Lessons on doing science from my father, Gerry Brown

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June 30, 2016

Dr. Gerald E. Brown was a well known nuclear physicist and astrophysicist who worked at Stony Brook University from 1968 until his death in 2013. He was internationally active in physics research from the late 1950s onwards, ran an active research group at Stony Brook until 2009, and supervised nearly a hundred PhD students during his life. He was also my father.

It's hard to write about someone who is owned, in part, by so many people. I came along late in my father's life (he was 48 when I was born), and so I didn't know him that well as an adult, but I did grow up with a senior professor as a father, and so he had a tremendous impact on not only my personal life but also my scientific career. Now that I am a professor I recognize this even more clearly.

Gerry (as I called him) didn't spend much time directly teaching his children about his work. When I was invited to write something for his memorial book, it was suggested that I write about what he had taught me about being a scientist. I found myself stymied, because to the best of my recollection we had never talked much about the practice of science. When I mentioned this to my oldest brother, Hans, we shared a good laugh because he had the same experience with our father!

Most of what Gerry taught me was taught by example. Here are some of the examples that I remember most clearly, and of which I'm the most

proud. While I don't know if either of my children will become scientists, I can think of few better scientific legacies to pass on to them from my father.

Publishing work that is interesting (but perhaps not right) can make for a fine career.

My father was very proud of his publishing record, but not because he was always (or even frequently) right. In fact, several people told me that he was somewhat notorious for having a 1- in-10 "hit rate" – he would come up with many crazy ideas, of which only about 1 in 10 would be worth pursuing. However, that 1 in 10 was enough for him to have a long and successful career. That this drove some people nuts was merely an added bonus in his view.

Gerry was also fond of publishing controversial work. Several times he told me he was proudest of the papers that caused the most discussion and collected the most rebuttals. He wryly noted that these papers often had the largest numbers of citations, even when they had turned out to be incorrect.

The best collaborations are both personal and professional friendships.

The last twenty-five years of Gerry's life were dominated by a collaboration with Hans Bethe on astrophysics, and they traveled to Pasadena every January until the early 2000s to work at the California Institute of Technology. During this month they lived in the same apartment, worked together closely, and met with a wide range of people on campus to explore scientific ideas; they also went on long hikes in the mountains above Pasadena (chronicled by Chris Adami, [1]). These close interactions not only fueled his research for the remainder of the year, but also were a deep personal friendship. It was clear that, to Gerry, there was little distinction between personal and professional in his research life.

Science is done by people, and people need to be supported.

Gerry was incredibly proud of his mentoring record, and did his best to support his students, postdocs, and junior colleagues both professionally and personally. He devoted the weeks around Christmas each year to writing recommendation letters for junior colleagues. He spent years working to successfully nominate colleagues to the National Academy of Sciences. He supported junior faculty with significant amounts of his time and sometimes by forgoing his own salary to boost theirs. While he never stated it explicitly, believe that he considered most ideas somewhat ephemeral, and that his real legacy – and the legacy most worth having – lay in the students and colleagues who would continue after him.

Always treat the administrative staff well.

Gerry was fond of pointing out that the secretaries and administrative staff had more practical power than most faculty, and that it was worth staying on their good side. This was less a statement of calculated intent and more an observation that many students, postdocs, and faculty treated non-scientists with less respect than they deserved. He always took the time to interact with them on a personal level, and certainly seemed to be well liked for it. I've been told by several colleagues who worked with Gerry that this was a lesson that they took to heart in their own interactions with staff, and it has also served me well.

Hard work is more important than brilliance.

One of Gerry's favorite quotes was 'Success is 99inspiration', a statement attributed to Thomas Edison. According to Gerry, he simply wasn't as smart as many of his colleagues, but he made up for it by working very hard. I have no idea how modest he was being – he was not always known for modesty – but he certainly worked very hard, spending 10-14 hours a day writing in his home office, thinking in the garden, or at work. While I try to work fewer hours myself, it has always been clear to me that sometimes hard work

is a necessary when tackling tricky problems: for example, the Earthshine publications (e.g. [2] came after a tremendously unpleasant summer working on some incredibly messy analysis code, but without the resulting analysis we wouldn't have been able to continue the project.

Experiments should talk to theory, and vice versa.

Steve Koonin once explained to me that Gerry was a phenomenologist – a theorist who worked well with experimental data – and that this specialty was fairly rare because it required communicating effectively across sub disciplines. Gerry wasn't attracted to deep theoretical work and complex calculations, and in any case liked to talk to experimentalists too much to be a good theorist – for example, some of our most frequent dinner guests when I was growing up were Peter Braun- Munzinger and Johanna Stachel, both experimentalists. So he chose to work at the interface of theory and experiment, where he could develop and refine his intuition based on competing world views emanating from the theorists (who sought clean mathematical solutions) and experimentalists (who had real data that needed to be reconciled with theory). I have tried to pursue a similar strategy in computational biology.

Computers and mathematical models are tools, but the real insight comes from intuition.

Apart from some early experience with punch cards at Yale in the 1950s, Gerry avoided computers and computational models completely in his own research (although his students, postdocs and collaborators used them, of course). I am told that his theoretical models were often relatively simple approximations, and he himself often said that his work with Hans Bethe proceeded by choosing the right approximation for the problem at hand – something at which Bethe excelled. Their choice of approximation was guided by intuition about the physical nature of the problem as much as by mathematical insight, and they could often use a few lines of the right equations

to reach similar results to complex computational and mathematical models. This search for simple models and the utility of physical intuition in his research characterized many of our conversations, even when I became more mathematically trained.

Teaching is largely about conveying intuition.

Once a year, Gerry would load up a backpack with mason jars full of thousands of pennies, and bring them into his Statistical Mechanics class. This was needed for one of his favorite exercises – a hands-on demonstration of the Law of Large Numbers and the Central Limit Theorem, which lie at the heart of thermodynamics and statistical mechanics. He would have students flip 100 coins and record the average, and then do it again and again, and have the class plot the distributions of results. The feedback he got was that this was a very good way of viscerally communicating the basics of statistical mechanics to students, because it built their intuition about how averages really worked. This approach has carried through to my own teaching and training efforts.

Benign neglect is a good default for mentoring.

Gerry was perhaps overly fond of the concept of 'benign neglect' in parenting, in that much of my early upbringing was at the hands of my mother with only occasional input from him. However, in his oft-stated experience (and now mine as well), leaving smart graduate students and postdocs to their own devices most of the time was far better than trying to actively manage (or interfere in) their research for them. I think of it this way: if I tell my students what to do and I'm wrong (which is likely, research being research), then they either do it (and I suffer for having misdirected them) or they don't do it (and then I get upset at them for ignoring me). But if I don't tell my students what to do, then they usually figure out something better for themselves, or else get stuck and then come to me to discuss it. The latter two outcomes are much better from a mentoring perspective than the former two.

Students need to figure it out for themselves.

One of the most embarrassing (in retrospect) interactions I had with my father was during a long car ride where he tried to convince me that when x was a negative number, -x was positive. At the time, I didn't agree with this at all, which was probably because I was only 7 or 8; I was also rather stubborn. While it took me a few more years to understand this concept, by the time I was a math major I did have the concept down; but in this, and many other interactions around science, he never browbeat me about it or got upset at my stupidity or stubbornness. I believe this carried through to his interactions with his students. In fact, the only time I ever heard him express exasperation was with colleagues who were acting badly.

A small nudge at the right moment is sometimes all that is needed.

A pivotal moment in my life came when Gerry introduced me to Mark Galassi, a physics graduate student who also was the systems administrator for the UNIX systems in the Institute for Theoretical Physics at Stony Brook; Mark found out I was interested in computers and gave me access to the compute system. This was one of the defining moments in my research life, as my research is entirely computational! Similarly, when I took a year off from college, my father put me in touch with Steve Koonin, who needed a systems administrator for a new project; I ended up working with the Earthshine project, which was a core part of my research for several years. And when I was trying to decide what grad schools to apply to, Gerry suggested I ask Hans Bethe and Steve Koonin what they thought was a promising area of research – their unequivocal answer was "biology!" This led to me applying to biology graduate schools, and ultimately to my current faculty position. In all these cases, I now recognize the application of a light touch at the right moment, rather than the heavy-handed guidance that he must have desperately wanted to give at times.

Conclusion

There are many more personal stories that could be told about Gerry Brown, including his (many, and hilarious) interactions with the East German secret police during the cold war, his (quite bad) jokes, his (quite good) cooking, and his (enthusiastic) ballroom dancing, but I will save those for another time. I hope that his friends and colleagues will see him in the examples above, and will remember him fondly.

Acknowledgments

I thank Chris Adami, Erich Schwarz, Tracy Teal, and my mother, Elizabeth Brown, for their comments on drafts of this article.

References

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