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4 M's to make sense of evidence – avoiding the propagation of Mistakes, Misinterpretation, Misrepresentation and Misinformation

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Summary

Osteopaths are expected to keep up to date with research evidence relevant to their clinical practice and to integrate this knowledge with their own experience and their patients' values and preferences. One of the potential challenges when engaging with research is to make sense of it, to decide if it is trustworthy, and if it is applicable to the complex and context-sensitive nature of clinical practice and the care of individual people. Clinicians are increasingly exposed to (deliberate and undeliberate) misinformation and overstatements which propagate easily, including via social media. This masterclass aims to facilitate critical thinking and engagement in research for clinicians to make better-informed decisions with their patients. It was developed to support osteopaths facing these questions with the aim of empowering them to judge research themselves, detect common fallacies in the conduct and reporting of different research designs, and to increase researchers' accountability. Ultimately, we hope that by reading and considering the guidance and examples in this paper, clinicians will be better equipped to optimise the use of their (and their patients') time when facing potential sources of evidence.

Mistakes, misinterpretation, misrepresentation and misinformation are discussed for each of these methods/methodologies: case reports, clinical trials, qualitative research, and reviews.

Introduction

Osteopaths are expected to keep up-to-date with research evidence relevant to their clinical practice (e.g. for UK GOsC-registered osteopaths see (General Osteopathic Council 2021) ref and for Swiss osteopaths see (OCPSan 2019)) and to integrate this knowledge with their own experience and their patients' values and preferences (Greenhalgh, Howick et al. 2014). There are a number of benefits in adopting evidence-informed practice (EIP). However, several theoretical and practical challenges have also been identified (Miles and Loughlin 2011, Leboeuf-Yde, Lanlo et al. 2013, Tyreman 2018, Anjum, Copeland et al. 2020, Kamper 2020). One of the potential challenges when engaging with research is to make sense of it, to decide if it is trustworthy, and if it is applicable to the complex and context-sensitive nature of clinical practice and the care of individual people (Kerry 2017). Tools to support critical analysis are not only required when reading research, but whenever osteopaths encounter information that could impact on the care or information they provide to their patients. Clinicians are increasingly exposed to (deliberate and undeliberate) misinformation and overstatements which propagate easily, including via social media. At the same time, they are naturally vulnerable to misinformation and need to be aware of their cognitive biases. This is not a problem specific to osteopathy and there are various reasons why this occurs (e.g., confirmation bias or anchorage bias (Gigerenzer and Brighton 2009, Saposnik, Redelmeier et al. 2016)). The challenge posed by EIP concerns all healthcare professions, and many (if not all) professions have had to incorporate EIP in some form or another. The translation of evidence into practice can be seen in the development of guidelines that inform healthcare pathways. Clinicians' use of guidelines varies and their decision-making seems to be based on hybrid sources of information (Wieringa

and Greenhalgh 2015). It is important to state that forty years after its inception, EIP, as a theory of practice, is not settled. Debates continue about all elements of EIP including the nature of evidence (Loughlin 2008), the role of patients' values and preferences (Greenhalgh, Howick et al. 2014, Louw, Marcus et al. 2017), and the role of the practitioners' judgement (Loughlin 2009, Woodbury and Kuhnke 2014). Osteopathy has not escaped the debate, and there have been calls for osteopaths to incorporate evidence into their decision-making for more than 20 years (Vogel 1994, Green 2000). Recent research of osteopaths' attitudes and skills is encouraging (Sundberg, Leach et al. 2018, Alvarez, Justribo et al. 2021), and there are signs that, globally, osteopaths have broadly positive views towards utilising evidence in their clinical practice, but feel less confident in their skills to integrate evidence into their clinical decision-making (Sundberg, Leach et al. 2018).

This masterclass aims to facilitate critical thinking and engagement in research for clinicians to make better-informed decisions with their patients. It was developed to support osteopaths facing these questions with the aim of empowering them to judge research themselves, detect common fallacies in the conduct and reporting of different research designs, and to increase researchers' accountability. Ultimately, we hope that by reading and considering the guidance and examples in this paper, clinicians will be better equipped to optimise the use of their (and their patients') time when facing potential sources of evidence. We hope that their autonomy and agency will be enhanced to decide if and how to apply evidence in practice, developing their expertise.

Building a house (knowledge) with strong foundations (research)

Knowledge and evidence can be seen as a house where all designs help to build different rooms that are equally important but have different functions (Jonas 2005, Walach, Falkenberg et al. 2006).

Researchers frequently confront ethical, methodological and practical constraints or challenges. Compromises have to be made during the publication process (e.g., due to word count limitations) and at times 'mistakes' are unavoidable. In fact, they are part and parcel of the research process that sometimes only become apparent towards the end of a study. It is also part of the development and maturation of any profession to be critically self-reflective of the methods and epistemology (i.e., the nature of knowledge and how to go about 'knowing it' (Richardson, Higgs et al. 2004)) which inform its practice.

Before discussing common study designs, we would like to define our interpretation of the concepts that are used in this masterclass. **Mistakes** refer mostly to the methods employed, what was "done" in the study, and could be defined as avoidable errors that may often be unrecognised by the authors. They would also include methods or techniques that are not accepted as adequate research practice. **Misinterpretation** refers to the analysis used in the study and how the data were interpreted. **Misrepresentation** relates to how the information is portrayed in the title, abstract, discussion, and conclusion. There is a fourth "M" that we would like to mention here, relating to the readership: **Misinformation**. It is the product of the first three M's with consequences for clinical practice and patient care. Misinformation occurs when inappropriate research designs or evidence quality are used to inform (erroneous) reasoning; when absence of evidence informs (poor) practice; and when inaccurate advice is propagated to peers or patients. Broadly, we address the question of when, as a reader, should I propagate information or not.

Whilst we have presented the 4 M's as distinct, and this is a somewhat crude separation to define our position, in reality there is overlap. One will influence the other or not sit clearly under 1 'M'.

The following sections will provide details about such fallacies found in case reports, randomised controlled trials (RCTs), qualitative research, and reviews, as these are designs that readers will frequently encounter in the literature.

To develop this masterclass, four osteopaths took part in a review and feedback process. They had had no formal training in research methods beyond their initial undergraduate training. They were each sent the manuscript with an osteopathic article (case report, RCT, qualitative study or review) and a pilot form. They were asked to read the masterclass and the article in their preferred order, and to send the pilot form back. They provided scores (0-10) and reasons for their score on: the general style of the article, the usability of the content, and the help the masterclass provided to assess the quality of the paper. There was a free-text box for further comments. The feedback was overall positive and the changes they proposed were made, apart from two. One was on how to interpret statistics: whilst it is outside the scope of this masterclass, it is very relevant and related to the 4 M's; we would like to draw the readers' attention to these references (Kamper 2019, Kamper 2019, Kamper 2019). Another comment was on using user experience design to help the readability, for which we do not have expertise in.

Case reports & case series

Case reports and case series usually describe an interesting, rare or an unusual evolution of a disease of one individual or few individuals. They are used to generate an in-depth investigation and understanding of a single patient in their real-world context (Yin 2018, Vaughan and Fleischmann 2020). This research design is a suitable strategy to investigate "how" and "why"-questions about a contemporary intervention and complex issues (Yin 2009). Even if the case report is traditionally perceived to be a lower-value form of evidence, this type of observational and descriptive research design has its rightful place to document and understand complex interventions in a more naturalistic way (Crowe, Cresswell et al. 2011). There are several types of case reports, including retrospective and prospective ones (of which the latter are considered more rigorous because of the ability to pre-specify the methodology), multiple or single cases. Further, case reports can be assessment reports, management reports or educational reports. Case reports resemble other research designs such as single case experimental designs (including n-of-1 designs) that introduce deliberate experimentation, or cohort studies that voluntarily observe exposed and non-exposed participants (Mathes and Pieper 2017).

The value of the case report is well recognised in many fields. Indeed, it can be used to generate hypotheses to be subsequently tested by other types of research design such as cohort studies or control randomized trials (Sun, Aliu et al. 2013, Nissen and Wynn 2014), to detect novelties (Nissen and Wynn, 2014), to warn a profession of potential complications of an intervention (Green and Johnson 2006). These reports can also promote the sharing of clinical expertise, help clinicians to solve difficult clinical problems and provide valuable teaching opportunities (Green and Johnson 2006, Nissen and Wynn 2014, Vaughan and Fleischmann 2020). Finally, case report can give the patient the opportunity to share their perspective.

The popularity of case reports has led to the need to develop tools that promote high-quality and well-written case reports (Riley, Barber et al. 2017). The most widely used reporting guideline is

the CARE (CAse REport) guidelines (Gagnier, Riley et al. 2013)) to improve transparency and completeness of case reports. A successful clinical case report must be well structured, be brief and convey a clear message (Green and Johnson, 2006). It includes elements of the patient case history, examination, accurate descriptions of the interventions, objective, reliable and valid measures and ongoing management with the aim of informing clinical practice. Several professions have documented how to use the CARE checklist in the context of their own discipline, including osteopathy (Vaughan and Fleischmann, 2020). Osteopathy is no exception to the trend of writing case reports; about one third of all osteopathic publications between 1980 and 2018 about the effect of OMT are case reports (Morin and Gaboury 2021). Although it may seem easy for a clinician to write, read or improve clinical decisions with a case report, several pitfalls are possible and should be identified by the reader. The most common problems with case reports are outlined in Table 1.

The main limitations of case reports include low possibility of generalization, cause-effect relationships cannot be inferred, and there is a danger of over-interpretation and distraction of the reader from common problems by focusing only on the unusual aspect of the cases (Nissen and Wynn, 2014).

Table 1 - 3M's in case reports

Warning signs	Types	Examples
Mistakes		
Unclear research question or no indication of the scope of the case	Case selection	Presentation of a case without any specific question or rationale
Absence of systematic measures before/during/after	Recall bias (in retrospective case studies)	Writing a case based on few clinical notes in the chart
No indication of the types of measurements, time period or no validated tools used	Lack of rigour	Non objectives, poor-quality data (anecdotic), absence of triangulation of data
Extensive literature review and very few information about the case	Combining a case and literature review	Long review that does not narrow down to the need for the case report
Presentation of only one aspect of the case with no alternative hypothesis discussed	Data collection and selection	Not presenting enough contextual information to understand clinical decision through the conclusions
No explanation about why this case is worthy or unique or what does it add to current knowledge	Absence of original contribution from the case	Many similar cases or RCT already published on the topic
Misinterpretation		
Difficulty in extracting the evolution of the case	Volume of non-relevant data	Too much information and data presented
Absence of tables with before and after outcomes / timeline	Inadequate / confusing presentation of data	Long descriptive text without any synthesis
Absence of information about potential confounders, natural evolution, or other possible reasons for the observation	Cause-effect relationship	Affirmation that an intervention helps for a condition without specifying the context and confounders

Emotional appeal on readers, impression that the intervention is fantastic	Overinterpretation (Nissem and Wynn, 2014)	Exaggerated conclusions from the results of a single case
No disclaimer that the case results cannot necessarily be generalized to all potential patient with this condition	Generalization	Letting people believe that the conclusion applies to many without taking into account the context
Misrepresentation		
Absence of the word case report in the title	Title	Title that let suppose interventional research
No mentioned that other types of design are required to validate hypothesis generated by the case	Claims and general statements	Conclusion that the approach or technique is effective based only on the case results
A case report should describe and not prove anything	Prove causation	Sentences such as: "this case proves that …"
No highlights of the differences found between the case and what is already known in the literature	Discussion is inconsequential	Typical and non-typical aspects of the case not clearly stated
No sentence summarizing what was learned from this case	Take away message	No clear suggestions or recommendations are made for clinicians or researchers

Clinical trials

Randomised clinical trials (RCTs) are comparisons between two or more groups of patients, receiving distinct interventions in order to evaluate the effects of one in comparison to the other (Jonas, 2005). Even though highly regarded, it is important to remember that each RCT is an experiment designed to give specific answers to clearly defined research questions. These effects can be about clinical outcomes, costs, safety concerns, or specific physiological responses. RCTs are important to inform clinical decision-making but require keeping in mind that the observed results are limited to the circumstances in which measures were taken. Furthermore, most RCTs tell us very little about other important factors, such as patient and practitioner experiences, preferences, and social context.

RCTs make use of randomisation to ensure that patients in all study groups are similar across known and unknown factors that may influence treatment outcomes. The choice of the comparator group for the test treatment is determined by the underlying study question. For example, comparing a treatment to a no-treatment control (similarly, 'waitlist' and 'time controls') can account for the natural history of the disease. It cannot, however, elucidate to which degree any observed effect is due to any specific components of the provided care. Placebo (also 'sham' and 'attention') controls are designed to isolate these specific effects. In doing so, well-designed placebo-controlled trials provide information on the potential true benefit of a specific targeted underlying mechanism. However, clinical trials may also use existing treatment as control (i.e., equivalence or comparative effectiveness trials).

Table 3 gives the main fallacies or errors that are useful to identify when assessing whether the results from a trial are applicable to specific clinical situations.

[Table 3: 3M's in RCTs, see below]

Warning signs	Types		Examples
Mistakes			
Unclear explanation of underlying mechanisms or theoretical models that justify the intervention under scrutiny.	Poor choice of intervention or control.	Rational	Providing general description of care without details such as “osteopathic manipulative treatment”.
Use of a control that is unlikely to be perceived as a credible treatment.		Blinding	Having participants lay down and wait alone in the control group.
Different management between groups other than for the component of interest.		Performance bias	Let practitioners talk to participants in the treatment group and not in the control.
Lack of power to identify minimal clinical important difference.	Lack of rigour in methods	Random error	Not plan a sample size large enough to detect the minimal clinical important difference.
Not using standardised and validated measuring instruments to evaluate outcomes.		Detection bias	Using a self-made questionnaire combining questions from different questionnaires to assess severity of symptoms.
Not blinding operator to group allocation.		Observation bias	Measuring pain threshold by the same person that is delivering the intervention.
Lack of measures put into place to assure data quality and avoid protocol deviations.		Quality control	Absence of protocol or ethical approval.
Not clearly distinguishing primary from secondary outcomes.	Lack of transparency in reporting	Random error	Choosing as an outcome multiple dimensions of a questionnaire.
Not comparing baseline characteristics between groups.		Selection bias	Avoiding providing baseline values for each group.
Not reporting reasons for drop-out.		Attrition bias	Not reporting outcomes for patients with severe side-effects who have stopped the treatment.
Not reporting what groups participants believed they were in (blinding success).		Performance bias	Simply reporting blinding to have worked.
Not reporting all results.		Reporting bias	Focusing on significant results only.
Misinterpretations			
Concluding on benefits when the primary outcome does not show significant differences between groups.	Shifting the goalpost	Reporting bias	Focusing on quality of life when the primary outcome was pain intensity.
Relying on multiple testing without statistical correction and then focusing on results that are significant.	Relying on multiple testing	Random error	Ignoring negative results when interpreting overall results.
Ignoring missing data without relying on multiple imputation or sensitivity analysis.	Not accounting	Attrition bias	Not reporting any missing data.

	for missing data		
Exaggerating effects between groups by modifying the scale or by focusing uselessly on within group difference.	Graphical distortion	Reporting bias	Assuming effects occur because significant effects within the group occurred over time.
Misrepresentation			
Use of specific reporting strategies to distract the reader from statistically non-significant results.	Spin reporting	Integrity	Even if non-significant, reporting results to be meaningful.
Inappropriate identification and recognition of potential biases and/or limitations.	Bias denial	Internal validity	Not reporting blinding issues in a trial where operators are not blinded.
Going beyond the trial's specific research question in interpretation or discussion. Making claims not supported by the data or that do not recognise the risk of false results inherent in this particular study.	Extrapolation	Poor contextualisation	Assuming that if an intervention modulates heart rate, it also increases resistance to stress.
Generalising to broad populations outside the trial or not outlining the limits of the supposed generalisability.	Exaggerated generalisability	External validity	Extrapolating results to other populations or conditions.

Qualitative research

The previous sections on quantitative methods are conducted with the view that there is a single truth and knowledge (epistemology) to be found 'out there' (ontology), and this is consistent with the assumptions which underpin the positivist and post-positivist paradigms (Guba and Lincoln 1994, Olson, Young et al. 2016). For example whether or not a treatment *is* or *is not* effective (statistically significantly) or whether or not a clinical assessment *is* or *is not* reliable or valid (e.g. by way of a Kappa score or an intraclass correlation coefficient). Research which adopts quantitative methods and methodologies tends to view knowledge as facts which can be discovered from direct observation and measurement to enable predetermined hypotheses to be accepted or rejected (Petty, Thomson et al. 2012). Quantitative researchers generally hold the view that there is a Truth to be found in relation to these research questions and that the knowledge of them is independent of the knower (i.e. objective), meaning that treatments are either effective (or not) and this knowledge is true, regardless of the personality, beliefs and values of the researchers (Petty, Thomson et al. 2012).

On the other hand, qualitative research takes a different view to truth, knowledge and reality, which, while the different qualitative theoretical methodologies might vary, the general difference is that in the social world, truth is multiple, local to the individual and socially constructed (Guba 1992). These assumptions are aligned with a constructivist or interpretivist research paradigms (Guba and Lincoln 1994). As a result, qualitative researchers may reject the view held by quantitative researchers that social reality can be accessed ('observed') by methods that are independent of their interests and values. The subjective position of qualitative research can make it a challenge to implement strict 'objective' criteria and standards for conducting and reporting qualitative research (Sandelowski 2015). As such, for many qualitative researchers,

research is a process of interpretation. The researchers themselves with all their values, knowledge and experiences are the instrument of that interpretation (e.g., during data collection and data analysis) (Petty, Thomson et al. 2012).

The value of qualitative research is that it offers insights, depth and context formed from a range of perspectives on a particular psychological, social process or phenomenon, which may have transferability to the readers' own personal setting and circumstances. While there is growing recognition that the evidence generated from qualitative studies offers significant value for evidence-based person-centred care (Anjum, Copeland et al. 2020, Thomson 2020), it is traditionally believed that the findings offer limited or insufficient evidence for causal relationships, such as the effectiveness of treatment interventions. With that said, recently philosophers of healthcare and science and clinicians have begun to articulate the important role of qualitative research in obtaining a rich and contextual understanding of the complex and unique 'causal story' of individual patients using the theoretical framework of dispositionalism (Anjum, Copeland et al. 2020). Furthermore, strong arguments have been made for the inclusion of qualitative research into evidence-based practice, policy and decision-making on the grounds that quantitative research alone is unable to provide a sufficient understanding of the complex relationship between the healthcare system and the outside world (e.g., socio-political and economic context) in which care the care of people, communities and populations takes place; qualitative research possesses a rich and diverse range of methods, methodologies and theories which can generate a detailed and holistic understanding of healthcare practice (Greenhalgh, Annandale et al. 2016).
There is a growing recognition of the complexity of clinical healthcare practice, such as how clinicians conceptualise common conditions like low back pain (see (Eriksen, Kerry et al. 2013, O'Sullivan, Caneiro et al. 2016)), the nature of clinician-therapist interaction (O'Keeffe, Cullinane et al. 2016), the crucial role of contextual factors in clinical outcomes (Rossettini, Camerone et al. 2020) and even the nature of causation itself in respect to the development of pain/illness and how different people may (or not) respond to therapeutic interventions (Anjum 2020). Therefore, real-world clinical practice (and the ultimate success of therapy) is highly subjective, individualised to the person/patient, influenced by a multitude of interacting factors in a context-sensitive environment.

The different underpinning theories and philosophies of qualitative research enables researchers to embrace complexity, rather than control for it. As such, the findings of qualitative studies offer insights and knowledge of the idiosyncrasy of individual patients, including their lived-experiences, psycho-social processes and social contexts and provide a valuable form of evidence to inform person-centred practice.

Table 4 - 3M's in qualitative research

Warning signs	Type	Example
Mistakes		
Too much / lack of diversity in participants (Pietkiewicz and Smith 2014)	Inappropriate sampling (Coyne 1997)	All participants samples from the same work/clinic location or setting.

Authors claiming to adopt an interpretivist position but conducting an inter-rater reliability analysis on the coding to ascertain the single object 'truth'.	Theoretical position vague, ambiguous or not stated.	Mixing and matching epistemological or ontological positions which are either not congruent with the research question stated or are incompatible with the chosen methods or inconsistent with each other.
Moving between and stating different methods and methodologies without transparent reporting of how these were utilised.	Methodological slurring (Baker, Wuest et al. 1992)	Using content analysis (Cho and Lee 2014) combined with grounded theory without a clear description about how the different methods.
Superficial descriptions of the study design e.g. 'an interview study' or 'a qualitative study' without details about of the methodology and methods.	Lack of transparency in reporting methods	A lack of detail in reporting or guideline not used to structure the methods. (e.g., COREQ (Tong, Sainsbury et al. 2007), SRQR (O'Brien, Harris et al. 2014)).
Mundane or seeming obvious unimaginative results reported.	Testing existing theory	A finding that 'chronic back pain negatively affects a person quality of life'.
Absence of how the findings relate to broader social theories (Jackson and A. 2012).	Lack of theoretical grounding or integration	A focus *only* on methods and little integration of broader extant social theories
Misinterpretation		
Not all participants quoted in results. Problematic especially in methodologies where power and marginalisation are the focus of the study (e.g., critical theory)	Selection of participants' quotations	Only quotes presented from a small number of participants.
Inconsistency between the researchers aims and the potential focus and goals of the chosen qualitative methodology.	Mist-match between research aims and qualitative methodology	Using phenomenology (methodological aim: to describe the lived experience of a phenomenon) to develop an understanding of the social processes (which would be better suited to a grounded theory approach) (Starks and Brown Trinidad 2007).
Ambiguity or a lack of detail about the researchers, who conducted the analysis, their relationship to the participants.	Lack of researcher reflexivity	Absence of declaration of the position, assumptions, background and views of the researcher.
Misrepresentation		
Broad and sweeping statements by the researchers in the discussion section.	Over generalising qualitative findings	"The attitudes and experiences of the participants in this study indicate that it is likely that *all* other similar people will feel and think in this way"
Causal claims should be carefully considered only in the context of individual patients rather than broad populations (Anjum, Copeland et al. 2020).	Making generalisable causative claims	Using the subjective reports of participants (e.g. their condition improved) to 'prove' the effectiveness of an intervention.

Absence of coherent and transparent theoretical position of the researchers which is consistent with paradigms of qualitative enquiry (Guba and Lincoln 1994).	Incorrect or misleading descriptions of the 'qualitative' study design	'Qualitative' studies using quantitative surveys or questionnaires to collect data on subjected phenomena and processes.

Reviews

Secondary research involves the collation and synthesis of existing research. Reviews are often conducted when enough data is published on a topic, but with an unclear overall answer or conflicting results. Reviews aim at providing an up-to-date summary of what is currently known. There are currently four main ways to review the literature in medicine, allied health and rehabilitation: narrative literature reviews, systematic literature reviews, meta-analyses (Rumrill, Fitzgerald et al. 2010), and scoping reviews (Pham, Rajić et al. 2014). Their methods should clearly be described to allow readers to assess their quality and trustworthiness.

Narrative reviews can be appropriate educational tools in the classroom but are no longer accepted for publication by many journals (Green, Johnson et al. 2006) due to the lack of clear selection criteria for articles (Cronin, Ryan et al. 2008). There are numerous examples in the osteopathic literature of recent narrative reviews on attractive topics, e.g., the fascial system or the five diaphragms, but readers should be mindful of the low quality of this type of review when reading them.

There are more reliable methods to combine and merge information from individual studies. Some will include mostly quantitative data (such as systematic reviews, with or without meta-analysis), qualitative data (qualitative meta-synthesis (Finlayson and Dixon 2008)), or a variety of study designs (such as systematic reviews with critical interpretive synthesis (Dixon-Woods M 2006)). These reviews have different epistemological positions (e.g., meta-analyses are more aligned with post-positivism and qualitative meta-synthesis with constructivism). Systematic reviews of quantitative data focus on a specific clinical problem: therapeutic, diagnostic or prognostic (Biondi-Zoccai, Lotrionte et al. 2011) and include different steps that are explicitly and clearly stated to allow independent reproduction by other researchers (Wright, Brand et al. 2007, Furlan, Pennick et al. 2009, Biondi-Zoccai, Lotrionte et al. 2011, Higgins JPT & Green S (editors) 2011)). They are effective at pinpointing weaknesses and fallacies in apparently sound primary studies (Biondi-Zoccai, Lotrionte et al. 2011). Systematic reviews of qualitative data hold different theoretical frameworks and researchers' position than systematic reviews of quantitative data; reality and knowledge are not perceived as objective, absolute and stable, but subjective, co-constructed and contextual. Whilst they tend to be more flexible and diverse in their methods, they should also be transparent in how they are conducted. As such, they follow explicit steps to allow readers to assess how results and conclusions were made, seeking to develop and refine theories and creating broader narratives of psychosocial phenomena, processes and experiences (Finlayson and Dixon 2008).

Another form of review are scoping reviews that are exploratory in nature; their broad research questions differentiate them from systematic reviews (Colquhoun, Levac et al. 2014). They are mostly used in healthcare (Pham, Rajić et al. 2014) and follow a distinct methodological framework (Arksey and O'Malley 2005). They can be conducted to examine the extent, range and nature of research activity; to determine the value of undertaking a full systematic review; to identify research gaps in the existing literature; and to summarise and disseminate research findings to policy makers, practitioners and consumers who might otherwise lack time or resources to undertake such work themselves (Arksey and O'Malley 2005).

The limitations of reviews include the quality of the studies included: if only few or low-quality studies are retrieved, conducting a systematic review may mislead readers about the strength of the evidence. Another limitation is the lack of their external validity to a clinical setting, i.e., knowing if results from systematic literature reviews can be applied to a single individual.

Table 5 – 3 M's in reviews

Warning signs	Type	Example
Mistakes		
Lack of balanced and nuanced discussion	Evidence cherry-picking (aka p-hacking)	Article which starts with a short introduction, followed by a long discussion on a topic with no clear description of the methods followed and reporting of the results
Not reported following the PRISMA statement		
Absence of registered protocol or unaccounted differences with protocol		Reviewers collecting several outcome measures from the included studies, but selectively reporting ones in favour of the intervention assessed
No methods section		
Only one database searched	Poor databases search	Review on osteopathy only searching PubMed where osteopathic literature is very limited.
Absence of definition of the study topic (using PICOS: Participants, Interventions, Comparisons, Outcomes, and Study design)		Search based on simple search terms with absence of use of MeSH terms, Boolean operators (OR, AND, NOT), or truncation (usually represented with an asterisk)
Limited number of search terms (synonyms and Medical Subject Headings (MeSH))		
Absence of duplicate and independent processes	Biases introduced by researchers	Screening, data extraction or quality appraisal of included articles conducted by only one investigator, or two but not independently.
Misinterpretation		
Comparing articles that use different outcome measures or populations with no acknowledgment	Data mishandling	Review with an exploratory research question leading to inclusion of articles with varied designs: authors synthesising all results together regardless of major differences in levels and types of evidence
Making effectiveness claims based on preclinical data		
Amalgamating well-powered and underpowered (e.g., pilot) studies		Review including clinical trials regardless of their power (i.e., including pilot studies and well-powered studies) and giving them equal weight in the final conclusions
Studies not reporting causation (e.g., cohort studies) but review misreporting results/conclusions	Correlation error	Using Patient Reported Outcome Measures (PROMs) to assess effectiveness of osteopathy
Using data that was not drawn from clinical encounters to hypothesise or justify clinical effectiveness	Poor clinical replicability	Systematic review on effects of spinal manipulations on pressure pain thresholds and review making recommendations for/against using these techniques in clinical settings
Misrepresentation		

Prioritising positive over negative findings not based on strength / quality of evidence in abstract/conclusion	Spinning	Suggesting that a therapy is effective or may be effective when results strongly suggest the opposite
Conclusion not aligned with research question	Misleading conclusion	Research question regarding effectiveness but conclusions based on adverse events

Conclusion

Four potential problems with evidence from four frequent clinical research designs were discussed: Mistakes, Misinterpretation, Misrepresentation and Misinformation, described in the context of case reports, clinical trials, qualitative research, and literature reviews. The first three fallacies were described as being related to errors, limitations or lack of information within the study publication. The fourth one can be prevented by osteopaths themselves by identifying when information is unreliable and should not be transmitted to patients and colleagues, including via social media. As clinicians it is important to remember that the further away the evidence is from what clinical practice looks like, the more care needs to be taken in the interpretation and extrapolation to clinical decision-making. This masterclass aimed at equipping clinicians in how to assess information and evidence related to clinical practice - a challenge as an ever-growing amount of evidence is shared and available. One of the limitations of this masterclass is the lack of specific tools for clinicians to use. Instead, we would like to draw the readers' attention to free resources that were specially developed for clinicians to assess research publication quality (Critical Appraisals Skills Programme 2021). Assessing the strength of evidence, however, provides little indications on what to do as a clinician in the absence of evidence. We may need to use less reliable knowledge, requiring even more careful interpretation. We would recommend readers to read (Leboeuf-Yde, Lanlo et al. 2013) on this topic. As clinicians, our knowledge, values and beliefs influence our patient management. Being able to decide whether to trust what we read is essential to the profession and to patients.

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Statement for conflict of interest

Paul Vaucher is an independent clinician providing osteopathic care and research services in osteopathic science. He also is a board of trustees member of COME collaboration, a Foundation promoting science in osteopathic medicine, and holds a position as Full Professor at the University of Applied Sciences and Arts Western Switzerland to provide support and assistance in osteopathic research and teaching in methodology and ethics. He is an editor at Mains Libres, a reviewer for IJOM and other medical indexed journals, and is scientific advisor for the Swiss Osteopathic Science Foundation.

Oliver P Thomson is an Associate Editor for IJOM but had no role in the reviewing process and decisions regarding this paper and is the curator and host of The Words Matter Podcast referenced in this masterclass.

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Implications for Practice

- This Masterclass helps osteopaths to make sense of research and decide how/when to apply research findings in their clinical practice;
- A simple framework to assess the literature is provided;
- Case reports, clinical trials, qualitative research, and reviews are detailed specifically.