Dr. Marshall Elliott Bloom

This is an oral history with Dr. Marshall Elliott Bloom, conducted on April 27 and May 12, 2022, about his career at the Rocky Mountain Laboratories (RML) of the National Institute of Allergy and Infectious Diseases (NIAID). The interviews are being done via Zoom. The interviewer is Dr. Victoria Harden, Founding Director, Emerita, Office of NIH History and Stetten Museum, National Institutes of Health (NIH).

Harden: Dr. Bloom, please state your full name, that you know that this interview is being recorded, and that you give permission for the recording.

Bloom: My full name is Marshall Elliott Bloom. I am aware that this session is being recorded, and I approve.

Harden: Thank you. You were born on September 9th, 1945, in Dallas, Texas, the son of Jerry Irving Bloom, who was a pharmacist, and Tama Lipshultz Bloom, a homemaker. You were the elder brother of you sister, Jacqueline Sue Bloom, now Stanfield. Please tell me about your family, about growing up in Dallas, and about your education through high school, especially about any family members or teachers who nudged you towards medicine as a career.

Bloom: As you mentioned, I was born in Dallas, Texas. My dad was a pharmacist. My dad was from Minneapolis, and my mom was from St. Paul. My father had three brothers, two of whom were pharmacists, and the other brother was a dentist. There were two sisters, one of whom married a Canadian dentist and the other who married a man named Maurice Odoroff [Maurice E. Odoroff], who was a biostatistician at NIH, many, many years ago. I didn't know him very well.

I was raised in Dallas, Texas, which was a segregated community at that time. I went through public elementary school there and to Hillcrest High School. At Hillcrest High School, I had a really good biology teacher by the name of Mr. Brooks, who for some reason talked a lot about parasites and insects and infectious diseases. I first learned about biomedical science at that point. And since my dad and his brothers were pharmacists and owned a drug store, I got exposed to a lot of people in the medical field. A lot of our family friends were physicians in one specialty or another.

As a high school student, my dad had me work at the drugstore as a clerk on weekends and during the summers. I continued to do that when I was in college for a couple years, so I became familiar with what you might call the retail end of pharmacy and medicine. I've always felt that working in a pharmacy at that young age and having to deal with the public helped me learn how to interact with other people. I became comfortable talking to people from a variety of backgrounds.

One of the interesting features of pharmacies in those days in the South—and probably all over the country—was what were called “delivery boys.” They were typically African-American men who would deliver prescriptions all over town. One of the “delivery boys” at my father and uncle's drug store was a very capable man by the name of Alan Wilson London, nicknamed Lonnie. Right around 1960, my dad made the decision to have Lonnie work in the drug store as a clerk. This was quite unusual in Dallas, in

the South, at that time. I had the opportunity to work side by side with Lonnie and learn a lot from him. He was quite a character; we had a pretty good time.

I went to Hillcrest High School. I'm Jewish, and there was a synagogue not too far from where we lived. In addition, it was during my high school days that I started playing the five string banjo. Those were the days of the Kingston Trio, Pete Seeger, The Limeliters, and a variety of other folk musicians. I was actually in a quartet called The Four Coachmen in high school. And so, I became interested in traditional music and folk music. That's an interest which I've retained to this day. I did not do any athletics or anything like that in high school, but I was interested in Latin and was president of the Latin Club there for one, possibly two years. That served me very well—we'll talk about that when we talk about my college career. I graduated in 1963. Hillcrest High School is still there, but I haven't been back. In 1972, the year that I moved to Hamilton, my parents moved from Dallas to El Paso. After they moved, I never went back to any of my high school reunions. I kept in touch with a small number of high school classmates, one former girlfriend, and another friend of ours who died of colon disease about five or ten years ago.

Harden: From 1963 to 1967, you attended Washington University [Wash U] in St. Louis and you majored in classics. Tell me why you chose Washington University, and then about your undergraduate education, any special professors. And what majoring in classics brought to your subsequent career.

Bloom: I ended up at Washington University largely because of a cousin of mine named Floyd Bloom [Dr. Floyd E. Bloom]. He was a brilliant guy who went to medical school. Floyd went to Southern Methodist University in Dallas as an undergraduate, but then he went to Washington University School of Medicine in St. Louis. He subsequently had a distinguished career in the fields of neuroscience, neuropharmacology, and neurobiology. Floyd spent a number of years at NIH in the National Institute of Mental Health and worked at St. Elizabeth's Hospital in D.C. He also served on the faculty at Yale. He moved to the Salk Institute in La Jolla, California, for a few years, and then transferred to the Scripps Research Institute, also in La Jolla, to start a division of neuropharmacology there. He was a prominent scientist and even had a TV show for a while. I got an earful about Washington University because Cousin Floyd had gone there, so that was where I ended up for university.

When I arrived at college, I knew I wanted to be pre-med and was interested in science because of both family pressure and personal interests. We had to take a number of advanced placement classes as entering freshmen. I placed out of freshman and sophomore Latin, so I was able to start third year Latin classes in the Classics department. I knew that there was a language requirement, and I recognized that my skills in interpretation of English literature and things like that were not strong points. By deciding to major in Classics, I was able to place out of the language requirement and also the literature requirement. That decision turned out to be a smart thing. I enjoyed my Classics classed and professors. They were all terrific.

I also quickly realized that being a classics major could give me an advantage as I went along. First of all, because there were a lot of English literature classes I didn't have to take, I was able to take a lot of chemistry and a lot of biology. So my major was in classics, but I was only about a course or two short of majoring in either—or both—chemistry or biology.

One of my best friends, whom I met in classics, was a woman named Judith Weissman [Dr. Judith Weissman]. Her dad was a chemistry professor and a member of the National Academy of Sciences named Sam Weissman [Dr. Samuel I. Weissman], who had worked at Los Alamos, like a lot of the chemistry professors at Washington University. Judy took me over to meet her family, and they became almost an adopted family for me when I was an undergraduate. Her father was a wonderful guy, a wonderful scientist, terrifically funny, and very, very smart. Her mom [Dr. Jane Loevinger Weissman] held a Ph.D. in psychology. It was Sam, Dr. Weissman, who introduced me to classical music, of which I'm still a devotee. He also introduced me to the fun of doing scientific research.

I got to meet a number of prominent scientists in the Weissman home. Sam was good friends with a woman named Rita Levi-Montalcini [Dr. Rita Levi-Montalcini], who won a Nobel prize in neurobiology and was a professor at Wash U. I also met a Hungarian Nobel Laureate named Albert Szent-Györgyi [Dr. Albert Szent-Györgyi], who, as a good friend of the Weissmans, would come and visit.

Dr. Weissman was a major influence in my deciding to pursue a career in research and in science. Some of the biology teachers were also incredibly important to me in my decision. One was Dr. Levi-Montalcini. Another was a neuroembryologist by the name of Viktor Hamburger [Dr. Victor Hamburger], who collaborated with Dr. Levi-Montalcini in the discovery of nerve growth factor, and, finally, a comparative anatomy teacher by the name of Florence Moog [Dr. Florence E. Moog]. Dr. Moog particularly had a reputation of teaching a very tough class. She was serious, but she was also very kind. One of the things that I experienced at Washington University as an undergraduate and as a medical student, was the number of women who were important professors. I like to think that has influenced my views.

When it became time to pick a major, I chose classics. I also kept playing the banjo, learned how to play the guitar a little, and played with different ensembles of friends when I was in college. I also was involved in student government a little bit, and was the president of our dorm for several years.

I wanted to go to medical school, and I applied to a number of schools, including Wash U Medical School. It and Harvard Medical School were the two I was really interested in, although I didn't consider myself to be a really a stellar candidate. I was offered an interview at Wash U Med School on a Saturday morning. The med school was right across Forest Park from the main campus, and there was a shuttle bus that went back and forth. I went over there for my interview and came back—and who knows how those interviews go, right? Especially so many years ago, I can't really remember. And then Monday morning I got a letter from the medical school and said, "Oh, crap. Well, I guess that's that." Much to my utter amazement, I was accepted into Wash U Medical School. I've seldom been so surprised in my life.

At that point, I dropped all my other applications except for Harvard. I interviewed in Boston with two faculty members whom I don't really remember. One I think was a pediatric neurologist, and the other one was a psychiatrist. I remember that the psychiatrist was a stern looking guy. He riffled through my file, he put his glasses down, and he said, "I see you're a Classics major." And I said, "Yes, sir. That's right." And he said, "What are you going to do if you don't get into medical school?" I decided that I probably shouldn't tell him I had already been admitted into Washington U. So I just looked at him with a deadpan expression and said, "Sir, I plan to start translating all the scientific journals back into Latin." And he gave me this very, very odd look. I probably should have made that remark to the other interviewer. I've always sort of felt that if I were interviewing a kid for medical school, and they had the nerve to answer a question like that, I'd accept him in a heartbeat. But I didn't get accepted to Harvard, so I ended up going to Washington University.

Harden: According to your CV, after you graduated in 1967 and enrolled in Washington University's Medical School, you returned to Dallas during the summers of '68 and '69 and attended summer school at Southern Methodist University. Why did you go to summer school? What did you study there before we get into your medical school career?

Bloom: I don't think those years are right because I did summer school when I was an undergraduate. So it would've been before 1967. I'm guessing it would've been the summer of '64 and maybe '65. I went to summer school at Southern Methodist University to take some courses I didn't want to take at Wash U because I would've had to forego a really good biology or chemistry class, and I didn't want to do that. And then I also decided to take physics at SMU summer school, because I knew that was going to be a real hardcore class at Wash U. And I didn't want to suffer the consequences. So I took physics at Southern Methodist University and actually thoroughly enjoyed it. So that's why I took those summer classes at SMU. And then of course I was still working at my dad's drug store during the summer when I was an undergraduate.

Harden: Tell me about your medical school training, especially about particular experiences, influential and professors, and other fellow students who might have influenced you.

Bloom: After I was accepted into Washington University Medical School, I remembered that my cousin Floyd had graduated from the medical school in 1960, and I recalled that he had done research in the anatomy department. So I decided to try and get a summer internship there before I actually started medical school. And so I did. I got a room in the dorm, I'm pretty sure, and I went to work for the man who was the acting chairman of the Department of Anatomy, an anatomist named Sam Lillard Clark Jr. [Dr. Sam Lillard Clark Jr.], whose specialty was the anatomy of lymphoid organs. He was also an electron microscopist, so that summer I learned the basics of doing electron microscopy. His father, Sam Clark Sr. [Dr. Sam Lillard Clark, Sr.], had been a famous neuroanatomist. The neuroanatomy textbook that we used was called Ranson & Clark [S. W. Ranson and S. L. Clark, *The Anatomy of the Nervous System, Its Development and Function,* 1961]. Dr. Clark, Sr. was one of the editors. We used to call it "Rank & Clank."

Dr. Clark, Jr., took me under his wing the summer before my freshman year of medical school. During my freshman year, he accepted a position as department of chairman at the new University of Massachusetts Medical School in Amherst. The summer after my freshman year, Dr. Clark moved to Boston and asked me to come with him that summer. He had a position at the Harvard School of Public Health on Huntington Avenue, where he worked until the new medical school was constructed. So I spent a second summer working with him, between my freshman and sophomore year of medical school. I lived in Cambridge and got to know Boston a little bit. And then came back to Wash U.

I'm going to tell you about the research I did at Wash U. I never got any papers out of it, but I did some research with a professor in pathology by the name of Joe Williamson [Dr. Joseph R. Williamson]. He was also an electron microscopist and was looking at kidney biopsies of people with lupus, doing electron microscopy on those.

Once I got attracted to pediatrics, I began working with a professor named Neil Middlekamp [Dr. J. Neil Middlekamp] at St. Louis Children's Hospital. He was also an electron microscopist and a virologist, and he let me do some electron microscopy on some of the biopsies and the material that they had there.

There were several professors whom I would call "main influencers" during my medical school training. One was John Kissane [Dr. John Kissane], a renal pathologist from Idaho. He was the first person to tell me about the Rocky Mountain Laboratory.* He became a good friend. He was one of the Baker Street Irregulars, or Sherlock Holmes devotees. He introduced me to Arthur Conan Doyle, author of the Sherlock Holmes canon. Dr Kissane was a wonderful man. He was a gross pathologist and specialized in renal pathology. He was very influential in directing me to research. Another major influence on my career was Ralph Feigin [Dr. Ralph D. Feigin], who was the head of infectious disease at St. Louis Children's Hospital. He was a brilliant guy with a terrific sense of humor. He became my mentor and avenue into the field of research on infectious diseases.

In our medical school class there were about 100 of us, plus or minus. I think there were about a dozen women in the class. And now, of course, the balance has shifted so that the majority of people in med school classes are women. I developed some long-lasting friendships with a number of the women who were in my class.

Med school and internship stories for men, my wife always says, are like war stories. They play the role of war stories for people who don't go into the service—all the escapades, adventures, and travails that we put up with as we went through medical school.

All of the classes were really good. I had another friend who became the chief resident of neurosurgery at Wash U, and I really enjoyed neurosurgery. But I found myself increasingly drawn to the area of pediatrics. I took care of a kid who had glomerulonephritis, a serious type of kidney disease, as a result of an Epstein Barr viral infection, the virus that causes infectious mononucleosis. I became intrigued as to how a virus might cause kidney disease. I did a report on experimental models of virus and disease in kidneys.

At that time there were two animal models of viral disease. One was lymphocytic choriomeningitis virus, LCMV, and the other one was called Aleutian disease in mink. Aleutian disease in mink turned out to be what we call an immune complex disease, where aggregates of antigens and antibodies deposit in the kidneys, disrupt kidney function and lead to renal failure. This is a process similar to what happens in people who have renal disease from systemic lupus erythematosus. So, I became really interested in viral infectious diseases of the kidney.

Harden: On June 18th, 1971, after graduating from medical school, you married Tonia Felix, whom you might tell me a bit about.

Bloom: I have already mentioned Judy Weisman, my good friend. Judy had gone on to get a Ph.D. in English and had returned to St. Louis to teach at Wash U and was living with her parents at the time. We had maintained our close friendship. Judy called me one day and said, "Marshall, I found the girl for you

* This facility was known as the Rocky Mountain Laboratory (singular) from 1931 until 1982, when its name became plural: Rocky Mountain Laboratories.

to marry. Here's what I'm going to do. I'm going to invite you over to my parents' house and I'm going to invite this girl, Tonia Felix, to come over, also." This was when I was a third year med student. I always knew that Judy was a much better judge of character than I was, so Tonia and I started dating and moved in together for my senior year of medical school. When medical school ended—med school ends around the 1st of June and you start your internship the 1st of July—we went on a camping trip to Western Colorado, where I had gone to camp as a kid. We did some camping and some hiking and ended up getting married on that camping trip. Immediately after we got married, Tonia flew to California to visit her grandparents, and I had to drive back to St. Louis to start my internship. So we were married the year of my internship.

Harden: Was Tonia a scientist?

Bloom: No, she had gone to Swarthmore College but following graduation came to St Louis where her father was a distinguished economics professor named David Felix [Dr. David Felix], who specialized in the economics of developing nations. They lived fairly close to where Judy and her family lived at that time. And I believe that Judy met Tonia in a history class. After they became friends, Judy called me up and the rest is history.

Harden: You did your internship at St. Louis Children's Hospital. Would you tell me about this year of your training?

Bloom: When I graduated from medical school, I was accepted into what were then called internships but are now first-year residencies. I did my internship at St. Louis Children's Hospital, where I had a wonderful training experience—terrific faculty, terrific nursing staff and ancillary care, wonderful patients, and a scientific approach to the practice of medicine and disease, what we now call pathophysiology and disease pathogenesis.

I could not have been trained at a better medical school anywhere in the world. And in fact, we had a couple of fellow pediatric interns who were from Harvard or some other high powered university, and it was clear that the medical education at Wash U was at least as good as theirs. As a pediatric intern during the Vietnam War, we had to make a choice as to what career options we were going to take. One of the options, which I know you've probably heard from other people you've interviewed, was to become what was called a "Yellow Beret" and go to the NIH to do research.

Harden: Before we leave your internship, I want to ask a couple more things. Your first two papers came out of your internship. The first one was on pediatric cardiology, and the second one was on tularemia, a paper on which you were first author. Will you tell me why you switched from cardiology to infectious disease and talk about your first experience in scientific publishing?

Bloom: One of the patients that I took care of was a young African American inner city boy who came into the hospital with a history of a very large lymph node on the side of his neck. One of the first things you would think of was tuberculosis, but it turned out it wasn't tuberculosis. After doing some

investigation—and reading some papers from the Rocky Mountain Laboratory—I suspected the oculoglandular form of tularemia. Now tularemia is not what you would think of as an inner city infection, but the boy had an uncle who went rabbit hunting out in St. Louis County. There is an old expression, "Never eat a rabbit you can catch," and evidently one of the rabbits that his uncle caught and brought home had tularemia and this young boy caught it. He was bumped around from hospital to hospital until he landed at St. Louis Children's.

At that time, the diagnostics for tularemia were not very good. The boy didn't have active disease at that time, and he'd had a lymph node biopsy of one of his enlarged lymph nodes. The results of the biopsy were nonspecific. Scientists at Rocky Mountain Laboratory had developed a skin test, and I made some inquiries to try to get some of that skin test antigen so that we could test it on this young man. I don't think we ever got any of that antigen, but I did learn some more about the Rocky Mountain Lab then. And we finally diagnosed his case by sending the lymph node biopsy to a woman whose name I think was Bertie Pittman [Bertie Pittman] at the Centers for Disease Control. She took a section of the lymph node and by using antibodies against the tularemia bacteria, was able to demonstrate that there were tularemia proteins in that lymph node. We felt that this confirmed the diagnosis of tularemia.

I was encouraged by the faculty to write that up as a case report called "Oculoglandular tularemia in an inner city child," making the case that just because somebody lives in the inner city doesn't mean they can't catch a disease which is considered to be rural. One of Dr. Feigin's mantras about taking histories from patients was to ask about pets, pica, travel, and consanguinity. I've never forgotten that. And I've drilled that into my younger son, who's an infectious disease doctor at Harvard now. So I did publish two papers from my internship work. The other was a paper about a familial heart block. We published that as a short letter.

Harden: When you finished your internship, you moved to Hamilton to begin your career at the Rocky Mountain Laboratory (RML). As you said earlier, you chose to discharge your military obligation during the Vietnam War as a "Yellow Beret" in the NIH Associates Training Program. Tell me about this.

Bloom: In the NIH training program at the time, there were three types of postdoctoral Associate appointments: Clinical, Research, and Staff. I ended up as a Research Associate.

When I had looked over the NIH's programs, I noticed a PI [Principal Investigator] at Rocky Mountain Lab by the name of William Hadlow [Dr. William J. Hadlow] and another by the name of Carl Eklund [Dr. Carl Eklund], were working on Aleutian mink disease. And because of my interests in renal disease and virus infections, this was an obvious place to end up. After my internship ended, Tonia and I packed up the car and a couple of cats, drove out to Montana and arrived right before the 1st of July in 1972.

I ended up under the mentorship of a bacteriologist named John J. Munoz [Dr. John J. Munoz], Jack Munoz, who worked on pertussis. After I was here about a year or so, I decided to begin some studies on Aleutian mink disease. The other individual who came to Rocky Mountain Lab at the same time I did was Bruce Chesebro [Dr. Bruce Chesebro], who was a couple of years ahead of me and had spent time with Henry Metzger [Dr. Henry Metzger] in Bethesda at what was then called the National Institute of Arthritis, Metabolism, and Digestive Diseases. Bruce was studying antibodies against phosphorylcholine, and he wanted to come out to RML to look at the immunogenetics of viral infections. He was also interested at that time in the disease of sheep called scrapie, which we now know as a prion disease.

Bruce had done some virology as a medical student. He was the person who got me started doing basic research into viruses.

Harden: I will come back to your research in detail, but first I want to ask you a different question. In 1972, Hamilton, Montana, was quite a different town from either St. Louis or Dallas. Tell me about RML when you arrived. Who was there? You've already mentioned a number of people. Tell me about the facility itself and how you and Tonia adapted to living in such a small town.

Bloom: At that time there was a weekly newspaper called the *Western News.* We subscribed to it, and it was a wonderful paper, very progressive, unlike anything in Montana anymore. It was written, edited, and published by a guy named Miles Romney [Miles Romney, Jr.]. Through the classified section, we were able to identify a house. The Director of Rocky Mountain Lab at that time was a veterinarian named Herbert Stoenner [Dr. Herbert G. Stoenner], and he helped us find the house. RML itself was quite a bit smaller than it is now. The buildings that were here at the time now comprise the Rocky Mountain Laboratories Historic District.

I remember that there was an actual switchboard operator named Irma Maus [Irma Maus] and her switchboard was just inside the front door of Building 1. It was an ancient switchboard where a person would call, and Irma would shuffle the plugs around to connect people. I don't recall what happened to that switchboard, but we may have given it to the Ravalli County Museum.

Before Tonia and I moved, we had called Dr. Stoenner for information and advice. I learned not to call RML during lunchtime, because Irma would leave the switchboard to have lunch. In the early 1970s, when you called RML during lunchtime or after hours, the phone got picked up elsewhere, and the response you got was usually, "boiler room," because the boiler operators were the only ones who were there 24 hours a day.

Hamilton had about 3,000 people at that time. The roads were paved and there was a single stoplight. There was electricity, but there were party lines in our homes. Some of the other faculty that were here—that's the term in use now, faculty—were Bill Hadlow, a real gentleman; Carl Eklund, John Munoz, and Edgar Ribi [Dr. Edgar E. Ribi], a physical biochemist who was studying *Mycobacteria* at the time.

Harden: Was Bill Jellison [Dr. William L. Jellison] still there?

Bloom: Bill Jellison, who was quite a character, had retired, but he was still around. There was also another, younger scientist working here by the name of Don Lodmell [Dr. Donald L. Lodmell], who was working on rabies with a RML staff member named Fritz Bell [Dr. J. Frederick Bell]. Don has received a Ph.D. from the University of Montana. I think he was the first Ph.D. in microbiology at the University of Montana. He then came back here to work. After a couple of years, he went out to the main NIH campus in Bethesda to work with Abner Notkins [Dr. Abner L. Notkins] in the dental institute before coming back to RML to start a program on rabies.

This is all leading up to a story about Bill Jellison. Bill was an ecologist, and he was always out in the woods. One day while we're sitting in our labs, this awful smell wafts down the hall. Jellison had found a

skunk, put it in a plastic bag and brought it in for Don to test. He just took that thing down the hallway to Don's lab at the end of the corridor, and the building smelled for some time.

One other thing about the Lab then were windows that you could raise and lower. The heating was steam radiators in each room or lab. There was no central ventilation system, and the only air conditioning was window units. There also were no biological safety cabinets at the Lab at that time.

There were also several other prominent investigators here at the time. One was Willy Burgdorfer [Dr. Willy Burgdorfer], who was a medical entomologist and who was working on ticks—although ticks are not insects but rather arachnids, so people who study them are properly called acarologists. Dr. Stoenner studied leptospirosis. I also remember during those early years, that our multipurpose conference room was located on the 3rd floor of Building 6. And we often had to use old lantern slides or transparencies when we presented.

It was an immensely fun place to work. The people were all very nice. A lot of the staff hiked, hunted and fished. Bruce Chesebro, who was really sort of my mentor throughout all this, was an avid outdoors person who had done a lot of hiking and hunting and fishing. I learned a tremendous amount about fishing, hunting and the outdoors from him, as well as an inestimable amount of science. He's one of the smartest people I've ever encountered.

In the 1970's anytime someone got a new shotgun or a new rifle, they would bring it into the lab and show it to everybody. Can you imagine that happening now at any federal institution? Not at all. The dialectic between people and firearms in that era was very different than it is today.

Harden: In 1978, your first child was born, Jesse David Bloom, and three years later, you had a second son, Seth Michael Bloom. Would you comment briefly on how you and your wife managed what is today called "work-life balance"?

Bloom: My wife was, at that time, active in the League of Women Voters, but primarily—and I don't consider this a pejorative term—she was a homemaker. I suppose any thoughtful man would be a bit embarrassed by the fact that there was no such thing then as "work-life balance." I mean, the husbands went to work, and the wives usually ended up taking care of the kids and all the family responsibilities. Tonia is a highly intelligent woman who has done amazing things at the community level out here, starting back around the time the kids were born. She was on one of the local school boards for, I think, 34 years. She went to Swarthmore. She had the opportunity to get a graduate history degree while she was at Wash U while we were in St. Louis, but she decided against it, and then we ended up getting married.

The kids were both born here. There was a parent-participation preschool that we helped start, and they went to that preschool. Then they went to one of the local school districts, the Corvallis school district, which isn't in Hamilton. It's an unincorporated community, but it has its own school district about seven or eight miles from Hamilton. Both Jesse and Seth got a phenomenal education at that school. Both of them were National Merit Scholars. And both ended up in science and are doing tremendously well.

Harden: You've already me that you had learned about Aleutian mink disease and had started working on it with Bill Hadlow, Bruce Chesebro, and Richard Race [Dr. Richard Race]. I believe that at least by the late sixties, there was thinking that perhaps Aleutian disease of mink might have been what was then called a slow virus disease and now called a prion disease. And I want to know about that early work in the seventies.

Bloom: This is not quite accurate. The term slow virus infections or slow infections was coined by an Icelandic physician-scientist named Bjorn Sigurdsson [Dr. Bjorn Sigurdsson]. Slow infections was a concept that he came up with for long, protracted infectious diseases that fail to induce a protective host response. The paradigm of those slow infections was scrapie, which is now the prototype of a prion disease. I never worked on scrapie early on. Aleutian disease of mink is the infection on which I focused. We knew about some work that an excellent pathologist at UCLA named David Porter [Dr. David D. Porter] had done, showing that Aleutian disease of mink was an actual transmissible virus infection that led to renal disease. The slow virus infections caused by prions, like scrapie, are almost all diseases of the central nervous system.

Bill was interested in slow virus infections and also in Aleutian mink disease. Once Bruce and I gained his trust, he began letting us do some experiments with the mink and with the virus. Aleutian mink disease is a disease that is clinically characterized by a bizarre increase in the gamma globulin fraction of the blood plasma. The gamma globulin fraction is where all the immunoglobulins and antibodies are found. In fact, the disease was also known in some places as infectious hypergammaglobulinemia because in some cases up to 50% of the serum proteins were gamma globulin. The only thing that comes close to something like that in the human disease world would be certain kinds of multiple myeloma.

In Aleutian disease, the tissues are infiltrated with special kinds of lymphocytes called plasma cells, which are responsible for secreting antibody. At that time, it was known Aleutian disease of mink was caused by a virus infection, and there had been some unsuccessful attempts to purify a virus from blood samples. I decided to undertake that project, and with Bruce's help was able to purify virus-like particles and demonstrate that in certain kinds of density gradients that virus infectivity correlated with these virus-like particles. They were small and looked like either a picodnavirus or a picornavirus, like poliovirus, or a calicivirus. “Pico” means “small.” The picornaviruses are small RNA viruses; the small DNA picodnaviruses were later renamed parvoviruses. I guessed that Aleutian disease virus was going to turn out to be a parvovirus based on its inactivation and chemical resistance profile. The first papers I published at RML were on Aleutian mink disease virus or AMDV as it is now known.

After I'd been here for a couple of years, Dr. Stoenner and the NIAID Scientific Director at that time, a man named John Seale [Dr. John R. Seale) asked me, "Would you like to stay for a third year?" I had fully expected to go back to St. Louis Children's Hospital and finish my pediatric residency and do an infectious disease fellowship. I called Ralph Feigin and I said, "Ralph," what should I do?" And he said, "My advice to you is to stay there and get as much research experience as you can. You're a very good clinician and you won't have forgotten that much, so I would advise you to go ahead and stay there for a third year." And so I accepted that third year. After the third year, they came back to me and said, "Well, what would you do if we made you a permanent staff member?" And I said, "Well, I would continue doing such and such, but if I accept the offer, I want to go to the main NIH campus in Bethesda for a couple of years and learn some molecular virology." I figured I was in a pretty good position. And again, I asked Ralph, “What should I do?" And he said, "Just keep doing the research because the clinical stuff will come back to you in a second." A week later, Dr. Stoenner called me in and said, "Okay, here's your

ticket. We're going to send you out to NIH. We've got an interview schedule lined up for you, and you're going to be staying with Steve Straus [Dr. Stephen E. Straus]." Steve Straus also worked on parvoviruses for a while. He died, unfortunately, very prematurely some years ago.

I went to Bethesda and interviewed with a few people. It was clear that Dr. Seale wanted me to work with an investigator in the Laboratory of Biology of Viruses on the third floor of Building 5 by the name of Jim Rose [Dr. James A. Rose], who was a friendly and very smart guy. He worked on adeno-associated virus, which is one of the parvoviruses.

Tonia and I didn't have any kids at this point, so we moved to Bethesda. For two years I worked with Jim on adeno-associated virus. My experience in that laboratory was amazing—I guess some people would say those were the halcyon days. I stumbled completely inadvertently into a circle of scientists who became the leaders of modern molecular virology. Norman Salzman [Dr. Norman P. Salzman] was the laboratory chief. Bernie Moss [Dr. Bernard Moss] was a section chief. Malcolm Martin [Dr. Malcolm A. Martin] was also a section chief. Malcolm and Bernie went on to become lab chiefs at NIAID. Jim Rose, with whom I worked, was the other section head. Jim was a smart guy, but he didn't place a premium on publications, so I published only one paper with him while I was there. But I made wonderful connections and friends during those two years: Peter Howley [Dr. Peter M. Howley], Ken Berns [Dr. Kenneth I. Berns], Richard Wyatt [Dr. Richard G. Wyatt], Al Kapikian [Dr. Albert Z. Kapikian], George Khoury [Dr. George Khoury], Mark Israel [Dr. Mark Israel], Malcolm, Bernie, Norman, and other people who floated in and out of that lab—I mean, it was like a real incubator or think tank for molecular virology. After two years, Dr Stoenner brought me back to Rocky Mountain Lab. Since then, I have always encouraged junior people at RML "If you have a chance to go spend some time on the main Bethesda campus, you should do it. The connections I made there were incredible." And I met Tony Fauci [Dr. Anthony S. Fauci] while I was there, too. Tony was a Clinical Associate, and Shelly Wolff [Dr. Sheldon M. Wolff] was his lab chief. I learned so much during those two years.

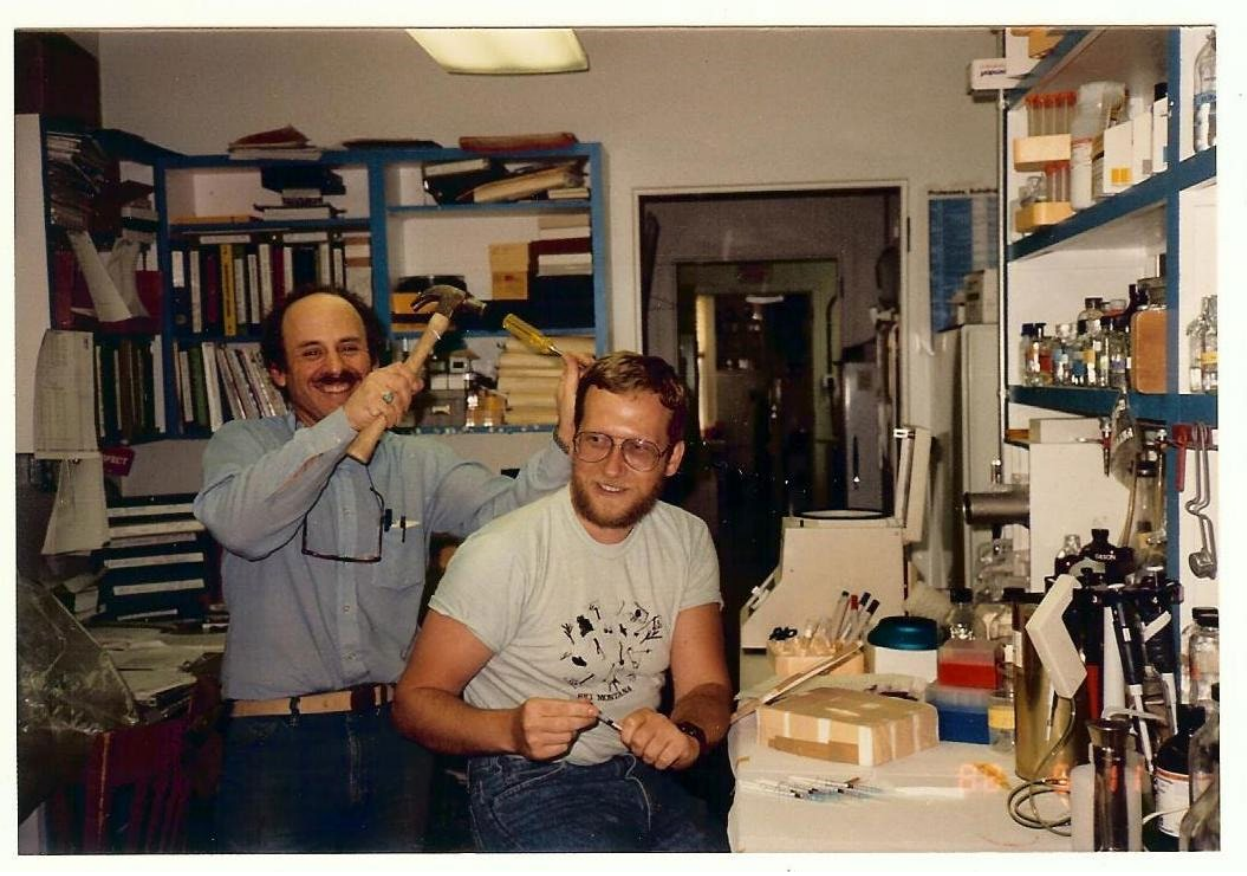
When I came back to RML, I started applying what I had learned about parvoviruses to the study of Aleutian mink disease virus. By that time, Bruce had started a research program on Friend virus, a retrovirus that infects mice. I didn't want to work on something that a lot of other people were working on, and there weren't that many people working on parvoviruses. The parvovirus people were also very friendly. So I stuck with parvoviruses.

We came back to Hamilton in the summer of 1977, and my first son Jesse, was born about a year later.

I continued working on Aleutian disease, which, in addition to being a cornucopia of interesting scientific questions, was also of major economic importance to the mink industry, because infected mink have reduced fertility and lower quality pelts. So the mink industry was interested in the research that I was doing, too, and actually funded some students to work with me over the course of the years.

Harden: By the late 1990s, you were able to construct a 3D structure of the parvovirus and define how it mediated infection, other aspects of its life cycle, and interaction with its host. Aside from one paper, in which you collaborated with scientists from the U.K., Denmark, and Purdue, all your work and publications came out of RML. Would you tell me about this later work?

Bloom: The 1980s and the 1990s were times when the fields of molecular virology, molecular biology and structural biology were really taking off. I was able to use molecular cloning, DNA, the old style M13 sequencing, cloning and sequencing to do some really cool studies on Aleutian disease. I jokingly say, "I became the world's leader in that field," but it was probably true because I was the only one working in that field. I had a couple of talented postdoctoral fellows who came from Denmark. One was Søren Alexandersen [Dr. Søren Alexandersen], who recently was appointed the deputy director of the Danish State Serum Institute. I've stayed in touch with him. I characterized Aleutian disease as a parvovirus and then studied the pathogenesis of the infection. It was difficult to obtain viruses from infected mink tissue and get them to grow in culture. There was what we would now call a host restriction that prevented the pathogenic viruses from growing in culture. I tried to identify determinants of virulence and *in vitro* replication competence. We finally got to a point where we had done about as much as we could do. It was a challenging system to work with.



Marshall Bloom and Soren Alexandersen, ca 1987.

The interesting thing about the disease itself was really the in vivo system, the correlates of infection, and the persistent infection in the animal model. Unfortunately, as people are learning now with SARS-CoV-2, mink are a challenging animal model to work with. There are a limited number of cellular markers. The animals are not inbred. They only have kits once a year—the babies are called kits. So it's even harder than working with a ferret. Mink were challenging to work with. We ran up against roadblocks in trying to look at the *in vivo* pathogenesis aspects of the disease. Nevertheless, we did what I think is really top-notch work.

We had done some molecular cloning, sequenced the virus, and constructed a full length molecular clone of the virus that grows in culture. Then I read a paper about in situ hybridization. I had a friend named Ashley Haase [Dr. Ashley T. Haase], who by now must be well into his 80s. He is chairman of the Department of Microbiology and Immunology at the University of Minnesota. Ashley was a big enthusiast for the technique of in situ hybridization and convinced me of the power of that method. The parvoviruses have a unique characteristic. The virus particle itself is rather small and only encapsidates a single strand of DNA. Most DNA viruses are double-stranded DNA. But the parvoviruses only encapsidate a single strand - the negative-sense or the anti-sense strand.

Harden: But it's not a strand of RNA, correct?

Bloom: No, it's not RNA, but DNA. There are lot of single-stranded RNA viruses, polioviruses, coronaviruses, influenza viruses. But the parvoviruses are the only single-stranded DNA viruses as far as I know, although there may be a new one I don't know about. The Aleutian disease parvovirus happened to contain the negative strand. When that virus infects a cell, the first thing it has to do is to convert the viral genome into double-stranded DNA. Once it's in a double-stranded conformation, then it can make RNA, direct the synthesis of proteins, and reproduce.

I recognized that we needed to know whether the virus that we could identify by doing immunohistochemistry was actually replicating or being sequestered in antibody complexes. I realized that we could use the technique of in situ hybridization. Since the DNA in the virus was single-stranded, we could use a single-stranded hybridization probe against the viral strand, and that would tell us where the virus was.

And since the replicative form of the virus was double-stranded, we could develop a probe against the other strand that would tell us where the virus was replicating. That's a technique called strand-specific in situ hybridization. By doing that, we were able to discriminate sites of virus replication from sites where virus was just being trapped in immune complexes or sequestered. My lab was the first to do that.

Harden: Wow.

Bloom: The concept got picked up by people working on a number of other virus systems. On the basis of that work, I got invited to participate in the 1990 Wallace P. Rowe Symposium on Animal Virology in Bethesda.

Harden: What else about your work on Aleutian disease of mink would you like to get on the record?

Bloom: Perhaps the most interesting thing was all this gamma globulin, much of which was antiviral antibody, but this massive host response was not neutralizing the virus in the mink. So once we had an *in vitro* model, the question was whether we could identify antibodies that would neutralize virus infectivity for cell culture? The Aleutian disease parvovirus had a strong attraction for cellular

membranes. In fact, in order to purify the virus particles, Dr. Porter at UCLA and I had to treat it with ether or Freon to get rid of the membranes. Then we could purify the virus particle. Dr. Porter found that if the virus were treated with Freon, he could incubate it infected mink serum and could show that the mink serum contained antibody in it that would neutralize the virus in cell culture.

That turned out to be interesting because the virus wasn't neutralized *in vivo*, and you couldn't protect animals by giving them serum from immunized or infected animals. Then the question became, "Where are these antibodies binding to the virus?" This is right around the time that Mavis Agbandje-McKenna [Dr. Mavis Agbandje-McKenna] and Michael Rossmann [Dr. Michael G. Rossmann] at Purdue were developing cryo-electron microscopy images of various parvoviruses. I contacted their group and collaborated with Mavis, a brilliant scientist from Africa who tragically died of ALS [amyotrophic lateral sclerosis] just a couple of years ago, on a cryo-EM study of Aleutian disease parvovirus. We got a low resolution model of that, which we published.

Then we made monoclonal antibodies against the virus and showed that the same monoclonal antibodies that would aggregate the virus into immune complexes would also neutralize the virus. It seemed that there was a single determinant on the virus particle that mediated a lot of the biological effects we saw *in vivo*. I think that this was some of the coolest stuff I worked on.

It was right around that same time—the late 1990s—that we were also working on the question of whether apoptosis, normal cell death, is involved in Aleutian disease infection *in vitro*. I was fortunate enough to attract a very smart Australian student by the name of Sonja Best [Dr. Sonja M. Best] to come work with me on Aleutian disease. She was from the Australian National University and came, I think, in 1997. This is right around when people started looking at apoptosis. Sonja did some amazing work using apoptosis inhibitors and things like that. We found through Sonja's work that replication of Aleutian disease virus actually depended on one of the elements of apoptosis. She identified that enzyme. It was the first demonstration that a DNA virus depended on an enzyme involved in the apoptosis pathway for permissive replication.

Harden: Wow.

Bloom: Sonja has excelled at RML. She became a research fellow, then a staff scientist, then a tenure track investigator, and just recently, she was selected to be Chief of the Laboratory of Persistent Viral Diseases (LPVD). Dr. Chesebro stepped down as laboratory chief within the last year, and Sonja was recruited to succeed him.

There are a lot of other people I haven't had a chance to mention who have been instrumental in my parvovirus work, primarily Kenneth Berns [Dr. Kenneth I. Berns], Neal Young [Dr. Neal S. Young], who's now with NHLBI [National Heart, Lung, and Blood Institute], both good friends and good collaborators.

Laboratory of Persistent Viral Diseases group photo, 1987-1988

Harden: I want to stop here for a moment. In the 2000s, you shift gears in your research to flaviviruses, but I'd like to set that aside for a moment and ask you to discuss the impact on RML of the terrorist attacks of September 11th, 2001. Both the Bethesda campus and the RML campus of NIH were transformed. Fences went up, and new buildings were built, and new responsibilities were given to the agency. But let's start with the day of the attacks. Tell me how it was at RML and about communications in the days afterwards with NIAID Bethesda, and about RML's interaction with the citizens of Hamilton.

Bloom: When the initial attacks occurred, I was at the gym before work. One of the women who worked there said, "Marshall, come here and look at this." We watched on TV the second plane hit that tower. This was stunning, and then I got dressed and came to work. My recollection is that NIH sent us all home. They said, "We're closing things down. Everybody needs to go home."

In 2001, I was still a PI working on parvoviruses. One offbeat thing about that time was that Tom Kindt [Dr. Thomas J. Kindt], who was the Director of NIAID's Division of Intramural Research [DIR] had come to RML for a one or two day visit with a couple of his colleagues. Of course, the White House grounded all planes that day, not knowing if more attacks were planned, so nobody could fly anywhere, and Dr. Kindt and colleagues were stuck here with the limited amount of clothing that they had brought with them because they expected to be here for only a day or so. Finally, he and his party ended up renting a private plane to go back to Maryland because General Aviation was still permitted to fly and they could land in Montgomery County, Maryland.

I was not involved in any of the senior scientific issues then, but it was pretty dramatic. I suspect that initially, there were more changes on the main campus in Bethesda than there were out here because we were located in the middle basically of nowhere and weren't considered a high-value target.

But in the aftermath of September 11, Dr. Fauci, in conversations with President Bush [George W. Bush], convinced the president to set up a big bio-defense program. And one element of that bio-defense program was to build a bio-safety level four [BSL-4] facility out here at RML. We had the real estate to do it, and we had a group of scientists here who could do that work. The Bethesda campus of NIH didn't have the real estate to build a facility like this on the main campus.

Harden: Over the next year or two, you began to serve on all sorts of committees and boards, such as the security operations advisory committee and the crisis management team at RML, the local emergency preparedness committee in Ravalli County, and the state of Montana's antiterrorism advisory committee. Throughout the 1990s, you had already received awards for service to NIAID. And so in 2002, the very next year after the terrorist attacks, you were named Associate Director of NIAID with responsibility for RML. What was the story about no longer having a separate Director of RML at that time? Bob Philip [Dr. Robert N. Philip] had retired in 1982, and I don't think anyone replaced him. Tell me about the change in your administrative responsibilities.

Bloom: It's a fairly long, complicated story that is related to the plans to build a BSL-4 lab at RML, and I was appointed Associate Director before serving on those committees. Initially, I was not involved at all, but I had a very good friend by the name of Karl Johnson [Dr. Karl M. Johnson], who was one of the fathers of BSL-4 research, and who actually discovered the Ebola virus. Karl and I had become good friends through trout conservation, and he had been appointed to a panel that was figuring out what sort of bio-safety level 4 facility to build out here at Rocky Mountain Labs. Karl had been with NIAID back in the sixties, when he was the director of MARU, the Middle America Research Unit, which was a part of NIAID and where he did his work on Machupo virus, among other things. MARU eventually was shut down, and consideration was given to having Karl come in the early seventies and become director of RML. Karl wanted to establish what at that time would have passed for a BSL-4 laboratory here, but the Institute didn't have the money or the interest. So Karl left NIAID and went on to a career at the CDC and then with the Army Fort Detrick and the University of New Mexico.

Karl chaired the peer review panel for the BSL-4. One day, I was sitting in my office, and he comes in and says, "You know, you're a jerk." Those aren't the exact words he used, but I realize the public is going to be reading this oral history. I'd known Karl pretty well by that time, so I responded, "Well, Karl, you've been telling me that for a long time. What's the occasion this time?" He said, "You are going to have the best BSL-4 facility here, and I keep asking people, where's Marshall? How come Marshall's not involved in this process? So how come you're not involved in this process?" And I said, "Nobody asked me." And he said, "When did that ever stop you?" And I said, "Look, I don't know too much about this." He looked at me and said, "You need to start working on one of these level-four pathogens." So I asked him, "What are those?" Because I that's about how much I knew. He said, "Well, there's Ebola virus, but I wouldn't work on that because Heinz Feldmann [Dr. Heinz U. Feldmann] is working on that in Canada, and he's the best in the world on Ebola." He said, "Maybe you ought to work on the tick-borne encephalitis viruses." So I asked him, "What are those?" And he proceeded to tell me. And I said, "That's a really good idea because we already work with those ticks out here because they vector Lyme disease."

Ixodes scapularis, the tick that transmits *Borrelia burgdorferi,* the Lyme disease spirochete, also transmits the tickborne encephalitis viruses which are flaviviruses. So I said, "Let me do some research on that. I'll think about it and see what I can do." I did a little investigation, and when Dr. Kindt, who was still Scientific Director, came out for a visit, I asked for a meeting with him. This was the summer of early 2002. I said, "You know, Dr. Kindt, I got a really good review in my last Board of Scientific Counselors review about my work on parvoviruses. But we're going to have a BSL-4 facility here, and I think I'd like to start working on one of those level 4 viruses." He says basically the same thing I had asked Karl: "What are those?" And so, luckily I was one step ahead of him. I explained to him about flaviviruses and *Ixodes* ticks and he said, "That's very interesting, let me think about it, and we'll see." About two or three weeks later, I get a phone call that says Dr. Kindt wants to talk to you." And I said, "Oh my God, what did I do now? What have I done wrong?" But when I took the call, Dr. Kindt said, "Tony and I have been talking, and we think there needs to be an Associate Director of NIAID for the Division Intramural Research at RML. We want to know if you'd do it?" And I said, "Sure." Dr. Kindt said, "What do you mean—you are not going to argue with me?" I said, "No, I have thought that NIAID needed one here for a long time. If you guys think I can do it, I'm happy to give it a try." That's how I got appointed as NIAID Associate Director for the Division of Intramural Research at RML.

Now, there had been Directors of RML from the time it was created to make Rocky Mountain spotted fever vaccine. Dr. Ralph R. Parker had been the first Director. In 1949, he died suddenly of a heart attack. Dr. Carl Larson took over and served from 1950-60. He was followed by Dr. Cornelius B. Philip, who was Director from 1962-64. Then Herb Stoenner was appointed and served until 1979. During the Reagan administration, there was something called the Grace Commission, which wanted to shut RML down. Dick Krause [Dr. Richard M. Krause] was NIAID director at that time, and Ken Sell [Dr. Kenneth W. Sell] was Scientific Director—that is, the Director of Intramural Research. To counter the Grace Commission's criticism of RML, Drs. Krause and Sell reorganized RML, changing its name from Rocky Mountain Laboratory, singular, to Rocky Mountain Laboratories, plural. There was going to be a virology program, a bacteriological program, a vector program and so forth and so on, which was reflected in the plural name.

At that point, they did away with the position of the directorship. The person in charge here then was what I guess you would call an executive officer, somebody running the administrative aspects of the show but who was not a scientist. In the early 2000's, that role was filled by an amazingly capable woman by the name of Pat Stewart [Pat Stewart], who also taught me a huge amount about managing operations and dealing with administrative chores. And so they appointed me Associate Director of the NIAID Division of Intramural Research, not associate director of RML. There is no separate director of RML.

Harden: Thank you for helping me sort through these changes. In 2004, you published a review of the book In *The Wake of Terror, Medicine and Morality In The Time of Crisis*. That suggested to me that you were also thinking seriously, very soon after 9/11, about what RML might contribute as the entire NIH transformed to include biodefense in response to the terror attacks. So tell me about what you were thinking about and wanted to see happen at RML.

Bloom: I don't want you to credit me with too much of the decision making here, because I was only one of the bit players, and most of this was being done in Bethesda. But when they appointed me as

Associate Director, my main job was to get the BSL-4 facility permitted, constructed, commissioned, and staffed. That was what they wanted me to do. The NIH's initial attempts to permit the facility involved a law called The National Environmental Policy Act. In essence, it says that everything the federal government undertakes has to be evaluated for environmental impacts. There are a number of different types of environmental review. The big one is what's called an Environmental Impact Statement, and that is a big, big, big deal. The NIH initially thought—and this was before I was Associate Director—that they could build the BSL-4 laboratory out here under an Environmental Assessment, which is not as in depth and comprehensive an analysis as an actual Environmental Impact Statement. Had I already been in the discussion about this, I would have pushed for an Environmental Impact Statement at that point, because I knew a lot about the National Environmental Policy Act because of my work in trout conservation and other environmental causes that we can talk about later. Shortly after I became Associate Director, the decision was made to do a full Environmental Impact Statement. I recognized that a project of this magnitude would raise serious concerns in the community and elsewhere. Thus, a totally solid, well justified, well thought out project like a BSL-4 lab needed to have a comprehensive environmental review if it was going to be accepted.

The building was constantly being referred to in the community and the media as the "BSL-4." I felt that this was not a helpful designation since it had an ominous connotation and focused only on a single aspect of the project. So sometime in 2002 I decided we should start using the term Integrated Research Facility or IRF. This was also a more accurate designation because the building integrated BSL2 labs, BSL3 labs, BSL4 labs, animal holding, lab and administrative offices, as well as conference rooms. And the project was thereafter called the IRF. The amusing thing was when NIAID began constructing the BSL4 lab on the Ft Detrick campus, the term IRF was coopted for that project, too. And today the Ft Detrick lab is called IRF and ours simply as RML.

When the Division of Environmental Protection at NIH, which is part of the Office of Research Facilities (ORF), decided to do an Environmental Impact Statement, I completely agreed and applauded them for making that decision. There were a number of hiccups along the way to getting this project permitted and getting the Environmental Impact Statement done. The way the process works is that first, you do some scoping meetings, you put out a draft Environmental Impact Statement, then you put out a full Environmental Impact Statement. And then the highest ranking official has to sign. This would've been the Director of ORF, who has to sign what's called a "record of decision" basically saying that the Environmental Impact Statement or EIS has met all the criteria that have been laid out and has addressed all the questions that have been asked.

The day after it was signed, or shortly after it was signed, NIH got sued by three local groups. Two of them were environmental groups, and I knew the leaders from working on other environmental causes, but this time I was on the other side of the table from them. They filed suit in federal court in Missoula with Judge Molloy [Hon. Donald W. Molloy]. They wanted a temporary restraining order, and they wanted a permanent injunction, so forth and so on. The lawyer representing NIH was David Lankford [David W. Lankford], who is now head of the NIH Office of General Counsel. Judge Molloy denied the temporary restraining order and the permanent injunction and mandated the NIH and the plaintiffs into a 14 hour negotiated settlement with a federal magistrate in Great Falls. On the NIH team were myself, a relative novice in all of this stuff, representing the NIAID and the scientific side; Dr. Deborah Wilson [Deborah E. Wilson], was head of the Division of Occupational Health and Safety; Leonard Taylor [Leonard Taylor, Jr.], head of the Office of Research Facilities, and David Lankford, the NIH lawyer assigned to this. We had a 14 hour marathon negotiating session with the plaintiffs at a courthouse in Great Falls, Montana.

The settlement agreement we arrived at had about 15 or 18 stipulations in it. It was a really productive session. By working through the magistrate with the plaintiffs and the plaintiff's lawyer—who was also a good friend of mine, although we were certainly in different rooms and not talking to each other—we were able to clarify the concerns that the plaintiffs had and address those concerns in the settlement agreement. A couple of the concerns that they raised turned out to be excellent ideas that we hadn't thought of. So we reached the settlement agreement, and it ended up as a better project. And the plaintiffs are still my friends now. We were friends before, and we're friends now. In fact, some of them have become advocates for RML and our programs, even the staunchest critics—one of them is on our bio-safety committee, and a couple of them are on our community liaison group, which I started shortly after I was appointed.

We started construction on the BSL-4 lab building in fall 2004. Pat Stewart and I were the NIAID people involved with overseeing that for the Institute. Eventually, we got to the point where we had to start thinking, okay, what kind of centrifuges and bio safety cabinets and stuff do we want? My technician, Jim Wolfinbarger [James B. Wolfinbarger], and Sonja Best, who was still working with me at that time, basically outfitted the BSL-4 building. There were some other people at RML who were also deeply involved, such as Mike Parnell [Dr. Michael J. Parnell], a veterinarian in our Veterinary Branch. We picked out all of the equipment, all the bio-safety cabinets, all the centrifuges, all the desks, all the computers, you name it. Sonja and Jim did most of that work. One of the best decisions I made was to recruit Dr. Nancy Hoe to be the first full time RML biosafety officer. Nancy had been a staff scientist working on bacterial pathogenesis, but she accepted the position and the challenge. Nancy was truly phenomenal and together we developed the RML BSL-4 biosafety program. Tragically she died from a lingering illness about a year ago, but the staff that she recruited are all exceptional.

Kathy Zoon [Dr. Kathryn C. Zoon] was the Director of DIR at that point. We decided that to create a new Laboratory of Virology and do a search to identify a Chief. There were two candidates, and we selected Dr. Feldmann, Heinz Feldmann, who had been on the peer review panel advising us as the project went along, as the laboratory chief. We assumed what's called “beneficial occupancy” in early 2008 and hired Heinz. Heinz came down from Winnipeg that year and assumed the position of Chief of the Laboratory of Virology. Kathy had made me Acting Laboratory Chief until Heinz arrived. When Heinz became Lab Chief, we got all the Select Agent approvals, other permits, and this and that. We actually started doing level-four work, I think, in September of 2009. In all sincerity and candor, I think this is the best BSL-4 program in the country. I want to make sure it is clear that in spite of the major role I had, I have never personally done research at BSL4.

Marshall Bloom with Anthony Fauci during IRF construction, June 2006

Harden: You also joined with the directors of almost all other BSL-4 facilities in North America to define the processes to select, train, and evaluate scientists and support staff. And because the CDC published it, it would have been a very deliberate process.

We're now two hours in, and I think we need to schedule another session. But before we end today, would you walk me through the transformation of the lab called the Laboratory of Human Bacterial Pathogenesis (LHBP) into its name change, in 2007, to the Laboratory of Virology (LV), of which you were acting chief before the arrival of Heinz Feldmann?

Bloom: Yes, that was a confusing time and the changes are a bit convoluted, but this is not quite correct. LV and LHBP are completely separate research programs, but I did serve as Acting Lab Chiefs for both. Let me try to clarify. When I was appointed Associate Director by Dr. Kindt, I was a Principal Investigator in the Laboratory of Persistent Viral Diseases.

Harden: And that was Bruce Chesebro's Lab, correct?

Bloom: Yes, he was the LPVD Lab Chief at that time. I remained in LPVD until 2007, when Kathy Zoon established the Laboratory of Virology (LV) and made me Acting Chief of LV. I held that position until we hired Dr Feldmann as permanent LV Lab Chief in 2008.

Completely separate from all the BSL-4 and LV matters, Kathy also had me serve as Acting Lab Chief of the Laboratory of Human Bacterial Pathogenesis (LHBP) from 2005 to 2007 until a variety of issues associated with the bacteriology and vector borne microbiology programs could be sorted out. A highlight of that was securing tenure for Frank DeLeo [Dr. Frank R. DeLeo] who is now Chief of the Laboratory of Virology at RML. It was during this time that I got to know Steve Holland who had been one of Frank's Bethesda mentors.

Continuation of Dr. Marshall Bloom's oral history, May 12, 2022

Harden: Dr. Bloom, let's dive into your personal laboratory research. Sometime around the early 2000s, you switched from research on Aleutian mink disease to research on flaviviruses. In 2006, you became Chief of the Tick-borne Flavivirus Pathogenesis Section in the Laboratory of Persistent Viral Diseases. Would you tell me why you chose flaviviruses to work on and what you learned about them? Flaviviruses cause Dengue, West Nile, Zika, yellow fever, hepatitis C, and various other very serious diseases.

Bloom: As we talked about previously, up until the early 2000's, I had worked on Aleutian mink disease parvovirus and had done pretty much all the work that I felt was possible to do on that model at that time. It was a difficult model to work with, particularly in trying to look at the animal correlates and the immunology, because Aleutian mink disease was an immuno-pathological disease where the actual disease manifestations were largely the consequence of the mink's immune response against the virus, and the immunological tools to study that just didn't exist. So that was one factor. The other factor was that, as I said, NIAID was about to build a BSL-4 facility here. After my conversations with Karl Johnson, which I talked about earlier, I decided to start working on tick-borne flaviviruses. The flaviviruses are a

rather large group of viruses, most of which are transmitted by arthropod vectors, either ticks or mosquitoes. The mosquito-borne flaviviruses are pretty well known, dengue, West Nile virus, Japanese encephalitis virus, yellow fever, and, of course, Zika virus. “Flavi” is derived from the Latin adjective “flavus” for yellow- my Latin background paying off. Yellow fever virus is the prototype of that virus family and the whole group is named flaviviruses.

In addition to the mosquito-borne flaviviruses, another lesser known group are vectored by ticks, primarily hard bodied or ixodid ticks. The best known and most important of the tick-borne flaviviruses are the various tick-borne encephalitis viruses in Europe and Eurasia. Those viruses are resident in and transmitted by the very same ticks that host the borrelia that cause Lyme disease, primarily *Ixodes scapularis*, *Ixodes ricinus*, *Ixodes cookei*, and a couple of others. Given the long history of research on tick borne agents at RML, it seemed logical to study these viruses. In fact, before I arrived at RML, Carleton Clifford [Dr. Carleton Clifford], Carl Eklund, and others had done some interesting work on these viruses—an overlooked fact.

Two tick-borne flaviviruses have been identified in the United States and North America up to 2022, which is when we're making this recording. One of them is called Powassan virus, also referred to as lineage 1. And the second lineage of Powassan virus is called deer-tick virus. Those viruses are carried and transmitted by the same *Ixodes* species ticks that host *Borrelia burgdorferi,* the agent of Lyme disease. And as mentioned, there is a long history of tick research here at Rocky Mountain Labs. In fact, you could honestly say that RML was built on a tick. Because of the long history of tick-borne infectious diseases studied here, it was an obvious choice for me to start working on a disease-causing virus transmitted by *Ixodes scapularis.*

In addition, one of the tick-borne flaviviruses is Langat virus, which was isolated in Malaysia many years ago. It does not cause disease in humans and is rated for studying at BSL-2. So because a BSL-4 laboratory was still aspirational at the time, my group could begin working on a tick-borne flavivirus rated for studying at the BSL-2 level. I also discussed this with Dr. Kindt and was given the okay to change virus models when I was still in the Laboratory of Persistent Viral Diseases.

Now the infections that Powassan and the other related viruses cause resemble the other arthropod-borne flavivirus infections in that most people who get them don't even know they've been infected. A fraction of people get an influenza-like illness. But about 10% or less go on to develop a severe neurological disease characterized by encephalitis, meningitis, and other serious neurological effects. Of that group, about 10% die. Roughly half the people who develop the neurological complications of the disease go on to have serious and chronic sequelae that can also cause death. Most recently, in 2019, Senator Kay Hagen from North Carolina died after a three-year bout of Powassan virus encephalitis. People in North Carolina maintain she caught it in Virginia, but nevertheless, she was infected and eventually succumbed.

So the flaviviruses were the viruses towards which I decided to shift my research focus. And Sonja Best, who was still a post-doctoral fellow with me, led the actual work the tick-borne Langat virus and the tick-borne flaviviruses. As we studied the literature that was available at that time, it became apparent that interferon had an unclear role in those infections – sometimes it was an effective therapy and sometimes it wasn't. So Sonja decided to investigate the role of interferons and innate immunity in tick-borne flaviviruses. And that turned out to be a super terrific area of study to this day and in which Sonja is deservedly one of the most prominent investigators. As mentioned, Sonja was recently selected as Chief of the Laboratory of Persistent Viral Diseases at RML, replacing Bruce Chesebro.

We talked about some of this already, but Sonja, my wonderful technician at that time, Jim Wolfinbarger, and I were in the Laboratory of Persistent Viral Diseases. But when the Laboratory of Virology was established I was made Acting Laboratory Chief until Dr. Heinz Feldmann was hired as permanent Chief of the Laboratory of Virology. Then, I became a Section Chief in that laboratory as well as remaining the Associate Director.

A few years after that, Sonja competed for and was selected as a tenure track investigator in the Laboratory of Virology. I was delighted and Sonja was able to continue independently all the superb innate immune studies that she was doing with a post-doctoral fellow named Travis Taylor [Dr. R. Travis Taylor] and the exceptionally talented Shelly Robertson [Dr. Shelly J. Robertson]. In other words, when Sonja established her Section in the Laboratory of Virology, Shelly and Travis went with her. So, I had the opportunity to start looking at aspects of flavivirus infections that weren't related to the innate immune response.

Sonja Best and Marshall Bloom in the Integrated Research Facility atrium, March 2009

Harden: In 2014, your section was renamed the Biology of Vector-Borne Viruses. Why did that happen?

Bloom: When I moved into the Laboratory of Virology, my section was the Tick-borne Flavivirus Pathogenesis Section. Around 2014, I decided that that name was too confining. I knew that I was interested in other vector-borne viruses and might end up studying them at some point. Thus, I decided

to change the name of my section to the Biology of Vector-Borne Viruses. And the interesting thing is that name harkened back to Norman Salzman's lab where I worked in the mid-1970s - the Laboratory of the Biology of Viruses. I always liked that terminology, the "biology of viruses," because when we talk about studying viruses, we can talk about the immunology of virus infections or the ultra-structure and pathology of virus infections, or the molecular biology of virus infections, or the host of the virus, or the arthropod vector of the virus. All these aspects comprise the biology of a virus, so I decided to rename my section as the Biology of Vector-Borne Viruses. Plus, BVBV had a snappy sound to it. That's why I changed the name.

Sonja Best named her section in Lab of Virology the Innate Immunity and Pathogenesis Section, but changes will occur after she formally becomes the new Lab Chief of LPVD, and the name is being changed from LPVD to something else. My long-time technician, Jim Wolfinbarger, retired, and I was fortunate to hire Danielle Offerdahl [Danielle K. Offerdahl], to come work with me as a technician and really a lab manager. BVBV had already moved to the building called the Integrated Research Facility or the IRF, which houses the BSL-4 lab. Danielle is just exceptional. She has a Master's degree, and she's as capable as any member of the scientific staff working in any of the labs here.

One of the things we were interested in exploring was comparing infection in mammalian cells and tick cells using tools of molecular virology as well as confocal and electron microscopy. So, some of the first work we did next were comparative studies of the virus, the biology of the viruses in tick cells and mammalian cells. In particular, we looked at what I call the cytoarchitecture, i.e., what the ultrastructure of the cell looks like when viruses infect the cells, and how the cell responds to virus infection at an ultrastructural level. Danielle did some really beautiful work comparing the cyto-architecture in mammalian cells and in the tick cells. This sounds easy, but it is not. If you work with mammalian cells in culture, they grow well. If you work with mosquito cells in culture, they grow really well. If you work with tick cells, they hardly grow at all. They're a big pain and a challenge to work with. But Danielle and the others in my group have mastered that. They grow very, very slowly and they're very, very finicky.

Without going into detail, what we found is that there were definite differences in the way that the cells respond to infection with the same tick-borne flavivirus. When the mosquito-borne flavivirus Zika virus emerged, Danielle compared Zika in mosquito cells and in mammalian cells. The ultrastructure and immunohistochemistry comparisons that she did turned out to be some of the finest.

I also recruited a very talented postdoc from Zimbabwe by the name of Luwanika Mlera [Dr. Luwanika Mlera], who had gotten his Ph.D. at Northwest University [North-West University /Noordwes-Universiteit] in South Africa.

Before going on, I have to digress here a little bit. After I had been appointed Associate Director, Richard Koup [Dr. Richard A. Koup], Tim Myers [Dr. Timothy G. Meyers], Frank DeLeo, and I were sent by NIAID over to South Africa to learn about the virology that was going on there and to visit the BSL-4 lab in Johannesburg. We were escorted around by Gray Handley [F. Gray Handley], who has just retired as the NIAID Associate Director for Global Research. We visited Johannesburg and we visited Cape Town. I met a number of terrific people at the University of Cape Town and got invited back to a Virology Africa Conference. I've been to four or five of those, they're always in Cape Town, and I always enjoy them. I've hired two postdocs as a result of those connections. I met Luwanika at one of these conferences and hired him. He came over and worked with me for a number of years—a very smart and capable investigator. After his postdoc, Luwanika obtained an excellent position the University of Arizona in Tucson where he was very successful. He recently left that job to pursue other career options.

Another area of virology in which I've always been interested is persistent virus infections. We decided to see if Luwanika could develop a persistent infection model with a tick-borne flavivirus using the BSL-2 Langat virus. Sure enough, he was able to develop a model of persistent Langat virus infection in the mammalian cells. His work produced some remarkable studies. Persistent virus infections are different from acute virus infections like the flu. We get flu and we get over it. The virus doesn't linger in the body of a healthy person. A persistent virus, in contrast, is one like HIV, which is causes chronic infection. Other viruses that establish persistence, like the herpes viruses, cause acute disease, but then they typically will go latent. There had been very little work to find out if tick-borne flaviviruses actually initiated persistent infection, so we decided to take a look at that. But when you consider persistent virus infections, there are three stages that you have to think about at a cellular level. One, the virus has to be able to initiate a persistent infection. The second stage is maintenance of the persistent infection in cells. Finally, in some cases it can break the persistence and terminate the infection. The viral and cellular factors responsible for initiating, maintaining and terminating the persistent infection are not going to be the same. One of the early theories about persistent virus infections was actually developed by my friends, David Baltimore [Dr. David Baltimore] and his wife Alice Huang [Dr. Alice S. Huang], back in the 1970s. They demonstrated defective interfering virus particles, viral genomes in which critical parts of the genome are missing and which cannot on their own replicate. But these defective interfering particles can be propagated in a cell that has a full length, intact virus. The intact virus is able to supply the viral factors that allow the production of the defective interfering genomes. It's more complicated than that, but one of the early theories about persistent virus infections was that they were mediated by defective interfering virus particles. In the mid to late 1970s, this became known as the Baltimore-Huang Hypothesis. And there was a very prominent virologist at Pittsburgh named Julie Youngner [Dr. Julius S. Youngner] who also worked on this for much of his career.

One of the things that Luwanika did in our lab was deep genomic sequencing of the viruses in persistently infected cells, in which you can do sequencing reactions a hundred thousand fold times. This lets you see teeny, tiny populations of molecules, which you might not otherwise see. And what Luwanika found was that at the initiation of persistent flavivirus infections, at least with Langat virus in the cells that we used, there were no defective interfering particles produced. But during the maintenance phase of the persistent virus infections, there were defective interfering viruses present. Not only that, but that almost all of the defective genomes had a specific piece cut out of them, which turned out to be very interesting. And so Luwanika did a number of excellent studies on persistence in mammalian cells, looking not only at the virus correlates, but also at the host cellular response during both the initiation and the maintenance of the persistent virus infection. The details are too complicated to try to go into here but offer a lot of insight.

Harden: Were these findings linked in any way to the new technology that became available to you? I've heard other scientists talk in superlatives about what the new technology made possible.

Bloom: Absolutely. The studies that I've described would not have been possible 25—even 15—years ago. With deep sequencing, you're able to look at every molecule in the tube. With RNA-seq, shorthand for deep sequencing of RNA with those techniques, and the associated computational technology to analyze the vast amounts of data, made Luwanika's findings possible. Not only looking at the virus itself, which is pretty easy, but also the transcriptome of the infected cells, which means the cellular RNA populations and profiles in those infected cells. We were able to study the differences between

uninfected, acutely infected, and persistently infected cells. The technology to do those studies would not have been available certainly in the 1980s, 1990s, or even the early 2000s. They were absolutely key.

Harden: Those technologies were a part of this new Integrated Research Facility that you now have. Was the Integrated Research Facility essential to do the work?

Bloom: Now none of what I've described was done at BSL-4. It was all BSL-2, with some BSL-3. My lab has never actually done work at BSL-4. The Integrated Research Facility, which houses the BSL-4 lab and which I spent so many years trying to get built and staffed, is where my office is. My lab today is in the building, but we don't need to use the actual BSL-4 part of the building.

But you reminded me of one of the things that I was able to help NIAID establish at RML, shortly after they made me Associate Director, and that was a technology branch. Now, NIAID's Division of Intramural Research (DIR) has had a research technology branch for many, many years. But there wasn't a formal part of it here at RML. It was originally called the RML Technology Section but subsequently got subsumed into the main NIAID DIR Research Technology Branch (RTB). Part of that technology branch was a phenomenal electron microscopy facility, which is presently headed by Elizabeth Fischer [Elizabeth Fischer]. The genomic section, which did all the DNA and the RNA analysis that I mentioned, was headed by a fellow by the name of Steve Porcella [Dr. Stephen Porcella], who's no longer with NIAID. I didn't need to reach outside of RML to do the experiments we wanted to do, which was a huge convenience. The people we had at the time, and the people that we have now, in that technology branch are as good as anybody anywhere else in the world. And some of them are probably better.

One of the things that we helped to establish was our high-powered electron microscopy unit. When we were building the Integrated Research Facility, Beth Fischer informed me about a new fancy electron microscope called Titan Krios, which is designed to do super high resolution electron microscopy. As part of the project for the Integrated Research Facility, we were able to purchase a Titan Krios microscope. We recognized the value of having the most sophisticated electron microscopy in close juxtaposition to research being done on things like tularemia, plague, Ebola virus, tick-borne flavivirus, and prions. For us this was a unique opportunity that was not going to be available in other institutions around the country. Thus, we were able to secure a Titan Krios, and the work that has come from utilizing that microscope--mine has been just a tiny part of it—has been astounding.

Harden: Was that the instrument that made some of the first pictures of the SARS-Cov-2 virus?

Bloom: It wasn't that particular Titan Krios, but the superb electron microscopy facility that we were able to set up has made so many of the public domain images that you see in the media and on TV. Beth Fischer and her staff take most of these images here at RML. Pretty much anytime you see a picture of Ebola virus or a coronavirus, or *Staphylococcus aureus,* or the bubonic plague bacteria, *Yersinia pestis,* they were taken here at Rocky Mountain Labs by Beth and our staff. I can't take credit for that, but I certainly take great pride in it.

Harden: Media people love to use these images, because they're high quality and they're public domain, produced by tax dollars, so they are free for anyone to use.

But to get back to your research on flaviviruses, I wonder how your decision to work on them might not have been made if you had been in academia. In academic institutions, people have to write grant proposals to get money. And so reading the trends—what research is being generously funded—might influence a scientist's choice of topics. In contrast, you and other NIAID scientists have the privilege of choosing what you want to work on so long as NIAID administrators approve and your work is evaluated as satisfactory or higher every four years by a Board of Scientific Counselors. I wondered if your work on flaviviruses was something that you understood needed to be done, but might not have been possible, if you were at an academic institution and had to get grant funding for the work.

Bloom: People do a lot of complaining, no matter where they work. Some people would say there are downsides to working in the NIH intramural program. I've been here for 50 years, and I have a lot of trouble seeing many of those downsides. But there are some huge benefits. One is that we are given relatively broad latitude to shift our focus without having to go and apply for new grants. If I already had a grant to work on Aleutian disease parvovirus, for example, but I suddenly wanted to start working on tick-borne flaviviruses, I would've had to come up with a grant proposal for that work, and the grant might not have been funded. But in the intramural program, I was able to say, "Dr. Kindt, Dr. Fauci, I'd like to shift my area of focus from Aleutian disease virus to tick-borne flaviviruses." Once I got the permission, I was able to divert my resources from parvovirus to tick-borne flaviviruses. Similarly, we were working on tick-borne flaviviruses, and then Zika virus came along. We were able to start working on Zika virus without having to submit a new grant proposal, or to respond to a new request for proposals. That's an advantage that people working at universities with funding from grants often don't have. Some of them are able to do that if they have other sources of funding, but in the NIH intramural program, it's exceptionally easy.

To me, this is a major selling point for the NIH intramural program. I always tell postdocs who are coming to work that one of the advantages of working in the intramural program is that you don't have to write grant proposals and don't have to teach courses. But I also say that one of the disadvantages of the NIH intramural programs is that you don't have to write grant proposals, and you don't have to teach classes. It is a double-edged sword because when postdoctoral fellows start looking for positions outside of NIH, having had the experience of writing grant proposals or teaching classes is an asset. It's a definite asset. But not having to do that during your research career as a postdoctoral fellow is also a huge asset because you can devote all of your time to your research work. You don't have to be teaching classes or writing grant proposals. The NIH Office of Intramural Training and Education, headed by Sharon Milgram [Dr. Sharon Milgram], and the NIAID Office of Research, Training, and Development, headed by Katie Soucy [Kathryn Soucy], do a world class job of offering grant-writing courses for the postdoctoral fellows and other trainees. These compensate for the experience that's lost to our trainees of not having to write grant proposals. Teaching classes is a little bit different. That's more of a challenge because NIH is not a degree-granting institution.

Harden: What else do you want to get on the record about your flavivirus research?

Bloom: In recent years, I became interested in what the virus does in the ticks it infects. There are a couple of things involved with that. It turns out that if a tick is infected with one of these tick-borne flaviviruses, and it bites somebody, it can transmit that virus in as short as 15 minutes—really fast. The Lyme disease spirochete takes several days to be transmitted, and a lot of the other tick-borne pathogens also take several days. I became interested in a simple minded question: what does that mean? How does that happen? Then I realized that the biology of the virus in the arthropod host, the tick, was not very well studied. And when you think about it, if you're a tick and you take a blood meal, and that blood meal has some virus in it, the virus goes into the mid-gut of the tick, which is the tick's digestive system. The virus has to do something in there. Then it has to get out of the mid gut. They have an open circulatory system, which means they don't have blood vessels. Everything just sort of swims around. So the virus has to somehow get into the salivary glands. And then it can be secreted in the tick saliva when the tick takes another blood meal. I wondered, "What does that actually look like? How does that actually work?" There had been precious little done about that. So I hired another postdoc from Purdue, by the name of Jeff Grabowski [Dr. Jeffrey M. Grabowski], who's no longer at RML, to start that area of research. I had Jeff set up a complicated culture system using organ cultures. The challenge of investigating a virus infection in the ticks is that you have to put an infected tick on an animal in order to infect the animal. The animal will then be infected. Next you have to put an uninfected tick on that animal and hope that the virus gets into the tick. Then you can study the virus infection in the tick. That sounds easy, but it's not that easy if you want to do a study in a controlled laboratory setting. You have to remember these ticks are the same *Ixodes scapularis* ticks that carry Lyme disease as well as flaviviruses. These are little ticks. They're not as big as the dog ticks. So I said to Jeff, "Let's see if we can dissect those ticks, take out the organs, and put them in culture. Let's see if they're viable in culture and if we can infect them." Jeff focused primarily on the salivary glands. We found out that yes, these cultures are viable for over 200 hours in culture. We can add virus to them. And the virus will reproduce in those salivary gland cultures. You cannot imagine how excited I got when Jeff got those results. And then we did some confocal microscopy looking at the viral proteins and salivary gland structure. And we were able to see the salivary glands in culture. The salivary gland of an arthropod is like a bunch of grapes. There is a stem and branches of the stems and then the grapes. The stems and branches would be hollow ducts. And each of the grapes would be a collection of about 10 or 12 cells forming what's called an *acinus*, which is a Latin word for a little bag. Each *acinus* has about a dozen or so cells in it. Those are the cells which secrete the components of the saliva. The saliva travels down the duct network and gets secreted during the blood meal. What we were able to show, is that the virus infects those salivary glands in culture. It infects some of the *acini*. This was exciting. It's what's called an *ex vivo* model because it's an organ culture. It's not in the intact tick, and it's not individual cells on their own. It's an intermediary step between studying in cell culture and studying in the whole tick. The surprising thing was, when we looked by electron microscopy in these salivary gland cultures that were infected, we could see actual virus particles. What we think that means, if our ideas are correct, is that the virus is sitting in the salivary glands, locked and loaded. As soon as the tick begins to feed, there's virus there already preformed, which can be secreted in the saliva, accounting for the very rapid transmission of the virus.

Harden: Wow.

Bloom: Jeff also did some other cool work. He showed that salivary glands, not only from female ticks, but also from male ticks, can be infected. This suggests that the male tick may also have a role in the transmission of these viruses. Jeff left about a year and a half ago.

In 2021, I recruited another African postdoctoral fellow, from Kenya this time, Missiani Ochwoto [Dr. Missiani J. Ochwoto]. Missiani is now repeating those same experiments with the tick midgut. You remember, as I said, the first barrier that the virus faces is after it gets into the midgut—it's got to get out of the midgut. And Missiani, who is doing that work, has also done some cool work combining standard virology, confocal microscopy, and electron microscopy. He has shown that the virus is able to infect the cells of tick midgut cultures. I think we're slowly piecing together the odyssey of how the tick-borne flavivirus navigates through the Scylla and Charybdis of the tick before it gets back to the salivary glands and gets secreted to initiate an infection.

Harden: Fascinating. You've talked a lot about your postdoctoral fellows, but you have also been interested in mentoring other young scientists. You've been a member of the University of Montana, College of Arts and Sciences advisory board and the U.M. global public health program. You have also taken a strong interest in mentoring Native American students and fostering their interest in STEM careers through programs like BRASS, the Biomedical Research After School Scholars. Would you talk about these initiatives?

Bloom: I was on the University of Montana advisory board for only a couple of years. But I was on the advisory council for the College of Arts and Sciences, which is now called the College of Humanities and Sciences. I served mostly in an advisory role, talking about different types of curricula. Remembering that I was a classics major in college, you may find amusing the shift that has taken place recently from STEM - that is, "Science, Technology, Engineering, and Mathematics" to STEAM, which is "Science, Technology, Engineering, *Arts*, and Math." If you really want to put out decent scientists, you better have a good English department. When kids ask, "What's the most important class I should take in college?" I tell them English composition, and speech and debate. Those skills will prove essential. If you're going into science, you're going to learn the science. That's for sure. But if you know English composition as well as speech and debate, you're going to be able to communicate, orally and in writing, which is absolutely important for scientists. You see people who are extremely capable and are very bright scientists and researchers but who can't string five words together in a sentence, five sentences into a paragraph, and five paragraphs into an essay.

Harden: You're not the only scientist who has said this. And of course, you're singing my song as a historian of science and medicine who writes for a living.

Bloom: But there are not enough of us saying it. That's for sure.

With respect to Native Americans, I've been fortunate to work with teachers at about four or five of our tribal nations here in Montana, including the Confederated Salish-Kootenai Tribes. Salish-Kootenai College (SKC) is a tribal college on the Flathead Reservation in Pablo, Montana. I have also worked with dedicated educators from the Blackfeet Nation and the Rocky Boy's Reservation in other parts of Montana. I have been able to help, in particular, two talented Salish-Kootenai kids, Josh Marceau [Dr. Joshua O. Marceau], and his brother Caleb [Dr. Caleb D. Marceau]. They came to Rocky Mountain Labs on a couple of different programs which were designed for students of underrepresented minorities. Caleb went on to do a postdoctoral fellowship at Stanford and is now working at one of the major

biotech centers in the Bay area. His brother, Josh, who actually did his Ph.D. with Dr. Feldmann here in the Laboratory of Virology, did postdocs at a few places. And he's now at the Fred Hutchinson Cancer Research Center in Seattle, working with an incredible scientist by the name of Julie Overbaugh [Dr. Julie M. Overbaugh]. They both stay in touch with me. I consider it a privilege to have mentored these two young men, who are such role models for other Native American youth, who certainly need role models.

Harden: You've also served on the editorial boards of various professional societies, and you've organized conferences on emergent diseases for the American Society for Microbiology. Would you talk about the importance of professional societies in your career? And how you found the time to do all this extra work?

Bloom: That's a really good question. After the September 11th, 2001, attacks, the term biodefense became prominent, largely through Dr. Fauci's efforts. The American Society of Microbiology (ASM), which is my favorite professional society, decided to start a series of annual conferences called Biodefense and Emerging Diseases. I think I attended all except possibly the very first one. If you look at the history of RML over the last hundred or so years, the work we do here has been on emerging infectious diseases. They weren't called that at the time, but Rocky Mountain spotted fever was an emerging infectious disease, as were Q fever, tularemia, Lyme disease and the prion diseases.

I realized that by participating in the ASM Biodefense or Biothreats Conferences, and getting on the organizing committee, I was able to put a spotlight on some of the investigators and the work that was being done here, at Rocky Mountain Labs, on emerging infectious diseases. I was able to get Katy Bosio [Dr. Catharine M. Bosio], an incredibly smart immunologist, virologist, bacteriologist, and metabolomics scientist, invited to be on the organizing committee for a couple of those conferences, too. She just did an astounding job. I feel it is important to get our scientists recognized in a broader area than they might otherwise.

The last ASM Biothreats Conference we held was in January of 2020. I had been able to invite Dr. Fauci, to come talk. And for the last conference in January 2020, I asked Tony, “Is there any chance you could talk about this new coronavirus that people are talking about, the one causing the outbreak in China?” And he did. I am pretty sure that the title of his talk was "Coronaviruses: More Than Just A Common Cold,” and it was one of the first talks that he gave on what is now COVID-19.

Harden: That statement brings me to my next question. Would you tell me, in some detail, what all RML did to respond to COVID-19 and how it interfaced with NIAID Bethesda?

Bloom: Well, the short answer is yes, I can. But to give you a complete answer, we'd need another couple of hours. My role in the COVID response here at Rocky Mountain Labs has not been as an investigator, although people working for me have done a few studies. My role was organizational, helping to manage the institutional response to the pandemic as it impacted operations at RML, keeping the lights on, keeping the snow plowed, keeping our stockroom supplied with gloves, gowns and so on.

There've been three major human coronavirus pandemics that we know about for sure. One was SARS, the Severe Acute Respiratory Syndrome in about 2003. SARS-CoV-1 was a wicked virus infection and

moved around the world, but it didn't have the capacity to spread widely like COVID-19 does. SARS-CoV-1 was infectious, but it wasn't as contagious as SARS-CoV-2. Dr. Feldmann actually did some work on SARS-CoV-1, but he was in Canada at the time. Then came MERS, the Middle East Respiratory Syndrome, also caused by a coronavirus, which popped up in 2012. RML's Dr. Munster [Dr. Vincent J. Munster] did a lot of field work and lab work on that virus, working with the receptors and things like that. Vincent is a tenured investigator, and both he and RML tenure track investigator Emmie de Wit had their antenna up for emerging pandemic coronaviruses.

As soon as the Chinese published the sequence of the virus that was responsible for the outbreak in Wuhan, they saw that sequence and recognized it was a coronavirus. And they met with Dr. Holland [Dr. Steven M. Holland], who is our Division Director. They informed Steve what they had learned so far and laid out a series of experiments that they were going to do as soon as they got some of the virus. They received the virus early on and the rest of that story is better told by others at this point.

The Laboratory of Virology, which is Dr. Feldmann's lab, by this point today has over 80 publications on COVID-19. LV leveraged its collective expertise and established some of the first and best animal models and demonstrated the efficacy of Remdesivir in several non-human primate models. They did some of the major testing on one of the non-mRNA vaccines, which was developed at Oxford. They also produced some of the best electron microscopy figures that you've seen.

So the research response at RML, with amazing support from DIR in Bethesda was rapid, robust, and critical. We could talk for a long time about all the things that people like Dr. Best, Dr. Bosio, Dr. Hackstadt [Dr. David W. Hackstadt], Dr. Hasenkrug [Dr. Kim J. Hasenkrug], and people in other laboratories have done. Not just the senior scientists but also all the trainees and support staff. Rocky Mountain Labs provided a nimble, effective, and important response to COVID-19. I think it's unmatched anywhere in the United States, particularly given the relatively small size of the program.

The research part was really astounding. However, it's important to recognize that research is based on having a lot of "get ready, get set" behind it. The lights have to stay on, and packages have to be delivered. Media has to be made or purchased; equipment has to be bought. I am confident that the entire response of Rocky Mountain Labs was essential, not just the scientists, but the maintenance people, the admin and purchasing people, the bio safety people, and the Veterinary Branch. Our vet branch had to deal with all these different animals and dozens of animal protocols. All of those people deserve an immense amount of credit because without their successful work and dedication, the research would simply not have been. It would've foundered. It would've not have been possible. I had a role in helping the response at an institutional level, along with a number of other people, including Josh Kellar [Joshua A. Kellar], the other RML Associate Director, the people from the NIH Office of Research Facilities, our bio safety staff, our safety staff and Veterinary Branch. Everyone here should take great pride in the fact that RML was able to do such an outstanding job.

Group photo of the RML Coronavirus volunteers, June 2021

Harden: On the public facing side, you were very much involved. The American public has been politically polarized over COVID-19. Talk to me about your interactions with the public during the pandemic.

Bloom: The former Montana governor, Steve Bullock [Stephen C. Bullock], was proactive in his response to COVID-19 in terms of lockdowns and things like that. They didn't go over very well with a lot of people in Montana, including a lot of the people here in our community, but it was interesting that The Lab here—and that's what it's called "The Lab"—has always been highly regarded by the people of Montana and our federal legislators. They provide total bipartisan support. When the pandemic started, we got a lot of phone calls. We have a capable public affairs specialist by the name of Ken Pekoc [Kenneth Pekoc], and all of the phone calls get routed through him. One day he contacted me and said, "There's a fellow from up the East Fork of the Bitterroot who wants to talk to you about COVID-19." And I said, "Oh gosh, okay." I started talking to him on the phone. He sounded like an older gentleman, probably not older than me, but a mature man. He said, "Are you working on this stuff?" And I said, "Yes, sir. We are." And then he said, "Well, God bless you for doing that." And I thought, "Wow, that wasn't what I was expecting." We also had some kids who came and wrote "Thank you" messages on the sidewalk. We also had a gentleman who gave us a banner, which we have put up in the entryway to one of the buildings here, a lovely photographic banner, thanking us for the work that got done here.

It's been frustrating because of the poor level of uptake of vaccine in our county and in Montana in general, and you didn't see a lot of masks being worn. We live in Ravalli County in western Montana, which by the way, is where the TV show “Yellowstone” is being produced right now. We didn't see a lot of people wearing masks out in the community, even when they were supposed to be wearing masks. We all wore them here at work, but you didn't see a lot of people wearing masks in public. A lot of

people also weren't following the guidance about avoiding bars or restaurants. And the vaccine uptake in our community, in our county was less than 60%. Which is really sort of remarkable when you consider that some of the best research and one of the vaccines was actually developed right here in the community. On the other side of town from RML, you have GlaxoSmithKline, which makes vaccines—not COVID vaccines at the moment—but makes vaccines and other anti-infective agents. It was ironic that this was the situation, but it wasn't unique around the country. Dr. Fauci has certainly been excoriated beyond any level of decency in the press and elsewhere. What that tells you is that we need to do a lot of education and community outreach world-wide!

We have hope that the outreach efforts that we do with our Biomedical Research After School Scholars (BRASS), other high school outreach that we do, the university outreach that we do, the tribal community outreach that we do—that all this will pay off with a population that is more informed. It is unfortunate that COVID-19 became so politicized. That had a caustic, negative impact on the way that vaccines, testing, and public health guidance were all received, not only in Western Montana, but in Idaho and, in fact, all over the parts of the country.

I want to stress a couple of things that we were able to do here at Rocky Mountain Labs. In August 2020, working with Seth Cooley [Seth A. Cooley], our industrial hygienist, we set up an asymptomatic testing clinic here at RML, where we collected nasal swabs and sent them back to Bethesda, to the Clinical Center with Dr. McKeeby's [Dr. Jon W. McKeeby] group, where they would test them and send us the results. We would do the tests on Monday, and we would usually get our test results back on Wednesday. That was a completely volunteer effort of about 1500 hours. Our maintenance staff converted our conference room into a testing center in about three days. We did testing from August 2020 until March 2022. We did a hundred individual clinics, 1500 hours of volunteer labor. We sent more than 6,000 swabs from asymptomatic staff members to Bethesda, and we only picked up seven positives out of that number. Our Occupational Medical Service dealt with people who were symptomatic. We have about 500 people working here, and over the course of the pandemic as of last Friday, we had 115 cases among our employees. None, as far as we can tell were the result of on-campus transmission; they were all community acquired. We set up procedures similar to the rest of the NIH. What do you do if you're sick? Stay home, don't come to work. If you get sick at work, go home, contact Occupational Medical Services, so on and so forth.

And then, in January 2021, again using staff volunteer labor led by Lt Commander Megan Brose [Lt. CDR Megan Brose], we set up a COVID vaccination clinic. We delivered a total of 779 doses of vaccine, 325 first doses, 329 second doses, one third dose and 125 boosters. We got 79% of the Rocky Mountain Labs staff vaccinated. Among the scientific staff, that number is about as close to a hundred percent as you can get.

The RML COVID-19 testing and vaccination center, January 2020

In January of 2020, our Emergency Management team led by Roger Laferriere [Roger R. Laferriere] set up an incident management team specifically for COVID-19 impacts. It was the first at the entire NIH because we got worried very early on, "Are we going to have enough N95 masks for the researchers? Are we going to have enough gloves? Are we going to have enough hypodermic needles and syringes and pipet tips?" That COVID 19 incident management team still meets today to deal with some of the ongoing impacts.

All of this non-scientific effort is what made the rapid, robust and critical research response possible- it took all of RML. As they used to say at NASA, if you asked the janitor, "What are you doing?' The janitor would say, "I'm helping to put a man on the moon." I like to think that this is the way Rocky Mountain Labs operates. If I ask a janitor or one of the maintenance staff, "What are you doing?", I hope the person will answer, "I'm helping to fight infectious diseases." That is kind of environment at Rocky Mountain Labs that I have tried to foster.

Harden: Effective leadership always starts at the top, and I think you have demonstrated that. Let's turn to some non-scientific questions beginning with your family activities. Several articles about you have referred to hiking, camping, cross-country, skiing, and fishing with your family. Tell me about living, working, and raising children in the Bitterroot valley.

Bloom: My wife is Tonia, whom, unfortunately, you won't be interviewing, but who will read this transcript with diligence and a very critical eye, I can predict. She is an exceptionally smart individual and capable beyond any imagination, extremely invested in the community. She was on the local school board for over 30 years, served several terms as chairman, currently serves on the local land trust board and on a number of other civic groups, including the League of Women Voters, another one of my favorite groups in the world. She reads the *New York Review of Books* from cover to cover every two weeks when it comes out and the *New Yorker* from cover to cover every week when it comes out. She's a phenomenal person and takes a great interest in what goes on here at Rocky Mountain Labs, although she doesn't have a scientific background.

We have two boys, the elder, Jesse [Dr. Jesse David Bloom], born in 1978, and the younger, Seth [Dr. Seth Michael Bloom], born in 1981. Over the course of the years, several of our closest family friends were prominent scientists. Stanley Falkow [Dr. Stanley Falkow], and his wife, Lucy Tompkins [Dr. Lucy S. Tompkins], from Stanford were almost like a second pair of grandparents to our kids. In fact, we referred to Stanley as "Zayde," which is the Jewish word for grandfather. He was a major scientific figure, a great advisor to me, and a major figure in the life of my kids. Lucy and Stanley had a significant role in steering Jesse and Seth into biomedical science and all of us were good fishing buddies. David Baltimore, Alice Huang, and Irv Weissman [Dr. Irving L. Weissman] are also as close as family, friends and good fishing buddies. They also were role models for me, advisors to me in a lot of different ways, and very fond of our kids.

When Jesse graduated from high school here, he went to the University of Chicago and got captivated by biomedical research. He worked with Susan Lindquist [Dr. Susan L. Lindquist] there, then went to graduate school at Caltech where he did his thesis with Frances Arnold [Dr. Frances H. Arnold], who won the Nobel prize a couple of years ago for her work on directed evolution. Jesse then stayed at Caltech to do a postdoctoral fellowship on influenza in David Baltimore's lab. From there, Jesse was recruited to the Fred Hutchinson Cancer Research Center in Seattle, where he's been for the last 10 or so years. He's an evolutionary biologist. He's interested in the evolution of influenza and has become a prominent expert in the area of coronavirus evolution. So he ended up "inside," as a scientist, though largely through no fault of mine. I always thought he was going to be an English major and a writer.

Seth must have hung around Stanley Falkow too much because he decided to go into microbiology. Stanley was one of the funniest people in the world, and my son Seth has a terrific sense of humor. They got along really well. Seth went to Wash U in St. Louis. He did an M.D.-Ph.D. program there, in internal medicine, and then he decided to do infectious disease research at Harvard. He's now in the Ragon Institute, at MIT, Harvard, and Mass General. Seth is studying the vaginal microbiome and doing well, just had a paper come out in *Nature*. They're both going to be prominent scientists, which is one of the reasons that I'm so keen to keep working as long as I can, because we all sort of still speak the same lingo.

Harden: Stepping outside of the family, would you tell me a bit about your musical endeavors. I understand that you're a virtuoso on the five string banjo.

Bloom: Two words you seldom hear in the same sentence are “virtuoso” and “five string banjo,” but I do play the banjo. In fact, I have two of them here in my office, one at home, one in Seattle, one in Boston and another one that's being made for me in Florida. I started playing the banjo when I was in high school, and I started playing relatively seriously again, about six or seven years ago. I have a friend named Banjo Jack Mauer, who's also a fishing guide. He's a banjo virtuoso. He and I try to get together about once a week and play. He plays the bluegrass style and I play what's called the old-time clawhammer style. And we're, we're pretty good. I'm not as good as he is, but the two of us are really good together. We donate in-house concerts for people to buy at different fundraisers. Somebody might buy a concert for $500. And then I always say, "For an extra a hundred bucks, we won't play the banjos," but nobody's taken us up on that.

In 2018, Dave Baltimore turned 80 years old. Alice organized an 80th birthday symposium and big party for David at Caltech and invited Jack and me to come down and provide the musical entertainment at the Athenaeum, which is the Caltech faculty club, at the big gala dinner as part of the symposium. We have another good friend, a neurobiologist named David Anderson [Dr. David J. Anderson] at Caltech, probably the foremost neuroscientist around. David Anderson plays the guitar really well, and when he comes up during the summer to visit the Baltimores, we get together and we play together. Jack, David and I performed for the Baltimore symposium.

My son Jesse was supposed to be one of the speakers at the symposium, but he and his wife were having a baby at exactly the same time, so he wasn't able to make it. But Jack and David and I played at Dave Baltimore's 80th birthday party in front of an incredibly prestigious audience. There were at least two former NIH directors there, Harold Varmus [Dr. Harold E. Varmus] and Elias Zerhouni [Dr. Elias Zerhouni]. A number of Nobel prize winners were there, including of course, David Baltimore himself. And I imagine that a substantial fraction of the audience were members of the National Academy of Sciences.

We're sitting up there on the stage at the Athenaeum, which has this wood, dark wood paneling—that’s where we're going to be playing. At the table in front of us was Eli Broad [Eli Broad], who has endowed a professorship for David Baltimore. Eli and his wife were sitting at the table with Dave and Alice and Irv Weissman and his wife and a few other people. Before we started, I went down to him and said, "Mr. Broad, I have to apologize to you, sir." And he said, "What for?" I said, "You're going to be sitting about 10 feet away from two banjos." And he kind of looked over and he said, "Well, I've always kind of liked the banjo." So we played there, and that was quite an experience. We had a really good time. Afterwards, all these famous scientists and Caltech faculty came up and said, "You know, I play the banjo too." That'll be the apex of my career as a banjo player I imagine.

Harden: Fishing, especially for trout has been—and I hate to use a cliche, but in this case, I think the word “passion” of yours is appropriate. You have become known as “Dr. Trout” because of your tireless work for trout conservation, especially in the Bitterroot Chapter of Trout Unlimited. Tell me why you've focused on trout and the many things you spearheaded to protect them.

Bloom: As I said earlier, Bruce Chesebro came to RML at the same time I did in 1972. He was an outdoors person and had done fly fishing, duck hunting, and bird hunting. He got me started in fly fishing, and I was captivated by it. I tell people that fly fishing is an auto-didactic tutorial in ecology because in order to catch trout, you have to know what the trout are eating, and they're usually eating

aquatic insects. So you have to learn some entomology, and then you have to learn some stream ecology to figure out where this type of insect lives, and that type of insect lives. And then you come to appreciate the critical role of water quality and habitat preservation and the environmental protection. I made my way through all of those different fields. I think you would find that a lot of scientists are avid fly fishermen—Harold Varmus, Dave Baltimore, Irv Weissman, Stanley Falkow, Mike Oldstone, Lynn Enquist [Dr. Lynn W. Enquist] are the ones who immediately come to mind.

Fishing was something that we did as a family when my sons were growing up. My wife is very capable, but she's not as entranced with fishing as we were. My son Jesse, who lives in Seattle, still does a fair amount of fly fishing.

The fish that are most commonly fly fished for are trout, which we have in Montana. There is no stocking of any of the streams of any of the rivers. All of the trout in Montana are wild trout. Wild trout means the fish reproduce on their own because the water quality and the stream condition are so good. So we have wild trout, but in addition, a management philosophy for wild trout is only practiced in the state of Montana. There is nowhere else in the United States where that is the overriding philosophy of trout management. Also in Montana, we have what are called native trout species. The primary ones that we have are called cutthroat trout, because they have a red slash under their gills. And there are two kinds of those. Another type of native trout is called a bull trout. It is an endangered species now. The Bitterroot River is a strong native trout fishery.

I first became interested in trout fishing and then in trout conservation. I got to know the people in the Montana Department of Fish, Wildlife and Parks through projects that we did in the 1980s. And then in December 1994, it had been noticed that the rainbow trout populations—and remember that the rainbow are a wild trout, but not a native trout—on several Montana rivers had started to decline dramatically. A talented veterinary pathologist, Beth MacConnell [Dr. Elizabeth MacConnell], identified what was killing the fish. It was an infectious parasitic disease called “whirling disease.” It had been known in Europe as a disease of trout, but it didn't have much impact on the fish because the fish had evolved some sort of a means to cope with it. It was introduced inadvertently into the United States and had spread around the country but had not been detected previously in the state of Montana. It's mostly a disease of aquaculture or commercial trout hatcheries and hatcheries are not a prominent feature of Montana’s wild trout fisheries.

Harden: Is it an infectious disease?

Bloom: Yes, it is. It's a parasitic infection. I didn't know anything about it until it was identified here in Montana in December of 1994 by Beth. Shortly thereafter, I was contacted by a friend, Pat Graham [Patrick J. Graham], who was the director of the Montana Department of Fish, Wildlife and Parks. He knew I studied infectious diseases. Pat had talked to Marc Racicot [Marc Racicot], who was the governor of Montana at the time. They asked me to put together and chair a task force of scientists, citizens, anglers, business people, and conservationists from around the state to try to learn more about whirling disease, what the impact was going to be in Montana, and what we should do about it. It's not very often that you get to use your job in your hobby. But as a person with some knowledge about infectious diseases, in 1995, Pat and I, along with others, put together a Montana whirling disease task force. I was able to recruit Stanley Falkow, Lucy Tompkins, as well as Dave Baltimore and Irv Weissman--all eminent scientists. Karl Johnson, the guy who discovered Ebola virus and who was a hardcore fly

fisherman, was also on the task force. He's about 93 or 94 now, and that was actually where I really first got to know him well. We were asked to take a look at the situation and come up with recommendations about what to do. We also started the Whirling Disease Foundation to raise private money to underwrite the research studies that the Department of Fish, Wildlife and Parks was not able to do.

Irving Weissman, Stanley Falkow, and Marshall Bloom fishing.

We decided early on that there would be a three-fold approach: research, management, and education, and they were the three goals we published in both an interim report and our final report. I got an award from the governor as well an award from the U.S. Fish and Wildlife Service for this effort. I feel it was really important that I and the other scientists were part this effort. The biologists at the Montana Fish, Wildlife and Parks Department, trout conservationists and others worried that Montana was going to throw up its hands and say, "We're going to solve this problem by stocking fish out of hatcheries again." That would've been the end of the wild trout and the native trout in the state of Montana. But through our Whirling Disease Task Force, the tremendous support of Governor Racicot and other people in his administration, the advocacy of Trout Unlimited, and the network that we were able to build on this Whirling Disease Task Force, we were able to fight off the interests that wanted to solve the problem by stocking, which is what they've done in all the other states.

In the intervening 26 or so years, the fish populations have largely recovered. The numbers went way down, but now, gradually, the numbers have started to come back up. There are plenty of other problems out there, some of which are infectious diseases, but whirling disease is no longer a major

problem in the waterways around the state. I am proud that we were able to protect one of Montana's finest natural resources.

Harden: I was also impressed with the fact that you could bring together people from diverse backgrounds: Dr. Weissman, as a scientist, the artist, Monte Dolack [Monte A. Dolack] and the fly rod builder, Tom Morgan [Thomas Morgan], to become Stewardship Directors of Trout Unlimited. Tell me about that.

Bloom: This ante-dated the whirling disease problem that I just told you about, which started in 1995. I had become president of the local Trout Unlimited chapter here in 1980. One of the guys on the board started calling me Dr. Trout, which was funny. But then when vanity license plates became available, I got the license plate, "Dr. Trout." It's been my license plate ever since. Tony Fauci has one of those license plates in his office, because I gave him an old one. Dave Baltimore's got one in his office, and Stanley Falkow had one in his office. I don't have any anonymity when I park at the grocery store. They know who I am, because that license plate is obvious.

I was president of the local chapter for about eight years. Then around 1990, they asked me to become president of the State Council of Trout Unlimited. I decided to help build awareness and prominence for the group. I established something originally called an advisory board, but it's now called the Stewardship Council. And it had the individuals you mentioned on it, Stanley Falkow was on it, Irv Weissman was on it, Bud Lilly [Whalen Francis "Bud" Lilly II], who owned the "Trout Shop", was on it. He was one of the early fishing guides and trout conservationists in Montana. A number of other people that you probably haven't heard of were also on it. One was a personal hero of mine named George Grant [George Grant], after whom a chapter of Trout Unlimited was named. There was also John Norman Maclean [John Norman Maclean], the son of Norman Maclean [Norman F. Maclean], who wrote the book *A River Runs Through It*. John and I became acquainted when they made the movie *A River Runs Through It* in Montana in 1991. I was one of the technical advisors on that movie. If you look through the credits at the very end, you'll see my name as a Trout Unlimited advisor. John and I became good friends, and I put John on that Stewardship Council. He lives in Northwest D.C., and I try to get together with John every time I come to Bethesda.

We were able to compile a group of individuals like that who were able to help us bring prominence to the organization and also help underwrite some of the projects the group wanted to do. Once I became the RML Associate Director, I couldn't be an officer of any of these organizations anymore. I'm still an active member and help them out, but I'm not an officer anymore. So, as you can see, I like to bring people from diverse backgrounds together in what is—to use the term now in vogue—an "incubator." If you get smart people together from different backgrounds, you're going to see some really good ideas come up. Now my wife might scoff at my saying this, but I try to do that both at work and in endeavors such as Trout Unlimited and music.

Harden: You have received a large number of awards, but in December 2020 you were named to the Montana Bioscience Alliance Hall of Fame, which was apparently an extra-special award for you. If I understand it correctly, this organization comprises people in government, the private sector and academia who seek to "commercialize, grow, and sustain globally competitive bioscience companies."

You were joining a number of other prestigious scientists who had received this honor. Would you tell me more about the organization and what the award meant to you?

Bloom: Well, you're right about the number of incredibly distinguished scientists who have been named to the Montana Bioscience Hall of Fame. Irv Weissman is a member. Maurice Hilleman [Dr. Maurice R. Hilleman] is also a member. He developed most of the childhood vaccines—measles mumps, German measles and was a Montana native. Lee Hood [Dr. Leroy E. Hood], another friend of mine who has a ranch in the Bitterroot valley, and Edgar Ribi [Dr. Edgar E. Ribi], an RML scientist who died in a plane crash in 1986. When Sharon Peterson [Sharon Peterson], the executive director of that group, called me and said that I was being inducted into the Montana Biosciences Hall of Fame, I was flabbergasted, and I've seldom been that flabbergasted in my entire life. The Montana Biosciences Alliance is a group that recognizes distinguished Montanans who are felt to have been strong advocates for the biosciences. I was very surprised that they chose me, but I certainly was delighted.

Harden: These are all the questions I have. Is there anything else you'd like to get on the record before we stop?

Bloom: I have already mentioned Pat Stewart, who was in essence the Executive Officer of Rocky Mountain Labs when they appointed me to be Associate Director. She and I were like a team, and a very good team. Her successor is Josh Kellar [Joshua A. Kellar], who's the RML Associate Director of Operations Management. Josh is an exceptional guy and a very close colleague, with a genius for organization and details. My title is now the RML Associate Director for Scientific Management.

You will recall that when I was appointed Associate Director back in 2002, I said that my main task getting the BSL-4 laboratory building—the Integrated Research Facility (IRF)—approved, permitted, constructed, staffed, and operating. I have a pretty strong experience, if not formal training, in community outreach and community relations and coalition building. So within six weeks of being appointed, I established a community liaison group for Rocky Mountain Labs. It had about two dozen members of the community on it—from the medical community, the education community, local government, nonprofit organizations, community organizations, environmental groups, and just a few independent citizens. It was already clear that two groups were going to be strong critics of Rocky Mountain Labs. One of them was the Friends of the Bitterroot, and another was Women's Voices of the Earth. I immediately decided that one of the first groups I wanted on the new RML community liaison group was the Friends of the Bitterroot, so that the community got to hear what they were saying and they got to hear what the community was saying. The community liaison group continues to meet today. We met monthly through the period of the IRF being constructed. And now we meet twice a year, and it's been a good group.

The other thing I did was establish a community public lecture series, and twice a year, except during the pandemic, of course, we have brought in prominent speakers from all over the world to talk about infectious diseases and related scientific topics. And we've had Tony Fauci do that, Stanley Falkow, Irv Weissman, Dave Baltimore, Karl Johnson, a few people from Europe, Hank Greely [Henry T. Greely], an attorney and law professor from Stanford, Maryland Pao from NIH, Martin Blaser [Dr. Martin J. Blaser] from NYU, talking about anti-microbial resistance. Tony's actually done it a couple times. We hold the lecture at Hamilton High School, and we've had as many as 700 people attend these lectures. That's

pretty amazing, 700 people in a town of 5,000. Calculate how many people that would mean in a town the size of Bethesda. There wouldn't be an auditorium big enough.

I also established a seminar series called the Distinguished Scientist Seminar series. In a lot of a seminar series, it's usually the principal investigators and the department heads who get to decide who's going to come and give the big lectures at your institution. I didn't think that was fair because I knew that there were speakers the postdocs would like to bring in, and some of the technical staff and others had people they would like to bring in as speakers, but their voices weren't going to be heard. So I established the Distinguished Scientist series and pay for it out of my own Associate Director budget. Postdoctoral fellows, students, technicians and principal investigators may all nominate speakers. The seminar committee selects the speakers, works out the schedule, and sends me the bill. This has been a very successful series and empowers a lot of staff to pick speakers

I also established with our trainees—the post-doctoral fellows and the post-baccalaureates, etc.---an RML Fellows Organization (RFO). They select coordinators, and I provide them with about $10,000 a year to bring in career panels and seminar speakers and events like that. This is a good group, and they're very thoughtful. They just recently compiled a manual for the postdocs coming to RML. There's a manual for postdocs who go to the main Bethesda campus of NIH, but things are a little bit different out here. The RFO is something else that I take satisfaction in.

I think that's about it. Vicky, you've worn me out. Finally, I tried to give credit to everyone that made significant contributions to the things we talked about. Doubtless, I left out a lot of people, so I want to apologize for that in advance.

Harden: Thank you so much, Dr. Bloom, for a wonderful oral history.