Interview with Brian Schmidt

Interviewer: Melina Bersten

Brian Schmidt is Distinguished Professor at the Research School of Astronomy and Astrophysics at the Australian National University, formerly known as Mount Stromlo and Siding Spring Observatories. He, together with Saul Perlmutter and Adam Riess, received the 2011 Nobel Prize in Physics "for the discovery of the accelerating expansion of the Universe through observations of distant supernovae". He has also received many other distinguished awards. In particular, he shared the 2006 Shaw Prize in Astronomy with Saul Perlmutter and Adam Riess. He received his Ph.D. from Harvard University in 1993. He was a postdoctoral fellow at the Harvard-Smithsonian Center for Astrophysics from 1993 to 1994. He moved to the Mount Stromlo Observatory in 1995.

Interest in Supernova Study Dates Back to Undergraduate Days

Bersten: First of all, I'd like to congratulate you for the Nobel Prize.

Schmidt: Thank you very much.

Bersten: I can imagine you are very happy. I would like to go back to the origin of it all. How did you get interested in the study of supernova?

Schmidt: I was an undergraduate at the University of Arizona and there was a project run by John McGraw who was my undergraduate supervisor. (One of two. Tom Swihart was my other one.) I was working for him on his project. After about a year, I was getting to the point where I could do something useful. One of the things they wanted to do was to discover supernovae in their data set. It was the first digital survey. It was like the Sloan Digital Sky Survey where the sky transited across the detector and they moved the information on the detector along with the sky, drift scanning as it's called. I liked the idea of discovering something new in the data. So I started looking for that as part of my work for him as an undergraduate.

Now, I had not intended to do that for my Ph.D. thesis and it was when Bob Kirshner visited to give the first Marc Aaronson Memorial Lecture at University of Arizona when I was finishing my last year of studies there and I was trying to figure out where to go to graduate school. Would I go to Caltech, would I go to Santa Cruz, or would I go to Harvard? Those were my three choices at the end and I just could not decide.

In the end, Bob came and gave a very good talk on supernovae and I said, "Well, I hadn't really thought of doing supernovae for my Ph.D. thesis, but I do find them interesting and here is someone who I can work with at Harvard." I said to him, "If I can work with you on supernovae then I'll come to Harvard." That's how I got started.

Bersten: At the beginning, you studied Type II supernova.

Schmidt: As an undergrad, in Arizona, we were just looking for them. We didn't care what type they were. But, at Harvard for my Ph.D. thesis, I did study Type II supernovae. I measured distances with them in a way that Bob Kirshner did for his Ph.D. thesis. When I came to Harvard, Bob gave me a project to go and look at supernova 1987A. I said, "No, I don't want to do that. I want to do something else. I want to measure the Hubble constant and I want to do it using this technique that you developed back in your thesis." I was going to improve it. He had a postdoc who could model the supernovae very accurately and do it with a radiative transfer code and so I was going to work with Ron Eastman, and to improve the method to measure the Hubble constant. That's what I really did for my Ph.D. thesis.

Bersten: What was the main reason that made you change the focus into Type Ia supernova? In other words, what was the main reason that motivated you to form the High-Redshift Team?

Schmidt: When I finished my Ph.D. thesis, I'd measured the Hubble constant to a level where the systematic error in the method was about the same as the statistical error. I felt I had got my answer and there wasn't much else to do with that method. I was looking to do other things. I was looking at how the explosions of Type II supernovae occur by measuring what their nucleosynthetic output was, relative to their mass, and to their explosion energy.

In 1994, Mario Hamuy visited us and showed us the work that they had done in Chile, which essentially said you could use Type Ia supernovae to make measurements that were roughly two to three times better than the supernovae I had used. At the same time, Saul Perlmutter called us up and said we have an object to look at, which we took a spectrum of with the MMT–I was actually at Harvard; it was Bob Kirshner, Pete Challis, and Adam Riess. When they said, "We think it's a High-Redshift supernova," I went and analyzed it. I got the same answer.

Eventually, we found out from Saul Perlmutter that that was one of seven objects they had discovered. They had been looking for them since 1988 and suddenly they found seven of them. That was a real eye opener

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to me that you could discover them and we knew how to use them. I literally, when I found those things out, dropped everything that I was doing and said, "There is an opportunity to go and measure q_0, the deceleration parameter, right now." To me, that was just the big thing to do and it was the most exciting thing I could think of doing. It was measuring what's the ultimate fate of the universe. I dropped everything I was doing to go measure that.

Bersten: At that moment you were quite young. Was that a problem for being a leader of the program?

As a Young Leader, Organized the High-Redshift Team like Organizing a Party

Schmidt: I was the leader of the team but not in a traditional sense. I wasn't a general telling everyone what to do. I was the consensus leader of a bunch of people working together as friends. I had to organize people, but it was like organizing a party. You'd just tell people to show up, and they do. They do that because that's just the way things work. It wasn't like I was a general.

Was there a problem being 27? There were some issues. I didn't have the ability to go out and write a grant that could pay for all of our activities. Every group got its own bit of money and showed up and worked together that way. That inability to have a bunch of computer hardware that we could all use together was a problem. We got around it by working really, really hard. But that was part of the difficulty of having someone young like myself going and running things. The good part is that this was the one thing I was working on. And not just me, Adam Riess, Peter Garnavich, you know, we were young and we worked on this one thing. We worked on it night and day and that was the only thing we worked on. That was one of the reasons we were able to make progress–that we were not distracted by other things.

Bersten: Could you tell me a little about the methodology and how many telescopes you used–and also, identify which was the most difficult task during the program?

Schmidt: In 1994 when we started, we had these new CCDs, and I was down in Chile talking to Nick Suntzeff, who was one of the leads of the Calán/Tololo Survey that had figured out how to use Type Ia supernovae. We discussed using the CTIO 4-meter. CTIO 4-meter had a new 2k x 2k CCD, so, 4 million pixels. I knew that the weather at Cerro Tololo was pretty much cloud free from November through March. One of the problems that you have when you go out and try to find supernovae is that you need two images. You need a before image and an after image. If it's cloudy one of those days, then you're dead. The second day is useless. If it's cloudy at the second day, then the first day image just sits there.

We needed to have a place that was guaranteed to give us two clear nights. Chile, the Atacama Desert, was really the only place that gives that good of weather, guaranteed. Plus that new detector meant we could do it. And, of course, the team at Cerro Tololo means we knew the instrument and telescope very well. So that's where we decided to work. Saul's team had not yet realized the possibilities at Cerro Tololo at that point. So we applied, and after we made our first discovery (SN 1995K), they realized the natural advantages of working at CTIO, and presumably thought, "Geez, that's a good place to work. We should do that too."

The challenge of actually discovering supernovae was hard. The software I had written wasn't very good. It worked, but I had written it in Australia and got it working in Australia. When it was shipped to the computers in Chile, many of the libraries didn't work. They had different operating systems. Everything broke. It was a nightmare. Every time I would show up there, things would break differently because the computer system had changed. I think in the end that was the hardest part. Then, I'd go to Hawaii and the same thing. Things would break again. The effort that we would go through to discover these supernovae because we didn't have a home base we could ship them to–and just have stable software–that was a real, real nightmare.

The other problem in 1995 was that we didn't have access to the Keck 10 meter telescopes. Now in 1995, Bruno Leibundgut and Jason Spyromilio with the ESO NTT, in very good conditions, managed to get a spectrum of our supernova at a redshift of 0.48. They spent almost all night and it took them about a month to reduce the data and actually see that it was a Type Ia supernova. But that was an all-night affair in very good conditions–in reality the 4-meter telescopes were too small to get spectra of these high-redshift supernovae on a consistent basis.

In the meantime, Keck had come online and Saul Perlmutter's group had access to Keck and they completely killed us in 1995. They found all these objects at the end of 1995, and they were able to get spectra of all of them. We had a bunch of objects, but we couldn't get spectra of them. We ended up with two lousy spectra–where they ended up with like 11. I was very worried, "Oh my God, without Keck, we're dead." Alex Filippenko had asked to join our team early in 1995. Because the teams were competitive and he was at Berkeley I said, "No, you need to continue to work at

Berkeley—I wouldn't want to poach you." At the end of 1995, he called me up again and said, "I really want to work with your team—I am a supernova physicist like you guys." At that point I said, "Okay," because without Keck, we weren't going to be able to compete with Saul, and Alex had a lot to offer to our team in terms of his knowledge of supernova and their spectra.

Bersten: Did you need to be in Berkeley to have access to Keck?

Schmidt: Berkeley and Caltech had access to Keck, and all of the University of California schools. The only person who studied supernovae in the whole system was Alex Filippenko and, of course, Saul's group. For us, he was of the same culture that we were, and the fact that he came back and said, "I still want to come and join your team," saved us. Without Alex and his ability to get spectra, we weren't going to survive because we just couldn't compete.

Bersten: Which was your reaction when you found the acceleration of the universe? Did you immediately understand the implications of these results or…?

The Data Showed Accelerating Universe in 1997: Convinced in 2000

Schmidt: Yeah. We had known that the cosmological constant gave you acceleration. That was the primary thing that the cosmological constant did. That was taught to me in school. It was taught to me as, "Look at these people who messed up, look at these people who messed up, look at these people who messed up, they all had bad observations, and they thought it was the cosmological constant." I didn't really worry too much about it. It was sort of an ongoing joke within cosmology.

At the end of the 1997 when Adam sent me a figure that had the data clearly showing a cosmological constant, I just assumed that we had made a mistake. I didn't say, "Oh, a cosmological constant!" I said, "Ah, what have we done wrong?" We went through that bit-by-bit and it suddenly started sinking in, "No, this isn't going to go away." In the end when we had gone through everything several times, it didn't go away. Then, I was resigned to the fact, and I said, "Okay," then I got excited. We're going to have to tell the world about this. I was like, "No one is going to believe us."

At the same time, Saul Perlmutter's group had published a paper in 1997 saying the universe was decelerating. We were getting a crazy answer. He was getting something sensible. I didn't know what to make of it. Then suddenly, we realized Saul was getting more or less the same answer as us. He didn't know we were getting the same answer as him, but we knew he was getting because he showed it at some talk in preliminary form—his data had changed effectively. I have to admit, still being unsure, I knew what our data said, but I kept on thinking there must be something we're missing. There must be something no one has thought of; not just us, but the whole field that we've left out.

Bersten: When did you change your feeling that something could have been wrong?

Schmidt: That feeling really didn't go away until 2000. I mean after 6 months, I said, "Okay, maybe we're right. We haven't done anything really stupid." Then in 2000, when the cosmic microwave background came out and showed that the universe was flat, definitively flat. When I saw that, I realized that it made it almost impossible for our observations to show anything other than acceleration. It pushed things, so the only sensible solution was that the universe was accelerating. At that point I said, "I'll be damned, we're right." That was in May of 2000 when BOOMERANG and MAXIMA came out and showed the first peak of the cosmic microwave background definitively. That's when I said, "Wow, we're right!" Before that I just wasn't sure.

Bersten: Did you expect the Nobel Prize?

Schmidt: No, one does not expect a Nobel Prize. To my mind, I was genuinely surprised because when you make a discovery of acceleration—well, what causes the acceleration? Well, we give it a name—*dark energy*—but we don't understand it yet. I would not be surprised if we don't understand it during my lifetime. Without understanding it, I felt it wouldn't be worthy of a Nobel Prize. The fact that it was given pretty timely—you know, I was only 44 last year—was a bit of surprise. Put it this way, I wasn't waiting up for the call.

Bersten: How did you know that you obtained the Nobel Prize?

Received a Call from Stockholm While Cooking Dinner

Schmidt: They make the announcement at 11:45 am in Stockholm. I didn't used to know this, but I know it now. That is 8:45 pm in Australia. My son had been out at an athletic event and we had gotten home. My wife and I were cooking dinner. I was making a green Thai curry with my wife. I was stir-frying. The phone rang. I answered it and I had a Swedish accent saying, "Is this Schmidt?"

Now earlier in the day, I had my graduate student giving me his wedding invitation. His wedding was on the 10th of December. He said to me, "When they call you tonight, just tell them you're busy." I looked at him and I had no idea what he meant. I said, "What do you mean?" He said, "When the guys in Sweden call you tonight, tell them you're busy," because his wedding turned out to be on the day that the Nobel Prize ceremony. Now, I have to admit I still was a little confused, but he explained it to me. When I got home and I got this call from a Swedish woman, I have to admit my first thought was, this is a practical joke by my graduate student who had set me up earlier in the day.

I said, "Yes, this is Brian Schmidt," and then she said, "Are you sure this is Schmidt? This is a very important phone call from Sweden." That was a kind of funny thing to say. I responded, "I am sure that it is," thinking this is a practical joke. Then she put on another person, a much older Swedish gentleman, and then I was like "Geez, I can't believe, my graduate student could have got two people from Sweden to do a practical joke." Then they told me and it became very apparent that it wasn't a practical joke. I have to admit I felt it's very much like when my first son was born. It was very exciting, but I felt sick in my stomach, too much excitement.

Bersten: I can imagine it was! In what sense does obtaining the Nobel Prize change your scientific career?

Schmidt: Well, the Nobel Prize changes your opportunities, but does not necessarily increase your scientific opportunities. It actually hurts science because the fundamental thing that you need to make progress in science is time. That is the thing that young people have in the field, the time to focus and to answer questions. What the Nobel Prize gives you is the increase in scope of who will listen to your ideas. It gives you the opportunity to make representations to government about why science is important, why education is important, about the big directions that science is going in the country.

That doesn't help me make my little telescope (SkyMapper) run better, it turns out. But it does give me the opportunity, I think, to improve the way we approach science at the national level. A lot of my time has been spent doing things like that over the last year, meeting with people and government, explaining how they need to look at the education system, how we teach physics in high school, how we fund universities, how we fund research, why funding of research is important.

The other thing with the Nobel Prize is—in some way that doesn't really make sense to me, but I felt myself—quite inspirational to people. I met a few Nobel Prize winners when I was young and I was genuinely inspired by them. I don't know why. I just was. Now that I am one of those people, I could see that being the Nobel Prize winner inspires people. It's like they were part of the discovery. It feels kind of weird to me. I feel like I am living in someone else's body. But, it's important. That inspiration is really important. I do feel I have a role of doing what Hans Bethe and Dudley Herschbach, and the people I met when I was young did for me, because it was an important part of my upbringing, meeting those people and being inspired by them.

Bersten: I hope you inspire me also.

Schmidt: Well, we'll see.

Bersten: I know that you didn't have much time to know the Kavli IPMU. But, what was your impression about this institute?

Diversity of Kavli IPMU Is Helpful to Work on the Question of Accelerating Universe

Schmidt: Well for me, it's great to see such a diverse group of people here. One of the features of coming to Japan, which I've been to many times, has been everyone has been Japanese, except for who is visiting on the day. When you come to the Kavli IPMU, it is incredibly diverse. People from all over the world, very clearly. It's a lovely building, very new and sleek, and seems to have very good facilities. There's lots of discussion at coffee. Not everyone holed up in their office like I used to see a lot. I think it has a real vibrancy. It's surprisingly western for better and for worse. But, it's still Japanese. It's not so western as not to be Japanese.

Bersten: Yeah, we have tea—I mean green tea (laughs).

Schmidt: Yeah. I thought it was further out from Tokyo. It's actually much easier to get here than I expected. Of course, the autumn colors are beautiful. It's a great time to be here.

Bersten: I agree with that. Here in the institute, we have different fields together, people from string theory, people from mathematics and astrophysics. Do you think that this type of interaction can help to find a great discovery and so on, for example in the case of acceleration of the universe? Have you got some interaction with people in other fields that help to deeper understand the phenomenon?

Schmidt: The accelerating universe is a special case because it's very clearly a question that goes across astronomy and fundamental physics. As an astronomer, I know what the universe is doing. I know how to measure it. But, the real fundamental string theory or quantum field theories and gravity theories may link into it. Being able to solve a problem, like what is causing cosmic acceleration, really requires theory—which is more non-astronomy theory, but more particle physics theory—combined with the knowledge the astronomers have. I think being able to work together on questions like that is very helpful.

There are some problems which are mainly astronomy. There are some problems which are mainly particle physics. But, there are techniques that have been developed in the different fields that are useful. There are techniques of how you build instruments that astronomers have done, that are useful within particle physics and vice versa. There are data reduction techniques that are useful. There are statistical techniques the mathematicians typically will know. As we're asking bigger and bigger problems, you really need to look at them from a broader perspective. I think having that expertise around is very helpful for everyone because you get insights of how to solve problems that are beyond your field, and that's where you're liable to have a revolution in understanding, when you take a new idea and bring it in. That's the advantage of having people in related fields, but not the same, all collocated.

Bersten: I agree. I would like now to ask something related to your current work. In which project are you working now and which is the fundamental question that you want to answer?

SkyMapper Project for Precise Photometric Survey of the Southern Sky

Schmidt: As an astronomer, I am always looking at places where I can do something that I think is unique. In 2002, I started a project to map the southern sky in a way analogous to what the Sloan Digital Sky Survey did in the northern hemisphere. But my observatory burned down destroying the telescope and the instrument that was going to do this survey. Since 2003, I have been leading a project to rebuild a new telescope, a mapping facility called SkyMapper. SkyMapper is about a very precise photometric survey of the southern sky.

It is able to do supernovae and map them out in the nearby universe very accurately in a way that no one else is really doing. But, it also allows us to do galactic physics, that is how the galaxy formed, being able to pick out the most chemically poor stars in our galaxy that were formed right after the Big Bang, piece together how the supernovae enriched our galaxy and how our galaxy formed. That is a project which I am spending most of my time working on. It will take almost a petabyte of data. That's a lot of data.

Bersten: You say that you are not only focusing on supernovae, but on galaxies, which is the main goal of the project that you mostly work?

Schmidt: The project probably has several goals. The main goal of the Sloan Digital Sky Survey was to measure the large-scale structure of the universe, but much of the expected science was actually done in Australia by the 2dF Redshift Survey before they got to it. But on the other hand, the Sloan Digital Sky Survey had even a bigger impact than they expected, because it made a huge impact in areas they did not expect. That was because it allowed them to do everything. They did large scale structure, they did asteroids, they did stars, they did galaxies. They even eventually did supernovae. And they did these things very well, indeed.

The thing that really interests me on SkyMapper is both the metal-poor stars tracing out the birth of our Milky Way, but, then there is also all this supernova stuff we can do. We're going to be able to discover several hundred supernovae a year and get really the most comprehensive set of data for a set of objects ever. Get spectra of all of them–every single one. That will be a wealth to understand supernova physics, and that is very close to my heart. But, I really think being able to go through and find every metal-poor star in the Milky Way so that we can figure out how the Milky Way formed and how supernovae, which make the heavy elements, enriched the universe right at the beginning. Well, that's really exciting to me too. That's one of the goals of that program.

And it really is meant to do lots of other things. It's meant to find stars that are thrown out when they get to close to the supermassive black hole in the Milky Way. SkyMapper can go out and find those things systematically because there're stars out that shouldn't be there and SkyMapper has the ability to measure the radius of the star, its temperature, and its chemical composition. The radius and the temperature tell you how bright it is, therefore how far away it is. We can go out and we can literally pick out and say, "That star is chemically rich out at a very large distance. Why is it there? It shouldn't be there." Then when you look at those things, those things inevitably are the stars that were thrown out by the black hole in the Milky Way's center. By getting a bunch of those things, we can measure, for example, how much they slow down on the way out because they've been thrown directly out from the center.

It also gives us an idea–is the Milky Way black hole binary or is it just one? Because if it's a binary···

Bersten: The central black hole?

Schmidt: Yeah, the central super-massive black hole, it could be a binary. This is a good way to check it, because if it is a binary, the stars won't all be thrown out in all directions equally. They'll be thrown out in a plane. I think those are really cool things to do.

Bersten: Do you think that obtaining a Noble Prize in the supernova field gives an extra motivation for new projects in supernova? In that sense, also, I would like to know your opinion related with the possibility to continue the use of Type Ia supernova for obtaining better precision.

Schmidt: Yeah. I think the supernova field has grown tremendously during my lifetime. When I finished my Ph.D. in 1993, I was literally the person in the world who finished their Ph.D. in supernovae of that year because there were only a handful of us. I mean I know everyone from that era, *everyone*, because there was only a few people doing it. Since the discovery of acceleration, there are many, many people studying supernovae. I don't know everyone anymore. I don't know all the work that's being done. It's so much, it's hard to keep track of.

I think the Nobel Prize itself probably won't change that. I think that had already occurred before the Nobel Prize. I do think that with Type Ia supernovae there are some interesting things they can still do. I think using them to do more work at high redshift is pretty tough now. The measurements have really been very well done out there and we are limited now by systematic uncertainties. But in the nearby universe, there is a sweet spot where we just don't have very many objects. The objects we have observed have not been observed as carefully and as uniformly as a high redshift. I do think there's still an opportunity which I want to use SkyMapper to measure the expansion in the universe very accurately in the nearby universe so we can tie it in to the distant objects better. I think we can probably improve our constraints by about a factor of 2 by doing that well.

Bersten: By nearby, what do you mean?

Schmidt: Nearby, I mean out to a redshift of 0.1; beyond a redshift of 0.03, closer to the redshift of 0.1. I also think the other thing supernovae provide an opportunity to do is to measure how gravity behaves on large scales. One of the things where dark energy could potentially be lurking is that it isn't dark energy at all. If it's really Einstein's theory of general relativity being a little different than we expect out at large distances, that is something we can potentially test using supernovae. Because they measure very precise distances, it means that by comparing the expansion rate with the distance, we can measure a velocity that's been induced by gravity.

With hundreds of these objects, we can actually measure the average motion of every part of space using the supernovae as test particles. That gives us a way to measure how gravity is behaving at the scales of hundreds of megaparsecs. Supernovae are really the only good way to do this right now. That is a way of testing general relativity in a regime where it hasn't been tested before. I think that's another interesting thing that we're going to try to do with SkyMapper.

Bersten: Can you identify a problem in the supernova field that, if solved, may lead to a revolution like the accelerating universe?

Scientific Revolutions Come from *Not* Knowing

Schmidt: I think that revolutions usually come as a surprise. You can't easily predict revolutions. If you could there wouldn't be any, it wouldn't be a revolution. There are some fundamental things. Today, we still don't understand what makes the Type Ia supernova. What causes the Type Ia supernova to explode? Now, do I think that's going to revolutionize dark energy? No. But, we have been working for 20 years now to try to figure that out and it's still a mystery. Is it one large star donating material to a white dwarf and then it reaches the Chandrasekhar mass and explodes? Is it two white dwarfs coming together to exceed the Chandrasekhar mass and exploding? Is it a star that donates material in the form of helium and the helium detonates and causes the thing to explode before it reaches the Chandrasekhar mass? Is rotation important and does the thing actually quit growing and blow up a billion years later after the thing slows down a bit? Those are all active scenarios and I can't tell you which one is right.

I just find that amazing, given how much effort has been put into it. There doesn't appear to be an answer that really works. I think that's a real big question. Is it a revolution? No. Revolutions come from surprises. What we do is we keep on doing the best job we can and then the surprise will come from someplace where we least expect it. That's the beautiful thing of basic research and why it's so important. The revolution comes from *not* knowing what the future is, not predicting it.

People say, "Well, why don't governments just fund the things that they know? Why do we spend money on basic research? Why don't we just fund how to make better X, Y, or Z?" Well, the answer is because if you always are trying to do what you already know, then you just make a better X, Y, and Z. You don't invent the Internet. You don't invent Wi-Fi, which was done by astronomers in Australia. You don't have that revolution that comes from basic research. You need to have both. The revolutions come from *not* knowing.

Bersten: Thank you very much. I wish I had more time to ask you more questions! Unfortunately we have to finish now.

Schmidt: Okay. Thank you.