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WHIG AND ANTI-WHIG HISTORIES[1]— AND OTHER CURIOSITIES OF SOCIAL PSYCHOLOGY

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In successive editions of the *Handbook of Social Psychology* (Lindzey, 1954), the focus of the history of the field shifted from the substantive ideas of nineteenth-century thinkers to the successful emergence of a *psychological* experimental social psychology in the twentieth. Countering this whiggish account, the dominant themes in the present issue involve attempts to portray two parallel paradigm shifts: from a "social" to an "asocial" social psychology, and from a broad-ranging theoretical-philosophical subject to a narrow experimental (psychological) science—changes initiated by Floyd Allport. But such a formulation may be called into question as another version of retrospective history—with inverted, anti-Whig valuations. © 2000 John Wiley & Sons, Inc.

THE WHIGGISH HISTORY OF THE *HANDBOOK*

The *Handbook of Social Psychology* (Lindzey, 1954) was an attempt to codify the recently emerged subdiscipline in its postwar flowering. Justifying the organization of the book, the preface claimed an editorial "bias in favor of . . . [beginning] with an awareness of the message of the past . . . " (p. viii). This historical introduction, written by Gordon Allport, outlined the philosophical-speculative roots and empirical beginnings of the rapidly expanding field. The chapter became a classic source in the history of social psychology.

Some time later, when I had begun to wonder why the development of our field failed to live up to the promises of knowledge production made by our "scientific method" doctrine, I gave Allport's story and his approach to history a closer reading. Sensitized by Thomas Kuhn's (1968) characterization of the main purpose of in-house "disciplinary" histories as intended to elucidate the concepts of the field, establish a tradition, and attract students, I was struck by the myth-making of Allport's presentation of the history of social psychology. All the faults of presentist history (Stocking, 1965) could be found in his portrayal of Auguste Comte as father of scientific social psychology, I thought. A more "critical examination" of our past leading to a better understanding of the present" (Samelson, 1974, p. 229) appeared called for. But I was not surprised when the editors of two mainstream journals rejected my manuscript, finding it, although "scholarly," of little interest to my colleagues—thus confirming Kuhn's point. In the third edition of the *Handbook* (Lindzey & Aronson, 1985), the Allport (G. W. Allport, 1985) chapter was reprinted once more with only minor changes (among them the omission of the Comte section).

A decade later, however, the fourth edition (Gilbert, Fiske, & Lindzey, 1998) eliminated Allport's chapter completely and without any comment (see Lubek & Apfelbaum, 2000, on Lindzey's reasoning). Obviously, by the end of the millennium practitioners the modern

1. Herbert Butterfield: [Whig history is] "the tendency of historians . . . to produce a story which is the ratification if not the glorification of the present" (1951, p. v).

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discipline no longer felt the need for a tradition reaching back to the pre-experimental nineteenth century and earlier. In its place, a new "usable past" was created for this young growth industry, showing pride in the fact that of all social psychologists ever alive, 90% were still living (Jones, 1998, p. 8). The tradition was now embodied in the successes of the *psychological* social psychology of the World War II decade (with brief references to its tentative beginnings in the preceding years). According to this new version, psychologists had, at least in terms of numbers of textbooks and journals, won the disciplinary struggle with sociology (p. 4). Some new organizational structures had attempted to preserve interdisciplinary linkages but had failed fairly quickly—for reasons not clearly stated, but presumably because "most of social psychology may be treated as a subdiscipline of . . . psychology" (p. 5). In the process, a casual reference to an (actually incomplete) definition of social psychology by Gordon Allport as focusing on the "thoughts, feelings, and behavior of individuals" (p. 3) served to show clearly that sociology was out of the running. The tradition had become one of empirical/experimental psychological social psychology, described in a victorious march from the institutions in (U.S.) Eastern urban centers into the hinterland and on to Europe. The need for an "origin myth" (Samelson, 1974) and a founding father of the thoroughly modern discipline had been supplanted by production statistics.[2]

Overall, the self-congratulatory message was one of massive growth. There had been some ups and downs in the past 50 years. Apart from pointing to a couple of glaring examples of "faddism" (risky shift, prisoner's dilemma), and some speculation about the waxing and waning of the popularity of certain phenomena/concepts, the authors listed several possibly relevant factors, such as the "zeitgeist" (!), the prestige of the innovator, or vulnerability to changes in ethical standards, but they did not attempt a serious analysis of the driving forces in any systematic way. The establishment had successfully weathered what had been called a crisis, but which in fact had been a only "a minor perturbation," produced by a "widespread need for self-flagellation, perhaps unique to social psychologists . . ."—overlooking the fact that there had been a "crisis" in the other social sciences, too. Although the challengers' criticism had "played into the hands of [hostile] political and academic forces . . . ," the effects were not noticeable in the production record (Jones, 1998, pp. 48–49).

In the first edition, Gordon Allport had considered the case for the importance of history as compelling but not all-compelling. There was no need for a "tour through a museum of oddities and antiquities" (1954, p. 3). What was required was a skillful selection in terms of the relevance of the past to the present. But in the universe of discourse of the new edition, the thoughts of the forerunners were no longer relevant. After all, any origin myth may be as good as any other as long as it spells out the proper message. And the message now was that "progress . . . . has been remarkable . . . ." The added statement that this progress could "not be measured either in terms of neat cumulative linear measurements or in terms of great reorienting breakthroughs but rather in the increased insights that gradually work their way into our cultural wisdom" sounded a bit like whistling in the dark. But "the future of social psychology [wa]s assured" (Jones, 1998, p. 49), as this thoroughly whiggish success story had it.

Not everybody's view of the present status of the field is as rosy. In a section entitled "The Missing Theoretical Foundation of Mainstream Social Psychology," David M. Buss (1996, p. 3) described the current field as too phenomenon-oriented. Just as the old astronomy

2. The author of the second quasi-historical chapter of the new edition did start out with a reference to the "Grecian philosopher" Aristotle, but quickly left this historical "anchor" behind to jump directly to whom *he* had selected as the "father of modern social psychology . . . Kurt Lewin" (Taylor, 1998, p. 60).

had been "filled with important observations of planetary motions, . . ." so current social psychology was filled with important empirically documented phenomena. But the field "currently lacks a powerful explanatory framework for explaining its panoply of phenomena" (p. 5). Even though I am not convinced that Buss's recommended framework, the new (neo-Lamarckian?) evolutionary psychology, is the proper solution, I believe that at least in part his charge against the mainstream is defensible. Though not identical in starting point or terms, Burnham's (1987) indictment of psychologists for their descent into specialist technology may point in the same general direction.

A New, Anti-Whig View

In somewhat different terms, the present special issue also raises questions about the current condition of social psychology and how it came about. Although the specific topics addressed vary widely across the contributors, a dominant theme in this issue involves attempts to describe or illustrate what are seen as two more or less parallel "paradigm shifts": one from a rich "social" to an impoverished "asocial" social psychology, and the other from a broad-ranging theoretical-philosophical subject to a narrow *psychological* experimental social psychology science—and then to pin the tail on the donkey(s), the leading one of which was Floyd Allport.

There can be little doubt that, at a general level, changes in these directions occurred in the mainstream of the developing discipline over the past century (cf. Morawski, 2000), and that Floyd Allport was one of the early exponents. But on closer inspection I do not find the treatment given here to these questions quite convincing.

Allport's "Program of an Experimental Social Psychology"?

As Floyd Allport later claimed in his autobiography, his 1924 book had ". . . *suggested* at least by *implication* the *possibility* of a new experimental science of social psychology" (1974, p. 9, emphasis added). Did these qualifications in phrasing indicate the retreat from a stance he had taken in 1924, or had there actually been no explicit announcement of a new "program"? When we look at the whole book itself beyond the first few pages, we discover that (a) the index does not contain any entry for an experimental social psychology program, experimental social psychology, or even experiment; (b) although Allport in the preface does mention the experimental method, he states that beyond presenting the "behavior viewpoint," the purpose of the book has been to "fit these [recent] experimental findings into their broader setting in social psychology . . ." (p. vi); and (c) in the rest of the book, Allport does not discuss more than a handful of experiments, primarily his own. Nor does he propose any specific new ones to add to the many nonexperimental observations and speculations he presents. Does this sound like the founding of a novel program of an experimental social psychology? Or is the later identification of a program, by others, only a retrospective creation, telescoping decades of subsequent developments into one book?[3]

In any case, however we choose to read Floyd Allport's 1924 text, we ought to look beyond what he may have said, to what he also did or did not *do*. When we explore his publication record, we discover that after his early and famous "social facilitation" experi-

3. Incidentally, such a view is reminiscent of the retrospective creation of J. B. Watson's behaviorist revolution in 1913 (Samelson, 1981)—although Watson's actual pronouncements had been much more provocative than Allport's had been.

ments, he appears to have conducted almost no additional experiments. Instead, one of Allport's less publicized but perhaps more noteworthy accomplishments was the establishment, at Syracuse, of the first Ph.D. program in Social Psychology by the late 1920s.[4] When we look now at the published work of his students in this early program, such as Daniel Katz, Dale A. Hartman, Richard L. Schanck, Richard S. Solomon, and George Vetter, we find here that they, too, performed practically no laboratory experiments. They carried out mostly field studies of one form or other, and what at the time were perhaps called experiments (involving only some variation in antecedent conditions and the subsequent determination of its effects), but without the elaborate methodology required later on, as spelled out—caricatured?—by Danziger (2000) as limited to effects that were "proximal, local, short-term, and decomposable." Given that Allport described himself as a partner in these dissertations and co-author of several of them, it is unlikely that the designs used in these studies were selected over his objection. And later on, none of his former students, as far as I know, became members of that purist group of founders of the Society of Experimental Social Psychology in the 1960s—or are known for creating another popular social experimental paradigm.

But enough of Allport's imputed "Program for an Experimental Social Psychology." Any subsequent methodological paradigm shift, or rather, slowly accelerating trend, does not appear to have been his or his students' doing, even if he had been in fact the first to suggest it. If his name was indeed invoked in support of such a "program" by later generations, we need to explain why these later generations chose, without his prompting, to institute such a program and why they would have found it useful to invoke his name. As for Allport himself, already by 1940 he had moved on, to the work to which he would devote the rest of his life—his event-system theory—beyond laboratory experimentation and individualist social psychology.

A SOCIAL INDIVIDUALISM?

In reference to his alleged creation of an asocial social psychology of isolated individuals, here is what Allport had to say in his autobiography: "after 1927 . . . I did not make any . . . statements implying the all-sufficiency of purely individual processes in explaining social phenomena. I soon came to see that in my [1924] book . . . I had not been sufficiently cognizant of the problem posed by what the sociologists were calling the 'social reality,' nor was I sufficiently appreciative of their efforts to solve it. All my work . . . after 1927 reflects the fact that I, too, was aware of this problem and was trying to help toward its solution" (F. H. Allport, 1974, p. 18).

Regarding the agenda of his *Institutional Behavior* (1933)—not a monograph but a series of essays many of which had been published previously, mostly in *Harper's*—he wrote: "By this time, I had come through my period of 'pure individual determinism' I had espoused in my *Social Psychology,* a view which sociologists had rightly criticized as slighting the actual reality. . . . In *Institutional Behavior* I made what was . . . a beginning by combining my critique of group hypostatization and the fictions connected therewith with an analysis of more substantive aspects" (pp. 16–17).

Thus it appears that any characterization of Allport's position as asocial individualist may be based on a presentist reading of selected quotes from some of his early writings. Such a reading omits first the contemporary context of his arguments, overlooks some distinctions

4. I am not aware of any other such program until after World War II.

he had made in his discourse, and ignores the fact that he, like other human beings, changed his stance and developed his ideas over time. As for the context, one should remember that advocates of the "new psychology" had seen part of their task as the deflation of rampant spiritualism and belief in immaterial ghosts or spirits. Thus part of Allport's campaign was an ontological argument that there existed no disembodied minds hovering over groups or crowds, notwithstanding current linguistic usage.[5] Yet while denying the (ontological) reality of group minds, he was not denying the (psychological) reality of the "group fallacy" and the effects in individuals' minds.

As he himself said later, he may have overstated his argument, when he was actually "not attacking the evident truth in the doctrines of social causality but [only] the failure to give them more specific meaning" (F. H. Allport, 1974, p. 15). Had he initially ignored the social dimension of individual cognitions, etc., as his early description of (only one type of) social influence as caused by the "sights and sounds" of others present—if taken literally—seemed to demonstrate? Soon afterward he introduced such concepts as the impression of universality, pluralistic ignorance, and private vs. public attitudes, all concepts that involved the awareness of others and their orientations to both each other and the nonsocial world, what later theorists called "simultaneous orientation" (Newcomb, 1953). This elaboration—of humans as existing in a "mutually shared world" (Asch, 1952) in which people in a group were not carbon copies of each other but instead played complementary roles—is the general case. It takes social psychology far beyond the more obvious but limited phenomenon preoccupying the early social psychologists, that of *similarity* produced by "holding a belief qua a member of a group" (Greenwood, 2000).

This simple older formula (somewhat like the naive view equating heredity with similarity) had turned out to be problematic, as empirical work progressed. In the first place, social psychologists found it quite difficult to differentiate *practically*—though not in the abstract—between any concrete individual's "individually vs. socially held beliefs." This distinction became even more difficult once researchers realized that any individual belongs not to one but to a variety of overlapping and non-overlapping groups. Furthermore, s/he did not even need to belong "objectively," but only might use a group (or person) as *positive* or *negative* reference group in the adoption of certain beliefs (which did not necessarily match the beliefs of whichever group the individual used as an anchor). In other words what Greenwood takes to be the early theoreticians' properly social conceptualization turns out to be a generic one, if not a stark oversimplification (and perhaps in Allport's language another version of the group fallacy). This enormous complication[6] of an initially simple concept may have been one of the reasons why the struggling social psychologies of midcentury were eventually overwhelmed by a simpler behavioristic and asocial formula. An academic structure based on departments competing for power and funding and hostile to "soft" interdepartmental thinking also played a part.

Finally, Allport's ideological agenda in writing *Institutional Behavior,* a set of essays with "no pretense of scientific knowledge" (1933, p. x), may have been misread. Although strongly arguing against group fallacies and for the reality of individuals, his was not the rugged individualism of the social Darwinists or of the free marketplace. His thesis was that "an individual's personality can be fully expressed only when he is given freedom of choice

5. As in "the juridical tradition to the effect that when twelve men come together to take joint action there are not twelve persons present, but thirteen (the thirteenth being the corporate mind of the group), the collective fiction becomes a bit grotesque" (p. viii).

6. For instance, the "social adjustment" function of attitudes, in Smith, Bruner, & White (1956).

and responsibility in an environment composed of other free and responsible individuals" (p. x). He attacked the rhetorical obfuscations of "legal process" and "miscarriage of Justice" in the trial of Sacco and Vanzetti, which hid the failure of individuals to accept their responsibilities behind collective abstractions. He attacked the consumer culture in terms that sound almost contemporary. He asked, "Are not the ideologies of today as full of illusory logic and superstition as those of yesterday? . . . [and] still abet, even while they conceal, the power and greed of men who would exploit their fellows?" (p. ix). One may not agree with his views on individualism and responsibility—although having experienced real-world collective ideology on my own body I personally might rather choose his position as the lesser evil. In either case, we should grant him his right to try a new orientation: an attempt to reinterpret both institutions and society in terms of the behavior of individuals (without holding him responsible for more recent asocial positions). This attempt soon led Floyd Allport (1940) beyond individuals into his collective event structure system—an approach little noticed and never taken seriously by the mainstream or by critics of the mainstream.

Thus it appears that some of the claimed double-paradigm-shift history presented may be another version of Whig history. Only now the founder allegedly inspiring the later development turns out not to be a hero but a villain; the tale is a presentist tale with inverted valuations (from good, progress, success to bad, regress, failure), but still with the retrospective errors of oversimplification of process, neglect of context, misinterpretation, anachronism (Stocking, 1965). Floyd Allport was morphed from a pioneer trying out a new approach into the Pied Piper of destructive experimental methodology and asocial, individualistic social psychology.

Falsificationism and Omissions in the Historiography of Social Psychology

Beyond such accounts, which neglected the historical complexities, one of the present analyses also reminds me a bit of Popperian conceptual reconstructions of the history of science, an approach superseded in the 1960s by more fine-grained and context-sensitive historical research. But another earmark of Popper's (1962) program is absent (as in so much historical work): his idea of falsification. The validity of historical arguments is not easy to establish, and usually is judged by the extent to which they fit with one's prior biases. Therefore it is incumbent on serious historians, in my opinion, to try to go beyond the quotes and documents supporting their working hypothesis, to look for other evidence that might falsify their thesis and put a different and unanticipated spin on their story. Without such a search, we are in danger of finding only what we had known already before we started—and some of the present contributions appear to have this flavor. In my own work, the most exciting steps involved finding material that slowly forced me to see the matter in a new and unaccustomed light (as for instance now in my discovery of another Floyd Allport, different from my stereotype).

Some time after calling, in 1973, for a new critical look at our history, I realized that there was not just one, but an array of different social psychologies out there, more like isolated or warring tribes than a scientific "community" (cf. Good, 2000). I decided then that, in order to understand this situation, I first had to understand what had happened in the parent discipline of general psychology. The modern drift toward rarefied experimentation first gathered speed and power in the (a-social) reductionism of behavioristic S-R reinforcement theory. As the fragmented social psychology neither shared a unifying credo nor possessed an organization powerful enough to control programs and positions, the domination of experi-

mental psychology by neo-behaviorist doctrine and practice spilled over into the rapidly expanding postwar subdiscipline. Thus my personal view of development of our field is not of a stream flowing straight from its source in Floyd Allport's program to the asocial shallows of the postwar period. Instead I see it as a set of rivulets, some of them stagnating, dammed up, or evaporating (cf. Cherry, 2000), and others swept up in the larger stream originating elsewhere, if still maintaining a more or less distinctive coloration. In order to chart the intricacies of this process, it will be necessary to provide a much closer account of the organizational structure and developments of the field, the distribution of positions of power and gate keeping, such as editors, reviewers, officials at funding agencies, and their interpersonal networking. Given the inaccessibility of the relevant sources, this task will be much more difficult than the analysis of published theoretical formulations. Some initial forays into these issues can be found in the work of Ian Lubek (e.g., Lubek, Thoms, & Bauer, 1992). But in this century, (social) science had changed from the work of individual thinkers into a vast professional enterprise geared to produce data sets supporting (at the 5% level) a multitude of little hypotheses within competing quasi-ideological orientations.

Lastly I would like—circling back to my beginning—to remark on one of the ironies I find in our textbook histories. The success story in them totally omits the fact that some of their respected heroes and innovators later in life found their old approaches wanting and forswore them totally, at the same time as novices in the field were being taught to follow in the old (abandoned) footsteps. To some degree this is true of Floyd Allport—who had moved from individual social psychology to his collective structure theory, in which individuals entered in only segmentally to keep the system's cyclic processes going. This is even more true of Leon Festinger, a major promoter of the lab experiment. His disciples turned him into the major figure of the field (even into the father of experimental social psychology) at the time that he himself was leaving social psychology for perception and sensation. Later, finding its "new knowledge . . . not impressive enough," he totally abandoned psychology, "closed [his] laboratory, deserted experimental methodology, and decided . . ." to pursue the important problems of human behavior in the quite nonexperimental field of paleo-archeology instead (Festinger, 1983, pp. ix, x).[7] The irony here, of course, is that the textbook writers never acknowledge Festinger's (or Floyd Allport's) negation, but only his earlier heroic deeds for the field, while unaware neophytes struggle on to solve puzzles in the prescribed style.

Perhaps one might conclude that, after a quarter century and in spite of several encouraging beginnings, "the history of social psychology, as a critical examination of our past . . . still remains to be written" (Samelson, 1974, p. 229)?

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7. Something similar might also have happened to the restlessly moving Kurt Lewin had he not died early.

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