criticized Gerber's derivation claiming that in his calculations Gerber had made an elementary mistake. But the mistake was Seeliger's, according to Roseveare 1982, p. 139.

34 The question of the problematic aspects of the relativistic derivation of Mercury's advance surfaced even at the level of the Nobel committee in 1921-1922: one of the experts, A. Gullstrand, Uppsala, "a scientist of very high distinction", had objected that "other, long-known

Finally, it is tempting to suppose that in science the logical clouds have a providential tendency to disappear rather soon. Not so. Synge, who was the author of one of the classic reference books on relativity, could write *half a century* after Einstein's first formulation of general relativity:

> [...] when one examines some "proofs" in the Neo-cartesian spirit, too often they seem to dissolve completely away, leaving one in a state of wonder as to whether the author really thought he had proved something. Or is the reader stupid? It is hard to say. *In any case I am still waiting for a rational treatment of the dynamics of the solar system according to Einstein's theory.* (Synge 1966, p. 14; italics added)

We can conclude that, even when handling mathematical statements in a professional stance, scientists do not act - as often as not - as logical purists. Instead they tend to rely on what authoritative colleagues say it is proved or will be eventually possible to prove, and by doing so they usually succeed in publishing work which, in turn, will propagate these assumptions. It follows that a historical reconstruction of *the rise and spread of 'acceptable simplifications'* of the original theories - a most interesting topic to which, unfortunately, but little attention has been paid by historians - is essential to the understanding of these theories as dynamical rather than timeless entities.

V. A common sense model of consensus formation

In view of all the problematic aspects we have discussed in the preceding sections, how came that the theory of relativity acquired in a few years the status of the orthodox opinion on light, gravitation, and the cosmos? I shall suggest an explanation which I find plausible, and I will put it in the form of a general model of consensus formation in the scientific (and other) communities. This model is within the purview of common sense and provides, if correct, an effective interpretive tool of many historical and contemporary episodes in science.

First of all, as the story we have reconstructed above exemplifies, it must be conceded that in many cases, and particularly when scientists are dealing with theories opposing each other at a high level, available experimental data and arguments are unsufficient to select a single theory as the 'best' one ('best' according to the standards explicitly declared by disputants). No doubt, a physical theory flagrantly at odds with empirical evidence *of the kind it was designed to explain* will be abandoned sooner or later, at least *if some other theory is available more in agreement with that evidence, and fulfilling the accepted agenda of what a formally 'good' theory ought to be*. However, agreement with empirical evidence and harmony with theoretical ideals (two requirements that are sometimes in conflict between them, and sometimes even in

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deviation from the pure two-body Newtonian law should be re-evaluated with general relativistic methods before there could be even an attempt to identify the residual effect to be explained", as Einstein's main biographer, A. Pais, reports (Pais 1982, p. 509). Pais – who, incidentally, does not mention Gerber's work - comments that this objection is "not very weighty"; but he does not justify in any way this dismissive remark, which I consider incorrect. As is well known, in the end Einstein got the 1921 Nobel prize (in 1922), but the motivation did not mention relativity.

internal conflict) are largely ambiguous and conjectural properties, which cannot be used, more often than not, to settle a controversy to every disputant's satisfaction.[35]

[786] So what happens, to break the symmetry between alternative theories, is that the extent one of them (let us call it T_0) enjoys these desirable qualities starts, for some reason, to be *heavily overstated* by a number of scientists who are authoritative in the field, and who own academic and editorial power sufficient to promote and destroy careers (in the case of relativity, one can mention Planck, Minkowski, Weyl, Eddington and others). These exaggerated claims may even be recognized as such by other scientists, but usually their mere utterance will succeed in provoking a migration of researchers toward T_0, to the detriment of rival theories which will cease to be developed and thus, in a few years, will come to be more or less forgotten. In the meantime the privileged theory T_0 will rapidly grow to become the object of ingent careeristic, financial, cultural and public image investments, and as a consequence it will gain some *real* advantages over its previous antagonists. By now any retreat to these cannot be considered anymore a genuine option, though a minority may still resist T_0's attraction: they are usually punished for their stubborness in various ways, from denial of grants to limited (or no) access to scientific journals and media. As for the shortcomings of T_0 (empirical anomalies, loopholes in arguments), they are reserved a special treatment. It may well happen one day that somebody discovers how to circumvent defects of T_0 which had been previously ignored or even denied by the mentioned authorities. But it may also happen that the new good (empirical, theoretical, and social) qualities, which have emerged during years or decades of massive dedication of the disciplinary community, render superfluous any special apology for those defects, which may even be licensed to be pointed out in mainstream journals. At this stage, in fact, T_0 has become *the received view*, so what were once labelled as its 'difficulties' or 'weaknesses' have been transformed into *research problems* for people working in the field, while its successes are celebrated dayly through different channels, usually in overoptimistic terms.[36] Only historical research - several decades afterwards - will be able to unravel the different strands which led to this situation of theoretical monopoly.

As I said, this simple explanatory model is part of what I consider very accessible common knowledge about human psychology and social interactions. One can check in one's personal experience some aspects of it, like the consistency constraints put on people by initial strong investments (in terms of money, public image etc.) in a single course of action, or the natural reluctance to jeopardize one's professional respectability - let alone one's job. There is also some interesting work of

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[35] Notice that there is no need to assume this as an a priori and universally valid principle: it is more than enough to take it as a sound working hypothesis, which must be checked by historical inquiry in each instance.

[36] Compare the following recent comment: "Where effort is directed by the hope of large grants into, say, the border territory of epistemology with cognitive science, the probability rises that the conclusion that will be reached is that long-standing epistemological questions can be quickly resolved or as quickly dissolved by appeal to this or that work in cognitive science; [...] No one is so naive as to imagine that large grants might be forthcoming to show that cognitive science has no bearing on those long-standing epsitemological questions [...]" (Haack 1997). Of course this situation is not unique to cognitive science: "There is evidence that the predictions of the flux of solar neutrinos varied [during the Sixties] with physicists' need for funding" (Collins & Pinch 1993, p. 129).

social psychologists which gives various degrees of support to the model,[37] but its main interest, from our present standpoint, is that it is an example of a nontechnical argument about science which is obviously very relevant to the understanding of the success of a scientific theory, and so to the understanding of the latter's place in what is called "scientific knowledge" at some point in history.

VI. Epilogue

We have seen that the relationship between science and common sense is much more complicated than many defenders of the primacy of specialistic knowledge would have us believe. Moreover, the increasing weight of large-scale research projects during the last half-century, each employing hundreds of specialists (the "Big Science"), is certainly not making things any simpler.[38] In fact the main reason why science cannot be left to the experts is that even experts have to act in many crucial instances as simple (if this is the right word to use) human beings. The most advanced theories in physics, like the theory of relativity, are part of a web of mutually supporting beliefs about a vast range of heterogeneous topics - from outcomes of measurements to the degree of proficiency of individual researchers -, and cannot be safely isolated from the rest. In other words the ladder of common sense is not only needed in order to climb to the gate of the scientific temple - it also keeps the temple standing.

[787] One should not confuse this position with the claim that 'reality is a social construct', which is sometimes discussed. In fact I do not know whether anyone has ever held this *in a literal sense*, but I doubt it, because it is so clearly self-defeating (is also *social* reality a social construct?).[39] Moreover, anyone writing an essay and referring to other people's articles or books is consciously stating plenty of factual propositions (about those articles and books, at least), which are meant to be taken by the prospective readers in the most 'naive realist' sense. The point which is made here is instead that *our conjectures about the physical world are, in part, cognitively subordinated to our conjectures about the human society*, and that common sense is our guide at the first approximation level for the latter (in fact for both).[40] This gives common sense a cognitive primacy which cannot be denied without seriously damaging the consistency of both the historian's task and the scientist's reliance on indirect sources of information (colleagues, journals, and books). My opinion is that most of what is

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37 See Cialdini 1988, chapter 3 (For instance: "Committment decisions, even erroneous ones, have a tendency to be self-perpetuating because they can 'grow their own legs'. That is, people often add new reasons and justifications to support the wisdom of committments they have already made. As a consequence, some committments remain in effect long after the condition that spurred them have changed" [p. 106]).

38 "In many branches, the more difficult it is to find credible access to the phenomenon, the more dependent the scientist is on apparatus built by others, on data gathered by still others, and on calculations carried through by yet others" (Holton 1995, p. 155).

39 To put it bluntly, what we say about the social dimension of science must be *true*, if it is going to play any role in our understanding of science.

40 To cite a favourite example of common sense 'debunkers', there is no need to refer to Riemannian geometry in order to realize that the Earth is actually flat at a first approximation!

found in the works of sociologists and historians of science with a sociological bent can be interpreted advantageously in this key.

This approach to science and common sense has important consequences as regards the *teaching* of science, and it is with some remarks on this that I wish to conclude. First of all, we should make a serious attempt to give at least an idea of the *intrinsic pluralism of science*, by pointing out alternative theories which may have been discarded not because they were hopelessly wrong, but for pragmatic reasons - like the wish (or the need) to avoid dispersion of resources.[41] Second, what we teach must be clearly rooted in the wider context which gives it human meaning and warrants the committment of participants and other people. We should try, as teachers, to convey to our students the feeling that science is not something indifferent to their efforts and understanding - that we are *asking* them to share with us a certain vision of (a segment of) the history of science and of the structure of the scientific community. Science is a participatory endeavour which cannot prosper unless we make the most of our natural ability to interpret people's actions and place them in the relevant contexts. To enlarge our students' awareness by leaving room in our lectures also for the workings of science as a historically conditioned social activity is, in my view, a much worthier aim than convincing them that science is on a completely different level from the other sectors of life. After all, if science is offered as something confusingly like a religious creed, there can be no wonder if various forms of irrationalism can live and grow in contemporary world side by side with it.

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[41] Of course, to teach more than one theory for a given phenomenology would be partially innovative only for the 'hard' sciences; for the 'soft' sciences this has always been done.

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