The Boulder and the Sphere: Subjectivity and Implicit Values in Biology

PETER ALPERT

Department of Biology
University of Massachusetts
Amherst, MA 01003-5810, USA

ABSTRACT: Science is inherently subjective. The experience of dissertation research in ecology showed how intuitively derived hypotheses and assumptions define the questions one asks and the variables one measures, and how idealised forms and generalised types facilitate analysis but distort interpretation. Because these conceptual tools are indispensable to science, subjectivity is ineluctable. This has moral implications. Scientists are responsible for the particular abstractions they select and must therefore accept some moral responsibility for the way their results are used. Those who use scientific results have an equal responsibility to acknowledge the significance of the methods and not just of the conclusions. In biology, subjectivity may also have a positive side. A wide consensus of ecological biologists accept, on the apparently neutral grounds of accumulated study, a set of generalisations that society at large treats more as philosophical beliefs. This category of implicit values in biology holds much promise for improving our relations with nature and each other.

KEYWORDS: biology, values, subjectivity

INTRODUCTION

The reliance of society upon science and the regulation of science by society have come to rest on the notion that science is uniquely objective and morally neutral. This notion is false, and to accept it leads to a mistaken relationship between science and society. The practice and even the theory of science are inherently subjective. Moreover, this subjectivity, rather than comprising merely the residue of human fallibility, may reflect a specific set of values.

These points bear upon the relationship of science and society in two main ways. First, when scientists are consulted on matters of government or social policy, the evidence that they present, however dry and factual its form, will always contain assumptions and approximations made on the basis of personal

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perspective. To take such evidence as simple fact, without explicitly examining its subjective side, ultimately discredits science and flaws policy. Second, society ought to allow science more of the creative license that it accords, at least in principle, to art. The values of science can exercise a benign effect on society, but only if science is treated as something more than just a technical tool. For example, to demand that scientific studies direct themselves only to specific societal concerns and produce immediate practical results will deprive society, not just of the important discoveries which are made incidentally, but also of the humane point of view with which science can temper all of social life.

This argument applies with some justice to all the disciplines of science. It holds particular relevance to the natural sciences, those which deal with relatively quantifiable phenomena that would take place with or without the benefit of human consciousness, and which especially enjoy, or suffer from, an undeserved reputation for amorality and unappealable truth. It may apply most strongly of all to biology. Perhaps because biology concerns itself with life, the most ineffable of phenomena, its practice is heavily patched with craft and imbued with a viewpoint from which society can greatly benefit.

My personal belief in this was the product of some four years of graduate research in plant ecology. I would like to speak from that fledgling experience about the methods and concerns of ecological biology as I encountered them. Molecular biologists and other natural scientists may find that their work has brought them to a different view. If so, I hope they will express it. The centre of my graduate research was my dissertation work on the physiological ecology of mosses in the chaparral of southern California. Most of the following discussion deals with how the field research for my thesis revealed a subjective side of biology, one which deserves explicit comment more than censure and is in any event indispensable. At the end, I will briefly list some of the basic perspectives in ecological biology which, I feel, help guide biologists through the subjective judgments the science requires. These attitudes offer guidance for the governance of human activities as well, and in this sense comprise a set of ecological values.

A HYPOTHESIS

My dissertation work began in a graduate course on bryophytes, where we learned that some mosses had the surprising ability to dry up without dying, or, in the vernacular, to tolerate desiccation. Even after years without water, kept in a state as dry as properly stored baking flour, some mosses begin to revive immediately upon rewetting. This seemed to me to equip these plants handsomely for life in arid habitats. Yet, as everyone knows, mosses are rare in deserts and brushlands. After reading around the subject, I formed a hypothesis, and so entered subjectivity.

My hypothesis, that it is specific cycles of drying and wetting in arid habitats which cause mosses to starve to death for carbohydrates, came neither from deduction nor induction. I thought it up. Having done so, I established a viewpoint which influenced all the research to follow. This sort of prejudgment might appear to depart from the unbiased search for truth. However, without an initial point of view, I would never have accomplished much of anything.

CHOICES AND ASSUMPTIONS

Thompson (1965) characterised science as the description and discovery of trivialities. Any particular phenomenon is by itself a minute detail of nature, and all important and useful results thus originate in the pursuit of minutiae. The trick is to pick, from the enormous selection of available trivia, the one which will lead to an important result, that is, a minutium of general significance. A hypothesis acts as an organising principle, assigning relevance to certain potential experiments and unimportance to others. For instance, I hypothesised that the response of mosses to cycles of drying rather than to any single drought explained their distribution. Once I had shown that they could survive the longest single drought likely to occur in their geographical area, I had no interest in further discovering just how long a drought they could survive. This alone saved years of waiting around for mosses to perish in packets on shelves.

Having picked an experiment, the investigator faces a second choice, the selection of crucial factors. Doing the same thing over and over again is an essential of science. One must show that the same actions produce repeatable results. Exact duplication of one’s actions being impossible, one has to decide which factors to try to keep constant, which to measure to check that they are, and which to disregard. One also has to decide what degree of error or uncertainty to accept in each of the important factors. These decisions depend upon one’s point of view and hypothesis.

One very simple technique required for studying the mosses was the determination of tissue water content, as grams of water per gram dry mass of moss. This involves weighing a moss sample, drying it in an oven, and weighing it again. In order to deal accurately with small masses, less than 0.05 grams of moss, it was necessary to prevent any exchange of water vapour between the air and the moss, which absorbs and loses water rapidly when given the opportunity. This was accomplished by collecting each sample in a vial, immediately corking the vial, weighing the whole assembly, uncorking and drying the vial and sample, recorking the vial as it emerged warm from the oven, letting it cool, and weighing the whole thing again. To assess the validity of this technique, I did some preliminary tests. I checked that corked mosses did not change in weight, and compared the measured water contents of a group of replicate samples, which I expected to be identical. I reweighed samples after drying for different lengths of time, to be sure my standard drying period would allow them all to come to equilibrium; and reweighed them after drying at a higher temperature, to be sure my standard temperature (80° C) left them with no more than 0.1% of their saturation water content (the smallest amount I could measure with my balances and the sizes of my samples). When the results met my expectations, within the sort of variation I judged tolerable, I accepted the technique. I disregarded such possible factors as size of cork, shape of vial, and amount of sample because I had no reason to expect that they would affect water content, the dependent variable in which I was interested.

As I used this weighing technique over the next two years, on thousands of samples with various balances and brands of corks, I kept an eye out for any novel inaccuracies. If I had been happy to remain forever a graduate student, I could have performed a new set of preliminary tests for every possible variation in conditions. Instead, I compared what I got with what I expected in each case. I learned that days of high humidity tended to raise the weight of the corks a little, and that electronic balances become more variable during rush hours, when people with paid jobs were turning off the lights at work and turning them on at home. This humble history exemplifies what might be called proof-of-the-pudding reasoning. One has cooked up a set of data using an experimental protocol. If the results taste alright, the recipe must be acceptable. There exist standards of taste, just as a chef has only a certain discretion to vary a soufflé. Within those standards, much is left up to the individual palate, and the palate is prepared by expectations.

Safeguards against poor or arbitrary chefs have been built into biology in the form of peer review of articles submitted for publication. Before accepting an article, the editor of a journal will send the manuscript to a few scientists he considers to be authorities on the subject. He will generally accept their recommendations for rejection, revision, or acceptance. Biological results published in standard journals are thus professionally taste-tested. Even so, the chances are that no reviewer would have bothered to question my technique for determining moss water content, in the unlikely event that an editor would have allowed me to freight a paper with a description of it. Only if another researcher decided to repeat my results or to adopt my method for further studies would a flaw be discovered by anyone but myself.

Similar to the craft of overlooking presumably unimportant factors is the craft of making assumptions. In his statistics course, which I attended in the same year as the bryophytes course, Richard Lewontin related assumptions to hypotheses as follows: an assumption is a hypothesis that one is not prepared to test. He cited the study of genetic versus environmental influence in humans. By comparing the similarity in behaviour of pairs of biological siblings raised together to that of pairs of children raised together in which one child is adopted, one can attempt to test the hypothesis that there is a genetic component to human behaviour. A necessary assumption is that adopted and biological children are treated alike. One could as fairly use the same experiment to test the assumption and assume the hypothesis, that is, to test for differential treatment of adopted versus biological children assuming that there is no significant genetic influence on human behaviour. The interpretation depends on the attitude of the experimenter.

PROOF

Like hypotheses, assumptions are proved in the eating. In selecting an experimental system to test my hypothesis that desiccation tolerant mosses are absent from dry places because of failure to maintain a favourable balance of carbon uptake and loss, I looked for a situation in which other potential factors appeared unlikely to be involved. I settled on a small group of species inhabiting soilless granitic boulders in a single gully, where there was plenty of unoccupied rock surface of the same type, no visible insect damage to the mosses, and no overhanging plant canopy. This allowed me to assume that geographical variation, competition, predation, and nutrient availability were at most minor factors in where the mosses might grow. The *a priori* criterion for these assumptions was reasonableness. Once the data were fully analysed, a second criterion was applied – success. When the analyses bore out my hypothesis (e.g., Alpert 1987; Alpert and Oechel 1985, 1987), the assumptions appeared justified. Until some researcher with a different point of view alights on the same group of mosses, the assumptions will go without direct examination.

Because successful explanation of a set of observations can in practice confirm both assumptions and hypotheses, the definition of success becomes critical. In biology, the rules of logical proof apply, and their weaknesses yield still more influence to the viewpoint of the researcher. Medawar (1965) is among those who have stressed the asymmetry of proof in science. What Medawar called the leniency of scientific deduction arises from the circumstance that a second statement follows from the first unless the first is true when the second is false. In other words, a false hypothesis can lead to true inferences, and be mistaken itself for true.

Another asymmetry is that hypotheses can only be disproved, not proved (Popper 1979). This practical handicap of scientific proof can be illustrated in statistics, the arbiter of biological results, by the topsy-turvy invocation of the null hypothesis. When one has systematically disturbed some plant or animal in an interesting way and is looking to see whether this has affected a chosen characteristic, one customarily compares a number of disturbed creatures with a number of undisturbed ones, the controls. Setting up this type of comparison between experimental treatments and controls constitutes a craft in its own right. To test the results statistically, one actually tests, not whether the treatment has had an effect, but whether it has not. That it has not, that the disturbed creatures still share the same characteristics as the undisturbed, control creatures, is known as the null hypothesis – the hypothesis that nothing interesting has happened. One is naturally anxious to show that something has, but because hypotheses can only be disproved, the best one can do is to prove that it has not not. One calculates the statistical probability that the observed differences between the sets of experimentally disturbed and control subjects could have occurred solely by chance variation, as if the experimenter had withheld his creative torments and the subjects showed only the vagaries of natural endowment. If the probability is sufficiently small, by convention less than 1 in 20, a number of things happen at once. The null hypothesis is rejected, the experimental treatment is said to have had a significant effect, and, with a little handspring of faith, the original hypothesis stands.

IDEALS

Throughout the business of framing and testing hypotheses in biology runs another source of subjectivity, the need to simplify and generalise observations in order to compare them. In the first flush of modern physics, Galileo apparently wrote that 'Nature is a book and the characters in which it is written are triangles, circles and squares'.[1] However, amid all the sunflowers, cinder cones, and planetary orbits, a perfect triangle or circle has yet to be found. From this, two conclusions have commonly been drawn: either Nature's book has been badly printed, or the triangles twinkle only in our eyes.

Marcuse (1965) maintained the latter. Interpreting earlier philosophy, he defined a common-sense, everyday world, or Lebenswelt; an idealised scientific universe; and an extreme form of the second, the *Ideenkleid*, or 'tissue of ideas … cast upon the life world so as to conceal it to the point of being substituted for it'. The common-sense world is the compound of empirical reality and our own consciousness. Science admits the same empirical reality, but distrusts our immediate experiences to provide adequate explanations, and replaces them with more general interpretations based on inference and abstraction. When one comes to believe that a true, scientific universe of perfect shapes underlies our flawed perceptions, and perhaps also some shoddy individual manifestations of universal phenomena, then one has wrapped the world in a spurious *Ideenkleid*. As Whitehead (1967) warned in his discussions of the 'fallacy of misplaced concreteness', one may then be abstracted, not only from minor details, but also from important ones that do not happen to square with the ideal.

Be they dangerous or not, one inevitably resorts to idealised forms during the course of a biological study, and often to whole systems of them. In the common-sense world, no two things are exactly alike. In the scientific universe, there are uniform processes and structures that obey constant laws and can be mathematically manipulated and compared from one example to the next. A biologist typically begins with common-sense observation, perceives a pattern or notices a singular phenomenon, refines this into an ideal form, and then seeks a fit between his idea and empirical reality.

The field data sheet is a modest, everyday example. It is immensely satisfying to photocopy a ream of personally designed data sheets. On each page, precisely the appropriate number of blanks await all the data necessary to unambiguously and efficiently test the hypothesis or describe the system at hand. Crisply, the data sheets are carried into the field.

On the first day at the field site, as mosquitoes swarm round and raindrops vie to distribute inked digits more evenly over the page, one finds that four measurements instead of one are needed to average out the effects of wind, that surface texture must be added to the list of variables, that half the plants have too few leaves for the planned comparison, that the test species hybridise, and that the field observations will require over six years of twelve-hour days. There one is in Borneo or Barrow, however, and one muddles through. The data sheets are carried back, a mystery of impromptu symbols.

Once back in a comfortable chair in front of the computer terminal, one's Procrustean dealings with real plants and animals seem far away. The non-numerical notes dutifully inscribed in the data sheet margins to recall the worst irregularities only cause an error when entered into the data file. Out comes the print-out, with margins snowy white. With each translation from the empirical world to an idealised system, and back again, some sense is lost. In a long study, the process from empirical survey to idealised design to systematic measurement to empirical conclusions is repeated many times.

After conceiving a test of my hypothesis on arid land mosses, I went forth to look for a study site. Following circumstance and convenience, I made my way through parts of California, Utah, Arizona, and Colorado. I learned to tell sun-dried moss from nameless debris, and returned with the image of my ideal study site. It would be a flat plain strewn at six-meter intervals with two-meter-tall, spherical granitic boulders. Each boulder would be completely bare except for the same four species of the moss genus, *Grimmia*, growing somewhat sparsely in separate zones characterised by a consistent range of compass direction and rock surface slope.

When I finally went for my field research to San Diego, a location picked for the presence of a particular professor, a fancy piece of equipment, and arid habitat, I spent two weeks combing the county for the boulders of my vision. Eventually, I found a 30° slope with nine granitic boulders 1-2 meters tall and 1-3 meters apart. Each boulder was virtually bare except for lichens and the same five species of moss, growing in fairly consistent patterns. An idealised hypothesis had guided a common-sense survey that produced an ideal site concept that led me, given various other constraints, to a side gully of Echo Valley, California. Was it fair to select a field site to test a hypothesis on the basis of how well it fit an image of the perfect system? Nolo contendere.

The next problem was to quantify the micro-distribution of the mosses on the boulders. What posed the greatest difficulty was how to describe a sampling point. A point on the surface of a perfectly spherical boulder of given height standing alone on a flat plain could be nicely characterised by just two topographic variables, compass direction and slope of the rock surface. Any other topographical features, such as height above the ground, would follow geometrically. Environmental factors such as amount of direct sun received and interception of rainfall could be estimated with some further geometry and a record of weather.

Elongated, flattened, and pitted with idiosyncratic crannies, my study boulders fell far short of spherical perfection. To solve the dilemma, I leaped back into the ether of the scientific universe, marshalling two fundamental ideal constructs, a perfect shape and a generalised type. Undeterred by the geometric errors of my boulders, I addressed them as spheres anyway and painted them with colourful, though non-toxic and scenically inoffensive meridians. I divided each boulder into four zones of direction and four zones of surface slope, making sixteen regions all told. I based my system of zones on common sense and my expectations. For example, 90° became one boundary because it seemed to me that the chances of rain wetting the rock were suddenly much less on overhangs. The number of zones reflected the research effort and statistics to come. The actual extent of each zone was established by projecting a boulder's lumpiness onto an airy sphere.

The sixteen zones, created in the likeness of portions of the surface of a sphere, enjoyed the same properties of identity and trigonometry. From the position of the sun and the light intensity received by a light sensor at a single orientation, I could calculate the average light intensity incident on a particular zone, and consider this to be the same for the analogous zones on every boulder. I could perform a statistical analysis of variance in moss cover between zones and boulders, and correlate the results with my estimates of light. A very messy situation was now a highly manageable one.

However, my handy zones did not exist. What I concluded from them has value only inasmuch as it corresponds to the way the real mosses grow on real, lumpy boulders. In truth, my spheres were the imperfect images of boulders, and the types of zones were differently distorted reflections of a particular portion of each boulder. Unfortunately, the appropriate reinversion of reality inherits more and more drag as research accumulates a history. When I return to examine a new hypothesis about the mosses, I will be highly inclined, with many hot days in the chaparral already behind me, to use the same system of zones. If someone else feels like investigating the ecophysiology of chaparral mosses, they may well use the same system to make their work comparable to previous research. After a number of studies, all in good conscience, a resistance to discarding the system will build up. These unreal zones will have acquired substance in the scientific universe.

This is of more than just academic concern, because scientists and society alike are especially gratified when scientific results lead to practical ones. Acquired substance in the scientific universe then gains weight in one's own neighbourhood. For instance, it comes time to develop Echo Valley. Moss ecologists are called in to evaluate the importance of *Grimmia* in maintaining the environment, and to advise on how many and which boulders should be preserved. At this point, the zones take on a major importance to survival of the mosses, and potentially to the rest of the inhabitants of Echo Valley not excluding ourselves. If the constructs through which we imperfectly know the real plants and rocks have departed too sharply from empirical reality, biology may give the wrong advice. If just *Grimmia* hangs in the balance, the consequences of bad advice may reverberate only quietly. More likely, human activity will involve organisms of broader ecological significance as well. The erosion of clear-cut hillsides and the ravaging of vegetation by deer relieved of their predators have already documented the potential effects of mistaken courses in ecological management.

The mossy boulders of Echo Valley received a much more thorough treatment than I have described, but a full description would lead to the same conclusion. What a biologist reports in the best of faith still depends upon constraints of time, training, funding, circumstance, and equipment; and, within these constraints, on choices of which questions to ask and which variables to test them by. Implicit in the subjective nature of these choices are what I call the values of biology.

VALUES

These implicit values fall into three categories, springing respectively from biology, from biologists, and from the natural systems that biologists use biology to understand. One example of the first category is how biology, in common with all science, places a premium on the quantifiable. As I rediscovered in Echo Valley, quantifiable variables are more powerful tools for statistical tests of hypotheses. A number of variables that seemed likely to be important in the distribution of mosses, such as fine surface texture on the boulders, were beyond my ingenuity or energy at quantification. After making several thousand stabs at quantifying texture with a miniature frame fitted with pins whose heads played like notes in front of a background of tiny lines, I resorted to a set of qualitative classes, 'very rough' to 'smooth'. However, like other variables that I tried to class qualitatively, this measurement of texture rarely showed any statistically significant relationship to where the mosses were. Whether or not texture was important to the mosses, it became unimportant in my analysis.

The second category of values let into biological studies through subjectivity are more personal ones. The opportunity to make choices also invites the biologist, consciously or unknowingly, to indulge beliefs that have little to do with his professional learning. As Goodwin (1972) suggested, if science involves a choice among subjective images or models, then scientists must accept some moral responsibility for their results. Although my own thesis research was probably even more innocent of political than of practical applications, when science involves politically sensitive issues, scientific results and especially their interpretations tend to correlate with the political views of the scientist.

For this reason alone, scientific results should be treated as a class of judgments. Belief in the objectivity of science, or perhaps manipulation of what is hoped to be a public faith in it, has generated such spectacles as doctors testifying before the US Congress as experts on when human life begins. If science's answer to where mosses grow on rocks depends upon choices made by the researcher, what axioms does Congress hope science will impart to them on the definition of life and humanness? An aeronautics engineer is not *ipso facto* the best man to schedule flights. Those who use scientific results to bolster political arguments have a responsibility, not merely to consider, but also to present the methods and judgments that went into the conclusions that favour their positions.

The third category of values is where subjectivity may add, not just heat, but also light to science. In ecological biology in particular, the drudgery of the field research that the stiff rules for hypothesis testing dictate may provide a dialectic link between scientist and subject. Few people can be driven to more protracted and painstaking observations of natural systems than graduate students in field ecology. Pushed by the demands for replication and proper sampling, I spent many hours crawling over boulders in the southern Californian chaparral, obeying the commands of my random number table. This brought me into intimate contact with the corner of nature occupied by my mosses, in its broiling as well as its balmy moments, and tested the reasonableness of my methods over and over under many conditions I did not choose. While I was imposing my ideal constructs on the system, it was challenging them back. The inherent subjectivity of science may work both ways, opening a two-way channel between perspective and perception. Science that requires the researcher to impose constructions on reality may open him to learning values from his subjects.

When humanists have considered that science has values, their tone has often been less than congratulatory. In Dostoyevsky's *Crime and Punishment*, a businessman lectures a student:

> ...according to the Russian proverb: 'If you run after two hares, you will catch neither.' Science, however says: love yourself first of all, for everything in the world is based on personal interest. If you love yourself alone, you will conduct your affairs properly, and your cloak will remain whole.

On the contrary, through values learned from nature, modern ecological biology may offer a perspective on life that promises to improve the lot of people in more than material ways. The alloy of values and empiricism in biology, instead of diluting its contribution to society, can strengthen it.

Craig et al. (1992) reach a similar conclusion for environmental policy. Interviews with European policy advisors revealed that many advisors believe in the intrinsic value of natural systems and species, but separate these personal views from their official, more utilitarian arguments for conservation. The authors agreed with Naess (1986) that better integration of value judgments and practical decisions might deepen arguments. This is in essence a call to more, not less subjectivity in policy-making.

The obvious objection, in policy as in science, is that environmental policy-makers or ecological biologists might hold common environmental values simply because people with such values go into such work. If so, their professional expertise is no special reason to bow to the wisdom of their values. What evidence is there that biology contains values learned from nature and not just from personal politics? An answer may lie in a set of widely held generalisations that current ecological and evolutionary biology teach with a spirit of dispassion, for their utility as explanations of natural phenomena and not for their philosophical appeal. It seems unintentional that these precepts should overlap with political and economic debates, and unlikely that they could have arisen solely by consensus of the varied political or social beliefs of biologists. Without any claim for their universal acceptance among biologists, here are six examples:

(1) *anthropo-eccentrism* – Man is one species among many. What appears to be mastery of nature in the short run may wind up looking more like disturbance of ecosystems in the long run.

(2) *diversity* – Variation is valuable. Genetic variation is the raw material of evolution; diversity among individuals, populations, and species is a source of ecological resilience and a multiplier of future potential;

(3) *context* – What is best or most successful depends upon ecological context. Non-transitivity of competitive ability among species in the same system and paradoxical effects of similar processes in different systems show that nothing is universally superior;

(4) *progress* – The frequent rise and demise of highly specialised species and the evolution of simpler as well as more complex form belie any inherent direction in evolution. The rise and fall of productivity, species diversity, and structural complexity at different points in the development of natural communities make it equally difficult to apply the notion of progress to ecology;

(5) *chance and history* – Chance and past events have potent effects on the present and future of natural systems. Prediction must be expressed as probability, and present understanding depends upon knowing what came

before.

(6) *continuity* – The living things that help drive natural systems, and probably the systems themselves, while sometimes very tough, cannot yet be and probably will never be reconstructed once disassembled.

Are these generalisations values? The very argument that they are not personal or intentional, that they merely describe natural conditions, might seem to argue not. However, some may already have led to prescriptions. In 1982, newly embarked upon postdoctoral research at Stanford University, I sat in on a lecture course by Harold Mooney and Peter Vitousek on a topic of which I had never heard, 'global ecology'. In 1992, the United Nations Conference on Environment and Development met in Rio de Janeiro to draft global agreements on use and conservation of the world's ecological systems (Robinson 1993). One of the most potent North American environmental laws, the US Endangered Species Act, embodies the generalisation on the importance of diversity. I cannot prove cause and effect in either case, but I hope that values filtered into biology through the subjectivity inherent in research do guide our participation in ecological systems. Certainly, some of the values implicit in biology have maintained a remarkable constancy since Darwin (1859) concluded *The Origin of Species* with the prose poem:

> There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone on cycling according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved.

NOTES

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[1] The source of this quote, like much of what I learned in graduate school, has been irretrievably lost.

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