

Darvon: Effectiveness and Safety

C. W. Pettinga of Eli Lilly and Company contends (Letters, 6 Apr., p. 6) that parts of R. Jeffrey Smith's article, "Federal government faces painful decision on Darvon" (News and Comment, 2 March, p. 857), "are misleading and not objective." It is Pettinga's letter that is misleading and not objective.

Pettinga says Smith creates the impression that Lilly has acted irresponsibly in promoting and discussing Darvon (propoxyphene hydrochloride) through the use of certain words and quotations, such as that "by not calling Darvon a narcotic, Lilly was not informing physicians about its narcotic properties." Writes Pettinga: "These insinuations ignore the fact that pharmaceutical manufacturers must conform to Food and Drug Administration-approved labeling that requires full disclosure of a drug's therapeutic usefulness, limitations, and adverse reactions or side effects."

Pettinga later notes that the National Academy of Sciences-National Research Council's 1969 review of propoxyphene confirmed the efficacy of 65-milligram doses of Darvon, but he ignores what the reviewers wrote under general comments, item VIII, about Lilly's labeling (1):

Although the package insert is in general acceptable, with the reservations noted above, one disturbing feature comes to mind when the insert is considered as a whole. An obvious effort has been made to avoid pointing out that dextropropoxyphene is structurally closely related to the narcotic analgesics methadone and isomethadone, that its general pharmacologic properties are those of the narcotics as a group, that poisoning produced by dextropropoxyphene is essentially typical of narcotic overdose (complicated by convulsions) and should be treated as such, and that the distinction in dependence-producing properties and abuse liability between dextropropoxyphene and various other narcotics is essentially quantitative, rather than qualitative.

That this effort, unfortunately, appears to have been successful, is attested to by the fact that the majority of house staff and attending physicians who make liberal use of Darvon assume that its pharmacology is basically similar to that of aspirin or phenacetin, rather than to that of the narcotics.

Pettinga also asserts that "Effectiveness and safety are the two major reasons why billions of doses of Darvon have been prescribed for millions of patients. No amount of salesmanship and promotion could generate the continued sale and use of a drug that did not perform as expected by both the prescribing physician and the patient." The first as-

sertion is an unsupported claim. The second suggests that the first must be true on grounds that it is inconceivable that it might be false.

As evidence to the contrary, consider comments by Louis Lasagna in 1976 and 1964, respectively:

1) Offering a list of reasons that might explain propoxyphene's "remarkable success record," Lasagna concludes (2, p. 20), "... the drug has been effectively advertised—Darvon is a household word, part of the vocabulary of many people."

2) "One is at first puzzled at the enormous popularity of oral *d*-[dextro] propoxyphene in the United States, in view of its less than brilliant performance in controlled trials. This is less of a paradox than it seems, however. Like some compounds of even more dubious analgesic merit (ethoheptazine, carisoprodol), *d*-propoxyphene is sold not only alone but in combination with aspirin. Most *d*-propoxyphene is sold in combination with an 'ASA' preparation. Since aspirin is an excellent analgesic, and preparations containing *d*-propoxyphene can be obtained in the United States without a narcotic prescription, there are two obvious reasons for its popularity" (3, p. 75).

Finally, the hearings on propoxyphene chaired by Senator Gaylord Nelson (D-Wis.) were held by the Senate Select Committee on Small Business, not by a subcommittee.

GERALD D. STURGES
Professional Staff, Select Committee on Small Business, U.S. Senate, Washington, D.C. 20510

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Burt's Missing Ladies

May I, as the journalist who wrote the first story in which the word "fraud" was used in connection with the work of Sir Cyril Burt, congratulate D. D. Dorfman on his article about Sir Cyril Burt (29 Sept. 1978, p. 1177). I would like to offer some additional evidence that has emerged through inquiries made by the *Sunday Times* and others over the last two years. Some of this information has been published in newspapers (1-6),

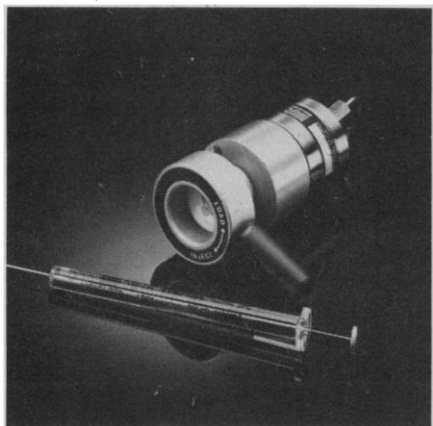
magazines (7-9), and in the *Bulletin of the British Psychological Society* (10, 11), where it is not readily available to American scholars.

The existence of Burt's research collaborators "Miss Margaret Howard" and "Miss J. Conway" has not been satisfactorily settled. Dorfman says, "it appears . . . that Howard did exist." However, if she did, she does not seem to have existed in the time and place referred to by Burt, nor did she write the papers that bear her name. And the lack of any evidence for the existence of Conway is even more important because it was she who was supposed to have gathered much of Burt's data.

I originally tried to trace Howard and Conway in order to check with them some of Leon Kamin's findings (12). The British Psychological Society (BPS) told me they had no record of either of these ladies, and two officials (13) volunteered their opinion to me that Howard and Conway were "pen-names" used by Burt. These officials told me that from time to time they had been approached for help in finding the ladies by researchers who sought permission to quote their papers (the custom in those days). The BPS officials said they had always referred the researchers back to Burt, who invariably said he was out of touch with Howard and Conway and gave permission for quotations on their behalf.

The BPS suggested I get in touch with Jack Tizard of the Institute of Education, London, who was also looking for the ladies and had approached several of Burt's old colleagues without success. I made additional checks at University College, London, and at the former London Day Training College, where Burt had held chairs, and also at London University (Senate House), where I found no records of Howard or Conway. Nor is there any record of their having been teachers in London state schools, although it is possible that they were associated with private schools. Furthermore, 18 of Burt's closest associates during each period of his life from the 1920's on knew nothing of the ladies (1), including those closest to him during his retirement, when the Howard and Conway papers were published. Advertisements in the [London] *Times* elicited no response, although such advertisements subsequently proved successful in tracing a deceased colleague of Burt's called Dorothy Wheeler (14).

Following the publication of these findings in the *Sunday Times* (1), John Cohen of the University of Manchester, a former student of Burt's, said that he



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had met a Margaret Howard in the psychology department at University College in the late 1930's (4, 6). Arthur Jensen later said (15) that Howard was a faculty member in the mathematics department of the University of London. Cohen never said this, and there is no evidence to support the claim.

After Cohen's report, I intensified efforts to find evidence of the existence of Howard and Conway—without success. I have written to more than 250 of Burt's former pupils and colleagues whose addresses were available from the British Psychological Association. Among some 100 who replied, none said they remember Howard or Conway. Burt refers to Howard's having mathematical expertise (16), but there is no record of a Margaret Howard graduating in mathematics from any university in the British Isles, Ireland, Canada, Australia, New Zealand, or South Africa at the relevant time. A Miss M. A. Howard, of 39 Brunswick Square, London WC1, is listed among the members of the British Psychological Society in 1924 (17), but she is not listed in earlier or subsequent lists. No further information about her is available because the records of the society were entirely destroyed by fire in 1946.

Recently Donald G. MacRae of the London School of Economics reported meeting a Miss Howard in 1949 or 1950 (18). She delivered to him corrected proofs of a journal article written by Burt. Nevertheless, a mystery remains. Burt told his housekeeper and secretary, Grete Archer, that Howard had emigrated when Archer asked Burt if she could send Howard reprints of the articles Howard had written with Burt (19). Burt also said that he did not have her address abroad. Furthermore, Archer says that she typed the papers published under the joint names of Burt and Howard and that Burt himself actually wrote them (19). During this period, 1952 to 1963, when Howard was said to be abroad and out of touch, Howard co-authored with Burt three full-length papers and a note and also published a full paper and three book reviews under her name alone (20).

By 1969 Burt was apparently in touch with Howard again, according to a letter he wrote to Douglas Pidgeon of the National Foundation of Educational Research (21, 22). However, Burt's secretary, Archer, never met Howard, and furthermore no correspondence with Howard and no reference to her in Burt's appointment diaries can be found (23). It is extraordinary that someone with such a long career in science cannot be identified with certainty, that no one has come

forward who actually knows who she was, and that no documentary evidence can be found for her existence.

Conway's case is even more curious. No one has emerged who knows anything at all about her. She is referred to by Burt in a paper of 1943 (24) but did not publish until 1958 and 1959 (25, 26), when she appears as sole author of papers in the journal edited by Burt, giving her address as Psychology Department, University College, London. She is referred to again in a paper of 1961 (27) and wrote two book reviews in 1959 and 1960 (28, 29). University College, London University, and the London Day Training College have no record of Conway. Further checks have proved more difficult because her first name is not known. Arthur Jensen has given her the name Jane (3), but I can find no documentary evidence for this forename. Those Miss J. Conways I have traced through records in other British universities have failed to have any connection with Burt.

Again Burt's papers contain no correspondence with Conway nor any record of appointments to see her. Burt told his secretary and others that Conway, like Howard, had emigrated. Archer understood they had emigrated before she joined Burt as housekeeper in 1950. Yet in 1955 Burt reported that Conway was collecting data and undertaking final computations (30). Between 1955 and 1958, when her first publication appeared, Conway seems to have doubled the number of separated identical twin pairs from 21 to 42 (25). Between 1958 and 1959, the number of twins analyzed appears to increase again by an unspecified number (26). By 1966, Burt had further increased the number of his separated monozygotic twin pairs by 11 to a total of 53, by now the largest sample in the world (31), although he himself had long before given up field work. This evidence for fraud has been pointed out by Ann Clarke of Hull University (5, 11), who with her husband Alan and Michael McAskie first suggested fraud in Burt's work.

The careers of Howard and Conway, outlined here, require explanation before credibility can be given to Burt's work. If these ladies did exist, and this now seems possible at least for Howard, the evidence suggests that they are not the people Burt said they were and that they did not do at least some of the things that he said they did.

I had no difficulty tracing other less-well-known students or associates of Burt's who are mentioned only in footnotes: for example, Miss Richardson, Miss Pelling, and Miss Molteno. Eliza-



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both Virginia Molteno is particularly interesting because Burt acknowledges her help, together with that of Howard and Conway, in finding twins (24). I have been in touch with Miss Molteno, now Mrs. Moody, who tells me that she never knew Howard or Conway; but even more curious, she never assisted Burt with his research work as Burt said she did, although she did study in his department and did publish work on twins with R. B. Cattell (32). This suggests the mechanism of the alleged fraud: Burt used the name of a real person and attributed work to her that she did not do.

Other evidence of fraud is not lacking. A third lady, M. G. O'Connor, whom Burt describes as an "Irish ex-student of mine" (33), cannot be found anywhere in Great Britain or Ireland. She was said by Burt to have assisted him with research on the the alleged decline in ability of London schoolchildren between 1914 and 1965, which was used in an article by Burt associated with an attack on comprehensive (nonselective) schools (22, 34).

Finally, crucial references that are supposed to supply details of Burt's experimental method cannot be traced. In the first major summary of his kinship studies (24, p. 89), published in 1943, for example, Burt refers to LCC (London County Council) reports, written when he was the LCC psychologist, as the source of his data. In a bibliography (35) based on information supplied by Burt and published in 1951, seven of these reports are listed as published by King and Son in the years 1914 to 1930. However, only three of these were ever published (in one volume), according to records of the LCC and King and Son (36).

Burt's whole corpus of work must now be suspect. Yet a little more than a year ago, his "classic" book *The Subnormal Mind* was republished (37) with a foreword by Hans J. Eysenck praising Burt's work. Elsewhere Eysenck has described Burt's work as "particularly valuable . . . because of the outstanding quality of design and statistical treatment of the studies" (38). Jensen described Burt's work as the "most satisfactory attempt to estimate the influence of heredity upon intelligence" (39) but later was among the first to point out errors (40). Burt's work has also been endorsed by at least three Fellows of the Royal Society: C. D. Darlington, J. L. Jinks, and J. A. Fraser Roberts; and has in addition had the backing of at least one Nobel laureate: William Shockley. And Cohen has described Burt as an "illustrious scholar" and a "polymath of Renaissance dimensions" (9). The time has now come to

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ask why Burt's work was looked at so uncritically by psychologists and others for such a long time. The answer might tell us something important about the role that power, charisma, and wishful thinking can play in bolstering support for scientific theory.

OLIVER GILLIE

Sunday Times,
Post Office Box 7,
Gray's Inn Road,
London WC1X 8EZ, England

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