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THE CONCEPT OF OBSERVATION IN SCIENCE AND PHILOSOPHY*

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Through a study of a sophisticated contemporary scientific experiment, it is shown how and why use of the term 'observation' in reference to that experiment departs from ordinary and philosophical usages which associate observation epistemically with perception. The role of "background information" is examined, and general conclusions are arrived at regarding the use of descriptive language in and in talking about science. These conclusions bring out the reasoning by which science builds on what it has learned, and, further, how that process of building consists not only in adding to our substantive knowledge, but also in increasing our ability to learn about nature, by extending our ability to observe it in new ways. The argument of this paper is thus a step toward understanding how it is that all our knowledge of nature rests on observation.

I

A philosopher of science has remarked that "There is one thing which we can be sure will never be observed directly, and that is the central region of the sun, or, for that matter, of any other star." The claim is certainly plausible: for the center of the sun, for example, lies buried beneath 400,000 miles of opaque material, at temperatures and pressures which surely make that region forever inaccessible to us. It must therefore

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be with considerable puzzlement that we encounter passages like the following:

> . . . neutrinos originate in the very hot stellar core, in a volume less than a millionth of the total solar volume. This core region is so well shielded by the surrounding layers that neutrinos present the only way of directly observing it (Weekes 1969, p. 161).
>
> There is no way known other than by neutrinos to see into a stellar interior (Clayton 1968, p. 388).

What explains this seeming contradiction between the claim of the philosopher and the usage of the astrophysicist? Is the philosopher simply showing his ignorance of the ingenuity of modern science? Or, if he is aware of the relevant scientific advances, is he perhaps laying down an injunction about how the expression 'directly observed' *ought* to be used, rather than denying that neutrinos offer a way of *learning* about the interior of a star? Or, alternatively, is the scientist using the term 'observation' and its cognates in ways which are at best only tenuously related to the philosopher's usage, and perhaps to ordinary usage as well, so that the scientist's way of speaking is misleading, at least to the non-scientist and perhaps even to the scientist himself? Or are the philosopher and the astrophysicist interested in entirely different and unrelated problems, which are reflected in their different usages of the same term, so that they are talking completely past one another even though their usages are, from their respective points of view, equally legitimate? Or are the usages perhaps related, but in ways more subtle and complex than might be supposed from these or other alternatives?

Since we began with the philosopher's remark, let us consider the matter from his point of view: that the astrophysicist's usage is loose or careless or misleading in some way. A look at the character of the experiments which have been conducted by Raymond Davis, Jr., continuously since 1967 to "observe" neutrinos coming from the center of the sun seems at first glance to bear out the philosopher's contention. For those experiments, which we will examine more closely later, involve the following complex chain of reasoning and activity. In the range of stellar masses which includes the sun, the basic energy-producing process is believed to be the so-called "proton-proton" sequence of reactions, initiated by the interaction of two hydrogen nuclei (protons). In the ensuing reactions, three alternate chains, each having a calculable probability, are possible. One of these involves the production of the radioactive isotope Boron 8 (^{8}B), which decays and releases a highly energetic neutrino. The neutrinos thus produced carry away a calculable amount of energy; and since the probability of later capture (and therefore of detection) of a neutrino is roughly proportional to the square of the neutrino energy, it would appear to make sense, if feasible within existing technology and desirable in view of research priorities, to set up apparatus capable of detecting them. Since the probability of capture, as well as the probability of the ^8B branch, are both calculable, information can thus be gained about the processes occurring in the sun, and models of it and its energy-producing processes can be tested. The "neutrino detector" used in these experiments consists of a 400,000-liter (610-tons) tank of cleaning fluid (perchloroethylene, C_2Cl_4) located in a deep mine (about 5000 feet) to shield it from other particles which might produce effects similar to those of a neutrino captured from the sun. The isotope ^{37}Cl, accounting for about one-fourth of natural chlorine, will undergo the process:

$$^{37}\mathrm{Cl} + \nu_e \rightarrow {}^{37}\mathrm{Ar} + e^-$$

followed by beta decay of the radioactive argon (half-life: 35.1 days):

$$^{37}\mathrm{Ar} \rightarrow {}^{37}\mathrm{Cl} + e^+ + \nu_e$$

(e^-: electron; e^+: positron; ν_e: electron neutrino). The radioactive argon must be removed from the tank before it decays; this can be done by bubbling helium through the tank. The argon is then separated from the helium by a charcoal trap and, finally, carried by stable argon gas to a detection chamber. There the decays of argon will be registered by a proportional counter, so that the number of neutrino captures are counted; these can then be compared with the predictions of theory.

Surely the elaborateness and sophistication of this procedure suggests that the philosopher has made a valid point! For what are *observed* here, we might be inclined to say, are not events occurring at the center of the sun, but at best only absorptions of neutrinos in our apparatus, or—perhaps more strictly—the decay of radioactive argon, or—more strictly still—only the individual registrations of the proportional counter, or—perhaps most strictly of all—only the sense-data (clicks, for example) in the consciousness of a perceiver; all else, strictly speaking, must be ascribed to inference. And the philosopher's attitude might seem to be bolstered even further when we look at some statements by scientists other than those quoted above, concerning the youthful but burgeoning subject of neutrino astrophysics—statements which, if we take the attitude suggested by the philosopher, might be viewed as being more careful, or less misleading, than the previous authors' talk of "direct observation". Some works, for example, speak in terms of the "detection" of solar neutrinos.

> R. Davis, Jr. (1964) undertook a first attempt to detect neutrino emission from the sun (Ünsold 1969, p. 319).
>
> This [neutrino] flux could be detected with present day technology (Reeves 1965, p. 149).

At other times, words like 'probe' or 'measurement' or 'information' play a role corresponding to that played by the expression 'direct observation' in the earlier quotations.

> Because neutrinos, once produced, can reach the earth without further interactions, they are a potential probe of the deep stellar interiors from which they are emitted (Ruderman 1969, p. 154).
>
> Neutrino astronomy can therefore give us direct information about the energy-producing core of the Sun (Ünsold 1977, p. 353).

Yet we must not be too hasty in taking such usages as scrupulous avoidances of the allegedly loose or misleading term 'observation' (or 'direct observation'). For in almost all the quoted works (as well as many others not cited here), such terms as 'probe', 'detection', etc., are either explicitly used interchangeably with '(direct) observation' or some cognate thereof, or else are pretty clearly considered to be reasonable alternatives to that term. A fuller quotation from Reeves (1965) will serve to illustrate this general tendency:

> Is there any hope of *observing* these solar neutrinos? . . . To answer this question we shall briefly review the state of affairs in neutrino astronomy. . . . It is worth noting that if Dr. Davis obtains a positive result, that is, if he actually *detects* some neutrinos from the sun, he will have given us for the first time a *direct proof* of the occurrence of nuclear reactions in the stars. . . . All these [present] proofs [of the occurrence of nuclear reactions in stellar interiors] remain indirect in nature. The clicks in Dr. Davis' tank would put a magnificent end point to our speculations. By the same token the clicks could tell us more about the nature of stellar interiors (Reeves 1965, pp. 149–151; italics mine).

These statements should be combined with the previously-quoted one, "This [neutrino] flux could be detected with present day technology", which occurs in the same context. Similarly, Ruderman—quoted above as an example of a user of the term 'probe'—continues by asserting that "Neutrinos can and probably soon will give us a *direct view* of the solar core" (Ruderman 1969, p. 154).

One cannot but hesitate in the face of such widespread usage among men who are undoubtedly highly sensitive to the concepts and techniques of their subject: can we, after all, go along with the philosopher and allege that they are *all* using the notion of "direct observation" loosely, or incorrectly, or misleadingly, in a kind of sociological aberration that philosophers must gently tolerate while realizing that it has nothing to do with "genuine" observation? On the other hand, even if we were to accept the view that their usage is perfectly clear, and is misleading only to the uninitiated, the possibility would still remain that it is nevertheless not that of either the philosopher or the ordinary man; and perhaps such divergences may be indicated by the alternative terms, like 'probe', which the astrophysicist uses to do (at least roughly) the same work as is done for him by the term 'observation'. There are other peculiarities of usage, too, that might lead us in this direction. For example:

> Only neutrinos . . . can enable us to "see" into the interior of a star (Bahcall and Davis 1966, p. 241).
>
> . . . neutrinos offer us a unique possibility of "looking" into the solar interior (Bahcall and Davis 1976, p. 264).

Although the use of quotation marks around "see" and "looking" is by no means universal, even in the writings of these two central figures in the field, the occurrence of the marks is clearly a warning that those terms are being used metaphorically. But what does that warning indicate about 'observe'? Is the use of that term, too, to be taken as "metaphorical"? Or is a distinction to be understood between 'seeing', 'looking', and similar terms having to do with *sense-perception* on the one hand, and *observation* on the other? What are the relations, if any, between sense-perception and observation in the sense (or senses) in which the astrophysicist uses the latter term? And of course we would like to know whether the astrophysicist's use of that term and its cognates is paralleled in other areas of science. The ultimate aim would be to gain insight into the ways in which observation and sense-perception are related to the understanding, acquisition, and testing of scientific ideas.

Again, is there any significance in the almost universal use of the qualifying adjective 'direct' in the astrophysicist's talk of "direct observation"? How much of the burden of import of the expression lies in the term 'observation' and how much in the term 'direct'? Does the use of that adjective mark the real difference between the astrophysicist's talk and that of the ordinary man or the philosopher?[1] And finally, as we shall see, there are ambiguities, or at least differences, in what is said by astrophysicists to *be* observed: sometimes it is said to be the central core of the sun, and at other times to be the neutrinos in Davis' apparatus. Does that divergence indicate confusion on the part of astrophysicists, or does it perhaps have a deeper significance, one which throws light on the role of observation in science?

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[1] But philosophers, too, have often claimed that observation in the "strict" sense must be "direct observation"; see, for example, Carnap (1950 and 1956), Hempel (1958). However, what is usually meant in such claims is that the observation should not be "indirect" in the sense that it takes place through any intermediary, like a mirror. (One must wonder why air, or even a vacuum, should not count as an intermediary.) We shall find that this is most definitely not the sense of "directness" the astrophysicist has in mind.

At the very least, it would be worthwhile to try to determine what does lie behind the astrophysicist's way of putting his point about neutrinos, especially since philosophers of science have made such a point in recent years about the necessity of understanding the way science actually proceeds. Certainly the issues which have arisen regarding the relations between ``observation'' and ``theory'', and the extent to which ``observation'' is ``theory-laden'', and the implications such theory-ladenness might have for the ways scientific theories are tested and the objectivity of that testing, claim, at any rate, to have to do with a sense of the term 'observation' which is relevant in science. Perhaps an analysis of this case might throw new light on some of the controversies that have afflicted the philosophy of science in recent years, and, indeed, since the days of Hume and Kant, about ``theory'' and ``observation'' and their relationships.

One point must be made before we proceed. The object of interest here is not a word or group of words, but rather the contrasts involved or implied in the use of those words. For our problem is that, in the use of such terms as those noted—whether 'observation', 'direct observation', 'detection', 'probe', or whatever—some kind of contrast is presumably being suggested, usually implicitly, by the writers quoted. And one of the primary purposes of this paper concerns the extent to which that contrast is clearly and adequately characterized as one between ``observation'' and ``theory''. This concern carries us to the central issue in contemporary philosophy of science: for whereas traditional empiricist and positivist philosophers rested their interpretations of science on this distinction, a number of recent writers have rejected it, at least in its traditional forms. But in doing so, they have paid a heavy price; for, as a result of abandoning the distinction, or, in other cases, of recasting the relations between observation and theory, they have ended by sacrificing the objectivity and rationality of the scientific enterprise. And the question should be, not *whether* science is objective and rational, but rather in what, precisely, its objectivity and rationality consist. It is at the resolution of these issues, and therefore at an understanding of the nature of the scientific enterprise and its achievements, that the present analysis is ultimately directed.

II

The key to understanding the astrophysicist's use of 'direct observation' and related terms in his talk about neutrinos coming from the center of the sun is to be found in the contrast between the information so received and that based on the alternative available source of information about the solar core, the reception of electromagnetic information (light-photons). In the latter case, our knowledge of the character of electromagnetic processes, and of conditions inside the sun, tells us that the mean free path of a photon—the distance it can be expected to travel without interacting with some other particle—is extremely short, well under one centimeter, under the conditions of temperature and pressure existing in the central regions of the sun where the photon is produced. Consequently, even allowing for reduced severity of those conditions with greater distance from the center of the sun, a packet of electromagnetic energy produced in the central core can be expected to take something on the order of 100,000 to 1,000,000 years to reach the surface, and in the intervening period will have been absorbed and re-radiated, or scattered, many times, the original character of the radiation (and therefore of the information carried by it) being altered drastically in the process. What was born in the energy-producing regions as a very high-frequency, short wavelength gamma ray finally emerges at the solar surface in the form of relatively low-frequency, long wavelength light typified by that in the visible and adjoining regions of the spectrum. After leaving the sun and passing into interplanetary space, the radiation will proceed with only a very low probability of interference, and even when it passes through our atmosphere, much of it will reach our eyes and instruments unaltered by interaction with atmospheric particles. It is in this sense that our ''direct'' electromagnetic information about a star comes from a very thin surface layer (whose thickness is a function of, among other things, the frequency of the light) called the ''photosphere'' of the star; all conclusions about the deeper regions based on that information must be ''indirect'', ''inferential''.

Contrast this with information about the central core received *via* neutrinos emitted therefrom. The extremely ''weak'' character of the interactions of neutrinos with other matter, and the consequent low probability of their being interfered with in their passage over long distances, even when passing through dense bodies, enables them to traverse freely the overlying layers of the star, the intervening interplanetary space, and our atmosphere, up to the sophisticated detectors which will, extremely infrequently, capture them. Any information they carry is thus unaltered by interactions along the way, even when they come from the central regions of the sun. In the words of one pioneer in the field, ''They are at one and the same time the most reliable and the most reluctant of messengers'' (Fowler 1967, p. 53).

My suggestion is that we take this contrast seriously as a basis for interpreting the expression 'directly observed (observable)' and related terms in contexts like those cited earlier about direct observation of the center of the sun. Let me, then, propose the following analysis:

x is directly observed (observable) if:

(1) *information is received (can be received) by an appropriate receptor; and*

(2) *that information is (can be) transmitted directly, i.e., without interference, to the receptor from the entity x (which is the source of the information).*

In the remainder of this paper, I will elaborate on this analysis, developing in detail its content and implications, and explaining the sense in which it is an ''analysis''. My discussion will show, among other things, that *specification of what counts as directly observed (observable), and therefore of what counts as an observation, is a function of the current state of physical knowledge, and can change with changes in that knowledge.* (What counts as ''knowledge'' in this context will be examined later.) More explicitly, *current physical knowledge specifies what counts as an ''appropriate receptor'', what counts as ''information'', the types of information there are, the ways in which information of the various types is transmitted and received, and the character and types of interference and the circumstances under which and the frequency with which it occurs.*

It will be convenient to divide our discussion into three parts:

1. the release of information by the source (the entity x);
2. the transmission of that information; and
3. the receptor of that information.

I will call these three elements of the ''observation-situation'' *the theory of the source, the theory of the transmission,* and *the theory of the receptor,* respectively, of the information.[2] It should be understood that separation of the observation-situation into these three components does not occur in scientific presentations; indeed, as we shall find, they are so intertwined that their separation is sometimes rather artificial and would serve no purpose from the scientific point of view. However, it will turn out that separating them in the way I have will make possible a fruitful discussion of certain problems that have been raised by philosophers concerning the relations between theory and observation.

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[2] Some, after reading what follows, might wish to argue that what I have referred to as a ''theory'' in each of these three cases is really a conjunction of what are properly called theories and statements of specific fact (initial and final, or boundary, conditions). However, although that distinction is very important for some purposes, my use of 'theory' in reference to their conjunction is a quite common one, and is perfectly appropriate in the present context, where their conjunction (e.g. in a ''theory of the sun'') is designed to explain a certain domain of information (including, for example, the observed luminosity and, to a lesser extent, radius of the sun), or to describe and explain the operation of a certain instrument.

I will discuss the three components in terms of the solar neutrino case. Because my concern in this paper is primarily with the concept of observation, and only incidentally with the role of observation in the testing and justification of theories, the second and third components, the theories of transmission and of the receptor of the information, are most relevant for our purposes. However, as we shall find, it is impossible to discuss these without an understanding of the theory of the source; and, in any case, we shall find that what counts in science as an observation is not fully separable from the testing and justification of theories. Therefore the theory of the source must also be discussed here.[3]

1. The Theory of the Source. Since the late 1930's, physicists and astronomers have developed a theory which, on the basis of a great many diverse considerations, appears to give an excellent account of the production of energy by stars.[4] According to that account, the energy is produced in the small central core of the star, where the temperature and density are great enough to generate nuclear reactions, of which the most important, in fully formed stars which have not exhausted their plentiful initial supply of hydrogen in their cores (''main sequence stars''), is the conversion of that element into helium. In that process, the slight excess of mass of four protons (hydrogen nuclei) over a helium nucleus is converted into energy according to the familiar $E = mc^2$ relation. That energy is then transmitted to the surface of the star (as will be described in the next section), whence it passes into space. As I indicated earlier, the primary mode of conversion of hydrogen into helium, with consequent release of energy, in relatively low-mass stars like the sun is the ''proton-proton chain''. This process may be schematically represented as follows:

$$4\,{}^{1}\mathrm{H} \rightarrow {}^{4}\mathrm{He} + 2e^{+} + 2\nu_{e}$$

(H: hydrogen; He: helium; e^+: positive electron [positron]; ν_e: electron neutrino. The superscripts refer to the atomic mass number [isotope] of

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[3] There are many excellent accounts of the solar neutrino experiment. My survey has relied on a large number of them, of which the following sample may be mentioned (I have included reports written at different stages of development of the experiment and by different authors): Bahcall (1967, 1973, and 1979); Kuchowicz (1976); Sears (1966); Shaviv (1971). Non-technical presentations are given in: Bahcall (1969); Bahcall and Davis (1976). A fine historical review of the experiment (up to 1980) is Bahcall and Davis (1981).

[4] Bethe and Critchfield (1938); Bethe (1939). Surveys of later developments and other pertinent aspects of the theory of the source of information from stars are found in Schwarzschild (1958); Clayton (1968); Cox and Giuli (1969); Motz (1970); Reddish (1974). A less technical, but excellent, survey, which includes a chapter on the solar neutrino problem, is Shklovskii (1978), Part II. Bethe has reviewed his development of the theory in his Nobel lecture (1968).

the element. Sometimes 'p', for proton, is written instead of 'H', and 'α', for alpha particle, instead of 'He'.) Thus each individual episode of fusion releases two neutrinos. However, the detailed process is far more elaborate, there being three alternate subchains by any one of which the final result can be achieved.[5] The individual nuclear reactions, and the probabilities of their occurrence under specific circumstances, are determined in part by the theory of nuclear reactions and in part by experiment. Where neutrinos are concerned, the theory of weak interactions—the only type of interaction in which those particles participate—plays a central role.

Since the neutrinos pass freely through the sun, and since the amount of energy released in each individual reaction, and the total amount of energy radiated by the sun, are known, it is an easy matter to calculate the total number of neutrinos emitted, and hence the total number received per square centimeter per second at the earth (approximately 10^{11} $\text{cm}^{-2}\ \text{sec}^{-1}$). However, the apparatus used in Davis' solar neutrino experiment—the only one operating as of this writing—is capable of detecting only the more highly energetic of those neutrinos. These are produced primarily in the third of the alternate subchains, and, in particular, in that part of that subchain in which the isotope boron 8 decays as follows:

$$^{8}\text{B} \rightarrow {}^{8}\text{Be} + e^{+} + \nu_{e}$$

(B: boron; Be: beryllium.) The relative frequencies ("branching ratios") with which each of the three subchains occurs is dependent on density and temperature (in the case of the latter, the dependence is particularly strong: the branching ratio for the crucial third subchain, for example, depends roughly on the thirteenth power of the absolute temperature under the conditions prevailing at the center of the sun). Thus the proportion of neutrinos *relevant to this experiment*—the energetic boron 8 neutrinos produced in the third subchain—is a function of the internal temperature and density of the sun, so that a theory of the source must include an account of those conditions, i.e., a model of the sun.

A normal star like the sun is an object that has remained remarkably stable over a very long period of time, the inward pull of gravity being delicately balanced by the outward pressure of the motion of particles and by the energy produced in its core by nuclear fusion and transported to its surface. Construction of a model for such normal stars rests on these assertions, or, more precisely, on a set of differential equations expressing them. These are: an equation asserting that the material of the star is in hydrostatic equilibrium, i.e., that the gravitational attraction is bal-

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[5] For the details of the proton-proton chain, see the Appendix to this paper.

anced at every point by the thermal pressure of the moving particles of the material and by the radiation pressure; an equation expressing the assumption that the energy source is nuclear fusion, i.e., that the total energy emitted by the star is equal to the sum of the energies released by all the individual nuclear reactions; and an equation expressing the assumption that energy is transported from the core to the surface by radiation and convection (the third major type of energy transmission, conduction, plays a wholly insignificant role). The star is further supposed to be a "perfect gas", one in which interactions between the particles of the gas can be ignored, and to obey the "perfect gas law" relating the pressure of the gas to its density and temperature. This assumption is justified by the fact that, at the very high temperatures prevailing in stellar interiors, atoms will be highly, even completely, ionized, their orbital electrons being stripped away to leave the separated nuclei and other particles so far apart in relation to their diameters and the forces acting between them that the latter can be ignored for many purposes.

According to modern theory, the fundamental parameters, from which all the others—including temperature and density, and indeed all aspects of the condition and structure of the star—entering into those equations can be calculated, are the mass of the star and the distribution of chemical composition in its interior (this is the "Russell-Vogt theorem"). The mass is known with considerable accuracy from gravitational interactions between the sun and its planets, but the chemical composition and its distribution must be estimated by a complex procedure. The highly successful modern theory of stellar structure implies that in a star of the sun's mass there are no deep convection currents mixing the material therein; what convection there is is confined to a rather superficial layer near the surface. With the further assertions, also well-founded, that the sun's energy is produced by thermonuclear fusion of hydrogen into helium, and that that process takes place in the deep interior of the star, it follows that as the sun grows older, the proportion (by mass) of helium (denoted 'Y') and the heavier elements ('Z') to hydrogen ('X') increases as the latter is gradually exhausted. It also follows that the surface material, since it is not mixed with deeper material and undergoes no nuclear transformations itself, has the same composition as the entire sun did at its birth. That conclusion depends on the additional assumption that the sun was homogeneous at its birth, the deeper regions indeed having had the same composition as the surface layers; but that assumption is supported by the modern theory of early stellar evolution, according to which a newborn star whose hydrogen-fusing nuclear reactions have not yet been ignited will pass through a stage in which complete mixing (convection) takes place. Hence spectroscopic observation of the sun's surface regions, or photosphere, will furnish information about the *primordial* composition of the sun. More exactly, X (proportion of hydrogen) and Z (proportion of elements heavier than helium) can be so determined; that of Y (helium) cannot, though evidence of its value elsewhere in the universe can be applied to give a reasonable estimate of its value in the primordial sun. A model of the *present* sun can therefore be obtained as follows:

(1) Construct a "zero-age" model of the sun for the time when hydrogen fusion reactions begin (i.e., when the sun enters the main sequence stage); this is done by assigning to the zero-age sun the mass of the present sun, the spectroscopically-determined value of the ratio of Z to X, and an initial estimate for X. (Z is determined from the observational value of Z/X and the estimate of X; since X, Y, and Z are fractional abundances, $Y = 1 - Z - X$. Clearly, one could begin with a different observationally-obtained ratio and a different parameter—X or Z; all such approaches give similar results.)

(2) Knowledge of the rates at which the energy-producing reactions take place is obtained from laboratory measurements where possible, and from theoretical calculations otherwise. Those reaction rates now allow calculation (on high-speed computers) of the change in proportion of X, Y, and Z after some specified time interval to give a second, later model. A sequence of later models is then computed, the last having the age assigned to the sun. (The process would be staggering without computers; on an IBM 7090, it takes about ten minutes.)

(3) *If* that final model assigns to the sun the luminosity it is observed to have, the estimate as to the initial value of X is validated; if such agreement is not found, the procedure is repeated using a slightly different value of X for the zero-age model, until a sequence of models is found which culminates in one having the observed luminosity of the sun. (Theoretically, the model should give the present solar radius as well as the luminosity; however, there are ambiguities concerning what counts as the radius of the sun, and in any case its value can be shown not to affect the production of neutrinos in the interior to any significant degree.)

In principle, the final model of the finally-adopted sequence gives the ^{8}B neutrino flux from the sun's core and therefore that to be expected at the earth; the third branch, in which that reaction occurs, contributes approximately 0.1% of all the neutrinos produced in the sun. Though this is a small proportion, the absolute numbers of them passing through the earth's surface is still "astronomical"—on the order of 100,000,000 per square centimeter per second. It is from among these highly energetic neutrinos that the detector to be discussed in Section 3, below, is designed to capture enough to provide information about what goes on inside the sun. (A small contribution of neutrinos sufficiently energetic for the detector also comes from some other reactions in the chain besides the ^{8}B one, but I will ignore that contribution here.)

However, exact values of each of the parameters entering into the calculations are not available, and account must be taken of the range of uncertainty. Most of the parameters were highly uncertain (relative not to the feasibility of the experiment, but rather to the accuracy desired) at the outset of the experiment, and have undergone numerous refinements in the intervening years. The possibility of error, or of unknown processes, remains, of course, though in both cases present knowledge indicates, often precisely, the kinds of things to expect. For example, one sort of "unknown processes" to look for are resonances; it is even known in many cases, on the basis of theoretical considerations, where (at which energies) resonances *might* occur, and what effects they might have on the production of the relevant neutrinos; and although laboratory measurements often cannot reach the low energy levels where the proposed resonance might lie, it is sometimes (as in the most significant case that has yet arisen) possible to get close enough to make the existence of the resonance highly unlikely. As to possible errors in quantitative measurements, these can be determined also. Models have been constructed varying all the parameters within the range of reasonable possible error; precise judgments can thus be made of the effect a variation in each of the parameters (within the limits of "reasonableness") would have on the production of ^{8}B neutrinos, so that the percentage change that would be incurred by the latter if the error were the maximum (reasonable) is known. The range of errors, and the possibility of resonances where they might have been suspected, have been reduced considerably or eliminated over the years. The assumptions embodied in the basic equations of stellar structure, too, can be altered so as to make them more "realistic"—e.g., by including recognition of deviations from the "perfect gas" state; an estimate of a contribution from the other major process of stellar fusion of hydrogen, the CNO cycle, has also been included. By now, the constraints have been so tightened that in a standard model—one which keeps within the limits that are declared reasonable in the light of the best information we have—any conjunction of errors that would change the predicted ^{8}B neutrino flux by more than a factor of two must be considered highly unlikely. As we shall see, given the strong reasons for accepting the "theory of the source"—that is, the best current theories of stellar structure, stellar evolution, stellar energy production, the relevant physical theories of nuclear reactions and the electromagnetic, weak, and gravitational interactions, and more specific physical theories like that of how energy is transported, plus the numerical values assigned to the various parameters entering into the picture—this error range is low enough for the reception of neutrinos in the experiment to be informative (Section 3, below).

The standard models work within certain assumptions. A review of a few of these will indicate some ways in which their ''reasonableness'' is assessed. I have already discussed the reasonableness of assumptions concerning the initial chemical composition, absence of deep convection, and the age of the sun. It is further assumed that the sun is spherically symmetrical, and that its rotation and magnetic field play no significant role in altering the expected production of ^{8}B neutrinos. Spherical symmetry not only simplifies the calculations, it is also reasonable given the radial character of the gravitational force and the net radial flow of radiation. The surface of the sun is observed to rotate very slowly and to have a magnetic field which is also insignificant with respect to the present problem. And although a rapidly rotating core or large internal magnetic field could be expected to affect the relevant neutrino production, and cannot be entirely discounted, such assumptions either have no independent support or would lead to further difficulties or both. Again, it is assumed that there has been no significant mass loss by the sun over its lifetime since it reached the zero-age main sequence stage. Mass loss can be significant in very massive stars and in giant stars (to say nothing of stars in violent explosion), but is not observed to be appreciable in lower- and mid-main-sequence stars, nor does the theory of stellar evolution, successful in so many other regards, countenance such loss. The mass-loss by the sun through the solar wind is insignificant for the present issue. This assumption thus appears to be quite justified. In addition to these astronomical assumptions, there are also broader physical ones. It is supposed, for example, that the constant of gravitation (and therefore the force between two gravitating bodies) has not varied over the lifetime of the sun. But that is an assumption made in the best theory of gravitation we have, the theory of general relativity.

The predictions of the standard models have not been borne out since the inception of the experiment in 1967: the reception of solar neutrinos by Davis' apparatus has been consistently lower than the rate predicted by the ''theory of the source'', the defect as currently calculated being on the order of a factor of three (and thus well outside the factor-of-two error range considered maximally reasonable for standard models). Naturally, the persistence of such disagreement has led increasingly to questioning of the astronomical and physical assumptions of the standard model. While it is not my purpose in this paper to consider the character of such ''non-standard models'', I will mention briefly *some* of the alternatives that have been considered. As would be expected from the preceding discussion, the following are among them: the existence of deep convection currents (either continuous or sporadic), magnetic fields, or a rapidly rotating core; denial of the assumption that photospheric abundances reflect initial composition, and the attempt to provide mechanisms by which the former are altered over the lifetime of the sun. But more extreme possibilities have also been considered—all of these also being available patterns of explanation in current physics. Among them are: variation of the gravitational constant over the lifetime of the universe and in particular over the lifetime of the sun (such variability being characteristic of some gravitational and cosmological alternatives to standard general relativity); neutrino decay or oscillation of neutrinos between the ''electron'' type and other types, so that if there are (for example) three different types (electron, muon, tau), only one-third of the total emitted will be detectable by apparatus designed to detect only electron neutrinos; the ubiquitous black hole, this time at the center of the sun; and even, perhaps at the very fringe of scientific speculation (but not for that reason necessarily incorrect), a rehauled weak interaction theory in which the existence of neutrinos is not assumed.[6] It should be added that there is very little reason to accept any of these proposed non-standard solutions of the solar neutrino problem; indeed, many of them suffer from extreme difficulties.[7]

2. The Theory of the Transmission. The key property of the neutrinos travelling between the sun and the earth is the extreme rarity of their interaction with any other particles, a property which is incorporated into modern weak interaction theory. With a cross-section for interactions generally in the neighborhood of 10^{-44} cm^2, they could be expected to pass through many light-years' thickness of solid lead with a miniscule probability of interaction. However, there are possibilities as to what might happen to them on their way from the sun that would affect their information-carrying possibilities; as we have seen, they might decay, or they might oscillate between different ''states'' while travelling from the solar core to the earth. Such possibilities have been considered, and the hypothesis of neutrino oscillations is currently attracting a great deal of attention. Despite the fact that they involve only one particle, such events are treated in modern physics as interactions, on a par with interactions between two or more particles. Hence it is necessary to understand the

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[6] An excellent survey of ''radical'' (non-standard) proposals (as of the writing of the article) is found in Kuchowicz (1976, pp. 327 ff.).

[7] The most serious possibility lies in the hypothesis of neutrino oscillations, which is being actively considered by physicists on other grounds. However, there remain grave doubts as to whether that hypothesis could really reduce the predicted neutrino flux to the observed level.

term ‘interference’ in the second condition of my analysis of ‘directly observed’ to include single-particle events of the sorts accepted by physics or considered as reasonable possibilities by physics. One might even consider reformulating that condition by replacing the term ‘interference’, which suggests a classical causal interaction between two entities, by ‘interaction’, where the concept of an interaction can be considered a generalization of the former idea in the light of modern physics. But this is not necessary, and because of similar associations of the term ‘interaction’, would not be that much of an improvement. What is necessary is that we keep in mind how current physics specifies what is to count as an interference or interaction.

More about how a “theory of transmission” functions is brought out by considering the transmission of photons. If the object x about which we are trying to obtain information were the surface of the sun, our interest here would be in the kinds of effects on the information carried to us by photons that are *possible* according to current physical theory, and that *actually* can be expected under the conditions existing between their origin in the solar photosphere and their capture by our photon receptors (telescopes, spectroscopes, eyes, etc.). Since the object x about which we are attempting to gain information in the solar neutrino experiment is the solar core, the interference we are interested in will include anything that happens to the radiation not only between the solar surface and our receptors, but also between its production in the solar core and the surface. The types of such interference that are possible are specified precisely by modern physics: they are the factors contributing to the opacity of the sun to the passage of photons, and consist of bound-bound, bound-free, and free-free absorptions, as well as electron scattering. With regard to each of these, the character of the interaction, and the probability of its occurrence under given conditions, are specified mathematically. Difficulties with regard to estimating each—that is, *definite and specific ways in which our knowledge falls short*—are also given detailed and precise treatment. (See, for example, the detailed treatment in Clayton 1968, especially pp. 170–232.) In the light of this general knowledge about types of photon interactions and the particular knowledge about conditions inside the sun, it is possible to draw the conclusion we saw earlier: that any information carried by the photons from the center of the sun is completely altered in the long and tortuous passage of energy from its production-point; strictly speaking, the photons released at the sun’s surface are not the same photons that were produced in the core. And thus it is impossible, through photons (electromagnetic energy), to observe directly the central regions of the sun. The same general sorts of considerations lead to the conclusion that it *is* possible to observe directly the surface of the sun through photon receptors, there being small chance of alteration of that information in the region between the surface of the sun and our receptors.

3. The Theory of the Receptor. In 1946, Bruno Pontecorvo discussed the possibility of detecting neutrinos by a process which is the inverse of radioactive beta decay.[8] The neutrino had originally been hypothesized to save the principles of conservation of energy and momentum in beta decay; according to Pauli's suggestion, the energy and momentum missing from the observed beta decay products were carried away by a hitherto-unknown particle, later christened the "neutrino". The beta decay process, thus understood, may be schematized as follows:

$$_{Z+1}{}^{A}Y \rightarrow {}_{Z}{}^{A}X + e^{+} + \nu_{e}$$

where Y is the radioactive nucleus, of atomic weight A (= number of nucleons [protons and neutrons] in the nucleus) and carrying a charge (number of protons) of $Z + 1$; X is the product nucleus, reduced in charge by one unit; as before, e^{+} is the positron, ν_{e} the electron neutrino. The inverse of this process would then consist of the capture of a neutrino to form a radioactive nucleus:

$$\nu_{e} + {}_{Z}{}^{A}X \rightarrow {}_{Z+1}{}^{A}Y + e^{-}.$$

The method discussed by Pontecorvo of detecting the neutrino consisted in using an appropriate substance for such capture and examining it for the presence of the expected radioactive nucleus; the latter would make itself known through its own decay. Pontecorvo laid down five desirable features that should be fulfilled as well as possible by the material used, the product resulting from the inverse beta decay, and the apparatus used in the detection experiment.

1. Because of the extremely low probability of the inverse beta decay process (neutrino capture) occurring, huge amounts of capture material would have to be used; therefore, "The material irradiated should not be too expensive."
2. "The nucleus produced in inverse β transformation must be radioactive with a period of at least one day, because of the long time involved in the separation."

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[8] Pontecorvo (1946). Use of the inverse beta decay process as a basis for construction of a neutrino receptor is an example of a *radiochemical* method. An alternative, nonradiochemical method would employ elastic scattering of neutrinos by orbital and free electrons:

$$\nu_{e} + e^{-} \rightarrow \nu_{e} + e^{-}.$$

This method, proposed and attempted by Reines, will not be discussed here.

3. "The separation of the radioactive atoms from the irradiated material must be relatively simple." Pontecorvo remarked that the best prospects would result if the radioactive atoms were of a rare gas, since such a substance, not entering into chemical combination with other atoms, would be easily separable by physical methods.
4. "The maximum energy of the β-rays emitted by the radio element produced must be very small . . . This is so because the probability of an inverse β process increases rapidly with the energy of the particle emitted." This condition amounts to asking that the probability of the inverse beta decay process occurring be as high as possible, and the threshhold for its occurrence as low as possible.
5. "The background (i.e., the production of [the radioactive nucleus] by other causes than the inverse β process), must be as small as possible." I will discuss this condition shortly.

In the light of these desiderata, Pontecorvo discussed the advantages and disadvantages of various substances which, *in the light of existing knowledge of nuclear reactions, the specific characteristics of those substances, and available detection methods,* might serve as detector material. Among them was chlorine 37. A more exhaustive study was later made by Alvarez (1949), and the pioneering work of these two men provided the basis for experiments by Davis in the mid-1950's using a chlorine receptor in an attempt to provide experimental confirmation of the existence of neutrinos. That receptor was much smaller than the one he would later employ in the solar neutrino experiment.

The substance used in both Davis' earlier and later experiments, perchloroethylene, C_2Cl_4, is in common use by dry clearners; it is plentiful and cheap.[9] The product, argon, is a rare gas, and therefore exists in solution in the liquid and is easily separable; the particular isotope formed is radioactive, with a convenient half-life. Appropriate counters exist, and, as we shall see momentarily, background effects can be estimated with confidence and are found to be small. I will briefly review three of the many considerations leading to confidence that the experiment does in fact provide significant information about processes in the center of the sun. These considerations can be viewed as answers to three basic questions about the reliability of the experiment.

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[9] With the clear conflict between theoretical prediction and observational result, and recognition of the fundamental importance of that conflict, what is considered "not too expensive" has undergone some revision. A new $25,000,000 experiment is now actively under consideration, using the rare and expensive substance gallium as capture material.

(i) *Can neutrinos from the sun be captured in great enough numbers to provide significant results*? The answer to this question lies in the calculation of the probability that an incoming neutrino of a given energy will undergo inverse beta decay with chlorine. Calculations in early 1963, taking into account only the then-known values for ground-state transitions from ^{37}Cl to ^{37}Ar, yielded a prediction of only about one neutrino capture per day in the proposed 100,000-gallon tank—too few (Bahcall, Fowler, Iben, and Sears 1963). But later that year, when Bahcall included transitions to excited states of ^{37}Ar (and especially a "superallowed" transition from the ground state of ^{37}Cl to a particular excited state of ^{37}Ar), the expected rate was dramatically increased. Even so, a capture rate of only about six per day was predicted; but that was sufficient, given the efficiency of the experiment, to provide meaningful information.[10] Because only the neutrinos from ^{8}B were energetic enough to feed the crucial "analogue" state of ^{37}Ar, those neutrinos from the sun would be expected to produce 90% of the reactions in the tank, even though they were believed to constitute only a small proportion of the total solar neutrino output.

(ii) *Would background effects be significant*? Prior knowledge indicated that radioactive ^{37}Ar nuclei can be produced by three other processes to which the tank is subject:

(a) protons produced by cosmic-ray muons through the reaction

$$^{37}\text{Cl} + \text{p} \rightarrow {}^{37}\text{Ar} + \text{n}$$

(p: proton; n: neutron);

(b) fast neutrons from radioactive decay of uranium and from the effects of alpha particles in the surrounding rock through the reaction

$$^{35}\text{Cl} + \text{n} \rightarrow {}^{35}\text{S} + \text{p}$$

followed by the reaction schematized under (a); and

(c) alpha particles produced from radioactive contamination of the receptor itself, through either of two known processes.

Thus the very atoms to be counted might be produced by processes other than those being investigated. The mile depth of the mine could be expected to provide adequate safeguard against the effects of muons. As an additional check, cosmic ray counts at various depths above the mine have been made; muon counters now surround the tank. Sulphur, whose

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[10] Bahcall (1964). Bahcall's article was followed by Davis (1964); these were the first published accounts showing the feasibility of the experiment.

presence could lead to production of ^{37}Ar, is present in the tank only in a proportion of one part per million; there are no fast neutron sources in the tank. With these precautions (some of which were present from the beginning, while others have been added over the years as the actual counting rate was found to be low and background effects correspondingly more important to control), the background effects are expected to be no more than about 0.5 captures per day.

Neutrinos from sources other than the sun (e.g., other stars; cosmological background) are calculated to be, normally, of too low energy to be captured. However, occasional celestial events of kinds that might be expected (e.g., collapse of a star to form a highly compact object) could produce short bursts of neutrinos of sufficient energy to be registered in Davis' detector. Such an explanation has been proposed for the anomalously high count found in Run 27 of the experiment.

(iii) *How efficiently can the ^{37}Ar atoms be retrieved and counted?* First, is the argon really free, existing in solution but not in any way tied to other atoms or molecules? After the low detection rates were coming in, it was suggested that perhaps the argon, after its production in the capture process, remains an ion, and becomes attached to a perchloroethylene molecule, or perhaps becomes ''caged'' in the molecule in which it was produced (Jacobs 1975). Both these possibilities have been shown to be highly unlikely. Though on capture of a neutrino by a ^{37}Cl nucleus, the ^{37}Ar is indeed probably ionized, argon is known to have a very high ionization potential, which means that it will, with very great probability, extract an electron from a neighboring molecule and become a neutral atom. Caging has been shown to be even less likely. Experiments have also shown that argon does not form stable compounds with chlorine.

Even given the freedom of argon, however, the difficulties of the extraction process might appear at first glance to be awesome. In this experiment, we are dealing with on the order of 200 ^{37}Ar atoms per run (expected rate; the actual capture rate is much smaller) to be retrieved from a tank the size of an olympic swimming pool—ten railroad tank cars of liquid, containing on the order of 2.2×10^{30} atoms of ^{37}Cl. How efficient is the method described earlier, of bubbling helium through the tank to carry out the dissolved argon? Several independent tests of the extraction efficiency have been performed. For example, 500 atoms of ^{37}Ar were added to the tank, and by thorough purging six times with 0.42×10^6 liters of helium over a 22-hour period, it was found that 95% of the argon atoms were recovered. An analogous procedure (but with much more argon, and of course using a stable isotope—one-tenth cubic centimeter of pure argon 36 or 38 to serve as carrier of the argon 37) is gone through each time the tank is purged. The efficiencies obtained are more than adequate. Similarly, the separation of the argon from the helium, and the efficiency of the counting mechanism, have been shown to be well within the range of efficiency required to furnish significant results.[11]

III

The preceding survey of the solar neutrino experiment indicates that prior information plays an extensive role in determining what counts as an "observation" in that case—as astrophysicists use that term. It is now necessary to provide a more general account, based on that survey, in order to deal with the issues raised in Part I, above. That general account will depart from the following considerations.

The body of physical science includes assertions about the existence of entities and processes which are not accessible to the human senses—assertions which involve the claim that those senses are receptive to only a limited range or type of events which form part of an ordered series of types of events, namely (to restrict ourselves only to the visual sense), the electromagnetic spectrum, ranging from extremely high-frequency gamma rays, with wavelengths as short as a billionth of those to which the eye is sensitive, to very long radio waves, of the order of a billion or a trillion times the wavelength of visual light. Thus a total range in wavelength of roughly the order of 10^{22} is encompassed, of which a range of only about 10^{-19} of the entire known range is accessible to human vision. The eye thus comes to be regarded as a particular sort of electromagnetic receptor, capable of "detecting" electromagnetic waves of the "blue" to "red" wavelengths, there being other sorts of receptors capable of detecting other ranges of that spectrum. This *generalized notion of a receptor or detector* thus includes the eye as one type.

But a further generalization of the notion of a "detector" or "receptor" is possible. For besides electromagnetic interactions, current physics recognizes three other fundamental types: the so-called "strong" interactions (responsible for holding atomic nuclei together, among other things), the "weak" interactions (which we have seen to govern the behavior of neutrinos), and the more familiar gravitational interactions.[12] The generalization of the notion of a "receptor" is made in the light of

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[11] A more detailed survey of these three questions, as well as other safeguards of the reliability of the experiment, are given by Davis (1978).

[12] Work in the last several years has begun to reveal deeper unities between at least some of these forces or interactions. The weak and electromagnetic interactions have been successfully incorporated into a unified theory, and work is progressing in the direction of joining this "electroweak" with the strong interaction. (Despite some progress, gravitation remains the most isolated of the four.) However, under the ordinary circumstances of the present universe, the unity of the fundamental interactions—even those for which an integrated theory exists—is in fact "broken", so that we are justified in speaking here of four operative forces.

the existence of these further sorts of interactions: an ''appropriate receptor'' can now be understood in terms of the instrument which is able to detect the presence of such an interaction, and therefore of the entities interacting according to the precise rules or laws of current physics.

Thus the extension of knowledge has led to a natural extension of what is to count as observational: the very fact that information received by the eye becomes subsumed under a more general type of information leads to the treatment of the eye as a particular type of receptor of that information. Further discovery that that type of information (electromagnetic) is only one of four types of information leads to a further generalization. It also produces a clarification of the concept of ''information'' relevant in examples like the neutrino case: for the four fundamental types of interaction lead to there being, as of the present epoch in physics (subject to the qualification in Footnote 12), four fundamental types of information emitted by objects; those same four types of interaction also govern the reception of that information. And the laws of current physics (the laws of the relevant type of interaction) also govern the sense in which that ''information'' *counts as information*: that is, how, and the extent to which, and the circumstances under which, the receptor-information can be used by us to draw conclusions about the source. The conditions under which such conclusions can be drawn are expressed, where observation is concerned, in the two conditions stated earlier as to when an object can be said to be ''directly observed''. For these purposes, however—as again we have seen in the solar neutrino case,—knowledge of the four fundamental types of interaction is by itself insufficient to permit the drawing of conclusions about the source—to permit us, that is, to say that a direct observation has taken place. Whatever one might say about the possibility of ultimately deriving all knowledge from fundamental theory, it remains the case that in the present state of science, knowledge of fundamental theory must be supplemented by other information. Both general laws about the *kind* of object the source is (in our case, the general laws of stellar structure) and specific information about the particular object (in our case, ultimately the mass and distribution of chemical composition, though in practice less fundamental information) must be added to the theory of the source. (The *kind* of specific information that is needed is known in a general form—in our case, it is embodied in the Russell-Vogt theorem.) A combination of fundamental theory and other knowledge, both general and particular, also plays a role in the theories of the transmission and of the receptor of the information. In the latter, for example, we must employ theories of nuclear reactions, experimental determination of reaction rates, cosmic ray physics, the chemistry of noble gases, the properties of cleaning fluid, information about the radioactive content of the rock walls of the cave in which the receptor is located, technological information as to how to air-proof the apparatus (and theoretical information as to why this must be done), technological information about the capabilities of radioactive-decay counters, both in general and in reference to idiosyncrasies of the individual counters employed, and much else, including information so specific that I did not even mention it in my account in Part II (e.g., methods of cleaning the tank before filling it). In all three components of the ''observation-situation'', moreover, the kinds of errors and inaccuracies to which the information is or may be subject is also given by current knowledge (in our case, for example, the range of uncertainties in reaction rates, and the types of background interference that might lead to the production of unwanted ^{37}Ar in the Davis tank, and how to overcome the dangers thus posed).

Thus we may say that what we have learned about the way things are has led to an extension, through a natural generalization, of what it *is* to make an observation, and furthermore that various relevant aspects of that knowledge are *applied* in making specific observations. It would therefore be a mistake to say that there is *no* connection between the astrophysicist's use of the term 'observation' with reference to this experiment and uses (at least certain ones) of that term which associate it with sense-perception; nor can the relation between the two sorts of uses be dismissed by saying that the astrophysicist speaks of observation only ''by analogy'' or ''metaphorically''. Rather, the relation lies in the fact that the astrophysicist's use is a generalization of (certain) uses having to do with sense-perception, and in the fact that whatever reasoning has led to our current understanding of the electromagnetic spectrum, the fundamental interactions (forces) of nature, and the means of receiving information conveyed by the entities and processes we have found to exist, functions also as *reasoning* leading to the generalization. The generalization, that is, is not made capriciously, arbitrarily, but rests on reasons.

But there is a still more general point—also of a rational sort—behind the scientist's extension of the concept of observation, and it is this further point that brings out the contrast between his ''observation'' and that with which the philosopher is usually concerned—that brings out, that is, the way in which the astrophysicist's usage *departs from,* and is not merely a generalization of, the usage or usages the philosopher has in mind. The philosopher's use of the term 'observation' has traditionally had a double aspect, and has played a double role. On the one hand, there is the *perceptual* aspect: ''observation'', as a multitude of philosophical analyses insist, is simply a special kind of perception, usually interpreted as consisting in the addition to the latter of an extra ingredient of focussed attention. ''The problem of observation'' is thus seen as a special case of ''the problem of perception'', to be approached only in the light of an understanding of the latter. On the other hand, there is the *epistemic* aspect of the philosopher's use of 'observation': the *evidential* role that observation is supposed to play in leading to knowledge or well-grounded belief or in supporting beliefs already attained. For the empiricist tradition in epistemology proposed that all knowledge (or well-grounded belief) "rests on experience", where "experience" was interpreted as sense-perception. In that tradition, as indeed in most other philosophy, these two roles have been identified: the question of observational support for beliefs or knowledge was interpreted as the question of how *perception* could give rise to knowledge or support beliefs.

In sophisticated areas of science, however, these two aspects have come to be separated, *and for good reason.* Science is, after all, concerned with the role of observation as evidence, whereas sense-perception is notoriously untrustworthy (in specific and rather well-known ways; the non-specific way or alleged way that leads to philosophical skepticism is completely irrelevant here). Hence, with the recognition that information can be received which is not directly accessible to the senses, *science has come more and more to exclude sense-perception as much as possible from playing a role in the acquisition of observational evidence;* that is, it relies more and more on other appropriate, but dependable, receptors. It has broken, or at least severely attenuated, the connection between the perceptual and epistemic aspects of "observation", and focussed on the latter. And this is only reasonable in the light of the primary concerns of science: the testing of hypotheses and the acquisition of knowledge through observation of nature.

It is true that there remains an important role for sense-perception in the acquisition of scientific knowledge. For after all, it is *we* human beings who have set up the "appropriate receptor", *we* who will use the received information as information. It follows that whatever information is received through the "appropriate receptor" must be transformed, in a final segment of the apparatus, into humanly-accessible form. Thus if the information comes in the radio region of the electromagnetic spectrum, or via weak interactions, it must be transformed into electromagnetic information in the visual wavelengths, or into audible clicks, or into readable printout, or the like. But we must be clear as to exactly what is involved here: the human perceiver need not be present when the information is received by the "appropriate receptor", nor need he even be present at the time the information is converted into humanly-accessible form. It has been an unquestioned assumption of the philosophical tradition that, for an "observation" to take place, the perceiver (human being) must be present when and where the information is received, and in a state and under circumstances in which he is capable of receiving that information. But as we see in the solar neutrino case, this assumption need not be satisfied. The counts of neutrino-reception, and a great deal of their interpretation, are made and recorded by computers and other electronic devices. In principle, a human perceiver need not drop by to pick up the information for years. Yet it still counts as observational. Though he has set up the receptor for the purpose of advancing his own knowledge, the human perceiver plays the role of a mere user of the information received and recorded. That is the sole remaining link between observation, in its role as evidence, and sense-perception, at least in the solar neutrino case.

But it is a link; and certainly any account of the knowledge-seeking enterprise must recognize our role as seekers of knowledge, as utilizers of information. Furthermore, unless we note that it is we who set up the experiment for the purpose of ''probing'' nature, the two conditions laid down earlier for the astrophysicist's use of 'x is directly observed (observable)' would fail to distinguish such interactions, as being *observations,* from all other interactions which, while they might be used for informational purposes (as in using the size and shape of a crater to draw conclusions about the size and direction of arrival of a missile), have not been set up with the explicit intention of gaining information, and which therefore presumably would not be spoken of as ''observations'' (of the missile). It might therefore be suggested that we recognize the importance of this point by explicitly formulating it as a third condition for 'x is directly observed (observable)', along the following lines:

> (3) the information is transformed by appropriate devices into humanly-accessible information which is (eventually) perceived (and used appropriately as information) by a human being.

But whatever the merits of this suggestion, it must be rejected: the fact that *we use* certain information in certain ways for certain purposes—as observations in the role of evidence—must be kept separate from the scientific (epistemic) considerations which lead to its being taken as observational evidence. For after all, the mere fact of our use of observation says nothing about what observation is, in the sense of saying how what counts as observational is shaped by the rest of our knowledge or well-founded beliefs. The reasons that make certain interactions, and certain receptors, appropriate for use in an observation-situation—that determine what can count as observational—are scientific, and are given by the information detailing (1) and (2); even the problem the observation is to deal with is, in sophisticated science, posed in the light of current knowledge or well-founded belief. Our use of that information, *at least insofar as that use is scientific,* is determined by those considerations; and it is therefore those considerations; that reasoning, that must be emphasized if we are to bring out the role of observation in science.

That conditions (1) and (2), taken by themselves, do not distinguish observations from other interactions is thus not a sign of their failure; on the contrary, *it is precisely the assimilation of observation to the general category of ''interactions'', and not its use by us, that constitutes the important point in understanding the role of observation in the search for knowledge and the testing of beliefs;* for that assimilation reflects the fact that ''observation'' has been, or at least has moved far toward being, integrated with the larger body of our best-warranted beliefs about nature. It is that process of integration that frees observation from the subjectivity of its philosophical associations and the unreliability of its ordinary ones; it is thus central to grasping what is involved in the ''objectivity'' of the search for knowledge and the justification of belief.[13] The distinction between observation and other interactions lies only in the use we make of certain interactions which we have found to occur in nature; and that distinction can be better made by calling attention to that use separately than by adding to (1) and (2) a further condition whose difference from the others would still have to be emphasized to prevent its obscuring their fundamental importance.

The conflation of the perceptual and epistemic aspects of observation is not unique to philosophers; it is also found in ordinary usage, and understandably, since ordinary people ordinarily observe (= acquire evidence) by perceiving (attentively, probingly, or however). The paradoxical sound of the astrophysicist's talk of direct observation of the center of the sun is thus not without basis; but neither can it be said that the astrophysicist is using the term 'observation' and its cognates in a way that is arbitrary, much less unrelated to its ordinary and philosophical uses. On the contrary, as we have seen, his is a generalization of and departure from those uses, focussing on the epistemic and suppressing the perceptual aspects, and is based on reasons, on what science has found to be the case in nature.

Some might even yet want to insist that we ought not refer to the activity I have been describing as ''observation''. Why not speak of it as a process of ''detection'', for example, or of ''obtaining experimental evidence'', *rather than* as one of ''observation''? My initial temptation is to answer as follows: call it what you like as long as you remember

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[13] Elsewhere—e.g., in (1981 and 1982)—I have referred to this process as one of ''internalization'' of the considerations adduced for and against beliefs, and have connected the process with that of the development of the concept of a ''reason''.

It should be remarked that the possibility of using information for the advancement of our knowledge in the ways I am describing is itself a matter of fact, and is not (nor does it rest upon) an *a priori* or necessary truth or assumption. Indeed, conditions (1) and (2) themselves are the products of experience and find their justification there.

These points will be developed more fully in the book of which the present paper is a part.

the roles that activity performs (its functions in the knowledge-seeking and belief-testing enterprise) and its relations to other activities and concepts (its reasoned departure from ordinary associations with perception). But there is more to say: for a slightly different perspective on these same points may deter those who would recommend not speaking of observation in the way the astrophysicist (and I) have been. First, although the astrophysicist's usage is a departure from the ordinary, it is a *reasoned* departure, characteristic, in that regard, of the departures science so often leads us to make in our beliefs. Second, its being a departure does not lessen the fact of its *relation* to what is ordinarily spoken of as "observation" (when it is related to perception): it is in part a generalization of that concept; and that relation, too, is one of rational descent. And finally, this "detection", as we are enjoined to call it, performs the very same primary epistemic roles assigned to observation by the empiricist tradition and at least some aspects of ordinary usage: of being the basis of testing beliefs and of acquiring new knowledge about nature. Indeed, it performs those roles *better* than they could have been performed without the "background knowledge" that science has accumulated and that enters into scientific observation. There is thus abundant reason for considering the word 'observation' to be appropriate in the contexts I have been discussing.

As a matter of fact, terms like 'detection' and 'experimental evidence', far from being alternatives to 'observation' in contexts like these, are closely related in their uses to that of the term 'observation'. An experiment is a situation, consisting of an appropriate receptor, set up to obtain an observation, in order to test a hypothesis or to gain information (currently unknown or more detailed) about some object or process (or perhaps to discover some new object or process). The word 'detection' (and 'detector', which I sometimes use as an alternative to 'receptor') emphasizes, as its etymology indicates, that something is being "uncovered", revealed. ('Receptor' emphasizes that that something is intercepted, captured, and comes from elsewhere. Since my interest is in the role of what is thus captured as information about the source from which it comes, I have generally preferred the term 'receptor' to 'detector'.) The "probing" aspect found in some ordinary uses of 'observation' is, of course, retained and accentuated in this scientific usage. It is thus no surprise that, in contexts where epistemic considerations are primary, as in the quotations given at the beginning of this paper, such terms should often be found in association with 'observation'.

In the expression 'directly observed' as it occurs in the context of the solar neutrino experiment, the implied contrast between 'direct' and 'indirect' is to be understood in terms of the contrast between claims about the center of the sun made on the basis of neutrino reception and those made on the basis of photon reception. Those made on the basis of the latter are, as we have seen, *inferential* in a very clear sense determined by the physical properties of photons and the conditions of their passage through the sun to its surface where they can be "directly observed". Although claims about the center of the sun based on photons, like those based on neutrinos, are "based on observation", the sense in which they are so based is not that of being "directly", but only "inferentially" so based. But since the operative contrast here is between "observational" and "inferential", the term 'direct' in 'direct observation' has the function only of emphasizing that conclusions about the source are being arrived at by observation, and not by inference based on observation. (That is, the idea of "indirect observation" plays no role at all.) I will return to the contrast between observation and inference later.

It is not the aim of the present "analysis" to give a set of conditions sharply defining the boundaries of applicability of the term 'observation'. Such an attempt would in any case be misguided; for one of the major implications of this essay is that, where our talk about nature is concerned, "meaning" and "knowledge" are not as separable as philosophers have often supposed. Our ways of talking about nature are intimately intertwined with our best-warranted beliefs about it. It follows that, where knowledge is incomplete, we cannot expect to find sharp boundaries of usage. But it also follows that, where knowledge is accumulated piecemeal, we cannot expect usage to be uniform either; and therefore I do not claim that the analysis I have given of 'observation' and its cognates as used in the context of the solar neutrino experiment necessarily applies, in all its details, to all cases of scientific use of the term. There are many areas of science where the everyday strong link between observation and perception is retained. Even in areas where it is not, the exact form of the departure from ordinary usage may be different in important respects from what it is in the case I have considered. (This is a major reason why the conditions stated earlier for 'x is directly observed (observable)' were given only as sufficient ["if . . ."] rather than as necessary and sufficient ["if and only if . . ."].) In elementary particle physics, for example, the tracks our instruments record are not separated from their "source" by the vast distances we have in astronomy, and the distinction between "theory of the source", "theory of the transmission", and "theory of the receptor" must be modified accordingly. The Heisenberg indeterminacy relations play an important role, as do the field-theoretic aspects of the interacting particles; the relation between observing instrument ("receptor") and observed entity ("source") assumes special characteristics because of quantum-theoretic considerations. Though in some fundamental sense such considerations (as far as we know) must be applicable in all cases, the problem of observation in quantum-theoretic contexts involves so many special complexities and difficulties of interpretation that any more generally illuminating features of observation are lost in the details. In the case of the solar neutrino experiment, on the other hand, the reasoning is not only unusually clear and unambiguous, it also contains features which are characteristic of the role of observation in wide ranges of scientific inquiry. Despite differences of detail, for example, the relations between receptor and source in this case are paralleled in such cases as telescopic, spectroscopic, and photographic reception of photons from distant objects. And the tendency toward objectivization of observational evidence is an ideal, if not a realization, throughout the scientific enterprise. In what follows, we shall find still more aspects of this case that can be expected to throw light on the role of observation in science.[14]

IV

We have seen, in the case of the solar neutrino experiment, the pervasive role played by what may be called "background information". It should be clear that this observation-situation could never have been set up had that background information, or a very large part of it, not been available. No doubt some of the information justifying the claim that an observation had been made or could be made might have been obtained *after* the observation was established as possible. The relevant chemistry of argon, for example, was in fact determined when the experiment had been in progress for some time. But without such ingredients as weak interaction theory, experimental information about reaction rates, the theory of stellar structure, knowledge of the properties of rare gases, the technological capabilities of existing proportional counters, and so forth, the experiment would not only have been impossible to perform, it would have been, in the most literal sense, inconceivable. It is thus that science builds on what it already knows, even where its observational capabilities are concerned. It *learns how* to observe nature, and its ability to observe increases with increasing knowledge (or decreases when it learns that it

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[14] Two special features of the solar neutrino experiment should be mentioned. First, it does not provide information about the direction from which the captured neutrinos come. This fact does not by itself, however, prevent the experiment from constituting an "observation of the solar core"; for, given our knowledge that the energy of background neutrinos is (normally) below the threshhold of the apparatus (and our knowledge of the relative insignificance of other background effects), that is the only place they *could* be coming from. In some possible solar neutrino experiments, including some now actively being contemplated, the direction of the incoming neutrinos will be ascertainable.

Secondly, there are subtleties about the notion of "observation" in this case because the expected neutrinos from the sun have *not* been observed. (The actual capture rate is consistent with *no* neutrinos having been received from that source.) Those subtleties, however, do not affect the basic points made in this paper, and I will pass over them here.

was mistaken in some piece of background information it employed). In the process of acquiring knowledge, we not only learn about nature, we also learn how to learn about it, by learning (among other things) what constitutes information and how to obtain it—that is, how to observe the entities we have found to exist, and the processes we have found to occur in nature.

The employment of background information in science—indeed, the necessity of employing it—has been termed by some philosophers the ''theory-ladenness'' of observation. In conformity with the mainstream of philosophical discussion, they have tended to treat that subject as that of the ''theory-ladenness'' of perception, a tendency that has obscured many of the real issues involved in scientific change, and much of their discussion (for example, about ''gestalt switches'' in the history of science) has been irrelevant to those issues. But let us now pass beyond those confusions and turn to yet another, this time arising out of the term 'theory-ladenness' itself. For putting the matter in such terms has led to a great deal of perplexity: does not the ''loading'' of observation amount to slanting the outcome of experiment? And does not such slanting imply that scientific testing is not objective, and indeed that what science claims is knowledge is only fashion or prejudice? Such perplexities, and the epistemological relativism they engender, trade in part on an ambiguity in the term 'theory'. For on the one hand, that term *is* used to refer to the background information which enters into the conception of an observation-situation.[15] But on the other hand, it is also often used in reference to what is uncertain (as when someone says, disparagingly, ''That's only a theory.'') Collapsing these two senses leads to thinking of background information in science as uncertain, and from there by various familiar paths to considering it arbitrary. But though it is true that the background information employed in science is *not certain* (in the sense that it could be mistaken, and in the sense that it involves a range of possible error), it is not for that reason *uncertain* (in the sense of being highly shaky or arbitrary). For wherever possible in its attempt to extract new information, what science uses as background information is the *best* information it has available—speaking roughly and in an idealized way for present purposes, but nevertheless appropriately, information which has shown itself highly successful in the past, and regarding which there exists no specific and compelling reason for doubt. (We learn what it is for beliefs to be successful, and what to count as a reason for doubt, and

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[15] However, in yet another sense of 'theory', in which what is ''theoretical'' is general and systematic, or deducible from general and systematic principles, not all background information can be classed as theoretical. For a great deal of it—as in the solar neutrino case—is of a quite specific character and is not deduced or, in the present state of knowledge at least, deducible from general principles.

when doubt is compelling in the sense of being serious enough to worry about.) But as we have seen in our study of the solar neutrino case, the range of possible error can (at least in best-case situations) be estimated with considerable accuracy and confidence, and can be taken into account in judging the possibility of extracting reliable information. Thus, in the solar neutrino case, solar models are constructed not only for *one* value of a parameter, the "most likely" one, but for the *range* of values which are judged reasonably possible in the light of what is known about the accuracy of the beliefs that are brought to bear in the construction of the models ("standard models").

Sometimes, however, the well-founded (successful and free from specific and compelling reasons for doubt) information brought to bear is insufficient to permit reliable information to be extracted. While *most* of the relevant background information is reliable in the sense just described (its error range being well-determined and narrow enough for the purposes at hand), some particular item of such information has too wide a range of error, or has one which is too poorly determined, to permit the conjunction of that item with the other, reliable items to yield significant information. In such cases, experiments may still be performed, in which some specific value (or range of values) is assigned to the "uncertain" parameter. And in *such* cases it makes sense to say that a *hypothesis* has been made in regard to that parameter.[16] The remaining information, successful and free from specific and compelling doubt, is taken as "knowledge"—knowledge *in the sense in which we have it.* If we call *that* information "hypothetical", we obscure the difference between it and the parameter which, in a clear and specific sense, *is* uncertain.

Calling all background information "hypothetical" or "uncertain"—calling it "theoretical" in the *second* of the two senses distinguished above—emphasizes that *all* our beliefs are "doubtful" in the sense that doubt *may* arise, and that that doubt *may* prove so compelling as to force rejection of the idea in question. But as we have learned in science (but perhaps not, alas, in philosophy), *the mere possibility of doubt arising is not itself a reason for doubt;* it is by itself *no reason* not to build on those beliefs which have proved successful and free from doubt, or regarding which the doubts that exist are either well-founded estimates of error ranges that are narrow enough, at least in some contexts, to permit useful investigation, or else are judged, on the basis of what we know,

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[16] Another sort of case in which the notion of "making a hypothesis" plays a working role (i.e. a role in which there is a working contrast between being and not being a "hypothesis") arises when the reasons for doubt of a proposition are of a qualitative rather than a quantitative sort. I will not discuss such cases here, however.

to be insignificant, not compelling, in some other specifiable way. (Indeed, on what else should we build?)

Thus the fact that what counts as "observational" in science is "laden" with background information does not imply that observation is "loaded" in favor of arbitrary or relative or even, in any useful sense, "uncertain" views. Nor does it imply that that background information cannot itself come to be subject to specific doubt and rejected. Though it is not my purpose in this paper to examine the testing of theories, I will just remark that, when specific reason for doubt does arise, as it has in the solar neutrino experiment, the background information will become subject to question. (See the discussion of "non-standard" proposals for solution of the solar neutrino problem at the end of Part II, Section 1, "The Theory of the Source", above.) There will in general be an at least rough ordering of that information with regard to what it is reasonable to question first and what it is reasonable to question only later, when other, more likely possibilities have failed.[17] The employment of background information, far from being a barrier to the acquisition of knowledge about nature, is the means by which such further information comes to be attained.

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[17] This comment is, of course, directed against Duhemianism. It is indeed a logical truth that, if the prediction of a conjunction of propositions is not borne out by observation, any one (or more) of the conjuncts may be rejected, logic itself giving no indication of which should be singled out for rejection. But what this shows is simply that formal logic does not exhaust what counts as reasoning in science; there, considerations do exist, in some cases at least, which suggest, sometimes even convincingly, that one rather than any other piece of background information is at fault.

Some philosophers (most vociferously, Feyerabend) find grounds for accusing science of subjectivity in the fact that the same theory is employed as background information in the theory of the source, the theory of the transmission, and the theory of the receptor of information. Thus, in the solar neutrino case, weak interaction theory is so employed. How, these philosophers ask, can we expect objectivity in science if the same theory is used in formulating the "hypothesis to be tested" (theory of the source) as in setting up the test of that hypothesis (theory of the receptor)? Indeed, we may add that, given the way we have found the world to be, it is *necessary* that this be the case: for all quantum-based theories imply certain symmetries between particle emission and particle absorption, and that implies, for example, that neutrino emission and capture are both described by the same theory (weak interaction theory). But this fact by no means makes it impossible that weak interaction theory might be questioned, modified, or even rejected as a consequence of the experiment. It is not a logical or necessary truth that it could be so questioned; but *as-a matter of fact,* we find that, despite the employment of the same theory in our account of both the source and the receptor, disagreement between prediction and observation results. And that disagreement could eventuate in the alteration or even rejection of weak interaction theory despite its pervasive role in determining the entire observation-situation. Feyerabend's argument supposes that conflict between prediction and observation will not, and perhaps could not, arise in such a case, and this mistaken idea is connected with the view that the world is a mere construction of our theories. What better proof that there is a theory-independent world could we ask for than the occurrence of such a conflict—the fact that there is "input" into the observation-situation which is independent of, and can conflict with, our theories?

These points enable us to dispose of yet another consideration that might lead some to question the appropriateness of the term 'observation' in contexts like that of the solar neutrino experiment. For surely, it might be argued, what is properly called *observation* should be wholly free of any *inference;* the latter consists of something added to, superimposed on, the former. Yet what the astrophysicist (and I) have been referring to as "observation" in the solar neutrino experiment obviously involves a great deal of inference. For example, in the theory of the source, we infer the chemical composition of the sun and its distribution in that body via complex calculations based on (among other things) the age of the sun, the theory of nuclear reactions, and the theory of stellar evolution, each of which is itself in turn the result of complex inferences. Therefore, according to this argument, the astrophysicist's usage is misleading, as obscuring an epistemically important distinction to which the philosopher, in his use of the term 'observation', is trying to call attention.

But in actuality it is the very contrary that is the case: it is the philosopher's usage, not the astrophysicist's, that obscures centrally important features of the difference between the inferential and the non-inferential in the search for knowledge. The philosopher, hypnotized by formal logic, views "inference" only in logical terms; and in the logical sense, the calculations and deductions involved in the solar neutrino case do have to be classed as "inferences"—as requiring the importation of background information ("premises", in the logician's way of viewing them) to make those calculations and deductions possible. But in the epistemically important sense—the sense which is central in the quest for knowledge—inference is spoken of rather in connection with reasoning and conclusions that we have specific reason to believe are doubtful; where the beliefs upon which we build are not subject to specific doubt, or at least to specific doubt which is significant enough to affect the needs or accuracy required in the problem at hand, the reasoning is not spoken of as "inferential". Thus in the case of electromagnetic information received from the surface of the sun, there *is* point in speaking of "inference" in connection with concluding from that surface information to conditions in the depths of the sun; and the reason we speak of "inference" in that connection is that we have specific reasons for being cautious about such conclusions. The *epistemically* important line between the non-inferential and the inferential is drawn in terms of the distinction between that which we have specific reason to doubt (but which we are nevertheless still able to use to a certain extent and for certain epistemic purposes) and that upon which we can build confidently. And this is just where one would expect the line to be drawn if we are trying to further our knowledge on the basis of what we have learned. (Of course, as I have said repeatedly, that on which we build confidently *can* always *be-*

come subject to actual specific and compelling doubt; our confidence *can* always turn out to have been misplaced.)

V

But there is still another problem with the philosopher's argument that, because ''inference'' (in the *logical* sense) is involved in what the astrophysicist calls ''observation'', it should not be so called. This further trouble has to do with the question of whether there is *any* epistemically relevant case in which an ''observational'' component can be distinguished which is in some absolute way free of any inference in the logical sense, that is, which does not require any antecedent belief in order to be useful in the quest for knowledge.

This problem brings us to the final point of this paper. Consider the following three sequences of descriptions of marks of various kinds on a photographic plate:

speck	dot	image	image of a star (or of a particular star)
smudge	streak	spectrum	spectrum of a star (or of a particular star)
scratch	line	track	track of an electron.

In each of these three sequences, as we move rightward, more ''background information'' is required.[18] Now certain philosophers have considered ''the problem of knowledge'' in something like the following way: we are to take as our starting-point the perceptual analogues of these dots, streaks, or lines (or perhaps specks, smudges, and scratches, or perhaps something still more, or even absolutely, ''neutral''), and try to see how we could pass from them, without use of any ''background beliefs'' whatever (whether claims to knowledge or otherwise), in the rightward direction of the sequence. But in the first place, that procedure is impossible (whether ''logically'' or ''historically''): considering the dot to be an image *requires* the importation of prior information or belief; dots and sense-data alike are too impoverished, by themselves, to serve even as potential bases for obtaining knowledge. Relevance to *being information,* and to serving as a basis for obtaining further information, too, is created by richness of interpretation, and scientifically *reliable* information is established by employing, as background information to establish that reliability, prior successful beliefs which we have no specific and compelling reason to doubt.

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[18] On a future occasion I will discuss in detail the general characteristics of the background information required in such transitions.

But in fact, in science (and indeed in ordinary life also), we do not "begin" (whatever that might mean) with the dots (or specks or sense-data) in our dealings with the world; *we use the vocabulary that is strongest given what we know* in the sense I have detailed. *It is only when specific reason for doubt arises (for example, when we find reason to think that what we have taken as an image of a star may be of a quasar or a galactic nucleus or a comet) that we withdraw our description to what is, with respect to the alternatives, the more "neutral" level of speaking of it only as an image (of something).* Further specific reasons for doubt may lead us to "retreat" again, to calling the mark a dot. And so forth, there being no clear reason to suppose that doubt might not arise at *any* level of description, whether our language is rich enough to provide a more neutral point of retreat or not. (In particular, no argument ever adduced by philosophers has shown that there is or must be some absolutely neutral level regarding which doubt *cannot* arise.) Thus the very problem of the sense-datum philosopher and his cousins is suspect: not only is his distinction between that which is *logically* inferential and non-inferential wholly beside the point where the quest for knowledge is concerned; it now begins to appear that that distinction cannot be applied to our descriptive language for any other purpose either, at least in the absolute sense the philosopher has in mind.

All this is only to say once again what has been said in Part IV, above: that we use our best relevant prior beliefs—those which have been successful and (at least in best cases) free from reasons for specific and compelling doubt—to build on. Only now we see a new application for that principle: for it holds just as well for our descriptions, and our vocabulary in general, as it does in the belief contexts discussed earlier. There would be *no sense* in describing a situation in a "weak" way (for example, in a way taken from the left part of one of the above sequences) when we have *no reason* to describe it in that way, and when all the reasons we do have make a stronger description appropriate. At best there could be only a humorous point, as when Calvin Coolidge, in response to the San Francisco mayor's boast that the cable cars before them had been painted in honor of the President's visit, replied with customary New England caution, "Yes, at least on one side". Coolidge no doubt did not intend his remark as a joke, but the fact that it is taken as one should at least jostle, if not embarrass, the chronic philosophical doubter. (Let me emphasize once more that, in speaking here, as elsewhere, of "reasons", I have in mind the sorts of reasons that have been found to be relevant in the actual knowledge-seeking enterprise: specific reasons, reasons directed at specific beliefs. I am not speaking of the kinds or alleged kinds lying behind philosophical skepticism—"reasons" for doubt which apply indiscriminately to any proposition whatever [equally to a proposition and its negation], and *which we have learned in science not even to consider as reasons*. Calling them ''reasons for doubt'' is just a misleading way of saying that, with regard to any specific claim, specific reasons for doubt *may* arise.)

The philosopher in question thus has the situation backwards: he sees the problem as one of how to justify moving rightward along description-sequences like the above; but it is impossible to proceed that way without background information, and in any case, in actual fact, both in science and in ordinary life, we proceed in the opposite direction—except, of course, when we can build rightward on the basis of what we have learned (that is, on the basis of background information which, at least in best cases, we have found to be trustworthy). In any situation, except when making a joke or in analogous (non-epistemic) cases, we approach a problem-situation with the strongest justified description, and only withdraw to less committal, more neutral ones when specific reason for doubt arises—and even then, we withdraw only as far as necessary with respect to the available reasonable alternatives.

There is an important moral to be drawn from this discussion. There are problems about the knowledge-seeking enterprise that have certain affinities with traditional philosophical concerns. For example, one important problem of the theory of *knowledge as we have it* is to try to understand the general character (insofar as there is a general character) of the reasoning by which science has extended our knowledge to regions of the electromagnetic spectrum beyond the visible (or, more generally, sensory) and to interactions besides the electromagnetic.[19] In dealing with that problem, we would be concerned—in common with much of traditional philosophy—with the lessons we could gain about the nature of reasoning in the search for knowledge; with how we are able to seek knowledge in an objective way and even occasionally to arrive at it; with the dispelling of conceptual confusions that lead to misunderstandings of that enterprise or to the denial of its possibility; and with other things as well. The problem even sounds like that of the philosopher we have been discussing: beginning with sense-perception, how do we extend our knowledge beyond it? The difference is that in the problem as I conceive it (and as is borne out by examination of the solar neutrino and other cases in science), we begin with sense-perception already infused with beliefs, some doubtful, some having the status of knowledge; and we try to determine how the knowledge-seeking enterprise does proceed, not

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[19] Such an investigation would depart from William Herschel's discovery of infrared radiation in 1800 and A. W. Ritter's discovery of ultraviolet radiation the following year, showing, respectively, that the energy radiated by a source like the sun extends beyond the red and blue ends of the visible spectrum.

how it would proceed in an imaginary situation in which we had nothing to rely on—a situation in which the seeking of knowledge would in any case be an impossibility.[20] The study made in this paper, limited though it is to an aspect of the development of the concept of observation, is both an example of and an argument in favor of the approach I have outlined.

The considerations I have put forth regarding the vocabulary with which we approach problems—namely, that we use the strongest vocabulary we are justified in using—apply as well to the allegedly "meta-scientific" term 'observation' as they do to, say, the "scientific" expression 'track of an electron': the relevant background information satisfying conditions of reliability as far as we have any reason to believe, we therefore say that we have made an observation (or at least would if the predicted interaction were to take place), and that that observation is of an entity, the central core of the sun (or of processes occurring there), rather than, in a spirit of skeptical caution without reason, saying that we have observed only absorptions of neutrinos in our apparatus, or only the decay of radioactive argon, or only the individual registrations of the proportional counter, or only the sense-data in the consciousness of a perceiver.[21]

The issues raised at the beginning of this paper have thus been resolved. The use of the term 'observation' by astrophysicists is not idiosyncratic or unrelated to certain aspects of ordinary and philosophical uses. Rather, it is an extension of such uses, in part a generalization

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[20] The problem as I have stated it has much in common with Quine's program of "naturalizing" epistemology (Quine 1969). Quine, however, often tends to emphasize what we have learned in psychology as illuminating the knowledge-acquiring process, rather than, as I have, the role of information directly relevant to the particular problem at hand. (What is relevant as background information in the attempt to extend knowledge in the neutrino experiment is not psychological knowledge, but rather the knowledge specifically relevant to the emission and reception of neutrinos under conditions existing in the sun, the Homestake gold mine, and the regions between.) My impression is that Quine's emphasis on psychology is a hangover from traditional approaches to epistemology via the problem of perception. In any case, he does recommend that we use whatever knowledge we have available to explain the process of gaining further knowledge, and I am completely in sympathy with that approach.

[21] These remarks must be interpreted carefully in the case of the solar neutrino experiment because, in that experiment, the predicted neutrinos from the sun have not unquestionably been observed. We have, that is, found reasons for doubting whether to say that we are observing processes occurring in the center of the sun, or whether we should "retreat" to saying that we are only observing neutrinos—not necessarily from the sun. (This point explains some of the apparently deviant ways, noted toward the beginning of this paper, which astrophysicists have of speaking of what is "observed" in this experiment.) Perhaps other retreats are in store; another, more sensitive experiment must be made. But on the other hand, we have not yet found reasons for doubting that we are at least observing neutrinos. These remarks are meant to be covered in the parenthetical qualification, "or at least would if the predicted interaction were to take place".

thereof, in part a departure therefrom, made on the basis of reasons, and designed to make the most of the epistemic role of observation. The philosopher of the sense-datum sort (at least) is dealing with a problem, *his* ''problem of knowledge'', which differs in crucial ways from that of the astrophysicist; but the former's problem is suspect, and in any case has no bearing on the knowledge-seeking enterprise as we engage in it, but conceives that enterprise in a way directly opposed to the way it is actually carried on, both in everyday life and in science. Nor is my criticism limited to sense-datum philosophers: *any* formulation of the problem of knowledge which conflates the problem of observation with the problem of perception, or which fails to recognize (and also to appreciate) the necessary role of background knowledge in the knowledge-seeking and knowledge-acquiring process—any formulation of it, in other words, which falls short of the ''naturalized'' way in which I have been dealing with it—will be a misconception which will fail to grasp important aspects of the scientific enterprise.

But in dealing with the issues which were raised at the beginning of this paper, we have gone far beyond them. For we have come to see that, and how, science builds on what it has learned, and that that process of construction consists not only in adding to our substantive knowledge, but also in increasing our ability to learn about nature, by extending our ability to observe it in new ways. These conclusions constitute an important step toward seeing how it is, after all, that all our knowledge rests on observation: a doctrine which is, as I hope I have shown, a rational descendant of traditional empiricism, generalizing and departing from what traditional empiricism considered to be the basis of knowledge, but generalizing and departing therefrom for good reasons; a doctrine which, while satisfying the deepest motivations of traditional empiricism—of accounting for the objectivity and rationality of the knowledge-seeking and knowledge-acquiring enterprise in terms of our interactions with nature—also succeeds, as traditional empiricism did not, in being faithful to that enterprise as we have learned to conceive and engage in it.[22]

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[22] In the book from which this paper is taken, these final remarks will be borne out in a discussion of the role of observation, as analyzed here, in the introduction, testing, and acceptance or rejection of scientific claims. There I will argue (among many other things) that no circle, vicious or otherwise, results from the conjunction of the view that all our knowledge rests on observation and the view (developed in this paper) that what counts as observational presupposes ''background'' information. Part of my argument can be summarized, if only very roughly, as follows. In a (purely hypothetical) situation in which we had no knowledge (or well-grounded belief) to rely on, we could still have *beliefs,* make *conjectures,* which could in further dealings with the world prove successful or unsuccessful. (We would also, of course, make conjectures as to what counts as success or failure.) Such successful beliefs, if they were attained, would then provide a point of departure for further building in the knowledge-seeking enterprise.

The possibility of so proceeding shows (as my argument will continue) that there is a correct insight in the philosophy of Karl Popper. However, his view will be found to be

APPENDIX*

The Proton-Proton Chain

Branch			*Reaction*	*Neutrino energy* (in MeV)
I (main)	Twice	(1)	$p + p \rightarrow {}^2D + e^+ + \nu_e$	c: 0 to 0.42
			or (for 0.25%):	
		(1a)	$p + e^- + p \rightarrow {}^2D + \nu_e$	1: 1.44
			and	
		(2)	${}^2D + p \rightarrow {}^3He + \gamma$	
		(3)	${}^3He + {}^3He \rightarrow {}^4He + 2p$	
II			After reactions (1) and (2):	
		(4)	${}^3He + {}^4He \rightarrow {}^7Be + \gamma$	
		(5)	${}^7Be + e^- \rightarrow \begin{cases} {}^7Li + \nu_e \\ {}^7Li^* + \nu_e \end{cases}$	1 (90%): 0.861 1 (10%): 0.383
		(6)	${}^7Li + p \rightarrow 2\,{}^4He$	
III			After reactions (1), (2), and (4):	
		(7)	${}^7Be + p \rightarrow {}^8B + \gamma$	
		(8)	${}^8B \rightarrow {}^8Be^* + e^+ + \nu_e$	c: 0 to 14.06
			or	
		(8a)	${}^8B + e^- \rightarrow {}^8Be^* + \nu_e$	1: 15.08
		(9)	${}^8Be^* \rightarrow 2\,{}^4He$	

1: linear energy spectrum (single line)
c: continuous beta spectrum
asterisk: excited state

γ: gamma radiation
D: deuterium
Li: lithium

A fourth theoretically admissible but improbable branch has never been observed, and is assumed not to occur.

*Adapted for present purposes from Kuchowicz (1976), p. 299, by permission of the Institute of Physics, Bristol, England, copyright holders.

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mistaken in supposing that a procedure of conjecture-and-refutation is the *only* one that could be employed under such circumstances. Far more importantly, though, his view errs in tacitly assuming that what is true of a hypothetical starting-point (whether logical or historical) of that enterprise is true of *all* stages or levels of it. That is simply not true, as I will argue: even if a procedure of conjecture-and-refutation were the only one used or usable in the alleged initial stages of knowledge-seeking (or the alleged foundation-levels from which knowledge is to be derived or constructed by "logical" means), once successful beliefs began to accumulate, they could be used as *positive* bases for introducing, testing, and accepting new ones. (To speak in more traditional but rather misleading terms, something analogous to "confirmation" procedures could then supplement "falsification" ones, and could even become the predominant methods of procedure; we learn how to learn.) Popper is far from being alone in assuming that the methods used by an epistemic analogue of a proposition-forming, logically-reasoning infant are the same ones as would be used at an epistemically maturer stage of the knowledge-seeking enterprise. Rather, such an assumption is characteristic of most traditional approaches to the understanding of that enterprise. But these are, as I have said, matters to be taken up more fully on a future occasion.

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