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Caveat emptor, caveat venditor, and Critical Incident Stress Debriefing/Management (CISD/M)

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Abstract
Mitchell (2004) and Robinson (2004) have expressed concerns regarding our recent article on debriefing (Devilly & Cotton, 2003). In this article we respond to their concerns, some scientific, some sociopolitical, and provide further substantiation regarding our conclusions. We conclude that CISD and CISM are indistinct approaches to trauma and should be treated as synonymous terms (CISD/M) until the necessary and sufficient elements of each are fully declared. Furthermore, based upon current evidence, we restipulate that CISD/M is an ineffective response to critical incidents for individuals, and that organisations need to revise their critical incident response policies to reflect the current weight of scientific evidence. There are currently no reliable studies demonstrating the efficacy of group debriefing.

Our recent article on CISD/M (Devilly & Cotton, 2003) has generated anticipated concerns from key advocates of this intervention (Mitchell, 2004; Robinson, 2004). We are delighted to have this opportunity to respond to their comments as it has always been our intention to raise critical debate on an intervention that appears, until recently, to have been implicitly accepted as efficacious, notwithstanding the lack of good quality research available. In essence, these authors claim that our article:

1. Confuses Critical Incident Stress Debriefing (CISD) with Critical Incident Stress Management (CISM).
2. Does not explain the results found by Flannery (1998) and the application of his Assault Staff Action Program (ASAP).
3. Inaccurately refers to Everly and Mitchell as directors of the International Critical Incident Stress Foundation (ICISF), and makes the innuendo that the results of any conclusions made by these authors are, therefore, biased.
4. Misattributes poor outcome from some RCTs to the CISD/M intervention rather than injury severity at intake.
5. Is "imprecise", "clinically and academically illegitimate", replete with "unsubstantiated and unscientific rhetoric" and "gives the impression that both peer and editorial review processes were inadequate" (Mitchell, 2004), and that "the argument for randomised controlled trials [upon which we based our arguments] as the only acceptable or desirable way to acquire knowledge is misguided" (Robinson, 2004).
6. Is inaccurate in suggesting that CISD/M organisations promote their services by referring to legal cases, and that our article did not offer any evidence that this does, in fact, occur.
7. Promotes "psychotherapy and mental health professional control" (Mitchell, 2004), yet at the same time promotes a "wait and screen for impact" approach (Robinson, 2004).
8. Alleges "unethical and bad practices without substantiation" (Robinson, 2004).

These claims are indeed sensational and worthy of close, dispassionate and reasoned inspection so as to judge their veracity. It is unfortunate that many of the points for which the authors demand substantiation are political in nature rather than of scientific

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interest. However, this process may, at least, provide a sociopolitical context for the evidence-based conclusions that we reached. We will address these claims in the space we have available to us.

In our original article we drew attention to the lack of operational definitions offered for CISM and also noted that Everly, Flannery, and Mitchell (2000), in a meta-analysis of CISM, included studies that only referred to, and utilised, CISD. We reiterate again here: one cannot have it both ways. Either CISD is different to CISM or it isn't. One cannot use CISD studies to back the efficacy of CISM if they are different interventions or comprise just one aspect of an intervention—not even by referring to one with such reassuring terms as "multi-component", "systematic" or "comprehensive". Indeed, in Mitchell's own reply (2004), he states that CISM "includes many tactics and techniques, but *is not limited to*" (emphasis added) and then progresses to list a host of services including feeding work crews, group crisis intervention and "*many other services*" (emphasis added). Yet again, even when replying to our charge that CISM has not been made logically distinct from CISD and has not been scientifically defined, both Mitchell and Robinson present CISM as possessing an unlimited number of features, provide an open-ended, infinite conjunction of interventions as constituting CISM and still do not offer the necessary and sufficient conditions as to what approximates CISM. This procedure is, therefore, unfalsifiable and is best regarded as pseudoscientific. Robinson, however, claims that our treatment of CISD and CISM is like comparing systematic desensitisation to cognitive-behaviour therapy. We reject this comparison. CBT has a large history of component analysis, investigating which strategies are useful, under which conditions and for whom. For example, relaxation training is very effective in the treatment of generalised anxiety disorder (Borkovec & Costello, 1993) yet is so unconvincing in the treatment of another anxiety disorder, PTSD, that it was recently used as a psychological placebo in a treatment outcome study (Marks, Lovell, Noshirvani, Livanou, & Thrasher, 1998).

Indeed, later in Mitchell's article (2004) he refers to Flannery's ASAP intervention as "clearly demonstrating that CISM, *even called by another name*, is a successful crisis intervention program" (emphasis added). It appears that anything involving intervention for distressed or victimised individuals could be considered CISM as it is currently defined (or, rather, not defined). To provide an analogy to demonstrate our point, one could think of CISD as a soft drink, say Coke™, and CISM as a copyrighted variety of drink that may or may not include alcohol, may or may not be liquid in form, and may or may not have Coke™ as one of the ingredients. One can only imagine how harshly such an approach to marketing would be responded to by drink manufacturers generally.

This brings us onto the claim by Mitchell (2004) that Flannery's ASAP intervention (1998, 1999) is, in fact, another name for CISM. For those who wish to know more about this intervention than is written "on the back cover" of Flannery's book and how it has been used by Mitchell to support CISD/M, we will attempt to explicate matters further. Flannery's ASAP intervention is a collection of strategies to be implemented in a State Hospital (where Dr Flannery works as a senior psychologist) to reduce absenteeism following assaults by patients, to provide support for the victims of assaults, and to also reduce the frequency of such assaults from occurring in the future. Contrary to Mitchell's claims that ASAP is another name for CISM, Flannery states that "ASAP is NOT the same as CISD/M. ASAP is a type of CISM approach only in that it uses multiple interventions. ASAP utilises interventions that are different from Dr Mitchell's CISD/M" (Flannery, personal communication 3rd November, 2003). We are puzzled by Dr Mitchell's interpretation of ASAP and the combining of different programs and outcome variables to support CISD/M. Interestingly, Dr Flannery is also listed as a member of the faculty for the ICISF. However, Flannery also states that "while I occasionally teach a course for ICISF, the ASAP work has not been done at ICISF in any way".

It is also claimed that we were factually incorrect in describing Everly and Mitchell as directors of the ICISF and that we make the innuendo that such authors "cannot be trusted because they developed the program". We stick by our original claim regarding directorship and will attempt to explain here, again, why meta-analyses should not overly rely on one research group's results and conclusion. Dr Mitchell is indeed listed as "President Emeritus" of the ICISF Board of Directors and Dr Everly is listed as "Chairman of the Board Emeritus" of the ICISF Board of Directors on the ICISF website as of 10th November, 2003. Recently, Dr Mitchell sent an almost identical letter of complaint to his current one (Mitchell, 2004) to the journal editor of *Prehospital Emergency Care* regarding Dr Bledsoe's analysis of CISD/M (Bledsoe, 2003) and signed his letter as "President Emeritus of ICISF" (Bledsoe, personal communication 19th November, 2003). We are, therefore, confused why Mitchell would state "Dr Everly and I are not the directors of ICISF — Please, let's everyone stick to the facts" (Mitchell, 2004).

However, a more substantive and scientifically important concern we have relates to the inherent problems of relying nearly exclusively on the producer of merchandise to also act as the evaluator of that merchandise. This is not to say that the producers are "untrustworthy", but rather may suffer from researcher allegiance effects (Gaffan, Tsaousis, & Kemp-Wheeler, 1995), particularly where the research study was not a blind randomised controlled trial (see Rosenthal & Lawson, 1964, for just one example of experimenter effects on outcome). We are confident that, to again draw on our soft drink analogy, the public would not feel much confidence in the results of a published study where the authors were the makers of Pepsi™ and where the "Pepsi-challenge" was investigated. Likewise, how confident would the public be about a new drug that had only been positively evaluated by the pharmaceutical company that invented, manufactured, sold and held the copyright licence to the drug? We are unclear, from either Mitchell or Robinson's reply, as to the reasons why psychological interventions should be held to lower standards.

Which brings us to the use of randomised controlled trials (RCTs) and the evidence we brought to bear on this issue. Mitchell claims that we have misled the field with our evidence, while Robinson claims that it is possibly unethical to conduct RCTs on debriefing anyway, and that this method of evaluation is not able to assess the "complexity" of human interactions and CISD/M. Robinson also claims that if one "*believes*" debriefing to be harmful then providing it in a RCT is unethical, while if one "*believes*" it to be beneficial then not providing it is unethical. We hope to show below how these arguments are specious. We also fail to see the relevance to the current debate of Robinson's example of a case study into EMDR, and so do not address this unusual comment.

The use of the RCT in psychology has been increasingly advocated, and used, since Eysenck's seminal meta-analysis into psychotherapy in 1957. Eysenck looked at a 2-year response to treatment based upon whether people had received psychoanalytic treatment or eclectic intervention for "psychoneurosis". While the method of investigation was rather primitive (e.g., subjective appraisals from clinicians of "cured, or much improved; improved; slightly improved; not improved, died, discontinued treatment) the results suggested that those who did not receive any treatment or just visited their GP were the most "improved", leading to the conclusion: "There appears to be an inverse correlation between recovery and psychotherapy; the more psychotherapy, the smaller the recovery rate" (Eysenck, 1957). Hence, the need for RCTs (and, incidentally, more tightly controlled meta-analyses) before the implicit acceptance of an intervention became of paramount importance, culminating in the establishment of the Cochrane Collaboration in 1993. This collaboration attempts to provide a repository of knowledge by conducting high quality meta-analyses into interventions being offered (medical and psychological) so as to advise on the applicability of various treatments for various presentations. While some of the early RCTs have been reasonably criticised for not assessing debriefing as it is typically practised, it is misleading to dismiss an accumulating body of high quality research that consistently shows the lack of efficacy of debriefing, and worse, that it may actually be harmful to a subgroup of individuals.

In our original article (Devilly & Cotton, 2003) we gave a brief précis of the methodology and results obtained by The Cochrane Review into debriefing. Since going to press this review database has been further updated (now incorporating 11 studies) and these English reviewers conclude: "There is no current evidence that single session individual psychological debriefing is a useful treatment for the prevention of post traumatic stress disorder after traumatic incidents. Compulsory debriefing of victims of trauma should cease" (Rose, Bisson & Wessely, 2003). As we originally mentioned, the thrust of this sentiment was also shared by the Dutch reviewers van Emmerik, Kamphuis, Hulsbosch, and Emmelkamp (2002)—the results of which were further commented upon by Gist and Devilly (2002)—with a specific reference to the negative effects of CISD. A recent review by an international group also recommended that "for scientific and ethical reasons, professionals should cease compulsory debriefing of trauma-exposed people" (McNally, Bryant, & Ehlers, 2003, p. 72). So important is the need to redirect our efforts in this area, that the American Psychological Society has made this comprehensive analysis of the "debriefing debate" available online at http://www.psychologicalscience.org/journals/pspi/. We make reference to these reviews to assure the reader that we are by no means voices in the scientific wilderness and are puzzled by the accusations of being "clinically and academically illegitimate" or even "imprecise". It is becoming clear that "belief" may indeed be a dangerous emotion when coming to judge the effectiveness of an intervention. In most cases, Mitchell and Robinson ask us to split increasingly finer rhetorical hairs as if this will somehow negate the overall direction of the data from published RCTs and well partitioned field studies; in other cases, they implore us to allow poor quality studies from in-house journals to "trump" published reports from first-tier scientific journals. They attempt in several spots to retreat from clear proclamations made in earlier articles and manuals regarding issues such as individual versus group formats ("one-on-one" encounters have always been endorsed), vicarious traumatisation by the debriefing experience (belied in the prescription to always "debrief the debriefers"), and the single-session application of CISD (still the dominant application from "trained" practitioners). None of these quibbles negates what has become a very robust, if initially somewhat surprising, set of findings.

For example, when discussing Mayou, Ehlers, and Hobbs's (2000) 3-year follow-up of a RCT into debriefing of car accident victims, Robinson (2004) claims that "the intervention group also had a higher mean injury severity score and longer hospital stay" than the non-debriefed group and that we "fail to acknowledge the possible role of severity of injury". We believe that Robinson has misunderstood the science underlying this study. Yes, when "type" of injury was assessed the intervention group had more severe injuries to their extremities, but there was no difference between the debriefed and non-debriefed groups in the proportion of participants who had high initial scores on injury severity, and there was no significant difference between the two groups on symptom severity at intake as assessed by the key outcome measures (the Brief Symptom Inventory, Derogatis 1992; Impact of Event Scale, IES, Horowitz, Wilner & Alvarez, 1979). In analysing the data, Mayou et al. checked whether injury severity or length of hospital stay could account for the higher IES scores in the debriefed group through an Analysis of Covariance and found this not to be the case. It is possible, however, that Robinson is being confused by the main, and somewhat alarming, result of this study with a statistical artefact. The main result was that, while there was no difference at 3-year follow-up between the debriefed and non-debriefed groups on trauma symptoms, when one looked at those who were the most *severely symptomatic* at intake, across *both* conditions, those who received debriefing did worse. To quote the original study: "Patients with high scores who received the intervention still showed symptoms, whereas those who did not receive the intervention improved and had scores comparable with those of patients with low initial scores" (Mayou et al., 2000).

Incidentally, this study also found that at follow-up those who were debriefed were more likely to have higher anxiety, depression, obsessive-compulsive and hostility scores, enjoyed being a car-passenger less, reported more severe pain, had more chronic health problems, had greater financial problems as a result of the accident, and reported a lower quality of life than those who were not debriefed—*even when appropriately co-varying for injury severity*. Such results are not unique in the debriefing literature and are extremely troubling. If anything they add weight to the need to conduct RCTs into untested practices—particularly by less compromised parties. Such studies do, however, raise the issue of "how" such a brief intervention could have such a toxic effect—a point that researchers are currently trying to address.

Further, both Robinson and Mitchell appear unconvinced that: (a) debriefing is a multi-million dollar industry; (b) that organisations (as opposed to those who "self-activate") build their business upon the back of such events; and (c) this industry uses tactics other than independent, evidence-based practice to infiltrate businesses. Both authors demand substantiation of these claims. So, let us consider the following: after the attack on the World Trade Center in New York, upwards of 9000 grief and crisis counsellors flooded the streets and therapy rooms of New York (Kadet, 2002); 28% of Americans throughout the country were offered counselling following this attack by their employers (Kadet, 2002); 30,000 to 50,000 people are trained by the ICISF every year (McNally, Bryant & Ehlers, 2003); to attend an ICISF one day training workshop[1] in "CISM: Individual Crisis Intervention" at the monthly ICISF conference currently costs US$228; in a recent ABC radio Background Briefing broadcast Mitchell replied to debriefing critics by stating "we've had the busiest year this year in training people to do this stuff, than we ever have in history. So hey, keep up the good work boys and girls, because what you're doing is you're just making people curious enough to come and take the training" (Latham, 2003); and at the last 7th annual ICISF "World Congress" in Baltimore the keynote speaker, Dr Reese (a faculty member of ICISF since 1995; Reese, 2003), facilitated a small group discussion entitled "Getting The Boss To Buy Into CISM". In light of these figures, we do not believe that anyone can seriously claim that debriefing is not a multimillion dollar industry, even for just one single company—ICISF. Mitchell demands substantiation of our claim that this is such a thriving business. From our research, the ICISF has increased sales every year from 1997 to 2001 as declared in their "return of organisation exempt from income tax form (Form 990)". In 2001, the declared total revenue of the ICISF was over US$1.88 million, of which both Mitchell and Everly were paid US$122,600 each (as president and CEO Emeritus, respectively). Of course these costs count as deductions and together with US$412,648 in speaker fees, US$203,815 in printing costs, US$66,000 in travelling expenses, US$257,198 in room costs, and US$30,408 for outside services, the ICISF reports an excess of only $107,730. It is also interesting to note that in these tax returns the term "CISD" is used as the explanation of the tax exempt activity—never CISM. Considering the number of companies that provide debriefing services around the world we would not be surprised if the figure for the industry actually ran into hundreds of millions of dollars (Australian or American). We have no problem with people making money and charging for services, but we do have problems when that very same service may be harmful.

In fact, besides teaching conference delegates how to entice "the boss to buy into CISM", Employee Assistance Programme providers sometimes explicitly refer to the Howell legal precedent in Australia (Innovative People Solutions, 2003)—the implications being self-evident. Indeed, Mitchell claims that our reference to the ICISF citing the Howell case "is a blatant misrepresentation of the facts" (p. 26). Upon further checking it appears that it was, in fact, their Australian sister organisation the Critical Incident Stress Management Foundation of Australia who made reference to this case in replying to the justifiable concerns expressed by Bledsoe (2002). In an internally produced article (available on the web) their President, Dr Robyn Robinson, states: "In Australia, at least, there have been two legal suits awarded to employees against employers (both for $AU750,000) for failure to provide adequate psychological support (the Howell and the Seedsman cases). There have been other cases before the courts for failure to provide services but, to my knowledge, no cases put forward because debriefing WAS offered" (Robinson, 2002, p. 5). While the authors (Devilly & Cotton, 2003) made no claim to offering a "legal opinion", precedent setting findings are indeed emerging. For example, Mr. Justice Owen's detailed judgement in *PTSD Group Claimants v. Ministry of Defence* (UK) includes an entire section devoted to the issue of psychological debriefing in which he concludes, after exhaustive evaluation of evidence, that debriefing cannot be supported as an effective approach to prevention or intervention and dismissed the claimant's generic pleading on that element.

Furthermore, at a recent North Atlantic Treaty Organization (NATO)—Russian workshop on terrorism, the general agreement was that:

> . . . there is still no consensus on the role, if any, of very acute interventions. Classic CISD debriefing can no longer be recommended. The balance between getting people to talk to people, and getting people to talk to professionals, has not been established. (NATO, 2002)

Which takes us onto the somewhat antithetical claims that we are (a) advocating a wait-and-see approach to providing support following trauma (Robinson, 2004), and at the same time (b) professionalising and pathologising reactions to acute incidents (Mitchell, 2004). We are unclear as to how these very different conclusions can be reconciled with our original article. We advocated an approach which is un-invasive and suggested the utilisation of respected organisational veterans in the support process. Indeed, we even emphasise that "the type of social support referred to here is the (nonclinical) everyday expression of care" (p. 148). Those who go on to develop longer-term pathological reactions should be seen by a professional trained in individualised formulations and one that is aware of the current evidence pertinent to that condition. We do not advise that a series of weekend workshops is a sufficient grounding in mental health to adequately provide services to those affected by more severe presentations (e.g., depressive disorders and posttraumatic stress reactions).

However, if these authors see us as advocating that debriefing-type services should be open to evaluation by experts in post-trauma reactions and that the techniques should be operationally defined, differentiated from other interventions and open to peer review, then we agree that this is one of our goals. It is in this light that we are perplexed by the claim that both the editorial and the peer-review processes preceding the publication of our article were inadequate (Mitchell, 2004). Our article did, in fact, go through a revision following reviewer comments prior to publication. However, this does raise an interesting point. In replying to our article both Mitchell and Robinson frequently recourse to citing either articles published in *The International Journal of Emergency Mental Health* or chapters of books published by the Chevron Publishing company. *The International Journal of Emergency Mental Health* is, in fact, a publication of the Chevron Publishing Company (of Ellicott City, Maryland), that was initiated to promote the growth of CISM. Chevron Publishing is a proprietary enterprise begun by the principals of the International Critical Incident Stress Foundation (ICISF), the organisation created by Mitchell and Everly. Chevron Publishing produces paperback manuals and books on CISD related topics and markets other CISD-related titles and merchandise such as ICISF baseball caps, CISM jacket pins and ICISF-logo jackets. Our purpose for drawing attention to this point is to provide a context within which to appraise the independence of the peer-review process that is usually associated with scientific journals and associated texts, such as the *Australian Psychologist*, when compared to the outlets that Mitchell and Robinson prefer to cite.

In summary, nothing in the responses by Mitchell and Robinson causes us to alter our position in any way. Indeed, we find their approach to the empirical status of debriefing to be unusual. To use the words of Dr Richard Gist, "The jury has returned the verdict on efficacy. The trial is over. And the question of efficacy is pretty much settled for everyone but the intervention marketeers'' (personal communication, November 2003). We are uncomfortable that the debate is shifting from a focus on reviewing and evaluating the evidence to what we can only characterise as emotionally charged defences of ideological positions. Our view is that we need to maintain a focus on the key practical issues: how can such a seemingly benign intervention interfere with resolution to such degrees as reported in some studies, and how should responsible employers assist individuals exposed to critical incidents? There are still aspects of this last question that remain to be further clarified, but it is now clear that, at least for individual approaches, it is not CISD/M.

Hence, while the buyer should beware (*caveat emptor*) when buying debriefing services, the evidence of a defective product is mounting to the point where it may be time for the seller to beware (*caveat venditor*).

Endnote

1 We provide further substantiating evidence for our claims at: http://www.swin.edu.au/victims

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