**FACT AND FANCY  
CHAPTER 16 "MY BUILT-IN DOUBTER"**

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**Chapter 16 -- "My Built-in Doubter" (page 184)**

Once I delivered myself of an oration before a small but select audience of non-scientists on the topic of "What Is Science?" speaking seriously and, I hope, intelligently.

Having completed the talk, there came the question period, and, bless my heart, I wasn't disappointed. A charming young lady up front waved a pretty little hand at me and asked, not a serious question on the nature of science, but: "Dr. Asimov, do you believe in flying saucers?"

With a fixed smile on my face, I proceeded to give the answer I have carefully given after every lecture I have delivered. I said, "No, miss, I do not, and I think anyone who does is a crackpot!”

And oh, the surprise on her face!

It is taken for granted by everyone, it seems to me, that because I sometimes write science fiction, I believe in flying saucers, in Atlantis, in clairvoyance and levitation, in the prophecies of the Great Pyramid, in astrology, in Fort's theories, and in the suggestion that Bacon wrote Shakespeare.

No one would ever think that someone who writes fantasies for pre-school children really thinks that rabbits can talk, or that a writer of hard-boiled detective stories really thinks a man can down two quarts of whiskey in five minutes, then make love to two girls in the next five, or that a writer for the ladies' magazines really thinks that virtue always triumphs and that the secretary always marries the handsome boss--but a science-fiction writer apparently *must* believe in flying saucers.

Well, I do not.

To be sure, I wrote a story once about flying saucers in which I explained their existence very logically. I also wrote a story once in which levitation played a part.

If I can buddy up to such notions long enough to write sober, reasonable stories about them, why, then, do I reject them so definitely in real life?

I can explain by way of a story. A good friend of mine once spent quite a long time trying to persuade me of the truth and validity of what I considered a piece of **pseudoscience** and bad pseudo-science at that. I sat there listening quite stonily, and none of the cited evidence and instances and proofs had the slightest effect on me.

Finally the gentleman said to me, with considerable annoyance, "**Damn it, Isaac, the trouble with you is that you have a built-in doubter."**

To which the only answer I could see my way to making was a heartfelt, "**Thank God**."

**If a scientist has one piece of temperamental equipment that is essential to his job, it is that of a built-in doubter. Before he does anything else, he must doubt. He must doubt what others tell him and what he reads in reference books, and, *most of all*, what his own experiments show him and what his own reasoning tells him.**

Such doubt must, of course, exist in varying degrees. It is impossible, impractical, and useless to be a maximal doubter at all times. One cannot (and would not want to) check personally every figure or observation given in a handbook or monograph, before one uses it and then proceed to check it and recheck it until one dies. *But*, if any trouble arises and nothing else seems wrong, one must be prepared to say to one's self, "**Well, now, I wonder if the data I got out of the *'Real Guaranteed Authoritative Very Scientific Handbook'* might not be a misprint**."

To **doubt intelligently** requires, therefore, a rough appraisal of the authoritativeness of a source. It also requires a rough estimate of the nature of the statement. If you were to tell me that you had a bottle containing one pound of pure **titanium oxide**, I would say, "Good," and ask to borrow some if I needed it. Nor would I test it. I would accept its purity on your say-so (until further notice, anyway).

If you were to tell me that you had a bottle containing one pound of pure **thulium oxide**, I would say with considerable astonishment, "You have? Where?" Then if I had use for the stuff, I would want to run some tests on it and even run it through an ion-exchange column before I could bring myself to use it.

And if you told me that you had a bottle containing one pound of pure **americium oxide**, I would say, "You're crazy," and walk away. I'm sorry, but my time is reasonably valuable, and I do not consider that statement to have enough chance of validity even to warrant my stepping into the next room to look at the bottle.

**What I am trying to say is that doubting is far more important to the advance of science than believing is and that, moreover, doubting is a serious business that requires extensive training to be handled properly. People without training in a particular field do not know what to doubt and what not to doubt; or, to put it conversely, what to believe and what not to believe. I am very sorry to be undemocratic, but one man's opinion is not necessarily as good as the next man's.**

To be sure, I feel uneasy about seeming to kowtow to authority in this fashion. After all, you all know of instances where authority was wrong, dead wrong. Look at Columbus, you will say. Look at Galileo.

I know about them, and about others, too. As a dabbler in the history of science, I can give you horrible examples you may never have heard of. I can cite the case of the German scientist, **Rudolf Virchow**, who, in the mid-nineteenth century was responsible for important advances in anthropology and practically founded the science of pathology. He was the first man to engage in cancer research on a scientific basis. However, he was dead set against the germ theory of disease when that was advanced by **Pasteur**. So were many others, but one by one the opponents abandoned doubt as evidence multiplied. Not Virchow, however. **Rather than be forced to admit he was wrong and Pasteur right, Virchow quit science altogether and went into politics.** How much wronger could Stubborn Authority get?

But this is a very exceptional case. Let's consider a far more normal and natural example of authority in the wrong.

The example concerns a young Swedish chemical student, **Svante August Arrhenius**, who was working for his Ph.D. in the University of Uppsala in the 1880s. He was interested in the freezing points of solutions because certain odd points arose in that connection.

If **sucrose** (ordinary table sugar) is dissolved in water, the freezing point of the solution is somewhat **lower** than is that of pure water. Dissolve **more sucrose** and the freezing point lowers further. You can calculate how many molecules of sucrose must be dissolved per cubic centimeter of water in order to bring about a certain drop in freezing point. It turns out that this same number of molecules of glucose (grape sugar) and of many other soluble substances will bring about the same drop. It doesn't matter that a molecule of sucrose is twice as large as a molecule of glucose. What counts is the **number of molecules and not their size**.

But if **sodium chloride** (table salt) is dissolved in water, the freezing-point drop per molecule is **twice** as great as normal. And this goes for certain other substances too. For instance, **barium chloride**, when dissolved, will bring about a freezing point drop that is **three** times normal.

Arrhenius wondered if this meant that when sodium chloride was dissolved, each of its molecules broke into two portions, thus creating twice as many particles as there were molecules and therefore a doubled freezing-point drop. And barium chloride might break up into three particles per molecule. Since the sodium chloride molecule is composed of a sodium atom and a chlorine atom and since the barium chloride molecule is composed of a barium atom and two chlorine atoms, the logical next step was to suppose that these particular molecules broke up into individual atoms.

**Then, too, there was another interesting fact. Those substances like sucrose and glucose which gave a normal freezing-point drop did not conduct an electric current in solution. Those, like sodium chloride and barium chloride, which showed abnormally high freezing-point drops, *did* do so**.

Arrhenius wondered if the **atoms**, into which molecules broke up on solution, might not carry **positive** and **negative** electric charges. If the sodium atom carried a positive charge for instance, it would be attracted to the negative electrode. If the chlorine atom carried a negative charge, it would be attracted to the positive electrode. Each would **wander off** in its own direction and the net result would be that such a solution would conduct an electric current. For these charged and wandering atoms, Arrhenius adopted Faraday's name "ions" from a Greek word meaning "wanderer."

Furthermore, a charged atom, or ion, would not have the properties of an uncharged atom. A charged chlorine atom would not be a gas that would bubble out of solution. A charged sodium atom would not react with water to form hydrogen. It was for that reason that common salt (sodium chloride) did not show the properties of either sodium metal or chlorine gas, though it was made of those two elements.

**In 1884 Arrhenius, then twenty-five, prepared his theories in the form of a thesis and presented it as part of his doctoral dissertation. The examining professors sat in rigid disapproval. No one had ever heard of electrically charged atoms, it was against all scientific belief of the time, and they turned on their built-in doubters.**

However, Arrhenius argued his case so clearly and, on the single assumption of the dissolution of molecules into charged atoms, managed to explain so much so neatly, that the professors' built-in doubters did not quite reach the intensity required to flunk the young man. **Instead, they** **passed him--with the lowest possible passing grade.**

But then, **ten years later**, the negatively charged electron was discovered and the atom was found to be not the indivisible thing it had been considered but a complex assemblage of still smaller particles. Suddenly the notion of ions as charged atoms made sense. If an atom lost an electron or two, it was left with a positive charge; if it gained them, it had a negative charge.

**Then, the decade following, the Nobel Prizes were set up and in 1903 the Nobel Prize in Chemistry was awarded to Arrhenius for that same thesis which, nineteen years earlier, had barely squeaked him through for a Ph.D.**

**Were the professors wrong?** Looking back, we can see they were. But in 1884 they were ***not*** wrong. They did exactly the right thing and they served science well. Every professor must listen to and appraise dozens of new ideas every year. He must greet each with the gradation of doubt his experience and training tells him the idea is worth.

Arrhenius's notion met with just the proper gradation of doubt. It was radical enough to be held at arm's length However, it seemed to have just enough possible merit to be worth some recognition. The professors *did* give him his Ph.D. after all. And other scientists of the time paid attention to it and thought about it. A very great one, Ostwald, thought enough of it to offer Arrhenius a good job.

**Then, when the appropriate evidence turned up, doubt receded to minimal values and Arrhenius was greatly honored.**

Or take another case. I have recently seen a news clipping concerning an **eighth-grader** in South Carolina who grew four sets of bean plants under glass jars. One set remained there always, subjected to silence. The other three had their jars removed one hour a day in order that they might be exposed to noise; in one case to jazz, in another to serious music, and in a third to the raucous noises of sports-car engines. The only set of plants that grew vigorously were those exposed to the engine noises.

The headline was: ***BEANS CAN HEAR--AND THEY PREFER AUTO RACING NOISE TO MUSIC*.**

Automatically, my built-in doubter moves into high gear. Can it be possible that the newspaper story is a hoax? This is not impossible. The history of newspaper hoaxes is such that one could be easily convinced that nothing in any newspaper can possibly be believed.

But lets assume the story is accurate. The next question to ask is whether the youngster knew what he was doing? Was he experienced enough to make the nature of the noise the only variable? Was there a difference in the soil or in the water supply or in some small matter, which he disregarded through inexperience?

Finally, even if the validity of the experiment is accepted, what does it really prove? To the headline writer and undoubtedly to almost everybody who reads the article, it will prove that plants can hear; and that they have preferences and will refuse to grow if they feel lonely and neglected.

This is so far against the current structure of science that my built-in doubter clicks it right off and stamps it: IGNORE. Now what is an alternative explanation that fits in reasonably well with the structure of science? Sound is not just something to hear; it is a form of vibration. Can it be that sound vibrations stir up tiny soil particles making it easier for plants to absorb water, or putting more ions within reach by improving diffusion? May the natural noise that surrounds plants act in this fashion to promote growth? And may the engine noises have worked best on a one-hour-per-day basis because they were the loudest and produced the most vibration?

Any scientist (or eighth-grader) who feels called on to experiment further, ought to try vibrations that do not produce audible sound; ultrasonic vibrations, mechanical vibrations and so on. Or he might also try to expose the plant itself to vibrations of all sorts while leaving the soil insulated; and vice versa.

Which finally brings me to flying saucers and spiritualism and the like. The questions I ask myself are: What is the nature of the authorities promulgating these and other viewpoints of this sort? and How well do such observations and theories fit in with the established structure of science?

My answers are, respectively, Very poor and Very poorly.

Published by Discus Books, March, 1972, Copyright 1962

 The original essay is available at <http://www.aracnet.com/~lewallen/builtindoubter.bml>

