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Time Past, Time Present, Time Future

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When I was a young man in my mid-twenties, and a new graduate student at Harvard shortly after the second World War—in fact it was 1948, the year of Fred Skinner's move to Memorial Hall from Indiana University—I took special delight in listening to the old-timers swap stories about the early days of psychology. I remember one conversation in particular in the long corridor outside the seminar room at Memorial Hall when E. G. Boring was introducing a visitor, Wolfgang Köhler, to the department's graduate students. Boring told Köhler of the days when he was E. B. Titchener's graduate assistant at Cornell. One of his duties was to stand at the door of the lecture hall where Cornell undergraduates were gathered waiting for Titchener's class. As the professor strode by making his entrance, he would hand young Mr. Boring his cigar, which Boring would then snuff out and hold throughout the lecture, handing it back to the old man to light up as soon as the class had ended. It seemed to me a perfect story about the subservient status of graduate students everywhere.

And now with the fullness of time, here I am, having myself become an old-timer, swapping stories about the old Columbia department; about Keller and Schoenfeld's rat-pack on the second floor of Schermerhorn Annex, and above all about Fred Keller who, praise-God, is still around to hear us acknowledge how deeply he affected our lives in the decades between 1940 and 1970 when all of us were young and touched with fire.

We are beginning to recognize that the period we spent together in Schermerhorn Hall was a uniquely golden era. The work we did then spawned or greatly advanced at least three of the dominant trends of modern experimental psychology: The experimental analysis of behavior; sensory psychology, especially vision and audition; and the mass flow of information accompanying the detection of weak signals, the first stirrings of the study of computer-like cognitive processes. Although ours was an era spanning the ascendancy of Keller and Schoenfeld's *Principles of Behavior,* Clarence Graham's election to the National Academy, and, so help me, the rise of mathematical psychology as distinct from psychometrics, we did not really understand how good our stuff was. I remember talking about the state of the Columbia department in the late 1950s with Fred Keller, Nat Schoenfeld, Clarence Graham, and Connie Mueller. It seemed to us then that we were all working in the cruel shadow of the department's former greatness.

Woodworth was still alive, in his 80s, living in scandalous extra-marital bliss with the former psychology librarian, a lovely lady I will identify only as "Mrs. T." Garrett, Poffenberger, and Warden, all recently retired, seemed to us to be towering older figures. As we studied their earlier work it did not strike us as particularly outstanding, and we sought ways to convince the administration—Ralph Halford, the Dean, and Jacques Barzun, the Provost—that the department deserved better support. Instead they fractured us into two departments: Psychology and Social Psychology. Time and historical perspective have healed all these ancient wounds. The departments remarried a few years afterwards, and the Keller-Graham era has now emerged in its proper superior relation to the Woodworth-Poffenberger period. All of us now understand how privileged we were to be there at a crucially important time, and to have worked under the control of such powerful stimulation. It was 18 hours a day and 126 hours a week, and it was the best time of my life.

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For me the perspective is even more sharply etched than for others. You see, I went back to Columbia in the winter of 1969 while the university was still totally preoccupied with the conflicts and the factionalism that had overwhelmed it in 1968. However you may view those days, you have to understand where I am coming from. In the spring of 1970 there was a very tense meeting between representatives of the junior faculty and me in which they told me of the manifold ways in which Columbia exploited and oppressed them, and they threatened me with collective bargaining. Fred Keller had retired by then, but the shaping accomplished by my time in Schermerhorn Hall was so powerful that I told the junior faculty representatives at that meeting in the spring of 1970 that while collective bargaining was their legal right if they sought it, they would have a hell of a fight on their hands. Let's have no more rhetoric about exploitation. I intended to treat them fairly. Columbia's future was in their hands, but I could also be a pretty resourceful adversary. I had been a junior faculty member at Columbia in the 1950s. I doubted there was anything they could teach me about the conditions of junior faculty life at Columbia that I did not already know.

I remember walking back to my office after that tense meeting recognizing that a fundamental change had taken place. Political tensions and moral outrage had so shattered the bonds of loyalty and trust that historically unified the diverse parts of the institution, the entire structure would have to be rebuilt. That realization only amplified my nostalgia for the days when Clarence Graham and Fred Keller were the senior members of the Psychology Department, and I regarded their approval and acceptance as the central goal of my life.

I came into the department from MIT in 1956, a very junior assistant professor. Graham was then chairman, or as we say now in non-sexist language, "Chair"; or as he used to say then, "Executive Officer," because he viewed the title "chairman," as unacceptably authoritarian. Fred Keller was a father figure even then; white haired, benign, gregarious, and addicted to Players cigarettes, which he lighted up from their distinctive tin box in what I can only describe as an impressive tribal ritual.

It could hardly be said that Fred and I were ideological soul-mates. I was captured by the conviction, which governs me even to this day, that stochastic processes offer a key to understanding the foundations of many processes in behavior not limited at all to the forms of sensory information processing that I was studying. Fred was, shall we say, modestly skeptical of that position. Nevertheless, he liked having heretical ideas around the department. We argued a lot, always pleasantly, and he displayed genuine interest, leavened with occasional puzzlement, in these arcane stochastic mechanisms: Yule Furry processes, general gamma processes, counting processes, birth, death and immigration processes; the mechanisms that really turned me on.

Fred would remind me of the hot water Skinnerians got into when they drifted too far from the basic cumulative record and became enmeshed in constructs such as the reflex reserve. But he sent his students to take my courses and he asked me to serve on his doctoral dissertation committees. It was valuable training for me and I learned plenty.

And we had much in common. Experience had soured me on statistical learning theory. At the time I felt that Bush and Mosteller and their students were caught in a cul-de-sac. I wanted to understand the stochastic methods they had developed, but to apply them to different problems. Keller agreed with that. He thought perhaps such ideas might contribute to sensory information processing but not to the analysis of behavior as he understood it. I do not think either one of us foresaw the potential of statistical learning theory for guiding the development of instructional software.

In any event, the warm relation that sprang up between senior professor Fred Keller and junior professor Bill McGill led to a generation of Columbia Skinnerians who were competent in the dis-

ciplines that both of us taught. I hesitate to name names, but their work has given a level of quantitative sophistication to the analysis of behavior that constantly delights me when I read today's journals.

Keller was a member of my Ad Hoc committee at Columbia. In fact he presented the case for tenure on behalf of the department in 1958. These things were supposed to be secret, but no great intelligence was required to learn what was going on in the Ad Hoc committee. I would meet Fred in the elevator in Schermerhorn and then worry for hours afterwards as to whether he seemed cooler than usual when speaking to me. If you can imagine Fred Keller as cool and distant, you are really anxious; and I was. He, of course, brought me through safely without a scratch. I was at last accepted into the little society formed by the department's executive committee, its permanent members, and that was everything I sought from life in 1958.

It did not last. In 1964 an offer came from the University of California, San Diego, and I simply could not turn it down. It provided an opportunity to construct a psychology department much more closely tuned to my own interests. I left for California a little teed off with the department over the disputes that are inevitable as you age and grow, and yet not realizing fully the privilege I had been given in serving at Columbia during a remarkably precious time. I was also worried about the city and Columbia's relation to it. And I was 43 years old, somewhat depressed because I could see my whole life stretching out ahead of me and somehow it wasn't good enough. I wanted fresh air and a fresh start. These things happen to us. It is the classical mid-life crisis. How could I have known that I would arrive in California just 3 weeks prior to the Watts riot, or that my appointment would be approved by the Regents on the day, the very day, of the "capture" of a police car in Sproul Plaza at Berkeley by thousands of radical students who sat down around it and prevented it from moving? How could I have guessed that a heretofore obscure figure, an actor, Ronald Reagan, would declare himself as a candidate for governor, and then run brilliantly against the civil unrest reflected by Watts, and by what he declared to be "the mess" in the University of California?

It was all a masterpiece of bad timing. Not long afterward Reagan was elected governor of California. The Regents dismissed the President of the university, Clark Kerr, in January 1967. Several of the Chancellors resigned and I was elected chairman of a faculty search committee to find a new chancellor at San Diego just as Martin Luther King was murdered in Memphis.

Columbia blew up in April 1968, and then Robert Kennedy was killed, shot dead at the Ambassador Hotel in Los Angeles while campaigning for the presidency in June 1968. All these extraordinary events, none of them even remotely anticipated when I left New York three years earlier, brought the University of California to its flash point as I sought to persuade professional administrators to come to La Jolla and be our chancellor. There were no takers. At the request of the new president of the university, Charles J. Hitch, I agreed to serve for a year beginning in August 1968.

Reagan abstained when the Regents elected me. He was suspicious of someone so close to the faculty. That got me off to a splendid start. Even the most committed radicals knew that if the governor was opposed, I could not be all bad.

And so I started off my contracted year in good condition, but things deteriorated rapidly. I have written (1982) about that incredible year, the Year of the Monkey in the ancient Chinese calendar. I will not bore you with an elaborate recapitulation. I still cannot quite fathom how we got through it. When you endure a violent or intense experience, you never really understand how things happen. You learn simply to hang on from day to day and if you are lucky, you get through it. The Regents told me at the end of the academic year in June, 1969 there was no way in which they would let me step out of the chancellor's job after what we had all been through together. Again, when people pass through

an intense ordeal together, deep currents of mutual loyalty and friendship are created. They are not easily put aside. I agreed to stay on.

Then just before Thanksgiving in 1969, Bill Cumming called from New York telling me not to be surprised if I should receive a call from the Columbia Trustees. They had learned about what happened the previous year at La Jolla and were interested. Our romance was brief and passionate. Ron Breslow, a chemist on the Columbia faculty and a very great man, told me it had become a moral question. I really had no choice. Take it or be responsible for inflicting a mortal wound on Columbia. I took it and then tried to explain my moral dilemma to the Regents.

Of course, in the end everything turned out well. Things had changed a good deal from those golden years in Schermerhorn in the 1950s, but we got through it. We began the slow, patient process of reconstruction. The task will never be completed in my lifetime but the machinery is in place, morale is restored, trust is there once more; and of course Columbia has no spiritual problem that half a billion dollars will not cure. Indeed, as we discovered when the university began slowly to turn around, even the money is there.

But there is a special point to this recitation bearing on Fred Keller. Some of us participating in this symposium live outside the strict ideological boundaries of the experimental analysis of behavior. All of us have had unusual, even peculiar careers. But I am sure you have already divined the thread of continuity that Don Cook perceived when he organized the symposium. We were all profoundly affected by Keller and Schoenfeld, and by their principles of behavior. We were affected when we were young and highly impressionable. What we learned then sustained us in remarkable ways as we confronted a changing technological environment, and the challenges of life.

In my case the most serious challenge arose when I was first forced to confront mass-scale unrest. I was not a combative person; quite the opposite. Threatening crowds frightened me. My instinctive inclination was to go somewhere and hide until things calmed down. But that could not be done. The first time I stood my ground with an angry crowd and tried to talk sense to them was truly an overwhelming experience. I have written about that in *The Year of the Monkey* (1982), and I invite you to read it if you are curious. It happened in La Jolla during 1968–69.

In any event, after the first few such confrontations, the fears began to fade and to be replaced by an analytical curiosity. What could be going on? I began to read avidly about crowd behavior, starting with the turn of the century work of Le Bon (1897) and Sigmund Freud (1921/1959), to Feuer's contemporary *Conflict of Generations* (1969) setting forth the now-familiar Oedipal explanation of youthful rebellion. None of these works was satisfying because none offered a plausible analysis enabling the reader to predict what might happen next in a volatile crowd situation.

I went through the literature on Ghandian protest which offered some insights but mostly wrong tactical advice. The best current source here is Light and Spiegel (1977). Then there were the sociological papers popular in the late 1960s offering either a game theory analysis of crowd behavior (e.g., Skolnick, 1969), which I found charming and essentially worthless, or slanted accounts of protests arguing that the protesters were morally right and therefore justified (see, e.g., Oglesby, 1969). All that is well and good unless you happen to be on the other side. When the protest is directed against you, what are you supposed to do? Give up? In fact the only glimmer of insight I found in this search of the literature was an extraordinary account of the English revolution, the Cromwell era, written by the Princeton historian Lawrence Stone (1965). He saw something important and he guided me.

With this single exception I was forced to discover what was occurring by studying crowd behavior itself in a disciplined way. I did it for more than 10 years, encouraged by my early exposure to Skin-

ner, Keller, Schoenfeld, Cumming, and a host of their students, and by the way I saw them analyzing reinforcement contingencies and response correlations.

Let me tell you what I discovered. It does not take long to explain at least in its most superficial form and it does have some significance for interpreting the crowd scenes in the news from the People's Republic of China. What I am describing is not science but scientific conjecture that may one day become science if analysts will only cease identifying with or against crowds and try instead to learn what is going on.

I found that the central fact about a demonstrating crowd is the existence of an elevated emotional state, usually, but of course not always, anger. The natural supposition was that the crowd is angry *about* something but generally this proved not to be the case. The emotion is diffuse. Everyone is diffusely excited. A sustained emotional atmosphere on campus, generated by local or national problems, seemed to give rise to this elevated emotional state, but at least in the beginning it is quite unfocused. If you ask people what is going on you will hear a variety of grievances. Even where some flagrant incident has occurred, it will be interpreted in diverse ways. The key point is that people are excited and upset, often angry and hostile, but typically they do not know or at least cannot say exactly what to do about it. Two people arguing may produce major changes in the grievances each one feels. This emotional chaos is nowhere more evident than when you ask people what should be done. Everyone proposes an idiosyncratic solution, some wild, some sensible, and no two individuals seem to agree on how to go about making things better.

Such diffuse or chaotic excitability is the formative matrix out of which all other things flow. When tensions are created by continuous and escalating irritations in the environment, diffuse anger or excitement is like a temperature phenomenon. When it is high, trouble brews easily. Crowds form readily. Rumors take on the appearance of reality and suggestibility is elevated. When the emotional temperature is low, crowds fail to form. It is then difficult to generate any sustained action. Apathy prevails.

But diffuse anger is typically unfocused. It generates brief outbursts that are truly dangerous, but no coordinated political action. The crowd requires a leader, or a leadership elite, figures who suddenly emerge to focus the anger; to say, as Mario Savio did to the students at Berkeley or as Mark Rudd did at Columbia, what is wrong and what should be done about it. The appearance of such a charismatic leader is a crucial moment in the evolution of an angry crowd into a political instrument. When that moment arrives, you, the president of the university, must be there, not somewhere else. That is when you can, and indeed if you want to affect the outcome, you must, inject yourself into the crowd dynamics. I was never able actually to take a crowd away from its leadership elite, but I was able to force them to debate, to introduce other arguments, other facts, and to suggest other directions. Typically it ended in stalemate rather than victory, but stalemate was usually enough to get us through another day, and that in the final analysis is what is really decisive in dealing with conflict.

So when you see large crowds in Tiananmen Square waving banners printed with meticulous care, and presenting spokesmen to answer questions of the Western media in English; or when you see clearly articulated political demands escalating from day to day, you cannot avoid noting how different such organized pressure is from the diffuse chaotic excitement of a spontaneous crowd. You then look for the charismatic leadership figures. These are the people who tell what is wrong and what to do about it. Such figures have not yet revealed themselves publicly in China, but my analysis tells me they are there. The media game is to make the demonstration seem entirely spontaneous, as though everyone rose up in a mighty surge determined to restore democracy to China. Not quite that simple, I fear.

Now, of course, I can hardly blame these primitive conjectures on Fred Kel-

ler. But he did in fact stimulate the curiosity about behavior that gave birth to them. Fred knows he bears this responsibility. I told him so when he blew into the president's office at Columbia one day in the mid-1970s on holiday from Brasilia.

Keller is now 90. I first met him in March 1956, more than 33 years ago. We worked out friendly terms of association long ago and we have stuck to it through all the intervening years. Neither of us has ever spoken an angry word to the other. But, in truth, I did not know what it all meant or how precious the association was, until many years after Fred Keller had retired. I have come to full comprehension only after much suffering and much experience with life.

When I first spoke to Don Cook about this presentation, I told him that I had once read some lines of T. S. Eliot expressing such feelings. The lines said in effect that we do not understand life's meaning until long afterwards. That is a challenge for Don, because he is universally well-read. He quickly found the reference in T. S. Eliot's *Four Quartets* (1971). Dear Fred this is for you:

We shall not cease from exploration
And the end of all our exploring
Will be to arrive where we started
And know the place for the first time.

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