History of Psychology

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Recurring errors among recent history of psychology textbooks

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Five recurring errors in history of psychology textbooks are discussed. One involves an identical misquotation. The remaining examples involve factual and interpretational errors that more than one and usually several textbook authors made. In at least 2 cases some facts were fabricated, namely, so-called facts associated with Pavlov's mugging and Descartes's reasons for choosing the pineal gland as the locus for mind–body interaction. A fourth example involves Broca's so-called discovery of the speech center, and the fifth example involves misinterpretations of Lloyd Morgan's intentions regarding his famous canon. When an error involves misinterpretation and thus misrepresentation, I will show why the misinterpretation is untenable.

The emphasis here is on examples of errors that multiple authors of history of psychology textbooks have made. Sometimes an author repeats an error through several editions of a textbook. "Clearly, there are different kinds of errors in scholarly research and publications. In some cases, a proposition that concerns facts is incorrect, but in other cases one may claim that an interpretation is wrong" (James L. Pate, January 25, 2004, e-mail, Cheiron listserv). Furthermore, sometimes the facts are incorrect because they have been fabricated.

Five examples of recurring errors are discussed here. One involves an identical misquotation that both textbooks' authors presented in quotation marks, showing that they had not intended to paraphrase. In two cases, Pavlov's mugging and Descartes's reasons for choosing the pineal gland as the site of mind–body interaction, some facts were fabricated. The remaining two examples involve omission of significant people who were responsible for Broca's so-called discovery of the speech center in

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the cerebral cortex, resulting in the misrepresentation of that discovery; and multiple misinterpretations regarding Conwy Lloyd Morgan's intentions when he constructed his famous canon. When misinterpretation and misrepresentation are involved, I will present evidence to show why it is a misinterpretation and why it is untenable.

I did not set out to find errors in history of psychology textbooks. Three examples emerged in conjunction with research I had done on Pavlov's mugging, Broca's so-called discovery of the speech center, and Morgan's canon, and I merely wanted to see how contemporary history of psychology textbooks presented the topics. I noticed the misquotation error coincidentally, when I used Thorne and Henley's (2001) textbook to teach a class in the history of psychology. The fifth example involves Descartes's reasons for choosing the pineal gland as the site of interaction of the mind and the body. Finger (1995) identified two histories of psychology textbooks that erroneously attributed to Descartes the belief that only humans had pineal glands, and I wanted to see how other textbooks were representing Descartes's reasons for choosing the pineal gland.

The recent (1990 or later) history of psychology textbooks surveyed are listed in the first section of the references. To explain some aspects of how the present research was conducted, it is pertinent that this article began as two presentations in Key Barkley Symposia on the History of Psychology (Thomas, 2004, 2006) at annual meetings of the Southern Society for Philosophy and Psychology. The textbook survey for the 2004 presentation was limited to the 18 most recent editions (none predating 1990) of textbooks readily available to me at that time. When I did the survey in early 2004, I was unaware of Hothersall's and Schultz and Schultz's 2004 editions. I added those and others not previously surveyed to the 2006 survey, with newer editions of some of the textbooks I had examined in 2004. For the 2006 presentation I added a new error, the one that involved Descartes and the pineal gland, and that resulted in a second survey of all the textbooks. I examined all editions of Fancher's, Hergenhahn's, and Thorne and Henley's textbooks because of their special association with two of the recurring errors: Fancher's and Hergenhahn's errors regarding Pavlov's mugging and Hergenhahn's and Thorne and Henley's Santayana misquotation.

Santayana quotation

"Those who cannot remember the past are condemned to repeat it" (Santayana, 1905, p. 284) is a well-known quotation that was included prominently in the Holocaust memorial at Auschwitz, and it has been used in numerous documentary and dramatic film productions and published accounts associated with the Holocaust. The quotation has been used in other contexts to extol the value of learning history.

"Those who do not know history are doomed to repeat it" is how the Santayana quotation appeared in Hergenhahn's (1986, p. 7; 1992, p. 4; 1997,

p. 4; 2001, p. 4) and Thorne and Henley's (1997, p. 3; 2001, p. 2; 2005, p. 3) history of psychology textbooks. This misquotation error appeared in both books in a section titled "Why Study the History of Psychology?" The misquotation was attributed to Santayana and was presented in quotation marks. None of the books cited a reference for the quotation.

Thorne and Henley (1997, 2001) may have based their misquotation of Santayana on Hergenhahn, because Hergenhahn misquoted Santayana in two editions that predated Thorne and Henley's first edition. Ironically, in Hergenhahn's first four editions, the erroneous quotation appeared in the first sentence of a subsection titled "Avoiding Repetition of Mistakes." Despite efforts to do so, I was unable to determine where or how the authors obtained the misquotation. Both may have copied a third author who misquoted it, or both may have committed the error independently. Hergenhahn corrected the error in his 2005 edition. Thorne and Henley perpetuated the error in their 2005 edition.

Pavlov's mugging

Documentation pertaining to this error has been provided elsewhere (Thomas, 1997a), and only an abbreviated account will be presented here. Thomas reported information about Pavlov's having been robbed in New York City in 1923 (*mugged* is an apt description) that conflicted in significant ways with accounts that were being presented in some history of psychology textbooks. The error involves different published accounts of Pavlov's mugging, all of which Thomas quoted. Only the account deemed most reliable, namely that from *The New York Times,* and the two accounts that have been the sources of recurring errors will be quoted here. Not quoted here are accounts by Cannon (1945/1968), whom Pavlov visited within a few days of the robbery, and by Babkin (1949), Pavlov's long-time colleague and biographer; however, their accounts corroborated and added details to the report of the robbery in *The New York Times.*

According to *The New York Times,*

> He [Ivan Pavlov] and his son [Vladimir] had hardly taken their seats on the train in the Grand Central Station when three men set upon the old man and snatched from him his pocketbook containing all their funds, $2,000. The porter and the son attempted to catch them but were unsuccessful, and the old man and his son left the train perplexed as to what they should do in their predicament. They finally got in touch with Dr. P. A. Levene of the Rockefeller Institute, and since have been the guests of the institute. ("Russian scientist," 1923, p. 3)

The least reliable account appeared in *Time, The Weekly News Magazine* (1928), 5 years after the robbery; this is usually cited as Gerow (1988), but Gerow's only role was to oversee a compilation of articles about psychology that had appeared in *Time* from 1923 to 1988.

According to *Time, The Weekly News Magazine,*

> Three years ago Pavlov came to America. Confused by rush and roar he sat for a moment on a seat in Grand Central Station, Manhattan. A small handbag containing much of his money lay on the seat beside him and with characteristic absorption in the seething human laboratory around him, he forgot his worldly goods completely. When he rose to go the handbag was gone. It had been taken from under his very nose. "Ah, well," sighed Pavlov gently, "one must not put temptation in the way of the needy." ("Conditioned reflexes," 1928, p. 20)

The unnoticed theft, the words spoken by Pavlov, and other fabricated details together with the error of placing Pavlov in New York in 1925 instead of 1923 render the *Time*/Gerow (1988) account erroneous and unreliable. Those who used it uncritically include Hothersall (1995), Schultz and Schultz (1996), and Thorne and Henley (2001). In fairness, Hothersall (1995) may have been published before he had access to the earliest information provided by Thomas (1994); Hothersall (2004) cited Thomas (1997a) and quoted *The New York Times* report. Schultz and Schultz (2000, p. 262), whose 1996 edition was identified by Thomas (1997a) for making this error, used the *Time*/Gerow version again in 2000 but noted, without citing a reference, that "several versions of this incident have been reported"; they did not say that the *Time*/Gerow version was unreliable. Schultz and Schultz (2004) eliminated all coverage of Pavlov's having been robbed. Thorne and Henley (2005, p. 328) summarized the *Time*/Gerow account and followed it with, "Thomas (1997) has argued convincingly that this account . . . is almost completely fabricated." Thorne and Henley then summarized a composite of *The New York Times* (1923), Cannon (1945/1968), and Babkin (1949) accounts.

Fancher provided the following erroneous accounts, which have been the source of another recurring error:

> On a trip to New York he [Pavlov] carried all of his money in a conspicuous wad protruding from his pocket; when he *entered the subway at rush hour,* the predictable felony ensued and his American hosts had to take up a collection to replace his funds. (1979, p. 279; emphasis added)
>
> All of his money—more than $800 in small bills—was jammed into a bulky wallet that protruded visibly from his jacket pocket. When Pavlov ventured onto *a crowded New York subway,* the predictable felony occurred. (1990, p. 300; emphasis added)

Except for locating the robbery on the subway at rush hour, Fancher's account bore similarities to Babkin's (1949) account. For example, Babkin said that he remembered the amount as being $800, but he did not dispute Cannon's (1945/1968) recollection that it involved more than $1,500; other sources (see Thomas, 1997a) indicated the amount was between $1,500

and $2,000. However, unlike Fancher, Babkin (1949, p. 107) wrote, "As they were boarding the train, several men surrounded Pavlov and began jostling him on the platform of the car," which is reasonably consistent with *The New York Times* report and with Cannon's (1945/1968, p. 185) report that Pavlov and son were robbed when they "entered an empty coach of the New Haven train and were followed by three rough-looking men."

In his 1996 edition, Fancher changed his account of Pavlov's robbery to be more compatible with that in *The New York Times*. Hergenhahn has quoted Fancher's (1979) erroneous account persistently (Hergenhahn, 1986, p. 237; 1992, p. 340; 1997, p. 346; 2001, pp. 342–343; 2005, p. 358), but beginning in 1996 Hergenhahn added, "(For other versions of Pavlov's mugging in New York, see Thomas, 1994.)" Hergenhahn did not explain his preference for Fancher's account.

Broca's so-called discovery of the speech center

Documentation for this section came primarily from Clarke and O'Malley (1968), Joynt (1964–1965), Krech (1963), Schiller (1979), Stookey, (1954, 1963), and Thomas (1997b). Again, a much abbreviated account of the pertinent evidence is necessary here. Broca's initial involvement was to supply two patients whose lesions defined the motor speech center in the cerebral cortex, and Broca extracted and presented the brains to the Société d'Anthropologie in 1861. In that narrow sense, Broca "discovered" the speech center. However, Broca provided these patients as a result of Ernest Auburtin's (also representing Jean-Baptiste Bouillaud) assertions regarding the cortical localization of speech during the April 4, 1861, meeting of the Société d'Anthropologie in Paris. At that time, Broca declined to take a position regarding the question of the cortical localization of speech.

During the aforementioned meeting on April 4, 1861, Auburtin stated that he would renounce his views about cortical localization of speech if a patient in Bouillaud's care did not bear him out when that patient's brain was presented at autopsy. It is unclear precisely why Broca intervened and offered for Auburtin's examination one of his own patients, LeBourgne, but perhaps it was because LeBourgne had a similar neurological history, he was in closer proximity, and his demise was more imminent. In any case, LeBourgne's brain was presented before the Société by Broca two weeks (April 18, 1861) after Auburtin had expressed his views regarding the cortical localization of a speech center.

Perhaps most pertinent to the question of who deserves credit for discovering the speech center, Broca stated the following during the May 2, 1861, meeting of the Société d'Anthropologie:

> During the last session [April 18, 1861] I showed you the brain of a man . . . [LeBourgne] in which a lesion of the frontal convolutions had abolished the faculty of speech. I felt obliged to present to the society this rare and

> curious fact which by strange coincidence has fallen into my hands at the same time that Mm. Gratiolet and Auburtin were discussing the site of the faculty of speech. But, while I inclined towards M. Auburtin's opinion, I did not intend to take part in the debate. I am expressing myself neither for or against specific localizations. (Clarke & O'Malley, 1968, p. 495)

Thus, it was Auburtin, also representing Bouillaud, who made the argument for localization of speech. Broca was an uncommitted bystander.

It is not my purpose to diminish Broca's important contributions. Broca, more than Auburtin and Bouillaud, pursued the research on cortical localization of speech, and Broca (1861) described Auburtin's and Bouillaud's contributions appropriately in his published account of LeBourgne's case. Broca's article has been available in English translation at least since 1960 (Bonin, 1960), and as Joynt (1964–1965, p. 206) noted, "Broca himself would have, undoubtedly, never claimed priority or novelty for many of his views on aphasia." Regarding the localization of speech in the cerebral cortex, Joynt (1964–1965, p. 208) noted, "It is also well to note that Broca in all his communications gave full credit to Bouillaud's earlier observations." Joynt also discussed Auburtin's contributions.

Broca did not misrepresent his role in the discovery of the speech center; subsequent writers did. When an author represents the discovery of the speech center as Broca's alone and fails to acknowledge Auburtin's and Bouillaud's contributions, that becomes an untenable error of misrepresentation regarding the question of discovery. Recognition of Franz Joseph Gall is also appropriate (e.g., Joynt, 1964–1965; Zola-Morgan, 1995), but his immediate contribution to the discovery of the speech center per se was much less than that of Auburtin and Bouillaud.

In contrast to authors of history of psychology textbooks, historians of neuroscience usually represent Auburtin's, Bouillaud's, Broca's, and even Gall's roles appropriately (e.g., Brazier, 1959; Clarke & O'Malley, 1968; Finger, 1994; Head, 1926; Krech, 1963; Marshall & Magoun, 1998; Plum & Volpe, 1987; Young, 1990; Zola-Morgan, 1995). A notable exception was Broca's biographer, Francis Schiller. Schiller (1979) acknowledged Auburtin and Bouillaud but argued that Broca deserved most of the credit for discovering the speech center. Stookey (1954, 1963) took a near-opposite view from Schiller. Both Schiller and Stookey relied heavily on the minutes of the meetings that were published in the *Bulletin Société d'Anthropologie.* However, Stookey cited the *Bulletin* far more extensively, and he cited evidence from those minutes that Schiller did not cite. For example, Stookey (1954, 1963) reported that on April 4, 1861, when the issue first arose before the *Société,* Auburtin described a patient with a gunshot wound that exposed frontal regions of the cerebral cortex but did minimal direct damage to the brain itself. The conscious patient's speech was arrested reliably by Auburtin's mechanical depression of the

anterior cortex. Having examined Schiller's and Stookey's arguments closely, I believe that Stookey made the stronger case (Thomas, 1997b).

The following history of psychology textbooks acknowledged *only* Broca in conjunction with discovery of the speech center: Bolles (1993), Brennan (2003), Goodwin (1999, 2005), Hunt (1993), Leahey (2001, although Leahey, 2004, mentioned Gall), Lundin (1996), Schultz and Schultz (2000, 2004), Viney and King (2003), Watson and Evans (1991), and Wertheimer (2000). Hergenhahn (2001, p. 217) named only Broca but wrote, "Other researchers have implicated the area . . . that Broca found to be damaged in the control of speech"; of course, writing "have" rather than "had" leaves the priority issue unclear. Hergenhahn (2005) noted Bouillaud's contributions that preceded Broca's, but Hergenhahn did not mention Auburtin.

Benjamin (1997; the applicable section was written by Stanley Finger, who may be described best as a historian of neuroscience), Fancher (1996), Hothersall (1995, 2004), and Thorne and Henley (2001, 2005) provided commendable coverage that appropriately acknowledged Auburtin's, Bouillaud's, and Broca's contributions to the discovery of the speech center. The authors of other textbooks listed in the first section of references that are not otherwise mentioned here (Benjafield, Robinson, and Smith) did not include discussion of the discovery of the speech center.

Most authors of history of psychology textbooks include coverage of the discovery of the speech center, and most of those who include it misrepresent it by omitting Auburtin's and Bouillaud's names and roles. The contrast between history of psychology textbooks that omit Auburtin and Bouillaud and those that acknowledge them (together with the overwhelming majority of historians of neuroscience, who include Auburtin's and Bouillaud's contributions) makes the omission of Auburtin and Bouillaud regarding the discovery of the speech center an untenable error of misrepresentation.

Misrepresentation of Lloyd Morgan's canon

Extensive documentation pertinent to this section may be seen in Thomas (1998, 2001, 2002), and an abbreviated presentation of the pertinent evidence is given here. Apparently, Lloyd Morgan's canon was presented first in an article by Dixon (1892), but the most frequently quoted source is Morgan's *An Introduction to Comparative Psychology* (1894, p. 53).

> In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale.

Morgan's canon has long been misrepresented as psychology's special case of the law of parsimony, Ockham's razor, or otherwise as expressing

Morgan's preference for the simplest explanation when alternative explanations are possible; hereafter, these three versions of a similar kind of misrepresentation will be discussed using the single term *parsimony.* Morgan's canon has also been misrepresented as expressing Morgan's opposition to anthropomorphism and to using anecdotes to study animal behavior, especially those associated with Romanes's *Animal Intelligence* (1882).

Regarding the canon and parsimony, 2 years before stating the canon, Morgan (1890, p. 174) wrote, "We do not know enough about the causes of variation to be rigidly bound by the law of parcimony." (Hamilton [1869, p. 546], who adapted the term from common usage to its special use in philosophy, also spelled it *parcimony.*) More significantly, and in the context of anticipating possible objections to the canon, Morgan (1894, p. 54) wrote:

> A second [anticipated] objection [to the canon] is, that by adopting the principle in question, we may be shutting our eyes to the simplest explanation of the phenomena. Is it not simpler to explain the higher activities of animals as the direct outcome of reason or intellectual thought, than to explain them as the complex results of mere intelligence or practical sense experience? Undoubtedly, it may in many cases seem simpler. It is the apparent simplicity of the explanation that leads many people to naively adopt it. But surely the simplicity of an explanation is no necessary criterion of its truth.

Clearly, Morgan did not intend that the canon advocate a preference for parsimony. Rather, he conceptualized a scale of psychological processes in terms of evolutionary development, with "higher" processes evolving from "lower" processes, and as may be seen in this quotation, in his view some processes he deemed to be lower might be more complex than other processes deemed to be higher. In later editions of *Introduction to Comparative Psychology,* Morgan revised the canon both to substitute the phrase "psychological processes" for "psychical faculties" and to clarify his intention that *scale* refer to evolutionary development:

> In no case is an animal activity to be interpreted in terms of higher psychological processes, if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development. (Morgan, 1903, p. 59)

Elsewhere, the case has been made forcefully that Morgan's canon was not anti-anthropomorphic. Consider the following:

> A glance into Morgan's books is enough to refute this assumption. "Introspection must inevitably be the basis and foundation for all comparative psychology" [Morgan, 1894, p. 37]. Any human introspection would necessarily be anthropomorphic. (Miller, 1962, pp. 214–215).

Referring to the misrepresentation of Morgan's canon as being a canon of parsimony and as being anti-anthropomorphic, Costall (1998, p. 18) wrote, "The extent to which the intentions of Morgan's canon have been misinterpreted is astonishing." Similarly, Wozniak (1993, pp. ix–x) discussed the misrepresentation of Morgan's canon as a principle of parsimony, and on the point of its being anti-anthropomorphic, he wrote, "Even worse, it is consciously anthropomorphic and based squarely on the adequacy of the psychologist's personal introspection." Wozniak also wrote, "It would be an interesting study in itself to trace the progressive distortion of Morgan's views and in particular the attribution to Morgan of the principle of parsimony" (p. ix).

When or on what basis did such "astonishing" (Costall's word) and "progressive distortion of Morgan's views" (Wozniak's phrase) rise to the level of being untenable misrepresentations in history of psychology textbooks? Efforts to correct the misinterpretation of Morgan's canon began as early as Adams (1928) and include subsequent vigorous efforts by Newbury (1954), Miller (1962), Gray (1963a, 1963b), Singer (1981), Costall (1993, 1998), Wozniak (1993), Costall, Clark, and Wozniak (1997), and Thomas (1998, 2001). Thomas (2001), who compiled numerous examples of the misrepresentation of Morgan's canon, wrote,

> It was deemed appropriate to include an author [as misrepresenting Morgan's canon] when that author associated Morgan's canon with Ockham's razor, the law of parsimony, or a simplicity criterion or represented Morgan's canon as being anti-anthropomorphic or anti-anecdotal, if that author did so without negating the association with regards to Morgan's intentions or did so without acknowledging any of the history of misrepresentation in conjunction with such associations.

Regarding whether Morgan's canon was intended to be anti-anecdotal and, as many have written, directed against Romanes (e.g., 1882), please consider the following quotations from Morgan. These were expressed contemporaneously with his original formulation of the canon (which was cited first by Dixon, 1892, before Morgan included it in his book in 1894). In his book *Animal Life and Intelligence* (1890, p. 362), Morgan wrote,

> I do not propose to bring forward a number of new observations on the highly intelligent actions which animals are capable of performing. Mr. Romanes has given us a most valuable collection of anecdotes on the subject in his volume on "Animal Intelligence."

Romanes died while Morgan's *Introduction to Comparative Psychology* (1894) was in final preparation, as Morgan acknowledged in a footnote highly complimentary of Romanes in the *Preface* (p. x). Morgan, a close friend of Romanes, also gave a eulogy for him that included the following:

> By his patient collection of data, by his careful discussion of these data in the light of principles clearly formulated; by his wide and forcible advocacy of his views, and above all by his own observations and experiments, Mr. Romanes left a mark in this field of investigation and interpretation which is not likely to be effaced. (Morgan quoted in Sanderson, 1895, p. xiii)

The "patient collection of data . . . [and their careful discussion] . . . in the light of principles clearly formulated," especially when contrasted with "[Romanes's] own observations and experiments" refers clearly to Romanes's collection of anecdotes as assembled and used in his book *Animal Intelligence* (Romanes, 1882). Had Morgan intended that his canon oppose the use of anecdotes, surely he would have chosen other bases on which to praise Romanes.

History of psychology textbooks that misrepresented Morgan's canon as a canon of parsimony are those by Benjafield (1996, 2005), Benjamin (1997), Brennan (2003), Goodwin (1999, 2005), Hergenhahn (2001, 2005), Hothersall (1995, 2004), Hunt (1993), Leahey (2001, 2004), Lundin (1996), Schultz and Schultz (2000, 2004), Viney and King (2003), and Wertheimer (2000).

Fewer authors addressed anthropomorphism in conjunction with Morgan's canon, but of those who did, Benjamin (1997), Brennan (2003), Goodwin (1999, 2005), and Lundin (1996) misrepresented it. In addition to Smith (1997) and Thorne and Henley (2001, 2005), Benjafield (1996, 2005), Hergenhahn (2001, 2005), and Schultz and Schultz (2000, 2004) represented Morgan acceptably on anthropomorphism. Robinson (1995, pp. 302) wrote that Morgan's canon was "the antidote for Romanes' version of anthropomorphism," but Robinson also implied that Morgan realized that some anthropomorphism was unavoidable.

Even fewer authors listed in the first section of references addressed whether Morgan's canon was anti-anecdotal. Of those who did, Goodwin (1999, 2005), Hergenhahn (2001, 2005), and Schultz and Schultz (2000, 2004) misrepresented it. That Morgan's canon was intended to be anti-anecdotal was implied by Benjafield (1996, 2005), Brennan (2003), and Watson and Evans (1991). Morgan did criticize and offer alternative interpretations for some of Romanes's anecdotes, and later (1932) he expressed greater reservations about using anecdotes than he had at the time he wrote the canon. After Romanes died, it became fashionable to be sarcastically critical of Romanes and his anecdotes. Some of that sarcasm and criticism (Washburn, 1908; Wundt, 1896/1901) has been found to be unjustified, and it seems likely that other examples will be found to be unjustified as well (Thomas & Cynkus, 2001), but that is a topic for another time.

Smith's (1997) and Thorne and Henley's (2001, 2005) coverage of Morgan and the canon may be characterized as being acceptable all-around,

even commendable, although Thorne and Henley (2005) represented Morgan's (1932) later views regarding anecdotes that, as noted, contrasted with his views at the time he wrote the canon.

Descartes's choice of the pineal gland as the site of mind–body interaction

Finger's discovery of a recurring error. Finger (1995), citing Descartes's published works and letters, compiled a list of 10 reasons why Descartes chose the pineal gland as the site of mind–body interaction. Perhaps the most widely cited sources are Articles XXXI and XXXII in Descartes's *The Passions of the Soul,* which include 8 of the 10 that Finger listed and that are often cited in English translations by Eaton (1927) and Dennis (1948; Dennis used Eaton). Finger also discussed an 11th reason that has been falsely attributed to Descartes, namely that the pineal gland was uniquely human. Finger provided indisputable evidence that Descartes knew that nonhuman animals had a pineal gland.

Finger (1995) did not provide an extensive history of the misattribution to Descartes that the pineal gland was uniquely human, but Finger quoted an example from Jaynes (1973) before writing, "A similar statement about the pineal being distinctly human can be found in several texts on the history of psychology" (Finger, 1995, p. 177). However, Finger cited only two examples of such texts, namely Fancher (1990, p. 28) and Hergenhahn (1992, p. 98). Apparently, Fancher (1990) had relied on Jaynes. Fancher (1996, p. 33) eliminated the "uniquely human" reason for Descartes's choosing the pineal gland but continued to cite Jaynes for "provocative discussion of Descartes' personality." Fancher did not cite Finger (1995) in conjunction with eliminating the "uniquely human" reason from the 1996 third edition, but in the *Preface,* Fancher acknowledged Finger's contribution to revisions in the third edition. Meanwhile, Hergenhahn (2005, p. 110) has persisted with the "uniquely human" misattribution to Descartes using language unchanged from that quoted by Finger (1995, p. 177). Viney and King (2003, p. 154) also made the error when they wrote, "Descartes also believed, erroneously, that animals do not have pineal glands." None of the other history of psychology textbooks surveyed made the "uniquely human" error.

A second Descartes-related recurring error. Benjafield (1996; pp. 24–25; 2005, p. 25) quoted a reference by Pastore to explain Descartes's choice of the pineal gland. Pastore's emphasis was on the pineal gland being a singular structure, which was one of Descartes's reasons (Eaton, 1927; Finger, 1995). However, Benjafield's description of the pineal gland, which immediately preceded the quotation from Pastore, was that it is "a singular structure at the base of the brain" (1996, p. 24; 2005, p. 25). "At the base of the brain" is another recurring error involving Benjafield, Leahey (2004, p. 139), and Wertheimer (2000, p. 33).

It is an error because "at the base of the brain" is anatomically incorrect. Additionally, if being "at the base of the brain" was cited as a reason Descartes chose the pineal gland, it would be directly opposed to one of Descartes's stated reasons for choosing the pineal gland. As discussed by Finger (1995, p. 172), reason 4 guiding Descartes's choice was that the pineal gland is "along the midline of the brain, near its center." Other reasons discussed by Descartes showed why such a central location was important to him (Eaton, 1927; Finger, 1995).

It is unclear whether Benjafield and Wertheimer intended "at the base of the brain" to be among Descartes's reasons; that is, they merely may have made the anatomic location error themselves. However, it is clear that Leahey (2004, p. 139) intended that a location at the base of the brain was a reason for Descartes's choice: "Being at the base of the brain, the pineal gland was, Descartes thought, the place where the nerve tubes from the body enter the brain." Leahey cited no source.

A third Descartes pineal-related recurring error. Benjamin's (1997) coverage of Descartes's choice of the pineal gland involved an adaptation of a 1971 paper written by Robert I. Watson. Watson's text was consistent with Descartes's in that it pertained to choice of the pineal gland. However, Watson erroneously added, "His choice of what is now known to be a vestigial organ of no functional significance whatsoever was based entirely on speculative reasoning" (p. 40). As Thorne and Henley (1997, p. 58) noted earlier, it is erroneous to say that the pineal gland is a "vestigial organ of no functional significance whatsoever." Hunt (1993, p. 67), who also cited appropriately some of Descartes's reasons for choosing the pineal gland, also added erroneously, "The pineal gland, vestigial in human beings, has no influence on either afferent or efferent nervous impulses—the mechanical reasons are unimportant."

Descartes pineal-related coverage that appears to be error free. Bolles's (1993), Schultz and Schultz's (2004), Smith's (1997), and Thorne and Henley's (2005) coverage appropriately followed some of Descartes's reasons for choosing the pineal gland, and they made no other errors that this author has found. Perhaps the best coverage of Descartes's reasons for choosing the pineal gland may be seen in Goodwin (2005, pp. 32–33). Goodwin blended quotations from Descartes with accurate, contemporary knowledge of the physiology of the pineal gland, and Goodwin cited Finger (2000) to make the point that Descartes knew that the pineal gland was *not* uniquely human.

How might recurring errors be explained?

Answers to this question at this time can only be speculative. Possibly some of the authors of the errors may be able to recall the source of their errors, but as noted earlier, my attempts to communicate with Thorne

and with Hergenhahn regarding the identical misquotation of Santayana were unsuccessful. To be precise, Thorne replied that he did not remember where he got it (Michael Thorne, e-mail, February 24, 2004), and I was able to communicate with Hergenhahn only through members of his publisher's (Thomson-Wadsworth) staff. Staff member Jennifer Klos wrote, "I am uncertain as to why the mistake was made" (Jennifer Klos, e-mail, March 25, 2004) but it was unclear whether Klos was speaking for Hergenhahn or for herself.

In any case, recurring errors are likely to share causes with primary errors in historical research. Martinez (2005), who refuted reports that Einstein's wife contributed significantly to the development of his theories in physics, constructed a 20-item scale of "Likely Reliability for Information Sources." Item 1 was deemed most reliable, and Item 20 was least reliable. Not stated explicitly but perhaps implied in conjunction with items rated 15–20 would be "using secondary sources." Harris (1979), who focused on misrepresentations in introductory psychology textbooks of Watson and Rayner's (1920) well-known study of conditioned emotions in "Little Albert," and Goodwin (1991), who focused on misrepresentations of Pavlov's conditioning apparatus in general psychology, comparative psychology, and learning textbooks, both pointed to too much reliance on secondary sources, and both cited Samelson's (1974) discussion of how myths and errors arise in historical research. Both Harris and Goodwin provided additional useful discussion and suggestions pertaining to why such errors and misrepresentations might occur.

Ironically, Goodwin (1991) committed a misrepresentation error that bears directly on the kinds of misrepresentations that he criticized. As his Figure 1, Goodwin presented the frequently used but inaccurate drawing of Pavlov's conditioning apparatus, namely a 1907 drawing by G. F. Nicolai in which the method of measuring saliva was the point of interest. As his Figure 3, Goodwin presented what he and others usually deem to be the most accurate drawing of Pavlov's conditioning apparatus, namely that depicted on page 271 of Pavlov (1928). Goodwin cited Morgan (1956) as being among the few examples of authors who had used the Pavlov drawing rather than the inaccurate drawing. Curiously, Goodwin failed to note that the drawing Morgan used was a significantly altered version of Pavlov's drawing. Among lesser alterations, Morgan's version includes a hand bell that is not seen in the Pavlov drawing. Morgan also changed the breed of dog in the apparatus, the hair color of the investigator from dark in Pavlov to light in Morgan, and the clothing of the investigator from a suit in Pavlov to a lab coat in Morgan. Additionally, the investigator's behavior was significantly altered. In Morgan's version the investigator is portrayed as taking notes, whereas in Pavlov's version the investigator's hands are resting palms down on a table. Presumably to justify the changes,

Morgan's figure legend noted that the drawing was "after Pavlov," and Morgan cited Pavlov (1928).

Although I have no evidence in most cases and only inferential evidence in a few cases noted earlier, it seems likely that the use of erroneous secondary sources contributed significantly to the recurring errors discussed here. Regarding the misrepresentations of Morgan's canon, I am inclined to agree with Wozniak (1993), who wrote, "It seems likely that Boring [Boring, E. G. (1929). *A history of experimental psychology.* NY: Century] was one of the more influential culprits" (p. ix).

Boring (1950) might have played a role also in the misrepresentation of Broca's so-called discovery of the speech center, as Boring's account omits any reference to Auburtin. However, Boring's account of Descartes and the pineal gland appears to be error free, and Boring did not address Pavlov's mugging or quote Santayana. In any case, it seems useless to speculate further regarding which, if any, secondary sources might have led to the recurring errors discussed here.

Obviously, differences in scholarly expertise contributed to the difference between how most historians of neuroscience accurately represented the discovery of the speech center and how most of the historians of psychology cited here did not. Relative expertise among psychological historians probably played some role in the less accurate versus the more accurate accounts of why Descartes chose the pineal gland and regarding the discovery of the speech center. For example, having a strong background in physiological psychology might reduce errors associated with histories of topics that bear on anatomy or physiology. Presumably most historians have special expertise within and beyond the scholarly study of history of psychology per se, and that may bear on how well they cover some topics.

Admittedly, the Santayana misquotation is trivial compared with the other errors discussed. It is an error only because it was a misquotation in quotation marks. Had it been presented as a paraphrase, there would have been no error. Keyes (1992) wrote an interesting book concerning misquotations and why they occur. Consistent with Keyes, one might suggest that for their purposes, namely to support the value of studying history, Hergenhahn's and Thorne and Henley's version of the Santayana quotation was "even better than the original" (Darryl Bruce, e-mail, July 13, 2004). Nevertheless, when one uses quotation marks, it would be prudent to be confident of and faithful to the source.

Conclusions

Writing a history of psychology textbook is a formidable undertaking, and those willing to do so deserve appreciation and respect. Surely, no author wants to commit errors or be subjected to the kind of criticism that an article such as this one implies. Nevertheless, some errors are difficult

to understand, especially in the case of Morgan's canon, when so many efforts have been made by so many scholars beginning as early as 1928 to correct its misrepresentation.

Because I was three for three in identifying recurring errors merely because I had done research on the topics of Pavlov's mugging, Broca's so-called discovery of the speech center, and Morgan's canon and was curious to see how the textbooks were representing the information suggests that a too high percentage of recurring errors might be detected by other researchers under similar circumstances. Given the breadth of the history of psychology, it may be unreasonable to expect that an error-free general history of psychology text can be written by a single author or a pair of authors, as was the case with the textbooks surveyed here (except Benjamin, 1997, which had its own problems). Perhaps it may be best to have specialists prepare or at least review each chapter in a general history of psychology textbook before publication.

Finally, many or perhaps most who teach the history of psychology do not do research in the history of psychology. Errors such as those revealed in the present work and in the work of others, including Harris (1979), Goodwin (1991), Martinez (2005), and Samelson (1974), point to the need to give careful consideration to the adoption of any textbook, especially when it addresses topics in the history of psychology. Less experienced teachers may want to consult more experienced teachers of history of psychology and may want, when possible, to read critical reviews of the textbooks they are considering.

Notes

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