THE TWO HUNDREDTH ANNIVERSARY OF BENJAMIN FRANKLIN'S TWO LIGHTNING EXPERIMENTS AND THE INTRODUCTION OF THE LIGHTNING ROD*

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1. INTRODUCTION

Two hundred years have elapsed since the first experiments indicated that the lightning discharge is an electrical phenomenon. Books and articles—on American history, on Franklin, physics, and the history of science—exhibit so much uncertainty and even confusion about this subject that the bicentenary of the performance of these experiments may fittingly be marked by an attempt to bring together the relevant printed and manuscript documents and to ascertain, in so far as possible, the sequence of events in Franklin's research on lightning in 1752.

The one fact about Benjamin Franklin's scientific career that is known to every reader of American history is that he once flew a kite during a thunder-storm; yet I am sure that most Americans would find extreme difficulty in answering the questions of precisely why he flew the kite and exactly what he learned in the process. Depicted in a famous Currier and Ives print (see fig. 1), the familiar picture of Franklin raising his kite during a thunder-storm has become dear to generation after generation, while his magnificent contributions—both theoretical and experimental—to the budding science of electricity, which gained him in his lifetime the adulatory title of the "Newton" of the subject, have been ignored or forgotten.[1] Hence, it is not particularly surprising that the significance of the kite experiment in terms of the development and acceptance of Franklin's ideas about electricitiy, or even lightning, has gradually become lost. Even those physicists who can readily understand what the experiment proved are apt to add such a statement as :"Perhaps the most wonderful part of it was that Franklin was not killed at once."[2]

A statement has been published in the *Proceedings* of this Society that the kite experiment was never performed at all:

> You have all heard the story of his kite-experiment, in which he [Franklin] got electric sparks between a kite-string and a key while the kite was flying in or near a thunder cloud. I regret to have to inform you that in the opinion of local historians this is just a myth, one of those legends which spring up from unknown sources to adorn the story of a great man.[3]

Rather than a myth or legend springing from unknown sources, the account of the kite experiment derives directly from Franklin himself. Even though there may be uncertainty about the actual day on which the kite was first flown, I do not believe that the performance of the experiment as such is subject to legitimate doubt.

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[1] The writer is at present completing a monograph on this subject which will be published by the American Philosophical Society. A preliminary report has appeared in the American Philosophical Society *Year Book* for 1949: 240–243, 1950; see also Benjamin Franklin, an experimental Newtonian scientist, *Bull. Amer. Acad. Arts and Sci.* **5** (4) : 2–6, 1952.

[2] Black, N. H., *An introductory course in college physics,* 388, N. Y., Macmillan, ed. 3, 1948.

[3] *Proc. Amer. Philos. Soc.* **91**: 17, 1947. Note that Franklin did not get "sparks between a kite-string and a key," but rather between the key and his knuckle.

FIG. 1. Franklin and the Kite, as represented by Currier and Ives. This "stock" representation of the great experiment contains one important error which is commonly found in pictures of the kite experiment. Franklin's son is shown as a young boy, although he was at least twenty-one years of age; about two years before the kite experiment, i.e., on 12 April 1750, Franklin had written to his mother about family affairs, noting: "As to your Grandchildren, Will is now nineteen years of age. . . ." Furthermore, Franklin grasps the string above the key, he is not sheltered, and he is near a house, etc. (Reproduced from the copy belonging to Mr. Ernest A. Hale of the Suffolk First Federal Savings & Loan Association, Franklin Street, Boston.)

In the following pages, I will indicate the evidence that the kite experiment was performed: not only by Franklin but by others. It may come as a surprise, however, to learn that this experiment had been independently conceived by a French experimenter, Romas. Actually, the electrical kite was the second test instrument that Franklin had devised to investigate the electrification of clouds; the earlier one was the insulated rod in that "sentry-box" experiment which had been brought to a successful issue even before Franklin raised his kite and which would have rendered the kite experiment unnecessary, had the news reached Franklin in time. I believe that the evidence in the following pages will fully support Franklin's statement that the kite experiment was performed before he had heard of the success in Europe of his sentry-box experiment. We may conclude that the kite experiment was performed by Franklin in June 1752, and that the first grounded lightning rods were introduced to the world in Philadelphia in 1752 by Benjamin Franklin—probably in June, but possibly in July.

2. FRANKLIN'S KITE EXPERIMENT

The letter in which Franklin described his kite experiment was written from Philadelphia on 1 October 1752. It was addressed to Peter Collin-

son, F.R.S., who had earlier provided Franklin with some simple apparatus for performing electrical experiments; many of Franklin's earliest contributions were addressed to Collinson.[4] A copy of the original letter, at present in the archives of the Royal Society of London, is reproduced as figure 2. It reads as follows:[5]

From Ben[n]: Franklin, Esq. Philadelphia
To P. Collinson Octo: 1: 1752

As Frequent mention is made in the Publick papers from Europe, of the Success of the *Philadelphia-Experiment* for drawing the Electrick Fire from Clouds by means of Pointed Rods of Iron erected on high Buildings &c., it may be agreeable to the Curious to be informed, that the same Experim[t]. has succeeded in Philadelphia Tho' made in a Different & more Easie manner, which any one may try 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 accomodated with a Tail, Loop, & String, will rise in the Air like those made of paper; but this being of Silk is fitter to bear the Wett & 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, riseing a foot or more above the Wood.

To the end of the Twine, next the hand, is to be tied a Silk Ribon; and where the Twine & Silk joyn, a Key may be fasten'd.

The Kite is to be raised, when a Thunder Gust appears to be comeing on (which is very frequent in this Country) & The Person who holds the String, must stand within a Door, or Window, or under some cover, so that the Silk Ribon may not be Wet; & care must be taken, that the Twine does not touch the Frame of the Door or Window.

As soone as any of the Thunder Clouds come over the Kite, the pointed wire will draw the Electric Fire from them, & 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.

When the Rain has Wett the Kite & 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 Electrical Experim[ts] be performed, which are usually done by the help of a rubbed Glass Globe or Tube, & thereby the Sameness of the Electric Matter with that of Lightning compleatly demonstrated.

I was pleased to hear of the Success of My experiments in France, & that they there begin to Erect points on their buildings. We had before placed them upon our Academy & State House Spires.

This letter was read at the Royal Society on 21 December 1752 and was published in the Society's *Philosophical Transactions* for 1751 and 1752. It was also printed in the *Gentleman's Magazine* and the *London Magazine* and was included in the second part of Franklin's book on electricity. In America it was published in Franklin's *Pennsylvania Gazette,* and reprinted in the Boston *Gazette* and in other publications.[6]

A further and corroborative source of information is provided by Joseph Priestley's *History and present state of electricity.* This book has a special value, as Jernegan has shown,[7] since Franklin was in close contact with Priestley and undertook to supply Priestley with the books he needed. Furthermore, Priestley wrote that he had "kept up a constant correspondence with Dr. Franklin, and the rest of my philosophical friends in London;

[4] *Cf.* Cohen, I. Bernard, *Benjamin Franklin's experiments: a new edition of Franklin's "Experiments and observations on electricity," edited, with a critical and historical introduction,* intro., ch. III, pt. 1, Cambridge, Harvard Univ. Press, 1941; also Brett-James, Norman G., *The life of Peter Collinson* [London], The Friends' Bookshop [no date].

[5] This letter is reproduced with the kind permission of the Royal Society of London.

[6] The text of this letter in *Phil. Trans. Roy. Soc.* **47**: 565–566, 1751 and 1752 is identical, word for word, with the manuscript copy and follows the same paragraphing. The only difference between the two is in punctuation and spelling, e.g., "Publick" [in the MS] for "public" [in the *Phil. Trans.*], "*Philadelphia-Experiment*" for "Philadelphia experiment," "Electrick" for "electric," "easie" for "easy," "Ribon" for "riband," "joyn" for "join," "soone" for "soon," "compleatly" for "completely," "riseing" for "rising," "comeing" for "coming," and generally, "&" for "and." When printed in the *Pennsylvania Gazette,* the *Gentleman's Magazine, London Magazine,* and the several editions of his book on electricity, the letter bore the date of 19 October 1752 rather than 1 October 1752, and the final paragraph of the letter was missing. On this score, see, *infra,* § 5, and especially footnote 50.

[7] Jernegan, Marcus W., Benjamin Franklin's "electrical kite" and lightning rod, *New England Quart.* **1**: 180–196, 1928. The value of Priestley's testimony had been recognized earlier by James Parton (*cf. Life and times of Benjamin Franklin* **1**: 295, N. Y., Mason Brothers, 1864), who, in turn, had obtained his information from Sparks. The latter had prefaced the section of his edition containing the electrical papers by a pair of long extracts from Priestley and Stuber (Sparks, Jared, *The works of Benjamin Franklin* **5**: 173–180, Phila., Childs & Peterson, revised ed., 1840).

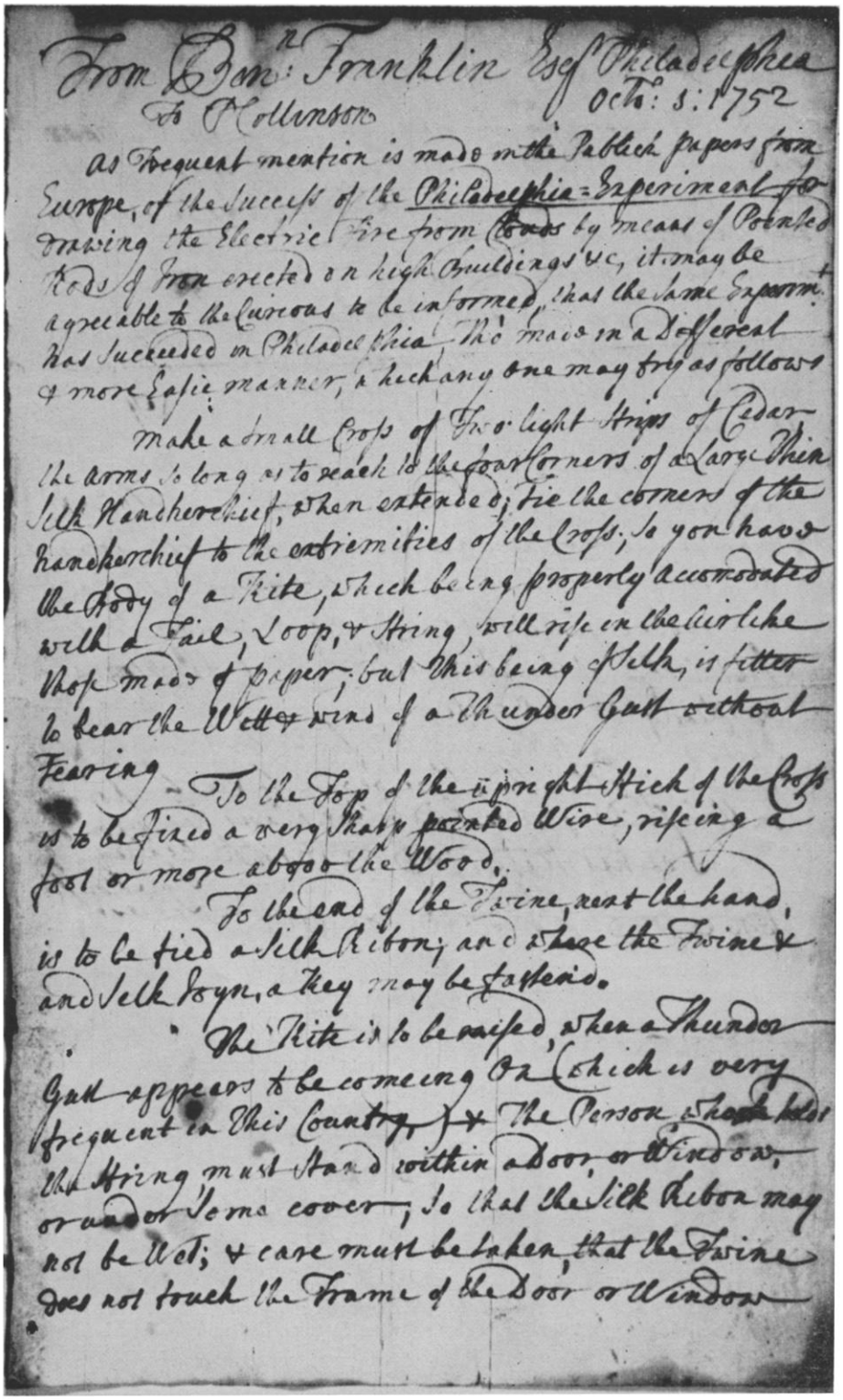
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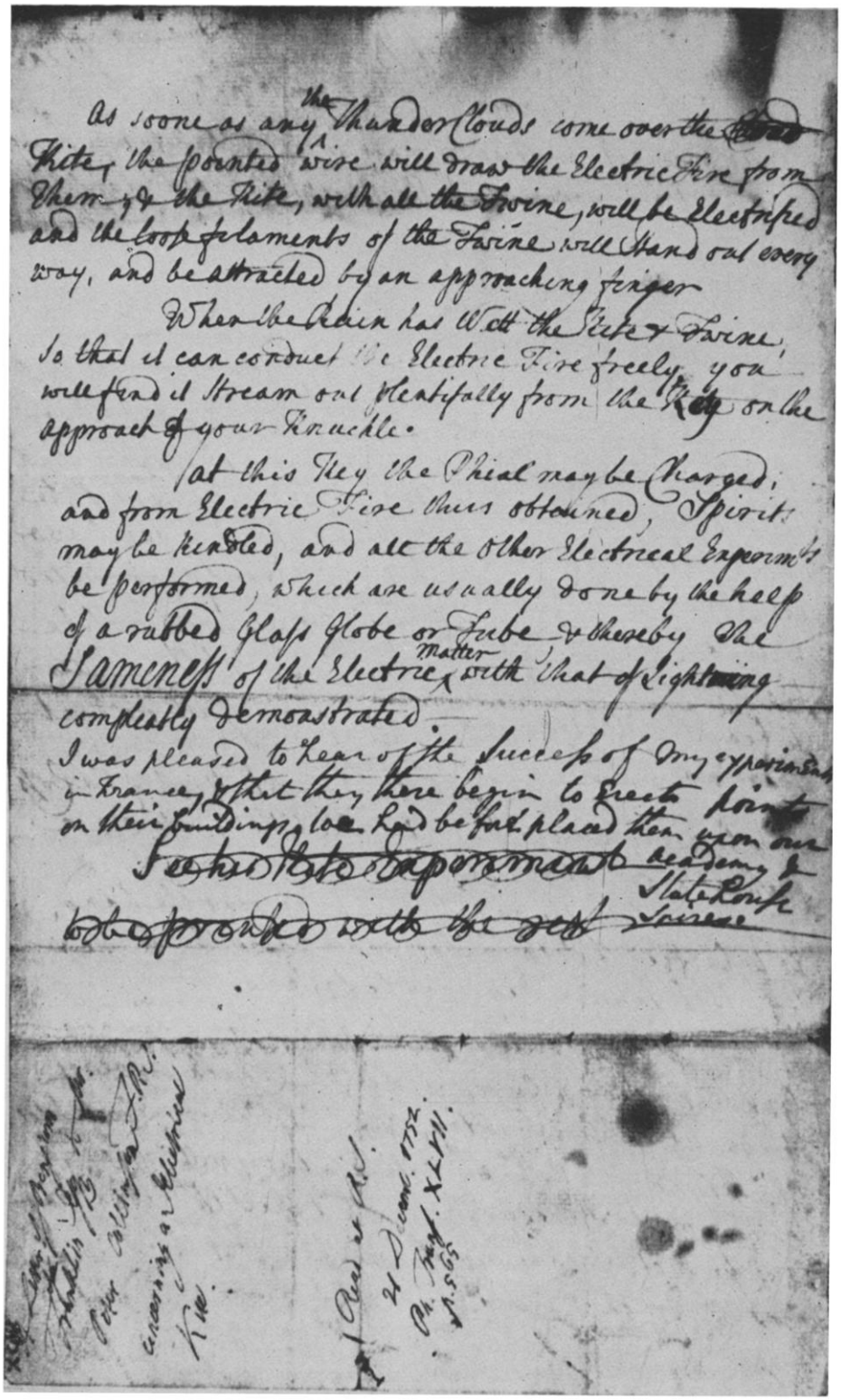
FIG. 2. An eighteenth-century transcript of Franklin's kite letter; the original is in the archives of the Royal Society of London and is reproduced by permission. This copy appears to be in the hand of Peter Collinson and, although the spelling of several words differs from the text printed in the *Philosophical Transactions,* the date is the



same, and the paragraphing is the same as the version published in the *Philosophical Transactions;* it should also be noted that the final paragraph about "points" erected in Philadelphia, is also included in this version.

and my letters circulated among them all, as also every part of my history as it was transcribed." [8] Priestley's description appears, therefore, to have been based on information provided by Franklin himself and, since Franklin read the manuscript, must have had the seal of his approval. This account is more detailed than Franklin's letter to Collinson; for this reason Carl Van Doren preferred Priestley's description to Franklin's, and printed it in his edition of *Benjamin Franklin's autobiographical writings,* noting that it "contains precise details about the kite experiment which Priestley could have had only from Franklin, to whom Priestley refers as 'the best authority.' " [9] Priestley's account follows:

As every circumstance relating to so capital a discovery (the greatest, perhaps, since the time of Sir Isaac Newton) cannot but give pleasure to all my readers, I shall endeavour to gratify them with the communication of a few particulars which I have from the best authority.

The Doctor, having published his method of verifying his hypothesis concerning the sameness of electricity with the matter of lightning, was waiting for the erection of a spire in Philadelphia to carry his views into execution, not imagining that a pointed rod of a moderate height could answer the purpose, when it occurred to him that by means of a common kite he could have better access to the regions of thunder than by any spire whatever. Preparing, therefore, a large silk handkerchief and two cross-sticks of a proper length on which to extend it, he took the opportunity of the first approaching thunder-storm to take a walk in the fields, in which there was a shed convenient for his purpose. But, dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to nobody but his son who assisted him in raising the kite.

The kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without any effect, when, at length, just as he was beginning to despair of his contrivance, he observed some loose threads of the hempen string to stand erect and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key, and (let the reader judge of the exquisite pleasure he must have felt at that moment) the discovery was complete. He perceived a very evident electric spark. Others succeeded, even before the string was wet, so as to put the matter past all dispute, and when the rain had wet the string he collected electric fire very copiously. This happened in June 1752, a month after the electricians in France had verified the same theory, but before he heard of anything they had done.[10]

On the basis of Priestley's report, we have further reason to believe that Benjamin Franklin performed the experiment as he described it in his letter to Collinson, and that the date was June 1752.

Stripped to its barest essentials, the kite experiment employed a long insulated conductor, terminating in a point at its uppermost end which was raised high above the ground; when a thunder-cloud passed overhead, Franklin momentarily grounded the insulated conductor by bringing his knuckle to the key, whereupon a spark passed between his knuckle and the key. The similarity between this experiment and the familiar experiments of the laboratory showed that thunder-clouds are electrostatically charged; hence their discharge must be an electrical discharge, differing in scale, but not in kind, from those produced in the laboratory.[11]

[8] From a letter from Priestley to Rotheram, dated 14 February 1766, quoted in Jernegan (ref. 7, *supra*), 187. In the preface to his history, Priestley wrote: "With gratitude I acknowledge my obligations to Dr. Watson, Dr. Franklin, and Mr. Canton, for the books, and other materials with which they have supplied me, and for the readiness with which they have given me any information in their power to procure," Priestley, Joseph, *The history and present state of electricity, with original experiments* 1: xi, London, C. Bathurst and T. Lowndes *et al.*, ed. 3 "corrected and enlarged," 1775. *Cf.* Walker, W. Cameron, The beginnings of the scientific career of Joseph Priestley, *Isis* **21**: 81–97, 1934, esp. pp. 87, 89.

[9] Van Doren, Carl, *Benjamin Franklin's autobiographical writings,* 76, N. Y., Viking, 1945.

[10] Priestley, History (ref. 8, *supra*) 1: 216–217. This account is preceded by a résumé of Franklin's letter to Collinson: "To demonstrate, in the completest manner possible, the sameness of the electric fluid with the matter of lightning, Dr. Franklin, astonishing as it must have appeared, contrived actually to bring lightning from the heavens, by means of an electrical kite, which he raised when a storm of thunder was perceived to be coming on. This kite had a pointed wire fixed upon it, by which it drew the lightning from the clouds. This lightning descended by the hempen string, and was received by a key tied to the extremity of it; that part of the string which was held in his hand being of silk, that the electric virtue might stop when it came to the key. He found that the string would conduct electricity even when nearly dry, but that when it was wet, it could conduct it quite freely: so that it would stream out plentifully from the key, at the approach of a person's finger. At this key he charged phials, and from electric fire thus obtained, he kindled spirits, and performed all other electrical experiments which are usually exhibited by an excited globe or tube."

[11] For a discussion of what actually is believed to occur in such an experiment, see, *infra,* fig. 5, and the material in § 6 above footnotes 61–64; also footnote 98.

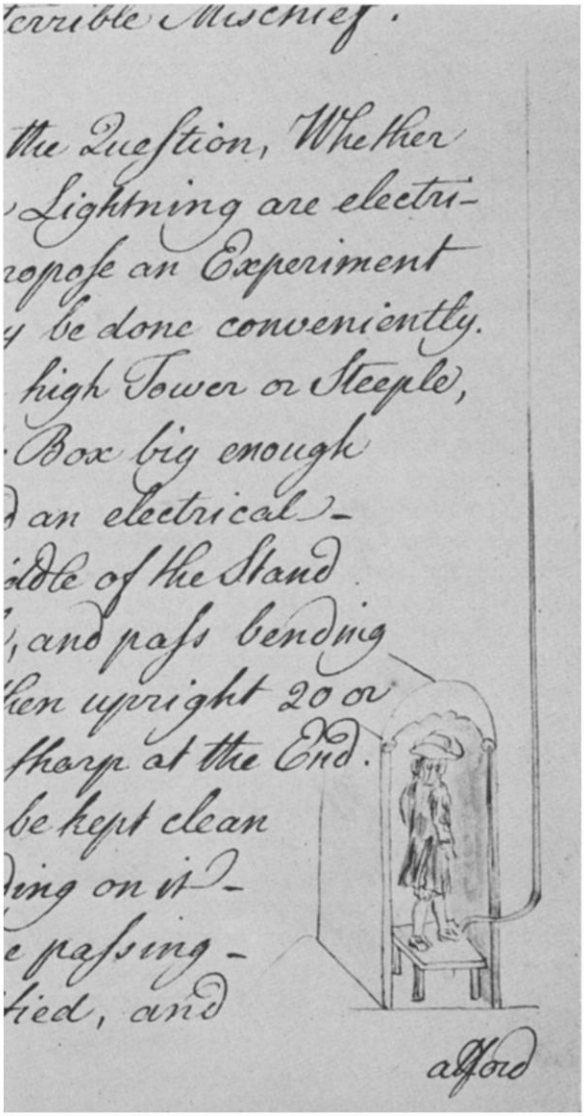


FIG. 3. Franklin's sentry-box experiment. This drawing is reproduced from the "Bowdoin MS" [12]—the drawing was made under Franklin's supervision, probably not by Franklin himself. (Reproduced with the permission of the American Academy of Arts and Sciences.)

[12] The "Bowdoin MS" was discovered by the writer during the preparation of *Benjamin Franklin's experiments* (ref. 4, *supra*) and is described on pages 152–154 of that work. It consists of a manuscript copy of all of Franklin's earliest papers on electricity, copied by two amanuenses and corrected by Franklin, and sent by Franklin to Bowdoin in 1750. As such it may be considered to replace the original MS copies of the letters, which no longer exist. In a number of instances, the "Bowdoin MS" gives a complete text, names of addressees, and dates, which are not to be found in either the printed editions of Franklin's book on electricity, or the collected editions of his writings; the text in *Benjamin Franklin's experiments* has been annotated to include the corrections made necessary by the "Bowdoin MS."

3. FRANKLIN'S SENTRY-BOX EXPERIMENT

The original experiment designed by Franklin to test the hypothesis that lightning is an electrical discharge between clouds, or between clouds and the earth, occurs in a paper entitled "Opinions and conjectures, concerning the properties and effects of the electrical matter, arising from experiments and observations, made at Philadelphia, 1749," and enclosed in a letter written by Franklin to Peter Collinson from Philadelphia, 29 July 1750. It reads as follows:

> To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending out of the door, and then upright 20 or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man should be apprehended (though I think there would be none) let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him.[13] [See fig. 3.]

It will be noted that if this experiment be stripped to its essentials, it is identical to the experiment of the kite; here, too, a pointed, insulated conductor is used to indicate whether thunderclouds overhead are electrically charged. In fact, the only real difference between the two experiments is that in the case of the kite the upper end of the pointed, insulated conductor is much higher in the air than the 20 or 30 feet above "some high tower or steeple" recommended by Franklin for the sentry-box experiment.

Franklin's description of the sentry-box experiment was published in his book on electricity, *Experiments and Observations on Electricity, made at Philadelphia in America,* issued in London in 1751.[14] A French translation, made by Jean François Dalibard (or d'Alibard) at the request

[13] *Benjamin Franklin's experiments* (ref. 4, *supra*), 222.

[14] A description of the various editions of Franklin's book on electricity may be found in *Benjamin Franklin's experiments* (ref. 4, *supra*).

of Buffon, was published in France in 1752.[15] This book created something of a sensation.

> The Philadelphian experiments . . . having been universally admired in France, the King desired to see them performed. . . . His Majesty saw them with great satisfaction, and greatly applauded Messieurs Franklin and Collinson. These applauses of his Majesty having excited in Messieurs de Buffon, D'Alibard and de Lor, a desire of verifying the conjectures of Mr. Franklin, upon the analogy of thunder and electricity, they prepar'd themselves for making the experiment.[16]

The above description is taken from the report of the Abbé Mazéas, in a letter to Stephen Hales dated 20 May 1752 and read at the Royal Society on 28 May 1752. Mazéas then described Dalibard's experiments, in which the insulated rod had been erected in a sentry box in a garden at Marly-la-ville; the "pointed bar of iron" was "40 feet high." During Dalibard's absence from Marly, the apparatus was entrusted to an "ancien dragon" named Coiffier. "On the 10 of May, 20 minutes past 2 afternoon, a stormy cloud having passed over the place where the bar stood, those, that were appointed to observe it [i.e., Coiffier and the village priest, Raulet], drew near, and attracted from it sparks of fire, perceiving the same kind of commotions as in the common Electrical experiments." Word was sent to Dalibard who, on 13 May 1752, read a detailed account of the experiment, and the Franklinian principles of electricity it illustrated, to the members of the Académie Royale des Sciences in Paris.[17] On 18 May Delor repeated the experiment with similar success, using "a bar of iron 99 feet high, placed upon a cake of resin, two feet square, and 3 inches thick."[18] Before long, others had repeated the experiment, Le Monnier, Abbé Nollet, and others, in France, Mylius and Ludolf in Germany, and, eventually, Canton, Wilson, and Bevis in England.[19]

If we accept Priestley's statement that the kite was flown in June 1752, then the Marly experiment of May 1752 antedated it by one month. This is perfectly consistent with Franklin's statement in the autobiography, when, after referring to the "capital [Marly] Experiment," he mentioned "the infinite Pleasure I receiv'd in the Success of a similar one I made soon after with a Kite in Philadelphia."[20] In 1768 Franklin wrote Dalibard a note in which he admitted freely that Dalibard was "the first of Mankind, that had the Courage to attempt drawing Lightning from the Clouds to be

[15] In his autobiography Franklin wrote that "A Copy of them [my electrical papers] happening to fall into the Hands of the Count de Buffon . . . he prevail'd with M. Dalibard to translate them into French, and they were printed at Paris." From Farrand, Max, *Benjamin Franklin's memoirs, parallel text edition, comprising the texts of Franklin's original manuscript, the French translation by Louis Guillaume le Veillard, the French translation published by Buisson, and the version edited by William Temple Franklin, his grandson, edited with an introduction and explanatory notes,* 384–385 ["original MS"], Berkeley and Los Angeles, Univ. of Calif. Press, 1949.

Dalibard, in the "avertissement" to his translation, noted that Franklin "pria en même tems M. Collinson d'en envoyer un des premiers exemplaires à M. de Buffon . . . [qui] m'a engagé à les faire paroître en François." (First French edition [F1], p. 4, second French edition [F2.1], pp. 2–3; *cf.* note 14, *supra.*)

The Abbé Nollet, in *Lettres sur l'électricité* **1**: 5, Paris, Chez H. L. Guérin & L. F. Delatour, 1754 (*ed. princeps* 1752), a book devoted largely to an attack on Franklin's experiments and theories, noted that soon after the appearance of the English edition of Franklin's book (1751), "Un particulier qui reçut cet Ouvrage à Paris, le traduisit en Français pour son propre usage, dit-on, & sans avoir dessein de le faire imprimer; cela est d'autant plus vraisemblable que cette traduction est un peu négligée; dans bien des endroits on a peine à entendre l'Auteur, & l'on manqueroit plusiers de ses expériences, si l'on n'avoit pas recours à l'Original Anglois.

"Quoi qu'il en soit, cet Ouvrage traduit dans notre langue, tomba entre les mains de M. de Buffon, Intendant du Jardin du Roi. Ce savant Académicien, goûtant beaucoup la doctrine de M. Franklin, & ayant répété & vérifié, à ce que l'on prétend, avec succès toutes les expériences que cet Auteur rapporte en preuves, crut obliger sa Patrie, en faisant publier cette traduction; & comme il étoit livré à des occupations plus importantes, il en abandonna le soin à un de ses amis nommé M. Dalibard, qui y joignit de son chef une histoire abrégée de l'Electricité; & cela forme un petit volume *in*-12. . . ."

[16] *Phil. Trans. Roy. Soc.* **47**: 534–535, 1751–1752.

[17] Dalibard published this memoir in full in the second edition of his translation of Franklin's book (1756 [F2.2; *cf.* note 14, *supra*], **2**: 67–125) and the major portion of it was included in the fourth and fifth English editions of his book (1769 [E4] and 1774 [E5]); *cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 257–262.

[18] *Phil Trans. Roy. Soc.* **47**: 535, 1751–1752.

[19] Accounts of these experiments were printed in *Phil. Trans. Roy. Soc.* **47**: 536–552 [experiments of Mazéas, Le Monnier, and (p. 550) of Ludolf as reported by Euler], 557–558 [expts. of Nollet, and Le Roy], 559 [expts. of Mylius and Ludolf], 567–570 [expts. of Watson, Canton, Wilson, and Bevis]. These experiments are discussed in more detail in two articles by the writer, Prejudices against lightning rods, *Jour. Franklin Inst.* (in press), and Franklin & Diviš, *Isis* (in press). Another who repeated the sentry-box experiment in France was Romas, who claimed that he had conceived this experiment long before Franklin, just as he claimed that he had anticipated the kite experiment; see, *infra*, § 8.

[20] *Benjamin Franklin's memoirs* (ref. 15, *supra*), 386.

subjected to your experiments." [21] We must next investigate the question of whether, when Franklin flew his kite, he had already learned of the European experiments performed according to the Franklin specifications of 1750.

4. WHEN DID FRANKLIN LEARN OF THE EUROPEAN SENTRY-BOX EXPERIMENTS?

The question of the date when Franklin received information about the successful issue of the sentry-box experiments performed in Europe has been studied by Abbott Lawrence Rotch,[22] Alexander McAdie,[23] Marcus W. Jernegan,[24] and Carl Van Doren.[25] Jernegan and Van Doren accepted Priestley's date of June 1752 for the kite, so that they endorsed the conclusion that Franklin had flown his kite before he had heard about the French experiments: Jernegan and Van Doren quoted Priestley's description of the kite experiment which I have printed above.[26] Priestley included in his history another discussion of this chronology, which reads:

> Moreover, though Dr. Franklin's directions were first begun to be put in execution in France, he himself completed the demonstration of his own problem, before he heard of what had been done elsewhere: and he extended his experiments so far as actually to imitate almost all the known effects of lightning by electricity, and to perform every electrical experiment by lightning.[27]

This statement is even more explicit than the one quoted earlier. Even so, all writers have not been willing to accept Priestley's testimony. Thus Rotch believed that "the experiment was not performed until later in the summer" than June,[28] and McAdie concluded:

> 1: Franklin himself does not give a definite date when a kite was flown.
>
> 2: It seems doubtful that the kite was flown in June or early summer 1752.
>
> 3: If flown, the date was probably not far in advance of the end of September 1752.[29]

Rotch's and McAdie's conclusions would, according to Jernegan, "take from Franklin a part of the honor which he has had because of the belief that he independently made the discovery, by his own experiment, of the identity of lightning and electricity." His "kite experiment would rather be a continuation of those already performed; an attempt to extend them further." The expression, "by his own experiment," is a little misleading; at the very least, it is ambiguous, since both experiments were Franklin's. Dalibard, in his report to the French Academy of Sciences on 13 May 1752, was explicit on this point: "In following the path that M. Franklin traced for us, I have obtained a complete satisfaction." [30] Furthermore, it is difficult to know in what sense the kite experiment might have been conceived as "an attempt to extend them [i.e., those experiments already performed] further," [31] since the results

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[21] Smyth, A. H., *Writings of Benjamin Franklin* **5**: 94, N. Y., Macmillan, 1907. *Cf., infra,* Priestley's statement printed above footnote 27.

[22] Rotch, A. L., Did Benjamin Franklin fly his electrical kite before he invented his lightning rod? *Proc. Amer. Ant. Soc.* **18**: 115–123, 1907; When did Franklin invent the lightning rod? *Science* **24**: 374–376, 1906; The lightning rod coincided with Franklin's kite experiment, *Science* **24**: 780, 1906.

[23] McAdie, A., The date of Franklin's kite experiment, *Proc. Amer. Ant. Soc.* **34**: 188–205, 1925.

[24] Reference 7, *supra.*

[25] Van Doren, C., *Benjamin Franklin,* 164–170, N. Y., Viking, 1938.

[26] *Cf., supra,* the extract quoted above footnote 10.

[27] Reference 8, *supra,* **1**: 206.

[28] Rotch relied chiefly on the testimony of Romas that Franklin knew of the Marly sentry-box experiment before he flew his kite; he concluded (correctly) that the news would probably not have reached Philadelphia in June 1752. Romas was in error, as may be seen in the discussion below, § 8. *Cf.* footnote 31, *infra.*

Rotch's chief aim in the article in *Proc. Amer. Ant. Soc.,* by his own admission, was to make known Franklin's directions for installing lightning rods, as published in "Poor Richard's almanack" for 1753 which, he maintained, "seems to have escaped the notice of all Franklin's biographers." Had Rotch consulted the best biography then available [Parton (ref. 7, *supra,* **1**: 297n], he would have found this passage about lightning rods reprinted *in extenso.*

[29] Reference 23, *supra*; see, *infra,* § 7 for a further discussion of McAdie's comments.

[30] *Cf.* his translation of Franklin's book (ref. 14, *supra*) [F2.1] **2**: 99. On pp. 72–73, he wrote, "There was lacking to that ingenious physicist [Franklin] but one last proof in order to produce complete conviction that the matter of thunder is absolutely the same as that of electricity; not being apparently too ready to acquire this proof by himself, he has shown us the means of obtaining it." (Freely translated from the French.)

[31] On this point Jernegan appears to have followed Benjamin, Park, *A history of electricity (the intellectual rise in electricity) from antiquity to the days of Benjamin Franklin,* 589, N. Y., John Wiley, 1898. Benjamin assumed that Franklin had heard about the French experiment before he flew his kite; that he had believed the rod of Dalibard was not high enough [it was far from being as high as Franklin had specified] to draw electrical fluid from the clouds themselves. "That sparks had been drawn from rods which ended in the air close to the earth's surface, and not within hundreds of feet of the

it provided were identical to those of the sentry-box experiment.

After reviewing the evidence, and especially on the basis of new information which he had uncovered, Jernegan concluded:

Benjamin Franklin proposed the identity of lightning and electricity but by his own admission, a French scientist, M. D'Alibard, was the first to prove his conjectures by "drawing lightning from the clouds to be subjected to your experiments." On the other hand, the evidence presented makes it more certain that Franklin did prove the identity of lightning and electricity, *independently,* and that his "electrical kite" was flown in June, 1752, before he had heard of the French experiments of May 10 and 18. Secondly, while Franklin was the first to propose lightning rods, and to give definite directions for erecting them, the evidence indicates that French scientists, acting on his suggestions, set up "sharp-pointed iron bars" on buildings and ships before May 26, 1752, before Franklin flew his kite, and before he himself had proved by experiment that they were a "preservative against thunder."

As I shall indicate below, there is still further evidence to support Jernegan's acceptance of Franklin's statement to Priestley that the kite experiment had been performed in June 1752 and, therefore, prior to his having received intelligence of the French experiments. On the other hand, I cannot agree that there is any good evidence that lightning rods [32] had been erected in France earlier than those Franklin appears to have erected in Philadelphia in June 1752, and I shall discuss this question at length in the following section.

Jernegan laid stress on a letter written by Franklin to Cadwallader Colden on 14 September 1752, in which he observed, "I see by Cave's *Magazine* for May that they have translated my electrical papers into French and printed them in Paris," [33] since this issue of the *Gentleman's Magazine* contained a letter from France describing experiments "in pursuance of those by Mr. *Franklin* . . . to find whether the tonitruous and electrical matter be not analagous," and referring to "bars." [34] Jernegan therefore concluded that Franklin knew by 14 September that "French scientists had placed iron rods on buildings and that they were a preservative against thunder." The inference is that Franklin knew about the French experiments two weeks before writing his letter about the kite to Collinson on 1 October; this point is also stressed by Van Doren.[35] A thorough examination of the *Pennsylvania Gazette* for the summer of 1752 has revealed that this information was in Franklin's hands almost a month earlier, that is, by the third or fourth week of August, since the issue for 28 August 1752 reprinted a letter concerning European lightning experiments with insulated rods.[36]

I am willing to accept Romas's computation [37] to show that Franklin probably could not have received news of Dalibard's experiment at Marly-la-Ville in June 1752. Dalibard's report was read in Paris on 12 May and Mazéas's letter to Stephen Hales was dated 20 May and was read at the Royal Society on 28 May. Since it would take a month or more for the news to reach Philadelphia from London, Franklin very likely would not have heard about this matter until early July—possibly the last days of June—at the very earliest, had either Collinson or Dalibard written him at once.

The account which Collinson eventually sent to Franklin was based on the Abbé Mazéas's letter, and reads:

If any of the thy Friends should take Notice that thy Head is held a little higher up than formerly, let them know; the *Grand Monarch of France* strictly commands the *Abbé Mazéas* to write a Letter in the politest Terms to the Royal Society, to return the King's Thanks and Compliments in an express Manner to *Mr. Franklin* of *Pennsylvania,* for the useful Discoveries in Electricity, and Application of the

clouds was not conclusive. This was the experiment in one sense, and yet, in another, it was not. It showed that the rods had become electrified—but not necessarily that the lightning had electrified them or had passed over them." Benjamin believed that the kite experiment was designed to answer the doubt so aroused, but there is no evidence whatever to support his view. We do know that Franklin believed that pointed conductors could "draw off" electrical fluid from charged bodies at great distances and, indeed, this was the basis of his theory of the action of lightning rods. If he had overestimated the height necessary for a rod to "draw off" the supposed electrical fire from the clouds, I cannot see why this should have necessitated another experiment. We know that by September 1752, when he had certainly known about the French experiments, he tested the electrification of clouds by the use of a rod he erected on his house in Philadelphia; *cf., infra,* § 6.

[32] I.e., in the sense of grounded conductors to prevent buildings from damage by a stroke of lightning; *cf., infra,* § 6.

[33] Smyth, Albert Henry, *The writings of Benjamin Franklin* 3: 98, N. Y., Macmillan, 1907.

[34] This letter from the *Gentleman's Magazine* is reprinted, *infra,* above footnote 53.

[35] Van Doren (ref. 25, *supra*), 168.

[36] This letter is reprinted, *infra,* above footnote 55.

[37] *Cf.* footnote 28, *supra,* and the discussion of Romas in § 8, *infra.*

pointed Rods to prevent the terrible Effects of Thunder-storms, I say, after all this, is not some Allowance to be made, if thy Crest is a little elevated? . . . I think, now I have stuck a Feather in thy Cap, I may be allowed to conclude in wishing thee long to wear it.

Thine,
P. Collinson.

I have not been able to locate the original of this letter, but it plainly could not have been written more than a day or two before 28 May, the date when Mazéas's letter was read at the Royal Society, and more likely it was written after 28 May. The portion printed above was quoted (without its date) by Franklin in a letter to Jared Eliot dated 12 April 1753. This letter begins:

The *Tatler* tells us of a Girl, who was observed to grow suddenly proud, and none cou'd guess the Reason, till it came to be known she had got on a new Pair of Garters. Lest you should be puzzled to guess the Cause, when you observe any Thing of the kind in me, I think I will not hide my new Garters under my Petticoats, but take the Freedom to show them to you, in a Paragraph of our friend Collinson's Letter, viz.—But I ought to mortify, and not indulge, this Vanity; I will not transcribe the Paragraph, yet I cannot forbear.[38]

Surely, this does not read like an introduction to an extract from a letter received by Franklin eight or ten months earlier. Rather, it gives the impression that Franklin had received Collinson's letter not too long before. That Collinson's letter had been in Franklin's hands when he wrote the kite letter on 1 October seems, therefore, very unlikely, although we must keep in mind that we have no direct evidence on way or the other.

The earliest dated letter from Collinson to Franklin mentioning the French experiments was written on 7 July 1752, and begins as follows:

I had the pleasure of my Dear friend's Letter of 21 March last with a guinea Inclosed but as I have Cash on hand I returned It by Moses Bartram. The Electrical Experim[en]ts have some thing very surprising in them, as all [thy letters] have. These our Friend Cave Intends to add to thy book as a Supplem-[en]t and then the Errata may be added before they are printed. Wee Shall wait the Return of the Autumn or Spring Ships. It's likely our Friend Kinnersley may add some others, under thy Direction.

By the Publick papers thou will See how thou has Sett the French to work.[39]

The last sentence above must refer to the accounts of the French experiments that were published in the *Gentleman's Magazine* or *London Magazine*. In a letter of 27 September 1752, Collinson told Franklin briefly that "all Europe is in agitation on Verifying Electrical Experiments on points—all commend the Thought of the Inventor—more I dare not say least I offend Chalk [?] Ears."[40]

Franklin must have received some word of the Marly experiments from Dalibard himself, but I have been unable to find a copy of this communication. In the "avertissement" to the second edition of his translation of Franklin's book, Dalibard wrote:

As soon as the first edition of this translation was completed, I sent a copy of it to M. Franklin, which put me into direct correspondence with him. I made known to him in time, the success of my experiment on thunder, I sent him the memoir which I had given to the Royal Academy of Sciences on 13 May 1752, such as it is in the second volume of the present work; he had been charmed by it and sent me, with his reply, his first supplement, the experiments of which I similarly verified. The second did not reach me until a long time afterwards.[41]

The earliest surviving letter I have found from Franklin to Dalibard is dated 29 June 1755. A truncated version of this letter was published in the *Philosophical Transactions* and was printed in Franklin's book on electricity, and in the various editions of his works.[42] The full text of this letter reveals that Franklin and Dalibard were never good correspondents. It begins:

[38] Smyth (ref. 33, *supra*) **3**: 124.

Volume 47 of the *Philosophical Transactions* (for the years 1751 and 1752), containing the Abbé Mazéas's letter, the accounts of the sentry-box experiments in France and in England and Germany, and also the letter about the kite, was printed in 1753. On 20 July 1753, Collinson wrote to Franklin about the antics of the Abbé Nollet who had been conducting a campaign against Franklin and now "his base and juggling intention" had been exposed: "Now on reading this & the King of France's approbation you will see in the Transactions, if the Furror Schould Rise again what will allay It—This will lead you to understand my former Paragraph. . . ." From an unpublished letter in the Library of the American Philosophical Society.

[39] The original manuscript letter is in the Library of the American Philosophical Society. *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 124.

[40] Unpublished letter in the Library of the American Philosophical Society.

[41] P. 23; *cf.* reference 14, *supra*. Freely translated from the French.

[42] Smyth (ref. 21, *supra*) **3**: 269–273.

For a long time I have owed you a reply to your last letter, dated 20 June 1754. I received it last January while I was in Boston in New England,[43] & since that time I have been so busy with my travels to different places & with public affairs that I am extremely in arrears with my correspondents.

I sent you last year a manuscript which contains some new experiments & some observations on thunder; I do not know whether you have received it, but it has since been printed in London, & I imagine that our good friend Mr. Collinson will have sent you a copy.

I thank you for the kindness you have had in sending me the four volumes of natural history of M. de Buffon, the maps, *&c.*

You desire my opinion of *Père Beccaria's Italian* book. . . .[44]

In the fourth and fifth English editions of Franklin's book, this was printed as an "Extract of a letter concerning electricity, from Mr. B. Franklin, to Mons. D'Alibard, at Paris, inclosed in a letter to Mr. Peter Collinson, F.R.S." [45] Collinson wrote Franklin on 20 July 1753 that " your Letter is forwarded to Mons. Dalibard by a safe Conveyance as soon as I received it & a Supplement with It—one I sent before to Mr. Buffon." [46]

But even though there is no way of determining the dates of the earliest Dalibard-Franklin correspondence, we may be certain that Franklin had not learned of the Marly experiments from him by mid-September 1752, and very likely even by 1 October when he wrote the kite letter to Collinson. For, on 14 September 1752, when Franklin wrote to Colden that he saw in the May 1752 issue of the *Gentleman's Magazine* that a French translation of his book had appeared, he expressed the "hope [that] our Friend Collinson will procure and send me a Copy of the Translation." [47] We do not know exactly when Franklin received a copy, but some time in the fall of 1753 he had received Nollet's volume of letters attacking his and had sent it to Colden. On 25 October 1753, he wrote Colden: "I send you herewith Nollet's book. M. Dalibard writes me, that he is just about to publish an answer to it, which, perhaps, may save me the trouble." [48] Colden's son David read Nollet's book and sent Franklin some comments on it which he included in the second supplement to his own book; in a letter to Colden of 1 January 1754,[49] Franklin acknowledged the receipt of David's remarks, which had been enclosed in a letter from Colden "of the 3d past." In this same letter Franklin mentioned his having received from London "the Supplemental Electrical Experim[ts]" and added that "Mr. Dalibard wrote me that he was preparing an Answer" to Nollet.

The conclusion at which we arrive is that we have no way of telling when Collinson first wrote to Franklin about the success of the Marly experiment of Dalibard and those of Delor and Buffon, although it does not seem likely that he had mentioned this topic prior to his letter of 7 July 1752.

[43] Franklin sent this letter of Dalibard's to James Bowdoin (see footnote 13, *supra*) in a letter dated 13 December 1753, ". . . soon after my return from New England, I received the enclosed from Monsieur Dalibard, wherein he tells me that he is preparing an answer, not only to the Abbé, but to some others that have wrote against my doctrine, which will be published the beginning of this winter." From Smyth (ref. 32, *supra*) **3**: 192.

In Sparks (ref. 7, *supra*) **6**: 193–194, there is an English translation of a letter from Dalibard to Franklin dated 31 March 1754, beginning: "I received on the 15th of January last, your obliging letter of October 28th." This letter is given under the incorrect date of 1752 by Jernegan (ref. 7, *supra*), 182, n. 5, and Houston, Edwin J., Franklin as a man of science and an inventor, *Jour. Franklin Inst.* **161**: 286, 1906.

[44] Freely translated from Dalibard's French version in the second edition of his translation (Paris, 1756) **2**: 307–319. The remainder of the letter is as in Smyth, save that two short paragraphs are missing. The first (page 312) states that Franklin "will be very glad to learn about the experiments of M. le Roy on positive and negative electricity, when you will be able to communicate them to me." The second (page 319) is the conclusion of the letter: "I feel that the natural history of M. de Buffon will give me much pleasure & will instruct me enormously (*infiniment*). Assure him, I beg of you, of my respects as well as M. de Fontserrière, both of whom have given me tokens of their regard in your last letter. I am *&c.* B. Franklin."

[45] *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 307–310. Smyth does not indicate that this letter is merely an extract.

[46] Unpublished letter in the Library of the American Philosophical Society.

[47] Reference 33, *supra*. If Franklin had not even received word of the French translation from Dalibard by 14 September, I assume he would not have received news of the Marly experiment directly from him until some time later than mid-September 1752.

[48] Smyth (ref. 32, *supra*) **3**: 164.

[49] Smyth (ref. 32, *supra*) **3**: 105–106 (following Sparks) printed this letter under the date 1 January 1753, as it is also printed in *Cadwallader Colden papers* **5**: 358–359 (*Collection N. Y. Hist. Soc.*, 1920). In modern style the date should obviously be 1 January 1754, since Franklin did not send Nollet's book to Colden until 25 October 1753. Furthermore, the supplement to Franklin's book on electricity was not yet available in America on 1 January 1753.

Franklin wrote Colden on 14 September that he had just read that there was a French translation of his book; had he previously known from Dalibard about the Marly experiments, he would have certainly been told by Dalibard that he was the translator of his book. Hence, I believe that there is no reason whatever to doubt Franklin's statement, as reported by Priestley, that he had not known of the French experiments when he claimed to have flown his kite in June 1752.

5. THE DATE OF FRANKLIN'S FIRST LIGHTNING ROD

The version of Franklin's letter to Collinson describing the kite as printed in the *Philosophical Transactions* differs from that which appeared in his book on electricity and which has been used by all the editors of collected and selected writings of Franklin, e.g., Sparks, Bigelow, and Smyth.[50] Whereas the date given in the *Philosophical Transactions* (and confirmed in the copy of the letter reproduced here as fig. 2) is 1 October 1752, the date given in Franklin's book and used by his editors is 19 October 1752. The latter is the date of the issue of the *Pennsylvania Gazette* containing the letter. Even more important than the date is the fact, first noticed by Hellmann,[51] that the final paragraph, although printed in the *Philosophical Transactions,* was omitted from the version in Franklin's book; it is also absent from the version in the *Pennsylvania Gazette* and the various editions of his writings.[52] This paragraph states:

> I was pleased to hear of the success of my experiments in France, and that they begin to erect points upon their buildings. We had before placed them upon our Academy and state-house spires.

Jernegan assumed that Franklin was referring to a report given in a letter from Paris, written on 26 May 1752 and published in the *Gentleman's Magazine* for May 1752. This letter was printed immediately following a long extract from Franklin's book, dealing with his lightning hypothesis, which was described as: "*A new* Hypothesis *for explaining the Phenomena of* Thunder, Lightning, *and* Rain. *Being an Extract from* B. Franklin's *Experiments and Observations on Electricity. Printed for* E. Cave, *and lately translated into* French *at* Paris." Following the extract from Franklin's book, we find:

> *The above Hypothesis is in part confirmed by some Experiments lately made in* France, *as appears by the following Letter from* Paris, *dated May* 26 N. S.
>
> From several electrical experiments performed by our best naturalists, in pursuance of those by Mr. *Franklin* in *Philadelphia,* to find whether the tonitruous and electrical matter be not analogous, it appears, that to fix on the highest parts of buildings or ships sharp-pointed iron bars of ten or twelve feet, and gilt to prevent rust, with a wire hanging down on the outside to the ground, or about one of the ship's shrouds, is a preservative against thunder. . . . The Sieur *Dalibard* having placed, in a garden at *Marly,* an iron bar on an electrical [i.e., insulated] body at the height of forty feet, was informed that on the tenth of *May,* about 20 minutes after two, a tempest passing over that spot, the parish priest and other persons drew from the bar such sparks and agitations as are seen in the common electrical performances. On the 18th the Sieur *de Lor* having fixed a bar at the height of 99 feet, on a cake of rosin, two feet square and three inches thick, drew coruscations from it during half an hour betwixt four and five, whilst the cloud was over it; these scintillations were perfectly like those emitted by his gun-barrel, when the globe is rubbed only with the brush, the same fire, the same crackling; whilst the rain, mixed with a little hail, fell from the cloud without any lightening or thunder, tho' it appeared to be the progress of a

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[50] Also Goodman, Nathan, *A Benjamin Franklin reader,* 381–382, N. Y., Thomas Y. Crowell Co., 1945; Mott, Frank Luther, and Chester E. Jorgenson, *Benjamin Franklin: representative selections, with introduction, bibliography, and notes,* 223–224, N. Y., etc., American Book Company, 1936.

[51] Hellmann, G., Ueber Luftelektricität, *Neudrücke von Schriften und Kartenüber Meteorologie und Erdmagnetismus* 11, Berlin, 1898.

This issue containing facsimiles of publications by Winckler, Franklin, Dalibard, and Lemonnier. Rotch (ref. 22, *supra*) called attention to Hellmann's discovery of the discrepancy between the two versions of this letter.

[52] The various editors of Franklin's writings based their versions on previous editions, hence ultimately on the first supplement to the first English edition of Franklin's book on electricity: *Supplemental experiments and observations on electricity, part II. Made at Philadelphia in America, by Benjamin Franklin, Esq.; and communicated in several letters to P. Collinson, Esq.; of London, F. R. S.,* London, E. Cave, 1753. [This is E1.2 in the classification given in *Benjamin Franklin's experiments* (ref. 4, *supra*), 141.] The final letter of that supplement, suppressed in the fourth and fifth English editions (1769 and 1774, respectively), printed immediately following the kite letter, was addressed to Collinson and read: "As you tell me our friend *Cave* is about to add some later experiments to my pamphlet, with the *Errata,* I send a copy of a letter from Dr. *Colden,* which may help fill a few pages; also my kite experiment in the *Pensylvania Gazette*; to which I have nothing new to add, except the following experiment towards discovering more of the qualities of the electric fluid. . . ." *Cf. Benjamin Franklin's experiments,* 161, note 29.

A Cut and Deſcription of a Machine, eaſily conſtructed, for making the Experiment by which Franklin's *new Theory of Thunder is demonſtrated.*

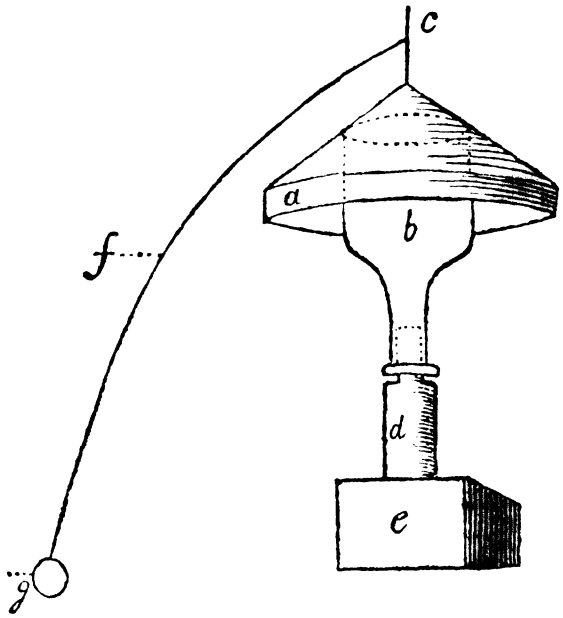

FIG. 4. Among the extensive discussions of Franklin's theory that lightning is an electrical phenomenon, and the various experiments demonstrating electrification clouds, that appeared in the *Gentleman's Magazine* for 1752, was a letter describing the simple apparatus shown here, contrived so that any one might perform the experiment for himself.

"*a*, Is a tin vessel, such as is used to cover dishes. *b* is a quart bottle or decanter. Pour a sufficient quantity of cement into the tin cover, and then press the bottom of the bottle upon it, that when the cement is cold they may adhere firmly together. On the top of the tin cover let there be fixed the spike *c* of any metal, and about 18 inches in length, terminating in a sharp point: then having stuck up the short stick *d*, in the block *e*; put the neck of the bottle over the other end, first cutting it with a shoulder, to prevent its going in too far. Place this machine on the top of the house in the open air, and having fastened the wire *f* to the spike *c*, bring it down to the window of the room in which the effect is to be observed, and fasten to the end of it the ball *g*, which may be of any metal, and need not be more than a quarter of an inch in diameter. When a thick cloud passes over the place, the spike will become electrified, and the electricity being prevented from running off by the glass bottle, will descend along the wire, and the ball being touched will emit the electrical sparks. A person standing on wax, if in contact with the wire, will become electrified, and may kindle spirits with his finger. In like manner on Mr *Franklin's* principles, if a sharp spike be placed on the top of a mast with a wire conductor fasten'd from it to the shrouds, it will prevent the mast from splitting, and the ship from damage by lightning. *Yours &c.*

"*N. B. The wire must not come in contact with any substance that is a conductor of electricity.*" [*Gentleman's Mag.* **22**: 327, 1752.]

In the "historical chronicle" of the same magazine for August 1752, there appeared a short account of the experiments in England made by Canton and Wilson "in consequence of the experiments lately made in *France* pursuant to Mr. *Franklin's* doctrine of thunder and lightning, and his suggestion of drawing off gradually the electrical fire from clouds by pointed bars." The account ended: "On this occasion we recommend to all gentlemen who take the laudable pains of keeping meteorological journals, that they would be diligent in watching what sort of clouds do chiefly affect the machine, and send their observations to be communicated to the public. It is easy to annex two little bells to the wire of the machine, with a clapper between, which without further trouble will give notice when it is electrified. Mr. *Watkins*, at Sir *Isaac Newton's Head, Charing Cross*, keeps them to sell so prepared." [*Ibid.* **22**: 383, 1752.]

tempest which had happened elsewhere. Both these experiments have been reported to the Royal Academy of Sciences, and both evince that thunder clouds may be deprived of their fire, by iron bars fashioned and fixed as above.[53]

We know that Franklin had read this account before writing to Collinson on 1 October since, as we have already seen, he referred to this issue of the *Gentleman's Magazine* in a letter to Colden dated 14 September 1752. We may note that the anonymous author of this Paris letter states that the experiments had been devised to discover whether "the tonitruous and electrical matter be not analagous," and that the conclusion warranted by the demonstration of the "analogy" was that sharply pointed iron bars—affixed to the highest parts of buildings and ships and grounded—are a "preservative against thunder."

I have found evidence that Franklin knew about such experiments even earlier than 14 September 1752. The *London Magazine*, during this period, was reporting the news about electricity just as the *Gentleman's Magazine*. The issue of the *London Magazine* for May 1752 had an almost identical letter from Paris, describing the work of Dalibard and Delor, which Franklin had before him when writing to Collinson on 14 September. The *London Magazine* for June 1752 carried a supplementary note on the French lightning experiments which reads as follows:

[53] *Gentleman's Mag.* **22**: 229, May 1752. *Cf. supra*, note 34.

To what was said of Lightning and Electricity in our last, . . . we shall add the following, which is also from Paris, June 12.

Tho' many very able and experienced naturalists have many years ago asserted, that lightning and the power of electricity were one and the same thing; which notion was grounded on the resemblance there was between their respective phenomena; yet resting satisfied with the conjecture only, they never pointed out any ways or means for the demonstration of the fact. Mr. Francklyn, however, of Philadelphia in America, carried this critical point much further, and has pointed out the means for making the experiment; in which particular point he has succeeded beyond expectation. Mr. Lemonier,[54] in particular, one of his most Christian majesty's physicians in ordinary, who is a member of the Academy Royal of Sciences, made the experiment accordingly at St. Germain en Laye, during the tempest which happened on the 7th instant; and planted in the garden of the Hotel de Noailles, an iron rod for that purpose. He plainly perceived, that at the first flash of lightning that fell on it, the rod was electrified in the same manner, and had visibly the same appearance, as it would have had in case it had been electrified according to art. Abundance of persons of indisputable credit were eye-witnesses of the effects it produced; from whence it is now demonstrable, that the effects of lightening and electricity are the same.

The July 1752 issue contained three communications on lightning as follows:

Further Remarks and Experiments in relation to Lightning and Electricity. . . .

Paris, June 30. Upon the steeple of the church of Plauzat, in Auvergne, is a cross of iron, not painted or gilt. The extremities of this cross form sorts of fleurs-de-lis with sharp points. Whenever there happens any great storm, accompanied with thick clouds and flashes of lightening, a luminous body is perceived upon every one of the extremities of this cross. According to an immemorial tradition, there very rarely happens to be any thunder at Plauzat, or in the neighbourhood, when this phenomenon appears. As soon as it is seen, people are certain that the storm is no more to be feared. The luminous bodies are of different colours like the rainbow, and the figure is conical. Sometimes they continue an hour and an half, if it rains ever so plentifully.

Brussels, July 3. The Sieur Torre having caused a pointed iron rod to be erected upon the top of his house, on the 23d ult. at night, tho' there was but a slight appearance of a storm, shining sparks were drawn from that rod; on the 26th at night a dark cloud covered the sky, and a heavy rain, mixed with hail, fell when people were surprized to feel and to see, that a finger held at the distance of two inches from the rod, excited very strong sparks. These phaenomena greatly increased upon a clap of thunder being heard.

Paris, July 7. M. le Noine,[54] the king's physician, has made a new experiment in electricity, at St. Germaine en Laye, which confirms the analogy of the effect it has to that of thunder; the weather being very cloudy, he caused a cake of rosin to be brought to the place, upon which he mounted, and without any other instrument he extended his hand above his head, as a thicker cloud than ordinary passed over him, and one of those who were with him having touched him to make him remark something, he instantly received a most violent shock, of which fact he has made report to the Royal Academy of Sciences.

The same issue contained, in the section entitled "Foreign Affairs," further information on Dalibard's experiments:

M. Dalibard, who frequently exhibits electrical experiments, got a bar of iron, or rather several joined together, to the length of 50 or 60 feet, erected at a village 7 or 8 miles from Paris, on the road to Compeigne: it was suspended by silken cords, and rested on glass bottles; so that supposing it could be electrified, it would not part with its virtue. One day a cloud passed over and discharged a clap of thunder, at which time M. Dalibard could draw sparks of fire from the bar, even at the distance of several inches. The flashes and sparks produced the pricking sensations as those from the conductor in the usual experiments. The diverging lucid stream was seen to issue from the pointed end of the bar; and every thing concurred to prove indisputably, that the bar was strongly electrified by the cloud. A gentleman who assisted at the experiment, upon slightly touching the rod unawares, received a violent stroke on his arm, and his clothes smelt all over of sulphur. The whole academy was entirely satisfied with the account, which clearly proved, that the matter of thunder and electricity is one and the same thing; and that it was practicable to extract thunder from a cloud, and direct it which way we please.

Franklin knew of these reports in the *London Magazine.* In the issue of the *Pennsylvania Gazette* for 27 August 1752, he published an "Extract of a Letter from Paris," taken from the May issue of the *London Magazine,* of which the text is substantially the same as that in the May issue

[54] Lemonnier or Le Monnier made a series of experiments, some in concert with Mazéas, with insulated rods and found that the experiment worked equally well if the rod was horizontal rather than vertical. His most significant results dealt with the electrification of the rod in the absence of thunder-clouds, a topic brought to a high state of investigation by the Abbé Beccaria (for whom see, *infra,* § 8). For some account of Lemonnier's research, see, *supra,* references 19, 51.

of the *Gentleman's Magazine* which I have reprinted above. On 28 September, three days before he wrote to Collinson about the lightning kite, he published another account of lightning experiments in the *Pennsylvania Gazette,* this one being similar in content to the note printed above (from the July issue of the *London Magazine*). The text printed by Franklin is even closer to (though not a word-for-word reprint of) a note in the "historical chronicle" of the *Gentleman's Magazine* for July 1752. Franklin's note reads:

Brussels, July 3. The Sieur Torre having caused a pointed Iron Rod to be erected upon the Top of his House, with Design, in some Measure, to dissipate the Fire which is in the Air, during the Time of a Storm, has succeeded therein beyond his Expectation; On the 23d of last Month, at Night, though there was but a slight Appearance of a Storm, shining Sparks were drawn from that Rod, but weaker than those drawn from an electrify'd Bar; on the 26th at Night a dark Cloud covered the Sky, and a heavy Rain, mix'd with Hail, fell, when People were surprised to feel and to see, that a Finger held at the Distance of two Inches from the Rod, excited very strong Sparks. These Phaenomena greatly increased upon a Clap of Thunder being heard; insomuch that the Sparks grew considerably longer, larger and brighter. There is Reason to believe by this Experiment, that the pointed Rod or Bar may be of great Use in diminishing the Quantity of Fire from whence Thunder is formed, and in preventing the fatal Effects of Lightning.[55]

Hence, when Franklin wrote to Collinson on 1 October 1752 about how glad he was to learn that the "French" were beginning to erect "points" on their buildings, he could hardly have helped having in mind the item from Brussels that he had just published in the *Gazette* five days earlier. This note states that Monsieur Torre's rod had been erected early enough to enable him to perform lightning experiments on 23 June 1752.

Even more relevant is a note that Franklin later printed in the *Pennsylvania Gazette* (9 November 1752), which reads:

Paris, August 5. Several Persons of Quality, have ordered Iron Rods to be fixed on the Tops of their Houses, to preserve them from the Thunder.

As we shall see in the next section, it is not entirely clear that Torre's rod was designed *primarily* for protective purposes, whereas those referred to in the above note dated 5 August 1752 were to be erected solely for the purpose of preserving houses "from the Thunder." I cannot help feeling that this news item was in Franklin's hands when he wrote the letter about the kite to Collinson on 1 October 1752. Franklin wrote about his pleasure in hearing of the success of his experiments in *France*—and I presume that he knew that Brussels was not in France—"and that they there begin to erect points on their buildings," a plain reference to more than one lightning rod erected on a building in France. Two months would have been sufficient time for news to have reached Philadelphia from Europe. Hence, I believe that the concluding sentence of Franklin's letter about the kite may legitimately be interpreted to mean that he had erected rods on two public buildings in Philadelphia earlier than the beginning of August 1752, i.e., in June or July 1752.

This conclusion is consistent with the report by Priestley that the kite experiment was performed in June 1752; since I assume that the lightning rods would not have been erected prior to experimental verification of the electrical nature of lightning, and there seems no indication of any other verification as early as June 1752 save the kite experiment.

Franklin's earliest suggestion of a lightning rod was included in a communication entitled "Opinions and conjectures, concerning . . . electrical matter, arising from experiments and observations, made at Philadelphia, 1749," and enclosed in a letter written by Franklin to Collinson dated 29 July 1750.[56] This is the same communication in which the sentry-box experiment was proposed. After describing a series of experiments in which charged insulated conductors had been discharged by a near-by pointed conductor, he wrote:

. . . may not the knowledge of this power of points be of use to mankind, in preserving houses, churches, ships, &c. from the stroke of lightning, by directing us to fix on the highest parts of those edifices, upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of those rods a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?[57]

To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment. . . .

[55] *Cf. supra,* note 36.

[56] Reference 12, *supra.*

[57] For further information on Franklin's early descriptions of lightning rods, see § 6, *infra.*

Then followed the proposal to erect a sentry box on the top of a high tower or steeple.

It should be noted that the only question in Franklin's mind that demanded experimental proof was whether "the clouds that contain lightning are electrified." Once proofs were at hand that such is the case, then there would be no doubt whatever in his mind that lightning rods would work and would deprive the clouds of their electrical fire before they could discharge it in a bolt of lightning. Sufficient laboratory data had been accumulated to support his view; if the clouds were electrified and if, then, the lightning discharge was merely a bigger spark discharge than that obtained in the laboratory (not in any way different in kind), there was certainly no reason to suppose that the change in scale would affect the action of pointed conductors in discharging charged bodies—whether small metal laboratory objects or gigantic clouds.

To do the protective job assigned to them, lightning rods had merely to be fixed on "the highest parts" of "houses, churches, ships, &c." By contrast, the test rod in the sentry-box experiment was thought by Franklin to require "some high tower or steeple." In all likelihood, the greater elevation envisioned for the test rod was to ensure a large effect, since Franklin knew that a pointed conductor will "draw off" the charge from a charged insulated conductor with greater "ease" at near distances than from afar, and he obviously would have wanted the results of the sentry-box experiment to be on a sufficiently large scale to be convincing. According to Priestley, Franklin "was waiting for the erection of a spire in Philadelphia to carry his views into execution; not imagining that a pointed rod, of a moderate height, could answer the purpose." It has been assumed that the spire in question was that on Christ Church.

If we accept Priestley's word that Franklin flew his kite in June, and that this experiment successfully indicated the electrification of thunderclouds, then Franklin had no further reason to delay the introduction of lightning rods to protect buildings in Philadelphia.

6. WHAT KIND OF LIGHTNING ROD DID FRANKLIN ERECT IN PHILADELPHIA IN 1752?

Many points of confusion exist with regard to early lightning rods. First, the distinction between grounded and ungrounded rods is not always made clear. Second, the action of the rods to prevent a stroke is not always kept distinct from their action in successfully conducting a stroke into the ground. Third, a considerable ambiguity exists about how proof may be obtained that the lightning rod is a "preservative against thunder."

In Franklin's communication of 1750, embodying the sentry-box experiment,[56] he described two types of lightning rod. One was grounded and its purpose was to "draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief." We may note that such *protective* rods, as recommended by Franklin, were always to be grounded. The second type of lightning rod, to be used in the sentry-box experiment, was ungrounded or insulated. The reason why this *test,* or *experimental,* rod was not grounded is, of course, that its function was to become charged when a cloud passed overhead. In terms of Franklin's theory, a grounded rod would draw off the electrical fluid from clouds and transmit this fluid into the ground, an indefinite reservoir, until the charge on the cloud (its excess electrical fluid) was all removed; in a very short time, the cloud would be discharged and an experimental test would be difficult. Actually, as we shall see below, a grounded rod will become charged by induction when an electrified cloud is overhead, and such charge will be bound there so long as the cloud is above the rod; hence *we* know that the experiment can succeed as well with a grounded rod as an insulated one. Franklin's idea was to use an insulated rod, which would draw off some but not all of the electrical fluid of the cloud, which would remain on the rod somewhat longer since it would not be immediately dissipated into the ground, but only by the slower point discharge whereby electrified bodies "throw off" their excess electrical fluid if they are pointed.

Franklin was aware of possible hazard to a man standing near an ungrounded rod during a storm, as in the proposed sentry-box experiment, even though he stood on an insulating stand. He suggested, therefore, that the man might draw sparks from the charged insulated rod by means of a grounded wire with an insulating wax handle, so that "the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him." Even so, he declared his faith that, as to possible danger, "I think there would be none," and he did not take similar protective steps in performing the kite experiment. Richmann's death a few years later, while performing a variation of Franklin's

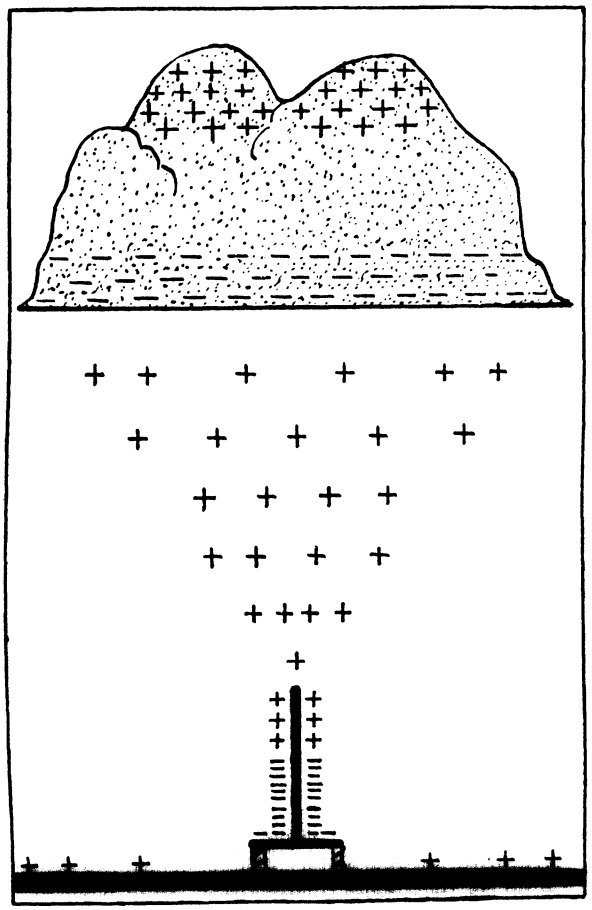

FIG. 5. Effect of an electrified cloud on an insulated rod. Reproduced, by permission, from Schonland, B. F. J., *The flight of thunderbolts*, 18, Oxford, Clarendon Press, 1950.

sentry-box experiment, indicated that the danger in such experiments was greater than Franklin had envisaged; although we may note that when Richmann was electrocuted during a lightning storm, he was standing on the floor and not on an "electrical" (or insulated) stand.

The lightning experiments performed in France, Belgium, Germany, and England in 1752 were made with insulated rods or test rods, which had the function (by design) of indicating the electrification of overhead clouds, not of affording protection. As I pointed out earlier, Franklin was certain that a grounded pointed rod would afford protection from the lightning—if, that is, the thunder-clouds were to prove to be electrically charged. Just as an insulated pointed conductor would draw off *some* of the electrical fluid from charged clouds, so a grounded pointed conductor would draw off *all* the electrical fluid from such clouds and render them harmless, i.e., incapable of occasioning an electrical discharge, or lightning stroke. We have seen this idea expressed in his communication of 1750 (describing the sentry-box experiment). It also was stated in another letter of the same year (probably a little earlier), in which Franklin suggested that the end of the rod might, for greater effectiveness, be "divided into a number of points": ". . . the electrical fire would, I think, be drawn out of a cloud silently, before it could come near enough to strike."[58] In "Poor Richard's Almanack" for 1753, announced in the *Pennsylvania Gazette* for 19 October 1752, Franklin merely indicated that the lightning would be attracted by the grounded rod and so preserve houses and ships from damage.[59]

The process whereby an insulated iron rod becomes charged is, according to our present state of knowledge, somewhat different from what Franklin envisioned. He assumed that if a cloud, say positively charged or with an excess of electrical fluid, passed directly overhead, then the insulated rod would draw off some of the electrical fluid from the cloud and become itself positively charged. We hold that the rod, in the presence of an overhead positively charged cloud, will exhibit positive electrification at the lower end and negative electrification at its upper end in a process known as "electrostatic induction"; incidentally, the first clear notions of electrostatic induction are due to Franklin, even though he did not apply them to this case.[60] If the electric potential is sufficiently great, some of the charge on the upper end of the rod will leak off, forming a glow discharge (which Franklin likened correctly to St. Elmo's fire or the "sailors' *corpusantes*"), and will move upward, being carried by ions (charged air molecules), and neutralize some of the charge on the bottom of the

[58] This letter was published in the *Gentleman's Magazine* **20**: 208, May 1750. *Cf.* Jernegan (ref. 7, *supra*), 189, note 26.

[59] The description of the lightning rod in Poor Richard for 1753 has been often reprinted. We may note that it advocated grounded, pointed, metallic conductors for buildings and ships: "A House thus furnished will not be damaged by Lightning, it being attracted by the Points, and passing thro the Metal into the Ground [or water, in the case of ships] without hurting any thing." The complete issue of Poor Richard for 1753 is reproduced in facsimile in Mott and Jorgenson (ref. 50, *supra*), 225-260.

[60] Franklin first applied the concept of electrostatic induction to the charging of a Leyden jar. This point is discussed at length in the writer's monograph (see reference 1, *supra*).

cloud. In discussing this phenomenon, B. F. J. Schonland, one of the foremost investigators of the lightning discharge in our time, notes: "The process goes on until the rod has acquired a considerable excess of charge *of the same sign as that on the base of the cloud.* Franklin's experiment does not draw electricity from the cloud but has the same effect as if it had done so." [61] Hence, we see how a positively charged cloud (or, a cloud whose lower portion is positively charged) causes an insulated rod to discharge negative electricity from its upper end, so as to be left positively charged and appear as if it had drawn off some electrical fluid from the positively charged cloud. In the same way, if a negatively charged cloud (or, a cloud whose lower portion is negatively charged) passes over the rod, the upper end of the rod becomes positively charged, the ions streaming upwards are positively charged, so that the rod is left with a residual negative charge, just as if the cloud had drawn electrical fluid from the rod. Hence the sign of the charge on an insulated rod provides (as Franklin believed) a reliable index to the sign of the charge on clouds passing overhead, or at least the sign of the charge on the lower part of the cloud. This process is indicated in figure 5.[62]

In the case of the rod being grounded, we have the situation indicated in figure 6. The "repelled negative 'induced' charge is no longer on the rod (having been repelled to a great distance) and the rod will discharge positive electricity so long as the cloud is near enough."[63] But, we must ask, does this phenomenon occur on a sufficiently large scale for the rod to be able to disarm the clouds of their charge and prevent a stroke? Schonland remarks on this score:

As stated originally by Franklin, it [i.e., the lightning rod] depended for its success or failure upon the degree to which the upward discharge of electricity between pointed rod and cloud could render harmless the charge on the cloud. The lightning-rod, if it was to work in this manner, could only do so if the point discharge from it did actually neutralize the charge on the cloud to an appreciable extent. In the laboratory experiments which Franklin had made with his electrical machine, earth-connected metal points certainly neutralized electrified bodies placed near to them. But it would seem unlikely that the same thing could happen on the much larger scale of Nature when a puny point, a few tens of feet high, faced a thunder-cloud a mile or more above it.

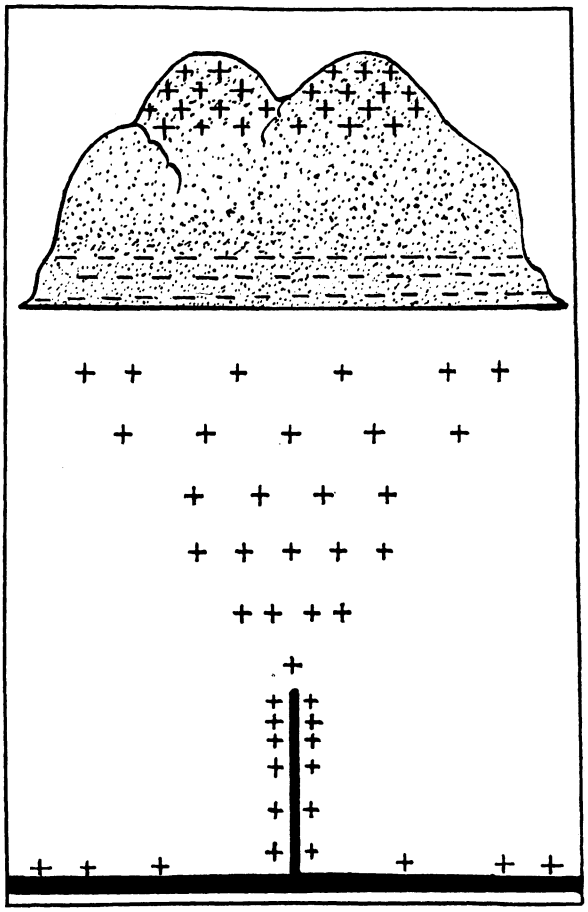

FIG. 6. Effect of an electrified cloud on a grounded or non-insulated rod. Reproduced, by permission, from Schonland, B. F. J., *The flight of thunderbolts,* 19, Oxford, Clarendon Press, 1950.

In actual fact it does not happen. A single point, or for that matter a multitude of points such as the tops of trees in a forest or the poles and chimneys of a town, has little effect upon the charge on the thunderstorm above it. None the less the lightning-rod, as is abundantly proven, has a very real virtue, because it "attracts" lightning to it and can lead a flash to ground without damage to the building to which it is attached.[64]

Franklin was quick to learn that a lightning rod can "also" protect a ship or a building by attracting the lightning and safely conducting the charge into the ground. Many of those who objected to the lightning rods did not appreciate

[61] Schonland, B. F. J., *The flight of thunderbolts,* 17, Oxford, Clarendon Press, 1950.

[62] The previous paragraph and the following one are based on the lucid discussion given by Schonland.

[63] Schonland (ref. 61, *supra*), 17.

[64] *Ibid.,* 23.

that Franklin advocated these two modes of action of rods: (1) to prevent a stroke, (2) to conduct a stroke into the ground.[65] On 29 June 1755 he wrote to Dalibard that he had been "but partly understood in that matter":

> I have mentioned it in several of my letters, and except once, always in the *alternative, viz.* that pointed rods erected on buildings, and communicating with the moist earth, would either *prevent* a stroke, *or,* if not prevented, would *conduct* it, so as that the building should suffer no damage. Yet whenever my opinion is examined in *Europe,* nothing is considered but the probability of those rods *preventing* a stroke of explosion, which is only a *part* of the use I proposed for them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage.[66]

We must now ask what kind of lightning rod Franklin had reference to when he wrote to Collinson on 1 October 1752 that "points" had been erected on buildings in Philadelphia in June 1752. Were they ungrounded test rods as devised for the sentry-box experiment or were they grounded rods for protective purposes? In his letter to Collinson, Franklin wrote: "I was pleased to hear of the success of my experiments in France, *and* that they begin to erect points upon their buildings." I have italicized the word *and,* since it emphasizes the sense of Franklin's letter as I read it, which seems to indicate that Franklin was pleased to learn that points were being erected upon buildings in addition to his pleasure at the success of his experiments. The points referred to by Franklin as having been earlier erected in Philadelphia were, I believe, designed to protect the Academy[67] and the State House from lightning. Franklin, I am sure, would not have exposed these two public buildings to any possibility of danger and he knew by the summer of 1752 that an ungrounded rod entailed some hazard; at the least, it did not provide the best possible protection. In his communication of 1750 to Collinson, and again in Poor Richard in the autumn of 1752,[68] he insisted in the plainest terms possible that protective lightning rods be grounded.

Although Jernegan[69] believed that the reference to "points" on the Academy and State House implied that they were not for protection, but were rather test rods, he did not raise the question of whether they were grounded or insulated. He argued that the appearance of the sentence about "points" erected in Philadelphia in a letter "concerned with the identification of lightning and electricity" implies that the "natural interpretation of these words would be that he placed 'points' on the 'Academy and state-house spires' for the purpose of experiment, and not for protection." But we have no indication of any experiment ever performed with these "points." Nor did Franklin ever imply elsewhere that he had used an insulated rod for lightning experiments before hearing of such experiments from Europe. Priestley insisted that Franklin had flown the kite before receiving the news of the European experiments, but neither he nor Franklin ever stated that he had performed the experiment with the insulated rod. Jernegan cited Priestley's statement:

> The Doctor, after having published his method of verifying his hypothesis concerning the sameness of electricity with the matter lightning was waiting for the erection of a spire in Philadelphia to carry his views into execution, not imagining that a pointed rod of a moderate height could answer the same purpose; when it occurred to him that by means of a common kite he could have readier and better access to the regions of thunder than by any spire whatever.

Jernegan concluded: "This would indicate that in June, the date given for the kite experiment, that 'points' had not been erected on high buildings at that date even for experimental purposes." But, as Franklin was to learn from Europe, the experiment does not require that rods be placed as high as he had thought. If he had actually erected experimental rods, he would have performed experiments on them and he never claimed to have done so. Furthermore, he would not then have needed to erect yet another experimental rod, as he did on his house in September. Finally, I cannot see why he would have erected two experimental rods—one would have been ample. I would, therefore, agree that no experimental rods had been erected in Philadelphia in June 1752, but conclude that those on the Academy and State House must, therefore, have been protective or grounded rods.

[65] *Cf.* the writer's article, Prejudices against the introduction of lightning rods, *Jour. Franklin Inst.* (in press).

[66] *Benjamin Franklin's experiments* (ref. 4, *supra*), 307–308.

[67] The fore-runner of the present University of Pennsylvania.

[68] Although the issue of Poor Richard containing the account of the lightning rod was "1753," the text of it must have been prepared earlier than 19 October 1752, since the *Gazette* for the latter date carried an advertisement that "Poor Richard's Almanack" was "on the press."

[69] Reference 7, *supra.*

FIG. 7. Portrait of Benjamin Franklin showing the lightning rod he erected in his house in September 1752. This portrait was painted by Mason Chamberlin in 1762 when Franklin was fifty-six years of age and is reproduced from an engraving made after the portrait by Freeman, and, according to the legend, was "Sold by M. Chamberlin in Stewart Street, Old Artillery Ground, Spittalfields." The price was five shillings. It shows the two bells with which the portions of the lightning rod terminated, the little ball [or clapper] suspended by a thread between the bells, and the two balls suspended by strings from the bell on the right, forming an electroscope, and indicating the presence of an electrified cloud above.

In September 1752, Franklin erected a lightning rod on his own house in Philadelphia. It took an interesting form and it provided the means for making an important discovery. This rod was contrived "to draw the lightning down into my house, in order to make some experiments on it, with two bells to give notice when the rod should be electrify'd: A contrivance obvious to every electrician," as Franklin wrote to Collinson in September 1753.[70] This instrument[71] was described by Franklin in greater detail in his paper "Experiments, observations, and facts, tending to support the opinion of the utility of long, pointed rods, for securing buildings from damage by strokes of lightning, read at the committee appointed to consider the erecting of conductors to secure the magazines at Purfleet, August 27th, 1772" as follows:

In Philadelphia I had such a rod fixed to the top of my chimney, and extending about nine feet above it. From the foot of this rod, a wire (the thickness of a goosequill) came through a covered glass tube in the roof, and down through the well of the staircase; the lower end connected with the iron spear of a pump. On the staircase opposite to my chamber door, the wire was divided; the ends separated about six inches, a little bell on each end; and between the bells a little brass ball, suspended by a silk thread, to play between and strike the bells when clouds passed with electricity in them.[72] [See fig. 7.]

Franklin's description of this rod as a device "to draw the lightening down into my house, in order to make some experiments on it" has, I believe, misled the modern historian. Jernegan observed, "It is clear, however, that this was not a rod set up for protection but for experimental purposes." Yet Franklin's description of the action of the rod indicates that, in addition to its use in experiment, it could provide adequate protection. Franklin related:

After having frequently drawn sparks and charged bottles from the bell of the upper wire, I was one night awaked by loud cracks on the staircase. Starting up and opening the door, I perceived that the brass ball, instead of vibrating as usual between the bells, was repelled and kept at a distance from both; while the fire passed, sometimes in very large, quick cracks from bell to bell, and sometimes in a continued, dense, white stream, seemingly as large as my finger, whereby the whole staircase was inlightened as with sunshine, so that one might see to pick up a pin.[73]

Since the separation of the bells was less than six inches, any quantity of lightning sufficient to do damage to an unprotected house could arc across this air gap and so be carried from the upper member of the rod to the lower member and be successfully conducted into the ground without causing any damage. Franklin knew that his rod would afford protection to his house on the basis of his investigations of the path followed by lightning when buildings were struck.[74] Thus, in his letter to Collinson describing the lightning rod with the bells, he wrote:

[70] Smyth (ref. 33, *supra*) **3**: 149. The discovery made by Franklin was that clouds appear to be charged negatively more often than positively and that, therefore, "for most part, in thunder-strokes, *'tis the earth that strikes into the clouds, and not the clouds that strike into the earth.*"

[71] A similar device was described by Dalibard in the second French edition of his translation of Franklin's book on electricity, **1**: lxxviii (histoire abregée) and **2**: 130–132, as having been invented by him a few days after he had made public his account of the Marly experiment (12 May 1752) and placed into operation on the rod which he had erected for Buffon at the *jardin du Roi*. The pointed insulated rod ended in a bell, like Franklin's, but the second bell was not attached to a grounded rod but was merely attached "à la muraille."

[72] Smyth (ref. 33, *supra*) **5**: 421.

[73] *Idem.*

[74] In 1758, while Franklin was in London, he wrote a letter to his wife about the bells: "If the ringing of the Bells frightens you, tie a Piece of Wire from one Bell to the other, and that will conduct the lightning without ringing or snapping, but silently," Smyth (ref. 33, *supra*) 3: 441. This letter plainly indicates that Franklin knew that the lightning was conducted through his rod system into the ground by a series of spark discharges as well as the "convection" set up by the clapper, a device similar to that introduced by Franklin to discharge the two coatings of a Leyden jar and to prove that the charges on the two coatings were equal in absolute magnitude though opposite in sign.

The subsequent history of this rod system is described in Montgomery, Thomas Harrison, *A history of the University of Pennsylvania from its foundation to A. D. 1770*, 75, note 18, Phila., George W. Jacobs Co., 1900: "These earlier experiments of Franklin were carried on in the house built by John Wister, No. 141 (now 325) Market street in 1731. 'It was in this house that Dr. Franklin . . . erected his first [?] lightning rod, an hexagonal iron rod, still in our possession, connecting it with a bell which gave the alarm whenever the atmosphere was surcharged with electric fluid. The ringing of the bell so annoyed my grandmother that it was removed at her request.' *Memoir of Charles J. Wister*, by his son, 1866, vol. i, pp. 21, 23. John Wister's son, Daniel, who was born 4 February, 1738–39, was a pupil at the Academy 1752-1754, as was also his cousin Caspar in 1752."

It would be interesting to know whether the family papers of the Wisters, or of other Academy pupils in 1752, contain any references to "points" or to the kite experiment.

In every stroke of lightning, I am of opinion that the stream of the electric fluid, moving to restore the equilibrium between the cloud and the earth, does always previously find its passage, and mark out, as I may say, its own course, taking in its way all the conductors it can find, such as metals, damp walls, moist wood, &c. and will go considerably out a direct course, for the sake of the assistance of good conductors; and that, in this course, it is actually moving, though silently and imperceptibly, before the explosion, in and among the conductors; which explosion happens only when the the conductors cannot discharge it as fast as they receive it, by reason of their being incomplete, dis-united, too small, or not of the best materials for conducting. Metalline rods, therefore, of sufficient thickness, and extending from the highest part of an edifice to the ground, being of the best materials and complete conductors, will, I think, secure the building from damage, either by restoring the equilibrium so fast as to prevent a stroke, or by conducting it in the substance of the rod as far as the rod goes, so that there shall be no explosion but what is above its point, between that and the clouds.[70]

One of the best known instances of Franklin's tracing the path of lightning in order to show how the lightning will depart from a simple path "to pass as far as it can in metal" occurred when the Newbury (Mass.) church was struck and was described in a letter Franklin wrote to Dalibard on 29 June 1755.[66] But Franklin was aware of this phenomenon at least as early as August 1752, since he published in the *Pennsylvania Gazette* for 6 August the following account:

Last Friday, early in the Morning, the Lightning struck two Houses on Society Hill, and did them considerable Damage, but hurt no Person. It was very remarkable in both Houses, that the Lightning in its Passage from the Roof to the Ground, seem'd to go considerably out of a direct Course, for the sake of passing thro' Metal; such as Hinges, Sash Weights, Iron Rods, the Pendulum of a Clock, *&c.* and that where it had sufficient Metal to conduct it nothing was damag'd; but where it passed thro' Plaistering or Wood work, it rent and split them surprizingly.[75]

I believe that the lightning rods erected in Philadelphia in 1752 were the first grounded lightning rods to be erected anywhere in the world for the purpose of protecting buildings from the lightning discharge. Dalibard's rod at Marly-la-Ville was erected in the middle of a garden and was insulated. Delor's rod was erected on his house in Paris, but it too was insulated, as was the apparatus used by LeMonnier, Mazéas, LeRoy, Cassini de Thury, and the Abbé Nollet. In England, Canton erected an insulated instrument built for the occasion of a tin tube with needles attached to the top and Wilson performed the experiment with an iron curtain rod projecting out of the window. Mylius and Ludolf in Berlin likewise used insulated rods.

The letters in the *Gentleman's Magazine* and the *London Magazine* often referred to the fact that experiments with insulated rods had proved the efficacy of lightning rods, but such statements do not imply that the insulated test rods were afterwards grounded in order to become effective protective rods, nor that rods for protection had been erected.[76] After quoting the letter from the *Gentleman's Magazine* for May 1752, *viz.*

From several electrical experiments performed by our best naturalists, in pursuance of those by Mr. *Franklin* in *Philadelphia,* to find whether the tonitruous and electrical matter be not analogous, it appears, that to fix on the highest parts of buildings or ships sharp-pointed iron bars of ten or twelve feet, and gilt to prevent rust, with a wire hanging down on the outside to the ground, or about one of the ship's shrouds, is a preservative against thunder.

Jernegan concluded that Franklin, who had read this letter, "knew by September 14, 1752 that there was a report that French scientists had placed iron rods on buildings and that they were 'a preservative against thunder.'" But the "report" does not warrant this conclusion. Plainly, it seems to me, the report implies that since experiments (with ungrounded conductors) have proved that lightning clouds are electrified, or that the "tonitruous and electrical matter" are "analagous," then (as Franklin has pointed out, since electrified clouds must follow the same laws as electrified bodies in the

[75] I believe that this important item, not hitherto cited in discussions of early lightning rods, confirms my earlier statement that Franklin would not have erected a wholly ungrounded lightning rod on a public building in Philadelphia in the summer of 1752. The implication is plain that lightning, attracted by the rod, would follow the metal as far as possible, but would then have to complete its path by travelling through plaster or wood work which it would rend and split. The rod erected by Franklin on his house in September 1752 avoided this danger since the choice of a path through metal meant that it had only to cross a small air gap and would not thereby hurt the house in any way. Hence, if Franklin had erected lightning rods in Philadelphia in the early summer of 1752, as I believe he did, they must have been grounded.

[76] A close reading of the reports in the *Phil. Trans.* (see ref. 19, *supra*) and those in the *Gentleman's Magazine* and *London Magazine* (quoted in § 5, *supra*) indicate that in every case the argument reads that (1) since lightning is electrical, (2) rods of some sort [grounding is not specified] can protect buildings or ships.

laboratory) "it appears" that a pointed grounded conductor "is a preservative against thunder."

An understandable confusion arises, of course, from the very nature of the experiments with insulated rods. They appeared to draw some "tonitrous matter" from the clouds and, thereby, become electrified. But, in drawing off "tonitruous matter," did they not lessen the striking power of the cloud overhead and so offer some protection? In this light, we can understand the beginning of the report from Brussels, which Franklin published in the *Pennsylvania Gazette,* in which Monsieur Torre's rod was said to have been erected "with Design, in some Measure, to dissipate the Fire which is in the Air," but the remainder of the communication does not make it clear as to whether the rod was grounded or insulated. Yet this account is written in terms so similar to current reports on insulated rods, such as those printed above, that I cannot help feeling that his rod, too, may have been insulted.

The Paris report of 5 August 1752 stated merely that several persons of quality had ordered protective rods to be erected on their houses, and if Franklin's specifications were used, these rods would have been grounded. I have been unable to find when (or, for that matter, whether) these rods were actually erected.

The "divided" lightning rod with bells that Franklin erected on his house in September 1752 afforded adequate protection and, if my reading of the final sentence in the kite letter be correct, two public buildings in Philadelphia were protected by rods in June or July 1752.

Eight years later, when Franklin was in England, he wrote Kinnersley that, despite the importance of the lightning rod "on our side of the water," . . . "Here it is very little regarded; so little, that though it is now seven or eight years since it was made publick, I have not heard of a single house as yet attempted to be secured by it." [77] Two months earlier, on 24 January 1762, he had sent the philosopher David Hume a description of the method for constructing lightning rods, omitting "the philosophical reasons and experiments on which this practice is founded; for they are many, and would make a book. Besides they are already known to most of the learned throughout Europe." Although the "philosophical reasons and experiments" were known throughout Europe, the practice was evidently not. "In the American British colonies," continued Franklin, "many houses have been, since the year 1752, guarded by these principles." [78]

7. FRANKLIN'S DELAY IN REPORTING THE KITE EXPERIMENT (AND OTHER SUBJECTS OF DOUBT IN THIS AFFAIR)

If, as I have attempted to show, there is no reason to doubt that Franklin had conceived and executed the kite experiment before hearing the news of the French performance of the sentry-box experiment, the reader may well inquire why so much space is necessary for the investigation of this question. Why should we not simply accept Franklin's and Priestley's word: especially since Franklin's statement in his letter to Collinson of 1 October 1752 and Priestley's account form a consistent picture with all of the evidence cited above? The answer lies in the fact that the June date has been seriously questioned and some investigators have concluded that Franklin must have flown the kite much later, at a time when he had already heard of the successful conclusion of the sentry-box experiment in France.[79] Furthermore, the statement has been made more than once that Franklin did not perform the experiment at all, or that, if he did, he did not report his results accurately in the letter to Collinson, which is then supposed to be a plan of an experiment to be made rather than an account of one that has already been made. Furthermore, as I pointed out in the beginning of this article, a statement has been made in the pages of the *Proceedings* of this Society to the effect that the experiment is nothing but a myth.

The most ardent critic of the kite experiment was Alexander McAdie, who concluded his article on "The date of Franklin's kite experiment" [23] with the following statement:

> The whole tenor of the letter of October 1 (19) 1752, indicates not so much an experiment actually performed as one projected and the results anticipated. For actually the phenonema are quite different. Franklin does not say in the concluding paragraph

[77] Smyth (ref. 33, *supra*) **4**: 146.

[78] *Ibid.*, 129.

[79] In other words, I have accepted Priestley's date of June 1752 and have indicated that since Franklin heard of the French experiments later in the summer, we have evidence consistent with Priestley's other statement that Franklin flew his kite before he learned what had been done in Europe. Others, e.g., Rotch (see ref. 22, *supra*), for whatever reasons, have assumed that Franklin must have performed the kite experiment after hearing of the French experiment and have then shown that he could not then have flown the kite as early as June.

that he actually charged a phial, etc. Only that it may be charged.[80]

McAdie gave a particular drubbing to Dr. Stuber, pointing out that his "account is in general terms; and what is rather surprising, explanatory and apologetic." Stuber's account is as follows:

It was not until the summer of 1752, that he was enabled to complete his grand and unparalleled discovery by experiment. The plan which he had originally proposed, was, to erect on some high tower, or other elevated place, a sentry-box, from which should rise a pointed iron rod, insulated by being fixed in a cake of resin. Electrified clouds passing over this, would, he conceived, impart to it a portion of their electricity, which would be rendered evident to the senses by sparks being emitted, when a key, the knuckle, or other conductor was presented to it. Philadelphia at this time afforded no opportunity of trying an experiment of this kind. While Franklin was waiting for the erection of a spire, it occurred to him that he might have more ready access to the region of clouds by means of a common kite. He prepared one by fastening two cross sticks to a silk handkerchief, which would not suffer so much from the rain as paper. To the upright stick was affixed an iron point. The string was, as usual, of hemp, except the lower end which was silk. Where the hempen string terminated, a key was fastened. With this apparatus, on the appearance of a thunder-gust approaching, he went out into the commons, accompanied by his son, to whom alone he communicated his intentions, well knowing the ridicule which, too generally for the interest of science, awaits unsuccessful experiments in philosophy. He placed himself under a shade, to avoid the rain—his kite was raised—a thunder-cloud passed over it—no sign of electricity appeared. He almost despaired of success, when, suddenly, he observed the loose fibres of his string to move towards an erect position. He now presented his knuckle to the key, and received a strong spark. How exquisite must his sensations have been at this moment. On this experiment depended the fate of his theory. If he succeeded, his name would rank high among those who had improved science; if he failed, he must inevitably be subjected to the derision of mankind, or, what is worse, their pity, as a well-meaning man, but a weak, silly projector. The anxiety with which he looked for the result of his experiment, may be easily conceived. Doubts and despair had begun to prevail, when the fact was ascertained in so clear a manner that even the most incredulous could no longer withold their assent. Repeated sparks were drawn from the key, a phial was charged, a shock given, and all the experiments made which are usually performed with electricity.[81]

I must agree with McAdie's acerb comment that if by the statement, "he placed himself under a shade," Stuber implied that Franklin stood under a tree to escape rain, he certainly was unaware of Franklin's "own previously published warning that it was very dangerous to stand under trees during a thunder-storm."[82] On the other hand, I am not so sure that I agree with McAdie's stricture: "Why should one who had made an estimate of what we may call the killing power of lightning, wish to expose his own son to probable death or at any rate intense shock?" I am sure that Franklin did not believe that he was taking his own life and that of his son into his hands when he performed this experiment. We may note, in this regard, that in his description of the original experiment of the sentry-box, he had stated that he did not believe there was any danger of the experimenter being killed by the lightning discharge during the performance of the experiment. McAdie points out that Franklin would hardly have gone to the common if he had desired to fly his kite "where none could see and comment." But he was thinking of a New England common in the center of the town, whereas the Philadelphia common was on the outskirts.

McAdie directed his criticism at Stuber, we may note, rather than at Priestley. "Dr Stuber knew Franklin intimately and it is said got the story of the kite from him," he declared. How reliable a witness was Stuber? An early nineteenth-century edition of Franklin's works referred to him as "one of the Doctor's [i.e., Franklin's] intimate friends."[83] Sparks stated cautiously that Stuber, "who resided in Philadelphia, . . . seems to have

[80] This is the final part of McAdie's conclusion, of which the remainder has been quoted, *supra*, above footnote 29.

[81] Quoted by McAdie. The above version has been corrected by a comparison with *The complete works in philosophy, politics, and morals, of the late Dr. Benjamin Franklin* 1: 107–109, London, J. Johnson & Longmans, Hurst, Rees, Orme, and Brown, second edition [no date].

[82] *Cf.* Franklin's letter to Dr. John Mitchel of 29 April 1749 "containing observations and suppositions, towards forming a new hypothesis for explaining the several phaenomena of thunder-gusts," in *Benjamin Franklin's experiments* (ref. 4, *supra*), 209, in which after noting that "As electrified clouds pass over a country, high hills and high trees, lofty towers, spires, masts of ships, chimneys, *&c.* as so many prominencies and points, draw the electrical fire, and the whole cloud discharges there," he concluded, "Dangerous, therefore, is it to take shelter under a tree, during a thunder-gust. It has been fatal to many, both men and beasts."

[83] Reference 81, *supra*, 98.

written from minute and accurate information." [84] Parton stated, "We owe our knowledge of what occurred on the memorable afternoon [on which the kite was flown], to two persons who had heard Franklin tell the story, namely, Dr. Stuber of Philadelphia and the English Dr. Priestley." [85] (What warrant there may be for placing the experiment in the afternoon, I do not know.) George Simpson Eddy, who provided McAdie with much of his information concerning Stuber, stated: "I have an edition of the Life and Essays of Dr. Franklin published in the Republic of Letters, a journal which was published in New York in . . . 1834. This life begins in No. 2 of that journal, page 171. On page 180 begins the continuation of Franklin's life written by Stuber, who is described as 'one of the Doctor's intimate friends.'" [86] Eddy apparently was willing to accept this statement that Stuber was a close personal friend of Franklin and did not express any disapproval of Parton's statement that Franklin had told the story in person to Dr. Stuber.

I do not know what the evidence is that Dr. Stuber was an intimate friend of Franklin, nor even that he got the story at first hand from Franklin.[87] There is no correspondence extant between Stuber and Franklin; he is not mentioned in any Franklin letter in the Smyth, Bigelow, or Sparks editions of Franklin's writings, nor was he of sufficient importance to be mentioned in Carl Van Doren's biography. The only information contained in Stuber's account, which is not to be found in Priestley's, is open to serious question, as McAdie pointed out, and leads one to believe that Stuber merely added a few personal embroideries to the account which he had obtained from Priestley.

But even if we may not place too much confidence in Stuber, I see no reason to doubt the credibility of Franklin's and Priestley's testimony. McAdie could not believe that Franklin had flown his lightning kite in June because no account of the experiment appeared in the newspapers: it seemed to him "quite improbable that a man so astute as Franklin and so keenly aware of the importance of this particular experiment, would have failed to publish a note, however brief, and preliminary, in the *Gazette.*" Yet the evidence indicates that Franklin was not in the habit of publishing brief and preliminary notes about his scientific discoveries in the *Gazette;* in fact, the publication of the kite letter in the issue of October 19 is, so far as I have been able to tell by an examination of the files of the *Gazette* during the years in which Franklin made his experiments, a solitary exception. And even in this case we may note that a communication had gone off to Collinson (and through him to the Royal Society) more than two weeks before the letter was published in the *Gazette.*

McAdie's next doubt was as follows:

> What is perhaps still more significant, E. Kinnersley, who was the chief expositor of the newly-discovered electric fire, and who was in close correspondence with Franklin (Franklin borrowed his "brimstone globe" March 2, 1752, and used it in making experiments in the spring of 1752) gave several public lectures, in which there is no mention of the kite experiment. In the Pennsylvania Gazette of September 14, 1752, there is an account of Kinnersley's lecture at the State House. And again in the issue of September 21, September 28, and October 19.[88]

[84] Sparks (ref. 7, *supra*) **5**: 173.

[85] Parton (ref. 7, *supra*) **1**: 295.

[86] From a letter written by Eddy to McAdie, 15 December 1923, quoted by McAdie (ref. 23, *supra*), 16-17. Eddy wrote: "Parton, Vol. 1, page 289, says (referring to the spring of 1752), 'nearly three years have rolled away since he had suggested *in his private diary* a mode of ascertaining whether lightning and electricity were really the same.' I do not know what Parton meant by 'private diary.' I think he must have been referring to the paper written by Franklin in 1749 and entitled 'Opinions and Conjectures, concerning the Properties and Effects of the Electrical Matter, arising from Experiments and Observations, made at Philadelphia, 1749.'"

Despite a number of errors, Parton usually knew what he was talking about and referred to genuine items. The "private diary" in question was one from which Franklin took an extract which he included in a letter to Dr. Lining dated 18 March 1755, which read: "*Nov.* 7, 1749. Electrical fluid agrees with lightning in these particulars: 1. Giving light. 2. Colour of the light. 3. Crooked direction. 4. Swift motion. 5. Being conducted by metals. 6. Crack or noise in exploding. 7. Subsisting in water or ice. 8. Rending bodies it passes through. 9. Destroying animals. 10. Melting metals. 11. Firing inflammable substances. 12. Sulphureous smell.—The electric fluid is attracted by points.—We do not know whether this property is in lightning.—But since they agree in all the particulars wherein we can already compare them, is it not probable they agree likewise in this? Let the experiment be made." *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 334.

[87] I have been unable to locate any biographical information concerning Stuber, save an occasional mention of his name.

[88] For Kinnersley's career, see *Benjamin Franklin's experiments* (ref. 4, *supra*), appendix one.

What reason can there be for Franklin having given Kinnersley the opportunity of making the first announcement of the lightning kite in Philadelphia? Considering the importance of the experiment, would we not rather have expected Franklin to have reserved for himself the first public statement about the kite, as he apparently did?

Finally, McAdie noted, "It would also seem that, once assured of the results, Franklin would have wasted no time in communicating with Peter Collinson to have the paper laid before the Royal Society." There are, I believe, two plausible reasons why Franklin might have delayed his report from June to October.

First, we must remember that Franklin was often slow (especially by current standards) in sending reports of his experiments to England. Thus the paper entitled "Further experiments and observations in electricity" was enclosed in a letter to Peter Collinson dated Philadelphia 29 April 1749, beginning: "Sir, I now send you some further experiments and observations in electricity made in Philadelphia 1748. viz . . ."; the famous paper entitled "Opinions and conjectures, concerning the properties and effects of the electrical matter, arising from experiments and observations, made at Philadelphia, 1749," was not sent to Collinson until 29 July 1750; and the letter written to Collinson in September 1753 (about the negative electrification of clouds) noted the dilatoriness at the very beginning when Franklin wrote, "In my former paper on this subject, written first in 1747, enlarged and sent to England in 1749, ..." The letter last mentioned, written in September 1753, described experiments which Franklin performed during the period from September 1752 to 6 June 1753.

A second possible reason for the delay, it seems to me, is that Franklin may very well have hoped for another opportunity to repeat the experiment before writing a full report for Collinson and the Royal Society. We know from a letter which Franklin wrote to John Perkins on 13 August 1752 that his affairs were pressing, that "business sometimes obliges one to postpone philosophical amusements." [89] The kite experiment was an almost incredible performance, and Franklin knew it. He might, therefore, well have deemed it necessary to have performed this experiment at least once more before publishing a formal account of it. This would certainly explain why Franklin did not write an account of the kite to Collinson immediately after hearing of the news about the French experiments, which, as I have shown above, occurred toward the end of August.

Yet another reason for the delay has been advanced by Van Doren in his splendid biography of Franklin. According to Van Doren, might not Franklin "deliberately have kept his secret till October so that he might publish at the same time, or almost the same time, in his newspaper and in his almanack the two most important pieces of his year's news? That is what he did. On 19 October his first account of the *Electrical Kite* appeared in the *Gazette*. The same issue advertised as in the press the new *Poor Richard* for 1753, which contained Franklin's first positive statement of *How to Secure Houses, etc., from Lightning.*" [90] The chief weakness in this argument, however, is that a description of lightning rods had already been published in the first edition of Franklin's book on electricity, which had already been out for a year.

Very likely, the delay on Franklin's part was caused by a multitude of factors, and the most important may have been a fear of ridicule. Many a modern commentator has called this experiment "foolhardy." Others still find it remarkable that Franklin and his son were not electrocuted. It certainly would have taken a great deal of courage for anyone to have said he had actually drawn down the lightning from the sky, or even that he had dared to fly a kite during a thunder-storm. Priestley points out, in the report which had its origins in Franklin's statements to him, that Franklin, "dreading the ridicule which too commonly attends unsuccessful experiments in science, . . . communicated his intended experiment to nobody but his son, who assisted him in raising the kite." To be sure this is a reference to the possibility of ridicule in case of failure, yet the notion of ridicule was clearly present in Franklin's own mind—even when he told Priestley about the experiment years later.

There can be no question but what it was difficult to take seriously a proposal to test the electrification of clouds by drawing down the lightning from heaven. The June issue of the *Gentleman's Magazine* (1752) contained "a letter from a gentleman at Paris to his friend at Toulon, concerning a very extraordinary experiment in electricity, dated May 14, 1752," which began:

[89] Smyth (ref. 33, *supra*) 3: 95–97.

[90] Van Doren (ref. 25, *supra*), 165.

You must remember, Sir, how much we ridiculed Mr *Franklin's* project for emptying clouds of their thunder, and that we could scarce conceive him to be any other than an imaginary Being. This now proves us to be but poor *virtuosi;* for yesterday I met a learned gentleman of the academy, who assured me that the experiment had been very lately tried with success. You may suppose I could scarce think him serious; however, I found that a memoir read at one of their assemblies had made so extraordinary an impression upon him, that I began myself to abate of my incredulity.

This gentleman was probably not alone in his sentiments, and I am sure that there were many who not only ridiculed Franklin's sentry-box experiment when it was first proposed, but who also could scarcely believe it when they were told that such an experiment had been successful. The British members of the Royal Society of London, despite the approval by some of them of Franklin's lightning hypothesis, did not make the experiment that Franklin had proposed. We do not know exactly why, and the reason may well have been that the "connoisseurs" laughed at it.[91] The laughter is all the more remarkable when we consider the splendid reception his earlier papers had received, even his preliminary statements on the electrification of clouds.[92] One use made of Franklin's conception of the electrification of clouds, as Collinson had written to him in April 1750, was by those who wished to solve the phenomena of earthquakes.[93]

The news that man had been able to draw the lightning from the skies was certainly astounding. Could the reports be true? We must remember that the first news of the lightning experiment originated in France; how many agreed with Collinson's sentiments in a letter to Franklin of 20 July 1753: "Wee know the French Very Well, subject to Levity, Hights & Extreams"?[94] When Watson published in the *Philosophical Transactions* of the Royal Society an account of the experiments with the insulated rod made by John Canton, Benjamin Wilson, and John Bevis, he noted that the effects were "trifling" when "compared with those which we have received from Paris and Berlin, but they are the only ones, that the last summer here has produced." Nevertheless, "as they were made by persons worthy of credit, they tend to establish the authenticity of those transmitted from our correspondents."[95] Surely the last part of this sentence indicates that there was not a universal trust placed by Englishmen in the reports from France and Germany.

It seems to me, therefore, that an important reason why Franklin did not at once make public the results of the lightning experiment in June was the fear that no one would take him seriously; he did not want to compromise his reputation. After he had heard of the news from France, he was then willing to publish a brief account of the kite, since he now had independent confirmation of what he had proved by means of this experiment.

No one who reads the pages of the *Pennsylvania Gazette* for the period from June to October 1752 can help but notice the many references to lightning, chiefly accounts of storms and their destructive effects during the early summer and news of European experiments in the late summer and early fall. Quite obviously, the subject of lightning was on Franklin's mind, as it well might have been if he had already performed

[91] "One Paper which I wrote for Mr. Kinnersley, on the Sameness of Lightning with Electricity, I sent to Dr. Mitchel, an Acquaintance of mine, and one of the Members also of that Society [i.e., the Royal Society of London]; who wrote me word that it had been read but was laught at by the Connoisseurs." Farrand (ref. 15, *supra*), 382.

[92] For example, Collinson wrote Franklin on 5 February 1750, "Your very Curious Pieces relating to Electricity and Thunder-Gusts have been read before the Society & have been Deservedly admired not only for the Clear Intelligent Stile, but also for the Novelty of the Subjects." *Benjamin Franklin's experiments* (ref. 4, *supra*), 80; *cf.* also pp. 82–84.

[93] Chiefly the Rev. William Stukeley, F.R.S., who mentioned Franklin by name in a paper explaining how earthquakes and lightning have the same cause—the electric fluid. *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*) and the article cited in reference 65, *supra.*

Stukeley entered the following comment in his diary, after hearing Franklin's letter of 1 October read at the Royal Society: "21 December 1752. At the Royal Society. Mr. Franklin, of Philadelphia, sent a pretty account of his extracting fire from the clouds, as singular in the invention as less operose and costly than those of the French astronomers. He makes a cross of two bits of cedar wood, tyes a silk handkerchief to the points by its corners, sets up a small iron half a foot long on that point which is the head of the kite, applys tail and wings to it as usual, a bunch of ribbands is to be between the end of the string and your hand, and then you fly it as ordinary kites when a cloud passes by loaden with the electric fire, and then you thus draw it down.—*Diary*, vol. xii., 2." From: The family memoirs of the Rev. William Stukeley, M.D. (2), *Publications of the Surtees Soc.* **80**: 466–467, 1885. *Cf.*, also, the article referred to in reference 65, *supra.*

[94] *Benjamin Franklin's experiments* (ref. 4, *supra*), 114, written apropos of the antics of Nollet in attempting to prove that Franklin's experiments would not work.

[95] *Phil. Trans. Roy. Soc.* **47**: 569, 1751 and 1752.

the kite experiment and had erected two lightning rods in Philadelphia. One such account, originating from Portsmouth, New Hampshire, 23 July 1752, and published in the *Gazette* for 6 August, was followed by a post-script of Franklin's reading: "A plain Proof of the Electrick Nature of Lightning." This post-script is all the more interesting in that the notice preceding it did not actually indicate such a proof at all, and simply stated:

> The main Mast of a Schooner at the North-end was struck by the Lightning; and altho' the Mast was shiver'd to Pieces by it (and the other Mast ruined by the Shock) till it came to a Ring that encompassed it (which it melted a little) yet below that Ring there were no Effects of it—A plain Proof of the Electrick Nature of Lightning.

Does this not read as if Franklin, having proved "the Electrick Nature of Lightning" (by the kite experiment), could not help adding a conclusion which, if not warranted by the facts reported from New Hampshire, was uppermost in his mind? He certainly knew what a "proof" was.

If we accept Priestley's statement that the kite experiment was performed in June, then Franklin's letter to Collinson of 1 October 1752 indicates that he did not hesitate long before erecting rods on at least two buildings in Philadelphia.

The kite experiment had proved to his own satisfaction that thunder clouds are electrified, or are charged bodies; hence according to everything that he had learned about electricity, the lightning rods should work. On this basis, we can understand why he did not include, in the version of his letter on the kite which he published in the *Pennsylvania Gazette,* the final paragraph about the erection of "points"; I believe that this topic is fully understandable as explained by Van Doren: "The rods on the Academy and State House were already known to Philadelphia." [96]

It is well to re-emphasize that once Franklin knew that clouds were electrified, he was certain that the lightning rods must work as he had predicted, since there was no reason to suppose that electrified clouds would behave differently with regard to pointed grounded conductors than ordinary electrified bodies in his laboratory. Jernegan and others indicate that Franklin had not in 1752 "proved by experiment that they [lightning rods] were a 'preservative against thunder.'" The only real proof to be had of the efficacy of lightning rods in preserving houses against lightning required that a bolt of lightning hit the rod and not destroy the house. The first such occasion arose eight years after the lightning rods had been first erected, i.e., in 1760, when the house of Mr. West was struck and was saved from destruction by the lightning rod which had been erected on it.[97]

If we can understand why Franklin might have held back any public announcement of the kite experiment from June until October, we must still answer certain other objections raised by McAdie. One of them is that Franklin did not repeat the kite experiment. Once Franklin had heard of the French experiments, which provided all the information he desired, further experiments with a kite were unnecessary. In September 1752, as we saw previously, he erected a form of lightning rod in his own house which provided him with ample means of making experiments on electrified clouds. The statement in his autobiography makes clear that Franklin thought that his sentry-box experiment as performed by Dalibard was superior to the kite experiment; certainly it was easier to perform. Once Franklin had heard, late in August 1752, of the European experiments, he immediately (September 1752) constructed the dual-purpose rod (to serve as a protective instrument for his house and as an experimental tool), since he now knew that the test instrument did not have to be as high as he had originally supposed. With this instrument, he easily verified for himself the results obtained in Europe and those previously obtained by him with the kite. Having, now, more than once proved the electrification of thunder-clouds, he wrote a note to Collinson about the kite.[98] All of Franklin's subsequent research on lightning was done with the rod, although (as we shall see in the next section) Kinnersley made further experiments with the kite.

[96] Van Doren (ref. 25, *supra*), 169.

[97] Sparks (ref. 7, *supra*) **5**: 375–377.

[98] Two further objections of McAdie to Priestley's account and Franklin's may be mentioned briefly. One is that the style ["tenor"] of the letter "indicates not so much an experiment actually performed as one projected and the results anticipated." I suppose McAdie referred to the form of statements such as, "You will find it [the electric fire] stream out plentifully . . .," or "the phial may be charged, . . . spirits may be kindled, . . .," etc., rather than "I found it . . .," or "a phial was charged, . . . spirits were kindled," etc. But a similar style was used by Franklin in earlier communications, e.g., that to Collinson of 1 September 1747, in *Benjamin Franklin's experiments* (ref. 4, *supra*), 179 ff.: Expt. III, "If a cork suspended by a silk thread hang between these two wires, it will play incessantly from one to the other . . ."; Expt. IV, "Place an electrised phial on wax . . ."; Expt.

8. THE LIGHTNING KITE EXPERIMENT IN THE MID-EIGHTEENTH CENTURY, AND ROMAS'S CLAIM TO PRIORITY

Among those who performed experiments with lightning kites in the mid-eighteenth century, one of the most interesting is the Frenchman Jacques de Romas, *assesseur au présidial de Nérac*,[99] since he claimed priority in the invention of this instrument. In a book supposedly dealing with lightning conductors, and the means of protecting houses from thunder, he devoted a considerable amount of space to establishing the grounds for his independent discovery. He related that "the first experiment on the electricity of thunder [at Marly] was announced to the public by all the gazettes and other periodical works,"[100] and he then decided to repeat these experiments, not—as he says—because he doubted their veracity, but rather to see whether there were new phenomena to be explored, which might be important "for the utility of civil society, or the progress of physics." He made some experiments with an insulated rod or bar but, wishing to increase the effects, he "plunged himself into meditation." Finally after one half hour the idea of the kite (*le cerf-volant des enfants*) presented itself to his mind. In a letter which he wrote to the Académie de Bordeaux on 12 July 1752 he announced his plan to use as a means of exploring the electrification of clouds "un Jeu d'enfant."[101] However August passed and the time of thunder-storms was over. He therefore waited until the following winter had passed and did not raise his kite until 14 May 1753, while a second experimenter watched the insulated rod erected on his house so that the two types of observation might be coordinated.

Romas insisted that "un Jeu d'enfant" referred unambiguously to a kite, and I see no reason to doubt his word. The paramount question in Romas's mind was whether Franklin had actually performed his kite experiment in June 1752, as Priestley said he had in his history of electricity, or whether Franklin had done it later. Plainly, to establish his own priority, Romas had first to show that Priestley's attribution of the month of June must be an error. But even assuming that Franklin's experiment had been made at the end of June, Romas wanted to prove that it would have been impossible for him to have received news of it earlier than the thirteenth of July; in other words, even if Franklin had thought of the kite earlier than he had, at least he wanted credit for independent invention. Romas asked: if Franklin had known him personally and had sent him a special message about the kite experiment at the end of June, could it have arrived in Bordeaux in

VI, "Place a man on a cake of wax, and present him the wire of the electrified phial to touch, you standing on the floor, and holding it in your hand. As often as he touches it, he will be electrified *plus;* and any one standing on the floor may draw a spark from him." At the end of his "Farther experiments and observations in electricity, 1748," Franklin referred to some experiments he and his friends planned to perform, and the style is quite marked: "Spirits, at the same time *are to be* fired. . . . A turkey *is to be* killed . . ." [my italics].

McAdie also states, "Franklin's conception, or perhaps the interpretation put upon the experiment and generally accepted, was that a cloud was a reservoir of electricity and the kite string a conductor. On the contrary, it appears to have been purely induction, not conduction. Had the kite string been wet enough to act as a conductor, the fibres would not have stood out." Two objections are indicated. First, the cord available in Franklin's day was a better conductor than McAdie supposed. For example, Stephen Gray's famous experiments on conduction and insulation and on induction made use of "packthread" for the conductor and silk lines for the insulator. But such cord is a better conductor when damp or wet, so that the system of metal in the kite, cord, and key was a better conductor when the rain had dampened the cord. Even if the phenomenon were, as pointed out earlier, one of induction and not conduction, the somewhat conductive (slightly) damp cord would produce the effect of fibres sticking out at a certain stage. McAdie may have underestimated the conductive power of eighteenth-century cord [packthread].

[99] *Cf.* Abria, Jérémie J. B., Rapport sur l'éloge de Romas, *Actes de l'Académie des Sciences, Belles-Lettres et Arts de Bordeaux* **15**: 441–446, 1853.

The Academy has also published a volume containing [1] Table historique et méthodique (1712–1875), [2] Documents historiques (1711–1713), and [3] Catalogue des Manuscrits de l'ancienne Académie (1712–1793), Bordeaux, Imprimerie G. Gounouilhou, 1879; *cf.* p. 39 (No. 150), p. 61 (No. 349, No. 350), p. 62 (No. 351, No. 353, No. 355), for descriptions of his unpublished communications, and for some account of MSS, see p. 185, p. 244 (No. 119), p. 245 (No. 142), p. 249 (No. 300, No. 315, No. 316, No. 317), p. 250 (No. 328 through No. 341), p. 278 (No. 1056), p. 279 (No. 1064)—the MSS in this list deal with scientific subjects, chiefly meteorology and electricity; they were (in 1877) in the Bibliothèque de la Ville (Bordeaux). We may note that two of the above MSS dealt with the trisection of the angle and perpetual motion. *Cf.* also p. 331 (No. 1552), p. 333 (No. 1556).

Three publications by Romas are listed by me in footnotes 100 and 109, *infra*, and a long study of his work in footnote 106, *infra*.

[100] *Mémoire sur les moyens de se garantir de la foudre dans les maisons*, 7, Bordeaux, Bergeret, 1776.

[101] *Ibid.*, 12. This letter is printed on pp. 105–106; it was apparently read at a meeting of the Academy on 17 July.

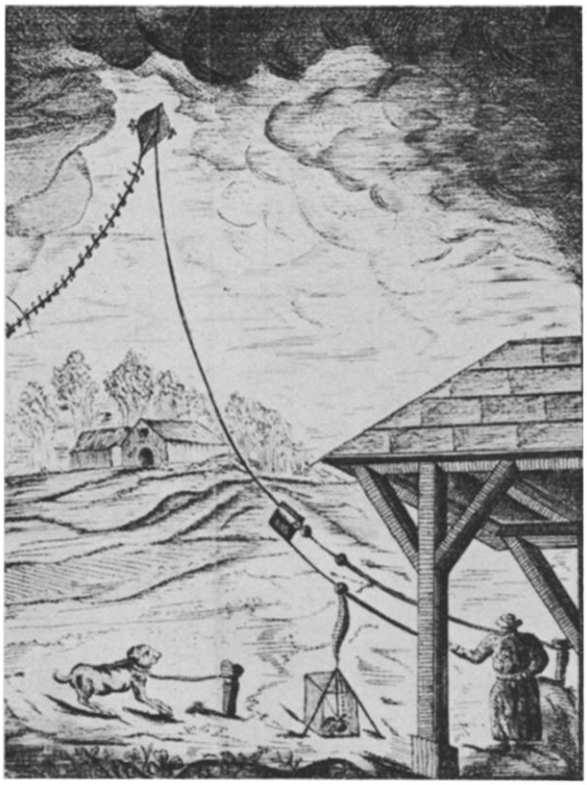

FIG. 8. One of the many experiments made by Romas with his electrical kite. Frontispiece to Romas, Jacques de, *Mémoire sur les moyens de se garantir de la foudre dans les maisons; suivi d'une lettre sur l'invention du cerf-volant électrique, avec les pièces justificatives de cette même lettre,* Bordeaux, Chez Bergeret, 1776. (Reproduced from A. L. Rotch's copy, now in Houghton Library, Harvard University.)

as little as thirteen days? But, he had never heard tell of Benjamin Franklin in June 1752 "and I do not have enough vanity to flatter myself that at this same time I had the honor to be known to him," [102]—to say nothing of the possibility of a ship getting the message from Philadelphia to Bordeaux within thirteen days! The first news of the alleged kite experiment of Franklin ("la prétendue expérience du Cerf-volant de M. Franklin" [103]), according to Romas, only arrived in the hands of his London correspondents by January 1753 [104] and it did not arrive in France until the fifteenth of January when Watson wrote a letter to Nollet about it; how then "could I have been informed about it by 12 July 1752?"

Romas had heard of the Marly experiment by reading an account in the *Gazette de France* for 27 May 1752, a copy of which arrived at Nérac, where he was stationed, only in the first days of June. [105] He assumed that at least an additional month would have been required to get the news to Philadelphia. Hence, if—as he believed—Franklin flew his kite only after hearing of the experiments of Dalibard and Delor, he could not have flown his kite in June as Priestley had asserted. Romas claimed, furthermore, that Priestley himself had indicated that Franklin's kite experiment postdated his learning of the French sentry-box experiments. The quotation which follows was alleged by Romas to have been taken from the 1767 (first English) edition of Priestley's history, and it was repeated *verbatim* by Merget in a supposedly definitive article on Romas written for the Academy of Bordeaux in the nineteenth century.

M. Franklin est le premier (nous savons maintenant à quoi nous en tenir sur cette priorité), qui ait soupçonné l'identité des éclairs et du fluide électrique; il a indiqué d'avance le moyen de constater cette identité, en proposant d'isoler à l'air libre, en temp d'orage, une aiguille électrisable par communication; le premier spectacle électrique que cet instrument ait offert, a paru en France sous les yeux de MM. de Lor et d'Alibard. *M. Franklin, animé par le succès de ces deux Messieurs,* éprouva lui-même le succès de son aiguille à Philadelphie, où il était alors. Ce physicien ayant eu aussi un heureux succès, pensa bientôt qu'au moyen d'un cerf-volant il pourrait se procurer un accès plus sûr et plus facile dans la région où s'engendre la foudre: l'idée de ce moyen se trouva juste, par l'épreuve qu'il en fit au mois de juin de la même année 1752, dans la compagne de Philadelphie, où il jugea à propos d'opérer sans autre témoin que son fils, pour éviter la risée des sots.[106]

It will be noted that the italicized phrase exactly contradicts the sense of Priestley's own words. This text as a whole does not come from Priestley's history at all,[107] although several phrases (e.g., the end of the final sentence) do; I suspect that Romas

[102] *Ibid.*, 109, 132–133.

[103] *Ibid.*, 110.

[104] *Ibid.*, 110; Romas was mistaken, since Franklin's letter about the kite was read at a meeting of the Royal Society on 21 December 1753.

[105] *Ibid.*, 133.

[106] Merget, [?], Etude sur les travaux de Romas, *Recueil des Actes de l'Académie des Sciences, Belles-Lettres et Arts de Bordeaux* **15**: 447–511, 1853. The italics in the above quotation are used by Merget (p. 484).

[107] I have searched diligently through the first three English editions and the French edition, without finding any passage such as that quoted above.

was quoting from a French review of Priestley and had never seen the original at all.

Romas tells us that on 19 October 1753 he addressed a letter to Benjamin Franklin,[108] along with two memoirs.[109] Franklin replied in a letter dated 29 July 1754, which Romas later printed in English and also in French translation, and which is reproduced in facsimile as figure 9. Although Franklin gave him "the hope that he would write again, I have never received any other letter."

On the score of Franklin's letter to him, Romas wrote:

> That which is worthy of being remarked well in this letter is that M. Franklin makes no claim to the invention of the kite [experiment]. That was, however, the time when he should have done so: he must have perceived in my letter, and more clearly still in the first memoirs [that I sent him], that I claimed to be the originator of this instrument.[110]

As a matter of fact, added Romas, he had also thought of making lightning experiments using an insulated bar (but one that ended in a ball rather than a point) in 1750, which was "more than a year before M. Franklin."[111]

Watson wrote a letter to the Abbé Nollet under the date of 15 January 1753, which was printed in French translation in a footnote to one of the two memoirs sent by Romas to Franklin, beginning: "M. Franklin has sent to the Royal Society, a fortnight ago, a very pretty electrical experiment for drawing electricity from the clouds."[112] There followed a description of the construction of the kite, precautions to be taken with it (including some not in the letter as printed in the *Philosophical Transactions*) and the experiments to be made with it. The note containing this letter then goes on to state:

> It seems by this letter that M. Franklin has used the kite prior to M. de Romas; but judging by the same letter and by the memoir of the latter, one will see that the effects were much greater at Nérac than at Philadelphia. This difference comes, it would appear, from the fact that M. de Romas garnished the cord of his kite with a metal wire, as one will see by reading his memoir.

If Franklin had flown his kite in June 1752, as I have every reason to believe he did, then Romas did not conceive of the same experiment until a month later, and since he did not fly his kite until 14 May of the following year, he thereby lost priority to Franklin both in the invention of the experiment and its performance. If Romas had actually conceived the experiment of erecting an insulated rod in 1750, it is a pity that he did not at once describe what he proposed to do and the means for doing it, since this would have given him priority in devising the first experiments to "draw the lightning from the skies." Since he neither published the idea, nor made the experiment prior to Dalibard's Marly experiment of May 1752, we are hardly entitled to give him credit for it. On the other hand, Romas may well have thought of using an insulated rod to test the electrification of clouds independently. As Franklin noted after quoting to Lining the extract from his diary about how he "came first to think of proposing the experiment of drawing down the lightning, in order to ascertain its sameness with the electric fluid," the thought "was not so much 'an out-of-the-way one,' but that it might have occurred to any electrician."[113] In any event, the possibility—even the probability— that lightning is an electrical phenonenon was not new in Franklin's day; what was original was an experiment to test this oft-expressed idea, and Franklin's was the first to be made public and to be carried into execution.

Although the note in Romas's memoir indicated that he had conceived the kite experiment independently of, but later than, Franklin, Romas—perhaps on the basis of Franklin's not asserting his own claim vigorously—concluded that Franklin might not have performed the experiment at all.

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[108] Romas (ref. 100, *supra*), 117.

[109] Romas implied that these were the two pieces published by the Académie des Sciences in the second volume of the "Mémoires des savants étrangers." If so, they were MS copies, since that volume was not published until 1755; furthermore, only one memoir by Romas appeared in the second volume, and the other one appeared in the fourth volume, but it was dated 1757!

[i] Mémoire, où après avoir donné un moyen aisé pour élever fort haut, & à peu de frais, un corps électrisable isolé, on rapporte des observations frappantes, qui prouvent que plus le corps isolé est élevé au dessus de la terre, plus le feu de l'électricité est abondant, *Mémoires de mathématique et de physique, présentés à l'Académie Royale des Sciences, par divers savans, & lûs dans les assemblées* 2: 393–407, 1755.

[ii] Copie d'une lettre écrite à M. l'Abbé Nollet par M. de Romas [De Nérac le 26 Août 1757], *Ibid.* **4**: 514–517, 1763.

For an English account, based on [i] above, see *Gentleman's Mag.* **26**: 378–380, 1756.

[110] Romas (ref. 100, *supra*), 118.

[111] *Ibid.*, 109.

[112] Freely translated from the French; printed in Romas (ref. 109 [i], *supra*), 395n.

[113] *Benjamin Franklin's experiments* (ref. 4, *supra*), 334.

TITRES
ET PIECES JUSTIFICATIVES,

QUI prouvent que M. de ROMAS a imaginé avant M. FRANKLIN le Cerf-volant Électrique.

LETTRE DE M. FRANKLIN A M. DE ROMAS.

Philadelphia, July 29. 1754.

SIR,

YOUR moſt obliging Favour of Octob. 19 with your two very ingenious Memoirs on the ſubject of Electricity, came not to hand till yeſterday. By this Veſſel, which is juſt departing for London, J can only acknowledge the Receipt of them, and aſſure you that the Correſpondence ſo kindly offer'd willbe extreamly agreable to me. A more particular anſwer J muſt defer till the next Opportunity;

in the mean time J ſend you a late Paper of mine on Lightning, which perhaps may not be publiſhed before this reaches your Hands. J am very reſpectfully,

SIR,

Your moſt obedient humble ſerv.t
B. FRANKLIN.

M. Romas.

TRADUCTION
De la Lettre de M. Franklin à M. Romas.

De Philadelphie, le 29 Juillet 1754.

MONSIEUR,

LA très-obligeante lettre dont vous m'avez favoriſé le dix-neuf Octobre, & vos

FIG. 9. Franklin's letter to Romas, in reply to Romas's letter and memoirs. Reproduced from Romas, Jacques de, *Mémoire sur les moyens de se garantir de la foudre dans les maisons; suivi d'une lettre sur l'invention du cerf-volant électrique, avec les pièces justificatives de cette même lettre,* 145–146, Bordeaux, Chez Bergeret, 1776. (Reproduced from A. L. Rotch's copy, now in Houghton Library, Harvard University.)

At any rate, we know that in 1764 he asked the Academy of Sciences to adjudicate his claim to priority. Two commissioners, Nollet and Duhamel, reported as follows:

> Having regard to all these proofs, we believe that M. de Romas had not borrowed from any one the idea of applying the kite to electrical experiments, and that one must regard him as the first author of this invention, until M. Franklin or some other makes known by sufficient proofs that he had thought of it before him. (4 February 1764.)[114]

A nineteenth-century partisan of Romas, Merget, noted:

> With his ordinary prudence, Franklin . . . remained with his mouth closed, as if he recognized on his part the justice of the judgment of the Academy; but this sly resignation, did not prevent him, three years later, in 1767, from letting his friend Priestley

[114] Freely translated from the French; Romas (ref. 100, *supra*), 149; *cf.* M. l'Abbé Bertholon, *De l'électricité des météores* 1: 32–55, Paris, Croullebois, 1787. Also Nollet, *Lettres sur l'électricité* 2: 228–248, Paris, H. L. Guérin & L. F. Delatour, 1754, asserting Romas's claims in a letter addressed to him.

speak of Romas in cavalier terms which we have transcribed above.[115] One can allege, it is true, in his justification, that he was ignorant of the declaration of the commissioners of the Academy; this is very possible without being in any way probable. But that which is beyond doubt, in any case, is that he knew in their extent the pretentions of his competitors; as the latter, under the date of 19 October 1753, had sent him two memoirs [109] where these pretentions were very clearly expressed, and where the experiment of the lightning kite, recounted in all its details, is presented as an original experience.

Franklin, it is true, never entered the lists in order to defend his own claims to the prior invention of the lightning kite. In scientific matters, his procedure was always that expressed by the lawyers' phrase *res ipsa loquitur*. In his autobiography, he related that he had not personally answered any of the attacks made on his ideas by the Abbé Nollet, having "concluded to let my Papers shift for themselves; believing it was better to spend what time I could spare from public Business in making now Experiments, than in Disputing about those already made." [116] At the height of the controversy in England as to whether lightning rods should end in balls or points, he wrote to Le Roy from London (30 March 1773) that "I have an extreme Aversion to Public Altercation on Philosophic Points, and have never yet disputed with any one, who thought fit to attack my Opinions." [117] A few months later, he wrote to Ingen-Housz that he would not answer a pamphlet by Wilson "against Points . . . being averse to Disputes." [118] When Ingen-Housz was embroiled in a dispute with Priestley over the problems of photosynthesis, Franklin wrote him:

I hope you will omit the polemic piece in your French edition and take no public notice of the improper behaviour of your friend; but go on with your excellent experiments, produce facts, improve science, and do good to mankind. Reputation will follow, and the little injustices of contemporary labourers will be forgotten; my example may encourage you, or else I should not mention it. You know, that when my papers were first published, the Abbé Nollet, then high in reputation, attacked them in a book of letters. An answer was expected from me, but I made none to that book, nor to any other. They are now all neglected, and the truth seems to be established. You can always employ your time better than in polemics.[119]

"Whatever some may think and say," he wrote to Ingen-Housz, "it is worth while to do men good, for the self-satisfaction one has in the reflection." [120]

In this spirit, he undertook no dispute with Romas. And, in a later publication, he generously referred to Romas's experiments:

M. de Romas saw still greater quantities of lightning brought down by the wire of his kite. He had "explosions from it, the noise of which greatly resembled that of thunder, and were heard (from without) into the heart of the city, notwithstanding the various noises there. The fire seen at the instant of the explosion had the shape of a spindle, eight inches long and five lines in diameter. Yet, from the time of the explosion to the end of the experiment, no lightning was seen above, nor any thunder heard. At another time the streams of fire issuing from it were observed to be an inch thick and ten feet long." [121]

Priestley devoted considerable space to Romas in his history and noted:

The greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose, was by Mr. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite. . . .[122]

Franklin's friend and co-experimenter Kinnersley also performed kite experiments. They are described in a letter written to Franklin on 12 March 1761, as follows:

Whether the electricity in the air, in clear, dry weather, be of the same density at the height of two or three hundred yeards, as near the surface of the earth, may be satisfactorily determined by your old experiment of the kite. The twine should have throughout a very small wire in it, and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points. I have tried

[115] Merget (ref. 106, *supra*), 490–491. Merget had reference to the following statement "from Priestley": "MM. de Lor et d'Alibard, dit-il, firent également l'expérience du cerf-volant en Angleterre, l'année suivante (ce qui est complètement faux), et M. de Romas voulant s'assurer par lui-même de ce qu'il entendait raconter à ce sujet, la répéta en France avec beaucoup plus d'appareil." My comments in footnote 107, *supra*, apply equally well here.

[116] Farrand (ref. 15, *supra*), 384.

[117] Smyth (ref. 33, *supra*) **6**: 28–29.

[118] *Ibid.*, 141–143.

[119] *Ibid.* **1**: 14.

[120] *Idem.*

[121] *Ibid.* **5**: 422n.

[122] Priestley (ref. 8, *supra*) **1**: 411.

the experiment twice, when the air was as dry as we ever have it, and so clear that not a cloud could be seen, and found the twine each time in a small degree electrized positively. The kite had three metalline points fixed to it; one on the top, and one on each side. That the twine was electrized, appeared by the separating of two small cork balls, suspended on the twine by fine flaxen threads, just above where the silk was tied to it, and sheltered from the wind. That the twine was electrized positively, was proved by applying to it the wire of a charged bottle, which caused the balls to separate further, without first coming nearer together. This experiment showed, that the electricity in the air, at those times, was denser above than below. But that cannot be always the case; for, you know, we have frequently found the thunder-clouds in the negative state, attracting electricity from the earth; which state, it is probable, they are always in when first formed, and till they have received a sufficient supply. How they come afterwards, towards the latter end of the gust, to be in the positive state, which is sometimes the case, is a subject for further inquiry.[123]

Nor was Kinnersley the only American to repeat Franklin's experiment; another was John Lining. In a letter dated 14 January 1754, Lining described his kite in the following terms:

The kite, which I used, was made in the common way; only, in place of paper, I covered it with a silk, called *alamode*. The line was a common small hempen one of three strands. A silk line, except it had been kept continually wet, would not conduct the electricity; and a wire, besides other inconveniences, would have been too heavy. I had not any instrument, whereby I could take the height of the kite; but, I believe, it was at least 250 feet high. It was flown in the day-time.[124]

Lining evidently used a key at the end of the kite-string, just as Franklin had, and he charged a Leyden jar from it, repeating the experiments usually made with the electrical machine at its prime conductor.

Another mid-eighteenth-century physicist to experiment on atmospheric electricity with a kite was the Abbé Beccaria, a staunch Franklinist, who did much to promote the use of lightning rods in Italy and who appears to have been the first person to be successful at electrolyzing metallic compounds. In one of his writings on atmospheric electricity, he related:

It was in the year 1756, that the frequent and continued use of kites, which other observers only used to make researches on the electricity of clouds, procured me a confirmation of what I had till then only conjectured, that is to say, that even during clear weather (except in the cases of a great dampness of the air, or of an impetuous wind) a mild weak electricity perpetually took place.

Kites were most useful instruments to me, for such first experiments on the state of the atmosphere. They rise to a great height, to a region where the difference of the atmospheric electricity uses to be greater; they gather great quantities of this electricity, by means of the pack-thread which holds them, and they retain it the better as they are capable of being insulated. . . . Now, a string made of the best silk, of a small diameter, and of great length insulates a kite extremely well; and it is an easy matter to keep it dry by warming it, or to change it, when it grows damp. Though I was at first ignorant of the contrivance of Sig. Romas, who interweaves the string which holds his kite with thin metallic wires, the same thought occurred to me the more naturally, as I was then exploring the accidents of the weaker electricity that takes place in serene weather.[125]

Accordingly to Priestley,

He made use both of kites and pointed rods, and of a great variety of both at the same time, and in different places. Some of the strings of his kites had wires in them, and others had none. Some of them flew to a prodigious height, and others but low; and he had a great number of assistants, to note the nature, time, and degree of appearances, according as his views required.

To keep his kites constantly insulated, and at the same time to give them more or less string, and for many other purposes, he had the string rolled upon a reel, which was supported by pillars of glass; and his conductor had a communication with the axis of the reel.[126]

Yet another to make experiments on atmospheric electricity with a kite was Peter Van Musschenbroek, one of the discoverers of the Leyden jar or condenser.[127] It should be noted, however, that

[123] Sparks (ref. 7, *supra*) **5**: 370–371.

[124] *Phil. Trans. Roy. Soc.* **48** (2): 758, 1754. *Cf. Gentleman's Mag.* **23**: 431, 1753.

[125] Beccaria, Giambatista, *A treatise upon artificial electricity . . . to which is added an essay on the mild and slow electricity which prevails in the atmosphere during serene weather, translated from the original Italian,* 449–450, London, J. Nourse, 1776. For Franklin's high opinion of Beccaria's work, see his letter to Dalibard (ref. 44, *supra*).

[126] Priestley (ref. 8, *supra*) **1**: 396.

[127] *Cf.* Mottelay, Paul Fleury, *Bibliographical history of electricity and magnetism,* 320, London, Charles Griffin, 1922, for a table of eighteenth-century experiments on atmospheric electricity.

apparently none of these experiments was made during a severe lightning storm, that in all cases the key and the kite string or wire were charged by electrostatic induction, and that the kite was used to advance knowledge of the electrification of the atmosphere even in serene weather. Thus the kite provides us with an example of the way in which a tool invented to solve a specific problem (the possible electrification of clouds) finds application in investigating a much larger class of phenomena.

9. CONCLUSION

Since electrical kites were flown by others, there is no reason to suppose that one might not have been flown by Franklin. Priestley's testimony, approved by Franklin, set the date of the experiment in June 1752, one month after the performance of Franklin's earlier (sentry-box) experiment. The implication of Franklin's letter of 1 October 1752, describing the kite to Collinson, is that some sort of metal "points" had been erected in Philadelphia in June 1752, hence earlier than the rod (with the warning bells) erected by Franklin on his own house in Philadelphia in September 1752.

No information has as yet been uncovered, despite a considerable research by a number of different individuals, to make us reject the testimony of Franklin and Priestley. Although a number of perplexing questions have been raised, they may all be answered in a reasonable way, although some conjecture is required on occasion, e.g., to explain the delay in publishing the experimental results. Further confidence in the accounts by Franklin and Priestley arises when we review the accumulated collateral material on lightning and lightning rods in the period from June to October 1752, all of which is consistent with the dates and other information they provided. In any event, it is difficult to think that Franklin, who was always exceedingly honest in reporting scientific information, would have in any way falsified the record.

I do not believe that all relevant information about this episode has been uncovered. Everyone who has ever investigated the question has been able to add something new. Perhaps the discovery of hitherto unknown Franklin correspondence or of diaries and letters of his friends or fellow inhabitants of Philadelphia, will some day reveal more details about the kite itself and the Philadelphia lightning rods. But, as matters stand now, in June 1952, two hundred years after the time when Priestley asserted with Franklin's approval that the kite was flown, I believe we are justified in celebrating the two-hundredth anniversary of the lightning kite experiment and the two hundredth anniversary of the first lightning rods—erected in Philadelphia in June or July 1752 soon after the kite was flown.[128]

[128] I am fully aware that many more statements in this article should contain such words as "very likely," "possibly," "probably," "may very well have," etc. Yet the reader will not, I am sure, have had any difficulty in separating statements of fact from interpretation. It is plain that I have followed a policy of believing what Franklin wrote or told Priestley and then seeing whether every other available scrap of information can be given a plausible explanation that squares with the Franklin-Priestley chronology. The critical reader must decide for himself whether this has been successful, keeping in mind that a different fundamental premise—e.g., that Franklin flew his kite only after learning about the Marly experiments—would lead to wholly different interpretations. I hope that some readers will be tempted to challenge some of my interpretations and conclusions and uncover further information in the process, so that I hope—to quote Franklin—that this article may excite "the attention of the ingenious to the subject, and so become the occasion of more exact disquisition and more compleat discoveries."