Research Article

Role of Task-Oriented Training in Upper Limb Motor Recovery Following Stroke: A Systematic Review and Meta-Analysis

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Abstract

Background. Upper limb (UL) impairment is among the most persistent and functionally consequential sequelae of stroke, and a large proportion of survivors do not regain useful arm and hand function. Task-oriented training (TOT) — the repeated, goal-directed practice of functionally meaningful activities, structured according to motor learning principles — has become the dominant paradigm in contemporary UL neurorehabilitation. Its magnitude of benefit, and the conditions under which that benefit is realised, remain contested.

Objective. To systematically identify, appraise and quantitatively synthesise the best available evidence on the effect of TOT on UL motor impairment, arm and hand function, and activities of daily living (ADL) after stroke, and to examine dose, training type and timing as moderators of effect.

Methods. A systematic search of MEDLINE/PubMed, the Cochrane Library (CDSR and CENTRAL), Embase, CINAHL, PEDro and Google Scholar was conducted for randomised controlled trials (RCTs) and high-quality systematic reviews with meta-analysis evaluating repetitive, task-specific or task-oriented UL training in adults after stroke. Reporting follows the PRISMA 2020 statement. Because valid de novo pooling requires participant-level summary data that were not uniformly retrievable, the quantitative component of this review synthesises published pooled effect estimates from methodologically robust meta-analyses, together with narrative appraisal of landmark RCTs. This design and its consequences are stated explicitly rather than concealed.

Results. The consolidated evidence base comprises 33 RCTs with 36 intervention–control pairs and 1,853 participants in the definitive Cochrane review of repetitive task training, together with 467 RCTs (N = 25,373) in the largest umbrella synthesis of physical therapy after stroke. Pooled estimates favour TOT for arm function (standardised mean difference [SMD] 0.25; 95% CI 0.01–0.49; 11 studies; n = 749), hand function (SMD 0.25; 95% CI 0.00–0.51; 8 studies; n = 619) and ADL (SMD 0.28; 95% CI 0.10–0.45). Effects were moderated by training type: mixed, multi-task programmes yielded SMD 0.42 (95% CI 0.17–0.67; 17 trials), whereas single-task (SMD 0.07; 95% CI −0.42–0.55) and whole-therapy approaches (SMD 0.10; 95% CI −0.24–0.43) did not reach significance. Higher practice dose conferred additional benefit (g = 0.35; 95% CI 0.26–0.45). GRADE certainty was low to moderate, limited principally by unclear risk of bias from incomplete reporting.

Conclusions. TOT produces small but consistent and clinically defensible improvements in UL function and ADL after stroke, and is superior to neurofacilitation-based comparators. The benefit is conditional: it depends on varied, adequately dosed, genuinely task-specific practice rather than on the label 'task-oriented'. Dose-matched trials such as ICARE indicate that when usual care already delivers structured functional practice, further structuring adds little. Future trials should report dose in repetitions rather than scheduled minutes and stratify by baseline severity and corticospinal tract integrity.

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1. INTRODUCTION

Stroke remains one of the principal causes of acquired adult disability worldwide. Estimates from the Global Burden of Disease Study 2021 place the number of prevalent strokes at 93.8 million (95% uncertainty interval [UI] 89.0–99.3) and incident strokes at 11.9 million (95% UI 10.7–13.2), with 7.3 million deaths and 160.5 million disability-adjusted life years (DALYs) attributable to stroke. Stroke was the third most common cause of death, accounting for 10.7% of all deaths, and the fourth most common cause of DALYs (5.6% of the global total) [1]. Critically for rehabilitation services, the decline in age-standardised mortality has not been matched by a proportionate decline in incidence, so the absolute number of survivors living with impairment continues to rise — a demographic reality that shifts the burden from acute care toward long-term functional recovery.

Within that burden, the upper limb occupies a position of particular clinical difficulty. Recovery of the paretic arm is slower, less complete and more variable than recovery of ambulation, and residual UL impairment disproportionately constrains independence in self-care, vocational re-entry and perceived quality of life. The arm is not merely a locomotor appendage; it is the instrument through which most instrumental activities of daily living are executed. Consequently, the question of which UL interventions work — and how much, for whom, and when — has become one of the defining empirical questions of neurorehabilitation.

Task-oriented training (TOT), also described in the literature as task-specific training, repetitive task training (RTT), or functional task practice, is the intervention that has most decisively displaced earlier paradigms. Its defining features are the repeated performance of an active motor sequence directed toward an explicit functional goal — reaching for a cup, manipulating a fastener, transporting an object — practised within a single training session and progressed in difficulty [2]. It stands in deliberate contrast to the neurofacilitation traditions (Bobath's neurodevelopmental technique, Brunnstrom's movement therapy) that dominated twentieth-century practice and that emphasised the normalisation of tone and the inhibition of pathological synergies prior to functional practice.

The theoretical warrant for TOT is drawn from two convergent literatures. The first is motor learning theory, which holds that skill is acquired through practice of the skill itself, that transfer is largely specific to the trained task, and that variable and contextually interfering practice schedules — although degrading acquisition performance — enhance retention. The second is the animal and human neuroplasticity literature, formalised by Kleim and Jones as a set of experience-dependent principles: use it or lose it, use it and improve it, specificity, repetition matters, intensity matters, time matters, salience matters, age matters, transference and interference [3]. TOT is best understood as the clinical operationalisation of these principles rather than as a discrete branded technique.

Despite widespread endorsement in clinical practice guidelines, the empirical picture is more equivocal than the strength of professional consensus would suggest. Pooled effect sizes for TOT on UL outcomes are consistently small; the certainty of evidence is repeatedly downgraded for risk of bias; and the largest dose-matched trial of a structured task-oriented programme, ICARE, failed to demonstrate superiority over dose-equivalent usual occupational therapy [4]. This tension — between a strong mechanistic rationale with broadly positive but modest pooled effects, and a negative flagship trial — is the substantive problem this review addresses.

The objectives of this review were therefore threefold: (i) to synthesise quantitatively the effect of TOT on UL motor impairment, arm and hand function, and ADL after stroke; (ii) to examine the type of training, the dose of practice, and the timing of delivery relative to stroke onset as candidate moderators; and (iii) to reconcile the pooled evidence with the results of dose-matched pragmatic trials in order to derive defensible clinical and research recommendations.

2. METHODS

2.1 Protocol and reporting

This review was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement. The review question was framed a priori using the PICOS structure. No amendments to the a priori question were made after the search was executed.

2.2 Eligibility criteria

Population: adults (≥18 years) with a clinical diagnosis of first-ever or recurrent ischaemic or haemorrhagic stroke, at any phase (acute, subacute or chronic), with UL paresis. **Intervention:** any programme in which an active motor sequence was performed repetitively within a single training session and was directed toward a clear functional goal, delivered to the paretic UL.

Comparator: usual care, conventional or neurofacilitation-based therapy, placebo or attention-control, or a lower dose of the same intervention.

Outcomes: the primary outcomes were UL motor impairment (Fugl-Meyer Assessment–Upper Extremity, FMA-UE), arm function (Action Research Arm Test, ARAT; Wolf Motor Function Test, WMFT) and hand function/dexterity (Box and Block Test; Nine-Hole Peg Test); secondary outcomes were ADL (Barthel Index; Functional Independence Measure), self-reported UL use (Motor Activity Log; Stroke Impact Scale hand domain) and adverse events.

Study design: randomised or quasi-randomised controlled trials, and systematic reviews with meta-analysis of such trials. Studies in which task practice was inseparably confounded with a distinct co-intervention (constraint-induced movement therapy with mitt constraint, robotic assistance, functional electrical stimulation, virtual reality) were considered separately and were not treated as evidence for TOT per se, since the active ingredient cannot be isolated.

2.3 Information sources and search strategy

MEDLINE/PubMed, the Cochrane Database of Systematic Reviews, the Cochrane Central Register of Controlled Trials, Embase, CINAHL, AMED, the Physiotherapy Evidence Database (PEDro), OTseeker and Google Scholar were searched from database inception. Search terms combined controlled vocabulary and free-text variants of the population (stroke, cerebrovascular accident, hemiparesis, hemiplegia), the intervention (task-oriented, task-specific, repetitive task, functional task practice, motor learning) and the target (upper limb, upper extremity, arm, hand). Reference lists of included reviews and trials were hand-searched, and clinical trial registries (ClinicalTrials.gov, WHO ICTRP) were screened for unpublished or ongoing work. No language restriction was applied at the screening stage.

2.4 Study selection and data extraction
Titles and abstracts were screened independently in duplicate against the eligibility criteria, with full texts retrieved for any record judged potentially eligible by either reviewer. Disagreements were resolved by discussion and, where necessary, by adjudication from a third reviewer. Extracted variables included study design, sample size, time since stroke, baseline severity, the content and progression of the intervention, dose (sessions, session duration, total scheduled hours and, where reported, repetitions), the nature of the comparator, outcome measures and time points, and effect estimates with measures of dispersion.

2.5 Risk of bias and certainty of evidence
Risk of bias in primary trials was appraised using the Cochrane risk-of-bias tool, with particular attention to allocation concealment, blinding of outcome assessors (blinding of participants and therapists being infeasible in this field), and completeness of outcome data. Methodological quality of trials was cross-checked against PEDro scores where available. Certainty of evidence for each outcome was rated using the GRADE approach, considering risk of bias, inconsistency, indirectness, imprecision and publication bias.

2.6 Approach to quantitative synthesis — a statement of transparency
The reader is owed an explicit account of what was and was not done. A statistically valid de novo meta-analysis requires the extraction of group means, standard deviations and sample sizes at matched time points from every primary trial. For a substantial proportion of the eligible trials, these data are reported incompletely, are available only as change scores without dispersion, or are recoverable only through direct correspondence with trial authors. Rather than impute or approximate those quantities and present the resulting figures as though they were independently derived, this review synthesises the pooled effect estimates published by methodologically robust meta-analyses that did have access to those data, appraises the consistency of those estimates against one another, and interrogates them narratively against the landmark primary trials. This is, in effect, a quantitative evidence synthesis of the umbrella type. Every effect estimate reported in Section 3 is attributed to its source and is verifiable in that source. No pooled estimate presented here was generated by the present authors, and none should be cited as such. This is a real limitation of the present work and is revisited in Section 4.5.

3. RESULTS
3.1 Characteristics of the evidence base
The consolidated evidence base rests on two pillars. The definitive Cochrane review of repetitive task training by French and colleagues included 33 trials contributing 36 intervention–control pairs and 1,853 participants [2]. The broader umbrella synthesis by Veerbeek and colleagues retrieved 467 RCTs comprising 25,373 participants, with a median PEDro score of 6 (interquartile range 5–7), from which 53 distinct physical therapy interventions were identified [5]. Included populations spanned the acute through chronic phases; sample sizes in individual trials were predominantly modest, with the notable exceptions of ICARE (n = 361) [4] and the Indian meaningful task-specific training trial of Arya and colleagues (n = 103) [6]. Comparators were heterogeneous, ranging from genuinely inert attention-control through to dose-matched conventional therapy — a distinction that proves to be decisive in interpreting the pooled results.

3.2 Effect on arm function
Pooled across 11 studies with 749 analysed participants, repetitive task training improved arm function relative to control with a standardised mean difference (SMD) of 0.25 (95% CI 0.01–0.49) [2]. The confidence interval, while excluding the null, approaches it closely; the estimate is statistically significant but of small magnitude by conventional benchmarks. GRADE certainty was rated low, downgraded principally because risk of bias in many contributing studies was unclear owing to poor reporting rather than to demonstrated methodological failure — an important distinction, since unclear risk is not the same as high risk, and the true effect may be larger or smaller than the point estimate suggests.

3.3 Effect on hand function and dexterity
Eight studies with 619 analysed participants yielded an SMD for hand function of 0.25 (95% CI 0.00–0.51) [2]. This estimate is of identical magnitude to that for arm function but sits precisely at the boundary of statistical significance, its lower confidence limit touching zero. The finding is consistent with the clinical observation that distal recovery is more resistant to training than proximal recovery, and with the neuroanatomical reality that fractionated finger movement depends on the integrity of the corticospinal tract to a degree that proximal reaching does not. Where the descending pathway is severely disrupted, no amount of behavioural practice will reconstitute the substrate on which dexterity depends.

3.4 Effect on activities of daily living
For ADL, the pooled SMD was 0.28 (95% CI 0.10–0.45) [2,7]. This estimate is notable because its confidence interval is both narrower and more clearly displaced from the null than those for the primary UL outcomes. That ADL benefit should be at least as robust as impairment-level benefit is precisely what the specificity principle predicts: TOT trains functional activities, so it should improve functional activities. It is, in the language of Veerbeek and colleagues, an intervention whose effects are 'mostly restricted to the actually trained functions and activities'

[5] — a constraint that is simultaneously the paradigm's greatest strength and its principal ceiling.

3.5 Moderator I — type of training

The most instructive subgroup analysis concerns the internal composition of the training programme. Mixed training — programmes in which multiple different tasks were practised — produced a moderate and statistically significant effect (SMD 0.42; 95% CI 0.17–0.67; 17 trials). By contrast, single-task programmes (SMD 0.07; 95% CI −0.42–0.55; 5 trials) and whole-therapy approaches (SMD 0.10; 95% CI −0.24–0.43; 2 trials) were not statistically significant [2]. The formal test for subgroup differences did not reach significance ($\chi^2 = 3.16$, df = 2, $P = 0.21$), which counsels caution: the contrast is suggestive rather than established, and the single-task and whole-therapy subgroups are sparsely populated and correspondingly imprecise. Nonetheless, the direction of the finding aligns with variable-practice predictions from motor learning theory, and the point estimate for mixed training is roughly two-thirds larger than the overall pooled estimate.

3.6 Moderator II — dose of practice

Two independent syntheses converge on a dose–response relationship. Lohse, Lang and Boyd, restricting analysis to trials that were not dose-matched, found that groups receiving more therapy improved beyond controls receiving less, with $g = 0.35$ (95% CI 0.26–0.45) [8]. Veerbeek and colleagues likewise reported strong evidence that a higher dose of practice is better, with summary effect sizes ranging from 0.21 (95% CI 0.02–0.39; $I^2 = 6\%$) for motor function of the paretic arm to 0.61 (95% CI 0.41–0.82; $I^2 = 41\%$) for muscle strength of the paretic leg [5]. The arm-specific estimate is the smallest in that range, indicating that the paretic UL is among the least dose-responsive targets in the stroke rehabilitation repertoire — a sobering counterpoint to the enthusiasm for high-intensity arm programmes. A structural limitation deserves emphasis: nearly all trials index dose as time scheduled for therapy, which is a poor proxy for the quantity that theory identifies as active — the number of purposeful repetitions actually performed.

3.7 Moderator III — timing

Veerbeek and colleagues reported significant differences with respect to timing post-stroke for 10 of the 53 interventions examined [5]. The evidence base does not permit a confident statement of an optimal window for UL TOT specifically. Two considerations complicate inference. First, spontaneous biological recovery is at its most vigorous in the early weeks, so early trials must demonstrate benefit over a rapidly rising background; second, most contributing trials recruit in the subacute phase, leaving the acute and late chronic phases comparatively underdetermined.

3.8 Comparison with neurofacilitation approaches

Where TOT has been compared directly against the neurofacilitation traditions under dose-matched conditions, it has generally prevailed. Arya and colleagues randomised 103 participants 4 to 24 weeks after stroke (mean 12.15 weeks; Brunnstrom arm recovery stages 2–5) to meaningful task-specific training or to a dose-matched standard programme based on Brunnstrom and Bobath techniques, delivered 4 to 5 days per week for 4 weeks, with 95 participants completing 8-week follow-up. Statistically significant between-group differences favouring task-specific training were observed at post-intervention and follow-up on the FMA, ARAT, Graded WMFT and MAL [6]. This result carries disproportionate evidential weight precisely because dose was matched: the comparison isolates the content of practice rather than its quantity. It is congruent with the broader umbrella conclusion that neurological treatment approaches show equal or unfavourable effects when compared with other training interventions [5].

3.9 The ICARE trial

The ICARE trial is the single most important discordant result and cannot be assimilated into a favourable narrative without distortion. Winstein and colleagues randomised 361 participants approximately 45 days after stroke with mild to moderate UL impairment (overall mean FMA-UE 41.6; 95% CI 40.7–42.6; median NIHSS 4, IQR 2–5) to a structured task-oriented programme (the Accelerated Skill Acquisition Program, ASAP; n = 119), dose-equivalent usual and customary care (DEUCC; 30 one-hour sessions over 10 weeks; n = 120), or monitoring-only usual and customary care (UCC; n = 122). The primary outcome was 12-month change in the log-transformed WMFT time score. No significant pairwise between-group differences emerged. Mean WMFT change was −8.8 seconds for ASAP, −8.1 seconds for DEUCC and −7.2 seconds for UCC; the ASAP versus DEUCC difference was 1.8 seconds (95% CI −0.8 to 4.5; $P = .18$), against a minimal clinically important difference of 19 seconds. The proportion improving on the SIS hand function score was 73%, 72% and 69% respectively. A total of 168 serious adverse events occurred in 109 participants [4].

3.10 Heterogeneity, certainty and safety

Certainty of evidence for the primary UL outcomes was rated low under GRADE, and moderate for selected secondary outcomes; the dominant reason for downgrading was unclear risk of bias arising from incomplete reporting [2]. Heterogeneity in the dose-response analyses was modest for arm motor function ($I^2 = 6\%$) but substantially higher for other targets (up to $I^2 = 77\%$ for sitting balance) [5]. Across the physical therapy literature synthesised by Veerbeek and colleagues, no adverse events were reported [5]; the serious adverse events recorded in ICARE occurred across all three arms, including the monitoring-only arm, and are best interpreted as reflecting the medical fragility of a post-stroke cohort rather than harm attributable to training.

Table 1: Pooled effect estimates for task-oriented / repetitive task training on upper limb and related outcomes.

Outcome	Pooled estimate (95% CI)	Studies / n	Certainty	Source
Arm function	SMD 0.25 (0.01 to 0.49)	11 studies; n = 749	Low	French 2016 [2]
Hand function	SMD 0.25 (0.00 to 0.51)	8 studies; n = 619	Low	French 2016 [2]
Activities of daily living	SMD 0.28 (0.10 to 0.45)	Cochrane pooled	Low–Moderate	French 2016 [2,7]
Mixed (multi-task) training	SMD 0.42 (0.17 to 0.67)	17 trials	Low	French 2016 [2]
Single-task training	SMD 0.07 (−0.42 to 0.55)	5 trials	Low	French 2016 [2]
Whole-therapy approaches	SMD 0.10 (−0.24 to 0.43)	2 trials	Low	French 2016 [2]
Higher vs lower therapy dose	g = 0.35 (0.26 to 0.45)	Non-dose-matched RCTs	Moderate	Lohse 2014 [8]
Dose effect, paretic arm motor function	SES 0.21 (0.02 to 0.39); I^2 = 6%	Umbrella synthesis	Strong evidence	Veerbeek 2014 [5]

CI = confidence interval; SMD = standardised mean difference; SES = summary effect size; g = Hedges' g. Subgroup difference by training type: $\chi^2 = 3.16$, df = 2, P = 0.21 (not significant).

4. DISCUSSION

4.1 Principal findings

Three conclusions follow from the synthesis. First, TOT confers a small but reasonably consistent benefit on arm function (SMD 0.25), hand function (SMD 0.25) and ADL (SMD 0.28) after stroke. Second, the benefit is conditional rather than automatic: it is contingent on the practice being varied and multi-task (SMD 0.42) rather than repetition of a single task (SMD 0.07), and on adequate dose (g = 0.35). Third, TOT outperforms neurofacilitation comparators when dose is held constant [5,6], but does not reliably outperform contemporary usual occupational therapy when that therapy is itself dose-matched and already functionally oriented [4].

4.2 Reconciling the pooled estimates with ICARE

The apparent contradiction between a positive pooled literature and a null flagship trial largely dissolves once the comparators are examined. Most trials contributing to the Cochrane pooled estimate compared TOT against usual care that was less intensive, less functionally directed, or both. ICARE compared a structured task-oriented protocol against usual occupational therapy delivered at an identical dose by licensed therapists — that is, against a comparator that had itself been substantially colonised by task-oriented principles over the preceding two decades. Under those conditions a null result is not evidence that task practice is inert; it is evidence that the incremental value of formalising and branding task practice, over and above what skilled clinicians already do, is small. The ICARE investigators' own framing is instructive: usual community-based therapy improves UL motor function, and more than doubling the dose of therapy did not produce meaningful differences in motor outcomes.

This reading is reinforced by the severity profile of the ICARE cohort. With a mean FMA-UE of 41.6 and a median NIHSS of 4, participants had predominantly motor hemiparesis of mild to moderate severity without substantial cognitive or sensory involvement — a group with considerable spontaneous recovery capacity and limited room for between-group separation. The trial therefore tested a specific and narrow proposition (does ASAP beat dose-matched usual care in mild-to-moderate subacute stroke?) and answered it in the negative. It did not test, and cannot adjudicate, whether task practice outperforms non-functional exercise, nor whether severely impaired patients benefit.

4.3 Mechanistic interpretation

The pattern of results maps closely onto the experience-dependent plasticity principles articulated by Kleim and Jones [3]. Specificity predicts that gains will be largest in trained activities and smallest in untrained ones, which is what the ADL-versus-impairment gradient shows. Repetition and intensity predict a dose–response relationship, which both Lohse and Veerbeek demonstrate. Salience predicts that meaningful tasks will outperform arbitrary movement, which the Arya trial supports. The superiority of mixed over single-task training is consistent with contextual interference: variable practice degrades acquisition but enhances retention and transfer, and trial endpoints are measured at retention.

Equally, the ceiling on effect sizes is mechanistically intelligible. Behavioural practice can only exploit the neural substrate that survives; where the corticospinal tract is extensively disrupted, the biological precondition for dexterous recovery is absent, and the intervention cannot supply it. Pooling severely and mildly impaired participants into a single estimate therefore averages responders with structural non-responders and dilutes the observed effect. This is a strong argument for stratification by corticospinal integrity rather than for abandoning the intervention.

4.4 Clinical implications

For practice, several implications follow. TOT should remain the default framework for UL rehabilitation; the evidence against neurofacilitation-based alternatives is consistent and dose-matched. Programmes should be constructed around a varied repertoire of functionally meaningful tasks rather than the massed repetition of one task. Dose should be maximised within tolerance, with the recognition that the paretic arm is comparatively dose-insensitive (SES 0.21) and that increments in scheduled time are not equivalent to increments in repetitions. Clinicians should calibrate expectations honestly: pooled effects of this magnitude describe a real but modest shift, and the ICARE data indicate that competently delivered functional therapy is already capturing much of the available benefit. Finally, the label matters less than the content — a programme is task-oriented if it consists of repeated, varied, goal-directed, progressively difficult practice, whatever it is called.

4.5 Limitations

The limitations of this review are substantive and are stated plainly. Most importantly, and as declared in Section 2.6, the pooled estimates reported here were not computed by the present authors from primary data; they are published estimates from source meta-analyses, appraised and synthesised narratively. This review therefore cannot offer a new forest plot, a new I^2 for the primary outcome, a new funnel plot, or a formal test of publication bias, and it inherits whatever limitations attach to the source syntheses. Presenting the work otherwise would misrepresent it.

Beyond that, four limitations belong to the underlying literature rather than to this review. Certainty of evidence is low for the primary UL outcomes, driven by unclear risk of bias from incomplete reporting [2]. Comparator heterogeneity is severe: 'usual care' denotes radically different content across trials and jurisdictions, which renders pooled estimates difficult to interpret and probably inflates heterogeneity. Dose is almost universally operationalised as scheduled time rather than repetitions, a measurement problem that the field has identified but not solved. And blinding of participants and therapists is impossible in this field, so performance bias cannot be excluded even in otherwise well-conducted trials.

4.6 Recommendations for future research

Four priorities emerge. Trials should report dose in repetitions performed, not minutes scheduled, since the theory that motivates the intervention specifies repetitions as the active ingredient. Trials should stratify by baseline severity and, where feasible, by corticospinal tract integrity, in order to separate biological non-responders from genuine treatment failures. Comparators should be characterised in sufficient content detail that readers can determine what was actually compared. Finally, the field would be well served by prospective individual participant data meta-analysis, which would resolve the moderator questions that aggregate-level subgroup analyses — such as the non-significant $\chi^2 = 3.16$ test reported above — are underpowered to settle.

5. CONCLUSION

Task-oriented training produces small, consistent and clinically defensible improvements in upper limb function and activities of daily living after stroke, with pooled standardised mean differences of approximately 0.25 for arm and hand function and 0.28 for ADL, on evidence of low to moderate certainty. Its superiority over neurofacilitation approaches under dose-matched conditions is reasonably well established; its superiority over contemporary, competently delivered, dose-matched usual care is not. The benefit is conditional on the practice being varied, meaningful and adequately dosed — mixed multi-task programmes yield roughly twice the effect of single-task repetition. The honest summary is neither that task-oriented training is transformative nor that it is ineffective, but that it is the correct organising framework for upper limb rehabilitation whose incremental yield, in a field where usual care has already absorbed its central insight, is real but bounded. Advancing beyond that bound will likely require better measurement of dose, stratification by biological recovery potential, and combination with adjuvant strategies rather than further refinement of the behavioural protocol alone.

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