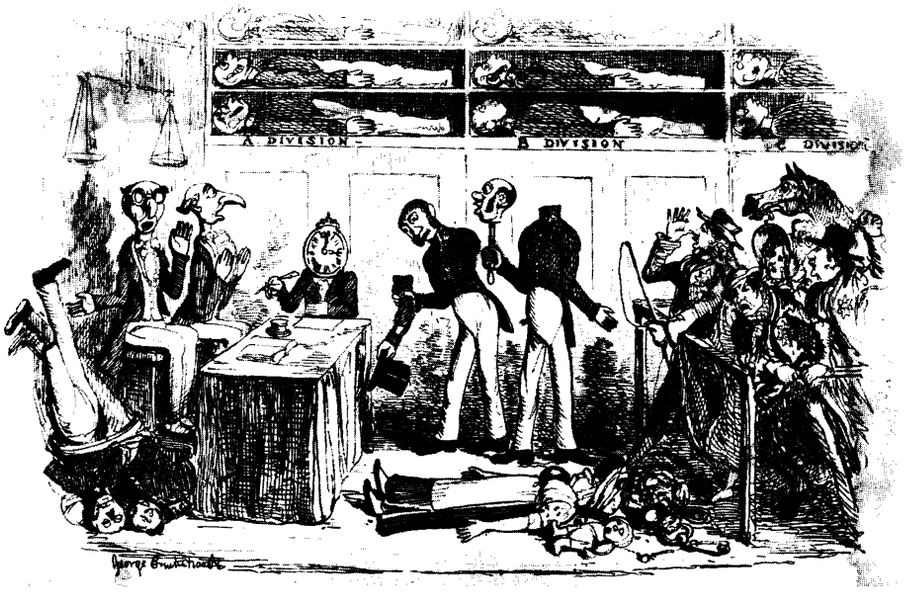

"A satire on both the pretensions of the [British Association for the Advancement of Science] savants displaying their mechanical wares (here automaton constables) and the police authority itself (note the jeering people). This 1838 cartoon [by George Cruikshank] accompanied a report sending up the 'Meeting of the Mudfog Association.' " [From *Bentley's Miscellany*, vol. 4, p. 209; reproduced in *The Politics of Evolution*]

devotion to an anatomical belief system that served to legitimize that struggle. Hostile to the teleological conservative views of William Paley and Georges Cuvier, they opted instead for the mechanistic outlook of Jean Baptiste Lamarck and Etienne Geoffroy Saint-Hilaire. Robert Grant, intellectual leader of the radical camp, imported to London such Continental ideas as a universal animal form, embryological recapitulation, arrested development, and species transmutation. Though not all of his followers endorsed transmutation, they admired his concept of self-generating life, dependent only on its organization for vital activity. Atheists took satisfaction in the materialism of such theories. The more numerous Dissenters preferred to regard them as signs of the inviolability of God's law. Either way, they opposed the voluntaristic teleology of Anglican conservatives. Wakley promoted Grant's views in *The Lancet*, and they received further endorsement in the *London Medical and Surgical Journal* and *British and Foreign Medical Review*, reform-minded journals that also took up the cause of the general practitioner in the 1830s.

The radicals did achieve some concessions. The University of London, created by the Whigs in 1836, granted degrees to Dissenters; and the Royal College of Surgeons accepted a more liberal charter in 1843. As leading critics like Marshall Hall and Richard Grainger were accepted into the ancient corporations, tensions relaxed; and though Robert Grant continued his opposition—at great personal financial cost—his influenced ebbed. Intellectually, he was outflanked by the young Richard Owen, who found a way to embrace a moderate version of philosophical anatomy uncontaminated by materialist overtones. Owen, deeply influenced by the idealist concepts of Samuel Taylor Coleridge, countered Grant's belief in universal type and recapitulation by drawing on the embryological work of Karl Ernst von Baer. Owen denied that there were any connections between Cuvier's four *embranchements* and insisted with Baer on a course of individual development that proceeded from a more general form toward the unique individuality of the species. At the same time, he set great store by the common homologies of animal morphology, rescuing them for respectable biology by emphasizing their basis in divinely conceived archetypes.

The general outline of Owen's thought is well known; but Desmond also analyzes the partisan character of his empirical studies of the platypus, the chimpanzee, and the Stonesfield "opossum." In each case Owen was able to reevaluate the organism in a way that made it seem an implausible link in a linear theory of evolution. Furthermore, Desmond shows that Owen won generous support from the Royal College of Surgeons for his outstanding catalogues of the College's Hunterian Museum, helping to defend the College from radical critics who accused it of neglecting this national trust. Eventually, Owen won generous patronage from the Conservative Peel government of the 1840s, largely in return for erecting an ideologically comfortable anatomy powerful enough to draw medical moderates away from the concepts of Geoffroy and Grant.

Until very recently, historians have tended to view Owen simply as a misguided opponent of Charles Darwin, and Desmond has done more than anybody to revise this simplistic view. His earlier study *Archetypes and Ancestors* (University of Chicago Press, 1986) gave a skillful account of the older Owen's contributions to evolutionary biology. Now he has produced a thoughtful assessment of the younger Owen too. However, this new assessment would have been impossible without his original investigation of the little-known radicals, malcontents, and Dissenters who occupied the lower rungs of London's medical hierarchy. In telling the story of their campaign for medical egalitarianism and their marching song of philosophical anatomy, Desmond has utterly revised a major chapter in the history of evolutionary thought, illuminating not only Owen's career but also those of Robert Chambers, Alfred Russel Wallace, and Charles Darwin. Previous historians have taught us about the reasons people once gave for believing in the unity of type; Desmond teaches us how it felt to think that way.

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Burt Again

The Burt Affair. ROBERT B. JOYNSON. Routledge (Routledge, Chapman and Hall), New York, 1990. xiv, 347 pp. $35.

On 24 October 1976, the London *Sunday Times* published a front-page story headed "Crucial data was faked by eminent psychologist." "The most sensational charge of scientific fraud this century," it began, "is being levelled against the late Sir Cyril Burt. . . . Leading scientists are convinced that Burt published false data and invented crucial facts to support his controversial theory that intelligence is largely inherited." The ensuing scandal forms the subject of Robert Joynson's book. Joynson's research is likely to inspire at least one more round in this controversy, for he argues that Burt has been unjustly maligned.

Even in a field as prone to public controversy as intelligence testing, the Burt affair forms an exceptionally dramatic and disturbing episode. Burt, a brilliant mathematician, author of *Factors of the Mind* (1940), and editor of the *British Journal of Statistical Psychology*, had been a pioneer in educational

psychology. His studies of intelligence also contained data on the largest number of identical twins raised apart—key evidence in debates between hereditarians and environmentalists. Knighted in 1946, Burt was Britain's most honored psychologist during his lifetime. As "long as psychology remains a subject of scientific inquiry," psychologist Leslie Hearnshaw noted in his eulogy following Burt's death in 1971, "he will live in its halls of fame" (p. 27).

By 1972, however, environmentalist Leon Kamin had begun to raise serious questions about Burt's science in answering hereditarian Arthur Jensen. Burt's articles on intelligence testing, Kamin observed, were suspiciously lacking in basic information, such as place, time, and type of test administered, and were filled with highly improbable coincidences, such as correlations that remained exactly the same—to three decimal places—even when sample sizes had more than doubled. By 1974, Kamin and Jensen had reached rare agreement: Burt's "correlations are useless for hypothesis testing," wrote Jensen; his numbers "are simply not worthy of our scientific attention," charged Kamin (p. 162).

Charging fraud was another matter. Kamin's writing, however, had interested *Times* reporter Oliver Gillie, who began searching for Burt's research assistants, Margaret Howard and Jane Conway. Finding no evidence of their existence, past or present, and being informed that these were probably pseudonyms invented by Burt, Gillie broke the story.

Caught in the ensuing crossfire was Hearnshaw, who at the time was working on a biography commissioned by Burt's sister. Hearnshaw agreed to examine the new charges, and when published in 1979 his study, *Cyril Burt, Psychologist*, described a gifted scientist whose early research had probably been genuine, but whose data, largely destroyed by wartime bombings, had probably been partly fabricated in postwar writings. Burt's personal and published papers since the 1940s, Hearnshaw concluded, suggested a pattern of deliberate deceit in claims about his role in his field's history, the quantity of new data collected, and the number of assistants helping him. In fact, of 40 different "authors" who published material in the journal Burt edited, over half may have been Burt himself, writing under pseudonyms. In light of Hearnshaw's findings, the British Psychological Society declared Burt guilty of fraud.

Joynson now proposes that the Burt investigation be reopened. The Society's actions, he argues, were premature, for the charges remain to be proven. Burt, he maintains, will be exonerated.

Joynson argues his case like a wily defense lawyer. Burt must be presumed innocent, he insists, until proven guilty beyond a shadow of a doubt. And the burden of proof must lie with the prosecution. Moreover, the standards of admissible evidence must be narrowed. Hearsay must be disallowed. Memories by contemporaries should be admitted only when accompanied by written documentation from the period in question. And all written evidence should be unambiguous. The defense has an easier task: it must prove only that one or more explanations besides fraud are possible.

Joynson's explanations for the many charges against Burt range from the plausible to the incredible. Burt, he argues, may simply have used outdated methods, or exercised poor judgment. Or he may have fallen victim to political enemies, jealous colleagues, or even a conspiracy of gossipmongerers. (At one point Joynson shows that several of those who now believe Burt guilty, including hereditarian Hans Eysenck, had once worked at Maudsley Hospital, where they may have heard unsubstantiated rumors about Burt's untrustworthiness.)

In this case, the best defense is a good offense. Joynson's main target is Hearnshaw's biography, which he blames for turning the tide against Burt and contends is full of errors and unproven assumptions. He focuses on four of Hearnshaw's main charges: that Burt changed the historical record; that his kinship studies, with their suspicious correlations and missing assistants, suggest fraud; that he lied about the sources for his final papers; and that he suffered from mental illness.

Joynson spends much time examining Burt's historical claims. Did Burt, as Hearnshaw contends, try to diminish Charles Spearman's contributions to factor analysis to promote his own? Hearnshaw's book documents Burt's pattern of distorting evidence to exaggerate his own priority; Joynson argues with him, footnote by footnote. Burt may have become "less deferential" toward Spearman, Joynson concedes, but this is hardly an indictable offense.

The kinship studies present a more central challenge, and here Joynson is most unconvincing. Critics have found Burt's published claims difficult to challenge precisely because he is so vague in supplying details. Nonetheless, of 64 correlation coefficients reportedly calculated on new samples in Burt's 1966 study, as many as 30 are overtly suspicious, for they repeat figures found in earlier articles. The most famous of these—an intelligence correlation of .771 reported in 1955 for 21 pairs of identical twins raised apart, and in 1966 for 53 pairs—Joynson believes may have been "a genuine coincidence" (p. 155). As for the others, Joynson proposes, Burt may simply have inserted old figures if he had no new data, without realizing the need to mention that these came from different samples. In any case, Joynson accuses critics of paying too much attention to Burt's suspicious figures and too little to the 34 that are new. After all, Joynson reasons, if repeated coefficients suggest fraud, then "by the same logic we must also now argue that the appearance of a new coefficient suggests that the data are genuine" (pp. 156–157). Such logic speaks for itself.

In considering Burt's "missing assistants," Joynson endorses an explanation offered by Burt's former colleague Charlotte Banks. Like Hearnshaw, Joynson believes that Howard and Conway may have been volunteer social workers whom Burt met before the war. Both also agree that Burt probably conducted no major new twin studies after 1950. The data in Burt's later papers, Joynson proposes, were prewar materials—materials misplaced in Burt's many moves, gradually rediscovered, and then published. This was possible, he argues, for Burt's secretary was, in Banks's words, "very accurate but couldn't file for toffee, and was very sensitive to criticism." "I am sure he would have promised her not to say the material had been lost," writes Banks (p. 180). To Joynson, this story suggests "possible answers" explaining postwar publications credited to Burt's prewar assistants.

Joynson's most serious charge against Hearnshaw concerns Burt's final papers. In 1969, Burt published test results reportedly gathered between 1914 and 1965 showing school performance declining. Hearnshaw quotes an interview that supposedly appeared in *The Guardian* in which Burt claimed that his tests had been given regularly to hundreds of schoolchildren—a claim Hearnshaw then proves to be a lie. Joynson, however, has been unable to locate any such interview. Such a charge must be answered. Nonetheless, even if found, Joynson argues, the interview will not prove decisive, since newspaper accounts are notoriously unreliable.

Joynson's final strategy is to discredit Hearnshaw's explanations for Burt's behavior—mental illness and childhood influences. There is no independent evidence, Joynson contends, that Burt suffered any mental instability. Moreover, whatever survival instincts Burt manifested had probably been learned not from the " 'gamin' subculture" that Hearnshaw claims he had known as a child living near the slums but from academic life, which "revolves around backbiting, innuendo, second-hand gossip, and abuse of confidence" (p. 256).

Such a defense leaves a strange effect.

Joynson may believe that he has exonerated his client; Burt, however, hardly leaves this courtroom with his reputation intact. Ironically, Joynson's "innocent" Burt emerges as an even less likable character than Hearnshaw's "guilty" Burt. Such a verdict, however, is acceptable to Joynson; Burt's methods may have been less than admirable, he argues, but they were short of criminal.

Joynson's arguments are sure to invoke detailed rejoinders from those now called "anti-Burters." Of more concern than Burt's posthumous reputation, however, is the broader question of standards of evidence, both scientific and historical, raised here. Joynson's research contains nothing to challenge the current consensus that, as scientific evidence, Burt's data are unacceptable. Moreover, even if one believed Burt innocent of conscious wrongdoing, the fact that such data were used in debates over educational policies and went unchallenged until the 1970s would still be scandalous.

Historians, however, can rarely invoke such strict standards in admitting evidence. Unfortunately, like Hearnshaw, they must often draw their conclusions from incomplete records, ambiguous writings, and the memories of contemporaries—the same materials Joynson uses to construct his alternative explanations. Burt may never have received his day in court; his place in history, however, must now be judged by the work he left behind.

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The Discourse of Primatology

Primate Visions. Gender, Race, and Nature in the World of Modern Science. DONNA HARAWAY. Routledge (Routledge, Chapman and Hall) New York, 1989. x, 486 pp., illus. $35.

This is a work of historical scholarship, assimilated to a visionary imagination. The author wants it "to be responsible to primatologists, to historians of science, to cultural theorists, to the broad left, anti-racist, anticolonial, and women's movements, to animals, and to lovers of serious stories" (p. 3). With so many diverse commitments and intended readerships, this is no ordinary scholarly study in form or content. Indeed, it should be seen as an ambitious response to the call within the well-established arena of the social study of science for new kinds of works that do full justice to the complexity of the construction of scientific knowledge and give a responsible critique of its authority. Works of such intent are more concerned with the mapping of the heterogeneous territory in which scientific knowledge is created than with its transformations through time. That is, connecting the work of scientists to its broadest resonances in culture and society might be emphasized at the expense of an orderly chronology of cause and effect.

There is at the same time a provocative questioning about the rendering of such a new kind of account: what are the limitations of external and of internal critiques of scientific authority? What kind of critique of a science might be powerful enough to alter the practices of the relevant scientists or to suggest viable new forms of inquiry? At what price in terms of cognitive shift and commitment to a Western scientific worldview? These are the sorts of questions that Haraway means to pose in her account of the science of primatology, poised between biology and anthropology and relevant to psychology and medicine as well.

Haraway's account can be read chronologically, but not without considerable breaks and distractions. The first three case studies, which can stand as independent essays, concern scientific treatment of primates before World War II: the career of the taxidermist Carl Akeley (periodized as 1908–1936), the creator of the American Museum of Natural History's African Hall; the career of Robert Yerkes (periodized as 1924–1942) and his laboratories for the study of primate biology and behavior; and the careers of C. R. Carpenter and S. A. Altmann (periodized as 1930–1955) and the emergence of field primatology.

However, Haraway's main interest in the book seems to be the development of post–World War II primatology in parallel with complex theoretical developments in physical anthropology, biology, and psychology. After a brief discussion of how primatology in part developed as *National Geographic* popular science, in which there is a vivid account of Jane Goodall's career, Haraway gives a detailed account of how a particular kind of physical anthropology was instituted through the success of Sherwood Washburn and his students. Since the process of institutionalization through networks she depicts underlies the present primatology research establishment in anthropology, this account is bound to be controversial.

Though there are two other interesting chapters in this section, on Harry Harlow and on the practice of primatology in Japan, India, and Africa, the chapter on Washburn and the "new physical anthropology" is key to the climax of the book, which consists of considerations of the work of four contemporary field primatologists: Jeanne Altmann, whose work is represented as centered on the "fundamental metaphor" of "dual career mothering"; Linda Marie Fedigan, from whose work "females previously consigned to a category of resource or matrix emerged . . . as active generators of lives and meanings"; Adrienne Zihlman, noted to be "a principal generator of a being called 'woman the gatherer' "; and Sarah Blaffer Hrdy, whose work is predicated on "the bedrock importance of competition, especially among females." It is with the project of feminist primatology, represented by the careers and writing of these figures, that Haraway herself is most sympathetic, and it is in this section of the book that her own commitments are most clearly expressed.

Indeed, the most striking feature of her text is Haraway's passionate statement and restatement of these claims. It is almost as if the episodes and bits of conventional history are platforms for the presentation of punchlines—stunning formulations progressively developed throughout the book. To me, these claims and the work itself rest on four foundations.

First, Haraway conceives scientific explanation and the production of knowledge as collective story-telling, the creation of narratives that are integrally related to other kinds of cultural narratives. Furthermore, she relies on the literary technique of allegory, whereby any story evokes other stories in the mind of its reader, to make the broad, sometimes startling range of associations between scientific discourses on primates and other kinds of cultural discourses. The intent here is to relativize scientific discourse and its authority so as to make it commensurate with other kinds of cultural phenomena. For example, as Haraway states (p. 377),

> *Primate Visions* is replete with representations of representations, deliberately mixing genres and contexts to play with scientific and popular accounts in ways that their "original" authors would rarely authorize. [It] is not innocent of the intent to have effects on the authorized primate texts in both mass cultural and scientific productions, in order to shift reading and writing practices in this fascinating and important cultural field of meanings for industrial and post-industrial people.

Second, the particular scientific narrative of primatology is constructed around a dualism between nature and culture. To ask how human are primates and how primate are humans has been a central dynamic of this science. Haraway's goal is to question the dualistic frames of thought, not only in primatology but more generally in the life, human, and cognitive sciences. As she puts it (p. 377),

> I am not interested in policing the boundaries between nature and culture—quite the opposite, I am edified by the traffic. Indeed, I have always