Round Table Talk: Conversation with Robert Williams

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Okamura: Bob, as you remember, we had a similar talk event in Kyoto 20 years ago at the time of the IAU (International Astronomical Union) General Assembly in 1997, and it is my great pleasure to have another event today.

Williams: I agree, Sadanori. Let's do our best to make it interesting.

Okamura: Yes. You are an influential person in the astronomical community, and you have initiated many important projects, both as a world-leading astronomer and the director of very strong astronomical institutes.

Today, we would like to hear the stories and episodes of such projects from you in a way that only you can talk about it. This kind of talk would be very useful for both young people and old people, like me, who are interested in the details of your projects and their influence on your career.

Williams: I hope to take this opportunity, especially with Nao with us, to comment on several aspects of large projects that I have been associated with but have not previously discussed publicly, specifically, why and how two independent teams became involved in the discovery of Dark Energy, and how it was that two giants of astrophysics were opposed to the Hubble Deep Field project and tried to talk us out of undertaking that observation.

Okamura: Shall we start with the supernova in 1987, when you were the director of Cerro Tololo Inter-American Observatory (CTIO)?

SN1987A, the First Visible Supernova since 1604

Williams: Fine. I remember SN 1987A very well.

Okamura: To my knowledge, it was the first visible supernova after the period of 400 years ever since the Kepler's Supernova in 1604. So there must have been great scientific fever and also turmoil in the observatory in the southern hemisphere.

Williams: I remember the morning that I went to my office in late February 1987. I lived on the AURA (Association of Universities for Research in Astronomy) compound in La Serena and our house was only a 3-minute walk from my office.

Okamura: Three minutes?

Williams: Yes, it was ideal. No commute, and I could walk home for lunch. I loved it and since I'm a workaholic I spent most evenings there also. In any event, I received a surprise visit from Bill Kunkel who was a staff member of Carnegie Observatories at Las Campanas. He came in the front door of the Tololo building and I was one of the early people there in the office. Bill put his head in my door and said, "Bob, I want to talk to you about something very important, and I want you to sit down." "Why should I sit down?" I asked, to which Bill insisted, "Well, when I tell you what I'm about to tell you I think you'd better be seated." Bill informed me, "We found a supernova and it is not in a distant galaxy. We believe that we have a naked eye supernova that Ian Shelton found last night."

That got my attention. It turns out that the Las Campanas observer Shelton and night assistant Oscar Duhalde independently discovered the supernova. Shelton was taking photographic plates of the Large Magellanic Cloud on consecutive nights. The plate taken on 23 February did not turn out well so he took another plate the night of 24 February. While comparing the two plates he noticed that a new object, bright enough to be seen with the naked eye, appeared on the second night that had not been there on the plate of the previous night. On that night, both Shelton and Duhalde had walked outside to observe the object and noticed the new supernova. Bill was telling me the very morning of the night that it had been discovered. It was the first supernova in the Local Group of galaxies in 400 years.

It was obvious we had to study the supernova immediately. As soon as other staff members arrived at work I got them together and thought of two things. First, we needed to undertake a scientific investigation and get the light curve, i.e., do photometry, and get spectra. It was obviously bright, so a small telescope would do because it was visible to the naked eye. I also thought that there would be tremendous public attention paid to it and so we all agreed that we had to do something that would satisfy the desire of the press to have information about the object. We needed to take some nice photos of it.

Later that day a team of us went up to the observatory to set up for the night to actually get images of it and we obtained one of the first colored photographs of the supernova. In the nights that followed local public officials and reporters from major U.S. magazines and papers came up to observe the object. I especially remember the Intendente (regional governor), a personal appointee of Gen. Pinochet.

Okamura: Military President Pinochet? He is known as a dictator.

Williams: Yes, the supernova occurred during the oppressive Pinochet regime, so all of the regional governors and officials were military personnel. I met with them often in order to maintain cordial diplomatic relations, and the Intendente came up on one of the very first nights to look through the telescope to see the bright star with his own eyes. I also recall the visit of Malcolm Browne, the science reporter from the New York Times, who spent several days with us in Chile and wrote a series of articles in the NY Times on 1987A. Time magazine also covered the story. There were a lot of outreach efforts during this time, as we undertook a photometric and spectroscopic study of the supernova.

Okamura: Which telescope did you use for photometry and spectroscopy?

Williams: Spectroscopy was done with one of the intermediate telescopes which was not heavily over-subscribed, so it was easy to schedule. We had an instrument that Steve Heathcote had developed, and it was not on the large 4-meter telescope at that time, so we began obtaining regular spectra. We also did photometry to get an accurate light curve. 1987A was so bright that even with a 0.4-meter telescope, we had to put a mask on it with holes because the brightness of the supernova would saturate even with the shortest exposure. Eventually, when the supernova got fainter we removed the aperture mask and continued doing light curve photometry for many months, and also acquired regular spectra which we interpreted.

Okamura: Right. So how many clear nights followed consecutively after the discovery day?

Williams: February and March were good summer months that were largely clear so that we were not bothered by bad weather early on.

Okamura: I see.

Found the s-Process Elements in the SN1987A Spectrum

Williams: What was really interesting was trying to interpret the supernova spectrum because it showed some unusual lines. I remember devoting much of my research effort to trying to understand the spectrum. I consulted all the atomic physics reference manuals trying to identify three or four lines that did not appear in other supernovae. I'll never forget making the identification of elements from what we call the "s-process," i.e., slow neutron capture process, elements of barium and strontium. I spent hours and hours looking for alternative identifications.

I recall having a spectrum in the infrared described to me that really helped the identifications. My colleague Jay Elias reported to me that there were some features out just beyond one micron in wavelength, where ionized strontium (Sr II) was one of the possible IDs.

I had already made a tentative identification of Sr II for two of the optical lines but because that element had not previously been observed in supernovae I had some doubts about it. However, one of the other spectral lines I found seemed to be due to barium, which is another s-process element, and this gave me more confidence that the s-process had unexpectedly been a part of the evolution of 1987A. I distinctly remember working alone on the weekend—it was a Saturday, 4 April, the day that Pope Juan Pablo II flew into La Serena on a well-advertised trip to South America. On that Saturday I was in my office, but everyone else in La Serena, including my wife, was out at the small La Serena airport to welcome the Pope as he arrived.

I remember vividly hearing the Pope's jet fly low over the CTIO offices because we were only several miles from the airport, and hearing this loud noise from Pope Juan Pablo II's jet flying over my office just within a few minutes of the time that I finally concluded that those spectral lines I had been trying to identify were the s-process elements strontium and barium! I'll not forget that moment.

Okamura: It is really a drama that the identification came with Pope. Was it the first evidence that s-process took place in a massive star that later became a core collapse supernova? So, even to you, a specialist of line identification, it was very difficult to identify the lines of strontium and barium, wasn't it?

Williams: Most of the lines I was able to identify fairly straightforwardly but there were these few that had not been observed before in supernovae. The strontium and barium identifications were the most consistent IDs. Our spectra provided clear evidence that slow neutron capture occurred in this core collapse supernova prior to the outburst, establishing this paradigm for a massive evolved SN progenitor for the first time.

Supernova Research at the CTIO

Okamura: By the way, at that time CTIO hosted a strong group of supernova researchers?

Williams: SN 1987A provided the impetus. Historically there had been interest in supernova research carried out at CTIO and Cerro Calán, which is the Chilean....

Okamura: Cerro Calán—Calán is the name of the place? The Calán/Tololo supernova survey is well known.

Williams: Correct. Cerro Calán, the observatory of the University of Chile, has been the national observatory of Chile. They had several researchers, primarily José Maza, who were interested in following supernovae. Even before I arrived in Chile in 1985 the Chilean 'El Roble' supernova survey had been successful in discovering supernovae. There were not a large number of people involved in it at that time, but after 1987A with the increased interest in supernovae, more people became interested. In particular, some Tololo staff members, Mark Phillips, Nick Suntzeff, and Mario Hamuy, began devoting much of their research time to 1987A and to other supernovae.

We hired Mario Hamuy at CTIO as a data reduction assistant just before the 1987A outburst. Mario had obtained an undergraduate degree in astronomy in Chile but he was not interested in continuing on with graduate studies at that time. Someone called his availability to my attention as a possible staff member so we interviewed him. I thought Mario would be an excellent hire because as the national observatory of the United States CTIO had the obligation to help all observers obtain and reduce their data. We needed someone like Mario who had data reduction experience. He was one of those people who was a great go-to person. By the way, I am pleased to say that Mario has just this year been awarded the Chile National Science Prize by President Bachelet.

After SN 1987A Mario, Mark Phillips, and Nick Suntzeff got together with José Maza and others to initiate a more intensive supernova program that would use CTIO telescopes to follow up study of SNe (supernovae) discovered. In 1989 the three of them began the new Calán/Tololo Survey to discover supernovae to understand the physics of the outbursts. Another of their motivations was to use them as standard candles for the determination of the Hubble Constant, and the results of this survey were important as lead-ins to the eventual campaign that led to the discovery of Dark Energy.

Okamura: I still remember very clearly the time when I read the paper by Phillips which reports the discovery of the linear relationship between the peak magnitude (M_{max}) of the Type Ia supernova and decline rate (Δm_{15}), later known as the "Phillips relation."[*1]
A brighter supernova declines more slowly. It became the key for the Type Ia Cosmology to calibrate the absolute peak magnitude of the Type Ia supernovae and greatly reduce its

[*1] See page 34.

dispersion.

Williams: Exactly. He did that in 1993 just at the time that I was about to leave Tololo to go up to Space Telescope Science Institute (STScI). I consider Mark's paper to be a seminal contribution to the cosmological distance scale.

Okamura: Yes, I would say that the paper laid the foundation of the future trend of using type Ia supernovae as a precise standard candle, although use of other methods was also investigated.

Williams: Without it, we would not have cosmic acceleration.

Okamura: Exactly. Supernovae research at CTIO led to cosmic acceleration.

Williams: Without it, we would still have a much less certain Hubble constant and possibly not yet evidence for cosmic acceleration. I agree that was a really fundamental work that Mark undertook and the origins of it were trying to understand 1987A, which was a different type of supernova, i.e., core collapse. But that was what got Mark really interested in supernovae, trying to understand exactly what the luminosities of the different kinds of supernovae were at maximum light.

Okamura: Okay. Let's move on to the Hubble Space Telescope.

Suzuki: Can I just have one question before? A different team, Berkeley team also collected supernovae data. At CTIO you collected data on various types of supernovae.

Williams: I'm not sure to what extent that is correct. It is certainly true that both Saul Perlmutter and Bob Kirshner used CTIO telescopes to gather data on supernovae. But it was Kirshner who was largely interested in the type II supernovae, trying to understand the physics of the outburst. I recall Saul at that time had a broader interest in using supernovae for distance measurements, so I believe he did have an early interest in SNe Ia. That said, the Supernova Cosmology Project which Saul eventually formed and led had not yet been formed at the time of 1987A.

So, yes, there were people at Berkeley who were interested in studying the supernova, but that was before the general interest in trying to determine the deceleration parameter, which came later. I would say that 1987A created an interest in discovering more supernovae and an appreciation that many of those would be type Ia's. That's when people started focusing more on the type Ia's and their use as standard candles, i.e., as distance indicators.

Suzuki: So, was 1987A the inspiration to collect light curves?

SN1987A Gave a Chance to Advance Nova Spectroscopy

Williams: It did motivate obtaining light curves. There is a chain of events here and 1987A did move this theme along. In fact, it was 1987A that started my own long-term interest in novae spectroscopy.

Suzuki: Oh, this is an interesting connection.

Williams: Following the 1987A outburst I spent much of my time trying to interpret its spectrum. Because we could only observe 1987A for ~5 hours each night I questioned what were we going to do with the telescopes those few hours in the night when 1987A wasn't in an ideal position in the sky for observations. Why not obtain data on novae? Several others of the CTIO science staff agreed and that started the program that led to our defining paper in 1990 on the classification of novae spectra.

Amateur astronomers discover most of the brighter novae visible and after discoveries were announced we would get spectra, which change quite rapidly. I became fascinated with the nova process and also the fact that everyone else was studying supernovae. I thought significant contributions could be made if a few people looked at novae, so we had all of these data that few other people were interested in. We ended up with a great collection of novae spectra which we made full use of.

I loved having all of these novae spectra to work with. In some way, when spectra are involved I admit to being a control freak. I worked a lot with Mario Hamuy, Steve Heathcote, and Mark Phillips on the data interpretation. We succeeded in putting out the major paper in 1990 that defined the novae classification system. Over a period of several years time we took data for about 15 novae and proposed an evolutionary paradigm. This never would have happened without 1987A because the novae program was ancillary to CTIO's extensive work on observing that supernova.

Okamura: Novae! Diversity and time evolution of novae spectra must have fascinated a competent spectroscopist. Another world came to you with 1987A. You are very fortunate. Okay. Let's move on to the Hubble Space Telescope (HST) because Nao Suzuki is with us. You do know that he is a member of the SCP (Supernova Cosmology Project) team, and so he would like to ask you some things about observations with HST on supernovae as a team member.

Williams: I can imagine. I am aware

that there are strong feelings about the fact that two teams pursued work on type Ia supernovae as distance indicators.
Suzuki: I think so.

Supernova Observations with the Hubble Space Telescope

Williams: Okay, let's talk about that. I would like to bring some facts to light about how and why that came about.
Suzuki: On the SCP side, it was reported by Gerson Goldhaber at the group meeting, on September 27, 1997, that the favored universe from our data is $\Omega_m = 0.3$ and $\Omega_\Lambda = 0.7$.
Williams: This was at a meeting of the SCP team? It demonstrates that one must publish to receive credit.
Suzuki: A month before, there was an IAU General Assembly Meeting in Kyoto. One of the highlights of that meeting was the result of the HST Key Project led by Wendy Freedman. She reported the latest measurement of the Hubble Constant which was inconsistent with the age of the universe for an Einstein-de Sitter Universe, namely, the matter dominated flat universe.
Williams: This is interesting in that Wendy was not a member of either supernovae team.
Suzuki: Right. The concordance model favored the lambda universe. The SCP team concluded that it must be an accelerating universe that we live in. They reported this at the AAS (American Astronomical Society) meeting in January 1998.
Okamura: I followed the SCP team from the published papers only. I remember having been a bit confused by the apparently sudden change of the conclusion in their papers.
Suzuki: Bob, when did you begin to believe in the lambda universe or accelerating universe?
Williams: It was later than that because I was concerned about the uncertain effects of dust on the brightness of the supernovae.

So, let me tell you my thinking and how it came about that two teams became involved in using SNe Ia for distances and deceleration. This was long before acceleration was suspected. Two teams had formed that wanted to extend the work of the Freedman/Mould HST Key Project on the Hubble constant, focusing particularly on the deceleration parameter. Following the first Hubble Telescope servicing mission in 1993 that corrected the telescope's spherical aberration, it was possible to get luminosities of distant supernovae more accurately because you could perform the subtraction of the background light of the galaxy much better. It was also in 1993 that Mark Phillips published his important work on refining the peak luminosities of SNe Ia. It was several years after this time that the Hi-z and SCP teams were formed, involving Brian Schmidt, Nick Suntzeff, Mark Phillips, Bob Kirshner, and Adam Riess for Hi-z, and Saul Perlmutter, Nino Panagia, and colleagues primarily on the West Coast and at Lawrence Berkeley Lab for the SCP. They began systematic programs on ground telescopes and with HST to study supernovae.
Suzuki: That's an early stage.
Okamura: Very early.
Williams: Yes, you can go back to the STScI Newsletter which is put out each quarter, and you will find listed all of the TAC (Telescope Allocation Committee) approved programs for that year. I have this information on my computer because several years ago someone called my attention to the book titled, ... what is it? A Four ....
Suzuki: *The 4 Percent Universe*.[*2]
Williams: Yes, *The 4 Percent Universe*. A colleague mentioned to me they thought some of the facts in the book were not accurate, and they asked me for my recollection of details recounted in the book. As a result, I wrote down my recollection of my involvement in what turned out to be the discovery of Dark

[*2] Richard Panek, *The 4 Percent Universe: Dark Matter, Dark Energy, and Race to Discover the Rest of Reality* (Houghton Mifflin Harcourt, Boston, New York, 2011).

Energy, and what I will tell you now is information that should be part of the historical record. Much of it, although not all, is publicly available information.

In HST Cycles 4 & 5 (1994-96) there were several programs devoted to supernovae research. Jeremy Mould and Wendy Freedman were leading the Hubble constant Key Project and Bob Kirshner was leading the SINS (Supernovae INtensive Studies) survey, whose interests at that time were focused more on Type II SNe and understanding the nature of SNe outbursts than on their use as distance indicators. I maintained a keen interest in these programs because the Director of Space Telescope Science Institute has the responsibility to give final approval to those programs recommended by the TAC process. Separate from this process I was also responsible for up to 10% of the observing time on HST to schedule Director's Discretionary Time (DD time) based upon my judgment of what important science results could come out of observations that were not necessarily considered as part of the TAC process.

In January 1996 Saul Perlmutter approached me at the San Antonio AAS meeting to discuss a proposal he wished to submit for DD time related to his work using distant SNe Ia to determine the distance scale and the deceleration parameter. I invited him to submit his proposal, realizing from our conversation that it was similar in goals and method to the proposal that Saul + co-Investigators had submitted to the cycle 6 TAC and which had not been approved by them for execution in the proposal competition. Saul did submit his DD proposal in early February, which I acknowledged.

Following each annual TAC meeting there are a large fraction of the observing proposals that are rejected because of the huge oversubscription of HST time. It became normal procedure for a number of these unapproved proposals to be immediately submitted to the Director for consideration for DD time. Saul's was one such proposal. During this period, I had established my own precedent of normally not approving TAC-rejected proposals for DD time. Rather, I preferred to save DD time for new initiatives, new discoveries, and time-critical observations. After reading the Perlmutter+ DD proposal, which I did think interesting, together with the TAC review comments in their evaluation I decided to not approve his proposal immediately, and I put the matter on the back burner in my thinking.

The situation changed several months later in May 1996 when the annual STScI May Symposium took place and caused me, and others, to become more excited about more extensive observations of SNe with HST. The symposium was devoted to the topic "The Extragalactic Distance Scale," and there were talks given by Wendy Freedman, Gustav Tamman, Bob Kirshner, Abi Saha, Marc Postman, and others on improved values of H_0 and the likelihood that HST observations could reveal the deceleration parameter, providing a determination of the mean density of the universe. A great deal of enthusiasm was generated for HST as a unique tool to be used for cosmological studies. I must admit to having been too short-sighted to not appreciate Saul's enthusiasm when he had talked to me in San Antonio.

By the end of the symposium I became convinced that HST should devote a significant effort to determining not just H_0 better, but also q_0, which is what Saul already had been advocating in his rejected cycle 6 and DD proposals. The Cycle 6 HST TAC had already met in November 1995 to recommend Hubble observations for the period July 96 - June 97, and only one program devoted to the cosmological distance scale was approved by the TAC for observations—a program headed by Mould to calibrate Cepheid variable stars in various nearby galaxies. The lack of additional programs was a disappointment in view of the excitement at the symposium.

I should say that Bob Kirshner and the SINS team also had an SNe proposal approved for Cycle 6 by the HST TAC. However, it was focused primarily on obtaining spectra of a few SNe especially in the UV as a means of understanding the physics of the different types of supernovae outbursts. Do realize, this occurred shortly after the large Hubble Deep Field (HDF) observational program, undertaken with a significant amount of DD time, had just been completed and made public, and had been very successful. So, my initial instinct was that the cosmological distance scale and its evolution in time was a similar important topic that could be attacked with HST, and DD time needed to be used to make progress immediately.

At this time, I made the decision to grant DD time to not just Perlmutter's SNe team, even though they had already approached me three months previously with a request for observing time, but also another significant team that was interested in this problem, the Hi-z team comprised of Brian Schmidt, Adam Riess, Nick Suntzeff, Mark Phillips, Bob Kirshner, and colleagues. This decision to not grant primacy to the first team, that of Saul and the SCP, to request DD time for the purpose of refining the galaxies velocity-distance relation caused much consternation and displeasure in northern California that remains to this day. They believe they deserved the right to explore the distance scale problem using SNe Ia without competition from another team. I understand and respect their concerns, but I can say that for me the rejection of Saul's proposal by the cycle 6 TAC together with their critical remarks about the proposal played an important role in my thinking that the involvement of an additional team in addressing more exact values of H_0 and q_0 was appropriate.

Suzuki: So, you are the man who initiated the controversial competition.

Offering DD Time to the Two Teams to Look at Distant SNe Ia

Williams: Yes, and I was encouraged to do this by my close colleague at the Institute, Nino Panagia, who was a member of Saul Perlmutter's SCP team and with whom I had spoken during the May symposium. HST was not planned from the Cycle 6 TAC process (which I had already approved!) to be looking at distant galaxies to determine the distance scale and its evolution for at least a year and a half. As Director, I was motivated to use DD time to get going on this problem, and its importance led me to involve several groups that had experience in addressing this topic. I unilaterally made the decision to offer DD time to two independent groups, which I would not have done had the Cycle 6 TAC recommended HST time for only one particular group.

Suzuki: Did you ask them to propose for HST time?

Williams: I did ask them after offering them DD time. The way in which this happened is as follows. At the May symposium, Saul Perlmutter, Allan Sandage, Brian Schmidt, and Adam Riess were not present. But Mark Phillips and Nick Suntzeff (members of the Hi-z team) and Nino Panagia (member of the SCP team) were there, and I'm very close with all of them from our having been colleagues together—with Mark and Nick at CTIO, and with Nino at STScI. At the end of the May symposium I arranged for a meeting with the three of them and Bob Kirshner (Hi-z team member) in my office, so we could discuss how to use HST to make progress on the distance scale and its evolution. During our discussion I suggested tentatively that I would be willing to consider the assignment of a moderate amount of DD time to both the SCP and Hi-z teams, of order 25 - 30 orbits. In this sense, Panek's assertion in *The Four Percent Universe*[*2] that only members of the Hi-z team were present at this meeting that initiated the search for the time dependence of H_0 is not correct. In fact, at that meeting SCP member Panagia strongly supported the idea of giving DD time to both groups.

Suzuki: Relatively small compared to today's standard.

Williams: Relatively small, absolutely. This was to initiate a cosmological supernova program. Amazingly to me, Kirshner was not very interested in using HST time for the problem! He felt that their ground-based observations would be adequate for good subtraction of the background galaxy light.

Suzuki: Really?!

Williams: Yes, in my office Bob insisted, "We do not need Hubble Space Telescope to do the galaxy subtraction. We have good enough data." I disagreed, "Look, this is the premier instrument available. If this is important science, use it." Well, after a few days discussion among themselves they changed their minds and agreed on the value of accepting an offer of HST DD time to try to determine q_o, separate from their previously approved Cycle 6 TAC SINS program. Soon after our meeting, I made the same offer of DD time to Saul's SCP team inasmuch as it was Saul who pioneered the important search technique for discovering new SNe with ground-based telescopes. As did the Hi-z team, Saul was happy to accept the DD time of 28 orbits that I offered to both teams, and both teams eventually submitted formal proposals for my approval.

Okamura: That's a story I have never heard so far.

Williams: It's the first time I have reported this information.

Suzuki and Okamura: Nobody knows.

Williams: That's correct. I know there are some hard feelings in Northern California about the fact that their efforts on the distance scale were joined by another group, but I don't look at it that way. Even SCP member Nino Panagia agreed that it would be wise to have two independent teams working on this problem. Had there been an approved Cycle 6 TAC evaluated program awarded to a team with that goal I would not have given DD time to another team during that Cycle. I may well have

encouraged another group to get interested in the problem for a future Cycle, but that situation did not present itself.
Suzuki: I'm impressed by your foresight because the Nobel Prize Committee chair told us that it was because of the two teams that they believed in the accelerating universe. In the past, the Nobel Committee members conducted analysis by themselves to confirm reported results and they checked if the experiment was legitimate. But for the accelerating universe, they didn't need to do so because the two independent teams got the same results.
Williams: It is an interesting story about how the two independent Dark Energy teams got going with Hubble Telescope observations, and it is appropriate for this to be public knowledge. Although there was competition between the SCP and Hi-z teams they did have a good professional relationship throughout the time they were working on the distance scale.
Okamura: Oh, that's your spirit, quite the same spirit as that shown when you decided the Hubble Deep Field.
Williams: Correct. The same philosophy, and involving public data.
Okamura: Okay. The Hubble Deep Field. Now, no one disagrees that it's a monumental work. However, I learned from the slides of your talk last week that this was not the case in the beginning. That story would also be very interesting.
Suzuki: Let me conclude dark energy first. When did you begin to believe the universe is accelerating? Myself, even after the SCP and Hi-z papers were published, I didn't believe it.
Williams: Yes, when WMAP produced the CMB peaks. That's when I really

believed it.
Suzuki: I see.
Williams: The BOOMERanG results were significant in starting to make me a believer, but just by themselves I worried about dust.
Okamura: I also started to believe it in 2003 after reading the paper of the WMAP First Year Observations.
Suzuki: For others, it was not a single moment but a long "AaaaaHaaa." But before I leave can I ask you one quick thing? We are commissioning Hyper Suprime-Cam supernova this November. We are beginning to find nice supernovae from Hyper Suprime-Cam on Subaru. What would you do with Hyper Suprime-Cam or Subaru?
Williams: I would search for more target galaxies in the z=1-2[*3] redshift range, which presents the greatest difference between the various cosmological models. Also, understanding how the physics of those objects differ based on their spectra should be studied. That's what I would focus on.
Okamura: I see. A spectroscopist can't stop looking at spectra! The Hyper Suprime-Cam should be quite useful to find suitable high-z targets for you.
Suzuki: We will need Keck, Gemini, VLT, Subaru and JWST to follow them.

The Hubble Deep Field

Okamura: Let me now turn to another subject. Observational cosmology and galaxies are not your main research field. How did the idea of the HDF come to you?
Williams: I've been interested in astronomy since I was a boy. The first job I had was delivering newspapers on my bicycle. When I got my first $25 from the newspaper route I bought a small refracting telescope. One of the first things I did was I took it out on a dark night to see what faint objects I could see, if I could see galaxies through it. Of course I could not see much in the Los Angeles suburbs with the small 2-inch telescope. But the fact is when an astronomer uses a telescope one of the things you want to try is to determine what distant objects you can detect. Fifty years later I had a larger telescope. One of the first things I thought should be done

[*3] z is the redshift, which is a measure of distance.

with Hubble Space Telescope is to try to see how far out in space we could see. Of course, given the breakthroughs that were occurring in cosmological studies there were genuinely more substantive reasons than that!

Okamura: I see. You became interested in astronomy so early. A $25 2-inch telescope was the mother of the HDF.

Williams: Even though that was not my primary research interest at the time, it seemed to me that since we had the premier instrument in the field we had to try to determine how successful HST would be in detecting high redshift galaxies. In the month of HST launch, April 1990, John Bahcall and his colleagues Raja Guhathakurta and Don Schneider wrote a comprehensive article in *Science*[*4] magazine on what discoveries could be expected from Hubble Space Telescope. In that article one of the predictions they made was that HST was "not likely to reveal a new population of galaxies." Their reasoning was solid; it was based on calculations and it made sense because of the cosmological effect that the surface brightness of distant objects decreases more rapidly than $1/(\text{distance})^2$, rather as $1/(1+z)^4$. Two of the powers of distance are the usual $1/(\text{distance})^2$. In addition, there is a stretching of the wavelength and time bands, so you have a one-over-distance-to-the-fourth relation that drops galaxy surface brightness extremely rapidly. Thus, Bahcall and colleagues understandably predicted that it would be difficult for the Hubble to make grand discoveries at large redshifts.

Okamura: This may have been a feeling shared by quite a few people in the community at that time. In their paper[*5] in 1995 which appeared just before the HDF, Chuck Steidel wrote, "Searches for galaxies at $z > 3$ have been spectacularly unsuccessful up to now, given the efforts devoted to the quest."

Williams: This all changed when Mark Dickinson, a postdoc colleague at STScI who had just gotten his Ph.D. and didn't yet have a permanent job, submitted a successful HST Cycle 3 proposal to study a rich cluster of galaxies, 3C 324, at redshift z=1.2. He was awarded 32 orbits in 1994, which at that time was by far the longest observation that had been performed on HST. The 3C 324 cluster has at its center a very strong radio source that ground-based photos had barely resolved. By contrast, Mark's HST image obtained in one passband was spectacular. It revealed in clear detail dozens of galaxies, almost all of which had very irregular morphology. It demonstrated in one powerful image HST's ability to image distant galaxies.

Each morning we had science coffee at the Institute and we talked science. Mark's image took center stage in our conversations for several weeks after he first showed it to us. It was very exciting to me and convinced me that we should undertake serious investigations of distant galaxies with HST. Using Director's Discretionary time was clearly the simplest way to move ahead immediately and also to ensure that the Institute could play a role in facilitating observations and data archiving.

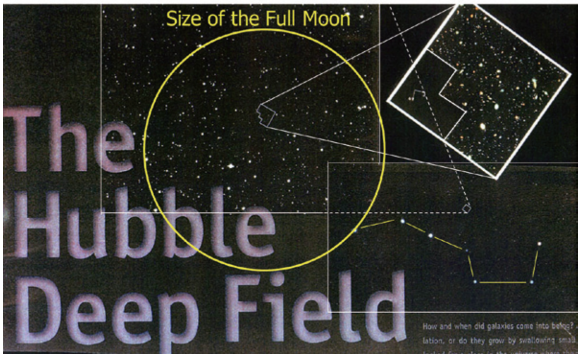
Initially I thought the thing to do was to issue a call for proposals to the community and see how the experts would respond. I convened an advisory committee of experienced researchers in extragalactic work, including Sandy Faber, Alan Dressler, Simon Lilly, Ken Kellerman, Richard Ellis, Len Cowie, Frazer Owen, and others, to ask them what they would do if they received Director's Discretionary Time. I was prepared to give most of my Director's Discretionary Time to the study of distant galaxies. I asked each committee member to speak for 15 minutes to suggest to me how they thought one could best use HST to study distant galaxies.

The questions the committee addressed were: What filters should be used? Should we go for a targeted or an untargeted field? That is, should we guarantee that we would detect galaxies in the image by looking at a known cluster of galaxies, or should we look at a blank field that would be more typical of the universe, but perhaps mostly empty? How many orbits should we use? What about the data; should it be made public? As Director's Discretionary Time I had the authority to waive the normal 1-year proprietary period that limits access only by the PI and co-Investigators.

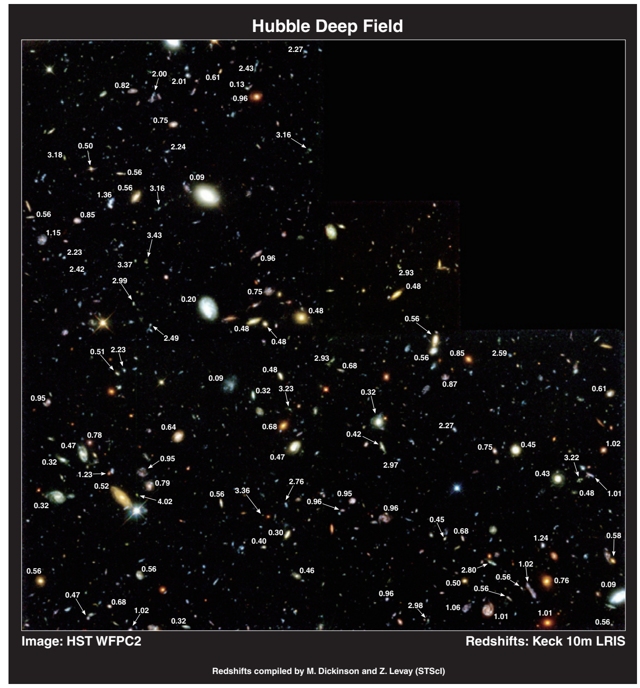
I was hoping for a committee consensus to emerge for all the above questions, but that did not happen. After debating the topic for a day there was no agreement on most of the major issues discussed. Half the committee thought we should go for a targeted field because at least we knew we would get something. The other half said, "No, that's a special case. Let's go for a blank field, i.e., no selection effects." The end result was there was no clear path that was laid out.

The next day I met with the

[*4] J.N. Bahcall, P. Guhathakurta, and D.P. Schneider, Science **248** (1990) 178.
[*5] C.C. Steidel, M. Pettini, and D. Hamilton, Astronomical Journal **110** (1995) 2519.



The Hubble Deep Field (HDF). The image at the upper central part shows about 30 arc minutes on the sky near the Big Dipper (Palomar Sky Survey). Superimposed on this image are the size of the full Moon and the location of the HDF. The upper right image is obtained from the HDF project. All these are shown against the background, which is a part of a poster produced by NASA. (Courtesy of Sadanori Okamura)



125 galaxies in the HDF image are given their respective values of redshift z measured by the Keck Telescope. (Image credit: M. Dickinson & Z. Levay)

young Institute scientists who were interested in distant galaxy research—Harry Ferguson, Mark Dickinson, Andy Fruchter, Mauro Giavalisco, and Marc Postman—and we discussed what to do. We debated the situation for several hours and jointly came to the realization that we could craft and conduct a program as well as anyone on behalf of the broader community. Plus, we understood the HST data better than most anyone and we could reduce it and make it available to the public. I therefore made the decision that we would undertake a large HST program ourselves. We would image one field and it would be a blank field. We would use several passbands because it could provide basic information about the mass distribution of stars, even though it meant a small loss of sensitivity. This had the added advantage of producing a colored photo for outreach purposes—not a small matter for an expensive telescope like HST with its troubled past. And, we would make the data public, but also provide a fully reduced dataset.

To determine the number of orbits for the program, which we named the Hubble Deep Field, we did some calculations that indicated that the signal-to-noise ratio of imaged faint objects would go from the linear regime to the square root of exposure time regime after 150 orbits, i.e., 10 days of continuous exposures. We therefore decided to image one deep area of sky with the primary HST camera for 10 consecutive days.

Early on we realized that it was important to have spectroscopic data to determine the redshifts, i.e., distances, of any galaxies imaged. We therefore took a two-orbit exposure of that particular field to give to Keck telescope, which had just begun operation as the only really large telescope in the world, and we asked Keck astronomers to start taking spectra of the brightest galaxies in the field that appeared in our 2-orbit image. Fortunately, astronomers from the three members of organizations that have access to Keck, University of California, University of Hawaii, and Caltech, agreed that they would start taking spectra of the brightest objects. This was 9 months before we actually began the HDF imaging with HST. By the time of the 10-day December 1995 HDF campaign they had already obtained 50 spectra, and eventually after 2 more years they had obtained 130 spectra of the HDF galaxies, which they posted on their own website.

Okamura: It's interesting. The agreement on taking the spectra with Keck telescope was made before the HDF campaign.

Williams: Chuck Steidel, Judy Cohen, Garth Illingworth, Len Cowie, David Sanders all contributed to getting the Keck spectra of the brightest HDF galaxies, and they made them public immediately, to their great credit. They had every right to keep the spectroscopic information proprietary for themselves, but they did not. These spectra gave huge value to what would otherwise have simply been a pretty picture.

Okamura: I see. Great!

Williams: The Keck spectra were used as a basis set to determine the photometric redshifts of all 2700 galaxies in the HDF, and I believe this is the single most important scientific result to come out of the Hubble Deep Field. The photometric redshift of any galaxy can now be determined with images, not spectra, taken with 5-6 passbands, which enables the distance to any galaxy to be determined. It is especially useful to have one passband in the infrared to help break the degeneracy of redshift values caused by hydrogen continuum emission. Currently, an image obtained in 6 or 7 passbands will yield a galaxy redshift having a dispersion of ~0.06, which is impressively small. Photometric redshifts have opened up the entire universe for study and interpretation.

Okamura: Yes, I agree with you. The photometric redshift was initially proposed in 1960s and a few practical applications appeared in 1980s. However, it is the HDF that made the technique, sometimes called 'poor-man's spectroscopy', a critical method indispensable to the study of distant galaxies. By the way, this amount of exposure time, 150 orbits, was unprecedentedly large for the HST. Probably the people who criticized the idea didn't imagine this long exposure.

Leading Astronomers Criticized the HDF Idea

Williams: They did not. If you look at the article in *SCIENCE*[*4] by Bahcall, Guhathakurta, and Schneider, they were considering a long exposure to be 1-2 orbits. In my mind Director's Discretionary Time is a specially useful tool that enables risky observations to be made that have a potentially great value if they succeed. This contrasts with the tendency of TAC committees to minimize risk in their evaluations and therefore downgrade proposals advocating uncertain returns.

I can tell you that my initial announcement of the 150 orbit HDF campaign to image a 'blank' field was met with support but also criticism in the community. Why use so much valuable telescope time to take an image that might contain few galaxies and nothing of interest when those orbits could support other TAC-approved programs that were likely to produce new results? I can tell you that both John Bahcall and Lyman Spitzer, the two great proponents of HST, were very much opposed to the idea of the HDF. John had already done studies that caused him to doubt that we would see many galaxies, and the HDF was to take place soon after the expensive First Servicing Mission. The public image of HST, which at that time was the most expensive science project in history (at US $3 billion), was terribly negative because of the spherical aberration fiasco. The reputation of HST and NASA had been disastrous and they were just recovering credibility.

John came down to the Institute from Princeton on two occasions to talk to me about his concerns. He strongly urged me not to go through with the HDF. We had very professional conversations. It was always a pleasure to talk with John, and he did have good reasons, saying, "Bob, I think this is not the time to do this and not the right way to go about it. There are too many orbits. You should wait to try something like this." I respectfully disagreed with him, "No, I think this really should be done. I am prepared to take full responsibility and fall on my sword if the end result is a failure." Lyman Spitzer also was worried about the HDF. He did not express his disagreement as overtly as John, but it was clear that the two of them had discussed things together. When Lyman attended the meetings of the Space Telescope Institute Council (STIC), of which he was a member, he would ask me during the open discussions, "Bob, are you sure you want to do this?" This was Lyman's style, indirect and diplomatic. He was trying to get his point across to me and to the important STIC. In spite of these understandable concerns, however, our HDF team and I never waivered from what we believed was a necessary observation to image the distant universe.

Okamura: Sometimes older people tend to be against new ideas. Do you think this was a factor?

Williams: I don't know that it was so much that as the fact that both John and Lyman had put an important part of their lives into Hubble Space Telescope. They wanted to make sure that this resource was preserved. There were never any hard feelings between us, but the strength of their opposition was never in doubt! It was not so much a matter of age as it was whether or not this was the time and opportunity to be a risk taker. They clearly did not feel that way about Hubble Space Telescope.

Okamura: To me, the decision of this unprecedentedly long exposure and the choice of four filters are the key elements of the great success of the Hubble Deep Field.

Williams: We would not have enabled photometric redshifts, obviously. The ultraviolet filter was sensitive to the hot star population and it led to the initial Madau plot of the star formation rate, which has been one of the fundamental results of the HDF.

Okamura: Yes. In the Madau plot, which shows the cosmic star formation rate as a function of redshift, two new points were added from the HDF in the frontier redshift range; one is at $<z>=2.75$ ($2<z<3.5$) and the other at $<z>=4$ ($3.5<z<4.5$) although both of them are lower limits. The nearer $<z>=2.75$ point came from 69 galaxies and $<z>=4$ point from 14 galaxies discovered in the HDF by the dropout technique.[*6]

Williams: By the dropouts, exactly. So that came out of it and these were very significant results. Subsequently, other results have followed, including the maps of the distribution of dark matter, gravitational lensing, cosmological acceleration, and black holes in galaxies.

Okamura: And also with the success of this HDF, it seems that the HST became as if a kind of survey telescope since then. A lot of deep surveys followed.

Williams: Yes, there was the COSMOS survey of 1 degree by 2 degrees. This mosaic of 7200 square arc minutes produced the very nice dark matter map. And then there were the Frontier Fields.

Okamura: The Frontier Fields, yes. It's another exciting campaign with spectacular images of rich cluster fields.

Williams: With the infrared passbands being used, there are now dropouts in the H band that enable redshift 7 galaxies to be detected. So there are some very distant galaxy candidates. And now with gravitational lensing the Frontier Fields have been able to detect candidates with redshifts z=10-11. Spectra have been taken of several of the candidates and based on the Lyman break, measured redshifts of z~11 are confirmed. HST is detecting galaxies very near the epoch of their formation.

Okamura: Yes, redshift of 11.2—that's recently confirmed and announced in March.[*7]

Williams: And there is another candidate at z=10.7, I believe it is. My colleague Dan Coe has a paper on several objects where spectra confirm redshifts around 10.

Okamura: It is the HDF that gave birth to all the success of these surveys.

Williams: Yet there are people who would say "Enough of the Deep Fields." A lot of orbits have been put to them and we should move on to other things. I understand that thinking, but I don't agree with it.

Status of the James Webb Space Telescope

Okamura: Okay. Time has passed too quickly and we have been talking more than an hour. Finally, could you tell me how is the present condition of the JWST (James Webb Space Telescope) and how secure is its launching schedule?

Williams: There were substantial cost overruns and delays in the project three years ago and there were some organizational problems. I think Goddard Space Flight Center agrees that the management structure was not optimal. The problem triggered a big review because substantially more money was needed to complete the project—far above the amount that NASA had originally projected. For the review NASA committed itself to making major changes in how it dealt with the contractors and the administrative structure within NASA that it was using. They convinced Congress that it was worth one final authorization of extra funds to complete the project, and Congress obliged. Since that time the project has been on schedule and within its budget. It has gone very well for the past 2-3 years and the launch date remains October 2018.

Okamura: Okay. The target is October 2018.

Williams: For several years it has not changed. Final integration and testing remain and at this point the detectors look good. But we all know that space is a risky business and particularly with pre-launch testing and launch itself things can go wrong that one does not anticipate. Because JWST will be stationed beyond the moon it will not be serviceable. It should yield tremendous returns on our understanding of the early universe and the nature of planets around other stellar systems.

Okamura: We really hope the successful launch of the JWST because we recently had a tragedy, as you know, of the Japanese X-ray satellite 'HITOMI'.

Williams: Very regrettable. We, too, have had some failures. As colleagues in science we are all in this together. We all hope to benefit from the discoveries that JWST will bring.

Okamura: Okay. Thank you very much. We have heard very very interesting stories today. We sincerely appreciate your taking time for this interview.

[*6] A method to estimate the redshift (or distance) of a galaxy from how it appears in several passband images. Ultraviolet (UV) radiation from distant galaxies at wavelengths shorter than the Lyman-alpha resonance line at 121.6 nm is absorbed by hydrogen atoms in the intergalactic space, and therefore it hardly arrives at the Earth, making the galaxies invisible (drop out) in the rest-frame UV images. As light from distant galaxies is stretched in wavelength due to the redshift, the local Lyman-alpha line wavelength shifts to visible or near infrared region, depending on the galaxy's distance. By looking at images taken in the several wavelength bands from shorter to longer, to see at which band a galaxy drops out, it is possible to roughly estimate the galaxy's redshift.

[*7] P.A. Oesch et al., Astrophysical Journal **819** (2016) 129.