Acceptance speech for the TGA Gold Medal Award, 2010

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Dignitaries on the dais,
fellow Laureates,
friends,

I am indeed honoured to be here today to receive this award in this august assembly in this historic city and cultural capital of Europe.

Bernardino Telesio and Galileo Galilei are both symbols of resistance to authority. Therefore, it is apt that a key reason why the award is being given to me is for having pointed out Einstein's mistake, and for having corrected it—for Einstein is one of the greatest figures of scientific authority today.

At the outset I would like to state that the issue is not so much the special theory of relativity, which is a very fine theory, even though it is counter to Newtonian intuition. There is no doubt at all that the theory was the work of a genius. The question is who was that genius: Poincaré or Einstein? The second question follows naturally from the first: compared to Poincaré, a mathematician, did Einstein, a non-mathematician, even *understand* the full mathematical implications of the theory of relativity?

The third question brings us back to the large mass of people who blindly follow scientific authority: following in the footsteps of Einstein, have *they* fully understood the special theory of relativity? If not, how should its understanding be corrected today? And what possible practical value does that correction hold for us tomorrow?

Unfortunately, instead of approaching these questions in the spirit of scientific enquiry, people react to them emotionally. Einstein is, for them, the biggest symbol of scientific authority, and they want to somehow hang on to the story they have heard about him from childhood. The less they know about the theory of relativity and its history, the stronger their belief, and the greater their distress that this symbol of scientific authority is being attacked. The issues could be easily settled in many ways: for example, the historical issue could be settled by reading the papers of Poincaré, Lorentz, and Einstein.

Somehow, most people cannot or will not read those papers, and instead proceed in a roundabout way, by reliance on authority, and through dubious guesswork. They guess that scientific authority cannot make such a mistake, exactly as people in Galileo's time guessed that religious authority was infallible. They start questioning the motives of the critic, and so on.

Physics texts play their own role in propagating such myths. Most physics texts (fortunately, not all) maintain that the Michelson-Morley experiment proved the absence of ether. The simple fact, which anyone can check (but most do not) is that the Michelson-Morley experiment was performed to discriminate between two ether theories: those of Fresnel and Stokes. The experiment came out in support of Stokes theory, which involved a mathematical absurdity, and was hence rejected by Lorentz. The whole myth of the Michelson-Morley experiment obscures the key point of relativity, which is that Newtonian physics never defined a proper clock; *therefore* it was impossible for the experiment to have measured the speed of light! Why Newtonian physics never defined a proper clock is another story, and I won't go into that here.

If we follow Poincaré's line of thought from 1898 to 1904, this point about the need to define a physical measure of time comes out with great clarity. Authoritative sources would tell us that Poincaré believed in ether or that he "waffled". However, those are plain falsehoods, as anyone can check by reading Poincaré, or even reading just the extensive quotes from him that I have provided in my books. It was Poincaré who coined the phrases "principle of relativity", and "Lorentz transform". In his celebrated 1904 paper he spoke of

> an entirely new mechanics, which would be, above all, characterized by this fact, that *no velocity could surpass that of light, any more than any temperature can fall below absolute zero.*

That is the theory of relativity in a nutshell.

Could Einstein have arrived independently at the theory of relativity? Such claims of "independent rediscovery", just when a dependent discovery was possible, are a scandalous part of current history of science. However, let us look at Einstein's case on its individual merits. It is well known that Einstein had read Poincaré's work on relativity from 1898 until 1902 with great excitement, and had discussed it with his friends. The only question is whether he read Lorentz's 1904 paper and Poincaré's 1904 paper. He denied reading those. However, as Whittaker first pointed out, Poincaré used the word "relativity" for the first time in his 1904 paper (he had earlier used the term "principle of relative motion"). Since Einstein's paper contained no new idea or formula, and repeated that word, Whittaker concluded that Einstein had borrowed his ideas. I further pointed out that Einstein casually used the strange terms "longitudinal mass" and "transverse mass" introduced very circumspectly by Lorentz in the very paper Einstein later denied reading. Whittaker's arguments, and mine, have been met with great hostility by those in scientific authority, though no one so far could address the points raised.

Cases where one student copies from another, but denies it, are commonplace for a teacher. The simple way to resolve such cases is to test the understanding of the students verbally. The one who does not understand has copied. One cannot thus interrogate the past, but mistakes are proof of lack of understanding. If a person claiming "independent rediscovery" shows lack of understanding through a mistake, that is proof of copying according to my "epistemic test". That is exactly what happened in this case: Einstein failed to understand what Poincaré, the mathematician, understood: namely, that relativity changes also the character of the equations of physics. They can no longer be the ordinary differential equations of Newtonian physics, but must be functional differential equations (which, Poincaré took for granted, must be retarded). Einstein never understood this aspect of relativity till his death. That settles the matter: Einstein published later, his claims of "independent rediscovery" are seriously suspect, and he never fully understood the implications of relativity. Possibly as a patent clerk he realized that he could copy ideas from frontline thinkers, for there is no legal patent on ideas. For almost a century now, it would seem, people have worshipped a false god of science.

There is a saying that people who do not learn from history are condemned to repeat it. In 1994, I pointed out, in my book *Time: Towards a Consistent Theory* (Kluwer), that the use of functional differential equations led to a shift away from the Newtonian paradigm of ordinary differential equations, going beyond textbook relativity. For example, the century old contradiction between Newtonian mechanics and the entropy law of thermodynamics could be easily resolved with functional differential equations. In 2004, exactly a century after Poincaré's seminal paper on relativity, I published the first solutions of the functional differential equations of retarded electrodynamics, in a significant physical context—that of the classical hydrogen atom. And, in 2005, exactly a century after Einstein's paper on relativity, and in a lecture intended to commemorate that event, Sir Michael

Atiyah, a person regarded as the leading mathematician in the world, repeated my claim, first made in my 1994 book, that the use of functional differential equations could also explain the puzzling features of quantum mechanics. Atiyah claimed "independent rediscovery", and even after he was personally informed of my work, the *Notices of the American Mathematical Society* ran a prominent article on his lecture, in June 2006, crediting Atiyah with the suggestion to use functional differential equations in physics, and referring to it as "Atiyah's hypothesis". My earlier work was credited only after a long correspondence, in a short and difficult-to-spot letter in the *Notices of the AMS* in April 2007.

I pointed out that such a belated acknowledgment, without an apology, was worth little. I again applied my "epistemic test" and pointed out that "Atiyah's hypothesis" involved a serious mistake. Functional differential equations are a natural consequence of relativity, their use requires no hypothesis, so the claim about "Atiyah's hypothesis" involved a conceptual mistake, apart from a historical mistake in crediting Atiyah. I wrote a letter to the journal, along these lines. The journal however refused to publish it, preferring to leave the mistake uncorrected. Although many prominent scientists from India and abroad signed a petition that the letter should be published, and the matter debated publicly, the Editor of the *Notices* and the American Mathematical Society ignored the petition and hung on to the decision to suppress the matter. This is how scientific authority functions at the highest level. One can well imagine how it functions at lower levels, and how much it misleads us about the truth. Those who place their trust in it deserve what they get: they and their progeny can continue to believe science is all about implicitly trusting those in positions of scientific authority. As for me, I am not in the business of mobilising popular opinion, or winning a popularity contest: my aim was to find the truth, and I have found it—the truth both about science, and about scientific authority. Knowledge was what I sought, and I have found it. That is reward in itself.

On the pleasant side, there are a number of interesting possibilities that can be explored with the new technique of functional differential equations. As I argued in my 1994 book, if we make absolutely no hypothesis, and drop even the traditional hypothesis of causality, then the functional differential equations of physics must be of mixed-type, and not retarded, as Poincaré had thought. This leads to a number of interesting consequences, for quantum mechanics on the one hand, and for biological organisms on the other. The qualitative consequences are already startling, for this physics is *non-mechanistic*, and leads to a structure of time, as I have explained in my books and papers. The further quantitative consequences I hope to explore in future. Apart from these fundamental areas, there are many other practical areas to which functional differential equations could apply—areas ranging from quantum computers, biological macromolecules, controlled fusion, the galaxy, and even the stock market. Such applications would be a fitting answer to those who worship scientific authority.

I thank the Academy once again for the honour it has conferred on me, and hope that it will succeed in its mission to promote reliance on open debate, rather than trust in authority, as more appropriate to science.

Thank you!