Noesis

The Journal of the Hoeflin Research Group

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Editorial

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Meeting in New York: I met with Keith Raniere and Chris Cole in New York City the evening of March 8. Among the topics discussed was a meeting for the entire membership that would be held the weekend of July 4th here in New York City or in this vicinity. Anyone who would like to participate is requested to prepare some topic for presentation before the other participants that you are knowledgeable about and that you believe would be of general interest to the group. Please send me a one-page summary of your topic for publication in Noesis prior to the meeting. I estimate that 6 to 8 of our members will attend.

Name change: It was also suggested that the name of our group be changed in order to achieve a more dignified public image. I suggested the name "Hoeflin Research Group," which Keith and Chris considered acceptable.

"Guinness" entry: It was further suggested that I try to have greater influence on how the Guinness Book of World Records handles its entry for "Highest IQ." I sent the editors of Guinness a two-page letter suggesting some improvements. I will probably reproduce that letter in a future issue of Noesis.

Another meeting on March 8: I also had a meeting with Marilyn vos Savant for lunch on March 8. Since Jeff Ward had told me that at least ten Mega Society members wanted to keep that old group going, I told Marilyn I would let them keep the name "Mega Society."

Dues: Only Richard May has sent me the dues that I requested for issues 25-36. As stated in issue 23, dues are now $20.00, but I will forgive $1.00 in dues for each page of material for Noesis that accompanies your dues payment. Eric Hart has sent 4½ pages of material, which I reproduce in this issue, so I am ho eful that he, too, will continue as a member for the next year. About 7 people were already credited with payment of dues at the time of the announcement in the last issue. I am hopeful that we will not lose more than 5 of our 17 members after the grace period for dues payment has elapsed.

Future goals: Keith Raniere and Chris Cole both seemed interested in using the Hoeflin Research Group as a sort of think tank. As for myself, I am tentatively considering working on a second doctorate, this time in the field of psychology with an emphasis on psychometrics. It would be interesting to find out if members of this group are systematically different from prople with progressively lower scores on the Mega Test on traits other than intelligence, such as on tests of personality or of creativity.

Here is an article that just appeared 3/7/88. It is interesting to see the transformation of information effected by reporters.

Keith Raniere

Area man needs megabucks for institute

By ROBERT HANLON
Record Reporter

TROY

TROY — At age 16, Keith Raniere dropped out of high school — not because he couldn't cut it, but because he decided to attend Rensselaer Polytechnic Institute.

Four years later, he left RPI with three degrees in mathematphysics and biology.

The soft-spoken, multi-faceted 27-year-old has nurtured a dream since then: to set up a non-traditional school called the Life Learning Institute.

The institute, which would teach intensive courses in judo, mathematics and other subjects, would complement traditional schools.

"Every individual is the best at everything because we are all unique," Raniere said. "To set them apart is denying the specialness of yourself."

Everyone is eager to absorb information, he said, but at their own rate. That eagerness is the basis of the institute.

"The amount of material people learn depends on them — on the framework in which they learn. A lot of people come out of schools and only know how to pass a test. They haven't retained everything."

The key to learning, according to Raniere, is to relate all knowledge to something already familiar to the person. That way, the person is able to build from a safe, familiar base of knowledge.

"Everybody likes to learn, but not everybody likes school. This (Life Learning Institute) makes learning enjoyable and fun."

He would open the first institute in the Capital District, but then spread the concept to larger cities.

While he has a board of directors and potential teachers, he admits he needs one thing: $250,000 to get the project off the ground.

By most measures, Raniere's a smart guy — perhaps one of the smartest people in the world.

"I was always very social, not the stereotypical misfit," he said. "I wasn't a very good student, "I was always very social...I wasn't a very good student, though. I got straight As, but wasn't enjoying the material."

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He was recently admitted to the Mega Society, a group based in New York City that includes people with intelligence quotients of more than 176.

Raniere's IQ is between 185-200, well beyond the traditional scale of IQ exams. Just three people worldwide have scored as high as Raniere on the 48-question Mega Society exam, he said.

Except for his IQ, Raniere's life seems typical.

He attended public schools in Rockland County, north of New York City, until he was a teenager. Then he went to the Rockland Day School, an exclusive school for smarter-than-average students.

When he dropped out of the Rockland Day School, he had a B-plus, A-minus average. He says it was pretty good for such a competitive school and would have made him class valedictorian the following year.

Raniere, who has taught computer, piano and other subjects, said he was bored with the traditional classes because he could gain more outside the classroom.

"I feel I've been a teacher all my life," he said, pointing out he taught himself college courses while a teenager.

Raniere has worked as director of microcomputers for the State Department of Probation in Albany for the past few months. His job involves organizing computer programs for the department at different prisons.

Prior to that, he worked for 1½ years at the State Department of Labor.

After graduating from RPI, Raniere was a computer consultant for area businesses. He also worked as regional coordinator for educational programs for Public Management Systems of New York.

Comments on Keith Raniere's Approach to Norming the Mega Test

by Eric Hart

Dear Ron: I appreciated the news of your meeting. My reasons for missing it were the same as usual. In addition, my resources have a way of not keeping pace with rises in my cost of living, and I must keep my telephone and travel expenses to a minimum. By the same token, the cost of repairing my typewriter has persistently encouraged me not to bother, since I write all my notes in longhand. As no one I know has an uncommitted working unit to lend me, there's no other way to write this than in my customary script. I'll try to upgrade it to "letter quality."

I've written down quite a few comments, more or less extensive, re the content of the newsletter. The bulk of these will just have to wait. I'll confine myself here to a short discussion of the radical norming method described by you in the last issue.

Information accessed by statistical means either originates within the sample or without it; statistical techniques are ideally neutral with respect to the information they distill from the raw data. External information pertains to the location of the sample within the space of all possible samples of like size, as opposed to the relative locations of data-points within the sample. We may thus distinguish immediately between conclusions derived from either type of information. Since norming the Mega test maps the internal distribution of scores to the external distribution of I.Q.'s, it requires external information.

Were we interested only in maximising internal variability for purposes of discriminating among test subjects, we'd probably employ a technique called "principle components" to weight the individual problems so as to produce the widest possible differentiation of subjects consistent with our lack of a theoretic norm on problem difficulty (a norm which would be based on the computational demands of the respective problems: you may recall that I'm an advocate of this "mechanistic" approach, which I've sporadically begun to develop). In this case, we're after somewhat more than a relative ranking, however.

The somewhat more to which I allude is generally sought among correlations with outside measurements. But we appear to have agreed that other I.Q. tests are inferior and not to be trusted. This limits our means to something known as a statistical bootstrap, one of a class of computation-intensive methods for the external location of a limited sample (to the extent of determining its error) within a facsimile of the universe of commensurate samples generated by random recombination of the available data. The stipulation of randomness is important, since it is what allows outward simulation. Because this concept arouses skepticism, we'll briefly examine its roots.

The bootstrap principle began in antiquity; it was present, for example, in ancient Greece. Traditionally, it amounts to a belief in immanent causation, which amounts to affirmation of the constructibility of the universe as it exists at any point in time from that as which it began. The most recent cosmogenic adaptation pertains to the immanence of present reality in the primal singularity which preceded the "big bang". The principle is no less friendly to theological interpretation, since it supports a definition of deity as that in which all things are immanent. But perhaps its most famous version was first given by George Berkeley and later recapitulated independently by the notable 19th century Austrian philosopher Ernst Mach, after whom it is now called Mach's Thesis: it holds that inertia is a function of the distribution of matter in space, and thus that the distribution of matter in space (compare "statistical distribution of test scores over humankind") is immanent in the local phenomenon of inertia (compare "internal distribution of scores within a restricted sample"). As this particular formulation was a driving force behind the development of Einstein's general theory of relativity, the statistical version is not without pedigree. Statistical bootstraps are, in fact, observed to work, but only for certain purposes and under certain definite conditions which can be difficult to establish from a local perspective.

Whereas the standard method relies on the random recombination of what are assumed to be random data, the new method - which attempts to define active percentiles at a level highly improbable for so limited a sample - seems to rely on orderly permutations of nonrandom data containing a much higher than expected proportion of rare intellects. True to its class, it seeks to relate the sample at hand to an exterior context by virtue of information contained within the sample. This implies that sufficient information is latent in the relations between data points and accessible by permutative analysis, this latter assumption seeming to be what distinguishes this method from the random-combinatorial version. From "many of these (48!) chains can be ignored", I infer that their expendability devolves not merely to negligible positive impact on discriminativity, but to actual inconsistency with the linearity criterion on the sequence of problem weights (the computative norm) from which the outward distribution is to be calculated... i.e., with the orderly prequantitative ranking of problems.

What are the requirements of such combinatorial inference? Let's have a quick and oversimplified look at some hypothetical sample to be tarified to the 10^{-7} level at its top end. Assume, analogously to your description, that each successive problem defines the top portion of the division associated with that just below it in discriminativity... i.e, that if a subject solves the "easiest" 10 problems, solution of the 11th will place him in the top $1/n$ of the division he inhabits for having solved the other 10. Assume further that the function producing the weights is regular... eg., that the upward convergence takes place by powers of $1/1.4$, according to which solution of the easiest 10 places a subject in the top $1/(1.4)^{10} \approx 1/30$ of the sample, solution of the next-hardest in the top $1/(1.4)^{11} \approx 1/40.5$, etc. Solution of all 48 problems will naturally place him in the top $1/(1.4)^{48} \approx 1/10^7$. The question now becomes whether, among a random group of 10^7 people in the outer universe, $\sim 10^7/(1.4)^r$ will solve the r^{th}-easiest problem $(1 \leq r \leq 48)$.

Since this question refers explicitly to the "outer universe", we need to bootstrap... to recombine our sample "parthenogenetically" with itself until we have the required amount of "data". Minor variations in viewpoint make no difference; in the absence of reliable outer correlations at the upper levels, the empirical approach is reduced to just this.

Suppose that now we (1) use a random function to create "other" samples of similar size with which we may compare ours and determine its deviation from true representativity; (2) constrain our recombinative function to compensate for this error; and (3) virtually generate 10^7 randomly-distributed test subjects on which to test our hypothetical norm. Unfortunately, the standard bootstrap fails us on the first step. Any random recombination of nonrandom data will preserve its skewed aspects, and we'd have no representative samples against which to make a valid comparison of our own.

Now the only thing we can do is attempt to tailor our initial recombinative function according to some blend of theory and outside measurements. But outside measurements are unreliable and sparse at critical scoring levels, and we have no consistent theory on what makes a problem discriminative. In any case, even partial reliance on outside measurements obligates us to them, since in using them we incorporate the assumptions underlying their methods of acquisition. This becomes an unwanted debt at higher, more improbable levels of rarification. Therefore, bent on the ultimate statistical incest, and in bold defiance of certain logical limitations on statistical certainty, we must devise a completely self-referential form of the bootstrap. How might we proceed? I assume (perhaps incorrectly) that the new method begins as follows.

Taking the total scores of all the subjects, we segregate those of like score from all the others in 48 disjunctive classes, which we label by score. Now take class 1: is there any consistency in which problem is solved? If so, we have our "easiest problem"; if not, we have competing hypotheses of varying probability concerning which problem is easiest. This ambiguity will become irrelevant as soon as we reach a class in which all these candidates appear together almost all of the time. But the effect of such ambiguities is to make of our total ordering of problems a "partial order" of problem equivalency-classes, much less powerful for discriminative purposes. And so we proceed, creating a weak norm on problem difficulty. We then use this to generate our version of the first principle component, or that set of problem-weights maximising internal variation of percentiles. Our purposes require that we stress variation at the extreme high end of the scale, which we've done by making the weighting function exponential... somewhat arbitrarily equating score with percentile.

But we still lack any obvious way to make this internal variation correspond to the outer scale, against which it persists in masquerading as a mere finer gauge over a limited range. We're trapped by statistical ambiguity inside a self-referential logic, where "pulling on bootstraps" only stretches them

I assume, then, that what we're trying to bootstrap is the variability of the first principle component among commensurate samples in external space, thus certifying our norm on problem difficulty and determining the reliability with which we can externalise our inner percentiles. Of course, statistical limitations on certainty will emerge and have to be accounted for.

There are several salient facts we've neglected to mention due to their extreme apparency. One is that our "partial order" on problem difficulty resolves, in the absence of large anomalies, to something very close to the samplewide solutative frequencies. By "anomaly" I mean a situation in which total score is not well-correlated with correct solution of particular problems. In this case, however, the new method would amount to a search for correlations of total score and "composite" problems irreducible, for normative purposes, to their separate components. But since these components are separately-soluble, we'd have theoretic chaos, and again be thrown back on the primary solutative frequencies. So, as statistical experience has shown time and again to be true, we can't improve appreciably on the global frequencies (and their averages) as long as we lack understanding of the parameters governing empirical observations... here, the parameters relating human intelligence to the structures of problems.

One other fact worth mentioning is even more obvious, and it is contained in the very definition of empiricism. We cannot, no matter how widely it is done, make propter hoc assumptions about phenomena (e.g., that they're normally-distributed) before verifying that these assumptions are satisfied by the data. This is a blueprint for the construction of artificial tautologies of the kind on which modern psychometrics is regrettably based, a circumstance to which responsible science is opposed. As it is now, psychometrics boils down to a theoretically-unfounded methodology for the acquisition of data interpreted and arranged according to unfounded statistical assumptions dragooned stand in place for an a.w.o.l axiomatics. The metaphor is quite apt mptions are no more dependable than their forced conscription would lead one. ften withering when seriously challenged...

The scientific method, which involves the experimental (empirical) validation of intelligent (theoretical) hypotheses in ways calculated to minimise artificial tautology given the logical barriers against its complete elimination, encounters a particular kind of well-meant interference upon the failure of intelligent hypotheses to present themselves as ordered. Modern statisticians have become a little Promethean in their outlook on deduction; many consider statistical methodology to be mathematically more difficult, more ambitious, and potentially more fruitful than mere recourse to logical inference. Historically, this can be associated with the partial supersession of Fisher's "uncertain inference" approach to statistics, which was considered deductive in a certain classical sense, by the "programming" approach, which tends to convert statistical analysis to a set of mathematical optimisation techniques employed on problems well-defined to the extent that their domains of definition are more or less counted and parametrised. Most recently, a new approach has grown out of the latter: it holds that extensions of a given base-space can be extrapolated from it in a way equivalent to the nondescriptive evaluation of unknown parameters over it. This, of course, is the "bootstrap" approach described above: data points are in effect "created out of nothing", or more accurately, from "hidden" (but combinatorially-accessible) information present in the base-sample.

But all of this has its limitations. The Promethean tendencies of those aspiring statistical titans who scorn pure deduction are a little overinflated, inasmuch as any algorithm, statistical, stochastic, or deterministic, is necessarily describable as a "deductive" inferential lattice. That is, like Prometheus himself, such statisticians risk defeat by that of which they behave in contempt. Naturally, we should take care to avoid the same fate.

What this means is that if we rely on a bootstrap for normative purposes, the results will be doubtful in the eyes of those who still doubt the method itself... and that may well include most longtime statisticians and virtually every "expert" in the testing of intelligence. We should thus use it conservatively... i.e., only where we can demonstrate conclusively how and why it works for the problem to which we're applying it. Perhaps this will become feasible upon a fuller explication of the procedure under discussion.

Eric -