than 10 years are also unrealistic in view of the time it takes to turn a scientific discovery into a prototype of a new invention.

Note that the definition says nothing about who makes the judgment. Indeed, the judgment will depend on the judge, in accordance with the uncertainities and the speculative nature of science policy decisions. Yet, this conception of the dichotomy provides a useful tool both for scientists and for science managers.

Reference

1. M. J. Moravcsik, *How to Grow Science*, Universe Books, New York (1980).

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More on Eddington

A Daniel come to judgment!

Paul Nawrocki is right on in his letter about Arthur Stanley Eddington in your March issue (page 81).

I presume that by regarding it as of less interest than the size of posters and relegating it to the very end of the Letters column, you were moved by the knee-jerk reaction which has characterized most of the physics establishment since 1932 in its attitude to Eddington—an attitude governed by implicit and unexamined presuppositions. For decades, physicists have tried to convince themselves that they have no metaphysical prejudices. In fact, of course, all of us do and they determine our approach to physics.

For any student of the history, philosophy or sociology of science, the story of the reaction of the physics community to the work of Eddington during the last two decades of his life is a rich mine which no one to my knowledge has begun to exploit.

As Nawrocki points out correctly, many ideas now touted as daring discoveries of contemporary physicists were announced 10 or 20 years before anyone else by Eddington. For example, a basic concept to which Nawrocki does not refer is that of "quasi-particle," which is often attributed to Landau. The idea is nothing other than the "top particle" of *Fundamental Theory* (FT) and that is simply a new name for the "added particle" of *Relativity Theory of Proton and Electron* (RTPE) of 1936. Eddington's discussion of the concept of particle on pages 31–32 of FT should be read carefully by every aspiring physicist.

I had the privilege of writing my thesis on RTPE during the years 1940–43 under the supervision of Leopold Infeld. Though his preoccupation with war research on electromagnetic theory left him little time, he kindly allowed me to lecture to him weekly on what, if anything, I understood of the thought of Eddington. He frequently expressed bafflement but encouraged me to go on.

For the record, here are a few Eddington stories.

One day, after returning from a conference in Washington, Infeld told me that at the meeting Gamow had whispered to him "Leopold, come into my office." They entered, Gamow closed and locked the door behind them and, in a conspiratorial voice, continued, "Look, I have received two free copies of Eddington's book to review. I will give you one. We can read them secretly and discuss them, but we must let no one know that we take it seriously. We would be considered insane."

A friend of mine studied under Oppenheimer in California and sought advice as to a good book to study relativity. The great man responded enthusiastically. "Why Eddington, of course, there is nothing better!" My friend returned after consulting the library catalogue. "Did you mean RTPE or the *Mathematical Theory of Relativity*?" Oppenheimer almost had apoplexy and spat out in scathing tones, "The latter of course. The other is garbage, absolute bilge." My friend slunk away, wondering how the man who wrote the perfect book on relativity (and also, incidentally, almost single-handedly created the science of astrophysics) had managed to write a whole volume of "bilge."

Ten or twelve years later, my friend was at the above-mentioned conference with Infeld and Gamow and listened to a talk by Oppenheimer on the remarkable properties of the fine-structure constant. Since Oppenheimer said essentially nothing that had not been in Eddington's papers of 1930–35, my friend asked Oppenheimer if he now had more sympathy for Eddington's ideas. Nuclear explosion! "No, of course not. They are absolute nonsense. Go and speak to X. He studied under Eddington and will tell you there is nothing in his theories."

In fact, X modestly disclaimed having fully penetrated the thought of Eddington but felt sure it was quite important.

There is much evidence that Eddington possessed a highly developed physical intuition which led him to zero in on the key points for understanding an extraordinary range of physical phenomena. A well-known astronomer told me that, not infrequently, he had witnessed a lecture by Eddington after which some bright young man had been able to demonstrate that there was an egregious logical or mathematical error in Eddington's argument. In each case, however, when the observational data were fully in, Eddington's conclusion proved to be essentially correct! Personally, I obtained a PhD by showing that RTPE contained a major and several minor errors. However, the more errors I found the greater conviction I developed that Eddington was basically correct and that he was one of the truly great geniuses of 20th century physics.

As A. V. Douglas has revealed in her sensitive and penetrating biography of Eddington, intellectually he was a loner and temperamentally opposite to the self-assured dogmatic masters who have created "schools" of physics which dominated the development of our science.

In recent years, applied physics (to use what Lewis Branscomb considers a no-no term) has achieved extraordinary successes. However, fundamental physics has been essentially ptolemaic. The voices which seem to me to have addressed basic issues in a serious manner were those of Alfred North Whitehead and Eddington. When an informed history of our era is finally written, Eddington may well emerge as the most prolific and creative genius in the physics of the 20th century.

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Delbruck scattering

Gunther Stent in his informative obituary of Max Delbrück (June, page 71) wrote about Delbrück scattering: "... in the 1950s Hans Bethe eventually demonstrated the existence of the phenomenon ..." In the name of historical and scientific accuracy I must take issue with this statement.

I am sure that Stent will agree with me that the existence of a natural phenomenon cannot be *demonstrated* by a theorist, no matter how outstanding that theorist might be. It can only be predicted, and that was first done correctly by Max Delbrück on the basis of quantum electrodynamics.

Now I happen to have worked on the theory of Delbrück scattering in the early 1950s with Hans Bethe and others at Cornell. We did not demonstrate its existence but we computed scattering amplitudes. In the elastic scattering of gamma rays by heavy atoms Delbrück scattering occurs coherently with nuclear Thomson and atomic Rayleigh scattering. Rather accurate computations are therefore needed for the analysis of such an experiment.

Our work stimulated R. R. Wilson who was also at Cornell at that time, to measure the effect.[1] His was the first in a long line of experiments which continue to this day. One might thus