

HIGH SCHOOL YEARBOOKS:

A REPLY TO SCHWARZ

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Schwarz's criticism of the Barthell and Holmes study of preschizophrenics using archival data is examined. The traditional assumption that nuisance variables like social class should routinely be "controlled" by case-matching or statistical suppression is challenged. Whether, and how much, shared variance should be removed in archival studies is shown to hinge upon a prior causal framework. The nearly universal tendency among social scientists to view correlations uncorrected for social class as "spurious" is condemned, absent a showing that its causal role in the given situation is such as to render the uncorrected correlation artifactual. It is argued that in most archival studies this assumption is highly problematic, often no safer than the substantive theory of interest itself. It is further argued that statistical control of nuisance variables is not, contrary to the usual belief, "playing it safe," since under several plausible assumptions such control will generate misleading results (e.g., will pseudofalsify a good causal theory).

In a recent contribution to this journal, Schwarz (1970) criticized the methodology of an archival study by Barthell and Holmes (1968) on grounds which are of general interest to social scientists, and which I have treated elsewhere (Meehl, 1969a, 1969b, expanded in 1970, 1971b). The Barthell-Holmes paper provides a beautiful example, in the area of psychopathology, of a *major* unsolved problem in nonexperimental social science research, and my present purpose is to call it forcefully to the attention of readers of the psychopathology literature. With the substantive merits of Barthell and Holmes' investigation (e.g., how much light it sheds on the schizophrenia question), I am not here concerned. I shall also set aside some psychometric issues, for example, bearing of base-rate values (Meehl & Rosen, 1955) upon causal interpretations, that would be important in an adequate overall assessment of their particular application of archival method.

Barthell and Holmes found that retrofollwed schizophrenic and (hospitalized) psychoneurotic patients had been less socially

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participant in high school than a control group. The archival measure employed was number of "activities" listed for each student in the high school yearbook. A control case was identified as the student whose picture was adjacent to the (subsequently) schizophrenic student's.

Schwarz argued that this method

lacked crucial and customary precautions in the selection of a control group [because] the basic question is whether a random sample [which Schwarz allows theirs arguably to be] is an adequate control group [p. 317],

which Schwarz holds it is not. Assuming *arguendo* that "customary" precautions would have included a matching on other variables than sex and race, are these customary precautions "crucial," as Schwarz alleged? I do not wish to assert dogmatically that they are not; but it is far from clear that they are, regardless of how "customary" they may be. Let me emphasize that I do not fault Schwarz, who expresses the current social science consensus. If he were idiosyncratic in his methodological assumptions, I should not think it proper to take up journal space with the present paper, which is longer than the papers of Schwarz and Barthell and Holmes combined.

Schwarz believes that Barthell and Holmes should have shown that their preschizo-

phrenics and controls did not differ as to intelligence, academic achievement, or social class. He suspects (and cites supporting literature) that a random sample of nonpreschizophrenics would differ on one or more of these nuisance variables from the preschizophrenic group. If so, he thinks, the data should be reanalyzed "incorporating either statistical or classificatory controls for that variable [p. 318]." My first heretical query to Schwarz is, "Why?" My second (contingent) query is, "If so, *how much*?"

I shall consider only social class, which is on present evidence the most likely of the three to be a schizophrenia-correlate. (Any reader who sees the point will find it easy to apply it, *mutatis mutandis*, to the other two, although the intelligence variable presents additional difficulties associated with systematic psychometric error.) Why should social class be "held constant," "controlled," or "corrected for" in an archival study of the kind Barthell and Holmes did? The received view, which Schwarz (quite pardonably) accepts without questioning, is that *if* a variable Z (= social class) is a correlate of two behavioral variables X (= high school activity) and Y (= subsequent schizophrenia), *then* its "influence" must be removed. Presumably this is because, absent such statistical control, the observed XY -correlation is somehow spurious or artifactual. These are my terms, not Schwarz's. I can't use his words for what is wrong, as he does not use any, that is, he does not *say* explicitly what is wrong. It is fascinating testimony to the grip of the received tradition that Schwarz *nowhere states* precisely what is "wrong" with the zero-order XY correlation reported, from the interpretative standpoint. He *takes it for granted*—as almost everyone does, apparently—that any difference between patients and controls on a nuisance-variable Z that is a correlate of X and Y constitutes, *ipso facto*, a defect in experimental design. All he says about it is that the two groups differ, apparently deemed a sufficient objection. That is the ubiquitous and unexamined principle I wish to challenge.

Let us set aside a purely "predictive" view of the Barthell-Holmes study, since their discussion indicates, albeit unclearly, a *causal-*

theoretical orientation (e.g., the locution "precipitating factor" is applied to social isolation in their abstract). Predictively, of course, an XY -relationship is what it is, and the value of a zero-order correlation as a basis of forecasting is not liquidated by what happens when we calculate, say a partial $r_{XY.Z}$ (although we may be concerned even there with the validity-generalization problem in a new practical context where the Z variance is greatly reduced—will r_{XY} in that population hold up?) The point is: statistical techniques like analysis of covariance, partial correlation, and casewise matching in "constructing" samples are almost always aimed at unscrambling a *causal* question. The reason we worry about the "influence" of a nuisance variable like social class is that we want our investigation to shed light on what influences what, and the presence of nuisance variables in the system is believed to complicate matters interpretively. I certainly do not wish to deny that it complicates matters; on the contrary, my complaint to Schwarz is that he makes them less complicated than they are, by presupposing the role of social class as a nuisance variable must lie solely on the *causal input* side. This we do not know, and I for one do not believe it. Further, there are causal chains with social class as an input variable in which its influence is mediated by social isolation as a penultimate link, in which case statistical suppression of the shared variance would mask a real effect of the kind Barthell and Holmes want to study.

Let us assume with Schwarz that in the (somewhat ill-defined) population of high school seniors sampled by Barthell and Holmes, both low social class and low social participation are associated with a higher probability of subsequently developing schizophrenia. (Throughout this paper, I neglect both sampling errors and trend size, that is, is the relation large enough to have *either* practical or theoretical significance? Most "statistically significant" correlations in this field are, of course, too small to possess either, and hence are worthless either clinically or scientifically, but that is a topic for another paper!) Thinking in terms of genetic, social, and psychological influences, what are some of

the plausible causal chains that might underlie such correlations? Consider these possibilities:

1. Lower-class students have less money to spend, hence join fewer activity groups, hence suffer more social isolation, which isolation helps precipitate schizophrenia.

2. Lower-class students are perceived by peers as lower-class, hence snobbishly rejected from activity groups *on a class basis*, hence suffer more social isolation, which . . .

3. Lower-class students tend to acquire less competent social skills in home and neighborhood, hence tend to be peer-rejected (but not on an explicit *class basis*), hence . . .

4. Lower-class children tend to be the victims of dyshygienic child-rearing practices, hence develop more battered self-concepts, exaggerated social fear, pathogenic defense systems, etc.—personality attributes that raise the odds of subsequent schizophrenia. These attributes also lead them to be lower social participators.

5. The genes predisposing to schizophrenia are polygenes contributing to anxiety-proneness, social introversion, low dominance, low energy level, low persistence, etc. These genetic factors tend to produce lesser social competence in the preschizophrenics, reflected in their low participation. But they received these genes from their ancestors, in whom these same genes tended to produce lesser social competence in our competitive, extraverted, energetic, work-oriented American culture (See Footnote 17 in Meehl, 1970).

6. While schizoidia is a Mendelizing (dominant) trait, uncorrelated with social class, whether a schizotype remains compensated or not (Gottesman & Shields, 1971; Heston, 1966, 1970; Hoch & Polatin, 1949; Meehl, 1962, 1964, 1971a; Rado, 1956, 1960; Rado & Daniels, 1956) depends upon (a) polygenes as in 5 above; (b) bad child-rearing practices as in 4 above; and (c) "accidental" stresses in adult life (e.g., foreclosed mortgage). Since *a*, *b*, and *c* are all correlates of parental family's social class, the incidence of (clinically diagnosable) schizophrenia is higher for lower-class schizotypes. And since both *a* and *b* influence social participation in high school, variables X and Z will be correlated.

This list of six hypothetical causal chains is merely illustrative (although I myself find all six eminently reasonable guesses, on what we now know). The reader will, given these examples, find it easy to cook up another half-dozen defensible chains. I need hardly add that those chains listed are not incompatible, but could (I think, probably do) operate jointly to generate the observed correlations. For a set of helpful diagrams delineating the ways in which causal arrows can account for correlations, see Kempthorne (1957, p. 285).

Which among the six chains, if operative in pure form, would require an archival researcher to "control" social class, on pain of wrongly interpreting an XY correlation that is "spurious" or "artifactual?" I submit it is not clear that *any* of them would, nor, assuming some would, to what *extent* shared components of variance should be suppressed. The substantive theory of interest, that social isolation is a *precipitator* of schizophrenia, is harmonious with Chains 1, 2, and 3, these three chains differing as to their *earlier* linkages between social class and the personality traits involved, but sharing the feature that the penultimate link is social isolation (however induced). Chain 4 does not fit the authors' precipitator hypothesis, but neither does it generate a spurious XY correlation. On the Chain 4 interpretation, the personality dispositions that predispose to schizophrenia "validly" overlap with those conducive to low high school participation; there is nothing spurious about the relationship, which is an even more intimate one than direct causal influence, to wit, identical elements. To what extent the self-maintained social isolation of a schizotype should be conceived as a *causal agent* rather than a *dispositional-sampling precursor* depends upon the unknown psychodynamic details of schizophrenic decompensation. But it is hardly conceivable that it would always be one and never the other. The impression one forms during intensive psychotherapy of borderline cases, whatever the therapists' etiological bias, is that these processes are typically characterized by marked feedback and autocatalytic features.

In Chains 5 and 6, social isolation is not literally a precipitant, but it is an indirect,

low-validity *indicator* of the same causal influence that gives rise to schizophrenia. To control for social class would be misleading because it would suppress statistically the set of valid causal factors (genes) that are responsible not only for both "output" variables (social participation and subsequent schizophrenia) but also for the nuisance variable (social class of parental family). This error, neglecting the possibility of *any* genetic influence upon social class, thereby taking it as axiomatic that social class functions solely on the causal "input" side, is undoubtedly the commonest methodological vice in contemporary social science research, so much so that Jensen (1969, p. 221) labels it the "sociologist's fallacy" (*his* language, not mine!). In Chains 5 and 6, we see that it would be grossly misleading to treat the XY relation as somehow "spurious" when uncorrected for Z, since the schizophrenia and participation variables are *psychologically* related; yet this perfectly respectable nonspurious relationship does not tend to support the Barthell-Holmes "precipitator" view. Hence a social class control imposed for incorrect reasons might nevertheless facilitate correct interpretation!

Suppose that Barthell and Holmes had concocted matched pairs with social class as the basis of matching. Since (on Schwarz's assumptions) both schizophrenia and social participation are class correlates, either the resulting sample of preschizophrenics will be atypical (higher class than usual), or the controls will be more lower-class than usual, or both. (The usual result of case-matching on even a few variables is marked reduction in both variance and *N* due to the constraints imposed in finding a "statistical twin," (see, for example, Chapin, 1955). What question would Barthell and Holmes then be asking and answering? Presumably,

Do high school students who later develop schizophrenia but who differ from the average preschizophrenic by coming from higher social class backgrounds, show less social participation than a non-random sample of *nonpreschizophrenic* controls selected for having somewhat lower class backgrounds than students generally?

Maybe the answer to this funny question is scientifically interesting, but it is unclear to me why it would be.

Of course my six causal-chain examples, while perfectly conceivable, and, as I think, plausible, were chosen with pedagogical intent, to make the point. I am not so foolish as to dispute the existence of spurious correlations, although I find that language misleading and prefer to speak of "incorrect interpretations" of correlations. My view is that unless a correlation arises from errors of sampling, measurement, or computation, it is not spurious. (There is a sense in which even the paradigm case of "spurious index correlation" ought not to be labeled spurious, since, after all, it *does* tell us how well one index can be predicted from the other.) Surely there are plausible causal chains in which failure to control social class would tempt an archival researcher to grossly erroneous inferences? Of course there are, as witness the following:

7. Lower-class students, having less money to spend, cannot afford to participate in as many social activities as middle-class and upper-class students. While this may distress some of them, it does not engender a pathogenic social isolation (i.e., has no causal role as a schizophrenia-precipitator in their post-school adult life). But the economic stresses and status frustrations they suffer as adults act (upon predisposed individuals) to precipitate schizophrenia. Hence the correlation found by Barthell and Holmes arises.

8. Polygenes contributory to schizophrenia in the Ss were contributory to lesser social competence in their parents, hence to their lower social class. Independently of student personalities, class snobbery in high school tends to "keep out" these students from certain social activities, although this exclusion exerts no appreciable pathogenic influence.

In Chain 7, the nuisance variable *produces* the correlation, but its influence on schizophrenia rate is not mediated by any social isolation link in the causal chain, nor is there any sharing of *psychological* links between the terminal branches, unlike that in Chains 1-5 and, perhaps Chain 6. In Chain 8, the nuisance variable *produces* one effect and *reflects* the causal antecedent (genes) of the other. In both chains, the correlational evidence for a theory involving a pathogenic role of social isolation cannot be properly interpreted unless the nuisance variable's "sta-

tistical influence" is removed. We note that whether social isolation is conceived strictly as a *pathogenic agent* or more loosely as a *psychological precursor* (i.e., perhaps partly pathogenic and partly dispositional) of schizophrenia makes no difference in these last two chains: either interpretation is erroneous, the nuisance variable here having served a *truly* "nuisance" role in the statistical system.

Contemplation of all eight hypothetical chains suffices to disabuse one of the easy notion that controlling social class is at least "playing it safe," the commonest response I get from students and colleagues when I raise these questions. It is obvious that in Chains 1, 2, and 3, for example, if the "control" of social class were to liquidate Barthell and Holmes' original finding, the received methodology would lead us to conclude wrongly. We would say: "Since—when proper controls exist—there is no correlation between low high school activity and subsequent schizophrenia, the substantive theory of interest (isolation as a precipitator) is discredited." But that would be a mistaken inference, for any of these three chains. One cannot label a methodological rule as playing it safe when it is likely to produce pseudo-falsifications, unless we have a strange philosophy of science that says we want wrongly to abandon good theories so as to avoid temporarily betting on false ones!

Of course Schwarz may reply,

I will amend my criticism thus: Barthell and Holmes should have reported *both* corrected and uncorrected trends. Given Meehl's arguments, I will not insist that their reported correlations are "wrong" or "spurious," or that the "corrected" relationships are the "true" ones. But surely we know more if we have both to think about?

With this I cheerfully agree. Insofar as I understand it, that is the line of thought behind path analysis. More generally, it is the rationale of all multivariate methods, where the more variables we put into the hopper, and the more we antecedently know about each one's causal role, the better able we are to interpret the total pattern of correlations. It goes without saying that statistical manipulations cannot provide an automatic "inference-machine," but the tendency in social science is to treat control of nuisance variables

in that way. Whether the number and nature of the five variables under discussion is such that we would know *importantly* more with respect to the task of causal interpretation is, I submit, impossible to say.

This is not the place to develop the theme further. Whether the revived interest in such old techniques as path analysis (Werts & Linn, 1970) will result in a general solution is not presently foreseeable, although I incline to doubt it. Simon and Rescher (1966) have presented a highly general formal analysis which, if I understand them correctly, suggests (proves?) that *no* statistical method can do the job. My sole aim in this paper is to call attention to the methodological mistake, repeated by Schwarz but in the best company of "establishment" social science, of assuming that social class and similar nuisance variables should always be "controlled" in archival research. Whether they should or not depends upon one's causal presuppositions, which in this kind of research are usually as problematic as the substantive theory being tested, sometimes more so.

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