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El contenido de este número fue seleccionado por el editor anterior.
The contents of this issue were selected by the former editor.

La *Revista Interamericana de Psicología* es publicada por la Sociedad Interamericana de Psicología (S.I.P.) para facilitar el intercambio de información profesional y científica entre los psicólogos de las Américas y para promover el desarrollo de la Psicología en el Hemisferio Occidental. La Revista se publica cada seis meses con artículos de todas las áreas de la psicología. Los manuscritos que pueden ser publicados por lo general pertenecen a una de tres categorías: Artículos Originales (teóricos, revisiones, empíricos, clínicos, educacionales o profesionales de no más de 20 páginas); Informes Breves (500 palabras); y Revisiones de Libros (por invitación). Aquellos artículos que sean relevantes a los temas y preocupaciones de la Psicología Interamericana o que reflejen cierta colaboración internacional recibirán especial atención. Los manuscritos pueden escribirse en Español, Portugués o Inglés. La suscripción anual es de US\$10.00 para individuos; US\$20.00 para instituciones. Todas las órdenes de suscripción, cambios de dirección, y otra correspondencia comercial debe enviarse al Gerente de la Revista.

The *Interamerican Journal of Psychology* is published by the Interamerican Society of Psychology (S.I.P.) to facilitate the exchange of scientific and professional information among psychologists throughout the Americas and to promote the development of Psychology in the Western Hemisphere. The journal is published semi-annually and accepts manuscripts in all areas of psychology. Accepted manuscripts normally fall within three categories: Original Articles (theoretical, reviews, empirical, clinical, educational, or professional – up to 20 pages); Brief Reports (500 words); and Book Reviews (by invitation). Articles which are relevant to the themes and concerns of Interamerican Psychology or which reflect a cross-national collaboration are encouraged. Manuscripts may be submitted in Spanish, Portuguese, or English. Annual subscription is US\$10.00 for individuals; US\$20.00 for institutions. All subscription orders, address changes, and other commercial correspondence should be addressed to the Managing Editor.

NOTA EDITORIAL

A partir de este número reiniciamos la publicación continuada de nuestra *Revista Interamericana de Psicología*. Para mí fué un gran placer el aceptar la invitación de la Junta Directiva de la sociedad Interamericana de Psicología de ser el cuarto editor de esta revista después de las excelentes contribuciones de Carl F. Hereford, Luiz F. Natalicio y Horacio J.A. Rimoldi. Espero lograr mantener y aumentar el alto nivel científico y profesional que mis predecesores han sentado y el producir una excelente revista con la colaboración de los importantes psicólogos que forman parte del grupo de Editores Consultores.

Nuestro objetivo será el publicar los mejores trabajos de la Psicología Interamericana. De esta manera esperamos facilitar el desarrollo de la psicología científica en todas las Américas y el reducir el aislamiento científico y profesional promoviendo comunicación y colaboración internacional.

Para implementar estos objetivos seguiremos aceptando manuscritos en todas las áreas de la Psicología. Los manuscritos que se considerarán para su publicación estarán en una de tres categorías: Artículos originales (teóricos, revisiones, empíricos, clínicos, educacionales, profesionales que no excedan 20 páginas), Informes cortos (500 palabras); y Revisiones de Libros (por invitación). En especial nos interesan aquellos artículos que sean relevantes a los temas y preocupaciones de la Psicología Interamericana o que reflejen colaboración trans-nacional. Los manuscritos pueden ser escritos en Español, Portugués, o Inglés.

Tanto yo como los Editores Consultores deseamos invitar a nuevos y a antiguos autores a enviarnos sus manuscritos para su posible publicación. Nuestro rol lo percibimos como teniendo dos objetivos principales: Primero, el seleccionar los artículos más interesantes para nuestros lectores en todos los países de las Américas. Segundo, el servir en una capacidad educativa proveyendo sugerencias constructivas y detalladas que le sirvan al autor para incrementar sus habilidades científicas y su carrera profesional.

Para facilitar este proceso, los manuscritos se estudiarán dentro de un proceso centralizado. Todos los manuscritos deben enviarse al Editor quién a su vez los enviará a dos revisores. Informes más detallados pueden encontrarse en la contra-portada de éste número de la revista.

Finalmente, deseo expresar mis agradecimientos a la Facultad de Artes y Ciencias de Florida International University por haberle provisto a la revista de una oficina totalmente equipada así como con una secretaria y al Centro de Asuntos Internacionales de la misma Universidad por los fondos para ciertos gastos de la Oficina Editorial de la revista. Vicky Eskenazi también se merece un agradecimiento especial por su concienzuda labor como secretaria de la revista durante el último año.

Al terminar esta nota editorial deseo indicar que el contenido de éste así como del próximo número de la revista contienen artículos aceptados para su publicación por el antiguo editor, Horacio J.A. Rimoldi.

Gordon E. Finley
Editor

EDITORIAL NOTE

Welcome to the resumed publication schedule of the *Revista*. It has been with a great deal of pleasure that I have accepted the invitation of the Board of Governors to become the fourth Editor of the *Revista*, following in the excellent footsteps of Carl F. Hereford, Luiz Natalicio, and Horacio J.A. Rimoldi. I look forward to maintaining and enhancing the high scientific and professional standards set by my predecessors and, with the assistance of our outstanding Board of Consulting Editors, to producing a truly exciting *Revista*.

Our Editorial Policy will be to publish the best that Interamerican Psychology has to offer. Our goals are to facilitate the development of scientific psychology throughout the Americas and to reduce scientific and professional isolation through communication and cross-national collaboration.

In pursuit of these goals, we will continue to accept manuscripts in all areas of Psychology. Accepted manuscripts normally will fall within three categories: Original Articles (theoretical, review, empirical, clinical, educational, or professional—normally not exceeding 20 typewritten pages), Brief Reports (500 words); and Book Reviews (by invitation). Articles which are relevant to the themes and concerns of Interamerican Psychology or which reflect a cross-national collaboration are encouraged. Manuscripts may be submitted in Spanish, Portuguese, or English.

The Board of Consulting editors and I most strongly encourage new as well as published authors to forward manuscripts for consideration. We see ourselves as having two Editorial roles. The first is the selective role in which we strive to bring to the readership the most interesting articles from throughout the Americas. The second is the educative role in which we strive to provide detailed and constructive advice which will serve to enhance the scientific skill and professional career of the author.

To facilitate this process, the *Revista* has returned to a centralized manuscript processing procedure. All manuscripts should be sent to the Editor who normally will send them out to two reviewers. Information on the submission procedure and on manuscript preparation may be found on the back cover of this issue.

Finally, I most gratefully would like to thank Florida International University's College of Arts and Sciences for furnishing the *Revista* with a fully equipped office and with secretarial assistance and FIU's International Affairs Center for operating funds for the Editorial Office. We all are indebted to Vicky Eskenazi who has served most conscientiously as secretary to the *Revista* during the past year.

In conclusion, I would like to note that the contents of this issue, as well as of the next issue, were accepted by the previous Editor, Horacio J.A. Rimoldi.

Gordon E. Finley
Editor

LA VALIDACION DE LA PREDICCIÓN DEL ÉXITO EN LAS DIVERSAS OCUPACIONES USANDO LOS TESTS DERIVADOS DEL MODELO DE LA ESTRUCTURA DEL INTELLECTO DE GUILFORD

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Este trabajo presenta una discusión de los diversos métodos utilizados en la predicción del éxito en diversas ocupaciones y luego analiza la importancia del método tradicional y de un método del sistema utilizando las habilidades mentales de Guilford. Los datos de 37 empleados demuestran que el método de sistema con los datos de las habilidades primarias es superior en predecir el nivel de desempeño de los empleados que los métodos más tradicionales.

This paper presents the different methods utilized in order to predict success in different occupations and then analyses the value of the traditional methods versus a systems method that takes into account Guilford's mental abilities. Data from 37 employees show that the systems method is superior to the traditional ones in predicting job success.

La validación de tests para la selección de personal se ha realizado, en el pasado, empleando un modelo que es derivado directamente del método usado en el laboratorio. Hay varias variantes del mismo, pero la más clásica, es la que se llama la validez de predicción. Esto consiste en contratar personal usando para ello un determinado test que se estima está relacionado con el cargo en cuestión, dejar al mismo trabajar seis meses y luego, estableciendo un criterio para medir el resultado en el trabajo, hacer una correlación entre el resultado del test y el criterio.

Otro tipo de validez consiste en hacer lo mismo con un grupo de personas que ya se halla trabajando, calcular una correlación similar con un criterio, y con base en ello hacer predicciones sobre quienes deben ser elegidos para dicha tarea. Luego se contratan personas que poseen dicho atributo en el nivel que se considera adecuado y, varios meses después, se calcula una nueva correlación con el criterio establecido.

Un tercer tipo de validez es llamada validez de contenido. Este es el caso típico de exámenes de conocimientos en los que se incluyen preguntas que reflejan el contenido del material dictado en el curso. En este caso, en general, no se hacen cálculos estadísticos.

Un cuarto tipo de validez es llamado validez sintética. El sistema consiste en usar criterios conseguidos en ambientes en los que hay muchos sujetos para ser aplicados a casos de menor escala. Por ejemplo, si una fábrica relativamente pequeña tiene dos o tres mecánicos de mantenimiento, no pudiendo usar un sistema para validar un test determinado, por el poco número de sujetos disponibles, usa los criterios que sirvieron para una fábrica muy grande en la que hay muchos mecánicos de mantenimiento.

Más recientemente se ha sugerido el uso de lo llamado en inglés "construct validity". Se crea una variable que es más bien una "construcción", basada en diversas ideas y se administran varios tests que, lógicamente, parecen medir esa construcción y se observa la relación entre estas medidas.

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El Criterio

Un aspecto importante de cualquier tipo de validación lo constituye lo que se denomina "el criterio", o sea la medida de éxito en el desempeño del trabajo. En los trabajos iniciales, se procuraba obtener un solo criterio, como por ejemplo, el número de piezas producidas. No obstante, con el tiempo, se vió que esto no era suficiente aún para las tareas más repetidas, pues había varios aspectos a ser tenidos en cuenta. Se estableció entonces el método de criterios compuestos. Varios jueces le dan diferentes pesos a diferentes aspectos del trabajo, se promedian estos y se obtiene una ecuación lineal con varias variables con un coeficiente para cada una. Se han propuesto otros métodos de sopesar el valor de cada uno de los criterios como por ejemplo: De acuerdo a la confiabilidad; proporcionalmente a la correlación con otras variables, los subcriterios con base en el grago en que permiten hacer predicciones (Hotteling, 1950); análisis factorial; sopesar para maximizar la diferencia entre individuos; dar pesos iguales; o pesar en términos de costo en dinero.

Cualquiera de los que antecede no son sino una variante del método estadístico, cada una de las cuales tiene sus méritos y sus defectos. Ghiselli (1956) ha propuesto usar criterios múltiples y Dunette (1963) ha llegado al extremo de decir "a la basura con el criterio" lo que constituye un reconocimiento de la enorme dificultad para establecer criterios útiles.

Predicción

Otro aspecto es el de cómo hacer predicciones. Aparte del método de correlación simple ya mencionado está el de predicción múltiple, mencionado con el uso de varios criterios a lo cual se añade el sistema de cortes múltiples en el que establece un mínimo para cada variable, el perfil ideal para la tarea, el de vallas múltiples que debe ir salvando el candidato antes de ser admitido, y el uso de variables moderadoras según el cual la variable moderadora cuando se modifica tiene efecto sobre la relación entre otras dos variables. Vroom (1960) por ejemplo, demostró que el razonamiento no verbal tenía cierta validez para predicción de supervisores siempre que se tratase de personas altamente motivadas.

Problemas

Pero todo lo que antecede no sirve más que para señalar que hay una serie de problemas a resolver en lo que respecta a la validación. Los problemas, sin embargo, no surgen tanto del método estadístico empleado sino del hecho de que se está en todos esos casos tratando de usar métodos de laboratorio que son muy útiles en el ámbito en que se pueden controlar todas las variables.

Uno de los problemas más corrientes que vemos es que rara vez, hay un número suficiente de personas realizando la misma tarea en exactamente las mismas condiciones. No creo que sea válido tampoco agrupar a personas que realizan tareas que poseen el mismo título como ser "gerente". Es muy diferente ser gerente de un supermercado, a ser gerente de una agencia de viajes, o de una fábrica de televisores, o de un sector de un hospital. El hecho de ser gerentes les da muy poco en común. Por lo tanto la búsqueda de *la o las* características necesarias para ser un buen gerente de cualquier cosa, es una quimera ilusoria. Lo mismo sucede para todas las demás ocupaciones.

Otro problema surge de las variaciones que ocurren a diario en las organizaciones. En las industrias se modifican productos, se cambian turnos, se desajustan las máquinas, se modifican las condiciones ambientales, hay ausentismo, etc., cosa que hace casi imposible mantener un control adecuado sobre todas las variables.

Otro problema surge del hecho de que las personas y las tareas son demasiado complejas como para que se pretenda obtener predicciones con tres o cuatro variables. Pensar hacer correlaciones entre cantidad grande de criterios, y muchas variables para una cantidad enorme de tareas en una industria, (tareas estos que están continuamente cambiando y para las cuales muchas veces hay muy pocos sujetos) es totalmente utópico. No obstante, se reconoce que si no se obtiene algún método de validación se corre el riesgo de caer en la charlatanería, pues el sistema que más agrada a uno puede dar una apariencia de bueno

para hacer predicciones cosa que otro por su parte no está dispuesto a aceptar.

Una Solución

En vista de los inconvenientes planteados, y frente a la necesidad de establecer algún tipo de validación, se ha procedido a crear un método muy diferente a todos los que se han propuesto. El método, como muchos de los que hemos establecido en Tecnología Social, se basa en parte en los métodos usados en otras tecnologías. En muchas de ellas no se procede a hacer el análisis de los detalles de los procesos sino que se estudia el producto terminado. Tal es el caso de la fabricación de condensadores para radio y televisión. En estos resulta poco práctico hacer ensayos en las diversas etapas de la fabricación. Más bien se analiza al condensador completo, terminado como sistema.

El método que propongo y que expongo aquí consiste pues en validar no las tareas individuales frente a diversos tests propuestos sino a todo el *sistema* de selección de personal particularmente aquel que se basa en el uso de las habilidades mentales que provienen del modelo de estructura del intelecto de Guilford.

El procedimiento de selección es el siguiente: Se hacen descripciones de tareas para las cuales se ha de elegir personal. Los supervisores son adiestrados en las habilidades mentales de Guilford. Luego, cada uno de ellos estima, conociendo las tareas y las habilidades que habilidades son necesarias y en qué grado para cada tarea. Se eligen candidatos que posean dichas habilidades en el grado deseado y el supervisor escoge entre esos candidatos a aquellos que crea son los más convenientes. Inclusive algunas veces, se ve obligados a elegir a alguien que no cumple con el requisito previo de poseer alguna habilidad mental en particular. Elegida la persona, se le pone a trabajar en la tarea.

Se plantean dos hipótesis: 1. Las personas elegidas según lo expuesto anteriormente serán eventualmente consideradas superiores, cualquiera sea su tarea en aspectos tales como entender las instrucciones que se les den. Esto es aplicable, tanto a gerentes de fábrica, como a operarios, o empleados administrativos. La superioridad sobre personas elegidas por los métodos convencionales deberá ser muy elevada y no simplemente a un nivel de 5% o de 1%. Como corolario se establece la hipótesis de que no deben surgir diferencias en aspectos de trabajo no relacionados con características intelectuales. 2. Las personas elegidas así, mostrarán una menor dispersión, es decir serán más homogéneas en los juicios que se emitan que las personas elegidas por métodos convencionales.

Método

En una fábrica que emplea 66 personas, se está procediendo desde hace dos años a elegir personas para muchos cargos, usando las habilidades mentales de Guilford. Se escogieron 37 personas elegidas de acuerdo con dichas habilidades mentales que ya tuviesen un tiempo suficiente de actuación como para poder ser juzgadas. Esto varió, desde tres meses para un operario común, hasta seis para un ingeniero de mantenimiento.

Se eligieron al azar 57 personas elegidas por sistemas convencionales. Esto se hizo tomando personas cuyas fichas, en el fichero de personal, estuviesen lo más próximo posible a las personas elegidas y que estuviesen en tareas que tuviesen alguna semejanza. Por ejemplo, en el caso de un mecánico tornero, se le comparó con un operador de molino, aunque las habilidades necesarias para ambas tareas son muy diferentes.

Se suministró a los supervisores de cada uno de los arriba mencionados un cuestionario con 15 preguntas en las que debían indicar si la persona estaba muy buena o muy inferior en su desempeño del cargo o puntos intermedios en una escala de 11 puntos. Se les presentó como un ensayo de un nuevo tipo de formulario para evaluar personal y debían hacerlo en forma anónima. Aunque el formulario indicaba el nombre, cargo y fecha de ingreso de la persona, para mantener el anonimato, debían arrancar la información que identificaba a las personas juzgadas después de responder y entregar los juicios en un sobre cerrado en un buzón. De esa forma nos asegurábamos que contestarían con la seguridad de que podían dar sus opiniones con entera libertad sin sentirse presionados.

Una vez recibidos los cuestionarios, se calculó la media y desviación standard de las respuestas a cada una de las seis preguntas consideradas cruciales. Estas son:

1. ¿ Entiende las explicaciones que se le dan sobre procedimientos a usar en su trabajo?
2. ¿ Ejecuta las instrucciones y explicaciones recibidas?
3. ¿ Soluciona problemas previstos y no previstos surgidos de su trabajo para las que se dió instrucciones específicas?
4. Entre las diversas funciones que forman parte de su trabajo, ¿ Sabe cuáles son las más importantes y cuáles las menos?
5. ¿ Las soluciones que encuentra a los problemas son de calidad?
6. ¿ Ejecuta puntualmente los planes u órdenes, dentro de los plazos previstos, o establecidos?

Se notará que, en todos los casos, las preguntas se refieren principalmente a la capacidad intelectual necesaria para realizar la tarea, cualquiera sea ésta.

Resultados

La tabla I. muestra los resultados obtenidos. De la misma se desprende que las diferencias son altamente significativas. En las preguntas 2, 4 y 6, que son las que reflejan en definitiva el resultado que se desea obtener, los valores de "t" son mayores a 5, o sea que $p < 0.000001$. Las diferencias, pues, son de gran envergadura a favor del nuevo sistema de selección. Para las preguntas 1, 3 y 5 las diferencias son también significativas pero con $p < 0.01$. Se nota, también, que las desviaciones standard son inferiores para los seleccionados con el sistema de habilidades mentales de Guilford, que con los del sistema convencional. Es cierto que al estar la media más alta, hay poca posibilidad de dispersión hacia valores más altos.

Tabla I
CALIFICACIÓN SEGÚN FORMA EN QUE FUERON ELEGIDOS

Pregunta	Método investigado			Método convencional			t
	N	X		N	X		
1	37	2.94	1.75	57	4.1	2.35	2.74*
2	37	2.79	1.18	57	4.62	2.30	5.10**
3	36	3.52	2.11	56	4.70	2.54	2.42*
4	36	3.05	1.46	57	4.84	2.51	5.65**
5	34	3.62	2.15	56	4.79	2.25	2.46*
6	37	3.16	1.18	57	4.66	1.98	5.30**

*. $p < 0.01$

** $p < 0.000001$

Se ha validado entonces el sistema entero, en vez de hacerlo para cada tarea. Esto muestra que hay una gran ventaja en usar las habilidades mentales de Guilford para la selección de personal para tareas muy diversas. Se establece como condición que parte del sistema validado consiste en que los supervisores estén bien adiestrados en el significado de las habilidades mentales y que puedan, así, juzgar bien si las tareas a ser consideradas requieren, o no, determinadas habilidades.

El método de validación por sistemas parece ser una mejora sensible sobre los intentos de validar mediante el estudio de casos individuales.

Falta aún otro aspecto que estimo necesario para validar un sistema de selección. Esto sería obtener la opinión de los seleccionados por ambos medios respecto a si consideran que el trabajo que realizan les es intelectualmente satisfactorio. Un buen sistema de selección debe satisfacer a ambas partes. Lamentablemente, debido a circunstancias ajenas al estudio no fue posible analizar esta parte del estudio que se espera poder llevar a cabo en una etapa futura.

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THE ADULT GROWTH EXAMINATION: A FOLLOW-UP NOTE ON COMPARISONS BETWEEN RAPIDLY AGING ADULTS AND SLOWLY AGING ADULTS AS DEFINED BY BODY AGE

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This paper presents an analysis of rapidly aging adults (10 men and women with body ages at least a decade older than calendar age) and slowly aging adults (9 women and men with body ages at least a decade younger than their calendar age) in contrast to a normal control group. Factors more typical of rapidly aging adults were low annual income, less than happy whole life rating, and cigarette smoking. There were also three times as many first born adults in the rapidly aging group as in the slowly aging group or the control group.

Este trabajo presenta un estudio de adultos que están envejeciendo rápidamente (10 hombres y mujeres cuya edad del cuerpo es por lo menos una década mas alta que la edad cronológica) y adultos que están envejeciendo despacio (9 mujeres y hombres cuyas edades del cuerpo son al menos 10 años menos que las edades cronológicas) así como un grupo de sujetos control. Los factores más típicos de los adultos que envejecen rápidamente es una bajo ingreso anual, una evaluación de su vida como menos que feliz, así como el consumo de cigarrillos. También se encontró que tres veces más sujetos eran primogénitos en el grupo de adultos que envejecen mas rápido, una relación que no se encontró en los otros dos grupos.

People age at different rates. Psychologists have traditionally focused their measurement tools of individual difference on mental and emotional growth, accepting body age as a constant best measured by the birth certificate or the calendar. Calendar age, however, can be a grossly misleading index of adult aging. Further, it now appears that longevity is strongly influenced by many of the environmental variables psychologists concern themselves with (Prehoda, 1968; Kohn, 1971; Segerberg, 1974; Leaf, 1975; Morgan, 1976).

Mortality is the usual measure of longevity. While measured with some accuracy, it is nevertheless a difficult index to work with in human research. One operates solely with senescent subjects, grimly awaiting differential mortality, or one assumes the experimenter will live long enough to accurately follow adult subjects through the decades. An alternative is to use a standardized test of adult aging, much as Murray (1951) advocated, to determine the consequences of interventions against aging or the consequences of environmental life styles on aging. The Adult Growth Examination (AGE) was developed for this purpose (Morgan, 1968, 1968 (a), 1969 (b), 1970, 1971, 1972; Morgan & Fevens, 1976).

One psychologist (Elkind, 1972) has already used the AGE as a measure of the effectiveness of an anti-aging intervention: Group hypnosis. In a study of 19 women, calendar aged 39 to 56 years, the 9 who received anti-aging hypnotic sessions all evidenced a drop in body age when retested three weeks after hypnosis. The median age loss was 11 years with a range of 3 to 18 years; all but the smallest drop were beyond the standard error of measurement in magnitude. The median

age loss of the control group, hypnotized without anti-aging instructions, was zero years with all changes falling within the standard error of measurement. Elrind's study is discussed in more depth elsewhere (Morgan, 1976).

Another use of the AGE test would be to differentiate rapidly aging adults from slowly aging adults on the basis of psychological, social, or biological characteristics. This was suggested by quite a few readers of the validation study on AGE published in this journal a few years ago (Morgan, 1972). Accordingly, the following preliminary analyses were made from the pool of people already having taken the AGE (in Nova Scotia, Canada) who had also completed extensive data forms on their environmental life styles and personal characteristics.

Rapidly aging adults were defined as those men and women whose body age was at least a decade older than their calendar age. Eight women and two men were so identified and made up this group. Their mean calendar age was 46 years (range 40 to 60 years) which was very close to the body age they estimated they would have (47 years) but the actual mean body age was 61 years (range 53 to 71+ years).

Slowly aging adults were defined as those men and women whose measured body age was at least a decade younger than their actual calendar age (more than two standard errors of measurement). Two women and seven men were so identified. Their mean calendar age was 53 years (range 40 to 60 years) with their self estimate of aging a mean 47 years, the same as the rapidly aging adults. Yet the mean body age was 38 years (range 19 to 49).

For purposes of comparison, a *normal aging* group was drawn by the criterion of a body age within (\pm) 5 years of the calendar age. Ten women and eight men comprised this group. The calendar age, body age, and estimated age for this group all averaged 52 years. These data are presented in Table 1.

Table 1

ANALYSES OF SLOW, NORMAL, AND RAPID AGE RATE GROUPS OF NOVA SCOTIANS OF BOTH SEXES: AGE IN YEARS BY BIRTH (CA), BY MEASUREMENT (BA), BY SELF ESTIMATE (EA)

Age rate group:	"Young" Slow Agers	Normal Agers	"Older" Rapid Agers
Number of People	9	18	10
Average CA:	53	52	46
Average BA:	38	52	61
Average EA:	47	52	47
CA range:	40-60	40-60	40-60
BA range:	19-49	36-65	53-71+
EA range:	30-60	32-60	35-55
Mean difference from CA:			
BA-CA:	-15	0	+15
EA-CA:	-6	0	+1
Mean difference from BA:			
CA-BA:	+15	0	-15
EA-BA:	+9	0	-14

Additional note on EA: Using CA as a reference point, 0% of the "young" slow agers estimated themselves *older* than their birth age (normal agers estimated themselves *older* 17% group) while "older" rapid agers estimated themselves *younger* than their birth age in only 20% of the group (normal agers estimated themselves *younger* 44% group). Thus incidence of mistaken direction of aging was low in both slow and rapid age rate groups.

Note that 100% of the slowly aging adults and 80% of the rapidly aging adults already knew, by self estimate, which direction their aging had gone, although the magnitude was often underestimated. The normal aging group was also quite accurate as to direction and magnitude of the congruence between their body and calendar ages. An observational note here is that virtually all rapidly aging adults could be distinguished from normally aging adults (and the latter from slowly aging adults) on the basis of appearance and behavior, signs not lost to their own self estimates.

The analyses of the self report data are presented in Table 2. These data suggest promising leads but, of course, the number of subjects is too small for any conclusions, particularly without cross validation. However, the results are consistent with more ambitious and affluent longitudinal longevity studies where mortality was the measure (Seegerberg, 1974): Rapidly aging adults have less money, smoke more, and characterize their lifetime as more unhappy than happy. In addition, there were three times as many firstborn adults in the rapidly aging group as in the other two groups. Fifteen other factors which did not differentiate the groups are given in Table 2.

Table 2

ANALYSES OF SLOW, NORMAL, AND RAPID AGE RATE GROUPS OF NOVA SCOTIANS OF BOTH SEXES: SELECTED DIFFERENTIATING FACTORS FROM QUESTIONNAIRE DATA

Age rate group:	"Young" Slow Agers	Normal Agers	"Older" Rapid Agers
Number of people	9	18	10
% Females:	44	56	80
Median annual income:	\$6000.00	\$6000.00	\$3881.00*
Maximum annual income:	\$20000.00	\$15000.00	\$6000.00
Lifetime happiness:	78%	67%	40%
Present happiness:	44%	56%	50%
First born:	22%	22%	66%
Median number of cigarette packs smoked per week over Lifetime:	0	0	2

Undifferentiating factors: Marital status, Marital satisfaction, Number of children, Number of occupants in house, Religion, Church attendance, Present health, Lifetime health, Physical appearance, Personal adjustment, Punctuality, Job satisfaction, Present happiness, Alcohol consumption, intercourse frequency.

* $P < .05$. Efficiency percentage (Morgan, 1968b) for differentiating rapid agers from normal group is 99% and from slow agers is also 90%.

Thus, the AGE test of individual adult aging has replicated a few of the basic factors associated with enhanced or retarded longevity when mortality was the measure. This footnote further demonstrates the way in which AGE may be used to test for further group differences associated with differential aging.

The assessment of adult aging for people 19 to 71 years calendar age offers the opportunity for individuals to monitor their own aging process and to trace the effects of self chosen interventions in diet, life style, etc. Students and professionals in psychology, sociology, biophysics, nutrition, medicine, and nursing now have the personal opportunity to apply immediate feedback on changes in the rate of aging to the life sciences, health care, and their development.

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TEST RÁPIDO BARRANQUILLA Y REVISED BETA EXAMINATION EN SUJETOS PUERTORRIQUEÑOS

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Las pruebas BARSIT (Test Rápido Barranquilla) y Revised Beta Examination fueron administradas a clientes puertorriqueños del programa de Rehabilitación Vocacional. Las anotaciones crudas en el BARSIT fueron convertidas a cocientes de inteligencia tipo Wechsler y los resultados obtenidos se compararon con los cocientes obtenidos con la prueba Beta. Los resultados señalaron un C.I. BARSIT promedio para esta muestra de 93.71, d.s.: 16.9, y un C.I. Beta promedio de 84, d.s.: 14.5. Aunque las dos medidas de C.I. correlacionaron a un nivel significativo (+.68), la diferencia entre el C.I. promedio en ambas pruebas fue significativa estadísticamente. Se encontró una correlación mayor entre el C.I. BARSIT y el nivel educativo de los sujetos que entre el C.I. Beta y la variable de educación.

The Barranquilla Rapid Survey Test (BARSIT) and the Revised Beta Examination were administered to Puerto Rican Vocational Rehabilitation clients. BARSIT raw scores were converted to Wechsler-type IQ's, and these scores were compared to the IQ's obtained with the Revised Beta. The obtained results indicated a mean BARSIT IQ of 93.71, sd: 16.9 and a mean Beta IQ of 84, sd: 14.5, for the sample. Although there was a significant correlation between both IQ measures (+.68), the difference between the average IQ for the two tests was statistically significant. BARSIT IQ scores were found to correlate significantly with the educational level of the subjects, but the same was not true for the Beta IQ scores.

Debido a la escasez de instrumentos de evaluación rápida del funcionamiento intelectual que hayan sido normalizados en poblaciones puertorriqueñas, el psicólogo puertorriqueño tiene que depender en gran medida de instrumentos normalizados en poblaciones extranjeras. Una forma de minimizar la posible discrepancia entre las normas extranjeras y la ejecución de sujetos puertorriqueños, a falta de normas locales, es utilizar pruebas no-verbales u otras normalizadas en poblaciones de habla hispana, aunque no sean puertorriqueños. Esta última alternativa supone una mayor similitud entre sujetos puertorriqueños y otros de origen hispano que con otros grupos culturales. Las pruebas Revised Beta Examination (Psy. Corp., 1957) y Test Rápido Barranquilla (Barranquilla Rapid Survey Test: BARSIT; Del Olmo, 1958) son ejemplos de una prueba no verbal (Beta) y una de tipo verbal normalizada en poblaciones suramericanas (BARSIT).

La prueba Beta, revisada por Kellog y Morton (1934) y re-normalizada por Lindner & Gurvitz (1946) es descrita en el *Manual* (1957) de administración como una medida de habilidad intelectual general para personas que son relativamente analfabetas o que no hablan inglés. Esta descripción ha sido criticada por Goldman (1965), quien ha señalado el hecho de que los autores de la prueba no presentaron datos estadísticos que apoyaran su idea respecto a la utilidad de la prueba con sujetos analfabetos o de habla no-inglesa. Particularmente, Goldman ha cuestionado si es posible que una muestra de re-normalización que no incluyó personas no-caucásicas, mujeres, ni individuos no-reclusos en prisión, pueda ser representativa de analfabetos o de poblaciones de habla no-inglesa.

El BARSIT, por otra parte, es una prueba verbal corta, de 10 minutos de duración la cual es descrita en el *Manual* de administración (Del Olmo, 1958) como "un test para obtener rápidamente un índice de inteligencia; para uso de escolares de habla hispana; para uso

de adultos con niveles de instrucción primaria". Por lo menos un estudio ha reportado el uso del BARSIT en muestras Puertorriqueñas. García-Palmieri & Suárez (1972), usando una muestra de veteranos Puertorriqueños hospitalizados por razones psiquiátricas, reportaron una puntuación cruda promedio de 31.8 puntos en el BARSIT (*d.s.*: 10.9). Esta puntuación cae alrededor de los percentiles 23 y 27 de las normas del BARSIT para adultos con más de 6 años de educación. Aunque la ejecución de estos sujetos en el BARSIT fué baja, se puede considerar consistente con los reportes encontrados típicamente en la literatura sobre el funcionamiento intelectual de pacientes psiquiátricos, los cuales tienden a funcionar por debajo del promedio para la población general.

El propósito de este estudio fue el de proveer información respecto a la relación y diferencias que se pueden obtener al utilizar las pruebas BARSIT y Beta con sujetos Puertorriqueños.

Método

Sujetos

La muestra consistió de 31 clientes referidos consecutivamente para evaluación psicológica por el programa de Rehabilitación Vocacional del gobierno de Puerto Rico. La gran Parte de estos sujetos habían reclamado ayuda económica de Rehabilitación Vocacional debido a la presencia de problemas de tipo psicológicos o en el área intelectual. La muestra, de 12 varones (39%) y 19 hembras (6%), tuvo una edad promedio de 25.77 (*d.s.*: 10.13), con una fluctuación de edades entre 16-51. La educación promedio fue de 10.97 años (*d.s.*: 2.17) con una fluctuación entre 6-15 años (la muestra incluyó cuatro personas que estaban cursando estudios a nivel universitario). Solamente se usaron en la muestra sujetos con seis o más años de educación escolar, que supieran leer, y que no tuvieran impedimentos físicos que les impidiera leer o entender las instrucciones.

Procedimiento

Los sujetos fueron examinados en grupos pequeños de no más de 10 personas. Todos los sujetos recibieron ambas pruebas. Las instrucciones de administración utilizadas fueron las especificadas en los manuales de las pruebas usadas. Para la prueba Beta, se usó la traducción castellana de las instrucciones incluidas en el Manual de la edición Española de la prueba (Kellog & Morton, 1972).

La prueba BARSIT consiste de 60 ítems, la mayoría de las cuales se contestan por medio de selección múltiple. La duración de la prueba es de 10 minutos y la puntuación cruda consiste del número de contestaciones correctas. La puntuación cruda es comparada con los normotipos de la prueba, consistentes en centiles obtenidos de varias muestras Venezolanas. Con el propósito de hacer comparables los resultados de las pruebas BARSIT y Beta, se preparó una tabla de conversión de puntuaciones crudas del BARSIT a cocientes de inteligencia con promedio de 100 y desviación standard de 15, usando las normas de trabajadores Venezolanos con 6 o más años de educación. Puntuaciones crudas menores de 15 fueron asignadas un C.I. de 65.

La prueba Beta es una prueba de tiempo, consistente en seis subpruebas cuyas puntuaciones crudas se convierten en puntuaciones a escala similares a las de las pruebas Wechsler. El total de estas puntuaciones a escala se compara con una tabla de conversión a cocientes intelectuales tipo Wechsler. Las normas incluyen una corrección por edad, por tanto existen normas para 9 grupos de edades diferentes, desde los 16 a los 59 años de edad.

Resultados

La puntuación cruda promedio en el BARSIT fué de 33.32, *d.s.*: 12.31, fluctuando las anotaciones entre 2 - 53 puntos crudos. El C.I. BARSIT fué de 93.71, *d.s.*: 16.9, fluctuando las anotaciones de C.I. entre 65 - 123. El C.I. Beta fué de 84, *d.s.*: 14.5, fluctuando las anotaciones entre 46 - 110. La prueba *t* para grupos correlacionados indicó una diferencia significativa entre los promedios de C.I. BARSIT y Beta ($t = 4.28, p < .001$). Cabe notarse que parte de la diferencia entre las puntuaciones de C.I. BARSIT y Beta se podría atribuir

alhecho de que el límite inferior usado con la prueba BARSIT fue de C.I.: 65, mientras que en el Beta se pueden obtener anotaciones más bajas. Sin embargo, sólo dos sujetos (6%) obtuvieron un C.I. menor de 65 en el Beta. Por otra parte, la correlación Pearson entre ambas medidas de C.I. fue de +.68, ($p < .01$). La correlación entre el C.I. Beta y la anotación cruda de BARSIT fue levemente menor ($r: +.66, p < .01$).

Cabe observarse que la anotación promedio más baja fue en la subprueba 4 del Beta. Este hallazgo es consistente con los de estudios con poblaciones estadounidenses, donde la anotación promedio en la subprueba 4 tiende a ser la más baja (c.f. Dudley, Mason, & Rhoton, 1973). Debido a la dificultad de los sujetos en el estudio presente con la subprueba 4, fué de interés el investigar cuánto subiría el C.I. Beta promedio de esta muestra al eliminar la subprueba 4 y prorratear el C.I. a base de las restantes subpruebas. Los resultados obtenidos con el Beta-4 indicaron un leve aumento en el C.I. promedio de la muestra, de 84 a 86.23 (d.s.: 15.52), fluctuando las anotaciones entre 39-113. Aunque la correlación del C.I. Beta-4 y el C.I. BARSIT (+.70) fue algo mayor que la del Beta completo con el BARSIT, la diferencia entre ambas medidas de C.I. continuó siendo significativa ($p < .01$).

Se encontró una relación significativa entre las anotaciones crudas y de C.I. BARSIT y el nivel educativo de la muestra ($r: +.46, p < .02$, y $.50, p < .01$, respectivamente), y una correlación no-significativa entre el C.I. Beta y el nivel educativo de la muestra ($r: +.29, p > .10$). No se encontró una correlación significativa entre las pruebas de inteligencia y la variable de edad.

Discusión

El presente estudio revela una correlación moderadamente alta entre la prueba BARSIT y la prueba Beta. El coeficiente de correlación obtenido entre los C.I. de ambas pruebas (-.68) compara favorablemente con los 11 coeficientes de correlación entre el Beta y otras pruebas presentados en el *Manual* (1957) del Beta. Estos revelan una fluctuación de correlaciones entre .36 y .75, con una correlación mediana de .66. Por otra parte, se obtuvo una diferencia altamente significativa entre los C.I. promedio obtenidos con cada prueba, siendo la diferencia de más de nueve puntos de C.I. Ante esta diferencia, cabe preguntarse si el BARSIT sobreestimó la eficiencia intelectual de los sujetos usados en esta muestra, o si el Beta la subestimó. Aunque la muestra usada en este estudio es limitada y no permite una contestación clara a esta pregunta, la información preliminar favorece la hipótesis de una subestimación intelectual por parte de la prueba Beta. Para ilustrar, cuatro sujetos cursando estudios a nivel universitario obtuvieron un C.I. promedio Beta de 87 (incluyendo un sujeto que obtuvo un C.I. de 77). El C.I. promedio de estos mismos sujetos en el BARSIT fue de 112.5, lo que es más consistente con la ejecución que se esperaría de personas cursando estudios a nivel universitario.

La puntuación BARSIT cruda promedio obtenida por los sujetos en este estudio es parecida a la obtenida por otra población clínica Puertorriqueña (García-Palmieri & Suárez, 1972).

Lo reducido de la muestra del presente estudio limita la generalidad de las conclusiones. Sin embargo, estudios conducidos subsiguientemente por el autor tienden a confirmar varios de los hallazgos del estudio presente. Usando dos muestras comparables de prisioneros puertorriqueños, a 100 de los cuales se les administró el Beta y a 99 el BARSIT, se encontró que la primera muestra obtuvo un C.I. Beta promedio de 80.52, d.s. 13.55, y la segunda un C.I. BARSIT promedio de 89, d.s. 14.65. La diferencia entre los C.I. promedios obtenidos por las muestras fue significativa al nivel .001 de probabilidad, y tiende a confirmar el hallazgo del presente estudio respecto a la tendencia de los C.I. BARSIT a ser mayores que los C.I. Beta. La correlación entre el C.I. BARSIT y la educación de los reclusos fue de .60 (comparados con .50 en el estudio presente), mientras que el C.I. BARSIT y la edad de los reclusos correlacionó -.21 (n.s.) (-.17 en el estudio presente). Con la prueba Beta se encontró una correlación de .55 entre la educación de los prisioneros y el C.I. Beta. Esta correlación fue más alta que la encontrada con los sujetos del presente estudio (.29). La correlación entre el C.I. Beta y la edad de los prisioneros fue de .27; aunque

más alta que la del estudio presente (-.08), tampoco fue significativa. Al igual que en el estudio presente, los prisioneros obtuvieron sus anotaciones más bajas, en promedio, en la sub-prueba 4.

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ESCALA DE ACTITUDES HACIA LA SOCIALIZACION DE LA MEDICINA II¹

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El análisis factorial de la matriz de intercorrelaciones entre los puntajes asignados por los sujetos-jueces a los setenta ítems de una escala de actitudes hacia la socialización de la Medicina, reveló que estas correlaciones pueden ser explicadas en términos de ocho factores complejos. La factorización de las correlaciones entre ambos factores primarios indicó que éstos pueden ser explicados por dos factores de segundo orden que, en líneas generales, concentran, uno, opiniones favorables y el otro, opiniones desfavorables a la socialización de la Medicina.

The factorization of the correlations between the scores given by judges to the seventy items of a scale towards socialized Medicine revealed that these intercorrelations can be explained in terms of eight complex factors. The factorization of the correlations between the primaries indicated that these factors can be explained, in turn, by two second order factors, one of which concentrates favorable opinions and the other, unfavorable opinions towards socialized Medicine.

El presente trabajo forma parte de una investigación más amplia que incluyó la construcción de una escala de actitudes hacia la socialización de la Medicina y el análisis estadístico de los ítems de la misma (Schufer de Paikin, 1973 a). Los objetivos del presente estudio son: determinar experimentalmente cuál es el número de dimensiones que se necesitan para explicar las correlaciones entre los ítems de la escala; descubrir empíricamente qué ítems están relacionados entre sí; interpretar los grupos de ítems así formados.

Método

La matriz de intercorrelaciones entre los puntajes asignados por los sujetos-jueces a los setenta ítems de la escala de actitudes hacia la socialización de la Medicina fue factorizada por el método de ejes principales. Para estabilizar las comunalidades se realizaron dos factorizaciones, la primera usando las comunalidades estimadas (el valor más alto de la columna) y la segunda, con las comunalidades resultantes del primer análisis. Con el fin de obtener la estructura simple (Thurstone, 1947) se aplicó a dichos factores una rotación Varimax-solución sucesiva- (Horst, 1965), dos rotaciones Promax (Hendrickson & White,

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1974) y cinco rotaciones oblicuas manuales. Las correlaciones entre primarios fueron factorizadas por el método centroide.³

Resultados

Los Factores

Factor A

Este factor se refiere a los aspectos: Asistencial, Organizativo, Económico, Profesional y Educativo del Sistema Socializado.

— asistencial: se refiere a la extensión de los servicios médicos a toda la población y a la calidad de los mismos. Por ejemplo: "Un sistema socializado brinda iguales posibilidades de atención a todas las clases sociales".

— organizativo: se refiere a la distribución eficaz de profesionales y equipos de diagnóstico y tratamiento y a las posibilidades que el sistema brinda al paciente de elegir el tipo de servicio médico que prefiera. Por ejemplo: "Todos los médicos tienen trabajo en un sistema socializado".

— económico: se refiere a que en el sistema socializado no existe la competencia económica, lo que evita la comercialización de la Medicina y hace que los médicos puedan cumplir más ampliamente su misión social. Por ejemplo: "Un sistema socializado evita la competencia económica entre los médicos".

— profesional: se refiere a la continua actualización profesional que este sistema requiere de los médicos y a la mayor dedicación de éstos a tareas de investigación. Por ejemplo: "En un sistema socializado se exige a los médicos una continua actualización profesional".

— educacional: trata de la adecuación de la enseñanza de la Medicina a las necesidades del país y de la mayor amplitud del currículum de esa carrera. Por ejemplo: "En un sistema socializado la enseñanza de la Medicina puede abarcar más ramas del conocimiento médico".

Todos los ítems agrupados en este factor representan opiniones favorables a la socialización de la Medicina, excepto dos ítems cuyos valores escalares los ubican en la zona de neutralidad del continuo. Sin embargo, al revisar las correlaciones originales se encontró que están altamente correlacionados con ítems favorables a la socialización de la Medicina, por lo que resulta consistente su inclusión en este factor.

Factor B

Este factor apunta a los aspectos: Asistencial, Científico y Profesional, expresando las consecuencias negativas de la aplicación de un sistema socializado.

— asistencial: se refiere al deterioro de la asistencia médica, pues se destruye la relación médico-paciente. Por ejemplo: "Un sistema socializado coarta la libertad individual tanto de médico como del paciente".

— científico: trata de la neutralización del progreso científico dado que la investigación queda relegada. Por ejemplo: "La Medicina socializada neutraliza el progreso científico".

— profesional: se refiere a la falta de progreso del médico, porque no se le ofrecen posibilidades de superación y se le hace perder el interés por adquirir nuevos conocimientos. Por ejemplo: "El sistema socializado no ofrece posibilidades de progreso al médico".

Los enunciados que se agrupan en este factor expresan opiniones desfavorables a la socialización de la Medicina, con excepción de dos ítems que representan opiniones neutras, pero que tienen altas correlaciones con ítems desfavorables a la socialización de la Medicina. Este factor tiene correlación positiva con los factores C y H.

Factor C

Es un factor bipolar, que se refiere a los aspectos asistencial y científico. Varios de los ítems que definen este factor tienen también saturación en el factor B.

— asistencial: se refiere al deterioro de la asistencia médica en el sistema socializado,

³ Las tablas correspondientes así como el resto de los ítems se hallan a disposición del lector, quien puede solicitarlas a la Biblioteca de CIIPME, Habana 3870. Buenos Aires, Argentina.

debido, entre otros factores, a la perturbación de la relación médico-paciente. Por ejemplo: "El sistema socializado no aumenta ni disminuye la calidad de los servicios médicos".

Es difícil interpretar la bipolaridad de este factor. Una explicación plausible sería que a una creciente despersonalización de la relación médico-paciente corresponde una disminución en la calidad de los servicios médicos.

— científico: incluyendo dos ítems: "El sistema socializado no acelera ni detiene el proceso científico de la Medicina". "Los descubrimientos en Medicina son independientes del sistema de atención médica imperante".

Factor D

Es un factor bipolar. En el polo positivo se agrupan ítems que indican que el cuidado de la salud es un servicio público. Por ejemplo: "Como la educación, la atención médica es un servicio público".

Factor E

Es también un factor bipolar; en el polo negativo se agrupan ítems que apuntan a los problemas económicos de la Medicina actual, es decir del sistema de práctica privada. Por ejemplo: "La Medicina actual es demasiado costosa para ser afrontada individualmente".

Oponiéndose a estos conceptos, en el polo positivo del factor se encuentran ítems que expresan una opinión favorable al sistema de práctica privada, refiriéndose, sin embargo, a otros aspectos del problema. Por ejemplo: "El sistema de práctica privada es el único que garantiza el alto nivel científico de los médicos".

Sin embargo, los enunciados con mayor saturación en este polo se refieren, como los del polo negativo, al aspecto económico, expresando por ejemplo que "El sistema socializado ni mejora ni empeora el problema económico del cuerpo médico".

Factor F

Este es un factor bipolar en cuyo polo positivo se agrupan ítems que apuntan al problema económico en lo que se refiere a su justa ubicación en el sistema de valores de la profesión médica en beneficio de su función social (esta interpretación concuerda con la realizada para los factores A y E). Por ejemplo: "El incentivo económico se reduce a sus proporciones justas en el sistema socializado".

El polo negativo de este factor está representado por dos ítems que se agrupan también en el factor E expresando una opinión favorable al sistema de práctica privada.

Factor G

Es un factor bipolar que concentra ítems que se refieren al aspecto organizativo. Por ejemplo: "El trabajo del médico está mejor organizado en un sistema socializado".

Factor H

Este factor está definido por ítems que apuntan al aspecto profesional, refiriéndose a las características negativas que el ejercicio de la profesión tendría en un sistema socializado a causa de: — falta de incentivo para el desempeño de la profesión, lo que la convierte en un trabajo rutinario. — burocratización del ejercicio de la profesión y de la atención médica por interferencia de organismos administrativos, con el consiguiente deterioro de la asistencia prestada.

Este factor tiene una alta correlación positiva con el factor B que, como se vio, también concentra ítems desfavorables a la socialización de la Medicina en lo que hace a la falta de progreso de la ciencia y del médico y al deterioro de la asistencia médica.

El Segundo Orden

La factorización de las correlaciones entre primarios indica que estas intercorrelaciones pueden ser explicadas por dos factores negativamente correlacionados. En el primer factor se agrupan los factores A, F y G; en el otro, los factores B, C, E y H. El factor D no tiene saturación en ninguno de los factores de segundo orden. En líneas generales, el primer factor agrupa los primarios que expresan opiniones favorables a la socialización de la Medicina en los aspectos asistencial, organizativo, profesional y económico. Y el segundo

concentra a aquellos primarios que hacen hincapié en el deterioro de la asistencia médica y en la falta de progreso de la ciencia y del médico que la implantación de un sistema socializado acarrearía.

Discusión

El análisis de los ocho factores que explican las intercorrelaciones entre los ítems de la escala indica que los dos primeros factores extraen la mayor parte de la varianza común y representan los dos polos opuestos del continuo favorable-desfavorable a la socialización de la Medicina. Sin embargo, estos polos opuestos no son simétricos, es decir, no están representados por ítems de contenido similar pero de signo inverso, sino que apuntan a distintos aspectos de los beneficios o perjuicios que la implantación de un sistema de Medicina socializada traería consigo.

En el polo favorable, los ítems expresan que un sistema socializado brinda iguales posibilidades de atención a todas las clases sociales, que todos los médicos tienen trabajo, que evita la competencia económica entre los médicos, etc. En cambio, en el polo desfavorable se agrupan ítems que indican que un sistema socializado deteriora la asistencia médica, en especial la relación médico-paciente y que detiene el progreso de la ciencia y del médico. Es decir que las razones por las cuales se estaría a favor o en contra de la socialización de la Medicina tienen distinto fundamento. Esto corrobora lo observado en una investigación anterior (Schufer de Paikin, 1973b) en la cual los médicos que sostenían que un proceso de socialización de la Medicina era necesario, fundaban su juicio en razones de "justicia social", como las expresadas en el primer factor; en cambio, quienes aducían que era innecesario se basaban en razones de índole "profesional" como las indicadas por el segundo factor. Esta diversidad se establece claramente en el factor E, donde los ítems detractores del sistema de atención privada apuntan a la parte económica y los que lo apoyan a la parte científica.

Los factores C y H apuntan a los mismos aspectos que el factor B, con el que están positivamente correlacionados, pero mientras el factor C hace hincapié en la calidad de la atención médica, el H hace referencia a la falta de incentivos y a la burocratización del ejercicio de la profesión en un sistema socializado.

Los factores E y F apuntan a distintos aspectos del problema económico y el factor G al aspecto organizativo, pero el agrupamiento se hace sin tener en cuenta el criterio "favorable-desfavorable".

El factor D, que no tiene correlación con ninguno de los otros, introduce expresamente la idea de que la Medicina es un "servicio público", idea que no está presente en los otros factores, ya que los que se refieren a ese aspecto lo hacen bajo la forma de "misión social".

De lo expuesto puede concluirse que, dado que la socialización de la Medicina es un problema tan complejo y que las actitudes y opiniones pueden tener distintos fundamentos, la escala debe tratarse no como un todo sino subdividirse en varias subescalas que tomen en cuenta todos los aspectos expresados por los factores obtenidos.

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ESTUDIO COMPARATIVO ENTRE ESTUDIANTES ARGENTINOS Y ESTADOUNIDENSES A TRAVÉS DEL TEST 16 PF DE R. CATTELL^{1,2}

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En este trabajo se realiza un estudio comparativo entre estudiantes argentinos y estadounidenses, utilizando el test PF de R. Cattell. Ambos grupos se analizan en términos de cada uno de los 16 factores de personalidad así como de las matrices de covarianza. Los resultados indican que la estructura factorial y los perfiles de personalidad son similares para ambos grupos, en tanto que cuando se analiza cada factor separadamente aparecen diferencias significativas en la mayoría de los casos.

A comparative study between samples of Argentine and U.S. students using Cattell's 16 PF Test is presented. Both groups were in terms of each one of the 16 personality factors as well as in terms of the covariance matrices. The data shows that the factorial structure and the personality profiles are similar for both groups, but significant differences were found, in most cases, when analysing each factor separately.

El objetivo de este trabajo es hacer un estudio comparativo entre estudiantes argentinos y estadounidenses utilizando como instrumento de evaluación el Test 16 PF (Cattell, 1970). Este test fue seleccionado entre otros posibles instrumentos verbales para la estimación de la personalidad por la abundante literatura que existe al respecto y por su aparente estabilidad factorial (Cattell, et al., 1970). Este instrumento consta de dos formas equivalentes A y B y provee información acerca de 16 variables factorialmente identificables.

Es importante analizar el problema desde dos puntos de vista:

- ¿ Existen diferencias en la estructura de personalidad entre la muestra argentina y estadounidense?
- ¿ Cuáles son los factores en los que existen diferencias, y si las hay, en qué dirección?

Mientras la segunda pregunta se refiere especialmente a diferencias o semejanzas en cada variable factorial en ambos grupos, la primera alude a diferencias o semejanzas en las interrelaciones que existen en ambos grupos entre los 16 factores. En este trabajo se tratará especialmente de analizar la segunda pregunta aunque se presentará información referida a la estructura factorial de ambos grupos.

Método

La forma A del test 16 PF fue traducida y adecuada al medio local (Cattell, 1972). Se aplicó a 150 estudiantes de una escuela técnica de varones, argentinos, cuya edad promedio era de 18,38 años, con una desviación estándar de 1,57 años. Se obtuvieron las medias aritméticas y desviaciones estándar para cada factor y una matriz de intercorrelaciones entre los distintos factores.

Se usaron las medias aritméticas y desviaciones estándar para cada factor correspon-

¹ Trabajo presentado en el XV Congreso Interamericano de Psicología celebrado en Bogotá, Colombia, en diciembre de 1974

² Este trabajo es parte de una investigación que realiza el Centro Interdisciplinario de Investigaciones en Psicología Matemática y Experimental (CIPME) bajo la conducción de su Director, Dr. Horacio J.A. Rimoldi, a quien las autoras agradecen las sugerencias y críticas que lo hicieron posible.

dientes a 1260 estudiantes estadounidenses, varones, de 17 años de edad y a 2106 sujetos de 20 años con las mismas características de los anteriores (IPAT, 1970, p. 6, p. 12), así como la matriz de intercorrelaciones entre los distintos factores, obtenida por Russell a partir de una muestra de 423 estudiantes estadounidenses, varones (Cattell et al., 1970, p. 103).

El grupo de estudiantes argentinos de 18 años se comparó, a nivel de medias aritméticas, con dos muestras de estudiantes estadounidenses de 17 y 20 años debido a que no se disponía de datos correspondientes a sujetos estadounidenses de 18 años.

En todos los casos se aplicó una prueba de significación de diferencias de medias de muestras no correlacionadas fijando el nivel de riesgo al 1%³. Las matrices de correlaciones se compararon utilizando una prueba de significación de diferencias de correlaciones para cada celda, fijando el nivel de riesgo al 1%⁴.

Resultados

En la Tabla 1 se presentan las medias y desviaciones estándar de los puntajes obtenidos en cada uno de los 16 factores de personalidad correspondientes a las muestras de estudiantes argentinos y estadounidenses respectivamente.

Tabla 1
Medias y Desviaciones Stándar de los diferentes grupos*

	Arg.	Arg.	EE. UU.	EE. UU.	EE. UU.	EE. UU.
	\bar{X}	D.S.	\bar{X}	D.S.	\bar{X}	D.S.
			17	17	20	20
			años	años	años	años
A	10.57 _a	2.42	9.03	3.01 _a	10.08	3.43
B	6.95 _b	1.68	7.04	2.17	8.83 _b	1.89
C	16.42 _{c,d}	4.11	14.03 _c	3.71	15.14 _d	3.90
E	11.33 _{e,f}	3.52	13.06 _e	3.66	13.93 _f	4.12
F	15.39 _g	4.18	15.31	4.27	16.44 _g	4.52
G	13.66 _{h,i}	3.57	11.04 _h	3.36	11.95 _i	3.75
H	15.44 _{j,k}	4.84	12.60 _j	5.01	13.75 _k	5.77
I	9.35	3.28	8.85	3.51	9.24	3.82
	Arg.	Arg.	EE. UU.	EE. UU.	EE. UU.	EE. UU.
	\bar{X}	D.S.	\bar{X}	D.S.	\bar{X}	D.S.
			17	17	20	20
			años	años	años	años
L	10.81 _{lm}	2.83	9.86 _l	3.08	8.97 _m	3.34
M	10.62 _n	3.05	11.01	3.51	12.69 _n	3.62
N	10.55 _{op}	2.53	9.25 _n	2.65	8.25 _p	2.68
O	10.89 _q	3.97	11.93 _q	3.79	10.18	3.93
Q ₁	10.11	3.27	9.72	3.05	10.16	3.23
Q ₂	10.79	3.22	10.12	3.50	10.28	3.73
Q ₃	13.37 _{rc}	3.01	11.07 _r	3.12	11.72 _s	3.27
Q ₄	10.95 _{tu}	4.46	13.33 _t	4.11	12.77 _u	4.82

*Medias con sub-índices iguales indican diferencias significativas entre las dos medias al nivel de $p < .01$

3 Fue imposible realizar un análisis más preciso por carecer de los puntajes originales correspondientes a las muestras norteamericanas.

4 Fue imposible hacer una comparación más rigurosa por no disponer de los puntajes originales de la muestra norteamericana.

Como puede observarse en 8 de los factores, las medias resultan significativamente diferentes en las 3 muestras, y en 10 factores cuando se comparan los estudiantes argentinos con los estadounidenses de 17 años de edad y en 11 factores cuando se compara la muestra de estudiantes argentinos con los estadounidenses de 20 años.

Discusión

En relación a la primera pregunta planteada en la introducción, las matrices de correlaciones correspondientes a los sujetos argentinos y estadounidenses resultaron semejantes, lo que indica que la estructura factorial de personalidad es similar en ambas muestras.

Para responder a la segunda pregunta se describirá a continuación el comportamiento de las tres muestras en cada uno de los factores.⁵

No existen diferencias significativas entre la muestra de estudiantes argentinos y las dos muestras de estudiantes estadounidenses en los siguientes factores: I (menor-mayor sensibilidad emocional), Q₁ (conservadorismo-radicalismo) y Q₃ (dependencia-independencia con respecto al grupo). En los factores A, B, F, M y O la muestra argentina difiere de una u otra muestra estadounidenses.

En los factores A y O, el grupo estadounidense de 17 años muestra mayor tendencia a la esquizotimia (incapacidad para emocionarse, apartamiento e indiferencia hacia los demás e incapacidad para expresar los sentimientos) y a la ansiedad (inseguridad en sí mismo, inadaptabilidad y dependencia con respecto a la aprobación o desaprobación de los demás) que el grupo argentino. Estas diferencias desaparecen cuando se realiza la comparación con el grupo estadounidense de 20 años de edad.

En los factores B, F y M las diferencias existen sólo entre el grupo argentino y el estadounidense de 20 años de edad. El factor B mide capacidad intelectual, es pues lógico que el grupo estadounidense de 20 años presente valores más altos que el grupo argentino de menor edad. En el factor F (estado de ánimo predominante relacionado con la introversión-extraversión) el grupo estadounidense de 20 años muestra mayor entusiasmo, alegría y despreocupación que el grupo argentino.

Con respecto al factor M (mayor-menor sentido de realidad) los resultados indican que el grupo argentino se caracteriza por un comportamiento más práctico, un pensamiento más pragmático y una conducta más convencional que el grupo estadounidense de 20 años.

En los factores C, E, G, H, L, N, Q₃ y Q₄ las diferencias entre el grupo argentino y las muestras estadounidenses son significativas. Con excepción de los factores E y Q₄ en todos los demás casos los puntajes de la muestra argentina son más altos.

Con respecto al factor E (sumisión-autoritarismo) el grupo argentino se inclina hacia el polo de sumisión, es decir que, de acuerdo con la interpretación de este factor, los estudiantes argentinos resultarían menos agresivos, menos dogmáticos, menos competitivos y más adaptados y dependientes que el grupo de estudiante estadounidense. Esto podría explicarse ya sea como resultante de que el grupo argentino está constituido por estudiantes provenientes de una escuela técnica con un sistema disciplinario estricto, o como una diferencia cultural determinada por un distinto nivel de exigencia en el medio argentino y estadounidense. Nuestros datos actuales no permiten aclarar este problema.

Con respecto al factor Q₄ (menor-mayor tensión interna) el grupo argentino parece tener menor tensión interna que cualquiera de los dos grupos estadounidenses, lo que implica menor frustración e irritabilidad y una mejor ubicación frente a los problemas. La descripción de otros factores, como por ejemplo C, H y M, parece indicar que el grupo argentino es consistentemente menos tenso y ansioso que los grupos estadounidenses.

Tanto el grupo estadounidense de 17 años como el de 20 muestran menor grado de fortaleza del yo (factor C) que el grupo argentino. Esto está relacionado con los resultados obtenidos en los factores Q₁ y Q₃ ya que, a mayor fortaleza del yo corresponde mayor grado de integración de sí mismo (Q₁) y menor tensión interna (Q₃). De los resultados obtenidos

5 La definición de cada factor fue tomada de Handbook for the Sixteen Personality Factor Questionnaire (16 PF) de R. B. Cattell et al.

en el factor G (menor-mayor fuerza del superyó) resulta que el grupo argentino es más responsable, rígido y perseverante que los grupos estadounidenses, así como más respetuoso de las reglas morales y la disciplina. Estos datos se relacionan con los hallados en el factor E.

En cuanto al factor H (mayor-menor susceptibilidad a la amenaza) los estudiantes argentinos muestran menor susceptibilidad a la amenaza que los grupos estadounidenses, lo que implica una tendencia a un comportamiento más audaz, a comprometerse afectivamente sin recaudos y a poseer una vasta gama de intereses. Los resultados obtenidos en el factor L indican que los estudiantes argentinos utilizan el mecanismo de proyección más que los grupos de estudiantes estadounidenses. Es difícil interpretar este resultado con relación a lo obtenido en el factor H.

Los resultados obtenidos en el factor N (mayor-menor capacidad analítica) indican que los estudiantes argentinos tienen mayor capacidad para hacer "insight" de sí mismos y de los demás, un razonamiento más exacto y calculador y mayor disciplina que los estudiantes estadounidenses.

Finalmente, el factor O₃ (menor-mayor integración de sí mismo) muestra que el grupo argentino posee un mayor grado de autocontrol, responsabilidad y consideración hacia los demás que los grupos estadounidenses. Esto coincide con los resultados obtenidos en el factor C.

En síntesis, los adolescentes argentinos y estadounidenses son semejantes en cuanto a su independencia afectiva, conservadorismo, radicalismo y grado de dependencia del grupo. Por otra parte los argentinos resultaron con mayor afectotimia, fortaleza del yo, utilización del mecanismo de proyección, capacidad analítica e integración de sí mismo.

En cuanto a los puntos a dilucidar requerirán una nueva investigación. Existe evidencia (Rimoldi et al., 1973) que de haberse utilizado técnicas proyectivas de personalidad los resultados podrían haber aclarado algunos de aquellos aspectos sujetos a controversia.

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NON-ADAPTIVE ASSERTIVENESS OF ANGLO AMERICAN AND MEXICAN AMERICAN CHILDREN OF TWO AGES¹

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A novel behavioral measure, the assertiveness pull scale, was developed to test the non-adaptive assertiveness and passivity of semi-rural, low income Anglo American and Mexican American boys and girls of 5-7 and 8-9 years. Contrary to previous results, children were often too assertive to maximize their outcomes. More Anglo American, male, and younger children were non-adaptively assertive than their Mexican American, female, and older counterparts. The cultural difference is consistent with previous research indicating greater non-adaptive competitiveness among Anglo American children compared to Mexican American children.

Este estudio utilizó una nueva medida conductual --- la Escala de Empuje Asertivo, para estudiar la asertividad no-adaptativa y la pasividad de niños Anglo- y Mexico-Americanos de población semi-rural de bajos ingresos entre las edades de 5 a 7 y 8 a 9 años de edad. En oposición a los datos antes encontrados, los datos de este estudio demuestran que los niños fueron por lo general demasiado asertivos. Una proporción mas alta de los niños Anglo-Americanos, varones, y mas jóvenes utilizó la asertividad no-adaptativa que los Mexico-Americanos, hembras de mayor edad. Las diferencias culturales son consistentes con investigaciones previas que indican una mayor competencia no-adaptativa entre los niños Anglo-Americanos que en en los niños Mexico-Americanos.

Anthropological studies have described Mexicans as more passive and less assertive than Americans (Diaz-Guerrero, 1965, 1967, 1971, 1972; Fromm & Maccoby, 1970; Kluckhohn, 1954; Kluckhohn & Strodbeck, 1961; Lewis, 1959, 1969). Experimental evidence supports that hypothesis: compared to Mexican, and in some cases Mexican American children, Anglo Americans are more competitive (Kagan & Madsen, 1971, 1972a, 1972b; Madsen, 1971; Madsen & Shapira, 1970; McClintock, 1974) as well as more independent and assertive (Kagan, 1974, 1975, 1976; Kagan & Carlson, 1975). A number of these studies, however, have compared rural and poor Mexican and Mexican American children with urban middle class Anglo American children so the observed differences cannot necessarily be attributed to cultural background apart from the influences of urbanization or class. Nevertheless, three studies have documented the greater competitiveness of Anglo American children compared to Mexican American children from similar economic class and urbanization backgrounds (Avellar & Kagan, In Press; Kagan & Madsen, 1972a; Madsen & Shapira, 1970). The observed differences also appear to have some generality across measurement situations, for competition was operationalized in three distinct ways in the three studies: choosing rewards, moving a chip, and pulling a string.

In all of the competition studies which involved Anglo American children, Anglo

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American children were nonadaptively competitive. That is, they use their time or moves struggling against each other rather than working together so they fail to obtain the toys for which they strive. In contrast, rural Mexican children do not compete often enough to maximize their outcomes: they let their peer have all the available rewards rather than compete for an equal share (Kagan & Madsen, 1972 a). Clearly, at least in potential competition situations, urban Anglo American children are non-adaptively competitive in contrast to rural Mexican children who are non-adaptively avoidant of competition. Diaz-Guerrero (1965) suggests that both active and passive cultures would benefit by adopting some ways of their counterparts.

The present investigation was designed to determine if low income semi-rural Anglo American children would be more non-adaptively assertive than low income semi-rural Mexican American children in situations which do not involve interpersonal competition. The study thus bears on two important issues: 1) Does the non-adaptive assertiveness of Anglo American children generalize to situations which do not involve interpersonal competition and 2) Will Anglo Americans differ from Mexican Americans when sampled from the same school in a similar semi-rural low income area? To answer these questions a novel behavioral measure, the modified assertiveness pull scale was developed.

A recent investigation (Kagan & Carlson, 1975) examined the adaptive assertiveness of children using an assertiveness pull scale in which children received chips for pulling a handle which was connected to a spring scale by a string. Children received more chips the harder they pulled, but only up to a point. If they pulled with more than 12 pounds of force, a string broke and they received no toys. The findings indicated more adaptive assertiveness among middle income urban Anglo American children than among low income semi-rural Anglo American and Mexican American children who did not differ from each other. Rural Mexican children were least adaptive, often not pulling the string near the 12 pound limit so they obtained only a few toys. The study indicated little non-adaptive assertiveness among any groups: Almost no children persisted in losing toys by breaking the string. This finding, however, was probably a function of the way non-adaptive assertiveness was measured, for the breaking string provided a dramatic visual and auditory warning that the children had been too assertive. In contrast, there were only vague cues warning of non-adaptive passivity, the children simply did not receive as many chips as they could. For the present investigation, the assertiveness pull scale was modified so that the same cues warned of non-adaptive assertiveness and non-adaptive passivity. Thus the present methodology was not biased toward finding more non-adaptive passivity than non-adaptive assertiveness. It was predicted that Anglo American children would show more non-adaptive assertiveness than Mexican American children from the same school in a semirural low income area.

Method

Subjects:

A total of 48 children participated in the experiment. The children were Anglo American and Mexican American boys and girls of 5-7 and 8-9 years from a semi-rural poor community in southern California. The children were equally divided by Culture, Age, and Sex. The two cultural groups did not differ in family income level. Almost all of the Anglo American and Mexican American children in the school are residents of the nearby community; the school has no program to bus children to or from other areas. Most of both the Anglo American and Mexican American children of the study are from stable families which have lived for a number of years in their present homes. All of the Mexican American children tested in the present experiment are from families in their second or third generation living in the United States; many are from their second or third generation living in the same community. All of the Mexican American children sampled speak English. The school enrollment is 60% Anglo American and 38% Mexican American.

Apparatus:

The assertiveness pull scale consists of a spring scale, a pull indicator, and a handle

connected to the scale. The device is constructed so that when the handle of the apparatus is pulled, the pull indicator advances, remaining at the greatest pull to which the scale has been subjected. As used by Kagan & Carlson (1975), the handle of the apparatus is connected to the scale by a string which breaks if pulled more than 12 pounds. On each trial children receive one reward for each two pounds of pull they exert, unless they break the string, in which case they receive nothing. This arrangement, repeated over trials, allows measurement of persistence in non-adaptive assertiveness (breaking the string) and persistence in non-adaptive passivity (not testing the outcome limit and not approaching the 6 reward maximum outcome), but the cues warning of non-adaptive assertiveness (breaking string) are far more compelling than the vague cues warning of non-adaptive passivity.

In the present experiment the assertiveness pull scale was modified by connecting the handle to the scale with a non-breakable pull rod instead of a breakable string, insertion of a visual shield, and rescaling the apparatus. By these changes it was possible to create a situation in which the same cues warn of non-adaptive assertiveness and non-adaptive passivity. Further, the changes made the expression and measurement of non-adaptive assertiveness more likely. A child faced with the modified apparatus can see only the shield and the handle extending from a slot in the shield. The modified apparatus is scaled so that subjects receive one more reward for each pound of pull they exert up to six pounds, but then receive one less reward for each additional pound of pull. At 12 pounds of pull the apparatus reaches a visible and physical limit, and no rewards are received.

Procedure:

Children were taken individually from their classrooms, where they were informed they would have an opportunity to obtain some toys by playing a game. After each child was seated, the experimenter explained the experimental reward contingencies as follows:

We are going to play a game and you can win some toys (box full of pens, whistles, combs, rings, and other party favors shown). The way you win toys is by pulling on this handle. After each pull of the handle you will get some chips, from zero to six. These chips are important because they may be exchanged for toys. The number of chips you get depends on how little or far you pull the handle. After each time you pull, you will set down the handle and then will get the chips. I want you to get as many chips as you can.

Children were measured for fifteen trials. Following each trial the experimenter counted out the number of chips earned, saying "You got (number) chips that time". If the child did not earn any chips on a trial, the experimenter simply said, "You did not get any chips that time". The number of chips received were thus the only cue children had as to the adaptivity of their assertiveness or passivity.

Results

Pounds of pull per trial were analyzed by a $2 \times 2 \times 2 \times 15$ (Culture x Age x Sex x Trials) analysis of variance.

There was a significant main effect of Culture, $F(1/40) = 6.96, p < .02$, indicating that Mexican American children were less assertive than Anglo American children. As a group, Anglo American children averaged 11.73 pounds of pull per trial; Mexican American children averaged 10.46 pounds of pull. This cultural difference did not interact significantly with trials. From the first trial to the last Mexican American children were less assertive than Anglo American children. Perhaps the clearest indication of the cultural difference in non-adaptive assertiveness is the number of children who pulled the handle to the limit on every trial. Twelve of the Mexican American children were always totally assertive; 21 of the Anglo American children were completely assertive, Chi square = 6.21, $p < .02$.

A significant main effect of trials, $F(14/560)$, $p < .01$, indicated a tendency of children to modify their non-adaptive assertiveness as trials progressed. Most subjects began by pulling the handle to the limit; every child except one Anglo American and six Mexican Americans began with a pull of 12 pounds. As trials progressed, however, within both the Anglo American and Mexican American populations older children began to modify their non-adaptive assertiveness more than younger children. This Age x Trial interaction was significant, $F(14/560) = 2.24$, $p < .01$.

There was also some tendency for girls of both cultures to modify their non-adaptive assertiveness more than boys following the first few trials. This Sex x Trial interaction was significant, $F(14/560) = 2.09$, $p < .02$). Following the first trial on which boys and girls were almost identically assertive, for several trials girls became somewhat more assertive and boys became somewhat less assertive. For the remaining trials however, boys maintained a fairly constant level of assertiveness whereas girls became less assertive.

Discussion

The most remarkable finding in the present experiment is the extent to which children persisted in non-adaptive assertiveness. In spite of instructions which clearly stated that "The number of chips you get depends on how little or far you pull the handle", most children pulled the handle to the limit trial after trial, receiving nothing. Rather than modify this non-adaptive assertiveness, children appeared to want to pull the handle beyond its clear visible and physical limit. Among almost all children who always pulled the handle to the limit, with the passage of trials there evolved a predictable sequence of progressively more assertive behavior. When their initial pulls yielded no rewards, children positioned themselves in their chair and placed a hand on the table, pulling yet harder against the visible physical limit. When that failed to produce rewards, they would stand up, pulling even yet harder. A few children went so far as to place a foot on the table to exert as much pull as they could. In response to frustration, then most children became more assertive, even when that was non-adaptive. There was, however, significant variation in this pattern due to Culture, Age, and Sex.

Mexican American children were not as assertive as Anglo American children. This difference is more easily attributed to cultural background rather than level of urbanization and/or socioeconomic class, for the children of the present experiment were all of the same semi-rural poor community. The cultural difference for the most part did not reflect a superiority of Mexican American children in learning the environmental reward contingencies, for there was no significant Culture x Trials interaction. Rather, Mexican American children were less assertive than Anglo American children from the first trial to the last. The results are most easily seen as a cultural difference in preference for assertive behavior with which the children came to the experimental situation. The findings of the present experiment are consistent with previous psychological and anthropological studies which indicate Anglo American children to be more assertive than children with a Mexican cultural background.

The age differences observed in the present experiment are less consistent with previous research, at least with studies of competition. Across a variety of situations older children are more competitive than younger children, and more often persist in non-adaptive competition (Kagan & Madsen, 1971, 1972b; Madsen, 1971). Apparently in the present experiment, with the heat of interpersonal conflict absent, more older children than younger children behave as rational problem solvers: When the assertive strategy fails them, older children more often try being more passive. Although Kagan and Carlson (1975) found older children more assertive than younger children on the assertiveness pull scale, the findings of that experiment and the present experiment are not in contradiction if rational problem solving is a more powerful predictor of age differences than assertiveness *per se*.

In the Kagan and Carlson study more assertive behavior of the older children led to more rewards; in the present study the lesser assertiveness of the older children led to more rewards.

Girls in the present experiment became less assertive than boys with the passage of trials. Following the first few trials boys maintained a fairly constant level of assertiveness whereas girls became decreasingly assertive. Perhaps the value on assertiveness in boys, like the value on assertiveness among Anglo Americans, decreased the probability for those groups of finding the non-assertive solution to obtaining the toys for which they were striving.

The present experiment does not directly examine factors which might cause the observed cultural difference in assertiveness. Although early level of assertiveness is probably determined through a child's interaction with his parents and siblings, as soon as a child enters school, assertiveness falls largely under the control of peer reinforcement patterns (Patterson, Littman, & Bricker, 1967). That Anglo American and Mexican American children growing up in an integrated school maintain different levels of assertiveness, even through age nine, may indicate that cultural differences in early training for assertiveness have a more enduring effect than previously thought. Alternatively, Mexican American children may maintain their preferred level of assertiveness by creating or being forced into a subcultural peer group. Our informal observations and those of school officials indicate that with increased age Mexican American and Anglo American children increasingly group by culture on the school playground and in school activities. This grouping may be a factor which maintains the cultural differences in levels of assertiveness. Further, the cultural differences in preferred level of assertiveness may also be a factor which causes the children to group by culture. Assertiveness is probably a very central dimension on which Anglo American and Mexican cultures differ. Among six cultures Anglo American mothers most encourage individual assertiveness and peer aggression; Mexican mothers most punish assertiveness and aggression, encouraging obedience rather than individual assertiveness (Minturn & Lambert, 1964).

The present findings indicate that greater assertiveness is not necessarily associated with greater ability to learn environmental reward contingencies. Although assertiveness may lead to better performance in certain kinds of situations, the present results suggest that an assertive response set may interfere with other kinds of problem solving. In situations in which problem solving depends on sensitivity to external limits and willingness to modify individual assertiveness, Mexican American children may perform better than Anglo American children.

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«CROWDING» AND HUMAN SOCIAL BEHAVIOR

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Recent and widely publicized predictions of world population figures exceeding 4 billion people by 1980 and 6 billion people by the year 2000 have generated considerable scientific and popular concern with the potential consequences of high density living conditions. Indeed, at least two of the most widely predicted consequences of a steadily increasing world population have at this time assumed the status of painful realities. Daily news reports originating from many areas of the world provide ample reminders that we are in the midst of both food and energy supply shortages. Whether increasing population pressures on a finite supply of natural resources will lead to the disastrous biological consequences predicted by some (Ehrlich & Ehrlich, 1970; Ward & Dubos, 1972) remains to be seen.

Somewhat independent of concerns regarding the influence of increased population on resource depletion, however, is the possibility that high population density may produce deleterious effects on human social and psychological functioning. It is the latter possibility with which the present review is concerned.

Popularized writings are uniformly in agreement that high population density exerts a disruptive influence on a broad range of human functions. In one review of such literature, Zlutnick and Altman (1972) discovered widespread consensus that high density conditions produce undesirable *physical* effects such as disease and slums, *social* effects including crime, riots, and war, as well as *interpersonal and psychological* effects such as withdrawal and aggression. The authors noted that support for the latter propositions generally was in the form of weak or non-existent empirical data and concluded that, regardless of their personal convictions that high density has serious negative effects on human behavior, the available scientific data regarding the issue were inadequate.

Despite numerous shortcomings, there exists, however, a substantial body of research relevant to the effects of high population density. According to data source, the available studies may be grouped, somewhat arbitrarily, into those dealing with *animals*, *correlational studies of humans*, and *experimental studies of humans*. Much of the remainder of this review will be devoted to descriptive and evaluative discussions of each of the above groups of studies.

Animal Studies:

Studies of the effects of high density and spatial limitation on animals have served as powerful stimuli in eliciting concern with the effects of similar variations on human populations. While some degree of inconsistency exists (Lawrence, 1974), the results of many animal studies are relatively clear in indicating that extremely high levels of population density seriously disrupt biological and social functioning. Perhaps the best known laboratory study concerning density and animal behavior was conducted by Calhoun (1962) using laboratory rats. Groups of rats in confinement were allowed to breed freely and were supplied with adequate food and water resources. Following an initially rapid rate of growth the colony size stabilized for a time and then began to decline. Associated with, and apparently responsible for, the decline were a number of pathological behaviors which emerged during the extremely high density period of colony existence.

Indiscriminant aggression, catatonic withdrawal, cannibalism, sexual aberrations, and disrupted reproductive functions were among the "symptoms" associated with high density, each of which served, ultimately, to reduce the den-

sity of the colony. Calhoun's findings with rats have also been obtained using mice, cats (Leyhausen, 1965), and monkeys (Southwick, 1967). In a field setting Christian, Flyger, and Davis (1960) have documented a similar pattern of events in a herd of freely breeding sika deer supplied with abundant food and water but confined to a small island. Density was observed to increase rapidly and peak at a high level followed by a dramatic increase in mortality rate. Autopsies revealed a variety of endocrine disorders interpreted as symptoms of *stress* produced by extremely high density.

The results of such studies are frequently cited as suggestive of similar behavioral reaction patterns in humans to high density conditions. Furthermore, in terms of theory development, the concept of *stress* has been widely and confidently invoked as proposed mediator of presumed density-behavior links in humans (Desor, 1972; Dooley, 1974; Esser, 1973; Stokols, 1972 a, b; Sundstrom, 1974). Casual generalizations from animals to humans is, of course, fraught with dangers (Lawrence, 1974) and, as will be discussed later, the wisdom of transplanting the mediating concept of stress (which is, itself, of dubious value) from animals to humans is open to serious.

Correlational Studies of Humans:

Despite the many pitfalls of generalizing directly from animal to human populations, there has been a rather natural tendency to perceive parallels between the spectre of seething masses of bodies in colonies of rats and the conditions that exist in major urban centers of the world. The extent to which a variety of individual and social pathologies are related to indices of population density has been investigated in a number of studies. The strategy generally has been to correlate various density measures such as population per acre or square mile, number of dwelling units per unit of space, number of persons per room, or similar measures with various indices of pathology such as crime rates, mortality rates, mental and physical health data, divorce rates, suicide rates, and others.

While the findings of such studies have been somewhat inconsistent, a number have revealed relationships between density and pathology indices. For example, Schmitt (1957) reported significant relationships between density and juvenile and adult crime rates in Honolulu. Marsella, Escudero, and Gordon (1970) reported that, among males in Manila, high density was positively associated with psychosomatic complaints, alienation anxiety, withdrawal, eruptive violence, and free-floating anxiety. Mitchell (1971), studying Hong Kong residents, found the incidence of worrying and unhappiness to increase as density increased. As a final example, Galle, Gove, and McPherson (1972) obtained positive correlations between density and pathology measures in Chicago.

At first glance, it would appear that the worst fears of those who would generalize from the animal studies to human populations were confirmed—high population density among humans leads to such undesirables as crime, psychopathology, disease, and aggression. Few investigators, however, have been more aware of the correlation-causation truism than those associated with correlational studies of density and pathology. Indeed, when variables such as social class, ethnicity, and income level are statistically controlled, many of the density-pathology relationships are considerably weakened or eliminated (e.g. Schmitt, 1966). Even when such relationships persist (Galle et al., 1972; Schmitt, 1966), directional causality is still difficult, if not impossible, to infer.

While providing few, if any, definitive answers to questions concerning density-behavior relationships, studies of urban populations have stimulated further interest in and investigations of the issue under more controlled conditions. In addition, such studies serve to emphasize the need for careful definitions of the variable of density. For example, different density-pathology relationships have been found when density has been defined as number of persons per room than when defined as number of persons per acre (Galle et al., 1972). It will become

apparent, however, that the "meaning" of density as a variable continues to be under dispute in current research and theory concerning this issue.

Experimental Studies of Humans:

The primary lures of experimentation in any field are, of course, that it offers increased control over both "central" and "extraneous" variables and the possibility of establishing directional causality relationships between variables. Two general paradigms for the experimental study of density and human behavior have emerged, each of which has its own advantages and disadvantages.

First, a number of investigators have placed human subjects in physical settings which vary in the amount of space allotted to each subject and have assessed the performance, affective, and social-behavioral consequences of density variations. Two procedures for manipulating density have been utilized. In the first, density variations are achieved through varying group size while holding room size constant. Loo (1973) refers to this procedure as a manipulation of "social density" while she describes "spatial density" manipulations as those in which group size remains constant while room or setting size is varied. Some evidence exists suggesting that social and spatial density variations produce different behavioral effects (e.g., Loo, 1972; Marshall & Heslin, in press), however, no consistent patterns of such differences are apparent.

The findings of studies in which human densities have been experimentally manipulated have been somewhat inconsistent. For example, Hutt and Vaizey (1966) observed heightened levels of aggressive behavior in young children as density increased, while Loo (1972) reported *lower* levels of aggression in young children under high than under low density conditions. Using college students as subjects, Russell Veitch and I examined the effects of density variations on the intra- and interpersonal affective responses of same-sex groups. Following 45 minutes of confinement to high or low density settings, subjects were assessed with respect to their affective states and their attraction toward an anonymous stranger. Both male and female subjects were more affectively distressed under the high than the low density condition and both sexes responded more negatively to the target person under high than low density conditions. In a more elaborate study Dooley (1974) observed more intra- and interpersonal negativity under high than low density conditions in which males performed a series of tasks requiring physical and social contact.

In a number of studies sex differences in response to high and low density have been reported. For example, in one study (Ross, Layton, Erickson, & Schopler, 1973), in terms of self ratings and ratings of other group members, males responded more negatively under high than low density conditions while females' responses were more negative under low than high density conditions. Similar findings were obtained in another study (Stokols, Rall, Pinner, & Schopler, 1973) in which males rated themselves as more aggressive under high than low density conditions and females responded in the opposite way. Freedman and his associates (Freedman, Levy, Buchanan, & Price, 1972) had male and female subjects in homogeneous or mixed-sex high or low density groups engage in a series of interactive tasks. High density males in same-sex groups responded more negatively than did those in low density groups and, once again, females in same-sex groups reacted more positively under high than low density conditions. In mixed-sex groups no effects of density were obtained. Viewed in isolation, the latter studies are strongly suggestive of the existence of sex differences in response to high and low density. It must be noted, however, that other data reveal no such differences (Griffitt & Veitch, 1971) or sex differences in the opposite direction (Marshall & Heslin, in press).

To further emphasize the lack of consistency among experimental findings in this area, some investigators have obtained no effects of density variations in intra- or interpersonal affective behaviors (Freedman, Klevansky, & Ehrlich, 1971; Emiley, 1974).

Empirical inconsistency is, of course, not unusual in the behavioral sciences and may be traceable to many factors. Most frequently, the major culprits are inconsistent independent variable manipulations and dependent variable assessments. Experimental studies of density and behavior, when viewed as a group, serve as excellent examples of such inconsistency. Density, itself, has been manipulated in a number of ways in a variety of physical settings and the nature of the activities engaged in by subjects has varied considerably across experiments. Similarly, a variety of definitions and measures of dependent variables such as interpersonal attraction, aggression, and so forth have been utilized. With such methodological variations, empirical inconsistency is the most probable result.

Some, however, have argued that conceptual rather than methodological difficulties are primarily responsible for the empirical confusion in the density-behavior literature. In studies of density-behavior relationships the terms "crowding" or "crowded" are frequently used as descriptive of high density settings. Some frequently cited authors (Desor, 1972; Stokols, 1972 a, 1972 b) have viewed the interchangeable use of the terms density and crowding as troublesome and called for distinctions between density as a physical parameter of settings and the concept of "crowding" viewed as a subjectively unpleasant psychological experience. High density is generally regarded as a necessary but not sufficient antecedent of perceptions and experiences of crowding. According to this conception, perceptions and experiences of crowding serve as the primary mediators of behavioral, affective, and physiological reactions to high density. In addition, from this perspective, it is of primary importance to identify these conditions under which subjects will perceive or experience physical settings as crowded.

The utility of the second major paradigm for the experimental study of density and behavior is, perhaps, most evident with regard to the latter issues. In this procedure (e.g. Cozby, 1973; Desor, 1972) subjects have been exposed to scaled simulations of high and low density physical settings and either asked to provide judgments of the settings or asked to place simulated human figures (usually clothespins) in such settings according to various criteria (i.e. until "crowded", uncomfortable, etc.). Among the factors identified by this procedure as determinants of perceptions of "crowdedness" are physical features of the setting such as design characteristics (Desor, 1972) and population density (Desor, 1972; Cozby, 1973), social characteristics of ongoing activities (Desor, 1972; Cozby, 1973), and personal characteristics of subjects such as "personal space" preferences (Cozby, 1973).

With perceptions and experiences of "crowding" pinpointed as the primary mediator of density-behavior relationships, a recent flurry of activity in the literature has involved several attempts to specify the conceptual nature of crowding and of those characteristics of high density situations which produce perceptions of "being crowded". For example, the concept has been regarded as an unpleasant subjective state experienced variously as "receiving excessive stimulation from social sources" (Desor, 1972), "perceived restrictions in one's freedom of choice" (Proshansky, Ittelson, & Rivlin, 1972), "the inability to adequately control interactions with others (Zlutnick & Altman, 1972)", "a motivational state directed toward the alleviation of perceived restriction and infringement" (Stokols, 1972 a), a disturbance of CNS functioning (Esser, 1973), and as a response to social "intrusions" and "interference" (Sundstrom, 1974). Stokols (1974) has recently reviewed and classified such theoretical speculations.

Regardless of the specific events or experiences described by each of the above theorists as leading to "crowding", all seem to agree that the extent to which individuals experience crowding, rather than density itself, determines the extent to which density will influence behavior. Most seem to agree, also, that the events linking high density and "crowding" are interpersonal in nature and that the major important consequence of "crowding" is increased negativity in interpersonal behaviors (Griffitt, 1974; Sundstrom, 1974).

The usual conception is one of a *linear* sequence of events in which (1) some conditions of high population density (in conjunction with various personal and social factors) lead to (2) experiences of "crowding" which then *lead to* (3) negative affective or behavioral responses to other people. That is, the experience of "being crowded" is seen as rendering other people negative sources of affect and thus, more negative stimuli than under conditions in which one does not feel "crowded". High density conditions will increase interpersonal negativity only when such conditions are perceived as "crowded".

A major consequence of this conception relevant to research activities has been a shift in focus from concern with the effects of density variations *per se* on behavior to a primary concern with the relationships between perceptions of "crowding" and behavior. Several "theories" have been forwarded to account for the determinants and consequences of "crowding" and physical density itself virtually has been banished to the trash bin of useless concepts (e.g. Desor, 1972; Stokols, 1972 a, 1972 b). My remaining comments will be devoted to questions concerning the utility and wisdom of this shift in focus with special attention devoted to the value of the concept of "crowding", itself.

Premature Theory Building:

An initial question concerns the utility, at this stage of the game, of invoking an hypothetical concept such as "crowding" to account for density-behavior relationships which, themselves, are rather inconsistent, unreliable, and difficult to replicate across seemingly similar settings. The initial attempts at "model" or "theory" building in this area (Desor, 1972; Stokols, 1972 a, 1972 b) were offered at a time when only a few scattered and inconsistent empirical findings were available. While the potential value of "good" theories as unifiers of seemingly diverse findings is not contested, theoretical development in this particular area has far outstripped empirical development and, as will be noted later, some of the currently popular theoretical formulations are remarkably unresponsive to empirical disconfirmations. Perhaps a bit more empirical consistency would be desirable prior to attempts to achieve conceptual consistency (Griffitt, 1974).

The Stress on "Stress":

A related question concerns the wisdom of somewhat arbitrary equations or linkages of the "experience of crowding" to concepts of stress (e.g. Dooley, 1974; Stokols, 1972 b; Sundstrom, 1974). The "crowding" as "stress" idea has obtained a virtual "death grip" on theorizing in this area and its casual acceptance has had a serious limiting influence on the examination of alternative possibilities. The conceptual and empirical ambiguities inherent in the notion of stress, itself, are well known (e.g. Appley & Trumbull, 1967; Baron, Byrne, & Griffitt, 1974; McGrath, 1970) and, if we are to seek conceptual blankets at this time, it would seem wise to examine some with tighter weaves.

Perceived "Crowding" and Behavior:

To some extent, the latter two concerns are matters of personal preference and certainly open to considerable debate. More important, however, are data relevant to the assumed utility of the concept of "crowding" as a predictor of density-behavior relationships. While most conceptions of density effects (Stokols, 1972 a, 1972 b; Desor, 1972) emphasize that not all high density situations will lead to negative interpersonal responses, there appears to be general agreement that perceptions of "being crowded" in such situations will result in negativity in interpersonal feelings. If "being crowded" is regarded as the critical factor in such settings, response variables which are viewed as consequences of "crowding" should closely correspond to assessed perceptions of crowding. A close examination of available data, however, reveals that patterns of perceived "crowding" differences and dependent variable differences frequently are at variance. For example, in two frequently cited studies (Ross et al., 1973; Stokols et al., 1973) both males and females perceived high density settings as more "crowded" than

low density ones but exhibited reverse patterns of interpersonal responses in relation to high and low density. As noted earlier, males responded more negatively under high than low density conditions while females' responses were the reverse of those males. In addition, while strong "perceived crowding" main effects were found for density variations, virtually no main effects of density on interpersonal responses were obtained. Clearly, patterns of perceived "crowding" and patterns of the hypothesized consequences of "crowding" did not coincide, rendering questionable the proposed crucial role of perceived "crowding" as a mediator of density-behavior relationships. Other inconsistencies between patterns of perceived "crowding" reactions and patterns of responses presumably mediated by perceived "crowding" subsequently have been reported (e.g. Cozby, 1973; Dooley, 1974).

It should be noted that the potential role of individual differences (sex, "personality", etc.) has not been ignored in theorizing concerning the impact of density variations on behavior. It is, however, significant that "personal factors" are most frequently proposed as mediators of the impact of density on perceived "crowding" rather than mediators of the effects of "crowding" on behavior (Stokols, 1972 a, 1972 b; Cozby, 1973). Thus, consistent relationships between perceived "crowding" and the presumed consequences of such perceptions would be anticipated. It is tentatively concluded, then, that relevant to the sequential process described earlier, the link between perceived "crowding" and behavior has not received clear support.

Alternative Directions:

If one seriously entertains the proposition that the concept of "crowding" is presently of limited utility in accounting for density-behavior relationships, those factors which have been proposed as determinants of the perception of "being crowded" may be viewed from a different perspective. As noted by Sundstrom (1974) the conditions most frequently suggested as determinants of the perception of "crowding" in high density situations generally involve interpersonal events which, themselves, elicit negative affective responses as well as negative interpersonal behaviors.

For example, close physical proximity to others frequently involves violations of "personal space" preferences (Sommer, 1969) leading to avoidance and negative responses to violators. The inevitable "personal space" invasions under high density conditions may produce direct effects on the positivity of interpersonal behaviors in such situations. In a similar vein, several other interpersonal events such as inappropriate eye contact, touching, body and facial orientations, and initiations of verbal interactions each of which in low density situations produce negative affective and interpersonal responses (e.g. Exline, 1972; Tuan, Heslin, & Nguyen, 1974; Heslin, 1974; Mehrabian, 1972) may be expected to increase in frequency under some high density conditions and render interpersonal behaviors more negative than under low density conditions.

In short, many of the events suggested as leading to differential perception of "crowding" are, even under low density conditions, capable of producing negative interpersonal behaviors. High density settings, in terms of probability alone, may be expected to produce increased frequencies of such negative interpersonal events or social intrusions (Sundstrom, 1974) and, thus, increased negativity in interpersonal behaviors. As noted by Sundstrom (1974), however, data concerning the effects of density variations on the intensity and/or frequency of such events are in short supply.

It is suggested here, then, that future developments and progress in this area will benefit most from a loosening of the death grip of crowding-stress formulations concerning the mediation of density-behavior relationships. The current "state of the art" allows few, if any, definitive conclusions regarding the impact of high population density on human social behavior. It is, however, proposed that detailed

examinations of the frequency and/or intensity of empirically demonstrated negative and positive interpersonal events as they are affected by density variations will prove more fruitful than premature attempts to develop comprehensive theories concerning the concept of "crowding" which, itself, is of questionable utility.

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TOWARD A VERIDICAL PSYCHOLOGY: THE SOCIAL PSYCHOLOGY OF THE PSYCHOLOGICAL EXPERIMENT¹

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When a research psychologist inquires about a position, he is almost always concerned about the availability of subjects and laboratory space to bring them to. Psychologists bring people to experiments rather than experiments to people. Our manipulanda and measurements are not focussed on natural life routines; instead, they represent to subjects an exotic interruption of these.

The advantages of conducting research in the experimenter's habitat are apparent. It affords a level of precision not readily attainable in the complex habitats of our human subjects. The question is, however: What is it that we are being so precise about? An animal in his own habitat and the same animal in an environment contrived by people behaves quite differently, and ethologists are well aware of the pitfalls of trying to generalize from one situation to the other. There is no reason to expect humans, also, not to show atypical behavior in an atypical environment.

Simply stated, people feel and think, and people who are subjects in psychological experiments are undoubtedly feeling and thinking *about* the experiment. They may be feeling fear about looking foolish or incapable of revealing something about themselves which may best be kept secret. They may be feeling anger about being in such a situation, or awe, or wariness, or curiosity, or skepticism, or utter delight at being the focus of an important person's attention. They may be wondering what the meaning of their responses are to the experimenter; what sort of responses he expects; whether he is divulging the true purposes of his experiment to them. Whatever the nature of these thoughts and emotions, they are most likely quite consuming, for to be a subject in a psychological experiment must be more than a run-of-the-mill event. The seers and prophets of earlier days have given way to Behavioral Scientists, and one does not stand easy before their scrutiny.

To the extent that the subject's feelings and thoughts about the experiment are prepotent, they will inevitably interact with the experimental manipulations and contribute to his responses. The venerable concept of *role* provides an apt framework for understanding this process. In its broadest definition, a role refers to *the cognitive and affective states of an individual related to his perception of his position in a given interpersonal situation*. People occupy many roles in the course of a day, which often call for divergent and contradictory behaviors, and to understand any bit of interpersonal behavior, we must be aware of the person's role at the time. Consider a loan officer of a bank talking to someone without collateral. The attributes of selflessness, altruism, generosity and sympathy would hardly apply to him, but if we watch the same person interacting with his family we would probably emerge with quite a different concept.

The psychological experiment is, after all, an interpersonal interaction between subject and experimenter and the subject's behavior can no more be isolated from his role than the banker's from the position he occupies. Further, the generalizeability of our observations from laboratory to life will suffer the same shortcomings as an attempt to construct generalities about the banker's altruism or generosity at home from his behavior at his job.

Concepts about role-related subject behavior are not new, but they have become newly respected. In a *Psychological Review* paper of 1933, Saul Rosenzweig outlined virtually all of the same concerns about sources of artifact in behavioral research that have obsessed psychologists for the past ten or so years. The profile of the subject that has emerged from this last decade of research is also not very different from Rosenzweig's speculations of forty years ago.

We have much evidence (c.f. Rosenberg, 1969) in support of a fairly obvious conjecture: that subjects are primarily motivated to present themselves as favorably as possible from the standpoint of psychological adjustment and effectance. Rosenzweig called this "pride" in 1933: We have come to know it as "evaluation apprehension". We are aware, also, that subjects often behave in accordance with their percepts of the experimental hypothesis. Rosenzweig termed this "compliance"; Martin Orne, 1962, spoke of it as responding to "demand characteristics". Silverman (1965) pointed out that compliance with demand characteristics is often based on evaluation apprehension; When the subject is unsure of what response will reflect favorably on him, he will tend to do what he perceives the experimenter expects him to on the assumption that typical behavior is normal behavior. Sigall, Aronson and Van Hoose (1970) showed that when the experimental situation is arranged so that favorable evaluation and compliance with demand characteristics entail competing responses, subjects do what is required for the former. Robert Rosenthal and a number of others (c.f. Rosenthal, 1969) have provided massive data that subjects' notions about the experimental hypothesis often come from subtle and covert cues by the experimenter, which we now refer to as experimenter-expectancy-effects, and which Rosenzweig labelled "suggestion error". Finally, as Silverman (1965), Masling (1966), Argyris (1968) and Jourard (1968) have suggested, and as several studies (e.g. Silverman & Kleinman, 1967; Cook, Bean, Calder, Frey Krovetz & Reisman, 1970; Silverman, Shulman & Wiesenthal, 1970) have demonstrated, under some circumstances, such as when subjects are frustrated or angry, they will deliberately respond opposite to their percepts of the experimenter's expectations.

Along with our discoveries about the nature of the subject role, there have been numerous reports illustrating how role-related motives confound experimental observations in diverse areas of psychological research, and it is on these that I want to focus in this review.

Some areas of research are easier to pick on than others; that is, the contrivances are fairly obvious. Attitude change studies, for example, are ideally suited for the investigation of role-related behavior. The subject in an attitude change study is usually presented with persuasive communications followed by opinion questions based on their contents and, despite the occasional vague attempts to distract him from the apparent purpose of these procedures or statements reminding him that there are no "right" or "wrong" opinions, it does not require a superior intellect for him to realize that persuasibility is being scrutinized. The questions, then, that must obsess him and guide his responses to significant extent, are: "Does this experimenter expect me to be persuaded?" and, "Why?".

The answers to these questions may vary across subjects and situations, inasmuch as persuasion has both negative and positive connotations. It may become associated in subjects' minds with non-critical conformity and overdependence, or with open-mindedness and flexibility. Any number of things about himself, the experimenter or the experimental context may affect the way the subject resolves his dilemma about the implications of showing persuasion or non-persuasion, which probably accounts for the notable non-replicability of attitude-change phenomena (e.g. Festinger, 1964; Lana, 1964; McGuire, 1966).

Let us sample some of the findings about artifact in attitude studies:

Silverman (1968) found that undergraduates were significantly more persuaded by the same arguments when they were led to believe that they were subjects in a "psychological experiment" than when they thought they were taking part in a student-sponsored survey. Moreover, the effect of being in an experiment was greater for females' attitude-change scores than males', which may challenge the validity of what one text writer (Middlebrook, 1974, p. 190) has called, "one of the most consistent findings in laboratory studies", that women are more persuasible than men. In fact, in the non-experiment condition, females were significantly *less* persuaded than males. Sherman (1967) discovered that the positive relationship between "other-directedness" and responsiveness to authoritarian-type persuasive appeals, reported in several studies (Katz, Sarnoff & McClintock, 1956; McClintock, 1958), was *reversed* when the original introductory remarks were altered to

make resistance to persuasion seem more desirable. Page (1969) found that the so-called classical conditioning of attitudes demonstrated by Staats and Staats (1957, 1958) occurred only with subjects who were aware of the purpose of the study. Silverman and Regula (1968) found that distraction during a persuasive message increased its effect, as reported by Festinger and Maccoby (1964), only for subjects who were cognizant that they were being intentionally distracted and thought its purpose was to test their powers of concentration. Silverman and Kleinman (1967) substantiated the frequently cited findings (e.g. Cowen, Landes & Schaet, 1959; Stricker, 1963) that experimentally induced frustration increases the expression of virtually any socially deviant attitude, obviously as an attempt of subjects to frustrate the experimenter in return.

Verbal conditioning studies are also easy to pick on. Levin, in 1961, showed that with a thorough and probing post-experimental questionnaire, about one third of subjects revealed awareness of the reinforcement procedures, in contrast to the five percent reported in prior studies with skeletal tests of suspicion. A number of subsequent studies (e.g. Bryan & Lichtenstein, 1966; Oakes, 1967) have shown conditioning with subjects classified unaware by the extended interview, which might testify to the veracity of verbal conditioning, *unless* it was demonstrated that a large enough proportion of these to produce the effects simply continued to conceal awareness. It may seem almost paranoid to accuse subjects of such blatant deception, but two further experiments left little doubt of it. In these, by Levy (1967) and White and Shumsky (1972) there was no question that subjects were aware of the reinforcements; each was thoroughly informed beforehand, in the waiting room, by a confederate of the experimenter posing as another subject who had just completed the task. Yet across studies, 27% maintained complete naivete throughout the extended interview.

We could continue to describe artifact based on role-related behavior in other 'easy to pick on' areas, such as conformity (Stricker, Messick & Jackson, 1967; Schulman, 1967) aggression (Page & Scheidt, 1971), dissonance theory (Rosenberg, 1965), sensory deprivation (Orne & Scheibe, 1964), and personality testing (Peabody, 1961; Lefcourt, 1966), but it may be more informative to consider some illustrations from the 'harder to pick on' category.

Researchers in the more conventional areas of psychology, such as learning, attention, problem solving, perception, psychophysiology, etc. may feel aloof to the problems of extraneous subject motivation. Experiments on these topics are more likely to be presented in a straightforward, seemingly uncontrived manner, and the measurements appear less susceptible to the kind of bias we have discussed. But this may be illusion. People do not abandon the motives that accrue to their roles simply because the options for behaving in accordance with these motives become less clear or more difficult.

For example, there have been fairly consistent laboratory findings that males are superior to females in quantitative problem-solving, but women are better at verbal learning (Maier, 1933; Carey, 1958; Hetherington & Ross, 1963; Littig & Waddell, 1967). When the experimenter is female, however, instead of the traditional male, quantitative problem-solving abilities of female subjects increase (Hoffman & Maier, 1966) and verbal learning decreases (Archer, Cejka & Thompson, 1961; Littig & Waddell, 1967). Women also become more astute at problem solving when a male experimenter indicates that he is doing the study to show that the sexes possess these abilities equally (Hoffman & Maier, 1966) or when subjects are tested in same-sexed rather than mixed-sex groups (Hoffman & Maier, 1961). Obviously the psychological experiment, with its connotation of psychological evaluation, elicits sex-role stereotypic behavior in learning tasks as well as attitude-change.

From the field of perception, consider the figural after-effect (FAE); that is, the tendency for after-images to be displaced away from original fixation points. For example, if subjects inspect two parallel lines, after-images of these lines will be farther apart. Demonstrated in dozens of studies since 1933, basic to the well-known neural satiation theory of Kohler and Wallach (1944), and called in Woodworth and Schlossberg's text (1960, p. 424) "one of the most dependable experiments for an elementary laboratory class", the FAE ranks among the most reliable principles we have anywhere in psychology. Dodwell and Gen-

dreau (1969), however, found that they were unable to obtain FAEs with any consistency across subjects, and that the only subjects who reported them were those who expected to. Their examination of the literature showed that the few studies that failed to replicate the FAE all used naive subjects. Thus, they designed an experiment, using two parallel lines as the inspection figure, in which subjects were subtly given expectations that their after-images would be farther apart or closer together, or they were given no expectations. The group with no expectations showed no effects and the others overwhelmingly reported whatever they were led to believe they would see.

Probably few psychologists are unfamiliar with the work of Hess and his colleagues on the effects of arousal on pupillary dilation. The classic findings of this group were that undergraduate males tended to dilate more to sexually provocative pictures of women than of men (Hess & Polt, 1960). Chapman, Chapman and Breje (1969), however, repeated this study with two experimenters, deliberately selected because of their different images and styles of interaction. One was formal, business like and aloof; the other was casual, friendly, and projected the image of a bon vivant. Only subjects of the latter showed differential dilation. Now it may be that subjects of the non-frivolous experimenter imposed constraints on their arousal, or that subjects simulate attentive responses where this would seem to earn the experimenter's approval. Whatever the reasons, it appears that the context of the psychological experiment is unsuitable to answer as basic a question as whether college males enjoy photographs of beautiful nude women more than photos of men.

Finally, we turn to the most widely used human psychophysiological measure, the galvanic skin response (GSR), which would seem to be almost inviolate to the subject's intentions. Nevertheless, Ellison, Davis, Saltzman and Burke (1952) reported a finding that should have received more attention than it did, using the GSR to study the effects of contrived feedback on lie detection. Subjects who were told after one trial that their lie had been discovered by their GSRs became harder to detect on a subsequent trial, while subjects who were told they had deceived the polygraph became easier to detect. There are several possible interpretations. The one I favor is that subjects have mixed feelings about not being detected. On the one hand it shows emotional control, but on the other, there is a popular notion that only pathological liars can deceive a polygraph. Thus, however subjects were told they performed they were motivated to show that they could do otherwise as well. On this basis, we must question the validity of many studies using the GSR. Subjects seem to be in more control of these responses than we surmise, and role-related motives may have just as much of an influence on this measure as an attitude change inventory.

This brief review by no means exhausts the literature on role-related motives, nor the areas of psychological research where they have been found to be a confounding influence. Moreover, the majority of these studies have been reported in the last decade or so, by investigators working for the most part on their own topics of specialization, and the yield of this limited effort suggests that there is probably not one existant area of psychology that is not vulnerable to these sources of error.

Of course with each discovery about sources of artifact, has come prescriptions for control. Rosenberg has stated: "Any experimental demonstration of some source of systematic bias and of the processes by which it operates immediately suggests procedures for the control and elimination of that source of bias (1969, p. 347)".

But here we must pause and critically ponder, for we are not dealing simply with "evaluation apprehension" or "demand characteristics" or "experimenter-expectancy effects", but a total, consuming set of attitudes and intentions on the part of subjects. These attitudes and intentions may be called *extraneous* factors by researchers, which fits them neatly into our concepts of experimental control, but from the subject's point of view, it would seem that any small thing he does which is not related to his role as such is indeed extraneous. Thus, if we reduce his apprehension about being evaluated, he might manifest his motive for approval by complying with demand characteristics. We can, perhaps, uncover and disguise all accurate cues regarding the experimental hypothesis, but this will not discourage the subject from trying to solve the riddle of what the experimenter expects him to do, nor from acting on his solution. Experimenter-expectancy effects can be eliminated

by automating the experiment and excluding the experimenter entirely, but we are still left with the subjects' percepts about the design, and the contention by Jourard (1968) that anonymity of the experimenter discourages any sort of self-disclosure or naturalistic responding.

We must acknowledge that there is a basic flaw in the concept of subject as "object" that has pervaded psychological research since the postintrospectionist era. The only way to prevent a psychological subject from behaving like a psychological subject is not to let him know that he is one. On this basis, the most encouraging development from these painful years of self-analysis is the moderate emergence of naturalistic methodologies. Although these methods have their problems too, they may herald an era when psychologists stop asking, "Where is my laboratory?" and "Where do I find my subjects?" and begin asking, "Where is the behavior that I want to study going on?" and "How do I test my theories about it while keeping it reasonably intact?" We may have fewer studies, but we will probably have sturdier laws of behavior.

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Several times in this paper I have referred to the writings of Sydney Jourard. He was a friend and colleague and his works were an inspiration to myself and many others. This paper, with the hope that it is worthy, is dedicated to his memory and his great contributions to Psychology.

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INTERPERSONAL ATTRACTION: DO WE KNOW ANYTHING AND ARE WE GOING ANYWHERE?'

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From the time that philosophers and artists first began to record their speculative explanations of mankind's physical and social surroundings, one of the crucial areas of concern has been that of interpersonal relationships. The spectrum ranges from friendship and love on one extreme to enmity and hate on the other. Early efforts to understand and to predict these contrasting interpersonal responses have included the essays of Aristotle (translated 1932) in the fourth century before Christ in which a relationship was proposed between similarity and attraction, the writings of Spinoza (translated 1951) in seventeenth century Holland in which a need to persuade others to agree with our views was hypothesized, and the effectance-like suggestion by Samuel Johnson (Boswell, republished 1963) in eighteenth century England that disagreement threatens one's sense of confidence.

Throughout these past two dozen centuries, many additional philosophers, historians, poets, and novelists added their own observations and hypotheses about the nature of positive and negative reactions of human beings to one another.

Early Research on Attraction

It was not until the late nineteenth century that empirical research on attraction began with Sir Francis Galton's (republished 1952) investigations of the variables determining who was likely to marry whom. Among his early findings was the establishment of the now familiar observation of husband-wife similarity with respect to a variety of attributes. Galton concluded that there was a strong tendency for "like to marry like", and later extensions of his work confirmed the generality of that phenomenon (Pearson & Lee, 1903).

The next half century was the scene of an ever increasing number of field studies in which investigators sought the antecedents of friendship, romance, and marriage. Over and over again it was found that positive interpersonal relationships were associated with similarity on interests, values, beliefs, and attitudes. Examples of such research range from an early study in Holland (Schuster & Elderton, 1906) through numerous American studies in the 1930's (e.g., Schiller, 1932) and into the 1940's (e.g., Richardson, 1940). Most of these investigators duly noted the weaknesses of correlational data obtained in field settings and warned against any unwarranted conclusions regarding antecedents and consequents.

It was not until the 1950's that research on attraction was designed in such a way as to permit the specification of causal relationships. Both in a study of the sequential development of friendships *in situ* (Newcomb, 1956) and in laboratory experiments involving reactions to opinion deviates in a group (e.g., Schachter, 1951), it became clear that Aristotle, Spinoza, Johnson and others were correct in identifying the role of attitudinal variables as determinants of attraction, rejection, communication, persuasion, and other interpersonal responses.

It should be noted that no one at that time or since has claimed that attitudinal variables are the sole determinants of attraction or even the most powerful determinants. Attitude similarity is, however, a pervasive, consistent, and easily manipulated variable, and much of the subsequent attraction research utilized it for these pragmatic reasons. Despite the empirical success of the investigation of attraction by way of attitude similarity experiments, there was also an unfortunate and unforeseen consequence. Many critics of this research (e.g., Levinger, 1972; Murstein, 1971a; Wright, 1971) apparently were unable to distinguish the specifics of the similarity effect from more general theoretical and empirical

concerns (Clore & Byrne, 1974). Much unnecessary verbal smog has been raised by such misunderstandings and by the sometimes hostile ripostes to them; as the atmosphere clears, perhaps everyone will find themselves able to see the forest as well as the trees.

Current Research on Attraction

During the last decade and a half, there has been a rapidly accelerating avalanche of experimentation in this area. Much of the research has focused on delineating the variables which influence attraction, establishing the interrelationships among the independent variables, and identifying the limits within which they operate.

Examples of the determinants of attraction include characteristics of the target person such as physical attractiveness (Berscheid, Dion, Walster, & Walster, 1971; Berscheid & Walster, 1974) and race (Byrne & Ervin, 1969), characteristics of the environmental setting such as propinquity (Byrne, 1961) and room temperature and crowdedness (Griffitt, 1970; Griffitt & Veitch, 1971), and characteristics of the interaction such as similarity of attitudes (Brewer & Brewer, 1968) and positiveness of evaluative exchanges (Byrne & Griffitt, 1966).

A second major empirical focus of research in this area has been the exploration of the consequences of differential attraction. Thus, many decision-making situations are found to be influenced by how much the decision maker likes or dislikes the person about whom the decision is made. Included here are judgments about a defendant in a trial (Efran, 1974; Griffitt & Jackson, 1973; Mitchell & Byrne, 1973), an applicant for a job (Griffitt & Jackson, 1970), a borrower requesting a monetary loan (Golightly, Huffman, & Byrne, 1972; Sung, in press), foster parents wishing to adopt a baby (Aves, 192), and candidates running for public office (Efran & Patterson, 1974). The effects of differential attraction on overt physical behaviors has been determined for several responses, including visual contact (Russo, 1975), physical proximity (Allgeier & Byrne, 1973), and compliance with a request (Baron, 1971). Of greatest theoretical importance has been research on the affective consequences of attraction including self-reported feelings (Byrne & Clore, 1970) and evidence of physiological concomitants (Clore & Gormly, 1974).

The burgeoning nature of this research and the success with which attraction has been pursued can best be documented by noting the number of recent monographs (e.g., Byrne, 1969; Byrne & Griffitt, 1973; Clore, 1975) and books (e.g., Berscheid & Walster, 1969; Byrne, 1971; Huston, 1974; Murstein, 1971b) which attempt to summarize the ever-expanding body of data. The solid role of attraction research is also evidenced by its regular inclusion as a major topic in undergraduate texts in introductory, social, and personality psychology.

Theoretical Developments

There are three primary theoretical formulations which have been developed to encompass the attraction data. Depending on whether one takes a confrontative or integrative stance (Byrne Clore, Griffitt, Lamberth, & Mitchell, 1973; Byrne & Lamberth, 1971), these theories may be seen as locked in fierce combat or as providing complementary and interlocking approaches to an important aspect of human behavior.

Cognitive theorists tend to emphasize man's intellectual activities and to conceptualize behavior as a function of information processing or the generation of internal rationales and justifications. The balance theories of Heider (1958) and Newcomb (1968) focus on the internal stresses and strains of the positive and negative reactions among an individual, another person, and any object about which they are communicating. In a much less systematic way, a number of individuals who are first or second generation products of Festinger's (1975) bailiwick have dealt with bits and pieces of attraction phenomena in the rational-cognitive tradition (e.g., Aronson & Linder, 1965; Mattee & Aronson, 1974; Sigall & Ostrove, 1975). Anderson (1971) has dealt extensively and in mathematically precise fashion with the way in which positive and negative interpersonal information is processed.

Interactional theorists tend to emphasize the dyadic relationship in terms of descriptive accounts of the operation of different variables at different levels of intimacy and of their

modification by feedback mechanisms. Interactionists tend to conceptualize the attraction process as falling along a developing continuum ranging from superficial contacts between strangers to a deeply committed love relationship (Levinger & Snoek, 1972). Much of the stress in this approach is on the way in which the determinants of attraction change as the relationship changes (e.g., Murstein, 1971c; Wright, 1969).

Reinforcement theorists tend to emphasize man's affective responses and the acquisition of evaluative dispositions through associative conditioning. The historical roots of this approach are in learning theory, but the utilization of such concepts in attraction range from a strict application of Hull-Spence behavior theory (e.g., Lombardo, Weiss, & Stich, 1973; Lott & Lott, 1974) to a non-doctrinaire application of learning concepts based on analogizing within a classical conditioning paradigm (Byrne, 1971; Byrne & Clore, 1970; Clore & Byrne, 1974). The reinforcement approach seems to have generated the greatest volume of research ranging from brass instrument learning experiments in the laboratory (Lamberth, 1971; Davis & Lamberth, 1974) to the application of learning principles in an interactive group situation with children (Lott & Lott, 1960).

New Directions for Attraction Research

It is never particularly safe to prognosticate about research because future directions are dependent on the inclination, imagination, and luck of countless individuals in countless research settings around the world. What seems to be an exciting prospect today may be revealed next year as a hopeless cul-de-sac. A student's insight tomorrow may lead us in a totally new direction that is impossible to divine from any existing knowledge. Recognizing these problems, I would nevertheless like to suggest three primary directions toward which current attraction research seems to be moving.

First, it has long been clear that the simple identification of the antecedents of attraction in factorial experiments is only a beginning step. Social psychologists have become serious about emulating those in other sciences by attempting to specify functional relationships in mathematical terms (Schonemann, Byrne, & Bell, 1975). Increasingly sophisticated formulations offer the possibility of ever more precise predictions of attraction and other interpersonal responses. In the future we shall be less and less content simply to proclaim that X has been found to have a positive or a negative effect on Y; rather, we will want to be able to specify the details of that function and to modify it whenever such modification yields a decrease in error variance and an increase in predictive power. The viability of the growing interest in relational formulae is shown by the fact that gentlemanly interchanges are periodically generated about the precise details of these algebraic creations (e.g., Insko & Wetzel, 1974; Kaplan & Anderson, 1973; Singh, 1973, 1974; Singh, Byrne, Gupta, & Clouser, 1974).

Second, the question of the applicability of research findings is one that has historically plagued scientists and, perhaps most disturbingly, behavioral scientists. We are currently assailed from the political left by those who demand instant social relevance, from the political right by those who demand instant financial or military returns on their investment, and from all directions by the general public who feel that we are basically working to explain things they already know in words they cannot understand. The meaningful application of psychological knowledge is an important goal but one not likely to be achieved by devoting our total research energies to practical problems or, on the other hand, by ignoring such problems in the Micaiberish hope that something useful may eventually turn up. Instead, it is vitally important that a portion of our efforts involve the attempt to take that which we know and apply it to whatever "real world" concerns we are able. In other words, the best strategy is to start with available solutions for which we must identify the appropriate problems. At the moment, it appears that what we know on the basis of attraction research can lead to behavioral engineering in surprisingly diverse areas. For example, data and theory based on the study of attraction have served as starting points for work on relatively obvious practical applications dealing with problems in the dating and courtship process (Byrne, Ervin, & Lamberth, 1970; Walster, Aronson, Abrahams, &

Rottman, 1966), the determinants of marital compatibility (Byrne, Cherry, Lamberth, & Mitchell, 1973; Levinger & Breedlove, 1966), and the extent to which interpersonal tolerance can be increased (Brink, 1974; Byrne, Allgeier, Winslow, & Buckman, in press; Clore & Jeffery, 1972; Hodges & Byrne, 1972). Less obvious have been the development of procedures which point toward removing biases from the judicial process (Griffitt & Garcia, 1975; Lamberth & Kirby, 1974), instituting improvements in job performance (Nelson & Meadow, 1971), facilitating the learning process (Byrne, Krivonos, & Friedrich, 1975; Lott, 1969), and helping to maximize positive interpersonal responses elicited by our environmental surroundings (Fisher, 1974; Fisher & Byrne, in press).

Third, a truly useful theoretical system does more than simply tie together the findings within a delineated problem area. At its best, a theory should permit the generation of hypotheses which have implications in quite different settings and, ultimately, it should provide a unified explanation of seemingly unrelated phenomena. Potentially, the reinforcement-affect model of attraction provides just such a theory. To date, this theoretical superstructure has proven extremely useful in providing links between attraction and such previously isolated areas of social-personality investigation as choice behavior (Bleda & Castore, 1973), aggression (Bell, 1975; Kelley & Byrne, 1974), altruism (Kelley & Byrne, 1975; Pandey & Griffitt, 1974; Weiss, Buchanan, & Lombardo, 1971), dishonesty (Bleda, White, & Byrne, 1975), reactions to aid (Fisher & Nadler, in press; Nadler, Fisher, & Lamberth, & Mitchell, 1974; Griffitt, May, & Veitch, 1974), and birth control (Allgeier, in press; Byrne, Jazwinski, DeNinno, & Fisher, 1975). The ultimate goal is a coherent theory of human behavior.

Concluding Thoughts

The specific outlines of attraction research may turn out to be totally inadequate as a roadmap for reaching the goal of a general behavior theory. It is certain, however, that such a goal will never be attained if we simply stand around the scientific start box and engage in orgies of self-doubt.

Possibly in part because of the current malaise that pervades the political and economic spheres of endeavor, it has become fashionable to bemoan the current state of social-personality psychology as a science and/or to cry out for new assumptions, new methods, and new approaches (e.g., Gergen, 1973; McGuire, 1973). I would like to register emphatic disagreement with the jeremiads of those who have given up on behavioral science as we know it. Day by day, any research problem is likely to seem limited, to be beset by occasional failures, and to appear unworthy of one's time and trouble. From a longer perspective, however, a very different view emerges. Our knowledge of attraction, as one example, has expanded enormously between the time Aristotle wrote *The Rhetoric* in ancient Greece and our discussions of related issues in Bogota, Colombia in December, 1974. If other sciences provide valid examples, future progress in our field is destined to advance at an ever more rapid rate. It would seem justifiable to reaffirm our convictions about the value of what we have been doing and our optimism about where we are bound.

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PERSONALITY DEVELOPMENT IN TWO CULTURES, A CROSS-CULTURAL LONGITUDINAL STUDY OF SCHOOL CHILDREN IN MEXICO AND THE UNITED STATES

Wayne H. Holtzman, Rogelio Díaz-Guerrero y Jon D. Swartz
Austin; University of Texas Press, 1975, 427 pags.

Este libro es el resultado de varios años de investigaciones dedicados a estudiar aspectos cognoscitivos, perceptivos y de personalidad de niños pertenecientes a dos culturas diferentes, representadas por muestras de la ciudad de Austin, EE. UU. y de la ciudad de México.

La obra está dividida en cuatro secciones y un apéndice. La sección primera informa al lector acerca de los antecedentes del estudio y sobre el diseño de investigación seleccionado. Además, describe los instrumentos de evaluación y los tipos de datos que se obtuvieron en ambas ciudades. La sección segunda, describe los estudios metodológicos y psicométricos que se hicieron con el propósito de seleccionar los tests, cuestionarios y entrevistas que, en cada cultura, eran importantes para una comparación transcultural. También se incluyen estudios acerca de la confiabilidad de las pruebas y varios análisis factoriales. La tercera sección comprende cinco capítulos con la mayor parte de los resultados obtenidos, distribuidos de la siguiente manera: el Capítulo 7 presenta los resultados en el área perceptiva-cognoscitiva, el Capítulo 8, trata del test de Holtzman, el Capítulo 9 se refiere a mediciones de la personalidad y de actitudes, el Capítulo 10 analiza variables relacionadas con los padres y el hogar y el Capítulo 11 presenta las actitudes y el sistema de valores de la madre. Finalmente la cuarta sección comprende tres capítulos: en dos primeros los resultados más importantes se integran y discuten, en el último se da un resumen de toda la investigación.

En el apéndice figuran aquellos instrumentos de evaluación que fueron diseñados específicamente para el estudio o que se construyeron con base en la modificación e integración de instrumentos de diversos autores. Estos son: a) la entrevista realizada con los padres, con la cual se obtuvo información acerca de las características familiares, estilo de vida, ambiente, etc. de cada niño; b) el cuestionario acerca de las actitudes de los padres, formado por una integración de ítems extraídos de diversas escalas; c) la codificación de los datos obtenidos en la entrevista con los padres; y d) el test de Asociación de Palabras, de Moran, en inglés y castellano.

Además de los cuestionarios y escalas que figuran en el apéndice, se administraron a los niños un conjunto de tests organizados en la siguiente forma: una batería básica, formada por la técnica de manchas de tinta de Holtzman (HIT), el test de la Figura Humana y los subtests Vocabulario y Cubos del WISC o del WAIS, se administró individualmente a cada niño y se volvió a repetir cada año. De las pruebas restantes, algunas se aplicaron solamente a los niños que estaban en 2º grado o en grados superiores; otras, se administraron durante dos o más años sucesivos, pero no en forma uniforme. Nótese que muchos de estos tests están formados por varias escalas y, por lo tanto, en número de puntajes que se obtuvieron para cada niño es realmente muy elevado y representa un material difícil de organizar y unificar. Si bien los autores superaron esta dificultad y lograron delinear rasgos que emergen con claridad referidos principalmente a diferencias culturales, de clase social y de sexo, el lector que no conozca bien todos los tests puede sentirse perdido ante tal abundancia de datos y análisis estadísticos o encontrar que la lectura de este libro se hace, por momentos, repetitiva y monótona. Sin embargo, el psicólogo versado en algunos de los instrumentos administrados y deseoso de ampliar su información dará la bienvenida a esta riqueza de material, pues encontrará referencias claras, datos numéricos precisos y deducciones detalladas y enjundiosas.

Los autores utilizaron un diseño longitudinal "sobrepuesto" que les permitió observar doce años de desarrollo del niño a lo largo de seis años de pruebas repetidas. En ambas

culturas, se formaron tres grupos de niños cuyas edades eran, al comenzar la investigación de 6,9 y 12 años. Los tests se volvieron a aplicar cada año, durante un período de seis, de modo que al finalizar la administración de las pruebas los tres grupos tenían 11, 14 y 17 años de edad.

Se debe destacar los esfuerzos que se hicieron por lograr el Máximo de confiabilidad al evaluar los datos, estudiando la concordancia entre los examinadores y por lograr pruebas y cuestionarios equivalentes, superando las diferencias de idiomas.

Los resultados revelaron claras diferencias entre ambas culturas en aspectos cognoscitivos, perceptivos, de personalidad y actitudes. También se observó una frecuente interacción entre cultura, edad, sexo o clase social.

Los análisis factoriales de HIT, realizados separadamente en cada cultura, arrojaron cinco factores bien definidos. Hay una clara interacción entre cultura, clase social y edad: La clase baja de niños pequeños expresan más fantasía y son más impulsivos que los pequeños mexicanos. Estas diferencias tienden a desaparecer en los niños mayores. Es también particularmente interesante la saturación de la escala de Movimiento (M) en un factor que incluye los subtests verbales del WISC. Los autores lo interpretan sugiriendo que M contiene un elemento de expresión verbal de la fantasía.

En resumen, este libro es un estudio experimental transcultural sólido, consistente y bien desarrollado. Su lectura será de gran interés para psicólogos, sociólogos y antropólogos. Además puede ser considerado como un libro de referencia para todo aquel interesado en la aplicación de pruebas psicológicas; encontrará en él un abundante material acerca de la confiabilidad, consistencia interna, invariabilidad factorial, etc., de muchos de los principales instrumentos de evaluación.

Ana María Insua



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