Race and crime: A reply to Cernovsky and Litman

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In a critique of my work, Cernovsky and Litman (1993) reproduced a table that I constructed from INTERPOL data showing that African and Caribbean countries reported twice the amount of violent crime (murder, rape, and serious assault) as European countries and three times that of countries from the Pacific Rim (Rushton 1990). Summing the crimes and averaging the years gives figures per 100,000 population, respectively, of 143, 74, and 44. These proportionate racial differences are similar to those found using official statistics from within the United States (Wilson and Hernstein 1985).

I used a standard 1-way ANOVA design to test whether these huge proportionate differences in mean levels of crime were statistically significant given the variance involved and found that they were. Cernovsky and Litman (1993) deconstructed these aggregates, first into pair-wise per crime t-tests, then into point biserial correlations, then into a metric of variance accounted for, and finally into the non sequitur that the prediction of crime in individual cases would result in 99.9% false positives!

Cernovsky and Litman's conclusions do not follow from their analyses. The "percent variance accounted for" argument is statistically correct but substantively erroneous, as discussed at length by Rosenthal (1984) and

Hunter and Schmidt (1990). The r^2 (and other indices of percent variance accounted for) are related in only a very nonlinear way to the magnitude of effect sizes that determine impact in the real world. Small correlations can have large impacts.

Rosenthal (1984) and Hunter and Schmidt (1990) provide numerous examples of how a "small" effect can have major practical consequences. I have transformed some of their examples from medical procedures and personnel selection into those concerned with criminal justice. Thus, in selection for parole, a validity coefficient of 0.40 should not be squared to mean that only 16 percent of the variance of recidivism is accounted for. Instead, using regression predictions, it means that for every 1 standard deviation increase in mean score on the selection procedure, a gain of the magnitude of a 0.40 standard deviation will result in outcome success – a substantial increase with considerable practical value. An effect size of even 0.10 for a parole procedure, for example, would increase the chance of success from 50:50 to 55:45.

A relatively small difference at the mean can generate rather large differences at the tails of the distributions (where most repeat offenders are to be found). A correlation of .16 for a greater black than white likelihood to break the law would mean that, at the 95th percentile of the distribution, about 7 percent of the perpetuators would be black and 4 percent would be white, a ratio of nearly 2:1. The Asian versus African correlations reported by Cernovsky and Litman (1993) based on INTERPOL data were double this (r= .32).

A correlation of 0.32 between a treatment and an effect means that an effect that accounts for only 10 percent of the variance could reduce the crime rate by almost 50 percent (Rosenthal 1984: 130). It is, therefore, quite rational for the public to attempt to reduce their chance of being victimized by avoiding individuals with perpetrator characteristics (age, sex, socioeconomic and other variables such as race; Rushton 1990). Thus Cernovsky and

Litman's (1993: 34) chastising me for commenting in the media is inappropriate.

Levin (1992) has examined some of the resulting philosophical issues about probable risk assessment and the rights to risk avoidance raised by the disproportionate differences. Levin holds that the taking of differential precautions is both logically and morally justified. He cites a parallel with rational choice theory in economics and rejects the arguments that differential perceptions of dangerousness are the result of "illusory stereotypes".

Cernovsky and Litman (1993: 35) cite a number of published critiques for a "plethora" of technical errors that I am supposed to have made. For example, they claim that I "erroneously" listed as supportive the large-scale study of cranial capacity by Beals, Smith, and Dodd (1984). It is Cernovsky and Litman's interpretation of this study that is in error and I refer the reader to tables 2 and 5 in Beals *et al.* (1984) so that they can see for themselves the hard data and statistically significant population differences in cm^3. Irrespective of interpretation, the rank ordering in this world review is in accord with my prediction. Cernovsky and Litman also fail to mention more recent empirical support for my hypotheses (Ellis and Nyborg 1992; Rushton 1992).

References

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