both developments strengthened each other, in particular in the reinforcement that the theoretical model calculations provided in clarifying the question of whether an incomplete phase separation was even possible in principle.[2]

These remarks are made to complement those in PHYSICS TODAY and not to take anything away from the quality of the work for which Edwards was rightly honored.

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FREDERICK SEITZ
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5/84

Earthies, airies: round two

As a devoted practitioner of "airy" science, I want to congratulate Helier J. Robinson for his fine article (March, page 24). Although I agree with most of his conclusions, there is one point I would like to dispute, at least partially. It concerns Robinson's statement that "Theoretical science is invented, and this requires creativity, for which there is no known method." I would suggest that an airy does have a *modus operandi* that has four identifiable stages: identification, digestion, recognition, formulation and solution.

The first stage is the "identification" of the problem. Problems, especially important ones, are not as a rule easily identifiable. For example, many airies who were Einstein's contemporaries at the turn of the century did not recognize the inadequacy of Newtonian mechanics. Even after the negative results of the Michelson–Morley experiment, some airies did not recognize the problem. But Einstein did, reputedly before the Michelson–Morley experiment.

After identifying the problem, there follows a period of "digestion." During this period, which may be of the order of minutes or decades, an airy mulls over (digests) the problem. The problem is similar to a puzzle with missing pieces and pieces that don't fit. When the problem fades from the airy's consciousness it recedes to the unconscious mind with the latter playing an important role in solving the puzzle. This sentiment has been expressed by Nobel laureate William Lipscomb who has been quoted as saying: "The unconscious mind pieces together random impressions into a continuous story. If I really want to work on a problem, I do a good deal of work at night—because then I worry about it as I go to sleep." This digestive period can be very frustrating because many times an airy may think he has solved the puzzle only to be rebuked. In this regard I am reminded of a remark by Robert S. Mulliken: "The man who woos nature for her secrets must develop enormous tolerance in seeking for ideas which may please nature, and enormous patience, self-restraint, and humility when his ideas over and over again are rejected by nature before he arrives at one to please her."

Most of the time, but not always, a solution to the problem begins with the "recognition" that the problem is "similar" to (or can be reduced to) another problem whose solution is known or that the problem is "amenable" to a known mathematical method or physical model. The key phrase here is "recognition of similarity or amenability." For example, Pierre-Gilles de Gennes of the Collège de France in 1972 "recognized" that the n-vector model in the limit $n \to 0$ and in zero magnetic field was "similar" to a self-avoiding random walk on a lattice. Because such a walk is an excellent model of a polymeric chain, this "recognition" made it possible for powerful scaling and renormalization group methods (the formulation and solution stage) to be brought to bear on the problem of polymer chain dimensions. This "recognition of similarity" between two branches of science has revolutionized the field of polymer physics in the past decade.

Two examples of "recognition of amenability" come from the field of critical phenomena. Kenneth G. Wilson, stimulated by interactions with Cornell colleagues Benjamin Widom and Michael Fisher, "recognized" that critical point phenomena were "amenable" to field-theoretic methods. Wilson's familiarity with field theory and renormalization ideas in particle physics and quantum electrodynamics provided the right knowledge base for him to develop a descriptive theory of critical phenomena in 1971. Wilson and Fisher also developed the ϵ expansion in 1972 (PHYSICS TODAY, March 1972, page 17). Again, this was an example of "recognition of amenability." Critical exponents can be calculated exactly in 4 dimensions by mean field (Landau) theory. In 2 or 3 dimensions the mean field theory fails, but every good airy recognizes that if a problem can be solved exactly, its range of applicability can usually be extended by perturbative methods. Wilson's and Fisher's key to solving this problem was to treat dimensionality as a continuous vari-

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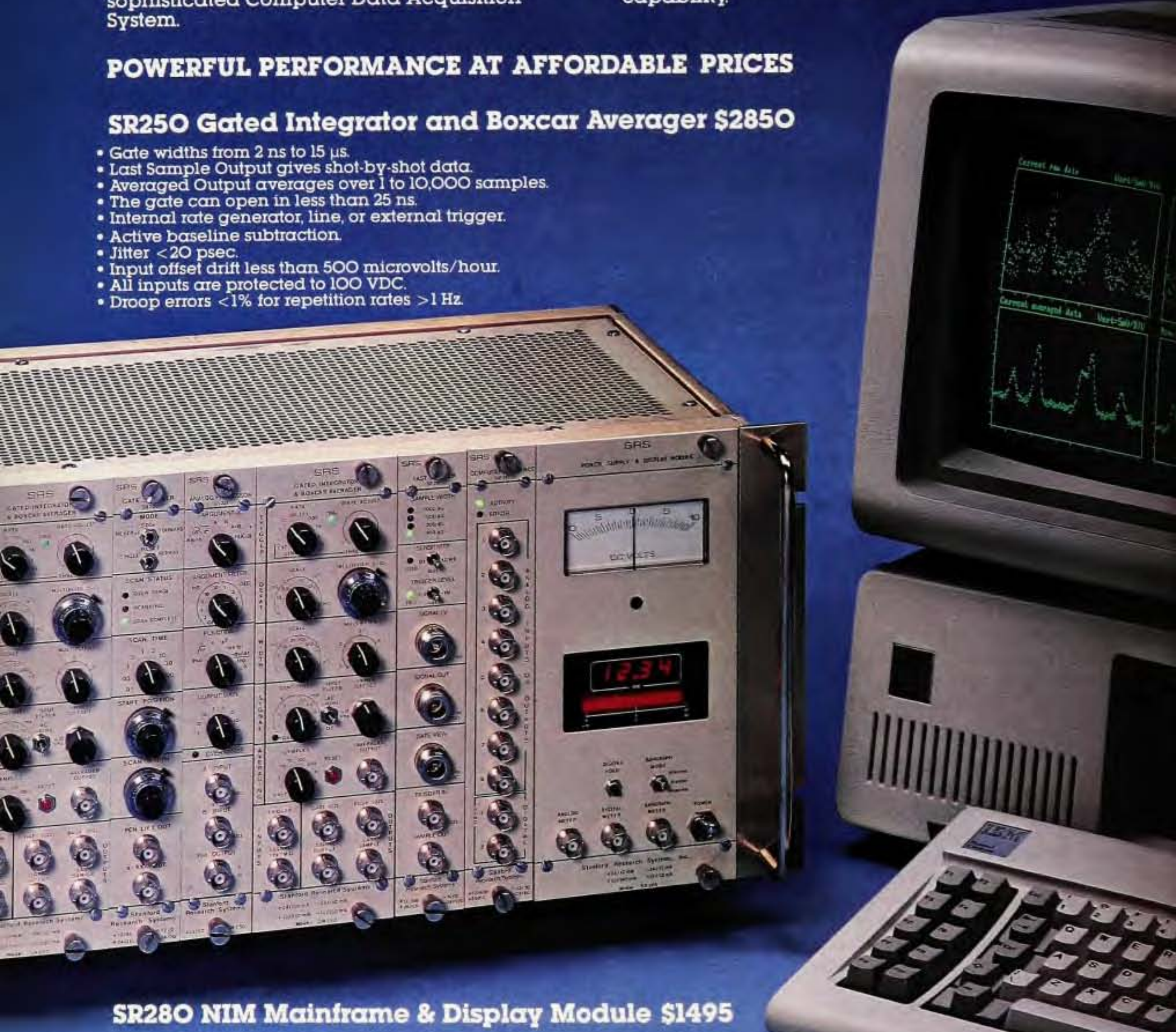

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able, $d = 4 - \epsilon$, and to perform a perturbation expansion in powers of ϵ. As we know, Wilson's exceptional abilities of "recognition" led to a major breakthrough in understanding the universality of critical phenomena and helped to earn him the physics Nobel prize in 1982.

After the recognition stage, the problem is mathematically "formulated." Mathematical formulation of the problem constitutes a formal solution of the problem. The descriptive equations may not have an apparent analytical solution or be the best possible description, but that is another problem! It is here that the process of identification, digestion and recognition can begin all over again. Even if an analytical or numerical solution to the original problem is developed, the solution might only be approximate or be otherwise flawed for any number of reasons. An airy views these imperfections as opportunities (problems). This iterative process by which the solution of a problem is advanced is well-known to practicing airies.

I would argue that what Robinson calls "creativity" is, to a large degree, what I term "recognition of similarity and amenability." The latter can be learned in the sense that the ability to recognize depends on the mathematical tools and physical models that have been learned and are at an airy's disposal. I believe this is an important point because it is a widely held notion that creativity is innate and cannot be learned. Einstein provides a fine example of the intimate relationship between creativity, recognition and learning. One of the grandest, if not the grandest, achievements of airy science is Einstein's general theory of relativity. It is significant that this grandest of Einstein's achievement was facilitated by his power of recognition and by something he learned as a student in Zurich. In 1912, after eight years of struggling with the problem of extending the special theory of relativity to a reference frame with acceleration (digestion), Einstein "recognized" that the appropriate geometry for his four-dimensional space–time world was not Euclidian, but Riemannian geometry. In discussing this breakthrough, he is purported to have said: "I happen to remember the lecture on the geometry (Riemannian) in my student years . . . I found (recognized) that the foundations of the geometry had deep physical meaning in this problem." (PHYSICS TODAY, August 1982, page 45).

If creativity can even be partially learned, it has important implications for the education of future airies.

ISAAC C. SANCHEZ
National Bureau of Standards
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4/84

I read Helier Robinson's article with interest but not complete concurrence. Where we begin to disagree is where he asserts there is a fundamental difference between theoretical and empirical perception. He assumes we are ". . . directly conscious of material objects around us . . ." when we perceive those objects. I assert we only think we are directly conscious of them because we have been perceiving them for so long, and they have behaved so consistently, that perception has become mostly just an ingrained habit. Because perception is so easy for us now, we do not recognize what a chore it once was for us to perceive anything. And most of us are not analytic enough with ourselves to realize what really goes on when we perceive.

My theory of perception says there is no fundamental difference between empirical and theoretical perception. All perception is of symbols (Robinson's "images") within us. The feeling that we are directly conscious of actual objects is just one more illusion. Before a symbol or image of an object exists within a person, he cannot truly perceive the object. If a person confronts an object for which he has no symbol, he creates a symbol for it by perceiving the symbols of things associated with the object, its properties, and synthesizing them into a whole. After this learning experience and only after it can he truly perceive the object. We are never directly conscious of material objects around us but always perceive them through a buffer of theories and symbols stored within ourselves. If there were a way of becoming directly conscious of objects, it would have to be via something akin to ESP, a mode of perception generally not recognized by scientists.

There are two kinds of reality. One kind exists in the mind only. It is the only reality we can know directly. It grows when we learn. The other kind exists outside our minds. We believe firmly that it exists, but we do not know it directly. We believe that objects in the world outside have properties that we can know through sensory stimuli. The stimuli we receive provide an opportunity for us to learn about and interact with the objects of the world outside. Through learning we can cause our internal reality, which is a set of theories, to become more like the external reality. The benefit of learning is that fewer of our experiences take us by surprise.

It is interesting and not just fortuitous that Robinson's empirically derived "images of theoretical reality" and my "reality that exists in the mind only" have a lot in common. Both contain everything that a person has perceived in his universe. Although my formulation avoids the rather bi-

continued on page 114

continued from page 15

zarre image of the inside surface of a skull with a radius of many parsecs, the underlying idea is similar. If Robinson did not have a hang-up about using "mind" and substituted it for "head," his theory would sound far less bizarre. The crucial difference between him and me is that he draws a sharp distinction between the empirical and the theoretical, while I insist a correct view of perception eliminates the distinction as a basic one and makes it meaningful only operationally, with respect to, say, a given experimental test of a specific hypothesis. . . .

Science is the deliberate attempt to confront the unexpected before it confronts us, so that we can create symbols and theories complete enough to keep our equanimity no matter what. The pursuit of scientific knowledge is our way of coming to terms with the external world in order to dominate it—that is, to control our minds in its presence.

The theoretical physicist manipulates symbols he perceives in the absence of external stimuli in order to create new worlds in his mind. He is always looking for patterns in these new worlds that are similar to patterns he believes exist in the external world. (If he does not stay alert for such similarities in patterns, we soon start calling him a pure mathematician.) If he sees patterns that are like those in the external world, he rejoices and rushes out to tell his colleagues that the external world behaves in some ways like the imaginary world he created. Maybe the similarities will prove to be superficial. If so, everyone will soon lose interest. But if the behaviors of his imaginary world and the external world seem to agree in detail, everyone looks closely at his imaginary world as he describes it to see whether it might behave in ways unlike any yet observed in the external world. If it does, physicists will get excited and say his theory "predicts something new."

At that point, experimental physicists will rush out to try to observe the predicted new phenomena. This does not in any way mean they will try to record in the external world the symbols that the theorist has dreamed up. On the contrary, they will explore how the new, predicted symbols interact with known ones—themselves also incapable of being recorded—to see how the interactions might show up in some way as a measurable effect. Measurable effects are not ". . . empirical perceptions . . ." through which the experimental physicists become ". . . directly conscious of the material objects . . ." they want to learn about. The measurable effect might be observed as a small variation in the reading of a voltmeter or an unusual track on a sheet of photographic film. The experimental physicists become aware of a specific material object only in their minds, like everyone else, and, like everyone else, they perceive it only to the extent that they have suitable theories.

What they perceive in actual experimental data is the trace of a chart recorder pen or a track on a sheet of film; they can perceive such things because they are smart and have long ago developed theories for chart recorder graphs or tracks on film. The significance of what they see is only in their minds. The mind, of course, is the only place where anything ever becomes significant.

If the experimental physicists find the phenomena predicted by the theorist, they will start saying the theorist's imaginary world has been verified, and everyone will start turning that imaginary world into the real world in his own mind. The theorist will be venerated for having in some sense created the new world that people now begin to live in.

The distinction between the imaginary and the real is only in our minds, and in choosing our theories we decide what forms reality will take for us. Our goal is to have a kind of reality we can live with comfortably, not one full of the unexpected that is always buffeting us with surprises. That is why not just any theory will do.

DON WINTERSTEIN
4/84 *Brea, California*

•

To me, who certainly is just a simple-minded "earthy," the article by Robinson is definitely nothing but a rather dispensable nuisance, and today I regret all the time that I spent trying to get some meaning out of it. But because I have taken all that trouble, I am now pressed to help other earthies get the necessary insight into what "Renascent Rationalisme" is worth.

The nuisance begins with Robinson's tactics, common to so many "airies," to start his article with a mixture of correct and quite untenable statements, then to redefine the meaning of a series of words of our earthy common-sense language to almost their contrary, then to formulate a "new theory" that has "emotionally appalling" consequences to everyone who has not yet totally rid himself of the familiar meaning of these words (old trick to attract publicity!) and finally to claim (without any real proof!) that this theory "resolves the problem of perception" and "explains science"! An audacious claim indeed!

The most important correct statements of Robinson's certainly are the following:

▶ "the great strength of common sense is that it is the cumulative practical wisdom of millenia of experience, and so, in general, highly reliable";

▶ "the weakness of the rationalist position is that they disagreed so much among themselves . . . that one cannot help but regard this disarray as evidence of a failure of method."

But next comes a very debatable statement:

▶ "Common sense is not completely reliable, as the history of ideas shows. This history is a history of corrections to common sense, and the corrections have all been made by airies."

This is simply not true! The "history of ideas" certainly took place somewhere between the oldest books of mankind, say the Old Testament and the Iliad, and today. But there we encounter perfectly the same concepts of reality and perception as in today's common sense: empirically real persons who see each other with the help of their empirically real eyes and who kill each other with the help of their empirically real hands and weapons. Clearly, there never occurred any correction whatsoever of the common-sense concepts of perception and reality through these three millenia, much less "corrections which have all been made by airies"! So, Robinson's primary argument, that "credence should always be given to both sides," earthies *and airies*, loosens all the ground under its feet.

Another important and certainly untenable statement of Robinson's is that "the weakness in the empiricist position stems from the existence of illusion." We earthies feel sure that the opposite is true: The fact that we are able to recognize empirically illusions as such, and to differentiate them from "correct" perceptions, proves to us the strength of our empiricist position! It is exactly *thanks to* the bent appearance of the half-immersed stick that we perceive that stick to be half-immersed!

The most important and irritating redefinition of word meanings appears in Robinson's definition of "theoretical reality" as "all that exists independently of being perceived." It is our common-sense convention of word meanings, based on the experience of millenia of empirical perception, that the empirically perceptible objects, including ourselves, "continue to exist, even when no one is perceiving them any longer." So, Robinson calls "theoretical reality" what common sense calls "empirical reality," while to common sense it means as much as the opposite, whether something is assumed to be "real" or only "theoretical." Can such a reversal of word meanings be helpful?

The next source of confusion lies in Robinson's insistence that there are two different types of perception, em-

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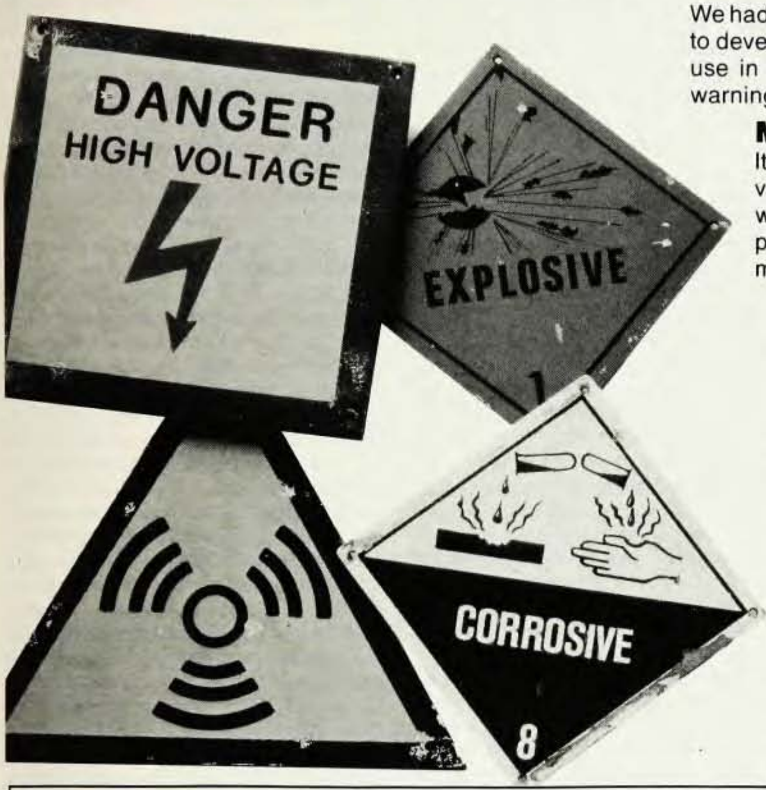

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pirical and theoretical. What he then offers, however, is simply two different descriptions of perception "as we all know it from experience," none of these descriptions being fully satisfactory to us simple-minded earthies. While we are used to describing perception with such words as "I perceive this (empirically real) pencil in my hand by feeling it between my (empirically real) fingers and by the sum of varying optical images that I obtain via my visual sense when looking at it from all sides," Robinson offers us as definition of his "empirical perception": I am directly conscious of this pencil," and as definition of his "theoretical perception": "I cannot perceive the pencil, I can only perceive the image of the pencil, which is inside my head."

To us earthies, these two formulations do not define two different kinds of perception, they are just two different descriptions of one and the same phenomenon, "perception," which differ from each other only in what is considered to be the perceiving subject. When I say "I perceive this pencil," then the subject "I" comprises the whole of my Self, body, senses and consciousness. When I say, instead, "I perceive only the image of that pencil as it is transmitted into my head via my eyes, neurons, and so on," then the subject "I" comprises nothing but my consciousness, and my senses are considered to be separate from "me."

So, to us there appears no good reason whatsoever to follow Robinson in his quite different and rather strange conclusion: If the two kinds of perception are to be combined, then that of which we are conscious must be the same in each case, meaning that the object of empirical perception must be the image of theoretical perception. However, the first is external and the second internal to our head! What has gone wrong?

Robinson's mistake here is so simple and evident that we need not follow him further in his effort to resolve his problem by administering us two different heads, an empirical and a theoretical one.

Perhaps it is more illuminating to discuss shortly what might be the central problem of all the "airy philosophers," who, like Robinson, have difficulties with our common-sense concepts of "reality." To me it appears quite evident that they all concentrate too much on the sole visual perception, and that they all ignore more or less the simple fact that our empirical perception of the three-dimensional world around us, with all the objects in it, is essentially based upon our experiences when walking around, grasping objects and hurting ourselves when we are too careless in our movements. Blind people, with no visual images at their disposal, do have perfectly the same common-sense concept of the empirical reality of walls, lampposts, and so forth, as we sighted ones. On the other hand, it is a very simple experiment for everyone to experience how quickly we lose all feeling for the three-dimensional reality around us when we restrict our perception to one-eyed immobile vision alone, by sitting motionless in our armchair, trying to avoid any eye movements and forcing us never to close the eyelids. In this position we are certainly unable to perceive the empirical reality of the three-dimensional world around us.

I do not want to be cynical, but let me conclude with the following question to Robinson: When he runs in the dark with his head into a solid wall of bricks, what has then happened? Did his theoretical self run with his empirical head against a theoretical wall, or everything the other way round? And is the bump that will develop on his forehead really only theoretically real (since it continues to exist when he sleeps and nobody is perceiving it)?

EGINHART BIEDERMANN
4/84 *Böblingen, Germany*

THE AUTHOR COMMENTS: I do not think that there is any basic disagreement between Isaac Sanchez and me. His description of the *modus operandi* of problem solving is a description of "method" in one sense of the word, while I was using another sense. My sense (which I should have made clear in the article) was the sense of a method that can be taught, as, for example, the Runge–Kutta method for numerical solution of differential equations or various methods of staining microscopic specimens. What he describes cannot be taught. I doubt that anyone can be taught to identify problems; it takes a certain kind of intelligence to do this. Digestion requires, as it were, an intellectual stomach, which is born, not made. Recognition requires a certain kind of genius also—didn't someone once define genius as the ability to think in analogies? I agree that once these three have been done, the last stage, solution, depends on mathematical tools, which are taught. But the moral of this is not that scientific creativity can be taught, but that it can be improved in those who have it by means of a good education in mathematics.

Addressing Don Winterstein's letter, I would like to make these comments. A more detailed description of empirical perception than I gave in the article would distinguish between interpreted and uninterpreted perception. Uninterpreted empirical perception is "the big buzzing, booming confusion" by which William James described the perception of a new-born child. Interpretation is a largely unconscious process of compounding data from different sense organs; correcting illusions; adding beliefs, value and judgments; recognizing; and identifying. This process of interpreting the raw data of perception results in interpreted empirical perception and is the kind of perception of which I spoke in the article; Winterstein identifies my empirical perception with uninterpreted perception and my theoretical perception with interpreted perception. This is because he uses the word "theoretical" in a somewhat different sense from me. For him, theories are all the beliefs that are added, as part of the process of interpretation, to the raw data of perception. For me, theories are scientific theories. These are a matter of belief, of course, but not all beliefs are theories; other kinds of belief are common-sensical, mythological, religious and metaphysical. I would agree with his *gestalt* view that scientific beliefs are added to sensory data in the process of interpretation, along with other beliefs. But the really important point is whether these theories also refer to any reality or not—specifically, in accordance with Leibniz–Russell, to a strictly imperceptible theoretical reality beyond the farthest horizon of the moment. For Winterstein they do not, while for me they do. As a consequence, Winterstein is trapped in the solipsistic predicament: He cannot assert the existence of anything outside his own present consciousness, except dogmatically. This is because, for him, there is nothing that could make a theory *true*. For me, a theory is more or less true insofar as it resembles the theoretically real world beyond the blue sky; for him, there is no such world. He does refer to "the external world," but it is unclear in his writing whether this is, in my terms, his empirical external world or the theoretical external world which is beyond his empirical blue sky; indeed, I suspect that he identifies these two worlds, in the manner of common-sense realism—a logically inconsistent escape from solipsism.

I began my article by pointing out that among scientists airies and earthies cooperate, but in philosophy they do not. Eginhart Biederman is, in this sense, much more of a philosopher than a scientist. Out of my attempt to present a balanced overview of earthy and airy attitudes, he selects my praise of earthies and criticism of airies as correct and my criticism of earthies and praise of airies as incorrect. Such prejudging does not merit reply, but some of his criticisms should be addressed.

Biederman denies that the history of ideas is a history of corrections to common sense and that these corrections have all been made by airies. A short catalog of onetime common-sense beliefs easily settles the matter: The Earth is flat, the Earth is at the center of the universe, the stars revolve in crystalline spheres, species are immutable, mankind was specially created, magic works, everything in the Bible is literally true, and so on.

On the question of illusion, I pointed out that we discover illusion by means of contradictions within perception, and so Biederman's claim that illusions are discovered empirically in no way disagrees with my position. The contradiction that he did fail to appreciate was the one between the belief that what we perceive around us is real simply because it is external, and the fact that some of this is illusory and therefore unreal.

On the question of identifying empirical reality and theoretical reality, Biederman's defense that this belief has been held for millenia is no answer. This belief—common-sense realism—is, I contend, another step in the history of ideas, another falsehood of common sense needing to be corrected. To appeal to common sense against this is merely question-begging. It is reminiscent of the clergy who argued that scientific discoveries did not falsify the Bible because the Bible said so. If he wants to refute me on this point, Biederman should criticize my argument. That he dislikes what he calls my "redefinitions" is not germane; they are stipulative definitions, made for the purpose of exposing error in "our common-sense convention of word meanings." This applies to my distinction between the empirical and the theoretical in defining such things a reality, perception and heads. Biederman's assertion, that in each case the empirical and theoretical are one, is merely dogmatic; in each case he has failed to follow my argument and then asserted as true that which I am arguing against.

It is incorrect to suppose that my position comes from considering perception as one-eyed immobile vision, in which there is no three-dimensionality. The Leibniz–Russell theory has no difficulty in accounting for the three-dimensionality of empirical worlds—both visual and tactile. It is not this three-dimensionality that gives empirical worlds their reality, as Biederman seems to think, although it does, of course, contribute to it.

Finally, Biederman's concluding question poses no difficulty. If I run my head into a brick wall in the dark, what happens is that my theoretical head bangs into a theoretical wall and this is reproduced in my empirical world as an image: that is, my empirical head bangs into an empirical wall. And my theoretical head then develops a bump that is imaged on my empirical head. The difference between the two bumps is that the theoretical bump continues to exist when I am asleep and not perceiving it, while the empirical bump ceases to exist while unperceived.

HELIER J. ROBINSON
University of Guelph
5/84 *Guelph, Ontario, Canada*

More on how to soar

In "The art and physics of soaring" (April, page 34), Lloyd Hunter seems to be unaware of the fact that physics reasoning provides an analytic expression for the shape of the sailplane polar curve.

This expression is

$$V_{\text{sink}} = A/V + BV^3$$

where A and B are constants.

The first term is caused by induced drag and the second by skin friction.

The induced drag is given by

$$F_I = \frac{C_L^2}{\pi A.R.}\frac{V^2}{V_0^2}$$

But since

$$C_L = \frac{V_0^2}{V^2}$$

Then

$$F_I = \frac{1}{\pi A.R.}\frac{V_0^2}{V^2}$$

The skin friction is given by

$$F_S = C_D\frac{V^2}{V_0^2}$$

I hope this may help the thoughtful reader understand what is going on.

JOHN V. KANE
4/84 *Bryn Mawr, Pennsylvania*

THE AUTHOR COMMENTS: I would like to thank John Kane for pointing out the analytical expression for the form of the polar curve. It is my experience that its use in calculating cross-country speed is unnecessarily cumbersome. The polar curve can be quite adequately approximated by a parabola over the range of speeds needed for such calculations. The results of such a calculation are good enough for effective flying.

LLOYD P. HUNTER
6/84 *Rochester, New York*

Canadian nuke tips

"By early summer the NRC expects to propose a rule . . . mandating conversion from high enrichment uranium to low . . . at university research reactors" was an item in the Bulletin of The American Physical Society **29** May/June 1984 (page 873). From the viewpoint of a reactor physicist, it makes more sense simply to change the design concept to that of the SLOWPOKE reactor. This reactor is on Canadian university campuses and elsewhere because of the following qualities:

▶ Critical mass 0.80 kg U-235 (93%) as a U–Al alloy: a dilute solution. If the US campus reactors used this fuel, they would all have to be stolen and chemically processed to make one bomb.

▶ Small, and cheap to buy ($700 000) and cheap to operate ($50 000/year), for a thermal flux of $10^{12}/20$ kW_t.

▶ Licensed in Canada for unattended operation.

Maybe it would be a good idea to close down all US university reactors and send nuclear engineers to Canada for hands-on experience. The campus reactors here cost ten times as much to buy and ten times as much to operate—money that could be better spent elsewhere: for example, on work that would led US commercial reactors approach the power cost from CANDU reactors (the Canadian commercial reactors that use D_2O and natural U), which is one half of that from US plants.

CARROLL B. MILLS
7/84 *Santa Fe, New Mexico*

Corrections

January, page S-9—In the 21st line of text in "What is the Cosmic X-Ray Background Telling Us About the Quasars Missing at High Red Shifts?," "evaluation" should read "evolution."

April, pages 35, 37—In "The art and physics of soaring" by Lloyd Hunter, there are several errors. The discussion of part b of figure 1 on page 35 distinguishes the various lines by structure. The figure, as printed, distinguishes them by color. To follow the argument, one must know that the "solid line" is green, the "dashed line" is orange and the "short dashed line" is blue. The same thing occurs in connection with figure 3 on page 37, where the "dashed curves" are now solid green lines. Also, on page 35, the slope of the polar curve is not the reciprocal of the glide ratio, as stated. Rather, the slope of a line from the origin to any point of the polar curve is the reciprocal of the L/D of the sailplane when flying at the speed representing that point.

April, pages 40, 41—Several errors appear here in the equations. In the second equation on page 40 Q should be V. In the fourth equation $D - h/S$ should be $(D - h)/S$. In the fifth equation $D - h/c$ should be $(D - h)/c$. In the first equation on page 41, $c + S_0/B$ should be $(c + S_0)/B$, and in the second equation C should be c. □