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# Why was there no place for personalistic thinking in 20th century psychology?

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## ABSTRACT

The present article identifies two major impediments to personalistic thinking in 20th century psychology: (1) positivism, and (2) a methodological commitment for which the term 'statisticism' has been coined. After explaining both that and why these two traditions have been so fundamentally inhospitable to Stern's ideas, it is suggested in the paper's conclusion that the gradual demise of these heretofore dominant -isms renders more favorable the prospects for personalistic thinking in 21st century psychology.

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The conceit of this special issue is that the personalistic perspective in psychology formulated by William Stern (1871–1938) during the first three decades of the 20th century might at long last earn some pride of place in the 21st. This very conceit begs the question posed in the title of the present article. Accordingly, the discussion that follows focuses on two broad doctrines that came to dominate mainstream thinking in psychology during Stern's time: (1) positivism, and (2) positivism's handmaiden, an orientation I propose to call *statisticism*. After establishing both that and why these two -isms were so contrary to Stern's thinking, it will be argued that to the extent that the grip of these two -isms on contemporary thought is loosening, the prospects for critical personalism in 21st psychology are improving.

### 1. The disciplinary context of critical personalism's historical emergence

It was 1900 when, as a 29 year-old *Privatdozent* at the University of Breslau (now Wroclaw, Poland), William Stern initiated that new sub-discipline of scientific psychology he proposed to designate 'differential psychology' (Stern, 1900). As Stern envisioned it, this sub-discipline would not replace or in any way compete with the general-experimental psychology formally established by Wilhelm Wundt (1832–1920) at Leipzig some two decades earlier, but would instead complement the general-experimental psychology

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by investigating just those psychological and behavioral differences between individuals (and groups) that were of no interest to the general-experimentalists of the day. Stern's guiding conviction was that attention to such differences would serve to highlight the need within psychology for a viable conception of human individuality. Meeting this need would be, according to Stern, the preeminent challenge facing the New Science as it moved into the 20th century (see Stern, 1900, p. V).

In 1911, Stern published a sequel to the 1900 book under the title *Methodological Foundations of Differential Psychology* (Stern, 1911). In comparison with the 1900 book, this latter work offered a much more extensive and technically detailed treatment of the basic concepts and research methods of differential psychology. *Methodological Foundations* quickly became a major reference work for differential psychologists not only among Stern's countrymen but among differential psychologists in the U.S. as well.

Due in no small part to these two works, but also to the important contributions Stern made in various substantive areas of empirical psychological research, including intelligence testing, child psychology, forensic psychology, and psychotechnics (see Bühring, 1996), he became, during his lifetime, a widely known and highly respected figure within the discipline (Bühring, 1996). Yet for all of his deserved renown during the first third of the 20th century, there was no time in Stern's life when he thought of himself exclusively as a psychologist. On the contrary, he always regarded himself equally – or even more so – as a philosopher, firmly convinced that no psychology could be adequate that was not firmly grounded in and critically informed throughout its intra-disciplinary sub-specialties by philosophical considerations. This conviction even surfaced in the 1911 text despite his primary concern in that text with methodological issues. Thus did he write in the book's preface:

"That my conception of the structure of the human individual and of psychological differentiation is not uninfluenced by my fundamental philosophical convictions is obvious. But since this book is devoted to the founding of an empirical science, I have reduced the philosophical aspects of the work to a minimum. For the justification of ideas many of which are discussed here only too briefly, the reader is referred to my philosophical book.<sup>1</sup> But I hope that the usefulness of the present work is not dependent upon agreement with the author's philosophical assumptions (which on many points deviate in non-trivial ways from the currently prevailing opinions)." (Stern, 1911, p. v; parentheses in original)

### 1.1. Stern's furor metaphysicus vs. the ascendant positivism

Evidence is scant that many of Stern's contemporaries within psychology ever bothered to consult his "philosophical book" as a complement to his methodological and empirical contributions (Lamiell, 2007). On the contrary, by 1911 psychology and philosophy were already well on their way toward a divorce, and in the immediately following years the enthusiasm over the prospects of a split was high in both disciplines (cf. Wundt, 1913). For reasons already indicated, however, Stern's thinking ran decidedly counter to this trend. Indeed, one can imagine that the ink on the pages of the aforementioned 1900 differential psychology text was scarcely dry when on July 31 of that same year Stern penned a letter to his friend and colleague, the Freiburg philosopher Jonas Cohn (1869–1947), expressing what he would later describe (Stern, 1927) as his *furor metaphysicus*:

"I am gradually moving away from psychology and becoming more and more a philosopher, and am carrying around with me so many ideas which will take me many years to formulate... What we need above all is a comprehensive world view, one that relates the psychological and the physical, that is anti-mechanistic, that is vitalistic-teleological<sup>2</sup>; one in which modern natural

<sup>1</sup> I take Stern to have been referring here to the book he published in 1906 titled: *Person und Sache: System der philosophischen Weltanschauung. Erster Band: Ableitung und Grundlehre [Person and Thing: A Systematic Philosophical Worldview. Volume I: Rationale and Basic Tenets]* (Stern, 1906). This was the first of the three volumes Stern published under the umbrella title *Person and Thing*, the series in which he set forth in detail that world view he called 'critical personalism.' Volumes II (*Die menschliche Persönlichkeit [The Human Personality]*) and III (*Wertphilosophie [Philosophy of Value]*) would not appear until 1918 and 1924, respectively (Stern, 1918, 1924).

<sup>2</sup> In later work, Stern would explicitly distance personalism from vitalism (see Stern, 1917, 2010), but he remained committed throughout his scholarly life to a teleological perspective (see, e.g., Stern, 1917, 2010, pp. 43–44).

science dogma is reduced to its true – that is, relatively inferior – value. This is a huge task, but I will work on it as I can." (Stern letter to Cohn, July 31, 1900; Lück & Löwisch, 1994, p. 33)<sup>3</sup>

Though it would in fact take Stern many years to formulate his ideas fully, it does appear that he got started right away. A small monograph titled *Preliminary Considerations for a World View*, though not published until 1915 (Stern, 1915), was actually written in 1901, so Stern must have been working on the text within a few months of this letter to Cohn. Stern attributed the publication lag in part to his youthful uncertainty, but also in part to his sense that the intellectual climate at the time was not especially favorable for the discussion of philosophical ideas (Stern, 1915; see also Stern, 1927).

In *Preliminary Considerations*, Stern posed the question 'What is a world view?' and then elaborated his decidedly neo-Kantian answer as follows:

"A facsimile of the world, an absolutely objective reflection of what is, in and of itself, is not possible for a mere mortal. *There is no object absent a subject.* [But at the same time], a worldview recognizes a positive reversal of this notion: *There is no subject without an object.* Because even if I cannot grasp the world in and of itself, I nevertheless grasp it as it is for me. Even if the human is no utterly neutral and smooth-surfaced mirror that faithfully reflects what is out there, but is instead a prism that refracts the world's rays of light into thousands of color gradations, it is nevertheless the light of the world that is thus refracted. A worldview is more modest than the old metaphysics, but it is also more demanding than skepticism and positivism. A world view renounces the notion of metaphysical knowledge in the sense of absolute truth, but it affirms the metaphysical impulse toward the greatest possible unification of all convictions and evaluations, and it strives to satisfy this impulse to the fullest degree humanly possible." (Stern, 1915, p. 4)

Without doubt, Stern's explicit embrace of metaphysical considerations placed him out of step with the mainstream thinkers of his day. After all, even before the 19th century had run its course, Stern's own mentor at the Friedrich Wilhelm University of Berlin (today the Humboldt University) and, later, senior colleague at the University of Breslau, the respected and influential Hermann Ebbinghaus (1850–1909), had publicly declared the alignment of mainstream psychology with a conception of scientific activity being promulgated by Ernst Mach (1838–1916), whose views would prove highly congenial to the logical positivists in their concerted attempt to rid scientific psychology of all traces of metaphysics.

An especially revealing statement by Ebbinghaus along these lines emerged in his 1896 critique of the famous call for a *verstehende* psychology that had been issued by Wilhelm Dilthey (1833–1911) two years earlier (Dilthey, 1894; Ebbinghaus, 1896). In a key passage, Ebbinghaus rebuked Dilthey for having attacked a psychology that no longer existed:

"Dilthey's reasoning runs as follows: (1) the scientific nature of physics is based on the presumption of the oft-mentioned qualitative and quantitative equation between causes and effects; (2) the explanatory psychology adheres to the ideal of science on the model of physics, (3) therefore psychology must share this presumption. But this argument errs in its very first claim. It may be that the majority of natural scientists holds to the assumption that the external world can be explained in mechanistic fashion, but the scientific status of their work is not dependent on this idea. One should recall the writings of E. Mach, who never tired in his efforts to [establish that] the mechanical explanation of things is not a necessary component of genuine and truly scientific investigations. Rather, when such occurs at all, it is a supererogatory matter, so to say ... [To see how Mach's ideas apply in psychology] one need look no further than to the law of association. Psychologists see a causal relationship in the co-occurrence of two sensations based on the fact that, over a series of instances, the mental image of one produces the other. No one claims on the basis of such a relationship that the effect must somehow be contained within the cause, or that there must be some sort of quantitative equivalence between the two. Indeed, however one might construe the process, it is difficult to understand what might be meant by such a claim." (Ebbinghaus, 1896, pp. 185–186, emphasis added)

<sup>3</sup> This and all other translations of German texts included in this article are those of the author unless otherwise noted.

During Stern's years as a doctoral student in Berlin (1888–1893), he had been greatly inspired by Ebbinghaus's experimental work, seeing in the discipline of such work real possibilities for extending the understanding of human mental and behavioral life beyond anything that could be achieved through metaphysical deliberations alone. However, Stern never embraced the positivism with which Ebbinghaus's views were so compatible. Moreover, in his intellectual autobiography published in 1927, Stern explicitly mentioned his regret over the fact that during his student days he had not availed himself more of Dilthey's tuition (Stern, 1927).<sup>4</sup>

Of course, mainstream thinking in early 20th century psychology aligned itself closely with the views of Ebbinghaus (see, e.g., Koch, 1985; Toulmin & Leary, 1985), and in the intellectual climate created by that alignment Stern's personalistic perspective could not possibly have thrived. So if, as Danziger (1979) and Blumenthal (1975) have persuasively argued, the positivist movement within psychology led to the repudiation of Wilhelm Wundt's views, the same can be said of that movement's untoward consequences for Stern's critical personalism.

## 1.2. Stern's critical embrace of statistical methods vs. an ascendant statisticism

Alongside positivism, a closely related but nevertheless distinct historical development of enormous – and for critical personalism decidedly untoward – consequence in 20th century psychology was the rapid proliferation of a belief in the power of statistical methods to reveal the empirical regularities – i.e., the 'lawfulness' – of human behavior. Wittingly or otherwise, mainstream thinking in psychology adopted the view of the 19th century historian Henry Thomas Buckle (1821–1862), who had argued that from carefully compiled statistical facts "more may be learned [about] the moral [i.e., behavioral] nature of Man (*sic*) than can be gathered from all the accumulated experiences of the preceding ages" (Buckle, 1857/1898, p. 17, brackets added).<sup>5</sup> I propose to call this unbridled faith in statistical concepts and methods *statisticism*, and insofar as this -*ism* entails an "obsessive devotion to or veneration, for" statistical evidence as the *sine qua non* of genuinely scientific knowledge in psychology (and other behavioral sciences), it may be said to qualify as a *cult*.<sup>6</sup> Ironically, some of Stern's seminal contributions to differential psychology, discussed above, quite possibly contributed to the establishment of this cult, though of course that was far from his intention.

The "Wundtian" or "Leipzig" model for experimental psychology (cf. Danziger, 1987, 1990) was not a discipline guided by statistical thinking about aggregates of subjects who have been 'sampled' from and can hence be regarded as 'representative' of 'populations.' On the contrary, the quest of the early experimentalists in psychology was for knowledge of the general laws presumed to regulate certain basic aspects of human consciousness, and since 'general' in this context meant *common to all*, the only sensible way of proceeding was via controlled studies of individual subjects, one at a time (Lamiell, 2003). Wundt denied neither the possibility nor the desirability of a psychology directed toward the study of 'higher order' mental and behavioral phenomena that would be informed by comparisons between individuals and groups, but such comparisons would not necessarily entail statistical considerations and, in any case, a *Völkerpsychologie* or 'cultural psychology' would be a distinctly non-experimental discipline prosecuted outside of the boundaries imposed by controlled laboratory investigations.

It was the adequacy of this latter position that was challenged by the differential psychology that Stern envisioned. Thus, he himself wrote that with the founding of the new sub-discipline the Wundtian "bifurcation of method" was no longer adequate, and that

" ... it has been necessary to modify the experimental approach so as to make it possible to investigate large numbers of persons with respect to more complex psychological processes and

<sup>4</sup> In a letter to Cohn dated May 17, 1909, Stern related that he had said a few words at Ebbinghaus's burial earlier that year, "in spite of everything that has long since distanced me from him" (Lück & Löwisch, 1994, p. 76). The intellectual gulf between Ebbinghaus's views and Stern's own is undoubtedly a large part of what Stern had in mind here.

<sup>5</sup> An extremely valuable treatment of the history of statistical thinking, on which I have drawn extensively in several works, has been provided by Porter (1986).

<sup>6</sup> The definition of *cult* on which I rely here is given on p. 321 of the 1975 American Heritage Dictionary of the English Language, New College Edition.

characteristics. Experiments entailing psychological ‘tests’ have been devised... This development has necessarily entailed the extraction of precise psychological findings from large-scale investigations involving a great many research subjects, and this in turn has meant that data acquisition procedures and statistical methods have made their way into our discipline.” ([Stern, 1914](#), p. 416)

As a differential psychologist, then, Stern was clearly both an advocate and a practitioner of the ‘large-scale investigations relying on statistical methods’ to which he alludes in this passage. However, his *critical* understanding of these methods and of the kind of knowledge that can be secured through their application incorporated a crucial distinction that was not widely grasped by his contemporaries and, as a result, was never securely incorporated into mainstream thinking. This was the distinction between knowledge of *individuals*, on the one hand, and knowledge of *attributes* with respect to which individuals have been differentiated, on the other (cf. [Lamiell, 1997](#)).

Stern’s understanding of this distinction was unambiguously incorporated into his elucidation of the four basic research schemes proper to differential psychology ([Stern, 1911](#)), which he depicted in tabular form on p. 18 of *Methodological Foundations* ([Stern, 1911, 1994](#)). There, ‘correlational’ studies of *many individuals with respect to two or more attributes* are clearly – and correctly – classified as yielding knowledge of ‘*attributes*’ (i.e., individual differences variables). In contrast, knowledge of individuals would require a form of investigation Stern called ‘psychography,’ entailing the study of *single individuals with respect to many attributes*.

However, as the neo-Galtonian paradigm for psychological research supplanted its Wundtian predecessor (cf. [Danziger, 1987, 1990](#)), mainstream thinking came to be dominated by an understanding of the statistical findings yielded by large-scale correlational studies according to which knowledge about variables in terms of which individuals have been differentiated just *is*, at one and the same time, knowledge about the individuals so differentiated (cf. [Lamiell, 2003](#)). With this (utterly misguided) development, the crucial distinction that had been drawn by Stern between these two kinds of knowledge was obliterated. This development meshed perfectly with the ascendant positivism discussed earlier in that it obviated all further concern for a theoretical/philosophical conception of persons that somehow went beyond what could be captured empirically by the study of *person variables* conforming to that model for inquiry that [Stern \(1911\)](#) had designated ‘correlational’ (see, e.g., [Thorndike, 1911](#)). It was just this development that led to the gradual transformation of the founder of differential psychology into one of the discipline’s most pointed critics.

Elsewhere, I have documented this historical development in some detail (see [Lamiell, 2003, 2006](#)). For present purposes, the following should suffice to make clear Stern’s settled position on this matter.

Following several weeks of travel in the U.S. in the fall of 1929, Stern published an article in which he shared his observations of prominent trends in American psychology. In this context noted the following:

“The face of American psychology is determined much more by the method of tests than by laboratory experiments... Since World War I, during which the entire American army was tested by means of a simple standardized intelligence test, the method of testing has been extended to an astonishing – and at times nearly worrisome – degree... When 17 years ago I introduced the concept of the ‘intelligence quotient’ as a principle of measurement for such intelligence tests, I had no idea that the ‘I.Q.’ would become a world-wide formula and one of the most frequently used technical expressions in psychology in America... But beyond that: now countless batteries of tests have been developed, standardized, and put into practice to measure countless other psychological functions: spatial perception, manual dexterity, attention, suggestibility, knowledge, mathematical ability, character traits, etc. always with emphasis on the objective, quantitative population norm, with respect to which the individual case is then scaled. ... [I]n America the major objective seems to have become that of mastering the method, and so to obtain quantitative values which can then be inter-correlated and statistically evaluated. [The] danger of mechanization here [is clear], ... and it is to be hoped that the high point of the testing culture will soon be overcome.” ([Stern, 1930](#), pp. 50–51)

The last statement of this quotation points unambiguously to Stern’s anti-mechanistic views, and to his deep concerns over the direction in which personality psychology was headed. In a subsequent

publication he would explicitly criticize attempts to represent persons in terms of what he termed “polysymptomatic profiles” of the sort that investigators would construct on the basis of series of standardized trait measurements, writing that such an approach leads

“... to an array of multifaceted empirical findings which are then combined in summary fashion into a profile or a listing of traits. This is now widely regarded as ‘personality research,’ and this is especially true in America.” (Stern, 1933, pp. 60–61).

Alas, the concerns Stern expressed here found little resonance among his contemporaries – the best efforts of Gordon Allport (1897–1967) and a relative handful of others notwithstanding (see, e.g., Allport, 1966; cf. Lamiell, 2003). What prevailed instead was the relentless pursuit of a practically useful and (seemingly) scientifically viable answer to chemistry’s Periodic Table of Elements (Lamiell, 2000). Lately, this effort has climaxed in the widespread consensus that “five factors are both necessary and reasonably sufficient for describing at a global level the major features of personality.” (Goldberg, 1993; McCrae & Costa, 1986, p. 1001)

It was thus to no avail that more than five decades prior to the publication of this statement, Stern had insisted that

“[t]he person is a unified whole, and has depth. ... A human being is not a mosaic, and therefore cannot be described as a mosaic. *All attempts to represent a person simply in terms of a sequence of test scores are fundamentally false.* (Stern, 1929, pp. 63–64, emphasis added)

Within the constraints imposed by the thinking that came to define mainstream personality psychology during the 20th century, there was simply no room in such thinking for a genuinely personalistic orientation.

## 2. And now?

Both positivism and statisticism were sparked and have since been fueled by the widespread desire to distance scientific psychology as far as possible from considerations of an essentially philosophical nature.<sup>7</sup> Insofar as the obscurity of personalistic thinking heretofore has in fact been a casualty of those historic commitments, it would seem that the prospects for personalistic thinking in 21st century psychology hinge importantly on a renewed appreciation for the indispensability of sound *conceptual* analyses to the integrity of the larger scientific enterprise. Fortunately, recent years have brought clear indications of developments in this direction, attributable to an ever-widening grasp of the heavy price in conceptual confusion that psychologists have paid for alienating themselves from philosophy. Two examples of this having rather direct implications for personalistic thinking can be mentioned here.

One is to be found in the recent work by Bennett and Hacker (2003), who have brilliantly analyzed countless instances in the published literature where cognitive neuroscientists have committed the so-called ‘mereological fallacy’ of attributing to *parts* of persons, e.g., to eyes or to brains or even to neurons, psychological functions such as perceiving, feeling, or judging that can properly be attributed only to *persons*. The need for a viable conception of ‘persons’ is, of course, obvious here, and in this light the potential relevance of Stern’s long overlooked ideas becomes manifest. In any case, Bennett and Hacker’s overriding objective has been to impress upon contemporary cognitive neuroscientists an appreciation for the point that the confusion embedded in the mereological fallacy is not an empirical one and hence cannot be resolved through *experimentation* of any sort whatsoever. What must be recognized, they argue, is that the confusion is a *conceptual* one and thus falls within the province of *philosophical* analysis. Grasping this basic point, Bennett and Hacker (2003) explain, can only contribute to the overall *betterment* of cognitive neuroscience.

A second development boding well for the prospects of personalistic thinking in the 21st century is the steadily growing recognition of the aptness of the distinction that Stern (1911) correctly drew between knowledge of individuals, on the one hand, and knowledge of attributes with respect to which individuals can be differentiated, on the other (see, e.g., Cervone, 2005; Harré, 2006; Lamiell, 1981,

<sup>7</sup> I will not belabor here the obvious point that each of these -isms itself entails philosophical commitments.

1987, 1997, 2003; Michell, 2004; Molenaar, 2004; Molenaar, Huizenga, & Nesselroade, 2002; Valsiner, 1986, 2005). With increased appreciation for the conceptual confusion that is always and necessarily embedded in any attempt to regard the aggregate statistical findings issuing from correlational studies of *individual differences* as interpretable in any way at all for *individuals*, the need for a coherent conception of persons as entities quite distinct from *person variables* will become ever more apparent.<sup>8</sup> Moreover, the gradual transcendence of what I have called ‘statisticism’ will create possibilities for Stern’s personalistic perspective that have heretofore been closed off.

Following Stern’s death in March of 1938, Gordon Allport wrote that although critical personalism had failed to gain a secure foothold in the psychology of the day, “the personalistic way of thought will yet have its day, and its day will be long and bright.” (Allport, 1938, p. 773)

It cannot be said that Allport’s prediction has yet been borne out. However, as the grip of positivism and statisticism on mainstream thinking in 21st century psychology gradually loosens, the day Allport forecast will perhaps finally dawn, and critical personalism, as old as it actually is, will receive its long overdue consideration as a ‘new idea’ in psychology.

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<sup>8</sup> There is a certain ambiguity in the very expression ‘individual differences’ which has, perhaps, been partly responsible for the confusion that has long plagued the discipline of personality psychology. The expression can sensibly be taken to refer either to the differences within an individual that one might discern by observing that same individual over time and across various situations – *intra-individual differences*, or to the differences between individuals that one might discern by comparing observations made of them at some given time in some given situation – *inter-individual differences*. As Molenaar et al. have made clear in various publications (see works cited), these are two vastly different meanings when, as is nearly always the case, usual assumptions of ergodicity are violated. It is this distinction, and its profound epistemic consequences, that mainstream thinking has failed to take onto account.

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