priori draw no inference, we are apt to transfer to inanimate objects, and to suppose that they have some such feelings whenever they transfer or receive motion. With regard to energies, which are exerted without our annexing to them any idea of communicated motion, we consider only the constant experienced conjunction of the events; and as we *feel* a customary connection between the ideas, we transfer that feeling to the objects, as nothing is more usual than to apply to external bodies every internal sensation which they occasion.

An Encounter with David Hume

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A DAY IN THE LIFE OF A HYPOTHETICAL STUDENT

In the physics 1a lecture hall, Professor Salvia[1] has had a bowling ball suspended from a high ceiling by a long rope so that it can swing back and forth like a pendulum. Standing well over to one side of the room, he holds the bowling ball at the tip of his nose. He releases it (taking great care not to give it a push). It swings through a wide arc, gaining considerable speed as it passes through the low portion of its swing beneath the point of suspension from the ceiling. It continues to the other side of the room, where it reaches the end of its path, and then returns. The professor stands motionless as the bowling ball moves faster and faster back toward his nose. As it passes through the midpoint of the return arc, it is again traveling very rapidly, but it begins to slow down, and it stops just at the tip of his nose. Some of the students think he is cool. "This demonstration," he says, "illustrates the faith that the physicist has in nature's regularity." (See Figure 1.)

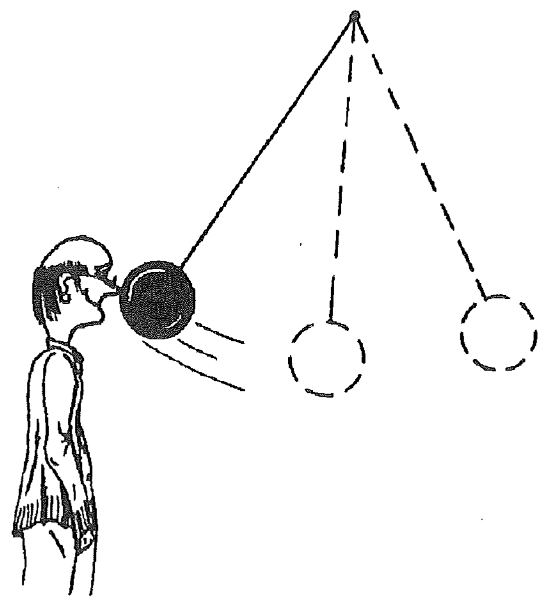

FIGURE 1 Prof Salvia's Pendulum. After swinging to the opposite side of the lecture hall, the bowling ball swings right back to the tip of the prof's nose, which remains motionless during the entire procedure.

Imagine that you have witnessed this demonstration just after your philosophy class, where the subject of discussion was Hume's *Enquiry Concerning Human Understanding*. You raise your hand. "How did you *know* that the bowling ball would stop where it did, just short of bashing your nose into your face?" you ask.

"This is a standard demonstration," he replies; "I do it every year in this class, and it has often been used by many other physics teachers." In an attempt to inject a little humor, he adds, "If I had had any doubt about its working, I'd have had the teaching assistant do it."

"Are you saying, then, that you trusted the experiment to work this time simply because it has been tried so many times in the past, and has never failed?" You recall Hume's discussion of the collisions of billiard balls. In the first instance, according to Hume, before you have any experience with material objects colliding with one another, you would not know what to expect when you see a moving billiard ball approaching a stationary one, but after a good deal of experience you confidently expect some motion to be transferred to the stationary ball as a result of the collision. As your experience accumulates, you learn to predict the exact manner in which the second ball will move after being struck by the first. But you cannot really accept that answer, and neither, you feel sure, will your physics professor. Without waiting for an answer, you follow up your first question with another.

"I have this friend," you continue, "who drives like a maniac. It scares me to ride with him, but he always tells me not to worry—he has never had an accident, or even a traffic ticket. Should I conclude—assuming he is telling the truth (just as I assume you are telling me the truth about this demonstration)—that it is as safe for me to ride with him as it is for you to perform the bowling ball trick?"

"It's not the same thing at all," another student chimes in; "you can prove, mathematically, that the pendulum will not swing back beyond its original starting point, but you certainly can't prove mathematically that your friend won't have a wreck. In a way it's just the opposite; you can prove that he is likely to have an accident if he keeps on driving like that."

"What you say is partly right," says Professor Salvia to the second student, "but it isn't only a matter of mathematics. We have to rely upon the laws of physics as well. With the pendulum we were depending mainly upon the law of conservation of energy, one of the most fundamental laws of nature. As the pendulum goes through its swing, potential energy is transformed into kinetic energy, which is transformed back into potential energy, and so forth. As long as the total amount of energy remains unchanged, my nose is safe."

Since you have not yet studied the concept of energy, you do not worry too much about the details of the explanation. You are satisfied that you will understand why the pendulum behaves as it does when you have learned more about the concepts and laws that were mentioned. But you do remember something Hume wrote. There are two kinds of reasoning: reasoning concerning relations of ideas, and reasoning concerning matters of fact and existence. Mathematical reasoning falls into the former category (relations of ideas) and consequently, by itself, cannot provide any information about matters of fact. The pendulum and the professor's nose are, however, matters of fact, so we need something in addition to mathematics to get the information we want concerning that situation. Professor Salvia has told us what it is—we need the laws of nature as well.

Since physics is your last class in the morning, you head for the cafeteria when it is over to get a sandwich and coffee. The philosophy class is still bugging you. What was it Hume said about bread? That we do not know the "secret power" by which it nourishes us? Now we do, of course; we understand metabolism, the mechanism by which the body converts food into energy. Hume (living in the eighteenth century) did not understand about power and energy, as he said repeatedly. He did not know why bread is suitable food for humans, but not for tigers and lions. In biology class, you recall, you studied herbiverous, carnivorous, and omniverous species. Biologists must now understand why some species can metabolize vegetables and others cannot. Modern physics, chemistry, and biology can provide a complete explanation of the various forms of energy, the ways they can be converted from one form to another, and the ways in which they can be utilized by a living organism.

Taking a sip of the hot coffee, you recall some other things Hume said—for example, remarks about the "connection" between heat and flame. We now know that heat is really a form of energy; that temperature is a measure of the average kinetic energy of the molecules. Now, it seems, we know a great deal about the "secret powers," "energy," etc., that so perplexed Hume. Modern physics knows that ordinary objects are composed of molecules, which are in turn composed of atoms, which are themselves made up of subatomic particles. Modern science can tell us what holds atoms and molecules together, and why the things that consist of them have the properties they do. What was it that Hume said about a piece of ice and a crystal (e.g., a diamond)? That we do not know why one is caused by cold and the other by heat? I'll just bet, you think, that Salvia could answer that one without a bit of trouble. Why, you wonder, do they make us read these old philosophers who are now so out of date? Hume was, no doubt, a very profound thinker in his day, but why do we have to study him now, when we know the answers to all of those questions? If I were majoring in history that might be one thing, but that doesn't happen to be my field of interest. Oh, I suppose they'd say that getting an education means that you have to learn something about the "great minds of the past," but why doesn't the philosophy professor come right out and tell us the answers to these questions? It's silly to pretend that they are still great mysteries.

After lunch, let's imagine, you go to a class in contemporary social and political problems, a class you particularly like because of the lively discussions. A lot of time is spent talking about such topics as population growth, ecology and the environment, energy demands and uses, food production, and pollution. You discuss population trends, the extrapolation of such trends, and the predication that by the year 2000 A.D., world population will reach 7 billion. You consider the various causes and possible effects of increasing concentrations of carbon dioxide in the atmosphere. You discuss solutions to various of these problems in terms of strict governmental controls, economic sanctions and incentives, and voluntary compliance on the part of enlightened and concerned citizens.

"If people run true to form," you interject, "if they behave as they always have, you can be sure that you won't make much progress relying on the good will and good sense of the populace at large."

"What is needed is more awareness and education," another student remarks, "for people can change if they see the need. During World War II people willingly sacrificed in order to support the war effort. They will do the same again, if they see that the emergency is really serious. That's why we need to provide more education and make stronger appeals to their humanitarian concerns."

"What humanitarian concerns?" asks still another student with evident cynicism.

"People *will* change," says another. "I have been reading that we are entering a new era, the Age of Aquarius, when man's finer, gentler, more considerate nature will be manifest."

"Well, I don't know about all of this astrology," another remarks in earnest tones, "but I do not believe that God will let His world perish if we mend our ways and trust in Him. I have complete faith in His goodness."

You find this statement curiously reminiscent of Professor Salvia's earlier mention of his faith in the regularity of nature.

That night, after dinner, you read an English assignment. By the time you finish it, your throat feels a little scratchy, and you notice that you have a few sniffles. You decide to begin taking large doses of vitamin C; you have read that there is quite some controversy as to whether this helps to ward off colds, but that there is no harm in taking this vitamin in large quantities. Before going to the drug store to buy some vitamin C, you write home to request some additional funds; you mail your letter in the box by the pharmacy. You return with the vitamin C, take a few of the pills, and turn in for the night—confident that the sun will rise tomorrow morning, and hoping that you won't feel as miserable as you usually do when you catch a cold. David Hume is the farthest thing from your mind.

HUME REVISITED

The next morning, you wake up feeling fine. The sun is shining brightly, and you have no sign of a cold. You are not sure whether the vitamin C cured your cold, or whether it was the good night's sleep, or whether it wasn't going to develop into a real cold regardless. Perhaps, even, it was the placebo effect; in psychology you learned that people can often be cured by totally inert drugs (e.g., sugar pills) if they believe in them. You don't really know what caused your prompt recovery, but frankly, you don't really care. If it was the placebo effect that is fine with you; you just hope it will work as well the next time.

You think about what you will do today. It is Thursday, so you have a philosophy discussion section in the morning and a physics lab in the afternoon. Thursday, you say to yourself, has got to be the lousiest day of the week. The philosophy section is a bore, and the physics lab is a drag. If only it were Saturday, when you have no classes! For a brief moment you consider taking off. Then you remember the letter you wrote last night, think about your budget and your grades, and resign yourself to the prescribed activities for the day.

The leader of the discussion section starts off with the question, "What was the main problem—I mean the really *basic* problem—bothering Hume in the *Enquiry*?" You feel like saying, "Lack of adequate scientific knowledge" (or words to that effect), but restrain yourself. No use antagonizing the guy who will decide what grade to give you. Someone says that he seemed to worry quite a lot about causes and effects, to which the discussion leader (as usual) responds. "But *why*?" Again, you stifle an impulse to say, "Because he didn't know too much about them."

After much folderol, the leader finally elicits the answer, "Because he wanted to know how we can find out about things we don't actually see (or hear, smell, touch, taste, etc.)."

"In other words," the leader paraphrases, "to examine the basis for making inferences from what we observe to what we cannot (at the moment) observe. Will someone," he continues, "give me an example of something you believe in which you are not now observing?"

You think of the letter you dropped into the box last night, of your home and parents, and of the money you hope to receive. You do not see the letter now, but you are confident it is somewhere in the mails; you do not see your parents now, but you firmly believe they are back home where you left them; you do not yet see the money you hope to get, but you expect to see it before too long. The leader is pleased when you give those examples. "And what do causes and effects have to do with all of this?" he asks, trying to draw you out a little more. Still thinking of your grade you cooperate. "I believe the letter is somewhere in the mails because I wrote it and dropped it in the box. I believe my parents are at home because they are always calling me up to tell me what to do. And I believe that the money will come as an effect of my eloquent appeal." The leader is really happy with that; you can tell you have an A for today's session.

"But," he goes on, "do you see how this leads us immediately into Hume's next question? If cause-effect relations are the whole basis for our knowledge of things and events we do not observe, how do we know whether one event causes another, or whether they just happen together as a matter of coincidence?" Your mind is really clicking now.

"I felt a cold coming on last night, and I took a massive dose of vitamin C," you report. "This morning I feel great, but I honestly don't know whether the vitamin C actually cured it."

"Well, how could we go about trying to find out," retorts the discussion leader.

"By trying it again when I have the first symptoms of a cold," you answer, "and by trying it on other people as well." At that point the bell rings, and you leave class wondering whether the vitamin C really did cure your incipient cold.

You keep busy until lunch, doing one thing and another, but sitting down and eating, you find yourself thinking again about the common cold and its cure. It seems to be a well-known fact that the cold is caused by one or more viruses, and the human organism seems to have ways of combating virus infections. Perhaps the massive doses of vitamin C trigger the body's defenses, in some way or other, or perhaps it provides some kind of antidote to the toxic effects of the virus. You don't know much about all of this, but you can't help speculating that science has had a good deal of success in finding causes and cures of various diseases. If continued research reveals the physiological and chemical processes in the cold's infection and in the body's response, then surely it would be possible to find out whether the vitamin C really has any effect upon the common cold or not. It seems that we could ascertain whether a causal relation exists in this instance if only we could discover the relevant laws of biology and chemistry.

At this point in your musings, you notice that it is time to get over to the physics lab. You remember that yesterday morning you were convinced that predicting the outcome of an experiment is possible if you know which physical laws apply. That certainly was the outcome of the discussion in the physics class. Now, it seems, the question about the curative power of vitamin C hinges on exactly the same thing—the laws of nature. As you hurry to the lab it occurs to you that predicting the outcome of an experiment, before it is performed, is a first-class example of what you were discussing in philosophy—making inferences from the observed to the unobserved. We observe the set-up for the experiment (or demonstration) before it is performed, and we predict the outcome before we observe it. Salvia certainly was confident about the prediction he made. Also, recalling one of Hume's examples, you were at least as confident, when you went to bed last night, that the sun would rise this morning. But Hume *seemed* to be saying that the basis for this confidence was the fact that the sun has been observed to rise every morning since the dawn of history. "That's wrong," you say to yourself as you reach the physics lab. "My confidence in the rising of the sun is based upon the laws of astronomy. So here we are back at the laws again."

Inside the lab you notice a familiar gadget; it consists of a frame from which five steel balls are suspended so that they hang in a straight line, each one touching its neighbors. Your little brother got a toy like this, in a somewhat smaller size, for his birthday a couple of years ago. You casually raise one of the end balls, and let it swing back. It strikes the nearest of the four balls left hanging, and the ball at the other end swings out (the three balls in the middle keeping their place). The ball at the far end swings back again, striking its neighbor, and then the ball on the near end swings out, almost to the point from which you let it swing originally. The process goes on for a while, with the two end balls alternately swinging out and back. It has a pleasant rhythm. (See Figure 2.)

While you are enjoying the familiar toy, the lab instructor, Dr. Sagro,[2] comes over to you. "Do you know why just the ball on the far end moves—instead of, say, two on the far end, or all four of the remaining ones—when the ball on this end strikes?"

"Not exactly, but I suppose it has something to do with conservation of energy," you reply, recalling what Salvia said yesterday in answer to the question about the bowling ball.

"That's right," says Dr. Sagro, "but it also depends upon conservation of momentum." Before you have a chance to say anything she continues, "Let me ask you another question. What would happen if you raised two balls at this

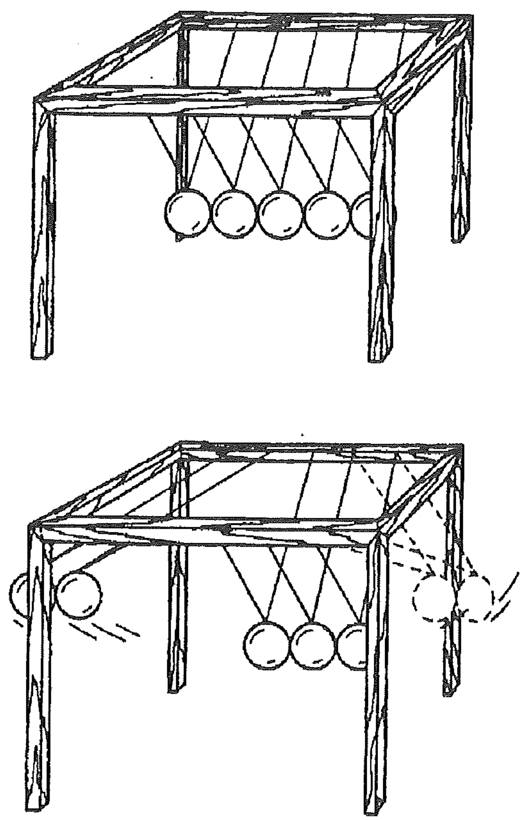

FIGURE 2 The Energy-Momentum Toy. When two balls at the right collide with the remaining three, two balls swing away from the left side. What happens when three on the right collide with the remaining two?

end, and let them swing together toward the remaining three?"

"I think two balls will swing away at the other end," you reply, remembering the way your brother's toy worked.

"Why don't you test it to find out if you are right?" says the instructor. You do, and you find that the result is as you had predicted. Without saying anything about it, you assume that this, too, can be explained by means of the laws of conservation of energy and momentum.

Dr. Sagro poses another question. "What will happen," she asks, "if you start by swinging three balls from this end?" Since there are only two remaining balls you don't know what to say, so you confess ignorance. She suggests you try it, in order to find out what will happen. When you do, you see that three balls swing to the other side, and three swing back again; the middle ball swings back and forth, acting as the third ball in each group. This was a case in which you didn't know what to expect as a result until you tried the experiment.[3] This was like some of Hume's examples; not until you have actually had the experience do you know what result to expect. But there is also something different. Hume said that you must try the experiment many times in order to know what to expect; nevertheless, after just one trial you are sure what will happen whenever the experiment is repeated. This makes it rather different from the problem of whether vitamin C cured your cold. In that case, it seemed necessary to try the experiment over and over again, preferably with a number of different people. Reflecting upon this difference, you ask the lab instructor a crucial question, "If you knew the laws of conservation of momentum and energy, but had never seen the experiment with the three balls performed, would you have been able to predict the outcome?"

"Yes," she says simply.

"Well," you murmur inaudibly, "it seems as if the whole answer to Hume's problem regarding inferences about things we do not immediately observe, including predictions of future occurrences, rests squarely upon the laws of nature."

KNOWING THE LAWS

Given that the laws are so fundamental, you decide to find out more about them. The laws of conservation of energy and momentum are close at hand, so to speak, so you decide to start there. "O.K.," you say to the lab instructor, "what are these laws of nature, which enable you to predict so confidently how experiments will turn out before they are performed? I'd like to learn something about them."

"Fine," she says, delighted with your desire to learn; "let's start with conservation of momentum. It's simpler than conservation of energy, and we can demonstrate it quite easily."[4] (See Figure 3.)

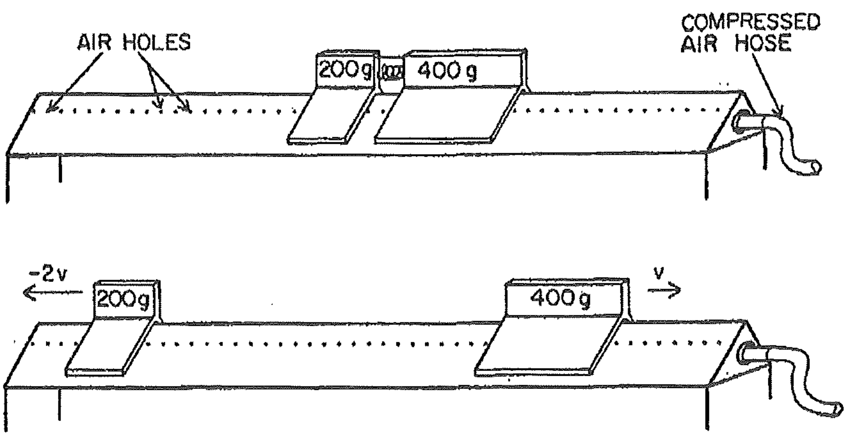

FIGURE 3 Cars on the Air Track. Top: Cars tied together against spring under tension. Bottom: Cars moving apart after "explosion." $400g \times v + 200g \times (-2v) = 0$. Momentum is conserved.

Your laboratory contains a standard piece of equipment—an air track—on which little cars move back and forth. The track is made of metal with many tiny holes through which air is blown. The cars thus ride on a thin cushion of air; they move back and forth almost without friction. Some of the cars are equipped with spring bumpers, so that they will bounce off of one another upon impact, while others have coupling devices which lock them together upon contact. Dr. Sagro begins by explaining what is meant by the momentum of a body—namely, its mass multiplied by its velocity.[5] "To speak somewhat quaintly," she says, "the mass is just a measure of the quantity of matter in the body.[6] Since, in all of the experiments we are going to do, it is safe to say that the mass of each body remains unchanged, we need not say more about it. You can see that each car comes with its mass labeled; this one, for instance, has a mass of 200 grams, while this one has a mass of 400 grams. We have a number of different cars with quite a variety of different masses. The velocity," she continues, "is what we ordinarily mean by 'speed' along with the direction of travel. On the air track there are only two possible directions, left to right and right to left. Let us simply agree that motion from left to right has a positive velocity, while motion from right to left has a negative velocity. Mass, of course, is always a positive quantity. Thus, momentum, which is mass times velocity, may be positive, negative, or zero. When we add the momenta of various bodies together, we must always be careful of the sign (plus or minus)."

With these preliminaries, you begin to perform a variety of experiments. She has various types of fancy equipment for measuring velocities, which she shows you how to use, and she also helps you to make measurements. You find that it is fun pushing cars back and forth on the track, crashing them into one another, and measuring their velocities before and after collisions. You try it with a variety of cars of different masses and with differing velocities. You try it with the ones that bounce apart after impact and with those that stick together. You always find that the *total* momentum (the sum of the momenta for the two cars) before any collision is equal to the *total* momentum after the collision, even though the momenta of the individual cars may change markedly as a result of the collision. This, Dr. Sagro explains, is what the law of conservation of momentum demands: when two bodies (such as the cars) interact with one another (as in a collision), the total momentum of the system consisting of those two bodies is the same before and after the interaction.

You ask her whether this law applies only to collisions; she replies immediately that it applies to all kinds of interactions. "Let's see how it works for a simple type of 'explosion,'" she suggests. She helps you tie together two cars, holding a compressed spring between them. You burn the string which holds them together and they fly apart. You measure the velocities and compute the momenta of each of the cars after the "explosion." It turns out that the momentum of the one car is always equal in amount but opposite in direction to that of the other. This is true whether the cars are of equal or unequal masses and whether the tension on the spring that drives them apart is great or small. "This is just what the law of conservation of momentum tells us to expect," she explains; "the momentum of each car is zero before the 'explosion' because they are not moving (each has velocity equal to zero), and so the two momenta after the 'explosion' (one positive and one negative) must add up to zero. That is what has happened every time.

"There are many other applications of the law of conservation of momentum," she continues. "When a rifle recoils upon being fired, when a jet engine propels an airplane, when a rocket engine lifts an artificial satellite into orbit, or when you step out of an untethered rowboat and are surprised to feel it moving out from under you—these are all cases of conservation of momentum."

"Is this law ever violated?" you ask.

"No," she answers, "there are no known exceptions to it." You leave the lab with the feeling that you know at least one fundamental law, and that you have seen it proved experimentally right before your eyes. You can't wait to tell your philosophy professor about it.

When you go to your philosophy class the next morning, the topic is still Hume's *Enquiry Concerning Human Understanding* and the problem of how we can have knowledge of things we do not observe. As the lecture begins, Professor Philo[7] is saying, "As we saw during the last lecture, Hume maintains that our knowledge of what we do not observe is based entirely upon cause and effect relations, but that raises the question of how we can gain knowledge of these relations. Hume maintained that this knowledge can result only from repeated observation of one type of event (a cause) to see whether it is always followed by an event of another kind (its effect). Hume therefore analyzed the notion of causality in terms of constant conjunction of events. Consider for a moment Hume's favorite example, the colliding billiard balls . . ."

You raise your hand. "It seems to me that Hume was wrong about this," you begin, and then you relate briefly yesterday's experiences in the physics lab. "If you know the relevant laws of nature," you conclude, "you can predict the outcomes of future experiments on the basis of a single trial, or perhaps even without benefit of any trials at all."

"But how," asks Professor Philo, "can we establish knowledge of the laws of nature?"

You had a hunch she might ask some such question, and you are ready with your reply, "We *proved* it experimentally."

"Well," says Professor Philo, "I'm not a physicist, so perhaps you had better explain in a little more detail just what the experimental proof consists of. You mentioned something about an explosion—how did that go?"

You explain carefully how the air track works, how the two cars were joined together with a spring under tension in between, and how they moved apart when the string was burned. "In every case," you conclude, "the momentum of the two cars was equal in amount and opposite in direction, just as the law of conservation of momentum says it should be."

"Now let me see if I understand your line of reasoning," says the professor in a tone that is altogether too calm to suit you. "If the law of conservation of momentum is correct, then the two cars will part in the manner you described. The cars did move apart in just that way. Therefore, the law of conservation of momentum is correct. Is that your argument?"

"I guess so," you reply a bit hesitantly, because it looks as if she is trying to trap you.

"Do you think that kind of argument is valid?" she responds.

"What do you mean?" you ask, beginning to feel a little confused.

"Well," she says, "isn't that rather like the following argument: If this defendant is guilty, he will refuse to testify at his own trial; he does refuse to testify; therefore, he is guilty. Would any judge allow that argument in a court of law?"

"Of course not," you reply, "but it isn't the same thing at all. We tested the law of conservation of momentum many times in many different ways, and in every case we got the expected result (allowing for the usual small inaccuracies in the measurements)."

"If I remember what you said," Ms. Philo goes on, "in one of your experiments you had one car with a mass of 200 grams and another with a mass of 400 grams, and in that case the lighter car recoiled with twice the speed of the more massive one. How many times did you repeat this particular experiment?"

"Once or twice, as nearly as I can recall."

"Yet, you seem to believe that the result would be the same, no matter how many times the experiment was repeated—is that correct?"

"I suppose so," you reply somewhat uncomfortably.

"And with how many different masses and how many different recoil velocities did you try it? Do you believe it would work the same way if the masses were thousands or billions of kilograms instead of a few grams? And do you suppose that it would work the same way if the velocities were very great—somewhere near the speed of light?"

Since you have heard that strange things happen when speeds approach that of light, your hesitancy increases, but you reply tentatively, "Well, the lab instructor told me that there are no exceptions to the law."

"Did she say that," asks Philo, "or did she say no *known* exceptions?"

"I guess that was it," you reply lamely, feeling quite crushed.

Professor Philo endeavors to summarize the discussion. "What is considered experimental 'proof of a law of nature' is actually a process of testing *some* of its logical consequences. That is, you ask what would have to happen *if* your hypothesis is true, and then you perform an experiment to see if it turns out that way *in fact.* Since any law of nature is a generalization,[8] it has an unlimited number of consequences. We can never hope to test them all. In particular, any *useful* law of nature will have consequences that pertain to the future; they enable us to make predictions. We can never test these consequences until it is too late to use them for the purpose of prediction. To suppose that testing *some* of the consequences of a law constitutes a *conclusive proof* of the law would clearly be an outright logical fallacy." The bell rings and you leave the class, convinced that she has merely been quibbling.

During your physics class you brood about the previous discussion in the philosophy class, without paying very close attention to the lecture. Similar thoughts keep nagging at you during lunch. The objections brought up by Professor Philo seem to be well-founded, you conclude, but you wonder how they can be reconciled with the apparent reliability and certainty of scientific knowledge. In desperation, you decide to talk it over with Professor Salvia during his office hour this very afternoon. When you arrive, you don't know exactly where to begin, so you decide to go back to the pendulum demonstration, which was the thing that got you started on this whole mess."When you performed that demonstration," you ask, "were you *absolutely certain* how it would turn out? Has it ever failed?"

"Well, to be perfectly honest," he says, "it has been known to fail. Once when a friend of mine was doing it in front of a large auditorium, the suspension in the ceiling broke and the ball landed right on his foot. He was in a cast for months!"

"But that's no fault of the law of conservation of energy is it?" you ask. "The breaking of the suspension didn't mean that conservation of energy is false, did it?"

"Of course not," he answers, "we still believe firmly in conservation of energy."

"But are you *certain* of the law of conservation of energy, or any other law of nature?" you ask, and before he has a chance to answer, you tell him about the discussion in the philosophy class this morning.

"So that's what's bothering you," he says, after hearing the whole story. "Professor Philo has an important point. No matter how thoroughly we have tested a scientific law—better, let's say 'hypothesis'—there is always the possibility that new evidence will show up to prove it false. For instance, around the close of the nineteenth century, many physicists seemed virtually certain that Newtonian mechanics was absolutely correct. A wide variety of its consequences had been tested under many different circumstances, and Newton's laws stood up extremely well. But early in the twentieth century it became clear that what we now call 'classical physics' would have to undergo major revisions, and a profound scientific revolution ensued. Modern physics, which includes quantum mechanics and relativity theory, was the result. We can never be sure that any hypothesis we currently accept as correct will not have to be abandoned or modified at some time in the future as a result of new evidence."

"What about the law of conservation of momentum?" you ask, recalling yesterday's experience in the lab. "The lab instructor said it has no known exceptions."

"That is correct," says Salvia, "and it is a rather interesting case. Conservation of momentum is a consequence of Newton's laws of motion; therefore, any consequence of conservation of momentum is a consequence of Newton's laws. But we now regard Newton's laws as not strictly true—they break down, for example, with objects traveling close to the speed of light—but conservation of momentum holds even in these cases. So we have a good example of a case where we believe a lot of consequences, but we do not believe in the laws (Newton's) from which the consequences follow."

It occurs to you that this is a rather important set of supposed laws; perhaps the philosophy professor was not merely quibbling when she said that it was not valid to conclude that a hypothesis is true just because we know many of its consequences to be true.

"Since you cannot be certain of any so-called law of nature," you ask, "why do you believe in them so firmly?"

"Because," answers Salvia, "we consider them very well confirmed. We accept well-confirmed hypotheses, knowing that we may later have to change our minds in the light of new evidence. Science can no longer claim infallible truth."

"Does that mean that scientific results are highly probable, but not absolutely certain?" you ask, trying to be sure you have understood what he has said.

"Yes, you could put it that way," he agrees.

You leave with the feeling that you have a pretty good comprehension of the situation. As a result of your study of physics and philosophy you now understand why science cannot claim infallibility for its findings, but must be content with results that are well confirmed. With that, you take off for the weekend. (And what you do with your weekend is your own business.)

HUME'S BOMBSHELL

A little tired, but basically in a cheerful mood, you arrive at your philosophy class on Monday morning. You meet the professor a few minutes before class outside the room, and you tell her very briefly of your conversation with the physics professor. You explain that you now understand why it is that scientific laws can never be considered completely certain, but only as well-confirmed hypotheses. With her help, and with that of Professor Salvia, you now understand what Hume was driving at—and you see, moreover, that Hume was right. She smiles, and you both go into the classroom, where she begins her lecture.

"Last Friday, as you may recall, we had quite a lively discussion about the status of scientific laws—the law of conservation of momentum, in particular. We saw that such laws cannot be proved conclusively by any amount of experimental evidence. This is a point with which, I am happy to report, many (if not most) contemporary scientists agree. They realize that the most they can reasonably claim for their hypotheses is strong confirmation. Looking at the matter this way, one could conclude that it is wise to believe in scientific predictions, for if they are not certain to be true, they are a good bet. To believe in scientific results is to bet with the best available odds.

"However," she continues, "while this view may be correct as far as it goes, Hume was making a much more fundamental, and I should add, much more devastating point. Hume was challenging not merely our right to claim that scientific predictions will always be right, but also our right to claim that they will usually, or often, or indeed ever, be correct. Take careful note of what he says in Section IV:

> Let the course of things be allowed hitherto ever so regular; that alone, without some new argument or inference, proves not that, for the future, it will continue so. In vain do you pretend to have learned the nature of bodies from your past experience. Their secret nature, and consequently all their effects and influence, may change, without any change in their sensible qualities. This happens sometimes, and with regard to some objects: Why may it not happen always and with regard to all objects? What logic, what process of argument secures you against this supposition?

He is saying, as I hope you understood from your reading, that no matter how reliably a law seems to have held in the past, there is no logical reason why it must do so in the future *at all*. It is therefore possible that *every* scientific prediction, based on *any* law or laws whatever, may turn out to be false from this moment on. The stationary billiard ball that is struck by a moving one may remain motionless where it is—while the moving ball may bounce straight back in the direction from whence it came, or it may go straight up in the air, or it might vanish in a puff of smoke. Any of these possibilities can be imagined; none of them involves any logical contradiction. This is the force of Hume's skeptical arguments. The conclusion seems to be that we have no *reason* to believe in scientific predictions—no more reason than to believe on the basis of astrology, crystal gazing, or sheer blind guessing."

You can hardly believe your ears; what is she saying? You raise your hand, and when you are recognized, you can hardly keep your intense irritation from showing as you assert, "But certainly we can say that scientific predictions are more probable than those based, for example, upon astrology." As you speak, you are reminded of the remark in contemporary problems last Wednesday concerning the coming of the Age of Aquarius.

Science has got to be better than *that!* As these thoughts cross your mind Professor Philo is saying, "... but that depends upon what you mean by 'probable,' doesn't it?"

The physics lecture today is on Newton's law of gravitation, and the professor is explaining that every bit of matter in the universe is attracted to every other by a force proportional to the masses and inversely proportional to the square of the distance between them. He goes on to explain how Kepler's laws of planetary motion and Galileo's law of falling bodies are (when suitably corrected) consequences of Newton's laws. You listen carefully, but you recognize this as another law that enables scientists to make impressive predictions. Indeed, Salvia is now telling how Newton's laws were used to explain the tides on the oceans and to predict the existence of two planets, Neptune and Pluto, that had not been known before. At the same time, you are wondering whether there is anything in what Hume seemed to be saying about such laws. Is it possible that suddenly, at the very next moment, matter would cease to have gravitational attraction, so that the whole solar system would go flying apart? It's a pretty chilling thought.

At lunch you are thinking about this question, and you glance back at some of the readings that were assigned from Hume's *Enquiry*. You notice again Hume's many references to secret powers and forces. Well, gravitation is surely a force, though there has not been any great secret about it since Newton's time. It is the "power" which keeps the solar system together. You remember reading somewhere that, according to Hume, you cannot know that it is safer to leave a building by way of the halls, stairways, and doors than it would be to step out of the third-story window. Well, Newton's law makes it clear why you don't want to step out of the third-story window, but what assurance have you that the building will continue to stand, rather than crashing down around your ears before you can get out? The engineers who design and build towers and bridges have a great deal of knowledge of the "secret powers" of their materials, so they must know a great deal more than Hume did about the hidden properties of things.

At this very moment, a lucky coincidence occurs—you see Dr. Sagro, your physics lab instructor, entering the cafeteria. You wave to her, and she sits down with you, putting her coffee cup on the table. You begin to ask her some questions about structural materials, and she responds by inquiring whether you would be satisfied if she could explain how the table supports the cup of coffee. You recognize it as just the kind of question you have in mind, and urge her to proceed.

"Certain materials, such as the metal in this table," she begins, "have a rather rigid crystalline structure, and for this reason they stick together and maintain their shape unless subjected to large forces. These crystals consist of very regular (and very beautiful) arrays of atoms, and they are held together by forces, essentially electrostatic in origin, among the charged particles that make up the atoms. Have you studied Coulomb's law of electrostatic forces?"

"No," you reply, "we are just doing Newton's law of gravitation. I think Salvia said electricity and magnetism would come up next semester."

"Well," she says, "these electrostatic forces are a lot like gravitational forces (they vary inversely with the square of the distance), but there are a couple of very important differences. First, as you know, there are two types of charges, positive and negative. The proton in the nucleus of the atom carries a positive charge, and the electrons that circulate about the nuclei have a negative charge. Two particles with opposite signs (such as a proton and an electron) attract one another, while two particles with like signs (e.g., two electrons or two protons) repel each other. This is different from gravity, because all matter attracts all other matter; there is no such thing as gravitational repulsion. The second main difference is that the electrostatic force is fantastically stronger than the gravitational force—roughly a billion billion billion billion times more powerful—but we don't usually notice it because most objects we deal with in everyday life are electrically neutral, containing equal amounts of positive and negative electric charge, or very nearly so. If you could somehow strip all of the electrons away from an apple, and all of the protons away from the earth, the force of attraction between the apple and the earth would be unbelievable.

"It is these *extremely* strong attractive and repulsive forces among the electrons and protons in the metal that maintain a stable and rigid form. That's why the table doesn't collapse. And the reason the coffee cup stays on top of the table, without penetrating its surface or slipping through, is that the electrons in the surface of the cup strongly repel those in the surface of the table. Actually, there is also a quantum mechanical force that prevents the weight of the cup from noticeably compressing the table, but we needn't go into that, because the effect is mostly due to the electrostatic forces."

Pleased with this very clear explanation, you thank her, but follow it up with another question. "Is there any logical reason why it has to be that way—why opposite charges attract and like charges repel? Can you prove that it is impossible for like charges to attract and unlike charges to repel? What would happen if *that* were suddenly to become the law?"

"It would certainly result in utter catastrophe," she replies, "with all of the atomic nuclei bunching up together in one place and all of the electrons rushing away from them to congregate elsewhere. But to answer your question, no, there is no logical proof that it couldn't be that way. In our physical world we find that there are, in fact, two types of charges, and they obey the Coulomb law rather than the one you just formulated."

"Can you prove that the world will not switch from the one law to the other, say, tomorrow?" you ask.

"No, frankly, I can't," she answers, "but I, and all other physicists assume—call it an article of faith if you like—that it won't happen."

There's that word "faith" again, you muse as you leave the cafeteria.

The more you think about it, the more clearly you see that the physicists have not shown you how to get around the basic problem Hume raised; rather, they have really reinforced it. Maybe this problem is tougher than I thought, you say to yourself, and you head for Professor Philo's office to talk further about it. "I was thinking about all these 'secret powers' Hume talks about," you begin, "and so I asked my physics instructor about them. She explained, as an example, how a table supports a coffee cup, but she did it on the basis of laws of nature—Coulomb's law of electrostatics was one of them. This law is very well confirmed, I suppose, but she admitted that it is quite possible to imagine that this law would fail tomorrow, and—if you'll pardon the expression—all hell would break loose. Now, my question is, how can we find out about these secret powers that Hume keeps saying we need to know? How can we discover the real underlying causes of what happens?"

"I think you are really beginning to get the point Hume was driving at," she replies, "namely, that there is *no way,* even in principle, of finding any hidden causes or secret powers. You can, of course, find regularities in nature—such as conservation of energy, conservation of momentum, universal gravitation, and electrostatic attraction and repulsion—but these can only be known to have held up to the present. There is no further kind of hidden connection or causal relation that can be discovered by more careful observation, or examination with some kind of super-microscope. Of course, we do discover regularities, and we explain them. For instance, Kepler's laws of planetary motion are regularities that are explained by Newton's laws of motion and gravitation, but these do not reveal any secret powers. They simply provide more general regularities to cover the more restricted ones.

"In his discussion of 'the idea of necessary connection,' Hume tries to bring out precisely this point. We can observe, as you were saying in class the other day, that recoil experiments always yield a particular type of result—namely, momentum is conserved. We have observed this many times. And now we expect, on future trials, that the same thing will happen. But we do not observe, nor can we discover in any way, an *additional* factor which constitutes a necessary connection between the 'explosion' and the subsequent motion of the cars. This seems to be what Hume had in mind when he wrote:

> These ultimate springs and principles are totally shut up from human curiosity and enquiry.

> Elasticity, gravity, cohesion of parts, communication of motion by impulse; these are probably the ultimate causes and principles which we ever discover in nature; and we may esteem ourselves sufficiently happy, if, by accurate inquiry and reasoning, we can trace up the particular phenomena to, or near to, these general principles.[9]

Hume is acknowledging that we can discover general regularities in nature, but he is denying that an additional 'connection' can be found. And Hume was dedicated to the maxim, as are modern scientists, that we have no business talking about things it is impossible in principle for us to know anything about.

"When he asks why we do, in fact, expect so confidently that the future experiments will have outcomes similar to those of the past trials, Hume finds that it is nothing other than a matter of psychological conditioning. When we see one type of cause repeatedly followed by a particular type of effect, we come to expect that the same type of effect will follow the next time we come across that kind of cause. But this is not a matter of logical reasoning. Have you heard of Pavlov's conditioning experiments with dogs?" You nod. "When the bell rings the dog starts to salivate. He is *not* reasoning that, since the sounding of the bell has, in the past, been associated with the bringing of food, therefore, on this occasion the food will (at least probably) appear soon after the bell rings. According to Hume's analysis, what is called 'scientific reasoning' is no more rational or logical than your watering at the mouth when you are hungry and hear the dinner bell. It is something you cannot help doing, Hume says, but that does not mean that it has any logical foundation."

"That brings up a question I've wanted to ask," you say. "Hume seems to think that people necessarily reason in that way—inductive reasoning, I think it is called—but I've noticed that lots of people don't seem to. For instance, many people (including a student in my current problems course) believe in things like astrology; they believe that the configuration of the planets has a bearing on human events, when experience shows that it often doesn't work that way." The professor nods in agreement. You continue, "So if there is no logical justification for believing in scientific predictions, why isn't it just as reasonable to believe in astrological predictions?"

"That," replies the prof, "is a very profound and difficult question. I doubt that any philosopher has a completely satisfactory answer to it."

MODERN ANSWERS[10]

The Wednesday philosophy lecture begins with a sort of rhetorical question, "What reason do we have (Hume is, at bottom, asking) for trusting the scientific method; what grounds do we have for believing that scientific predictions are reliable?" You have been pondering that very question quite a bit in the last couple of days, and—rhetorical or not—your hand shoots up. You have a thing or two to say on the subject.

"Philosophers may have trouble answering such questions," you assert, "but it seems to me there is an obvious reply. As my physics professor has often said, the scientist takes a very practical attitude. He puts forth a hypothesis; if it works he believes in it, and he continues to believe in it as long as it works. If it starts giving him bad predictions, he starts looking for another hypothesis, or for a way of revising his old one. Now the important thing about the scientific method, it seems to me, is that it works. Not only has it led to a vast amount of knowledge about the physical world, but it has been applied in all sorts of practical ways—and although these applications may not have been uniformly beneficial—for better or worse they were successful. Not always, of course, but by and large. Astrology, crystal gazing, and other such superstitious methods simply do not work very well. That's good enough for me."[11]

"That is, indeed, a very tempting answer," Professor Philo replies, "and in one form or another, it has been advanced by several modern philosophers. But Hume actually answered that one himself. You might put it this way. We can all agree that science has, up till now, a very impressive record of success in predicting the future. The question we are asking, however, is this: should we *predict* that science will continue to have the kind of success it has had in the past? It is quite natural to assume that its record will continue, but this is just a case of applying the scientific method to itself. In studying conservation of momentum, you inferred that future experiments would have results similar to those of your past experiments; in appraising the scientific method, you are assuming that its future success will match its past success. But using the scientific method to judge the scientific method is circular reasoning. It is as if a man goes to a bank to cash a check. When the teller refuses, on the grounds that he does not know this man, the man replies, 'That is no problem; permit me to introduce myself—I am John Smith, just as it says on the check.'

"Suppose that I were a believer in crystal gazing. You tell me that your method is better than mine because it has been more successful than mine. You say that this is a good reason for preferring your method to mine, I object. Since you are using your method to judge my method (as well as your method), I demand the right to use my method to evaluate yours. I gaze into my crystal ball and announce the result: from now on crystal gazing will be very successful in predicting the future, while the scientific method is due for a long run of bad luck."

You are about to protest, but she continues.

"The trouble with circular arguments is that they can be used to prove anything; if you assume what you are trying to prove, then there isn't much difficulty in proving it. You find the scientific justification of the scientific method convincing because you already trust the scientific method; if you had equal trust in crystal gazing, I should think you would find the crystal gazer's justification of his method equally convincing. Hume puts it this way:

> When a man says, *I have found, in all past instances, such sensible qualities conjoined with such secret powers:* And when he says, *Similar sensible qualities will always be conjoined with similar secret powers,* he is not guilty of a tautology, nor are these propositions in any respect the same. You can say that the one proposition is an inference from the other. But you must confess that the inference is not intuitive; neither is it demonstrative: Of what nature is it, then? To say it is experimental is begging the question. For all inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar powers will be conjoined with similar sensible qualities.[12]

If the assumption that the future is like the past is the presupposition of the scientific method, we cannot assume that principle in order to justify the scientific method. Once more, we can hardly find a clearer statement than Hume's:

> We have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must evidently be going in a circle, and taking that for granted, which is the very point in question.[13]

"The principle that the future will be like the past, or that regularities which have held up to the present will persist in the future, has traditionally been called *the principle of uniformity of nature.* Some philosophers, most notably Immanuel Kant, have regarded it as an a priori truth.[14] It seems to me, however, that Hume had already provided a convincing refutation of that claim by arguing that irregularities, however startling to common sense, are by no means inconceivable—that is, they cannot be ruled out a priori. Recall what he said:

> . . . it implies no contradiction that the course of nature may change, and that an object, seemingly like those which we have experienced, may be attended with different or contrary effects. May I not clearly and distinctly conceive that a body, falling from the clouds, and which, in all other respects, resembles snow, has yet the taste of salt or feeling of fire? . . . Now whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *a priori.*[15]

"Other philosophers have proposed assuming this principle (or something similar) as a postulate; Bertrand Russell, though not the only one to advocate this approach, is by far the most famous.[16] But most philosophers agree that this use of postulation is question-begging. The real question still remains: why should one adopt any such postulate? Russell himself, in another context, summed it up very well: The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil."[17]

"Nevertheless," you interject, "can't we still say that scientific predictions are more probable than, say, those of astrology or crystal gazing?"

"It seems to me you raised a similar question once before," Professor Philo replies, "and I seem to recall saying that it depends on what you mean by the term 'probable.' Maybe it would be helpful if I now explain what I meant."[18] You nod encouragement. "The concept of probability—or perhaps I should say 'concepts' of probability—are very tricky. If you were to undertake a systematic study of confirmation and induction, you would have to go into a rather technical treatment of probability, but perhaps I can give a brief hint of what is involved.[19] One thing that has traditionally been meant by this term relates directly to the frequency with which something occurs—as Aristotle put it, the probable is that which happens often. If the weather forecaster says that there is a 90% chance of rain, he presumably means that, given such weather conditions as are now present, rain occurs in nine out of ten cases. If these forecasts are correct, we can predict rain on such occasions and be right nine times out of ten.

"Now, if you mean that scientific predictions are probable in *this* sense, I do not see how you could possibly support your claim. For Hume has argued—cogently, I think—that, for all we know now, *every* future scientific prediction may go wrong. He was not merely saying that science is fallible, that it will sometimes err in its predictions—he was saying that nature might at any moment (for all we can know) become irregular on such a wide scale that any kind of scientific prediction of future occurrences would be utterly impossible. We have not found any reason to believe he was mistaken about this point."

"That must not be the concept of probability I had in mind," you remark; "I'm not quite sure how to express it, but it had something to do with what it would be reasonable to believe. I was thinking of the fact that, although we cannot regard scientific hypotheses as certain, we can consider them well confirmed. It is something like saying that a particular suspect is probably guilty of a crime—that the evidence, taken as a whole, seems to point to him."

"You have put your finger on another important probability concept," the professor replies. "It is sometimes known as the rational credibility concept. The most popular contemporary attempt (I believe) to deal with Hume's problem of inductive reasoning is stated in terms of this concept. The argument can be summarized in the following way. Hume has proved that we cannot *know for sure* that our scientific predictions will be correct, but that would be an unreasonable demand to place upon science. The best we can hope is for scientific conclusions that are probable. But when we ask that they be probable, in this sense, we are only asking that they be based upon the best possible evidence. Now, that is just what scientific predictions are—they are predictions based upon the best possible evidence. The scientist has fashioned his hypotheses in the light of all available information, and he has tested them experimentally on many occasions under a wide variety of circumstances. He has summoned all of the available evidence, and he has brought it to bear on the problem at hand. Such scientific predictions are obviously probable (as we are now construing this term); hence, they are rationally credible.[20] If we say that a belief is irrational, we mean that it runs counter to the evidence, or the person who holds it is ignoring the evidence. And in such contexts, when we speak of evidence, we are referring to inductive or scientific evidence.

"Now, the argument continues, to ask whether it is reasonable to believe in scientific conclusions comes right down to asking whether one ought to fashion his beliefs on the basis of the available evidence. But this is what it means to be rational. Hence, the question amounts to asking whether it is rational to be rational. If the question makes any sense at all, the obvious answer is 'yes.' "

"That answer certainly satisfies me," you say, feeling that Dr. Philo has succeeded admirably in stating the point you were groping for. "I'm glad to know that lots of other philosophers agree with it. Do you think it is a satisfactory answer to Hume's problem of induction?" You are more than a trifle discouraged when she gives a negative response with a shake of her head. "Why not?" you demand.

"This argument seems to me to beg the question," she replies, "for it assumes that the concept of evidence is completely clear. But that is precisely the question at issue. If we could be confident that the kind of experiments you performed in the physics lab to test the law of conservation of momentum do, in fact, provide evidence for that law, then we could say that the law is well supported by evidence. But to suppose that such facts do constitute evidence amounts to saying that what has happened in the past is a sign of what will happen in the future—the fact that momentum was conserved in your 'explosion' experiments is an indication that momentum will be conserved in future experiments of a similar nature. This assumes that the future will be like the past, and that is precisely the point at issue. To say that one fact constitutes evidence for another means, in part, that the one provides some basis for inference to the occurrence of the other. The problem of induction is nothing other than the problem of determining the circumstances under which such inference is justified. Thus, we have to resolve the problem of induction—Hume's problem—before we can ascertain whether one fact constitutes evidence for another. We cannot use the concept of evidence—inductive evidence—to solve the problem of induction.

"There is another way to look at this same argument. If you ask me whether you should use the scientific method, I must find out what you hope to accomplish. If you say that you want to get a job teaching physics, I can tell you right away that you had better use the scientific method, at least in your work, because that is what is expected of a physicist. If you say that you want to enjoy the respect and prestige that accrues to scientists in certain social circles, the answer is essentially the same. If you tell me, however, that you want to have as much success as possible in predicting future events, the answer is by no means as easy. If I tell you to go ahead and make scientific predictions, because that is what is considered reasonable (that is what is meant by fashioning your beliefs on the basis of evidence), then you should ask whether being reasonable in this sense (which is obviously the commonly accepted sense) is a good way to attain your goal. The answer, 'but that's what it means to be reasonable,' is beside the point. You might say, 'I want a method that is reasonable to adopt in order to achieve my goal of successful prediction—that is what I mean by being reasonable. To tell me that the scientific method is what is usually *called* reasonable doesn't help. I want to know whether the method that is *commonly called* reasonable is *actually* a reasonable method to adopt to attain my goal of successful prediction of the future. The fact that it is usually considered reasonable cuts no ice, because an awareness of Hume's problem of induction has not filtered down into common usage.' That's what I think you should say."

"Couldn't we avoid all of these problems," suggests another student, "if we simply resisted the temptation to generalize? In social science, my area of interest, we find that it is very risky to generalize, say, from one society to another. An opinion survey on students in the far west, for example, will not be valid when applied to students attending eastern schools. Wouldn't we be better off to restrict our claims to the facts we know, instead of trying to extend them inductively to things we really don't know?"

"The opinion you have offered bears a strong resemblance (though it isn't identical) to that of an influential British philosopher.[21] He has presented his ideas persuasively, and has many followers. Hume, he says, has proved conclusively that induction is not a justifiable form of inference; it is, consequently, no part of science. The only kind of logic that has a legitimate place in science is deductive logic. Deductive inferences are demonstrative; their conclusions must be true if their premises are true. These inferences are precisely what Hume called 'reasoning concerning relations of ideas.' The crucial point is that they *do not add to our knowledge* in any way—they enable us to see the content of our premises, but they do not extend that content in the least. Thus, from premises that refer only to events in the past and present, it is impossible to *deduce* any predictions of future facts. Any kind of inference which would enable us to predict the future on the basis of facts already observed would have to be of a different sort; such inference is often called 'ampliative' or 'inductive.' If science contains only deductive inferences, but no inductive inferences, it can never provide us with any knowledge beyond the content of our immediate observations.

"Now this philosopher does not reject scientific knowledge; he simply claims that prediction of the future is no part of the business of science. Accordingly, the function of scientific investigation is to find powerful general hypotheses (he calls them *conjectures*) that adequately explain all known facts that have occurred so far. As long as such a generalization succeeds in explaining the new facts that come along it is retained; if it fails to explain new facts, it must be modified or rejected. The sole purpose of scientific experimentation is to try to find weaknesses in such hypotheses—that is to criticize them or try to refute them. He calls this the 'method of conjectures and refutations,' or sometimes simply, 'the critical approach.'

"The main difficulty with this approach—an insuperable one, in my opinion—is the fact that it completely deprives science of its predictive function. To the question of which method to use for predicting the future, it can give no answer. Astrology, crystal gazing, blind guessing, and scientific prediction are all on a par. To find out what the population of the world will be in 2000 A.D., we might as well employ a psychic seer as a scientific demographer. I find it hard to believe that this can constitute a satisfactory solution to the problem of employing our knowledge to find rational solutions to the problems that face us—problems whose solutions demand that we make predictions of the future course of events. Tempting as it is to try to evade Hume's problem in this way, I do not see how we can be satisfied to admit that there is no rational approach to our problem."

"But perhaps there is no answer to Hume's problem," says still another student; "maybe the only hope for salvation of this world is to give up our blind worship of science and return to religion. We have placed our faith in science, and look where we are as a result. I believe we should adopt a different faith."

There's that word *again,* you note to yourself, as the professor begins her answer: "Though I heartily agree that many of the results of science—*technological* results, I think we should emphasize—have been far from beneficial, I don't think we can properly condemn scientific *knowledge.* Knowledge is one thing; what we choose to do with it is quite another. But that's not the issue we are concerned with. I do not see how anyone could deny that science has had a great deal of success in making predictions; no other approach can possibly present a comparable record of success. And, as time goes on, the capability for predictive success seems only to increase. It would be an utterly astonishing piece of luck, if it were sheer coincidence, that science has been so much luckier than other approaches in making its predictions. If anyone can consistently pick a winner in every race at every track every day, we are pretty sure he has more than good luck going for him. Science isn't infallible, but it is hard to believe its predictive success is just a matter of chance. I, at least, am not prepared to say that science is just one among many equally acceptable faiths—you pays your money and you takes your choice. I feel rather sure that the scientific approach has a logical justification of some sort." With that, the bell rings, the discussion ends, and everyone leaves—none by way of the window.

It just isn't good enough, you say to yourself, after listening to your physics professor lecturing, with demonstrations, on the law of conservation of angular momentum. You don't know whether you're dizzier from the discussion of Hume's problem in the philosophy class or from watching student volunteers in this class being spun on stools mounted on turntables. In any case, you decide to look up Professor Philo after lunch, and you find her in her office.

"Look," you say a bit brusquely, "I see that Hume was right about our inability to prove that nature is uniform. But suppose that nature does play a trick on us, so to speak. Suppose that after all this time of appearing quite uniform, manifesting all sorts of regularities such as the laws of physics, she turns chaotic. Then there isn't anything we can do anyhow. Someone might make a lucky guess about some future event, but there would be no systematic method for anticipating the chaos successfully. It seems to me I've got a way of predicting the future which will work if nature is uniform—the scientific method, or if you like, the inductive method—and if nature isn't uniform, I'm out of luck whatever I do. It seems to me I've got everything to gain and nothing to lose (except a lot of hard work) if I attempt to adhere to the scientific approach. That seems good enough to me; what do you think?"[22]

"Well," she says quietly, "I tend to agree with that answer, and so do a few others, but we are certainly in the minority. And many difficult problems arise when you try to work it out with precision."

"What sorts of difficulties are these?" you ask.

"There are several kinds," she begins; "for instance, what exactly do you mean by saying that nature is uniform? You cannot mean—to use Hume's quaint language—that like sensible qualities are always conjoined with like secret powers. All of us, including Hume, know this claim is false. Bread which looks and tastes completely harmless may contain a deadly poison. A gas which has exactly the appearance of normal air may suffocate living organisms and pollute the atmosphere. That kind of uniformity principle cannot be the basis of our inferences."

"That's quite true," you answer, "but perhaps we could say that nature operates according to regular laws. Ever since I began to think about Hume's problem, I have been led back to laws of nature."

"Your suggestion is a good one," she replies, "but modern philosophers have found it surprisingly difficult to say precisely what type of statement can qualify as a possible law of nature. It is a law of nature, most physicists would agree, that no material objects travel faster than light; they would refuse to admit, *as a law of nature,* that no golden spheres are more than one mile in diameter. It is not easy to state clearly the basis for this distinction. Both statements are generalizations, and both are true to the best of our knowledge."[23]

"Isn't the difference simply that you cannot, even in principle, accelerate a material object to the speed of light, while it is possible in principle to fabricate an enormous sphere of gold?"

"That is precisely the question at issue," she replies. "The problem is, what basis do we have for claiming possibility in the one case and impossibility in the other. You seem to be saying that a law of nature prevents the one but not the other, which is obviously circular. And if you bring in the notion of causation—causing something to go faster than light vs. causing a large golden sphere to be created—you only compound the difficulty, for the concept of causation is itself a source of great perplexity.

"Suppose, however, that we had succeeded in overcoming that obstacle—that we could say with reasonable precision which sorts of statements are candidates for the status of laws of nature and which are not. We then face a further difficulty. It is obvious that some tests of scientific laws carry greater weight than others. The discovery of the planet Neptune, for example, confirmed Newton's laws much more dramatically than would a few additional observations of Mars. A test with particles traveling at very high velocities would be much stronger evidence for conservation of momentum than would some more experiments on the air track in the physics lab. It is not easy to see how to measure or compare the weight which different types of evidence lend to different scientific hypotheses.

"Scientific confirmation is a subtle and complex matter to which contemporary philosophers have devoted a great deal of attention; some have tried to construct systems of inductive logic that would capture this kind of scientific reasoning. Such efforts have, at best, met with limited success; inductive logic is in a primitive state compared with deductive logic. Until we have a reasonably clear idea of what such inference consists of, however, it is unlikely that we will be able to go very far in meeting the fundamental challenge Hume issued concerning the justification of scientific reasoning. Unless we can at least say what inductive inference is, and what constitutes uniformity of nature (or natural law), we can hardly argue that inductive reasoning—and only inductive reasoning—will prove successful in predicting the future if nature is uniform. And even if those concepts were clarified, the argument would still be intricate indeed."

"Do you think there is any chance that answers to such problems can be found?" you ask.

"I think it's just possible."

"Thanks," you say as you get up to leave.

"And my thanks to you," she replies. "You cannot possibly know how satisfying it is to talk with someone like you—someone intelligent—who takes such philosophical problems seriously and thinks hard about them. If you keep it up, you might be the very person to find some of the answers. I wish you well."

NOTES

1. Professor Salvia is a descendant of Salviati, the protagonist in Galileo's dialogues. The name was shortened when the family emigrated to America.
2. Dr. Sagro is married to a descendant of Sagredo, another character in Galileo's dialogues.
3. If you really did know, please accept the author's apologies.
4. Please note that "demonstrate" is ambiguous. In mathematics it means "prove"; in physics it means "exemplify." Hume uses this term only in the mathematical sense.
5. Hume, using the terminology of his day, refers to it as the "moment" of the moving body.
6. This is Newton's definition; it is somewhat out of date, but adequate in the present context.
7. She is a direct descendant of Philo, the protagonist in Hume's "Dialogues Concerning Natural Religion," most of which is reprinted in this anthology.
8. Professor Philo realizes that it would be more accurate to say that a statement or hypothesis expressing a law of nature must be a generalization, but she does not wish to introduce unnecessary terminological distinctions at this point.
9. In section IV, part I, anticipating the results of the later discussion.
10. All of the attempts to deal with Hume's problem which are treated in this section are discussed in detail in Wesley C. Salmon, *The Foundations of Scientific Inference* (Pittsburgh: University of Pittsburgh Press, 1967); this book will be cited hereafter as *Foundations.*
11. This is an inductive justification; see *Foundations*, chapter II, section I.
12. David Hume, *Enquiry Concerning Human Understanding* (hereafter, *Enquiry*), section IV, part II.
13. Ibid.
14. For discussion of justification by means of synthetic a priori principles, see *Foundations*, chapter II, section 4.
15. *Enquiry*, section IV, part II.
16. For discussion of the postulational approach, see *Foundations*, chapter II, section 6.
17. Bertrand Russell, *Introduction to Mathematical Philosophy* (London: Allen & Unwin, 1919), p. 71.
18. The "probabilistic approach" is discussed in *Foundations*, chapter II, section 7.
19. An elementary survey of philosophical problems of probability is given in *Foundations*, chapters IV–VII. References to additional literature on this subject can be found there.
20. We are assuming, of course, that these predictions are properly made. Scientists are only human, and they do make mistakes. One should not conclude, however, that every false prediction represents a scientific error. Impeccable scientific procedure is fallible, as we have already noted more than once.
21. This refers to the "deductivist" position of Sir Karl Popper. This approach is discussed in *Foundations*, chapter II, section 3.
22. This approach is due mainly to Hans Reichenbach; it is known as a "pragmatic justification" and is discussed in *Foundations*, chapter II, section 8.
23. Further elementary discussion of this issue can be found in Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), § 5.3. A more technical and extensive treatment of related issues can be found in Nelson Goodman, *Fact, Fiction, and Forecast*, 2nd ed. (Indianapolis, Ind.: The Bobbs-Merrill Co., 1965).