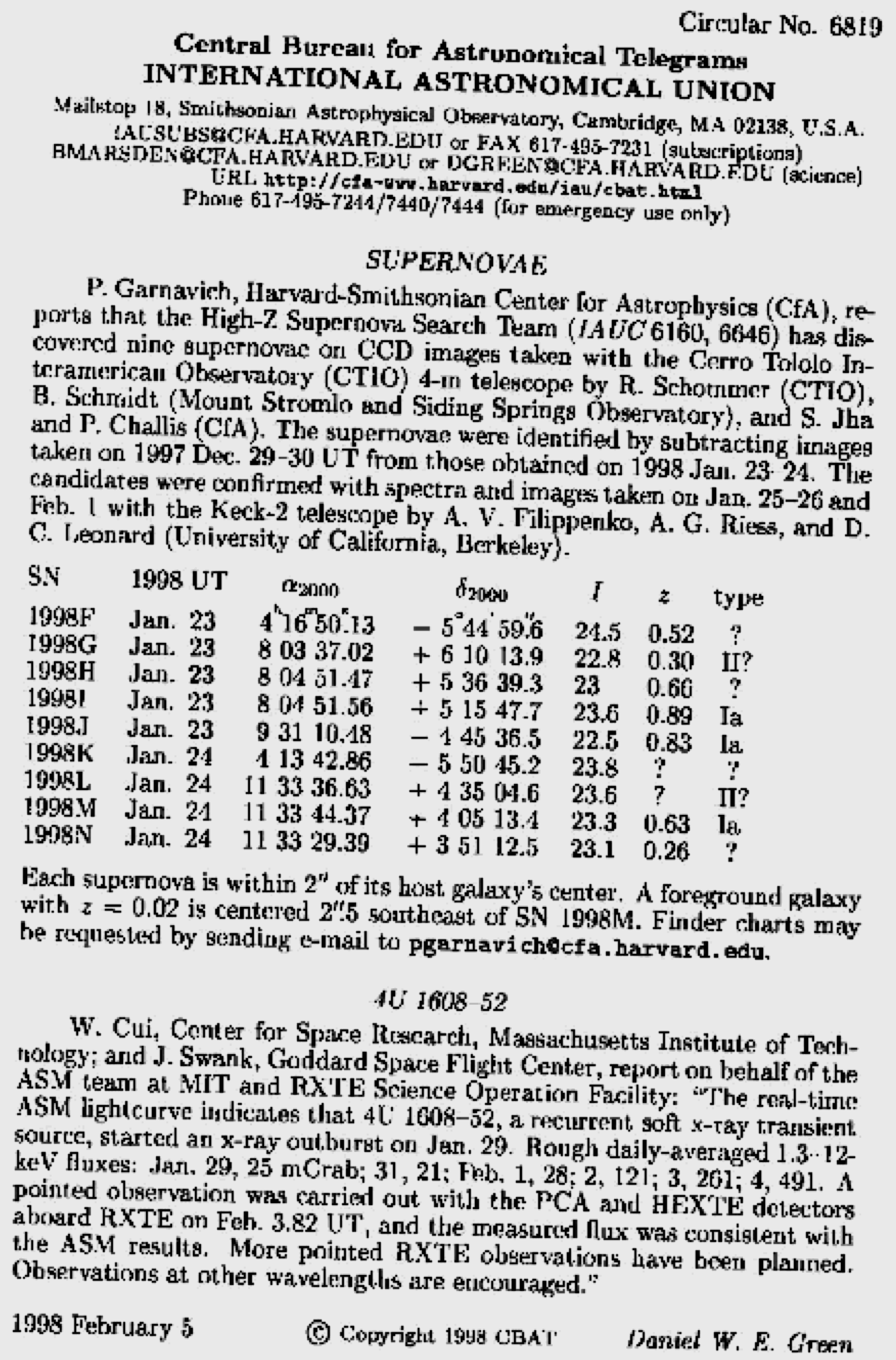
HST targets. And we know *where* we are going to find them. We are going to find them in the fields we observed in last month's dark run and for which we are going to make observations this month. We know where our target fields are. So we know we are going to find supernovae *when* we have observing time and *where* we point the telescope.

The search fields are about half a degree across, which is precise enough for the Space Telescope schedule to be constructed. This needs to take into account a zillion technical details. Ones I know of are the timing of the telescope's orbit, the direction to the sun, and the time it takes for the telescope to turn from the previous object to ours, moving at the stately pace of the minute hand of a clock. And there are lots more I don't have the neurons to know (but Ron Gilliland does). We tell the Institute boffins the precise position of the newly discovered supernovae we want to observe, they insert those details into the schedule, check, double-check, transmit, and execute. Elapsed time: about a week.

Is it worth all this bother? Absolutely. In 1997, the high-z team pulled this off, discovering supernovae on schedule at CFHT or Cerro Tololo, getting their spectra at Keck and at the MMT in Arizona, early light curves from the University of Hawaii's 88-inch telescope, and after delivering the precise target list on a weekday, we obtained a beautiful sequence of observations with HST starting one week after the Keck spectra and extending over the next 80 days.[5] While our original motivation for using HST was the wonderful imaging that makes photometry more precise, we also benefited from the absence of weather and the fact that moonlight doesn't light up the sky when you are above the atmosphere. The observations took place exactly as planned, which hardly ever happens on the ground, and we could time them in the optimum way to learn about the light-curve shape.

One difficult part of these measurements was making certain that the measurements from HST and from the ground agreed. To do this, we carefully matched ground-based and HST measurements of 15 background stars in the HST images that did not vary, which were bright enough to see from the ground, but not so bright they overwhelmed the HST's CCD detector.



Circular No. 6819

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SUPERNOVAE

P. Garnavich, Harvard-Smithsonian Center for Astrophysics (CfA), reports that the High-Z Supernova Search Team (*IAUC* 6160, 6646) has discovered nine supernovae on CCD images taken with the Cerro Tololo Interamerican Observatory (CTIO) 4-m telescope by R. Schommer (CTIO), B. Schmidt (Mount Stromlo and Siding Springs Observatory), and S. Jha and P. Challis (CfA). The supernovae were identified by subtracting images taken on 1997 Dec. 29–30 UT from those obtained on 1998 Jan. 23–24. The candidates were confirmed with spectra and images taken on Jan. 25–26 and Feb. 1 with the Keck-2 telescope by A. V. Filippenko, A. G. Riess, and D. C. Leonard (University of California, Berkeley).

SN	1998 UT	α_{2000}	δ_{2000}	I	z	type
1998F	Jan. 23	$4^{h}16^{m}50^{s}.13$	$-5^{\circ}44'59''.6$	24.5	0.52	?
1998G	Jan. 23	8 03 37.02	+ 6 10 13.9	22.8	0.30	II?
1998H	Jan. 23	8 04 51.47	+ 5 36 39.3	23	0.66	?
1998I	Jan. 23	8 04 51.56	+ 5 15 47.7	23.6	0.89	Ia
1998J	Jan. 23	9 31 10.48	− 4 45 36.5	22.5	0.83	Ia
1998K	Jan. 24	4 13 42.86	− 5 50 45.2	23.8	?	?
1998L	Jan. 24	11 33 36.63	+ 4 35 04.6	23.6	?	II?
1998M	Jan. 24	11 33 44.37	+ 4 05 13.4	23.3	0.63	Ia
1998N	Jan. 24	11 33 29.39	+ 3 51 12.5	23.1	0.26	?

Each supernova is within 2" of its host galaxy's center. A foreground galaxy with $z = 0.02$ is centered 2''.5 southeast of SN 1998M. Finder charts may be requested by sending e-mail to `pgarnavich@cfa.harvard.edu`.

4U 1608–52

W. Cui, Center for Space Research, Massachusetts Institute of Technology; and J. Swank, Goddard Space Flight Center, report on behalf of the ASM team at MIT and RXTE Science Operation Facility: "The real-time ASM lightcurve indicates that 4U 1608–52, a recurrent soft x-ray transient source, started an x-ray outburst on Jan. 29. Rough daily-averaged 1.3–12-keV fluxes: Jan. 29, 25 mCrab; 31, 21; Feb. 1, 28; 2, 121; 3, 261; 4, 491. A pointed observation was carried out with the PCA and HEXTE detectors aboard RXTE on Feb. 3.82 UT, and the measured flux was consistent with the ASM results. More pointed RXTE observations have been planned. Observations at other wavelengths are encouraged."

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Figure 10.5. An International Astronomical Union Circular from the Bureau for Astronomical Telegrams reporting the results of two nights of searching in 1998. Some of these supernovae were observed with the Hubble Space Telescope. Courtesy of the Central Bureau for Astronomical Telegrams.

Peter Garnavich, a postdoc working with me at the CfA, now on the faculty at Notre Dame, took responsibility for getting the HST data reduced, and by the end of 1997, we were finally in a good position to say our first words about cosmology. Based on the data in hand, we did not agree with the LBL team's earlier conclusion as discussed in Princeton and published in July. They had found evidence for deceleration, corresponding to Ω_m near 1. In their data, that meant the supernovae appeared a little brighter than

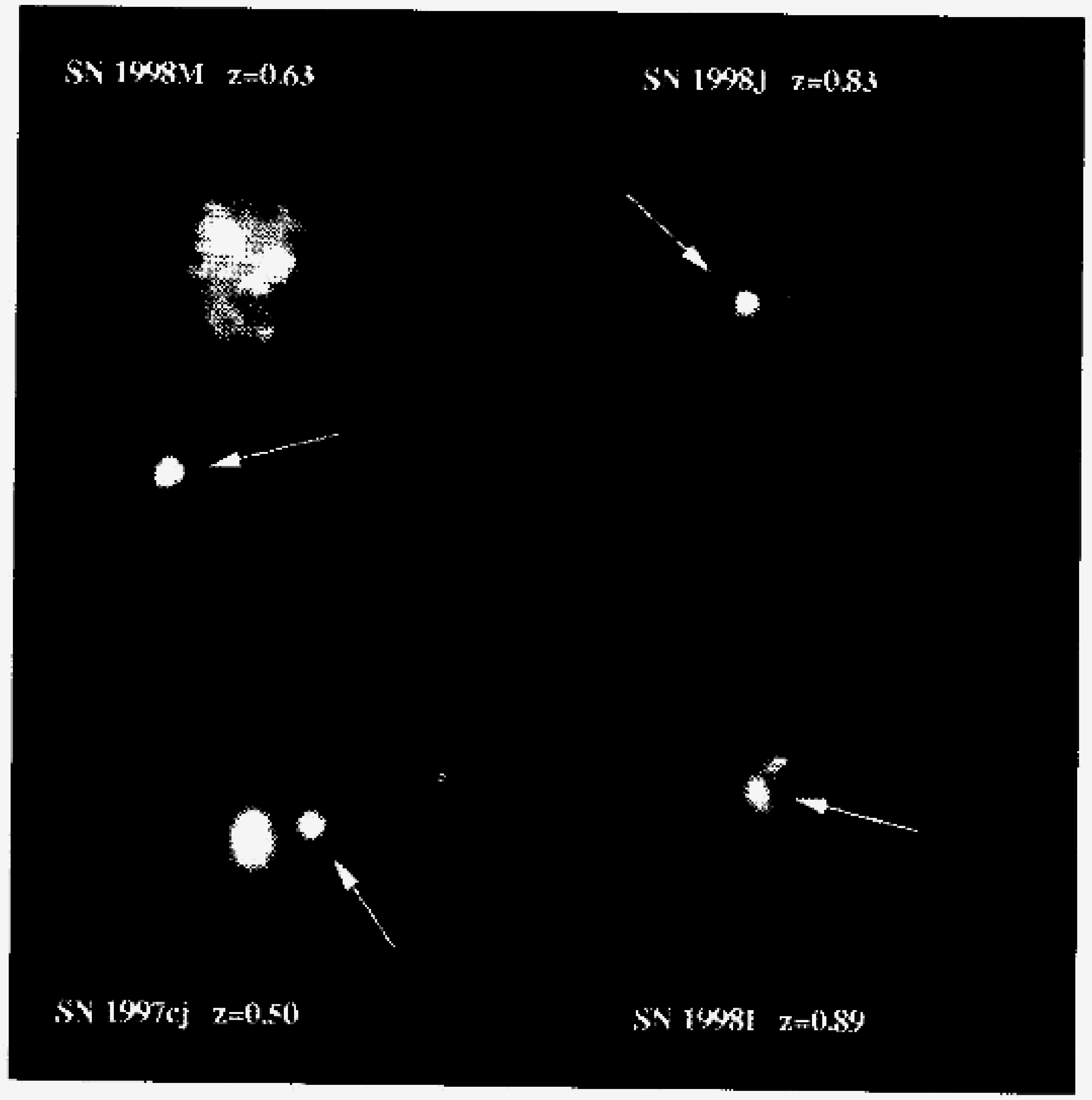

Figure 10.6. **High-z supernovae observed with the Hubble Space Telescope.** Courtesy of Peter Challis; High-z team/NASA. (Also see color insert)

they would in a freely coasting lightweight universe. When Garnavich plotted up the data, our supernovae showed no such effect. Although the data were too scanty to tell us the whole history of cosmic expansion, they were adequate to rule out $\Omega_m = 1$. We worried a little that the LBL team had published a contrary result. But this was hard work, and there were many ways to go wrong. We decided not to worry too much about the other guys, to judge our own measurements by our own internal standards, and to hope for the best.

The vivid way to state Garnavich's conclusion is that we showed that the universe would expand forever. That seemed like interesting news, so we sent in an abstract for the forthcoming meeting of the American Astronomical Society (AAS), which was going to be in Washington, D.C., in January 1998. We didn't yet have enough data to say whether there was or was not cosmic acceleration, so we were silent on that point.

Meanwhile, the other team was changing its tune. In [illegible] 1997, they had published an article in *The Astrophysical Journal* [illegible] data pointing to large Ω_m. Now, at the end of 1997, we heard [illegible] had a new result submitted to *Nature*, with an HST observation [illegible] their own, which claimed the opposite. With the addition of ju[illegible] new supernova, augmenting their sample of seven, they now [illegible] that their evidence pointed the other way, toward low Ω_m. The [illegible] new supernova had observations from HST, so it was presumably better data and, if calibrated carefully, carried more weight than the earlier work. Still, for one object to turn July's conclusion on its head seemed extraordinary. We had no way to check their work, since neither of their papers published the details of the light curves and spectra. In any case, the SCP also submitted an abstract for the upcoming AAS meeting (which we read carefully!) stating clearly that they now found evidence for low Ω_m. On the subject of cosmic acceleration, though, that abstract was silent.

In the Fall of 1997, the Institute for Theoretical Physics (ITP) at the University of California, Santa Barbara, sponsored by the National Science Foundation, held a workshop on supernovae. I had never taken a sabbatical in 21 years as a university professor, my personal life was in transition, and this seemed like the right time for a break from the routine. Unlike New England, where a nice day is a rare thing highly prized, Santa Barbara is a place where almost every day is pleasant. People lose their sense of urgency. Play tennis? Oh, maybe tomorrow. It will be nice tomorrow. Physicists are not entirely immune to the charms of this place, but they run on more tightly wound internal springs than most Santa Barbara residents. Play tennis? Oh, maybe tomorrow. Today let's figure out supernova light curves.

Although the ITP is really a place for theoretical physics, and it would be false to say I am a theorist and misleading to say I am a physicist, they treated me very well. Sort of like a pet Bernese mountain dog. A little out of place in Santa Barbara, not very good at retrieving ducks, but amusing. As a service for the ITP, I gave a public talk for the local community on high-redshift supernovae and the quest for understanding cosmology. Unfortunately, in the fall of 1997, we were not quite to the moment of having an important result—we knew how to do the problem, we had some data in hand, but we didn't quite have the answer.[6]

As a reward for my public-spirited behavior, David Gross, the director of the Institute for Theoretical Physics, and Adam Burrows, an organizer of the supernova workshop, gave me a made-up union card in the International Brotherhood of Theorists. Decorated with a spilled coffee cup and stubbed out cigarette butts, it declares the theorist's self-referential motto: *Cogito ergo sum.* I carry it around in case I think I am a theorist.

The real theoretical physicists at the ITP were very attentive to cosmology—it is a fast-moving field where the data might demand new physics. The cosmological constant was a well-known problem in theoretical physics. My office had a spectacular view of the ocean, including surfing undergraduates and swimming dolphins, but right across the hall at the ITP was Sean Carroll, a young postdoc who had been one of the brightest and most interesting astronomy graduate students at Harvard (as a student, he shared an office with Brian Schmidt).[7] Sean was a precocious author of a review article on the cosmological constant written in 1992, with Bill Press of Harvard and Ed Turner of Princeton. The review summarized the problem from the point of view of astronomers, looking for evidence, and from the point of view of theoretical physics, reasoning from the nature of particles and fields. Though the value of the cosmological constant allowed by astronomical observations in 1992 might have been as large as Ω_Λ equals 1, the simplest theoretical prediction gave $\Lambda = 10^{120}$ (that's 1 followed by 120 zeroes!). More sophisticated theoretical reasoning could make this 10^{80}, or perhaps 10^{50}, but there was no theoretical reason that very bright people could think of why Λ should be a small number like 0.1 or 0.6 or even 17. Faced with the astronomical reality of a small (compared to 1 with 50 zeroes) cosmological constant, many theorists suspected it would be exactly zero. This is a good second guess. But not everything that's infinite cancels out.[8]

Sean Carroll's article made it clear that there was no positive evidence for a value of Λ that was different from zero, just upper limits from the absence of various effects that Λ would cause. Looking backward, it is amusing to see that Sean's 1992 article makes no mention of supernovae as a possible way to see if the universe was accelerating, as a small Λ might make it do. The work of the Vikings at the Danish telescope in Chile in 1985, which was aimed at this goal, simply hadn't made it onto the theoretical radar screen. But as a problem in theoretical physics, the cosmological constant was a real riddle.[9] Steven Weinberg, a distinguished particle physics theorist, has called the cosmological constant "a bone in our throat."

Even though I didn't yet have anything definitive to report, Sean's antennae were up for any hints that the cosmological constant might become respectable again. The ITP is a center for the revolution in particle physics that is trying to build a new theory for the quantum mechanical forces that operate at the subatomic level *and* that incorporates gravity. General relativity had been around since 1916, and quantum mechanics was developed in the 1920s, but there was still no quantum theory of gravity that united these two powerful pillars of twentieth-century physics, and building that bridge was a serious quest for theoretical physics. Right down the hall from me were people working on developing string theory, which holds out the best hope for making a single theory that covers *all* the known forces. One challenge for this new theory is to provide a natural explanation for a small value of the cosmological constant by connecting the quantum world with gravity. You really didn't need an astrophysical measurement of Λ to know it was small compared to 10^{120}, so for years the subject was mostly a private conversation among the theorists. This was about to change.

For the supernova tribe, the "work" of the workshop included discussing the physical origin of the effect we were using to make type Ia supernovae better standard candles. What accounted for the fact that some of the thermonuclear supernovae were extra bright, and some were dim? And why were the light curves different for bright supernovae and dim ones? Those seemed like tractable questions, and the assembled explosive types, including Friedel Thielemann (recently on the Harvard faculty, now Herr Professor Doktor in Basel), Adam Burrows, Ken Nomoto, Wolfgang Hillebrandt, my nocturnal tag-team wrestling partner Craig Wheeler, Dave Arnett, past CfA postdoc Phil Pinto, and my one-time student Ron Eastman, seemed like people who could help answer them.

After all, it was not enough to have a practical, empirical way to use SN Ia to measure precise cosmic distances. If you didn't also understand them, you might get fooled when you looked at distant galaxies where the chemistry was different and the stars were, on average, younger. One possibility was that both the bright and the dim supernova came from very similar objects crammed up against Chandrasekhar's upper mass limit for white dwarfs, but that some had more radioactive power for their light curves because they fused more nickel in the explosive flame that ripped through these stars. An alternative was that some of the exploding white dwarfs were not at the Chandrasekhar limit, but came from lower mass stars that exploded in a different way that accounted for the range in SN Ia brightness.

The decline rate seemed to have something to do with the atmospheres. If there was a lot of heat supplied from radioactive nickel, the atmosphere might stay warm and opaque longer, making a slower decline rate for the intrinsically bright objects. The dim ones would cool off and turn transparent sooner. These were just ideas, and they needed to be worked out in more detail to become convincing explanations for the data. Santa Barbara was a place to do that work. We could always play tennis tomorrow.

As Thanksgiving approached, the air in Santa Barbara was full of talk about exploding white dwarfs of differing light output when Gerson Goldhaber, a senior member of the Supernova Cosmology Project, came to tell us what they were doing. Gerson comes from a distinguished family of physicists: husbands and wives, uncles and aunts, cousin and nephew physicists from coast to coast. Gerson was a veteran of experimental particle physics, having been in the middle of work on exciting new particles of the 1970s that led to the physicists' Standard Model. Well known and highly respected by the physicists, Gerson was in slightly unfamiliar terrain among the astronomers.

An imposing gray-bearded figure, Gerson spoke slowly in a rich European accent, pulling gently on a pair of broad suspenders that stretched over his convex figure. Like many other successful physicists, he had succumbed to late-onset astrophysics, taking on the challenge of searching for high-redshift supernovae with the same intensity he used to find the charmed mesons. At LBL, they had spent several man-years building their own computer software to sift the supernovae from repeated images, while our high-*z* team had woven together software from existing astronomical programs that did equivalent tasks. Astronomers and physicists are tribes from different parts of the forest, and Gerson didn't know many of the people in the room or that the hot topic among the supernova theorists at Santa Barbara was to account for the differences in light output of type Ia supernovae.

He started his talk with a picture of a candelabra and spoke of standard candles. He told us that supernova explosions were all identical. By measuring the apparent brightness, the LBL group had developed a method to measure the distances to supernovae and to measure the history of cosmic expansion. I thought it was useful, if not polite, to break in.

"Gerson. The people around this table are trying to understand the reason why type Ia supernovae are *not* alike. It's too simple to say, at least to this group, that all SN Ia are identical."

Gerson didn't like it one bit. He bristled, then turned formally to Friedel Thielemann. "Mr. Chairman, must I endure these interruptions?"

Friedel smiled and said this was a workshop, that the interchange of ideas was important, and that a free discussion was our style. Then he gave me a glance that meant, "Bob, shut up, and stop causing trouble."

At dinner that night at a French restaurant in downtown Santa Barbara, I was polite, if not useful. Gerson's afternoon talk had been mostly about methods and didn't have much about the LBL team's latest results. I was interested in learning exactly what had made them change their conclusions by 180° from July to November. But I wasn't able to learn anything about new results at Berkeley from Gerson. He was very discreet, and did not discuss the *Nature* paper that was being refereed (but not by me!). Gerson deftly steered the conversation to the comparative merits of French restaurants near CERN, the giant particle accelerator near Geneva. My fiancée, Jayne

Loader, had no trouble drawing him out on this delicious topic. Gerson didn't seem at all interested in the progress of our high-z team. I modestly volunteered we were several months behind them.

"You mean several years," Gerson said.

I didn't say anything, but toward the end of 1997 we were already beginning to see hints of something more interesting than just a low-Ω_m universe that would expand forever. Adam Riess was assembling our high-z data at his office in Campbell Hall, on Berkeley's main campus, just down the hill from the LBL team. Adam thought he was beginning to see evidence for cosmic acceleration. Our data showed that the distant supernovae were *fainter* than they would be in a low-density universe. Faint supernovae meant larger distances. Larger distances meant cosmic acceleration. Every time he tried to use the data to determine Ω_m without Λ the value for the mass kept coming out *negative*. That wasn't right. So he added in Ω_Λ, and the best fit to the data points kept giving a value of the cosmological constant that was bigger than zero. As the data trickled in, Adam added more supernovae to the analysis. The statistics were beginning to make the case for the cosmological constant.

I did not like this result. The cosmological constant was a bad companion. For the past 50 years, every sensible paper either began with "we assume $\Lambda = 0$," or just assumed it without saying so. Even if Jerry Ostriker and Paul Steinhardt were making the case for Λ, and Mike Turner at Chicago had tried out Λ in recent years, they were just theorists being provocative. This was not a Greek letter that a well-behaved observer ought to be seen with. How could we be sure there wasn't a dumb mistake somewhere in the long chain of data reduction? Had somebody else checked the numbers?

Adam said that Brian Schmidt concurred with the analysis. I still thought this was a result that would go away as we accumulated more data, and I did not like the idea of going out on a limb and then being forced to crawl back. I had done that once with SN 1987A.

Summoning my dignity, I said, "Adam, the punishment for being wrong should be as big as the reward for being first."

"Reward?" Adam said. "You're going to give me a reward?"

In December 1997, Jayne, our bull terrier Albert, and I decamped from Santa Barbara for a few weeks in Pasadena at Caltech. Fritz Zwicky was long gone, my thesis advisor, Bev Oke, had retired to Victoria, B.C., Jim Gunn had been in Princeton for 17 years. Leonard Searle had retired. Wal Sargent was still there, but the Robinson Lab was a different place. In a way, the mid-1970s had been a high-water mark at Caltech. When the 200-inch reigned supreme, Palomar Power dominated the astronomical scene. Then followed an unpleasant two decades of parity as 4-meter telescopes sprung up around the world in the 1970s and 1980s to challenge the hegemony of the Big Eye. This was good for me, good for science, but not so great for Caltech.

Now, the Caltech astronomers once again had the kind of advantage they liked. With Caltech holding one-third of the time on the two Keck telescopes, a Caltech astronomy professor once again had about 10 times the observing power of anyone else. That's the way they like it.

They set me up on the second floor in Robinson Lab, the quarterdeck where most of the faculty had their offices. This was rarefied air for someone who had worked in the engine room of the second sub-basement. I couldn't even find my way down to 0013. The way was blocked with radio astronomers. I shared the second-floor office with Richard Ellis, who was visiting from Cambridge, where he was Plumian Professor, Eddington's successor. Richard had been leading the way in studying how galaxies evolved over time, and had also contributed to studying high-redshift supernovae. Richard had worked with the Danes in the subject's pre-history to follow up their supernovae, and Richard was now working with Saul Perlmutter, helping the LBL team with observations at the Isaac Newton Telescope and elsewhere.

One December day at the end of 1997, Richard and I were both in the office while I was having a long telephone conversation about the high-z results with Adam Riess. Miss Manners requires the accidental eavesdropper to act as if one has heard nothing. And Richard was working with the LBL team, so I tried not to give him too difficult a test of his discretion. To Adam I said, "Un hunh," "I see," and "How do you feel about that?" like a psychologist on TV. But the office was too cozy and his brain too active: Richard couldn't help filling in the blanks, and in the end, he could not resist a comment. As he was walking out the door, he turned to me, gurneyed up his Welsh face, and said,

"It *can't* be the cosmological constant."

"It can't be," I agreed, making a face of equally authentic disgust.

The weeks passed quickly in Pasadena while Adam and I went back and forth about the latest results. Did we really believe we were seeing the effects of a cosmological constant? We hadn't reached a resolution by 1 January 1998. Down Colorado Boulevard at the Rose Bowl, Michigan beat Washington State 21–16 and was dubbed the national champion. Go Blue! My son Matthew, a University of Michigan senior, came to town for the festivities. We didn't see all that much of him: Wolverines are everywhere, and Matthew had plenty of friends in southern California to share the triumph.

At the equivalent astronomical event the next week, Peter Garnavich presented our team's evidence on eternal cosmic expansion at the American Astronomical Society meeting in Washington, D.C. Our handful of supernovae favored a low value of Ω_m. Or, more vividly, no slowing down, expansion forever! Go Blue!

Peter shared the podium at a press briefing with Saul Perlmutter. Saul said that they had concluded based on the same seven supernovae from before plus one new one observed with HST that the world was *not* coming to an end. Contrary to their previous result, the SCP now favored a low value for the observed slowing of the universe. Plus, Saul showed an impressive new plot based on observations of 40 supernovae.

What was most interesting was what the SCP did *not* say about their Hubble diagram. At this gathering, with many very interested reporters present, neither team dared to claim they had demonstrated cosmic acceleration, the signature of the cosmological constant. Jim Glanz, then of *Science* magazine, could see where the SCP data might be heading, and wrote a news article for *Science* that tried to anticipate the next step, but at that moment in January

1998, Saul Perlmutter was not ready to say they had seen accelera tion. Saul delicately stuck to the subjunctive, as if he were indicatin a supposition contrary to fact. Glanz quoted him as saying, "If [th results] hold up, that would introduce important evidence that ther is a cosmological constant." Saul wasn't ready to stick his neck out.[1

Neither were we. Adam Riess had made Peter Garnavich prom ise not to say anything in Washington about the new data we wer working on—he could show the beautiful points from our HST ob servations, but say nothing about the additional data that was point ing toward Λ.

Peter Garnavich carefully studied the posters the SCP ha brought to display at the Astronomical Society meeting. None o them claimed that the SCP had evidence for cosmic acceleratio because they had not yet come to a firm conclusion on how t handle "systematic effects," mostly reddening by dust. This was ex actly the point I had been trying to make to Saul since that awkwar referee report in 1993—if you don't understand the dust, you can say anything about cosmology.

Now it was time for our team to get serious. The SCP would no sit on the fence indefinitely. They were smart guys and they woul either figure out what to say about dust or sweep it under the ru before too long. Was our high-*z* team ready to climb out on the limb where the data were pushing us? The distant supernovae wer coming out about 25 percent dimmer than they would appear in universe with $\Omega_m = 0$. Dim supernovae implied acceleration, if the weren't dimmed by dust, and our observations in two filters suggested that there wasn't much dust.

How reliable was our result? We had 16 decent objects, 10 wit reasonable estimates of the uncertainty from multicolor observations of the light-curve shape to improve the accuracy and precisio of the distances. If we believed the formal 3σ error estimates fro Gaussian statistics, the chances were 3 in 1000 that this was a ba luck sample in a universe that was actually decelerating. If you believed the error estimates, the odds were about 300 to 1 that we were living in an accelerating universe. Did we believe the erro estimates? Did we trust in Gauss?

Well, yes and no.

Yes, the methods using light-curve shapes gave the right-sized errors for samples of nearby supernovae. It was like asking how many Cheerios are in a cereal bowl. You could estimate the number, and also estimate how far off from the true number each sample might be due to chance. Gauss knew how to do this. Bill Press had a recipe for doing this in his mathematical cookbook *Numerical Recipes* and Adam had made it work for the multicolor light-curve shape method.

And no, there might be additional problems in the much sketchier data for distant supernovae that somehow we were not accounting for properly. Maybe we were doing the equivalent of crunching the cardboard with a vigorous twist of the micrometer—getting consistent, but wrong, results.

For the moment, we kept our lips sealed while we tried to decide how seriously to take our own evidence. Bruno Liebundgut had to attend an Alpine conference at the end of January 1998, and bite his tongue. Bruno didn't hurt himself skiing at Moriond, but he had to restrain himself during the discussion of supernova Hubble diagrams. He showed the same data that Garnavich had showed in Washington. In the two weeks that had passed, people had gotten used to living in a universe that would expand forever, and this result now seemed as exciting as cold oatmeal. Bruno did not show the additional data points that made us think we were seeing cosmic acceleration. Somebody from the SCP showed their 42 objects, which looked pretty impressive. But they still did not claim that the data showed we lived in an accelerating universe because they didn't quite know what to do about the "systematics."

Inside our team, we were debating exactly how to proceed—whether to write a quick, short paper that might be wrong but would stake a claim to the discovery of acceleration, or to take more time to write a more thorough paper that would show all the evidence. Everybody on the high-*z* team weighed in. We had a conference call—always a dubious proposition, but worse when you have participants in Europe and in Australia. Somebody is always half-asleep. We exchanged e-mail. Lots of e-mail.

Adam Riess was doing the heavy lifting for this paper, drawing together all the data, working out the implications, and dealing out the writing assignments. So we all gave him advice. Conflicting advice. After all, this was a collaboration, not an army.

I didn't like the result. I didn't think we were smarter than Einstein and he had tripped on the cosmological constant. I did not want to make a mistake. I hadn't liked being wrong about the progenitor of SN 1987A and I did not want to be wrong about the history of cosmic expansion. On 12 January 1998 (at 10:18:31 A.M.) I wrote,

> I am worried that the first cut looks like you might need some lambda. In your heart, you know this is wrong, though your head tells you [that] you don't care and you're just reporting the observations. . . . It would be very silly to say "we MUST have nonzero lambda" only to retract it next year.

While Peter Garnavich was in Washington, Adam dropped out of sight for a few days to return to New Jersey to marry his MIT classmate, Nancy Schondorf. At the reception, one of Nancy's cousins asked about a news story he had read in the paper that morning. It said the universe would expand forever. Did the groom know anything about this?

"I am familiar with that work," Adam said.

Adam wrote us all a long e-mail (on 12 January 1998 at 6:36:22 P.M.). This was two days after the wedding, just before leaving for their honeymoon, the traditional time for writing e-mail to scientific colleagues.

> The results are very surprising, shocking even. I have avoided telling anyone about them for a few reasons. I wanted to do some cross checks (I have) and I wanted to get a ways into writing the results up before Saul et al. got wind of it. You see, I feel like the tortoise racing the hare. Every day I see the LBL guys running around, but I think if I keep quiet I can sneak up . . . shhhh. . . . The data require a nonzero cosmological constant! Approach these results not with your heart or head but with your eyes. We are observers after all!

Alex Filippenko was all for going ahead fast. His logic was simple. The data pointed toward cosmic acceleration, the LBL team was close to the same conclusion, but not quite ready to take the plunge, so let's publish first. Alex wasn't too worried about being wrong.

"It is possible that there's some sort of subtle systematic effect, but if so, I think it's going to take a long time to figure out."

Writing from Australia, Brian Schmidt was more conflicted:

> It is true that the new SNe say that the complete sample of ~12 objects gives Ω_Λ greater than zero with over 90% confidence. . . but how confident are we in this result? I find it very perplexing and I think we should really try to take the high ground here scientifically. . . . Let's put out a paper we can be proud of—quickly.

Nick Suntzeff weighed in from Chile with good advice on physical training for Adam.

> I really encourage you to work your butt off on this. Everyone is right. We need to be careful and publish good stuff with enough discussion to make it believable to ourselves. . . . If you are really close to being sure that lambda is not zero—my god, get it out. I mean this seriously—you probably never will have another scientific result that is more exciting come your way in your lifetime.

In the end, we decided to let Gauss be our guide, and to go ahead. If the data said the cosmological constant was a 3σ result, then we were going to say it was a 3σ result and live with the consequences. Less than 1 percent chance of being wrong. Bet $30,000 to win $100. But don't bet your pets.

I had been invited to speak at the Dark Matter meeting that UCLA organizes every other year, but the February dates conflicted with my return to Harvard. So I was driving across America, with Jayne and Albert the bull terrier, seeking motels that take pets, while Alex Filippenko carried the high-*z* team banner to Marina Del Rey. Gerson Goldhaber and Saul Perlmutter spoke first, showing evidence for time dilation, strong evidence for Ω_m being too small to halt cosmic expansion, and tentative evidence for possible Λ but they were still not quite ready to say that they understood the systematic effects well enough to be certain. Alex presented our team's data and analysis of 16 supernovae at redshifts from $z = 0.16$ to 0.97, comparing them with 27 nearby supernovae from a combined CfA

and Calán/Tololo sample. The Hubble diagram for these superno
vae indicated that the universe was not just expanding, and not ju
destined to expand without limit. Alex said clearly that our superno
vae provided evidence that cosmic expansion *had sped up durin*
the last 5 billion years.[11]

We were totally unprepared for the press onslaught that starte
on 27 February. Alex left town to be the tour guide on an eclips
expedition in Aruba. When Adam Riess got to his Berkeley offic
that day, the phone was ringing. CNN had a camera crew rollin
across the Bay Bridge—could they interview him? In 15 minute
The next day, Adam appeared on *The News Hour*, his father's favo
ite show. The press was really interested in the accelerating un
verse, but even more interested in how we felt about the results,
if this would somehow affect the universe. Brian Schmidt w
quoted as saying, "My own reaction is somewhere between amaze
ment and horror."

Saul Perlmutter's group had been struggling with the same set
questions, doing their best to get it right. Their data pointed towar
acceleration, but they weren't quite ready to say they believed th
result in January 1998 at the AAS meeting or Moriond or in Februar
at the Dark Matter meeting. They were worried about the right wa
to treat the absorption of supernova light by dust. We had spen
the past five years taking data on nearby supernovae and the
working out the way to use light curves and colors to measure du
absorption. We took the plunge in February. In April 1998, Gerso
Goldhaber explained his view of this sequence of events to the *Ne*
York Times: "Basically, they have confirmed our results. They onl
had 14 supernovae and we had 40. But they won the first point i
the publicity game."[12]

It was all very well to submit abstracts to meetings, give pres
briefings about your mental states, and talk at conferences, but th
real scientific product is a refereed journal paper. The high-*z* tear
concentrated on getting the data into a form suitable for public ir
spection with the evidence shown as clearly as possible and th
conclusions stated as strongly as the evidence would support. W
tried to be our own most caustic critics, probing the weak points
the evidence and exposing the assumptions to debate. By 13 Marc

we had done a job that was not perfect, but good enough. And sometimes good enough is good enough.

We decided to send the manuscript to *The Astronomical Journal* instead of *The Astrophysical Journal* as an inside joke. The other team said they had used a "physics based" approach. Since I didn't know what that meant, it seemed vaguely amusing to use a journal with "astronomical" in its title. Also, we knew the *AJ* publishes things faster. "Observational Evidence from Supernovae for an Accelerating Universe and a Cosmological Constant" was refereed, accepted on 6 May, and appeared in the September 1998 issue. We concluded the abstract of the paper with a long litany of possible sources of error, and then concluded, "Presently, none of these effects appears to reconcile the data with $\Omega_\Lambda = 0$."

All along, we had made the case that it was a good thing for two independent groups to carry through this work. We were very interested to see exactly what the SCP had done. Their paper, "Measurements of Omega and Lambda from 42 High-Redshift Supernovae" was submitted to *The Astrophysical Journal* on 8 September 1998, accepted in December, and appeared in the June 1999 issue.

Although the two programs were independent, the conclusions reached were the same: supernovae at redshift near 0.5 were about 25 percent fainter than they would be in an $\Omega_m = 1$ universe. The distant supernovae were, with a few exceptions where the teams helped each other out with observations, not the same. The data reductions were done by different methods. The ways that light-curve shapes were employed to correct for the variation in SN Ia brightness were different. We handled dust absorption in different ways. But despite these differences in detail, the conclusions were, as Saul neatly put it, "in violent agreement."

Although, as Gerson Goldhaber had correctly noted, we had fewer distant supernovae, 16 to their 42, on average, each of our points had about half the error. I think this was the good effect of having Nick Suntzeff as a leader of the high-*z* team plus the power of the statistical methods we had developed to analyze supernova light curves. The ability of a data point to tell you something decreases as the square of the scatter, so our 16 points with small

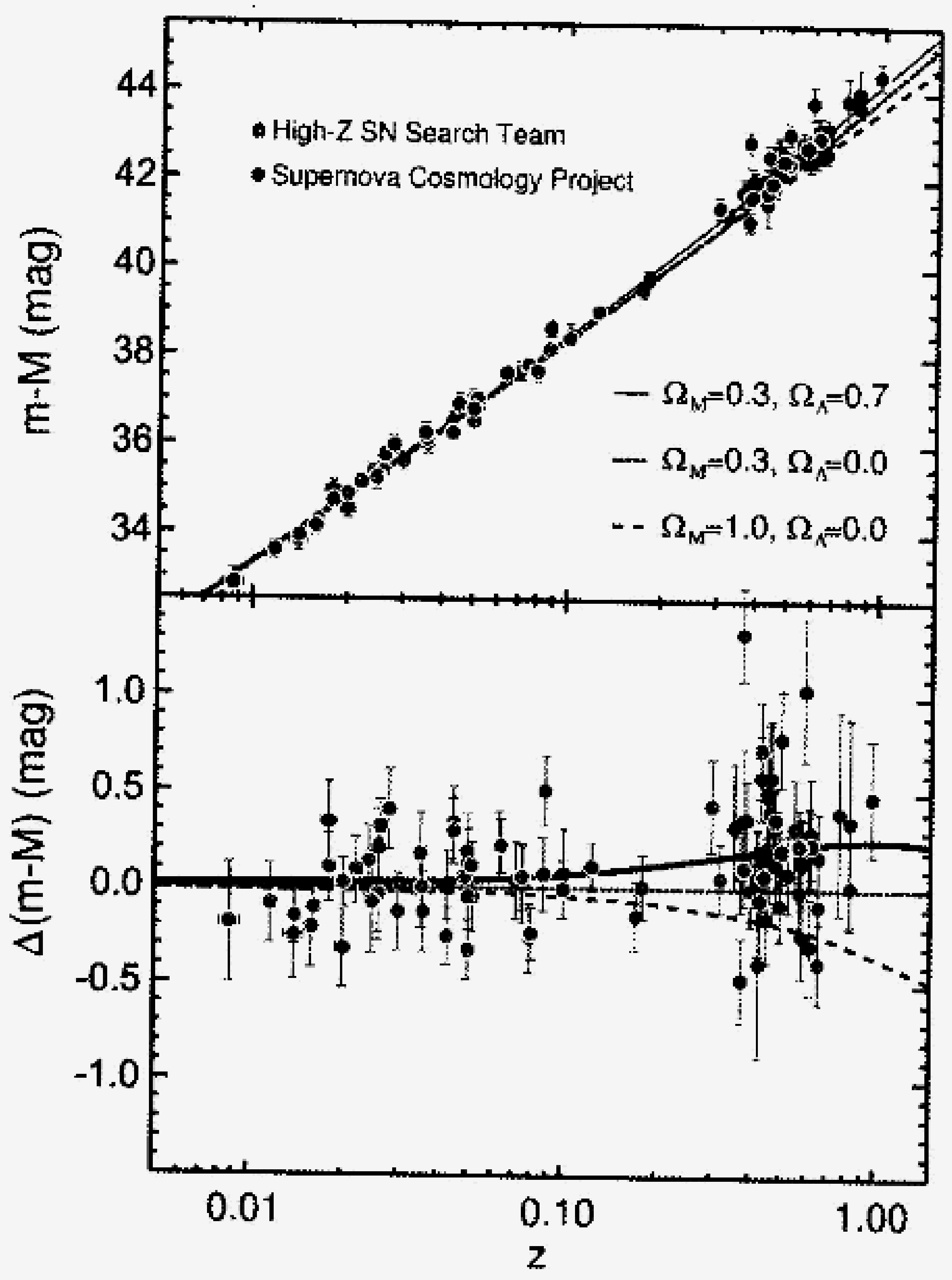

Figure 10.7. **The Hubble diagram for high redshift supernovae.** The small departure from the dotted line in the upper panel is the evidence that we live in an accelerating universe. In the lower panel, the 45° slope, which is just the inverse square law, has been removed. The points certainly lie above the downward curving line of long dashes, which is the prediction for $\Omega_m = 1$ with no cosmological constant. Most of the points also lie above the dashed horizontal line which is the prediction for $\Omega_m = 0.3$, with no cosmological constant. The only way to get up to the solid line (which is formally the best fit to the data) is to include the effects of acceleration. Points from both the high-z team and the supernova cosmology project are shown here. The high-z team points are fewer, but have equal weight because of smaller uncertainties.

scatter were just as helpful in telling something about cosmology as their 42.

And the something was, you needed Λ to match the data. Since there is an invisible contest between Ω_m, which slows cosmic expansion, and Ω_Λ, which speeds expansion up, the supernova results provide information about the difference between the attractive effects of matter and the accelerating effects of dark energy. The supernova results showed that acceleration is winning now, stretching out the distance light has to travel from a supernova at redshift 0.5 to our telescopes. The supernova results measure $\Omega_m - \Omega_\Lambda$, and they showed that this quantity must be smaller than zero. You cannot do that without Λ, or something very much like it. It's a little like stepping on a scale and finding your weight is *below* zero—something beyond the usual gravitational attraction must be going on! So far, the supernova data are the *only* evidence that the universe is accelerating, and the only measurement that shows the effects of Λ directly. As Sir Frank Dyson said of the gravitational bending of light, "I was myself a skeptic and expected a different result." Me, too.

The cosmological constant might have been Einstein's biggest blunder and part of Eddington's journey into the theoretical wilderness, but the evidence from supernovae shows that we need it, or something very much like it, to understand the world we live in. This is no longer a matter of esthetics or introspection or stubble from Occam's razor. We need to learn to live with Λ.

Of course, Brian Schmidt's horror made us take extra steps to be certain that the small extra dimming of distant supernovae was not due to some other effect. If somebody was going to find a flaw in this work, we thought it would be best if we did it ourselves. So we tried hard to see if we could show our own result was wrong, or misguided, or if we had missed some important source of error that was not described by the statistics of the data points.

We knew it wasn't Malmquist bias. Malmquist bias selects the brightest objects near the limit of a survey. But we weren't seeing supernovae that were extra bright, we were seeing objects that were extra dim. But there is more than one way to go wrong.

We know that when we look to redshift 0.5, we're loo [illegible]
about one-third of the way to the Big Bang, about 5 bill [illegible]
So the stars will all be 5 billion years younger. Does ag [illegible]
difference to supernova properties?

We know that the universe has grown richer in heavy [illegible]
partly through the action of all the supernovae that have [illegible]
in the past 5 billion years. Does chemistry make a dif [illegible]
supernova properties near and far?

And we know for sure that many astronomical in [illegible]
have come to a bad end by misunderstanding dust. Cou [illegible]
old dust, not acceleration, make the distant supernovae [illegible]

These are serious questions to which the answers [illegible]
complete. Our job now is to examine these possibili [illegible]
they have misled us into the temptation of ascribing to [illegible]
an effect that truly belongs to evolving stellar population [illegible]
ing chemical composition or dirt.

As for the ages of stars, we know that galaxies today [illegible]
citizens with distinct demographics. Elliptical galax [illegible]
little current star formation, so all the stars are old, like [illegible]
tion of an Arizona retirement community. In contra [illegible]
irregular galaxies often have very active star form [illegible]
more like Ann Arbor, a town full of boisterous you [illegible]
well as a quiet older population. Those galaxies hav [illegible]
including massive stars that blow up as SN II in m [illegible]
5 billion years. They also have a quiet population [illegible]
that putter around while the young stars live fast, die [illegible]
leave a beautiful neutron star corpse. So different typ [illegible]
galaxies provide places to study the effects of a young [illegible]
lation of stars.

Interestingly, type Ia supernovae have been foun [illegible]
of galaxies. It is worth looking to see if the SN Ia in s [illegible]
there is recent star formation, differ from the SN Ia [illegible]
where there is not. That would provide a clue to whe [illegible]
back in time makes a difference in the brightness of the [illegible]
From the Calán/Tololo data plus the CfA data, we hav [illegible]
up a set of over 50 well-observed supernovae in nea [illegible]