

# Perspectives

## Anecdotal, Historical and Critical Commentaries on Genetics

*Edited by James F. Crow and William F. Dove*

### **The Other Fly Room: J. T. Patterson and Texas Genetics**

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**T**O geneticists, “The Fly Room” means the *Drosophila* lab at Columbia University in New York. Here T. H. Morgan and his brilliant students, C. B. Bridges, A. H. Sturtevant, and H. J. Muller worked together in a single room and laid the foundation for the magnificent edifice that is *Drosophila* genetics. The room was small (16 by 23 feet) and it was very crowded, for often there were other students and visiting researchers. But the close confinement brought a benefit. As STURTEVANT (1965, p. 49) described it, “There was an atmosphere of excitement in the laboratory, and a great deal of discussion and argument about each new result as the work rapidly developed. . . . There was a give-and-take atmosphere in the fly room. As each new result came along, the group discussed it freely. The published accounts do not always indicate the sources of the ideas. It was often not only impossible to say, but it was felt to be unimportant, who first had an idea.” The room was used continuously from Morgan’s early days until the group moved to CalTech in 1928.

#### H. J. MULLER’S TEXAS YEARS

Meanwhile, Muller was recruited in 1920 by J. T. Patterson to join the Zoology Department at the University of Texas. (Apparently, a position was offered to Sewall Wright, for he often spoke in his characteristic self-deprecating way of the great favor he did for Texas—by not accepting the offer, he opened the way for a future Nobel Prize winner.) Patterson was able to arrange for a fly room for Muller in 1925. It was intended as a mimic of the original Morgan model in being a single room in which all worked together and results were discussed freely. It was larger (72 by 24 feet) than its Columbia prototype, with four long desks providing a total of 16 work stations. It was kept at 21° all year round, necessary for raising *Drosophila* in the long, hot Texas summer.

(It was also a popular place for human habitation during the summer months.) Attached was a room for preparing media and washing glassware.

Patterson immediately proceeded to obtain financial help from outside sources and succeeded in obtaining a generous grant from the Rockefeller Foundation, matched by University funds, to support the fly lab. This support continued for the next 30 years. At first the fly room was used only by Muller and his students and visitors. But soon the great success of Muller’s work attracted his colleagues. Patterson, an embryologist, joined the *Drosophila* group as did the cytologist, Painter, who soon made an enormous contribution by exploiting the giant salivary gland chromosomes. Muller’s students, among them C. P. Oliver, Bentley Glass, and Wilson Stone, added to the intellectual liveliness.

Muller made good use of the fly room. He was able to continue on a much larger scale his various studies, especially mutation, which started during his student days at Columbia. His establishing radiation-mutagenesis (MULLER 1927) brought him instant fame and, 2 decades later, the Nobel Prize. He devised the *ClB* protocol that permitted the quantitative recovery of X-linked lethals (MULLER 1928). In a very short time he had observed more mutations than the total that had been found up to that time. His fame attracted visitors. One was George Snell, later to win the Nobel Prize for his work on the genetics of histocompatibility in the mouse. Muller also had Russian guests. Two of them, I. J. Agol and S. G. Levit, were among the first to be “liquidated” during the Stalin terror. For a personalized account of Muller and his work, see CROW and ABRAHAMSON (1997).

#### J. T. PATTERSON AS LEADER

The driving force in Texas genetics was J. T. Patterson. Originally trained as an embryologist with C. O. Whitman at Chicago, he joined the Texas faculty in 1908. On arriving in Texas, he spent most of his research efforts studying polyembryony, first in the armadillo,

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J. T. Patterson, 1878–1960.

and then in various parasitic wasps (for a review, see PATTERSON 1927). The armadillo article (PATTERSON 1913) was a classic and was widely read. According to Patterson, the cost of reprints made a large dent in his year's income—in those years authors paid for reprints out of their own pockets. The most striking example was *Paracopidosomopsis floridanus*, which produced as many as 2000 embryos from a single egg (PATTERSON 1921). (Every Texas Zoology graduate student was familiar with *Paracopidosomopsis* and for most this was the longest word in the English language. It did not, however, equal “antidisestablishmentarianism.”)

At the same time, Patterson became increasingly interested in the activities in the field of genetics. He maintained a summer residence at Woods Hole and was closely acquainted with the *Drosophila* activities of T. H. Morgan and his students. It was here that he became greatly impressed with Muller and early on vowed to try to get him to Texas. With this background it was an easy transition for him to work in the fly room. With

Muller, he demonstrated that some mutations induced by X radiation could be reversed—implying that mutations could be “progressive” (PATTERSON and MULLER 1930). This had an important influence at the time, for in the early days it was thought that mutations were really the result of some sort of loss of the gene rather than a change in its structure. Actually, subsequent research has shown that, contrary to Muller's belief, and as Stadler thought (ROMAN 1988), the great bulk of radiation-induced mutations are indeed chromosomal. PATTERSON (1929) also did extensive studies of radiation-induced somatic mutations, and he studied the formation of gynandromorphs and other mosaics, using mutant markers as tracers of embryonic lineages (PATTERSON 1931).

Meanwhile, T. S. Painter also began to participate in the *Drosophila* group by joining Muller in initiating cytological observations on the chromosome changes produced by radiation, permitting the comparison of physical distances in metaphase chromosomes with linkage map distances (PAINTER and MULLER 1929). For this work Painter was well equipped. After receiving his graduate degree at Yale in 1913 studying cytology with Alexander Petrunkevitch, he spent a year on a postdoctoral fellowship with the eminent cell biologist, Theodore Boveri, at the University of Würzburg. With this experience he developed a broad background in cell biology as well as becoming informed on the use of cytological techniques suitable for *Drosophila*. Difficulties in analysis of metaphase chromosomes led Painter to look elsewhere. BALBIANI (1881) had found that the Dipteran, *Chironomus*, had large banded chromosomes in its salivary gland nuclei. This was later found to be true for Diptera in general. Painter pursued the matter with *D. melanogaster* and showed that a very close correspondence in sequence existed between the genetic map and the cytologically visible bands, permitting precise location of mapped genes among the bands (PAINTER 1933).

Muller left the fly room and the University of Texas in 1932, never to return. In the brief 5-year period between 1927 and 1932 the laboratory had become world famous, precipitated by Muller's discovery of radiation mutagenesis. In the ensuing years Patterson kept the fly room activity going full steam with a group of able graduate students. Especially noteworthy were three of these, Sarah Bedichek, Meta Suche, and Wilson Stone. Sarah Bedichek left to work with Haldane, but Meta Suche remained at Texas for several years. Stone gradually emerged as the intellectual force behind the various experiments and, as the deepest thinker, became increasingly important over the years.

After Muller left, the group continued work on some problems originally suggested by him. One of the best known studies was on production and analysis of radiation-induced translocations (PATTERSON *et al.* 1934). Patterson combined different X-IV translocations to



W. S. Stone, 1907–1968.

produce males hyperploid for small regions of the X or females hypoploid for the same regions. The object was to see whether there was a single sex-determining factor or whether the determination was polygenic. None of the small duplications or deficiencies throughout the X chromosome reversed the sex, but a small region near *garnet* and *pleated* (13A2–13A6 in the salivary map) was inconclusive, since the aneuploids were always lethal. PATTERSON (1938) clearly hoped to find a single gene that produced females when disomic. In his article he said, “The negative evidence might be regarded as indicating the presence of a major sex gene for this region” (p. 206). But, fortunately, he was cautious. The following sentence read: “But such evidence is not critical because hyperploid males for certain other regions were practically inviable” (p. 206). Viable hyperploids for this region were eventually obtained (CROW 1946) and were male, although by this time the answer was also clear from other evidence. Patterson’s hope for a single sex factor evaporated.

In the mid-thirties Patterson began to tire of *Drosophila* genetics, the pursuit of which involved tedious shuffling of countless numbers of etherized flies under the microscope, and in effect went on vacation from the fly room, leaving graduate students and postdocs to carry on. Being Patterson, he did not retire, but merely

changed focus. He started by going back to an old love—the collecting of Indian artifacts, which abounded in central Texas. During the next 2 years he published three articles on corner-tang flint artifacts.

Then came a rejuvenation of his interest in *Drosophila*, this time in speciation. Sturtevant and Dobzhansky had earlier made forays into this field with *D. pseudoobscura*, but Patterson, recognizing that the salivary gland chromosomes made it possible to correlate chromosomal changes with species differences, undertook the ambitious, grand scheme of collecting new species hitherto unknown and analyzing them along with known species for chromosomal differences. The fly room became immediately converted from a *melanogaster* room to a *Drosophila* species room. Graduate students went out into the field, frequently with Patterson himself, now in his sixties, to collect *Drosophila* locally.

The result was the most extensive study of *Drosophila* species ever attempted. In 1941 Patterson purchased a Dodge panel truck that was used to collect flies throughout the contiguous United States and much of Mexico. The early collections were made by Gordon Mainland and one of us (R.P.W.), who broke a leg during a collecting trip and was replaced by a new graduate student, Marshall Wheeler. Wheeler was soon the mainstay of the taxonomic work and remained at Texas for the remainder of his career. He became the successor to A. H. Sturtevant as the leading *Drosophila* taxonomist (e.g., WHEELER 1981).

In the late 1930s Patterson was frustrated by his inability to publish data *in extenso* in standard journals and started publication through the University of Texas. The Texas bulletins soon became *de rigueur* for workers in *Drosophila* speciation. A series of 9 volumes was published between 1940 and 1957. Then a new series was started, the first one in 1959 dedicated to Patterson on his eightieth birthday. In addition to several articles in the first series, he co-authored a book (PATTERSON and STONE 1952).

During this period Stone provided the ideas and the depth of insight while Patterson provided the driving energy. Stone was the brain and Patterson the backbone of this highly productive group. Stone went on to become a leader, not only in the Zoology Department, but in the University System as a whole. He was a reluctant administrator, but he accepted responsibility and did a good job. For example, he was an important contributor to establishing the M. D. Anderson Hospital Research Center in Houston. He was also active nationally in various ways such as serving on the NIH Genetics Study Section (see CROW and OWEN 2000).

In 1951 the genetics group at Texas moved to a new building. Each person now had a separate laboratory and the “fly room” disappeared. Patterson was never quite the same, and his research zeal quickly diminished. He died in 1960 at the age of 82.

Patterson’s early work on polyembryonic hymenop-





A view of the truck used to collect *Drosophila* throughout the United States and much of Mexico.

tera was almost forgotten for 75 years. But there is now renewed interest, thanks in large part to technical advances (STRAND and GRBIC 1997; GRBIC *et al.* 1998). *Paracopidosomopsis floridanus* has been rechristened *Copodisoma floridanum*. As an extreme example of large evolutionary changes in development with very little in adult morphology, it is of special interest to those in the field of evo-devo (RAFF 2000).

#### PATTERSON AS A PERSON

We were both graduate students of J. T. Patterson at a time when the *Drosophila* species studies were getting under way. Here we add a few memories that round out the image of this colorful personality.

**Memories of a graduate student (R.P.W.):** Patterson often referred to himself as a pot-bellied Irishman. He was sort of pear-shaped, but definitely not a couch potato. When I first knew him in 1940, he was in his early sixties and as full of energy as a 20-year-old. He generally arrived at the Lab around 3:30 or 4:00 AM, went home at 12 noon for lunch and a nap, returned at 2 PM, and worked until about 5:30. On Saturday morning he took Mrs. Patterson to do the week's shopping at the markets so he did not arrive until noon. He arrived late on Sundays, about 9 AM, and spent most of the day writing letters. This regimen was maintained every week without regard to holidays, which he totally ignored. Mrs. Patterson once told me that he had really slowed down, because he used to return every day after dinner to work until about 10 PM.

He enjoyed working with graduate students and spent a great deal of time with them. But he never interfered with their work. His motto was "sink or swim." You had to have your own program and be inner directed, or

you sank. At first, *Drosophila* species collecting was done locally from garbage cans at state parks starting at about 4 AM. You had to be ready when he came by for you with his car at your digs.

When I was in my third year of graduate work, World War II was on. He told me to finish up before I was drafted. I told him I had not done enough to write a proper dissertation for a Ph.D. He insisted, so I wrote up what I had and gave it to him. After reading it, he came back, saying, "I didn't know you were working on this; write it up properly for submission to your committee." About a year after I got my degree I finished the work and wrote a fairly decent paper.

**More memories (J.F.C.):** I entered graduate school at the University of Texas as Patterson's student in the fall of 1937. His first remark was: "You are blonder and skinnier than I thought you would be." He then proceeded to tell me about his arrowhead collection. It quickly became apparent that the gruff manner hid a much kinder inner person. I also quickly learned that everyone called him Dr. Pat.

The *Drosophila* lab was notable for three things: (1) cool temperature, most welcome in the Texas summer; (2) ether fumes; and (3) smoke from Dr. Pat's pipe. I don't know how close the ether was to a combustible concentration, but any such fear certainly didn't inhibit Dr. Pat's continually lighting and relighting his smoldering pipe.

Patterson's memory was remarkable. He would remember minute details, such as numbers in a table. Only once did I challenge his memory. It had to do with the number of *Drosophila mulleri* trapped on a particular day. I had been there and thought I knew, but he was correct.

Finally, Dr. Pat had a taste for colorful vulgarity; in

the vocabulary of the time, his talk was “earthy.” I was greatly honored to be invited to give the dedicatory address at a new Texas building named after him. I was visiting CalTech at the time and asked Sturtevant if he had any anecdotes about Patterson that I could use. He said that he knew dozens, not one of which was suitable for a dedicatory address.

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