Nettie M. Stevens and the Discovery of Sex Determination by Chromosomes

*By Stephen G. Brush**

I

DURING THE FIRST DECADE of the twentieth century it was established that the sex of almost all many-celled biological organisms is determined at the moment of fertilization by the combination of two kinds of microscopic entities, the X and Y chromosomes.[1] This discovery was the culmination of more than two thousand years of speculation and experiment on how an animal, plant, or human becomes male or female; at the same time it provided an important confirmation for the recently revived Mendelian genetics that was to become a central part of modern biology.

According to most biologists and historians who have written on the subject, the crucial step in the discovery of chromosomal sex determination was taken in 1905 by Nettie M. Stevens (1861–1912) and Edmund B. Wilson (1856–1939). But the scientific and chronological relation between their contributions has rarely been specified, and the role of Stevens, who died in 1912 before she could attain a reputation comparable to that of Wilson, has sometimes been forgotten. In fact neither Stevens nor Wilson is now given adequate recognition by writers of texts and popular works on biology; most of the credit for the establishment of modern genetics usually goes to Thomas Hunt Morgan (1866–1945), who would not accept the chromosome theory until several years after the work of Stevens and Wilson had been published.

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[1] This conclusion was not accepted by all biologists in subsequent decades; during the 1920s and 1930s there was strong support for a quantitative "balance" theory of sex determination, with no sharp turning point in development. See C. B. Bridges, "Triploid Intersexes in *Drosophila melanogaster*," *Science,* 1921, *54*:252–254; "The Origin of Variations in Sexual and Sex-Limited Characters," *American Naturalist,* 1922, *56*:51–63; "Sex in Relation to Chromosomes and Genes," *Am. Nat.,* 1925, *59*:127–137; "The Genetics of Sex in Drosophila," in *Sex and Internal Secretions,* ed. E. Allen (Baltimore: Williams and Wilkins, 1932), pp. 55–93. R. Goldschmidt, "The Determination of Sex," *Nature,* 1921, *107*:780–784. W. E. Castle, "The Quantitative Theory of Sex and the Genetic Character of Haploid Males," *Proceedings of the National Academy of Sciences,* 1930, *16*:783–791. G. E. Allen, "Opposition to the Mendelian-Chromosome Theory: The Physiological and Developmental Genetics of Richard Goldschmidt," *Journal of the History of Biology,* 1974, *7*:49–92.

Nettie Maria Stevens (1861–1912). Photograph taken in 1904 (courtesy Carnegie Institution of Washington).

In this article I do not attempt a definitive assessment of Stevens' contribution to the chromosome theory of sex determination; that would require much more knowledge of the technical details of her work and that of others than I possess. My purpose is primarily to call attention to some documents (held at the Carnegie Institution of Washington) that illuminate her relations with Wilson and Morgan, and perhaps to persuade historians of biology to investigate her work more thoroughly.[2]

II

Nettie Maria Stevens was born July 7, 1861, in Cavendish, Vermont. Her father was a carpenter; she was the second of three children, the elder of two daughters. She attended Westfield State Normal School in Massachusetts to prepare for a teaching career. Her interest in science may have been aroused during summer courses at Martha's Vineyard in 1890 and 1891. She saved enough money from her teaching jobs to go to Stanford University, where she enrolled in 1896 and received a B.A. degree in 1899.[3] The following year she obtained her M.A. from Stanford and entered Bryn Mawr College as a doctoral student.

One might think that going to a small women's college would be fatal to the career of an aspiring scientist at that time, but in this case the opposite was true. Two of the leading American biologists, E. B. Wilson and T. H. Morgan, taught successively in the biology department at Bryn Mawr. Both were on the editorial board of the *Journal of Experimental Zoology* and otherwise influential in the scientific establishment. Though Wilson had gone to Columbia in 1891, he retained close ties with Morgan, who stayed at Bryn Mawr until 1904. As a student and later a colleague of Morgan's, Stevens was able to keep in touch with current research in the fast-moving fields of genetics, cytology, and embryology. As a promising woman scientist who had already published nine papers by the time she completed her Ph.D. in 1903,[4] she was eligible for special fellowships and prizes. The Bryn Mawr President's European Fellowship enabled her to study at the Naples Zoological Station and at the University of Würzburg, where she worked with the German biologist Theodor Boveri. Then she received an award of $1,000 from the "Association for Maintaining the American Woman's Table at the Zoölogical Station at Naples and for Promoting Scientific Research among Women," offered for the best paper written by a woman on a scientific subject.[5] A postdoctoral research assistantship from the Carnegie

[2]The most comprehensive accounts of the history of this subject are G. E. Allen, "Thomas Hunt Morgan and the Problem of Sex Determination, 1903–1910," *Proceedings of the American Philosophical Society,* 1966, *110*:48–57, and S. Gilbert, "Sex Determination and the Embryological Origins of the Gene Theory," Masters thesis (Johns Hopkins University, 1975). Gilbert says "Wilson was the major spokesman and Nettie Stevens the major source of evidence" for chromosomal determination of sex (p. 42).

[3]I am indebted to Rosamund Bacon of Stanford, California, for detailed information on Nettie Stevens' early life. The article by Hans Ris is the best published biographical source so far but contains some inaccuracies: see *Notable American Women,* ed. E. T. James, *et al.* (Cambridge, Mass.: Harvard University Press, 1971–1972), Vol. III, pp. 372–373. See also *The Bryn Mawr Alumnae Quarterly,* June 1912, pp. 124–126; *Stanford Alumnus,* Feb. 1913; "Life" inserted at the end of N. M. Stevens, "Further Studies on the Ciliate Infusoria, Licnophora and Boveria." Ph.D. dissertation (Bryn Mawr College, 1903). A comprehensive account of Stevens' life and work is being prepared by Marilyn Ogilvie at the University of Oklahoma.

[4]*Bryn Mawr Alum. Q.,* June 1912, p. 125; these papers dealt primarily with regeneration in different organisms. I have not found a complete bibliography of Stevens' publications; a fairly extensive list can be found in the card catalogue of the Bryn Mawr College Library.

[5]N. M. Stevens, "A Study of the Germ Cells of Aphis Rosae and Aphis Oenotherae," *Journal of Experimental Zoology,* 1905, *2*:313–333 (dated Dec. 20, 1904; published in the Aug. 1905 issue).

Institution of Washington allowed her to continue research at Bryn Mawr, unburdened by teaching duties during the crucial years 1904–1905, when her research on the sex chromosomes was being done.

III

During the previous decade Boveri, W. S. Sutton, Wilson, and others had found strong evidence for the individuality of chromosomes as identifiable parts of cells (reidentifiable after division) and suggested that each chromosome may be responsible for a definite part of the hereditary endowment of an organism. Chromosomes could be duplicated during the process of reproduction but otherwise remained unaffected by their environment; thus they seemed to behave like August Weismann's "germ plasm," which was supposed to be isolated from the rest of the organism and excluded the possibility that acquired characteristics could be inherited. In the first edition (1896) of his influential book *The Cell in Development and Inheritance,* Wilson collected evidence that the seat of heredity is in the chromosomes in the cell nucleus. This treatise helped prepare American biologists to receive Mendel's theory when it was independently rediscovered and advocated four years later by Hugo de Vries, Carl Correns, and Erich Tschermak von Seysenegg[6] and organized support for the Sutton-Boveri chromosome hypothesis.

But Wilson, Morgan, and most other biologists were not yet ready to accept the idea that sex is completely determined by chromosomes at the moment of fertilization. Instead, many believed that environmental factors such as nutrition play a major role in determining the sex of the developing embryo.[7] Wilson wrote, in the 1900 edition of *The Cell,* that "sex as such is not inherited. What is inherited is the capacity to develop into either male or female, the actual result being determined by the combined effect of conditions external to the primordial germ-cell."[8] In 1903 Morgan concluded an extensive review of the subject with the paragraph:

> Our general conclusion is that while recent theories have done good service in directing attention to the early determination of sex in the egg, those of them which have attempted to connect this conclusion with the assumption of the separation of male from female primordia in the germ-cells have failed to establish their point of view. The egg, as far as sex is concerned, appears to be in a sort of balanced state, and the conditions to which it is exposed, even when it is not fully formed, may determine which sex it will produce. It may be a futile attempt to try to discover any one influence that has a deciding influence for all kinds of eggs. Here, as elsewhere in organic nature, different stimuli may determine in different species which of the possibilities that exist shall become realized.[9]

In 1902 C. E. McClung suggested that the "accessory chromosome," discovered earlier by Hermann Henking, plays an important part in sex determination. His

[6] H. J. Muller, "Edmund B. Wilson—An Appreciation," *Am. Nat.,* 1943, *77*:5–37, 142–172; see pp. 29 ff.

[7] Allen, "Thomas Hunt Morgan," p. 49; Gilbert, "Sex Determination," pp. 5–8. According to W. E. Castle, "The last forty years have seen the rise, culmination, and at least incipient decline of a plausible but fundamentally erroneous idea about sex,—the idea that it is subject to control through the environment of the developing organism. The latest manifestation of this idea is found in Schenk's theory of sex-control in man through regulation of the mother." See Castle, "The Heredity of Sex," *Bulletin of the Museum of Comparative Zoology, Harvard College,* 1903, *40*:189–218, p. 190. He cites L. Schenk's theory presented at the International Zoological Congress in Berlin, Aug. 1901.

[8] E. B. Wilson, *The Cell in Development and Inheritance* (2nd ed., New York: Macmillan, 1900), p. 145. Wilson allowed this statement to remain in reprints of the book as late as 1919 (copy in University of Maryland Library).

[9] T. H. Morgan, "Recent Theories in Regard to the Determination of Sex," *Popular Science Monthly,* 1903, *64*:97–116.

hypothesis was that this chromosome is "the bearer of those qualities which pertain to the male organism."[10] This hypothesis turned out to be wrong, but it attracted the attention of other workers such as William Bateson and Miss E. R. Saunders at Cambridge University and W. E. Castle at Harvard.[11] There is some disagreement among later writers as to how much credit McClung should receive in the discovery of chromosomal sex determination, but there is no doubt that his hypothesis was extremely important in stimulating work by others.

Stevens was quick to see the possibilities opened up by McClung's hypothesis in conjunction with the Mendelian theory of heredity. Morgan's letter to the Carnegie Institution, November 19, 1903, in support of her application for a grant, makes it clear that she was the one who wanted to investigate sex determination by chromosomes, whereas he, the senior (though younger) partner of the team, was still under the influence of environmental theories (see Appendix). Yet this difference of opinion did not prevent them from collaborating. In fact, given a situation of mutual respect, the disagreement was fortunate for Stevens in the long run, since it meant that she published her results under her own name alone. If Morgan's name had been on her 1905 publication, the scientific world would undoubtedly have given most of the credit to him. (See Sec. V, below.)

Both Morgan and Wilson were enthusiastic in recommending Stevens to the Carnegie Institution. Morgan wrote that "of the graduate students that I have had during the last twelve years I have had no one that was as capable and independent in research work as Miss Stevens . . ." (see Appendix). Wilson stated: "I know Miss Stevens' work well, and it is of a very independent and admirable character from every point of view. I consider her not only the best of the women investigators, but one whose work will hold its own with that of any of the men of the same degree of advancement."[12]

Stevens' initial letter of application to the Carnegie Institution, dated July 19, 1903, indicated her situation quite clearly: she needed money to live on, and "College positions for women in Biology this year seem, however, to be very few." She wanted to pursue the "histological side of the problems in heredity connected with Mendel's Law" (the complete letter is given in the Appendix).

The grant was awarded, and Stevens proceeded with her research, which involved detailed examination of the chromosomes of several insects and comparison with the sex of the progeny. In a paper on the germ cells of aphids completed at the end of 1904, she failed to find McClung's extra chromosome, but the direction of her research was clear: a review of the current state of research had convinced her that "the evidence is overwhelmingly on the side of the view that sex is determined in the egg; but to the question how sex is determined in the egg, no thoroughly convincing answer has yet been given."[13]

[10]C. E. McClung, "The Accessory Chromosome—Sex Determinant?" *Biological Bulletin,* 1902, *3*:43–84, p. 72. McClung's subsequent work was not very fruitful; see letter from E. B. Wilson to C. D. Walcott, Oct. 24, 1903, in Wilson file at Carnegie Institution of Washington.

[11]W. Bateson and Miss E. R. Saunders, "Experimental Studies in the Physiology of Heredity," *Reports to the Evolution Committee of the Royal Society,* Report I (London: Harrison & Sons, 1902), pp. 138–139. Castle, "The Heredity of Sex."

[12]E. B. Wilson to C. D. Walcott, Nov. 27, 1903; letter in Stevens file at Carnegie Institution of Washington, quoted by permission.

[13]Stevens, "A Study of the Germ Cells," p. 328. Morgan remarked that "Miss Stevens denied at first the presence of an unpaired sex chromosome in the spermatogenesis, but later corrected this error. She failed to note, at first, that the male had fewer chromosomes than the female, but later recognized this difference." T. H. Morgan, "The Scientific Work of Miss N. M. Stevens," *Science,* 1912, *36*:468–470, p. 469.

Stevens was more fortunate with *Tenebrio molitor,* the common mealworm; males are produced by spermatozoa containing one chromosome that is clearly much smaller than the corresponding chromosome in the spermatozoa that produce females. On May 23, 1905, she submitted to the Carnegie Institution a manuscript on "Studies in Spermatogenesis" for publication in the Carnegie monograph series. It was sent on May 29 to Wilson, as a member of the institution's advisory committee, for his opinion. He returned it on June 13 with the brief statement: "It is in every way a most admirable piece of work which is worthy of publication by any learned society, and I do not hesitate to recommend it to you for publication by the Institution."[14]

Stevens' monograph on spermatogenesis was published in September 1905. After describing her experiments with *Tenebrio,* she concluded:

> Since the somatic cells of the female contain 20 large chromosomes, while those of the male contain 19 large ones and 1 small one, this seems to be a clear case of sex-determination, not by an accessory chromosome, but by a definite difference in the character of the elements of one pair of chromosomes of the spermatocytes of the first order, the spermatozoa which contain the small chromosome determining the male sex, while those that contain 10 chromosomes of equal size determine the female sex. This result suggests that there may be in many cases some intrinsic difference affecting sex, in the character of the chromatin of one-half of the spermatozoa, though it may not usually be indicated by such an external difference in form or size of the chromosomes as in Tenebrio.[15]

IV

It is generally stated that E. B. Wilson obtained the same results as Stevens, at the same time. Roughly speaking, this is true, but the statement must be qualified in three ways. First, Wilson happened to choose a species in which the male has one less chromosome than the female, whereas Stevens investigated the much more common case in which the male has a small chromosome (Y) corresponding to the large chromosome (X) in the female. It could later be argued that the cases are "the same in principle,"[16] but in the context of early-twentieth-century biology the Wilson case (X,O) looks like a simple reversal of McClung's hypothesis and does not bring out clearly the dominant-recessive feature that distinguishes the modern (X,Y) theory.

Second, Wilson probably did not arrive at his conclusion on sex determination until after he had seen Stevens' results. This is perhaps the most important point established by the documents at the Carnegie Institution. Morgan, in his obituary of Wilson, implied that Wilson discovered the crucial difference between the chromosome numbers in *Anasa tristis* (22 in the female, 21 in the male) before Stevens submitted her paper:

> The question is sometimes asked as to the priority of Stevens' and Wilson's papers. Stevens' paper was handed in on May 15, 1905[17] and printed in September of that year. In

[14] E. B. Wilson to R. S. Woodward, June 13, 1905; letter in Stevens file at the Carnegie Institution, quoted by permission.

[15] N. M. Stevens, *Studies in Spermatogenesis with Especial Reference to the "Accessory Chromosome"* (Washington, D.C.: Carnegie Institution of Washington, Publication No. 36, September 1905), p. 13. The paper is dated May 15, 1905, but Stevens waited a week before sending it to the Carnegie Institution in the hope of getting the official name of one of the species she had studied; see the letter of transmittal from Stevens to R. S. Woodward, May 23, 1905, in the Stevens file at the Carnegie Institution.

[16] T. H. Morgan, "Biographical Memoir of Edmund Beecher Wilson 1856–1939," *Biographical Memoirs of the National Academy of Sciences,* 1940, *21*:315–342, p. 333.

[17] It was actually sent on May 23 (see above, n. 15), but this does not affect the priority question.

> Wilson's paper "Studies on Chromosomes" I (dated May 5, 1905; published August 1905) he says in a footnote: "The discovery, referred to in a preceding footnote, that the spermatogonial number of Anasa is 21 instead of 22, again goes far to set aside the difficulties [of McClung's hypothesis] here urged. Since this paper was sent to press I have also learned that Dr. N. M. Stevens (by whose kind permission I am able to refer to her results) has independently discovered in a beetle, Tenebrio, a pair of unequal chromosomes that are somewhat similar to the idiochromosomes in Hemiptera and undergo a corresponding distribution to the spermatozoa. She was able to determine, further, the significant fact that the small chromosome is present in the somatic cells of the male only, while in those of the female it is represented by a larger chromosome. These very interesting discoveries, now in course of publication, afford, I think, a strong support to the suggestion made above; and when considered in connection with the comparison I have drawn between the idiochromosomes and the accessory show that McClung's hypothesis may, in the end, prove to be well founded."[18]

But Morgan overlooked the fact that the "preceding footnote" in Wilson's paper, mentioned in the above quotation, was not in the original paper as submitted on May 5; it begins "Since this paper was sent to press I have determined beyond the possibility of doubt, I think, that the number of spermatogonial chromosomes in Anasa tristis is 21, not 22. . . ."[19] Wilson emphasizes in this footnote the special care he took to verify this result, which disagreed with the results of other biologists; it is hardly likely (and he does not claim) that he established the result in the period of less than four weeks between May 5 and the time he received Stevens' paper from the Carnegie Institution for review.

It is true that Wilson's paper "Studies on Chromosomes" (part I) was published before Stevens'—in fact it appeared in the same issue of the *Journal of Experimental Zoology* (August 1905) as Stevens' paper on aphids that had been submitted to this quarterly journal in December 1904.[20] But in view of Wilson's position on the editorial board, the fact that his paper was published with a relatively short time lag (three months, compared to eight months for Stevens') perhaps should not be used to establish his priority.

Nevertheless, one may still reject the above two arguments—one may claim that Wilson did arrive at essentially the same conclusion as Stevens, before he received her paper at the end of May 1905. Then look at his short article, dated October 3, 1905, published in *Science* on October 20 of that year. This is the article usually cited as the first report of Wilson's discovery of sex determination.[21] Although he begins by stating that there is "no doubt that a definite connection of some kind between the chromosomes and the determination of sex exists in these animals," he concludes by reverting to a semi-environmental theory:

> . . . great, if not insuperable, difficulties are encountered by any form of the assumption that these chromosomes are specifically male or female sex determinants. It is more

[18] T. H. Morgan, "Biographical Memoir," p. 333, quoting from E. B. Wilson, "Studies on Chromosomes. I. The Behavior of the Idiochromosomes in Hemiptera," *J. Exp. Zool.*, 1905, *2*:371–405, p. 403.

[19] *Ibid.*, p. 399.

[20] The fact that Wilson's article was published in the August 1905 issue of the *Journal of Experimental Zoology* does not necessarily mean that it actually appeared before Stevens' monograph, which was dated September 1905. According to Alice Baxter, "In a letter from T. H. Morgan to Ross G. Harrison (September 11, 1939, Ross G. Harrison collection at Yale University, New Haven, Conn.), the former inquired as to who deserved priority. Harrison answered in a letter (September 14, 1939, Ross G. Harrison collection) saying that he had received Stevens' paper a day before Wilson's." Alice Levine Baxter, "Edmund Beecher Wilson and the Problem of Development: From the Germ Layer Theory to the Chromosome Theory of Inheritance," Ph.D. dissertation (Yale University, 1974), p. 308.

[21] E. B. Wilson, "The Chromosomes in Relation to the Determination of Sex in Insects," *Science*, 1905, *22*:500–502.

> probable . . . that the difference between eggs and spermatozoa is primarily due to differences of degree or intensity, rather than of kind, in the activity of the chromosome groups in the two sexes; and we may here find a clue to a general theory of sex determination that will accord with the fact observed in hemiptera . . . during the synaptic and growth periods . . . these chromosomes play a more active part in the metabolism of the cell in the female than in the male. The primary factor in the differentiation of the germ cells may, therefore, be a matter of metabolism, perhaps one of growth.[22]

Wilson in 1905 was reluctant to come down as firmly as Stevens on the side of instantaneous sex determination by the mere presence or absence of a particular type of chromosome. Perhaps his greater knowledge of the complexities of the data available at that time made him properly cautious about jumping to such a simplistic conclusion; perhaps Stevens was only making a rash generalization unjustified by the evidence. Her understanding of the dominant-recessive properties of X and Y chromosomes, based on Castle's modification of Mendelian inheritance,[23] was not the same as the modern view (in which Y is always dominant and X always recessive), so one cannot accuse Wilson of rejecting a completely correct theory.

Nevertheless, there is no doubt that as late as 1906 Stevens was still ahead of Wilson in realizing the significance of their discovery. She wrote:

> Wilson[24] suggests as alternatives to the chromosome sex determinant theory according to Mendel's Law, (1) that the heterochromosomes may merely transmit sex characters, sex being determined by protoplasmic conditions external to the chromosomes; (2) That the heterochromosomes may be sex-determining factors only by virtue of difference in activity or amount of chromatin, the female sex chromosome in the male being less active.[25]

She could cite evidence against both of these alternatives, and concluded: "On the whole, the first theory, which brings the sex determination question under Mendel's Law in a modified form, seems most in accordance with the facts, and makes one hopeful that in the near future it may be possible to formulate a general theory of sex determination."[26]

V

Nettie Stevens benefited from the encouragement and support of the scientific establishment during the time she was doing her most important work, but it does not appear that she later gained a reputation or material rewards commensurate with her accomplishments. Failure to win a Nobel Prize (if one thinks the discovery deserved it) can be explained by the fact that the significance of sex chromosomes in genetics was not generally appreciated before her death; Morgan had to wait until 1933 for his

[22] *Ibid.*, pp. 501–502.

[23] Castle, "The Heredity of Sex"; Allen, "Thomas Hunt Morgan," p. 49. Castle's theory assumed both male- and female-producing eggs as well as male- and female-producing sperm, with selective fertilization.

[24] E. B. Wilson, "Studies on Chromosomes. III. The Sexual Differences of the Chromosome-Groups in Hemiptera, with some Considerations on the Determination and Inheritance of Sex," *J. Exp. Zool.*, 1906, *3*:1–40, submitted Dec. 8, 1905.

[25] N. M. Stevens, *Studies in Spermatogenesis. Part II. A Comparative Study of the Heterochromosomes in certain Species of Coleoptera, Hemiptera and Lepidoptera, with Especial Reference to Sex Determination* (Washington, D.C.: Carnegie Institution of Washington, Publication No. 36, Part II, October 1906), p. 55. To balance the impression of Wilson's position given by this quotation, I quote his own statement ("Studies on Chromosomes," p. 26): "The observations here brought forward, together with those of Stevens on Tenebrio, establish the predestination (in a descriptive sense) of two classes of spermatozoa, equal in number, as male-producing and female-producing forms . . . it is evident that a substantial basis now exists for . . . the Mendelian interpretation of sex-production worked out by Castle."

[26] Stevens, *Studies in Spermatogenesis, Part II,* p. 56.

trip to Stockholm, and Wilson never did make it.[27] More serious is the fact that most modern textbooks, if they mention Stevens at all, give the impression that she worked with or following Wilson.[28] Because of Wilson's more substantial contributions in other areas, he tends to be given most of the credit for this discovery, as a result of the operation of the "Matthew effect" noted by sociologist Robert Merton.[29] ("Unto every one that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath"—Matthew XXV: 29). The most extreme example of this effect is the occasional ascription of the discovery of chromosomal sex determination to T. H. Morgan, simply because he is considered the most important American geneticist in the first half of the twentieth century and hence the only one mentioned in superficial accounts.[30]

Those who seek outstanding female scientists to inspire the next generation of talented women to follow scientific careers seem to have overlooked Nettie Stevens. This may be partly because most books that do mention her work identify her only as "N. M. Stevens," giving no hint of gender.[31]

Even Bryn Mawr College was somewhat tardy in recognizing the achievements of its most eminent woman biologist, as the *Stanford Alumnus* pointed out in its obituary (February 1913). Her highest position there was Associate in Experimental Morphology. The Trustees of Bryn Mawr finally created for her a research professorship, but she was never able to occupy it; shortly afterwards she died of carcinoma of the breast, on May 4, 1912, at Johns Hopkins Hospital in Baltimore.

APPENDIX. STEVENS' APPLICATION FOR A CARNEGIE RESEARCH GRANT AND MORGAN'S LETTER OF RECOMMENDATION[32]

Mt. View, California
July 19, 1903

Sec'y of the
Carnegie Inst.,
Washington D. C.

Dear Sir—President Thomas of Bryn Mawr College advised me some time ago to apply for one of the Carnegie fellowships and continue research work instead of teaching next year, but considering the condition of my finances, I thought it better to

[27]One indicator of current reputations is the length of articles in the *Dictionary of Scientific Biography:* Wilson's is 14 pages long, Morgan's is 12 pages (both by G. E. Allen), but Stevens has no article at all.

[28]Of 11 authors who mention the names of both discoverers, 7 say "Stevens and Wilson" and 4 say "Wilson and Stevens." This is perhaps about what one might expect from random choice with a preference for alphabetical order. But 6 authors give credit *only* to Wilson: Carl Correns (1913), L. Doncaster (1914), Conway Zirkle (1959), Isaac Asimov (1960), M. J. Sirks and C. Zirkle (1964), E. J. Gardner (1972). One author, A. M. Winchester, gives all the credit to Stevens in one book (1966) and all the credit to Wilson in another (1972). (Complete references will be supplied on request.)

[29]R. K. Merton, "The Matthew Effect in Science," *Science,* 1968, *159*:56–63.

[30]Ruth Moore, *The Coil of Life: The Story of the Great Discoveries in the Life Sciences* (New York: Knopf, 1961), p. 217. G. R. Taylor, *The Science of Life: A Picture History of Biology* (New York: McGraw-Hill, 1963), p. 322. J. J. Fried, *The Mystery of Heredity* (New York: Day, 1971), p. 41. J. H. Otto and A. Towle, *Modern Biology* (New York: Holt, Rinehart & Winston, 1973), pp. 152–153.

[31]T. H. Morgan *et al.* (1915), C. B. Bridges (1939), F. A. E. Crew (1946), B. Dawes (1952), A. H. Sturtevant (1951), E. A. Carlson (1966), L. Levine (1969), U. Mittwoch (1973). (Complete references will be supplied on request.)

[32]Both letters are in the Nettie Stevens file at the Carnegie Institution of Washington; published by permission of the Carnegie Institution.

try for a position to teach. College positions for women in Biology this year seem, however, to be very few; and I should like to know whether there are still any fellowships to be awarded this year. I have been doing research work for four years and should prefer to continue to do that instead of teaching if there were no money question involved, but I am dependent on my own exertions for a living, and have used nearly all that I had saved while teaching before I began my college-work seven years ago.

I am especially interested in the histological side of the problems in heredity connected with Mendel's Law, and know that there is need of a great deal of painstaking work along that line.

So far my research work has been on two new species of Protozoa, problems in regeneration, ovogenesis and spermatogenesis of Sagitta and Planaria lugubris.

I heard rumors before I left Mass. of a school for research work, in which the members were to receive salaries and give their time to investigation. That is exactly what I should like, an opportunity to devote my time to research work, and freedom from anxiety over the money question.

Yours truly

N. M. Stevens

References:—
Prof. T. H. Morgan, Woods Hole
Prof. C. O. Whitman, " "

• • •

Bryn Mawr Penna Nov 19 1903

To the Carnegie Institution of Washington:

Dear Sirs:

At my suggestion Miss N M Stevens has made an application to the Carnegie Institution for a research Assistantship and I beg to urge as strongly as possible her appointment. Of the graduate students that I have had during the last twelve years I have had no one that was as capable and independent in research work as Miss Stevens and now that she has her degree she is devoting all of her time to research. <work> [deleted] Miss Stevens has not only the training but she has also the natural talent that is I believe much harder to find. She has an independent and original mind and does thoroughly whatever she undertakes. I fear to say more lest it may appear that I am overstating her case.

I have begun a piece of research work in collaboration with Miss Stevens that will take a year or more to bring to completion provided we can work together which will be impossible after January unless Miss Stevens obtains the appointment she seeks. The question of the factors that determine the sex of the egg is one that is now coming rapidly to the front and bids fair to give results of far reaching importance not only of theoretical but of practical interest as well. Our first problem wil[l] be to examine the conditions in the aphids where it appears possible to cause the appearance of the males and females (as contrasted with parthenogenetic forms) by changing the food. I am carrying out the experimental side of the work and Miss Stevens is examining at the same time the internal changes in the egg (the origin of the eggs, the number of

polar bodies formed, the number of chromosomes present in the males, females, and parthenogenetic forms, together with the question of the reduction divisions).
At the same time Miss Stevens proposes to examine in another more favorable form the so-called accessory chromosome in eggs and sperm-cells and its possible relation to sex.
I trust that the importance of the questions involved, as well as Miss Stevens special aptitude for the work, will prevail upon the Carnegie Institution to consider her application favorably. It is also of the greatest importance to me to have some one working with me on this problem and I know of no one who is so well suited to carry out work of this sort as Miss Stevens.

Respectfully yours
T. H. Morgan
Professor of Biology
Bryn Mawr