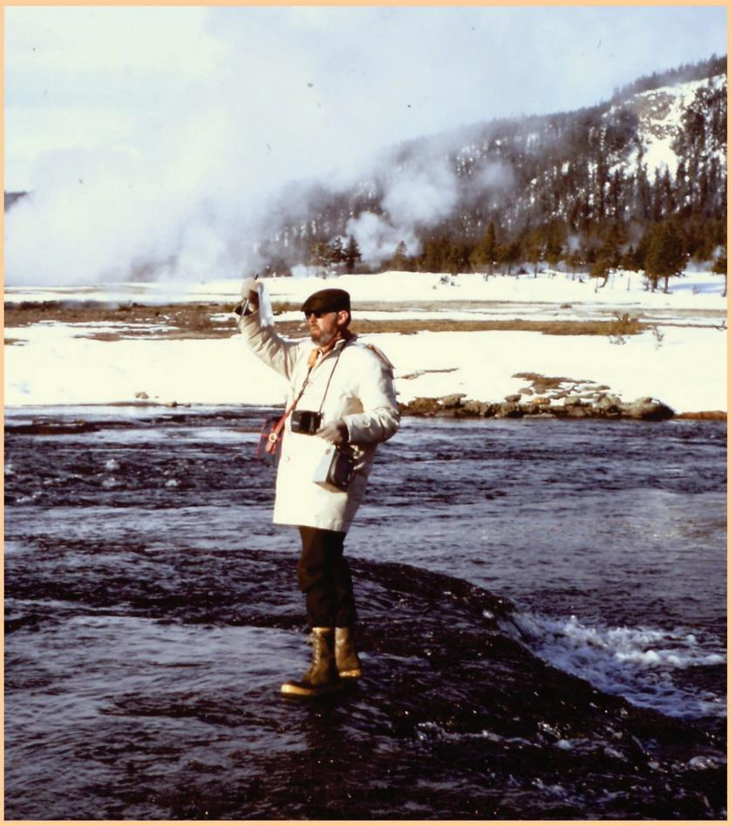
# A Scientist in Yellowstone National Park

*Thomas D. Brock*



A 50-year adventure

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Editorial, design, and production carried out or supervised by Thomas D. Brock.

# Preface

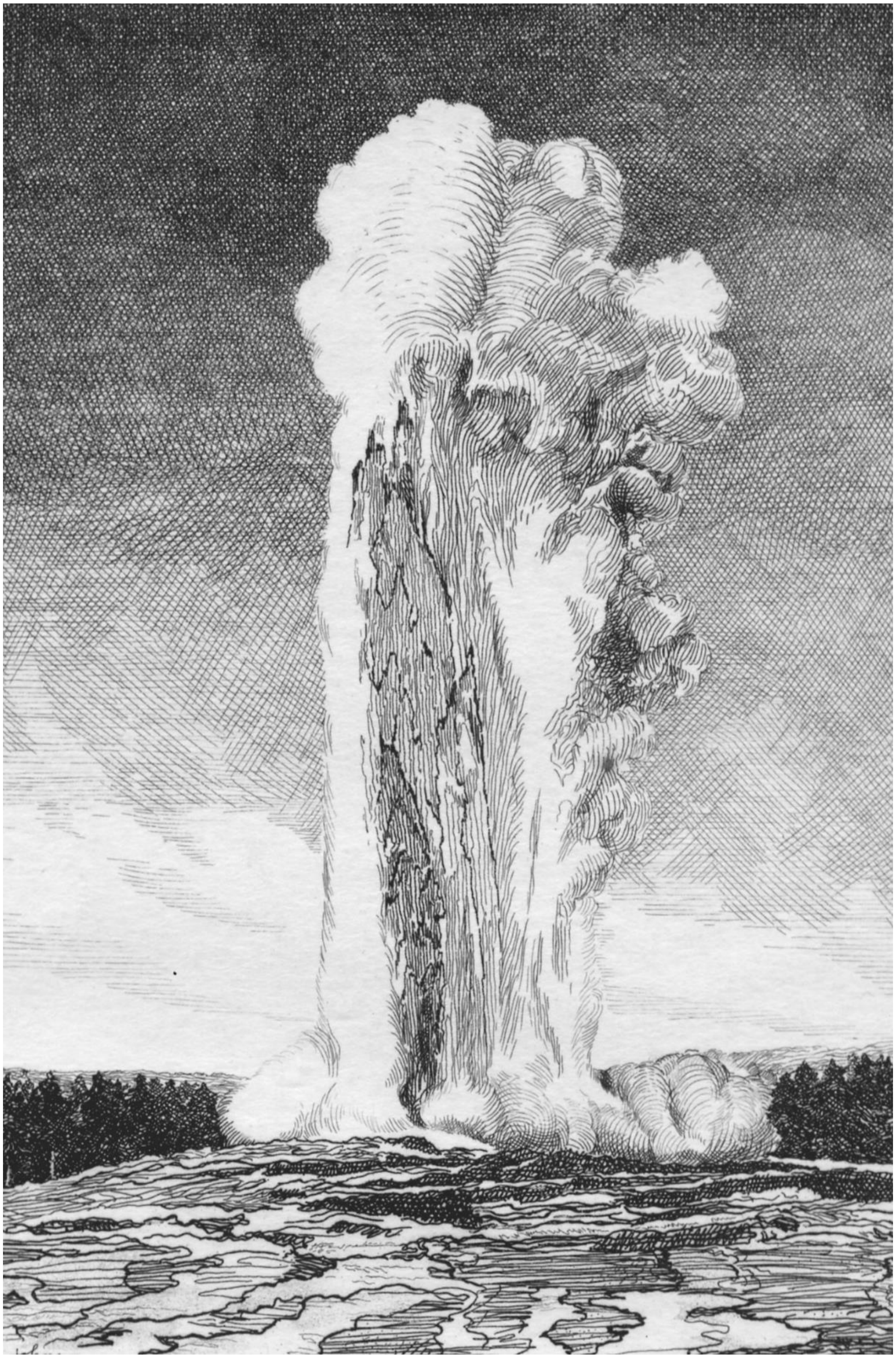
Scientific research is a glorious occupation, and probably no category is more fascinating than field work in a National Park such as Yellowstone. It was my good fortune to have been able to carry out research in the Park with a dedicated group of students and research associates for over 10 years. This research depended greatly on financial support from two federal agencies, the National Science Foundation, and the U.S. Atomic Energy Commission (now subsumed under the Department of Energy). It also depended greatly on support in numerous ways from the two universities where I was employed, Indiana University-Bloomington, and University of Wisconsin-Madison. Finally, since its inception Yellowstone National Park has recognized and encouraged scientific research in the Park, and without the Park's permission, none of this work could have been carried out.

The work we did is called *basic research*, which means that it was carried out to discover new facts and principles, and without any practical aim. It is a truism in science that basic research is the foundation from which applications arise. This Yellowstone research is one of the best examples of this truism, as is briefly explained in the chapter: "Legacy of my Yellowstone work."

Although this book might be called a "memoir", I emphasize that it is not based on memory, but on documents that were created at the time. See "Documentation" for details. It is thus closer to "real" history of science than to a memoir. In the same way, the numerous photographs reproduced here were taken at the time of the events. I have "saved" all of this material. Indeed, in addition to my professional papers now held by the University of Wisconsin-Madison Archives, there is a large room in my home that contains many boxes and filing cabinets storing copies or original records used to write this book.

Thanks to my wife Kathie Brock for editing the whole manuscript, and for her constant advice and for listening to me talk about this project. She lived through it with me! Thanks also for the important perspectives that my daughter Emily and son Brian gave to me.

# Contents



# Introduction

The work I am best known for took place in Yellowstone National Park. It started simply, increased gradually, rose to a crescendo, and then stopped (at my initiative), but its consequences have lived on ever after. Very satisfying, and very surprising. Who knew what it would lead to?

As I read some of my papers now, over 50 years later, I suspect that many people may have no idea of the difficulty of doing many of these studies under field conditions.

Also, as far as I can tell, in the years that have gone by, although lots of people have sampled hot springs in Yellowstone for new organisms, there has been little follow-up on most of the extensive work we did on hot springs as *ecosystems* or *biogeochemical sites*. The sort of work that we did was unique!

Within the first two weeks of my work in Yellowstone National Park I knew that nearby research facilities were essential. The idea was to bring the laboratory to the field because you certainly could not bring the field to the lab. Also, with Yellowstone National Park, because of its difficult and complex regulations and the vast distances, it was essential to LIVE as close as possible. Even Bozeman, a 2.5 hr drive one way (5 hrs round trip) was too far away. That was why West Yellowstone, Montana was the only place to locate the laboratory.

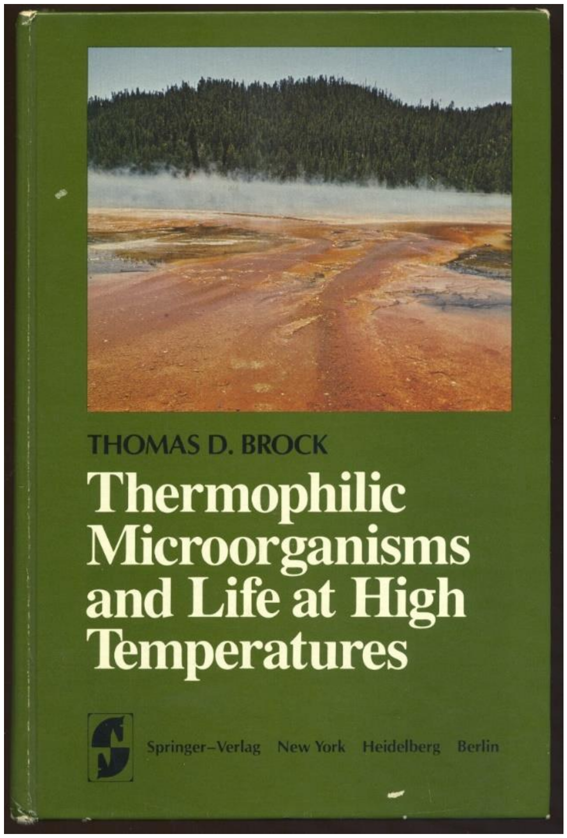
I am convinced that much of my Yellowstone work could not have been done without the West Yellowstone lab. As far as I can tell, no one has followed up on this work, or done similar work in other thermal ecosystems. No one has set up a field laboratory near thermal areas, or thinks about the hot spring as a microbial (biogeochemical) ecosystem. Almost all of the more recent work that others have done uses the hot spring not as a *system* but as a source of organisms. This is not to say that work in the research laboratory work is not important, but the geothermal habitat is the place where evolution has taken place, and to understand it, one must know how the organisms grow on site..

I have done numerous Google searches but find nothing analogous to what we did, even though this work was done in 1965-1975. In fact, our early papers continue to be cited extensively (see ResearchGate for data). As an example, the original *Sulfolobus* paper has been cited over 11,000 times.

So, although the West Yellowstone lab was not full blown immediately, and once it started it expanded slowly, it played a major role in this work.

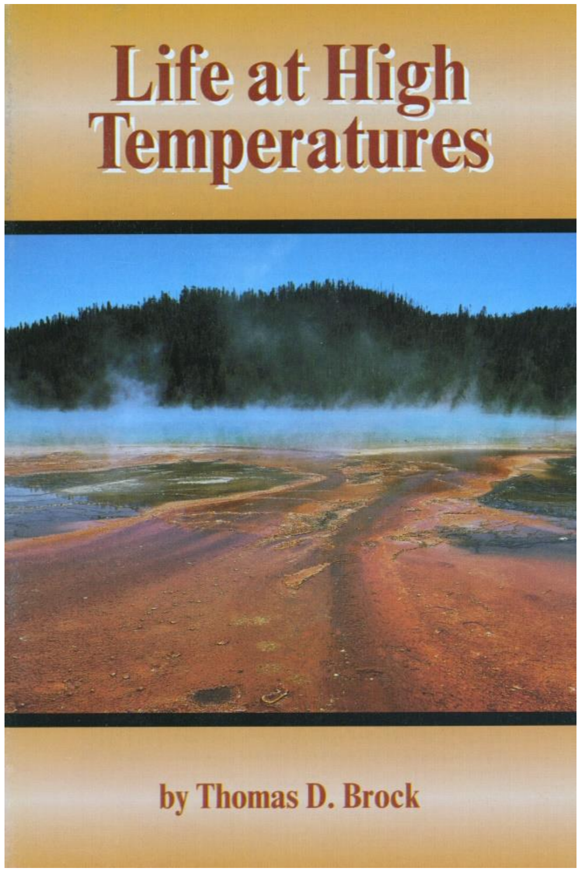
After this work was completed, I wrote a book summarizing the research. That book focused on the *science*, and most of the personal history was left out. Later, I wrote for the Yellowstone Association a small publication suitable to be sold in the Visitor Centers. Both of these publications are still available, free, on the Internet. Also, at the end of this book is an extensive bibliography of the works I and my students and associates published.

# The Big Book on Life at High Temperatures




*Book: Thermophilic Microorganisms and Life at High Temperatures; Springer Verlag, 1978.*
***Springer book cover.jpg***
*This is available on-line via UW-Madison Digital Collections:*
http://digicoll.library.wisc.edu/cgi-bin/Science/Science-idx?id=Science.BrockTher

# The Little Book on Life at High Temperatures




*Life at High Temperatures; Yellowstone Association for Natural History, History & Education, Inc. Yellowstone National Park, Wyoming 82190. Published 1994 Ken Todar of the UW-Madison Bacteriology Department developed an on-line version of this booklet, which can be found at the following URL:*
https://instr.bact.wisc.edu/themicrobialworld/LAHT/b1.html
***Life High Temperatures YA booklet 1994.jpg***

## First Steps into Microbial Ecology

I started work in microbial ecology in 1963 (at age 36), after an 11-year period working in antibiotics, microbial physiology, genetics, and molecular biology. At the time I had three grants, one from the National Institutes of Health on streptococcal physiology, one from the U.S. Army on genetics of streptococci, and one from the National Science Foundation on physiology and genetics of mating in yeast. I had technicians, graduate students, and post-doctoral fellows working on these topics.

I had been interested in microbial ecology since graduate school days. Some of my graduate school associates at Ohio State University were plant ecologists and chided me: “Why don’t you do microbial ecology, Brock?” As a person who had grown up loving the outdoors, this scold stuck with me. I had learned about the value of doing field work from John Wolfe, my plant ecology professor at Ohio State University.

In early 1963 I received a 5-year Research Career Development Award from the National Institutes of Health, and I was suddenly freed from a lot of other duties. With a 12-month paycheck guaranteed, my summers were now open. I had always been interested in lakes and aquatic habitats. (I grew up with a distant view of Lake Erie from my front porch.)

Marine microbiology was in vogue, and an influential symposium had been held at the Morrison Hotel in Chicago April 20-24, 1961. Although I missed this symposium, I became familiar with it through the proceedings volume that was published in 1963.

In early 1963 I learned about the Friday Harbor Laboratories, the marine laboratory of the University of Washington (Seattle). It sounded like a great place for doing marine microbiology during the summer. I had spent some weeks in the Puget Sound area just after World War II when I was in the Navy, so I knew the area. I communicated with the director of the laboratory. One thing led to another, and I arrived at Friday Harbor Laboratories on the last day of June 1963.

After a couple of weeks stumbling about, I discovered *Leucothrix mucor*, a widespread marine bacterium, and spent the summer working on that. I brought cultures back with me to Indiana University and spent some time in the fall and early winter working on them. Although *Leucothrix* is not the subject of this memoir, it was indirectly responsible for me getting interested in *cold* sulfur springs (its relative *Thiothrix* lives in such springs), which led me inexorably to *hot* springs, some of which also contain sulfur. I also began to formulate plans for my second book, a small textbook on microbial ecology.

Using the VW minibus that my wife Louise and I had converted into a camper, I drove alone to Florida at Christmas 1963, stopping at the Florida State University Alligator Point Marine Lab for *Leucothrix* work and then on to south Florida where I visited Caribbean Gardens and saw a large sulfur spring with lots of whitish bacterial material. It was there that I saw *Thiothrix* for the first time.

In the summer of 1964 I went back to Friday Harbor Laboratories, driving by way of the Tetons and Yellowstone. This was around July 10, and the weather was marvelous. I saw my first hot spring at the West Thumb Geyser Basin, framed in the background by the frigid waters of Yellowstone Lake. Who could resist this image?

I was stunned by the colors and the abundance of phototrophs in the thermal waters flowing across the sinter into the lake. A ranger was giving what was called a “Geyser Cone Talk”, explaining the geology of hot springs and geysers. “How about the microbes,” I asked? “Blue-green algae” he said. I filed this away in my mind.

After a visit to Old Faithful and Norris Geyser Basin I left the Park by way of the North Entrance and continued on to Seattle and Friday Harbor Laboratories. I spent the summer doing research on *Leucothrix* and *Cristispira* (a bacterial symbiont of clams), and did extensive work on my new microbial ecology book. It was now clear to me that microbial ecology had tremendous potential and I intended to really get serious about it. (I was several years ahead of the ecology game, as the environmental movement really got started with the first Earth Day in April 1970.)

From Friday Harbor Laboratories I took a trip to Olympic National Park—August 15, 1964—where I visited Olympia Hot Springs and saw *Thiothrix* and blue-green algae (now called cyanobacteria) appearing together, with cyanobacteria growing at the higher temperature. The *Thiothrix* came in only when temperature had cooled to about 25°C. This was my first sampling of a thermal gradient.

I returned home from Friday Harbor Laboratories by way of Yellowstone, looking not for cyanobacteria but for *Thiothrix* in sulfur springs. I collected samples on August 21, 1964 at Norris, the Paint Pot area, Monument Geyser Basin, and Mud Volcano Basin, all sulfur-rich areas. Although I did not know it at the time, all of these areas were much too hot or much too acid for *Thiothrix*. The only concrete thing that developed from that visit was the photo Louise took of me sampling near the Mud Volcano area, a photo that has been widely used.

Although I was disappointed by my second visit to Yellowstone National Park, I began to think more about springs, hot or sulfur, in earnest.

Sometime in early 1965, I learned about Surtsey, the new volcanic island off the south coast of Iceland, and the support for research visits via the U.S. Atomic Energy Commission (AEC). Iceland attracted me because of its extensive thermal areas, and the classic research that had been done there. I made inquiries and found that although a visit to Surtsey was required, I could also do work in any Icelandic thermal areas. Here was an opportunity to visit. My simple application was funded, and I began to plan for a two-week visit to Iceland in late July, early August 1965.

By this time my microbial ecology book was essentially finished, and coming back from Florida after Christmas 1964 I stopped at the University of Georgia to talk about my book with Eugene Odum, the distinguished and influential ecologist. I was very familiar with the paper that his brother Tom had published on Silver Springs, a large cold-water spring

in central Florida: [Howard T. Odum; Ecological Monographs; Vol. 27, No. 1 (Jan., 1957), pp. 55-112 Trophic structure and productivity of Silver Springs, Florida.]

Eugene Odum knew that microorganisms were extremely important in ecosystem function and was very enthusiastic about my book. We discussed extensively the whole ecosystem concept. Tom Odum's  Silver Springs paper got me thinking about model systems in which ecological principals could be worked out.

In my mind I put together Odum's concepts and the upcoming Surtsey visit, and decided that I should visit Yellowstone again before going to Surtsey.

# West Thumb Geyser Basin

*Who could resist this image? A boiling spring (foreground) flowing into frigid Yellowstone Lake and providing habitat for thermophiles. A complete thermal gradient. I often used this photo at the beginning of talks I gave at various universities. It shows what has become the "classic" Vee-shaped pattern formed by photosynthetic microbes, illustrating the upper temperature limit for photosynthetic life. West Thumb Geyser Basin, Yellowstone Lake in background. June 1, 1968.*
***PICT0180.jpg***

# Sampling a small hot spring

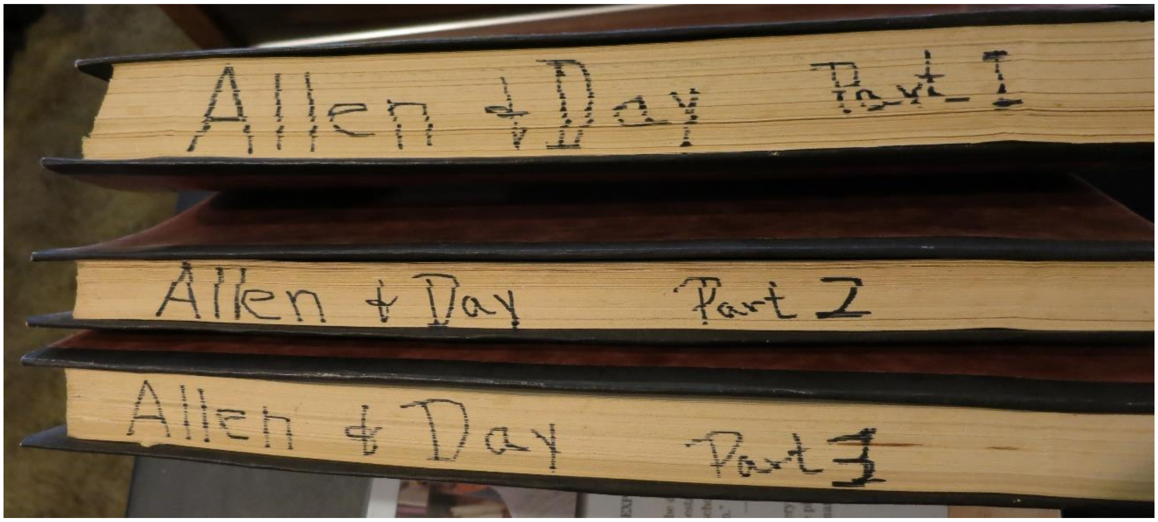
*Tom Brock sampling a small hot spring adjacent to the Yellowstone River, August 23, 1964. Photo by Louise Brock.*
***PICT0221.jpg***

## Getting some background on Yellowstone science

I spent a lot of time in the winter/spring of 1965 searching the scientific literature for information on thermal areas. I quickly discovered a reference to the Allen and Day book on hot springs of Yellowstone [Allen, E.T. and Day, A.L. 1935. Hot springs of the Yellowstone national park. Carnegie institution of Washington, 525 pp.] A copy of that book was in the Indiana University Geology library. Xerox had by that time become available, so I arranged to have a whole copy made and bound. This book became the "bible" for my field work in the Park.

In 1963 a scientist named Ellis Kempner had published in Science an interesting paper entitled "The Upper Temperature Limit for Life". It was based on some preliminary work he had done on thermal gradients in Yellowstone, measuring biological activity via the uptake of radioactive phosphate (32-P). Although Kempner was wrong about the upper temperature for life, his paper was crucial in getting me started on using radioisotopes to measure activity of microbes directly in hot springs.

# Geology of Yellowstone hot springs and geysers

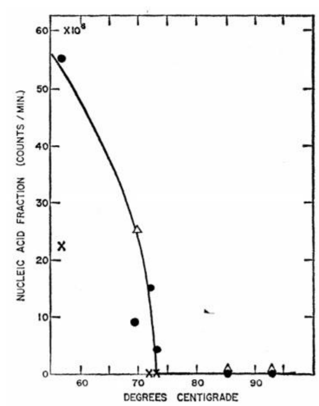



*Xerox was a relatively new device when these copies were made of the Allen & Day treatise on the Yellowstone hot springs in 1964. These have been my "bible" for over 50 years!*
***Allen & Day copies-4380,jpg***

# Kempner's paper in Science

**Upper Temperature Limit of Life**

Abstract. *Samples of microorganisms from the hot springs of Yellowstone National Park have been collected and tested for the ability to utilize radioactive phosphorus. No evidence for growth was found above 73°C.*




*Graph from Kempner's paper.*
*Kempner, E.S. 1963. Upper temperature limit of life.*
*Science Vol. 142: 1318-1319.*
***Kempner 1.jpg***
***Kempner 2.jpg***

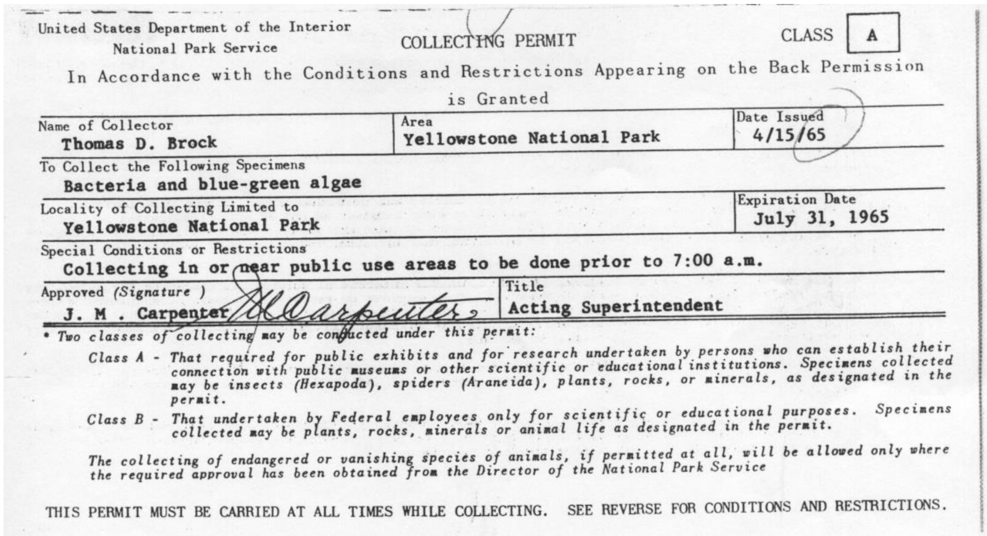
## The First Permit for Yellowstone Research

When I applied to Yellowstone National Park for a collecting permit that I submitted in March 1965, I used as a title: “Thermal Control of Primary Productivity”. In deciding on this title, I had in mind Tom Odum’s paper on Silver Springs, Florida. Perhaps being forced to think up the title directed my thinking about the research project.

The June 1965 work at Yellowstone was going to involve measuring what the ecologist calls “standing crop”. For a microbial system, that meant measuring chlorophyll, protein, and nucleic acid of cores taken from microbial mats at different temperatures along the thermal gradient.

To work out the methods, I used an algal mat in a small cold spring that I had discovered in the woods on the center of the Indiana University campus. I took cores of this mat and Louise and I did preliminary extraction of chlorophyll and assays for protein, RNA, and DNA. We verified that the technique would work and that good, consistent data could be obtained. We then put together reagents and equipment that we would need for similar work at Yellowstone. The key equipment was a Bausch and Lomb Spectrophotometer, Sorvall centrifuge, motorized homogenizer, and hot plate. I also brought a small Zeiss phase microscope of the kind we used in the microbiology teaching laboratory. Most important of all was a Yellow Springs Instrument Company thermistor and probe, which made measuring temperatures in hot pools or thermal gradients relatively easy. (We also brought along a laboratory mercury thermometer for calibrating the thermistor and for cross-checking the temperature measurements.)

# The First Collecting Permit



United States Department of the Interior
National Park Service

COLLECTING PERMIT

CLASS A

In Accordance with the Conditions and Restrictions Appearing on the Back Permission is Granted

| Name of Collector | Area | Date Issued |
|---|---|---|
| Thomas D. Brock | Yellowstone National Park | 4/15/65 |

To Collect the Following Specimens
Bacteria and blue-green algae

| Locality of Collecting Limited to | Expiration Date |
|---|---|
| Yellowstone National Park | July 31, 1965 |

Special Conditions or Restrictions
Collecting in or near public use areas to be done prior to 7:00 a.m.

| Approved (Signature) | Title |
|---|---|
| J. M. Carpenter | Acting Superintendent |

* *Two classes of collecting may be conducted under this permit:*

*Class A - That required for public exhibits and for research undertaken by persons who can establish their connection with public museums or other scientific or educational institutions. Specimens collected may be insects (Hexapoda), spiders (Araneida), plants, rocks, or minerals, as designated in the permit.*

*Class B - That undertaken by Federal employees only for scientific or educational purposes. Specimens collected may be plants, rocks, minerals or animal life as designated in the permit.*

*The collecting of endangered or vanishing species of animals, if permitted at all, will be allowed only where the required approval has been obtained from the Director of the National Park Service*

THIS PERMIT MUST BE CARRIED AT ALL TIMES WHILE COLLECTING. SEE REVERSE FOR CONDITIONS AND RESTRICTIONS.

*My first Yellowstone collecting permit, issued April 15, 1965. My application had been submitted in late March.*
***Collecting permit 1965 to part.jpg***

# Yellowstone 1965

Our first work day in Yellowstone was June 19, 1965. We stayed at Old Faithful in what was called a Campers Cabin, where our first biochemical assays were run. For the first two days I mainly surveyed hot springs, looking for good thermal gradients. Because we were forbidden from sampling in public areas after 7 AM, the very public Upper Geyser Basin areas were essentially off limits.

On June 20, I surveyed the Lower Geyser Basin and made my first description of an microbial vee—in the Firehole Pool effluent. This was important because it confirmed Kempner's observations that that there was an upper temperature for phototrophic organisms at about 73 C.

However, in some of the flowing springs in the White Creek area there were pink "streamers" at 88 C, much higher than Kempner's "upper temperature for life". Microscopy and biochemical assays quickly showed that these were living organisms.

We took cores of microbial mats at different locations in the thermal gradient at Firehole Pool and brought them back to our cabin at Old Faithful. There was only one electrical outlet (the light bulb in the ceiling) and when operating colorimeter, hot plate, and centrifuge, we had to stop one thing to do the other, since we had no 3-way adaptor.

On June 21 we continued to survey, looking at Biscuit Basin, more springs in the Lower Geyser Basin, Norris Geyser Basin, and Nymph Creek (a beautiful green, later identified as *Cyanidium caldarium*, a eukaryote).

We stopped at Park headquarters at Mammoth Hot Springs, to check in with the naturalists and let them know that we were in the Park. There I talked with Brian Harry (deputy chief naturalist) who told me that Ken Temple of Montana State was working in Yellowstone and we should make sure our work didn't interfere with his. I called Temple from a pay phone and he asked me to visit. On June 22 we went to Bozeman. When we arrived (in rather grungy field clothes) we discovered that I was to give a seminar and that the department chair, Robert McBee, had organized a lunch for us (at 4B's Restaurant), including Temple, limnologist John Wright, their wives, and some graduate students.

Temple indicated that his work was still very preliminary and that it certainly would not conflict with ours.

Of more interest to me was John Wright's work on the Madison River. Wright was doing primary production work, which followed the lines of Eugene and Howard Odum. Further, he had a field laboratory just inside the Park boundary near West Yellowstone, and there would be space for us. His field lab was in a building owned not by the Park Service but by the Yellowstone Park Company at an area called "The Barns", which was along the Madison River near W. Yellowstone. The facility not only provided plenty of electrical outlets, but tables, and kitchen facilities. We could even sleep there (on the floor, using our sleeping bags and foam pads).

We stayed at The Barns from June 22 until around July 1, sampling various thermal gradients and then doing assays in the evening. Despite all-day rains, we got good samples. I quickly realized how useful it was to have a permanent research laboratory so near the hot springs.

# The Barns: Our First (Temporary) Laboratory in Yellowstone


*This building, or one similar to it, was where Louise and I set up our laboratory during the last week of June, 1965, courtesy of MSU limnologist John Wright. I remember that the building was painted yellow. There were several other buildings nearby. They were all on a flat area near the Madison River. The area was called "The Barns", because in the early years horses were kept here for use with tourist groups. When we were there, the buildings were mostly unused, but a few bus drivers still bunked here, driving the large yellow buses that took tourists around the Park. All of the buildings are now gone, but there is still a gravel road here, providing access to what is called "The Barns Hole", for fishing. (See Fly fishing on the Madison River.) Image courtesy of the Yellowstone Heritage and Research Center.*
***Driver's bunkhouse Riverside 1951.jpg***

# The B & L Spectronic 20, a workhorse of our research in the early years.


*The B & L Spectronic 20 was a versatile instrument, that we used for chlorophyll, protein, and nucleic acid assays in the first summer and for many summers thereafter.*
***B & LSpec 20 from EBay.jpg***

# The centrifuge, a vital instrument for our research


*Ivan Sorvall desktop centrifuge. Another work horse in the Yellowstone research lab.*
***Sorvall centrifuge image from EBay.jpg***

The details of the methods we worked out are given in a paper we published in Limnology & Ocenography: “The measurement of chlorophyll, primary productivity, photophosphorylation, and macromolecules in benthic algal mats.” Vol. 12, 600-605 (1967). Here is a summary:

- Samples were taken in the field with brass cork borers and transferred to glass vials or tubes containing spring water.
- For photosynthesis measurements, each vial was injected (syringe) with radioactive bicarbonate.
- After incubation, a small amount of liquid was removed (for later measurement of the amount of radioactivity), and the biological activity stopped by adding a small amount of formaldehyde.
- Vials were returned to the laboratory in West Yellowstone for processing.
- The volume of each vial was made up to exactly 5 ml with water and the contents transferred to a 15 ml polypropylene centrifuge tube into which a Teflon homogenizer fit snuggly.
- The algal core was homogenized using an electric stirring motor. This created a uniform suspension from which subsamples could be taken.
- 0.5 ml of homogenate was filtered through a membrane filter, washed, and glued to a planchet for counting in a Nuclear-Chicago gas-flow counter with thin window.
- The remaining homogenate was centrifuged in the tube, at 5,000 rpm for 10 min, which sedimented all the chlorophyll and protein-containing material.
- The supernatant was discarded and the pellet suspended in 5 ml acetone and rehomozgenized.
- The tube was again centrifuged and the acetone extract decanted into a cuvette for chlorophyll determination.
- Chlorophyll was measured on a B & L Spectronic 20 colorimeter at 665 nm (red-sensitive photocell), and chlorophyll a was calculated following Odum.
- The pellet from the acetone extraction was resuspended and used to measure protein and nucleic acid.
- The procedure makes it possible to obtain radioisotope data on the same sample for which chlorophyll, protein, and RNA are measured.
- Thus, even heterogeneous natural samples can be measured quantitatively.

We left Yellowstone around July 2, leaving by way of the Northeast Entrance—Beartooth Highway—saw heavy red snow (snow algae). (Later Anne and Jerry Mosser, and I studied the activity of these organisms at 0 C.) Returned via Billings, eastern Montana, South Dakota, Iowa. Home July 6. Until July 23 we did a lot more assays on samples we brought back with us.

I left for Iceland in late July 1965, on the way spending a few days doing *Leucothrix* work at John Sieburth's marine laboratory at Narragansett Bay, Rhode Island. My visit to Iceland was successful, and I not only sampled another thermal gradient, and made some relevant observations on Surtsey, but was also able to do some *Leucothrix* work (with radioisotopes) on the SW marine coast.

## The National Science Foundation (NSF) Proposal

Although I had three grants underway at Indiana University, none of them were ecological. I now had two separate ecological projects, either of which could be developed extensively: 1) Marine microbiology, involving work on *Leucothrix* or other microbes at Friday Harbor Laboratories; and 2) The Yellowstone project.

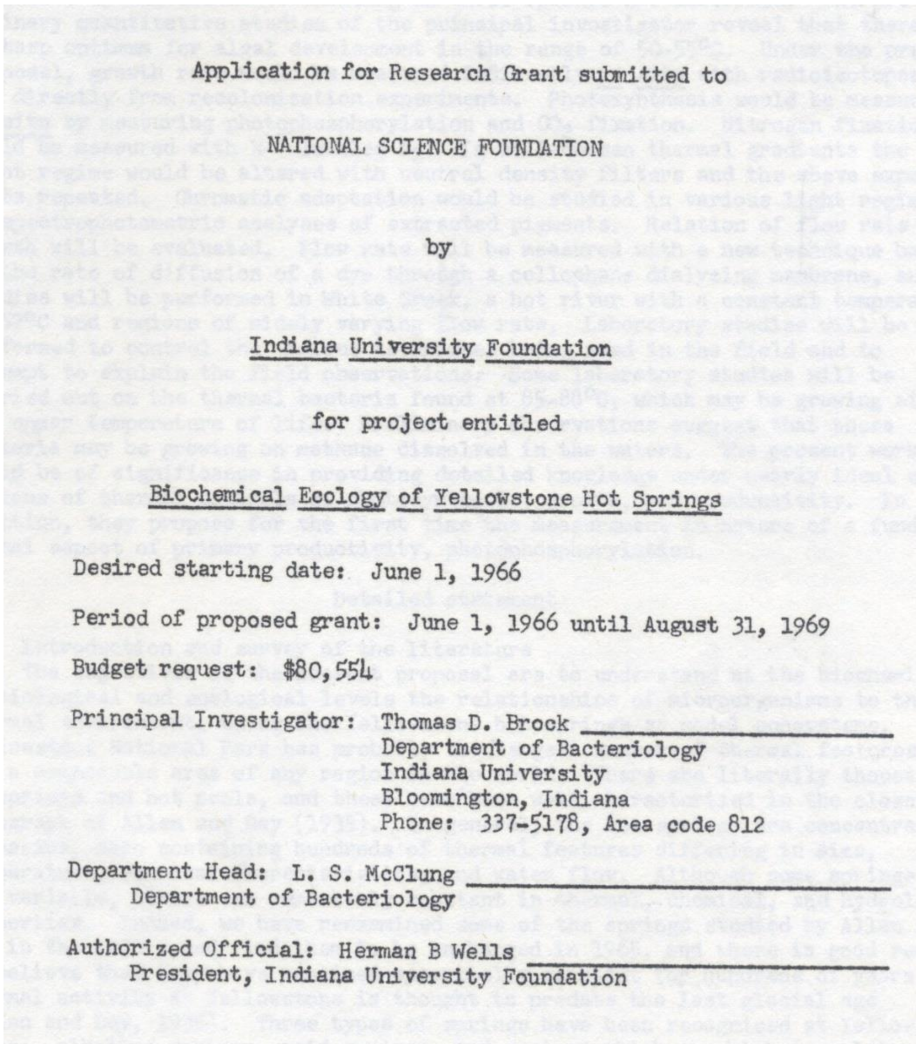
It was not quite like tossing a coin, but the Yellowstone project won. I wrote up a proposal to National Science Foundation and it was submitted on November 22, 1965. (The *Leucothrix* work continued via the help of technician Pat Holleman and research at Friday Harbor Laboratories by two of my graduate students, Mike Kelly and Judith Bland. Eventually, there were 11 papers on the *Leucothrix* "sideline" published out of my laboratory.)

I have a mimeographed copy of that first National Science Foundation proposal. The proposal is concise, well written, and full of ideas. It had a strong ecological focus, but involved extensive work using biochemistry, radioisotopes, and microbiology. The language was very ecological, and Eugene Odum's impact is evident. This may have been why it was given to the Environmental Biology (later General Ecology) program at National Science Foundation.

A very important part of this first proposal was the discussion of the previous work that had been done on hot springs. This derived from the extensive library research I had done beginning in late 1964 after my return from Friday Harbor Laboratories and the west. I started my literature survey with the Indiana University Library and the Lilly Rare Book Library. I then went to the John Crerar Library in Chicago. (Chicago was a 4-hour drive from Bloomington.) This famous library may be the largest scientific and medical library in the world, and is open to the public. In addition to the card catalog search terms "hot springs" and "geysers", I used "balneology", which deals with the medical uses of hot springs. This was the start of my work to learn about hot springs around the world.

Works I cited in my National Science Foundation proposal, in addition to Allen and Day, included Barth's big monograph on hot springs and geysers of Iceland; Copeland's extensive (but outdated) monograph on Yellowstone cyanobacteria; Emoto's 1933 review (in German) of microorganisms in hot springs, mainly in Japan; Petursson's 1961 review of thermal cyanobacteria of Iceland; Setchell's important early paper (1903) on the upper temperature of life; Tuxen's 1944 review of hot spring insects of Iceland; and Vouk's 1950 book: "Grundriss zu einer Balneobiologie der Thermen".

# The first research proposal to NSF



Application for Research Grant submitted to

NATIONAL SCIENCE FOUNDATION

by

Indiana University Foundation

for project entitled

Biochemical Ecology of Yellowstone Hot Springs

Desired starting date: June 1, 1966

Period of proposed grant: June 1, 1966 until August 31, 1969

Budget request: $80,554

Principal Investigator: Thomas D. Brock
Department of Bacteriology
Indiana University
Bloomington, Indiana
Phone: 337-5178, Area code 812

Department Head: L. S. McClung
Department of Bacteriology

Authorized Official: Herman B Wells
President, Indiana University Foundation

*Cover page for the first NSF grant proposal on the Yellowstone work. In those days, most university departments did not have Xerox machines, so the proposal was typed on a mimeograph master and multiple copies printed.*

***NSF 1966 proposal cover page.jpg***

The words “Biochemical ecology” in the title were somewhat pretentious and probably unclear. Later, National Science Foundation changed the title to: “Biochemical ecology of microorganisms in a thermal environment”. I was never privy to why “Yellowstone” was dropped. In subsequent revisions, I changed “biochemical ecology” to “microbial ecology”.

The first sentence of the Abstract lays out the program: “The objectives…are to understand at the biochemical, physiological, and ecological levels the relationships of microorganisms to the thermal environment, using the Yellowstone hot springs as model ecosystems.” I reviewed my previous work, which showed that thermal control of microbial growth and productivity is an important fundamental problem and that “the Yellowstone hot springs are an excellent natural laboratory for these studies.” I cited a paper of mine that was already in press in Nature: “Temperature optima for algal development in Yellowstone and Iceland hot springs,” based on the summer 1965 work in Yellowstone and Iceland. I described in considerable detail the characteristics of hot springs and the creation of thermal gradients. I described the biochemical methodology we had already perfected.

The experiments to be done were described in detail, the scheduling was laid out, the justification of the budget was clear. In all, this was a very well-written proposal. The proposal met favorable reviews and was funded (at a slightly lower budget and for two rather than four years). NSF renewed this grant every two years for the duration of my work in Yellowstone.

## First oral presentation on the Yellowstone work

After my return from Iceland, on August 19, 1965 I submitted an abstract of a paper to be presented at the annual meeting of the Ecological Society of America, which was being held with the American Association for the Advancement of Science in Berkeley right after Christmas. The title was “Thermal control of primary productivity in hot springs of Yellowstone and Iceland.” The brief abstract discussed the hot spring as an excellent model for such a study, because of the existence of stable thermal gradients. The term “steady state” was used.

After my Berkeley talk, I was queried by Bob Lenn, a graduate student at U.C. Davis who was just completing his M.Sc. with Charles Goldman on microbial mats in Drakesbad Hot Springs, in Lassen Volcanic National Park. He asked about a job for next summer at Yellowstone, and I agreed tentatively to hire him provided my National Science Foundation grant was funded. The grant was funded and I did hire Lenn, who helped me do my first photosynthesis experiments using radioactively-labeled bicarbonate.

Soon after the Christmas meeting, Louise and I left Bloomington for Italy by way of the Azores and Portugal for a semester sabbatical, not to return until early May 1966.

On the way to Portugal, we stopped at the Azores, where there are extensive hot springs on the island of Sao Miguel. The Azores is in the middle of the Atlantic Ocean, and like Iceland is on the Mid-Atlantic Ridge. Our work on these hot springs was the first that had ever been done on this isolated area.

In March 1966, while I was in Italy, I received a letter from National Science Foundation telling me that my proposal had received “favorable attention and would probably be funded.” In early May, right after I returned, I received a call from NSF saying that the proposal was being funded, for 2 years, and asked me to submit a revised budget. When would the starting date be? I asked. June 1. Three weeks away!

On May 10 I submitted the revised budget, and shortly after that I asked the department to put in an application to hire a new research technician (Bob Lenn) for the summer. I also reserved a station wagon for the summer from the Indiana University motor pool.

We were off and running!

# Sampling a hot spring in the Azores

*TDB making notes at a thermal pool in the Furnas Valley thermal area, Sao Miguel, Azores, Portugal. Most of the springs were highly disturbed, quite a contrast to the pristine and highly protected springs in Yellowstone. January 15, 1966*
***PICT0224.jpg***

# Yellowstone 1966

I had already made rental arrangements for the summer of 1966 in anticipation of NSF approval for the grant. Actually, I had decided to go to Yellowstone in 1966 even if the grant had not been approved, using my own money. I had reserved Ellen Daley's big green cabin for Louise and I for August—and a housekeeping cabin at a motel for June through August for Bob Lenn and his wife. Bob Lenn and I met in early June in West Yellowstone. Technicians Pat Holleman and Sally Murphy, and Louise spent a lot of time getting reagents and supplies ready.

I left Bloomington on June 3 in the Indiana University station wagon alone. The trip was via Kansas City, Rock Springs and Jackson (WY), and into Yellowstone past West Thumb Geyser Basin (1650 miles; 28 hours driving time). Yellowstone Lake was still ice-covered except for the thermal input areas near the West Thumb Geyser basin. The weather was great and I took some pictures of microbial mats that I have used often since in talks. I also took photos of ephedrid flies that were living in the cooler areas of the mats. (I had learned of the existence of these flies after a query by Frank Young of the Indiana University Zoology department). Bob Lenn and I set up a primitive lab in a housekeeping cabin, using the supplies and equipment I had brought along (including radioactively labeled bicarbonate).

I had been using radioactively labeled compounds in the laboratory for a number of years, and had a license from the Atomic Energy Commission. However, I was careful to inform the Yellowstone Superintendent's office of what we were doing. I explained that only low levels of radioactivity would be used and that it would all be confined to sealed containers. I received a letter from the Superintendent's office giving me permission to do isotope experiments in the Park.

Lenn and I toured the Park and identified some thermal areas and I decided to focus on Mushroom Spring in the Great Fountain Geyser area, where Louise and I had worked in 1965. We got started, doing preliminary carbon dioxide incorporation studies on cores from microbial mats, plus protein and chlorophyll determinations.

In addition to Lenn, I had as visitor W.D.P.Stewart, a British ecologist who was studying nitrogen fixation in cyanobacteria using the stable isotope 15-N. I had met Bill in London in April 1966. He was temporarily in the U.S. at UW-Madison, where major research on nitrogen fixation in all sorts of creatures was being carried out,. At my invitation he had come to West Yellowstone. Although he was mostly self-contained, he used some of our space at the motel.

I left Lenn on his own in West Yellowstone with Stewart and went to Seattle for the American Society of Limnology and Oceanography meeting, where I gave a paper on *Leucothrix*. There I met Richard Castenholz for the first time. He had been working on cyanobacteria and other organisms in some hot springs in Oregon, and had cultures of the dominant cyanobacterium, a *Synechococcus.* Castenholz visited me at Yellowstone several times and we became good friends.

I flew from Seattle to Chicago and Bloomington, keeping track of Lenn's work over the phone and by letter. I spent the middle of the summer working on my yeast and *Leucothrix* projects.

For the August research 1966 trip to Yellowstone, Louise, technician Sally Murphy, and technician Pat Holleman spent days packing everything for the month's research. Louise, Sally, undergraduate student Hudson Freeze and I drove out to Yellowstone in another motor pool car (a Plymouth sedan, heavily overloaded) in early August.

We set up a larger laboratory in Daley's big green cabin. The gas flow counter (for counting radioactivity) arrived from the manufacturer by air freight and was set up. We discovered that they had sent the wrong lead weight for the detector. After a phone call they sent another by air freight. (Nice to have an airport with commercial service only a mile away!)

We got a surprising amount of work done that August—mostly at Mushroom Spring—and opened up many new problems. Bob Lenn left late in August for a teaching job in California. Although we quickly extended his work, the experiments he had done earlier in the summer were crucial for our more detailed photosynthesis studies.

We all left the second week of September—Sally's husband John (who had flown out in late August) and Sally drove the Plymouth sedan back. Louise and I drove the IU station wagon—Hud Freeze flew back to get there in time for fall classes.

Before we left, Freeze and I took samples of mat material from the upper end of the thermal gradient at Mushroom Spring. We also collected large amounts of Mushroom Spring source water to use making up culture media. Later, back in Bloomington, Freeze isolated from the mat samples what came to be called *Thermus aquaticus*.

Although *T. aquaticus* is easy to isolate and culture, there was one "trick" that turned out to be important. The culture medium needed to be *fairly dilute* in concentration of organic nutrients. To Mushroom Spring water, additions were made of 0.1% tryptone and 0.1% yeast extract. These concentrations are much lower than used in most bacteriological culture media. I used these low concentrations because of my previous experience isolating other aquatic microorganisms, especially from marine environments.

I also isolated *Cyanidium* using material Sally collected at an acid location in Obsidian Creek.

In all, 1966 was a good year!

# Getting ready for Yellowstone

*Getting ready for Yellowstone in Jordan Hall Room 359. Probably late July 1966. Sally Murphy, Pat Holleman, Louise. A long list of tubes, pipettes, reagents, and equipment had been prepared and everything was carefully packed. We had a Plymouth sedan from the IU motor pool that was heavily loaded. We limped into West Yellowstone after three days on the road. Sally and Louise went along with me to Yellowstone and Pat remained in Bloomington to handle the other research projects.*

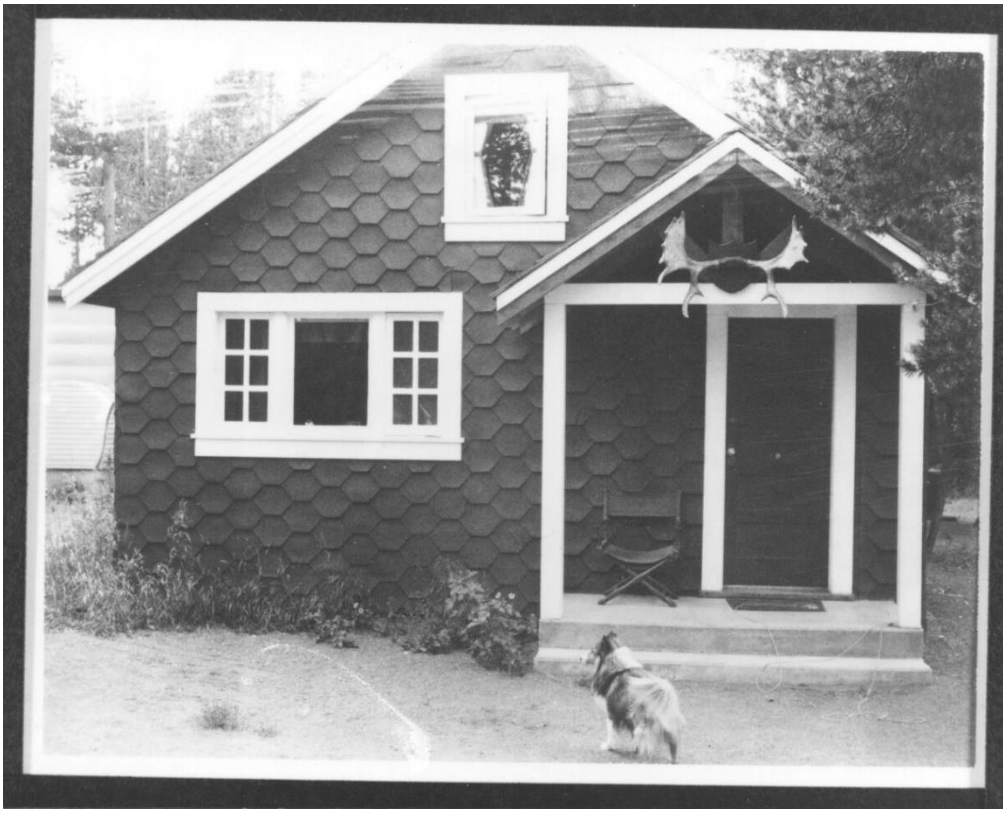
***Sally Louise Pat getting ready for Yellowstone 1965 BS263.jpg***

# First observations at Firehole Pool

*Firehole Pool; Aug 8 1966; studying the organisms in the "Vee"; Louise; Sally Murphy; Hud Freeze; Bob Lenn; note field box in foreground.*

***Firehole Pool 1966 Louise Sally Murphy Hud Freeze Bob Lenn 8-8-1966 BW283.jpg***

# August 1966 lab: Daley's big green cabin



*Daley's big green cabin that we rented in the early years to use as our field laboratory. It had two bedrooms, kitchen, and bath. The gas flow counter was set up in the front bedroom. The kitchen was converted to a lab (see photos below), although we also fixed meals there. West Yellowstone; 1966.*

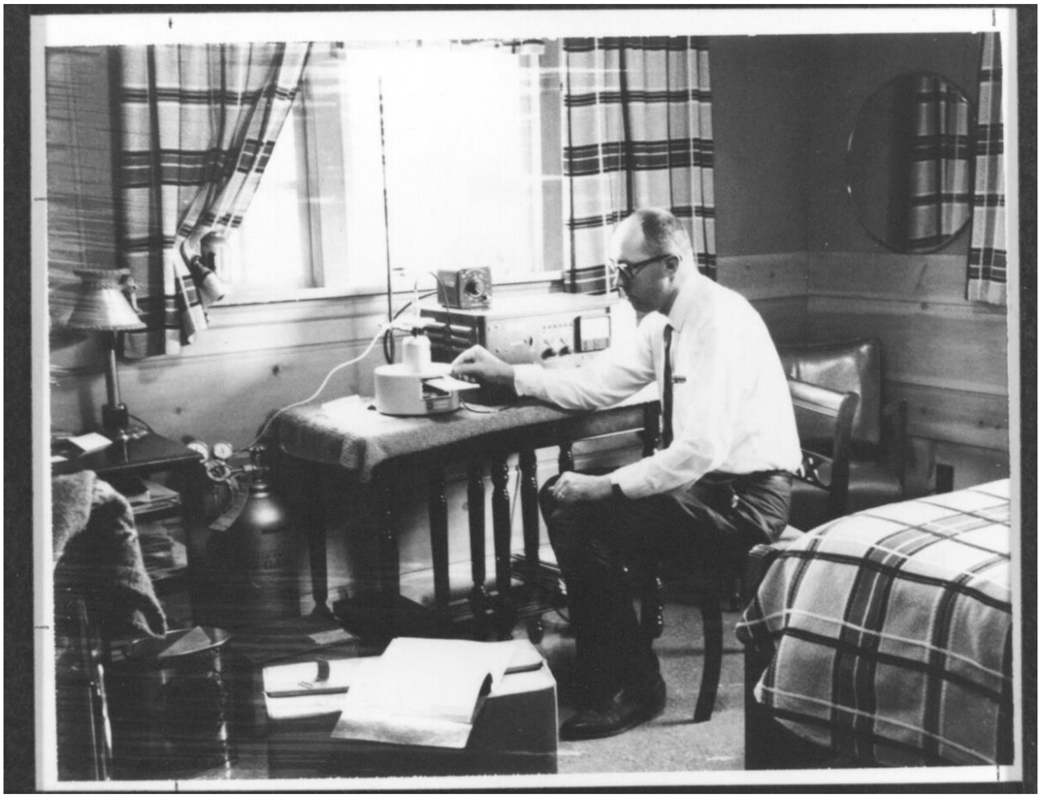
***Daley cabin Korby 1966 BC126.jpg***

# The big green cabin in 1967

*Daley's green cabin, where the Yellowstone work first started! The station wagon came from the IU motor pool. The wagons in those days were heavy boats. However, the Interstate system was in its early stages, so there were lots of normal roads to deal with. September 2, 1967.*

***PICT0196.jpg***

# Counting radioactive samples in the bedroom



*Tom Brock counting radioactivity, Nuclear-Chicago gas flow counter; front bedroom of Daley cabin; Q gas tank; end-window manual counter; scaler; August 1966. Why is the operator wearing a white shirt and tie? (Days long ago)*

***Daley gas flow counter TDB 1966 BC124.jpg***

# One end of the kitchen laboratory

*Daley cabin; August 1966; Louise; kitchen was also the lab; Spectrophotometer; centrifuge;homogenizer; reagents; radioactive glassware washed in the bathtub. In those days women often wore skirts to work?*

***Daley lab interior Louise 1966 BC127.jpg***

# Other end of the kitchen laboratory

*Daley cabin interior; August 1966; part of the kitchen lab; pH meter;two-pan balance; pipettes and reagents; Louise.*

***Daley lab interior Louise 1966 BC125.jpg***

## Winter Research in the Park

In 1966 I learned about the winter research program that Vincent Schaeffer of State University of New York at Albany was running out of Old Faithful. Schaeffer was an atmospheric scientist (he had discovered the phenomenon of cloud seeding), and since the early 1960s had been running a winter research program based in an old Park Service building at Old Faithful.

Schaeffer had discovered that when a geyser erupts in the cold of winter it produces a supercooled fog that is somewhat analogous to an atmospheric cloud. By introducing various artificial nuclei such as silver iodide or dry ice he could create mini-snow storms, analogous to what happens in the atmosphere. In those pre-snowmobile days, the pristine air of the Old Faithful area made it an ideal location for this sort of work. (See papers 129 and 130 in the bibliography for references to the history I wrote of Schaffer's winter work.)

Schaeffer had very broad interests and realized that all kinds of scientists, even biologists, might be interested in spending some time at Old Faithful in the winter. By the mid 1960s his winter program had evolved into a four-week multi-disciplinary project, involving all sorts of scientists. Supported by the National Science Foundation, he invited a select group of scientists to spend a week at Old Faithful in January or early February. I heard about it and in summer 1966 contacted Schaeffer.

He was quite interested in my work and invited me to participate in his program. I did so in January 1967 and again in late January-early February 1968.

Those were the days before there were any winter facilities at Old Faithful. The roads in the Park were not plowed, but kept open by the snowcats that came up occasionally from West Yellowstone or Park Headquarters at Mammoth. There was a single Park Ranger at Old Faithful in the winter, who got around by snowcat.

My research site, Mushroom Spring, was about 10 miles away from Old Faithful. The Park Ranger gave me a "lift" to the entrance to the Firehole Lake Loop Road, but I had to "walk" the last two miles on snowshoes. Among other things, I had to detour around a large bison who was browsing in some of the grass made snowless by thermal activity.

I was able to visit Mushroom Spring and determined that the spring was still flowing and the microbial mat was still present. However, the thermal gradient was considerably compressed, and the color of the mat was a dark green, showing adaptation by the cyanobacteria to the reduced light availability in winter. Although I did not do a detailed study, I was able to show that the microbial mat was a dynamic system, with the microbes moving up or down the channel depending on the extent of the thermal gradient (summer or winter).

# The bunkhouse and commissary for the winter research program

*Yellowstone Field Research Expedition (YFRE), February 1, 1967. The bunkhouse was an empty NPS building in the Service Area of Old Faithful. Shower and sleeping facilities were in another building. Because there was no heat in the shower area, the water had to be kept running constantly to prevent freezing. It was quite exciting stepping out of the hot shower into the cold building! Personnel and supplies were brought in by rented snow cat but travel around the geyser basin was either by foot or snowmobile.*
***PICT0183.jpg***

# The dining room, where seminars were also run

*A full time cook was hired for the duration of the expedition. Each night after dinner a seminar was held.*
***PICT0187.jpg***

# Observing insects in action near Castle Geyser

*Tom Brock and Vince Schaefer observing brine flies at a small spring near Castle Geyser. January 1967. Photo courtesy of SUNY Albany*

***brine fly spring brock and Schaefer 2 resized.jpg***

# Winter research group in February 1968

*Researchers participating in the Yellowstone Field Research Expedition. Tom Brock is in the light jacket at the rear. Noted grizzly bear researcher John Craighead is kneeling in front of the man in the blue jacket. Early February 1968.*

***yfre group 1968 tom brock resized.jpg***

# Tom Brock at Mushroom Spring, January 1968

*In our early years at Yellowstone, I was interested in what happened to the springs, and their microbial flora, in the winter. This is me at Mushroom Spring on January 28 1968, working with grad student Bill Doemel and Louise on a radioactivity experiment. Once we found out that the springs flowed all year round, and that the thermal gradient was shortened, and that the microbial mats were still there but had moved up the outflow channel due to their temperature optima, and had greatly increased their chlorophyll content in response to reduced solar insolation, we quit going in the winter, as it was a real hassle.*

***PICT0171.jpg***

## A Research Laboratory in Yellowstone??

Even in June 1965, while we were doing our first extensive assays out of “The Barns” just inside the Park boundary at West Yellowstone, I knew I wanted to have permanent laboratory facilities in or near the Park. After returning to Bloomington, one of the first things I did was write MSU limnologist John Wright offering to be a joint applicant to National Science Foundation for a Yellowstone lab. He never answered, so I looked into other possibilities.

In my first letter to Vince Schaeffer, I had raised the possibility of a year-around research laboratory. It turned out that he was also thinking about a permanent laboratory and had already made arrangements to discuss this with the Park Superintendent in August 1966. Since I was to be in West Yellowstone then, Schaeffer asked me to accompany him on the visit.

I picked up Schaeffer at the West Yellowstone airport on August 24 1966 and on the 25th we drove to Park Headquarters in Mammoth, where we met John and Frank Craighead, grizzly bear researchers who had a small temporary research facility in a concessionaire’s building in the Canyon Village area. Our three research groups (representing wildlife, microbiology, and physical science) discussed with the Superintendent the possibility of developing a permanent facility, possibly funded by the National Science Foundation. The conversations were fairly detailed but it became clear to me that the Superintendent did not want any “non-park” facilities inside Park boundaries. In fact, he was unhappy about the existing Craighead operation. Additionally, it was uncertain how much longer Schaeffer would be permitted to operate his winter program. (As it turned out, the last year that Schaeffer’s program was permitted was in 1970, after which his program was closed for good.)

After the meeting with the Superintendent, Schaeffer and I both concluded that there was no possibility of a permanent research facility inside the Park. He agreed with me that the best location would be West Yellowstone. At that time the rather large facilities of the Union Pacific Railroad at West Yellowstone were standing idle, as passenger service had been terminated a few years earlier. The buildings were in very good shape and could be easily adapted. Schaeffer was enthusiastic about getting one or more of these buildings for a lab.

Schaeffer put together a small committee of scientists to look into acquiring the Union Pacific property. He had convinced Charles Bradley, who was at that time a Dean at Montana State University in Bozeman, to be chair of the committee. In the summer of 1967 Schaeffer and I talked with the West Yellowstone mayor and city officials about using one of the railroad buildings. However, it was clear to me that the city did not want to use the railroad buildings for research, despite the “status” this might confer, but as a museum and convention center. Since tourism was the principal “industry” in West Yellowstone, this made sense to me. After these discussions, I decided that I must develop my own research facility.

**Advantages of West Yellowstone for a research laboratory**

West Yellowstone was the ideal location for a laboratory working on hot springs and geysers. It was immediately adjacent to the Park boundary. It was accessible all year by automobile, and in the summer direct by air and or via "Karst Stage" from a larger airport at Bozeman. In winter, one could fly to Idaho Falls and rent a car for a two-hour drive to West Yellowstone.

West Yellowstone was the most widely used entrance to the Park and was immediately adjacent to the Park boundary. It was 14 miles by an excellent Park road to Madison Junction. From there it was about 15 miles south to the Lower Geyser Basin or north to Norris Geyser Basin. Thus, it took less than an hour to reach a wide variety of hot springs suitable for research work.

West Yellowstone itself had over 100 motels, many cabins and housekeeping units, lots of restaurants, grocery stores, gasoline stations and auto repair facilities, a lumber yard, a bank, post office, snowcat service and snowmobile rental for winter, and an airport with air freight and scheduled airline service. (The West Yellowstone airport was only a couple of miles from town.)

Thus, I decided to work toward developing a laboratory in West Yellowstone. It would take time, and would involve a lot of negotiations with Indiana University, with NSF, and with local real estate people. But by September 1967 we had taken the first steps on the way to what came to be called the Laboratory of Thermal Biology.

## Adaptation to the upper temperature limit by *Synechococcus*

One of the striking things was the observation that near the upper temperature limit for phototrophic life, the microbial development was very meager. A few degrees below the upper temperature, defined mats started to form, but right at the upper limit there was just a thin film, barely visible. The cyanobacterium was microscopically similar throughout a wide temperature range from 70-73 C (the upper temperature limit) to 50-55 C, where the mats were the thickest.

One of the most interesting things I discovered was that the thin film at the upper limit was not struggling to survive, but was **optimally adapted** to that temperature. It functioned better here than at lower temperatures. I showed this by measuring photosynthesis with radioactive carbon dioxide, doing what I called "temperature transfer" experiments, as will be explained below.

The results of this work were published in a short paper in 1967. (Brock, Micro-organisms adapted to high temperatures. Nature: 214: 882-885.) This paper has been widely cited, but the broad implications for understanding evolution have not been followed up.

The experiments I did are the kind that can only be done in nature, because that is where the organisms have evolved. I note that this sort of work could not be done without having laboratory facilities nearby that permitted frequent and extensive monitoring. Driving home to Bozeman every evening was hardly an option!

This work developed out of the first work done with the cyanobacterial mats in 1965. The largest amount of microbial biomass was present at around 55 C, and the upper limit was about 73 C. In 1966 I came back to Yellowstone prepared to measure photosynthesis with radioactive carbon dioxide.

Here is the crux of the story:

> *"Of the micro-organisms which can grow at environmental extremes, one must distinguish between those which are optimally adapted to the extremes and those which grow better in less extreme conditions. This is especially important because an extreme environment is usually recognized as one in which growth of the organism is slow. An experimental approach to this must permit the direct measurement of the environmental variable in the habitat in which the organism is growing and in which it has therefore evolved. It is essential to examine the organism directly in its natural environment rather than in cultures isolated from such habitats, because it is not possible to be certain that the cultures are indeed representative of the natural material, and it is usually impossible to duplicate in the laboratory all aspects of the natural environment."*

The work was done in Mushroom Spring, which had a good stable thermal gradient. Permanent stations were established along that thermal gradient, and temperature measurements were carried out almost daily over a period of a month, to ensure that the

temperatures were (relatively) stable. The spring was monitored from Aug 8 to Sept 6 1966 and a figure in the paper shows all the temperature readings.

The work involved taking replicate samples of cyanobacterial biomass from each location and measuring photosynthetic efficiency (uptake of carbon per unit chlorophyll to standardize for sample variation) at a variety of temperatures above and below the location where it is growing. The incubation sites were in the outflow channel at various places along the thermal gradient at appropriate temperatures. Since the vials used were small, I had no problem finding suitable sites for the incubations.

In all cases there was a sharp optimum for photosynthesis at the habitat temperature, and lower photosynthesis rates at temperatures above and below the optimum. The graph) shows representative data.

# Photosynthesis of hot spring cyanobacteria

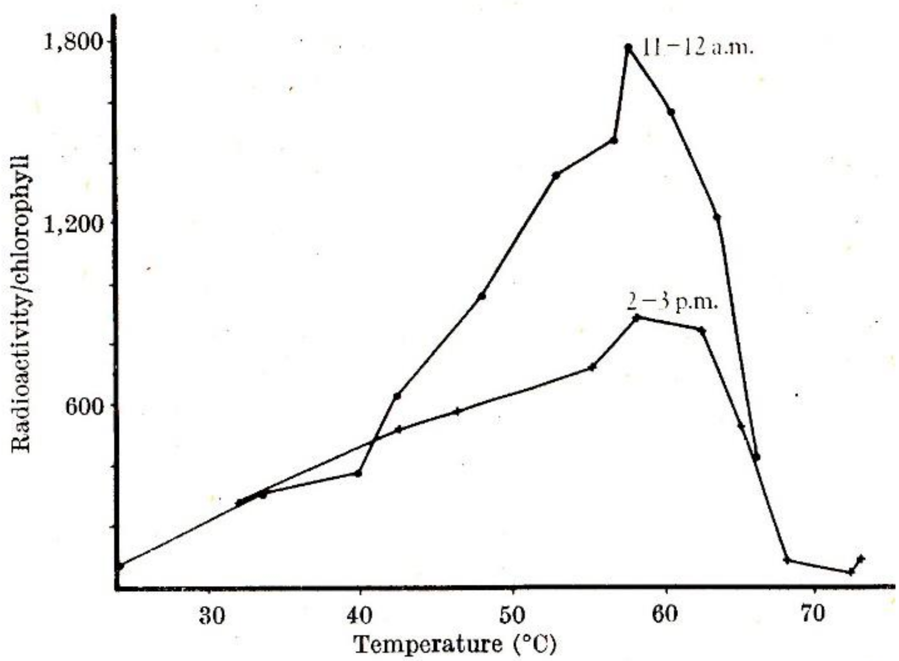



Fig. 3. Photosynthetic efficiency of algal cores taken from a location at 58·5° C and incubated at various temperatures. Curves are for separate experiments on August 29, 1966 (11–12 a.m.), and August 21, 1966 (2–3 p.m.). Values on the ordinate are c.p.m./$\mu$g of chlorophyll. Representative values for peak point of 11–12 a.m. series are: radioactivity, 33,300 c.p.m./core; chlorophyll, 19·2 $\mu$g/core.

*Figure from Brock 1967 (see Bibliography for citation).*
***Nature paper Figure 3.jpg***

The same type of experiment was done for samples from various temperatures along the thermal gradient. The final result was a graph plotting habitat temperature on one axis and optimum temperature on the other.

Figure 4 of the Nature paper (copied here) has the key data. The optimum temperature of each population is plotted against habitat temperature and there is a surprisingly good straight line. The conclusion is that the population taken from close to the upper temperature limit of 70 C was optimally adapted to that temperature.

The conclusions drawn from this work are quoted here from the original paper:

> *"From these results, we can conclude that the blue-green algae [cyanobacteria] in question have evolved so that their optimum temperature is the same as their environmental temperature. This is true even for the organisms growing at 70 C, which is close to the upper temperature at which blue-green algae can grow anywhere on Earth….Thus, although this temperature is so extreme that the biomass of organisms found there is very small…they photosynthesize better there than they do at lower temperatures. It seems that this is a remarkable and fundamental observation; it suggests that the reason why photosynthetic algae are not found on Earth at temperatures greater than 73-75 C is because of some inherent limitation in the organization of their cellular material which is impossible for them to overcome by further evolutionary changes."*

"This conclusion is even more striking when the antiquity of thermal algae is considered…" I discuss the fossil evidence for the antiquity of cyanobacteria.

"If so, this would mean that thermal algae (cyanobacteria) have had more than two billion years to evolve, and if they cannot grow at temperatures higher than 73-75 C it is not because of lack of time."

My reason for going through this work in detail is to indicate the importance of having laboratory facilities close to the field. The summer when this work was done (1966), was before we had established any permanent laboratory facilities. We had set up our lab temporarily for the month of August in Daley's big green cabin. Processing of samples took place in the kitchen, cleaning of radioactive glassware in the bathtub, and the radioactive counter was set up in the front bedroom. This paper credited Sally Murphy, Hud Freeze, Louise Brock, and Bob Lenn for help with the field work.

The following year I devised a technique for measuring the growth rates of the *Synechococcus* at temperatures at 69 C, just below the upper limit. The doubling time in Mushroom Spring was 40 hours, which is a fairly rapid growth rate, considering the extreme conditions.

# Temperature optima across the thermal gradient

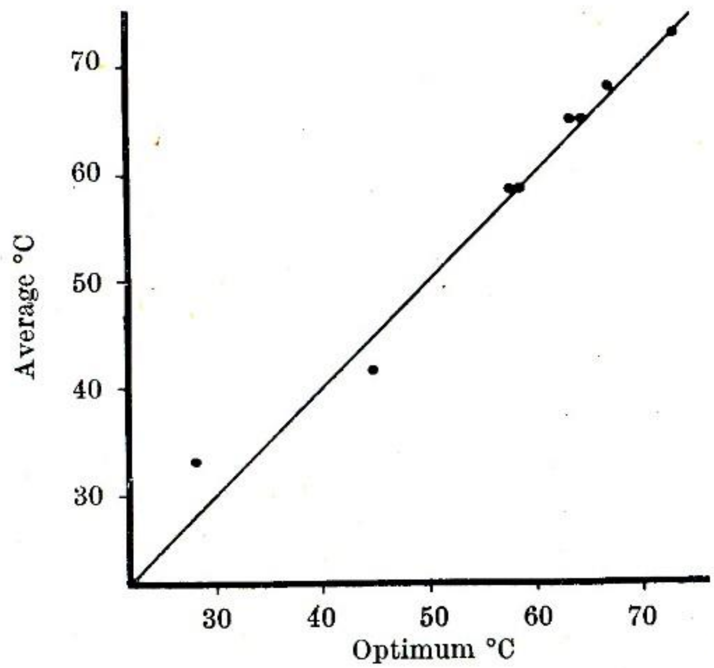



Fig. 4. Temperature optimum for photosynthetic efficiency and average temperature of each location. The ordinate was determined from data of Fig. 1. The abscissa was determined from optima for separate experiments such as shown in Fig. 3.

6

*Figure 4 from Brock 1967 (see bibliography)*

***Nature paper Figure 4.jpg***

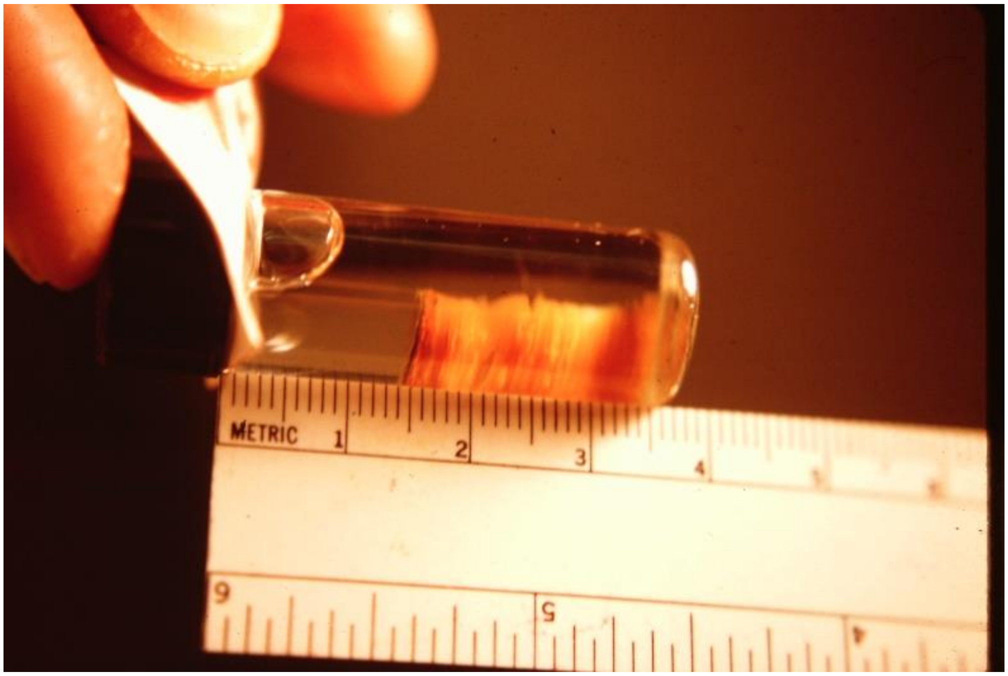
# Equipment for measuring radioactivity in 1966


*Scaler of the type used for counting radioactivity in 1966. This was a manual counter. For accuracy, we counted at least 1000 counts and used a timer to determine counts-per-minute (cpm). There were no computers. We used a slide rule to multiply and get square roots*

.

***nucleo instrument scaler.jpg***

# Typical core from the cyanobacterial mat




*This is what a core of microbial mat at Station VI (about 55 C) looks like. Many laminations, but only the thin top layer is photosynthetic. We did the radioactive carbon experiments in vials such as this. The label tab also served as a handle. Sept 9, 1996. [**link Washout Experiment**]*
***PICT0172.jpg***

# Mushroom Spring

*This is the way Mushroom Spring looked in its full glory, in the early years of our work. The source temperature was slightly above 70 C, just below the upper temperature for phototrophs. It had a constant flow. The single outflow channel provided an extended thermal gradient, both for microbial development, and as locations for incubating vials. The temperature at the outflow was 69.5 C. (The sample that led to the first* Thermus aquaticus *culture was taken just at the outflow location.) Photo from June 23 1967.*
***PICT0168.jpg***

# The Decisive Year: 1967

By 1967 I had become strongly committed to a major research program in Yellowstone. I had not only published some important papers, but had been invited to write a major review paper on life at high temperatures for *Science*. The paper had been written and was being edited during the summer of 1967. (It was published in the late fall of 1967.) I planned to continue the work on microbial mats and do a detailed survey for presence of bacteria in boiling springs. My first graduate student to do research in Yellowstone, Bill Doemel, would be working. Sally Murphy would be back as technician, joined by her husband John who I had hired to do some more routine work.

Louise left Bloomington in late May 1967 to go to West Yellowstone and set up the lab, arriving on May 31. Her rather detailed notes regarding setting up the lab in Daley's big green cabin are written in research data book Y-III-58-60. Also, there is a complete inventory of what we had taken to West Yellowstone in the front of Book Y-IV.

I was in Italy for the month of May 1967, finishing the work I had started at the Naples Stazione Zoologica. I returned to Bloomington around June 4. After a week working with my graduate students and technicians, I flew out to Yellowstone via Frontier Airlines and was in the field June 14, 1967.

In 1967 we rented Daley's large green cabin for the whole summer, and their next-door pink trailer for July-August. The green cabin was used for the lab, and was also lived in by John and Sally Murphy and Bill and Nancy Doemel. Louise and I lived in the pink trailer after Bill and Nancy arrived in late June. Bill and Nancy drove out with the second I.U. station wagon several days after getting married.

It was during this summer that I began to realize that sharing laboratory and living accommodations was not a good idea. Labs should be for lab work. Personnel should have their own accommodations separate from their work.

Since West Yellowstone had lots of rental housing facilities, it was relatively easy to find living accommodations. I established a policy that students and technicians would be reimbursed for any excess rent over what they had to pay in Bloomington. (As it happened, rents were cheaper at West Yellowstone than at Bloomington, so I never had to resort to this option. And most people were delighted to spend the summer in Yellowstone National Park!) To ease the burden of finding housing, I developed a list of housekeeping facilities in West Yellowstone that would be suitable to rent, and put my people in contact with the cabin owners.

Our research during the summer of 1967 involved Doemel's work on *Cyanidium*, temperature optima of bacteria in the thermal gradients, more isolations of *Thermus*, and light adaptation of the algae. Another important work was the discovery of bacteria in boiling water. This work involved the use of immersion slides in boiling or superheated springs.

I had been invited to lecture in a symposium on extreme environments that was being held by the British Society of Applied Bacteriology in Belfast in mid July 1967. I wanted to be able to have a written paper on something unpublished, in addition to the oral presentation I made on the general Yellowstone work. The simplest seemed to be a study of temperature optimum of the bacteria living with or above the cyanobacteria along the thermal gradient. Louise developed a technique using radioactive glucose to measure bacterial activity *in situ* and we did a number of temperature transfer experiments at Octopus Spring.

Also in 1967 we did a number of other projects: lots of light reduction experiments on algae using fabric bags, studied the vertical zonation phenomenon in the mats using autoradiography, installed neutral density glass to observe the effect of light reduction, played around with radioactive sulfur and phosphorus for studying the activity of the pink bacteria, plus initial insect studies. These studies provided great backgrounds for more detailed studies later, especially by students and post-docs.

One of the most interesting studies I did in 1967 was black-out experiments for quantitative growth rate studies (see below). I liked this work because it made use of the steady-state nature of the outflow channels. I called these “the washout experiments.” We built some covers that would block the light over the cyanobacterial mats without impeding the water flow, and then made quantitative counts of decrease in cell numbers. Because the organism was unicellular (*Synechococcus)*, counts were made with a Petroff-Hausser counting chamber. The population began to decrease within a day after darkening, and the loss rate was exponential for 2-3 decades. Half times were calculated from these loss rates. The growth rates were considerably lower than those of the same organism in laboratory culture. Also, the growth rate during recolonization was different from that of washout.

# Measuring bacterial growth in Perpetual Spouter

*Early work on bacteria living in boiling water. The group is working at Perpetual Spouter, a boiling, superheated spring arising from a small crevice. I had placed slides in this spring and discovered that they were rapidly covered with bacteria. I took the whole group for a Sunday hike and we stopped to see this phenomenon. Bill Doemel stooping, with thermistor. Louise leaning over his shoulder. Nancy Doemel and Sally Murphy standing. Park Naturalist Ed Leigh observing. Photo taken July 2, 1967.*

***Perpetual spouter people view 2 7-2-1967.jpg***

# Lunch break in the back reaches of Norris Geyser Basin

*Lunch break on a Sunday hike into the back reaches of Norris Geyser Basin. This is the 100-spring-plain area. The pool in the foreground is Cinder Pool, an interesting spring that has amorphous sulfur globules floating on the surface. Apparently, the temperature at depth is above the melting point of elemental sulfur and liquid sulfur rises to the surface and forms aggregates with trapped gas bubbles. A unique thermal feature.*

***Lunch break Cinder Pool 7-2-1967.jpg***

**Belfast and Loch Ewe (Scotland)**

I flew to Ireland around July 13, 1967, deplaning at Dublin and after several days drove to Belfast for the Society for Applied Bacteriology meeting. I gave on oral presentation on the general hot spring ecosystem, but the written paper I sent in later was on the bacterial temperature transfers experiments. After the meeting, I flew to Glasgow, Scotland, drove a rented car to Loch Ewe, and worked with the Scottish Marine Research Group, and did more *Leucothrix* work, including temperature optima, later published with graduate student Mike Kelly. I had a rather pleasant brief stay at Loch Ewe, living in a crofters cottage for a British pound a day room and board.

At Loch Ewe I had lots of interesting discussions on marine ecosystems with the Aberdeen research team of John Steele and Alan Munro and some rather late evenings at the local pub. I drove back to Glasgow via Edinburgh, then flew to London, stayed with Willie Donahue and wife Milicient Masters, gave a seminar on my yeast genetics work at William Hayes lab at Hammersmith Hospital, then flew back by TWA to Washington, then to Indianapolis where I was met by my secretary Linda Detwiler. I spent several days in Bloomington working with the graduate students, then back to Yellowstone, probably about July 31. I was away from Yellowstone for 2.5 weeks. (This was typical travel for me in those days, as I was keeping all my various research projects going at the same time.)

In order to find out more about the hot spring insects that were living on the cyanobacterial mats, in 1967 I brought animal ecologist Richard Wiegert (University of Georgia) to spend a week with us. He clarified much about the insects and he and Louise did isotope studies, measuring the incorporation of radioactivity from cyanobacteria into the ephedrid flies. Louise finished these studies on her own and they ended up in a good paper (written by Louise with me and Wiegert as co-authors). (Later, Wiegert got his own NSF grant and came out in 1968 on his own with students.)

On August 31, 1967 Doemel and I left to return to Bloomington in one IU wagon, returning home via Thermopolis, Ogalalla, Nebraska, and St. Charles, Missouri. Louise stayed on in the green cabin until about Sept 27 and caught the effects of a major hailstorm in mid-September that demolished (temporarily) the Mushroom Spring cyanobacterial mat. (See published "Catastrophe" paper)

The summer of 1967 was very productive scientifically, because we had done enough preliminary work in 1965 and 1966 to know what questions were significant, carefully planned what to do, and brought along lots of supplies and equipment. Louise was highly productive, I was involved very closely in most of the work, both in planning, executing, and interpreting. Also, we did not have too many people involved. Doemel was just beginning and I had plenty of time to advise him.

Another important thing that took place in 1967 was the publication of my lead article in *Science:*. (Brock TD. 1967. "Life at high temperatures. Evolutionary, ecological, and biochemical significance of organisms living in hot springs is discussed." *Science* 158(804):1012-9.) This article was written at the invitation of the editor. It was widely

read and I had dozens of reprint requests. In addition, a number of people wrote me personally, of whom the most important was Mercedes Edwards, an electron microscopist who collaborated with me on the fine structure of *Thermus aquaticus* and the microbes living in boiling water. I also had a number of requests for cultures of thermophilic bacteria, and sent out *Thermus aquaticus* (yet unnamed) to quite a few laboratories.

I also had invitations to lecture about thermophiles at a number of commercial laboratories, including Monsanto and Proctor & Gamble.

By the end of the summer of 1967 I knew that we had a good thing going, and that the possibilities were endless. We had already published some good papers and the results of this summer would lead to more. By August 1967 I was convinced that we needed to set up more permanent facilities.

Sometime in the summer or fall of 1967 I wrote a popular article dealing with the Yellowstone work for *Natural History Magazine*, published by the eminent American Museum of Natural History. Although this article was quickly accepted for publication, getting it into print became a nightmare, as the editor was close to incompetent. There was correspondence back and forth for months, and the final article was (to me) a big disappointment. However, two important things did arise from this article. The first, and most important, is that it put me in contact with a film maker at Encyclopedia Britannica, leading to an outstanding 15 minute movie of our work in Yellowstone. The second was that it put me in touch with the American Museum of Natural History folks in New York, who created an interesting diorama of hot spring life based on my work.

# Exploring the back country on a rainy day

*Tom Brock, Nancy Doemel, and Bill Doemel on a rainy day in the back country. We were visiting Imperial Geyser in the Fairy Meadow area. At that time, the Fountain Freight Road was open to automobiles and we could drive close to the site. Now it takes a bicycle or a long hike to reach this spot. Fairy Meadows has some very interesting thermal features. Among others is Spray Geyser, which has thick microbial mats growing in the constant spray. August 1, 1969.*
***PICT0190.jpg***

# The Washout Experiment

From the beginning I viewed the Yellowstone hot springs as ecosystems that could be studied directly in nature. As far as I know, to this day no one has repeated the simple experiment that I describe in this short paper.

This simple and revealing study permitted me to measure the growth rate of the thermophilic cyanobacterium *Synechococcus* directly in nature. This was done in August 1967 and was published in the Journal of Bacteriology in 1968. (It is today available on-line.

The technique was surprisingly simple. Most of the work was done at Mushroom Spring, which had a constant flow rate from about the upper temperature for growth of cyanobacteria at 70 C down to ambient temperature. *Synechococcus* is found over the range from about 55 C to 70 C, although most of the work was done at the higher temperatures.

*Synechococcus* requires light, and its growth is balanced by washout due to the erosion caused by the flow of the spring water. Since this is a steady state system, if light is cut off by covering a section of the outflow channel, the cyanobacterium will no longer be able to grow and will wash out. It was the rate of washout that I measured.

In preliminary experiments I found that if the outflow channel was darkened, within a week the *Synechococcus* was gone and the color of the mat turned to orange (the color of the undermat).

Since *Synechococcus* is a unicellular organism, the rate of washout can be measured by taking samples and counting the number of cells present.

At the time the “washout” experiment was done, we had been studying Mushroom Spring for two years, and during that time the flow rate of the spring had been surprisingly constant.

Cores of microbial material were removed with a cork borer 0.28 $cm^2$ in area. The cores were placed in vials topped up to 5 ml with spring water containing preservative. The cores were brought back to the laboratory (at this time the lab was Daley’s big green cabin), and homogenized with a Teflon homogenizer in a polypropylene centrifuge tube. One ml of homogenate was removed for counting with a Petroff-Hausser counting chamber. The rest of the homogenate was used for chlorophyll assay.

Cell counting was done with the Zeiss phase microscope. For each sample, at least 100 cells were counted. When the cell count was low, the chamber was filled several times. Because the volume of the counting chamber was known, it was possible to calculate the # of cells/ml of homogenate. Since all the homogenates were identical in volume, the change in cell number per unit area with time could be expressed.

To darken the mat, we constructed a wooden frame to which an opaque sheet was attached. The fencing and lumber were purchased at the West Yellowstone lumber yard. The frame was designed to straddle the outflow channel. Opaque black plastic sheeting 10 mils thick was wrapped around plastic-coated welded fencing and fastened with black plastic tape to create a stiff flat form 45 X 60 cm in size. This form was suspended from a nylon rope Samples were taken only from the central region under the cover where total darkness existed. (The total darkness of this central area was verified by using radioactive carbon dioxide to show that no took place under the dark cover.)

The data are plotted semi-logarithmically and the dilution rate, which is equivalent to the cell division rate, calculated. At Mushroom Spring at 69.7 C, the doubling time was 40 hours. The result at Grassland Spring, at 72.3 C, was faster, 22 hours. (The flow rate at Grassland Spring was faster, so if the population is to be maintained the washout rate must be faster.)

The growth rates measured here were considerably slower than those with *Synechococcus* growing under optimum conditions in the laboratory. “This result emphasizes the point that the growth rate in nature is usually less than that observed under the most favorable laboratory conditions.”

Louise was a co-author on this paper. Also credited were Terri Bruce, Sally Murphy, and John Murphy. Since Terri Bruce was never with us at Yellowstone, I assume she did some of the Petroff-Hausser counting on samples we brought back to Indiana University.

# The darkening technique

*View of the procedure for darkening the channel. Mushroom Spring, Station II. One of the reviewers to the paper requested that I remove this photograph as "it would only elicit envy from readers." This is actually a typical attitude of many people who thought our work at Yellowstone was a lark rather than serious business. I insisted that the photos remain.)*

***PICT0174.jpg***

# The washed out mat

*The mat after a week in the dark. Mushroom Spring, Station IIA. Microscopy showed that all of the cyanobacterial cells had washed out.*
***PICT0219.jpg***

# Washout data

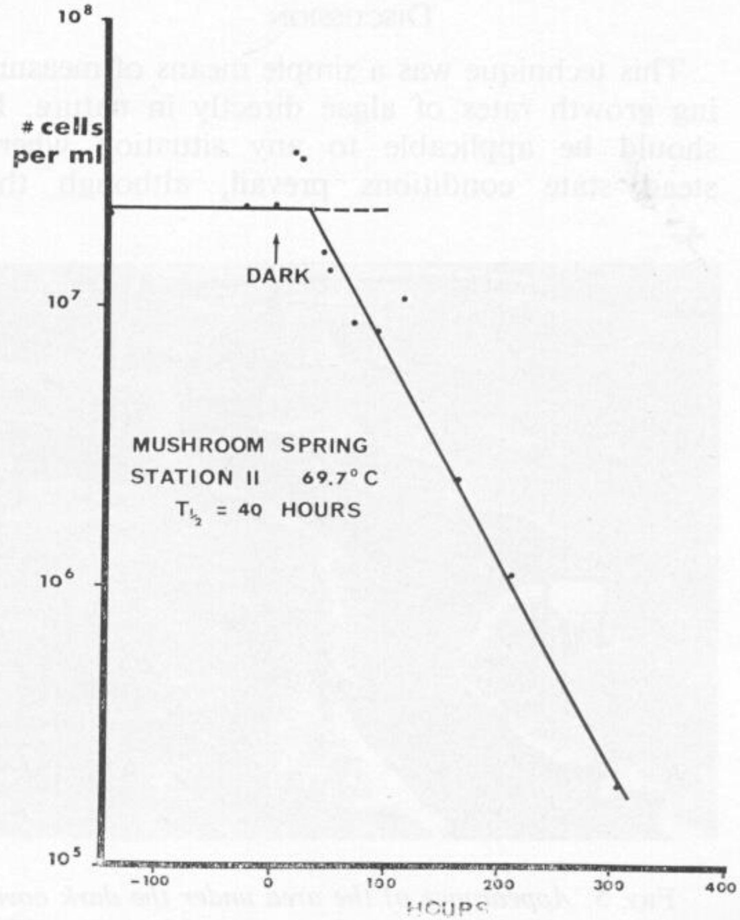



FIG. 4. *Loss rate at Station II, Mushroom Spring. Day zero, 15 August 1967.*

*Typical washout rates. Graph from the published paper (Brock and Brock, 1967).*
***Washout graph Mushroom 1968.jpg***

# Exploring Yellowstone Thermal Areas

Almost from the beginning of my work in Yellowstone, I was exploring the Park, getting acquainted with the area. I had a large topo map of Yellowstone which I had mounted on foam core and put on the wall in my office in Bloomington. I also had a series of 7 ½" quadrangle maps of those parts of the Park where thermal features were easy to get to. These quadrangles had the following names: Mammoth, West Thumb, Canyon Village, Madison Junction, Old Faithful, and Norris Junction. I also had guide books such as the Haynes guide, which had details about major thermal features. And I had my Allen and Day photocopy. Later, Park geologist George Marler provided me with a more up-to-date version of much of the Allen & Day work.

I walked every boardwalk and studied its thermal features, and made written or mental notes on how to get off the boardwalks into the "back country", or how to avoid the boardwalks entirely.

When we went in the field, we carried our equipment in **field boxes**. These boxes were 2 X 1 foot and 1 foot deep, with handles at the narrow end. We tied a nylon rope through the handles so that we could carry the box at our side. This sort of box was ideal for carrying equipment such as thermistors, racks of test tubes, radioactive isotopes, reagents, etc. Food, extra clothing, and sundries were carried in a backpack.

In the early years, I spent a lot of time looking for hot springs with outflow channels that had good thermal gradients. When I found one, I either took records then, or made notes about returning later for measurement and sampling. Generally, I carried a field box with a thermistor, pH paper, and sampling bottles. For sampling deep boiling pools we used forceps or tied sampling bottles onto a fishing rod. (Folks seeing us in the back country asked how the fishing was!)

I always had my field book with me, either in my pocket or in the field box.

We always got off early to avoid the traffic. The road from West Yellowstone to Madison Junction was a scenic fourteen miles, with Seven-Mile Bridge at the half way point. Here trumpeter swans were usually seen in the broad shallow area of the Madison River.

Madison Junction is where the Firehole River and Gibbon River join. If you turn right you go past Firehole Falls upstream along the Firehole River to the Lower Geyser Basin and the Old Faithful area. If you turn left, you go past Gibbon Falls up the Gibbon River north to Norris Junction and Norris Geyser Basin, where the road either continues further north to Roaring Mountain and Mammoth Hot Springs, or east on a spur road to Canyon Village and the Grand Canyon of the Yellowstone River. Once at Canyon Junction, a right turn (south) leads along the mighty Yellowstone River to the Mud Volcano area.

Almost everywhere along what is called the Grand Loop Road there were thermal areas, either near the road, or on short spurs, or via short or long backcountry trails.

# The Madison River near Seven-Mile Bridge

*Trumpeter swan along the* Madison River *near Seven Mile Bridge.*
*A frequent early morning view on the way to work! August 7, 1972.*
***PICT0257.jpg***

# Winter along the Firehole River in the Upper Geyser Basin

*The steam shows best in the early morning in winter, and especially when it is backlighted. This is the Firehole River as it flows through the Upper Geyser Basin, and gives an idea of how many thermal features there are. January 28, 1971. This photo was taken during our trip with an Encyclopedia Britannica film crew, in the days when the Upper Geyser Basin was still pristine in winter.*

***PICT0255.jpg***

# Researchers at Locomotive Spring in the Norris Geyser Basin

*Locomotive Spring area of the Norris Geyser Basin. Kathie Brock, Anne Mosser, Jerry Mosser. This image was also used in my textbook Biology of Microorganisms, 6th edition, and in many later editions. August 24, 1971.*

***PICT0256.jpg***

# Lab group about to hike to Shoshone Geyser Basin


*Lab visitors and researchers group about to start on the 10 mile hike to Shoshone Geyser Basin. L to R: unknown; Mercedes Edwards (back to camera); Louise Brock; Barbara Rice (graduate student from Utah State); Bill Doemel; Gary Darland; Tom Bott. In those days, the road to Lone Star Geyser was still open, saving 4 miles on the hike. Bill and Tom have sampling equipment. We always combined business with pleasure! Saturday, July 12, 1969.*
***PICT0147.jpg***

I explored most of these areas, always with the 7 ½" topo map of that area, often with a guide book as well. I usually left the weighty Allen and Day book (3 separate volumes of Xerox;) in the car. Always I had a camera along, one of four Beseler Topcons with single-lens reflex and through-the-lens light meter. (The through-the-lens light meter did not become available for the Nikon until later.) I made lots of color 2 X 2 slides (which I still have, organized in loose-leaf notebook binders).

In the early years I either explored alone or with Louise. Later, when there were students and/or post-docs, we went in groups, especially on weekends, which were days off from real work. (All my life I have worked a seven-day week.)

The Lower Geyser Basin was so large and diverse that one could spend a whole field season just there. (And I did, the first two years.)

As far as access by road was concerned, Yellowstone was a moving target. During the years I worked there, the Park kept closing side roads and spurs, often even some important roads, forcing traffic onto the few remaining main roads. Road building was a constant activity. Fortunately, we were able to get some important work done before road closures made it impossible to work easily in some areas.

Mushroom Spring and Octopus Spring, on the east side of the Lower Geyser Basin, are two big thermal features that we worked on frequently. They turned out to be ideal locations for field research. They were close to the parking area at Great Fountain Geyser, yet hidden from public view. Mushroom was behind a group of trees, and Octopus was hidden by a small hill. Lots of people hung around Great Fountain waiting for an eruption, but no one ever noticed us or came to see what we were doing. We saw quite a few of its eruptions from a distance, and occasionally close up when we were arriving or leaving.

Also, while working on these two features, I would explore other nearby areas. I discovered an important feature, Grassland Spring, while waiting for some incubations to end at Mushroom. I explored the whole White Creek valley, with its hot creek and dozens of upstream thermal features and several superheated pools, while working at Octopus. Some of these (unnamed) pools became important research sites.

# Octopus Spring, a favorite research site

*Octopus Spring, one of the best studied hot springs in Yellowstone, in the thermal White Creek drainage. We discovered this spring in 1965 (called Pool A in those days). There were several Ph.D. dissertations that used the large microbial mat in the foreground. The outflow in the background was where the pink bacteria were discovered, and the source pool was also well studied by Tom Bott. Photo July 19, 1970. Louise Brock working.*

***PICT0217.jpg***

Other features on the east side of the Lower Geyser Basin are Hot Lake, a large thermal lake, Pink Cone and Bead Geysers, and White Dome Geyser.

Also present in the Lower Geyser Basin is a curious artificial structure called “The Tank”, which was built by the U.S. Army in the late 1800s to provide a swimming pool for soldiers. Although the soldiers are long gone, The Tank is still present, and retains its tepid water. The water source is a side branch of Hot Lake which has cooled to about body temperature by the time it fills the Tank. Although swimming is now forbidden, it used to be a favorite swimming hole for Yellowstone “Savages” (employees of the Yellowstone concessionaire). (I went swimming in the Tank once, in early evening, when it was still permitted. The water level was deep enough so you could dive.) An internet search failed to dig up a reference to the Tank, but it was still there the last time I visited (2014).

The Firehole River runs through the middle of the Lower Geyser Basin and Mushroom and Octopus are on the east side. On the west side is a road that used to be called the Fountain Freight Road, which crossed the Firehole River and brought you to the trailhead for the Sentinel Meadow and Fairy Creek thermal areas. The trail to Sentinel Meadow goes near Boulder Spring, a superheated spring with constant water flow. Tom Bott did his major research at Boulder Spring, parking off the road just across the bridge. He needed close access because he lugged heavy storage batteries to the site.

Sentinel Meadow brought you to Steep Cone and Flat Cone, superheated pools (not geysers) and the source of some of my first immersion slide studies.

# A *Thermus aquaticus* site

*This unnamed boiling spring is on a terrace just above Great Fountain Geyser, and provides a view of the whole Lower Geyser Basin. In the colorless area above the microbial "vee" is a nice population of Thermus aquaticus. John Bauld working in the background. June 2, 1970.*
***PICT0220.jpg***

# Steep Cone: prime site for bacteria living in superheated water

*Anne Mosser and Tom Brock at Steep Cone in the Sentinel Meadow. I was doing microscope slide immersion studies at all the superheated pools I could reach. Later we did quite a bit of filming with Encyclopedia Britannica at this spring. Sentinel Meadow is one of the more exciting thermal areas in Yellowstone National Park and is reachable by a 2 mile hike from Ojo Caliente geyser. Photo, August 11, 1967*
***PICT0214.jpg***

When the Fountain Freight Road was drivable its whole distance, we used to enjoy eating lunch at the Goose and Feather Lake Picnic Area, reachable now only on foot or bicycle.

There are dozens of other features in the west side of the Lower Geyser Basin, mostly along the scenic Firehole River. This is the warmest part of the Firehole River and by August the temperature may reach 25 C or more. Once I put on a face mask and floated through this whole “reach” of river, thinking it would be too warm for trout. I did see trout, but only in the deeper pools where the water was cooler. (Warm water is less dense than cold and floats on top.)

Another favorite site for research was Norris Geyser Basin, considered by Allen and Day to have the hottest temperatures in the park. There are no springs with extensive cyanobacterial mats, but lots of acidic springs that create unique microbial habitats. Most of the crowds are centered near the museum and boardwalks, but it isn’t difficult to duck out of sight and explore the back reaches. (At one time the Norris museum had a nice display of microbiology based on my work, with photos I provided.) Bill Doemel had one of his best *Cyanidium* sites near the main Norris area, and Ray Lynn did a major study on a *Zygogonium* algal mat that was in an acid area adjacent to the main thermal area.

A few miles north on the road from Norris is Nymph Creek, the best *Cyanidium* mat in the Park, where Doemel did a lot of studies. The adjacent Nymph Lake, with a pH around 4, receives a lot of its water from the creek. (One time Kathie and I put a canoe into Nymph Lake and studied the whole shoreline.)

A few miles farther north along the Grand Loop Road is Roaring Mountain, with effluents at pH of 2 and a complete thermal gradient from over 85 C to less than 20 C. These low-pH habitats are especially interesting because cyanobacteria do not grow, but only eukaryotic algae. Since the upper temperature limit for eukaryotes is 60 C, the only phototroph present is *Cyanidium*, which is a eukaryote. We did lots of work at this site, always making sure to stay out of view of the public.

Carl Fliermans did a lot of his Ph.D. work at Roaring Mountain. Some of Bill Samsonoff’s immersion slides in the 80 C pH 2 creek led to my discovery of *Sulfolobus*.

# Analyzing the soil in an acidic location

*Carl Fliermans taking a core sample of a hot acid soil. Canyon area. . This sort of habitat is common throughout the park wherever fumaroles or thermal vents exude hydrogen sulfide-rich steam. (Note the yellow streaks; all due to elemental sulfur crystals.) June 1, 1970*
***PICT0252.jpg***

# The film crew working at Roaring Mountain

*Roaring Mountain no longer roars, but produces plenty of steam and sulfur. We did lots of research here, and this was the principal site of Carl Fliermans Ph.D. dissertation. The Encyclopedia Britannica film crew is photographing Tom sampling hot acid soil. Photo July 20, 1971.*

***PICT0199.jpg***

# Lab group visiting Roaring Mountain

*Lab group visiting Roaring Mountain L to R: Dave Smith, Bob Belly, John Bauld, Carl Fliermans, Kathie Brock. Chuck Boylen stooping in foreground. The wooden stakes indicate that the group is standing in the middle of the transect that Fliermans had set up the previous summer. Smith and Belly would also be working in acid thermal areas in 1971. Photo June 5, 1971.*

***PICT0160.jpg***

# Thermal gradient at Roaring Mountain

*Good view of the thermal gradient at Roaring Mountain. Most of the heat derives from the mountain itself, while in the level plain there are few thermal features and the temperature gradually falls. The lodgepole pine tree manages to find a spot free of thermal activity. A lot of research was done at this thermal gradient by Fliermans and others. Photo taken June 19, 1973.*

***Roaring Mt B-W 6-19-1973 N100.jpg***

# Lab group visiting Octopus Spring

*Lab group visiting Octopus Spring, June 8, 1971. Identified from L to R: Chuck Boylen, Kathie Brock, John Bauld, Carl Fliermans (red jacket), Bob Belly. Hidden are K. Boylen and D. Smith.*

***PICT0161.jpg***

# The Five "Sisters"


*An old sign (now gone) at the Five Sisters spring (White Creek area), and the 5 researchers. Boylen, Smith, Bauld, Belly, Fliermans. They spontaneously gathered around the sign and I snapped the photo. June 8, 1971.*

***PICT0163.jpg***

# Sulfur-rich acid spring in the Amphitheatre Springs area

*Another group of acid thermal features along the Grand Loop Road is Amphitheatre Springs, which has numerous effluents rich in sulfur.*
*Here we are showing a group of visitors the* Cyanidium *mat at a lower temperature. August 3, 1968.*

***PICT0251.jpg***

# Sampling a small but very hot sulfur spring

*Kathie Brock measuring the temperature of a hot sulfur spring. Such springs are a common site in the acid thermal areas. This particular spring is forming elemental sulfur in its effluent, due to the spontaneous oxidation of hydrogen sulfide.. Virtually any site such as this is ideal for a research study. September 5, 1971. Under the microscope* Sulfolobus *cells can be seen attached to the sulfur crystals.*
***PICT0245.jpg***

Mammoth Hot Springs, near Park headquarters, is a unique site where massive amounts of a calcium carbonate mineral called travertine are being deposited along hot spring effluents. Temperatures are not much more than 55 C. This area is so public that I never worked there.

The back reaches of the Canyon area have some very interesting thermal features, but it took so long to get there from West Yellowstone that I did no more than just look around.

The Mud Volcano area, however, was too interesting to ignore. Although this area has an extensive boardwalk with heavy foot traffic all day long, Jerry and Anne Mosser found a great spring just out of sight (which they called "Moose Pool") that had good *Sulfolobus* populations. We did lots of *Sulfolobus* work in this area.

# Sylvan Springs: A major research area

*Sylvan Springs, at the edge of Gibbon Meadow. The Grand Loop Road is visible in the distance. This was a major research area for us, especially for* Sulfolobus *and the general study of sulfuric acid production. Dozens of hot pools, both small and large. Sylvan Springs was also a research area for* Zygogonium. *Photo was taken July 31, 1974.*

***PICT0208.jpg***

# Turbid Lake: Not thermal, but rich in hydrogen sulfide

*Turbid Lake, on the east side of the Park. In years past the main road to the East Entrance went near this site. When the new road was built along the east side of Yellowstone Lake and over Sylvan Pass, the old road was kept as a Service Road and we were able to drive the 3 mile dirt road to launch our canoe. This lake is a fascinating microbial system. It is cold, but receives massive amounts of hydrogen sulfide from vents in the bottom. Bacterial oxidation results in formation of sulfuric acid. As I recall, the pH was below 5. The turbidity is due to sulfur and bacteria. At the far end of the lake, a large colony of Canada geese was nesting. The summer after our visit, the road was permanently closed. Photo August 11, 1971.*

***PICT0211.jpg***

**Potential Research Areas**

In the early 1970s the Park was thinking about possibly setting aside certain areas of the Park for research. I was asked to make suggestions, and in 1975 I sent the list shown here to the Chief Naturalist's office (Via letter Aug 4 1975 to West District Naturalist. My list had 21 sites, and for each one I described briefly its important features. I am fairly certain that no research areas were ever designated. However, the list is still interesting, and probably most of these sites still exist..

| **USGS Quadrangle** | **Area** |
|---|---|
| Tower Junction | Washburn and Inkpot hot springs |
| Mammoth | Bath Lake |
| Mammoth | New Highland Terrace |
| Mammoth | Nymph Creek |
| Mammoth | Roaring Mountain |
| Mammoth | Amphitheater Springs |
| West Thumb | Heart Lake Geyser Basin |
| Canyon Village | Turbid Lake |
| Canyon Village | Ebro Springs |
| Canyon Village | Springs south of Black Dragon's Caldron (Mud Volcano area) |
| Canyon Village | Crater Hills Sulphur Springs |
| Canyon Village | Clear Lake and springs east of it |
| Old Faithful | Shoshone Geyser Basin |
| Old Faithful | Cascade Springs (east of Biscuit Basin) |
| Madison Junction | Fairy Meadows including Spray and Imperial Geysers |
| Madison Junction | Sentinel Meadow, including Boulder, Steep Cone, and Queen's Laundry |
| Madison Junction | White Creek area (whole valley) |
| Madison Junction | Rabbit Creek area (whole valley) |
| Norris Junction | Geyser Springs Group |
| Norris Junction | One Hundred Spring Plain (Norris Geyser Basin) |

| | |
|---|---|
| Norris Junction | Porcelain Terrace (Norris Geyser Basin) |
| Norris Junction | Sylvan Springs |

## Developing the first lab trailer at West Yellowstone

The development of the laboratory at West Yellowstone happened in a somewhat haphazard way.

After I returned from Belfast and Scotland at the end of July 1967, I started looking around seriously about available land in West Yellowstone that might be purchased. At Loch Ewe the Scots had a house trailer right on the beach, and I did my radioisotope and incubation experiments there. I saw how convenient this was. I returned home determined to find some arrangement for a permanent laboratory.

West Yellowstone was fairly extensively developed, but there were still vacant lots west of the town center, scattered here and there among small motels, houses, and commercial buildings. One full-sized vacant lot was just west of the Daley property, and adjacent to the Golden West Motel. It fronted on Madison Street and backed up on an alley (alleys ran behind all the lots in West Yellowstone). Just before I returned to Bloomington in early September 1967, the motel owner offered me this lot for $10,000. Although it lacked facilities, hook-ups could be obtained from the motel. (This was the lot we eventually purchased.)

Also, at that time the Daleys offered me rental space on land of theirs where we could put a trailer. This would be at the back of their own lot and near the big green cabin that we had been using for housing and lab space. Rental, complete with water, gas, sewage, and electricity hook-ups, would be $30 per month.

On September 6 1967, immediately after I returned to Bloomington. I sent a letter to National Science Foundation requesting permission to use some of my existing funds (no more than $4500) to purchase a **travel trailer** to use as a mobile lab. The National Science Foundation approved this on September 18 1967.

In a sense, the NSF approval was the critical step; and I was now on my way to creating a laboratory in West Yellowstone. However, I decided that I needed more information on travel trailers before making a final decision. I talked with Professor Bernard Loft in the IU School of Recreation who knew a lot about travel trailers as he was a consultant with the Holiday Rambler Co., an Indiana company. Loft informed me that one could buy a mobile home with much more space for the same money as a travel trailer and recommended that would be better for our needs.

How did a travel trailer differ from a mobile home?

# Travel trailer versus mobile home

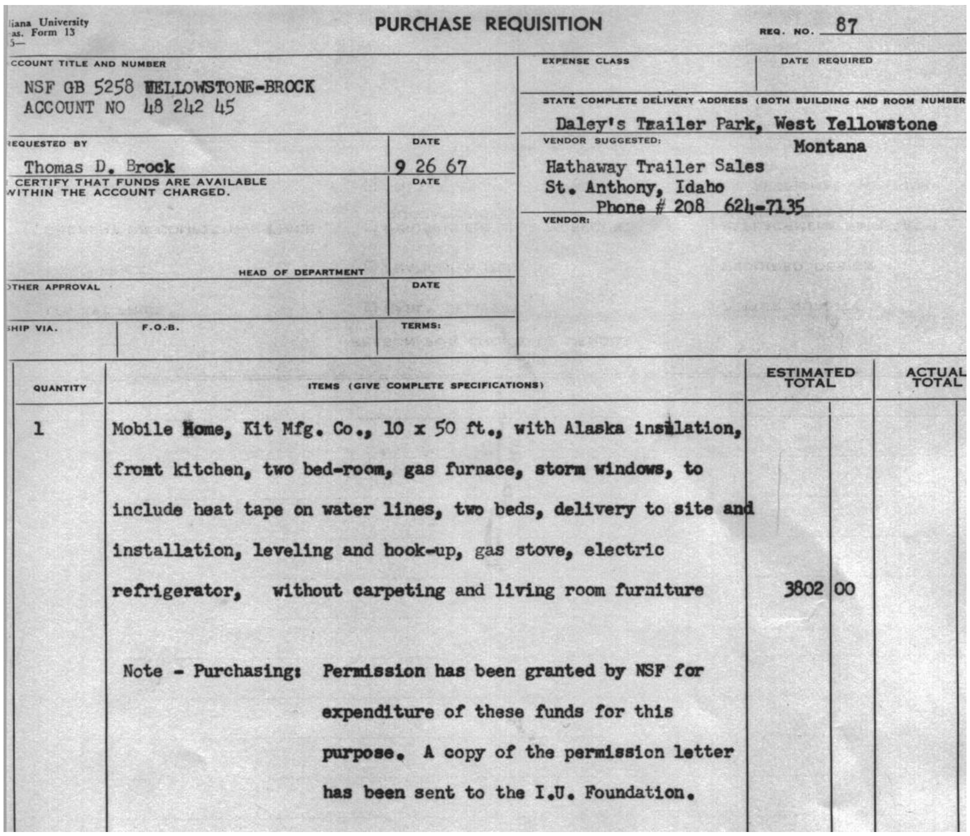
| | | |
|---|---|---|
| Travel trailer | Towed behind an automobile or pickup truck. Stop for one night and then move on. Designed for long-distance travel. Could be used as a mobile lab but would not have very much lab space if it were also used for sleeping and eating. | Relatively expensive per square foot of usable space |
| House trailer | Movable on highway but remains in place for months or years. Essentially a pre-packaged house. | Relatively inexpensive per square foot of usable space |

The evening after I talked with Bernard Loft, Louise called to report on her work and I told her we should buy a house trailer to put on Daley's lot. The National Science Foundation approval was general enough that I did not need to contact them again.

In mid-September 1967 Louise and Ellen Daley drove to Idaho and visited Hathaway's Trailer sales in St. Anthony, Idaho. Danny Hathaway gave Louise a good offer on a house trailer, $3802, for a 10 X 50 ft. unit with stove and refrigerator but otherwise unfurnished. The price included delivery to Daley's lot and complete hook up to water, sewage, gas, and electricity. Since I had NSF approval, I was able to get verbal approval from Indiana University Purchasing, and I told Louise to buy the trailer.

The trailer was delivered to West Yellowstone and Louise had enough time to get the unit set up for winter work before she left at the end of September 1967.

# Buying the trailer committed us to a long range program at Yellowstone.



iana University
as. Form 13

PURCHASE REQUISITION

REQ. NO. 87

CCOUNT TITLE AND NUMBER
NSF GB 5258 YELLOWSTONE-BROCK
ACCOUNT NO 48 242 45

EXPENSE CLASS

DATE REQUIRED

STATE COMPLETE DELIVERY ADDRESS (BOTH BUILDING AND ROOM NUMBER
Daley's Trailer Park, West Yellowstone
Montana

EQUESTED BY
Thomas D. Brock

DATE
9 26 67

VENDOR SUGGESTED:
Hathaway Trailer Sales
St. Anthony, Idaho
Phone # 208 624-7135

CERTIFY THAT FUNDS ARE AVAILABLE
VITHIN THE ACCOUNT CHARGED.

DATE

VENDOR:

HEAD OF DEPARTMENT

THER APPROVAL

DATE

HIP VIA. | F.O.B. | TERMS:

| QUANTITY | ITEMS (GIVE COMPLETE SPECIFICATIONS) | ESTIMATED TOTAL | ACTUAL TOTAL |
|---|---|---|---|
| 1 | Mobile Home, Kit Mfg. Co., 10 x 50 ft., with Alaska insilation, front kitchen, two bed-room, gas furnace, storm windows, to include heat tape on water lines, two beds, delivery to site and installation, leveling and hook-up, gas stove, electric refrigerator, without carpeting and living room furniture | 3802 00 | |
| | Note - Purchasing: Permission has been granted by NSF for expenditure of these funds for this purpose. A copy of the permission letter has been sent to the I.U. Foundation. | | |

*Laboratory of Thermal Biology: the first step. Purchase order for first house trailer to be installed on a rented lot next to Daley's. This purchase and installation was carried out by Louise Brock with help from Ellen and Ed Daley.*

***House trailer purchase order 9-26-1967 YellowstoneLab early years.jpg***

## Winter use of the first lab trailer

Bill Doemel, Louise, and I flew to Idaho Falls the end of January 1968. Ed Daley met us at the airport and drove us to West Yellowstone. He had turned on heat in the new trailer and we could stay there.

I was involved in Vince Schaffer's field research expedition at Old Faithful for one week. (This was my second time with Schaffer's group.) Bill and Louise studied cyanobacterial mats at Mushroom Spring, *Cyanidium* for Bill's thesis work, and hot spring insects. Total winter stay 2 weeks. Louise and Bill rented snowmobiles (Ski-Doos) from Harold Young, so they could get into the Park. There was a lot of snow, making foot travel very difficult, and I don't think the whole stay was too productive, but we did get to measure photosynthesis with radioactive carbon at Mushroom Spring, more or less completing the light reduction studies.

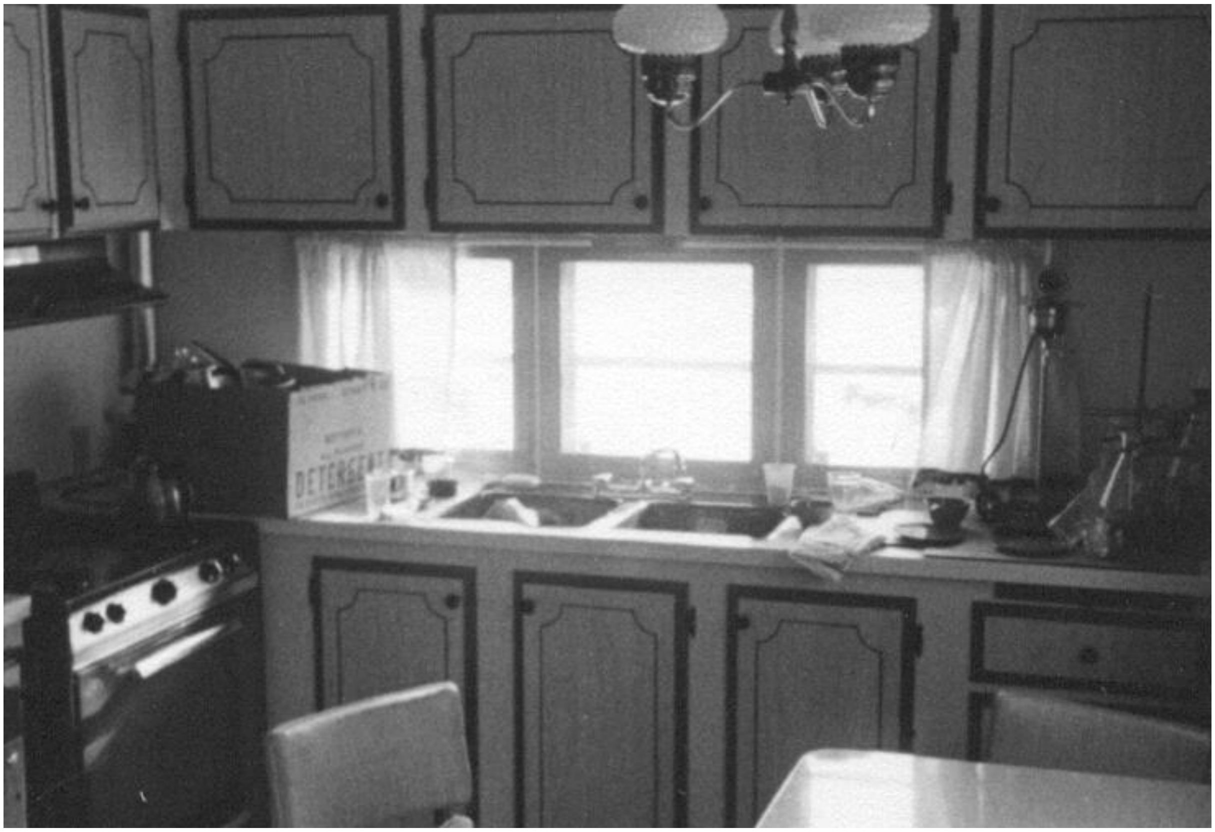
While Bill and Louise were working at Mushroom, I was at Old Faithful with Schaffer's program. I did a lot of immersion slides studies of bacteria living in boiling springs. This work at the superheated springs in the Old Faithful area was something that I could not do in the summer, when the area was crowded with visitors. Thus, my winter work was integrated into my summer research and shows up peripherally in some of the papers on bacterial growth in boiling springs.

# The laboratory trailer winter 1968

*The lab trailer in the snow, the first winter after purchase. January 30, 1968. Louise and Bill Doemel spent a week here while TDB was at Old Faithful for the Field Research Expedition.*

***PICT0188.jpg***

# The interior of the laboratory trailer, January 1968



*Lab interior during the time when field work was being done in the winter of 1968. Minox photo January 28, 1968.*

***Lab interior Minox 1-28-1968 BW153.jpg***

# Research studies at Mushroom Spring, February 6, 1968

*Feb 6, 1968. Bill Doemel and Louise Brock measuring temperature in the outflow channel of Mushroom Spring. Nearby there was 2 meters of snow on the ground.*

***PICT0166.jpg***

# Rest stop at Seven-mile Bridge on a warm afternoon in February

*A rest stop at Seven-mile Bridge, on the way back to West Yellowstone. We rented the three Ski-doos and the extra trailer to haul our supplies and equipment. The road was only open to snow machines, both large SnoCats, and the one-person Ski-doos. In those days there was not yet much snow use along the Park roads. The weather was rarely as good as this photo shows. One night there was two-feet of powder snow. Even snow shoes were not useful with that much new snow. To reach Mushroom Spring, we had to take off our snow shoes and walk up the edge of White Creek (where the snow had been melted by the hot water). February 6, 1968.*

***PICT0184.jpg***

# Land purchase in West Yellowstone

During the brief January 1968 visit, I managed to have a further conversation with John Olson of Golden West Motel about the purchase of the vacant lot next to Daley's. The lot was still available for $10,000, and since in the winter land sales hardly took place, I would have time to acquire the money.

When I was back in Bloomington I called Harry Day (a chemist and friend of mine), who was Associate Dean of Research, about having Indiana University purchase this lot. He asked me to make a proposal.

The proposal is shown on the next two pages. Although the lot price was $10,000, I requested $15,000 because we would need to spend money for water and sewage, among other things.

This proposal appears to have been clearly well thought out and complete. It makes a good case for research in Yellowstone, and lays out succinctly the advantages of basing the operations in West Yellowstone. The University could conclude that this proposal would put Indiana University into a unique situation with regard to research and education in Yellowstone. I was careful to make it clear that this to be a "general" facility which could involve other IU faculty, and suggested the possibility of student education.

Happily, the Indiana University Research Committee was interested in my proposal. Also, George Bloom, who was Indiana University's real estate manager, thought the proposal made a lot of sense. Note that I had been explicit that no permanent structures would be placed on the lot, so that if (or when) my research project ended, Indiana University could sell the lot, probably at considerable profit. (In fact, when I moved to UW-Madison in 1971 Indiana University did make a small profit when they sold the land to the Wisconsin Alumni Research Foundation.)

In February 1968 I received an Option to Purchase agreement from George Olson which I sent on the Indiana University Real Estate manager George Bloom with an attached map.

There were numerous memos back and forth over the next few months working out the details of this transaction. Although it took six months of discussion, letters, memos, and negotiations, persistence paid off, and on July 1, 1968 the land deal was closed and we were off and running!

The land title was in the name of "THE TRUSTEES OF INDIANA UNIVERSITY". The legal description was Lot 12, Block 19 of the Townsite of West Yellowstone.

An enhancement to the arrangement was that the seller of the lot told me that there was already a trailer on the lot we were buying. Would we be interested in renting it? This turned out to be a suitable arrangement. We did rent that trailer and used it for living accommodations for some of our people in summer 1968, while our own trailer remained on Daley's lot.

# Proposal to purchase a lot in West Yellowstone; page 1

TD Brock

To: Mr. T.E. Randall, Secretary
From: T.D. Brock

Dept. Research Committee
Dept. Microbiology

Subj. Proposal for Research Committee
Date. September 19, 1967

This is a request for funds for the purchase of a small plot of land in West Yellowstone, Montana, for use in the establishment of a field laboratory for research in microbial ecology, biogeochemistry, and other areas of biology and geology. The request is for $15,000.

I. Background

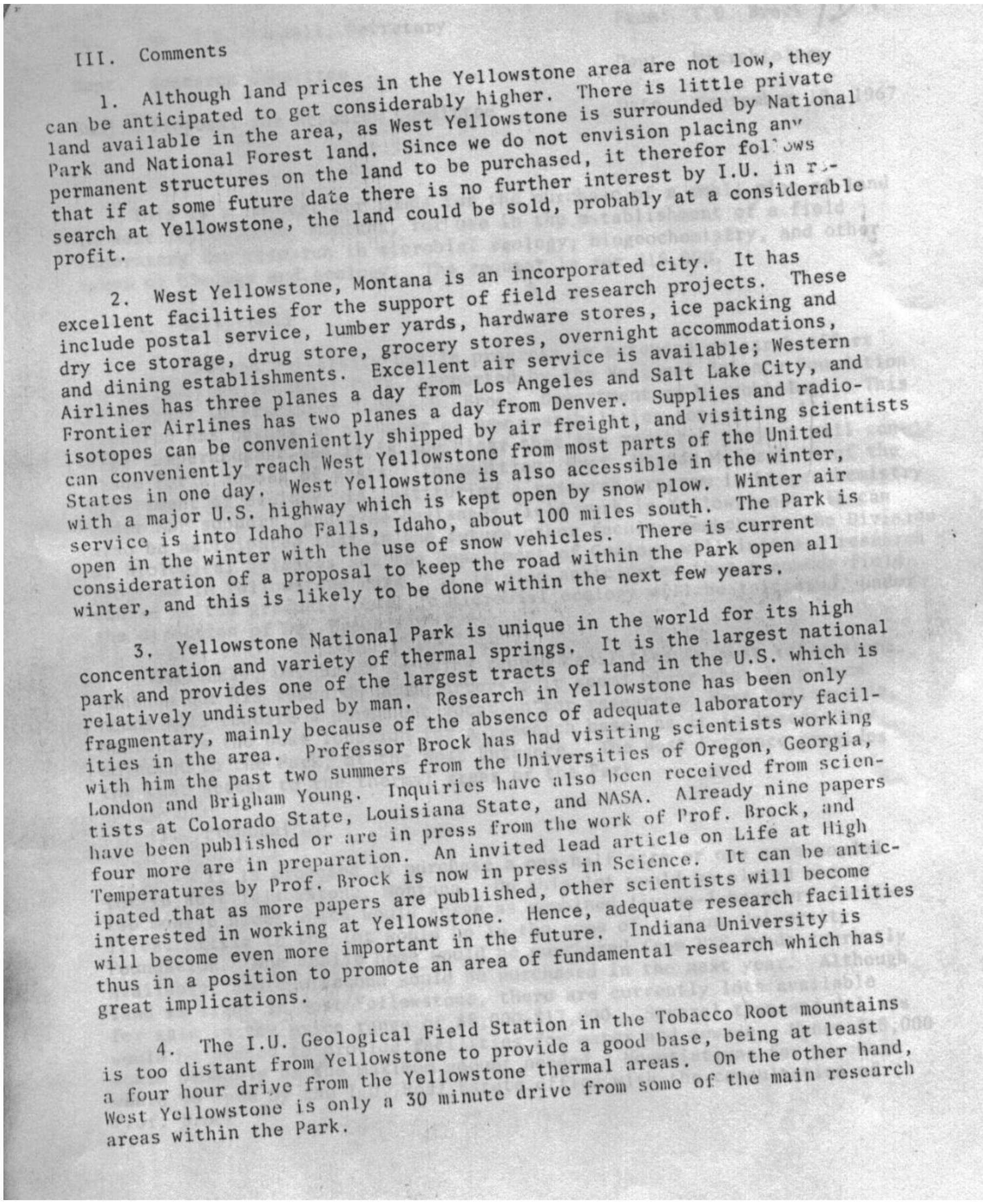
Indiana University has in progress an extended research effort in Yellowstone National Park, supported by the National Science Foundation by a grant to Professor Thomas D. Brock, Department of Microbiology. This research has opened up a number of new possibilities concerning life at high temperatures, and it is now clear that the research project will continue for a number of years. In addition, Prof. Warren Meinschein of the Department of Geology, is instituting a research program in biogeochemistry under NSF support, and also envisages field work in Yellowstone. It can also be anticipated that in the future other faculty members in the Division of Biological Sciences and the Department of Geology will initiate research programs in Yellowstone Park. It is also anticipated that a summer field course at the graduate level in microbial ecology will be initiated, under the direction of Dr. Brock.

Currently, Dr. Brock is using rented facilities in West Yellowstone, Montana for his research headquarters. It seems clear that long-term needs will require a permanent location for research. West Yellowstone, Montana is the best location for a research base, as it is immediately adjacent to the Park, at the West Entrance. The West Entrance provides the best access to the thermal areas of the Park.

II. Proposal

It is proposed to purchase a one-half acre or one acre wooded lot in West Yellowstone, Montana. On this lot would be placed one or two mobile homes which would serve as combined living-laboratory facilities. Title to the lot would be in the name of Indiana University Foundation. One mobile home would be purchased from NSF funds currently available, and the second would be purchased in the next year. Although land is tight in West Yellowstone, there are currently lots available for sale in the price range of $8,000-$12,000. Several thousand dollars would be needed to provide facilities for water and sewage. Hence $15,000 would probably be the maximum amount needed. Negotiations for purchase would be made by the I.U. Real Estate office with the consultation of Prof. Brock.

# Proposal to purchase a lot in West Yellowstone; page 2



III. Comments

1. Although land prices in the Yellowstone area are not low, they can be anticipated to get considerably higher. There is little private land available in the area, as West Yellowstone is surrounded by National Park and National Forest land. Since we do not envision placing any permanent structures on the land to be purchased, it therefor follows that if at some future date there is no further interest by I.U. in research at Yellowstone, the land could be sold, probably at a considerable profit.

2. West Yellowstone, Montana is an incorporated city. It has excellent facilities for the support of field research projects. These include postal service, lumber yards, hardware stores, ice packing and dry ice storage, drug store, grocery stores, overnight accommodations, and dining establishments. Excellent air service is available; Western Airlines has three planes a day from Los Angeles and Salt Lake City, and Frontier Airlines has two planes a day from Denver. Supplies and radio-isotopes can be conveniently shipped by air freight, and visiting scientists can conveniently reach West Yellowstone from most parts of the United States in one day. West Yellowstone is also accessible in the winter, with a major U.S. highway which is kept open by snow plow. Winter air service is into Idaho Falls, Idaho, about 100 miles south. The Park is open in the winter with the use of snow vehicles. There is current consideration of a proposal to keep the road within the Park open all winter, and this is likely to be done within the next few years.

3. Yellowstone National Park is unique in the world for its high concentration and variety of thermal springs. It is the largest national park and provides one of the largest tracts of land in the U.S. which is relatively undisturbed by man. Research in Yellowstone has been only fragmentary, mainly because of the absence of adequate laboratory facilities in the area. Professor Brock has had visiting scientists working with him the past two summers from the Universities of Oregon, Georgia, London and Brigham Young. Inquiries have also been received from scientists at Colorado State, Louisiana State, and NASA. Already nine papers have been published or are in press from the work of Prof. Brock, and four more are in preparation. An invited lead article on Life at High Temperatures by Prof. Brock is now in press in Science. It can be anticipated that as more papers are published, other scientists will become interested in working at Yellowstone. Hence, adequate research facilities will become even more important in the future. Indiana University is thus in a position to promote an area of fundamental research which has great implications.

4. The I.U. Geological Field Station in the Tobacco Root mountains is too distant from Yellowstone to provide a good base, being at least a four hour drive from the Yellowstone thermal areas. On the other hand, West Yellowstone is only a 30 minute drive from some of the main research areas within the Park.

*Copy of original document regarding the laboratory at West Yellowstone.*

***West Yellowstone lab proposal to IU page 1 Yellowstone Lab early years.jpg***

***West Yellowstone lab proposal to IU page 2 Yellowstone Lab early years.jpg***

# Laboratory of Thermal Biology

In March 1968 Dean Day asked me to create a name for the laboratory. After considerable thought, I decided on "Laboratory of Thermal Biology."

Note that by the time the memo creating the name was written, I was now the Principal Investigator of an Ecology Training Grant from the USPHS, and was in the process of getting a contract for Yellowstone work from the Atomic Energy Commission. For the summer of 1968 I anticipated 8 people: 2 post-doctorates (Tom Bott and Ray Lynn), one research associate (Louise), one research assistant (Sally Murphy), one graduate student (Bill Doemel), an hourly assistant (Maxine Cohen) and one part-time secretary (Nancy Doemel).

The general description of the Laboratory of Thermal Biology was something that might be sent to someone who might send an inquiry. Note the list of major equipment items. (By 1971 this list would be expanded considerably.)

# Naming the laboratory

INDIANA UNIVERSITY

INTERDEPARTMENTAL COMMUNICATION

Harry Day

Research & Advanced Studies

Laboratory of Thermal Biology
Indiana University
W. Yellowstone, Montana

FROM: T. D. Brock

DEPT. Microbiology

DATE. March 7, 1968

TELEPHONE. 7-5178

This will be a small research laboratory operated by the Department of Microbiology for the purpose of carrying out field research by faculty and students of Indiana University in Yellowstone National Park. The laboratory will be under the direction of Dr. Thomas D. Brock. Facilities at the laboratory will permit the study of biochemical, microbiological and physiological aspects of the growth of organisms in thermal environments. Study is also anticipated using the hot springs as models for problems of thermal pollution. Financial support for this laboratory is from a research grant to Dr. Brock from the National Science Foundation, an Ecology Training Grant from the U.S. Public Health Service, and from the Indiana University Foundation. Support is also being requested from the Atomic Energy Commission. In the summer of 1968, eight people will be working out of this laboratory: two postdoctorate fellows, one research associate, one research assistant, one graduate student, one part-time secretary, and an hourly assistant, in addition to the director.

cc: Howard Gest

*First description of the proposed lab.*

***Lab Thermal Biology first description 3-25-1968 TDB to Day.jpg***

# Facilities of the new laboratory

Laboratory of Thermal Biology
Indiana University
West Yellowstone, Montana 59758

1968?

Director: Dr. Thomas D. Brock

Mailing Address: Box 562
West Yellowstone, Montana 59758

(Dept. of Microbiology, Indiana University
Bloomington, Indiana 47401)

Telephone: (406) 646-7631 W. Yellowstone
(812) 337-5178 Bloomington

Location: In city of West Yellowstone, Montana

Permanent Staff: Dr. Thomas D. Brock
Mrs. M. Louise Brock
1 Technician

Facilities: No living accomodations. Laboratory is established in large mobile home on land owned by Indiana University. Expansion anticipated. Winterized. Gas, electricity, water. Carl Zeiss Universal phase-fluorescence microscope, other phase microscope, dissecting microscopes, Beckman DB-G recording spectrophotometer, Nuclear-Chicago gas-flow counter, Sonic oscillator, analytical balance, Spectronic 20 colorimeters, pH meter, facilities for microbiological work, field instruments for temperature, light, pH. Photographic darkroom. Photomicroscopy. Autoradiography.

Research in progress: Currently all research is conducted in Yellowstone National Park. Microbiology, ecology, biochemistry of organisms living in thermal environments. Ecology of ephydrid flies. Supported by NSF and AEC.

Research Projects that Would Be Encouraged: Collaborative work on all aspects of the biology of hot springs and other thermal habitats, especially zoological and botanical.

*Formal description of the West Yellowstone lab*

***Lab Thermal Biology formal description undated Yellowstone Lab early days.jpg***

## Yellowstone research support from the U.S. Atomic Energy Commission (AEC)

The Atomic Energy Commission (AEC) was a major U.S. department in the post-war and cold war years. Although its main focus was on weapons, it did have initiatives on peace-time uses of atomic energy, as well as on ecological matters related to extreme environments and habitats that were (or might be) devastated by atomic explosions. Among other things, they were supporting the studies taking place on Surtsey, the new island off the south coast of Surtsey.

The chief of the ecological program at AEC was plant ecologist John Wolfe, who coincidentally had been my plant ecology professor at Ohio State University. During one of my visits to Surtsey, I ran into Wolfe in Reykjavik and we ended up having dinner together. At that time my Yellowstone work was in its early stages, and I explored with him the kinds of research grants AEC might support. Later, I talked with him on the telephone from Washington, and we exchanged letters.

Among other things, AEC was interested in "thermal pollution", because nuclear power plants created lots of hot water which they dumped into natural bodies of water. I saw the possibility of relating my Yellowstone work to this topic. Also, we were using radioisotopes in our work and any research using isotopes was encouraged by AEC.

In January 1968 I submitted a proposal to AEC entitled "Microbiology of thermally polluted environments." Almost immediately I received a telephone call indicating that my proposal would be funded as a contract starting on May 1, 1968. Although the original proposal actually involved some work on the effluent from a power plant on the Wabash River, the main work involved Yellowstone. I soon learned that AEC was not actually interested in the Wabash River work, but in the Yellowstone work. Thus, the in the second year and thereafter only the Yellowstone work was supported.

The AEC contract essentially doubled my research support for the Yellowstone work and enabled me to hire more people and start new projects. Once the contract was funded, it essentially continued indefinitely (as did the NSF grant).

# Yellowstone 1968

1968 was the first year of our "official" West Yellowstone laboratory. Among other things, I bought a leather-covered Guest Book that I kept in a conspicuous location in the main room and asked visitors to sign. This provides partial documentation of who visited us each year, since not everyone signed.

I drove out alone in an I.U. station wagon, arriving in the Park May 27, 1968. I stopped in Jackson and visited grizzly bear researcher Frank Craighead. (I got to know him and his twin brother John the previous winter at the Old Faithful winter research expedition.) From the Tetons I went in through the South Entrance and stopped again at West Thumb Geyser Basin, photographing microbial "vees" on May 27.

Louise and technician Pat Holleman drove out in our personal Ford wagon. Pat had been my technician for several years but was somewhat footloose now because her husband Jerry, a physical chemist, had been drafted and sent to Vietnam. Post-doc Tom Bott and his wife Sue drove out in their own car.

In 1968 we had just the single lab trailer on Daley's lot, and all personnel lived privately. The Botts lived with Pat in the trailer owned by the motel, which was on the lot which we were buying. Louise and I lived in Daley's pink trailer and Bill and Nancy Doemel lived in a small silver trailer at Wagon Wheel trailer court. Nancy worked in the lab and typed the new textbook I was working on (Biology of Microorganisms) and correspondence. Ray Lynn, who had received his Ph.D. at Indiana University in phycology, was a post-doc just for the summer, staying in one of Mrs. Clark's cabins. (His faculty job at Utah State started at the end of the summer.)

Dick Wiegert, last year's visitor from the University of Georgia, now had his own NSF grant working on the brine fly food web. He and his crew rented Mrs. Daley's two green cabins and had their own mobile lab trailer at Facer's trailer court across the street from Daley's. I had hoped that Wiegert and Louise could collaborate on the insect studies but he had his own students and went off separately.

# Tim Daley cleaning a cutthroat trout on the shore of Yellowstone Lake

*Ellen and Ed Daley's son Tim. He was a high school student in the summer of 1968 and we hired him as a dishwasher and general all-around helper. Here he is cleaning cutthroat trout on the shore of Yellowstone Lake. June 12, 1968.*

***PICT0135.jpg***

# Lab group at the Upper Geyser Basin

*High school student Mary Davis (a visitor from Bloomington) and phycologist Ray Lynn hiking in the Upper Geyser Basin. Electron microscopist Mercedes Edwards behind. We often did "recreational" hikes on Sundays. August 1968.*

***PICT0137.jpg***

# Electron microscopist Merecedes Edwards

*Mercedes Edwards feeding a ground squirrel. Mercedes was an electron microscopist at the State Laboratory of Hygiene in Albany, NY. and first contacted me after reading my review in Science. We became good friends and published several papers together, including one on the fine structure of* Thermus aquaticus. *(Mary Davis in the background.) August 14, 1968.*

***PICT0140.jpg***

Around July 11, 1968 I went flew to Pittsburgh, lecturing at Pymatuning Lab of the University of Pittsburgh. I then spent a couple of weeks in Bloomington writing and advising graduate students Marge Crandall (yeast mating) and Paul Ray (*Thermus aquaticus*). I then flew to Kalamazoo (Gull Lake Laboratory of Michigan State University), where I visited Peter Hirsch and gave a lecture, and then returned to Yellowstone.

Mary Davis (a family friend who was a piano prodigy from Bloomington, 15 years old) visited for three weeks and Louise spent a lot of time with her. Indiana colleagues Martin Dworkin and David White visited for two weeks in early August. Also in early August, Mercedes Edwards (electron microscopy) and Norma Lang (phycology) visited, staying in a cabin at the Western Motel. I became more involved with visitors and had less time for my own research. In August 1968 we had 21 days of rain (and snow), the worst summer weather we (or West Yellowstone) ever had. This was not the best summer for hosting visitors!

Early in June I had hurt my back again, 17 years after a laminectomy to correct a previous injury. After several weeks my back improved and I even went on a backpacking trip in August with Mary Davis, Louise, and Nick Collins (of Wiegert's group) to Heart Lake Geyser Basin.

Our research was going well: Tom Bott's work was pivotal for the high temperature bacteria. Bill Doemel's work on *Cyanidium* was getting along well. Technician Pat Holleman helped whoever needed assistance, either in the laboratory or field. (Pat was one of the best technicians I ever had!)

Visitors in 1968:

- Donald White; geothermal geology; USGS, Menlo Park, CA
- Pat Muffler; geothermal geology; USGS, Menlo Park, CA
- Al Truesdale; geothermal geology; USGS, Menlo Park, CA
- Richard Castenholz and students; thermal algae; University of Oregon
    - Beverly Pierson; John Meeks
- Richard Wiegert; population ecologists; University of Georgia
    - Peter Fraleigh; Nicholas Collins
- Claude ZoBell; marine microbiologist; Scripps Institute of Oceanography; La Jolla, CA
- A.J. Bauman; lipid chemist; NASA, Ames Research Laboratory
- Martin Dworkin; microbiologist; University of Minnesota
- David White; microbiologist; Indiana University
- Paul Dunaway; ecologist; Oak Ridge National Laboratory

- Jeff Swinebroad; Atomic Energy Commission
- Bill Osburn, Atomic Energy Commission
- C.A. Tryon; animal ecologist, University of Pittsburgh
- Mercedes Edwards; electron microscopist, NY State Dept of Health, Albany
- Normal Lang, phycologist, University of California, Davis
- Deborah Delmer, plant physiologist, University of Colorado, Boulder
- Vern McMahon; biochemistry; University of Wyoming, Laramie
- Wolfgang Heinen, microbiologist, University of Nijmegen, Netherlands
- Lee D. Miller, remote sensing, Colorado State University, Fort Collins
- John Craighead, grizzly bear researcher, University of Montana
- Frank Craighead, grizzly bear researcher, State University of New York, Albana

## Expanding the Laboratory

As part of the budget for my new AEC contract, I had asked for funds for a mobile laboratory. After using the existing trailer for a while, I decided that this should be another house trailer, considerably larger than the first one. This new trailer was purchased in August 1968.

This unit was set up on the new IU lot which we had finally acquired title to on July 1, 1968. The first lab trailer remained on the Daley lot through the summer of 1968 and was not moved to its final resting place until mid-July 1969 after our own water and sewage had been set up. In 1968 water and sewage for the new trailer came from the Golden West Motel, as per our original agreement. We would not get our own water and sewage facilities until summer 1969.

As I was on sabbatical September 1968 through May 1969, I remained at West Yellowstone in September along with Louise and Pat after everyone else had gone back to Bloomington. I was to spend the fall semester in Yellowstone (on Indiana University funds) and the spring semester at NASA's Ames Research Laboratory (being paid by NASA).

In October 1968 we did some preliminary work fixing up the AEC trailer for a lab. The shelves were installed, desk and table made, etc. Louise and I continued to live in Daley's pink trailer. Pat lived in the rear bedroom of the new lab trailer until we closed up.

That fall at West Yellowstone was grimly dull, as West Yellowstone became almost a "ghost town" after Labor Day. We did accomplish a few things not easily done during the high season, such as more study of the superheated springs in the Upper Geyser Basin, but in general the stay in Yellowstone in the fall was not too useful. I am firmly convinced that the field season at Yellowstone should be no longer than four months and probably more like three months. However, less than three months is too short a time. You need time to complete on-going experiments, but not so much time that "field fatigue" sets in. Field work is great fun and fascinating, but it is very hard work, much harder than laboratory work!

After leaving Yellowstone on October 18 I drove to Utah State with Louise, and gave a seminar. At Utah State I met microbiologist Fred Post who had been doing some work on Great Salt Lake. Since I was interested in working on halophiles (another group of extremophiles), I talked at some length with Post about how to find good sampling locations on the lake. This discussion was very valuable when I finally got around to working at Great Salt Lake in 1973-1975.

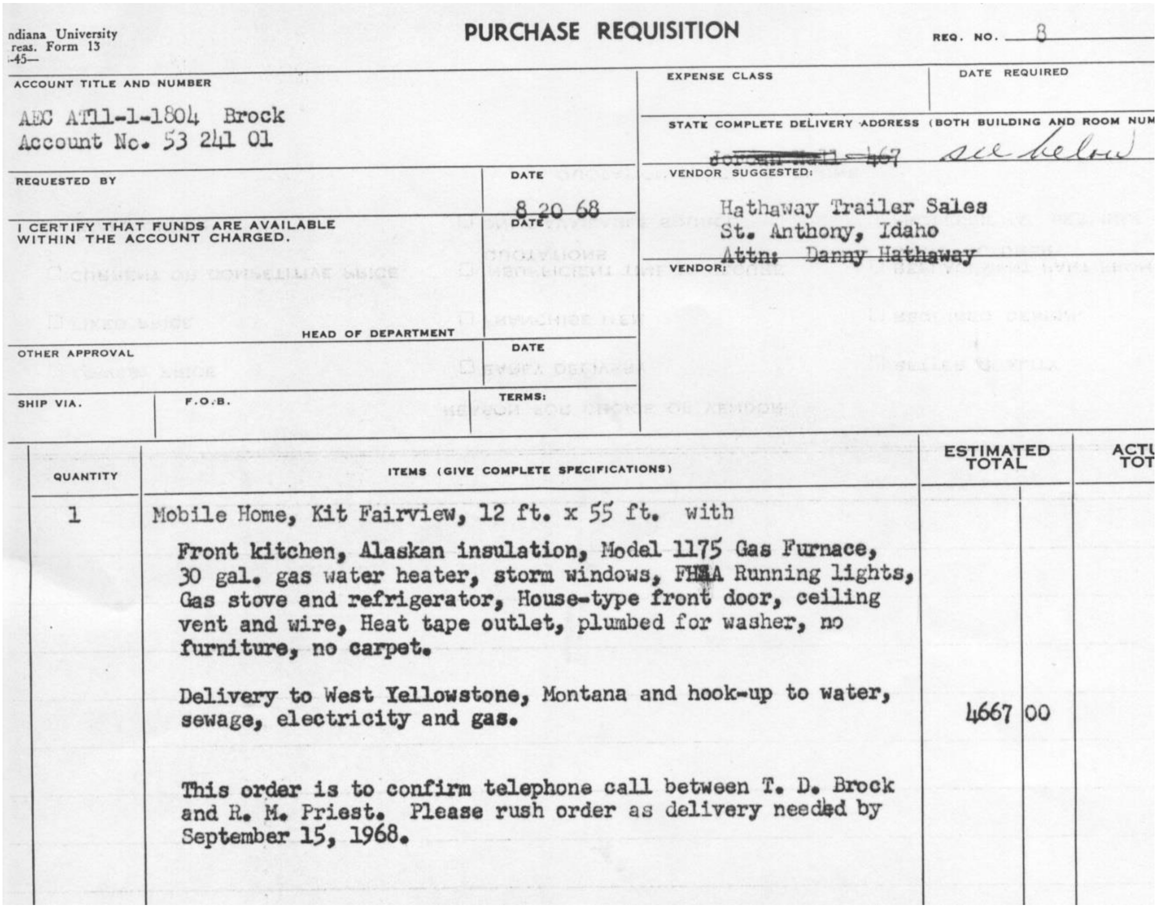
From Utah I flew to Las Vegas, rented a car, drove through Death Valley (more extreme environments) and eventually to San Diego, where I participated in a National Institutes of Health site visit for a major research program at UCSD (LaJolla). I flew home from there.

At the end of 1968, after the larger trailer purchased with AEC funds had been installed, we owned the lot, but did not have our own water and sewage.

The water and sewage for the large trailer (called IU Office in the diagram) was coming from the Golden West Motel, as per our purchase agreement. The water and sewage for the smaller trailer (called IU laboratory in the diagram) was coming from the Daley's, as per the rental agreement on their lot.

The final arrangements were not completed until the middle of 1969 after a new well and a new sewage connection were made.

# The second laboratory trailer



ndiana University
reas. Form 13
-45—

PURCHASE REQUISITION

REQ. NO. 8

ACCOUNT TITLE AND NUMBER
AEC AT11-1-1804 Brock
Account No. 53 241 01

EXPENSE CLASS

DATE REQUIRED

STATE COMPLETE DELIVERY ADDRESS (BOTH BUILDING AND ROOM NUM
~~Jordan Hall 467~~ see below

REQUESTED BY

DATE 8 20 68

VENDOR SUGGESTED:
Hathaway Trailer Sales
St. Anthony, Idaho
Attn: Danny Hathaway

I CERTIFY THAT FUNDS ARE AVAILABLE WITHIN THE ACCOUNT CHARGED.

DATE

VENDOR:

HEAD OF DEPARTMENT

OTHER APPROVAL

DATE

SHIP VIA.

F.O.B.

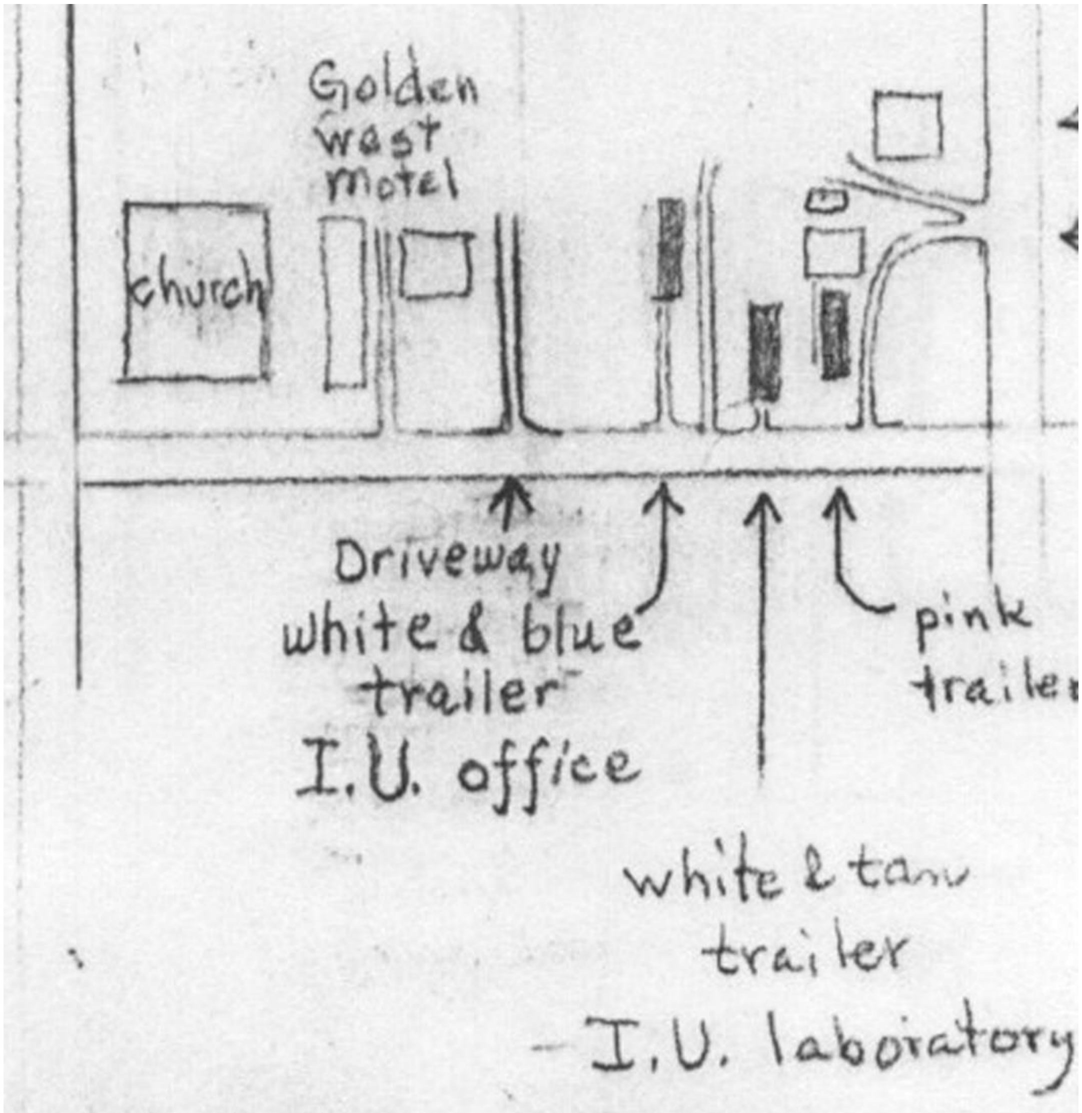
TERMS:

| QUANTITY | ITEMS (GIVE COMPLETE SPECIFICATIONS) | ESTIMATED TOTAL | ACTU TOT |
|---|---|---|---|
| 1 | Mobile Home, Kit Fairview, 12 ft. x 55 ft. with Front kitchen, Alaskan insulation, Model 1175 Gas Furnace, 30 gal. gas water heater, storm windows, FHA Running lights, Gas stove and refrigerator, House-type front door, ceiling vent and wire, Heat tape outlet, plumbed for washer, no furniture, no carpet. | | |
| | Delivery to West Yellowstone, Montana and hook-up to water, sewage, electricity and gas. | 4667 00 | |
| | This order is to confirm telephone call between T. D. Brock and R. M. Priest. Please rush order as delivery needed by September 15, 1968. | | |

*Purchase order for 2nd trailer, AEC funds*

***Trailer 2 AEC PO 8-20-1968.jpg***

# The laboratory arrangements at the end of 1968




*Sketch of temporary lab arrangement after 2nd trailer purchased*

***IU lab set-up West Yellowstone 1968.jpg***

# Arranging for water and sewage; I

95 lab

INDIANA UNIVERSITY
INTERDEPARTMENTAL COMMUNICATION

TO: J. Franklin
DEPT. Treasurers Office
SUBJ. Request for Funds

FROM: Thomas D. Brock TDBrock
DEPT. Microbiology
DATE. November 1, 1968
TELEPHONE. 7-5178

This is a request for funds to permit installation of water and sewage lines at our W. Yellowstone, Montana research laboratory. I estimate this will require about $3000.

1. Background. In early July 1968, Indiana University acquired title to a lot in W. Yellowstone, Montana for use in the research projects of T.D. Brock and associates. As part of the purchase agreement, the seller agreed to provide water and sewage for two years. The option to purchase was signed by I.U. in April 1968, and the seller interprets this as the date from which the two years agreement for water and sewage would be provided. We currently have one large mobile home on the I.U. lot, and our other mobile home is on an adjacent rented lot. We hesitate moving the second mobile home to the I.U. lot until final arrangements for water and sewage are made, since the location of the mobile home will depend on the location of pump and piping. W. Yellowstone, Montana has a sewage treatment plant and all that is required for sewage is a connection to the line at the rear of the I.U. lot. A water plant does not exist, but the ground water supply is excellent and wells are easily made.

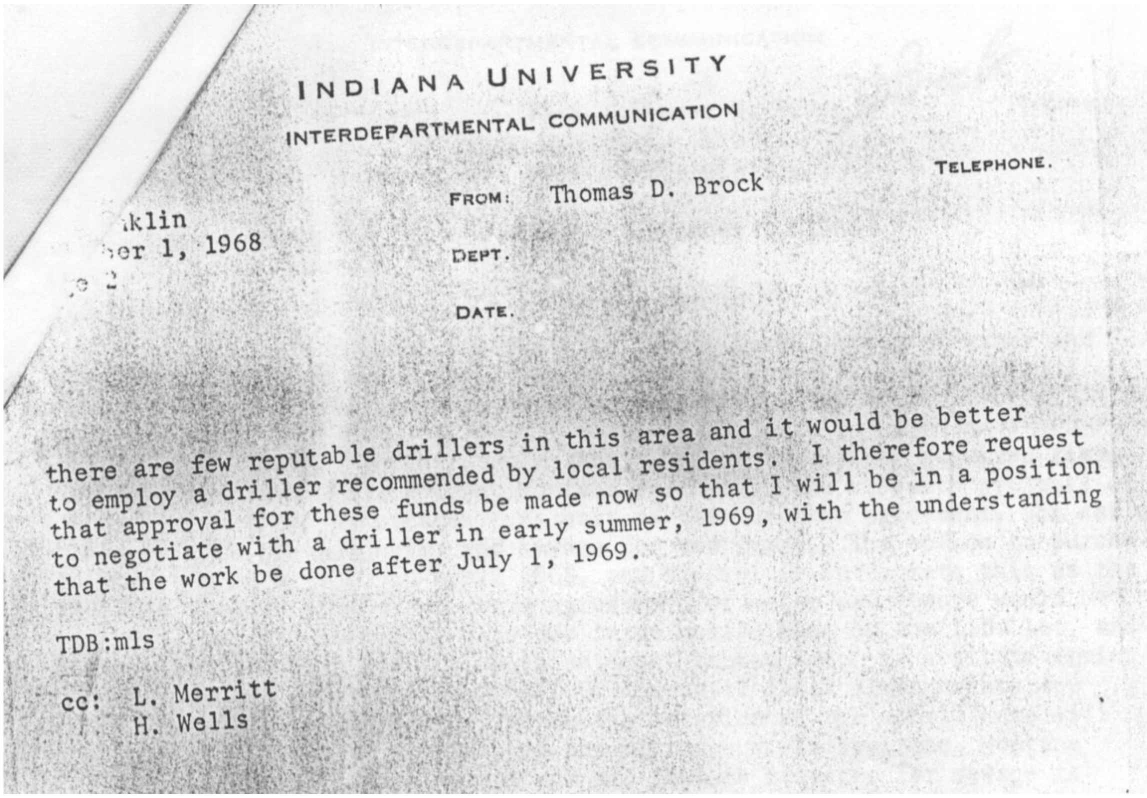
2. Requirements. From conversations with knowledgeable local residents I have ascertained that sewage connection would cost about $1000 and $2000 would be required for well drilling, cost of pump, lines, etc. Thus about $3000 would be required to complete the installation. In my original request to I.U. for funds to purchase the lot, I asked for $15,000, to include cost of both lot and water and sewage. The lot which was purchased cost only $11,000, and with the $3000 yet needed for water and sewage the sum would be $14,000, which is less than the $15,000 originally requested.

3. Timing. Because of the long winter and deep snow at W. Yellowstone, water and sewage installation must be done in summer or early fall. Since our agreement on water and sewage runs only until April 1970, this means that installation must be carried out sometime in 1969. I would therefore like to request that funds be provided for fiscal year 1970, to be spent sometime after July 1, 1969. Since there is no well driller resident in W. Yellowstone, arrangements will have to be made with a firm either in Bozeman or Dillon, Montana. Because of the distance involved, the driller will probably like to do the work at a time when he has other projects in the W. Yellowstone area. If necessary I will get a bid for this work, but I would prefer to handle this without competitive bidding, since

*Request for funds for water and sewage*

***Lab Thermal Biology water sewage page 1.jpg***

# Arranging for water and sewage; II



INDIANA UNIVERSITY

INTERDEPARTMENTAL COMMUNICATION

klin FROM: Thomas D. Brock TELEPHONE.
er 1, 1968 DEPT.
e 2 DATE.

there are few reputable drillers in this area and it would be better to employ a driller recommended by local residents. I therefore request that approval for these funds be made now so that I will be in a position to negotiate with a driller in early summer, 1969, with the understanding that the work be done after July 1, 1969.

TDB:mls

cc: L. Merritt
H. Wells

*Request for funds water and sewage page 2*

***Lab Thermal water sewage page 2.jpg***

**Sampling at a U.S.G.S. drill hole**

One unusual opportunity took place in the late fall of 1968. It was after the tourist season had almost closed, and I was somewhat freer to operate in public areas.

In 1967-1968 the USGS had carried on an extensive drilling program in the Park. This expensive endeavor was funded by NASA, although all the supervision and scientific work was done by USGS geologists from the Menlo Park, California unit. It was a little tricky doing this work, as the drill rig was installed on a heavy truck, which had to driven off-road to each drill site. I'm not sure how many sites they drilled, but I visited three sites, Biscuit Basin, Porcupine Hills, and Norris.

By this time, I had discovered bacteria living in most of the neutral/alkaline boiling springs, and was interested in whether the thermal water at depth contained bacteria. Bob Fournier, one of the USGS geologists, was working in the Park in late September and was willing to obtain a sample of water for me. This came from the drill hole at the Porcupine Hills site, which is in Fountain Flats in the Lower Geyser Basin. The drill hole itself was capped. (They weren't interested in creating any artificial thermal vents!) To sample, thin stainless steel tubing was sent down the hole to where the water was. As I recall, the depth of this drill hole was about 100 feet. Once the tube had reached its depth, the pressure at the surface was released and water filled the tube. Of course, the water cooled as it rose to the surface, so my sample was at ambient temperature.

I brought the sample back to the West Yellowstone laboratory for processing. I saw no bacteria (or any other creatures) when I looked at a few drops of this water directly through the microscope. (This would have been unexpected and would have meant the bacterial population was quite high!)

I then filtered a large volume (100 ml) of water through a membrane filter whose pores were so small that no bacteria would go through. I used an acridine orange stain to see bacteria that might be on the filter. Again, no bacteria were seen.

I wasn't surprised to find no evidence of bacteria in the sample. Two reasons: The water temperature at depth was well over 200 C and the water probably lacked nutrients that would support the growth of bacteria.

Another outcome of the USGS drilling program was the production of detailed maps of many areas. I was given access to these maps by Donald White and Robert Fournier, and made copies on the primitive copy machine (3M thermo-fax copier) we had at West Yellowstone. These maps were later used by Charlene Knaack to make the detailed maps that were published in my Springer-Verlag monograph.

# Drilling at the edge of the Porcelain Basin in the Norris Geyser Basin


*Drill rig on the north edge of the Porcelain Basin in Norris Geyser Basin. The rig was able to drive to this site because at the end of 1966 the Grand Loop Road had been moved, and the old road was still in place. (The old road ran just below the Norris Museum.) The old road site is now a trail. The photo is from Summer 1968.*

***USGS drill rig Norris 1968 B-W.jpg***

# Collecting water from a USGS drill hole

*Tom Brock watching as Bob Fournier removed a sample of water from drill hole Y-13 at Porcupine Hills, in the Lower Geyser Basin. Except for sampling, the hole remains covered and locked. The water is being removed with stainless steel tubing. The sampling depth was about 100 feet. The water goes through the complete coil of cable and into a sampling container. Careful microscopy showed no evidence of bacteria. September 28, 1968.*

***PICT0181.jpg***

**Fall-Winter 1968: A crisis**

After we left West Yellowstone, I was quite busy in the fall of 1968, partly because of speaking invitations that came as a result of my Life at High Temperature article in *Science.* Also, my *Principles of Microbial Ecology* book was now out and I was getting lecture requests related to that work.

I had agreed to give a lecture series on microbial ecology at Macdonald College in Montreal followed by a keynote address at meeting of an American Society for Microbiology branch in Albany, N.Y. After both of these engagements, in early November, I went on to New York City . to confer with my publisher about art work and format for the upcoming "Biology of Microorganisms".

It was at this time, just before Thanksgiving, that my back started really bothering me. I flew home from New York, then about a week later drove east in a rented car, stopping at Chillicothe, Ohio (to see my mother; who was 77 years old), then to Morgantown, W.Va. and Philadelphia for lectures. This year I had been appointed to the research grant reviewing panel for the general ecology division of the National Science Foundation. After Philadelphia I went to Washington for the NSF panel meeting.

Now my back was really bothering me, and I had to stand up during most of the time at the NSF meeting.

I returned home by air and on Thanksgiving day my back completely gave out and it was clear I was in trouble. This changed my plans for the rest of the school year.

## Bacterial growth in boiling water

The discovery of the pink bacteria at the highest temperature in the Octopus Spring channel made it seem likely that this organism or some other bacterium would be growing in the boiling pool itself. This was easy to show by immersing microscope slides in the pool and removing them after a few days. The microscope revealed lots of bacteria, many of them in microcolonies which provided strong evidence of growth.

In 1967, as I was preparing the final version of my major article for *Science*, I placed glass microscope slides in a number of boiling springs in order to get good photomicrographs of high-temperature bacteria. This became a major study for me in August of 1967. I put slides in boiling or superheated springs in many boiling or superheated springs in the Lower Geyser Basin and Norris Geyser Basin, retrieving them after a week.

This was a very simple experiment. When I went in the field, I brought with me several boxes of microscope slides and a roll of thin Nylon cord. The slides were labeled with a diamond pencil. At each spring I cut a shank of cord, tied a slide to one end and tied the other end to a rock, then tossed the slide into the pool. The following week I retrieved all the slides, let them air dry briefly, and put them in a slide box. At the West Yellowstone lab I examined each slide with the Zeiss phase microscope, using a water-immersion lens.

Every boiling or superheated spring that was at neutral or alkaline pH showed good evidence of bacteria. In most case, the bacterial coating was so dense that the slide had a cloudy film.

# Studying the growth of high-temperature bacteria at Octopus Spring


*Working in the source and outlet of Octopus Spring July 19, 1970. Louise Brock. Slides, cover slips, and radioisotopes being used. Note the "Do Not Disturb" sign. Although tourists occasionally showed up here, they never disturbed anything.*

***PICT0222.jpg***

Although my slide study certainly suggested that growth was actually taking place in boiling water, it was necessary to prove this experimentally. This outstanding study was Tom Bott's work.

Bott came to IU in early 1968 as a USPHS post-doc fellow from UW-Madison, where he had worked on the ecology of *Clostridium botulinum*. He spent the spring semester at Bloomington working out methods for studying the growth of bacteria on surfaces, using normal habitats such as rivers and springs. (This work was published: Bott and Brock; Limnology & Oceanography "Growth and metabolism of periphytic bacteria: methodology". Volume 15: 333-342 (1970))

Bott hit upon a simple way of proving that bacteria were actually growing on the slides, and not just attaching from the environment. This involved the use of germicidal ultraviolet (UV) radiation. A slide which had been in the habitat for a few hours or a day was brought to the edge of the stream and without letting it dry it was placed in a dish with a shallow amount of water. It was then irradiated with germicidal UV for 10 minutes, long enough to kill all the bacteria that had attached and started to grow. Since germicidal UV does not penetrate through glass, the underneath of the slide would have untreated bacteria.

The irradiation source was a hand-held device containing a shortwave UV bulb emitting germicidal radiation of 254 nm. It was powered by a 12 volt storage battery with a Terado inverter to convert to 110 volt alternating current. These were heavy duty batteries, and Bott had two so that one could be charging while the other was being used. A heavy-duty battery carrier was used to lug the batteries to the field site.

Bott was at Yellowstone for two summers, 1968 and 1969. The growth rate work was done in the summer of 1968.

By the time Bott started at Yellowstone, I had done a detailed survey of springs, and had found a number of boiling or superheated springs that were suitable for this work. A spring had to be near enough to the road so that it could be reached fairly easily, and it had to have a good hard rocky edge so that it was possible to work safely.

Bott's work showed conclusively that the bacteria were growing in boiling water, and at surprisingly fast rates. (Doubling times varied from 2 to 7.5 hours.) The work was published in *Science*: "Bacterial growth rates above 90 C in Yellowstone hot springs. Vol. 164, pp. 1411-1412.

# Tom Bott working at Boulder Spring

*Tom Bott working along the effluent of Boulder Spring, July 29, 1968. This is a very hot part of the channel as shown by the fact there is no microbial color. This is a very typical work shot, showing thermistor, timer, test tubes incubating, ice bucket. This was the summer when he was doing the growth rate studies. The irradiation treatments were done in this hot effluent, after which the slides were returned to the boiling pool. At no time were the slides out of water.*

***PICT0216.jpg***

# Data from Bott's important *Science* paper

Table 1. Bacterial counts on irradiated (UV) and unirradiated microscope slides. Irradiated slides were treated eight times at approximately equal intervals during the immersion period.

| Treatment | Immersion time (hr) | Average number of cells per field: Top of slide | Average number of cells per field: Bottom of slide |
|---|---|---|---|
| | *Geyserino* | | |
| UV | 74.5 | 3.6 | 889 |
| UV | 74.5 | 1.4 | 431 |
| Control | 74.5 | 948 | 1385 |
| | *Porcupine* | | |
| UV | 138.75 | 1.4 | 149.0 |
| Control | 138.75 | 158.1 | 118.1 |
| | *Stepbrother* | | |
| UV | 72.0 | 8.2 | 136 |
| UV | 72.0 | 1.6 | 7.4 |
| Control | 72.0 | 42.4 | 34.0 |
| | *Boulder Experiment 1* | | |
| UV | 74.5 | 0.56 | 144 |
| UV | 74.5 | 0.88 | 120 |
| Control | 74.5 | 250 | 125 |
| | *Experiment 2* | | |
| UV | 72.0 | 9.4 | 889 |
| UV | 72.0 | 1.6 | 961 |
| Control | 72.0 | 1361 | 626 |
| | *Boulder Effluent* Experiment 1* | | |
| UV | 12.0 | 2.62 | 32.2 |
| Control | 12.0 | 30.7 | 24.7 |
| | *Experiment 2* | | |
| UV | 12.0 | 4.0 | 29.6 |
| UV | 12.0 | 0 | 26.5 |
| UV | 12.0 | 0 | 31.3 |
| Control | 12.0 | 32.0 | 23.2 |
| | *Pool A* Experiment 1* | | |
| UV | 53.0 | 2.0 | 32.5 |
| Control | 53.0 | 10.5 | 15.8 |
| | *Experiment 2* | | |
| UV | 33.0 | 2.5 | 39.0 |
| UV | 33.0 | 2.2 | 39.2 |
| Control | 33.0 | 37.9 | 44.8 |

* Counts for Pool A and Boulder Effluent are number of cells per microcolony rather than number of cells per field.

20 JUNE 1969

*Bott's data proving that growth was occurring on the slides. The cell numbers on the unirradiated sides of the slides was much higher than that on the irradiated sides. Table 1 from Bott and Brock, Science 1969.*

***Bott Science 1969 Table 1.jpg***

# Growth curves of bacteria in 90 C water

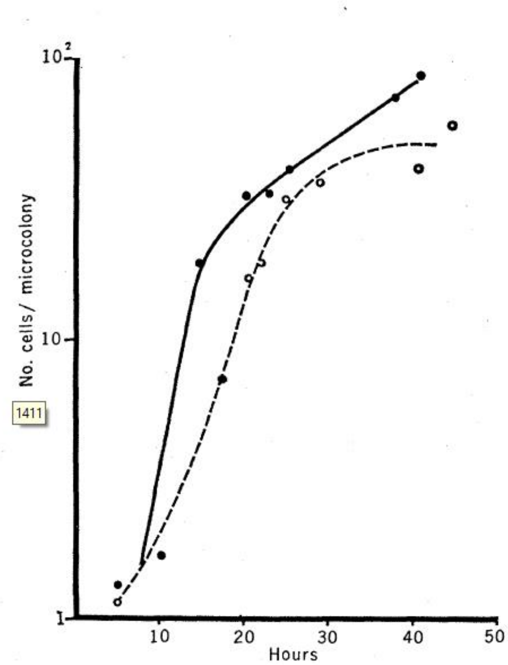



Fig. 1. Growth curves of bacteria in Pool A. Results of two independent experiments.

*Growth curves of bacteria living in Octopus Spring (Pool A). The organism in this pool grows as microcolonies, so growth rate was calculated by counting the number of cells per microcolony at different times. From Bott and Brock* Science *1969.*

***Bott Science 1969. Fig 1.jpg***

# Summary data from boiling or superheated pools

Table 2. Summary of bacterial generation times for various springs.

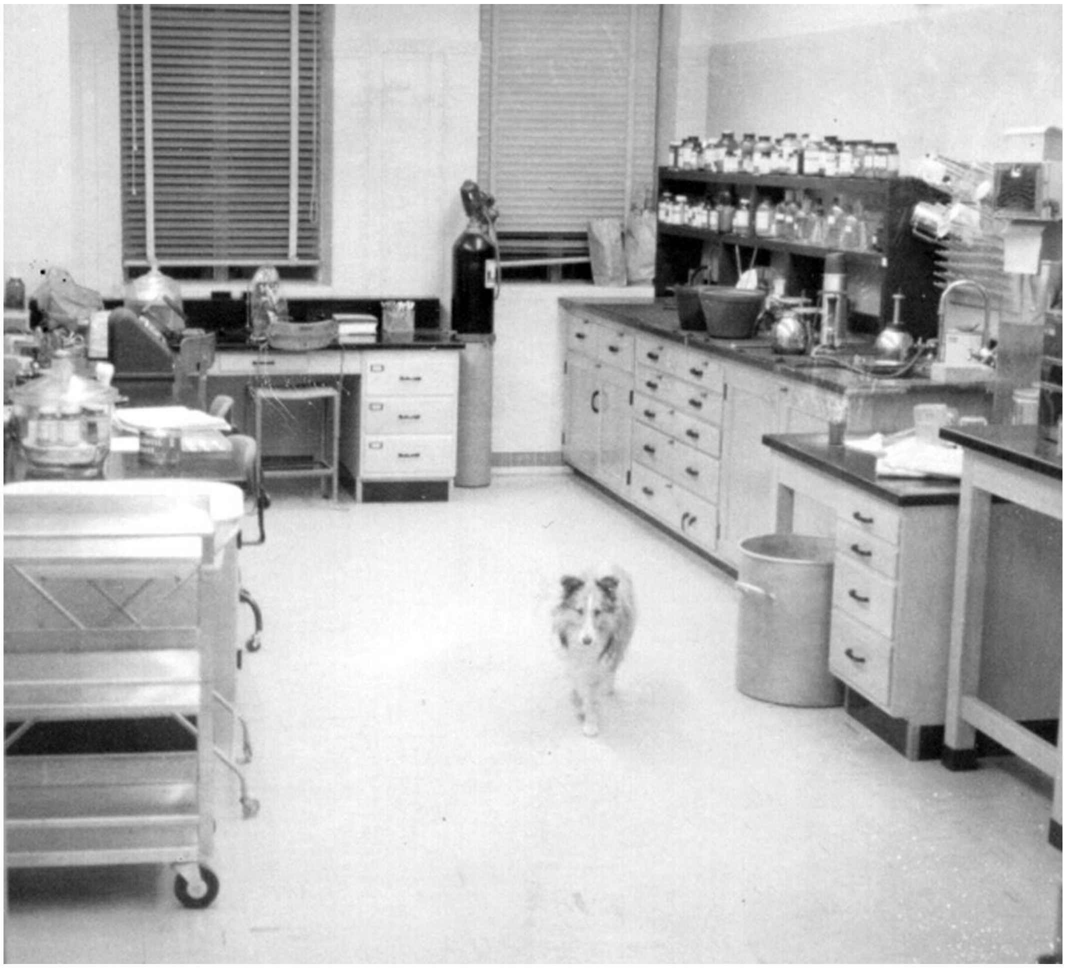
| Experiment | Temperature range* (°C) | *p*H | Generation time (hr) |
|---|---|---|---|
| | *Geyserino* | | |
| 1 | 93.0–94.5 | 8.5 | 4.0 |
| 2 | 92.5–94.5 | | 5.0 |
| | *Porcupine* | | |
| 1 | 92.8–94.8 | 8.6 | 6.0 |
| 2 | 94.2–95.2 | | 6.0 |
| | *Stepbrother* | | |
| 1 | 91.8–92.5 | 8.65 | 5.5 |
| 2 | 91.9–93.0 | | 3.5 |
| | *Boulder* | | |
| 1 | 90.0–91.8 | 8.9 | 7.5 |
| 2 | 90.5–91.5 | | 4.75 |
| 3 | 90.3–91.8 | | 3.0 |
| | *Boulder Effluent* | | |
| 1 | 78.0–80.5 | 8.9 | 2.2† |
| 2 | 79.0–80.5 | | 2.2† |
| 3 | 79.8–81.8 | | 2.1† |
| | *Pool A* | | |
| 1 | 90.5–92.0 | 8.1 | 3.0† |
| 2 | 89.0–91.0 | | 2.0† |

* Temperature ranges listed encompass measurements made during the experiments, which usually extended over 2 to 3 days. Measurements of *p*H were made with a glass electrode at the temperature of the spring. † These values calculated on the basis of rate of increase in number of cells per microcolony. All others are from counts of number of cells per microscope field.

*Summary of growth rates (generation or doubling time) in boiling or superheated hot springs. The doubling times are surprisingly fast! From Bott and Brock Science 1969.*

***Bott Science 1969 Table 2.jpg***

# Jordan Hall room 359



*Jordan Hall, Room 359, Indiana University. Where all the original work on* Thermus aquaticus *was carried out. Photo from 1961. I often brought Korby with me on the weekends!*

***Jordan Hall 361 1961.jpg***

## Yellowstone 1969.

Because of my back I was unable to go to Yellowstone in the winter of 1969 as we had intended. Since we had already planned the trip, Bill Doemel and Hud Freeze went instead. Bill wrote a very nice detailed report of this trip that is in the Yellowstone 1969 file folder. One accomplishment was the discovery of that Mushroom Spring was no longer flowing and that a new geyser very near (which we called Toadstool Geyser) had replaced it. (Although Mushroom Spring eventually started flowing again, it was never the great site it had been.) They had very heavy snow and great difficulty in getting around in the Park on Ski-Doos. They did take a number of samples of various habitats, and Bill got to see his *Cyanidium* habitats in the winter.

My back operation (spinal fusion) took place in late January 1969 (42 years old) at Methodist Hospital in Indianapolis. On the way to the hospital I was so laid up that I could not sit up in the car. I laid in the back of the station wagon while Louise drove from Bloomington to Indianapolis. After the operation and two weeks in the hospital, I was brought back in a divided cast to Bloomington by ambulance.

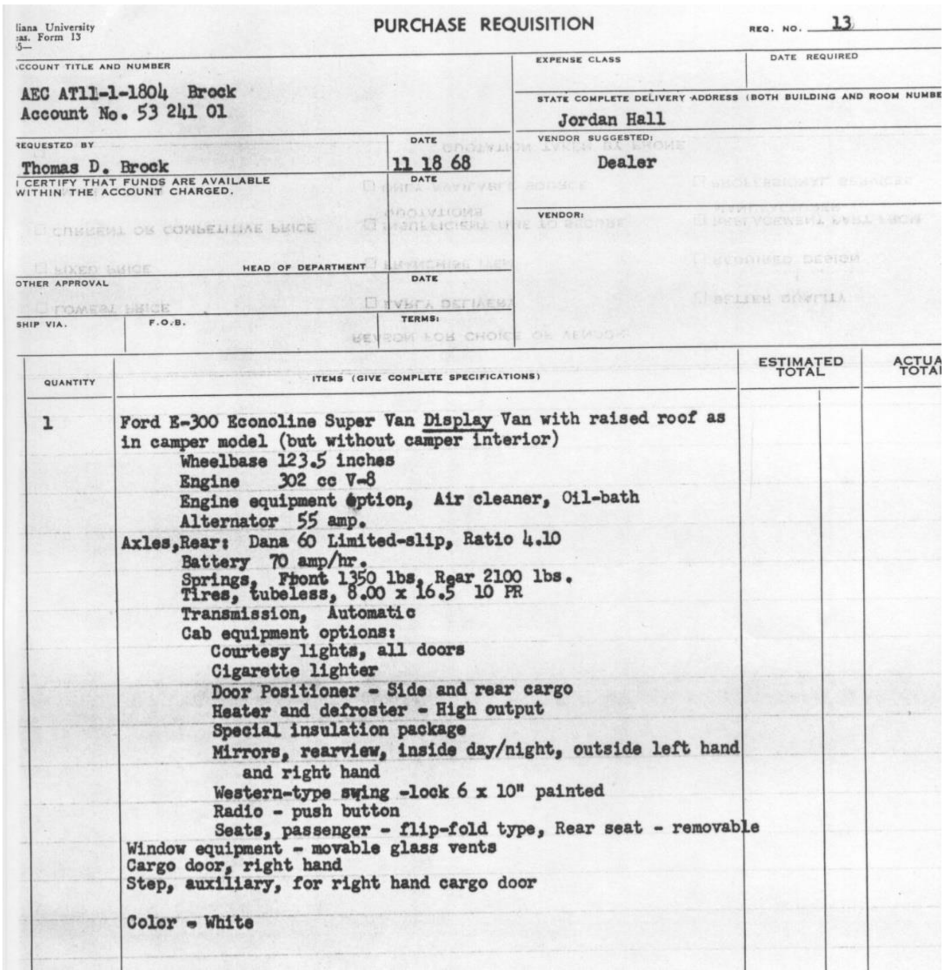
I was on my back for two months in Bloomington. Since her husband Jerry was in Vietnam, Pat Holleman lived with us. This was good, because she and Louise could keep each other company. My hospital bed was in the living room, and visitors came often. (Our house was only five blocks from Jordan Hall.) I advised students and I had a phone by the bed and did lots of calls. I also managed to get some crucial papers published, including the major one with Tom Bott for *Science* on growth rates of bacteria living in boiling water.

After my two months at home, healing my back, I went back to Methodist by ambulance. They x-rayed me and pronounced me all right and I sat up and tried to walk again. I had a back brace for several months. After two months on my back I had to learn how to walk again. I did lots of walking and swimming. One week after I was back on my feet I started to teach and finished out the semester April-May 1969.

Gary Darland joined the lab in March 1969 and began studies on acidophiles, opening up a whole new line of work, and discovering some important new organisms.

The AEC contract had been funded during the previous year and in addition to the second house trailer, a Ford Econoline 300 Van was purchased in November 1968. We purchased a specially modified van that could be used as a **real** mobile lab. The roof was cut and remounted on rods so that it could be raised to give more head room. Interior units we designed involved a sink, table for microscopy, and a bed. Although this new unit was only occasionally used to its fullest capacity, I did find it very useful in some critical Yellowstone experiments (incubations at temperatures above boiling) done in the parking lot by Great Fountain Geyser. Also, Bill Doemel and I used it to visit a number of important thermal areas between Yellowstone and the west coast.

# Purchasing the mobile laboratory



liana University
as. Form 13

PURCHASE REQUISITION

REQ. NO. 13

ACCOUNT TITLE AND NUMBER

AEC AT11-1-1804 Brock
Account No. 53 241 01

EXPENSE CLASS | DATE REQUIRED

STATE COMPLETE DELIVERY ADDRESS (BOTH BUILDING AND ROOM NUMBE

Jordan Hall

REQUESTED BY: Thomas D. Brock — DATE: 11 18 68

VENDOR SUGGESTED: Dealer

I CERTIFY THAT FUNDS ARE AVAILABLE WITHIN THE ACCOUNT CHARGED.

DATE

VENDOR:

HEAD OF DEPARTMENT — DATE

OTHER APPROVAL

SHIP VIA. | F.O.B. | TERMS:

| QUANTITY | ITEMS (GIVE COMPLETE SPECIFICATIONS) | ESTIMATED TOTAL | ACTUA TOTAL |
|---|---|---|---|
| 1 | Ford E-300 Econoline Super Van Display Van with raised roof as in camper model (but without camper interior)<br>Wheelbase 123.5 inches<br>Engine 302 cc V-8<br>Engine equipment option, Air cleaner, Oil-bath<br>Alternator 55 amp.<br>Axles,Rear: Dana 60 Limited-slip, Ratio 4.10<br>Battery 70 amp/hr.<br>Springs, Front 1350 lbs. Rear 2100 lbs.<br>Tires, tubeless, 8.00 x 16.5 10 PR<br>Transmission, Automatic<br>Cab equipment options:<br>Courtesy lights, all doors<br>Cigarette lighter<br>Door Positioner - Side and rear cargo<br>Heater and defroster - High output<br>Special insulation package<br>Mirrors, rearview, inside day/night, outside left hand and right hand<br>Western-type swing -lock 6 x 10" painted<br>Radio - push button<br>Seats, passenger - flip-fold type, Rear seat - removable<br>Window equipment - movable glass vents<br>Cargo door, right hand<br>Step, auxiliary, for right hand cargo door<br><br>Color - White | | |

*Request for purchase for mobile lab*

***Ford Van PO 11-18-1968 AEC.jpg***

# The mobile lab in the field

*In mid-August 1969 Bill Doemel and I used the Ford van for a very successful trip through Idaho, Nevada, Oregon, and California, visiting a lot of thermal areas, as well as salt pans in SF Bay.*

*The photo shows the mobile lab at Steamboat Springs, Nevada, August 29, 1969. The top has been raised to provide more head room while working.*

***PICT0223.jpg***

**Travel and accommodations summer 1969**

The spring of 1969, Doemel left early for Yellowstone, arriving about May 8. Because I was still having difficulty after my spinal fusion sitting for long periods of time, I flew and Louise drove out by herself in our personal Ford Station Wagon. To break up the trip, I flew to Denver and met her there. She had driven on earlier. We stayed with Debbie and Tom Delmer at Boulder for a day, then I flew on to Yellowstone. Bill met me at the airport. Louise arrived the next day. Nancy Doemel and Maxine Cohen (technician; mainly dishwasher) drove out together.

The Doemels lived in the pink trailer, Louise and I lived in the rear bedroom of the new large lab trailer and Botts lived in one of Mrs. Morris's cabins. Darland drove through to Seattle, left his family, then came back alone to live in a small trailer near the Mormon Church. He then decided it was large enough and brought his wife, two kids, and dog back from Seattle. Maxine Cohen, hired as a general worker and dishwasher, lived in Mrs. Clark's small cabin.

This was the summer that we finished setting up the lab trailers. First we had to set up the water and sewage. The well was drilled by Carl Hollenseiner, Dillon, MT, who had a lot of experience drilling West Yellowstone wells.

The pump, pressure tank, and piping to the trailers was done by West Yellowstone local handyman Bert Fields

The bigger trailer purchased from the AEC contract remained on the IU lot where it had been placed in August 1968, but connected to the new water and sewage. The small trailer that had been on Daley's lot since 1967 was now moved behind the larger trailer and set at right angles, and then hooked up. Gas and electric were connected to both trailers. This work was done by the contractor, but Doemel, Louise, Darland, and I spent quite bit of time levelling the trailers. A diagram of how the lab trailers looked after 1969 is shown below.

West Yellowstone has a fantastic aquifer that is at the bottom of about 125 feet of porous gravel. The water is cold and very soft. I spent some time watching the well driller work. He told me he knew exactly where he would hit water, and he was correct. Once the well was in, Bert Fields installed the pressure tank and pump. According to Bert Fields' specifications, a Myers 3-wire submersible pump was used and a 120 gallon pressure tank. There was no possibility of insulating the water supply, so it was necessary for us to drain the water from the whole lab system at the time we were leaving for the winter. (Despite our best efforts, drainage was not always complete so that when we arrived in the spring, there would sometimes be a little ice in the lines.) The water system always worked very well, and we never had any trouble providing enough water for both trailers. Because the water was so soft, we did not need much distilled water.

See later for detailed instructions on how the lab had to be closed for the winter.

Our notice of completion of well was dated July 12 1969. The sewer installation came afterward in mid-July 1969. (Before that, we had been connected to the Golden West Motel sewage.) The sewer installation was done by Joe Wilson of Earsel’s Service. There is a map in the ***Yellowstone lab early years*** file showing the location of the wye for this service as 44 feet east of manhole number 40. The main sewer line runs along the middle of the alley behind the IU lot.

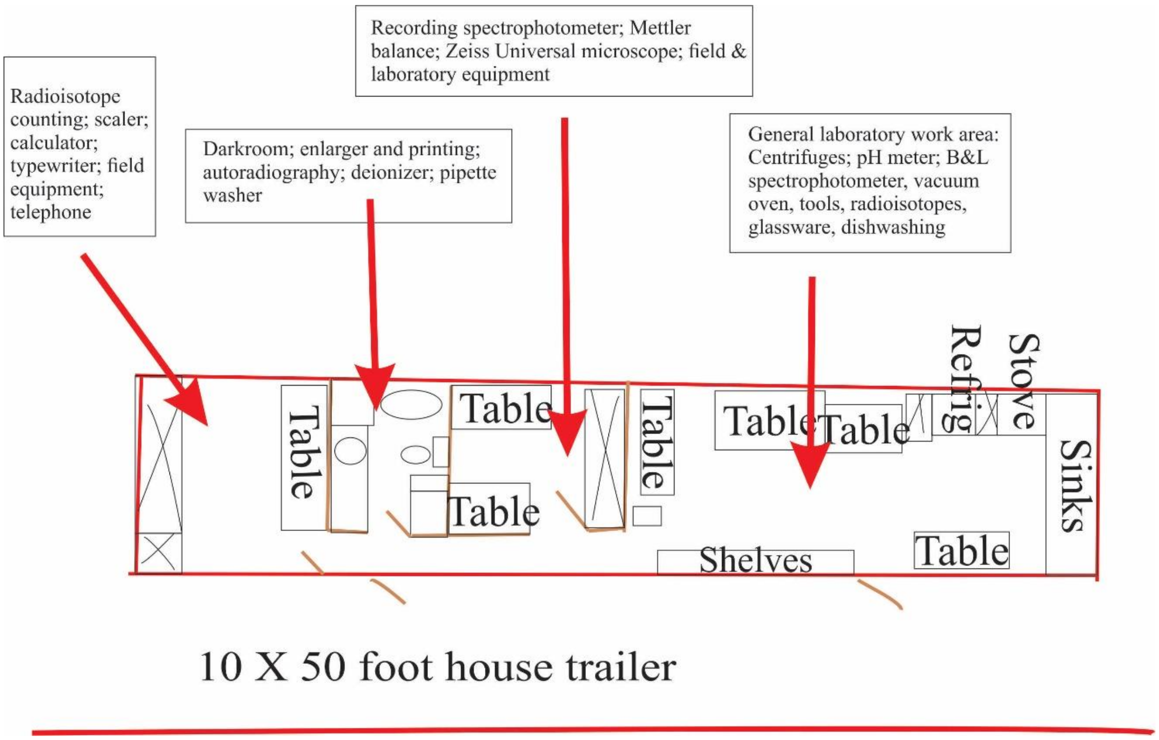
There was lots of paper work needed in getting all these services installed. I did it all myself, learning as I went along. The experience in setting up the West Yellowstone lab trailers was very useful in later years.

# The well and water tank for the lab


*Well and pressure tank at the rear of the lot. The pump was a submersible, situated at the bottom of the 125-foot well. Since the water system was unheated, it had to be drained at the end of every summer, and restarted the following spring. To protect the tank and water system, we later purchased a small prefabricated utility building from Sears and installed it near the well. The pressure tank was placed inside the well. See photo of the rear of the laboratory from 1974. [**link lab 1974**] Photo July 13, 1969.*

***PICT0154.jpg***

# Floor plan of the small lab



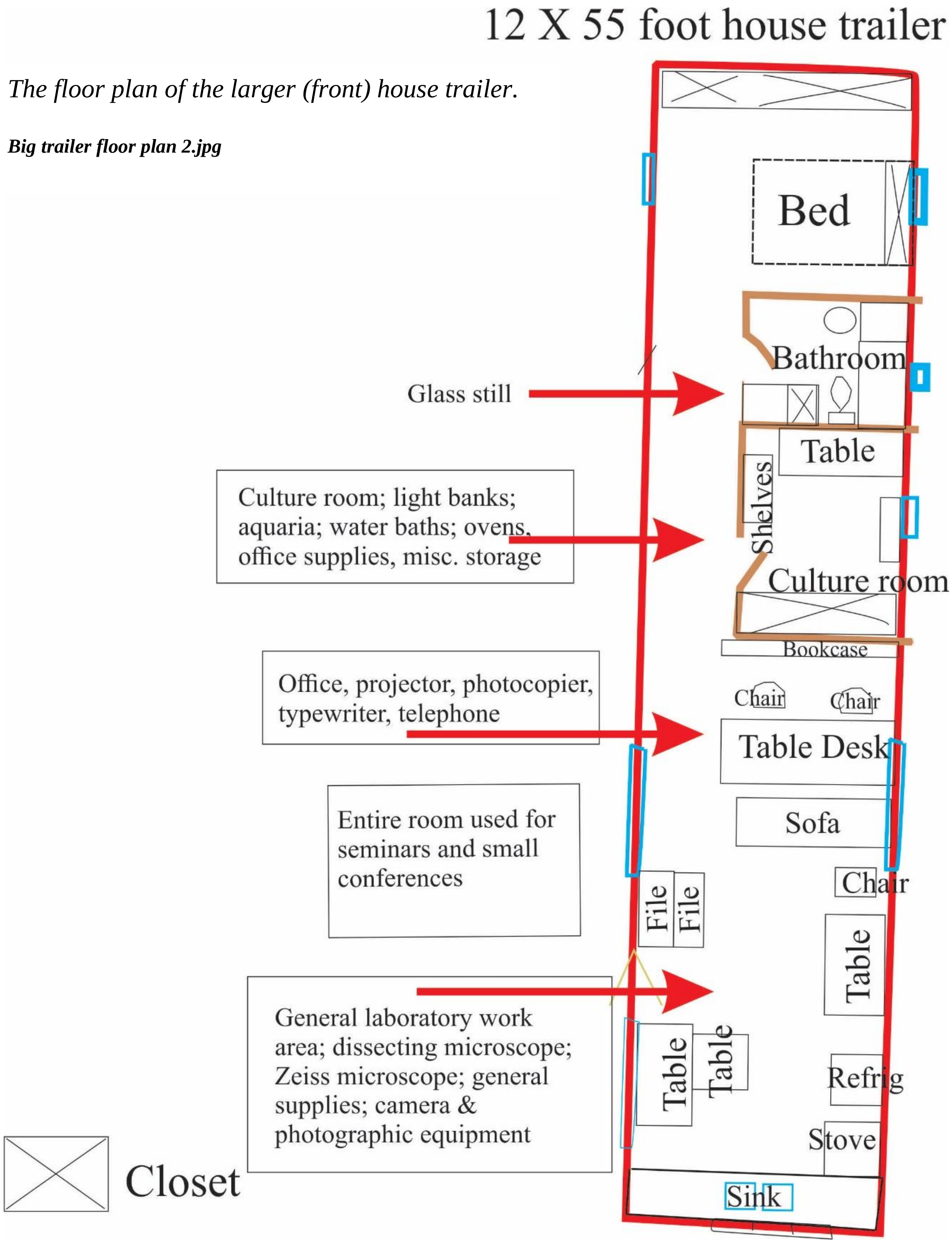

*The floor plan of the smaller (rear) house trailer.*

***Rear lab floor plan 2.jpg***

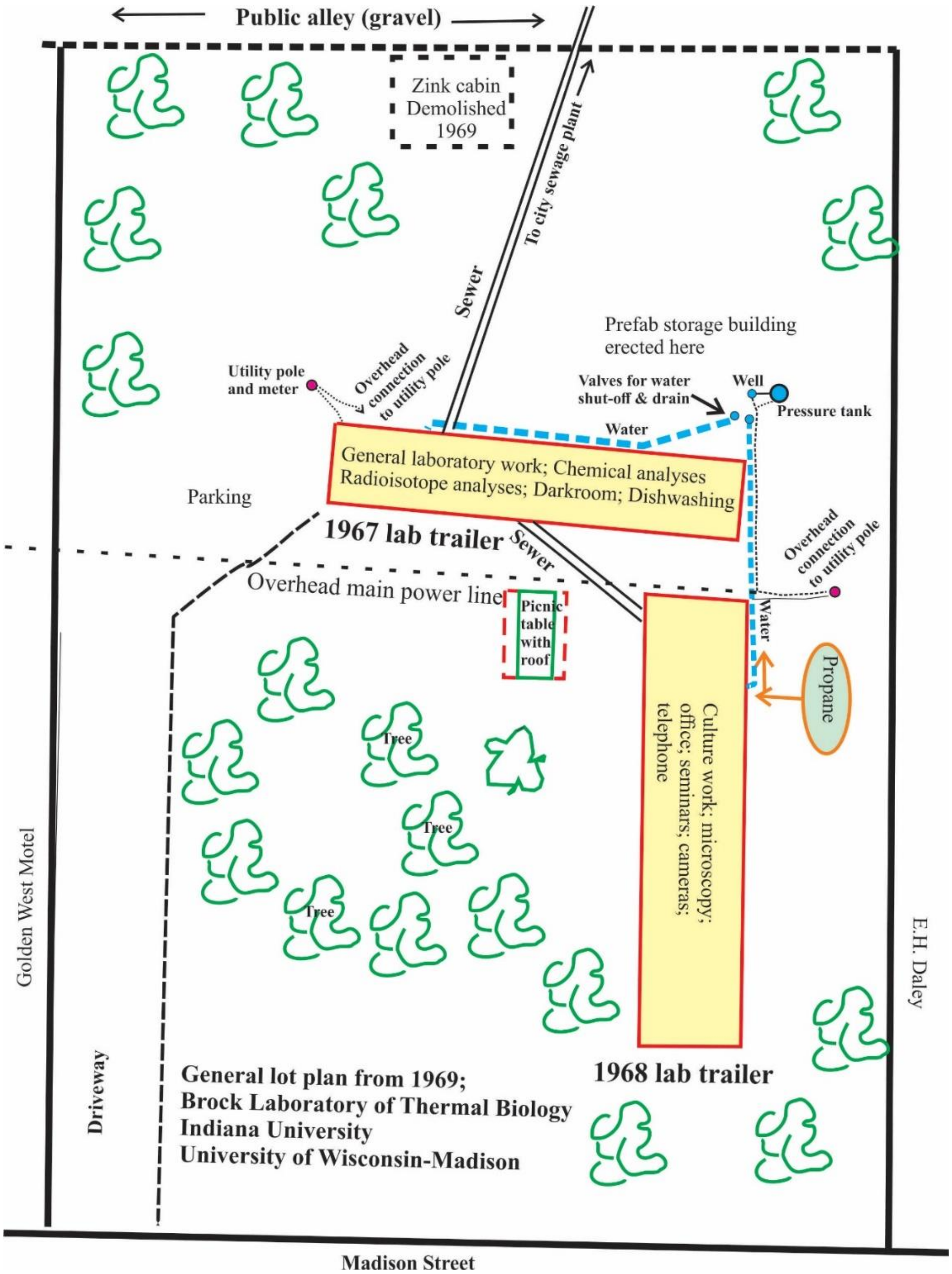
# Floor plan of the large lab

*The floor plan of the larger (front) house trailer.*

***Big trailer floor plan 2.jpg***



# Overall layout of the laboratories




*2016 graphic by T.D. Brock based on original sketch in 1969 by contractor. The original also shows that the former water supply line was still available for emergency.*

***IU plot overall no scans screens.jpg***

A local newspaper, the Yellowstone Sentinel, for July 10 1969, had an extended article on our work in Park. There is a photo showing me with tie and lab coat and Louise with lab coat. Lots of details, including who was in the lab that summer: Tom Bott, Gary Darland, Bill and Nancy Doemel, Maxine Cohen.

## Cleaning up Mushroom Spring

As noted earlier, Mushroom Spring had stopped flowing in the early winter of 1969 and was no longer a suitable research site. Through the years since 1965 we had been doing intensive work on this spring, and had set up permanent numbered research stations, as well as wooden channels that guided the flow of water. With the spring now defunct, we removed all the material and brought the area back to its natural condition.

# Artificial channel used in our study of Mushroom Spring

*We installed wooden channels in Mushroom Spring and a few other locations to create reproducible microbial mats that could be sampled precisely. These channels remained in place for several years. The photo here was taken in February 6, 1967, at a time when nearby there was about 6 feet of snow on the ground.*

***PICT0173.jpg***

# Cleaning Mushroom Spring

*Removing all the installed wood etc. from Mushroom Spring, July 22, 1969. My goal was to return this spring to its natural condition, with no evidence that it had been tampered with. From L to R. Gary Darland, Tom Bott, Bill Doemel, and Louise. The view is looking west across the Great Fountain Geyser outflow, with the Firehole Lake Loop Road at the far edge of the marsh.*

***PICT0149.jpg***

This was Tom Bott's second summer in Yellowstone. He spent most of the time trying to get radioisotope incorporation into organisms from Boulder Spring. Near the end of summer he discovered that the organisms needed sulfide for activity. Because he was leaving in mid-August for a job at the Stroud Water Research Laboratory in Pennsylvania (connected with the Academy of Natural Sciences in Philadelphia), Louise took over the Boulder Spring project.

Darland and I worked together studying the distribution at high temperatures of bacteria in hot springs of various pH value (published in *Science*). Also, Darland isolated a new species of spore-forming *Bacillus (B. acidocaldarius;* later discovered to be a new genus, *Alicyclobacillus).* Interestingly, food microbiologists discovered that Darland's organism was implicated in the spoilage of apple or other acidic fruit juices. The canning industry had always assumed that bacterial spores would not germinate at pH below 4.6 and that acid-tolerant organisms are not very heat resistant and would be killed by pasteurization. However, living spores of *Bacillus (Alicyclobacillus) acidocaldarius* may be present in fruit juice, survive pasteurization, and germinate and cause spoilage. (Information obtained from Wikipedia) This is a modest but interesting example of a practical application of our Yellowstone work.

Louise began the summer working on the hot spring insects, sorting out flies living in acid and alkaline springs. The insect taxonomist at the U.S. National Museum had told us that the ephydrid flies in the acid and alkaline habitats were the same species, but I did not believe this, since the larvae were so different. We insisted that he take another look at the adults, and he finally found enough difference to warrant establishing another species. Thus, *Ephydra bruesii* in the alkaline springs and *E. thermophile* in the acid ones. The latter is a Yellowstone endemic.

Louise was also occupied during the month of July 1969 with helping visitor Mercedes Edwards on electron microscope studies of thermophiles as well as general lab organization now that the two trailers were in their final positions. In early August, Louise began working on the Boulder Spring project, following Tom Bott's sulfide lead, and this occupied her for the rest of the summer, and on though the summer of 1970. Barbara Rice, a graduate student from Utah State, spent the summer with us, working on *Mimulus* (monkey flowers) that live along the edges of thermal creeks. She and Louise became friends, sometimes hiking together. I helped them on their hikes by running car shuttles.

I spent the summer on culture studies, trying to isolate flexibacteria (later discovered to be the phototroph *Chloroflexus*) from the cyanobacterial mats (partly successful) and culturing Boulder Spring organisms (unsuccessful). I also studied cyanobacteria of more acid springs. Near the end of the summer, after everyone but Louise had left, Bill Doemel and I made a fast (one week) but profitable trip to Hunters Hot Springs (Oregon), Lassen, The Geysers, Pacific Grove (where we talked with the eminent microbiologist Cornelius van Niel), Yosemite, Mono Lake, Steamboat Springs and Beowawe Nevada, and Great Salt Lake.

Also this summer, we had several visitors. In June I spent two days with noted Dutch microbiologist Hans Veldkamp. And in August Bob Burris, W.D.P. Stewart, and associates from U.W. Madison visited for one week and did nitrogen fixation (acetylene reduction) studies. They had their own self-contained mobile lab in a small house trailer, and parked it on the back end of the IU lot.

Other visitors we had in 1969 were:

- John Shepherd, Colorado State University, Fort Collins
- Ivan Palmblad, Botany, Utah State University
- Barbara Rice, Botany, Utah State University
- Lee Miller, Colorado State University
- Gordon McFeters, microbiologist, Montana State University
- Heinrich Walter, Stuttgart, Germany
- John Aronson, biochemist, State University of New York, Albany
- Derek Hoare, microbiologist, University of Texas, Austin
- W.D.P. Stewart, microbiologist, University of Dundee, Scotland
- Tim Mague, biochemist, University of Wisconsin-Madison
- Edward J. Rykiel, Jr. ecosystem ecologist, University of Georgia
- Robert Linn, U.S. National Park Service, Washington, D.C.

We closed the lab on Sept 6, 1969. Doemel drove the Ford van back and Louise and I returned in our personal Ford wagon via Thermopolis, Casper, Nebraska, Iowa, and Illinois, having tire trouble, wheel trouble, rear window trouble, but eventually arriving home at 4:00 a.m. a few days later.

John Bauld arrived in the lab the fall of 1969. We took our first trip to the New Hope gob piles and discovered a self-heating gob pile, the later source of *Thermoplasma*..

# Dutch microbiologist Hans Veldkamp on a short visit

*Hans Veldkamp, a distinguish Dutch microbiologist and a close friend of Cornelius van Niel, paid us a visit in the summer of 1969. Here he is with Marge Louden, Louise, and Korby, on the edge of Mammoth Hot Springs. Van Niel gave Veldkamp his professional papers, including the lecture notes for his famous summer course at Pacific Grove Marine Laboratory and I later spent time in Veldkamp's laboratory studying these notes. June 29, 1969.*

***PICT0144.jpg***

# Wisconsin researchers studying nitrogen fixation

*Distinguished Wisconsin biochemist Bob Burris visited our lab in the summer of 1969. Here he is with Scottish phycologist W.D.P. Stewart, measuring nitrogen fixation in cyanobacteria with the acetylene reduction technique. They had their own small mobile lab (in a house trailer), and parked it next to our lab for a week. They found that only the cyanobacteria that lived at temperatures below 50 C fixed nitrogen. August 19, 1969.*

***PICT0200.jpg***

# Turning a hot spring into a research area!

*Dick Wiegert's experimental channels, set up to study growth and behavior of hot spring insects. Wiegert was a pioneer at computer modelling of ecological processes. He found this good-flowing spring in a somewhat hidden meadow off the Firehole Lake Loop Road. With YNP permission, he engineered the whole spring, capturing the water and having it flow across these wide boards. The microbes grew very well and formed thick mats at temperatures below 50 C. Here he is discussing his research with a group of visitors. August 19, 1969.*

***PICT0146.jpg***

## Notes on the West Yellowstone lab

Electricity came from Idaho via Fall River Rural Electric Cooperative, Inc. Because of the long distance, and the wooded area that the wires came through, we occasionally had outages due to blowdowns. In the late 1960s there was a television repeater tower on Lion Head, a few miles from West Yellowstone. (There was no cable in those days.) The repeater picked up a single broadcast station from Idaho Falls. Although we had no television set, others did, and we were able to watch Neil Armstrong walk on the mood on July 21, 1969.

Louise was unpaid during all her time working at Yellowstone, but was able to claim expenses for travel (from my Federal grants).

The nearest city of any size was Bozeman, Montana, about 90 miles away. The drive was through part of the Park and then north along the Gallatin River. We went to Bozeman for major hardware, heavy grocery shopping, books, and "culture". Owenhouse Hardware was an important place to shop, especially for items needed to make the house trailers useful as labs. During our years working in Yellowstone, the drive along the Gallatin was still mostly undeveloped, but in the early 1970s the Big Sky Development came in and changed everything.

Montana State University in Bozeman generously allowed us library privileges, and we were able to open an account at their Chemical Stores. Also, by the end of the 1960s we were able to use MSU disposal facilities for low-level radioactive items, although we also stored a lot of radioactive waste to take back to Bloomington (or Madison).

Complete check lists of items taken to West Yellowstone are present inside several of the data books and copies are in the file folder labeled "Yellowstone lab early years". The list of items we brought from Bloomington is very large. The items changed somewhat as the years went by, but most of the items we brought in the first years remained on the list for the rest of the project.

A major piece of equipment that we brought every summer after 1966 was a Mettler Analytical Balance. The sales rep of the Mettler company showed Sally how to pack it, and we followed those instructions each year. This balance was essential because we were making lots of culture media as well as preparing reagents for various assays.

Some general-purpose items were left in West Yellowstone all winter. However, to avoid freezing, they were stored in the basement of Daley's house.

Finding suitable field research sites was primarily my job, and I wrote detailed instructions on how to find sites so that those needing them could find their way on their own. There is a handwritten set of instructions in the "Yellowstone lab early years" folder for finding springs (and their effluents) that we worked on in the late 1960s. Most of these springs had official names, but some were names we gave them. The areas listed are: Terrace Spring, Beryl Spring, Monument Geyser Basin, Artist's Paint Pots, Geyser Spring Group, Sylvan Springs area, Chocolate Pots, Norris Geyser Basin, Norris Annex,

Nymph Creek, Nymph Lake, Bijah Spring, South and North Twin Lakes, Roaring Mountain, Obsidian Creek, Amphitheater Springs, Ojo Caliente, Pocket Basin Area, Boulder Springs, Sentinel Meadows, Fairy Springs Group, Imperial Geyser area, Firehole Lake Loop Road, Firehole Pool, Twin Butte Vista, Geyserino, Pool A, Stepbrother, Shelf Spring and Bead Geyser, Zomar Spring, Sulfide Springs.

**Office, Transportation, Lab Functions 1969-1970**

Neither computers nor internet service existed. We used IBM electric typewriters. I brought out lots of stationary, etc., and carried on lots of correspondence. I used my West Yellowstone P.O. box as the return address. I generally had a secretary at West Yellowstone to do my typing. Nancy Doemel did that in the late 1960s. Bonnie Bauld was there in the summer of 1971. Carol Bohlool did my typing in 1973. Charlene Knaack typed in 1974-1975, in addition to working as my technician and taking care of general problems in the lab. In addition, I had a full or half-time secretary in Bloomington and/or Madison who took care of big typing jobs, such as research papers.

For calculating data we used slide rules. These once essential and now ancient devices were used for multiplication, division, squares, and square roots.

Our post office box was Thomas D. Brock, Box 562, West Yellowstone, Montana 59758. Starting in 1969 the students had a separate P.O. box for their mail.

**Post cards**. In those days, I worked a lot from reprints, and I both wrote for and sent reprints. Once the Yellowstone work was going well, I had a large number of postcards printed with Louise's photo of Vee Spring with Great Fountain Geyser erupting that I used for reprint requests.

**Transportation**. One or two university station wagons were used for transport from Bloomington (or Madison) to West Yellowstone. These were paid for by research grants. There are lots of P.O.s for these, which provide documentation of travel. Gasoline and service were covered by University credit card. Unfortunately, at IU the only card available was Standard Oil of Indiana (Amoco). In the West, it was hard to find this brand, so one had to drive very carefully, and fill up whenever a station appeared. Once I ran out of gas on a lonely stretch of highway in the middle of Idaho. Fortunately, I had some spare gasoline with me.

**Shipping.** Equipment often had to be shipped to West Yellowstone. Air freight was available, and there was a "curio" store in West Yellowstone that had the air freight agency. We usually knew when something was coming and informed the agent to look for a package for us. This sometimes included radioactive phosphate, since this had a fairly short half-life. The agent did not seem to mind handling radioactive materials (of course they arrived well packaged).

**Counting radioactive samples.** In our first full field year in 1966, we purchased a Nuclear-Chicago gas flow counter, which came with a tank of Q gas. (The scaler that was used with the counter was an Indiana University instrument that no one had been using.)

A lead weight was also involved. This was ordered to be delivered to a motel. The equipment arrived all right, but the lead weight was incorrect. After a long-distance call, they sent the correct one by air freight. When we ran out of Q gas, we had the vendor ship a new tank via Railway Express in Bozeman, with delivery service to West Yellowstone by Karst Stage. We returned the empty cylinder the same way. (FedEx did not exist.)

**Entrance to the Park**. Because we were driving state-owned vehicles, the Park Service issued us employee permits. A sticker went along with this that was fastened to the side door. This made it possible for us to bypass the frequent long lines of vehicles waiting to get into the Park. After 1970 each student/associate carried a collecting permit.

**Telephone**. Once we had our lab, we were able to have a telephone installed. This was Mountain States Telephone, from Bozeman. The phone was in the larger (front) trailer. The rental for the three months in 1967 was $32.00! The phone number was 406-646-9379.

**Shopping**. Although Bozeman was the best location for most of our needs, Idaho was actually closer. The nearest city was Ashton, Idaho, which was 55 miles away. A string of small cities went south from there to Idaho Falls, which had an airport with passenger service. When Louise and Ellen Daley went shopping for a house trailer, they went to St. Anthony, ID, where there was a dealer. I was able to get a P.O. through Indiana University in a few days, and Louise was able to buy the trailer and have it delivered before she left West Yellowstone at the end of September.

**Major equipment in the lab**

By the early 1970s we had learned what major equipment we really needed to have available.

Nuclear-Chicago Gas flow counter

Beckman beta-mate Scintillation counter

Beckman DB-G spectrophotometer

Carl Zeiss Universal microscope with trinocular eyepiece and epifluorescence

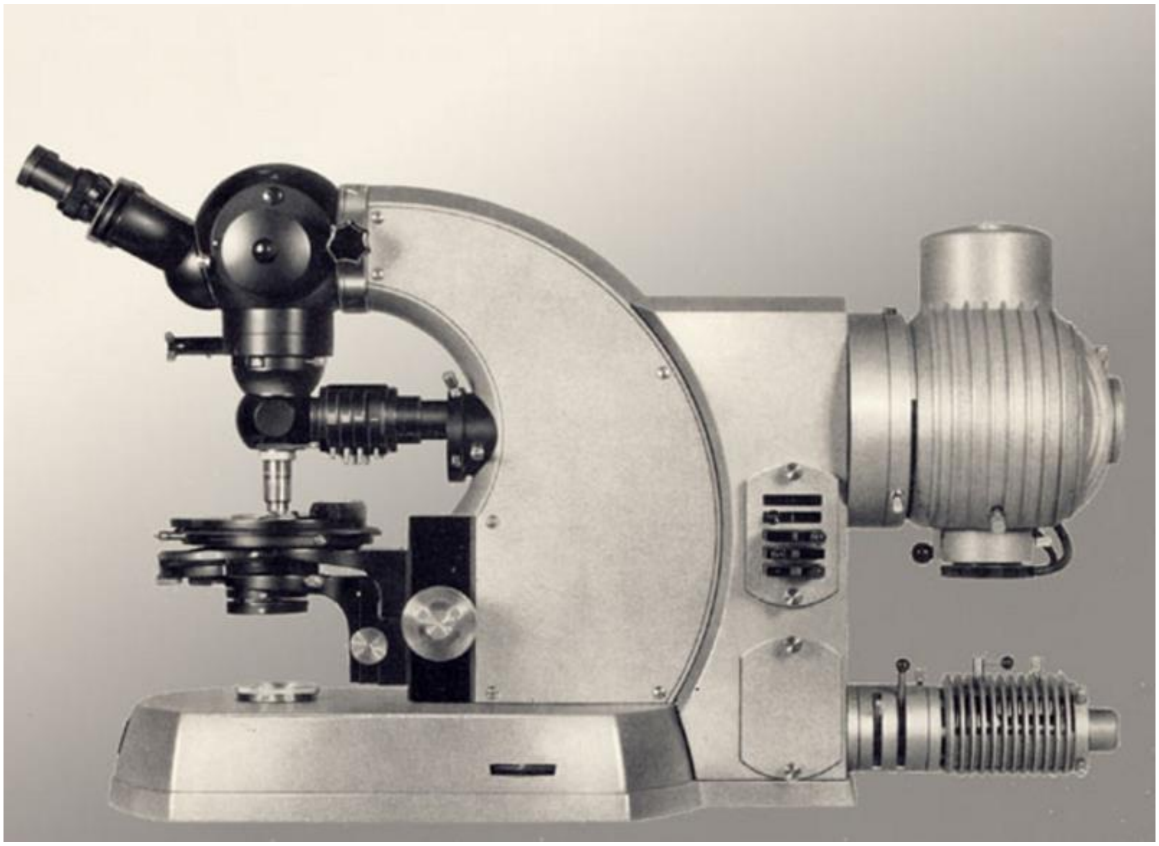
Carl Zeiss dissecting microscope

Branson sonifier

Mettler single pan analytical balance

Complete darkroom equipment for developing and enlarging B/W film. Durst enlarger.

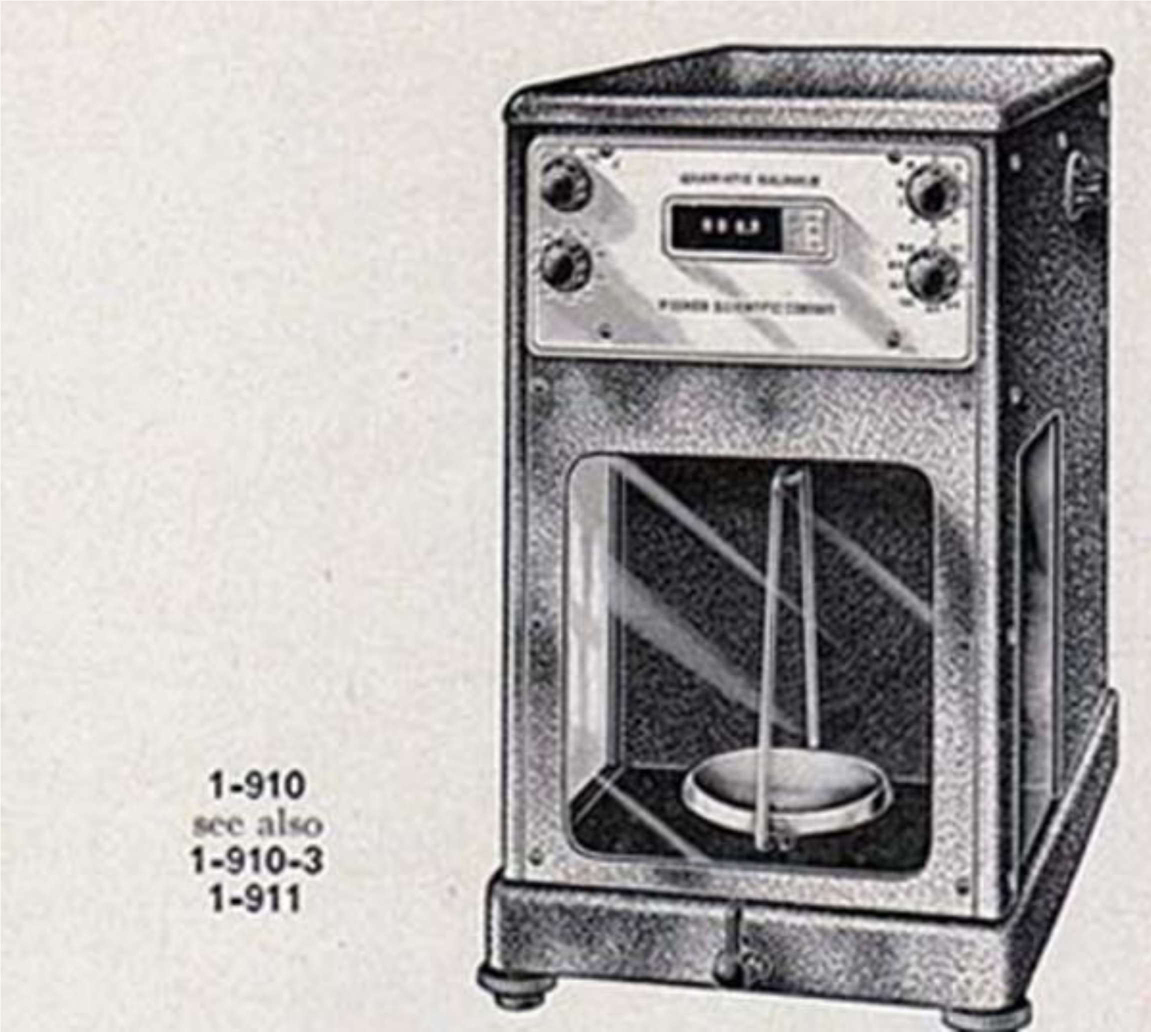
# The Zeiss microscope



*Carl Zeiss Universal microscope with light and epifluorescence systems, as the equipment looked in the late 1960s. This photo is from a screenshot off the web. The photo is of a set-up similar to ours, except ours had a trinocular head (binocular for viewing and a third eyepiece for the camera) so that photomicrographs could be taken. We had a complete set of objectives, eyepieces, and condensers. Epifluorescence observations were either chlorophyll (which fluoresces red) or acridine orange staining (RNA fluorescing red and DNA green). The upper light source (used for epifluorescence) was a high-intensity (100 watt) mercury vapor lamp. This set-up with a 100 X water immersion lens was used for examining slides that had been immersed in boiling springs. For counting, we placed a grid into one of the eyepieces or used a Petroff-Hausser counting chamber. For quick photomicrographs we used a Polaroid camera but generally we used a Topcon 35 mm camera, often with Panatonic-X film, developing the film in our darkroom and using our Durst enlarger to make 5 X 7 or 8 X 10 prints.*

***Zeiss Universal with epifluorescence from screenshot.jpg***

# Mettler analytical balance, an important piece of equipmentt




*The Mettler single-pan balance revolutionized chemistry and biology after World War II. No laboratory could be without one. The Mettler salesperson gave Sally detailed instructions on how to pack one for transport. Students of mine who did not go to Yellowstone were able to use a balance in another lab.*

***Mettler gram-atic balance screenshot.jpg***

# Before computers: the slide rule


*The slide rule was once an essential tool. Multiplication, division, squares, and square roots were all calculated with this simple device. Only approximate answers could be obtained, but these were generally good enough. Also, you learned quickly how to set the decimal point for answers. Imagine multiplying 2.67 X 134.5 on this device. Believe it or not, we did it! About 1970 we acquired our first electronic calculator (a Monroe) that would do square roots. This was a major advance, since we did lots of square roots for our radioactive counting work. A major advance was the HP-35 scientific calculator, which was introduced in 1972 and sold for $395! (I bought one, of course!)*

***Slide-rules-2017-4566.jpg***

## Rules for use of the lab and for doing field work in the Park

When there was just Louise and me, we did not need any rules, but once we had a permanent lab and other people, we needed to be sure that things operated correctly. This lab was not like a university lab, where order occurred because of years of tradition or because janitors and other service employees existed. Everyone had to do his or her own clean-up. We had large trash cans outside and hired a garbage and trash service to empty them on a weekly basis. But who emptied the inside trash cans into the big ones? Some people would never empty the cans, but others would. Sometimes the most brilliant people were the least helpful!

There is a two-page typed document that I created in 1969 which lays out rules for the lab, and also for working in the Park. This gives some insight into the mechanics of the lab. For instance, there was a Royal typewriter (probably not electric) and a Monroe Calculator. There were books and reprints that could be read or checked out. We worried about dust and kept the windows closed. (Many of the roads in West Yellowstone were unpaved in those days.)

We were especially concerned about behavior inside the Park, as the success of our project depended upon having access to thermal features. We MUST follow all NPS rules.

# Rules for working in the Park

We are working in the Park by the courtesy of the National Park Service. We are forbidden to collect or work in public use areas after 7:00 A.M. Try to be courteous to Park personnel and tourists. If asked what you are doing, explain that you are doing research on the biology of hot springs with a group from Indiana University. Do not volunteer excessive information, mention the use of radioisotopes, or indicate which springs you are working on (except to Park Rangers and Naturalists, and then only if asked). Do not give glowing accounts of interesting thermal areas off the road; these could easily become tourist areas someday and our use of them would be restricted. Try to park your car at a parking area or pull-off; if it must be parked along the road, choose an inconspicuous spot. If you plan to sleep all night in the Park, be sure to report this to the ranger station nearest the site you plan to park.

*Once we had two lab trailers, I was not interacting with everyone as frequently, and had to create these rules.*

***Rules for working in the Park 1969.jpg***

# The Zink cabin


*This cabin was on the lot we purchased. This photo was taken July 13, 1969, just before it was demolished. I know nothing about its history but assume that at an earlier time there may have been several cabins on our lot. At the time we were working, West Yellowstone was going through a transition from rustic facilities to plush ones. There were few national motel chains. Now (2017) there are very few facilities that aren't national chains. We were fortunate to be there when we were, because not only were land prices low, but there were lots of small cabins that our students could rent at reasonable cost.*

***PICT0150.jpg***

## The Joy of Field Work in Yellowstone

Yellowstone was a fascinating place to do field work, with a huge number of thermal features, each unique in its own way. I loved tromping through the back reaches of Yellowstone and stopping at a thermal feature and watching it do its “thing”. Or looking at it as a potential research site. I also loved spotting a column of steam off in the distance, and figuring out how to reach it, taking care not to get my feet wet (or burned).

At first I was oblivious to the possible danger of falling in, but it wasn’t long before I realized that many thermal features were potential killers. Safety had to be the #1 concern. Fortunately, we never had any serious accidents. We often walked through thermal areas, keeping free of steam vents or bubbling springs. We soon learned that green plants did not grow where the temperature was hot, so stepping on a plant was generally a safe thing to do. Charlene Knaack remembers (note sent September 2016) walking in my footsteps, reasoning that if I had not broken through, she would be safe. (In those days I was always in the lead!)

Acid thermal areas were more dangerous than alkaline ones. This is because the high acidity of the soil, together with the high temperature, resulted in extensive breakdown of the underground bedrock. A semi-hard crust might cover an area of hollow hot ground. One had to be careful not to step on fragile ground. Twice (once in New Zealand and once in Iceland) I broke through the surface into underground hot mud. I felt the ground giving way and the heat. My boot came up with a heavy coating of hot red mud, but I was not burned. However, during a NASA-sponsored trip to Iceland, two scientists not cognizant of the potential danger did get seriously scalded at Krisuvik, a very acid thermal area. They ended up on crutches for the rest of the two-week trip.

Working in Yellowstone National Park was at times maddening because of Park Service regulations. The 7 AM rule was the most difficult to deal with. My first collecting permit, issued April 15 1965, stated: “Collecting in or near public use areas to be done prior to 7:00 a.m.” When I read that I queried John Good, the Chief Naturalist, as to how “public use area” was defined, as my impression was that all of Yellowstone National Park was “public use.” Good responded: “I don’t think you will have any trouble doing your collecting. In the geyser basins there are many pools which are a few hundred feet away from the roads and boardwalks where you can do whatever is necessary….The purpose of our restrictions is to keep Park visitors from seeing your activities and concluding that they can do the same things you do.”

With John Good’s letter as my guide, I decided that any place we studied must not be visible from the road. In fact, even a small hill blocking the view would be enough. This is why, in 1966, my first summer of extensive work, I worked on two springs that were ideal, Octopus and Mushroom. Both were only a short walk from a parking area, were not visible to the public, and had very interesting outflow channels.

The parking lot for Octopus and Mushroom was right across from Great Fountain Geyser, a large geyser that erupts about once or twice a day. The Old Faithful Visitor Center often chalked up the approximate time for the next eruption, which brought a lot of cars to the

parking lot. Depending on our schedule, we might find the parking lot full, which was annoying. Other times we were ahead of the crowd. Since we often left a vehicle at the site all day, we were often there when Great Fountain Geyser erupted. In fact, people in our crew often knew the vagaries of Great Fountain Geyser better than the Park Service rangers. "No, it's not going to erupt for another hour. It won't erupt until hot water is flowing all the way over the edges of the sinter."

The photo of "Vee Spring" with Great Fountain Geyser erupting in the background, taken by Louise, is a classic.

I had some favorite areas that I loved to hike to.

- Sentinel Meadow (Boulder Spring, Steep Cone, and Queen's Laundry)
- White Creek valley (Buffalo Pool, Three Sisters, Stepbrother, the hot waterfall). There isn't a single stream entering the White Creek valley that isn't thermal!
- Hundred-spring plain (Norris Geyser Basin)
- Roaring Mountain (South effluent)
- Nymph Creek (great *Cyanidium* mats, very easy access)
- Amphitheatre Springs (many acid, thermal springs and seeps)
- Heart Lake Geyser Basin (a long walk, but worth it)
- Shoshone Geyser Basin (since the road to Lone Star Geyser closed, best to reach by canoe or kayak)
- Sylvan Springs (the greatest!)

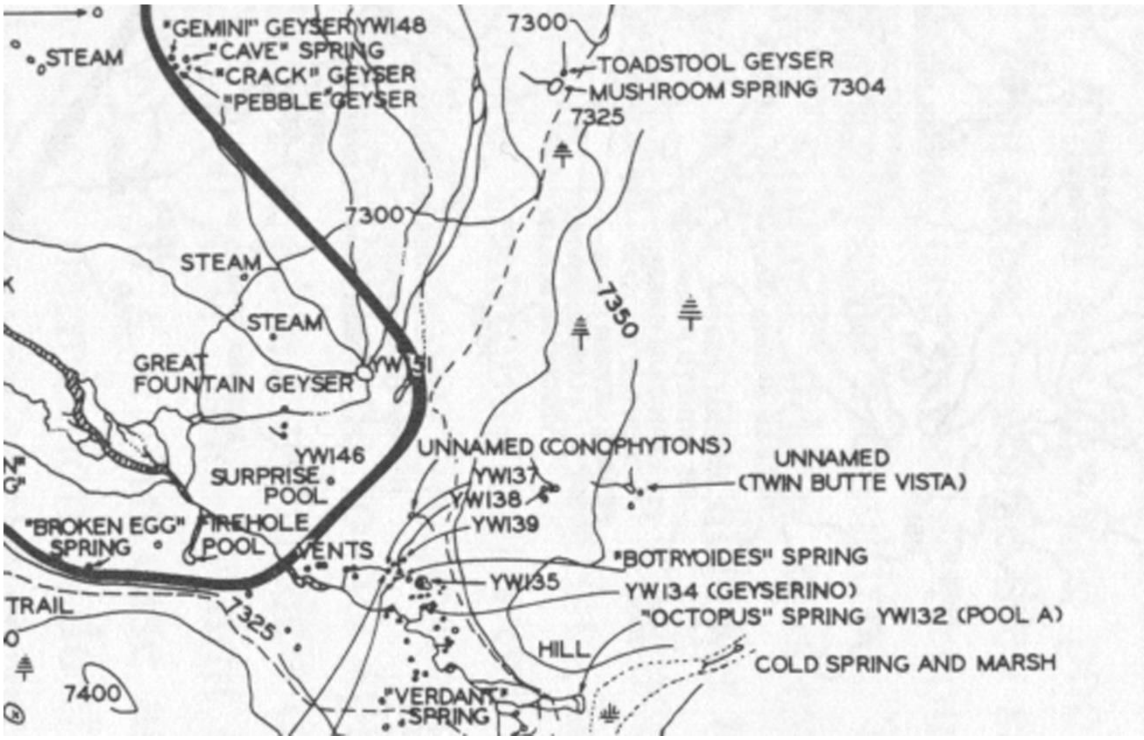
Many more thermal areas appeared in our notes in the 1970s.

# Vee Spring and Great Fountain Geyser

*Vee Spring on September 24, 1967, with Great Fountain Geyser erupting in the background. This photo was used in my paper featured in* Science.

***Vee Spring Louise photo 9-24-1967 BW356.jpg***

# Map of the Great Fountain Geyser area




*Enlarged map of the Great Fountain/White Creek area showing the locations of Mushroom and Octopus Springs. Map drawn by Charlene Knaack from an original USGS map as published in my book:* Thermophlic Microorganisms and Life at High Temperatures.

***Great Fountain scan 2 Mushroom Octopus.jpg***

When I first started working in Yellowstone the White Creek valley was infested with grizzly bears, because a garbage dump was located in Rabbit Creek, the next valley to the south. (Closed in 1970). In 1968 I was doing what is called a “diurnal” measurement of photosynthesis using radioactive carbon, which meant I had to be on site for the whole 24 hours. Thinking to get ahead of the game, I set up a tent near Mushroom Spring that I was going to use to store my equipment the next day. Then I slept in my station wagon all night at the Great Fountain Geyser parking lot. There seemed to be quite a bit of activity at one point during the night. When I got up at 6 AM for my first readings, the tent had been torn down. Grizzly?

In the 1970s, after we discovered *Thermoplasma* and *Sulfolobus*, we did quite a bit of work in acid thermal areas. One site that was especially good for low pH habitats was the southern effluent of Roaring Mountain, where a tiny hill blocked the view from the road, even though the northern part of RM was in full view of the public. We filmed part of the movie we made for Encyclopedia Britannica in this area. We spent most of a day with the film crew there without the public being the wiser. One time, Bill Doemel and I accidentally strayed too far north during a visit. Suddenly, I realized that dozens of people were streaming up from the parking lot: “What are you seeing?” Animals, of course, is what they meant.

After a few years you began to understand the psychology of tourists!

There were occasions when a thermal feature was so interesting that we **had** to visit it, even if it was in full view of the public. This was the case with Sulfur Caldron, a major feature in the Mud Volcano area, that is just below a major parking lot and viewpoint. It was a 1.5 hour drive from West Yellowstone to Sulfur Caldron. There was a big crew involved in this particular study, and the work was going to take at least an hour. We got everything ready the night before, got up a 4 AM, and drove at “warp speed” to the site, eating breakfast on the way. We got there at 6 AM and started work immediately. Fortunately, visitation was light at that time of morning, so everything worked out all right.

It was interesting that Yellowstone National Park issued what they called a “Collecting Permit”, which implied that you were going to collect a “sample” and then leave. To me, a thermal feature was not just for sampling, but for research. We did numerous experiments where we were often at a site the whole day.

We probably worked more at Octopus Spring than at any other site. Originally, we did not know it had a name (very few springs in Yellowstone have signs next to them), and called it Pool A (we also had Pool B, C, and D). It was only after we had done years of work on this spring, and published papers, that I discovered it was called Octopus Spring. In fact, it was one of the springs studied in the original Hayden Survey of Yellowstone. The name was appropriate because the large round pool had two separate outlets that looked like the arms of an octopus. (So many people have studied Octopus Spring through the years since our work that it is now labeled on the Google Earth Map of Yellowstone and you can search for it.)

Some sites took a half hour or more to reach from the parking area but were so interesting that it was worth it. This was the case with Sylvan Springs, a large area of acid hot springs, fumaroles, and hot ground, that was in the Gibbon River area. Even though this area was in complete view of the main Madison/Norris road, it was so far away that no one looked at it. (The meadow east of it was often full of elk.) For some studies we lugged lots of equipment to that site. We parked at the Gibbon River Picnic Area, and crossed Gibbon River on a large log (an intimidating experience for some), carrying all our equipment. Then we walked along the edge of the large Gibbon River meadow, avoiding the wet areas, until we reached the lower end of Sylvan Springs. Once there, we could spend the day and no one was the wiser.

# Sylvan Springs, one of my favorite research sites

*Sylvan Springs general view, looking toward Gibbon Meadows. The Grand Loop Road is on the other side of the Meadow. This was an ideal place to work on the biogeochemistry of hot acid springs. There were dozens of small and large springs, mostly pH 2-3. Photo July 31, 1974.*

***PICT0208.jpg***

# Yellowstone Stromatolites

The term "stromatolite" is generally used to refer to paleobiological structures that are found primarily in ancient (Precambrian; over 1-3 billion year old) rocks. The term usually refers to laminated structures that have characteristic shapes that have been interpreted as phototropic growth structures. Some of these ancient stromatolites have microscopic fossils resembling modern-day cyanobacteria. Because of their ancient lineages, stromatolites are used as evidence for the origin of photosynthetic (oxygen-evolving) biochemistries. Modern equivalents of stromatolites have been found in some specialized marine environments, as well as in geothermal habitats such as Yellowstone.

The first report of stromatolitic structures in Yellowstone was made by Walter H. Weed in the late 19$^{th}$ century, and numerous paleogeologists have examined these structures since.

Since my 1967 *Science* paper, I had been interested in Yellowstone stromatolites, and had encouraged geologists to study them. I was especially fortunate that Malcolm Walter, a young and very active Australian paleobiologist, was at Yale on a two-year fellowship and contacted me about a visit to our West Yellowstone laboratory.

Malcolm came first in 1971 and returned in 1972. Also in 1972, the distinguished paleobiologist Preston Cloud (U.C. Santa Barbara), came for a week's visit. This was at the time when John Bauld, a graduate student in my laboratory (who incidentally was Australian), was working on the *Chloroflexis/Synechococcus* mats in the effluents of alkaline hot springs. In these mats the laminations are created not by oxygen-producing cyanobacteria, but by *Chloroflexus*, which is a phototrophic and phototropic (moving toward the light) bacterium that does not evolve oxygen. (Don't confuse "troph", which refers to nutrition and "trop", which refers to movement.)

It seemed to me (and others) that if stromatolitic structures could be made by organisms that did not evolve oxygen, this would possibly suggest a different interpretation of some of the biogeochemistry of the Precambrian earth.

During the period 1971-1975, I encouraged work at Yellowstone on stromatolites. Bill Doemel, who had returned to work with us as a post-doctorate in the summers of 1970 and 1973-1975, did some major work on the *Chloroflexus* mats, leading to several important papers. Bill brought along with him several undergraduates from Wabash College who were especially helpful, most importantly Don Weller.

Eventually, we published quite a few papers on Yellowstone stromatlites, including two review papers in a book that Malcolm Walter edited for Elsevier Publishers.

# Paleogeologists examining habitats for Yellowstone stromatolites

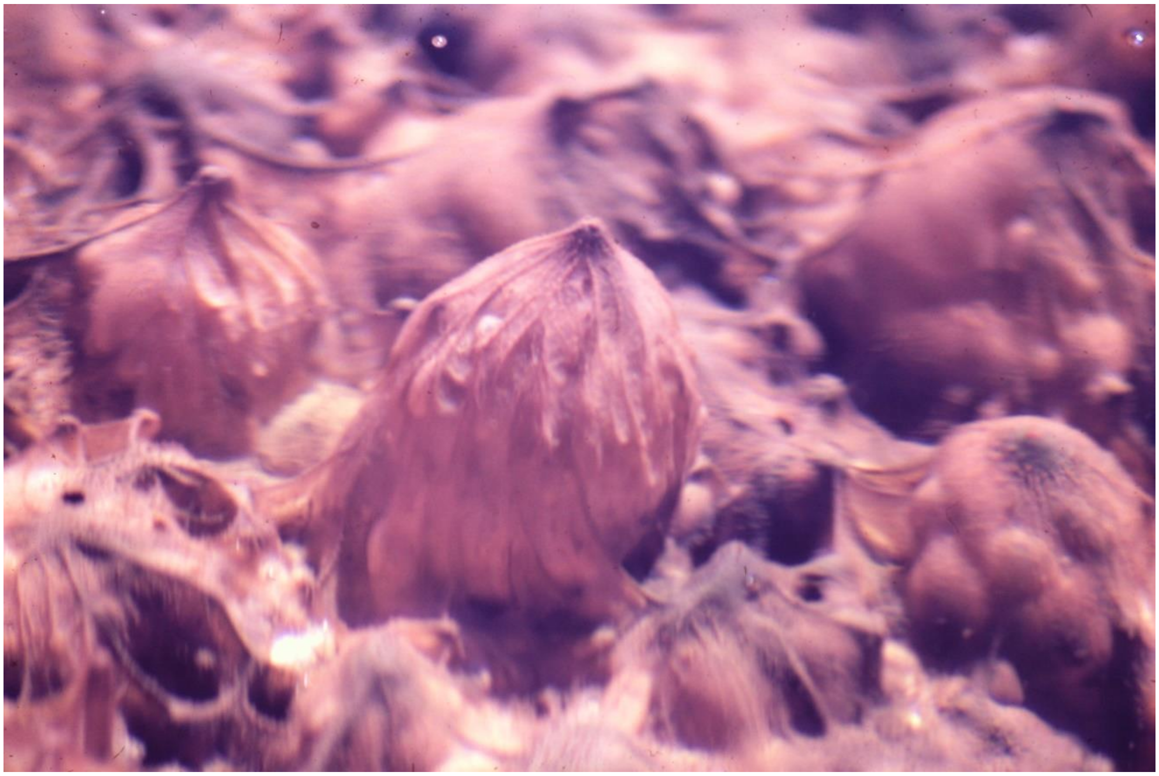
*Noted geologist Preston Cloud and Australian paleogeologist Malcolm Walter surveying an area of stromatolitic structures in the Fairy Meadow Area. Walter had been studying these structures in what he called Conophyton Pool for several years. Photo 20 July 1972.*
***IMG0274.jpg***

# Stromatolite site: Large outflow of hot water from the Fountain Paint Pots area

*A stromatolitic-rich area in the Fountain Paint Pots area. The boardwalk is at the top end of this outflow. With Bonnie as "explainer" to tourists, I walked off the boardwalk and around the end of this outflow in order to obtain this photo. This spring was probably studied by Walter Weed in the 19th century. Photo August 7, 1972*

***IMG0276.jpg***

# Close-up of Yellowstone stromatolites



*Close up photograph of living stromatolites in a pool at the lower end of the Fountain Paint Pots effluent. These structures are about 1.5-2 inches high. They are made by Phormidium, a cyanobacterium. Similar structures are occasionally seen as fossils in Precambrian rocks. Photo August 7, 1972.*

***IMG0277.jpg***

# Dissecting Yellowstone bacterial stromatolites

*Studying the origin of laminations in the stromatolitic structures formed by Chloroflexus at Octopus Spring. Bill Doemel dissecting and Don Weller assisting. This is a good example of sophisticated field work in action! July 26, 1973.*

***Img0278.jpg***

# Doing field work in other parts of the world

My first funded project was in Iceland, and the Yellowstone work was done to work out methods I was to use in Iceland. But once I got into the Yellowstone work, it was clear that this was to be my primary focus, and thermal areas in other parts of the world would be visited only secondarily.

However, there were good reasons to visit other places. Because of the high altitude of Yellowstone, water boils at around 92.5 C. To get higher temperatures, one must visit thermal areas at lower altitudes, or in marine habitats. Certainly, Iceland qualifies here, but is difficult to study. I went to other places, and New Zealand was the most productive.

Also, there were interesting biogeographical questions that could only be considered by visiting other sites.

Other locations were mostly visited when I was travelling to distant areas for other reasons. This made sense both for time and money.

A summary is given on the accompanying table and photos and descriptions of areas I visited can be found at the end of this book.

# Thermal areas visited around the world.

| Location | Year | Notes |
|---|---|---|
| Beowaiwe, NV | 1969 | Isolated thermal sources and geysers in desert; later destroyed by geothermal drilling |
| Calistoga, Calif | 1968 | *Thermus aquaticus* culture |
| Dominica | 1971 | Greater Antilles, Caribbean; *Sulfolobus, Cyanidium* |
| El Salvador | 1970 | Ahuachapan (United Nations thermal energy project); *Sulfolobus* |
| Furnas Valley, San Miguel; Azores | 1966 | In the middle of the Atlantic Ocean; interesting biogeographically |
| Hunter Hot Springs, Oregon | 1969 | Castenholz research site; low-altitude boiling springs |
| Iceland | 1965 | Thermal gradient research; also Surtsey |
| Iceland | 1966 | Thermal gradient research; also Surtsey |
| Iceland | 1970 | Thermal gradient research; also Surtsey; NASA funded expedition |
| Iceland | 1972 | Kjerlingarfjoll (*Sulfolobus*); also Surtsey |
| Italy | 1966 | Ischia, Agnano, Solfatara |
| Italy | 1967 | Ischia, Agnano, Solfatara; under sea hot springs |
| Italy | 1968 | Ischia, Agnano, Solfatara |
| Italy | 1969 | Ischia, Agnano, Solfatara |
| Italy | 1970 | Naples research laboratory; Pisciarelli; *Sulfolobus* |
| Japan | 1970 | *Cyanidium*, thermal cyanobacteria; acid hot springs |
| Kilauwea, Hawaii | 1969 | In the middle of the Pacific Ocean; interesting biogeographically |
| Araro, Michoacan, Mexico | 1970 | Open area with numerous thermal features, including piped steam vents |
| New Zealand | 1969 | Extensive research work |
| New Zealand | 1970 | Extensive research work; travel partly paid by Cyanophyte symposium |
| Steamboat Springs, NV | 1965 | Convenient to reach from Reno; fumaroles; |
| Steamboat Springs, NV | 1969 | Geothermal drilling dried up all surface activity |
| The Geysers, Calif | 1965 | *Cyanidium* habitat |
| The Geysers, Calif | 1969 | *Cyanidium* habitat; geothermal power installation |
| The Geysers, Calif | 1971 | Large geothermal field, formerly natural, but drilling has eliminated surface activity |

| | | |
|---|---|---|
| The Geysers, Calif | 1972 | Large geothermal field, formerly natural, but drilling has eliminated surface activity |
| Yoghurt Hot Springs, Utah | 1972 | Warm carbonate-rich source |

# Yellowstone 1970

The summer of 1970 was a fateful one in many ways, mostly of which were not anticipated ahead of time.

There was no winter work in Yellowstone this year. Louise and I went to Hawaii, New Zealand, and Australia in Dec. 1969-January 1970. After Australia I flew to Madras, India for a symposium on Cyanophytes (now called cyanobacteria). After the meeting I returned to the U.S. via Japan, where I was guided to thermal areas by Yatsuka Saijo from Nagoya University and Kimio Noguchi from Tokyo Metropolitan University. I returned home in late January 1970, just in time to teach my spring course in microbial ecology.

The work on thermal areas in New Zealand was quite productive and this was one of the best field trips I had. In Japan I visited several thermal areas and realized the difficulty of fieldwork in Japan (especially the language barrier), but made some nice observations of *Cyanidium* at Kusatsu and of *Synechococcus* at Onikobe.

The following summer Saijo visited us at Yellowstone and in 1971 we had Noguchi and two associates for the whole summer.

# Limnologist Yatsuka Saijo at Norris Geyser Basin

*Visiting limnologist Yatsuka Saijo from Nagoya University in Japan, in the back reaches of the Norris Geyser Basin. The previous winter he had guided me to some thermal areas, and an acid lake, in Japan. The area shown here is part of the "100-spring plain" in a more remote area of Norris. Photo June 7, 1970.*

***PICT0156.jpg***

During the spring of 1970 in Bloomington we worked out sulfide and iron assays, did a lot of gob pile studies at the strip mines, and got ready for the summer at Yellowstone. I had discovered the Pachmayr thesis (in German) which had very useful assays for sulfur species. I translated the key parts for use by others to develop the same assays. Pachmayr; Ph.D. thesis, Ludwig-Maximilians University, Munich, 1960. The assay for sulfide is described in detail in the Boulder Spring paper.

See the discussion of Gary Darland's work on *Thermoplasma*, a major discovery.

In 1970 Doemel finished his thesis and took a faculty position at Wabash College. Gary Darland isolated *Thermoplasma*. Paul Ray finished his Ph.D., working on the biochemistry of *Thermus aquaticus* lipids. Greg Zeikus, working on the protein-synthesizing system of *T. aquaticus*, finished during the summer and came out to Yellowstone for post-doctoral work on the Firehole River in July-August. Carl Fliermans arrived from Kentucky as a new student and began gob pile studies in anticipation of studies at Yellowstone the summer of 1971. Dave Smith also came as a new student and Smith and Fliermans worked out a method for measuring $^{14}CO_2$ fixation in soil, a technique which we used extensively in Yellowstone during the early 1970s.

Jerry Mosser, who had been an honors undergraduate in my lab in the early 1960s, signed on as a summer post-doc after finishing his Ph.D. at Rockefeller University. He and Louise drove the Ford van (our mobile lab) out in late May, 1970, arriving sometime before May 27, to open the lab and get things underway. Frances (Frankie) Foy and I drove out in early June with my personal Ford Wagon. John Bauld and Jeffrey Davidson (undergraduate student) drove along behind us in the I.U. station wagon. We went via Abilene, Kansas; Rawlins, Wyoming; Togwotee Pass, and West Thumb, arriving in West Yellowstone after two nights on the road.

Frankie stayed in one of Mrs. Clark's cabins, and John and Jeffery rented one of Mrs. Morris's cabins. Jerry and Anne Mosser also had one of Mrs. Morris's cabins, although Jerry had to fly back to N.Y. in early June to graduate, and he and Anne then drove back to West Yellowstone in their VW "bug". Carl and Ruth Ann Fliermans arrived in early June, also renting one of Mrs. Morris's cabins. Bill and Nancy Doemel came out as soon as Nancy's school was out, and they lived in Daley's pink trailer.

For the second summer, Louise and I lived in the rear bedroom of the large lab trailer, although the rest of this trailer was used for research. Post-doc Mike Tansey arrived around August 1, eventually staying in one of Wadsworth's cabins, and post-doc Katherine Middleton also arrived around August 1, staying in the second bedroom of the pink trailer with Bill and Nancy. Greg and Shelly Zeikus arrived around July 6 and stayed in one of Wadsworth's cabins. Bill Samsonoff, one of Sam Conti's students, spent about two months. He brought most of his own materials (for electron microscopy) and stayed in a small trailer at Wadsworth's.

This summer we had the largest and one of the most complicated program of research. In addition, I was out of the country twice. Around June 12 I flew to Washington for an NSF panel meeting, then on to Iceland for the NASA- sponsored field trip to Iceland and

Surtsey. Bill Doemel also was on this trip, and hence was away the last two weeks of June also.

Returning from Iceland around July 1, I went to Bloomington, gave Greg Zeikus his final oral, and then came to Yellowstone. During my absence, a big windstorm had come up on June 27, and did a lot of damage, including completely destroying the roof of the I.U. station wagon. This put us short a research vehicle, and the mobile lab got a lot of use just transporting personnel and supplies.

In July, I arranged a trip for the whole lab to visit the copper mine and leaching dumps in Butte, Montana, a very interesting artificial acid habitat. This was a very enlightening trip, giving me real insights into an important practical use of acidophilic sulfur- and iron-oxidizing bacteria. In the late 1970s I visited quite a few other copper mining areas in Arizona and Utah.

The summer's work went reasonably well. Louise finished up the temperature optima measurements of bacteria living in Boulder Spring and Octopus Spring in admirable fashion. I isolated lots of *Sulfolobus* (then thought to be *Thermoplasma*). Jerry Mossser did a nice study on fluctuating springs (Bead Geyser) and Greg Zeikus's work on the Firehole River went well. Carl and John got their studies underway, although neither led to anything specific during their first summers.

We had a visit during the summer by Israeli microbiologist Moishe Shilo and his daughter, and I took them to see some of the "tourist" areas, as well as the microbial habitats.

Deborah Delmer worked on the thermal ecology of the snapdragon plant *Mimulus* summer 1970. This plant grows profusely along the edge of thermal streams and hence was of interest biochemically. She published a brief paper on her work.

Other visitors this summer were:

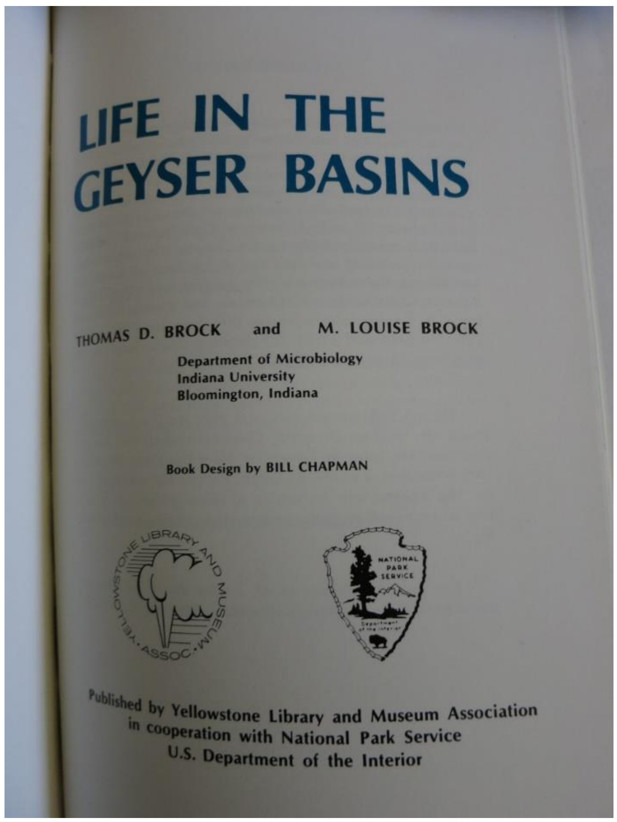
- Barbara Rice, Botany, Utah State University
- Judith Bland, Ph.D. student, Indiana University
- Marion Fields, canning bacteriologist, University of Missouri, Columbia
- Ron Fokman, Beckman Instruments, Seattle
- Rodger Mitchell, Zoology, Ohio State University
- Bill Samsonoff, electron microscopist, New York
- Patricia Morrow, Stanford University
- David Darby, geologist, University of Minnesota, Duluth

- Melvin Silverman, NASA Ames Research Laboratory, Moffet Field, CA
- Richard Castenholz and students, University of Oregon
- William Dunmire, Chief Naturalist, Yellowstone National Park
- Jay Summer, Wildlife Research Unit, University of Montana (Missoula)
- Robert Anderson, Civil Engineering, Montana State (Bozeman)

This summer Louise and I finished work on an interpretive booklet that the Park had asked us to write called “Life in the Geyser Basins”. This was published in 1971 and was very successful. It remained in print for some years, finally being superseded by “Life at High Temperatures”, published in 1994.

# Preface of the booklet for Yellowstone National Park

*"Our studies of the Yellowstone hot springs began in 1964 with very little more than a thermometer and a microscope. That was sufficient to infect us with the fascination of Yellowstone, with the willingness to transport research equipment halfway across the continent and back, with the desire to cope with the handicaps of winter visits, and with the eager anticipation of seeing how things will have changed since we were there last. Increased knowledge has not cured this infection. On the contrary, we continue to see the Yellowstone hot springs as sources of intriguing questions waiting to be asked and answered, as sources of beauty and enjoyment for both scientist and tourist."*




*This booklet was quite successful! It sold 10,000 copies in the summer of 1971 and was reprinted several times. It remained in print for some years, eventually being superceded by "Life at High Temperatures" in 1994.*

***Life Geyser Basins title page-4376.jpg***

Mike Tansey breezed in as a new post-doc about August 1, 1970 and got quite a bit started on thermophilic fungi. Kathie Middleton arrived and spent August becoming acquainted with a variety of projects in the lab, as she was hired partly to take over running the lab. She spent most of her time trying to get the Boulder Spring bacteria to grow—an abortive study.

I went to Mexico in early August for the International Microbiology Congress, then went on to El Salvador for a quick and fruitful survey of hot acid areas (new *Sulfolobus* strains), then back to Yellowstone, being gone one week.

I provided a list to West District Ranger of our people in the lab doing independent research work 1970: Mosser, Zeikus, Doemel, Fliermans, Louise

I also submitted, as requested, to Yellowstone National Park Superintendent Jack Anderson a summary of work done: 10 research papers published; 2 reviews; Life in the Geyser Basin booklet for Yellowstone; West Yellowstone lab expanded and improved. 5 post docs, 3 grad students, 2 undergrads, and 5 visiting scientists.

Jeffrey Davidson left in mid-August, and Bill Doemel a little later, but the others stayed until early September. We prepared to close the lab, but Louise then stayed a few weeks to finish the Boulder study. Tansey and Mosser also stayed on a while after the rest left. Frankie took the bus home, John drove the I.U. Ford van, Carl drove home alone in his car, (Ruth Ann having gone home earlier with relatives), I drove my Ford Wagon home and Kathie drove her Mustang. (She and I stopped in the Tetons to canoe the Snake River.)

Near the end of the summer the roof of the damaged I.U. wagon got repaired. Fortunately, West Yellowstone had a body shop that could repair it, but it took the mechanic most of the summer to find an undamaged top that he could buy from a salvage auto dealer.

Louise, staying by herself to finish up the Boulder Spring work, used the repaired IU wagon in September, returning to Bloomington around Sept 23.

During the summer of 1970 at Bloomington Bonnie Hodler had started working as my secretary, replacing Mary Swarthout who was pregnant. Dave Smith, starting as a graduate student, stayed in Bloomington and did a few gob pile studies plus some phosphate ester stability studies. Gary Darland finished up in Bloomington that summer, and went off for a teaching job at Cal State San Luis Obispo. In the fall of 1970, Bob Belly arrived as a post-doc, and took up the *Thermoplasma* and gob pile studies.

The fall of 1970 I had the largest lab group on record, three post-docs (Tansey, Belly, Kathie), five graduate students (Rich Weiss, Smith, Bauld, Fliermans, and Judith Bland), plus two undergraduates (Jeffrey Davidson and Diane Smith), plus dishwashers (mainly Frankie), Bonnie as typist, Ruth Ann as a second typist and occasional dishwasher, and Mary Swarthout who typed manuscripts at home. This was also about the peak year for my research productivity.

# Samping Bijah Spring

*Dick Castenholz and students (on the left) sampling at Bijah Spring, August 26, 1970. To his right are Kathie Brock and John Bauld. Although Castenholz' research mainly focused on Oregon hot springs, he generally made an annual visit to Yellowstone, usually bringing his students along. Dick was a superb phycologist and I always learned a lot by tagging along on his Yellowstone visits. August 26, 1970.*

***PICT0142,jpg***

The state of my laboratory at the end of summer 1970 is shown by this letter I wrote to the chairman of the department at Penn State, who had inquired about my possible interest in moving.

> *"However, I should emphasize that I am not unhappy at Indiana and that I have a large and rather exciting laboratory and group of individuals working with me. I currently have five graduate students, three post-doctorates, two honors undergraduates, a research associate, as well as a full-time and a part-time secretary working in my laboratory. This work is supported by two major research grants (National Science Foundation and Atomic Energy Commission) plus a variety of departmental and training grant funds. In addition, I have a fairly elaborate field laboratory at West Yellowstone, Montana for our ecological work on natural thermal and acid environments in Yellowstone National Park. This laboratory, on land purchased by Indiana University, is able to accommodate 6 to 8 researchers for full-time summer research at a reasonably high level of sophistication."*

I had been invited to visit the CNR Laboratorio per la Chimica e Fisica di Molecole di Interesse Biologico in Arco Felice, Naples, Italy as a consultant. This was a basic research group supported by the Italian government that was working on thermophiles. I spent the last two weeks of October 1970 there. I was in the midst of the *Sulfolobus* studies and was able to isolate more strains from samples I took at a small thermal area near Solfatara called Pisciarelli. I returned home by way of England, where I gave a seminar at the University of Bath.

In December 1970 Kathie and I went to Mexico and visited an interesting thermal area at Araro, in Michoacan province. By now, my Bloomington work and my Yellowstone work had been so greatly merged, they were virtually one.

In the “Yellowstone 1970” file folder is a complete copy of a German translation of the Natural History article. Also, a French magazine called *Science & Vie* asked me to write an article for translation into French. This turned out to be a nice article. I was also asked about that time to write an article for Scientific American. I offered them the English version of the French article, but they were not interested in that.

An AEC expense sheet in the files shows who was paid from that contract in 1970: Ilse Yoder, Pat Holleman, Hud Freeze, Gary Darland, Maxine Cohen, Mary Swarthout, H. Moeller (dishwasher), Don Marlow, Frances Foy, plus 3 on the Wabash River work under the direction of Warren Silver of Indiana State University (Terre Haute).

In the fall of 1970 there was lots of correspondence with Japanese geochemist Kimio Noguchi regarding his proposed research visit to Yellowstone in the summer of 1971. This was to be a joint National Science Foundation grant from the US-Japanese Cooperative Program. This was funded, and Noguchi and two junior scientists worked in Yellowstone from June through August 1971. Their approach was to collect lots of water samples for assay of stable chemicals back in Tokyo, and to only do the unstable ones in Yellowstone. I got them set up to live and work in Daley’s two green cabins. Although they were somewhat underfoot all summer, and some of my students had to help them find suitable hot springs to sample, their work was a real boon, as they would assay any spring we took them to. A lot of the chemical data in my Springer book came from this collaboration.

I also spent some time in the winter of 1970 working with NPS museum people from the Park office in Harpers Ferry, W.V. regarding an exhibit to be set up at the small museum at Norris Geyser Basin. This exhibit opened in the summer of 1971 and was quite successful, although it only lasted for about 10 years.

# Boulder Spring: Life at 90 C

In 1969, his second year at Yellowstone, Tom Bott had focused on the bacteria in a single site; Boulder Spring. We knew they grew there, and fairly rapidly. The interest now was in their physiology, especially are they optimally adapted to the temperature at which they are found? Culture attempts had so far been unsuccessful.

Boulder Spring was a convenient site to study. It was not far off the Fountain Freight Road, which in those days was open to autos. It had two very hot boiling pools that were interconnected, creating a constantly flowing small hot creek that provided a convenient site for incubations. Although the steam from this spring was visible from the road, the site itself was not in full view of the public, so that long-term studies could be done here.

We ended up publishing a single paper that combined Bott's work, that of Louise on the incorporation of various radioactively labeled compounds, and Mercedes Edwards electron microscope studies. I ended up pulling this all together and doing the writing.

Isotope incorporation studies were carried out with bacteria growing on cover slips. The incubations were done at the spring and the uptake stopped by adding formaldehyde. The cover slips were then returned to the lab for analysis. Counting was either with the gas-flow counter, for 14-C compounds, or with a Beckman scintillation counter, for 3-H (tritium) labeled compounds.

Note the value of doing the analyses at West Yellowstone. Data were available immediately, could be interpreted, and another experiment done the next day. Although most experiments were done directly in the field, with the radioactive compounds added only a few minutes after removing the cover slips from the spring, for some more complicated experiments the cover slips, immersed in spring water, we brought back to the West Yellowstone lab.

Electron microscopy (EM) was carried out on bacteria which had colonized either glass slides or Mylar strips, with incubation times of 6-20 days. The slides or strips were fixed directly in the field. The advantage of the Mylar strips is that they could be cut into small squares and processed directly for EM.

Throughout most of the summer of 1969, when this work was being done, Tom Bott had trouble getting consistent uptake of radioactive compounds. About a week before he left for a job in Pennsylvania he discovered that if a small amount of sulfide was added to the incubation, consistent uptake took place. We then discovered that Boulder Spring waters contain significant amounts of sulfide (there was no pronounced sulfur smell). Since sulfide oxidizes spontaneously, this explains the inconsistent uptakes first noted.

Once the conditions for getting consistent uptake of labeled compounds had been worked out, temperature optima were determined (temperature transfer experiments). The temperature optima were fairly broad, varying from 80-90 C. Very little or no uptake occurred at 95-97 C. This temperature (above the boiling point at Yellowstone's altitude) was achieved by adding a large amount of Epsom salts to the water to increase the boiling

point of the water bath. We bought the Epsom salts at the West Yellowstone drug store, virtually depleting their supply of this classical laxative. The graphs showing the temperature optima with different substrates are shown in the figure.

Details of the work can be found in the published paper, which is available on-line via NCBI. Do a search for Brock Bott Boulder Spring.

This is one of the last studies we did on bacteria living in hot springs of neutral or alkaline pH. Beginning in 1971, we started working with bacteria living in hot acid springs, such as the newly discovered *Sulflobus*.

# Working at the upper source of Boulder Spring

*The upper source at Boulder Spring. Collecting gas for analysis. August 20, 1969. Louise Brock*

***PICT0213.jpg***

# Incubations at the lower source pool, Boulder Spring

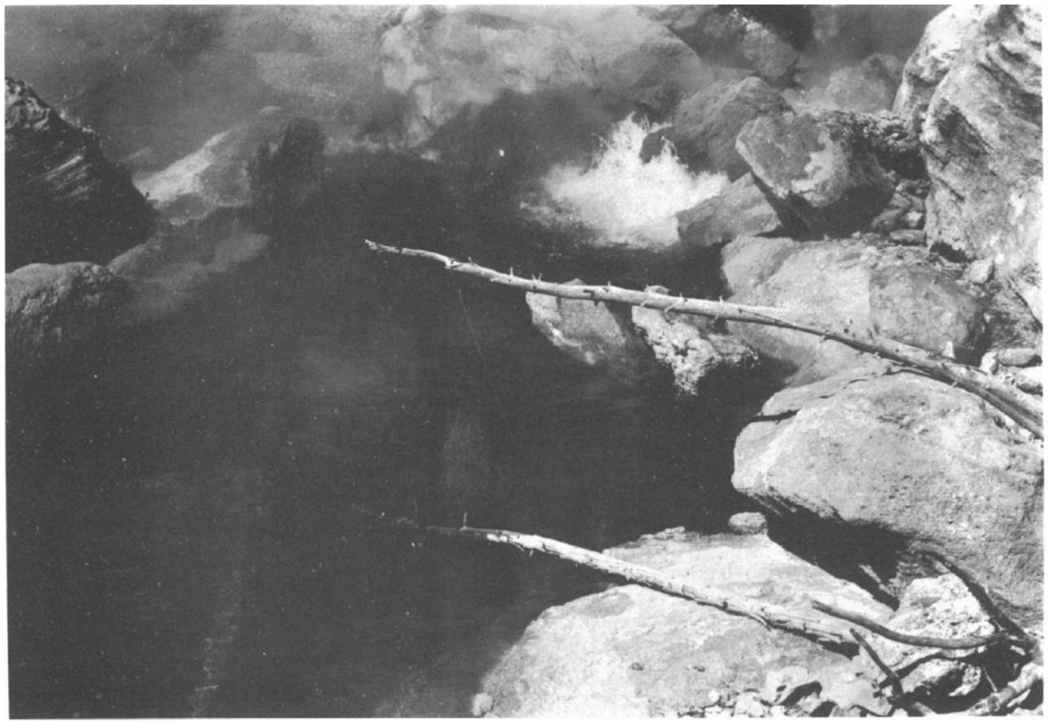


FIG. 2. *Colonization site. The lower source pool is erupting in the background. Racks for cover slips were suspended from the logs shown.*

***Boulder paper Fig 2.jpg***

# Lab set-up for incubating samples in boiling water, Boulder Spring

*Incubation set-up for boiling water. To seal the shell vials without damage the corks were lined with two layers of plastic film. The test tube racks are metal coated with Plastisol. Each vial contains two cover slips separated by a small glass rod. September 13, 1969.*

***PICT0215.jpg***

# Published results from the Boulder Spring work

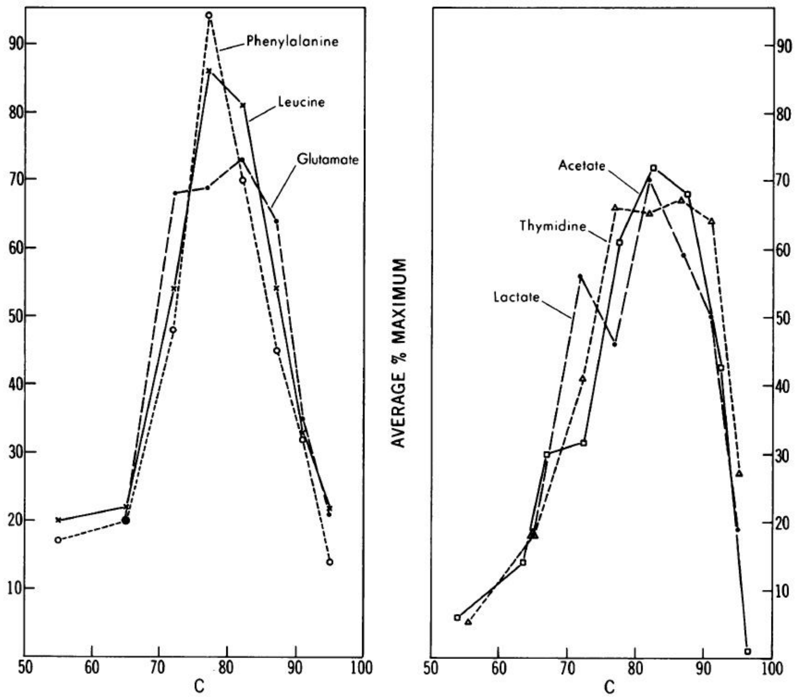



FIG. 11. *Temperature optima for the incorporation of radioactive compounds by bacteria from Boulder Spring. Data are averages within temperature intervals of percentage of maximal incorporation obtained in different experiments. The number of separate experiments done with each compound were as follows:* $^{14}C$*-acetate, 11;* $^{14}C$*-leucine, 7;* $^{3}H$*-phenylalanine, 6;* $^{3}H$*-thymidine, 6;* $^{3}H$*-glutamic, 5;* $^{14}C$*-lactate, 4. All experiments were done in aerated Boulder Spring water to which 13 μg of sulfide per ml was added. For actual radioactivity data typical of that obtained in these experiments, see Table 4.*

***Boulder paper Fig 11.jpg***

# Housing in West Yellowstone

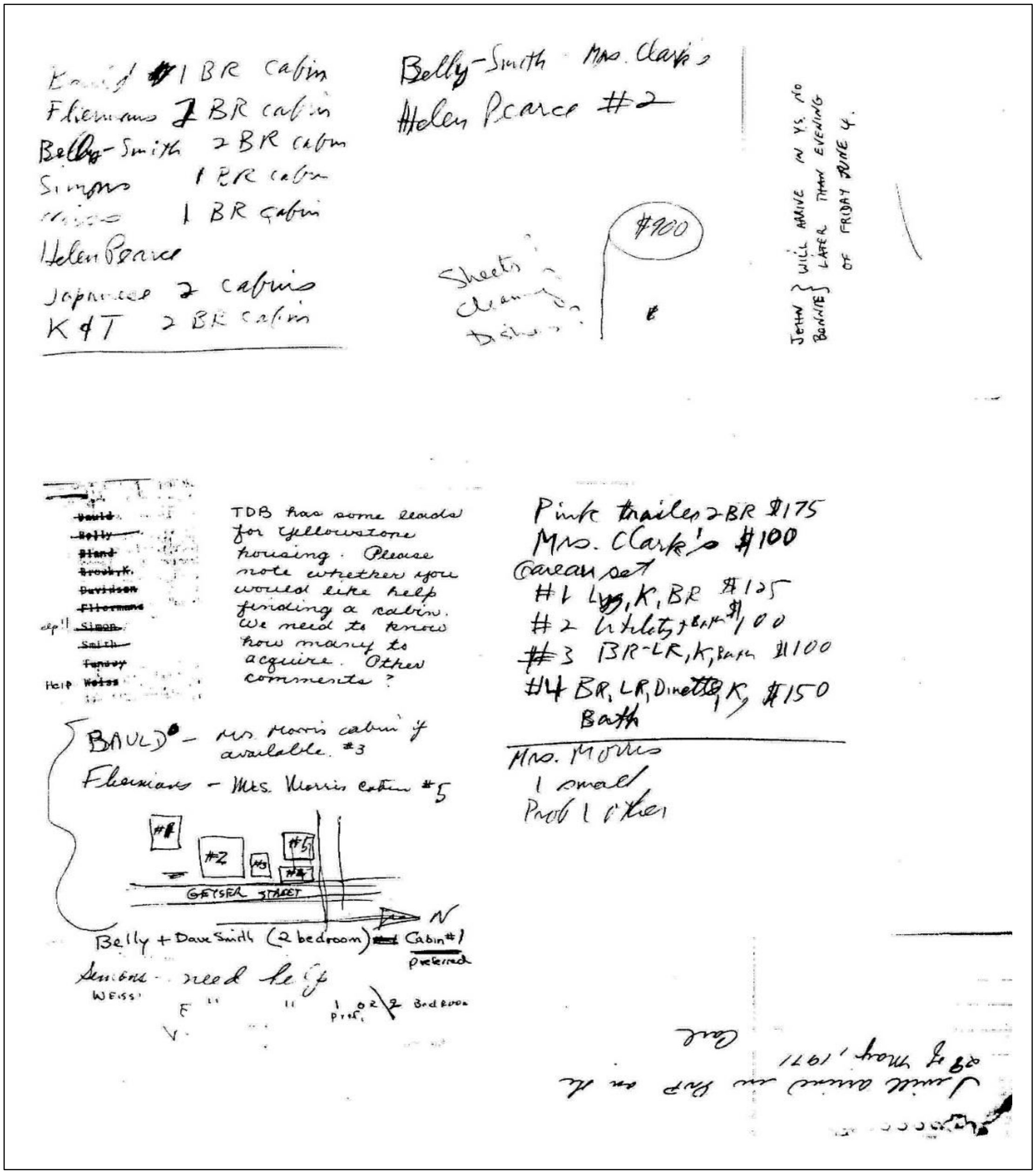
One of the major attractions of being based in West Yellowstone (as opposed to inside the Park) was that there was a lot of housing available for our students and employees at modest cost. All of the units were rented had kitchens and living rooms as well as bedrooms. By the end of 1970 we had developed a fairly good reputation in West Yellowstone, and people were happy to rent to us.

I had established the policy that students or employees coming to West Yellowstone would pay for their own summer housing, but if their rent was more than what they would pay in Bloomington, then Indiana University would cover the difference (from my research grants). Fortunately, housing was cheap enough in West Yellowstone that this option never had to be used. Most students and technicians were happy to have the opportunity to work at Yellowstone!

In a file folder labeled “Yellowstone Lab 1970”, I found a note about setting up housing for people. It gives some insight into how I handled things. This note was probably made in early spring of 1971 before we knew we were going to move to Wisconsin. When the Wisconsin news came (April 1971), students had a choice of staying at Indiana University to finish or moving to Wisconsin. Bauld, Belly and Smith elected to move and Fliermans and Weiss stayed behind. But either way, they were to spend the summer of 1971 in Yellowstone.

The rates shown in the notes give an idea of how inexpensive things were.

# Housing in West Yellowstone


Bauld #1 BR Cabin
Fliermans 2 BR cabin
Belly-Smith 2 BR cabin
Simons 1 BR cabin
[illegible] 1 BR cabin
Helen Pearce
Japanese 2 cabins
K & T 2 BR cabin

Belly-Smith Mrs. Clark's
Helen Pearce #2
$900
Sheets?
Cleaning?
Dishes?

JOHN } WILL ARRIVE IN YS NO
BONNIE } LATER THAN EVENING
OF FRIDAY JUNE 4.

Bauld
Belly
Bland
Davidson
Fliermans
Simon
Smith
Weiss
Help

TDB has some leads for Yellowstone housing. Please note whether you would like help finding a cabin. We need to know how many to acquire. Other comments?

Pink trailer 2BR $175
Mrs. Clark's $100
Caravan set
#1 Lvg, K, BR $125
#2 Utility + Bath $100
#3 BR-LR, K, Bath $100
#4 BR, LR, Dinette, K, $150
Bath
Mrs. Morris
1 small
Prob 1 other

BAULD – Mrs. Morris cabin if available. #3
Fliermans – Mrs. Morris cabin #5
#1 #2 #3 #4 #5
GEYSER STREET
N
Belly + Dave Smith (2 bedroom) Cabin #1 preferred
Simons – need help
WEISS " " 1 or 2 Bed room pref.

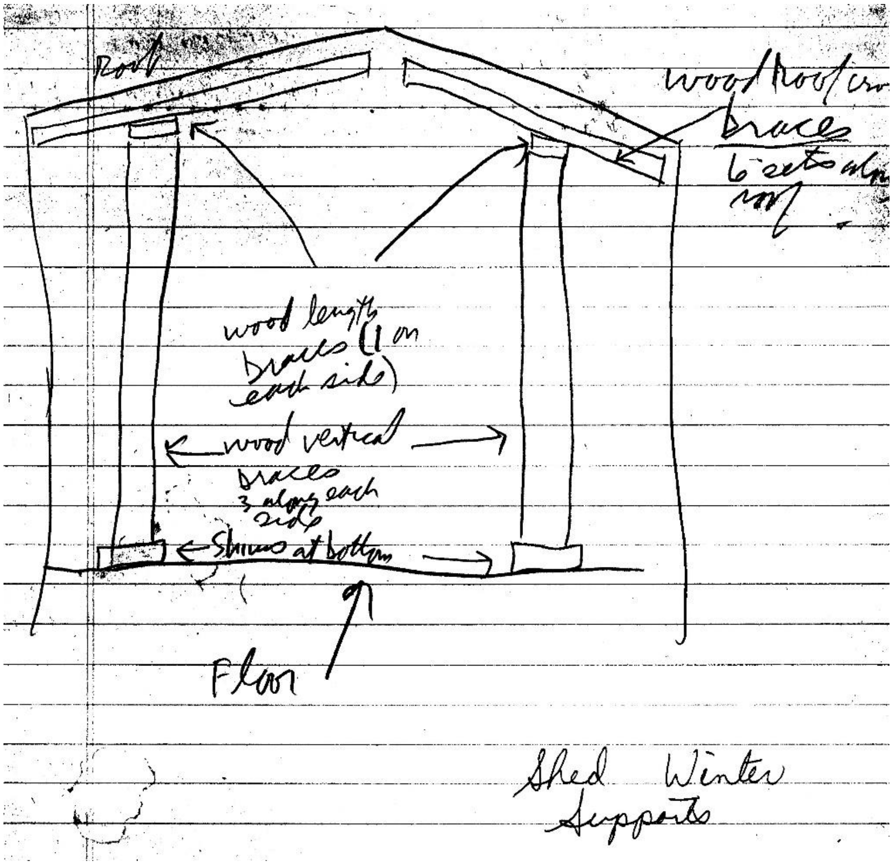
28 May, 1971

***Housing West Yellowstone planning for 1971.jpg***

**Dealing with the heavy snow**

House trailers and regular prefab sheds have notoriously fragile roofs. Many house trailers which are permanently installed in snow country have supplemental roof covers, supported independently of the trailer, to keep the main snow off their roofs. We never went to that extreme, but when we closed down in the fall, we put up interior beams/braces to support the roof. The diagram below shows how this was done for our prefab storage shed and a similar system was used for the two trailers. This diagram is mostly in my handwriting.

# Battling the heavy snows




***Roof support diagram shed 1970.jpg***

# Management of the Laboratory: 1970s

By the end of 1970 I had been operating the laboratory long enough that I knew what problems might arise. In my usual way, I wrote down a set of instructions for those involved, especially those arriving before the lab had opened.

**Brock Notes to Lab Personnel beginning early May 1971 (See also another version copied from original farther down.**

- Need more tables for large trailer. Definitely need a typewriter table. Be sure tables are sturdy. Inquire Mrs. Wadsworth or up-town where stores are opening
- On a fairly warm day, shovel out the driveway for the Van. [As usual, there was still lots of snow in early May.]
- Make arrangements for garbage and trash pickup. Inquire at Brower's drug store (he's the mayor also) about who's doing it this year.
- The West Yellowstone Bank is accommodating and will usually cash checks for our people. Kathie and I have a checking account there.
- The West Yellowstone Bank will do photocopying for 25 cents a copy. [Xerox was still fairly uncommon!]
- Housing for lab people. May need to make final arrangements in several cases. Keep your ears open. Mrs. Wadsworth has agreed to rent 2 cabins for $100/month each.
- Deliveries of lab stuff will be by Garrett Freight Lines. The local rep is Ron Spainhower and he knows we are here. Garrett parks their big trailer across from Duncan Lumber.
- Some deliveries will come Parcel Post. Make sure post office people know who you are, so they will give you our stuff.
- Ford Van (mobile lab).
    - Any service over $10 must be authorized by university motor pool
    - Have carburetor adjusted for high altitude. Best place for this and other minor service is West Gate Standard (Whitey's) near Park entrance)
    - If you break down in the Park and have to be towed, try to get Whitey to tow you to West Yellowstone. (The Park tow truck is more expensive.)
    - Be sure to have any service done in West Yellowstone rather than Park.
    - Get employee stickers for vehicles at West Gate entrance. Mention my name.
- Any leaks on trailer roofs will be fixed by Hathaway Trailer Sales, St. Anthony, Idaho
- Before June 1, remove all storm windows. Put labels on the windows indicating where they came from. Place all windows in the shed.
- Trailer #1 is back (smaller) trailer
- Trailer #2 is front (larger) trailer
- Water. If any leaks develop, if water pressure drops, if pressure tank stops functioning, or anything else related to water, call Bert Fields

- Gas. Valve is at tank. When turning it on in spring, must light all pilot lights (water heaters, furnaces, stoves, ovens). In winter, simply turn off valve at tank and the furnaces and water heaters. If any problems with gas, leaks, furnace quits, etc. call Montana Propane. The head man is Bob Leithead.
- Electricity. Any problems, call Kip Jelly. See phone book for his number. Circuit breakers are in back rooms of each trailer.
- Mail. Introduce yourself at P.O. and collect all mail for both boxes.
    - The general university box is 157. You should have the combination
    - Brock personal box is 562. Combination is F, I ½ , G ½
- Guest Book. Have any visitors sign
- Unpacking. Save all boxes!
- Deliveries. Save all packing slips.
- Purchases. Save all receipts for petty cash.
- Turn furnaces down at night to 55, up to 65 in day.

**Further notes on opening and closing the West Yellowstone lab**

When we opened up the laboratory in May 1971 we found a number of problems. This led me to put together a further list of instructions on how to open and close the lab. Obviously, lots of nasty detail that had to be done here are handled routinely by permanent service people at home. It was not obvious when we started how much of this stuff there was. Kathie and I managed to handle it. Ultimately, I was responsible, but in 1972, I formalized this by hiring post-doc Jerry Mosser as lab manager (funded from my WARF account). After Jerry moved on, Charlene Knaack took over. She handled completely the huge amount of detail attendant on closing the lab for the last time.

# How to open and close the lab, 1971

1971

Instructions for Opening and Closing Yellowstone Lab
and Names of Useful People

A. Closing
1. Turn off water in shed using long metal pole.
   Are 2 holes--do both.
   If turn overly far, water is drained from trailer.
   Instructions are written in pencil on 2x4 under floorboard of cabin.
   This is a 3-way valve.
2. Open outside water valves, put air pump on sink faucet and blow it all out.
3. Turn off propane valve under red cap on tank.
4. Hot water heaters in both trailers: first turn off heater, then attach hose and drain.
5. Turn off electricity. Each trailer is separate. Do both. On poles outside; pull out, reverse, put back in (the entire fuse holder unit).
6. Remove (and leave off) the screw-off cap on a pipe which comes down from the trailer outside--near the outside faucet.
7. Put antifreeze in toilet: soak out water first. Use plumber's helper to force antifreeze down pipes.
8. Put newspaper over windows.
9. Seal doors with tape.
10. Cover furnace vent with can.
11. Put snow-removal ladders up against each trailer.

B. Opening
1. In spring, reverse above. Have Montana Propane check gas furnaces, stoves, and water heaters.
2. Re-level the trailers. Use jack and level. Need hydraulic jack. Cannot trust the built-in jack. Can borrow jack and level from Harold Young--give him a few bucks.

C. Useful names for required services:
Electricity: G.V. (Kip) Jelly--call him Kip.
Plumbing: ~~James Curry--C&H Plumbing. No telephone but lives in trailer on north west side of town.~~ Bert Fields
Snow shoveling: Gene King, Box 605. Reach through Al Lapp, 522 Firehole Avenue, 646-7549.
Telephone repair: J.P. Stradley, 124 Geyser.
Miscellaneous repairs, etc.: Ron Spainhower, 309 Dunraven, 646-7791
Gas: Montana Propane, 16 Geyser, 646-7610.
Winter storage of our freezables: Harold Gooding, 321 Hayden, 646-9398.
Groceries, cold storage, and general intellectual chit-chat: Cal Dunbar Food Roundup.

***Lab instructions opening closing 1971.jpg***

# Yellowstone Winter 1971

We were employed by Indiana University until the end of June 1971, then UW-Madison (except for students Fliermans, Weiss, and Bland, who elected to remain at Indiana University).

In early Jan 1971, I was in Minneapolis working on the Program Committee for the American Society for Microbiology meeting scheduled for that city in May. I then flew from Minneapolis to Billings and Kathie flew from O'Hare to Billings. There we met three people (David Dietrich, Bert von Bork, and Ulf Bergstrom) from Encyclopedia Britannica, drove with them to Mammoth, and took a snow machine to Old Faithful for the night. We brought sleeping bags to use at Old Faithful. Park Service Naturalist John Douglas came along in the snowcat and more or less looked after us.

We had marvelous weather. I got good winter temperatures on the Firehole River, plus a miscellany of other things. There is a nice photo of me with the film crew at Castle Geyser, where they are filming close-ups (probably hot spring flies).

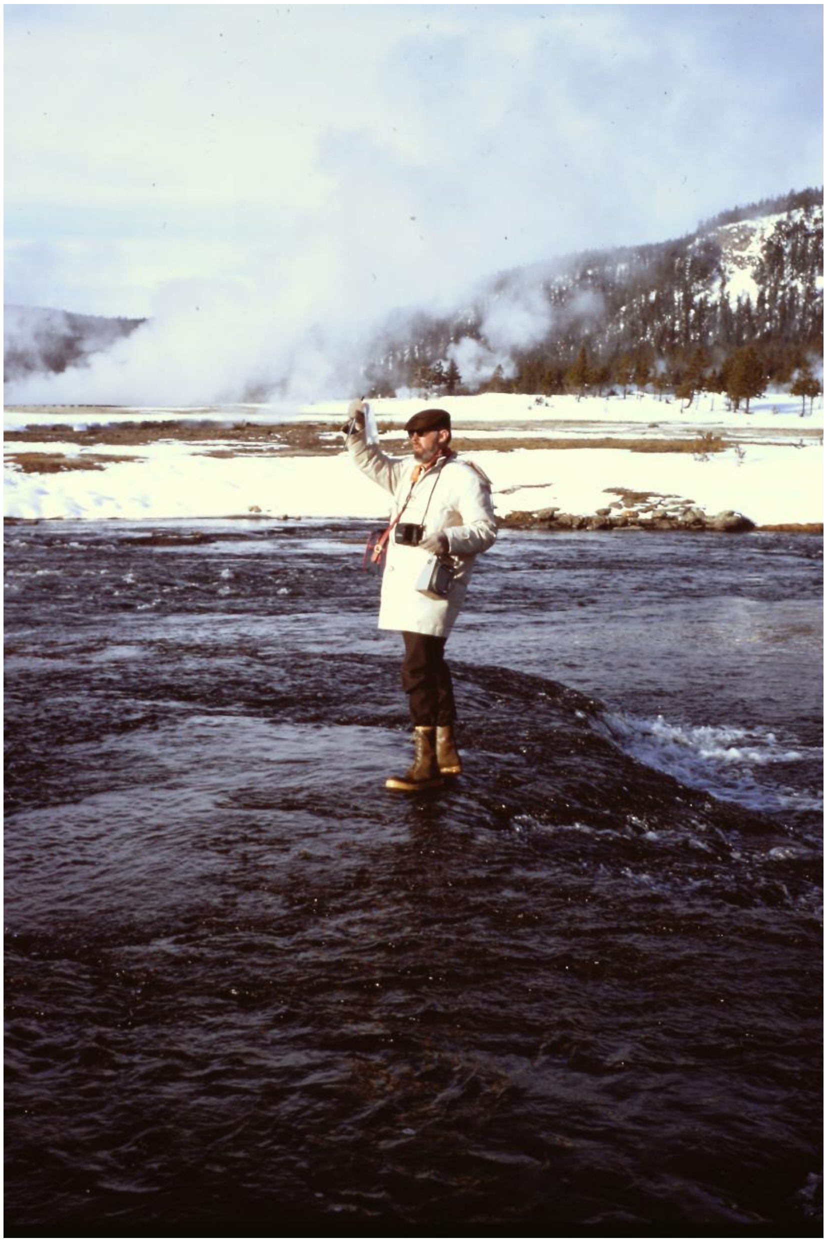
After the winter trip to Yellowstone, Kathie and I went on to the Bay Area, visited The Geysers, Calif. Then Steamboat Springs, Nevada (which had changed) then home.

# Winter travel under ideal conditions!

*The SnoCat that Encyclopedia Britannica (EB) hired for our trip to Old Faithful. We started at Mammoth Hot Springs. There was no snow there, so we had to drive the road to the upper terraces to access the snow machine. This was a great trip as we had complete control of the schedule, stopping wherever we wished. January 28, 1971.*

***PICT0178.jpg***

# Tom Brock sampling in the Firehole River; January 1971



*Sampling in the Firehole River during the winter Encyclopedia Britannica trip. January 28, 1971. I was able to carry out temperature measurements along the main channel of the river. These data were used for the Boylen research on the Firehole, done the following summer. In the background is the Biscuit Basin Geyser Basin. Note the L.L. Bean boots, waterproof at the bottom and leather above. These were the favored boots of many doing winter research in the Park.*

***PICT0185.jpg***

# Film crew working at Castle Geyser; January 1971

*Kathie and Tom Brock at Castle Geyser with the Encyclopedia Britannica film crew. They are doing close-up shots of brine flies in action. January 28, 1971.*

***PICT0179.jpg***

# Yellowstone Summer 1971

Because of the changes in school schedules at Indiana University, we decided to open the lab early in 1971. Belly drove the loaded van from Bloomington to Minneapolis for the American Society for Microbiology meeting (2-7 May 1971), then on to Yellowstone. Kathie and I left a bit early from the American Society for Microbiology meeting, flew to Billings, then drove a rented car to West Yellowstone. We were in West Yellowstone May 6, 1971.

The lab had lots of snow and it was quite a hassle to get things going as pipes had leaked. Bert Fields came to our rescue and we eventually got things underway. We got Belly oriented, then Kathie and I flew back to Bloomington for the big move (household and IU research lab) from Bloomington to Madison.

**********************************************************************

**How much did UW-Madison pay for the West Yellowstone LAB?**

Letter 9/1/1972 from UW-System to Indiana University enclosing WARF check for

$14,578.33 agreed purchase price for land, whole lab, and all the fittings.

**********************************************************************

Tansey drove out from IU in mid-May 1971 and he and Belly got the lab set up, doing a lot of good work. The large trailer roof had bowed in from the snow (poor shoveling) and Koski fixed this for about $300. The last two weeks of May were unbelievably hectic, as we were in Bloomington packing the Indiana University lab and our Bloomington home for the move to Wisconsin, finished packing for the summer work at Yellowstone, and left Bloomington for good on May 31. The movers did not come to Bloomington until July, when the house we bought in Madison would be available. One of my undergraduate students, Diane Smith, met the movers at our Bloomington house and made sure that the house was clean and empty. The same mover then came with the partially loaded truck to IU-Jordan Hall and loaded the lab materials. Diane took care of that also.

Carl and Ruth Ann Fliermans drove out to Yellowstone from Bloomington in their own car in late May, Kathie and I came from Madison in our new Ford Wagon in early June, John Bauld and Bonnie Hodler (now married) drove in John's car in early June, Dave Smith drove the I.U. wagon out (accompanied by Jeffrey who went on to L.A.), Rich Weiss and wife Susie came out in mid June. Chuck and Kathy Boylen arrived in early June from California, he having his own funds for summer work from NASA. Chuck did a project on the Firehole River and Kathy Boylen (who had a M.Sc. from UW-Madison) worked for me as a technician.

Living arrangements: Tansey and wife had a cabin at Wadsworths, Belly and Smith shared an apartment at West Park Apartments, John and Bonnie also had an apartment there, Carl and Ruth Ann had one of Mrs. Morris's cabins, the same one Mossers' had had the previous summer and Bott's the summer before that. Kathie and I had the pink

trailer, Weiss's stayed a while at K-Z motel, then camped, and the Boylen's had an apartment at the Holiday Motel.

The summer was busy but the spirit was relaxed. Projects were: Smith-soil *Cyanidium*; Fliermans-soil sulfur-oxidizing bacteria; Tansey-thermophilic fungi mainly in *Cyanidium* mats; Belly, *Thermoplasma* and other acidophilic thermophiles, Boylen-algae in the Firehole; Weiss-ecology of *Sulfolobus*; Bauld-*Chloroflexus* and cyanobacterial mats. Kathie and I finished *Sulfolobus* culture work and did a number of miscellaneous studies. Kathie and Kathy did a major sampling of acid lakes in the Park and Kathy Boylen carried further my project on the lower pH limit of cyanobacteria (blue-green algae).

Kathie and I drove to Seattle and the west coast for one week to visit my graduate student Judy Bland at Friday Harbor Laboratories and check on her *Leucothrix* work

In mid-July Kathie flew to Madison from West Yellowstone and met the movers, overseeing the transfer of both the lab and house materials and then flew back to West Yellowstone.

We had lots of visitors. The working visitors were the Encyclopedia Britannica photographers, the three Japanese chemists, and the geologist Malcolm Walter.

In addition, here are visitors that signed the Guest Book:

- Raymond Lynn, Botany, Utah State University
- Walter Konetzka, microbiologist, Indiana University
- Rodger Mitchell, Zoology, Ohio State University
- William Schopf, geology, UCLA
- Bruce Fowler, Biology, Colby College, Maine
- Gene Rutledge, U.S. National Laboratory, Idaho Falls, ID
- John Chamberlain, Santa Fe, NM
- Richard Voigt, San Diego
- Herb Lightman, Hollywood, CA
- Roderick Hutchinson, Dept Earth Sciences, Iowa State University, Ames
- Ann Heuer, Biology, California State University, Hayward
- Michael Wynne, Falls Church, VA

# Film crew at Octopus Spring

*Encyclopedia Brittanica film crew, Bert von Bork and Ulf Bergstrom, working at Octopus Spring, July 13, 1971. The EB people were here for more than a month in 1971. In addition to "Life at High Temperatures", they filmed a movie called "Geyser Valley", which was primarily visual and musical (for younger school grades). They were well equipped (Ariflex 16 mm).*

***PICT0197.jpg***

**Below is a list of miscellaneous things that happened in 1971, taken from the Yellowstone 1971 file folder.**

My letter to Mrs. Morris reserving 3 cabins for couples.

Letter to Chief Naturalist Bill Dunmire Dec 4, 1970. I expect 8 independent workers in Yellowstone next (1971) summer, plus Japanese geochemists.

Spore conference Oct 9 1971 at Lake Geneva: “Geothermal habitats as natural reservoirs for thermophilic spore-forming bacilli. *Bacillus acidocaldarius* (Darland’s new species). Note: per ATCC web site, this species is now called *Alicyclobacillus acidocaldarius* (Darland and Brock) Wisotzkey et al. (ATCC 43030) from soil from acid fumarole in Hawaii. There is a whole lot in Bergey’s Manual about the genus *Alicyclobacillus*, with lots of species. Interesting lipids. Important in canning industry because it may spoil mildly acidic fruit juices. So the organism that Darland and I discovered is now the type species of a new genus. This is thus the fourth new genus that was discovered during the Yellowstone research. This work was published in the Journal of General Microbiology (U.K.).

Lots of correspondence in 1971 folder re talks given at various places.

Letter 6/11/1971 to Charles Medvick from the Indiana coal producers re our move to Wisconsin. Regret that my Indiana coal work will have to be curtailed.

Letter in late Oct 1971 from Rick Hutchinson (later; Yellowstone National Park geologist) re changes in thermal activity at Norris. Mentions our work there with the Japanese. Presumably sampling done under NPS supervision. Mentions visit to our West Yellowstone lab.

Letter to Judy Bland Aug 25, 1971 that Korby (our dog, who had been lost in the Beartooth Mountains) had been found. We will be closing up lab on 27 Aug 1971 and will be in Madison by 30 Aug.

Feb 1971 Submission letter to J. Bact re Boulder Spring paper length. Also, copy of handwritten intro that I probably wrote in the Dallas airport (August 1970) on way to Mexico plus various versions of the introduction for the Boulder paper.

7/29/1971 letter to Paul Ray at KY re lipid paper on *Thermus* accepted; Notes on other work; mentions Bill and Nancy miss being at Yellowstone

6/7/1971; letter to my mother (Helen Brock); have 10 people this year; Sunday drove to Red Rock Lakes (Montana; bird sanctuary)

8/2/1971; KMB letter to her mother (Eleanor Middleton) re things going on; climbed South Teton 7/31-8/1/1971

AEC budget May 1 1970 thru Apr 30 1971: personnel paid: Belly, Kathie Brock, John Bauld, Jeffrey Davidson; Diane Smith, Ilse Yoder, Frances Foy, Ruth Fliermans; Bonnie Hodler; Mary Swarthout (a few hundreds); Also purchased scintillation counter ($4139)

AEC Mar 30 1971; request $1000 to purchase a new scaler for our Nuclear-Chicago gas flow counter. Current scaler over 20 yrs old and unreliable.

Fall 1971; TDB wrote the script for the EB film

2/9/1971; letter to Donald White (USGS) about our previous visit; visited Steamboat Springs Nev afterward and found no springs any more; lots of drill holes upstream

6/4/1971 letter from Malcolm Walter re working in Yellowstone; he hopes to come out soon

6/17/1971 To Helen Arthur (Storeroom Manager at Indiana University) Re Q gas tank and charges from Matheson; can she handle? (Just at the end of my IU years)

6/19/1971 re Mosser research assoc position I had offered him at the American Society for Microbiology meeting (presumably using some of my new WARF funds); that I had offered him earlier

6/25/1971 letter to Mosser; could keep position open until American Society for Microbiology 1972; goes into details

7/16/1971; letter to Zeikus, now a post-doc in Illinois; Firehole project delayed because of late-melting snow and turbid water, but Boylen now working fine; I had offered Zeikus the possibility of working on anaerobes in Yellowstone for the summer

7/8/1971; letter from Pat Holleman in Massachusetts; 7/16/1971 letter to Pat H in Mass; they will be visiting Yellowstone; tells her about move to UW-Madison; our furniture moved in yesterday; closing West Yellowstone lab Aug 27 1971; Jim Watters from Sierra Club will be here for Beartooth hike.

7/16/1971 letter to Pat H; have 4 post-docs, 3 grad students plus visitors, 3 Japanese; also EB film; Bonnie is here for summer (with JB) and Jerry Mosser will be here by Aug for his new position

7/20/1971 letter to Castenholz re New Zealand hot springs; Bauld planning to visit Pierson to learn how to culture *Chloroflexus*; Meeks gave a seminar at West Yellowstone and will be offered a post-doc for 3 months next year (1972)

7/21/1971 Received approval from the West Yellowstone school board to use their audio-visual room for a seminar on Friday 23 July 8:30 PM

8/11/1971; Anne and Jerry Mosser flying to West Yellowstone Aug 30 and leaving Aug 28. Then on to Msn to find a house.

12/8/1971; long letter to Castenholz; TDB/KMB just returned from Dominica; "After sweating through several days in the jungle I wondered why anyone would study hot springs in the tropics." Re Noguchi sulfide assays in Yellowstone; not very high anywhere but highest Calcite Springs; re Bauld work on stimulation of *Chloroflexus* by sulfide; long paragraph re algae in Japanese hot springs; will write Meeks re next summer

12/15/1971 (from Madison) letter to Tansey; miss him since candy jar is hardly ever filled now (Tansey stayed at IU for faculty position); re Belly finding *Thermoplasma* in gob piles (21 out of 30 sampled). Bact Revs article having 500 more reprints made; mentions sewage engineers or soil platers; about other papers coming from Yellowstone work; joint with Belly on microbes in *Cyanidium* mats

# Laboratory group West Yellowstone 1971

*Lab group in 1971, in front of the two lab trailers.: L to R: Japanese chemist Aikawa; Bonnie Bauld; Prof. Noguchi; Mike Tansey, Kathie Brock, Bob Belly, John Bauld, Carl Fliermans, Kathy Boylen, Malcolm Walter, Dave Smith, Rich Weiss, Chuck Boylen, Japanese chemist Goto.*

***Lab group 1971 color.jpg***

## Lower pH limit for cyanobacteria (blue-green algae)

Determining the lower pH limit for cyanobacteria was an important study that could only be done directly in nature. It also demonstrates the importance of the West Yellowstone laboratory, because this work could not have been done without those facilities. It was published in *Science* in early 1973, from work done in the summer of 1972.

I can't remember when I first started to realize that cyanobacteria did not live in low pH environments. The studies on *Cyanidium caldarium* may have played a role, because I knew that although this species, like the cyanobacteria, had the pigment phycocyanin, it was a eukaryote.

My acid mine drainage (strip mine) work may have also played a role, because I knew that all the phototrophs in such drainages were eukaryotes (algae).

By the time I started this work, I had been all over the world studying thermal environments, many of which are quite acidic.

By 1972, when this work was done, I had traipsed all over the back reaches of Yellowstone National Park, and had seen many acidic environments, some of which were not thermal. Acid lakes are common throughout the regions of the park with geothermal activity, and some of them have normal temperatures. I had a complete collection of the 7 ½" topographic maps for Yellowstone, which showed locations of many of these small lakes. I made a long list of these lakes, and marked them on topo maps.

Another motivation for this work was that Kathy Boylen was working with us in West Yellowstone and I knew her capabilities because she had worked in the lab all last school year (1971-1972) at UW-Madison. Finally, Kathie Brock was available to accompany Kathy Boylen in the remote backcountry areas. (By all reports, the two women had a "ball" doing the field work.)

Kathy and Kathie were instructed to visit all the lakes they could find (they ended up with 22 lakes), measure the pH, and sample for algal populations from the shoreline. All biological samples, as well as water samples, were brought back to the laboratory for precise pH measurements and microscopy. (We used special plastic (Whirl-pak) bags to hold water samples.)

I did the microscopy myself. In addition, Kathy Boylen set up enrichment cultures, using our standard algal culture medium adjusted to the pH of the water from which the sample was taken. The cultures were incubated in our culture room in the big trailer, under lights at either room temperature or 35-37 C. Roger Stanier had reported that a temperature of 37 C is somewhat selective for cyanobacteria over eukaryotic algae. After two weeks incubation I examined the cultures microscopically for the presence of eukaryotes or prokaryotes.

The data in the table give a range of lakes and creeks with pH varying from 1.0 (Sour Lake in the Canyon area) to 8.65 (Swan Lake in the Mammoth area).

Eukaryotic algae were found either naturally or after culturing in almost every sample. Prokaryotic phototrophs (*i.e.* cyanobacteria), on the other hand, were never found below pH 4, and were occasionally missing at pH close to 5.

In addition to the Yellowstone work, in the published paper I also discussed my observations at various other locations, such as Iceland, Italy, Japan, and New Zealand. (The New Zealand work included the observations of a pH gradient in the Waimangu Caldron area.)

I also discussed the literature, not extensive, but all finding that cyanobacteria do not live at low pH.

In the *Science* paper I drew some important evolutionary conclusions.

> *"…there seems to be an acid barrier which eukaryotic algae, but not blue-green algae [cyanobacteria], have been able to overcome….it seems evident that in the portion of the Precambrian when cyanobacteria existed and eukaryotic algae did not, acidic habitats would have represented uncolonized niches within which eukaryotic algae, if they evolved, could have reproduced without competition from blue-green algae [cyanobacteria]."*

Although this paper is occasionally cited, I know of no work following up on it, which is strange, since it is an important evolutionary discovery. However, I had a report (12/27/2016) from ResearchGate that this article recently had 100 "reads".

# The lower pH limit for cyanobacteria (blue-green algae)

Table 1. Presence of eukaryotic and blue-green (prokaryotic) algae in lakes and streams of various *p*H values in Yellowstone National Park. Cultures were set up in duplicate and incubated at both room temperature and approximately 37°C. A plus sign indicates that at least one culture was positive, at either temperature; ng indicates no growth.

| Location | *p*H | Temp. (°C) | Natural samples | | Cultures | |
|---|---|---|---|---|---|---|
| | | | Eukaryotic | Prokaryotic | Eukaryotic | Prokaryotic |
| Sour Lake | 1.9 | 27 | + | — | + | — |
| Unnamed lake* | 2.1 | 22 | + | — | + | — |
| Unnamed pond† | 2.3 | 29 | + | — | + | — |
| Obsidian Creek‡ | 2.35 | 23 | + | — | + | — |
| Clear Lake | 2.5 | 17 | + | — | + | — |
| Sieve Lake | 2.5 | 40 | + | — | + | — |
| Obsidian Creek | 2.7 | 18 | + | — | + | — |
| Beaver Lake | 2.7 | 16 | + | — | + | — |
| Beaver Lake | 2.8 | 14 | + | — | + | — |
| Obsidian Creek | 2.85 | 24 | + | — | + | — |
| Nymph Lake‡ | 2.9 | 22 | + | — | + | — |
| Obsidian Creek | 3.2 | 17 | + | — | + | — |
| Turbid Lake | 3.25 | 25 | + | — | + | — |
| Sedge Creek | 3.4 | 17 | + | — | + | — |
| Nuphar Lake | 3.8 | 12 | + | — | + | — |
| North Twin Lake | 4.1 | 17 | + | — | + | + |
| Obsidian Creek | 4.5 | 14 | + | — | + | — |
| Nymph Lake | 4.8 | 22 | + | — | + | + |
| Lake 7847§ | 5.0 | 30.5 | + | + | + | + |
| Obsidian Creek | 5.5 | 10 | + | — | + | + |
| South Twin Lake | 5.5 | 12 | + | + | + | + |
| Nymph Lake | 5.7 | 22 | + | + | + | + |
| Nymph Lake | 5.8 | 32 | + | + | + | + |
| Obsidian Creek | 5.9 | 16 | + | + | + | + |
| Obsidian Creek | 5.95 | 16 | + | + | + | + |
| Obsidian Lake | 6.1 | 20 | + | + | + | — |
| Obsidian Creek | 6.3 | 12 | — | — | + | + |
| Scaup Lake | 6.4 | 17 | + | + | + | + |
| Beach Lake | 6.6 | 20 | + | + | + | + |
| Harlequin Lake | 6.7 | 17 | + | + | + | + |
| Yellowstone Lake | 6.7 | 16 | + | — | + | + |
| Obsidian Creek | 6.9 | 15 | + | + | + | + |
| Squaw Lake | 7.7 | 17 | + | — | + | + |
| Rush Lake | 7.7 | 17 | + | + | + | + |
| Goose Lake | 7.85 | 17 | + | + | + | + |
| Feather Lake | 8.35 | 17 | + | + | + | + |
| Swan Lake | 8.65 | 20 | + | + | ng | ng |

* Small lake with no outlet south of the Mud Volcano area, near Lake 7847 (U.S. Geological Survey Quadrangle, Canyon Village, Wyoming). † Small turbid pond at Mary Bay, Yellowstone Lake, north of East Entrance Road. ‡ At Nymph Lake and Obsidian Creek various *p*H values were found at different sites. § Lake 7847 is south of the Mud Volcano area and is at an elevation of 7847 feet (2392 m) (U.S. Geological Survey Quadrangle, Canyon Village, Wyoming).

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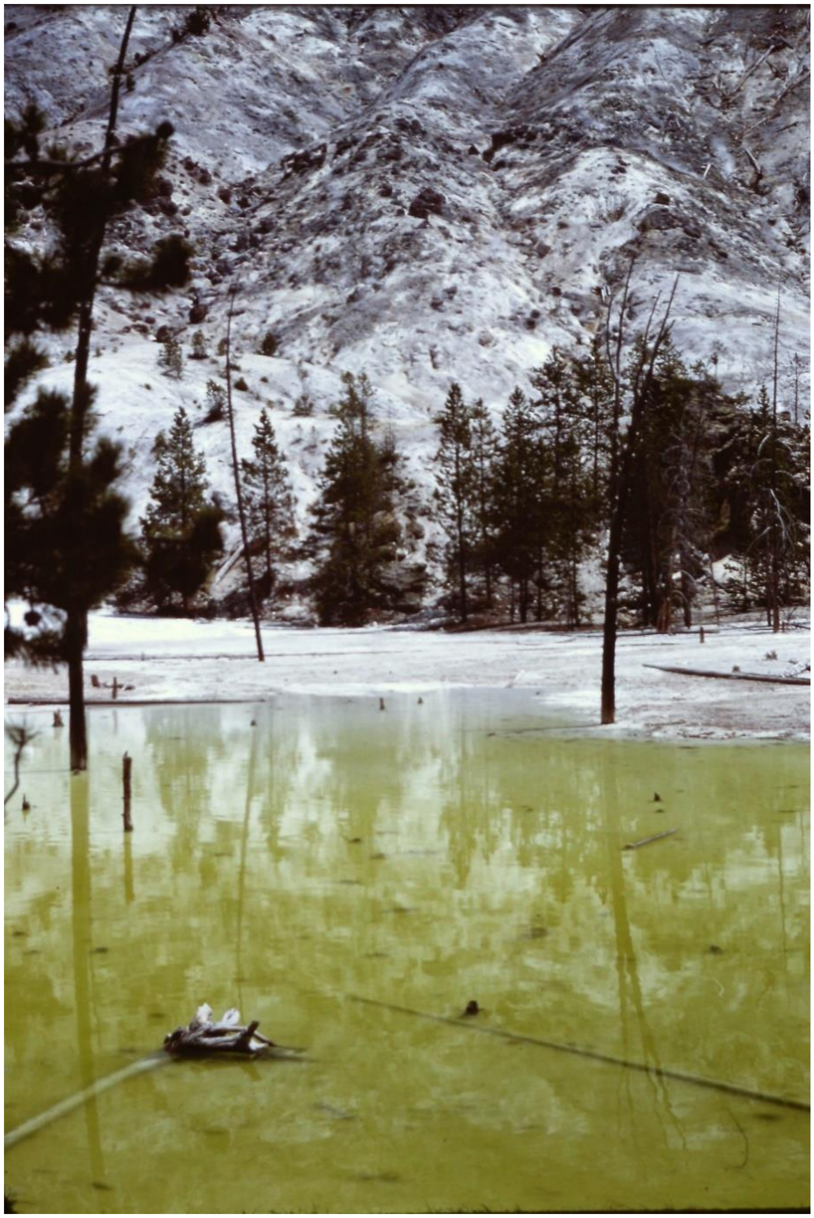
***Brock lower pH Science Table 1.jpg***

# Lemonade Lake, by Roaring Mountain



*Lemonade Lake at the foot of Roaring Mountain. A typical source used in the pH study. Non thermal. Photo taken June 5 1969. This is a view from the parking area.*

***PICT0212.jpg***

# Clear Lake, in the Canyon area

*Clear Lake, an acidic lake in the Canyon area. There is a large acidic thermal area along the Sour Creek Trail, about a mile south of Artist Point. Clear Lake is the largest feature. It is not hot, so presumably gets its acidity from seepage from the surrounding thermal area. In 1971 this lake had a pH of 2.5. Photo taken June 29, 1974.*

***Clear Lake B-W 6-29-1974 N181.jpg***

# Sour Lake, the most acidic lake in the Park

*Sour Lake, a large, moderately cool, acid lake in the Mud Volcano group. When we measured it in 1971 the pH was 1.5, making this the most acidic lake we sampled. The foam is mostly organic, derived from extensive algal growth. Photo taken July 22, 1975.*

***Sour Lake 7-22-1975 N223.jpg***

# Vapor-dominated thermal systems: research 1968-1972

Yellowstone has a large number of springs and soils that are very acidic and have a completely different geochemistry from those of the main geysers and boiling springs. They also have completely different microbiology.

The geologists call these "vapor-dominated" systems. The origin of these vaporous systems is both geological and microbiological processes, the latter information based on my work.

The manner in which vapor-dominated systems arises is the following.

- The systems are perched above the deep thermal waters, so that most of the moisture is derived from steam or from cool groundwater.
- Steam rising from underground sources is rich in hydrogen sulfide.
- As the steam reaches the surface, it comes in contact with oxygen, and the hydrogen sulfide oxidizes spontaneously to elemental sulfur.
- Elemental sulfur is relatively stable and deposits as crystals among thermal vents and within or on the surface of the soil.
- Bacteria, both lower-temperature rod-shaped (*Thiobacillus)*, and higher temperature spherically lobed-shaped Archea (*Sulfolobus*), oxidize the sulfur to sulfuric acid.
- The sulfuric acid seeps into the the soil and rocks, and especially at higher temperatures causes extensive breakdown of the bedrock.
- Groundwater derived from rain and snow moves into vents and depressions, and extracts sulfuric acid from the soil, thus creating acid springs.
- These acid springs may be cool, warm, or hot, depending on whether they are steam heated.
- Most of the hot acid springs exist only as pools that do not have outflow channels, but flow by underground seepage.

Bill Doemel's work on *Cyanidium* had gotten me interested in acidic springs and effluents, but I had done no work myself until my accidental discovery of acid mine drainage in Indiana strip mines.

# First work on acid hot pools in Yellowstone 1969

Gary Darland joined my lab as a post-doctoral fellow in early 1969 and was in Yellowstone for that summer. His first project was to study bacteria living in hot acid waters, and he and I made an extensive survey of those springs in Yellowstone. In that work, we sampled dozens of springs, measuring temperature and pH, and collecting water samples for microscopic examination. The intent of this work was to see if bacteria lived at high temperatures under low pH conditions.

This led to a paper Darland and I published entitled “The limits of microbial existence: temperature and pH.” Brock TD, Darland GK. 1970. *Science* 169(952):1316-8. The conclusion was that, as far as low pH, bacteria were absent from very high-temperature acid sites (>80 C), but were present in all neutral to alkaline sites even up to the boiling point. As it turned out, this study was done before we had discovered *Sulfolobus*, which was probably present in some of the low pH/high temperature springs with temperatures below 90 C, but was missed because of its unusual morphology.

However, it still appears to be true that no acidophiles live at temperatures greater than 90 C. (Horikoshi, K., A. Antraanikian, A.T. Bull, F.T. Robb, and K.O. Stetter (editors). 2010. Extremeophile Handbook. Springer-Verlag, New York.)

# Sampling an acidic hot spring in the Norris Geyser Basin

*Gary Darland sampling at Emerald Pool, an acidic pool, in the Norris Geyser Basin; July 23, 1969.*

***PICT0239.jpg***

# One of the many hot, acid pools at Norris Geyser Basin

*Gary Darland (with Yellowstone National Park ranger as observer) sampling at one of the numerous hot acid springs in the Norris Geyser Basin. Norris has a small museum generally staffed by a ranger during the summer. There was a lot of interest in our work and the Norris rangers often tagged along. Photo July 23, 1969*

***PICT0240.jpg***

# The limits of microbial existence, temperature and pH

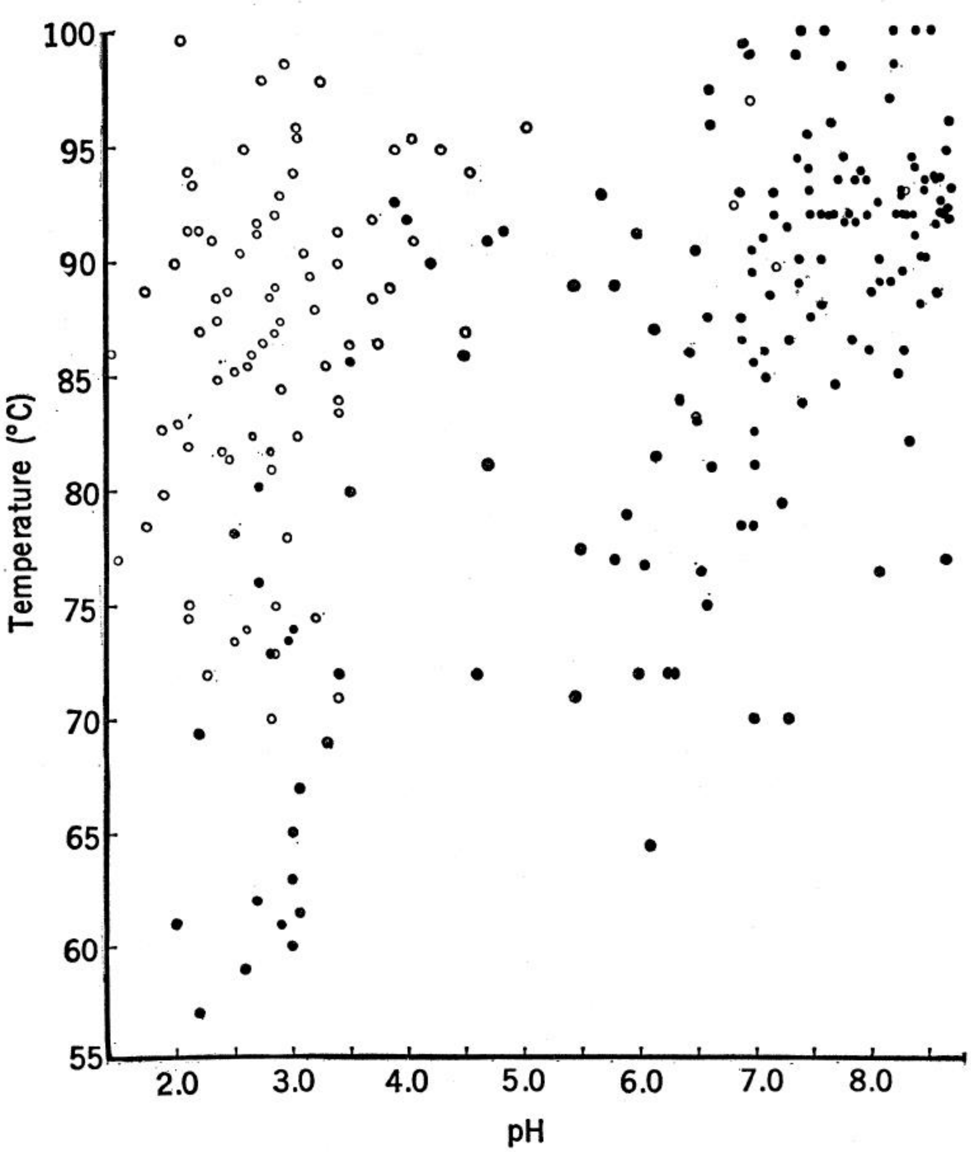



Fig. 1. Presence of bacteria in hot springs of various temperature and *p*H characteristics. Each dot represents the results for a single spring. (Open circles) Bacteria absent; (solid circles) bacteria present. Data for Icelandic studies or for springs with *p*H greater than 8.5 are not given.

***Brock Darland Science Fig 1.jpg***

## Coal refuse piles and self-heating thermal environments

The southwestern corner of Indiana had quite a few coal mines, some fairly large. They were about an hour's drive from Bloomington and hence quite easy to get to. These mining processes can lead to the formation of very low pH effluents. The phenomenon is called **acid mine drainage** (AMD) and is widely known as a serious pollutant in the coal mining regions of eastern U.S.

Indirectly, I became acquainted with Charles Medvick, who was a forester with the Indiana Coal Association. Medvick was involved in restoration of lands that had been damaged by strip mining. In the summer of 1968, when I was back in Indiana briefly, Medvick gave me a tour of the Indiana strip mine area, showing me some of the habitats.

The Indiana coals that I studied were bituminous ("soft") coals that were removed by surface mining. They were fairly high in sulfur, which was present primarily in the form of pyrite, an iron sulfide mineral. Before it is used, most of the pyrite is removed mechanically and deposited in large coal refuse piles (called "gob piles" by the miners). When pyrite is exposed to oxygen of the air, it oxidizes, either spontaneously or through microbial action, leading to the production of sulfuric acid. Water seeping down through gob piles from rain or snow leaches out the sulfuric acid, together with iron and other minerals, and flows into nearby streams or rivers. These effluents often have pH values as low as 1.8-3 and coal mining is a major source of water pollution.

The discovery of acid mine drainage in Indiana was an important step on my way to the discovery of thermoacidophiles. It was fortuitous, as I was simply looking for sites near home where I could do research of possible relevance to my Yellowstone work.

My first view of the Indiana coal mining area was in July 1968 when I was back in Bloomington briefly. However, it was not until September 1969 that I discovered the self-heating phenomenon in coal refuse piles, and realized that I had here in Indiana habitats not only of low pH but of high temperature that might contain thermophilic acidophiles such as are present in Yellowstone. Self-heating occurs in the coal refuse piles because the oxidation process produces heat, and the heat is trapped within. Under certain conditions, the temperature can get high enough that the residual coal material in the pile can catch on fire. One can recognize a self-heating pile because it either liberates steam or smoke.

According to my Indiana photographic dates, I visited the gob pile at the Friar Tuck Mine in September 1969, after the summer when Darland's acidic spring work had been done. In this gob pile self-heating was observed with temperatures around 55 C. I took photos of the steam emanating from this gob pile. In one photo a thermistor is visible. I remember being stunned to find steam and moderately high temperatures in southern Indiana. Here was a "real" connection to Yellowstone.

Either Gary Darland was present on the first gob pile trip, or I brought samples back to Bloomington for him to culture. Since he had been involved in the Yellowstone low pH

work from the previous summer, it was logical that he should be the one to try to culture thermoacidiphiles from the self-heating coal refuse material.

The Friar Tuck Mine provided the sample from which the first culture of *Thermoplasma* was isolated.

Darland isolated *Thermoplasma* during the late fall 1969 and studied it for most of the winter. When we began to think that this new organism might be a mycoplasma, I arranged for the electron microscopy (EM) to be done by my friend Sam Conti (then at the University of Kentucky), who I had worked with on other EM projects. Sam assigned his student Bill Samsonoff to this project. The EMs were very convincing. We published the paper in *Science* on Dec. 25, 1970. The authors were Darland, Brock, Samsonoff, and Conti.

# Dragline used to remove the overburden to reach the coal seam


*My microbial ecology class visiting the Peabody Coal mine in SW Indiana. This is a "drag line" which removes the coal overburden so that the coal seam can be reached. Once the coal is removed, the overburden is replaced. When I was working here in the late 1960s, restoration of the overburden was in its primitive stages, and was often not done at all. March 14, 1970.*

***PICT0130.jpg***

# The coal seam with its sulfur-rich coal

*Removing coal from the coal seam. The coal is transported to a processing mill where the sulfide-rich material is removed by gravity. The coal refuse material (called a "gob pile") is piled separately from the overburden. Sulfur- and iron-oxidizing bacteria oxidize the sulfide-rich residue, creating sulfuric acid ("acid mine drainage"). Self-heating may also occur, increasing the temperature of the gob pile. March 14, 1970.*

***PICT0131.jpg***

# Iron sulfide-rich coal refuse pile

*My first coal refuse pile, colloquially called a "gob" pile. The black material is the iron- and sulfide-rich part of the coal seam that is removed from the coal at the mine. At the time I was working here, these areas were open to the public without restrictions. March 27, 1971*

***PICT0120.jpg***

# Self-heating coal refuse pile, a source of *Thermoplasma*

*Typical self-heating location in a gob pile. Steam is usually seen best when the site is backlighted by the sun and the humidity is high. Sometimes the fuel becomes ignited, leading to flames and smoke. Photo April 1, 1969*

***PICT0123.jpg***

# Sampling a self-heating coal refuse pile

*I often took my whole lab to the gob piles. Here Carl Fliermans and Dave Smith are taking samples. The thermistors we used in Yellowstone came in handy here. Feb 27, 1971*

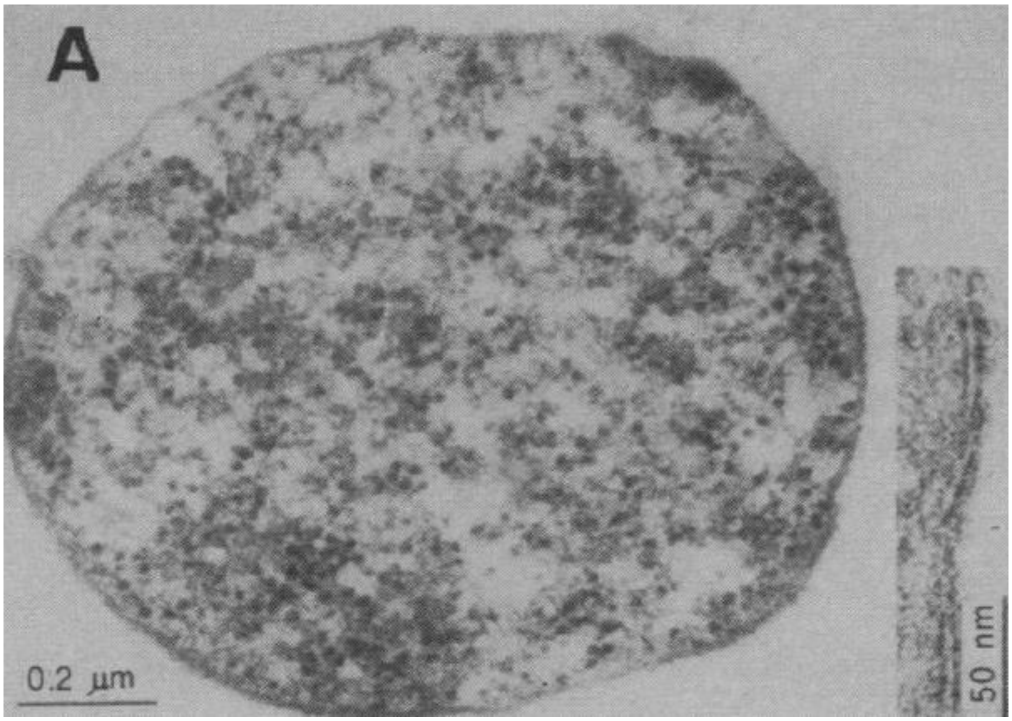
***PICT0122.jpg***

# Class carrying out a radioisotope experiment in an area of high acidity

*I used these sites for microbial ecology class experiments. Here is the class (and teaching assistant) carrying out a radioactive carbon fixation experiment. March 21, 1970*

***PICT0129.jpg***

# Electron micrograph of Thermoplasma acidophilum




*Electron micrograph of a thin section of* Thermoplasma acidophilum, *taken from the original paper in* Science *(Darland, Brock, Samsonoff, Conti 1970). There is no cell wall, like other mycoplasmas. However,* Thermoplasma *is an Archea and unrelated to the other mycoplasmas.*

***Thermoplasma EM Science paper.jpg***

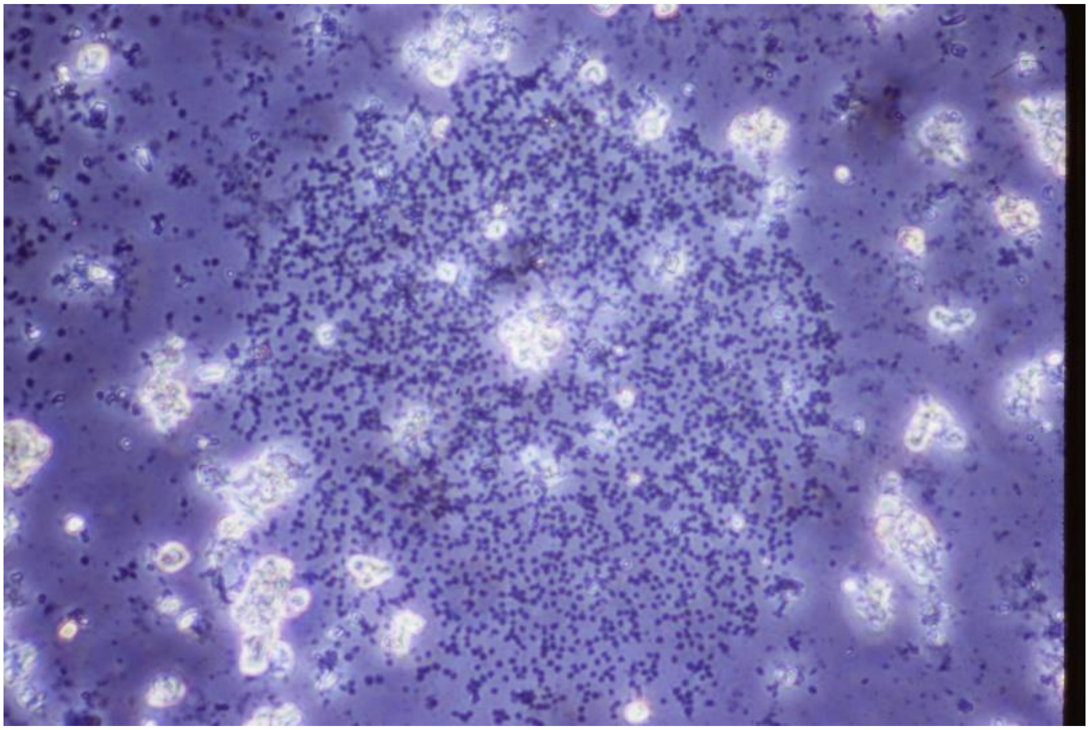
Bob Belly joined the laboratory at Bloomington as a post-doc in the fall of 1970 and took over the *Thermoplasma* work after Darland had left for a teaching job in California. Although Belly did mostly laboratory work, he was present on visits we made to the Indiana strip mines. Also, Belly was in the West Yellowstone lab the summer of 1971 when we did a lot of work looking for *Thermoplasma* in acid springs.

One of my (few) regrets of moving from Indiana University to UW-Madison was that I was no longer be able to visit strip mines. Later, I discovered a good strip mine area in Illinois (3-hour drive from Madison) and Belly and I were able to finish a nice study there. (See Belly and Brock, 1974). Also, Belly did a lengthy trip to western Pennsylvania where there were many coal mines with gob piles. Although he was unsuccessful in isolating *Thermoplasma* in Yellowstone, he succeeded in finding a lot of new isolates from Pennsylvania gob piles. This work, and other *Thermoplasma* work done with post-doc Ben Bohlool, was published in a paper that Belly gave at a *Thermoplasma* conference in New York. (Belly, Bohlool, and Brock 1973. Annals of the New York Academy of Sciences. See bibliography for complete citation)

Although *Thermoplasma* is a very interesting organism, it is only a moderate thermophile, with an upper limit about 65 C.

Sam Conti was quite excited about all the new organisms we were finding at Yellowstone and arranged for his student Bill Samsonoff to spend most of the summer of 1970 with us. Bill focused on bacteria living at high temperature and low pH. One site he got interested in was Roaring Mountain, where there is a pH 2.0 effluent that starts at 88 C and can be followed down until it is at ambient temperature. Bill used my slide immersion technique and found spherical-shaped cells forming fairly good colonies at temperatures in the mid 80s C. When I looked at his slide, I assumed that he had found a high-temperature form of *Thermoplasma*. However, Roaring Mountain is a sulfur-rich area, and *Thermoplasma* does not grow on sulfur (although it does reduce it to hydrogen sulfide.

# The first view of Sulfolobus



*Phase photomicrograph taken at the West Yellowstone laboratory. The slide had been immersed in hot acid water at Roaring Mountain (88 C, pH 2.0) for over a week. The cells are not Thermoplasma but Sulfolobus (lobed spheres). July 31, 1970*

***PICT0209.jpg***

At the same time, I was setting up cultures from other hot acid springs, using organic nutrients or elemental sulfur as energy source. Since my samples had come from springs in the high 70s or 80s, I was using 75 C as the temperature for my cultures.

Soon I was getting good growth of organisms that looked like those on Samsonoff's slides. All the cells in my cultures were somewhat spherical, but not completely round like *Thermoplasma*.

I isolated *Sulfolobus* from Yellowstone habitats in the summer of 1970. I also sampled hot acid areas in El Salvador in August 1970 after attending the International Congress of Microbiology in Mexico City, and these samples also yielded *Sulfolobus* cultures.

When I went to Italy in October 1970 I learned that the scientists in the Naples laboratory had also isolated *Sulfolobus* but did not know what it was. They also were not aware of my work. In my two weeks in Italy I was isolated more cultures from samples taken from Pisciarelli, a thermal area on the opposite side of the small mountain that flanks Solfatara, an historic acid thermal area known since Roman times.

In the fall/winter 1970 and spring of 1971 my IU lab was working on both *Sulfolobus* and *Thermoplasma*. A major focus was on the cell wall. We did EM work on *Sulflobus* (Richard Weiss), biochemical analyses, and antibiotic sensitivity (Kathie Brock) to show that *Thermoplasma* had no cell wall and that *Sulfolobus* had an atypical cell wall. When we returned to Yellowstone in the summer of 1971 we had already clarified the distinction between these two organisms, and started to work on the ecology of *Sulfolobus*.

Also in the summer of 1971, Bob Belly made valiant but unsuccessful attempts to isolate *Thermoplasma* from hot acid springs. (Some years later, American and Japanese groups were successful in isolating *Thermoplasma* from hot springs.)

By the end of the summer of 1971 I had good cultures of *Sulfolobus*, not only from Yellowstone but from other thermal areas in other locations.

In December 1971 Kathie and I visited a large hot acid area in Dominica (Caribbean) and samples from there also led to good cultures. Further isolates were made from Iceland and the Caribbean. See table for summary.

Locations where samples were collected that yielded *Sulfolobus* cultures

| **Country** | **Location** | **Details** |
|---|---|---|
| U.S.A. | Yellowstone | Congress Pool |
| U.S.A. | Yellowstone | Growler Spring |
| U.S.A. | Yellowstone | Locomotive Spring |
| U.S.A. | Yellowstone | 7 other Norris hot acid springs |
| U.S.A. | Yellowstone | Roaring Mountain |
| U.S.A. | Yellowstone | 2 at Amphitheater Springs |
| U.S.A. | Yellowstone | 6 at Sylvan Springs |
| U.S.A. | Yellowstone | 2 in Canyon area |

| | | |
|---|---|---|
| U.S.A. | Yellowstone | 2 in Mud Volcano area |
| U.S.A. | Yellowstone | 4 in Shoshone Geyser Basin |
| El Salvador | Ahuachipan | 3 at Sauce thermal area |
| Italy | Agnano/Solfatara | 4 at Pisciarelli |
| Iceland | Kerlingarfjoll area | 2 unnamed springs |
| Iceland | Geysir area | 2 unnamed springs |
| Iceland | Krisuvik area | 5 unnamed springs |
| Dominica | Valley of Desolation | 4 unnamed springs |
| Dominica | Wotten Waven | 2 from hot soil |

Later, Ben Bohlool isolated cultures from New Zealand and showed that some of them were the same as cultures from Yellowstone.

The first *Sulfolobus* paper was published in early 1972 in Archiv fur Mikrobiologie (see bibliography).

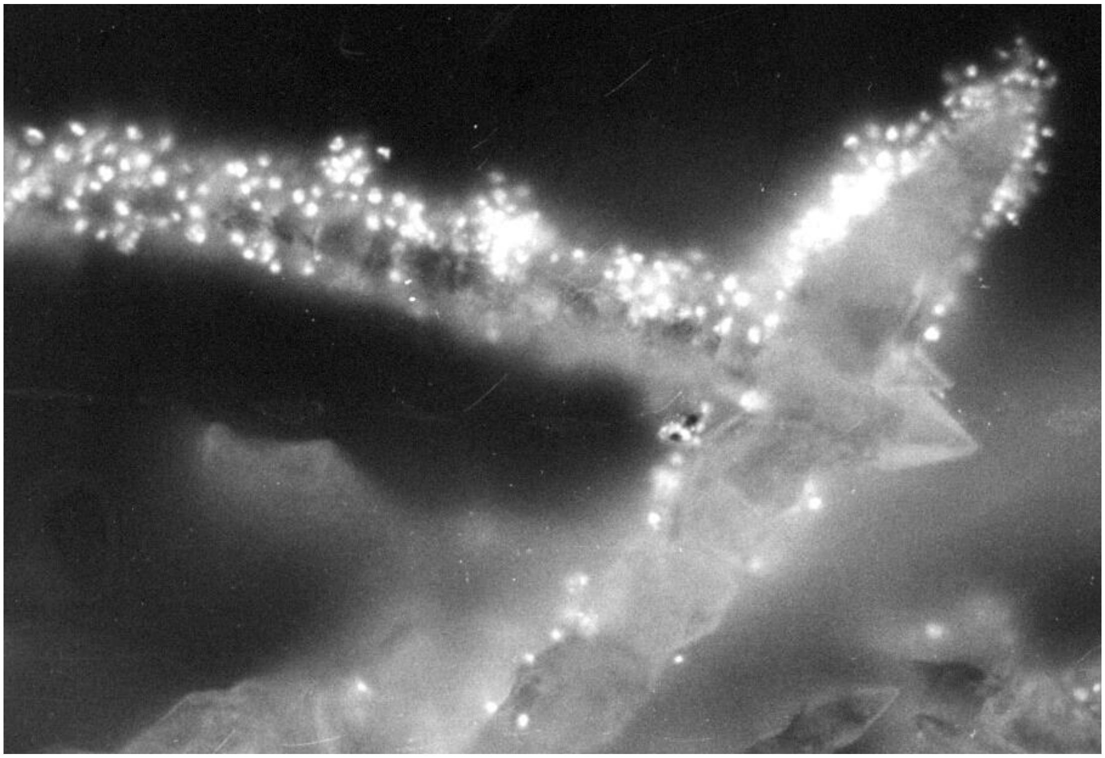
In the summer of 1971, as the first *Sulfolobus* work was wrapping up, Carl Fliermans was in the middle of his studies in Yellowstone on the ecology of sulfur-oxidizing bacteria in hot acid soils. He was able to show using the radioactive carbon fixation method that autotrophic bacteria were very active in these thermal soils. Also, he cultured *Thiobacillus* (at temperatures around 50-55 C) and *Sulfolobus* (at temperatures above 60 C). Fliermans' work was published in August 1972.

Geochemists studying the origin of sulfuric acid from elemental sulfur had shown that this was due to biological activity, but the only organism known to do this was *Thiobacillus thiooxidans*, which is active only at temperatures below 50 C. *Sulfolobus* was widespread in much hotter acid areas, and is responsible for sulfur oxidation at those temperatures.

By the fall of 1971 my lab had moved from Bloomington to Madison. In addition to the students and post-docs who came with me (Bauld, Smith, and Belly), I hired a new post-doc, Douglas Shivvers, who had both a geological and microbiological background. I assigned him the problem of measuring oxidation of elemental sulfur by *Sulfolobus* cultures using radioactive sulfur. He showed that *Sulfolobus* oxidized elemental sulfur to sulfate (sulfuric acid). He also showed that cells of *Sulfolobus* attached to sulfur crystals as they grew, and that at the end of the incubation, 10 times as many cells were attached than were floating free. (Shivvers and Brock, Journal of Bacteriology, Vol. 114; pp. 706-710; 1973).

In the summer of 1972 Jerry and Anne Mosser demonstrated that *Sulfolobus* is an important biogeochemical agent in hot acid springs and solfatara soils.

# Bacteria (*Sulfolobus*) attached to elemental sulfur crystal



Sulfolobus *cells attached to sulfur crystals. The cells have been stained with a fluorescing dye and then viewed with a fluorescence microscope. Such images can be seen either in laboratory cultures or in samples of sulfur collected in nature. It is now known that the cells attach to the sulfur crystals by fibrous structures called pili. This is what could be called a "textbook" photo! I took this on June 20, 1973.*

***Sulfolobus sulfur crystals 6-20-1973 N101.jpg***

# Typical vapor-dominated area at Roaring Mountain

*Kathie Brock and Anne Mosser at a typical vapor-dominated thermal area: fumaroles and hot pools at Roaring Mountain. August 24, 1971.*

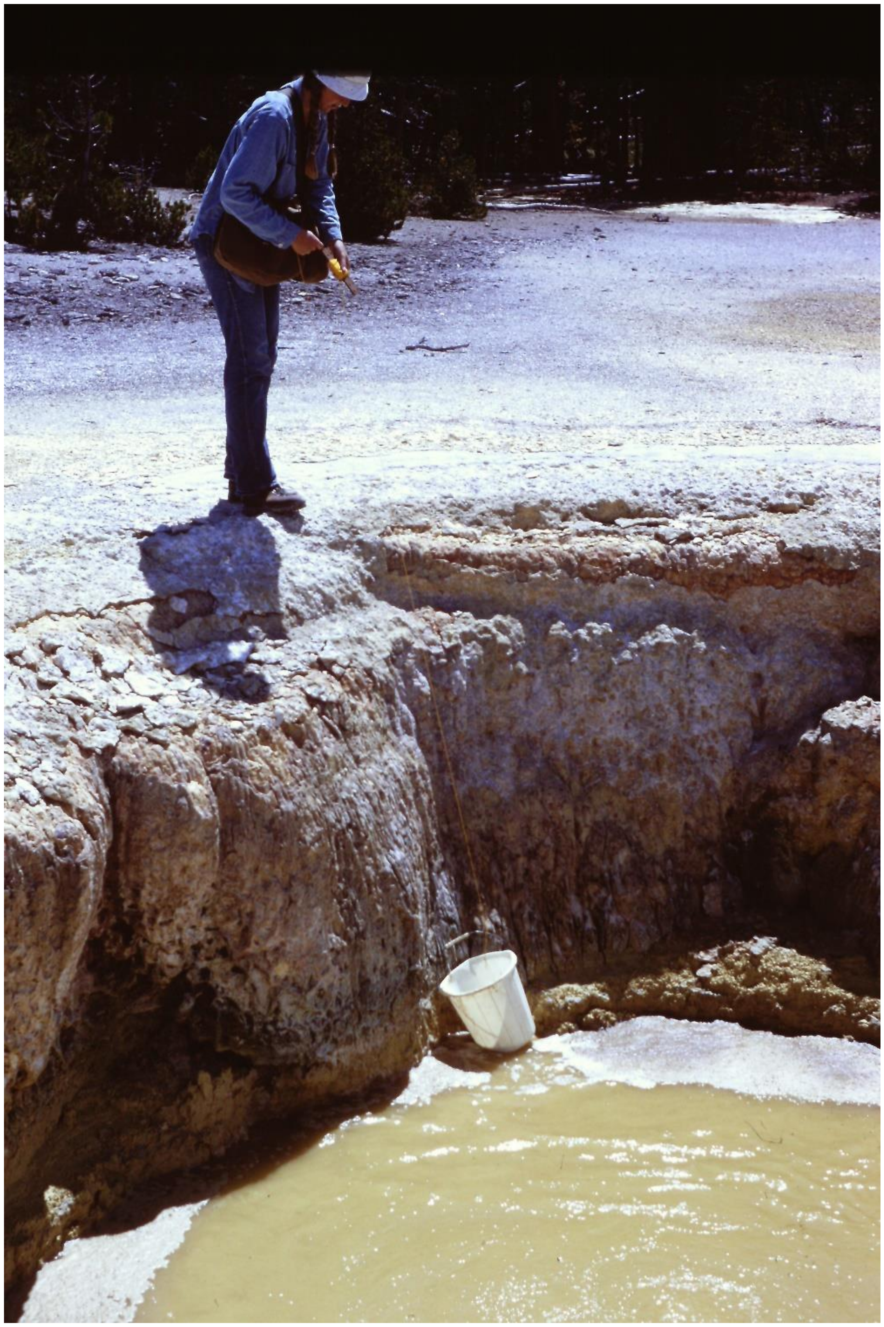
***PICT0244.jpg***

# Evening Primrose pool in the Sylvan Springs area; 1972

*Anne Mosser measuring the temperature of Evening Primrose pool in the Sylvan Springs area. August 16, 1972. At this time the temperature was >90 C, too hot for* Sulfolobus. *The pool was full of sulfur, and there were no bacteria. Sometime before the following summer of 1973, the temperature of the pool dropped to about 55 C.* Sulfolobus *then flourished, oxidized all the sulfur, and the pH dropped to less than 2. This event provides additional evidence that 90 C is above the upper limit for growth of* Sulfolobus.

***PICT0249.jpg***

# Sampling Evening Primrose Pool 1974



*Susan Entenmann sampling Evening Primrose pool, July 31, 1974. This was after the temperature had dropped and the sulfur was all oxidized to sulfuric acid by* Sulfolobus.

***PICT0233.jpg***

## Summary of the early *Sulfolobus* work

The *Sulfolobus* work proceeded in the following sequence: Study of its ecology in summer 1970 (Samsonoff; Brock) and determination of the parameters of pH and temperature in nature by direct microscopy (Brock); isolation of cultures and showing that the isolated organism was different than *Thermoplasma* (Brock, Brock, Belly, and Weiss); demonstration that *Sulfolobus* would grow both heterotrophically on organic compounds and autotrophically on elemental sulfur (Brock); study of its ecology in solfatara soils (Fliermans); demonstration that it would oxidize radio-labeled elemental sulfur in cultures (Shivvers); use of the radio-label technique to demonstrate elemental sulfur oxidation in nature (Mosser and Mosser).

The steps just presented demonstrate the way microbial ecology works; starting in nature, moving into the lab, and moving back and forth from the lab to the field as the work progresses.

Ben Bohlool had joined my lab in the fall of 1972 from the University of Minnesota. Ben had been working with soil microbiologist Ed Schmidt on the use of fluorescent antibodies in microbial ecology. I set him up with the facilities for making antibodies in rabbits against *Thermoplasma* and *Sulfolobus*. He did quite a bit of cross typing between various strains of each of these two genera, and published papers on this. In addition, he used fluorescent antibodies to study the ecology of *Thermoplasma* in the coal refuse piles. Some of the *Thermoplasma* work was done with Bob Belly. (See papers: Belly, Bohlool, and Brock, Annals NY Academy Sciences 1973; Bohlool and Brock Archiv fur Mikrobiologie 1974; Mosser, Bohlool, and Brock J. Bacteriol 1974; Bohlool and Brock Applied Microbiology 1974; Bohlool and Brock Infection and Immunity 1974).

Ben also spent the summer of 1973 in Yellowstone, where he worked with Jerry and Anne Mosser, Jim Zabrecky (high school student from the Explorer's Club), John Connor (from Wabash College) and me on *Sulfolobus* work primarily at Sylvan Springs. This latter work was a classic example of integrating field and laboratory work and is discussed below.

Mosser, Bohlool, and Brock 1974: Growth rates of *Sulfolobus acidocaldarius* in nature: J. Bact 118, 1075-1081; Brock and Mosser 1975. Rate of sulfuric acid production in Yellowstone National Park. Bull. GSA 86: 194-198.

The following is an outline summary of the *Sulfolobus* work in Yellowstone, both in the field, and in the West Yellowstone laboratory.

1. Periodic measurements of chemistry and bacteriology of the hot acid springs showed that they are steady state systems, even though there was no measurable inflow or outflow of thermal water.

2. Concluded that the growth of *Sulfolobus* in these hot acid springs is analogous to the steady state growth of *Synechococcus* in Mushroom Spring, studied in 1968-69.

3. How to measure flow rate? Could add a tracer material and measure its disappearance

4. Chloride ion was chosen because of its high water solubility, stability at high temperature and low pH, low volatility, low cost, and available in large quantities in the field area, and lack of toxicity to *Sulfolobus*. Also, all acid springs had relatively low natural levels of Cl, 1-30 mcg/ml. The stability and lack of volatility of Cl at the temperature and pH of the springs was shown (by Mosser) in preliminary lab experiments at the West Yellowstone lab. Toxicity was tested using lab cultures. Also, Cl was not absorbed onto solfatara soil and thus would not remain in the system for a long period.

5. **The importance of the West Yellowstone lab in the acid thermal studies.** The use of Cl as a tracer was first conceived after we were already in West Yellowstone. We needed to do a lot of lab work to show that it was a valid tracer. Our West Yellowstone lab was well supplied with chemicals and equipment so we could do the necessary experiments.

6. Chloride in the form of table salt is cheap and widely available. There was not enough table salt in West Yellowstone for all the work we were doing. (We needed 100-pound bags.) Bill and Nancy Doemel were on a shopping trip to Bozeman and bought what we needed.

7. The assay method for chloride is a titrimetric method using silver nitrate as the titrant and phenolphthalein as the indicator. We got the chloride method from the American Public Health Association manual (a widely available source). We looked up the method in the MSU library and obtained the reagents and glassware (burette) from Chemical Stores at MSU. The work was done in 1973 when Mossers and Bohlool were at West Yellowstone. The titration assays were done by John Connor, who was one of the Wabash College students (with Doemel) that we had that summer. We did lots of baseline studies first, since there are a number of things that can interfere with the chloride assay. Sulfide had to be eliminated by adding peroxide and the pH had to be adjusted to 7-10. Titration was with silver nitrate and the end point was a pinkish yellow end point.

8. The first titrations after adding salt were done in the field, but subsequent titrations were done at the West Yellowstone lab (chloride is stable indefinitely). A 100 ml water sample was used. 1 ml of peroxide was added to oxidize reduced sulfur compounds. The pH had to be adjusted to 7-10. The titration indicator was phenolphthalein. The titrant was 0.0141M silver nitrate (2.395 g AgNO3 in 1000 ml distilled water. 1 ml = 500 mcg Cl. Silver nitrate is light sensitive so the titrant had to be stored in a brown bottle.

9. How much salt to add? Most Yellowstone springs are low Cl. The surface area and depth were measured for the springs to be studied. We then calculated how much salt to add raise the chloride concentration to 100-400 mcg/ml (about 10 times background).

10. See the Mosser, Bohlool, Brock paper for details of how the mixing was done.

11. Adding salt to the huge Moose Pool required six people (Mosser, Bohlool, Zabrecky, Connor, Weller, Doemel) and the brine was distributed by throwing into the pool with plastic buckets. July 1973

12. Chloride dilution rates were done for 12 springs. The rate of dilution was exponential. Figure 1. Half times varied from 10 to 27 hrs in the small springs and 28-35 days for the two large ones (Moose, Sulfur Caldron).

13. Draining and refilling. This was an alternate approach and added significant new information. It was done at Sylvan Springs, where we had complete control of the habitat. This was an ideal place to work on the biogeochemistry of hot acid springs. There were dozens of small and large springs, mostly pH 2-3. Siphoning was Bohlool's idea. The small springs studied were on a gentle slope so it was possible to siphon the water out of a small spring and down the hill. We brought a long hose, filled it with spring water, and ran it down the hill. The upper end of the hose was placed into the bottom of the spring and the siphoning began. The drainage was complete within less than 0.5 hr. Washed out the bottom of the spring with *Sulfolobus*-free water. Spring water started refilling almost immediately. The first fresh water that returned was free of *Sulfolobus*, but due to new growth the cell numbers returned to normal within a day, or several days. *Sulfolobus* growth rates in the siphoned springs were much faster than steady state.

# Measuring the depth of a large hot spring

*Weighted line used to measure the depth of a large hot spring (Moose Pool), to determine volume for salting. Tom Brock, Anne Mosser, Jerry Mosser, August 16, 1972*

***PICT0206.jpg***

# The chloride titration apparatus in the field

*Chloride titration apparatus set up adjacent to Moose Pool. June 27, 1973*

***PICT0205.jpg***

# Mixing the brine

*Mixing brine, in preparation for "salting" a very large spring. The buckets were used to bring water into the large garbage pail. The salt was mixed with a canoe paddle. Anne Mosser, Ben Bohlool, and Jim Zabrecky; 8-27-1973*

***PICT0237.jpg***

# Tossing brine into the hot spring

*Ben Bohlool "salting" Moose Pool 6-27-1973. A crew of six working simultaneously completed the whole job in 15 minutes.*

***PICT0236.jpg***

# Siphoning the water out of a small hot spring

*Ben Bohlool completing the job of draining (by siphon) all the water in a small hot acid spring in the Sylvan Springs area in order to observe regrowth. Photo July 24, 1973*

***PICT0235.jpg***

# Changes in chloride, chemistry, and *Sulfolobus* in White Bubbler Spring

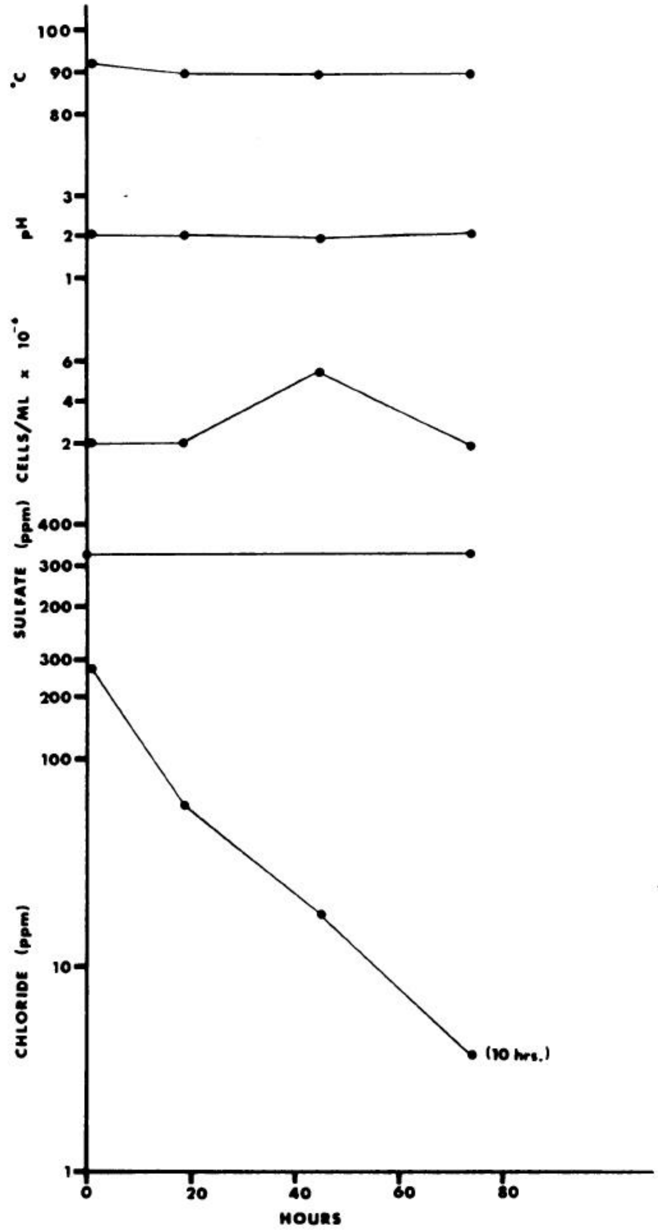



FIG. 1. *Change in concentration of chloride with time after addition to White Bubbler Spring. The estimated half-time for chloride dilution is shown in the parentheses. Also presented are data for sulfate, temperature, pH, and concentration of Sulfolobus cells.*

***Sulfolbus growth rate Fig 1.jpg***

# Changes in chloride in a series of hot springs

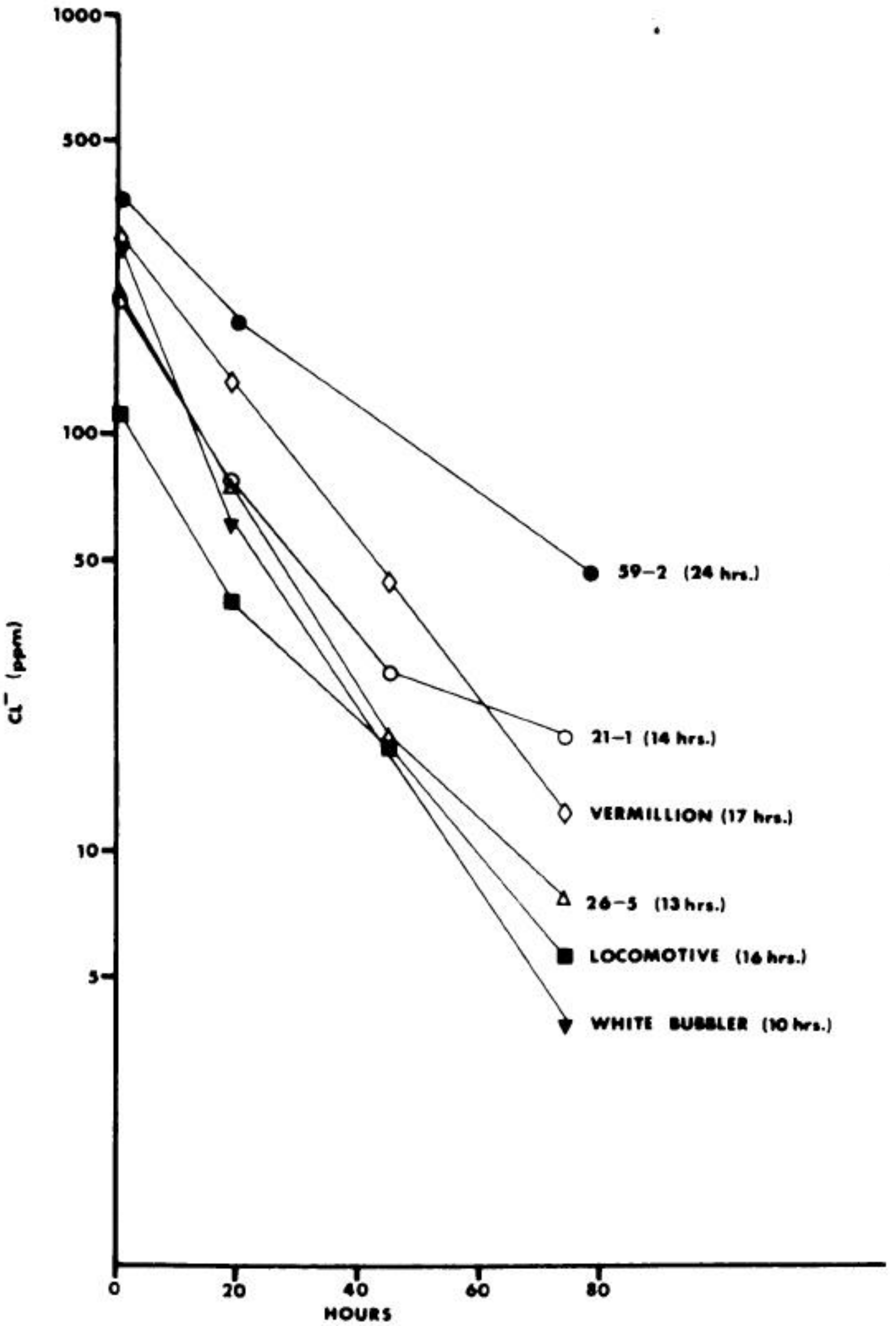



FIG. 2. *Chloride dilution in several small springs. Estimated half-times for chloride dilution are given in the parentheses. At site 21-1, chloride concentration had reached the natural background level by the final sampling time, and the dilution rate was estimated from the data from the first three sampling times.*

***Sufolobus growth rates Fig 2.jpg***

# Regrowth after removing water from small hot springs

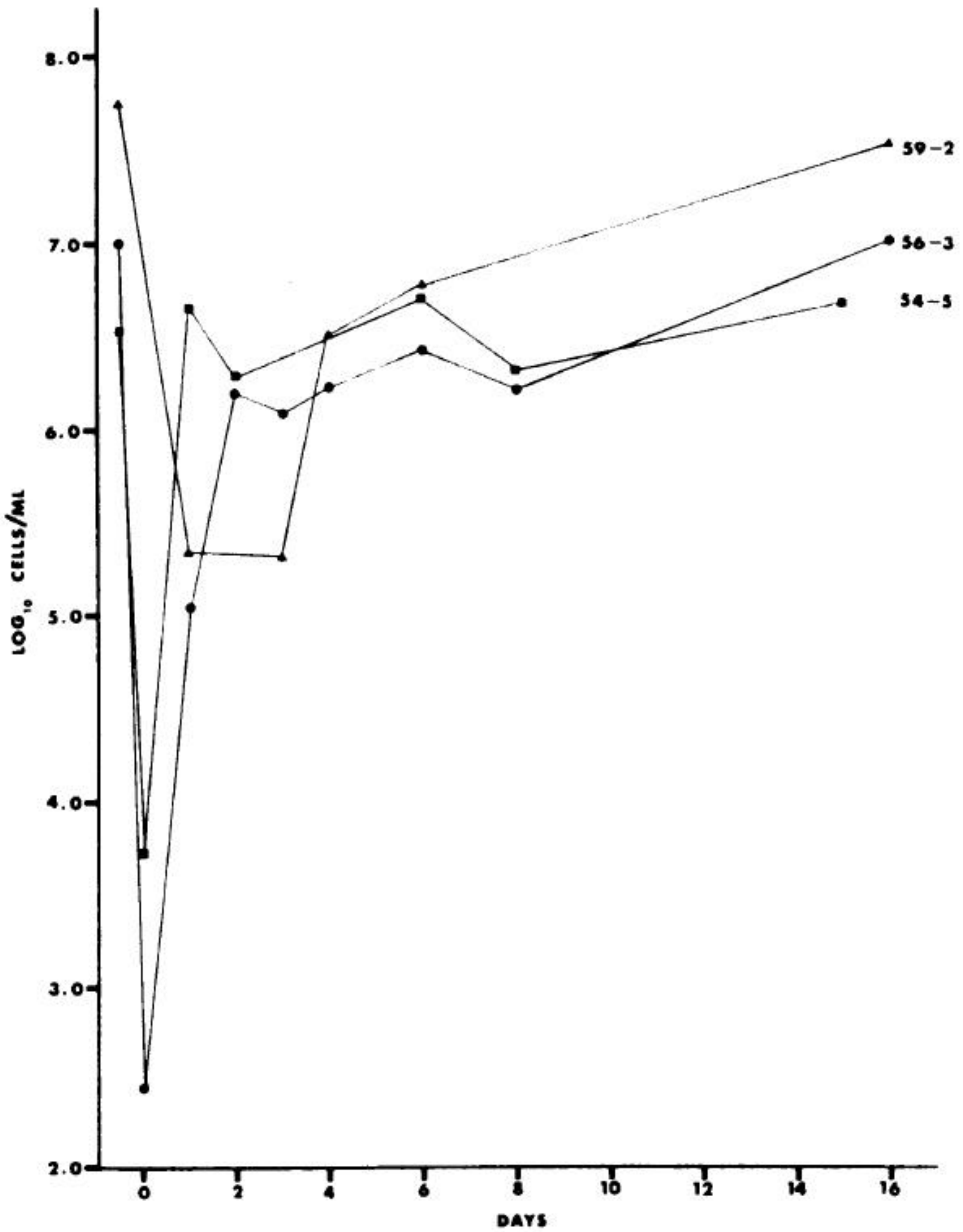



FIG. 5. *Rate of increase of cell numbers in three springs which were drained, cleaned, and allowed to refill. Spring 56-3 did not contain Sulfolobus but contained a high-temperature Thiobacillus. Data for this spring are given for comparison.*

***Sulfolobus growth rates Fig 5.jpg***

**Mechanism and rate of sulfuric acid production in Yellowstone National Park**

In addition to its microbiological interest, the *Sulfolobus* work just discussed had considerable interest to geologists and geochemists.

The work of Anne and Jerry Mosser using radioactive sulfur and Carl Fliermans studying the ecology of hot acid soils, provided real insights into the mechanism of sulfuric acid production.

All of this work led to the final paper that Jerry Mosser and I published in the Bulletin of the Geological Society of America (86: 194-198 (1975). Here is the Abstract:

> *"Production of sulfuric acid in vapor-dominated hydrothermal systems is primarily a bacterial process. The rate of production of sulfuric acid was measured in springs in several acid-altered areas in Yellowstone National Park. Most of these springs lack surface water flow, but water enters and leaves these springs at approximately constant rates via underground seepage. The rate of water exchange in these steady-state systems was measured by enriching the springs with sodium chloride and measuring the rate at which the chloride ion was diluted. In all cases, the added chloride was diluted at an exponential rate, and half-times for dilution were calculated. The rate at which sulfuric acid was being produced was calculated from a knowledge of dilution rate and volume of the springs and from measurement of the sulfuric-acid concentrations of the waters. In several small springs, flow rate was measured more directly by draining the springs and measuring the rate at which water returned.*
>
> *These studies showed that water in acid springs enters as cold acid ground water, which is steam heated within the source pool. It was possible to estimate how much of the sulfuric acid in a given spring could have been produced in situ, and how much entered by underground seepage. In springs with pool volumes of 2,000 liters or less, most of the sulfuric acid was produced outside the spring, probably by bacteria present in the nearby acid-altered soil. In springs with pool volumes around one million liters, most of the sulfuric acid was produced in situ by resident bacterial populations. The techniques used may have wider utility in biogeochemical investigations."*

**Ferrous iron oxidation by *Sulfolobus***

The final major work on *Sulfolobus* in Yellowstone was its role in oxidation of ferrous iron and reduction of ferric iron. This work was done under my overall direction but more specifically supervised by Jerry Mosser. Two undergraduates were responsible for most of this work, Sandra Petersen, who had been working in my laboratory in Wisconsin, and Susan Entenmann (later Cook), who came to us at Yellowstone from North Carolina State University via the Explorer's Club of New York.

This work showed that *Sulfolobus* was able to oxidize ferrous iron to the ferric state under aerobic condition, and to reduce ferric iron to ferrous under anaerobic conditions.

Iron-rich hot springs, especially in the Norris Geyser Basin, are generally red, due to the presence of ferric oxides. At the low pH values and high temperatures, the oxidation of ferrous to ferric is almost always due to the action of *Sulfolobus.*

This work was published in the following paper: Brock, Cook, Petersen, and Mosser, (1975) “Biogeochemistry and bacteriology of ferrous iron oxidation in geothermal habitats.” *Geochimica et Cosmochimica Acta* 40: 493-500. This paper reports on much laboratory work that was done in the West Yellowstone field laboratory.

# Bacteria and the Grand Canyon of the Yellowstone

*The Upper Falls of the Grand Canyon of the Yellowstone River. This canyon shows dramatically the power of bacteria in geological processes. The sulfuric acid they produce causes rapid breakdown of the rocks, permitting the river to quickly carve the canyon. Photo taken June 29, 1974.*

***Canyon YNP 6-29-1974 N181.jpg***

# Sulfur Caldron, in the Mud Volcano area

*Sulfur Caldron, one of the most acidic thermal features in the Park. The Grand Loop Road and parking lot above are supported by the stone wall. Steve Zinder sampling. Photo taken July 22, 1975.*

***Sulfur Caldron Zinder 7-22-1975 N223.jpg***

# Yellowstone 1972

**1972 school year.**

Douglas Shivvers came for a one-year post-doctorate, and did studies on oxidation of radioactive sulfur by *Sulfolobus*. Kathy Boylen did a lot of projects, concentrating mainly on isolating *Thermus aquaticus* from Madison laundries, but also doing thermophiles in hot food and looking for *Thermoplasma* in human stomachs (unsuccessful). Undergraduate Beverly Mejchar helped Kathy Boylen second semester, after doing dishes first semester. Bob Belly took a trip to gob piles in Pennsylvania and got lots more *Thermoplasma* strains. [See these papers: Belly RT, Bohlool BB, Brock TD. 1973. The genus *Thermoplasma*. Annals of the New York Academy of Sciences 255:94-107; Belly RT, Brock TD. 1972. Cellular stability of a thermophilic, acidophilic mycoplasma. J. Gen. Microbiol. 73(3):465-9]

Dave Smith and John Bauld continued analyzing the work they had done the previous summer in Yellowstone, and finished prelim exams. Dave Ward began studies on hydrocarbon oxidizers in Lake Mendota, which he carried through the summer and all the way to his Ph.D. (1975). Chuck Boylen finished analysis on his Firehole River data and did two projects on Lake Wingra through the ice (in winter 1971.

**Summer of 1972.**

Jerry Mosser joined the group after the American Society for Microbiology meeting, taking over day-to-day responsibility for the Yellowstone lab. He drove the Ford mobile lab packed full of supplies and equipment and towed a U-Haul trailer behind. Jerry's wife Anne drove their vehicle, following along behind Jerry. We also had lots of material shipped directly from laboratory supply houses. We saved considerable money on transportation by leasing a second station wagon (Ford) from Bozeman Motors for $5.00/day and $0.04 per mile. Jerry and Anne Mosser and Dave Smith opened the Yellowstone lab around May 10, 1972.

Kathie and I went to Europe on May 18, 1972, spent some time in Italy and Switzerland, two days in Paris, three days in London, a week in Norway, a week in Uppsala at a meeting on Methods in Microbial Ecology, then on to Iceland for two weeks in the interior and on Surtsey. The Iceland trip was very exciting, and we did a lengthy Land Rover trip behind the glaciers and into the major thermal area of Kjerlingafjoll. We found more *Sulfolobus* habitats, worked on high temperature algae, and finished the Surtsey work (published in the Scandinavian ecology journal *Oikos*). Returned to Madison July 8 for a week, then on to Yellowstone by car. Because of the Europe and Iceland trips Kathie and I were only in Yellowstone about five weeks, but the lab ran smoothly under Jerry Mosser's supervision.

Other summer people were summer post-doc Jack Meeks (from Castenholz lab), and Mike Sininsky (a Junior Fellow at Harvard out of Konrad Bloch's lab, who was on his own). John Bauld and Dave Smith were back and Bonnie Hodler (now Bauld) came out

July 1. Chuck and Kathy Boylen returned. Dave Smith's wife Velma washed dishes all summer. Anne Mosser was employed for the summer as a post-doctorate.

Bob Belly stayed in Madison, as did Beverly Mejchar and Dave Ward.

John Bauld drove out around mid May with Chuck and Kathy Boylen. Jack Meeks arrived in mid-June, as did Mike Sininsky. Malcolm Walter was in the lab for six weeks, leaving in late July.

Living arrangements: Jerry and Anne Mosser, West Park Apartments; Dave and Velma, same; John and Bonnie, same; Chuck and Kathy Boylen, house of Billy Smith's; Malcolm and Marilyn Walter, cabin at Bunkhouse Motel; Jack Meeks, pick-up camper parked on vacant lot near Catholic Church; Kathie and I, Daley's pink trailer; Mike Sininsky, a trailer of Mrs. Adams (formerly Mrs. Morris).

The group was very congenial this summer, perhaps the most congenial we ever had, and they got some significant work done. Jerry and Anne Mosser's project especially worked well and we got a nice paper in *Science* (Bacterial origin of sulfuric acid in geothermal habitats; *Science* 1974; 179: 1323-1324). Malcolm Walter's stromatolite work was fairly exciting, and a week visit from distinguished biogeologist Preston Cloud put the cap on it. Cloud chartered a small airplane and we had a nice flight over the Park; Cloud, Walter, Jerry, and I. The pilot took the door off the plane next to where I was sitting so that I could get better air photos. I made sure my seat belt was well fastened and got lots of good air photos!

I did a little field work, but mainly kept on top of other people's work. I had been interested in pink snow algae since I had first seen them years ago. Jerry and Anne Mosser and I initiated some low-temperature work, seeking out frozen lakes at high altitude in the Beartooth's. Anne and Jerry did a preliminary experiment on temperature optimum for snow algal growth and we looked forward to further work in the summer of 1973.

We closed the lab August 25, 1972, although Kathie and I left five days earlier to attend the AIBS meeting in Minneapolis. I gave talks at the Phycology Society meeting on algae of very acid environments and at the Ecological Society of America on Boylen's work on the Firehole River.

**Fall 1972-Spring 1973**

John Bauld and Dave Smith finished up. Mike Madigan came on in the fall and took over the *Chloroflexus* work from Bauld. Iva Gurland typed. Jim Hoffman worked as technician and he also worked with Jerry Mosser on a very detailed study of temperature strains of *Sulfolobus*. Ben Bohlool arrived as a post-doc and worked with Belly on *Thermoplasma*, starting immunological work, and with Jerry on *Sulfolobus*.

# Kathie and Tom Brock; Yellowstone; 1972

*Kathie and Tom at outside the West Yellowstone laboratory, August 1972.*

***Kathie Tom large B-W Yellowstone Aug 1972 N060.jpg***

# Yellowstone 1973

## Summer 1973

This was one of the best summers. Jerry got the lab organized well (his detailed notes on opening and closing West Yellowstone lab were very useful for all of us). He went out in mid-May after the American Society for Microbiology meeting in Miami Beach. Ben and Carol Bohlool came out about the same time as Jerry. Carol Bohlool worked as secretary and typist all summer. After the American Society for Microbiology meeting I did some field work on cyanobacterial mats in the Florida Keys and saw my first marine mat since getting into the stromatolite work.

Bill Doemel (supported by a summer post-doc position from my WARF funds) came out and brought two Wabash College students, John Connor and Don Weller (both supported by the college). (The Wabash College connection worked very well for the next three summers.)

We took only one UW-Madison wagon, left the van with mobile lab in Madison, and shipped all the lab stuff by truck. (This eliminated the need for renting and pulling a U-Haul trailer.) Back in Madison, Dave Ward continued his hydrocarbon work on Lake Mendota. Technician Jim Hoffman did thermal pollution work in the MG & E power plant effluent into Lake Monona, plus handling orders, mail, etc), Mike Madigan began his Ph.D. research on *Chloroflexus* physiological ecology, and undergraduate Sandy Petersen did dishes.

At Yellowstone, we worked on *Sulfolobus* and *Chloroflexus*. Connor from Wabash worked with Jerry on *Sulfolobus* growth rates, Ben Bohlool did *Sulfolobus* serology and worked with Jerry on the salting and siphoning experiments. Later Jerry and Anne Mosser did some low temperature work phytoplankton at the high-altitude lakes in the Beartooth Mountains that we had discovered last year. They also did some work measuring the effect of temperature on photosynthesis in snow algae. Connor helped on some of this. Jim Zabrecky, an honors high school graduate from Hammond, Indiana, sent by the Explorer's club, worked with Ben and did a marvelous job.

Doemel and Weller worked on *Chloroflexus* mats, discovered the phenomenon of aerotaxis, an interesting behavioral phenomena that led to a nice paper in *Science* on the formation of laminated bacterial mats.

Living arrangements: Doemels (now with son Chris) in pink trailer, Kathie and I in Daley's large green cabin with Emily, Mossers in Billy Smith's house that Boylen had used the previous summer, Weller and Connor in a trailer owned by West Park Apartments, Zabrecky in a tiny cabin owned by Billy Smith, Ben and Carol in a small house owned by Billy Smith (former city hall).

Kathie and I returned to Madison in early August to get ready for school. The rest came later around mid-August.

# Zabrecky and Bohlool sampling at Amphitheatre Springs

*Jim Zabrecky and Ben Bohlool sampling a small hot sulfur spring in the Amphitheater Springs area.June 19, 1973. Zabrecky had just graduated from high school in Hammond, Indiana, and was sent to us by the Explorer's Club of New York City. He worked with Ben all summer and did an outstanding job. He was planning to be a biochemistry major at IU.*

***Zabrecky Bohlool Amph springs 6-19-1973 N101.jpg***

**Fall 1973.**

Ben Bohlool finished up and left around the end of September for New Zealand. Jerry wrote up his *Sulfolobus* work and worked on sulfide and iron oxidation. Dave Ward's hydrocarbon work had gone well and was being amplified. Mike Madigan continued his work on sulfide oxidation by *Chloroflexus*.

In 1973 I hired Charlene Knaack as my full time technician and she and I did a lot of water potential work, following up on some of Dave Smith's observations. We did a lot of work on ferrous carbonates, which did not work. Sandy Higgens typed and Cynthia Cirillo was hired to do dishes. Sandy Petersen, who had been a dishwasher, began lab research on *Chloroflexus*, working with Madigan. (She went on to medical school and became an obstetrician.) Undergraduate Pat Remington also worked all year, doing autoradiography primarily and helping others with odd jobs. (Pat is now Associate Dean of the School of Medicine and Public Health at U.W. Madison.) I had one of the smallest groups in years, but got a lot done myself.

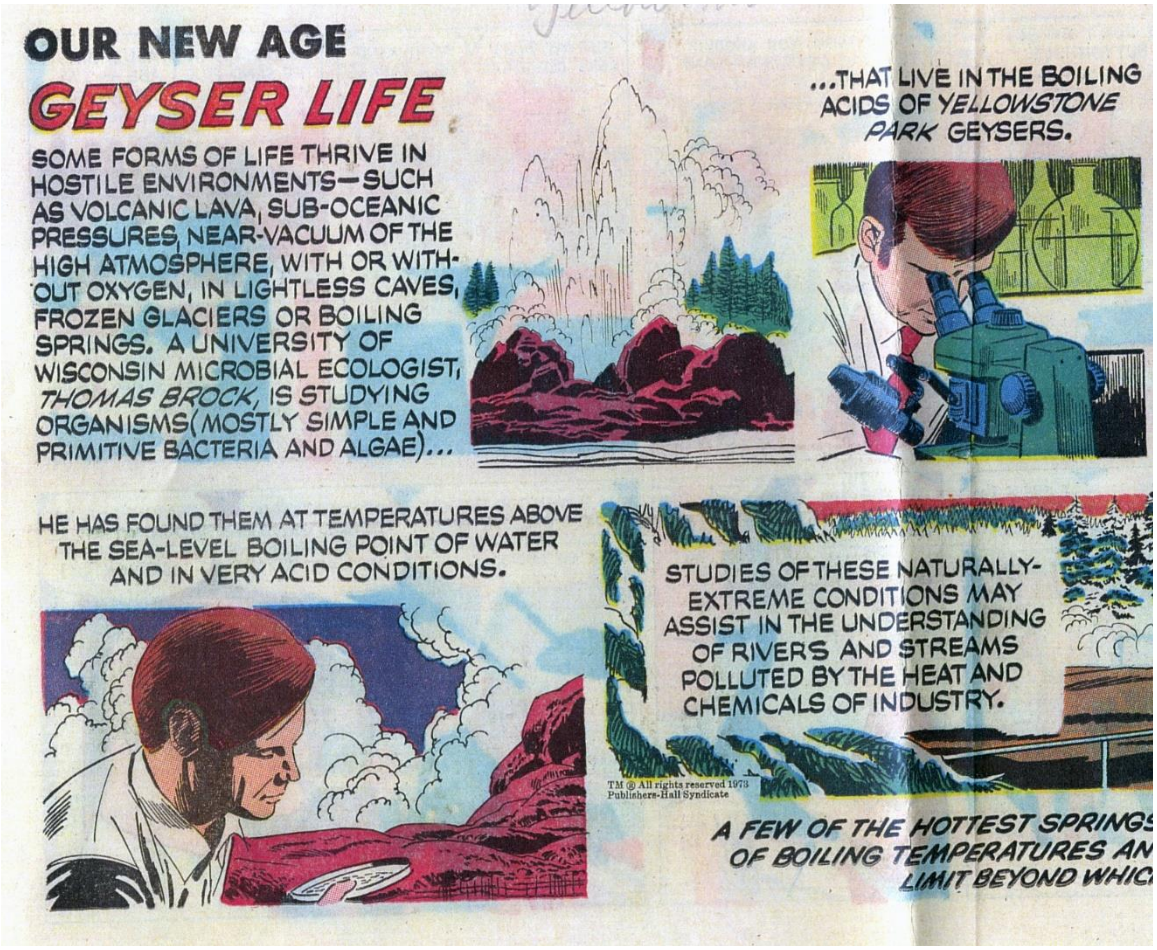
On January 3, 1974 I sent in my annual investigator's report to the Park Superintendent. We had been doing research in the Park for 8 years. This year our focus was on stromatolites; algal-bacterial mats: and *Sulfolobus*.

# Lab group at the West Yellowstone laboratory, 1973

*Lab group in 1973. Photo taken at the rear of the lot. L to R: Mosser, Weller, Connor, TDB, KMB holding daughter Emily, Betlach, Zabrecky, Doemel (holding son Chris), Nancy Doemel, Ben Bohlool Carol Bohlool.*

***1973 lab group scan BS76.jpg***

# Tom Brock in a Sunday comic strip




*"Geyser Life", Part of the Sunday Supplement entitled "Our New Age" created by Althestan Spilhaus. This featured the work of my Yellowstone group in August 1973. The text was picked up from a press release from the National Science Foundation. The original of this cartoon is in my possession, obtained for me by a New York contact. [Framed, in kitchen at 1227 Dartmouth Road, Madison, WI 53705.] This image scanned from part of a color copy.*

***Our New Age single.jpg***

## Yellowstone 1974

Another great summer. After nine years of research work in Yellowstone we had become efficient at setting up and operating the laboratory and the physical aspects of outdoor research. I had learned to anticipate problems. Also, the lab group worked well together, and cooperated willingly when this was needed. It helped greatly to have Jerry Mosser continue to manage operations, and Charlene to work with me on my own research.

Jerry Mosser and Bill Doemel drove out in the Ford van and a U.W. station wagon, accompanied by Pat Remington and Sandy Petersen. Anne Mosser came later in Mosser’s car, not working for us except incidentally. Charlene and her husband Kirit came out about the end of May. Two of Doemel’s students from Wabash College, Don Weller and Jim Cook, came about the first of June. Kathie, Emily, and I came about the first of June. Nancy Doemel and son Chris did not come until the first of July. Another Explorer’s Club person, Susan Entenmann (from the Entenmann Baking family), with a B.S. in Chemistry from North Carolina State, came in early June.

Visitors: Greg Zeikus for three weeks in July (looking for thermophilic methanogens), Stepko Golubic (phycologist and cyanobacterial specialist from Boston University) for the month of July. He and I did an extended trip to Great Salt Lake.

We did a lot of darkroom work this summer with my new Durst enlarger. Pat Remington was especially helpful in getting this set-up. I was using a medium-format Mamiya RB67, mostly B/W, size 120 film, and Pat did most of the film developing and contact prints. He and I did a lot of 5 X 7 and 8 X 10 enlargements. Most of the habitat photos in my Springer-Verlag book came from this summer’s photography.

Jerry did a little on *Sulfolobus* but spent most of the summer on snow algae, making two extended trips to the Beartooth’s. We later published several papers on photosynthesis in the snow (see Bibliography).

Charlene took over more and more of the running of the lab.

Doemel, Weller, and Cook worked on *Phormidium* and *Chloroflexus*. Pat helped them frequently. As the *Phormidium* work started to get more interesting we did more and more on that and let the *Chloroflexus* work slide, although a fair bit was completed on it also.

Susan Entenmann and Sandy Petersen worked together on iron-oxidation by *Sulfolobus* and got a fairly good amount completed. See the paper published in *Geochimica et Cosmochimica Acta*. John Haury, a Harvard Junior Fellow, was a visitor for part of the summer. He participated only as an observer.

The Great Salt Lake research I did this summer is discussed in a separate section.

Keeping up with all the action kept me busy off and on through the summer and fall.

Kathie, Emily, and I returned to Madison very early in August to get ready to go to Norway (I was giving a paper at a thermal pollution conference sponsored by the United Nations; paper 170), and Doemel returned a little later. Mosser, Remington, and Sandy followed about the same time, leaving Charlene to close the lab.

Living arrangements in 1974: Mossers, Billy Smith's house; Doemels, the house Ben had used the previous year; Kathie, Emily and I, Daley's large green cabin; Pat Remington, the cabin Zabrecky had last year; Sandy and Susan rented a red cabin from Billy Smith, as did Jim and Don. A romance developed between Jim and Susan. They later got married.

Kathie, Emily, and I went to Norway for the thermal pollution meeting (sponsored by the United Nations), then a week vacation in the mountains (rain). Then a week in London, then five days in Bermuda (looking at marine cyanobacterial and algal mats, quite exciting), then home.

Other people involved in Madison or Yellowstone in 1974. Dave Ward (who came through Yellowstone on the way to American Society for Limnology and Oceanography meeting in Seattle). On return he then did field work in Northern Wisconsin. Mike Madigan (who did EM work with Chloroflexus), Cynthia Cirillo (typing and dishwashing), Jim Hoffman (part-time on sulfur water chemistry).

Steve Zinder joined in fall of 1974 after his M.S. from Colorado State (he had visited our Yellowstone lab during the summer). Doug Caldwell started as post-doc in early October. Pat Remington went off to France. Charlene and I did a lot of work on salinity effects on microbes in Great Salt Lake and other water potential work. Jerry Mosser finished up his work on snow algae.

# Lab group in 1974, in front of trailers

*Lab group in 1974, in front of lab trailers. L to R: Doemel, Knaack, Kathie with Emily, Tom, Remington, Weller, John Haury (kneeling), Cook, Golubic, Petersen, Entenmann, J. Mosser, A. Mosser. This image (in B/W) used in the Springer monograph.*

***PICT0164.jpg***

# Laboratory of Thermal Biology front view 1974

*The Laboratory of Thermal Biology in 1974, view from front showing both trailers.*

***Yellowstone lab trailers from front no people 6-10-1974 N211.jpg***

# Laboratory of Thermal Biology, rear lab trailer, 1974

*Closer view of the rear lab trailer, viewed from the street side. 1974.*

***Yellowstone rear lab trailer from front 6-10-1974 N211.jpg***

# Rear view of the lab trailers, 1974

*Rear view of the laboratory trailers, June 10, 1974. The ladder was used to get on the roof for snow shoveling. Bicycles were very useful in West Yellowstone, to reach the post office, grocery store, or bank. We generally had a couple available for general use.*

***Lab at West Yellowstone from rear 6-10-1974 crop 1.jpg***

# Other installations at the rear of the lab trailers

*Rear view of the small lab trailer and other installations. The pressure tank for the water system was inside the small utility building (purchased from Sears). The well is just to the left of the big pine tree. The utility building was also used for storage of supplies and bulky items. Its roof had to be propped up before we left at the end of the summer to keep it from collapsing during the heavy snows. (See diagram of support system in another figure.)*

*The large tank contains propane gas for heating and cooking.*
*There is a picnic table under the open patio roof. In addition to lunch or coffee breaks, this table found lots of use for lab work, such as filling tubes or calculating data on warm afternoons. It was also used for carpentry and other construction work.*

***Lab at West Yellowstone from rear 6-10-1974 crop 2.jpg***

# Interior of the large lab trailer: office

*The office portion of the front room of the large lab trailer. Most of the books were for reference. The only telephone was here. July 5, 1969.*

***PICT0192.jpg***

# The front room of the large trailer, microscope table and kitchen

*Front room of the large lab trailer, showing the microscope table. July 5, 1969.*

***PICT0193.jpg***

# Research on Great Salt Lake

My Great Salt Lake research was an offshoot of my Yellowstone work and led to some interesting research papers, as well as several reviews.

In 1968 I was asked by the Society for General Microbiology to participate in their annual symposium to be held in April 1969. The general theme was “microbial growth”, and they asked me to do a presentation on growth in extreme environments. The procedure was that each speaker would write an extended review of the assigned topic, and these reviews would be available to the audience ahead of time. Thus, the talk itself would “take off” from where the printed review ended. (As it happened, I had to miss the meeting because of my spinal fusion, but the paper got written and published.)

My review (*Microbial growth in extreme environments*) discussed not only high (and low) temperature, but high salinity as well. This gave me the opportunity of reviewing the literature on saline lakes such as the Dead Sea and Great Salt Lake.

The North end of Great Salt Lake is about a four hour drive from West Yellowstone. This was too far for an out-and-back in the same day, but not bad with an overnight in the Ogden or Brigham City area.

My first visit to Great Salt Lake was in early February 1968, on our way back to Bloomington from West Yellowstone after our winter visit. We stayed overnight in Salt Lake City, rented a car, and drove to the south end of Great Salt Lake. It was not an opportune time for a visit, but did give me a little idea of the magnitude of the lake. After that visit, I did a lot of library research and found a number of books and papers on the lake.

The water level of Great Salt Lake varies drastically, depending on snowpack in the adjacent Wasatch Mountains. Kathie and I visited Great Salt Lake in August 1971, using the Ford Mobile Lab. At that time the water level was low enough so that the causeway to Antelope Island was dry, and we spent quite a bit of time on the island.

Charlene, Kathie, Emily, and I went to Great Salt Lake in July,1974. By then the lake level was much higher. We visited primarily the north end of the island, where the salinity is the highest. We drove to Rozell Point and put in the canoe, taking samples. This work eventually led to two papers I wrote on halophilic algae and cyanobacteria.

Interesting sidelight on the 1974 Great Salt Lake work: The famous environmental artist Robert Smithson had done an installation at Rozell Point called “Spiral Jetty” in April 1970. I knew nothing about this, or about Smithson. Charlene and I put the canoe in at Rozell Point in July 1974. I was looking for structures called “algal bioherms” that were supposed to be present in the lake. However, Spiral Jetty was completely covered with water and we canoed right over the top of Smithson’s structure. My canoe paddle hit something. “An algal bioherm”, I declared, and took a photo. I still have this photo of what I now know was the top of Spiral Jetty.

According to Wikipedia, “Spiral Jetty” is supposed to be Smithson’s central work. Built entirely of mud, salt crystals, and basalt rocks, it forms a 1500-foot-long; 15-foot-wide counterclockwise coil jutting from the shore of the lake. Smithson died in 1973.

In July Steve Golubic and I went back to the Great Salt Lake for several days. I had made contact with Utah Geological Survey and they took us out on the south arm of the lake in their research vessel. Golubic did quite a bit of diving, looking (unsuccessfully, I believe) for possible algal bioherms. He and I also camped on the beach (the Spring Bay area) on the north arm of Great Salt Lake, where the salinity is the highest.

In 1975 I was back again to Great Salt Lake again, this time with Dave Ward, who did a project on hydrocarbon degradation in hypersaline environments.

I returned several more times during the late 1970s while working on halophilic phototrophs (see Bibliography).

# Spiral Jetty, the masterwork of environmental artist Robert Smithson, at Great Salt Lake

*Smithson created the spiral jetty at Rozell Point on the North Arm of Great Salt Lake. It was a massive earth-moving job, carried out by a contractor following Smithson's design. The structure still exists and is visible when the lake level is low enough. It was completely underwater when I canoed over the top of it in 1974. Photo: NY Times.*

***Spiral-jetty.jpg***

# Yellowstone 1975

This is the only year for which I do not have data recorded at the time. For some reason, my notes stop at the end of 1974. The information in this chapter is put together from memory and from a review of the published papers.

People in the West Yellowstone lab in 1975: Bill Doemel; David Ward; Stephen Zinder; Michael Madigan; Charlene Knaack; Timothy Parkin; Kathie Brock; Tom Brock.

Doemel and Parkin were from Wabash College

Although Dave Ward used our facilities, he did not do any Yellowstone work. He worked in Great Salt Lake, studying the effect of hypersaline environments on hydrocarbon oxidation. He collected samples at Great Salt Lake (on 14-15 July 1975) and brought them back to West Yellowstone for incubation and analysis.

For equipment in 1975, see the list of all items returned to Madison at end of summer.

Projects: Tom; mainly supervision; also the growth rate of cyanobacterial mats (the big paper published with Doemel in *Applied and Environmental Microbiology* in 1977). Charlene; running lab; water potential work on hot acid soils. Parkin: Wabash undergraduate working with Doemel. Madigan; light adaptation in cyanobacterial mats and ecology of *Chloroflexus;* Dave Ward; effect of salinity on hydrocarbon oxidation in Great Salt Lake. Zinder; sulfur gases in Yellowstone hot springs.

This was the last summer at Yellowstone and we spent a lot of time getting ready to close. We had a lot of equipment here, and needed to get it all back home safely. Charlene handled almost all of this, and there is an extensive inventory.

Zinder's project involved superb work with a Hewlitt-Packard gas chromatograph with flame photometric detector sensitive to sulfur compounds. We had planned this project when Steve first came to UW-Madison, and I ordered the equipment (~$10,000) then, so that he would have time to learn how to use it. This project worked well and we got a lot of unique data. His work turned out to be mostly geochemistry rather than biology (and was therefore published in a geochemical journal).

I believe Mike and Steve roomed together.

# Lab group 1975

*1975 Yellowstone lab group photo: Left to right: Kathie (holding Brian); Tom; kneeling Charlene with dog; Bill Doemel; Tim Parkin; Mike Madigan; Nancy Ward; Dave Ward with dog; Steve Zinder; Carol Yarbrough (local girl; dishwasher); on ground Emily; far right Barbara Stone (babysitter).*

***Yellowstone lab 1975 from Zinder.jpg***

## The decision to close the West Yellowstone lab

If I had stayed at Indiana University I might have continued working at Yellowstone indefinitely, but moving to Wisconsin brought me into an arena with extensive freshwater habitats, lakes of all sizes, rivers and creeks. There was never any "pressure" for me to do more "normal" ecology, but it would have been surprising if I had not felt some of the "spirit" of Birge and Juday (Wisconsin's famous limnologists) all around me. Among other things, there was an Oceanography and Limnology Ph.D. program and I was put on that committee. One of my first new students at Wisconsin, Dave Ward, elected to do a study on Lake Mendota. Kathie and I had a canoe and a kayak, and were living within a few blocks of Lake Mendota. I had always been interested in lakes, and in limnology. I remember thinking that I should perhaps do some work on habitats that were of interest to the taxpayers of Wisconsin.

At that time, the broader significance of my Yellowstone work was not evident. Taq polymerase had not been discovered (nor had PCR), nor had deep-sea vents nor Archaea, and there was no strong push toward extremophiles.

The paragraph below from a letter to Castenholz, written before the last summer in Yellowstone, expresses my thoughts on closing the lab:

# Closing the Laboratory of Thermal Biology I

*******************************************************

9 April 1975

Dr. Richard Castenholz
Department of Biology
University of Oregon
Eugene, Oregon

Dear Dick,

I thought you might like to know that this will be our last summer in Yellowstone, at least with any significant amount of research. I have decided it is time to move on to other things, and I think we have done what we can do there. Obviously, more problems keep arising, but I suspect fresh approaches by others would be preferable. We will continue the blue-green and sulfur work at Madison, but oriented more towards lakes and (possibly) marine systems. I think it is time to return to "normal" environments. What will happen to the lab? Hopefully, someone else will take it over. Zeikus has expressed an interest (he did quite a bit on high-temperature methanogenesis last summer), and perhaps others will come also. I hope to spend at least a few weeks there in the summer of 1976, Do you have any interest in taking it over? It would probably cost you about $25,000 to $30,000, a rather small amount of money these days. If no one comes along to take it over, we can easily abandon it, as we have very little permanent installation. The trailers have already depreciated to zero and can be sold, and the land (which is owned by WARF) could probably be readily sold. In 1975, the lab will be open from about May 15 to August 15, and I will be there from June 1 to Aug. 1. I will have three graduate students, Doemel, and a technician, and we will be doing lots on Chloroflexus and Phormdium, plus a new project on sulfur gases in solfataras, using a rather elaborate GC we now have. Will we see you this summer?

Regards, Tom

***Closing lab 1975 letter to Castenholz 1.jpg***
***Closing lab 1975 letter to Castenholz 2.jpg***

# Closing the Laboratory of Thermal Biology II

Here's another insight into what the lab looked like after we had closed it down at the end of summer 1975 (from a letter I wrote to Bob Ramaley, my former colleague at Indiana University and now at Nebraska, on Nov. 25 1975):

Concerning the laboratory. It will probably not be used next summer by anyone, as Zeikus is planning to go to Germany. However, it is hardly worth opening up for a few weeks, as it takes quite a bit of time to turn on all services, get electricity reconnected, and make sure everything is running properly. Also, there are no supplies, glassware, or equipment left, as we moved everything out.

Perhaps you would be interested in starting a major project in Yellowstone and would be able to buy the facility? I think the whole thing could be sold for about $20,000, including land, trailers, water and sewage connections, etc. This is a lower price than it would go commercially, but I know what WARF has invested in the facility, and think they would sell it for that. This isn't really very much money when considered in terms of what laboratory facilities normally cost. Perhaps you could find the money in Nebraska if you used the argument that it is actually an investment, since at the end of the project the whole thing could be sold off.

Let me know what you think about this idea.

Sincerely yours,

Thomas D. Brock
E.B. Fred Professor
of Natural Sciences

TDB:ng

***Letter to Ramaley lab in 1976.jpg***

### Closing the West Yellowstone lab for the last time

Charlene Knaack handled most of this with her usual high efficiency. By mid-summer 1975 we were starting to get things organized for the close-down. There is an inventory of equipment etc. that we were transporting back to Madison.

Although I no longer have any details, I assume that all of these items were brought back in either university or private vehicles.

Charlene stayed to the bitter end and emptied all cupboards of debris, removed tape labels from doors of cupboards, and did a complete cleaning of all shelves, etc. As I said: "I would like the trailers to look half-way decent in case they get put up for sale."

In fact, the property was sold by the owner, the Wisconsin Alumni Research Foundation (WARF) at a profit. The small rear trailer was moved away but the larger front trailer remained and was lived in for many years. It was still there in 2014 (45 years old), although not apparently used any more.

Charlene remained my technician at Madison through the early parts of the new research on Lake Mendota, but around 1980 moved to Bozeman, where she married, raised a family, and lived the rest of her life (and still lives).

The two-page letter to UW-Madison Risk Management dated 7-18-1975 has a complete list of items that we had in the West Yellowstone lab. The prices were at 1975 values, but only approximate, as nothing was appraised.

# Inventory of laboratory equipment in 1975

July 18, 1975

Mr. Thomas C. Halvorsen
Risk Management
722 University Avenue
Madison, Wisconsin 53706

Dear Mr. Halvorsen:

The following is a list of laboratory equipment which will be transported sometime near the end of August 1975 from our Lab in West Yellowstone, Montana back to Madison, Wisconsin:

| | |
|---|---|
| Packard Gas Chromatograph | $12,000 |
| Pyreheliometer | 200 |
| Esterline Angus Recorder | 750 |
| Thermometers | 55 |
| Photo Equipment | 2,000 |
| Monroe Calculator | 800 |
| Texas Instrument Calculator | 50 |
| Zeiss Microscope and Accessories | 3,000 |
| Zeiss Dissecting Microscope | 2,000 |
| Keithly Electrometer | 700 |
| Solarimeter | 300 |
| Orion pH meters - 2 | 700 |
| Sargent Welch pH meter | 340 |
| Corning pH meter | 700 |
| pH and Special Purpose Electrodes | 400 |
| Oxygen Meter, YSI and Probe | 500 |
| Thermistor Bridges and Probes - 4 | 600 |
| Conductivity Meter | 300 |
| Band L Spectronic 20 | 450 |
| Gas Regulators - 10 | 1,000 |
| Beckman DB-G Spectrophotometer | 4,000 |
| IBM Electric Typewriters - 2 | 1,000 |
| Sears Generator | 300 |
| Evinrude Outboard Motor | 200 |
| Torsion Balance | 10 |
| Mettler Balance | 300 |
| Sunbeam Mixer | 25 |
| Osterizer Mixer | 25 |
| Canoe, paddles, car top carrier, life vests and cushions | 250 |
| Servall Centrifuges - 2 | 600 |
| Servall Centrifuge Superspeed | 800 |
| Lumetron | 200 |
| β-Mate Liquid Scintillation Counter | 3,000 |

(*continued*)

# Inventory of laboratory equipment in 1975; page 2

Mr. Thomas C. Halvorsen -2- July 18, 1975

| | |
|---|---|
| Gas Flow Detector and Scaler | $2,000 |
| Barnstead Demineralizer | 50 |
| Constant Temperature Circulators - 4 | 400 |
| Talboy Homogenizing Motors - 3 | 75 |
| Corning Heater-Stirrers - 2 | 150 |
| Thelco Incubator Oven | 375 |
| Thelco Water Baths - 3 | 510 |
| Drying Ovens - 3 | 150 |
| Vacuum Oven | 200 |
| Paper Cutter | 30 |
| Eppendorf Pipets - 5 | 250 |
| Pressure Cookers - 2 | 80 |
| Monitaire Sampler | 300 |
| Fly Wheel Pumps - 2 | 20 |
| Geiger Counter | 200 |
| Air Pumps - 2 | 50 |
| Inverter | 25 |
| Flask Heaters - 3 | 90 |
| Snowshoes - 2 pair | 60 |
| Branson Sonifier | 1,000 |
| Canister Vacuum Cleaner | 40 |
| Vacuum Pumps- 2 | 100 |
| HiVac Pump | 250 |
| Versa Vice | 15 |
| Volt-Ohm Milliammeter | 50 |
| 3M Overhead Projector | 300 |
| Waders - 4 pair | 60 |
| Lab Chairs - 2 | 130 |
| Fiberglass Aquaria - 2 | 30 |
| Goose Neck Lamps - 2 | 10 |
| Fluorescent Desk Lamp | 15 |
| Slide Projector | 100 |
| Postal Scale | 10 |
| Metal Drawer Filing Cabinets - 2 | 50 |
| Test Tube Racks - 25 | 75 |
| Pipettes & Cans | 375 |
| Pipette Jars 4 4 | 60 |
| Pipette Washers - 2 | 60 |
| Carboys - 6 | 120 |
| Boiling Pans - 4 | 40 |
| Hamilton Syringes | 400 |
| Wrist Action Shaker | 100 |
| Temp-Blok Heaters - 6 | 300 |
| Magnetic Stirrers - 2 | 80 |
| Sulfide Distillation Apparati | 100 |
| Anaerobic Culture Apparati | 300 |
| Soil Cores - 2 | 20 |
| Hydrometers - 3 | 10 |
| Clamps, Rings Stands, Connectors | 40 |
| Rubber Boat and Paddles | 40 |
| Propane Stove | 30 |
| Laboratory Glassware | 3,500 |
| Tools | 500 |
| Electrical Supplies | 200 |

***Lab items returned to UW page 1.jpg***
***Lab items returned to UW page 2.jpg***

In a way it was a risky business dropping the Yellowstone project. I had two major research grants supporting that work. Would I be able to transfer those grants to Wisconsin work?

Fortunately, the granting agencies were tolerant. The new Lake Mendota work was immediately focused on blue-green algae (cyanobacteria), analogous to those in hot springs, and NSF permitted the shift. By this time Atomic Energy Commission had become the Energy Research and Development Administration (ERDA; later to become the Department of Energy). About 1972 Pres Cloud had asked me to be a member of a NAS committee on Mineral Resources and the Environment, and this got me into thinking about the sulfur cycle in a broader sense. For the final report of this committee, I wrote a big review on sulfur and the environment which served as the basis for a new research proposal to ERDA. Fortunately, this grant was funded, and supported some of the Lake Mendota work.

During those Lake Mendota years I continued to have good students and post-docs: Mike Madigan, Steve Zinder, Tim Parkin, Bob Fallon, Carlos Pedros-Alio, Al Konopka, Alex Zehnder.

# Legacy of my Yellowstone and extreme environments work

## Taq polymerase and PCR.

The practical development of the polymerase chain reaction (PCR) depended on the discovery of *Thermus aquaticus*. When my 1967 *Science* paper was published, even though this was before the Brock and Freeze paper describing *Thermus aquaticus* had been published, I had lots of requests for cultures of thermophiles and I sent out that organism. By the time the 1969 paper was published, I had isolated *Thermus aquaticus* from many locations, even including hot tap water. When describing a new genus and species, one is required to submit cultures to a recognized culture collection. I deposited several strains at the American Type Culture Collection, including YT-1, the strain that Freeze had first isolated. (We called YT-1 the "type" strain.) These cultures then became widely available. Later, we isolated many more strains, as have other researchers around the world.

Hudson Freeze did a superb job purifying and characterizing the thermostable aldolase of Taq. Also, Paul Ray and Greg Zeikus showed that the membrane and protein-synthesizing systems were thermostable. All of this work provided a foundation for later biochemical work by others.

The first enzyme from *T. aquaticus* acting on DNA was that of Sato (published in 1978) on the Taq1 restriction enzyme, which cut DNA at a specific site. Later, Chien, Edgar, and Trela described a DNA polymerase from *T. aquaticus*. When Kary Mullis developed PCR, he was using an enzyme from *E.coli*. David Gelfand, a colleague of Mullis at Cetus, got the idea of using an enzyme from a thermophile. Around 1983 or 1984 he bought from ATCC cultures of all thermophiles they had available and tested them. *Thermus aquaticus* YT-1 turned out to be the best. (personal communication from David Gelfand). The rest is history. (*T. aquaticus* YT-1 is mentioned in Cetus's original patent application.)

## Deep sea vents

The discovery of the deep sea vents in 1977 put a new perspective on our Yellowstone work. Thermal areas were obviously more widespread in the world than had been known, and provided a much broader range of temperatures. Although *in situ* study of microbes in the vents is vastly more difficult than at Yellowstone, I believe that our research shows what sorts of studies might be done. Methods can be worked out at Yellowstone and then transferred to the depths. As far as I can tell, most of the **microbial "vent"** research has been in cooler areas of the vents, and has focused on activity of chemosynthetic microbes. However, the data so far available show that the very hottest effluents from the deep-sea vents are sterile. To date, the highest temperature at which an organism has been cultured is about 121 C.

**Archaebacteria (Archaea)**

This concept was first announced in 1977 by Carl Woese and its significance gradually became clear over the next decade. This has probably been the area where our Yellowstone work has provided the most extensive impact to the microbial world. *Thermoplasma* and *Sulfolobus* were the first two thermophilic Archaea discovered, and the work on the lipids of these two organisms by Langworthy provided important insights. Langworthy discovered that the lipids of these two organisms contained ether-linked lipids instead of the usual ester-linked ones. When this discovery was made, Langworthy and I both hypothesized that the ether linkages made the cell membranes stable under hot, acid conditions. Although this is certainly the case, other Archaea living at neutral pH also have ether-linked lipids./or

I sent cultures of *Thermoplasma* and *Sulfolobus* to the German culture collection (Deutsche Sammlung fur Mikrobiologie; DSM), which made them widely available. Publication of my book in 1978 provided what was essentially a field guide to Yellowstone thermal areas, with specific directions on how to find springs that had particular microbes..

## Later years: Continuing commitment to Yellowstone National Park

Even though my research program at Yellowstone ended in 1975, my interests in Yellowstone did not end. Through the years up to the present I have remained in contact with the Park in many ways. I have visited frequently and have participated in activities and meetings. My children Emily and Brian were virtually “born” in Yellowstone, but later we visited the Park frequently as a family. We went on several ski trips in the Yellowstone/Bozeman area, both downhill and cross country.

For 8 years (1998-2005) I was on the Yellowstone Association Board, the educational arm of the Park, and participated in their twice-yearly Board meetings (May and late Sept/early Oct).

Reflecting my interest in history, I have also acquired an extensive collection of Yellowstone memorabilia: books and other printed materials---some new but mostly fairly old and/or rare. Many of these go back to the early years of the Park (1880s-1890s).

**Yellowstone 1980**

Bill Doemel brought a class of Wabash College students to the Park in mid-summer and asked me to meet them in the field. I spent a whole day with the class and we visited various thermal areas where I demonstrated measuring and sampling techniques.

After this work, our whole family spent a week at Nine Quarter Circle dude ranch in Taylor Creek, just off the Gallatin Highway and a few miles from Yellowstone National Park.

# Wabash College students: 1980

*Wabash College students and their professor (Bill Doemel; right; standing), at Twin Buttes Vista (a good* Thermus aquaticus *site). August 1980.*

***PICT0279.jpg***

**Yellowstone 1985**

I drove to the Park with my 10-year old son Brian and we visited a number of thermal areas. Kathie was backpacking in California and Emily was at language camp in Minnesota.

**Yellowstone 1986**

In July the whole family drove to Yellowstone and did a tour of thermal areas. Our kids were now 11 and 13.

**Yellowstone 1988 (the year of the fires)**

In July Kathie, Brian, and I drove to Yellowstone. This was the year of the fires and smoke plumes were visible all over. After a few days in the Park, I took Kathie to the West Yellowstone airport, and she flew to the Bay Area for a backpacking trip. Brian and I exited from the Northeast entrance, with plumes of smoke all over. I took Brian to language camp in Minnesota and returned to Madison. I followed the fires with great interest on the news media.

In early October 1988, I returned to Yellowstone after the fires were out and did an extensive tour of the whole park, photographing the results and the extent of the fires. I set up a few photo points of particularly extensive fire damage and followed them in subsequent years.

**Yellowstone 1989**

In August 1989 I returned to the Park to observe the results of the first growing season after the fires. As expected, fireweed (*Epilobium*) was in full bloom everywhere and tiny lodgepole pine seedlings carpeted the formerly “black” areas. The thermal areas, of course, were not affected by the large fires, and their microbial populations were unchanged. (PICT0282.jpg).

# 1989: Quick recovery from the 1988 fires!

*Fireweed (Epilobium) in bloom, the first growing season after the 1988 fires. Near Tower Junction on the Grand Loop Road. August 1989.*

***PICT0282resized.jpg***

**Yellowstone 1993**

I was back again in July 1993. Kathie and I and our son Brian (now 18 years old) visited some of our key research areas, including the White Creek and Great Fountain areas, Octopus Spring, Boulder Spring and Mushroom Spring. (PICT0284.jpg) (PICT0286.jpg)

**Yellowstone 1994**

During the years 1982 through 1992 I had a parallel career as President of Science Tech Publishers, a book publisher that did “high-end” technical, scientific, and college textbooks. Through this work I became very experienced in book design, especially computerized typesetting and color separations. Having now retired from publishing, I decided to do a new edition of “Life in the Geyser Basins”, to be called “Life at High Temperatures.” Since this was now the age of PCR and Taq polymerase, it seemed an appropriate time to bring the book up to date. I did the complete production job for this new addition: writing, design, color separations, and provided final “camera-ready” files for the printer.

**Yellowstone 1997**

In the 1990s I started to collect both old and rare books, and also new books on the history of the Park. I also began collecting Yellowstone ephemera (old maps, guide books, photo collections, old postcards, etc.).

This year (September 28, 1997 to January 11, 1998 the National Gallery of Art in Washington mounted the first retrospective exhibit of Thomas Moran works, drawings, paintings, etc. Moran’s monumental oil painting of the Grand Canyon of the Yellowstone takes partial credit for the establishment of Yellowstone National Park as the first Park in the world. Kathie and I went to Washington to see the exhibit. At the same time another museum had an exhibit on the history of roadbuilding in Yellowstone Park, a fascinating exhibit that made use of quite a bit of Yellowstone ephemera of the sort I had been collecting.

A few weeks later, October 12-15, 1997, I was in the Park at Mammoth Hot Springs, to attend the conference entitled “People and Place in Yellowstone”, which primarily dealt with the history of the Park. In addition to the many interesting presentations, there were exhibits, including some dealing with ephemera.

# Tom Brock at Boulder Spring; 1993

*Tom Brock at Boulder Spring. The stream in the foreground is very hot. This spring appeared unchanged from when Tom Bott did his outstanding work in 1968. Photo July 14, 1993.*

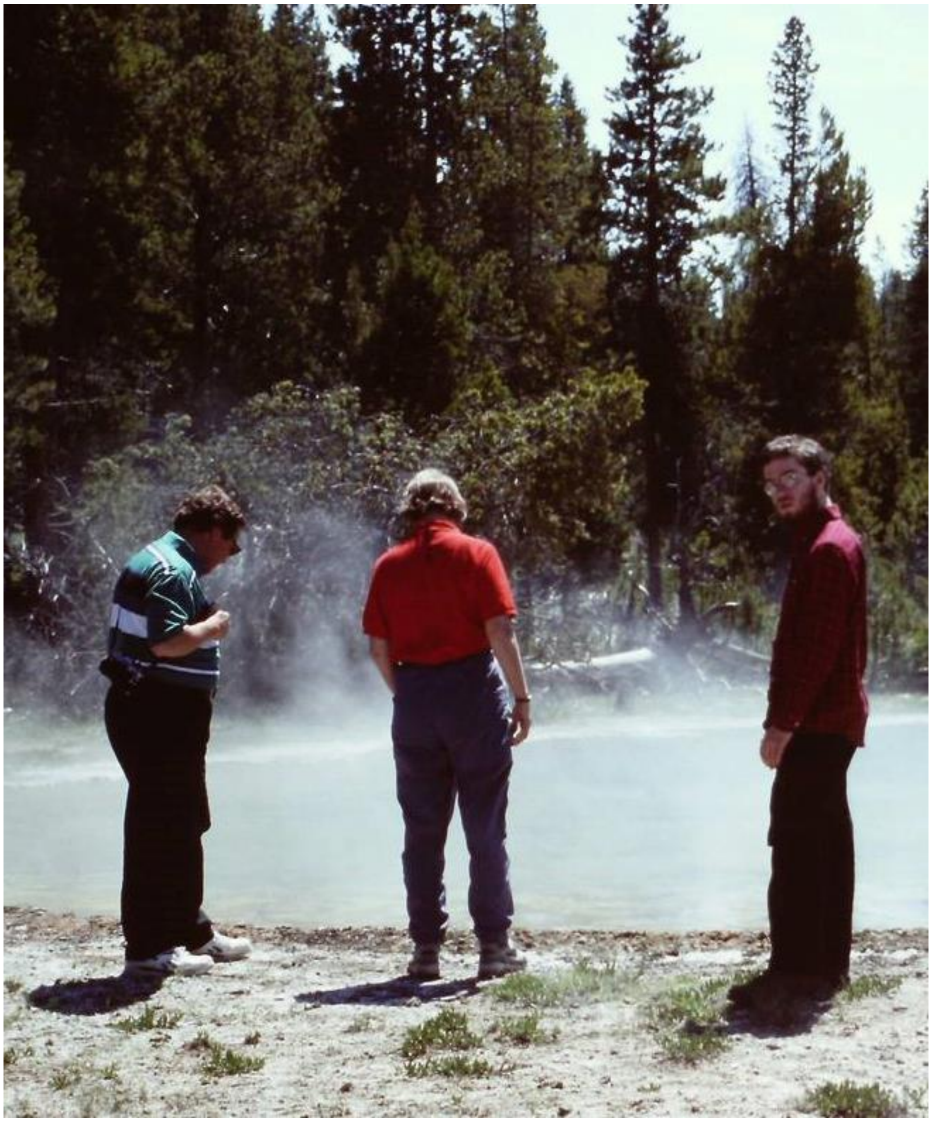
***PICT0284.jpg***

# Tom Brock at the outflow from Mushroom Spring

*Tom Brock pointing to the site where the inoculum for the first culture of* Thermus aquaticus *was taken in September 1966. Mushroom Spring. This spring had gone through changes since 1966. It stopped flowing in late 1968-early 1969, but then restarted flowing sometime in the 1980s. Photo taken July 15, 1993.*

***PICT0286.jpg***

# Mike Madigan, Kathie and Brian Brock; Mushroom Spring 1994



*We never get tired of returning to Yellowstone! Here are Mike Madigan and Kathie and Brian Brock viewing Mushroom Spring, June 1994.*

***madigan kathie brian yellowstone.jpg***

**Yellowstone Association Board**

At the 1997 conference I met Thomas Offutt,a Board member of the Yellowstone Association. Tom knew my name from my research work. By this time, PCR and Taq polymerase were frequently in the news, and Offutt knew of my connection and was aware of the significance for the Park. He asked me if I would be interested in being on the Yellowstone Association (YA) board. I agreed. I heard from YA Executive Director Pat Cole that winter and my first YA Board meeting was Fall 1998. The Board had a number of committees, and I ended up on the Education Committee. I served on the YA Board until the end of 2005. In addition to the regular Board meetings, I also participated in special meetings focused on the long-term mission of YA and its role in the National Park. My extensive intimate knowledge of Yellowstone back country may have been helpful in these various discussions.

I was probably an ideal person to be on the YA Board. The Yellowstone Association is what the NPS calls a "Cooperating Association." These Associations are non-profit organizations dedicated to helping their National Parks. YA is the modern offshoot of the Yellowstone Museum and Natural History Association, that had started in the early 1930s. YA as a functioning organization arose in the late 1970s, after my research had terminated. Originally, its goal was to offer field courses as the Yellowstone Institute, and later it expanded into publishing and operating the Park book stores.

At the time I came on the Board, the Yellowstone Association had an extensive program of educational courses for the public that were organized through the Yellowstone Institute. The Director of the Institute asked me to give a course on thermophilic microorganisms. The "book" people at YA suggested that the "Life at High Temperatures" book that was published in 1994 should be reissued with a new cover. I agreed, and sometime in the early 2000s this version was published. As far as I know, it is still being sold in the Visitor Center bookstores.

**Yellowstone 1998**

In August 1998 Kathie and I visited Yellowstone. Among other things, I was asked by Janet Chapple, who was writing a Yellowstone guide book, to show her some interesting thermal areas. We took her to Sentinel Meadows, one of the interesting "back country" thermal areas that has a maintained hiking trail.

# Sentinel Meadow in 1998

*Sentinel Meadow, a quiet valley with interesting thermal features. This area can be reached by a rustic 2-mile trail. The two features visible are Flat Cone and Steep Cone. I took guidebook author Janet Chapple back here on August 10, 1998. Flat Cone and Steep Cone are in view.*

***Sentinel Meadow with cones 8-10-1998 N515.jpg***

# Tom Brock at Steep Cone 1998

*Tom Brock at the edge of Steep Cone. This was one of the first superheated springs that I studied when looking for living bacteria in boiling water. It was also used extensively by the Encyclopedia Britannica film crew. Photo August 10, 1998.*

***Tom Steep Cone 8-10-1998 N515.jpg***

# Queen's Laundry with the remains of the Norris "spa"

*Queen's Laundry, at the west end of Sentinel* Meadow. *The structure shown was built by Superintendent Norris in the 1880s. Photo August 10, 1998.*

***Queen's Laundry YS 8-10-1998 N515.jpg***

**Yellowstone Institute Courses 1999-2000**

**Three day course:** I gave a 3-day Institute course May 9-11, 1999, just after the spring YA Board meeting held at the Old Faithful Snow Lodge. At this time the course was somewhat of a "reach" for me, as I no longer had any equipment or supplies. Fortunately, UW agreed to provide what I needed. I obtained most of the routine supplies from the department stock room, and a good Zeiss microscope from the teaching laboratory. The department also gave me a budget of $500, mainly used to purchase a thermistor and probes. (My UW colleague Ken Todar helped get all these items ready.)

There were 13 participants in the course, which was considered a good registration, since the course was being offered "off-season". One participant, a resident of West Yellowstone, was a commercial guide in the Park. Three participants were Park employees. Another participant was a naturalist/photographer living in Gardiner, MT who taught other courses for YA. One was a geologist very knowledgeable about Yellowstone thermal areas. Two participants were from Madison, WI! The rest were from various states (ID, CO, WA, FL).

Kathie and I drove out with all the equipment, arriving two days before the YA Board meeting. We did preliminary checking of springs, determined access possibilities, and started some immersion slide studies. In the first class evening I showed the Encyclopedia Britannica movie, followed by a lecture/slide show. I had gone through all my Yellowstone slides and had put together an orienting talk for the night before the first day of field work.

For the field work I ran into a major problem with the Park Service. A grizzly bear had been spotted in the White Creek area so the loop road to Great Fountain Geyser and the Mushroom/Octopus Spring area was closed. My request for special treatment was in vain, so I had to make other arrangements. I ended up spending most of the time before lunch at Biscuit Basin and the Fountain Paint Pots Nature Trail. Then we went to Roaring Mountain to see acidic habitats. After lunch, I led the group off the trail at Norris Geyser Basin and into the 100-spring plain. This latter hike worked out well, especially since the hot spring geologist on the trip knew the names of all the features in the back country.

Monday evening I had the microscope set up in a corner of the Snow Lodge bar, and showed the participants what the microbes looked like.

The weather was typical May in Yellowstone, windy, cloudy, with spitting snow.

My former student Dave Ward, now on the faculty at Montana State, had developed an extensive research program at Yellowstone, using modern molecular methods. I arranged for him to talk to the group. Since we could not get to Octopus Spring (grizzly problem), we ended up at Midway Geyser (Grand Prismatic Spring). where Dave gave an excellent talk.

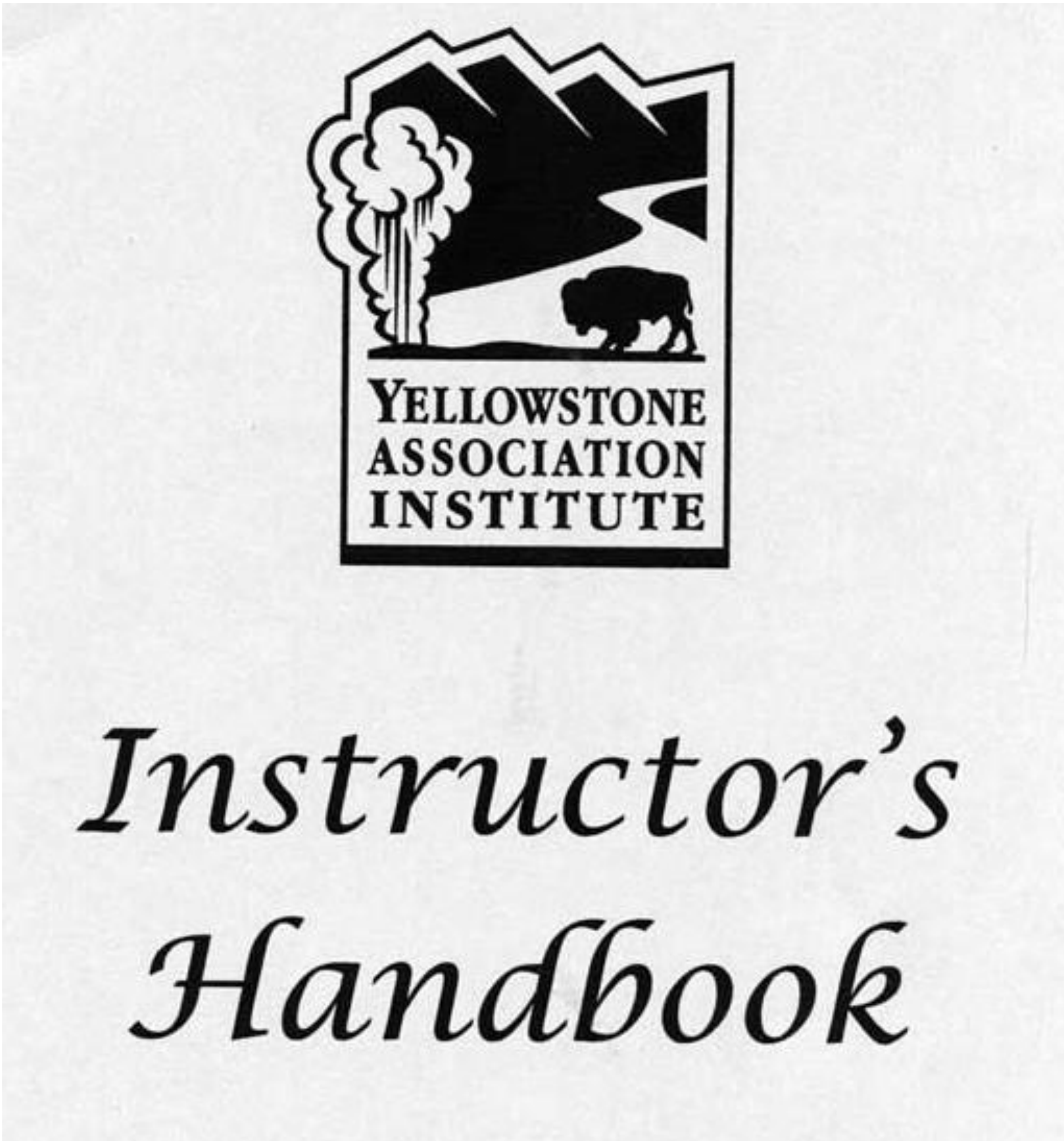
**One-day course:** The course was a success but was a great deal of work to organize. So the next year I did a one-day course in which only my major research areas were visited. I

gave this course twice, on Saturday, July 22, 2000 and Sunday, July 23. I had 13 participants on Saturday and 11 on Sunday. One of the Sunday attendees was Professor Reinhard Bachofen from Switzerland, who I had met before in Switzerland. I also had Deborah Steele, who I knew as a naturalist at the Buffalo Bill Museum in Cody.

This course was a pleasure to run, as there was very little to prepare ahead of time. Also, the Yellowstone weather was "perfect" (in mid-July).

I combined this visit with genealogical research at the Family History Center in Salt Lake City (SLC). I flew from Madison to SLC, rented a car, and drove to West Yellowstone (where I stayed the whole time; Three Bears Lodge). After the two days of the course, I returned to SLC and spent five days doing family history research. Then home. I was gone 8 days.

# Yellowstone Institute Instructor's Handbook




*The Institute was well organized. I was given this Handbook with detailed instructions.*

***YA-Instructor-Handbook-1999 cropped.jpg***

# Life at High Temperatures Course Outline

**LIFE AT HIGH TEMPERATURES: 501**
**COURSE OUTLINE**

**Note: This schedule is subject to change due to weather.**

**July 22, 2000: Course 732; July 23: Course 733; July 30: Course 743**

9:00 AM. Meet at the Great Fountain Parking Lot along the Firehole Lake Loop Road. Orientation on the origin and nature of Yellowstone's thermal areas. Brief overview of life at high temperatures.

9:30 AM. Short walk to Octopus Spring, an alkaline spring that has been heavily studied by scientists. Observe microbial mats in the outflow channels. Demonstration of sampling, structure of mats, kinds of microbes present. Observation of "pink bacteria" found at very high temperatures in the outflow channel. Discussion of life in boiling water.

10:00 AM Walk up White Creek, a large thermal creek that drains the White Creek valley. Visit Five Sisters Spring, Buffalo Pool, Three Brothers, and Stepbrother. Observe techniques for studying bacteria in boiling water.

11:00 AM Return to Octopus Spring. Climb hill to Twin Butte Vista Spring and observe "yellow bacteria" in the outflow channel. Walk to Mushroom Spring, the site where the first extreme thermophile (*Thermus aquaticus*) was discovered in 1966. Observe other hot springs in the area.

11:45 AM. Return to Great Fountain Parking Lot. Drive to Roaring Mountain, eating lunch in our cars along the way.

12:45 PM Visit Roaring Mountain, a large thermal hill where sulfur is deposited and oxidized to sulfuric acid. Park on the west side of the road near the south end of the area. We will climb through a 1988-burn area to the south effluent, the most acidic flowing spring in the Park. Discussion of acidophiles, thermoacidophiles, and sulfur-oxidizing bacteria. Climb among highly eroded rock to the water source.

2:00 PM Drive south from Roaring Mountain about two miles to Nymph Lake, a small lake considerably altered by acid waters. Visit Nymph Creek, the principal source of Nymph Lake. Observe iron and sulfur deposition. Observe the heavy development of *Cyanidium caldarium*, a thermophilic, acidophilic alga (eucaryote) that is the dominant organism in the creek.

2:45 PM Drive to Norris Geyser Basin Parking Lot. Walk to the museum and the overlook of the Porcelain Basin. Discussion of the geology and chemistry of Norris hot springs. Walk to the Tantalus Creek area and the Norris Back Basin. Observe Cinder Pool, Realgar Spring, and other springs highly altered chemically. Observe Perpetual Spouter, the spring where bacteria were first discovered living in boiling water.

4:30 PM Return to Norris Parking Lot. Drive to Norris Picnic Area (on the road to Canyon) for a wrap-up discussion. Detailed maps will be available to show the locations of other interesting thermal areas that can be visited on your own.

5:00 PM Class ends.

**Thomas D. Brock**
E.B. Fred Professor of Natural Sciences Emeritus
University of Wisconsin-Madison

***YA-one-day-course-2000.jpg***

**Yellowstone 2001-2005**

I was in the Park for several days in May and October each year for the YA Board meeting. Some of these years I stayed a little longer. Most years I combined the trip with visiting my former technician Charlene Knaack, who had married and was raising a family in Bozeman.

**Yellowstone Science articles published 2002 and 2003.**

In 2001 I wrote a history of Vince Schaeffer's Yellowstone Field Research Expeditions. The YA Board meetings were generally held at Mammoth Hot Springs, where the Yellowstone Archives and Research Library was housed and accessible. Conversations with the editor of Yellowstone Science encouraged me to write this history. With help from Schaeffer's university at Albany, I was able to obtain a lot of good photos. Because of the length of my article, it was published in two parts, in 2001 and 2002.

**Yellowstone 2002**

In 2002 the YA Institute offered a course on invasive plants, taught by a Yellowstone Park employee. I took this partly to see how the Institute worked for a student, and partly because Kathie and I had been restoring Pleasant Valley Conservancy for a few years and I was interested to see how work in the U.S. west might be different from that in the east. The course was held in early July at the YA Institute headquarters at the historic Buffalo Ranch in the Lamar Valley. This course got me into some amazing back-country locations I never would have seen otherwise.

As soon as the the Institute course was over, I joined a group taking a geology field trip being conducted by Prof. Robert Smith of the University of Utah. Smith was the world expert on Yellowstone geology, especially the volcanic and thermal features of the Park. This trip was being offered by YA as a "high-end" course (fund-raiser). It was an outstanding trip, and at its completion we had the opportunity to see Yellowstone from the air, using a small plane that could fly at low altitude.

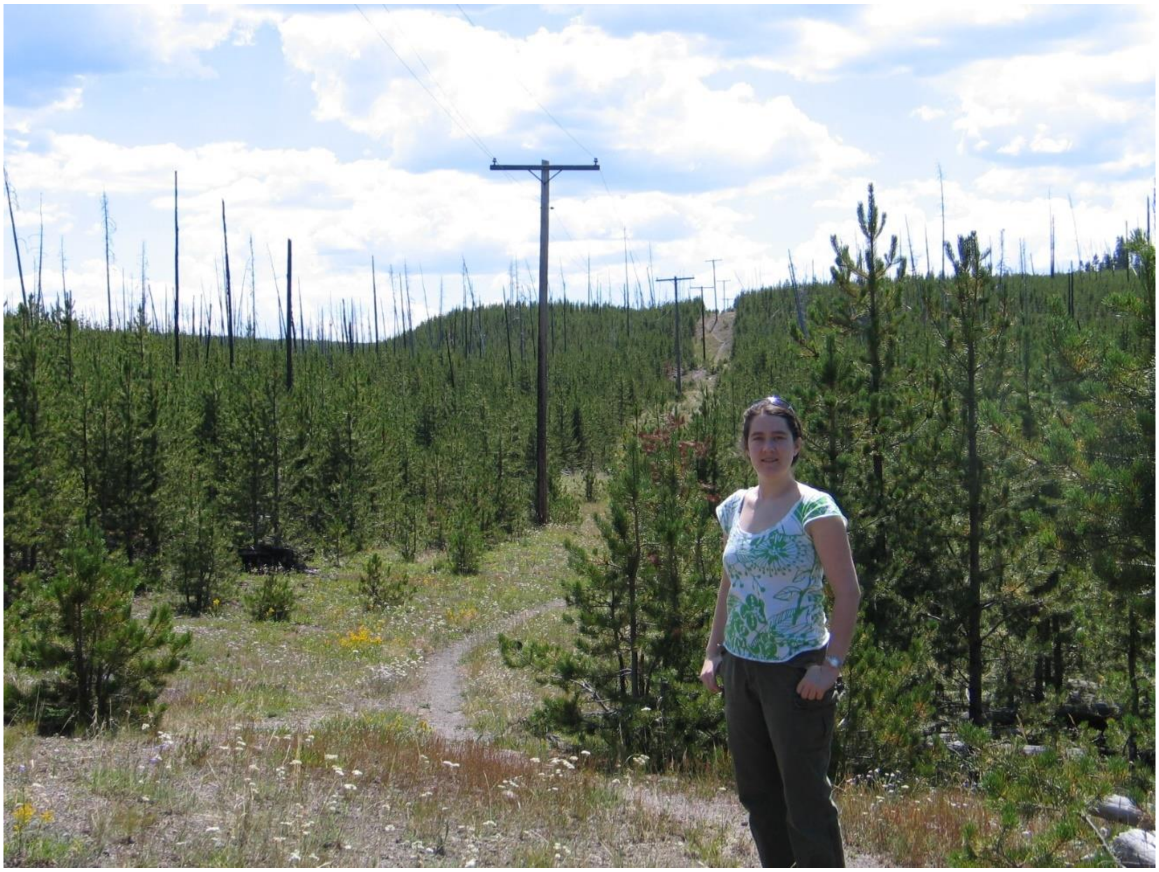
**Yellowstone 2004**

For some of the YA Board meetings, Kathie came with me. In late September 2004 she and I revisited thermal areas that we had worked on years before.

**Yellowstone 2005**

This year my daughter was a post-doctoral fellow in the Institute for the American West at Stanford University and had been asked to organize an intern program for Stanford students at Yellowstone. In August 2005, after her program was underway, she and I spent a few days in Yellowstone visiting areas where I had worked 20-some years ago. The image here is of Emily standing next to one of my photo points from the 1988 fires, illustrating the recovery of the lodgepole pine forest after 17 years.

# Monitoring the recovery from the 1988 fires



*In 1988 I set this power line as a photo point for observing recovery of the lodgepole pine forest from the fires. This photo (daughter Emily) was taken early August 2005, 17 years later. The location is about a mile south (uphill) from Madison Junction on the Grand Loop Road. The power line can be easily seen crossing the road.*

***IMG_1268.jpg***

In October 2005 I was asked to lead a trip of YA Board members and staff into a thermal area where we had worked. This was to be an afternoon trip after the Board meeting ended. I chose Sentinel Meadow, which is one of the thermal areas relatively easy to reach but which has a very remote feel to it. The group car pooled from Old Faithful to the parking area on the Fountain Freight Road. We then walked on the bridge across the Firehole River by Ojo Caliente and took the Sentinel Meadow trail.

The Sentinel Meadow trail goes right past Boulder Spring, where we did so much work in 1968-1970. I discussed this research briefly. We then followed the trail into Sentinel Meadow itself. As we walked into the Meadow two coyotes starting calling from one side of the meadow to the other. In the middle of the meadow is Steep Cone, one of the more dramatic thermal areas in the Park, and one that I studied a lot. (We had also done a lot of filming with Encyclopedia Britannica at Steep Cone.) Another interesting feature in Sentinel Meadow is Queen's Laundry, a very large spring with a large outflow that forms colorful pattern as it spreads out over the sinter. In addition, Queen's Laundry has an interesting historic structure, a small building that was built as a tiny spa by Superintendent Norris in the late 1880s.. Unfortunately, just before we reached Queen's Laundry, a trip member saw in the distance a grizzly bear. Immediately, the trip was terminated (not by me!) and we left Sentinel Meadow. The group saw some of the sights (sites), but unfortunately not at the leisurely pace I had hoped for.

**Yellowstone 2014**

This was the last year I visited the Park ((88 years old). 18-21 July 2014. Kathie and I flew to Bozeman, rented a car, and drove through Paradise Valley to the North entrance. Then we toured the Park, revisited key research areas, such as Roaring Mountain, Norris, Great Fountain area, and Octopus Spring. Octopus Spring had changed considerably. Its second effluent (which gave it the name "Octopus") had essentially quit flowing. Also, the large mat that Doemel and I had studied so extensively in 1974 was much smaller in size.

In traveling the Park this trip, I was interested to read quite a few interpretive signs that were based on my research. See especially the sign at Sulfur Caldron with information on "thermoacidophiles"! Amazing! I also found in the 2014 Park newspaper an extensive article based primarily on my work.

**Yellowstone 2016**

In the fall of 2016 the Yellowstone Association merged with the Yellowstone Foundation to form a new entity called Yellowstone Forever. All of the YA activities (Visitor Centers; publications; Institute courses; headquarters in Gardiner) still exist, functioning under the umbrella of the new organization.

# Thermoacidophiles interpretive sign at Sulfur Caldron


*It is satisfying to see signs like this at appropriate places throughout the park.*

***Thermoacidophiles sign IMG_2942.jpg***

# Personnel at the Laboratory of Thermal Biology

Personnel at the Laboratory of Thermal Biology, West Yellowstone, Montana, 1966-1975. Note that the lab did not exist until September 1967. Before that, all research was carried out in rented cabins or motel rooms.

There are three tables here. The first is an abbreviated time line. The second provides more detail, and provides information about each person and their work at the lab. The third is a detailed time line.

Summer of Year

| **Name** | **1966** | **1967** | **1968** | **1969** | **1970** | **1971** | **1972** | **1973** | **1974** | **1975** |
|---|---|---|---|---|---|---|---|---|---|---|
| BauldB: S | | | | | | X | X | | | |
| BauldJ: G | | | | | | X | X | | | |
| Belly: PD | | | | | | X | | | | |
| BohloolB: PD | | | | | | | | X | | |
| BohloolC: S | | | | | | | | X | | |
| Bott: PD | | | X | X | | | | | | |
| BoylenC: PD. | | | | | | X | X | | | |
| BoylenK: T | | | | | | X | X | | | |
| Cloud: V | | | | | | | X | | | |
| Cohen: T | | | X | | | | | | | |
| Connor: U | | | | | | | | X | | |
| Cook: U | | | | | | | | | X | |
| Darland: PD | | | | X | | | | | | |
| Davidson: U | | | | | X | | | | | |
| DoemelN: S | | X | X | X | X | | | | | |
| DoemelW:G/PD. | | X | X | X | X | | | X | X | X |
| Entenmann: U | | | | | | | | | X | |
| Fliermans, C: G | | | | | X | X | | | | |
| Fliermans, R: S | | | | | | | | | | |
| Foy: T | | | | | X | | | | | |
| Freeze: U | X | | | | | | | | | |
| Golubic: V | | | | | | | | | X | |
| Haury: V | | | | | | | | | X | |
| Holleman: T | | | X | X | | | | | | |
| Kathie Brock | | | | | X | X | X | X | X | X |
| Knaack: T | | | | | | | | | X | X |
| Lenn: T | X | | | | | | | | | |
| Lynn: PD | | | X | | | | | | | |
| Madigan: G | | | | | | | | | | X |
| Meeks: PD | | | | | | | X | | | |
| Louise Brock | X | X | X | X | X | | | | | |
| MosserA: PD | | | | | X | | X | | X | |
| MosserJ: PD | | | | | X | | X | X | X | |
| Murphy: T | X | X | | | | | | | | |
| Noguchi: V | | | | | | X | | | | |
| Parkin: U | | | | | | | | | | X |
| Petersen: U | | | | | | | | | X | |
| Remington: U | | | | | | | | | X | |
| Samsonoff: V | | | | | X | | | | | |
| Sininsky: V | | | | | | | X | | | |
| Smith: G | | | | | | X | X | | | |
| Tansey: PD | | | | | X | X | | | | |

| Name | 1966 | 1967 | 1968 | 1969 | 1970 | 1971 | 1972 | 1973 | 1974 | 1975 |
|---|---|---|---|---|---|---|---|---|---|---|
| Von Bork: V | | | | | | X | | | | |
| Walter: V | | | | | | X | X | | | |
| Ward: PD | | | | | | | | | | X |
| Weiss: G | | | | | | X | | | | |
| Weller: U | | | | | | | | X | X | |
| Wiegert: V | | X | X | | | | | | | |
| Zabrecky: U | | | | | | | | X | | |
| Zeikus: V | | | | | X | | | | X | |
| Zinder: G | | | | | | | | | | X |

Key: G, graduate student; PD, post-doctorate; S, secretary; T, technician; U, undergraduate student; V, visitor

Key to Yellowstone project personnel

| Last name | First name | Status | Notes |
|---|---|---|---|
| Bauld | Bonnie | S | IU/Wisc |
| Bauld | John | G | IU/Wisc |
| Belly | Bob | PD | IU/Wisc |
| Bohlool | Ben | PD | Wisc |
| Bohlool | Carol | S | Wisc |
| Bott | Tom | PD | IU |
| Boylen | Chuck | PD | Wisc |
| Boylen | Kathy | T | Wisc |
| Cloud | Preston | V | UC Santa Barbara |
| Cohen | Maxine | T | IU |
| Connor | John | U | Wabash College |
| Cook | Jim | U | Wabash College |
| Darland | Gary | PD | IU |
| Davidson | Jeffrey | U | IU |
| Doemel | Bill | G/PD | G: IU; PD: Wisc |
| Doemel | Nancy | S | IU |
| Entenmann | Susan | U | Explorer’s Club |
| Fliermans | Carl | G | IU |
| Fliermans | Ruth Ann | S | IU |
| Foy | Frances | T | IU |
| Freeze | Hudson | U | IU |
| Golubic | Stepjko | V | Boston University |
| Haury | John | V | Harvard Junior Fellow |
| Holleman | Pat | T | IU |
| KMB | | PD | IU/Wisc |
| Knaack | Charlene | T | Wisc |
| Lenn | Bob | T | IU |
| Lynn | Raymond | PD | IU |
| Madigan | Mike | G | Wisc |

| Last name | First name | Status | Notes |
|---|---|---|---|
| Meeks | Jack | PD | Wisc |
| Louise | | T | IU |
| Mosser | Anne | PD | Wisc |
| Mosser | Jerry | PD | Wisc |
| Murphy | Sally | T | IU |
| Noguchi | Kimio | V | Japan (with 2 associates) |
| Parkin | Tim | U | Wabash College |
| Petersen | Sandy | U | Wisc |
| Remington | Pat | U | Wisc |
| Samsonoff | Bill | V | Conti lab (University of Kentucky) |
| Sininsky | Mike | V | Harvard Junior Fellow |
| Smith | Dave | G | IU/Wisc |
| Tansey | Mike | PD | IU |
| Von Bork | Bert | V | Film crew |
| Walter | Malcolm | V | Yale/Australia |
| Ward | David | PD | Wisc |
| Weiss | Richard | G | IU |
| Weller | Don | U | Wabash College |
| Wiegert | Dick | V | University of Georgia |
| Zabrecky | Jim | U | Explorer's Club |
| Zeikus | Greg | G/PD/V | IU/Wisc |
| Zinder | Steve | G | Wisc |

Key: G: graduate student; PD: post-doc; S: secretary; T: technician; U: undergraduate student; V: visitor

## Time line

| Year | Who | Facilities | Principal activities |
|---|---|---|---|
| 1964 | TDB; Louise | None | Collecting |
| 1965 | TDB; Louise | Rented Yellowstone cabins; The Barns | Surveying; standing crop; first biochemistry; discover pink bacteria at high temperature; Paper #1 |
| 1966 | TDB; Louise; Lenn; Murphy; Freeze; [Visitor Stewart from Scotland] | Rented West Yellowstone cabin; Daley green cabin; gas-flow counter | First 14-C studies; temperature transfers; isolation of Thermus; Papers #5, #9, #19 |
| 1967 | TDB; Louise; Murphy; Bill and Nancy Doemel [Visitor: Wiegert] | Daley's green cabin; Mettler balance, Spectrophotometer; gas-flow counter<br><br>Sept first lab trailer | First Cyanidium; Light adaptation; vertical zonation; blackout expts to measure growth rate; 32-P photophosphorylation; insects; bacteria in superheated springs; isolate more Thermus; 14-C uptake in bacteria; Papers #3, #4, #7, #10, #14 |
| 1968 | TDB; Louise; Holleman; Tom and Sue Bott; Bill and Nancy Doemel; Ray Lynn [Visitors: Dworkin, Dave White; Mary Davis; Mercedes Edwards; Normal Lang] | Lab trailer with all equipment; 2nd trailer in August; purchase lot for trailers | High temperature bacteria; Cyanidium; Zygogonium; electron microscopy; getting both trailers set up; Papers #12, #15, #16, #17, #18, #21 |
| 1969 | TDB; Louise; Bott; Darland; Bill and Nancy Doemel; Maxine Cohen; Holleman [Visitor: Barbara Rice, monkey flowers] | Final installation of the 2 lab trailers; Ford Econoline mobile lab; culture facilities; | TDB in New Zealand & Japan; Thermoplasma discovered in Bloomington; First high temperature acid springs; culture bacteria from mats; insects in acid habitats; electron microscopy; Boulder Spring; TDB/Doemel trip to west coast in Ford mobile lab; Papers #25, #26, #27, #28, #32, #38, |

| Year | Who | Facilities | Principal activities |
|---|---|---|---|
| 1970 | TDB; Louise; Tansey, Kathie, Bill and Nancy Doemel; J. and A. Mosser; Frankie Foy; Bauld; Davidson; Zeikus; [Visitors: Samsonoff; Moishe Shilo] | 2 lab trailers plus picnic table with roof for outdoor work in nice weather; mobile lab | Doemel and TDB to Iceland; TDB to El Salvador; Life in the Geyser Basins booklet for Yellowstone National Park; visit to Butte strip mines; Boulder Spring and Pool A studies; isolation of Sulfolobus; fluctuating spring study; Firehole River; Roaring Mt acid thermal gradient; Filamentous bacteria in the microbial mats; Papers #24, #34, #39, #40, #41, |
| 1971 | TDB; KMB; Belly; Tansey; Fliermans; Bauld; Bonnie; Dave Smith; Weiss; Boylens (2); [Visitors: Walter; Japanese (3) EncyBritt film] | 2 lab trailers plus mobile lab; Japanese in Daley's green cabins | Move to Madison; Cyanidium in soil; Hot ground sulfur bacteria; thermophilic fungi; Chloroflexus and microbial mats; Sulfolobus; Firehole River; acid lake study and low pH of cyanobacteria; Papers #47, #50, #52, #54, #55, #61, |
| 1972 | TDB; KMB; Smith; Bauld; J. and A. Mosser; Meeks; Velma Smith (dishwasher); Boylens (2); Bonnie; [Visitor: Walter; Cloud; Sininsky] | 2 lab trailers plus mobile van; scintillation counter | TDB and KMB in Iceland; Sulfur oxidation in acid hot springs; Cyanidium in soil; Chloroflexus and mats; Firehole River; Papers #57, #64, #66, #67, |
| 1973 | TDB; Jerry and Anne Mosser; C. and B. Bohlool; Doemel; Connor; Weller; Zabrecky; | 2 lab trailers (left mobile lab in Madison); scintillation counter | Chloroflexus mats; Sulfolobus growth; salting acid springs; snow algae; Papers #70, #72, #75, #79 |
| 1974 | TDB; KMB; J. and A. Mosser; Doemel; Remington; Petersen; Knaack; Weller; Cook; Entenmann; Zeikus [Visitor: Golubic] | 2 lab trailers; darkroom; Mamiya | Snow algae; Sulfolobus; Chloroflexus and Phormidium mats; iron oxidation by Sulfolobus; water potential in solfatara soils; Great Salt Lake; Papers #81, #82, #83, #89, #91, #97, #102, |
| 1975 | TDB; Doemel; Ward; Zinder; Madigan; Knaack; Parkin | 2 lab trailers; gas chromatograph with sulfur detector | Closing lab; Great Salt Lake; sulfur gases; Chloroflexus mats; light adaptation of mats; growth rate of algal mats; water potential of hot acid soils; Papers #90, #106, #107 |

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## Doing field work in other parts of the world

My first funded project was in Iceland, and the initial Yellowstone work was done to work out methods. However, once I got into the Yellowstone work, it was clear that this was to be my primary focus, and thermal areas in other parts of the world would be visited only secondarily.

But there were good reasons to visit other places. Because of the high altitude of Yellowstone, water boils at around 92.5 C. To get higher temperatures, one must visit thermal areas at lower altitudes, or in underwater marine habitats. Also, there were interesting biogeographical questions that could only be considered by visiting other sites.

Certainly, Iceland qualifies here, but because of its geography it is more difficult to study than Yellowstone. I looked for other places, and New Zealand was top of my list.

Other locations were mostly visited when I was travelling to distant areas for other reasons. This made sense both for time and money.

Also, I wanted to see if the same thermophilic acidophilic organisms occurred around the world. Were the same genera and species present? And if so, were they the same strains?

Except in one rare exception, we isolated *Sulfolobus* cultures from similar habitats all over the world. These acid, thermal habitats are often small areas within a large area of "normal" habitats. How are these organisms spread world-wide? How do they find these tiny habitats?

## Thermal areas visited around the world

**Location;**Year;Notes

**Beowawe, NV;**1969;Isolated thermal sources and geysers in desert; later destroyed by geothermal drilling

**Calistoga, Calif;**1968;*Thermus aquaticus*

**Dominica;**1971;Caribbean; Boiling Lake; *Sulfolobus, Cyanidium*

**El Salvador;**1970;Ahuachapan (United Nations thermal energy project); *Sulfolobus*

**Furnas Valley, San Miguel; Azores;**1966;In the middle of the Atlantic Ocean; interesting biogeographically

**Hunter Hot Springs, Oregon;**1969;Castenholz research site; low-altitude boiling springs

**Iceland;**1965;Thermal gradient research; also Surtsey

**Iceland;**1966;Thermal gradient research; also Surtsey
**Iceland;**1970;Thermal gradient research; also Surtsey; NASA funded expedition
**Iceland;**1972;Kjerlingarfjoll (Sulfolobus); also Surtsey
**Italy;**1966;Ischia, Agnano, Solfatara

**Italy;**1967;Ischia, Agnano, Solfatara; under sea hot springs
**Italy;**1968;Ischia, Agnano, Solfatara
**Italy;**1969;Ischia, Agnano, Solfatara
**Italy;**1970;Arco Felice research laboratory; Pisciarelli; *Sulfolobus*
**Japan;**1970;*Cyanidium*, thermal cyanobacteria; acid hot springs

**Kilauwea, Hawaii;**1969;In the middle of the Pacific Ocean; interesting biogeographically; Sulfur Banks

**New Zealand;**1969-1970;Extensive research work; travel partly paid by Cyanophyte symposium

**Steamboat Springs, NV;**1965, 1969;Convenient to reach from Reno; fumaroles; geothermal drilling

**The Geysers, Calif;**1965, 1969, 1971, 1972;*Cyanidium* habitat; Large geothermal field, formerly natural, but drilling has eliminated surface activity

## The Waring Guide to Hot Springs of the World

The Waring guide to hot springs of the world was a major resource. (Waring, Gerald A. "Thermal springs of the United States and other countries of the world---a summary. Geological Survey Professional Paper 492; 1965.) This extremely valuable USGS publication came out in 1965, just at the time I was starting my serious Yellowstone work. I bought a copy and kept it handy in my office. Among other things, the report had detailed maps to the locations of hot springs. The map of Iceland alone has 516 separate thermal areas described, mapped, and referenced. Some of the entries had detailed descriptions, including chemistry. (The references alone, 3733 entries, provide an outstanding bibliography.)

The Waring publication is now available on line at this URL:
https://pubs.usgs.gov/pp/0492/report.pdf

Many of the areas of the world I visited were in the throes of developing geothermal power. If drilling had been successful and power was being generated, then any surface geothermal springs or hot ground might no longer exist. This was the case in some of the later years I visited The Geysers, California. Also, in the major geothermal power project near Lake Taupo in New Zealand, development had already gone too far, and, Wairakei, the major natural thermal area, had already been ruined. In other cases, a thermal feature might have been "captured" for use as a spa, which might mean that the thermal feature was no longer flowing on the surface. Fortunately, for my work in The Geysers, Californa, and Beowawe, Nevada, I reached the sites before geothermal power work had gone too far.

# Dominica

Dominica is one of the Windward Islands in the Caribbean Sea. It is mostly covered with rain forest but is home to the world's second-largest boiling lake, conveniently called Boiling Lake. I knew about this site from the Waring book, and sent an inquiry to the tourist bureau in the capital city of Roseau. I received an answer that if I stayed at Island House, a resort up in the mountains, that establishment would arrange for my visit to the boiling lake.

In 1971 I was a Waksman Lecturer for the American Society for Microbiology and was asked to give a talk at the annual meeting of the Puerto Rico chapter, which was taking place on the Sunday of Thanksgiving week. After my lecture, Kathie and I flew to Dominica and checked in for a week at Island House.

The proprietor of the guest house arranged for a guide to take us through the jungle to the Boiling Lake thermal area. As I recall, it was a three-hour hike, with a trail that was barely visible. However, Dennis, our guide, was very agile and very considerate, and we had no trouble keeping up with him. (I was 45 years old.).

The first thermal area that we reached had an extensive area of hot acid ground, very much like habitats of Norris. I immediately saw areas with nice *Cyanidium*, and many small bubbling springs that were almost certainly habitats for *Sulfolobus*. We had our thermistor, of course, and wide-range pH paper, so it was easy to find sites that were likely to have *Sulfolobus*.

After a brief box lunch provided by Island House, we spent an hour taking samples. Our guide was worried because we had not reached Boiling Lake yet, although we could see its steam column past the next hill. Because we were in the tropics, where it gets rapidly dark at six PM, and because it was going to be at least a three-hour hike back, I elected to forget about Boiling Lake, because we were finding what we needed right there. I was right, and samples yielded either *Sulfolobus* and *Cyanidium* cultures which we isolated on return to Madison.

We got back just before dark. On the way back, Dennis stopped at a small palm tree and with his machete cut out a nice stalk of "hearts of palm", which Island House fixed for our dinner.

# Dominica: The Valley of Desolation I

*Kathie and guide "Dennis" taking a lunch break at the edge of the Valley of Desolation, before starting our sampling. November 24, 1971.*

***PICT0080.jpg***

# Dominica: The Valley of Desolation II

*Plenty of thermal activity here,* a*nd lots of typical hot acid habitats.*

***PICT0082.jpg***

# Dominica: The Valley of Desolation III

*Kathie taking notes.*

*PICT0081.jpg*

# El Salvador

Ed Deevey, a program director at NSF, told me about the geothermal energy project that was underway in El Salvador. In August 1970 I was going to Mexico City for the International Congress of Microbiology, and arranged to fly down to El Salvador after the meeting. I wrote ahead to the geothermal project office and gave them my flight arrival information. When I arrived at the airport I was met by Dick Glover, the New Zealand geologist, who I had met the year before at Rotorua. I spent three days in the area, staying at the International Hotel in San Salvador. (This was politically a quiet time in El Salvador.)

The thermal activity at El Salvador is in the Ahuachapan area in western El Salvador near the Guatemalan border. When I was there, the geothermal work on energy development was under the auspices of the United Nations.

Glover had arranged for me to be taken to the thermal areas by a local geologist whose wife was a microbiologist. This couple took me not only to the main geothermal project, but also to some natural thermal areas reachable only by 4-wheel drive truck. We spent a whole day touring and sampling. Several new strains of *Sulfolobus* came from El Salvador.

# El Salvador I

*El Salvadorian geologist and his microbiologist wife standing next to a natural thermal feature. August 13, 1970.*

***PICT0031.jpg***

# El Salvador II

*Geothermal power does not come from surface thermal areas, but from deep within the ground. Wells must be dug (and capped). The two cylinders are exhausting waste steam. August 13, 1970.*

***PICT0032.jpg***

# El Salvador III

*A thermal area in El Salvador made into a soccer field. The open hill beyond had nice sites for Sulfolobus. August 13, 1970.*

***PICT0253.jpg***

# Japan

Japan is famous for its hot springs and the ryokan (traditional Japanese inns) that are often associated with them. Most of the hot springs in Japan have been "captured" but some had free-flowing sources. I needed to have a "local guide" who could help me find my way. Through scientific contacts I was able to arrange for two Japanese scientists who took me around. I visited Japan in January 1970 on my way home from the Cyanophyte meeting in India. This was unfortunately winter in Japan, and most of the springs I visited were in areas with heavy snow.

Yatsuki Saijo, a limnologist from Nagoya University, was working on an acid lake in the northern part of the main island of Honshu. I traveled with him and his graduate student by train from Tokyo to a mountainous area west of Sendai. In that area was Onikobe Hot Springs, one of the "famous" springs of Japan. This spring is of neutral pH and supports good populations of thermal cyanobacteria. I was interested in seeing Onikobe because there had been quite a few Japanese papers published on this spring.

I was also led to several hot acid springs by Kimio Noguchi, a geochemist from Tokyo Metropolitan University. (The following year Noguchi and two associates would spend the summer with us in Yellowstone.)

# Onikobe Hot Springs, a famous spa in northern Japan

*The edge of Onikobe Hot Springs, with Saijo's student taking samples. January 19, 1970.*

***PICT0017.jpg***

# Lake Katanuma, an acidic lake in northern Japan

*Getting ready to sample Lake Katanuma, an acidic lake in northern Honshu. Saijo and his student. We lugged all the equipment by train from the Tokyo area. The lake was moderately thermal but fortunately was frozen enough to walk on. The raft was towed along for safety. January 16, 1970. Katanuma is a crater lake in the Narugo volcanic system. It is 21.3 meters deep and has a pH of 2.0-2.2.*

***PICT0030.jpg***

# Kusatsu, a famous acidic hot spring


*Kusatsu, another famous hot spring in Japan. This spring has a pH of 3.0 and has a large flow, enough to support many hotels and ryokans. Prof. Noguchi standing at the edge of the channel. Another site we visited was Manza Hot Spring, a very acidic spring (pH 1.0) serving a major ski area a few hours west of Tokyo. Photo taken January 19, 1970.*

***PICT0019.jpg***

# New Zealand

This was an outstanding trip.

New Zealand is a great country, especially for one interested in thermal areas. When I was there (Dec 1969-Jan 1970), it was very inexpensive ($8.00 U.S. for a motel room with kitchen; $2.00 for a whole bag of groceries); the people were great (they all spoke English!), the weather was good (their summer), and the thermal areas very accessible, very well organized, and very interesting. Because of the lower altitude, temperatures of boiling springs were at least 8 C greater than at Yellowstone (99-101 C).

My visit was part of an extended Asian trip that arose because I was invited to lecture at a Cyanophyte symposium in Madras, India. Louise and I left in early December 1969. We spent several days on the big island of Hawaii (the thermal areas at Kilauea); then on an overnight flight to Auckland, for our visit to the North Island. This is where all the really interesting thermal areas are situated.

I had written the geological people on the North Island ahead of time, and was put in contact with Richard B.Glover, whose specialty was geochemistry of hot springs. He gave me detailed maps of the thermal areas, and also took us on a trip to some areas he thought might interest us. It turned out that no microbiological research had been done on New Zealand thermal areas, so we had a free hand to do what we wanted.

We rented a VW “bug” for the three weeks we were there. We started out with a motel reservation for one night in the Rotorua area. After that, we reserved just one night ahead, never having trouble finding a room, despite the fact that this was the Christmas holiday season.

There seemed to be no restriction on access to any thermal area, or to any individual site. No one stopped us from visiting or sampling and there were good trails everywhere. Some of the thermal areas in the Rotorua area were owned (or managed) by the Maori people, but we were able to visit these also without restriction.

We had our usual field equipment: thermistor with several probes, pH meter, Zeiss phase microscope, many WhirlPak bags, sample vials, plastic sampling bottles, microscope slides, mailing tubes (for sending samples back to Bill Doemel for culture). We bought anything additional that we needed at hardware or grocery stores (for instance, nylon cord and string for slide studies; a bamboo fishing pole for sampling where the water could not be reached from the edge).

My extensive work in Yellowstone hot springs made it possible for me to focus on the especially interesting New Zealand springs and skip those of little research interest. A quick measurement of temperature and pH was all it took to decide whether a spring should be sampled.

# Tom sampling at Waimangu Caldron, New Zealand

*Tom Brock sampling at Trinity Terrace, at the edge of Waimangu Caldron. The outflow provided a pH gradient from above 7 to less than 3, with essentially constant temperature. This provided another piece of evidence regarding the lower pH limit of cyanobacteria. Photo Dec 19, 1969.*

***PICT0228.jpg***

# Sampling a hot spring at Oraki-Korako, New Zealand

*Louise sampling a spring at Oraki-Korako. The blue bag with red trimmings served as our small field bag for many expeditions. Big enough to carry sampling bottles/bags, field book, pH paper. The indispensable thermistor is on the grass next to the spring. New Zealand had many springs with large effluent streams. Photo Dec 16, 1969.*

***PICT0258.jpg***

# Geothermal power area, New Zealand

*New Zealand has been a pioneer in geothermal power development. This is an overview of the Wairakei Geothermal power development, near Lake Taupo. Removing superheated steam from the depths of a thermal area drastically modifies its surface manifestations. The natural Wairakei thermal features near this power installation had almost completely dried up. Photo Dec 15, 1969.*

***PICT0259.jpg***

# Lake Rotokawa, New Zealand

*Dick Glover's Land Rover on dry sinter at the edge of Lake Rotokawa. Our first day "Dick" took us around some of the thermal areas. The detailed maps he supplied were indispensable. Dec 24, 1969.*

***PICT0261.jpg***

# Entrance to the Tikitere thermal area, New Zealand


*Decorated gate at the entrance to the Tikitere thermal area. The style of decoration is Maori. Photo December 20, 1969*

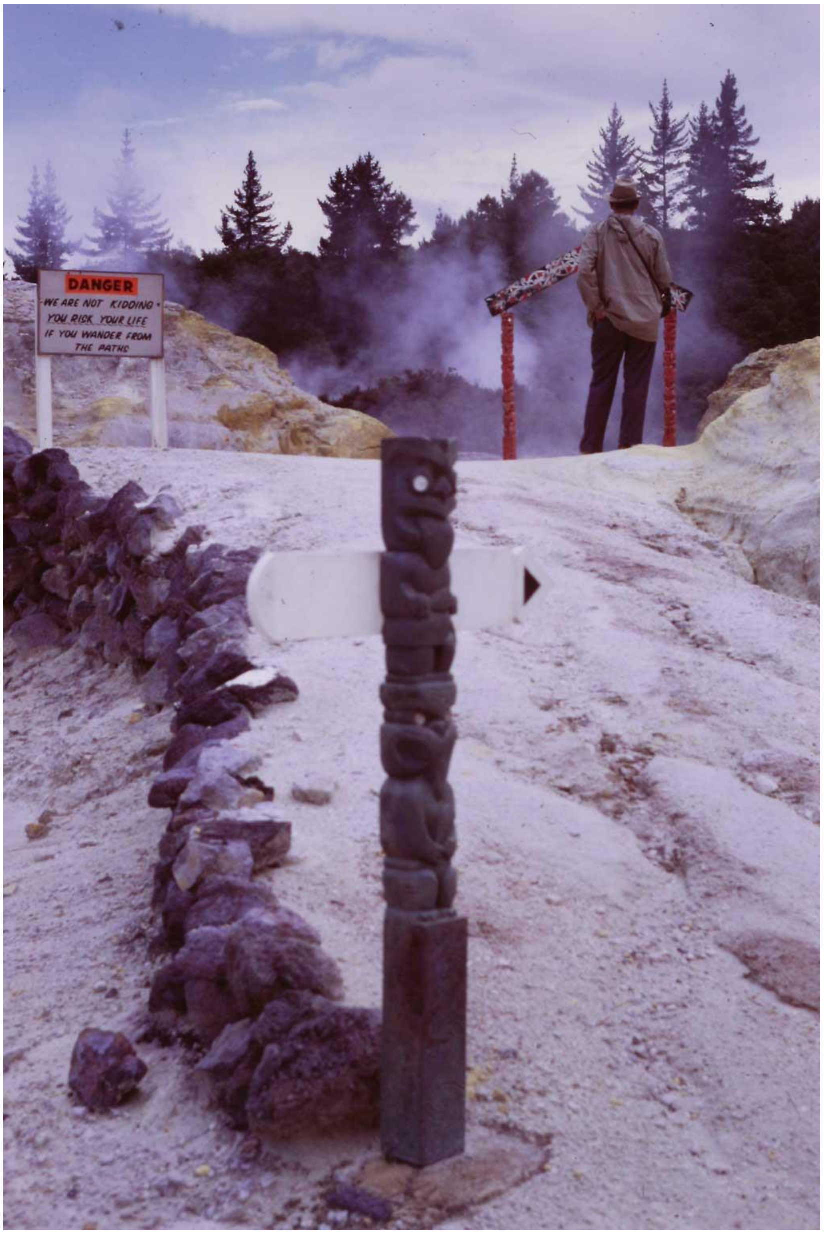
***PICT0262.jpg***

# Hot, acid thermal area, Tikitere, New Zealand

*Tikitere thermal area. Louise walking carefully between various hot acid thermal sites. The blue shoulder bag with red trimmings carried sampling materials. The black patches on the acid sinter are colonies of Zygogonium, similar to what is found in Yellowstone. New Zealand had vast areas of acid thermal habitats. At the time we visited New Zealand, we had not yet discovered Sulfolobus. Later, Ben Bohlool isolated typical Sulfolobus from New Zealand habitats. The stream nearby is a trout stream. Photo December 20, 1969.*

***PICT0263.jpg***

# Whakarewarewa., the main thermal area in the Maori reservation




*TD Brock on the path in Whakarewarewa. This area is owned by the Maori but is open to the public. We sampled many of the boiling pools in this area. The sign says:* ***Danger. We are not kidding. You risk your life if you wander from the paths.*** *Photo December 20, 1969.*

***PICT0264.jpg***

# Waiotapu, one of the tourist attractions in New Zealand


*Waiotapu (Maori for "sacred waters") is one of the tourist attractions of the New Zealand thermal area. There are numerous geysers and hot springs at this site. Lady Knox Geyser erupts daily at 10:15 AM. No, not another Old Faithful, but a geyser that will erupt when it is "soaped". We watched the supervisor pour a whole box of soap chips into the geyser cone. Within minutes it erupted, sending out a huge wash of soapy water. Tourists were told that the erupting water was not hot, and were invited to stand under the splash. Several people did so, but started to look uncomfortable being covered by soapy water. See Wikipedia for details on Waiotapu and Lady Knox Geyser. I've also witnessed geyser soaping in the Geysir area of Iceland. Fortunately, this practice never got started at Yellowstone! Photo December 19, 1969.*

***PICT0265.jpg***

# Louise sampling at Waimangu Caldron, New Zealand

*Louise sampling at Trinity Terrace with Waimangu Caldron in the background. Note the WhirlPak bags, which worked well for taking small samples of water or microbial material. This acid lake is said to be the largest hot spring in the world. The caldron was formed by an eruption of nearby Mount Tarawera in 1886. When I visited, this lake had a pH of 3.8 but the temperature was above 60 C, so there were no algae present because the pH was too low for high-temperature cyanobacteria, but too high for eukaryotes. The acidity is due to the oxidation of sulfide by high-temperature bacteria, probably Sulfolobus (which had not yet been discovered at the time of my visit). See other photo for a discussion of the interesting pH gradient that I found here. The whole area is protected as a Scenic Reserve, now administered by the New Zealand Department of Conservation. Photo December 19, 1969.*

***PICT0266,jpg***

# Measuring bacterial growth in a New Zealand boiling spring

*A boiling spring rich in sulfur at the Maori reserve Whakarewarewa (Rotorua). Note the simple experiment. A nylon cord was tied at one end to a microscope slide and at the other end to a rock. There are seven slides immersed in this pool. The slides were left for a week and then retrieved and examined under the microscope. Despite the fact that this area was open to the public, the experiment was not disturbed. Photo December 18, 1969.*

***PICT0267.jpg***

# Maori swimming pool in the thermal reserve, New Zealand

*Most of the springs and pools are too hot for swimming, so this warm pool was built for use by Maori children. The Maori Reserve at Whakarewarewa (Rotorua). Photo December 18, 1969.*

*PICT0268.jpg*

# Volcanic landscape in the North Island, New Zealand

*The North Island of New Zealand is an area of active volcanism. Although there were no eruptions when I was there, several of the volcanos in this region have erupted since. Ngauruhoe erupted in 1977, Okataina Caldera in 1973, Ruapehu in 2007, and Tongariro (on left) in 2007. Photo December 23, 1969.*

***PICT0270.jpg***

# Ketetahi hot spring, in the mountains of New Zealand


*After finishing our sampling schedule on the hot springs, we did a mountain hike which brought us up to this hot spring at 4500 feet elevation. December 23, 1969*

***PICT0271.jpg***

# Italy

My Italian work on hot springs overlapped with the marine work I was doing at the: Stazione Zoologica, Naples. My first work in Naples was done on a sabbatical from Indiana University in spring 1966, before I had done any of my major work at Yellowstone, but I managed to visit several of the Italian thermal areas in the Bay of Naples.

The Naples thermal areas are ancient, having been used by the Romans at the time of Tiberius (AD 14-37). Most had been heavily modified and were hence of little interest for research. The only one that was still in its “natural” state was Solfatara. It was not until October 1970, after we had discovered *Sulfolobus* and was working on it intently, that a visit to the Solfatara area made sense. This was when I had been invited to spend two weeks in a laboratory in the area of Arco Felice and help the Italian workers who were working on thermophiles. As it happened, they had independently discovered *Sulfolobus!*

# Solfatara, a famous thermal area in the Naples area


*Solfatara, Italy. This is a small isolated acid thermal area in a Naples suburb near Pozzuoli. The site was known to the Romans. Theoretically, one must have a guide to visit this area. However, I never saw a guide, or anyone else who might be in authority. A small area for tent camping was at the edge of this thermal area. Photo May 1, 1966.*

***PICT0064.jpg***

# Another view of Solfatara

*Solfatara, Naples, Italy. The remains of a building where one might receive "treatment" for an ailment. The Italians still believe in the therapeutic powers of hot springs, fumaroles, and mineralized waters. Cyanidium and Sulfolobus abound on this hillside. On the opposite side of this hill is Pisciarelli, an undeveloped acid thermal area, from which I isolated several strains of Sulfolobus. Photo May 1, 1966.*

***PICT0065.jpg***

# Abano spa, near Padua in Northern Italy

*Abano, a spa in northern Italy. This site has been used since Roman times, and is mentioned by Pliny. There were no natural hot springs, and this artificial channel was the closest thing I could find to a thermal gradient. Unfortunately, the temperature (Louise checking) was below 50 C. Photo March 25, 1966.*

***PICT0063.jpg***

# Iceland

I discussed my first visit to Iceland and Surtsey in the main text, as it was critical in getting me started on the Yellowstone work.

I visited Iceland four times (1965, 1966, 1970, 1972), each generally for a two-week visit except in May 1966 when it was only one week. Although continually interesting, Iceland was not nearly as easy for field work as New Zealand. The thermal areas were scattered over vast areas, the road system was limited, and the facilities were either primitive or non-existent.

However, it was a pleasure to visit the Geysir Geothermal Field, where the first thermal feature named "geysir" exists. Although Geysir no longer erupts, a few other features still erupt. On my first trip to Geysir, I watched, appalled, when Strokkur was "soaped" to induce eruption for a bus-load of British tourists.

The best trip was the one that Kathie and I took in June-July 1972. We rented a Land Rover and went off into the north country, staying in small unfurnished huts that were scattered about in the interior. This trip culminated at Kjerlingarfjoll, an large acid thermal area where *Sulfolobus* was very common. This far north there is 24 hours of light, making sleeping difficult. One day we started out to sample at 4 PM and finished near 10 PM.

# The Land Rover in the far north of Iceland

*The Land Rover in the interior of Iceland. On the way to Kjerlingarfjoll. This was late June when there was still snow in the mountains. We forded 15 rivers on the way to Kjerlingarfjoll. Photo July 2, 1972.*

***PICT0230.jpg***

# Tom Brock sampling an active fumarole on Surtsey, the new volcanic island off the south coast of Iceland

*Tom Brock at a large steam vent in Surtsey, the new volcanic island. This was my fourth visit to Surtsey, and the most fruitful. My work here led to a nice paper in the Scandinavian journal Oikos that still gets cited. Photo by Kathie. July 4, 1972.*

***PICT0229.jpg***

# Hawaii

The Hawaii islands visit took place on the way to New Zealand.

The Hawaiian Islands are interesting biogeographically because of their great isolation in the middle of the Pacific Ocean. Our visit was to the big island of Hawaii, which is the island with the most thermal activity. When I was there (December 1969) the volcano Kilauwea was erupting. Also, there were numerous steam vents along the roads. But the most interesting site was Sulfur Banks, small but active, that should have provided habitat for *Cyanidium* and *Sulfolobus*. However, both seemed to be absent, both visually and from attempts at culture. (We sent samples back to IU for Doemel to culture.)

The most interesting hypothesis is that, since Hawaii is so new, there was not enough time yet for inocula to reach suitable habitats. The Big Island is about 300,000 years old, and is a long distance from any other thermal area at which these thermoacidophiles might be present.

# Sampling at the Sulfur Banks

*Tom Brock  sampling at the Sulfur Banks on the Big Island of Hawaii. Photo December 6, 1969.*

***PICT0227.jpg***

# The Geysers, California

The Geysers, in Sonoma County, California, is now the largest geothermal power complex in the world but when I first visited in December 1965 development was just starting and the area was still mostly a natural area. As the years went by, geothermal power development continued, and now (according to Wikipedia) there is a complex of 22 geothermal power plants taking steam from more than 350 wells.

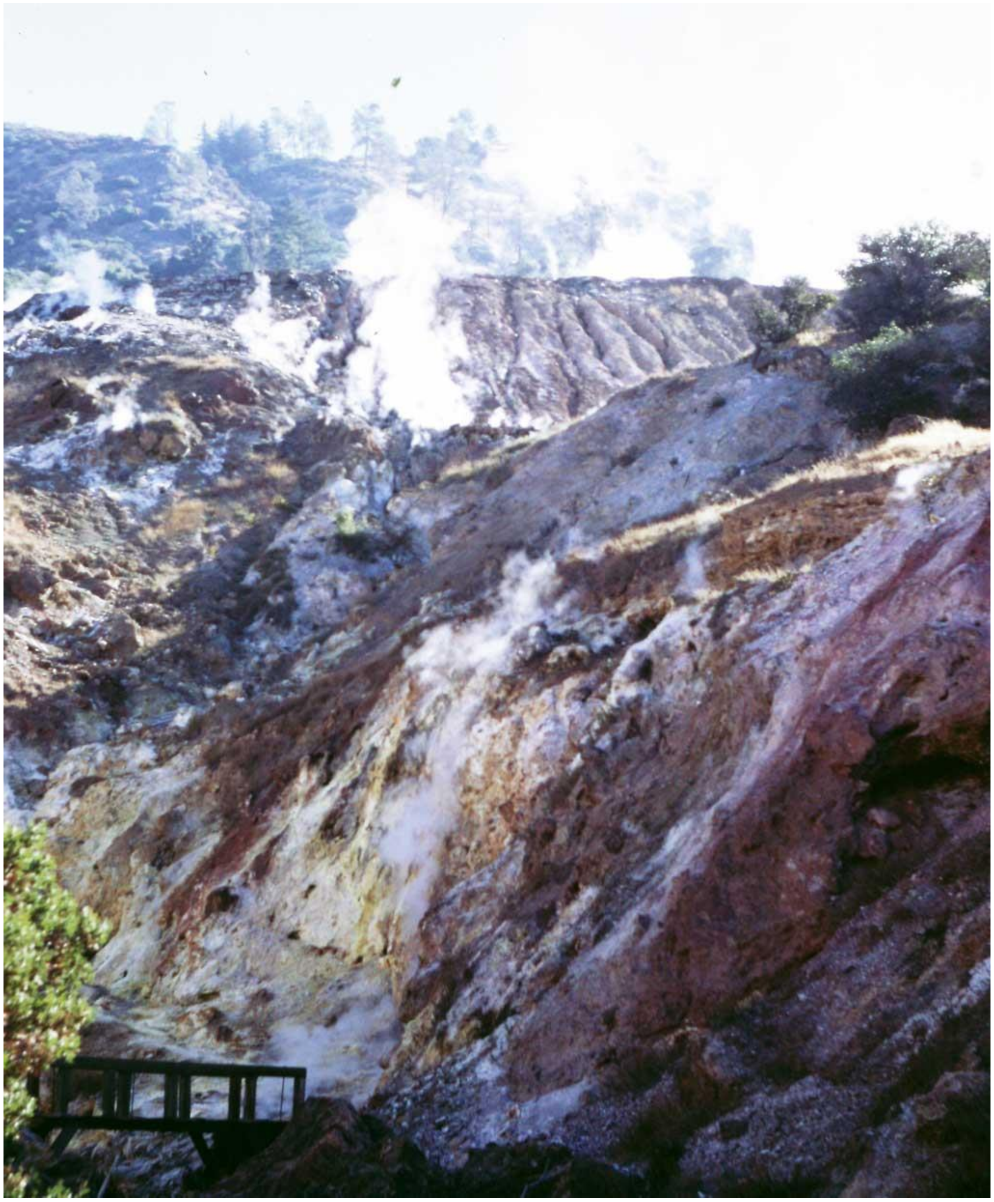
My interest in The Geysers first arose because it was mentioned in Mary Belle Allen's article on *Cyanidium* in 1959. She isolated this organism first from Lemonade Spring, at The Geysers, and stated that the temperature where found was 70-75 C. If this temperature were correct, it would make *Cyanidium* the most thermophilic eukaryote known. However, measuring temperatures of hot acid areas is difficult, as there are so many microenvironments. My visits to The Geysers were an attempt to verify Allen's record. In 1969 Doemel and I visited the site together and could find no *Cyanidium* site at The Geysers where the temperature was greater than 55-57 C. This is the upper limit for *Cyanidium* found world-wide.

# The Geysers, a major geothermal installation in the Mayacamas Mountains, California

*Visit to The Geysers, California January 31, 1971. Kathie and friends.*

***PICT0037.jpg***

# Lemonade Creek in The Geysers, California, in 1969



*Lemonade Creek in The Geysers valley. Typical hot acid habitat where Cyanidium was first isolated in North America by Mary Belle Allen. Careful temperature measurements by Doemel showed the upper temperature limit of 55-57 C. Drilling has become so extensive since the 1970s that it is unlikely that any of this thermal area still remains. Photo August 26, 1969*

***PICT0040 crop&adjust.jpg***

## Lassen Volcano National Park

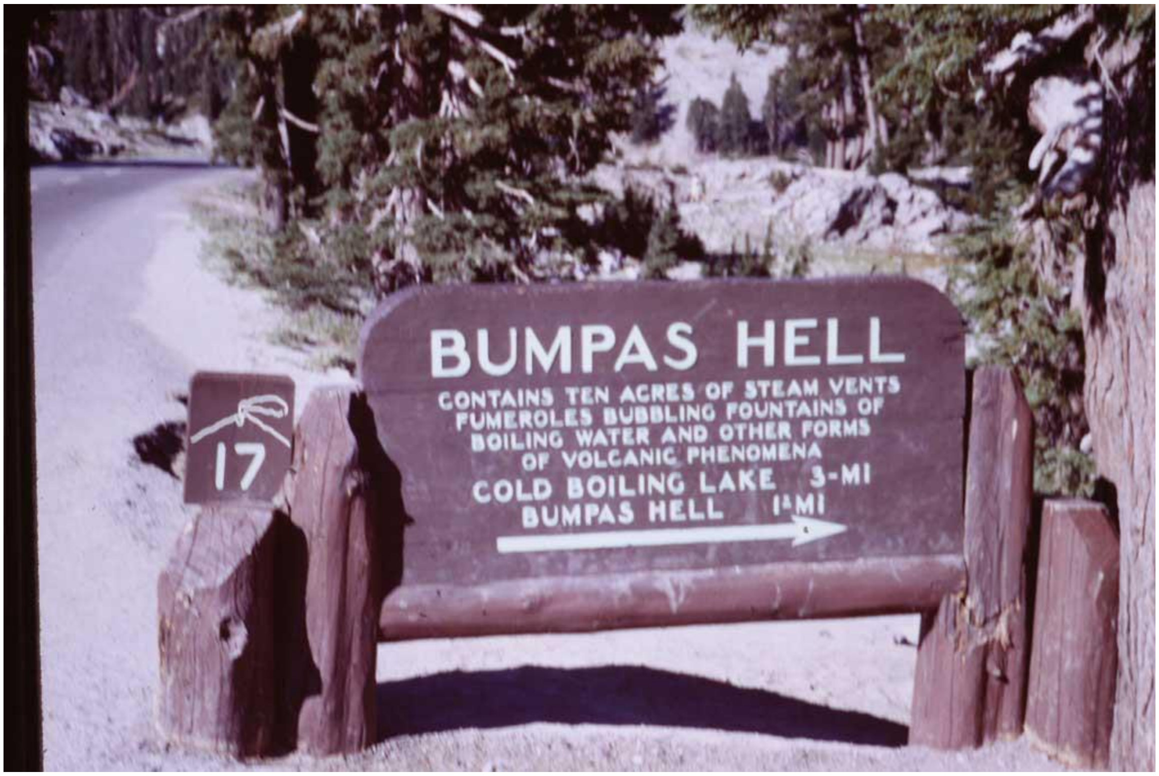
On our one-week western trip, Bill Doemel and I visited a number of thermal areas in western United States. Most of these I had identified from the Waring book on hot springs of the world. Lassen Volcano National Park was particularly interesting because of thermal areas surviving from the last eruption of Lassen Volcano in 1915.

# Bumpass Hell, Lassen Volcano National Park


*Bumpas Hell, a thermal area in Lassen Volcano National Park. Photo August 24, 1969.*

***PICT0041.jpg***

# Lassen Volcano has several isolated thermal areas




*We also visited Drakesbad Hot Spring, a small alkaline spring with cyanobacterial mats similar to those at Yellowstone, and Devil's Kitchen, a three-mile hike through a virgin ponderosa pine forest. All of the thermal areas at Lassen were similar to those we had been working on at Yellowstone. Photo August 24, 1969.*

***PICT0044.jpg***

## Beowawe, Nevada

This was a former natural thermal area in the Whirlwind Valley, Nevada, about 10 miles east of the town of Beowawe. At the time we visited on August 29, 1969 there were indications that drilling for geothermal power was in the offing, but most of the thermal features were still undisturbed. At the time of our visit, we were able to study the *Cyanidium* mats in the effluent streams flowing readily from the hot springs.

Since our visit, Beowawe has been turned into an extensive geothermal power plant. It is one of the largest geothermal fields in the state of Nevada, with some of the highest subsurface temperatures. Because of the geothermal power development, it is likely that none of the habitats we studied persist.

# Thermal well at Beowawe, Nevada

*A geothermal well at Beowawe. There were no buildings or any sign of activity, but this drill hole was letting off constant steam. Photo August 29, 1969.*

***PICT0093.jpg***

# Beowawe, Nevada thermal features

*Beowawe. Thermal effluents flowing out and disappearing into the desert sands. Photo Aug 29, 1969. This was one of the locations where Doemel and I studied the temperature relations of Cyanidium.*

***PICT0094.jpg***

# Research After Yellowstone

It is worth telling briefly how I was able to tie my Yellowstone work into the study of the ecology and limnology of lakes that I did in the years 1976-1990. This is a very simplified treatment of a fairly complicated project. Mostly this will focus on the year 1976.

Lake Mendota is famous as a typical eutrophic lake, studied extensively since the 19th century by researchers at the University of Wisconsin-Madison. Blue-green algal cyanoacterial) blooms started to appear in the lake in the 1880s and have been a regular summer phenomenon ever since.

Lake Mendota could be seen out my office window, and nearby was the Crew Dock, where the racing crews put in their shells. I still had a fair bit of WARF money and used some of it to start doing lakes research. I bought a Boston Whaler and a 75 HP Evinrude outboard motor. We named the Whaler "The Procaryote", an appropriate name for a bunch of bacteriologists.

We set up a mooring near the Crew Dock, and bought a small rowboat to use as a dinghy. It was a five-minute walk to get to the lakeshore, and usually another 10 minutes to get the Whaler going. Thus, in 15-30 minutes after leaving the building, we could be in the middle of Lake Mendota. Quite a contrast to research in Yellowstone!

However, some Lake Mendota work had already been going in my laboratory for four or five years. The first graduate student to join my lab after I moved to Wisconsin was Dave Ward, who elected to work on Lake Mendota rather than Yellowstone. His project was entitled "Environmental limitations on microbial degradation of hydrocarbons in temperate lakes." Most of his work was on Lake Mendota, but he also spent one summer work in northern Wisconsin lakes. Although I advised Dave closely, he developed this whole project on his own, and watching Dave, when I started my lakes work in 1976, I had a good "feel" for what this sort of work was like.

For the first summer, Pat Remington and Kathy Olson were hired to conduct an extensive monitoring program. The Meteorology Department had converted an old house on Second Point (southeast shore of the lake) into a field laboratory, but were no longer using it. We set up our field laboratory in the living room of this house and used a nearby dock for our boat. The program was to monitor the cyanobacterial blooms as well as the key chemical and physical components of the lake. We monitored 8 stations and 6 depths, sampling every week from ice-out to ice-in. The idea was to get an integrated picture of the whole lake. We did routine limnological sampling (temperature and light profiles, Secchi disk). We took samples for cell counting and for chlorophyll. And we took samples for water chemistry, especially phosphorus (generally the limiting factor in most freshwater habitats), as well as other chemical components.

Most of the research methods we used had been developed many years ago by limnologists and were easy to follow. I had always been interested in lakes and limnology, and here I was actually doing that kind of research. As usual, I did a lot of

reading, both historical and scientific, and was able to integrate my research with the vast amount of previous work on Lake Mendota. Although there was a Limnology Laboratory and a Water Chemistry group on campus, I set up my own research program completely independently of them. After all, we were doing microbial ecology, not something that either of those groups did.

The first thing I discovered was that there was no shortage of undergraduate students interested in working part-time or full-time in the summer, especially on Lake Mendota. There was no problem regarding housing or transportation, as there had been at Yellowstone. Since I had been able to keep my NSF grant and AEC contract, I had no difficulty with funding. Also, there were new graduate students interested in lake work. And I also had post-doctorates and visitors. The summer of 1976 my lab was bigger than ever.

Graduate students who worked in one way or another on Lake Mendota or other Wisconsin lakes were Steve Zinder, Tim Parkin, Bob Fallon, Carlos Pedros-Alio, and Jenny Clyne. Post-docs involved were Al Konopka, Alex Zehnder, and Kjeld Ingvorsen. Visitors included Sergei Belyaev (Russia), Tony Walsby (England), David Lee (Canada), Hans van Gemerden (Netherlands). Technicians or student helpers involved were Ruth Kamrath, Jim Hoffman, Charlene Knaack, Joan Sesing, John Gustafson, David Janes, and Kathy O’dea.

# The lab group at UW-Madison, summer 1976

*Lab group summer 1976. The photo was taken on the crew dock. From L to R: Mike Madigan; Tony Walsby, Tim Parkin, Steve ,Zinder, Linda Pfotenhauer, Kathy O'Dea, Bob Fallon, Ruth Kamrath, Kathy Olson, Terri Swim, Pat Remington, Hans van Gemerden, Al Konopka, John Gustafson, Frank Remington, Denise Desens, Tom Brock. Tony Walsby was a visitor from England whose interest was on flotation by gas vacuolated cyanobacteria. Van Gemerden was a visiting professor from the Netherlands. Konopka was a post-doc. Madigan, Parkin, Zinder, and Fallon were grad students. Madigan and Zinder had been in Yellowstone the previous summer. Kamrath was a half-time technician. Missing was Charlene Knaack, who returned in 1977. Photo mid-July 1976.*

***Lab group 7-1976 1st Lake Mendota year BW241 see key on scan 2.jpg***

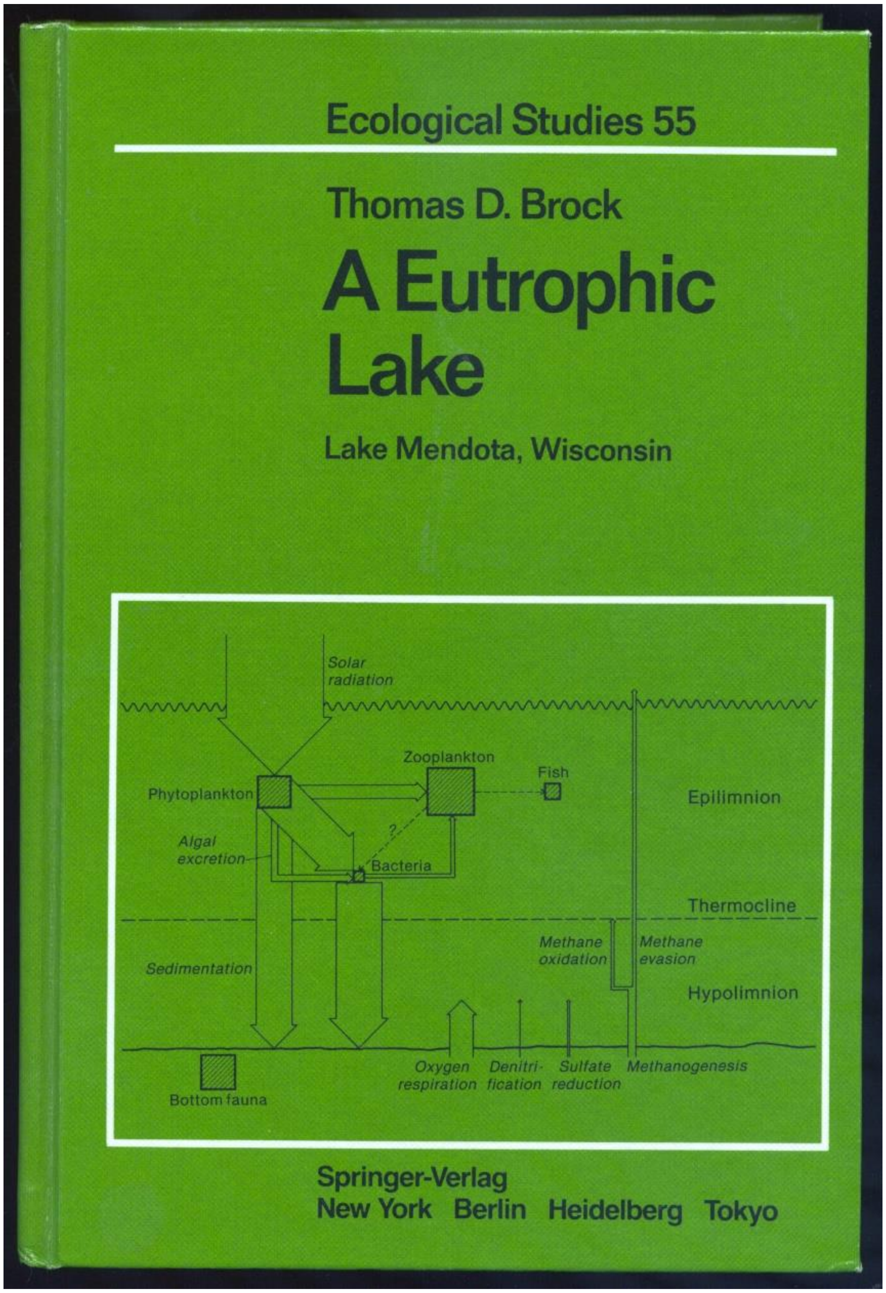
By the time I started the Lake Mendota project, mainframe computers had become widely available to academic scientists via what was called “time sharing”. Also in 1976 the world was on the cusp of developing what came to be called **personal computers**. My Lake Mendota work led me almost immediately into computers, which led me into computerized typesetting and publishing.

Over the next decade (1976-1986), lots of lakes work was done in my laboratory, not only on Lake Mendota, but on other lakes in Wisconsin, some large and some small. I also spent quite a bit of time at or near the Trout Lake Station that the Department of Limnology had in Vilas County in Northern Wisconsin.

In addition to the 40 research papers on lake-related work, I also wrote an extensive monograph on Lake Mendota published by Springer-Verlag.

See the Bibliography at the end of this section, which is separate from that above covering the Yellowstone papers.

# The Lake Mendota monograph




*The Lake Mendota book.*

***Lake Mendota book cover.tif***

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## Documentation

As a sometime historian of science I am aware that memory is faulty, and that what really shows what happened are written documents made at the time of the event. When I was closing out my lab and office in 1990-91 and moving to smaller quarters at the University of Wisconsin-Madison, I spent weeks creating a useful archive of the Yellowstone work. I bought a large Canon copier with a sheet feeder and had it delivered to my UW-Madison office. There I went over all my files and copied what I thought were the critical items. The originals were placed in archival boxes and donated to the University Archives, where they are available to any scholar. Saved in the UW-Madison archive are all key documents such as field and laboratory data books with raw data, correspondence, grant proposals and progress reports, and photographs. A complete index of the UW Archive boxes was made and bound copies prepared. The citation to this index can be found in the UW-Madison Library computer catalog: "*Index to the Thomas D. Brock papers at the University of Wisconsin-Madison Archives." Published by Thomas D. Brock, 2006. 186 pp. Non-circulating.*

For the present memoir, I am using the Canon copies I made in 1990-91 which are present in my home office. Since most of the Yellowstone work was published, the papers themselves also are part of the history. During the time that I was working, all research was published as printed (hard copy), and reprints were provided to the author. I had a single copy of each reprint bound (8 volumes from 1951 through 2005). A copy is at the UW-Madison archives, and another copy is in my personal library. Thus, as I write, I have access to all the key correspondence and other written documents, plus copies of the original published paper for study and comparison. In addition, there is the Yellowstone monograph published by Springer-Verlag.

In addition to the material listed above, there is a detailed memoir that I wrote at the time the work was being done. Sometime in the early 1970s I had these handwritten notes typed. I found the handwritten notes for more recent years in my files and had them typed in 2015. Thus, the typescript version of these notes, is in my files and provides the great detail that the present book reflects. This typescript contains all the information about which person lived in which cabin, who drove what vehicle from Indiana University or University of Wisconsin to West Yellowstone, what route was driven, and many other sundry details. Also, the Guest Book started in 1967 has names of most visitors to the West Yellowstone laboratory.

A listing of all the papers that deal with my work on Yellowstone, other thermal areas, and other sorts of extreme environments (low temperature, salinity, acidity), can be found in the bibliography in this memoir. Many of these papers are also available on the Internet and the links can be found by a Search. (For an almost complete list of my published work, see the Department of Bacteriology website.)

# The End