

Rehabilitation for torture survivors: Six evidence myths and their implications for future research

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Abstract

Whilst it is established that torture survivors suffer from complex, multiple and often severe and enduring physical, psychological, social, welfare and many other difficulties; and that rehabilitation as reparation should be holistic, interdisciplinary and specialist, majority of the research on rehabilitation focuses increasingly and almost exclusively on psychological interventions. Further, assumptions that this research provides evidence of which are effective psychological interventions may underpin and skew services funded and provided to torture survivors. In this paper we challenge some of those assumptions, and discuss the conceptual, theoretical, epistemological and methodological limitations of this research and implications for future research.

Introduction

What is rehabilitation as reparation for torture survivors? At the heart of that question is the fundamental issue of what can restore a ‘sense of being human’ to someone brutalised by torture, from whom it has been stripped (Patel, 2019a). What is meant by the sense of being human is fluid, multifaceted and diverse; it is historically, geographically, politically, socially and culturally contextualised. What, then, should rehabilitation entail, and how do we meaningfully establish the

effectiveness of rehabilitation activities and interventions for torture survivors?

Rehabilitation, as a form of reparation for the human rights violation of torture, is defined in the United Nations Convention against Torture’s General Comment number 3 on article 14 (‘General Comment’) as “the restoration of function or the acquisition of new skills required as a result of the changed circumstances of a victim arising from torture or ill-treatment” and as seeking “to enable the maximum possible self-sufficiency and function for the individual concerned, and may involve adjustments to the person’s physical and social environment. Rehabilitation for victims should aim to restore, as far as possible, their independence; physical, mental, social and vocational ability; and full inclusion and participation in society.” It is a definition, arguably, which does not go far enough – or more specifically, if it is taken out of the context of the entirety of the General Comment, it would be easy not to recognise that rehabilitation as reparation is more than health: it is a part of redress.

By contrast with these aspirations in international law, many rehabilitation services for torture survivors and research current in this area focus predominantly on psychological interventions as ‘treatment’ (see Patel, 2020 for a fuller review), and start with a Western framework of psychopathology (to be ‘treated’), as-

sessed by Western standardised instruments, and offering Western-developed solutions, usually individual, and rarely contextualised by culture, political or religious perspectives, or by history. Assumptions on the part of service providers and funders, about what is and is not evidence of effective treatment, skew the conceptualisation of what is available and what should be practiced as rehabilitation. In this paper we challenge what we see as the most important of those assumptions.

Our systematic reviews and meta-analyses (Patel et al., 2014; Hamid et al., 2019; see also Patel et al., 2016) found relatively few studies (restricted to randomised controlled trials); all were of individual psychological ‘treatment’, usually compared to ‘no treatment’, and primarily aimed at reducing post-traumatic stress disorder (PTSD), a psychiatric disease category. Benefits of these interventions were few and weak; confidence intervals were wide and methodological biases common, undermining confidence in estimates of ‘treatment’ effects. These conclusions were considerably less optimistic than those of other widely cited reviews of the field (Campbell, 2007; Crumlish & O’Rourke, 2010; McFarlane & Kaplan, 2012; Nicholl & Thompson 2004; Weiss et al., 2016), generating discussion (Pérez-Sales, 2017, Williams, 2017). The reviews and the editorial that discussed their major differences (Pérez-Sales, 2017), and other examinations of the body of research in this field (Montgomery and Patel, 2010; Jaranson and Quiroga, 2011; Carlsson et al., 2014) strongly suggested that reviews, and their constituent trials, were being cited by clinicians and researchers without the critical appraisal common in many other branches of health-related evidence. This is not to disregard the considerable practical problems of conducting research on interventions to improve well-being in torture sur-

vivors, nor the difficulty of obtaining scarce funding to do so, but to urge better use of those efforts and resources to build more robust understanding in the field. We therefore address what appear to us to be common myths and misconceptions about psychological research findings and evidence, and make a series of practical proposals to use both in evaluating existing psychological and psychosocial research with torture survivors and in designing future studies.

Evidence myths

Myth 1: Good evidence is research-based

The ubiquitous influence of what has come to be known as evidence-based practice in healthcare, and the many critiques of it (e.g., Kerridge, 2010; Miles, 2009), are relevant to rehabilitation services. However, evidence-based practice, as an approach to the evaluation of the best available research evidence and its application to rehabilitation for torture survivors, is limited in at least two ways. First, rehabilitation is not only ‘clinical’ or health practice, but evaluation focused only on physical or psychological health can only ever be that, and cannot be extrapolated as evidence for holistic rehabilitation (social, welfare, legal, educational, vocational etc.). Second, in applying the methodology of evidence-based practice, initially developed in biomedicine, to rehabilitation of torture survivors, the cultural limitations, or specificity of the research methods, of the underlying ethics and of the interpretation of the evidence, lead to the risk of decontextualising research findings, and ignoring their contextual specificity, or limitations – all of which are obscured in the unquestioning, general application to rehabilitation practice.

The ambition of evidence-based practice approaches - to ensure equitable access to

the best available healthcare interventions in order to maximise positive outcomes and minimise harm - is laudable. Yet evidence-based practice relies on hierarchies of evidence, constructed within realist epistemologies and positivist methodologies for evidence-generation, and the criteria for judging 'quality' or 'best' evidence are narrowly defined by such hierarchies. Other methods of evidence-generation, which value different types of evidence, are overlooked, leading to a hegemony of a particular (Eurocentric and positivist) research and evidence. Much evidence on what is helpful to torture survivors, their families and their communities, their different contexts, is to be found within the wisdom of families, within communities, within the work of civil society and community-based organisations - which may not adhere to the hegemonic discourse of evidence-based practice, but may have significant and weighty contributions to understanding what helps, and what is valued by those communities.

In making assumptions about what is 'good' evidence, based on Eurocentric epistemologies and methodologies, we generalise findings of specific types of research, with specific and narrowly-defined populations, to all torture survivors, and we advance and impose rehabilitation services which are based on our hierarchies of 'good evidence', and our narrow understanding of what is rehabilitative, ignoring the lived realities, wisdom and diverse experiences of survivors and their communities.

In summary, basing interventions, rehabilitation services and the funding of those services on unexamined assumptions of 'evidence-based practice' leads to epistemic injustice (Spivak, 1998), if not to other harms to torture survivors and their families and communities - the very thing which the evidence-based practice approach seeks to prevent.

Myth 2: All research leads to quality evidence

For years, narrative review was the only way of summarising evidence in health research. All methods were undeclared and nonreplicable, from search and selection of eligible studies to integrating findings, drawing conclusions and making recommendations. This often led to an array of reviews, apparently of the same area, drawing on overlapping bodies of evidence, but weighting that evidence differently and drawing different conclusions. Thus, as a set, these reviews presented an extensive, if not comprehensive, account of past research endeavours, but could not achieve consensus on the implications for people currently receiving health treatment/care. Users of those reviews - health practitioners, funders, commissioners of services - likewise made undeclared selections from the reviews according to what suited their purposes and preferences.

From this chaos arose the movement for systematic reviewing, initially of quantitative studies, using transparent and replicable methods that could cumulatively build a high-quality body of evidence. One of its strengths was combining data from similar studies such that they gave an estimate of size of effect (treatment benefit), rather than a majority view as in narrative reviews. Another was that it took account of weaknesses in research methodology that could bias results or conclusions. For instance, a common bias in psychological intervention studies is to use multiple outcome instruments to quantify, for instance, changes in mood or particular psychological symptoms, and then to report only those (often a minority) that showed satisfactory change with treatment. A second common bias particularly relevant to the area of psychological interventions is that of small size studies, which tend to produce more extreme results. Since those with positive results are more likely to reach publication than those that show no

change or worsening, those small studies tended to inflate the apparent benefits of psychological interventions in narrative reviews and even some systematic reviews (Dechartres et al., 2013), although in the latter case the bias would be noted and lower confidence assigned to the estimate of treatment effects.

The Cochrane Collaboration developed a range of systematic review methods (<https://cochrane.org>), initially for quantitative research studies, including identification of particular biases and ways of quantifying their detrimental effect on certainty in overall findings; Cochrane reviews are seen as the hallmark of reliability (Chalmers & Altman, 1995) for quantitative research. However, the quality of systematic reviews varies (Dechartres et al., 2013, Maassen et al., 2020), and evidence-based medicine in general has been criticised for discounting valuable clinical experience in deciding on the best treatment for each individual patient in favour of algorithms, and failing to incorporate patients' values or even to involve them in decisions (Kelley et al. 2015), criticisms also highly relevant to psychological practice. Findings about how well treatments work, on average, become rules applied to all, supplied in guidelines that constitute valuable material for bureaucratic and managerial medicine (Greenhalgh et al., 2014). Greenhalgh and colleagues (2014) argued "for a return to the [evidence-based medicine] movement's founding principles—to individualise evidence and share decisions through meaningful conversations in the context of a humanistic and professional clinician-patient relationship".

In the field of rehabilitation for torture survivors, the majority of studies conducted and reported are psychological, reviewing psychological 'treatments', using quantitative research methods, adopting the framework of evidence-based medicine and applying it to

psychological interventions for torture survivors. From the broadest review level to the smallest individual study of effectiveness of an intervention, there is an assumption that both problems and outcomes can be adequately quantified. In psychology, self-report is the near-universal quantification method, using 'standardised' questionnaires that arise from the same roots as clinical diagnoses, and validated against them. Yet there are numerous weaknesses at each stage of that process, each of which can only be addressed very briefly here.

First, many psychological processes, including those of importance in psychological difficulties, are not accessible to consciousness, and even if they are they may be hard to articulate in terms recognisable, meaningful and understandable to Western-trained researchers, or may be subject to self-censorship. Second, questionnaires are constructed to maximise reliability by selecting very similar items and excluding more peripheral ones, yet this is in no way representative of the importance of included or excluded items, nor does it necessarily provide a good approximation to broader everyday experience. For instance, whereas many people who experience anxiety also report some depressive thinking, and many people who feel depressed also experience anxieties, the separation of anxiety and depression in Western psychiatric diagnostic classification systems means that the questionnaires also reflect this artificial and Eurocentric construction of the distinction between the two. Third, the scoring of questionnaires – the translation of frequency or importance of a thought or feeling, for instance, identified by a questionnaire item – arbitrarily assigns weight to each item (usually the same weight to all), and combines them in totals and sub-totals that are then matched against 'norms' to interpret in clinical terms: to decide that the

client's score falls within a clinical population or within a non-clinical population. All these processes of questionnaire construction draw on data from largely English-speaking populations, often White (Heinrich, 2020), from the Global North, and compliant with psychiatric classification. Extrapolation to other populations with diverse cultures, languages, beliefs and ways of understanding distress, is fraught with problems (Johnson, 2006; McHorney & Fleishman, 2006; Sousa & Rojjanasrirat, 2011) and is rarely adequately addressed in research. All these speak to the need for a more flexible and inclusive understanding of what kind of research can provide good quality evidence, discussed below.

In summary, not all research leads to quality evidence; and evidence-based practice is highly questionable when based on outcomes of poor research, and when the limitations of the research, including its Eurocentricity, are not addressed when evaluating the quality and the applicability of the 'evidence'.

Myth 3: Efficacy of psychosocial interventions can only be demonstrated by randomised controlled trials (RCTs)

Systematic reviews of treatment efficacy largely restrict eligibility to randomised controlled trials (RCTs), where randomisation of research participants to treatment or control arms is used to reduce the variance in outcomes due to extraneous factors, making it easier to attribute differences in outcome to different effects of the treatment over the control arm/s. The control is often no treatment, a realistic clinical situation, or in psychological trials may be an educational or supportive or other intervention of similar contact time to psychological treatment/interventions, but without the intended therapeutic content. This is in contrast to the simple and common evaluation of psychological in-

terventions that assesses variables of interest before and after interventions on all clients, but cannot distinguish effects of the interventions from those of natural recovery, the benefits of contact with clinicians and, in group settings, contact with other clients, and other extraneous and often unquantified influences.

RCTs have long been regarded as the 'gold standard' of treatment efficacy testing, in medicine and increasingly in psychology, although observational methods including single case studies are also highly rated in evidence-based medicine (Howick et al., 2011). However, RCTs have many disadvantages. One of the major ones is that of restricting eligibility for the trial according to population characteristics, discussed under myth 4, but often excluding people with multiple problems, or who are more likely to drop out of psychological care; examples are people with insecure accommodation or undetermined legal status, as is common in torture survivor populations, or who do not speak fluently the language of the rehabilitation service provider. Some RCTs on psychological interventions with torture survivors (see reviews by Hamid et al. 2019; Patel et al. 2014) have recognised these issues and taken a more inclusive approach to recruitment, not without its own difficulties. Another disadvantage is that the interventions are multi-component – they consist of a series of separate or combined components of presumed therapeutic benefit, delivered by a trained therapist – and no RCT can disaggregate the effects of each component from the others, yet the assumption that they are always synergistic, and to all participants, is rarely tested or even raised as a concern.

A further problem is the interaction of interventions (standardised) with client characteristics to produce different outcomes. The assumption of homogeneity of the participant population means that average results are re-

liable and useful for planning, but we cannot easily tell whether results differed in systematic ways according to how those participants were at the start of interventions. However, the clinician/practitioner and client want to know how well interventions will work for that particular person. Single case methods (Morley, 2018, Vlaeyen et al., 2020), where they can be applied, offer the possibility of testing truly personalised care (Howick, 2011). Outcomes for single case studies need to be countable, so rather than using questionnaire scores, personally relevant events such as nightmares, hours of sleep, distance walked or people spoken to per day, are used. While further description is beyond the scope of this paper, the method is far more accessible in clinical practice than are RCTs, although often demanding for the client in terms of repeated data collection.

There are further problems with reliance on RCTs. They are complex and often expensive to run, meaning that economies are made that can compromise quality and confidence in results. One of the most important of these is the problem of small size, described above. In our 2019 systematic review, we analysed 15 trials with a total of 1,373 participants, and a range from 10 (i.e. 5 in each of the treatment and control groups) to 165 per trial. Further economies on staffing undermine efforts at assessment and statistical analysis blind to client assignment, adequate translation or interpretation of assessment and treatment materials, and frequency and length of follow-up. All these make for a field with generally rather small trials with moderate to serious problems of bias, and therefore efficacy estimates that might be in the right direction, but whose size is entirely uncertain.

In summary, the efficacy of psychosocial interventions for torture survivors cannot be assessed only by RCTs, and the reliance on RCTs is problematic on conceptual and meth-

odological grounds, as well as re-casting the impact of torture as a disease category, and rehabilitation interventions as ‘treatment’.

Myth 4: RCT results can be generalised across all torture survivors as a homogeneous group

The disadvantage of the RCT design is that, in ensuring minimal variance in the population to be randomised and in the delivery of treatment/interventions, the trial may depart so far from usual rehabilitation practice, with its highly heterogeneous populations and practitioners who are trained to differing levels and deliver interventions in varied ways, that it cannot easily be generalised to wider populations of torture survivors. For instance, many of the studies of psychological interventions for PTSD largely recruit from people who have had road traffic accidents, or suffered violent assault or other crime, but within the context of a relatively safe civil society. While there is no doubt that many of the difficulties that are experienced in these populations – nightmares, flashbacks, high arousal to particular cues, for instance – are also experienced by survivors of torture who may similarly be diagnosed with PTSD, equating the psychological difficulties or needs of the two populations makes huge and unwarranted assumptions (for critiques, see Summerfield, 1991, 2001; Patel, 2011). Yet where RCTs supposedly demonstrate that ‘treatment X’ is effective in road traffic accident survivors for reducing PTSD, or, rather, reducing scores on PTSD questionnaires, ‘treatment X’ will be offered confidently to all other populations whose post-trauma difficulties are seen as the main target of psychological intervention.

In summary, torture survivors, their backgrounds and their experiences of torture, and the aftermath, differ considerably and they are far from constituting a homogenous group, nor a distinct ‘clinical population’. The out-

comes of RCTs on psychological interventions cannot be generalised to all torture survivors, nor do they capture the range of psychological and other difficulties and outcomes of interventions for torture survivors included in the studies reviewed.

Myth 5: Western psychiatric nosologies apply universally

Mental health as the outcome of treatment is rarely defined, beyond the WHO's original conceptualisation of 'a state of complete physical, mental and social well-being and not merely the absence of disease or infirmity'; absence (or sub-threshold score) of disease is the key concept used traditionally in psychological and psychiatric interventions, or 'treatment'. This has meant not only that diagnostic systems for identifying and classifying disease have become commonplace in evaluating outcome, but also that our understanding of research outcomes has reduced the complexity, intensity and vast breadth of the experience of human distress and suffering to the measurement of the reduction of assumed 'abnormalities' which focus on certain 'symptoms' – of distress. These systems of psychiatric disease classification (DSM and ICD) are widely criticised for their application of the biomedical model to psychological and mental health, for pathologizing normal distress and for their Eurocentricity, including in the research on which they draw, and its interpretation and generalisation (see Kirk and Kitchings, 1992; Horowitz, 2015; Rapley et al., 2011; Johnstone, 2009; Fernando, 2003; Boyle, 2003).

Each of these classification systems is repeatedly revised, and except for the very small number of organic symptoms, refer to behaviours and experiences that assume consensus on social normativity or deviance (Rashed & Bingham, 2014), even though these be-

haviours and experiences change with each revision: for instance, homosexuality was classified as disease in the three earliest versions of DSM. The tendency has been for both systems to expand, and much of the debate concerns whether they include and pathologise 'normal' variations in mood, such as depression after bereavement, and temper tantrums in children (both now included in DSM-5 diagnoses), which further questions their applicability to diverse communities with diverse beliefs about what is 'normal' and understandable human distress.

The diagnosis of PTSD is highly problematic, not least because it was invented (Young, 1995) for a specific purpose, in that it arose within a particular political and social context, but also because of its widespread and uncritical, reductionist use with torture survivors, with the unexamined assumptions that this disease category has universal conceptual and cultural validity, and that it meaningfully captures, with its array of symptom criteria, the multiple and intersecting impacts of a gross human rights violation such as torture (Summerfield, 1999, 2001, 2009; Patel, 2011). Yet PTSD has become a brand, used as a shorthand for extreme distress following torture, and it is now the currency of researchers, funders and rehabilitation services, and widely used in studies with torture survivors. Like several other disease categories in classification systems, in the most recent DSM-5, PTSD has become far more inclusive by loosening and widening the criteria, so much so that it has been described as 'amorphous' (Galatzer-Levy & Bryant, 2013), with the calculation that there are now over 636,000 different ways that presenting symptoms and difficulties can meet the criteria for the diagnosis of PTSD. With each iteration of DSM, the symptoms have broadened to include more that are common transitory responses to stress, and to

identify yet more experiences that are far from those of war or torture as ‘causes’ of PTSD.

ICD-11 has taken a narrower approach (Hyland et al., 2018), and separated PTSD from complex PTSD (CPTSD), developed using statistical methods. It claims to apply better to repeated and multiple trauma, where no escape was possible (Brewin et al. 2017; Cloitre et al. 2019), and to incorporate fewer of the common symptoms of anxiety and depression. Yet neither system used by researchers, ICD or DSM, takes any account of the wider concerns about cultural validity – and in the case of torture survivors, the social and political functions of using disease categories to attempt to capture the immeasurable impacts of torture as a human rights violation – as ‘abnormalities’. By comparison with the attention to PTSD, and the uncritical application of the diagnosis, other common problems in survivors of torture that cause immense distress and adversely affect everyday functioning, such as chronic pain (Amris et al. 2019; Rasmussen 1990) and traumatic brain injury (Burnett & Peel 2001) are little investigated, discussed, or addressed in rehabilitation of torture survivors.

In summary, a reliance on Eurocentric nologies for psychiatric disease categories is problematic on methodological and conceptual grounds, and for their assumptions of universality, making them inappropriate for assessing outcomes of holistic and multi-faceted rehabilitation for torture survivors from diverse cultural, linguistic, political and social contexts; and with diverse experiences of torture alongside other human rights violations.

Myth 6: Rehabilitation for torture survivors is equivalent to psychological ‘treatment’ of psychiatric ‘diseases’

Conceptualising the many difficulties experienced by torture survivors as PTSD and other individual ‘pathologies’ hugely simplifies the

magnitude and breadth of the impacts of torture, the ‘ripples of harm’ (Patel, 2020) on individuals, families and communities, whilst pathologizing severe and enduring distress as a result of human rights violations, as if to be understood by psychiatric disease categories and locating the problem within the individual psyche. Such reductionist, narrow conceptualisations also construct, incorrectly, the tasks and success of rehabilitation to the reduction of symptoms of an assumed psychiatric disorder (‘evidence’) and as ‘treatment’ of a pathology, rather than recognising rehabilitation as a form of reparation and as addressing the wide ripples of harm of torture with corresponding ‘ripples of care’ (Patel, 2020). Rehabilitation, then, is not synonymous with psychological interventions – it encompasses a multitude of interventions and outcomes in relation to the medical, psychological, social, welfare, educational, vocational, legal, and humanitarian concerns and needs of torture survivors and their families.

In 2014 we published our first systematic review and meta-analysis on rehabilitation interventions for torture survivors (Patel et al. 2014); in 2019 we published a substantially larger update review and meta-analysis (Hamid et al. 2019). Both reviews started with a search for psychological, social and welfare interventions, all seen as interrelated aspects of rehabilitation, but we both found only psychological interventions among the eligible RCTs. Even common outcomes in studies of psychological interventions, such as quality of life, were only evaluated by a minority of studies. None of the studies addressed wider issues of health, impacts on families and communities, social participation or engagement, and only about half referred to the multiple serious problems faced by torture survivors. Unsurprisingly, dropout rates were high, and even where statistically significant gains were made

from the psychological interventions, participants' post-'treatment' scores (disregarding concerns about validity of the questionnaires used for those participants) were still often in the clinically severe range. The implication was that even when survivors continued to live in conditions of ongoing threat and insecurity, where they had not been able to access justice, or to be reunited with their families, or to be assured asylum and safety; when they were homeless or had inadequate accommodation; where they were separated from family and friends and social networks; where they struggled to subsist and faced racist threats and attacks and so forth – their psychological symptoms could be effectively addressed in a vacuum, expecting benefit even if none of their social conditions changed.

In summary, how far this falls short of the rehabilitation aim “to restore, as far as possible, [survivors'] independence; physical, mental, social and vocational ability; and full inclusion and participation in society” is all too clear. Psychological ‘treatment’ of ‘diseases’, such as PTSD, does not encompass holistic rehabilitation, and research which implies this decontextualises torture and the suffering it causes; narrowly defines the problem as psychological and locates the problem within the individual, whilst defining outcome (or lack of change) narrowly as symptom-reduction.

Implications for future research

Rehabilitation for torture survivors and their families and communities is broad, and ideally and necessarily contextualised, with a process of providing specialised, collaborative and coordinated interdisciplinary care. As such, our research endeavours need to move beyond the replication of studies testing psychological interventions alone, and beyond the myopic attention to reducing assumed disease symptoms. These studies have many methodo-

logical limitations, discussed by study/review authors, as well as limitations in how they have been interpreted in the field by researchers and practitioners (e.g., Montgomery and Patel, 2010; Jaranson and Quiroga, 2011; Carlsson et al., 2014; Patel et al., 2016; Patel, 2020). They require detailed consideration, beyond the scope of this chapter, but since their limitations significantly constrain what can be gleaned from the literature to inform rehabilitation practice with torture survivors, they are summarised here.

Conceptual limitations and implications

Conceptual limitations are often overlooked in research on rehabilitation with torture survivors, and these limitations can underpin many other weaknesses of the study and the conclusions which may be derived from them.

1. Rarely do studies start with a conceptualisation of rehabilitation for torture survivors (for a fuller discussion, see UN General Comment 3; Patel, 2011; Sveass, 2013), instead conflating rehabilitation with ‘psychological treatment’. Studies could elaborate on how their focus on psychological or other interventions is part of, or is related to, rehabilitation; and outline the limitations of the study, guarding against generalisations to or from other populations.
2. Studies rarely define participants specifically as torture survivors, and assume wide-ranging definitions. Some studies use definition-based checklists that compare participants’ experiences with those definitions, insofar as study participants are able to use the checklists, but do not report in a transparent way how many of their study population were torture survivors. Other studies make no reference to a definition, or do not distinguish torture

survivors from other refugees, asylum seekers, or internally displaced people in their study population. While some experiences may overlap, and study participants may share current conditions (e.g., living in a refugee camp, seeking asylum), the risks and restrictions they face, and their rights, may differ substantially. The legal and political context of torture survivors cannot be set aside in relation to research processes and outcomes, and average outcomes or generalisations for the entire study population may not be true of the subset of torture survivors, who may respond differently to rehabilitation interventions or be more likely to drop out. Providing information about how a study has defined who can be considered a torture survivor is essential information.

3. To compound the problem, reviews may combine results of these studies with those of studies with torture survivors alone. Again, this makes it impossible to generalise with any confidence from review findings. Avoiding combining such diverse studies is difficult but important, and at the very least, these limitations must be made transparent.
4. Study populations are often heterogeneous with respect to personal data, including baseline scores describing physical or psychological health status. Further, torture survivors may face additional socioeconomic stressors, lack of access to justice, absence of justice and of reparation, multiple losses and ongoing traumas, all potentially impacting on their psychological wellbeing and response to any psychological, social or other rehabilitation interventions. Such baseline differences can be hard to characterise and we have little understanding of how they might interact with content and process of inter-

ventions, but research study therapists are in a good position to generate hypotheses about such interactions that might be testable in meta-analyses that use individual data.

5. A further level of complexity arises from the context in which psychological interventions, often rather narrowly conceptualised, interact with other forms of rehabilitation (including social, legal, and educational interventions). Few studies note or reflect on the effectiveness of the psychological interventions in this context, or in the context of external events of significance for the torture survivor, for example, political changes in their country of origin, reprisals against and worrying or absent news of family members, and the vagaries of seeking asylum status. It is not helpful to attribute all gains or disappointing outcomes to the psychological intervention, without considering these factors in the study analysis, interpretation and discussion of study limitations.
6. It is common for the target of psychological intervention to be PTSD diagnosis (more specifically, symptom-reduction to sub-threshold levels), whether or not that is the predominant concern of participants, and sometimes eclipsing all other forms of distress, or sub-threshold psychological/psychosocial difficulties. Further, those studies that attribute PTSD entirely to the experience of torture implicitly equate symptom reduction with modifying the assumed main/only impacts of torture, whilst also obscuring other current sources of distress: legal proceedings, societal racism, marginalisation and ostracisation, insecure living conditions, poverty etc. Studies should broaden their focus on the widespread impacts of

torture and be transparent about their focus, and the limitations of their study with respect to underlying assumptions, and their neglect of other sources of suffering and distress impacting on torture survivors and on their engagement with psychological interventions.

Theoretical limitations and implications

In any research, interventions used need to provide a rationale for the choice of intervention, specifically for torture survivors, and details of any adaptations made and of the rationale. It is common in RCTs for the convictions of the research team and delivering clinicians to substitute for a theoretical rationale, but this fails the requirement of equipoise that is required for a good quality RCT (John, 2017; Sackett, 2000). The theoretical and evidence base for the particular intervention requires description in a detailed protocol that constitutes part of the public registration of that trial (Cybulski et al., 2016; DeAngelis et al., 2004).

1. Many studies make reference to torture, traumatic experience, and PTSD as rationale, with a basis in observational studies of torture survivor populations. They lack an explanation of why PTSD is prioritised, or singled out, by researchers and clinicians as the target of treatment, and they lack any exploration with torture survivors of what they perceive as their problems in need of intervention. As noted earlier, this practice decontextualises the nature and effects of torture, excluding the wider environment that can exacerbate or maintain distress, and presents suffering from torture as an individual, and usually only as a psychological, problem (Patel, 2011), in turn used to justify the choice of individual psychological interventions. Studies should provide a rationale for the interventions, noting their limitations, and justifying their treatment priorities in terms of how these fit into the wider aim of rehabilitation.
2. As described in Myth 5, the psychological interventions commonly considered for use by researchers and clinicians working with torture survivors are based on frameworks of understanding and models of psychological change developed in the West (and then ‘tested’ in low-income countries, conflict areas etc.), with a focus on reduced symptom counts in the target categories of distress (PTSD, depression, anxiety, etc.). The meaning of distress, health and wellbeing need to be elaborated beyond Eurocentric constructions, including (for instance) spiritual, familial and social health, and interactions between these and the context within which the individual is living: family, community, and legal and political context. Studies should broaden their focus on a range of rehabilitation interventions, and note the limitations and culture-boundedness of Eurocentric models of interventions used.
3. In an attempt to recognise the different cultural and political context in which these Eurocentric theoretical frameworks are applied, with all their embedded values and assumptions about gender, morality, and social relationships, some studies make ‘cultural adaptations’, assuming and reasserting a normative framework that is still Eurocentric, without defining in detail ‘culture’, whose culture and in what context (since cultures are not homogenous and bound by language or nationality), and what are these ‘adaptations’. Studies should make clear how such

‘adaptations’ are developed, by whom, and on what theoretical and empirical foundations; as well as detailing the underlying assumptions of the assumed norms, and related limitations of the study.

4. Beyond the effects of the contextual factors described on the impact of any psychological intervention, and the experience of participation in a study, the ‘ripples of harm’ of torture extend beyond the individual. Interventions are often provided in a setting in which multiple and intersecting systems and processes of power and oppression (e.g. sexism, racism, economic, homophobic) impress on study participants. Studies should note these contexts and consider an intersectional analysis to fully grasp what individual psychological intervention can and cannot do, and to interpret outcomes in that light, noting the related limitations of the study.

Epistemological limitations and implications

Often research on rehabilitation, including psychological research, neglects a discussion of epistemology and the implications for the research process. Epistemology, however, is hugely important, since all researchers hold assumptions about the world that guide their research agendas, practice and conclusions (Chamberlain, 2015). Epistemology inevitably influences theoretical orientations and chosen research methodologies, with underlying assumptions about what can be known (ontology), how and what is valued or devalued as knowledge, and the role of the researcher throughout the research process, during design, method, analysis, interpretation, discussion and reporting of research outcomes.

1. The largely positivist epistemology of torture survivor treatment studies ignores the roles of researchers and their influence on the research process. It also deems symptom counts to be more important and less subject to bias than, for instance, survivor-based experience and meaning-making as evidence, or evidence from experienced clinicians. This assumption is examined below. As with theoretical limitations, the epistemological stance of researchers in any study needs to be stated, in particular, with transparency on what is valued and devalued as evidence in the research.
2. Rehabilitation as an overall aim of intervention implies not just changes for the individual, but also changes in the individual’s environment, including their social environment. Meaning is central to these processes, and understanding what is rehabilitative and enabling for an individual, in what situations, goes far beyond symptom counts. The perspectives and experiences of survivors and practitioners thus afford invaluable insights into what helps, and what can be seen as ‘rehabilitative’. One implication for future research is that a wider range of research epistemologies beyond positivism, and a broader range of qualitative and quantitative methodologies, are used as they may yield a wider range of evidence with can inform practice, services and community programmes.

Methodological limitations and implications

There are many methodological challenges in designing treatment studies, particularly in interventions with torture survivors, but there is also a substantial literature exploring and testing how these challenges may best

be addressed within the constraints of any particular study, aiming for highest possible quality and usefulness of the research outcomes for the wider field and for populations targeted. Grant et al. (2018) offer detailed CONSORT guidelines extended for psychological and social interventions, particularly RCTs, and Montgomery and Patel (2011), Patel et al. (2016) and Baird et al. (2017) all explore the specific area of torture rehabilitation. Common methodological limitations of the predominantly quantitative studies on rehabilitation for torture survivors, and their implications, are outlined below.

Sample

1. Small samples are not only underpowered to show the changes or differences they claim, but are particularly liable to produce large effect sizes (Dechartres, 2013, 2014); these studies in turn contribute to questionable summary statistics in meta-analyses (Turner et al., 2013). Where samples are inevitably small, it would be better to use single case methods than group-based analyses.
2. Sampling processes are rarely adequately described: there are barriers and facilitators to arriving at the point where eligibility criteria are applied, and many of those earlier selection procedures depend on judgements made, often by individuals peripherally involved in the research processes (such as referrers to a service), on whom to approach, or how and where to distribute invitations to participate. A careful account of these procedures allows more nuanced and contextualised understanding of the results and recognition of the limits of generalisation.
3. Unlike standard clinical studies in high-income countries from the Global North, such as studies of interventions for depression, participants in studies with torture survivors may be recruited on the basis of their legal status (e.g. including/excluding asylum seekers), their nationality, or language (for practical reasons), or experiences of torture. Before intervention they are typically assigned a PTSD (or other) diagnosis or score, although they may not be required to exceed a threshold severity score on a PTSD questionnaire, or to reach caseness; often those who may be suffering severely but do not meet PTSD criteria are excluded. Generally, there is a lack of necessary detail reported on how participants were selected (and excluded) for studies, with most studies recruiting their sample based on severity of symptoms, psychiatric diagnoses or care-seeking behaviour. Attention to this detail, and limitations in study recruitment practices, should be explained by researchers.
4. As described in Myth 4, samples of studies reported are mostly heterogeneous and highly variable, including in their experiences of torture (if specified) and other traumatic experiences, environmental, socioeconomic and political stressors and injustices and support structures; in period of resettlement (if refugees or asylum seekers); and country, cultural, ethnic, linguistic and other backgrounds. This presents particular problems in the outcomes being unquestioningly generalised to all/other torture survivors. Additionally, the duration of distress, and any previous psychological interventions and their results, are rarely reported, yet references to chronicity of symptoms and 'treatment-resistance' are made in defining target populations. In the light of sample heterogeneity, complete description of the sample cannot be realised, but sup-

plementary information and data sharing make collection of more such information, of possible use to further analyses; they may also allow combination with other researchers' samples. This is an area where more reference to the wider literature on psychological wellbeing would suggest variables that are rarely described, such as social support/isolation; disposable income/material deprivation; and physical health status.

5. While many studies are only able to recruit small samples, often because of inadequate resources, many also show high attrition rates which further reduce the power of the study, and that introduce questions about acceptability of the intervention. Study participants may stop attending services and 'treatment' offered for an infinite number of reasons, including many unrelated to the intervention or to those carrying it out, and it is very hard to obtain from those who drop out the reasons for doing so. Nevertheless, psychology as a discipline is poor at considering the risk of harm from interventions, from distress worsening as a result of interventions to undermining confidence in the possibility of any psychological/psychosocial improvement. Studies should note attrition rates, possible reasons why and explain what follow-up methods were used, and their limitations.

Procedural

Psychological therapies rely on the specialist training and skills of those who deliver the content, so that experience and competence (Kim et al., 2006; Wampold & Brown, 2005; Webb et al., 2010) may contribute to variance in outcome, as may other personal

qualities of the therapist (Heinonen & Nissen-Lie 2020) and the quality of the therapeutic relationship (Baier et al. 2020). Training type and duration, assessment or qualification, and experience of researchers and those providing interventions, specifically with the study population, are all relevant but often not specified in study methods. In the context of work with refugees and torture survivors, information on the gender, ethnicity, cultural background, primary language, qualifications and level of experience as therapists, of those providing interventions, are also required.

1. Modelled on medical research, RCTs using psychological interventions often rely on closely supervised interventions, which also depend on the vehicle of therapeutic relationship (Baier et al. 2020). The convention for RCTs, again following medical methodologies, is that treatment methods should be protocolised and manualised, and then scrutinised for adherence by participants and therapists, as should control conditions. If we do not truly know what was done in the intervention, and how it differs from the control condition, outcomes are uninterpretable or have huge margins of error around them. However, manualisation and adherence to a manual go against all principles of flexible, personalised, responsive and contextual therapeutic and rehabilitation interventions. With torture survivors, manualised interventions neglect the wider scope of rehabilitation, and narrow the focus to psychological interventions, viewing any positive changes as solely a result of those interventions (as opposed to the other factors and interventions which may impact on the survivor's well-being). In RCTs, it is essential to describe interventions clearly, so that they

can be replicated, reviewed, and built on. However, it is easy for inexperienced therapists in studies and researchers to either be highly protocol-driven, neglecting the specific needs of torture survivors, or to fall back on a non-directive counselling style or simplistic advice-giving, and this inevitably affects the outcomes of the interventions offered to survivors. Any interventions in studies on torture survivors should be designed for maximum effectiveness, in other words, maximum benefit for the participant/client. In studies using psychological interventions this likely demands adaptations and adjustments in the moment, and during the course of intervention, to address the needs of the participant/client – as such, the specific needs of the survivor should be prioritised over study protocols and researcher interests. Discussion of these pragmatic and ethical aspects, and limitations and evaluations of interventions offered (and by whom) must be included in study reports.

2. RCT trial therapists are commonly supervised during the course of the trial to ensure adherence to protocol and to keep interventions true to the theoretical basis of the intervention. Issues of the quality of supervisors (including qualifications, experience and competency) to encourage, guide, even to retrain or remove therapists with poor performance, are therefore extremely important, but rarely mentioned in studies. Again, this refers not only to conventional therapeutic competences but also familiarity, contextual knowledge and skills in work with the study population – torture survivors – particularly where local health workers or lay persons are trained as therapists, but their adjustments in the interventions may

be insufficiently understood or even derogated by supervisors unfamiliar with local context and sociocultural norms. Studies should provide details on supervisors, their professional backgrounds and nature and level of experience with torture survivors, their practices and decisions during the study, noting limitations relevant to the evaluation of the study.

3. The choice of comparison group or groups in RCTs requires justification. The common waiting list (no treatment) control provides control for the passage of time, somewhat less important in chronic than in acute psychological difficulties, and for attention from researchers at a minimum on assessment occasions. It does not control for possibly therapeutic components, such as having therapist time and attention, the induction of hope, and in group interventions, meeting others with similar problems, although some waiting list control treatments do have elements of these. People can improve in waiting list conditions across a range of problems, more than people not included in the trial, which is a more realistic approximation of the usual option. In addition to what is provided as part of the waiting list control, participants may take initiatives in trying other rehabilitation interventions over this time, but they are rarely asked if they have done so.

Although most trials have a fixed number of sessions of specified length, those with a minimum and maximum number rarely describe how different therapists decide on the number, or how this is treated in analysis, since early discharge can be a result both of rapid improvement and of lack of improvement (Barkham et al., 2006). Further, particularly in groups, members may attend less than 100% of sessions,

and this may affect efficacy, but that question cannot be addressed without the data. Particularly in the light of high attrition, and the multiple possible reasons for attrition from intervention, positive evidence of acceptability of the intervention to the study population increases confidence in replication and generalisation, but is rarely provided in studies with torture survivors. Some trials analyse data only from those participants considered ‘completers’ by having attended a minimum percentage of sessions, but even that assumes equivalence of content across sessions that is unlikely to be true. Further, some interventions are given as part of a larger therapeutic or rehabilitation programme with the wider community from which participants are drawn, meaning that they could be attending other individual, group or community-based interventions at the same time as the psychological intervention of interest.

4. A minimum follow-up of 12 months for interventions offered to torture survivors in studies seems appropriate, ideally longer. This is often difficult in the light of resources for intervention; unstable living conditions, and other circumstances that can make follow-up assessment hard to complete. However, participants’ reflections on psychological interventions and on their wellbeing over the intervening time can offer valuable insights. There may also be other important changes in participants’ safety and security, legal status, or other living conditions, for better or worse, that are relevant when interpreting follow-up data.

Studies should provide relevant detail on the choice of comparison groups, on how session lengths are decided and vary, on attrition rates and possible reasons, on

follow-up assessments and relevant other contextual changes in the study participants’ lives which may impact on their wellbeing; and studies should note all related limitations of the methodology.

Assessment instruments

Assessment is a complex and fraught undertaking that in studies with torture survivors is too often separated from the theoretical basis of the study and from the specific and varied psychological difficulties of participants, and their cultural and social context. Some of the specific limitations of studies are outlined below.

1. Consistent with the psychiatric disorder classification system as the basis of intervention studies, almost all studies with torture survivors use psychiatric diagnosis-specific symptom assessment, quantifying usually by severity, frequency, or interference with daily life. In some studies, this may be complemented by a general health assessment, but these tend to preclude assessment of change in distress across broader domains, and ignores the numerous ripples of harm of torture and the desired and potential changes in social wellbeing, and wellbeing within families, groups and participants’ community or communities. As a theoretical basis, studies should situate their assessments within a sound understanding of torture and its wide-ranging impacts (beyond simplistic and reductionist notions of ‘symptoms’ and ‘disorder’).
2. Assessment instruments are developed and tested for psychometric quality in a particular culture, with norms of expression, expectations of disclosure (including to clinicians and to strangers such as researchers), and with particular theories

about how emotions are or are not experienced and associated with one another and with somatic symptoms. They are also often designed to meet the requirements of a diagnostic system, so that, for instance, anxiety-related items are removed from depression questionnaires, and depression-related items from anxiety questionnaires, although many of the psychological difficulties co-occur and are inter-related. Used with people from cultures with diverse theories of distress, the content of typically Eurocentric instruments may poorly represent the construct (e.g., 'depression'), assuming that there is conceptual equivalence and cultural validity. Even where there is agreement across different cultural groups on the concept and the particular difficulties or symptoms that are relevant, the associations between items may be very different in different cultures (and diverge from those in Eurocentric assessment instruments), threatening reliability and interpretation, particularly where these use factor scores derived in the original population. Where instruments are used, they should demonstrate cultural and other validity for the population under study.

3. Many studies comment in passing on the limited validity (including cultural validity) and reliability of instruments developed in one population and setting and used in very different ones, and some use standard or local translations, or on-the-spot interpreters to deliver those assessments. However, there is little to no discussion of the ethical implications of using lay interpreters/translators, or of translating instruments without formal cultural validation. Validity is not infrequently asserted by researchers and study authors without any supporting reference or other

information, but validation is an ongoing enquiry into the performance of an instrument within any particular population and setting, not a fixed property of the instrument, as often implied. A common form of validation (in the normative culture within which the instrument is developed) is comparison with existing questionnaires for the same construct, and/or diagnostic categories, both of which are culturally-bound. Understandings of well-being can vary across cultures and communities, and particular presentations or difficulties may be misinterpreted by researchers, or simply ignored where the instruments used do not capture culturally-specific understandings of distress and well-being. Studies should not only ensure cultural validity (not merely translations) and other validity, they should note the limitations of the study, and address ethical considerations.

4. There is some heterogeneity in the outcome instruments used in studies with torture survivors, with variability in the content addressed. Consensus recommendations for assessment instruments would be a welcome initiative to reduce this heterogeneity and improve overall quality of evaluation. Meanwhile, as is common for psychological intervention, most outcomes are assessed by self-report, with all the problems identified above, or less often by diagnosis-based on self-report or clinician interview. Studies could make use of external indicators, such as attending training or employment, making or extending social ties, or making quantifiable steps towards some other aspirations of torture survivors. Changes in the intervention and control groups also need to be reported in terms of effect sizes and actual mean

change, and the variance of both, not only in terms of statistical significance of change. Some changes that meet required statistical criteria for change fall far short of a desirable clinical improvement, particularly when participants start with severe levels of symptoms as is often the case in RCTs with torture survivors. Additionally, selective reporting is a common bias in this field (Hamid et al., 2019) and all outcomes should be reported, whether they changed as predicted or not (Goldacre et al., 2019).

These problems are not easy to address. While external indicators of well-being can be added, and a broad range of outcomes agreed with the help of survivors themselves, the central problems remain: that self-report questionnaires are conceptually culturally-bound, and even where concepts seem to align, professionally and properly constructed translations (Sousa & Rojjanasrirat 2011) are resource-demanding and not guaranteed to achieve equivalence with the original, let alone cultural validity. Solutions will only emerge from more open acknowledgement of these problems, and critical appraisal of novel initiatives.

Bias

In quantitative research within positivist epistemologies, bias is seen as diminishing the quality of the research and limiting the applicability of its findings.

1. Absence of blinding in RCTs is seen as potentially leading to bias, but blinding of therapists to psychological intervention conditions in RCTs is very rarely possible, and blinding of participants often unsatisfactory, depending on the nature of the comparator. The best proxies are

to try to achieve equivalent participant expectations of benefit from treatment and control arms, assessed after assignment to condition; of course, it is hard to generate positive expectations of a totally inactive control; and to aim for equipoise, described above in relation to therapist allegiance. At the very least, studies should report on the details of the procedures used and the limitations of the study.

2. Many RCTs with torture survivors provide no information on sample size decisions or on power, and are likely to be underpowered; others underrecruit or suffer more attrition than expected. This is a serious problem that should elicit more caution in reviewers in relation to results of these studies. Researchers should provide information on sample size decisions and on power, noting the limitations of the study.
3. It is common for RCT analysis to drop participants who failed to complete assessments (or psychological intervention or control), but it is unlikely that data are missing at random, so important information is lost and the analysis compromised by this practice. There is a good literature on methods for dealing with missing data, which researchers may draw upon (e.g., Little & Kang, 2015), and information on missing data and methods used should be reported.
4. Therapist allegiance may go beyond enthusiasm for a particular therapy: in some studies, many trials may be published by one research group, or by a close network. Conflict of interest statements should be universal, even though at present studies with torture survivors often only provide information about financial interests in the intervention under test: some clinicians and research-

ers do have a direct financial stake in the particular version of therapy they seek to propagate, and others may derive indirect benefits. Transparency by researchers is both important for the evaluation of the research, and as an ethical practice (e.g. Munafò et al., 2017).

5. Most reviews of studies with refugee and/torture survivors only include studies in English, although they do not necessarily make this explicit (e.g. Weiss et al, 2016; Bunn et al., 2015), nor comment on the possible implications for their conclusions of excluding those in other languages, such as Spanish, French, or Arabic. This exacerbates the existing bias towards studies conceived, designed, led and published predominantly by Western researchers, from the Global North, even if conducted in a range of countries. To broaden our understanding of what is effective as rehabilitation for torture survivors in different social, political, cultural and economic contexts, a range of studies and rehabilitation work globally should be valued, where it is likely disseminated in a range of languages, in a range of places, beyond English-language, Global North-dominated academic journals.

Reporting

Comprehensive, transparent and ethical reporting is a cornerstone of quality research.

1. The quality of reporting of studies on all the aspects described above varies considerably. While word limits can constrain effective reporting, some authors willingly provide further and detailed information on request. Unfortunately, a culture of proprietorial interest in the authors' own

studies, and unwillingness to engage in critical debate, is worsened by competition for scarce funding. Poor reporting seriously weakens attempts to summarise the field, qualitatively or quantitatively, and slows progress towards better therapeutic practices for torture survivors. Guidelines on many aspects of methodology (Grant et al., 2018), and online resources, can be used by researchers to share protocols, detailed methods, full anonymised data, and to engage in discussions to enrich the next generation of research. Researchers are encouraged to improve the quality of their reporting on conceptual, theoretical, epistemological and methodological aspects and limitations of their studies.

2. Participants' own views and perspectives on the research are rarely represented, even though some funders and ethics committees may require such consultation with the population targeted by the study. Participants can supplement our understanding and improve the quality of research by contributions at all points from conceptualisation and design to interpretation when the study is complete, but the research team need to invite and genuinely engage with and respect those contributions.

Ethical limitations and implications

Ethical considerations in any research with human participants are paramount, and these are particularly complex and weighty in research with torture survivors whose experiences of torture involve abuses of power and the breach of every conceivable ethical principle of humane conduct towards another human being.

1. Obtaining informed consent when par-

ticipants may be unfamiliar with the processes of Western research studies, and with the language in which it is conducted, is far more complex than implied by many studies with torture survivors. There is a far greater risk of implicit or explicit deception, coercion, implied benefit, or misleading by inadequate explanation of options and of the burden and possible disadvantages and risks of taking part in the research. It is, of course, hard to establish freedom of choice when the choice is between apparent help in the context of a research study or nothing, or when the apparent offer of help via research participation comes with implied benefits (economic, legal etc.). These and other ethical considerations are rarely discussed in detail in the literature and researchers could elaborate on the ethical considerations, specific to research with torture survivors, and consider how information can be effectively conveyed, to seek meaningful and valid consent, what opportunities can be provided to clarify and answer questions, and how fully informed consent is supported by such processes.

2. Power differentials between researchers, their funding and institutional bodies, and the study participants are a reality, and should be acknowledged by researchers. They have important implications for the cultural and gender appropriateness and acceptability of the interventions to the participants, participants' expectations and adherence, and study implementation and dissemination.
3. Studies may risk compromising participants' confidentiality, safety and security, particularly in research conducted in settings where torture survivors face ongoing threats to their own and their families' safety. Yet, it is rare for studies

to detail the measures taken to minimise these risks, and to consider unexpected risks to study participants, including in follow-up.

Conclusions

In providing some suggestions for better design, conduct, and reporting of studies, we have drawn on our experience of providing rehabilitation to torture survivors, conducting research and reviewing research on psychological interventions for torture survivors. Conducting quality research with torture survivors, particularly research on what is beneficial to survivors, as rehabilitation, is extremely difficult and fraught with ethical, conceptual, methodological and philosophical challenges – with which we must collectively engage – though first and foremost, we must focus on what rehabilitation as a form of reparation means and entails. At present, the quality of research in the field is very weak, and as such, the available research precludes convincing and firm conclusions and recommendations to be made for rehabilitation practice. This represents a serious risk of wasted research resources applicable in medicine (Glasziou & Chalmers 2018), as in research on the rehabilitation of torture survivors.

To move towards improved rehabilitation for torture survivors, we need to recognise rehabilitation as a complex, interdisciplinary set of practices and activities, not only psychological interventions; and we need to recognise rehabilitation as a human right to reparation. As such, we need to embed human rights principles in all research, and we cannot simply say 'more research is needed' – instead, we need to ask *what is rehabilitation for torture survivors, what kind of research is really needed, valued, by whom and in which context, for whom and to what end?* (Patel, 2020). Our focus should be

to ensure quality rehabilitation as a form of reparation for torture survivors, their families and communities, and this should always come before benefits to academia and to research teams.

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