

A Case of "Gray Plagiarism" from the History of the History of Computing

Michael Davis

E-mail: davism@iit.edu

Abstract

Claiming as one's own what one knows to be the discovery of another is certainly plagiarism. But what about merely failing to acknowledge the work of another where one does not give the impression that the discovery is one's own? Does it matter how easy it was to make the discovery? This paper analyzes a case in this gray area in academic ethics. The focus is not on the failure to attribute itself but on the attempt of an independent scholar who, believing himself to be the victim of "gray plagiarism", sought a forum in which to make his complaint. The story could be told from several perspectives. I shall tell it primarily from the perspective of the complainant, an outsider, because I believe that way of telling it best reveals the need to think more deeply about how we (acting for the universities to which we belong) assign credit, especially to scholars outside, and about how we respond when someone complains of a failure to assign credit. My purpose is not to indict individuals but to change a system. This paper updates a case I first described in 1993.

"Am I a dog, that thou comest to me with staves?" — I Samuel 17: 43 (Goliath's last words)

Introduction

Claiming as one's own what one knows to be the discovery, idea, or writing of another is certainly plagiarism. But what about merely failing to acknowledge the work of another where one does not give the impression that the discovery is one's own? Does it matter how one came upon the knowledge in question, whether in a book, as a referee for a journal, or from private correspondence? Does it matter how easy it was to make

the discovery? This is a gray area in academic ethics.

While I shall be concerned with this gray area here, I shall not try to provide a definitive answer to the questions just posed. Instead, I shall describe what happened when an independent scholar, believing himself to be the victim of "gray plagiarism", sought a forum in which to make his complaint. The story could be told from several perspectives. I shall tell it primarily from the perspective of the complainant, an outsider, because I believe that way of telling it best reveals the need to think more deeply about how we (acting for the universities to which we belong) assign credit, especially to scholars outside, and about how we respond when someone complains of a failure to assign credit. My purpose is not to indict individuals but to change a system. That was also my purpose when I first published a description of this case in 1993. Though much about the way universities handle "plagiarism" has changed since then, and though I have also learned much about the case since then, the problem described has, I think, not changed much—as I shall try to show as I bring the case up to date.¹

The Discovery

The case began with a footnote in the history of mathematics. The July 1983 issue of *Annals of the History of Computing* carried an article entitled "Babbage's Letter to Quetelet, May 1835". At the article's heart was a modern translation into Eng-

lish of a much older French translation from English. The French translation, printed in 1835 in the *Bulletin of the Royal Academy of Science, Letters, Fine Arts of Belgium*, is significant because it contains the first mention in print of Babbage's Analytical Engine, a precursor of today's computer.² The Babbage in question is Charles Babbage who—as author of *Reflections on the Decline of Science in England*—was also an early critic of misconduct in science.

The introduction to the article in the *Annals* noted: "The exact date of the letter is not clear, and the original is not known to exist." The article's author, Alfred W. Van Sinderen, was a long-time collector of Babbage's manuscripts, of his published works, and of works about him. Though Van Sinderen earned his living as chief executive of the Southern New England Telephone Company, his judgments carried weight with Babbage scholars. Even in the 1980s, the history of computing was still a pocket of scholarship that 19th century "gentlemen scholars" would recognize, a domain in which amateur and professional mixed on more or less equal terms. The history of computing is not unique in this respect. There are similar pockets in astronomy, archeology, numismatics, and so on.

Herman Berg, the chief character of our story, shared Van Sinderen's interest in Babbage. Without a college degree, Berg corresponded with many academics, museums, and libraries concerning common research interests. He also studied mathematics and foreign languages. During the academic year 1971-72, he was studying Japanese at the University of Kansas. There, at a meeting of the Scuba Club, he met an accountant from Kansas City who, hearing he lived in Detroit and was interested in computers, urged him to look up a brother-in-law, "Buzz" (Bernard) Galler, at the University of Michigan's Computing Center in Ann Arbor (an hour from Detroit). Berg looked up Galler during the winter break. Galler gave him "the grand tour" and urged him to come to Ann Arbor as a student. While indi-

cating an interest, Berg pointed out that he had another year's commitment to Japanese at Kansas. They would not meet again for a decade.

In August 1983, Berg got permission to sit in on a course in software engineering at the University of Michigan. The course was to be team taught. Though it was Galler's teammate who granted the permission (on condition that Berg explain a software project to be assigned), this was the class that reintroduced Berg to Galler. By then, Galler was Editor-in-Chief of the *Annals*.

While Galler was not often in class, he did see much of Berg's presentation of a topological sorting algorithm. After class a few days later, they had a long conversation. Berg told Galler of his life during the intervening decade, including studies at the University of Wisconsin, and of his interests in the history of mathematics. Berg was then working his way through the *Proceedings of the 4th International Statistical Congress, London, 1860*. Like the reading of Babbage's letter to the Belgium Royal Academy in 1835, this congress was an important event in the early history of computing. The Scheutz computing machine (a realization of Babbage's idea) was in use at the General Registry. Babbage invited congress attendees, including Florence Nightingale, to see it work. Quetelet was there too.

Seeing how excited Berg was about the *Proceedings*, Galler told him something of the internal workings of the *Annals*, including the history of Van Sinderen's recently published article, and gave Berg a copy, suggesting he see whether he could do what the *Annals'* reviewers had failed to do.

Why did Galler do that? Berg offers this explanation (based on what Galler told him at the time): Both reviewers had wanted to delay recommending publication until sure the original letter could not be found because, if it could be found, the translation would be unnecessary (and therefore not worth publishing). Neither

reviewer was quick to give up the search. Meanwhile, Van Sinderen had complained to Galler about the time it was taking the journal to make a decision. Galler eventually forced the reviewers to decide, leaving both them and him not quite satisfied. Apparently, Galler saw Berg as an opportunity to put to rest remaining doubts about publishing Van Sinderen's paper.

Early in January 1984, Berg began his research. After half-a-dozen Friday visits to the University of Michigan Graduate Library, searching paper catalogues, and after following many leads that led to dead ends, Berg realized that Van Sinderen had missed some holdings of Babbage correspondence in Brussels. Using an early online index (key entry by a UM librarian), Berg was led from Babbage to a file of Quetelet's correspondence (which, the printout said, was in the Archives of the Royal Academy).³ Berg wrote the Academy for a copy of the file on 22 February. By 26 March 1984, he had before him a photocopy of the missing English version of the letter in Babbage's own hand and dated 27 April 1835.

Complications

Pleased with what he had found, Berg phoned Galler the next day (while in Ann Arbor on other business). Berg expected warm congratulations. He got something else. As Berg remembers their talk, Galler almost immediately changed the subject to a letter Berg had written to Van Sinderen the month before. It was, Berg recalls, a long letter in which he praised Van Sinderen's translation, explained how he came to examine Van Sinderen's paper, and told Van Sinderen about some sources he had discovered. Berg also mentioned the delay in publication, sketched what he knew, and concluded that Van Sinderen was owed an apology (which, apparently, Van Sinderen took as an apology).

Galler said Van Sinderen had "laid it in to him" for telling tales out of class. Why had Berg, of all

people, been offering an apology for something the *Annals* had done? Galler seemed to view the letter both as a breach of confidence and as hurting Berg's relationship with Van Sinderen. Galler's tone was severe: Berg had no business offering an apology to Van Sinderen, no business repeating what Galler told him about the workings of the *Annals*.

Badly shaken by this exchange, Berg tried to repair the damage. As soon as he had hung up the phone, he sent Galler a copy of the Babbage letter (through campus mail), hoping that seeing the document might help Galler calm down. Berg then went to the office of the University's Vice President for Academic Affairs, looking for an explanation of what he had done wrong and advice about what to do next. A secretary made an appointment for him with Robert Holbrook, an economist then serving as Associate Vice President for Academic Affairs (and as member of the University's Joint Task Force on Integrity in Scholarship). The appointment was for a few days later.

When they met at the appointed time, Holbrook treated Berg cordially, heard him out, and then declared that Berg's ignorance should excuse the breach of editorial confidentiality. And, he added, Berg's discovery was in any case significant enough to outweigh such a small sin. Berg left with the impression that Holbrook might "straighten Galler out".

Berg also wrote letters of apology to Van Sinderen and to the two outside reviewers (whom he had referred to by name). Berg's letter to Van Sinderen seems to have worked. In a letter dated 25 June 1984, Van Sinderen thanked Berg for "your 'peace offering'", adding that it was "not really necessary, as I always have positive thoughts about people who are interested in Charles Babbage."

Soon after mailing these letters, Berg dropped by Galler's office. This visit went no better than

the phone conversation. Galler tried to convince Berg that the discovery was not important enough to warrant publication in the *Annals*, certainly not worth a note—no, not even a letter to the editor to correct the historical record. After all, Galler argued, the letter differed in only small ways from the translation Van Sinderen had made from the French. The differences were not important to the later history of computing.

Berg could not understand Galler's response. Had Berg not found the lost "ur-letter" of computing? Had he not shown that it still existed, dated it, and provided the full text? Had he not done what Galler's reviewers, two senior scholars in the field, tried unsuccessfully to do for more than a year? Until his discovery, who could say how long the original letter was or how well Quetelet had translated it? Scholars would hereafter know that Quetelet had omitted three paragraphs at the beginning and two at the end (and what those paragraphs said). They would have Babbage's exact words. If Van Sinderen's now-unnecessary translation had been worth publishing, why not Berg's original?

These questions led to another. Programs in the history of science were still rare in the 1980s; programs in the history of mathematics, rarer still. The University of Michigan had neither. Galler's own background was in mathematics (Ph.D., University of Chicago), not history of any sort. His work was far from the literary, scientific, or industrial "archaeology" to which Berg's discovery belongs. Could it be that Galler's editorial judgment in this area was unreliable? To answer that question, Berg wrote to others in the field describing his difficulties with Galler and asking their opinion of his discovery.⁴

Galler soon heard of these letters. On 25 April 1984, he wrote Berg asking him to come in to "discuss some of the letters you have written". They met in May. The tone of this meeting was different from the one before. While urging Berg to stop writing "those letters", Galler no longer

dismissed Berg's discovery altogether. Instead, he urged Berg to do "more" with the Babbage letter. Berg mentioned a number of archives he could check. The meeting ended.

Berg left dissatisfied. The "more" Galler was asking seemed more or less what Van Sinderen had already done. Berg might turn up something new (as he had just done). But, without a clear idea of what he was looking for, he was unlikely to beat Van Sinderen a second time at what he did best. What was more likely was that Berg would simply waste time. Then one of two possibilities might be realized: either Van Sinderen's false claim would remain in print unchallenged, or another scholar would do what Berg had done. If that other scholar made the same discovery independently and published it, Berg would get no credit for what he had done. (In history, as in science, credit goes entirely to the first to publish a discovery, not the first to make it.) Berg therefore felt he could not just do as Galler asked (though he did try to do that too, keeping Galler informed of mostly unsuccessful efforts to get access to various archives).

About this time, the University of Michigan issued its first *Guidelines for Maintaining Academic Integrity*. (This was the work of the Joint Task Force of which Holbrook was a member.) The *Guidelines* included advice on maintaining priority for a discovery when publication has been blocked. Berg used the *Guidelines* as a checklist. So, for example, he donated a copy of the Babbage letter (and related documents) to the University of Michigan Library (which the Library duly acknowledged on 14 May 1984). He also wrote anyone active in the field whom he had not already told, sending each an "unprint" (that is, a copy of the original Babbage letter, a brief summary of what Berg had done, and a copy of Van Sinderen's article).

Much of this must have made Galler unhappy. As Berg recalls their next meeting (early July 1984), Galler told Berg he had been receiving

phone calls advising him to publish Berg's discovery as a letter. As Galler recalled the meeting (letter of 10 August), he told Berg he would continue to help with Berg's history activities *provided* Berg "dropped the extraneous correspondence dealing with personalities and past events which were really none of your business". If Berg did not drop the correspondence, Galler "would have nothing further to do with [Berg]".

Berg did not do as Galler asked. For example, on 24 July he wrote the History of Science Society, sending them a copy of the Babbage letter and—by way of explanation—stating:

Dr. Bernard Galler is in no hurry to publish it even as a letter to the editor announcement to correct the historical record. I find it difficult to separate the mind games he has been playing with me from his editorial judgment. Dr. Galler has backed off from a position giving me no credit to allowing me to publish at some later date when I have an unspecified 'more'. Feeling initially blocked by him I sent copies to all of those I was aware of [being] actively involved in Charles Babbage studies. Thus, even if I was never published, I would have in some form fulfilled a scholarly obligation to communicate my results to others. Currently, it seems like Dr. Galler is still dissembling with me as he scrambles to cover himself with his reviewers and editorial board members.⁵

These letters did not always have the effect Berg intended. For example, Van Sinderen responded to Berg's letter of 30 July with a two-and-a-half page synopsis of their correspondence. While he ended by urging Berg to forget the past, he clearly was upset that Berg should "write me again, page after page of concerns and speculations about who did what to whom containing, among other things, unfounded suspi-

cions that it was [one of the two reviewers], a close friend of mine, who delayed publication of my article in the *Annals*."

On 10 August, after receiving a copy of Van Sinderen's letter to Berg (dated 31 July), Galler wrote Berg again: "[You] did not take my advice [but] continued to participate in the kind of activity which can only be destructive to your relationships with other historians." Therefore (with "great reluctance"), he had to "terminate" his relationship with Berg. Galler did, however, add that Berg could continue to submit work to the *Annals*. Any submissions would be sent out to reviewers in the usual way: "There will be no bias against you."

This letter ended their relationship. Doubting Galler would treat him better than he already had, Berg submitted nothing to him again. Until Galler retired as Editor-in-Chief in 1987, Berg's contact with the *Annals* only concerned other matters and these contacts were always with other editors. Berg's two-page note on the missing letter did not appear in the *Annals* until January 1992—five years after Galler's departure.⁶ The crucial event in our story occurred during the years between.

Plagiarism?

In 1989, New York University Press published *The Works of Charles Babbage* in eleven volumes. Volume 3 contains, among other things, Babbage's papers on the Analytical Engine. Pages 12-14 reprint (in English translation) minutes of the general meeting of the Belgian Royal Academy of Science in which Quetelet read the letter from Babbage (and in which Babbage then stepped forward to speak briefly on another topic, Sir John Herschel's plans for a meteorological study). An asterisk beside the title signaled a footnote. The footnote began, "This article is an English version (not strictly a translation) of [the famous 1835 letter] which immediately precedes

it [in its French version] in this volume." After giving credit to Lovelace's partial translation (1843) and to Van Sinderen's complete translation, the editor indicates that

[in] preparing this English version...use has been made of a letter from Babbage to Quetelet, preserved in the Quetelet Collection in the Bibliothèque Royal de Belgique, Brussels. This letter, which is written in English and dated 27 April 1835, is believed to be the same that Quetelet read to the general meeting of the Academie, 7-8 May, 1835. The text of this letter has been used, lightly edited for readability, in the version below. Van Sinderen's translation has been used for the French text which did not form part of Babbage's letter.

The note gives no credit to anyone for finding the missing original. It just says that the letter is in the Royal Library. Van Sinderen receives two mentions for his now-unnecessary re-translation; Berg, nothing for finding the original.

Berg first read this note early in December 1889. The more he looked at it, the more disturbing he found it. There was, first, a shortening of the title of Van Sinderen's article. The date, and only the date ("of May, 1835") had been omitted from the reference (replaced by the usual mark of elision). Had the date not been omitted, Berg thought, it would have been obvious that Van Sinderen did not know of the letter's actual date (now indicated in print for the first time). Was the editor trying to hide the irrelevance of Van Sinderen's retranslation?⁷ Second, there was that reference to the "*Bibliothèque* Royal". The Royal Library had, Berg believed, transferred its Quetelet collection to the Royal Academy several decades before. The scholar who found the letter on his own should not have made that error.⁸

Last, as far as Berg could see, the letter was (except for light editing and the omission of the first three and last two paragraphs) the one he

had discovered. Only three of the eleven paragraphs Berg had before him—that is, the minute's one introductory paragraph and two concluding paragraphs—were independent of his discovery. If there was any reason to credit either the French translation or Van Sinderen's retranslation, was there not more reason to credit Berg for finding the original? The original preempted all translations. Berg also noted that the *Works* gave no indication that it had received permission to publish the letter. There was, Berg thought, a good reason to get the appropriate archive's permission: scholarly custom. Berg had sought, and received, that permission—which was granted on condition that the Academy receive proper credit in print. Apparently, the *Works of Babbage* had not received a similar letter of permission.

Berg held in his hand what was likely to be for decades the definitive edition of Babbage's work, opened to the page supposedly containing the text of the most famous letter in the history of computing. Yet, what that footnote told readers is that they had before them neither the original letter Berg had found nor Van Sinderen's translation of the French version of Quetelet, but something new, a mix of the two "edited for readability". The rest of the original, though available for inclusion, was omitted. What could explain this?

The explanation could not be that the editors had confined themselves to previously published work. They did not claim to have such a policy. And, in fact, they had not followed such a policy. They had, for example, included what seemed to be a previously unpublished "Statement to the Duke of Wellington". Berg supposed the worst. Someone was trying to slip by without recognizing his contribution to Babbage scholarship.

Martin Campbell-Kelly, of Warwick University, England, was the Editor-in-Chief of the *Works*. He was also a member of the editorial board of the *Annals*.⁹ But he was not (Berg supposed) personally responsible for the footnote.

An editorial undertaking on the scale of the *Works* requires considerable delegation. There were four "consulting editors". One of these, Allan Bromley, University of Sydney, Australia, seemed to take responsibility for the part of Volume 3 relevant here.¹⁰ Not coincidentally, he was one of those to whom Berg had announced his discovery. Indeed, Bromley had written a friendly ("Dear Herman") note of acknowledgement (19 June 1984):

Thank you for your letter of 22 May and the information enclosed. I was particularly interested to read Babbage's letter to Quetelet, especially the comment "but it will take many months to work out all the details". How true that proved to be!....¹¹

There could be, Berg thought, no doubt that, since Bromley was responsible for that section, he had used Berg's research without giving credit.¹² What could Berg do?

Search for a Forum

Berg could see no point in writing Bromley. What could he write someone he believed guilty of plagiarism? What could such a letter accomplish? Nothing, perhaps, but writing Bromley might have been a good idea anyway. Consider what writing others accomplished:

On 4 January 1990, Berg wrote the Australian Academy of Science (AAS), providing relevant documents and asking for an investigation. The AAS responded within two weeks: "The matter to which you refer is exceedingly complex, but this Academy has neither the facilities nor indeed the ability to investigate the possible misdeeds of scientists working in this country..." Berg then wrote the chancellor of the University of Sydney, Bromley's home institution. He received no response, not even a courtesy acknowledgement of receipt. After what he considered a decent inter-

val, Berg wrote Australia's Prime Minister. The Attorney-General's Department responded on 12 October. Treating Berg's letter as an inquiry concerning copyright infringement, an Assistant Secretary for International Trade and Law said the government could do nothing because Berg neither held nor claimed a copyright in his discovery.

Well before this, Berg had realized that "blowing the whistle" was not going to be as easy as he had thought.¹³ He had therefore begun to make complaints wherever it seemed appropriate. He wrote Campbell-Kelly's home institution, Warwick University. When he received no response there, he wrote the British Prime Minister who (6 September 1990) referred Berg's complaint to her Attorney-General. A year later, one Detective Inspector Smith wrote Berg that Scotland Yard had examined the materials Berg sent and could find no evidence of criminal wrongdoing. A month after that the British Patent Office wrote that "there is nothing we can add to Inspector Smith's letter."

Berg also wrote the *Works'* publisher, New York University Press. When he received no response, he wrote New York University itself. Again, no response. Since Pickering and Chatto published the British edition of the *Works*, Berg wrote them as well. On 30 March 1990, one of their directors wrote back, thanking Berg for his letter, adding "I am truly sorry you feel aggrieved over the Babbage letter, but I am afraid that there is nothing I can do about it. If you wish to pursue the matter, I suggest you correspond with Dr. Campbell-Kelly." Though Berg did not then know it, one of his letters, a complaint addressed to Warwick's Office of the Chancellor (29 December 1989) had already reached Campbell-Kelly and another, the one addressed to the British publishers of the *Works*, would do so in May 1990. Not only did Campbell-Kelly not answer either of these, he also failed to answer when Berg wrote him directly (9 May 1992) warning that I, a specialist in ethics, was soon to publish a

paper concerning the complaint.¹⁴

Berg also sought the help of the Government of the United States, writing the Office of Science and Technology Policy (7 January 1990). They advised him (8 February) that because they lacked the "capacity to investigate individual scientific cases," he should take his complaint to "relevant research institutions and sponsoring federal agencies". Berg thereupon began writing every federal agency that might be "relevant": the National Science Foundation (17 February 1990), Department of Health and Human Services (21 February 1990), Department of Labor (15 March 1990), Office of Government Ethics (16 July 1990), United States Information Service [no date], Department of Education (5 October 1990), the Postal Service [no date], Department of Justice (26 September 1991), and Commission of Customs (10 January 1992). The dates indicate the first date he wrote. He wrote to some of these agencies several times. All responded each time he wrote. But, by 1992, Berg was receiving responses like this one from NSF:

We have received your letter of June 19, 1992. This and any future letters, on matters we have already addressed, will not be answered because to do so will waste valuable resources.

Berg even wrote the King of Belgium who passed the complaint to his Cabinet Chief. That exalted officer wrote Berg (3 September 1991) to confirm the "can't-do-anything" letter that the Prime Minister's Diplomatic Advisor had sent a few weeks before. No one in Belgium seemed to care that a document in the Royal Academy had been published without permission or proper credit.

This list of addressees, though interesting in its own way, is worth our time primarily because of what it tells about Berg's search for a forum.

Whatever we think of the complaint itself, we must agree that Berg's search for an avenue of redress was reasonably thorough. So, to dissolve any suspicion that he might have gotten a hearing had he done a little more, I must report much more of what he did (though far from all). Berg sought the help not only of the appropriate universities, the *Works'* publisher, and several national governments, but of some lesser governments, politicians, professional societies, and even the news media.

Because the *Works'* publisher was located in New York City, Berg wrote New York City's Police Department, the District Attorney of the County of New York, and the Citizen's Action Center (an arm of the City's Office of the Comptroller). Each of these thought his complaint lay in someone else's jurisdiction. The State's Department of Law referred him to the State's Department of Education (20 August 1991). By then, Berg had exchanged several letters with an Assistant Commissioner for Higher Education Services in New York. That administrator eventually suggested (20 May 1991) that Berg write the editors of the *Works* and, if they did not respond satisfactorily, "consult a lawyer to see whether your claim has any legal standing." Michigan's Attorney General gave much the same advice (25 November 1991).

The Center for Law in the Public Interest referred Berg to the law firm of Hall & Phillips (Los Angeles). The opinion of Hall & Phillips was that "while you have a legitimate grievance, this is not the type of grievance we handle" (24 April 1992). They did not say who did.

Politicians showed no interest whatever. The White House referred him to the Office of Government Ethics (30 October 1990). That Office had already referred him to the Designated Agency Ethics Official in the Department of Education (2 August 1990). The Office of the Vice President simply thanked him for writing (10 December 1990). Congressman Dingell, known

for his hearings on fraud in science and protection of whistleblowers, declined to help Congressman Conyers' constituent, sending Berg's letter on to him (8 November 1991). On 15 April 1992, Congressman Conyers wrote, "This is an issue that needs to be worked out at universities and other educational institutions." Bill Clinton, then a candidate for President, wrote that he had "turned your letter over to my staff for review and study" (27 November 1991). That was the last Berg heard from him (except for a letter of 5 August encouraging Berg "to work for the changes you want in America"). Berg heard nothing at all from any of the other candidates competing in the 1992 presidential primaries. The President's Office of Management and Budget sent Berg's complaint to the Inspector General at the Department of Education (2 December 1991).

Back in England, Mr. Kinnoek, Leader of the Opposition, suggested Berg apply to the Patent Office or New Scotland Yard (9 January 1992). Berg had already done both. The obvious (and not so obvious) venues of remedy still open were becoming few indeed.

The professional societies might seem a more likely venue than politicians. In fact, they proved no more helpful. Some came out sounding much like the politicians. For example, both the American Association for the Advancement of Science (28 December 1989) and the National Academy of Sciences (29 December 1989) pleaded lack of resources to investigate such a charge in what sound like form letters. (But each also pointed out work it had done to raise ethical standards in science.) The Phi Beta Kappa Society's rejection (18 January 1990) at least sounds like the work of an individual. Noting the society had "nothing to do with your difficulty", the letter concluded it would be inappropriate to get involved now. The letter closed by suggesting that Berg write the American Historical Association's newsletter *Perspectives* (18 January 1990). Berg followed that suggestion and soon learned that the American Historical Association "[did] not feel that it can

intervene in this situation and is unable to advise you regarding other courses of action" (7 May 1990).¹⁵

Among the professional societies responding in this way were the New York Academy of Sciences (30 May 1990), the Mathematical Association of America (13 November 1991), the (British) Institution of Electrical Engineers (26 November 1991), and the Computer Society of the Institute of Electrical and Electronic Engineers (January 1992).

While most professional societies gave non-committal responses, a few offered comfort (without offering help). The Secretary of the Industrial Mathematics Society responded (18 January 1990): "Your bull dog tenacity is to be admired and, like one person wrote, 'You'll have difficulties because you don't have the important three letters after your name: Ph.D.'" The (British) Royal Society for the Encouragement of Arts, Manufactures & Commerce (or, rather, the editor of its journal) wrote (9 March 1990):

I was most interested to receive your letter and documentation about your discovery of the Babbage letter. It is extremely sad as well as wrong for discoveries not to be correctly attributed. In case it is any help I am forwarding copies of your material to our reviewer because of his special interest in Babbage. I am sorry I cannot do more.

The Chairman of the International Commission on the History of Mathematics wrote in a similar vein (23 May 1990): "I wish I could say that the kinds of difficulties you mention in regard to your research on Quetelet and Babbage were relatively uncommon in academic circles, but as you've discovered, they are not so uncommon as most people suspect."

More telling, perhaps, was the response of the Executive Director of the Commission on Professionals in Science and Technology (13 May 1992).

Having informed Berg that, though already "familiar with your story", she could not help, she added:

It is surprising that academic institutions, which usually jump all over people for plagiarism, have not hastened to give you credit for discovering the Babbage-Quetelet correspondence....Your situation is not uncommon for women, who often are not credited with their work, while one or more men claim it as their own.

Yet, to the (British) Council for Science and Society must go any award for the best reason for doing nothing. Having thanked Berg for the documents sent, the Council's chair observed (6 December 1990), "Unfortunately, the Council is now being forced to shut down, as we are caught in a financial crisis."

Berg even tried to go public. Mary McGory (*The Washington Post*) declined, pleading "[it] is over my head and out of my line" (4 October 1991). William Buckley (*National Review*) thought Berg's case "too far removed from too many of our readers' understanding and interests" (15 October 1991). John Maddox (*Nature's* editor) was "sorry to have to echo what your other correspondents have had to say, that I am afraid there is nothing we can do to help" (18 May 1992). Dear Abby was sorry too, "but since legal matters are out of my area of expertise, I cannot help you" (no date). Dennis Selby, Assistant to *The Nation's* Editor, judged that "it is a question of attribution rather than plagiarism [but, in] any event, *The Nation* is not the most appropriate venue for this matter" (30 June 1992). *Time* simply thanked Berg for writing (2 July 1992).

Perhaps out of desperation, or whimsy, Berg went even farther in search of a forum. The International Court of Justice at The Hague told him they had no jurisdiction (22 March 1990), as did the International Criminal Police Organization (2 July 1992). On the assumption that religions

might be concerned with a violation of the Commandment "thou shalt not steal", Berg wrote a number of religious organizations. Some did not write back. Those that did—the Rabbinical Council of America (5 November 1990), the Church of Jesus Christ of Latter-Day Saints (11 March 1991), Bob Jones, Chancellor of Bob Jones University (23 June 1992), the Archbishop of Detroit (6 July 1992), the Archbishop of Milwaukee (23 July 1992), and even the Papacy (14 June 1991)—all declined to get involved.

Nor did Berg give up after publication of my articles in 1993 and 1994. His search for a forum has continued to this day. I know that in two ways: first, by phone calls or emails I have received as a result of a Berg postcard alerting the recipient to "the mother of all plagiarism cases" and recommending me as a source for the details. I have not kept a log of these contacts, but among those I still recall are: a number from newspapers, including the *Harvard Crimson*; several from law enforcement agencies, including the Wayne County Sheriff's Department (Detroit) and the Secret Service; and several scholars who have published on plagiarism. Second, Berg himself continues to update me on his activities, sending me copies of responses he has received. Among these are the following: the Christian Coalition (3 April 1995); Michigan State University (8 May 1995); the Robert Wood Johnson Foundation (1 May 1995); Government Accountability Project (20 May 1995); the National Whistleblower Center (6 January 1996); the Christian Guidance Department of the Billy Graham Evangelistic Association (18 October 1996); the U.N. Secretary General (14 January 1997); the Commission on Higher Education, Middle States Association of Colleges and Schools (24 November 1997); the Copyright Directorate, Patent Office, United Kingdom (15 July 1998), a second try perhaps justified by a change of government; the Office of Vice President for Research, the University of Michigan (15 January 1998); the International Court of Justice (2 September 1998); the Georgia Bureau of Investigation (24 November

1998); Harry Reasoner (12 January 1999); the Norwegian Supreme Court (14 December 1998); the Biblioteca Apostolica Vaticana (27 January 1999); U.S. Senator Spencer Abraham (14 June 1999); the Office of Research Integrity of the Office of Public Health and Science, U.S. Department of Health and Human Services (29 September 2002); and the United States Attorney for the Eastern District of Michigan (7 September 2004). Barbara Bush, writing from the "Office of George Bush" even replied by postcard (date not legible): "Thank you for your very special message. It was kind of you to write and share your thoughts with me, and I so appreciate your having been in touch."

Ironies and Insights

The ironies of this story are many. I will note four:

First, following the (good) advice of the University of Michigan's *Guidelines for Maintaining Academic Integrity*, Berg published informally what he could not formally publish in a journal. Informal publication was supposed to assure him appropriate recognition. Perhaps it still will. Berg's "unprint" lodged in enough files around the world to provide undeniable proof that he made the "discovery" by early 1984. But that "unprint" could instead have allowed others to use the discovery without crediting him. The *Guidelines'* advice presupposes the ability of scholars to recall the source of an inspiration, a willingness to credit unpublished work, and agreement that the discovery was significant enough to deserve credit (as online indexes became ever more accessible and inclusive, making the discovery seem easier). In any case, what following that advice could not do was prevent someone else from making the discovery independently (as seems to have happened).

Second, those institutions best situated to investigate the charges seem to be the very institu-

tions least likely to respond to a complaint—or, at least, a complaint made by a non-academic outside the institution. Those responding to Berg's complaint, everyone from the Australian Academy of Science to Scotland Yard, from the Vatican Library to Barbara Bush, were generally those least well situated to investigate, whether for lack of resources, jurisdiction, or knowledge. How odd that Dear Abby should have more to say about Berg's complaint than did the University of Sydney or Warwick, or New York University Press!

Third, the silence of the institutions that should have responded has not been good for anyone. The complaint did not die but faded into a shadow darkening the reputation of everyone connected with *The Works of Charles Babbage*. When I first wrote about this case, I did not appreciate how thorough Berg's pursuit of a venue had been. He informed me of responses to his letters (or, more often, postcards) by sending me a copy of the response. He neither kept originals nor recorded them. He occasionally reported sending a complaint somewhere and receiving no response, for example, "Warwick University". Naturally, I assumed that that meant only one letter of complaint. Thanks to Campbell-Kelly, I have a better idea of how hard Berg worked to get a hearing (though this example dates from after my first article on the case). Here is what Campbell-Kelly tells us of what "a complaint to Warwick University" actually meant: "[After a first letter of the Vice-Chancellor, 10 November 1993,] Berg sent packages to the Registrar of the University, to the Chairs of two University Faculties, as well as to the Vice-Chancellor again."¹⁶ In each case, the package included a copy of my *Accountability in Research* paper (Davis, 1993).

This irony is frequent in cases of whistleblowing. We, the public, initially have only half the story, a complaint resting on clear and substantial evidence, but still only a complaint. Until the other side has been heard, any judgment we make must be provisional. But, while the other

side does not defend itself publicly, we (the public) must make a judgment (however provisional). And, in this case, what could we conclude from the evidence available to Berg except that Berg had been wronged? Silence is a poor defense—even if one has no other.

That was where things stood in 1993 when I first described Berg's case in *Accountability in Research*. I had met Berg three years before at a conference on "Ethics and the University" that inaugurated Wayne State University's Center for Academic Ethics (12 October 1990). To Berg, who was then just beginning his search for recognition, the conference must have seemed a natural place to find help. He came up after my talk and asked whether I would be interested in documents from the "the century's biggest plagiarism case". How could I refuse? A month later I had on my desk fifty or so documents, mostly letters. I soon realized that, though far from being the century's biggest plagiarism case, Berg's was a case full of interesting detail, detail concerning not plagiarism (however defined) so much as a complaint alleging plagiarism. I decided to write an article on his case and began a correspondence. I tried to tell the story I found in the documents he sent. Sometimes he corrected me, pointing out details I had missed in what I already had. Sometimes he provided further documents, proving some claim I doubted or disconfirming some inference I had drawn. He also helped me to understand his field of scholarship, different in so many ways from mine. Eventually, I had before me an essay that fit the documentary evidence, satisfied Berg, and fit my sense of how things might happen. Though Galler seems to have found him hard to work with, I did not.

If I were writing that article now, I would, I think, at that point have sent a draft to Bromley, Campbell-Kelly, and Galler.¹⁷ But, in the early 1990s, I had at least two reasons not to. The first was that I thought (and still do think) I had all the evidence I needed for a study of the *com-*

plaint. I had no doubt about the authenticity of the documents. What the letters revealed about the complaint was what interested me, not whether Bromley, Campbell-Kelly, or Galler were involved in a conspiracy to deny Berg credit for his discovery. I was sure they would tell a different story. The second reason I did not ask for pre-publication comments was that academics at that time were using the threat of legal action to prevent publication of articles about scientific misconduct they considered damaging. The journal in which I planned to publish, like most academic journals then (and now), could not afford a court fight. I knew nothing about Bromley or Campbell-Kelly, except that they were probably too far away to make a law suit practical. But Galler was closer—and I had met Galler. I had served on a university committee with him in the late 1960s when I was a graduate student (and he was already a senior faculty member). I had the impression that, while basically good-hearted, he could also be tough, especially if he thought of himself as protecting an institution he respected. I had no trouble imagining him suing an academic journal to prevent publication of what he considered defamation.¹⁸

I later added a third reason to this list. By complaining to the universities who employed them, and to their publishers, Berg had in fact already given them many chances to respond. Long before my article was published, they had learned of Berg's complaint through their respective universities, through their common publishers, and through professional friends, but had done nothing. For example, when the Vice Chancellor (chief academic officer) of Warwick University asked Campbell-Kelly what to do with Berg's complaint, which had come on a postcard, Campbell-Kelly chose to wait for something more formal.¹⁹

This brings me to a fourth, and last, irony. After almost five years of not responding to Berg's charges, the three did respond—three times. For the first response, they each sent a letter to the

editor of *Accountability in Research*. These, together with my reply to each, were published in 1994. That same year—in response to Berg's solicitations—the *Mathematical Intelligencer* published a short version of my original article.²⁰ Immediately following it, but before Campbell-Kelly's response, are four pieces of evidence:

- 1) an excerpt from the original Babbage letter
- 2) the same passage in the Quetelet translation
- 3) the same passage in the Van Sinderen re-translation; and
- 4) the version in the *Works*.

The title over these was "Check it Out". It was obvious that the Van Sinderen retranslation differed dramatically from the other three. In the shadow of that evidence, Campbell-Kelly's defense seems strained—or, at least, off balance.

Galler's successor at the *Annals*, J.A.N. Lee, seems to have found this debate format an unsatisfactory way to dispose of the controversy. He therefore published a long description of the case in the *Annals* itself, quoting at length from the *Annals'* own files. Many of the quotation are interesting; some informative. Lee helps us to see how much goes into editing a significant academic journal. And, of course, he adds details to show—what I would have thought obvious already—that Berg was, if not "the contributor from hell", at least far from the submissive scholar an editor might hope for. The article ends with increasingly long quotations from Bromley, Campbell-Kelly, and Galler, so many and so long in fact that Lee seems to become the compiler rather than the author, another opportunity for the three to respond in their own words.²¹ What the three sets of responses certainly help us to understand is what happened. What they fail to do is justify the failure to respond earlier, for ex-

ample, by asking for their own university to investigate. What they said, in effect, is that they could not see anything amiss in what they did. They could not see what the fuss was about. They could not see why anyone would write about the case or why two respectable journals would publish on the subject. They did not understand how much damage a "little guy" properly armed can do—the reason I began this article with Goliath's last words.

While this third response to my article seemed to satisfy them, it did not satisfy Berg. He continued his quest for official recognition for his discovery. Probably, an erratum to the *Works* would be enough.

Five Lessons

These ironies, though amusing, do not justify retelling Berg's story in a serious journal like *Plagiarism*. Only insight into plagiarism (however widely or narrowly understood) can. I think we may state that insight as five lessons.

The first is that there is a *geographic* misfit between the fields in which we would hope to prevent plagiarism and the institutions we have for preventing it. The sun never sets on those whom Berg seemed justified in accusing. Like the field they work in, the history of computing, they gird the globe. Yet, the institutions that have original jurisdiction over a case like Berg's are geographically small units, universities. None has control over more than one important party in the case. What is true of Berg's case is, in this respect at least, likely to be true of many others as well.

The second lesson is that many universities did not know (and may still not know) what to do with a complaint like Berg's. Only in the last two decades, primarily in the United States, have universities established formal procedures for dealing with cases of scholarly misconduct. Though largely designed to deal with wrongdoing in the

laboratory rather than in the library, a university having such procedures should have a routine for responding to a complaint like Berg's. Such a university might give Berg no more satisfaction than Scotland Yard did, but it would not have fallen into the embarrassing silence of Warwick University.

A third lesson to draw from Berg's story is that there is a problem of (what lawyers call) "diversity of citizenship" for procedures designed to prevent plagiarism (and otherwise maintain academic integrity). The scholarly world is not, as we often suppose, coextensive with the academy. In some fields, the history of computing being only one, there are significant numbers of non-academics. We need to imagine cases involving, for example, the CEO of a power company, a journal editor with academic standing, and a mathematician working in industry. Some of the non-academics will not have a doctorate. Yet, as several of Berg's correspondents noted, academics seem to discount the work of non-academics, especially if they do not have a doctorate, to feel no obligation to credit a non-academic's work when they would credit an academic's.²²

This prejudice, if it exists, would make problematic the granting of original jurisdiction to universities in cases where one of the parties is not an academic. The U.S. Constitution addresses a similar problem arising when the citizen of one state sues the citizen of another. Rather than force the citizen of one state to appear in a court where she would have the disadvantage of being an outsider, the Constitution allows either party to the case to remove the suit to federal court. Every citizen is entitled to a forum not likely to be biased by state citizenship. Perhaps the academy needs to develop something similar to hear cases like Berg's, an international scholarly court, the members of which would include independent scholars as well as academics.

A fourth lesson to learn from Berg's story is that the boundaries of plagiarism are far from

settled. No one responding to Berg doubted that failing to attribute the discovery of Babbage's letter was at least a minor wrong (assuming the editors derived knowledge of the original from Berg). There were, however, several who thought that the wrong in question was not plagiarism but (as Dennis Selby put it) a failure to attribute.²³ While everyone seemed to think he knew what the standards of attribution (or plagiarism) were, there seemed to be substantial disagreement on them even in a field as small as the history of computing. Stating standards is part of determining what they are (or shall be). Everyone in the Berg case would have been better off had there been generally accepted formal standards for attribution.

A fifth lesson is that scholars do not yet agree on *how* serious the wrong Berg complains of actually is (whether or not they think it plagiarism). Even among Berg's academic correspondents, a common view seems to be that the wrong done Berg is (like Berg's breach of Galler's confidentiality) quite petty. So, for example, one professor of history at Cornell commented: "If it were a straightforward case of plagiarism, we could raise some hell, but it is only neglect of the courtesy to acknowledge that you found the letter... [which is only] inexcusably rude."²⁴ A senior administrator at Boston University drew much the same conclusion (26 November 1991): "[You] were rather shabbily treated by Professor Bromley, who should have had the courtesy to acknowledge you in print as the individual who located the original English version of Babbage's letter....however, this does not appear to be a case of plagiarism."

My own view on this is quite different (as is Berg's). There are, it seems to me, degrees of failing to credit (as there are degrees of plagiarism). For example, failing to credit when giving credit would only be saying what everyone already knows is not wrong at all; but, using the work of another, where people might well assign you credit for it if you do not tell whose work it is, is

A Case of "Gray Plagiarism"—Davis

(I think) as bad as much plagiarism (and, indeed, is hardly distinguishable from it). Silence can be a default claim of credit—and the gravamen of plagiarism is failing to give credit where credit is due. But that is a topic for another article. What is clear, I hope, is that we need more discussion both of what plagiarism is and of how bad failing to credit is (whether or not called "plagiarism"). Discussion of these questions should go on in every academic department. But, as Berg's story

warns, we cannot be satisfied with settling such questions department by department, or even nation by nation. Scholarly communities—even small ones like the history of computing—cross departmental, national, and even continental boundaries. What is needed is international discussion of standards detailed enough to give insight into a case like Berg's. This journal is a good place to start.

REFERENCES

- Bromley, A. G. (1984). Letter to the Editor. *Accountability in Research*, 3, 283-284.
- Davis, M. (1993). Of Babbage and kings: A study of a plagiarism complaint. *Accountability in Research*, 2, 273-286.
- Davis, M. (1994). Righting the history of mathematics, or how sausage was made. *Mathematical Intelligencer*, 16, 21-26.
- Davis, M. (1999) *Ethics and the University*. London: Routledge.
- Campbell-Kelly, M. (Ed.). (1989) *The Works of Charles Babbage*. New York: New York University Press.
- Campbell-Kelly, M. (1994). Letter to the Editor. *Accountability in Research*, 3, 286-289.
- Galler, B. (1994). Letter to the Editor. *Accountability in Research*, 3, 281.
- Lee, J. A. N. (1995). On 'Babbage and Kings' and 'How Sausage Was Made': And now the rest of the story. *IEEE Annals of the History of Computing*, 17, 7-23.
- Muses, C. (1995). The unique reach of cybernetics in our *fin-de-siecle*. *Kybernetes*, 24, 6-20.
- Nissenbaum, S. (1990, March). The plagiarists in academe must face formal sanctions. *Chronicle of Higher Education*, 28, A52.
- Roberts, C.J.D. (1996). Letter from Babbage to Quetelet. *IEEE Annals of the History of Computing*, 18, 72-73.
- Weiss, Eric A. (1995). Of Babbage and kings. *Computing Review*, 36, 126.

NOTES

1. Much of this article appeared in an earlier version in one or more of the following places: Davis, M. (1993). "Of Babbage and kings: A study of a plagiarism complaint." *Accountability in Research*, 2, 273-286; Davis, M. (1994). "Righting the history of mathematics, or how sausage was made." *Mathematical Intelligencer*, 16, 21-26; or Chapter 6, *Ethics and the University* (Routledge, 1999). My thanks to an anonymous reviewer for this journal for suggesting improvements, to Kelly Laas for checking some citations, and to Herman Berg for again catching errors others missed.
2. Actually, the letter's fame is a bit more complicated. Strictly speaking, not the French version but an 1843 retranslation into English is famous. This first appeared when the Countess Lovelace (Lord Byron's daughter) translated from French Menabrea's paper on the Analytical Engine. It was in this English-French-English version that the Babbage letter became famous (rather than in Babbage's own words). See *The Works of Charles Babbage* (Campbell-Kelly, 1989).
3. Letter from Berg, 19 April 2006.
4. Berg believed that his discovery should have pleased Galler. He seems not to have realized a) that his discovery meant that Galler had made a mistake when he forced his reviewers to decide while their doubts remained and b) that Berg's quest for publication amounted to asking Galler to admit that mistake in print. Even a relatively decent person might wish Berg to take his discovery and quietly disappear.
5. According to Berg, the purpose of this letter was not so much to inform HSS of his discovery as to inquire whether the discovery merited a prize or, at least, a letter of praise he might use to strengthen his claim that the *Annals* should publish a report of it.
6. Why did Berg not simply publish his discovery elsewhere? He tried, but there are not many journals interested in the history of computing. Those he wrote advised him that the *Annals* was the appropriate place for a note correcting a claim made in Van Sinderen's article.
7. Campbell-Kelly (1994) was later to explain the elision of the date in this way: "The date May 1835 given in the title of van [sic] Sinderen's paper was omitted only on grounds of editorial clarity. My judgement was that to have included the date would have called for a convoluted and irrelevant explanation." Almost Berg's point. The only disagreement is over what that explanation would have been *and* whether it would have been irrelevant.
8. Berg always admitted that it was possible that between March 1984 and early 1989 the Quetelet letter was moved to the Royal Library (and then back to the Royal Academy), owing to extensive renovation, but he could find no one who knew of such a move. Campbell-Kelly (1994) thought the error to result from confusion. The letter had come with:

a compliments slip bearing the legend "De la part du Centre national d'histoire des science". We were unaware that the Centre was located in the Académie rather than in the Bibliothèque.

This would explain the error if the scholar in question would suppose he knew the location of the Centre without checking. For a later version of this explanation, see Lee (1995, p. 16). The truth may simply be that no one knows how the mistake was made.
9. Was he then one of those to whom Berg announced his discovery? We might think so. Campbell-Kelly seems too important to Babbage scholarship for Berg to have missed him. But Campbell-Kelly denies ever receiving an announcement before the *Works* appeared and Berg does not disagree. He has no memory of sending the announcement to Campbell-Kelly (though he does recall sending them to several people with

A Case of "Gray Plagiarism"—Davis

whom Campbell-Kelly collaborated on the *Works*). Berg's method of tracking his correspondence is to keep responses, not to keep copies of his own letters. If Campbell-Kelly did not respond, Berg would have no document to prove he sent him the announcement.

10. See Campbell-Kelly (1989, pp 22-27). Bromley has since denied responsibility for such details (Lee, p. 18). Those who follow controversies concerning research misconduct will recognize a pattern of claiming credit freely but declining blame as much as possible. Campbell-Kelly, it must be said, does not fit that pattern. He explicitly accepted full responsibility for the footnote after my article reported Berg's criticisms. See Campbell-Kelly (1994, p. 287).
11. Berg considered the phrase in quotes to be a smoking gun. The exact words appear in the version of Babbage's letter Berg discovered but not in any of the others. Since Bromley did not suggest that he had already discovered the original letter on his own (or knew of it in some other way), that phrase could (Berg thought) only mean that his first knowledge of it must have come from Berg. (The explanation, though right, turned out to be irrelevant to the charge of plagiarism—as explained in the note above.)
12. But see Lee (1995, p. 19) for Campbell-Kelly's description of how editorial work was done.
13. Of course, strictly speaking, Berg was not a whistleblower but an aggrieved party. For a justification of this strict way of speaking, see Davis, M. (1996). "Some paradoxes of whistleblowing." *Business and Professional Ethics Journal*, 15, 3-19.
14. I learned of this notice to Campbell-Kelly only when I read Lee's article (1995, p. 20). I was surprised; I had asked Berg to say nothing about my paper to anyone—for a reason explained below (fear of legal pressure to prevent publication). Luckily, no harm was done. Campbell-Kelly again chose to do nothing.
15. For a somewhat better experience with the AHA about the same time, see Nissenbaum, S. (1990). "The plagiarists in academe must face formal sanctions." *Chronicle of Higher Education*, 28, A52.
16. Davis M. (1994, p. 27).
17. I have not sent them a draft of this version because I have their responses in print already and their memory of events would have decayed considerably since they last wrote on the subject. They have already had their say about those facts about which they can speak with authority.
18. I was, as it turned out, doubly wrong. After publication, the distant Campbell-Kelly, not the nearby Galler, had his solicitors threaten to sue *Accountability in Research* if it did not retract my article. The editor (figuratively) wiped the sweat from his brow, crossed his fingers, and refused.
19. See his "Letter to the Editor" (Campbell-Kelly, 1994, pp. 287-288).
20. I also corrected the one error I had made in the original, describing the *Works* as "complete" when they were simply "definitive".
21. Though Lee did not invite me to respond to his article, he did send a draft to Berg who responded with comments. It happened that I was to give a talk at Lee's university (Virginia Polytechnic Institute and State University) on 23 March 1995. Knowing him as the editor of the *Annals* who had published Berg's paper (and not as one of my critics), I dropped him a note suggesting we might meet. Soon after I arrived at VPI, someone handed me a large manila envelop with a note saying he was sorry he could not meet me; he was then out of town but hoped I would comment on the enclosed draft. I did make some minor suggestions, especially softening the tone a bit, for example, eliminating the sentence, "Unfortunately for the alleged conspirators, if Davis had done his homework then he would [have] realized that there was no viable story

A Case of "Gray Plagiarism"—Davis

here and [there] would have been two publications less in his growing curriculum vita, and two journals would have been able to publish two other papers from legitimate researchers."

22. Bromley gave a plausible explanation of why he was not responsible for failing to credit Berg (though the explanation may make many wonder why his own name was so prominent both in the *Collected Works* and in advertisements for it): the letter had been located independently by Jim Roberts, an independent scholar in London working directly for Campbell-Kelly. Roberts (Bromley claimed and Roberts later confirmed) knew nothing of Bromley's possession of a copy of it; indeed, he knew nothing of Berg's discovery; he had simply worked in the same systematic way Berg had (though, doubtless, with better computer support) (Roberts, C.J.D., 1996). Roberts was unacknowledged anywhere in the *Collected Works*, even though he did an extensive and careful study of all the available Babbage correspondence. Why? The reason, according to Bromley, is that it "never crossed my mind to name the many people I knew to have seen or studied these archives." Archival work no more deserves academic credit than (it seems) washing bottles in a lab. (Lee, 1995, p. 18). Here is another failure to attribute resting on the same attitude toward archival work. Roberts seems to be a "ghost researcher" (that is, the scholarly equivalent of a "ghost writer"), someone to be heard but not identified. For how this attitude looks to an academic in a closely related field, see C. Muses. (1995). "The unique reach of cybernetics in our *fin-de-siecle*", *Kybernetes*, 24, 6-20, esp. pp. 16-18.
23. I must admit to little sympathy for this view. It seems to narrow the definition of scholarly plagiarism to what can be prosecuted in court as plagiarism (essentially, a violation of copyright). For courts, "plagiarism" is a technical term. Much that academics might consider plagiarism (for example, copying a method of tabulating data or using information another develops) receives legal protection through the law of patent, trade secrets, or the like—or none at all. In any case, the narrow definition seems wrong as a matter both of usage and of scholarly policy. "Plagiarism" comes (according to my dictionary) from the Latin word for kidnapping (or plundering, especially the taking of someone's "slaves"). "Ideas" as well as "original words or writing" can be plagiarized. Talk of "plagiarizing discoveries" seems to stretch ordinary language not at all (though perhaps "plagiarizing an invention" does). More important, in the commerce of scholarship, one's discoveries are at least as important as one's writing and, as Berg's story makes clear, just as susceptible to being plundered (that is, used without credit—credit being the customary payment among academics). Most, perhaps all, arguments against plagiarism in the narrow sense seem to be arguments against plagiarism in a sense broad enough to include using another's discovery without giving credit. The issue is not what plagiarism "is" so much as what forms of conduct (plagiarism or not) should be subject to formal disciplinary procedures, or at least dealt with in explicit rules, and which should be left to the less formal procedures of the research community (as much promise breaking is). For the opposite view, see Eric A. Weiss's note concerning "Of Babbage and kings" (1995, p. 126).

24. This letter (18 May 1989) does not in fact refer to the *Works* but to Berg's general feeling that he had somehow become "unmentionable" in Babbage circles. Yet, though it was written before publication of the *Works*, the distinction it makes, even the terms it makes it in, seem prophetic (and hence, worth quoting at this point).

Michael Davis is Senior Fellow at the Center for the Study of Ethics in the Professions and Professor of Philosophy at the Illinois Institute of Technology, Chicago, IL. Among his recent publications are: *Thinking Like an Engineer* (Oxford, 1998); *Ethics and the University* (Routledge, 1999); *Profession, Code, and Ethics* (Ashgate, 2002); *Actual Social Contract and Political Obligation* (Mellen, 2002); and *Engineering Ethics* (Ashgate, 2005). He has also co-edited (with Andrew Stark) *Conflict of Interest in the Professions* (Oxford, 2001).