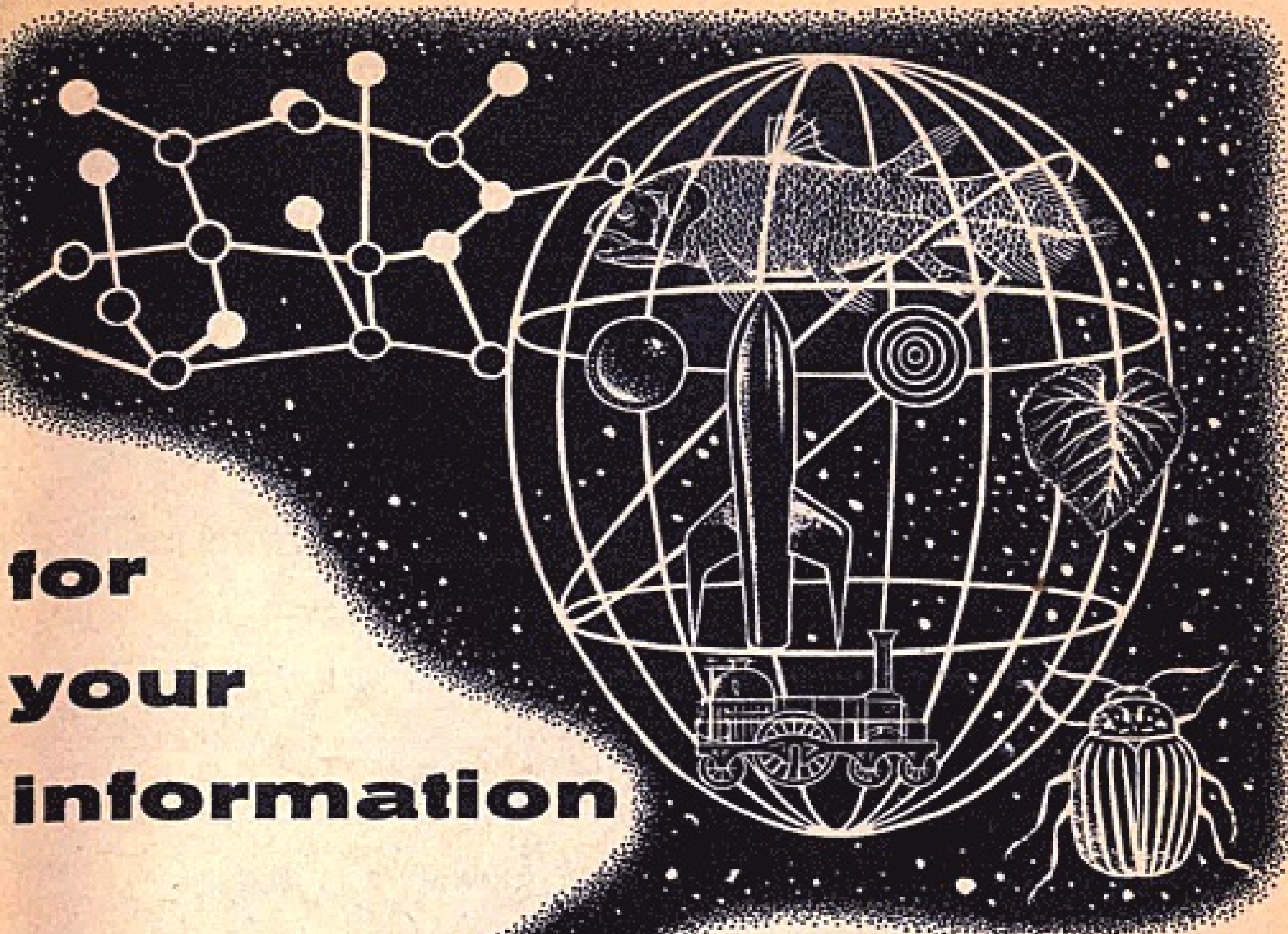
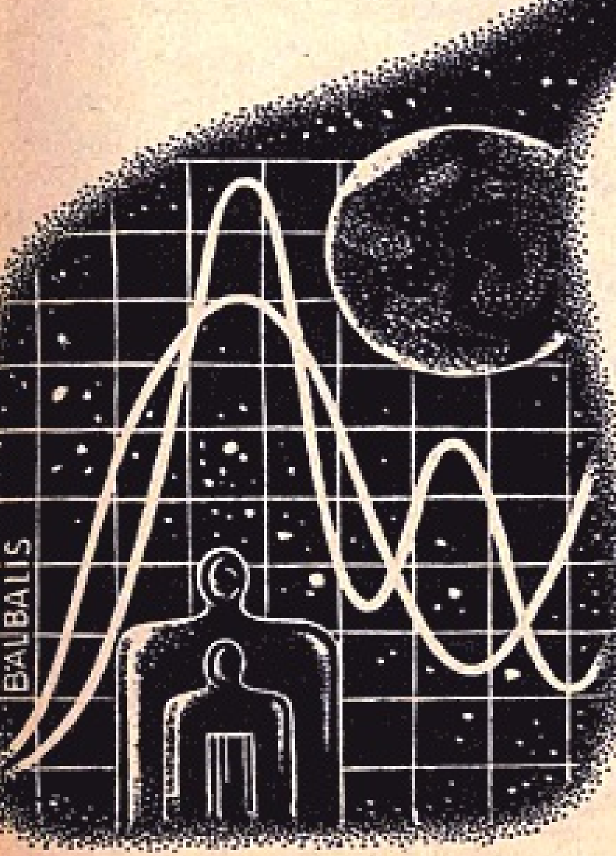


for your information



BY WILLY LEY

THE AIR ON THE MOON

IT WAS a very clear, very cold night in Texas as we walked across the campus. A nearly full Moon was straight overhead and we stopped for a moment to look at it. "Gosh," said one of the girls, "it's so clear you can actually *see* there is no air on the Moon!"

And that is, of course, what every textbook since Giovanni Battista Riccioli's *Almagestum novum* of 1651 has said: there is no air on the Moon. And when modern astronomers speak about a "lunar atmosphere," they are not contradicting this idea, even if it may seem so at first glance. It all depends on your definition. If by "atmosphere" you mean something that you can breathe and which exerts enough pressure on your body to keep the body fluids liquid, then the Moon has no atmosphere. But if your definition of atmosphere is more scientific — a collection of free molecules of gases on the surface of a body of planetary or near-planetary mass — then the Moon does have an atmosphere.

It is, in fact, a very special atmosphere, consisting exclusively (we think) of those gases which are collectively referred to as "rare gases" or, sometimes, as "noble gases." The first of these terms needs no explanation; they *are* rare. The second term, used in Europe, refers to the fact that they do not combine with anything to form any chemical compounds; they do not, you see, associate with the common elements. Why, they are so "noble" that they do not even associate with each other!

Just to be thorough, I have to add that there are American chemists who call them "the inert gases." There is some justification for this because the name of one of them, argon, is the Greek word for "lazy" or, more politely, "inert." However, the term is rather misleading because some ordinary gases such as nitrogen or carbon dioxide can be rather inert too, depending on circumstances, and are so called in industry. To call the rare gases inert gases can (and in the classroom *does*) lead to misunderstandings.

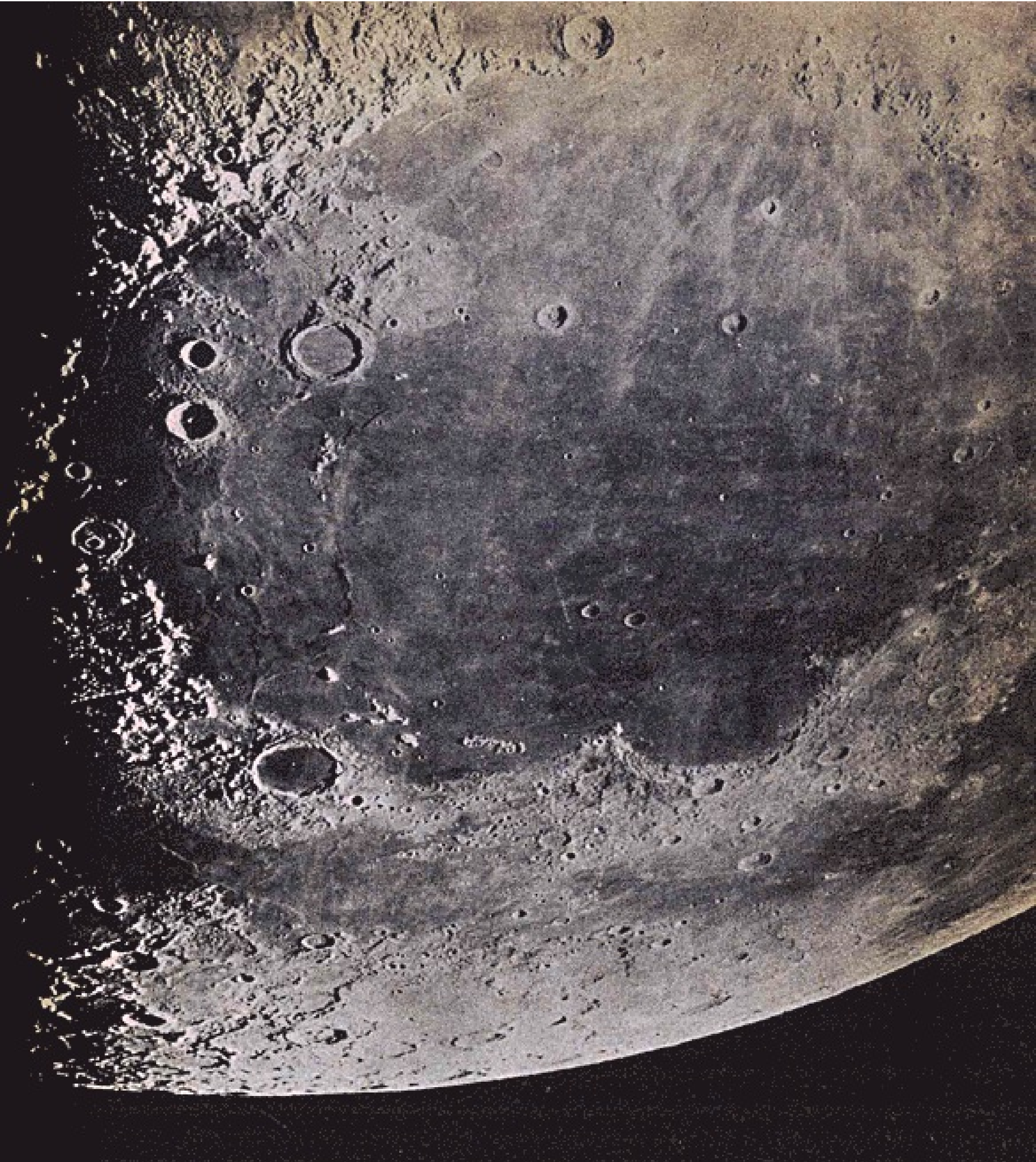
For reasons which will become apparent in a moment, it is necessary to give a table of these gases, together with their atomic weight.

Name	*Meaning*	*Atomic Weight*
Helium (*He*)	sun	4.0
Neon (*Ne*)	new	20.2
Argon (*A*)	inert	39.9
Krypton (*Kr*)	hidden	83.7
Xenon (*Xe*)	stranger	131.3
Radon (*Rn*)	- - -	222.0

Radon was originally named Niton (shining), but the name was later changed to indicate its close relationship to radium.

THE REASON for giving the weights is this: As the temperature increases, the velocity of the molecules (or atoms) of a gas increases, too, and for a given temperature the lighter gases have, of course, higher velocities.

Since the escape velocity of the Moon is not very high — 1.5 miles per second — the



OUR MOON, area of the *Mare imbrium*
"so clear that you can see there is no air there."

Photograph by Mt. Wilson Observatory

molecules (or atoms) can reach this escape velocity if the temperature is high enough. Then they will simply depart for unspecified points somewhere in space, the lighter gases first. The surface temperature on the Moon can be 135° centigrade (275° Fahrenheit) and a useful rule, first stated by Dr. Harold C. Urey, is that it will not be able to hold onto anything with an atomic weight less than 60. Now this seems to restrict the list of the gases which could stay at the Moon's surface to the last three, but radon is a kind of theoretical case only. It is radioactive with a half-life of only 3.85 days, so that it disappears for reasons of its own.

In reality, then, the Moon can hold krypton and xenon.

Theoretical reasoning says that it should be mostly xenon, because xenon-129 must be produced steadily from the radioactive isotope of iodine, iodine-129. The number of xenon atoms produced per second on the Moon must be about 5000 million. The production rate of krypton is far lower, so that the lunar "atmosphere" would be likely to consist of 95 per cent xenon and 5 per cent krypton.

Radioactive processes always produce helium at fairly high rates, but helium is too light to be kept by the Moon. Neon is also too light, but argon is an interesting borderline case. Its weight is about 40, which is lighter than the lower limit postulated by Dr. Urey. On the other hand, that figure of 275° Fahrenheit is rather high and so might the production rate of argon be. The source of argon would be the radioactive isotope of potassium, potassium-40.

As Dr. Isaac Asimov pointed out, 3600 grams of potassium-40 are likely to decay every second. Eleven per cent of this potassium-40 turns into argon-40 (the remainder becomes calcium-40) which means a production per second of nearly 400 grams (close to 14 ounces) of argon-40. Even though a good percentage of this argon must dissipate in space, these figures indicate that the lunar atmosphere should be composed of argon, krypton and xenon.

Possibly traces of another gas must be added, carbon dioxide. Last year the Russians observed a phenomenon on the Moon which they described as a volcanic eruption. English and American astronomers did not doubt the observation itself, but inclined to the belief that it was a carbon dioxide outbreak, which carried surface dust up with it and therefore looked more substantial than it really was.

Now the molecular weight of

carbon dioxide is 44, just about the same as argon. Therefore the same reasoning applies to it, but the question mark is the production rate. The only source of carbon dioxide on the Moon would be volcanism — and the most anybody can say about this is that there might be some lunar volcanic activity left — with fairly much emphasis on the word "might."

How high this highly attenuated mixture of xenon and krypton with some argon and possibly some carbon dioxide extends is a question which can cause a long and inconclusive debate. At any event, the idea of catching some with a closely orbiting rocket is not feasible. It will need a soft landing of an instrument capsule to find out.

GALILEO GALILEI AND THE LEANING CAMPANILE OF PISA

AT ONE TIME early in his career, Galileo Galilei grew very doubtful about the idea that had come down from Aristotle, namely that the speed of falling objects varies in accordance with their weight. That, for example, an object weighing ten pounds will fall ten times as fast as an object weighing only one pound.

Galileo Galilei was at that time professor at Pisa, the city of the famous leaning tower (which was and is called the Campanile), and one day in 1590, he assembled his pupils and as many of the faculty as he could round up at the foot of the Campanile and he dropped a cannon ball and a musket ball from the top of the tower. They struck the ground simultaneously and from that day on Aristotelian teachings were disbelieved.

Of course it's a familiar story. It can be found in all books on Galilei and any popular book dealing with the history of science.

A few months ago, however, I received a letter from a Mr. Cockroft in New Zealand who had come across an article in an American magazine in which it was loudly stated that this "whole silly story" had been invented by an English writer by the name of R. A. Gregory in a book published in London in 1917, but had since been thoroughly disproved by Lane Cooper, Professor of English at Cornell University.

When the letter arrived I happened to be on a long lecture tour on the West Coast. All I could do at the moment was write a letter in reply, promising to check the facts as soon as I could. I also ordered a copy of Professor Cooper's book.

For the rest of the lecture tour I wasn't quite happy. I had used the story of Galileo Galilei's experiment from the leaning tower myself, but because it had been a minor incident in my case, I had not specially checked on it. While flying to my next engagement I tried to remember what Galilei himself had said in his famous *Dialogues . . . concerning Two New Sciences.* He did use the famous argument there that the Aristotelian concept was illogical. If a larger stone fell faster than a smaller one, what would happen if the two were tied together? Would they fall still faster because they weighed more? Or would the lighter stone hamper the movement of the heavier one? I seemed to remember that he had spoken of experiments.

A FEW lectures later I had dinner in the home of a gentleman who had a large library. Yes, a biography of Galilei was among his books and it contained the story of the leaning tower and stated that Galilei (who was then about 26 years old) had not published anything while at Pisa. My host remarked, after I told him about the inquiry I had received, that the original source was probably buried in a volume with a title like "Correspondence between Galileo Galilei and So-and-so" — such letter collections are always a treasure for biographers but a nuisance for a man who wants to find something in a hurry. For this reason we didn't even try the university library the next morning, though there would have been time to do so.

If this were a story, I would have Professor Cooper's book waiting for me when I came home. Since this isn't a story, it was not. I went on two more lecture tours and forgot all about it. But then Professor Cooper's book (entitled: *Aristotle, Galileo and the Tower of Pisa*) did come and a week or so later I got around to reading it.

It is a most careful work, quoting all the sources in their original tongues as well as in translation. That the general feeling of confusion emerges from its pages is not Professor Cooper's fault. The story itself is confused.

To make it as clear as circumstances permit, let us examine the whole thing piecemeal, beginning with what Aristotle said. "An iron ball of one hundred pounds, falling from a height of one hundred cubits, reaches the ground before a one-pound ball has fallen a single cubit." But these are Galilei's words (from the *Dialogues*) and not the words of Aristotle. Then what did Aristotle himself say? Well, finding that out is not so simple.

Aristotle did not write systematically about motions and fall, which is to say he did not develop a theory of his own. He was arguing against the atomists, whom he disliked, and his statements are meant to show how wrong they were. However, there is one sentence which seems to be the one Galilei had in mind: "For any two portions of fire, small or great, will exhibit the same ratio of solid to void; but the upward movement of the greater is quicker than that of the less, *just as the downward movement of a mass of gold, or lead, or of any other body endowed with weight, is quicker in proportion to its size.*" This is quoted from his book *On the Heavens.*

Elsewhere (in *Physics*) he stated: "We see that bodies which have greater *rhopé* either of weight or of lightness, if they are alike in other respects, move faster over an equal space, and in the ratio which their magnitudes bear to each other." The Greek word *rhopé* unfortunately is quite rubbery in its meaning; the Latin editions of Aristotle use *velocitas* or *celeritas* (both meaning "speed," of course) where the original uses *rhopé.* But in English the words "impulse," "momentum," "trend" or even "tendency" might be used with nearly equal justification.

ARISTOTLE, then, did not actually speak of "fall, velocity of," but of movement, and Galilei's rendering which has replaced the original phrasing might be considered a "popularization."

However, it was "always" taken to apply to the speed of falling bodies too. Ioannos Philoponus, who wrote a commentary on Aristotle's *Physics* in 533 A. D., said that the reasoning was "absolutely false" and that this was one of the cases where the phenomenon can better be tested by observation than by logic. "If you take two masses greatly differing in weight and release them from the same elevation, you will see that the ratio of times in their movements does not follow the ratio of the weights, but [that] the time difference is extremely small; so that if the weights do not greatly differ, but one, say, is double the other, the difference in the times [required for falling a given distance] will be either none at all or imperceptible."

Now that we know the concept Galilei was fighting, we come to the crucial point. When did he drop the cannon ball and the musket ball from the leaning Campanile and what did he say about it himself?

The answer, surprisingly, is that he *never* said a word about it. In his writings (and not only his published books but many of his letter are known) he never even mentions Pisa's Campanile. And while he wrote much later that one could easily test his ideas by dropping weights from a high place, such as a tower, he nowhere says that he did it.

Then who originated the story?

It can be found for the first time in a biography of Galilei by Vincenzio Viviani. Viviani completed his manuscript in 1654, twelve years after Galilei's death. That it was not printed until 1717 is probably unimportant; it was then customary to let manuscripts and handmade copies of manuscripts circulate among learned men, like Vincenzio Viviani himself.

Viviani described the great event in rather dry language. He said that Galilei had disproved many of the conclusions of Aristotle: "among others that the velocities of moving bodies of the same material, [but] of unequal weight, moving through the same medium, did *not* mutually preserve the proportion of their weight as taught by Aristotle, but all moved at the same speed; demonstrating this with repeated experiments from the height of the Campanile of Pisa in the presence of the other teachers and philosophers, and the whole assembly of students . . . "

Three things will be noted: Viviani does not mention the weights of the bodies dropped and does not say what they were. He also does not give the year — but since Galilei was in Pisa from the latter part of 1589 to the early part of 1591, as he says elsewhere, he probably did not think it necessary to be specific. But while everybody else at a later date spoke about *one* dramatic demonstration, Viviani calmly said "repeated experiments."

THE historical problem here is, of course, the reconciliation between Galilei's own silence and Viviani's statement. Before I get to this, it must be mentioned that Galilei's experiment was not as unique as it is usually portrayed. Hieronymus Cardanus, in his book *Opus Novum de Proportionibus,* published in Basel in 1570, began a chapter with the straight statement: *Si duae sphaerae ex eadem materia descendant in aëre eodem temporis momento ad planem ueniunt.* "Two spheres made of the same material, falling in (meaning through) air, will arrive at a plane at the same instant." The word "plane" is to be understood in its mathematical sense; the illustration given by Cardanus looks as if he had a table top in mind.

And the Dutchman Simon Stevin, in a book published in Leyden in 1605, wrote: "The experiment against Aristotle is like this: take two balls of lead (as the eminent Jean Grotius . . . and I did some time ago), one ball ten times the other in weight; and let them go together from a height of 30 feet down to a plank below . . . the sound of the two in striking will seem to come back as one single report."

And at some time before 1612 a defender of Aristotle, Giorgio Coresio, did drop things from the tower of Pisa. Another man, Jacopo Mazzoni, had made such experiments finding Aristotle wrong.

Coresio said that this was due to the fact that Mazzoni did not go high enough; he probably just dropped things from his window, whereas Coresio had gone up all the way to the top of the tower and at this great height had found Aristotle's assertion correct.

Am I, in quoting all these other authors, delaying a decision of the main question? No, I have quoted these contemporary facts because their existence, in my opinion, is the clue to the historical riddle.

The riddle is the silence of Galilei himself — unless he used Viviani as his mouthpiece. This is what the editors of the Italian edition of Galilei's works believe, namely that Galilei told Viviani about the Pisa experiments when he was old. There is no doubt that they had close personal contact and it is known that Galilei was blind for the last five years of his life. He might very well have told Viviani things which he had never written down.

He might even have said something that wasn't so. History is full of statements ascribed to voluble old men which could not be checked and which are, to say the least, doubtful. There is no reason to think that Viviani invented the story. Galilei himself might have, but I don't think so. That he did not mention it earlier in his writings can have several reasons.

ONE assumption is that he did mention it in some early letters which were not kept by their recipients or were destroyed by accident before they saw print.

Another possibility is that, being busy all his life and often harrassed, he just didn't. That such a thing can happen is within my own experience. Some time ago I came across a discussion, bolstered by high-speed photographs, showing that if you drop a drop of milk in water the resulting splash will carry some of that milk upward. Whereupon

I told my wife that I, as a schoolboy (about seventh grade), amused myself by trying to hit a floating piece of wood with thrown pebbles and discovered that the splash from a stone was clean, while the splash from a clump of earth was decidedly dirty. It had at that time intrigued me so much that I forgot all about the floating wood and even called classmates to watch the effect. I may have missed a "discovery" by waiting for others to photograph drops of milk.

In thinking all this over, one point occurred to me which I haven't seen mentioned anywhere else. Modern writers have made much of the paramount importance, the decisiveness, of the drop from the Campanile. If it had been so decisive, contemporary literature should be full of it. Since contemporary literature literally doesn't say a word, the German Emil Wohlwill (in 1909) was the first to conclude that the whole story probably was not true. And Lane Cooper, a quarter century later, drew the same conclusion for the same reason.

But what if the experiment, though carried out "repeatedly" as Viviani says, simply did not impress the contemporaries?

We do have convincing and much more recent examples.

Everybody in the airplane and airline business now makes a large to-do every December 17th about "Kitty Hawk Day." But the actual Kitty Hawk Day, when the Wright brothers flew for the first time, hardly got into the newspapers. *All* the recognition originally received for the first flight could be reprinted on one page of this magazine.

Likewise March 16, 1926, is the day the first liquid-fuel rocket in all history (Goddard's) lifted itself off the ground. Historic day, isn't it? Again the publicity received would fit this magazine page — and the editor could specify nice large type because of the importance of the event and the meagerness of the recognition. Sometime in 1932 the first Russian liquid-fuel rocket (Blagonravov's) rumbled upward. Certainly a historic date, too, but it seems that even the Russians do not know the date; I have only seen the year mentioned.

And the first liquid-fuel rockets in Germany (mixed system Oberth, Riedel, von Braun and Ley) received publicity only because we made it — we needed publicity to raise money.

It is quite conceivable that Galilei rounded up his students and some faculty members, made the demonstration — and did not impress anybody. This would be reason enough for his later silence. All the more so because he could not even claim priority, since Cardanus had written about it in 1570, twenty years prior to Galilei's Pisa tenure, and even Simon Stevin's book appeared ahead of Galilei's own *Dialogues*. If the Pisa experiments did not impress anybody, Galilei had no reason to talk about them in his later works. They had not made him famous and they were not even new.

Until he reached old age and had stopped being active and started reminiscing.

NOTE ON EARLY SCIENCE FICTION

"CAN ONE really doubt . . . " intelligent life has developed on other planets [somewhere in our galaxy] and that these intelligences have learned how to build interstellar ships . . . . that they have used these ships to colonize countless other planets . . . . that they might have had to destroy painlessly unsuccessful evolutionary developments on such other planets to replace "painful evolution by painless colonization" . . . . that they have left other planets undisturbed "as a biological reservoir" [or as control experiments, as we might call it] . . . and that they have refrained from communicating with us [and possibly other planets like ours] waiting for us to take the first step in interstellar communication?

Let me say first that these thoughts were written down in about 1933, but were probably conceived by the man who wrote them at a much earlier age, since in 1933 he was over seventy years old.

Now stop for a moment; don't read on. Try to figure out who said this.

The answer is:

Not a science fiction writer, but Konstantin Eduardovitch Ziolkovsky, who wrote the first of the modern treatises on space travel in 1898 and saw it published in 1903.

ANY QUESTIONS?

I saw an article in a photographic magazine proving that the pictures which the Russians said they took of the other side of the Moon are fakes. Where does that put us in the space race? I have also read that the Russians claim to have discovered a new planet beyond Pluto. Is there any truth to this?

Erwin Richter
Farmingdale, Long Island.

As for the first item, I can only say that all reputable scientists the world over accept the pictures released by Russia as genuine. Personally I don't have the slightest doubt. That somebody crops up with the intention of keeping his eyes firmly closed is no novelty at all. For instance, I recently got a phone call from somebody who still maintains that the Earth is flat. So this "revelation" has absolutely no bearing on the space race; we still are where we were before, several years behind, with no prospect of narrowing the gap during 1960 and little chance for doing this in 1961.

As for the "discovery of a major planet beyond Pluto," I can report that this isn't so — the "claim" seems to have been a mistake in translation. The actual story is that the Russian astronomer Edward Denisiuk, at the observatory near Alma-Ata, photographed an unlisted "object" in August 1957. He did not then know what it was; it could have been a new comet. Near the end of 1959 the "object" was found on other plates taken in 1957 and a preliminary orbit calculation could be made, proving that it was an unlisted asteroid. Just who changed this into a "trans-Plutonian planet" will probably never become known. The culprit has not owned up yet and it seems very unlikely that he ever will.

How many elements are there now, including the man-made ones?

Welson S. Mann
Hollywood, Calif.

The usual statement is that there are 92 elements, from No. 1, hydrogen, to No. 92, uranium. But actually three of these 92 do *not* occur in nature. They are No. 43, technetium; No. 61, promethium (formerly called illinium); and No. 85, astatine. On the other hand No. 94, plutonium, usually a man-made element, does occur in nature, though in quantities far far smaller than even radium.

The man-made elements, so far, are No. 93, neptunium; No. 94, plutonium; No. 95, americium; No. 96, curium; No. 97, berkelium; No. 98, californium; No. 99, einsteinium; No. 100, fermium; No. 101, mendelevium, and No. 102, nobelium.

However, the ones above plutonium have been made in such small quantities that they would be invisible to the naked eye, in some cases invisible even under a laboratory microscope.

— WILLY LEY