

Author's Response

Sir:

Mr. Hicks has almost nothing new to say about the Saks and VanderHaar article, which he purports to comment on. Instead, he devotes most of his letter to rehashing other, older issues that had been raised at far greater length elsewhere and which one would have thought had been soundly put to rest quite some time ago. In addition, Hicks raises yet other issues which can be resolved only by future research (rather than by Hicks's unsupported conclusory assertions). I will, however, respond to several of his points, principally to refer readers to more informed and more informative sources.

1. Risinger et al. and the research record on claims of handwriting expertise.

Readers who are interested in learning about the full range of challenges (1,2) to Risinger et al. (3) and the responses to the challenges (4,5) are invited to spend some thoughtful time with those exchanges. Readers who do so will find that Hicks's claim that the paper is "riddled with inaccuracies" is a very wild exaggeration indeed. (Consider what Hicks provides readers by way of support for his reckless claim: three talks/papers that are unpublished, unseen (by us), unavailable (to us and no doubt to most readers of *JFS*), old, and unanswered. Hicks cites none of the published criticisms or rejoinders (1–4)).

Here, I will offer only a few specifics. Hicks's complaint that, "After citing several tests, two of them very old (1939 and 1975), the authors concluded that "...no available evidence demonstrated the existence of handwriting identification expertise" puts a misleading spin on Risinger et al., making it appear that the authors jumped to a premature conclusion after reading only a little of the available research. In reality, after reviewing in some detail the intellectual history of the handwriting field and in considerable detail the legal history of asserted handwriting identification expertise, Risinger et al. report painstaking efforts to locate any empirical research whatsoever that tested the extreme claims of expertise commonly made by handwriting examiners. We found *nothing* that directly tested those claims, and some data (FSF proficiency studies), which suggested that the claims were overblown.

That little or no empirical research existed on the fundamental question faced by courts is not a failing of Risinger et al.—that article merely documented the fact. Several commentators, including some relied on by Hicks, affirmed our conclusion. (Kam et al. write of "an acute lack of empirical evidence on the proficiency of document examiners" and say that "it is widely agreed that testing of professional document examiners and acquiring data on their abilities ... are necessary." Galbraith et al. said there was an "admittedly sparse history of carefully controlled empirical studies. ... there certainly has been a shortage of studies. ...") Moenssens agreed that "Document examiners have not done the kind of empirical research that could have and should have been done. ... On that the critics are absolutely correct."). Indeed, Hicks himself implicitly concedes Risinger et al.'s conclusion about the paucity of research when he refers to the article's "very beneficial effect of stimulating more research."

To those document examiners who insisted that innumerable such studies existed but that Risinger et al. failed to find them, we suggested a simple way to prove it: come forward with the studies or their citations. Thirteen years passed before the document examination community was able to offer up one such study: an

article in German published in a European journal (6). We readily concede the existence of a study we had not originally found. Ironically, however, the study's findings further weaken the field's claims of expertise.

2. Research post Risinger et al.

According to Hicks, the research stimulated by Risinger et al. represented "extensive testing... to investigate the claimed abilities of FDE's" and that "[t]hese research projects have determined that the basic tenets of handwriting identification... have been found to be valid. ..."

Of the eight studies Hicks cites, one was a small pilot study (7), one was unpublished (and I have never seen it) (8), one tested no discernible skill that handwriting examiners had claimed to possess (9), one did not test handwriting examiners at all or anything about the tenets of handwriting examination (10), another did not test handwriting examiners and failed to confirm the one tenet of handwriting identification that it sought to test (11,12), and another (13,14) was a reanalysis of data from one of the other studies (9). Moreover, several of these studies suffer from important research design defects, which render their findings uncertain. This is hardly "extensive" by any definition and provides sound answers to nothing.

Of these studies (including the completely irrelevant ones), Hicks asserts: "All of these findings are contrary to the assertions of Saks, Denbeaux, and Risinger." Hicks's contention cannot possibly be true, if only because Risinger et al. made no assertions about how such studies would turn out when and if they were conducted. On the truth or falsity of handwriting examiner claims, Risinger et al. were entirely agnostic. That remains our posture. We review the limited but slowly growing research testing the handwriting field's claims, and we explicate both the strengths and the weaknesses of the emerging research.

3. The most supportive ground for admission

Hicks strenuously objects to Saks and VanderHaar's statement that, "At present, the most supportive ground for admission of handwriting identification expert opinion testimony would seem to be its 'general acceptance. ...'". In the end, however, Hicks himself is unable to avoid the same conclusion, when he states that "this factor [general acceptance] was clearly met many decades ago." How could the other *Daubert* factors—informed only recently by a very few empirical studies, of doubtful methodological quality, addressing only a small number of handwriting examiner claims, and found wanting by numerous courts—possibly be as supportive as general acceptance? The one thing handwriting examiners could always count on is that they are willing to say that they believe in themselves. Why Hicks would even want to take issue with this veritable truism is a puzzle.

Readers interested in the debate over *Daubert's* various scientific factors as they pertain to the admissibility of asserted handwriting identification expertise, and how the courts have responded to that debate, can read a regularly updated review of the case law (14).

4. The Saks and VanderHaar study

Finally, Hicks has a few things to say about the Saks and VanderHaar study.

First, Hicks repeats what we wrote in our earlier response (15), namely, that the document examiner sampling frame was somewhat less homogeneous than intended by our design, but then he ignores the additional information we provided, which suggests that our document examiner respondents were nevertheless well qualified examiners of many years, experience.

Second, Hicks revisits the question of “problems” with the study. He offers no new criticisms of the study; indeed, he does not offer any at all. Instead, he suggests a bizarre equation between the document examiners who participated in the study and an alleged 39 examiners who, “according to Kelly and Carney,” wrote to us “pointing out problems with” the study. Hicks offers nothing to further illuminate the research or its methodological strengths and weaknesses; his purpose seems to be limited to defaming the individuals conducting the research. (Is it not strange that Kelly and Carney (16) claim knowledge of the exact number of messages received by us researchers as well as the contents of those messages? We take that as confirmation of an orchestrated effort to boycott the research.) What Kelly and Carney presumably know, but Hicks does not, is that only a few of those messages offered substantive criticisms of the study; the rest either said, in effect, “me too” or simply declined to participate. What is important about these 39 (or whatever number) messages is the content of the criticism. If one sound criticism were made, that would be meaningful. If 3900 unsound criticisms were made (even if they were independent rather than being echoes of each other), they would count for nothing.

The actual substance of the criticisms, which Hicks shows no interest in, were, as noted in our earlier response (15), thoroughly aired by reviewers, responded to, carefully weighed within *JFS*'s editorial process, most of them judged to be without merit, and one or two valuable ones heeded and taken into account in reanalyzing data and revising the manuscript. Those criticisms most important to evaluating the study were discussed within the article itself. (For example, one major criticism was in reality merely a suggestion of another way to design the study, not the “correct” way to approach such research. That the method used was entirely adequate (indeed, preferable to the alternative method suggested) was agreed with by reviewers and by the editor. Both approaches, and why the one employed was chosen, were all explained in the published article).

Third, Hicks complains that the word “atomized” is not found in the document examination literature. He never states what problem he thinks this might create for the research. In drafting propositions for the study, we aimed to crystallize concepts, not to mimic terms and phrases. Sometimes a perfectly good English word efficiently captures the meaning of a longer phrase. We doubt that any examiners would not know what “atomized” means, but any who did not know could readily infer it from the context (since “atomized elements of the writing” was juxtaposed against “the writing as a whole”), or they could consult a dictionary, find that it means “to treat as made up of many discrete units,” and should have instantly recognized the concept as one familiar to their field. Does Hicks think that document examiners have a limited grasp of the English language, cannot figure out contextual meanings, do not own dictionaries, or cannot think beyond rote training?

Finally, Hicks complains that the study's Proposition 10 (“By looking at a person's writing on paper . . . it is possible to accurately infer the muscle movements that created the writing”) was “a gross distortion of what Osborn says in his book.” Hicks invites readers to consult our source, Osborn's 1910 book (17) and see for themselves. Any reader who does so will find that Osborn unquestionably embraces the concept, beginning at page 106 and elaborating for half a dozen pages or more. A small sampling of Osborn's discussion:

Writing is in reality the track or visible record of a movement and necessarily is greatly changed when produced in a

different manner, and a thorough study of any writing must necessarily give attention to this fundamental question. . .

Free natural writing is the almost unconscious visible expression of firmly established muscular habits based on fixed mental impressions of certain forms or outlines. These muscular habits, as well as the mental patterns, differ in a marked manner in different individuals, and this variation radically affects the visible result. . . The principal so-called movements in writing are described as the finger movement, the hand movement, the forearm movement the whole arm movement and their various combinations. . . (p. 106)

In the next two pages Osborn describes the distinguishing characteristics of writing resulting dominantly from each kind of movement.

Having to reply to repetitive nonsense and ad hominem attacks does become tiresome. On the other hand, the nature and quality of the attacks carry lessons of their own.

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