The *Drosophila* Group: The Transition from the Mendelian Unit to the Individual Gene

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As William Bateson conceived it when he coined the term "genetics" in 1906, this new field would combine the approaches used to study heredity and variation and their relation to evolution. At first heredity and variation were thought to be separate phenomena, involving different mechanisms. The confusion of these two concepts was not cleared up by sweeping these items under the rug called genetics. It was not until 1921 that the current view, formulated by Hermann Joseph Muller, was forcefully expressed:

> It is commonly said that evolution rests upon two foundations – inheritance and variation; but there is a subtle and important error here. Inheritance by itself leads to no change, and variation leads to no permanent change, unless the variations themselves are heritable. Thus it is not inheritance *and* variation which bring about evolution, but the inheritance *of* variation, and this in turn is due to the general principle of gene construction which causes the persistence of autocatalysis despite the alteration in structure of the gene itself.[1]

The conviction which Muller maintained in 1921 was not possible in 1906. It required the union of cytology, breeding analysis, and Darwinism. This union took place between 1910 and 1915 at Schermerhorn Hall at Columbia University under the leadership of Thomas Hunt. Morgan and his students using the fruit fly, *Drosophila melanogaster,* and it culminated in the publication of *The Mechanism of Mendelian Heredity* (1915) by the chief contributors to that union: Morgan, Alfred Henry Sturtevant, Muller, and Calvin Blackman Bridges.

The key to the success of the *Drosophila* group was the theory of the gene. The unit of heredity was assigned to the chromosome and numerous phenomena became explainable through the combined cytological and genetic observations and experiments carried out by the group. The gene itself was named earlier, by Wilhelm Ludwig Johannsen

1. H.J. Muller, "Variation Due to Change in the Individual Gene," *Amer. Nat.*, *56* (1922), 32-50.

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in 1909, but Morgan was reluctant to use that term and retained the more widely used term "factor." By 1917 "gene" had caught on and had completely replaced "factor." The factorial hypothesis was thereafter known as the theory of the gene.

In the nineteenth century hereditary units were seriously considered by Mendel, Darwin, and Spencer. Mendel's use of the concept in breeding analysis was frustrated by his failure to confirm the universality of his *Pisum* results. He tried Nägeli's hawkweed seeds (*Hieracium*), and learned that hybirds did not form in the F_1 and that there was no segregation in the F_2. He may well have lost interest in his experiments after this disappointment. If Nägeli did not regard Mendel's work as a form of "Pythagoreanism," at least he could not have regarded it as more than a curiosity.

Spencer's physiological units were part of a highly speculative system of philosophy, too far removed from the biologist's main interests in natural history. Darwin's hypothesis of gemmules, which reluctantly embraced Lamarckism, gave his theory of pangenesis a chance for an experimental test. His cousin, Francis Galton, demolished the theory with blood transfusions in rabbits of different pelt colors. The predicted color changes among the progeny failed to occur. The absence of detectable gemmules in the blood so weakened the theory that it rapidly fell into disrepute.

With the growing recognition of the importance of the cell nucleus and its chromosomes, attempts were made to assign these hereditary units to the chromosomes. Wilhelm Roux, for example, conceived them to be linearly arranged in chromosomes. This would permit an equal distribution of hereditary units whenever cell division took place. It was good speculation, but no more so than that of August Weismann, who tried to use the distribution of these units in explaining development, or of Hugo De Vries, who saw in them the origin of new species by discontinuous changes (which he called mutations, but not in the sense we think of them today).

Morgan was not alone in his rejection of such speculation. It seldom led to experiments and it stifled progress in the field by maintaining too philosophic an approach. What Morgan respected was data, good experimental design, and the labor to see the work through. When Mendelism was touted after 1900 he refused to accept it. When the chromosome theory was pushed in 1902 and 1903 by Edmund Beecher Wilson and his students, Morgan opposed it. For several years he poked holes in the Mendelian claims of gametic purity, in segregation ratios, and in the Darwinian mechanism of evolution through imperceptibly fine gradations of variation.[2] If Morgan had so little enthusiasm for the main components of genetics, how did it happen that the *Drosophila* group became world reknowned as the champion of the very views that Morgan at first rejected?

There can be little doubt that the *Drosophila* group provided the major evidence for the theory of the gene. In the U.S. William Castle fought against it vigorously until 1919. In England Bateson scathingly attacked the chromosome theory but reluctantly admitted in 1921, after visiting Morgan's laboratory, that there was something to it after all. The biometric school under Pearson was even more conservative – they rejected both the chromosome theory and Mendelism. Bateson had decisively routed them on the Mendelian issue, but it required Sewall Wright, R. A. Fisher and J. B. S. Haldane to restore the quantitative approach to genetics, this time merging the theory of the gene, as the *Drosophila* group had proposed it, with the Darwinian variation that the biometric group had upheld all along. In the Netherlands, De Vries sought an alternative to Darwinism and he thought he had found it in the discontinuous speciation which emerged in the Oenotheras. His theory crumbled under the cytological analysis of Shull, Cleland, Renner, and Gates and the genetic analysis of Muller. By 1919 De Vries's mutation theory was scrapped and neo-Darwinism prevailed. The *Drosophila* group no longer had contenders and had now become spokesmen for the dominant viewpoint of genetics.[3]

The proper origin of modern genetics must be assigned to the *Drosophila* group. Almost all of their major claims and experiments were worked out between 1910 and 1915. These include sex linkage, crossing-over, nondisjunction, the correspondence of chromosome number and linkage groups, and the Mendelian basis of complex character traits. Traditionally, Morgan is given credit for sex-linkage and crossing-over, Sturtevant for devising the chromosome map, Bridges for nondisjunction, and Muller for analyzing the nonconformable complex character traits. Traditionally, too, the group is depicted in idyllic terms. Sturtevant, in retrospect, claimed that

> this group worked as a unit. Each carried on his own experiment but each knew exactly what the others were doing, and each new result was freely discussed. There was little attention paid to priority or to the source of new interpretations.

2. Garland Allen, "Thomas Hunt Morgan and the Problem of Natural Selection," *J. Hist. Biol., 1* (1968), 113-139.
3. E.A. Carlson, *The Gene: A Critical History* (Philadelphia: W.B. Saunders, 1966).

What mattered was to get ahead with the work. There was much to be done; there were many new ideas to be tested, and many new experimental techniques to be developed. There can have been few times and places in scientific laboratories with such an atmosphere of excitement and with such a record of sustained enthusiasm. This was due in large part to Morgan's own attitude, compounded of enthusiasm combined with a strong critical sense, generosity, open-mindedness, and a remarkable sense of humor. No small part of the success of the undertaking was also due to Wilson's unfailing support and appreciation of the work – a matter of importance partly because he was head of the department. . . . Because of the close cooperation in the work it is very difficult to trace the individual contributions to the development in this period.[4]

The actual sequence of ideas, experiments, and interplay contributed by the members of the group has never been carefully studied. Among contemporary geneticists it was well known that Muller, Edgar Altenburg, and Alexander Weinstein formed an anti-Morgan group and that Sturtevant had shaped the viewpoint of a much larger segment of geneticists at Columbia and at the California Institute of Technology. The reliability and diffusion of Muller's views on the *Drosophila* group were diminished by his personality conflicts with John T. Patterson and T. S. Painter at Texas in the late 1920's and later by his departure from the United States from 1932 to 1940. The new generation of geneticists – George Beadle, Jack Schultz, Theodosius Dobzhansky, Donald Frederick Poulson, Carl Clarence Lindegren – emerging from Cal Tech learned their history of the fly lab from Sturtevant, who retained a lifelong romantic glow about his formative years under Morgan. Sturtevant also was reluctant to go into details about the origin of ideas and the personality conflicts that existed. He was a gentleman, and he would quickly change the subject when pressed on the individual contributions of the members of the group or if asked to comment on Muller's version of the *Drosophila* group.[5]

For the historian, primary sources on the *Drosophila* group are meager. All of the major contributors are now dead. Altenburg gave a lengthy account in a series of interviews in 1966 and 1967. Muller was too ill to carry out the interviews he had agreed to do. Bridges died in 1938 and Morgan destroyed his personal papers for him because Bridges' private life was already legendary among geneticists.[6] Morgan

4. A.H. Sturtevant, "Thomas Hunt Morgan," *Biogr. Mem. Nat. Acad. Sci., 33* (1959), 283-325.

5. A.H. Sturtevant, personal interview, spring 1967, California Institute of Technology, Pasadena, Calif.

6. Ibid. Bridges was an open advocate of free love and never held a teaching position because of this reputation.

himself had little interest in personal history. He cleaned his files out every five years because he did not want to use more than one file case.[7] Sturtevant carefully selected his own correspondance and papers for donation to the Millikan Library at Cal. Tech. They are practically devoid of polemical, personal, or contradictory statements. This reflects, in part, Sturtevant's habit of being gentlemanly, and also his notorious habit of not answering correspondence, especially on controversial matters.[8] Altenburg's correspondence and notes were almost completely destroyed by a fire set by an arsonist in 1965 at St. Thomas' College in Houston, Texas. Weinstein is an extremely cautious person and he was not willing to go into details about the *Drosophila* group when interviewed. The major source of materials rests in the Lilly Library, where Muller's complete correspondence and notes exist in uncensored condition. Muller never threw anything out and his personality emerges with full humanness from his 30,000 letters.[9]

It is ironic that Sturtevant labored so hard to preserve an image but left few source materials to justify it. Muller, who had much more reason to prune his correspondence, left everything and in so doing permitted a far more extensive account of the *Drosophila* group than has yet been published or acknowledged. The Morgan-Sturtevant interpretation is based on obituaries of Bridges by Morgan, of Morgan by Sturtevant, and on the chapter "The Fly Room" in Sturtevant's *History of Genetics* (1965).[10] A secondary source essentially agreeing with the Sturtevant-Morgan view is presented by Jack Schultz in his review of Carlson's *The Gene: A Critical History*.[11] An unpublished manuscript by Bridges in Schultz's possession narrates one late phase (1916–1918) of the *Drosophila* group and provides an alternate account of the analysis of the first chromosome rearrangements encountered in *Drosophila*.[12] First-hand data also exist in the publications between 1910 and 1919 which the *Drosophila* group wrote individually or jointly.

7. Ibid.
8. H.J. Muller to C.H. Waddington, 24 July 1940, Muller Archives, Lilly Library, Indiana University, Bloomington, Ind.
9. See E.A. Carlson, "Indiana University: The Muller Archives," *The Mendel Newsletter* (American Philosophical Society), no. 4 (Nov. 1969), 1-2.
10. T.H. Morgan, "Personal Recollections of Calvin B. Bridges," *J. Hered., 30* (1939), 355-358; A.H. Sturtevant, *A History of Genetics* (New York: Harper & Row, 1965).
11. Jack Schultz, "Innovators and Controversies," *Science, 157* (1967), 296-301.
12. C.B. Bridges, "Cromosome Rearrangements in *Drosophila*," original typescript in papers of Jack Schultz, Institute for Cancer Research, Fox Chase, Philadelphia, Pa.

The Muller-Altenburg account exists largely in Muller's correspondence, in the autobiographical sketches Muller prepared for Vavilov and for the National Academy of Sciences, in the unpublished manuscript on *Drosophila* genetics for Baur's *handbuch* (written in 1925–1927), in a few published but hard to obtain sources, and in several notes which Muller drafted listing the accomplishments of the various members of the group. Altenburg left a smaller number of comments in what remains of his correspondence but he provided extensive commentary in his interviews.

Membership in the *Drosophila* group is usually restricted to its chief contributors – Morgan, Sturtevant, Bridges, and Muller. Altenburg was also active in the group discussions from 1911 to 1915. Weinstein joined the group about 1914, and between 1912 and 1916, F.N. Duncan, John S. Dexter, Shelley R. Safir and Roscoe R. Hyde also participated. L.S. Quackenbush was there for a year when Sturtevant began his studies but he left because he had asthma and could not continue living in New York City's climate.[13] Harold Henry Plough was also a later addition to the Morgan laboratory, as were Charles Robert Plunkett, Otto L. Mohr, Donald Elwood Lancefield and Alfred Francis Huettner, but they were all in the laboratory prior to 1920 and after 1916. Morgan's laboratory technicians and female graduate students (most of whom did not complete their Ph. D. degree) included Mildred Hoge, Eleth Cattell, Elizabeth Wallace, Sabra Tice, Mary B. Stark and Clara B. Lynch, all between the years 1912 and 1915.

Morgan's interests in heredity came later in his career. He was an embryologist who received his training at Johns Hopkins (Ph. D. 1890) under William Keith Brooks and H. Newell Martin. Brooks was too metaphysical to suit Morgan, but he got him interested in using live materials. Martin emphasized the experimental approach to animal physiology. Morgan published extensively on problems of regeneration, differentiation, and cell lineage. His interest in genetics was aroused about 1900 when he visited De Vries's laboratory in Amsterdam. Morgan used breeding analysis in mice, guinea pigs, and pigeons with little success in hunting for De Vriesian mutations. He also used cytology to study life cycles in aphids and other insects, especially emphasizing changes in chromosome number (ploidy level) between sexes or in different stages of development. He took on *Drosophila* through the direct or indirect urging of Castle, who had in 1905 published on the variations in productivity of laboratory-raised fruit flies. It is not clear whether Frank

13. Sturtevant interview, 1967.

E. Lutz or Castle urged Morgan to try using flies. Fernandus Payne claims that Morgan expressed his interest in flies to him and that he collected the first flies for Morgan by placing a jar with cut fruit on the ledge outside his laboratory window about 1907.[14]

Both Sturtevant and Morgan acknowledge a search for mutations as the motivating factor in the first *Drosophila* work at Columbia.[15] Those sought were, however, not the mutations Morgan eventually found, but the far more dramatic anticipations of species changes in keeping with the remarkable findings De Vries had shown him a few years before. For a little over two years Morgan found nothing remarkable and was losing interest when in January 1910 he cultured a wild strain of flies, some of which had a dark trident pattern on the thorax. He called these *with*. In March 1910 a new mutant arose; it produced a dark blemish at the junction of the wing and the thorax. This Morgan designated as *speck*. Also in March he found a body color somewhat darker than the normal gray-amber; he called it *olive*. His well known white-eyed fly arose in early May; as did beaded wing and another, independent, olive body color mutant. With this sudden shower of mutations occurring, which fitted the predictions of De Vries's mutation theory, Morgan wrote a short note, "Hybridization in a Mutating Period in *Drosophila*," and submitted it on May 18.[16] He then worked out the reciprocal crosses but, contrary to popular belief, he did *not* identify the case as an X-chromosome mutant. He was unable to do so because sex linkage had earlier been reported for the moth Abraxas and for canaries. Both cases showed the opposite of Morgan – the mutant appeared in the female more often than the male.[17] In June the mutant rudimentary wings was found. Morgan crossed this with white after determining it was also sex-linked but he did not find a simple linkage between them. They seemed to assort independently! Morgan did not know that white and rudimentary were some 50 map units apart. The next sex-linked mutant, miniature wings (August 1910) clearly showed

14. Fernandus Payne, personal interview, August 1967, Indiana University, Bloomington, Ind.

15. A.H. Sturtevant, "Reminiscences of T.H. Morgan" (unpublished paper presented at Woods Hole, Mass., 16 August 1967), Millikan Library, California Institute of Technology, Pasadena, Calif.

16. T.H. Morgan, "Hybridization in a Mutating Period in *Drosophila*," *Proc. Soc. Exp. Biol. and Med.*, 7 (1910), 160-161; presented 18 May 1910.

17. In the fruit fly as in man the female is XX and the male is XY. In birds and lepidoptera (moths and butterflies) the XX is male and the XY is female. Since an XY shows the mutant carried on the X, recessive or not, in fruit flies it will be the male, in Abraxas and birds it will be the female who shows these sex-linked traits more frequently.

linkage with white eyes. It was about 35 map units from white. While the results of these crosses were emerging, vermilion eyes (November 1910) was isolated, but it could not easily be tested with white eyes.[18] The abundance of sex-linked mutants and the decisive data from the white-miniature cross provided a clear instance of linkage (or coupling and repulsion as Bateson's group called it), and permitted the *joint* acceptance of the X-chromosome as the carrier of the sex-linked genes and crossing-over (or shifting, as Morgan first called it) as the essential mechanisms for these two new phenomena in *Drosophila.*[19]

While sex linkage can properly be attributed to Doncaster and Traynor and recombination to Bateson, they can only be so attributed as observations of new non-Mendelian phenomena. The chromosome theory assimilated both of them but Morgan came to this not by prior expectation but the critical test of using *three* mutants to establish the relationship of these two events to the X-chromosome itself.[20] He was assisted, too, by the appearance of Janssen's chiasmatype theory, which depicted, in Morgan's mind, the kind of segmental exchange between homologous chromosomes which would lead to shifts or crossing-over of maternal and paternal genes in such homologous pairs.[21]

It was these two findings which launched the *Drosophila* group, and Muller acknowledged that they were bombshells even though they were still imprecise as Morgan presented them.[22] His symbolic designation of sex determination was erroneous and Muller pointed out the contradictions, hidden assumptions, and misleading symbolism which Morgan used in working out the white-eye case. Muller gave his views late in 1911 to the Biology Club at Columbia. His unpublished paper "Erroneous Assumptions Regarding Genes" was tactless in its outspoken criticism of Morgan, especially in the indirect jibe at Morgan's position on *a*

18. Vermilion eyes with white eyes on the same chromosome would be white-eyed. Thus a crossover class (w v) could not be distinguished from a non-crossover class (w v^+). The white-miniature cross, however, would clearly distinguish the non-crossovers, white (w m^+) and miniature (w^+ m), from the crossovers, white-miniature (w m) and normal (w^+ m^+).

19. T.H. Morgan, "The Application of the Conception of pure Lines to Sex-Limited Inheritance and to Sexual Dimorphism," *Amer. Nat., 45* (1911), 65-78; presented 29 Dec. 1910. See also T.H. Morgan and C.B. Bridges, *Sex-Linked Inheritance in Drosophila,* Carnegie Institution Publication no. 237 (Washington, D.C., 1916).

20. Carlson, *The Gene*, p. 45, and Sturtevant, *History of Genetics,* p. 43.

21. T.H. Morgan, "Random Segregation versus Coupling in Mendelian Inheritance," *Science, 34* (1911), 384; F.A. Janssens, "La théorie de la chiasmatypie," *La Cellule, 25* (1909), 389.

22. H.J. Muller, Dr. Calvin B. Bridges, *Nature, 143* (1939), 191-192.

priori reasoning: "where we discard it we generally make the worse mistake of unconsciously making baseless *a priori* assumptions." [23]

Morgan also recognized that the distance between genes is related to the frequency of crossovers. But it was Sturtevant who conceived of a quantitative method, mapping, as the best use of this relation. He was nineteen and still an undergraduate when he took Morgan's data home and worked late into the night constructing his first map in the winter of 1911. [24] Surtevant was Morgan's favorite student. He was impressed, the year before, when this sophomore gave him an unsolicited paper on coat color inheritance in horses. He gave him desk space in his laboratory, 613 Schermerhorn Hall – Morgan's desk was in the middle of the room, but he usually worked standing up and examined his flies with a jeweler's loup. Shortly after Sturtevant was added, Morgan looked for a bottle washer and hired Bridges, who, like Sturtevant, had taken Morgan's undergraduate course in zoology (a one-time event for Morgan) and who was in financial straits. [25]

Bridges had unusually keen eyesight for spotting mutants. Sturtevant, who was color-blind, was not good at adding to the store of new mutants. While preparing to wash a discarded bottle, Bridges spotted a bright orange-eyed fly (with his naked eye!). This was the vermilion mutant (November 1910) and very quickly Morgan recognized Bridges' value to the *Drosophila* work. [26] Bridges was inventive and handy with laboratory equipment. He introduced the dissecting microscope. He phased out Morgan's laborious procedure for isolating flies through consecutive empty bottles and substituted etherization with etherizers. He standardized the food media and he constructed constant-temperature incubators. [27]

Muller had received his B.A. in June 1910, just at the time Morgan was completing his analysis of the sex-linked ratios of his white-eye crosses. He had been much impressed by Wilson and was fully committed to the chromosome theory and the Mendelian basis of all character traits and vital functions, a view which he presented, with eugenic implications, to the Peithologian Society that year under the title

23. H.J. Muller, "Erroneous Assumptions Regarding Genes" (unpublished address to the Biology Club, Columbia University, 1911), Muller Archives.
24. Sturtevant interview, 1967.
25. Ibid.
26. Ibid.
27. T.H. Morgan, "Personal Recollections of Calvin B. Bridges," Millikan Library; H.J. Muller, "Thomas Hunt Morgan," *Science, 103* (1946), 550-551.

"Revelations of Biology and Their Significance."[28] He spent a year at Cornell Medical School studying physiology, and in 1911 transferred to the physiology department at Columbia, where he studied nerve pulse transmission for a master's degree. During these two years he kept in contact with Bridges and Sturtevant, usually stopping by on Thursday, his one free day in a busy week which included duties as a teaching assistant, course work (including Morgan's courses in embryology and in experimental zoology), tutoring English to foreigners at night school, and working as a hotel clerk or bank runner.[29] He occasionally stopped by Morgan's house (a half block from Schermerhorn Hall) for the weekly science readings which Morgan enjoyed with cheese, beer, or, for special celebrations, squid Neopolitan style prepared by Morgan himself. At one such meeting in 1911, Sturtevant presented his first map and described Muller as jumping with excitement when he heard it.[30]

Sturtevant had calculated his map distances using the ratio of crossover classes to non-crossover classes. This was how Bateson's group reported their coupling and uncoupling ratios. Muller pointed out that the ratio of crossovers to the total of crossover and non-crossover progeny was more effective in calculating distance. Sturtevant also assumed linearity by using the relation $AB + BC = AC$ where ABC were linked in that order. The distances did not work out precisely, even when Sturtevant found classes of double crossovers and added these to determine map distance.[31] Castle had spotted this weakness in the Morgan-Sturtevant model and proposed a three-dimensional representation based on the exact map distances. He rejected the oversimplified and forced attempts to generate linearity when the experimental data clearly contradicted it.[32] Muller argued with Sturtevant about this defect in crossover theory.[33] While he believed Castle was wrong, he felt that the evidence for linearity would come from summing small distances, thus eliminating the effects of the multiple crossovers. Further, he devised the concepts of coincidence and interference to account for

28. H.J. Muller, "Revelations of Biology and Their Significance" (paper presented to the Peithologican Society at Columbia University, 24 March 1910), Muller Archives.

29. H.J. Muller, autobiographical notes prepared for N.I. Vavilov (1936), Muller Archives.

30. Sturtevant interview, 1967.

31. Sturtevant, *History of Genetics*, p. 47; Muller to Hugo Iltis, 11 Jan. 1951, Muller Archives.

32. W.E. Castle, "Is the Arrangement of the Genes in the Chromosome Linear?," *Proc. Nat. Acad. Sci.*, *5* (1919), 25-32.

33. E. Altenburg, personal interview, Nov. 1966, Houston Tex.

the new observations that he and others in the group made that these multiple crossovers did not usually occur in the predicted frequency one would expect from the independent frequencies of crossing-over for two adjoining regions.

Muller's main disappointment in the Group was Morgan's reluctance to accept him with the same enthusiasm he had for Sturtevant and Bridges.[34] Muller worked to exhaustion in outside jobs while going to school. He was financially desperate, yet Sturtevant and Bridges were paid for doing *Drosophila* work. According to Sturtevant, Muller did not have desk space in 613 Schermerhorn and as soon as Altenburg began showing his undeviating loyalty to Muller, he too was placed in exile in the boy's graduate room.[35] Altenburg wanted to do a *Drosophila* project for his Ph.D. thesis but Morgan discouraged him, taking him to a store room with a stagnant aquarium. Morgan dipped his finger in the water, lifted it to a light bulb and, after observing it, told Altenburg "there's a lot of *Daphnia* in here – why don't you work on that?"[36] Altenburg could scarcely control his fury and he switched to the Botany Department where, with Muller's advice as an ex-officio sponsor, he constructed a chromosome map for the linked traits of *Primula*. Muller, of course, did not have his Ph.D. when he supervised Altenburg's research.

34. Ibid.

35. See Sturtevant, "Thomas Hunt Morgan" (1959), p. 295, where he says that while Altenburg was among those who had a desk in the "fly-room," Muller was not. See also Muller, autobiographical notes (1936), p. 5. In a letter to Sturtevant (30 April 1959, Muller Archives), Muller denied the claim that Morgan had not assigned him a desk in room 613. "I did occupy a regular desk in the 'fly-room' for a longer time than I think any of those mentioned on that page [295] occupied one, namely, from September 1912 until September 1915. Moreover, I do not think anyone except Morgan himself, you, and Bridges preceded me in the 'fly-room' unless possibly Eleth Cattell and/or Altenburg did. My desk was in the southwest corner of the room, just south of Bridges' desk. Do you not remember that? I did all my *Drosophila* work there during those three years, although, since I had (for financial reasons) to hold down the job of assistant in the laboratory of the general biology course at the same time, I could not spend nearly as much of my time in the 'fly-room' as you and Bridges did. It was a matter of the deepest regret to me that, in view of my having to earn my way through work in physiology, first at the Columbia and then at the Cornell Medical Schools in 1910-12, I was not able to do extended research on *Drosophila* earlier than the fall of 1912." Sturtevant's inability to remember Muller in the fly-room suggests that most of their working hours on flies did not coincide or that Sturtevant unconsciously ignored Muller when he was at work.

36. Altenburg interview, 1966.

Scientific issues divided the Group as much as the conflict of their personalities. Linear linkage was the first issue to cause trouble. Muller felt that his contributions to the critical tests for the model were absorbed without acknowledgment. In 1912 and 1913 Muller introduced marker genes as means of following chromosomes over numerous generations. He used crossing-over as a tool to identify nonvisible genetic components which he called modifier genes. By this dual method of using marker stocks and genetically "dissecting" the chromosomes for their modifiers he resolved the nonconformable cases, especially beaded wings and truncate wings, both of which seemed to be cases contradictory to Mendelian inheritance.[37] Morgan had not resolved their complexity and had put them aside. Muller insisted so much that complex Mendelian explanations underlie *all* character traits that Morgan regarded him as a zealot.

The depth of this split is seen in many letters exchanged by Muller and Altenburg. When Muller read Sturtevant's obituary of Morgan he commented: "Please read Sturtevant's obituary of Morgan in the records of the Genetics Society of America . . . note his emphasis on the 'facts' that Morgan always saw to it that everyone got full credit and (2 or 3 lines below) that no one knew or cared from whence things originated! How can these two things be true at the same time?"[38]

Altenburg hinted that Muller should write an article on Morgan to set the record straight,

> stating without equivocation and without glossing over, that Morgan definitely was badly confused on practically all fundamental issues in genetics (even on the chromosome theory itself!), and it should be stated just as clearly that he did not arrive at the correct viewpoint himself after awaiting the experimental results, and then convince his "students" along with the rest of the world, but just the opposite was true – that the experimental results pointed to the correct conclusion many years before he accepted those conclusions and that his "students" and Wilson were mainly responsible for setting him straight, often by dint of long and hard argument. It should be made clear, too, that Morgan's contributions to genetics were just three things: (1) proof that one chromosome contained more than one gene, (2) that he pointed out that Janssen's chiasmatype theory explained the

37. Beaded and truncate, unlike white or miniature, which were clear-cut unchanging recessive mutants, varied in size and shape. Furthermore they could not be rendered homozygous because they were lethal in the homozygous condition. Since they were partly dominant as visible wing mutations it was not known that they were also recessive lethals. The combination of not Mendelizing and varying in their character made it seem that they were exceptions to Mendelian genetics.

38. Muller to Altenburg, appended footnote to a letter from R.A. Fisher to Muller, 5 March 1946, Muller Archives.

recombinations between gene pairs in the same chromosome pair, the term "crossing over" merely being another term for "chiasmatypie" and (3) that more crossing-over would be expected over longer distances. It should be made clear, too, that these contributions were all made by 1911. I believe that your own important part in setting Morgan straight ought also to be made clear, since Bridges had nothing whatever to do with this, and Sturtevant very little (in fact he, too, was confused at times, but of course he had a much faster mind than Morgan). Wilson's part was his early and clear advocacy of the chromosome theory, and this too should be emphasized. [39]

Altenburg did praise Morgan for

his good personal traits (such as his democratic association with us, his ability for leadership, his pioneering ("don't use a razor to chop a tree down with") . . . . He undoubtedly had streaks of brilliance and went right to the point. But of course this was not a sustained brilliance; he went from the sublime to the ridiculous, and most of the time he stayed with the ridiculous, as when he thought he could disprove the need for natural selection because of dominance or when he insisted that 1X= ♂ 2X= ♀; or even worse when he attempted to explain multiple factor cases as late as 1910 by lack of segregation and considered them as evidence against Mendelian segregation. [40]

In advising Muller on how to write Morgan's obituary, Altenburg counseled him: "don't get soft all of a sudden; remember that Morgan did you incalculable damage, not only by his steals, but also by keeping you from getting a job." [41]

In a review of the Sturtevant and Beadle text (1940) written to Sturtevant but never sent, Altenburg objected to the historical treat-

39. Altenburg to Muller, 24 March 1946, Muller Archives.

40. Ibid.

41. Altenburg to Muller, 13 Feb. 1946, Muller Archives. Morgan, like Castle, was not at first convinced that a quantitative trait could be interpreted by a Mendelian mechanism in which several component genes are segregated out by independent assortment. Until careful studies of complex quantitative traits were published, many geneticists shared the reservations Castle had about going overboard on Mendelism. Castle interpreted the variation of a quantitative trait as minute fluctuations in the function of the gene itself. The character trait, to many biologists, seemed too complex to be a product of the particulate actions of individual genes. Morgan, approving and quoting Oscar Riddle, cited the weakness of the Mendelian view: "with an eye seeing only *particles*, and a speech only symbolizing them, there is no such thing as the study of process possible ... It has been possible, I think, to show by means of what we know of the genesis of those color characters that the Mendelian description – of color inheritance at least – has strayed very wide of the facts; it has put factors in the germ cell that it is now certainly our privilege to remove; it has declared discontinuity where there is now proved continuity; it has postulated preformation where there is now evident epigenesis." Quoted from p. 509 of T.H. Morgan, "Recent Experiments on the Inheritance of Coat Colors in Mice," *Amer. Nat.*, *43* (1909), 494-510.

ment. Muller and Altenburg maintained that proof of the chromosome theory came from sex linkage and from the correspondence, in size and number, of chromosomes and linkage maps in *Drosophila*. Sturtevant called Bridges' work on nondisjunction "final and convincing proof."[42] It did not seem compelling to Altenburg that abnormal cases should be more convincing than normal ones, although "it is true that some people are more attracted by the abnormal than the normal . . . . However, we cannot be influenced by such people in making an historical appraisal. This must be based on the evidence and on logic – not on the emotional peculiarities of individuals; none of this is to detract from the genius of Bridges." Muller added a comment to this last remark, "I disagree since the non-disjunctional evidence wasn't figured out in advance by Bridges especially that there'd be a Y in ♀; that was my idea."[43]

Sturtevant attributed Muller's falling out with Morgan to a much later date – 1920 – when Muller was an Instructor at Columbia. Sturtevant claimed that Muller wanted a permanent position at Columbia which was denied him. Muller thought Morgan had vetoed him but Sturtevant claimed that it was Wilson himself who felt he would not work out at Columbia.[44]

Morgan's views on natural selection were usually ambivalent. In 1905 he was so impressed by De Vries's mutation theory that he used it as a more satisfactory model of evolution. His confusion about Darwin's theory is revealed, I believe, by this illustrative passage: "the time has come, I think, when we are beginning to see the process of evolution in a new light. Nature makes new species outright. Amongst these new species there will be some that manage to find a place where they may continue to exist. How well they are suited to such places will be shown, in one respect, by the number of individuals they can bring to maturity. Some of the new forms may be well adapted to certain localities, and will flourish there; others may eke out a precarious existence, because they do not find a place to which they are well suited, and cannot better adapt themselves to the conditions under which they live; and there will be others that can find no place at all in which they can develop, and will not be able even to make a start. From this point of view the process of evolution appears in a more kindly light than when we imagine that success is only attained through the destruction of all rivals. The process appears not so much the result of the destruction of vast numbers of individuals, for the poorly adapted will not be able to make even a beginning. Evolution is not a war of all against all, but it is largely a creation of new types for the unoccupied, or poorly occupied places in nature." Quotation from p. 63 of T.H. Morgan, "The Origin of Species Through Selection Contrasted with Their Origin Through the Appearance of Definite Variations," *Popular Science Monthly, 47* (1905), 54-65.

42. Sturtevant, *History of Genetics,* p. 48, and Sturtevant interview, 1967.

43. Altenburg to Sturtevant, 19 June 1940 (letter never sent to Sturtevant), with appended 1966 notes by Muller, Muller Archives.

44. Sturtevant interview, 1967.

This seems an unlikely basis for the split, even if it were true that Wilson blocked Muller, because both Muller and Altenburg constantly referred to the events from 1911 on as the basis for the break-up, particularly the fights over linear linkage, multiple factors, sex determination, and, later on, chromosome abberrations.[45] In all these instances Muller adopted views which turned out to be correct. That he did so in a tactless or heavy-handed fashion we may infer from the frequent references to laboratory arguments, not discussions, in forcing these views on Morgan and Sturtevant. Aggravating the situation was Morgan's habit of assimilating his students' ideas, frequently without giving them individual credit for their contributions. It should be kept in mind, however, that Morgan widely publicized their viewpoint and made the work of the *Drosophila* group acceptable faster than they could have done on their own. He also fostered a research atmosphere and respect for critical experiments which all of his students accepted axiomatically.[46]

Because Morgan discarded most of his correspondence, few items exist expressing his feelings about Muller. One surviving document does reveal the underlying distrust. In 1934, Morgan received word from an editor that Painter had protested the early publication of an article by Bridges on salivary chromosomes. Painter thought Bridges had indirectly learned of his work and used Morgan's influence to rush his own work into print before Painter. Morgan's analysis is very diplomatic:

> Painter is probably not quite so violent as he appears to be in his letter. He must have lost his head completely, and I think it might do no harm to let him know that that sort of thing does not go well with other scientists. Perhaps this may clear the skies somewhat and open up more friendly relations between our laboratories which is highly desirable and which I thought really existed until this outburst of Painter's – which confidentially I think goes back to Muller's influence on the group in Austin. Muller's attitude has always been antagonistic to us, although he has generally managed to keep this under cover and we have consistently ignored it, treating him in the most friendly way, because we regarded his attitude as wrong and inexcusable.
>
> I hope Muller will not transfer his attitude to the Russian group of geneticists.[47]

Morgan is not altogether sincere when he claims that Texas and Cal

45. See Muller to Altenburg, 5 March 1946, Muller Archives.
46. H.J. Muller, "Thomas Hunt Morgan," *Science, 103* (1946), 550-551.
47. Morgan to Jones, 1 Nov. 1934, Millikan Library.

Tech (or Columbia University before Morgan's move to Cal Tech in 1928) had friendly relations before Painter's outrage over Bridges' integrity. Throughout the 1920's the Texas group considered itself a rival of Morgan's group in genetics and boasted of its occasional "homeruns," such as Muller's radiation mutagenesis and Painter's discovery of salivary chromosomes. It is also difficult to see why Morgan was oblivious to Muller's frustrations, even if they were exaggerated.

Muller's attitude to Morgan can best be described as ambivalent. His finest tribute was made in 1921 in a speech given at Cold Spring Harbor.

> We should not forget the guiding personality of Morgan who infected all the others by his own example – his indefatigable activity, his deliberation, his jolliness and courage. His simplicity and dazzling liveliness of character, his flashing wit together with astuteness in detecting the most vital aspects of work (in the field of Mendelism and mutation) at a time when many other biologists were ending up in blind alleys – these are the qualities which attracted to Morgan and his work a group of young people, interested in new problems. He provided the initial impetus which to a significant degree stimulated further work. [48]

The dark side of the relation is clearly indicated in Muller's discussion of the multiple factor theory. In advising Altenburg on writing Morgan's obituary, he told him to play down the linear linkage controversy and emphasize the multiple factor issue.

> It has too little been realized in general how important the proper development of the multiple factor theory was, first, in making it possible to show that the inheritance of all variations found at that time in Drosophila (and by implication elsewhere) was in fact stable and Mendelian, chromosomal, and not subject to contamination (vis; the truncate, Beaded, and dichaete cases); and secondly, that the multiple factor case thus revealed converted the "mutation theory" contrary to the ideas and wishes of its early proponents into the basis for essentially Darwinian selection of "continuous variations" after all. Of course a few people, such as Lock, had realized this to some extent at least, especially after Johannsen's work, but even such people as Tine Tammes, and G. H. Shull still thought multiple factors as confined to a relatively few definite genes (e.g. duplicate genes) and certainly Morgan, like Castle, was all off on this subject, which was a keystone to the whole of the interrelations of genetics with evolution, physiology, etc. To see what I mean, for instance, just look up Morgan's suggestions about Cuenot's yellow mouse work in Science in 1905. After I wrote up multiple factors and gene-character relations, first in my criticism of Castle's work in 1914 and second, eighth, and ninth chapters of the 1915 edition of the *Mechanism of Mendelian Heredity*, and after you and I had both rubbed the thing in numerous

48. H.J. Muller, "A Decade of Progress in *Drosophila*" (1921), translated from the Russian by Joel Wilkinson, Muller Archives. Published in Russian in *Uspehi Eksperimentaljnoi Biologii, 1* (1922), 292-322.

> discussions among the Drosophila Group, Morgan began to see the light, and in 1916 in his *Critique of the Theory of Evolution* he gives me chief credit, after Johannsen, for working this out, on his pages 166–169, so I can here "quote chapter and verse" for his recognition of it. I could also quote chapter and verse for his consciously deciding to withhold the credit from me by calling attention to the second edition of the same book, which he got out under the title of *Evolution and Genetics* in 1925 (same publisher, Princeton University Press) for here, though he again quotes Johannsen, he omits his account of the method of markers and the truncate case, together of course with my name. He does not however, reduce his references to *Drosophila* as far as I can see.[49]

As a postcript to the letter Muller mentions a strange incident:

> I inadvertently forgot to say, as an illustration of Morgan's lack of understanding of multiple factors, that he thought the case of black and yellow in Drosophila so strange that he counted something like two hundred thousand flies from the cross between the two mutants. I don't know whether he expected to find "fractionation" or directed mutation or what, but of course everything came out according to Hoyle or rather Mendel.[50]

The deterioration of relations continued after Muller left Columbia. Muller maintained his independence and focused his work on the individual gene, culminating these studies with the artificial induction of mutations. As Muller's contributions grew so did his feelings that his ideas were freely pirated by his colleagues. This was the basis of his rupture with Patterson and Painter, whom he had originally converted to *Drosophila* work after his appointment at Texas.[51] Muller was also

49. Muller to Altenburg, 5 March 1946, Muller Archives.

50. Ibid. Perhaps Morgan thought that the normal body color was more complex than the sum of its non-mutant genetic components. The combined yellow and black body color in the recombinant yellow-black is not the same body color as the gray amber of normal flies and it is somewhat sooty and patchy in appearance. He may have hoped to induce the flies to reveal a fluctuating relation of these components by using the double mutants with the wild type in heterozygous association on the assumption that the normal gray amber would be unstable under such a condition. If he could fractionate such characters, as Castle, for example, had claimed, then he would find blacks with yellow flecks or yellows with black flecks. Instead each gene was reextracted without any contamination from the association, as either a uniform black or yellow.

Morgan was also somewhat dubious of statistics and felt that his contemporaries would be impressed by an experiment if the results were overwhelming. In illustrating a sex-linked cross where there was "not a single brown female in 11,000 grandchildren," Morgan added his commentary that "such results cannot fail, I think, to impress those who take a sceptical attitude toward the modern study of heredity." See p. 27 in T.H. Morgan, "Heredity of Body Color in *Drosophila,*" *J. Exp. Zool.*, *13* (1912), 27-45.

51. Muller to Altenburg, undated Tuesday, probably in Feb. 1930, Muller Archives. The letter begins: "Please let me know by return mail" and ends (footnote) "compensate for it."

enraged at Bridges for what he believed to be theft of his ideas on gene evolution through duplication and the proof that bar-eye was an instance of duplication of a chromosome segment.[52]

In these priority conflicts we witness Muller's least appreciated and most criticized personality trait. In some instances he was correct, but he made too much of an issue by protesting in private and in print. It cost him dearly and made it difficult for him to obtain jobs and to disseminate his own ideas.[53] If this is acknowledged to be Muller's major failing, it should also be accepted that Morgan's role in the *Drosophila* group has been romanticized and overinflated, and that Sturtevant's interpretation is severely biased by the favored position he held throughout the long association he had with Morgan.

For the historian, scrutiny of the *Drosophila* group is valuable as a study of the conflict of ideas and personality clashes which led to one of the most significant biological concepts of our times – the theory of the gene.

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52. Muller to J. Alexander, 27 June 1946, Muller Archives. See also H.J. Muller, "Bar Duplication," *Science, 83* (1936), 528-530.
53. L.C. Dunn to Muller, 16 Nov. 1936, Muller Archives.