S. S. STEVENS AND THE ORIGINS OF OPERATIONISM*

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Despite influencing the social sciences since the 1930s, S. S. Stevens' "operationist" philosophy of science has yet to be adequately understood. I reconstruct Stevens' operationism from his early work and assess the influence of various views (logical positivism, behaviorism and the "operational viewpoint" of P. W. Bridgman, among others) on Stevens. Stevens' operationism emerges, on my reconstruction, as a naturalistic methodological directive aimed at agreement, founded in turn on the belief that agreement is constitutive of science, the scientific community, and objectivity. Further, I show that operationism is historically and philosophically independent of the views mentioned above.

1. Introduction. In four papers published between 1935 and 1939, S. S. Stevens, a 32-year-old Harvard psychologist experimenting on audition, articulated and defended a philosophy of science he called "operationism." The debate surrounding this term and the approach to science with which it was subsequently associated is familiar lore to philosophers of science. However, in presenting operationism as the reflection of logical positivism in an American mirror tainted in roughly equal parts by pragmatism and experimental physics, the familiar lore embodies broad and significant inaccuracies concerning both the original sense of operationism and the historical context within which it was a viable methodology. One desired effect of this paper, the point of which is to examine Stevens' operationism and the intellectual context in which it was developed, is to make apparent the extent to which the operationism lore misdescribes the historical place of operationism and consequently sells short its interest and even its viability.

Stevens' operationism has received inadequate historical attention (for relatively brief accounts, see Benjamin 1955, Leahey 1980, Smith 1986,

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Woodward 1990, Rogers 1989, Walter 1990, and Koch 1992). This is perhaps because Stevens' intellectual development gives the impression that his philosophical interests waned after the 1930s. After three papers in 1935 and 1936 describing operationism, and an influential review article in 1939, few of Stevens' papers over the next thirty years mention operationism or the philosophy of science, and those that do offer nothing novel (see, for example, 1942/1983, 1947, 1951, 1958, 1959, 1966a, 1966b, and 1966c). Notably, Stevens was not among the contributors either to a *Psychological Review* symposium on operationism in 1945 or to the proceedings of a session sponsored by the Institute for the Unity of Science on "The Present State of Operationism" in late 1953 (published in *Scientific Monthly* in 1954), though both featured prominent operationists and their critics and both were centerpieces of a vigorous debate ignited in part by Stevens' earlier papers.[1] The ready inference is that Stevens had lost interest in operationism, or even rejected his earlier views, as indeed Woodward (1990, 871) suggests.

In fact, Stevens never abandoned his views, nor did his philosophical interests clearly wane after 1939. In 1946, while declining Ernest Nagel's invitation to participate in a conference on "Methods and Philosophy in the Natural Sciences," Stevens nevertheless described the topic as "dear to [his] heart" and lamented the "six long years since [he] had time to think about [his] old friend Unity of Science" (Stevens to Nagel, April 1946, SSSP 2.12).[2] In 1960 Stevens wrote Donald Campbell that Stevens (1936) still struck him as an "interesting, fast-moving summary of the operationalist point of view," adding that he wished he "could think as well of all [his] past efforts!" (Stevens to Campbell, 13 September 1960, SSSP 2.14). Thirteen years later, reflecting on the longevity of his "intellectual products," Stevens declared that "the operational outlook . . . seems currently to have lost its vogue," but that "its day will probably come again . . . for the issue remains too basic to disappear" (1974, 418–419). Clearly the absence of papers devoted to operationism after 1939 reflects satisfaction, not apathy.

There is a deeper reason why Stevens has received little historical attention. For several decades operationism has been regarded as untenable,

[1]The first symposium included E. G. Boring, P. W. Bridgman, H. Feigl, H. E. Israel, C. C. Pratt, and B. F. Skinner; the second G. Bergmann, Bridgman, A. Grünbaum, C. G. Hempel, R. B. Lindsay, H. Margeneau, and R. J. Seeger. Stevens declined an invitation to the 1945 symposium, citing war research (Stevens to Langfeld, 26 September 1944, S. S. Stevens Papers, Harvard University Archives (cited hereafter as 'SSSP'), Box HUG FP 2.12 (cited hereafter without the label, 'HUG FP')). He was a session discussant in the *Scientific Monthly* symposium (Stevens to P. Frank, 27 Nov. 1953, 2.12), this limited role reflecting re-immersion in his own research earlier that summer after what he later called "seven lean years" (Stevens 1974, 413).

[2]This and all subsequent passages from the papers of S. S. Stevens are published here by permission of the Harvard University Archives.

and Stevens defended it enthusiastically. While I do not aim to establish the tenability of Stevens' operationism, I will show that it differs significantly from the operationism attacked by, for example, Hempel (1954). For example, Stevens was not obviously committed to Bridgman's claim that a "*concept is synonymous with the corresponding set of operations*" (Bridgman 1927, 5; emphasis in original. See Koch 1992, 265, 274ff, for whether Bridgman was committed to this thesis.). More importantly, Stevens' operationism had a practical aim; it was "a sensible philosophy for the laboratory" (Private Notebook, 1932, 21, SSSP 2.45). Partly for this reason, I would argue, it resonates with recent philosophy of science—in its naturalism, rejection of foundationalism, and appreciation of the social dimensions of scientific knowledge. Stevens' operationism should be of interest to present-day philosophy of science.

Nevertheless, there has not yet been a thorough and accurate examination of Stevens' philosophical views or their context. Accordingly, Section I reconstructs Stevens' operationism from his 1935 articles, highlighting his emphasis on agreement and leaving open the question of whether he *identified* the meaning of scientific concepts with operations. So understood, Stevens' operationism is a richer philosophy than (and an awkward fit with) operationism as usually understood.

Section II addresses sources, particularly those sources which have been presumed to have shaped Stevens' operationism but in fact played a negligible role. The independence of Stevens' views from the views of Bridgman, E. C. Tolman, B. F. Skinner, W. v. Quine, and the Logical Positivists is established, despite the post hoc associations forged by Stevens and others (see Stevens 1966a). Here aspects of Stevens' role in the transmission of logical positivism to the United States are also illuminated. Finally, reasons are offered against attributing too much of Stevens' operationism to Stevens' advisor and subsequent colleague, E. G. Boring.

2. Reconstructing Stevens' Operationism. Stevens' operationism is a broad methodological directive founded upon a theory of agreement between people. Since Stevens took agreement to be essential to science, it is rightly regarded as a philosophy of science as well, broadly construed.

'Agreement', on Stevens' view, is constitutive of scientific community, and of objectivity and science as well. "Science" Stevens emphasized, "is a thing agreed upon by members of society" (1935a, 327; see also 1935b, 517; 1936, 97; and 1939, 227). That agreement should be taken as a *defining,* not merely *contingent,* feature of science is indicated by Stevens' discussion of disagreement:

> The observation of the ornithologist who discovered that sparrows speak English because he heard them daily talking to him in English does not get into zoölogy simply because there is no agreement as to

> the fact. . . . [I]f society finds it cannot persuade him to observe only what other people can observe, he is judged insane. (ibid., 327; see also 1936, 1939)

Individuals who disagree are *ipso facto* excluded from the community, without regard to the truth of their claims or the character of their method, which for that matter are also a matter of agreement. "The 'true' value of a physical constant . . ." Stevens wrote, "is true because physicists agree that it is true, and, if someone convinces physicists that the value is not true, it will thereafter be false. Of course each individual may think that he has his own private standards of truth. . . . The only difference is that the scientist's standards . . . conform to those of his associates" (1936, 97; see also 1966a, 219). Consequently, science for Stevens is necessarily public.

The deepest threat to science so understood is disagreement. Throughout Stevens' papers, disagreement appears in this guise; as "useless controversy" in need of silencing (1935a, 323), or "ceaseless argument and dissension" in need of elimination (1935b, 517). Disagreements for which resolution and subsequent agreement are not foreseeable pose the most serious threat, and they seemed to Stevens to revolve around scientific concepts and be prevalent in psychology. Moreover, the disagreements turned not so much on particular concepts or even *kinds* of concepts, but on whether a given concept applied in a given case. Hence, the specification of the *general form* of the rules to guide the application of a concept, such that no disagreement would arise in the application of a concept guided by rules of that form, is called for.

If operations, in the generic sense, are regarded as a primary element of such rules, we see the question as Stevens saw it, namely: with what kinds of operations should a concept be associated such that attempts to apply it do not result in unresolvable disagreement? The answer would go by the name "operationism":

> The revolution that will put an end to the possibility of revolutions is the one that defines a straightforward procedure for the definition and validation of concepts. . . . Such a procedure is the one which tests the meaning of concepts by appealing to the concrete operations by which the concept is determined. We may call it *operationism.* It ensures us against hazy, ambiguous and contradictory notions and provides the rigor of definition which silences useless controversy. (1935a, 323; emphasis in original)

This statement is programmatic—it says what operationism will do, not what it is. At the same time, it suggests that operations "define," "determine," or in some manner exhaust the meaning of concepts—a view I call the *semantic thesis.* As we shall see, there is some question whether Stevens

adopted the thesis, as opposed to giving operations a merely non-semantic role in determining the application of concepts.

Filling in this programmatic account requires specifying (1) what constitutes agreement and (2) what counts as an operation. Stevens "defines" 'operation' by denoting an instance, specifically, the operation he takes to be associated with the length of medium-sized objects. "[W]e can," he says, "determine a length by applying to a certain object a measuring stick. What we mean by the length of the object is that the ends of the object coincide with certain marks on the measuring rod" (ibid., 323–324; cf. Bridgman 1927, 5). Denoting an operation is something less than defining it; the example refines 'operation' perhaps only in implicitly presenting operations as composed of a gross physical behavior (in the case, laying a measuring rod along an object) and an observed result (the coincidence of the ends of the object with marks on the rod) (cf. Hempel 1954, 215). Even so, Stevens regarded denoting as sufficient to establish denoting *itself* as an operation, indeed as the operation "fundamental to most other[s]." "Suppose," Stevens imagined, "someone asks us what a meter is. The obvious thing is for us to show him a meter-stick, or, in other words, to denote a meter. That is an operational procedure" (ibid., 324). Here denoting is denoted—not a promising means of conveying the concept to the uninitiated. Stevens' sense of 'operation' is, however, conveyed all the same: 'operation' means gross behaviors, especially simple pointing.

Denoting is actually just half an operation; a *result* must be specified for every case in which denoting might be employed to settle disagreement, at the expense of ignoring attempted but failed denotings. Results are in the form of discriminations—"the concrete differential reactions of the living organism to environmental states" (1935b, 518). For sometimes:

> the operation of pointing to a thing cannot be employed because of the fact that the thing itself cannot be discriminated, *i.e.* it cannot be differentially sensed. . . . *Discrimination,* therefore, is the *sine qua non* of any and every operation including that of denoting. In this sense discrimination is the fundamental operation of all science. (1935a, 324; emphasis in original. See also 1935b, 1936, 1939.)

Elsewhere (1935a, 328), Stevens recognized discrimination qua *result* instead of operation. Discrimination serves to characterize agreement as well; specifically, agreement amounts to shared discriminatory capacities, i.e., the disposition to behave alike in like circumstances (as is explicit in Stevens 1947).

With Stevens' sense of 'agreement' and 'operation' in hand we have his operationism, not as a thesis about the meaning of concepts, but as a methodological dictum directed toward engendering agreement. It holds, simply, that *every scientific concept must be accompanied by a rule for its*

application which is expressible solely in terms of acts of denotating and associated discriminations.

The accompaniment may be latent; that is, an operational concept may not be (nor ever have been) *explicitly* or *consciously* associated with a rule of the required sort. Such a rule must only be *available* to resolve disagreement, should disagreement occur (1935a, 324). Nor are operational concepts guaranteed to apply in any particular case; rather, the guarantee (with some qualifications—see Stevens 1939, 230) is of eventual agreement over whether the operational concept applies in a particular case.

Disagreements over the application of operational concepts can still occur, and their resolution will take time, the shorter the better. In such cases, agreement is reached via an "operational regress," i.e., iterated appeals to successively less problematic concepts presumed to occur constituitively in the contested concept. "In practice, the operational regress need be pursued only until agreement is reached," Stevens noted, although "it may be necessary to appeal to more basic operations, until agreement is had or until the cause of the dissension is determined." The retreat ultimately arrives, if necessary, at "the more simple discriminatory responses about which there is unanimity" (1935a, 327; see also 1936, 94–95).

Such unanimity should be expected for operational concepts only. For non-operational concepts, the regress ends differently: "[When] in making an explanation we land in a system of terms whose meaning we cannot denote, we are forced to admit, either that we do not understand the thing we are trying to describe, or that the concept itself is without empirical meaning" (ibid., 327). There is no real difference between these two, for failures of agreement on Stevens' view can be no more the fault of the concept than of those who disagree. A failed operational regress *is* a local failure of denotation, which in turn is a local failure of discriminatory capacities to "match up" between the participants in the regress.

Stevens realized such failures were to be expected, as discriminatory capacities are "limited, and also variable" across people and time. Consequently, "a penumbra of uncertainty surrounds even the most precise and obvious operations" (ibid., 325; 1936; 1939. Cf. Bridgman 1927, 33–36). This plasticity of discrimination would appear to render operations risky touchstones of agreement at best, and encourage skepticism about scientific agreement, and science itself.

In fact, Stevens is not at all skeptical. Maintaining the promise of science, he concludes that rules for the application of concepts must be formulated with especial sensitivity to human discriminatory capacities, for only then can we expect agreement about our concepts and when they apply. Since "it is the sole business of psychology to test and measure the discriminatory capacities of the organism" (ibid., 325), scientific method,

insofar as it includes operationism, relies upon the sciences which study discriminatory capacities, psychology in particular. Science, Stevens wrote, "is conditioned in part upon the nature of the human experimenter," and "since it is the business of psychology to investigate the characteristics of the observer, we now have valid reason to propose psychology as the propaedeutic science" (1936, 94; see also 1935a, 325).[3]

Psychology provides the data on the basis of which rules for operational concepts are formulated, but it earns no methodological exemptions for the service. Psychology for Stevens is methodologically a natural science, a point conveyed by frequent distinction between the scientific psychologist and the object of study (the "psychology of the other one," borrowing a phrase of Max Meyer's (1921)), a distinction Stevens took to be common to other sciences, essential to the public character of science, and thus essential to scientific psychology. "The relation of the psychologist to the object of his investigation," Stevens wrote, "is fundamentally not different from that of any other scientist to his subject-matter. . . . Consequently, psychology regards all observations, including those which a psychologist makes upon himself, as derived from 'the other one' " (1935a, 328; see also 1935b, 517; 1936, 96; 1939, 228). For experiments conducted on these terms, "at any moment another psychologist could . . . take over the task of experimenter without altering the results" (ibid., 328). Otherwise, the experiments betray private, i.e., *non-*, science.

Not only was psychology given no breaks for being propaedeutic, it was left with a puzzle as well, one which confronts any scientific method made to depend upon sciences themselves subject to the method. To see how it confronts Stevens' notion of psychology, consider that time when it had amassed negligible knowledge of human discriminatory capacities. Initial research on discriminatory capacities would then risk guidance by operationally unsound concepts, that is, concepts associated with rules of application which engender disagreement. Perhaps such concepts will be revealed in practice, leaving psychology back at its starting point, none the wiser about discrimination. If not, these concepts would remain to infect subsequent research and thus the conceptual repertoire of other sciences. How do we know our psychology is not infected by such concepts? To pursue a study of *psychologists'* discriminatory capacities is, of course, to move the problem back a step.

This puzzle has recently received attention from philosophers (Kitcher

[3]This expression of psychology's "propaedeuticism" appears as the conclusion of an argument from the "subjective revolution" of special relativity, in which absolute space and absolute time were rejected upon recognition of "the relativity of the system to the observer" (1936, 93–94). Propaedeuticism is later arrived at by reflection on discrimination, at which point Stevens' reader is referred to the quoted passage. Stevens' "argument from relativity" is prefigured in (1935a, 323) but does not recur in Stevens (1935b) (where it would not be expected) nor in Stevens (1939) (where it would).

1992, Kornblith 1994). Stevens recognized it only gradually (e.g., 1935b, 517, and 1935a), and was not overly worried. He responded by dismissing its starting point, the psychology ignorant of people, and argued that in the *actual* situation we take our concepts in hand and attempt to improve the rules which guide their application, foregoing guarantees of a foolproof methodology:

> Psychology studies . . . the organism which determines in part the nature of science. But, it will be objected, this is equivalent to saying that psychology studies the organism which determines the nature of psychology—an obvious circle. . . . [T]he statement is circular, but none the less true. The statement is circular, because, whether we like it or not, we are dealing with a universe of continuities with no ends to grasp. Nevertheless we must start somewhere, and so we plunge *in media res,* and, although we then face the problem of an infinite regress, we try to understand rather than worry about it. (1936, 94)

This is a description of how to go on, or of how scientists *do* go on. As such, it fairly anticipates the philosophical discussions noted above.

A different issue was of more concern to Stevens. His notion of psychology abutted a vision of psychology as the study of "the given," i.e., immediate and private experience. Stevens traced this latter notion to Wilhelm Wundt (ibid., 91), and argued against it to preserve his own, different, vision. The given's inaccessibility furnished one argument to this end: "if we agree that operations must be public," Stevens noted, "we exclude from consideration any uniqueness of our own private experience. . . . [W]e can deal with the inner life only as it appears in the operations of report. . . ." (1935b, 522; see also 1936, 95; 1939, 239). Against its immediacy, Stevens argued that:

> the simplest phenomenological observation is really a complex response of an organism with a long history, and is therefore itself a construct. . . . [E]ven the most elementary experience, such as seeing a color and recognizing it . . . is conditioned upon the subject's previous history and present attitude, and therefore exhibits the 'inferential' character that is essential to constructs. We can say nothing about organisms without histories—they do not exist. (ibid., 523)

Neither argument should sway a devotee of the given. The first helps itself to Stevens' operationism, and thus only illustrates its incompatibility with the given, and the second simply denies the given's existence, sketching how an argument might go if one knew more physiology.

This aside, Stevens described how ejecting the given from psychology discredited the notion that science is constructed from immediate experience, perhaps with certainty. Attempts to understand science as such were

recast by Stevens as the use of a concept of experience from which the objectionable content had been drained, leaving one associated with certain discriminations. "The *experience* . . ." Stevens concluded, "upon which physical science is founded would seem to be nothing more than . . . the sum total of the discriminatory reactions performed by human beings, for *to experience* is, for the purpose of science, *to react discriminatively*" (ibid., 521; emphasis in original. See also 1936, 95). Stevens' operationism is, strictly speaking, neither empiricist nor foundationalist.

As already noted, I have presented Stevens' operationism absent the claim that operational concepts are *synonymous* with operations. Thus this reconstruction diverges from standard treatments of operationism *per se* (e.g. Hempel 1954, Leahey 1980, and Smith 1986, among many). To Stevens I have ascribed only the view that with every scientific concept should be associated a rule for its application, expressible solely in terms of denotations and discriminations. The semantic thesis is an optional addition to this. Whether *Stevens* took his operationism to include the semantic thesis is difficult to ascertain—there is textual and circumstantial evidence in both directions. For example, Stevens seems careful to note that nothing more than rules for the application of a concept are at issue in operationism, for example when he summarizes operationism as the view that "a term . . . has meaning . . . if, and only if, the criteria of its applicability or truth consist of concrete operations" (ibid., 517–518), or when he describes "operational treatment" as "an examination of the criteria which determine the word's applicability in any particular instance" (ibid., 524; see also 1935a, 323; 1936, 94). Further, nowhere in his first three papers does Stevens cite Bridgman's canonical passage that "*the concept is synonymous with the corresponding set of operations,*" (1927, 5; emphasis in original) nor even did he use the expressions 'synonymy' or 'sameness of meaning' to convey his views.[4] The capacity of the present reconstruction to fulfill the demands of a scientific methodology on Stevens' own terms might also be taken as evidence that Stevens' operationism can be pried apart from the semantic thesis.

Yet if Stevens never explicitly adopted the semantic thesis, he never rejected it either, and other passages suggest he took operations to exhaust concepts semantically. His descriptions of operationism as providing "a straightforward procedure for the definition . . . of concepts," and of "referring any concept for its definition to the concrete operations by which knowledge of the thing in question is had" (1935a, 323; see also 1935b,

[4]Stevens mentions Bridgman in his (1935a) only to say that his "is the model [of operationism] which is most directly applicable to psychology" (p. 323). Criticism of Bridgman's imaginary operations follows (1935a, 324). Bridgman's canonical statement *is* quoted in Stevens (1939), but not obviously with approval, and indeed just before criticism of Bridgman (1927) as "rich in example but poor in precept" (1939, 224).

517) are prime examples, albeit tempered perhaps by Stevens' description of definition as "the sum total of the criteria (operations) by which we determine the applicability of a term" (1935b, 519). "When we have formulated a description by way of experimenting . . ." he writes elsewhere, "the meanings of our words can never transcend the operations which went into their determination" (1936, 93). Finally, the semantic thesis seems to inform his treatment of sensation. "The *sensation red,*" according to Stevens, "is a term used to denote an 'objective' *process* or event which is public and which is observable by any competent investigator" (1935b, 524; emphasis in original). Though textual evidence may favor the incorporation of the semantic thesis, mild confusion is a better diagnosis on the whole. Quite possibly Stevens never considered or even recognized the distinction.

3. Stevens' Operationism In Its Context. My reconstruction draws primarily on Stevens (1935a) and (1935b), less so on Stevens (1936). Stevens (1935a) and (1935b) emerged, at the suggestion of E. G. Boring ('Operational Basis of Psy, running comment' (Boring to Stevens), p. 7, SSSP 2.10), from a single unpublished paper (of which only an outline remains) written by Stevens in Fall, 1934, a year and a half after he completed his Ph.D. in psychology at Harvard and during his first year as a National Research Council Fellow in Hallowell Davis' laboratory at Harvard Medical School working on the physiology of hearing. Understanding the source of Stevens' operationism is a matter of understanding this earlier unpublished paper.

This de-emphasis of Stevens (1936) and (1939), papers often taken to be more mature and accurate expressions of Stevens' operationism, is justified by their content and history. Stevens (1936), for example, appeared in and was explicitly written for *Philosophy of Science,* in an effort to influence that journal and the philosophers who read it ('Stevens Psy. as Prop. Sci.' (Boring to Stevens), dated September 1935, SSSP 2.10); as a result it extends in content beyond (1935a) and (1935b) only in its discussion of psychology's propaedeuticism. Stevens (1939), on the other hand, covers ground not covered in Stevens' earlier papers, in part because it is a review paper. Stevens' discussion of operationism, logical positivism, behaviorism, the "science of science," and their relation is, however, not always well thought-out, as is evident from Stevens' repeatedly confused discussion of Carnap's syntactic program. Stevens' reading knowledge of German was limited (G. Stevens, personal communication), so he likely encountered Carnap's program no earlier than Carnap's translated contribution to the first number of *Philosophy of Science* in 1934. By 1939 Stevens had read at least the Introduction and Part V of *The Logical Syntax of Language* in its 1937 English translation. Quite possibly he

heard Quine's three "Lectures on Carnap" at Harvard in November, 1934 (see Creath 1987), though they are not reflected in his notes or papers. In the course of presenting philosophy as the "*formal structure theory of the language of science*" in his (1934a, 9; emphasis in original), Carnap had distinguished connotative and formal "viewpoints" (p. 8) or "modes of expression" (p. 13) from which the logical analysis of a language may be pursued, and warned against confusing sentences in the connotative mode, which are about the language under discussion, with "object-sentences," i.e., sentences about ordinary objects. He presented sentences in the connotative mode next to the same sentences in the formal mode, noting that "in the case of several of these examples . . . only on translation [into the formal mode] do we see that we are dealing with assertions concerning the language" (ibid., 13; see also Carnap 1937, 288ff).

Stevens, in his 1939, conflated Carnap's *aspects of* a language with his own distinction between formal and empirical *propositions in* a language. Taking himself to be paraphrasing Carnap, Stevens claimed:

> There are two types of propositions: *formal* and *empirical.* The formal propositions are arrays of symbols without empirical reference. They are language, mathematics, and logic *as such.* Empirical propositions are those in which these arrays of symbols have been identified with observable events. (1939, 228; see also 223, 235, 242, 251ff)

This distinction led Stevens to deny semantic content to formal sentences (ibid., 251; 1936, 101), a position only apparently aligned with Carnap's (1937) view that the sentences of pure syntax are "without content" (p. 7), for Carnap would recognize many of Stevens' "formal" propositions as object-sentences. Stevens appears to have missed Carnap's point that in approaching language syntactically one does not deny "that the words and propositions have a meaning; one merely averts methodically from meaning" (Carnap 1934a, 10; see also 1937, 5). The confusion is compounded first when, having described formal propositions as "language, mathematics, and logic *as such,*" Stevens describes them as stating "the rules and procedure for combining words or symbols" (1939, 235; also 237), and again when sentences from a psychology text are sorted into "syntactical," "semantical," and "empirical" classes (ibid., 251ff), an exercise superficially resembling Carnap's translation of connotative sentences into the formal mode. Stevens' confusion did not go unnoticed; Boring complained that the distinction as Stevens drew it was "not clear . . . [or] validated," that it didn't "mean anything" to him, and was "not real or intelligible" (Boring to Stevens, 11 October 1938, SSSP 2.10).[5]

Stevens' confusion has little bearing upon his operationism, which was

[5]This and all subsequent passages from the letters of E. G. Boring are published here by permission of the estate of E. G. Boring.

developed outside the linguistic bent of the logical positivists. It suggests that Stevens (1939) was a compilation of not entirely understood sources hurriedly joined to Stevens' earlier views, an evaluation supported by consideration of its history. Despite "ennui" over operationism in late 1935 (Boring to Stevens, 26 October 1935, SSSP 2.10), in Fall 1937 Stevens consented to review recent philosophy of science for the *Psychological Bulletin.* Two years, and several exchanges with John McGeoch, the *Bulletin*'s editor, later, Stevens wrote McGeoch reporting his progress: "I suddenly decided to go to work. I went down to the library and carried out successive loads of books. . . . I am well into the middle of something I am calling, tentatively to be sure, Psychology and the Science of Science. I am not entirely sure what that means. . . ." (Stevens to McGeoch, 23 September 1938, SSSP 2.10). The 65-page typescript (including an annotated bibliography of sixty-six items) went to McGeoch three weeks later. Boring warned Stevens a few days before, "you have had emotional experience of sensing salvation in a new idea, and you are not able to evaluate critically. . . . You don't have time to get and consider deliberate criticism" (Boring to Stevens, 11 October 1938, SSSP 2.10). Boring's warnings went unheeded, and Stevens (1939) thus ought to be taken as a restatement of Stevens' views as of late 1934. Potential influences on Stevens' operationism can be assessed accordingly.

I will begin with P. W. Bridgman, the Harvard physicist often presumed to have influenced Stevens (see, e.g., Woodward 1990). Archival evidence establishes both that Stevens' first exposure to Bridgman came in late 1933 or early 1934, and that the central elements of Stevens' operationism were in place in Stevens' thinking by that time. The unpublished "Materialism", dated 1933 and perhaps written for Boring's seminary on consciousness in Spring of that year, presents the central elements of Stevens' operationism *sans* mention of Bridgman or operations. Indeed, this paper's "system, or outlook," which, Stevens claimed, would "[provide] adequate methodological premises for proceeding to the development of psychology as a natural science," was described as "derived from the writings" of Max Planck, Harold Chapman Brown, and K. N. Kornilov ("Materialism," 1933, SSSP 2.45). In fact, Stevens draws upon his experimental determinations of the relation of a tone's frequency and intensity to its perceived "volume"—the psychological "space" it takes up—to make his points. In these experiments, "the discriminatory response of the observer was related to . . . a frequency meter and a volt meter. . . . The relationship . . . can be taken as indicative of the manner in which the 'subjective' experience of tonal volume depends upon the frequency and the intensity of a sound stimulus" (ibid.). The motivation for this reliance on the "discriminatory response" in the scientific approach to tonal volume, moreover, is the desire for a science absent irresolvable disagreement. The same desire

brings Stevens to propose a "psychology of the other one," albeit not by that name: "The case is not altered when the experimenter serves as subject, for then it is upon his own responses that he bases his conclusions. . . . He observes his own consciousness indirectly, just as he observes the consciousness of a third person" (ibid.).

The themes of discrimination and the psychology of the other one are present in this early paper, then, and agreement motivates both. Further, these are deployed against the immediately given, which, Stevens insisted, is "a discriminatory act issuing from the nervous processes." "Those who talk of sights and sounds as immediately given . . ." Stevens added, "are following the *great delusion*" (ibid.; emphasis in original). While it is probably appropriate to place these themes with Stevens as late as Fall 1933, still absent the terminology of 'operations' and 'operationism', it is worth noting that they also appear (at times verbatim) in a notebook Stevens kept in 1932 (Private Notebook, 1932, SSSP 2.45). There Stevens also described knowledge as a matter of agreement: "one's discriminatory responses are not *knowledge* unless a sufficient percentage of the rest of mankind either have or admit the possibility of having the same sort of ability to respond" (ibid. p. 20; emphasis in original).

All the major elements of Stevens' operationism were in place at least by Fall 1933. None of Stevens' notes or papers from this time mention Bridgman or operations. Conversely, Stevens' notes for his presentation in Boring's Spring 1934 seminary on the data of psychology contain references to the "operational outlook" and are filed alongside Stevens' notes on Bridgman (1927) (Personal Notes, SSSP 2.45). It appears that between the middle of 1933 and early 1934 Stevens encountered Bridgman (1927) (or Bridgman himself) and formulated his own understanding of science, already articulated in some detail, in Bridgman's terms.[6]

That Stevens did so is hardly surprising, for Bridgman's conservative desire to systematically identify and eliminate those concepts whose flaws engender the disturbance of a scientific revolution (Moyer 1991, Walter 1990) resonates with Stevens' ideas, and at any rate Bridgman had invited the extension of the operational outlook to "all our habits of thought" (Bridgman 1927, 31). Moreover, Bridgman, an eminent and "tough-minded" experimental physicist, might have appeared as an ally to Stevens, who in Spring, 1934, was a year beyond his Ph.D. with little prospect for an instructorship at Harvard (Stevens 1968, 601), and was hoping for a National Research Council fellowship to support work on hearing with Davis. An extension of Bridgman's operational outlook to psychology, in short, promised to further not only Stevens' philosophy of science but his professional interests as well.

[6]Pace Koch (1992, 269) and Boring (1950, 656), Stevens was probably not introduced to Bridgman by Herbert Feigl, who had left Harvard a few months before Stevens' arrival in Fall, 1933. For Stevens' contact with Feigl, see below.

Still, Stevens' philosophy was his, not Bridgman's (cf. Koch 1992, 263–264). This was evident by late 1934 or early 1935, when Bridgman rejected the main tenets of Stevens' operationism in discussions with Stevens. As Bridgman later wrote A. F. Bentley,

> [Stevens] has talked with me at length about a couple of his papers before publication and professes to be most enthusiastic for "operational ideas". . . , but I simply cannot make him see that his "public science" and "other one" stuff are just plain twisted. I have also discussed with him his "basic act of discrimination" without making much impression. . . . (Bridgman to Bentley, 4 May 1936, Percy Bridgman Papers, Harvard University Archives, 4234.10.[7] Quoted in Walter 1990, 184)

Though Stevens' attempted alliance with Bridgman fell through early on, Stevens' operationism was unaffected. Mention of Bridgman in Stevens' publications is consequently guarded and critical (e.g. 1935a, 323–324; 1935b, 520; and 1939, 224–225, 227).

E. C. Tolman and B. F. Skinner stand out as psychologists who might have influenced Stevens' operationism. Stevens likely encountered Tolman's work in one of the ten psychology courses he had completed by 1934 (Records for S. S. Stevens, Graduate Records, Harvard University Registrar) or in his reading for the comprehensive exams in psychology, taken in May, 1932. Although Stevens' notes for his presentation in Boring's 1934 seminary include supportive references to Tolman (1932) (or possibly a preprint of Tolman 1935) (Loose Notes, SSSP 2.45), Stevens regarded Tolman as more competitor than ally. Boring regarded Tolman similarly. In notes for a talk, Stevens attacked Tolman's distinction between "independent" and "perspective" properties (Tolman 1935, 363; see Smith 1986, 110ff, for discussion), the former on Stevens' understanding "got at" by measurement, giving "absolutely correct values of these characters," the latter "essentially illusions." Stevens argued predictably that "in the two cases we are only comparing two discriminations" (Notes titled 'E. C. Tolman', for a talk at Clark University delivered 15 January 1935, SSSP 2.45). Stevens' attitude may well be the result of this criticism, or vice-versa—in either case, Tolman cannot be regarded as an influence upon Stevens.

Much the same can be said of B. F. Skinner, whom Stevens first encountered in 1931 while Skinner was a National Research Council Fellow in the laboratory of Harvard physiologist W. J. Crozier (Stevens 1974, 404). Skinner had cited Bridgman in his doctoral dissertation in late 1930, likely the first psychologist to do so (Skinner 1945, 291), and he and Ste-

[7]This and all subsequent passages from the papers of P. W. Bridgman are published here by permission of the Harvard University Archives.

vens no doubt encountered each other while both worked with Davis at the Harvard Medical School in 1932 and 1933. Skinner praised Stevens' operationism articles, writing of the first that it was "essentially what I have always supposed behaviorism to represent" (Skinner to Stevens, 16 June 1935, SSSP 2.10[8]) and calling the second "a damn nice piece of work" and "the best statement of the behavioristic attitude toward subjective terms now in print" (Skinner to Stevens, n. d., SSSP 2.10).

Divergent approaches to psychology overwhelmed their affinities, however. Skinner was disturbed by Stevens' emphasis on discriminatory capacities, complaining that *capacities* were less important than "properties organisms actually *do* use in setting up classes," and that at any rate it was too early in the game for discrimination. "If you approached the behavior of an organism as an object of scientific study . . ." Skinner wrote, "it would be a long time before you would reach the field of discriminatory capacity" (Skinner to Stevens, 16 June 1935, SSSP 2.10). Skinner may well have not understood or appreciated discrimination's central role in Stevens' operationism, suggesting that Stevens' views were formulated absent Skinner's influence. Different approaches to psychology render this plausible. The differences gave Skinner occasion to chide Stevens: "If you are really interested in the behavioral foundations of scientific method or scientific thought, why not get out of that historical psychological bog known as the field of sensation? Remember men, you're fighting for dear old Harvard" (Skinner to Stevens, n. d., SSSP 2.10).

C. I. Lewis also deserves mention in this context. Stevens took no philosophy at Harvard, but he undoubtedly heard Lewis lecture and may have read Lewis (1934) (although there is no good evidence that he did either). The influence of Lewis' "conceptual pragmatism" as expressed especially in Lewis (1929) is difficult to assess. On the one hand, several aspects of Lewis' (1929) resonate with Stevens' operationism, particularly Lewis' emphasis on agreement as a primary aim of his "reflective" philosophical method and his pragmatic analysis of agreement (pp. 20–21); his deployment of discrimination to answer questions about the reality of various entities (p. 13); and his repeated recognition of "common reality" as a "social achievement" (p. 111). Further, in a footnote (cited in Tolman 1932, cited in turn in Smith 1986, 100), Lewis sketched the argument against immediate experience later offered by Stevens:

> The supposition of a difference in immediate experience which is *not* to be detected through divergence in discrimination . . . is a notion very difficult to handle. Because such difference would, *ex hypothesi,* be ineffable. We can have no language for discussing what no

[8]This and all subsequent passages from the letters of B. F. Skinner are published here by courtesy of the B. F. Skinner Foundation.

> language or behavior could discriminate. And a difference which no language or behavior could convey is, for purposes of communication, as good as non-existent. (Lewis 1929, 112; emphasis in original)

Stevens' copy of Lewis (1929) is thoroughly annotated in pencil, and although there is no way of knowing when he actually read it, a blank check from a Palo Alto bank used as a bookmark suggests its purchase for an upper-level epistemology course for Winter 1930.

On the other hand, nowhere in Stevens' corpus is a debt to Lewis acknowledged. Further, Boring, in comments to Stevens, refers to Lewis as a "remote adversar[y]" ('Operational Basis of Psy, running comment' (Boring to Stevens), p. 7, SSSP 2.10). This is, no doubt, for good reason; Lewis (1929) defended both the given and the a priori (albeit from a pragmatic point of view—see Kuklick 1977), notions which Stevens strived to reject in any form. It is thus not clear what, if anything Stevens took, or thought he took, from Lewis.

Stevens' contact with W. v. O. Quine, who received his Ph.D. from Harvard in 1932 and was, with Skinner, a member of the first class of Harvard Junior Fellows in 1933, is easier to pin down. Stevens may have heard Quine lecture on Carnap in November, 1934, but by Quine's recollection they were first acquainted six years later when Stevens, Carnap, and Philip Frank organized a "Science of Science Discussion Group" at Harvard. Even then, according to Quine, there was "no significant link" in their "professional concerns," despite "congenial philosophical outlooks" (Quine, personal communication).

It will be no surprise, given Stevens' apparent isolation from local philosophers and his rudimentary knowledge of German, that in the course of developing his operationism he was hardly aware of logical positivism. It might have been otherwise had he arrived at Stanford as an undergraduate in time to encounter Moritz Schlick, there for the 1929 Spring semester, or not arrived at Harvard only a few months *after* Herbert Feigl left in Spring, 1931. As it was, Stevens' first exposure to logical positivism probably came via the first volume of *Philosophy of Science,* published in January 1934, in which Stevens read Carnap and Feigl. An outline of the paper which split into (1935a) and (1935b) records both Stevens' aforementioned misunderstanding of Carnap's syntactic program (Stevens there described language as having "two uses," formal and empirical ('Operationism' Outline, SSSP 2.45)) and his dissatisfaction with Carnap's "directly verifiable predicates" (as Alan Richardson has pointed out to me, this is likely a corrupted reference to the "directly verifiable propositions" Carnap mentions at the start of his (1934b)). These, along with Feigl's "language of data" (Feigl 1934, 429ff), Stevens opposed as akin to immediate experience. Prudently, Stevens choose not to criticize Carnap,

Feigl, or Bridgman—who had spoken of "unanalyzable" operations "apprehended . . . by personal experience" (Bridgman 1934, 105)—to great extent in publication, though he did do so privately (Stevens to Boring, 12 December 1934, SSSP 2.12). Instead, Stevens initially framed his views as an extension of "Bridgman and Carnap to [psychology]," for they had stopped "at immediate experience: 'unanalyzable' and 'directly verifiable predicates' respectively" ('Operationism' Outline, SSSP 2.45). This allowed Stevens to align his views with Carnap's and Bridgman's, while still permitting dissociation if necessary. However, this strategy was not employed in publication. Perhaps reflecting Stevens' disinclination for the linguistic turn, Carnap is only mentioned as the originator of a "linguistic operationism" less "directly applicable to psychology" than Bridgman's version (1935a, 323), and Feigl's "language of data" is criticized only briefly in a footnote in Stevens (1935b).

This survey of the range of potential influences on Stevens shows that it is a mistake to consider Stevens' operationism a manifestation of either Bridgman's views, behaviorism, or logical positivism, though it has some affinity with versions of the latter. Even apart from this, it would be natural to identify E. G. Boring—Stevens' advisor and a psychologist of enormous energy and influence—as a motive force behind Stevens' operationism. Stevens suggested as much in a tribute he wrote of Boring in 1968: "In 1935 a series of papers on operationism began to appear. . . . The papers appeared under my name, but it can be proved from page upon page of editorial criticism that large segments of those papers were generated more by Boring than by me" (Stevens 1968, 597).

Yet Stevens' attribution is in the end too generous. Boring's comments on Stevens' papers are indeed extensive. For the four drafts of Stevens (1935a) Boring returned twelve pages of typed, single-spaced running commentary. For the draft of Stevens (1935b) there are two pages of similar comments, for (1936) three pages, and for (1939) five—all in addition to marginalia and the conversations that Stevens preferred to writing. (Boring's comments are collected in SSSP 2.10 and quoted at length in Stevens 1968). Clearly there was ample opportunity for Boring to mold Stevens' operationism. Yet Boring's comments typically addressed style—an important matter to Boring, but of little significance in this context. Of the thirty-seven comments Boring offered on the first draft of what became Stevens (1935a) and (1935b), some paragraphs in length, twenty-four are concerned *solely* with style, e.g., with economical writing, the use of the first-person-singular, the avoidance of "moralizing," and the need for section titles.

The remaining thirteen raise points of contention between Boring and Stevens that turned often on issues central to Stevens' operationism. For example, four times Boring mentioned the indefiniteness of ostension and

its effect upon Stevens' reliance upon discrimination, complaining at one point that "you can not denote indigo merely by pointing to indigo objects. You have also to point to not-indigo objects. . . . You are here saying that discrimination is crucial. Well, you can't discriminate the absent. It is fundamental to show the [subject] in this case the not-indigo . . . and you won't have done a good job if you show him only five or six examples of not-indigo" ('Operational Basis of Psy, running comment' (Boring to Stevens), pp. 3–4, SSSP 2.10). Stevens' rigid distinction between concepts associated with different operations (1935a, 324) also met fierce resistance. "I have not the slightest doubt that I am right in this matter . . ." Boring wrote. "Unless you allow for generalization in your operationism, you wreck science" (ibid., 3). (Stevens' nominalism survived these attacks to be represented in his publications—see 1935a, 324.) On the other hand, a discussion of consciousness was omitted from subsequent drafts after Boring criticized Stevens' presentation of his views, in the process distinguishing his approach from Stevens': "If you are going to mention consciousness at all, you have got to be perfectly clear as to the operational distinction between the conscious and the unconscious. If you agree with 'Professor Boring' . . . then the *unconscious* is conscious, but it would be much nicer for you not to agree with him, and for you to get this distinction made operationally" (ibid., 7; emphasis in original). Perhaps underscoring his intellectual distance, Boring called the draft "difficult and not so very interesting." "Everybody . . . has wanted to air his opinions about [psychology's] fundamental principles . . ." he added, "and the paper makes the impression of being just one more such" (ibid., 8).

Boring's sentiments did not prevent him from writing text for importation into Stevens' paper. Amid his comments on the first draft of (1935a) he offered approximately seventy-three typed lines for this purpose. It is impossible to tell how much was intended to clarify Stevens' own ideas, and how much reflected Boring's views. The question is moot, however, as only eighteen of Boring's lines ultimately appeared in Stevens (1935a). Stevens was apparently not compelled to accept Boring's recommendations, even when accompanied by Boring's characteristic forcefulness. Not that Stevens rejected *all* of Boring's suggestions. Notably, he acknowledged Boring for convincing him to regard operational regress as a regress to "simple discriminatory responses" rather than experiences (Stevens to Boring, 12 December 1934, SSSP 2.10), a position which brought his operational viewpoint into greater coherence with his critique of experience.

In short, Boring's comments are detailed, critical, and often abrupt. They record not the molding of a disciple, but a healthy "give and take" between a vigorous senior professor and his energetic and (likely over-) confident former student. That Stevens valued Boring's paternal and sometimes obsessive attention to his philosophical project, while never-

theless regarding it as his own, is suggested by the penultimate line of a note to Boring, attached to the draft that would become (1935b). Stevens wrote, "I feel a great confidence that if this is so bad that I should spend the rest of my life trying to live it down, if it were published, you will kill it here and now" (Stevens to Boring, 30 January 1935, SSSP 2.10).

4. Conclusion. I have attempted to recover S. S. Stevens' philosophy of science and its intellectual sources, emphasizing that which did *not* influence Stevens' views at the expense of that which did, namely, Stevens' early scientific work on audition and Boring's (1933) views on consciousness. These are topics for another paper. Nor have I exhausted the plausible but non-actual sources. Rogers (1989), for example, has argued that operationism emerged and was maintained as "an incredibly useful solution to a number of very important problems that were impinging on the field of psychological testing" (p. 146). While some circumstantial evidence connects Stevens to the testing movement,[9] and indeed Rogers' thesis might be true of some psychologists, I would argue that it is false of Stevens, whose interests in methodology stemmed from local concerns, independent of psychological testing. The argument has, however, not been made here.

Among the other topics which I have not addressed but which warrant pursuit, one bears special mention. Given its opposition to immediate experience, endorsement of the social aspect of scientific knowledge, and thorough-going naturalism, the history of Stevens' operationism *after* its publication—as received not only by psychologists, but by philosophers and other scientists—is perhaps of both historical and contemporary interest. This dimension of Stevens' philosophy of science, properly explored, would reveal much not only about American psychology in the 1930s, but about the development of the philosophy of science in the United States.

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[9]Stevens encountered Lewis Terman at Stanford and T. L. Kelley at Harvard, both important figures in the testing movement, not to mention Boring, whose (1923) was significant to the debate over psychological measurement (see Rogers 1989, 146; Hornstein 1988, 10). Further, Stevens reported an early interest in intelligence measurement (Stevens 1974, 401).

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