A True Tale of Science and Discovery

Lawrence A. Curtis

Environmental Research Advances Series

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A TRUE TALE OF SCIENCE AND DISCOVERY

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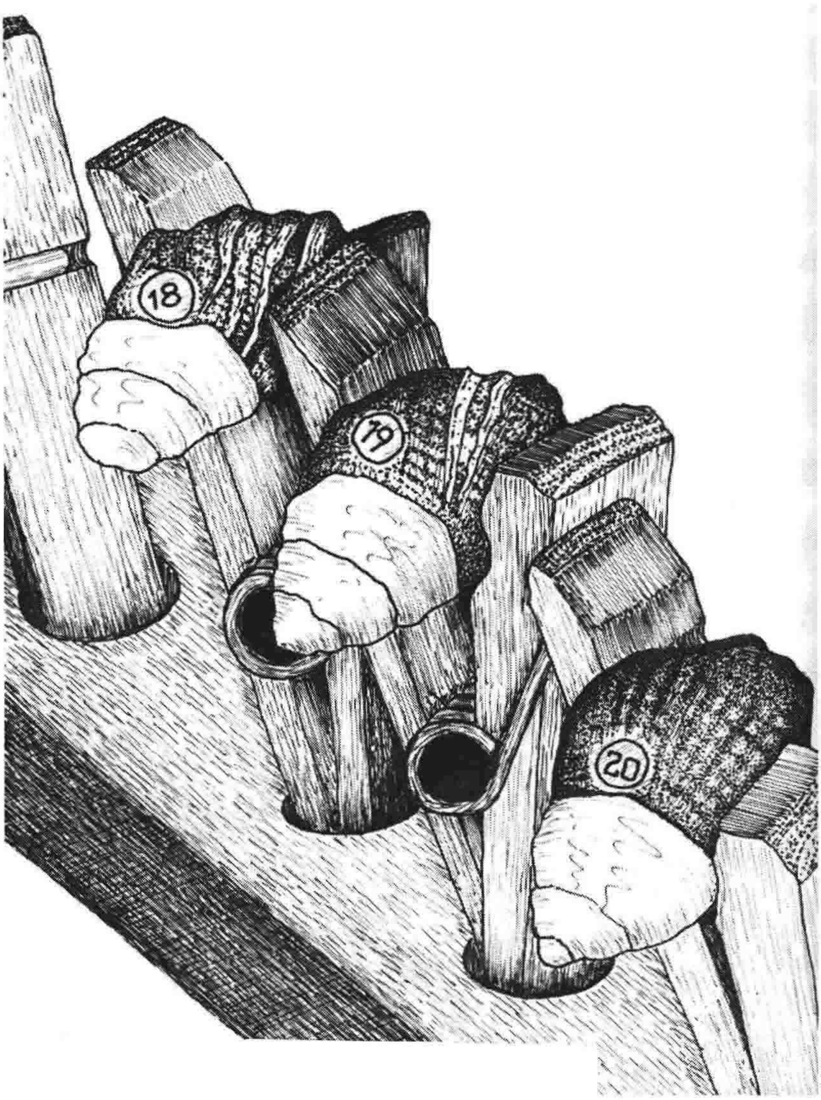

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PREFACE

An attempt is made herein to portray one example of how human knowledge was increased. It concerns marine biology, but is presented in a way that makes it available to a reader not versed in that area of knowledge or science in general. My purpose has been to illustrate how one piece of science was done for those who have never been involved in doing scientific work. I hope that I have succeeded in doing that. I thank all those who advised me in various ways during the process of writing this. In particular I would like to acknowledge my sister, Tina Sculerati, and two friends who helped me a great deal, Karen Hubbard and Anne Colwell.

CONTENTS

Chapter 1

THE START

This is an account of people, a place, a time, and the pursuit of knowledge. You are about to embark on one tale of how an increase in the knowledge of the natural world happened. In one guise or another it is an oft repeated tale. This time it concerns me, some of my students, and an obscure, but gloriously complex, marine organism known unofficially as the Eastern Mud Whelk. A drawing of it is presented as a frontispiece to this chapter. This snail is currently officially known as *Ilyanassa obsoleta.* (It's pronounced Il-ee-ah-nassa ob-so-lee-ta.) As recently as the 1960's its scientific name was *Nassarius obsoletus*. Before that its moniker was *Nassa obsoleta* and historically other designations have been applied. People who applied the names were impressed with the fact that the snail lived in the mud and often had a highly deteriorated shell. The word roots, "ilya" (mud), "nassa" (basket), and "obsoletus" (worn out, decayed, old) tell it all: older snails look like eroded mud baskets. Let me provide a word about scientific names. They are used to be as sure as possible exactly which species you are talking about. Common names may be used and they are perhaps easier and more comfortable, but they can refer to more than one organism. For example, around the world there are many different species known as "mud snails," but there is only one *Ilyanassa obsoleta.* [The word "species" is unusual in that it is used in both the singular (a species) and plural (several species) senses.] The first part is the generic name and there can be many species within a single genus. The second part is the specific name. Used together, these names signal only one kind of organism. You are not supposed to refer to a species with its generic name, but I guess I can get away with it here. There is only one species in the genus *Ilyanassa,* and that species is the star of this tale.

I first became aware of this beast while working toward my Masters Degree at the University of New Hampshire in the 1960's. The Piscataqua River meets the

Atlantic Ocean near Portsmouth, New Hampshire and extends inland about 12 miles to terminate in Great Bay. This estuary witnessed much of my work and play while I was there. The snail was not my central scientific interest at the time, but when the tide was out it littered the shoreline.

At the age of 31 in 1973, I got my Ph.D. in Biological Sciences from the University of Delaware. It is important to say here that about a year later I came down with insulin-dependent diabetes. That was a major surprise! It wasn't much of a limitation in the 1970's and early 1980's, but later on it made my work and play much more challenging. My dissertation was about the life cycle of a marine worm, *Sabellaria vulgaris*, which, needless to say, almost nobody cared the first thing about. It seems to be my fate to take up with organisms that do not claim global attention. Nevertheless, life cycles of organisms are central to understanding biology and I found the business of investigating them interesting and a challenge.

In 1975 I managed to obtain a position as Assistant Professor at a branch of the University of Delaware in the southern part of the state. At the time this academic unit was called the Parallel Program. The idea was that, with emphasis on the first two years of University education, course work parallel in quality and rigor to that provided on the main campus would be made available to local students. I could get behind that. If I had just settled into my instructive duties and forgotten about research, that would have pleased administrators of my academic unit, but my nature and perceived duties dictated otherwise. A university professor is supposed to fulfill three basic responsibilities, teaching, research, and service. I considered the third, which basically amounts to committee work, to be a somewhat necessary evil. University administrators have essentially one strategy. If they want something from the faculty, they get the faculty to form a committee to figure out how to give it to them. If faculty want something from them, they get faculty to form a committee to dash and dissipate their energies. So the request won't have to be honored, not anytime soon anyway. Either way, a faculty member on or running a committee is likely (not always) to be involved in a waste of time and whenever possible he or she would do well to avoid this activity.

I wanted to pursue especially teaching and research. Teaching was rewarding to me and I put much effort into it. I taught what was requested of me, proposed and developed new courses, and did such service as I could not avoid, but what to do about research? For a time I tried to go on with studying my dissertation worm, but my interest at the time required taking plankton samples and that was a prohibitively expensive activity.

Let me take a side-step here to make a pledge to readers, similar to one that the late Stephen J. Gould made to his readers in one of his many essays on science. It will be necessary to deal with matters of science, but I will not lie to you. That is true for any purpose, but especially I will not do it in the name of simplification. In fact, there is nothing all that complicated to report here. Above all, I want this to be assessable to persons that are interested, but not versed in the science involved. The biology of animals without backbones (invertebrates) and estuarine ecology are the main fields with which I deal. A reader not acquainted with these disciplines should be able to get along fine. Close attention will sometimes be needed, but I will attempt to explain things so that someone not familiar with the terms and concepts involved can easily follow. Years of teaching have taught me that scientific lingo often prevents people from understanding science. There is no value in weighing a non-scientific reader down with unnecessary terms, but if used wisely terms are useful tools of explanation. Otherwise they just obfuscate.

Perhaps the term plankton is not too necessary to explain, but let me begin with that. It refers to organisms that do not swim strongly and drift at the mercy of currents. These organisms need not be microscopic (tiny), but in this case they were. I was interested in the planktonic developmental stages (larvae) involved in the worm's life cycle. To sample these larvae and pursue this line of research required boat time and a variety of other expensive items. My resources were very limited (non-existent) and I soon recognized that this line was not available to me. I decided an intertidal animal that could be sampled simply by walking out when the tide was down was much more realistic, and so I ultimately focused in on the mud snail as an object of research. Little did I know at the time what a wondrous journey I was starting.

Before they knew what a pain in the neck to them my research endeavors would become, administrators agreed to setting me up with some laboratory space in the Cape Henlopen Laboratory of the University's College of Marine Studies complex in Lewes, Delaware. I had worked there during my dissertation research. It was not much, only one room and no equipment, not even a microscope, but it was my place to work and get started. My lab space was located in an old military building on Cape Henlopen at the mouth of Delaware Bay. The building had been used to store and dispense mines during WWII to prevent enemy submarines from entering the Bay. It was big, a simple rectangle with hoists to move heavy mines around. The roof and asbestos sides were not tight. Some rain and lots of wind would seep in around the edges. It sat maybe a quarter of a mile from the Bay. The first time I experienced it most of the rooms inside did not have walls that reached all the way to the floor, strictly a place to work in summer. Now, inside there were posh, mostly watertight rooms that could be air conditioned and heated. My Ph.D. advisor, the late Dr. Franklin C. Daiber, stepped forward and made the microscopes I had used during my dissertation research available to me. They were very good microscopes, one dissecting (a Wild M5) and one compound (a Wild M20). Now I had some basic equipment and could get started. On the Delaware Bay side of Cape Henlopen there is a sandflat that harbored a huge population of *Ilyanassa*. They occurred along shore in a band about 1,500-2,000 meters long and anywhere from about 25 to 200 meters wide. Serendipity clicked. My lab was there, I had a little equipment, I wanted an intertidal animal to work on, and there was a handy snail population. I did not know much about this snail, but I had some small familiarity from my New Hampshire days, i.e., I knew what its name was, *Nassarius* something. I had a lot to learn, but the juxtaposition of circumstances was pronounced. I began to ponder: what was going on with these snails; why was the species so abundant and successful?

Some characterization of our protagonist is appropriate. Research seeking new information should always start with what is already known and my first job was to look into the literature on this snail. It was first described by T. Say in an 1822 publication. It has been the subject of study on and off at least since then. It is a native and, if for no other reason than its abundance, an ecologically important animal along the Atlantic coast of North America from the Bay of Fundy in Canada to about northern Florida in the U.S.A.. About a century ago it was accidentally introduced on the Pacific coast and it now also occurs there. Along shores in estuarine environments there can be sometimes 1,000's per square meter. The snail is black and the largest ones are generally about an inch (25-30 mm) long (high, in snail anatomy). So this is not a big, dynamic creature. It has neither plume nor pelage. It is not repulsive, but not lovable either. It does not lend itself to film documentaries. *Ilyanassa* is mainly a deposit feeder, an organism that consumes bottom sediments and digests out contained nutrients, but it also eats macroalgae (algae visible to the naked eye) and, most importantly, it conspicuously aggregates on and consumes dead meat (carrion). It is probably best described as a scavenger. That this snail can eat both plant and animal material is unusual because snails tend to be either herbivore or carnivore. This is because the enzymes that aid in digestion of plant material cannot coexist with enzymes that digest animal material. The omnivore capability was the snail trait that initially interested me. Could it be a key to the snail's numerical abundance? Off we go.

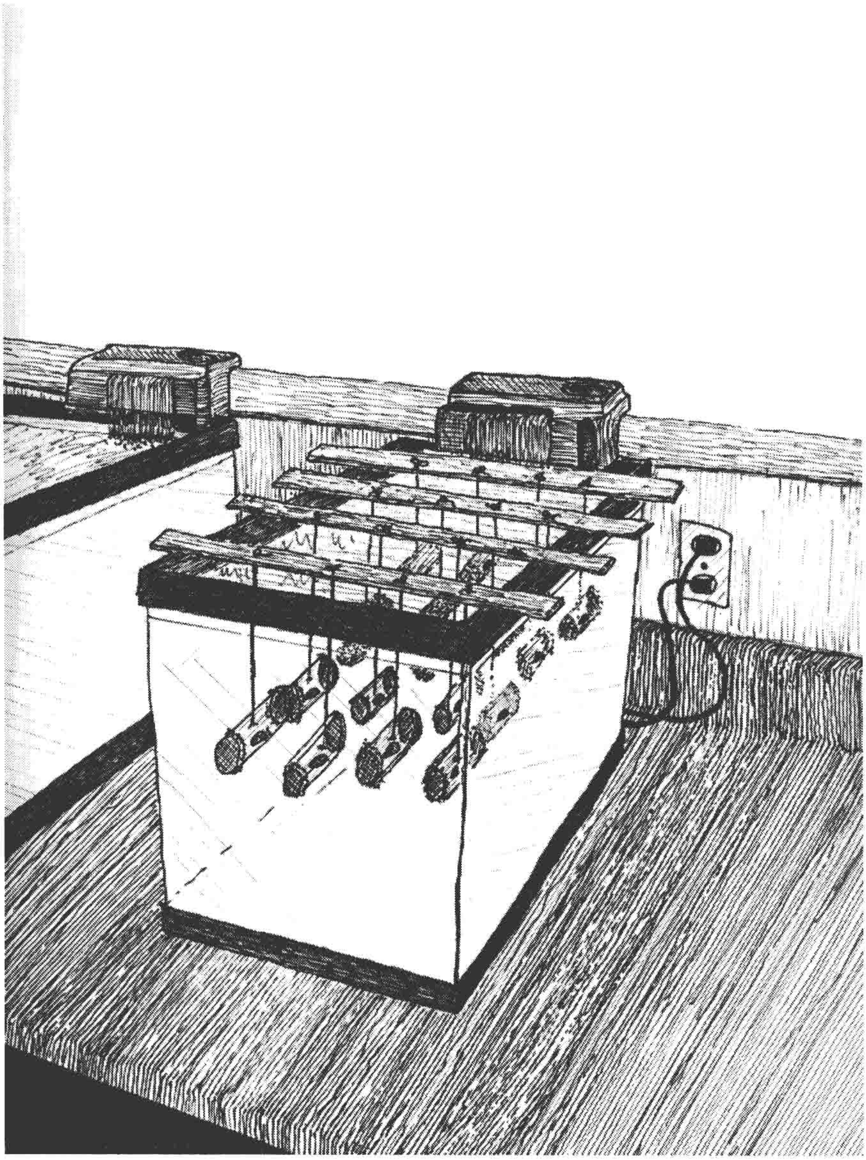
Here are some instructions for dealing with this tale. The telling of this saga necessitates that it will be somewhat autobiographical in nature, but that is not my point. It is the tale of a search for biological knowledge. Its central feature is a sort of a down-in-the-trenches rendition of what was found out, how it was found out, and how understanding of the natural world was enhanced. Consequently, considerable attention will have to be devoted to the circumstances under which discoveries were made. My goal in writing this is to give an interested non-scientist an insight into how one contribution to ecological science proceeded. To take you into this world of field biology it will be necessary to take you into the problems and concerns that were of interest to a me as I worked various research projects. Things that interest a person tell you much about that person and what he or she is trying to do. As indicated above it is not so much the person I seek to have you to follow, but rather the process of scientific discovery. A grasp on that process will probably be of greatest value to my intended reader. Taking you on a trip into my world of discovery will require that I maintain a balance. On the one hand I will need to broach problems of science that you likely have not thought about. I suppose I won't totally avoid getting wonkish about certain issues, but it is necessary to tell my intended tale. So have a little patience with me if I err in places on that score. Some over indulgence in information would happen whatever area you might decide to look into, whether it be Appalachian quilts, golf, a martial arts discipline, or perhaps the manufacture of cardboard boxes. We all, certainly including me, have stuff to learn. On the other hand I recognize that I do not need to take you into every last bit of relevant detail to make my point(s). I spoke above about the terms used in science. They all have some concept behind them and if they are not explained one can very quickly get lost. Their overuse can put anyone off that is not used to dealing with them. Some terms will have to be used, but I shall strive to keep their meanings and pertinence to the issues at hand clear. I will also keep their number under control. That also goes for the concepts I employ and any arithmetic I might have to use. By keeping things abundantly clear I shall always be keeping my intended reader in mind.

As perhaps you have already gathered, this will not be a tale of what might be called "high profile" research and discovery. It was not well funded and did not have much moral support from the powers that be. It certainly was not carried out with the aid of plush research grants. Taxpayers went virtually unscathed. It was a bootstrap operation from start to finish. Nevertheless, I like to think that some good science was produced and our knowledge of the natural world enhanced. Research often proceeds in this manner. In the history of biology, more often than not discovery has begun with curiosity driven, lonesome activity. This is a useful lesson for a person that has not (yet) been involved with science. You too could or can do it. It mostly takes interest and desire.

One more thing is useful to include here. I include this bit of reality because scientific endeavors are often portrayed as if they were perfectly done by supremely competent beings. How many times have you heard a conclusion of some sort denounced because it is *non-scientific*? Perhaps it deserves denouncement, but had it been described as a *scientific* conclusion it would automatically be given more respect. Science as a means to find things out is deserving of great respect, but its conclusions do not arise out of flawlessness. Nobody can be a perfect, all-knowing scientist. After all, no one in any venue can be so perfect. It would be inconceivably boring in any case. Did I always do what I did in the best way possible? Was I always correct in every aspect? Not likely, but no scientist can expect to always be immersed in such correctitude. It's just not in the game. As is true in any quest of discovery, I have not always been so competent. There was always trial, error, and outright mistake involved. As a consequence, any conclusion of science is open to question on the basis of new interpretation or new evidence. That very definitely is in the game. What is needed in a scientist is dedication and cleverness in the search for the truth and the keeping of careful records so you can retrace your steps and correct the inevitable mistakes. This is more in keeping with the nature of scientific endeavors than is some kind of flawlessness.

Science is a product of people, a time, and a place all enmeshed and intertwined. Were it someone else driving this research and different colleagues involved, that alone would alter the product. Some of the same phenomena would undoubtedly have been discovered, but the emphasis would almost surely have been different. If time were shifted, either more or less knowledge about the snail's biology would have been available. This too would surely have altered what was discovered. Variability is a cornerstone of biology. Organisms on planet earth are different from place to place (and time to time) because of genes possessed and environmental circumstances. The interdependence of these variables, biological variability, people, time, and place, make it impossible for other circumstances to produce the same result. Science in general is not often thought of this way. It is supposed to consist of universal truths, independent of the circumstances under which they were discovered. This would perhaps be more true in the physical sciences. There variation is considered "error," some sort of mistake in taking measurements. For example, all electrons are expected to weigh the same. If you find differences among them, you are dealing with measurement error. In biology, variability is caused by a combination of measurement error and real differences in organisms caused by different genes and environments. For example, if you are studying the amount of oxygen consumed by snails, there will be differences among snails, partly from measurement error and partly from real differences among the individuals you test. Because of these dual sources of variability, the results of a biological study are particularly contingent on circumstances. This is especially true of a study done outside the laboratory in the field. All this is not to say that no general truths can emerge from biological field studies. While variables are more profuse in the field and firm predictions are harder to come by, we can nevertheless come to understand generally what the important factors are and how they affect individual organisms and populations. Science can certainly be done in the laboratory, but it is my belief that, if it is at all possible, science done out in nature is the way to go and I have done most of my work there. Field research is often very challenging and arriving at an explanation of results more difficult (more variables to deal with), but out in the field is where organisms live and where they function normally.

Science is supposed to be hypothesis-driven. That is, you make some observations of the natural world, form an hypothesis (an educated guess) to explain them, devise a test of the hypothesis, test it, and then see if results of the test support or refute the hypothesis. In brief, these are the steps of the scientific method, at least as usually portrayed. A fairly worn analogy, not my creation, may help illuminate how science is done, some of the issues, and my general approach. Doing science as outlined just above is akin to observing a barn wall, defining a circle (the hypothesis) on the wall, firing an arrow at the circle (the test), and seeing if the arrow finds (results, data) the circle. If it does, the hypothesis is supported and tentatively accepted. Tentatively always, as there could be other explanations for the outcome. This is a perfectly valid way of doing science, but not always a possible approach. When we are exploring the unknown, often no hypothesis can be formed before some data are in hand. We don't know where to draw the circle. So we fire the arrow at the wall (take observations), see where it lands, and then go up and draw a circle around it. This would be an after-the-data-are-in hypothesis, as opposed to one formed before a test is run. Some say this is not a valid way to do science because no prediction is made and you don't know where the arrow is supposed to land. It just lands and after the fact you try to justify why it landed there. If the arrow had for some reason been pulled to another place on the wall, if observations had been otherwise, the circle (the after-the-fact hypothesis to explain observations) would have been drawn in another place. In the end it seems that both ways of doing science are valid and needed. Sometimes an hypothesis can be formed; other times no expectation as to what is going to happen is available and we must be led to our tentative explanation of observations (the hypothesis) by data in hand. I and my colleagues have often (not always) found ourselves in the latter position, taking field observations in a systematic way, not really knowing what to expect, and trying to make sense of where the arrow landed. You have to be honest, no fudging. Data acquisition must not be designed, the arrow must not be aimed, to favor a certain outcome. If not fudged, this too is a perfectly valid way of doing science and I have often used it. It is my hope that understanding of the natural world was increased a bit by what I and my colleagues have found out and what I have to tell.



Experimental setup for the laboratory testing of an *Ilyanassa obsoleta* diet