Abstract

Background: Simulation has demonstrated superiority over purely didactic instruction in multiple contexts, and educationalists have embraced this modality for enhancing access to clinical skills. However, there remains uncertainty if increasing the realism (fidelity) of simulation equipment heightens performance. To address this within nursing and allied health, this review examines if increasing equipment fidelity improves learning outcomes.

Methods: A systematic search of; CINAHL, Academic Search Complete, AMED; British Education Index, ERIC, MEDLINE, PsycARTICLES, PsycINFO, Maternity and Infant Care, INTERMID, Google Scholar, American Doctoral Dissertations, EThOS, ClinicalTrials.gov and ISRCTN registers was conducted for trials comparing two or more fidelity levels for knowledge, psychomotor or affective/non-technical outcomes. Data extraction and quality appraisal were performed and independently verified. Subgroup meta-analyses were undertaken (where viable), at post-intervention, intermediate, and long-term assessment time-frames.

Results: 18 RCTs and quasi-experimental trials containing ~1192 participants met the inclusion criteria. Almost ¾ of included trials exhibited high risk-of-bias. Training on higher-fidelity mannequins was associated with improved performance immediately post-intervention when compared with training on lower-fidelity mannequins for knowledge (p<0.00001) and psychomotor outcomes (p<0.00001). A similar directional effect for affective/non-technical skills was considered less robust due to substantial weaknesses in available studies. During follow-up testing at intermediate (1-3 weeks) and long-term (1-6 months) data points, there was insufficient evidence to determine any advantage in the use of higher fidelity mannequins. Repeated-intervention training was also insubstantially reported.

Conclusion: Higher-fidelity mannequins exhibited modest advantages when testing closely followed training. However results need to be confirmed using a larger number of high quality RCTs. A greater body of research using repeated-interventions and extended time-frames is also required before the influence of sustained training with alternative mannequins can be fully elucidated.

Keywords: Simulation, Fidelity, Knowledge, Psychomotor, Non-technical, Nursing, Allied Health, Systematic Review.

Introduction

Simulation has been defined as a strategy used by educators to recreate clinical tasks thereby allowing learners to acquire ‘skills, knowledge and behaviours’ within a safe environment (MacKinnon, 2011). Whilst simulation cannot replace actual clinical experience, learners can make mistakes, practice skills, and receive feedback, in order to increase competence and confidence before applying what they have learnt back in clinical practice (Maran and Glavin, 2003).

Reduced staffing, less time available for mentoring, and increased competition for high quality placements, challenges the ability of educators to expose students to enough cases at the appropriate acuity levels to establish competency (Higgins et al., 2010; Stayt and Merriman, 2013). The NMC and HCPC advocate the use of simulation to provide ‘patient-safe’ experiences that might otherwise be unavailable to students on placement (NMC, 2005; NMC, 2006; HCPC, 2014). National training initiatives using simulation to facilitate the development of non-technical skills is also occurring (NHS England, 2015), making simulation an increasingly significant component of both undergraduate and postgraduate training curricula. High-fidelity patient-simulators are full-body mannequins with extensive physiological feedback capabilities and the greater authenticity afforded by these mannequins is often assumed (rightly or wrongly) to possess a pedagogical advantage over simpler, less life-like models (Dunnington, 2014).

Although simulation technologies have an important role to play in enabling high-quality patient outcomes, educators need to demonstrate consistent educational gains in comparison to more traditional modes of teaching in the most cost-effective way possible (DoH, 2011). Increasing realism of simulator equipment invariably leads to higher levels of engineering, and the price of the simulator goes up (Maran and Glavin, 2003). High-fidelity simulation takes longer to develop, requires greater staffing, training, and technical support, all resulting in an increased drain on budgets (Alinier, 2011). Researchers have suggested that whilst higher-fidelity may produce moderate learning gains, these are not justified by the far greater costs involved (Schiavenato, 2009; Lapkin and Levett-Jones, 2011). There is some evidence that simulation training can also directly enhance patient outcomes (Boet et al., 2014: McGaghie et al., 2014), although the potential part simulator fidelity might play is currently unknown.

The majority of reviews comparing simulation to traditional (didactic) techniques point to its superiority across a range of knowledge, technical and non-technical skills (Cook et al., 2012; Yuan et al., 2012; Lapkin et al., 2010; Warren et al., 2016; McGaghie et al., 2011; Cooper et al., 2011). Additionally, some reviews have reported inconclusive results (Cant and Cooper, 2010; Warren et al., 2016). Although debriefing has been identified as the single most important design feature in enhancing learning outcomes during simulation (Shinnick et al., 2011; Decker et al., 2013; Levett-Jones and Lapkin, 2014), there remains less evidence on the relative influence of other components of simulation in different contexts. Few large-scale systematic reviews in nursing and allied health have focused on mannequin fidelity and learning outcomes. Shin et al., (2015b) and Kim et al., (2016) looked at simulation fidelity and concluded that overall medium-fidelity has the greatest utility. However, these reviews excluded studies comparing modes of simulation with each other, instead only including those with control groups receiving no or traditional education, preventing determination of the relative effect that different levels of fidelity may have in specific contexts. This review fills a gap by exclusively including comparative studies examining the influence of fidelity on knowledge, psychomotor and non-technical outcomes.

Methods

We have conducted a systematic review and meta-analyses on comparative quantitative experimental studies. Conduct and reporting was based on the PRISMA protocol for systematic reviews and meta-analyses (Moher et al., 2009) and the Cochrane Handbook for Interventions Version 5.1.0. (Higgins and Green, 2011).

Inclusion/exclusion criteria

RCTs and quasi-experimental designs were considered. Studies could be conducted on members of any healthcare profession (with the exception of medicine), undergoing assessed simulation in any context, and at any educational level (for example undergraduate or post-graduate), and from any country (if available in the English language). Studies containing mixed populations of medics and other health professionals were excluded unless data for this reviews target population/s could be extracted.

Fidelity is not one simply defined concept and ‘simulation-fidelity’ can be influenced by, but is not the same as ‘simulator-fidelity’ (Tun et al., 2015). Three dimensions of fidelity are proposed; realism of the patient (or mannequin), realism of the healthcare facility, and realism of the clinical scenario. Tun’s model closely mirrors that of Beaubien and Baker, (2004) which was adapted from the work of Rehmann et al., (1995), describing equipment fidelity, environment fidelity and psychological fidelity respectively. Simulation is used in education in a wide-variety of forms, however this review specifically examines the equipment fidelity component of simulation, and even more specifically the effect of mannequin realism within the simulation scenario. Therefore, studies where any of the following interventions were used as the sole comparator were excluded:

1. Virtual-reality computer simulations/gaming.
2. Role-play/case-studies.
3. Standardised patients.
4. Paper based simulations
5. Simulations involving cadaveric or animal tissue.
6. Audio generation

Studies were only considered if they compared two or more mannequin fidelity-levels categorised as high, medium, or low. Performance was scored by objective measures of:

1. Knowledge acquisition.
2. Psychomotor performance.
3. Demonstration of appropriate affective behaviours and non-technical skills (such as empathy, communication skills, critical-thinking, leadership and situation-management).

Objective measures were considered to be those that could be independently assessed by examiners using an assessment tool. This review excluded any studies which only compared subjective opinions or scores candidates may have had of their own performance.

Search strategy

Using the pre-set inclusion/exclusion criteria, all searches were conducted by the first researcher, and any studies presenting uncertainty were discussed with the second researcher until agreement was reached. As no previous systematic review was identified that covered this review’s specific intention, no date-limits were set, and all databases were searched from their inception until May 2017. The following databases were searched: CINAHL Complete, Academic Search Complete, AMED, British Education Index, Education Resource Information Centre (ERIC), MEDLINE, PsycARTICLES, PsycINFO, Maternity and Infant Care, INTERMID and Google Scholar. Grey literature searching was conducted using American Doctoral Dissertations and EThOS. Searches for ongoing trials were made via the ClinicalTrials.gov and ISRCTN registers. Finally, a hand-search of reference lists from all included studies and related systematic reviews was undertaken. See Appendix A for details of keyword searches.

Study selection

The combined articles from all database searches were imported into Proquest® Refworks. Screened references were either accepted or rejected on the basis of title/abstract. Those accepted were retrieved as full-texts and checked against the inclusion/exclusion criteria. All remaining rejected full-text papers had the reasons recorded for tabulation (Appendix B). Any papers presenting uncertainty were discussed between researches until consensus was reached. The complete process for selecting studies is summarised as a PRISMA flow-diagram (Moher et al., 2009), see fig. 1.

Data-extraction

A data-extraction form designed to the specifications of this review (Appendix C), was used to collect the following information from reviewed studies: citations, details of participants, settings, study aims and design, interventions, assessment instruments, and outcomes. The form was independently piloted by both reviewers on a sample of studies. Initial results were cross-checked and discussed before the form was adjusted and full data-extraction was independently undertaken by both researchers on all studies. The final extractions were again cross-checked to ensure results were comparable. Using these methods, whilst inter-rater reliability was not formally calculated, there was a high level of agreement between researchers.

Critical-appraisal

To determine overall risk-of-bias and validity of included studies, a critical-appraisal was independently performed on each study by both researchers and the results cross-checked, resulting in 100% agreement for risk-of-bias scores. The modified tool was constructed (Appendix D) based on the ‘McMaster Critical Review form for Quantitative Studies’ (Law et al., 1998) and the ‘Quality Assessment Tool for Quantitative Studies’ produced by the Effective Public Health Practice Project’ (EPHPP, 1998). The EPHPP (1998) tool was added to formally assess ‘risk-of-bias’ using specific criteria, ranking the appraised data into studies that show either ‘high’, ‘moderate’ or ‘low’ risk-of-bias. This tool is accepted across a range of measures including reliability (both inter-rater and test-retest) and content and construct validity (National Collaborating Centre for Methods and Tools, 2008).

Initially, critical-appraisal was not used to exclude studies considered of weaker quality. This is particularly contentious if the body of evidence is already small and demonstrates much heterogeneity, as it risks excluding findings that can be useful to sub-population analysis and limits the ability to identify future research requirements (Pettigrew, 2015). Instead studies were rated to allow consideration of the relative importance of their findings and all studies that could be combined within a meta-analysis were initially included. Subsequently, sensitivity analysis were planned to exclude high-risk studies (where feasible) for each outcome and assessment time-frame (post intervention, intermediate, and long-term).

Data-analysis

A meta-analysis of pooled effect-sizes was planned where studies were homogenous enough in terms of study parameters (Gomm et al., 2006). A random-effects model (at 95% confidence) was used within RevMan5.3 statistical analysis software. For continuous outcomes, standardised means were used. Where data was of insufficient quantity or quality to be combined within the meta-analyses, results were presented graphically using mean difference or odds-ratio (as appropriate). Effect sizes were calculated using the Z statistic (at 95% confidence) and those < 1 were not considered significant. Statistical heterogeneity was calculated using the I2 statistic and was also assessed by comparison of forest-plots using both fixed and random effects models. As not all studies could be examined within a statistical meta-analysis a narrative synthesis of findings was also conducted.

Results

A total of 1126 potentially relevant records were identified, of these 1102 records were retrieved through primary database searches, and 24 records were retrieved from ancillary databases. All records were examined against the eligibility criteria and on this basis 973 were excluded. The remaining 91 records were obtained as full-texts (with the exception of one dissertation). Full-texts were checked against inclusion/exclusion criteria and 73 were rejected outright and unsuccessful attempts were made to contact the author of one further dissertation (Lantier, 1992), therefore whilst this study currently sits within the ‘Characteristics of Excluded Studies’ table (Appendix B), it status remains unclassified at the time of writing. This left 17 studies meeting the eligibility criteria. A hand search of reference lists returned a further report (Jeffries and Rizzolo, 2012), bringing the final number of studies eligible for review to 18.

Study characteristics

Eighteen studies containing a total of ~1192 participants, satisfied the eligibility criteria. Where reported, the average ratio of within study gender split was ~81% female and ~19% male. The countries of origin were: 12 American (Blum et al., 2010; Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Kardong-Edgren et al., 2007; Kardong-Edgren et al., 2009; Arnold et al., 2013; King and Reising, 2011; Rodgers et al., 2009; Weiss et al., 2016; Grady et al., 2008; Konieczny, 2016), 3 Australian (Lapkin and Levett-Jones, 2011; Levett-Jones et al., 2011a; Lee et al., 2008), 2 British (Harper et al., 2016; Crofts et al., 2006), and 1 Jordanian (Aqel and Ahmad, 2014).

The majority of studies (14) were conducted on nurses (Blum et al., 2010; Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Lapkin and Levett-Jones, 2011; Levett-Jones et al., 2011a; Kardong-Edgren et al., 2007; Kardong-Edgren et al., 2009; Arnold et al., 2013; King and Reising, (2011); Rodgers et al., 2009; Aqel and Ahmad, 2014; Grady et al., 2008; Konieczny, 2016). One study looked at midwives within a mixed-professional population (Crofts et al., 2006), one study examined respiratory therapists (Weiss et al., 2016), one was conducted on operating department practitioners (Harper et al., 2016), and one on paramedics (Lee et al., 2008).

Outcomes

Twenty-seven individual learning outcomes were assessed in total across all studies reviewed.

Thirteen studies examined the effect varied mannequin/model fidelity levels have on knowledge outcomes (Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Levett-Jones et al., 2011a; Kardong-Edgren et al., 2007; Kardong-Edgren et al., 2009; Arnold et al., 2013; King and Reising, 2011; Rodgers et al., 2009; Harper et al., 2016; Weiss et al., 2016; Aqel and Ahmad, 2014; Konieczny, 2016).

Five papers examined psychomotor outcomes (Harper et al., 2016; Weiss et al., 2016; Aqel and Ahmad, 2014; Crofts et al., 2006; Grady et al., 2008).

Nine studies measured changes in affective or non-technical outcomes, including skills such as critical thinking, clinical reasoning, leadership and communication (Blum et al., 2010; Bultas et al., 2014; Decelle, 2015; Lapkin and Levett-Jones, 2011; Arnold et al., 2013; King and Reising, 2011; Rodgers et al., 2009; Lee et al., 2008; Crofts et al., 2006).

In respect to the type of educational subjects or activities being researched, 8 studies examined scenarios involving management of various patient conditions including the deteriorating patient (Blum et al., 2010; Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Lapkin and Levett-Jones, 2011; Levett-Jones et al., 2011a; Kardong-Edgren et al., 2007; Kardong-Edgren et al., 2009).

Four studies looked specifically at the management of advanced life support (ALS) scenarios (Arnold et al., 2013; King and Reising, 2011; Rodgers et al., 2009; Lee et al., 2008).

The remaining papers assessed the following skills; airway-management (Harper et al., 2016; Weiss et al., 2016), medication administration (Konieczny, 2016), CPR performance during basic life support (Aqel and Ahmad, 2014), obstetric delivery in the presence of shoulder dystocia (Crofts et al., 2006), and nasogastric and urinary catheter tube insertion (Grady et al., 2008).

It was not feasible to examine outcomes specifically in terms of high, medium, and low-fidelity equipment due to inadequate data comparing each category. Instead outcomes were analysed in respect to relative fidelity level (higher versus lower). Further details of all reviewed studies can be found in Table 1. Characteristics of Included Studies.

Risk-of-bias

Globally, 13 studies (72%) were deemed to be of high risk-of-bias (Blum et al., 2010; Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Lapkin and Levett-Jones, 2011; Levett-Jones et al., 2011a; Kardong-Edgren et al., 2007; Arnold et al., 2013; King and Reising, 2011; Rodgers et al., 2009; Lee et al., 2008; Crofts et al., 2006; Grady et al., 2008). Four studies (22%) were rated as moderate risk-of-bias (Kardong-Edgren et al., 2009; Harper et al., 2016; Weiss et al., 2016; Aqel and Ahmad, 2014) and 1 study (6%) was low risk-of-bias (Konieczny, 2016). A breakdown of how risk-of-bias was awarded for individual studies and outcomes can be seen in Figure 2.

Knowledge outcome post-intervention

Ten studies reported the results of knowledge tests conducted immediately following the simulation intervention (Bultas et al., 2014; Decelle, 2015; Jeffries and Rizzolo, 2012; Levett-Jones et al., 2011a; King and Reising, 2011; Rodgers et al., 2009; Harper et al., 2016; Weiss et al., 2016; Aqel and Ahmad, 2014; Konieczny, 2016).

Four studies demonstrated a statistically significantly greater gain in knowledge in the group using higher-fidelity mannequins compared to that using lower-fidelity equipment (Rodgers et al., 2009, (p0.002); Harper et al., 2016, (p0.0006); Aqel and Ahmad, 2014, (p≤0.001); Konieczny, 2016, (p<0.05)).

Six Studies were unable to demonstrate any significant difference in knowledge scores between groups immediately following simulation training Decelle, 2015 (p0.922); Levett-Jones et al., 2011a (p>0.05); King and Reising, 2011 (p>0.05); Weiss et al., 2016 (p>0.05)). It should be noted that 2 studies did not publish p values. Bultas et al., 2014 did not report significance, however whilst the low-fidelity group had a greater mean knowledge score (23.38) than the high-fidelity group (22.63), given the small sample-size in this study, this difference would have been unlikely to have reached a significant level. Whilst Jeffries and Rizzolo, 2012 reported that no significant differences were found between groups, statistical data was not provided.

Knowledge outcome at follow-up 1-2 weeks

Five studies examined knowledge outcomes at 1-2 weeks follow-up, none were able to demonstrate a statistically significant difference between knowledge gained by groups trained on different mannequins. (Levett-Jones et al., 2011a (p>0.05); Kardong-Edgren et al., 2007 (p>0.05); Kardong-Edgren et al., 2009 (p>0.05); Arnold et al., 2013 (p0.12); King and Reising, 2011 (p>0.05)).

Knowledge outcome at follow-up 2-6 months

Four studies reported on knowledge scores at 2-6 months follow-up. Three of which showed no significant difference between fidelity groups at these time-points (Bultas et al., 2014, (p0.668); Kardong-Edgren et al., 2009, (p>0.05); King and Reising, 2011, (p>0.05)). One study reported a significant difference favouring the high-fidelity group at 3-months. (Aqel and Ahmad, 2014, (p≤0.001)).

Knowledge outcome – statistical analysis

To discover if a pooled analysis would reveal further information, all studies (with appropriate data) using standardised means were combined within a random-effects model. Initially, all studies were included (figure 3.) and then a sensitivity-analysis was performed by removing those at high risk-of-bias, to negate the influence of weaker studies (figure 4.).

Prior to a sensitivity-analysis the effect-size for knowledge scores immediately post-intervention is non-significant although somewhat marginal (p0.07). When knowledge was tested at 1-2 weeks post-intervention there was no significant differences between fidelity groups (p0.47), or when re-tested at 3-6 months (p0.23). A meta-analysis conducted after removal of high-risk studies revealed a highly significant difference in knowledge scores post-intervention favouring higher-fidelity (p<0.00001), but no significant difference at 1-2 weeks (p0.76) or at 3-6 months (p0.23). An overall effect estimate across all time-points was not conducted as participants from studies contributed data to more than one time-point and therefore a total meta-analysis (including all sub-groups) would have violated the ‘assumption of independence of outcomes’ (Thomas et al., 2012).

Where possible missing statistical data was converted and calculated for studies not reporting in means and standard deviations. However, 5 studies (Jeffries and Rizzolo, 2012; Weiss et al., 2016; Bultas et al., 2014; Kardong-Edgren et al., 2007; King and Reising, 2011) were unable to be included within the meta-analysis due to data issues, although all these studies were also at high risk-of-bias, and therefore would have been excluded during the sensitivity-analysis.

Psychomotor outcome

Five studies examined psychomotor outcomes (Harper et al., 2016; Weiss et al., 2016; Aqel and Ahmad, 2014; Crofts et al., 2006; Grady et al., 2008). It was not possible to pool the complete data-set within the meta-analysis, as only 3 studies reported means and standard deviations (Harper et al., 2016; Aqel and Ahmad, 2014; Grady et al., 2008), one of which was presented as sub-groups according to gender only (Grady et al., 2008). In addition, a wide range of testing time-frames were reported across few studies.

Psychomotor outcome post-intervention

Two studies tested skills performance post-intervention, both of which favoured higher-fidelity training (Harper et al., 2016 (p0.048); Aqel and Ahmad, 2014 (p≤0.001)).

Psychomotor outcome at follow-up 1-3 weeks

Two studies tested psychomotor outcomes at 1-3 weeks post-intervention (Weiss et al., 2016; Crofts et al., 2006), showing differing results.

There was no difference in groups of respiratory therapy staff performing airway skills on different fidelity mannequins (Weiss et al., 2016 (p<0.05), although this study provided nominal data only.

Whereas midwives performing obstetric deliveries on medium-fidelity mannequins performed significantly better than those trained on low-fidelity models (Crofts et al., 2006) for; likelihood of delivery (p0.002), shorter delivery interval (p0.004); higher chance of delivering the posterior arm (p0.001), and total amount of force applied (p0.006). Although groups did not differ significantly for the peak fundal pressure applied (p0.242).

Psychomotor outcome at follow-up 1-3 months

Two studies reported outcomes at 1-3 months and both favoured higher-fidelity training (Grady et al., 2008 (p<0.05); Aqel and Ahmad, 2014 (p≤0.001)).

Affective/NT outcome post-intervention

Four studies reported the results of affective/NT performance tests conducted during or shortly following the simulation intervention (Decelle, 2015; Lapkin and Levett-Jones, 2011; Rodgers et al., 2009; Lee et al., 2008).

Three of these demonstrated significantly improved performance when using higher-fidelity equipment (Decelle, 2015; (p0.007); Lapkin and Levett-Jones, 2011 (p0.001); Rodgers et al., 2009; (p0.010)).

However, the remaining study failed to demonstrate a difference (Lee et al., 2008 (p0.30)).

Affective/NT outcome at follow-up 2-3 weeks

Three studies reported on affective/NT scores at 2-3 weeks follow-up (Arnold et al., 2013; King and Reising, 2011; Crofts et al., 2006).

Only one paper found a significant difference in performance across 4 different scenarios which favoured the high-fidelity group (King and Reising, 2011 (p0.001)).

One was unable to show any difference across a range of assessed items (Arnold et al., 2013 (p>0.05). In the 3rd study (Crofts et al., 2006) two groups of midwives performed comparably with respect to calling for a doctor or midwife (p0.113) and stating the problem (p0.225). However, those trained on the medium-fidelity mannequin were significantly less likely to call for paediatric support than the low-fidelity group (p0.003).

Affective/NT outcome at follow-up 2-6 months

Two studies examined follow-up periods of 2-3 months, both favouring higher-fidelity training (Bultas et al., 2014 (p<0.001); King and Reising, 2011 (p0.001)).

Affective/NT outcome repeated-interventions

One study examined the effect of sustained interventions over an expanded timeframe of 13-weeks. No difference was found between fidelity groups when testing occurred at mid-term (~1.5 months; p0.57) and during the final weeks of the course (~3 months; p0.41), (Blum et al., 2010).

Affective/NT outcome – statistical analysis

As all affective/NT outcomes were rated at high risk-of-bias it was not possible to perform a sensitivity-analysis. Where dichotomous data was presented an odds-ratio was used (King and Reising, (2011), and for a second study generic-inverse-variance was used to derive standard error from mean-difference and confidence intervals (Lee et al., 2008). The results of 3 studies could not be displayed graphically due to reporting methods (Bultas et al., 2014; Arnold et al., 2013; Crofts et al., 2006). See figures. 6., 7., and 8.

Effect size comparison

Effect sizes (which represent the magnitude of difference between higher and lower fidelity outcomes) were also examined at the different data time-points against the number of studies available in each category (see Appendix E).

Discussion

The results of this review indicate that immediately post-intervention, training on higher-fidelity mannequins has the potential to increase learning outcomes. This was clearer for knowledge and psychomotor outcomes, where if risk-of-bias is taken into account the advantage remains, and less so for affective outcomes as the studies were all of high risk-of-bias, therefore negating the possibility of any sensitivity-analysis.

For knowledge outcomes the advantage gained by those trained on higher-fidelity mannequins was marginal (p0.07) before a sensitivity-analysis was undertaken to remove weaker studies, after which the advantage of using higher-fidelity equipment on knowledge outcomes becomes highly significant (<0.00001). This can be partly explained by removing the influence of an outlier study favouring lower-fidelity mannequins (Decelle, 2015). In this study, groups were not randomised and were non-comparable for baseline knowledge pre-intervention. However, when students were re-tested for knowledge weeks or months after an intervention, there was no evidence the advantage gained by higher-fidelity trained groups had been maintained.

Results show a similar pattern for psychomotor outcomes with a significant difference favouring higher-fidelity training immediately post-intervention when removing high-risk studies (p<0.00001). This directional effect was again no longer evident 1-week post-intervention (Weiss et al., 2016), although this study used a nominal pass-fail converted to a median score for each group rather than mean scores, which may have been less sensitive to differences. The one study examining outcomes at 3-months did show an increase in retained psychomotor skills (p<0.00001) in the higher-fidelity group (Aqel and Ahmad, 2014). These results need to be viewed with caution in light of the small number of studies available for comparison.

For affective/NT outcomes, once more there appeared to be a trend towards favouring higher-fidelity when outcomes were measured immediately post-intervention, however (similar to knowledge and psychomotor outcomes) this advantage was no longer evident at intermediate follow-up points, although did appear to be present again at long-term follow-ups (in accordance with psychomotor results). However, given the high risk-of-bias conferred on all studies within this category, no solid conclusions can be drawn for this outcome.

Integration of this review with previous literature reveals some broad agreement for its findings. Ilgen et al., (2013) completed a meta-analysis comparing performance during emergency medicine scenarios and found that higher-fidelity conferred small to moderate benefits in skills performance when compared to lower-fidelities. Cheng et al., (2015) also discovered by meta-analysis that during ALS scenarios higher-fidelity training resulted in significantly better skills performance than lower-fidelity training (p0.01), but only marginally favoured knowledge immediately post-intervention, which was non-significant (p0.14), and no significant differences existed between groups for skills performance at a 1-year follow-up. This review did not exclude studies at high risk-of-bias. Cheng et al., (2014) examined paediatric studies and found higher-fidelity significantly favoured non-timed skills (p0.001) and favoured (but not significantly) timed-skills (p0.18) and knowledge (p0.32). Mundell et al., (2013) again looking at resuscitation training found high-fidelity was favoured over lower-fidelity but the results were non-significant for knowledge (p0.14) and skills performance (p0.07). These results may indeed point to the subtlety of the high-fidelity effect.

Examination of effect-sizes (Appendix E) were found to be largest for psychomotor outcomes which agrees with the findings of Kim et al. (2016) who concluded that higher-fidelity training had a greater effect on psychomotor outcomes than knowledge or affective skills. However, the results reported here also show a stronger effect of higher-fidelity training on knowledge outcomes immediately post-intervention than has previously been seen in the literature. Generally larger effect-sizes were seen when testing occurred immediately post-intervention than at later time-points, with the exception of a very large effect favouring higher-fidelity at long-term data points for psychomotor skills (1 study). However, data-interpretation should take into account all of the following short-comings; that all the reviews studies could not be included, that whilst there is a lack of data across all outcomes, this is particularly evident for later time-points, and for psychomotor and affective/NT outcomes in comparison to knowledge outcomes, and that all affective/NT studies were deemed to be at high risk-of-bias. Therefore if a greater quantity and quality of data across outcomes had been available, the conclusions drawn may have been different.

The potential advantage gained initially by higher-fidelity mannequins was less evident at later time-points across the majority of outcomes, possibly due to a reduction in the data-set. Although not universal, it is generally accepted that students’ knowledge and skills will deteriorate post-educational intervention unless practice is sustained (McGaghie et al., 2010). Additional confounders may also enter the fray during extended assessment time-frames. Participants return to different placements or work arenas and are therefore exposed to inconsistent experiences in the interim, this becomes more consequential the greater the time that has passed since the original intervention. There is an argument for looking at the effects of repeated-interventions with feedback over longer time-frames. This has been termed ‘deliberate practice’ (Ericsson et al., 1993). Evidence suggests this approach enhances outcomes (Shin et al., 2015a), and may more closely mirror the reality of curriculum design. Unfortunately only 1 study included in this review employed repeated-interventions (Blum et al., 2010) and was unable to show any influence of fidelity-level on outcome, this was insubstantial evidence to draw conclusions from.

Examination of forest-plots revealed that confidence intervals became larger and p values less significant when switching from fixed to random-effects models, indicating the presence of heterogeneity (Higgins and Green, 2011). This supports the decision to use a random-effects analysis. Using the I2 statistic, the level of heterogeneity was seen to vary between 22% (low heterogeneity) to 96% (considerable heterogeneity). This was not surprising as there was substantial diversity between studies in terms of subject matter, populations studied, intervention methodologies, and assessment tools used.

This review spans across nursing, midwifery and allied health, examining directly comparative trials on mannequin fidelity adding to the body of educational research in the field of simulation. Whilst comprehensive database searches were undertaken, there remains the possibility of missing research, especially grey-literature, as one known un-retrievable dissertation may have been eligible (Lantier, 1992). In addition, studies not available in English were excluded and their inclusion might have been influential. The results of the analysis are suggestive only, due to the relatively low number of studies examining each outcome. Time-periods for analysis were arbitrarily chosen and alteration of these might produce different results. Results may be more generalizable to the nursing profession than for other health professionals whose populations were under-represented within the data. Also as the majority of participants tested were female the results may be less applicable to populations exclusively or predominately male.

Some studies could not be included in pooled meta-analysis because of insufficient reporting. There was a lack of sample-size calculations performed with only 4 out of 18 trials reporting this (Decelle, 2015; Kardong-Edgren et al., 2009; Aqel and Ahmad, 2014; Crofts et al., 2006), none of which met their recruitment target, raising the concern that the majority of studies were underpowered and would therefore have been unable to detect significant differences between groups. More studies across all outcomes are required using consistent methodologies and validated measurement tools for similar categories of intervention. To achieve external validity research should include repeated-measures designs, with standardised follow-ups, and determine if measured effects can then be translated into improved clinical performance.

For those interested in short-term outcome improvements higher-fidelity might be justified, but as most educators aspire to attain long-term developmental gains for their students and improved clinical practice, the picture is not so clear. The results presented here may not justify the higher investment costs incumbent upon increasing complexity. Educationalists may also wish to consider broader subjective student views on simulation training before making important purchasing decisions.

Conclusion

Whilst there remains an onus on educators to decide the most appropriate mannequin to use in local contexts, there is evidence that higher-fidelity mannequins may have the potential to improve learning outcomes to a greater degree than lower-fidelity models, at least in the short-term. However, this review is let down by two major factors which was the quantity of available studies examining each outcome and the overall quality of these studies. If these points were addressed it would increase confidence in the conclusions drawn. Therefore a repeated review is recommended once a greater body of primary research becomes available to confirm these findings.

Unfortunately due to a lack of follow-up studies, the influence of mannequin fidelity on sustained learning (including that conducted as repeated-interventions), could not be established. Further research is needed using longer term assessment time-frames and repeated interventions to allow educators to evaluate the longer term influence of alternative mannequin fidelities and decide whether increased expenditure on higher fidelity models is fully justified.

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