Lone Geniuses In Popular Science

The Devaluation of Scientific Consensus

DAVIDA CHARNEY
University of Texas at Austin

Popular accounts of scientific discoveries diverge from scholarly accounts, stripping off hedges and promoting short-term social consequences. This case study illustrates how the "horse-race" framing of popular accounts devalues the collective sharing, challenging, and extending of scientific work. In her best-selling Longitude, *Dava Sobel (1996) depicts John Harrison's 18th-century invention of a marine chronometer, a groundbreaking precision instrument that eventually allowed sailors to calculate their longitude at sea, as an unequal race with Harrison as beleaguered hero. Sobel represents the demands of the Board of Longitude to test and replicate the chronometer as the obstructionist machinations of an academic elite. Her framing underreports the feasibility of the chronometer and its astronomical rival, the lunar distance method, which each satisfied different criteria. That readers accept Sobel's framing is indicated by an analysis of 187 reviews posted on Amazon.com, suggesting that popular representation of science fuels cynicism in popular and academic forums.*

Keywords: *rhetoric; 18th century; popular science; science journalism; scientific discourse; history of science*

The way in which scientific research is represented to the public has become an increasingly important topic for rhetoricians, sociologists, and historians of science, as well as for those who train professional communicators and journalists (Myers, 2003). Information about science flows back and forth across at least two contested boundaries: one between the domain of science and the domain of public media,

---

Author's Note: I acknowledge with thanks the UT College of Liberal Arts for a Dean's Fellowship in Spring 2001, the collegiality of the Department of History and Philosophy of Science at Cambridge University, and the hospitality of the Newnham College fellowship.

WRITTEN COMMUNICATION, Vol. 20 No. 3, July 2003 215-241
DOI: 10.1177/0741088303257505

and one between the media and the public sphere. The social stakes are high: the public's ability and willingness to engage in policy debates that involve scientific methods and data; public support for science, in terms of the amount and allocation of research dollars; and public acceptance of cutting edge applications and technologies.

The historically fraught relations between scientists and journalists have been well described by Dorothy Nelkin (1995). Ideally, the public's beliefs and attitudes about science would be drawn from texts that scientists and journalists agreed were accurate and worthy of public notice. But the very notions of accuracy and newsworthiness are at the heart of the conflicts between journalists and scientists (Reed, 2001). As rhetoricians of science have long observed, accounts of discoveries or inventions in scientific journals differ significantly from accounts of the same events in popular media (Fahnestock, 1986/1998; Myers, 1985b; Rowan, 1989). Greg Myers (2003) provides a timely reminder that comparisons between pairs of texts provide a quite limited view of the discursive interactions among scientists, journalists, and the public. However, the pattern of these differences clearly suggests that those in the scientific domain construe a "newsworthy" result and an "accurate" report differently than those in the public media:

- For scientists, results are important if they bear on the nature of a phenomenon, its causes, or its effects; for journalists, important findings are those with great or immediate implications for the public.
- For scientists, news is a robust set of findings that is likely to stand up to challenges, while for journalists, news is what is novel, nonroutine, or even aberrant.
- For scientists, accuracy depends on qualifying one's claims to avoid inaccuracy or overgeneralization; for journalists, stripping the qualifiers and hedges out of the claims does not sacrifice accuracy—it increases clarity and interest value.
- Scientists accept that results are often mixed, inconclusive or contradictory, while journalists expect clear and definitive results.
- Scientists deal with uncertainty and contradiction by modifying their hypotheses and continuing the research, while journalists question its value.

A separate set of concerns can be raised about the flow of information across the second contested boundary, between the media and the public. While journalists see themselves serving as "watchdogs" for democracy (Reed, 2001), this role can be undermined by the conventional ways in which they frame information for the public. As Dorothy Nelkin (1995) notes, journalists tend to portray scientists as heroes or scoundrels. Scientific inquiry is framed as a competitive horse race in which "winning" means being first to reach "the answer" despite having to overcome formidable obstacles. The effect of the horse-race frame on public perceptions of science has received little scholarly attention. But its effects may be similar to those of contemporary coverage of political campaigns. In over 20 years of studies, Kathleen Hall Jamieson has found that the horse-race frame that is typically used for election coverage is a Procrustean bed: material from speeches, debates, and position papers is stretched, squeezed and cut to fit into the frame (Waldman & Jamieson, 2003). Journalists heighten attention to the candidates' latest standing in public opinion polls and to remarks in which they score points against an opponent. At the same time, journalists truncate material on the issues in specific ways that obscure important distinctions between the candidates' positions. The result, Jamieson suggests, is that the public is not uninformed but rather misinformed.

As applied to science, the horse-race framing arguably has a more profound effect than in election coverage, for three reasons. First, the public has relatively easy access by other routes to a candidate's speeches and statements, many of which are geared toward a public audience. While it is possible for the public to seek out scholarly journals, these articles generally are not written with the public in mind. So journalists play a far more important role as interpreters of scientific information than of political information. Second, candidates for public office in the United States run as individuals, so the standard focus on individuals in news stories does not in itself distort public understanding of the election process. News stories about science also tend to focus on individuals. They often highlight one author of a just-published research article or conference paper with brief comments from one or two other independent scientists, including a proponent of a rival approach. But science is a communal venture. The fact that an individual researcher is often singled out for attention in news stories may well distort perceptions of the larger scientific debate, just as stories about a single speech would distort perceptions of the deliberative legislative process. Third, science is a collaborative venture. In the natural sciences, in particular, research is not generally conducted by individuals but by teams. Certainly, the fact that the contributions of numerous coauthors are not routinely acknowledged is one source of tension between scientists and journalists (Reed, 2001). But the consequences of the horse-race framing may go well beyond bruised egos.

In this article, I will argue that the horse-race frame devalues the process of scientific consensus-formation itself. Ultimately, such a claim needs to be backed by the types of studies that Jamieson conducts, including careful analysis of a large number of news articles or books. As a step toward encouraging such research, my goal here is to examine an extended case in detail to indicate the aspects of the process that I believe are truncated and even distorted. In particular, I will argue that the horse-race frame is built on the expectation that scientists reach certainty, a determinate and exclusive answer to their research questions. The framing highlights scientific results and derogates scientific disputes. Rather than portraying disputes as an integral part of scientific inquiry, the horse-race framing turns disputes into matters of sportsmanship, like persistently challenging a referee's call on a key play. In a quest for victory, nonscientific considerations hold sway, such as personal gain and sociocultural biases. This framing fits poorly with the less dramatic Popperian view in which scientists are engaged in a communal process of critical inquiry that proceeds in fits and starts toward provisional consensus, rather than universal agreement and absolute certainty. For Popper (1971), it is openness to challenge—not the supposed neutrality or disinterestedness of individual scientists—that defines scientific objectivity (p. 213).

The devaluation of scientific consensus-making in news stories can easily be observed. For example, in an article in the *Los Angeles Times* a few years ago, reporter Carol J. Williams (2001) takes the Nobel Prize committee to task for biased and errant selections. She opens by telling of two competing approaches to cancer research in the early 20th century, with an American, Francis Rous, investigating viruses as cancer-causing agents and a Dane, Johannes Fibiger, investigating the effects of parasites and worms. In Williams' account,

> The judges of the Nobel Prize in medicine chose the wrong honoree. The worm won. Fibiger received the 1926 award a month before his death, an intercession of fate that spared him the humiliation of seeing his life's work debunked a handful of years later. Rous eventually received recognition from the arbiters of medical advancement—55 years after publishing his theory that tumors can be caused by viruses.

Despite acknowledging that scientific findings must be judged in their historical contexts, Williams seems miffed that "no one in the hallowed realm of the awards committee" would admit to her that the award to Fibiger was a "mistake." On the grounds of Fibiger's "errant discovery" and "half a dozen other misguided awards in the hard sciences," Williams clearly views the Nobel Prizes in the sciences as deserving of the same kind of criticism leveled against the literature and the peace prizes, as "too subjective and vulnerable to the influences of politics, lobbying, cronyism and quirky judgment."

I am not at all inclined to dismiss the idea that scientists are subject to the same array of appeals to reason, emotion, and reputation as any other person making a decision. As I have argued elsewhere (Charney, 1996/in press), it is precisely because individual scientists are biased, despite their training, that they need to use objective scientific methods that are open to scrutiny and challenge. However, I do take issue with Williams's account on other grounds. She suggests that Rous was deprived of something rightfully his, that Fibiger's work had no merit whatever, and that the reluctance of today's scientists to discount Fibiger's Nobel Prize amounts to obstinate face-saving. In fact, biologist William Campbell (1997) reports that the merits of Fibiger's ideas are now being reconsidered. Recent findings suggest that the infections and irritations caused by parasites can contribute to stomach cancer. Campbell makes out a case that the popular debunking of Fibiger was overly severe, delaying exploration of the role of infections in certain types of cancer. Given the continual need to reconsider earlier research in the light of later findings, it is not quite fair to equate questionable judgments of scientific merit to bad calls by a referee.

News stories such as this are suggestive but far too short to represent how scientific consensus building is misrepresented in a horse-race frame. In what follows, I will analyze the horse-race frame in Dava Sobel's (1996) popular book *Longitude*. The book recounts the life-long efforts of a self-taught 18th century clockmaker, John Harrison, to invent a marine chronometer, a ground-breaking precision instrument that eventually became the standard means for sailors to calculate their longitudes at sea. Sobel's book, is an important, if not prototypical, case, for several reasons. First, because she wrote a book-length treatment, Sobel, a former *New York Times* science writer, was not subject to the constraints of deadlines and editorial preferences to which Nelkin (1995) attributes many of the problems of newspaper accounts. Second, the book itself is one of the most successful recent science popularizations. According to book reviewer Fritz Lanham (1998), it spent 41 weeks on the *New York Times* best-seller list, was translated into 22 languages, and was named book of the year in Great Britain. Another version with illustrations appeared in 1998 (Sobel & Andrewes), and in 2000, a television adaptation was broadcast starring Jeremy Irons, Michael Gambon, and Sam West (Sturridge, 2000). Third, while Sobel's framing is clearly drawn from Harrison's own version of the story that circulated at the time and became the popular view thereafter, she also cites several alternative scholarly accounts, including biographies of other major figures (Howse, 1989) and detailed descriptions of the chronometer's development (Gould, 1923/1960). As such, it seems fair to take Sobel's framing as the product of deliberate choice. In the rhetorical analysis that follows, I will draw on Sobel's text and many of the same sources she consulted.

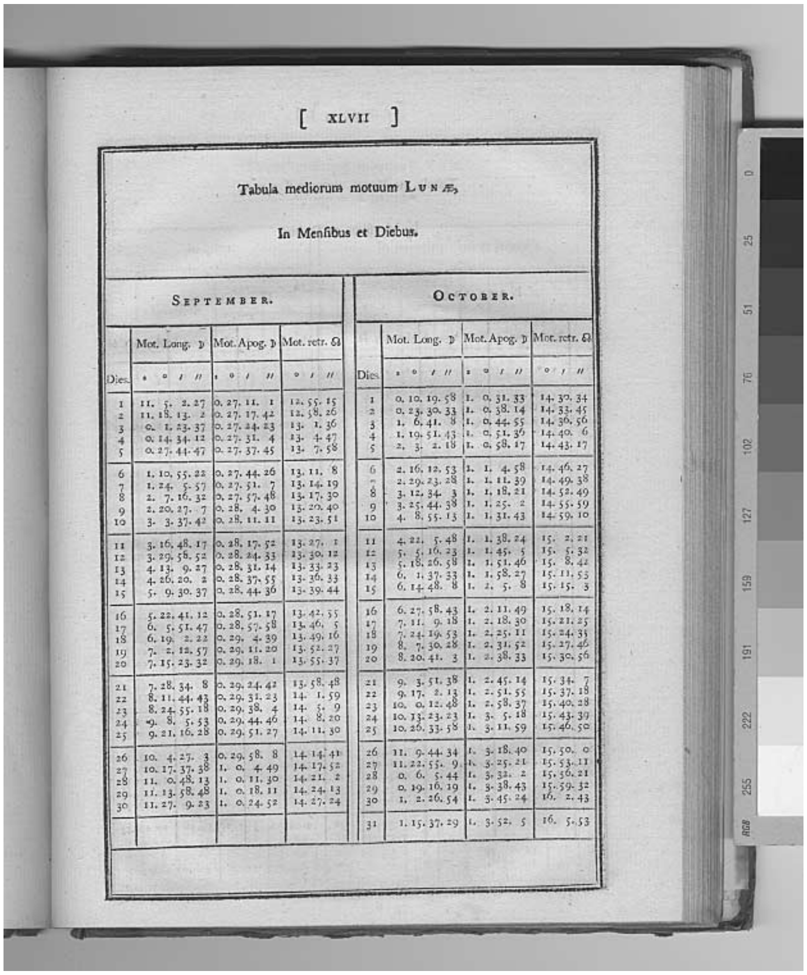
THE QUEST FOR LONGITUDE

Finding a reliable way to determine a ship's longitude at sea was one of the most important scientific problems of at least the last 500 years. Navigators recognized early on that distance traveled longitudinally was related to timekeeping, that noon was progressively later in the west than in the east. Sailors who could compare local time with the time at a point of known longitude could calculate their positions. In the early 1550s, two methods were proposed: one astronomical (measuring distances between known objects in the night skies whose positions at different times could be predicted) and one mechanical (calculating time differences between the current location and the home port). Concerted efforts to solve the problem were made all over Europe—observatories were founded and university chairs in astronomy were established. Yet it took over 200 years for these solutions to be developed to the point where they might be practically applied. And amazingly enough, both the astronomical and mechanical approaches reached fruition at the same time, the 1760s, in the same place, London, England. The approaches vied for eligibility for the generous Longitude Prize, established by act of Parliament in 1714.

The account of this endgame is at the heart of Dava Sobel's popularization, *Longitude: The True Story of a Lone Genius who Solved the Greatest Scientific Problem of His Time*. Sobel recounts the story of an unschooled carpenter, John Harrison, who teaches himself clock making. Over a period of 40 years (1730-1773), he invents a series of four marine chronometers. The chronometers may be viewed online in a detailed and sympathetic online exhibit, "John Harrison and the Longitude Problem," (2002) posted by the Royal Observatory at Greenwich at http://www.rog.nmm.ac.uk/museum/harrison/. The design efforts behind these chronometers revolutionized clock making and set new standards for mechanical precision. By keeping the chronometer reliably set to the time at the home port, such as Greenwich, by establishing the local noon through observations of the sun, and by converting minutes of time into degrees of longitude, sailors could calculate how far east or west of the homeport they had traveled. The last of Harrison's chronometers (H-4) became the prototype for the standard tool for calculating longitude that lasted until the advent of radio and satellite signals. In 1773, Harrison collected a payment that brought his financial reward over £20,000, a sum equivalent to millions of dollars in today's currency.

The competing astronomical method, the lunar distance method, was also brought to fruition at this time through the labors of the Astronomer Royal, Nevil Maskelyne. This approach involved painstaking compilation of observations and testing of hundreds of formulas to predict the relative positions of the moon, the sun, and the stars at various times all over the world. Maskelyne made his own observations, but more important, he systematized the records of astronomers worldwide and centuries old, including those of Galileo, Newton, and Edmund Halley. The first editions of Maskelyne's *Nautical Almanac and Astronomical Ephemeris* and its companion *Tables Requisite* were published in 1767. This approach was based on the relative ease of establishing the local time at noon. Using a sextant, a navigator would observe the distance between the moon and some identifiable planet or star in the nighttime sky. Then, the *Almanac* made it possible to look up the expected distance between those objects at the prime meridian (Greenwich) at that date and time. The difference was the distance traveled east or west from the meridian, which could then be recalculated in terms of degrees of longitude. When Maskelyne published tables of precalculations, the procedure could be completed in about 30 minutes.

Clearly, the astronomical approach is much less photogenic than the chronometers (Figure 1). But Maskelyne's *Almanac* and *Tables* were highly valued. Working 4 years ahead and drawing on additions and corrections from correspondents worldwide, Maskelyne supervised 49 annual updates of the *Almanac* until his death in 1811



[XLVII]

Tabula mediorum motuum LUNÆ,

In Menſibus et Diebus.

	September.				October.		
Dies.	Mot. Long. ☽	Mot. Apog. ☽	Mot. retr. ☊	Dies.	Mot. Long. ☽	Mot. Apog. ☽	Mot. retr. ☊
	s ° ′ ″	s ° ′ ″	° ′ ″		s ° ′ ″	s ° ′ ″	° ′ ″
1	11. 5. 2. 27	0. 27. 11. 1	12. 55. 15	1	0. 10. 19. 58	1. 0. 31. 33	14. 30. 34
2	11. 18. 13. 2	0. 27. 17. 42	12. 58. 26	2	0. 23. 30. 33	1. 0. 38. 14	14. 33. 45
3	0. 1. 23. 37	0. 27. 24. 23	13. 1. 36	3	1. 6. 41. 8	1. 0. 44. 55	14. 36. 56
4	0. 14. 34. 12	0. 27. 31. 4	13. 4. 47	4	1. 19. 51. 43	1. 0. 51. 36	14. 40. 6
5	0. 27. 44. 47	0. 27. 37. 45	13. 7. 58	5	2. 3. 2. 18	1. 0. 58. 17	14. 43. 17
6	1. 10. 55. 22	0. 27. 44. 26	13. 11. 8	6	2. 16. 12. 53	1. 1. 4. 58	14. 46. 27
7	1. 24. 5. 57	0. 27. 51. 7	13. 14. 19	7	2. 29. 23. 28	1. 1. 11. 39	14. 49. 38
8	2. 7. 16. 32	0. 27. 57. 48	13. 17. 30	8	3. 12. 34. 3	1. 1. 18. 21	14. 52. 49
9	2. 20. 27. 7	0. 28. 4. 30	13. 20. 40	9	3. 25. 44. 38	1. 1. 25. 2	14. 55. 59
10	3. 3. 37. 42	0. 28. 11. 11	13. 23. 51	10	4. 8. 55. 13	1. 1. 31. 43	14. 59. 10
11	3. 16. 48. 17	0. 28. 17. 52	13. 27. 1	11	4. 22. 5. 48	1. 1. 38. 24	15. 2. 21
12	3. 29. 58. 52	0. 28. 24. 33	13. 30. 12	12	5. 5. 16. 23	1. 1. 45. 5	15. 5. 32
13	4. 13. 9. 27	0. 28. 31. 14	13. 33. 23	13	5. 18. 26. 58	1. 1. 51. 46	15. 8. 42
14	4. 26. 20. 2	0. 28. 37. 55	13. 36. 33	14	6. 1. 37. 33	1. 1. 58. 27	15. 11. 53
15	5. 9. 30. 37	0. 28. 44. 36	13. 39. 44	15	6. 14. 48. 8	1. 2. 5. 8	15. 15. 3
16	5. 22. 41. 12	0. 28. 51. 17	13. 42. 55	16	6. 27. 58. 43	1. 2. 11. 49	15. 18. 14
17	6. 5. 51. 47	0. 28. 57. 58	13. 46. 5	17	7. 11. 9. 18	1. 2. 18. 30	15. 21. 25
18	6. 19. 2. 22	0. 29. 4. 39	13. 49. 16	18	7. 24. 19. 53	1. 2. 25. 11	15. 24. 35
19	7. 2. 12. 57	0. 29. 11. 20	13. 52. 27	19	8. 7. 30. 28	1. 2. 31. 52	15. 27. 46
20	7. 15. 23. 32	0. 29. 18. 1	13. 55. 37	20	8. 20. 41. 3	1. 2. 38. 33	15. 30. 56
21	7. 28. 34. 8	0. 29. 24. 42	13. 58. 48	21	9. 3. 51. 38	1. 2. 45. 14	15. 34. 7
22	8. 11. 44. 43	0. 29. 31. 23	14. 1. 59	22	9. 17. 2. 13	1. 2. 51. 55	15. 37. 18
23	8. 24. 55. 18	0. 29. 38. 4	14. 5. 9	23	10. 0. 12. 48	1. 2. 58. 37	15. 40. 28
24	9. 8. 5. 53	0. 29. 44. 46	14. 8. 20	24	10. 13. 23. 23	1. 3. 5. 18	15. 43. 39
25	9. 21. 16. 28	0. 29. 51. 27	14. 11. 30	25	10. 26. 33. 58	1. 3. 11. 59	15. 46. 50
26	10. 4. 27. 3	0. 29. 58. 8	14. 14. 41	26	11. 9. 44. 34	1. 3. 18. 40	15. 50. 0
27	10. 17. 37. 38	1. 0. 4. 49	14. 17. 52	27	11. 22. 55. 9	1. 3. 25. 21	15. 53. 11
28	11. 0. 48. 13	1. 0. 11. 30	14. 21. 2	28	0. 6. 5. 44	1. 3. 32. 2	15. 56. 21
29	11. 13. 58. 48	1. 0. 18. 11	14. 24. 13	29	0. 19. 16. 19	1. 3. 38. 43	15. 59. 32
30	11. 27. 9. 23	1. 0. 24. 52	14. 27. 24	30	1. 2. 26. 54	1. 3. 45. 24	16. 2. 43
				31	1. 15. 37. 29	1. 3. 52. 5	16. 5. 53

Figure 1. Page from *Tabulae Motuum Solis Et Lunae: Novae Et Correctae*, compiled by Tobias Mayer; revised, edited, and published by Nevil Maskelyne in 1770.
SOURCE: Collection of the Harry Ransom Center for the Humanities, University of Texas, Austin.

(Howse, 1989). *Almanacs* have been continuously published ever since, though the tables have not been updated since 1907. Yet it is due to the enduring international influence of Maskelyne's work that Greenwich was established as the prime meridian, because Maskelyne's almanacs and tables always took Greenwich as the starting point. The widespread adoption of these works is also attested by the fact that Lewis and Clark had an *Almanac* with them on their expedition in 1805 ("Observations," 1995).

In characterizing Harrison as "the lone genius who solved the greatest scientific problem of his time," Sobel frames the narrative about longitude as an unequal race, pitting one unlettered craftsman against the elite scientific community of astronomers, whose approach was the product of centuries of collaborative observation and testing. The climax comes in 1764 after Harrison's H-4 surpasses the accuracy criteria set out in the Longitude Act on a voyage to Barbados. At this point, the Board of Longitude awards Harrison half the prize on three conditions: the design of the chronometer was to be fully disclosed graphically and verbally, the chronometer itself had to be capable of duplication in a timely and economical way, and the chronometer and its copies were to undergo further tests of accuracy. Harrison resists these conditions strenuously, believing that the full amount should be paid before the design was disclosed (Gould, 1923/1960, p. 60). In the end, in Sobel's phrase, he "knuckles under" (p. 131). Yet it still took the intervention of George III and a special act of Parliament to procure the final installment of £8,750 in 1773.

While Sobel includes the key elements of the factual record, she plays up Harrison's heated version of the story. She depicts Harrison as the defenseless underdog whose patently obvious victory was delayed while he became old and infirm. The Board of Longitude is portrayed as biased and obstructionist, trying by fair means and foul to gain the prize for the obviously inadequate lunar distance method. And Maskelyne is portrayed as intervening directly to cheat Harrison of his rightful reward. This framing of the story is achieved through direct ascription of motives to various parties, the selection of details, and most important, through use of a narrative structure that hinders comparative analysis of the two methods.

SOBEL'S CHARACTERIZATIONS OF HARRISON, THE BOARD OF LONGITUDE, AND NEVIL MASKELYNE

Sobel depicts Harrison throughout in heroic terms, even, perhaps facetiously, comparing him to another carpenter-martyr who spent "the first thirty years of his life in virtual anonymity before his ideas began to attract the world's attention." More important, Sobel portrays Harrison's claim to the prize in 1764 (at the end of the first trial of H-4) as entirely valid. Here is how Sobel concludes the narrative of the first H-4 voyage to Jamaica: “The prize should have gone to John Harrison then and there, for his Watch had done all that the Longitude Act demanded, but events conspired against him and withheld the funds from his deserving hands” (p. 122). For Sobel, the occasion of meeting the accuracy criterion is sufficient for a valid claim. Therefore, she paints as obstructionist the Board’s call for further tests, duplication of the watch, and provision of detailed drawings and explanations of the design.

But Sobel also depicts Harrison as deserving of the prize for reasons that are not relevant to the Longitude Act:

- the priority of Harrison’s demonstration of accuracy (pp. 83, 122)
- the uniqueness, beauty and delicacy of the chronometers as objects (pp. 86-87, 106-107, 145)
- Harrison’s inventiveness in counteracting the effects of climate, heat, wetness and motion (pp. 71, 103)
- the length and intensity of Harrison’s 40 years on task (pp. 9-10, 101, 145, 148)

These features, that play up the “heroic” aspects of the frame, certainly make Harrison’s story appealing, but they have little bearing on the adequacy of the chronometer. Many astronomers over the centuries had labored their entire lives over the longitude problem without success, and many creative, if not beautiful, instruments had been developed, including the telescope. Yet Sobel presents the value of Harrison’s work as so obvious that the Board’s skepticism was inappropriate: “Instead of the accolades [Harrison] might have expected for his achievements, he was to be subjected to many unpleasant trials” (pp. 98-99).

In her opening chapter, Sobel represents the Board of Longitude as deliberately conniving to deny Harrison the prize:

> [Harrison’s] every success, however, was parried by members of the scientific elite, who distrusted Harrison’s magic box. The commissioners charged with awarding the longitude prize—Nevil Maskelyne among them—changed the contest rules whenever they saw fit, so as to favor the chances of astronomers over the likes of Harrison and his fellow “mechanics.” (p. 9)

Sobel repeats the Harrisons’ suspicions that individual Board members were out for personal gain (p. 117). She even goes so far as to suggest that they were incompetent—the Board “may have been incapable of understanding [H-4's] mechanism" (p. 123), and that they feared the unknown—"something unseemly attended the sea clock in the eyes of scientists and celestial navigators. Something facile. Something flukish. In an earlier era, Harrison might have been accused of witchcraft for proposing such a magic-box solution" (p. 99).

At the outset, Sobel introduces Nevil Maskelyne as "a special enemy" of Harrison's, "whose tactics at certain junctures can only be described as foul play" (p. 9). She describes him as Harrison's "nemesis" (p. 129) and as the astronomers' "handpicked henchman." While she does concede that Maskelyne is "more an antihero than a villain, probably more hardheaded than hardhearted" (p. 111), she goes out of her way to cast personal aspersions on him. In her brief recap of his early life, she makes him out as excessively solemn and self-important (p. 112). Of his work on the lunar distance method, she describes him as "boasting" (p. 125) and "quite flushed with accomplishment" (p. 122), and she describes him as "airily" dismissing the chronometer (p. 142). She repeats Harrison's accusations that he deliberately mishandled the chronometers (p. 137) as well as hearsay about his "ill will hexing the watch" (p. 140) and his "chortling" (p. 149) over the obstacles he had put in its way.

Most important, Sobel frequently treats the lunar distance method as a patently inadequate approach, rather than as an alternative that was at least equally plausible. Sobel does recount the centuries-long history of the method and the celebrity of the thinkers who contributed to it. But she turns this history against the lunar distance method, both by using it as evidence of the personal investment of the astronomers in the success of this approach and by using its celebrity to create dramatic contrast with the humble origins of the chronometer (pp. 60, 98). She treats the presentation of evidence on behalf of the lunar distance method as evidence of the bias of the Board (pp. 129-130), though she does finally acknowledge Maskelyne's work as an "enduring contribution to navigation" (pp. 135, 166).

In challenging Sobel's framing of this account, two main issues will be addressed: (a) the fairness of the criteria by which the Board of Longitude judged the two approaches and (b) the historical context for scientific discourse at the time. While there is ample evidence that the members considered astronomical solutions as likelier to succeed, a good case can be made that, in practice, the Board of Longitude was fair in its judgments of both the marine chronometer and the lunar distance method.

JUDGING THE RIVAL APPROACHES

Harrison's case (and Sobel's on his behalf) is based on the narrowest possible reading of the criteria spelled out in the Longitude Act. The Longitude Act focuses primarily on accuracy, operationalizing this term as a set of consistent predictions of location within a margin of one half of one degree (or 30 nautical miles) over a long voyage between England and the West Indies. The conditions of the shipboard trial (stated in the Act of 1714 in the singular) are to be specified by the Board. The Act also specifies that the winning method is to be practicable and useful. Finally, besides the top prize of £20,000, other prizes and incentive grants were allowed for approaches that met somewhat looser standards of accuracy or that were deemed useful.

The debate turns on the meaning of the terms *practicable* and *useful*. The Act itself seems to have been vague on these criteria. Sobel barely mentions them. But the way in which they were ultimately operationalized can be pieced together, as shown in Table 1, which also summarizes the outcomes for each of the rival approaches.

First, consider the merits of the lunar distance method. Maskelyne had been using tables to plot longitude fairly accurately for several years by the time he published the first edition of his *Nautical Almanac* and *Tables* in 1767. Maskelyne reported on two voyages using an earlier set of lunar distance tables by Tobias Mayer in 1762 and 1764 with an accuracy within 32 nautical miles (Howse, 1989). The practicability and usefulness of Maskelyne's almanac derived from its clear presentation of a comprehensive set of data; the tables precalculated parts of the conversion from sextant readings and table entries to degrees of longitude. Useful almanacs depended on having enough observations to map the expected distances from the moon to identifiable stars visible in various parts of the world at various times of year. To be useful, new and improved almanacs were needed year after year.

Some advantages to the method were cost, size, and availability. Even in the mid-18th century, printed almanacs were inexpensive and easy to duplicate; a sextant and lunar tables were readily available for £20 (Sobel, 1996, p. 153). Paper charts were also of a manageable size and weight. The robustness of the approach, that is, its applicability worldwide under various conditions, was mixed. The robustness of the method was not endangered by variations in temperature or climate, wetness, or the pitch of the boat, but it was vulnerable to the vagaries of time, weather and date. The moon and other known astronomical objects (such as the sun, planets or stars) had to be visible, so

Table 1

Reconstructed Operational Definitions of Accuracy, Practicability, and Usefulness Criteria Applied by the Board of Longitude to the Lunar Distance Method and the Marine Chronometer and Outcomes for Each Approach

Operational Criteria as of 1765	Lunar Distance Method	Marine Chronometer (H-4)
Reliable accuracy: time stays within a half degree on voyage between Britain and West Indies	Satisfactory and improving; tested on voyages and by observations worldwide	More than met by H-4 on Barbados trial; met by other models on other shorter voyages
Cost to outfit the fleet; size can be accommodated in range of vessels	Low cost (£20); lightweight; small	High cost (at least £200); 2 years to reproduce; size and weight successively reduced
Robustness (ease of use and maintenance)	Requires calculation; limited by weather and season; hours to complete; easy to maintain	Easy for anyone to read at any time; delicate instrument requiring regular winding and maintenance
Replicability of instrument	Easy to reproduce; familiar to scholars and sailors worldwide	Unfamiliar original design; only one of each model of chronometer in existence
Public disclosure of methods and results to allow challenge and improvement	Frequent publication and sharing of updates and corrections	Designs, descriptions, viewing of clockworks all kept under wraps

the method was not applicable several hours of the day and many days in the year. When it could be used, the lunar distance method required skill and time, careful readings and calculations that initially took about 4 hours but were eventually reduced to 30 minutes. However, such calculations were already familiar to a ship's crew because of the ubiquity of other navigational instruments, such as the sextant. Finally, the methods and results of the approach were widely shared and available for scrutiny, challenge, and additions. In fact, keeping the almanacs accurate and up-to-date required international cooperation.

The balance sheet for the chronometer method is nearly the complete opposite. Its high degree of accuracy was established in its first official public trial, when H-4 surpassed the requirements on a long voyage to Jamaica in 1761 and again in a voyage to Barbados in 1764. Earlier versions of the chronometer had also displayed high accuracy in one or two public tests. Harrison's confidence in the accuracy and reliability of the chronometers, however, was based on years of testing conducted in the strict privacy of his workshop without public attestation. In several respects, the chronometer was also practical and useful. The clock's major advantage was that it would maintain its accuracy at any time of day and in any location. Harrison's most important innovations tackled the obvious threats to the reliability of a mechanical device posed by variations in climate, dampness, temperature, and the roll and pitch of a ship at sea. These innovations, which Harrison constantly improved, display great ingenuity, anticipating strategies of materials engineering. Assuming the chronometer was set properly at the outset and kept time reliably, time at the home port was immediately available. Assuming that local noon could be established, anyone could read off the home port time and compare it to local time. The weakness of the chronometer lay in replicability, cost, and robustness.

In 1765, H-4 was the unique instance of a chronometer with a highly original and unfamiliar design—a design that Harrison kept completely secret. He was unwilling to provide drawings, demonstrations, or access to the chronometer itself. If the chronometer were a one-of-a-kind fluke, if it was impossible for Harrison or anyone else to duplicate, then it was useless to the hundreds of ships in the British fleet. In fact, Harrison needed more than 2 years to create a replica working only from his diagrams and notes. Larcum Kendall, another clockmaker commissioned to create a duplicate with H-4 on hand, also needed 2 years to make a single working copy. Kendall's chronometer came at a cost of £500; he predicted it would take many years before the chronometer could be reproduced for £200 or less (Sobel, 1996, p. 153). In fact, it took 13 years before a factory succeeded in producing chronometers (1785) when the price finally dropped to between £65 and £80. While 5,000 chronometers were in use worldwide by 1815, chronometers were not routinely available on Royal Navy ships until the 1820s (Sobel, 1996, p. 192).

Finally, a single instrument like a chronometer was not very robust. This was not a Timex that could take a licking and keep on ticking. William Harrison took extraordinary precautions on the initial test voyage to Jamaica to avoid wetting H-4 (Sobel, 1996, p. 121). Sobel also reports on Harrison's great indignation at Maskelyne's apparent rough handling of the chronometers when he took them to Greenwich for testing (p. 137). But the complaint in itself is evidence that the chronometer would also be vulnerable to hard knocks on ships at sea. A chronometer could be lost overboard or broken or drenched or allowed to run down. Resetting it would require an independent way to calculate time at the home port, such as by the lunar distance method.

Was the Board of Longitude (including Maskelyne) biased in favor of the lunar distance method? Probably so. Yet, as this analysis suggests, the members were not unreasonable in asking Harrison for more than a single demonstration of accuracy. In short, the chronometer method was "practicable but not generally useful, whereas the lunar distance method was useful but not practicable" (Howse, 1989, p. 77). At sea, the two methods complemented each other so well that the best solution would be to have them both available at the same time (as Maskelyne noted); this was in fact the practice of navigators for many years.

With the acumen of hindsight, however, we can see each approach as emblematic of its time, the advent of the Industrial Revolution. Harrison's approach was revolutionary because it tackled a long-standing problem through a new unfamiliar technological mechanism that embodied abstract principles of nature. But Maskelyne, who exploited existing navigational technologies, was also revolutionary in his approach toward information by pushing the scientific community toward easily shared communicative practices that endure after any individual technology becomes obsolete.

THE BOARD OF LONGITUDE AND THE DEVELOPING SCIENTIFIC COMMUNITY

In 1773, George III, an amateur astronomer himself, took Harrison's part and leaned on Parliament to give him a special grant for the remaining sum of prize money. As Sobel puts it, "[George III] took the Harrisons under his aegis and helped them circumvent the obdurate board by appealing directly to the prime minister, Lord North, and to Parliament for 'bare justice,' as William [Harrison] called it" (p. 148).

This image of George III as the defender of the little man is perhaps surprising to those of us in the United States who are used to seeing George III as a tyrant. However, in the alternative account that I will present here, George III comes across as equally negligent of the will of the people in the Harrison affair as in the treatment of the American colonies. George III's intervention undermined the nascent processes of social construction in the growing scientific community that at this very time was developing methods for judging claims on objective merits.

In the history of scientific rhetoric, the 18th century is just the time that the purposes and protocols for reporting scientific experiments were themselves at a formative stage. In his survey of articles from the *Philosophical Transactions of the Royal Society*, Charles Bazerman (1988) describes the early 18th century (the time of the establishment of the Longitude Act in 1714), as a time when experimental reports focused on simple demonstrations, showing that a phenomenon occurred or could be made to happen. Later, at the time when Harrison was working most feverishly on his chronometer, reports in the *Transactions* were beginning to be couched in terms of problems or puzzles that experiments might help to resolve. This is also when experiments took the form of a sequence of trials each with a reasoned variation. Using an example from 1770, Bazerman notes that "the distinctions between trials become important as events in consciousness, so at least the crucial differences between trials become defined" (p. 70).

Dwight Atkinson's (1999) recent analysis of the *Transactions* from 1675 to 1975 supports and extends Bazerman's findings. Atkinson notes changes during this period along five textual dimensions: from authorial testimony to information production, from narrative to nonnarrative structure, from situation-dependent references to explicit external references, from overt to tacit persuasion, and from concrete and active language to abstract (passivized) language. On all but the last dimension, major turning points appear to have occurred between 1725 and 1775. While Atkinson dates the emphasis on problem-solving in scientific discourse a bit later than Bazerman, he agrees that the period between 1725 and 1775 was a time of increasingly detailed descriptions of methods and procedure, with illustrations of experimental apparatus becoming increasingly common. Scientific arguments were geared to an active audience of readers, displaying efforts to preempt objections or rebut explicit challenges. Overall, then, this is a time when experiments and their results increasingly become community property.

In fact, Nevil Maskelyne himself played a role in pushing the Royal Society toward policies of sharing data. According to his biographer Derek Howse (1989), Maskelyne complained that previous Astronomers Royal had considered their works and their instruments to be their own property rather than public property. These proprietary attitudes not only impeded Maskelyne's progress when he was appointed, but that of astronomers and navigators worldwide. Maskelyne pushed for the regular publication of interim observations (Howse, 1989, p. 44) and for requiring regular publications from future astronomers using the Royal Observatory. He took charge of testing and publishing the lunar tables of his predecessors, such as Bradley and Mayer. (He even published the diagrams and transcript of Harrison's explanations of the workings of H-4.) He worked to make science a community without borders, helping other European countries to found observatories. He steadily cooperated with his international counterparts to produce translations of the *Almanac* and its tables, even when France and England were at war (Howse, 1989, p. 121). In fact, his active dissemination of his tables year after year, with all distances calculated from Greenwich, led the French and other countries to give up on recalculating the tables with their own observatories as starting points. When Maskelyne's versions became widely adopted, Greenwich became the sole practical choice for the international prime meridian.

The Board of Longitude would certainly have reflected contemporary scientific views as it included three chaired professors of mathematics at Oxford and Cambridge, the president of the Royal Society, and the Astronomer Royal, as well as government ministers and top ranking officers in the Navy (Sobel, 1996, p. 54). The developments in scientific method of this period, therefore, may well have been reflected in the Board's operational definitions of accuracy, practicability, and usefulness. If the Board did indeed change its criteria over time, as Harrison charged, its actions were arguably based on rising standards of evidence across the entire scientific community rather than arbitrary acts of class bias against Harrison or other artisans.

In fact, the marine chronometer is part of a larger story about partnerships among scientists and artisans at this time. In the 18th century, scientists, artisans, and manufacturers worked increasingly closely together to develop the precision instruments needed to address scientific and commercial problems. While some might deplore this trend as a forerunner of the military-industrial complex, Larry Stewart (1998) argues on the contrary that in 18th century England, "The machine and industry were seen to benefit the public at the expense of private oligarchic interests" such as the landed gentry and the church hierarchy. Public knowledge, interest in, and access to scientific information were relatively high. Today's sharp lines between scientist, engineer, merchant, and consumer did not then exist. Michael Mahoney (1993) notes that many London clockmakers worked on custom-designed instruments for scientists and several were admitted to the Royal Society. In far more than the case of the chronometer, scientists and artisans together set standards of precision that quickly became routine in science, industry, and domestic settings (Stewart, 1998).

In all these ways, scientists were forming themselves into a self-governing republic, a republic that was not reserved exclusively for the elite or those patronized by wealthy sponsors. Because they bring criteria and results into the open, scientific collaborations have historically transcended political, religious, racial, and national boundaries (Kitcher, 1993; Porter, 1995).

In this context, it is the attitudes of John Harrison that seem tradition-bound. Harrison, by all accounts, was not only secretive and protective of his designs, but he was also unable to produce clear exposition. Even Sobel admits that his description of the workings and assembly of H-4 “utterly defied understanding” (Sobel, 1996, pp. 143-144). A good portion of Harrison’s difficulties with the Board, therefore, may have been due to the different values they placed on knowledge sharing and their different abilities to do so.

Given Harrison’s secretiveness, the Board’s preference for the lunar distance method is even more understandable. The very novelty and unfamiliarity of Harrison’s designs would likely have played against easy acceptance, no matter whether he was a clockmaker or a chaired professor. The scholarly practice of judging what is novel in the context of what is known (Kaufer & Geisler, 1989) was apparently well rooted by the 18th century. Then as now, as a scientist in Greg Myers’s (1985a) well-known study put it, “Total originality is sure death” (p. 48).

So when George III supports Harrison’s petition to Parliament in 1773 for the remaining £8,750 that Harrison felt he was owed, the king was imposing his authoritarian will against the more republican judgment of the scientists and admirals who were basing their decision on scientific and pragmatic grounds. George III and Parliament were perhaps forerunners for today’s congressional representatives who circumvent scientific review boards, such as the National Science Foundation, in order to approve earmarked grants to scientists in their home districts (Payne, 2003).

THE BIASES OF THE BOARD RECONSIDERED

The most serious charge against the Board of Longitude that Harrison made (and Sobel on his behalf) was that it deliberately obstructed his progress through preference for "scientific" rather than "mechanical" approaches. It is worth considering just what actions were taken by the Board of Longitude and the elite scientific community of the time to support Harrison's work. Scattered across Sobel's more or less chronological narrative is evidence that Harrison received substantial support during his long self-directed period of research and development. From 1740 to 1773, he received the following recognition and material support:

- testimonials presented to the Board of Longitude signed by 12 Fellows of the Royal Society in 1746, the Copley gold medal awarded by the Royal Society in 1749, a fellowship in the Royal Society offered to Harrison (declined), admission of his son William to the Royal Society later;
- stipends from the Board of Longitude: one of £500 around 1740 (at the completion of H-2), another five stipends of £500 each between 1741 and 1760 (at the completion of H-3);
- a grant of £1,500 after the first public test of H-4 in 1762, an additional £1,000 paid after its second trial in 1764; and
- a grant of £8,500 awarded in 1765 on condition of receipt of plans, replicas, and further accuracy data.

Thus, the Board awarded £13,800 to Harrison even before the intervention of George III, who brought the total reward to Harrison over £20,000. These sums cannot be translated into current dollars in any very meaningful way. But they can be placed in the context of other salaries and costs at the time. According to historians Jacob and Reid (2001), £700 in start-up funds was enough to launch James M'Connel and John Kennedy in a steam-powered cotton manufacturing company with two other partners around 1791. When they had succeeded enough to dissolve this partnership and reincorporate on their own in 1795, M'Connel and Kennedy each realized a profit of £816. They went on to become two of the most important cotton barons in Manchester. (Another instructive source on salaries is "Professions in 18th Century.") Apparently, even £1,000 was a substantial amount at the time. A sum of £20,000 was certainly a fortune. So it is fair to say that despite its skepticism about the chronometer, the scientific community acknowledged its usefulness and value early on and the Board generously supported its development for nearly 40 years.

Both as chair of the Board of Longitude for a time and as an active developer of the lunar distance method, Nevil Maskelyne was certainly in a position of conflict of interest. Yet the inclusion of the Astronomer Royal on the Board of Longitude was specified by the Longitude Act itself. Maskelyne's biographer, Derek Howse, found no contemporary evidence, beyond Harrison's accusations, that Maskelyne had sabotaged the chronometer. In fact, Maskelyne never applied for the prize money himself. Howse explicitly rejects today's popular but unfounded depictions of Maskelyne as the "evil genius who tried to deprive the poor illiterate Yorkshire carpenter of his just rewards." Yet Sobel chose to make just this characterization, despite acknowledging access to Howse's biography as well as to Howse himself.

The most serious charges against the Board are that it employed delaying tactics to give extra time for the lunar distance method to prove itself and that it arbitrarily changed the criteria midstream. However, the lunar distance method was held to equally exacting standards of repeated testing on multiple voyages. Rupert Gould (1923/1960), who restored all of Harrison's chronometers to working condition, presents a far fairer view than Sobel's. He considers the struggles between Harrison and the Board as "natural" given the amount of public money at stake and suggests that "there was a good deal to be said on both sides" (p. 56). It is true that the Longitude Act of 1714 was vague about the deliverables required in exchange for its rewards, but if the Board of Longitude had turned over the entire £20,000 prize to Harrison at the end of a second successful voyage in 1764 with no delivery of material or intellectual property, it would have been guilty of reckless use of public funds.

POPULAR UPTAKE OF SOBEL'S FRAMING

There is no easy way to assess the effect of Sobel's horse-race framing on the public. However, an approximation is possible based on spontaneous comments from readers, in particular, the 187 postings on the *Longitude* page at Amazon.com. Obviously, the people who made the posts were entirely self-selected: They are not representative of all nonfiction readers or even of all readers of this book. Instead, they represent, first, people who felt strongly enough about the book to take the time to write, and second, people who have the resources and the savvy to use technology for this purpose.

Each posting was accompanied by a rating of the book ranging from 1 to 5 stars. Based on a tabulation of these ratings, most readers liked the book. About 70% gave the book ratings of 4 or 5 stars. Those who didn't like the book mainly criticized its lack of detail and explanation; many also complained about the lack of illustrations. The posts ranged in length from 5 to 1,247 words, with an average of 158 words. Those who liked the book (awarding it 4 or 5 stars) wrote slightly longer posts than those who awarded fewer than 4 stars (165 words vs. 141.3 words, respectively).

A key aspect of Sobel's framing is the absence of any explicit analysis of the strengths and weaknesses of the two methods against a common set of criteria. The readers' comments reflect Sobel's selective focus on the accuracy criterion to the exclusion of practicability and usefulness. Only 42 of the readers (23%) mention feasibility in any way. Of these, 13 simply say that Harrison solved the problem or that the chronometer worked. The other 29 explicitly mention accuracy or other criteria. Only 4 of these mention any drawbacks to the clock, such as the delay before mass production. Only 2 mention anything in favor of the lunar distance method.

Readers also echoed Sobel's focus on aspects of Harrison's efforts that had nothing to do with the criteria for a solution but that reflect the heroic frame. Over half the comments (106) mention Harrison. These uniformly favorable comments most frequently focus on the duration and difficulty of Harrison's work (33 use the term persistence, 30 say he was beset, 22 mention the lifelong effort, 13 single-mindedness). Many mention his status as an outsider (19 call him an "outsider," 12 use the term "lone," 8 that he was "lower class"). Another frequent comment concerns Harrison's quality of mind. These posts follow one of two themes, either Harrison as a "diamond in the rough"—the "genius" whose brilliance shines through (29 posts) or Harrison as the "common man" who shows up the elite by dint of hard work (22 posts). Finally, 21 posts call him a "hero" and 13 mention his final "triumph." Thirty posts mention the marvelous quality of Harrison's creations, their beauty, originality, ingenuity, primacy.

Readers largely accepted Sobel's negative characterizations of the Board and of Nevil Maskelyne. One third of all readers (64 posts) mentioned the Board of Longitude in some way. Of these, 43 posts accused the Board of using unscrupulous means, politics, or bias against Harrison. The vast majority of those who criticized the Board gave the book as a whole high ratings; 87% of these readers gave it 4 or 5 stars. Here are a few representative excerpts:

> I loved the story of Harrison doggedly doing what he knew was right in the face of so many enemies who were so unscrupulous. (June 1998)
>
> Harrison had to contend with the British Royal Admiralty Establishment where Maskelyne, a proponent of Lunar Distance Method tried his best to damn the clocks by subjecting them to fake trials. (June 2001)
>
> Also, the contrast of Harrison against Maskelyne was extremely well presented. I felt nothing but disgust at the political machinations which impeded Harrison's well-deserved recognition and reward. (January 2001)
>
> Apparently, red tape and professional jealously [*sic*] are timeless qualities of human nature. (June 2000)

This pattern of comments suggests that Sobel's readers by and large reconstructed or even exaggerated her framing of the story: Those administering the prize are bureaucrats who are set on their preconceived notions of the truth, rather than scientists rigorously evaluating two plausible but imperfect approaches to a high-stakes problem. Of course, there is no way to know to what extent Sobel fostered, reinforced, or simply elicited these views. It is clear that very few readers stepped outside the frame to challenge Sobel's characterization. Those who gave low ratings to the book were mainly critical of its lack of depth and explanation. They seem to have been aficionados of clock making or navigation looking for more technical detail. Only two readers criticized Sobel's framing:

> The author goes to some lengths to present other scholars in an adversarial role, but the "other side" was not well represented. Other points of view were hinted, but not explored or explained. (July 2000)
>
> Harrison wasn't even a "lone" genius. Although he faced unfair obstacles from some astronomers, the text notes that John Harrison was supported financially throughout his life by government-appointed board of astronomers. He was aided by his son, and found allies in important men such as Halley and King George. The "lone genius against all odds" certainly sounds like an exciting, inspiring story, but it wasn't that dramatic. There was 20,000 pounds at stake. Big surprise that Meskalyne [*sic*], a competitor, tried (ineptly, unlike a decent villain) to sabotage Harrison. I think you have to be pretty simple to find this story terribly inspiring. (August 1998)

In short, horse races sell. It is an open question whether other kinds of frames that represent the process better might not also sell.

CONCLUSION

The analysis of Sobel's *Longitude* suggests that the horse-race frame heightens attention inappropriately to certain aspects of scientific research. First, adopting the perspective of an individual scientist or inventor sets up inappropriate oppositions, such as hero-villain or individual-system. The account may accurately reflect the views and experiences of that individual, but it sidelines the broader scientific conversation on the problem. Second, the race frame focuses attention on a particular moment or "finish line" when a judgment of success is to be made, rather than favoring a dynamic, long-term perspective. This focus on the "moment" is also a tendency that I have critiqued in academic accounts of the rhetoric of science (Paul, Charney, & Kendall, 2001). Third, the structure of narrative accounts may work against the kind of summative or comparative analysis that is necessary for evaluating the merits of competing scientific approaches.

At the conclusion to a recent news article in the *Cincinnati Post* on the mixed messages emerging from cancer researchers on the value of mammograms, a spokesperson for the National Breast Cancer Coalition comments that "these controversies should spur women to insist that scientists focus on finding better ways to detect breast cancer and test them appropriately so such confusion doesn't happen again" (Neergaard, 2002). In this all too common popular view, if scientists cannot produce definitive results, then they are not doing their jobs properly. The horse-race frame may well encourage public expectations of fast, definitive breakthroughs, expectations that are doomed to disappointment.

Greg Myers (2003) gives a useful critique of the simplistic idea that if only the media portrayed science appropriately, the public would make better judgments. He argues against the "dominant view" of popularizations that condemn popular accounts as distortions of an original valid text. Rather, he recommends viewing academic and journalistic accounts as competing discourses and practices within a broader scientific terrain. And he argues that many factors influence the public's willingness to believe scientific findings, noting in particular the degree to which the public participates in the active construction of believable or discreditable identities for public authorities.

Of course, the public is not a passive receptacle for the media's representations. Increasingly, with fast electronic access to governmental and scientific websites, members of the public can seek out information from government agencies, think tanks, advocacy groups, and academic sources. Stakeholders in any public debate that involves scientific evidence are quite likely to gain access to multiple forms of information about science (Myers, 2003). Yet, in any accounting, the popular media have extraordinary influence on public views, particularly in the construction of character prototypes for public officials, agencies, and disciplinary groups. If one effect of the horse-race frame is to discredit "the system," as Sobel discredits the Board of Longitude, then these accounts may breed cynicism about the scientific community itself as well as any deliberative body.

More research on the extent of the horse-race frame in popular science and its effect on public perceptions of scientific disputes is certainly warranted. While I am arguing that popularizations can misinform the public, I recognize that this case has to be supported with many forms of representation and not solely with individual texts. Sobel's account (and mine) are not translations of an original scientific account, but constructions from (mainly) secondary historical sources. It is quite possible that the problems that I am pointing to occur mainly in certain kinds of book-length popularizations. I certainly am not suggesting that all popularizations are egregious misrepresentations. Many make contributions to knowledge in both public and academic communities. For example, Danette Paul (2002) has noted that James Gleick's (1987) popular book *Chaos* has played an important role in the formation and growth of the field of dynamic systems theory. Similarly, while Tracy Kidder's (1982) Pulitzer Prize winning book *Soul of a New Machine* follows the genre of an heroic epic (Katz, 1992), it provided important insights into the engineering design process that are still considered valid (Francis & Sandberg, 2000). And a scientist's own rendering of a line of research is also apt to follow a heroic, horse-race frame, as does James Watson's (1980) *Double Helix*. Watson, at least, is perfectly candid about the personal standpoint of the account; he delights in his own iconoclastic lack of disinterestedness. But less benign applications of the horse-race frame are not at all rare, even for more recent inventions. *New Yorker* writer Malcolm Gladwell (2002) recently criticized the "lone genius" framing of a popular account of the travails of an inventor of television.

The horse-race frame is hardly the only one available. Scholars in the humanities and social sciences have long since abandoned the agent-centered or "great man" approach in favor of a social constructivism that emphasizes the cultural, historical, and political forces that shape a discovery or invention. Sociologists of science, for example, have investigated the interactions among the public, commercial, and academic groups involved in designing and disseminating new technologies (Klein & Kleinman, 2002). Taking this kind of frame public would be more difficult than a simple heroic narrative, but it sure would make a great story.

REFERENCES

Atkinson, D. (1999). *Scientific discourse in sociohistorical context: The* Philosophical Transactions of the Royal Society of London, *1675-1975*. Mahwah, NJ: Lawrence Erlbaum.

Bazerman, C. (1988). *Shaping written knowledge*. Madison: University of Wisconsin Press.

Campbell, W. (1997). The worm and the tumor: Reflections on Fibiger's Nobel Prize. *Perspectives in Biology and Medicine*, *40*(4), 498-505.

Charney, D. (in press). Empiricism is not a four-letter word. In S. Selber & J. Johnson-Eilola (Eds.), *Central works in technical communication*. Oxford: Oxford University Press. (Reprinted from *College Composition and Communication*, *47*[4], 567-593, 1996)

Fahnestock, J. (1998). Accommodating science: The rhetorical life of scientific facts. *Written Communication*, *15*(3), 330-350. (Reprinted from *Written Communication*, *3*, 275-296, 1986)

Francis, D. H., & Sandberg, W. R. (2000). Friendship within entrepreneurial teams and its association with team and venture performance. *Entrepreneurship: Theory and Practice*, *25*(2), 5-25.

Gladwell, M. (2002, May 27). The televisionary: Big business and the myth of the lone inventor. *The New Yorker*, 112.

Gleick, J. (1987). *Chaos: Making a new science*. New York: Penguin.

Gould, R. (1960). *The Marine Chronometer*. London: Holland Press. (Original work published in 1923)

Howse, D. (1989). *Nevil Maskelyne: The seaman's astronomer*. Cambridge, UK: Cambridge University Press.

Jacob, M., & Reid, D. (2001). Technical knowledge and the mental universe of Manchester's early cotton manufacturers. *Canadian Journal of History*, *36*, 283-304.

John Harrison and the Longitude Problem. (2002, January 23). *Royal Observatory Greenwich*. Available from http://www.rog.nmm.ac.uk/museum/harrison/

Katz, S. B. (1992). Narration, technical communication, and culture: *The soul of a new machine* as narrative romance. In M. Secor & D. Charney (Eds.), *Constructing Rhetorical Education* (pp. 382-402). Carbondale: Southern Illinois University Press.

Kaufer, D. S., & Geisler, C. (1989). Novelty in academic writing. *Written Communication*, *6*, 286-311.

Kidder, T. (1982). *The soul of a new machine*. New York: Avon.

Kitcher, P. (1993). *The advancement of science: Science without legend, objectivity without illusions*. New York: Oxford University Press.

Klein, H. K., & Kleinman, D. L. (2002). The social construction of technology: Structural considerations. *Science, Technology and Human Values*, *27*(1), 28-52.

Lanham, F. (1998, Oct 4). Dava Sobel: The illustrated longitude. *The Houston Chronicle*, p. 5.

Mahoney, M. (1993). Longitude in the context of the history of science. In W. Andrewes (Ed.), *The quest for longitude*. London: Science Museum.

Myers, G. (1985a). The social construction of two biologists' proposals. *Written Communication*, *2*, 219-245.

Myers, G. (1985b). Nineteenth century popularizations of thermodynamics and the rhetoric of social prophecy. *Victorian Studies*, *29*, 35-66.

Myers, G. (2003). Discourse studies of scientific popularization: Questioning the boundaries. *Discourse Studies*, *5*(2), 265-272.

Neergaard, L. (2002, Oct 10). Mixed messages continue on breast cancer. *The Cincinnati Post*, p. B1.

Nelkin, D. (1995). *Selling science: How the press covers science and technology* (Rev. ed.). New York: Freeman.

Observations of latitude and longitude at all remarkable points. (1996, June 19). *An exhibition of maps and navigational instruments*. On view in the Tracy W. McGregor Room, Alderman Library, University of Virginia, July 10 to September 26, 1995. Retrieved from http://www.lib.virginia.edu/speccol/exhibits/lewis_clark/ch5.html

Paul, D. (2002). *Spreading chaos: The role of popular scientific literature in interdisciplinary diffusion*. Paper presented at 10th biennial meeting of the Rhetoric Society of America, Las Vegas, NV, May 25, 2002.

Paul, D., Charney, D., & Kendall, A. (2001). Moving beyond the moment: Reception studies in the rhetoric of science. *Journal of Business and Technical Communication*, *15*, 372-399.

Payne, A. A. (2003). The effects of congressional appropriation committee membership on the distribution of federal research funding to universities. *Economic Inquiry*, *41*(2), 325-346.

Popper, K. (1971). *The open society and its enemies* (2 vols., 5th ed.). Princeton, NJ: Princeton University Press.

Porter, T. (1995). *Trust in numbers: The pursuit of objectivity in science and public life*. Princeton, NJ: Princeton University Press.

Professions in 18th century Britain. (2000, February). History faculty lecture notes, Warwick University. Retrieved from http://www.warwick.ac.uk/fac/arts/History/teaching/courses/gender/prof.html

Reed, R. (2001). (Un-)Professional discourse? Journalists' and scientists' stories about science in the media. *Journalism*, *2*(3), 279-298.

Rowan, K. (1989). Moving beyond the what to the why: Differences in professional and popular science writing. *Journal of Technical Writing and Communication*, *19*, 161-179.

Sobel, D. (1996). *Longitude: The true story of a lone genius who solved the greatest scientific problem of his time* (paperback ed.). New York: Walker.

Sobel, D., & Andrewes, W. J. H. (1998). *The illustrated longitude*. New York: Walker.

Stewart, L. (1998). A meaning for machines: Modernity, utility, and the eighteenth-century British public. *The Journal of Modern History*, *70*(2), 259-295.

Sturridge, C. (Writer/Director). (2000). *Longitude* [Motion picture]. United Kingdom: Granada Film Production.

Waldman, P., & Jamieson, K. H. (2003). Rhetorical convergence and issue knowledge in the 2000 presidential election. *Presidential Studies Quarterly*, *33*, 145-164.

Watson, J. (1980). The double helix: A personal account of the discovery of the structure of DNA. In G. Stent (Ed.), *Norton critical edition*. New York: Norton.

Williams, C. J. (2001, December 2). Mistakes and oversights: Occasionally, those judging physics, chemistry and medicine are susceptible to the imperfections of human nature. *Los Angeles Times*, p. 40.

Davida Charney is a professor in the Division of Rhetoric and Composition at the University of Texas. Her work focuses on argumentation, the rhetoric of science, and the study of reading and writing processes. Her research has been published in such journals as Written Communication, Research in the Teaching of English, *and the* Journal of Business and Technical Communication.