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LETTERS

XYY Genotype

Barbara J. Culliton, in an article about the suspension of XYY screening at the Boston Hospital for Women (News and Comment, 27 June, p. 1284), reports that the original XYY study was “premature” and that today “all responsible scientists insist that the XYY chromosome is quite innocent of causing any crime.”

Let me attempt to set the record straight. The first survey that demonstrated an excess of men with an additional Y chromosome in an institutionalized population was conducted by myself and my colleagues (1) among a group of mentally subnormal male patients in a state hospital, an institution for patients “who require treatment in conditions of special security on account of their dangerous, violent or criminal propensities.” We reported our observations on 197 such patients, 266 randomly selected newborn males, 209 randomly selected adult males, and an additional 1500 males whose chromosomes we had examined. We found seven males with an XYY chromosome constitution in the patient population, none in the 475 randomly selected males, and only one in the remaining 1500 males ($x^2 = 13.8, P = .0002$). Our conclusion, “the finding that 3.5% of the population we studied were XYY males must represent a marked increase in frequency by comparison with the frequency of such males at birth,” could hardly be considered premature by even the most conservative standards.

Further studies, both of men in mental and penal settings and of control populations were undertaken. The results of these investigations were excellently and exhaustively reviewed by Hook (2). Consideration of the facts show (i) that the original observations have been amply confirmed; (ii) that the excess of males with an abnormal chromosome constitution in mental-penal settings is not confined to XYY individuals but also applies to XXY men and, most dramatically of all, to men with an XXYY chromosome constitution, who are found 100 times more frequently in mental-penal settings than among the newborn; and (iii) that, while the excess of men with an abnormal sex chromosome constitution is most marked in mental-penal groups, it is also evident among men in exclusively penal and exclusively mental settings.

We know nothing as yet about the mechanism of action of the additional sex chromosomes nor their effects, if any, on the intelligence and behavior of the majority of affected individuals in the population at large. It seems reasonable to suppose that human behavior, like virtually all other human traits, is determined both by genes and environment and that the possession of an abnormal chromosome constitution may make its carrier particularly susceptible to the effects of an adverse environment.

Those who consider “the attempt to determine a genetic basis for antisocial behavior a diversion with harmful effects” have succeeded in suppressing a research project which was deemed by peer review to meet the rigorous ethical and scientific standards rightfully required of all research involving human subjects.

The suppression of this project denies to XXY, XYY, and XXYY men, their families, and society the liberty to understand and intelligently modify the behavioral effects of a high-risk genotype.

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References


Despite the implication in Culliton’s article, there is a clear association of the XYY genotype with deviance, as judged from the frequency of XYY men in security settings compared to the rates in newborn or adult populations. While the nature and extent of this association are still not defined, the first report (1) has been amply confirmed and would be better described as “seemingly” rather than “premature” [see (2) for review]. Those who deny evidence for a “link” between this genotype and criminality can only mean that there is still no direct evidence for a causal connection between the two; there is no question that there is an association. But Culliton appears to endorse an even stronger view when she states “all responsible scientists insist that the XYY chromosome is quite innocent of causing crime.” The issue is, however, a complex one not subject to such simple generalizations, and revolves about our understanding of causality and human behavior. The XYY genotype may well contribute to the eventual problems of the affected male by resulting in patterns of neural organization that affect cognitive function or produce other behavioral “difficulties” (of the type Walzer and others have described) which tend to make it harder for such individuals to cope with environmental stress-
es. While there is no direct evidence for this view, the data that are accumulating appear to make it increasingly plausible. (Such a model does not assume that preventable or remedial environmental factors make no contribution to either behavioral difficulties in earlier life or deviance in later life.) The connection postulated between the genotype and deviance is not an inevitable one; whether it is "causal" awaits universal agreement on the definition of the term as applied to human behavior genetics.

Statements such as Culliton's or debate as to whether the XYY genotype is "guilty" or "innocent" only polarize the issues without addressing them. The important questions concerning the XYY, XXY, and XYYY genotypes are what factors—physiological, psychological, social, and their interactions—are associated with the increased frequency of affected males in security settings and mental institutions, and what we may learn about the possible contribution of such factors to the ultimate behavior of all individuals, irrespective of genotype.

Ernest B. Hook
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References

Bicentennial Bells

Constance Holden, in her article "The Bicentennial: Science loses out" (News and Comment, 8 Aug., p. 438), mentions the American Revolution Bicentennial Administration's plan for 4 July 1976: "The afternoon is to be devoted to town meetings and speeches, and at 4 p.m. (11 A.M. Hawaii time) all the bells in the nation will ring out simultaneously."

Has anyone considered what the effect might be of all that simultaneous sound vibration?

Darlene C. Schmidt
Public Information Office, American Society for Quality Control, 161 West Wisconsin Avenue, Milwaukee, Wisconsin 53203

Journal Reviews

It has long struck me as odd that scientific journals are not reviewed in "journal review" sections of scientific magazines somewhat analogous to the book review sections that are so familiar.

Critical reviews of journals would be of interest to the scientists who read them or publish in them. They would also be of value to librarians and others who must decide which journals to take on subscription. Librarians currently have little to go on except citation counts, the significance of which is controversial.

I would like to see a respected scholarly or professional organization, one free of financial interest in the journals that would be reviewed, undertake to publish critical reviews of scientific journals at intervals of, say, 5 years. The organization that comes immediately to mind is the AAAS, and Science is the obvious publication in which the journal reviews should appear. If each issue of Science carried reviews of 5 journals, 260 journals could be reviewed each year, or 1300 in 5 years.

The scientist invited to review a journal obviously should be a person of distinction and should not have an ax to grind. On the other hand, complete innocence of involvement with any journal as an editor or member of an editorial advisory or publication board is unlikely to be found in the case of many persons of the requisite scientific distinction. A listing of current or recent connections of that type, following the name of the reviewer, would make plain at least some of his current entanglements.

The journal review should include certain standard information about the journal's history, sponsorship, size, circulation, and cost, which should be furnished to the reviewer by staff, but the heart of the review would lie in qualitative assessment of what function the journal is serving, what clientele it caters to, where it stands with respect to comparable journals, and what trends of emphasis or quality can be discerned.

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Particle Discoveries at SLAC

Martin Deutsch and Samuel C. C. Ting wrote letters published in the 5 September issue of Science (p. 750) with respect to the exciting discoveries in high energy particle physics. These letters contain selected references to conversations pertaining to the history of the new particle discoveries, reports of which were published in Physical Review Letters of 2 December 1974 (1, 2).
These developments have been among the most exciting events in the recent annals of physics. I personally was a witness during the weeks preceding the actual announcement of the spectacular sharp peaks (1), when the members of the Lawrence Berkeley Laboratory (LBL)-Stanford Linear Accelerator Center (SLAC) team struggled to understand the anomalous counting rates observed near a collision energy of 3.1 GeV. What finally became the now famous sharp peak initially manifested itself through a peculiarly high point at 3.2 GeV. Further scanning exhibited a lack of reproducibility of readings near 3.1 GeV, since the energy of the storage rings was not controlled commensurate to the sharpness of the peak and therefore malfunctions were suspected. After all relevant parameters were put under control, the spectacular peak initially of the 3.1 GeV particle, followed very soon thereafter by the discovery of the 3.7 GeV psi particle, became obvious.

There is no question that the Massachusetts Institute of Technology–Brookhaven National Laboratory discovery represented a very difficult and superbly instrumented piece of work in high-energy experimental physics, and the authors deserve full credit for that achievement. Similarly, the independent LBL-SLAC discoveries represented a spectacular demonstration of the powers of electron–positron storage rings in discovering new particle states and in exploring the spectroscopy and intrinsic properties of such particles. This should be a joyous occasion for all physicists.

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Promising Chimpanzee

The important article "Putting a face together" by David Premack (18 April, p. 228) opens with the following statement: "Chimpanzees do not, so far as is known, construct copies of existing or imaginary figures by any device—drawing, assembling pieces of existing material, or otherwise." In fact, a paper published 66 years ago presented suggestive data that were recognized as theoretically important in comparing the mental abilities of apes and humans. This was an intriguing account by Witmer (1) of the remarkable performing chimpanzee, Peter. He was able to accurately copy, with chalk on a blackboard, the letter W drawn by Witmer. When asked to do so again, Peter complied. Witmer was a respected psychologist, and Peter's performance was observed by several other astonished persons. No differentiation was made, however, between copying the figure and copying the writing movements. S. J. Holmes, in his 1911 book (2), reproduced a photograph of a blackboard with the letters that Peter copied and stated in his review, "It is unfortunate that more extended and thorough experiments were not carried out with so promising a subject."

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1. L. Witmer, Psychol. Clinic 3, 179 (1900)

Those of us whose university libraries do not have the texts in question are indebted to Burghardt for calling them to our attention. It is not clear whether the animal copied the trainer's movements, visual product, or both, but in any case the example can be contrasted with that of Sarah, one of our chimpanzee subjects. Her visual production was not based on copying; she regularly reassembled the face without an external model. The relation between copying an item and reconstructing it from memory is an interesting one. Certainly common sense suggests that it is possible to copy items which cannot be reconstructed from memory. On the other hand, we have some recent findings suggesting that, in some cases at least, if the subject cannot reconstruct an item from memory, it cannot copy it ever.

In pursuing the matter of what one must know in order to be able to reassemble a face from memory, we gave Sarah disassembled pictures of faces different from previous ones. The parts were no longer eyes, nose, and mouth, but either (i) conjoint canonical parts, such as an eye joined to the nose; or (ii) disassembled canonical parts, such as an eye cut into four arbitrary pieces. Sarah reassembled the face from the conjoint pieces but failed to reassemble the disassembled eye. Moreover, when given an assembled eye (identical to the disassembled one) as a model, she was no more successful in copying the eye than she was in reassembling it from memory.

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2. J. J. Albert et al., ibid., p. 1404.

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RESEARCH NEWS

(Continued from page 1076)

(phosphorylcholine at NIH, a derivative of vitamin K at Johns Hopkins, and 2,4-dinitrophenyl groups at Argonne) compared to ordinary antigens, and they interact with only a few residues in the combining site. They might miss the ones involved in triggering conformation changes. Furthermore, the antibodies were studied in the crystalline state, and the results may not be applicable to what happens when the proteins are in solution.

At least one group of investigators, including I. Z. Steinberg and J. Schlessinger of the Weizmann Institute of Science in Rehovot, Israel, has evidence that antibodies in solution undergo a conformation change when they bind antigen. They determined the effect of antigen binding on the circular polarization of fluorescence of antibodies. The investigators observed changes only with large antigens and not with phosphorylcholine.

The picture of antibody structure emerging from all this is one in which certain segments of both variable and constant domains form a structural framework that has changed little throughout the course of antibody evolution. Several investigators pointed out that the resemblances in the three-dimensional structures of the different domains support the hypothesis that they all originated from duplication of a single primordial gene. When changes in amino acid sequences did occur in the framework regions, they were such as to not markedly disturb the basic folding pattern. On the other hand, alterations outside of this framework, for example, in the hypervariable regions of variable domains, can give rise to antibodies with different specificities. Alterations in the non-framework sequences of constant domains would permit the evolution of domains capable of performing different functions.

Because of the similarity between the Bence-Jones dimer and the Fab fragments, Edmundson thinks that the dimer may represent a prototype for a primitive antibody, and a possible intermediate in the evolution of the four-chain immunoglobulin molecule. He suggests that the rotation of the constant domain relative to the variable one was a critical step in the evolutionary process because it means that different amino acid residues would be needed for maintaining the association of each domain pair. Those not involved in the interaction would necessarily also be different and hence the domains could evolve to perform different functions. The eventual result would be immunoglobulins with the structures and functions that we know today.—JEAN L. MARX

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