TO ALEXANDER DALLAS BACHE

Bache Papers, Smithsonian Archives

Princeton Wednesday evening
Dec 17th 1834

My Dear Sir

I send you this evening my drawings[1] & hope they will reach you in time for the Friday evening meeting of the Phil Society.[2]

I would apologize for my thus occupying so much of your time but you know that I shall always be willing if able to discharge the debt. The following is the title of my paper which you can alter in any way to suit your more refined taste in such matters.

Description of a galvanic battery intended to produce intensity or quantity with some observations on the peculiar effect of a long conductor in producing sparks in a galvanic circuit. By Joseph Henry Prof. of Natural Philosophy in the College of New Jersey, Princeton, (Late of the Albany Academy) Member of the Royal Physical Society of Edinburgh &c.[3]

This title will need a little explanation. I have been induced to add the second part "Some observations &c" on account of a paper by Mr Faraday in the last (Nov) Num of the London & Edinburgh Magazine, p 349.[4]

By a reference to this paper you will see that Mr Faraday has come across my fact of the production of a spark by means of a long wire. I am now anxious to publish my observations on this point as soon as possible and

[1] Presumably the ones, as amended, which appear as Plate 22 (facing p. 218) in the fifth volume (n.s.) of the *Transactions* of the American Philosophical Society illustrating Henry's "Contributions I: Battery." See the frontispiece to this volume.

[2] December 19, 1834. The subsequent history of these drawings and Henry's text will appear in later documents.

Apparently the drawings reached Bache in time. The printed proceedings of the American Philosophical Society for that date state:

> Dr. Bache, on the part of Prof. Bache, explained some modifications of the galvanic battery made by Prof. Henry of Princeton, which, with the peculiar effect of a Long Conductor in producing Sparks in a Galvanic circuit, will be the subject of a communication to be made at a Subsequent Meeting.

"Dr. Bache" is Franklin Bache. *Early Proceedings of the American Philosophical Society . . .* (Philadelphia, 1884), p. 662.

[3] As will come out subsequently in this volume, Henry's work ultimately appeared as two separate pieces published back-to-back, the first on the new galvanic battery, the second on the influence of the spiral conductor on increasing the intensity of electricity. The latter, responding to Faraday's work, is dealt with extensively in documents below.

[4] "On the Magneto-electric Spark and Shock, and on a Peculiar Condition of Electric and Magneto-electric Induction," *Phil. Mag.*, 1834, 3d ser. *5*:349–354. This is a letter to the editor, Richard Phillips, and is dated October 17, 1834. The December issue, pp. 444–445, has a later letter (November 20, 1834) containing corrections. Although these were reprinted in Faraday's *Experimental Researches in Electricity*, 2:204–211 (London, 1844), they were not part of Faraday's series of experimental researches in electricity, parts of which were appearing simultaneously in *Phil. Mag.*

do not know a more ready method than to append them or to incorporate them with my description of the battery. Fortunately I published a notice of the fact soon after I discovered it in the same No of Sillimans journal which contains your paper on the magneto electric spark sometime in 1832. It is however so long since I have seen the article that I know not what I have said on the subject. Since then I have made a number of additional observations in reference to this subject which are at variance with the views of Mr Faraday. These I will carefully repeat.[5] Mr F has probably not seen my paper or has forgotten the fact.[6]

The same journal contains a remark which perfectly agrees with my opinion in regard to the magnetometer formed of a long wire, p 349. It is stated that a pair of plates one inch in diam will produce with the applica-

[5] Henry's paper, "On the Production of Currents and Sparks of Electricity from Magnetism," appeared in the July 1832 number of *Silliman's Journal* (22:403–408). The passage in question is the last paragraph of the article:

> I have made several other experiments in relation to the same subject, but which more important duties will not permit me to verify in time for this paper. I may however mention one fact which I have not seen noticed in any work and which appears to me to belong to the same class of phaenomena as those before described: it is this; when a small battery is moderately excited by diluted acid and its poles, which must be terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken: but if a wire thirty or forty feet long be used, instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury a vivid spark is produced. If the action of the battery be very intense, a spark will be given by the short wire; in this case it is only necessary to wait a few minutes until the action partially subsides and until no more sparks are given from the short wire; if the long wire be now substituted a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire; I can account for these phaenomena only by supposing the long wire to become charged with electricity which by its reaction on itself projects a spark when the connection is broken.

Faraday and Henry elaborated their experimental findings and explanations—Faraday in his Ninth Series and Henry in "Contributions II: Spiral Conductor." The parallelisms between the two investigators are striking, giving rise to a particularly tangled priority question. Questions of scientific content and of the very real psychological crisis facing Henry are dealt with in documents appearing subsequently. At this point all Henry knows is that Faraday has published findings in an area which Henry "had not seen noticed in any work," jeopardizing his claim to original discovery.

The "Bache paper" is actually extracts of two letters from Joseph Saxton, "Notice of Electro-Magnetic Experiments" (*Silliman's Journal*, 1832, 22:409–415) sent by Bache to Silliman in advance of publication by the Franklin Institute's *Journal*, a precedent, as it were, for what will occur to Joseph Henry in this priority race with Faraday.

[6] Henry's 1832 article was in the appendix of the July 1832 issue; the relevant passage was the very last paragraph. Henry is probably correct in his inferences; it could have been missed or the last paragraph forgotten in the intervening time period. When the Smithsonian Institution published its two-volume edition of Joseph Henry's *Scientific Writings* (Washington, 1886), the reprinted text of the July 1832 article had a heading added before the final paragraph, "*Electrical self-induction in a long helical wire*" to call attention to the basis for the priority claim. What Faraday did or did not know about Henry's work will be elaborated below in footnote 2 of John Vaughan's letter of August 5, 1835.

tion of mere spring water an effect not much less than that produced by a very large pair of plates with the application of strong acid &&c. Read the paper as I know you will.[7]

Can you give me any information about the "*beautiful theory established by Ohm*"? Where is it to be found? I am anxious to see his papers. See the same page of the journal.[8]

[7] Henry knew Bache would read the article as it was the description of Gauss's magnetic observatory at Göttingen. At the very end of the article, just above the start of Faraday's letter to Richard Phillips, is a concluding paragraph describing electromagnetic apparatus devised by Wilhelm Eduard Weber (1804–1891, *Poggendorff*). Henry is referring to Gauss's account of Weber's running of wires, "nearly 6000 feet" between the astronomic observatory and the cabinet of natural philosophy, one end being attached to the magnetic intensity apparatus. At the other end, Weber produced currents by batteries: "It is wonderful how a single pair of plates placed at the other extremity immediately impart to the magnetic rod a motion equal to considerably more than a thousand divisions of the scale." Henry's sentence about the plates one inch in diameter is a paraphrase of the text. These results are in accordance with Henry's work, as in the modulus of conduction experiments of September 9, 1834, above.

[8] This occurs on p. 349 in the account by Gauss of the apparatus at his magnetic observatory immediately after the report of experiments with the pair of plates of one-inch diameter. The full sentence reads: "This circumstance, however, is after all not surprising, as it only tends to confirm the beautiful theory first established by Ohm." This is the first time Henry ever heard of the work of Georg Simon Ohm (1789–1854), the German mathematical physicist best remembered for the law bearing his name. Henry's knowledge and use of the work of his German contemporary is important in its own right; such knowledge and use has bearing on historiographic questions arising from the literature on the reception of Ohm's work.

Clearly, neither Henry nor Bache (see his reply of January 3, 1835, below) had heard of Ohm. We assume Henry's eagerness to learn of the theory but have no evidence of Henry's concrete knowledge of Ohm's Law until 1837 during the course of Henry's European trip. Volume three will treat this incident in which Charles Wheatstone gave Henry a copy of an article in French dealing with Ohm written by M. H. von Jacobi. Ohm's work afterwards is assimilated into Henry's without any difficulties discernible to us. In later volumes we will discuss instances in which Ohm's Law surfaces in Henry's experimental work and in print. The natural philosophy lectures to his students incorporated Ohm's work; a few unpublished fragments refer to Ohm—for example, in connection with unipolar conductors. Henry's Library contains a copy of the French translation (Paris, 1860) of Ohm's 1827 monograph, *Die Galvanische Kette, mathematisch bearbeitet.*

A small but important body of secondary literature exists on how Ohm's contemporaries received his contributions. Ohm's Law is now seen as the product of the interaction of experiment and mathematics regarded by many as exemplifying the best of modern physics. Its reception is viewed almost as a litmus test. Henry's case has relevance to this literature. In "The Reception of Ohm's Electrical Researches by his Contemporaries" (*Phil. Mag.*, 1944, 7th ser. *35*:371–386), H. J. J. Winter notes that Faraday's and Henry's researches were made without knowledge of Ohm's Law. Relying upon a garbled passage in an article by Mary Henry, Winter cites Henry's query to Bache in this letter, giving her incorrect date, December 17, *1833*. (Mary Henry, "America's Part in the Discovery of Magneto-Electricity—A Study of the Work of Faraday and Henry—1," *The Electrical Engineer*, 1892, *13*:27–30, especially p. 30. On the same page she cites another letter without date or addressee, asking about Ohm and his theory. Such a document has not turned up.) Henry's query to Bache appears in Winter, we feel, because of the date. The implication is that the reception and acceptance came towards the end of the decade, being signaled by Ohm's Copley Medal in 1841 and the appearance of his work in translation in the same year in Taylor's *Scientific Memoirs* (2:401–506). Given the laudatory reference by Gauss and the existence of other favorable notices in German by 1834, there is every probability that active investigators of

I recollect that in my paper mentioned above there are some typographical blunders which render parts unintelligable I believe but they do not affect the point in question.[9]

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electromagnetism would behave like Henry—be curious and try to find out the new law. Hegelians impeded Ohm's reception in Germany according to Winter. Given the strong and prompt support Ohm received from many eminent German scientists, this seems dubious. That reason is certainly not applicable to Great Britain and the United States.

A later article by Morton L. Schagrin, "Resistance to Ohm's Law," *American Journal of Physics*, 1963, *31*:536–547, continues to be cited although little evidence is offered for its thesis. In Schagrin's view, Ohm inevitably encountered resistance because his contribution was a conceptual innovation. To support this, Schagrin treats one of the men discussed by Winter, G. F. Pohl, and has a misleading footnote to another German scientist, P. Erman (1764–1851). Winter cites a few other instances, such as Berzelius's opposition to Ohm's views on unipolar conductors. One could just as easily list an equal number of eminent scientists backing Ohm. The "proof" of the resistance apparently rests on the time required for Ohm to get a full professorship in a German university and that hinges, in all probability, on the general state of the German universities and on academic politics. Ohm was not unique in not receiving quickly a chair upon publication of a notable research finding.

The articles in question assume resistance because they assume Ohm's Law, now taken for granted, must have been viewed as a revolutionary innovation by contemporary scientists. A contrary case is possible, that prior work had paved the way for Ohm's acceptance. In an 1823 article by Oersted and Fourier ("Sur quelques nouvelles expériences thermoélectriques," *Annales de chimie et de physique*, 1823, 2d ser. 22:375–389) occur these words:

> . . . it is evident that, in the case where a current A of an intensity equal to that of another current B, but of a larger quantity, is presented to a conductor which is sufficient only to transmit the quantity of B, this conductor ought to be capable of transmitting a part of the current A equal to current B; and if we suppose A to have an intensity stronger than that of B, the transmission of A will be yet more abundant. [p. 384; translation from Pierson, see below]

L. Pearce Williams has shown how close Faraday came to discovering the law (*Michael Faraday* [New York, 1965], pp. 210–211). One could argue that the sense of the law was implicit in the researches of Faraday, Henry, and others. Certainly the subject area was actively under investigation as evidenced by, for example, the "Record of Experiments" entries of September 9 and 10, 1834, above, on the modulus of conduction.

We suspect many contemporaries regarded Ohm's work as interesting and useful but not an overwhelming conceptual innovation. The Law was usable in a narrow positivistic sense without inhibiting the development of various causal explanations. Among these scientists (Henry being one example) there was not resistance but recognition of a convenient advance. Ohm is explicitly absent from the indexes to Faraday's work even though the simple equation was within his competence. Faraday's conceptual concerns were elsewhere. References to Ohm are fairly scarce in Henry's publications and personal papers. Henry did send Ohm a copy of "Contributions II: Spiral Conductor" addressed to Berlin rather than Nuremberg. Winter (p. 380) quotes an 1842 letter of Ohm listing "Henry in England" as one of the investigators favoring his theory. There is evidence of closer connections of Henry with Lenz and Jacobi in distant Russia. The conceptual innovations animating Henry were those of Ampère and Faraday, not Ohm.

In addition to Winter and Schagrin, we are indebted to an unpublished paper by Stuart Pierson, "Is Ohm's Law Ohm's Law?" and to Kenneth L. Caneva for an advance copy of his *DSB* article on Ohm, particularly the comprehensive bibliography. Henry's Address Book in the Smithsonian Archives has the note about the reprint sent to Berlin.

[9] None are obvious to us in Henry's paper, *Silliman's Journal*, 1832, 22:403–408. He may be referring to omitted punctuation which creates awkward run-on sentences on pages 405 and 407. As Henry wrote to Bache, the meaning is clear in the text on self-induction. Henry was probably mistakenly thinking of the typographical errors which occurred on pages 404 and 407 of an earlier article of his,

Young Mason[10] has promised to superintend the engraving of my drawing should the Society agree to publish it. I wish him to attend to the following corrections. 1st The wheel is too wide in the direction of its axis *ie* the teeth are too long. In the machine they are not more than an inch whereas in the drawing they appear comparatively about 2 inches long. 2nd In the plan the plates are a little too thin, or the spaces between the several elements are a little too great.

3. There is a small mistake in the arrangement of the conductors which I have corrected in the rough drawing which I send with the others.

4. The cups should be shown at the sides of the straight conductors on the lower half of the plan since they are wider than the connectors. This is also shown on the plan drawing which I have made. Mr Mason will understand these corrections and they can be made by the engraver.[11]

Almost every moment of my time has been occupied by my college duties since my return from Phild I wish to finish my course of lectures entirely in the Winter session so that I may have some leisure for experiment &c in the Summer.

I regret that I did not see my very kind old friend Mr Vaughan when last in your city. I called at his rooms but did not find him in. Will you give him many thanks for his kind offer of the use of the library &c.[12] I hope to meet with him at the Whister Party on Saturday evening but was disappointed.[13] I am now in the humor for working and will give you my paper before New Year. Give my respects to Courtnay[14] [and] Dr Ludlow[15] —tell him he is expected at Princeton before he commences his duties in the university—and also to Mrs. Bache.

I find that I shall have to use a wrapper so I may as well fill my sheet.[16] Mr Alexander returned today from his southern expedition. On the whole had an interesting tour and is well satisfied with his observations. Spent

"On the Application of the Principle of the Galvanic Multiplier to Electro-Magnetic Apparatus . . . ," *Silliman's Journal,* 1831, *19*:400–408.

[10] S. Rufus Mason.

[11] From Plate 22 of APS *Transactions,* n.s. *5* (see the frontispiece to this volume), this is Joseph Yeager (ca. 1792–1859), active in Philadelphia from 1809 to circa 1845. Yeager mostly etched portraits and engraved views of scenery and buildings; he was also a print seller and publisher of children's books. In 1848 Yeager became President of the Harrisburg and Lancaster Railroad, later part of the Pennsylvania system. *DAB.*

[12] A reference to John Vaughan's letter of December 8, 1834, above.

[13] We assume Henry was at the Wistar Party of December 13, 1834, and at that date discussed his paper, perhaps even the priority conflict with Faraday.

[14] E. H. Courtenay.

[15] Reverend John R. Ludlow.

[16] We assume Henry had to have an additional sheet as a wrapper because the drawings referred to at the start of the letter were enclosed. Consequently, this sheet could be used for further writing because space for the address was not needed. Frugally, Henry was using up the inner sheet. Prior to the postal reform of 1845, mail rates were determined by the number of sheets, not by weight as at present.

some days with Paine[17] in Charleston. Mr A. says that the shooting stars were not more numerous on the evening of the 13^{th} & 14^{th} than usual. He saw it stated in the papers that Mr Paine was on shipboard and requested the watch to keep a sharp look out. A greater number than usual was not seen. Mr Alexander has received 22 letters from different Post masters along the line of the total Eclipse in answer to a circular addressed to them. Shooting stars were seen in two places during the total darkness. Paine does not believe in Olmsteads Theory, neither does Bowditch.[18] A comet under the circumstances supposed could not revolve twice in the same orbit on account of the great perturbations from the earth & moon.[19]

You will probably read with much interest the article in the Edinburgh & London Mag. which I have alluded to on the Magnetic Observatory. The instrument with a long wire is I suppose intended to indicate the changes of atmospheric electricity by the use of a galvanometer made with a very long wire coiled many times around the needle box.[20]

[17] Robert Treat Paine (1803–1885), a member of a distinguished Boston family, graduated from Harvard in 1822. An active astronomer until his death, Paine was a practicing attorney of independent means. He helped establish the *American Almanac* and contributed the astronomical content from 1830 to 1842. During his active lifetime he observed four total and five annular eclipses. For nearly sixty years Paine maintained a continuous meteorological record. Alexander was presumably with Paine at Beaufort Arsenal, South Carolina, to observe the total solar eclipse of November 30, 1834. American Academy of Arts and Sciences, *Proceedings*, 1886, *21*:532–535. Clark A. Elliott, "The American Scientist, 1800–1863: His Origins, Career, and Interests" (Ph.D. dissertation, 1970, Case Western Reserve University), p. 321.

[18] Nathaniel Bowditch. Denison Olmsted's theory of the annual recurrence of the meteor shower was being tested by attempting to count shooting stars to see if they were more or less numerous at the time of Olmsted's prediction of their return.

[19] Olmsted's explanation of the meteor swarms of November 12–13, 1833, was that they were from the tail of a comet. From contemporary observations and from past descriptions of showers of shooting stars in the literature, Olmsted postulated a "cloud of small bodies" in an orbit of a half a year around the sun which met the earth each year at about the same time. Henry here expresses doubt that such a small body could continue in such an orbit in view of the perturbations to which it was subject.

Olmsted's theory was not clearly disproved until 1867 by John Couch Adams. This particular meteor swarm was named the Leonids because the apparent radiation point was from the head of Lion constellation. After 1834 the Leonids displays became scarce and a thirty-three year orbit was proposed. Adam's calculations confirmed the latter, as did later observations. When the 1899 Leonids failed to appear, further calculations disclosed that the orbit was displaced by the gravitation of the larger planets, now passing twice the prior distance from the earth.

For Olmsted, see his "Observations on the Meteors of November 13th, 1833," *Silliman's Journal*, 1834, *26*:132–174 and A. Pannekoek, *A History of Astronomy* (New York, 1961), pp. 419–422.

[20] Henry here misinterprets Gauss's intentions, which were not to measure changes in atmospheric electricity. The description here (pp. 348–349) does not give a full picture of what Gauss and Weber were doing. Henry, a man greatly interested in problems involving atmospheric electricity, erroneously concluded that the large-scale, sensitive, galvanometer was to detect charges in the air. In an earlier description, Gauss stressed that his "stout, heavy needles . . . were the most sensible and convenient galvanometers both for the strongest and weakest energies of the galvanic current." A simple wire sufficed for the former; "for very weak energies a multiplier is wound

My page is full and the slowly moving finger of time "points to the west of the key stone of nights black arch."[21]

Your Friend J.H.

Please reccollect that my battery consists of 88 plates or elements similar to those now in your possession, that I can change it from a [calorimotor][22] of one element to a battery of two &c up to one of 88 elements. One trough can be used or even one element separately. Different parts of the battery can be used at the same time producing electricity of different intensity (See next margin).[23] Thus while one half of the battery is arranged as a deflagrator the other may be acting as a calorimotor and the combined effects of these instruments determined. A series of experiments have been made in reference to insulation and non insulation.[24]

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round the box containing the needle." Using the observational method Gauss devised, "the measurement of galvanic forces may be conducted with a degree of ease and precision unattainable by the hitherto employed laborious modes by means of observed times of vibration; and it is literally true that by it we are enabled to follow from second to second the gradual increase of the intensity of a galvanic current. . . ." This possibility of precision measurement, as in astronomy, is not as clear in the November 1834 article in the *Phil. Mag.* Instead, there is the reference to Ohm (cryptic to Henry) and the account of transmission of current from the cabinet of curiosities to the magnetic observatory in Göttingen followed by a prediction about telegraphy. Gauss, we infer, links Ohm's Law to this possibility of precision measurement. The setup for terrestrial magnetism, by the coiling of wire around the needle box, is adapted to measurement of even minute flows of galvanic current.

The quotations are from *Abstracts of the Papers Printed in the Philosophical Transactions . . .*, 1837, *3*:173.

[21] "That hour, o'night's black arch the keystane." Robert Burns, *Tam o'Shanter*, stanza 7.

[22] In a slip of the pen, Henry originally wrote "magnetometer," clearly an error. In his published description of the battery (p. 219), the point in the letter is made using the correct term, as he does later in this paragraph.

[23] The text immediately preceding is in the margin of the first page of the original. From "Thus while one half . . ." to "and non insulation," the text is in the margin of the second page. The remainder, to the postscript, is in the third-page margin. Having given his initials at the very bottom of the page, Henry scribbled on in the margins in this sequence. The postscript, on the last page, we assume was added after these words about the battery.

[24] These experiments are described by Henry on p. 221 of "Contributions I: Battery":

> This apparatus readily furnishes the means of making comparative experiments on the difference produced by partial and perfect insulation. When no higher degree of intensity is required than that afforded by eight pairs of plates, perfect insulation is obtained by the eight separate troughs. In higher degrees of intensity the partitions in the troughs furnish the means of perfectly insulating forty-eight of the elements: this is effected by simply charging with acid every other cell in each of the troughs, and connecting the corresponding element by conductors, which pass over the intermediate elements without touching them: with this arrangement we have six cells in each trough separated from one another by a cell without acid, or in effect by a stratum of air. For comparison with these a set of troughs has been constructed without partitions.
>
> The want of perfect insulation is not very perceptible in the common experiments of the deflagration of large and perfect conductors; but where the decomposition of a liquid is attempted, or the battery required to act on a small or imperfect conductor, the loss of power is very great, the apparatus partially discharging itself through its own liquid, and the intensity at the poles does not increase with a short interruption of the current.
>
> There is also considerable loss on account

I find that it is very important in an electro magnetic experiment where two currents are required to act simultaneously not to draw them both from the same battery but in all cases to use two unconnected batteries even should one battery be equal in power to the two.[25]
P.S. You will find a drawing of the US for $5, enclosed Much obliged—a friend in need &c.[26]

of imperfect insulation even in the case of low intensity, and when the poles are connected by a perfect conductor. In one experiment with an arrangement of five pairs, and the poles united by a conductor composed of thirty strands of copper bell wire, each forty feet long, the loss was found to be at least one seventh, as measured by the quantity of zinc surface required to be immersed in order to produce the same magnetic effect.

[25] Henry reached this conclusion as a result of his experiments of September 9 and 10, 1834 (printed above as the "Record" entries for those dates). In attempting to determine the modulus of conduction, he discovered that the better conductor of two drew a disproportionate amount of current from a single battery, to the point of virtually discharging it. When he used two batteries, one for each conductor whose properties he was testing, Henry obviated this effect.

[26] We do not know what this transaction is all about.

TO JOHN TORREY

Torrey Papers, Library, New York Botanical Garden

Princeton Saturday
Morning Dec. 20th 1834

My Dear Sir

Since my receipt of your kind letter of the 13th[1] I have been unusually busy. Do not smile at the old excuse. I have been in Phila and of cours was obliged to do double duty on my return.

Besides this Prof. James Alexander[2] had the misfortune to lose his younger little son. The child died on monday after an illness of a few hours from a sudden attack of croup. The family are much grieved with the loss as the little fellow was a universal favourite—the name sake of his grandfather[3] and of the most interesting age of about 2 years and a half.

Owing to this melancholy event I have heard Prof. Alexanders recitations as well as my own.

[1] Not found. Torrey noted on the first sheet of this letter "Recd 25th. Ansd Dec. 29th." His reply of the 29th is printed below.

[2] James Waddel Alexander, for whom see above, Trustees' Minutes, April 9, 1834, footnote 10.

[3] Archibald Alexander, for whom see below, Henry's "List of Persons . . ." [ca. August 21,] 1835, footnote 40.