DRAFT
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Then and Now

I have been informed that this symposium is intended to be "forward-looking", rather than retrospective. I will in this talk violate that constraint. My recollections of Sid go back in time further than those of Persis. Very few, if any, other persons in this room can make that statement. So I will begin with some reminiscences, before moving on with some physics babble, which will overlap in part with subject matter in the talks following mine.

I. Bj and Sid at MIT (1955-1956)

Our first encounter was often recalled by Sid himself. There I was in the back of the classroom while Sid was lecturing on atomic physics (or maybe classical mechanics---I do not recall clearly). Sid noticed me, because I was clearly not paying attention to his words. He wandered down the aisle for a closer look, and found me doing homework for other classes. Nevertheless I aced the exams. That was enough for Sid to label me as a person of interest.

My interpretation of this is that, by my junior year, I had discovered the mountains via the MIT Outing Club. Every weekend we members would pile into the back of a hearse, together with members from the girl's schools in the area, and head off to the White Mountains for hiking, rock climbing, canoeing, etc. Getting homework done in Sid's class was from that point of view very useful. Sid of course gave very clear lectures. But, as best as I can recall, he stuck close to the textbook. For me it was more efficient to read the book than listen to Sid.

My strongest memories of that era, however, were the informal evening seminars for physics majors that Sid held in the living room of his house, across the river from MIT. He and others on the MIT physics faculty really cared about us undergraduates. I not only obtained a close acquaintance with Sid that way, but also with co-sponsor Fred Zachariasen, who later on wrote with Sid an excellent monograph on the electromagnetic form factors of the nucleons.

II. Bj and Sid at Stanford (1956-1963)

When I was debating whether I would go to Stanford or to Harvard (where I had a generous offer of scholarship aid), I had a chat with Fred Zachariasen on the way back to MIT from one of Sid's evening seminars. Fred was horrified by the Harvard prospect: for me to become a Schwingerian was a fate worse than death. The fact that I still vividly remember that encounter probably implies that Fred had some influence on my choice of Stanford.

In any case, in 1956 there was indeed a remarkable emigration from MIT to Stanford. In addition to Sid, Fred, and me, there was nuclear physicist Charlie Schwartz, along with postdocs Burton Richter and Henry Kendall. I in fact had already encountered Burt at MIT. He did his PhD thesis on their 300 MeV synchrotron, while I was doing a senior thesis in the same lab. Mine was on neutral K's, with Bernie Feld as supervisor. BNL emulsion data on the K decays were being analyzed there by Dave Ritson. He invited me to have a look at the pion tracks through his microscope. After five minutes of eyestrain, I decided that if this were experimental physics, I wanted no part of it. Anyway, it would not be long before Dave followed us out to Stanford.

As you have already heard, Sid came to Stanford as a QED skeptic, with ideas for a program for testing its predictions at short distances (at that time "short distance" meant proton size or less). He was, I am sure, influenced by his close friend and mentor, QED pioneer Viki Weisskopf. Before long, Burt Richter was putting together an experimental test. And half of my thesis, under Sid, was doing tedious calculations on the theory behind Burt's experiment. Sadly, there was no aid from the primitive computers of the day, and Sid, Steve Frautschi, and I did all the calculations in triplicate.

The Stanford physics program responsible for much of the 1956 MIT exodus had to do with its electron linac and the electron-nucleus elastic scattering program led by Bob Hofstadter. Inelastic scattering from nuclei also was in the game. Sid and Charlie Schwartz worked on that in 1958. And that work at the nuclear physics level helped in shaping the SLAC deep-inelastic scattering program. I can say that the importance of the process in my own mind was influenced by their work. The possibility of seeing constituent structure within the nucleon was recognized by us all. The obstacle was the need for a relativistic description. By now, thanks to QCD, that has arrived. But it was not easily attained.

In the early 60's, when I was a postdoc, Sid asked me to coauthor The Books. I agreed, even though I got advice that this was not what postdocs should be doing. But I wanted to learn field theory better, and there was that pleasure-factor of working closely with Sid. During the creation of the books, I was mostly on the road, and it was easy to procrastinate (I consider myself an expert in that field.). Finally, Sid laid down the law, and during one summer I was placed in voluntary confinement in the tower of Drell House, only to be released upon the completion of the books. This sentence was of course not all that unpleasant, and I treasure those memories of being a temporary extension of the remarkable Drell family circle.

III. Bj and Sid at SLAC (1963-1979)

It was not too long after the first appearance of the SLAC proposal that Sid joined Project M (M for Monster). I had the opportunity to stay at Stanford or to join the project, and opted for SLAC almost simultaneously with Sid. The decision was for sure a difficult one for Sid, because it meant leaving students and teaching behind. There were in those days quite contentious relations between the physics department on campus and SLAC regarding "the right to teach" and "the right to have students". But Sid opted to support Pief and the physics, at the possible expense of losing much of his contacts with teaching and with students. As it turned out, Sid answered that challenge in a very satisfying way.

Project M was housed in a big warehouse on the Stanford campus. Half of the warehouse was a machine shop, and the other half was cubicles. While we were there, Sid worked up the "Drell Process". It was a mechanism of hadron photoproduction which was instrumental in the design of secondary beams. They in turn supported a significant part of the future experimental program. It is also worth mentioning that two of the cubicles were occupied by promising theory-group candidates we failed to hire. One was John Bell, who created Bell's Theorem in the din present in the Project M warehouse. The other was Tini Veltman, who created much of his computer program Schoonship there.

By the late 1960's we moved out to SLAC. And it did not take all that long before the personality of the SLAC theory group was created, thanks largely to Sid's influence. Seminars were characterized by informality. Sid had a way of asking questions that appeared to be hopelessly naïve. But after a round of discussion, his follow-up questions would be anything but naïve. These encounters energized the rest of us to also dare to ask dumb questions.

This informal atmosphere was most apparent in the November Revolution of 1974, in the days following the discovery of the Ψ at SLAC's SPEAR storage ring. The theory conference room was the venue of a nonstop series of presentations and discussions. There was a remarkable sharing of intellectual property. No one seemed to be eager to rush his or her precious ideas into print. Everyone felt part of a big adventure, with the prize simply being the understanding of what this discovery really meant.

Sid also was busy doing theory during the seventies. He was especially energized by the important results of the deep-inelastic scattering program. One result was the "Naïve Drell-Yan Process" I love the name. Naïve Drell strikes again. The formula is very simple and the descriptor is in my opinion very appropriate. I personally had thought about that process, and had concluded that the correct description would be more subtle and complex. It took a while for me to realize that their "naïve" version was in fact quite robust.

IV. From the Then to the Now (1979-2017)

From 1979 to 1989, midlife crisis, plus an attractive invitation from Leon Lederman, sent me off to Fermilab. By the time I returned to Stanford, Sid had moved more into his public service role. I moved gradually toward gravitation and cosmology. Therefore I will fast-forward to 2017 and, in the remainder of this talk, express a few opinions on avenues of research which I believe deserve a greater level of attention. Needless to say, these avenues do not include weak-scale supersymmetry or string theory.

My theme will be that one's choice of descriptive language is often important. It was, I believe, Sid (via Herbert Goldstein's excellent textbook) who taught me that the royal road from classical mechanics to the quantum theory was not via F = ma , but rather via the nonlinear partial differential equations which determine Hamilton's Principal Function.

V. General Relativity

The classic language of GR is Riemannian: the behavior of the metric tensor $g_{\mu\nu}$ provide the raw material of the theory. But alternative languages are out there. My own choice, "gauge gravity", goes back to the early days of GR (Elie Cartan). Distinguishing features include the following:

1) Inclusion of spin ½ particles in GR require use of this formalism.
2) Gauge gravity is an O(3,1) gauge theory. It is more closely related to EPP than is the Riemannian version.
3) The 10 components of $g_{\mu\nu}$ are replaced by 16 vierbein components and the 24 components of the gauge potential, generally called the "spin connection:

   vierbein: e_μ^A spin connection: ω_μ^{AB}

   $$g_{\mu\nu} = e_\mu^A \eta_{AB} e_\nu^B \qquad R_{\mu\nu}^{AB} = \{\partial_\mu \omega_\nu - \partial_\nu \omega_\mu + [\omega_\mu, \omega_\nu]\}^{AB}$$

4) The MacDowell-Mansouri version of gauge gravity is an O(4,1) extension, where the vierbein and spin connection are synthesized. They comprise together the forty components of the O(4,1) gauge potential A_μ. The Lagrangian of this version is almost pure topological:

   $$A_\mu^{5A} \sim \sqrt{\Lambda}\, e_\mu^A \qquad A_\mu^{AB} \sim \omega_\mu^{AB}$$

   A, B, CD = 0,1,2,3

   $$\mathcal{L} \sim 10^{120} \int d^4x \, \langle \Phi^5 \rangle F_{\mu\nu}^{AB} F_{\lambda\sigma}^{CD}$$

5) The reduction of the MM action into the O(3,1) components does yield a topological term in addition to the Einstein term and a cosmological-constant term (required by the formalism!):

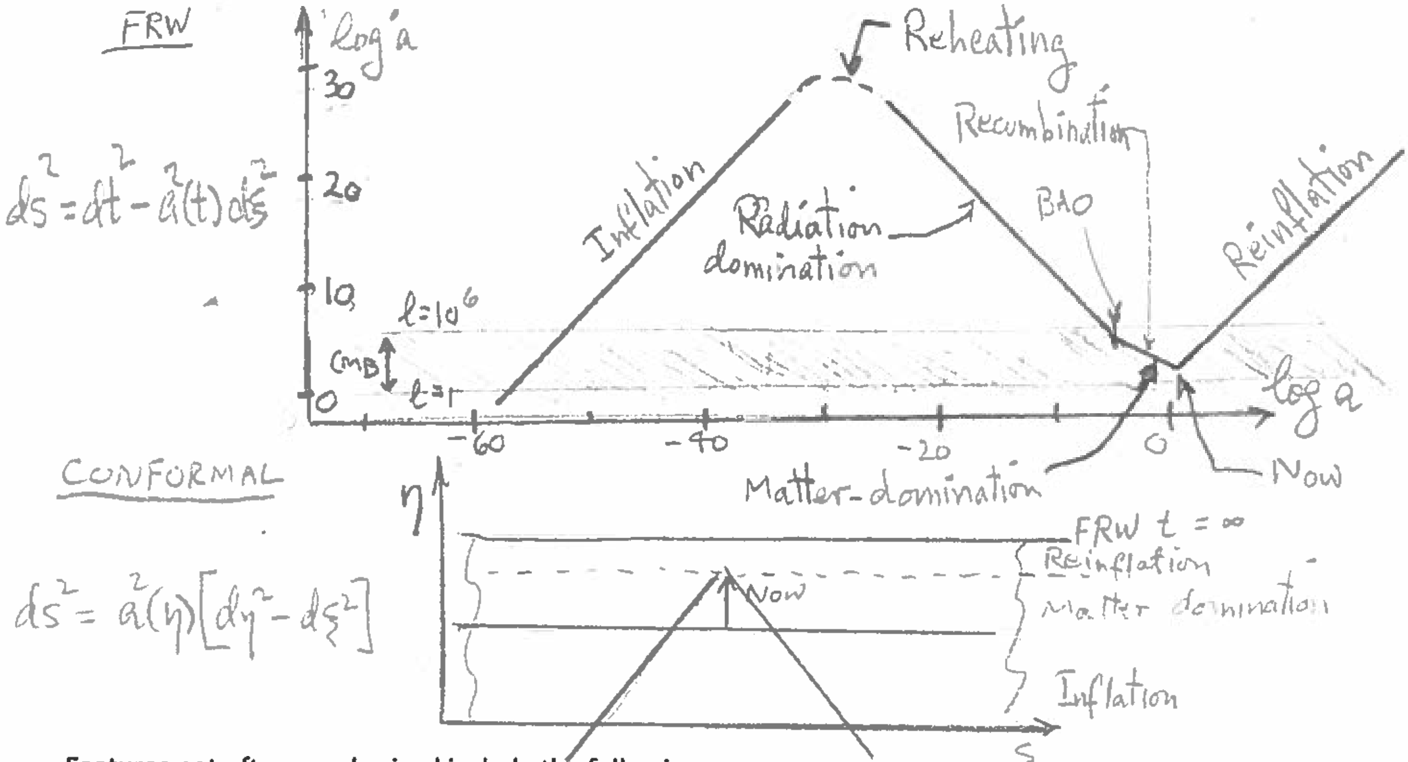
$$S_{MM} \sim \overset{\text{(Gauss-Bonnet)}}{\frac{M_{pl}^2}{\Lambda}\int RR} + \overset{\text{(Einstein-Cartan)}}{M_{pl}^2\int eeR} + \overset{\text{(Cosmological constant)}}{\Lambda M_{pl}^2\int eeee}$$

I dub this Gauss-Bonnet topological contribution "darkness". It appears to be closely linked to the (necessary) appearance of dark energy in the formalism.

VI. Cosmology

The FRW description of cosmology is, at the math level, gloriously simple. The physics content is gloriously rich. After all, it is nothing less than a broad-brush history of the entire universe.

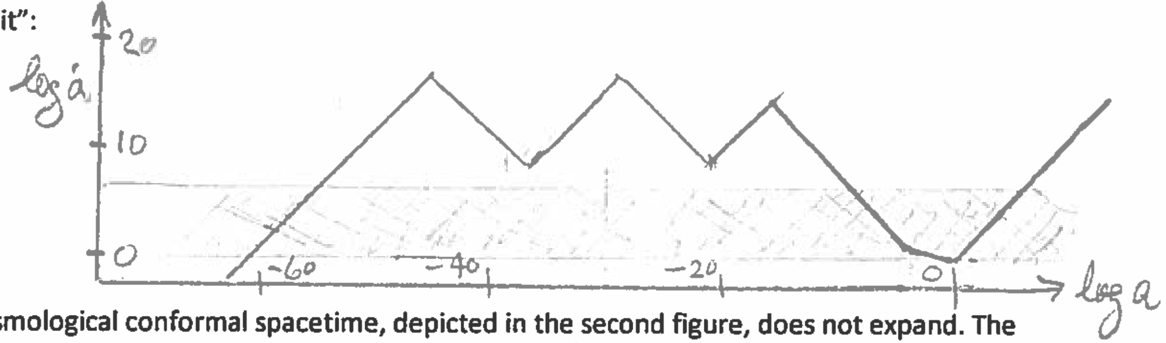
My personal, simplistic summary of cosmology is not commonplace in textbooks or in the literature. It consists of two pictures:



Features not often emphasized include the following:

1) CMB data are relevant only in the shaded ("foothill") region. Information from a specific CMB multipole ℓ only affects the expansion along a horizontal line at elevation ℓ in the first figure.
2) No experimental data exist to constrain the properties of the "summit" regions in the first figure. This is especially the case for the phenomenon called "reheating".
3) The magnitude of the inflationary dark energy, which determines the location of the left-hand "ridge" in the first figure, is uncertain by many orders of magnitude. Inflationary dark energy (usually expected to be of order GUT scale) could be as low as the TeV scale without creating direct difficulties with existing experiments.

4) There in principle might be more than one “reheating” event, leading to more than one “summit”:



5) The cosmological conformal spacetime, depicted in the second figure, does not expand. The evolution of conformal time is very slow except where $\dot{a}$ is minimal:

$$d\eta \equiv \frac{dt}{a(t)} \quad \Rightarrow \quad \frac{d\eta}{d(\log a)} = e^{-(\log \dot{a})}$$

VII. Dark Energy

We no doubt will all agree that the dark-energy problem is a very profound one, affecting in fundamental ways our views of elementary particle physics, of cosmology, and of general relativity. It may well take a very long time to understand the true nature of dark energy. Nevertheless, it is an ideal problem for an octogenarian like myself to work on.

For me, the aforementioned “darkness” is as central a quantity as is dark energy. As I already noted, within MacDowell-Mansouri gauge-gravity cosmology, it expresses itself as the Gauss-Bonnet contribution to the action. Its Lagrangian is a total time derivative:

$$S_{GB} = 2\pi \int dt \, \frac{dN}{dt}$$

$$\text{“Darkness density”} \equiv \frac{N}{V} \sim \left(\frac{M_{pl}^2}{\Lambda}\right)\left(\frac{\dot{a}}{a}\right)^3$$

During inflation, the darkness density is essentially constant and is very large. During radiation-dominated or matter-dominated expansion of the universe, the darkness density decreases. In our reinflationary future, the darkness density should remain constant. It is curious that it is now of the order of the QCD scale.

So I have been searching for other ways of connecting this darkness density to dark-energy density. And just within the last few weeks, I encountered a novel, new avenue to pursue. It came via a new paper by Alejandro Perez and Daniel Sudarsky (arXiv 1711.05183).They suggest that,

1) Planck-scale granularity of spacetime is what drives the creation of dark energy.
2) Pre-existing dark energy is extinguished at reheating, with conversion into radiation.
3) The purported spacetime granularity only couples to EPP degrees of freedom containing a fundamental mass parameter.

4) The master equation that drives all of this is

$$\frac{d\Lambda}{dy} \sim \frac{R^2}{M_{pl}} \sim \frac{(\rho - 3p)^2}{M_{pl}^5}$$

5) Analysis of this expression leads to the conclusion that the present-day dark energy was created during the electroweak era, when the temperature of the primordial plasma was of order 100 GeV. The consequent prediction is, very roughly

$$\Lambda \sim M_{pl}^2 \left(\frac{M_{EW}}{M_{pl}}\right)^7 \sim M_{pl}^2 \left(\frac{10^2}{10^{19}}\right)^7 \sim 10^{-119} M_{pl}^2$$

This provocative result is enough to keep me quite busy nowadays.

I have a website: bjphysicsnotes.com. For more details on the above material (except of course for the Perez-Sudarsky idea, which is brand new), please see the entries "Hawaii Lectures, Chapter XII" and "Darkness: What Comprises Empty Space?"