Revista de Historia de la Psicología
1985, Vol. 6, Núm. 4, 287-316

SIR CYRIL BURT IN REVIEW: AN EMPIRICIST IN THE DOCK

KENNETH A. KAVALE
University of Iowa

CHRISTOPHER A. SINK
University of California
Riverside

INTRODUCTION

With the advent of behaviorism this century, a controversy developed regarding human intelligence further intensified. The longstanding question remained: Is intelligence a product of nature or nurture? Various psychologists (e.g., VERNON, 1979; EYSENCK, 1980; JENSEN, 1969) argued that intelligence was, for the most part, a product of heredity, and hence, genetic in origin. One man, Sir Cyril BURT, during his extensive career as a researcher in this area, contributed prodigiously to the resolution of the above inquiry. C. BURT's work was exemplary and served as evidence for the heritability (h^2) of the higher mental functioning ($h^2 = .80$). His studies spanned nearly 60 years, including a variety of subjects, but primarily in the field of intelligence.

Following BURT's death, many environmentalists began to intensify their clamour for recognition. One such individual, Leon KAMIN, reviewed BURT's publications and uncovered discrepant data and findings. This discovery was

heralded by the nurture side of the intelligence debate as revelatory of something far more fundamental. This faction now had greater justification to pronounce that the environment is the major impetus to the development of intelligence. With the outright rejection of BURT's voluminous data, this coalition has subsequently become more resolute in their theoretical stance.

In this paper, BURT's background, his contribution to the field of psychology, and the criticisms of his work and results will be investigated. The primary analysis will be of BURT's inconsistent empirical findings and his questionable dealing with a journal and his research associates. In addition, a review of the psychological community's reactions to BURT's contradictory behavior, various justifications for his misdeeds, and finally, closing remarks are forthcoming.

SYNOPSIS OF C. BURT's BACKGROUNDS AND FRAME OF REFERENCE

Cyril BURT was born March 3, 1883 in Westminster, England. The BURT family raised young Cyril in the same vicinity in which the MILTON family had brought up John, two centuries earlier. It thus appeared that even from a young age, C. BURT was destined for notoriety. His father was a London physician and earnest desired that his son would follow in his place. Cyril's ancestors included other physicians, church clergy, and mathematicians, one being Sir Isaac BARROW, the private tutor of Isaac NEWTON at Cambridge. His education was instituted at an early age, where as a young man, he was able to master Latin and various other classical languages (VALENTINE as cited by BANKS and BROADHURST, 1966).

Education and the pursuit of the "classics" was stressed explicitely by BURT's grandfather. It is no surprise that BURT obtained an Oxford scholarship in this discipline. He distinguished himself in the classics, philosophy and subsequently, in his primary subject, psychology. BURT also studied for four months at the University of Wurzburg, Germany. Here in Wurzburg, BURT by no means confined himself to the psychology of intellectual processes. He soaked himself in as much psychology as possible. He heard distinguished figures like von FREY on the sense organs, and K. BUHLER on the psychophysiology of education...(HEARNSHAW, 1979).

One significant development in BURT's burgeoning career was his association with Francis GALTON. He interacted with GALTON on several occasions, and became keenly interested in GALTON's statistical work in individual differences and psychology. From these early years with GALTON, BURT's later academic pursuits centered arount the genetic differences between persons. In addition, BURT labored under McDOUGALL at Oxford, and there, investigated mental capacities. BURT found himself in the company of scientists who were

researching intelligence; men such as FLUGEL, BROWN, and SPEARMAN.

BURT rapidly advanced through a variety of academic posts, beginning with a lectureship in psychology under SHERRINGTON at Liverpool University. Following his tenure in Liverpool, BURT served as an Educational Psychologist for the London County Council (L.C.C.). It is during this crucial period that BURT had access to thousands of student files in the London area. He was thus able to collect vast amounts of data for subsequent kinship studies (HEARNSHAW, 1979) Throughout this era BURT continued to be extremely ambitious and had earned a widespread reputation. With his increasing creditability came the highly prized faculty position at University College, London in 1931. Here, between 1931 and 1950, Cyril BURT gained the respect of colleagues worldwide and his publications were sought after by scholarly journals. It is also during this time and following his retirement from the faculty chair, that BURT served as chief contributor to the *British Journal of Statistical Psychology*. BURT's distinguished career ended in 1971, when he died fron cancer.

We will at this point review BURT's two major topics of empirical investigation: (1) intelligence tests; and (2) the heritability of mental capacities.

BURT's PRIMARY AREAS OF RESEARCH

HEARNSHAW (1979) described one of BURT's experimental thrusts in the following manner:

> BURT's own research was primarily focused on the problem of finding the most suitable kind of test for assessing intelligence, and, in particular, on determining whether tests involving higher and more complex mental functions might not show a closer connection with general intelligence than was shown by simpler mental function such as sensory discrimination and motor reactions (S→R theories) (p. 27).

In general, BURT desired to confirm SPEARMAN's mathematical methodology and his notion of a general factor ("g") of intelligence. This aim, in fact, was achieved. BURT never deviated from this concept; however, he also recognized and supported other theorists' contentions that there are group factors of intelligence.

Although BURT had no formal training in mathematics, he did, according to most statisticians and HEARNSHAW, his biographer, become a competent mathematician, specifically in the area of statistical methodology. He was one of the first to grasp the importance of PEARSON's correlational approach and actually made numerous technical improvements and extensions (e.g., multiple correlation, biserial correlations) of this statistical approach (HEARNSHAW, 1979.,

p.23). With these empirical techniques and others (e.g., factor analysis) BURT investigated the mental differences between sexes and the role of inheritance in determining mental characteristics.

The dogmatic confidence in the inheritance of mental abilities became the theoretical construct pivotal to BURT's notions of intelligence and his later research. In BURT's publication, "The Inheritance of Mental Characters" in 1912, he had already demonstrated his theoretical mastery and extensive knowledge of the biological and psychological literature. At this point, BURT began publishing material and correlations between intelligence of parents and children, social class, and family size.

As a disciple of GALTON, BURT's genetic conceptions of intelligence aligned themselves with the hereditarian position. BURT perceived a mental trait as "an innate, general, cognitive ability" (BURT, 1955, p. 162). One can summarize BURT's position on intelligence in his own remarks:

> I have now reviewed the wide variety of evidence -observational, introspective, and experimental, biological, physiological, and statistical, bearing on our initial question. The results are mutually supporting; and, appart from certain minor modifications or extensions, seem abundantly to confirm the threefold hypothesis that I tentatively put forward over forty years ago in the forerunner of this *Journal*: namely, that there is a general factor making for efficiency in all mental activities, that this factor is essentially cognitive or directive, and that the greater part of the individual variance (at least 75 per cent (BURT, 1955)) found in this factor is attributable to differences in genetic constitution This triple conclusion suggested a modernized formula for the abstract conception to which so many different writers had been lead, viz. 'innate, general, cognitive ability (1). If, therefore, we are to retain the word 'intelligence' as a technical term in psychology, this still seems to be the best definition (BURT, 1955, p. 176).

BURT thus put little credence in the concept that the environment plays a large or significant role in the development of intelligence (see KAMIN, 1979). If one uses careful selection of tests and systematic checks on one's results, BURT conceptualized that the effect of the environment can be virtually eliminated (BURT, 1966).

Finally, given that BURT was educated during the era of the Eugenics movement in England, this "genetic" perspective tended to influence all his subsequent work and hence, his findings (HEARNSHAW, 1979).

BURT's CONTRIBUTIONS TO PSYCHOLOGY

The achievements, and the subsequent accolade by the British monarchy of Cyril BURT, are numerous and impressive. In the first book presented to BURT in his honor, six years before his death, VALENTINE (as cited by BANKS & BROADHURST, 1965) recounted his contributions:

> He was already the first psychologist to apply Mendelian genetics to the study of mental inheritance, and his adoption of a multifactorial theory of heredity to account for the way mental abilities are distributed (notably in his studies of identical twins reared apart) will, if confirmed, have far reaching practical implications (p. 17).

BURT, in 1971, was awarded the American Psychological Association's highest honor, the Edward Lee Thorndike Award. Rarely had a foreigner earned such respect and admiration in American psychological circles as obtained by BURT. In fact, JENSEN (1969) introduced many of BURT's concepts into his investigations and relied substantially upon his findings. JENSEN (1969) wrote: "Probably the most distinguished exponent of the application of these methods to the study of intelligence is Sir Cyril BURT, whose major writings on this subject are a 'must' for students of individual differences (BURT, 1955, 1958, 1959, 1961, 1966; BURT & HOWARD, 1956, 1957)" (p. 33). It is precisely these above studies that we will later examine in terms of their empirical reliability.

JENSEN (1972, 1974) also clearly demonstrated his affinity for BURT's empirical procedures in this comment: "In the theoretical aspects of the applications of quantitative genetics to psychometric data, BURT was outstanding, ahead of all others of his time, as he was, too, in psychometric theory in general" (1974, p. 25). VERNON (1979), EYSENCK (1976, 1977, 1978, 1980), WADE (1976), and many other writers also reiterated BURT's contributions and defended his integrity.

BURT's accomplishments are most aptly reviewed by HEARNSHAW (1979) in these words:

> BURT, however, was a powerful theorist. He was extremely erudite, and had an extensive knowledge, not only of psychology and its history, but of many developments in contemporary science, both physical and biological. He had an expert grasp of the principles of statistics, and, as an applied psychologist by training, a sound insight into practical problems and practical usefulness of intelligence testing (p. 71).

HEARNSHAW (1979) further identified the major areas of "Burtial" influence, They are recited below:

A) In the field of mental testing, BURT was:

1) a pioneer in construction of verbal group tests of intelligence;
2) responsible for revisions and standarization of the BINET test for British use;
3) responsible for devising a widely used battery of scholastic achievement tests;
4) known for contributions in test technology; and
5) a leader in application of factor analysis to test results.

B) In the field of vocational, military psychology, and civil service, BURT's work was regarded as useful;

C) BURT was a forerunner in the "child-centered" approach to child guidance.

Moreover, HEARNSHAW (1979) argued that BURT's research as well as his widely accepted authority in the intelligence field, provided empirical support for the selectivity of the English educational system; and hence, he advised the committee which had earlier instituted the 11 examinations. In fact, KAMIN (as cited in EYSENCK & KAMIN, 1981) postulated that,

> on the strength of Cyril BURT's enthusiastic argument that a test given to a chield at the age of 11 could measure its 'innate ingelligence', it was decided to use the results of tests administered of 11-year-olds to 'stream' children into three separate -and far from equal- school systems (p. 94).

Although, BURT eventually regarded 11 years of age as too early for the use of selective testing (STEPHENSON, 1980) he continued to maintain that selective educational placement could be based upon "a doctrine that intelligence was mainly inherited" (HEARNSHAW, 1979, p. 121). "I.Q." consequently, acted as a substantial contributor in the selective educational system introduced after World War II. In 1944, the Educational Act was legislated and thereon affirmed the destiny of a multitude of children, chiefly on the basis of I.Q.

In brief, BURT believed that an adolescent should be psychologically assessed at or around age eleven in order that his/her "innate intelligence" might be measured. This might in turn delineate where the child would eventually attend school. If he scored in the elevated range, this individual may have been encouraged to seek advanced academic education, whereas the low achiever (i.e. low scorer) may have been counseled to pursue a vocational or technical field. These hypotheses lead to severe repercussions and eventually to their ultimate demise.

BURT's RESEARCH: A CRITICAL ANALYSIS

It is now appropriate to discuss the explosive controversy that arose over BURT's research methodology and his data. Leon KAMIN, a Princeton University

psychologist, known primarily for his analysis of the conditional reflex, and therefore, a relative "outsider" to the intelligence field ultil 1972 (WADE, 1976) began lecturing publically regarding the unsoundness of the empirical foundations of BURT's edifice. Although his views were widely disseminated prior to the publishing of his book, *The Science and Politics of I.Q.* (1974), the nature and the extent of BURT's flagrant research improprieties had seemingly gone unnoticed by the general psychological community. Two years hence, following the academic debate over BURT's discrepant data by JENSEN (1974), KAMIN (1974) and others, GILLIE, a medical correspondent (who incidentally, was a geneticist) published an article in the *Sunday Times* (1976, p. 1) claiming that Cyril BURT was in fact a fradulent individual. This journalistic piece sparked a public discourse that has not yet terminated. The issue at stake revolves arount BURT's extremely questionable results, and in turn, what effect the deletion of his evidence will have on the notion of the inheritability of intelligence.

The criticisms of BURT's studies are numerous and far reaching; as a result, we will attempt to adequately evaluate these following areas of dispute: (1) his method of data collection; (2) the actual findings; (3) the validity of BURT's major research associated, namely M. HOWARD and J. CONWAY; and finally, (4) the fashion in which BURT, as editor, controlled the *British Jounal of Statistical Psychology.* In addition, the rebuttals of various authors to many of these contentions will be included.

KAMIN's (1974, 1976) first assault on BURT's published studies can be subsumed under the heading of "inconsistencies in data collection and the imprecise reporting of those results'." KAMIN (1974) commented, "The papers of Professor BURT, it must be reported, are often remarkably lacking in precise descriptions of the procedures and methods that he employed in his I.Q. testing. The first major summary of his kinship studies, a 1943 paper, presents a large number of I.Q. correlations, but virtually nothing is said of when or to whom tests were administered, or of what tests were employed" (p. 36). JENSEN (1974) did, in fact, observe from most of BURT's publications that his kinship studies were formulated upon three types of mental measurement including "(i) a group test of intelligence containing both non-verbal and verbal times, (ii) an individual test of intelligence (the *London Revision of the Terman-Binet*) used primarily for standarization, and for doubtful cases); (iii) a set of performance tests, based on the *Pintner-Paterson* tests and standardized by Miss GAW (1925)" (as cited in BURT, 1966, p. 140).

The difficulty surfaced regarding BURT's reported I.Q. scores, because he "adjusted" these scores and published them under the subtitle of "final assessments'." BURT (1958) stated how he compiled the scores ("final assessments")

in these remarks:

> The final assessments for the children were obtained by submitting the marks from the group tests to the judgment of the teachers who knew the children best: where the teacher disagreed with the veredict of the marks, the child was interviewed personally, and subjected to further tests, often on several occasions. The assessments for the adult of the family were naturally far less accurate (p. 8).

It then became obvious that BURT employed "adjusted assessments" of I.Q. rather than raw test scores. BURT and HOWARD (cited in KAMIN, 1974) provided a rather unorthodox retort to critics who questioned them on this procedure by stating, "We...are perfectly willing to admit that, as a means of estimating genotypic differences, even the most carefully constructed tests are highly fallible instruments, and that their veredicts are far less trustworthy than judgments of the pupil's own teachers..." 9p. 40). JENSEN (1974) responded to BURT's I.Q. testing procedure by commenting, "But one may question whether the subjective element in this procedure is one that can be wholeheartedly recommended in scientific research on the genetics of mental abilities. Since it is not completely explicit, it cannot be completely objective, and therefore, not entirely repeatable by other investigators" (p. 5).

KAMIN (1974) further pointed out that BURT (1943), in fact, did not trust the judgments of experienced teachers as to their students' I.Q.s, but rather these scores should be obtained by objective intelligence tests. Subsequently, it appears that BURT (1958) changed his position and reverted to submitting the results to teachers' scrutiny. No rationale is provided from BURT for this discrepancy in testing procedure.

KAMIN (1974) also demonstrated how there is no technique of determining what examinations were administered to BURT's subjects (twins). BURT employed a group test of intelligence, which provided a high degree of reliability (.97) and was used over a 45-year period, but as KAMIN (1974) stated, "The test produced a twin correlation of .77, repeatedly, whether cases were dropped from or added to the sample. We cannot, however, locate the test." (p. 41). KAMIN continued, "We can only note that there is no way of knowing what test(s) he used, how well they were standarized, or how test scores might have been combined. We do not know what was correlated with what in order to produce the coefficient of .77."

HEARNSHAW (1979) and KAMIN (1974) also exhibited unmistaken evidence that BURT did not discuss other essential subject information and characteristics such as the sex of the twin pairs, the extent and duration of their separation, nor their precise age at testing. In fact, JENSEN (1974) quoted

BURT (1966) regarding how he determined the zygosity of the twins in the study. After examining the quote one discovers that for many pairs of twins (BURT does not specify the number), the ascertainment of whether they were monozygotic (MZ) or dizygotic (DZ) was based upon "the impressionistic judgment of an expert observer." BURT repeatedly used subjective techniques in order to delineate subject groups (MZ or DZ).

In terms of the methods in which BURT recorded the assessment of economic and cultural status and intelligence, KAMIN (1974, 1976) pointed out a number of conflicting and inconsistent statements. KAMIN (1976) presented the following regarding these above discrepancies in BURT's work:

> We should comment...on the unique virtue of BURT's study -the provision of quantitative socioeconomic class data. Here too there are problems. In 1959, when only forty-two separated twin pairs were available, it was clearly indicated that at least four children of 'professional' parents had been reared in 'orphanges' (CONWAY, 1959); but, in 1966, with the sample size increased to fifty-three pairs, it was reported that precisely two children of such parents had been reared in 'residential institutions.' Furthermore, a comparison of marginal totals in BURT's 1966 table with the individual socioeconomic data for the same twin which BURT later gave to JENSEN and SHOCKLEY, indicates that, in at least six cases, the classification of a twin was changed after 1966. The twin data were collected by BURT over a period of some fifty years, and it seems quite possible that assessments both of intelligence and socioeconomic class were subjected to a continuing process of revision and refinement (p. 249-250).

To lend credence to this argument, HEARNSHAW (1979) indicated that the two dullest twins were reported to have I.Q.s of 66 in 1966. The list mentioned by KAMIN, placed them with I.Q.s of 68 and 63. Also, the two brightest subjects had I.Q.s of 136 and 137 in 1966, were subsequently altered in this list to 131 and 132. Obviously, some of BURT's I.Q.s and social class ratings were discrepant over time.

JENSEN (1974) also demonstrated numerous careless errors made by BURT in reporting data. JENSEN (1974) summarized BURT's unreliable data collection methodology and the imprecision of the results tables in this annotation:

> The reporting of kinship correlations at times with and at times without noting the sample size, the rather inconsistent reporting of sample sizes, the higher than ordinary rate of misprints in BURT's published tables..., and the quite causal description of the tests and the exact procedures and methods of data analysis all stand in quite strange and marked contrast to

the theoretical aspects of BURT's writing in this field... (p. 25).

KAMIN (1981) in the book, *The Intelligence Controversy,* reaffirmed his convictions more emphatically than JENSEN: "With hindsight, it seems almost incredible than BURT's data could ever have been taken seriously. To begin with, BURT never provided even the most elementary information about how, where or when his purported data had been collected" (p. 99). Finally, BURT's biographer, HEARNSHAW (1979) struck the final blow to BURT's published results in this comment: "the data which BURT used for his calculation were poor and unreliable.." (p. 61).

INCONSISTENT KINSHIP CORRELATIONS

In light of the fact that BURT's data base is open to criticism, many writers have exposed inconsistent kinship corelations given the fluctuating sample sizes (N) so widely presented in BURT's publications. JENSEN (1974, 1978), KAMIN (1974, 1976) and McASKIE (1978) all have clearly demonstrated the faulty correlational analysis employed by BURT.

In 1943, BURT published the article, "Ability and Income", where he analyzed 156 pairs of DZ twins, reared together, that ultimately produced a correlation (r) of 0.54. In this study, he also reported 62 pairs of MZ twins, with 15 pairs having been reared separately. For those 47 MZ twins reared together, he calculated on $r = 0.86$ and the other 15 couples, the r was 0.77. BURT claimed that this correlations for twins had been published earlier in various London County Council (LCC) reports and in theses written by his students, but according to HEARNSHAW (1979) "no trace of such reports nor any reference to them in the authority's archives (was found). Nor did any of his postgraduate students work on twin material" (p. 130).

For the following 12 years BURT did not publish twin data under his own name until 1955, when the article entitled, "The Evidence for the Concept of Intelligence" was reproduced in the *British Journal of Educational Psychology.* In this work, he attributed much of the research to Miss CONWAY, his associate. In fact, Miss CONWAY was noted for supplementing BURT's data and "who had been responsible for the final computations" (BURT, 1955, p. 167). The identity and whereabouts of this female researcher will be analyzed later. TABLE 1 presents the relevant twin data given by BURT in 1955.

It can be observed that the N has increased in each of the three categories of twin pairs, but astoundingly enough, the r's for the DZ and MZ groups under the heading of "group tests" remained equivalent from BURT's 1943 correlations.

HEARNSHAW (1979) also indicated that, "The data were, indeed, never presented in a manner commensurate with their importance" (p. 230). Only extremely sketchy details were provided about the study's methodology. Consequently, can one rely on the similarity of correlations from the 1943 to 1955 papers? With the increasing sample sizes, one would expect at least modest changes in the correlations reported.

In 1958, with BURT's publication "The Inheritance of Mental Abilities," the correlations were repeated from the previous paper, but BURT now asserted he had collected "over 30" cases of MZ twins, who were reared apart. For this group of "over 30" MZ separated twins, the r's were listed as follows:

(1) Group Test of Intelligence ——.771; (2) Individual Test of Intelligence ——.843; and (3) the "Final Assessments" of Intelligence ——.876. Why each group's r's remained static, whereas the sample size significantly increased was not elucidated by BURT. There were no precise figures for the number of subjects in each group (i.e. MZ, DZ). It is likely that the correlations would remain invariant given the additional samples? JENSEN (1974), KAMIN (1974) and others, responded negatively to this question.

BURT himself did not publish twin data again until 1966, but CONWAY (1958, 1959), his alleged research associate, continued research efforts. She claimed to have inflated the total N for MZ twins reared separately to 42 pairs. In the 1958 work entitled, "the Inheritance of Intelligence and its Social Implications," the correlations for these 42 pairs was reported as 0.771. The consistency of this statistic remains intact throughout BURT's and CONWAY's papers.

Finally, in BURT's 1966 paper on "The Genetic Determination of Differences in Intelligent: A Study of MZ Twins Reared Together and Apart," the sample sizes were repeatedly altered and yet, many of the correlations were printed as if reproduced from his earlier findings. TABLE 2 presents the 1966 data in an abbreviated form.

HEARNSHAW (1979) adequately summarized the difficulties in these results: "By 1966 the MZ separated group had grow to 53 pairs, and there were changes reported in sizes of all the other groups, some by way of addition (MZ groups) and others, stragely, by way of subtraction (DZ group). In spite of changes reported in the sizes of the groups, (sometimes large, in the sizes of the groups,) the correlations in many cases remained identical to three places of decimals!" (p. 231).

JENSEN (1974) further clarified the inconsistencies in BURT's varying correlations and sample sizes and their effect on hypothesis testing in these comments:

> But the most serious problems with BURT's presentation of all these correlations are the often unknown, ambiguous, or inconsistent sample sizes and the invariant correlations despite varying Ns from one report to another. I count altogether no fewer than 20 pairs of invariant correlations for various kinships with differing Ns in each case. If the Ns are questionable, the standard errors of the correlations are necessarily in doubt, and without estimates of the standard error, ipso facto, the correlations are useless for hypothesis testing...I see no justifiable alternative conclusion in regard to many of these correlations. Hypothesis testing depends on data of determinate reliability (p. 24).

KAMIN (as cited in EYSENCK & KAMIN, 1981) stated his conclusions, "the I.Q. correlations that BURT claimed to have observed in his separated twins are quite literally incredible...The kinds of data collected by scientists in the real world simply do not behave with such incredible stability" (p. 101).

Writers such as JENSEN (1974) and KAMIN (1974) have attempted to reconstruct the original raw data to better understand BURT's published correlations. HEARNSHAW (1979) suggested these efforts were fruitless since a massive amount of information was destroyed during the WWII bombing of BURT's office in London. Without the actual data, the twin correlations must consequently be regarded as unreliable and not worthy of serious scientific consideration in measuring what effect genetics has on the heritability of intelligence.

If the previous contentions are accurate, can we be justified in deducing that BURT systematically fabricated his data to match his "genetic" predispositions? The question remains as to whether these empirical inconsistencies were merely oversights or were intentionally manufactured.

BURT's RESEARCH ASSOCIATES

Cyril BURT, throughout his long and productive career in the field of intelligence, relied heavily upon the diligence of two coworkers, M. HOWARD and J. CONWAY. Each of these women is cited, not only as co-authors to many of BURT's papers, but also as individual investigators in the area of intelligence. BURT alluded to at least one of these two women in most of his later studies from 1944 onwards (cf. BURT, 1955, 1957, 1958, 1966). BURT often referred to their findings as corroborating his own results.

Consequently, it was a startling revelation by O. GILLIE in the London *Sunday Times* of October 24, 1976, that perhaps BURT's so-called collaborators were untraceable. GILLIE purported that he could not locate any solid evidence

that BURT's two chief co-authors were in fact scientists. He stated (1976), "I must be considered a possibility that Margaret HOWARD and J. CONWAY never existed, but were the fantasy of an aging professor who became increasingly lonely and deaf" (p. 1). Two weeks hence, the *Sunday Times* (1976) reported that Miss HOWARD was at least not entirely a figment of BURT's imagination. In the 1930's she was, despite the newspaper's failure to locate her in the records, discovered to have, in fact, existed. JENSEN (as cited by GILLIE, 1979) argued that Miss HOWARD was a faculty member at the University of London. This contention was subsequently debunked by various investigators including HEARNSHAW (1970) and GILLIE (1979).

This second newspaper article seemed to pacify the psychological community, although the controversy regarding the two women's authenticity was far from being resolved. EYSENCK (1977), with the publication of the following story, regarded the identity mystery solved when he wrote in *Encounter* (1977):

> But first let us look more closely at the accusation. The first relates to the alleged non-existence of two of BURT's co-authors, both female, and both associated with him in the authorship of papers published in the *British Journal of Psychology* (edited at the time by Sir Cyril), Professor John COHEN, of Manchester University, has since written to say that he was in point in fact acquainted with one of the two women when he was at University College, as a student of BURT's and that Mrs Margaret HOWARD did indeed exist. This rather weakens the force of this particular charge. One also wonders how seriously Dr GILLIE took his task of investigation when he failed to contact Professor COHEN, known as one of BURT's most eminent pupils at the same time, or indeed myself, who was also one of BURT's pupils at the same time. I think I must conclude that this first charge is not only unfounded by carelessly amateurish and poor investigative procedure (p. 20).

VERNON (1979) and JENSEN (GILLIE & JENSEN, 1977) were also satisfied by Dr. COHEN's report of meeting M. HOWARD. JENSEN remarked in *The Education Digest,* "The speculation that Jane Conway and Margaret Howard were fictitious persons is already half debunked by the positive identification of Miss HOWARD (*London Times,* November 10, 1976)" (p. 45). And VERNON wrote the following: "In fact, HOWARD's credentials were verified..." (p. 72). The discussion, fortunately, did not cease these grand pronouncements on the part of EYSENCK, JENSEN and VERNON.

O. GILLIE (1979), the medical correspondent for the *Sunday Times,* responded in an article in *Science* (p. 1035-37), that in fact, the identities of the women remained a major source of controversy and mystery. GILLIE attempted

to trace HOWARD and CONWAY through the British Psychological Society (BPS). He was informed that there was no record of either of these ladies. It was suggested that these individuals were "pen-names" used by BURT in the course of his writings.

GILLIE (1979) made additional inquiries at the University College, London, and at the former London Day Training College, where BURT had held chairs, and at London University (Senate House), where he discovered no records of either HOWARD or CONWAY. There was no mention of these individuals as being teachers in London state schools. Undaunted, he contacted 18 of BURT's closest associates during the period of his life from the 1920's onwards. Only through an advertisement in the *Sunday Times* did GILLIE receive a response from Professor COHEN, as described previously.

GILLIE (1979) related his further attempt to locate HOWARD or CONWAY in these comments:

> 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 100 who replied, none said they remember HOWARD or CONWAY. BURT refers to HOWARD's having mathematical expertise, 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 WCI, is listed among the members of the *British Psychological Society* in 1924, but she is not listed in earlier or subsequent lists (p. 1036).

HEARNSHAW (1979) concluded the following regarding HOWARD's work: "There is, indeed, very little doubt that BURT was himself the author of HOWARD's contributions. There is the evidence of content and style; and on 7 April 1962 BURT gave the game away in a diary entry--'chiefly doing HOWARD's reply to ISAACS'" (p. 244).

Given that HOWARD has yet to be linked directly and positively with her own research, it is probable that a similar situation occurred with CONWAY. GILLIE (1979) made reference to the fact that no one has emerged who can even vaguely identify her person. She was mentioned in various articles by BURT, but after re-examining all available records, GILLIE (1979) deduced that CONWAY does not have any connection with BURT. Like HOWARD, GILLIE evaluated BURT's personal papers and found no correspondence with CONWAY, or was there any record of appointments to see her.

GILLIE (1979) reiterated his thoughts about these two research associates in this way:

> 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...(p. 1036).

HEARNSHAW (1979) continued the indictment upon these "fictious individuals" and BURT's conjuration by asserting:

> HOWARD and CONWAY were members of a large family of characters invented to save his face and boost his ego. No doubt this exercise, which other editors are known to have indulged in, tickled BURT's well-developed sense of humor, as well as very often providing him with excuses to expound his own views under his own name by way of reply. Finally, and most important of all, HOWARD and CONWAY enabled BURT to maintain the fiction that he was still actively engaged in research and in the collection of material of twins...this pretense of on-going research...from the diaries reveals as a complete fabrication (p. 245).

BURT AS EDITOR OF THE BRITISH JOURNAL OF STATISTICAL PSYCHOLOGY

As an editor of a journal, one's role is that of a monitor, accepting and revising those publications which are suitable to the genre of the periodical. The editor has seemingly the last word on what and how a submitted work will be reproduced. Obviously, such an individual has a great deal of power and influence over what is printed. With these facts in mind, BURT's role as an editor of the *British Journal of Psychology (Statistical Section),* which subsequently was transformed into the *British Journal of Statistical Psychology* will be analyzed. In 1947, BURT along with his professional colleague G.H. THOMPSON founded the first journal. With both scientists as joint editors, the aims of the periodical were stated as, "the publication of original or expository articles dealing with the following subjects: (a) quantitative methods in all branches of psychological research; (b) mathematical and statistical techniques for the evaluation of psychological data; (c) researches and results of which a main feature is the application of such methods..." (as cited by HEARNSHAW, 1979, p. 191).

The goals of this journal were highly objective, noble, and would appear to be responsive to all opinions on a specific issue under debate. The publication however was extremely predisposed toward one theoretical perspective; that

of, Sir Cyril BURT. HEARNSHAW (1979) expounded upon BURT's own contribution to the journal by stating:

> There was an over-loading of articles on factor analysis, and an excessive number of articles by BURT himself. Nearly one-third of the material in the first two volumes was contributed by BURT personally, and over the seventeen years when he was joint or sole editor, he supplied no fewer than 63 articles or long critical reviews under his own name, and almost certainly under pseudonyms...The journal became, and indeed had been in effect from the start, a vehicle for BURT, and he even referred to it as 'my journal' (p. 192).

EYSENCK (1980) provided a similar response regarding BURT's handling of "his journal": "He (BURT) continued to fill his journal with articles of his own, and apparently sometimes published his own articles under the names of other people, some of whom seemed to have been invented for the purpose. There is no doubt that BURT erred grievously in taking his duties as editor role rather lightly" (p. 184).

During the period when BURT and THOMPSON were co-editors, 1947 through 1954 (actually until 1950, when BURT assumed full control, nonetheless with THOMPSON's name attached (HEARNSHAW, 1979, p. 192)), 23 articles out of a total of 71 publications by BURT alone were reproduced in the journal. In 1954, because of THOMPSON's death, BURT became the sole editor, and the publication title was changed to the *British Journal of Statistical Psychology.* In 1957, BURT acquired another co-editor, J.W. WHITFIELD. Finally, in 1963, BURT turned over the editorship and virtually his total responsibility to R.J. AUDLEY.

By analyzing BURT's complete bibliography (HEARNSHAW, 1979), one uncovers some peculiar patterns regarding where he published his research and more importantly, his concept of the hereditability of intelligence. Examination reveals that the number of Burtian contributions published in the *British Journal of Statistical Psychology* far exceeded what would be normally accepted by any scholarly and reputable journal. When the editor changed, and therefore, the power base, to R.J. AUDLEY, BURT reproduced only four articles in this periodical. What this suggests in that BURT exercised dictatorial authority over the journal, and thereby could propagate his own theoretical positions without the threat of academic review and revision (HEARNSHAW, 1979; EYSENCK, 1980). Once his authority waned, his views had to be published in other journals. Although BURT did compose numerous papers on the subject of intelligence and the kinship research in "non-Burtian" journals, he used the *British Journal of Statistical Psychology* improperly and without regard for other scholarly rebuttals.

A significant question must be proposed. Why were BURT's works not thoroughly reviewed and investigated during his lifetime, given the aforementioned criticisms?.

THE LACK OF SCIENTIFIC SCRUTINY

For approximately 66 years, BURT managed to avoid any severe evaluation of his kinship studies, all the while his reputation continued to develop and expand. Not until L. KAMIN's (1974) devastating analysis, was BURT's work placed under close inspection. It appears that only post-mortem, was there any psychologist willing to openly rebuff the eminent BURT himself.

HEARNSHAW (1979) briefly touched upon the three possible explanations of the aforementioned dilemma. Firstly, BURT had demonstrated an impressive mastery of quantitative genetics for a couple of decades, and thus, became "the authority". In fact, P. VERNON, a major researcher in the area of intelligence, portrayed the prestige and dominance of BURT in this comment: "there were certainly grave doubts although nobody dared to put them in print, because BURT was enormously powerful...He would write a 50-page paper denouncing any criticism" (as cited in WADE, 1976, p. 918). With so much leverage over "his" own journal, BURT could employ his formidable prose style and statistical prowess to ridicule any threats. As evidence to his ability to intimidate and sway a protagonist, O'NEIL (1980) wrote this about BURT:

> As an Antipodean who met BURT only once (in 1952 and quite informally) but who had been reading from 1931 onwards much of his work, I formed the following opinions: he was a very learned man who irritatingly paraded his learning, a man with tremendous range and depth of intellectual penetration, but who was patently so vain, a man of great originality who was painfully assertive about his priority in various matters. I felt that he was not fairly representing his role in the development of factor analystic methods...but I attributed this to his vanity. It never occurred to me that he might be a cheat (p. 175).

With the control over a journal to lend credence to his theories and readily accessible channel to refute and denounce his opponents, the academic community, respecting BURT's influence and stature, seemingly trusted his reputation and therefore, his empirical findings.

The second reason put forward by HEARNSHAW (1979) on how BURT managed to deflect the critics was due to the nature and results of his research. BURT claimed to have accumulated a larger population of separated MZ twins than any other investigator, thus BURT's studies were unquestionably valuable.

KAMIN (1974) elucidated the importance of his findings:

> Professor BURT's work during a lengthy lifetime has had a major impact on all facets of the study of I.Q. heritability. There are, for example, various categories of kinship for which the only existing I.Q. correlations have been provided in BURT's publications. Those publications and those of his colleagues and students, are almost limitless in number. They furnish us with a veritable treasure of I.Q. data (p. 35).

Since NEWMAN, FREEMAN, and HOLZINGER (as cited by VERNON, 1979, p. 175) had reported 19 pairs of separated Mz twins, SHIELDS, 37 pairs, and JUEL-NIELSEN, 12 pairs, BURT's population (53) fell not far short of the total for the three studies combined. The correlation for these MZ pairs in relation to intelligence was also the loftiest ever reported ($r = .86$, individual test). With these provocative outcomes, BURT had finally demonstrated that the environments of twins were not substantially correlated; consequently their surroundings had little influence on intelligence.

Finally, in relation to the second explanation, KAMIN (as cited by WADE, 1976) and KAMIN (1974) hypothesized that since BURT's work was fundamental for the concept of hereditability of intelligence, it was not challenged; and thus, accepted without much debate. KAMIN wrote: "Every professor knew that his child was brighter than the ditch-digger's child, so what was there to challenge?" (as cited by WADE, 1976, p. 918). BURT simply pushed the hereditarian case to the forefront, which was subsequently explicated further by others such as JENSEN and EYSENCK. JENSEN's (1969) seminal article in the *Harvard Educational Review* employed, without hesitation, BURT's published conclusions and data to buttress his own theoretical conceptualizations. In short, KAMIN (1974) and GOULD (1981) indicated that BURT's "uncompromising hereditarianism" was central, and he suggested that BURT's colleagues were unconsciously privy to the same, because of their failure to expose his malefactions (STEPHENSON, 1980). BURT's studies went unquestioned by virtue of the fact that they "confirmed" what the hereditarians had earlier presumed to be the case.

WERE BURT's STUDIES INTENTIONALLY FRADULENT? THE RESPONSES OF THE SCIENTIFIC COMMUNITY

With KAMIN's (1974, 1976) expositions and JENSEN's (1974) exhaustive analysis, there were no longer any doubts regarding BURT's empirical irregularities and falsehoods. Although each author found comparable difficulties and inconsistencies, the conclusions reached about the motivation

for those difficulties were entirely different. For example, KAMIN (as cited in EYSENCK & KAMIN, 1981) announced, "The numbers left behind by Professor BURT are simply not worthy of our current scientific attention" (p. 47). Later, KAMIN (1981) further clarified this comment: "The clear implication -that BURT had invented the data in order to support his ideas about social and educational policy- was left for the reader to make" (p. 102). Thus, KAMIN advocated that we view BURT as an intentional fraud. Later, DANIELS (1976) agreed with KAMIN and remarked that BURT's data was "faked," and consequently JENSEN's work, founded upon BURT's research, now bears little weight.

JENSEN (1974) responded from the heredity camp in this statement about BURT's investigations: "It is almost as if BURT regarded the actual data as merely an incidental backdrop for the illustration of the theoretical issues in quantitative genetics, which, to him, seemed always to hold the center of the stage" (p. 25). JENSEN thus did not feel BURT was fradulent in his published reports, but BURT simply lacked attention to empirical detail, which seemed to be insignificant to BURT's overall message. Four years later, JENSEN (1978) further substantiated his assertion, "I found no evidence of such theoretical bias in BURT's errors, and I later offered as the most parsimonious explanation sheer carelessness on BURT's part, however damaging that interpretation certainly must be to his scientific reputation, and however incongruous it may appear in light of his superb technical command of psychometrics, statistics, and quantitative genetics" (p. 500). In sum, JENSEN continued to defend the man, BURT, and therefore, BURT's conclusions, although admitting the investigative inconsistencies.

HERRNSTEIN on July 16, 1973 (as cited by KAMIN, 1977), composed a letter to a reporter regarding KAMIN's expose of BURT's findings and immediately the attacks and counter-attacks began to surface. HERRNSTEIN in this letter accused KAMIN of being the "cheat," even though he was conscious of BURT's fallacious data. LOEHLIN, LINDZEY, and SPULHER (1975) also detected fault with KAMIN's judgments relating to BURT's work. They criticized him for exaggerating BURT's impropieties and for his exposition of the alleged discrepancies. FULKER (1975) affirmed these criticisms of KAMIN.

SCHWARTZ (1976) reiterated KAMIN's charges and went on to assert that the heritability of intelligence is zero. He pointed out that the majority of prior evidence for the genetic basis of intelligence, like BURT's, was extremely slanted and deceptive. SCHWARTZ (1976) stated: "The literature in question shows that the overwhelming majority of the investigators were

committed hereditarians". He continued, "The picture emerges of a group of committed individuals trying to show over a period of 70 years that I.Q. performance and its related measures are inherited. The tip of the affair is BURT's doubtful data" (p. 331). Obviously, SCHWARTZ's environmentalist presuppositions are implied in this pronouncement.

Following GILLIE's (1976) front-page story, KAMIN's (1974) book, and SCHWARTZ's (1976) article in *New Scientist,* EYSENCK, a former graduate assistant under BURT submitted his retort to the controversy. In a letter, EYSENCK (1976) asserted the following regarding SCHWARTZ's (1976) publication:

> The article by Dr. SCHWARTZ ("After Burt, what's left," 11 November, p. 330) suggesting that the discovery of certain inconsistencies and errors in the data published by Sir Cyril BURT (a discovery first published by Professor A. JENSEN in *Behavioral Genetics* in 1974), leaves no evidence for the widely accepted hypothesis that the broad heritability of I.Q. is somewhat between 65 percent and 90 percent, is disingenuous in the extreme...There are only some indications of the careless and bizarre nature of Dr. SCHWARTZ's argument; to maintain that all the available evidence is consistent with zero genetic variation in I.Q. performance is to throw away all scientific credibility (p. 488).

EYSENCK affirmed SCARR-SALAPATEK's review of KAMIN's (1974) book which claimed, "There ought to be a special corner in Marxist heaven for those who sacrifice their scientific reputation to their politics. No greater gift has any man..." (as cited by EYSENCK, 1976, p. 488). EYSENCK ultimately incriminated the entire work of those who significantly reduced the importance of the heritability of intellicence and BURT's data in this response:

> It is perhaps, revealing that the attack on the orthodox genetic view has been played out almost entirely on the pages of newspapers and popular magazines, not, as one would have expected, in professional journals (*Heredity, Behavioural Genetics,* etc.), and that it has been mounted by people with little standing in the specialities involved, and with no history of scientific contribution to the topic. Scientific readers will draw their own conclusion from these facts.

The controversy went public with GILLIE's (1976) front-page allegation that BURT fabricated data to fit the predictions of his favored genetic theories. McASKIE (1978), from the University of Hull, England, intensified the declarations

of GILLIE, by re-analyzing BURT's material. His aim was to determine if JENSEN's (1974, 1978) assertions (i.e. anomalies in BURT's kinship data were due to carelessness) had any merit. As a matter of course in the investigations, McASKIE (1978) observed: "A look at the error pattern suggests that it is not easily interpreted as carelessness, and an analysis of digital preferences, in those of BURT's figures that JENSEN reports, points more to invention than no genuine derivation" (p. 496). McASKIE (1978) continued these personal rebuttals and vendettas in his reply to JENSEN's defense of BURT:

> It is a great pity that JENSEN chose to write so illprepared a reply to the fraud allegations concerning BURT. JENSEN does not even appear to have applied some of the tools of his trade in trying to distinguish between fraud and carelessness. He had no right to suppose that people suggesting fraud were merely speculating, nor was he particularly informed about the background of the *Sunday Times* article by Oliver GILLIE or the political persuasions of those involved. "Sheer surmise and conjecture, and perhaps wishful thinking" are words that JENSEN was not in a strong position to throw accusingly at others on this issue (p. 498).

The polemics between the hereditarian school and the environmentalist position surged onwards regarding the nature and the explanation of BURT's malefactions. (For a sample of additional articles further examining this controversy, the reader is directed to these publications: BROAD & WADE, 1982; CLARKE & CLARKE, 1974, 1977; EYSENCK, 1980; FULKER, 1975; GOULD, 1981; RIMLAND & MUNSINGER, 1977; ROWE & PLOMIN, 1978; TIZARD, 1977). (See also *Association of Educational Psychologists Journal,* 1983, *6, 1.)*

The furor has somewhat quieted with the publication of a definitive biography by HEARNSHAW (1979). In this comprehensive book, BURT's life and work were reviewed. HEARNSHAW (1979) commented: "The veredict must be, therefore, that at any rate...beyond reasonable doubt, BURT was guilty of deception...He falsified the early history of factor analysis; he produced spurious data on MZ twins; and he fabricated figures on declining levels of scholastic achievement" (p. 259). Even though KAMIN (1974, 1976), CLARKE and CLARKE (1974) and others in the environmentalistic mold severely chastised BURT's work, HEARNSHAW (1979) does not dismiss him outright. He wrote: "It would disregard the assessments of contemporary experts in their appraisal of his work, and it would give insufficient weight to his many scholarly and practical achievements" (p. 259). How then does he account for BURT's deception and deliberate alteration of his data?

HEARNSHAW (1979) penetrated into BURT's psyche and personality to reconcile the contradictory behavior. He perceived BURT as product of his

era and thereby, conformed to its "genetic" undertones. HEARNSHAW suggested the hypothesis that BURT was a very lonely man, socially isolated individual, and yet, highly ambitious and a dominating personality. Mounting external threats and stress eventually led BURT to increasingly more irrational reactions. He cites six different events in BURT's life-span that triggered his "paranoia" and caused him to degenerate to the level of unscientific deception.

HEARNSHAW (1979) presented the first major setback as that of BURT's 20 year marriage succumbing to dismal failure. Apparently BURT would have nothing to do with his wife following the separation. HEARNSHAW (1979) believed that this breakup permanently damaged BURT. He explained the effect on BURT in this way: "There are good reasons to think that this was a heavy blow to BURT's pride. The expert in human nature had failed in the most intimate of human relationships. It was, too, a material blow, as he had hoped that his much younger wife, whose medical training he had helped to finance, would support him in his old age" (p. 275).

The second setback was the destruction of a large portion of his papers and research materials in the air raids of April, 1941. Without his raw data, it seemed unlikely that he could complete the twin studies in process. HEARNSHAW (1979) commented about this loss: "It rendered impossible the realisation of the ambitious he had set himself, and was for him a disaster of catastrophic dimensions" (p. 275).

The third calamity to strike BURT was the slow deterioration of his health. In 1941 he began to develop regressive symptoms of MENIERE's disease. This illness affected his hearing, and also his sense of equilibrium. From this year onwards his life had to be conducted with many restrictions. BURT became fearful of travel; and therefore, his public exposure was greatly curtailed.

The fourth area of difficulty came from his own academic department following his retirement. BURT strongly desired that his chair pass to one who would continue in the tradition of individual psychology. HEARNSHAW (1979) explained how BURT tried to manipulate this transition period: "He made vain attempts to thwart the changes introduced by his successor; and in the end had to be debarred altogether from the department. It was a humiliating defeat" (p. 275).

Fifthly, BURT lost control over the *British Journal of Statistical Psychology* in 1963. For sixteen years this had been his principal avenue for the publication of his findings and views. To have lost this was to remove his primary source of power and prestige. He fervently desired to remain in authority, but in the end, BURT relinquished the editorship at the dictates of the British Psychological Society.

The final blow to BURT's edifice was aptly expounded upon in HEARNSHAW (1979):

> The final setback was the gradual erosion of the system of selective secondary education BURT had done so much to promote, the widespread abandonment of the use of intelligence testing at the age of eleven plus, and the questioning of the whole complex of ideas on which selective education had been based... BURT still thought himself right; but more and more he seemed to be engaged in a rearguard defense of a rejected cause, more and more to be surrounded by left-wing critics and by psychologists who had been infected by behaviourist and environmentalist heresies (p. 276).

In summary, the multiple setbacks to BURT's life may have had a devasting influence on his personality and self-esteem. BURT, finding himself deminishing in personal and academic predominance, became susceptible to the "regressive changes in the personality, and a recrudescence of earlier patterns in behaviour, which began to obtrude both in his personal relationships and in his published work" (HEARNSHAW, 1979, p. 284). Although SAMELSON (1980) disagreed with the psychopathology concept, EYSENCK (1980) concurred with this explanation by stating: "I think it is easier to forgive Sir Cyril his misdeeds when we understand the personality structure which gave rise to his behaviour" (p. 187). It then appears that HEARNSHAW (1979), EYSENCK (1980) and O'NEIL (1980) all believed that BURT's fradulent actions can be accounted for via his "psychopathology."

The debate has not terminated with HEARNSHAW's work. For authors such as BROAD and WADE (1982), CHOWN (1980), CLARKE and CLARKE (1980), GOULD (1981), KELLOGG (1982) and MACKINTOSH (1980), the motive for BURT's fraud continued to be discussed and reviewed. For example, KELLOGG (1982) added a provocative twist to this continuing drama. He theorized: "Cyril Burt's reading of Sherlock Holmes may have contributed to his intense belief in the genetic origins of human behavior" (KELLOGG, 1982, p. 69).

GOULD (1981), in the book *The Mismeasure of Man,* hypothesized that the course of BURT's male factions were rooted in his hereditarian bias and the reification of intelligence as a single, measurable entity. Subsequently, GOULD writes:

> I will demonstrate that BURT's hereditarian argument had no foundation in his empirical work (either honest or fraudulent), and that it represented an a priori bias imposed upon the studies (BURT's) that supposedly proved it. It also acted, through BURT's zealous pursuit of his idée fixé, as a distorter of judgment and finally an incitement to fraud (1981, p. 274).

Finally, BROAD and WADE (1982) seemed to portray BURT as a "master of deception" in these remarks:

> But the man who impressed JENSEN with his nobility of intellect possessed a grievous flaw: he was a cheat. He invented data out of whole cloth to support his own theories and confound his critics. He used his making of statistics and gift of lucid exposition to bamboozle alike his bitterest detractors and those who acclaimed his greatness as a psychologist (p. 204).

CONCLUSION

A review of literature regarding Cyril BURT's life, work, and his malefactions has been provided. It is unfortunate that BURT's spurious results were not thoroughly analyzed prior to his death, for without his input one cannot adequately resolve these contradictions. The publication of the HEARNSHAW (1979) biography of BURT has clarified many of the questions regarding his life and influence in the field of intelligence and its inheritability. A lingering difficulty continues to remain Did BURT consciously attempt to manipulate and modify his data to make it more compatible with his genetic predisposition and other studies in this area? Although there was an attempt to reveal crucial evidence of BURT's wrongdoing, the many remaining significant problems makes it difficult to speculate on the motivation underlying BURT's transgressions.

Speculation resolves around the tenets of either the environmentalist camp or the genetic school of intelligence. If BURT intentionally altered the data, KAMIN thus claims that perhaps much of the entire collection of results on the high heritability of intelligence may be dubious. Understandable, KAMIN fervently desires this to be so, in order that his position may be further strengthened. On the other hand, if BURT did not perpetrate "the fraud" with malicious intent possibly because of psychopathology, then the genetic persuasion would feel vindicated. EYSENCK (1980, 1981) reluctantly acknowledged BURT's direct falsifications, but attributed them not to the unscientific desire to support his perspective on intelligence, bur rather to undue stress in BURT's life. This anxiety turned inward and manifested itself in the many technical errors.

The debate will continue until both opponents realize their particular prejudices and how they malign their empirical objectivity. It is probably correct to suggest that the scientific evidence for the high correlation between intelligence and heredity appears to be relatively sound and worthy of merit (VERNON, 1979). The strong correlation found in numerous publications must be reckoned with from the environmentalist position. Without considering BURT's work in the argument, these studies remain as a testimony to his original

premises regarding the genetic basis of intelligence.

Finally, two major weaknesses in the scientific process itself emerge. SAMELSON (1980) reiterated the need for two aspects in psychological research to be further reinforced. We discussed the imperative requirement for all studies to be internally and externally subject to, first, criticism and review, and second, to replication. He wrote the following regarding these processes and BURT's works: "Still, at the technical level we have to admit, and face up, to some instances of failure of two major mechanisms that supposedly safeguard the integrity of scientific knowledge: (public) critical analysis and replication. To shrug them off as unfortunate accidents of the past seems too easy and ignores their impact on the world beyond the scientific discipline" (p. 623). EYSENCK (1981, p. 692) belittled these conclusions and claimed that the psychological world should "cease beating our breasts in this curiously masochistic fashion about discrepancies in minor details...as in BURT's case..." But BURT's research was extremely influential and to describe it as only "minor" is devoid of any objective and empirical validity, precisely what SAMELSON desired the psychological fjeld to acquire. In short, if an issue, such as the heritability of intelligence, becomes one where various factions cannot apply the scientific principles of review and replication, that debate is little more than psychologists exhibiting their "ego-strengths."

SUMMARY

For over a half century, Sir Cyril BURT explored the nature-nuture controversy through a series of twin studies. As a primary contributor to the notion of the heritability of intelligence, many modern-day psychological theoreticians point to his work as either exemplary of misguided idealism or blatant forgery. The purpose of this paper is to review BURT's historical roots, philosophical underpinnings, and empirical findings in an attempt to understand the present controversy surrounding his work. Additionally, this paper will also review the psychological community's reactions to BURT's contradictory behavior, and summarize the various justifications for the alleged misdeeds. It was concluded that BURT's empirical malefactions were real; and hence, demonstrate an inner drive for power within the academic community. Although the impact of BURT's investigative and personal improprieties on the belief that intelligence is primary genetic in origin continues to be speculative. This paper reinforces the need for critical analysis and replication of all scientific research.

RESUMEN

Durante más de medio siglo, Sir Cyril Burt exploró la controversia "nature-nurture" a través de una serie de estudios con gemelos. Como uno de los principales contribuyentes a la noción de heredabilidad de la inteligencia, muchos teóricos actuales señalan su trabajo como un modelo de un idealismo equivocado o como una descarada falsificación.

El propósito de este artículo es revisar las raíces históricas de Burt, sus fundamentos filosóficos, y datos empíricos en un intento de comprender la actual controversia en torno a su trabajo. Además, se revisan las reacciones de la comunidad psicológica a la contradictoria conducta de Burt, y se resumen las diversas justificaciones para sus supuestas fechorías. Se concluye que las falsificaciones empíricas fueron reales; y demuestra además un impulso interno de poder dentro de la comunidad académica, si bien el impacto de las incorrecciones personales y de investigación sobre la creencia de que la inteligencia es primariamente genética en su origen continúa siendo especulativa, este artículo enfatiza la necesidad de llevar a cabo un análisis crítico y una réplica de cualquier investigación científica.

BIBLIOGRAFIA

BANKS, C. & BROADHURST, P.L. (Eds.): *Stephanos: Studies in psychology presented to Cyril Burt.* London. University of London Press, 1965.

BROAD, W. & WADE, N.: *Betrayers of the truth.* New York. Simon and Schuster, 1982.

BURT, C.: "The inheritance of mental characteristics". *Eugen. Rev.,* 1912, 4, 168-200

BURT, C.: "The evidence for the concept of intelligence". *British Journal of Educational Psychology,* November 1955, *25,* 158-177.

BURT, C.: "The distribution of intelligence". *British Journal of Psychology,* 1957, *48,* 161-175.

BURT, C.: "The inheritance of mental ability". *American Psychologist,* 1958, *13,* 1-15.

BURT, C.: "Class difference in general intelligence". *British Journal of Statistical Psychology,* 1959, *12,* 15-33.

BURT, C.: "Intelligence and social mobility". *British Journal of Statistical Psychology,* 1961, *14,* 3-24.

BURT, C.: "The genetic determination of differences in intelligence: A study of monozygotic twins reared together and apart". *British Journal of Psychology,* 1966, *57,* 137-153.

BURT, C. & HOWARD, M.: "The multifactorial theory of inheritance and its application to intelligence". *British Journal of Statistical Psychology,* 1956, *9,* 95-131.

BURT, C. & HOWARD, M.: "Heredity and intelligence: A reply to criticisms". *British Journal of Statistical Psychology, 1957, 10,* 33-64.

CHOWN, S.: "Review of *Cyril Burt, Psychologist,* by L.S. Hearnshaw". *British Journal of Psychology,* 1980, *71,* 171-172.

CLARKE, A.D.B. & CLARKE, A.M.: *Mental Deficiency.* 3rd ed. London. Methuen, 1974.

CLARKE, A.M. & CLARKE, A.D.B.: "Sir Cyril Burt". *Bulletin of the British Psychological Society,* 1977, *30,* 83-84.

CLARKE, A.M. & CLARKE, A.D.B.: "Review of *Cyril Burt, Psychologist",* by L.S. Hearnshaw. *British Journal of Psychology,* 1980, *71,* 172-173.

CONWAY, J.: "The inheritance of intelligence and its social implications". *British Journal of Statistical Psychology,* 1958, *11,* 171-190.

CONWAY, J.: "Class difference in general intelligence". *British Journal of Statistical Psychology,* 1959, *12,* 5-14.

DANIELS, N.: "I.Q., intelligence, and educability". *Philosophical Forum,* 1976, *6,* 56-69.

EYSENCK, H.J.: "After Burt". *New Scientist,* November 25, 1976, *72,* 488.

EYSENCK, H.J.: "The case of Sir Cyril Burt: On fraud and prejudice in a scientific controversy". *Encounter,* January 1977, *48,* 19-24.

EYSENCK, H.J.: "Sir Cyril Burt and the inheritance of the I.Q.". *New Zealand Psychologist,* 1978, *7,* 8-10.

EYSENCK, H.J.: "Burt and the inheritance of intelligence: Evaluation of a controversy". *Zeitschrift fuer Differentielle und Diagnostische Psychologie,* 1980, *3,* 183-199.

EYSENCK, H.J.: "On the fudging of scientific data". *American Psychologist,* 1981, *36,* 692.

EYSENCK, H.J. & KAMIN, L.: *The intelligence controversy.* New York. Wiley & Sons, 1981.

FULKER, D.W.: "Review of *The Science and Politics of I.Q.", by L.J. Kamin. American Journal of Psychology,* 1975, *88,* 505-519.

GILLIE, O.: "Crucial data was faked by eminent psychologist". London. *Sunday Times,* 24 October 1976.

GILLIE, O.: "Burt's missing ladies". *Science,* June 8, 1979, *204,* 1035-1038.

GILLIE, O. & JENSEN, A.: "Did Sir Cyril Burt fake his research on heritability of intelligence?: The charge and a defense". *The Education Digest,* March 1977, *42,* 43-45.

GOULD, J.S.: *The mismeasure of man.* New York. W.W. Norton & Co., 1981.

HEARNSHAW, L.S.: *Cyril Burt, Psychologist.* London. Hodder & Stoughton, 1979.

JENSEN, A.R.: "How much can we boost I.Q. and scholastic achievement?. *Harvard Educational Review,* Winter 1969, *39,* 1-123.

JENSEN, A.R.: "Sir Cyril Burt". *Psychometrika,* 1972, *37,* 115-117.

JENSEN, A.R.: "Kinship correlations reported by Sir Cyril Burt". *Behavior Genetics,* 1974, *4,* 1-28.

JENSEN, A.R.: "Burt in perspective". *American Psychologist,* May 1978, *33,* 499-503.

JINKS, J.L. & EAVES, L.J.: "I.Q. and inequality". *Nature,* March 22, 1974, *248,* 287-289.

KAMIN, L.J.: *The science and politics of I.Q.* Potomac, MD: Lawrence Erlbaum, 1974.

KAMIN, L.J.: "Heredity, intelligence, politics, and psychology: I". *The I.Q. controversy: Critical readings.* New York. Pantheon Books, 1976.

KAMIN, L.J.: "Burt's I.Q. data". *Science,* January 21, 1977, *195,* 246-248.

KELLOGG, R.L.: "The case of the suggestible psychologist". *Psychology Today,* August 1982, *16,* 69-70.

LAWLER, J.M.: *I.Q. heritability and racism.* New York. International, 1978.

LOEHLIN, J.C., LINDZEY, G. & SPUHLER, J.N.: *Race differences in intelligence.* San Francisco. Feeman, 1975.

MACKINTOSH, N.H.: "Review of *Cyril Burt, Psychologist*", by L.S. Hearnshaw. *British Journal of Psychology,* 1980, *71,* 174-175.

MCASKIE, M.: "Carelessness or fraud in Sir Cyril Burt's kinship data?: A critique of Jensen's analysis". *American Psychologist,* May 1978, *33,* 496-498.

O'NEIL, W.M.: "Review of *Cyril Burt, Psychologist*", by L.S. Hearnshaw. *British Journal of Psychology,* 1980, *71,* 175-176.

RIMLAND, B. & MUNSINGER, H.: "Burt's I.Q. data". *Science,* January 21, 1977, *195,* 248.

ROWE, D. & PLOMIN, R.: "The Burt controversy: A comparison of Burt's data on I.Q. with data from other studies". *Behavior Genetics,* 1978, *8,* 81-83.

SAMELSON, F.J.B.: "Watson's little Albert, Cyril Burt's twins, and the need for a critical science". *American Psychologist,* 1980, *35,* 619-625.

SCHWARTZ, J.: "After Burt". *New Scientist,* November 11, 1976, *72,* 330-331.

STEPHENSON, W.: "Professor Kamin, Sir Cyril Burt, and hyperbole". *American Psychologist,* 1980, *35,* 1144-1146.

TIZARD, J.: "The Burt affair". *University of London Bulletin,* 1977, *41,* 4-7.

VERNON, P.E.: *Intelligence, heredity and environment.* San Francisco. W.H. Freeman & Company, 1979.

WADE, N.: "I.Q. and heredity: Suspicion of fraud beclouds classic experiment. *Science,* November 26, 1976, *194,* 916-919.

TABLE I: CORRELATIONS FOR BURT's 1955 STUDY

(N) No OF PAIRS	ZYGOSITY	ENVIRONMENT	INTELLIGENCE CORRELATIONS FOR: GROUP TEST	INDIVIDUAL TEST	FINAL ASSESSMENTS
83	MZ	Reared Together	.944	.921	.925
21	MZ	Reared Apart	.771	.843	.876
172	DZ	Reared	.542	.526	.551

TABLE 2: CORRELATIONS FOR BURT's 1966 STUDY

(N) No OF PAIRS	ZYGOSITY	ENVIRONMENT	INTELLIGENCE CORRELATIONS FOR: GROUP TEST	INDIVIDUAL TEST	FINAL ASSESSMENTS
95	MZ	Reared Together	.944	.918	.925
53	MZ	Reared Apart	.771	.863	.874
127	DZ	Reared Together	.552	.527	.453