BENJAMIN FRANKLIN
As a Scientist

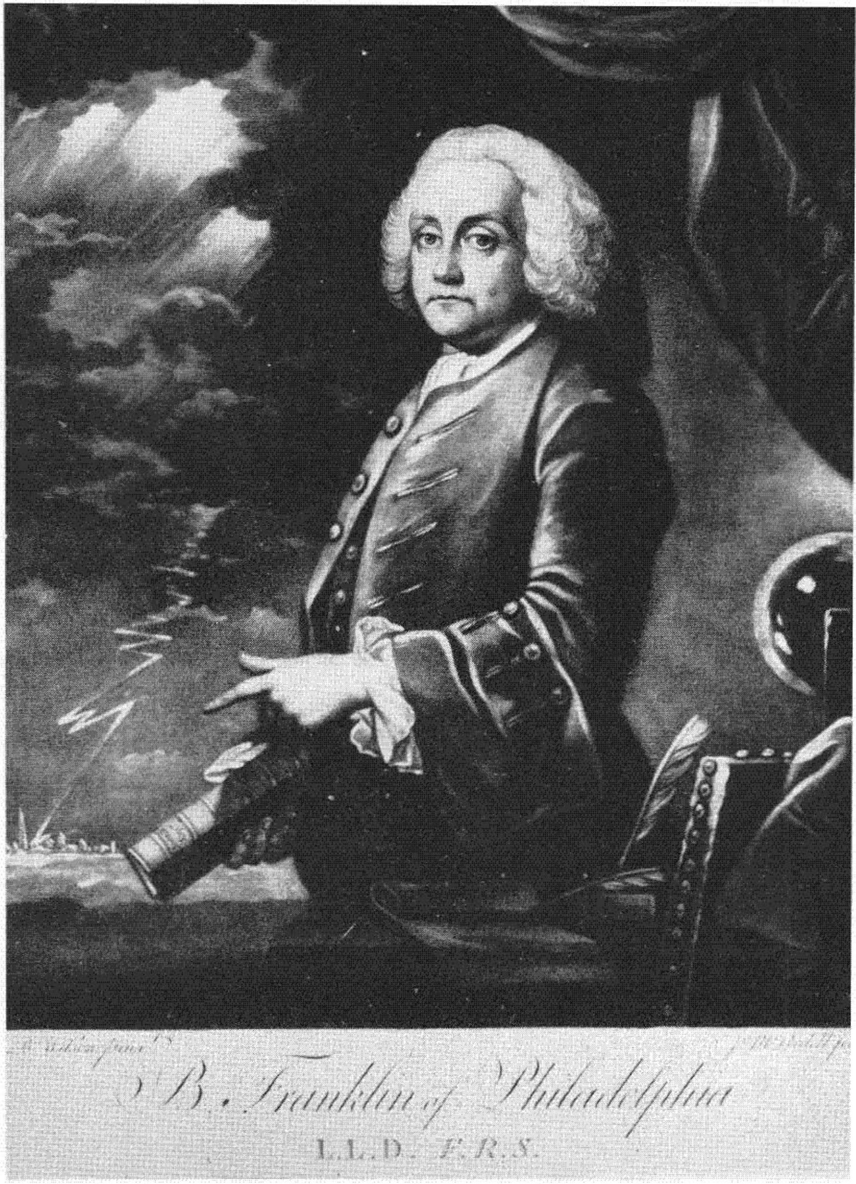
By ROBERT A. MILLIKAN

BENJAMIN Franklin is perhaps the only American in that relatively small group of men of any time or country who, without having been either the head of a state or a military hero, have yet gained so conspicuous a place in history that their names and sayings are known the world over. Although he lived 200 years ago in what was then a remote corner of the earth, far from any of the centers of world influence, yet his name and traits are still widely known. May I quote a paragraph from a short biography of Michelson which I published in the Scientific Monthly for January, 1939:

"It will probably be generally agreed that the three American physicists whose work has been most epoch-making and whose names are most certain to be frequently heard wherever and whenever in future years the story of physics is told are Benjamin Franklin, Josiah Willard Gibbs, and Albert A. Michelson. And yet the three have almost no characteristics in common. Franklin lives as a physicist because, dilettante though he is sometimes called, mere qualitative interpreter though he actually was, yet it was he who with altogether amazing insight laid the real foundations on which the whole superstructure of electrical theory and interpretation has been erected. Gibbs lives because, profound scholar, matchless analyst that he was, he did for statistical mechanics and for thermodynamics what Laplace did for celestial mechanics and Maxwell did for electrodynamics, namely, made his field a well-nigh finished theoretical structure. Michelson, pure experimentalist, designer of instruments, refiner of techniques, lives because in the field of optics he drove the refinement of measurement to its limits and by so doing showed a skeptical world what far-reaching consequences can follow from that sort of a process and what new vistas of knowledge can be opened up by it. It was a lesson the world had to learn. The results of learning it are reflected today in the extraordinary recent discoveries in the field of electronics, of radioactivity, of vitamins, or hormones, of nuclear structure, etc. All these fields owe a large debt to Michelson, the pioneer in the art of measurement of extraordinarily minute quantities and effects."

In that paragraph I have tried to appraise Franklin's place among American scientists. Let me now express my personal judgment as to his place in world science. If I were asked to list by centuries the 14 most influential scientists who have lived since Copernicus was born in 1473, I should include the name of Franklin.

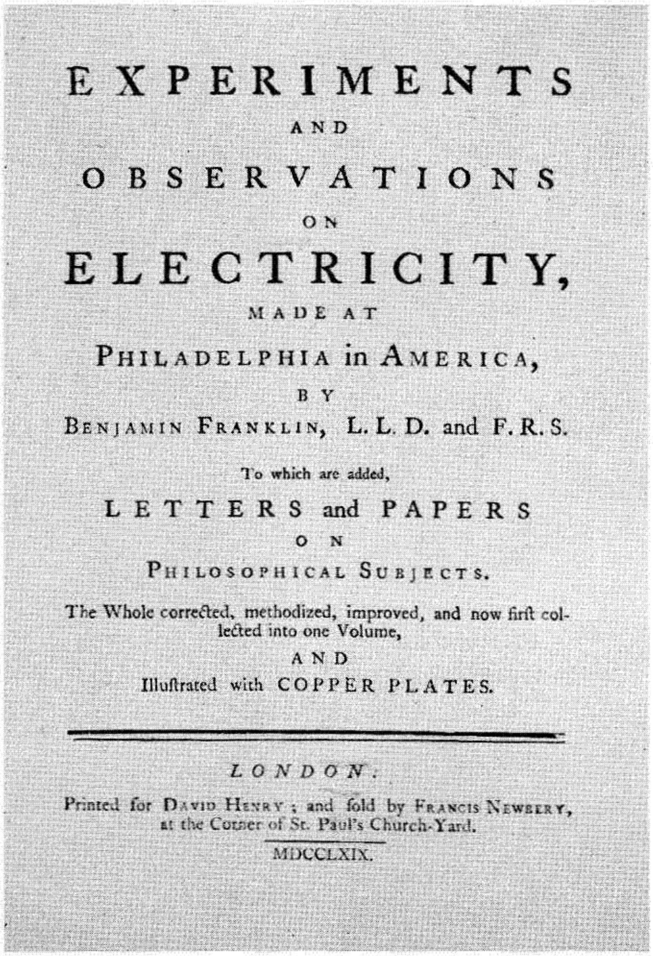
There will doubtless be those, especially among Europeans, who will say, "Why do you give Franklin so high a place when there were but seven of his 84 years, namely, from 1746 to 1753, in which he pursued science at all, also when he wrote, so far as I can discover, not a single scientific paper designed for publication in a scientific journal?" His private letters to his friend Peter Collinson, which he never expected to be published at all, are practically the sole source of our knowledge of his scientific work. Even his own estimate of his scientific achievement was so small that in his autobiography he makes but casual reference to it.



"B. Franklin of Philadelphia"—reproduction of artist's portrait showing Franklin holding a copy of the book published in 1769 describing the Franklin electrical experiments and observations.

The answer to the foregoing inquiry is that I have been guided in the placing of Franklin on such a list primarily by the significance of his contributions as measured by the influence they exerted in the development of our modern world. I have not been concerned at all with the erudition of the man, the profundity or extent of his scholarship, nor even by the magnitude and difficulty of the problems which he solved.

No one, however, can read these letters to Peter Collinson, published in 1774, without being amazed by the fact that Franklin without any previous training whatever in either the technique or the history of physics and with almost no contact with what others were doing or had done, within two years of the time of his first experiment had acquired a keener insight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into the fundamental nature of electrical phenomena, not merely than any one had acquired up to his time, but even than any of his successors acquired for the next 150 years, when, about 1900, the scientific world returned essentially to Franklin's views.

To justify this statement and to bring to light the extraordinary quality both of Franklin's physical insight and of his power of induction I shall make most of the remainder of this article consist of a few direct quotations from Peter Collinson letters which the editor informs us were being printed "without waiting for the ingenious author's permission to do so."

The first letter, dated March 28, 1747, reads:

"To Peter Collinson, Esq.; F. R. S. London

Philadelphia, March 28, 1747

"Sir,

"Your kind present of an electric tube*, with directions for using it, has put several of us on making electrical experiments, in which we have observed some particular phaenomena that we look upon to be new. I shall therefore communicate them to you in my next, though possibly they may not be new to you, as among the numbers daily employed in those experiments on your side the water, 'tis probable some one or other has hit on the same observations. For my own part, I never was before engaged in any study that so totally engrossed my attention and my time as this has lately done; for what with making experiments when I can be alone, and repeating them to my Friends and Acquaintance, who, from the novelty of the thing, come continually in crouds to see them, I have, during some months past, had little leisure for any thing else.

"I am, etc.

"B. Franklin."

*A straight three-foot glass tube as big as your wrist.

Now as to some of the experiments themselves. The very first one of them, done within a few months of the time he first heard of electricity, contains the key to his invention of the lightning rod. Note from the following how skillfully and strikingly he arranges his electrostatic experiments by making the length of the suspension of the cork ball very long. After 200 years of the development of electrostatics these experiments cannot be made more tellingly today than by setting them up and performing them exactly as Franklin directed nearly 200 years ago. He writes:

"The first is the wonderful effect of pointed bodies, both in *drawing off* and *throwing off* the electrical fire. For example,

"Place an iron shot of three or four inches diameter on the mouth of a clean dry glass bottle. By a fine silken thread from the ceiling, right over the mouth of the bottle, suspend a small cork-ball about the bigness of a marble; the thread of such a length, as that the cork-ball may rest against the side of the shot. Electrify the shot, and the ball will be repelled to the distance of four or five inches, more or less, according to the quantity of Electricity.—— When in this state, if you present to the shot the point of a long, slender, sharp bodkin, at six or eight inches distance, the repellency is instantly destroyed, and the cork flies to the shot. A blunt body must be brought within an inch, and draw a spark, to produce the same effect. To prove that the electrical fire is *drawn off* by the point, if you take the blade of the bodkin out of the wooden handle, and fix it in a stick of sealing-wax, and then present it at the distance aforesaid, or if you bring it very near, no such effect follows; but sliding one finger along the wax till you touch the blade, and the ball flies to the shot immediately."

Here is where he learned that his lightning rod had to have a good ground in order to work at all. He continues:

"To show that points will *throw off* as well as *draw off* the electrical fire, lay a long sharp needle upon the shot, and you cannot electrise the shot so as to make it repel the cork-ball. . . . Or fix a needle to the end of a suspended gun-barrel, or iron-rod, so as to point beyond it like a little bayonet; and while it remains there, the gun-barrel, or rod, cannot by applying the tube to the other end be electrised so as to give a spark, the fire continually running out silently at the point."

I can find no evidence that prior to Franklin the electrical properties of points had been discovered at all. He continues:

"The repellency between the cork-ball and the shot is likewise destroyed, (1) by sifting fine sand on it; this does it gradually; (2) by breathing on it; (3) by making a smoke about it from burning wood; (4) by candle-light, even though the candle is at a foot distance: these do it suddenly. . . . The light of a bright coal from a wood fire; and the light of a red-hot iron do it likewise; but not at so great a distance.

"The light of the sun thrown strongly on both cork and shot by a looking-glass for a long time together, does not impair the repellency in the least. This difference between fire-light and sun-light is another thing that seems new and extraordinary to us.*"

The insight shown in the last three lines, in which he correctly makes particle carriers (ions, we now call them) from the match do the discharging while sunlight produces no ions and therefore does not discharge, is unbelievably penetrating for a date 200 years back, though the conception of neutral particles being first attracted and then repelled is of course definitely wrong.

The next experiment, with its interpretation, is probably the most fundamental thing ever done in the field of electricity. Get it exactly in Franklin's words:

"1. A person standing on wax, and rubbing the tube, and another person on wax drawing the fire, they will both of them (provided they do not stand so as to touch one another) appear to be electrised, to a person standing on the floor; that is, he will receive a spark on approaching each of them with his knuckle.

"2. But if the persons on wax touch one another during the exciting of the tube, neither of them will appear to be electrised.

"3. If they touch one another after exciting the tube, and drawing the fire as aforesaid, there will be a stronger spark between them than was between either of them and the person on the floor.

"4. After such strong spark, neither of them discover any electricity.

"These appearances we attempt to account for thus: We suppose, as aforesaid, that electrical fire is a common element [we now call "electrical fire" electrons], of which every one of the three persons above mentioned has his equal share, before any operation is begun with the tube. *A*, who stands on wax and rubs the tube, collects the electrical fire from himself into the glass; and his communication with the common stock being cut off by the wax, his body is not again immediately supply'd. *B*, (who stands on wax likewise) passing his knuckle along near the tube, receives the fire which was collected by the glass from *A*; and his communication with the common stock being likewise cut off, he retains the additional quantity received.—To *C*, standing on the floor, both appear to be electrised; for he, having only the middle quantity of electrical fire, receives a spark upon approaching *B*, who has an over quantity; but gives one to *A*, who has an under quantity. If *A* and *B* approach to touch each other, the spark is stronger, because the difference between them is greater; after such touch there is no spark between either of them and *C*, because the electrical fire in all is reduced to the original equality. If they touch while electrising, the equality is never destroy'd, the fire only circulating. Hence have arisen some new terms among us: we say *B* (and bodies like circumstanced) is electrised *positively; A, negatively*. Or rather, *B* is electrised *plus; A, Minus*. And we daily in our experiments electrise bodies *plus* or *minus*, as we think proper.—To electrise *plus* or *minus*, no more needs to be known than this, that the parts of the tube or sphere that are rubbed, do, in the instant of the friction, attract the electrical fire, and therefore take it from the thing rubbing; the same parts immediately, as the friction upon them ceases, are disposed to give the fire they have received, to any body that has less."

The next two long letters are taken up largely with what he calls "M. Muschenbroek's wonderful bottle," accidentally discovered in Leyden one year earlier, 1746, now known as the Leyden jar, and with explaining all such effects just as we do today in terms of the opposite charges or the inner and outer coats. Thus, to use his exact words:

"At the same time that the wire and top (inside coat) of the bottle is electrified positively or plus the bottom (outside coat) of the bottle is electrified negatively or minus, in exact proportion: i.e., whatever quantity of electrical fire is thrown in at the top an equal quantity goes out at the bottom." And "Again, when the bottle is electrised, but little of the electrical fire can be drawn out from the top by touching the wire unless an equal quantity can at the same time *get in* at the bottom. Thus, place an electrised bottle in clean glass or dry wax and you will not, by touching the wire get out the fire from the top."

These chapters, too, contain the uncannily clever experiment of showing, just as we do today, that the charge resides in or on the dielectric. How many of us realize that the familiar classroom experiment of removing the coats of a Leyden jar and touching each of them, then putting them back again, and after that getting a strong spark by connecting the replaced coatings with a wire was devised by Benjamin Franklin in 1749? Again, he says:

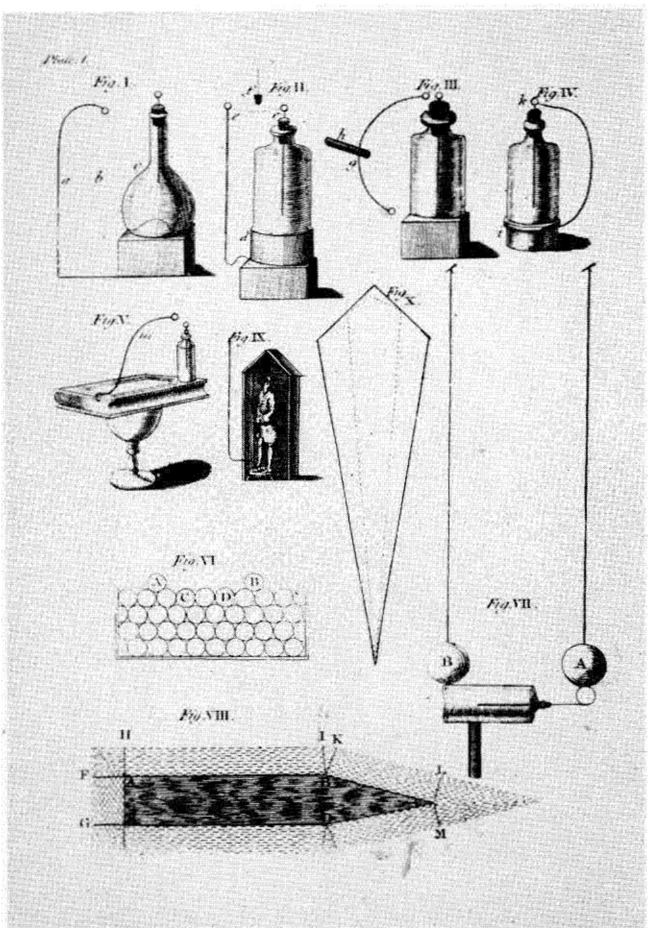
*"This different effect probably did not arise from any difference in the light, but rather from the particles separated from the candle, being first attracted and then repelled, carrying off the electric matter with them."

"There is one experiment more which surprises us, and is not hitherto satisfactorily accounted for; it is this: Place an iron shot on a glass stand, and let a ball of damp cork, suspended by a silk thread, hang in contact with the shot. Take a bottle in each hand, one that is electrified through the hook, the other through the coating: Apply the giving wire to the shot, which will electrify it *positively*, and the cork shall be repelled; then apply the requiring wire, which will take out the spark given by the other; when the cork will return to the shot: Apply the same again, and take out another spark, so will the shot be electrified *negatively*, and the cork in that shall be repelled equally as before. Then apply the giving wire to the shot, and give the spark it wanted, so will the cork return: Give it another, which will be an addition to its natural quantity, so will the cork be repelled again: And so may the experiment be repeated as long as there is any charge in the bottles. Which shows that bodies having less than the common quantity of electricity, repel each other, as well as those that have more."

In that last sentence Franklin states clearly that matter which had lost its normal amount of electricity was self repellent. In modern terms the atom is neutral when it has its full complement of electrons. When any of these are removed the nuclei repel one another.

In some of his letters, notably the fifth, Franklin goes off into long and incorrect speculations as to the difference between the terms "electric bodies per se" and "non electric bodies." But this adds to, rather than subtracts from my own appreciation of him, for no human being could possibly have seen correctly all the ele-

(*Continued on Page* 16)



Copperplate line cut appearing as "Plate I" of the book, illustrating the experiments described in the Franklin letters to Collinson.

ing these non-ferrous materials into exchanges, pressure vessels, etc. This "know how" will be very useful to the new petro-chemical industry. Some examples of non-ferrous equipment are: copper or Everdur reaction-chambers, nickel salt-handling equipment, Monel or Inconel evaporators, and solid Hastelloy (nearly non-ferrous) for plastic-compounding.

Many of the newer cracking and reforming operations involve dehydrogenation. In these, as well as in the direct hydrogenation process, the presence of free hydrogen may cause the phenomenon known as hydrogen-penetration. This causes progressive deterioration of the steel and to date, I am told, no fully satisfactory remedy has been found. Vessels have been made with walls twice as thick as would otherwise be required, but still the hydrogen seeps through.

Now a word about abrasion. Without referring too specifically to the mechanism of the several new catalytic processes, it may be said that at least two of them use finely divided solids as catalysts, and these fine solids are caused to flow in suspension in fluids. During this flow, and in subsequent separation (in one process), the solid particles act abrasively. Equipment handling this mixed flow condition may be either of abrasive-resistant material like the workable low-manganese alloy steels, or, the anti-corrosion claddings or liners, by virtue of their generally better physical properties, hardness and tensile strength, may offer long enough economic life. If temperature permits, one should not overlook the fact that rubber linings are often resistant to both abrasion and corrosion.

CONCLUSION

The development of the new applied science of petrochemistry is just beginning. As new processes, new reactions, new catalysts are discovered, and new products are developed from petroleum there will be more new equipment—perhaps unlike any we have yet seen. That is the only conclusion with which this article can end.

Photo on page 13 courtesy the Lummus Co.

Benjamin Franklin

(Continued from Page 9)

ments of a huge and thus far completely unexplored field, and his wrong steps give him opportunity to show his greatness by the way he goes to work to discover and to admit his error. Thus, he writes as follows:

> "*Query*, Wherein consists the difference between an *electric* and a *non-electric* body?
>
> "Answer, The terms electric *per se*, and non-electric, were first used to distinguish bodies, on a mistaken supposition that those called electrics *per se*, alone contained electric matter in their substance, which was capable of being excited by friction, and of being produced or drawn from them, and communicated to those called non-electrics, supposed to be destitute of it: For the glass, etc., being rubb'd, discover'd signs of having it, by snapping to the finger, attracting, repelling, etc. and could communicate those signs to metals and water.—Afterwards it was found, that rubbing of glass would not produce the electric matter, unless a communication was preserved between the rubber and the floor; and subsequent experiments proved that the electric matter was really drawn from those bodies that at first were thought to have none in them. Then it was doubted whether glass and other bodies called *electrics per se*, had really any electric matter in them, since they apparently afforded none but what they first extracted from those which had been called non-electrics. But some of my experiments show that glass contains it in great quantity, and I now suspect it to be pretty equally diffused in all the matter of this terraqueous globe. If so, the terms *electric per se*, and *non-electric*, should be laid aside as improper; and (the only difference being this, that some bodies will conduct electric matter, and others will not) the terms *conductor* and *non-conductor* may supply their place."

Without doubt the most profound paragraphs in all of Franklin's letters are the following, written in 1749:

> "1. The electrical matter consists of particles extremely subtile, since it can permeate common matter, even the densist metals, with such ease and freedom as not to receive any perceptible resistance.
>
> "2. If any one should doubt whether the electrical matter passes through the substance of bodies, or only over and along their surfaces, a shock from an electrified large glass jar, taken through his own body, will probably convince him.
>
> "3. Electrical matter differs from common matter in this, that the parts of the latter mutually attract, those of the former mutually repel each other. Hence the appearing divergency in the stream of electrified effluvia.
>
> "4. But though the particles of electrical matter do repel each other, they are strongly attracted by all other matter.
>
> "5. From these three things, the extreme subtility of the electrical matter, the mutual repulsion of its parts, and the strong attraction between them and other matter, arise this effect, that, when a quantity of electrical matter is applied to a mass of common matter, of any bigness or length, within our observation (which hath not already got its quantity) it is immediately and equally diffused through the whole.
>
> "6. Thus common matter is a kind of spunge to the electrical fluid. And as a spunge would receive no water if the parts of water were not smaller than the pores of the spunge; and even then but slowly, if there were not a mutual attraction between those parts and the parts of the spunge; and would still imbibe it faster, if the mutual attraction among the parts of the water did not impede, some force being required to separate them; and fastest, if, instead of attraction, there were a mutual repulsion among those parts, which would act in conjunction with the attraction of the spunge. So is the case between the electrical and common matter.
>
> "7. *But in common matter there is (generally) as much* of the electrical as it will contain within its substance. If more is added, it lies without upon the surface, and forms what we call an electrical atmosphere; and then the body is said to be electrified."

In these paragraphs Franklin states with great succinctness what later became known as the Franklin one-fluid theory, and after 1900 was known as the electron theory. In his day and for 150 years thereafter it received very scant consideration in the old world, and the so-called two-fluid theory of Aepinus, put forward a little later, was universally taught in textbooks the world over up to the triumph of the electron theory in 1897 under the active leadership of J. J. Thomson, who himself pointed out that this electron theory was in essential particulars a return to the theory put forth by Franklin in 1749. For Franklin's electrical matter consisted of extremely subtle mobile particles (now called negative electrons), which in order to make matter exhibit its common or neutral properties had to be present in each kind of matter (we now say in each kind of atom; but the atomic theory had not been formulated in 1749) in a particular number, an increase in which number made it exhibit electrification of one sign, a decrease, an electrification of the opposite sign. In Franklin's theory only one kind of electrical matter was mobile, the other sign of electrification appeared when the mobile kind was removed so that it could no longer neutralize the effect of the opposite kind which *inhered in the immobile part of the matter (i. e., in the* nucleus).

The Franklin theory was mathematically identical with the two-fluid theory, but while the former was a definite and profound physical theory the latter was a hold-over from medieval mysticism. It came from the age of the so-called "imponderables"—an imponderable or weightless heat theory, the caloric—and the imponderable electric fluids. Such vague, tenuous, contradictory ideas were ill at home in the highly realistic, practical mind of Franklin. They were justified, like Faraday's lines of magnetic force, as analytical conveniences but not as physical realities. Franklin introduced a definite physical theory which rendered unnecessary such fantastic conceptions as two weightless and hence non-existent fluids introduced for purely *ad hoc* purposes, and then told to destroy each other, also for *ad hoc* purposes.

Let us now return to Franklin's discussion of points and their properties of throwing off or drawing off the electrical fire. He says, very modestly and wisely:

> "These explanations of the power and operation of points, when they first occurred to me, and while they first floated in my mind, appeared perfectly satisfactory; but now I have written them, and considered them more closely, I must own I have some doubts about them; yet, as I have at present nothing better to offer in their stead, I do not cross them out; for even a bad solution read, and its faults discovered, has often given rise to a good one, in the mind of an ingenious reader."

Then in the next paragraph note how clearly he sees the necessity of eliminating unnecessary hypotheses, i. e., he adopts the scientific principle of "minimum hypothesis."

> "Nor is it of much importance to us, to know the manner in which nature executes her laws; it is enough if we know the laws themselves. It is of real use to know that china left in the air unsupported will fall and break; but *how* it comes to fall, and *why* it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it."

He then describes some discharging effects of points conducted on a larger scale than he had before attempted, and in a later paper dated November 7, 1749, he enumerates all the known points of resemblance between lightning and electricity, and concludes with the comment:

> "The electric fluid is attracted by points. We do not know whether this property be in lightning but since they agree in all points in which we can compare them, it is not improbable that they agree likewise in this. Let the experiment be made."

In June, 1752, he made it, carrying out in a shed with his son the experiment which he describes as follows in his letter of October 19, 1752, to Peter Collinson.

> "As frequent mention is made in public papers from Europe of the success of the Philadelphia experiment for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, etc. It may be agreeable to the curious to be informed that the same experiment has succeeded in Philadelphia, though made in a different and more easy manner, which is as follows:
>
> "Make a small cross of two light strips of cedar, the arms so long as to reach to the four corners of a large thin silk handkerchief when extended; tie the corners of the handkerchief to the extremities of the cross, so you have the body of a kite; which being properly accommodated with a tail, loop, and string, will rise in the air, like those made of paper; but this being of silk is better to bear the wet and wind of a thunder gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp pointed wire, rising a foot more above the wood. To the end of the twine, next the hand, is to be tied a silk ribbon, and where the silk and twine join, a key may be fastened. This kite is to be raised when a thunder-gust appears to be coming on, and the person who holds the string must stand within a door or window, or under some cover, so that the silk ribbon may not be wet; and care must be taken that the twine does not touch the frame of the door or window. As soon as any of the thunder clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified, and the loose filaments of the twine will stand out every way, and be attracted by an approaching

finger. And when the rain has wet the kite and twine, so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle. At this key the phial may be charged; and from electric fire thus obtained, spirits may be kindled, and all the other electric experiments be performed, which are usually done by the help of a rubbed glass globe or tube, and thereby the sameness of the electric matter with that of lightning completely demonstrated."

In a further letter written in September, 1753, he says: "In September 1752 I erected an iron rod to draw the lightning down into my house, in order to make some experiments on it." He carried on these experiments for some months to learn whether the clouds were positively or negatively electrified, and after many trials he says:

"I concluded that the clouds are *always* electrified *negatively*, or have always in them less than their natural quantity of the electric fluid.

"Yet notwithstanding so many experiments, it seems I concluded too soon; for at last, June the 6th, in a gust which continued from five o'clock P. M. to seven, I met with one cloud that was electrified positively, though several that passed over my rod before, during the same gust, were in the negative state."

The foregoing shows what most commendable scientific care he took in his experiments and what caution he used in drawing conclusions.

But he did not stop with making scientific experiments. His active and practical mind was not satisfied until he had applied it to the useful end of the invention of the lightning rod, as indicated in the first paragraph of the letter of October 19, 1752, quoted above.

After his definite proof of the identity of lightning and electricity he was recognized by the most distinguished English scientists by being elected to the Royal Society, and was presented for the year 1753 the Copley medal of the Society, the highest honor within the gift of the world's most illustrious scientific body.

Santa Fe Dam

(*Continued from Page 6*)

1943, at which time heavy and prolonged rains caused flood conditions of major proportions to develop along the river channel. During this week the highest rainfall intensity recorded to date in the United States was measured in the mountains a few miles east of the dam, where 25 inches of rain fell in 24 hours. Tremendous quantities of material were washed down into the reservoir area, and wide gullies were cut in the upper end of the reservoir borrow pit. Floating debris partially choked up the trash racks, causing the water to be backed up in the reservoir and threatening two of the grizzly plants with inundation, but quick work of removing trash with a dragline eliminated the hazard. Construction work was halted by this and succeeding storms for a total of five weeks and clean-up work continued for many weeks more; yet in spite of delays, construction was completed four months ahead of schedule.

Principal credit for maintaining the production schedule regardless of delays was due to the fine spirit of co-operation between the contractors, represented by Project Manager R. F. Rasey, and the U. S. Engineering Department. J. G. Morgan was resident engineer for the government.