**The effectiveness of online pain resources for health professionals: a systematic review with subset meta-analysis of educational intervention studies**

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**Abstract**

Online educational interventions are increasingly developed for health professionals and students, although graduate and undergraduate medical curricular often contain limited information about how to assess and manage pain. This study reviews the literature on the effectiveness of pain-related online educational resources. Studies were identified via a search of Medline, PsychINFO, Web of Science, CINAHL, PubMed, Scopus, Cochrane Library, Google Scholar and OpenGrey databases. Search terms included three concept blocks: (i) type of intervention - online education, computer-based, e-learning, web-based, and internet-based; (ii) population – pediatrician, physician, nurse, psychologist, medical; (iii) outcome - pain\*. Thirty-two studies (13 randomised controlled trials, 5 non-randomised controlled trials, 14 single-group pre-post studies) were included. Ten provided data for inclusion in a series of between-groups meta-analyses. Post-intervention, participants receiving online instruction had significantly greater knowledge compared to those receiving training as usual/alternative training (Hedges’ *g* = 0.80, 95% CI: 0.12, 1.49), and students had significantly greater skills compared to students receiving training as usual (*g* = 1.34, CI: 0.38, 2.30). No significant differences were found for confidence/competence (*g* = 0.02, CI: -0.79. 0.84) or attitudes/beliefs (*g* = 0.16, CI: -0.48, 0.79). Although online educational resources show promise in improving learner knowledge, considerable heterogeneity exists between studies in quality, design, educational content, and outcomes. Further methodologically robust RCTs are required to establish the effectiveness of online educational interventions and a greater understanding of the key features of successful online resources, including cognitive interactivity. Few studies assessed health outcomes for patients, remaining a major priority for future investigations.

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Pain, particularly when it is chronic, is a significant clinical problem for both children and adults [23; 48; 54]. Despite its high prevalence and the considerable individual suffering and societal cost associated with it [8; 9], graduate and undergraduate medical curricular contain limited information about how to assess and manage pain [10; 59]. The situation is similar in the training of other health care professionals, and only at specialty level the topic is covered in sufficient depth and breadth [1]. To address this gap there has recently been a proliferation of online resources that address specific pain types (acute, chronic, end of life), populations (children, adults) or more generic topics (assessment). It is unclear, however, how effective these resources are and what features enhance their effectiveness.

In a meta-analysis of internet-based instruction for health professions learners internet-based learning was associated with large positive effects for knowledge outcomes, skills, learner behaviours and patient effects compared with no intervention, but was similarly effective to traditional teaching methods for knowledge, satisfaction and skills [19]. Studies that have explored factors affecting the experience and success of online learning (e.g., [13; 15; 64; 68]), have focused largely on learner characteristics. Nevertheless, factors such as interactivity, flexibility, and course design have been advocated as important on learning outcomes and retention [37; 83]. The aim of the present study was to review the literature pertaining to pain-related online educational resources for health professionals and ascertain their effectiveness on knowledge, confidence/competence, attitudes/beliefs, skills, and clinical practice outcomes, along with features that may enhance it.

**Method**

PRISMA guidelines were followed [61] and the protocol [53] was registered on PROSPERO (CRD42017059619) [71].

**Literature Search**

Studies were identified via a search of Medline, PsychINFO, CINAHL (title, subject terms, abstract) Web of Science (title), PubMed (title/abstract), Scopus (title, abstract), and Cochrane Library databases (Title/Abstract/Keywords). The grey literature was searched via Google Scholar (title) OpenGrey (main search field), and the reference lists of included studies. Search terms included three concept blocks: (i) type of intervention - *online education*, *computer-based*, *e-learning*, *web-based*, and *internet-based*; (ii) population – *pediatrician*, *physician*, *nurse*, *psychologist*, *medical*, (iii) outcome - *pain*\* (full details are provided in Supplementary Material 1). Searches were made between 1st January 1995 and 31st May 2017 (by 1995 the internet had become privatized and commercialized with navigation easier via the growing use of graphical browsers [81]). An anonymous reviewer suggested adding several terms to the population concept block (*physiotherapy*, *occupational* *therapy*, *interprofessional*, *chiropractor*, *osteopath* and *health* *profession*). An additional search was therefore conducted, with all other parameters remaining the same. Two additional studies were found and added to the review [7; 76], which are classified as ‘Additional records identified through other sources’ in the PRISMA flow diagram (Figure 1). Search results are references were transferred into the digital reference manager Endnote X7, which were then scanned for duplication using an automated search engine within the software. All titles and abstracts were then screened independently by two review authors (AF and DS) to identify potentially eligible studies. The full text of these potentially eligible studies were then retrieved and assessed for eligibility independently by the same two authors. Disagreements at any stage were resolved via discussion with a third author (CL).

**Inclusion Criteria**

Studies were required to meet the following criteria to be included in the review:

1. Available in English language.
2. Used an Internet-based system for training and evaluating outcomes of online pain education material on one or more of the following outcomes of interest: a) knowledge, b) skills, c) attitudes/beliefs, d) clinical practice, e) health outcomes for patients.
3. Included a sample of health professionals at any stage of practice and/or medical students at any stage of training.

In order to provide as comprehensive review of the literature as possible, randomised and non-randomised controlled trials and also single-group pre-post studies were included.

Studies were excluded if they i) reported no outcomes of interest, ii) reported satisfaction or feasibility of the intervention only, iii) used a single-group post-test only design, iv) evaluated a computer intervention that resided only on the client computer or CD-ROM, or v) made use of the Internet for administrative or secretarial purposes only. In order to provide as comprehensive review of the literature as possible, randomised and non-randomised controlled trials and also single-group pre-post studies were included.

**Data Extraction**

Data from eligible studies were extracted into standardized, pre-piloted forms (developed by CL and DS) by one member of the research team (DS), which were subsequently checked for accuracy by another member of the team (CL). For inclusion in the analyses studies had to provide data pertaining to, or enabling the calculation of, effect sizes and standard deviations. Study authors were contacted with requests for missing information pertaining to analyses or quality judgements.

**Study Quality Assessment**

The quality of randomised and non-randomised controlled trials were assessed using the Cochrane Risk of Bias tool [39] on domains assessing selection bias (random sequence generation, allocation concealment), performance bias (blinding of participants and personnel), detection bias (blinding of outcome assessment), attrition bias (incomplete outcome data), and reporting bias (selective reporting). The tool was operationalised as intended by the Cochrane Collaboration [39; 40], which involved rating each study as having low, high or unclear risk of bias on each of the six domains. Support for each judgment included a comment and, where application, a quote from the published report. Two authors (DS and AF) independently performed the risk of bias assessment (Kappa = .874), with any disagreements resolved by discussion where necessary with a third author (CL) (Table S1).

Single-group pre-post studies were assessed using the Quality Assessment Tool for Before-After (Pre-Post) Studies with No Control Group [63] with minor modifications to wording to improve applicability to the present review (e.g., simplifying references to ‘test/service/intervention’ to ‘intervention’). The tool includes 12 items phrased as yes/no questions chosen to help reviewers assess the internal validity of a study. Items relate to the study question, eligibility criteria and study population, representativeness of study participants to the population of interest, enrolment of all eligible participants, sample size, description of intervention, outcome measures used, blinding of outcome assessors, follow-up rate, statistical methods, number of post-intervention tests, and use of group-level statistical analyses for individual-level data. The tool was operationalised as intended by National Institutes of Health [63], which involved answering each item as ‘yes’, ‘no’, ‘not reported’, ‘not applicable’ or ‘cannot determine’. A comment was provided as support for each ‘yes’/’no’ judgement made. Two authors (DS and AF) independently performed the assessment (Kappa = .949), with any disagreements resolved by discussion where necessary with a third author (CL) (Table S2).

According to the GRADE (Grading of Recommendations Assessment, Development and Evaluation) working group, if the total number of participants in a systemic review is less than that required for a single adequately powered study of the intervention (a threshold known as the optimal information size) the quality of evidence may be downgraded [31]. A power calculation was therefore conducted in GPower [26] using conventional statistical parameters. A two-tailed test was conducted as these are much more common in scientific research than one-tailed tests [27]. Cohen [16] proposed benchmarks for small (*d* = .3), medium (*d* = .5) and large (*d* = .8) effect sizes. A medium effect size (*d* = .50) [16] was selected as a conservative estimate based on these benchmarks, and also following a preliminary review of the relevant literature (specifying an effect size is typically the most difficult part of performing a power analysis [17]). A power level of .80 was entered as it has been recommended researchers should aim for sample sizes providing at least this level of power [16]. An alpha of .05 was entered as this is a commonly used, albeit arbitrary, cut-off point for testing statistical significance [62; 88].

**Meta-Analytic Procedures**

Data was analysed where possible for knowledge, confidence/competence, attitudes/beliefs, skills, and clinical practice outcomes. Confidence/competence was not a planned analysis, but was identified as an outcome after obtaining and reading the eligible studies included in the review. There was insufficient data to meta-analyse health outcomes for patients as we had initially intended. A description of each outcome variable (adapted from [19]) is provided in Table S3. Between-groups analyses examined differences between health professionals/medical students receiving online training versus no training, and online training versus training as usual/alternate training interventions. Within-groups analyses examined differences between pre- and post-test outcomes for participants receiving online training.

**Between-groups analyses**. Hedges’ adjusted *g* effect sizes (standardized mean difference) for between-group comparisons were computed using group means and standard deviations in Review Manager 5.3 [75]. A random-effects model was used, which assumes the average effect size varies between studies, and therefore heterogeneity is to be expected [5; 6]. Although random-effects models have less statistical power than fixed-effects models, their use is advocated as results may be generalised to similar studies not included in the actual analysis [6; 78]. Cochrane’s Q and the I² statistic were used to assess study heterogeneity. With Cochrane’s Q, a significant result is indicative of heterogeneity. The I² statistic describes the percentage of variability in effect estimates due to heterogeneity as opposed to sampling error [40].

**Within-groups analyses**. Cohen’s *d* effect sizes were computed for pre- versus post-test comparisons based on study means and the average standard deviations [20; 49]. A random-effects model was used to compute average effect sizes using ESCI [20]. An unbiased estimate of the population effect size, referred to as *d*unb, was calculated [20]. As *d* overestimates the population effect size, especially for smaller sample sizes, the adjustment is advocated [20; 21; 38]. A positive effect indicates higher post-test scores, whereas a negative score indicates higher pre-test scores. Cochrane’s Q and the I² statistic were used to assess study heterogeneity.

**Moderators of intervention outcomes and funnel plots.** Where data from 10 or more studies was available [40] meta-regression was performed [55] to examine whether cognitive interactivity ratings, length of time, duration, and number of sessions were predictive of effect size. Each moderator was included as a binary predictor variable (i.e., interactivity - lower/higher; length of time and duration – shorter/longer; number of sessions – single session/multiple sessions) [27]. Similarly, small study effects were assessed via funnel plots for analyses including ten or more studies [40] (Figure S1).

**Methodological Decisions**

Methodological decisions pertaining to the meta-analyses are described in Supplementary Material 2.

**Results**

S**earch Results**

The literature search and study selection process is shown in Figure 1. From an initial identification of 3248 records, 32 studies meeting the inclusion criteria were retained for the review, of which 24 provided data for inclusion in the subset meta-analyses. Fourteen studies had a single-group pre-post design, and 18 were classified as randomised or non-randomised control trial (13 and 5 studies respectively). A description of each study is provided in Table S4, and a description of the education intervention used in Table S5. The most common reason for record exclusion was that the study only discussed the development or rationale for online education, or provided no data on relevant outcomes. Additional but less common reasons included the fact that the intervention did not explore online education for pain education material specifically, the intervention was not provided online, or that full results were not provided in English.

**Narrative Review**

**Summary of Identified Studies**

Thirty-two studies met the eligibility criteria for this review. Eighteen were conducted in the USA and the remaining in Canada, Brazil, the UK, Finland, the Netherlands, Italy, Germany, Spain, Hong Kong and Australia. The majority of studies did not report sufficient details on the intervention development team or the precise roles performed by different members. Most interventions were designed for qualified physicians/clinicians, although nine were designed specifically for medical students. Adult pain was most commonly covered in the educational materials. Only six studies reported covering paediatric pain specifically, and two studies reported covering both adult and paediatric pain. Thirteen studies did not explicitly state whether their educational materials covered adult or paediatric pain however, although careful reading suggests adult pain was covered in all of these (with one also covering paediatric pain). Failure to provide key details such as this is a significant problem however [43].

Considering specific topics, nine studies focused on chronic pain specifically, five studies acute pain, seven studies palliative care, with the remainder less specific (e.g., covering pain management or pain assessment generally). Ten studies provided details on the intervention learning outcomes in the report. Interventions varied widely in their contents and structure, although most featured a single module/course which required 60 minutes or more to complete, and could be accessed across multiple sessions/log-ins. Twelve studies were rated as possessing higher levels of cognitive interactivity, with the remaining rated as having lower levels. The software used to develop the interventions were described in eight studies only. Ten studies provided a URL for the intervention, although four of these were no longer accessible at the time of writing this report (September 2017).

**Methodological Quality**

The risk of bias ratings for randomised and non-randomised controlled trials (Table S1) showed nine out of seventeen studies to have a high risk of bias for ‘random sequence generation’ as they did not report randomising participants to intervention or control groups. Only four studies had a low risk of bias for ‘allocation concealment’, whereby the randomisation method likely ensuring allocation concealment. All studies were considered to have a low risk of bias for ‘blinding of participants and personnel’. It was impractical in the majority of cases for participants and personnel to be blinded to condition, but also unlikely that outcomes would be influenced by this. All but two studies were also rated as having a low risk of bias for ‘blinding of outcome assessment’. Either the outcome assessor was blinded to the participant’s condition, or the outcome was based on self-report data/medical documentation and therefore unlikely to be influenced by assessor knowledge of group. The remaining two studies were rated as having an unclear risk of bias as the outcome measures used were scored by the assessor who was not blinded to group. Ratings for ‘Incomplete outcome data’ were the most diverse. Nine studies were rated as having a low risk of bias as either no attrition was reported, or attrition appeared balanced across groups or an intent-to-treat analysis was performed. Four studies were rated as having a high risk of bias as data did either not appear missing at random or intent-to-treat analysis was not performed. The remaining studies were rated as having an unclear risk of bias due to lack of detail. All studies were considered to have a low risk of bias for ‘selective reporting’ as all pre-specified outcomes were discussed in the reports.

Considering methodological quality assessments for pre-post group interventions (Table S2), all studies stated the study question or objective, and where applicable stated eligibility criteria. All but one study recruited participants representative of the intended intervention target audience, although only four enrolled all eligible participants who met the pre-specified entry criteria. The intervention was clearly described and delivered consistently in eleven studies, and eleven studies also reported all ­relevant *p*-values. A number of notable limitations were apparent however. None of the studies provided a power calculation which would provide confidence that a large enough sample had been recruited, and ten did not report psychometric properties of the outcome measures used. It was only clear in three studies that no attrition had occurred, and three studies reported attrition greater than 20%. In the remainder it was either unclear how missing data had been accounted for, or an intent-to-treat analysis was not performed. Furthermore, none of the studies reported that the people assessing the outcomes were blinded to the participants’ exposure or intervention, and only one study recorded relevant outcome measures at multiple times following the intervention. Two studies conducted the intervention at a group level but did not take into account the use of individual-level data in their statistical analyses.

**Knowledge**

Knowledge was assessed in 22 studies, two using standardized measures [36; 89], one assessing knowledge through academic assignments [52], three adapting questions from previous resources [25; 47; 86], and the remainder used specifically developed questions/questionnaires. Twelve studies reported significant gains in knowledge pre- to post-intervention [2-4; 25; 35; 36; 58; 67; 70; 73; 79; 89], one reported gains but did not include details on statistical significance [74], two reported no significant gains [86; 94], and two reported a decrease in knowledge scores [11; 45]. Few studies assessed knowledge at a subsequent follow-up, with two reporting significant gains in knowledge were maintained [36; 89], one reporting knowledge scores remained higher than pre-intervention scores but it was not clear if significantly so [66], and one reporting significantly lower scores during a retention test [45]. Overall, these studies varied in quality, some of which reported high levels of attrition [2; 70; 86]. There was no notable difference in quality between studies which reported significant gains and those which did not however.

Twelve studies compared knowledge outcomes in an intervention group to a control condition. Of the studies which included a training as usual/alternative training control group, three reported significantly greater gains in the online intervention group [47; 52; 87], one reported greater knowledge scores in the online intervention group but it was not clear if this was statistically significant [58], two reported no significant differences between groups [36; 76] ([36] received ratings of low risk of bias on each domain of the Cochrane Risk of Bias tool), one reported significantly greater gains in the alternative training than the online group (different educational subjects in each group) [45], and one reported no difference between high and low interactivity online training groups [89]. Of the studies which included a no-training control group, two reported significant greater knowledge scores post-intervention in the intervention group [67; 74], which for one study was maintained at follow-up [66]. One study reported no significant differences between groups [11] although had very small sample size of ten participants and four completers per group.

**Confidence/competence**

Confidence/competence was assessed in ten studies, all which used specifically developed questions/questionnaires, except one [70] which adapted questions from previous resources. Four studies (two adult and two paediatric) reported significant increases pre- to post-intervention in the intervention group [3; 12; 67; 70], although for one [67] improvements were shown for confidence in communication but not symptom management. One study reported higher confidence scores post-intervention although these were not statistically significant [86], and one study reported no significant increases although had a very small sample size [11]. One study reported post-intervention data only, finding 54% of participants were confident in assessing paediatric pain [35].

A number of RCTs compared post-intervention scores between groups, with one study reporting significantly greater confidence in the intervention group compared to a no-training control group [67], although this was not maintained at 18-month follow-up [66]. Another study reported significantly greater confidence in the intervention group compared to an alternative training control group [87], and another reported higher confidence in the intervention group compared to a no-intervention control group although statistical significance was not reported [12]. Two studies reported significantly higher confidence in the alternative training control group than the online intervention group [76; 90], although for one study this difference was only found in regards to delivering single cognitive behavioural sessions, with no significant differences found in regards to delivering six group sessions [76]. One study reported no significant differences between intervention and control groups [11], although as mentioned had a very small sample size of four completers per group.

**Attitudes/beliefs**

Attitudes/beliefs were assessed in nine studies, three using standardized measures [36; 76; 89] and the remainder using specifically developed questions/questionnaires or adapting one adapting questions from previous resources. Of the single-group intervention studies, two focused on paediatric pain. One study with a large sample reported significant improvements in beliefs concerning legitimacy of self-reported pain, use of analgesics and effects of pain [91], and the other with a much smaller sample reported significant improvement in attitudes towards paediatric tolerance of opioids, opioids delaying diagnosis, children’s accuracy of pain reports, and believing what paediatric patients say about their pain [3]. Of the two studies focused on adult pain, which overall were both of a low quality, one found significant improvements in attitudes towards the importance of addressing pain at each clinic visit [4]. The other [7] provided survey data regarding procedural planning and performance before and after implementation of a program to teach interprofessional approaches to management of procedural pain, although sample characteristics were not provided and it is likely that different clinicians were surveyed at the two time-points. At baseline 100% of physicians agreed or strongly agreed that pain and anxiety were well controlled, compared with 96% of physicians post-implementation.

Considering the RCTs which varied in quality, one found the attitude ‘Good palliative care enables good death’ was significantly higher in the educational group than the no-training control group post-intervention [41]. Another study with a very small sample size found nurses in the intervention group showed 43% improvement pre- to post-intervention in pain attitudes, although there was no difference between intervention and no-training control groups post-intervention [11]. One study found no significant increases in agreement to the statement ‘it is important to prevent children’s pain and distress during immunization injections’ for intervention sites [12]. One study found physicians receiving high interactivity online training were less likely to endorse using tamper-resistant formulations and dosing across time than those receiving low interactivity online training [89]. Examining change scores from baseline to post-intervention, one study found participants in the control group showed a significantly greater reduction in biomedical attitudes to low back pain management compared to the online intervention group, but no significant difference for psychosocial attitudes [76].

**Skills**

Only five studies assessed clinical skills, each using a different measure as described. There were two single-group intervention studies, both with interventions focused on paediatric pain. One used a standardized questionnaire and reported scores for both pain assessment and administration of opioids increased significantly from pre- to post-intervention [91]. Another with a large sample size used specifically developed questions, finding a significant increase in responses of ‘frequently’ or ‘very frequently’ on the use of behavioural instruments for assessing paediatric pain severity, and the prescription of opioids on a fixed schedule and as needed for initial post-operative pain. Increases were also shown for recommending nonpharmacologic treatment options and the use of local anaesthetics prior to venepuncture, although these were not statistically significant [3].

Three RCTs based on adult pain assessed skills. One study, which received ratings of low risk of bias on each domain of the Cochrane Risk of Bias tool, used a checklist completed by a faculty observer and a post encounter subjective-objective assessment plan (SOAP) note. Online training produced a significantly higher score on the abdominal pain SOAP note than the standardized patient training, although no differences between groups were shown for headache [90]. Another study assessed skills using an objective structure clinical examination (OSCE), reporting that students who completed the online training module performed significantly better in the OSCE than students not exposed to the module [93]. The most recent study assessed skills via the Cognitive-Therapy Scale-Revised-Pain (CTS-R-Pain) which was completed based on a 15 minute recorded treatment session. No significant differences were found between participants completing online training and those completing a face-to-face workshop [76].

**Clinical Practice**

Eleven studies included clinical practice as an outcome, six of which were assessed via inspection of data from medical records [35; 44; 46; 70; 85; 91] and the remainder via specifically designed questions/questionnaires. Considering single-group pre-post interventions, one large study of sixteen nursing homes reported a significant increase in residents with Alzheimer’s/dementia in nursing homes identified with mild to severe pain over time, and a significant decrease in residents with undetected pain [85]. One study with a high attrition rate reported improvements in nurses’ analgesic administration [91], while another reported a significant increase in pain intensity documentation pre- to post-intervention [70]. Srivasatava and colleagues stated that after the intervention most participants reported they had started using or increased their use of treatment agreements and urine drug screening, although data were not provided [86]. Another study reported no significant improvements in adherence to the Dutch Clinical Practice Guideline (CPG) in paediatric palliative care, although had a very small sample size of five paediatric nurse specialists [44]. Habich and colleagues [35] reviewed sixty patient medical records two weeks after the educational intervention, reporting among other findings that 88% of all pain assessments at triage, post-intervention and prior to discharge were documented using an appropriate pain scale and included a pain score.

RCTs varied in quality, although all reported some significant effects. One study reported the use of at least one newly recommended pain-relieving strategy, and willingness to use new strategies, significantly increased pre- to post-intervention for the intervention group but not the no-training control group [12]. Another study found that at follow-up, compared to pre-intervention, both intervention and alternative training control groups were less likely to prescribe opioids when the patient requested them, more likely to have the patient sign an opioid contract, and more likely to have the patient complete a treatment agreement [87]. Kalinowski and colleagues [46] randomised nursing homes to either the intervention or no-training control group. A significant increase in the number of nursing home residents prescribed at least one nonpharmacological therapy from baseline to 3-month follow up was found in the intervention but not the control group. At 3-month follow-up significantly more nursing home residents in the intervention group than the control group were prescribed therapeutic nonpharmacological therapies. No significant difference was found at 6-month follow-up. Mistraletti and colleagues [60] found use of pain assessments using validated tools significantly increased from pre- to post-test in both intervention and control groups (the latter received the same training at a later time), remaining high at 3 and 6 month follow-up periods. Use was also significantly higher in the intervention group post-intervention than the control group pre-intervention. Most recently, Trudeau and colleagues reported both high and low interactivity online training groups showed significant improvements pre-intervention to follow-up in behaviours related to opioid prescribing [89].

**Health Outcomes for Patients**

Only three studies reported health outcomes for patients, two from medical records [70; 91] and one using standardized questionnaires [66]. Two single-group pre-post studies reported a significant reduction in pain ratings post-intervention for paediatric [91] and adult pain [70], although one had a particularly high attrition rate [70] and the other a small sample size [91]. In the only RCT [66], changes over time in the severity of symptoms, family anxiety, and overall quality of life were not statistically significant. Furthermore, no significant differences were found between intervention and no-training control groups, despite this study being rated as having a low risk of bias on five of six domains.

**Meta-analysis Results**

Full details for each meta-analysis are provided in Tables 1 and 2, and forest plots of pooled effect sizes are provided in Figures 2 and 3 (individual forest plots for each separate analysis are provided in Figures S2 and S3). Sensitivity analysis was conducted where evidence of significant heterogeneity was found.

**Between-Groups Analyses**

A power calculation revealed the optimal information size for between-groups analyses to be 128 participants. Six studies with knowledge as an outcome compared online training versus training as usual/alternative training and provided eligible data [36; 47; 52; 58; 76; 87] (Analyses 1: intervention *n* = 220, control *n* = 334, Hedges’ *g* = 0.80 [95% CI: 0.12, 1.49], *p* = .020, Cochrane’s Q = 57.66, I2 = 91%. Analysis 2 (sensitivity analysis): intervention *n* = 141, control *n* = 273, Hedges’ *g* = 0.53 [95% CI: 0.11, 0.96], *p* = .010, Q = 13.39, I2 = 70%). Analysis was also conducted comparing online training versus training as usual in medical students only, with eligible data from three studies [47; 52; 87] (Analysis 3: intervention *n* = 134, control *n* = 269, Hedges’ *g* = 1.28 [95% CI: 0.31, 2.24], *p* = .009, Q = 27.88, I2 = 93%. Analysis 4 (sensitivity analysis): intervention *n* = 55, control *n* = 198, Hedges’ *g* = 0.84 [95% CI: 0.12, 1.56], *p* = .020, Q = 4.66, I2 = 79%).

Two studies with eligible data comparing online training versus alternative training [76; 87] included confidence/competence as an outcome (Analysis 5: intervention *n* = 103, control *n* = 101, Hedges’ *g* = 0.02 [95% CI: -0.79. 0.84], *p* = .950, Q = 4.56, I2 = 78%). Two studies with eligible data comparing online training versus no training [12; 41] included attitudes/beliefs as outcomes (Analysis 6: intervention *n* = 67, control *n* = 58, Hedges’ *g* = 0.16 [95% CI: -0.48, 0.79], *p* = .630, Q = 2.79, I2 = 64%). Three studies with eligible data compared online training versus training as usual/alternative training [76; 90; 93] included skills as outcomes (Analysis 7: intervention *n* = 48, control *n* = 49, Hedges’ *g* = 0.77 [95% CI: -0.45, 1.99], *p* = .220, Q =13.17, I2 = 85%. Analysis 8 (sensitivity analysis): intervention *n* = 21, control *n* = 21, Hedges’ *g* = 0.28 [95% CI: -0.19, 1.58], *p* = .78, Q = 3.97, I2 = 75%). Analysis was also conducted comparing online training versus training as usual in medical students only with eligible data from two studies, which also served as another sensitivity analysis for Analysis 7 (Analysis 9: intervention *n* = 43, control *n* = 42, Hedges’ *g* = 1.34 [95% CI: 0.38, 2.30], *p* = .006, Q = 3.81, I2 = 74%).

**Within-Groups Analyses**

Fourteen studies measured knowledge outcomes pre- and post-intervention and provided eligible data [2; 3; 35; 36; 45; 58; 66; 67; 70; 73; 74; 79; 87; 89; 94] (health professionals and medical students combined: Analysis 1: *N* = 1590, *d*unb= 0.97 [95% CI: 0.66, 1.29], *p* < .0001, Q = 311, I2 = 96%. Analysis 2 (sensitivity analysis): *N* = 1562, *d*unb= 0.86 [95% CI: 0.55, 1.17], *p* < .0001, Q = 279.89, I2 = 95%). Analyses were also conducted in medical students only (Analysis 3: *N* = 1070, *d*unb= 1.33 [95% CI: 0.70, 1.96], *p* < .0001, Q = 283.87, I2 = 98%. Analysis 4 (sensitivity analysis): *N* = 1042, *d*unb= 1.10 [95% CI: 0.46, 1.73], *p* = .0007, Q = 254.54, I2 = 98%). Four studies collected data at a subsequent follow-up point [36; 45; 66; 89] (Analysis 5: *N* = 348, *d*unb= 0.26 [95% CI: -0.13, 0.65], *p* = .190, Q = 44.54, I2 = 91%. Analysis 6 (sensitivity analysis): *N* = 308, *d*unb= 0.48 [95% CI: 0.31, 0.65], *p* < .0001, Q = 5.52, I2 = 45%).

Three studies measured confidence/competence outcomes pre- and post-training and provided eligible data [67; 70; 87] (health professionals and medical students combined: Analysis 7: *N* = 182, *d*unb= 0.58 [95% CI: 0.30, 0.86], *p* = .0001, Q = 6.11, I2 = 67%). Analysis 8 (sensitivity analysis): *N* = 94, *d*unb= 0.50 [95% CI: 0.10, 0.89], *p* = .014, Q = 3.14, I2 = 68%). Six studies measured attitude/belief outcomes pre- and post-training and provided eligible data [4; 12; 41; 76; 89; 91] (health professionals and medical students combined: Analysis 9 : *N* = 432, *d*unb= 0.49 [95% CI: 0.17, 0.80], *p* = .002, Q = 51.03, I2 = 88%. Analysis 10 (sensitivity analysis): *N* = 408, *d*unb= 0.33 [95% CI: 0.07, 0.59], *p* = .013, Q = 28.81, I2 = 83%). Four studies examined clinical practice outcomes pre- and post-training and provided eligible data [12; 87; 89; 91] (health professionals and medical students combined: Analysis 11: *N* = 377, *d*unb= 0.34 [95% CI: 0.12, 0.55], *p* = .002, Q = 14.85, I2 = 73%. Analysis 12 (sensitivity analysis): *N* = 289, *d*unb= 0.42 [95% CI: 0.25, 0.58], *p* < .0001, Q = 4.79, I2 = 37%).

**Meta-regression Analyses**

Meta–regression was conducted for the within-groups analysis of knowledge comparing pre- and post-intervention scores in health professionals and medical students combined (analysis 1 in Table 2). The hierarchical regression models were not significant for cognitive interactivity (Q = 0.79, *p* = .38, *R*2 = .05, *k* = 15), length of time (Q = 0.73, *p* = .59, *R*2 = .03, *k* = 10), number of sessions (Q = 0.05, *p* = .83, *R*2 = .004, *k* = 14) or duration, (Q = 0.05, *p* = .83, *R*2 = .004, *k* = 13). Non-significant results were also found for the within-groups sensitivity analysis of knowledge (analysis 2 in Table 2); cognitive interactivity (Q = 1.20, *p* = .27, *R*2 = .09, *k* = 14), number of sessions (Q = 0.02, *p* = .90, *R*2 = .002, *k* = 13) or duration, (Q = 0.03, *p* = .88, *R*2 = .003, *k* = 12).

**Funnel Plots**

There was evidence of asymmetry in the within-groups analysis of knowledge and also in the subsequent sensitivity analysis which removed the study with the largest effect size [73]. Caution has been advocated in the interpretation of funnel plots [20; 40], although the asymmetry observed may be due to publication bias, or possibly heterogeneity between studies which itself may have arisen due to variations in intervention design.

**Discussion**

The aim of this review was to provide an overview of the online pain education literature and determine if online-based pain instruction is associated with improved outcomes in health professionals and students compared with no intervention and traditional instructional methods. Between-groups meta-analysis revealed that, post-intervention, participants receiving online instruction had significantly greater knowledge compared to those receiving training as usual/alternative training (health professionals and students). Although most of the confidence intervals overlapped [31; 33; 40], significant statistical heterogeneity exists between these studies even after performing sensitivity analyses. This likely stems from variations in quality, design and educational content. As such, and in combination with many high risk of bias ratings some of these RCTs received, we interpret these results tentatively and advise caution [33] pending further high quality RCTs. The between-groups analysis also revealed significantly greater skills in participants receiving online instruction compared to those receiving training as usual (students), although once again significant heterogeneity is present warranting caution in interpretation of results. No significant differences were found in attitudes/beliefs compared to those receiving no training (health professionals) and no differences in confidence/competence compared to those receiving training as usual/alternative training (health professionals and students). Data from two studies only were available for each of these analyses however, and we therefore consider the effectiveness of online pain resources for health professionals in terms of attitudes/beliefs and confidence/competence to be uncertain at present.

Although RCTs are higher in the evidence hierarchy than single-group or observational studies [30; 34], we nevertheless included single-group pre-post studies to provide as comprehensive review of the literature as possible. Researchers can use our review to choose online educational interventions for further evaluation in RCTs based on their performance in a non-randomised investigation, and also further develop such interventions based on the educational content and structure of the existing ones that we review in detail (Table S5). Within-groups meta-analysis revealed significant improvements for health professionals and students pre- to post-intervention for knowledge, confidence/competence, attitudes/beliefs, and clinical practice. The obvious limitation with these analyses however is the lack of a no training or training as usual/alternative training control group with which to make comparisons of gains. Although the majority of confidence intervals overlap in most analyses, significant statistical heterogeneity is nevertheless found in all but three analyses (all of which were sensitivity analyses). As such, we recommend researchers move away from conducting single-group studies where possible.

Despite the tentative nature of the between-groups meta-analytic results, these are nevertheless in agreement with previous health-related reviews that have shown online interventions to be particularly effective in improving knowledge and skills [19; 28; 50; 92]. All studies included in these analyses tested the educational intervention on the intended population [32]. As noted the optimal information size [31] for between-groups analyses was 128; all between-groups analyses of knowledge had a sample size greater than 128, although skills analyses did not. A further limitation was the predominance of studies using specifically developed or adapted measures without reporting psychometric properties however (i.e., four of six studies in the knowledge analysis, both studies in the skills analysis), which therefore reduces confidence in results [34]. Improvements in attitudes, clinical practice and patient effects have been explored less frequently, and although some support has been provided in former reviews, studies have yielded inconsistent results (heterogeneous) [19; 50]. Although the gap between what is known and what gets done in practice (i.e., the know-do gap or the knowledge translation gap [51]) is well documented and various models have been described, an accepted standard approach has yet to be widely adopted. Specifically focusing on physicians, Pathman and colleagues [65] have proposed the ‘awareness–agreement–adoption–adherence’ model to describe the process. First, the physician becomes aware of a new finding or practice, moves to a process of agreement with it and then to an adoption of it, either on a trial or irregular basis. Finally, the physician adheres to the practice. These stages of learning are important when considering the effect of educational interventions, although disappointingly very few of the studies reviewed described the theoretical framework within which their intervention was developed. Translating knowledge into action within healthcare is a complex, evolving and dynamic process, and systematic approaches to address it are urgently needed.

The development of new and innovative technologies in the provision of e-learning is growing exponentially, and e-learning can potentially facilitate the three domains of medical education (i.e., knowledge, skills and attitudes) [14]. Online learning has a number of advantages enabling adult learners to tailor their learning according to their unique needs, giving them autonomy over their learning and increasing intrinsic motivation. It also facilitates the adoption of a reflective approach towards one’s own practice therefore promoting enhanced learning. The attributes of accessibility and convenience are also important because learners decide when and where they are receptive to learning. Although there are many advantages of online learning, challenges and disadvantages are also present, including social isolation, de-individualised instruction, cost, technical problems and poor instructional design [18; 84]. While the majority of studies in the present review reported some form of participant evaluation of the educational intervention itself, the precise questions and format varied. We strongly encourage both quantitative and qualitative evaluation by the learners as standard in future research.

It is true that the amount of pain-related online instruction for health professionals has increased dramatically in the last ten years, although most has not been rigorously evaluated. In fact, evaluation of online learning in general has been characterized as in its infancy. A high number of records were excluded from the present review as they only discussed the development or rationale for online education, and provided no actual test of the intervention or data on relevant outcomes. Furthermore, the single group pre-test post-test design was a common research design adopted by fourteen studies in the present review, although as stated above the obvious limitation of this approach is the lack of a comparison group. Direct comparison of results from randomised and non-randomised trials should only be made with caution, as these two approaches provide answers to different questions. However, it is interesting to note that gains in knowledge were observed in individuals receiving online training relative to both pre-intervention measures of knowledge and also relative to participants completing training as usual or alternative training. A further limitation of the reviewed literature is that most research has focused on participants’ satisfaction and not on change in clinical practice or impact on patient and health outcomes. While assessing the effect on patient’s health status can be constrained by ethical and logistical considerations in gaining access to data, assessing whether learners have tried to use their competence in practice seems crucial in an assessment of the effectiveness of a learning intervention.

Despite the clear importance of evaluating interventions, their description in published reports remains worryingly poor, which itself presents significant difficulties for researchers wishing to conduct replications [43]. The body of research included in the present review is sadly no different to recent reviews of pharmacological and non-pharmacological interventions (e.g.,[24; 42]), with key intervention details missing from many reports including whether educational materials covered adult or paediatric pain, intervention learning outcomes, and the software used to develop and host the intervention. The inclusion in the published report of a URL to an actual or demonstration version of the intervention would provide a simple yet effective way of disseminating the precise intervention contents and outcome/assessment methods used, yet this was only included in 10 reports (with only working in six instances at time of writing). We strongly recommend future studies adopt this strategy, as it is likely an exact replication could only be achieved via this method. Recently developed guidelines have been proposed for the reporting of evidence-based educational interventions [69] and internet-based interventions [72] which we also encourage researchers to follow.

As noted many studies used questions/questionnaires specifically designed by the authors which had not been formally validated and of which no psychometric properties were reported. For example, knowledge outcomes in 12 studies were assessed in this manner. Specifically designed questions/questionnaires are tailored to the educational content of the online intervention, allowing the researchers to explore changes across time or differences between groups. Without knowledge of psychometric properties, however, confidence in the results is limited. Most studies also failed to provide details on statistical power which would provide confidence that a large enough sample had been recruited, and the majority only provided data at one point post-intervention thus making it unclear whether significant benefits are maintained over time.

The variations among instructional methods and the rapid advancement of technology make it difficult to determine which elements contribute to an effective online learning environment. Interactivity is considered essential for learning [57; 77]. Mixed results for the effectiveness of interactivity in multimedia learning have been reported however, likely stemming in part from different conceptualisations across studies [22]. Although limited analyses were performed, no evidence was found for interactivity as mediator of knowledge outcomes in the present review. Interventions varied quite widely in the degree of interactivity offered to learners, ranging from questions/cases with tailored feedback (e.g., [25; 70]) to virtual patients (e.g., [89; 90] and video game elements [36] (along with a number of studies which did not provide sufficient information on intervention interactivity). Only one study directly compared two online interventions with high and low levels of interactivity, finding no differences between groups on knowledge or clinical practices (although physicians in the higher interactivity MAP-PC group were less likely to endorse these over time compared to those in the lower interactivity active control group) [89]. There is evidence that higher levels of cognitive interactivity are associated with better learning outcomes in some studies (e.g., [29; 80]) but not others (e.g., [56; 82]). It has been suggested however that behavioural processing underlying excessive behavioural interactivity may in fact impede meaningful learning rather than facilitate it [82].

In conclusion, the results of this review and meta-analysis provide tentative support for the effectiveness of online pain educational resources at improving learner knowledge. Although other significant effects were found in the meta-analyses, the combination of significant statistical heterogeneity, small number of included studies and, for within-groups analyses, lack of comparison groups means these results should only be considered preliminary. Future studies would benefit from following recently developed guidelines in the reporting of educational interventions [69]. There is a need for greater understanding as to which features of online interventions are the most important, along with future RCTs specifically assessing health outcomes for patients.

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**Figure Legends**

Figure 1. Flow of records for inclusion in the narrative review and meta-analyses

Figure 2. Overall pooled effect sizes for each between-groups analysis on outcomes of knowledge, confidence/competence, attitudes/beliefs and skills, created in Microsoft Excel

Figure 3. Overall pooled effect sizes for each pre- versus post-intervention/follow-up analysis on outcomes of knowledge, confidence/competence, attitudes/beliefs and clinical practice, created in Microsoft Excel

**List of Supplemental Digital Content**

Supplementary Material 1 – Search Strategy

Supplementary Material 2 - Methodological decisions

Figure S1 – Funnel plots for within-subjects analyses with data from 10 or more studies.

Figure S2 – Forest plots created in Review Manager showing overall effect sizes for each

between-groups analysis conducted

Figure S3 – Forest plots created in ESCI showing of overall effect sizes for each within-

groups analysis conducted

Table S1 – Risk of bias assessments

Table S2 – Quality Assessments

Table S3 – Outcome variable and moderator definitions

Table S4 – Study characteristics

Table S5 – Description of study intervention