Annex 1

(Copy of acknowledgment which appeared in the *Notices of the American Mathematical Society*, April 2007.)

Annex1

Letters to the Editor

Retarded Differential Equations and Quantum Mechanics

G. W. Johnson and I wish to draw attention to the work of C. K. Raju that is related to some of the ideas discussed by Sir Michael Atiyah in his talk "The Nature of Space", which we reported on in the June/July 2006 issue of the *Notices*. Ideas suggesting a link between retarded differential equations and quantum mechanics were put forward some years ago by Raju, and we, along with Atiyah, believe they deserve attention. Interested readers are encouraged to read, in particular, the following papers written by Raju:

1. *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994 (Fundamental Theories of Physics, vol. 65), ch. 5b "Electromagnetic time" (pp. 116–122), and ch. 6b "Quantum mechanical time" (pp. 161–189).
2. *The Eleven Pictures of Time*, Sage, 2003, pp. 298–302.
3. "The electrodynamic 2-body problem and the origin of quantum mechanics", *Foundations of Physics*, **34**, (June 2004), 937–962.

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The Proof of the Poincaré Conjecture

In recent months there has been considerable attention devoted to the proof of the Poincaré conjecture that was started by Hamilton and completed by Perelman, both among mathematicians and in the media. Although most of the reports are largely true to fact, some incorporate false statements or innuendo which I believe are irresponsible. I am writing this letter to set the record straight.

Let me say first that the Hamilton-Perelman proof of the Poincaré conjecture is a great triumph for mathematics in general and for geometric analysis in particular. I am privileged to have participated in the nurturing of geometric analysis from its infancy to its adulthood. There were the good old days when ideas were shared and new frontiers were explored. It was during this period that the Ricci flow was introduced and investigated by Hamilton. Thirty years later, geometric analysis has reached maturity, and the proof of the Poincaré conjecture is perhaps its most spectacular success to date. I expect many more successes to come.

The achievements of Hamilton and Perelman in solving the conjecture, especially their major breakthroughs on singularities of nonlinear parabolic systems and the structure theorem for 3-dimensional manifolds, are unparalleled. They far exceed the established standards for Fields Medals. I fully support, and have always said so, the award of the Fields Medal to Perelman. (In my view, Hamilton clearly deserves the Fields Medal also, but he is not eligible at this time because of the age restriction.) For anyone to suggest in words or a cartoon that my position has ever been anything but that is both offensive and completely untrue.

Proving the Poincaré conjecture is an intricate and daunting process. In a work of this scale, it is understandable that when Perelman released his manuscripts on arxiv.org, several key steps were merely sketched or outlined. These manuscripts posed a tremendous challenge for the math community to digest. For two years many top experts in the field of geometric analysis worked hard and made steady progress in understanding and clarifying Perelman's papers. At the end of 2005, Cao and Zhu completed a three-hundred-plus-pages manuscript that provided a complete account of the Hamilton-Perelman proof of the Poincaré conjecture. This paper provides the proof in a form that finally can be understood by researchers in the field.

This past summer while I was in China, I held a press conference and also gave a public lecture on the Poincaré conjecture. My press conference addressed a group of Chinese reporters. Its intention was to encourage young Chinese mathematicians and scientists to be more ambitious and seek the frontiers of research being done worldwide and not just in China. Young mathematicians in China need not just encouragement but a better perspective of what the most exciting and promising directions of research are. My public lecture in Beijing on June 20 [2006] was addressed to the mathematics community and a large group of string theorists. In that talk I focused on the achievements of Hamilton and Perelman. Since Cao and Zhu managed to put together in writing the details of the deep ideas of Hamilton and Perelman, I praised them as well, hoping this would encourage their fellow mathematicians in China.

Over the years I have inherited from my teacher S. S. Chern the strong belief that it is the duty of any mature mathematician to train the next generation. Since he and I both come from China and there are many talented young Chinese mathematicians who are not exposed to modern mathematics, we have spent a lot of time helping mathematicians and students in China. We devoted a lot of time discussing the challenges and working together to address them. Thanks to his leadership, there are now many outstanding Chinese-trained mathematicians in Western universities. Over the last twenty years, Chern and I have also been trying to develop mathematics within Chinese universities. Because of the Cultural Revolution, the recovery has been slow. But thanks also to the help of many friends from the West, the situation is improving.

There have been uninformed reports on how the Cao-Zhu paper was handled by the *Asian Journal of Mathematics*, as well as on the joint work by Lian, Liu, and me on the mirror symmetry conjecture. Regarding the former, rumor has it that the normal peer review process had been tossed out the window. On the contrary, it took the journal several months to go through the established process until the paper was accepted for publication. After receiving the submission in December 2005, I asked, without success, several leading experts on geometric flows, including Perelman, if they would referee the paper. Under the circumstances, I myself took on the referee's task. After attending more than sixty hours of

Annex 2

(Some relevant extracts from the 1994 book *Time: Towards a Consistent Theory*, chp. 5b, the 2004 paper "The electrodynamic 2-body problem and the origin of quantum mechanics". [Chp. 6b ("Quantum mechanical time") of the above book, connecting FDE to quantum mechanics via the structured-time interpretation of quantum mechanics, is too long to include here.] Also relevant extracts from the book *The Eleven Pictures of Time*, which explains matters in a non-technical way. These were three references to my work acknowledged in the April 2007 issue of the *Notices*.)

1 Introduction

Annex 2

Time: Towards a Consistent Theory, chp. 5b

CHAPTER IV ended with the hope that the introduction of the field may help to resolve the paradoxes of thermodynamics. But in Chapter VA we saw that the introduction of the field seems to create more problems than it solves.

Let us, for the moment, forget about the field, or else let us adopt the point of view that the field is a dispensable intermediary between particles. Let us look, instead, at the nature of the many-body problem (of electrodynamics), and the novel features arising from the *finite speed of interaction*: to measure time, or anything else, one must postulate that the speed of light is constant.

What are the consequences of the finite speed of interaction? We recall Poincaré's remark: '*The state of the world will depend not only on the moment just preceding, but on much older states.*' In both cases we obtain what Poincaré called (p 65) '*equations of finite differences*'. As Poincaré further argued, the substance of physics lies in its mathematical formalism — the 'mechanical explanations' are redundant. So, between fields and particles, it matters little which mental picture we feel comfortable with.

Poincare on FDE

How does the finiteness of the speed of interaction affect the underlying equations? To see this, consider a system of n particles. In the field picture, an accelerated charged particle e_1 gives out retarded radiation which is incident upon other charged particles e_2, e_3, ..., e_n. The outgoing retarded wave accelerates other charged particles at *later* or retarded times. In the particle picture, only these accelerations matter: the acceleration of e_1 at time t depends upon the acceleration of e_2, e_3, ..., e_n, at past or retarded times, say $t-\tau_2$, ..., $t-\tau_n$. Ignoring, for the moment, the question of self-action and radiation damping, the difference between the field-picture and the particle-picture does *not* show up mathematically.

2 The two-body problem of electrodynamics

2. 1 Formulation

For example, the equations of motion of two charged particles *i* and *j*, in *one* dimension, interacting solely through retarded radiation (and without radiative damping) take the form:[1]

$$\ddot{z}_i\,(s_i) \;=\; \frac{k}{m}\,\frac{[1+\dot{z}_i^2(s_i)]^{1/2}\;\; sgn\;[z_i(s_i)-z_j(s_j)]}{\{[z_i(s_i)-z_j(s_j)]\dot{z}_j(s_j)-\,|\,z_i(s_i)-z_j(s_j)\,|\,c\dot{t}_j(s_j)\}^2}\;\;, \tag{1}$$

where (t, z), with the appropriate subscript, denote the coordinates of the world-lines of the particles, dots denote differentiation with respect to the proper times s_i, s_j of the two particles, and $(i,j) = (1,2)$ or $(2,1)$. Given s_i, the *retarded proper time* s_j in the above equation, corresponds to the point at which the backward null cone from the point $(t_i(s_i), z_i(s_i))$ meets the world line of particle *j*. This is obtained from (see Fig. 5):

$$c\Big[\,t_i(s_i)-t_j(s_j)\Big] \;=\; |\,z_i(s_i)-z_j(s_j)\,|\,. \tag{2}$$

The difficulty of having two independent variables can be removed by rewriting (1) as

$$\frac{v'_i(t)}{[\,1-v^2{}_i(t)/c^2\,]^{3/2}} \;=\; \frac{(-1)^i\,k}{m_i\tau^2{}_{ji}(t)}\;\cdot\;\frac{c-(-1)^j v_j(t-\tau_{ji}(t))}{c+(-1)^j v_j(t-\tau_{ji}(t))}, \tag{1′}$$

where $k \;=\; -e_1\,e_2/c^2$, the explicit retardations

$$\tau_{ji}\,(t) \;=\; t - t_j\,(s_j) \tag{2′}$$

are obtained from equation (2), primes denote differentiation with respect to *t*, and $v_i = z'_i$ is the velocity.

Reforming the notation (to use bars rather than subscripts), and using units with $c = 1$, these equations may be rewritten:

$$\frac{v'(t)}{[\,1-v^2(t)\,]^{3/2}} \;=\; \frac{b}{\tau^2}\;\cdot\;\frac{1+\bar{v}(t-\tau)}{1-\bar{v}(t-\tau)}\,, \tag{3a}$$

$$\frac{\bar{v}'(t)}{[\,1-\bar{v}^2(t)\,]^{3/2}} \;=\; -\frac{\bar{b}}{\bar{\tau}^2}\;\cdot\;\frac{1-v(t-\bar{\tau})}{1+v(t-\bar{\tau})}\,, \tag{3b}$$

where $b = -k/m$, $\bar{b} = -k/\bar{m}$, and

$$\tau(t) = \left| x(t) - \bar{x}(t-\tau) \right|, \tag{4a}$$

$$\bar{\tau}(t) = \left| \bar{x}(t) - x(t-\bar{\tau}) \right| . \tag{4b}$$

We can now see the difference quite clearly: *the character of the differential equation has changed.* The system of equations (3) is no longer a system of simple differential equations, but is a system of 'difference-differential equations' or a system of ordinary differential equations (o.d.e.'s) with *retarded deviating arguments*. The 'accelerations' of the particles at time t depend upon their velocities v, $\bar{v}$ at past or retarded times, $t-\tau$, $t-\bar{\tau}$.

What is the significance of this change? To study the two-body problem of electrodynamics, one must study o.d.e.'s with deviating arguments: even the simplest qualitative features of such o.d.e.'s completely destroy the Newtonian paradigm, and suggest a resolution of the paradoxes of thermodynamics, and the paradoxes of advanced action.

Effect on Newtonian paradigm

2. 2 Some definitions

A first-order o.d.e. has the form

$$x'(t) = f\,(t, x(t)), \tag{5}$$

where f is some function (generally non-linear). It is well known that the most general system of o.d.e.'s can be reduced to a system of such equations, i.e., to the form (5), regarding x as a vector, if necessary. It is also well known that (under some mild requirement of continuity on f) prescription of the 'initial value' $x(0)$, at an instant $t = 0$, determines a unique solution $x(t)$ of (5) in a neighbourhood $[-\delta, \delta]$ of $t = 0$. This is the Newtonian paradigm.

Newtonian paradigm defined.

In contrast, a differential equation with deviating arguments has the form

$$x' = f\,(t, x(t), x(t-\tau)\,). \tag{7}$$

The 'dependent variable', the function x, appears for more than one value of its argument, the 'independent variable', t.

An equation with deviating arguments is classified as *retarded*, or *history dependent*, if the highest order derivative of the unknown function appears for exactly one value of the argument, and this argument is not less than all the arguments of the unknown function and its derivatives appearing in the equation. For example,

$$x'(t) = f\,(t, x(t), x(t-\tau(t))\,) \tag{8}$$

is called retarded if $\tau(t)>0$.

Similarly, the equations of motion of charged particles interacting solely through advanced radiation correspond to *anticipatory* behaviour, or an o.d.e. with *advanced* deviating arguments,

$$x'(t) = f\,(t, x(t), x(t+\tau(t))). \tag{8}$$

The definition requires the same proviso as above, except that now the argument of the highest order derivative must be less than or equal to all the other arguments of the unknown function, i.e., $\tau(t)>0$ in (8), or $\tau(t)<0$ in (7).

More generally, a system of charged particles interacting through both advanced and retarded radiation displays *partly anticipatory* behavior, corresponding to an o.d.e. with *mixed-type* deviating arguments:

$$x'(t) = f(t, x(t), x(t-\tau_1(t)), x(t+\tau_2(t))). \tag{9}$$

From now on, equations of the type (7), (8), and (9) will be referred to as retarded, advanced, and mixed-type o.d.e.'s. The mathematical theory of such differential equations with deviating arguments, also known as functional differential equations, differs from the mathematical theory of the usual differential equations.

2. 3 The recurrence paradox and the past-value problem

Van Dam and Wigner[2] considered equations involving both retarded and advanced fields. They asserted (without proof) that instantaneous positions and velocities were sufficient to determine unique trajectories.

Now, with electromagnetic interactions taken into account, the many-body equations of motion (3) are retarded o.d.e.'s. However, for the simplest model of even a retarded differential equation, modeling a history-dependent situation, it is inadequate, in general, to provide initial data at a point. Consider, for instance, the o.d.e with constant retardation $\pi/2$,

$$x'(t) = x(t-\frac{\pi}{2}), \quad t \geq 0\ . \tag{10}$$

To obtain a unique solution, it is insufficient to specify only the state at one point of time, say $x(0)$. Thus, $x = \cos t$ and $x = \sin t$ are obvious solutions, and since the equation is linear $x = a\cdot\cos t + b\cdot\sin t$ is a solution for arbitrary constants a and b, and both a and b cannot be determined from a knowledge of $x(0)$.

Past history is required

Since the behavior is history-dependent, it is more reasonable to ask for a unique solution after prescribing the *past history*, i.e., an *initial function* $x = \varphi$, over the relevant part of the past: the interval of retardation, $[-\pi/2, 0]$.

In general, a unique solution of the past-value problem for the retarded system,

$$x'(t) = f(t, x(t-\tau_1(t), t-\tau_2(t), ..., t-\tau_n(t))), \tag{11}$$

may be obtained under the following sufficient conditions.[3] (i) All delays, τ_i, are bounded, and (ii) some technical conditions such as a local Lipshitz condition and a continuity condition are satisfied.

From the point of view of thermodynamics, the interesting conclusion is the following. The hypotheses underlying the recurrence paradox have been destroyed: *there is no longer a unique trajectory through each point of phase space*. More than one trajectory may pass through each point of phase space; trajectories may intersect (Fig. 1).

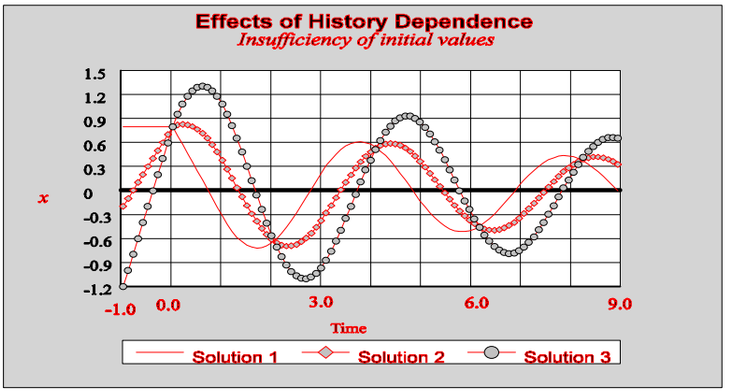

Fig. 1: Effects of history-dependence

Three solutions of a model first-order delay equation, showing the effects of history-dependence. All three solutions have the same initial value x(0), though the past histories prescribed over the time-interval [-1, 0] are different. Note also the discontinuity in the derivative of solution 1, visible at t=0.

2. 4 The reversibility paradox: time asymmetry of delay

For retarded o.d.e.'s the intersection of trajectories takes place preferentially towards the future, in a way that destroys the hypothesis underlying the reversibility paradox. An ordinary differential equation is time symmetric: it may be solved either forward or backward in time. From a knowledge of the current values, Newton's laws may be used to predict the future or retrodict the past. However, a retarded o.d.e. relates past causes to current effects. Such an equation may be solved forward in time, but not, in general, backwards in time.

Consider the following ordinary, linear, retarded differential equation with constant coefficients, and constant retardation r:

$$x'(t) = a\,x(t) + b\,x(t-r), \tag{12}$$

with b different from zero and $r>0$. To solve the equation backwards, it is only necessary to solve an algebraic equation,

$$x(t-r) = \frac{x'(t)-a\,x(t)}{b}, \tag{13}$$

to obtain the solution on $[t-2r, t-r]$, given $x = \varphi$ on $[-r, 0]$. For nonlinear equations, this already means that backwards solutions will not be unique. For the case under consideration, suppose $a\neq -b$ and we prescribe $\varphi(t) \equiv k$, a constant, on $[-r, 0]$, and ask for a backwards solution for $t \leq 0$. Then (13) implies that $x(t) = -ak/b$ so that the unique solution of the algebraic equation (13) fails to be continuous, and hence differentiable. Therefore, a (continuous) backwards solution of (12) does not exist in general.

Of course, one could think of choosing a final function in such a way that the solution exists. But then the solution would, in general, fail to be unique. Consider

$$x'(t) = b(t)\,x(t-1), \tag{14}$$

where b is any sufficiently smooth (e.g. continuous) function which vanishes outside $[0, 1]$, and with

$$\int b(t)\,dt \;=\; -1. \tag{15}$$

For example,

$$b(t) \;=\; \begin{cases} 0 & t \le 0, \\ -1 + \cos 2\pi t & 0 \le t \le 1, \\ 0 & t \ge 1. \end{cases} \tag{16}$$

For $t \le 0$, (14) reduces to $x'(t) = 0$ so that, for $t \le 0$, $x(t) = k$ for some constant k. Now if k is *any* constant then, for $t \in [0,1]$,

$$\begin{aligned} x(t) \;&=\; x(0) + \int_0^t x'(s)\,ds \\ &=\; x(0) + \int_0^t b(s)\,x(s-1)\,ds \\ &=\; x(0) + x(0)\int_0^t b(s)\,ds, \end{aligned} \tag{17}$$

since $x(s-1) \equiv k = x(0)$ on [-1, 0]. Hence, using (15), $x(1) = 0$ no matter what k was. But $x(1) = 0$, and $b(t) = 0$ for $t \ge 1$, implies, by (14), that $x(t) = 0$ for *all* $t \ge 1$. Consequently, (14) does not admit a unique backwards solution even if we prescribe future data for all future times $t \ge 1$. Thus, if φ differs from 0 on $[1,\infty)$ there are no backward solutions. But, if $\varphi \equiv 0$ on $[1, \infty)$, the solutions branch into the past (Fig. 2), and there is no way to pick a unique solution from the infinity of continuous solutions that are available.

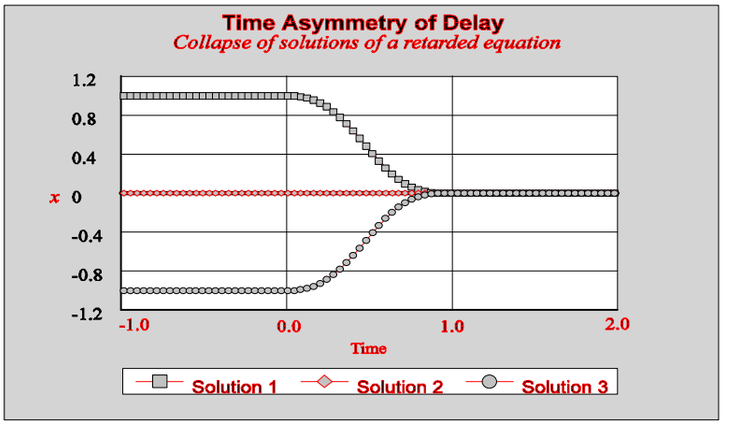

Fig 2: Time asymmetry of delay

Three solutions of the retarded equation (14) which collapse towards the future. The different past histories, presecribed over the time-interval [1,0], all result in the same future solution for t≥1. Retrodiction is hence impossible from future data prescribed over t ≥ 1. Teleological 'explanations' are impossible with history-dependent evolution.

2. 5 Preacceleration: the Taylor-series approximation

We saw in Chapter VA that the study of radiative damping and, in particular, the Schott term, leads to equations that are of the third order in time, resulting in the preacceleration of the electron. Dirac, in 1938, obtained these equations by means of a Taylor expansion which seems unavoidable.[4] Many other authors[5] have attempted similar approximation procedures, using a Taylor series expansion to get rid of retarded/advanced expressions, in dealing with the two-body problem in electrodynamics and gravitation. Physically, this procedure means that we model a history-dependent system by an instantaneous system with additional degrees of freedom.

Einstein's mistake

This procedure is known to be, in general, invalid. This may be seen from the following counter-example,

$$x'(t) = -2\,x(t) + x(t-r), \tag{18}$$

where $r>0$ is a small constant. Every solution of this equation is bounded[6] and tends to zero as $t\to\infty$. But if we choose the Taylor series approximation to the right hand side and truncate after two terms, we obtain

$$x'(t) = -2\,x(t) + [x(t)-rx'(t) + \frac{1}{2}\,r^2x''(t)], \tag{19}$$

which admits exponentially increasing solutions $x(t) = c\,\exp(\alpha t)$, with $\alpha > 0$. Thus, the Taylor approximation of (18) by (19) leads to qualitatively incorrect behaviour, no matter how small r is, so long as $r>0$.

It may be shown that it is not the order of the approximation which is at fault: with instantaneous data, even an infinite number of degrees of freedom is inadequate. The order of the approximation does, however, make a difference from the numerical point of view, as pointed out by El'sgol'ts,[7] 'since the transition is equivalent to the rejection of the term with the highest order derivative in an *unstable-type differential equation* with a small coefficient before the highest derivative.'[Emphasis mine]

In the usual treatment of the numerical solution of retarded o.d.e.'s, attention is focused upon the discontinuities that might arise at the ends of delay intervals (e.g. Solution 1 of Fig. 1). However, one would expect the general electrodynamic many-body problem to be 'stiff': there could be oscillations at widely varying frequencies. In view of the Dahlquist barrier,[8] A-stability fails for any rule higher than the trapezoidal rule, so that the Taylor approximation could be numerically misleading for derivatives of order greater than two.[9] Thus, Dirac was perhaps right in a way when he rejected the higher order terms as too complex to apply to 'a simple thing like the electron'.

To summarize, the origin of the Schott term in the Lorentz-Dirac equation of motion is mathematically dubious, and can result in qualitatively incorrect behaviour, though it may yet provide a more robust numerical approximation than would be obtained by the inclusion of higher-order relativistically covariant terms. The alternatives that have been proposed,[10] to the Lorentz-Dirac equation, have not proved satisfactory.[11]

VIB QUANTUM-MECHANICAL TIME

ABSTRACT. We present a brief exposition of the orthodox axiomatic approach to q.m., indicating the relation to the text-book approach. We explain why the usual axioms force a change of logic. We then explain the attempts to *derive* the Hilbert space and the probability interpretation from a new type of 'and' and 'or' or a new type of 'if' and 'not'. Included are the Birkhoff-von Neumann, Jauch-Piron, and quantum logic approaches, together with an account of their physical and mathematical obscurities.

Instead of entering the labyrinth of subsequent developments, which seek new algebraic structures while accepting the old physical motivation, we present an exposition of the structured-time interpretation of q.m., which seeks a new physical motivation.

We saw in Chapter VB that, with a tilt in the arrow of time, the solutions of the many-body equations of motion are intrinsically non-unique. In Chapter VIA we had indicated how this non-uniqueness relates to a change in the logic of time. We now explain how the resulting changes in the logic and structure of time lead to a new type of 'if' and 'not', of the kind required by q.m., while escaping from the criticism which applies to the earlier 'quantum logic' approaches.

We briefly indicate the analogy between this logic and the temporal logic required for the formal semantics of parallel-processing languages like OCCAM, and distinguish the structured-time interpretation from the superficially similar many-worlds interpretation and the transactional interpretation of q.m.

1 Introduction

THE preceding chapter introduced the problem of a non-trivial structure of time: the (local) topology of time, in the real world, might be different from that of the real line. The real-line topology differs from the mundane view of a past-linear future-branching time, used to demarcate and validate physics. Moreover, there is possible incoherence about the structure of time, even within physics, as different structures may be simultaneously implicit.

We explained how the notion of a structure of time could be formalized in terms of properties of the earlier-later relation (U-calculus) or, more generally, using an appropriate (temporal) logic.

This chapter deals with two earlier claims (a) that an appropriately structured time could be related to the change of logic required by the axiomatic formulation of q.m., and (b) that the hypothesis of a tilt in the arrow of time implies such an appropriate structure. The other consequence of the basic hypothesis, viz. non-locality, is hardly a serious drawback since we saw in Chapter VIA that locality is a fuzzy and metaphysical requirement which lacks a basis even in classical physics.

To reiterate, the aim of this chapter is to present an exposition of the structured-time interpretation of q.m. which relates the many-body equations of motion of non-local classical (relativistic) mechanics, the emergence of a logical structure, or a non-trivial topology of time,[1] and the mathematical formalism of q.m.

§ 2 presents an exposition of the orthodox Hilbert-space axiomatics of q.m. and relates it to the usual textbook approach. § 3 explains why the orthodox axiomatic approach forces a change of logic and goes on to present an exposition of the 'quantum logic' approach, its relation to the Hilbert-space axiomatics, and its obscurities. The idea is to distill the body of q.m. to its algebraic and logical 'skeleton'. Finally, § 4 presents the structured-time approach,

From, "The electrodynamic 2-body problem and the origin of quantum mechanics":

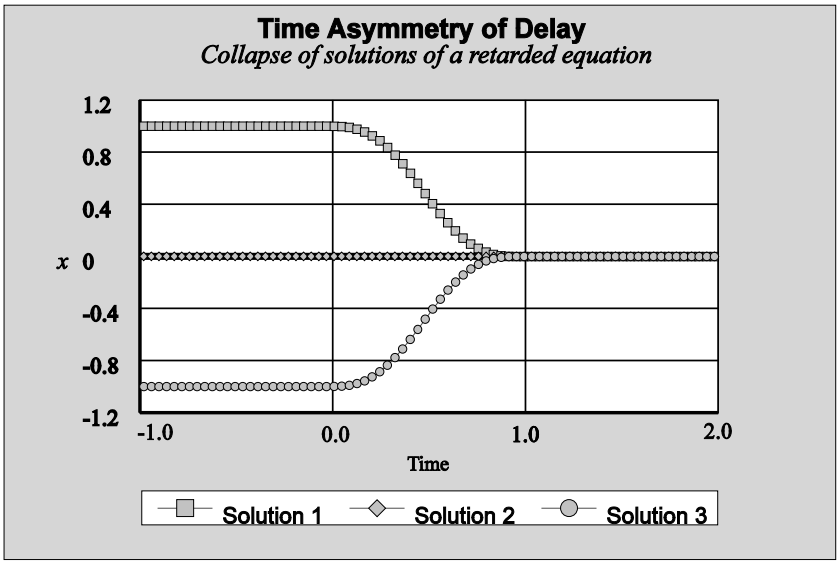

Fig. 1. The figure shows three different solutions of a retarded FDE $\dot{x}(t) = b(t)x(t-1)$, for a suitable choice of the function $b(t)$. Three different past histories prescribed for $t \leq 0$ lead to three different solutions all of which coincide for $t \geq 1$. Such a phase collapse is impossible with ODE where trajectories in phase space can never intersect. Because of this phase collapse, FDE, unlike ODE, cannot be solved backward, from prescribed future data.

Thus, on grounds of the known mathematical differences between FDEs and ODEs, it is reasonable to doubt, a priori, that the full electrodynamic force (6) can be validly approximated by the Coulomb force, by neglecting the vector potential.

A good way to settle this doubt is to put the matter to test by comparing the Coulomb-force solution with the solutions obtained using the full electrodynamic force. This requires a solution of the 2-body problem with the full electrodynamic force, and we accordingly proceed to calculate such a solution.

2. SOLVING FDES

2.1. Method 1

The mathematical features of FDEs briefly recapitulated above suggest that retarded electrodynamics involves a "paradigm shift" from "classical mechanics", for we now need past data rather than instantaneous

Need for past data as a paradigm shift.

data. In a debate on this question at Groningen, H. D. Zeh argued against any such paradigm shift. Zeh maintained that there was no need for past data, since data prescribed on a spacelike hypersurface (corresponding to an instant in spacetime) was adequate to solve the system of Maxwell's partial differential equations (PDEs) together with the Heaviside–Lorentz equations of motion for each particle.

The 1999 debate on the need for past data or a paradigm shift

Given all fields on a spacelike hypersurface, one can evolve them forward in time for a small region. *Given all fields*, in the vicinity of a single particle, its equations of motion reduce to a system of ODEs which can be solved in the usual way. We will call this method 1. We note that it implicitly involves the simultaneous solution of a *coupled* system of PDEs and ODEs.

2.2. Method 2

In contrast to method 1, the FDE formulation of the electrodynamic 2-body problem may be geometrically visualized as follows.[13] Assuming retarded potentials, the force on particle 1 at a given spacetime point $(a, 0)$ is evaluated as follows. One constructs the backward null cone with vertex at $(a, 0)$, and determines where it intersects the world line of particle 2, at a point (b, t), say (see Fig. 2). Given the world line of the particle 2 in a neighborhood of (b, t) one evaluates the resulting Lienard–Wiechert potential at $(a, 0)$, and uses this retarded potential to calculate the force on particle 1 at $(a, 0)$. This is the force given by (6). The force on particle 2 at any point is calculated similarly. Clearly, we can solve for the motion of either particle, only if we are given the appropriate portions of the *past* world line of the other particle. We will call this method 2.

2.3. Relating the two Methods

The Groningen debate brought out the following difficulty. *Both* the above methods seem to have the *same* underlying physical principles (Newton's laws of motion, possibly in a generalized form suitable for special relativity + Maxwell's equations). How, then, can these principles admit fundamentally incompatible interpretations of instantaneity and history-dependence?

2.4. The Need for Past Data

This issue can be readily resolved as follows. If retarded propagators (i.e., Green functions, or fundamental solutions of the wave equation, or retarded Lienard–Wiechert potentials) are assumed, then the field at any

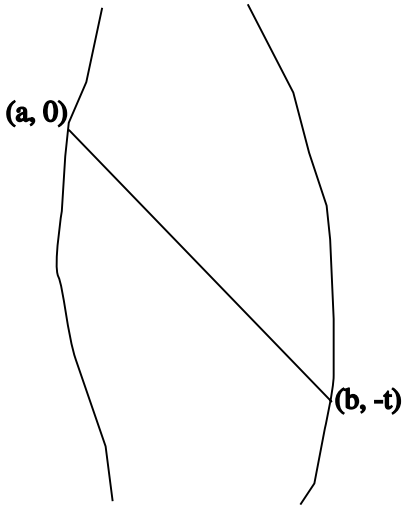

Fig. 2. To calculate the force acting on particle 1 at $(a, 0)$, one draws the backward null cone with vertex at $(a, 0)$. If this intersects the world line of the second particle at $(b, -t)$, one calculates the Leinard–Wiechert potential at $(a, 0)$, due to the motion of particle 2 at $(b, -t)$. Thus, determination of the present force on particle 1 requires a knowledge of the past motion of particle 2.

point x relates to particle movements in the past at the points x_a and x_b on the respective world lines of particles a and b, where the backward null cone from x respectively meets the world lines of the two particles (Fig. 3). Thus, to know the field at a point x we should know the particle world lines at and around the *past* points x_a, and x_b. If we do this for every point x on a spacelike hypersurface, the points x_a, and x_b will, in general, cover the entire past world lines of the two particles. Thus, in the 2-body context, given the assumption of retardation, prescribing the fields on a spacelike hypersurface is really equivalent to prescribing the *entire* past histories of the two particles. That is,

instantaneous data for e.m. fields = past data for world lines of particles

That is, the PDE + ODE method 1 only *hides* the underlying history-dependence of electrodynamics. (The above remarks need to be appropriately modified if, instead of assuming retardation, one assumes advanced or mixed-type propagators. For example, if we use advanced propagators, then 'anticipation' should be used in place of 'history-dependence', etc.)

2.5. FDE vs. ODE + PDE

While *both* methods require past data on the motion of the two particles, the intuitive schema underlying method 1 is currently inconvenient

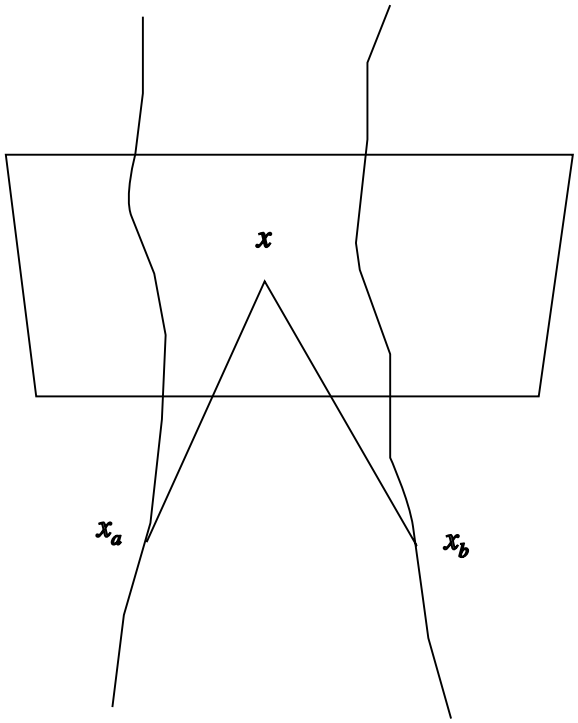

Fig. 3. Method 1 seems to requires only instantaneous data corresponding fields on a Cauchy hypersurface. Assuming only two particles and retarded propagators, the fields at any point x on the hypersurface relate to past particle motions at x_a and x_b. As x runs over the hypersurface, the points x_a and x_b will, in general, cover the entire past. Thus, in the retarded case, initial data for the fields is the same as past data for the world lines of the particles.

for the actual process of obtaining a solution. Formally speaking, there is no well-known existence and uniqueness theorem for a *coupled* system of PDEs + ODEs. [Separate existence theorems are, of course, known for PDEs (Maxwell's equations, in this case), and for ODEs (to which the particle equations of motion reduce, *if* all fields are given)]. Neither is there any well known numerical algorithm which converts the intuitive method of iteratively solving coupled PDEs + ODEs into an actual process of calculation.[14] Both formal proof and a numerical scheme can very likely be developed without much difficulty. However, neither is available as of now, so method 1 cannot, as of now, be used to obtain a solution of the full-force electrodynamic 2-body problem.

In contrast, for FDEs there is already a formal existence and uniqueness theorem from past data.[15] Further, there are well known numerical algorithms and tested computer programs available[16] (though they do not have all of the most desirable numerical characteristics, and the current code has not been formally proved to be error free). Accordingly, method 2 is currently the method of choice.

Incidentally, the PDE + ODE method 1 seems to need data on the *entire* past trajectories of the two particles. This is *more* information than is usually required for the FDE method 2. [Because of the numerical stiffness of the underlying equations, in practice, one is able to numerically solve the classical hydrogen atom with method 2 for only short time periods, of the order of a femto second (10^{-15} s), for which only a very short portion of the past history is needed.] That is, method 1, despite its appearance of preserving instantaneity and not needing any information from the past, actually seems to need *more* information about the past than method 2! A very careful analysis of the method would presumably show that the solution by method 1 actually uses information from only that part of the Cauchy hypersurface within the Cauchy horizon, so that information across the entire hypersurface is not needed, and the two methods are really equivalent. At present, however, that is still a conjecture.

3. SOLUTION OF THE ELECTRODYNAMIC 2-BODY PROBLEM

3.1. Prescription of Past History and Discontinuities

Physical considerations for prescribing past data.

How should the past history of the two particles be prescribed? Existing physics provides no guidelines to help answer this question: exactly like fields on a spacelike hypersurface in method 1, it permits us to prescribe the past particle motions more or less arbitrarily.

This has two consequences worthnoting. Thus, (1) mathematically, the past data for a FDE is *not* required to be a solution of the FDE, and (2) the mathematical theory of FDEs tells us that a discontinuity may well develop at the initial point where the prescribed past data joins with the solution of the FDE.

From a physical point of view, this past motion may be regarded as a bound or constrained motion—constrained, perhaps, by additional mechanical forces—and the discontinuity at the initial point may be attributed to the sudden removal of the additional constraint at $t = 0$. These discontinuities propagate downstream. While it is known from the general theory[17] that the discontinuities of a retarded FDE are typically smooth-

ened out over time, i.e., that they move over to successively higher derivatives, we must recognize that the problem we are solving is (except in 1 dimension) technically a neutral[18] FDE (even though only retarded Lienard–Wiechert potentials are being used), so that no such smoothing need take place, and the discontinuities may persist.

The discontinuity creates a mathematical difficulty: for, in classical electrodynamics, the equations of motion are formulated assuming that the particle trajectories are at least thrice continuously differentiable (C^3). The numerical solution of the FDE by higher-order Runge–Kutta methods also assumes a similarly high level of smoothness, which assumption is not justified if the past history is prescribed arbitrarily.

This problem of discontinuities obviously is a suitable topic for further research, and in the sequel we shall set it aside and rely on the methods for handling discontinuities that are already incorporated into existing computer codes like RETARD and ARCHI [19], which handle discontinuities for example by switching to low-order polynomial extrapolation or predictor–corrector methods near a discontinuity. Both these programs are so written that they permit a discontinuity even in the function (positions, velocities) at the initial point, though we can always arrange initial conditions so that there is a discontinuity at most in the derivative (acceleration) at the initial point. We will prescribe past and initial data in such a way that discontinuities are confined to the acceleration and higher derivatives—though, like 'quantum jumps', it is not at present clear whether these are the only sorts of discontinuities that are 'physically acceptable'. The ARCHI program enables the tracking of such discontinuities, classified as 'hard' or 'soft' according to the theory of Willé and Baker.[20] Hard discontinuities are those that propagate instantaneously between components, while soft discontinuities are those that propagate over time. In the present context, since there are no advanced interactions, all discontinuities are soft, except for discontinuities between components (velocity, acceleration) representing derivatives. (Further, the ARCHI programme has certain refinements—such as the use of a cyclic queue to store past history—that are not immediately relevant.)

General strategy to prescribe past history.

3.2. The Classical Hydrogen Atom

To return to the physics, since our aim is to test how well the Coulomb force approximates the full electrodynamic force, let us consider the case of the classical hydrogen atom, without radiative damping. Suppose an electron and proton are for $t \leq 0$ constrained to rotate rigidly in a classical circular 2-body orbit, calculated using the Coulomb force. What

will happen when this constraint is removed at $t = 0$? With the past history prescribed in this way, we now have a complete problem that can be solved by method 2.

The final prescription of past history, followed by a solution of the past value problem.

3.3. Equations of Motion

That is, we take the non-relativistic equations of motion to be given by the Heaviside–Lorentz force law for each particle:[21]

$$m\frac{d\mathbf{v}}{dt} = q\,(\mathbf{E} + \mathbf{v}\times\mathbf{B})\left[+\frac{2}{3}\frac{q^2}{4\pi\epsilon_0 c^3}\frac{d^2\mathbf{v}}{dt^2}\right]. \tag{14}$$

The radiative damping term given by square brackets in (14) has been dropped, since it is not immediately relevant to our purpose of deciding whether the electrodynamic force (6) can be approximated by the Coulomb force. (We know that, in the absence of radiation damping, with the Coulomb force, the initial "Keplerian" orbit should remain stable.) We also take the mass to be constant though, relativistically, only the proper mass is constant. This assumption, again, is appropriate to the comparison we wish to make between the Coulomb force and the full electrodynamic force.

3.4. Notation for the Explicit System of Equations

A slight change of notation is helpful for the explicit calculation. We let

$$\kappa = \frac{q_1 q_2}{4\pi\epsilon_0 m_1}, \quad \mu = \frac{m_1}{m_2}. \tag{15}$$

The mass ratio μ is dimensionless, while κ has dimensions of L^3T^{-2} (=length in units in which $c = 1$). Denote the 3-dimensional trajectories of the two particles by $\mathbf{r}_1(t)$, $\mathbf{r}_2(t)$. The delays τ, $\bar{\tau}$, and the vectors $\mathbf{R}_1$, $\mathbf{R}_2$ are defined by the simultaneous equations

$$c^2\tau^2 = R_1^2, \quad \mathbf{R}_1 = \mathbf{r}_2(t) - \mathbf{r}_1(t-\tau), \quad R_1 = ||\mathbf{R}_1||, \tag{16}$$

$$c^2\bar{\tau}^2 = R_2^2, \quad \mathbf{R}_2 = \mathbf{r}_1(t) - \mathbf{r}_2(t-\bar{\tau}), \quad R_2 = ||\mathbf{R}_2||. \tag{17}$$

Since here we consider only retarded solutions, for which $\tau, \bar{\tau} > 0$ we need to solve only the equations

$$R_1 - c\tau = 0, \tag{18}$$

$$R_2 - c\bar{\tau} = 0, \tag{19}$$

Yet another argument for the relevance of FDEs to quantum mechanics.

4.5. Discrete Spectrum and FDEs

The *discreteness* of the observed hydrogen spectrum has been regarded as another compelling argument against classical electrodynamics. Solutions of FDEs can, however, admit an infinite discrete spectrum.

Thus, for example, consider the *retarded harmonic oscillator*, given by the linear, second order, retarded FDE

$$\ddot{x}(t) = -x(t-1), \tag{50}$$

It is easy to see that a function of the form

$$x(t) = e^{z_k t}, \tag{51}$$

for complex z_k, is a solution of the Eq. (50) if and only if z_k is a solution of the*quasi*-polynomial equation

$$z^2 e^z = -1. \tag{52}$$

It is equally easy to see that the quasi-polynomial equation (52) has *infinitely many* complex solutions $z_k = x_k \pm i\, y_k$, and it is known[25] that the roots are discrete, with no cluster point, and that the large magnitude roots are given asymptotically by the approximate expression

$$y_k = 2k\pi + \epsilon_1(k), \tag{53}$$

$$x_k = -\ln y_k + \epsilon_2(k), \tag{54}$$

where $\epsilon_1(k) \to 0$, and $\epsilon_2(k) \to 0$ as $|z_k| \to \infty$.

(Had we dropped the retardation from (50), and applied the same procedure instead to the usual harmonic oscillator, given by the ODE $\ddot{x}(t) = -x(t)$, that would have led, in the well-known way, to a quadratic polynomial $z^2 = -1$ with exactly one pair of complex conjugate roots: i, and $-i$.)

Since the equation (50) is linear, any linear combination of these infinitely many oscillatory solutions of the form (51) is again a solution. Setting aside questions of convergence, it is clear that any *finite* linear combination of the form

$$x(t) = \sum_{k=1}^{n} a_k\, e^{z_k t}, \tag{55}$$

with z_k given by (54), say, is also a solution of (50). That is, in physical terms, the retarded harmonic oscillator, governed by the simple linear

FDE (50), exhibits an infinite spectrum of discrete frequencies. The general solution is a convergent linear combination of oscillations at an *infinity* of discrete ("quantized") frequencies. As in quantum mechanics, to determine a unique solution one needs to know an "initial" function. If the initial function is prescribed arbitrarily, we can expect discontinuities.

The above considerations remain valid for any linear FDE with constant coefficients and constant delay.[26] The locally linear approximation[27] suggests that such "quantization" is also to be expected for the non-linear FDEs of the electrodynamic 2-body problem. (For the FDE (12) this approximation may be obtained simply by replacing f by its first-order "Taylor" expansion, and then freezing the values of the delays t_r, in a neighborhood of the point at which we want to approximate the solution.)

Mere discreteness of the spectrum does not, of course, mean that the FDE formulation, using retarded electrodynamics, will correctly reproduce the actual spectrum of the hydrogen atom. Equally, mere discreteness of the spectrum was not an adequate argument *against* the classical hydrogen atom, which was prematurely rejected on the basis of the ODE formulation of the 2-body problem of electrodynamics. Though a further investigation of FDEs may still lead to a rejection of the classical hydrogen atom for the right reasons, it could, on the other hand, well help to clarify the "missing link" between classical and quantum mechanics, as in the structured-time interpretation of quantum mechanics.[28]

5. CONCLUSIONS

1. The Coulomb force is not a good approximation to the full electrodynamic force between moving charges.
2. The classical hydrogen atom was prematurely rejected on the basis of the ODE formulation of the electrodynamic 2-body problem.
3. The n-body problem involving electrodynamic interactions—as formulated, for example, in current software for molecular dynamics—needs to be reformulated using FDEs.

6. ACKNOWLEDGMENTS

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Mr Suvrat Raju (Harvard) for discussions, and to an anonymous referee for comments that have greatly helped to improve the presentation of the paper.

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3. Some attempts have been made to study the 2-body problem in 1 and 2-dimensions, and some approximation procedures have been suggested for three dimensions, but none of these are critically relevant to the question at hand. For a quick review see C. K. Raju, chap.*Time: Towards a Consistent Theory* (Kluwer Academic, Dordrecht, 1994) chap. 56. For the 1-body problem, see C. F. Eliezer, *Rev. Mod. Phys.* **19** 147(1947): G. N. Plass, *Rev. Mod. Phys.* **33**, 37–62. (1961) For the 2-body problem, see J. L. Synge, *Proc. R. Soc.* **A177** 118–139: (1940) R. D. Driver, *Phys. Rev.* **178** 2051–2057, (1969) D. K. Hsing, *Phys. Rev.* **D16** 974–82; (1977) A. Schild, *Phys. Rev.* **131**, 2762; (1963) C. M Anderssen and H. C. von Baeyer, *Phys. Rev.* **D5**, 802; (1972) *Phys. Rev.* **D5**, 2470; (1972). R. D. Driver, *Phys. Rev.* **D19**, 1098; (1979) L. S. Schulman, *J. Math. Phys.* **15**, 205–208, (1974); K. L. Cooke and D. W. Krumme, *J. Math. Anal. Appl.* **24**, 372–387; (1968). H. Van Dam and E. P. Wigner, *Phys. Rev.* **138B**, 1576; (1965). **142**, 838 (1966).
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6. Cf. Griffiths, *Electrodynamics*, 3rd edn., Eqs. (10.65), (10.66) and (10.67), pp. 438–439.
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History of solution of retarded em 2-body problem

From The Eleven Pictures of Time

9

The Tilt in the Arrow of Time

Einstein's Mistake

Einstein's mistake

Relativity changed the notion of the instant; for an instant consists of all events that are simultaneous with it, and relativity changed the notion of simultaneity. But relativity also changed the notion of *instantaneity*; a possible change realised by Poincaré, but not by Einstein who made a mathematical mistake about it—a mistake that he did not correct till the end of his life.

What is instantaneity? Let us begin with the conventional idea that physics provides a 'causal' description of the world by relating causes to effects—physics describes how the state of the world *now* relates to its future states. The arrow continues to fly because of its state at the preceding instant, and physics enables us to calculate the future motion of the arrow, if the present state is known. We saw earlier (p. 178) that this 'causal' interpretation of physics is deceptive. The best argument against such an interpretation is Poincaré's philosophy that physics is defined by its mathematical equations, and not by the interpretations we assign to these equations. Instantaneity means that 'physical law is a differential equation', so that actually the state now decides *both* past and future states, so that all states are decided by the state at any one given instant. Thus, the formulation of physics using differential equations essentially means that the state of the world at any time is decided by its state at any one instant. One may say the arrow is flying now because it was flying a moment ago; with equal facility one may say the arrow is flying now because it will fall to the ground a moment later.

Why not simply say that the arrow is flying now because the archer released it from his bow two seconds earlier? There is a

difficulty if one believes with Aristotle and Augustine that the past has ceased to exist; if so, locating causes in the past would make them non-existent! Therefore, the present motion of the arrow can be linked to the past action of the archer only through an intermediate chain of causes. This belief in the non-existence of the past is incompatible with relativity, as we have already seen—after relativity, the past does not cease to exist. Parts of the past may continue to exist in Buddhism, where the criterion of existence is causal efficacy, and there may be a possible delay between cause and effect. Therefore, Buddhists would say that a past event continues to exist if it has not yet produced its effects.

The philosophical difficulty of locating causes in the past.

The Philosophy of Contact

The belief that the past has ceased to exist is closely related to another belief in the desirability of explanation by contact: causes proximate in time are presumably also proximate in space. The philosophy of contact elevates this to a metaphysical principle: a physical explanation *must* locate causes not only in the immediate past, but in the immediate vicinity—the action of one body on another must be explained through physical contact between the two bodies. The archer cannot influence the motion of the arrow after it has left the bow, for he has lost contact with the arrow.

One observes, of course, the interaction between bodies that are evidently *not* in contact—like a pair of magnets. The aether, as an all-pervasive invisible substratum, was first introduced to help explain by contact such observed interactions between two spatially separated bodies. The aether was imbued with all manners of astounding properties to prevent this principle of action-by-contact from being falsified. In present-day physics (including relativity and quantum mechanics), the underlying philosophy of contact is preserved through the notion of the all-pervasive invisible substratum of the field, which has substituted the aether.[1]

Why the field is like the aether (hence should be rejected).

The philosophical belief that only action by contact needed no explanation has an old history. Orthodox Indian philosophy (Nyāya-Vaiśeṣika) advocated the philosophy of *contact* (*saṁyoga*) and the related notion of *aether* (= sky= *ākāśa*).[2] Just at the time when numerous Indian texts were being translated and imported into renaissance Europe, this philosophy of contact and aether was

adopted by Descartes. On the other hand, Francis Bacon was 'most ready to ascribe to action at a distance without any material medium…those [phenomena] which savour most of witchcraft, magic, astrology, and telepathy',[3] so that action at a distance often continues to be called 'spooky' in present-day discourses on physics. (This terminology has been particularly prominent in the debate over Bell 'locality': quantum mechanics is somehow 'spooky' because it enables interaction between particles that 'cannot' be in contact.)

Under such Cartesian and Baconian influence it came to be believed that a physical explanation, to be one, must relate a cause here and now to an effect here and now. The aether, it was thought, brought in clarity—also banishing spooks, like daylight. The natural mathematical corollary to this was instantaneity: that physical law must necessarily have the form of a differential equation.

Newtonian mechanics is characterised by this instantaneity. Though the force behind instantaneity was the idea of action by contact,[4] Newtonian physics only half-accepted the idea. Newtonian physics consists of two parts: Newton's laws of motion, and Newton's law of gravitation. Neither part can yield physics by itself, and the two must be combined to give physics.[5] Newton's laws of motion explained motion *here and now* using forces acting *here and now*, but Newton's law of gravitation explained the forces acting *here and now* using the positions of *distant* bodies *now*. After special relativity, the last 'now' in the previous sentence is not quite meaningful.

Why relativity compels fde.

The sun, for example, is a distant body. Suppose it is switched off now. When will the earth first wobble in its orbit? After relativity, we believe that the information that the sun has been switched off will take some time to travel to us; we believe that this time cannot be shorter than the time taken by light to travel that distance—a little over eight minutes. So we will continue to see the sun, as it was before being switched off, for the next eight minutes, after which the earth will wobble. But according to Newtonian physics, the earth ought to wobble right away.

This thought experiment suggests a fundamental incompatibility between Newtonian gravitation and relativity, especially in interactions involving transients. Let us try to test this incompatibility. One cannot switch off the sun, so where should one look

for gravitational interactions involving transients? The galaxy provides an example. (Though speculative, this is easier to explain than better examples.) While the planets of our solar system have gone around the sun many times since the solar system came into being, and have reached a steady state, our sun and other stars in the galaxy have gone around the centre of the galaxy barely a couple of times since the galaxy came into being.

Dark Matter or the Failure of Instantaneity?

And, in fact, Newtonian gravitation does not correctly describe the observed motion of stars around the galactic centre. The stars seem to be moving far too fast. The amount of matter in the galaxy seems too small to hold together stars rotating at such high speed. The discrepancy is put down to something we do not observe, and something we cannot hope to see: dark matter in the galaxy—matter not in the form of stars, which hence cannot be seen.

Now, *a priori* one can accept that there may be non-luminous matter in the galaxy. One could even accept that matter in the galaxy is mostly dark, in the ratio of 10:1. But it is difficult to accept *the peculiar way in which this dark matter is required to be distributed*—with its density increasing *outwards* from the centre of the galaxy, and reaching a constant value where the density of luminous matter reaches zero. This peculiar *distribution* of dark matter seems an artificial hypothesis invented only to save Newtonian gravitation from being refuted.

Why not simply accept that Newton's laws fail to describe the situation correctly? Newton's laws are good for practical travelling to the moon, they are good to describe quickly the planetary orbits from which they were back-calculated, but they fail to describe the rotation of the galaxy.

History Dependence

Let us try to understand this failure using the previous example of the sun being switched off. The question there was whether the gravitational force on a planet due to the sun relates to the sun as it is now or as it was last seen. This question appears with renewed

force when we look at galactic rotation. Unlike a few planets going round a relatively very massive central body, in the case of the galaxy we have millions of stars going round a common centre. Each of these stars interacts gravitationally with all other stars. In calculating the gravitational force of star A on star B, at time *t*, should we use the position of star A at time *t*, or the position of star A as *last seen* from star B?

Relativistically, the second choice is preferable. (Though both these choices are technically incorrect, the following arguments apply perfectly well to the technically correct choice, where force depends upon both position and velocity.) Suppose we make the second choice. What difference does that make? To Einstein, it made no serious difference. Einstein only half-rejected the aether: he did not reject it in the sense of action by contact or instantaneity.[6] He thought, following Bacon, that action without contact was 'spooky', and stated as much while formulating the Einstein–Podolsky–Rosen paradox (Chapter 8). Poincaré, being a mathematician, understood that rejecting aether would change the equations of physics, making them what he called 'equations of finite differences'.

> Does our ether actually exist? We know the origin of our belief in ether. If light takes several years to reach us from a distant star, it is no longer on the star, nor is on the earth. It must be somewhere, and supported, so to speak, by some material agency…The same idea may be expressed in a more mathematical and more abstract form…in ordinary mechanics the state of the system under consideration depends only on its state at the moment immediately preceding; the system therefore satisfies certain differential equations. On the other hand, if we did not believe in the ether, the state of the material universe would depend not only on the state immediately preceding, but also on much older states; the system would satisfy *equations of finite differences*. The ether was invented to escape the breaking down of the laws of general mechanics.[7] [Emphasis mine.]

Today we would call these 'delay differential equations', or 'functional differential equations'. The names are unimportant, and what is important is this: if aether and action by contact are rejected, then, as a first step, instantaneity has to be replaced by *history dependence*.

Delay differential equations and history dependence for the layperson.

Annex 3

(Annotated chronology, including list of my prior publications.)

Annex 3

Annotated chronology, including list of my talks, publications, reviews, citations, and news reports, etc. relating to dissemination of my ideas about retarded and mixed-type functional differential equations (FDE) and the relation to quantum mechanics.

1989: "Quantum Mechanics and the Micro-Physical Structure of Time." Invited talk delivered at the International Conference on the Conceptual Foundations of Quantum Mechanics, New Delhi, Dec 1989. (Linked Hilbert-space axiomatics of qm to a lattice derived by a temporal logic corresponding to structured time.)

1992: "On Time 5a: The Electromagnetic Field", *Physics Education* (India) **9** (1992) 119–128. (Explains why the field, like the aether, originates in the ideology of action by contact [and hence should be discarded].)

1992: "On Time 5b: Electromagnetic time", *Physics Education* (India) **9**(3) (1992) 251–265. (Paper explains how retarded, advanced, and mixed-type functional differential equations arise in direct action electrodynamics [if fields are discarded] and how this resolves the paradoxes of thermodynamics.)

1993: On Time 6a: Bell and Non-Locality, **10** (1993) 55–73. (Explains why nonlocality is an essential feature of qm. [Hence retarded FDE do not suffice to get all of qm.])

1993: "On Time 6b: Quantum-mechanical time", *Physics Education* (India) **10**(2) (1993)143--161. (Paper explains how mixed-type FDE lead to quantum mechanics via a structure of time.)

1994: *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994 (Fundamental Theories of Physics, vol. 65). (Consolidated the above work in a prestigious international publication for wider dissemination.)

1996: "Time Travel and the Reality of Spontaneity." Invited talk delivered at the Seminar on Reality in Physics and Philosophy, Calcutta, Feb 1996. (Paper explained a key empirical consequence of dropping the assumption of causality, using the theory of mixed-type FDE. The paper, although accepted for publication by *Found. Phys*., with very favourable referee reports, was published only ten years later.)

1996: "Simulating a Tilt in the Arrow of Time: Preliminary Results." Invited paper presented at the Seminar on *Some Aspects of Theoretical Physics*, Indian Statistical Institute, Calcutta, 14–15 May 1996. (Paper presented and compared computational solutions of ordinary, retarded, and (Lévy-perturbed) stochastic differential equations for the two body problem. Also explained how to solve mixed-type FDE.)

1996: J. F. Woodward, "An Essay Review of C. K. Raju's *Time: Towards a Consistent Theory* (Kluwer Academic: Dordrecht)", *Foundations of Physics* **26** (1996) 1725–1730. (Review pointed out that progress in physics is made not by adding hypotheses, but by dropping them, and that I have dropped the hypothesis of causality.)

1996: "Time Travel and Schrödinger's Cat." Invited talk, International Symposium on *Seventy Years of Schrödinger's Wave Mechanics*, New Delhi, Dec 1996.

1997: "The Tachyonic Anti-telephone and Tolman's Grandfather." Invited talk at the *Heisenberg Colloquium,* Shimla, Aug 1997.

1998: "The Electrodynamic 2–body Problem and the Origin of Quantum Mechanics." Paper presented at the International Symposium on *Uncertain Reality*, New Delhi, 5–9 Jan 1998. (Presented the solution of the retarded (delay) differential equations corresponding to the 2-body problem. Was sharply criticised by Kamal Datta.)

1999: Similar seminar at Dept of Maths, University of Southampton (included discussion with C. J. S. Clarke, and P. T. Landsberg).

1999: "Relativity: History and History Dependence." Paper presented at the *On Time* Seminar, British Society for History of Science, and Royal Society for History of Science, Liverpool, August 1999.

1999 "Mathematics and Culture." Reprinted in *Philosophy of Mathematics Education*, 11 at http://people.ex.ac.uk/PErnest/pome11/art18.htm. (Highlighted the mathematical mistake made by Einstein in a paper on the relativistic many-body problem published in the *Annals of Mathematics*. [He "Taylor"-expanded a retarded FDE, in powers of the delay, to reduce it to an ODE, showing he didn't understand the mathematics of FDE.] The mathematics of this error had been explained in the 1994 book, with a brief reference to Einstein.)

1999: "Time Travel." Invited talk at the International Seminar, *Retrocausality Day*, University of Groningen, September, 1999. (My claim became the focus of debate, during and after the conference, with H. D. Zeh arguing that ordinary differential equations + fields were all that were needed for physics, and a new mathematical understanding could not by itself bring about a paradigm shift in physics.)

2000: January, Seminar at University of Pittsburgh.

2003: *The Eleven Pictures of Time*, Sage, 2003. (Explained the consequences of various types of FDE for **laypersons** in chapter 9 on "The tilt in the arrow of time", under the headings "Einstein's mistake", "History dependence" etc.)

2003: Press release and newspaper reports related to *The Eleven Pictures of Time*, at www.11picsoftime.com.

2004: Don Miller, Review of *The Eleven Pictures of Time* in *Time and Society* London, **13**(2), September 2004. (Noted, in passing, the point about Poincaré and Einstein.)

2004: "The Electrodynamic 2-Body Problem and the Origin of Quantum Mechanics" *Found. Phys.* **34** (2004) 937–62. (First published solution of a past value problem in physics for the retarded FDE [delay differential equations] of electrodynamics. The past data was prescribed using Bohr orbits. The heuristic insight was provided by pointing out the delay torque, easily understood with a retarded inverse square law [first mentioned in the paper on "Simulating a tilt"]. The paper was cited especially by Jaume Gine in several papers posted on the archive since May 2005; Gine too tried to link retarded FDE to quantum mechanics in his own way, and mailed copies of his papers to me and to several others.)

2004: "Were Newton, Einstein Plagiarists", *Hindustan Times* (Bhopal edition) 14 Sep 2004, p. 3 headline. Quoted in the Math News Archives for 1–20 Sep 2004, at http://www.dm.unito.it/personalpages/cerruti/mathnews0904.html.

2004: 8 Nov. "Prof. Raju's charge of plagiarism found correct." *Hindustan Times*, *Bhopal Live*, front page headline. Related UNI [news-agency] dispatch of 23 Nov. "MP based mathematician's work plagiarized abroad". (This gave another journalistic

account of the "epistemic test" that those who copy make mistakes because they don't fully understand what they are copying. This test was systematically used in my complaint against Almeida et al. to the University of Exeter.)

2005: "Why Time Travel is Possible but Time Machines are Not". Talk in the series Conceptual and Historical Issues in Modern Physics, Depts of Physics and Philosophy, at Univ. of Sydney, 20 October 2005.

2005: 21 October, Sir Michael Atiyah's Einstein lecture.

2005 24 October. Atiyah's lecture at the Kavli Institute of Theoretical Physics.

2005: Following up on son's email of 26 October, my email of 6 Nov to Atiyah, politely pointing out that his talk showed insufficient understanding of the linkages between FDE and quantum mechanics on the one hand, and FDE and classical field theory on the other. (Alternatively, he demonstrated a lack of understanding of how the theory FDE links with the theory of coupled ordinary and partial differential equations, as explained in my 2004 paper.) Coming from so great a mathematician, such a blunder is difficult to understand. I also pointed out some further radical consequences of this line of thought not only for physics, but also for mathematics, and politely reminded him of Poincaré etc.

2005: 19 Nov. Atiyah's response, stating that his ideas are "fluid" and that they are not necessarily sensible.

2006: G. W. Johnson and M. Walker, "Sir Michael Atiyah's Einstein Lecture", *Notices of the American Mathematical Society*, **53** (6) June/July 2006, pp. 674–78. Available at http://www.ams.org/notices/200606/comm-walker.pdf. Under the heading "A New Paradigm?" the article stated "Atiyah noted that all physical models since Newton, including even quantum mechanics, have assumed one basic premise—that we can *predict the future from full knowledge of the present*. Atiyah suggested an alternative to this paradigm: Perhaps we need full knowledge of the present *and the past* in order to predict the future. That is, maybe the universe has memory." Since I have needed to refer to these two situations so frequently, they have been given special names, "Instantaneity" and "History dependence" in my works. The authors however refer to history-dependence as "Atiyah's hypothesis", and go on to specify, "Atiyah's hypothesis possibly leads to several interesting consequences: 1. The mathematics used in physical theory would become more difficult, since all previously used mathematics in physics assumes that knowledge of the present suffices.

2006: "Time Travel and the Reality of Spontaneity", *Found. Phys.* July 2006.

2006: 31 October. My email to Johnson and Walker, suggesting that they should retract the erroneous phrase "Atiyah's hypothesis" since the idea was put forward by me long before Atiyah, since Atiyah was certainly informed about it prior to their publication, and since no hypothesis is necessary. I also suggested that a claim of independent rediscovery on behalf of Atiyah would need to be publicly debated against the background of numerous such claims of independent rediscovery on behalf of well-known Western names like Copernicus, and using a single standard of evidence, instead of the double standards used in racist history.

2006: 16 Nov. My fax to Johnson and Walker, confirming my email, and pointing out also the violation of the AMS ethical guidelines published in the same issue as their article.

2006 16 Nov -- Dec; Further email exchanges with Johnson and Walker. Response from Atiyah.

2006 11/.12 Dec Petition to Chancellor of Johnson and Walker's university.

2006 12 Dec-28 Dec. Response to Atiyah. Further email exchanges.

2006 29 Dec. Walker submits a letter for publication in *Notices*.

2007 2 Jan. A. Magid, Editor of *Notices* acknowledges the letter, and indicates he is likely to accept it.

2007 ca. 15 March. Publication of Walker's letter in April issue of *Notices*.

2007 29 March. Atiyah inaugurates a mathematical conference in Paris, and speaks of radical new foundations for physics. Again discusses delay equations.

2007 9 April. My letter submitted to *Notices*, Is it Ethical?

2007 7 May. Receipt of my letter is acknowledged after two reminders.

Annex 4

(Extracts from Johnson and Walker article, "Sir Michael Atiyah's Einstein lecture", *Notices of the American Mathematical Society*, June/July 2006.)

From Johnson and Walker's article on "Sir Michael Atiyah's Einstein Lecture..."

Annex 4

additional 6 (or 7) dimensions are hidden from our normal experience of reality.
2. Whereas more classically, the basic objects such as electrons, protons, and quarks, could be thought of as point particles, string theory interprets such objects as being very small "strings". This allows for a "smoothing out" of the singularities that arise in the classical picture when such particles come close together. Even the singularities associated with mixing quantum mechanical and gravitational forces are resolved in this manner.
3. Very sophisticated geometry is used, involving a vast amount of mathematics, both old and new.
4. No unique model or picture has emerged out of string theory, but rather several versions exist. These different theories are now known to be different facets of the same theory. What has happened to the "real world"?
5. Quantum mechanics remains the basic framework.

String theory has had a remarkable and mysterious impact on pure mathematics, leading to many new concepts and results. In some cases, such results have been given proofs in the traditional mathematical sense. In other situations, the "results" merely fit well with known mathematical results or accepted features of string theory. In particular, string theory has had an impact in

1. algebraic geometry, by addressing enumerative questions concerned with counting algebraic curves satisfying certain conditions;
2. knot theory, by construction of new topological invariants of knots that can sometimes distinguish a knot from its mirror image;
3. four-dimensional geometry, by giving new, unexpected, and very deep results that are unique to four dimensions; and
4. various branches of algebra.

A New Paradigm?

If a "theory of everything" emerges from string theory, we will discover a universe built on *fantastically intricate mathematics*. In particular, the Calabi-Yau manifolds that make up the hidden dimensions are extremely complicated. Atiyah suggested that it is not satisfying that the true theory would be so complicated—even writing down the terms of the theory requires a vast amount of background.

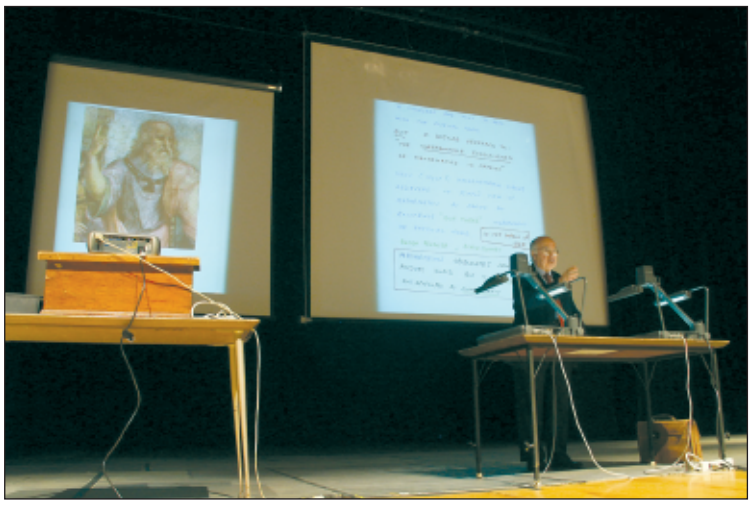
Perhaps, according to Atiyah, a new paradigm is needed; perhaps the complicated mathematics appearing in string theory is merely "in the eye of the beholder". That is, maybe we do not understand the fundamental nature of reality well enough, and this misunderstanding is leading to such exceptionally complicated mathematics. String theory, from this point of view, is only our method of approximating a simple reality. Perhaps, Atiyah suggested, we should follow Einstein and question quantum mechanics.

In order to make progress, we might need to dispense with some piece of accepted dogma. Relativity, quantum mechanics, and string theory have already dispensed with many previously held tenets, and so one might ponder whether there remains any such dogma left to throw away. Atiyah noted that all physical models since Newton, including even quantum mechanics, have assumed one basic premise—that we can *predict the future from full knowledge of the present*. Atiyah suggested an alternative to this paradigm: Perhaps we need full knowledge of the present *and the past* in order to predict the future. That is, maybe the universe has memory. As a simple example, the notion of the velocity of an object is viewed as being a property of the present, but, in reality, to measure velocity one needs to know not only where the object is now but where it was a moment earlier.

Atiyah's hypothesis possibly leads to several interesting consequences:

1. The mathematics used in physical theory would become more difficult, since all previously used mathematics in physics assumes that knowledge of the present suffices. With the new paradigm, for example, retarded (or delay) differential equations would become necessary.
2. Since we do not have complete knowledge of the past, uncertainty would arise. This might shed light on the uncertainty inherent in quantum mechanics.
3. Perhaps the complicated mathematics of string theory arises from our attempt to understand the full implications of the theory of general relativity without incorporating the knowledge of the past.

Atiyah does not promote discarding older, time-tested physical theories. Rather, such a new



paradigm ought to build on the old theories, much as relativity builds on Newtonian mechanics.

Speculations and Questions

There are various attitudes among physicists toward string theory. Some dismiss it as fancy mathematics that is unrelated to the real world, since string theory makes no testable predictions. Others believe the mathematical applications of string theory give confidence in the physical insights and indicate that the theory is on the right track. From this point of view, mathematical applications become a kind of alternative to experimental evidence. A third point of view is that we should continue to push forward with string theory in the hope that the new results and ideas that emerge will serve as a guide for finding a final unified theory.

Atiyah concluded his talk by speculating on the meaning of all this—quantum field theory, string theory, and their mathematical applications. What will the future physical theory look like? The aim is to unify quantum mechanics, the physics of the very small, with general relativity, the physics of the very large. Supersymmetry is a symmetry in which physical laws are unchanged when bosons and fermions are interchanged. Superstring theory, a supersymmetric string theory, is a perturbative approach, one that Atiyah compared with the theory of epicycles developed by Ptolemy. But what is the real theory; that is, what is being perturbed? Is it M-theory, a currently incomplete theory unifying all five versions of string theory? Is the universe really built using all this sophisticated machinery or is this an example of mathematics imposed by us? Perhaps the real physics is simpler and one should adhere to the dictate of Occam's razor—concepts should not be multiplied beyond necessity. Do we need to modify quantum mechanics? Atiyah closed by saying "This is for young people: Go away and explore it. If it works, don't forget I suggested it. If it doesn't, don't hold me responsible."

The second Einstein Public Lecture in Mathematics was delivered on April 29, 2006, in conjunction with the AMS Spring Western Sectional Meeting at San Francisco State University. Benoît Mandelbrot of Yale University spoke on "The nature of roughness in mathematics, science, and art".

All photographs used in this article are courtesy of Gregg Johnson (Suitefreedom.com).

Annex 5

(Copies of correspondence to show that Atiyah was explicitly informed as early as 26 October 2005 of my work in Annex 3, and that he had received this, and acknowledged reading my 1994 book, the 2004 paper, and its relevance well before the Johnson and Walker article.)

Annex 5

From : Suvrat Raju <suvrat@physics.harvard.edu>

Reply-To : suvrat@physics.harvard.edu

Sent : Wednesday, October 26, 2005 11:15 PM

To : M.Atiyah@ed.ac.uk, Michael.Atiyah@ed.ac.uk

Subject : functional differential equations (fwd)

Attachment : em2body.pdf (0.31 MB)

MIME-Version: 1.0
Received: from us19.unix.fas.harvard.edu ([140.247.35.199]) by mc3-f23.hotmail.com with Microsoft SMTPSVC(6.0.3790.211); Wed, 26 Oct 2005 16:16:51 -0700
Received: from ls04.fas.harvard.edu (ls04.fas.harvard.edu [140.247.34.104])by us19.unix.fas.harvard.edu (8.12.11/8.12.11) with ESMTP id j9QNFB2v019922;Wed, 26 Oct 2005 19:15:11 -0400
Received: by ls04.fas.harvard.edu (Postfix, from userid 4132)id 09DB21C048; Wed, 26 Oct 2005 19:15:10 -0400 (EDT)
Received: from localhost (localhost [127.0.0.1])by ls04.fas.harvard.edu (Postfix) with ESMTP id EBD3024035;Wed, 26 Oct 2005 19:15:10 -0400 (EDT)
X-Message-Info: JGTYoYF78jEWG70NpA1i6Tqcc+6EY/qHfy3P4r7UMfg=
X-X-Sender: raju@ls04.fas.harvard.edu
Return-Path: suvrat@physics.harvard.edu
X-OriginalArrivalTime: 26 Oct 2005 23:16:51.0849 (UTC) FILETIME=[58FD3F90:01C5DA83]

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Content-Type: TEXT/PLAIN; CHARSET=US-ASCII
Content-ID: <Pine.LNX.4.58.0510261912401.21410@ls04.fas.harvard.edu>

Dear Professor Atiyah,

I am a graduate student at Harvard. I have been working, with Prof. Shiraz Minwalla, on aspects of the AdS/CFT correspondence.

I heard your very interesting talk at the KITP, on whether the Universe has a history, via webcast.

As was brought up in the questions after the talk, relativity necessitates the replacement of ordinary differential equations with functional differential equations. As Prof. Gross mentioned, this is currently done in modern Physics by introducing fields.

However, a comprehensive analysis of the physical and philosophical implications of replacing ordinary differential equations with functional differential equations was first done more than 10 years ago and may be found in the book "Time: Towards a Consistent Theory. By C.K. Raju. Dordrecht, Netherlands: Kluwer (1994) (Fundamental theories of physcis, 65)."

Prof. Raju considers functional differential equations of both the 'retarded' and the 'mixed' type. Moreover, in Chapter VI, he shows how it is possible to reconstruct the formal structure of quantum mechanics from this.

Moreover, last year, Prof. Raju also published the first ever solution to retarded functional differential equations in the concrete physical context of a two body electrodynamic system. These results may be found in "The electrodynamic 2-body problem and the origin of quantum mechanics",Found. Phys. 34 (6) June 2004, 937--62.

I have attached a pdf vesion of the paper to this email.

Thank you.

Regards,

Suvrat Raju

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Content-Transfer-Encoding: BASE64
Content-Disposition: ATTACHMENT; FILENAME="em2body.pdf"
Content-ID: <Pine.LNX.4.58.0510261912310.21410@ls04.fas.harvard.edu>

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Return-path	<suvrat@physics.harvard.edu>
Received	from mta1 ([172.16.28.189]) by pop2.vsnl.net (vsnl mail server) with ESMTP id <0IP300K08RG0WI@pop2.vsnl.net> for c_k_raju@vsnl.net; Sat, 29 Oct 2005 0 +0530 (IST)
Received	from us20.unix.fas.harvard.edu(us20.unix.fas.harvard.edu [140.247.35.200]) b mta1.vsnl.net (vsnl mail server)with ESMTP id <0IP3004H8RFYCU@mta1.vsnl.n c_k_raju@vsnl.net; Sat, 29 Oct 2005 09:08:24 +0530 (IST)
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Received	by ls04.fas.harvard.edu (Postfix, from userid 4132) id B5FF31C065; Fri, 28 Oct 2 -0400 (EDT)
Received	from localhost (localhost [127.0.0.1]) by ls04.fas.harvard.edu (Postfix) with ESM AEF1F24035; Fri, 28 Oct 2005 23:38:19 -0400 (EDT)
Date	Fri, 28 Oct 2005 23:38:19 -0400 (EDT)
From	Suvrat Raju <suvrat@physics.harvard.edu>
Subject	Re:
X-X-Sender	raju@ls04.fas.harvard.edu
To	"C. K. Raju" <c_k_raju@hotmail.com>, c_k_raju@vsnl.net
Reply-to	suvrat@physics.harvard.edu
Message-id	<Pine.LNX.4.58.0510282337190.5933@ls04.fas.harvard.edu>
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Original-recipient	rfc822;c_k_raju@vsnl.net

Here is Atiyah's reply :-) Maybe you can write to him directly.

---------- Forwarded message ----------
Date: Fri, 28 Oct 2005 21:40:34 +0100
From: Michael Atiyah <M.Atiyah@ed.ac.uk>
To: suvrat@physics.harvard.edu
Subject: Re: functional differential equations (fwd)

Der Mr Raju,

Thank you for drawing my attention to the paper and the book. They seem
relevant to my ides and I have found them helpful.

Incidentally are you relatd to the author of the book?

Michael Atiyah.

Quoting Suvrat Raju <suvrat@physics.harvard.edu>:

> Dear Professor Atiyah,
>

Home | My MSN | Hotmail | Search | Messenger Sign Out Web Search: Go

Hotmail Today | Mail | Calendar | Contacts

c_k_raju@hotmail.com Messenger: Offline

Reply | Reply All | Forward | Delete | Junk | Put in Folder | Print View | Save Address

Inbox

From : Michael Atiyah <M.Atiyah@ed.ac.uk>

Sent : Saturday, November 19, 2005 10:18 AM

To : "C. K. Raju" <c_k_raju@hotmail.com>

Subject : Re: functional differential equations

MIME-Version: 1.0
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X-Message-Info: JGTYoYF78jEHjJx36Oi8+Z3TmmkSEdPtfpLB7P/ybN8=
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.1; en-US; rv:1.4) Gecko/20030624 Netscape/7.1 (ax)
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References: <BAY104-F19650E5869D0CBDA0292E8CB620@phx.gbl>
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X-Scanned-By: MIMEDefang 2.33 (www . roaringpenguin . com / mimedefang)
Return-Path: M.Atiyah@ed.ac.uk
X-OriginalArrivalTime: 19 Nov 2005 10:19:15.0878 (UTC) FILETIME=[B1C70C60:01C5ECF2]

View E-mail Message Source

Content-Type: text/html; charset=us-ascii
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Dear Professor Raju,

Thank you for your message and my apologies for this late reply.

There is certainly some overlap in our ideas about the relevance of retarded differential equations to physics, but my own ideas are still fluid and far from settled. If they converge to anything sensible I will pass them on to you.

While I was in Santa Barbara, California, my attention was drawn to Feynman's thesis (just recently published under that title by World Scientific Press - author Laurie Brown). Retarded and advanced differential equations arise there in the context of the dynamics of charged particles and you might find the discussion interesting.

Michael Atiyah

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Annex 6

(My response to the belated acknowledgment, submitted to the Notices of the AMS.)

Annex 6

Is this Ethical?

C. K. Raju
c_k_raju@vsnl.net

M. Walker's letter (*Notices*, April 2007), concerning the article by Johnson and Walker, "Sir Michael Atiyah's Einstein Lecture..." (*Notices*, June-July 2006), and drawing attention to my past work, is a step in the right direction, but the whole truth needs to emerge.

In two lectures given in October 2005, Atiyah acknowledged no prior work, claiming credit for connecting retarded differential equations to quantum mechanics. This was called "Atiyah's hypothesis", and hailed as a potential paradigm shift in the Johnson-Walker article. Consequently, it came to be discussed as "Atiyah's new basis for quantum mechanics" (e.g. http://physicsforums.com/archive/index.php/t-96806.html). Thus, the fact is that my prior work *was* suppressed.

Atiyah certainly knew of my prior work at the latest by 26 Oct 2005, when my 2004 paper and a reference to my 1994 book were mailed to him, and he acknowledged reading them, and accepted the similarity. This was well before the Johnson-Walker article (shown to Atiyah before submission), which still did not refer to my work. Atiyah (personal communication) justified this on the grounds that he does not regard the *Notices* article as a "serious" publication. Thus, at least in the case of the *Notices* article, suppression of my past work was deliberate.

The ethical guidelines of the AMS (*Notices*, June–July 2006) aim to ensure appropriate allocation of credits. That could have been achieved, even without citing references, by clarifying that Atiyah was not presenting original work. Such a clarification would have been especially appropriate considering that Atiyah's talks were widely publicised, and the wide readership of the *Notices* which has traditionally served to establish priority. Atiyah, however, stated to the contrary "don't forget I suggested it" (also quoted in the *Notices* article). This is an unmistakable claim of priority—especially given the deliberate suppression of my prior work. Is this ethical?

Secondly, Atiyah (personal communication) has claimed to have independently rediscovered my idea. Is *this* claim ethical (regardless of its validity)? Thus, the AMS ethical guidelines explicitly state that "a claim of independence may not be based on ignorance of widely disseminated results"—for such ignorance can so easily be feigned. And, my work was undoubtedly published and widely disseminated earlier, through books, articles, conferences, website, and newspaper reports. (Further details of this are at http://www.11picsoftime.com/Atiyah_annexes.pdf.) Thus, Atiyah's claim of "independent rediscovery" is unethical according to those guidelines. This is particularly unfortunate since Atiyah has added nothing new by restating as a conjecture what I had earlier claimed as a theorem; this has only obscured important aspects of my theory, such as the connection of the structured-time interpretation of quantum mechanics to quantum computing.

Thirdly, is Atiyah's claim of independent rediscovery valid? Given the long list of claims of "independent rediscovery" of revolutionary ideas, since Copernicus, various criteria to test the veracity of such claims have been proposed in my forthcoming book *Cultural Foundations of Mathematics: The Nature of Mathematical Proof and the Transmission of the Calculus from India to Europe in the 16th c. CE* (PHISPC vol. X, part 4, Pearson Education, in press).

One such criterion is a commonsense "epistemic test": if two students turn in very similar answers, one way to ascertain whether copying has taken place is to test the students' understanding. One cannot thus directly interrogate the past—but mistakes may expose lack of understanding. Does Atiyah's claim of "independent rediscovery" pass the epistemic test?

There is a conceptual error on a subtle point in (a) Atiyah's video-recorded Einstein lecture of 21 October 2005, (b) his webcast talk of 24 October 2005 at the Kavli Institute of Theoretical Physics, and (c) the *Notices* article (approved by Atiyah). For example, the *Notices* article states "all physical models... have assumed one basic premise—that we can *predict the future from full knowledge of the present.* Atiyah suggested an alternative to this paradigm: Perhaps we need full knowledge of the present *and the past* in order to predict the future." [Emphasis original] This is incorrect: as any physicist will understand, past information is already contained in the field. Since field and particle pictures are equivalent, functional differential equations *do* arise in *classical* physics if one uses only the particle picture. The functional differential equations I solved in my 2004 paper (*Found. Phys. 34*, 937–62) were those of *classical* physics, although I explicitly needed to

prescribe a past history for their solution.

The physics has been available since Heaviside and Lorentz—but the relevant functional differential equations were simply not earlier solved in any significant physical context by physicists. That is, my proposal is not a new physical hypothesis but a correct (though novel) mathematical understanding of long-existing physics: a thesis not a hypothesis. If functional differential equations were applied, say, to finance (as I have suggested) that would be a new hypothesis; but in physics itself no new hypothesis is needed.

There *are* many surprises here—for example, the solution of the correctly formulated electrodynamic 2-body problem shows that the classical hydrogen atom is not necessarily unstable, contrary to what has been believed for a century. However, the paradigm that is overturned is only the "Newtonian paradigm"—a term carefully defined by me in my 1994 book, *Time: Towards a Consistent Theory* (p. 118), and *not* equivalent to classical physics with which it can easily be confounded, as Atiyah has done.

It is clear from his videotaped lectures and the above quote that Atiyah has somehow failed to comprehend that functional differential equations already arise in existing physics. The very terminology of "Atiyah's hypothesis", apart from being historically objectionable, is conceptually inaccurate—for no fresh hypothesis is needed. Atiyah's error on this point was no slip, for this point about fields did not register with him, even though (a) it was forcefully raised by David Gross during Atiyah's talk at the Kavli Institute (see the streaming video of Atiyah's talk at http://online.kitp.ucsb.edu/online/strings05/atiyah/), and was (b) subsequently reiterated in the email of 26 October 2005, sent by my son to Atiyah. Further, (c) Atiyah persisted in this error while approving the *Notices* article. An error repeated thrice shows lack of understanding. As a Field medallist, Atiyah is highly regarded, but he has erred in understanding the mathematics of the field.

On the epistemic test, this error of understanding points also to a further ethical error, in addition to those already mentioned. Mere belated acknowledgement of the undeniable fact of my past work is inadequate to undo all this, especially since Atiyah has not expressed any remorse so far, for any ethical error. It is now for the mathematics community at large either to recognize this as an ethical error and censure it, or else to encourage others to emulate the example set by Atiyah, and avoid acknowledging past work, until pressed to do so. The only third possibility I can see is to explicitly adopt double standards.

Annex 7

(Descriptive account of events as they unfolded since October 2005.)

Background: Atiyah's claim

Sir Michael Atiyah was awarded the Fields Medal 1966, and Abel Prize (2004) (both awards at the level of the Nobel prize). He has been former President of the Royal Society, London, and is currently the President of the Royal Society, Edinburgh. Atiyah gave the Einstein lecture on 21 October 2005, during a meeting of the American Mathematical Society at the University of Nebraska-Lincoln.[1] This lecture was organized to commemorate the 100th anniversary of Einstein's "miraculous year" during which he published his paper on the theory of relativity.

During the course of this lecture, Atiyah, regarded as one of the greatest mathematicians alive, proposed a new mathematical paradigm for physics, *a la* Newton, suggesting that this might resolve the long-standing mystery of quantum mechanics. Atiyah repeated this claim also in his talk of 24 October 2005 at the Kavli Institute of Theoretical Physics, California, "Does the Universe Have Memory".[2]

Atiyah's lecture was reported in the *Notices of the American Mathematical Society* in June/July 2006, by G. W. Johnson and M. Walker.[3] Under a section called "A New Paradigm?" they referred to this as "Atiyah's hypothesis". They explained that the substance of of "Atiyah's hypothesis" was that physics should use knowledge of the past to decide the future, and not merely knowledge of the present, and that his might lead to quantum mechanics. They quoted Atiyah as saying "don't forget I suggested it". This journal enjoys a very wide circulation, hence, this article was widely noticed. This idea started being discussed[4] in the physics community as "Atiyah's new basis for quantum mechanics".

[1] A streaming video of the talk is at `http://www.math.unl.edu/~bharbourne1/Atiyah.html`. For pictures of the overflowing audience, see `http://www.math.unl.edu/~bharbourne1/AMS05/ag.html`.

[2] For a streaming video of this talk, see `http://online.kitp.ucsb.edu/online/strings05/atiyah/`.

[3] The article is at `http://www.ams.org/notices/200606/comm-walker.pdf`. The key parts are on pp. 677–78

[4] e.g., `http://physicsforums.com/archive/index.php/t-96806.html`

Background: my earlier work

Over a decade earlier, I had made some very similar claims in my book *Time: Towards a Consistent Theory* (Kluwer Academic, 1994), published by a reputed publisher, which has enjoyed an international circulation since then.

In my book I had proposed the mathematics (of functional differential equations) as a new basis for physics. I had stated and explained in detail how taking into account knowledge of the past, using retarded (delay) differential equations, led to departures from the Newtonian paradigm. Further, I had linked the new mathematics of functional differential equations to axiomatic quantum mechanics (as a theorem, rather than a conjecture as Atiyah made out).

As happens in the case of a paradigm shift, many physicists had difficulty in understanding the new mathematical techniques I was proposing, and found it difficult to accept the surprising consequences. With a view to initiate a dialogue with colleagues, who found my point of view hard to understand, I had first published these ideas in an expository manner in a pedagogical forum (*Physics Education*, India) in a series of ten articles "On Time" from 1990–93 (Of these 10 articles, parts 5b and 6b are most directly relevant.) These articles were later consolidated into the book.

My initial computations with retarded (delay) differential equations led to conclusions that were very surprising even from the viewpoint of text-book physics at the school level. Hence, I presented these ideas at various national and international conferences, and lectures. (A list of these conferences is attached.) For example, in 1999 alone I spoke on these ideas at a seminar at the University of Southampton (C. J. S. Clarke and P. T. Landsberg), at a conference on *Time* in Liverpool (where I spoke on "Relativity: History and History Dependence"), at a conference in Groningen, and subsequently at Utrecht (Dennis Dieks).

This often led to heated debate, as at the one-day meeting in Groningen in 1999, where, both during and after the conference, Prof. H. D. Zeh objected very strongly to the claim of a paradigm shift because he did not grasp that the paradigm shift was coming about not through a new physical hypothesis, but through an improved mathematical understanding of old physics.

In my book, *The Eleven Pictures of Time* (Sage, 2003), I explained, at the level of a layperson, the consequences of dependence upon the past. I also went into the underlying philosophical changes that were required in

physics. (The proposal was that the field, like the aether, originated in the philosophy of action by contact, and should be rejected like the aether, since it only provided an inadequate and misleading mental picture.)

In 2004, I eventually published the first solutions of these retarded functional differential equations in a significant physical context.[5] I explicitly prescribed past history, and pointed out the surprising consequences for quantum mechanics. (H. D. Zeh is one of the editors of the journal.)

This was hardly the end of all surprising consequences, and even on 20 October 2005, I gave a lecture at the University of Sydney on how this very new mathematics impacts time travel.[6]

Sequence of events

I first heard about Atiyah's lecture from my son. Since my son is doing his PhD in string theory at Harvard, he watched Atiyah's lecture of 24 October 2005 by webcast. Noticing the similarity with my ideas he immediately sent me an email. I was then visiting the National University of Singapore, where I had limited access to the Internet and could not download the 19 MB video of Atiyah's talk. So, I asked my son to respond immediately and he sent a mail to Atiyah on 26 October 2005, referring to my 1994 book, and attaching a copy of my 2004 paper. Atiyah acknowledged this mail, and the relatedness of the ideas. Subsequently, I wrote to Atiyah, who responded the first time, but not the second time, and there the matter seemed to rest.

After I came to know about the Johnson and Walker article (*Notices*, June/July 2006), I wrote to Johnson and Walker, in October 2006, urging them to publish a retraction, acknowledging my prior work. They resisted in various ways (by not responding, then delaying matters, then trying to deflect me into a private conversation with Atiyah, etc.) Eventually, I even petitioned the chancellor of their university—a specialist on IP rights, who did not respond. I attached annotated excerpts to demonstrate the overlap

[5] "The Electrodynamic 2-Body Problem and the Origin of Quantum Mechanics", *Foundations of Physics*, **34** (2004) 937–62. Preprint available online at `arxiv.org/pdf/quant-ph/0511235`.

[6] "Why time travel is possible, but time machines are not". For the announcement of this talk, see `http://www.usyd.edu.au/time/conferences/CHIMP-Spring-2005.htm`. This was later published as "Time Travel and the Reality of Spontaneity" in *Foundations of Physics* **36** (7) (June 2006), pp. 1099-1113.

of ideas.[7] A copy of this petition was also sent to the *Notices*. Eventually, in Dec 2006, Johnson and Walker agreed to write a letter to the *Notices*, setting out my references.

This letter, mentioning my past work, has recently been published in the April 2007 issue of the *Notices*.[8]

Present status

While the undeniable fact of my past work has now been acknowledged, Atiyah has expressed no remorse for having done anything unethical, and he continues to propagate his claim on those ideas. Most recently, he spoke on the same topic ("Some radical thoughts on the foundations of physics") while inaugurating a conference in Paris on 29 March 2007.[9]

So, is belated acknowledgment of my work enough? In my rejoinder, "Is this Ethical?" (submitted for publication to the *Notices of the AMS* on 9 April 2007), I have argued that:

1. Atiyah's claim of independent rediscovery of my ideas is unethical according to the AMS ethical guidelines,[10] solely on the ground that my work was published, and widely disseminated from over a decade earlier. (The AMS ethical guidelines do not require anyone to prove that deliberate copying has taken place: an author is expected to be well informed of the past work on his topic.)

2. However, in the present case, one can go a step further and show that my past work was *deliberately* suppressed by Atiyah: whether or not he knew of my work at the time of his Einstein lecture of 21 October

[7] These were similar to the annexes attached with this petition, which have been revised and updated. These annexes may be downloaded from `http://11picsoftime.com/Atiyah_annexes.pdf`.

[8] Notices of the AMS *54*(4) p. 472, available at `http://www.ams.org/notices/200704/commentary-web.pdf`.

[9] The conference page is at `http://www.institut.math.jussieu.fr/projets/ao/evenements/2007/ncg/program.html`. This only gives the title of his talk. The contents of the talk are described in French in a blog at `http://math-et-physique.over-blog.com/article-6246666.html`. The equation mentioned there has been considered in my book and paper.

[10] ironically published in the same June-July 2006 issue of the *Notices* pp. 701-03, and available at `http://www.ams.org/notices/200606/from-ethical.pdf`.

2005, he certainly knew of it at the time of the June-July 2006 article in the *Notices*. Had my work been acknowledged, that most-noticed part of the article could not have been written. But not only did my work go unacknowledged, Atiyah was still quoted as saying "don't forget I suggested this". Nor has Atiyah retracted this claim till now. This establishes that what was done was done deliberately.

3. In fact, though this is not at all required, one can go still further, and prove that Atiyah's claim of independent rediscovery of my ideas lacks credibility, from the very beginning. Thus, Atiyah has exposed his lack of understanding through the following mistake. Atiyah has advanced the idea of history dependence as a *hypothesis*, but the fact is that (a) the notion of the field, in present-day physics, already incorporates history dependence, albeit (b) in a non-transparent and inconvenient way. Atiyah's mistake is no oversight. The point (a) was brought to Atiyah's notice by David Gross, 2004 Nobel prize winner in physics, during Atiyah's talk of 24 October 2005. It was also brought to his notice in my son's letter. But Atiyah persisted with this mistake while approving the Johnson-Walker article. According to the commonsense rules of evidence proposed in my forthcoming book, Atiyah's lack of understanding is evidence of transmission, and negates his claim of "independent rediscovery", *ab initio*. (Atiyah has made another mistake, related to quantum mechanics, that I haven't gone into in my letter since that format does not provide enough space, and few people understand these aspects of quantum mechanics, related to its non-locality.)

   Something happened to my letter submitted to the *Notices*, and its receipt was finally acknowledged only on 7/8 May 2007. That is where matters stand as of date.

Annex 8

(Copies of email correspondence with Johnson and Walker and subsequently Atiyah leading up to acknowledgment.)

Annex 8

msn® Hotmail®

c_k_raju@hotmail.com Printed: Thursday, November 2, 2006 3:06 AM

From : C. K. Raju <c_k_raju@hotmail.com>
Sent : Tuesday, October 31, 2006 6:40 AM
To : gjohnson@math.unl.edu, mwalker@math.unl.edu
CC : notices@math.ou.edu, amagid@ou.edu
Subject : Sir Michael Atiyah's Einstein lecture

Attachment : Atiyahcorrespondence.pdf (0.17 MB), HindustanTimes8Nov04_and_UNI.pdf (0.24 MB)

MIME-Version: 1.0
X-Originating-IP: [59.161.91.43]
X-Originating-Email: [c_k_raju@hotmail.com]
X-Sender: c_k_raju@hotmail.com
Received: from bay0-omc2-s26.bay0.hotmail.com ([65.54.246.162]) by bay0-imc3-s33.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Mon, 30 Oct 2006 22:40:19 -0800
Received: from hotmail.com ([207.46.10.210]) by bay0-omc2-s26.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.1830); Mon, 30 Oct 2006 22:40:08 -0800
Received: from mail pickup service by hotmail.com with Microsoft SMTPSVC; Mon, 30 Oct 2006 22:40:07 -0800
Received: from 207.46.10.254 by by122fd.bay122.hotmail.msn.com with HTTP;Tue, 31 Oct 2006 06:40:04 GMT
X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlK+3Wz9fWrJ/U=
X-OriginalArrivalTime: 31 Oct 2006 06:40:07.0760 (UTC) FILETIME=[67D0F500:01C6FCB7]
Return-Path: c_k_raju@hotmail.com

To,

Professor G. W. Johnson <gjohnson@math.unl.edu>
& Professor Mark Walker <mwalker@math.unl.edu>

Dear Professors Johnson and Walker,

This refers to your article, "Sir Michael Atiyah's Einstein Lecture", published in the Notices of the American Mathematical Society in June 2006 at

http://www.ams.org/notices/200606/comm-walker.pdf .

I wonder if you discussed the article with Professor Atiyah prior to publication?

My objections relate to a couple of passages in your article which I quote below.

==========
Quote 1.

Atiyah noted that all physical models since Newton, including even quantum mechanics, have assumed one basic premise---that we can predict the future from full knowledge of the present. Atiyah suggested an alternative to this paradigm: Perhaps we need full knowledge of the present and the past in order to predict the future. That is, maybe the universe has memory....Atiyah’s hypothesis possibly leads to several interesting consequences:

1. The mathematics used in physical theory would become more difficult, since all previously used mathematics in physics assumes that knowledge of the present suffices. With the new paradigm,
for example, retarded (or delay) differential equations would become necessary.

2. Since we do not have complete knowledge of the past, uncertainty would arise. This might shed
light on the uncertainty inherent in quantum mechanics."

==========
Quote 2.

Do we need to modify quantum mechanics? Atiyah
closed by saying “This is for young people: Go
away and explore it. If it works, don’t forget I suggested it. If it doesn’t, don’t hold me responsible.”

==========

To understand what is objectionable about this you may also like to peruse the attached email correspondence with Professor Atiyah, soon after this lecture.

The claim that classical physics involves functional differential equations (FDE) which lead to quantum mechanics was first argued by me in a series of ten papers, "On Time" published in the journal Physics Education, during 1990--93, and especially in the articles "5b: Electromagnetic time" and "6b: Quantum-mechanical time". This was again published (as corresponding chapters) in a book "Time: Towards a Consistent Theory" (Kluwer Academic 1994) together with some additions, and a detailed formal proof that much of the axiomatic structure of quantum mechanics could be derived starting from FDE, but that certain aspects of quantum mechanics need to be modified. An account of this, accessible to the layperson, was published in another book, "The Eleven Pictures of Time" (Sage 2003). The first solution of retarded (or delay) differential equations, in a realistic physical context (of the hydrogen atom), was published by me in "The

electrodynamic 2-body problem and the origin of quantum mechanics" in Found. Phys. 34 (2004) 937--62.

Apart from publications, various aspects of my thesis had been widely discussed in presentations made at many earlier international conferences, since 1989, for example, at the international conference _Retrocausality Day_ at Groningen in 1999. At this conference, and in subsequent email discussions, my claim that FDE's require a paradigm shift became a persistent issue of debate as reported in the above 2004 article.

Against this background of publications and public debate stretching back to some 15 years earlier, and in these days of easy Internet access, I hence find it most extraordinary that you should refer to this as "Atiyah's hypothesis" and that Professor Atiyah should tell young people "Don't forget I suggested it." It is, of course, well known that the Notices of the American Mathematical Society is a common vehicle used to establish priority.

To go a step further, as one who also does a bit of the history of science, I appreciate the following. Suppose, for the sake of argument, that _I_ had claimed to have originated ideas that had been repeatedly published from over a decade earlier by Atiyah, this would have been instantly called a clear case of plagiarism. No one would have believed for a moment that I would not have gone to the library and seen a book, which is known to experts. Cynics might even have called it a case of the "50 cent solution". (This is the name used in the book "Write Any Book in 28 Days", whose author, Nick Daws, recommends that ideas should be mined by going to a second-hand book shop, and buying for 50 cents a book which is from ten to twenty years old. Obviously, less than ten years is too recent, and more than twenty years is too old.) However, in the reverse situation, when Atiyah claims to have originated ideas that I have been publishing on for so long, it might seem natural to argue that it is a case of independent rediscovery.

Note that, in the first (hypothetical) case the onus of proving innocence would be on me, and in the second case also the onus of proving guilt would be on me. Thus, in either case, the onus of proof would be on me.

If this unfair situation seems natural it is because so many key claims of the Western history of science are based on the systematic use of this trick of shifting the onus of proof, or the use of a double standard of evidence.

For example, it has been known for over 50 years that Copernicus' model is identical to the earlier models of Ibn as Shatir and the Margaha astronomers. However, some reputed historians still argue that Copernicus could have made an independent rediscovery. The very same historians use a completely different standard of evidence to claim the "Hellenic" origin and transmission of various ideas. Thus, the Western history of science relies heavily on the trick of seizing credit by shifting the onus of proof, and varying the standard of evidence with the person concerned.

Similarly, Fermat's famous challenge problem (on "Pell's" equation) is a solved exercise in a text of the Indian mathematician Bhaskara-II, from some four centuries earlier---a very curious case of "independent rediscovery", since the two numbers involved are so very large (nine and ten digits), and yet they are the same.

These two cases are well known, but details of these and several other such cases may be found in my forthcoming volume "Cultural Foundations of Mathematics: The Nature of Mathematical Proof and the Transmission of the Calculus from India to Europe in the 16th c. CE" (PHISPC and Pearson Education, in press). The point is that the claim of famous Western names to many revolutionary discoveries hangs on the systematic appeal to such double standards of evidence. In my opinion, such double standards of evidence should, of course, be rejected.

So, if such a claim of "independent rediscovery" is going to be made, in the present case, I think it needs to be put on the table and discussed publicly along with the double standards of evidence used to support the long series of earlier such claims. Such a public discussion would also make clear whether it would be a good idea to make these double standards universal, so that we can have a great proliferation of such claims of "independent rediscovery" from all quarters of the world, and not only from famous names in the West.

I may add that, in the above volume on "Cultural Foundations of Mathematics", I have proposed to replace the above double standard by a new and uniform standard of evidence. This new standard was applied in another recent case to decide the issue of transmission vs independent rediscovery (please see attached newspaper reports, and letter of apology). If it becomes necessary, I will publicly explain, at an appropriate point of time exactly how that standard of evidence convincingly applies also to the present case.

I would appreciate if you would print a retraction, pointing out that what you have called "Atiyah's hypothesis" relates to a thesis that has been argued out by me in several publications and public debates over more than a decade. (I call it "thesis", not "hypothesis" since I have made no new physical assumption at all, as explained in more detail in the above publications, and as pointed out in the review of my book by J. F. Woodward in Found. Phys. 26 (1996) 1725--30.)

I look forward to your response,

With good wishes,

C. K. Raju

Attachments:

1. Email correspondence with Professor Atiyah.
2. Copy of news reports (Hindustan Times 8 Nov 2004, UNI despatch, 23 Nov 2004), and letter of apology.

Copy to:

Professor Andy R. Magid
Editor, Notices of the American Mathematical Society
<notices@math.ou.edu>,
<amagid@ou.edu>

16 Nov 2006

Professors G. W. Johnson and M. Walker
Department of Mathematics,
University of Nebraska-Lincoln
203 Avery Hall, Lincoln, NE 68588-0130
USA
Tel: 001-402-472-3731
Fax: 001-402-472-8466

Dear Professors Johnson and Walker,

This is to confirm that I have sent you an email (copy attached) on 31 October 2006, together with the listed attachments. The email pertained to the article "Sir Michael Atiyah's Einstein Lecture" (*Notices of the AMS*, **53** (6) June/July 2006, pp. 674–78), and asked you to retract the phrase "Atiyah's hypothesis" used in that article, and to acknowledge my published work in this direction (linking functional differential equations to quantum mechanics) from over a decade earlier.

Ironically, the same issue of the *Notices of the AMS* carries detailed ethical guidelines of the Society (pp. 701–03) that are very pertinent to the issue at hand, and from which I quote extensively below.

Why is this issue important? "to help in the preservation of that atmosphere of mutual trust and ethical behavior required for science to prosper". (p. 701)

What is the material loss due to incorrect attribution? "The correct attribution of mathematical results is essential…because it encourages creativity, by benefiting the creator whose career may depend on the recognition of the work". (p. 701)

Can't one claim to have independently arrived at the same idea? "A claim of independence may not be based on ignorance of widely disseminated results." (p. 702) (My results have been widely disseminated, through two books, papers, and a number of conferences and seminars, my website, and even newspapers.)

Furthermore, the same ethical guidelines point out the responsibilities of mathematicians, which include the following.

"To use no language that suppresses or improperly detracts from the work of others." (p. 702) (The phrase "Atiyah's hypothesis" does exactly that.)

"To correct in a timely way or to withdraw work that is erroneous." (p. 702)

I am waiting for that last bit to happen. Although 15 days have passed, I have received no acknowledgment, nor any indication of what you propose to do (or in what time frame).

While the AMS may not be able to enforce its ethical guidelines, it can perhaps clarify (through statements or actions) whether there are separate ethical guidelines for cases where the individuals involved are well-known mathematicians.

A confirmation copy of this fax is also being sent by post (Registered Air Mail).

With good wishes,

Yours sincerely,

C. K. Raju
Fax: 91-11-2272-4533

Copy to

Professor Andy Magid
Editor, Notices of the AMS
Department of Mathematics
601 Elm, PHSC 423,
University of Oklahoma,
Norman, OK 73019-0001
USA

Fax: 001-405-325-7484
001-405-325-5765

msn® Hotmail®

c_k_raju@hotmail.com Printed: Monday, November 20, 2006 3:59 AM

From : Mark Walker <mwalker5@math.unl.edu>
Sent : Saturday, November 18, 2006 5:44 AM
To : "C. K. Raju" <c_k_raju@hotmail.com>
CC : gjohnson@math.unl.edu, mwalker@math.unl.edu
Subject : Re: Sir Michael Atiyah's Einstein lecture

MIME-Version: 1.0
Received: from mathstat.unl.edu ([129.93.181.2]) by bay0-mc2-f12.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Fri, 17 Nov 2006 21:44:21 -0800
Received: from [192.168.1.101] (24-208-96-092.cable.inebraska.com [24.208.96.92])(authenticated bits=0)by mathstat.unl.edu (8.13.1/8.13.1) with ESMTP id kAI5iHTA030862(version=TLSv1/SSLv3 cipher=DHE-RSA-AES256-SHA bits=256 verify=NO);Fri, 17 Nov 2006 23:44:20 -0600
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References: <BAY122-F2C434EE3785AEB18CF14ACBF90@phx.gbl>
Return-Path: mwalker5@math.unl.edu
X-OriginalArrivalTime: 18 Nov 2006 05:44:21.0643 (UTC) FILETIME=[98CF85B0:01C70AD4]

Dear C K Raju,

I do apologize for not replying more promptly to your original e-mail. My colleage, Jerry Johnson, was traveling and we only had a chance to discuss it a few days ago.

Jerry and I feel it would be best if you were to correspond directly with Atiyah on this matter. The article we wrote is, I believe, a fairly accurate description of Atiyah's talk, but neither of us would claim to have a detailed understanding of this field.

Best Regards,

Mark Walker

msn® Hotmail®

c_k_raju@hotmail.com Printed: Sunday, November 26, 2006 11:46 AM

From : C. K. Raju <c_k_raju@hotmail.com>
Sent : Tuesday, November 21, 2006 12:07 PM
To : mwalker@math.unl.edu, gjohnson@math.unl.edu, mwalker5@math.unl.edu
CC : notices@math.ou.edu, amagid@ou.edu
Subject : Re: Sir Michael Atiyah's Einstein lecture

Attachment : FaxtoJohnsonandWalker16Nov06.pdf (0.02 MB)

MIME-Version: 1.0
X-Originating-IP: [59.161.77.102]
X-Originating-Email: [c_k_raju@hotmail.com]
X-Sender: c_k_raju@hotmail.com
Received: from bay0-omc3-s38.bay0.hotmail.com ([65.54.246.238]) by bay0-imc1-s37.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Tue, 21 Nov 2006 04:07:16 -0800
Received: from hotmail.com ([65.55.136.85]) by bay0-omc3-s38.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.1830); Tue, 21 Nov 2006 04:07:12 -0800
Received: from mail pickup service by hotmail.com with Microsoft SMTPSVC; Tue, 21 Nov 2006 04:07:12 -0800
Received: from 65.55.136.123 by by131fd.bay131.hotmail.msn.com with HTTP;Tue, 21 Nov 2006 12:07:08 GMT
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Return-Path: c_k_raju@hotmail.com

Dear G. W. Johnson and M. Walker,

Thank you for your response. Your advice on what I should be doing seems completely unrelated to the issues at hand, which are the following.

Your article (Notices of the AMS, July 2006) has given rise to questions of ethical propriety because you have highly praised and given credit to Atiyah for some of my original ideas. Although my ideas were published from over a decade earlier, Atiyah has no published work in this direction, and your article is the sole published basis of his claim to those ideas. In my fax of 16 November (copy attached) I had clearly explained to you how this action of yours is contrary to the ethical guidelines of the AMS, and how this has caused me material injury. In my earlier email (of 31 October), I had explained to you how this action of yours has been instrumental in furthering a systematic process of concocting history by appropriating credit for revolutionary ideas---a process that has materially damaged the interests of entire communities, including specifically the Indian community, by reinforcing a racist stereotype. Finally, your actions have left thousands of readers of the Notices misinformed about the true facts about what is very probably the next big thing in physics. The question, therefore, is what _you_ should be doing to correct this state of affairs that you have been instrumental in bringing about, whether by design or by accident.

To reiterate an example, the phrase "Atiyah's hypothesis" coined by you, and used in the impugned article, suppresses my past work in this direction. Under the section "A New Paradigm?" you state "Atiyah suggested an alternative to this paradigm", etc. etc., suggesting that these ideas linking functionaldifferential equations to quantum mechanics are all original with Atiyah, when, in fact, I have already published on them, from over a decade earlier. Furthermore, this language has been used by you in the Notices, which is a common vehicle used to establish historical priority. Since these objectionable and incorrect claims were published under your signature, I have to ask you and not any one else to retract them.

As for the origin of the erroneous claims---whether you misunderstood Atiyah, or were misled by him, or had a prior understanding with him---I have not offered (or formed) any opinion so far, and I am sure the Editor of the Notices will provide you some space to present your explanation for the origin of the error.

I have only pointed to the indubitable error of your statements (of attributing to Atiyah ideas on which I have been publishing for over a decade), and asked you to correct your article. In doing this, I have allowed you the benefit of the doubt, and proceeded on presumed innocence. Benefit of doubt and presumption of innocence, however, is not something to which you are permanently entitled. If your error was an inadvertent or innocent one, then you would naturally have been eager to correct it at the first opportunity. If, however, you fail to retract your erroneous claims even after your error has been pointed out to you, the doubt is settled, for the inference is unavoidable that your error was wilfully intended, by design. Delay or reluctance in retracting will invite similar inferences.

Your excuse that you are not experts in the field is remarkable for its ethical insensitivity. A journalist need not be an expert in the field in which he reports, but if he fails to correct an error, even after it has been brought to his notice, that is unethical. What is required from you is not expertise, but a sense of responsibility. A public error requires a public correction, not a private admission of incompetence---unless the very intention is to maintain the error.

Secondly, the ethical guidelines of the AMS explicitly point out that responsibility devolves on mathematicians to be knowledgeable about what they write about. Contrapositively, you should not write about things you are not knowledgeable about, and if you do so, you are not being responsible according to those guidelines. That is, in the context of a scholarly journal, a plea of ignorance is tantamount to an admission of irresponsibility (else every plagiarist could claim ignorance with facility). By those standards, you ought not to have used categorical language ("all previously used mathematics in physics assumes that knowledge of the present suffices") if you did not know this to be true of your own knowledge. Remarkably, however, while lack of expertise did not prevent you from irresponsibly allocating credit, and advancing categorical claims, you seem to regard it as justification for not retracting those claims! This is very curious. If such justification were to be accepted, it would permit ethical norms to be systematically bypassed by plagiarists working in teams, with one party using a plea of inexpertise as justification both to advance and to sustain false claims on behalf of the other party.

Thirdly, my expositions about retarded differential equations and quantum mechanics are pitched at the level of undergraduate and graduate students (e.g. "Electromagnetic time", Physics Education (India), 9(3) (1992) 251--65, and chps. 5b and 6b in Time: Towards a Consistent Theory, Kluwer Academic, 1994). I have also explained these things at the level of a layperson (The Eleven Pictures of Time, Sage, 2003, chp. 9, sections on "Einstein's mistake" pp. 298-99, and "History dependence", pp. 301-03). Understanding the similarity of my work to what you have attributed to Atiyah does not, therefore, require a very high level of mathematical competence. Surely you can also see, for example, that my paper on "The electrodynamic 2-body problem and the origin of quantum mechanics" (Found. Phys. 34 (2004) 937--62) solves a system of retarded (delay) differential equations, and links this to quantum mechanics---just the thing you have categorically asserted has not been done before Atiyah. Assuming that you are expert enough to understand what you yourself have written, if you still fail to acknowledge this it can only be because you have some very strong motive to deny what is so manifest and obvious.

You state that you have reported Atiyah's talk only "fairly accurately", hence possibly inaccurately. Exactly which parts are inaccurate? Is the part about "Atiyah's hypothesis" accurate or inaccurate? Given, further that, by your own admission, you are inexpert in the field, exactly what special measures did you take to ensure accuracy? Did you show a draft of your article to Atiyah prior to publication? (I asked you this question also in my earlier mail, and I record here that you have avoided answering it, though answering it could have helped to clarify matters.) Or did you compound the irresponsibility by not bothering to ensure accuracy? In this context, your gratuitous advice to me to correspond privately with Atiyah acquires some very unpleasant connotations---especially since you are well aware that I have already done that, since I attached the correspondence with Atiyah in my previous email to you.

To summarise, your violation of the AMS ethical guidelines is established, and there remains only the question of whether this was intentional. I was hoping that the matter could be settled amicably without having to petition the authorities of your university, or having recourse to a court of law, for redress. Accordingly, I would like to give you one more opportunity to make amends voluntarily, by publishing a straightforward retraction pointing out that what you have called "Atiyah's hypothesis" should properly be called "Raju's thesis" according to the facts and references that are now certainly at your disposal.

If I may give you a piece of advice in return: please do not underestimate the sense of injustice and outrage that your actions have evoked not only in me, but in a large community of Indians, and please do not also underestimate the resolve to right such wrongs, and expose the truth. Therefore, please do not imagine that you can somehow evade the substantive responsibility for your actions by means of quibbles.

A copy of this email is being faxed to you for confirmation.

Sincerely,

C. K. Raju

Copy to:

Prof. Andy Magid
Editor
Notices of the AMS

=======original message=======

From: Mark Walker <mwalker5@math.unl.edu>
To: "C. K. Raju" <c_k_raju@hotmail.com>
CC: gjohnson@math.unl.edu, mwalker@math.unl.edu
Subject: Re: Sir Michael Atiyah's Einstein lecture
Date: Fri, 17 Nov 2006 23:44:24 -0600
MIME-Version: 1.0
Received: from mathstat.unl.edu ([129.93.181.2]) by bay0-mc2-f12.bay0.hotmail.com with Microsoft SMTPSVC (6.0.3790.2444); Fri, 17 Nov 2006 21:44:21 -0800
Received: from [192.168.1.101] (24-208-96-092.cable.inebraska.com [24.208.96.92])(authenticated bits=0)by mathstat.unl.edu (8.13.1/8.13.1) with ESMTP id kAI5iHTA030862(version=TLSv1/SSLv3 cipher=DHE-RSA-AES256-SHA bits=256 verify=NO);Fri, 17 Nov 2006 23:44:20 -0600
X-Message-Info: LsUYwwHHNt0X+wLt7+kHeBtwm9ahVD07TDEwhvbXJvg=
User-Agent: Mozilla Thunderbird 1.0 (Windows/20041206)
X-Accept-Language: en-us, en
References: <BAY122-F2C434EE3785AEB18CF14ACBF90@phx.gbl>
Return-Path: mwalker5@math.unl.edu
X-OriginalArrivalTime: 18 Nov 2006 05:44:21.0643 (UTC) FILETIME=[98CF85B0:01C70AD4]

Dear C K Raju,

I do apologize for not replying more promptly to your original e-mail. My colleage, Jerry Johnson, was traveling and we only had a chance to discuss it a few days ago.

Jerry and I feel it would be best if you were to correspond directly with Atiyah on this matter. The article we wrote is, I believe, a fairly accurate description of Atiyah's talk, but neither of us would claim to have a detailed understanding of this field.

Best Regards,

Mark Walker

msn® Hotmail®

c_k_raju@hotmail.com Printed: Sunday, November 26, 2006 11:47 AM

From : Mark Walker <mwalker5@math.unl.edu>
Sent : Tuesday, November 21, 2006 3:34 PM
To : "C. K. Raju" <c_k_raju@hotmail.com>
CC : mwalker@math.unl.edu, gjohnson@math.unl.edu, notices@AFTERMATH.math.ou.edu, amagid@ou.edu
Subject : Re: Sir Michael Atiyah's Einstein lecture

MIME-Version: 1.0
Received: from mathstat.unl.edu ([129.93.181.2]) by bay0-mc9-f10.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Tue, 21 Nov 2006 07:34:44 -0800
Received: from [129.93.180.219] (math-097.unl.edu [129.93.180.219])by mathstat.unl.edu (8.13.1/8.13.1) with ESMTP id kALFYdRY003031;Tue, 21 Nov 2006 09:34:42 -0600
X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlK3oXsmRrh6gU=
User-Agent: Thunderbird 1.5.0.8 (X11/20061109)
References: <BAY131-F5395365097EE7B0FB3DB0CBEC0@phx.gbl>
Return-Path: mwalker5@math.unl.edu
X-OriginalArrivalTime: 21 Nov 2006 15:34:45.0298 (UTC) FILETIME=[92319120:01C70D82]

Dear C. K. Raju,

I am about to leave town for several days and therefore I can only, at this time, reply briefly to some of the points in your long message.

I had intended (but forgot to do so) to mention in my previous message that Atiyah did indeed see a draft of this article prior to its publication. You say you included copy of correspondence with Atiyah, but in fact you included a copy of an e-mail from someone else to Atirah, his reply to that e-mail, and a short e-mail from Atiyah to you (but no messages from you). I have little idea of what you wrote to Atiyah. It is for these reasons that I suggested you contact him directly.

If you would prefer, I can forward your messages to us on to him.

Sincerely,

Mark Walker

C. K. Raju wrote:

> Dear G. W. Johnson and M. Walker,
>
> Thank you for your response. Your advice on what I should be doing seems completely unrelated to the issues at hand, which are the following.
>
> Your article (Notices of the AMS, July 2006) has given rise to questions of ethical propriety because you have highly praised and given credit to Atiyah for some of my original ideas. Although my ideas were published from over a decade earlier, Atiyah has no published work in this direction, and your article is the sole published basis of his claim to those ideas. In my fax of 16 November (copy attached) I had clearly explained to you how this action of yours is contrary to the ethical guidelines of the AMS, and how this has caused me material injury. In my earlier email (of 31 October), I had explained to you how this action of yours has been instrumental in furthering a systematic process of concocting history by appropriating credit for revolutionary ideas---a process that has materially damaged the interests of entire communities, including specifically the Indian community, by reinforcing a racist stereotype. Finally, your actions have left thousands of readers of the Notices misinformed about the true facts about what is very probably the next big thing in physics. The question, therefore, is what _you_ should be doing to correct this state of affairs that you have been instrumental in bringing about, whether by design or by accident.
>
> To reiterate an example, the phrase "Atiyah's hypothesis" coined by you, and used in the impugned article, suppresses my past work in this direction. Under the section "A New Paradigm?" you state "Atiyah suggested an alternative to this paradigm", etc. etc., suggesting that these ideas linking functionaldifferential equations to quantum mechanics are all original with Atiyah, when, in fact, I have already published on them, from over a decade earlier. Furthermore, this language has been used by you in the Notices, which is a common vehicle used to establish historical priority. Since these objectionable and incorrect claims were published under your signature, I have to ask you and not any one else to retract them.
>
> As for the origin of the erroneous claims---whether you misunderstood Atiyah, or were misled by him, or had a prior understanding with him---I have not offered (or formed) any opinion so far, and I am sure the Editor of the Notices will provide you some space to present your explanation for the origin of the error.
>
> I have only pointed to the indubitable error of your statements (of attributing to Atiyah ideas on which I have been publishing for over a decade), and asked you to correct your article. In doing this, I have allowed you the benefit of the doubt, and proceeded on presumed innocence. Benefit of doubt and presumption of innocence, however, is not something to which you are permanently entitled. If your error was an inadvertent or innocent one, then you would naturally have been eager to correct it at the first opportunity. If, however, you fail to retract your erroneous claims even after your error has been pointed out to you, the doubt is settled, for the inference is unavoidable that your error was wilfully intended, by design. Delay or reluctance in retracting will invite similar inferences.
>
> Your excuse that you are not experts in the field is remarkable for its ethical insensitivity. A journalist need not be an expert in the field in which he reports, but if he fails to correct an error, even after it has been

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c_k_raju@hotmail.com Printed: Sunday, November 26, 2006 11:49 AM

From : Mark Walker <mwalker5@math.unl.edu>
Sent : Tuesday, November 21, 2006 5:21 PM
To : "C. K. Raju" <c_k_raju@hotmail.com>
CC : mwalker@math.unl.edu, gjohnson@math.unl.edu, notices@AFTERMATH.math.ou.edu, amagid@ou.edu
Subject : Re: Sir Michael Atiyah's Einstein lecture

MIME-Version: 1.0
Received: from mathstat.unl.edu ([129.93.181.2]) by bay0-mc3-f11.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Tue, 21 Nov 2006 09:21:29 -0800
Received: from [129.93.180.219] (math-097.unl.edu [129.93.180.219])by mathstat.unl.edu (8.13.1/8.13.1) with ESMTP id kALHLClC018759;Tue, 21 Nov 2006 11:21:12 -0600
X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlK3oXsmRrh6gU=
User-Agent: Thunderbird 1.5.0.8 (X11/20061109)
References: <BAY131-F5395365097EE7B0FB3DB0CBEC0@phx.gbl>
Return-Path: mwalker5@math.unl.edu
X-OriginalArrivalTime: 21 Nov 2006 17:21:29.0860 (UTC) FILETIME=[7B9C2C40:01C70D91]

Dear C. K. Raju,

Another brief comment I meant to mention is that you can view a video of Atiyah's talk itself by following the link in the following website

http://www.math.unl.edu/~bharbourne1/AMSkeepers/OctoberAMSPhotos.html

Sincerely,

Mark Walker

C. K. Raju wrote:

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>
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>
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> As for the origin of the erroneous claims---whether you misunderstood Atiyah, or were misled by him, or had a prior understanding with him---I have not offered (or formed) any opinion so far, and I am sure the Editor of the Notices will provide you some space to present your explanation for the origin of the error.
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> Your excuse that you are not experts in the field is remarkable for its ethical insensitivity. A journalist need not be an expert in the field in which he reports, but if he fails to correct an error, even after it has been brought to his notice, that is unethical. What is required from you is not expertise, but a sense of responsibility. A public error requires a public correction, not a private admission of incompetence---unless the very intention is to maintain the error.

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c_k_raju@hotmail.com Printed: Sunday, November 26, 2006 11:30 AM

From : C. K. Raju <c_k_raju@hotmail.com>
Sent : Saturday, November 25, 2006 3:46 AM
To : mwalker@math.unl.edu, gjohnson@math.unl.edu, mwalker5@math.unl.edu
CC : notices@math.ou.edu, notices@AFTERMATH.math.ou.edu, amagid@ou.edu
Subject : Re: Sir Michael Atiyah's Einstein lecture

MIME-Version: 1.0
X-Originating-IP: [59.161.1.81]
X-Originating-Email: [c_k_raju@hotmail.com]
X-Sender: c_k_raju@hotmail.com
Received: from bay0-omc3-s28.bay0.hotmail.com ([65.54.246.228]) by bay0-imc1-s40.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Fri, 24 Nov 2006 19:47:02 -0800
Received: from hotmail.com ([65.55.136.117]) by bay0-omc3-s28.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.1830); Fri, 24 Nov 2006 19:46:15 -0800
Received: from mail pickup service by hotmail.com with Microsoft SMTPSVC; Fri, 24 Nov 2006 19:46:15 -0800
Received: from 65.55.136.123 by by131fd.bay131.hotmail.msn.com with HTTP;Sat, 25 Nov 2006 03:46:12 GMT
X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlKagRhjiGbCrU=
X-OriginalArrivalTime: 25 Nov 2006 03:46:15.0226 (UTC) FILETIME=[41DE59A0:01C71044]
Return-Path: c_k_raju@hotmail.com

Dear G. W. Johnson and M. Walker,

Thank you for your two emails. As I too had to travel out of town, the day I received your mail, and have not yet returned, I will be brief.

I await your detailed response, and I hope you will finally take up the immediate issues which you have avoided addressing in your past three emails over the last three weeks. (a) Are you willing to retract, voluntarily and promptly, the error in your impugned article? (b) Was your error intentional?

The egregious and unprecedented feature of the present case is that you claimed originality for Atiyah, admittedly in consultation with him, several months AFTER he was indubitably made aware of my work. The correspondence with Atiyah was attached to show that by 26 October 2005, five days after his Einstein lecture, a copy of my 2004 paper on retarded differential equations, and a reference to my 1994 book, had been mailed to Atiyah, and he replied that he had read my paper and book, and that he accepted the similarity with my past work (which is anyway manifest and undeniable). Your report appeared only in July 2006, but still did not acknowledge my prior work.

Whatever the circumstances leading to your error, this does not absolve you of your personal responsibility to correct expeditiously the erroneous claims published under your signature.

Sincerely,

C. K. Raju

> From: Mark Walker <mwalker5@math.unl.edu>
> To: "C. K. Raju" <c_k_raju@hotmail.com>
> CC: mwalker@math.unl.edu, gjohnson@math.unl.edu, notices@AFTERMATH.math.ou.edu, amagid@ou.edu
> Subject: Re: Sir Michael Atiyah's Einstein lecture
> Date: Tue, 21 Nov 2006 09:34:39 -0600
> MIME-Version: 1.0
> Received: from mathstat.unl.edu ([129.93.181.2]) by bay0-mc9-f10.bay0.hotmail.com with Microsoft SMTPSVC (6.0.3790.2444); Tue, 21 Nov 2006 07:34:44 -0800
> Received: from [129.93.180.219] (math-097.unl.edu [129.93.180.219])by mathstat.unl.edu (8.13.1/8.13.1) with ESMTP id kALFYdRY003031;Tue, 21 Nov 2006 09:34:42 -0600
> X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlK3oXsmRrh6gU=
> User-Agent: Thunderbird 1.5.0.8 (X11/20061109)
> References: <BAY131-F5395365097EE7B0FB3DB0CBEC0@phx.gbl>
> Return-Path: mwalker5@math.unl.edu
> X-OriginalArrivalTime: 21 Nov 2006 15:34:45.0298 (UTC) FILETIME=[92319120:01C70D82]
>
> Dear C. K. Raju,
>
> I am about to leave town for several days and therefore I can only, at this time, reply briefly to some of the points in your long message.
>
> I had intended (but forgot to do so) to mention in my previous message that Atiyah did indeed see a draft of this article prior to its publication. You say you included copy of correspondence with Atiyah, but in fact you included a copy of an e-mail from someone else to Atirah, his reply to that e-mail, and a short e-mail from Atiyah to you (but no messages from you). I have little idea of what you wrote to Atiyah. It is for these reasons that I suggested you contact him directly.
>
> If you would prefer, I can forward your messages to us on to him.
>
> Sincerely,

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c_k_raju@hotmail.com Printed: Tuesday, November 28, 2006 9:03 AM

From : Michael Atiyah <M.Atiyah@ed.ac.uk>
Sent : Monday, November 27, 2006 11:39 AM
To : "C. K. Raju" <c_k_raju@hotmail.com>, Mark Walker <mwalker5@math.unl.edu>, Gregory Moore <gmoore@physics.rutgers.edu>
Subject : response

MIME-Version: 1.0
Received: from smtp814.mail.ird.yahoo.com ([217.146.188.74]) by bay0-mc9-f14.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2444); Mon, 27 Nov 2006 03:39:07 -0
Received: (qmail 57564 invoked from network); 27 Nov 2006 11:39:06 -0000
Received: from unknown (HELO ed.ac.uk) (lily.atiyah@btinternet.com@81.152.113.56 with plain) by smtp814.mail.ird.yahoo.com with SMTP; 27 Nov 2006 11:39:05 -0000
X-Message-Info: LsUYwwHHNt3X0h2MNoG2gTbCxO3tEwhC/A+N3UqHzJ4=
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User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.1; en-US; rv:1.4) Gecko/20030624 Netscape/7.1 (ax)
X-Accept-Language: en-us, en
Return-Path: M.Atiyah@ed.ac.uk
X-OriginalArrivalTime: 27 Nov 2006 11:39:07.0650 (UTC) FILETIME=[A5FA2E20:01C71218]

Dear Professor Raju,

I was sorry to hear from Mark Walker that you felt that I was appropriating your ideas without acknowledgement. I never intended to do so and apologize if I have given that impression.

For some years past I have been mulling over very vague ideas about a new foundation for physics, including quantum mechanics. My real motivation for this came from string theory and that remains my ultimate goal. At various times, before audiences of physicists I have broached such ideas and have received a critical but constructive response. This was the situation when I spoke at KITP the webcast of which elicted the message from your son about your own work. I then made contact with you and also read your book and papers, and we had a friendly exchange of emails. I found your ideas helpful and I publicized your work among my physics colleagues at KITP. Greg Moore at Rutgers, with whom I workd on these questions, would I am sure confirm this.

So far neither you nor I are anywhere near the first goal of explaining quantum mechanics in terms of retarded differential equations. The ideas are intriguing but the obstacles both conceptual and technical remain formidable.

It is a pity that we live in different continents since discussion between us might help us both to make progress. While quantum mechaincs remains a long term goal I have also been more ambitious, exploring quantum field theory and gravity, where there are some encouraging signs, though I may be deluding myself.

As yet my ideas are too inconclusive to merit publication, but if I ever I do get round to publishing anything I will certainly give you all the credit that is due to you.

I do not regard the vague remarks in my lectures(webcast or written up by others) as serious publications. Unfortunately the new electronic medium turns casual conversation into stone.

I hope your son is enjoying Harvard.

Michael Atiyah

msn® Hotmail®

c_k_raju@hotmail.com Printed: Friday, May 11, 2007 8:39 AM

From : C. K. Raju <c_k_raju@hotmail.com>
Sent : Tuesday, December 12, 2006 6:23 PM
To : M.Atiyah@ed.ac.uk
CC : gjohnson@math.unl.edu, mwalker@math.unl.edu, amagid@ou.edu, notices@AFTERMATH.math.ou.edu
Subject : RE: response

MIME-Version: 1.0
X-Originating-IP: [59.161.42.48]
X-Originating-Email: [c_k_raju@hotmail.com]
X-Sender: c_k_raju@hotmail.com
Received: from bay0-omc2-s38.bay0.hotmail.com ([65.54.246.174]) by bay0-imc3-s39.bay0.hotmail.com with Microsoft SMTPSVC (6.0.3790.2444); Tue, 12 Dec 2006 10:23:32 -0800
Received: from hotmail.com ([65.55.136.89]) by bay0-omc2-s38.bay0.hotmail.com with Microsoft SMTPSVC(6.0.3790.2668); Tue, 12 Dec 2006 10:23:23 -0800
Received: from mail pickup service by hotmail.com with Microsoft SMTPSVC; Tue, 12 Dec 2006 10:23:22 -0800
Received: from 65.55.136.123 by by131fd.bay131.hotmail.msn.com with HTTP;Tue, 12 Dec 2006 18:23:18 GMT
X-Message-Info: LsUYwwHHNt3660MmjhEvYg2f34OAemlKagRhjiGbCrU=
X-OriginalArrivalTime: 12 Dec 2006 18:23:22.0565 (UTC) FILETIME=[9B3ABF50:01C71E1A]
Return-Path: c_k_raju@hotmail.com

Dear Professor Atiyah,

I don't quite follow you. Are you suggesting that some sort of acknowledgment to me is implicit in the phrase "Atiyah's hypothesis"? Anyway since you seem to agree that my prior publications ought to be publicly acknowledged, please advise Johnson and Walker accordingly.

You will appreciate that the immediate point under discussion is the Johnson and Walker article (in the Notices, June/July 2006) which suppresses my past work through inappropriate language, approved by you despite full knowledge of my work long before publication of that article.

As for events before 26 October 2005, that requires a separate discussion, since my book was published 12 years ago. You are doubtless aware of the AMS guidelines which rightly characterize as unethical any claim of independence based on ignorance of widely disseminated work. Therefore, I am surprised that you still seem to be making such a claim in a roundabout way. If you think your case is somehow exceptional, I will be pleased to discuss that publicly, especially after my next book comes out in a couple of months---for it details ways of testing the veracity of claims of independent rediscovery.

The connection with quantum mechanics was proved by me as a theorem in my book: Time:Towards a Consistent Theory (Kluwer, 1994, chp. 5b and 6b). My 2004 paper "On the electrodynamic 2-body problem and the origin of quantum mechanics" (Found. Phys. 34 pp. 937--62) solved the retarded differential equations of classical physics for the hydrogen atom. The solution shows that, for a century, physicists have wrongly believed (as commonly stated in physics texts) that the classical hydrogen atom has been proved unstable. The same paper also explained how the retarded harmonic oscillator has an infinite discrete spectrum---long thought to be a characteristic feature of quantum mechanics. Are you challenging the theorem or the solution?

Of course, if you have suddenly changed your opinion as regards the value of retarded differential equations for quantum mechanics, and now propose to criticise my theory, you are welcome to do so in any public academic forum, and I will respond suitably.

"Casual conversation", whether or not cast in stone, is one thing, and it is altogether another thing for an annotated account of it to be published in a very visible public space with your prior approval despite full knowledge of the past work suppressed by the annotations. To reiterate, it is the latter point which is currently under discussion.

Thank you for enquiring about my son. He is enjoying Harvard which is famed for upholding the highest institutional traditions.

Sincerely,

C. K. Raju

Annex 9

(Newspaper report of an earlier case to show how the same criterion was applied in that case to prove transmission. (A key concerned person in that case has now apologized.) Source: *Hindustan Times, Bhopal Live*, 8 Nov 2004, front page headline, and subsequent UNI despatch.)

Prof Raju's charge of plagiarism found correct

UK varsity warns lecturer

Shams Ur Rehman Alavi
Bhopal, November 7

THE SCIENTIFIC fraternity in the United Kingdom (UK) has been taken by a storm after the charges of plagiarism levelled against a faculty member of the prestigious University of Exeter by the renowned City-based mathematician Professor C K Raju were found to be correct.

The university has issued a warning to the defaulter faculty member Dennis Almeida who allegedly plagiarised the path-breaking works of Prof Raju in the field of mathematics and published them in his name.

A mathematician and physicist, Dr C K Raju is well known for his research in which he has claimed that European mathematicians did not invent calculus, which was perhaps transferred from India to the West. He was staggered to find his unpublished work plagiarised by the UK lecturer early this year.

Soon after the 'discovery', Dr Raju, who is Head of Centre for Computer Science with the Makhanlal Chaturvedi National University of Journalism, and the university's the

Prof C K Raju

then DG Sharad Chandra Behar took up the issue with the Vice-Chancellor of the University of Exeter in March this year.

Recently, the VC of the University of Exeter, Professor Steve Smith, wrote a letter to Behar that a investigation was undertaken after the complaint of Dr Raju and after the enquiry was conducted, a written warning has been issued to Almeida that he must adhere to conventionally accepted tenets of authorship/acknowledgement in publications and any failure in future would be referred to a tribunal constituted under the university's statutes which could lead to his dismissal from the university.

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UK varsity warns lecturer

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Dr Raju had charged Almeida, Lecturer in Mathematics Education at the School of Education in Exeter, Dr Zadorzhnyy, Department of Classics, University of Liverpool and another person for plagiarism of his innovative research.

According to Dr Raju, he had placed an advertisement for Research Associate on his project, Madhava and the origin of calculus on the internet in 1998, following which, Almeida had contacted him showing his interest in history of mathematics.

Almeida came to India to meet Raju and after his return to England raised some funding from the university for the project. In process, he reportedly gained access to Raju's unpublished works by saying that this was needed to raise more funds.

The association between the two continued until mid-2000.

However, Almeida then allegedly co-opted Dr George Joseph, Reader in Economics Department of the University of Manchester, on the grounds that his British citizenship will help in raising more funding. The terms subsequently became unacceptable for Dr Raju because it appeared to him that Almeida wanted to take over Dr Raju's ideas but giving him only a side role.

The association thus ended but to Raju's astonishment, he found that his thesis was presented by Dr Joseph in a conference in December 2000 and then Almeida, John and Zadorzhnyy wrote a paper, "Keralese mathematics: its possible transmission to Europe and consequential educational implications" in a journal in 2001.

Dr C K Raju, who played a leading role in the development of India's first supercomputer, Param, is an author of several books. He has questioned scientists like Einstein and Isaac Newton and challenged the long-existing beliefs that calculus was invented in Europe.

He has written several books including the famous, 'The Eleven Pictures of Time'.

The eminent Bhopal mathematician was astounded when he found that his unpublished work was freely used without credit to him and it appeared on a website recently. The complaint to the VC, University of Exeter was made and now the reply from the university and the findings of enquiry have at last proved the Indian mathematician correct.

MP-based mathematician's work plagiarised abroad

By Pawanpreet Singh Saluja

Bhopal, Nov 23 (UNI)

A member of the UK-based University of Exeter's School of Education and Lifelong Learning has been found guilty of plagiarising a Madhya Pradesh-based-mathemetician's work pertaining to the manner in which the concept of differential calculus spread to Europe in the 16th century.

In a recent letter to former Makhanlal Chaturvedi National University of Journalism Director-General Sharad Chandra Behar - a copy of the missive was obtained by UNI - University of Exeter Vice-Chancellor Steve Smith said that, subject to findings of an investigation initiated following Prof C K Raju's complaint, Mr Dennis Almeida has been asked to adhere to "conventionally-accepted tenets of authorship/acknowledgements in publications" and warned that "any failure in future to adhere to this principle... could lead to his being dismissed."

"A warning isn't enough. But that's the action the university has taken. I would use the incident to explain how this (transmission of knowledge such as calculus) happened in the past and how it continues today," Prof Raju quipped during an interview to UNI. According to the city-based mathematician, Mr Almeida got access to his work as the result of an acquaintance that developed after the latter contacted Prof Raju following a website advertisement placed by him for seeking a research associate for his Indian National Science Academy-funded project 'Madhava and the Origin of Calculus'.

"Mr Almeida expressed a desire for joint work and promised to raise the required funding. Although he couldn't contribute much academically, I accepted the

proposal," Prof Raju explained.

He alleged that Mr Almeida used various pretexts to obtain access to his unpublished work in a bid to raise finances and even brought in Dr George Joseph - a reader in the University of Manchester's Economics department - saying his British citizenship would prove helpful in raising the amount.

"However, unacceptable terms were proposed later and it appeared that I was being sidelined. So I preferred to opt out," Prof Raju said.

"Later, Mr Almeida collaborated with my former research associate J John, who had been asked to resign from my project with INSA on moral grounds and was in possession of some of my project material," he alleged.

The mathematician claimed that Mr Almeida, Mr John and Mr Zadorzhnyy of the Department of Classics, University of Liverpool, copied his ideas while coming up with a paper called 'Keralese Mathematics: Its Possible Transmission to Europe and the Consequential Educational Implications', which was printed in an "obscure" publication called 'Journal of Natural Geometry' brought out by 'Jnan Bhavan'.

But Prof Raju claimed that Mr Almeida and his co-authors, who also had access to some of the mathematician's work, fell into the "boobytrap" laid by him in order to protect originality.

"They copied certain mistakes too and that depicts their lack of understanding of elementary concepts," he added.