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LETTER TO THE EDITOR

Authors' Response to Speth et al Commentary on

See Original Dror et al Article [here](#)

See JFS Editor-in-Chief Preface [here](#)

See Speth et al Commentary on [here](#)

Editor,

Speth et al. express serious criticisms of our study about cognitive bias in manner of death decisions based on our data of over 1,000 death certificates and experimental data from 133 participants. As explicated below, these criticisms are incorrect and unfounded.

Regarding the death certificate data, Speth et al. claim that "more than 75% of the death certificates reviewed" were certified by coroners.

1. This claim is factually false: Actually, *less than 20%* of the certificates in the dataset were certified by coroners.
2. Speth et al.'s claim is troubling not only because it is inaccurate, but also because the claim misses the point. We are interested in bias in manner of death decisions, *regardless of who makes them*. Indeed, we specifically make this point in our paper that "depending on the case and jurisdiction, the manner of death may be determined by medical examiners, forensic pathologists, or coroners who frequently rely on the opinion of the forensic pathologist in determining the manner of death—in this paper we use them interchangeably" [1].
3. Speth et al.'s criticisms, which are similar to those expressed in other letters, seem to concede that bias affects manner of death decisions, but that board-certified forensic pathologists are somehow immune from this effect. According to them, bias affects "others," such as coroners (who are legally qualified to make manner of death determinations). This demonstrates and reflects the hallmark of the *bias blind spot*, which is well-documented [2] and has also been shown specifically in the forensic sciences [3].
4. Speth et al. further fall into the *fallacy of expert immunity* [4], suggesting that expertise, professional training, certification, or experience protects board-certified forensic pathologists from bias. Expertise has no protection from bias [5], and forensic experts have been shown to be as susceptible to bias as novices [6].
5. Even if we entertain the Letter's false claim that the biases revealed in our death certificate data [1] are attributable to coroners, examination of the data shows that to be incorrect. If we remove all the coroners' death certificate data, the bias still remains significant, $\chi^2(1) = 3.74$, $p = 0.05$ (we also refer readers to our reply to another Letter where we analyze the death certificates by cause of death, age, sex, and other factors).

The Letter also claims that "it is unclear how [we] classified Black vs. White" and "there is no way [we] could determine" if any Hispanic

children were included. These claims are likewise all false. First, we did not determine the child's race; each child's race was stated on their death certificate. Second, the death certificates separately indicated the child's race (e.g., White, Black, Asian, etc.) and their ethnicity (i.e., Hispanic or not). We excluded all Hispanic children from our analysis, including where the child's race was identified as White or Black.

Regarding the experimental dataset of 133 participants [1], Speth et al. make a series of additional claims based upon Kelley et al.'s "Good Practice in the Conduct and Reporting of Survey Research" [7]. However, their Letter appears to misapprehend and repeatedly misapply Kelley et al.'s paper to our study:

1. They first claim that "the survey had not undergone a robust process of development." However, the Kelley et al. requirement applies when "there is no attempt to control conditions or manipulate variables; surveys do not allocate participants into groups or vary the treatment they receive" (p. 261 [7]). However, in our study [1] we did all of these things, including manipulating variables and randomly allocating participants into groups. Surveys that do not have such a design—and therefore cannot, for example, compare between groups—require a different process of development. This does not apply to our study.
2. Speth et al. also criticize our experiment as not "tested on a pilot sample [from] the target population." However, again, according to the reference upon which the Letter relies, Kelley et al. [7] clearly state that piloting serves "to identify whether respondents understand the questions and instructions, and whether the meaning of questions is the same for all respondents" (p. 263 [7]). Our experiment analyzed a simple question, which read (verbatim): "What manner of death would you choose?" and gave five response options (i.e., natural, accident, homicide, suicide, and undetermined). Whether participants in our experiment had difficulty understanding this simple question was not an issue, nor did the question's wording (which was identical between conditions) suggest a particular response. Again, this issue raised in the Letter does not apply to our study.
3. Additionally, Speth et al. express a serious concern about our response rate, which they dub "perilously low." However, the Kelley et al. paper cited and relied upon by the Letter explicitly states that "it is unwise to define a level above which a response rate is acceptable, as this depends on many local factors" (p. 264 [7]).

The fact of the matter is that the 18.7% response rate in our study is strikingly consistent with other forensic pathology studies published in *JFS*. For example, one study [8] states that they sent "approximately 1,115 emails... to the NAME



membership” and received “210 surveys completed”—a response rate of 18.8%. Similarly, another study [9] states that they sent “a total of 1,098... email invitations to all members of the National Association of Medical Examiners (NAME) with email addresses on the roster” and received 192 responses—a response rate of 17.5%. In both cases, the authors themselves stated that “this response rate is characteristic of NAME surveys.” These studies and others—which obtained very comparable response rates to our study—have not faced criticism or calls for retraction. The principal difference between those papers and ours is that ours is about bias. This criticism therefore says far more about Speth et al. than it does about our paper.

Relatedly, Speth et al. also question why “no effort was made to find out why” many individuals chose not to participate in our study. This would not only be impossible (insofar as our participants were anonymous), but moreover, we cannot ethically compel a response from anyone who declined to participate. And, again, this criticism is selectively applied to our study, while never applied or raised about other studies that had comparable numbers of individuals who chose not to participate in the study (e.g., [8,9]).

Furthermore, Speth et al. repeatedly note that we did not attempt to find out why participants responded as they did (i.e., “undetermined,” “homicide,” or “accident”). Our data clearly answer this question: Each participant group read the information that was identical apart from two pieces of irrelevant, non-medical information. Hence, the differences between the two groups’ decisions are explained by the different contextual information they received. Simply put, the answer to the question “why did participants respond as they did?” is because of irrelevant, non-medical information, which is exactly our point.

Speth et al. also claim that “the survey lacked sufficient details and depth to allow respondents to reach a competent decision.” If this were true, we would have had 100% of the participants decide that the manner of death was “undetermined”—but the actual data show that only 59% decided undetermined. The data show that 41% of participants clearly felt that they *could* reach a conclusive decision—a conclusive biased decision due to the irrelevant non-medical information.

Based on all of these unfounded and factually inaccurate criticisms, Speth et al. label our paper an “abject failure” and call for its retraction. This is unfortunate, as our paper represents the beginning of long-overdue research studies and discussions about the issue of bias in forensic pathology decisions. A careful reading of our paper [1] will reveal that we are transparent about the limitations of our data, and we explicitly call for more research to further elucidate the sources of bias in these decisions, as well as suggest ways to minimize them.

It is unfortunate that instead of confronting the issue of bias in forensic pathology, this Letter sets out to discredit the first paper on this issue in the hopes of making it and the issue of bias disappear. Indeed, we have now received many Letters and complaints that seek to deflect attention from the “elephant in the room” by levying unfounded criticisms. Attacking the paper, its authors, and calling for

retraction will not move the domain forward to address the issue of bias. On the contrary, it sends a clear message of denial, refusal to discuss the actual issue, and discouragement of research in this area.

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