the data are controlled by independendent variables. Pursuant to this suggestion, a new way of analyzing data is now being proposed, *existential statistics*. Within this approach it becomes necessary to realize that data possess free will and can choose to show an effect or not to. It becomes our task to facilitate these data in expressing themselves more fully. There are at least three added benefits to this approach: (a) The problem of statistical versus clinical significance need no longer concern us. This is because with existential statistics, the goal is not *significance*, but rather *meaning*. Furthermore, the alpha level can be set wherever the data want it to be— It simply becomes a matter of helping them to allow it into awareness. (b) It will no longer be necessary to affront the data by subjecting them to transformations because they are "not normal." (c) With only minor modifications, the existing statistical terminology can, in many cases, be retained. At first I felt that in my honor *df* should stand for degrees of Friedman, but I yield to the suggestion of my colleague Chuck Cowart, who proposed that *df* denote degrees of free will. This is just one example of the type of modification that may occur, but other examples will come to mind (if they want to).

Computer Packages Revisited: A Comment on Strahan

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Strahan (March 1982) addressed a criticism of a shortcoming of SPSS: a failure to warn the user in its CROSSTABS routine when more than 20% of the cells had fewer than 5 observations. He made three statements I consider unfortunate.

First, few statisticians might find the above rule restrictive. If anything, statisticians tend to be stricter than substantive investigators when it comes to assumptions and violations of them.

Second, he attempted to excuse a shortcoming of SPSS by remarking that, "Actually, other packages as well do not include warnings of statistical assumption violations." With regard to the above situation, SAS (which he mentioned by name later), does specifically provide that warning.

Finally, he remarked that, "Until rather recently, the Statistical Analysis System (SAS) . . . described at best enigmatically what its Proc Anova (sic) routine was doing to unequal-*N* data."

The SAS User's Guide (SAS Institute, 1979) states:

> Since ANOVA makes no check as to whether your design is balanced or is one of the special cases described above, using it with unbalanced data will almost surely produce spurious results.
>
> **If you use ANOVA for the analysis of unbalanced data, you must assume sole responsibility for the validity of the output.** (p. 121)

The SAS 1976 *User's Guide* (Barr, Goodnight, Sall, & Helwig, 1976) contained exactly the same two paragraphs (except that in the 1976 manual, the last paragraph was set in capitals rather than bold face). The 1972 version of the SAS contained a completely different ANOVA procedure. The SAS 1972 *User's Guide* (Service, 1972) contained a paragraph on page 138 and two entire pages (152, 153) on a precise explanation of how it computed sums of squares for unbalanced data. I will not refer to the mimeographed SAS manual available at NCSU prior to 1972 because I don't believe Strahan was referring to it.

I consider none of the above enigmatic: Both the 1976 and 1979 manuals stated plainly that unbalanced data probably should be analyzed using PROC GLM rather than PROC ANOVA; the 1972 manual plainly explained how it computed sums of squares.

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H. H. Goddard and the Immigrants

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Now that the—surely avoidable—comedy of errors about the source of quotes from H. H. Goddard has run its course (Dorfman, 1982; Gersh, 1982; Herrnstein, 1981; Kamin, 1982), it is high time to ask the more important question: What do the cited figures about feebleminded Jews, Russians, and other groups represent? After all, these figures were only a small part of a 30-page-long article by Goddard (1917). Not too well written, the article contained some peculiar uses of data, somewhat contradictory statements, phrases that may sound offensive today, and surely more than a single theme. To summarize it in a way acceptable to readers with different predilections is not easy, as is shown by an earlier dispute ("Two Immigrants," 1917; Winkler & Sachs, 1917).

It is perhaps easier to state what the article, contrary to some polemical uses, did *not* say. It did not assert that 80% (or 40%, the figure adopted later in the article) of Russians, Jews, and so on, *in general*, were feebleminded; not even that such figures were representative of *all immigrants* from these "nationalities," although at least on page 244 Goddard tried to minimize the limitations of his sample. Although it expressed surprise and dismay, the article was not a racist diatribe. It did not call for legislation to restrict immigration either in general or from these "nationalities," not even the immigration of "morons" from these groups. In fact, Goddard said that given proper care, the moron immigrant "is vastly happier in this country than in his native land" (p. 270) and may be socially useful as a menial laborer. Nor did Goddard assert that the moron status of the immigrants tested was innate. Summing up his earlier, diverging comments, he addressed the question of hereditary defect or "apparent mental defect by deprivation" in the final pages and said: "We have no data on this point, but indirectly we may argue that it is far more probable that their condition is due to environment than that it is due to heredity" (p. 270).

One may well wonder whether Goddard, who at that time was quite convinced of the hereditary nature of intelligence and feeblemindedness, really believed what he said or whether he was just protecting his rear. But in any case, it should be obvious that anybody seriously interested in the historical issue should read the whole article for her/himself and not just accept my or anybody else's brief quotations because they fit with expectations. Going beyond this one article one finds, incidentally, that the books on feeblemindedness and intelligence written by Goddard during this period made either no mention at all of immigrants, races, or nationalities, or only cursory mention without any indication that they presented a special problem (Goddard, 1914, 1915, 1919, 1920; see also Goddard, 1912, for a defense of the immigrants).

As for the legislative efforts between 1921 and 1924 to restrict the "new" immigration, in which some other psychologists indeed participated, I have not yet found any evidence that Goddard played an active role in them (Samelson, 1975, 1979) apart from being one of 120 members of the Advisory Council of the American Eugenics Society, which supported these activities. Goddard wrote, and did, some rather problematic things, beyond his actual work with the feebleminded; but he seems to have been less ethnocentric or "racist," at least in his publications, than a goodly number of his compatriots. It is a bit unfair to hang a man by a few short quotes; it is also bad history.

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The Saber-Tooth Tiger: One More Time

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Radford's (April 1981) comment on my article (Sprinthall, April 1980) brought forth a comment by Roseman (February 1982) concerning the real identity of an author I had referenced. J. Abner Peddiwell was indeed Harold Benjamin, who did serve as dean of the College of Education at the University of Maryland. And just to spread the glory a bit further, Benjamin was also a member of the faculty at the University of Minnesota, where I learned of his work, pseudonym and all. He was indeed both brilliant and humorous. I used the reference simply because it states the educator's dilemma so succinctly and memorably.

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On Eron on Television Violence and Aggression

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Eron's (February 1982) discussion in this journal of the results from his second longitudinal study, the *Chicago Circle Study* (CCS), of the relationship, in young people, between TV violence viewing (TVVV) and the trait of aggression contains one mystifying omission: No longitudinal findings are reported! There are nonlagged (i.e., contemporaneous) correlations between TVVV and aggression in abundance (see Eron's Table 1), but not a single lagged (i.e., longitudinal) one. These nonlagged correlations, by themselves, tell us nothing about the question that Eron's longitudinal study was presumably to answer: What is the long-term effect of watching violent TV on the development of the trait of aggression?

At the same time that he does not report longitudinal results, Eron uses the nonlagged correlations to suggest that a positive, lagged correlation between TVVV and aggression has been found—that is, a long-term effect such as was found (for boys) in his earlier 10-year longitudinal study of the 1960s, the Rip Van Winkle Study (RVWS; Eron, Huesmann, Lefkowitz, & Walder, 1972). Not only is this