Reply to Palmer, Schaal, and Chiesa

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I found the commentaries (Chiesa, 1998; Palmer, 1998; Schaal, 1998) to be particularly helpful in providing additional examples and explanations of Skinner's movements away from S-R reflexology. I found them less helpful in elucidating the arguments I had advanced. I often found, for example, that their difficulties were more with what Skinner said than with what I said, although my reporting of what Skinner said may have presented the problem—like a messenger bringing bad news for previous assumptions.

In saying that he believed "this change [in Skinner's position] occurred rapidly, in a few saltations, in the mid-1930s and that he did not substantially alter his views thereafter" (p. 93), Palmer presents an a priori assumption without showing how that assumption is supported. Palmer simply assumes that Skinner's later statements after the mid-1930s are theoretically unimportant or a fairly direct implication of his 1930s views as presented under different circumstances—as if understanding Skinner's work in the 1930s is like understanding a major premise in a syllogism in which the subsequent conclusion is regarded as implicitly contained in the premise or as if Skinner had thought through all aspects of his new theory in the 1930s and was simply unpacking and laying parts of it out for view in his later years. I find difficulties with this position as presented by Palmer. One, to advance his case, Palmer disputes some of the things that Skinner said; however, Palmer hides his disagreements with Skinner by presenting his disagreements as though

they were with what I said. For example, Palmer (p. 93) takes issue with my statement, "Skinner's actual mistake at the time had been to follow Watson and the conditioned reflex rather than Russell and the law of effect" (p. 76). My statement is in reference to Skinner's (1976/1977) statement, which preceded mine in the same paragraph: "It would be a long time before I saw the mistake which Russell and Watson were making and in which I concurred" (p. 299). The "mistake" that Skinner "concurred" in was in following S-R psychology and the conditioned reflex, as presented by Russell in the section that Skinner was talking about and in which Skinner noted Pavlov's principle of the conditioned reflex. Two, such a position results in an excessively streamlined account in which conflicting evidence is simply omitted. For example, in downplaying Skinner's commitment to determinism, Palmer conveniently ignores Skinner's (1947) strongest statements in support of determinism. Three, this view relies on speculations about private events for support. Speculating on what Skinner appreciated or "failed to appreciate" (p. 95) or on what Skinner's intentions were (e.g., "an attempt to remind," p. 95) is a rather weak form of support. Four, this view is tangential to the primary argument I was making as to whether Skinner could have provided more clarification of his views throughout his work (a clarification that might have prevented some of the misinterpretations of his work). Whether Skinner's later positions could be foreseen or not by someone such as Palmer does not address whether Skinner could have clarified his views for less prescient readers. Five, this view relies on some awkward reconstructions, and

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I could not always follow some of Palmer's points or understand the point of some of his comments; for example, in regard to Skinner's diagram (p. 94), is Palmer saying that the S-O-R formulation in that diagram is suitable and consistent with Skinner's selectionist three-term contingency or not? Six, presumably in furthering this view, Palmer provided some useful points in his commentary on the S-R position and Skinner's movement away from it, but he did not provide a commentary of comparable usefulness on the position that Skinner moved to or on the changes that occurred in Skinner's movements from the one position to the other. The more important point is what Skinner's final position is. In my view, an adequate representation of the selectionist position that Skinner finally adopted requires giving some attention and significance to Skinner's statements beyond the 1930s, particularly as they affect the import of his three term contingency (cf. Moxley, 1996).

I liked many of Schaal's comments. However, I found that Schaal advanced some points as though they were in opposition to my views when I thought his points actually were more in support than in conflict with my views. Here are five instances. One, although this may have been an inadvertent misstatement, Schaal (p. 97) says, "My guess is that misinterpretations do not usually reflect readers' detection of subtle *changes* in Skinner's writing over the decades." I agree with that statement, but I did not imply that misinterpretations usually reflect a detection of subtle changes. Two, Schaal says I assert that Skinner "was a reflexologist" (p. 98) well into the 1950s. That is not what I said, and it misstates what I implied. I contended that Skinner's views changed over an extended period of time, not overnight. In other words, my argument was not that Skinner was a reflexologist (and all that it implies) into the 1950s and then reached a point at which he was a selectionist (and all that it implies).

My argument was that Skinner made some changes and adjustments in his statements over time which were more or less at odds with or importantly different from what he had previously said, and he did not provide as much clarification in this regard as he might have. Three, Schaal calls it a misinterpretation when I identified as problematic for a reader the fact that

Skinner (1945) referred to "the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus" (*Cumulative Record*, 1972, p. 380) and identified only two terms in the contingencies of reinforcement, a stimulus and a response. (p. 78)

Such an *identification*, however, is not an *interpretation* in the typical use of that term, and Schaal's finding three terms elsewhere in Skinner's paper does not eliminate but adds to the problem for the reader. Four, in Schaal's (pp. 99–100) discussion of Skinner's use of *setting* as opposed to *discriminative stimulus*, I think his argument is more with what Skinner said (which amounts to saying that Skinner could have been clearer) than with my report of it. Five, Schaal says that Skinner may not have been "as committed to a position on this topic [determinism] as Moxley suggests" (p. 100). Rather than arguing that Skinner remained unwaveringly committed to determinism, I pointed out that Skinner's commitment to determinism changed over time; and I agree with Schaal that the use of "spontaneous" for first instances of operants is problematic for someone who would maintain a classical determinist position. I also liked Schaal's point about "the theoretical importance of chance correlations among behavioral events" (p. 101). However, I do not believe that Skinner's use of "spontaneous" in one place and his investigation of "accidental stimulus–reinforcer correlations" (p. 100) in another means that he necessarily rejected determinism there or elsewhere. There is no seeming end to the ways in which a deep

determinism can be putatively rescued from appearances.

Chiesa's characterization of my views in her two points of exception is not accurate. In regard to her first point about overstating Skinner's determinism, a distinction should be made between Skinner's concept of determinism and the aspect in which he viewed it, such as his commitment to that concept. I believe that his commitment to that concept varied, but I am not convinced that his concept varied. What then was that concept, and why do I assume it was similar to a view such as Laplace's? One, as Chiesa points out, Skinner provided little in the way of explanation as to what his concept was; I believe that this lack of explanation makes more sense if Skinner accepted the received view of determinism at the time, which was that of Laplace. There would then be less need to provide clarification. If Skinner had a view of determinism that was different from that of Laplace, Skinner would have been more obligated to provide an explanation of his differences from the received view. It would then be more puzzling to explain why he did not do so. Two, in coupling the term *complete* to determinism, Skinner suggests he was referring to a strong or strict determinism. I do not know how a complete determinism would not also be a strict determinism. If there is a complete determinism that is not strict, what is it? Three, Skinner's (1947) claim that he looked forward to a time when predicting the behavior of an individual "would be done by determining the values of certain constants in equations" (p. 39) is a close match to Laplace's claim for the predictive power of the world formula (even if Skinner is silent on retrodiction to events in the past). In what way then is Skinner's claim for the predictive power of equations not to be thought of as strict determinism? Four, Skinner may not have referred directly to Laplace, but he (e.g., 1979/1984) did refer to Mach (pp. 64, 66, 116, 123, 358) and Poincaré (pp. 66, 83, 116, 123), who both

discussed a scientific determinism such as that of Laplace (e.g., Mach, 1960, p. 558; Poincaré, 1963, pp. 1–14, 112–113, also cf. 1952, pp. 213–214), and I don't think a failure to find a direct citation to Laplace for a widely received view is particularly problematic. Five, whether or not Skinner's concept of determinism was a strict one, my main point is that his commitment to determinism declined in his later years to the point at which it was irrelevant for his behavior analysis. As for Chiesa's second point, I did not argue that "some of Skinner's critics may have been confused by a perceived shift from strict determinism to selection as a causal mode" (p. 104). If anything, my argument implied that a *failure* to see the shift in Skinner's commitment to determinism in his later years might lead to misrepresenting his final views. For example, to the extent that classical determinism is seen to imply classical mechanism, a continuing allegiance to determinism may be seen by some as a continuing allegiance to classical mechanism (and perhaps S-R psychology).

Further clarification of my views in regard to Skinner and determinism—with a more detailed discussion of the background and the sources that may have influenced Skinner's shift from determinism to random variation—may be found in Moxley (1997).

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