

AK Memories – J. Angrist; March 2020

Alan Krueger died a year ago this week. I think of him often, and would like to mark the anniversary of his death by sharing a story of my time with Alan.

Alan and I had a lot in common. First, we were both born in the third quarter; that is, we're QOB 3s. In fact, Alan was only one day older than me. We used to joke that this explains his relative success: it's an age effect!

I wish now that our tracks had crossed as often recently as they once did. Alan's track brought him from the Harvard Ph.D. program to an assistant professor position at Princeton. Mine ran the other way. But our intersections were more than professional. In Fall 2016, we met for pizza on the Upper West Side and discussed the common difficulties faced by my father Stan and Alan's uncle Bob. Stan and Bob suffer from post-polio syndrome, which is as hard to treat as it is rare. These two remarkable survivors swapped coping strategies as a result of that bit of personal collaboration between son and nephew.

I remember Alan as a friend, collaborator, and occasional academic rival. Alan's competitive streak was most visible in his tennis game. He was a strong, aggressive player. I don't play tennis, and only once made the mistake of accepting Alan's challenge to a game of ping pong. But we both liked to compete. Our relationship in the 1990s was characterized by tension as well as warmth and respect. Later, when Alan became a major public figure, I sometimes missed or avoided opportunities to talk. My mistake.

Alan and I wrote 8 papers together. Our lives were most entwined from the late 1980s through 2001, when we completed our final collaboration. Our last paper reviewed recent progress in applications of instrumental variables (IV) methods, and was published in the *Journal of Economic Perspectives*. We were proud of the essay and the evolution in empirical work to which we felt we had contributed.

Princeton vs. Harvard

Alan arrived in the Princeton Industrial Relation Section in the fall of 1987, where he grew deep roots. Orley Ashenfelter, one of my thesis advisors, had identified Alan's promise as the result of a chance encounter between Alan and Ginna Ashenfelter, Orley's late wife. Alan sat next to Ginna as they flew to the ASSA meetings.

When Alan was on the job market, I was a 3rd year Ph.D. student happily ensconced in the Princeton Industrial Relations Section. The Section was led by Orley and staffed by Dave Card and a rotating crew of their students and coauthors. My office mates and I knew we had landed in the masters' workshop. This feeling sometimes engendered a little hubris. At first, I sided with the Princeton junior hiring committee, which had initially passed on Alan. But Alan's charm and intellect soon won me over. And, by 1989, I was set to become a Harvard man too.

AK91

In 1988, Alan and I embarked on the project that yielded our 1991 compulsory schooling paper using quarter of birth (QOB) to instrument schooling in a wage equation (published in the QJE) and a companion piece on two-sample IV published in '92 (in JASA). This project began as a discussion of identification strategies for the effect of World War II military service on earnings (my thesis used draft lottery number to instrument Vietnam veteran status). The WWII study was our first joint working paper, released in 1989, but published (in the Journal of Labor Economics) only in 1994. Alan and I had discovered that WWII veterans were drafted in birthday order, and so an IV identification strategy was born. But it remained to discover exactly which endogenous variable this birthday instrument was to be an instrument for!

AK91 and AK92 were a hit. We were sometimes invited to present this work together, and were doing that at U Penn when my wife Mira went into labor with our first child in March 1989. How did our spectacularly fruitful collaboration begin? We were lucky to live and work in an applied micro incubator. In those days, faculty and students would clock long hours in the Section, discussing ongoing projects and potential projects. Ashenfelter, Card, Krueger, IRS grad students—Janet Currie, John DiNardo, Tom Lemieux, Brian McCall, Sheena McConnell, and Steve Pischke, among others—and many visitors, would spend most of the day in the basement of Firestone Library (some, like Orley, on the afternoon/evening shift). I'd take a family dinner break around 5:30 and come back. Dave Card never left.

Alan and I circled around various identification strategies and how to make the most of them. We worried about coherence and contradictions within and between projects. The WWII paper had been undermined by the compulsory schooling paper, since quarter-of-birth (QOB) affects schooling as well as veteran status. But this became an opportunity for us to adjust IV estimates for omitted variables bias, and we enjoyed grappling with that too. In 1990, we applied for an NSF grant (my first) to fund work on the QOB project and on a project using draft lottery numbers to construct instrumental variables estimates of the returns to schooling.

BJB

The NSF projects evolved in response to other work. We learned of the initially terrifying weak instruments problem in 1993, when it was reported by Alan's Harvard classmate and coauthor, John Bound, and then-grad-students, David Jaeger and Regina Baker. It had long been known that two-stage least squares (2SLS) estimators are biased. So what!? The BJB paper showed that the fact that 2SLS estimators are biased towards the corresponding ordinary least squares (OLS) estimates was relevant for AK91, which relied on many weak instruments to improve precision. This threatened our agenda since the big AK91 finding was that 2SLS and OLS estimates of the returns to schooling are similar. The many-weak story broke when I was back in Israel on furlough from military reserve duty (I taught at Hebrew U from 1991-96). Tired and anxious in an isolated outpost, I couldn't have been more miserable.

It was Alan's idea to do a "garbage instruments" experiment to prove BJB wrong. This experiment uses random numbers with the same marginal distribution as the real instruments in a 2SLS procedure that mimics ours. Great idea, Alan! In June 1993, we withdrew an accepted paper that used draft lottery dummies to instrument schooling. The estimates this yielded were pure finite-sample bias; there was just no first stage. But soon enough, BJB morphed into an inspiration for creative solutions to weak-IV problems. Some of this work was done with my friend and longtime collaborator Guido Imbens, as well as with Alan. Happily, when these solutions were tried, the AK91 findings mostly held up.

The Joy of Research

That period of trepidation, excitement, and understanding was a high point in my life, and I believe in Alan's as well. I remember the joy in our work, the exhilarating sense that we were on to something useful and interesting.

Alan made many important contributions beyond those I'm describing here, some perhaps more enduring than the work with me. He was more eclectic than I am and interested in data collection for its own sake, always eager to nail down the facts. His work with David Card and Larry Katz on the minimum wage was innovative by virtue of original data collection as well as by applying new research designs to a classic question. Writing our "Empirical Strategies" *Handbook of Labor Economics* chapter in the late 1990s, Alan insisted we draft sections on data collection and measurement error. I recall Alan's excitement and pride at having established a survey research center in Princeton. I still prefer to get my data in the mail (or email).

Our intellectual paths diverged later partly because of Alan's expansive curiosity and enthusiasm for questions that I didn't see as exciting. Perhaps I was unable to keep the perfect from being the enemy of the good, even when Alan's version of "good" was very good indeed. My undergraduate econometrics reading list includes 8 Krueger papers, only one with me.

Early on, Alan schooled me in the art of clear, engaging expository writing. Clarity came naturally to him, but he thought it could be learned and was ready to teach. My students today may be interested to know that: (a) I had not previously considered writing important and (b) Alan didn't have to tell me twice. I was motivated partly by fear of being out-classed by Alan, even as we worked together. Later, I wasn't surprised to see Alan writing for the New York Times and other standard-setting outlets. Unlike most academics who dive into popular media, however, Alan remained committed to rigorous empirical scholarship.

Like me and many other economists of our generation, Alan was an assimilated Ashkenazi Jew. My economist's view of religion is that it's a coping strategy, meant to help people manage the inevitable challenges posed by the pull of desire and the pain of loss. Jewish teaching forbids us from responding to loss with solipsistic reverie or crippling regret. Better by far to dwell together on Alan's many contributions and insights, from which his friends, collaborators, and rivals have benefited so much.

AK stop for lunch in Jerusalem on a walk with their boys (in 1992 or 1993)

