To Seek the Truth in the Face of Authority: The Work of R.A. Waldron

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Editor's note: Richard A.Waldron passed away in Northern Ireland on May 24 of this year, shortly before he was to retire from the University of Ulster. Just prior to his death, Professor Waldron had agreed to join the editorial board of APEIRON, and contributed an article on stellar collapse which he eagerly looked forward to seeing published in this journal (see page 4). Richard A. Waldron's youthful enthusiasm for physics and his compulsive desire to discover nature's secrets were at once endearing and inspirational. Thomas E. Phipps Jr. has generously agreed to produce an account of his work, and offers this appraisal of the debt owed by science to an extraordinary human being.

The untimely death of Professor Richard A. Waldron, M.A., Sc.D., F.Inst. P., F.I.M.A., C.Eng., F.*I.E.*R.E., de prives scientific scholarship of one of its most original and courageous minds. The present writer, who has known Professor Waldron only through core spondence, and that for less than a year, is not qualified to provide a proper eulogy. Since a scientist is known by his works, I shall comment on the small portion of Waldron's extensive work that has come to my attention.

Primarily, Waldron was a modern advocate (1966, 1977, 1979a,b, 1980) of the "ballistic" theory of light due to Ritz (1908). According to this view the photon (subject to Galilean kinematics) acquires the velocity of its emitter, relative to which its speed is *c*. The concomitant variant of electromagnetic theory can be represented in field theoretical terms, but is more efficiently expressed in terms of a law of direct force action between charges. The latter tradition of electromagnetic description predates Maxwell and goes back at least to the work of Wilhelm Weber before 1850. Weber was the first—and some would say the only—"true relativist," in that he employed no frame- or observer-related velocity parameters of the type that subsequently made their appearance in the Lorentz force law. Waldron proposed (1981) to replace Coulomb's law of force between two charges by a force law of the form

$$F = \frac{q_1 q_2}{4\pi\varepsilon_0 r^2}\sqrt{1+\left(\frac{u}{c}\right)^2} = \frac{q_1 q_2}{4\pi\varepsilon_0 r^2}\left(1+\frac{1}{2}\frac{V^2}{c^2}+\ldots\right)$$

where V is the relative velocity between the two charges. Except for an important algebraic sign, this agrees to second order (neglecting acceleration) with a proposal (Graneau 1985) of Weber's for a relative velocity dependent force law of the form

$$F = \frac{q_1 q_2}{4\pi\varepsilon_0 r^2}\left(1-\frac{1}{2}\frac{r'^2}{c^2}+\frac{rr''}{c^2}\right)$$

Waldron (1981a) gave to his formula an interesting and original interpretation; namely, that V was related to the velocity u appearing in the theory of Lorentz-Einstein by

$$\sqrt{1+\left(\frac{V}{c}\right)^2} = \frac{1}{\sqrt{1-\left(\frac{u}{c}\right)^2}}$$

and that V was the relative velocity of charges measurable by clocks at rest in the laboratory. For this reason (since $u \to \infty$ as $u \to c$) he predicted that superluminal velocities should be observable in the laboratory. A more conventional interpretation, which would not lead to such a conclusion, is that V is the Einsteinian proper velocity of a charge—namely, $V = \mathrm{d}r/\mathrm{d}\mathbf{t}$, where t is particle proper time, and that u is ordinary frame-time velocity, $u = \mathrm{d}r/\mathrm{d}t$. Such an assumption leads to precisely the above functional relationship between V and u. But it would mean, contrary to Waldron's interpretation, that V is not measurable in the laboratory because it depends on the reading of a clock co-moving with the high-speed particle. Later (1980), Waldron was led to modify his formulation, in the light of further empirical evidence, but he still maintained the desirability of doing experiments in which actual times of flight of high-speed particles are measured directly rather than inferred from electro magnetic theory. In this there is no questioning that his position was scientifically both sound and conservative.

Thus we trace one of the principal feature's of the man's character that distinguish him from both the average "normal scientist"—who never questions whatever indoctrination higher education may bestow on him—and from the run-of-the-mill scientific heretic—who tends to fix upon one idea and cling to it against all evidence. Waldron was intensely curious (in the classic way the ideal scientist is supposed to be) about the description of nature and was driven by that curiosity continually to modify his hypotheses in response to the facts. Such willingness to entertain new ideas but not to fall irrevocably in love with them marks the delicate balance that is essential to the attainment of a maximum rate of scientific progress... but that is regrettably rare among professional practitioners of our day.

There is another major feature of the above-de scribed situation that has broad implications for all of physics; namely, that *interpretation* looms as the major stumbling block to the joining-up of mathematics with physics. The attitude prevalent today is that the main problem in theoretical physics is to hit upon the right mathematical formalism, and that physical interpretation will follow as a simple—perhaps even unnecessary—adjunct. In other words, the math is the hard part and interpretation is practically automatic. But the facts of the history of science point in precisely the opposite direction. They suggest that again and again physicists have from diverse viewpoints hit upon similar-looking mathematics, and have agreed about formulas, but have differed, sometimes bitterly and permanently, on the physical interpretation needed to give operational meaning to the formulas. Quantum measurement theory is a particularly notorious example of how even perfect agreement about the mathematics leaves wide areas for disagreement about interpretation. In short, history can be read as saying that the mathematics of physical description is the easy part... the hard part the part that perennially gives rise to lasting disputes among intelligent people—being the interpretation.

Waldron's approach to electromagnetic theory shows that even within the supposedly "conquered hinterlands" of classical physics dramatically different physical interpretations—and mathematical formulations—may ultimately prevail. Maxwell was well aware of this enduring aspect of the physicist's problem, for he remarked (Maxwell 1954) (in reference to field *vs*. action-at-a-distance modes of description), "The comparison, from a philosophical point of view, of the results of two methods so completely opposed in their first principles must lead to valuable data for the study of the conditions of scientific speculation." Unfortunately, the conditions of the scientific dialogue have so far deteriorated in our time that Waldron found his speculations castigated (Anonymous Reviewer 1980) as evidence of "inadequate understanding of Lorentz invariance." This is rather as if contemporary authority in reviewing Maxwell's *Treatise* had judged it to reveal inadequate understanding of action-at-a-distance. Thus we en counter another invariant aspect of scientific enterprise: Original thinking of any kind "opposed in first principles" to whatever is popular at the moment will be judged by prevailing (*i.e.*, by irrelevant) first principles, and thus by definition will be misjudged.

The guiding idea behind Waldron's electromagnetic work, like that behind Weber's, is that all of electro dynamics is derivable from a single formula for the force (or potential) between two charges, as a function of the instantaneous separation distance between the charges and time derivatives of that distance. Any effects of retardation of action are described by the time derivatives. Weber showed, and Waldron's work confirms, that not only electric but inductive and magnetic effects can be derived from such a precept. The idea is exciting in its superb simplicity. Since it has never been empirically discredited it deserves the continuing attention of physicists who may now or in the future seek alternatives to Einstein's physics.

Waldron's attempt (1984) to extend the same descriptive scheme to gravitation, substituting for c a "speed of gravity" parameter, g, seems less successful, since he had to assume that $g = c/\sqrt{6}$ in order to match the precession anomaly usually attributed to Mercury, and also was off by the traditional factor of 2 from the recognized value (given by general relativity) of the gravitational deflection of light. Laplace showed already in the eighteenth century that if $g < 10^8\, c$ it is difficult to reconcile Newtonian mechanics with the observed long-term stability of the solar system. In other words, regardless of the ultimate truths revealed by general relativity, gravity gives a remarkably precise simulation of instant action at a distance... so precise that no observation to date has ever falsified it. In much the same way, Newton's third law, though discredited by relativity theory, has never been discredited by observation. Indeed, the modern physicist has been so busy discrediting all things connected with the history of his craft that he has almost maneuvered himself into discrediting Mother Nature herself—in that he at tributes to her the mendaciously deceptive behaviour of simulating or imitating with annoying exactitude certain noncovariant simplicities for whose lack of scientific truth mankind has Einstein's enduring and solemn word.

Waldron did not confine to electrodynamics his challenges to authority. He gave an ingenious argument (1983a, 1990a) to show that black holes do not exist because they cannot form. He questioned that the universe is expanding (1981b) and gave a non-Doppler explanation (1985) of the Hubble redshift in terms of a possible aging effect (instability) of the photon. He also proposed a model of the spinning photon (1983b), of which a sympathetic reviewer remarked (Anonymous reviewer 1983), "... he has taken the ballistic viewpoint of light and related phenomena further along the road than anyone before him."

At the time he died Waldron seemed to be reaching a crescendo of productivity, with a paper (1990b) on the form of force laws not yet in press. He was contemplating retiring to Ipswich in Suffolk from the Department of Mathematics at the University of Ulster. There is little doubt that in his retirement he would have contributed as much again to physics as ever before. Those who from a basis of knowledge and experience yield to an inner compulsion to seek the truth in the face of all human authority are rare spirits indeed. Planck said that new ideas in science triumph by outliving their detractors. The premature silencing of men like Hertz, Ritz, O'Rahilly and Waldron suggests a slightly less salutary role for death in the history of science. Are they not bad ideas, perhaps, that need death to sustain them?

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