

Intensive, Detailed, Exhaustive

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Arthur Jensen's bibliography is characterized as breathtaking and his scientific work as intensive, detailed, exhaustive, fair-minded, temperate, and courageous. Specific articles and books are targeted as must reading. I argue that Jensen's characterization of the influence of the Berkeley psychology department in the 1940's reflects his own intellectual biases rather than those of the department. Jensen's work is praised as an extension of the British Biological-Theoretical Tradition which attempts to integrate psychological, biological, social genetic, sociological, and cultural processes in a coherent theoretical framework. A new definition of Jensenism, based on the Jansenist heresy, is provided.

Upon reading both Arthur Jensen's bibliography and his new book, "The g Factor: The Science of Mental Ability", in the same week only one word comes to mind—"breathtaking". Reading his bibliography is as much a delight as reading his books and papers, truly an intellectual feast. As a fellow Galtonian I will point out a few facts the casual reader might miss if they did not count items and have not read much of Jensen's work; a) he is the first author on 357 of the 384 items, b) he is the sole author of 319 of the 384 items, c) he has four citation classics, d) he has published nearly 10 items a year (including books) since 1962, e) there is no indication that he is slowing down, and f) the quality is not only superb, it is getting better! One disconcerting feature of the bibliography is the paucity of items that have been reprinted. I was stunned, for example, to see the classic 1977 article, "Cumulative deficit in IQ of blacks in the rural South", reprinted only once. It is still the definitive paper on the topic. I suspect that the reason so few papers have been reprinted is the same one that has resulted in his not having been given the numerous honors other scientists of his stature have already received. He has dared to study and speak straight forwardly about important issues that most other social scientists only whisper about—race and class differences in IQ, lack of bias in intelligence testing, the biological basis of general intelligence, genetic influences on intelligence, and fallacious research methods in developmental psychology. This point can be nicely illustrated by comparing the way Jensen was treated

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when he visited the University of Minnesota in 1976 and the way Todd Risley was treated on a recent visit. Jensen had been invited to speak on his new work dealing with test bias by the Institute of Child Development (ICD). I had been asked by Scarr, the invitee, to sit in the front row of the auditorium with her because she had heard that he might be attacked. Attacked he was. He was overwhelmed on stage by some extremely hostile members of the audience¹. She, I, and the police in attendance had to escort him out to safety. He was able to make a presentation to a small audience at ICD later in the day. What the University community was not allowed to hear was a synopsis of work that has now become the definitive statement on test bias, work which has completely reversed professional opinion on this issue. Almost everything which has followed is derivative. In 1997, Risley was invited to the University of Minnesota by the Institute of Child Development where he expounded on his findings reported in the book, "Meaningful differences in the every day experiences of young American children" (Hart & Risley, 1995). Hart and Risley reported on a long-term within-family correlational study in which they show a high correlation between parental language diversity and children's IQ. This work was cited by President Clinton during the 1997 White House Conference on Children (UPI, 1997). In his work Jensen has repeatedly emphasized the behavior genetic dictum that correlations between parental behavior and child behavior computed on biological relatives reared together are completely uninterpretable. This fundamental methodological flaw, repeatedly committed by many psychologists, is a simple variation on the argument that "correlation does not mean causation". For reasons, that I cannot fathom, warnings about this elementary flaw have still not been incorporated into many introductory statistics and methodology text books (an exception is Ellis (1994)). One has to ask about the viability of a science that allows the consistent repetition of a serious methodological flaw pointed out and solved by Galton (by the use of the adoption design) over 150 years ago. It is not as if no one noticed Galton's admonitions. The problem was discussed in great detail by Burks (1928a, 1928b, 1938). In recent years it has been written about in great detail by Meehl (1970, 1971, 1978), Scarr (1981, 1992, 1997, 1978) and in other guises by Plomin (1994).

WHAT EVERY PSYCHOLOGIST SHOULD READ

Upon examining his bibliography I am embarrassed at the number of Jensen's publications that I have not read. That will not, however, keep me from making some recommendation to readers who are much less familiar with his work. From the early work read, "The Stroop Color-Word Test: A review" (Jensen & Rohwer, 1966). The 1969 Harvard Educational Review (HER) article, "How much can we boost IQ and scholastic achievement?", (Jensen, 1969) is still a gem as are the replies to critics. Some critics have argued this article is a citation classic because it is often cited solely for purposed of refutation. I have no doubt that many who cite it for the purpose of refutation have not read it. I recommend it, however, because it is a true classic. Better yet read his book *Genetics and Education* (Jensen, 1972) in its entirety as it contains the HER article and numerous other superb papers. Jensen, of course, makes a few mistakes now and then as Kamin (1975) points out in his review of this book. The history of one the mistakes is fascinating. Jensen reprinted a graph that included a data point, for dizygotic twins reared apart- a sample of IQ kin data that did not exist at the time. According to Kamin this kind of error reflects the bias of those who take a genetic position. Locurto (1991), however, informs us that the graph came from

an article by Heber, Dever, and Conroy (1968). The senior author of that paper was in fact a well known environmentalist (see pages 63-66 in Locorto's book for a discussion of Heber).

If you are somewhat interested in behavior genetics, don't know much beyond high school genetics, and would like a primer in quantitative genetics read, "Genetic and behavioral effects of nonrandom mating" (Jensen, 1978). If you want to know something about psychometrics and the issue of bias in mental testing the definitive work is still "Bias in mental testing" (Jensen, 1980a). If you are short on time the Behavior and Brain Science summary of "Bias in mental testing" (Jensen, 1980b) will give you a very good overview of the bias issue. If, like me, you have wondered about Stephen J. Gould's veracity and competence in the mental ability domain you must read Jensen's review of "The mismeasure of man". The title of the review is "The debunking of scientific fossils and straw persons" (Jensen, 1982) and it is among Jensen's very best book reviews. I would recommend it be followed up with Phil Rushton's review of the revised edition of the same book (Rushton, 1997). If you still need more criticism of Gould read Dennett's (1995) assessment of Gould. Alas as I write these words I find that S. J. Gould has been elected president of the American Association for the Advancement of Science (AAAS). The only solace I can garner from this event is that the AAAS once elected Margaret Mead as its president (Freeman, 1983; Freeman, 1991; Freeman, 1992). Mistakes will be made, but some seem more egregious than others.

While doing my simple counts of Jensen's work it occurred to me that Jensen would have analyzed "the data" differently. He would have argued that it is imperative to remove redundancy and artifacts, he would have grouped the papers by type, by source of publication, by decade, etc., and he would have thrown much more light on the topic. To use the title of one of his book reviews it would have been "Intensive, detailed, exhaustive". Indeed these three terms capture much of the flavor of Jensen's writings. I should also add fair-minded, temperate, and courageous. For someone who has been attacked so vituperatively, both in public and in the published literature, I continue to be astounded at the lack of anger and hostility in his replies and the astuteness with which he dissects the arguments of his critics. To use a psychoanalytic metaphor, I am inclined to believe that he sublimates anger and hostility into mental energy-see his astute discussion of the construct of mental energy in Jensen (1997).

I suspect if you asked other Galtonians what they would recommend as "must reading" the list would be somewhat different from mine. There is so much excellent material to chose from that if only a few lists were combined the final list would virtually exhaust his bibliography.

Jensen's writings are virtual tutorials on how to write science and how to deal with controversy - stick to the available evidence, put all the evidence in it's full context, carefully explain the methods, their rationale and the assumptions, acknowledge the lack of evidence when it does not exist and avoid ad hominem arguments. In other words stick to the evidence and be intensive, detailed and exhaustive.

A DIGRESSION ON BERKELEY AND WHO INFLUENCES WHOM

I found Jensen's description of how, in the psychology department at Berkeley in the 1940's, genetic influences on individual differences were neglected somewhat misleading.

His description should have been tempered by the recognition that Tolman in a very early paper titled, "The inheritance of maze-learning ability in rats" (1924) took a clear position on the importance of genetic factors as they influence behavior. Gerald McClearn (1962) provides a concise history of this period at Berkeley. Tolman strongly encouraged Tryon to study genetic influence on behavior and they collaborated to develop a self-recording maze to collect data from the selectively bred animals (Tolman, Tryon, & Jeffress, 1929). Tryon published at least 12 papers on individual differences and genetic influences on learning ability in rats between 1929 and 1941. The first, in 1929, was titled "The genetics of learning ability in rats". This research program resulted in the famous Tryon mazes—bright and maze-dull rat strains. Heron (1935) replicated the Tryon work at Minnesota shortly thereafter. Most psychologists are not aware of the fact that Heron published, with Skinner, (Heron & Skinner, 1940), a paper comparing the rate of bar pressing in the maze-bright and maze-dull rats (the "brights" had a higher rate!). My point here is that the idea of genetic influences on behavior was alive and well at Berkeley when Jensen was there. For some reason it did not "infect" him. I am sad to report that much the same thing happened to me. I entered Berkeley as a sophomore in 1963 and also received an education strongly biased in the direction of experimental (environmental) psychology. As a graduate student in the same department, however, I recall Tryon's spellbinding introductory psychology lectures—I was a teaching assistant in the course—that incorporated behavior genetic findings. Tryon anticipated Jensen's work and the arguments of "The Bell Curve" (segregation of cognitive classes in American society) by many years. My colleagues at Minnesota tell me that Patterson did also, a claim supported by calls from his students in the 40's asking me what the fuss about "The Bell Curve" was all about, "Wasn't it old news?". The importance of the ideas Tryon was talking about simply did not fully register in my mind. I did not relate them to my own interests in personality and social psychology. Gerald McClearn was also on the Berkeley faculty, teaching Behavior Genetics at this time (he went on to Colorado to found the Institute for Behavioral Genetics), but unfortunately we did not have any contact. Fortunately, Harrison Gough—my advisor—required me to read the first textbook in behavior genetics (Fuller & Thompson, 1960) for my special exams and this gave some sense of the field. I also recall Frank Barron presenting, in a very positive manner, the classic meta-analysis of the IQ literature by Erlenmeyer-Kimling and Jarvik (1963) updated in 1981 by Bouchard and McGue (1981)—to an Institute of Personality Assessment and Research seminar. No one seemed to have been aware of the importance of this paper. Nor did they take it seriously, as IQ was out of style in those days. The intrepid Barron, however, had already carried out an early twin study of creativity (Barron & Parisi, 1976). The importance of work in behavior genetics remained only on the periphery of my consciousness until the appearance of Jensen's 1969 HER paper. I had taught a course on Human Intelligence at the University of California Santa Barbara using the textbook by Hunt (1961). Even though I had only a rudimentary knowledge of behavior genetics I had found the book very unsatisfactory in its treatment of genetic influences. Jensen's monograph exploded on the scene like a bombshell and I immediately wrote and asked him for a copy². The 1969 monograph and Jensen's subsequent writing have changed the field of behavior genetics and individual differences in fundamental ways. I report this long anecdote about the Berkeley psychology department because I believe we really do not know why or how people are influenced by the environmental context in which they find themselves. Why, for example, did Jensen become enamored with Hull's theory instead of

Tolman's which explicitly recognized the role of heredity and individual differences? Jensen, of course agrees with me on this point. As he succinctly puts it, "It always amazed to see psychologists offering glib explanations of some immensely complicated behavioral individual incident when psychological science has not even provided explanations for comparatively simple phenomenon.....".

MEMBERSHIP IN THE LONDON SCHOOL

Now that I have castigated others for the sin of assuming they know how we have been influenced by our environment I will proceed to commit the same sin. One consistent feature of Jensen's research career is his love of theoretical models with elemental parts and clear quantitative implications. These feature characterize Hullian learning theory, the serial position effect, the verbal learning (experimental) tradition he found himself in at the Human Learning Center in Berkeley, the Level I-Level II theory of group differences, and quantitative behavior genetic theory. This pattern of intellectual interests early on led him to become a member of the The London School.³ I prefer to call the London School the British Biological-Theoretical Tradition because, a) the latter term puts the origins of the group in a large context (Darwin and Galton came well before the University of London which is the London referred to in the term London School), b) it describes the approach of the group and, c) it provides a nice contrast it with what I call the French Clinical-Therapeutic Tradition. The British Biological-Theoretical Tradition has been attacked on a variety of grounds (reductionistic, anti-egalitarian, racist, cold and heartless, etc.) but the most vehement arguments have been against its biological orientation. Consider the following quote, "The interpretation of IQ data has always taken place, as it must, in a social and political context, and the validity of the data cannot be fully assessed without reference to that context. That is in general true of social science, and no amount of biology-worship by "behavior geneticists" can transfer IQ testing from the social to the biological sciences (Kamin, 1974, p. 2). Lewontin, Rose, and Kamin (1984) have extended this argument to all behavioral traits including psychopathology. These critics have cut to the heart of the matter. The goal of British Biological-Theoretical Tradition has indeed been, since the time of Galton, to integrate psychology, biology, and genetics (Bouchard, 1996). "The g Factor: The science of mental ability" is a direct descendant of Galton's book "Hereditary Genius" and Spearman's book "The abilities of man" (1927). It is a brilliant work. It pushes the goal of British Biological-Theoretical Tradition a giant step forward. I challenge the reader to examine Jensen's magnum opus and decide for him or herself if it has crossed the threshold from the social to the biological sciences. It is worth noting that E. O. Wilson's recent book, "Consilience: The Unity of Knowledge" (Wilson, 1998), defends a very similar but even broader research program.

While reading "The g Factor" I was struck by Jensen's detailed knowledge of the lives of many of the important historical figures in the IQ story. His bibliography explains why he is so knowledgeable. In 1984 he wrote bibliographic entries for the Encyclopedia of Psychology on Galton, Pearson, Spearman, and Thurstone (#238-241) and in 1994 he wrote bibliographic entries for Galton, Spearman, and Eysenck for the Encyclopedia of Intelligence. More recommended reading.

“JENSENISM”

Jensen reported in his commentary in this issue the definition of “Jensenism”, taken from current dictionaries, in order to try to free himself from its grip. I don’t think this is possible, consequently, I thought it might be worth preparing a proper and more comprehensive definition that praises Jensen. My definition is anticipatory and includes the effect of his *magnum opus*, “The *g* Factor”. My definition mimics the definition of an older heresy—Jansensim (See encyclopedia Britannica 15th Ed., Micropaedia, p. 515).

Jensenism: A scientific movement of unorthodox tendencies (heresy) that appeared chiefly in the United States in the late 1960’s. The movement was scientific (religious) in origin, arising out of the theoretical (theological) problem of reconciling the empirical observation of massive and important individual differences in intelligence as well as a large and persistent black-white difference in intelligence (lack of divine grace) with the belief that all men are created equal (human freedom). Jensenism exalts the influence of the genes (grace) made available by mother nature (Christ the Redeemer). According to the doctrine, genes are capable of explaining most of the differences; and it puts forth the scientific (Augustinian) arguments regarding the necessity of genes for any explanation of the differences, the infallible efficacy of genes, and demonstrates the absolutely arbitrary character of environmental explanations. Consistent with this pessimistic view of man’s nature and freedom are its rigoristic views on scientific method and quantification.

The publication of the manifesto of Jensenism, “The *g* Factor”, after attempts to censor it, aroused violent controversy. The work was accused, chiefly by Psychologists (Jesuits), of divesting freewill of all reality and of rejecting the universality of the redemption by environmental means. Nevertheless, the Jensenist interpretation of the empirical evidence spread. It was defended by many disciples and it attracted many influential converts.

The establishment, in the pages of the New York Times (Papacy), struck out against Jensenism with the publication of a devastating review of “The *g* Factor” (*the Bull of Cum Occasione*) which among other things condemned the five propositions of Jensenism on the relationship between black-white differences in IQ and genes.

Jensensim is a complex movement, based more on a commitment to scientific method (a certain mentality and spirituality) than on specific doctrines. It is an attempt, in line with that of the Reformers, to reform psychology (the Church) in the spirit of early science (Christianity). It opposed what, in its view, was a compromising approach to true scientific method (Christian theology) and practice but was rejected by psychology (the Church) as an exaggerated and unorthodox position.

NOTES

1. See Scarr (1987) for details.
2. Jensen had been on the Berkely faculty in Educational Psychology (located in Tolman Hall, the same building as Psychology) while I was a student but, as far as I can recall, I had not met him. I did know his collaborator W.G. Rohwer. I add this comment because I am proud to have been accused of being a formal Jensen student (Nelkin & Lindee, 1995) even though I did not have the privilege. I am, of course, one of Jensen’s students in the intellectual sense. Paraprasing my colleague David Lykken, I have never read anything by him that did not teach me something useful. Jensen’s monograph arrived but it was not an official reprint, it was one of the ones he had had to print up himself as the Harvard Educational Review had decided that it would not provide reprints without binding in the criticisms it was soliciting for publication in a subsequent issue. This action was to atone for the error (sin) of publishing Jensen’s article in the first place. My use of the term sin is intentional as the contro-

versy over IQ seems to have more of a religious fervor to it than controversies over other issues in individual differences. This unconcionable censorship has repeated itself with the refusal of a major publisher to publish "The g Factor" (Lamb, 1997).

3. Brand in his depublished book, also titled "The g Factor" (1996), provides an informative discussion of the London School (p.172).

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