Synthesis of Evidence about Stay-at-Work/ Return-to-Work (SAW/RTW) and Related Programs

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Glossary of Terms

EBEI	Evidence-Based Early Intervention, an intervention that occurs before an injured or ill worker is receiving federal disability benefits, and is based on evidence of effectiveness
Meta-analysis	A numerical method of combining estimates across studies and ascertaining which features of studies explain variation in estimates
Narrative review	A type of review article that does not attempt to examine a systematic or representative sample of studies
RCT	Randomized Control Trial
Review article	A scientific study that describes other studies, rather than analyzing novel data
RTW	Return to work
SAW	Stay at work
Systematic review	A review article that attempts to examine a systematic or representative sample of studies, and may or may not synthesize the studies using meta-analysis

Executive Summary

Stay-at-Work/Return-to-Work (SAW/RTW) programs help workers who experience an illness or injury to remain at work or, if they have separated from employment, return to work as soon as medically feasible. The U.S. Department of Labor (DOL) seeks to build the evidence base about SAW/RTW programs and develop intervention design options and evaluation strategies. Towards that goal, DOL has contracted with Abt Associates to conduct the ***Stay at Work/Return to Work (SAW/RTW) Models and Strategies*** project. To carry out the study, Abt is:

- conducting a comprehensive review of SAW/RTW programs to describe the initiatives that are operating;
- reviewing evidence about the effects of SAW/RTW programs;
- analyzing publicly available data to examine pathways from illness/injury to federal disability benefits to develop early intervention pathways and target population profiles; and
- developing intervention and evaluation design options.

This document is the ***Review and Synthesis of the Evidence about SAW/RTW and Related Programs***. It summarizes evidence regarding the effects of SAW/RTW programs on employment and disability benefits application. This document builds on the conceptual framework and scan of programs presented in the ***Synthesis of SAW/RTW Programs, Efforts, Models, and Definitions (Epstein, et al, 2020).*** The program synthesis document describes the current context in which states and private entities operate SAW/RTW programs and the types of service models they use. In this document, we review the evidence of effectiveness for the programs identified in the synthesis as well as all other evidence found in a broad scan of the relevant literature, including both literature reviews and individual studies.

The evidence reviewed in this document focuses primarily on *early interventions,* which we define as those that occur prior to application for the Social Security Disability Insurance (SSDI). The rationale for examining early interventions is to identify program strategies that might promote workplace retention and prevent long-term disability and the need for federal benefits. It is important to note that such interventions may not necessarily be early relative to the onset of the injury or illness.[1]

We designed this evidence review to inform DOL about what is known about existing program models for the purpose of developing new evaluation design options. We did not limit the evidence reviewed to particular types of research designs. However, to be included in the scan of evidence, a study had to provide sufficient information to assess whether observed effects were due to an intervention of some kind. In practice, we had to limit the synthesis to studies that had some form of comparison group (not necessarily randomly assigned, or even adjusted for baseline differences), because it is not possible to compute a comparable effect size for a single case or pre-post study design (i.e. the only effect size possible would have to assume no changes could occur in the comparison group over time, which makes these effects sizes not comparable to effect sizes computed with a comparison group).

We present the findings to guide future work to develop evaluation design options. Since evaluations provide evidence about program impacts, one goal of this document is to highlight where gaps exist in the

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[1] Information available about the programs identified in the program synthesis and the evidence review was not sufficient to classify interventions as early or late relative to the onset of injury or illnesses.

evidence. However, the design of future DOL SAW/RTW demonstration programs should maximize the chances of improving outcomes, hence another goal of this document is to identify which program models may produce substantively meaningful positive impacts.

Methods

This evidence review considers 377 original sources (including journal articles, reports, and websites) including sources about the 68 programs described in Epstein et al. We found that only 11 of the programs discussed in the program synthesis have been evaluated in studies reporting direct evidence of impacts. Of these, six are not early interventions, because they involve current disability beneficiaries.[2] We excluded the six late-stage interventions from the evidence review. In Appendix E of Epstein et al, we discuss evidence available from the other five programs that have been evaluated.

Of the 377 sources, we removed 90 that did not include an evaluation, and 35 that did not did not evaluate outcome measures that can generate relevant findings. We also eliminated 111 studies with publication dates prior to 2008, two studies of programs that are not early interventions,[3] one study that did not involve a comparison group, and 17 that did not study a return to work intervention. We also identified 28 review articles that we analyzed separately in Chapter 3.

We included the 87 remaining studies in a meta-analysis of evidence, discussed in Chapter 4. We coded each study according to:

- the program model examined (employer-provided accommodations, financial incentives for employers or workers, information, medical management, employment services and training);
- type of disability (mental health, musculoskeletal, or other); and
- other study features such as the quality and relevance of evidence presented.

Of the 87 articles, 72 provided “high-quality” evidence (16 judged more relevant,[4] and 56 less relevant) and 15 provided “low-quality” evidence (5 judged more relevant and 10 less relevant).[5] To ascertain how estimated effects vary with program model or other study characteristics, we analyzed that larger body of evidence using meta-analytic regression, which is the most efficient way to synthesize findings (DerSimonian & Laird, 1986).

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2 The excluded programs are: 1) Work Incentives and Planning Assistance (Livermore, Prenovitz, and Schimmel 2011), 2) Ticket to Work (Livermore, Mamun, Schimmel, and Prenovitz 2013), 3) Accelerated Health Insurance/Benefits (Michalopoulos et al. 2011), 4) the Youth Transition Demonstration (Fraker et al. 2014), 5) the Job Corps program, and 6) the Kentucky and Minnesota SGA Project Demonstrations (Sevak, Kehn et al. 2017; Sevak, Martin et al. 2017

3 We excluded studies on the effects of six late-stage interventions that we had identified in the program synthesis. Our search for this report also found studies about two other late-stage interventions and we excluded those from the evidence review.

4 Our external validity ratings range from 0 (not generalizable to U.S. contexts) to 5 (nationally representative). We label studies with external validity scores of 2 or higher as “more relevant” and studies with external validity scores of 0 or 1 as “less relevant” (most of these are from settings outside the U.S.).

5 Our internal validity ratings range from 0 (no evidence) to 5 (most rigorous evidence). We rate studies with internal validity scores of 4 and 5 as having “high-quality evidence” and studies with internal validity scores of 1, 2, and 3 as “low-quality evidence.” We developed these standards to be compatible with review standards used in the What Works Clearinghouse Standards 4.0 (IES 2017) or the Department of Labor’s Clearinghouse for Labor Evaluation and Research (CLEAR 2015).

Results

The evidence reviewed varies widely in its quality, that is, its credibility to identify causal impacts. The evidence also varies in the breadth of its applicability or relevance for the U.S. workforce. Many studies with high-quality evidence have limited relevance because of a non-U.S. geographic scope. Because the rest of the social welfare system is expected to interact with early intervention effects, and foreign settings typically have very different institutional context, we judge the external validity of non-U.S. for inferences about effects in the U.S. to be low.

The results of the meta-analytic regression suggest that the higher-quality evidence shows larger impacts for program models that include employment services, such as the Individual Placement and Support model. Our review of systematic reviews indicates that program models have generally positive results for individuals with musculoskeletal conditions, including low back pain, and some models show positive results for individuals with mental illness. However, this may be because the program models more often used for musculoskeletal conditions and mental illness offer employment services. In contrast, we do not see any average advantage in impacts for musculoskeletal conditions and mental illness in our meta-analytic review of studies.

On balance, the available evidence on SAW/RTW programs tends to be low-quality, and findings from high-quality studies are difficult to interpret. Most studies do not include numerical information on outcomes, and others do not include sufficient information to estimate program impacts. Most studies that do provide estimates of program impact either have:

- high internal validity (credibility to identify causal impacts in a given setting), but are not generalizable to a broad U.S. context (many experiments are conducted outside the U.S.), or
- low internal validity (less credibility to identify causal impacts in a given setting), but are more relevant to a broad U.S. context.

A further consideration for assessing the quality of evidence is precision (statistical power). In general, studies with larger sample sizes have greater statistical power and can conclude with high confidence that estimated impacts fall within a narrow range. One way to assess adequate statistical power is to ascertain whether any impacts differ statistically from zero, or equivalently whether confidence intervals are large relative to estimated effects, since we may expect that in a large group of interventions with generically nonzero effects, we should see effects that are statistically significant if the studies are adequately powered. Another way to judge power is to ascertain whether confidence intervals are large relative to effects that would be policy-relevant.

The evidence about SAW/RTW programs in existing reviews is often limited to one type of program, or type of condition (for example musculoskeletal conditions, including low back pain, or mental illness). For those conditions, we identified numerous randomized control trials with high internal validity (credibility as to the identification of causal impacts). For low back pain, evidence reviews show statistically significant positive impacts, suggesting that there is sufficient power to detect impacts. Evidence reviews also point to statistically significant positive impacts for individuals with mental illness. Mental illness (40 percent) and musculoskeletal conditions (20 percent) were the most commonly targeted disability types in the articles reviewed, so the majority of the evidence is relevant for understanding impacts on SAW/RTW for those conditions.

In contrast to the findings from the literature reviews, the results of our meta-analytic review of all individual studies finds few stable patterns in how impacts vary with disability type or program model, i.e. patterns of results differ across different classes of studies, and even when pooling all studies most characteristics of studies exhibit wide confidence intervals (meaning effects could be much larger or much smaller comparing across studies with or without that feature). We do not detect any statistically significant advantage in impacts by disability type: there is insufficient sample size to detect whether programs tend to produce larger or smaller impacts when targeting mental illness, musculoskeletal, or other conditions.

We also find that medical interventions, accommodation, informational and financial incentive models have no average advantage or disadvantage relative to the other models, though the estimates are again imprecise. However, our review of individual studies indicates that SAW/RTW programs that include employment services and training tend to have larger impacts on average, with effects about 1.1 larger on the log odds scale, a finding driven by high-quality evidence using experimental designs.

This review also finds that measured impacts are systematically different depending on the type of evidence, when we examine interactions with program models, which is indicative of selection bias[6] in quasi-experimental evidence, though there is no clear pattern as to the nature of that bias.

Implications for the Next Steps in the Project

This review of evidence highlights several open questions for further study. The review finds a lack of evidence about program effects, in the sense that confidence intervals are large (so we cannot precisely predict the impact a future intervention might have). There is also little evidence on what types of programs produce larger impacts, for whom. The review also highlights the importance of obtaining a large sample size for any future evaluation, as most individual studies suffer from low statistical power. Finally, it suggests that employment services and training may be important program components with potential to increase employment rates of people with disabilities. The tradeoffs between a large sample size and a cost-effective evaluation design imply that one attractive design would randomize a low-cost intervention across a very large sample and use administrative data to measure outcomes.

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[6] Selection bias refers to the fact that individuals who choose to participate in an intervention are different from those who choose not to. When an intervention is not randomly assigned, or characteristics are not otherwise adjusted for, we cannot infer that any difference in outcomes arises from the intervention rather than the intrinsic difference between individuals who choose to participate and those who choose not to. Selection bias can also arise from a selection mechanism that involves someone else making choices about who participates, if a similar difference in characteristics arises. This bias is the major challenge that studies with high internal validity overcome, i.e., a high internal validity study or strong evidence has low risk of selection bias.

1. Introduction

This report summarizes recent evidence on the effectiveness of Stay-At-Work/Return-To-Work (SAW/RTW) programs to help injured or ill employees gain or retain attachment to the labor force, and to avoid reliance on Social Security Disability Insurance or other federal disability benefits.

- **The goal of this report is to summarize the strength and relevance of findings from evaluations of SAW/RTW programs, as well as the size of the impacts of those programs on employment and receipt of disability benefits.**

The Abt team recorded key features about each study that provided estimates of program effects. First, the team assessed the credibility (or internal validity) of the evidence. Next, the team evaluated each study as to its relevance (or external validity). The team recorded the type of program evaluated in the study and the type of condition or disability examined in the study. The team then synthesized the evidence by looking across this large number of studies to describe how estimated effects vary on average. These findings will inform evaluation design options that the team will develop in Task 4 of the project.

Epstein et al, ***Synthesis of SAW/RTW Programs, Efforts, Models, and Definitions,*** catalogued the types of programs being implemented by a variety of stakeholders in a wide range of settings—state agencies, private employers, and private insurers. That document deliberately considered a broad range of SAW/RTW programs or strategies and did not impose any restrictions on whether researchers had evaluated the program. The program synthesis describes programs that include demonstrations that have concluded, demonstrations that are currently active, programs with evaluations in early stages, and even programs with no planned evaluations. The team used four sources to identify those programs and strategies: (1) a review of previous work completed under the S@W/R2W Policy Collaborative, supported by ODEP (ODEP 2018), (2) a structured online search for literature from academic and non-academic sources, (3) consultation with experts in SAW/RTW interventions from the private and public sectors, and (4) targeted web searches of six large employers and 10 private disability insurers. Altogether, these methods identified 68 SAW/RTW programs. We excluded six of those programs from this evidence review because they are interventions targeted to SSDI beneficiaries.

The set of programs covered by the evidence we reviewed for this report is larger than the set of programs discussed in Deliverable 2.1. This report includes evidence that pertains to (1) one-time, temporary programs or strategies, (2) smaller-scale efforts focused, for example, on particular medical diagnoses or professions, and (3) programs and initiatives in contexts outside the U.S.

This report provides a comprehensive summary of the evidence available about SAW/RTW programs and evaluation activities currently underway. The findings from the program synthesis and this evidence review pertain to a wide range of programs, programmatic enhancements, and innovative policies that we will consider when developing evaluation design options. In the program synthesis we included both programs with strong evidence and programs with no prior evaluations and those with relatively low-quality evidence. Similarly, we consider all types of programs to develop evaluation design options, not only those with a strong evaluation record.

The remainder of this report proceeds as follows:

- Chapter 2 summarizes the selection criteria and review methods.

- Chapter 3 presents a summary of the evidence available in 28 review articles identified in our search.
- Chapter 4 synthesizes evidence from 87 studies identified in our search using meta-analysis.
- Chapter 5 summarizes conclusions of the review.

Appendix A displays the results of the random effects meta-analyses in figures. Appendix B discusses the technical methods we used in the meta-analysis and Appendix C lists all studies included in the review.

2. Review Methods

Towards developing future intervention and evaluation options, this review seeks to identify evidence of the effect of SAW/RTW initiatives on employment and receipt of federal disability benefits. Further, we want to understand how evidence of the effect of SAW/RTW initiatives might vary by the evaluation design, which we discuss further below.

Section 2.1 explains how we selected evidence for review from the many sources we considered. Section 2.2 explains how we evaluated the strength and relevance of the evidence studies, and how we summarize the impact sizes.

2.1 Selection of Evidence for Review

We identified the studies reviewed in three ways:

1. We searched for and reviewed evidence (articles and websites) for the 68 programs identified during the search process for the program synthesis.[7] However, we found that only 11 programs had direct evidence of impacts, and of these, 6 are not early interventions. We excluded evidence about the six late-stage interventions from this this review. We discuss the evidence about the five early intervention programs in Appendix E of Epstein, et al. For some of these five programs, we found multiple studies with direct evidence of impacts, and we include each in the sample of studies reviewed in Chapter 4 of this report.
2. Because we found scant evidence of effectiveness for the 68 programs identified in the program synthesis, we searched the academic literature for evidence about other programs, much of which focuses on programs operating outside the U.S[8]. When conducting the search for the program synthesis, we found several review articles, research syntheses, and meta-analyses. These types of articles were not relevant to the program synthesis. However, we included all studies cited in these articles and published in 2008 or later in this evidence review. We limited the scope of the review to studies published in the last 10 years to ensure that we considered the most relevant evidence—in which findings are still applicable, and where the technologies, laws and public policies examined are still relevant. We also included articles referenced in those reviews. A small proportion of these articles supplied evidence of effectiveness, which we review in Chapter 4.
3. The review articles cited many articles in the *Journal of Vocational Rehabilitation* and in the *Journal of Occupational Rehabilitation*. Since the most recent articles in those journals had not been included in the already-published reviews, we searched all articles in those two journals from 2016 to March 2018 for evaluations of RTW/SAW programs.

   - **These three search processes identified 377 articles and websites for review.**

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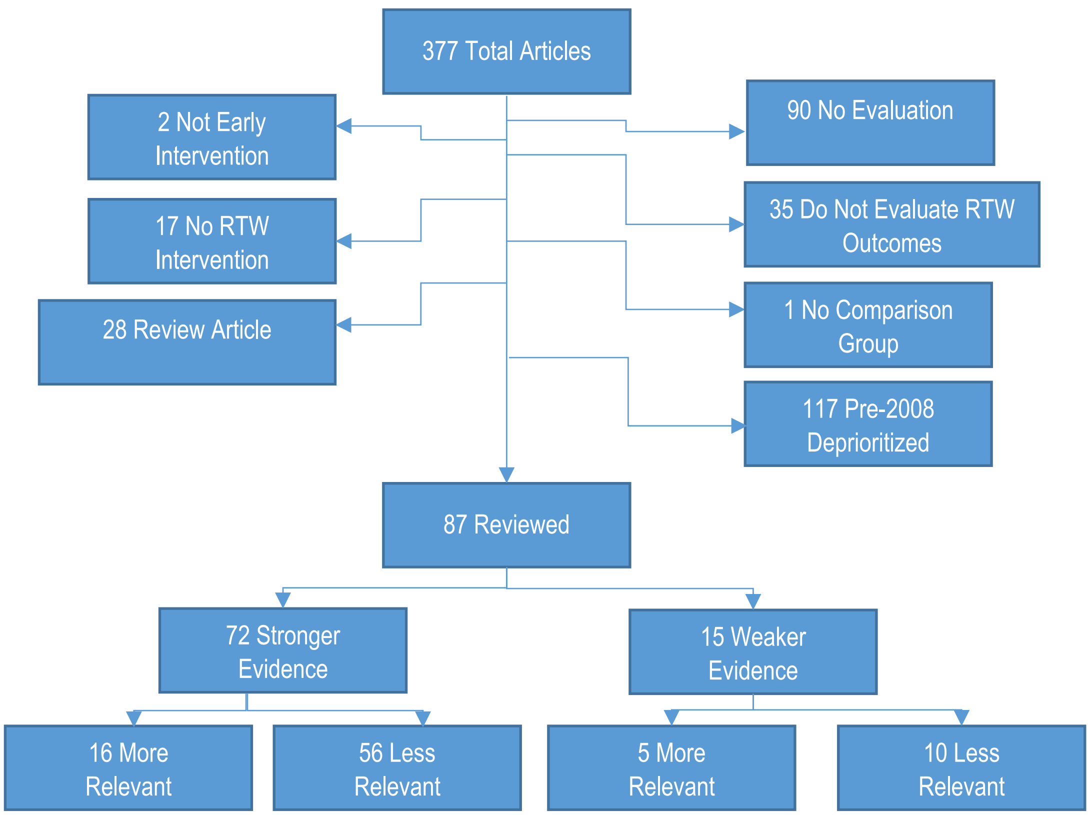
7 We searched for the programs by name, in Google Scholar.

8 We searched Google and Google Scholar to identify the 68 programs using these terms: stay at work, return to work, preferred worker, occupational health, workers compensation, short-term disability, vocational rehabilitation, plan for employement, job accommmodations, and case managementused.

Exhibit 2.1 provides a Consolidated Standards of Reporting Trials (Schulz et al. 2010) diagram that shows the number of articles or websites reviewed and counts of articles or websites discussed in Chapters 3 and 4. Of the 377 identified articles, we removed 90 that did not include an evaluation, and 35 that did not did not evaluate outcome measures that can generate relevant findings. We also eliminated 111 studies with publication dates prior to 2008, two studies of programs that are not early interventions[9], one study that did not involve a comparison group, and 17 that did not study a return to work intervention. We also identified 28 review articles that we discuss in Chapter 3.

We included the 87 remaining articles in the meta-analysis of evidence reported in Chapter 4. Of the 87 articles, 72 provided "high-quality" evidence (16 judged more relevant, and 56 less relevant) and 15 provided "low-quality" evidence (5 judged more relevant and 10 less relevant), using the standards described in the next section.

Exhibit 2.1. Consolidated Standards of Reporting Trials Diagram



Chapter 3 describes findings from the 28 review articles that are themselves reviews of evidence. These reviews of evidence are not included as independent sources of evidence in the systematic review of evidence. We synthesize findings from the 87 individual studies using meta-analysis in Chapter 4.

9 We excluded studies on the effects of six late-stage interventions that we had identified in the program synthesis. Our search for this report also found studies about two other late-stage interventions and we excluded those from the evidence review.

2.2 Evidence Review Process

To review the evidence, the Abt team developed a five-step process and applied it uniformly across all 87 articles reviewed.

- **We labeled each study (1) as having "high-quality or low-quality evidence" and (2) as being "more or less relevant." We then (3) recorded numerical estimates of employment and disability receipt, and coded (4) program model and (5) disability type examined in each study.**

Sections 2.2.1 and 2.2.2 describe how we assign internal validity ratings and external validity ratings. Section 2.2.3 describes how we code the reviewed studies with respect to program impacts on employment and disability benefits (including application or receipt of federal disability benefits). Since these are the twin goals (increasing employment and decreasing benefit receipt) of a SAW/RTW program, we have limited the measures we examine to only these two outcome measures.

2.2.1 Ratings for Evidence Strength

Evidence is "high-quality," i.e., has high "*internal validity,*" if it convincingly establishes that an intervention *causes* the difference in mean outcomes reported (including the case where there is no difference in outcomes); otherwise, it is low-quality.[10] The most rigorous studies use random assignment to a treatment condition that receives the intervention or to a control condition that does not receive the intervention.[11] These studies also maintain low attrition in both assignment groups over the course of the study. Evaluations that use random assignment designs allow us to expect that differences in outcomes for the treatment and control groups reflect only the influence of the intervention, and not any underlying differences in the composition of the two groups.

Several review standards offer researchers methods for evaluating the internal validity of a study. Internal validity refers to how well a study can establish causal effects of the intervention on the outcomes measured, that is, what we refer to as "quality" of the evidence. Our internal validity ratings range from 0 (no evidence) to 5 (most rigorous evidence) and are explained below in Exhibit 2.2. We rate studies with internal validity scores of 4 and 5 as having "high-quality evidence" and studies with internal validity scores of 1, 2, and 3 as "low-quality evidence." We developed these standards to be compatible with review standards used in the What Works Clearinghouse Standards 4.0 (IES 2017) or the Department of Labor's Clearinghouse for Labor Evaluation and Research (CLEAR 2015).[12]

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10 Strong evidence does not mean evidence of positive impacts; here it means evidence that, due to the design and implementation of the study, is expected to be free from selection bias.

11 Typically, the control condition is "usual care," or business as usual, but sometimes is a waiting list for treatment or another active treatment.

12 Unlike a full What Works Clearinghouse review, our reviewers did not complete a review workbook for each study reviewed, each study was reviewed by only one reviewer, and reviewers did not evaluate the reliability of the outcome measures of each study. This omission is purposeful: we are searching for all evidence regarding two specific outcomes (employment and disability benefit receipt). Unlike a What Works Clearinghouse or Clearinghouse for Labor Evaluation and Research review, we assign ratings from 0 to 5, and then further label those ratings as "strong," "weak," or "no" evidence.

Exhibit 2.2. Ratings of Internal Validity (quality of evidence)

5	Experimental design, or Randomized Control Trial, with: – Attrition is less than 65% and differential attrition is less than 10%; AND – Intent to treat analysis (no noncompliance, or failures to adhere to RCT protocol); AND – No confounding factors; AND – Symmetric outcome data collection in treatment and control groups; AND – Analysis includes statistical adjustments for selected baseline characteristics on which equivalence between experimental groups was not established; AND – Confirmation that intervention occurred with fidelity to design/description.
4	Experimental design with: – No confounding factors; AND – Symmetric outcome data collection in treatment and control groups; AND – Attrition is less than 65% and differential attrition is less than 10%.
3	Quasi-experimental design, such as matched comparison group, difference-in-difference, or regression discontinuity designs, or experimental designs not included above, with: – Concurrent comparison cases; – Established baseline equivalence of treatment and comparison groups OR adjustment for baseline differences; – Symmetric outcome data collection in treatment and control groups; and – Confirmation that intervention occurred with fidelity to design/description.
2	Quasi-experimental design with poor equivalence of treatment & comparison group (standardized differences exceeding 0.25 on any baseline variables); or QED with unknown baseline equivalence, including time series or pre-post studies with no comparison group.
1	Focus groups, interviews, case studies.
0	No outcomes measured.

Note: This deliverable labels studies with an evidence quality (internal validity) rating of 4 or higher as "high-quality" evidence, and studies with an evidence quality (internal validity) rating of 1 to 3 as "low-quality" evidence. Studies with a rating of 0 are categorized as providing "no" evidence.

2.2.2 Ratings of Evidence Relevance

The "external validity" of an evaluation is the ability to generalize research findings broadly to all types of disabilities and across the U.S. Our external validity ratings range from 0 (not generalizable to U.S. contexts) to 5 (nationally representative) and are explained in Exhibit 2.3 below. We label studies with external validity scores of 2 or higher as "more relevant" and studies with external validity scores of 0 or 1 as "less relevant" (most of these are from settings outside the U.S.).

Exhibit 2.3. Ratings of External Validity (relevance of evidence to U.S. labor market)

5	Nationally representative of workers with a wide variety of work impairments.
4	State-wide sample representative of workers with a wide variety of work impairments (for example, can include Workers Compensation claimants only).
3	State or nationally representative of workers in specific occupation or industry.
2	Injury- or illness-specific, for example lower back pain only or mental illness only.
1	Site, occupation, and injury-specific.
0	International programs, including nationally representative studies in a non-U.S. context, or a review article including many levels above.

This deliverable labels studies with an evidence rating of 2 or higher as "more relevant" and other studies as "less relevant."

2.2.3 Analysis of Impact Sizes

We recorded impacts on the two outcomes of interest: employment and disability benefits. To enable us to characterize how impacts on the two outcomes vary across studies, we also record two other study features: the type of disability examined and program model(s) used in the intervention.

- **We analyzed programs' effects on employment and disability benefit use.**

By Impact. For studies providing evidence, we analyzed programs' effect on employment and disability application or receipt, i.e., the impact estimates. Out of 87 studies that provide evidence, 82 reported impacts on employment, and 13 reported impacts on SSDI application or receipt of disability benefits. Impacts are reported for binary outcomes (e.g., working vs not working), and typical follow-up periods range from six months to two years, though some studies report on follow-up over five years.

Some studies report impacts at multiple follow-up periods; we form a weighted average of each study's reported estimates using a within-study meta-regression (regressing impact on follow-up time, using the methods explained in Appendix B) and assign each study's predicted estimate to the longest follow-up period. This puts studies that report multiple estimates on an equal footing with studies that report a single estimate of impact, and predicts using the best measure of impact and the variance for that estimate (excluding the empirical Bayes or posterior mean estimates of random effects).

Researchers reported impacts on employment and benefit receipt in many different ways: hazard or risk ratios, odds ratios or log odds ratios, percentage point differences, percent increases, and other metrics. We converted all impact estimates on employment or benefit receipt to log odds ratios, so we can interpret estimates (roughly) as percent changes in odds ratios. The log odds ratio is a standard measure of effect size, which puts all effects on a common scale.[13] The log odds ratio also produces symmetric interpretations. For example, an intervention that increases the proportion working from 40 to 50 percent would have an effect size of 0.4, and an intervention that decreases the proportion working from 50 to 40 percent would have an effect size of -0.4 which is not true of other scale-free summary measures. If we instead measured the percentage change in the proportion working in the first case, we would calculate a

[13] The log odds is a good effect size measure because it is approximately normal even when underlying data are very skewed. Also known as the logit transformation, the log odds for a proportion p is the natural log of the odds p/(1-p), or ln(p/(1-p)) and the log odds ratio for two proportions p and q is ln(p/(1-p)) minus ln(q/(1-q)).

25 percent increase (for a 10 percentage point change), and in the second case, we would calculate a 20 percent decrease (for a 10 percentage point change).

The log odds ratio is the only measure in which the numerical estimate is unaffected by how we define the outcome group (e.g., those at work, or those not at work). We also estimated the precision of those estimates (i.e., the standard error of that log odds ratio) using the number of study participants in the treatment and comparison groups.[14]

- **We also coded for disability type and for program model.**

By Type of Disability. In addition to recording impacts for each study, we coded three types of disability: musculoskeletal, mental health conditions, and other. "Other" includes conditions that are neither musculoskeletal nor mental health. "Other" also includes studies that examined a broad range of disability types, possibly including musculoskeletal and mental health conditions among a wider range of disability types. A study that includes a variety of conditions, but explicitly mentions musculoskeletal conditions as being included, would be coded 1 (true) for both musculoskeletal and other.

By Program Model. In addition to recording impacts and specific types of disability examined for each study reviewed, we also recorded five indicators characterizing the programs as belonging to one or more of the five broad program models defined in Deliverable 2.1 (counts of how often each indicator was used in the studies included in subsequent analysis is shown in Exhibit 2.4):

1. ***Employer-provided accommodations***: "Reasonable accommodations," as included in Title I of the Americans with Disabilities Act (ADA), allow individuals with a disability to successfully perform their job duties.[15] Common examples include physical changes to a workplace, the provision of assistive technologies, or modifications to the policies and procedures that determine a worker's duties. Using this broad definition, we coded the SAW/RTW interventions that include employer-provided accommodations.
2. ***Financial incentives for employers and workers***: The accommodation strategies discussed above approach the issue of SAW/RTW only as a matter of an individual's capability to carry out his/her job duties and do not attempt to shift the worker and the employer's financial motivations regarding a return to work. The financial returns to work, for the worker and the employer, are conceptually distinct from functional capacity or capability to work. Therefore, we coded separately those SAW/RTW programs that offered financial incentives for employers or workers to encourage employment. We identified two primary types of employer-targeted incentives: those implemented through WC programs and those implemented through the tax code. We also identified policies operated through workers' compensation and short-term disability insurance programs to lessen disincentives to work that arise for workers. These include not allowing vacation and sick time to accrue during the absence, holding the job open for a defined period of time, setting proactive return to work policies, and communicating with workers during the absence. Insurers can also implement programs to improve the attractiveness of returning to work. Some of these programs operate through

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14 For two groups with sample sizes N_1 and N_2, with relevant proportions p_1 and p_2, the standard error of the log odds ratio is $[(N_1p_1)^{-1} + (N_1(1- p_1))^{-1} + (N_2p_2)^{-1} + (N_2(1- p_2))^{-1}]^{1/2}$.

15 "Accommodations," U.S. Department of Labor, accessed February 22, 2018, https://www.dol.gov/odep/topics/Accommodations.htm

incentives to employers to offer the worker an easy return to work. . For more discussion of types of incentives, see Epstein et al, ***Synthesis of SAW/RTW Programs, Efforts, Models, and Definitions***.

3. ***Information***: Information programs offer services delivered by a "Return-to-Work Coordinator," case manager, or other individual that facilitates communication between stakeholders. We also include programs that assist workers to navigate the disability benefits system and other services, such as those available through the health care or workforce development system. This component also includes "technical assistance" services typically delivered by an industry expert to help employees understand their benefits or employers to modify their workplace. In some cases, the programs and interventions identified in our review are comprised only of an information component. These also included technical assistance strategies to assist employers, or technical assistance to American Job Centers.
4. ***Medical Management***: The programs coded in this category attempt to facilitate or improve upon the delivery of medical care with a focus on occupational health to promote employment retention. Examples include providing guidance to physicians on best practices in occupational health, incorporating a physician's input into transitional or alternative work arrangements, or lowering the cost of medical care through subsidized insurance premiums. Services also often include incentives for altering occupational health practices, best practice guidelines, or educational services to change behavior.
5. ***Employment Services and Training***: Employment services facilitate entry into a new occupation/position, likely following functional rehabilitation and typically delivered during a period of unemployment. This component includes re-employment services to assist with navigating the labor market, such as job search, resume writing, and interviewing assistance. These programs can also include higher intensity services like occupational skills training delivered either in a classroom, lab, or an on-the-job setting.

Exhibit 2.4 tabulates the frequency of each component across the 87 studies reviewed. Information components are the most commonly studied, followed by medical management components. Financial incentives are the least commonly studied component. In all, we identified more than 120 types of services in review, which means some programs included more than one type of service. Roughly 40 percent of studied programs included more than one type of service.

Exhibit 2.4. Program Model Counts in Individual Studies Reviewed

Employer-provided accommodations	29
Financial Incentives to employers or workers	3
Information	45
Medical management	34
Employment Services/Training	17

Note: Counts are based on tabulations of results from authors' review of 87 studies. Roughly 40 percent of the programs that were evaluated in the studies we reviewed included more than one type of service.

2.2.4 Synthesis

- **We use a meta-regression to describe how impacts vary with program model, disability type, and evidence quality.**

As we discuss in Chapter 3, it appears that the available evidence may not support the conclusions in many review articles. For this evidence review, we use a formal meta-analytic framework to discipline our conclusions. We chose the framework that is least susceptible to manipulation to synthesize the findings from all identified studies. We combined the impact estimates and the program model information in a meta-analytic regression framework (DerSimonian and Laird 1986; Knapp and Hartung 2003). Specifically, we use a random-effects meta-regression to look for whether certain program components or disability types are associated with greater impacts on employment or SSDI application. For reasons explained in Chapter 3, we split the evidence into stronger and weaker evidence for some of the analysis in Chapter 4, to mitigate the effect of selection bias in each study.

3. Evidence Findings—Literature Reviews

Our search for evidence yielded 28 review articles. In this chapter we discuss evidence available in these review articles. In general, the literature reviews that have examined the body of evidence on SAW/RTS programs find that most studies provide no evidence, and for those that do provide plausible evidence of impacts, the impacts are close to zero in most cases and do not differ statistically from zero. The chapter focuses on two types of literature reviews: narrative reviews and systematic reviews. Narrative reviews, do not attempt to systematically collect and summarize all existing evidence. Systematic reviews carefully document the collection process for evidence. These systematic reviews describe all studies regardless of the findings, though some authors exclude studies with lower quality designs in any quantitative analysis of effects and meta-analyses give less weight to studies with larger standard errors.

Previewing the findings to be presented below, some findings are consistent across narrative and systematic reviews. Each type of review finds evidence that for musculoskeletal conditions; especially those involving low back pain, a variety of interventions are effective. In addition, each type of review finds evidence that, for individuals with mental illness, supported employment models are effective, but not other interventions. Beyond those two findings, the high-quality evidence tends to show that interventions are ineffective or have very small impacts (in the sense of modest estimated effect sizes that tend to be statistically not distinguishable from zero). It is only when averaging across studies that we find statistically significant evidence of modest impacts.

3.1 Narrative Reviews

Narrative reviews are common, partly because there is a paucity of high-quality studies reporting outcome data with a comparison group design. As Johnston et al. (2009) and Johnston, Sherer, & Whyte (2006) point out, there are very few high-quality meta-analytic reviews and most studies do not provide evidence with good internal validity, in which outcomes can plausibly be traced to the intervention being studied. Because such narrative reviews are a common form of evidence, we discuss some of the better-known reviews here. Before doing so, we emphasize that the selection of studies for these reviews is not systematic or representative of all evidence. Further, many of these studies do not connect the numerical estimates to the description of the findings. That is, the stated finding of a narrative review may contradict the numerical evidence in the average impact of cited studies. The balance of this section provides a pair of examples for these claims.

Huber et al. (2017) claim that:

> *A review of the literature in the field published over the past few decades reveals a steady increase in outcomes research demonstrating the positive impact of specific interventions in vocational rehabilitation services (Bolton, Bellini, & Brookings, 2000; Dean et al., 2014; Fleming et al., 2013; Glasner-Edwards& Rawson, 2010; Leahy et al., 2014; O'Neil, Mamun, Potamites, Chan, & Cardoso, 2014; Pruett et al., 2008) and in other rehabilitation-related areas (Bond, 2004; Huber et al., 2009; Marshall & Lockwood, 1998; Workman et al., 2012)"* (Huber et al. 2017; p.12).

However, Bolton, Bellini, & Brookings (2000) did not show impacts, but rather reported a correlation between receipt of rehabilitative services and future employment. Another cited paper, Dean et al. (2014), describes evidence that does not support the claim that the paper is cited to support. First Dean et al. (2014, p.2) state that the:

> *results may suggest that vocational rehabilitation programs can effectively reduce SSDI/SSI roles for certain subgroups, (p.2)*

Then the authors write that they:

> *caution against drawing this type of causal conclusion… [as] this analysis is largely descriptive, and does not formally address the fundamental methodological problem involved in drawing such inferences from observational data.*

- **This highlights a common feature of narrative reviews: that impact estimates in cited articles do not always support the broad claim made in the narrative.**

Stapleton et al. (2015) has similar issues. Many claims in the paper lack evidence to support them. On page 4, the paper claims:

> *Considerable success can be achieved by providing supports to workers with [musculoskeletal] conditions (particularly [low back pain]), mental health conditions, or other chronic conditions for which adherence to treatment is critical.*

However, the paper's summary of the evidence does not necessarily support that statement. Table II.1 cites Hullegie and Koning (2014) as providing evidence of positive impacts for individuals age 40–58, especially males. The paper states:

> *One intriguing finding from the Netherlands: that country's early intervention system appears to have had greater success among male workers between the ages of 40 and 58 who experienced unexpected hospitalizations than for other workers who experienced unexpected hospitalizations—males ages 25–39 or females in either age group. (Stapleton et al. 2015, p.4).*

The reforms in the Netherlands enhanced employer incentives, by making employers responsible for paying sickness benefits and by strengthening monitoring obligations. However, the reforms were universal, leaving no comparison group. Hullegie and Koning (2014) instead compare return to work after an unscheduled hospital admission before and after the reforms, across demographic groups, and conclude older male workers return at relatively higher rates after the reform, but this is driven by falling rates of work among younger workers.[16] Thus, the Hullegie and Koning (2014) paper does not truly provide evidence of positive impacts, or better impacts for male workers between the ages of 40 and 58.

Similarly, Stapleton et al. (2015) show a summary table showing early intervention having positive impacts for musculoskeletal conditions and other low mortality conditions (Williams et al. 2007, Waddell et al. 2008, Van Oostrom et al. 2009, Wickizer et al. 2011, Dibben et al. 2012, Hoefsmit et al. 2012,

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[16] Hullegie and Koning (2014), "For disabled men aged 25–39 the relative employment rates initially increased, but this was reversed in 2005, the year in which the law that makes employers responsible for the first two years of sickness pay became effective. The relative employment rate of disabled men aged 40–58 seems unaffected by the reforms."

Bevan 2015, Linton et al. 2016, Richmond et al. 2015), mental health conditions (Killackey et al. 2008, Dibben et al. 2012, Hoefsmit et al. 2012, Rost et al. 2004), and chronic conditions requiring adherence to treatment.[17] The paper further claims "Numerous studies point to employer cooperation as a key component of success in staying at work or returning to work" (Stapleton et al. 2015, p.5) though no evidence is adduced. Many of the cited studies do not provide evidence of impact (e.g., Linton et al. 2016), and others review evidence without regard to study quality and conclude with mixed findings, for example, that "the only area that currently offers sufficient evidence for effect-size meta-analysis appears to be in the area of workplace-based interventions for low back pain, where there are several early and recent meta-analyses" (Dibben et al. 2012, p.26).

- **It is difficult to draw a reliable conclusion from narrative reviews, as they neither provide an unbiased selection of studies, nor do they formally synthesize numerical findings across studies.**

In summary, this discussion suggests that the selection of studies in narrative reviews makes it impossible to judge whether the selection creates a bias toward positive or negative findings. In addition, the absence of a formal weighing of evidence on a common metric make it difficult to assess whether claims of the common findings in the literature are in fact supported by the preponderance of evidence. For this reason, we use narrative reviews primarily to identify additional studies that we should include in our review.

3.2 Systematic Reviews

In contrast to narrative reviews, systematic reviews attempt to summarize all evidence on a topic, recent evidence, or a representative sample of that evidence. However, because of the comprehensiveness of systematic reviews, many of them limit their attention to a specific type of disability or intervention. In many of these reviews, a limit is placed on the scope in one way (e.g., only care coordination models are considered, which we consider an "informational" program model), but the studies are further disaggregated in another way (e.g., for cases where the primary condition is chronic low back pain). This makes it difficult to draw conclusions about how average impacts vary as these multiple features of a study vary. Here we consider 10 such reviews, to illustrate these points and highlight the need for the analysis conducted in Chapter 4. Because each study defines the interventions in scope in a different way, the studies examined may overlap, and distinctions across systematic reviews and their conclusions are frequently obscure. We discuss these reviews primarily to motivate the analysis that follows, not to suggest any of these findings should be taken as dispositive.

The two broadest reviews we found were Waddell, Burton, and Kendall (2008) and Kuoppala and Lamminpää (2008). Waddell, Burton, and Kendall (2008) reviewed 450 studies published from 2000–2007 on interventions related to "common health problems that account for two-thirds of long-term sickness (mild/moderate musculoskeletal, mental health and cardiorespiratory conditions)" that could

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[17] The references regarding chronic conditions requiring adherence to treatment are: Birnbaum et al. (2010) and Burton et al. (2007) on adherence with antidepressant use; Carls et al. (2012) on adherence for diabetes, hypertension, dyslipidemia, asthma/ chronic obstructive pulmonary disease and congestive heart failure; Gifford et al. (2014) review on adherence for diabetes, cardiovascular, depression; Hagen et al. (2014) on oral hypoglycemic agents; Jinnett et al. (2012) on medication adherence and filing short-term disability claims; Loeppke et al. (2011) on adherence to statins in CAD and absenteeism; and Wagner et al. (2012) on adherence to antihypertensive medication regime.

improve "work outcomes (staying at, returning to and remaining in work)." For the purposes of their review, "vocational rehabilitation was defined as whatever helps someone with a health problem to stay at, return to and remain in work," and we may thus interpret this review as including all types of SAW/RTW programs. The authors conclude that interventions can improve work outcomes for those with musculoskeletal conditions. However, they claim that there is little evidence in the study that treatments for mental illness improve work outcomes,[18] and there is no evidence of impact for cardiorespiratory conditions.

Kuoppala and Lamminpää (2008) found "moderate evidence" in 45 studies that "return-to-work programmes decrease long sick leaves" with a risk ratio (RR)[19] of 0.46, and a range of 0.25–1.10. They also conclude that multimodal rehabilitation decreases the risk of disability pension (RR 0.64, range 0.52–1.14). However, the risk ratio confidence intervals include 1, so we should conclude the findings were not statistically significant. That is, while the point estimates indicate promising impacts, we cannot conclude by any reasonable standard of statistical significance that the results were not due to chance.

- **Systematic reviews that disaggregate by condition or type of intervention find some impacts, though estimated impacts tend to be small.**

We examined systematic reviews focusing on interventions targeting musculoskeletal conditions. Van Vilsteren et al. (2015) found, for workers with musculoskeletal disorders, that workplace interventions reduced time to first return to work and cumulative duration of absence from work. The review also examined time to lasting return to work and the authors found only low-quality evidence for workplace interventions. Palmer et al. (2011) conducted a systematic review of community- and workplace-based interventions to reduce sickness absence and job loss in workers with musculoskeletal disorders. Interventions included prescribed exercises, behavioral change techniques, workplace modifications, and provision of extra services. Most interventions appeared modestly beneficial: participants were more likely to avoid job loss related to musculoskeletal disorders, and the median reduction in sickness absence was about one day per month. However, RTW was not significantly higher.

Other systematic reviews examine interventions for workers with mental health conditions. Nieuwenhuijsen et al. (2014) synthesized evidence on interventions to improve return to work among individuals with depression. The study found "moderate quality[20] evidence that a work-directed intervention added to a clinical intervention reduced sickness absence (SMD -0.40; 95% CI -0.66 to -0.14; three studies)."[21] Enhancing primary or occupational care with cognitive behavioral therapy reduced sick leave, per the authors, who "found moderate quality evidence based on three studies that telephone or online cognitive behavioral therapy was more effective in reducing sick leave than usual primary or occupational care (SMD -0.23; 95% CI -0.45 to -0.01)." However, after reviewing the literature for adults

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18 "There is limited evidence that symptomatic treatments for depression (by medication, psychotherapy or a combination of both, and including Cognitive Behavioural Therapy) in themselves improve occupational outcomes and no clear evidence on the magnitude of any effect" Waddell, Burton, and Kendall (2008; p.21). This conclusion motivates our exploration of all impacts in Chapter 4.

19 The risk ratio (RR) is a measure of effect size commonly used when comparing across studies, which is similar to the odds ratio when both risks are small.

20 Nieuwenhuijsen et al. (2014) "judged 14 studies to have a high risk of bias and nine to have a low risk of bias."

21 SMD refers to Standardized Mean Difference, or difference in means divided by the pooled standard deviation, a summary statistic used in meta-analysis when studies assess the same outcome but measure it in different ways. The SMD allows analysts to standardize the results of studies to a uniform scale.

with adjustment disorders (sometimes called exogenous, reactive, or situational depression, which occurs when an individual is unable to adjust to or cope with a particular stress or a major life event) Arends (2012) found that cognitive behavioral therapy did not significantly speed return to work compared with no treatment.

Some systematic reviews find employment-oriented interventions can produce positive effects on employment for those with severe mental health conditions.

Some systematic reviews have found that employment-oriented interventions (which we may think of as related to the program synthesis concept of Employment Services or Training) can produce positive effects on employment for those with severe mental health conditions. Suijkerbuijk et al. (2017) conclude that supported employment produces better outcomes than medical treatment alone for individuals with severe mental illness.

Studies focused on chronic pain reach a variety of conclusions. Examining Cognitive-Behavioral treatments or cognitive behavioral therapy for low back pain, Richmond et al. (2015, p.14) conclude that in "the majority of studies, when compared to other typical physiotherapy-based treatments, a cognitive behavioral intervention is more effective" at reducing pain, but "when a single study with large effect sizes was removed from [the] analyses, the pooled estimates were reduced and were more consistent with previous meta-analyses of cognitive behavioral versus other active treatments." A similar study by Kamper et al. (2015) found that "Multidisciplinary biopsychosocial rehabilitation interventions were more effective than usual care (moderate quality evidence) and physical treatments (low quality evidence) in decreasing pain and disability in people with chronic low back pain. For work outcomes, multidisciplinary rehabilitation seems to be more effective than physical treatment but not more effective than usual care." Williams et al. (2012) reviewed evidence on psychological therapies for the management of chronic pain and found "Cognitive behavioral therapy but not behavior therapy has small effects on disability associated with chronic pain."

Two recent reviews of return-to-work programs examine coordination of care (which is included in the program synthesis concept of an informational intervention). Schandelmaier et al. (2012) reviewed nine trials from seven countries (eight focused on musculoskeletal conditions and one on mental conditions) that generally followed participants for 12 months or less. The study found that "moderate quality evidence suggested a benefit of return to work coordination on proportion at work at end of follow-up (risk ratio = 1.08, 95% CI = 1.03 to 1.13; absolute effect = 5 in 100 additional individuals returning to work, 95% CI = 2 to 8)." A more recent review by Vogel et al. (2017) of return-to-work programs that incorporate coordination of care searched 4,261 articles, found 183 articles with potentially relevant findings, and analyzed 14 of them that provided experimental findings on these return-to-work programs.

The two review articles on coordination models included the same nine studies, but Vogel et al. (2017) add more studies then separately aggregate by length of follow-up, which leads to (1) finding no significant impacts overall and (2) a pattern of point estimates that shift from favoring RTW programs at short-run (less than one year) follow-up to favoring usual practice at longer run follow-up periods. This may be due to the two studies (Lindh 1997 and Jensen 2012) that have follow-up beyond one year, but also produced negative point estimates at one year.

There are two studies in each of the two reviews (Schandelmaier et al. 2012 and Vogel et al. 2017) of coordination models that find positive impacts. These two studies are not examining programs that clearly differ from those in other studies, except insofar as they focus on musculoskeletal conditions and low back pain. As Vogel et al. (2017, p.21) report, "Eliminating two outliers, both clearly in favour for return-to-work coordination programmes (Donceel 1999; Lambeek 2010), reduced heterogeneity for time to return to work to a moderate level, even though these programmes showed no obvious differences to other studies." On balance, return-to-work programs that rely on case coordination seem to offer no advantage over alternatives, except possibly when treating only musculoskeletal conditions and low back pain. All of the cited studies that meet our inclusion criteria for the evidence review are included in the meta-analysis in Chapter 4.

- **We find many instances of systematic reviews on the same narrow topic reaching contradictory conclusions.**

In short, the evidence is mixed. However, none of the systematic reviews look across all of the available evidence; instead, each focuses on a pre-specified subset of available evidence.

3.3 Comparing Systematic and Narrative Reviews

The previous two sections show strongly contrasting results. Narrative reviews tend to conclude that programs work; most systematic reviews conclude that programs do not work. In the prior section, we illustrated that the systematic reviews that do conclude interventions work often do not find statistically significant impacts, or are overturned by subsequent re-analysis. This section attempts to explain the divergence between the takeaways from narrative and systematic reviews.

To weigh the available evidence, the systematic reviews try to retrieve all studies meeting explicit criteria. The alternative is that researchers draw studies according to some implicit criteria, e.g., what he/she remembers or what he/she considers important. In as much as doing so draws on prior opinion, prior opinion can unconsciously influence the inclusion of evidence (Wason 1960; Kunda 1999). However, systematic reviews tend to exclude weaker evidence that may show promising results for programs not yet evaluated using an experimental design. A useful middle ground is a systematic review that does not exclude nonexperimental evidence, a form of which we report in Chapter 4.

- **There is substantial evidence of selection bias in individual studies, and selected sampling in reviews.**

Some of the weaker evidence referred to in narrative reviews shows impacts that are inconsistent with the stronger evidence. For example, there is almost no evidence that interventions that rely on coordination of care are effective, in the sense of detecting positive impacts, when we examine only high-quality evidence. Yet Wickizer et al. (2011) conclude that the COHE model produces large gains in return to work and reductions in application for disability benefits. This may be because the study uses a difference-in-difference design with substantial baseline differences between the treated and comparison groups. Additionally, Clayton et al. (2011) reviewed 31 studies which evaluated initiatives with an individual focus (improving an individual's employability or providing financial support in returning to work) and found that "many of the studies suffered from selection bias[22] into these programmes of more work-ready claimants."

[22] Note that in this context, selection bias refers to participants having intrinsically better outcomes on average (e.g. there are unobserved factors that promote better outcomes and also lead people to participate in a program), so that a comparison of outcomes makes program effects more positive than the true causal impact.

These findings motivate our choice to divide the evidence we present in Chapter 4 into stronger and weaker evidence to mitigate the effect of selection bias in each study. Even when effects measured via these two different types of studies do not differ statistically, combining studies that suffer from selection bias with those that do not is not strictly justified in a meta-analytic framework, as we cannot assert that the underlying effect being measured is the same in the two kinds of studies. Splitting the studies into high- and low-quality evidence enables us to see whether the underlying effect being measured seems to differ across the two kinds of studies, both qualitatively, and in the sense of a statistical difference.

This is one kind of selection bias that plagues individual studies. Another kind of selection bias is a problem in the selection of studies: that certain kinds of studies are drawn for review, due to implicit cognitive bias.

4. Synthesis of Evidence

To better understand the body of evidence about SAW/RTW programs, we use a random-effects meta-analytic regression to synthesize findings across all the 87 studies we identified that provide any evidence of impact.[23] This is the most efficient means of summarizing numerical estimates across studies and allows us to compare across multiple study features at once.

- **We use a meta-analytic regression to summarize findings.**

We measure all effects on the log odds scale, for both binary outcomes (employment or disability benefit receipt), which ensures estimates are scale-free and can be compared across disparate settings. We first compute a single effect size estimate for each outcome (employment or disability receipt) in each study. Each study reviewed that provides estimates at multiple follow-up points provides one data point, which we construct separately via a within-study random-effects meta-analytic regression. Then we impute the predicted effect size and the standard error of the prediction to the last date of follow-up, so we have one aggregated point estimate per study, per outcome.[24]

We first explore the overall average impacts across all reviewed studies. Because our review of narrative and systemic review articles indicates a high risk of selection bias in individual studies, we next explore the overall average impacts for those with high-quality evidence (Internal Validity rating at least 4, for experimental designs). Third, we explore the overall average impacts for low-quality evidence (lower Internal Validity rating, for studies not using an experimental design). For each, we examine impacts on both employment and disability benefits.

Columns 1 and 3 of Exhibit 4.1 show the average impacts. Specifically, they are the results of meta-analyses (or equivalently, meta-regressions with just a constant). Models that include an indicator for weaker evidence are shown in columns 2 and 4. For neither model is the impact on disability benefits statistically distinguishable from zero. Apparently this is in part because each included study has low statistical power. However, the average impact on employment is large,[25] and the average impact is not statistically distinguishable across stronger and weaker designs in column 2. That we cannot distinguish strong and weak evidence statistically in this model does not imply that we must combine these studies. That weaker evidence tends to have lower magnitude impacts could reflect negative selection into interventions, that is, individuals with worse average outcomes are more likely to participate in the studied interventions. In any case, we believe it is useful to analyze the 72 stronger and 15 weaker studies separately, and we do so in the next two sections.

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23 The random-effect meta-analytic regression is a generalization of meta-analysis: with just a constant term in the regression, a random-effect meta-analytic regression is a random-effects meta-analysis. The results of the random effects meta-analyses corresponding to columns 1 and 3 in Exhibit 4.1 are displayed as figures in Appendix A. See Appendix B for details on methods.

24 With multiple estimates per study, one can include multiple estimates from each study in the overall meta-regression and adjust standard errors for correlations with study, but it is more straightforward to first aggregate the estimates within a study to a single meta-analytic average, so we have one estimate per study. The random effects meta-analytic average incorporates the chance variation across estimates in its estimate of the overall impact that would be estimated across many hypothetical re-estimations.

25 As an example, an increment to the log odds of one half corresponds to an increase in the probability of working from 50 percent to 62 percent, or from 10 percent to 15 percent.

- **The average impact on employment is positive, and higher quality studies tend to estimate larger impacts.**

Exhibit 4.1. Overall Effect Size Averages

	(1)	(2)	(3)	(4)
	Employment Effect Size		Disability Benefits Effect Size	
LowQualityDesign		-0.181 [-0.539,0.176]		0.0514 [-0.391,0.494]
Constant (Mean effect)	0.459*** [0.313,0.605]	0.499*** [0.334,0.663]	-0.0983 [-0.298,0.101]	-0.117 [-0.385,0.152]
Observations	82	82	13	13
I2	0.626	0.630	0.876	0.877

Note: Not all studies report impacts for both outcomes, so the sample sizes refer to the studies that report on the relevant outcome. 95 percent confidence intervals in brackets, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Exhibit 4.2 reports estimates from models that disaggregate effects on employment. Column 1 disaggregates by program model, column 2 by disability type, and column 3 by length of follow-up data collection. Only program type (column 1) shows statistically significant variation, though point estimates in column 2 suggest positive effects for musculoskeletal conditions could be driven by selection and the point estimate on follow-up time in column 3 suggests impacts may decline with increasing follow-up time. There is no statistically significant difference (the p-value for a joint F test for all terms involving Low-Quality Design appears in the last row of the table) between stronger and weaker evidence in this pooled model, but the only two significant coefficients lead to two different conclusions for the two types of evidence. This leads us to conclude we should analyze these groups of studies separately.

Exhibit 4.2. Meta-analytic Regression on Study Characteristics

	(1)	(2)	(3)
	Employment	Employment	Employment
LowQualityDesign	0.139 [-0.760,1.038]	-0.146 [-0.633,0.340]	-0.105 [-0.715,0.505]
Accommodation	-0.146 [-0.509,0.218]		
Financial Incentive	-0.458 [-1.267,0.351]		
Information	0.165 [-0.285,0.615]		
Medical Management	-0.109 [-0.534,0.317]		
Employment Services, Training	1.170*** [0.638,1.702]		
LowQualityDesign*Accommodation	0.0515 [-0.777,0.880]		

	(1)	(2)	(3)
	Employment	Employment	Employment
LowQualityDesign*FinancialIncentive	0.154 [-0.970,1.278]		
LowQualityDesign*Information	-0.366 [-1.238,0.505]		
LowQualityDesign*MedicalManagement	0.391 [-0.330,1.111]		
LowQualityDesign*EmploymentServicesTraining	-1.243* [-2.188,-0.299]		
Musculoskeletal		-0.496 [-1.501,0.509]	
Mental		-0.354 [-1.407,0.699]	
Other Disability		-0.520 [-1.640,0.601]	
LowQualityDesign*Musculoskeletal		0.736 [-0.981,2.452]	
LowQualityDesign*Mental		-0.155 [-1.045,0.735]	
Followup			-0.0181 [-0.222,0.186]
LowQualityDesign* Followup			-0.0300 [-0.318,0.258]
Constant	0.293 [-0.198,0.784]	0.963 [-0.123,2.049]	0.523*** [0.230,0.816]
Observations	82	82	82
p-value of joint test on all LowQualityDesign Variables/Interactions	0.104	0.725	0.718

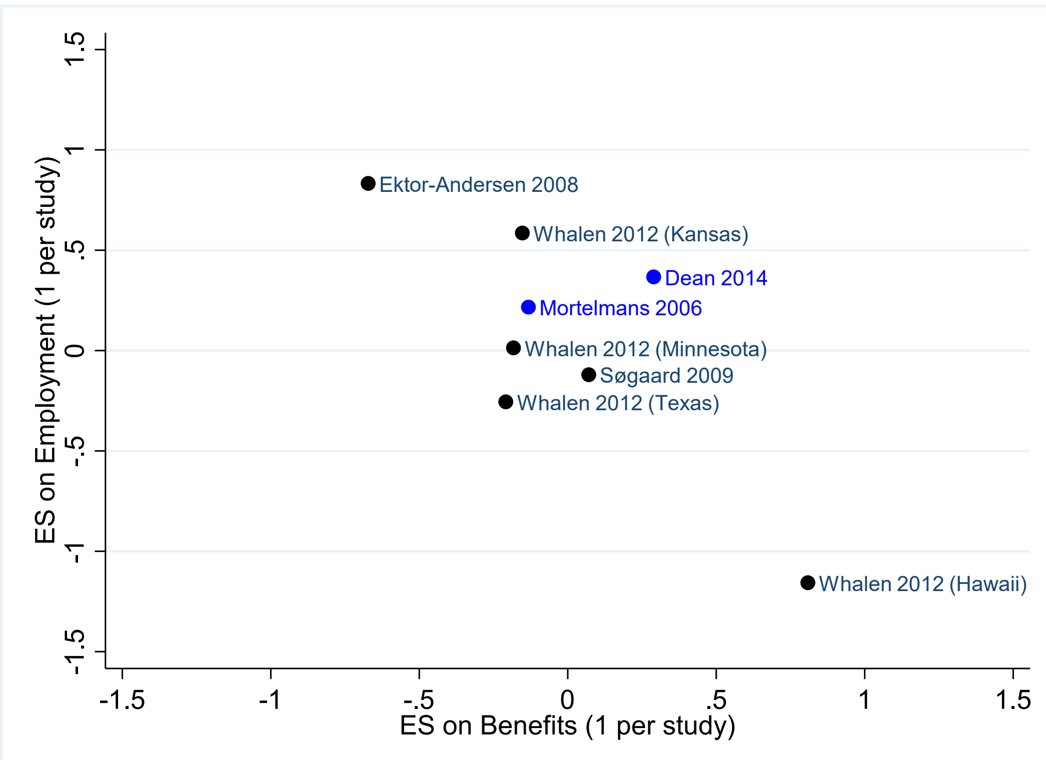
Note: Not all studies report impacts for both outcomes, so the sample sizes refer to the studies that report on the relevant outcome. 95 percent confidence intervals in brackets, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The difference between all high-quality and low-quality evidence is not statistically significant, in the other pooled meta-analytic regressions (final row of columns 2 and 3). Frequently, patterns in the data are overlapping, and we cannot distinguish statistically[26] which of several competing hypotheses we can reject about what explains differences in impact estimates across studies. To explore these patterns further, we separately analyze the high-quality and low-quality evidence in the following two sections of the document.

26 In a meta-analytic regression on all study characteristics, we sacrifice too many degrees of freedom, and confidence intervals become very large; only the coefficient on Employment Services and Training differs statistically from zero. But the regression with all study characteristics included many estimated coefficients, and we should expect at least one to differ statistically from zero based on chance alone. The many-dimensional pooled meta-analytic regression also suffers excess heterogeneity across studies, and we do not report it here.

There are very few studies with both employment and benefits impacts reported, shown on a scatterplot in Exhibit 4.3, where the two weaker designs are shown as blue dots, and they are bracketed by the stronger evidence, shown as black dots. Statistically, one cannot reject that this pattern is a cloud of random noise, with mean zero in both dimensions. A priori, we may expect a negative correlation between effect sizes on employment and benefits, if interventions that are effective will tend to increase employment and lower benefit receipt and other interventions will tend to decrease employment and raise benefit receipt rates. That we observe no statistically significant pattern of this type may suggest that some interventions work better than others or it may simply reflect sampling variation due to the low power of studies reviewed.

Exhibit 4.3. Studies with Both Employment and Disability Benefits Effect Sizes (ES) Reported



Note: High-quality evidence shown in black and low-quality evidence in blue.

In the following two sections, we separately analyze the high-quality evidence first, including only experimental designs, then the low-quality evidence, including only quasi-experimental designs.

4.1 High Internal Validity Studies

This section considers high internal validity studies. Ideally, we would examine a body of evidence that has extremely high internal validity (credibly causal impacts) and external validity (nation- or state-wide representative sample of multiple types of disability), but only one exhibits both. The Kansas Demonstration to Maintain Independence and Employment, described in Whalen (2012), encompasses a wide variety of conditions or disability types, and has a statewide sample, for an external validity rating of 4. In Hawaii, the demonstration study recruited individuals with diabetes, and in Minnesota and Texas, participants all had behavioral health conditions (Whalen 2012), so these studies are rated 2 on the external validity metric (site- or disability-specific interventions). Similarly, the remainder of the high internal validity studies have lower external validity, with 15 rated as 2 on the external validity metric, and 52 others rated zero because they are in settings outside the U.S.

For the 72 studies with stronger designs that include sufficient data to compute effect sizes and standard errors, we list the 16 studies rated with high internal validity and greater external validity in Exhibit 4.4 below and the 56 studies rated with high internal validity and lesser external validity in Exhibit 4.5 below.

Exhibit 4.4. Characteristics of Studies with High-quality Evidence and Greater Relevance

Cite	Location	Measures Employment	Sample Size (Employment Impact)	Measures Disability Applications	Sample Size (Disability Applications Impact)	Musculoskeletal	Mental Health	Other	Accommodation	Financial Incentive	Information	Medical Mgmt.	Employment Serv./Training	Internal Validity	External Validity
Bohman 2011	Texas	Yes	1,480	No	N/A	0	1	0	0	1	1	0	0	5	4
Bond 2015	Illinois	Yes	87	No	N/A	0	1	0	1	0	0	0	1	4	2
Davis 2012	Alabama	Yes	85	No	N/A	0	0	1	1	0	0	0	1	4	2
Godges 2008	California	Yes	36	No	N/A	1	0	0	0	0	1	0	0	4	2
Lerner 2015	USA	Yes	380	No	N/A	0	1	0	0	0	1	0	0	4	2
Matchar 2008	USA	Yes	437	No	N/A	0	0	1	0	0	1	0	0	4	2
Rosenheck 2017	USA	No	N/A	Yes	363	0	0	1	0	0	0	1	1	4	2
Sacks 2008	Pennsylvania	Yes	198	No	N/A	0	1	0	0	0	1	0	0	4	2
Schoenbaum 2001	USA	Yes	1,158	No	N/A	0	1	0	0	0	0	1	0	5	2
Smith 2002	USA	Yes	219	No	N/A	0	1	0	0	0	0	1	0	5	2
Twamley 2012	USA	Yes	58	No	N/A	0	0	1	1	0	0	0	1	4	2
Wang 2007	USA	Yes	604	No	N/A	0	1	0	0	0	1	0	0	5	2
Whalen 2012	Minnesota	Yes	451	Yes	500	0	1	0	0	0	1	1	0	5	2
Whalen 2012	Kansas	Yes	826	Yes	1,155	0	0	1	0	0	1	1	0	5	4
Whalen 2012	Texas	Yes	1,146	Yes	1,585	0	1	0	0	0	1	1	0	5	2
Whalen 2012	Hawaii	Yes	146	Yes	184	0	0	1	0	0	1	1	0	5	2

Note: N= 16 studies.

Exhibit 4.5. Characteristics of Studies with High-quality Evidence but Lesser Relevance

Cite	Location	Measures Employment	Sample Size (Employment Impact)	Measures Disability Applications	Sample Size (Disability Applications Impact)	Musculoskeletal	Mental Health	Other	Accommodation	Financial Incentive	Information	Medical Mgmt.	Employment Serv./Training	Internal Validity	External Validity
Andersen 2015	Denmark	Yes	54	No	N/A	1	0	0	0	0	0	1	0	5	0
Bejerholm 2015	Sweden	Yes	87	No	N/A	0	0	1	1	0	0	0	1	4	0
Bethge 2010	Germany	Yes	169	No	N/A	1	0	0	0	0	1	0	0	4	0
Bethge 2011	Germany	Yes	149	No	N/A	1	0	0	0	0	1	0	0	4	0
Bjorneklett 2013	Sweden	Yes	236	No	N/A	0	0	1	0	0	1	0	0	5	0
Brendbekken 2017	Norway	Yes	284	No	N/A	0	0	1	0	0	1	0	0	5	0
Brouwers 2006	Netherlands	Yes	194	No	N/A	0	1	0	0	0	1	1	0	5	0
Bültmann 2009	Denmark	Yes	113	No	N/A	1	0	0	0	0	1	0	0	5	0
Busch 2011	Sweden	No	N/A	Yes	110	0	0	1	0	0	1	0	0	4	0
Carlsson 2013	Sweden	Yes	33	No	N/A	1	1	0	0	0	0	1	0	4	0
Ektor-Andersen 2008	Denmark	Yes	575	No	N/A	0	0	1	0	0	1	0	0	5	0
Fleten 2006	Norway	Yes	990	No	N/A	1	1	0	0	0	1	0	0	5	0
Hagen 2010	Norway	Yes	238	No	N/A	1	0	0	0	0	0	1	0	5	0
Henchoz 2010	Switzerland	Yes	67	No	N/A	1	0	0	0	0	1	0	0	4	0
Heslin 2011	England	Yes	135	No	N/A	0	1	0	1	0	0	0	1	5	0
Hoffmann 2014	Switzerland	Yes	100	No	N/A	0	1	0	0	0	0	0	1	4	0
Hubbard 2013	Scotland	Yes	18	No	N/A	0	0	1	0	0	1	0	0	4	0
Jensen 2012	Denmark	Yes	351	No	N/A	0	0	1	1	0	0	1	0	4	0
Killackey 2008	Australia	Yes	41	No	N/A	0	1	0	1	0	0	0	1	5	0
Kittel 2008	Germany	Yes	300	No	N/A	0	0	1	0	0	0	1	0	5	0
Krogh 2009	Denmark	Yes	83	No	N/A	0	0	1	0	0	0	1	0	5	0
Lambeek 2010	Netherlands	Yes	134	No	N/A	0	0	1	1	0	0	1	0	4	0
Li-Tsang 2008	China	Yes	63	No	N/A	0	0	1	0	0	1	0	1	5	0
Linton 2016	Sweden	Yes	140	No	N/A	1	0	0	0	0	1	0	0	4	0
Martin 2013	Denmark	Yes	168	No	N/A	0	0	1	1	0	1	0	0	5	0
Michon 2014	Netherlands	Yes	148	No	N/A	0	1	0	1	0	0	0	1	5	0
Myhre 2014	Norway	Yes	406	No	N/A	0	0	1	1	0	0	1	0	4	0
Netterstrom 2013	Denmark	Yes	140	No	N/A	0	1	0	0	0	1	0	0	4	0
Noordik 2013	Netherlands	Yes	143	No	N/A	0	1	0	0	0	0	1	0	5	0
Ntsiea 2015	South Africa	Yes	80	No	N/A	0	0	1	1	0	1	0	0	5	0
Odeen 2013	Norway	Yes	93	No	N/A	1	0	0	0	0	1	0	0	4	0
Oshima 2014	Japan	Yes	37	No	N/A	0	1	0	1	0	0	0	1	4	0
Pedersen 2015	Denmark	Yes	400	No	N/A	0	1	0	0	0	1	0	0	4	0
Rebergen 2009	Netherlands	Yes	240	No	N/A	0	1	0	0	0	0	1	0	5	0
Rossignol 2000	Montreal	Yes	110	No	N/A	1	0	0	1	0	0	1	0	4	0

Cite	Location	Measures Employment	Sample Size (Employment Impact)	Measures Disability Applications	Sample Size (Disability Applications Impact)	Musculoskeletal	Mental Health	Other	Accommodation	Financial Incentive	Information	Medical Mgmt.	Employment Serv./Training	Internal Validity	External Validity
Schene 2007	Netherlands	Yes	59	No	N/A	0	0	1	0	0	1	0	0	5	0
Scholz 2016	Switzerland	No	N/A	Yes	8,050	0	0	1	1	0	0	1	0	5	0
Schultz 2008	Canada	Yes	34	No	N/A	1	0	0	1	0	1	1	1	4	0
Shete 2012	India	Yes	44	No	N/A	1	0	0	0	0	1	1	0	4	0
Streibelt 2014	Germany	Yes	102	No	N/A	1	0	0	0	0	1	0	0	5	0
Søgaard 2009	Denmark	Yes	836	Yes	836	0	1	0	0	0	0	1	0	5	0
Tamminga 2013	Netherlands	Yes	133	No	N/A	0	0	1	1	0	0	1	0	4	0
Thunnissen 2008	Netherlands	Yes	108	No	N/A	0	0	1	0	0	1	0	0	4	0
Tsang 2009	Hong Kong	Yes	111	No	N/A	0	1	0	1	0	0	0	1	4	0
Van Oostrom 2010	Netherlands	Yes	145	No	N/A	0	0	1	1	0	0	0	0	4	0
Van Beurden 2015	Netherlands	Yes	119	No	N/A	0	0	1	0	0	0	1	0	4	0
Varekamp 2011	Netherlands	Yes	115	No	N/A	0	0	1	1	0	0	0	0	5	0
Vlasveld 2013	Netherlands	Yes	126	No	N/A	0	1	0	1	0	1	0	0	4	0
Volker 2015	Netherlands	Yes	220	No	N/A	0	1	0	1	0	0	1	0	4	0
Vonk 2014	Netherlands	Yes	209	No	N/A	0	0	1	0	0	1	0	0	4	0
Waghorn 2014	Australia	Yes	208	No	N/A	0	1	0	1	0	0	0	1	4	0
Willert 2011	Denmark	Yes	102	No	N/A	0	0	1	0	0	1	0	0	4	0
Wong 2008	Hong Kong	Yes	92	No	N/A	0	1	0	1	0	0	0	1	4	0
van der Klink 2003	Netherlands	Yes	153	No	N/A	0	1	0	0	0	1	0	0	4	0
van der Feltz-Cornelis 2010	Netherlands	Yes	60	No	N/A	0	1	0	0	0	0	1	0	4	0
van Vilsteren 2017	Netherlands	Yes	150	No	N/A	0	0	1	1	0	1	0	0	5	0

Note: N = 56 studies

Exhibit 4.6 below summarizes the results of a random-effect meta-analytic regression[27] of effects on employment across all 72 studies. The average impact across studies (weighted by relative precision) is 0.514 on the log odds scale, a large impact.[28] This corresponds to an increase in employment rates from 53.5 percent (average control group employment across studies) to 65.8 percent. Disaggregating this impact by study characteristics—using meta-analytic regression in column 2—implies that programs with an employment services and training component have substantively and statistically significantly higher impacts than programs without this component, and the average program with no employment services and training component has a substantially lower impact. The average program with no employment

27 See appendix B for methods.

28 There is no universally accepted definition of "large" impacts, but an effect size of about 0.20 is often called "small" while 0.50 is called "medium" and 0.80 is considered "large" (Hill et al. 2007).

services and training component has an impact that is not statistically significantly different from zero (since the constant term, and all other coefficients, are not statistically different from zero). Put differently, the overall positive impact of the programs hides the fact that employment programs have a large impact and other programs have a smaller impact that is not distinguishable from zero.[29]

Because there are so few estimates of impacts on application for or receipt of disability benefits, we are not able to estimate the way in which those impacts vary with study characteristics and can only report the average impact (column 3). The result does not differ statistically from zero, but the point estimate is large relative to other programs designed to lower the rates of disability receipt among beneficiaries (-0.0878 on the log odds scale corresponds to a decrease in disability benefit receipt rates from 17 percent to 15.8 percent). The confidence interval includes both relatively large decreases in receipt of disability benefits (e.g., a reduction from 17 to 14.3 percent) and modest increases in receipt of disability benefits (e.g., an increase from 17 to 17.5 percent).

That the estimate for disability receipt has a smaller magnitude than the estimate for employment may indicate that reductions in the fraction of people applying for disability benefits are harder to achieve than increases in the fraction working, for a variety of reasons. It may be that there are more marginal workers than there are marginal applicants for disability benefits. This is consistent with the view that the tradeoff between work and nonwork is a close call for many workers with disabilities (and high disutilities of work given labor market conditions) but that disability benefits are relatively well-targeted at the group facing the most severe barriers to work.

Exhibit 4.6. Meta-analytic Regression Results for Stronger Evidence

	(1)	(2)	(3)
	Employment	Employment	Disability Benefits
Accommodation		-0.143 [-0.552,0.266]	
Financial Incentive		-0.474 [-1.414,0.467]	
Information		0.172 [-0.307,0.651]	
Medical Management		-0.111 [-0.565,0.343]	
Employment Services, Training		1.176*** [0.594,1.759]	
Musculoskeletal		-0.246 [-1.104,0.612]	
Mental		-0.259 [-1.144,0.627]	
Other		-0.257 [-1.212,0.697]	

29 These findings are robust to including indicators for specific levels of internal or external validity, or maximum length of follow-up.

	(1)	(2)	(3)
	Employment	**Employment**	**Disability Benefits**
Constant	0.514*** [0.335,0.693]	0.561 [-0.519,1.640]	-0.0878 [-0.209,0.0330]
Observations	69	69	9
I^2	0.656	0.497	0.253
p-value for test of H_0 of zero heterogeneity	$1.30\text{x}10_{-14}$	0.00000841	0.218

Note: 95 percent confidence intervals in brackets, * *p* < 0.05, ** *p* < 0.01, *** *p* < 0.001

4.2 Lower-quality Evidence

Having considered the studies with high-quality evidence above, we now consider the remaining 15 studies with lower quality evidence (Exhibit 4.8). The impacts (all coefficients in Exhibit 4.8) do not differ statistically from zero, except for the overall impact on employment (without disaggregating by study features). Further, the pattern of point estimates is not even broadly consistent with the findings from the stronger evidence. For example, the overall impact on employment has a confidence interval whose upper bound of 0.431 is lower than the point estimate 0.514 in Exhibit 4.6, column 1. However, we see from Exhibit 4.1 that this difference is not itself statistically significant. No estimates in column 2 are significantly different from zero, but the sign of the point estimates differs, with four of the seven coefficients we can compare differing in sign: information and employment services or training have positive point estimates in high-quality evidence but negative in low-quality evidence, whereas studies examining programs using medical management or targeting musculoskeletal conditions exhibit the opposite pattern. Of course, we can also refer to tests of differences in Exhibit 4.2 to see that programs with employment services and training component are associated with large positive impacts on employment in high-quality evidence but no effect in low-quality evidence, and this is the only statistically significant difference across the two bodies of evidence.

The differences across the two bodies of evidence may be due to a correlation between the quality of implementation of an intervention, and the quality of evidence generated, or it could be due simply to chance. In summary, we can conclude little from the body of low-quality evidence (studies with internal validity rated lower than 4) alone, without reference to the body of high-quality evidence. We might have expected that selection bias would lead us to find uniformly positive impacts in the low-quality evidence, while effects were more muted in the high-quality evidence, but we do not see this pattern. If anything, the high-quality evidence shows larger impacts overall, but there is substantial heterogeneity across studies and types of studies that makes finding any statistically significant differences hard at best. The tremendous variability of results further underlines that the variance of outcomes for any one study is high, and one should avoid cherry-picking results that look favorable.

Exhibit 4.7. Study Characteristics, Low-quality Evidence

Cite	Location	Measures Employment	Sample Size (Employment Impact)	Measures Disability Applications	Sample Size (Disability Applications Impact)	Musculoskeletal	Mental Health	Other	Accommodation	Financial Incentive	Information	Medical Management	Employment Serv./Training	Internal Validity	External Validity
Braathen 2007	Norway	Yes	193	No	N/A	0	0	1	0	0	1	1	0	3	0
Dean 2014	Virginia	Yes	5,163	Yes	5,163	0	0	1	0	1	0	1	1	2	2
Dewa 2009	Canada	Yes	124	No	N/A	0	1	0	1	0	0	1	0	2	0
Grossi 2009	Sweden	Yes	22	No	N/A	0	1	0	0	0	0	1	0	3	0
Hamersma 2008	Wisconsin	No	N/A	No	N/A	0	0	1	0	1	0	0	0	3	4
Howard 2009	Texas	Yes	1,441	No	N/A	0	0	1	1	0	0	0	0	3	2
Jensen 2013	Denmark	Yes	200	No	N/A	1	1	0	0	0	1	0	0	3	0
Karlson 2010	Sweden	Yes	148	No	N/A	0	1	0	1	0	1	0	0	2	0
Karrholm 2008	Sweden	No	N/A	Yes	128	0	0	1	0	0	0	1	0	2	0
Lander 2009	Denmark	Yes	161	No	N/A	0	1	0	0	0	1	0	0	3	0
Langi 2017	USA	Yes	2,521	No	N/A	0	0	1	0	0	0	0	1	3	4
Linden 2014	Germany	Yes	609	No	N/A	0	0	1	0	0	1	0	0	2	0
Mortelmans 2006	Belgium	Yes	1,564	Yes	1,564	0	0	1	0	0	1	0	0	3	0
Stapelfeldt 2011	Denmark	Yes	120	No	N/A	0	0	1	1	0	0	1	0	3	0
Wickizer 2011	Washington	No	N/A	Yes	71,696	0	0	1	0	0	1	1	0	3	4

N= 15 studies with low-quality evidence

Exhibit 4.8. Meta-analytic Regression on Study Characteristics, Low-quality Evidence

	(1)	(2)	(3)
	Employment	Employment	Disability Benefits
Accommodation		-0.0200 [-0.742,0.702]	
Financial Incentive		-0.314 [-0.879,0.252]	
Information		-0.251 [-0.942,0.441]	
Medical Management		0.330 [-0.185,0.844]	
Employment Services, Training		-0.0844 [-0.653,0.484]	
Musculoskeletal		0.458 [-0.667,1.583]	
Mental		-0.0857 [-0.841,0.670]	

	(1)	(2)	(3)
	Employment	**Employment**	**Disability Benefits**
Constant	0.296*** [0.161,0.431]	0.425 [-0.157,1.008]	-0.0400 [-0.725,0.645]
Observations	13	13	4
I^2	0.353	0.277	0.962
p-value for test of H_0 of zero heterogeneity	0.100	0.227	0

Note: Not all studies report impacts for both outcomes, so the sample sizes refer to the studies that report on the relevant outcome. t statistics in parentheses, * p < 0.05, ** p < 0.01, *** p < 0.001. 95 percent confidence intervals in brackets, * p < 0.05, ** p < 0.01, *** p < 0.001

5. Discussion and Next Steps

This document has reported the results of a review of the evidence concerning stay-at-work/return-to-work programs. Specifically, the review has considered evidence about the 28 review articles, and 87 individual studies. The studies that present high-quality evidence of impact do not tend to allow generalization to the entire United States, either because they represent a small geographic area, or take place outside the U.S. This presents challenges to generalizing from the existing evidence.

To address this challenge of synthesizing the evidence, we used meta-analytic regression to review the available evidence in the 87 individual studies formally and objectively. The results of that meta-analytic regression suggest that the stronger evidence shows larger impacts for program models that include employment services, such as the Individual Placement and Support model. Our review of systematic reviews indicates that program models have generally positive results for individuals with musculoskeletal conditions, including low back pain, and some models show positive results for individuals with mental illness. However, this may be because the program models more often used for musculoskeletal conditions and mental illness offer employment services.

In contrast, we do not see any average advantage in impacts for musculoskeletal conditions and mental illness in our meta-analytic review of studies. A caveat to this suggestion is that there is insufficient statistical power due to the small number of studies to rule out quite large systematic advantages or disadvantages for other program types, or by disability type. In addition, longer follow-up tends to be negatively associated with measured impact, though in most groups of studies, we cannot separately identify the difference in measured impacts due to follow-up, program model, and disability type, because there simply are not enough studies to conduct the required meta-analytic regression.

Further, differences between systematic reviews (most of which employ meta-analysis or meta-analytic regression) and narrative reviews in the conclusions drawn indicates a large risk of bias in non-systematic reviews. Differences between findings based on high-quality and low-quality evidence are significant, but we cannot identify any clear pattern, i.e., quasi-experimental designs do not exhibit systematically larger or smaller impact estimates, though the experimental and quasi-experimental designs are associated with different programs models, and each single difference across these designs has a very large confidence interval.

Because of the low statistical power inherent in this synthesis due to the paucity of evidence, we must be careful not to conclude that "absence of evidence" for impacts or differences in impacts is the same as "evidence of absence" of impacts. In some sense, there is at least a very small impact of any program, whether positive or negative, and there are differences across programs (though the differences may be very small), and our failure to detect impacts or differences in impacts means first and foremost that our confidence intervals are too large, primarily because samples are too small to detect the differences that may exist.

However, the highest quality evidence that is most relevant to the U.S. labor market comes from a handful of studies concerning the Individual Placement and Support model for individuals with mental illness, which incorporates extensive employment services and counseling, and the Demonstration to Maintain Independence and Employment evaluations conducted in Kansas, Minnesota, Texas, and Hawaii. While the Individual Placement and Support studies find large positive impacts on employment, on average, the Demonstration to Maintain Independence and Employment studies find little to no impact and have confidence intervals small enough to rule out impacts comparable to those found in the Individual Placement and Support studies.[30]

The range of negative and positive findings could indicate that programs with imperfect targeting or poor implementations yield negative impacts on employment (or increase disability benefit receipt). On the other hand, the wide range of findings may simply result from sampling variation. To be sure, precision is low for most studies, which tends to indicate studies are underpowered, a common design flaw. Meta-regression can overcome this problem only to some extent. Overall, perhaps the most important finding from the current evidence review is that low-quality evaluation designs and small sample sizes have provided little conclusive evidence about the effects of SAW/RTW programs. There is a robust finding from high-quality evidence that programs that involve employment services or training do have large impacts, but most differences across programs could be large in both negative and positive directions. This finding poses challenges for developing evaluation options.

5.1 Next steps for developing Evaluation Design Options

Future evaluation designs must take this challenge seriously and must weigh tradeoffs among quality of research design, necessary sample sizes, and evaluation costs. A substantially larger sample size allows researchers to construct tight bounds on impacts, including in some cases for subgroups. However, the gain in precision per increase in sample falls rapidly as the sample size gets larger, because we need a fourfold increase in sample to achieve a doubling of precision. Estimating impacts for subgroups requires even larger sample sizes. For example, estimating impacts for men and women would require four times as large a sample if men and women are equal fractions of the study sample, but far more than four times as much sample if the fractions are very unequal.

However, carrying out evaluations with sufficiently large sample sizes can be costly—if the researchers must recruit and enroll participants, conduct primary data collection, and program operators must implement the intervention at a large scale. The costs of attaining the large samples needed to detect intervention impacts pose a tradeoff for pursuing evaluation designs that will produce high internal validity. A traditional experimental research design (with participant recruitment, random assignment, and primary data collection) yields high internal validity, but has costs that increase approximately at a linear rate as the sample size increases while precision increases much more slowly. On the other hand a nonexperimental evaluation may be less costly to carry out with large sample sizes because they do not have costs that increase linearly in sample size. However, a nonexperimental design sacrifices internal validity as well as the causal inferences we would like to draw about impacts. As we consider evaluation design options, the team will consider approaches that can reduce the costs of an experimental evaluation to some extent, for example, using administrative data sources instead of surveys to measure outcomes. Another alternative is to exploit digital interventions (such as emails or other zero-cost electronic messaging) or low-cost nudges (such as redesigned forms or application or reporting processes) where the cost does not increase linearly in sample size, but these interventions often have smaller impacts as well.

These tradeoffs imply difficult choices when designing future evaluation options. Given the superior properties of a large sample size and concerns about the cost of an evaluation, we recommend that large-

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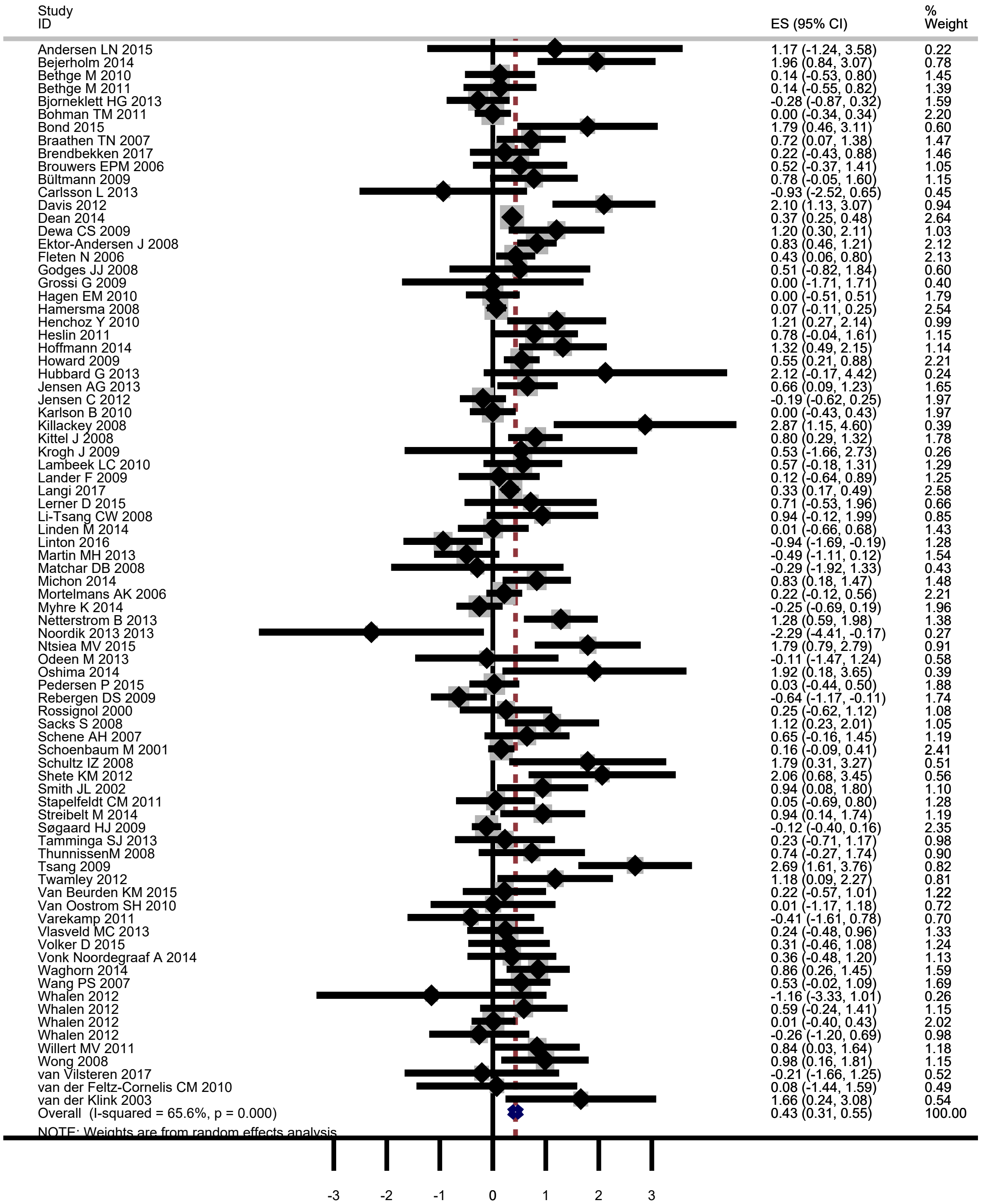
[30] For the effect on employment, a meta-analysis of the 4 state-specific studies in Whalen et al. (2012) gives a confidence interval of (-0.527,0.622) on the log odds scale, while a meta-analysis of the 4 most relevant high-quality studies (Bond 2015; Davis 2012; Twamley 2012; Allaire 2005) gives a confidence interval of (0.704,2.53). No equivalent comparison is possible for effects on disability benefit receipt or application.

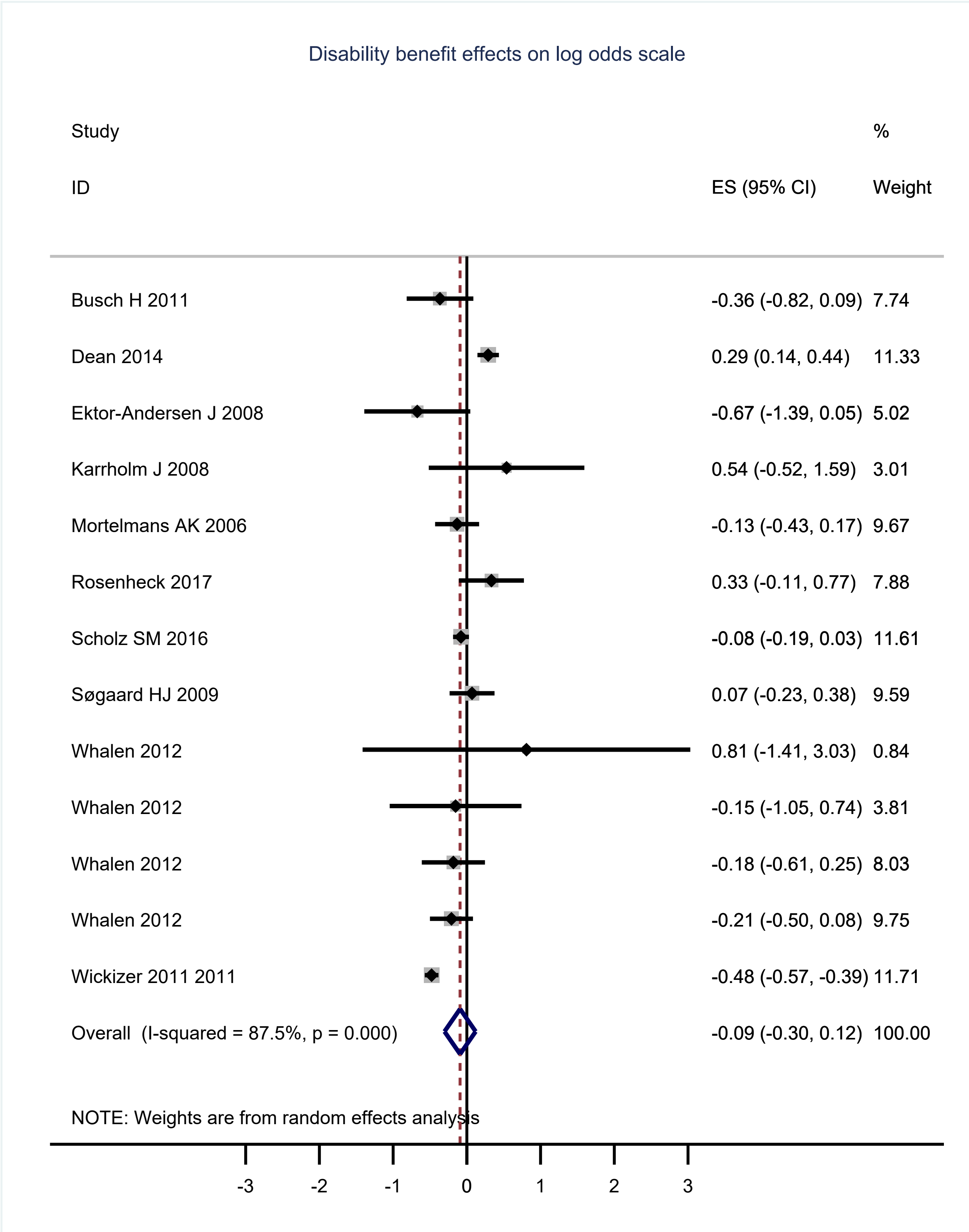
scale low-cost interventions make up at least some of the evaluation designs explored in the future. The most promising impacts from this evidence review concern programs that include employment services and training, but these are also among the most costly evaluations to implement in a large-scale experiment. Recent evidence on low-cost interventions indicates that mailings can effectively change behavior, but that the design of the mailing matters little (Juras et al. 2016), undercutting claims about behaviorally informed design (that is, the style of the messaging may matter less than the message itself, for informational interventions).

With many open questions about the effects of SAW/RTW programs, we also recommend considering design options that would address more than research question. A factorial design would maximize power given that more than one research question exists in each study. Essentially, a factorial design can answer more questions with better power because it exploits extra dimensions in the assignment to treatment arms. For example if we wish to test two dimensions of treatment, for example incentives and information, and if we postulate two directions in which we can alter these relative to the status quo, we can compose a three by three matrix of treatment conditions, for nine conditions (including business as usual). In this example, instead of a sample more than 20 times as large, we would need only a sample a little more than twice as large to measure improvements in both dimensions.

Appendix A. Meta-analytic Average Across All Studies

Employment effects on log odds scale




Disability benefit effects on log odds scale
Study
ID
ES (95% CI)
%
Weight
Busch H 2011
-0.36 (-0.82, 0.09)
7.74
Dean 2014
0.29 (0.14, 0.44)
11.33
Ektor-Andersen J 2008
-0.67 (-1.39, 0.05)
5.02
Karrholm J 2008
0.54 (-0.52, 1.59)
3.01
Mortelmans AK 2006
-0.13 (-0.43, 0.17)
9.67
Rosenheck 2017
0.33 (-0.11, 0.77)
7.88
Scholz SM 2016
-0.08 (-0.19, 0.03)
11.61
Søgaard HJ 2009
0.07 (-0.23, 0.38)
9.59
Whalen 2012
0.81 (-1.41, 3.03)
0.84
Whalen 2012
-0.15 (-1.05, 0.74)
3.81
Whalen 2012
-0.18 (-0.61, 0.25)
8.03
Whalen 2012
-0.21 (-0.50, 0.08)
9.75
Wickizer 2011 2011
-0.48 (-0.57, -0.39)
11.71
Overall (I-squared = 87.5%, p = 0.000)
-0.09 (-0.30, 0.12)
100.00
NOTE: Weights are from random effects analysis
-3
-2
-1
0
1
2
3

Appendix B. Meta-analytic Regression Methods

Looking across studies, there is sampling error in measuring the impact of a program in any one study, which shrinks as the sample size in a study is larger, but there is also variation across studies in true impacts. A random effects meta-regression accounts for both types of error.

Using estimates y_i from N independent studies, each with estimated variance v_i (the square of the standard error of the estimate, s_i), we conduct a random-effects meta-regression using the aggregate-level data with variance-weighted least squares. Meta-regression, also known as meta-analytic regression, extends variance-weighted least squares by estimating an additional component of variance τ^2 characterizing the variance of the error term e_i in equation 1 below.

$$y_i = x_i\beta + u_i + e_i \qquad \text{[Equation 1]}$$

In Equation 1, u_i is assumed to be a normally distributed error term with known standard deviation s_i (the subscript i indicates that the variance will in general vary across units of observation, i.e. studies or estimates), and e_i assumed to be a normally distributed error term with variance τ^2 to be estimated, assumed constant across units. The additional error term e_i represents the random variation of the true impact across studies around an across-study mean true effect. The error term u_i represents sampling variation in the single-study estimate of that study's true mean impact $x_i\beta$+ + e_i (note that u_i does not appear here, as this expression is equal to y_i - u_i).

The matrix x represents characteristics of studies, and also includes a constant. If only a constant is included in x, then the model is equivalent to random-effects meta-analysis, which is why meta-regression is a generalization of meta-analysis. As in other regression methods, if the elements of x are highly correlated, then the variance of each estimate will be appropriately inflated. This implies that collinearity reduces the chance of each element being judged statistically different from zero, but appropriately so.

We use residual maximum likelihood (REML) to estimate the additive (between-study) component of variance τ^2. We use the modification suggested by Knapp and Hartung (2003) to the variance of the estimated coefficients, accompanied by the use of a t-distribution in place of the standard normal distribution when calculating p-values and confidence intervals.

The command used to implement these models is due to Harbord and Higgins (2008). We conducted all estimation in Stata 15.

The percent of total variation that is due to e_i as opposed to u_i is denoted I^2 in tables (and we can test the null hypothesis that this proportion is zero).

Appendix C. References

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