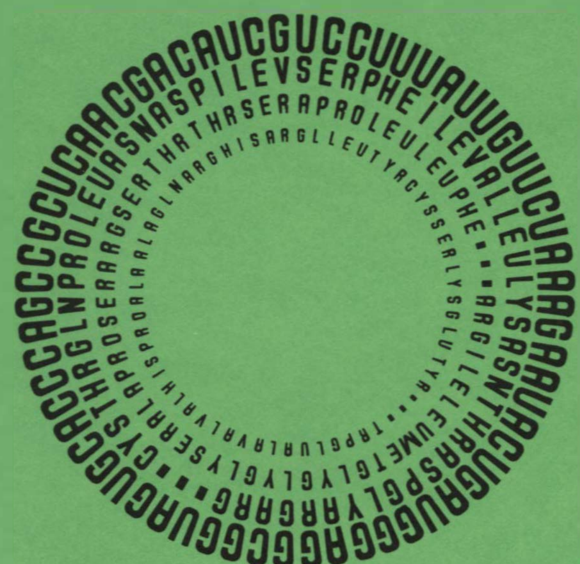
Midwest Bioscene



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Cover: *Circular Genetic Code* © John R. Jungck

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The Isolated Heart Laboratory Revisited

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Review of IHL's History

This article reviews issues in the design of computer-assisted learning materials through a case study of the *Isolated Heart Laboratory*, (*IHL*) (1). The program was developed in 1983 in response to a need for a new way to learn about cardiovascular physiology. This need arose from the desire on the part of my colleague in the Veterinary School at Washington State University to teach cardiovascular function using a laboratory-based experimental approach. However, investigative study of the cardiovasclar system required students design and conduct experiments of extreme difficulty--in particular, the preparation and maintenance of isolated heart preparations. The target audience of the original program was schools of veterinary and human medicine. In these schools one or more of the follow methods were used to teach experimental cardiovascular physiology: the important experiments were discussed but not conducted; experiments were demonstrated by a team of faculty experts; experiments were attempted by teams of students who spent most of their time learning surgery, anesthesia, and instrumentation and little learning physiology. In each case, the result was little integrative learning about the complex functions of the cardiovascular system.

The hypothesis for the design of *IHL* was that a computer simulation could be faithful to the experimental laboratory but without the distractions of non-physiologic issues. The original program was written for the IBM PC using the only available graphics adapter available in 1983 (CGA) and the 8087 numeric co-processor. It has been enthusiastically received by both students and teachers. Subsequently a new program, *Cardiovascular Systems and Dynamics* (*CVSAD*) (2), was developed that allowed experimentation with the left ventricle (the original *IHL*), heart-lung, systemic circulation, and a complete circulatory loop.

Integrative labs

IHL is one of a number of computer-simulated microworlds for science learning.

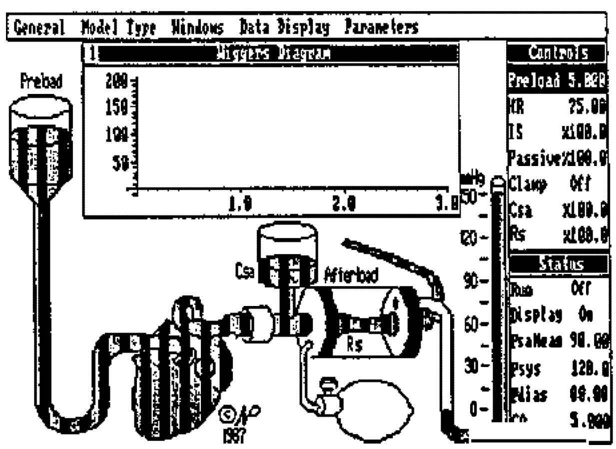

Figure 1
Drawing of an isolated heart preparation. This figure provides intutition into the nature of the "real" experimental laboratory. Students and faculty often point to the drawing in the process of discussing an experiment.

These programs cover a wide range of topics in biology, from evolution (3), through genetics (4), microbiology (5), and population biology (6); they share a common instructional philosophy. This has been called "post-Socratic" (4) or "design learning" (7). The fundamental premise in each case is that students must learn both content-specific and general principles of science through the solution of realistic problems in the domain. This learning is not rote memorization, but a "construction of shared meaning" between faculty and student (8). Such a premise is not new, the investigative labs of the 1960's attempted to facilitate the same kinds of learning. That goal is not always possible with "wet" laboratories, as in the case of cardiovascular function, because of difficulty, cost, or other constraints.

Original Design Goals

All computer-simulated laboratories have faced similar design problems. These challenges can be roughly described as: student input to the program; program output to the student; interactions with the operating system; and strategies to facilitate program adoption and classroom use by others. Different authors have adopted different solutions based on the hardware and operating systems and programming tools available.

Simple operation for first-time users

A significant design concern in *IHL* was to enable the first-time user to run the program without extensive knowledge of DOS or computers. In fact the first versions of the program were supplied on an autobooting disk. At the time of the original design, keyboards, not mice, were predominant and I disparagingly described keyboards as presenting students with "96 wrong buttons."(9). My solution was function keys--thinking that keys not found on a typewriter dedicated to a specific function, ie entering letters or ending lines of text, were better suited to new tasks. Six functions keys were assigned generic functions useful everywhere in controlling the program.

Graphical interface--visual tutoring

A graphical interface was central to the design strategy for *IHL* and has continued to shape my thinking about student's interface to simulations (10). Specifically, it was important that the student interact with a drawing of the laboratory apparatus as it appeared in their textbooks. In *IHL* the drawing is based on the original report of a similar experiment by Ernst Starling (11), figure 1. The iconography of the real laboratory was important because the computer was to stand in for certain (difficult) aspects of the animal

Graphics should be used to teach not just entertain

laboratory but not to replace the intuitive "feel" to be gained by exploring with real apparatus. For example, the pressure filling the left atrium was depicted by a pressure reservoir (to left in figure 1). In the original version of the program, the student controlled the pressure by graphically raising and lowering this reservoir. The pressure within the Starling resistor was recorded on a mercury-filled manometer with moving mercury, rather than a pressure gauge, to further emphasize the physical meaning of pressure in hydraulic systems--difference in the height of a fluid. Such fidelity between the graphics and the student's input were impossible to maintain, however, where heart rate or heart strength where concerned. None-the-less, the intention was to "visually tutor" by providing a "realistic" laboratory icon.

Visual tutoring had another role in the program, it shaped the form for data displays. *IHL* was the first widely distributed cardiovascular teaching program to present the operation of the left ventricle in terms of the pressure-volume plot, figure 2. This diagram was first reported by Suga and Sagawa (12) in the early 1970s. It is a very compact notation for showing the events of the cardiac cycle and how changes in the heart and its hemodynamic environment interact. The decision to use this graphic notation in parallel with the more traditional data vs time plots presented students with a wordless comparison between the two notational schemes and allowed them to adopt the more modern and more powerful one.

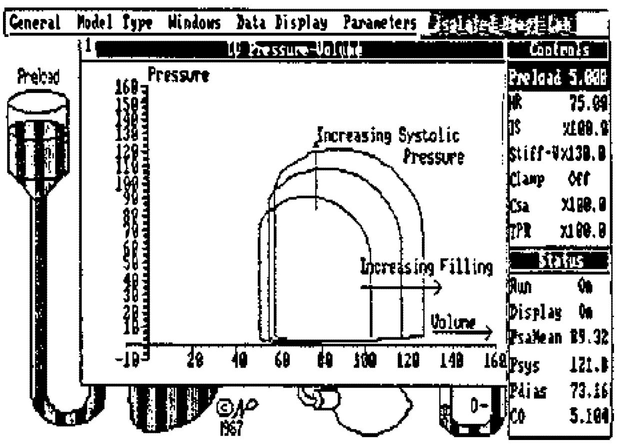

Figure 2
Here the data window is enlarged, partially obscuring the apparatus. In the experiment preload pressure (left) has been raised in two steps. Increasing pressure increased both filling volume and peak pressure.

Lessons learned from IHL version 1

In 1983, the color graphics adapter (CGA) was the only graphics device in existence for the IBM line of computers. The brief history of microcomputing graphics (mainly Apple II) indicated that one or two graphics modes were all that would be available for a small personal computer. Consequently *IHL* was designed to optimize its interaction with the bitmap of the CGA graphics screen. Alas, no sooner had that task been completed than Hercules introduced a very different "standard." And IBM has followed with EGA, PGA and now VGA graphics. The original program was never planned for this diversity and while it can run in the CGA mode on many boards, it is less and less satisfactory. Now graphics tools for the IBM solve this problem with "device independent" graphics.

Another decision from 1983 is almost too painful to mention: *IHL* was designed using UCSD Pascal which required the P-system operating system. While a DOS compatible version of it exists now, P-system's incompatibilities have been a millstone, because each version of DOS or new hardware required a new version of the P-system. Now that good native code compilers are avialable I have abandoned UCSD Pascal.

Keyboard input–on screen memory aids

The function key design for controlling *IHL* worked, but such a strategy, by definition, had no way of becoming standardized. As user experience with such programs as MSWord, 1-2-3, and other menu-driven interfaces grew, my arbitrary assignments of "generic" functions became less desirable. Not having on-screen menus means the program requires the user to remember all the information about its operation and users complained they could not remember the keys. *IHL* now uses pull down menus activated by letters.

Instructors need to personalize a program

More than once I was asked if *IHL* could support some activity that was a personal interest of a particular instructor. The answer was always no, the program would have to be rewritten. However, over time it became clear that these requests represent a basic human need--the personalization of a tool in the process of adopting it. Thus, changing the terminology in menus, turning menu functions on and off, and putting an identifying name on the upper right corner of the screen have been added to *IHL*. An important goal of this effort was to prepare the program for translation to other human languages, especially Spanish, German, French and Italian.

Another need is for faculty to edit the dataset that drives the model itself. There are two aspects to this problem: the values of the data, is it scaled for humans or dogs; and the nomenclature for the data, again the need to personalize names. Jungck and Calley introduced this idea with their "construction kit" concept (13). As part of the design of a fault diagnosis game (14) these problems were resolved by putting names, values and legal ranges for the data into a file. These files can be used by the new version of *IHL*.

Students need to manipulate data

Students need to personalize the data from a computer simulation in another way--they need to manually manipulate it. In the original version of *IHL* cardiac function plots, venous return curves, and other "processed" data were plotted by the computer. I did not learn the significance of this error until I visited James Covell at the UCSD Medical School. He didn't tell his students that the program could make such plots, rather, he required they read the raw data values from pressure-volume or strip-chart displays and create the graphs by hand. As a result, his students were involved in the learning process because they could not be spectators. The new version of *IHL* recognizes this lesson in two important ways. First a crosshair permits accurate reading of data values from graphs. Second, the graphs can be dynamically rescaled as needed to make measurements with the necessary precision. Figure 3 shows this rescaling in action.

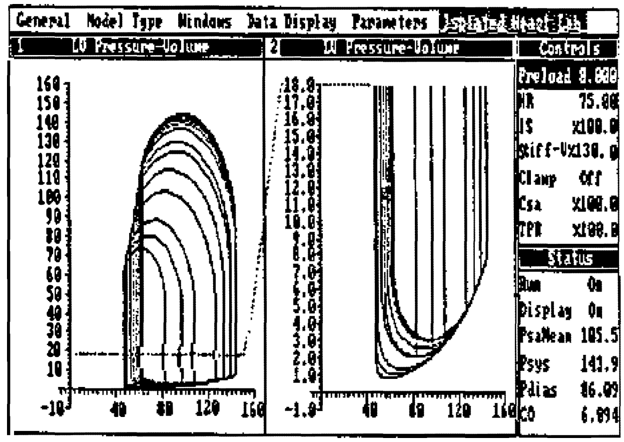

Figure 3
Exploration of the diastolic portion of the pressure-volume loop is simpler when the graph is rescaled. The right panel shows the program plotting only the 0 - 18 mmHg section of the pressure axis while the left panel simultaneously plots the full data range.

Shopper's Guide

There is a growing body of software being offered for college instruction. When you review these programs, some of the lessons from *IHL* should be kept in mind. Easy operation is an obvious criteria, but be careful that the science is not simplistic. Flexibility is important, steer away from programs that are too didactic--that you would not want to learn from yourself. Graphics should be used appropriately--to teach not to entertain. Students should be able to: design their own experiments then manipulate and interpret the data off-line and draw and defend their own hypotheses. Avoid programs with one "right" answer.

The program should be flexible enough for you to use it as a lecture demonstration.

Future plans

Work is underway to integrate simulations with other teaching media. One such vision integrates simulations with diagnosis problems based on the same model (15). An exploration seeking to understand how to combine textbooks with investigative computer laboratories is underway. The HyperCard program for the Macintosh offers one possibility. Textbooks with figures and problems based on simulations are another.

The new version of *Isolated Heart Laboratory* should be available for the IBM by fall of 1988. It will require 640K of RAM, a graphics adapter and disk drive. It should work on networks and possibly with "mice." The new version is planned to have a workbook specifically for undergraduates, both freshman non-majors and upper division majors.

Notes

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References

1. Peterson, N.S. and K.B. Campbell. 1984. Simulated Laboratory for teaching cardiac mechanics. *The Physiologist* 27: 165-169.
2. Peterson, N.S., and K.B. Campbell. 1985. Teaching cardiovascular integrations with computer laboratories. *The Physiologist* 28: 159-169.
3. Place, F. 1985. Evolve: A computer simulation for teaching labs on evolution. *American Biology Teacher* 47: 16-24.
4. Jungck, J.R. and J.N. Calley. 1985. Strategic simulations and post-Socratic pedagogy: constructing computer software to develop long-term inference through experimental inquiry. *American Biology Teacher*. 47: 11-15.
5. Schmidt, T.G. 1985. Dr. Sanger's apprentice A computer aided instruction for protein sequencing. *The Physiologist* 28: 458-459.
6. Wimsatt, W.C. 1986. Simulations in population biology. *Wheels for the Mind*, Apple Computer Inc.
7. Peterson, N.S. J.R. Jungck, D.M. Sharpe, and W.F. Finzer. 1987. A design approach to science. *Machine Mediated Learning* 2: 1&2, 111-127.
8. Novakand Gowan, 1984, *LearningHow To Learn*, Cambridge University Press: New York, NY.,
9. Peterson, N. 1984. Designing a simulated laboratory. *BYTE* 9: 287-296.
10. Peterson, N.S., K.B. Campbell. 1986. Designing interfaces for microcomputer simulations that teach integrative physiology. Trans. *Summer Computer Simulation Conference*, Society for Computer Simulation, La Jolla CA, 390-394.
11. Starling, E.H. *Linacre lecture on the Law of the Heart*. London: Longmans, 1918.
12. Suga H, Sagawa K, and L. Demer. Determinants of instantaneous pressure in canine left ventricle: time and volume specification. *Circ Res* 46: 256-263, 1980.
13. Jungck, J.R. and J.N. Calley. *Genetics: Strategic simulations in Mendelian genetics.* COMPress: Wentworth, New Hampshire.
14. Peterson, N.S., S.A. Feiner, D. Barbee. In press. Diagnostic Practice Reinforces Lessons From Laboratories. *Computers in Life Science Education*, National Resource for Computers in Life Science Education, Mail Stop RC-70, University of Washington Seattle WA 98195
15. Peterson, N.S., K.B. Campbell, R.H. Hopkins, and S.A. Feiner. 1985. An integrated cardiovascular teaching laboratory. *The Physiologist* 28: 447-448.

A MOUSE BEHAVIOR EXERCISE FOR FRESHMAN BIOLOGY

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Introduction

Experience has shown that students are likely to take more interest in working with live animals in laboratory than with preserved specimens, and that an exercise on animal behavior is better taught with living material than with diagrams or simulations. The difficulty for the biology teacher is to design an exercise that can teach some aspect of animal behavior using live animals that can be depended upon to act "properly" for the lesson.

After trying various exercises employing animals from arthropods to fishes, Western Illinois University has adopted a mouse behavior exercise for freshman biology classes that was previously developed for an ethology course at Ohio State University. The exercise described below uses only a modest amount of material. It has been reliable in developing expected results and has been enjoyable for the students.

In brief, the students place a mouse on a specially-marked board and record the activity of the mouse as it may be affected by the presence of walls or other environments on the board.

Materials Needed

The activity board (we use one for each 8 students) is a 2-foot-square piece of 1/4-inch plywood, marked with painted lines as in Figure 1. Removable walls to use with the activity board are made from 5-1/2-inch-wide boards cut so they can be stood on edge on the periphery of the activity board to make continuous walls (i.e., two boards that are nominally 1 x 6 x 24 inches and two boards that are nominally 1 x 6 x 22-1/2 inches). Boards are prevented from falling over by affixing pieces of Velcro® at the contact surfaces. In addition to the four walls, part of the exercise calls for using a pair of nominal 1 x 6 inch boards that are only 12 inches long.

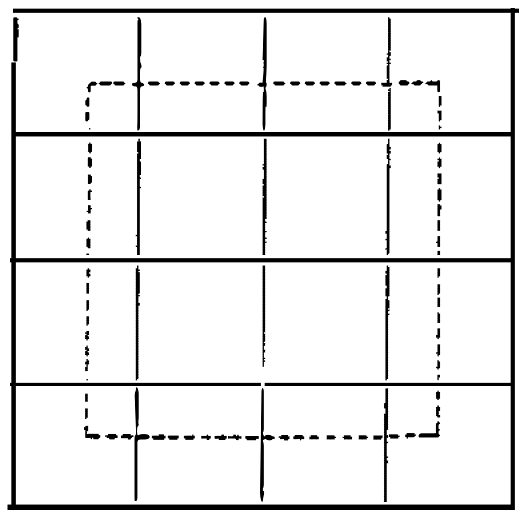


Figure 1
Activity board. Dashed line is painted red, and includes half the area of the activity board.

For colleges without their own mouse colony, pet-shop mice should work very well. Additional information on the behavior and care of mice can be found in reports by Bielfeld (1985), Silverman (1978), and Fredricson (1953). Each team of eight students will need one male and one female; having the sexes be different colors helps the students tell the gender of the animal under observation.

Procedures

We caution the students to pick up the mice only by the tail, and to expect the mice to squirm unless their feet are on a surface. Most students lose their hesitancy about picking up a mouse after watching the instructor demonstrate; being in a group of eight accommodates the few students that still do not want to touch the animals.

The students do a series of 6-minute-long experiments, in which one student calls "time check" each 10 seconds, while others record where the mouse was on the activity board at each time check (as a measure of location preference), another records the number of black lines crossed in the 6 minutes (as a measure of degree of activity), and others record the number of face-washings or defecations.

Experiment One

In one experiment, the walls are placed on the board to make a box, the mouse is placed on the red line, and the students measure whether the mouse has a preference for being near the walls (outside the red square) or away from the walls (inside the red square). A sample set of data collected by one team in one class is shown in Figure 2.

Experiment Two

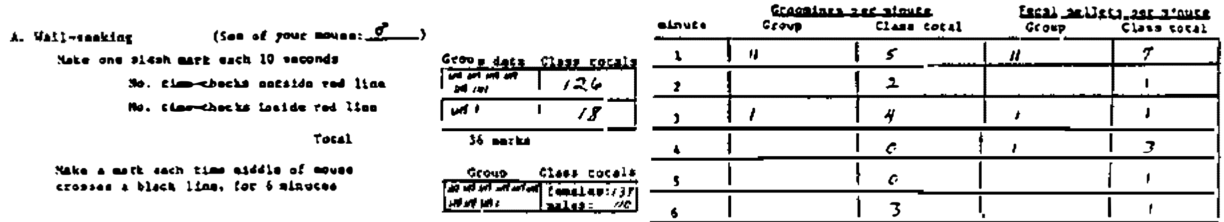
In a second experiment, the students answer the question, "Was the mouse really seeking the walls, or was it just avoiding the center of the open area?" by placing the walls on edge under the board, so the mouse can walk to the edge of the board and look over a 6-inch high cliff. (Generally, the mouse prefers to be near the walls in the first experiment, and near the edge in the second experiment.)

Experiment Three

A third experiment gives the mouse a choice between being near a wall or near an edge by putting one of the walls on the board and the board on the remaining three walls; the students record the mouse's location at each time check as being nearer the wall or nearer the cliff edge.

Experiment Four

A fourth experiment determines if a mouse prefers to be between two walls (in an alley) rather than just alongside one wall. Short boards, the length of two black squares, are placed a mouse-width apart in the center of the activity board and the mouse started between them. Students record the position of the mouse as being in the alley or along the outside of the alley wall, or away from any wall, at each time


Make one slash mark each 10 seconds

Total

36 marks

Make a mark each time middle of mouse crosses a black line, for 6 minutes

minute	Group	Class total	Group	Class total
1	11	5	11	7
2		2		1
3	1	4	1	1
4		6	1	3
5		6		1
6		3		1

Figure 2 Sample data from the first part of the exercise.

check The results of the first four experiments (above) are tabulated by each group of eight students, then totaled for the whole class before having a class discussion on interpreting the results.

The data collected on the number of black lines crossed are compared between males and females (some teams use one sex, some the other) to determine the more active gender.

The data on defecations per minute, and groomings per minute, are compared for each minute interval in the first six minutes of experimentation, as a measure of nervousness and whether that goes up, down, or is constant for the 6-minute duration of the experiment.

Experiment Five

After finishing the first four experiments, social interactions are studied by having two mice on the activity board at the same time.

A mouse of the opposite sex is added to the activity board with the team's original mouse. At each 10-second "time check", some students record whether the mice are close together (in the same or touching squares) or are farther apart; another student records which sex is following which, if it can be determined, and others note which mouse performs various other social interactions such as sniffing the partner (and which part of the body is sniffed). In discussing class data, we separate data in which the visitor mouse was a male from data in which the visitor was a female.

Experiment Six

A second experiment in social interaction is similar to the previous one except that both mice are the same sex. Students then determine if the attraction, if evident, between the two mice in the first experiment was due to being of the opposite sex.

General Questions

We conclude the exercise by having the students speculate on the natural selective advantage of the behaviors they observed, with rather direct questions such as, "In the wild, which sex of mouse should be more exploratory and which more inclined to stay in one place?"; "Is wall-seeking learned or innate? What evidence do you suggest? What is its value to a wild mouse?"; "If you had to set a trap to catch a mouse in your house, would you set it at a wall or in the center of the room? Is your behavior in that regard learned or innate?"

Conclusion

Our students seem really to enjoy this lab exercise. It does not touch upon many aspects of animal behavior that we would like to teach, but it is so reliable that we prefer it over other alternatives we have tried.

Classes above the introductory level could easily adapt these experiments to include exercises in statistical analysis well beyond the simple comparing of means.

Literature Cited

Bielfeld, H., 1985. "Mice: Everything about Care, Nutrition, Diseases, Behavior, and Breeding." Barron's, Woodbury, NY.

Fredrickson, E., 1953. "The wall-seeking tendency in three inbred mouse strains *(Mus musculus)*." Journal of Genetic Psychology 82: 143-146.

Silverman, P., 1978. "Animal Behaviour in the Laboratory." Pica Press, NY.

Feminist Perspectives on Science

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Introduction

The idea for this article is the result of preparation for a course that Jeff Ramsey and I will be coteaching at Chicago during winter quarter on the history and philosophy of women in science. The course will consist of 5 sections. First, we will highlight some of the contributions women have made in science. Second, we will examine the blocks which have impeded women's participation in science: social, psychological, educational, and institutional barriers which have resulted in their low participation. Third, we will examine how science has viewed women. How have the fields of biology, psychology, and anthropology contributed to a particular idea of woman and woman's nature? How have particular findings changed these attitudes or been used to maintain an intrenched view? Fourth, we will then examine the feminist critique of this research and discuss various feminist epistemologies that have been suggested as correctives to the problems feminists see with the practice of science. Finally we plan to use primatology as a case study to explore the above issues in more depth.

Obviously it is quite impossible to deal with all these issues in the amount of time within one small article. Thus, I have decided to focus primarily on the just one issue: the feminist critique of science. I admit to being highly critical of much feminist scholarship and will mention some of the problems I have with it. Nevertheless, feminists have raised many important issues which transcend the issue of discrimination of women is science: such as the nature of the practice of science, the meaning of objectivity, and how science shapes our view of ourselves. These issues are of concern to broad areas of research within the history and philosophy of science. While feminists have not been the only ones to raise these issues, they have added an important and different voice to the discussion. Also I should say different voices, for as you will see, there are many different feminist critiques of science.

Would the practice of science be fundamentally different if all along it had been mainly practiced by women instead of men?

Before I go any further, I want to say that I am not a philosopher and it is Jeff Ramsey who will primarily be dealing with this aspect of the course. Not only am I not a philosopher, but I think its fair to say I even have had a strong bias against philosophy. Being trained originally as a biologist, I like many practicing scientists took a dim view of philosophers of science coming into science and telling me what the important questions are that I should be investigating. At best, I found most of the issues they raised interesting in an abstract sort of way. More often I found much of what they said plain irrelevant to my concerns as a scientist and now even as an historian of science. However, for the first time I have found myself interested in epistemological questions because of the issues that feminists have raised. Thus, what follows is a nonphilosopher's guide to feminist epistemologies, emphasizing the issues that I think will be of most interest to scientists.

Western society is a scientific culture -- that is science has permeated every aspect of our lives. Not only does it effect how we think about public institutions, about broad areas of research, from star wars to the war on cancer (notice the language in these two examples), but it has invaded the most private part of our everyday lives. From what we should have for breakfast, to how we should be raising our children, to what our sex life should be like, science has shaped our views. Today, only rarely does a scientist pursue questions of pure intellectual interest. Ninety-nine plus percent of all research today is expected to have applications to social projects.

Can science be enlisted to aid feminist goals?

Given this, feminists are asking a different question than most traditional philosophers of science. They are not asking "how do we know what we know?" But rather can science be used for emancipatory projects when science is so intimately bound up with Western, masculine, and bourgeois modes of thought? As the title of Sandra Harding's book; The Science Question in Feminism, implies, the question is not the woman question in science, i.e. why are there so few women in science, but rather the science question in feminism. Harding writes in her preface that science has "evolved from a reformist to a revolutionary position" (Harding, 1986). Initially the concerns were about discrimination : Why were women effectively barred from practicing science in any significant numbers? But relatively quickly the criticism moved to a different level. The very nature of the scientific enterprise was questioned. In its most radical form, the critique claims that the practice of science is not just sexist, but racist, classist, and culturally coercive -- that is, science represents the values of the West and of capitalism. Contrary to the claim that science is progressive, many feminists believe that it is used to maintain regressive social policies. In this larger context it becomes much more difficult to define good science. Is the problem bad science or science as usual?

Now as I mentioned, feminists are not the only ones to raise these issues. Struggles against colonialism, racism, capitalism, the counter culture movement of the 60s, the ecology and anti-military movements have also addressed the issue of use and abuse of science. Feminist criticism at its best incorporates the significant insights of these movements, but it also asks why specific feminist concerns have been such a low priority in the agendas of these other movements. I would argue that many of the feminists of the 70s came to their views as a direct result of the sexism they experienced as part of the new left move-ment of the 60s. The absence of feminist concerns in these movements has many reasons, but to discuss them would divert us from the specific issue of feminism in science.

At its most fundamental level feminist theory questions the whole definition of gender. Why and how is the division of labor determined by gender? How is our perception of the categories "natural' and "social" influenced by our definitions of gender? Gender difference is probably the most ancient and fundamental way we have of categorizing the world around us. Cultures may differ as to specific gender classifications, but nevertheless most cultures do assign a gender to various non human entities, for example, hurricanes, nations, and vehicles. Thus, one of the goals of feminist theory is to define gender as an analytic category -- that is to examine how gender is used to think about and organize social activity and to show that it is not a natural category resulting from sex differences.

To my mind, the fundamental problem with feminist epistemologies of science is that they seem to be trying to resolve a paradoxical situation. If we define feminism as a movement for social change and thus research should be guided by the goals of liberation and equality - how can we claim that such politicized research is going to be more objective, less biased than the research stategies it replaces?

Nevertheless, I think today no one can deny that much of science, particularily biology is inherently biased. When we read about flowers being raped, prostitution in apes, homosexuality in worms, one has to wonder about the objectivity of a scientist who would write up his research results using such language. When an experimenter extrapolates findings from insects to humans, or generalizes the results of experiments done on white European males to the entire human population, the validity of the results must be questioned. And this is just the type of criticism that was first made by feminists, known as feminist empiricism. They argue that the above is an example of bad science.

Indeed, Jeanne Altmann, a primatologist who is editor for The Journal of Animal Behavior found that when she demanded an operational defintion of terms that was applied accross the board, to both sexes, to young, juvenile, and adults, she found that terms such as rape, coy, prostitution, etc. virtually disappeared from the journal. She did not have to come out with an edict that said "Thou shall not use the term rape in a scientific journal article." There is no doubt that this type of criticism has gone far in improving the standards for research, particularily in the biological sciences. It also is the least threatening of the various feminist critiques because the basic methodology of science is not challenged. The idea that science is objective, value neutral remains intact. The problem is identified as bad science, not science as usual.

By instituting proper controls, improving methodology, correcting termininology such as removing sexist language one will be able to replace androcentric bad science with good science.

Feminist empiricists, however, do go somewhat further in that they claim that feminists (whether men or women) are more likely to produce unbiased results than non feminists as a group because feminists are more sensitive to androcentric bias and because of their enlarged perspective. Movements for social liberation "make it possible for people to see the world in an enlarged perspective because they remove the covers and blinders that obscure knowledge and observation" (Millman and Kanter, 1987). Thus, it is movements for social liberation that have increased the objectivity of science rather than the norms of scientific practice.

Martha McClintock, a behavioral endocrinologist, immediately noticed when she started doing research on the reproductive behavior of rats that the female rat had more of a role than to just sit there and allow the male to mount her -- Something that had been escaping her male colleagues for generations. As an undergraduate, she made a reputation for herself by showing that women's menstrual cycles become synchronized if they spend large amounts of time together. Now as she pointed out this was common knowledge to women who live in all female dorms. Some of her friends were totally amazed that she got a scientific paper out of this information. Yet male scientists were totally flabergasted and the paper documenting this finding with the appropriate statistical analysis appeared in *Nature*.

Feminist empiricists have faith that the basic methodology of science is powerful enough that it can correct any problems of bias.

Feminist empiricism thus has a somewhat contradictory stance. I think if Jeanne Altmann or Martha McClintock would allow themselves to be labeled, they would put themselves in the feminist empiricist camp. Nevertheless, they both admit without any qualms that they come to their respective disciplines with different perspectives as women and as women they asked different questions than their male colleagues. To my mind this is quite a radical statement, although I know they do not see it that way.

The Feminist Critique

The radical feminist critique goes much deeper than just attacking the use of sexist terminology or criticizing methodology. It not only claims that the methods used to test hypotheses and interpret results are flawed; it also claims the very framing of questions is fundamentally biased. What lines of research are considered viable and result in valuable pieces of information? What hypotheses are suggested, what accounts for evidence? Borrowing from Kuhn, the criticism is that observations are theory laden, theories are paradigm laden, and paradigms are culture laden. Just deciding what measurement to make, making it, and who makes it all influence the result. In this analysis there is no such thing as value neutral, objective facts. Implicit in this claim is that the researcher himself or herself influences the result.

Now even though I called this the radical feminist critique, one could argue that a version of this critique has many adherents among more traditional philosophers of science. Even in physics one can point to theories which specifically incorporate the problem of factors influencing the results. For example, the Heisenberg Uncertainty Principle states that the process of making a measurement perturbs the system such that it is impossible to simultaneously know both the momentum and velocity of a particle. Relativity theory, of course, addresses the whole issue of how one's frame of reference influences what one observes. Thus, most scientists would certainly agree that many factors influence the results one obtains. However, where many, if not most will part company with the feminists is the idea that ones sex, or race, or class, or nationality also influences what one observes. Feminist epistemology, thus, is an attempt to explain why who engages in the practice of science, in particular, mainly men as opposed to women has a profound influence on the nature of the scientific enterprise.

Feminist empiricism obviously has many strengths to it. Of all the varying feminist view points it is most concrete in its suggestions for change and identifying biases. But as I hope I've shown there are certain limitations to it. Although it acknowledges that women often do ask different questions, it does not really address the question as to why this should be so -- that is it does not deal with either the questions of why science can be considered masculine or how science can be used for emancipatory social projects.

The Feminist Standpoint

Although there are exceptions, most of these theorists make use of object relations theory and/or a Marxist analysis in their critique of science. Object relations theory is a modified version of Freudian theory that is used to explain how men and women come to have particular gender specific models of themselves, others, and nature. Nancy Chodorow, Dorothy Dinnerstein, Jane Flax, and Evelyn Fox Keller have been the principle theorists responsible for interpreting object relations theory within a feminist framework.

Briefly summarized, object relations theory claims that infants must separate and individuate from their primary caretakers who in most cultures are females. However, male infants must separate from the mother figure who is biologically different from him. He must "exercise will and control to not become socially a devalued woman" (Harris, 1986) Thus he must create a strong sense of separation and control. While girl infants must also individuate from their mother, they nevertheless will eventually become a person like their mother. Thus according to object relations theory masculinity is defined through achievement of separation while feminity is defined by maintaining attachments. Boys get strong messages for separation while females receive messages for merging. Quoting Chodorow (1978) "The basic feminine sense of self is connected to the world. The basic masculine self is separate." Object relations theory is used to explain among other things: militarism in males and the concept of nature as female and something to be exploited.

The radical feminist critique goes much deeper than just attacking the use of sexist terminology or criticizing methodology.

Keller argues that separation and autonomy are consonant with the values of competance and mastery which become linked with the idea of domination and control. These values become the norm for the practice of science and are also labeled as both progressive and moral. As Keller says, it takes no great leap of the imagination to see the emphasis of power and control so prevalent in the rhetoric of western science as a projection of male consciousness. However, she believes neither control nor domination are intrinsic to autonomy or to scientific knowledge.

Jane Flax asks: "What form of social relations exist such that certain questions and ways of answering them becomes constitutive of philosophy?" She claims that for philosophers the human dilemma consists of splits between mind and body, reason and sense or emotions, just the sort of split that males must achieve to have a masculine sense of self. Thus, what is seen as problematic for philosophy and by extension science is linked to males' sense of what it means to be masculine. Flax argues that since females do not have this dilemma in achieving their sense of self, a philosophy utilizing the female perspective will be superior because questions will arise from a sense of self as relational and reciprocal rather than dominating.

Gender is made, not born.

Nancy Hartsock also uses object relations theory to explain why adult females experience themselves relationally and concretely while males experience themselves more abstractly. She combines object relations theory with a Marxian analysis. But she questions the validity of the duality: mental vs manual labor. Women's daily concrete activity both at home and as a wage earner belie a simple duality of mental vs manual labor. The original Marxist analysis was incomplete because Marx did not build into his theory the consequences of the fact that making or raising a human being is much different than making a chair. Hartsock argues that specific intellectual skills become attached to increasingly separate spheres of public and private life. Therefore, intellectual qualities associated with gender do not have a biological attachment to men or women, but rather are a response to the requirement of public or domestic spheres of society. Again the message is the same. Hartsock claims that science and its conceptual tools were in the past constructed almost entirely by men and thus represent male cultural experience. Western masculinity in science is therefore a bias produced both by the over representation of both science and men in the public sphere.

Finally, Hilary Rose like Hartsock makes use of Marxist categories, but does not use object relations theory. For her science recreates the division of mental and manual labor, male and female labor. She claims that differences in the production of scientfic knowledge lie along the axis of craft vs industrial sciences with the sciences such as psychology, and anthropology being more open to women and organized along craft lines. She believes a more appropriate epistemology for the sciences is to be found in the craft labor embodied in these sciences. These sciences have shown that appeals to the subjective are legitimate and that intellectual and emotional domains can be united in good scientific research. Such research based on holism and complexity is as successful as that based on reduction and linearity. I will be exploring this claim in more depth.

Qualifications

Now I have to say I have a lot of problems with much of the material I just presented, but I will mention just a few. It is quite interesting to me that feminists have adopted object relations theory which is bascially a version of Freudian theory which to my mind is quite sexist in its interpretation of development. They seem to accept Freud's notion that as males learn they are different from females, that it follows that females will be seen as inferior. Now even if we accept this as true then where does this idea of female inferiority come from? It can only come from a society that is already based on patriarchial power relationships. Thus we need to look beyond pyschoanalytic theory and the family to find the sources of unequal power relationships.

On a more personal level, I am not a developmental psychologist, but I am a mother of a girl and a boy and as I watch my children grow I am not at all convinced that object relations theory is a very good description of what is happening. That is not to say that I think the patterns of development are the same. I guess it is somewhat fitting that as a feminist commited to nonsexist child raising, I have two children who embody many of the stereotypes of male and female behavior. I have a daughter, who is more of a mother than I am. This is a child who breastfed her stuffed animals when she was two. My son on the other hand was making car sounds when he was 3 months old. My daughter goes to sleep with all her stuffed animals, my son with a pile of trucks and cars. But on the issues of separation and autonomy that are such an intrinsic part of object relations theory, I do not see any significant difference. If anything, my daughter is more independent and assertive than my son.

A Marxist analysis to me is incomplete because it claims that the historical construction of gender roles accompanied the rise of modern market economies. Even the most superficial reading of history documents the existence of gender roles virtually from the beginning of recorded history. That in fact is perhaps the underlying question in feminist scholarship. If gender is not a natural category based on sex difference then how do gender roles come to be? All one has to do is read some Aristotle to realize that sexism in science didn't begin with the rise of capitalism.

One more theme which runs throughout the feminist criticism of science is how science at least since the 16th century has been described repeatedly in the language of sexuality and gender. Both science and medicine use sexual metaphors describing nature as a woman to be unveiled, unclothed, and penetrated by masculine science. This sexual metaphor is alive and well in high energy physics, particularly among researchers involved in building nuclear weapons. But rather than give you some of those examples, I want to quote from Richard Feynman's Nobel lecture. I think that Feynman is a perfect example of the idealized stereotypic view we have of a scientist: brilliant, witty, a very talented player of congo and bongo drums and working in theoretical physics.

> That was the beginning, and the idea seemed so obvious to me and so elegant that I fell deeply in love with it. And, like falling in love with a woman, it is only possible if you do not know much about her, so that you do not see her faults. The faults will become apparent later, but after the love is strong enough to hold you to her. So I was held to this theory, in spite of all difficulties, by my youthful enthusiasm... So what happened to the old theory that I fell in love with as a youth? Well, I would say it's become an old lady, who has very little left that's attractive in her, and the young today will not have their hearts pound when they look at her anymore. But, we can say the best we can for any old woman, that she has become a very good mother and has given birth to some very good children . Feynman, 1966.

Postmodernism

A third general category of feminist epistemology, postmodernism, is grappleing with the problems created by these other critiques. Sandra Harding, Elizabeth Fee, and Donna Haraway fall into this camp. Western philosophic traditions have tended to categorize thought as a series of dualisms: reason vs feeling, fact vs value, culture vs nature, science vs belief, public vs private. Reason, fact, objectivity becomes associated with rational discourse and scientific knowledge which became identified as masculine. Feeling, value, subjectivity became defined as irrational and unpredictable and identified as feminine.

Although these dualisms have been one of the primary targets of feminist criticism of the conceptual scheme of science, I think you can see from my discussion that these dualisms often appear in the feminist thinking about gender and sex. While Harding would ultimately like to eliminate gender, she also realizes that it virtually is impossible to totally separate the cultural and biological aspects of our sexual identities. Besides men and women are different and they have different experiences. Part of that difference is based in biology. How can feminism articulate these differences and whether these differences might have implications for social policy without feeding into the "biology is destiny" rhetoric? Harding reminds us "these dichotomies are empirically false, but we cannot afford to dismiss them as irrelevant as they structure our lives and our consciousness" (Harding, 1987).

Nevertheless, Harding as others are trying to get beyond these dualities. Elizabeth Fee claims that epistemologies of science developed by non Western societies have much in common with that of the feminists from Western capitalist societies. Conceptions of nature that in one context denounced as masculine are seen as European, colonial, white or bourgeois in others. Harding finds many similarities between African and feminist world views. But Harding warns us that to assume all Africans, let alone all colonial people share distinct ethics, world views is certainly a gross over simplification. But it is probably equally presumptuous to assume women share a common view as well. After all our identities are not just determined by sex, but race, class, and ethnicity as well. Yet one possible unifying theme is that the dominant/dominated power relations are reproduced within scientific knowledge. Science lets us understand, use, manipulate and control the natural and social world, but they do this my means of our human relationships. The scientist can't step outside his or her social personal and thus Fee (1986) claims that the "idea of a pure knowing mind is an epistemological conceit."

With biological determinism alive and well in a variety of disciplines, it is crucial that we make distinctions between culture and nature, gender and sex.

For me, one of the most interesting people working in feminist epistemology of science is Donna Haraway. For Haraway the whole question of good vs. bad vs. science as usual is misguided because it presupposes that feminists get the explanation right and better than the non feminists. For her, feminist science is about changing boundaries - what is not included in a category is as important as what is. For example, if the criticism is raised that science is used for regressive and destructive purposes -- i.e. why is most physics research concerned with making weapons of death and destruction rather than for peace time uses, the claim will be made that this question is not a scientific question, but one of social policy. But I would argue otherwise.

Feeling, value, subjectivity became defined as irrational and unpredictable and identified as feminine.

Scientists are an important lobby. They in large part do determine what kinds of scientific projects get funded. It is precisely because of this ivory tower attitude about the purity of science and its goals that allows research on weaponry, biological and chemical warfare to continue on the scale that it does. Thus, the practice of science does not just involve the solving of the technical problems to build a nuclear warhead, but the decisions that are made which say yes, this is a worthy scientific project and should be pursued.

If we change the boundaries of what we take to be science we change what needs to be explained. The actual historical and philosophic development of science is only one of a possible range of developments. We can't envision a feminist science because we don't live in a feminist society.

Conclusion

In summary, if we claim that the very asking of questions as well as the methodology is fundamentally biased, then we are left with some very basic questions about the scientific enterprise. What do we define as good science? Does the term feminist science have any real meaning? Would the practice of science be fundamentally different if all along it had been mainly practiced by women instead of men? Would physics and the reductionist approach to problem solving that it epitimozes be regarded as the - dare I say it - queen of the sciences -- the top of the hierachy. I don't think the previous discussion has unequivally answered these highly loaded questions, but it certainly has raised them and started to address them.

References

Chodorow, N. The Reproduction of Mothering: Psychoanaylysis and Sociology of Gender. Berkeley: University of California Press, 1978.

Fee, E. "Critiques of Modern Science," in Feminist Approaches to Science. (R. Bleier, ed.) Oxford: Pergamon Press, 1986.

Feynman, R. , "The Development of the space-time view of quantum electro-dynamics," *Science* 153: 699-708. Quoted in E. Fee, 1986.

Harding, S. The Science Question in Feminism. New York: Cornell University Press, 1986.

Harding, S., "The Instablity of the Analytical Categories of Feminist Theory," in Harding, S. and J. O'Barr (ed.) Sex and Scientific Inquiry. Chicago: Chicago University Press, 1987.

Millman, M. and R. M. Kanter. Editorial Introduction in Another Voice: Feminist Perspectives on Social Life and Social Science. (New York: Anochor Books, 1975). Quoted in S. Harding, 1987.

BIG SCIENCE, LITTLE SCIENTISTS

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Introduction

In the 20 February 1987 issue of Science four related reports appeared. (Selkoe, et al., 1987; Goldgaber, et al., 1987; Tanzi, et al., 1987; St. George-Hyslop, et al., 1987) They all concerned the amyloid proteins associated with Alzheimer's disease. I would like to compare two of these reports with respect to the research strategies employed. The two I choose are not the two closest in the objectives of their research. However, the reason for my choice will become apparent.

The issue of Science in question is the one in which a generation of "breakthroughs" in the understanding of Alzheimer's disease was presented to the public. The links with Downs' syndrome were firmly established, as was the finding that a major, if not the major, gene implicated in Alzheimer's disease was located on the long arm of chromosome 21 -- not a surprise given the earlier association with Downs' syndrome. Readers of Science over the subsequent course of 1987 will know that further refinements show the association of the two insults to be far from a simple one, and that there's a lot more work to be done. In any case, none of the authors pretends that the findings reported are in any sense a "final answer" to the problem of Alzheimer's disease. This, of course, does not take away any importance from the work I will discuss.

The two pieces of work I will compare are that of Selkoe and his associates, and that of Goldgaber and his associates. The major finding of the first group is that the amyloid proteins implicated in both Alzheimer's disease and Down's syndrome can be found not only in naturally aging humans, but also in other aged primates, and in aged polar bears and dogs. Thus, there may be non-human sources of experimental systems suitable for advancing our understanding of the function (and malfunction) of the amyloid proteins. The major findings of the second group are clear from the title of their report: "Characterization and Chromosomal Location of a cDNA Encoding Brain Amyloid of Alzheimer's Disease."

Even despite the differences in method naturally attendant on the differences in the two projects, they can be compared with respect to their more general choice of research strategies and procedures. A full comparison necessarily would involve a careful reading of the two papers, but, always remembering the limitations imposed, we can still see some useful and interesting differences between the two projects without a line by line comparison.

The Selkoe paper, and the project it reports, proceed through a series of subtle

Do laboratories with huge BUDGETS use these resources instead of their BRAINS?

inferences guiding each new step of the investigation. The carefulness of these inferences makes the progress of the research very economical. When a next series of assays is embarked on, this is because earlier results are examined and compared, and many potential next steps are ruled out. This logical progression of inferences makes the paper easy to follow. We are, as it were, given all the premises, and invited to evaluate the logic leading to each conclusion. Each conclusion is the choice of a next step in the research strategy. We always know the justification of this next step.

The Goldgaber report is a different sort of paper. It reports the results of having an army of technicians embark on a campaign consisting of literally thousands of assays using a mega-arsenal of reagents, measurements, and techniques. At each stage, the results of an assay are examined, and the thousands of inconclusive ones discarded. One success among thousands will push the research forward to the next stage. Now, this research was done at NIH. The budget was ample, the army of workers available, and the equipment and chemicals in abundant supply. These conditions led to success.

The first questions in a comparison of the two projects is "Could Selkoe and his group have gotten their results quicker or more efficiently if they had marshalled the same quantity of resources employed by the other group?", and "Could Goldgaber and his group have produced their results with a more modest outlay of resources?" These are questions I am not equipped to answer, and I leave them to those who have the relevant expertise. But if we are very clear that we are in no way questioning the "bottom line" success of the two programs -- both groups got their results -- we have a right to reflect on the way that scarce funds are utilized. Is there a tendency for laboratories with huge budgets to use these resources instead of their brains?

Perhaps in this case the research in the NIH lab was highly intelligent, and the massive barrage technique fully justified. We know from other cases that some research can only be done at great expense in laboratories commanding great resources. For example, Bruno Latour and Steve Woolgar (1986) show how the search for the molecular structure of TRF (TRH) reached a turning point at which everyone

The public has no idea if the science they support is efficient or economical.

recognized that only two laboratories in the world had a reasonable chance of completing the research. To give other laboratories an "equal chance" to pursue the research would simply have been a waste of money. But as citizens concerned with adequate and productive funding of research, we surely have the right to expect that our money isn't substituting for brainpower, and that scientists in hot pursuit of a research goal are not throwing apparatus at problems as a potential quick alternative to serious and careful thought. These worries are valid whether or not the case we are considering is a genuine case in point.

From the point of view of the ordinary person, this potential problem is made more worrying because of the knowledge that would be required to monitor research. Only highly trained biologists are capable of doing that. But these highly trained biologists are understandably concerned far more with pushing their own research to successful conclusion than they are with the efficiency of their colleagues -- especially colleagues who are getting results. The fact is that the general public really has no idea if the science they support is efficient and economical, and there is no obvious way this situation could be altered.

However, as important as the previous considerations may be, they are not the main point I want to make about the two reports. I am more concerned to address a question to biology teachers. Which piece of research would you like your students to adopt as a model for their future scientific work? Of course, this can be a question for all science teachers. Good science teachers everywhere, at every educational level, are engaged in a task that has many aspects. Surely two of the major aspects are teaching a respect for science and generating enthusiasm for doing science. It takes a lot of respect and enthusiasm to carry students through the years of study. The prospect of a well paying job, by itself, won't get them over the rough spots. There are lots of well paying jobs that require less hard work than it takes to become a good scientist.

In order for a teacher to create the respect and enthusiasm needed, there has to be something to respect and be enthusiastic about. Students needn't stake their lives on an eventual Nobel prize (though a depressing number of people seem to do so), but students do have to feel that somewhere down the line their own creative intelligent effort will pay off; that their ingenuity and other special gifts will win the day when others have faltered. In other words, pride must be a significant part of the motivation of every young scientist.

Students need to see successful cooperation in a research group is much more important than winning a competition with another group.

Good science teachers try to foster that pride. They move as fast as they responsibly can through the "cookbook" phases of laboratory work into phases that offer challenges to the creativity and intelligence of their students. They want their students to feel like working scientists just as quickly as they can. They want students to see that science has rewards and satisfactions that surpass those of some other activities. Perhaps, in these days of wall-to-wall athletic competition, they even try to get students to see that successful cooperation in a research group is much more important than winning a competition with another group. After all, for the good scientist, the establishment of a useful scientific result is the main point. Winning a competition is entirely secondary.

Now, I'm sure that the scientists in Selkoe's group feel justifiable pride in their results and in the skill, subtlety, and creativity it took to get those results. Perhaps the scientists in Goldgaber's group feel the same way. (see endnote). But in the latter case looms the specter of assembly line science. Much of the work was mechanical and routine. The chance for skillful thinking was minimalized. Perhaps it had to be so, but if so, then the scientific future offered to students threatens to be a very dull one as mechanized routine replaces intelligence, and as drones replace thinkers. It's not hard to see the day when almost all those trained as scientists are placed on the assembly line, and only the privileged few have creative control over the process of research. People worry a lot about the "alienation" of industrial workers whose jobs are so routinized and boring that they can no longer feel pride in what they do. Now this same prospect faces young prospective scientists.

Ironically, if such a situation comes to pass, the best science teachers will become the worst. For they'll create expectations and enthusiasm that will, more often than not, be frustrated. They'll prepare their students for career satisfactions that will be denied to them. Most jobs in science will be for second class citizens who have to look up wistfully at the few who do the rewarding things their science teachers told them *they* would be doing.

Now no one would deny that "big science" is here to stay. The questions scientists know how to ask get more sophisticated. The apparatus gets more sophisticated, as do the techniques. It is inevitable that the most costly scientific instruments will be concentrated in a few places. And it must also be said that even being a drone has its satisfactions if the hive comes up with a way to reverse the effects of Alzheimer's disease or finds a vaccine for AIDS. Nonetheless, for the sake of our students we must monitor the rise of big science in a critical spirit. We ought to be sure that if some scientific tasks are routinized, this doesn't condemn scientists to drudgery, but frees them up to do the challenging jobs that emerge. We ought to be very careful about how big science organizes its greatest resource, the intelligence and dedication of the young people who move into its ranks. It's far from obvious that the most efficient way to produce scientific results is to lock these people into a research assembly line.

Every good science teacher has a big stake in the way that big science becomes organized. The consequences of its organization will filter down into the classroom very quickly, affecting attitudes, morale, and expectations. If big science demands willing but unthinking workers, then the science teacher may have to forego the pleasure of watching students become excited about their own ability to do science. It may be counterproductive to encourage creative and imaginative thinking. If the students are headed for the life of the cookbook, then maybe training in the techniques of the cookbook is all that should be offered them. I certainly don't look forward to such a state of affairs, and I'm sure that many good science teachers join me in this attitude. If so, we have to think very hard about how we can shape the way big science develops.

References

1. Dennis J. Selkoe, et al., 1987, Conservation of brain amyloid proteins in aged mammels and humans with Alzheimer's diesease. Science 235, 873 ; Dimitry Goldgaber, et al., 1987, Characterization of chromosomal localization of a cDNA encoding brain amyloid of Alzheimer's disease. Science 235, 877 ; Rudolph E. Tanzi, et al., 1987, Amyloid ß protein gene: cDNA, mRNA distribution and genetic linkage near Alzheimer locus.Science 235, 880 ; and Peter H St George-Hyslop, et al., 1987, The genetic defect causing familial Alzheimer's disease maps on chromosome 21. Science 235, 885.
2. Bruno Latour and Steve Woolgar, 1986, Laboratory Life: The Construction of Scientific Facts. Princeton University Press, Princeton.
3. I certainly don't mean to be "picking on" this particular piece of research. Many other examples could be used for the same purpose.

STROBOMICROSCOPY: A QUANTITATIVE, NONINVASIVE BIOPHYSICS LAB

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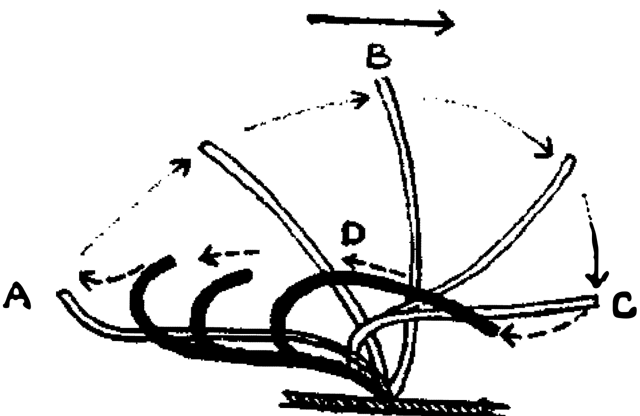
Introduction

Biophysics as an investigative methodology is an approach seldom used in undergraduate biology curricula. Though the principles of physics may not be essential to understanding all biological phenomena, they can provide new avenues for the teaching of biological principles. The biophysical approach provides upper level courses with new questions to ask about common biological functions and new methods of investigating those functions.

As undergraduates, we have discovered that an understanding of the physics of biological mechanisms can offer new insights into biological phenomena that we often take for granted. The research which we present in this paper was undertaken as a laboratory exercise for a junior/senior- level biology course. The noninvasive technique used in this project offers an alternative to the typical style of "grind and find biology" so often associated with undergraduate level biology courses. This work is well suited to the undergraduate level as it provides an easy method of gathering quantitative and qualitative data and it illustrates the often avoided fact that physics and biology are inseparable disciplines. The underlying objective of this experiment is to investigate the biophysics of movement at the microscopic level, particularly, the motion of cilia.

The rhythmic beating of cilia is a superb example of simple harmonic motion. (Fig. 1) The stroke of a cilium beat can easily be



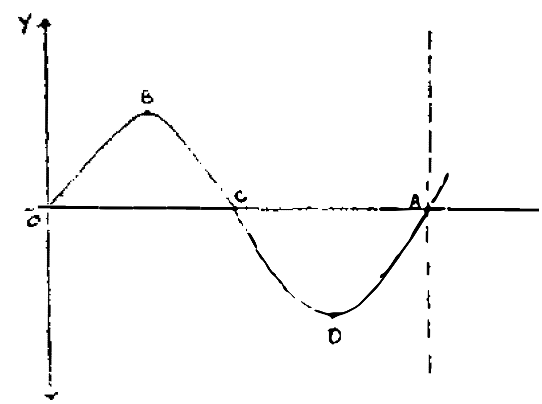


Figure 1
(*Left*) The beat of a typical cilium consists of a straight-armed effective stroke (white cilia), followed by a curling return stroke (gray cilia). (*Right*) A cilium beat described as a sine wave, [A] the beginning of the effective stroke, [B] the maximum extension of the effective stroke, [C] the end of the effective stroke and the beginning of the return stroke, [D] the maximum folding of the return stroke (From Satir 1974).

described by a sine wave. Position of the cilium in the cycle can be plotted against time on a Cartesian coordinate system without the need of trigonometry. These oscillations can be equated with the more familiar examples of a swinging pendulum or a spring. Derivations of complicated equations are not necessary to understand this natural cyclic phenomenon. However, simplifying the motion of cilia by relating it to a macroscopic phenomena obfuscates another reality about the motion of microscopic organisms.

For ciliated protozoa and the ciliated cells of higher organisms, water is actually a highly viscous fluid. The effort required for a cell to move one cilium through water has been compared to the effort required for a human to move an oar through molasses.

Biophysics can provide new avenues for the teaching of biological principles

Reynolds numbers, a measure of the relative effects of viscosity, density, size, and velocity, of materials, are significant to understanding microscopic life (Prucell 1977). This physical parameter, however, is counterintuitive and not so easily equated with every day phenomena. This difficulty, in itself, provides a valuable lesson. It is important to realize that aspects of life which we consider invariable may be infinitely variable at the microscopic level. Beside the difficulties of movement, the small size of these organisms also results in their being effected by the slightest of changes in their environment. Slight variations in temperature, light intensity or ion concentration can have drastic effects on the functioning of these organisms.

After some preliminary classroom discussion of the above considerations, we chose to investigate the effects of temperature changes on the frequency of ciliary beating as a way to study the biophysics of ciliary motion. We chose to work with the ciliated protozoan, *Stentor coeruleus* . The use of a protozoan precluded the need to dissect a frog or other large animal and by working with a whole cell, we were actually working with the whole organism.

Stentor coeruleus is a large, freshwater protozoan. These funnel shaped, single-celled organisms have longitudinal rows of cilia running the length of the body and an oral cavity encircled by several rows of cilia. The large cilia surrounding the oral cavity are easily observable under low magnification. The cilia on the surface of the cell are interconnected and move in a synchronized, oarlike stroke. There are two parts to the stroke: the effective stroke, in which the cilia is outstretched and moves somewhat diagonally, in an anterior-to-posterior direction; and the recovery, or return stroke, in which the cilia is bent over the body, a conformation which reduces water resistance as the cilia returns to its starting position (see Fig. 1; Satir 1974).

Experimental Design

To begin our investigation of the harmonic motion of cilia, we hypothesized that increases in temperature should result in an increase in the cilia beat frequency (CBF) which we defined as the time required for a single cilium to complete one oscillation. We proposed that CBF should increase linearly with temperature. Our intention was to cover a wide temperature range, beginning at near freezing and increasing the temperature in one or two degree increments until some upper limit, perhaps some state of hyperthermia, was reached.

To observe the anticipated changes in CBF it is necessary to somehow stop the motion of the cilia without actually interfering with the motion. Numerous investigators (Bradshaw, 1974; Cooper and Woolley,1982; Otter and Salmon, 1979) have shown a strobe light to be useful in determining the frequency of beating cilia. (Many students may associate a strobe light with timing a car and this too, can be used as an example of harmonic motion.) The pulse image which is created by the strobe light allows for easy identification of harmonic motion in the cilia. We observed harmonic motion in the cilia by adjusting the strobe frequency to the oscillatory frequency of the cilia.

The strobe produces flashes of light at specific intervals of time which can be regulated by a dial on the strobelight. The strobe measures the frequency of flashes as oscillations and records the oscillations as RPM's. The completion of one beat of a cilium is analogous to one oscillation of simple harmonic motion and, therefore, one period of a sine wave. The periodicity of the wave allows for easy stroboscopic analysis of the CBF by adjusting the flash rate of the strobe until it is synchronized with the beating cilia. When the flash and a cilium are in synchrony, the flash will illuminate that cilium at the same point in its motion in every stroke, the motion will appear to have stopped (Fig. 2). The CBF can then be read directly from the strobe setting. The harmonic motion of the cilia also presents the possibility of erroneous readings by synchronizing the strobe at too low a frequency. In this case, the flash of the strobe would actually be "freezing" every other (or every third, etc.) beat of the cilia causing the reading to be half (or a third, etc.) the actual value.

To produce the anticipated changes in the CBF we had to to build a microscope slide in which we could control the temperature of

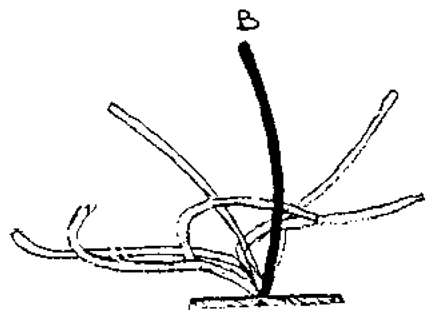


Figure 2
Synchronizing the flash with the CBF "freezes" the cilia, e.g. if the strobe flashes just as the cilium reaches position B during each beat, the cilium will appear to be stopped at that position.

the slide surface. We realized that we had to control the temperature of the media surrounding the stentor while it was on the slide. It was necessary that heat be transferred quickly to the media and that the desired temperature could be maintained and monitored.

Materials

Stentor

Cultures of *Stentor coeruleus* were obtained from both Carolina Biological Supply and Nasco Biological. The cultures were maintained according to manufacturers' specifications.

Experimental Apparatus

Two polyvinyl slides, 1mm thick, were separated 5 mm with glass spacers and sealed with G. E. silicon glue. In one side we placed two glass tubes to act as ports for water to circulate through the slide (Fig. 3). To provide a controlled temperature we used a circulating Precision waterbath (model R40, GCA). A flexible plastic tube was run from the waterbath to the exit port of the slide. A tube was run from the other port of the slide to a miniPump (model 2.C.96, Milton Roy Company). The pump circulated the water through the slide and back into the waterbath (Fig. 4).

On the surface of the slide we mounted a temperature transducer (AD590, Analog Devices) directly above the outflow tube (see Fig. 3). The temperature was displayed on a Data Precision Multimeter (Model 2480R) in degrees Kelvin to one decimal place. A correction factor was determined using a least squares fit to convert the reading to degrees celsius.

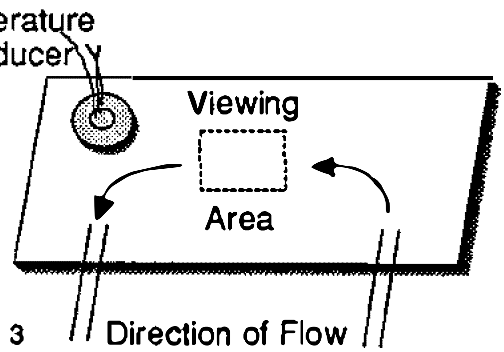


Figure 3
The Experimental Apparatus: The Slide and temperature transducer.

Methods

The stentor were magnified 100x under an Olympus dual headed microscope (model BH, Olympus Optical Company). Because the stenor were so active it was necessary to use as large a field as possible to be able to track them and still maintain focus on the cilia. The regular Olympus lamp was removed and replaced by the lamp of a Strobotac strobelight (model 1531, General Radio Company) (Bradshaw, 1974). The temperature of the waterbath and slide were allowed to equilibrate for 30 min prior to an experiment.

One experiment was defined to be all the readings taken on a single stentor. The individual stentor were isolated from the culture and placed on the slide using a capillary pipet. We were careful to place the media containing the stentor on the area of the slide that was exposed to the greatest water flowthrough. The media transferred with the stentor was kept to a minimum in order to keep the observational area small. The media was allowed to equilibrate on the slide for 5 min before any readings of frequency were taken. The dual headed scope was essential as the protozoans were highly mobile and it was necessary for one person to manipulate the microscope stage continuously in order to keep the stentor in the field. A second observer controlled the strobe frequency.

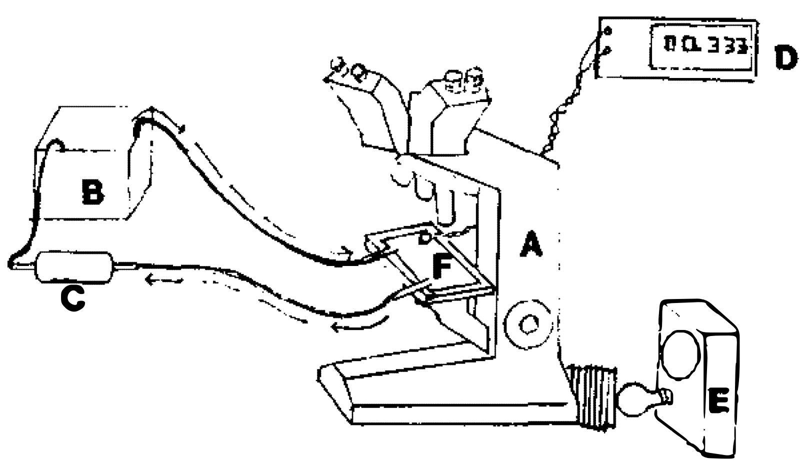


Figure 4
The Experimental Apparatus:
[A] the scope, [B] the waterbath, [C] the pump, [D] the DMM, [E] the strobe, [F] the slide

When the slide (and the media) reached the desired temperature, the strobe was adjusted to its maximum rate (6200 flashes/min), this was done to avoid erroneous readings at lower multiples of the actual CBF as explained earlier. The strobe rate was slowly decreased until the motion of the cilia appeared to stop. Frequency readings were accepted when both observers agreed that the cilia were 'frozen' (Fig. 5). At this point the temperature, time, and frequency were recorded. The water bath was then adjusted upward one or two degrees. In order to replenish the media lost to evaporation and to keep the drop small enough to maintain an even temperature, a syringe was used to add media to the slide as the drop became depleted. Readings were double checked in most cases to ensure that we were not reading a lower multiple of CBF.

Figure 5. Photograph: The "Frozen" Cilia of *Stentor coeruleus.*

In the first experiments, frequency readings were taken approximately every four degrees of temperature increase in order to get a general idea of the resulting change in CBF. Subsequent readings were taken in one to two degree increments. Our equipment was not accurate enough to adjust the slide temperature in increments less than one degree at a time, nor could we lower the temperature of the slide below 9°C. Therefore, to obtain enough readings from each experiment, it was necessary to raise the slide temperature above the normal limit for stentor. Unfortunately, most of our experiments ended with the death of the stentor. If the death occurred within 3 min after a reading was taken, that point was discarded. If the stentor died before three points were collected, that experiment was discarded. These measures were taken because we often did not realize that a stentor was dying until after a reading was taken and those readings could not be trusted to be an accurate representation of the effect of temperature on CBF.

Several control experiments were performed in which the temperature of the slide was kept nearly constant and readings were taken periodically over 1.5 hrs. These were done in order to determine the natural variation of ciliary beat frequency of individual stentor over a constant temperature.

Results

We tested a total of 36 stentor of which 13 experiments were discarded according to the criteria mentioned above. From the 23 remaining experiments, 125 data points were collected and these were plotted as a function of temperature (Fig. 6). Our results did not conform to our expectations of linearity at a statistically significant level ($p<=0.05$). We found that CBF increased

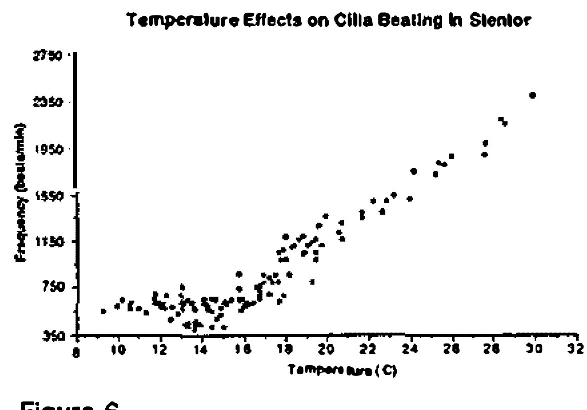


Figure 6
Cilia Beat Frequency (beats/min) as a function of Temperature (°C)

with temperature above 16°C. Below this temperature there was no correlation found between CBF and temperature.

We did not quantify, but did observe several temperature related behaviors of the stentor. The stentor appear less active at lower temperatures. Below 20°C, they spent most of their time attached to the surface of the slide. However, at some point they would release and begin swimming around the water drop. When the temperature reached approximately 25°C, a different behavior was observed. At this point, many of the stentor we observed again attached themselves to the slide and would begin to twist. In every case, a bulge would develop on the body, which would eventually burst and leak cytoplasm until the stentor totally disintegrated.

Discussion

We conclude from our results that the frequency of ciliary beating is associated with change in temperature of the surrounding media. Increasing temperature results in an increase in the frequency of the oscillations. However, our assumption of a linear relationship between temperature increase and increase in CBF was not substantiated. We have not explored the underlying mechanism of ciliary motion and therefore can draw no conclusions on the biochemical reasons for the effects of temperature on the frequency of oscillation. Other workers have suggested that increases in temperature also result in an increase of the amplitude of the oscillations (Eshel and Priel 1986).

Apart from the analysis of the data, we feel that this experiment was a success in many ways. Our hypothesis was simple, but testing that hypothesis required some imagination. To control the variables that we wanted to test we had to build some simple but specialized equipment. To collect the data required a tremendous amount of team work. Avoiding erroneous readings required an understanding of the physical principles involved. It became a lesson in the inexactness of scientific investigation, "freezing" the cilia was a sometimes frustrating exercise of repetitions and compromise. As for qualitative data, by observing the many different stentor under the microscope, it became apparent that there is much variation among single-celled organisms. We developed a true appreciation for the complexity and the inflexible limits of life at the microscopic level.

We want to emphasize the appropriateness of this lab for undergraduate level biology courses. The exercise is simple and straightforward. The ease of gathering the data and the quality of the data collected make it a satisfying experiment. It need not be set up with cookbook instructions, though some prelimary classroom discussion of the physical principles would be helpful. The strobomicroscopy method can be applied to other organisms as well, or it may be used to investigate other parameters which may effect the motion of cilia. These include the effects of changing ion concentration, magnetic fields or electric fields or the strength of the light source. Testing any of these other parameters would not require the same equipment which we built to test the effects of temperature changes, though, we believe that building equipment to suit the task should not be discouraged.

There are many cyclic phenomena in nature which are amenable to harmonic motion analysis. The advantages of studying cilia of a protista include the fact that protista, as a group, are under-represented in upper level biology courses. Usually, protista are only examined in introductory biology courses. Another advantage of using a protist is that by working with a whole organism the changes observed are changes of the whole organism. At this level of investigation other effects of the experiment may lead to more questions and further investigations. Pertinent information may be gathered by obseıving the behavior, or other seemingly unrelated phenomena associated with the manipulations of the ciliary motion, such as the behavior of the stentor at different temperatures which we observed.

We believe that it is important for lab exercises to be open-ended. Students should not have the feeling that there is a definite end-point which must be reached. Science does not work that way. Any question answered should necessarily lead to the asking of more questions. This experimental method can be offered to the student as a means of exploration. It is not difficult to understand, nor to put into operation and the simplicity of collecting the data leaves more time to appreciate the qualitative aspects of the organims being studied.

Aknowldgements

We would like to thank Dave Waller, technical assistant at Beloit College, for supplying the strobe light and minipump. We are also very grateful to Physics professor Dave Dobson for supplying us with the temperature transducer and the digital multimeter. And especially to Dr. John Jungck whose special insight and unconventional approach to the teaching of science has inspired and enlightened all of us.

References

Bradshaw-McNally, M., 1974, Harmonic motion portrayed by the beat of cilia. Senior thesis, Clarkson University, Potsdam, NY.

Cooper, T. G. and Woolley, D. M., 1982, Stroboscopic illumination for the assessment of hyperactivated motility of mouse speımatozoa." J. Exp. Zoo. 223, 291-4.

Curtis, Helena, 1968, The Marvelous Animals. New York: The Natural History Press.

Eshel, D. and Priel, Z., 1986, Spectral characterization of ciliary beating temperature dependence on spectral parameters. Biophysical Chemistry 25, 215-222.

Otter, T. and Salmon, E. D., 1979, Hydrostatic pressure reversibly blocks membrane control of ciliary motility in paramecium. Science 206, 358-61.

Patterson, David J., 1981, The behaviour of cilia and ciliates. J. Biological Education 15(3), 192- 202.

Purcell, E.M. 1977, Life at low Reynolds number. Am. J. Physics 45, 3-11.

Satir, Peter., 1974, How cilia move. Sci. Am. 231(4), 45.-53.

Tartar, Vance, 1961, The Biology of Stentor. Oxford: Pergamon Press.

USE OF A LOCAL INFORMATIONAL RETRIEVAL SYSTEM (DATATRIEVE) BY UNDERGRADUATES

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Introduction

Like many small colleges, our library lacks a full complement of primary sources for students to do in-depth reviews of the literature to support their term papers and senior theses. We had been instructing students in the use of BioAbstracts and Dialog. Also, our library faculty was doing an excellent job in demonstrating the use of alternative information sources. Nevertheless, students were somewhat hamstrung by their inability to actually secure primary sources. Further, many students were waiting weeks to receive an interlibrary loan, sometimes even when a biologist on campus was receiving the journal! With the advent of a DEC PDP 11/70 on campus in the late 1970's a system program called DATA-TRIEVE (DTR) came with it. DTR is an interactive program that allows information to be stored and accessed.

DTR comes with a standard demo program called Big Boats, which is a file of information on a hypothetical marina that sells large yachts. Information such as beam, sail size, cost and type are stored and may be retrieved for any boat or group of boats by any stored parameter. For example, a list of all yachts of a certain dimension, cost, and classification may be requested.

During a seminar on the new computer, we explored with DEC personnel the possibilities of constructing a DTR retrieval system unique to our needs and eventually adopted DTR with modifications unique to our needs, which included the following:

1) ease of access to students and faculty from any terminal on campus linked to the PDP 11/70;

2) secure, tamper-proof files; and

3) ease of data entry.

Following is a brief explanation on how our system operates.

The information we store in DTR consists of a series of coded descriptions of books, journals, student-produced papers, bibilographic lists of the computer searches of BioAbstracts (DIALOG) that have been done by Ripon students, reprints owned by the Biology faculty, etc. Each reference (no matter what it is) is coded into four information fields that describe the content of the item and its location. These four fields are KEYWORDS, NAME, VOLUME NUMBER, and ISSUE NUMBER.

Retrieval

The first field is a series of keywords known as the KEYWORD field. A KEYWORD field is exactly 75 characters long. If fewer than 75 characters are needed to describe the reference, the remaining spaces are filled with blanks automatically by DTR. We do not separate keywords by the use of blank spaces (etc.) as we felt that it would just waste space. If more than 75 characters are required to write the keywords which have been invented for the reference, then two separate entries must be made. In that case, each group of keywords has a few of the most important keywords held in common between them. Each KEYWORD field is linked to a series of three smaller fields that give information about the location of the reference. They are NAME field, VOLUME NUMBER field (VN), and ISSUE NUMBER field (IN).

The NAME field can be the name of the journal from which the reference came or the initials of the faculty member who owns the reprint. The NAME field is 10 characters long. VN is the volume of the journal or, in case of reprints, the reprint number of the faculty member who owns the reprint. The VN field is four characters long (0 through 9999 possible). The issue number is the issue number of the journal. The IN field is two characters long (0 through 99 possible). In the case of reprints it is simply a double zero. In the journal series Annual Reviews of (e.g., Ecology and Systematics) the issue number is the order number of the paper in that volume.

A complete DTR reference is 91 characters long (75+10+4+2=91). Three examples are shown in Figure 1.

Important Points of DTR

1) There are no spaces used to separate words. This is done to save space in the DTR file. (Although this saves space, it can cause some problems in doing DTR searches if running two words together creates another word [i.e., 'false hits']).

2) The keywords can be nested to some degree to save space. What is meant by nested is that the last letter or letters of one word can double as the first letter or letters of the next word. As an example (first reference above), note the nesting of ULTRASONIC and CONTROL as ULTRASONICONTROL.

3) The references in DTR are stored in upper case letters, but may be accessed by using upper or lower case letters.

DTR references are sequentially added to the DTR file so it continues to grow (at times daily). As of 1 December 1987 there were approximately 12,000 references in our DTR file. Since some of these are multiple inputs of a single reference (item), there are actually some number slightly less than that.

```
ULTRASONICONTROLMATERNALBEHAVIORPARENTALRODENTSINFANTS
  OUNDSLITTERATTUSN....AMER.ZOOL....19.2
DECOMPOSITIONLEAFLITTERASPENFORESTBIRCHCLEVEENERGYWEIGH
  TLOSSFOLIAGE........ECOLOGY......52.4
LEAFLITTERSOILARTHROPODSAVORYCRYPTOSPHERE................................R
  LW........77.0
```

Where periods (.) represent a null character.

Figure 1
DATATRIEVE retreival structure.

The Biology faculty believe that this system has substantially improved student term papers by providing them with easy access to a select source of primary literature that they did not have before. DTR is not without its problems, however. These include:

1) As the system is constituted, there is NO field for the author's name or page numbering. Both would be very valuable and could be done relatively easily if a new file were created.

2) During peak use of our system it can take more than 15 minutes to run a search. However, if few people are on the system, a search of all 12,000+ references can be completed in less than one minute.

3) Due to the interest of certain faculty members our references are slanted towards some rather specific fields, mainly ecology and invertebrate zoology.

We teach the use of DTR along with BioAbstracts, Dialog and other information retrieval systems in our writing intensive course Scientific Writing in Biology, a course that has been running for more than a decade.

Availability of DTR

Anyone who is interested in DTR should contact Dr. R. L. Wallace, Biology Department, Ripon College, P. O. Box 248, Ripon, WI 54971-0248

TRICHOMYCETES IN THE CLASSROOM

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Kearney State College
Kearney, Nebraska 68849

Introduction

Fungi of the class Trichomycetes are generally found living within the digestive tract of various arthropods. Probably due to this inconspicuous habitat and also to the lack of publicity, few instructors of the Biological Sciences make use of these organisms in the classroom. It is generally necessary to dissect the host digestive tract to observe them; however, this skill can easily be developed with a minimum of practice. In a very few species, a portion of the fungal thallus may protrude from the anus and can be observed under low magnification without host dissection; however, this is the exception.

These fungi are obligated to live with their respective hosts and have not been found living under natural conditions in nature away from the host. These fungi, for the most part, obtain their nutrients from the gut contents and the majority of them are considered to be commensals.

Arthropod hosts for these fungi include terrestrial millipeds (Diplopoda), pill bugs (Isopoda), crayfish (Decapoda), springtails (Collembola), Mayfly larvae (Ephemeroptera), stonefly larvae (Plecoptera), midge, mosquito and blackfly larvae (Diptera) and others (see Lichtwardt, 1986 for a complete list). For initial classroom use, I would suggest blackfly (Simuliidae) larvae as they probably have the greatest likelihood of infestation.

These larvae are often found in flowing waters attached to rocks in riffles, or on sticks or plant parts. A pair of forceps (not sharp) may be helpful for removing the arthropods from their substrate. They should be placed in collecting jars with a small amount of water and placed on ice as soon as possible. They may be maintained in the laboratory by placing them in petri dishes approximately one-third filled with water and storing them in the refrigerator.

Living hosts provide the best trichomycete specimens as a fungal decay process begins soon after death of the host organism. Preservatives often distort the fungi and also make host dissection difficult, so their use is not recommended.

Methods

Dissection of blackfly larvae will require use of a dissection microscope, preferably with variable magnification. The host specimens may be placed in a drop of water on a glass slide or in the bottom half of a petri dish. Fine tipped, quality jeweler's forceps and micro dissection needles will be needed to complete the dissection.

The anal segment and head are removed with a sharp razor blade or scalpel. The gut may be removed from the posterior end of the body and moved to a fresh drop of water. The transparent peritrophic membrane (midgut) will often come with the gut but in some cases may slide out the anterior end of the body when the head is excised. It is often full of algae and other food materials. These can often be removed by grasping one end of the membrane with forceps and lifting it out of the water several times. The transparent membrane may be observed by placing it in a drop of water on a slide with a coverslip for microscopic examination. (If one has difficulty with the gut sticking to the forceps try drawing them between your fingers to coat them with body oil before starting the dissection.)

These fungi are relatively transparent and are best viewed using a phase contrast microscope. If an ordinary light micro-scope is used, remind students to reduce the light intensity. Addition of the common fungal stain and fixative lactol-phenol with a small amount of cotton blue stain will increase the contrast and aid the location of the fungi. Fixatives often somewhat distort the fungi; therefore, most measurements given for species descriptions are made in distilled water mounts while the fungus is in a living condition.

The trichomycete *Harpella* spp is the only genus reported from the peritrophic membrane of blackfly larvae and has been found in nearly all sites which have been investigated. This fungus is non-branching with a simple holdfast at the base by which it is attached to the gut lining. From 2 to 10 curved to coiled spores are formed as the thallus matures . These spores produce, usually, 4 fine appendages which in some cases may be seen coiled within the generative cells (see Moss and Lichtwardt, 1980). As the peritrophic membrane grows from the anterior end, the most mature fungi will be found towards the posterior of the gut; however, zygospores are very rare.

Harpella spp has not yet been grown in axenic culture - perhaps this would be a challenge for the most enterprising of Students.

The hindgut is lined with a noncellular material composed primarily of chitin and is a location where several different species of trichomycetes may be removed by grasping one end with forceps and pulling it between a second pair of partially opened forceps. When the epithelium is removed, mount the hindgut in water and add a coverslip. If the gut contains excess debris it may be opened up by using minute needles or flushed out as described above for the peritrophic membrane.

Several species (i.e., *Smittium, Pennella, Simuliomyces* and *Paramoebidium*) may be present in the hindgut (see Lichtwardt, 1986 for identification). The hindgut is shed with each larval molt and, therefore, must become reinfested following ecdysis. This is perhaps a reason why the hindgut is less likely to be infested than the peritrophic membrane.

The hindgut of mosquito larvae may be infected with *Smittium* spp, however not all populations are infected. For classroom demonstration I would suggest checking the mosquito larval population before class use. The larvae are relatively easy to dissect involving only decapitation with a razor blade and removal of the hindgut by pulling the anal area from the body with forceps. The epithelium must be removed as described for black fly larvae.

If infested mosquito larvae are found other larvae in the same confined container will often also become infested within 24 hours. (See Williams and Lichtwardt, 1972). Members of the genus *Smittium* have been axenically cultured and students might be encouraged to culture these from mosquito larvae. (See Lichtwardt, 1986 for techniques).

Conclusion

Students who are reading ecological studies on aquatic organisms which might contain trichomycetes in their digestive tract could see if the possibility of fungal effects on the host organisms is discussed in these papers. They should also be reminded to consider potential biological associations anytime that living systems are used for ecological investigations. In summary there is a fascinating and diverse group of organisms living within the guts of arthropods which can be used to stress interrelationships of the biological world. When mosquito, black fly or midge larvae are in your field collections will you and your students be tempted to investigate their digestive tract? Perhaps a new species or even an undiscovered pathogenic species awaits discovery.

References

Moss, S. T. and R. W. Lichtwardt, 1980. Harpella leptosa a new species of Trichomycetes substantiated by election microscopy. Canadian Journal of Botany 58:1035-1044.

Lichtwardt, R. W., 1986. Trichomycetes: Fungal Associates of Arthropods. Springer-Verlag. New York. 343 p.

Williams, M. C. and R. W. Lichtwardt, 1972. Infestation of Aedes aegypti larvae by axenic cultures of the fungal genus *Smittium (Trichomycetes).

MEETINGS:

The 15th Annual Undergraduate Research Symposium in Biology
St. Mary's College, Winona, Minnesota 55987 — 22 April 1988
Contact: Dr. David R. McConville, Department of Biology
(507)-457-1542

Council on Undergraduate Reseach (CUR) — 13-14 July 1988
"Undergraduate Research - Funding, Operation, and Role in Faculty Recruiting" --- Carleton College, Northfield, Minnesota
Contact: CUR-Carleton National Conference, Chemistry/Geology Office, Mudd Hall, Carleton College, Northfield, Minnesota 55057

AMCBT Annual Meeting, Beloit College, Beloit, Wis. — 22-24 Sept 1988
Contact:Cathy D. Hunt, Department of Biology,Henderson Community College, Henderson, Kentucky 42020

Illinois Association of Community College Biologists Annual Meeting
Contact: Jerry Hinkley, College of Lake County, — ???2-4 Oct 1988
19351 W. Washington St., Grayslake, IL 60030 — (312) 223-6601 ext 322

NABT Annual Convention, Chicago, Illinois — 16-20 Nov 1988
Contact: NABT 1988 Convention, 11250 Roger Bacon Drive, #19, Reston, Virginia 22090 (DATE FOR ABSTRACTS: March 15)

Notes from the Executive Secretary

Inasmuch as this is the first issue of the New Year, I would like to take this opportunity to wish all of you a very productive and satisfying new year. With this issue, I would also like to welcome John Jungck as the new editor of *Bioscene.* John has been a member for many years (since 1967), presenter of many computer programs at Annual Meetings, and also a former editor of *The American Biology Teacher.* I'm sure I speak for all of the membership when I say, "Welcome to *Bioscene,* John."

Still on *Bioscene,* but on a different tack, I would like to express my, and I'm sure all of the membership's, deepest thanks to Bill Doemel, the outgoing editor. All of us owe Bill a great deal for the hard work he did in changing the format of *Bioscene* as well as greatly improving its content. Many thanks, Bill.

Those of you who would like to see your names in print, as well as having your writings published, send them on to John Jungck (Dept. of Biology, Beloit College, Beloit, WI 53511). He will be more than happy to work with you on getting your gems into *Bioscene.*

The Steering Committee met the weekend of December 12th to go over any business deemed necessary for AMCBT, as well as to plan for the 1988 Annual Meeting. Having attended numerous of these meetings, I feel that this one was quite productive. Several items are noteworthy at this time, and I will go into them in a little more detail.

Bill Andresen, our President, did a limited survey of some members and non-members regarding their perceptions of AMCBT, which he presented at this meeting. One result was something that many of us have known for some time, namely *our organization is not as well known as we would like.* Bill is going to develop a marketing strategy to put us before more of our colleagues and increase our membership. This will result in some changes with regards to our state membership chairs which are not completely worked out. We will be in touch with these individuals in the near future.

Another result which seemed to jump out of the responses was the need for us as a group to carry on year-round what we do best at the Annual Meeting, namely act as resources for one another. Consequently we are going to try to set up a Resource Bureau, and for this we need your assistance. I know that we are all professionals, and that we are experts in one or more areas of biology, but how willing are we to share this expertise with others? Can we put your name down, at least at first in the computer and then as quickly as possible in some type of brochure, to help someone who has a problem with a topic, a course, a piece of equipment, computer software, etc., in short almost anything that would relate to the teaching of biology, even including some of the administrative skills which many of you have acquired in flying departments by the seat of your pants? These are the kinds of things that get talked over and talked about between sessions at the Annual Meeting and which can make such a difference for some of us in our own endeavors.

To start this, when I send you your annual dues notice at the first of the year, I will have some questions for you to answer on the form which you are to return to me. The form will also include a request for some phone numbers, both home and office. Please note, these numbers are to be used only by this office and will not be released to anyone (unless you have agreed to be a resource person). We do not give your telephone numbers to anyone. I hope that you will all seriously consider participating in this venture.

With respect to the Annual Meeting, at this point the program is in the process of being planned. The meeting will be held from Thursday the 22nd of September through Saturday the 24th of September at Beloit College, Beloit, WI. If you would like to participate in any way yourself, or if you have a name to suggest, or some format suggestion to make, please relay this information to Cathy D. Hunt, Department of Biology, Henderson Community College, Henderson, KY 42420, the program chair.

One note about an innovation in the meeting introduced at the Springfield, MO meeting, namely the morning breakfasts included in the registration charge. Comment from those attending was very positive and encouraged the continuation of this practice, so look for it again next fall.

In looking forward to the next meeting please remember that we will again be electing officers, a president-elect, two steering committee members, and a secretary. If you are interested in standing for election to any of these offices, or have in mind the name of someone you feel you would like to see in any office, please send your or their name to the chair of the nominating committee, Ann Larson, Department of Biology, Sangamon State University, Springfield, IL 62794-9243.

While you're in the nominating mood, you might consider nominations for Honorary Life Membership. AMCBT has honored some of its members with this designation and is willing to do so again, although there is nothing in our procedure or constitution which says we must do so. It is not an automatic award for lengthy service, but rather an award for significant contributions to AMCBT and to the teaching of biology--the critical part of our name. If you wish to nominate someone for this award, please contact Joseph Kapler, Department of Biology, Loras College, Dubuque, IA 52004-0178, chair of the Honorary Life Committee, for further infonnation about this procedure.

At this point, having violated a basic rule of mine regarding overlong communications, I am going to stop. Again, I would like to wish all of you all the best for 1988.

Edward S. Kos
Executive Secretary

Letter to the Editor

Dear Editor:

Early last year I was in the middle of checking the species of plants that were in flower in a local city park. This was one of a series of visits every three days for a phenological study I've had going since 1984.

I suddenly stopped, stamped my foot hard and said, "Damn!", with vigor. I was alone, but to me it would have made no difference if all the members of the forest had ears. I was not happy with the situation. Here was a valuable natural resource, a park with over 200 species of vascular plants, the majority native and wild, and no one gave a damn except me. I've tried to interest local grade school and high school biology teachers and offered my help. Besides the public school system there is a good Lutheran system K-12. None of the buildings for either were more than two miles away. Zero!

What's more, I've talked to local women's groups, Rotary Club, church groups, and the local historical society. "You have beautiful pictures of flowers."

Oh, yes, occasionally the local city council has talked of selling off part of it for a condominium. After all, it's valuable lakeshore property.

And that is only part of the story!

I had just received my copy of AMCBT's *Bioscene*. There was not a single field biology oriented article in it. I even very softly swore, just a little bit, at Bob Buchholz because he was given credit for unearthing some of the articles for that issue. Since we are (were?) friends, I chose my vocabulary very carefully. I checked my *Bioscenes* back for five years and could find only four articles of a field nature. Have we all forgotten that biology did not begin in the laboratory. What on earth are your students led to believe? Is all that biology amounts to consist of genes, bacterial cultures, comparative anatomy, and those subjects to be studied in a nice, dry, air conditioned laboratory? It's no wonder so few are environmentally conscious.

I'll have to admit I was encouraged when I learned that the theme of our '87 annual meeting was "biodiversity." Now all I'm hoping is that some of this year's program will penetrate the course work taught back home.

If our students and potential teachers don't get this from you, where will they learn of this vital information?

The battle to save some of our biological resources from destruction is only slowed, not won, and not even that in some parts of the world.

Russel O. Wagner
University of Wisconsin-Platteville
Retired

P.S. This was prepared to squeeze into the Springfield meeting, but an opportune moment did not seem appropriate. Also, in Lake Mills the Boy and Girl Scouts have shown a token interest.

Minutes of the Editorial Board of *Midwest Bioscene*

The editorial Board of the Midwest Bioscene (AMCBT) met October 10, 1987 at Southwest Missouri State University, Springfield, Missouri.

Dr. John Jungck accepted the position of Editor of the Bioscene.

John can cover editing and layout of the Bioscene. However, due to higher costs at Beloit, the printing must be done at the Central Office of AMCBT.

John requested that articles be submitted as early as possible.

The board members discussed the areas of publication and each member signed a sheet indicating the areas they felt most comfortable with in soliciting articles.

It was decided to divide the Bioscene into two areas: 1. articles and 2. communication, curriculum, etc.

The following questions arose:

If articles are published in Bioscene can they be published elsewhere? John indicated that this had been done in the past.

Can articles previously published elsewhere be published in Bioscene? John indicated they should first be modified.

Does a person need to be a member of AMCBT in order to publish in Bioscene? No articles can be accepted from nonmembers.

A discussion followed in which suggestions for soliciting articles of a non-biology nature came from the board members. Some suggestions included review of textbooks, new courses, heart transplants, etc.

The members of the editorial board should continue to solicit articles for the Bioscene throughout the year. Our new editor, John Jungck, is very capable and experienced and we need to keep articles coming his way.

Bill Doemel is to be commended for his fine work as past editor.

Respectfully,

Ray Reed

Thirty-Second Annual Meeting

Association of Midwestern

College Biology Teachers

Of interest to all of us as we endeavor to find new ways to teach the essential concepts and essence of college biology and direct the preparation and development of primary and secondary teachers in this years theme: "Influencing Excellence and Innovation in Science Teaching."

As John Jungck and I prepare this Beloit, Wis. (convenient jaunt from Chicago) Sept. 22-24, 1988 program we are planning speaker and workshop highlights on topics such as "concept mapping," "conceptual learning," and "critical thinking." Field trips, three-hour workshops and film festival will be highlights of this year's meeting.

Plan now to attend. Better yet, plan now to participate. Return the Call for Papers form and be listed on this year's program. I am looking forward to hearing from you.

Cathy Hunt

1988 Program Chairman

Return to:
Cathy D. Hunt
Department of Biology
Henderson Community College
Henderson, Kentucky 42020

CALL FOR PRESENTATIONS

1988 AMCBT PROGRAM PARTICIPANTS

Suggested Areas of Focus:

Labs that work, Teaching methods that work, Training Teachers, Retraining Teachers, Writing Skills, Software Exchange, What's New/What's Hot

Application to Participate

Deadline: April 15, 1988

Name ______________________ Institution ______________

Address ______________________ Phone ______________

Check one _______ Oral Presentation (45 min. including discussion)

_______ Poster Session

_______ Other (specify) ______________________

Title of Presentation: ______________________

____

Abstract: (Please include a two or three line explanation of your presentation) __________

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Application for Membership

ASSOCIATION OF MIDWESTERN COLLEGE BIOLOGY TEACHERS

NAME ______________________________ DATE __________

TITLE ____

DEPARTMENT ____

INSTITUTION ____

CITY ____________ ______________ STATE _____________

ZIP CODE ______________

ADDRESS PREFERRED FOR MAILING ______________________

CITY ____________________________ STATE ____________

ZIP CODE ______________

PHONE NUMBER _______________________________________

MAJOR INTERESTS:

( ) 1. Biology

( ) 2. Botany

( ) 3. Zoology

( ) 4. Pre-professional

( ) 5. Teacher Education

( ) 6. Other

RESOURCE AREAS:

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RESEARCH AREAS:

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____

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Have you been a member before? _______ If so, when? ________________

Mail To

Edward S. Kos
Executive Secretary, AMCBT

AMCBT Central Office
Department of Biology
Rockhurst College
Kansas City, MO 64110

CURRENT DUES ARE $10.00