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of which I hope in a few weeks to send you an account from the transactions of the American Phil society.⁴

I hope you will find it to your own interest as I am sure it will be to that of science to continue publication of the memoirs and be assured that I will do all in my power to make the work known *<and cause it to be subscribed for in this country.>* and properly encouraged in this country.

I shall always retain a lively recollection of my visit to England and of particularly

It will be much to be regretted if you cannot find it to your interest to continue the publication of the scientific memoirs. There is no work more wanted and none which can have a more important influence on the cause of science in every part of the world where the English lang[uage] is spoken.

I do not receive the numbers of the Philosophical magazine quite as promptly as I could wish. My last number is one for March. I get it through O. Riche Red Lion Square.

⁴ Henry's "Contributions III: Electro-Dynamic Induction" was reprinted in the *Phil.*

Mag., 1840, 3d ser. 16:200-210, 254-265, 551-562.

TO ALEXANDER DALLAS BACHE¹

Bache Papers, Smithsonian Archives

Princeton Aug 9th 1838

My Dear Bache

I start tomorrow morning at 1½ past 3 o'clock to put on board the ship Toronto, the magnetic needles which I have vibrated in this country and which I hope will reach London in proper time to meet you there, just before you leave for America. I do not know when you have concluded to sail but suppose you will start just after the meeting of the British Association next month. I send the Package directed to the care of Mr Vaughan and will request the Capt of the Toronto² to deliver it with his own hands.

I have not made out the observations but send you an exact copy of the

¹ Although Henry wrote at least one other letter to Bache between the time he left Europe and the time Bache returned home, this is the only such letter extant. In this August 9 letter, closed hastily on August 10, Henry reflects on his experiences in Europe and informs Bache of developments at home. Two

partial copies by Mary Henry are in the Henry Papers. The August 9 portion is printed in Nathan Reingold, ed., *Science in Nineteenth-Century America: A Documentary History* (New York, 1964), pp. 81-90.

² Robert H. Griswold (*Henry Papers*, 3:514).

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records. The observations are made according to the old method. Since I was most accustomed to this I concluded there would [be] less danger of mistake. In the series of observations made in London I was assisted by Mr Robinson the Instrument maker of Devonshire street. We found no difficulty in identifying the place [of] observation as described in your letter.³ The observations in this country were made at Princeton with the assistance of Mr Alexander. The results with Rusty and Bright from your observations in London and from mine and Robi[n]son at the same place you will find do not well agree. One cause of the difference is the long exposure the needles were subject to in the interval. Another and probably the principal cause is that when we came to examine the needles one of which as you may recollect was fastened into the lining of my hat and the other into the tail of my coat the stirrups which were formed of tin foil were rubbed into pieces. We collected the fragments weighed them with accuracy and substituted an equal weight of fine platina wire. The compensation was not however complete since it was impossible to give the mass precisely the same distribution as in the case of the tin foil stirrup. You will find in the square box two additional needles made by Robinson. He neglected to mark these and the letters put on are by myself. There is a possibility that I have not given them the proper mark, that is the one called A perhaps should on the record be called B. You will however easily determine this by a comparison of the times. I did intend sending the needles by the last London packet but was not able to get to the ship in time. I however preferred to send them by the Capt of the Toronto since I came home with him and can put more confidence in his care of the articles. They will in all probability reach London at a proper time before your departure and with the least delay. So much for the needles and now for other matters. I have twice visited Philadelphia since I wrote to you⁴ concerning my visit to your Mother's. One was a short visit of a day or less the other was with Mrs H and consisted of 4 or 5 days. My time however was continually occupied with some experiments in Dr Hares laboratory on electricity with his large electrical machine and battery. This was at the First of May and since then I have heard nothing from the city. My time since my return to America has been constantly occupied and since the commencement of the last College year I have had scarcely a moments leisure. Not only my ordinary duties were to be attended to but also much was expected in the way of working up the lea way of my long absence. Besides this I commenced during the spring vacation a series of experiments on electricity which have engaged my attention at every moment not devoted to professional duties. I hope you will pardon me for

³ Of July 16, 1837 (*Henry Papers*, 3:410-412).

⁴ Letter not found.

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not having written to you frequently as I promised and as I really intended. I have commenced since my last several letters but did not finish them at the time—did not like to send a short letter, procrastinated from day to day, suffered much from stings of conscience on account of the same, consoled myself a little with the reflection that I had heard nothing from you &c &c.

I have now again got into my old track and things go on with me something after their usual fashion. When I first returned from England however I was much dissatisfied with many things both in reference to my own affairs and those of the public generally. An absence from a place for a single year makes many changes and you will probably find My Dear Bache that the two years you have been absent has made as great a change in the disposition of things which you once controlled as if you had been consigned to the grave during that period. Persons with whom you have associated and perhaps controlled have learned to do without you and measures and plans which you have laboured to advance are set aside for others not as good but carried into operation because the production of others and those on the spot. Besides this the charlatanism of our country struck me much more disagreeably when I first returned than before or even now. I often thought of the remark you were in the habit of making that we must put down quackery or quackery will put down science. You have probably heard of the wonderful sensation produced in the country by magnetic machines. A company was formed in New York which succeeded in raising \$12,500 dollars for experiments on the machine and after much puffing and the expenditure of the above mentioned sum the whole of course fell through.⁵

⁵ Henry is referring to a joint stock association set up to provide money for the development of Thomas Davenport's electric motor. Davenport patented his invention on February 25, 1837. A week later, he and his partner Ransom Cook, also a rural mechanic, entered into a financing arrangement at the suggestion of Edwin Williams (1797–1854, *DAB*). Retaining most of the stock themselves, they authorized Williams to sell the remaining 1,000 shares to raise \$50,000 to be applied toward experimental work and living expenses. Williams agreed to pay Davenport and Cook \$12,000 within thirty days of the agreement. The proprietors eventually received only half this amount and in March 1838 filed a bill of complaint in the New York Court of Chancery against Williams, alleging evasiveness and fraud in his agency. Williams subsequently went bankrupt.

Davenport's attempts at further development of his motor drew a variety of responses from the scientific community. In 1835 Henry

had praised Davenport's ingenuity but advised him to abandon his motor except as a curiosity for exhibit. Henry, and others, felt that the cost of producing power from a motor was far too high, in comparison with steam, to make it a viable alternative (*Henry Papers*, 2:416n–417n, 445–448, 452n). Charles G. Page, also working on motors at this time, regretted that the invention was "a subject of mercenary speculation, when in reality it has no value except as an experiment . . ." (*Silliman's Journal*, 1839, 35:107). Benjamin Silliman strongly endorsed further development and encouraged the public to support it regardless of potential financial reward (*ibid.*, 1838, 33:194). Thomas P. Jones, editor of the Franklin Institute *Journal*, was also an enthusiastic supporter and felt electric motor power would prove to be cheaper than steam power (*Franklin Institute Journal*, 1837, 20:342–343).

From 1837 until 1843, Davenport remained in New York City trying to improve his motors. He finally returned to Vermont, having failed

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Again a great sensation was produced by the magnetic Telegraph of Professor Morse who first claimed or his Friends for him the entire origin of the project.⁶ But the most disgusting piece of charlatanism which has been got up in this country since your departure is that of Dr Sherwood in connection with the committee of naval affairs in the senate of the United States. Dr S. brought before congress last session a great discovery in magnetism no less than that of the solution of the whole problem of terrestrial magnetism, a ridiculous and puerile affair. It was however referred to the committee of naval affairs who reported on it in the most flattering terms, stated that from the opinion of several scientific gentlemen as well as their own examination the discoveries and inventions of Dr Sherwood were of the highest importance, worthy the confidence of the public and the patronage of congress. They proposed to bring in a bill for the reward of Dr S. but fortunately for the honor of the country congress adjourned previous to this disgrace being inflicted on the country. I will attach to the package a copy of the report for your inspection. 5000 copies extra of the report were ordered printed for the edification of the people of the United States and of the world, for I have no doubt but many of these will find their way across the atlantic.

This article you will say is a disgrace to the country. I have given a notice of it and made a protest, in behalf of the scientific character of the United

in his attempts, including a request to Congress for a federal appropriation, to get adequate financial backing.

DAB, s.v. "Davenport." Walter R. Davenport, *Biography of Thomas Davenport* (Montpelier, 1929), esp. pp. 114-117. King, pp. 265, 269. A copy of the bill of complaint is in the Henry Library.

⁶ Although S. F. B. Morse had conceived of his telegraph in 1832 and been working on it since 1835, he did not begin demonstrating it until September 1837. In the next few months he greatly increased its effectiveness and gained widespread publicity and praise. Responding to a request from the Secretary of the Treasury, who had been asked by the House of Representatives to report on a telegraph system for the United States, Morse demonstrated his invention in February 1838 at the Capitol in Washington. Enthusiasm for Morse's electromagnetic telegraph eclipsed the rival semaphoric telegraph proposals. On behalf of the House Committee on Commerce, Chairman F. O. J. Smith strongly endorsed Morse's system in his report of April 6, 1838. He also claimed its originality:

This invention consists in the application, by mechanism, of galvanic electricity to tele-

graphic purposes, and is claimed by Professor Morse and his associates as original with them; being so, in fact, as the committee believe, letters-patent have been secured, under the authority of the United States, for the invention.

House Reports, 25th Congress, 2d Session, 1837-1838, No. 753, p. 1 (the report is reprinted in Thomas C. Cochran, ed., *The New American State Papers: Science and Technology* [Wilmington, Delaware, 1973], 8:52-61). Carleton Mabee, *The American Leonardo: A Life of Samuel F. B. Morse* (1943; reprint ed., New York, 1969), chapter 17.

In a climate of popular excitement, there was little inclination for an analysis of Morse's debt to basic research, the similarity between his system and those of foreign rivals, or the specific technical contributions of his associates. These questions were to arise later and plague Morse for the rest of his life. His famous dispute with Henry did not begin until the late 1840s. Prior to this, he and Henry were on friendly terms. Henry publicly endorsed Morse's telegraph system and willingly advised him on technical problems. See their correspondence in April and May 1839 (printed below).

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States, against the custom of publishing scientific articles among the documents of congress before their true character is ascertained.⁷ Bad as this is it is not quite equal to a memorial published among the documents early last winter on the subject of the explosion of steam boilers. The author set forth the wonderful fact that the explosions were generally produced by the generation of *negative* and *positive electricity* in the boiler!!⁸ I think you will be somewhat displeased with several of the selections for the Franklin Journal since your departure. Sillimans Journal in the last numbers is tolerably decent but contains little that is new or interesting but for the year before this it was filled with a mass of trash relative to electricity and electro magnetism which would disgrace the annals of electricity. One Fellow actually stated in a paper on electro-magnetism that he had succeeded in making the magnets by substituting copper instead of iron within the coil.⁹ I had an interview with Professor Silliman and complained to him of the injury done to the character of american science by such publications. He said that if I could see what he rejected I would scarcely complain of what he inserted.¹⁰ I also hinted the importance of having collaborators in

⁷ Henry's protest is printed above, July 1838.

A second Naval Committee report, essentially an expansion of Sherwood's memorial and not an evaluation, appeared on February 22, 1839. Referring to criticisms of the first report, the committee defended its actions:

They, however, deem the subject of sufficient importance to have the views of the memorialist extensively spread before the country, that they may thereby attract the attention of scientific men, and lead to investigation which will tend to establish the correctness of his theory.

Senate Documents, 25th Congress, 3d Session, 1838-1839, No. 253.

⁸ The January 1838 memorial was by Jonathan Morgan of Portland, Maine. Morgan argued that the causes of steam boiler explosions could not be determined through observation and experiment but only through analogy. Claiming that steam boiler explosions had the same effect, except in magnitude, as "electrical explosions in the heavens," he concluded that they shared a cause: "the sudden mutual and violent attraction of positive and negative electricity." Although citing the Franklin Institute experiments in support of his theory, he chided its committee for "going upon the mechanical instead of the philosophical principles of the case." One of Morgan's proposals was to equip steam boilers with modified lightning rods. *House Documents*,

25th Congress, 2d Session, 1837-1838, No. 100 (reprinted in Thomas C. Cochran, ed., *The New American State Papers: Science and Technology* [Wilmington, Delaware, 1973], 6:358-372).

⁹ In claiming a unique form of motor, Benjamin Rush McConnell mentioned that he got nearly equal results from electromagnets made of either copper or iron wrapped in iron wire (*Silliman's Journal*, 1838, 33:188-190).

The most sensational article on electricity appeared in the next number (pp. 394-398) under the title "Extraordinary Case of Electrical Excitement, with Preliminary Remarks by the Editor," communicated by Dr. Willard Hosford of New Hampshire at Silliman's request. Hosford described how a "lady of great respectability . . . became suddenly and unconsciously charged with electricity" during an aurora in January 1837. The lady's electrified state, manifested by her ability to give off sparks, lasted until mid-May. Hosford studied the effects of variation in her clothing and mood, atmospheric pressure, humidity, temperature, etc., finding that her greatest "electrical power" was at 80° F. Far from suggesting skepticism, Silliman found "the belief in the facts to be universal, particularly on the part of persons of judgment and science. . . ."

¹⁰ Henry recalled this conversation in a letter to G. P. Fisher after Silliman's death:

In reply to some remarks on an article of less scientific merit than the general stan-

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the different departments of science for a journal of the kind. The hint was not however taken & shortly after the name of the Professors son was attached to the Journal.¹¹ I have the highest personal regard for Professor Silliman and feel much attached to him for his kindness to me and his readiness to give me often much more than my proper share of credit. Still I am now more than ever of your opinion that the real working men in the way of science in this country should make common cause and endeavour by every proper means unitedly to raise our scientific character, to make science more respected at home, to increase the facilities of scientific investigations and the inducements to scientific labours. There is a disposition on the part of our government to advance the cause if this were properly directed. At present however Charlatanism is much more likely to meet with attention and reward than true unpretending merit.

But I fear I will tire you with this long story of ills and proposed remedy and I will now change the subject to that of the British association which you are about to attend. You have seen the report of the last meeting in the Atheneum and found honorable mention of your own name there.¹² My recollections of the meeting on the whole are pleasant. There were however some circumstances which were at the time not very agreeable and which I mentioned briefly in my last letter.¹³ The principal of these was a slight altercation I had with Dr Lardner relative to the speed of American steam boats. I had resolved to keep myself perfectly cool and not to put myself too much in advance. I concluded, on account of my character at home more than from a desire to show myself at the meeting, to give to the mechanical section a few words on the subject of the internal improvements in the

dard of the Journal, Professor Silliman once said to me,—“Could you see what I reject, and the amount of correspondence which such rejection involves, you would not be surprised that I should occasionally suffer an article to appear not strictly in accordance with my own views.”

George P. Fisher, *Life of Benjamin Silliman*, 2 vols. (New York, 1866), 2:333.

¹¹ On the title page of volume 34, July 1838. Benjamin Silliman, Jr. (1816–1885) had only recently graduated from Yale and was assisting his father in his laboratory and lectures. His later career as a chemist and geologist closely paralleled that of his father: as Professor of Chemistry at Yale, as the author of popular scientific textbooks, as a mining consultant, and as editor of *Silliman's Journal*. DSB. DAB.

In 1840 Asa Gray began collaborating with the Sillimans on botanical material; see below, Torrey to Henry, January 15, 1840, especially footnote 5.

¹² Bache did not attend the 1837 British Association meeting at Liverpool. As the result of an earlier conversation, however (*Henry Papers*, 3:238), Baden Powell began a communication on heat radiation by referring to Bache's work:

The object of this communication is to call the attention of the Section to the researches of Prof. Bache of Pennsylvania, which seem not to have been so fully appreciated in this country as they deserve.

Athenaeum, 1837, p. 745.

¹³ Henry's letter of October 1, 1837 (*Henry Papers*, 3:506–510).

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United States and to the Physical section an account of my researches on the lateral discharge. I had been requested by Mr Tanner of Philadelphia to present a map to the society containing all the lines of railways and canals in this country up to 1836. In presenting this map I occupied the time of the section about 6 or 7 minutes on some general remarks relative to the topography of the country, the extent of navigable rivers, canals and railways constructed &c. My remarks were listened to with much attention and received with great applause; but just as I was stepping from the platform a person among the audience arose and requested to ask me one question in reference to the speed of American steam boats. He said he wished authentic information. The chair man said the question was not strictly proper and I might answer it or not as I chose. It did not belong to the subject of my communication. I stated that I had no objection to give what Information I possessed on the subject—that I was not a practical engineer, although I had paid some little attention to steam navigation in the United States, that I had lived on the banks of the Hudson and had been in the habit of going up that river in the swift boats, and that I had gone the distance of 150 miles from N York to Albany up the river in the space of 9 hours but that there was a tide in the river of from perhaps 2 to 3 miles an hour at the time which assisted the passage. Dr Lardener then arose and stated that the section did not want popular information of the kind I had given and that he did not believe that a vessel ever passed through the water with the velocity I had stated. The chairman Dr Robinson the astronomer then interfered and made some very severe remarks on the turn the affair had taken and the impropriety of treating a Foreigner who had favoured the Association with a communication in such a manner &c &c. Fortunately I did not get in the least degree excited although the whole room was in a state of commotion. I next made some explanatory remarks and stated that in reference to science I did not consider myself a Foreigner, that truth and science was of no country and that I wished no more courtesey shown my communications than those made by the other members of the association. I next made some statements relative to the length breadth &c of our boats, quantity of fuel burned &c. I then left the room when Mr Russel of Edinburgh the wave man took up the cause and informed the section what I had done for science &c &c. The affair though very unplesant at the time and entirely unprovoked or unlooked for by me ended rather pleasantly than otherwise since during my stay in Liverpool I met constantly with marked attention and many persons introduced themselves to me and expressed their regret that I should have any cause to think that America and Americans were not highly esteemed in England and particularly in Liverpool.

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When my paper on electricity was presented at the Physical section the room was much crowded and the communication was received apparently with much interest.

My opinion however is that it is not in very good taste for a stranger to occupy much of the time of the section and had I not been urged to the affair and supported in it by the example of De La Rive and Leibig the chemist I would have hesitated to offer any thing.¹⁴ I however took good care to make my communication as short as possible, my drawings on the black board previously prepared and my lesson well covered. I was much pleased at the association to make the acquaintance of Professor Moll of Utrecht. He promised to write me and send a copy of an engraved portrait of himself but I have not yet received the letter or the article.¹⁵

Daniel and Faraday were there and on every occasion treated me with marked attention. Faraday was of great importance to me since he made it a point to come to me even through a crowd and introduce me generally to all around. These introductions I found had more weight than even Mr F. appeared to attach to them. I found it necessary to walk with much circumspection and can easily see how our Friend Dr H. of Philadelphia erred in his intercourse with the science of Great Britain.¹⁶ There is a great prejudice and perhaps in some respect a just one, (from the persons who have visited England) against Americans. They treat us with great kindness and I have no doubt but many of the persons with whom you and myself associated had a real respect for us and were prompted by proper feelings to show us the attention we received but still there was in my case the appearance of a little hesitation in allowing the same merit in public as in private. You may probably remark something of the same kind. It may arise from the in-

¹⁴ De La Rive presented two communications to the mathematical and physical science section of the British Association in 1837; Liebig presented one to the chemistry and mineralogy section. Both were, unlike Henry, corresponding members of the Association. *BAAS Report, 1837* (1838), part 2, pp. 10, 27, 38.

Liebig's communication, read by Faraday, was more than politely received. In his concluding remarks, he chided the host country for giving too little attention to organic chemistry. The section responded by asking Liebig to prepare a report on organic chemistry. *Athenaeum*, 1837, p. 748. For the relationship between this assignment and Liebig's *Organic Chemistry in Its Application to Agriculture and Physiology* (1840), see Margaret W. Rosister, *The Emergence of Agricultural Science: Justus Liebig and the Americans* (New Haven,

1975), p. 27.

¹⁵ Henry was apparently unaware that Moll had died in January. A later notation in his address book (p. [14], Box 17, Henry Papers, Smithsonian Archives) reads:

Promised to send me his picture but he died before the end of the year.

¹⁶ Assuming that the friend is Robert Hare, the exact nature of his errors is unclear. After a long conversation with him in February 1837, Henry reported that Hare "was highly pleased with his reception by Scientific men in England and France but remarked that he had attempted rather too much and thus were he to do the thing again particularly at the association he would bring forward only a few articles and have these well prepared." *Henry Papers*, 3:153.

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fluence of the aristocratical and political Institutions of the country and the general low opinion which is [?]entertained relative to American science and literature.

I think you will not be much impressed with the scientific character of the British association. There is such a mixture of display of ignorance and wisdom—of management in the compliments given and the honors received that the whole makes rather an unfavourable impression on a person admitted a little behind the scenes. I was at first a little at a loss to know what good was promoted by the meeting. A little reflection however showed me that the principal advantage of the Institution is the amount of money it gives to real working men for the prosecution of their respective branches.¹⁷ This money is in the hands of the committees and they are wisely composed only of those who have some reputation for science. The great body of the members have no voice in the management of the Institution and in this respect the society is quite as aristocratical as the government of the nation.

This arrangement however I am far from considering improper. On the contrary were it otherwise the *third* and *fourth* rate men would soon control the affair and render the whole abortive and ridiculous. Much has been said since my return of the propriety of a meeting of the kind among us,¹⁸ but I am convinced a promiscuous assembly of those who call themselves men of science in this country would only end in our disgrace. At the close of the meeting votes of thanks were given in some form or other to almost all the principal men connected with the association. The names of four Foreigners were mentioned namely Moll, De La Rive, Leibig & my own. Also a vote of thanks was given to our Minister Mr Stevenson¹⁹ who rendered himself very popular by a speech which he made at one of the dinners. I was pleased at this since when he first arrived I feared he would have made himself ridiculous with some remarks he was about to make at the Physical section on the subject of Light. I told him rather plainly that the subject did not belong to his line and that he knew nothing about it. At this he appeared somewhat offended. His speech however made proper amends; he left the association well pleased with himself and with the good opinion of all who heard him.

I informed you that I had visited Sir David Brewster, staid two days with him and was highly delighted with my visit.²⁰ He gave me a letter to Dr Sprague of Albany which contains some remarks relative to American

¹⁷ Henry made similar comments in an October 1837 letter to Charles Coquerel (*Henry Papers*, 3:511–512).

¹⁸ For one such proposal, see John Collins Warren to Henry, September 29, 1838, and

Henry's reply of October 16, both printed below.

¹⁹ Andrew Stevenson (*Henry Papers*, 3:212).

²⁰ See *Henry Papers*, 3:473–483, 490–492.

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Science and makes honorable mention of our names. Dr Sprague proposed to me to publish the letter²¹ but as I did not know how you would be pleased with the notice I thought it not worth while without some special reason to take this step. I found your Friend Professor Stevelly²² a very fine fellow and I am much indebted to him for his kind attention to me while at the association. We will however be able to form a more proper estimate of the different persons when we come to compare notes.

But to turn to another subject. Do you recollect the project we had of procuring for this country a set of the Elgin marbles.²³ On my return to England I again spoke to Mr Children on the subject and he gave me the directions how to proceed in the affair, to get some of the most influential persons of Philadelphia to join in an application and if possible to have this backed by the government. On my First visit to Philad^a after my return I called with Mr Rogers on Mr Biddle, informed him that while we were together in London we had been informed that the marbles could be obtained without much difficulty by a proper application &c. He appeared pleased with the affair and said he would give the matter proper attention. He however appeared to misunderstand the proposition for since it came from you he concluded that the copies were to be obtained for the Gerard College and said that he would place the matter before the board of trustees at the next meeting. I again called on him relative to the same affair but he was then much engaged and said that the gentleman sent to this country by the Bank of England²⁴ had promised to attend to the subject on his return to England and that he had seen a short time before an account that any person could procure copies by merely paying for the expense of the work.

²¹ Not found.

²² John Stevelly (ca. 1795–ca. 1867) was Professor of Natural Philosophy at the Belfast Royal Academical Institution from 1823 to 1849. A native of Cork, he received a B.A. from Trinity College, Dublin, in 1817. From 1849 until his resignation in 1867, he was Professor of Natural Philosophy at Queen's College, Belfast. Queen's College historians describe him as a "veteran popularizer of science" and a "man of wide learning." The *Royal Society Catalogue* lists nine articles by Stevelly on mathematics and meteorology. Theodore William Moody and J. C. Beckett, *Queen's, Belfast, 1845–1949*, 2 vols. (London, 1959), 1:117. *Queen's College Calendar, 1882–1883* (Belfast, 1882), p. cxc. John Jamieson, *The History of the Royal Academical Institution, 1810–1960* (Belfast, 1959), pp. 52, 64. George Dames Burtchaell and Thomas Ulick

Sadleir, eds., *Alumni Dublinenses: A Register of the Students, Graduates, Professors and Provosts of Trinity College in the University of Dublin (1593–1860)* (Dublin, 1935), p. 780.

At the 1837 British Association meeting, Stevelly was a secretary of the mathematical and physical science section. The *Athenaeum* reports of the meeting record many comments by him, including some on Henry's lateral discharge presentation (*Athenaeum*, 1837, p. 717).

²³ See *Henry Papers*, 3: 530–532.

²⁴ Two agents of the Bank of England were in Philadelphia to protect the Bank's loan of £6,000,000 to American banks in England by collecting debts owed to the American firms. W. Marston Acres, *The Bank of England from Within, 1694–1900*, 2 vols. (London, 1931), 2:465–466.

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Here the affair ended and I have heard nothing more in reference to it. Will it not be well for you to make some further inquiries relative to the expense of procuring, the mode, &c in order that something may yet be done to procure a set for this country.

While I was in London my Friend Henry James wished to procure something as a present to the Albany Institute. I advised him to get a copy of the Rosetta Stone from the Museum and gave him a letter to Mr Gray²⁵ with the request that he would assist my Friend to procure the article. The stone was copied, the whole cost was only 5 dollars and the Institute was delighted with the present²⁶ & the papers from Georgia to Maine proclaimed the generous deed.

Our new house is just finished and we are about to enter it or at least hope to move in a few days. The college is still in a flourishing state. The University of Pennsylvania is also in tolerably good condition. Dr Ludlow²⁷ I think is increasing in popularity and gradually extending his influence. Your successor²⁸ appears to be a very kind and worthy man but exceedingly queer in his manner. He is said not to be very successful as an experimenter in the way of Chemistry and Natural Philos before his class. Professor Vethake has lately published a work on political economy. He comes out against the Banking System of the country and is in favour of protecting literary as well as other property.²⁹ I have heard nothing from Dr Lock since my return except a communication of his in Silliman and another to the American Phil Society on the dip variation &c.³⁰ Saxton has a good situation in the mint. He spent a few days with me in the Spring at Princeton. We had a very pleasant time in experimenting. Professor Daubeny started for Europe on

²⁵ John Edward Gray (*Henry Papers*, 3:229). We have not found Henry's letter.

²⁶ The copy of the Rosetta stone from the original in the British Museum is no longer in the Albany Institute.

²⁷ John R. Ludlow, Provost of the University of Pennsylvania (*Henry Papers*, 1:106n).

²⁸ Roswell Park (*Henry Papers*, 3:87).

²⁹ For earlier comments on Vethake's book, see Henry's letter to Thomas Hun of March 5, 1838, above. Vethake considered his attempt to analyze "immaterial or intellectual products" in addition to material property a "bold innovation" (p. vii). He did not specifically address the question of international copyright, something which Henry endorsed and which American authors were beginning to advocate. Henry's Library contains a copy of *A Plea for Authors, and the Rights of Literary Property*. By an American (New York, 1838).

The author illustrated how the U.S. copyright laws, which extended protection only to American authors, encouraged the pirating of foreign works; American authors were driven out of the market by the necessarily higher cost of publishing their works.

³⁰ John Locke's letters to Silliman of January 28 and February 10, 1838, appeared as "Prof. Locke on Magneto-Electricity, and Electro-magnetical Machines," and "Additional Remarks, by Prof. Locke, on Electricity Produced by Motion, and on Motion produced by Magnetic Electricity," *Silliman's Journal*, 1838, 34:125-130, 130-132. "On the Magnetic Dip at Several Places in the State of Ohio, and on the Relative Horizontal Intensities of Cincinnati and London," read to the American Philosophical Society on June 15, 1838, appeared in *APS Transactions*, 1839, n.s. 6:267-273.

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the 8th of July. I did not have the pleasure of meeting with him while he was in the country.³¹ Dr Torrey is much engaged in preparing a general Flora of the United States. One volume of the work is now in the Press.

The scientific expedition it is said will sail in the course of a few weeks. It is now under the command of Lieut. Wilks. Johnson, Coats³² and several others have been dismissed in the process of cutting down the number of scientific corps.

You have probably learned by the papers that this summer has been one of the warmest for many years. The thermometer at Princeton has stood at 100° in shade several times. Pixii has treated me in a very shameful manner. I purchased of him a large collection of articles which I paid for in advance. These he promised to send to New York so that they would reach the Custom house about the time I might be expected to arrive. I waited very impatiently for them all winter and have only received them within about 6 weeks of the present time. When I came to examine the invoice I find that he has still not finished a part of the order & I know not when he will send the remainder. Also when the chemicals bought of Robiquet³³ &c were unpacked there was a deficiency of articles to nearly the amount of 200 Francs. So much for the honesty of the French.

New York Aug 10th 1838

Dear Bache

In order to be sure not to miss the stage I did not go to bed last night. I left home at 1/2 Past 3 and have just arrived in this city. The ship sails in about an hour. I scribbled nearly all night but have not had time to read what I have written. My best respects to Mrs Bache and your sister.

All your Friends will be rejoiced to see you home again and none more than

Yours as ever
Joseph Henry

P.S. I will direct this letter to the care of Mr Vaughan.³⁴ I have also written one to him.³⁵

My other letter was to Huttenger³⁶ and Co Bankers Paris.

³¹ For Daubeny's visit to the United States, see *Henry Papers*, 3:517-518.

³² Reynell Coates, a Philadelphia physician, was one of the less qualified members of the scientific corps. By his own admission, his appointment as one of the zoologists was due to political influence and not ability. William Stanton, *The Great United States Exploring Expedition of 1838-1842* (Berkeley, 1975), p. 47. W. J. Snape, "Reynell Coates (1802-1886): Politician, Poet, Editor, Naturalist, Lecturer

and Physician," *Transactions of the College of Physicians of Philadelphia*, 1968, 35:112-118.

³³ Pierre-Jean Robiquet (1780-1840, *DSB*), a manufacturer and retailer of chemicals as well as a research chemist. Henry had bought supplies for J. R. Chilton.

³⁴ The letter is addressed to Bache in care of Petty Vaughan.

³⁵ Not found.

³⁶ i.e., Hottinguer.