letters

this field. It is more misleading to list a few individuals in this context, without adequate differentiation, than it was to use only Giacconi's name in a single sentence on the subject. The only other person who merits some consideration for the title of inventor was Hans Wolter, who was interested primarily in microscopes and did not envision the application of his work to astronomy. It is perhaps arguable whether Wolter's theoretical work, divorced from this application, or Giacconi's creation was the more important step for telescopes. Historically, we have always considered the optical microscope and telescope to be different inventions, and, partly because we are not certain whom to credit, we often associate them primarily with their early successful users, such as Leeuwenhoeck and Galileo. Perhaps in this case we should give each person credit in his primary field of interest, recognizing Giacconi for the x-ray telescope and calling Wolter the inventor of the (aplantic) x-ray microscope, which today is becoming an important scientific instrument almost thirty years after Wolter's original work.

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Basic vs. applied

Even though some of us think that the dichotomy of "basic" and "applied" does not describe very well the research we do, and even though numerous attempts have been made in the past to suggest alternative terminologies, the dichotomy appears to be too deeply ingrained in science policy people and in the public the world over. The best may be, therefore, to see to it that these terms are used as appropriately as possible. It is in this vein that I want complement Lewis Branscomb's remarks (March, page 9).

The terms of "basic" and "applied" research may arise also in historical studies of science when we have benefit of hindsight to assess the impact of a piece of research. In the overwhelming fraction of the cases, however, these terms are used in connection with present or future research, that is, in the context of the management and performance of contemporary science. My comments, therefore, on which I recently elaborated elsewhere,[1] will try to be useful in that framework.

The comments can be summarized as follows: Although the dichotomy of "basic" and "applied" contains many ambiguities and carries a pluralistic meaning, once we realize these it is possible to use these terms in a functional and operational way.

Since the results of contemporary or future research cannot be assessed yet, we must, as Branscomb implicitly suggests, rely on the motivations of the researcher and of the supporter of research to be able to make a classification. In particular, one might say that research is "basic" if the motivation is to extend our general body of knowledge about nature and "applied" if the motivation is to provide a basis for applications external to science itself. The modifier "external" is important; particle physics has uses in cosmology but that *per se* does not make particle physics an applied science.

The above definition has many ambiguities and implies a multiplicity of meanings. Whether a piece of research is "basic" or "applied" will depend on the time scale we focus on. Every piece of research which is judged good by criteria internal to science will eventually, directly or indirectly, have an impact on some application outside science, but the time interval between research and application might vary drastically from case to case.

The classification will also depend on whether we consider the researcher or the supporter of research. In many of the best industrial laboratories research will be viewed as "basic" by some of the researching scientists while the motivation of the management in sponsoring that research may be "applied."

Furthermore, even in the eyes of a given person or organization, the motivation may be multidimensional, mixing "basic" motivations with hopes for "applied" byproducts.

That "basic" and "applied" motivations can be present side by side is, much of the time, an asset. In a heterogeneous society, the more different justifications exist for a certain activity, the firmer the societal support will be for it.

It might appear that in light of these complexities of the dichotomy of "basic" and "applied," it is useless in a practical sense. I do not believe this to be the case. Particularly to strike some kind of a balance among the various motivations society has for supporting science, it is still useful to use the dichotomy provided its usage is appropriately circumscribed. In particular, we might call a piece of proposed research "applied" if there is a good chance that it will lead, in a specified area of application, to foreseeable use in the next 10–15 years. The time interval in this definition is not unique, and somebody may want to replace it, for example, by "25 years," though such long intervals may be unrealistic in the view of commercial expectations and also from the point of view of predictability. Intervals less 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