

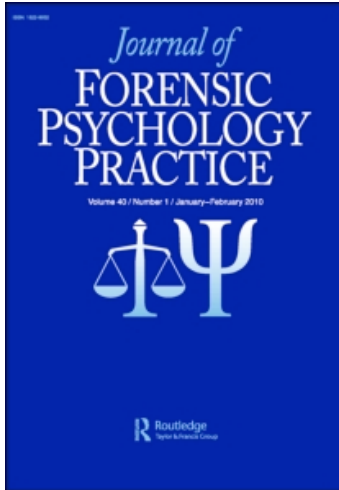
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### The Importance of Knowledge Cumulation and the Search for Hidden Agendas: A Reply to Kocsis, Middeldorp, and Karpin (2008)

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## COMMENTARY: PART 1

# The Importance of Knowledge Cumulation and the Search for Hidden Agendas: A Reply to Kocsis, Middeldorp, and Karpin (2008)

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*In a recent narrative review and meta-analysis of the criminal profiling (CP) literature, Snook, Eastwood, Gendreau, Goggin, and Cullen (2007) found that self-labeled profilers and/or self-labeled profilers/experienced detectives did not decisively outperform lay individuals in their ability to produce accurate profiles. Combined with their finding that the CP literature is based largely on commonsense rationales, they cautioned police officers about using CP in investigations. In a recent issue of this journal, Kocsis, Middeldorp, and Karpin (2008) (KMK) challenged some of the methodological decisions of Snook et al. and questioned their interpretation of their meta-analytic findings. In addition, KMK suggested that the work by Snook et al., and that of Bennell, Jones, Taylor, and Snook (2006) is based not on a desire to move profiling research forward but on a hidden agenda to promote an investigative psychology (IP) approach to profiling. In response, we argue that KMK have failed to consider important issues of knowledge cumulation when criticizing the meta-analysis by Snook et al. and that KMK's views with respect to IP have no basis in our previous research. Instead of trying to find a nonexistent hidden agenda in*

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*our work, we suggest that efforts would be better directed toward conducting high-quality CP research.*

*KEYWORDS* criminal profiling, investigative psychology, meta-analysis, predictive validity, psychological profiling

In a recent article entitled *Taking stock of criminal profiling: A narrative review and meta-analysis*, Snook, Eastwood, Gendreau, Goggin, and Cullen (2007) synthesized the extant literature on criminal profiling (CP). Their narrative review revealed that the CP literature is based largely on common-sense rationales (e.g., anecdotes) and is, for the most part, not grounded in science. The two meta-analytic studies by Snook et al. showed that self-labeled profilers and/or self-labeled profilers/experienced detectives did not decisively outperform lay individuals in their ability to produce accurate profiles. Given the pseudoscientific nature of CP and the relatively low level of predictive accuracy achieved by professional profilers, Snook and his colleagues urged police officers to be cautious about using profilers in their investigations until a number of high-quality studies supported their usefulness (see Snook, Cullen, Bennell, Taylor, & Gendreau, 2008 for further concerns about CP).

In a recent issue of this journal, Kocsis, Middeldorp, and Karpin (2008; henceforth referred to as KMK) raised a number of problems with the Snook et al. 2007 study. In this article, we address the concerns raised about some methodological decisions made by Snook et al. and about how Snook et al. interpreted their findings. We also respond to the accusation by KMK and Kocsis (2006a) that research by Snook et al. and the work by Bennell, Jones, Taylor, and Snook (2006) are based not on a desire to move profiling research forward but on a hidden agenda to promote an investigative psychology (IP) approach to profiling.

## KNOWLEDGE CUMULATION ISSUES

### Defining Profiling

KMK criticized Snook et al. (2007) for including experienced investigators in the experimental group (i.e., the profiler group) for their first meta-analysis. KMK argued that it was invalid to combine experienced investigators with “profilers” because investigative experience is not synonymous with being a profiler. We agree with that criticism insofar as it highlights one of the major difficulties in the field: agreeing upon an operational definition of “profiler.” As stated by Snook et al., there is no consensus definition of who is or is not a profiler. In many jurisdictions, anyone can be considered a profiler if

he or she provides predictions to a police agency regarding an unknown offender based on crime scene evidence. As such, deciding who should be considered profilers in a meta-analysis of the type reported by Snook et al. is an arbitrary task.

Experienced police investigators were included in the profiling group in the first Snook et al. meta-analysis because they often provide profiles to law enforcement agencies (e.g., Girod, 2004; Jackson & Bekerian, 1997), and investigative experience is one of the criteria that is argued by some to be important for profiling (e.g., Hazelwood, Ressler, Depue, & Douglas, 1995). However, in light of the aforementioned definitional issues, Snook et al. conducted a second meta-analysis that included only self-labeled profilers in the experimental group. Given that the results from both analyses are provided in Snook et al., we are unsure why KMK view this as a contentious issue. If readers believe that the inclusion of experienced investigators into a profiler group is invalid, they can simply attend to the results of the second Snook et al. meta-analysis.

### Analyzing Data From Methodologically Flawed Studies

KMK contend that Snook (Snook et al., 2007) contradicted himself by including data in a study that he argued contained methodological limitations (see Bennell et al., 2006). KMK further argued that Snook must believe that Kocsis' research is free from limitations because he included Kocsis's data in his meta-analyses. In our view, these are strange arguments. KMK suggests that any researcher who conducts a meta-analysis that includes low-quality studies must believe that those studies are methodologically sound. However, what of the many meta-analytic studies that include "study quality" as a moderator variable, which is a commonly accepted practice (Lipsey & Wilson, 2001)? Are KMK really suggesting that all researchers who conduct a meta-analysis contradict themselves by coding a study as "low quality" and then including it in their analysis? This is an odd position to take because some low-quality studies (as determined by design quality) may offer much of the same information as other studies that use better quality designs (Carlson & Schmidt, 1999).

To be clear, the intention of Snook et al. (2007) was to include all available research (regardless of quality) that has examined the accuracy of criminal profilers in mock profiling exercises. As for any meta-analysis wherein studies of varying quality are examined, this in no way suggests that Snook accepted "the methodological integrity of the original studies that produced the data" (KMK, 2008, p. 254). Snook maintains that Kocsis' studies contain a range of methodological limitations and are low in ecological validity, and Snook et al. were clear that the results of their meta-analyses should be treated as tentative (especially as studies by Kocsis contributed the

majority of effect sizes). When higher-quality studies of profiler accuracy are conducted, further meta-analytic research can be done with “study quality” coded as a moderator variable, thereby determining whether high-quality studies result in similar findings.

## Data Inclusion

KMK stated that the Snook et al. study does not represent a comprehensive analysis because it “omits key data such as the additional original data pertaining to profilers in Kocsis” (2003, p. 251). There are two issues here that need to be addressed. First, and most important, the data that Snook et al. omitted were inaccessible. That is, it was not possible to separate those “additional” data from the originally published data (see Kocsis, 2003, p. 133 for an ambiguous account of how the data were included in his analysis). These additional data were never published separately but, for whatever reason, were added to previously collected data. We cannot see how, in good faith, Snook et al. can be criticized for not analyzing data that could not be accessed.<sup>1</sup> Second, the Snook et al. meta-analyses contained two studies that were not analyzed in Kocsis (2003): Pinizotto and Finkel (1990) and Kocsis, Middledorp, and Try (2005). These two studies make up 50% of all studies examining the predictive accuracy of profilers (wherein the necessary data were provided to conduct the meta-analysis), thus providing more knowledge than what can be garnered from relying solely upon Kocsis (2003).

## Sample Size

Even if one accepts KMK’s argument that the meta-analytic results reported by Snook et al. (2007) reflected favorably upon professional profilers, CP researchers need to consider whether their database is substantial enough to make any positive recommendations about the predictive accuracy of profilers. For example, consider KMK’s claim that, “where some form of criminal profiling is undertaken, a suitably qualified profiler is most likely capable of predicting a greater amount of accurate information about the probable offender than what other persons (i.e., nonprofilers) may achieve” (p. 258). This statement is based on the performance of fewer than 20 profilers and pales in comparison to what other research areas require before similar claims are taken seriously.

Consider, as but one example, research related to offender “get tough” programs on recidivism rates. Only relatively recently have people started to

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<sup>1</sup> In hindsight, Snook and his colleagues should have contacted Kocsis to obtain this additional data for their meta-analysis. However, we did contact Kocsis about obtaining this additional data so that we might address the concerns raised by KMK in this reply. He indicated that he was not willing to provide us with the data for this purpose.

accept the fact that such programs have little effect on reducing recidivism rates, and this finding is based upon studies with a collective sample size that exceeds 400,000 (with more than 500 effect sizes; Smith, Goggin, & Gendreau, 2002). Similar examples could be provided from other domains, such as personnel selection research (e.g., Schmidt & Hunter, 1998). The point here is that researchers in these fields are cautious about making strong claims that could impact policy decisions even when sample sizes are much larger than is the case in the profiling context. In the Snook et al. study, the largest effect size of  $r = .32$  was based on a sample size of 447 and 18 effect sizes (see Table 3, Snook et al., 2007). Of the 447 participants, only 19 were profilers (and it is unknown whether the same profilers were used in multiple studies). Consequently, we maintain that there is a rush to judgment by KMK regarding the positive forecasting abilities of profilers.<sup>2</sup>

### Confidence Intervals

We also ask readers to consider the width of the confidence intervals (CIs) presented in Snook et al. (2007). Arguably, the primary goal of knowledge cumulation is to increase the precision of an estimate (Schmidt, 1996). It is not good enough to simply say that a finding is statistically significant, because  $p$  values give little information about the utility of results or the potential of a finding being replicated (Cumming & Finch, 2005; Gendreau & Smith, 2007). Again, drawing upon the largest effect size (see Table 3, Snook et al., 2007), the CIs were egregiously wide in our view. It was interesting to note that KMK fail to mention this issue in their rebuttal, but we ask readers to consider whether they would have confidence in a result (e.g., the largest effect size for professional profilers where  $r = .32$ ) that can potentially vary between .10 and .54. Further replications of this result means that 95% of CIs would overlap with this range. Where might the eventual level of predictive accuracy be when much more evidence, from higher-quality studies that are ecologically valid, is gathered?

Related to this issue is the fact that a fixed-effects model was used in Snook et al. (2007) to estimate the CIs. The use of this model arguably results in effect estimates that are less conservative than effects generated through the use of alternative models. If a random-effects model had been used, the CIs might be even wider than those reported in the Snook et al. study (for a summary of this issue, see Gendreau & Smith, 2007; Schmidt, Oh, & Hayes, 2009). In fact, it is possible that every CI in Table 3 in Snook et al. would include 0, which would indicate (even more than the study by Snook et al. already does) that profilers are no more accurate than lay individuals.

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<sup>2</sup> To borrow Carl Sagan's motto, extraordinary claims requires extraordinary proof. The burden of proof rests with profilers (and CP advocates), who make extraordinary claims about their forecasting abilities, to prove their worth in experiments.

## Binomial Effect Size Display

The binomial effect size display (BESD) was also used in Snook et al. (2007) to provide a practical estimate of the effect size. For illustrative purposes, the single largest effect size in favor of the CP hypothesis was  $r = .32$ . In other words, there was a 32% improvement in overall accuracy for the profilers over the non-profiler group. However, unless the base rates of accuracy are 50%,  $r$  can be inaccurate (cf. Gendreau & Smith, 2007). For example, if we found that the base rate accuracy for profiling predictions was actually 20% (rather than the assumed 50%), the BESD might well be halved depending on other factors (e.g., sample sizes). At this stage, the base rate for profiler accuracy is unknown. Therefore, until the base rate level of profiling accuracy is well established, the BESD values should be interpreted with extreme caution (Gendreau & Smith, 2007), thus explaining why Snook et al. were tentative in considering  $r = .32$  a strong indicator of profiling accuracy.

## A SEARCH FOR HIDDEN AGENDAS: WHAT THEORETICAL QUANDARY?

Beyond these issues, we also question the accusation made by KMK (and by Kocsis, 2006a) that Snook and Bennell are biased researchers who are not presenting their results in an objective fashion to move profiling research forward but were instead trying to advance a hidden agenda to promote an IP approach to profiling. We are baffled as to how Kocsis and his colleagues have come to this conclusion. We assume it has to do with our relationship with the founder of IP, Professor David Canter, with whom we studied for our doctoral degrees and continue to associate with in a variety of professional capacities.<sup>3</sup> If this is the basis of KMK's assumption, we think that this is ludicrous.

This accusation that we were attempting to promote an IP-based approach to profiling through our research is odd for several other reasons. First, in neither of the targeted papers did we reference "investigative psychology." Frankly, we are not even clear what an "investigative psychology approach to criminal profiling" is, given that people who have studied IP appear to advocate different profiling methods (e.g., Alison, West, & Goodwill, 2004; Canter & Fritzon, 1998; Godwin, 1999). Second, the recommendations we proposed in Bennell et al. (2006) as a way of trying to

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<sup>3</sup> Although one of us (Bennell) serves on the editorial board for *Journal of Investigative Psychology and Offender Profiling* (JIPOP) and two of us (Snook and Bennell) have published in JIPOP, this is not synonymous with promoting an IP-based approach to profiling. We are, however, advocates of examining investigative practices from a scientific perspective.



improve the type of research conducted by Kocsis were clearly not one-sided. If implemented, some of our recommendations might benefit the profilers, whereas others might benefit non-profilers. Third, we have been more critical than most (we hope in a constructive way) of the work emerging from IP (e.g., Snook, Canter, & Bennell, 2002), and anyone who has read our work knows that we do not currently endorse profiling, regardless of approach, due to a lack of compelling data to support its use (e.g., Snook et al., 2008). Last, we are in a position to be objective researchers in the field of CP because we are not involved in providing profiling advice (and therefore have no stake in how profilers perform) and do not advocate any “approach” to profiling. The same cannot be said of Kocsis (e.g., crime action profiling; Kocsis, 2006b).

### SO, WHERE TO FROM HERE?

Though we do not see any merit in KMK’s accusation that our research can be seen as an attempt to advance a pro-IP stance, we do not disagree that it may be valuable in the future to compare the various CP approaches. We believe that defining these various profiling approaches may be more difficult than KMK realize, but we have nothing against such research. We encourage it.

However, we also maintain that there are limitations with Kocsis’ studies that need to be resolved; some of these limitations are fundamental to his research program. For example, beyond examining the predictive accuracy of professional profiles, Kocsis’ research involves the identification of skills that facilitate accurate profiling (see Bennell, Corey, Taylor, & Ecker, 2008 for further critique of this issue). In his skills assessment studies, Kocsis assumes that various groups (e.g., university students) possess certain skills (e.g., a capacity for logical reasoning) that have been deemed important for profiling success (Hazelwood et al., 1995). Yet, Kocsis (2006a) does not think it is imperative to measure the skills that he claims his participants possess. This is evidenced by the fact that he has only ever conducted a formal assessment of one skill (i.e., reasoning using the 16PF) in one of his studies (Kocsis, Hayes, & Irwin, 2002). However, even in this case, no data were provided from this assessment.<sup>4</sup>

Rather than formally assessing skills, Kocsis prefers to assume that his participants possess certain skills. For example, Kocsis assumes that science students must have a greater capacity for logical reasoning than any of his other sampled groups because they are required to objectively analyze data

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<sup>4</sup> It is debatable that a formal assessment of investigative experience was conducted by Kocsis et al. (2002) for the various law enforcement groups that they tested.



as part of their degree (Kocsis, 2006a, p. 468). However, just because Kocsis thinks this is the case does not make it so (and if he is wrong, so too may be his conclusions). Despite any claim to the contrary, it is clear that many university students do not necessarily possess high levels of logical reasoning (e.g., Pithers & Soden, 2000; Williams & Stockdale, 2003), and this is true even in disciplines wherein we might expect to see high levels of reasoning ability (e.g., engineering; Holvikivi, 2007). Furthermore, it is simply not logical to assume that students possess more reasoning skills than some of Kocsis' other participant groups. For instance, why would practicing psychologists not possess more of this skill than undergraduate students (some of whom may not be particularly successful) given that the former likely hold both a master's and a doctorate degree (whereby many would have been trained using the scientist-practitioner model)? In the end, it is vital to establish the presence of this skill instead of assuming it exists. The issue here is not to rehash criticisms of Kocsis' research that have been debated elsewhere (Bennell et al., 2006; Kocsis, 2006a) but to point out that there are more pressing issues (the assessment of participant skills being just one) that need to be tackled in the CP literature beyond searching for a hidden IP agenda in our research.

By way of conclusion, we would like to reiterate our reason for conducting the original narrative review and meta-analyses. As outlined in Snook et al., the frequency with which CP has been used by police forces has grown steadily over the past 30 years, and CP techniques are now used commonly in investigations worldwide. Despite this increased usage and concurrent increase in CP literature, little was known about the validity of this technique. Therefore, we felt that a systematic review of the area was needed to assess the nature of the evidence for CP and in turn inform practitioners of the validity of this technique.

The narrative review, which was not mentioned in KMK's critique, revealed that the CP field relies on weak standards of proof (i.e., common sense rationales). Findings from the two meta-analyses revealed that profiling groups did not decisively outperform comparison groups in predicting the characteristics of an unknown offender. We concluded that CP is a pseudoscientific investigative technique that has questionable validity. We stand by this conclusion. Given that the impact of CP on investigations is unknown (and may very well be negative), we believe that police forces should avoid using CP until there are sound empirical data supporting it as an effective investigative tool.

We leave the reader with this final thought about the state of CP research on the predictive accuracy of profilers. If we wish to make this research more scientific, we must ask: What is the hallmark of successful sciences (e.g., chemistry, physics)? In our opinion, this means obtaining precise estimates of a population parameter (e.g., predictive accuracy of profiling predictions) through careful replication via high-quality research with clear, objective

definitions of predictor variables (e.g., characteristics of the profiler, profiling approach being used, and the data being used to make the predictions). We are a long way from that happening in the field of CP but urge CP researchers to pursue this goal.

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