nounced: Francis H. Slack, M.D. (Tufts), director of the laboratories of the Boston Board of Health, to be professor of bacteriology; J. S. Hughes, A.M. (Ohio), to be assistant in chemistry, and C. H. Clevenger, A.M. (Chicago), and Edward Bartholow, A.B. (Kansas), to be assistants in mathematics.

The *Journal* of the American Medical Association states that considerable dissatisfaction has been manifested in the medical and lay press of Hungary toward the appointment of Dr. L. Nekam to the chair of dermatology in the University of Budapest on the recommendation of Count Fichy, minister of public instruction, whose appointment has been sanctioned by Emperor Francis Joseph. The committee of the medical faculty had proposed the names of Drs. Török and Marschalko, to the general board whose duty it was to investigate and report on the applicants. This body entrusted this duty to a theologian, who ignored the proponents of the medical faculty and appointed Dr. Nekam, with the resulting dissatisfaction.

It is announced that a national office of French universities and schools has been inaugurated under the presidency of M. Paul Deschanel, of the French Academy. Professor Paul Appell, of the University of Paris, and Professor Georges Lyon, of the University of Lille, have been elected vice-presidents and Dr. Raoul Blondel has been appointed director. The new department is to be installed at the Sorbonne, and its object will be to make known to foreigners the educational resources of France.

DISCUSSION AND CORRESPONDENCE

SELECTIVE FERTILIZATION AND THE RELATION OF THE CHROMOSOMES TO SEX-PRODUCTION

Explanations as to what one has really said or meant make dull reading, but are sometimes pardonable in the interest of accuracy. Some one has said (was it W. K. Clifford?) that there are some subjects concerning which it is often difficult to be sure what others mean, and not always easy to be sure what one means oneself! Perhaps selective fertilization and its relation to the "sex-chromosomes" is one of these. At any rate, I find with some surprise that a number of recent writers seem to regard me as an advocate of a conception that I have from the first held to be improbable. The hypothesis of selective fertilization (with all that it implies) may be true, but it is not true that I have anywhere, to my knowledge, maintained or advocated it. On the contrary, already in the second of my "Studies on Chromosomes" this hypothesis was characterized as "*a priori* very improbable" (1905, p. 539), and I have since steadily sought to find an interpretation of the cytological facts that would not involve such a way of cutting the Gordian knot of the sex-problem.

In my third "Study" (1906), where this question was first fully considered, I suggested for purposes of analysis, two possible ways of interpreting the observed facts, but advocated neither owing to insufficiency of data. The first (characterized, rather unluckily, as the "Mendelian interpretation"), assumed, "for the purpose of analysis," that "the two sex-chromosomes, which couple in synapsis and are subsequently disjoined by the reducing division, are respectively a male-determinant and a female-determinant"—*i. e.*, that the two bear opposing or alternative male- and female-determining factors or "genes." Analysis brought out the fact that this assumption led to selective fertilization as a necessary corollary. But even in my first preliminary paper (1905) it was pointed out that this interpretation encountered "great, if not insuperable difficulties." Regarding this, the third "Study" states, "It has not been my intention to advocate the foregoing interpretation, but only to set forth as clearly as possible the assumptions that it involves" (p. 33). Admitting that it "might in fact give the true solution of the problem," I nevertheless "endeavored to seek for a different interpretation that might escape the necessity for assuming selective fertilization" (p. 33). The second interpretation, representing such an attempt, was based on the quantitative re-

lations of the "sex-chromosomes" without assuming alternative male and female genes. It was pointed out that each of the two suggested interpretations included or involved "assumptions which without additional data must be considered as serious difficulties. . . . Additional data will therefore be required, I think, to show in what measure either of the two general interpretations that have been considered may approach the truth" (p. 38). In view of so explicit a statement of my position it is rather astonishing to learn from a recent publication[1] that in my third "Study," because of the difficulties of the second interpretation, I "maintain the alternative view, that the allosomes have qualitative differences that are sex-determining, with Mendelian dominance, and with selective fertilization" (p. 3). It is equally disconcerting to read, further on, that "Boveri, in opposition to Wilson's explanation, does not believe that one chromosome has a male and the other a female tendency, but that they differ only in activity" (p. 5). There is here no indication of the fact that the view opposed by Boveri to mine is also mine, having been put forward as a part of my second interpretation (!).

Not until three years after my third "Study" did I take a more definite position in regard to this question, and then one decidedly against selective fertilization. In the fourth "Study" (1909, sent to press in February, 1908) it was stated only that the first interpretation "should not be rejected without further data, and especially not until the question of selective fertilization has been put to the test of direct experiment" (p. 97). In the fifth "Study" (1909) this question is not taken up. Finally, in two general reviews of the whole subject in its broader bearings[2] selective fertilization is treated as so improbable as almost to invalidate any interpretation into which it enters. I am therefore again somewhat at a loss to comprehend how another recent writer can say that after framing several theories of sex I have at length adopted as my "latest view" one that "not only assumes a great complication of gametic representatives, but also involves selective fertilization."[3]

I am very willing to take whatever may be my just share of blame for such misunderstanding—even though I think it might have been avoided by a little more care in reading. It may be due partly to the fact that I did not at first see that my second (quantitative) interpretation was no less Mendelian than the first, as Castle has since pointed out. Beyond this, a certain ambiguity may have been caused by too great brevity in certain passages of the fourth and fifth "Studies," where the question of qualitative differences of the "sex-chromosomes" is touched upon. These brief references took for granted the context supplied by the full critical discussion given in the third "Study," and the ambiguity disappears, I think, when this is borne in mind. One instance may be given from the fifth "Study," which contains the statement, "I believe that if the idiochromosomes be the sex-determinants their difference is probably a qualitative one" (p. 189). In this passage the careless omission of the words "in the male" after "difference" obscures the meaning and might readily mislead a reader who had not the full context in mind. No ambiguity will be found, I hope, in the two reviews already cited, where the general conclusions from my own and other investigations in this field are brought together.

Lastly, I have not committed myself to the view that the "sex-chromosomes" represent the exclusive factors of sex-determination, though in several places they have been provisionally assumed to be such in order to discover the consequences of such a view. Other possibilities are pointed out in several of my papers on the subject, and I have gone no farther than to maintain the probability that these chromosomes are "one of the essential factors." This question, like that of selective fertilization, seems to me an open one; and until both questions have received a certain

[1] Montgomery, *Biol. Bull.*, XIX., 1, 1910.

[2] SCIENCE, February, 1909; *Science Progress*, April, 1910.

[3] Geoffrey Smith, *Q. J. M. S.*, February, 1910.

answer, the meaning of the cytological facts will not become entirely clear.

Edmund B. Wilson

Woods Hole, Mass.,
August 5, 1910

Higher Education in Pittsburgh: A Comparative Study of Salaries

In the numerous articles on the question of college and university salaries that have recently appeared there seems to be a perfect unanimity of opinion that, considering the high services rendered, the salaries of teachers are altogether too small. The conviction seems quite general that teachers are less adequately paid than any other class of workers. The figures published in the bulletins and reports of the Carnegie Foundation for the Advancement of Teaching have further deepened and enforced this conviction.

In these reports two classes of figures have been given, the average and the maximum salaries of professors and other teachers. But the minimum salaries of teachers and the salaries of presidents have not been given. Had these been included in the reports it is quite likely that the conception would have been still further deepened that teachers are poorly paid. In some institutions the minimum salaries are distressingly low, and afford the best basis for reckoning the actual conditions. After an experience of some years in the University of Pittsburgh I have been interested in a comparison of salaries which I herewith present as possibly of general interest.

I have not been able to obtain figures for all the institutions I wished to include in the comparison, as the view seems to prevail that the business of universities, other than state institutions, is the private affair of the trustees and need not be given to the public. The figures I give have been taken from official reports and from Carnegie Foundation publications, or have been received directly from officers of the various institutions. In all cases the figures used are the salaries of full professors, and for the academic year 1908–09 only, except where comparison is directly made with other years. No doubt in some cases the figures for the past year, 1909–10, would differ from these, but they are not yet available.

A curious fact about Pittsburgh is that the high school pays uniformly better salaries than the university, except in the single case of the heads of the institutions. In the high schools of the city, the minimum for professors is $2,000, in the university $1,200; while the maximum in the high schools is $2,500, and in the university $1,800. Similarly, the high-school principals receive $3,000, and the university deans $2,000. On the other hand, the director of high schools receives $4,000, while the chancellor of the university receives $7,500. Thus it appears that high-school teaching pays much better than university teaching, but high-school administration pays only a little better than half as well. Every year it happens, therefore, that students in going from the high school to the university pass up to teachers receiving much less than their preparatory teachers, but come under a chancellor who receives almost twice as much as their high-school director. It may be said in passing that the high school has a regular schedule of salaries, whereas none exists for the university, each teacher being engaged on an individual salary.

It should be said in fairness that the foregoing figures for the University of Pittsburgh are in some respects different from those of previous years. For some time preceding the academic year of 1908–9, one salary of $2,500 had been paid. But for that year, that and another of $1,800 were dispensed with, and in their places two of $1,500 and $800 were given, the latter to an instructor. A saving of $2,000 was thus made for the university; but as the chancellor for the same year received an increase from $6,000 to $7,500, the net saving to the university was only $500.

An interesting set of facts can be obtained by a comparison of the average salaries of professors of the University of Pittsburgh for several successive years. The second annual report of the president and treasurer of the Carnegie Foundation for the Advancement of Teaching, published October, 1907 (p. 24),