So let us take the step to redeem ourselves of the guilt. Let us prove to the world that we are not a bunch of spineless impotents. We must show the world whose side we are on.

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Birth of synchrotron

In his article on the birth of the synchrotron (February, page 31), Edwin McMillan mentions his letter to the editor of the *Physical Review*, in which he said in reference to his getting the idea for the synchrotron, "It seems to be another case of independent occurrence of an idea in several parts of the world, when the time is ripe for the idea."

He probably should have added to that statement, "and you are lucky enough to be in a place where people will listen to new ideas."

The first time I heard this idea—in almost the same words McMillan wrote to Lawrence—was from Robert Moon at the University of Chicago. It was in 1939 at the seminar where Sam Allison first spoke about the discovery of nuclear fission and the possibilities of a nuclear bomb; perhaps it is because of the juxtaposition of the two events that I so vividly recall it. Moon said to me, "People say that there is a relativistic limit to the power of a cyclotron, due to defocusing with the relativistic increase in mass. But I think it would be easy to overcome this by just frequency modulating the Ds to keep up with the particle mass."

Nothing happened to Moon's idea at the time, just as he was unable to get the co-ax line used on the cyclotron he had designed and built in 1936: The head of the project said it would have to be built like that at the University of California, since Lawrence was the expert! So it was Dunning who built (and received credit for) the considerably improved cyclotron with the far more efficient co-ax tuned circuit.

Moon is still active at the University of Chicago, where he now has been for over fifty years. And I have been unhappy about his not getting the credit he should have received, for over thirty years!

I have checked my recollection with Moon, who confirms my memory precisely. We seem neither of us to have lost all our memory, despite our advancing years!

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THE AUTHOR COMMENTS: The condition for resonance in a relativistic cyclotron demands a certain relation between the magnetic field strength, the frequency of rotation and the particle energy; to maintain this relation as the energy increases during the course of the acceleration, one (or both) of the other quantities could be made to change with time according to a properly designed schedule. This fact has been known to accelerator designers ever since Bethe and Rose pointed out the existence of the relativistic limit in 1937.

Franklin Offner in the foregoing letter tells of one case of the recognition of this fact, by Robert Moon at Chicago in 1939. He does not say whether Moon accompanied his suggestion with the idea of phase stability; if not, it would understandably have been considered impractical because of the high degree of precision that would seem to be required to maintain resonance over large numbers of turns of the particles. I suspect that many people made the same suggestion but never carried it farther because of just such practical considerations.

The existence of phase stability alters the situation. With the particles locked into step with the accelerating frequency, the requirement for precision is enormously relaxed, and the allowed number of turns of the particle can be very large indeed, allowing the attainment of very high energies. It is the recognition of this behavior and its consequences that furnishes the crucial step in the invention of the synchrotron, not just the idea of modulating with time the frequency or the magnetic field strength.

My letter of 4 July 1945 to Ernest Lawrence, mentioned by Offner, and the attached description of the synchrotron principle, are partially quoted in my historical article in the February issue. The remainder of the "brief description" is devoted to a discussion of phase stability, telling how it arises and what it does. This is the key part of the communication, the part that convinced people that the synchrotron actually would work, the part without which the letter would not have been written at all. I did not quote from this part in the historical article because of my feeling that the general reader just wants to know that stability exists, not the nuts and bolts of how it works, while the expert will know these things already.

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Earthies, airies square off

In "A theorist's philosophy of science" (March, page 24), Helier J. Robinson does the community of science a great disservice by restirring the fetid broth of "experimentalist vs. theorist," concluding with the ill-conceived notion (copped from Feyerabend?) that somehow all novelty in science is preceded by theory, with the fairly explicit implication that theoretical work represents a higher level of human achievement than that of the "empiricists." The article is such a hodgepodge of ill-founded assertion in almost every sentence that space would be wasted in a detailed reply; instead, a general statement seems more in order.

Robinson thinks that physical, and certainly mathematical, theory can be produced independently of perceived observation, citing such examples as "we never perceive molecules, force fields, mass, kinetic energy, or any other theoretical entity." This idea rests on a gross misunderstanding of how scientists work, and how their work differs from that of non-scientists. Everyone, from infant to theorists like Robinson, abstracts from what is perceived to form models. Clearly, we have no "real" tree in our heads to compare with what we see, but merely a pattern-recognition algorithm that lets us say "Yes, that *is* a tree, and a pine at that." The accuracy of our models is constantly being reviewed, and refined. Science has gone beyond this simple stage by becoming more and more quantitative in its description, or at least communicating with an agreed-on set of descriptors, so that we can compare *my* idea of a molecule with *your* model, and perhaps by so doing refine the generalized notion of a model molecule. Of course nobody directly perceives a molecule, but we see a world that seems to be rather well "explained" by the model of molecules we have constructed. Often, the agreed-on language used by scientists in describing models is itself rather abstract, that is to say, mathematical, and appears in some cases to be unrelated to "reality," somehow a pure creation of the "mind."

I would challenge Robinson to produce a definite example of some theory that has no roots in perception. My contention is that all theory originates from attempts to refine models, just as all experimentation does. If sometimes the connection to perception is less clear, because predictions cannot be yet put to experimental test, this seems to me to be the consequence of tautological manipulation of elements of an "improved" model, which, however, if carefully analyzed, will be found to relate back to observation. The work of Einstein in developing special relativity theory would have been a futile exercise in tautology had there been no Michelson–Morley experiment to propel the "improvement" of a model. By the same token, research in mathematics represents an even further extension of tautological manipulation.

Before Robinson has retreated so far into his theoretical head as to be unable to perceive anything, I suggest he widen his philosophy shelf with a few books. The sometimes startling ideas of Alfred Korzybski in the original text, *Science and Sanity*, may give Robinson a theoretical headache, so I recommend seeing him through other eyes, notably those of Anatol Rapoport in *Mathematical Models in the Social and Behavioral Sciences* and also *Operational Philosophy*.

In light of the tone of Robinson's text, a rebuttal to this letter might run along the lines "Oh, yes, an expected response. What would you expect from somebody who reads Korzybski? Another knee-jerk empiricist who believes that all knowledge comes from perception." Well, with a sharp snap of the knee, I reply, "You're right, Robinson. But I think it's up to *you* to prove otherwise, without a lot of misleading claptrap about 'theoretical reality' and 'theoretical heads.'" To show where one's "theoretical head" might lead, I refer readers to a classic paper, "Relations between fundamental physical constants," by J. E. Mills, in the esteemed *J. Phys. Chem.* **36**, 1089–1107 (1932). I would like to nominate Robinson's contribution to membership in this distinguished company.

EMORY MENEFEE
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"A second point about the prediction of novelty is the curious fact that only mathematical theories are capable of it." Helier J. Robinson elevates this proposition to the status of "the most important problem in all the philosophy of science." (As one example he instances Maxwell's equations leading to Hertz's discovery of radio.)

Molecular models are based on a body of knowledge known as stereochemistry, but as yet only the model of a molecule not much more complicated than H_2O could be said to be in some sense the embodiment or illustration of a mathematical theory. Crick and Watson built a model of DNA compatible with the stereochemistry of its components. The mathematical theory of x-ray diffraction was used in ensuring that the model was compatible with other empirical evidence known to them, but the power to predict novelty lay in their theory of the structure which the model represented. Would it be fair to say that genetic engineering owes as much to Crick and Watson as electrical engineering owes to Maxwell? Crick and Watson could be described as empiricists, but their theory emphatically did not lead only to "predictions of repetition."

While it may not be too relevant, it is a fact that Maxwell invented a mechanical model of the electromagnetic field that has sometimes been described as the scaffolding he used to erect his theory. Peter Bono might find inspiration there!

WILLIAM COCHRAN
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A colleague urged me to read Helier J. Robinson's incredible article "A theorist's philosophy of science." To make his argument seem reasonable, Robinson creates a fictional view of empirical science: "The empiricists believe that what we perceive around us is reality."

Any rudimentary investigation of perceptual thresholds shatters such a belief. If, as the data indicate, each of us perceives differently, which of us perceives "reality"? I trust few empiricists claim to have perceived reality. Perhaps that sort of claim should be