

Author's Response

Sir,

There were two persons who authored the article Matte has commented on. The second author (JP) is out of the country and, while I am confident he would agree with the tenor of my response, I am obliged to point out that these words are mine alone. I also must note for the record that in footnote 7 of the critique I am referred to as a "graduate and former staff member of the Reid College in Chicago" (1). I am not and have never claimed to be a graduate of the Reid College. I am not now nor have I ever been a staff member of that "college." I was, however, employed in various positions not related to that "college" at John E. Reid and Associates, Chicago.

The person who wrote the critique at hand is known as an advocate for a specific polygraph testing procedure, the eponymously named [Cleve] Backster Zone Comparison Technique (hereinafter the BZCT). This method is merely one of many variations in the most frequently administered polygraph testing procedure in the United States, what is generically known as the Comparison ("Control") Question Technique (CQT). While it is common to find that variations in the CQT are identified by the name of a person who made an alteration in the testing protocol, in principle, they all function in roughly the same way. That is described in the article that Matte has challenged, so I won't bother to reiterate that information here (2). Also described in the article is the rationale for what appears to be at the core of the Matte critique, the difference between so-called "exclusive" and "nonexclusive" comparison questions. Because the rationale for these two question types is also described in the original article, I won't restate it here.

In his critique, Matte spends a bit over six pages of text giving the reader background information, most of it focused on the BZCT and the reasons for the way it is structured and administered. He concludes that section by stating that the test structure used in our study was a "hybrid" rather than the BZCT and was therefore not consistent with the method he prefers. Matte then implies that our findings were as reported because of our testing structure and because an "Either-Or" rule used in the BZCT was not implemented. This rule has never been reported to be of value in any peer-reviewed scientific journal. In the paper on that topic to which Matte refers, the only published assessment available, the findings showed something quite different from what he reports (3). In that paper, the BZCT testing that was carried out using the "Either-Or" rule did not produce an outcome that was significantly different from what was obtained by a different testing method that did not employ that rule. Matte's statement about what was reported in that paper is misleading.

The information in those pages might be an accurate description of the BZCT, but it is all irrelevant. In our study, we chose not to employ that testing procedure, for good reasons. First, there isn't a single empirical study in the scientific literature in which that procedure has been used as Matte described it. There is one report in which something similar was used; more on that later. Second, there are scientifically acceptable empirical studies in which the specific distinguishing features of that procedure have been tested. So far, all of that research has shown that those features, for example, "Either-Or" rule and "symptomatic" questions, do not have their intended effect. On the other hand, the procedures we used in

our study have been reported in the published research and also are widely applied in field settings.

Matte then goes on for another five pages in which he offers 12 specific points of objection, all of these said to be "violations" of the rules pertaining to the procedure he prefers. There is no reference to any scientific report or any citation of an empirically demonstrated finding. Each of Matte's 12 points is merely a statement of opinion without any theoretical or empirical grounding. The following is an example worthy of mention: In point no. 12 Matte states: "It is well known and documented⁹ that in spite of the claim that both relevant and control questions are reviewed with the examinee between charts, the emphasis is clearly on the nonexclusive control questions" (1). I single out this sentence because it has no relevance to what was reported in our paper. Like the other points made in the Matte critique, it is simply a personal comment, in this case, made up out of thin air and without any foundation in regard to our study.

Matte must have missed one of our hypotheses. We structured our research so as to include two general testing approaches, the MGQT and the ZoC. (MGQT and ZoC are merely abbreviations for somewhat different variations in the generic procedure, CQT.) As we explained, this was because of the discrepancy between the earlier Podlesny and Raskin (4) results and those reported by Horvath (5). Our interest was in whether or not the ZoC, the general procedure for which the exclusive CQ was developed, would produce a more favorable outcome than would the MGQT, which, generally, is known to make use of the nonexclusive type of CQ. We found no overall difference between those two testing approaches, but there was a statistically significant difference in both approaches depending on the type of CQ that was used. The exclusive CQ was less effective than the nonexclusive, irrespective of the testing approach, suggesting, Matte's criticism notwithstanding, that the exclusive CQ simply does not function as its proponents maintain. Matte seems to believe that our findings were as reported because we did not use his preferred version of testing. Truth be told, in the single report in which that method was reportedly used, it yielded the lowest overall accuracy among the methods included and, according to the authors, "the Backster system of [scoring] produced a larger number of inconclusive outcomes [than other scoring methods] in all groups" (6, footnote 1, p. 181).

There are over 25 training schools accredited by the American Polygraph Association (7), most in the United States and 12 in other countries. Only one of them—surprise, it's the training school administered by the same person for whom the BZCT described by Matte is eponymously named—focuses primarily on the approach Matte described. Even though Matte and a few others have advanced it as the "preferred" approach for a number of years, the only empirical assessment of it found in any peer-reviewed, scientific publication is what has already been mentioned. However, specific features of that approach have been subjected to scientific review. For example, the use of "symptomatic" questions, a feature that Matte emphasizes, has been researched; the results were not supportive. The researchers pointed out that the "Use of outside-issue [symptomatic] questions does not affect the validity of the CQT" (8, p. 72). Moreover, they also reported that "Concerns about other undiscovered crimes overwhelming relatively weaker relevant questions [which is the basis for the use of symptomatic questions in the BZCT] appear to be groundless" (8, p. 73).

Of course, there is no way to know for certain what would have occurred had we done things differently in our study. However, science is on our side. Our study was the fourth to appear in the literature on the difference in outcomes between testing involving exclusive and nonexclusive comparison questions; one of these was carried out in the field, and three were carried out under more controlled, laboratory conditions. All of these were referenced in our paper. In one of these studies, there was a slight advantage to the exclusive type of question, but this was in respect to persons who were truthful. In that study and in the other three studies, the use of exclusive comparison questions did not show the intended effect. Those questions did not reduce errors on deceptive persons (false negatives) nor did they reduce the number of inconclusive outcomes. Moreover, we note that our paper was published in 2008, over 3 years ago. The data were presented at professional conferences prior to that date. That is more than sufficient time for those who chose to do so to carry out a constructive replication of our study. Neither Matte nor anyone else has done so. We are prepared to revise our views should additional scientifically sound research necessitate that. Until then, however, neither Matte's personal opinion nor his preference for unscientific "theory" and principles and untested practices persuades us that there is any merit in his position. The score is now 4-0. In our view, the position advanced by Matte—as well as others who subscribe to the rationale he holds to—is demonstrably wrong. It is better, in our view, to find a more satisfactory explanation for the CQT and how "comparison questions" function than to debate arguments unguided by sound scientific theory and ungrounded in empirical findings.

The one point made in the Matte critique that is of scientific interest is that laboratory-based research may yield some results that differ from what is seen in real-life testing. This is a concern that has been commented on regularly in the literature. Fortunately, it is also an issue that has been subjected to scientific scrutiny. The research that has been carried out shows, contrary to what Matte states, that while there are differences in field versus laboratory findings, in general, the most dependable difference is that laboratory physiological response data in "lie detection" studies tend to be more subdued; that is, the physiological responses in that environment are less dramatic than what is seen in the field (9). This seems reasonable in that in the laboratory, examinees have less of a reason to be concerned about being detected in a "lie" or, on the other hand, about being wrongfully identified as a "liar." There is another side to this issue, though. There are some topics that are best addressed, and maybe can only be addressed well, in a laboratory environment. Consider, for example, the issue of countermeasures. It can be seen that those who engage in efforts to defeat polygraph testing in real-world testing would be difficult if not impossible to subject to research. In the laboratory, however, this would not be the case. Moreover, because research in "lie detection" usually requires a reasonable measure of "ground truth" (i.e., a criterion that can be used to confirm that an examinee "lied") that is relatively easy to enforce in a laboratory environment. This is true, of course, even if one is interested only in the difference produced, if any, by the type of comparison question that is employed, as in our study. In the field, it is much more difficult to establish ground truth and that is one reason why field research is much less likely to be found in the literature and when found, more likely to be challenged on methodological grounds than research carried out in a laboratory environment.

In closing, a general comment is in order. Research on polygraph testing needs to be seen in the broader context of what is

now referred to as credibility assessment (CA). Polygraph instruments are only one form of technology used in CA. Today, active research is under way to assess whether other technologies might supplement or replace the traditional polygraph instrument. We now can find research on brain-imaging techniques (e.g., fMRI), brain-wave activity (e.g., P300), laser Doppler vibrometry, eye tracking, and thermal imaging, among others. In addition, testing processes, that is, the methodologies used in the presentation of stimuli (test questions) and the way testing protocols are presented, are very likely to differ as a function of the technology at hand. Research is under way to examine these issues and while what has been shown so far is promising, much more needs to be done (10). I mention these technologies, by the way, not because of the special merits of these particular methods but to lead the reader to consider this point: more and better scientifically acceptable research in the CA field is necessary. In spite of the limitations in the current research base, as pointed out by the National Research Council (NRC) (11), commentary such as that appearing in the critique to which I've responded, mere speculative "theorizing," personal preference, and opinion without any scientific support, reveals nothing of interest except for the extent to which practitioners who might agree with the comments made by Matte, must be encouraged to move in a different direction than is now evident. The so-called psychological structure and theoretical concept that Matte says underlies the BZCT and the claim that that approach is capable of accounting for the many variables that may influence polygraph testing in the field is, to use a colloquial term, mere psychobabble. There is nothing in any scientific literature that comports with what is set forth by Matte.

Polygraph testing and other CA approaches are historically wedded to the forensic sciences and, as a practical matter, their purposes, in the main, involve forensic issues. The current effort to enhance the scientific underpinnings of the various forensic techniques generally, as recommended by the NRC in its 2009 report (12), must also extend to CA. Practitioners in that area must strive for all of those things brought so forcefully and clearly to the forefront by the NRC; practitioner credentialing, school accreditation, quality assurance, enforceable ethical codes, certification, and more and better scientific support are as important to polygraph testing specifically and CA generally as they are to the forensic techniques specifically commented on by the NRC.

There is plenty of room for disagreement about certain scientific findings in polygraph testing, other methods of CA, and in other areas of the forensic sciences. But there ought to be no room for arguments that don't even reach the threshold science requires. Matte's critique falls far short in that regard. Nevertheless, in spite of the reluctance of some in the practitioner community to embrace scientifically supportable practices, the past three decades or so have seen real advancements. Scientific attention, albeit at an uneven and less than desirable pace, has enhanced our understanding of polygraph testing, other approaches to CA, and the forensic sciences generally. It is hoped that trend will strengthen in the future. That, at the least, will diminish the appeal of unfounded, imaginary, and fanciful ideas passed off as reality on those who might otherwise have been influenced.

References

1. Matte JA. Commentary On: Horvath F, Palmatier JJ. Effect of two types of control questions and two question formats on the outcomes of polygraph examinations. *J Forensic Sci* 2008;53(4):889-99. *J Forensic Sci* 2011: In Press. DOI: 10.1111/j.1556-4029.2011.01936.x

2. Horvath F, Palmatier J. Effect of two types of control questions and two question formats on the outcomes of polygraph examinations. *J Forensic Sci* 2008;53(4):889–99.
3. Meiron E, Krapohl D, Ashkenazi T. An assessment of the Backster “either-or” rule in polygraph scoring. *Polygraph* 2008;37(4):240–55.
4. Podlesny JA, Raskin DC. Effectiveness of techniques and physiological measures in the detection of deception. *Psychophysiology* 1978;15(4):344–59.
5. Horvath F. The utility of control questions and the effects of two control question types in field polygraph techniques. *J Police Sci Adm* 1988;16(3):198–209.
6. Honts C, Hodes R, Raskin D. Effects of physical countermeasures on the physiological detection of deception. *J Appl Psychol* 1985;70(1):177–87.
7. American Polygraph Association. Polygraph schools accredited by the APA. *APA Mag* 2011;44(3):58–9.
8. Honts C, Amato S, Gordon A. Effects of outside-issues on the comparison question test. *J Gen Psychol* 2004;131(1):53–74.
9. Pollina DA, Dollins AB, Senter SM, Krapohl DJ, Ryan AH. Comparison of polygraph data obtained from individuals involved in mock crimes and actual criminal investigations. *J Appl Psychol* 2004;89(6):1099–105.
10. Pollina DA, Horvath F, Denver J, Dollins A, Brown T. Development of technologies and test formats for credibility assessment. In: Columbus A, editor. *Advances in psychology research*. New York, NY: Nova Science Publishers, 2008;11–36.
11. National Research Council. *The polygraph and lie detection*. Washington, DC: Committee to Review the Scientific Evidence on the Polygraph, Division of Behavioral and Social Sciences and Education, National Academies Press, 2003.
12. National Research Council. *Strengthening forensic sciences in the United States: a path forward*. Washington, DC: Committee on Identifying the Needs of the Forensic Science Community, National Academies Press, 2009.

Frank Horvath,¹ Ph.D.

¹Professor Emeritus, Michigan State University,
East Lansing, MI.

E-mail: horvathf@bellsouth.net