Journal of the History of Biology **33:** 113–139, 2000.

Sex Cells: Gender and the Language of Bacterial Genetics

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Abstract. Between 1946 and 1960, a new phenomenon emerged in the field of bacteriology. "Bacterial sex," as it was called, revolutionized the study of genetics, largely by making available a whole new class of cheap, fast-growing, and easily manipulated organisms. But what was "bacterial sex?" How could single-celled organisms have "sex" or even be sexually differentiated? The technical language used in the scientific press – the public and inalienable face of 20th century science – to describe this apparently neuter organism was explicit: the cells "copulated," had "intimate contract," "conjugal unions," and engaged in "ménage à trois" relationships. And yet, to describe bacteria as sexually reproducing organisms, the definition of sex itself had to change. Despite manifold contradictions and the availability of alternative language, the notion of sexually active (even promiscuous) single-celled organisms has persisted, even into contemporary textbooks on cell biology and genetics. In this paper I examine the ways in which bacteria were brought into the genetic fold, sexualized, and given gender; I also consider the issues underlying the durability of "bacterial sex."

Keywords: bacteria, bacteriology, conjugation, gender, genetics, language, sex

In the middle years of the twentieth century, from 1946 to 1960, the fields of bacteriology and classical genetics converged on a phenomenon which came to be described as "bacterial sex." The manner in which biologists subsequently exploited this discovery vividly illustrates the interplay of cultural and scientific forces which were far from objective and neutral. The object of scrutiny was a strain of bacteria called *Escherichia coli* K-12 (*E. coli* K-12); *E. coli* K-12 reproduced by fission – in other words, asexually. In terms of classical genetics, which depended almost exclusively on the technique of experimental breeding, *E. coli* (like all its bacterial cousins) was therefore irrelevant. Moreover, bacteria lack the physical structures which had by that point come to signify "genetics" at the microscopic level – a distinct nucleus and visible chromosomes. So not only was *E. coli* technically unavailable for genetic study, it was by definition unlike known genetic organisms. Nonetheless, biologists in the 50s and 60s described this particular bacterial strain

as having two "sexes" that "copulated." In the process, *E. coli* K-12 became gendered: anthropomorphic sexual characteristics and behavior were regularly attributed to the strain. This gendering was not simply a rhetorical device or a purely linguistic construction. The design of the exploratory experiments were themselves shaped by assumptions of sex and gender; unsurprisingly, these experiments then produced results which inevitably validated those assumptions. Thus, a scientific understanding of *E. coli* was constructed, both physically and linguistically, to explicitly gendered specifications.

In part, the process of sexualizing these single-celled organisms was motivated by disciplinary and experimental demands and ambitions. However, even as these demands changed or disappeared, the model of *E. coli* K-12 as a sexually differentiated, sexually reproducing organism was maintained; moreover, it was maintained despite the existence of simpler and more precise heuristic language. How and why was this metaphor constructed and adopted, and what made it so durable?

Conjugation: Mating Genetics and Bacteriology

> *The genetic definition of a gene implies sexual reproduction. It is only through the segregation and recombination of genes during meiosis and fusion of gametes that the gene exhibits its unitary property. In bacteria, for example, in which cell reproduction is vegetative, there are presumably units functionally homologous with the genes of higher organisms, but there is no means by which these can be identified by the techniques of classical genetics.* (Beadle, 1945)[1]

In 1946, the field of molecular genetics was only just emerging from its biochemical foundations; the broad and pervasive influence which it would have on all fields of biological endeavor was not yet apparent. The term "genetics" still meant classical Mendelian genetics, which used the techniques of plant and animal breeding to derive genetic information. Indeed, the existence of genes as more than theoretical entities had only recently been confirmed.[2]

[1] G. W. Beadle, with E. L. Tatum, performed the experiments underpinning one of the tenets of molecular genetics: that genes encode specific enzymes.

[2] Between 1904 and 1915, T. H. Morgan et al produced a body of evidence strongly suggesting that the "hereditary particles" or "genes" were located on the chromosomes. After Painter's 1933 discovery of the *Drosophila*'s giant salivary gland chromosomes, the link between genes and chromosomes, and between chromosomal and phenotypic change was confirmed. This proof solidified the claims of genetics upon the physical animal, and foreshadowed the rise of a chemical theory of genetics. See Brock, 1990, pp. 12–13 and Allen, 1975, pp. 41–72.

The hypothesis that DNA was the physical substrate of genetic information – the confirmation of which was to catalyze the rise of molecular biology as a discipline – had produced its first experimental success just two years previously, in 1944.[3] Bacteria were generally excluded from genetic studies and inferences, eliminated on the grounds that sexual reproduction was the only reliable (and readily analyzed) technique for producing genetic reassortment. In their simplest form, the experiments underpinning all of classical genetics involved the mating of two sexually reproducing organisms, each marked with some visible heritable trait. By the very nature of sexual reproduction, recombination of the genetic material would take place, and the progeny of the cross would be marked by some of their parents' visible attributes. Geneticists could collect data about the genes implicated, their locations, and the cellular actions resulting from their expression by calculating the distribution of each trait. In the absence of other mechanisms for producing genetic mixing, sexual reproduction itself became central to (and was often conflated with) the project and process of classical genetics, even as its techniques expanded to include cytogenetics in the 1920s and 1930s.[4] Since bacteria were known to reproduce asexually by simple division (mitosis), bacteriology was considered as a separate and incompatible area of study, interesting solely for its medical applications, if at all. In fact, some scientists argued that bacteria entirely lacked a genetic system, since they lacked the separate nucleus and visible chromosomes of the usual genetic organisms, as well as a sexual mechanism.[5] Thus, the study of bacteria was largely the province of medical scientists and biochemists, while genetic experimentation was constrained by the cumbersome and slow reproductive cycles of multicellular organisms. By no means was bacteriology a peripheral science; however, like developmental biology, it was a separate one from genetics. In neither case is that separation so distinct today.

The situation was to change in 1946, with the work of Joshua Lederberg, a 21-year-old medical student on a research internship, and E.L. Tatum, an established bacteriologist and biochemical geneticist. Lederberg and Tatum discovered that genetic traits could be transferred between cell lines in *Escherichia coli* K-12, a bacterial strain which reproduces by mitosis and which is constitutively haploid.[6] The scientific literature surrounding this

[3] See the Avery, McCarty, Macleod experiment on the effect of purified DNA on cells in culture. Their conclusion, that DNA was the carrier of genetic information, was still a matter of heated debate.

[4] See Kay, 1993, pp. 122–125.

[5] See Hayes, 1968, pp. 179–180, 546. See also Huxley, 1942, pp. 131–132.

[6] That is, in the mature normal cell, there exists only one copy of the bacteria's genetic information. Humans and higher animals are diploid, possessing copies of each of their parents' genomes.

discovery implies that it grew spontaneously and inevitably from the development of new bacterial mutants – results in part of wartime work with penicillin and chemical mutagens – combined with the long-delayed incorporation into theory of Blakeslee's 1904 description of "sexual reproduction" in the fungus *Neurospora crassa*.[7] In light of this strategic deployment of inevitability and "naturalness," it is revealing to read Joshua Lederberg's own description of the conscious intent with which he set out to discover whether bacteria exchanged genetic material:

> The most important incitement [to begin the *E. coli* experiments] was probably that Avery, McCarty and MacLeod on February 1, 1994 published their well-known paper . . . I have a very vivid recollection of the encounter with that paper, and the problem that it then presented about how to merge microbiology, genetics and molecular chemistry . . . So I thought . . . "Well, bacteria have been transformed; maybe they would be the right material to do transformation experiments and study the role of DNA, but let's see if they have a genetics." For genetics, they have to be crossed. . . . The fact was that there had never been any critical test for recombination and so [our experiment] was designed for it.[8]

In Lederberg's initial letter to Tatum in 1945, he wrote suggesting that it would be "advantageous to use stocks of heterogeneous origin, in the event that there exist mating types, sterility factors, etc."[9] His choice of terms – "mating types," "sterility factors" – suggests that from its inception, the proposed project was a search for bacterial recombination through a sexual mechanism.[10] Lederberg has subsequently described this venture as "looking for bacterial sex," and even in that first 1945 letter, he characterized it in the language of sex: "no adequate investigation of a genetic nature has been made to demonstrate the existence or absence of sexual recombination in bacteria." The conflation of sex and genetics – or more precisely of sex, sexual reproduction, and genetic recombination – is persistent, and deserves further investigation. After all, sex – and sex as it occurred in higher plants and animals, as a part of reproduction – had been the only known mechanism by

[7] Lederberg, 1987, pp. 28–29. See Brock, 1990, p. 75; and Blakeslee, 1904, pp. 205–319.

[8] J. Lederberg, interview by author, 1993. Lederberg's correspondence (e.g. his letter to Tatum of 19 September 1945, available from the National Library of Medicine's Profiles in Science site at http://www.profiles.nml.nih.gov, accessed and printed 9 April 1999) at the time confirms his recollection of deliberately searching for bacterial sex.

[9] A digital image of the original letter is available from the National Library of Medicine's archive site at http://www.profiles.nml.nih.gov (accessed 9 April 1999); another version can be found in Brock, 1990, p. 81.

[10] J. Lederberg, interview, 1993. See also his letter to Tatum, 19 September 1945 as published in Lederberg, 1987; and alternative version as cited in Brock, 1990, p. 81.

which genetic information could be transferred or reassorted since the field of heredity emerged as a scientific focal point in the second half of the nineteenth century.[11]Given this extremely close association, Lederberg's search for a specifically sexual genetics in bacteria, and his corresponding early interpretations of genetic recombination in *E. coli* as having a "sexual mechanism" could be seen as an almost inevitable elision of classical genetic theory and the new evidence of recombination. However, Lederberg also makes it very clear that his goal was to discover whether bacteria could be used in genetic study, especially since evidence of genetic recombination had been found in other micro-organisms: "These successes only dramatized the importance of finding a sexual stage, if it existed, in a variety of microbes. If bacteria could be crossed, a new repertoire of biological materials for experimental analysis would be available to physiological genetics and biochemistry . . . The compelling motive was to allow the exploitation of DNA transformation in an organism with manifest genomic structure, to further the launching of what is today called "molecular genetics."[12] Lederberg's use of the language of sex and conjugation, as well as the existing genetic terminology, was motivated by a need to recreate *E. coli* as a genetic organism, similar to other higher organisms, a vital step in the campaign to link bacteriology and genetics.[13]

Lederberg and Tatum designed their first bacterial experiment using two strains of *E. coli*, each of which required a different pair of nutritional supplements to grow in culture. In essence, this work reproduced a standard eukaryotic breeding experiment, but with prokaryotic organisms. They mixed the two strains together in a medium containing all four supplemental chemicals, and allowed them to divide and multiply freely. Lederberg and Tatum then spread the mixture of bacteria onto plates of unsupplemented medium, and discovered that colonies of bacteria still appeared. Since neither of the pure "parent" strains could grow on these plates, and since all of the mixed bacterial cells had been washed – to free them of any nutrients or chemicals secreted by the other cell populations – it seemed clear that genetic exchange had occurred between the two types of bacteria. Some small proportion of the offspring had biochemical capabilities derived from *both* of the *E. coli* strains, rather than the single pair of traits which would have been passed on by simple division.

Lederberg and Tatum presented their initial discovery to the Cold Spring Harbour Meeting in late 1946. This annual meeting was attended by many

[11] See Allen, 1975, pp. 41–72.

[12] J. Lederberg, 1987, pp. 31–32.

[13] J. Lederberg, personal communication and interview, 1993. See also Lederberg, 1986, pp. 627–639.

of the established leaders in American biology, especially those in the field of genetics. Most significantly, the Phage Group, including Max Delbruck, Salvador Luria and Andre Lwoff were present to hear and respond to Lederberg and Tatum's results. This high-profile coterie of researchers worked with bacterial viruses (phage); consequently they were experienced with the basic organisms and techniques used by Lederberg and Tatum. Because of this shared expertise, the Phage Group formed a crucial audience, both in terms of spreading the information and in terms of confirming (and giving authority to) Lederberg and Tatum's results.[14]

The experimentation on *E. coli* continued in Lederberg's new laboratory at Wisconsin-Madison, and expanded to a number of other groups. Joshua Lederberg produced the first map of the *E. coli* genome in 1947. This map demonstrated the utility of the new organism as an experimental tool, and provided proof that most *E. coli* genetic traits displayed Mendelian behavior. The value of a simple, fast-growing, and genetically recombinatory organism as a tool for new types of genetic studies was an important factor in the excitement over *E. coli*. In 1948, Joshua Lederberg and his then-wife, Esther Z. Lederberg reported their method of replica plating and further developments in existing selection procedures. These two techniques enabled a far more efficient and rapid collection of valuable and rare mutations in the bacterial population. The mutants in turn facilitated the isolation of new genetic markers in *E. coli*, most pertinently, that of resistance to the antibiotic streptomycin. In 1950, Bernard Davis proved that cell-to-cell contact was necessary for recombination to occur. This evidence was used to strengthen the analogy to higher animals, and to deflect objections that the purported "sexual conjugation" was merely a case of mixed auxotrophic cells releasing complementary nutrients into their shared media.[15] The one consistent assumption shaping all of these experiments was that the transfer of genetic material could be analogized to sex as it occurred in sexually reproducing organisms. Joshua Lederberg had initially modeled the "sexual" system in *E. coli* on that of the protozoan *Paramecium*, in which pairing cells actually fuse together, allowing the formation of a fully diploid cell, like that formed by the fusion of sperm and egg. In fact, the mechanics of Lederberg's model, and the language with which he described it – especially his use of

[14] Lederberg has discussed at length (interview, 1993) the importance of this meeting in convincing other researchers of the validity of his findings, airing their doubts and further questions, and disseminating information about the organisms and techniques used in discovering the recombinatory events. For more information on this, see Lederberg, 1987; Brock, 1990, pp. 82–85. For more information on the Phage Group, and its role in the rise of molecular genetics, see Brock, 1990; Cairns, Stent and Watson, 1966; and Kay, 1993.

[15] "Auxotrophs" are organisms which have nutritional requirements for substances they are not able to synthesize themselves.

the term "zygote" to refer to the paired *E. coli* cells – clearly presumes the equivalence of the bacterial process to gametic fusion in higher animals. This presumption of sex persisted despite increasingly evident differences between bacteria and higher organisms, including marked differences in their recombinatory processes. Unsurprisingly, Lederberg's assumption of a bacterial "sexuality" modeled on the physiology and genetics of sex in higher organisms did not function, either scientifically or socially, as a neutral hypothesis but as a normative one.[16] By 1951, attempts to accommodate all of the new information about bacterial recombination and the bacterial genome within Lederberg's original model of sexual reproduction and recombination by cell fusion had led to the proposal, in a paper presented to the Cold Spring Harbor Symposia, of a branched four-armed chromosome. The paper in which this hypothesis was presented "became famous in the genetics community because of its length and inordinate complexity," to the extent that even participants in the events have later described it in terms of a Kuhnian paradigm crisis.[17]

The actual physical processes behind the genetic effects of conjugation, or "bacterial sex" were little studied in the five years following Lederberg's discovery of recombination. The reasons for this lacuna were both theoretical and technical, and will be discussed below. However, in 1952, with the work of William Hayes on the kinetics of "mating" in *E. coli* K-12, the sexualization of bacterial recombination initiated by Lederberg's genetic model was completed with a sex-based physical model. Hayes' experiments involved a mixed population of cells, one strain of which passed a resistance to the other strain, which subsequently passed the resistance on to its clones. Hayes designed his experiment specifically to test for a double recombination – in other words, cells had to possess traits drawn from both of the initial bacterial strains to survive. Such an experiment automatically eliminated at least 75% of progeny cells. This drastic cull contributed substantially, if artifactually, to the durability of the sexual analogy in bacterial genetics. First, it erased the fact that all of the cells, whether recombinant or not, produced progeny – and therefore were "fertile." Second, the methodology obscured the fact that that the surviving cells progeny were not themselves half-breeds, but exact clones of one (perhaps recombinant) original cell. Hayes correctly

[16] For additional examples of such normative assumptions in molecular biology, see The Biology and Gender Study Group, 1988, pp. 61–76; Oudshoorn, 1990, pp. 243–261; Spanier, 1995; Fausto-Sterling, 1985. For another perspective on the normative effects of "sex" on biological ideas (in this case of species), see Schloegel, 1999, pp. 93–132.

[17] Norton Zinder, co-author of the paper, described it as "overwhelming . . . I would say that the data in that paper are all really quite good. I would also say that almost every interpretation in that paper is wrong . . . If you ever wanted to pick a symposium that represents what Kuhn calls a crisis in revolutionary science, this symposium is it." Brock, 1990, p. 87.

interpreted his results as stemming from some genetic difference between the two strains of *E. coli* K12. He suggested that such a difference indicated an inherent "sexual polarity" in the *E. coli* population. More importantly, any new resistance in the previously sensitive parental strain was rendered invisible. This aspect of the experiment's design hid the fact that all of the cells – including the one which was, on the basis of this experiment, termed "male" and "infertile" – in fact produced offspring. It also hid the occurrence of partial transfer of genetic information. Neither of these two characteristics of "mating" fit easily into a sexual model based on higher animals, but since they were neither expected, nor theoretically desirable, given the prevailing sexual model, Hayes did not test for them. Without such complications, the definition of *E. coli*'s recombinatory process as sex-based was readily accepted as conclusive and satisfying within the scientific community. The discovery in 1953 of the F factor, a small circular piece of DNA which encoded the ability to transfer itself and one or more other traits – the actual vector of the recombinant genetic information in bacteria – was incorporated in the canon of bacterial genetics, but the sexual analogy to higher animals remained unchanged by this startling departure from standard models of "sex."[18]

With the acceptance of *E. coli* as sexual, and therefore genetic organisms, bacteria became fit subjects for genetic examination by classical as well as by the new molecular techniques. After the isolation of the F-factor, and the discovery of its unidirectional transfer, a number of new investigative paths opened up. The use of bacteria as a genetic study-tool began, and its value and strength was rapidly increased by further investigation of the "mating" phenomenon. The kinetics of *E. coli*'s recombinatory process remained a focus of attention, and differences between that process and those of higher animals were further defined. Nonetheless, the terminology of sex and sexuality, and the explicit language of gender which informed that terminology, were retained.

Early published reports on the discovery used the language of mammalian sexuality to describe what was taking place in *E. coli* on the molecular and cellular levels, despite the fact that no evidence as to the mechanism underlying that movement was known.[19] By 1956, however, this type of

[18] Hayes, 1953a, pp. 72–88; Cavalli, Lederberg and Lederberg, 1953, pp. 89–103.

[19] See for example, the original Lederberg/Tatum paper in 1946, which discusses "the occurrence of a sexual process," considers "cell fusion" to be the only means of recombination, and postulates a "hypothetical zygote." Lederberg and Tatum, 1946, p. 558. A 1947 article acknowledges that "A complete analogy cannot be drawn at present between the inheritance of bacterial characters and the Mendelian processes of higher forms," but implies that such an analogy will soon be proven appropriate (Tatum and Lederberg, 1947, p. 681). Later in 1947, Lederberg makes the comparison to humans explicit: "It seems likely that an analogous

genetic exchange and recombination had been sufficiently well characterized in terms of mechanism and effects that a model had become established. The model presented was phrased in terms of "mating" male and female cells engaging in "sexual conjugation."[20] Various qualifications and reinterpretations were appended to this model, as further experimentation turned up new types of bacterial recombination and as genetic analysis gained resolution. The "sexuality" of bacterial recombination, however, remained essentially unquestioned despite the tide of evidence that in fact the "mating" process in bacteria was very different indeed from conventional sexual reproduction.

In a case such as this, where socially weighted metaphors and terminology whose implications are clearly under-determined within the experimental system are used to define and explain that system, a number of interrelated questions arise. First, how much was known when the language itself was defined, and does that language accurately and precisely reflect the available knowledge? Was it under-determined, or did it imply more than the data themselves? Second, at what point was this language, and in this case, the model which it implied, proposed and in what context? Was the underlying model integral to the scientific research, either in terms of providing shape and impetus or in terms of locating the research strategically? Finally, how pervasive were the language and the model – were alternative explanations or alternative sets of terminology available? By interrogating the language and assumptions of model and metaphor in this rather narrow and even technical way, I hope to build a framework within which to address broader issues of gender and culture in science.

Bacterial Recombination: "Mating" and the Linguistic Construction of Bacterial "Sex"

> *And I imagined besides, whenever I saw two little animals entangled together, either swimming or lying still, that they were a-copulating.* Anthony van Leeuwenhoek[21]

Anthony van Leeuwenhoek, in 1692, had the forthrightness to use the term "imagined" to describe his interpretation of his microscopic observations.

comparison of the number of somatic and generative cells in an organism like the oak tree, or man (especially the female of the species) would give ratios similar to those prevailing in *E. coli*" (Lederberg, 1947, p. 523).

[20] Hayes, 1952, pp. 118–119. Moreover, those "male" and "female" cells were respectively active and passive, their roles were described as energy-expending versus energy-consuming, and the female was defined as lacking or defective in the essential active quality of the male.

[21] Anthony van Leeuwenhoek, Letter 71 (7 March 1692), in Dobell, 1932.

Moreover, he made it clear that he saw the cells as very small, but normal animals; thus in assuming that they reproduced sexually, he was not challenging established understandings of the process. As with the geneticists and bacteriologists described here, working some two and a half centuries later, Leeuwenhoek chose the metaphor of sex to explain and describe a newly discovered phenomenon, making his cells – new and exotic – fit into the accepted knowledge of the time. His psyche was certainly well attuned to the scientific norm, as he was followed by an industrious line of voyeurs in assigning to the single-celled denizens of the microscopic worlds an active and anthropomorphic sex-life. However, as that world became better known, and as the role of sex was defined in terms of its genetic as well as its reproductive component – in other words, as biology entered the twentieth century – the naturalness of the sexual metaphor to the life-cycles and behavior of bacteria became questionable. Experiments to text for sexual behavior were done in the early years of the century and produced inconclusive or flawed results; interest died, as geneticists focused on other organisms.[22] However, when microorganisms again impinged upon the geneticists' domain, with the publication of the Avery, McCarty and Macleod experiments transforming single cells with introduced DNA fragments, the metaphor of sex returned as well. Centuries of experimental exploration of Leewenhoek's "little animals" had in no way altered the naturalness and inevitability with which sexual behavior (and gendered roles) were attribute to all living creatures.[23]

The fact that sexual recombination was the only known instance of comprehensive genetic exchange, combined with the need to make *E. coli* genetics intellectually assimilable to established genetic models, make Lederberg's decision in 1946–1947 to use sexual language and a sexual model fairly easy to understand, tactically and scientifically. However, by 1951, when the Lederberg group was driven to postulate a four-armed chromosome in *E. coli* to explain their results within the model of normal, gamete-like sexually fusion, it was clear that something was very different about bacteria. The model, as we have seen was stretched almost beyond the limits of understanding, even for the experimental community involved. As this point, one might have expected an alternative model to have been accepted with some relief. Since Lederberg and his lab proposed no real change in their model, and no change appeared in their language, it may seem that no alternative was available.

In fact, the very same 1951 presentation in which Lederberg et al. proposed an entirely novel chromosome structure for *E. coli* also described a new

[22] See Brock, 1990, p. 75.

[23] Anthony van Leeuwenhoek, Letter 71 (7 March 1692), in Dobell, 1932.

form of genetic exchange – and thus a new kind of recombination – which had been discovered in *Salmonella* (a bacterial species closely related to *E. coli*) by J. Lederberg and N. Zinder. They called this phenomenon "transduction," and initially defined the term as "any genetically unilateral transfer in contrast to the union of equivalent elements in fertilization."[24] However, transduction was not accepted as an alternative model for recombination in *E. coli*, and the Lederbergs' presentation of the *E. coli* material was uninflected by this non-sexual pattern for bacterial genetic exchange. Instead, the initial broad definition of "transduction", which within the year would have subsumed the phenomenon already known as "conjugation", was quickly discarded for a narrower one which limited transduction to processes involving transfer of bacterial DNA by viruses.[25] Nor was the Lederberg group alone in cleaving to the notion of bacterial "sex": he persistent attraction of the sexual model is clearly visible in William Hayes' early work on *E. coli* genetics.

William Hayes, like Lederberg, came to bacteriology from medicine; initially, he worked with *Salmonella*, but turned to *E. coli* to do genetic studies – and in particular studies of the kinetics of mating. In 1952, Hayes reported a unidirectional transfer of genetic material during conjugation. By this time, the idea of "transduction" had begun to be assimilated into genetic theory. However, the definition, and thus the inclusiveness of the term "transduction" was not yet settled. In his initial research report on unidirectional genetic transfer, Hayes hypothesized an "infectious heredity" or some kind of viral particle as the vehicle of recombinatory material: "the known facts of recombination, and especially its marked enhancement by small sub-mutagenic doses of ultra-violet light and the presumptive one-way transfer of the genetic agent . . . suggest the possibility that this agent might be a virus."[26] But his paper did not just describe an unequal transfer of DNA; it also defined an unequal role for the two strains in the production of recombinant progeny: "The incompetence of W677 [an F– streptomycin-sensitive strain]

[24] Zinder and Lederberg, 1955, p. 681.

[25] Zinder and Lederberg, 1952, pp. 679–699; Lederberg, pp. 75–107.

[26] Hayes, 1952, pp. 118–119. Streptomycin is an antibacterial chemical used by Hayes to "investigate the dynamics of recombination;" it blocked recombination in non-resistant cells, while the medium would allow only cells which had already undergone recombination to survive. Thus the rate at which had already undergone recombination to survive. Thus the rate at which recombination occurred could be deduced from the number of survivor-colonies to appear when streptomycin was added at longer or shorter intervals after mixing. This aspect of the experiment was a total failure. The accidental discovery of unidirectionality which emerged from it was based on the fact that the recovery of recombinant colonies depended on which of the two intermixed strains possessed resistance to streptomycin, despite the fact that recombination was known to have occurred. Basically, one of the two pairing cells had to remain alive after recombination had taken place, and the other didn't.

becomes intelligible if we suppose that the role of W677 is primarily the vital one of accepting genes and incorporating them into its genetic structure."[27] In his experiment, the cell which lacked the transmissible DNA – the "incompetent" passive receptor of actively transmitted DNA – was also the cell in which recombination and the production of recombinant clones took place. This artifact of experimental design would later play an important part in the process of gendering the role and nature of both participating cells.

As further investigations of the kinetics and mechanism of DNA transfer and recombination of *E. coli* were made, the similarities between "conjugation" and the virus-medicated phenomenon of transduction became increasingly apparent. Hayes summarized his theory in 1953: "It may be said that the F agent resembles temperate phage in the stability of its relationship with the host cell and in the efficiency of its transmission to sensitive cells."[28] Further, he observed that a hypothesis holding the F agent to be the "genetic vector": "neatly explained the dependence of mating on the presence of F in one of the parents, the unidirectional transfer, the differential effect of the streptomycin on the fertility and viability of F+ cells and the high frequency of inheritance of . . . recombinants under conditions mitigating against free infection."[29]

Despite its apparent success in explaining the known facts of bacterial recombination, the language of infection – epitomized by Hayes' term "infective heredity" – was never fully incorporated into the dominant or popular scientific discourse on bacterial genetics. Joshua Lederberg opposed the viral explanation of conjugation, and swiftly pointed out its failings and ambiguities. Hayes first mentioned his alternative to a sexual model in January of 1952. Only three months later, Lederberg's group submitted a paper redefining Hayes' "infective agent" as only one factor in determining "sex compatibility" – something which could affect the ability of bacterial strains to "mate" with one another, but which was a part of the bacterial genome.[30] Lederberg et al. first argued that "the system of sexual compatibility was previously obscured by its unique inheritance via an infective agent." This claim was paradoxical when interpreted within

[27] Hayes, 1952, p. 119.

[28] Hayes, 1953b, p. 79. "Temperate phages" are bacterial viruses which are able to integrate into the host bacterium's genome and remain here for several generations, only emerging to become infective viruses again under certain conditions – for instance, if their host cell is no longer viable or stable. "Lysogeny" is the name for this entire process, and "sensitive cells" are those which can be affected (or infected) by some specific agent.

[29] Hayes, 1953b, p. 79.

[30] We now understand that the F agent exists both as a transmissible plasmid (a small separate chromosome or circle of DNA) and as strand of DNA integrated into the bacteria's own genome.

the context of work on sexual compatibility in other species: if the "compatibility" is "sexual," then the system is by definition neither unique nor mediated by infection, but by the sexual fusion of gametes or of the "parent" cells. The paper subsequently dismissed the similarities between viral transmission (transduction) and what Lederberg had described as bacterial "sex":

> The infective transmission of F+ at once calls to mind the latent bacteria viruses, especially since strain K-12 itself is lysogenic for such a phage, [lambda]. However, the filterability of bacteriophage at once differentiates it from the F+ agent, as well as from the gametes themselves . . . The cell-to-cell transmission of F+ in short term experiments is far more efficient than that of [lambda]. These agents share the property of infectious transmission, but there is no evidence that [lambda] is either a determinant or an agent of genetic recombination, and it is sharply delineated from F+ in other respects.[31]

The Lederberg group presented a complex argument for the location of the F+ factor in the bacterial genome (rather than as a transmissible agent in the cytoplasm).[32] To maintain his model of *E. coli* recombination as an example of heterosexual sex, Lederberg proposed a new constraint on bacterial sex: "sex compatibility" or "relative sexuality." However, these states depended on the existence of some kind of difference between the "sexes" which was *inherent* to the organisms and not contingent upon viral infection. It is unsurprising, then, that the one part of Hayes' discovery which Lederberg et al. never questioned was the unilaterality of the genetic exchange. Given the historic assumption of unequal parental contribution in mammalian sexual reproduction, a unilateral transfer of DNA between the cells could readily by taken as confirmation of essential, rather than contingent difference.[33]

This 1952 paper also notes an important disciplinary result of Lederberg's model, in terms of the effect that his assumption of a *sexual* exchange mechanism had on the direction of further research into bacterial recombination:

> The inference of a sexual mechanism in *E. coli* has rested almost entirely on indirect genetic evidence .... In an unanalyzed [sexually

[31] Lederberg, Cavalli, and Lederberg, 1952, p. 724.

[32] Their argument was based on the fact that incompatible phenotypes could be shown to carry the F factor, while viruses which had no apparent "agent" for transmission could still produce recombination. Actually, both instances could be explained by the experimental methods used to produce them, as became apparent in later investigations.

[33] See for example Fausto-Sterling, 1986; Martin, 1987; Schiebinger, 1989; Spanier, 1995 for historical and modern analyses of this issue.

> undifferentiated] system, the rare occurrence of sexual fusions must be ascribed to chance. With a probability as low as one per million that a given pair of cells would mate, there is little likelihood of successful and verifiable microscopic observations. For this reason the research of the past five years [since the 1947 publication] has been devoted largely to the exploitation of crossbreeding for physiological and formal genetic analysis, rather than to studies on the sexual mechanism itself.[34]

In other words, assumptions underlying the early sexual model caused the focus of further research to be mainly on the effects of transfer, rather than on its mechanism. The unquestionable "naturalness" of this sex-based model for Lederberg's colleagues and co-workers in genetics is evidenced by the fact that only Hayes, an outsider trained in medical bacteriology instead of genetics, decided to look at the mechanisms. Intriguingly, in this paper, Lederberg et al. also redefined the word "fertility", stripping it of its non-genetic connotations of the production of progeny:"*Fertility* will refer to the experimentally observed production of recombinants from crossable parents." Thus "fertility" was no longer linked to the ability to produce offspring, whether sexually or asexually, but refered solely to the production of new genetic combinations – a fairly major change. Later in 1952, Lederberg would also redefine his own term, "transduction", writing: "transduction is functionally and perhaps phylogenetically, a special form of sexuality."[35] This shift reversed his original intention of creating a universal category for genetic exchange in bacteria, and illustrates his commitment to the sexual language, perhaps because bacterial "sexual conjugation" had been so successfully integrated into the broader field of genetics. These two redefinitions made "fertility" dependent on recombination and made even virus-mediated recombination a kind of "sex." Thus they eliminated some of the more obvious contradictions inherent in the use of sexual terminology for a bacterial process remarkably like "transduction." In doing so, they also made both the word "sex" and the sexual model far more inclusive, and thus more powerful in terms of both applicability and robustness, creating a kind of "meta-sex" which could incorporate any type of large-scale exchange of genetic material by any means. To a significant extent "sex" disappears as a non-genetic phenomenon at this point.

Lederberg's conservative opposition to the infection model, while powerful, was insufficient to explain why the viral construct was so firmly rejected, in favor of the sexual model. The viral model was based on newer, and there-

[34] Lederberg, Cavalli, and Lederberg, 1952, p. 728.
[35] Lederberg, 1952, p. 413.

fore less "naturalized" or assimilated scientific information and discoveries; the existence of temperate viruses, and of viruses which infected bacteria only became known in 1922 and 1915 respectively. The (comparative) unfamiliarity of this information and of the experimental subjects and techniques involved may have played a role in the negative response to Hayes' hypothesis and model. Only fairly elite groups, like the Phage Group, would have been comfortable with these developments.[36] Moreover, using a non-sexual model for recombination would also have required an explicit re-evaluation of the basic assumption of classical genetics, a more difficult – or less scientifically productive – task to accomplish than the type of re-evaluation implied by Lederberg et al.'s new (and unchallenged) definitions for sex and fertility. As his research focus shifted from clinical bacteriology to bacterial genetics, Hayes too adopted a sexual metaphor; indeed, the same 1953 review article which detailed the similarities between integrated viruses and the F-agent in *E. coli* nonetheless described the unilateral transmission as a sign of "sexual differentiation" in *E. coli*. The cells were already taking on sexes analogous to those in higher organisms. Hayes referred to "F− oogametic" cells, and "F+ gametes" and, even more explicitly, to "ménage à trois experiments."[37] In perhaps the most entangled passage in the paper, and the one most indicative of a general confusion over the type of genetic activity to which the term "sex" could appropriately be applied, Hayes stated that: "Previous work on the effects of streptomycin and ultraviolet light on recombination . . . had suggested the possibility of *sexual differentiation determined by a genetic vector of virus nature*. Accordingly, experiments were set up to investigate whether mating type transformation could be effected by infection."[38] (My italics). In these two sentences, "sex" was to be determined – if not created – by infection, and confirmed by transformation (in which free, purified DNA is introduced into a cell, causing its phenotype to change). Clearly, to use the term "sex" to describe such a transfer of DNA was to radically change its established meaning, even in genetics.[39] Yet Hayes' definition provoked no reaction within the genetics community.

[36] In fact Brock indicates that Delbruck, who had been hostile to Lederberg's sexual model, responded better to the Hayes explanation. See Brock, 1990, p. 90. On Delbruck and the intellectual climate more generally, see Kay, 1993, pp. 243–268.

[37] Hayes, 1953b, pp. 84, 86, 90.

[38] Hayes, 1953b, p. 77.

[39] Defined in Adelberg, 1960, p. xviii: "The essential features of sexuality are: (i) the formation of the diploid fusion cell or zygote, containing the fusion nucleus; (ii) the synapsis of homologous chromosomes, followed by the formation of recombinant chromosomes; and (iii) the segregation of chromosomes through the process of nuclear division." None of these processes occur in infectio, or in viral integration.

The Appropriation and Naturalization of Gender

> *I must say to you, as I've oft-times said already, that 'tis not my intention to stick stubbornly to my opinions, but as soon as people urge against them any reasonable objections, whereof I can form a just idea. I'll give mine up and go over to the other side.* (Anthony van Leewenhoek, 1694)

Instead of replacing the language of sex with a language of infection, Hayes' evidence that *E. coli* recombination depended on a transmissible agent served to reinforce the sexual metaphor. Given the unquestioned assumption of a sexual mechanism for bacterial recombination, Hayes' discovery of unidirectionality of transfer – and thus of inequality between the two bacteria involved in genetic transfer – simply extended the conceit. In fact, it was precisely this lack of parity between the cells which enabled his colleagues to categorize the bacteria as "male" and "female." Certainly, explicit gender-labeling did not occur before Hayes described the unequal genetic and material contributions of the involved bacteria. Rather, under the earlier model proposed by the Lederberg group, the cells were equivalent in amount and contribution of genetic material, and both were believed to donate genes to the other and its daughter cells. It is illuminating to note that the genetically equal (and promiscuous!) cells proposed in this model were never labeled by gender. In discussing his initial model, Joshus Lederberg explained the absence of sexes as a case of Occam's Razor: "It seemed to me at the time the most parsimonious conclusion to assume no more asymmetry than the facts demanded, and since at first I could find no evidence of mating types – the original strains that I had all mated with one another very promiscuously – the idea of reciprocal exchange required the least additional assumptions and I had no basis to say that A was an male and B was a female . . . I couldn't say very much more about the details of the interaction . . ."[40] The idea of unequal "parental" contribution would also have disrupted Lederberg's carefully drawn parallels between sex in higher animals and recombination in bacteria. However, the fact that a gender-based system of labeling was seen as *simple* and appropriate after the discovery of an unequal genetic contribution offers an insight into the perceived naturalness of a model of male and females as unequal and as respectively defined by sufficiency and deficiency, possession and lack. "Gendering" was absent in experimental descriptions of similar systems based on equal genetic contribution to and material propagation of daughter-cell recombinants. For example, descriptions of *Paramecia* "mating," in which each of the involved cells contribute genetic material to the other and each produces recombinant

[40] J. Lederberg, interview, 1993.

daughter cells, discriminate between the protozoa in terms of their "mating types" rather than by sex – or gender.[41] Likewise, Lederberg, despite his highly sexed language of "mating," "copulation," and "promiscuity," did not gender his bacterial protagonists until there was also a system of *disparity* between the two participating cell-types. The existence of difference alone was insufficient to call forth the language of gender; inequality was required.

This differentiated system was constructed in much the same way as sexual reproduction in mammals; as with the animal model, bacterial recombination was described in terms of gendered sex roles matching those found in human society: "The conclusion was that viable [F−] cells are essential to the fertility of the cross because they alone form the zygotes in which the whole process of recombination occurs; they are exclusively recipients of genetic material. The role of [F+] cells, on the other hand, is to act as genetic donors; once they have performed this function they are dispensible."[42] The function of the first parent or gene donor was simply to fertilize the other parent or gene recipient, which then became the "zygote cell."[43] As I will show below, the donor cell came to be explicitly described as "male", the recipient as "female."[44] Even contemporary illustrations of "bacterial sex" follow gender stereotypes: in one set of early electron micrographs of *E. coli* cells, the F− or "female" cell was chosen from a bacterial strain which is morphologically "short and plump," while the chosen F+ ("male") strain is "typically long and narrow." The picture was oriented with the F+ cell on top of the F− cell, and the bacteria were captioned as being "in intimate contact."[45]

[41] *Neurospora* are also described as being of different "mating types," rather than as males and females; however, this may be due to the fact that the fungi are in fact hermaphroditic. *Neurospora* usually reproduce asexually, by simple division; when they do use recombinatory reproduction, the process in described as one in which haploid "male" spores produced by one mating type are received by long, slender "female" filaments protruding from a cell of the other mating type. The two types differ by one gene. Intriguingly, the gender of the filaments and of the spores seems to have been determined solely by the fact that the "female" organ "receives" genetic information, and that the "female" cell produces the recombinant clones. Structurally, one could certainly argue that the analogy to higher animals which is inherent in the description of the fungi as having "sex" and as having male and female "sex organs" would require that the male organ should be the protrusive and filamentous one, while the small round spore could be analogized to an egg. However, it is clearly the perceived direction of *information* transfer which determines the gender-attributions. This is of course paralleled in the case of *E. coli* K-12.

[42] Hayes, 1968, p. 555.

[43] As in Lederberg and Tatum, 1946, p. 558. The use of the term "zygote" appears consistently in the literature until the 1960s, when it becomes less frequent.

[44] Wollman, Jacob and Hayes, 1956, pp. 141–162.

[45] Wollman, Jacob and Hayes, 1956, pp. 141–162.

Revealingly, these two strains were regularly chosen for electron micrographs of "mating," although other phenotypically distinct strains were readily available.

E. A. Adelberg summarized the role of Hayes' discovery as it was seen in 1960: "The discovery of F made possible the recognition of two mating types in *E. coli*: F+, harboring F and behaving as genetic donors, or males; and F−, lacking F and behaving as genetic recipients, or females."[46] Here, again, the language is suggestive: the discovery of a transmissible factor which coded for gene transfer allowed the "recognition" – not "inference," "interpretation," or "deduction" – of two "mating types." Adelberg naturalized Lederberg's successful reconstruction of Hayes' "infective agent" into a sex determinant, although he avoided the by-then discarded language of "relative sexuality." He also made the designation of donors and recipients as male and female seem unremarkable – the words "donor" and "male," "recipient" and "female" are treated in this text as interchangeable. Finally, the lingering traces of Hayes' initial model of infection can be seen in Adelberg's choice of the term "harbor," a word commonly used in reference to disease vectors, to describe the F+ state. Moreover, it is exactly the terminology used by Hayes in 1953.[47]

The Adelberg introduction is an example of the consensus language that rapidly emerged to defuse the internal debate over how to describe recombination in *E. coli*. This language, however, does not resolve the contradictions and confusions arising from the original choice to model bacterial recombination upon sex in higher organisms. It also fails to correct the gender-based language which followed the Hayes discovery. One remarkable example of the contradictions and paradoxes created by the continued use of the sexual metaphor appears in Francois Jacob and Elie Wollman's much-cited 1961 monograph, *Sexuality and the Genetic of Bacteria*:

> Conjugation in *E. coli* K12 takes place between donor bacteria which can be considered analogous to males and recipient bacteria which can be considered analogous to females. *The fact that in certain physiological states donor bacteria can play the role of recipients does not seriously affect this fundamental distinction*. Such recipient phenocopies are indeed genotypically male and retain their donor potentialities.
>
> Sexual differentiation is dependent on a specific sex factor, the presence or absence of which conditions the male or female character of a strain. A peculiarity of the mating system of *E. coli* is the existence of different types of males, *none of which has all the attributes of a*

[46] Adelberg, 1960, pp. xxix–xxx.
[47] Hayes, 1953b, p. 83.

> *male gamete*. The two extreme types, F+ and Hfr, are determined by the state of the sex factor in the cell: it is autonomous in F+ bacteria and integrated in the Hfr types. Any single Hfr type is in general able to transfer with high efficiency only part of its genome. *The ability to transfer the total genome is the attribute of the aggregate of Hfr types* . . . [a very deceptive way of describing the fact that no single pairing results in the transfer of the entire genome, but that every individual gene can be transferred] Segments of the chromosome may become incorporated into the sex factor and be transferred serially upon conjugation. The sex factor of bacteria has accordingly very special properties which *differentiate it from the genetic determinants that are an integral part of the chromosome. In its behavior, it resembles the genetic material of temperate bacteriophages.*[48]

Here, clearly, the attempt to fit all of the available information, especially the virus-like traits of the recombinatory agent – its ability to become a part of the host genome, the unilaterality of transfer, its size, and so on – into a dichotomous system of males and females has required great logical contortions. By 1960, even fairly conservative authors modified the sexualization of bacterial recombination for internal use; Adelberg introduced the idea of bacterial sex as "a number of processes . . . which embody the principal features of sexuality . . . They differ from the sexual process of higher organisms however, in two particulars."[49] Moreover, the unmodified, uncorrected use of explicitly sexed and gendered language had increasingly become the province of introductions and popularizations. Very few subsequent scientific reports announced their sexual metaphors as frankly as those of A. L. Taylor et al, whose papers were entitled "Linkage analysis with very high frequency males in *Escherichia coli*" (1960), and "Evidence for a closed linkage group in Hfr males of *Escherichia coli* K-12" (1961). Instead, appellations of "male" and "female" were often restricted to introductory and closing material, both in experimental papers and in textbooks and essays. Thus, Jacob and Wollman used the male/female analogy to introduce the phenomenon of gene-transfer in bacteria. However, having defined the cells by their respective sex- roles, Jacob and Wollman subsequently refer to the cells as "donor" and "recipient." or F−, Hfr, and F+, using the descriptors "male" and "female" only parenthetically. For example, in a discussion of "sexual" types, they return to the subject of *E. coli* pairings: "The sexual type of a bacterial strain depends on a specific genetic structure, the sex factor, the presence or absence of which conditions the

[48] Jacob and Wollman, 1961, pp. 198–199; my italics.
[49] Adelberg, 1960, p. xviii.

donor (male) or recipient (female) character of the strain."[50] In the next paragraph, they shift to "F+" and "F−", and do not return to using sexual language until the concluding paragraphs, which I have quoted at length above.[51]

The Persistence of Sex and Gender

Given the possibility of describing genetic transfer in bacteria in terms other than those used to describe animal sexuality and given the remarkable similarity of the F factor to a virus, one must question the continued use of the sexual metaphor, and especially of the terms "male" and "female." A survey of published experimental reports has proven to be rather unrewarding since an established model is neither explained nor justified until it is called into question or ceases to work.[52] Fortunately, a number of other sources exist, partly because of the participants' own sense that these developments were of great historical importance, and partly because the scientists involved in the investigation of bacterial "sex" were or became very prominent. In his 1966 contribution to the Delbruck festschrift, *Phage and the Origins of Molecular Biology*, E. L. Wollman referred in passing to the mechanism by which a bacterium which contains a prophage (an integrated piece of viral DNA) could cause infection and induction of viral development in its partner through conjugation. He noted that he and Jacob initially called this occurrence "erotic induction," "but later renamed it more decorously 'zygotic induction.' "[53] Two factors about this statement should be considered; first, it clearly indicates the light in which, however neutrally described, "bacterial sex" was seen by its investigators. Second, the gleeful description of their original name, and of their decision to rename the phenomenon "more decorously" illustrates the attractiveness of "sexy" language, even to its inventors.

William Hayes also seems to take particular enjoyment in using the sexual metaphor, extending it into a fully formed conceit in a set of lectures to the Royal Society, complete with "intimate contact" and "conjugal union."[54]

[50] Jacob and Wollman, 1961, p. 181.

[51] Another place where the underlying assumption of physical sexes in the bacterial participants in recombination becomes evident, even in papers and texts which never use the descriptors "male" and "female," is in graphics and tables; the male and female symbols are frequently added to images and used in heading blocks, replacing the F+ and F− designations used in the text.

[52] Kuhn, 1970, and more specifically on biological texts, Myers, 1990, ff. 2.

[53] Wollman, in Cairns, Stent and Watson, 1966, p. 219.

[54] Hayes, 1966b, pp. 3–4. Here, as above, I use "conceit" in its technical sense, as an extended or structuring metaphor.

These two lectures, given in 1965 and 1966, comprise the richest published texts for gender analysis. His first lecture is particularly revealing about the linkage, implied even in the original analogy of recombination to sex, between DNA transmission and the physical act of intercourse in higher animals: "It would be reasonable to expect that chromosome transfer, once initiated, would be completed in not more than a generation time. However it turns out that in nutrient broth at 37 degrees C, in which generation time is 20 to 30 minutes, the transfer of the whole chromosome takes about 90 minutes, so that this is the only organism I know of which is blessed by the fact that the duration of the sex act may be up to four times longer than the normal lifespan."[55] Hayes liked the image so much (or it was so successful with his audience at the Royal Society) that he repeated it in the next lecture, embellished with a description of "premature separation of the bacterial couples" in which "a proportion of the length of the chromosome already injected into the female may be pulled out again by the retreating male."[56] Thus the socially sophisticated *E. coli* could not only perform *ménages à trois*, but could practice coitus interruptus within them.

The duration of bacterial recombinatory connections was a popular subject in other labs as well. In his Nobel lecture in December 1965, Francois Jacob made very similar remarks about the "conjugal bliss" of the cells, in which "the male slowly injects its chromosome into the female."[57] Indeed, this was one of the relatively few aspects of *E. coli*'s recombinatory mechanism which was not subject to the general restraint (post-1960) about explicitly sexed language. Another was the discovery that the F factor coded for the production in the donor cell of a thin tubular extrusion, whose central passage was of sufficient diameter to allow the passage of a DNA molecule. This protrusion was described as "the male sex organ of bacteria" in research reports as well as lectures and textbooks, and was named a "pilus."

If the researchers found the humor and innuendo allowed by the sexual metaphor attractive, they must also have found its gender-based roles comfortably familiar. The tradition of constructing animal behavior in terms of human cultural hierarchies is a rich and well established one.[58] The imposition of female sex on the bacterium which produced progeny, in parallel with the discussion of it as "passive" in the "mating" process and as "lacking"

[55] Hayes, 1966b, p. 233.

[56] Hayes, 1966a, p. 5.

[57] Jacob, 1977, p. 220.

[58] The literature in this area is correspondingly rich; Donna Haraway's classic *Primate Visions: Gender, Race and Nature in the World of Modern Science* (New York: Routledge 1989) offers an excellent introduction and overview, while Coral Lansbury's *The Old Brown Dog: Women, Workers, and Vivisection in Edwardian England* (Madison: University of Wisconsin Press, 1985) incorporates rich literary sources.

in an essential factor fits well with that tradition.[59] Similarly, the description of a promiscuous "male" bacteria, necessary only for "fertilization" of the "female," but nonetheless the active member of the recombinatory partnership – after all, sex was for the F+ bacterium, "an energy requiring act" – matched the state of human sexual and reproductive standards in the western world earlier in the century.

Attempts to displace the sexual model failed, although they introduced new and productive ways of examining the phenomenon. Hayes' viral language stimulated new growth in the field, especially in the area of kinetics and mechanics, and linked bacterial recombination with developments in phage genetics, like lysogeny. Nonetheless, his discovery and the growth it produced were both subsumed by the sexual model, and in fact reinforced its language. Opposition by the founder of the field, and Hayes' own apparent confusion about the compatibility of the two models of recombination certainly did not help his case, but they do not explain its final failure. As a final factor in the retention of the sexual language, consider Joshua Lederberg's analysis of why "conjugation" was retained as the name for the transmission of the F factor, in the face of his early, and short-lived attempt to replace it with "transduction":

> We talk now about conjugation . . . [which] has a very restricted meaning in the bacterial context . . . Conjugation now means what we know about conjugation in K-12. So it starts out as a definition, but it's become a description of detail that's subsequently added, and until one had that detail it might have gone a different way and the word conjugation would still have been used. That's a fairly general principal in the development of usage, that the labels tend to stick to a concrete phenomenon and then tag along with the phenomenon.[60]

In this analysis, the conservatism of science with respect to its (culturally resonant) terminology essentially overrides radical changes in the understanding of the objects to which its terms actually refer. This clearly overthrows the strong version of the scientific claim that new knowledge replaces the old as it is discovered and proved; rather, such knowledge is merely incorporated unless it urgently demands re-evaluation and change.

[59] See Hayes, 1966a, pp. 230–245; Hayes, 1968; Hayes, 1966b, pp. 1–19. Also Jacob and Wollman, 1961; and Adelburg and Burns, 1960, pp. 321–330.

[60] Lederberg, interview, 1993.

Conclusion

The effects of gendered language and sexual metaphors on the development and expansion of the field of bacterial genetics are, of course, difficult to determine. I think it is reasonably clear that the use of sexual language and of a sex-based model in the initial stages of research allowed the phenomenon to be assimilated into the realm of classical genetics. From that position, bacteria were poised to become both powerful research tools and valid subjects of genetic research and analysis; they could be moved into the new mainstream of biology. Thus strategically, the language of the early model was effective.

Later, the metaphor was less useful, and in fact may have slowed development in some areas of research. In teaching materials, both for students and for practicing scientists, the attention paid to F− strains ranges from minimal (and reductionist) as in Wollman et al. (1958), "Whereas no essential difference has been recognized between different strains of recipient (or F−) bacteria, two main types of donors may be distinguished," to non-existent, in the case of Hayes' 1968 textbook, in which F− is effaced: "We have seen that the mating system of *E. coli* comprises two types of donor, F+ and Hfr."[61] These "female" bacteria were, in part, experimentally rehabilitated from their undifferentiated state in 1960, but the data produced by those experiments obviously never filtered into the textbooks.[62] It is now apparent that some F− strains, in conjugation with Hfr bacteria transmit genetic material to the "male" Hfr cell. However, this has only emerged thirty years after the initial model was proposed.

Also, the language of sex can leak into descriptions of experiments in which *E. coli* was merely a vector. For example, in James Watson's 1962 Nobel lecture, he narrated a set of experiments exploiting *E. coli*'s capacity for plasmid transfer and selective recombination: "These experiments (into the stability of bacterial templates) were of several types. One studied the effect of adding or destroying specific DNA molecules. Sudden introduction was achieved by having a male donor introduce a specific chromosomal region absent in the recipient female. Simultaneously the ability of the male gene to function . . . in the female cell was measured."[63] Obviously the gene is neither male nor female, nor did Watson intend to imply that the gene had a sex. Unfortunately that implication was the result of his casual language, and through the rest of his description of the experiment, the situation remains

[61] Wollman, Jacob and Hayes, 1956, pp. 141–162; and Hayes, 1968, p. 669.

[62] Adelberg and Burns, 1960, p. 329: "F− strains may also vary in several sexual properties . . . When the virgin and recombinant F− strains are converted to maleness with a particular sex factor, the latter acquire a significantly higher fertility . . . . Thus, at least four types of F− strains exist, and a much larger number is predictable."

[63] Watson, 1977, pp. 189–190.

unclarified. Another important fact is also made evident in this passage: the meaning and clarity of Watson's description would in no way have been hampered had he simply referred to the two vector cells as "donor" and "recipient," rather than male and female. However, he – like Hayes, and like Wollman, and even like the Rockefeller University's publicity department, in discussing Lederberg's discovery of bacterial recombinants – chose to use the more colorful and "sexy" terms.[64]

Even in modern genetics textbooks, the male/female analogy is used, often and rather ironically following a disclaimer about its lack of validity. For example, in Suzuki et al.'s *Introduction to Genetic Analysis* (1989), the existence of plasmid-mediated transduction is introduced as follows: "This kind of unidirectional transfer of genes was originally analogized to a sex difference, with the donor being termed 'male' and the recipient 'female.' Although the terms 'male' and 'female' still persist, it should be stressed that in this type of gene transfer, one organism receives genetic information from a donor; this recipient is changed by that information. In sexual reproduction, two organisms donate equally, (or nearly so) to the formation of a new organism, but only in exceptional cases is either parent changed."[65] Disappointingly, despite this statement defining the inappropriateness of the analogy, Suzuki et al. continue their exposition using the gendered terms. Students still learn that bacteria come in sexes and in genders, active and passive, productive and reproductive. The analogy has in many respects become obscure, as a very small area of very large field. Lederberg notes that probably only a dozen labs are working on bacterial recombination as a subject of inquiry, although perhaps a majority of biology labs use plasmids as investigative tools.[66] Certainly, it is not being used – as similar analogies were in the early years of this century – to validate or to naturalize repressive social norms. It is, however, a striking example of the pervasiveness of cultural influences and assumptions in the construction and definition of even neuter scientific subjects. It therefore indicates a need for more study of such subjects and for a better understanding of how and why metaphors of sex and gender continue to thrive, and to retain their traditional power in the (at least self-proclaimedly) hostile soil of molecular biology.

64 Schwartz, 1990, p. 1.
65 Suzuki et al., 1989, pp. 224–225.
66 J. Lederberg, interview, 1993.

Acknowledgments

I would like to thank both Joshua and Esther Lederberg for the insights they offered me as I began to write this paper, as well as the many friends and colleagues who have read and discussed it with me since. For their encouragement in the final stages I owe a particular debt to Rima Apple, Harriet Ritvo, and Londa Schiebinger, and for their close reading and generous comments, I am grateful to four anonymous readers and the editors of this journal.

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