transferred political power from an administration sympathetic to those theorists who promised data and hypotheses that justified the social welfare programs of the Great Society, to an administration prepared to listen to and support the work of more pessimistic and conservative theorists, and prepared to use their findings as a rationale for cutting back such social welfare programs. The time was right for the expressions of just those emphases in which Jensen's article differs from Hyde's.[2] I suspect, if and when a more favorable social climate develops, that Professor Hyde's contribution will be rescued from its undeserved obscurity. If this comment contributes to that end, I shall be thankful.[3]

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[2] Lest the reader suspect me of committing the genetic (sic) fallacy, I am not trying to appraise which of the two views is more adequate. For detailed treatment of this question, see Block and Dworkin (in press).

[3] As the reader probably will have perceived by now, Professor "Hyde's" article was in fact written in 1967 by Professor Jensen. See Jensen (1967).

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## The Strange Case of Dr. Jensen and Mr. Hyde?

Substantive criticism has been tried and it apparently has failed.

Probably no other single article in the history of psychological publications has been subjected to so many niggling and nit-picking commentaries in so brief a time as my essay in the *Harvard Educational Review* (*HER*; Jensen, 1969). (For a bibliography of 117 articles about the *HER* article, see Jensen, 1973b, pp. 356–364.) Now we have Dworkin's (1974) comment, which adds a new wrinkle by pointing out some changes of position or emphasis between one of my articles published in 1967 and the *HER* article of 1969. Ascribing the *HER* article to Jensen and the pre-*HER* article to Hyde is a most novel twist indeed. One is used to seeing it the other way around.

Views and conclusions based on ideological dogma, it is true, should not be expected to change. But on the frontiers of an empirical science, why should anyone be surprised if an investigator's hypotheses, interpretations, and emphases show some changes over a two-year period? Any differences in this respect between my 1967 and 1969 articles simply reflect the results of my continuing study of the matters in question. As any scientist, my thinking is influenced by the research of others and by the results of my own investigations, which, since 1967, have involved the testing by me and my assistants of more than 15,000 children. When I wrote the 1967 article the extremely important Coleman Report had not yet appeared, nor the nationwide survey of compensatory education programs by the U.S. Commission on Civil Rights. A number of studies related to my Level I–Level II theory of mental abilities were undertaken between 1967 and 1969. The evolution of my thinking during this period is clearly reflected in several of my pre-*HER* articles (e.g., Jensen, 1968a, 1968b, 1968c, 1968d). Between the 1967 and 1969 articles I spent a year at the Center for Advanced Study in the Behavioral Sciences, which provided the opportunity to delve more thoroughly into the literature on the genetics of mental abilities. I had actually gone there with plans for a book on the psychology of the culturally disadvantaged, a book I conceived of as an elaboration of my 1967 article (Jensen, 1967). But the further I studied the research literature of the 1960s on the causes of individual differences in mental abilities and scholastic achievement, the more clearly it appeared to me that the doctrinaire disparagement of genetic thinking, and even more often the complete neglect of genetics in the study of human differences related to educability, amounted to a corruption of behavioral science by social ideology. I have always regarded behavioral science as continuous with biology.

I doubt that the changes in my views between 1967 and 1969 are of any greater magnitude or are essentially different in character than are the changes that can be noted in any other two-year period of my research career, which I have chronicled elsewhere (Jensen, 1974c). One should have no difficulty noting the continuing changes of a similar nature throughout my writings since the *HER* article: (*a*) I have been led to a clearer distinction between learning ability and intelligence by my continuing investigations and modifications of the Level I–Level II theory of mental abilities and of the interaction of Levels I and II with social class and race (Jensen, 1970, 1971, 1973e, 1974b); (*b*) the notion of the environmental threshold in intellectual development, first expressed in 1967, has undergone modifications in light of evidence, criticism, and parsimony in theoretical formulation, because the same phenomenon can be explained without this concept and in terms of a simpler model of genetic and environmental influences (Jensen, 1973a, pp. 175–179); (*c*) my studies of culture bias in mental tests have led me to disbelieve the most popular explanations of certain ethnic group and social class differences in IQ (Jensen, in press); (*d*) I have summarized the evidence that has led me to even greater doubts than I had in 1969 about purely environmental explanations of particular racial and social class differences in intelligence (Jensen, 1973a, 1973f); (*e*) my 1967 interpretation of the Skodak and Skeels (1949) study did not have the benefit of my reanalysis of their original data by the methods of quantitative genetics (Jensen, 1973d); (*f*) measures such as Head Start, performance contracting, and busing to achieve racial balance, in light of studies since 1969, appear to me now less promising as means of improving education than at the time I wrote the *HER* article. Consequently, I have since put more emphasis on taking account of developmental readiness for school learning and on diversified educational programs in accord with *individual* differences in abilities (Jensen, 1973b, 1974a).

Finally, I believe that psychology can develop as a science only as it stays clear of subservience to political ideologies. I will continue to deplore any trends to the contrary.

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## Further Comment on the Risky Shift

A comment on Dorwin Cartwright's (March 1973) article is, by the Journal's formal rules, no longer timely. But because I saw letters on the subject in the December 1973 issue, and because my own primary reaction to this research has not been reflected in anything I have read, I will make my reaction explicit and brief.

I think my chapter "Group Dynamics" in *Social Psychology* (Brown, 1965) bears some responsibility for having made risky shift experiments popular for a time among young social psychologists. In fact, I have read only a small part of the risky shift literature because my main work leaves little time for this reading. Cartwright's article seems to make its points in a scholarly, gentle fashion, and I have no quarrel with it. However, there is an important idea here, much more abstract than the risky shift, which, as far as I know, has been largely overlooked.

In *Social Psychology*, the term *risky shift* was not used because it is an incorrect nominalization for the phenomenon in question. What happens is that people usually make advance, private recommendations of action to a hypothetical *other* person in a particular set of story problems which are less risky than the recommendations made by group consensus after the group convenes, all private positions are made known, and often there is a discussion of the story problem. As far as I can see, there is no risk in the shift. It is not a shift that is risky to the persons making it, but rather a shift to recommendations involving greater risk than the central tendency of the original recommendations. Better, then, to say "shift to increased risk" phenomenon, but obviously it is not worth writing a letter to criticize the choice of a name for a phenomenon. Neither is it worthwhile making the defensive observation that the phenomenon was used in *Social Psychology* simply as a concrete example around which to build a discussion of thinking about group dynamics. What follows is, perhaps, worth a letter.

The explanation of the term *shift to increased risk*, which the chapter in my *Social Psychology* particularly favored, is as follows: For a certain set of story problems, certainly not all, most Americans probably think it desirable to be a bit more audacious