This review has summarized some of the more recent test findings which show that comparative studies of various national and racial groups may not be made with existing tests, and which show, in particular, that one of the most pretentious of these comparative racial studies—the writer's own—was without foundation (p. 165).

In his survey[5] of the information available on the question of racial differences, Professor Klineberg clearly indicates the artificial character of the "facts" of racial difference, and states that "the conclusion came first, and the 'facts' were found to justify it" (p. 344).

In the recent studies of race differences in which careful attempts have been made to control some of the non-racial variables affecting performance, as well as in the best conducted of the earlier studies, the purported differences in performance as among races have not been demonstrated with any degree of clarity. In summary, it can be said that, so far as the psychological evidence is concerned, on the one hand there has been no clear demonstration of facts which are unequivocably interpretable as evidence for the existence of racial differences, while on the other hand there does exist a growing body of evidence which indicates that as cultural factors are controlled the obtained differences in psychological characteristics as among races tend to diminish to the point of insignificance.

HERBERT G. BIRCH

REGENERATION OF ADULT MAMMALIAN SKELETAL MUSCLE IN VITRO[1]

IN the course of an *in vitro* study of normal synovium from the patellar region of an adult rat, we have noted the appearance, growth and spontaneous contraction of striated muscle fibers from the muscle component of the explants.

On the 14th and 15th days of culture, muscle fibers were noted in two cultures; on the 16th day, the fibers began to contract spontaneously; and on the 18th day, the fibers still contracting, cross striations were observed. A portion of the explant also contracted rhythmically and spontaneously. These cultures were kept for six days more, during which time they were washed and fed about three times. The fibers increased in number and length. During this period they retained the capacity to contract spontaneously and rhythmically, at different rates and intervals. One fiber with no connection to the explant was also seen contracting. The contraction was most vigorous and involved the greatest number of fibers immediately after washing and feeding. Often there was a period of relatively rapid rhythmic contraction followed by a period of inactivity, after which contraction was resumed.

The outgrowing muscle fibers were ribbon-like structures with nuclei occurring singly, doubly, or in threes, fours or fives along their length. Longitudinal striae could be seen in the ribbons. Cross striae were elusive in nature, appearing, disappearing and reappearing. The majority of fibers showed no cross striae throughout their length even when contracting.

The muscle fibers are similar in structure and behavior to those of rat embryos (intercostal region) grown in this laboratory. In both cases the Maximow method was used, *i.e.*, a flying coverslip upon which the cells grew undisturbed except when being washed and fed. The only differences observed were that the embryonic muscle fibers were broader and had a greater number of nuclei. Longitudinal striae could be seen; cross striae were elusive, as in the case of the adult fibers. The muscle fibers in the embryo began to contract spontaneously at between four and eight days. One culture retained its capacity to contract for over a month.

In summary: Striated muscle from adult rat was seen to regenerate and to contract spontaneously *in vitro;* in appearance and behavior these fibers were similar to embryonic rat striated muscle fibers cultivated by the same method.

Since innervation of any sort is eliminated, this capacity of skeletal muscle to fibrillate *in vitro* offers a method for further analysis of contractility.

IRENE A. POGOGEFF
MARGARET R. MURRAY

ORTHOGRAPHY OF SCIENTIFIC NAMES

THE recent article by Dr. Harold Kirby[1] is a fine summary of the complex problem of transcription and orthography in scientific nomenclature. It omits one point which has had considerable importance in the past and continues to plague systematists. Transliteration has a strong nationalistic flavor. Many authors have been concerned to produce a version of the Greek or Latin term which looked French, Italian or German, etc. The real problem is: Are we rendering letters or sounds? Actually we compromise the matter by doing sometimes one and sometimes the other. This difference is illustrated by the official Russian and Library of Congress systems of transliterating Russian. The first conveys the spelling in Roman without helping a Western European to pronounce the word, while the latter gives the pronunciation but no idea of the Cyrillic spelling.

To look at some of Dr. Kirby's examples: *Agchylostoma* gives the Greek spelling but not the sound,

[5] Otto Klineberg, "Race Differences," Harper and Brothers, N. Y., 1935.

[1] From the Department of Surgery of the Presbyterian Hospital, and the Laboratory of Surgical Pathology, Columbia University, New York, N. Y.

[1] SCIENCE, 100: 425–427, 1944.

Chlamidomonas and others represent the usual and incorrect sound of upsilon. *Y* and *ph* are letters or combinations that are unfamiliar or unpronounceable to Italians. I doubt if most Italian printers of the seventeenth century even had *y* in their fonts. The omission of initial *h* in Aplosporidium is simply the omission of a silent letter which is not a letter but a diacritical mark in the original Greek. The Germans often used *ö* for *oe* even in Latin words where no transliteration is involved.

The rules are not really precise as to the language in use. Greek and Latin are mentioned, but it is not made clear that we are to transliterate Greek into Latin according to classical Latin custom rather than according to the varying usages of modern languages. *Strombidion* is a correct transliteration from Greek, but the word remains Greek in form, not Latin. Doubtless an educated Roman would have had no difficulty in understanding the Greek ending. The only course seems to be to enforce correct transliteration into Latin by amending such barbarous forms as *Flebotomus*.

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TRANSLITERATION OF RUSSIAN WORDS

WHILE it would appear from Dr. Hoare's note in SCIENCE for December 15, 1944, that my previous note (June 16, 1944) contained a view contrary to his, the truth appears to be that we are "on the same side of the fence."

I agree thoroughly that a universally applicable Russian transliteration system is an admirable idea, and I hope that such a system will be adopted eventually.

The second part of the matter under discussion touches a somewhat different point. I believe that a person using a transliterated Russian word, be it on a file card or in a research notebook, will be compelled, at one time or another, to attempt to pronounce it in talks with the fellow workers. It is at this point that a difficulty will enter if the transliteration system used contains letters or symbols which are not found in this person's native alphabet. I simply feel that this trouble is best resolved by the use of closest phonetic counterparts in any given language for the Russian letters. Thus, for an English-speaking person the use of ordinary English letters would appear to be a good solution. This is essentially what is done in the *Chemical Abstracts* system. Incidentally, I should like to point out that the change of the Russian orthography a quarter of a century ago did not invalidate the C.A. system. In effect, the change of orthography affected only the total number of letters in the alphabet by elimination of letters which already had their phonetic counterparts (much to the delight of schoolboys, I can assure you). Thus, the phonetic features of the language were unchanged and the *Chemical Abstracts* system is perfectly usable as a pretty good phonetic transliteration system for both new and old orthographies.

The spelling of Czech in my note was my own personal oversight. Incidentally, this word presents some interesting points. It seems to me that for an English person the spelling "Chekh" is closer to the currently used pronunciation than is the usual "Czech" spelling. Frankly, I am at a loss as to how an English-speaking person would pronounce the C-z combination.

In closing this discussion, permanently I hope on my part, I wish to add that, inasmuch as Russian is my native language, it is possible that I fail to see some of the difficulties encountered by a non-Russian speaking person. I avoid the transliteration difficulties, etc., by keeping notes, etc., in whichever language is necessary.

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SCIENTIFIC BOOKS

SCIENCE IN THE UNIVERSITY

Science in the University. By members of the Faculties of the University of California. 332 pp. 10 photographic plates. 31 figures. Berkeley and Los Angeles: University of California Press. 1944. $3.75.

THE title of this interesting volume is a misnomer. The individual reader or librarian who should order it under the impression that "Science in the University" relates broadly to either might find the book disappointing. Actually it is a compilation of occasional addresses and papers by 19 scientists[1] of the University of California, published in commemoration of the seventy-fifth anniversary of the university's founding. About half of the chapters are concerned with state and regional topics such as "The California Current," "Evolution of a Sierran Landscape" and "Subsidence and Elevation in the Los Angeles Region," or with specific contributions made by University of California scientists to genetics, hydrography,

[1] Robert Grant Aitken, J. R. Oppenheimer, Joel H. Hildebrand, Carl L. A. Schmidt, G. Ross Robertson, Jakob Bjerknes, H. U. Sverdrup, William C. Putnam, U. S. Grant, O. L. Sponsler, Richard B. Goldschmidt, Charles B. Lipman, Claude E. Zobell, Ralph W. Chaney, Loye Miller, D. R. Hoagland, J. M. D. Olmsted, Knight Dunlap and S. J. Holmes.