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Human Studies

A Journal for Philosophy and the
Social Sciences

ISSN 0163-8548

Volume 34

Number 3

Hum Stud (2011) 34:237-255

DOI 10.1007/s10746-011-9179-8



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Empirical Race Psychology and the Hermeneutics of Epistemological Violence

Thomas Teo

Published online: 15 June 2011
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Abstract After identifying the discipline of psychology's history of contributing pioneers and leaders to the field of race research, epistemological problems in empirical psychology are identified including an adherence to a naïve empiricist philosophy of science. The reconstruction focuses on the underdetermined relationship between data and interpretation. It is argued that empirical psychology works under a hermeneutic deficit and that this deficit leads to the advancement of interpretations regarding racialized groups that are detrimental to those groups. Because these interpretations are understood as actions that bring harm to certain racialized groups, and because these actions are made in the name of science and knowledge, the term epistemological violence is applied. Reflections regarding the meanings and consequences of this term in empirical psychology and the human sciences are presented.

Keywords Epistemological violence · Scientific racism · Psychology · Hermeneutics

Introduction

In the middle of 2010 the press reported that a school board member in Marysville (Washington State) had suggested to his colleagues that different racial groups display different academic achievements due to different brain sizes. According to *The Seattle Times*, the person wrote in an e-mail message that “east Asians and their descendants averaged larger brain size, higher intelligence and social organization than Anglo-Saxons and their descendants, and that Anglo-Saxons, in turn, averaged

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higher scores in those dimensions than did Africans and their descendants” (Flandro 2010). He contended that due to this biological difference certain racial groups were incapable of achieving academic success and that it would explain the district’s achievement gap (Shaw 2010). As publicized, he did not develop such justifications himself—his arguments were based on the research of the Canadian psychologist Rushton (1995).

How is it possible that individuals who are involved in education or psychology neglect the huge amount of critical, historical, philosophical, sociological, and anthropological information regarding the concept of race, intelligence, achievement, class, and so on, which challenges such arguments as described above? Certainly, the notion of differences in ability between racialized groups fulfills long-held stereotypes, but another source of such attitudes may be found in scientific, empirical studies from the discipline of psychology. Rushton is just one recent example of the many psychologists who have advanced the notion of race differences in mental life. Indeed, the discipline of psychology has a long history of contributing pioneers and leaders to the field of race research and scientific racism (see Gould 1996; Jackson and Weidman 2004, Richards 1997; Tucker 1994; Winston 2004).

Race Psychology

In tracing that history, it can be seen that founders of the modern discipline of psychology were engaged in race psychology. Francis Galton (1822–1911), considered one of the pioneers of the discipline, argued that Europeans were by nature more intelligent than “primitive races” and recommended the quantification of levels of racial intelligence. One of the founders of social psychology, Gustave Le Bon (1841–1931), understood races as physiologically and psychologically distinct entities that possessed separate race souls, although Le Bon did not base his arguments on empirical methods. Paul Broca (1824–1880), one of the founders of “neuropsychology”, was convinced that non-European races were inferior to European races and used a variety of scientific studies to prove his preconceived conviction. In the United States, pioneers of psychology such as Granville Stanley Hall (1844–1924), the first president of the American Psychological Association (APA), argued that “lower races” were in a state of adolescence—a claim that provided justification for segregation policies in the United States.

In particular, in the first half of the twentieth century empirical race psychology was prominent and influential. Leading American psychologists, including the APA presidents Robert M. Yerkes (1876–1956), who played a decisive role in army testing, and Lewis Terman (1877–1956), who supported segregated education, spearheaded race psychology. Popular in race psychology was also the study of the *mulatto hypothesis*, which suggested that a greater proportion of white “blood” in a black person’s ancestry would lead to higher intelligence (see Teo 2004). Although an important shift occurred from studying race to researching prejudice in the discipline, especially after 1945 (Samelson 1978), empirical race studies have never completely vanished, and some of the most prominent current advocates of race psychology and scientific racism are psychologists.

In 1997 the eminent psychologist Raymond B. Cattell (1905–1998), who is known to most psychology students for his studies on intelligence and personality, was chosen by APA for one of its highest awards, the *Gold Medal Award for Lifetime Achievement in the Science of Psychology*. Critics of the nomination pointed out that Cattell (1937) had early in his life praised the racial-hygienic laws of the Third Reich, and that he had later hoped that races would be separated into groups who would form non-interbreeding species (Cattell 1987) (for a critique, see Tucker 1994). An APA committee was set up to investigate the case but Cattell withdrew his name from consideration and died a few months later.

Probably more academic attention was given to Arthur Jensen (1969) for his highly cited article in which he argued that because of physical differences between races there should also be “genetically conditioned behavioral characteristics” (80) that would include mental abilities. Because intelligence has a genetic component, he argued, “it seems not unreasonable ... to hypothesize that genetic factors may play a role” (82) in producing racial differences in IQ. However, both propositions are speculative. In the first case, having genetically conditioned behavioral characteristics has nothing to do with differences between “races” and, in the second case, the “hypothesis” is not a scientific hypothesis that had been tested but rather is an interpretation with consequences.

Rushton and Jensen (2005) summarized *Thirty Years of Research on Race: Differences in Cognitive Ability* by concluding that the “Black–White IQ difference is partly heritable” (278). They argued that the “denial of any genetic component in human variation, including between groups, is not only poor science, it is likely to be injurious both to unique individuals and to the complex structure of societies” (285). Of course, human variation has a genetic component, yet no data exist to determine that differences in mental life between groups are genetic. Similarly, the Harvard psychologists Herrnstein and Murray (1994) drew in the *Bell Curve* the conclusion that Blacks were disadvantaged by both environment and genetics.

Rushton, whose research was and remains well funded (e.g., by the *Pioneer Fund* where he became president), has continued to publish in mainstream psychology journals, including APA journals. He points out that between 1986 and 1990 he was the 11th most cited psychologist (see Rushton 1999). Rushton (1985, 1995, 1999) starts with the assumption of the existence of three major races (*Oriental*s, *Whites*, *Blacks*) that can be categorized according to a variety of physical and psychological characteristics; for example, he describes Blacks as being by nature more aggressive, less intelligent, and less law-abiding than Whites and Asians. According to his view, Whites take the good Aristotelian middle ground between Blacks and Asians (see Aalbers 2002). The data, which themselves have been challenged for their selectiveness are interpreted by Rushton within the *r-K* life history theory that was originally developed within evolutionary biology for animals and plants (see Wilson 1975) (for a critique of his work, see e.g., Code 1993; Fairchild 1991; Lieberman 2001; Peregrine et al. 2003; Peters 1995; Weizmann et al. 1990; Winston 1996).

Rushton (1985) suggested that a *K* reproductive strategy creates few offspring whereas the *r*-strategy produces a large number of offspring. These strategies correlate with traits such as parental care, infant mortality, life span, intelligence,

social organization, altruism, law-abidingness, sex drive, and other characteristics. Rushton speculated that Asians follow the *K* strategy and Blacks follow the *r* strategy, and argued that *r-K* life history theory “explains the three-way pattern of differences in brain size, IQ, and behavior” (Rushton 1999: 82). It should be mentioned that there is no evidence that this interpretation can be applied scientifically to humans (see also Weizmann et al. 1990).

Critics of Rushton’s research have been accused by him and his supporters of political correctness (Rushton 1999: 96). For instance, one supporter, Gottfredson (2005), suggested that “lying about race differences in achievement is harmful” (318), implying that critics who challenge the meaning of race differences are dishonest. However, the examination of psychology textbooks shows that race differences are reported on a regular basis; the question remains how to interpret these differences. It should also be mentioned that Rushton has had substantial success and influence in the discipline; he himself reported that 52% of scientists believed that the Black–White IQ difference was partly genetic (Rushton 1999: 102). He was referring to a study by Snyderman and Rothman (1987), published in one of the leading journals of the discipline (*American Psychologist*), in which it was reported that the majority of psychologists and educational experts in intelligence testing “feel” (137) that the Black–White difference in IQ is partially heritable. Although this *American Psychologist* study is more than 20 years old, it appears that Rushton’s work has received a certain degree of credibility in psychology based on the fact that he uses empirical methods and is quick to employ the latest technologies in his research.

Thus, race psychology’s past successes and to a certain degree the current shaping of discourse can be attributed to race psychologists’ accepted usage of empirical mainstream methods. Race psychologists have applied and continue to apply the standard methods of the discipline and have continued to invoke these methods in empirical comparison of various racialized groups. Based on naïve empiricist notions, differences have been and continue to be considered “real” natural differences. Accordingly, theoretical, historical, philosophical, and conceptual challenges to this type of research are quickly dismissed by the argument that critics do not use empirical methods and statistical testing.

Streams of Critique of Race Psychology

The basic epistemological problem in empirical psychology is an adherence to a “positivist” philosophy of science, or to be more precise, and in order not to caricature the intentions of positivist philosophers, the acceptance of naïve empiricism, which assumes the power of so-called facts without asking how facts are constituted and interpreted. Such an attitude is combined with a lack of self-reflection (Habermas 1967/1988), whereas the notion that psychologists should look at the social function of psychology, the social formation of facts, and the historical nature of the psychological subject matter (Horkheimer 1937/1992) remains incomprehensible within a discipline that understands itself as a natural science, or at least, tries to emulate the natural sciences.

Under Mach's authority (see Winston 2001), mediated through various behaviorist but also physiological research traditions, and standard in current mainstream psychological methodology, psychologists have given up on the idea of asking for the *why* of phenomena in favor of determining a relationship between phenomena. This means, for instance, that social distress itself is not a topic of interest, but what is of interest is the relationship between social distress (operationalized and measured) and depression (operationalized and measured). Similarly, the racialization of groups, which requires historical and interdisciplinary knowledge, is not itself a topic of interest, but the relationship between race and an outcome variable (such as school performance) is of interest. With Mach (1905/1976) it appeared that once it was established that y is a function of x , there was no need for further interpretation. As the theoretical psychologist Marx (1951) expressed it parsimoniously: the goal of psychology was and is finding the "functional relationships between variables" (6). Accordingly, there is no need for reflection and interpretation—at least so it seems. Psychology is, as Holzkamp (1983) phrased it so well, a *psychology of variables*.

There is a second set of problems associated with a naïve empiricist tradition. Although Mach thought that the functional relationship between variables would lead to the end of the concept of causality, a concept that was metaphysical for Mach, psychologists reinterpreted the functional relationship as causality, once it was established in an experiment, not realizing that the concept of causality itself needs interpretation, as the many discourses in the history of Western thought show, from Aristotle (2001) who discussed four "causes", to Francis Bacon (1965) who allowed only for the "efficient cause", to Hume (1748/1988) who argued that causation is only a habit of the mind and that inferences from experience are effects of custom, to Kant (1781/1965) who argued that causation was an a priori category imposed onto our experiences. This reinterpretation led to the notion in psychology that a bad hair day *causes* low self-esteem (see Vilar 2000), and that *paths* in statistical analyses of correlations can be interpreted as causal. Interpreting differences between racialized groups as causal in terms of heredity is just one consequence.

A third set of problems derives from the procedure of empirical research in psychology that allows for an infinite generation of studies by varying independent and dependent variables with or without including intervening variables. For example, Hunter and Sommermier (1922) studied level of intelligence based on degree of American Indian blood, and developed the following categories: pure Indian blood, three-quarters Indian blood, one-half Indian blood, and one-quarter Indian blood. A psychology of variables allows for a generative research and a publication industry by varying the "race" of the participants and determining its impact on psychological outcomes such as cognition, emotion, motivation, behavior, and so on. Cognition can be divided into intelligence, memory, attention, and so on; memory can be divided into short-term, long-term, or episodic memory, and so on. For each concept a variety of measures exist. Age, gender, education, socioeconomic status (SES), and other psychological measures could be included as intervening or as independent variables. Each study will produce results, and it is not improbable that, because of the different life situations and experiences of

racialized groups, group differences will be found. Race psychologists within a hereditarian framework then interpret results of difference as occurring “by nature”.

The “common sense” in empirical psychology suggests that if empirical results show that differences exist, then it is not unreasonable to assume that these differences are partially inherited. No direct data exist to determine that point and the argument is based on the conflation of within- and between-group differences (see also Gould 1996). When this type of common sense appears in psychology critical attention is required. Race psychology and racist interpretations have been criticized from various points of view. Within a critique of the *context of discovery* (e.g., Reichenbach 1938) the task of the critic is to examine why psychologists are interested in studying what they study and may include reconstructions regarding the underlying cultural, political, economic, and personal interests as well as identifying or challenging the social origins of hypotheses, concepts, and theories (see also Danziger 1997).

However, critical studies on the history and culture that led to race and racism, including studies of scientific racism, as well as studies pointing to the funding of race research and the goal of such funding agencies (see Tucker 2002), are often not accepted by the mainstream in psychology because empirical results still show differences, regardless of who did the funding. It is also not uncommon to accuse critics of having political interests, whereas hereditarian race researchers portray themselves as neutral and objective. Because many psychologists are no longer trained in the human and social sciences (i.e., many psychologists identify themselves with the natural sciences), they are often not convinced by reflections on the philosophy, history, and politics of race psychology, or they consider them irrelevant to the empirical results.

Within a critique of the *context of justification*, selective sampling or selective data reporting, as well as the reliability, validity, and objectivity of the concepts and instruments used, or the presentation of correlation as causation, and so on, has been pointed out (e.g., Cernovsky and Litman 1993; Zuckerman 2003). The methodological critique of empirical research belongs to the standard discursive practices of psychologists. This line of critique also challenges the usage of the statistical tools and operationalizations used. However, race psychologists to a certain degree have improved their methodological-statistical procedures on the basis of such critiques. It has thus become more difficult to convince psychologists of the necessity of a conceptual critique (e.g., of the importance of the historical embeddedness and historical operationalization of intelligence). A reflection on the nature of concepts in general in psychology (Sugarman 2009) and on assumptions in psychology (Slife et al. 2005) is less of interest to psychologists, although or because they go to the heart of the discipline.

The concept of heritability has been challenged, as has the notion that within- and between-group differences refer to different things (Cole and Cole 1993; Gould 1996; Lewontin 1995). Heritability is “the percentage of individual differences (differences across people) in a trait that’s due to genetic differences” (Lilienfeld et al. 2010: 159). Thus, the term has no meaning regarding an individual but only has meaning in terms of a population. These authors, concerned with myths in popular psychology, admit that even scientists—and I would include most

hereditarian race psychologists—have provided false interpretations of heritability. But the authors write in the same chapter that extraversion is about 50% heritable, which itself propagates the myth of a true value of heritability when indeed the heritability of personality traits has no true value, as the average number of children per couple has no true value, and rather depends on historical factors. Heritability of a trait does not mean that differences found between groups can be interpreted as inherited! In addition, the concept of race has undergone many challenges but a conceptual critique of “race” is considered unconvincing against the reality that as an independent variable it creates “facts”.

Within a critique of the *context of practices* one needs to look at how race research is used by academics and in the public domain as well by teachers, educators, and social workers, and at how results from empirical psychology are used. However, the argument in this article focuses on a critique of the *context of interpretation*, by which I mean an analysis of the relationship between theory, data and discussion and an analysis of the quality of the interpretation of data in psychological studies. To philosophers and social scientists the hermeneutic deficit of empirical psychology may be obvious, but in my view it is crucial because the idea of the *underdetermination* of theory (Duhem, Quine, see below) is accessible to mainstream empiricist psychologists even if they are unaware of the problem. The idea that theories or hypotheses do not determine a specific empirical research strategy or that empirical results do not determine a specific interpretation appears to be a starting point for a critique of hereditarian race psychology, and is accessible to mainstream psychologists. In addition, the ideas that for each empirical result several interpretations are possible and that choosing an interpretation that involves ideas of inferiority or has negative consequences for racialized groups is a form of action in need of reflection, seem more acceptable to mainstream psychologists than is a focus on the *context of discovery*.

Theory, Data, and Interpretation

The problem of the relationship between data and interpretation is neither new in philosophy nor in psychology. In 1964 Klaus Holzkamp (1964/1981) published a monograph dedicated to this problem: *Theory and Experiment in Psychology: A study critical of its foundations*. In this book he addressed the relationship between experimental practices and theoretical conceptualizations. He concluded that theoretical conceptualizations are, as one would say in English-speaking contexts, *not determined* by experimental data. More specifically, he argued that the theoretical interpretation of experimental results is not binding, and that there exist no criteria in experimental psychology to establish particular theoretical interpretations as valid. Indeed, the book addresses an issue that has not been solved in the nearly five decades since its publication.

Holzkamp (1964/1981) labeled the ambiguous relationship between theory and experiment in psychology as the problem of *representation*, meaning that the problem concerns the mode in which experimental propositions are representative (or not) of theoretical propositions. The problem arises because for any given

experimental proposition, additional theoretical propositions can be supplemented through *interpretation*, and because each experimental proposition has infinite theoretical meanings. There exists no methodological principle that forces a researcher to interpret given experimental propositions in a specific way. On the other hand theoretical propositions allow for a variety of experimental designs, so that both elements in this process demonstrate plural meanings (see 31).

The assignment of theoretical and experimental propositions is to a certain degree arbitrary. Holzkamp distinguished between *subject representation*, which referred to the issue of the representation of participants in an experimental study regarding theoretical propositions about humans. This relationship between theoretical and experimental propositions in this reflection is different from the statistical problem of sampling—that is, the problem of subject representation addresses *theories* about humans. *Environmental representation* refers to the problem regarding the way in which the environment of the experimental reality is representative of the theoretical propositions of the theoretical world (our world). The same problem applies to *behavioral* and *experiential representation* (e.g., in which ways do experimental propositions regarding anxiety represent anxiety in the lifeworld, or real world, of persons?).

Holzkamp attempted to develop a set of recommendations in order to produce a higher degree of representation of experimental propositions for the theoretical propositions (see also Holzkamp 1968). However, a few years later, after his *critical-theoretical* and later the *socialist* turn, he distanced himself from his own positive methodological recommendations. He criticized himself for believing that a solution to the problem of representation could be found within an experimental psychology dependent on variables, when that belief was clearly misguided because despite “all verbal agreement, nobody cared about the book when doing experiments, not even its own author” (Holzkamp 1964/1981: 277) [my translation] (for a short history of critical psychology, see Teo 1998).

It should be pointed out that Holzkamp was not the first to deal with this problem. Indeed, the original argument was not developed in the context of the social but rather the natural sciences: Pierre-Maurice-Marie Duhem (1861–1916), to whom this idea is originally attributed, was listed by Holzkamp (1964/1981) besides Henri Poincaré (1854–1912), Hugo Dingler (1881–1954), Karl Popper (1902–1994), and Eduard May (1905–1956) as primary non-empiricist influences on his philosophy of science. The physicist Duhem (1905/1954) suggested that experiments in physics always contain observations of phenomena *and* theoretical interpretations. In North America the underdetermination of theory by data is often associated with Quine (1969). Yet, Holzkamp was the first who systematically studied this phenomenon in psychology.

The repression of hermeneutics in psychology has been manifested in two ways: (a) the assumption that an understanding of data is somehow self-evident and constitutes a process that requires no special training; and (b) the assumption that interpretations have no consequences. Since the nineteenth century, when the science of mental life was established as an empirical discipline, psychology has been plagued with the assumption that hermeneutics does not permeate empirical research. One could trace this idea back to Ebbinghaus (1896) who argued against

Dilthey's (1894/1957) concept of a hermeneutic psychology. Ebbinghaus believed that a natural-scientific approach in psychology would solve all problems when studying human mental life.

The disregard of hermeneutics in mainstream psychology has led to a variety of other problems: the idea that theories, once they have been tested, need no further conceptual challenge; the notion that the relationship between empirical results and interpretative discussions is obvious; the assumption that empirical knowledge contains only facts; and the belief that facts speak for themselves. Historically, embracing the natural sciences and the philosophy of positivism were both seen as a way of overcoming the hermeneutic problem. It was assumed in the discipline that hypotheses are tested and theories are *verified* (positivism) or *corroborated* (critical rationalism).

The idea that there is no one-to-one relationship between data and theory or interpretation has been discussed in the natural sciences in the context of the *underdetermination thesis*. Duhem (1905/1954) and Quine (1969) suggested that radically different theories can be supported equally on empirical grounds. It is not generally understood in psychology that data allow for a variety of interpretations; and although each interpretation may be related to data, each interpretation is also connected to a general worldview, a theory, a scientific community, and a larger socio-historical context, particularly when research addresses socially relevant topics. This lack of understanding also explains how eminent scientists could not see the limitations of their interpretations (see Gould 1996).

From an ontological and epistemological point of view the *realm of data* is not identical to the *realm of the interpretation of data* (Fig. 1). Indeed, if results determined interpretations, then psychologists would not need to present *discussions*

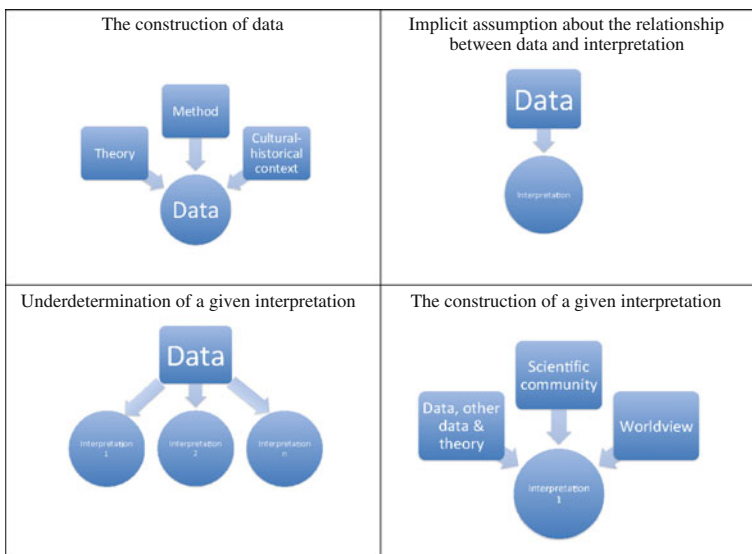


Fig. 1 From data construction to the problem of data interpretation

because results would be sufficient by themselves. An empirical article could end with the results section without any further discussion. Readers of an article, however, would perceive such a practice as deficient because they are interested in the interpretation of the results by the author, the implicit assumption being that the author of a study is also an expert on the interpretation of the results. I would label this a scientific *halo effect*.

Interpretation is left to the hermeneutic competence of the individual researcher, who might not be aware of the role of understanding in interpretation and of his/her own hermeneutic deficits. In the context of scientific racism, sexism, and classism, I suggest that the methodological part is only secondarily responsible for biased research and that the larger part is due to the hermeneutic deficit of researchers. This hermeneutic deficit appears when the epistemological, the ontological, and, indeed, the ethical meaning of studying group differences is not understood, and when rules, criteria, and guidelines for valid interpretations are not provided by the discipline.

The notion of understanding or presenting one interpretation (of many possible) as knowledge in empirical psychology is problematic. Psychologists present interpretations as knowledge or confer on them the label of knowledge because they are published in a scientific journal. This is problematic because of the *hermeneutic deficit* in the nature of empirical research (i.e., the underdetermination of interpretation). Indeed, empirical psychology turns a hermeneutic deficit (one interpretation among many possible) into a surplus (one interpretation is presented as knowledge). The hermeneutic surplus, the presentation of a particular interpretation as knowledge, imparts meaning to data and makes results understandable. Data are understood better than if they were to present themselves. Thus, interpretations have a hermeneutic function for the authors themselves, for peers, and for other readers.

Most clearly the hermeneutic surplus is expressed in textbooks and in the mass media. Textbooks and mass media do not report detailed data but provide a hermeneutic summary of the results based on authors' hermeneutic interpretation of the results. Sometimes this is solely expressed in one sentence, as in the following: "Many studies have shown that (racialized) group A is less empathic than (racialized) group B and empathy has a high heritability coefficient". Of course, it should be mentioned that presenting only the data and leaving out the discussion section in a scientific article would not remedy the situation because interpretations would be confined to laypersons' interpretations of data. Given the political affordances of certain topics we should not expect that this would lead to better interpretations. Indeed, the field would be left with a pandemonium of interpretations, or worse, interpretations that reflect "common sense" ideologies of racism, classism, and sexism.

Although not emphasized in this argument, data themselves are embedded in a hermeneutic context that makes them possible. Data on *race* might not be relevant ontologically but socio-historically. Thus, a deconstruction of the interpretations of data on *race* must examine not only empirical data but must also interrogate the ontological relevance of (psychological) variables such as *race*. Indeed, such an examination would be the first step in any hermeneutic enterprise: to analyze the natural or social kind status of psychological variables, or the representative or

simulated nature of psychological categories (see Danziger 1997). Although this analysis would have hermeneutic primacy in the context of understanding scientific research, such an analysis is neglected in a process where a large number of psychologists share the same ideas about the logic of research. Thus, my focus is on a hermeneutic critique of the relationship between data and interpretation, which is at the core of empirical research itself.

From a psychological point of view it is interesting to understand the hermeneutic surplus. An interpretation has credibility because an expert expresses it. However, the expert often does not have hermeneutic training, or interprets results within his or her *disciplinary matrix* (Kuhn 1970). Results are organized within a worldview and if one does not share the worldview then the data do not make sense. Ramachandran (1997) published an article with the title “Why do gentlemen prefer blondes?” and discussed the adaptive advantages of being blonde. Later, he admitted that it was a hoax in order to challenge the quality of interpretations in evolutionary psychology. However, evolutionary psychology provides just one of many examples showing the problematic nature of how data are interpreted (let alone how data are generated). The problem of the hermeneutic surplus not only applies to certain branches of psychology but to empirical psychology in general.

The hermeneutic surplus goes hand in hand with the rhetoric of “facts”. But facts or empirical knowledge, or even truth, contain data *and* interpretations. One could label this phenomenon as a self-misunderstanding of empirical psychology: Although it is clear that data and interpretations (discussions) are separated in a scientific article, authors often present their discussions as knowledge and facts. Historical examples demonstrate the meaning of a hermeneutic surplus more clearly because common sense interpretations or interpretations embedded within a *Zeitgeist* have shifted away from these historical interpretations, and the absurdity of the discussions is often clearly seen (e.g., for a discussion of the absurdity of the belief that Italians are by nature less intelligent than Northern Europeans, see Gould 1996).

Epistemological Violence

My argument is focused on the *interpretation of data* and on the lack of hermeneutic competence. I suggest that interpretations, as most often expressed in the discussion section of empirical articles, are a form of action, and if concrete interpretations (e.g., group X is by nature less intelligent than group Z) have negative consequences for groups—even though alternative, equally plausible interpretations of the data are available—then a form of violence has been committed. Because the interpretations are presented as knowledge, or because they emerge from science, they represent *epistemological violence* (EV) (see also Teo 2008, 2010).

Thus, my definition does not refer to the way knowledge is produced, or how prejudices lead to biased knowledge in general, or to the notation that knowledge may be used in a negative way, but refers to the interpretation of data to the detriment of the *Other*. One could make the argument that any study on racialized

group differences is a form of epistemological violence. However, such an approach would not be playing the same language game as empirical psychologists play, and, thus, my concept of EV is limited to the interpretation (discussion, or combined results/discussion) part of an empirical article. This specific focus and limitation provides a framework for concrete analysis.

There are at least two forms of EV in the interpretation part: the interpretation itself can be a form of violence, for instance, because the concept of *race* is not challenged and when psychological group differences are understood as inherited; and the interpretation can be violent because specific policy recommendations are made or accepted (e.g., regarding the separation or segregation of the two groups). Traditional psychologists will have fewer problems with the second kind of EV, because it reinforces the distinction between facts and decisions. However, the first form of EV might be more contentious among traditional psychologists because it requires an understanding of the historical and theoretical situatedness of concepts, as well as an acceptance of the idea that empirically validated research itself can have a negative impact on human groups, and because the interpretation is underdetermined (e.g., the interpretation that one group is by nature less empathic than another group can have negative impact).

In 1955 J. L. Austin delivered his famous *William James Lectures* at Harvard University on “How to do things with words”. Austin (1962/1975) pointed out that certain utterances (performative sentences) are parts of doing and action. Similarly, I suggest that interpretations of data are inherently underdetermined in empirical psychology and that choosing and expressing specific interpretations, out of many possible equally plausible interpretations, and which have negative consequences for the *Other* racialized group, are a form of action (violent action). It might be considered problematic to label academic interpretations, for instance, those contained in journal articles, as actions. This is not what Austin had in mind in his classification of speech acts, but I suggest that the idea of interpretations as actions is a useful device. Going to the laboratory, arranging a room for an experiment, talking with subjects, entering data into a computer, conducting data analyses, and publishing the interpretations of data, are all forms of action. The scientist (author) has *intentions* when writing interpretation (s/he wants to do justice to the data, support a certain theory and reject another one, communicate with other authors, convince the public, promote an agenda, etc.); and the interpretations have *consequences* for readers who themselves might draw certain conclusions regarding the *Other* or disseminate the interpretations to more readers (e.g., as would a journalist reporting about a study).

In terms of its temporal dimension, an interpretation understood as violent action has a delayed consequence. Whereas hitting someone has an immediate impact, an interpretation that is not read has no impact; yet, an interpretation that is read by many—or by a single individual—may produce a large impact. The interpretation that group X by their very nature is less intelligent than group Y has no consequence when nobody reads or hears about this interpretation. However, the interpretation may have a large impact if it is accessed by others and there is a societal willingness and receptiveness towards such interpretations. It should be emphasized that scientific interpretations have a special status because they are not performed within

an everyday context and because of the status of science in society. This status includes the idea that interpretations represent truth.

Actions have an internal side, which refers to the intentions of the person executing the action, and they have an external side, in having consequences or in changing something in the world. Yet, interpretations are more on the side of physical actions than they are on the side of thinking or believing. If I say that group X is by nature more violent than group Y, based on empirical studies, then the statement has real implications, when such a statement is expressed by a scientist and in a cultural-historical context where this concept has meaning and sciences hold a particular status.

In summary, I propose that an interpretation of data that does harm to the *Other* is a violent interpretation, and more specifically, a form of violent action when the *Other* is constructed as inferior. Because this violent action is committed in the name of science, the term EV is justified. Interpretations in the context of empirical studies are themselves actions, epistemological actions that have an authoritative quality in our society because they are performed by scientists. An interpretation in the context of empirical psychology or any human science gives meaning to data that do not have meaning per se. Again, one could make the argument that the establishment of data itself requires a hermeneutic action. This may be correct, but this is not the primary topic of this reconstruction.

Interpretations are actions and often interpretations have action impetus. We can distinguish at least three levels of action impetus (for further action):

- (a) In *descriptive interpretations* the focus is on representing and summarizing what has happened in an empirical study. The process of representing and summarizing requires some hermeneutic competence, but it does not necessarily imply action. In that sense the descriptive interpretation has the least action impetus. For instance, if a study finds that racialized group X has shorter hair than racialized group Y, then we could argue that this is a descriptive interpretation. Of course, if having shorter or longer hair has a moral standing in a given culture, a descriptive interpretation may become a *normative interpretation* (see below).
- (b) A *normative interpretation* takes place in the context of a community, and in such a context, labels, words, and concepts may have moral meanings. The choice of labels in an interpretation may have a normative meaning. If a researcher argues that group X individuals are lazy in comparison to group Y individuals, then a process of normative interpretation has taken place. There is clearly an ethical dimension to this characterization. Alternatively, the researcher might have labeled the same behavior as “relaxed”. The statement would take on a different meaning: group X individuals are more relaxed in comparison to group Y individuals. This also counts as a normative statement but with different implications for the *Other*. These interpretations, regarding laziness and relaxedness, have a moderate action impetus because someone might conclude, in the former case, that something should be done about the laziness of group X people, and in the latter case, that others should aspire to be as relaxed as members of group Y. Admittedly, it might be difficult to

- choose a completely neutral concept, given that many concepts in everyday language that refer to human characteristics have normative connotations.
- (c) A *prescriptive interpretation* has the highest action impetus because recommendations are expressed explicitly. For instance, the suggestion that group X people should be put into a particular type of school or educational stream because they are lazy is clearly a recommendation for practice or policy. Despite differences in terms of the character of interpretations and their action impetus, it should not be forgotten that all published interpretations are forms of actions, as they are the results of actions such as writing. From the perspective of a writer, interpretations as expressed in articles or books are *past* actions, but from the perspective of a reader they are *current* actions on the part of the writer, in the sense of becoming actualized in the process of reading. Further, such interpretations may be considered future actions insofar as they prescribe future policy.

Because interpretations are actions, we can evaluate the concrete consequences of interpretations. If interpretations are beneficial to the *Other*, then they may still be underdetermined by the data, but they may not lead to harm. On the other hand, some actions have harmful consequences for *the Other*. In such cases, communities of the negatively constructed *Other* should be the source for establishing the criteria for harm. There exists collective violence (wars), individual violence (one person against another), and violence executed by scientists (epistemological violence, or EV). EV is likely to be asymmetrical: EV executed by scientists cannot be countered easily by rejection because the name of science has a higher status than theoretical criticism expressed by a marginalized *Other*.

Following Habermas (1971), one could argue that the communicative situation between the researcher and the *Other* (e.g., as a member of group X) is the opposite of an ideal speech situation. The *Other* can reject the interpretation, but because the scientist accomplishes the interpretation in the name of science, he or she will have a privileged authority. Truth claims are asymmetrical: whereas the scientist can argue that the *Other* is motivated by political or moral concerns, he or she is interested in truth; if the *Other* argues that the scientist is motivated by political interests, then the *Other* can easily be rejected. Because of power inequalities that exist in a particular society, the scientist can refer to his or her training and methodology; essentially, the scientist can refuse to listen or take seriously the truth claims made by the *Other*.

I rely on the concept of EV to identify interpretations that construct the “Other” as problematic or inferior, with implicit or explicit negative consequences for the “Other,” even when empirical results allow for meaningful, equally compelling, alternative interpretations. I would not agree with Derrida (1967/1976) that all forms of interpretations are violent. The interpretations of *epistemic violence* by Derrida as well as Spivak (1988) are too general to provide an analytical tool for addressing consequential differences in interpretation in empirical psychology.

It should also be mentioned that it may be useful to distinguish between manifest and latent forms of EV in empirical psychology. If an expert argues, based on empirical data, that “it seems not unreasonable” to argue that group X individuals

are by nature less intelligent than group Y individuals, and then suggests that this interpretation is a hypothesis to be tested in future research, then EV has still been committed. This interpretation expressed by an expert scientist *after* the production of data is still a form of epistemological violence. However, because the expert does not present it as a fact but as an argument, one should label it as a more latent form of epistemological violence. I suggest that transforming the rhetoric of facts into the rhetoric of legitimate arguments is still a masked form of epistemological violence. For instance, if a research psychologist finds a difference in income between group X and group Y and argues that the difference *may* have to do with group X's greed, expressed as a hypothesis and not as a fact, then we still should label it as epistemological violence. Expressing a hypothesis that may have negative consequences for groups is a form of action that must be analyzed (see also the discussion surrounding Summers 2005).

Thus, the shift in rhetorical justification of EV can also be analyzed in terms of socio-historical changes. Historical examples show that interpretations that were epistemologically violent were often presented as scientific facts and knowledge. Given the shift in public opinion, and on the background of multicultural realities, such interpretations are nowadays more often presented as hypotheses—although manifest forms of EV have not disappeared in academia. However, a shift in *Zeitgeist* does not change the understanding of interpretations—reframed as hypotheses—as forms of EV in new clothes. These hypotheses are presented after the fact, after data have been generated, and constitute theoretical interpretations of data that are rephrased as hypotheses.

Although I would argue that interpretations should include ethical criteria and hermeneutic responsibility, I am aware that such ideas will not persuade academics that might perceive the issue as another attack on academic freedom. Instead, it can be pointed out that the problem of interpretation in regard to EV is a *methodological* issue. The criteria for the validity, reliability, and objectivity of hermeneutic interpretations need to be elaborated. For the time being, my intent is to raise awareness regarding the problem of interpretation as a form of action with consequences.

Conclusion

Interpretations are forms of action, and the supposed hermeneutic self-evidence is a self-misunderstanding or a rhetorical tool in empirical research. The combination of the hermeneutic deficit and surplus is responsible for the re-occurring emergence of scientific noise in race psychology (see also Richards 1997). Given the lack of relevance of many psychological studies, one could argue that the content of the interpretation that a researcher chooses does not make a practical difference. This argument may be true, but it prevents a sharpening of the hermeneutic skills of researchers and of their thinking about the consequences of interpretations. This empiricist credo, which neglects the fact that knowledge contains results and interpretations, that theories involve a process of understanding, that results do not determine interpretations, and that so-called facts contain results and interpretations,

becomes particularly relevant when it is applied to human groups and their differences.

Interpretations have an impact on people. The denial of the impact of interpretations, a problem that is not understood in empirical psychology, can also be traced back to the concept of values in psychology and the idea of value-neutrality. Mainstream psychologists both past and present have emphasized that *fact* (what is) and *value* (what ought to be) are two different domains that should be kept separate. The problem is that, in the social sciences, these two domains are inherently intertwined. Even in the natural sciences, as environmental issues such as global warming or the effect of human activity on the environment show, some topics are not simply research issues but are topics that suggest implications for action. To remain value-neutral on these issues would be a political stand in itself. A critical reflection should challenge the empiricist idea that science is neutral on political issues and concerns.

The concept of EV demonstrates that the traditional separation of “is” and “ought” is problematic. As research is not neutral, so are interpretations not neutral. Choosing an interpretation that has negative consequences for a group is not a value-neutral action (and of course, value-neutrality itself is a value). Supporters of epistemologically violent interpretations are sometimes aware of the consequences of their interpretations and may be instrumental in advancing a specific worldview of political and social ideas. For instance, if someone were to suggest that we cannot boost IQ for group X children and that therefore early childhood education programs are unnecessary, then this interpretation has to be understood within a larger social context.

Critical hermeneutics is useful not only for pointing out shortcomings of traditional approaches; it can also be used to better understand the world—in this case empirical research in the human sciences—in order to change it. I concur with Habermas (1967/1988) that hermeneutics alone is insufficient for understanding empirical research if hermeneutics is not accompanied by a critical reflection in all contexts. Yet, understanding and interpretation have an emancipatory purpose—as the previous reconstructions show. In a 2008 publication on this topic I had suggested a methodological solution to the problem and a change to the ethical code of psychologists (Teo 2008). I favored the establishment of a *hermeneutic collaboration model* (see also Joseph 2004: 340–342). According to this model one researcher (or group of researchers) executes the study and produces the data and another group of interpreters (who may range from adversarial, indifferent, or sympathetic to a given program) provide a set of interpretations of the data. For example, if a researcher studies ‘race’ differences in intelligence, then the researcher does not provide the discussion; instead this would be the task of four to six different interpreters.

In hindsight such suggestions remain *voluntaristic* and do not do justice to the social and embodied nature of empirical psychological research. Psychology has been an extremely successful discipline in Europe and North America in terms of academic and professional expansion. However, success does not necessarily imply an epistemological contribution or the ethical–political quality of its practice. Race psychology has been successful in gaining public attention and in attracting funding,

and its practitioners have led successful academic careers at some of the best-known universities. There is no need for race psychologists to participate in any collaboration model or to adhere to an ethical code that warns of violent interpretations. Maybe the best solution would be to ignore these types of studies—but it would leave the public discourse entirely to the influence of hereditarian race psychologists.

What remains for the historian and theoretician of psychology is to perform the critical function of science, or to phrase it in more contemporary terms, of “bullshit studies on race psychology”. A core feature for Frankfurt’s (1986/2005) definition of bullshit is a lack of concern with truth and an indifference towards reality. Although Frankfurt focused on public life, this can also be applied to race psychology (see also Proctor and Schiebinger’s 2008, program of *agnotology*). Indeed, race psychology is often not only empirically false but also phony, and race psychologists are often bluffing. But faking things does not mean that the bullshitter gets everything wrong (given the social formation of facts). Still, the bullshitter misrepresents what he or she is up to. Unfortunately, this misrepresentation involves the production of bullshit with enormous consequences. Accordingly, the epistemological, ontological, and ethical shortcomings need to be given voice. My reflections on epistemological violence in race psychology represent one of these voices.

Acknowledgments This article is based on a paper presented in the fall of 2010 at the 7th *California Roundtable for Philosophy and Race* at Northwestern University. It is a continuation of my work on epistemological violence (Teo 2008, 2010) supported by a *Social Sciences and Humanities Research Council of Canada (SSHRC) Standard Research Grant*.

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