

“strength” of the male erection. I’m not sure what the specific qualms of other editors were, but I am still pleased that the note about that “Peter Meter” got published. Psychologists need not be prissy!

The rest of my “faded images” do not involve anything having to do with editorial policy. I have fond memories of Charlie and his family and Menno playing with Billy, the Fersters’ oldest child, and Mara, the Dinsmoors’ oldest. I have good memories of get-togethers with the Dinsmoors in Bloomington,

Fourth-of-July trips to the Azrins in Anna, and visits to the Angers in Kalamazoo. There was always lots of shoptalk about the Journal and psychology in general. Those were good and busy years I spent in “India-no-place,” as Charlie called the then-undoubtedly-provincial capital city.

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REMINISCENCES, YOU SAY?

Reminiscences, you say? Yes, I have some—in fact a whole ossuary full of them, some that got there in merriment, some in despondence, some in scientific elation or frustration, and some in the dreary course of mere working days. You want those dating from JEAB’s and SEAB’s births? I’ll try to pull out a few. I won’t guarantee their chronological order, and there will certainly be missing details, but these hopefully will be supplied by more historically alert oldsters than I who will be writing their own memories for you. I’ll try to be accurate, though, in attaching the bones I do dig out, in the spirit of that happy skeletal ramble, “The head bone connected to the neck bone, the neck bone connected to the shoulder bone . . . , the shin bone connected to the ankle bone, the ankle bone connected to the foot bone, and the foot bone connected to nuthin’ a-tall”!

As I look back, the earliest event which actually foreshadowed the births of JEAB and SEAB was our first trip to Indiana. By “our” I mean the Columbia gang. It was a sort of mini-convention of people who were soon to be dubbed “Skinnerians”; a mini-parturition of a “movement” in experimental psychology

whose roots were in *The Behavior of Organisms*. The gang members’ focus was the new undergraduate psychology curriculum which we had introduced at Columbia College.

I don’t know whose idea that trip was, but I have always assumed that it was Fred Skinner’s. I do recall that it was first mentioned to me by Fred Keller, and that I endorsed the



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idea immediately and heartily while not entirely sure of just who would agree to go. When the time came, though I do not recall it that way myself, others assure me that several of our Columbia gang, as a sort of small advance party, went out by train. More Columbia-ites came later by one means or another. I could not now recite who made up the final Columbia contingent, but perhaps another reminiscer than myself will include that roster. In any case, it turned out that the Columbia group was the larger part of the conference participants, the smaller being Skinner's own following at Indiana. That fact, while I noted it, failed at the time to impress my untutored sense of history. I was more intrigued by making acquaintance with *compari* from elsewhere than Columbia. The most important person for me to see, however, was Skinner in the flesh, with the chance to observe his personal and scientific style.

At the meeting, in the usual tradition of scientific conventions, we gave our little research papers. (One day, I should like to change this fossilized and sterile format of psychological conventions; I know several improvements that can be made, but that's another story.) I don't recall the substance of any of those papers, not even my own (did I give one?). But we were mightily pleased with ourselves, which contributed to the jovial air of our social hours.

When we left Bloomington, we felt ourselves as bearers of something different in the science of behavior; we thought of ourselves as forward-looking; and, we had a new identity as an in-group, as a "movement." On the trip back to New York, I don't recall anyone voicing reservations about what had happened, none about the immediate or future consequences of our meeting, none about what the scientific continuance might be. The possibility of our having our own journal never came up, which now is interesting since it was soon to happen.

(There was a second Indiana trip a year or so later, and I mention it more as an aside than as a key part of the JEAB story. Our "movement" was by then already developing a reputation, so that this second meeting drew several "non-Skinnerians," people who had political antennae, who had heard that something was up, and whose push it was to be "up" also alongside developments which might one day be important. I remember the after-

noon one of these "outsiders"—he had been a fellow graduate student of mine, so I knew him well, but he was vague about how he had learned of this Indiana trip—met me in the gents' room and asked me, a bit quizzically and defensively, but with a conspiratorial grin to show he was a right guy, whether I understood what "they" were talking about in the meeting room, and did I "go for it" myself!)

Some time afterward, word got to me (via whom?) that perhaps we ought to have a journal "of our own." In the "movement" the idea was, as I recall, taken up warmly. Some were for it, I think, out of the high spirits and pride-of-group created by the Indiana trips; others welcomed it because they were having difficulties getting published in the standard APA journals whose editors were being sniffingly negative about the "operant" manuscripts submitted to them. While I was not having such difficulties, I foresaw a strong growth of operant research and even greater trouble for "movement" publications in the future. Largely on that account, I joined at once in endorsing the idea of "our own" journal.

My understanding at the time, so far uncontradicted to me, was that the journal idea was Skinner's, and that the title being bruited about was also his. That title, still in use, seemed to me somewhat overlong and a bit awkward, but not enough of either to urge a veto. There was a touch of proclamation and pretention in the words "experimental analysis," but not so much as to require rejection, while "behavior" was altogether fitting in our title. I think I first heard JEAB pronounced JAY-AB by Murray Sidman (did he make it up?), and the later SAY-AB for SEAB was an inevitable follower. (In his public addresses and private conversations, the meticulous J. R. Kantor afterward scorned such *sorglos* phonetics and staunchly used JEEB-SEEB. Some of us had once considered the *ee* substitutes for *ay-a*, but found them a bit *ugh-y* to the ear, and so JAY-AB and SAY-AB remained with us.)

When we got down to concrete details of the journal, one of the earliest thoughts was to have a rotating board of editors. By that, we hoped to avoid the hardening of the intellectual arteries that can so easily, and often does, beset a journal in any field, including the

sciences. We tossed about terms of three or five years (I voted for the former), but my later observation was that no matter how long an editor was in, he was usually reluctant to pass the office along, it being regarded as an "honor" or "recognition" rather than just work. For my part, even more important than term of office was what our editorial policy would be toward submitted manuscripts. I believed that we ought be broadly permissive in our acceptance and treatment of papers sent us, that we ought never put literary handcuffs on authors, that we not be punitive in our editorial reviews—in short, that we not be overbearing in our requirements for publication, so long, of course, as a paper was honest and its findings replicable. (I still think that a correct policy. Unhappily, some of our editors soon displayed the same hardening of censorious attitudes they had been quick to criticize in other journals, for example, with respect to a particular vocabulary which they insisted an author use. This exclusiveness and meddlesomeness quickly alienated potential contributors, some of whom I ranked as really gifted researchers, to the point that a few renounced JEAB as an outlet for their work. To me this has meant that thought must also be given, when a journal is founded, to the shaping of editors' behavior.)

Aside from terms of office for editors, we did not, as I recall, examine knowledgeably any of the technical details of issuing a journal. I was appalled by the blithe spirit wafting about. I did not myself know much about founding a journal, as opposed to running an already established one, but I guessed enough to be stricken with worry. In the event, I was right to worry, but the journal finally did appear, and that was all that really mattered.

The central and essential person in the inaugural production of our journal was Serena. She had been head of the "service bureau" at Columbia which did the university's smaller "cold type" printing (the publishing of volumes was done by C.U. Press). By the time the idea of our journal took form, she had left the bureau and had become a pioneer in bringing photo-offset methods out into the mass commercial printing market from the small specialized shops then using those methods on short-run jobs. She was a recognized figure in

journal production, and when she put her expertise at our disposal without charge, it gave us a mighty headstart. Her modesty, and the general unawareness in our group of her contribution, limited me to showing her on our journal's inside front cover as only our "production consultant" (for form's sake, I did ask Charles Ferster for his agreement to even this small acknowledgment). Doubtless, in the end we would have put out our journal without her, but the effort and the expense would have been not inconsiderably greater, and that would have delayed inestimably JEAB's appearance. (The threat of a higher expense was especially daunting in the face of our mite of a treasury of "interest-free loans.")

I took responsibility for various decisions respecting the journal, in part because Serena could help me avoid bad ones, in part because no one else was hankering to make them, and in part because I liked the production side of the journal more than the editorial one. The other face of a journal is, of course, the procurement and handling of manuscripts, since without anything to publish, even the most gung-ho crew of would-be journal founders must succumb. For that, attention of a different sort was needed. Charlie Ferster took up that work (I don't recall how and by whom he was chosen—or did he volunteer?), and he turned out to be happy and effective in midwiving manuscripts. (Amusingly, his distance from the technical side of our production was apparent, in his correspondence with me, in his continual references to "Sarina.")

In line with my accepted responsibility, I made a number of decisions about the journal (though I scrupulously wrote Ferster of every one in his office as "executive editor," requesting his agreement or counter-suggestion). Among these were (see the issue of Volume 1, Number 1, January, 1958): quarterly issues (I hedged against the journal's success by saying "at least four times a year" on the inside front cover); number of pages per issue; page size, column width, and single column; both margins justified; simple staple binding; and so on. Placing the printing, overseeing the product, pricing the job, and attendant production matters (including even the choice of paper and cover stock) were in Serena's hands. Serena also later put us into Listamatic, which

is a storage and retrieval system for handling, in our case, subscribers' addresses for journal mailing. Postal regulations regarding second-class matter (a great saving in journal mailing) included permissible mailing delays past our stated dates, which forced upon us a healthy promptness in getting out issues, as well as some rather noisy gritting of teeth.

I wrote the inside front cover, with Serena at my elbow dictating the "Instructions to Authors" for my rewrite. I remember composing the two sentences which I believed did, or should, characterize the journal. One of these spoke my impatience with journals that are prissy and otherwise offensive about permitting quotation of stuff from their pages. I gave advance permission to quote from JEAB, adding only a *soupeçon* of academic hauteur to still any timorousness among my colleagues: "Reproduction for scientific and scholarly purposes . . . following receipt of written request." The other and more important sentence bore on "group design" experiments, and on statistical evaluations of data such as "null hypothesis" testing for "significance"; by implication, the sentence spoke against machinations which claim to side-step classical replication, and in favor of a cumulating weight of data. It is a marginal view in modern psychology, but has always had its backers. Skinner and some of the rest of us were uneasy about the nearly universal reliance on statistical maneuvers. We felt that behavioral effects demonstrable and replicable in individual organisms could not be dismissed. Such was the origin of my declaration that JEAB was a journal "primarily for the original publication of experiments relevant to the behavior of individual organisms. [But also, to broaden our publication range:] Review articles and theoretical papers will also be considered for publication." While this sentence still appears in JEAB, it is today honored more in the breach than in the observance, and that, to my mind, is a loss worth regretting. In any case, both sentences were there to convey the openness and permissiveness (the same that I thought should govern manuscript acceptance) that were to be distinctive and novel about "our" journal.

A word about JEAB's financing. No matter how frugal we would try to be, some outlays of money had to be anticipated. It was from

Murray Sidman, I recall, that I first heard the notion, mentioned earlier, that those among us who could would advance an "interest-free loan" to JEAB. We did, and therewith made up our initial treasury. (The suggested loan was \$50 per person. In those days, especially with many of us dependent on junior-level academic salaries, that was a not inconsiderable sum. Incidentally, though meant to be temporary, to my knowledge those loans were never repaid even after JEAB became affluent. There were several reasons, some of them even good ones, for this transmutation of loans into gifts, but in my mind's ear I could hear donors murmuring *sic transit gloria mutui*.)

One raid I made into our bank balance was a disbursement on behalf of JEAB's front cover. I had scoured Columbia's libraries to see what other journal covers, in or out of the natural sciences, looked like. I put together the design, chose green on grey stock as our colors, and then had to get the title done. I turned to a friend of Serena's and mine, Melvin Loos of the C.U. Press, who directed me to a calligrapher named Ogg. I wanted something distinctive, so Ogg composed JEAB's title (as it still appears) in an original letter of his own for which I paid him \$40. This was not a small outlay given our modest bank balance, but I thought it justified because of the letter's originality and handsomeness, the length of the title, and the use in the title of both upper and lower case in both roman and italic. Also, Ogg agreed not ever to sell that letter to anyone else. Its cover is a journal's first foot forward, and I decided that an attractive impression made by JEAB was worth the extravagance of \$40. I believe it has proved so.

Let me close these reminiscences with an aside about SEAB. That worthy came into being as an after-thought. As JEAB's first issue neared, someone (who was it, anyway?) passed along word that our blessed event needed to be legitimized, that the baby needed a papa in the guise of a "publisher." A proper parent, it seemed, could be a Society whose business, even if *only* business, was publishing the journal. Presto, the "Society for the Experimental Analysis of Behavior." But nothing is ever that simple: the Society in its turn needed proper incorporation, proper officers, and some other proper things. That threatened new expenses,

legal ones this time, against which my new-fledged thriftiness (only recently bruised by the outlay to Ogg) rebelled. I made several efforts to find a well-heeled and seducible foundation which would pick up our legal tabs as *their* contribution to JEAB's contribution to scientific progress. I reckoned that one of them might have its own legal staff doing its in-house and, so to speak, its out-house, work. But I failed to raise anything save a few eyebrows in admiration for my brazenness, or possibly ingenuousness. In a pique over the failure, and having no taste anyway for legal twistings, I retreated from the problem, so that I cannot say who among us got it done, nor what it cost, nor who became the Society's first officers (except that Skinner was not one of them; he had stayed out of the organizational scene of JEAB, too, in the belief, I supposed, that it would be merely mischievous for us to be charged with being "plain Skinnerians" or

"only Fred Skinner's boys"—a charge which was promptly made anyhow).

Reminiscences, you said? There you have some. The journal prospered right from Volume 1, Number 1, after which its career was that of a rapidly growing subscription list, better-looking but costlier production methods, and fewer burdens on authors. In short, JEAB was successful. Perhaps the oldsters of the next generation will recount that career when their turn comes to reminisce proudly. The present oldsters' pride is in having founded JEAB—but that is in them, I think, venial sin.

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COUNTERPOINT

Photos and signatures show I was there at the Founding in 1957, but I'm not a good witness. Dreamlike images of meeting rooms seem correctly dated by being smoke-filled. But what was said? The SEAB incorporation papers were signed in a dimly lit office that I think had blue wallpaper. But who was there? Joe Brady, Dick Herrnstein, and I for the journal, but who else? In contrast, vivid memories of 1957 are from home: of the night, for example, that the heater-ventilator failed on the first commercial model of a "baby box," with our son inside.

Some generalities come to mind. I was involved in JEAB, of course, mainly by being in (and from) the right place at the right time. Also, I had congenial attitudes, which came from psychophysics. Functional relations from a few individual subjects? Sure; the vision journals were full of papers with complete curves from each of two or three subjects. I had no trouble getting my psychophysical papers published, but it was evident that similar work,

along behavioral lines, was having a hard time. Also, a new journal was needed to contain and give coherence to the increasing volume of behavioral work that was coming out.

So I could be enthusiastic about the Journal, though not about its awkward name. And though I was not, nor have I ever been, a behaviorist. "Movements" in science seemed rather quaint; the very idea of joining an "ism" still sets my teeth on edge. Science exhibits an astonishing variety of methods and predispositions, and I tended to stick with the advice of my remarkable, unpopular physics teacher: Respect definition, observation, sound logic, and good judgment. No proscription there of hypothesis testing, group data, statistics, intervening variables, theories, representations, or models; just know what you are about.

The Journal has succeeded notably, while maintaining to some degree a unique point of view. One source of both the success and the viewpoint was surely Fred Skinner's passion for the control of behavior. Knowing how to