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Stuart Shapiro by David Zierler

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**DAVID ZIERLER:** OK, this is David Zierler, Oral Historian for the American Institute of Physics. It is April 29, 2021. It's my great pleasure to be here with Professor Stuart Louis Shapiro. Stu, it's great to see you. Thank you for joining me.

**STUART SHAPIRO:** Thank you for inviting me.

**ZIERLER:** To start, would you please tell me your title and institutional affiliation?

**SHAPIRO:** Yes, I'm Professor of Physics and Astronomy at the University of Illinois, Urbana-Champaign.

**ZIERLER:** Now, this is an endlessly interesting topic for me, and that is the relationship between physics and astronomy at various institutions. They mean very different things, they're in very different proximity at various institutions. How do these things work at Illinois?

**SHAPIRO:** Right, very good question. Because indeed, as an astrophysicist, I've always felt that the underpinnings of astronomy and astrophysics have their roots in fundamental physics. And at Illinois, it's a rather interesting division. We have an astrophysics group in the department of physics. And in some sense, my primary home is the department of physics. There's also an astronomy department, in which I'm a member as well. And there's also the National Center for Supercomputing Applications, NCSA, which has a small astrophysics contingent. And I've been associated with that institution over the years. So there are really several sites for doing astronomy and astrophysics, and that has worked very well. Of course, many of us have joint appointments, like I do, and multiple access to resources. There are seminars in astronomy and astrophysics in all of these departments. And students that I've worked with and trained have come from each one of them.

So that's how it works. Different departments, but one united thrust, although there's a somewhat different emphasis. We are largely theoretical in the physics department. Historically, we have been almost all theoretical, but now, some of our cosmology faculty have an observational program. And astronomy, too, is split, but many more observers are in astronomy, and there are areas in astronomy, such as radio astronomy, that are not in the physics department, but there is certainly overlap in interest.

**ZIERLER:** A question related to nomenclature that would ask you to draw on your perspective beyond the University of Illinois, and that is the terms astrophysics, astronomy, and cosmology. How have they changed over the course of your career? Where is the interlap, where are the boundaries, and where has theory led experiment, and where has experiment led theory in the course of your career?

**SHAPIRO:** Well, I've always called myself an astrophysicist. That, again, is because the underpinnings of astronomy and any kind of study of celestial phenomena, I think, are based on fundamental physics. I would say astronomy as a word often connotes more of an observational, and perhaps even qualitative, survey of the celestial sphere. But I don't think there's a rigorous partition, except maybe in my own usage, where I'm very clear that I'm an astrophysicist. Cosmology is a sub-branch of astrophysics. There are several branches that have clearly overlapped. For example, there's general relativity theory, which has been my recent focus. But of course, general relativity theory is the underpinning of modern cosmology. The expansion of the universe, the Friedmann model, Big Bang nucleosynthesis, and the cosmic microwave background must all be studied in terms of general relativity.

But I view cosmology not as distinct from astronomy and astrophysics, but a subarea. Stellar structure is another subarea, and so are the physics of the instellar medium and stellar dynamics. Cosmology is just one subarea, an important one at that, which as you know has mushroomed in recent years. It was once a subarea that was largely theoretical, following Hubble’s discovery of receding galaxies and the early Big Bang models. But with the discovery of the cosmic microwave background (CMB), the launching of space-based instruments and the construction of larger telescopes, it is now a fully observational field as well as a theoretical one. One of the largest. And it's also, I think, a field that has benefited from the increased difficulty of doing experimental high energy particle physics, as we seek to probe matter at higher and higher energies. It's hard to build accelerators that reach the highest energies relevant for many theoretical models, and numerous particle physicists have wandered into cosmology because the early universe provides one of the best laboratories for analyzing high energy physics. In fact, it's the only laboratory that can reach the highest energies where particle interactions of a certain type are realized.

**ZIERLER:** Another nomenclature question, a much more specific one. Your emphasis of fundamental physics, I wonder if you might help clarify some misperceptions that the field of fundamental physics has a hierarchy to it. In other words, the term fundamental might incorrectly suggest that there's something about this that's more important than other branches of physics.

**SHAPIRO:** Well, no, I don't regard the concept of “fundamental” as being equivalent to “more important”. I do regard it as being more general – a foundation upon which applications are built. I regard general relativity (GR), for example, as a fundamental theory. The work that I may do on, say, colliding black holes or neutron stars, by solving the equations of GR is essentially applying this fundamental theory. I think that high energy physics, which looks at the basic constituents of matter and constructs the basic equations that describe it, is a fundamental area. Anything we then do that applies particle physics and nuclear physics to study cosmology or neutron stars, or even to do radiology, are all applications built on this foundation. And again, the applications are not less important, but there is a root underpinning to them. Perhaps it's the underlying field equations -- the basic equations that underlie all of the applications and problem-solving that follow – that makes one area more fundamental than another.

**ZIERLER:** A more topical and recent question. As a theorist, it might be assumed that in the pandemic, it might be a particularly productive time for you. You might have more bandwidth to work on theoretical problems that you might not otherwise have. You don't have the challenge of experimentalists, who have issues with access to their experiments and their laboratories. But I wonder, on the other hand, has the lack of in-person, interpersonal connections been problematic? Has it been difficult not being able to see your colleagues, work at the whiteboard, bump into people at the hallways of conferences? How have the past 14 months or so been in these regards?

**SHAPIRO:** Well, the answers to your questions are basically yes and yes. Has it been a very productive period? The answer is yes. A lot of the administrative tasks that are associated with coming to an office 9:00 to 5:00 and having one's door open for all visitors have been put aside. And there's a certain quietude of working at home with pencil, paper, and laptop as your main companions for hours on end. Those are often the times we seek in our offices, and now we have unlimited access to that. I managed to work on a number of research projects during this period and even co-authored a book that just appeared this month, so this time has been very fruitful. The technology of Zoom has proven quite amazing. In fact, it's enabled more frequent interactions among colleagues from far off places than one might otherwise have imagined during normal times. For example, when we host colloquia and seminars during normal times we have outside speakers who travel to the university, and we can't really have many that come from the far reaches of the planet. It's too costly for us and too time consuming for them. But with Zoom, we've had speakers from all over the world. It was relatively easy and inexpensive for them to give a talk from the comfort of their own offices. So it has had that advantage. However, on the downside, there's nothing like having a colleague walk into your office with an interesting query, something puzzling him or her, taking you to the blackboard, and the two of you going off on an idea.

**ZIERLER:** And this is spontaneous, as you're emphasizing.

**SHAPIRO:** Yes. That has certainly been lacking. Also, there are the wonderful breaks during the day that come from, again, a student or colleague wandering into your office with an anecdote. Did I hear this bit of news? Or, have I seen this paper? Those are times you put down your pencil, you push your laptop away, and are all ears. Such moments are both illuminating and relaxing. I miss that. That's been absent.

**ZIERLER:** Because on Zoom, everything is formalized. Everything has to be segmented into predetermined periods of time.

**SHAPIRO:** Exactly. And spontaneity in general is sort of lost with Zoom, although it's a great, amazing tool. What a coincidence that the pandemic occurs in an era when Zoom is available. If this happened 20 years ago, it would've been a greater disaster for science, as well as for personal interactions in general. In my own case, speaking of personal interactions, I have another grandchild who was born last July, and we're only going to meet her for the first time in the next few weeks, almost a year later.

**ZIERLER:** Oh, wow, congratulations.

**SHAPIRO:** Thank you. Because of the travel restrictions and health concerns, we couldn't meet her personally, although we've certainly interacted over Zoom and Facetime. So there's good and bad, and we make the best of it.

**ZIERLER:** Let's take it all the way back to the beginning. Let's start, first, with your parents. Tell me about them and where they're from.

**SHAPIRO:** I'm a New Englander. I was born and raised in Connecticut, and so were my parents. My dad was born and raised in Bridgeport, Connecticut. His father died at a very young age. My dad, his brother, and his mother had a difficult but manageable life, marked by the Great Depression. My dad always wanted to be an engineer, but because of financial constraints, he opted to train for teaching. It was easier to get into that field and it required fewer extra years of college. He went to Central Connecticut State University, which originally was a teacher’s college (he later got his Masters Degree at Columbia University Teachers College), and became a junior high school science teacher. And if you were to ask me the logical question, how did I get into science, my dad is the answer.

**ZIERLER:** And he probably had abilities that he was never able to tap into because of his limitations in society.

**SHAPIRO:** Exactly. And so, he got into junior high teaching, and then throughout most of my life, I knew him as a high school principal. And that's where he spent the majority of his career, in Bridgeport as a high school principal. He would commute from our home in the New Haven area, where my mother was born. My mother skipped several grades prior to college and graduated from college at the age of 19. She went to Southern Connecticut State University. She, too, became a teacher and also trained as a social worker. My mom later became head of the New Haven District Child Welfare division. And that was her background.

**ZIERLER:** What were your parents' politics? Nixon versus Kennedy, would those kinds of things be discussed at the table when you were a boy?

**SHAPIRO:** Nixon vs. Kennedy – no, a third, progressive party instead!

**ZIERLER:** Wow. What motivated your parents? What were their interests?

**SHAPIRO:** Well, they came from backgrounds that were lower middle and working class. My grandparents on my mother’s side owned a little grocery store in New Haven, and it was always a struggle, although they managed to earn a living with this little store. Both my parents had to work as teenagers. But they became involved in political activities, such as union organizing, the peace movement, and other progressive campaigns in the 30’s, in response to not only the Depression, but to the upsurge of the labor and other left-wing movements. They were strong supporters of Roosevelt and the New Deal, and campaigned to push that program further to the left. They were anti-war, although when fascism was raging, my dad was drafted into the Air Force as an officer. He was stationed in England for most of the war. In fact, there are some interesting anecdotes.

My dad was in charge of helping load the bombs onto airplanes that took off from England and flew over Gemany. And he would also be in charge of caring for the pilots that came back. One of his commanding officers was Jimmy Stewart, the actor. And on one occasion, when Jimmy Stewart and the pilots returned from a mission, my dad had figured that this rather unappealing powdered ice cream that the Air Force packaged could be whipped up into tasty thick milkshakes. So he arranged for milkshakes to greet the pilots on their return, for which Jimmy Stewart wrote him a letter of commendation, expressing great appreciation for the great boost of morale that my dad provided his crew.

**ZIERLER:** What a claim to fame.

**SHAPIRO:** [laugh] So I've always been keen to watch Jimmy Stewart movies. I feel he's sort of part of the family [laugh].

**ZIERLER:** When did you start to get interested in science?

**SHAPIRO:** I think at a very young age – through my dad, and through some books. A lot of the books he had on science were how-to books containing little experiments and projects. I built a lot of electronic gadgets, a little telegraph, a telephone, a number of small crystal radio sets, and some electronic board games. And then, in elementary school, I formed a little science club, and we would meet during the summer. Some of it was serious, where we'd talk about some serious things we might read in Popular Astronomy or Scientific American. Others were less serious, UFOs and things of that nature. One of my favorite book series then was “Tom Corbett: Space Cadet”, which I devoured in its entirety.

**ZIERLER:** Did you follow the space race from the early 1960s?

**SHAPIRO:** Of course. That was already when I was in high school. But even before then, I acquired a strong interest in astronomy. On holidays, we'd occasionally travel from New Haven into New York City. And one of our annual stops was Hayden Planetarium, now the Rose Center. And that was terrific. Also, Yale University had a great natural history museum with a little astronomy section. I'd wander into that a few times every year. I was given, at around age 10 or 11, a gift of a small telescope – not a very serious one, but enough to get me started by looking at the craters of the Moon, the moons of Jupiter, and the like. And that leads to an interesting story, when I turned 13, which was sort of a big occasion. That's the year in which I had a bar mitzvah. Although, we were not a religious family, we did keep certain traditions. I always summarize that period by the slogan that it was “when Harlow Shapley came to my bar mitzvah.” Let me explain.

Harlow Shapley was the great dean of American astronomy. He was Head of the Harvard Observatory and astronomy department for much of the 20th century. And he'd made many great contributions. He was also very progressive politically. He was an opponent of Joe McCarthy, as well as an opponent of the House Un-American Activities Committee, and a proponent of the preliminary nuclear test ban treaty. He was also very outspoken. And for the week before my bar mitzvah, our rabbi, who was also progressive, had invited Harlow Shapley to come to our temple to address the congregation. The invitation was largely on the basis of Shapley’s progressive views and his being in the news on that score, and but also knowing that I was very interested in astronomy.

So Shapley came to the temple. But before giving his talk, I was ushered into the rabbi's office with Dr. Shapley and my parents. And after a short time, my dad, knowing that I had requested a

 a more sophisticated telescope ( a six-inch reflector) as a gift for my 13th birthday, was a little concerned and asked Dr. Shapley point-blank: "Should a boy of 13 really own a telescope of this magnitude?" To which Dr. Shapley replied emphatically, "By all means, and you must make sure he has photographic equipment to go with it!" After we talked, he autographed a book for me, entitled “Of Stars and Men”, which I still possess.

**ZIERLER:** That's great. Let the record state that Stu just held up the book for me to see.

**SHAPIRO:** So again, this was at a very early age. The space race, then, took over and brought my interest to the forefront, which helped catapult me further. I should also point out an interesting sidelight, I was a subscriber of Scientific American, and on occasion I would pick up Popular Astronomy as well. I was certainly fascinated by the articles in Scientific American, but terribly frustrated. Because although the subjects were terrific and the concepts amazing, I could never really learn from these articles how these things were derived. Where did these ideas get proven? How did these notions really reach fruition? The words alone, which Scientific American eloquently relayed, were not sufficient to convince me that these were right, or wrong, or would survive. And so, I realized at a very young age that mathematics, and physics, and equations had to be understood if you were going to really participate in this kind of endeavor.

**ZIERLER:** Of course, this is the origin story of your career as a theorist specifically.

**SHAPIRO:** And even in high school, knowing where I might be headed professionally, I knew that it would be math and physics courses to the hilt.

**ZIERLER:** Now, did you have a strong curriculum in math and science in high school?

**SHAPIRO:** Yes, I did. In fact, sometimes I argue my learning was never equal to that after that.

**ZIERLER:** That's high praise considering where you went off to.

**SHAPIRO:** Yes. But my high school was Amity High School in Woodbridge, a suburb of New Haven. And many of the teachers there were either spouses of Yale professors, or taking courses at Yale, or loosely affiliated with Yale. And we were the beneficiaries of that. For example, there was a new math program called SMSG that was developed by Yale mathematicians. I still think it's the finest way to teach the conventional algebra, algebra 2, geometry, and trigonometry pre-calculus curriculum. It was a very modern, abstract approach and I benefited from that. In addition, the space race triggered the National Science Foundation and several universities to bring experts together to devise a new physics curriculum called PSSC. I was a beneficiary of the textbook they wrote and the associated lab exercises they developed.

So the early advanced placement programs and accelerated pace of learning in science and math were promoted, including tracking, and they were positive elements of my early education. Now tracking has been controversial in recent times, i.e., pulling out students by ability and putting those of comparable abilities together in the same classroom. I confess, I was a beneficiary of having classrooms of people all of high aptitude, so that the teachers could move quickly through lots of topics. So that's where it began, and I claim that was a very important foundation. I also benefited from two summers at the Mount Hermon School, where the NSF organized advanced summer programs in several disciplines for high school students, including physics.

I confess that I was shocked when I arrived at Harvard because I found that I was not alone in my experience. Indeed, there were many talented students from all around the world with advanced training who found their way to this campus. And the level of both instruction and learning that one had to do on one's own increased dramatically.

**ZIERLER:** Was the draft something that you had to contend with between high school and Harvard?

**SHAPIRO:** Very good question. I was born with a congenital heart defect, and I had open heart surgery at age 21 for a ventricular septal defect (vsd) repair. So I was 4F all through college because of my heart condition. 4F was a category that meant you were not draft eligible. Now, that does lead to an interesting aspect of my life because, as you know, the Vietnam War was raging during that entire period that I was an undergraduate and graduate student.

**ZIERLER:** 1965 is a very interesting time to arrive at Harvard.

**SHAPIRO:** Yes, and 1969, my final year, was especially interesting. Now even in high school, I was an opponent of the war in Vietnam. And my valedictory speech at my 1965 high school graduation focused on that, which was somewhat controversial. There was both condemnation from some quarters, and high praise from others. After that I participated in anti-war demonstrations as an undergraduate at Harvard. These were small at the beginning, in '65, and became bigger as the years progressed, particularly as a result of the draft. I didn't need to burn a draft card because of my 4F standing, but I certainly participated in anti-war activity. I believed that it was an unjust and unwarranted war. I think, in fact, that this view is the general consensus now. The year 1969 was significant because of the continuing war effort with the new administration in Washington, and, at Harvard, these developments triggered a massive student strike. Although I was not a leader of that strike, I was certainly an active bystander. And all during that time I was also finishing up my honors thesis.

It was a honors thesis under the guidance of Icko Iben, a Profesor visiting from MIT who was world-renowned in the theory of stellar structure and evolution. My thesis was on the solar neutrino problem, which was just then becoming a hot topic. So I was going back and forth between anti-war rallies and computers during that time. I have a magazine picture here that is revealing, so let me show you. I was invited to give a talk in 2008 at Harvard on research that I'd done on compact binary star collisions, gravitational collapse, and the generation of gravitational waves. I began by noting that it was a much more turbulent time when I was at Harvard than in 2008. My opening slide was a picture of the cover of the Harvard Alumni Bulletin in the year 1967. Shown there was a small anti-war demonstration in front of the famous John Harvard statue. And there's a fellow marching there who is prominently displayed in the line of marchers. He is holding a sign that says, "Bring the Troops Home Now!" The associated article is entitled, *The Voices of Dissent*. And if this view of the cover that I am holding up now for you to see wasn't close enough, here's a closer view zooming in on that fellow. [laugh]

**ZIERLER:** This is you.

**SHAPIRO:** That fellow is me, yes. So that activity was, indeed, very much part of my life. It constituted my complementary activity during that period, and even beyond, continuing at Princeton, where I was a graduate student.

**ZIERLER:** Was your intention to pursue physics from the beginning at Harvard? Was that the plan right from the start?

**SHAPIRO:** Physics and astronomy, yes. I was basically a physics major in my program of courses, but in my final semester, in order to do an honors thesis in astrophysics, I had to become an astronomy major. By that time, I was taking graduate math and physics courses. In fact, I stayed away from astronomy courses on the grounds that astronomy is really applied physics, and wouldn't it be better to get as far as one can in the basis physics and math curriculum before one studies their application to astronomy?

So I took, essentially, two courses in astronomy, and one was Icko Iben's stellar structure and evolution course, which was a great course. Absolutely great. And that's where I met him and got involved with research in my final semester. And to this day, I advise all of my advisees who want to do astrophysics to focus on physics and math, and take astrophysics courses at the graduate level after you have all of the underpinnings. You can't really study astrophysics unless you know classical mechanics, statistical mechanics, thermodynamics, quantum mechanics, relativity, electromagnetism, etc. So why not wait? Get those firmly under your belt first.

**ZIERLER:** What faculty at Harvard was interested in general relativity when you were an undergraduate?

**SHAPIRO:** That's a very good question. Almost none.

**ZIERLER:** Yeah, it was a backwater.

**SHAPIRO:** Yes. Professor David Lazer. who was my official advisor, was a bit of an exception. Although, I didn't interact that much with him. He worked in cosmology at the time, which of course utilizes general relativity.

**ZIERLER:** The term cosmology was used in a serious sense at that time?

**SHAPIRO:** Oh, yes. But only in the latter part of my undergraduate career did it begin to become the observational endeavor that it is today. For example, in my final year, just prior to graduation, I was inducted into Sigma Xi, one of the major science societies. And as a gift, we were given the latest issue of the American Scientist magazine. And that magazine had an article by Bruce Partridge on the recent discovery of the cosmic microwave background (CMB). And that discovery later led to a Nobel Prize for the Bell Labs physicists Penzias and Wilson. However, Princeton played a very big role in realizing the cosmological significance of that detection. And Partridge, Peebles, Dicke, and Wilkinson all were instrumental in probing that radiation, even prior to and following its detection. And that really triggered my interest and further stimulated my desire to go to Princeton for graduate studies, although by then, I think I'd already accepted their offer to attend. So cosmology was already on board as a serious science by that time, but nothing like it is today.

**ZIERLER:** What were your first interactions with Bill Press?

**SHAPIRO:** He was a classmate of mine at Harvard, although I never knew him then.

**ZIERLER:** Oh, you didn't? That's interesting.

**SHAPIRO:** In fact, there are about a dozen physics or astronomy majors at Harvard at the same time as I who became well-known physicists or astrophysicists but whom I never knew as an undergraduate! I'll get to your question in a minute, but I did became very acquainted with one person, Ted Einstein, who became my roommate in my final two years. Ted is a condensed matter physicist on the faculty at the University of Maryland and was a PhD student of Schrieffer at Penn. But a lot of these Harvard colleagues I only later discovered were also classmates. So Bill Press I only met later on when I was at Cornell. He was a close colleague of Saul Teukolsky, with whom I collaborated. Press would visit Cornell and give a colloquium every now and then, and that was my first encounter with him. He also invited me to give a seminar at Princeton when he was on the faculty there.

**ZIERLER:** What kind of advice did you get, or not, as an undergraduate about programs to apply to, people to work with based on your interests and abilities?

**SHAPIRO:** You mean for graduate school?

**ZIERLER:** Graduate students, your professors, who was telling you where to go, including possibly staying at Harvard?

**SHAPIRO:** Right, to which I also applied and got in. And the answer, really, is by reputation -- I knew that if you wanted to do relativity, you go to Princeton. That's where Einstein was. Of course, he was not alive nor at the university, but once was a fixture at the Institute for Advanced Study. The distinction between the university and Institute wasn't all that clear to me at that time. But I learned there's certainly significant interaction between these two institutions. But Princeton, you knew, was where the latest developments in relativity and cosmology were taking place. That was where the concept of a black hole was being developed, although I don't think I was at all aware of black holes prior to

arriving at Princeton. (Believe it or not, there was such an era when one could be so ignorant!) But neutron stars and pulsars were already hot topics which I had heard about. I remember that I once attended a colloquium at Harvard where the Cambridge University scientists involved with the discovery of pulsars came and gave a talk. I remember the diverse theories that were presented there for the origin of pulsars – not only neutron stars but even the so-called “little green men” theory, i.e., other civilizations signaling us. I think I attended that colloquium in my final year, although it might've been the prior year. So I certainly heard about neutron stars as an undergraduate, but as for black holes, that had to wait until my arrival at Princeton.

**ZIERLER:** Now, to go back to this question about physics departments and astronomy departments, did that factor into your thinking at all, the way that Princeton organized these disciplines in ways that were different from Harvard?

**SHAPIRO:** Yes. And that's a good point because Princeton, after my own heart, had a department not called “astronomy”, but rather the “department of astrophysical sciences”. And that seemed to be right on.

**ZIERLER:** As a blended approach, you mean.

**SHAPIRO:** Right. I also recall a long discussion I had at the time with Ed Purcell. He was a Nobel Laureate at Harvard who taught my very first class in electromagnetism out of his just-released textbook, which by the way, I consider the best physics textbook of all time. Later, in my third year, I was asked to be an instructor in his course, using that textbook. But even later on, when deciding on graduate schools, I had a discussion with him on the Princeton physics versus astrophysics departments, and we went back and forth. Purcell was a close colleague of both Spitzer, with whom he had collaborated on the theory of interstellar dust grains and who chaired the astrophysics department, and also Bob Dicke, who was in the physics department. So Purcell gave me the notion that either department was a good option. I settled on astrophysics as my home department, thinking that while I wanted to do general relativity, in case that didn't pan out, I was also intrigued by stellar evolution and other astrophysical topics, so it would be prudent to join a department where there were more faculty doing research in these other astrophysical areas.

**ZIERLER:** How, then, did you first connect with Jim Peebles?

**SHAPIRO:** When I arrived at Princeton, I was more or less told that each semester, prior to embarking on a PhD, you would work with a different faculty member on a different research project. There were only a very few students admitted each year to the astrophysics department at Princeton. (I think there were three students my year), so such intense interaction with faculty on research was very possible right upon entrance. By the way, one of the other students who entered Princeton when I did was Richard Gott, who was also a contemporary of mine at Harvard, but whom I only met in final year as an undergraduate. We became much closer at Princeton, In fact, Rich was my office mate for all our years at Princeton. Now we were told we had to identify a faculty member to work on a research project, and I knew from that earlier reading of the Partridge article that Jim Peebles , a member both of both the physics and astrophysics departments, was involved with cosmology and the CMB. I wanted to see what they were all about. So I made an appointment and I asked Jim if I could do a research project with him. He kindly agreed. Jim posed my very first project, which resulted in my very first publication, which had to do with the calculating density of baryonic matter in the universe.

**ZIERLER:** Now, was that what Jim himself was working on at that time?

**SHAPIRO:** Well, he was not doing that particular calculation, but instead exploring the significance of the CMB. But he structured my project with me, and I went off and did it. Of course, I interacted with him all the way, looking at data in what was a brand new catalogue of galaxies at the time (the de Vaucouleurs catalogue) and redoing a calculation that had been performed two times earlier, once by Jan Oort and later by Sidney van den Bergh, to calculate how much visible baryonic matter exists in galaxies. This was at the very beginnings of concerns about not being able to close the universe with a critical amount of baryonic matter alone – the “missing mass” problem. In fact, the possibility of a sub-critical, open universe was still very viable at that time. And we wanted to recalculate how much visible baryonic matter was actually present in galaxies. I confirmed that the density of visible, baryonic matter constituted only a small percentage of the total matter density in the universe, lending further support that “dark matter” would have to account for the “missing mass” indicated by other measurements.

**ZIERLER:** On this question of reanalysis of all of this data, at the time, what was going on in observation and experimentation that made this analysis possible? What advances had there been in these years?

**SHAPIRO:** Well, these were ground-based, optical telescope observations that I was using. In addition to the optical catalogue mentioned above, I know that Peebles got in touch with Allan Sandage at Mt Palomar, since, as Jim told me, Sandage would often stash lots of new data in his desk drawer before publishing. And we wanted to get access to that. Every galaxy counted in our survey. As for other instruments at the time, I was not and am not an observer, so I can't give you the details. But except for ground-based observations (optical and radio), and a few infrared balloons, there wasn't much being done in cosmology obervationally, to my knowledge. As I went on in Princeton, there began the mushrooming of X-ray astronomy. And that development indirectly launched what I worked on for my PhD thesis – and for that matter, much of the rest of my career. But cosmology was mostly ground-based, including the microwave detectors at Bell Labs and on top of Princeton's laboratory.

So that's how I met Jim, and that was a fruitful interaction for almost a year. A year or so later, I asked Jim if he would be my PhD thesis advisor. And he graciously agreed, and he directed me to another hot new area, and that area involved black holes and gas accretion onto black holes. We didn't know know then whether black holes really existed, but before I earned my degree, the X-ray satellite Uhuru had discovered Cygnus X-1. This object was a binary system containing a massive normal star with an unseen, compact companion that had a mass inferred to be much bigger than three solar masses. We therefore knew that this companion exceeded the upper mass limit of a neutron star. And so, it became the first credible black hole candidate.

The question then arose: can we identify the black hole indirectly by the electromagnetic radiation emitted by the hot gas that flows from the normal companion onto the black hole? Or what if there are isolated black holes floating around in our galaxy, immersed in interstellar gas? Can we identify them via the gas that's radiating before it's swallowed by the black hole? And so, addressing these questions constituted my project. I believe I was the first to calculate in general relativity the total luminosity and the radiation spectrum from gas accreting onto a black hole. My calculation was, in those days, a restricted spherical accretion analysis, like the classic Bondi problem, except that I performed it in general relativity and took account of the heating and cooling of the gas and the total radiation that we would detect and the spectrum we would measure on Earth from the radiating gas.

**ZIERLER:** Given, as you say, there wasn't much opportunity in GR at Harvard, were you largely self-taught in GR at Princeton? Would you talk with Bob Dicke? What was your education in GR?

**SHAPIRO:** No, no, no. Princeton was the GR mecca. There really were only two main meccas in the US at the time. There was Princeton, which I'll describe, and there was Caltech with Kip Thorne, who was a Princeton protege…

**ZIERLER:** Yeah, but this is brand new at Caltech.

**SHAPIRO:** True, Kip had only recently arrived, but was already making a strong impact and assembling a strong group. And even in my final year at Harvard, during my last two weeks, a draft copy of what later became MTW (the Misner, Thorne, and Wheeler GR textbook) was circulating. That book has become the bible for GR for me and many others.

**ZIERLER:** You're saying then that Maryland would be the third mecca?

**SHAPIRO:** Well, at that time, I would've said the University of Texas. With Bryce deWitt, Cecile deWitt-Morette, Larry Shepley and others.

**ZIERLER:** I was thinking North Carolina, but he had moved at that point.

**SHAPIRO:** There was a whole group at Texas and where the Texas Symposium originated. And I think even Roger Penrose had a visiting appointment there. And that's where John Wheeler went after he had to retire from Princeton.

**ZIERLER:** So you missed Wheeler, you didn't get to interact with him?

**SHAPIRO:** Oh, no, no, no, on the contrary. My first year at Princeton, there were two courses that I took that were seminal. One was general relativity. The fall semester was taught by Charlie Misner, who was, I think, on sabbatical from Maryland, and the spring semester was taught by John Wheeler. That year was a little bit of a hodgepodge. MTW was not complete, and I think Misner handed out only a few chapters that, unfortunately, were not in the order in which they finally appeared. So there was a large amount of independent reading one had to do, but the excitement of that subject was conveyed at the highest level by these two experts. One, of course, a very mathematically oriented genius, Charlie Misner, was deep in the mathematical underpinnings of the subject and the other John Wheeler, had great physical breadth and overview. And walking into Wheeler’s class every session, we would see the blackboard filled with colored chalk diagrams, really artistic illustrations, illuminating the phenomena he was about to describe. And he would always draw a little magnifying glass that zoomed into one small, but crucial, portion, which he would blow up for emphasis. Incidentally, as a salute to John Wheeler in the book that I wrote with Saul Teukolsky on black holes, neutron stars, and white dwarfs, there's one figure in that book with a little magnifying glass zooming in on a point. And on the handle of the magnifying glass, we have the initials JAW for John Archibald Wheeler [laugh]. So that was one course, relativity.

The other course I took that first semester of my first year at Princeton was Jim Peebles's course on cosmology. Jim had just come back from a sabbatical at Caltech. And he now had the first draft of his new book, his first book on “physical cosmology”. And that was, I think, one of the first cosmology courses at a modern level ever taught. It was a terrific course. John Wheeler sat in the back of that class, taking notes in that class with the rest of us, and had his notes mimeographed. They would be signed “notes from anon” and would be handed out to us the next lecture.

By taking those two courses in the physics department, as well as quantum mechanics, I could not take the stellar dynamics course that every student was supposed to take in the department of astrophysical sciences that semester. And as “punishment”, I was assigned, in my second semester, a project in stellar dynamics with Lyman Spitzer. I facetiously describe that as punishment. In fact, it was one of the greatest opportunities of my life to do that project. Because I got introduced to a whole new area and a whole new way of doing computational physics.

**ZIERLER:** What was Spitzer working on at that point?

**SHAPIRO:** He, like most great scientists of that era, had several things going on at once. With me he worked on globular cluster dynamics, or, more generally, the physics of large N-body systems that interact exclusively by their gravitational forces in Newtonian theory. So that would embrace clusters of stars, such as globular clusters and dense galactic cores. The evolution of these systems was described by the Fokker-Planck equation, which treated weakly collisional systems comprised of particles that would orbit many, many times without undergoing significant changes in their energy or angular momentum. But over time, due to the cumulative effect of gravitational scattering with distant neighbors, their energies and angular momenta would evolve, and their orbits would therefore change. I thus was introduced to the formalism to describe this evolution. Prior to this experiemce I had not even heard of the problem or seen the relevant equations, let alone know how to solve them.

But not much later in my life, but after I left Princeton, I combined my interests in general relativity and black holes with stellar dynamics to work on problems that involved both areas. These included star clusters around supermassive black holes (the equilibrium stellar density and velocity profiles, the stellar tidal disruption rate by the black hole, and the cluster evolution with time) and, more recently, clusters of dark matter particles, including those around supermassive black holes. This work all began with, and would not have happened without, my research semester with Lyman Spitzer. I also learned a neat skill from him, which I will now describe.

 For the problems we solved, I did all of the computer coding. We then would go over the output very carefully together, and we would design the next set of calculations together, but I would do all the coding and run all the simulations. But Spitzer was also very instrumental in debugging my code, without ever having written a single line or even looking at my code! And here's how he did it. At one point, I handed him some numerical output and he looked at the column of numbers. He didn't like what he saw. I then saw him take a pencil, and next to the output, he would write down the difference between adjacent numbers. He then pointed out to me that there was a noticeable discontinuity, a glitch, in the new column of differences that he had created. It wasn't that the numbers themselves appeared discontinuous, but their derivative certainly did. And he said, "There's no physical reason why this should be. Can you explain it?" Well, I couldn't explain it, but I went back, and in those days, we would punch input data onto cards, as we didn't have the personal PCs we have now, and sure enough, one of the data cards I had generated was punched incorrectly. As a result, I was inputting the density profile of this cluster incorrectly at one point. And Spitzer discovered that there was a typo by noticing an anomaly in the output at the first derivative level!

So why was this experience important for me? This was important because I've since worked with many students on many projects, some simultaneously, and I do not have the time to write the codes myself for many of the projects we're working on. Moreover, I don't even have time to read over their codes once they have written them. Therefore, I have to find ways of debugging and assessing the validity of their output. While there are many methods and tricks that I have developed, they all began with realizing that Spitzer, who didn't write any code, had ways to assess the validity of the output and thereby make valuable contributions in computational astrophysics. So I've always regarded him, although he didn't write code, as a bonafide computational astrophysicist at the highest level.

**ZIERLER:** What was Jim's style as an advisor? Did he give you a problem and send you off? Would you be in his office every day?

**SHAPIRO:** More of the former than the latter. At the time, I might've been a little frustrated. But it served me well, because when reaching a dilemma or a point of confusion, rather than having him bail me out all the time, although there were times that he did, I would have to think about the problem a little harder a second and third time and figure it out myself. And that process served me well. But Jim was someone I could always go to with the deepest question, and I would often do so. What amazed me most about Jim was that he could go to the blackboard when I would come in with a query, and, without consulting a book or any notes, write the key equation on the blackboard because he knew it and then point out where or what was important, what was not important, and just proceed. He had an enormous depth and breadth to advise me, which, of course, were attributes that have characterized his entire career. Cosmology is really all of physics and astrophysics dumped into the early universe. And Jim knows the whole gamut of physics, and I was able to appreciate and benefit from that.

Recognizing this early on has served as a guiding principle for me. I concluded, "That's how you become a top researcher. You've got to be broad. And you can't be over-specialized because there's something you may not know that may turn out to be very relevant.”

I also noted early on that Jim was left-handed. And I once overheard Jerry Ostriker, another Princeton astrophysicist who was also left-handed, proudly declare that left-handed people had a higher IQ statistically. Now I'm not left-handed, so of course I was a little disappointed. But my dad was! So I figured, "OK, at least I have some of the genes". So as I was saying, with his left hand, in his slanted handwriting, Jim would go to the blackboard and be able to clarify a subtle issue for me. But the bulk of my thesis work I did independently. He would assess it, and I might then revise it a bit, but then I would go off and continue working. In retrospect this experience was just great.

**ZIERLER:** At what point in your interactions with Jim did both of you decide that you were ready to defend?

**SHAPIRO:** It wasn't as formal a moment. I think what happened was, I had two papers either accepted and/or published at one point that had emerged from my thesis and they formed a nice, unified story. It was the early spring of my fourth year, and at Princeton, you never stayed beyond four years. Moreover, I'd already authored or co-authored three publications before these: the one in cosmology that came out of my earlier work with Peebles, one in cluster dynamics with Spitzer, and another in cosmolgy with Joe Silk, who was a post-doc then at Princeton. So the thesis papers were my fourth and fifth publications. So by the spring of my fourth year it was time to defend.

**ZIERLER:** Besides Jim, who else was on your committee?

**SHAPIRO:** Lyman Spitzer and Francis Perkins, a plasma astrophysicist whom I consulted on some of the magnetohydrodynamic issues that arose in my analysis. Now two years prior to that I had to pass a department PhD oral qualifying exam. My committee then consisted of Peebles, Ostriker, Spitzer, and Sam Treiman, a well-known high-energy physicist. As I told you, I was very eager to be regarded as a bonafide physicist, so I had taken Treiman’s rigorous field theory course. I must say, as I told you, Purcell's textbook was the finest textbook I ever read. But Sam Treiman's lectures were the finest lectures I ever attended. He would essentially put his elegant lectures onto the blackboard, and my notebook from that course, which I still have, could be made into a textbook with little editing. They were beautiful lectures, and I learned quantum field theory and QED from them. And that was the area that Treiman quizzed me on during my qualifying exam. In fact, his questions concerned the very last part of that course, which was on renormalization theory. Thank goodness I was on top of that at the time.

**ZIERLER:** It's not your field, but right around the defense, were you aware of all the excitement in QCD with Gross and Wilczek?

**SHAPIRO:** No, not really. That was a different world. And string theory was not quite on board yet. However, it is relevant to relate a story about my professor for my first-year graduate quantum mechanics course at Princeton. It covered some relativistic quantum mechanics, such as the Dirac equation, things of that sort. We would meet in the evening and the professor did not deliver the most exciting or original lectures because he more or less mirrored closely the textbook. But then a little over three years later, on an airplane, reading one of these airplane magazines, there appeared a column about the great new discovery of string theory. And right there was a photo of the guy who triggered this, and it was the professor who taught this uninspired quantum mechanics class – John Schwarz! Talk about originality! This guy whom I thought was rather dull was a founding father of string theory! Who would have guessed?

**ZIERLER:** Anything memorable from the oral defense? Any memorable questions?

**SHAPIRO:** Yes, here's the most memorable thing. I had studied profusely Peebles's book on physical cosmology. After all, Peebles was going to be at the exam, and cosmology was one of the areas that I presumed I was going to be quizzed on. But Jim asked me nothing about cosmology! Zero. I had studied and could've answered anything he might throw at me in that area. Instead, his questions were exclusively on black holes and black hole physics. And although I had some formal course background on black holes, any expertise on that topic was still a couple years away. Thank goodness his questions were general enough that back-of-the-envelope, order-of-magnitude answers were sufficient for the most part. I think I worked my way through them OK. But I was terrified since I had not really reviewed or prepared anything on this topic. This was, remember, prior to my thesis, which ultimately revolved around black holes.

Back to the thesis defense, which is what you asked about originally. That was nothing really memorable. I was on top of that subject, and the questions were pretty much apple pie. I recall that Spitzer asked me to generalize how I would think about this problem if the medium enveloping the black hole was different from the assumed interstellar medium that I had considered. I had treated HI and HII regions, and he asked about molecular hydrogen regions. So I had to think on my feet a little bit. But most of the questions were softball questions. I basically presented a respectable seminar of the topic of accretion onto black holes.

**ZIERLER:** Now, what post-doc opportunities were you considering at this point?

**SHAPIRO:** I guess I had applied to several places, but my first choice, with some advice from Jim Peebles, was actually to go to Cornell. I had met Ed Salpeter earlier. He was at the Uhuru symposium at Princeton that I mentioned and, later that summer, he was a session chair at an American Astrophysical Society meeting at which I gave a paper. He came over to me, and encouraged me to apply to Cornell. I conveyed that to Jim, and Jim thought that going to Cornell would be a great idea. At the time I applied Bob Wagoner was at Cornell. He was a------------------------------------------- relativist. And so, with X-ray astronomy and compact objects, where Salpeter played a role, and a relativist on board, it seemed like it would be a good fit.

**ZIERLER:** Now, what about Kip Thorne? Was Caltech in the mix, also?

**SHAPIRO:** I had met Kip earlier, but no, Caltech was not in the mix then. I was more interested in working on black holes in an astrophysical setting, rather than on the physics of isolated black holes, which I considered the main thrust of Kip Thorne’s group at the time. I was inserting black holes in radiating gases and in media that could be observed by telescopes, such as gamma ray, X-ray, and optical telescopes. That was my focus. So, I wasn't as serious about Caltech as I was about other places. Also, as I'm thinking back, I was always East Coast oriented. I was born and raised in the East, and I'm sort of covered with Ivy. Born and raised in the New Haven area, I worked summer jobs in the Yale Physics Department, went to Harvard as an undergraduate, and Princeton as a graduate student. Cornell was next on the bucket list. Moreover, staying a few hours drive from my home in the New Haven area was important, and that was a consideration.

**ZIERLER:** Now, was it conveyed to you, at least informally, that if everything worked out, you would be invited to join the faculty at Cornell?

**SHAPIRO:** No. [laugh] I had no such expectations. But in those days, we weren't as worried about things like that. At least, I wasn't. I had the attitude that things would all work out in the end. I don't know why I had that attitude. I had no reason to. But what happened was, after my first year there, a couple of the faculty, including Salpeter, were planning to take sabbaticals, and the department needed a course to be taught in high-energy astrophysics. I was asked if I would do it, even though I was a postdoc. As an inducement, they said, "Your two-year postdoctoral appointment would then be extended a third year." So, I thought, "Well, it would be an interesting experience, teaching a graduate course. Fine." I co-taught that course with Franco Pacini, who's now deceased, regrettably, but was one of the early pulsar theorists from Italy and who was on sabbatical at Cornell. We focussed on different areas on different weeks, but I wound up giving most of the lectures. The notes that I prepared for that course were the beginnings of my later book with Teukolsky on the physics of compact starts. Then, at the beginning that second year of mine at Cornell, a job position opened up for assistant professor in the astronomy department. I was encouraged to apply and I did.

In fact, the odd thing was that I was also organizer that semester of the weekly astronomy colloquia. So, when candidates came in to give their job talks, it was my job to greet them, arrange for lunches, sometimes take them out to dinner, maybe even show them the campus. So here I was, the guide for all of the candidates who were also applying for this same position that I was! It was thus my role to welcome and introduce them to Cornell. I found that a little weird, but I don't know, it was a unique time. Somehow I wasn't so concerned. If this job didn't open up, something else would. Even when I was a graduate student, a teaching position opened up at Berkeley, and Jim Peebles told me to apply for it. I didn't get it, but I was astounded to have been thought of in that role at that time. So I wasn't overly concerned. As it turned out, the decision at Cornell was made on December 6, which happened to be my birthday. I remember that it was Carl Sagan, right after the faculty meeting at which they reached their decision, who popped into my office and announced "Happy birthday, you got the position!” That was it.

**ZIERLER:** When did your collaboration with Saul really get going?

**SHAPIRO:** Pretty early after he arrived.

**ZIERLER:** Which was when?

**SHAPIRO:** My second postdoctoral year, when he was a beginning assistant professor.

**ZIERLER:** So '75?

**SHAPIRO:** '73-'74 was my first year, so '74-'75 was when he arrived. I had met all of the people coming to interview for that position and heard their talks. While of course I was not involved with any decision, but was consulted from time to time on what I thought about the candidates. So Saul arrived, and so I already knew about his contributions and strengths. And one of my attributes is that I'm always thinking about new problems, some of which I can't solve immediately, but nevertheless are interesting. I'm always trying to gear them or reduce them to aspects that make them solvable. So I had such a problem, and it involved white dwarfs. It also involved general relativity in the post-Newtonian limit. I knew a lot about white dwarfs and Saul was a professional relativist, so I posed the question to him and invited him to join me. "Let's take a look at this." And we did. I think that was our first project. The results provided one of the PPN parameters that were famous in their day as a means of assessing the validity of general relativity and/or distinguishing it from other candidate relativistic gravitation theories. We came up with one of the PPN parameters that had not been previously calibrated.

**ZIERLER:** And of course, through Saul, I'm sure by osmosis, you got a lot of Kip Thorne.

**SHAPIRO:** Absolutely. Saul was strongly influenced by Kip, his PhD advisor. Saul's general relativity course was based on the course Kip gave at Caltech. When I was invited to teach GR at Cornell, and Saul graciously stepped aside to let me do that, I had a copy of Saul’s notes, which heavily influenced my own notes. By teaching it, I re-examined general relativity from the bottom up. And that was great. A great experience for me. The next time I interacted with Saul began a longer collaboration. I had published some very simple Newtonian models of stellar collapse to look for the gravitational wave burst that would be emitted. In the early days, we thought stellar collapse would be our prime source of observable gravity waves. Not binary mergers yet, that came a little later. But my models of stellar collapse really needed to be upgraded to general relativity to be reliable.

So I posed the problem to Saul, and he immediately told me that there was a big problem in doing stellar collapse in GR due to the fact that collapse remnants – black holes – have real, physical singularities inside them – regions of infinite spacetime curvature (thus infinite tidal fields). Consequently, if you went to a computer, and you simulated a stellar collapse in GR, then most often you would encounter this singularity and so the numbers in your output would blow up and your codes will crash! And they will crash long before the entire star had a chance to collapse all the way and long before you were able to watch gravitational waves propagate far enough out on your numerical grid to measure. This complication constituted a serious problem that had not been resolved. Well, I said, "Great. Let's look into that!" And that's how we got into numerical relativity.

**ZIERLER:** This is also a great example of, given the breadth and depth of your collaboration with Saul, each of your respective education, intellectual sensibilities, how it became successful as a collaboration over the long term.

**SHAPIRO:** Right. No question about it. I brought some relativity and a strong astrophysical background and Saul brought some astrophysics and a deep relativity background. We both had some mathematical and computer skills, but Saul served to augment mine considerably. In fact, working with him imparted the notion that I have carried with me the rest of my life, which is that no well-posed mathematical problem is beyond solving. One might have to be clever, one might have to think hard about it, and there ultimately might be tricks that need to be invoked, but no problem is unsolvable. When we worked together our typical format was to be in the same room at the same time. We would both be at the computer and would write code together, which meant that each of us had an intimate knowledge, line by line, term by term, of these massive codes. Sometimes I would call him at night and say, "You remember the term we added in this subroutine this morning? I don't think it was right." He knew what I was saying, and I knew what he was saying. That was a period that two of us together could do that. It's become more difficult now for two people to work this way; as many of the numerical relativity codes have become longer and more complex they seem to require large teams of code builders to write and debug.

**ZIERLER:** What were some of the advances in computation at this point that made numerical relativity possible, and what were some of the limitations that you might have perceived that were holding your work back?

**SHAPIRO:** Well, at that time, there were very few people working in numerical relativity and even fewer problems that had been simulated successfully. Larry Smarr and his colleagues were working in the field, as was Nakamura in Japan, York at Chapel Hill, Matzner at Texas, Centrella at Drexel, and Piran in Israel, but there were very few others doing numerical relativity. The number of groups in the world numbered fewer than a dozen. Saul and I managed to make some advances and we solved some very interesting astrophysical problems. Most people, including us, tackled problems with a high degree of spatial symmetry in those days (i.e., spherical or axisymmetry), partly because of computer resources, partly because you needed to learn how to solve the simplest problems first. So spherical stellar collapse and axisymmetric, head-on, binary black hole collisions were treated by us and others. One did not perform full 3D simulations at that time. We first published a couple of papers on spherical stellar collapse to black holes that solved the Einstein field equations coupled to relativistic hydrodynamics. Then we determined that if we worked with collisionless matter as a source of the gravitational field instead of a gaseous fluid, we could make quicker advances in numerical relativity. The reason was that hydrodynamic fluids experience shock waves so that ideal gas flows are discontinuous, whereby the corresponding hydro equations have their own subtleties to contend with. By contrast, collisionless matter, which could model, say, stars in a star cluster, can be described by simpler equations that are much easier to work with. We could then focus on developing better techniques to solve the Einstein gravitational field equations without worrying about the matter equations. By the way, our move to collisionless matter relates back to my training with Spitzer, which helped prepare me for this application.

So this was how we gave birth to the field of “relativistic stellar dynamics on the computer”. Among other things we learned how to choose gauge conditions for solving the field equations, which made for big advances in finding solutions. As in other fields, gauge quantities are not physical quantities, but mathematical interlopers that help us solve the equations and determine the physical variables. Choosing gauge variables wisely makes solutions easier to obtain. We were able to discover gauge variables that allowed us to follow collapse to its endpoint, avoiding the appearance of singularities at the centers of black holes. We were able to hold back the advance of time at the center to postpone the formation of a singularity there, while the rest of the cluster completed its collapse. So that was an advance. Next we extended our simulations to axisymmetry and two spatial dimensions and allowed for clusters with spin. With this development we were able to collide two black holes head-on by following the collapse of two clusters to individual black holes when they were apart and then tracking their collision and merger.

Our treatment was complementary to the work Larry Smarr was doing. He considered two black holes called “eternal black holes”. They existed as two black holes from the beginning of time before undergoing a head-on collision. Recall, we took two star clusters that were far apart, followed their individual collapse to black holes, and then pursued them as they got closer and closer together and eventually merged. We both were solving head-on black hole collisions. These two calculations represented was the first time a black hole collision was simulated successfully and could reliably measure the emitted gravitational waves.

Saul and I probed all kinds of properties, confirmed various global theorems and assessed several speculations regarding the physics of black holes for the first time, addressing things that could not be derived with pure paper and pencil. Such things included the “hoop conjecture” of Kip Thorne. This conjecture posited that black holes with horizons form when and only when a mass becomes sufficiently compacted into a region whose circumference in every direction is less than or equal to the circumference of a black hole of the same mass.

We were able to validate that conjecture, at least for a large number of numerical examples we probed. We could also assess speculations about star cluster collapse that mimicked theorems about fluid star star collapse, such as the so-called mass limit for neutron stars. Neutron stars cannot exist above a certain mass, or they would promptly collapse. Similar, but incomplete, theorems had been proven for star clusters by Kip Thorne, and Jim Ipser. We were able to show by simulations that those theorems likely could be completed. In particular, we were able to show that, indeed, there's a maximum compaction, or central red shift, for a star cluster beyond which it would collapse to a black hole and we could follow that collapse to completion. Years earlier, the Soviet physicists Zel’dovich and Novikov wrote about this and said that “a numerical investigation of such collapse is badly needed, but up to now no one has had the fortitude to attempt it”. I've cited this quote in talks, boasting half-facetiously that Saul and I finally mustered the fortitude to solve this problem!

That problem, in turn, led to some interesting ideas that are still floating around. One involves a formation mechanism for the supermassive black holes observed in most galaxies and quasars. If you take a star cluster or a galaxy with a very compact core of stars, maybe even a core of ordinary, stellar-mass black holes and/or neutron stars, and let it go for a billion years or so, it will undergo the “gravothermal catastrophe”, a phenomenon that Spitzer explored. The core will get denser and denser, but smaller and smaller in both mass and radius. This is purely scattering phenomena in a self-gravitating, large N-body cluster, as stars scatter by gravitational (Coulomb) interactions off other distant stars. And so, you get a slow, or “secular”, core contraction, the gravothermal catastrophe. The core gets more and more compact, higher and higher in stellar density and velocity dispersion, but smaller and smaller in mass and radius. We realized that that core would eventually become relativistic and then, as in our simulations, would undergo catastrophic (rapid) collapse to a black hole. We had simulated that very dynamical collapse – a galaxy with a small, relativistic stellar core that undergoes collapse. What did it leave behind? A supermassive black hole at the center and the rest of the stars in the galaxy remaining outside in stable orbits about the black hole.

Well, why is that interesting? It's interesting because most every galaxy we observe today, including our own, has a supermassive black hole at its center. Could this have been a mechanism for forming that supermassive black hole in the first place? Answer? We don't know. More preferred these days is the scenario by which so-called first-generation, or Population III, stars of a few hundred solar masses burn out and collapse to black holes. These black holes form the seeds of supermassive black holes by undergoing subsequent gas accretion and merger with other seed black holes when their host galaxies merge. They then build up to supermassive black holes. That route may be true, but it does confront a problem, namely the issue of available time. Specifically, is there enough time in the life of a universe for galaxies to form stars that undergo collapse to seed black holes, then merge with each other and accrete gas sufficiently to give rise to the billion solar mass black holes that we now see? We're running into problems as we discover black holes at higher and higher red shift, which means that they formed earlier and earlier in the universe. It means that there's less and less time for black hole growth through this process. Therefore, maybe supermassive black hole formation must undergoe another process. Maybe it is the one I suggested with Saul, i.e. core collapse galaxies form supermassive black holes because the clusters of smaller black holes and/or neutron stars become relativistic by the gravothermal catastrophe and subsequently undergo catastrophic collapse.

Another idea that I later introduced a couple of decades ago is that halos of “self-interacting dark matter” – clusters of dark matter particles that undergo occasional collisions via strong interactions in addition to gravitational scattering, as has been suggested by Spergel and Steinhardt – might undergo the same gravothermal catastrophe that we talked about for stars. And the dark matter cluster may undergo the gravothermal catastrophe until it, too, becomes relativistic in its core and undergoes catastrophic collapse to a black hole, just like we had simulated. I've published papers describing that scenario. This story has resurfaced lately, now that dark matter is observed to be so prevalent. That wasn't our understanding when I first worked on stellar dynamics with Spitzer way back at Princeton. But now, ideas come together and get refined, and maybe they offer a new, viable mechanism for supermassive black hole formation. We don't know yet. The deeper we go out in red shift and find them, the broader must be our thinking about mechanisms for forming these young supermassive black holes. This is a big mystery. I hope I'm alive when that mystery is resolved.

I remember Ed Purcell, going back to my undergraduate days, talking about the possible existence of magnetic monopoles, a topic which was always a little hobby of his. He once uttered softly to the side that he hoped that he would still be alert enough to appreciate how that story is resolved. I'm not sure it has been resolved. But I echo that sentiment. I would like to know how supermassive black holes form in our universe. I have ideas, and I've worked on different solutions. Which one is correct, if any?

**ZIERLER:** What were some of the initial responses to numerical relativity, both from within the physics community that was not involved in this new area of research and among computer scientists who might have been confused, or at least interested in what these astrophysicists were doing with these computers?

**SHAPIRO:** Well, astrophysicists in general regarded it as an esoteric subbranch.

**ZIERLER:** A subbranch of GR?

**SHAPIRO:** Yes, of GR. And the proof is in the pudding. Numerical relativists struggled to get faculty positions, and that sort of tells you what their standing was. Now, in the early days, numerical relativists really had to have other research lines that they were working on. They had to be profound relativists, fairly good computational physicists, and usually with another research hobby. But still, it was hard for us to train students who would get jobs in that subarea. Among computational physicists, the field began to be appreciated more and more, in part because of an interesting development. At Cornell, Ken Wilson, having won a Nobel Prize, used his platform to tell Congress that the US needed to vastly upgrade its supercomputing infrastructure. That was at a time when Japan was really taking leadership in that area. That lobbying gave rise to the National Supercomputer Centers, one of which at Cornell Ken Wilson became director. We had a good relationship with Ken. In fact, until Ken left Cornell for Ohio State Saul and I would have lunch with him from time to time. We never worked in the same area as he did, but he was always interested in what we were doing computationally and made sure we had access those early IBM supercomputers that he brought to Cornell.

At the same time, Larry Smarr had arrived at the University of Illinois, and became the director of NCSA, another one of the original national supercomputer sites. Larry, of course, was into numerical relativity. What could be better for the field? So numerical relativity became a significant fraction of what was done at two of the leading national supercomputer centers, just because of Larry at Illinois, and our work at Cornell. Then, when the Grand Challenge project on black holes was launched by the NSF, Saul, Larry and I were all co-PIs. This was a computational project to simulate binary black hole mergers in anticipation of a future gravitational wave detector that was under development (LIGO). This Grand Challenge project brought together not only numerical relativists, but computer scientists to work on developing parallel infrastructure for the first time in our field so that large numerical relativity codes could run on parallel machines. That project was not totally successful, and the simulation of the merger of binary black holes initially in circular orbit about each other did not emerge from that. The reason was largely because once you break spherical and axisymmetry and go to full 3D, as required by this problem, the standard numerical relativity formulations we were using at that time, the so-called Arnowitt-Deser-Misner, or ADM, formulations, always led to unstable numerical behavior and often a computer crash.

This problem was not entirely due to the singularities that formed inside the black holes, which could be accommodated to some degree by gauge choices and “black hole excision” (i.e. removing the black hole interiors from the computational domain), but by the fact that the formulation itself was not hyperbolic symmetric. This meant that whenever you had little numerical (e.g. roundoff) errors in your simulation, they would not propagate off the computational grid, but instead grow in time, and then lead to instabilities. One realized that the ADM formulation of GR, although identical to the Einstein equations analytically, led to problems numerically. That made the Grand Challenge an unsuccessful effort in the end, although much infrastructure was built for later use. The solution really required other breakthroughs.

One of those breakthroughs I was involved with, and that was the Baumgarte-Shapiro-Shibata- Nakamura, or BSSN, formulation of general relativity, where we recast the Einstein field equations in such a way that they were not only analytically identical, but they put the equations into a form that was more wave-like, more like solving a wave equation. Hence if you got little errors that cropped up, as you always do in a numerical code, they would propagate off the grid at the speed of light and not build up and cause instabilities. And that development led to a host of triumphs. The biggest triumph you may be aware of was the 2006 simulations by three groups: Frans Pretorius at Princeton, the Rochester group then at University of Texas at Brownsville, and the NASA Goddard group. These were three independent calculations, which all succeeded in merging binary black holes from circular orbits and recording the emitted gravitational waves. Two of these teams, Rochester and NASA Goddard, used our BSSN formalism to solve that problem, while Pretorius had yet a different formalism. These developments triggered the launch of numerical relativity as a way of tackling all kinds of problems in full 3+1 dimensions.

At this time BSSN and versions built around it are adopted in a majority of codes that are written today to solve, for example, binary neutron star mergers, black hole-neutron star mergers, stellar collapse problems, etc. Our own code, the Illinois General Relativistic Magnetohydrodynamics, or the Illinois GRMHD code, was constructed by me and my group of postdocs and grad students over a period of twenty years. During that time we gradually added new modules that reflected greater and greater physical complexity and realism (e.g. magnetic fields, realistic nuclear equations of state, electromagnetic radiation and neutrino transport etc). Now it has been embedded as a module in the open-source Einstein Toolkit. Today, numerical relativity, following the detection of gravitational waves by LIGO/Virgo, is, I would say, in high demand. There are many ways to prove that assertion. First of all, faculty positions have opened up. In Illinois physics I could not get approval to hire another person in numerical relativity to join the faculty for over 24 years. But in the last few years, we've hired two new people. I think that mirrors what's going on elsewhere. As another indicator, I published a textbook in 2010 with Thomas Baumgarte entitled “Numerical Relativity: Solving Einstein's Equations on the Computer”. Since 2015, when the first detection of gravitational waves occurred, its sales have dramatically increased. It's still a very small field, so we're not talking about the textbook for a first-year physics course. But I could easily tell following that detection what's happened to numerical relativity. It's now quite a respectable and growing field. In some sense, it is too respectable, because there are so many conferences and meetings, it's just hard to keep up. And so, I've got to employ my postdocs and graduate students to keep me up to date with developments, using their antennae as well as my own.

**ZIERLER:** What was your earliest point of contact with LIGO, and who was it? Was it Kip Thorne?

**SHAPIRO:** Oh, yes. I first interacted with Kip as a grad student at Princeton. He came to give a colloquium on the hoop conjecture, which I mentioned earlier. I had little idea what was going on then with this topic. Little did I know that I would be performing simulations relevant for that later on. But I definitely remember that he gave a talk on the hoop conjecture. That stuck with me. And then, there was that conference at Princeton, organized by Princeton Astrophysical Sciences, on the new Uhuru X-ray discoveries that I mentioned. There the very first pulsating X-ray stars, which we now know as neutron stars accreting gas from normal companion stars, as well as Cygnus X-1, the first credible black hole candidate, were all discussed. At that meeting the Uhuru director, Giacconi, who later got a Nobel Prize for that work, gave a talk, several theorists gave talks; Kip attended that meeting and we chatted about gas accretion. I don't remember his talk at the time, but very soon after that, Kip published his seminal Les Houche paper with Novikov in which the Thorne-Novikov relativistic accretion disk around a black hole model was presented. We had a little chat at the meeting because I was doing my thesis then and was working on a complementary problem, the spherical accretion problem, i.e. flow from a more or less spherical distribution of gas that you might find when the black hole is immersed in a cloud, or the interstellar medium, or what later turned out to be a more turbulent, magnetized disk which becomes very bloated and not thin near a black hole. So we had a conversation then. The following summer I again interacted with Kip at a NATO summer workshop that Martin Rees organized at Cambridge University. Even before these meetings, as I told you, as an undergraduate, in my last two weeks at Harvard there were draft copies of random chapters from his forthcoming book, MTW, which I got a hold of and xeroxed. (Were it not for a flood in my parents' basement, I'd still own it today!) So I've interacted with Kip over the years. He was a member of the Grand Challenge consortium, and I’ve interacted with him for decades in conjunction with colloquia and seminars that he and I have given and conferences we both attended. Kip has also invited me to Caltech on a couple of occasions. So there have been interactions all throughout my career.

**ZIERLER:** What were some of the advances in Big Bang nucleosynthesis during your Cornell years?

**SHAPIRO:** Well, there might have been even more, had Bob Wagoner, for whom I came to Cornell to work, not left the month before I arrived. He left to go to Stanford. I'll always resent the fact that Cornell never told me about his planned departure when I was deciding where to go for my postdoc, because they knew about that. But he left, and he, of course, is one of the developers of modern Big Bang nucleosynthesis. The Wagoner code was one of the first big codes that determined the helium abundance in the early universe. Prior to that time, of course, the early studies, e.g., the Alpher-Bethe-Gamow work, brought that issue to light. But I cannot say that I knew of ongoing work in that field when I was at Princeton, but I knew in detail the nature of that problem through Peebles's course; his first book discusses, in a beautiful way, the physics of Big Bang nucleosynthesis. It's a calculation you can do analytically to first approximation. In fact, when I taught GR, both at Cornell and Illinois, I had almost a quarter of a semester on cosmology, where I would derive the primordial helium abundance on the blackboard. I think that is one of the greatest calculations of all time.

As the cobwebs are being removed from my brain, I now remember that I wrote a paper at Princeton with Joe Silk. The origin of this paper was John Wheeler's GR course. John Wheeler assigned a research project. He told every student they had to turn in a paper. As I had also been taking Peebles's course in cosmology, and I understood the Big Bang nucleosynthesis formulation, and I wondered about fluctuations in, say, the local temperature of gas, because it seemed that there was a miracle that took place in Big Bang nucleosynthesis: the rates of weak interactions and the time scale for the universe expansion worked out just right that we got the percentage by weight of helium to be neither 0% nor 100%, but about 25%, and, hence, provide the necessary building blocks for stars and life. And I thought, "Well, what if there were fluctuations in, say, the temperature of gas at that time? There could be hot regions and cold regions. How would that change the story?"

I don't even remember the details of the calculation, but I wrote a paper, and then Joe Silk helped put that into a more cosmological, up-to-date context. So that was a contribution of mine way back when. Later on I wondered how the story might be changed when it was discovered that neutrinos had nonzero mass and I coauthored a paper on that topic (The answer is that the results are unchanged). I think most of the work since then has gone into more sophisticated codes, more sophisticated understanding of nuclear reactions and observations to find sites of clean, primordial helium, i.e., helium not generated in stars, but helium originating from Big Bang nucleosynthesis. To a large extent, observations have confirmed the calculations. And I find that a miracle. That was one of the first great miracles of my life, to think that I could be writing on the blackboard something happening in our universe when it was but a minute old, and we could confirm it today! Here we are, 12 billion years later, and when it was a minute old, something happened, and we can understand what happened! Is that crazy, or what?

**ZIERLER:** That's pretty good. What is the cosmic censorship hypothesis, and how did your work with Saul come to challenge or violate it?

**SHAPIRO:** Cosmic censorship is a famous proposal by Penrose that if you have gravitational collapse from any kind of realistic initial data, meaning some finite glob of mass-energy outside of which is vacuum, and the glob undergoes collapse to a singularity where the tidal field becomes infinite, that singularity would always be situated inside a black hole, i.e., clothed by an event horizon. The consequence would be that the singularity could not be “seen” from the outside, or even influence physical behavior or physical laws outside the black hole, because any signal emitted from inside a black hole--inside an event horizon--could not propagate outside the event horizon. Hence the laws of physics and general relativity would remain valid outside and be protected from the formation of singularities, which would always be inside black holes.

Now a region of infinite spacetime curvature and infinite tidal forces such as we find inside classical black holes would be most unphysical. We don't really believe that such singularities actually arise in nature. For example, the whole last century was built on removing singularities from quantum field theory via renormalization. We do not as yet have a viable quantum theory of gravity that might do something similar for singularities that arise in classical GR. Now, it has been argued that, "Well, when we have a quantum field of gravity, quantum mechanics will smooth out those infinities and make them finite." But of course, I would still argue although they would be finite, they still will be absurdly large. I think that still would be uncomfortable for general relativity, to think that there were these huge tidal fields hanging around all the time in the universe, were it not for cosmic censorship, which, if true, saves the day by constructing an event horizon around them.

Well, Saul and I realized that there might be a bit of a problem if you assume that the hoop conjecture and Penrose's cosmic censorship conjecture were both correct. Because suppose you had a long string of matter. This string could undergo a collapse, particularly if there were no pressure to support it. So say one had a very long string of collisionless particles of very small, but finite, thickness, with the particles initially at rest and uniform in density; it will undergo collapse. The result is well-known in Newtonian physics: the string will collapse to a singularity of infinite density and infinite tidal field in the form of a long, thin needle. Kip Thorne himself had studied as a graduate student the collapse of infinitely long strings in GR and found a similar result. So now, imagine if a very long, but finite string collapsed to a long, thin needle. You couldn't take a hoop that had the circumference of a black hole of the same mass as the string and cover this collapsed string in all directions if it were sufficiently long. Do you see the problem? According to the hoop conjecture, a black hole would not then form and the singularity would not be clothed by an event horizon.

So something had to give, motivating us to set up and simulate a long string of collisionless particles that underwent collapse. The results indicated, insofar as we could determine, an infinite, or very large, tidal field arose, but no black hole. Now, the “no black hole” conclusion came with caveats. A black hole could, in principle, ultimately form at very late times. In fact, GR requires that you reserve your final judgment about the appearance of a black hole until time reaches infinity. Did or did not a black hole eventually form? Well, we can't integrate to infinite times with our computers. We must stop them at some point. And with some gauge choices the appearance of a black hole can be postponed almost indefinitely. Moreover, at some point, even with our most optimal gauge conditions, the integrations develop some numerical error. We understood all of this. However, as we integrated longer and longer with more and more computer resources to increase the accuracy we still didn't see a black hole. But we could not make that final assessment rigorous. But it looked awfully suspicious.

So we published a paper that argued that the cosmic censorship conjecture might be in trouble. Now, this was a rather peculiar and artificial scenario, a long string of collisionless particles. Where would that arise in our universe? But nevertheless, it was a point of principle. Now, since that publication, several decades later, there's been another set of calculations with a more advanced code on a faster computer that suggests that a black hole does indeed form. They've been able to investigate the problem with higher resolution and with different gauge conditions, and they find evidence of an horizon. I still have had some questions about that: was the string sufficiently long? If a black hole does form, what does it say about the hoop conjecture? Etc, etc. I may go to my grave asking some questions about that. But I think the consensus is that cosmic censorship is salvaged, even in this peculiar case.

**ZIERLER:** What was Penrose's response to this paper?

**SHAPIRO:** That's very interesting. I know you've talked to Saul, and he probably told you. Penrose came to Cornell to give a talk, and I think it was on Penrose tiles. (Speak about breadth, Penrose is an amazing guy, a Nobel Laureate now. ) We arranged to meet with him in his visitor’s office. And I must tell you, in anticipation of that meeting, I was more nervous than I've ever been interacting with anybody. Because here I was with Saul, challenging cosmic censorship while confronting one of the brightest minds of the century! But to my amazement and surprise, he was most welcoming, and supportive of our effort. He suggested that if the simulations were accurate even if cosmic censorship were still true, they would have other consequences because of the visibility of the singularity inside certain regions of the black hole that heretofore he didn't think was possible. So it was a delightful conversation. Now he never said, "Well, that's it, cosmic censorship does not hold". No. Nor did we. But I think he was most supportive of our line of research, realizing that numerical relativity was probably an avenue that one had to attack this problem until one came up with better means to prove a theorem.

**ZIERLER:** What you're saying is that Penrose cared more about the science than his ego.

**SHAPIRO:** Absolutely. And to this day, whenever I'm challenged by somebody, whether it's pleasant or unpleasant, I always remind myself, it can't be more challenging than confronting Roger Penrose. I met that challenge, I can meet any challenge. Because his intellect, I think, surpasses almost anyone that I've ever met. And the fact that it could be a fruitful exchange was a high point of my career.

**ZIERLER:** What were the circumstances surrounding your decision to move to Illinois?

**SHAPIRO:** Well, it was an unpleasant one. The administration and I had disagreements, strong disagreements.

**ZIERLER:** At Cornell.

**SHAPIRO:** At Cornell. Many years earlier, I had been courted by University of Illinois. They made me a rather nice offer in the 1980s and were trying to recruit me, as were a few other places. And rather than deal with this conflict at Cornell, I decided to leave.

**ZIERLER:** Now, were you in an administrative capacity at Cornell?

**SHAPIRO:** No, but it had to do with administering a small fraction of my research funding and things of that nature. I always believed that I could do good science working either at a university, in my home attic, or in the back of a pickup truck, if I had to. I wanted to insure that my work be uninterrupted and that was my prime concern. So there was a parting of ways. My wife, who was head of a department at Cornell, was chosen to be head of a department at Illinois in advance of our arrival. In fact, she became the first female head of a department in the School of Agriculture, which happened to be where her department of human and community development was located. She's a social worker by training. By the way, without her support over the decades while leading her two departments, publishing numerous articles and several books, tending to our two children in sickness and in health, and putting dinners on the table, I could not have accomplished very much at all.

So there was a parting of ways, by which time I had published almost 200 papers while at Cornell, somewhat less than half of all my publications to date. I also wrote a well-received textbook with Saul while at Cornell and we edited another. Since that time, I've written two more textbooks, both with Thomas Baumgarte, who came with me from Cornell to Illinois as a postdoc. And the BSSN formalism came out of my Illinois era. So I've had a rich experience at both places.

**ZIERLER:** To go back to Larry Smarr and Illinois as a center of supercomputation, was that also attractive to you specifically?

**SHAPIRO:** Oh, sure. When the original offers were made to me by Illinois in the 1980s, Larry was not yet there. But when I made the transition in 1996, NCSA was already a center of numerical relativity and computational physics. Even before I arrived at Illinois officially, Larry appointed me a Senior Research Scientist at NCSA, which enabled me to funnel some of my existing federal research grants through NCSA and access NCSA supercomputers.

**ZIERLER:** What was attractive about NCSA? What did it offer you in terms of resources that you might not have had anywhere else?

**SHAPIRO:** Well, at the time, I had significant resources at Cornell because it, too, was an NSF national supercomputer center. But soon after I left, Cornell was eliminated from being designated as a national center. Only a few centers were left, and NCSA was one of them. And what it offered primarily were supercomputer resources. In addition to the national competition for these resources, you had certain privileges by being a local member. In addition, and this was very important, it had powerful visualization tools. The kind of work we do where we collide neutron stars and black holes in three dimensions, and we generate reams of numbers as output, the only way to properly assess what's going on, and sometimes the only way to get your first glimpse at a qualitative phenomenon that you might not otherwise have anticipated, is to see a visualization of that simulation. So you needed high speed graphics and sophisticated visualization software templates around which to build your own visualization software in order to watch these interactions take place. Now we bought some rather primitive hardware when I was at Cornell. But NCSA had an entire visualization lab that was available to me. And it assisted my building one of the things I am most proud of, and that's my undergraduate research team. This team helps us develop our own visualization tools that we use every day todiagnose our supercomputer simulations.

Already in the early 1980s at Cornell, I assembled my first undergraduate research team. These were undergrads were finishing sophomores who were not yet far enough along in their studies to do general relativity, but could be involved in our research by helping us visualize our computational output. The way I put it to them is that they could see the solutions to the general relativity equations before they even knew what those equations look like. Subsequently , as advanced juniors or seniors, they could see those equations by taking my GR course and learn how they are derived. But for starters, I wanted to have them help us by constructing snapshots and then movies of our collisions, i.e., our colliding black holes, our colliding neutron stars, and our colliding star clusters. Ever since that period, I have maintained a group of between fix and six undergraduates who help us with our visualization software and get an early training in GR.

One of my most recent undergraduate teams helped Baumgarte and me proofread and revise our second textbook, “Numerical Relativity: Starting from Scratch”, which was geared to students with their physics and mathematics background. I discussed the book with the team one day a week during a semester, and we went through all the chapters and the problems. The students were very helpful. Basically, I was able to introduce many young people as undergraduates to the fields of numerical relativity, relativistic astrophysics, and GR even before they would start learning about these topics formally in the classroom. I'm particularly proud of that because there are now well over a couple dozen well-regarded faculty members in physics and other science areas at institutions around the country who began their research careers as members of my undergraduate research team.

**ZIERLER:** Now, did you get more heavily involved in neutrino astrophysics when you got to Illinois?

**SHAPIRO:** Yes, I continued my involvement, but there also were other projects I did in neutrino astrophysics while at Cornell that we have not talked about. One was with my graduate student Paul Schinder, where we built what was we called a “neutrino atmosphere” on top of a nascent neutron star. We know that you can build an electromagnetic photon atmosphere on top of planets and normal stars, and those atmosphere calculations tell you how the photons propagate through the atmosphere and what we see, i.e., the electromagnetic spectrum, from these objects. This constitutes a very important component of stellar and planetary structure. Well, we know that neutron stars are prodigious producers of neutrinos, particularly when they are newborn. When they're newborn, that's their prime emission mechanism, neutrinos. So young neutron stars, and also neutron star remnants that form when neutron stars merge, are prodigious sources of neutrinos. So Paul Schinder built the neutrino analogue of a photon atmosphere. With another graduate student, Rob Duncan, who later was famous for inventing “magnetars” and Ira Wasserman, another Cornell astrophysicist, I worked on a neutrino project involving neutrino winds blowing off newborn neutron stars. What could a neutrino wind do to the surrounding material? What rate of matter would be blown off with the neutrinos? This is important when supernovae are formed and leave a hot neutron star remnant. Can the neutrino wind plus outflowing matter blow off the surrounding stellar mantel? What do those winds look like? So we worked on that. And then, with Cornell collaborators, I worked on another problem in the early universe with neutrinos that I hinted at earlier. Following my study of Big Bang nucleosynthesis, I was always asking questions about how to rock the boat. Well it was reported when I was at Cornell that neutrinos had been discovered to have nonzero mass. They had been thought of as massless for all those previous years and suddenly, they had a mass! Well, we all knew that if neutrinos didn't have mass, they had only one helicity (spin) state, whereas if they had mass, they had two bonafide spin states.

So if you wrote down their Fermi-Dirac thermal equilibrium distribution without mass, it would describe half as many neutrinos that you would have if they had mass. And I wondered, given all those miracles of timescales that were happening with Big Bang nucleosynthesis, would that sudden appearance of twice as many neutrinos ruin the existing calculations of Big Bang nucleosynthesis? Because after all, neutrinos comprised a prominent contributor to the mass-energy density in the early universe and that density determined the rate at which the university was expanding. And if I double that neutrino component, the universe would expand more slowly. And would that be a problem?

So we worked on that and learned soon that no, the distinction between left- and right-handed neutrinos would persist, and only left-handed neutrinos would be involved in these interactions. The other component would not play the damaging role that I was concerned about. So in a nutshell, there was a lot of neutrino work that I did as a side hobby when I was at Cornell. This, again, highlights the point that it pays to be a little broad, and it pays to have some strong physics underpinnings if you want to be able to absorb the latest and greatest news and incorporate it into your life and the mysteries you're trying to solve.

While at Illinois, neutrinos have played a significant role. They are certainly involved with our compact binary merger calculations. And as we speak, I have a graduate student who is now working with me to incorporate neutrino processes into our Illinois GRMHD code. Heretofore, we've focused on putting realistic nuclear equations of state into those codes and, for the first time, magnetic fields, arguing that all neutron stars have magnetic fields. What role do they play in a binary merger and in the merger aftermath? Which leads to a very interesting development. One of my more significant contributions involved deciphering the role of what we called “hypermassive neutron stars”. A hypermassive neutron star is a term we coined in a paper that I published with Baumgarte and Masaru Shibata, a well-known numerical relativist who was a postdoc of mine way back in 1999. It was in that paper that these relativistic configurations were first constructed numerically and then evolved and shown to be stable on dynamical timescales. Now one knows that a spherical, non-spinning neutron star has a maximum mass, which we now believe to be around 2.3-2.5 solar masses or so. Well, if you spin it up, it acquires centrifugal support and can support a higher mass. But if you spin it up rigidly, i.e. with a uniform angular velocity as in a solid body like the Earth, then the additional mass it can acquire is not much higher, typically only about 20% higher. The reason is that you can't endow a star with too much rigid rotation before the outer layer flies off. Models of these higher mass, rigidly rotating stars were originally constructed and dubbed “supramassive neutron stars” in a series of papers I wrote with Greg Cook and Teukolsky in the early 1990’s.

If, however, you allow for differential rotation in a star, that is, motion like the planets have, where the inner planets move with faster angular velocity than the outer planets, you can support a very significant amount of additional mass in a neutron star. In fact, you can even support several times the mass of a spherical neutron star for some idealized equations of state. These differentially rotating, very massive neutron stars we called “hypermassive neutron stars” and the name has stuck. We discussed their potential properties back in 1999 and in follow-up papers because we imagined that they could indeed form when binary neutron stars merged. In fact, they might even form when supernovae collapse to neutron stars, since there is no reason for the remnant to be rigidly rotating at birth. But here's the interesting thing: eventually, hypermassive stars can't remain in differential rotation forever because when two layers of matter move at different angular speed, they rub against each other, causing friction. Such friction is due to viscosity in gaseous fluids. And that friction will drive the star to rigid rotation in the core on viscous timescales, while pushing some of the outer layers further and further from the center. Total angular momentum is conserved, but the core becomes rigidly rotating. When this happens the star usually can no longer support its excess mass and will undergo catastrophic collapse to a spinning black hole, typically one immersed in an ambient disk of gas. Sometimes this collapse can be hastened by gravitational wave emission, which occurs whenever the hypermassive neutron stars form with an nonaxisymmetric shape. This is a scenario that can happen when binary neutron stars merge, which we have simulated.

Lo and behold, it may well be that we have recently witnessed the formation of a hypermassive neutron star for the first time! This may have occurred when LIGO/Virgo detected a merging binary neutron star for the first time in 2017. The reason is that, although these gravitational wave detectors could not record the final merger or its aftermath because the frequencies of the gravitational waves emitted during this late stage of the merger were too high to be detected by these interferometers, gamma-ray satellites could detect the gamma-ray burst. From our theoretical simulations we believe that the only way to generate a gamma-ray burst from a binary neutron star merger is when the merger remnant first forms a hypermassive neutron star and then collapses a little while later to a spinning black hole surrounded by a disk of gaseous debris. Such a hypermassive remnant, which can survive for many rotation periods before collapsing, may be crucial to allow enough time for the gas to wind up and amplify the magnetic field. Such a field ultimately forms a funnel above the poles of the spinning black hole through which a relativistic jet of gas is launched, confined by the magnetic field and propagating outward, triggering a gamma-ray burst.

So relativistic configurations that we constructed and probed a few decades ago may have been detected indirectly recently. We don't know for sure, but probably in my lifetime we will get a better confirmation, because with future instruments like the Einstein Telescope in Europe and the Cosmic Explorer in the US, which will measure gravitational waves at higher frequency, the actual birth and subsequent collapse of a hypermassive neutron star might be detected and we may be able to confirm that they do form nature and behave as we have described.

**ZIERLER:** And best case, what's the time scale on this?

**SHAPIRO:** That's a good question. I don't think I can answer that. I think the next telescope that's in the official pipeline is the LISA interferometer, 2034. I don't think the Einstein Telescope has reached that level of effort as yet, let alone funding support, but it has been endorsed by European agencies. But on the other hand, I've always argued that with each exciting, novel LIGO/Virgo discovery, those startup dates get moved up because more effort and funding is put into the development of the next instrument. So maybe it's ten years. Ten years is something we used to say about LIGO ever since the early 1980s. Every year, we'd say, "in ten years." And of course it took longer. But it did happen. And I'm pretty sure, now, with these discoveries that LIGO/Virgo has made, it too will happen with these newer instruments. I think it's a priority for the community. I guess I could say, looking back, I'm fortunate that I've lived in the era where black holes became commonplace. First they arose mathematically, then understood theoretically, and now even detected. The same is true for gravitational waves. I've lived through the pioneering era of gravitational waves. And with them one of my own fields has moved to the forefront, numerical relativity. I'm grateful for that.

**ZIERLER:** On that point, with Baumgarte, what were your motivations in writing the book, *Numerical Relativity*? Did the field need a standard text? Were you looking to update something from earlier? What were some of the considerations there?

**SHAPIRO:** There are two books with Baumgarte, and they had different motivations. The first book was written for people who basically had a GR background, a theoretical background, and wanted to embark on numerical relativity, but were stymied because of the fact that there really were not any soup-to-nuts books from which to learn the craft. There were a couple of other books, but they were very mathematical. Their focus was on proving, in rigorous terms, some of the properties of the formulations of GR that had since come out, like BSSN and others, and were not so tightly tied to the most recent simulations. I had in my back pocket a huge number of actual simulations that had been done over several decades of work to go with our new formulation.

Also, our book came after these first binary merger calculations of 2006 came out, and with LIGO on the horizon, it looked like a perfect time to write a book. There was a need to teach people how to do numerical stuff, there was a formulation that was working and simulating binary mergers, there was a lack of a pedagogical textbook (although there were numerous papers that you could assemble) and there was LIGO on the horizon. All of that mandated a book. And so, we set out to write it. Also, Thomas and I like to write books to organize our own thinking from soup-to-nuts and clarify things. There's nothing better, and nothing more painful sometimes, than clarifying an idea or correcting a misconception, than having to write about it for all time. And that's what happens when you write a book. Now, that was the 2010 book. And as I said, its sales are now moving up because of LIGO/Virgo.

But the problem with that is, we noticed, in the last few years, there are lots of people that really don't have the time or the training to go into general relativity in great depth. They may already be post-docs. They may already be graduate students working on a related project. Or astrophysicists in another area. But they want to start solving problems, and they need the tool of numerical relativity. They need to run these codes, one of which is ours. There are several open source codes, like the Einstein Toolkit, which anyone can pull off the shelf and run. But most people who would pull them off the shelf would have no clue as to what's inside. So what we wanted to do, as we said in our introduction, was to “open up the hood” so that people can peer inside and have an idea, without a formal training taking many years of work in computational physics and general relativity theory, of what's going on in these codes, so they can work with them. We wanted to answer the key question: what is numerical relativity all about? So we just published a book that came out this month. It is called *Numerical Relativity: Starting from Scratch*, where we don't assume GR, but we do assume that a reader is familiar with undergraduate E&M, special relativity and classical mechanics, but not necessarily GR.

We take our readers from where they are to a familiarity with at least the underpinnings and basics of numerical relativity. And the way we do it is to remind them of things they're familiar with, like the equations of Newtonian gravitation, then making them special relativistic via scalar waves, then moving on to electromagnetism. They know electromagnetism, as they've taken undergraduate courses. What we do is to build on top of these simpler and more familiar field theories, i.e., scalar wave theory and electromagnetism, the new mathematical machinery that's useful in numerical relativity. We don't derive every equation in the book, and we don't cross every “t” and dot every “i”. But by the time people are done, they know what the basic GR equations are that are solved in these numerical relativity codes that they can pull off the shelf and run. Plus, we provide them with a few small codes and plotting routines that they can run to solve a few simple problems. And they can display them on their laptop screens. And so, it's a real pedagogical tool earmarked for students at a more elementary level of training and background. And we thought that's necessary because to go through our first textbook, requiring that one surmount the potential barrier of learning GR first, to finally tackling numerical relativity, may take a considerable effort. Many nonexperts cannot afford to make that effort before they want to tackle problems requiring the tool of numerical relativity. So maybe this second book will be a help for them.

**ZIERLER:** Bringing our conversation closer to the present, given your long theoretical work in both of these areas, I wonder how you might compare and contrast your scientific reaction and also your emotional reaction to the detection of the gravitational wave from LIGO and the photograph of the black hole from the Event Horizon Telescope. How did each of these major moments in detection affect you as a scientist, as an individual working in these areas?

**SHAPIRO:** I was very familiar with the the Event Horizon Telescope detection because one of my Illinois colleagues is Charles Gammie, who was one of the key people responsible for the theoretical simulation of accretion onto a black hole and its observed luminosity, leading to the crescent-shaped image that was detected. How do I compare those two discoveries? Well, I always argued long before the Event Horizon Telescope was even on the books that the cleanest way to assess a black hole and its properties and to validate or invalidate general relativity was via a measurement of a gravitational, not an electromagnetic, wave. That assessment is based on the fact that photons, or electromagnetic waves, undergo lots of things after they're emitted, and they're not emitted directly from a black hole (they can't be!), but from gas around a black hole, and then they undergo lots of interactions with matter. Indeed, if it were not for matter interactions, the photons would not be produced in the first place and we wouldn't “see” a stationary black hole. So I always believed that the cleanest validation of general relativity and probe of a black hole are provided by the gravitational waves that they generate.

And so, indeed, when that first gravitational wave detection took place, a host of things came with it. The gravitational waveform was consistent with the binary “inspiral” waveforms that had been calculated assuming GR. The “ringdown” waveform, emitted after the black holes merged but while the remnant was settling down to a stationary Kerr black hole, was totally consistent with the ringdown waveform that had been calculated many years ago assuming GR. This was an extremely clean system. And we know black holes are very simple. They can only have three properties, mass, charge, and spin. And astrophysical black holes don't even have net charge, as they are discharged early on, we think, by even the smallest amount of ambient plasma. So these are simple objects in vacuum spacetime, with very strong gravitational fields -- the best thing we could ask for to test general relativity-- and this was all accomplished by the gravitational wave detection.

Now, the Event Horizon Telescope certainly satisfied a longstanding desire that we all have to get close to a black hole and see it with “our own eyes”. We all have that wish that humans could wander close and really “see” a black hole and identify its event horizon without being harmed. And this observation essentially satisfied that desire. The magnetized accretion and photon emission calculations are very sophisticated. They're not terribly dissimilar from the calculations we have done in the context of binary mergers, although our black holes are in a dynamical state. The Event Horizon Telescope observed black holes that were assumed to be in a final, stationary (Kerr) state, and the gas around it was assumed to be nongravitating (but magnetized). So these stationary black holes are simpler to deal with on the one hand, but the level of sophistication of the magnetohydrodynamics and radiation transport that was implemented was at a higher level than what we currently employ typically. I thought this was a beautiful result.

Had the EHT result come first, before gravitational waves, I think I still would have been saying, "Let's wait to see what the gravitational waves are telling us about these strong vacuum gravitational fields in their purest state, because I'm not quite sure what those photons are doing, how they're interacting. Do we have it right? Have we forgotten a process by which the photons interact with matter? Do we have our magnetic field behavior correct? After, all they're turbulent magnetic fields. Turbulence is an ongoing field of both numerical and theoretical interest. It is very difficult to simulate. Are we absolutely sure we got that right?" But the fact that the comparison of theory and observation fit together nicely is a strong point. And I welcome it as a great milestone. I just heard a talk recently about forthcoming measurements that are going to be made, both from EHT and its successors, and they have me very excited. So I guess the bottom line is, again, I'm glad I lived to see that. I'm glad my mind is still active enough to appreciate it, and that a nearby colleague was involved. So I share in that thrill and excitement.

**ZIERLER:** What did it feel like to be awarded the Hans Bethe Prize of the American Physical Society particularly in light of the circumstances under which you left Cornell?

**SHAPIRO:** Well, I won't hide the fact that it was satisfying on a couple of levels. Certainly, scientifically, it was unexpected. There was an email, which I could very well have purged because I get lots of email, and especially email from organizations that I may respect but don't have time to look at. So it was a total surprise. I interacted with Hans while at Cornell. The most interesting interaction occurred at the time Saul and I had written our first draft of our book on the physics of compact objects. There were a couple of chapters which heavily involved things that Hans Bethe had done with nuclear equations of state and neutron stars. We were passing out chapters to various people and colleagues to look at. We wanted their opinions and their suggested revisions before we published. And we gave it to Hans, who was well on in his years and retired, but still came into work every day and was still very active in many ways. So both for our interest and as a courtesy, we gave a couple chapters to Hans to read.

I didn't really expect a detailed reply. I was hoping for something positive, of course, but Hans was very busy. Saul, I think, was on sabbatical when I got a call from his secretary. She said "Hans looked over your material and thought he would chat with you briefly about it. Can you come by his office?" I immediately replied, "Absolutely," So I went to his office, expecting some light commentary and hopefully something positive, as there definitely was a need for that kind of book at that time. There really were no textbooks that were useful at tying together these different strands on black holes, neutron stars and white dwarfs and their observational status. So what did I get? Hans pulled out a ream of notebook pages (which I still proudly possess!), on which he made comments as he went through our chapters line by line, page by page, remarking on each paragraph on these pieces of paper. Sometimes, the lines would be, "Great derivation. This is nice." Other times the lines would be, “You would find a better reference here [ref],which gives an improved model”. And what interested me was two-fold: First, this preeminent Nobel laureate had taken hours to go through our book in more detail than anybody else had. He's the world's expert in this. This is unbelievably terrific! Second, I was amazed that at this stage in his career, and this would be about 1982, well after he had retired, he still cared that a lot of those references to which he turned my attention were to his own papers. He retained a sharp memory of his past work and was still very keen that it be put into the latest textbooks and duly recorded, even though he must, by this time, have had hundreds of papers, and dozens of books citing his work, plus a Nobel Prize and all kinds of other prizes. He still cared the same way that a beginning graduate student or young faculty member like myself at that time would care about such things. That was great! Now, viewing this from my present vantage point, I totally understand that. [laugh] What's the big deal? But at that time, it was a revelation. So I still have those notes, and at the beginning of my talk on receiving the Hans Bethe prize, I showed excerpts from his notes to us and highlighted a few of them that were particularly interesting. So yes, it was a great thrill to get that award. A surprise and a thrill!

**ZIERLER:** Just to bring our conversation right up to the present, in the recent past two years, what have been some of the most important issues you've been focused on?

**SHAPIRO:** Well, in light of the fact that we're now seeing, for the first time, things that I've worked on for the past 20 and 30 years, we are expanding and deepening our understanding of events like black hole-neutron star mergers, of which there are now a couple of strong candidates. We're trying to understand neutron star mergers better. In both cases we have been able to simulate for the first time the launching of bonafide relativistic jets from the poles of the spinning black hole remnants. Such jets are crucial for the generation of gamma-ray bursts from such mergers. As I told you, we're incorporating neutrino transport into our big supercomputer simulations, which already have magnetic fields, modern nuclear equations of state, and other elements that make them physically more realistic. In other words, now that you are starting to get real data, you're obliged to compare with theory. And to do that, you've got to deepen your theory. I've also been looking at supermassive star collapse. That's because we still have that mystery, "Where did these supermassive black holes that inhabit the centers of all galaxies come from?" And some people think, as I mentioned, the process starts with the collapse of massive--and perhaps supermassive--stars.

Also, I'm looking at dark matter and some dynamical problems associated with that, some of which I summarized earlier. I formed of an informal team consisting of a high-energy physicist, Jesse Shelton, a cosmologist, Brian Fields, and myself, a relativist, to look at the gamma-rays that have been detected coming from the center of our Galaxy. Could they be from dark matter annihilations at the center of our Galaxy? Which begs the question, what happens to dark matter in the vicinity of Sagittarius A\*, the four-million solar mass black hole sitting at the center of our Galaxy? We've recently published papers addressing that question. Dark matter and its dynamical effects on stars and how it's affected by black holes is a hot topic that I'm working on. Also, I just finished my second textbook with Thomas that I described earlier. That certainly took up lots of time.

I think binary compact star mergers, supermassive star collapse, and dark matter dynamics encapsulates where most of my current interests reside. The most recent published paper I co-authored had to do with the suggestion that one way to probe detect dark matter is if it accumulates at the center of a star. Indeed, if it accumulates at the center of a neutron star, it might destroy the neutron star, which would contradict the fact that neutron stars exist. One dark matter candidate is a huge number of small black holes, perhaps a background of primordial “mini-black holes”. They must be bigger than 10^{15 }grams, so that they have not evaporated by now via Hawking radiation, but much smaller than the mass of the sun. But if all of these little black holes permeating our Galaxy found their way into the cores of neutron stars they might accrete the rest of the neutron star and destroy it, so that we wouldn't see neutron stars now. Which is not the case, as we observe radio pulsars (neutron stars) galore in our Galaxy.

So Thomas Baumgarte, his student and I employed his numerical relativity code and performed simulations of a tiny black hole residing at the center of a neutron star. This topic, I guess, caused me to reflect back on my earliest days doing spherical accretion onto black holes. But now, the accreting gas comprised a cold, massive neutron star outside the black hole, not a hot, thermal medium of infinite extent with negligible self-gravity, as in the classic Bondi problem. However, as a warm-up, we first solved the relativistic Bondi problem for a stiff, cold, equation of state, inserting the black hole in an infinite medium with negligible self-gravity. Then we inserted the black hole in the neutron star and found the maximum lifetime of the neutron star as a function of the black hole mass. By determining that, we could rule out a whole range of primordial black holes masses as viable dark matter candidates. This research was a product, in some sense, of Zoom and the Covid pandemic, because Thomas and I were on Zoom all the time, and we could interact almost daily. Prior to this strange time, Thomas would travel and spend a couple weeks at Illinois. As a former postdoc here, he's very familiar with the campus and still has a couple of other friends at Illinois. But Zoom made our collaborative work much easier.

Before we end, by the way, I do want to talk to you about one aspect that we haven't covered. And that's sports. I don't know if you're interested in that, but I'm passionate about a couple of sports. And anything that summarizes any period of my life has to mention that. For example, baseball has always been my passion, now and as a youngster.

**ZIERLER:** Now, in Connecticut, this could be either Boston or New York. You have a choice.

**SHAPIRO:** You bet. And I made all the wrong choices! I lived halfway between Yankee Stadium and Fenway Park. So I'm a Red Sox fan. Now, originally, I was a Brooklyn Dodger fan when I was 7 and 8 years old. I had a Brooklyn Dodger hat. And then, when the Dodgers left to go to California, I was irate. My family would spend a week at Cape Cod vacationing, and during that time, I began listening to Red Sox baseball. This was close to the last years of Ted Williams. Well, when we came home, I took my blue Dodger hat with a white “B” and I painted the “B” red. I now became an official Boston Red Sox fan. (When the filmmaker Ken Burns heard that story from me, he said if he had known it earlier, he would've put it in his baseball documentary!) But in any case, I became a Red Sox fan. But when I was a kid, there were no New England Patriots. The New York Giants were the only Connecticut NFL team at that time. I became a big Giants fan.

Well, it was the Yankees that went on to win World Series after World Series. And of course, we now know the Patriots have gone on to win many Super Bowls. But I've had the pleasure of watching the 2004 baseball season and the subsequent four World Series that the Red Sox won this century, and also the three Super Bowls the Giants have won. I'm a Knicks fan, which was another mistake that has turned out to be even more tragic. And although I'm not a big hockey fan, I'll root for the Rangers. So I made all the wrong choices in my career. But I want to tell you about one moment. My own career had its peak when I was 11 years old. I pitched my team to playoffs. That was a big moment. But that was probably its peak moment. The rest of it was summer camp softball, and backyard wiffle ball, and things like that, including Little League.

 But here's an anecdote that is really worth noting. You know that the home run record set by Babe Ruth in 1927 – 60 home runs – stood as a milestone for decades. And I considered that single season home run record to be the most prestigious record in all of sports.

**ZIERLER:** More than DiMaggio's hitting streak?

**SHAPIRO:** Yes, the streak certainly turns out to be more unrealizable, but I've always thought that the home run record is the most significant and entertaining, although one could debate that. But there it is. And you know that in 1961, Roger Maris broke that record.

**ZIERLER:** With an asterisk.

**SHAPIRO:** Right, with an asterisk. Well, I was at that game. Can you see this?

**ZIERLER:** Yeah. Stu is showing me the program from that game. Oh, the box score. Look at that.

**SHAPIRO:** Oh, I keep score at all games I go to. Like my daughter, but unlike my son. And there's the score card from that game. And I had all sorts of memorabilia, and I'll tell you about that in a minute. But I was at the game with my younger brother Paul and my dad because I knew breaking the record might be a possibility. And my dad, as a high school principal, was able to go to colleagues at sports supply stores in town and get us tickets. But that's only the beginning of the story! You know that the Maris record was broken by Mark McGuire in 1998, when he hit 70 home runs. I had already moved to Illinois by then. And here is the score card for the game in which that record was re-set. And that was in 1998. So I saw the record re-set, but that’s not the end of the story.

**ZIERLER:** Oh my gosh, you have Barry Bonds?

**SHAPIRO:** So in 2001, following the delay in MLB baseball because of 9/11, I got a call from my son. This was a time people weren't flying, you may remember. But the season had been postponed, reset, and the Dodger-Giants series, which had been scheduled earlier, was put to the last three games. I got a call from my son, who was a freshman at Johns Hopkins University at the time, who said that I had a “family obligation” to see what might happen in this final series regarding the home run record. And so, my wife and I flew to San Francisco. There were not many people on the plane. But here is the score card for that game.

**ZIERLER:** Holy smokes.

**SHAPIRO:** In fact, for McGuire, I saw the final three games. I saw him hit 67, 68, 69, and 70. I went for the weekend because I had no idea when his last at-bat would be. And for the Bonds series, I saw three games. So I saw him hit 71, 72, and 73. So the bottom line is, I have seen all of the record-setting home runs in person (!) since Babe Ruth's on three coasts spanning the period of four decades, from when I was 13 in 1961 to 2001. And I ask you, as a journalist, is there anyone else who can make that claim?

**ZIERLER:** There's no way. There's no way.

**SHAPIRO:** I have not found the answer to that. I've not looked hard enough, but I would love to know the answer.

**ZIERLER:** I'll put it like this. I would say that there's a greater likelihood that you'll be around to see the results of the Einstein Telescope than seeing another game where Bonds's record is broken. I'll put my money on that one.

**SHAPIRO:** Well, I'm just pleased to recount this high point of my career as a fan. Too bad a lot of the other memorabilla from the Maris game was washed away by the flood in my parents’ basement. Otherwise it was destined to be my ticket into the baseball Hall of Frame, which was collecting some memorabilla from that game. What a pity!

I'm a golfer now and have been for years. I'm currently about a ten handicap. I've been as low as seven. I have two holes in one. I'm a serious golfer. In other words, I'll go out in the middle of the week in good weather and hit golf balls. When I came to Illinois, I located myself within a couple blocks of the first tee of a golf club, at which I'm a member. Just this past weekend, with another astrophysicist, Gil Holder, the two of us teamed together to win the flight of our tournament, and it was a great event. So I just thought that since sports occupies a large part of my waking hours, that sort of had to be included.

**ZIERLER:** Highly relevant, and I wouldn't have known for myself.

**SHAPIRO:** [laugh]

**ZIERLER:** Stu, it's been a great pleasure spending this time with you. I'm so glad we were able to do this, and it's been a blast spending this time. I'm so glad we were able to get all of your thoughts on the record for history. Thank you so much.

**SHAPIRO:** Well, it's been a delight. You've been great, and you've stirred up memories.

**ZIERLER:** Mission accomplished.

[End]