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On Sustaining Critical Discourse with Mainstream Personality Investigators

Problems and Prospects

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ABSTRACT. In an *American Psychologist* article published in 1981, the author of the present contribution began a critique of the epistemic tenets of the traditional individual differences framework for personality research, which has long dominated the field. Though at first that article and others published soon thereafter generated some needed critical discourse within the discipline, mainstream investigative practices remain now just as they have long been, and, in the meantime, the critical discourse itself has largely ceased. In the present contribution, the author relates his attempts to understand these developments through historical research into the roots of mainstream thinking. Given the nature and depth of these roots, the continuing resistance to change within the mainstream is more readily understood. Nevertheless, it is argued, the need for such change remains, and it is observed that in the prevailing intellectual climate of the discipline, such change still does not appear imminent.

KEY WORDS: critical discourse, historical considerations, individual differences, individuals, personality psychology, statistical thinking

Beginning with an article published in the *American Psychologist* in March 1981 under the title 'Toward an Idiothetic Psychology of Personality' (Lamiell, 1981) and continuing to the present, I have been arguing along lines sharply critical of the traditional individual differences approach to empirical personality research for what is now roughly a quarter of a century. My contention, at its core, is that knowledge about the *differences between* individuals cannot properly be regarded as knowledge about *individuals* (Lamiell, 1997). If this is so and if it is knowledge about individuals that any scientific psychology of personality worthy of the name would seek, then the paradigm for personality research that has dominated the field for nearly all of the 20th century, and now

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into the 21st, must be abandoned in favor of some alternative(s) better suited to the discipline's actual knowledge objectives.

In a newly published book, Rom Harré (2006) expresses astonishment that, by now, this argument has not prompted a radical revision in the customary research practices of mainstream personality investigators. Yet no such revision has to date occurred, and there is no conspicuous evidence that one is imminent. If anything, it would appear that traditional thinking has actually become *more* entrenched during the past twenty-five years, as evidenced most clearly by the now widely accepted notion that the so-called 'Big Five' dimensions of personality differences can be regarded as scientific personality psychology's long-sought answer to chemistry's periodic table of elements (Lamiell, 2000; cf. Angleitner, 1991; Digman, 1989, 1990; Goldberg, 1993; McCrae & Costa, 1986, 1987, 1995).[1]

Quite in keeping with this, McAdams and Pals (2006) state in an article purporting to offer 'fundamental principles for an integrative science of personality' (the article's subtitle) that 'the new trait psychology heralded by the Big Five is arguably the most recognizable contribution personality psychology has to offer today to the discipline of psychology as a whole and to the behavioral and social sciences' (p. 204). Alas, this is doubtless true. What is even more daunting is the fact that nowhere in their lengthy article do McAdams and Pals offer even the slightest hint that a literature exists that explains the fundamental incoherence of the sort of thinking that has issued in this 'new trait psychology'.

It was therefore with considerable reluctance that I initially met the request of me by the co-editors of this Special Issue, Lisa Osbeck and Michael Tissaw, to contribute an article reflecting on my experiences in trying to engage contemporary mainstream personality investigators in a serious and sustained critical examination of their field. On the whole, those experiences to date have been rather less than satisfying, and the overall results anything but gratifying.

On further consideration, however, it seemed to me that I might have something helpful to contribute to this project after all, and that in any case I ought to try. For through the historical research that has been my principal scholarly preoccupation for most of the last sixteen years, I have acquired a much better understanding than I had in 1981 of the nature and depth of the paradigmatic commitments that have shaped, and continue to dominate, mainstream thinking about what a scientific psychology of personality should be. In this retrospective essay, I situate my long-running critique of mainstream thinking within the context provided by a sketch of this larger history. My objective is to acquaint readers with the roots of mainstream personality psychology's key epistemic tenets and, in the process, to reiterate the importance of sustained critical appraisal of those tenets to the discipline's long-term intellectual health.

Early Reactions and Unanticipated Developments

Two years after its publication, the aforementioned *American Psychologist* article was hailed by Rorer and Widiger (1983) in their contribution to the *Annual Review of Psychology* as 'the single most important paper' (p. 448) addressing theoretical issues in personality psychology that had been published within the time period under review. Combined with the numerous requests I was receiving for reprints of the article, this appraisal encouraged me to believe that the work might well have its intended effect of clarifying for mainstream personality investigators the epistemic limits of the concepts and methods fundamental to their traditional research practices.

Through the 1980s and into the early 1990s, opportunities abounded for me to elaborate on my ideas. This I did through regular presentations at conferences as well as through journal articles (e.g. Lamiell, 1982a, 1986a, 1991; Lamiell & Trierweiler, 1986), book chapters (e.g. Lamiell, 1982b, 1986b, 1986c, 1990a, 1990b, 1992), and a book (Lamiell, 1987). Not surprisingly, my central claims were met with skeptical reactions among mainstream researchers (e.g. Dar & Serlin, 1990; Ozer, 1990; Paunonen & Jackson, 1986a, 1986b; Wittig, 1982), but at the time, the important thing seemed to me to be that mainstream researchers were *engaging* those ideas and not simply ignoring them. Consistent with this trend, an interview spotlighting my critical perspective on the field was published in a mainstream personality theories textbook (Ross, 1987), and two years later, in a review in the journal *Personality and Individual Differences,* McReynolds (1989) described my 1987 book as one which 'merits the careful attention of all workers in the field' (p. 133). Then early in the 1990s, I was asked for (and granted) permission to reprint my 1981 *American Psychologist* article in an edited volume titled, encouragingly, *The Restoration of Dialogue: Readings in the Philosophy of Clinical Psychology* (Miller, 1992). In all of this, then, I could find ample evidence that at least some in the mainstream had been engaged by my critical writings.

However, as critical reactions to my arguments began to surface in the literature, I found reason to believe that my critics' understanding of their own methods had neither the depth nor degree of clarity that I had supposed. In particular, the above-cited articles by Wittig (1982), Paunonen and Jackson (1986a, 1986b), Dar and Serlin (1990), and Ozer (1990), all of which were written as attempts to refute some aspect(s) of my challenge to the validity of traditional thinking in personality research, revealed numerous misunderstandings of—and internally contradictory assertions about—the statistical concepts and measurement operations that are utterly foundational for studies of individual differences.[2] What is more, and what I certainly had not anticipated, my subsequent clarifications of those misunderstandings and internal contradictions (Lamiell, 1982a, 1990b; Lamiell & Trierweiler, 1986) were in turn neither rejoined nor conceded. Instead, the conversation simply stopped, and traditional research practices have since continued as if no challenge to their validity and conceptual coherence had ever been mounted at all.

In the face of this development I concluded that, for a time at least, further debate with advocates of mainstream thinking would be fruitless and I decided to direct my efforts instead to learning more about how such thinking had become so entrenched within the discipline to begin with. Of course, in pursuing this question I inevitably made myself party to the cessation of dialog with mainstream personality investigators. But given where that dialog had led up to then, this seemed to me not an altogether untoward development.

Stern Lessons

Actually, my scholarly pursuits had first been nudged in an historical direction in 1984, though I did not realize it at the time. In May of that year, I attended the Second European Conference on Personality at the University of Bielefeld in Germany, where I presented a paper elaborating on the ideas that I had introduced in the 1981 *American Psychololgist* article (Lamiell, 1986b). In the wake of that presentation, several European colleagues urged me to look into the works of the German philosopher and psychologist William Stern (1871–1938), assuring me that, given my own intellectual persuasions, I would find much of interest and value in his writings.[3] In this those colleagues were most assuredly correct. It would take me some time to discover this, because in 1984 I had no knowledge of the German language and most of Stern's writings had not been published in English translation. However, by the time I began a sabbatical semester at the University of Heidelberg in January of 1990, I had progressed sufficiently in my study of German to begin reading Stern's works.

In his 1911 book, titled *Methodological Foundations of Differential Psychology*, Stern set out the discipline's scientific agenda in terms of four basic investigative schemes, each of which could be articulated in terms of the two basic concepts of *attributes* and *individuals* (see Lamiell, 2000, p. 5; Stern, 1911, p. 18). In what Stern called *variation research*, investigation would be focused on the distribution of measures of a *single attribute* across *many individuals* within some population. *Correlational research* would extend variation research to the determination of degrees of covariations among measures of *two or more attributes* across *many individuals* within some population. In the research scheme that Stern (1911) called *psychography* (*die Psychographie*), the investigative focus would be on a *single individual* characterized in terms of *many attributes*, while in *comparison research*, one would investigate similarities and divergences in the attribute profiles of *two or more individuals*, each of whom has been characterized in terms of *many attributes*.

In laying out these four research schemes, Stern (1911) drew attention to the fact that variation and co-variation studies actually generate knowledge about *attributes*, with the individuals serving in such studies merely as 'place-holders' along the scales of measurement used to define quantitatively the variable(s) of interest. It is not until an investigator turns to psychography, Stern argued, that she/he directly engages the 'problem of individuality' (see below). In this research scheme, the investigator would inevitably be faced with questions of an essentially developmental nature, and in pursuing answers to those questions, variation and co-variation studies would serve no essential function.

As I acquainted myself with Stern's 1911 *Methodological Foundations* text, I could see immediately at least two striking parallels between his views and ideas I had advanced in the 1981 *American Psychologist* article.[4] First, my claim in the article that the reliability and validity coefficients issuing from personality trait studies reveal the psychometric properties of *constructs* (attribute-measurement method units; cf. Campbell & Fiske, 1959), and not anything about the behavior patterns of *persons*, was entirely consistent with Stern's claim that variation and co-variation studies generate knowledge of attributes and not of individuals. Second, my argument that in abandoning individual differences research in favor of studies of individuals personality psychologists would inevitably be re-directing their attention to questions of a developmental nature articulates fully with Stern's understanding of the direction in which psychographic studies of persons would inevitably lead.[5]

Unquestionably, the discovery of these parallels between my own still relatively nascent ideas and those of no less a luminary than the founder of differential psychology himself bolstered my convictions about the essential validity of the position I had staked out before reading Stern's 1911 work. Sobering in all of this, however, was the realization that mainstream thinking about empirical personality research had not remained faithful to Stern's views on these fundamental matters. The obvious question was: why? Of course, my pursuit of an answer to this question led me still deeper into the discipline's history, and hence still further from discourse with contemporary mainstream investigators.

An Alternative Perspective on the 'Problem of Individuality'

In his 1900 book, through which, it might reasonably be said, Stern founded differential psychology, he used the very first sentence of the foreword to declare *individuality* the 'problem of the 20th century' (Stern, 1900, p. v). By this Stern meant that the biggest challenge facing the New Science as it moved into the new century would be to somehow coordinate knowledge about *people in general* with knowledge of *persons in particular*.[6] The experimental psychology of the day was explicitly and exclusively devoted to the quest for knowledge of the former sort, and Stern believed that a differential psychology would serve to highlight the need for knowledge of the latter sort. It bears emphasis here that Stern *never* believed that a differential psychology could *meet* that need. On the contrary, he was persuaded from the very outset of his scholarly life that an understanding of human individuality would require a comprehensive system of thought, a *Weltanschauung* (or world-view), that would extend beyond the limits of empirical psychology—however fashioned—and, inevitably, into the domain of metaphysical considerations (cf. Bühring, 1996; Lamiell, 2003; Stern, 1917, 1927). Stern stated this conviction explicitly in the 1911 *Methodological Foundations* text, but by then another perspective on the 'problem of individuality', quite incompatible with his views but fully in step with ascendant positivistic-empiricistic sensibilities (see, e.g., Danziger, 1979), was also being advanced for consideration.

Uncannily, it was also in 1911 when a monograph by E.L. Thorndike (1874–1949) titled *Individuality* appeared (Thorndike, 1911). In that work, Thorndike set forth a view according to which it would be variation and co-variation studies that would center the scientific study of personality. First, investigations of an essentially factor-analytic nature would serve to isolate the basic dimensions of personality differences on which the construction of multi-attribute personality profiles would necessarily depend. In turn, further empirical research, also of a correlational nature, would answer questions about (a) the sources of individuals' personality characteristics in nature and nurture, and (b) the lawful manifestations of those characteristics in various domains of human behavior. In the former case, attribute (trait) measures would serve, in effect, as dependent variables, while in the latter case they would serve as the independent variables. In Thorndike's scheme, psychographic investigations, if undertaken at all, would be strictly ancillary endeavors, structurally dependent upon findings gained through co-variation studies, and never truly generative of scientific knowledge about human individualities.

They are, of course, Thorndike's fingerprints that appear all over the literature of mainstream 20th-century personality psychology, and in this light one can only wonder why McAdams and Pals (2006) characterized the contemporary Big Five framework as 'the new trait psychology' (p. 204). In fact, it is a veritable monument to Thorndike's (1911) vision. It is also the virtual antithesis of Stern's views, qualifying from his perspective as nothing more than the currently popular rendition of a framework for 'personality studies' that he repeatedly scorned (e.g. Stern, 1921, 1925, 1929, 1930, 1933).[7] Couched in the terminology used by Stern (1911), we should have to say that in following Thorndike (1911), mainstream personality psychology made of itself a discipline devoted to the generation of knowledge of *attributes* rather than knowledge of *individuals*. This distinction, however, is one that is effectively occluded from the perspective advanced by Thorndike (1911).

Thorndike (1911) wrote of the correlation between measures of two traits in a group of individuals as indexing 'the extent to which the amount of one trait possessed by an individual is bound up with the amount he possesses of some other trait' (p. 21). Given Thorndike's estimable talents in matters quantitative, it is startling to read him making this claim without qualifying it in a critically important way. Specifically: it is true that when the correlation between measures of two traits in a group of individuals is *perfect*, it is the case for each and every individual within the group for which that correlation has been computed that his/her standard score on one of the variables is identical in magnitude to his/her standard score on the other variable. In this sense, one could say under this empirical circumstance that each individual's two standard scores are perfectly 'bound up' with one another. But a perfect correlation marks the *only* empirical circumstance under which Thorndike's claim holds, and this is an empirical circumstance that is never in fact realized. Under all conditions that actually do obtain empirically, correlations of the sort Thorndike was discussing reveal nothing at all about the extent to which the measure of an individual on one variable is 'bound up with' his/her measure on a second variable. The only way to ascertain this given a less-than-perfect correlation is to examine the data *for that individual*.[8]

Since, in practice, correlations of the sort Thorndike (1911) was discussing are never perfect, they are always, and quite literally, knowledge about *no one*. Left unqualified in the way just described, however, Thorndike's claim invites the understanding that *whatever* their magnitude, such correlations are *not only* knowledge about the extent to which two variables are 'bound up with one another' in some sample/population, *but also* and perforce, knowledge about the extent to which measures of an individual with respect to those two variables are 'bound up with,'—i.e. correspond in magnitude to—one another. It is in exactly this sense that, from Thorndike's perspective, the distinction drawn by Stern (1911) between knowledge of attributes and knowledge of individuals is occluded, and once this has happened, correlational research does indeed seem to render superfluous any additional investigative scheme for studying individuals scientifically. It was due to the widespread adoption of this view among mainstream personality investigators that, already rather early in the 20th century, Stern's understanding of the 'problem of individuality' gave way to the view advanced by Thorndike (1911).

Unarguably, however, the fundamental premise of Thorndike's view, namely that the inter-attribute correlations gleaned from studies of individual differences are interpretable for individuals, is utterly *false* under all empirical circumstances that actually obtain. The question therefore begs: why did that premise nevertheless become the linchpin of 20th-century empirical personality psychology's epistemic canon? More troubling still is the question: why has the traditional view persisted even after its fallaciousness has been pointed out? Pursuit of these questions drew me still deeper into the history of the discipline, and with that still further away from direct engagement with its contemporary practitioners.

The Historical Roots of Accepted Research Practices in Contemporary Personality Psychology

Statistical concepts and methods as they are employed in contemporary psychological research had no comparable role in the general experimental psychology that Wilhelm Wundt (1832–1920) famously—if somewhat mythically (cf. Koch, 1985)—launched in Leipzig in 1879. To be sure, such concepts and methods were sometimes brought to bear in the course of data analysis, but their function was to estimate errors of measurement in experiments carried out on single subjects (i.e. in experiments where $N = 1$), and not to estimate parameters of variables (means, variances, co-variances, etc.) in studies of aggregates of subjects sampled from populations (i.e. in studies where N = many; cf. Boneau, 1998; Danziger, 1987, 1990).

The early experimentalists were in search of the *general laws* presumed to govern mental life, and any apparent contradiction between this fact and the practice of conducting experiments on single subjects is resolved by an understanding of the meaning of 'general' to the experimentalists of the day. The German word for 'general' (in the sense relevant here) is *allgemein*, itself a contracted form of the expression *allen gemein*, meaning *common to all*. The early experimentalists were operating—correctly—on the view that there would be no way to adduce evidence of general laws in the sense of 'common to all' except through the investigation of individual cases.

As it happened, however, the Wundtian approach to psychological experimentation did not last long, and this was due in no small part to the fact that it seemed singularly ill-suited to the production of knowledge that could be applied practically in the world outside the laboratory. Wundt himself was not especially troubled by this. On the contrary, he believed that psychology's orderly development as an experimental science would be better served if the research questions taken up for investigation were determined by theoretically and philosophically grounded considerations arising from within the field, and not dictated by practical exigencies arising from without, in such institutions as schools, hospitals, business and industry, the military, and so on (Wundt, 1910, 1913). In ever-growing numbers, however, Wundt's contemporaries favored the separation of psychology from philosophy, and the rapid development of the former discipline as an applied science (Danziger, 1990).[9]

As a means to that end, the correlational research methods that had been pioneered by Francis Galton (1822–1911) and further refined by his student and protégé, Karl Pearson (1857–1936), seemed ideal, and it was through the increasingly widespread adoption of these methods that a 'Galtonian' model for psychological research came to supplant the original 'Wundtian' model. As Danziger (1987) has explained, however, this development was itself not entirely unproblematic.

For one thing, it was not clear how research of an essentially *correlational* nature, which is what Galtonian inquiry was (and is), could serve the objective of discovering *causal* relationships in studies of behavior. It also was not obvious how research issuing in aggregate statistical indices revealing 'general' truths in the sense of 'on average' could also be regarded as suitable for discovering 'general' truths in the original and decidedly non-statistical sense of 'common to all'.

The solution to the first of these two problems was achieved through the invention of the *treatment group* method of experimentation (Danziger, 1987). This invention was a variation on the use of statistical methods to compare naturally defined groups (e.g. persons differing in age, sex, race, etc.), a feature of Galtonian-style inquiry from its beginning. Psychologists came to see that there was nothing that would logically preclude the use of the same methods to statistically compare groups that had been created by an investigator. Participants could, after all, be sampled representatively from populations and then assigned at random to different experimental conditions. Dependent variable means for the different treatments could then be compared statistically, and the results of such analyses could be taken to warrant statements about the causal effects of those treatments. Because this approach to psychological experimentation is in fact simply a variation on the original Galtonian framework (and not in any sense a refinement of Wundtian experimental methods), Danziger (1987) christened the emergent model 'neo-Galtonian', and it was this model for experimental research that mainstream researchers came to see as viable in the quest for knowledge about the causes of behavior.

Left to be resolved was the second of the two problems noted above, that is, the problem of reconciling knowledge of empirical regularities holding true 'generally' in the sense of 'on average' with the theoretical objective of discovering laws that could be said (within the limits of induction constraining all scientific inquiry) to hold true 'generally' in the sense of 'common to all'. Wittingly or otherwise, the solution to this problem materialized in the widespread adoption by psychological researchers of a view of the essential nature of statistical knowledge that had been championed around the middle of the 19th century by the British historian Henry Thomas Buckle (1821–62). In his highly influential work *History of Civilization in England*, Buckle (1857/1898) advanced a view of statistical knowledge that, for its part, deviated significantly from the view that had previously been articulated by the Belgian statistician Adolphe Quetelet (1796–1874).

Well before Wundt opened his experimental laboratory in Leipzig in 1879, Quetelet had argued that a statistically-based science of human behavior, a *social physics*, would be a discipline yielding knowledge of *l'homme moyen*, that abstract entity known as 'the average man', and *not* one suited to accounting scientifically for the behavior of real individuals. To Quetelet, empirical deviations of real individuals from *l'homme moyen* represented nothing more than random *errors* attributable to Nature, and therefore could properly be disregarded in the formulation of the laws of social physics. As he expressed himself in a seminal 1835 work:

> If one seeks to establish... the basis of a social physics, it is he (*l'homme moyen*) whom one should consider, without disturbing oneself with particular cases or anomalies, and without studying whether some given individual can undergo a greater or lesser development in one of his faculties. (cited in Porter, 1986, pp. 52–53)

In short, the lawfulness revealed by social physics would be a strictly aggregate-level lawfulness, and, clearly, there was no basis there for any sort of a science of personality.

Contra Quetelet's view, however, Buckle was thoroughly persuaded that aggregate-level lawfulness serves as a kind of 'window' onto individual-level lawfulness—however narrow the aperture might be due to the limits of empirically established knowledge at a given stage in the development of a particular line of inquiry. From this it would follow, Buckle reasoned, that a relentless pursuit of more such knowledge would eventually reveal those general laws *fully* explanatory of the actions of *real* individuals in a fashion *common to all* of them. Far from representing nature's random errors, as Quetelet had contended, the deviations of real individuals from *l'homme moyen* manifested at any given point in time would properly be regarded, from Buckle's perspective, as evidence that not all of the factors determinative of the behavior under investigation had yet been incorporated into the statistical analysis.

Buckle's view was far from universally shared by his 19th-century contemporaries. As just explained, his ideas departed significantly from Quetelet's. Similarly, the Leipzig mathematician M.W. Drobisch (1802–96) contended that 'it is only through a great failure of understanding [that] the mathematical fiction of an average man... [can] be elaborated as if all individuals... possess a real part of whatever obtains for this average person' (cited in Porter, 1986, p. 171), and in his book-length treatise titled *The Logic of Chance* the British logician John Venn (1834–1923) explained why probabilistic statements based on aggregate statistical considerations cannot properly be understood as claims to knowledge about individual cases (Venn, 1888).

Despite such critical writings, it was Buckle's perspective that was widely adopted by early 20th-century research psychologists as the epistemic cornerstone of the neo-Galtonian research paradigm, and in this way that perspective became integral to the methodological canon of scientific personality research as well.

Concluding Reflection

When I began writing critically about individual differences research as a framework for a scientific psychology of personality, I could scarcely have imagined how firmly the epistemic tenets of that framework are rooted in a tradition of thought about the essential nature of aggregate statistical knowledge that extends back to the middle of the 19th century (Porter, 1986). In this light, the fact that Thorndike's (1911) perspective on the problem of individuality in scientific psychology was so well received within the mainstream is quite understandable.

Whether he knew it or not, Thorndike's views were, unlike Stern's, wholly consonant with Buckle's understanding of aggregate statistical knowledge as a kind of 'window' onto individual level functioning, and, as recounted above, that is an understanding that was winning resonance within the scientific psychology of the day anyway. For unless aggregate statistical knowledge was viewed as informative about individual-level functioning, the neo-Galtonian paradigm for psychological research could not even have *seemed* adequate as a substitute for Wundtian-style inquiry in the scientific quest for knowledge of psychological laws of general validity in the sense of 'common to all'. In that event, some variant of the original practice of investigating individual subjects individually would have had to be retained in the interest of fidelity to psychology's objectives as a basic science, whatever—and however worthy—its additional objectives as an applied science might also have been, and however serviceable in this latter regard research might have been seen to be that was driven by the statistical methods adequate to the discovery of empirical regularities having general validity in the sense of 'true on average'.

In the light of these historical considerations, I find it easier to understand (though not, of course, to finally accept) the prevailing disinclination of mainstream personality investigators to engage in sustained critical discourse about their own long-standing methods and interpretive practices. After all, at issue here is the validity of epistemic convictions that are deeply engrained within—and profoundly determinative of—a methodological canon that has dominated empirical research within the discipline for most of an entire century. It was perhaps therefore inevitable that a critical challenge to the soundness of those convictions—particularly one seeming to offer little guidance in the direction of alternatives[10]—would eventually encounter an intellectual stone wall.

And yet as noted above, the same historical analysis that uncovers the deep historical roots of mainstream epistemic convictions also reveals that the soundness of those convictions was contested from the very beginning.[11] By incorporating this material into my most recent critical analysis of the field (Lamiell, 2003), I have sought to document the long-standing precedent for my argument that those convictions cannot withstand close critical scrutiny.

In his review of my 2003 book, the cultural/developmental psychologist Jaan Valsiner (2005) expressed appreciation for its historical component (of which this article provides but a sketch), but at the end found himself still wondering whether adherents of mainstream thinking would rise to the challenge the book presents. For the short term, at least, the prospects for that happening do not seem bright. Consider, for example, the response by one contemporary researcher who was asked to review a detailed prospectus for that book. In 1999, in a note to the book's publisher, the prospective reviewer (whose identity remains unknown to me) declined as follows:

> I am a traditional Individual Difference [*sic*!] psychologist. The philosophical and historical approach taken by Lamiell was of no interest to me and, I suspect, is of little interest to most people in the field. ... I suggest that you have someone more attuned to his philosophical predilections review this manuscript .... Sorry that I could not be of greater assistance to you. I tried to read the manuscript and I found it of no interest and of little relevance to the kinds of issues I address in my work.

The ahistorical and anti-philosophical sentiments expressed here vividly reflect the 'cult of empiricism' about which Toulmin and Leary (1985) wrote so cogently. Alas, fully two decades after those authors' insightful contribution, evidence of this cult continues to abound within the mainstream literature of personality psychology. Perhaps reference to one more especially revealing—and troubling—example of this will suffice as a platform for my concluding point.

About a decade ago, two currently prominent personality investigators published an article titled 'Trait Explanations in Personality Psychology', in which they reiterated their beliefs concerning the conceptual integrity of traditional individual differences research as a framework within with to pursue the discipline's scientific objectives (McCrae & Costa, 1995). Nowhere in their article did McCrae and Costa address themselves directly to the issues raised in the critique of the traditional paradigm that, already by then, I had been articulating in the published literature for well over a decade. Indeed, they did not even acknowledge, through a simple listing of some relevant references, the fact that that critique even exists. Instead, those authors blithely presumed throughout their discussion, in full accordance with long-standing conventions, the conceptual integrity of traditional trait psychology for securing scientific explanations of individual behavior. Along the way, they took care to remind their readers, in a transparently positivistic fashion, that 'explanations need not specify causal mechanisms,' (p. 246), and that 'as scientists we understand ... that a real understanding of causes is evident in some level of prediction and control' (pp. 248–249). Disguising but thinly their disdain for philosophical discourse on these and related matters, McCrae and Costa proclaimed: 'We scientists are the experts in this particular game, and it is up to the philosophers to explain how we manage to make reasonably correct inferences' (p. 249).

Tellingly, the stance adopted by McCrae and Costa in this passage effectively forecloses the possibility of conceptual/philosophical analysis leading to the conclusion that the inferences about individuals commonly made by mainstream personality investigators based on studies of individual differences in fact are *not* 'reasonably correct' after all. Under the constraints imposed by this foreclosure, genuinely *critical* discourse within the discipline cannot even be initiated, let alone sustained. It is for this reason, I believe, that the very estimable efforts of other contemporary scholars who have mounted theoretical and philosophical critiques of mainstream thinking within the field (e.g. Harré & Tissaw, 2005; Martin, Sugarman, & Thompson, 2003; Rychlak, 1988; Smythe, 1998; Trierweiler & Stricker, 1998) have to date likewise failed to result in any sustained, critical re-appraisal of traditional views and their correlative investigative practices.

Intellectually speaking, this is perhaps as worrisome a state of affairs as could exist within a scientific discipline. Yet this seems to be the state of affairs now prevailing within the mainstream of personality psychology. If this is not the case, then evidence of that fact has escaped me, and I will greatly welcome correction. On the other hand, if such is the case and if there is to be any change in this state of affairs, then that change will have to be effected by younger scholars whose critical sensibilities have not already been blunted by dogma and/or compromised by careerist concerns, and who therefore remain genuinely open to the argument—one with its own deep historical roots—that the most basic epistemic tenets of individual differences research as a framework for the scientific study of personality are fundamentally and irremediably flawed.

Notes

1. These putative elements are (for now) said to be (high vs low) *neuroticism*, *extraversion*, *openness*, *agreeableness* and *conscientiousness*.
2. To cite just one example here: Dar and Serlin (1990) criticized me for discussing correlations of the sort generated through studies of individual differences in terms of means and standard deviations, instead of in terms of the ‘usual’ parameters of correlational analyses, regression slopes and additive constants (Lamiell, 1990a). I had thus to remind them in my rejoinder (Lamiell, 1990b) that regression slopes and additive constants are *themselves* defined in terms of means and standard deviations!
3. I am especially grateful to Lothar Laux and the late Jean-Pierre deWaele in this connection.
4. Of course, it was precisely because the European colleagues whom I met in Bielefeld in 1984 already saw these parallels that they advised me in to look into Stern’s writings.
5. Stern was quite explicit in his view that, however extensive and sophisticated psychographic methods of investigation might become, they would never obviate the need for biography. This point further underscores Stern’s conviction that a psychology of the person would inevitably be *developmental* in its knowledge objectives.
6. Stern highlighted this challenge with an aphorism borrowed from Goethe: ‘Das Besondere unterliegt ewig dem Allgemeinen; das Allgemeine hat ewig sich mit dem Besonderen zu fügen’ (‘The particular is ever subordinate to the general [even as] the general must ever accommodate the particular’).
7. Hence, all posturing by contemporary researchers as faithful latter-day adherents of Stern’s ideas (e.g. Eysenck, 1990) amounts to little more than a perpetuation within the field of an ‘origin myth’ (cf. Lamiell, 2006; Samelson, 1974).
8. For a vivid empirical illustration of this point, see Lamiell (1987, p. 101).

9. It should be noted that while Stern was very much in favor of the development of an applied psychology—indeed, together with Otto Lipmann (1880–1933) he founded the Berlin Institute for Applied Psychology in 1906 and, two years later, the German-language *Journal of Applied Psychology*—he did *not* endorse displacement of the general experimental psychology, and, with Wundt, he adamantly opposed the divorce of psychology from philosophy (cf. Stern, 1917).
10. This is not to say that nothing at all has been accomplished along these lines, but much remains to be worked out. Interested readers might wish to refer to Ch. 9 of Lamiell (2003).
11. In addition, numerous critical voices other than my own have been raised since then (see, e.g., Allport, 1937, 1961; Brunswik, 1943; Carlson, 1971; Lewin, 1935; Valsiner, 1986). But with the arguable exception of Allport, these contributions were more in the nature of isolated observations than of sustained programmatic critiques.

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