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.

Edwin M. McMillan
University of California
4/84 *Berkeley, California*

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
4/84 *Richmond, California*

•

"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
University of Edinburgh
4/84 *Edinburgh, Scotland*

•

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 left to philosophers.

Still attempting to find "reality," Robinson writes, "One thing cannot be dissimilar to what it is." This view ignores material process. It speaks volumes for errant theory.

Not content to create a fictional view of empiricism, Robinson's apple analogy (page 30) raises some question about the author's logical use of language: "Suppose we are told that the apple is in the box and also that the apple is outside the box." The author then goes on directly to: "The only way to make both of these true at once is to allow two apples or two boxes or both." Unmitigated hogwash! The context of "... *the* apple is in *the* box," indicates only *one* apple and only *one* box. On the basis of this faulty analogy, Robinson generates his solution to his problem of empirical vs. theoretical reality.

No amount of philosophical theorizing can change our perceptions. The philosophy of science cannot be rewritten to create a new "Reality" (with a capital "R"). Science in the 20th century has accepted a more "relative" understanding of perception as one process in a changing universe. To base our theoretical structures on a verbal inconsistency seems a clear step backward toward the metaphysics of 2500 years ago.

EARL HAUTALA
4/84 *Martinez, California*

•

As an "airy," I was a receptive audience for Helier J. Robinson's fascinating exposition of the Leibniz–Russell theory for reconciling rationalism and empiricism, but I think even "earthies" will appreciate it for the indispensable role played by empirical perception.

I couldn't help noticing that the startling solution to the paradox of empirical object and theoretical image seems to lead to the inescapable conclusion that everyone's "theoretical head" must be the *same one*, since the location of any individual's "theoretical head," namely outside (and surrounding) the observable universe, seems to be a unique place.

RUTH E. KASTNER
3/84 *Greenbelt, Maryland*

•

I read with great interest the paper by Helier J. Robinson about philosophy of science, but I must interject some remarks.

First, while I admire the depth of the analysis of the connections between the theoretical object, the theoretical image and the empirical object, as well as the bright solution of the problem by G. W. Leibniz and by Bertrand Russell, I wonder whether any *earthy* physicist (worth the name) has ever been doubtful about the facts Robinson presents as "consequences for science" just revealed by their theory—particularly that "the scientist must be objective," that "data should be quantitative rather than qualitative," and that "experiments should be repeatable."

Second, although Robinson reports that Karl Popper has pointed out that "empirical science is almost always a matter of testing theories," he himself, somewhat later, just drops "almost" and states that "*all* research experiment is concerned with testing theories." He thus misses the not-unusual case of the observational discovery that theorists not only do not predict, but fail to explain; such a case generally results from an experimental improvement or technical refinements, and yields the best reward the author can get for his efforts.

Third, that "only mathematical theories are capable of [prediction of novelty]" is not curious at all. It simply results from the invention of algebra:

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Just replace each physical quantity with a symbol, and connect these symbols by an equation (for example, $PV = nRT$), based upon a discrete number of empirical measurements and thereafter admitted as an established theory, which means that it holds whatever the numerical values of the various symbols; then you can play with this equation and others and predict from their combinations a new relation between some of the symbols, and finally check it by experiments. But this procedure has a limit, because we are unable to imagine, so to speak, *all* the valid combinations between equations; and unpredicted observations (see above) do correspond to not-yet-known mathematical combinations. This situation has been beautifully explained by Eugene N. Parker in his Guest Comment (September 1979) dealing with the Sun, a most striking example because of the tremendous variety of structures and phenomena it displays:

> The Sun is an obstinate reminder that while we may possess all the basic partial differential equations of classical and quantum physics, the rich variety of solutions of those equations extends far beyond present knowledge and imagination . . . . The problem is to guess from the observations what the underlying physical effects may be, and then to establish the ideas firmly with the appropriate theoretical studies and further observations.

The last approach by Parker is just the reverse of the one quoted by Robinson. . . .

Finally, I am not sure at all that it is the true Pythagorean airy claim "All is number" that has been endorsed by Galileo and other famous names. Indeed, what Pythagoras and his followers believed in was the magic of the *integers*, brought to acme with their discovery of the existence of the five—no more, no fewer—convex regular polyhedra. Plato had the same belief, and millenia later, integers reappeared with atomic and quantum physics; but macroscopic physics needs continuous variables and functions.

May I guess, anyhow, that, because Robinson clearly states that he presents a theorist's philosophy of science, he will accept experimentalists having one of their own?

JEAN RÖSCH
Observatoires du Pic-du-Midi et de Toulouse
4/84

•

In "A theorist's philosophy of science," a number of intriguing insights are discussed: the temperamental dispositions that exist among scientists, who are divided into the "earthies" and the "airies"; general dominance of the empirical as contrasted to the theoretical elements in the philosophy of science; and the special relationship that exists between some mathematical theories and reality. Because this relationship may be the most seminal discovery in the entire enterprise of science, I share the assessment of the great importance to be attached to these mathematical theories in the philosophy of science.

Even so, there are many claims in the paper that are dubious. For example, though the psychologists will have to tell us whether empirical perception is really all that independent of theoretical perception, it seems misleading at best to extend that distinction by creating an "empirical reality" and a "theoretical reality." Certainly these words can be defined to have the meaning specified in the article. But to do so raises serious questions of their relevance to science, which has traditionally sought to grasp a single reality by whatever strategies and methodologies succeed.

Second, empirical science is distinguished in the paper from theoretical science by several properties, one of which is that the former "describes the world" and the latter "explains it." While the empirical methodology of the laboratory differs from the theoretical activities carried out in an armchair, any *description* of the world, however elemental, has empirical and theoretical features inextricably intermixed. The simple statement "The Moon is round tonight" has meaning only after a constantly changing flow of sensations involving shape, color and direction has been reduced by a leap of the imagination to a single object, the Moon. Whether this reduction is a description (empirical) or an explanation (theoretical) of the world is difficult to determine. These difficulties are compounded when dealing with such entities as stars, paramecia, molecules and photons and by the use of complicated experimental apparatus in which the design and accumulated data have essential dependence on a theoretical framework.

Fortunately, these kinds of problems do not have much bearing on one of the most important conclusions of the paper, which states, "Because prediction of novelty requires a theory to be true by similarity to reality, mathematical theories must succeed because reality is mathematical." This statement stands on its own merits, independent of most of the analysis contained in the article.

WENDELL G. HOLLADAY
Vanderbilt University
Nashville, Tennessee
5/84

THE AUTHOR COMMENTS: I would like first of all to commend and sympathise with Emory Menefee's "knee-jerk." A knee-jerk reaction is reprehensible only when it is in defense of a prejudicial position, and Menefee's is in defense of the foundation of all science: its empirical origin. Menefee seems to believe that I wish to deny this, in favor of an exclusively theoretical science, but this is quite incorrect. I did say in the article that airies and earthies cooperate in science but not in philosophy, and that I wanted to correct this imbalance in philosophy of science. To do so I had to emphasize the airy side of philosophy of science, but this does not mean the rejection of the earthy side—except for the rejection of the errors of common-sense realism. So, in reply to Menefee, I state that I do not believe that any physical theory can be produced independently of empirical observation. What Menefee has to say about models is basically what I am saying about theories—we are not really in disagreement here, except in the use of the word "model." And his challenge to me to produce a definite example of some theory that has no roots in perception misses the mark, because I agree with him that if such a theory were to exist, it would certainly not be scientific.

William Cochran makes the important point that it is possible to get predictions of novelty from non-mathematical science. If I had had more space in the article to dot i's and cross t's, I would have said that "Only mathematical theories can predict novelty" is a statement of a rule that, in fact, has exceptions. Cochran had pointed out one of them. Another, even clearer, is Mendelieff's thoroughly empirical periodic table, which predicted new elements from its gaps. I maintain, however, that these are exceptions that prove the rule. (It could also be argued that a double helix is a mathematical concept.)

Regarding Earl Hautala's letter, my claim that empiricists believe that what we perceive around us is reality is, in other words, the claim that empiricists are common-sense realists. That is, they believe that all that we perceive around us, provided that it is public and non-illusory, is real, in the sense of continuing to exist when unperceived. (Or, as I put it once these terms had been defined, the empirically real is theoretically real.) This view used to be called naive realism, but today so many philosophers believe in it that they have renamed it direct realism, reserving the term naive realism for the belief that *everything* perceived, including illusions, is real. Hautala believes that I attribute naive realism to empiricists, whereas I attribute direct, or common-sense, realism to them. I also claim that common-sense realism cannot be true because it is self-contradictory, and advocate its replacement with the Leibniz–Russell theory. This, psychologically, is the most difficult point in the whole article, because common-sense realism is so basic a belief for all of us.

That one thing cannot be dissimilar to what it is is a logical principle rather than an errant theory. It is easily proved: Whatever A and B may be, if they are dissimilar, then there is some predicate, F, such that A is F and B is not-F; if A and B are one, then one thing is at once F and not-F, which is impossible—hence A and B are two. "Dissimilarity entails plurality," or "qualitative difference entails quantitative difference," are equivalent ways of stating this principle. The principle denies the common-sense belief that *one* thing can change with time. A change is a dissimilarity over time and the principle requires that the supposedly one thing before the change and after the change must be two because they are dissimilar. This means that identity (or oneness or individuality) and change are incompatible. Common sense has it otherwise, particularly with personal identity: 'I am the same person today as I was yesterday, even though I have changed in the interval" is an expression of this belief. This clash between common sense and logic is the philosophic problem of change, which goes back to Heraclitus ("All is change") and Parmenides ("Only the One is"). As with common-sense realism, the error arises from over-simplification and is unimportant in daily living. The identity or individuality of something can be specified as a four-dimensional space–time world-tube, defined by continuity of its parts; the temporal parts of it are many, and their dissimilarities are changes. Thus I as an entire lifetime am one, but I-yesterday and I-today are two dissimilar temporal parts of this one. So Hautala's citing of material process does not refute the principle.

Hautala has misunderstood my analogy of the apple and the box. The whole point is that it is logically impossible for *the* apple to be both inside and outside *the* box. So, if we want to preserve as much of the truth of each statement as possible, the best answer is to deny one or the other definite article, or both—which is what I did. If we cannot deny that what we perceive is outside our heads and also that it is inside our heads, then we do least damage to our belief system by allowing a duality of each.

I have to disagree with Hautala's claim that "No amount of philosophical theorizing can change our perceptions." Whenever we correct an illusion, we "philosphically theorize" from

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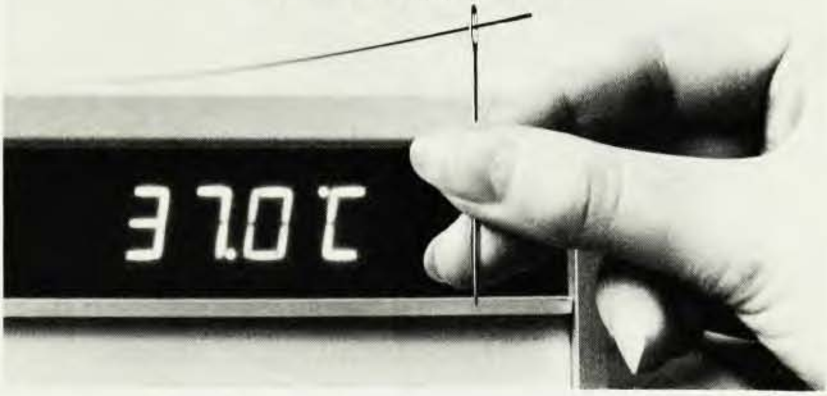

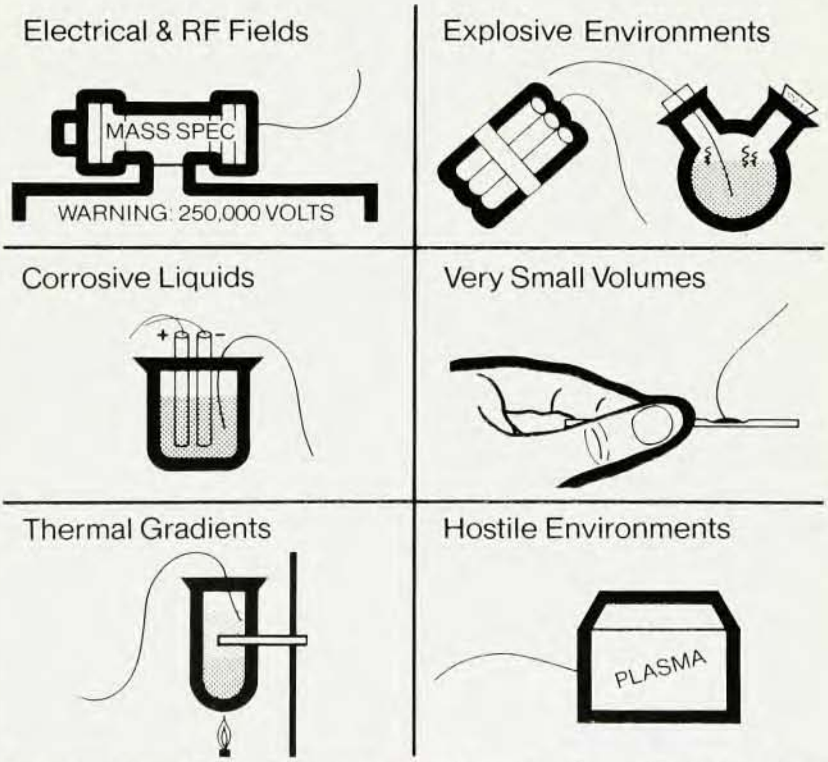

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the illusion to a belief that corrects it. This may not change the perception literally—the illusion persists—but it certainly changes our response to it. That the largely automatic and unconscious process by which we do this can be called "philosophical theorizing" shows that philosophy is not, as Hautala seems to believe, a process entirely divorced from everyday affairs.

Ruth Kastner's praise is gratefully received. She is, however, mistaken when she speaks of the observable universe. I avoided the term "universe" because it means the totality of existents, so that it is meaningless to speak of more than one universe—as I wanted to do in distinguishing the empirical and the theoretical. This is why I use the term "world." We each have our own empirical world, bounded by the blue sky, and with our own empirical head at the center; beyond the blue sky is the inside surface of our own theoretical head; beyond this theoretical head are other theoretical heads (each containing its own empirical world if the person concerned is conscious) and the rest of the theoretical reality that is planet Earth. Beyond the theoretical atmosphere (not to be confused with any empirical blue skies) is the rest of the enormous theoretical world. Kastner is confusing this latter with empirical worlds: The latter are enclosed by theoretical heads, the former is not.

I have several comments on the letter by Jean Rosch. First, the Leibniz–Russell theory does not reveal, as a discovery, that scientists must be objective, data should be quantitative and experiments should be repeatable—these facts of science are of course well known, as Rosch points out. What the theory claims to do is to *explain* why these criteria are successful in empirical science, by relating them to their common basis in publicity of empirical data.

Second, my claim that *all* research experiment is concerned with testing theories was confined to sophisticated sciences, such as physics and chemistry, in which there is successful cooperation between airies and earthies. This does not exclude the possibility of accidental discoveries during the course of these experiments, but the point is that the experiments are designed to test theories, not merely to search for accidental discoveries.

Third, perhaps he is right in saying that it is not curious at all that only mathematical theories can predict novelty. I can only say that *I* find it curious, and that I find his explanation of this less satisfactory than mine.

Finally, if Pythagoras intended his "All is number" to be understood literally, then Rosch is correct, whereas if he intended it metaphorically, then I may be correct.

I would certainly concede the right of experimentalists to have their own philosophy of science, but would much prefer that there be *one* philosophy of science, which does justice equally to experimentalists and theorists. If I had not wanted to be provocative, I would have called my article "A cooperative philosophy of science" rather than "A theorist's philosophy of science," since cooperation between airies and earthies is one of the great strengths of science (and the lack of it one of the great weaknesses of philosophy). Indeed, I am at present completing a book with this title; it has grown out of the writing of this article.

Contrary to Wendell G. Holladay's reading, I made no claim that empirical perception and theoretical perception are independent. According to the Leibniz–Russell theory, empirical perception is the last stage of theoretical perception. I did not make this explicit in the article, which is no doubt why Holladay misunderstood me on this. The distinction between empirical and theoretical realities does not stem from this, but is one discoverable in our language. That is, the word "real" has several meanings, and these are two of them. Normally, common sense identifies them—that is, all that we perceive around us that is public and non-illusory is believed to continue to exist when unperceived. The reason that the Leibniz–Russell is so difficult psychologically is that it shows this common-sense belief to be self-contradictory, and replaces it with an alternative. This does not mean that the single reality of science, of which he speaks, is lost: There is only one theoretical reality, which happens to contain, as parts, empirical realities inside theoretical heads.

Finally, I quite agree with him that in most scientific description the theoretical and the empirical become intermixed. I would have acknowledged this if I had had more space; as it was, I oversimplified.

A history of nuclear forces

Your Search and Discovery news story in March (page 20) on relativistic treatment of low-energy nuclear phenomena is an excellent summary of a promising direction in nuclear physics. I sincerely appreciate your citation of Dudley Miller's and my work [*Phys. Rev.* **5**, 241, (1972)] as a first attempt to do the shell model relativistically. Shortly after this work was completed, the Mansfield amendment, which prohibited DOD from sponsoring fundamental research, ended my nuclear research funding by AFOSR. Since Dudley and other former research associates were carrying the relativistic torch so well at major nuclear centers, I concentrated on my other research areas.

It might be of interest to your broad readership to add an account of our earlier involvement with the possibility that the complex behavior of nuclear interactions might be due to relativistic effects surviving the cancellation of large static terms in a scalar-vector theory. This concept was presented at a meeting of the American Physical Society in 1949. The abstract [*Phys. Rev.* **76**, 460, (1949)], in part, states.

> If two fermions are coupled five vectorially (scalar + four-vector) with a five-vector field then the chief explicit interaction will be $V_{ab} = (1 - \beta_a \beta_b - \alpha_a \cdot \alpha_b) v(r)$. The simple Diracian form has the following interesting properties: (a) The non-relativistic terms cancel; (b) Its Pauli equivalent to the order v^2/c^2 contains static, spin–spin, and tensor interactions as well as velocity-dependent interactions ... which appear suitable for nucleons. Thus the strong spin dependence of nuclear forces may be due to relativistic forces which become important if the large non-relativistic forces tend to cancel.

Unfortunately, the concept was contrary to conventional wisdom of the time and the manuscript giving the details was rejected by the *Physical Review* with the letter,

> I regret that the Board of Editors has not recommended the publication of your paper, "On Generalized Meson Theories of Nuclear Forces." It is considered by them to be almost entirely speculative, and that the chance that it will have something to do with physics or that it will have a beneficial influence on the progress of the subject is remote.

Conventional wisdom in nuclear physics in the 1950s and 1960s favored hard-core phenomenological N–N interactions, which caused strong two-body correlations in N-nuclear interactions. These correlations lead to effects very similar to those of velocity- or momentum-dependent forces (*Phys. Rev.* **119**, 1031, 1960), and to this day there is confusion between the two effects. The discovery of vector mesons in 1960 revived the meson theoretic approach, and several groups were able to achieve reasonable fits to higher N–N phase-shift data with vector mesons, but only after introducing a scalar or effective scalar meson field. Noting this development, we published the essential substance of my 1949 manu-

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