

Commentary—Extending the Boundaries of Systematic Case Study Research: Conceptual and Methodological Issues

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ABSTRACT

In this commentary we discuss the two examples of systematic case study research in this issue: Miller et al., (2021), who continue the development of the quasi-judicial Panels of Psychological Inquiry method by applying it to a child client with an autistic spectrum condition; and Bohart et al. (2021), who apply their research jury approach to a video recorded case of Emotionally-Focused Therapy for couples. We open by briefly summarizing the main issues addressed in our previous commentary (Stephen & Elliott, 2011), which involved the same authors; we also note some key developments in systematic case study research over the past ten years. The rest of our commentary is divided into three parts. First, we look at more general conceptual issues in systematic case study research, including situations in which systematic case studies are likely to be most useful; the problem of overly broad research questions; the definition and assessment of outcome; and the thorny issue of causality. In the second part, we turn our attention to methodological issues raised by the two articles, returning to the questions of what counts as evidence in systematic case study research (here the use of observational methods for assessing client change and change processes), but also to the processes by which research judges or jurors make decisions about knowledge claims and methods for generalizing from one case to other cases. In the final main section, we offer more substantive commentary on Miller et al. (2021), from the point of view of autism research. We start by putting the DIR/Floortime intervention in context before raising key diagnostic issues that we think circumscribe the case and spelling out uncertainties about the nature of the intervention used. We round off this section with a set of proposals for future systematic single case research on interventions for autism. We close our commentary with a brief set of recommendations.

Keywords: systematic case study research; autism; emotionally focused therapy for couples; qualitative research; case studies; clinical case studies

1. INTRODUCTION: THE STORY SO FAR

In our commentary (Stephen & Elliott, 2011) on an earlier set of articles that include the same first authors (Miller, 2011; Bohart et al., 2011), we began with a brief introduction to the

Hermeneutic Single Case Efficacy Design (HSCED) method, which had in turn been inspired by and built on the work of Bohart and colleagues in particular (e.g., Bohart & Boyd, 1997; Bohart & Humphreys, 2000). We presented the main procedures used in HSCED studies, including the collection of both direct or confirming evidence linking therapy process to outcome and indirect or disconfirming evidence (Elliott, 2002). We described how HSCED came to incorporate adjudication (the systematic use of affirmative and skeptic positions and judges) into its basic procedures (e.g., Elliott et al., 2009; others).

We then turned our attention to the studies at hand, first exploring the *sources* of evidence employed (i.e., client, therapist, observers), an issue that will be important in our commentary here. We advocated for the use of Rich Case Records that include a range of information from different perspectives and obtained by different methods (qualitative and quantitative). Also relevant in our discussion was the use of witnesses, including therapist and supervisor.

Second, we explored the issue of how *evidence is tested* in the various systematic case study methods. For example, the cross examination of evidence, generally used metaphorically, is taken literally in Miller's (2011) Panel of Psychological Inquiry (PPI) method, as the PPI Advocate and Critic interrogated therapist and supervisor in front of the panel of judges. We also noted the difficulty of enacting the role of Critic or Skeptic in both the PPI method and HSCED studies and steps that have been taken to help overcome the reluctance of researchers in this role. Another evidence-related issue was whether and how to provide judges with criteria for evaluating evidence.

A third set of key issues we looked at involved proof, including knowledge claims, burden of proof, and standards of proof. We noted that Miller's PPI method typically considers a broad range of *knowledge claims*, whereas HSCED emphasizes (a) whether or how much the client changed and (b) whether/how much therapy was responsible for client change. In contrast, the PPI method also tests knowledge claims about client diagnosis, treatment integrity, and generalizability. (The first two of these are background claims in HSCED research but are not a main focus, while HSCED studies handle the generalizability claim in a quite different manner, which we will discuss here again in more detail.) We then highlighted similarities between *burden of proof* in PPI and in HSCED (it belongs to the party putting forward the knowledge claim, usually the advocate or affirmative side. Next, we discussed *standard of proof*, the third proof-related issue, in terms of the subjective probability levels associated with different standards and argued in favor of intermediate standard ("clear and convincing evidence", roughly >80% probability) between "preponderance of the evidence" (>50% probability) and "beyond a reasonable doubt" (>95% probability).

The fourth set of issues we considered had to do with the *adjudication process* itself, beginning with the lack of current knowledge about the process of judging PPI or HSCED cases but also including the number and selection of judges.

We concluded with a discussion of recommended future developments, specifically calling for the following:

- (a) further clarification of the advocate/affirmative and critic/skeptic roles;
- (b) careful consideration of how best to respect the experience of the client and therapist whose encounter is the subject of the adjudicated case study;
- (c) development and clarification of explicit and appropriate standards of proof; and
- (d) further research into the processes by which jurors/judges evaluate the evidence. (Stephen & Elliott, 2011, pp. 239)

In this follow-up commentary, we will revisit the above issues in relation to the two studies presented here in this special section.

According to Google Scholar, our previous commentary has been cited 27 times. We are pleased to note that the great majority (15) of these citations were used to support systematic case studies (and more particularly HSCED studies; e.g., Wall et al., 2014). Citations were most commonly made by UK-based researchers (10), but also included researchers from Canada (9), Chile (4), Portugal (2), the Czech Republic (2) and Finland (1). Our commentary was most often cited by researchers representing nonmainstream psychotherapies (more on this later), including Lifespan Integration therapy (9), transactional analysis (4) and cognitive analytic therapy (3).

Since our previous commentary we see two key developments. First, Benelli et al. (2015) reported an extensive, detailed systematic review of thirteen HSCED studies, resulting in a checklist of 28 evaluative criteria. Second, Wall et al. (2017) and Stephen et al. (2021) have extended HSCED to include multiple case designs in which collections of 3 or more cases are presented together in the same report, including comparing good and poor outcome cases. For example, in Stephen et al., for the poor outcome cases, the affirmative case sought to prove that the change experienced by the client at the end of therapy was deterioration, rather than improvement. Then, the data generated by the eight HSCED studies (affirmative and skeptic cases, adjudication panel decisions) were synthesized and compared using qualitative metasynthesis in order to develop an understanding of similarities and differences in the therapeutic processes that might explain the contrasting outcomes.

2. GENERAL CONCEPTUAL ISSUES IN SYSTEMATIC CASE STUDY RESEARCH

In this section we look at a set of more general issues in systematic case study research such as the two approaches used in this special section and in HSCED. It is our view that these issues have been somewhat neglected in previous writing about systematic case study methods, so it is our goal here to raise awareness about them in order to stimulate further discussion.

2.1. Reasons/Benefits for Doing Systematic Case Studies

The two cases presented here provide us with an opportunity to consider situations in which systematic case studies are most appropriate or interesting. This issue is particularly relevant given their time-consuming nature.

Most obviously, Stiles, Hill & Elliott (2015) have argued for the use of systematic case studies as an initial stage of research on *marginalized or emerging psychotherapies* (MEPs), which include both new treatments but also applications of more established therapies with new client populations (e.g., athlete performance difficulties; Wall et al., 2017). This point applies here to the Miller et al. (2021) PPI approach, where the novel client population is early childhood autism. However, this rationale does not apply to Bohart et al. (2021), involving as it does a treatment (Emotionally-Focusing Therapy for couples; EFT-C; Johnson, 2004) that has been examined in a large body of controlled and uncontrolled group studies and is well-established both in terms of evidence and in clinical practice.

There is, however, another reason for doing systematic case studies, which is to develop and demonstrate the use of *new psychotherapy research methods*. That is the case for both Miller et al. (2021) and Bohart et al. (2021). Up until now, most systematic case study research has primarily relied on client (and to a lesser extent, therapist) self-report data collection instruments. However, both of these cases make extensive use of third-party observational data. Miller et al. report the use of a set of formal, quantitative psychological assessment observation instruments, including both a broad measure of toddler cognitive, language, motor, social-emotional and adaptive developmental level (the Bayley Scales) and the Autism Diagnostic Observation Scale (ADOS-2; Lord et al., 2012). Bohart et al. take a different route, carrying out extensive qualitative observations of a complete set of recordings of a case of EFT-C, making use of the fact that therapy session recordings are themselves a form of rich case data (cf. Elliott & Timulak, 2021).

Beyond this, Miller et al., highlight the value of the cross-examination process allowed by the Panel of Psychological Inquiry; they provide transcribed excerpts of some of these cross-examinations, thereby exemplifying the original sense of the word *dialectic*, i.e., “truth through dialog” (Gadamer, 1979).

Further, the two case studies can be used to explore the potential payoffs of these methodological developments. For us the main conclusion is that it is indeed possible to carry out systematic case study research that can support strong causal inferences without relying on client self-report testimony. In the Miller case the identified client is a small child with an autistic process, which requires the use of expert witnesses including the therapist and her supervisor as well as the child’s guardian. In the Bohart et al. (2021) case we also have to rely on observation rather than self-report, because the only data available are the video recordings of the couple’s therapy sessions. In spite of these limitations, the panel of judges in Miller et al. (2021) found themselves generally comfortable making strong causal inferences linking client change to Ronan’s treatment at the Center. Similarly, after watching the videos of the ten

therapy sessions, the researchers in Bohart et al. were convinced that the clients had changed because of therapy. Thus, these two case studies provide proof of concept for extending the range of kinds of data that can be used in systematic case study research.

2.2. The Importance of Research Questions/Focus

In spite of the generally successful application of observation in assessing client outcome and making causal inferences, these two systematic case studies raise several conceptual issues that clearly need attention going forward.

One of the main benefits from studies like these is to help us clarify our research questions. Commonly-studied questions or knowledge claims (e.g., Did this therapy cause client change?) are simplistic, making them difficult to answer, and even unproductive. In Miller et al (2021) the PPI judges struggled for conceptual clarity about the issue of whether “treatment” consisted of the work that Kristen, the student intern, did with Ronan as opposed to the larger treatment program at the Center (for more on this issue, see section 4.2.2 below). In addition, the Advocate in Miller et al. failed to separate out the question of whether the client changed from the question of whether treatment was responsible for client change, requiring the Critic to raise the issue as a counter-claim (with implications for the burden of proof).

An overly broad research question is like a blunt instrument. It is our view that good systematic case studies require better questions. Better how? For starters we think that research questions should be more precise and less global. For example, what we mean by “treatment” needs to be spelled out, based on one’s research interests or previous investigation. In some cases, defining the treatment probably needs to be its own question, as in the two PPI studies we have seen. As we elaborate in the next two sections, what is meant by “outcome” and “cause” also need to be specified. Actually, we think that the PPI method and HSCED offer critiques of each other regarding the questions that need to be asked and complement each other in this regard. For example, Heinrich (2012) benefitted from our previous commentary (Stephen & Elliott, 2011) by adding PPI-style questions to HSCED, examining for example the extent to which the client data were consistent with Emotion-Focused Therapy and what reasonable generalizations to other socially anxious clients could be made from the data.

We will come back later to a more general point: Systematic case study designs need to be more cognizant of the complexity of therapy process and outcome.

2.3. What is Outcome? How Do We Assess It?

Most commonly, outcome is assessed using quantitative psychometric measurement instruments. In systematic case study research these allow us to compare client pre and post test data to published norms and clinical cut-off values for establishing “caseness” (i.e., whether the client is in the clinical range or not) and also whether any pre-post change can be considered to be unlikely to be due to chance: “reliable change” (Jacobson & Truax, 1991).

Of course, it can be difficult to determine in advance how a client will change, and it is probably impossible or at least not worth the effort of assessing clients on all possible ways in which they might change. That is where qualitative approaches to assessing outcome come in: For example, it is common practice in HSCED studies (e.g., Elliott et al., 2009) to ask clients at the end of therapy how they view themselves as having changed over the course of therapy. The present studies take an even broader view of outcome, encompassing qualitative observation of the clients in early and late sessions of therapy. This is consistent with a conversation analysis perspective on therapy outcome, which advocates an alternative approach to studying therapy outcome, using close examination of client talk at the beginning and end of therapy for conversational phenomena labelled as client “preference” or “alignment” with the therapist (Sutherland et al., 2013).

2.4. Questions of Causality

In both case studies, the authors find themselves forced to take on the question of establishing causality. As Cook and Campbell (1979) argued, humans evolved to look for people-scaled, middle-level causal processes, which are neither microscopic nor astronomical. Outside of randomized clinical trials (RCTs), psychotherapy researchers, like most social scientists, profess an aversion to inferring or even discussing causality in their research, while at the same time being drawn to it like a moth to a flame. Thus, causality sneaks in, in the form of jargon terms like “structural equation modelling,” “functional analysis,” and “change processes.” Speaking personally, it is a relief to read Miller et al.’s (2021) PPI and Bohart et al.’s (2021) observational case methods, which are unashamedly causal in their focus. We suspect that the close examination of the data afforded by direct observation of single cases like these may make it easier rather than harder to see the possible causal processes, an observation made by Mohr (1999) a generation ago.

For Miller et al. (2021), this takes the form of a set of requirements derived from British common law: “a clinician who (1) agrees to offer professional clinical services to a client; (2) actually offers those professional clinical service to the client that the clinician believes will be helpful; (3) achieves the desired therapeutic outcome (4) as foreseen by the clinician; and (5) but for the therapeutic services offered in (2), the outcome observed in (3) seems unlikely to have occurred.” (p. 176). In comparison to HSCED, we can see in this formulation both the broadening of the research questions (discussed in section 2.2) and the parallel to the affirmative and skeptical positions in the HSCED method.

Bohart et al (2021) also want to make a causal inference: that emotionally focused couple therapy brought about (in the sense of “contributed to”) substantial change for Carl and Sandra. For these researchers, however, this is just a starting point; their real interest is to elucidate the specific nature of the causal processes involved (“change processes”). In other words, they want to know *how* and *why* EFT-C helped bring about change for Carl and Sandra.

However, from our point of view, even this attempt to specify causal influence does not go far enough. To begin with, as Bohart and Tallman (1999) have pointed out, therapies (and

therapists) do not literally cause clients to change, because clients, not therapists or interventions, are the active change agents in psychotherapy. This means that Bohart et al.'s (2021) question needs to be further specified as "How did these clients use emotionally focused therapy for couples (EFT-C) to cause themselves to change?" This might sound like semantic nitpicking, but in fact it has important implications for how we think about our work as therapists. Is the therapist the "prime mover" of the client's change? (Gottman & Markman, 1978). Or do they simply provide resources or opportunities for clients to change themselves? We believe that therapists behave quite differently depending upon which of these two positions they start from.

Beyond this, the causal processes operating in psychotherapy are likely to be far more complex than either (a) EFT-C => Carl and Sandra change, or (b) Carl and Sandra => elicit resources from Rebecca => which they then use to change themselves. As both case studies illustrate, a multiplicity of causes are operating at the same time, sometimes adding to (or interfering with) one another in straightforward ways, while at other times they interact, moderate or potentiate each other. In addition, there can be reverse causation, such as when early client change enhances client engagement in key therapy processes.

In fact, this complexity makes studying entire therapy cases unwieldy, so that the researcher gets lost in the welter of different but overlapping and interacting change processes, or, worse, multiple change processes with different names that may actually be the same thing (e.g., "alliance" and "psychological contact"). This points to the need to work with smaller units of change, such as particular therapy sessions or even within-session significant events or episodes. Elliott (1993) developed Comprehensive Process Analysis (CPA) for just this purpose: to unpick the tangle of influences that bring about client change in therapy in single pivotal events, including their connections to other important events in therapy or in clients' lives (e.g., Elliott et al., 1994).

3. METHODOLOGICAL ISSUES

3.1. *What is Evidence?*

3.1.1. In Miller et al. (2021) and Bohart et al. (2021). In Miller et al, the claims put forward by the advocate are supported by a combination of documentary evidence (master's thesis; organizational records of clinical work; a follow up report from mother of client) and the testimony of key witnesses (therapist and supervisor). What became clear on reading Miller's account of the process was the impact of the hearing itself which enabled new and unanticipated evidence to be generated. Although the original intention in inviting evidence from the therapist's supervisor was to corroborate the therapist's testimony, it became clear during the hearing (p. 174) that the supervisor was also able to provide direct evidence arising from their own work with the client and as center director. This enriched the range of evidence available, and enabled the judges to broaden the questions that they wanted to ask, allowing multiple perspectives to be explored. As Miller et al. note, the PPI resembled a "highly structured clinical case conference" (p. 174).

For Bohart et al. (2021), the evidence being evaluated took the form of video recordings of therapy sessions; however, they did note (p. 6) that in other cases the case history could be widened to other records generated during the clinical work. In scrutinizing the recordings for evidence that supported (or did not support) a conclusion that the couple in therapy had changed and that therapy had played a role, Bohart et al. identified two specific forms of evidence that the jurors had sought: first, what the clients *said* (i.e., *content*), noting that they gave certain client content more weight (e.g., spontaneous utterances, concrete examples, patterns over time that imply change or lack of change; p. 212); and, second, what the clients *did* in the session (i.e., *process*). It was these pieces of evidence that Bohart et al.'s jury extracted from the recordings and used in their decision-making process.

3.1.2. Methodological Pluralism in Systematic Case Study Research. What does it take to bring evidence up to standards such as a “preponderance of the evidence” [51%] or “clear and convincing” [70-80%]? What does it take to *convince* judges or jurors? What makes evidence “weak” or “strong”? Different kinds of knowledge claims probably require different criteria for weighing strength of evidence. To start with, evidence can be evaluated on the basis of relatively straightforward criteria such as *clarity/obviousness* and *relevance*. However, we are particularly interested in more complex criteria:

Most obviously, *corroboration* requires the use of multiple kinds of evidence (e.g., client vs therapist vs observer; quantitative vs qualitative), deriving from a position of methodological pluralism (Slife & Gantt, 1999) or converging operations (Cook & Campbell, 1989). In our experience, this criterion turns out to be more challenging than it sounds, given the frequent disjunctions between different kinds of data. (See Elliott & Shapiro, 1992, for a clinical example and a corroboration rating scale.) Corroboration is a strength of Miller et al. (2021); it is also easily found in Bohart et al. (2021), who reported both multiple indicators of client change (p. 222) and multiple lines of evidence linking therapy to client change (p. 225). For its part, HSCED makes explicit use of corroboration as an evidence standard when it requires at least two kinds of evidence connecting therapist process to client outcomes.

A more complex criterion is *absence of noncorroboration in the face of attempts to falsify*, marked by good-faith attempts to find contradictory evidence that will throw in question the validity of positive indicators of change or therapeutic causality. This is the function of the Critic in Miller et al. (2021) and Skeptic in HSCED, but it is also found in Bohart et al.'s attempts to find non-EFT explanations for client change.

3.2. Making Decisions

The Miller et al. (2021) and Bohart et al. (2021) case studies are based on two different models of decision-making used within various legal systems: a panel of judges (impartial experts in the law) versus a panel of jurors (peers). Although representing differing types of decision-makers, the decision-making processes demonstrated in these cases appear very similar. In each case, the individual participants in the process make notes, reflect separately, meet together to discuss their views, and arrive at a decision. The final decisions are reached

according to different mechanisms: by unanimous/majority decision (Miller et al.) or by consensus (Bohart et al.). In conducting these case studies, both groups of authors had a specific interest in exploring the decision-making process. The main focus for Miller et al. was proof of causation, adapting ideas from personal injury law (“who injured whom”) to psychotherapy (“who benefited whom”). Bohart et al. were interested in exploring how “convergence of evidence” (p. 212) occurs: how a group of jurors with different perspectives construct a plausible pattern from various sources of circumstantial evidence, ruling out alternative explanations, to reach consensus or “spontaneous convergence” (p. 213). In the process, both groups of authors not only explored their particular interests but also experienced the challenge proposed by the other group. Particularly, in Miller et al., the views of the individual judges in Miller et al. did not converge in their decisions, while in Bohart et al., the jurors in Bohart et al., highlighted their struggle to feel confident in making decisions about causation as they grappled with the complexity of the change process that they observed. We will return to these themes after reviewing the development of our thinking in relation to the decision-making process followed in the HSCED method.

In early HSCEDs (e.g., as discussed in Stephen et al, 2011), each member of the adjudication panel reached their decision independently using a separate Decision form that offered Likert-type scales to record answers to the main HSCED questions that captured the amount of change perceived and the probability that this had occurred. These included: (1a) To what extent do you think the client changed over the course of therapy? (1b) How confident are you that the client showed at least “substantial” change over the course of therapy? (2a) To what extent do you think that the client’s change was due to the therapy? (2b) How confident are you that the client’s change was at least “substantially” due to the therapy? Members of the adjudication panel were also invited to identify what evidence presented in the affirmative and skeptic cases mattered most to them in reaching their conclusions, and to explain how they made use of this evidence. So far, this process of decision-making sounds similar to that followed in the Miller et al. case. However, the method used to bring the panel members’ decisions together was different. In these examples of early HSCED studies, final decisions were produced by calculating the mean or median scoring on each scale. Median scores were preferred, as these enabled a weighting of the scores that better represented the range of decisions produced by the panel members.

However, in a recent study (Stephen et al., 2021), a different approach was followed: adjudication panels received the HSCED material in advance, then met to discuss their views in order to reach a decision by consensus. The researcher who prepared the HSCED (both affirmative and skeptic views) was available to answer questions from panel members, if required. The decision-making discussion was audio recorded, and the decision reached on each HSCED question summarized and presented in the same Decision form used in earlier HSCED studies. An interesting aspect of this process was that the eight HSCED studies were conducted and adjudicated by the same group of researchers, eight Masters in Counseling and Psychotherapy students carrying out this credited research project as part of their studies. Each student produced one HSCED study, and took part in three adjudication panels. The process followed a “known groups” design: four of the HSCED cases were identified as “good outcome”

cases and the other four as “poor outcome cases,” according to quantitative outcome data. Researchers assigned to good outcome cases worked together in a study group as they developed their own cases, and formed the adjudication panels for the poor outcome cases, and *vice versa*. As a result, members of adjudication panels were not familiar with the details of the cases until the adjudication.

We did, however, note (Stephen et al., 2021, online supplemental material) that the adjudication panels for good outcome cases were more clearly convinced by the evidence produced by the affirmative case (that improvement had occurred by the end of therapy, and that this change was due to the therapy), than the adjudication panels for poor outcome cases were convinced that deterioration had occurred. It may be that the panels were influenced by their prior focus on a particular type of outcome (good v poor): for example, the members of adjudication panels who developed HSCEDs on good outcome cases may have been more inclined to recognize contrasting evidence suggesting improvement in the cases of clients who, according to quantitative outcome data, seemed to have deteriorated by the end of therapy. Similarly, the panel members who had worked on preparing arguments that clients had deteriorated by the end of therapy may have been more easily convinced by evidence of improvement in the good outcome cases, as this may have appeared more clear-cut, less ambiguous. It is also possible that attribution bias (the tendency to attribute internal factor—such as the therapist’s efforts—as the cause of positive outcomes, and external factors—such as the client’s life situation—as the cause of negative outcomes) may have been present in the results of the panels’ decision-making: that the therapy had been a significant factor (70-80%) in the improvement that occurred in the good outcome cases, but less influential (35-50%) in the deterioration that occurred in the poor outcome cases.

Our recent example of conducting HSCED adjudications highlights some basic challenges in the decision-making processes likely to occur within an adjudicated case study model. However, as mentioned above, both cases presented in this issue demonstrate another dimension of the complexity that we recognize as central to this process. The judges in Miller et al. (2021) upheld three of the four claims proposed by the advocate, rejecting Claim 3 and the critic’s counterclaim. Similar reasoning influenced both decisions: that the wording of the claim/counterclaim was too narrow, proposing that outcome was the result of the individual therapist’s work. The majority of judges felt that it was impossible to decide on the exact nature of the causation, in other words, to distinguish who held “responsibility for improvement” (p. 174) in this case, as this would require separating the impact of one therapist who was working with the client within a multi-professional, holistic therapeutic environment.

Similarly, Bohart et al. (2021) expressed decreasing confidence in their conclusions as they moved from considering (1) if change had occurred, and (2) had psychotherapy caused the change, then (3) identifying the change mechanisms that may explain the change that occurred. They concluded that change is a complex interaction between the therapist and the client, noting “how much more complex the change process looks from the point of view of this kind of analysis than what you get from looking at change through the lens of a randomized trial” (p. 231). We valued their assertions (p.211) that, in contradiction to the assumptions of

randomized trial designs, a closer look at the therapeutic process demonstrates that, due to therapist responsiveness, there is (a) no true independent variable, and due to client agency, (b) no true dependent variable: the change process occurs within the interaction between client and therapist. This is our experience as HSCED researchers, and is the main reason that our original interest in this method as a means for assessing outcome has evolved into an interest in exploring therapeutic process and, by seeking patterns in change processes across multiple cases, building “case law.” We will discuss our experience of this process in more detail below.

3.3. Generalizability Again: Developing “Case Law”

Both Miller et al. (2021) and Bohart et al. (2021) are interested in developing case law. For Miller et al., case law would take the form of “a body of validated clinical practice knowledge in psychology” (p. 132) in which a clinician’s work has been reviewed and validated. For their part, Bohart et al. argue that a series of case histories is required to “draw more definitive conclusions as to how therapy contributes” (p. 232).

On the other hand, the official position in HSCED is that the best basis for establishing the generalizability of systematic case studies is through specifying relevant mediator and moderator variables (Elliott et al., 2009). Thus, Stephen et al. (2021) used their qualitative metasynthesis of moderator and mediator variables across a series of eight HSCED studies as a means to develop generalizable conclusions analogous to case law. First, the eight case studies (two groups of cases: four good outcome, four poor outcome) were conducted using the HSCED method. Next, the qualitative data generated from each case (affirmative case, skeptic case, adjudication) was synthesized using a generic descriptive-interpretive approach (Elliott & Timulak, 2021). In this process, no distinction was made between the outcome of each case, enabling the development of a conceptual framework built with the evidence generated about therapeutic processes that occurred across the cases. Once this had been done, it was possible not only to distinguish between the two groups of cases within the overall analysis, but also to identify “general’ and typical” findings (i.e., evidence was found in all or almost all cases) within each group.

Finally, this “convergence of evidence” identified therapeutic processes shared by each group of cases, irrespective of outcome, and ones that appeared to have occurred for clients in one or other group. On one hand, this represents a development toward creating case law, by synthesizing a series of cases. However, to stay with the quasi-judicial concept, case law should evolve in response to the judgements made in each case; in HSCED the judgement is the decision made by the adjudication panel. The decisions produced by the panels in the eight cases in this study were not sufficiently rich for the metasynthesis to be drawn solely from this source. Therefore, work continues to develop an adjudication process in which the decision issued by the panel contains sufficient detail to provide a basis for building case law.

4. COMMENTARY ON MILLER ET AL. (2021) FROM THE PERSPECTIVE OF AUTISM RESEARCH AND PRACTICE

4.1. Background

In the United States in the 1970s various large projects were funded to find evidence for intervention models that could treat autism. Several of these models were subsequently trademarked and extensively franchised, including Lovaas' ABA model, TEACCH, Greenspan's DIR/Floortime, and Son-Rise to name a few. These were (and still are) mostly comprehensive treatment models and require training to an accredited standard and affiliation with a center of excellence.

Probably best known of these models is Applied Behavior Analysis (Lovaas, 1977; 1987), generally viewed as an effective treatment for people with autistic spectrum process (see review by Hyman et al., 2020). However, this approach is increasingly being contested in practice and from the autistic community, on the basis of being control based and potentially abusive and harmful (e.g., resulting in post-traumatic stress difficulties; Kupferstein, 2018; McGill & Robinson, 2020).

In contrast, in the case of Ronan, Miller et al. (2021) looked at a modified version of Greenspan's (Greenspan & Wieder, 2006) DIR/Floortime program. This is a comprehensive treatment program—as such it aims to address many skills and is not a focused practice intervention. Having adults follow the child's lead and engaging in rapid back and forth play is a large part of this treatment. These principles are also principles used in other methods (e.g., the Son-Rise programme developed in the 1970s by Kaufman (1995). The typical training involves first training parents in the DIR/Floortime approach and then requiring them to practice this with their child for 15 hours per week. Home visits of 3-4 hours are made by trainers once/month. It is worth noting that there can be treatment fidelity issues in how this is applied.

In addition, treatment approaches in the autism field are typically adapted to meet particular client profiles. In this case, Michele Fouts, the supervisor, made the adaptations to the model and then supervised the student intern's practice. However, in Miller et al.'s original article it was not clear what modifications were made to this model and what implications this might have for the knowledge claim that the treatment was responsible for changes that occurred or that the results could be generalized to other young children diagnosed as on the autistic spectrum.

4.2. Key Considerations re: Miller et al. (2021)

4.2.1. Diagnostic Issues: Trauma vs Autism vs. Learning Disability? Ronan met the criteria for Autism Spectrum Disorder (ASD; American Psychiatric Association, 2013), moderate to severe. This diagnosis would indicate a possible co-occurring learning disability (IQ below 70 with limited language/verbal communication); clarity on this is needed to understand

the likely role of the treatment or whether the child was also developmentally delayed as a result of childhood attachment trauma.

Like the Critic, we are particularly concerned about the role of trauma in Ronan's history, either as a competing explanation for his repetitive behaviors and communication difficulties, or as an amplifying process that made his difficulties worse. To begin with, Ronan's aunt believed that there was neglect, that he had often been left alone in a room. The absence of early human connection and relational attachment bonding, as was the case with the Romanian orphanage scandal, can result in children developing a set of features sometimes referred to as "quasi-autism" (Rutter et al., 2007): self-stimulating behaviors, lack of language development, and failure to seek human interactions for comfort. In addition, to date there has been little attention to the possibility of the dual diagnosis of autism and trauma. As a result the prevalence of trauma and related symptomology in ASD is unknown, but there does appear to be an overlap. There is however, growing recognition that individuals with ASD are at increased risk of experiencing traumatic childhood events and of being detrimentally affected by them (Kerns et al., 2015).

In Ronan's case, a trauma diagnosis could not be discounted based on the evidence presented; this has implications for the stability of an autism diagnosis. Longitudinal studies tracking the outcomes of adopted Romanian orphans (Sonuga-Barke et al., 2017) have reported that placement into high-quality foster care after infancy but before the age of 2 is associated with a dramatic decrease in the prevalence of repetitive behaviors by age 5 (Zeanah et al., 2005). If there is trauma and not autism, you would expect to see a similar symptom presentation but rapid development when provided with a relationally secure environment and appropriate nurturing intervention.

A similar argument could also be made for fetal-alcohol syndrome, or additional co-occurring learning disability (IQ below 70) more generally. Questions requiring further consideration might include: What is a fetal-alcohol related presentation? What is autism? What is "severe autism"? The latter diagnosis often indicates a learning disability co-occurring with autism. At a minimum both trauma history and learning disability are likely to be important moderator variables (i.e., complicating factors) that are likely to decrease generalizability of the results of this case to other young clients on the autistic spectrum.

4.2.2. What Was the Intervention? We have already discussed the complexity of the DIR/Floortime intervention and uncertainties about how it was implemented in this case study, making it difficult or even impossible to attribute Ronan's changes to his sessions with Kristin Mount. However, beyond this, his home environment changed just prior to the center intervention being implemented, which is key and cannot be ignored as a factor in the developmental gains Ronan displayed. As Michele Fouts, the supervisor and designer of the local application of the Floortime model, stated in cross-examination, "You can't just have a therapeutic center to see huge gains. You also have to have a safe, stable, responsive home" (p. 166).

In conjunction with the stable and secure home setting is the provision of additional support systems that would have been put in place to support children with a diagnosis of ASD. We would want to ascertain what other additional interventions Ronan received as part of his ongoing treatment plan. We would expect that he received a range of supportive interventions, which would complicate further the causal claims made for Mount's relational approach. These might include the following:

- Speech & Language Therapy would assess Ronan's communicative profile and, if required, would provide him with an augmentative communication system, such as Picture Exchange Communication System (PECS; Bondy & Frost, 1994).
- Occupational Therapy would assess Ronan's adaptive functioning on a range of adaptive skills, but more importantly would conduct a Sensory Profile Assessment. This would pick up hypo-and-hypersensitivities, in order to identify whether there is an additional Sensory Processing Disorder (SPD).
- If Ronan did have an additional SPD this would ideally lead to Sensory Integration Therapy, another effective intervention for autistic children (Schoen et al., 2019).
- Further, we would want to understand whether intervention programs that operate through adaptations to the environment were provided at the center. For example did the Center follow the structured teaching principles of Treatment and Education of Autistic and Related Communication Handicapped Children (TEACCH; Schopler et al., 1982)? These principles follow a low arousal approach promoting visual clarity which structures visually both teaching tasks and navigation tasks.

These are just to name a few approaches that might be used to support the profile of autism; we would want to know the extent that Ronan had access to these and other approaches.

4.3. Proposals for Systematic Case Study Research on Emotion-Focused Group Therapy for Individuals on the Autistic Spectrum

Turning to a different population of individuals on the autistic spectrum, Robinson and Elliott (2017) have developed a small group humanistic-experiential Emotion-Focused Therapy for young people and adults with autistic process (EFT-AS). This therapy is most appropriate for autistic clients who wish to engage in a talking therapy with others and to focus on self-reflection, especially if they have had trauma-related experiences through isolation, rejection and painful victimization as a result of being autistic in a predominantly neurotypical world (Robinson, 2018). EFT-AS involves self-empathy and interpersonal empathy tasks and uses video-assisted IPR (Interpersonal Process Recall) to help clients slow down and attend to their experiences during interactions with others. Miller et al., (2021) has inspired us to imagine what systematic case study research might look like if applied to this approach.

4.3.1. Client Assessment. If we begin with the client, then first we would want to describe the client presentation in considerable detail. Clarifying the key aspects of the client's presentation would support the knowledge claim that the client met diagnostic criteria for autistic spectrum disorder as well as specifying the client's individual pattern of differences and the

associated level of distress. This would also aid the generalizability of results as well as providing information about the client's baseline functioning and key areas of functioning to track. For EFT-AS this would include a number of specific assessments: diagnostic screening (diagnosis clarification and clinical status); Autism Quotient (Baron-Cohen et al., 2001); primary outcome measures (including Empathy Quotient (Baron-Cohen & Wheelwright, 2004); Alexithymia (TAS-20, Parker, Taylor & Bagby, 2003); Emotion Regulation Questionnaire (ERQ; Gross & John, 2003); and individualized outcome measure (Personal Questionnaire; PQ; Elliott et al., 2014).

4.3.2. Treatment Integrity. Treatment integrity is the issue of whether the intervention was delivered as specified in published descriptions (treatment manuals), and competently. This is a key but all too often neglected issue in systematic case studies. Treatment integrity can be enhanced through training and supervision, and can be measured using instruments such as the Person-Centered and Experiential Psychotherapy Scale (PCEPS; Freire et al., 2014), but adapted for EFT-AS and rated by independent raters.

4.3.3. Outcome. In assessing client outcome, it is always useful to assess general psychological distress (e.g., as measured by the CORE Outcome Measure; Barkham, et al., 2013), along with individualized presenting difficulties (e.g., Personal Questionnaire; Elliott et al. 2014). In addition, a key outcome for EFT-AS is helping clients with autistic process develop empathy for self and others while working through their emotional injuries from previous difficult or traumatic experiences. For this, we would use a within-session process measure of outcome, specifically the Client-Emotional Processing Scale for Autism Spectrum (CEPS-AS Robinson & Elliott, 2016). This observer instrument measures emotional processing within four subscales (emotion-regulation, empathy, self-reflection and mental representation) using a five point process rating scale. Raters would be trained in the CEPS-AS to assess change on the four subscales comparing client performance in early versus late sessions.

4.3.4. Change Processes. Qualitative data collection instruments would be used to capture client experiences of therapy, on a session level using the Helpful Aspects of Therapy (HAT) form (Elliott et al., 2006). The treatment model also includes an ending session in which clients are asked to reflect on their experiences across treatment and exchange views on self, others, and helpful and hindering aspects of therapy. A follow-up interview post therapy would provide a further opportunity to ascertain if the therapy had any impact on client change process. These data would be particularly useful for carrying out task analyses to create change process models for different kinds of important process in EFT-AS, models that can be tested in later research (see Robinson, 2018; Robinson, 2020).

For example, clients with autistic process report having difficulties understanding their own and other people's emotions as well as being misunderstood by others. The concept of misempathy is a recurring theme, as evidenced by reports that autistic people lack empathy (Baron-Cohen, 2005), whilst others claim there is a mismatch between neurotypical and neurodivergent forms of intersubjectivity (Robinson et al, 2020). EFT-AS supports neurotypical-neurodivergent intersubjectivity (therapist-client) and neurodivergent-neurodivergent

intersubjectivity (client-client) using both self-empathy and interpersonal empathy tasks as a means for accessing and working through cognitive and affective empathy (Robinson & Elliott, 2017).

5. PARTING THOUGHTS: LIMITATIONS/EVALUATION

Having reflected on the two studies in this special section, we are left with several key take-home points. First, if adjudication methods were to be more widely adopted, we wonder about the possibility of strong personalities and polarized political positions dominating or distorting the process. Gadamer (1979) wrote about unforced consensus through open dialogue, but that requires all judges or jury members to take part in the process with empathy, flexibility and open-mindedness. This shifts the burden to the researcher in designing the make-up of the panels of people who are being asked to pass judgement on the knowledge claims.

Second, as we saw here, conceptual clarity regarding knowledge claims matters. Muddled knowledge claims lead to muddled or unsatisfactory findings. Why not separate out change versus causality claims? Isn't this a principle of civil law: separately evidencing (a) harm and (b) the causal role of the complainant in the harm?

Third, we think it is time to consider applying standards of good practice to systematic case studies. We had hoped in this commentary to pilot test Benelli et al.'s (2015) 28 recommendations for HSCED research as a set of standards for evaluating systematic case study research. Unfortunately, we ran out of time, energy and journal space before we could do this, so it remains for now on our to-do list.

In conclusion, we are pleased to see the further evolution of systematic case study methods. We look forward to following future developments in one of our favorite research genres, valued for its blend of lived clinical practice, careful scientific inquiry, and openness and creativity.

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