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Todd L. McKerchar

Edward K. Morris

Nathaniel G. Smith

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A Quantitative Analysis and Natural History of B. F. Skinner's Coauthoring Practices

Todd L. McKerchar
Jacksonville State University

Edward K. Morris and Nathaniel G. Smith
University of Kansas

This paper describes and analyzes B. F. Skinner's coauthoring practices. After identifying his 35 coauthored publications and 27 coauthors, we analyze his coauthored works by their form (e.g., journal articles) and kind (e.g., empirical); identify the journals in which he published and their type (e.g., data-type); describe his overall and local rates of publishing with his coauthors (e.g., noting breaks in the latter); and compare his coauthoring practices with his single-authoring practices (e.g., form, kind, journal type) and with those in the scientometric literature (e.g., majority of coauthored publications are empirical). We address these findings in the context of describing the natural history of Skinner's coauthoring practices. Finally, we describe some limitations in our methods and offer suggestions for future research.

Key words: B. F. Skinner, coauthorship, scientometrics, history of behavior analysis

Over the course of his professional career (1930–1990), B. F. Skinner published 304 primary-source works (Morris & Smith, 2003).¹ In them, he founded a science of behavior—the experimental analysis of behavior

(e.g., Skinner, 1938; see Catania, 1998); “fathered” its application—applied behavior analysis (e.g., Skinner, 1953, 1968; see Morris, Smith, & Altus, 2005); and established their philosophy of science—radical behaviorism (e.g., Skinner, 1945, 1957, 1974; see Moore, 2008). The consequence was a new discipline—behavior analysis. For the most part, Skinner is solely credited for these contributions, yet in each case, he collaborated, in part, with mentors, peers, junior colleagues, and students.

This article is based on a presentation by McKerchar, Morris, and Smith entitled “Skinner's Coauthors: Who, When, and Why?” in *Studies in Skinner's Science and System: Coauthors, Journals, and Citations* (E. K. Morris & N. G. Smith, Cochairs), a symposium conducted at the 2003 meeting of the Association for Behavior Analysis. We thank Victor G. Laties for his insightful comments as the symposium's discussant.

Correspondence may be addressed to Todd McKerchar, Department of Psychology, Jacksonville State University, 700 Pelham Road N, Jacksonville, Alabama 36265 (e-mail: tmckerchar@jsu.edu).

¹ Morris and Smith's (2003) original bibliography of Skinner's primary-source publications listed 291 works overall, 289 of them published during Skinner's 61-year career (1930–1990). However, 16 more works have been discovered since then, 15 of them published over the course of his career (e.g., Skinner, 1935, 1988) and one published posthumously (i.e., Skinner, 2006). None of them, though, was coauthored. An updated bibliography is available from Nathaniel G. Smith, Department of Applied Behavioral Science, University of Kansas, Dole Human Development Center, 1000 Sunnyside Avenue, Lawrence, Kansas 66045 (e-mail: ngsmith@ku.edu).

The evidence of Skinner's collaboration is his coauthored publications (see Appendix), some of them seminal and some now canonical. (The coauthored publications in the Appendix are not repeated in the reference section of this paper.) Skinner and Heron (1937), for example, “foreshadowed modern behavioral pharmacology” (Dews, 1978, pp. 1115–1116); Estes and Skinner (1941) conducted a pioneering laboratory investigation of anxiety (see Millenson, 1967, pp. 433–455); Skinner, Solomon, and Lindsley (1954) provided one of the first systematic extensions of Skinner's science to human behavior (Rutherford, 2003); Rogers and

Skinner (1956) presaged the implications of Skinner's science and philosophy for freedom and dignity (Skinner, 1971); Ferster and Skinner (1957) made a fundamental contribution to the experimental analysis of behavior (Morgan, 2010); Holland and Skinner (1961) provided an early example of programmed instruction in behavior analysis (Morris, 2003); Epstein, Lanza, and Skinner (1981) conducted an innovative behavioral simulation of cognition (Epstein, 1984); and Skinner and Vaughan (1983) offered an original application of Skinner's science to self-management in old age (see Epstein, 1997).

The significance of these publications notwithstanding, Skinner's coauthoring practices have not been systematically described or analyzed. That is the purpose of our paper. We offer quantitative and qualitative analyses of Skinner's practices, the former through the coding, categories, and counts of his coauthored publications, the latter in a narrative of their natural history. Before turning to our methods, though, we briefly review the literature on the origins and nature of scientific collaboration and coauthorship and the related scientometric literature.² Although this literature mainly describes group-based regularities, it informed our approach to reviewing Skinner's practices as an individual. It also provides context for understanding his practices and suggests some avenues for future research.

² See *Scientometrics: An International Journal for all Quantitative Aspects of the Science of Science, Communication in Science and Science Policy* (1978 to the present). According to the journal, "Scientometrics is concerned with the quantitative features and characteristics of science. Emphasis is placed on investigations in which the development and mechanism of science are studied by statistical mathematical methods" (e.g., bibliometrics, citation analyses; see <http://www.springer.com/computer/database+management+%26+information+retrieval/journal/11192>).

Collaboration and Coauthorship

History of science. Science is a cultural, social, and individual practice. As a social practice, it includes the process of collaboration and collaboration's common product—coauthorship. The origins of collaboration lie in the Scientific Revolution (1600–1750), when artisans, craftsmen, inventors, manufacturers, and natural philosophers needed increasing assistance to act effectively on their subject matters. Collaboration became even more common as science was professionalized (Beaver & Rosen, 1979a, 1979b; Simonton, 2002, pp. 345–346). As Beaver and Rosen (1978) describe it,

"Professionalization" refers to a dynamic organizational process which led to a revolutionary restructuring of what had been a loose group of amateur and full-time scientists into a scientific community. "Professionalization" redefined how science was done, who did it, where it was done, who paid for it, what its practitioners wanted, and how they became scientists. (p. 66)

Collaboration and coauthorship provided (and continue to provide) scientists with practical and professional benefits. Collaboration provides resources such as knowledge, efficiencies, technical expertise, instrumentation, materials, specialization, personnel, facilities, and financial support (Beaver & Rosen, 1978; Katz & Martin, 1997). Intellectual, motivational, and social factors, of course, also play a role (Beaver & Rosen, 1978; Katz & Martin, 1997). Coauthorship, in turn, provides a means of assigning credit to contributions and for professional advancement. For instance, junior scientists who publish with senior scientists demonstrate their competence to peers and enhance their visibility in the scientific community (Beaver & Rosen, 1978, 1979b). In turn, senior scientists who publish with junior scientists enhance their status through sustained or increased publications and may direct their

field's future by influencing their coauthors (Beaver, 2001).

Scientometrics. The scientometric literature on coauthorship is of more recent origins. In the main, it describes a shift from an individual to a teamwork model of science over the past century, that is, from small science to big science, especially after World War II (Price, 1963). Wuchty, Jones, and Uzzi (2007), for instance, reported that the percentage of coauthored articles in science and engineering increased from 53% to 82% between 1955 and 2000, an increase that occurred in 170 of 171 subfields (99.4%). In addition, the mean number of authors per article nearly doubled, increasing from 1.9 to 3.5. As for citation rates, single-author papers were more frequently cited than coauthored papers in 1955, but by 2000, the reverse was true. In fact, by 2000, a coauthored paper in science and engineering was 6.3 times more likely to receive 1,000 or more citations than a single-authored paper.

The social sciences have followed a similar pattern. Wuchty et al. (2007) reported that coauthored social science articles increased from 17.5% to 51.5% between 1955 and 2000, an increase that was observed in all 54 subfields (see also Zuckerman & Merton, 1972). In addition, the mean number of authors per article nearly doubled, increasing from 1.4 to 2.5. In the social sciences, psychology had the greatest growth in the percentage of coauthored publications (75%). For instance, coauthored articles in the American Psychological Association's (APA) journals increased from 34% to 77% between 1949 and 1979, and the mean number of authors per article increased from 1.5 to 2.2 (Over, 1982; see also Mitchell, 1961; M. Smith, 1958). Again, these trends have been more marked since World War II (Simonton, 2002, p. 316). Although these increases may reflect changes in the patterns of acknowledgment for collaboration, in the

main, they are attributed to changes in practice (Beaver & Rosen, 1978).

Coauthoring practices not only vary across time, but also on a variety of other dimensions. For example, coauthored manuscripts are more likely to be accepted for publication than single-authored manuscripts (Gordon, 1980), to appear in subfields that are more highly funded (Price & Beaver, 1966), to be found in a field's core and most prestigious journals (Beaver & Rosen, 1979b), and to have higher impact scores (Lawani, 1986; Wuchty et al., 2007). Moreover, citations to coauthored articles are more highly correlated with salaries than are citations to single-authored articles (Diamond, 1985), although this and the other relations vary across fields. In addition, coauthoring practices vary across kinds and types of publications (Berelson, 1960). For instance, empirical publications are more likely to be coauthored than conceptual publications in science and in general (Gordon, 1980; Price, 1963), as well as in psychology (Over, 1982; M. Smith, 1958). Likewise, psychology journals that publish empirical articles (i.e., "data-type" journals; e.g., *Journal of Experimental Psychology*) are more likely to publish coauthored works than journals that publish conceptual articles (i.e., "word-type" journals; e.g., *Psychological Bulletin*; see Jakobvits & Osgood, 1967; Over & Smallman, 1973). With this as background on the origins and nature of scientific collaboration and coauthorship, we turn to Skinner's coauthoring practices.

METHOD

We identified Skinner's coauthored publications and his coauthors by consulting the latest version of Morris and Smith's (2003) bibliography of Skinner's primary-source publications. For this, we first coded the publications for their forms: journal articles (including proceed-

ings), books (including manuals and workbooks), chapters, book reviews, abstracts, and other. Second, we coded them for their kinds: empirical (i.e., data based), conceptual (e.g., history and theory), and methodological (e.g., apparatus). Third, we identified the journals in which Skinner published his coauthored works and coded them for their types: data-type (i.e., primarily empirical; e.g., *The Psychological Record*) or word-type (i.e., primarily systematic or conceptual; e.g., *American Psychologist*). Fourth, we analyzed Skinner's overall and local rates of coauthored publications over the course of his career. Throughout, we compared his coauthoring practices with his single-authoring practices.

RESULTS

Coauthors

Of Skinner's 304 publications, 35 (12%) of them were coauthored with 27 different authors. Skinner was the first author on 10 of them (29%) and the last author on 23 (66%) of them. He was the first author on nine (32%) of his 28 two-author publications.

His most frequent coauthor was William T. Heron, with whom he published six works between 1937 and 1940. William H. Morse was his next most frequent coauthor, with five publications in 1957 and 1958, followed by Robert Epstein, with four publications in 1980 and 1981. Had we excluded abstracts, Skinner's most frequent coauthor would have been Morse, with five publications; Heron and Epstein would have been next with four publications each. As for his other coauthors, Skinner published three times with Robert P. Lanza between 1980 and 1982 and twice each with James G. Holland in 1960 and 1961, Ogden R. Lindsley in 1954, and George S. Reynolds in 1962 and 1963. He published once with 20 other coauthors between 1930 and 1983: T. Cunliffe Barnes, Brand Blanshard, Crane Brinton,

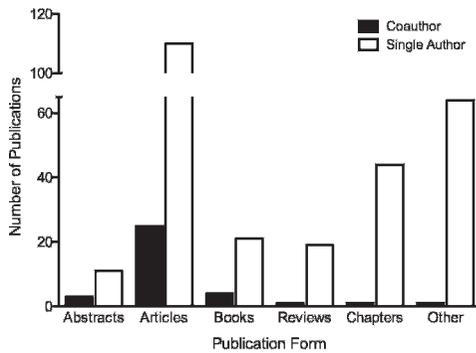


Figure 1. Number of Skinner's coauthored and single-author publications according to their publication form.

Samuel W. Campbell, A. Charles Catania, Stuart W. Cook, William J. Crozier, William K. Estes, Charles B. Ferster, Alexander Forbes, Hiram Haydn, Sue-Ann Krakower, Alfred Kroeber, Joseph W. Krutch, Elizabeth F. Lambert, Ivor A. Richards, Carl R. Rogers, Harry C. Solomon, James Starr, and Margaret E. Vaughan.

Publication Form and Kind

Form. Figure 1 shows the number of coauthored and single-author publications for the six major categories of publication forms. Twenty-five (71%) of Skinner's coauthored publications were journal articles, four (11%) were books, three (9%) were abstracts, and one each (3%) was a chapter, a book review, and an "other" (i.e., poems in a literary magazine; see Richards & Skinner, 1962). (Because they were rounded up and down, these percentages may not add to 100%.) In comparison, 110 (41%) of his 269 single-authored publications were journal articles, 21 (8%) were books, 44 (16%) were chapters, 19 (7%) were book reviews, 11 (4%) were abstracts, and 64 (24%) were "other," such as comments and discussion (e.g., Skinner, 1930, 1983a) and letters to the editor (e.g., Skinner, 1962, 1990).

Kind. Figure 2 shows the number of coauthored and single-author pub-

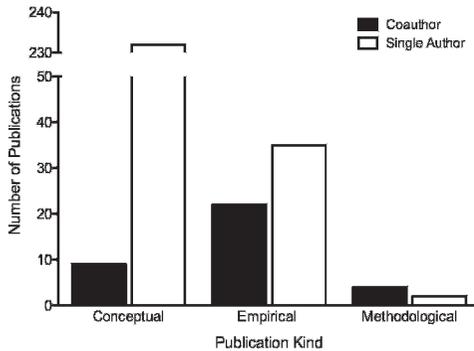


Figure 2. Number of Skinner's coauthored and single-author publications according to their publication kind.

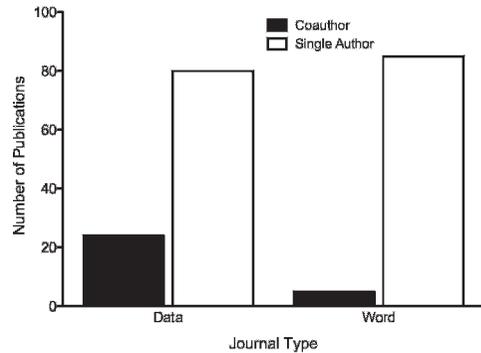


Figure 3. Number of Skinner's coauthored and single-author publications in word-type and data-type journals.

lications for the three major categories of publication kind. Nine (26%) of Skinner's coauthored publications were conceptual, 22 (63%) were empirical, and four (11%) were methodological. In comparison, 232 (86%) of his single-authored publications were conceptual, 35 (13%) were empirical, and two (1%) were methodological.

Journal Type and Journals

Journal type. Figure 3 shows that of Skinner's 29 coauthored journal articles, 24 (83%) were published in predominantly data-type journals, and five (17%) were published in predominantly word-type journals. In comparison, 80 (48%) of his 165 single-authored journal publications were published in the former, and 85 (52%) were published in the latter.

Journals. Skinner's coauthored publications appeared in 15 different journals. Six (21%) were published in the *Journal of the Experimental Analysis of Behavior (JEAB)* between 1958 and 1982, five (17%) in *The Psychological Record (TPR)* between 1937 and 1940, three (10%) in *Science* between 1956 and 1982, and two each (7%) in the *Journal of Comparative and Physiological Psychology (JCPP)* between 1947 and 1957, the *Journal of General Psychology (JGP)* in 1930 and 1931, and the *Psychological Bulletin* between 1937 and 1939. His remaining nine coauthored arti-

cles were published once each (3%) in the *American Journal of Physiology*, *American Journal of Psychology*, *American Psychologist*, *The American Scholar*, *Behaviour Analysis Letters*, *Journal of Experimental Psychology*, *Journal of Nervous and Mental Disease*, *Philosophy and Phenomenological Research*, and the *Proceedings of the National Academy of Sciences* between 1933 and 1980.

In comparison, 12 (7%) of Skinner's 165 single-authored journal publications were published in *JEAB*, two (1%) in *TPR*, 14 (9%) in *Science*, 20 (12%) in *JGP*, and seven (4%) in the *Psychological Bulletin*. Of his other single-authored articles, two (1%) were published in the *American Journal of Psychology*, 18 (11%) in the *American Psychologist*, one (1%) in the *American Scholar*, three (2%) in the *Journal of Experimental Psychology*, and four (2%) in the *Proceedings of the National Academy of Sciences*. None of Skinner's single-authored works were published in *JCPP*, *American Journal of Physiology*, *Behaviour Analysis Letters*, *Journal of Nervous and Mental Disease*, and *Philosophy and Phenomenological Research*.

Publication Rates: Overall and Local

Overall rate. Over Skinner's 61-year career (1930–1990), his 35 coauthored works were published at a rate of 0.6 per year (Figure 4, top). Ten

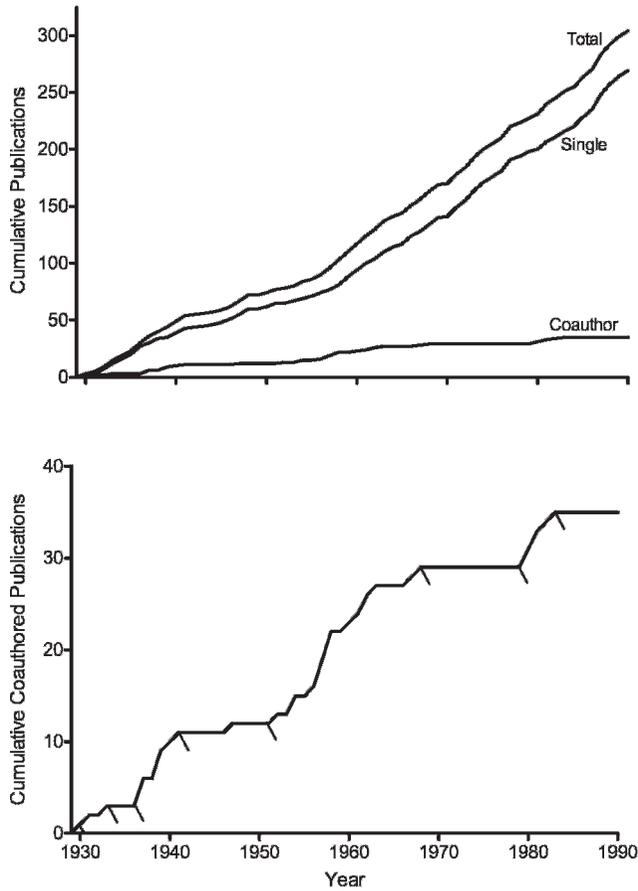


Figure 4. Cumulative records comparing Skinner's coauthored, single-author, and total scientific publications between 1930 and 1990 (top). Skinner's coauthored publications (bottom); the y axis is scaled to provide more detail in the variation across time.

(29%) were published before World War II (1930–1940), one (3%) during the war (1941–1945), and 24 (69%) after the war (1946–1983). His rates of coauthored publications during these periods were, respectively, 0.9, 0.2, and 0.6 per year. The mean numbers of authors per coauthored work were, respectively, 2.1, 2.0, and 2.3; his overall mean numbers of authors per work were 1.2, 1.1, and 1.1. In comparison, between 1930 and 1990, Skinner's single-authored works were published at a rate of 4.4 per year (Figure 4, top), 39 (15%) were published before the war, nine (3%) during the war, and 221 (82%) after the war. His rates of publishing them during these periods were,

respectively, 3.5, 1.8, and 4.9 per year.

Local rates. Figure 4 (bottom) shows a cumulative record of Skinner's coauthored publications, scaled so that variations in them can be better discerned. The downward pips in the record mark eight relatively distinct points of change in his rate of publishing coauthored works over his career. These were 1930–1933: three (9%) coauthored works at 0.8 per year; 1934–1936: no coauthored works; 1937–1941: eight coauthored works at 1.6 per year; 1942–1951: one (3%) coauthored work at 0.2 per year; 1952–1968: 17 (49%) coauthored works at 1.0 per year; 1969–1979: no coauthored works; 1980–

1983: six (17%) coauthored works at 1.5 per year; and 1984–1990: no coauthored works. In comparison, his numbers and rates of single-authored publications during these periods were, respectively, 12 (5%) at 3.0 per year, 15 (6%) at 5.0 per year, 16 (6%) at 3.2 per year, 22 (8%) at 2.2 per year, 69 (26%) at 5.8 per year, 64 (24%) at 5.8 per year, 18 (7%) at 4.5 per year, and 53 (20%) at 7.6 per year.

DISCUSSION

Over the course of his career, Skinner coauthored 35 (12%) of his 304 works with 27 different coauthors. He was the first author on about 25% of them, publishing most frequently with Heron, Morse, and Epstein. Over 65% of his coauthored publications were empirical, compared to 13% of his single-authored publications. Just over 70% of his coauthored publications were journal articles, compared to nearly 41% of his single-authored publications. And, over 80% of his coauthored publications appeared in data-type journals, in comparison to 17% of his single-authored publications. The journals in which he published his coauthored works most frequently were *JEAB* and *TPR*, with about 20% each. He published coauthored works at an overall rate of about 0.6 per year. Less than a third were published before World War II at a rate of about 0.9 per year, in comparison to 15% of his single-authored publications at a rate of 3.5 per year. More than two thirds of his coauthored works were published after the war at a rate of about 0.6 per year, in comparison to over 80% of his single-authored publications. His local rates of coauthored publications ranged from 0 to 1.6 per year, in comparison to a range of 2.2 to 7.6 of his single-authored publications.

In what follows, we (a) compare our quantitative analyses of Skinner's coauthoring practices with those re-

ported in the scientometric literature and with Skinner's single-authoring practices; (b) describe and analyze the natural history of his coauthoring practices; (c) address some limitations in our methods and results; and (d) offer some suggestions for future research.

Quantitative Comparisons

Skinner's coauthoring practices were consistent with those in the natural, social, and psychological sciences of his day. For instance, he coauthored more journal articles than books and book chapters; he coauthored more empirical works than conceptual works; and he coauthored more works in data-type journals than in word-type journals (see Beaver & Rosen, 1979b; Jakobvits & Osgood, 1967; Over, 1982; Over & Smallman, 1973). These results are consistent with the finding that colleagues who contribute to the conduct of research (e.g., running subjects, analyzing data) are more likely to become coauthors than those who contribute to a publication's conceptual genesis or development (see Spiegel & Keith-Spiegel, 1970). Skinner's greater number of coauthored empirical publications thus likely reflects these differences in his colleagues' contributions to them (see Jakobvits & Osgood, 1967; Over & Smallman, 1973).

Skinner's single-authoring practices were also consistent with those in the natural, social, and psychological sciences for these forms and kinds of articles and journal types. The main inconsistency was that Skinner's rate of publishing coauthored works was lower after World War II than before (see Price, 1963). However, this is largely explained by his publishing fewer empirical articles and more conceptual articles later in his career than earlier, a change that is glossed over in the scientometric literature. As a side note, Skinner could have published more empirical articles

after World War II if *Schedules of Reinforcement* had not been published as a single work, but instead as articles. As Skinner (1983b) later recalled,

So many interesting things were happening in our research that Charlie [Ferster] and I had little time to write reports. We gave a few papers ... and discussed our work, but unreported cumulative records and protocols were piling up. Obviously we should write a book. (p. 109)

As for the journals in which Skinner published his coauthored works, these appeared to be a function of what was available and his interests at the time (e.g., earlier vs. later in his career). For example, his seven publications in *TPR* occurred between 1937 and 1940, when he was an editor for the journal's experimental submissions. He may have felt some obligation to publish in *TPR* at this time. In fact, J. R. Kantor, who served as the journal's founding editor, wrote to Skinner that "it would be very desirable if The [Psychological] Record could serve as a rallying ground for studies on stimulus-function. I am hoping that in the not too distant future you will contribute a paper to the Record" (Skinner, 1928–1979, J. R. Kantor to Skinner, March 12, 1937, Harvard University Archives).

We turn now to a natural history of Skinner's coauthoring practices, where we consider some of the factors that may account for them and changes in them, and integrate our findings with the literature on collaboration and coauthorship.

A Natural History of Skinner's Coauthoring Practices

Harvard. During the early part of Skinner's career, first as a graduate student (1928–1931), then as a National Research Council Fellow (1931–1933), and eventually as a Junior Fellow in Harvard's Society of Fellows (1933–1936), he published 27 single-authored and three coau-

thored publications, the latter among his earliest (i.e., Barnes & Skinner, 1930; Skinner & Crozier, 1931; Lambert, Skinner, & Forbes, 1933). Skinner's first coauthored scientific publication was with T. Cunliffe Barnes,³ an assistant in Harvard's Department of General Physiology, which was chaired by William J. Crozier. Although Skinner was in Harvard's Department of Philosophy and Psychology, he spent more time in Crozier's lab for reasons described below. Barnes and Skinner undertook a research project on geotropisms in ants. Skinner traced the ants' paths as they walked on a piece of paper, the angle of which was systematically controlled by tilting the paper. Skinner (1979) reported later that he learned more about human behavior—Barnes' behavior—than about ant behavior. He described Barnes as an "eccentric young Canadian" (pp. 18–19).

After writing a "vitriolic" review of Franklin Fearing's (1930) *Reflex Action: A Study in the History of Physiological Psychology*, Skinner gave it to Crozier to read. According to Skinner (1979), Crozier "toned down a phrase or two ... and added his name as co-author because the paper needed more authority" (p. 64). Skinner and Crozier never published empirical works together, but Crozier did provide Skinner with tangible resources that supported his research (e.g., facilities, funding; see Skinner, 1979, pp. 25–26; 1983b, p. 91). Cro-

³Skinner's first coauthor was his father (Skinner & Skinner, 1928). In 1904, President Theodore Roosevelt commissioned an Anthracite Board of Conciliation in the aftermath of the famous anthracite coal strike. The Board, in turn, commissioned *A Digest of Decisions of the Anthracite Board of Conciliation* that could be used by lawyers in subsequent court cases. At his father's invitation, Skinner wrote summaries of over 1,100 decisions reached by the courts. As for their coauthorship, Skinner's father is listed as second, but "for prestige only." The book was copyrighted under B. F. Skinner's name alone (Skinner, 1976, p. 286).

zier also modeled an empirical and inductive style of science in the tradition of Francis Bacon (L. D. Smith, 1986), Claude Bernard (Thompson, 1984), and Jacques Loeb (see Skinner, 1979, p. 45; 1983b, p. 91; see also Hackenberg, 1995), in which experimental control was the goal of science. However, although Crozier's scientific style influenced Skinner's metascience (e.g., research on the organism as a whole), it did not much influence the content of his science. Skinner analyzed operant behavior, not tropisms.

Alexander Forbes likewise provided tangible resources for their research on motor nerve thresholds (e.g., equipment, facilities) at the Harvard Medical School, but he also did not much influence the content of Skinner's science (see Lambert, Skinner, & Forbes, 1933). In fact, as Skinner (1979) later recounted, "I never fully understood what we were doing, and our paper was written primarily by Lambert and Forbes" (p. 119). This comment notwithstanding, Skinner's collaboration with Forbes was noteworthy in at least one respect: Both Skinner and Forbes were remarkably innovative in scientific instrumentation, recording, and design. Their technical abilities contributed significantly to the contributions they made to their respective fields relatively early in their careers. Their contributions, though, were developed independent of each other: Skinner in the experimental analysis of behavior (Skinner, 1956), Forbes in experimental physiology (see Fenn, 1969; Zottoli, 2001).

Although Skinner's collaborations and coauthored publications might have contributed to Barnes's, Crozier's, and Forbes's careers, the effect was seemingly not substantial. However, Crozier was able to advance his style of science through Skinner, albeit in psychology, not in general physiology. The publications may also have demonstrated Skinner's competence and enhanced his visibil-

ity (see Beaver & Rosen, 1979a, 1979b), but they did not initially advance the content of his science. He advanced it, at first, on his own. As he wrote his mentor and friend, Percy Saunders, at Hamilton College in December, 1928:

I am working as hard as I have ever worked, but freely—with time and subject matter of my own choosing. ... My fundamental interests lie in the field of Psychology, and I shall probably continue therein, even, if necessary, by making over the entire field to suit myself. (Skinner, 1979, p. 38)

He was a bit of an iconoclast (Coleman, 1982).

Minnesota. Iconoclast or not, soon after he took a position at the University of Minnesota (1936–1945), he began collaborating with Heron, his more established colleague, who also had an interest in animal behavior (e.g., Heron, 1922, 1935). Although the impetus for their collaboration was more Heron's than Skinner's, as an untenured instructor, Skinner may have wanted to establish a research program quickly, as well as be collegial. Their research, though, was not exactly in Skinner's style. Whereas the data presented in *The Behavior of Organisms* (1938) was typically the behavior of sets of four rats considered individually, with Heron, Skinner conducted research with large numbers of rats, averaged the data, and conducted tests of statistical significance (Skinner, 1979, p. 214; see also Skinner, 1956, pp. 227–228). With a grant Heron received from the Ford Foundation in 1938, they constructed a battery of 24 boxes, each with two levers. Despite Skinner's initial excitement over this new research procedure, he soon grew skeptical. He noted, for example, the difficulties of maintaining 24 mechanical boxes, as well as keeping track of the large number of rats needed to conduct the research (Skinner, 1979, p. 223). Moreover, with so many subjects, they found it difficult to analyze the

data between the daily sessions. To increase their efficiency, Skinner built a “summarizer” that automated the averaging of the daily results; however, he eventually abandoned the statistical approach with large numbers of subjects in favor of the “old magic number of four in each experiment” (Skinner, 1979, p. 223).

Skinner also conducted research with Heron on the effects of pharmacological agents and biologically based individual differences (e.g., Heron & Skinner, 1937, 1940). Regarding the latter, he was skeptical again. Although they initially observed a difference in the rate of lever pressing by maze-bright and maze-dull rats during extinction, when they corrected the rates for differences in the rats’ feeding schedules, the difference disappeared (see Skinner, 1979, p. 227). Thus, although Heron was Skinner’s most frequent coauthor, their collaboration, as with that with Barnes and Forbes, did not have much influence on the style or content of his science. As for Heron, his professional interests turned from behavioral pharmacology and genetics to, eventually, hypnosis (e.g., Heron, 1957).

Skinner published 21 other works at Minnesota, but just two of them were coauthored. One of them was with Heron’s graduate student, Cook, on research related to verbal behavior (i.e., Cook & Skinner, 1939) and another was with his own student, Estes, on conditioned “anxiety” in rats (i.e., Estes & Skinner, 1941, see Skinner, 1979, p. 240). This coauthoring practice became Skinner’s norm: He published 18 of his remaining 24 coauthored publications with students, either his or someone else’s. In doing so, he provided them with practical and professional support, including knowledge, facilities, and funding.

Collaboration and coauthorship may not, of course, always correspond. For instance, collaboration may not result in coauthorship. To

some extent, this is true of most empirical research. Few scientists work without some assistance. Thus, when collaboration occurs on single-author publications, the counts of single authorship lead to underestimations of collaboration. For example, Skinner’s (1941) abstract, “Some Quantitative Properties of Anxiety,” was single-authored, yet he conducted the research in collaboration with Estes. In fact, he noted this in his abstract: “W. K. Estes collaborated in the design and conduct of this experiment” (p. 539). This made this publication an underestimation of Skinner’s collaborative research. When the research was eventually published, though, Estes was the first author (i.e., Estes & Skinner, 1941).

Harvard again. After chairing the psychology department at Indiana University (1945–1948), where he published no coauthored works, Skinner returned to Harvard for the remainder of his career. There, he established the Pigeon Lab in 1948 and began supervising a significant number of graduate and postdoctoral students. In remarking on his collaborations during the 1950s, he wrote, “I had looked upon my years of collaboration with Charlie Ferster [1950–1955] as my Golden Age as a behavioral scientist, but Bill Morse came up to the same mark” (Skinner, 1983b, p. 133). With Morse, his student, Skinner published on a variety of topics, among them stimulus control, schedules of reinforcement, and superstition (see Morse & Skinner, 1957, 1958; Skinner & Morse, 1957, 1958a, 1958b). With Ferster, a research assistant but essentially a postdoc, they published *Schedules of Reinforcement* (Ferster & Skinner, 1957; see Ferster, 1970). As noted above, though, had Skinner chosen to, he could have published many coauthored works with Ferster based on their research (see Skinner, 1983b, p. 109). Instead, they mainly confined their collaboration to their 1957 book (but see Ferster, 1959; Skinner, 1958).

As for whether Skinner's coauthoring practices enhanced his status at this time by increasing his publications or directing the field's future through his coauthors, they may have, but not likely to an extraordinary degree. As for whether his practices allowed his students and junior colleagues to demonstrate their competence and enhance their visibility, they likely did (see Beaver & Rosen, 1978, 1979a, 1979b). First, his students and junior colleagues were the lead authors on nearly 75% of Skinner's coauthored publications. Second, as may be gleaned from Skinner's (1979, 1983b) autobiography, his coauthors' authorship orders were seemingly consistent with their contributions to their publications. Indeed, Skinner was self-conscious of this point regarding *Schedules of Reinforcement*:

Cooperative research sometimes raises a question of credit. When reporting our experiments in Stockholm, I had listed myself as director of the project, but had said that "Dr. Charles B. Ferster had served as principal investigator." Nevertheless, I was the sole author of the paper and got credit for it. ... The book that we had discussed with publishers was "by Skinner and Ferster," but I was a professor of psychology and Charlie a mere research fellow, and if my name came first, he would be cast in the role of an assistant. I [made] a last minute change so that when Charlie saw his first copy he would read "by C. B. Ferster and B. F. Skinner." (Skinner, 1983b, p. 110; see also Skinner, 1981)

Although the Pigeon Lab (1948–1998) remained active long after *Schedules of Reinforcement* was published, Skinner stepped increasingly aside as he turned to other topics. One of them was Lindsley's extension of Skinner's science to human behavior at the Metropolitan State Hospital between 1953 and 1965. This yielded two coauthored publications: Lindsley and Skinner (1954) and Skinner, Solomon, and Lindsley (1954) on an early experimental evaluation of operant conditioning techniques with the behavior of "psychotics" (Rutherford, 2003). A

second topic was education (Skinner, 1968; see Morris, 2003), on which he coauthored two works with Holland, one on teaching machines (i.e., Skinner & Holland, 1960) and the other their programmed textbook (i.e., Holland & Skinner, 1961; see also Skinner & Krakower, 1968). A third topic was Skinner's increasing interest in conceptual issues, as he neared formal retirement in 1974 (e.g., Skinner, 1971, 1974).

Although his later conceptually based publications tended to be single-authored, as is consistent with the scientometric literature (Over, 1982; M. Smith, 1958), some of his earlier conceptual works were coauthored: (a) Brinton, Krutch, Kroeber, Skinner, and Haydn (1952), which was a transcription of a roundtable discussion among the authors on the possibility of a science of human behavior (see Skinner, 1983b, pp. 105–107); (b) Rogers and Skinner (1956), a debate on the science's implications for freedom and dignity; and (c) Blanshard and Skinner (1967), a debate on the nature of consciousness.

Some of the foregoing publications illustrate, again, that collaboration and coauthorship may not always correspond, but now in the other direction: Coauthorship may not be the result of collaboration in the usual sense. One example is Solomon's contribution to Skinner, Solomon, and Lindsley's (1954) "A New Method for the Experimental Analysis of Behavior of Psychotic Patients." Although Solomon was the director of the Boston Psychopathic Hospital and helped to secure research space for Skinner and Lindsley at Metropolitan State Hospital, he apparently did not collaborate in the manner that would typically result in coauthorship. In a letter to Skinner, Solomon wrote, "I see no reason at all why my name should appear on the paper. Certainly I have contributed nothing more than moral support and done none of the work. I

am serious about this so please have it Lindsley and Skinner or Skinner and Lindsley as you see fit” (Skinner, 1928–1979, H. C. Solomon to Skinner, January 21, 1954, Harvard University Archives). In this case, coauthorship was mainly a courtesy. Today, this would be inconsistent with APA’s ethical standards, as described in the 2010 edition of the *Publication Manual of the American Psychological Association* (p. 18). In addition to Solomon’s demurring, coauthorship on this paper is ambiguous because, although the byline lists Skinner, Solomon, and Lindsley, Skinner’s name is in italics, but Solomon’s and Lindsley’s are not; moreover, Solomon and Lindsley are designated as “collaborators.” In the end, we retained Solomon and Lindsley as coauthors because all three names were in the byline.

Lesser examples of coauthorship are “ensemble” publications, for instance, Skinner’s paper with Rogers, “Some Issues Concerning the Control of Human Behavior: A Symposium” (Rogers & Skinner, 1956). Although Rogers and Skinner were colleagues and friends, their publication was not based on a systematic collaboration between them prior to its submission or publication, but only on their debate at the 1956 meeting of the APA (Skinner, 1983b, pp. 121–122). Skinner wrote the first section; Rogers responded to it, in part; and Skinner responded to Rogers, in part. This was the extent of their collaboration on this article. Skinner’s publication with Brinton, Krutch, Kroeber, and Haydn (1952) was little different (see also Blanshard & Skinner, 1967; Richards & Skinner, 1962). In these cases, coauthorship may be an overestimation of collaboration. As an aside, authorship order is not always an accurate gauge of coauthor contributions, especially when research advisers, as a rule, list themselves as the last author (e.g., Philips, Philips, Fixsen, & Wolf, 1971; see Goodall, 1973;

Risley, 2005, p. 286) or in disciplines that assign authorship alphabetically (e.g., physics; see Over, 1982).

Skinner’s final coauthored publications were with his student, Epstein, and their colleagues, Lanza⁴ and Starr, and then with Skinner’s colleague, Vaughan. The former were laboratory-based behavioral simulations of cognition and communication prompted by Epstein, which resulted in five publications (Epstein, Lanza, & Skinner, 1980, 1981; Epstein & Skinner, 1980, 1981; Lanza, Starr, & Skinner, 1982). Skinner’s publication with Vaughan was their collaboration on *Enjoy Old Age: A Program of Self-Management* (Skinner & Vaughan, 1983).

Whether Skinner collaborated and published with his students and colleagues at this time to sustain or increase his productivity or to influence the field’s future, the evidence is scant. He had little reason to sustain or increase his productivity. Indeed, he had no difficulty advancing his science and philosophy on his own throughout his career (e.g., Skinner, 1938, 1953, 1957, 1974). Whether his students and junior colleagues demonstrated their competence to their peers and enhanced their visibility in the scientific community (see Beaver & Rosen, 1979a, 1979b), this no doubt occurred. Some did so in the ordinary course of collaboration, others were perhaps more proactive (see Epstein, 1997).

Limitations

Our review of Skinner’s coauthored publications has provided quantitative information and qualitative descriptions that begin to inform an analysis of his collaborative and coauthoring practices. It is

⁴Robert Lanza was not Skinner’s student (nor were some other coauthors, e.g., Cook). He was a medical student at the University of Pennsylvania who was conducting an independent study with Skinner (R. P. Lanza, personal communication, July 13, 2010).

not, however, without limitations. Our methods, for instance, did not systematically assess the extent to which Skinner's coauthored publications were actually based on collaborative work or whether the contributions were independent but joined. This likely led to some overestimations and underestimations of Skinner's actual collaborations. In addition, we did not code for Skinner's footnotes or acknowledgments in his single-authored publications—collaboration that fell short of authorship. This would have provided a more complete assessment of his practices. Finally, we did not systematically address the kinds of knowledge, skills, equipment, facilities, and finances Skinner and his coauthors brought to their collaborations or at which points in Skinner's career.

Future Research

These limitations can be addressed in future research, along with additional analyses of Skinner's practices, as well as with those in behavior analysis overall.

Skinner's practices. First, further quantitative analyses of Skinner's coauthoring practices might expand our understanding of their impact. The relative impact of his coauthored publications could be compared with that of his single-authored publications (see Wuchty et al., 2007). This might be accomplished through Web of Science or Google Scholar citation counts, calculations of his impact factor from these counts, and counts of how many of Skinner's coauthored articles were reprinted and how often. Their relative impact might also be assessed by comparing the prestige of the journals in which Skinner published his coauthored and single-authored articles (see Beaver & Rosen, 1979b). Journal prestige could be assessed by consulting published surveys of journal ratings and rankings (Beaver & Rosen, 1979b) and impact factors (Lawani, 1986; Wuchty et al., 2007).

Second, a combination of quantitative and qualitative analyses might inform some apparent inconsistencies between Skinner's coauthoring practices and those found in the scientometric literature. For instance, although the relative rate of coauthoring in science increased after World War II (Price, 1963), Skinner's relative rate decreased (i.e., his percentage of coauthored works). This is a measurement issue, however: a difference in cross-sectional measures of coauthorship across scientists (i.e., between subjects) and a longitudinal measure of coauthorship within a scientist—Skinner (i.e., within subject). Cross-sectional measures may show an increase in coauthored works over time for sociology-of-science reasons. For instance, the size of cohorts of scientists who coauthor may increase (i.e., those who enter a field in successive years). Also, the need for collaboration may increase to promote effective action in research and grant funding (see, e.g., Beaver & Rosen, 1978). Although the latter may be true for individual scientists, longitudinal analyses may show a relative decrease in coauthored works over time for a variety of reasons. For instance, some scientists' interests shift from empirical to conceptual topics over the course of their careers, the latter of which are less likely to be coauthored. Also, scientists who become public intellectuals, such as Skinner, may be increasingly invited to present and publish works that describe their research and perspectives, also resulting in works that would not likely be coauthored (see Coleman, 1982).

Third, further qualitative analyses might be undertaken on the sociology and politics of Skinner's coauthoring practices, for instance, the coauthors he sought out compared to those who presented themselves naturally (e.g., graduate students, but not those who constructed his apparatus; see Conner, 2005), as well as practices that varied on dimensions of honorific

inclusion or strategic omission. This might explain why Skinner was the second author of his two-author publications at a greater than chance level. These analyses would require significantly more archival research than we conducted, largely of Skinner's correspondence with his coauthors and theirs with him (e.g., the Archives of the History of American Psychology, Harvard University Archives).

In addition to these internal analyses of Skinner's coauthoring practices, his practices might also be compared to those of his neobehaviorist colleagues (e.g., Guthrie, Hull, Spence, Tolman). This might reveal differences related to their methodological styles (e.g., within-subject vs. between-subjects methods) and scientific goals (e.g., prediction and control vs. theoretical coherence and correspondence) that might inform assessments of their relative success, albeit speculative at best.

Behavior-analytic practices. Behavior-analytic coauthoring practices, both as a whole and in relation to other sciences, also warrant analysis. For instance, the extant scientometric analyses might be replicated with the field's practices. This would include changes in practices over time, for instance, in its basic and applied journals (e.g., *JEAB*, *Journal of Applied Behavior Analysis*) and conceptually based journals (e.g., *Behavior and Philosophy*, *The Behavior Analyst*). Comparisons might also be made across kinds of publications (e.g., empirical, conceptual, methodological) and types of journals (e.g., data-type, word-type), and, eventually, across the forms of publication (e.g., journal articles, chapters, books). These findings could become the basis of a descriptive science of behavior-analytic science. In addition, analyses of these practices could be compared with those of other sciences (e.g., physics, biology, psychology). If these and the foregoing analyses were published in sciento-

metric journals, this would make evident the existence and vibrancy of behavior analysis among the sciences, which is now likely unknown to their readers.

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APPENDIX

A CHRONOLOGICAL BIBLIOGRAPHY OF B. F. SKINNER'S COAUTHORED PUBLICATIONS

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