TOP TURNS TEN: A FENCE SITTER'S VIEW

Gordon Watts
University of Rochester, 1989-1994 (**CDF**)
Brown University, 1995-1999 (**DØ**)

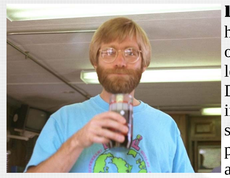
Yes, I know that isn't the Run II logo — I couldn't find it!!!

I say **FENCE SITTER** because I've been a member of both the CDF and DØ collaborations. Don't let anyone fool you — the competition between the two can be very intense. As a graduate student I was on CDF. I got my Ph.D. in 1994 with Paul Tipton at the University of Rochester. My thesis was "*EVIDENCE FOR TOP QUARK PRODUCTION IN ANTI-P P COLLISIONS AT S**(1/2) = 1.8-TEV*". I was hired as a post-doc on DØ by Brown University. I switched experiments about 4 months before the Discovery papers were submitted to PRL. The result was I was listed as an author on both the CDF discovery and evidence paper, but not on any of the DØ papers; I'd just not been working at DØ long enough to earn my wings.

Since I spent many of my "formative years" on CDF I to this day have trouble differentiating the two experiments. For example, at CDF the Silicon Vertex Detector is called the SVT. This detector was crucial to the top quark discovery. DØ didn't have a Silicon detector in those days, but we do now. It is called the Silicon Microstrip Detector (SMT). I constantly get those two things confused. Making it worse, in DØ there is an algorithm called SVT!!
How different are the two experiments? Not that different. People make up the experiment and imbue it with a personality and there are strong personalities in both experiments that have helped to shape their character. But both have lots of smart people, and in the end, that is how the physics gets done.
I've considered returning to CDF twice — once when I was searching for a job as a professor in 1999 as well as a time more recently. I'm sure I would have been both happy and productive had I returned. And I definitely miss working with a lot of the people in CDF. At the same time I'm very happy to have my DØ colleagues! In the end I have liked working on both experiments. Inertia gives me a slight preference for DØ right now but both were comfortable homes.

PEOPLE
CDF

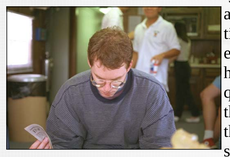
On CDF I was a graduate student at the University of Rochester. I was Paul Tipton's first graduate student. Rochester had hired him during my second year. We worked in a pretty close-knit group: Paul leading, with Brian and Richard as post-docs, and then me as a graduate student later joined by Philip and Kirsten. The below pictures are from 1993-1994 — so right during the run-up to the Evidence paper.



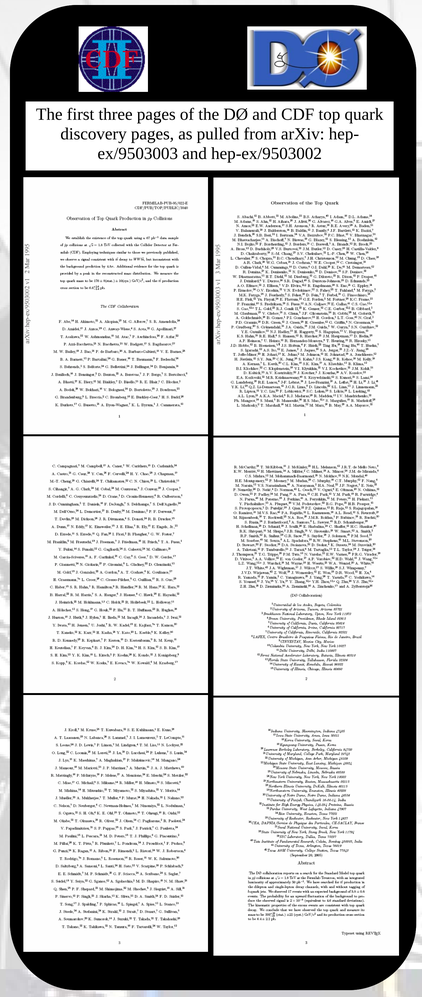
PAUL TIPTON, the only professor at Rochester working on the top quark. He had negotiated time off from teaching so he could be out at Fermilab continuously. By the time I'd arrived I think he had already hired Brian and Richard. At least, I don't remember a time when the two of them weren't working with us.
During the actual analysis he kept track of the Monte Carlo as well as keeping in touch with the other big players on the analysis. As a graduate student I sometimes got the feeling there were two levels going on. Those that I and the post-docs saw and lived in and then a world he and Tony Liss and others operated in. This was a high profile analysis and certainly had politics surrounding it — most of which I'm quite grateful to Paul for shielding me from!

BRIAN WINER was the elder post-doc in the Rochester Group. Looking back, I honestly think he was the *glue* that held things together. He was the one that kept the production jobs running, the data skimming. And he was the guy that carefully cross checked the numbers and kept the rest of us honest. I'm no longer willing to admit how many of my mistakes he saved me from stating publicly!
He was also the guy than ran the CDF NCAA basket ball pool. He had a program, written in FORTRAN (of course) that would calculate the odds for everyone's pick. And update them as the new scores came in. It was a master piece, statistically. I think he got yelled at for spending too much time on that only once… He is now a professor at Ohio State, and looks a little different…

RICHARD HUGHES was the younger post-doc in the Rochester group. Constantly bursting with ideas, he was always wondering "Why not!?" as in —why can't we do it this way!?! Sometimes he was right and Brian and Paul would hold him back — and someone else would then implement his idea (which must have sucked). He had (and has) boundless energy. I remember him doing a lot of the quick studies to test out a new cut or a new cross check. And then working with Brian to get them in the final version of the analysis. The two made an excellent team and I'm not surprised that they continue to work together — Richard is also a professor at Ohio State (and looks different!).



WEI-MING YAO, now a professor at Berkeley, was a post-doc during the evidence and discovery phase. He was working on b-tagging at the time — identifying b-quarks by looking for evidence of their long life-time. While Paul Tipton and the rest of worked on the lepton+jets channel, I remember **TONY LISS** working on the di-lepton channel, and Claudio Campagnari. Groups would play golf; by not playing I wonder what analysis discussions I missed?

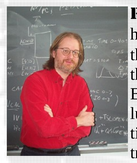


The first three pages of the DØ and CDF top quark discovery pages, as pulled from arXiv: hep-ex/9503003 and hep-ex/9503002

PEOPLE
DØ

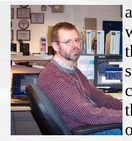
I was a member of DØ for only 4 months before the top quark discovery paper was submitted. While I did do work on the analysis it was relatively minor. At that point people on CDF were my friends and I was getting to know the people on DØ. As such I have no pictures from that era on DØ, and many of my memories of the people at that time are hopelessly confused by later years of getting to know them as friends and collegues

One of the most colorful characters of the time was **BILL COBAU**. Always lively, I remember him leading the lepton+jets search. I clearly remember one meeting we were discussing something that was a distraction. I think he cut off and buried the conversation in one word. Bill left the field a few years later — and gave a well attended public talk on what was wrong with HEP. And named names. It was quite a talk (UTeV, on getting (or not) a job). "Fell on his sword…" was a common comment… he is missed.



RICH PARTRIDGE (that is a recent picture of him), one of the professors at Brown where I was hired, also played a large role in the top discovery along with his post-doc **JEFF BANTLEY**. Jeff was the senior Brown post-doc and was running the dilepton portion of the search. Jeff was pretty much the nicest guy ever, and tall as all heck. I'll never forget my first conversation with Rich, however. Both he and Dave Cutts, the other professor at Brown, interviewed me separately. With Rich it was a lunch trip. The interview was just after the Evidence paper from CDF had been released. DØ, at that time, saw nothing. The first words when I met him for lunch were "I don't believe your thesis". I tried to look at it as a free meal at that point and was quite surprised when I got the job offer. I think I've learned as much from Rich about being a physicist as I did from my advisor, Paul.

HERB GREENLEE was a post-doc at the time — now on the scientific staff at Fermilab. He is very quiet (almost shy) and very smart. If it got noisy or someone started protesting during his talk he would remain quiet waiting until the other person finished. I also remember that about 90% of the time he was right, and I think he knew it, so he didn't mind giving the person interrupting his talk enough rope to hang themselves. He was also the author of the most amazingly complex Excel spreadsheet. We used this to calculate the final cross section — it contained everything — all the raw numbers, the statistical errors, the systematic errors, etc. And because he was talking about the cross section he would always be last on the agenda. The people feeding him numbers as inputs would always go first. Of course, they would change the numbers between the last email and the talk and so Herb would have to start off his talks with "This was accurate 3 hours ago…" I always thought that was unfair — it should have been him first.



SCOTT SNYDER, a graduate student at SUNY Stony Brook, would always give talks in his bare feet. He was ahead of the curve (as always); he'd designed an extension to FORTRAN that made it object oriented. You got the feeling from listening to Scott that no calculation and no software was too complex for him to write in a night. And he was there many many late nights.

JIM COCHRAN and **MEENAKSHI NARAIN** are hard to forget, though I know them so much better now than I did then; it is almost impossible for me to think of them in terms of the actual discovery other than they were very active and weren't sleeping. The stories....

C++ VS. FORTRAN

Actually, there was no *vs.* back then. C++ wasn't on the horizon! But boy was it different...

- We used **DEC MINI-VAXES** and the VMS operating system. This is still, by far, my favorite OS. The guy that wrote it went on to write Windows NT which certainly does not have the following in HEP physics that VMS did! DEC has long since ceased to exist. *Such a shame*.
- **UNIX** (not Linux) made its first appearance at the end of this period — and during the run-up for the Discovery paper. Thought I'm not 100% sure, I don't think Unix took part in any of the analysis for the Evidence paper.
- It is interesting to note that today both experiments have almost totally exterminated **FORTRAN** from their software libraries. In general, the only bits left are the Monte Carlo generators, written by the theorists — who are generally a bit slow in this sort of thing (a joke!!). Though the decision has been made, there is still debate over this — ATLAS has more FORTRAN code in it that either DØ or CDF!
- Data was still stored on **MAGNETIC TAPE**. Today we have tape robots; in general less than 30 seconds after you ask for a tape it is mounted and data is flowing. Back then there were operators that would bring a tape from the tape storage room and mount it by hand. If you had a batch job that used tape you'd have to watch it closely and if it was hung waiting for a tape for a while you'd make a call to the Computer Center to make sure the operator had seen your jobs tape request. A lot of late nights were spent watching for this!
- **COLOR SCREENS** were only just becoming inexpensive enough to purchase in large numbers, so very few of us (in particular, the graduate students!) didn't have them. And the monitors were huge! But we did have multiple windows!
- **ETHERNET** was shared 10-base-T, not switched 100-T as it is today.
- There were no **LCD PROJECTORS** back them. Everything was printed on plastic — mostly black&white. This meant that many times the talks weren't consistent because the numbers from one talk hadn't been fed to the author of the second talk in time!
- **PORTABLE LAPTOPS** and **WIRELESS** weren't. The result was everyone paid attention during meetings instead of answering email or submitting jobs.
- **DSL AND CABLE MODEMS** hadn't been invented: there were many more people physically working late at DØ and CDF buildings than there are now.

THE MEETINGS

Almost all the action happened in the meetings. Though everyone was a heavy user of email the real discussions, the brain storms, the "Oh my goodness!" moments — all would happen in meetings.

First, the **MEETING ROOMS**. At DØ the big one was The 9th Circle. For CDF it was The Blackhole. Fitting names for both the comfort and the amount of time spent within. While the Blackhole is still going strong, the 9th Circle has been retired and now serves as extra office space. Both of these rooms are designed to hold between 40 and 60 people. In the thick of the analysis, however, often more than 60 would attend. Seats were first-come-first-serve. It was not uncommon to see some of the senior professors sitting on the floor for 3-4 hours crossing and uncrossing their legs. Graduate students often were late because they were busy preparing slides, which meant...

Think of it — 60 physicists, mostly male, packed into a room designed for 40 for 3-4 hours. That *designed for* includes the A/C system. You didn't really notice what was happening until you left for a bathroom break. On return you'd be hit be a wall of human sweat. Ugh. Getting yourself back into the meeting room was difficult sometimes. This was a bit worse in the Blackhole, which was inherently more air tight than DØ's 9th Circle, build out of a double-wide trailer. But, the 9th Circle had direct access to the outside, so people would stand outside the door, with it partially open, smoking while watching talks. They would take the last draw on their cig, walk inside and back to their seat while exhaling. You'd expect people to complain — but both the co-spokes people of the experiment (Paul Grannis, father of DØ, and Hugh Montgomery) were smokers…

There are a few meetings that are etched in my memory. In talking to others I've discovered they may not have occurred the way I remember them. The **EVIDENCE MEETING.** At CDF I was in one of the top meetings. It was an important pre-conference meeting. Each analysis had a small excess, but nothing that couldn't be called a fluctuation — in short, an excess that could be ignored. Towards the end of the meeting, a wise old man, whose name and face I'm afraid I've forgotten, raised his hand and said words to the effect "I think we have a problem. If I add up all the excesses across all the channels I think it is more than 3 sigma." Meaning — it was significant and there could be top there! The room was dead silent as all of us were attempting to run through the numbers on our own. All the results for the conferences were pulled. I don't think CDF put out a new top quark search result for almost a year — until the Evidence paper went out. I suspect I'll never attend another meeting like that in my career. You could almost feel the tingles crawling up your spine.

THE ALIGNMENT MEETING. An analysis like the search for the top quark requires a huge amount of infrastructure. B-quark identification, one of the key technologies that allowed the Tevatron detectors to find the top quark, is a particularly complex bit of infrastructure. First step is getting the Silicon detector aligned. This is very difficult to do — we needed to know the positions of the detector planes better than we could measure them before we installed them. This means we had to account for every possible deformation and then fit to real data to determine how much the detector had warped or sagged. Except we forgot one thing: when we put together the mathematical model we forgot that that the detectors could be offset radically. Ops. I clearly remember the meeting where Wei-Ming announced that he'd figured that out. It sticks in my mind because that is something that anyone could have figured out — even me as a graduate student. Yet it stumped about 40 or 50 of us for almost three months.

THE DØ INDUCTION MEETING. I have very few memories of the pre-discovery meetings at DØ. Except for one. It was my first top meeting. Pretty much everyone seemed to know who I was, though I didn't know anyone there. I'd basically walked out of CDF the week before, and walked into DØ. Boaz, one of the top group conveners, pulled Rich Partridge aside and asked if I could be trusted… Hopefully, I've measured up...

GREG AND THE BEACH

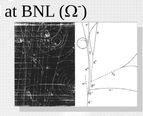
I'll never forget when Greg Landsberg **INTERRUPTED A DATE** of mine to complain about the top Evidence analysis. In the Summer of 1994 I went to the NATO school for high energy physics in St. Croix. This particular one was right after the Evidence paper had been released. Both DØ and CDF graduate students were attending. Bill Carithers (Berkeley) was one of the current CDF co-spokespeople and came down to the summer school to deliver 4 lectures on the evidence paper and why everyone should believe it.
He was asked questions repeatedly by the DØ graduate students. I got a lot of the questions as well. Unfortunately, I didn't have a very global view on the analysis and there were a number of questions I couldn't answer directly.

But there is one set of Z evening — which is always gorgeous in St. Croix. I was sweet on one of the women attending the summer school and had managed to get a set of beach chairs and a bottle of wine. We were sitting on the beach enjoying the wine and the sunset when… up marches Greg Landsberg. He plunks himself down and starts to talk about what a farce the CDF work is and laments, at the end, that people in CDF have even managed to pull the wool over the eyes of the graduate students (i.e. me!). I'm not sure how he missed the bottle of wine, but it certainly added something *je ne sais quoi* to the evening (don't

THIS IS THE MOST RANDOM POSTER I'VE EVER WRITTEN. In fact, I wasn't planning on doing it at all. But the more I thought about it the more I realized how much the hunt for the top quark shaped who I am as a physicist — even down to the subject matter that now interests me.
I've been amazingly fortunate in my career. I've got a tenured position at the University of Washington, I'm a member of the DØ and ALTAS collider experiments. I can't help but wonder how different it would have been had I'd worked on a different analysis when I was a student. In a sense, I think I've got the top quark search and discovery to thank for opening a lot of doors for me.
This poster is a collection of thoughts and memories. In that sense it is very subjective. I was a graduate student when this happened; **there is no way I remember all details** correctly — especially given my normally faulty memory. So please don't get mad if I've left something out or got something wrong! I maintain a blog for Quantum Diaries (http://qd.typepad.com/4) and I find myself writing this poster in much the same style as that blog.

1964

Strange Quark Discovered at BNL (Ω^-)

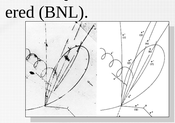


1966

I was born.

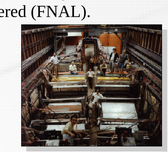
1974

Charm Quark Discovered (BNL).



1977

Bottom Quark Discovered (FNAL).

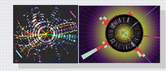


Me, 1977

Me, 1991

1995

Top Quark Discovered!



Higgs?
SUSY?
Extra Dimensions?
Nothing!?

THE FUTURE — SHOULD YOU WEAR SHADES?

I was very lucky to see one discovery in my lifetime. It was an amazing coincidence that I was a student working on the top quark search when it was discovered. Will I be so lucky again?

Since 1995 a lot has happened in the world of physics. We've discovered that there is Dark Matter and Dark Energy. We've further constrained the Standard Model to the point where we think the Higgs should be with in reach of either the Tevatron, or, more likely, the LHC. If SUSY or some other theory is to help with EWSB it is likely we will see evidence for it before the LHC is done.

So it is likely that we will see something as important — if not more important — than the top quark in the next 20 years. But it won't be the same. I'm not a student and my relationship with the experiments I work on has fundamentally changed. I'll miss that.