A BRIEF HISTORY OF RESEARCH SYNTHESIS

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Science is supposed to be cumulative, but scientists only rarely cumulate evidence scientifically. This means that users of research evidence have to cope with a plethora of reports of individual studies with no systematic attempt made to present new results in the context of similar studies. Although the need to synthesize research evidence has been recognized for well over two centuries, explicit methods for this form of research were not developed until the 20th century. The development of methods to reduce statistical imprecision using quantitative synthesis (meta-analysis) preceded the development of methods to reduce biases, the latter only beginning to receive proper attention during the last quarter of the 20th century. In this article, the authors identify some of the trends and highlights in this history, to which researchers in the physical, natural, and social sciences have all contributed, and speculate briefly about the "future history" of research synthesis.

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> If, as is sometimes supposed, science consisted in nothing but the laborious accumulation of facts, it would soon come to a standstill, crushed, as it were, under its own weight. The suggestion of a new idea, or the detection of a law, supersedes much that has previously been a burden on the memory, and by introducing order and coherence facilitates the retention of the remainder in an available form. . . . Two processes are thus at work side by side, the reception of new material and the digestion and assimilation of the old; and as both are essential we may spare ourselves the discussion of their relative importance. One remark, however, should be made. The work which deserves, but I am afraid does not always receive, the most credit is that in which discovery and explanation go hand in hand, in which not only are new facts presented, but their relation to old ones is pointed out. (Rayleigh, 1885, p. 20)

So said the professor of physics at Cambridge University in his presidential address to the 54th meeting of the British Association for the Advancement of Science held in Montreal in 1884. More than a century later, research funding agencies, research ethics committees, researchers, and journal editors in most fields of scientific investigation have not taken his injunction seriously. It is true that there have been some improvements recently in the scientific quality of "stand-alone" reviews. When assessing the relation between "new facts" and "old facts" in the Discussion sections of reports of new research, however, scientists very rarely use methods designed to reduce the likelihood that they and their readers will be misled by biases and the play of chance (Clarke & Chalmers, 1998).

SOME EARLY EXAMPLES OF RECOGNITION OF THE NEED FOR RESEARCH SYNTHESIS

Efforts to reduce the likelihood of being misled by biases and chance in research synthesis have quite a long history (Cooper & Hedges, 1994; Hedges, 1987a; Hunt, 1997). In the 18th century, for example, James Lind, a Scottish naval surgeon, was confronted with a plethora of reports about the prevention and treatment of scurvy. The title page of his famous treatise on the disease declares that it contains "An inquiry into the Nature, Causes, and Cure, of that Disease. *Together with a Critical and Chronological View of what has been*

published on the subject [italics added]." Lind (as cited in Hampton, 1998) observed in his text,

> As it is no easy matter to root out prejudices . . . it became requisite to exhibit a full and impartial view of what had hitherto been published on the scurvy, and that in a chronological order, by which the sources of these mistakes may be detected. Indeed, before the subject could be set in a clear and proper light, it was necessary to remove a great deal of rubbish. (p. x).

A couple of decades later, Arthur Young, a gentleman farmer who played a pioneering role in the development of sample surveys, noted that "it is impossible from single experiments, or from a great number, in different lands, separately considered, to deduce a satisfactory proof of the superiority of any method" (as cited in Brunt, 2001, p. 181).

In the early 19th century, the French statistician Legendre developed the method of least squares to solve the problem of combining data from different astronomical observatories where the errors were known to be different (Stigler, 1986), and by the end of the century, some impressive examples of application of the principles of research synthesis had begun to appear. In 1891, for instance, Herbert Nichols published a 76-page review of theories and experiments on the psychology of time.

It was not really until the 20th century, however, that the science of research synthesis as we know it today began to emerge. In 1904, Karl Pearson, director of the Biometric Laboratory at University College London, published a key paper in the *British Medical Journal*. Having been asked to review evidence on the effects of a vaccine against typhoid, Pearson gathered data from 11 relevant studies of immunity and mortality among soldiers serving in various parts of the British Empire. He calculated correlation coefficients for each of the 11 studies (noting that these were very variable and discussing how this variation might be explained) and then synthesized the coefficients within two subgroups, thus producing average correlations (Table 1).

Three years later, Joseph Goldberger (as cited in Winkelstein, 1998), who was working in the laboratory that later became the National Institutes of Health, published an analysis of statistics on bacteriuria in typhoid fever in the District of Columbia. Warren Winkelstein (1998) noted how Goldberger's analysis addressed many of the criteria that research syntheses are now expected to satisfy:

TABLE 1
Inoculation Against Enteric Fever

Correlation Between Immunity and Inoculation			
I.	Hospital staffs	+0.373	±0.021
II.	Ladysmith garrison	+0.445	±0.017
III.	Methuen's column	+0.191	±0.026
IV.	Single regiments	+0.021	±0.033
V.	Army in India	+0.100	±0.013
	Mean value	+0.226	
Correlation Between Mortality and Inoculation			
VI.	Hospital staffs	+0.307	±0.128
VII.	Ladysmith garrison	–0.010	±0.081
VIII.	Methuen's column	+0.300	±0.093
IX.	Single regiments	+0.119	±0.022
X.	Various military hospitals	+0.194	±0.022
XI.	Army in India	+0.248	±0.050
	Mean value	+0.226	

> First, a review of the literature identifies pertinent studies. Goldberger identified 44 studies and provided comprehensive references in a bibliography. Second, specific criteria are used to select studies for analysis. Goldberger used a newly developed serum agglutination test to separate reliable studies from those he considered unreliable. Third, data from the selected studies are abstracted. Goldberger tabulated the raw data from 26 selected studies. Fourth, statistical analysis of the abstracted data is implemented. Goldberger calculated the mean rate of bacteriuria from the pooled data. (p. 717)

Goldberger's attention to each of these steps is an early exemplar of the need to distinguish these two distinct methodological challenges in research synthesis—first, to take measures to reduce bias, then to consider whether meta-analysis can be used to reduce statistical imprecision.

There are other examples of approaches to research synthesis during the first half of the 20th century. In 1916, for example, Thorndike and Ruger derived average results from two experiments comparing the effects of outside air and recirculated air in classrooms on children's ability to add, check numbers and letters, and to find and copy addresses. In 1933, Peters presented a summary of more than 180 experiments on the effects of "character education" on schoolchildren in Pennsylvania. And during the 1930s, research synthesis also began in physics (Birge, 1932) and agriculture (Yates & Cochran, 1938).

A NOTE ON TERMINOLOGY

A variety of terms have been used to describe all or some of the processes to which we have alluded—particularly *research synthesis, systematic review*, and *meta-analysis*.

Our reason for using the term *research synthesis* is primarily because the term has been used extensively by the social scientists who led the development of the science and practice of this kind of research over the post–World War II period.

We might have chosen *systematic review* as an alternative term. There are certainly instances of use of the term *systematic review* earlier than *research synthesis* (Mandel, 1936), but it is uncertain whether use of the former during the pre–World War II period reflected the very structured process that we understand by the term today. Although it was used in the 1970s (Shaikh, Vayda, & Feldman, 1976), it was not until the late 1990s that the term *systematic review* became more widely used. This probably reflected two factors in particular. First, it was the term used by Cochrane (1989) in his foreword to a compilation of research syntheses relating to many aspects of care during pregnancy and childbirth published during the late 1980s (I. Chalmers, Enkin, & Keirse, 1989). The term was subsequently promoted by people concerned to draw a distinction between a process involving measures to control biases in research synthesis and the optional element of that process involving quantitative, statistical procedures, for which they suggested reserving the term *meta-analysis* (I. Chalmers & Altman, 1995; Egger, Smith, & Altman, 2001).

Glass introduced the term *meta-analysis* in 1976 in a presidential address stressing the need for better synthesis of research results. Those who liked neologisms adopted it rapidly, and it was used in the titles of some of the earliest substantive texts on statistical methods for quantitative synthesis (Hedges & Olkin, 1985). It became gradually clear, however, that the word was being used in a variety of ways and that it was intensely antigenic to some people, particularly those who challenged the use of quantitative synthesis to reduce statistical imprecision. Thus, Eysenck (1978) referred to "mega-silliness," Shapiro (1994) to "shmeta-analysis," and Feinstein (1995) to "statistical alchemy for the 21st century." These critics and others showed no appreciation of the need to adopt methods to reduce bias in reviews of research—regardless of whether statistical synthesis could be used to

reduce statistical imprecision. Restricting the term *meta-analysis* to the process of statistical synthesis seemed a way of helping people understand that the science of research synthesis comprises a variety of methods addressing a variety of challenges.

This convention has now been adopted in some quarters. For example, the second edition of the publication *Systematic Reviews* is subtitled *Meta-Analysis in Context* (Egger, Davey Smith, & Altman, 2001), and the fourth edition of Last's (2001) *Dictionary of Epidemiology* gives definitions as follows:

> SYSTEMATIC REVIEW The application of strategies that limit bias in the assembly, critical appraisal, and synthesis of all relevant studies on a specific topic. Meta-analysis may be, but is not necessarily, used as part of this process. (pp. 176-177)
>
> META-ANALYSIS The statistical synthesis of the data from separate but similar, i.e. comparable studies, leading to a quantitative summary of the pooled results. (p. 114)

A definition of our chosen term—*research synthesis*—will have to await publication of the fifth edition of the dictionary!

REDUCING STATISTICAL IMPRECISION IN RESEARCH SYNTHESIS (META-ANALYSIS)

The development of methods for reducing statistical imprecision in research synthesis (meta-analysis) antedated the development of methods for controlling biases. Most statistical techniques used today in meta-analysis have their origins in Gauss's and Laplace's work (Egger, Smith, & O'Rourke, 2001), which was disseminated in a "textbook" on "meta-analysis" for astronomers published in 1861 by the British Astronomer Royal (Airy, 1861). Karl Pearson's (1904) use of statistical methods for research synthesis (see earlier discussion) at the beginning of the following century is an early example of the use of these techniques in medical research. A statistical paper published a few years later by the physiologists Rietz and Mitchell (1910-1911) considered what kind of information a series of experiments can produce.

Several statisticians working in agricultural research in Britain in the 1930s developed and applied these approaches in that field

(Cochran, 1937; Fisher, 1932; Pearson, 1933; Tippett, 1931; Yates & Cochran, 1938). In particular, Ronald Fisher (1932), in his classic text *Statistical Methods for Research Workers*, noted that "although few or [no statistical tests] can be claimed individually as significant, yet the aggregate gives an impression that the probabilities are lower than would have been obtained by chance" (p. 99).

Fisher (1932) then presented a technique for combining the *p* values that came from independent tests of the same hypothesis. Interest in research synthesis among statisticians continued through the Second World War, and Fisher's work was followed by more than a dozen papers published on the topic prior to 1960 (see, e.g., Cochran, 1954; Jones & Fiske, 1953; Mosteller & Bush, 1954).

These statistical procedures for combining results of independent studies were not widely used until the 1960s, when social science research began to experience a period of rapid growth. By the mid-1970s, social scientist reviewers in the United States found themselves having to deal with, for example, 345 studies of the effects of interpersonal expectations on behavior (Rosenthal & Rubin, 1978), 725 estimates of the relation between class size and academic achievement (G. Glass & Smith, 1979), 833 tests of the effectiveness of psychotherapy (M. Smith & Glass, 1977), and 866 comparisons of the differential validity of employment tests for Black and White workers (Hunter, Schmidt, & Hunter, 1979). Largely independently, the research teams addressing these issues rediscovered and reinvented Pearson's and Fisher's solutions to the problem they faced. In discussing his solution, Gene Glass (1976) coined the term *meta-analysis* to refer to "the statistical analysis of a large collection of analysis results from individual studies for purposes of integrating the findings" (p. 3). By the middle of the following decade, Rosenthal (1984) had presented a compendium of meta-analytic methods.

The publication of *Statistical Methods for Meta-Analysis* by Hedges and Olkin in 1985, a key methods paper by Richard Peto and his colleagues published the same year (Yusuf, Peto, Lewis, Collins, & Sleight, 1985), and the proceedings of a meeting convened by the U.S. National Heart, Lung and Blood Institute and the National Cancer Institute published as a special issue of *Statistics in Medicine* in 1987 all helped to secure recognition of the practice of quantitative synthesis of research among statisticians.

REDUCING BIASES IN RESEARCH SYNTHESIS

The development and adoption of methods to reduce biases in research synthesis has tended to lag behind the development of methods to reduce statistical imprecision. With the massive increase in the scale of scientific research after the Second World War, people working in a wide variety of fields began to recognize a need to organize and evaluate the accumulating bodies of research evidence (see e.g., Chase, Sutton, & First, 1959; Greenhouse, 1958; Herring, 1968; Lide, 1981; Lide & Rossmassler, 1973; Schoolman, 1982). It soon became clear that research synthesis threw up a far more complex range of methodological issues than simply the choice of methods for statistical synthesis. In many of the physical sciences, for example, research synthesis became referred to as "critical evaluation," with a substantial emphasis on discovering biases in the individual experiments themselves and developing sets of values of related physical properties that were as consistent and free from bias as possible (see Rosenfeld, 1975; Touloukian, 1975; Zwolinski & Chao, 1972).

The challenge was spelled out well by an American social scientist, David Pillemer (1984), who characterized the usual approach to reviews as

> subjective, relying on idiosyncratic judgments about such key issues as which studies to include and how to draw overall conclusions. Studies are considered one at a time, with strengths and weaknesses selectively identified and casually discussed. Since the process is informal, it is not surprising that different reviewers often draw very different conclusions from the same set of studies. (p. 28)

With a growth of acknowledgment that methodological rigor is needed to secure the validity of research reviews, just as it is for primary research (Cooper, 1982; Jackson, 1980), there was increased appreciation of the range of methods required to prepare unbiased syntheses of research. Social scientists in the United States led the way in this respect. They recognized, for example, that the methods used to select evidence for inclusion in reviews were potentially major sources of bias, particularly as methodological research began to reveal that researchers were more likely to report studies that had yielded "positive" (statistically significant) results. A study of reports published in a sample of psychology journals published in the late

1950s revealed that a very high proportion reported statistically significant results (Sterling, 1959). Investigations of the magnitude of the resulting publication biases made it clear that efforts to control biases in research synthesis would need to address these (Hedges, 1984; Rosenthal, 1979).

With some isolated exceptions (Beecher, 1955; Greenhouse, 1958), people working in health research were relative latecomers to research synthesis. In 1972, Cochrane drew attention to the adverse consequences for the British National Health Service of collective ignorance about the effects of many elements of health care, and in an essay published in 1979, he observed that "it is surely a great criticism of our profession that we have not organised a critical summary, by speciality or subspeciality, adapted periodically, of all relevant randomised controlled trials" (p. 8).

Cochrane's emphasis on randomized controlled trials was relevant to one element of an issue that had emerged among social scientists, namely, which criteria to use for judging when studies could be regarded as sufficiently unbiased for inclusion in research syntheses.

A few "critical summaries of randomized trials" in health care were done during the 1970s (Andrews, Guitar, & Howie, 1980; "Aspirin After Myocardial Infarction," 1980; I. Chalmers, 1979; T. Chalmers, Matta, Smith, & Kunzler, 1977; Stjernsward, Muenz, & von Essen, 1976), but it was not until the following decade that research syntheses of health research began to appear in any numbers and that the scientific issues that needed to be addressed were articulated clearly for people in the health professions. In Kenneth Warren's (1981) seminal book on coping with the biomedical literature, Edward Kass (1981) noted that "reviews will need to be evaluated as critically as are primary scientific papers" (p. 82). Cynthia Mulrow began that process in a seminal article published in the *Annals of Internal Medicine* in 1987 that concluded that review articles published in four major medical journals had not used scientific methods to identify, assess, and synthesize information. Other influential articles addressed to a medical readership were published the same year (L'Abbé, Detsky, & O'Rourke, 1987; Peto, 1987; Sacks, Berrier, Reitman, Ancona-Berk, & Chalmers, 1987).

During the late 1980s, global collaboration among investigators responsible for randomized trials in cancer and cardiovascular disease resulted in research syntheses based on collaborative reanalyses of

individual patient data derived from almost all the randomized trials of certain therapies (Advanced Ovarian Cancer Trialists' Group, 1991; Antiplatelet Trialists' Collaboration, 1988; Early Breast Cancer Trialists' Collaborative Group, 1988). These endeavors became yardsticks against which the scientific quality of other research syntheses in the field of health care would be judged. International collaboration during this time also led to the preparation of hundreds of systematic reviews of controlled trials relevant to the care of women during pregnancy and childbirth. These were published in a 1,500-page, two-volume book, *Effective Care in Pregnancy and Childbirth* (I. Chalmers et al., 1989), deemed an important landmark in the history of controlled trials and research synthesis (Cochrane, 1989; Mosteller, 1993). Three years later, the results were published of a similar project assessing the effects of care of newborn infants (Sinclair & Bracken, 1992).

Within the social sciences, the importance of this phase in the history of research synthesis was reflected in Lipsey and Wilson's (1993) assessment of more than 300 quantitative research syntheses of behavioral and educational intervention studies and Cooper and Hedges's (1994) 570-page *Handbook of Research Synthesis*.

Within health care, the practical importance of improving the scientific quality of reviews was given great impetus by an analysis conducted by a group of researchers led by Thomas Chalmers and Frederick Mosteller: A comparison of textbook advice on the treatment of people with myocardial infarction with the results of systematic syntheses of relevant randomized controlled trials showed that valid advice on some lifesaving treatments had been delayed for more than a decade, and other forms of care had been promoted long after they had been shown to be harmful (Antman, Lau, Kupelnick, Mosteller, & Chalmers, 1992). This report made it abundantly clear that the failure of researchers to prepare reviews of therapeutic research systematically could have very real human costs.

ACADEMIC RECOGNITION OF RESEARCH SYNTHESIS AS RESEARCH

Over recent decades, research synthesis has been widely seen within academia as second-class, scientifically derivative work, unworthy of mention in reports and documents intended to confirm the scientific credentials of individuals and institutions. Indeed,

systematic reviews are sometimes characterized as "parasitic recycling" of the work of those engaged in the real business of science—which is to add yet more data to the atomized output of the overall scientific enterprise.

As Bentley Glass (1976) noted more than a quarter of a century ago,

> The vastness of the scientific literature makes the search for general comprehension and perception of new relationships and possibilities every day more arduous. [Yet] the editor of the critical review journal finds each year a growing reluctance on the part of the best qualified scientists to devote the necessary time and energy to this task. (p. 417)

As Glass observed elsewhere in the article,

> The man who adds his bits of fact to the total of knowledge has a useful and necessary function. But who would deny that a role by far the greater is played by the original thinker and critic who discerns the broader outlines of the plan, who synthesises from existing knowledge through detection of the false and illumination of the true relationships of things a theory, a conceptual model, or a hypothesis capable of test. (p. 417)

Horder's (2001) recently published discussion of the relationship within developmental biological thinking between the organizer concept (articulated in the 1920s) and the concept of positional information (proposed in the 1970s) provides a compelling contemporary illustration of the kind of review for which B. Glass (1976) was calling. Horder concluded his review by noting that "'science' must be acknowledged as being a historical edifice: it not only consists of the latest results, but, more accurately, it is composed of the sum total of a massive accumulation of earlier-acquired data, interpretation and assumptions" (p. 124).

Most people within contemporary academia have not yet recognized (let alone come to grips with) the rationale for and methodological challenges presented by research synthesis. Neither have they grasped that the rationale applies in all spheres of research, not only in the areas of applied social and medical research in which it has begun to flourish. Researchers in applied medical research who have begun to apply the methods of rigorous research synthesis to animal experiments (Horn, de Haan, Vermeulen, Luiten, & Limburg, 2001; I.

Roberts, personal communication, July 2001), for example, have begun to uncover some unsettling findings. A systematic review of the effects of a calcium antagonist (nimodipine) in animal model experiments of focal cerebral ischaemia has raised questions about whether it was ever justified to proceed to controlled trials in humans involving nearly 7,000 patients. A systematic review of the studies in patients did not detect any evidence of beneficial effects of this drug (Horn & Limburg, 2001).

As early as 1971, Feldman wrote that systematically reviewing and integrating research evidence "may be considered a type of research in its own right—one using a characteristic set of research techniques and methods" (p. 86). In the same year, Light and Smith (1971) noted that it was impossible to address some hypotheses other than through analysis of variations among related studies and that valid information and insights could not be expected to result from this process if it depended on the usual, scientifically undisciplined approach to reviews.

In 1977, Eugene Garfield drew attention to the importance of scientific review articles to the advancement of original research: Review articles have high citation rates, and review journals have high impact factors. He proposed a new profession—"scientific reviewer" (Garfield, 1977)—and his Institute for Scientific Information went on to cosponsor (with Annual Reviews Inc.) an annual award for "Excellence in Scientific Reviewing" administered by the National Academy of Sciences (Garfield, 1979).

In the early 1980s, this reviews-as-research perspective was made explicit in two papers published in the *Review of Educational Research*. First, after examining the methods used in 36 review articles sampled from prestigious social science periodicals and concluding that "relatively little thought has been given to the methods for doing integrative reviews," Jackson (1980) proposed six reviewing tasks "analogous to those performed during primary research." A couple of years later, one of us (HC) drew the analogy between research synthesis and primary research and presented a five-stage model of research synthesis involving problem formulation, data collection (the search for potentially eligible studies), data evaluation (quality assessment), data analysis and interpretation (meta-analysis when appropriate), and public presentation (Cooper, 1982). The paper also applied to research synthesis the notion of threats to inferential

validity that had been introduced by Campbell and Stanley (1966) for evaluating the design of primary research (also see Cook & Campbell, 1979).

The promotion of this perspective was given impetus by the publication of two important books in the early 1980s. The more "scholarly" of these was a multiauthor issue of *Evaluation Studies Review Annual* edited by Richard Light (1983) that contained 15 contributions addressing methodological issues and procedures, followed by 20 separate articles illustrating how the methodologies had been applied in practice. In 1984, Richard Light and David Pillemer published their highly readable and influential book titled *Summing Up: The Science of Reviewing Research*. This became a key resource not only for their fellow social scientists but also for the people who were beginning to take this agenda seriously in health care. Building on the principles and resources developed by social scientists, Oxman and Guyatt (1988), for example, published guidelines for assessing the scientific quality of reviews in health care research.

Academic recognition of the science of research synthesis has been growing over recent years. There are examples of its wholehearted incorporation in the methods used in some areas of basic research (e.g., small particle physics and some areas of psychology) and in some areas of applied research (e.g., education and some aspects of health care). As Mark Petticrew (2001) noted in an article exposing some myths and misconceptions about research synthesis, there are research syntheses in such diverse topics as advertising, agriculture, archaeology, astronomy, biology, chemistry, criminology, ecology, education, entomology, law, manufacturing, parapsychology, psychology, public policy, zoology, and even eyewitness accounts of the Indian rope trick.

Even the graphical devices for presenting the results of research syntheses show similarities across widely different spheres of investigation. A form of presentation now often referred to as a "forest plot" (Lewis & Clarke, 2001) plots point estimates from different experiments along with their error bars. This form of presentation is now widely used by health researchers but has also been very commonly used by physicists. For example, Taylor, Parker, and Langenberg (1969) used this method to illustrate the empirical evidence from 12 experiments on an atomic constant called the fine structure constant (Hedges, 1987b).

Because the eye is drawn to the longer error bars in these forest plots, data from the less informative studies have a relatively greater visual effect. To compensate for this distorting feature, boxes with sizes reflecting the inverse of the variance of the estimate derived from each study have been used to mark the point estimates. This device was introduced during the 1980s, principally by medical researchers, and appears to have been inspired by a paper published in 1978 by McGill, Tukey, and Larsen (S. Lewis, personal communication, August 2001).

Even when no study within a group of related studies is sufficiently large to be informative, forest plots may help to reveal a discernable pattern. For example, to test the hypothesis that a widely used form of resuscitation used in critically ill patients—infusion of human albumin solution—reduces mortality, the Albumin Reviewers (2001) analyzed mortality data in 18 randomized trials. In 4 of these trials, none of the participants died, and the number dying in the remaining 14 trials ranged from only 1 to 12. Nevertheless, not only did the forest plot of estimates derived from the 64 deaths that did occur provide no evidence to support the use of a treatment that has been used widely for more than half a century, it actually suggested that human albumin solution increases the risk of death in critically ill patients.

Partly because research synthesis sometimes yields unwelcome results that challenge strongly held opinions and other vested interests, there is very variable acceptance of the scientific principles on which the process is founded. For example, although there is a strong tradition of research synthesis among American social scientists, only a tiny minority of British social scientists has any experience of this form of research, and many appear to be actively hostile to it. Within health research too, attitudes to research synthesis can vary dramatically. Thus, although the *New England Journal of Medicine* published some very important research syntheses during the 1980s, the journal has been overtly hostile to reports of such studies more recently.

As we discuss next, however, we believe that the future status of research synthesis as research is more likely to be shaped by forces outside academia than by those within it. Consumers of research have begun to point out more forcibly that "atomized," unsynthesized products of the research enterprise are of little help to people who wish to use research to inform their decisions.

THE USE OF RESEARCH SYNTHESES TO INFORM POLICY AND PRACTICE

One of the forces shaping perceptions of research synthesis is the growing appetite for research evidence among policy makers, practitioners, and the public more generally. This appetite started to become manifest during the last decade of the 20th century, but earlier examples exist. In a biographical article about the statistician Frank Yates, Michael Healy (1995) noted that

> as the war began and it became clear that phosphate and potash fertilizers were going to be extremely scarce, Yates with E. M. Crowther, the head of the Chemistry Department at Rothampsted, brought together and analyzed all the published experiments on fertilizer responses that they could lay their hands on (Yates & Crowther, 1941). . . . An example of its findings is the statement that the application of 1 cwt/acre of sulphate of ammonia at a cost of £4m would be expected to yield an extra crop to the value of £11m. As a result of this study, fertilizer rationing in the UK was placed on a rational basis and some of the survival of wartime Britain can be set to its credit. Other studies of a similar nature were undertaken at the same time, notably one on the feeding of dairy cows (Yates, Boyd, & Pettit, 1942). It was to be some twenty years before other fields of application began to realise that it was absurd not to look critically from time to time at the collected results of experimental work before deciding upon action, whether in the application of the research or in deciding upon a programme for further research. (p. 277)

It is indeed "absurd not to look critically from time to time at the collected results of experimental work before deciding upon action," but it was not really until the late 1980s that acceptance of the need for research synthesis among policy makers and practitioners emerged, if only because the volume of primary data they were having to cope with was becoming overwhelming. Eleanor Chelimsky (1994), formerly Assistant Comptroller General for Program Evaluation and Methodology at the U.S. General Accounting Office, described the situation that she and her colleagues faced at the beginning of the 1980s:

> I hoped that synthesis could dramatize, for our legislative users, not only what was, in fact, known, but also what was *not* known. In that way, I thought we could then focus attention on what needed to be learned (and how to learn it), in time to answer that policymaker's questions before, say, the next program reauthorization. Based on the

> legislative record for some programs, it seemed obvious that, on the one hand, the distinction between well-established knowledge and mere opinion was not always recognized, and on the other, that what needed to be research *as a next step* was sometimes not even glimpsed. . . . In short, it seemed reasonable to try to develop a systematic method for using synthesis as a way to channel relevant existing information to answer specific congressional questions. (pp. 3-4)

By 1994, 30 research syntheses had been prepared for Congress by the U.S. General Accounting Office on topics ranging from access to special education to the effectiveness of chemical weapons (Chelimsky, 1994).

Syntheses of the results of controlled trials in cancer, cardiovascular disease, and the various forms of care offered to women during pregnancy and childbirth became increasingly accepted during the 1990s as helpful by those wishing to make more informed decisions in health care. Research syntheses were identified for early support when a Research and Development Programme to support the U.K.'s National Health Service (NHS) was launched in 1991 (Peckham, 1991), and this was reflected in the creation of two centers—the NHS Centre for Reviews and Dissemination and the U.K. Cochrane Centre—to help tackle this agenda.

During the 1990s, the importance of research synthesis also became acknowledged among those considering proposals for new research. The NHS Health Technology Assessment Programme and the British and Dutch Medical Research Councils, for example, all began to require systematic reviews of existing research as a precondition for considering funding for proposed additional studies. In Denmark, the national research ethics committee system began to require applicants for ethical approval of proposed new research to show by reference to syntheses of existing evidence that proposed new studies were necessary and that they had been designed to take account of the lessons from previous research (I. Chalmers, 2001). These developments among organizations responsible for the funding and ethical approval of research began to force academia to take research synthesis more seriously. This trend is likely to be given further impetus by the widely publicized death of a young volunteer in a physiological experiment, the design of which had been inadequately informed by a systematic review of preexisting evidence about hazards (Clark, Clark, & Djulbegovic, 2001).

In a history of research synthesis published in 1997, Morton Hunt concluded that systematic reviews of research evidence appear to be having an influence on policies and practices in schools, hospitals, state welfare programs, mental health clinics, courts, prisons, and other institutions. Today's questions about the deployment of limited resources for the benefit of the public may not be those about phosphate and potash fertilizers to which answers were sought more than half a century ago, but the potential for research synthesis to inform decisions about policy and practice remains substantial and still inadequately exploited.

This is not to suggest that there have been no areas in which rigorously conducted systematic reviews have been uncontentious, even when the component studies of the review have been controlled experiments. Reactions to the Cochrane review of the effects human albumin solution in critically ill patients (Albumin Reviewers, 2000) provide a celebrated or notorious example, depending on one's point of view. Reviews of observational data can be relied on to generate even more heat, however, particularly if meta-analysis has been used to synthesize data from nonexperimental studies (Egger, Schneider, & Davey Smith, 1998).

USING ELECTRONIC MEDIA TO KEEP RESEARCH SYNTHESES UP TO DATE AND CORRECT

The growth in appetite for research syntheses among policy makers, practitioners, people using services, and others is a growth in appetite for information that is up to date and correct. This reasonable expectation has posed additional challenges to the research community. The potential for meeting these challenges increased dramatically with the evolution of electronic publishing. In the late 1980s, the international group that had prepared syntheses of research on the effects of forms of care offered during pregnancy and childbirth published their findings in various forms, one of which used electronic media (I. Chalmers, 1988). This meant that syntheses published on paper could be updated and corrected as new data or errors were identified.

At the end of 1992, the U.K. Cochrane Centre was established to draw on this experience and to facilitate the creation of an international network to prepare and maintain systematic reviews of the

effects of interventions across the whole of health care. At the end of the following year, an international network of individuals—the Cochrane Collaboration—emerged from this initiative (Antes & Oxman, 2001; Bero & Rennie, 1995; I. Chalmers, 1993; I. Chalmers, Sackett, & Silagy, 1997; Dickersin & Manheimer, 1998; Oxman, 2001). Since the launch of *The Cochrane Database of Systematic Reviews* in 1995, the research syntheses that have been published by this still young organization have been having an encouraging effect on the content of international guidelines and policies in health care.

Others have recognized that considerable scope exists for extending the collaborative, international arrangements developed by the Cochrane Collaboration for preparing, maintaining, and disseminating research syntheses. In his presidential address to the Royal Statistical Society in 1996, Adrian Smith, professor of statistics at Imperial College London, welcomed the creation of the Cochrane Collaboration and asked,

> But what is so special about medicine? We are, through the media, as ordinary citizens, confronted daily with controversy and debate across a whole spectrum of public policy issues. But typically, we have no access to any form of systematic "evidence base"—and therefore no means of participating in the debate in a mature and informed manner. Obvious topical examples include education—what *does* work in the classroom?—and penal policy—what *is* effective in preventing reoffending? Perhaps there is an opportunity here for the Society—together with appropriate allies in other learned societies and the media—to launch a campaign, directed at developing analogues to the Cochrane Collaboration, to provide suitable evidence bases in other areas besides medicine, with the aim of achieving a quantal shift in the quantitative maturity of public policy debates. (pp. 369-370)

The same principles that have led to the rapid evolution of the Cochrane Collaboration were adopted when the Campbell Collaboration was inaugurated at the beginning of the 21st century. This sibling organization, which draws particularly on the wealth of relevant experience among social scientists in the United States, is preparing, maintaining, and disseminating systematic reviews of the effects of social and educational policies and practices (Boruch, Petrosino, & Chalmers, 1999; Campbell Collaboration Steering Group, 2000). Importantly, the Cochrane and Campbell Collaborations will work

together to develop methods to improve the quality of research syntheses (Clarke & Cooper, 2000).

THE "FUTURE HISTORY" OF RESEARCH SYNTHESIS

> Upon this gifted age, in its darkest hour,
> Rains from the sky a meteoric shower of facts . . .
> They lie unquestioned, uncombined.
> Wisdom enough to leach us of our ill is daily spun;
> But there exists no loom to weave it into fabric . . .
>
> —Edna St. Vincent Millay (1892-1950)
> "Huntsman, What Quarry?"

> An immense and ever-increasing wealth of knowledge is scattered about the world today; knowledge that would probably suffice to solve all the mighty difficulties of our age, but it is dispersed and unorganised. We need a sort of clearing-house for the mind: a depot where knowledge and ideas are received, sorted, summarised, digested, clarified and compared.
>
> —H. G. Wells
> (quoted in *The Sunday Independent*, August 30, 1997)

Although it is widely agreed that science is cumulative, people have only very recently begun to acknowledge that scientists have a responsibility to cumulate scientifically. As this article has shown, there is scattered evidence that this has been acknowledged by some scientists for at least a century, but it was really only during the last quarter of the 20th century that the need to develop and apply methods to improve research synthesis became more widely recognized.

So far, most of the resulting activity has been directed at preparing stand-alone research syntheses. As Lord Rayleigh (1885) noted more than a century ago, however,

> The work which deserves, but I am afraid does not always receive, the most credit is that in which discovery and explanation go hand in hand, in which not only are new facts presented, but their relation to old ones is pointed out. (p. 20)

The digestion and assimilation of old material and the integration of new material with existing evidence are both essential elements of

TABLE 2
Classification of Discussion Sections in Randomized Controlled Trial Reports Published in May 1997 and May 2001 in Five Major General Medical Journals

Classification	*May 1997* (n = *26*)	*May 2001* (n = *33*)
First trial addressing the question	1	3
Contained an updated systematic review integrating the new results	2	0
Discussed a previous review but did not attempt to integrate the new results	4	3
No apparent systematic attempt to set the new results in the context of other trials	19	27

scientific endeavors, and this needs to be reflected in the methodological quality of the Discussion sections of reports of primary research. As the data in Table 2 show, even in papers published in five highly respected general medical journals, it remains very rare for the results of new controlled trials to be set in the context of systematic reviews of other, similar studies (Clarke, Alderson, & Chalmers, 2001; Clarke & Chalmers, 1998).

Some years ago, the editor of this journal suggested that a case could be made for calling for a moratorium on proposals for additional primary research until the results of existing research had been incorporated in scientifically defensible reviews (Bausell, 1993). Although he may have thought this a radical a proposition at the time, there is evidence that funders of research are beginning to take account of such views.

The future status of and investment in research synthesis thus seem more likely to be shaped by external pressures from the users of research information than by traditional attitudes within academia to this kind of work. Indeed, we predict that we are moving toward a time when the public will begin to ask increasingly penetrating questions about why it has taken academia so long to begin to practice the kind of scientific self-discipline for which Lord Rayleigh called in 1885.

More radically, the public may also begin to ask why researchers addressing similar or related questions do not collaborate effectively or make their raw data publicly available for others to exploit. The advantages of collaborative investigations using pooled raw data have been made abundantly clear by the global clinical trialists' collaborations in cancer and heart disease in particular (Advanced Ovarian

Cancer Trialists' Group, 1991; Antiplatelet Trialists' Collaboration, 1988; Early Breast Cancer Trialists' Collaborative Group, 1988). Physicists have led the way in making raw data publicly available in electronic form (Ginsparg, 1998). As Gene Glass (2001) noted, "Meta-analysis was created out of the need to extract useful information from the cryptic records of inferential data analyses in the abbreviated reports of research in journals and other printed sources" (p. 12). We agree with him that the future history of research synthesis should be based increasingly on the creation of publicly accessible archives of raw data.

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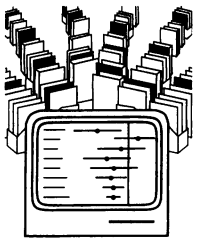

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Systematic literature reviews including meta-analyses are invaluable scientific activities. The rationale for such reviews is well established. Health care providers, researchers, and policy makers are inundated with unmanageable amounts of information; they need systematic reviews to efficiently integrate existing information and provide data for rational decision making. Systematic reviews establish whether scientific findings are consistent and can be generalised across populations, settings, and treatment variations, or whether findings vary significantly by particular subsets. Meta-analyses in particular can increase power and precision of estimates of treatment effects and exposure risks. Finally, explicit methods used in systematic reviews limit bias and, hopefully, will improve reliability and accuracy of conclusions.

Systematic literature review is a fundamental scientific activity. Its rationale is grounded firmly in several premises. Firstly, large quantities of information must be reduced into palatable pieces for digestion. Over two million articles are published annually in the biomedical literature in over 20 000 journals[1]—literally a small mountain of information. For example, about 4400 pages were devoted to approximately 1100 articles in the *BMJ* and *New England Journal of Medicine*, combined, in 1992. In a stack, two million such articles would rise 500 m. Clearly, systematic literature review is needed to refine these unmanageable amounts of information. Through critical exploration, evaluation, and synthesis the systematic review separates the insignificant, unsound, or redundant deadwood in the medical literature from the salient and critical studies that are worthy of reflection.[2]

Secondly, various decision makers need to integrate the critical pieces of available biomedical information. Systematic reviews are used by more specialised integrators, such as economic and decision analysts, to estimate the variables and outcomes that are included in their evaluations. Both systematic and more specialised integrations are used by clinicians to keep abreast of the primary literature in a given field as well as to remain literate in broader aspects of medicine.[3,4] Researchers use the review to identify, justify, and refine hypotheses; recognise and avoid pitfalls of previous work; estimate sample sizes; and delineate important ancillary or adverse effects and covariates that warrant consideration in future studies. Finally, health policy makers use systematic reviews to formulate guidelines and legislation concerning the use of certain diagnostic tests and treatment strategies.

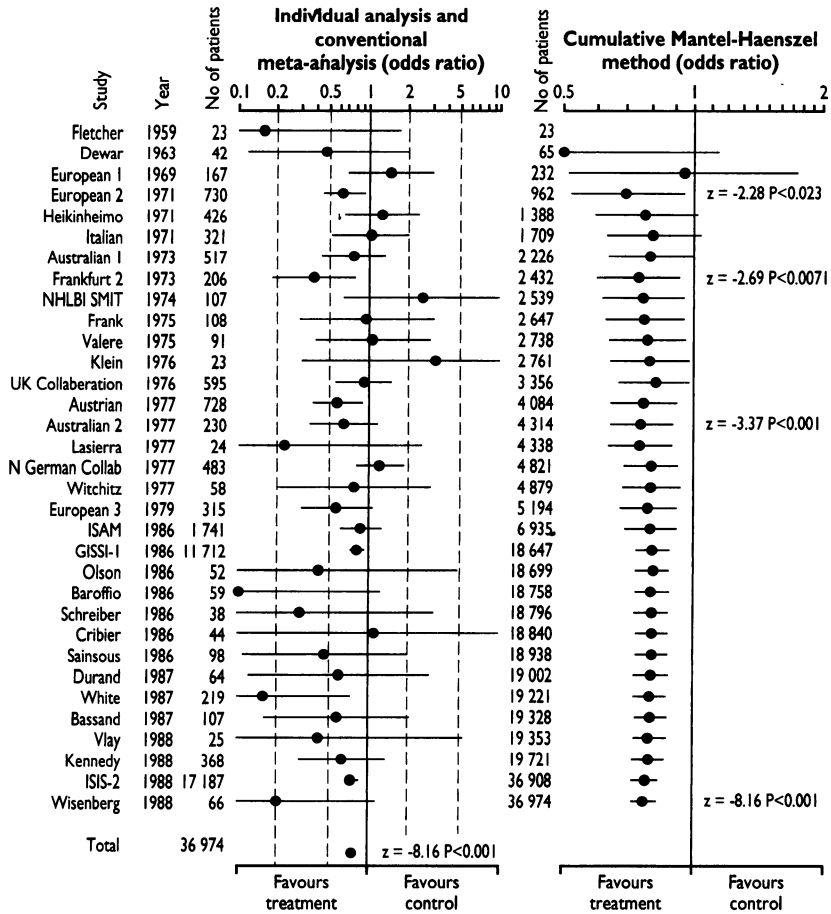

FIG 1—*Conventional and cumulative meta-analysis of 33 trials of intravenous streptokinase for acute myocardial infarction. Odds ratios and 95% confidence intervals for effect of treatment on mortality are shown on a logarithmic scale*

An efficient scientific technique

Thirdly, the systematic review is an efficient scientific technique. Although sometimes arduous and time consuming, a review is usually quicker and less costly than embarking on a new study. Just as important, a review can prevent meandering down an already explored path. Continuously updated literature review, as exemplified by the Oxford Database of Perinatal Trials, can shorten the time between medical research discoveries and clinical implementation of effective diagnostic or treatment strategies.[5] A landmark example of cumulative meta-analyses and its benefits is shown in figure 1, which gives odds ratios and 95% confidence intervals for 33 trials that compared intravenous streptokinase with a placebo or no therapy in patients who had been hospitalised for acute myocardial infarction. The left side of the figure shows that the effect of treatment with streptokinase on mortality was favourable in 25 of the 33 trials, but in only six was statistical significance achieved. The overall pooled estimate of treatment effect given at the bottom significantly favoured treatment. The right side of the figure shows the same data presented as if a new or cumulative meta-analysis was performed each time the results of a new trial were reported. The years during which the treatment effect became statistically significant were 1971 for a two sided P value of $<0{\cdot}05$, 1973 for a P value of $<0{\cdot}01$, and 1977 for a P value of $<0{\cdot}001$. This cumulative type of review indicated that intravenous streptokinase could have been shown to be life saving almost 20 years ago, long before its submission to and approval by the United States Food and Drug Administration and its general adoption in practice.

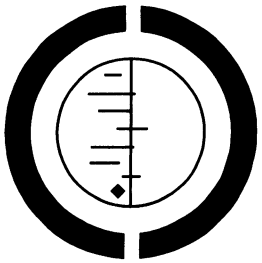

FIG 2—*The Cochrane Collaboration logo shows how pooling data reveals the significance of treatment effects*

Generalisability, consistency—and inconsistency

Fourthly, the generalisability of scientific findings can be established in systematic reviews. The diversity of multiple reviewed studies provides an interpretive context not available in any one study.[7] This is because studies addressing similar questions often use different eligibility criteria for participants, different definitions of disease, different methods of measuring or defining exposure, different variations of a treatment, and different study designs.[8]

Closely related to generalisability, a fifth reason for systematic reviews is to assess the consistency of relationships. Assessments of whether effects are in the same directions and of the same general magnitudes, given the variance in study protocols, can be made. More specifically, systematic reviews can determine consistency among studies of the same intervention or even among studies of different interventions (for example, varying doses or intensities or classes of therapeutic agents).[9] Consistency of treatment effects across different diseases with common underlying pathophysiology and consistency of risk factors across study populations can be ascertained.

Conversely, a sixth reason for systematic reviews is to explain data inconsistencies and conflicts in data. Whether a treatment strategy is effective in one setting and not another or among certain subjects and not others can be assessed. Furthermore, whether findings from a single study stand alone for any reason such as uniqueness of study population, study quality, or outcome measure can be explored.

Power and precision

Seventhly, an often cited advantage of quantitative systematic reviews in particular is increased power. Quantitative reviews or meta-analysis have been likened to "a tower of statistical power that allows researchers to rise above the body of evidence, survey the landscape, and map out future directions."[10] An example of meta-analysis improving statistical power is shown in the Cochrane Collaboration's logo (fig 2), which depicts effect sizes of seven trials that evaluated the effects of a short course of corticosteroids given to women expected to give birth prematurely. Only two trials had clear cut, statistically significant effects, but when data from all of the studies were pooled the "sample size" and thus power increased, yielding a definitive significant combined effect size that indicated strongly that corticosteroids reduce the risk of babies dying from complications of immaturity. The advantage of increasing power is particularly relevant to conditions of relatively low event rates or when small effects are being assessed.

Eighthly, quantitative systematic reviews allow increased precision in estimates of risk or effect size. On the right side of figure 1 the cumulative meta-analysis shows that increasing sample size from temporally consecutive studies resulted in continued narrowing of confidence intervals even though efficacy had been established in the early 1970s.[6] Particularly noteworthy, two very large trials—the 1986 study of the Gruppo Italiano per lo Studio della Streptochinasi nell'Infarto Miocardico (GISSI) involving 11 712 subjects and the 1988 second international study of infarct survival (ISIS-2) involving 17 187 subjects—did not change the already established evidence of efficacy, though they increased precision by narrowing the confidence intervals slightly.

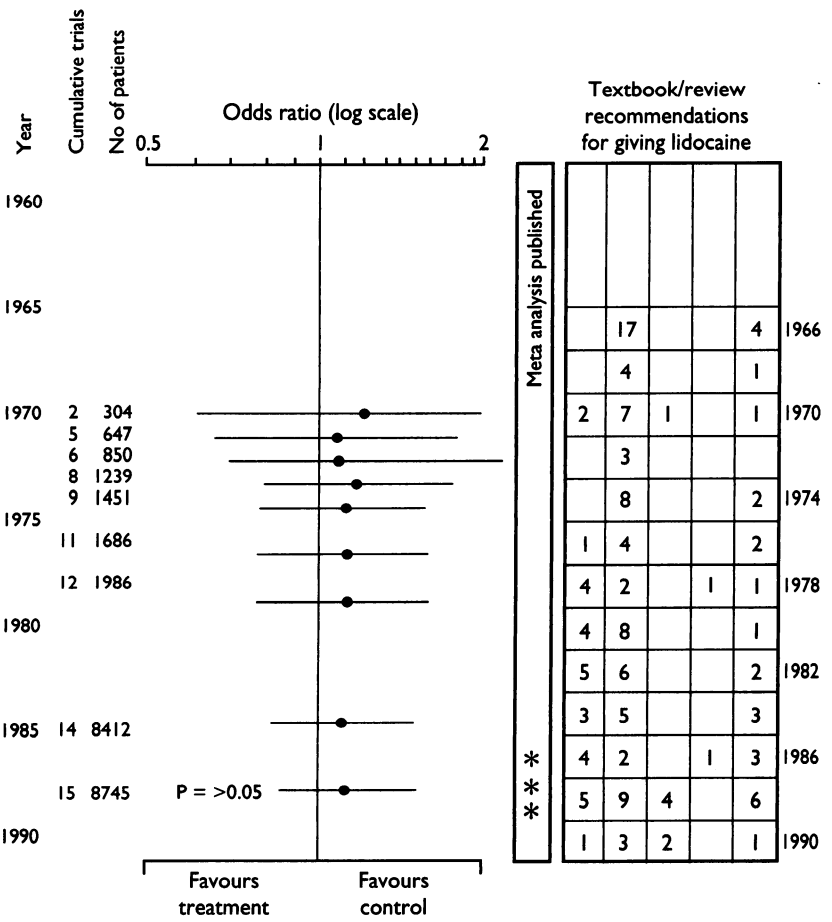

FIG 3—*Cumulative meta-analysis by year of publication or randomised controlled trials of prophylactic lidocaine for acute myocardial infarction, and recommendations of clinical expert reviewers (adapted from Antman et al[14])*

Accurate assessment

A final rationale for systematic reviews is accuracy, or at least an improved reflection of reality. Traditional reviews have been criticised as haphazard and biased, subject to the idiosyncratic impressions of the individual reviewer.[11] Systematic reviews and meta-analyses apply explicit scientific principles aimed at reducing random and systematic errors of bias.[12] But whether such reviews will lead to greater reliability, and by inference greater accuracy, is not yet established clearly.[8]

At the very least, the use of explicit methods allows assessment of what was done and thus increases the ability to replicate results or understanding of why results and conclusions of some reviews differ. In addition, reviewers using traditional methods are less likely to detect small but significant effects than are reviewers using formal systematic and statistical techniques.[13] Finally, traditional review recommendations lag behind and sometimes vary significantly from continuously updated or cumulative meta-analyses.[14] Figure 3 shows that pooled data from 15 randomised trials published before 1990 found no mortality benefit associated with prophylactic lidocaine for acute myocardial infarction. Despite this evidence, most pertinent traditional reviews continued to recommend prophylactic lidocaine. Antman *et al* have shown also that many effective treatments for reducing mortality due to acute myocardial infarction, such as intravenous magnesium, are not being recommended as often as they might be.[6 14]

Summary

There are a myriad of reasons to herald systematic literature reviews including meta-analyses. The hundreds of hours spent conducting a scientific study ultimately contribute only a piece of an enormous puzzle. The value of any single study is derived from how it fits with and expands previous work, as well as from the study's intrinsic properties.[15] Through

systematic review the puzzle's intricacies may be disentangled.

The vast amount of available information underscores the value of systematic reviews. As T S Eliot asked in his poem "The Rock," "Where is the knowledge we have lost in information?" Moreover, decision makers of various types are inundated with unmanageable amounts of information. They have great need for systematic reviews that separate the known from the unknown and that save them from the position of knowing less than has been proved.[16]

Advantages of the systematic review are many. Whether scientific findings are consistent and can be generalised across populations, settings, and treatment variations or whether findings vary significantly by particular subsets can be gleaned. Unique advantages of quantitative systematic reviews or meta-analyses are increased power and precision in estimating effects and risks. Hopefully, both qualitative and quantitative systematic reviews, with their explicit methods, will limit bias and improve the reliability and accuracy of recommendations.

I thank Dr Rosalva M Solis for her assistance in the preparation of this article.

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Lesson of the Week

Cystitis and ureteric obstruction in patients taking tiaprofenic acid

Frederick G Mayall, Robert W Blewitt, William G Staff

Take a full drug history, asking particularly about recent treatment with tiaprofenic acid, in any patient presenting with unexplained chronic cystitis

Three cases of cystitis associated with tiaprofenic acid, a non-steroidal anti-inflammatory drug, have been reported.[1] These patients recovered once the drug had been stopped, and none came to any permanent harm. We have encountered eight additional cases. Several of these patients had severe disease, which in one case was life threatening.

Case reports

CASE 1

A 69 year old woman presented with intolerably painful frequency and sterile haematuria. She had a long history of arthritis and had taken tiaprofenic acid for about two years. Intravenous urography showed normal upper tracts but she had a small contracted bladder with reddened friable mucosa on cystoscopy. Two months later she developed renal failure (blood urea 39 mmol/l, creatinine 236 μmol/l). An ultrasound examination showed bilateral ureteric obstruction and severe hydronephrosis. At surgery both ureters were thickened over their entire length and she had a cystectomy and ureteric diversion into an ileal conduit. The resected bladder was contracted and had a thickened wall. Much of the epithelial surface was ulcerated and replaced by granulation tissue.

Histological examination showed a dense chronic inflammatory infiltrate in the lamina propria with prominent eosinophils. This extended into the epithelium with associated spongiosis and into the muscle of the bladder wall with associated fibrosis. Similar changes were seen in the ureteric off cuts, causing marked luminal stenosis (figure).

After the operation she stopped taking tiaprofenic acid and her renal function rapidly returned to normal. Several months later she developed haematuria and her renal function deteriorated. She had started taking tiaprofenic acid again. She stopped the drug and her impaired renal function and haematuria resolved.

CASE 2

A 65 year old woman presented with frequency and nocturia which had become increasingly severe over a year. She had been taking tiaprofenic acid for more than two years for arthritis. Intravenous urography showed dilated upper tracts. Cystoscopy showed a small bladder with a friable "cobblestone" mucosa. A bladder biopsy showed prominent mucosal oedema and a moderate chronic inflammatory infiltrate in the lamina propria with frequent eosinophils. The epithelium showed glandular metaplasia. Her symptoms continued despite stopping the tiaprofenic acid, and a month later a cystectomy and a ureteric diversion into an ileal conduit were performed. Virtually all of the epithelium showed glandular metaplasia, and eosinophils were abundant. There was also extension of the inflammation and fibrosis into the muscle of the bladder wall and into both ureters causing severe luminal stenosis.

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Dr Mayall.

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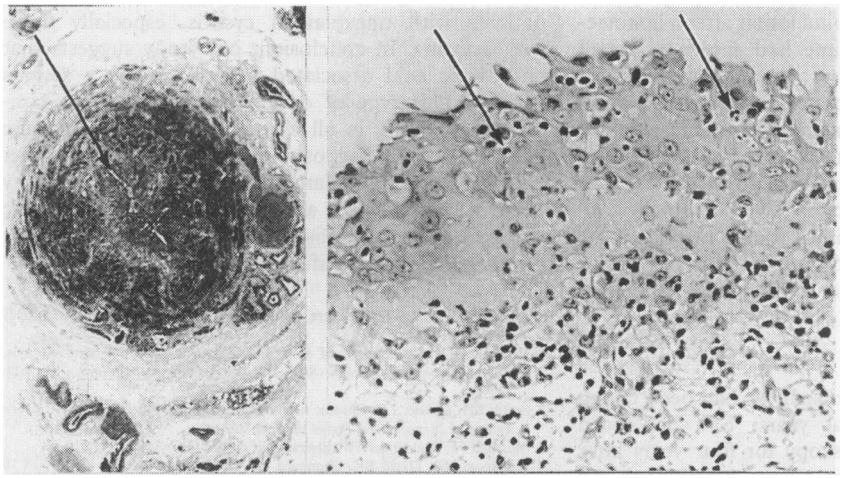

Left: off cut of ureter from case 1 showing chronic inflammation reducing the lumen (arrowed) to a slit (haematoxylin and eosin stain). Right: bladder mucosa from case 1 showing chronic inflammation, epithelial spongiosis, and (arrowed) intraepithelial eosinophils recognised by their bilobar nuclei (haematoxylin and eosin stain).

RESEARCH

A Systematic Review of the Research Literature on the Use of Phonics in the Teaching of Reading and Spelling

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Jill Hall *

* University of York
** University of Sheffield

department for
education and skills
creating opportunity, releasing potential, achieving excellence

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** University of York*

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We are also grateful to the members of the Steering Group who supported and advised us at each stage of the review with helpful comments and suggestions.

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Glossary

Analytic phonics: A form of phonics teaching in which sounding-out is not used. Instead, teachers show children how to deduce the common letter and sound in a set of words which all begin (or, later, end) with the same letter and sound, e.g. *pet, park, push, pen*.

***Attrition:** Often participants are lost during a trial and cannot be included in the analysis. This is termed attrition or is sometimes known as mortality.

***Bias:** A term denoting that a known or unknown variable (rather than the intervention) is, or may be, responsible for an observed effect.

Cluster randomized trial: In trials using this form of design the unit of allocation includes more than one individual, e.g. class, school.

***Concealed allocation:** This is where the researchers, participants and teachers are prevented from knowing in advance the allocation of an individual, i.e. the allocation has been concealed from them.

***Confidence intervals:** These indicate the level of uncertainty surrounding an effect size. The point estimate of effect of any intervention will always be imprecise. The level of the imprecision is dependent upon the sample size and event rate in the treatment groups. The use of confidence intervals (usually 95%, but sometimes 99% or 90%) reflects this imprecision in the study results.

***CONSORT:** Consolidated Standards for Reporting Trials is the methodological standard adopted by many medical journals for publication of randomized controlled trials.

***Controlled trial (CT):** This usually means a study with a control group that has been formed by means other than randomization. Consequently the validity of the study using this design is potentially threatened by selection bias.

***Co-variates or confounders:** These are variables that are associated with outcome. Randomization is the only method that ensures that both known and unknown co-variates are equally distributed among treatment groups.

***Effect size:** When an outcome variable is measured on a continuous scale (e.g. changes in a test score) the improvement or decrement is described in standard deviation units, which is termed the effect size.

***Funnel plot:** A method of assessing whether there is any publication bias. The effect size of each study is plotted against its sample size. Small studies will have large random variations in their effect sizes, which will be scattered along the x-axis close to the bottom of the y-axis. Larger studies will be higher up on the y-axis and less scattered along the x-axis. A review with no publication bias will show a plot in the shape of an inverted funnel.

Heterogeneity: When studies have different characteristics, e.g. different populations or different outcome measures.

***ITT analysis: Intention to teach analysis:** This is where all participants are analysed in their original randomized groups; it is the most robust analytical method. Once participants have been allocated to their respective groups it is important that they remain in those groups for analysis, to avoid bias. A common, but incorrect, method is to exclude some participants after randomization for a variety of reasons. One approach is to do what is termed 'an on-treatment analysis' – this is where only those participants who demonstrate treatment fidelity are included in the analysis. Unfortunately, this can lead to bias, as those participants who complete treatment are likely to be different from those who do not. Intervention-received analysis can therefore produce a biased result.

Intra-cluster correlation (ICC): The statistical correlation between members of the same group (e.g., pupils in the same class).

Meta-analysis: A meta-analysis is a method of combining the results of two or more RCTs statistically.

Meta-analysis: fixed effects model: The fixed effects model of meta-analysis assumes that the variability is exclusively because of random sampling variation around a fixed effect.

Meta-analysis: random effects model: The random effects model of meta-analysis assumes a different underlying effect for each study, and takes this into consideration.

Normal quantile plot: A graphical method of assessing possible publication bias (as well as assessing whether the observed data fall within a normal distribution and whether or not the studies come from a single population). The observed effect sizes are plotted against the effect sizes in a normal distribution. If there is an indication of publication bias there will be a gap in the curve around zero.

Onset-rime: A form of phonics teaching in which sounding-out is not applied (at least not in the early stages) to every letter but just to the initial consonant, and then the remainder of the word as a unit, for example 'kuh – at' – 'cat'.

Phonics instruction: A set of approaches to the initial teaching of reading and writing which focus on the relationships between letters and sounds.

***Publication bias:** Not all RCTs are published. There is a well-established tendency for trials that produce negative or null effects to be less likely to be published than positive trials. Unless a systematic review includes these negative trials it can give a misleading optimistic assessment of the intervention. Existence of publication bias can be detected by using funnel plots.

***Randomized Controlled Trial (RCT):** This is where two or more groups have been formed through random allocation (or a similar method). This is the only method that ensures that selection bias is eliminated at baseline.

***Regression to the mean:** This statistical phenomenon occurs when test results are, by chance, some distance away from the mean. Consequently at post-testing the 'extreme' results will tend to regress to the mean. When selecting participants on extreme test results (e.g. very poor pre-tests) there will be an apparent dramatic improvement on post-test because of this effect (irrespective of the teaching method). Randomization automatically controls for

regression to the mean effects. Nevertheless, it can still have an influence if the groups are unbalanced at baseline on pre-test scores. This imbalance can be adjusted for by a multivariate analysis.

***Sample size calculations:** Trials in educational research commonly exhibit a Type II error. This is where the sample size is insufficient to show, as statistically significant, a difference that is educationally important. Reviews of educational interventions have shown that most interventions will, at best, only lead to an improvement in the region of half a standard deviation and quite often somewhat less. Statistical theory shows that to reliably detect (with 80% power) half a standard deviation difference as statistically significant ($p = 0.05$) for a normally distributed variable requires a minimum sample size of 126 participants between the two arms. Studies that are smaller than this risk erroneously concluding that there was not a significant difference when actually there was. Therefore, a good quality study ought to describe the reasoning behind the choice of sample size.

***Standard deviation:** is a measure of spread or dispersion of continuous data. A high standard deviation implies that the values are widely scattered relative to the mean value, whilst a small value implies the converse.

Synthetic phonics: A form of phonics teaching in which sounding-out is used. For reading, this is based on the letters in printed words and is followed by blending their sounds to produce a spoken word which the learner should recognise. The classic example is 'kuh – a – tuh' – 'cat'. For writing, sounding-out is based on a spoken word which the learner knows and is followed by writing the corresponding letter for each sound.

Systematic review: A review where explicit methods have been used to identify, select and include studies fitting pre-specified criteria.

* Definitions reproduced from Torgerson (2003, pp.vii-x)

EXECUTIVE SUMMARY

Introduction

The Department for Education and Skills (D*f*ES) commissioned the Universities of York and Sheffield to conduct a systematic review of experimental research on the use of phonics instruction in the teaching of reading and spelling. This review is based on evidence from randomised controlled trials (RCTs).

Key findings

The effect of phonics on reading:

- Systematic phonics instruction within a broad literacy curriculum was found to have a statistically significant positive effect on reading *accuracy*.
- There was no statistically significant difference between the effectiveness of systematic phonics instruction for reading *accuracy* for normally-developing children and for children at risk of reading failure.
- The weight of evidence for both these findings was moderate (there were 12 randomized controlled trials included in the analysis).
- Both of these findings provided some support for those of a systematic review published in the United States in 2001 (Ehri *et al*., 2001).
- An analysis of the effect of systematic phonics instruction on reading *comprehension* was based on weak weight of evidence (only four randomized controlled trials were found) and failed to find the statistically significant positive difference which was found in the previous review.

The effect of synthetic and analytic phonics (see definitions below):

- The weight of evidence on this question was weak (only three randomized controlled trials were found). No statistically significant difference in effectiveness was found between synthetic phonics instruction and analytic phonics instruction.

The effect of phonics on spelling:

- The weight of evidence on this question was weak (only three randomized controlled trials were found). No effect of systematic phonics instruction on spelling was found.

Definitions

Phonics instruction: Literacy teaching approaches which focus on the relationships between letters and sounds.
Synthetic phonics: The defining characteristics of synthetic phonics for reading are sounding-out and blending.
Analytic phonics: The defining characteristics of analytic phonics are avoiding sounding-out, and inferring sound-symbol relationships from sets of words which share a letter and sound, e.g. *pet, park, push, pen.*
Systematic phonics: Teaching of letter-sound relationships in an explicit, organised and sequenced fashion, as opposed to incidentally or on a 'when-needed' basis. May refer to systematic synthetic or systematic analytic phonics.

Aims of the review

The review investigated how effective different approaches to the initial teaching of reading and spelling are in comparison to each other. The review questions were:

How effective are different approaches to phonics teaching in comparison to each other (including the specific area of analytic versus synthetic phonics)?

How do different approaches impact on the application of phonics in reading and writing, including beyond the early years?

Is there a need to differentiate by phonics for reading and phonics for spelling?

What proportion of literacy teaching should be based on the use of phonics?

Background

Phonics teaching is a much debated area of literacy teaching. The National Literacy Strategy (NLS) (D*f*EE, 1998) recommended a mixed approach that included an element of phonics instruction, but it has been argued that such an approach might lead to confusion among young children, and that phonics should be the predominant method of word identification they are taught. However, there is disagreement as to which method of phonics teaching is most effective.

A method of resolving uncertainty between different approaches to teaching is to conduct a randomized controlled trial (RCT). An RCT is where two or more groups of children are formed randomly and each group receives a different form of instruction. If one group makes significantly better progress it can be inferred that the form of teaching they received was more effective, because all other factors which might influence the outcome are controlled for (with the exception of chance).

Methods

Systematic review methods were used throughout this review. That is, as far as possible all relevant RCTs were identified and included. Non-systematic reviews may give misleading results if it is not clear why some studies were included and others were not, and may be subject to reviewer bias. The only two previous systematic reviews in this field were published in the United States (Ehri *et al*., 2001; Camilli *et al*., 2003). The present review updated the previous reviews, broadened the sources of information which were searched, and adopted more rigorous criteria for identifying relevant studies.

The studies included were RCTs which focused on the use of phonics instruction in English, in order to ensure a fair comparison between the effectiveness of systematic phonics and of alternative approaches to reading instruction. Data were extracted from each included RCT and put into a meta-analysis[1].

[1] A meta-analysis is a method of combining the results of two or more RCTs statistically. In educational research the method is particularly helpful as many educational RCTs are too small to identify possibly

Findings

The review identified a total of 20 RCTs, of which only one was UK-based (Johnston and Watson, 2004, experiment 2). All were concerned with the initial teaching of reading (and, in a few cases, spelling); the children studied were mostly between five and seven years of age, but four of the trials included children up to age 11.

The current review found that systematic phonics teaching was associated with better progress in reading *accuracy*. This effect was seen across all ability levels. However, the weight of evidence (from RCTs) on reading *comprehension* was weak, and no significant effect was found for reading comprehension.

The review found no evidence for the superiority of either synthetic or analytic phonics instruction over the other – but there were only three small RCTs on which to base this comparison. Similarly, phonics instruction did not appear to affect progress in spelling, but again there were only three relevant RCTs. Therefore, this does not provide strong evidence for or against the use of phonics in the teaching of spelling.

It was not possible to analyse how different approaches impacted on the application of phonics in reading and writing beyond the early years because only three RCTs used follow-up measures.

Conclusions

Systematic phonics instruction within a broad literacy curriculum appears to have a greater effect on children's progress in reading than whole language or whole word approaches. The effect size is moderate but still important. However, there is still uncertainty in the RCT evidence as to which phonics approach (synthetic or analytic) is most effective.

Recommendations

For teaching

- Systematic phonics instruction should be part of every literacy teacher's repertoire and a routine part of literacy teaching.
- Teachers who already use systematic phonics in their teaching should continue to do so.
- Teachers who do not use systematic phonics in their teaching should add it to their routine practices.
- Systematic phonics should be used with both normally developing children and those at risk of failure.

However,

- There is currently no strong RCT evidence that any one form of systematic phonics is more effective than any other.
- There is also currently no strong RCT evidence on how much systematic phonics is needed.

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significant differences between groups. By combining several small studies it is possible to identify moderate but important effects.

- Two other areas on which the existing research base is insufficient are whether or not phonics teaching boosts comprehension, and whether phonics should be used to teach spelling as well as reading.

For teacher training

- The evidence that systematic phonics teaching benefits children's reading accuracy further implies that learning to use systematic phonics in a judicious balance with other elements should form part of every literacy teacher's training.

For research

- A large UK-based cluster-randomized controlled trial would enable further investigation of the relative effectiveness of systematic synthetic versus systematic analytic phonics instruction with children with different learning characteristics.

1. Introduction

How best can children be enabled to learn to read and write? Views on this perennial and important question differ, and disagreements are sometimes passionate, especially over the place in the reading curriculum of phonics, that is, approaches which focus on the relationships between letters and sounds.

In early 2005, the Department for Education and Skills (D*f*ES) commissioned the Universities of York and Sheffield to conduct a systematic review of experimental research on the use of phonics instruction in the teaching of reading and spelling. It builds on a systematic review conducted in the United States by the National Reading Panel's phonics subgroup (Ehri *et al*., 2001), which concluded (p.393) that 'systematic phonics instruction helped children learn to read better than all forms of control group instruction'. An updating of that review was especially relevant in 2005 because of the publication of the first relevant British experiment (Johnston and Watson, 2004, experiment 2). This review is based on evidence from randomised controlled trials (RCTs).

2. Objectives: The review questions

The main research question for the review was:

> **How effective are different approaches to phonics teaching in comparison to each other (including the specific area of analytic versus synthetic phonics)?**

Supplementary questions were:

> **How do different approaches impact on the application of phonics in reading and writing, including beyond the early years?**
>
> **What proportion of literacy teaching should be based on the use of phonics?**
>
> **Is there a need to differentiate by phonics for reading and phonics for spelling?**

3. Definitions

An overall definition of phonics as 'approaches which focus on the relationships between letters and sounds' appears to be generally accepted. However, definitions of synthetic and analytic phonics are varied and contested. Therefore, the authors of this review adopted the rigorous definitions outlined by Brooks (2003). Since these incorporate two technical terms, those terms are defined first:

> ***Phoneme***: A distinctive speech sound, that is, one which makes a difference to the meaning of a word. For example, the initial phonemes in *bat, pat* are /b, p/.
> ***Grapheme***: A letter or combination of letters used to spell a phoneme, for example the letters <p, sh> spelling the phonemes /p, ʃ/ in *push*.

Brooks' definitions (Brooks, 2003, pp.11-12), which were in turn based on those of Strickland (1998, p.31), were as follows:

> ***Synthetic phonics*** refers to an approach to the teaching of reading in which the phonemes associated with particular graphemes are pronounced in isolation and blended together (synthesized). For example, children are taught to take a single-syllable word such as *cat* apart into its three letters, pronounce a phoneme for each letter in turn /k, æ, t/, and blend the phonemes together to form a word. Synthetic phonics for writing reverses the sequence: children are taught to say the word they wish to write, segment it into its phonemes and say them in turn, for example /d, ɒ, g/, and write a grapheme for each phoneme in turn to produce the written word, *dog*.
>
> ***Analytic phonics*** refers to an approach to the teaching of reading in which the phonemes associated with particular graphemes are not pronounced in isolation. Children identify (analyse) the common phoneme in a set of words in which each word contains the phoneme under study. For example, teacher and pupils discuss how the following words are alike: *pat, park, push* and *pen*. Analytic phonics for writing similarly relies on inferential learning: realising that the initial phoneme in /pɪg/ is the same as that in /pæt, pɑːk, pʊʃ/ and /pen/, children deduce that they must write that phoneme with grapheme <p>.

The definitions of synthetic and analytic phonics for **reading** used by the US National Reading Panel (see Ehri *et al.*, 2001, p.395) were essentially equivalent to the relevant parts of those just given:

> Synthetic phonics programs use a part-to-whole approach that teaches children to convert letters into phonemes (e.g., to pronounce each letter in *stop*, /s/-/t/-/a/-/p/) [N.B. The correspondence of letter <o> to phoneme /*a*/ is correct for many US accents] and then to blend the phonemes into a recognizable word. Analytic phonics uses a whole-to-part approach that avoids having children pronounce sounds in isolation to figure out words. Rather children are taught to analyze letter-sound relations once the word is identified. For example, a teacher might write the letter P followed by several words, *put, pig, play, pet*. She would help students read the words and recognize that they all begin with the same sound that is associated with the letter P.

Brooks' definitions have been adopted here because they focus on the defining and distinctive characteristics of each approach. They were applied to all the studies included in the analyses in this review. Other definitions of synthetic phonics in particular incorporate several other features; see Appendix A for a discussion of these, including why they were not adopted.

4. Previous research (1): Narrative reviews

The first significant research-based contribution on the role of phonics in initial instruction was Jeanne Chall's *Learning to Read: The Great Debate* (1967). The existing research literature was reviewed comprehensively, and a series of observations were conducted in classrooms, including some in Britain. The research concluded that, across many studies and learners:

- phonics enables children to make faster progress than no phonics;
- code-emphasis approaches (i.e. phonics and onset-rime[2]) enable children to make faster progress than meaning-emphasis approaches (i.e. whole-word and whole-language approaches, including look-and-say);
- phonics plus an emphasis on the meaningfulness of the texts being read enables children to make faster progress than phonics alone; and
- synthetic phonics enables children to make faster progress than analytic phonics.

Similar conclusions were reached by Bond and Dykstra (1967) and Pflaum, Walberg, Karegianes and Rasher (1980). During an update, Chall (1989) saw no reason to alter her conclusions, and indeed considered them strengthened by additional evidence that had accumulated. Chall's work and the other reviews mentioned above were cited by Marilyn Jager Adams (1990) in the second significant book on the question, which reached the same conclusions.

However, all the studies mentioned above were narrative reviews. Narrative reviews rely very heavily on the reviewers' judgments and are liable to be influenced by their preconceived ideas. One of the strong motives for the growing interest in and popularity of systematic reviews is precisely that they offer a less subjective and more methodical way of arriving at conclusions. A systematic review is a review where explicit methods have been used to identify, select and include studies fitting pre-specified criteria, in order to minimise bias in the review.

[2] For a definition of onset-rime, see the Glossary

5. Previous research (2): The Ehri et al. (2001) systematic review and the Camilli et al. (2003) replication

Ehri *et al.* (2001) was one of two systematic reviews of this field undertaken prior to this review (the second was the Camilli *et al*., 2003 replication, discussed below). The aim of the Ehri *et al*. (2001) review was to search for, retrieve and synthesize the experimental research base since 1970 for evidence of the relative effectiveness of systematic phonics instruction, unsystematic phonics instruction, and reading instruction without a phonics element. Another aim was to assess the evidence for differential effects depending on different characteristics of learners, for example age or grade level, and attainment level (normally-attaining children or those experiencing difficulties or disabilities in learning). Ehri *et al.* (2001) found 38 studies which met their inclusion criteria, and used the results of those studies in a meta-analysis[3]. The meta-analysis found an overall statistically significant positive effect size for phonics instruction on reading of 0.41[4]. This effect size was reasonably strong, and would mean 16 more children out of 100 would succeed on a standardized reading accuracy test with a mean of 50% than children who did not receive systematic phonics – see Torgerson, 2003, p.86.) Ehri *et al.* concluded:

> Systematic phonics instruction helped children learn to read better than all forms of control group instruction, including whole language. In sum, systematic phonics instruction proved effective and should be implemented as part of literacy programs to teach beginning reading as well as to prevent and remediate reading difficulties. (Ehri *et al.*, 2001, p.393).

Of Chall's four conclusions mentioned above, Ehri *et al.* (2001) provided renewed support only for one, which can be restated as:

- systematic phonics instruction (of whatever variety) enables children to make faster progress than unsystematic or no phonics

[3] This is a statistical method for combining the results of several studies so that any finding is based on a larger sample and should be more robust than if derived from just a few studies.
[4] Effect sizes are a way of indicating the impact of an intervention that is independent of the particular test or other measure used, and they can therefore be used to compare different interventions.

where 'systematic' implies an organised and structured teaching programme rather than an approach where phonics is introduced incidentally and occasionally. Their analyses yielded no significant evidence to support Chall's other conclusions.

More recently, however, the original meta-analysis was replicated (Camilli *et al.*, 2003) using the same 38 studies as in the original analysis, plus an additional three with phonemic awareness outcomes, and minus one that did not have a 'no treatment' control group. In the re-analysis, data were extracted from the 40 studies with specific regard to the treatment characteristics, namely the 'degree' of phonics or 'mixture' of phonics with other literacy activities, and whether the different conditions received 'equal study' time (Camilli *et al.*, 2003, pp.8, 23). The researchers compared systematic phonics instruction with the full range of treatment controls.

They reported a reduced effect size of 0.24 (i.e. approximately 10 extra children out of 100 would succeed on a relevant test) for the comparison between 'systematic' and 'less systematic' phonics instruction, and concluded that 'the advantage of systematic phonics instruction over some phonics instruction is significant but cannot be clearly prioritized over other influences on reading skills' (Camilli *et al*., 2003, p.30). Using a statistical model called regression analysis they also showed that tutoring and whole language-based reading activities had similar effect sizes to systematic phonics instruction (effect sizes of 0.39 and 0.29 respectively; approximately 16 and 12 extra children respectively out of 100 succeeding on a relevant test). This re-analysis suggested that phonics instruction had value, but so did other teaching approaches, and that the research findings would not justify exclusive use of any one approach; indeed, a judicious balance incorporating phonics and other approaches might be justified.

Methodological limitations of the Ehri et al. (2001) review

In terms of standard, rigorous systematic review procedure (see Torgerson, 2003, and references given there), the Ehri *et al.* (2001) review had several limitations which the present review aimed to avoid:

- Its results may have suffered from the effects of publication bias because only trials published in peer-reviewed academic journals were included, and trials not so published were not. There is evidence (Torgerson, 2003, chapter 6) that experiments with negative

or null results are more likely to be rejected by the editors of academic journals. In the Ehri *et al.* (2001) review this may have resulted in an over-estimate of effect if any relevant 'unpublished' trials had such results. This is because if null or negative studies exist but remain unpublished, locating them and including them in a meta-analysis may reduce the overall effect. In this context, 'published' has the special meaning 'published in a peer-reviewed academic journal', so 'unpublished' trials in the corresponding sense are not necessarily inaccessible – they may exist as conference papers or dissertations, for example, and therefore appear in databases – and should be searched for and included if relevant and of sufficient quality; the present reviewers did this. For further detail on publication bias see Appendix C.

- The Ehri *et al.* (2001) review included both randomized and non-randomized controlled trials. The problem with this approach, and the reasons for using only randomized trials in this review, are given in section 7.
- In the Ehri *et al.* review a total of 66 comparisons from the 38 trials were included. Given that each trial had only one control group, this means that the children in many of the control groups were counted more than once. Double- (and in one case quadruple-) counting of the control groups in comparisons to calculate effect sizes would therefore have had the effect of artificially increasing the sample size by counting the control group sample twice and, in turn, spuriously increasing the precision of the estimated effect. Ehri *et al.* (2001, p.340) acknowledged that their effect sizes were 'not completely independent across comparisons'. This means that their findings may be an overestimate of the 'true' effect. In the current review each control group was counted only once.
- In Ehri et al.'s review (2001), there was some indication of heterogeneity (dissimilarity in samples and/or teaching approaches) between studies. In the current review this issue is explored in the analysis section.
- Although Ehri *et al.* (2001) examined the methodological quality of the included trials, they did not investigate this in a systematic way. The current review includes an appraisal of the quality of individual trials.
- Finally, Ehri *et al.* (2001) only included trials comparing a phonics intervention with a no-phonics or unsystematic intervention, and in particular did not compare synthetic and analytic phonics approaches. They excluded five controlled and randomized controlled trials which evaluated the relative effectiveness of synthetic versus analytic phonics instruction. The current review includes an analysis of this topic.

6. The British context

The history of phonics over the same period in Britain has in general paralleled the US debates[5], though with less intensity and polarization of opinions, and with fewer relevant empirical research studies.

Attention to phonics

The various editions of the National Curriculum for English in England had little to say about phonics, but the National Literacy Strategy (NLS) *Framework for Teaching* (D*f*EE, 1998) included it as one of its 'searchlights' (strategies for identifying words and comprehending text).

Closer attention to phonics in England can be dated from a seminar on phonics instruction held by the Office for Standards in Education (Ofsted) in London in March 1999 which brought together many of the key authorities. The UK Reading Association (now the UK Literacy Association) held two conferences on the topic in 1999 and 2000 (Cook, 2002). In its report on the first four years of the NLS, Ofsted (2002) praised some aspects of the teaching of phonics in primary schools in England but criticized others. In response, the Standards and Effectiveness Unit (SEU) of the D*f*ES undertook a consultative process in early 2003 addressing the question: *To what extent, and in what ways, does the phonics element of the National Literacy Strategy need modifying?* As part of the process a one-day expert conference on phonics was held in London on 17 March 2003, and the process as a whole resulted in a report (Brooks, 2003). The report recommended some revisions to the phonics element of the NLS and stated the need for some focused research.

Trends in attainment, and doubts about them

Levels attained in the Key Stage 2 (age 11) tests in England rose steadily in the period 1995-2000, then plateaued until 2003, then rose again in 2004 and 2005[6]. The Reading Reform Foundation (RRF), however, maintained that the rise in Key Stage 2 attainment concealed large numbers of children who were still not achieving adequate literacy levels, that the version of phonics in the NLS was not 'truly' synthetic or was actually analytic, and that the

[5] See section 4: Previous research (1): narrative reviews.
[6] For a convenient graph for 1995-2003 see Tymms (2004); for 2000-05 see DfES (2005).

searchlights model continued to legitimize lack of attention to phonics relative to, for example, working words out from context or illustrations (see Chew, 2005 and the RRF Newsletter, *passim*). The RRF advocates 'synthetic phonics first, fast and only', and this review was partly designed to investigate whether research supports exclusive use of synthetic phonics (or any other variety) at a rapid pace from children's entry into school. The RRF's policy and (largely) the dearth of research supporting it are discussed in detail in Appendix B.

Recent British research

In 2004, two British studies were published which used experimental methods to investigate the relative effectiveness of different varieties of systematic phonics instruction (Johnston and Watson, 2004; Hatcher *et al.*, 2004).

The Clackmannanshire research (Johnston and Watson, 2004) contained two studies. Experiment 1 was conducted in 1997-2004, but only the first four years were reported in the article cited. The study compared a synthetic phonics group with two analytic phonics groups (one of which also received phonemic awareness training designed to help children distinguish phonemes[7] in spoken words), and found an advantage for the synthetic phonics group – but this group had received teaching at a faster pace than the others, which meant the comparison was not entirely valid. Experiment 2 (conducted in 1995-96) equalized this variable by teaching all classes at the same pace; here the three groups were synthetic phonics, analytic phonics and no phonics. An advantage was found for the synthetic phonics group in this study too.

In Experiment 1 the groups (whole classes) were allocated to conditions by the researchers on the basis of indices of deprivation: they allocated the classes which were, on average, most 'deprived' to the synthetic phonics condition, in order to make it more difficult for any advantage this group showed to be attributed to their having had a head start. However, this may imply that part of the greater progress of the synthetic phonics group was due to regression to the mean. This is a statistical artefact whereby the lowest or highest scorers in a group on a first test occasion are likely to be nearer the overall mean of the group on the second testing, due to error of measurement effects. In the relevant Clackmannanshire study

[7] See Section 3: Definitions.

this was a design fault, since it means that part of the synthetic phonics group's greater progress was probably illusory.

The fact that in Experiment 1 the classes were allocated to conditions by the researchers also means that it was a controlled trial, not a randomized trial. As this review only included studies using the more robust design using randomization, Experiment 1 was excluded. However, in Experiment 2 children were allocated to conditions at random (Rhona Johnston, personal communication, April 2005), and that study was therefore an RCT and was included in the analyses in this review.

In the latest of their ongoing series of studies in Cumbria, Hatcher *et al.* (2004) investigated whether adding various extra phonic activities to a teaching sequence which already included synthetic phonics would benefit children relative to that teaching sequence alone. The teaching began when the children were aged four and a half on average, and lasted for five terms. The children were assessed with a battery of tests at the outset and at three points during the experiment. The study began with 524 children in 20 classes, one in each of 20 schools. The classes were allocated to one of four groups matched on pre-test scores, five classes per group, and the groups were then randomly allocated to one of three interventions or to the control. A total of 114 children were lost to the study for various reasons, so that data at the four time points were available for 410. Hatcher *et al.* reported some analyses for the whole of this sample, but mainly on two sub-samples: normally developing children (n = 273), and children at risk of reading failure (n = 137). The latter sub-sample was defined as 'the poorest third of children based upon the[ir] average [pre-test] scores p.340). The authors concluded:

> 'There were no selective effects of the different experimental teaching programmes for normally developing children. However, for those children identified as being at risk of reading failure, training in phoneme skills resulted in selective gains in phoneme awareness and in reading skills… A reading programme that contains a highly structured phonic component is sufficient for most 4.5-year-old children to master the alphabetic principle and to learn to read effectively, without additional explicit phonological training. In contrast, for young children at risk of reading delay, additional training in phoneme awareness and linking phonemes with letters is beneficial.' (p.338)

The Hatcher *et al*. (2004) study was not included in the analyses in this review because it compared different versions of synthetic phonics; hence it could not validly be analysed together with studies comparing systematic phonics (of whatever variety) with unsystematic or no phonics instruction.

Political attention

Recently the teaching of reading, and especially phonics, has attracted political attention. The House of Commons Select Committee on Education and Skills held an enquiry into teaching children to read in late 2004-early 2005; its report appeared in the Spring of 2005. Subsequently, the British Government announced (3 June 2005) the setting up of the Rose Review, which would concentrate on good practice in the teaching of reading, including good practice in the use of phonics, and report in early 2006; and (26 July 2005) a large set of phonics pilot projects to begin in the Autumn term of 2005.

The history of this area in the last few years has therefore been one in which phonics has become a topic of lively educational debate in Britain; hence the need for an independent and objective review of the research literature.

7. The use of randomized controlled trials in effectiveness research

The most robust method of assessing whether an intervention is effective or not is the randomized controlled trial (RCT). This is because, if participants are allocated on any other basis, one cannot be sure whether (except for chance differences) the experimental and control groups were similar before receiving or not receiving the intervention, and it therefore becomes impossible to disentangle the effects of the intervention from the characteristics of the people allocated to it. Techniques can be used to attempt to control for potential confounding from known variables, but they cannot adjust for unknown variables.

The two main reasons for using random allocation are to avoid regression to the mean effects[8] and to avoid selection bias. Forming comparison groups using random allocation deals with regression to the mean as it affects both groups equally and the effect is cancelled out. Selection bias occurs when the groups formed for comparison have not been created through random allocation and when the two groups formed are different in some way that can affect outcomes.

The Ehri *et al.* (2001) meta-analysis included 38 studies, of which only 13 were randomized controlled trials, and the other 25 were non-randomized controlled trials. The problem with including both types of trial in a meta-analysis is that pooling two study types can lead to a biased result, because non-randomized trials by definition cannot control for unknown sources of difference between groups. Whilst the apparent precision of the estimate may increase (i.e. small confidence intervals around the effect size), the estimate itself may be incorrect. For this reason, in the current review only randomized trials were included.

[8] For definition see Glossary

8. Methods

Systematic review methods as outlined in Torgerson (2003) were used throughout the conduct of the current review.

(This section includes only the details essential to understanding the way in which the results in the next section were obtained. For other technical matters, namely screening, quality assurance, data extraction, calculation of effect sizes, estimation of publication bias, and statistical evidence of study heterogeneity, see Appendix C.)

Locating the trials

The starting point in identifying trials for potential inclusion in this review was the 13 RCTs included in Ehri *et al*. (2001). Of these, nine compared systematic synthetic phonics instruction with unsystematic or no phonics teaching, two compared systematic analytic phonics instruction with no-phonics controls, and two compared other phonics interventions with no-phonics controls.

In order to locate any further potentially relevant published or unpublished randomized controlled trials a number of searches were undertaken. The original searches carried out by Ehri *et al.* (2001), covering the Education Resources and Information Center (ERIC) and PsycINFO (psychological literature) databases of research studies, were replicated, updated to 2005, and extended to capture unpublished trials. Three extra databases were searched: SIGLE, ASSIA and BEI (see Appendix D for details of these, and for the search strategies for each database).

The reviewers also wrote to Linnea Ehri to request bibliographic details of (a) the five published studies excluded from the original review because they compared instruction in synthetic phonics and instruction in analytic phonics; (b) the studies they identified but excluded because they were unpublished; and (c) any studies the reviewers knew of that should be included in the update (the last request was also sent to Camilli).

Inclusion criteria

Trials with the following characteristics were included:

- randomized controlled trials focusing on the teaching of phonics in English, and comparing either:

  (a) the effectiveness of instruction using *systematic* phonics with that of instruction providing *unsystematic* phonics instruction, or *no phonics* instruction (but where the control condition included some alternative reading instruction[9]); or

  (b) the effectiveness of synthetic phonics instruction compared with analytic phonics instruction.

and

- trials that measured reading as an outcome, reported statistics permitting the calculation or estimation of effect sizes, and involved interventions that might be found in schools.

Exclusion criteria

Trials were excluded if they:

- were not randomized controlled trials;
- did not evaluate either the relative effectiveness of systematic synthetic or analytic phonics instruction or some other form of systematic phonics instruction versus no phonics instruction (but an alternative reading instruction);
- were 'short-term laboratory studies with a limited focus' (Ehri *et al.*, 2001), e.g. a study in a psychology laboratory lasting for a few hours;
- lacked reading as an outcome;
- lacked statistics allowing calculation or estimation of effect sizes;
- primarily investigated phonemic awareness instruction or phonological awareness instruction (such studies were also excluded in Ehri *et al*., 2001); or
- compared two or more kinds of synthetic phonics instruction.

Calculation of effect sizes

For all the trials included in this review, effect sizes were calculated based on a mean of reading accuracy, a mean of reading comprehension (where applicable) and a mean of spelling (where applicable). Where possible standardized test results were used; experimenter-devised tests were used only where there was no alternative standardized test.

[9] This was not always the case in the Ehri *et al.* (2001) review, as described by Camilli *et al.* (2003).

In a few cases test results could not be included because both groups were at floor in the outcomes measured (had scores close to or at zero) or because the standard deviation was larger than the mean which indicated the data were extremely skewed (this applied to Lovett et al. (1990) third and fourth reading accuracy tests, and to Skailand (1971) second post-test). Too few studies used vocabulary measures to usefully pool the data. In addition, too few studies included follow-up assessments to usefully pool the data.

Meta-analyses

Two principal meta-analyses were undertaken:

- **Systematic phonics instruction versus alternative reading interventions: whole language/whole word ('look-and-say')**
- **Synthetic phonics instruction versus analytic phonics instruction.**

For the first of these meta-analyses, the comparators for the calculation of effect sizes were interventions using systematic phonics instruction (of any kind) compared with control groups using unsystematic or no phonics instruction, but using some kind of systematic reading instruction (e.g. whole word or whole language). For the second of these meta-analyses, the comparators for the calculation of effect sizes were interventions using systematic synthetic phonics instruction compared with interventions using systematic analytic phonics instruction.

9. Results

Searching and screening

The results of the searches and the first and second stages of screening are presented in Appendix E. De-duplication of the results from the electronic searches was done hierarchically in this order: Ehri *et al.*, PsycINFO, ERIC, ASSIA, BEI, SIGLE.

A total of **6114** potentially relevant studies were identified through the searching of the five electronic databases, through contact with the author, and through searching the Ehri *et al.* review. After the first screening, **101** potentially relevant papers were identified. **One** paper was unobtainable. The other 100 papers were then re-screened according to the pre-established inclusion criteria and definitions of synthetic and analytic phonics instruction.

Included studies

A total of **20 RCTs (in 19 papers)** were included at the second stage. Despite searching exhaustively in the grey literature databases only one unpublished RCT (Skailand, 1971) was found. Details of the method of retrieval of the included studies are given in Appendix F, and details of the interventions and control treatments of all the included studies, and the comparisons relevant to the review, are given in Appendix G.

Excluded studies

One of the trials included in the Ehri *et al.* (2001) meta-analysis (Gittelman and Feingold, 1983) was excluded from this review because it did not contain a phonics instruction intervention group. Although this paper stated that one of the interventions was 'motivated reading remediation…following the principles of the phonics method' (Gittelman and Feingold, 1983, p.170), it also stated that 'wherever possible, whole word recognition was introduced to enable the development of smooth, efficient, rapid reading and to avoid over-reliance on phonetic word analysis.' Clearly this intervention was *not* systematic phonics instruction. Indeed it closely resembled some of the unsystematic phonics instruction or no phonics instruction conditions used in the Ehri *et al.* analysis as comparators to systematic phonics instruction.

A second trial from the Ehri *et al.* (2001) review was excluded (Mantzicopoulos *et al.*, 1992)

because the control condition was not an appropriate comparison as the children did not receive a reading intervention: 'TEACH does not provide direct reading instruction to vulnerable readers' (p.574). In addition, this trial suffered from huge attrition. A total of 437 'at risk' kindergarten children were randomized (p.575), but 'only 168 children with complete scores were still in the intervention study at the end of second grade' (p.576), an attrition rate of 269 or 62%. However, the authors claimed an attrition rate of 280 (p.582) and in the results table (Table 4, p.582) the total n = 87. Clearly this study should have been excluded on two grounds: lack of an appropriate control group, and huge attrition leading to likely attrition bias. The authors discussed these problems at length in the paper (p.582).

The other 79 papers were excluded because they were not RCTs or because they did not include a systematic phonics instruction treatment group and an appropriate control group (where pupils were given reading instruction involving non-systematic or no phonics instruction).

Publication bias

On the basis of the included trials, some evidence of publication bias was detected (see Appendix C). If trials with null or negative results exist but were not accessible even through the grey literature, this would imply that the estimated effect sizes derived in all the meta-analyses reported below may be too large. This should reinforce the need for caution in interpreting them.

Main analysis

Of the 20 trials included at the second stage, six were excluded from the main meta-analysis (systematic phonics v. unsystematic or no phonics) because the experimental treatments were different varieties of systematic phonics instruction (e.g. synthetic phonics instruction versus onset-rime phonics instruction), and the control groups did not receive any comparable reading instruction (Fayne and Bryant, 1981; Lovett and Steinbach, 1997; Lovett *et al.*, 2000; Sullivan, 1971; Walton *et al.*, 2001, Exp. 1; Walton *et al.*, 2001, Exp. 2).

In addition, two studies were excluded from the meta-analyses because they were cluster trials (Berninger *et al.*, 2003; Brown and Felton, 1990); however, details of these are given here for comparative purposes and because some information from them is used qualitatively later in this section.

Two studies included in the Ehri *et al.* review (Lovett *et al.,* 1989, 1990) did not report numbers in the intervention and control groups separately (only total numbers). Although both Ehri *et al.* (2001) and Camilli *et al.* (2003) used estimates in their analyses, the present reviewers were able to obtain the actual numbers from the authors, and therefore include calculations based on them in the meta-analysis.

This left 12 RCTs in the main meta-analysis comparing systematic phonics instruction with an alternative reading intervention.

Table 1 contains information about each of the 12 RCTs included in the main analysis and the two cluster trials. The table includes information about study design, participants, intervention and control treatments, and the outcome measures used in the calculation of effect sizes. It also reports the effect sizes for word reading accuracy, comprehension and spelling as calculated for this review.

Table 1 and Appendix G show that all 14 trials reported outcome measures for reading accuracy; also that, of the 12 individually randomized trials, four reported outcome measures for reading comprehension, and three for spelling at first post-test.

Table 2 contains the quality assessments of the 14 trials. This table is based on the modified CONSORT guidelines for quality assessment of RCTs. (The Consolidated Standards for Reporting Trials are the methodological standard adopted by many medical journals for publication of randomized controlled trials; see Altman, 1996 and Altman *et al*., 2001.) These guidelines include assessment of whether the individual trials reported method of random allocation and sample size justification, and whether or not assessment of outcomes was 'blind' (conducted by people who did not know which condition individuals belonged to). (See Appendix G for details about the synthetic phonics interventions, the other phonics interventions, the control interventions and the comparisons relevant to the review. See Appendix J for details of the data extracted from each included study, and Appendix K for the raw data extracted from each study for the calculations of effect sizes.)

Table 1: Characteristics of the included RCTs

(See appendix H for the abbreviations used.)

Author, date	Study design	Participants	Intervention/control	Sample size	Outcome measures used in calculation of effect sizes by the reviewers: word reading accuracy (reading comprehension; spelling)	Effect size, as calculated by the reviewers (mean of word recognition and word attack measures; also mean of comprehension measures, mean of spelling measures, and synthetic versus analytic, where applicable), & confidence interval
Berninger *et al.* (2003)	Cluster RCT, 24 clusters; 2 children in each cluster	Second grade (age 7) children at risk for persistent reading problems and disabilities	Word recognition versus reading comprehension (whole language)	48 (word recog. N = 24; reading comp. n = 24) Effective sample size adjusting for clustering: 34	WRM word identification and word attack subtests administered; only word attack results reported, because this test showed a positive result: possible researcher bias	Accuracy: 0.3 (-0.38 to 0.97)
Brown and Felton (1990)	Cluster RCT, 6 clusters, 3 in each arm (8 children in each cluster)	Children at risk for reading disability in G1 (age 6)	Code emphasis versus context emphasis instruction for acquisition of word identification and decoding skills (synthetic phonics versus look-and-say)	47 (code n = 23; context n = 24) Effective sample size adjusting for clustering: 12	WRM word identification and word attack subtests	Accuracy: 0.24 (-0.89 to 1.37)
Greaney *et al.* (1997)	Ind. RCT	'Disabled readers' * G3 – G6 (ages 8-11)	Rime analogy training or item-specific training (onset-rime versus look-and-say)	36 (18 in each group)	Burt NZ raw score Neale (1988) raw score	Accuracy: 0.29 (-0.37 to 0.95)
Haskell *et al.* (1992)	Ind. RCT	Normally attaining first grade (age 6) pupils	Phoneme level training group versus whole-word level training group	24 (12 in each group)	Experimenter-devised tests: Reading regular words Reading exception words	Accuracy: 0.07 (-0.73 to 0.87)
Johnston and Watson (2004), Exp. 2	Ind. RCT	Normally attaining Primary 1 (age 5) children	Synthetic phonics group versus no-letter training group (look-and-say)	92	BASWRT	Accuracy: 0.96 (0.42 to 1.50) Synthetic versus analytic: 1.32 (0.77 to 1.82)

* In the New Zealand context, these are 'children who fall within the bottom 1% to 2% of beginning readers' (Greaney *at al*., 1997, p.646).

Table 1: Characteristics of the included RCTs, cont.

Author, date	Study design	Participants	Intervention/control	Sample size	Outcome measures used in calculation of effect sizes by the reviewers: word reading accuracy (reading comprehension; spelling)	Effect size, as calculated by the reviewers (mean of word recognition and word attack measures; also mean of comprehension measures, mean of spelling measures, and synthetic versus analytic, where applicable), & confidence interval
Leach and Siddall (1990)	Ind. RCT	Normally attaining first grade (age 6) pupils	Direct instruction versus paired reading (look-and-say)	20 (10 in each group)	Neale (1988) accuracy (Comprehension: Neale comprehension)	Accuracy: 0.80 (-0.11 to 1.71) Comprehension: 0.56 (-0.33 to 1.45)
Lovett *et al.* (1989)	Ind. RCT	'Disabled readers' *, mean age 10.8 years	Decoding skills programme group (DS) versus oral and written language stimulation group (OWLS, whole language)	121 (DS n = 60, OWLS n = 61)	WRAT-R reading subtest PIAT reading recognition SORT GORT connected text (GORT comprehension; spelling: WRAT-R spelling subtest, PIAT spelling)	Accuracy: 0.22 (-0.14 to 0.57) Comprehension: 0.08 (-0.28 to 0.44) Spelling: 0.07 (-0.29 to 0.42)
Lovett *et al.* (1990)	Ind. RCT	'Disabled readers' *, mean age 8.4 years	REG ≠ EXC versus REG = EXC (look-and-say)	36 (18 in each group)	WRAT-R reading subtest GORT accuracy Experimenter-devised tests; results given only for experimenter-devised tests: possible researcher bias	Accuracy: -0.19 (-0.85 to 0.46)
Martinussen and Kirby (1998)	Ind. RCT	Kindergarten (age 5) pupils assessed as low performers on phonological processing measures	Successive phonological group versus meaning group (whole language)	28 (13 in phonics group; 15 in meaning group). Attrition n = 2 from phonics group	WRM word attack; word identification; word reading (Ball and Blachman); results at floor for meaning group in word attack test, therefore not calculated (spelling: 'invented spelling')	Accuracy: 0.44 (-0.31 to 1.19) Spelling: 0.30 (-0.44 to 1.05)

* Children referred to the Learning Disabilities Research Program at the Hospital for Sick Children in Toronto, Canada who scored at least 1.5 years below expectation on EITHER word recognition accuracy OR reading speed (see Lovett *et al*., 1989, pp.97-98).

Table 1: Characteristics of the included RCTs, cont.

Author, date	Study design	Participants	Intervention/control	Sample size	Outcome measures used in calculation of effect sizes by the reviewers: word reading accuracy (reading comprehension; spelling)	Effect size, as calculated by the reviewers (mean of word recognition and word attack measures; also mean of comprehension measures, mean of spelling measures, and synthetic versus analytic, where applicable), & confidence interval
O'Connor and Padeliadu (2000)	Ind. RCT	G1 (age 6) children nominated as 'very poor readers'	Blending versus whole word conditions	12 (6 in each group)	Experimenter-devised tests – total words read (taught and transfer words) (spelling: experimenter-devised tests – taught and transfer words)	Accuracy: 0.53 (-0.62 to 1.68) Spelling: -0.15 (-1.28 to 0.99)
Skailand (1971)	Ind. RCT	Normally-attaining kindergarten (age 5) children	Grapheme/phoneme group versus whole word (look-and-say) group	42	Experimenter-devised tests – recall of words; transfer to similar words and syllables	Accuracy: -0.17 (-0.78 to 0.44) Synthetic versus analytic: -1.03 (-1.64 to –0.41)
Torgesen *et al.* (1999)	Ind. RCT	Kindergarten (age 5) children with weak phono-logical skills	PASP versus RCS	90 (45 in each group)	WRM-R Word attack Word identification	Accuracy: 0.07 (-0.34 to 0.48)
Torgesen *et al.* (2001)	Ind. RCT	Children between the ages of 8 and 10 identified as 'learning disabled' (= having learning difficulties)	Embedded phonics versus Auditory Discrimination in Depth Program	50	WRM-R Word identification GORT-III accuracy TOWRE/SWE (comprehension: WRMPCT-R; GORT-III comprehension)	Accuracy: -0.31 (-0.87 to 0.45) Comprehension: 0.05 (-0.50 to 0.60) Synthetic versus analytic: -0.25 (-0.66 to 0.17)
Umbach *et al.* (1989)	Ind. RCT	First grade (age 6) students having difficulty with reading	Reading mastery (direct instruction) versus Houghton-Mifflin (look-and-say)	31 (15 in direct instruction, 16 in basal programme)	WRM Word identification Total reading (comprehension: WRMPCT)	Accuracy: 2.69 (1.72 to 3.67) Comprehension: 1.08 (0.33 to 1.84)

Table 2: Quality assessment of the included RCTs

Author, date	Reporting of method of allocation	Sample size justi-fication	Intention to teach analysis	Blinded assessment of outcome	Comments
Berninger *et al.* (2003)	N/S	N/S	N/S	N/S	Attrition N/S
Brown and Felton (1990)	N/S	N/S	N	N/S	48 children randomized, yet only 47 mentioned in results section (1 lost from code group)
Greaney *et al.* (1997)	N/S	N/S	Y	Y	No attrition
Haskell *et al.* (1992)	N/S	N/S	Y	N/S	
Johnston and Watson (2004), Exp. 2	N/S	N/S	N	N/S	Attrition n = 7. Random allocation only confirmed through contact with author
Leach and Siddall (1990)	N/S	N/S	Y	N/S	
Lovett *et al.* (1989)	N/S	N/S	Y (for first battery of tests)	N/S	Numbers in each of the treatment groups requested and received from authors. Numbers only available for first battery of tests
Lovett *et al.* (1990)	N/S	N/S	Y	N/S	Numbers in each of the treatment groups requested and received from authors
Martinussen and Kirby (1998)	N/S	N/S	N	N/S	Attrition n = 2 in phonics group. Results at floor for word attack test (meaning group)
O'Connor and Padeliadu (2000)	N/S	N/S	Y	N/S	
Skailand (1971)	N/S	N/S	Y	N/S	
Torgesen *et al.* (1999)	N/S	N/S	Y	Y	
Torgesen *et al.* (2001)	N/S	N/S	N/S	N/S	Attrition n = 10 for two-year follow-up test
Umbach *et al.* (1989)	N/S	N/S	Y	N/S	

N/S = not stated

As can be seen from Table 2, none of the 14 trials reported method of random allocation or sample size justification, and only two reported blinded assessment of outcome. Nine of the 14 trials used intention to teach (ITT) analysis[10] (this could be explained by the fact that some educational researchers do not routinely report attrition, and imply that there were no drop-outs, which may not in fact be the case). The trials were, therefore, variable in quality but all were lacking in their reporting of some issues that are important for methodological rigour. Quality of reporting is a good but not perfect indicator of design quality. Therefore due to the limitations in the quality of reporting the overall quality of the trials was judged to be

[10] For definition, see Glossary.

'variable' but limited. Combined with the small number of included trials (12 in the main analysis) and the relatively small sample sizes (the largest was 121) the overall quality of the evidence base for the main analysis (systematic phonics v. unsystematic or no phonics) was judged to be 'moderate'. The overall quality of the evidence base for the other three analyses (comprehension, spelling, synthetic versus analytic) was judged to be 'weak'. This was because the trials again had limitations in their reporting and there were only three or four trials in each of these meta-analyses.

Systematic phonics instruction versus whole language or whole word intervention (1): Word accuracy

For measures of word accuracy and word identification, in 11 of the included trials the effect size for word accuracy was positive, and ranged from extremely small (Haskell *et al.*, 1992; Torgesen *et al.*, 1999), through moderate (Berninger *et al.*, 2003; Brown and Felton, 1990; Lovett *et al.*, 1989; Martinussen and Kirby, 1998; O'Connor and Padeliadu, 2000), to large (Leach and Siddall, 1990) or extremely large (Johnston and Watson, 2004, Exp.2; Umbach *et al.*, 1989). Only the two extremely large effect sizes were statistically significant. In three of the included studies the effect size was negative and small (Lovett *et al.*, 1990; Skailand, 1971; Torgesen *et al.*, 2001), but in no case was it statistically significant.

Of the 14 RCTs, 12 were individually randomized studies. These were pooled in a meta-analysis (Figure 1). The 'fixed effects' model of meta-analysis assumes that the estimate of effect holds for all the studies in the meta-analysis. The 'random effects' model assumes that the studies in the meta-analysis are a random sample of all the studies in that field.

Figure 1 shows that, using the fixed effects model, there was a statistically significant effect of phonics instruction on reading accuracy of 0.27 [11]. Using the random effects model there was a statistically significant effect of 0.38 [12]. This finding gave some support to the main finding of the Ehri *et al*. review.

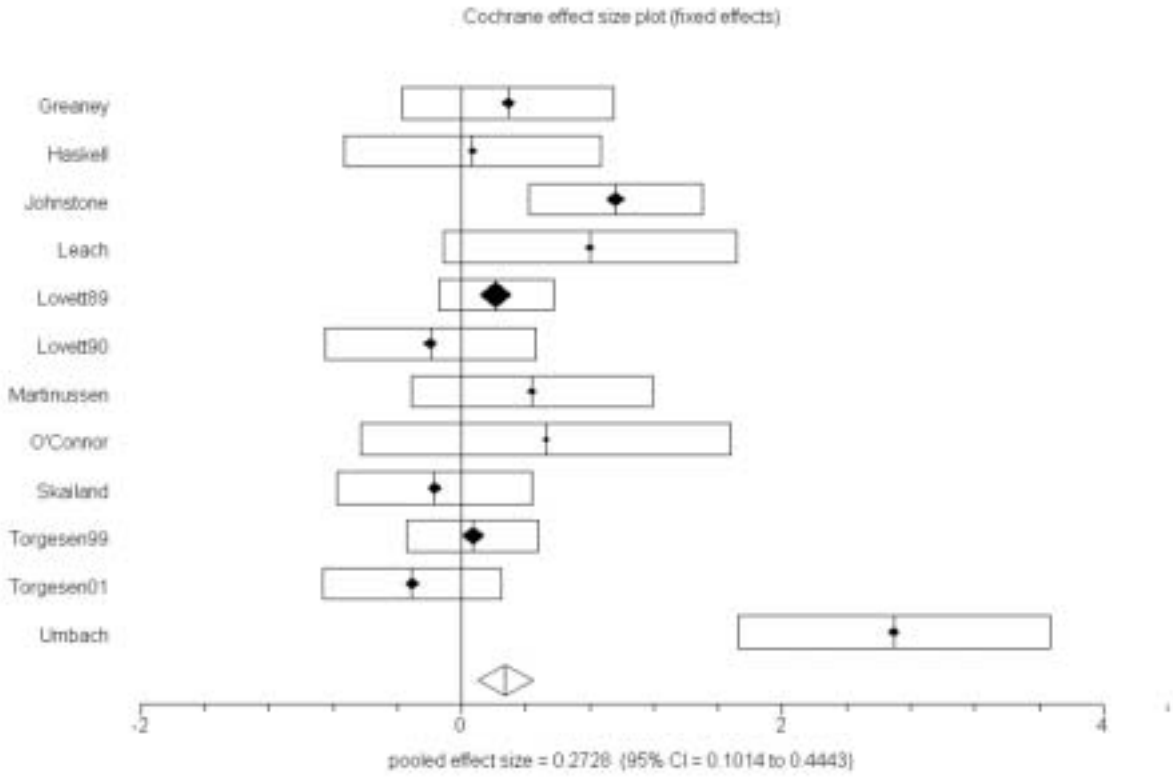
Sensitivity analysis

A sensitivity analysis was undertaken, by re-running the meta-analysis after the removal of the outlier (Umbach and Halpin, 1989). Using the fixed effects model the effect size was

[11] (p=0.002), d=0.27 (0.10 to 0.45).
[12] (p=0.002), d=0.38 (0.02 to 0.73).

reduced to 0.20 (statistically significant, CI 0.02 to 0.37, p=0.03). Using the random effects model the effect size was reduced to 0.21 (not statistically significant, -0.03 to 0.44, p=0.09). The results of the sensitivity analysis mean that caution is required in any interpretation of the meta-analysis because the removal of one small trial reduced the overall effect size and using one model of meta-analysis made it not statistically significant.

Figure 1: Meta-analysis of the 12 individually randomized trials



Systematic phonics instruction versus whole language or whole word intervention (2): Comprehension

Four of the 12 RCTs included in the main analysis used comprehension as an outcome measure at immediate post-test (Leach and Siddall, 1990; Lovett *et al*., 1989; Torgesen *et al*., 2001; Umbach *et al.*, 1989). **The pooled estimate of effect size for these four trials using the fixed effects model was 0.24 but this was not statistically significant[13].** Using the

[13] Approximate 95% CI = -0.03 to 0.51, not statistically significant p=0.08.

random effects model the pooled effect size was 0.35 and again this was not statistically significant[14].

Systematic phonics instruction versus whole language or whole word intervention (3): Spelling

In addition, three of the 12 studies included in the main analysis used spelling as an outcome measure at immediate post-test (Lovett *et al.*, 1989; Martinussen and Kirby, 1998; O'Connor and Padeliadu, 2000). **The pooled estimate of effect size for these three trials using the fixed effects model was 0.09 but this was not statistically significant[15].** Using the random effects model the pooled effect size and confidence intervals were identical.

Systematic synthetic phonics instruction versus systematic analytic phonics instruction

Three studies directly compared systematic synthetic phonics instruction with systematic analytic phonics instruction (Johnston and Watson, 2004; Skailand, 1971; Torgesen *et al*., 1999). **The pooled estimate of effect size using the fixed effects model was 0.02 but this was not statistically significant[16].** Using the random effects model the pooled effect size was also 0.02 and not statistically significant[17].

Proportion of literacy teaching devoted to phonics instruction

In order to address the questions 'What proportion of literacy teaching should be based on systematic phonics instruction?' and 'Should phonics instruction focus on phonics for reading *and* phonics for spelling?' data were extracted from the 14 included studies on the amount of instructional time in each of the experiments, and on what other literacy instruction participants were receiving, including whether or not the interventions included spelling instruction (see Table 3).

Regarding the question about proportion, phonics may be taught exclusively, non-exclusively (as part of a wider literacy curriculum), or not at all. As can be seen from Table 3, in nine of the trials the reading intervention comprised phonics instruction within the context of a broad literacy curriculum (i.e. non-exclusive phonics teaching). Only one trial (Brown and Felton, 1990) compared exclusive phonics instruction for reading (and spelling) with no phonics

[14] Approximate 95% CI = -0.09 to 0.79.
[15] Approximate 95% CI = -0.22 to 0.40, not statistically significant p=0.56.
[16] Approximate 95% CI = -0.27 to 0.31, not statistically significant p=0.87.
[17] Approximate 95% CI = -1.23 to 1.26.

instruction. (In the other four trials the relevant information was not stated.) There was therefore insufficient RCT evidence on which to compare exclusive with non-exclusive use of phonics. Data from the nine trials of non-exclusive use of phonics was then investigated to see whether progress in literacy correlated with **amount** of systematic phonics instruction received within the broader curriculum.

As cross-referencing Tables 3 and 1 shows, the amount of instructional time varied from less than 2 to about 160 hours, and the trial which detected the largest effect for reading accuracy (Umbach *et al*., 1989) was the trial with the longest length of intervention. However,

- that trial had one of the smallest sample sizes;
- the trial which detected the second largest effect size for reading accuracy (Johnston and Watson, 2004, Exp. 2) had one of the shortest lengths of intervention (and an average sample size for this group of trials); and
- although, as previously demonstrated in the main meta-analysis, the pooled effect size was positive and statistically significant, and in 11 out of the 14 trials a positive effect was found for systematic phonics instruction compared with whole language/whole word instruction, this was statistically significant in only two individual trials.

Again, this means that there was insufficient RCT evidence on which to recommend an amount of phonics instruction.

Phonics for reading and phonics for spelling

With regard to the question about phonics for reading and phonics for spelling, though there was moderate evidence of the benefits of teaching **reading** through systematic phonics, the evidence on teaching **spelling** through systematic phonics was not yet conclusive because of the small number of relevant trials (3). Therefore the RCT evidence cannot yet be used to determine whether phonics should, or should not, be used to teach spelling as well as reading.

Phonics for reading and spelling beyond the early years

It was not possible to analyse how different approaches impacted on the application of phonics in reading and writing beyond the early years because only three RCTs used follow-up measures.

Table 3: Details of instructional time and of instruction received by the intervention groups

Author, date	Amount of instructional time in experiment	Information about what other literacy instruction participants were receiving, including whether or not the interventions included spelling instruction
Berninger *et al.* (2003**)**	2 x weekly sessions of 20 minutes each for a total of 24 lessons. **Total instructional time = 8 hours.**	**Supplemental reading instruction** – children 'did not miss any work for the regular reading program' (p.105). 'Overall, in the regular reading program, the children appeared to receive background reading instruction that included both word recognition and reading comprehension' (p.108) – 'balance reading instruction'. **Reading and spelling:** 'During the last ten minutes of the word recognition training, children engaged in reflective activities such as classifying a spelling unit according to different pronunciations associated with it or generating words to illustrate different phonemes associated with the spelling unit' (p.105).
Brown and Felton (1990)	**Total instructional time = Not known.**	'**All reading instruction** was provided for these children by research teachers' (p.226). **Reading and spelling:** 'Spelling was taught as one component of the reading lesson with spelling lists developed from the words introduced in each unit of reading instruction' (230).
Greaney *et al.* (1997)	30 mins of individual instruction 3 or 4 times per week for 11 weeks. **Total instructional time = 16.5 hours to 22 hours.**	The instruction provided was **in addition** to the regular classroom reading program. 'The method of classroom reading instruction to which all the children were exposed adhered to the 'whole language' philosophy of teaching reading' (p.646). **Reading and spelling.**
Haskell *et al.* (1992)	15 x 20 minute sessions over a six-week period during language arts instruction time in school. **Total instructional time = 5 hours.**	Interventions **replaced part** of the language arts time. **Spelling not mentioned.**
Johnston and Watson (2004), Exp. 2	Seen twice a week for 15 mins on 2 separate days, with two non-intervention days in between. Continued for 10 weeks, 19 sessions per child in total. **Total instructional time = 4.75 hours.**	The children were extracted from class for extra tuition **in addition** to their normal reading programmes (p.344). **Spelling not mentioned.**
Leach and Siddall (1990)	10-15 minutes per day, each weekday for 10 weeks. **Total instructional time = 8.3 to 12.5 hours.**	**Additional support:** 'All reading sessions were conducted by parents in their own homes' (p.351). **Spelling not mentioned.**
Lovett *et al.* (1989)	All groups: 40 treatment sessions. Seen in pairs. Each session 50 to 60 mins, 4 times per week for 10-week period. **Total instructional time = 33 hours to 40 hours.**	**Additional treatment programme –** children were seen in special laboratory classrooms; no attempt was made to control for the other literacy experiences of the children (p.96). **Reading and spelling –** 'The Decoding skills Program is an instructional program in which attention is focused exclusively on the acquisition of word recognition and spelling skills' (p. 95).

Table 3: Details of instructional time and of instruction received by the intervention groups, cont.

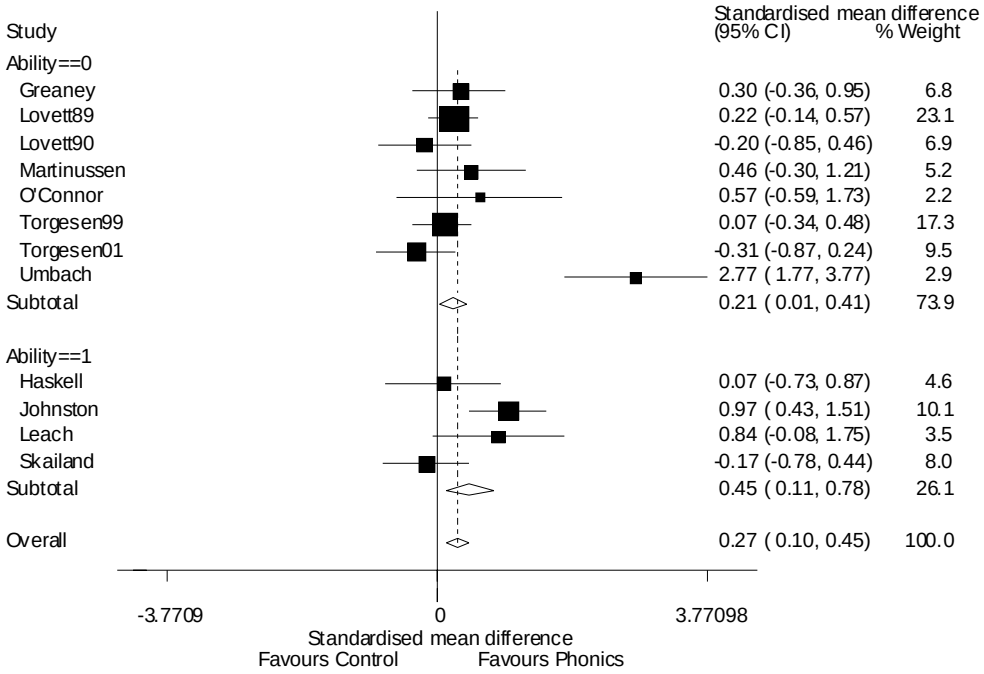
Author, date	Amount of instructional time in experiment	Information about what other literacy instruction participants were receiving, including whether or not the interventions included spelling instruction
Lovett *et al.* (1990)	A total of 35 sessions was conducted in each program. All sessions lasted 60 minutes and were conducted four times a week. **Total instructional time = 35 hours.**	**Additional treatment programme –** children were seen in special laboratory classrooms; no attempt was made to control for the other literacy experiences of the children (p771). **Reading and spelling –** 'Training in word recognition and spelling was addressed.' (p.771).
Martinussen and Kirby (1998)	The length of the intervention was eight weeks, with two or three 20-min sessions per week. Due to occasional absences the number of sessions received by individual children ranged from 17-20 sessions. **Total instructional time = 5.6 hours to 6.6 hours.**	**N/S – whether supplementary or additional or part replacement etc.** **Spelling not mentioned.**
O'Connor and Padeliadu (2000)	Ten training sessions of 10-13 mins each. **Total instructional time = 1.6 hours to 2.1 hours.**	The treatments were conducted **in addition to regular reading instructional time**, which consisted of large group discussion and choral reading of Big Books, writing in journals, and independent silent reading in all classes (p. 168). **Reading and spelling (p.165).**
Skailand (1971)	Two 15-min periods for ten weeks. **Total instructional time = 5 hours.**	**N/S – whether supplementary or additional or part replacement etc.** **Spelling not mentioned**
Torgesen *et al.* (1999)	**Total instructional time = specific data not available (reviewer estimate = approx. 34 hours)**	'As a rule, we tried to schedule children for our instructional interventions at a time in their school day **that did not interfere with their regular classroom reading instruction**…regular classroom instruction was primarily literature based and guided by a whole-language philosophy, with phonics being taught on an as-needed basis rather than systematically' (p. 583). **Spelling not mentioned**
Torgesen *et al.* (2001)	**Total instructional time = 67.5 hours.**	**N/S – whether supplementary or additional or part replacement etc.** 'This training substituted for the time the children would normally have spent in their learning disabilities resource room' (p. 37) **Reading and spelling**
Umbach *et al.* (1989)	**Total instructional time = specific data not available (reviewer estimate = approx. 160 hours)**	**N/S – whether supplementary or additional or part replacement etc.** **Spelling not mentioned**

N/S = not stated

Study heterogeneity

Table 3 also suggests that there was significant heterogeneity among the 14 RCTs (and this was confirmed statistically – see Appendix C). Some studies were undertaken with children with reading difficulties or disabilities, and others with normally attaining children (educational heterogeneity). To explore whether this could be a cause of the observed heterogeneity, a meta-analysis was undertaken to assess whether or not there was a statistical interaction between the effect of phonics instruction and learner characteristics (Figure 2).

Figure 2: Main meta-analysis subdivided by learner characteristics

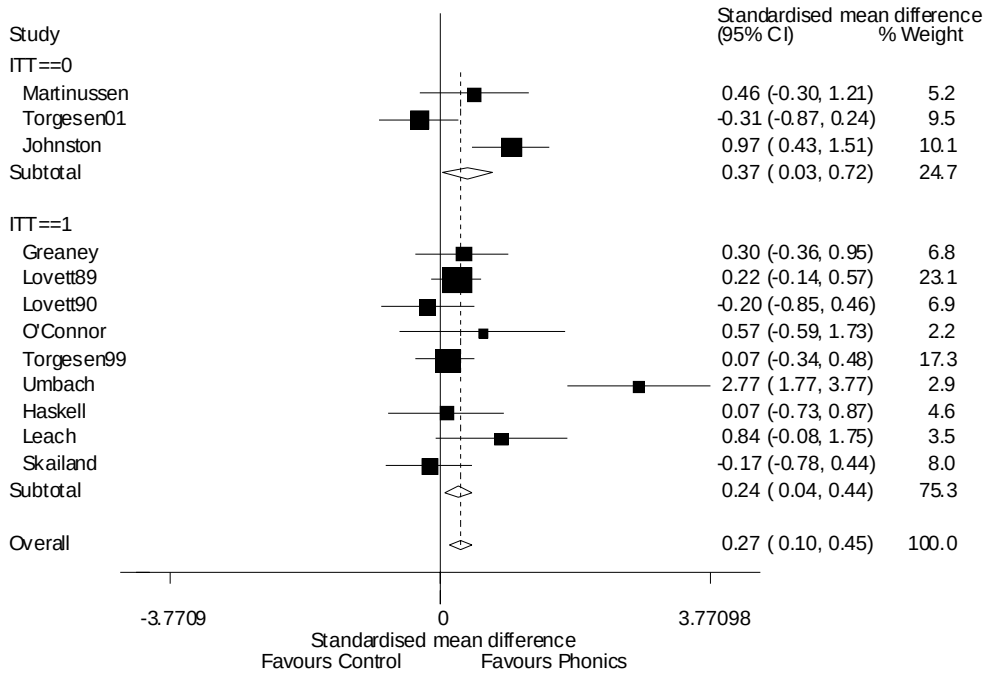


As Figure 2 shows, phonics instruction tended to produce a larger effect size (0.45) for normally attaining children (studies from Haskell downwards) than for children with reading disabilities and difficulties (0.21). However, the test for interaction was not statistically significant (p=0.24). Therefore, there was no statistical evidence to support the belief that the effectiveness of phonics instruction was different for learners with different characteristics. Th finding that the effect size for systematic phonics instruction was similar for children with all learner characteristics supports one of those reported by Ehri *et al*. (2001).

The studies also differed in whether or not they used intention to teach analysis (procedural heterogeneity). (Intention to teach analysis means that all participants are analysed in their

original randomized groups; it is the most robust analytical method. Also see Glossary.) In Figure 3 an analysis is shown of whether or not studies differed in their results by the use of intention to teach analysis

Figure 3: Main meta-analysis subdivided by ITT or no-ITT



ITT = Intention To Teach

As Figure 3 shows, studies that used ITT analysis tended to have smaller effect sizes (0.24 compared with 0.37); however, this apparent interaction was not statistically significant (p=0.72). Therefore, there is no evidence that the use of ITT analysis affected the results of the studies.

Judgement of evidence

When considering the findings reported above it is important to express them in an **overall judgement of the evidence based on three things: the strength of the effect, the statistical significance of the effect, and the quality of evidence on which these are based.**

In general, the strength of an effect can range from small (effect size around 0.2) through medium (effect size around 0.5) to large (effect size around 0.8). An effect can be statistically significant at the level of $p < 0.05$ (which means that there is a 95% probability of the effect

not having occurred by chance) or it can be non-significant (which means that there are no statistically significant differences between the groups). The confidence intervals are also instructive here and indicate the level of uncertainty around an effect. If the confidence intervals are wide this indicates a high level of uncertainty, and if they cross through zero this indicates that the effect is not statistically significant. The quality of evidence relates to the sample size of the individual trials, the methodological rigour of the individual trials and the number of trials included in the analysis.

The reviewers concluded that:

- none of the findings of the current review were based on strong evidence because there simply were not enough trials (regardless of quality or size);
- some findings were based on moderate evidence (because there were a few trials of variable quality with small sample sizes);
- some findings were based on weak evidence (because there were very few trials with small sample sizes and variable quality); and
- in a few cases there was insufficient evidence to support any finding.

The quality of the evidence for a finding and its effect and/or statistical significance may be independent of each other. It would be desirable to base recommendations for changes in teaching on highly statistically significant medium to large effects based on good quality of evidence (either several moderately sized, good quality trials or on one well-designed very large trial in a normal school setting). But since there are no such findings at present, it is necessary to proceed on the basis of the evidence that is available.

Summary of findings

Heeding the cautions expressed in the previous subsection, the current review's findings can be found in Table 4.

Table 4: Summary of findings, by research question, answer, quality of evidence, strength of effect, statistical significance, and implications for teaching

Research question	Answer	Quality of evidence	Strength of effect	Statistical significance	Implications for teaching
Does systematic phonics instruction enable children to make better progress in reading *accuracy* than unsystematic or no phonics?	Yes *	Moderate	Small (effect size = 0.27)	Highly statistically significant (p=0.002)	No warrant for NOT using phonics – it should be a routine part of literacy teaching
Did the evidence for the finding above differ according to whether or not researchers had used intention to teach analysis?	No	Moderate	Small (effect sizes = 0.24 and 0.37 respectively)	Not statistically significant (p>0.05) N.B. The non-significant value implies no difference between the groups.	(n/a – methodological question)
Does systematic phonics instruction enable both normally-developing children and those at risk of failure to make better progress in reading *accuracy* than unsystematic or no phonics?	Yes *	Moderate	Medium and small (effect sizes = 0.45 and 0.21 respectively)	Not statistically significant (p>0.05) N.B. The non-significant value implies no difference between groups.	No warrant for NOT using phonics with either group – both normally-developing children and those at risk of failure can benefit
Does systematic phonics instruction enable children to make better progress in reading *comprehension* than unsystematic or no phonics?	Not clear	Weak	Small (effect size = 0.24)	Not statistically significant (p=0.08)	No clear finding from research on whether or not phonics boosts progress in comprehension
Does systematic phonics instruction enable children to make better progress in spelling than unsystematic or no phonics?	Not clear	Weak	Very small (effect size = 0.09)	Not statistically significant (p=0.56)	No warrant from research for either using or not using phonics to teach spelling
Does systematic synthetic phonics instruction enable children to make better progress in reading *accuracy* than systematic analytic phonics?	Not clear	Weak	Very small (effect size = 0.02)	Not statistically significant (p=0.87)	No warrant from research for choosing between these varieties of systematic phonics

Table 4: Summary of findings, by research question, answer, quality of evidence, strength of effect, statistical significance, and implications for teaching, cont.

Research question	Answer	Quality of evidence	Strength of effect	Statistical significance	Implications for teaching
What **proportion** of literacy teaching should be devoted to phonics?	(Insufficient evidence)				No warrant from research for any change to existing practice
What **amount** of literacy teaching should be devoted to phonics?	(Insufficient evidence)				No warrant from research for any change to existing practice
Should phonics be used in the teaching of spelling as well as reading?	(Insufficient evidence)				No warrant from research for any change to existing practice
Should phonics be used beyond the early years?	(Insufficient evidence)				No warrant from research for either using or not using phonics beyond the early years

n/a = not applicable
n/s = not stated

* Finding supports one reported by Ehri *et al*. (2001), but with a reduced effect size.

10. Discussion

Two of the main findings of the current review supported those of Ehri *et al.* (2001), namely that systematic phonics instruction enables children to make better progress in reading accuracy than unsystematic or no phonics, and that this is true for both normally-developing children and those at risk of failure. However, there were some important differences. The overall effect size of 0.27 was substantially lower than Ehri *et al.*'s estimate of 0.41 (implying that approximately 12 extra children out of 100 rather than approximately 16 extra children would succeed on a relevant test). This reduction in the effect size may have been due to the inclusion of new trials from the updated searches, and/or to some features of the Ehri *et al.* review, namely:

- the fact that they included non-randomized as well as randomized trials;
- their use of estimated rather than actual numbers in the different groups in two studies;
- their use of what was essentially an untaught control group as the counterfactual in some comparisons (this is likely to have exaggerated the effects of phonics teaching); and
- not adjusting for clustering effects in the calculation of the mean effect size in the cluster trial (Brown and Felton, 1990) which Ehri *et al.* included but which was excluded from the main analysis in the current review.

Quality issues

None of the 14 included trials reported method of random allocation or sample size justification, and only two reported blinded assessment of outcome. Nine of the 14 trials used intention to teach (ITT) analysis. These are all limitations on the quality of the evidence. The main meta-analysis included only 12 relatively small individually randomised controlled trials, with the largest trial having 121 participants and the smallest only 12 (across intervention and control groups in both cases). Although all these trials used random allocation to create comparison groups and therefore the most appropriate design for investigating the question of relative effectiveness of different methods for delivering reading support or instruction, there were rather few trials, all relatively small, and of varying methodological quality. This means that the quality of evidence in the main analysis was judged to be 'moderate' for reading accuracy outcomes. For comprehension and spelling outcomes the quality of evidence was judged to be 'weak'. This was due to the very small number of relevant trials and their sample sizes. For the secondary analysis looking at the

relative effectiveness of synthetic versus analytic phonics instruction the evidence base was again judged to be ‘weak’, mainly due to the tiny number of trials included in the analysis (3), and the fact that all these trials were relatively small.

11. Conclusions

This section is organised largely around the four original research questions.

1. How effective are different approaches to phonics teaching in comparison to each other (including the specific area of analytic versus synthetic phonics)?

The current review has confirmed that systematic phonics instruction is associated with an increased improvement in reading *accuracy*. The effect size is 0.27, which translates into a 12% absolute improvement in a reading accuracy test that is standardised to have a score with a mean of 50% for children not receiving systematic phonics (see Torgerson, 2003, p.86). In other words, of 100 children **not** receiving systematic phonics instruction, in a test 50 would score 50% or more, compared with 62 children who would score 50% or more if they **did** receive systematic phonics instruction. The current review has also confirmed that this is true for both normally-developing children and those at risk of failure.

However, there was little RCT evidence on which to compare analytic and synthetic phonics, or on the effect of systematic phonics on reading *comprehension* or spelling, so that it was not possible to reach firm conclusions on these issues.

2. How do different approaches impact on the application of phonics in reading and writing, including beyond the early years?

It was not possible to analyse how different approaches impacted on the application of phonics in reading and writing beyond the early years because only three RCTs used follow-up measures.

3. Is there a need to differentiate by phonics for reading and phonics for spelling?

This question could not be tackled directly because none of the RCTs had addressed it. However, there was a difference in the findings, in that systematic phonics instruction was found to benefit children's reading *accuracy*, but there was insufficient evidence to reach firm conclusions about impact on reading *comprehension* or spelling.

4. What proportion of literacy teaching should be based on the use of phonics?

Again, there was insufficient RCT evidence on which to base a firm conclusion.

Limitations

Even where the RCT evidence was considered strong enough to draw conclusions the findings need to be treated with caution. There was significant heterogeneity within the meta-analysis, which could not be explained by the *reported* design characteristics of the included trials or by the educational characteristics of the children included in the studies. This could be explained by the difference in the lengths of the intervention or by the interventions differing between trials. It is also unclear whether systematic phonics teaching was beneficial to *all* children with different learner characteristics, as for example very few trials included English speakers of other languages or a design capable of comparing the relative effectiveness of the interventions for girls and boys.

Only one of the included trials was undertaken in Britain, which raises concerns about the applicability to the British context of results based largely on research elsewhere. In addition, the strong possibility of publication bias affecting the results cannot be excluded. This is based on results of the funnel plot (see Appendix C). It seems clear that a cautious approach is justified.

Generally the trials were small and few in number, and the quality of reporting of their methods was variable, but all trials only included small sample sizes. In addition, there was huge variation in the amount of phonics teaching, ranging from just a few hours to well over 100. The evidence in this review did not provide any warrant for exclusive teaching of reading using a phonics approach, but rather provided moderate evidence for using a systematic phonics approach within a broad literacy curriculum.

It was not possible to draw any firm conclusions with regard to the proportion of time that should be devoted to phonics instruction. Basing recommendations for a particular proportion of time to be spent on phonics on such slender evidence base would not be wise. Therefore, at least one large RCT, if not more, should be undertaken to confirm or refute the overall promising effect of systematic phonics and also to explore the amount of phonics instruction children should receive.

12. Recommendations

For teaching

- **Since there is evidence that systematic phonics teaching benefits children's reading accuracy, it should be part of every literacy teacher's repertoire and a routine part of literacy teaching, in a judicious balance with other elements.**

- Teachers who already use systematic phonics in their teaching should continue to do so; teachers who do not should add systematic phonics to their routine practices.

Moreover, **there is no RCT evidence for one common objection to the use of phonics**:

- There is no justification for withholding phonics from either normally-developing children or those at risk of reading failure – both may benefit and it should be used with both.

However, otherwise there is little warrant in these findings for changes to existing practice. In particular,

- There is currently no strong RCT evidence that any one form of systematic phonics is more effective than any other.
- There is also currently no strong RCT evidence on how much systematic phonics is needed.
- Two other areas on which the existing research base is insufficient are whether or not phonics teaching boosts comprehension, and whether phonics should be used to teach spelling as well as reading.

For teacher training

- The evidence that systematic phonics teaching benefits children's reading accuracy further implies that learning to use systematic phonics in a judicious balance with other elements should form part of every literacy teacher's training.

For research

- The point was made in section 10 that none of the findings of this review have strong evidence in their support, so what is needed is a well-designed RCT to shed clearer light on the key findings. The current review therefore recommends a large UK-based cluster-randomized controlled trial to confirm the findings of this review and to investigate further the relative effectiveness of systematic synthetic versus systematic analytic phonics instruction with children with different learning characteristics.

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Appendices

APPENDIX A: Extended definitions of synthetic phonics

Some authors would wish to add several features to the definition of synthetic phonics given in the main text, namely:

- *The words on which children exercise their phonic skills must be unknown to them in their written form.*
- *Fast pace***.**
- *There should be an initial phase in which children exercise their phonic skills only on letters, then single words, before attempting to apply them to words in text (including books)***.**
- *At each stage, phonics for reading must precede phonics for spelling***.**

These extensions were not adopted in this review because each of them could, in theory, be a feature of analytic phonics instruction or some other initial teaching approaches, and therefore is not necessarily *only* part of the definition of synthetic phonics instruction**.**

APPENDIX B: Phonics 'first, fast and only'

Some synthetic phonics advocates (see the *Reading Reform Foundation Newsletter, passim*) insist that the technique should be used 'first, fast, and only'. This section examines whether any research evidence was found to support this.

'First': This part of the approach appears to mean that children should receive phonics teaching as soon as they enter school and before they are taught other decoding strategies. With home support, some children have already made a start on reading (and possibly writing) before they start school, so that with them the 'first, fast and only' approach is impractical. On the other hand, although many other children seem to learn to read without much phonics at all, especially if the literacy environment at home and at school is rich and broad, the main findings in this review do support the view that any children who have not yet started to read at school entry should immediately receive systematic phonics teaching.

However, teaching of some aspects of phonics, some of it not wholly informal, now occurs routinely in Foundation stage settings in England, that is, with 4 and 5 year-olds. In some other countries, especially in Scandinavia, literacy teaching is explicitly the task of the first years after school entry, and forbidden in earlier stages, and children's progress does not appear to be hampered by this (Elley, 1992). However, many of the languages involved have less complicated orthographies than English, and there is now substantial evidence that the complicated orthography of written English (plus the complex syllable structure of spoken English) is a factor in the slower rate of literacy learning of English-speaking children (Seymour *et al.*, 2003; Ellis *et al.*, 2004). Is this a reason for starting literacy teaching so early, or for adopting methods that will increase the rate of learning once children are of statutory school age? No direct research evidence on this was found. There are practical examples (the Reading Reform Foundation (RRF) has several) of successful teaching of phonics to 4-year-olds, and the THRASS (Teaching Handwriting, Reading and Spelling Simultaneously) system has also demonstrated this capacity. But these only show that this *can* be done, not whether it *should*. This would require strong research evidence that a start before age 5 enables children to make better initial progress, and sustain it. No such evidence was found in this review.

'**Fast**': On this factor it would be easy to be misled by the Clackmannanshire experiments (Johnston and Watson, 2004). Experiment 1 compared fast synthetic phonics with slow analytic phonics, thus confounding two variables. Experiment 2 compared fast synthetic phonics with fast analytic phonics, but this implies nothing about the effect of the fast pace. To check that would require an experiment comparing, for example, fast synthetic phonics with slow synthetic phonics, and no such experiment is known to have been carried out. Instead there are assertions that a fast pace is crucial, and practical

examples (again, the RRF has several) of successful rapid teaching of synthetic phonics. But again these only show that this *can* be done, not whether it *should*.

'**Only**': Two major differences from much current practice may be implied by the use of the word 'only': no use of other word-identification strategies besides phonics (whereas use of various strategies is thought to be implied by the searchlights model), and a brief early phase within phonics teaching when books are not used. No conclusive research evidence was found on either. Synthetic phonics advocates maintain that working words out from the context, illustrations or knowledge of grammar or the whole word are all tantamount to guessing, mislead children into thinking they can always work things out this way, fail to equip them with an effective strategy for identifying unfamiliar words, and leave them confused. In his report to the D*f*ES, Brooks (2003) argued that the searchlights model makes an assumption about children's ability to divide their attention which seems to have been falsified by research. He also inclined to the view (Brooks, 2002) that children should not have to try to work out which word-identification strategy to use whilst trying to identify a word – but this was assertion, not evidence.

Some synthetic phonics advocates argue that, in the very earliest stages of phonics teaching, children should be exposed only to letters and their sounds, not to whole words (though these should come in very shortly); and this seems to imply that in the initial stages children should not be asked to try out their phonics skills on text, i.e. books. It is extremely easy to caricature this view as 'They don't want children to have books', as parts of the media have. But, as we understand it, these synthetic phonics advocates are not saying this – no-one has said 'Stop reading to your child at home' or 'Abolish story time in school' (the Hatcher *et al.*, 2004 experiment seems to show that this is unnecessary) – but only stating that, as a logical part of the teaching sequence and for as limited a period as is necessary, children in the first stages should develop their phonics skills on single letters, then on single words, and only after that on words in sentences. It is an empirical question whether such a programme would enable children to make better progress than one in which phonics is applied to words in text from the outset. Only one of the 14 included trials in this review investigated exclusive phonics versus no phonics, and there is therefore not enough RCT evidence either to support or contradict this suggestion.

APPENDIX C: More details on the systematic review methods used

Screening and quality assurance: procedures

All the located studies were double screened using titles and abstracts, where available, and on the basis of criteria adapted from the original criteria (Ehri *et al.*, 2001, p.400). Screening was undertaken by two reviewers (including for all databases by the Principal Investigator or the Director) working independently and then meeting to discuss any differences in decisions to include or exclude articles, with the exception of the records retrieved through the re-run of the original search on ERIC – 1970-2000. This database was screened by the Principal Investigator, and a random sample of 10% was generated and double screened by a second reviewer. A Cohen's Kappa statistic was calculated to assess the inter-rater reliability of the screening.

Screening and quality assurance: results

For databases where two reviewers screened the entire database, the agreement between reviewers was high. Disagreements occurred only on whether or not the trials should be included according to the intervention criterion. One reviewer was consistently more inclusive (JH), and included in some cases trials that evaluated phonemic awareness instruction or phonological awareness instruction. In all cases agreement to include or exclude was secured after discussion to resolve any differences. For the screening of the 10% random sample of the ERIC database of unpublished literature, the Cohen's Kappa measure of agreement was 1 (perfect agreement). Therefore it was not considered necessary for any further double screening to be undertaken.

Full agreement was established on whether or not to include papers at the second stage of screening (screening of full papers), and on the appropriate comparison and outcome measures to be used in the calculation of effect sizes.

Data extraction

The consistency with which Ehri *et al.* applied the definitions of synthetic and analytic phonics to the trials in their analysis was checked; and Brooks's definitions (see the main text) were applied to all the RCTs included in this review. Data were extracted by two team members independently from each included RCT in the following categories: bibliographic details; study design; participants (including specific learner characteristics); details of the interventions and control group treatments; outcome measures (including all the raw data necessary for re-calculation of effect sizes); sample size; and reported effect size. Initial agreement between the two independent extractions and calculations was high; full agreement was established through discussion.

In order to address the questions 'What proportion of the literacy teaching should be based on systematic phonics instruction?' and 'Should phonics instruction focus on phonics for reading *and* phonics for spelling?' data were extracted from all the studies in the main analysis on the amount of instructional time in each of the experiments, and on what other literacy instruction participants were receiving, including whether or not the interventions included spelling instruction.

Calculation of effect sizes

The main meta-analysis pooled the effect sizes of individually randomized trials that compared systematic phonics instruction with a reading intervention for three outcomes (word accuracy, comprehension and spelling), using the computer software package 'Arcus Quickstat'. The fixed effects model was used for the standardised mean differences in the meta-analyses as this was the model adopted by Ehri *et al.* (2001), but the random effects model was also calculated and both sets of statistics are reported for the two principal meta-analyses. To investigate possible sources of heterogeneity, sub-group analyses were performed according to learner characteristics and methodological variation in the trials, using the computer software package 'STATA'. The secondary meta-analysis pooled the effect sizes of individually randomized trials that compared systematic synthetic phonics instruction with systematic analytic phonics instruction, using the computer software package 'Arcus Quickstat'.

Two of the included RCTs were cluster RCTs (Berninger *et al.*, 2003; Brown and Felton, 1993). When participants are allocated in a cluster or class randomized RCT the correlation between pupils needs to be taken into account when estimating the confidence intervals. To do this an adjusted sample size after adjusting for the effects of clustering was calculated. The formula applied was: 1+(m-1) x ICC, where m is the average size of the cluster and ICC is the intra-cluster correlation. The ICC used was from a recent RCT of information and communication technology and spelling/reading undertaken with colleagues from York and Sheffield (Brooks *et al.*, 2005; 2006, in press). This ICC was 0.45.

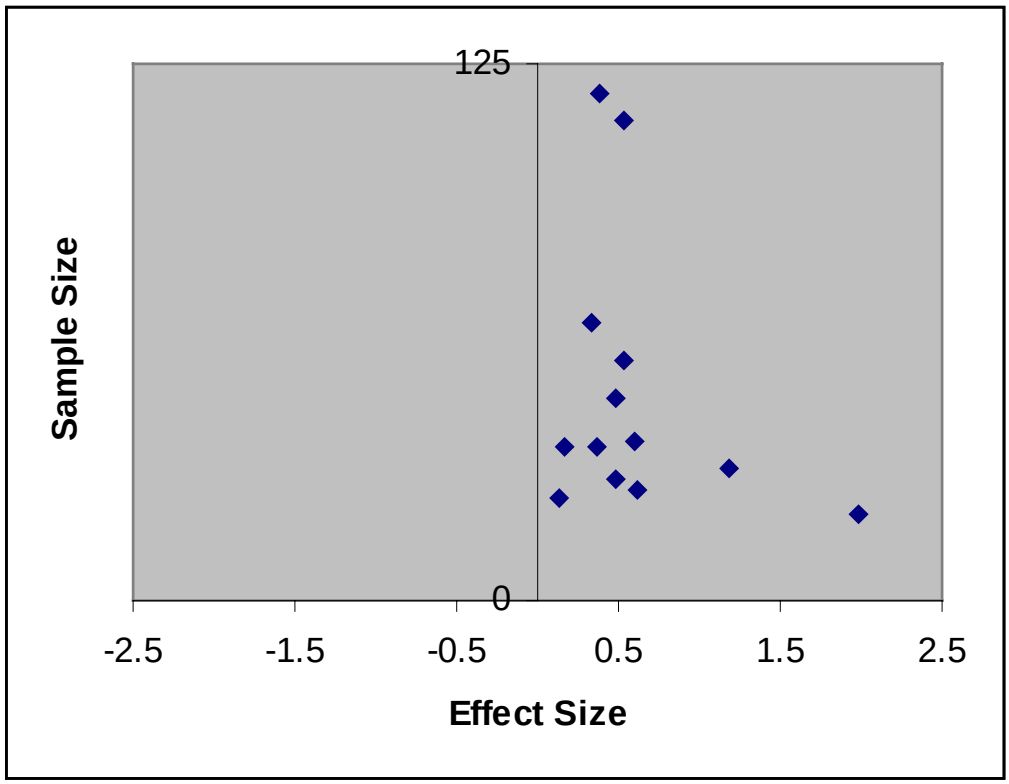
The formula s.d. = $\sqrt{n}$ x SE was applied to calculate the standard deviation for the one paper where the s.d. was not available, but where the standard error (SE) was available (Lovett *et al.*, 1989).

Estimation of publication bias

One of the inclusion criteria in the Ehri *et al.* (2001) review was that the trials had to be journal articles that had been peer-refereed. Including this criterion could have potentially increased the risk of overestimating the effect size of the intervention, as it is more likely that negative studies will have been excluded. Figure 4 (reproduced from Torgerson, 2003, p.68) is a funnel plot of the effect sizes of the 13 RCTs included in the Ehri *et al.* review.

Figure 4 shows that there were no studies in that set reporting a negative effect of systematic phonics instruction compared with all forms of control, despite the small sample sizes of the included studies. This is *prima facie* evidence for publication bias, since it seems highly unlikely that no RCT has ever returned a null or negative result in this field (and the present review did find some negative results). Although the Ehri *et al.* results suggested that systematic phonics teaching is probably an effective strategy, this conclusion might have been modified if notice had been taken of this evidence of publication bias.

Figure 4 Funnel plot of randomized trials from the systematic review of systematic phonics instruction, showing possible presence of publication bias



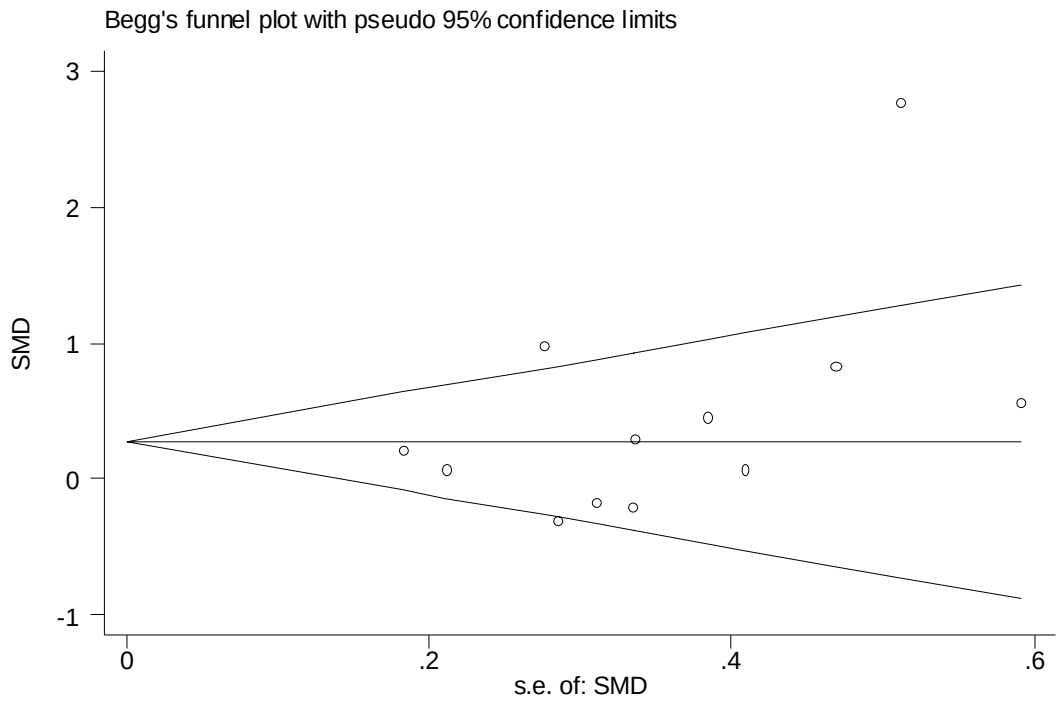
Another way of estimating the likelihood that publication bias has occurred is to calculate the fail-safe *n*, that is, the number of unlocated or excluded (or yet-to-be-conducted) studies with contradictory findings that would need to exist to reduce the estimated effect size to statistical non-significance. Ehri *et al.* (2001, p.431) did calculate a fail-safe *n* (860). This high figure suggests that, at the current rate of appearance of relevant RCTs, it would be many decades before their main finding was overturned, even if all further RCTs had contradictory results; thus it gives the impression that the finding is highly secure. However, Ehri *et al.*'s method of calculating the fail-safe *n* was not the standard version as just summarized, but: how many studies of effect sizes **below 0.2** (rather than zero or negative estimates) would be required to indicate that their 43 comparisons of 0.2 and above were

'statistical exceptions'? The number required with null or negative effects would be smaller, though probably still large enough to sustain confidence in their finding.

The current review found only one unpublished study, with an effect size of -0.17 (a negative result). Publication bias may still have been present, however. The average effect size of the revised meta-analysis was 0.27 (95% CI 0.10 to 0.45). For a study to have 80% power to observe this estimate with a 5% significance would require a sample size of approximately 400. All the studies in the review were insufficiently powered to show this difference. Indeed, the average size of the studies included in the review would only have 80% power to observe an effect size of 0.85. This suggests, therefore, that there are similarly powered studies that have smaller, not statistically significant, effect sizes that remain unpublished even within the grey literature.

To test informally for potential publication bias in the updated review a funnel plot was drawn and the Egger statistical test for asymmetry was calculated (Egger *et al.*, 1997). The resulting funnel plot (Figure 5) does suggest asymmetry, but the Egger test for asymmetry was 0.17, which is not statistically significant. Although there is no statistical evidence for publication bias, it cannot be ruled out due to the small number of studies in the analysis.

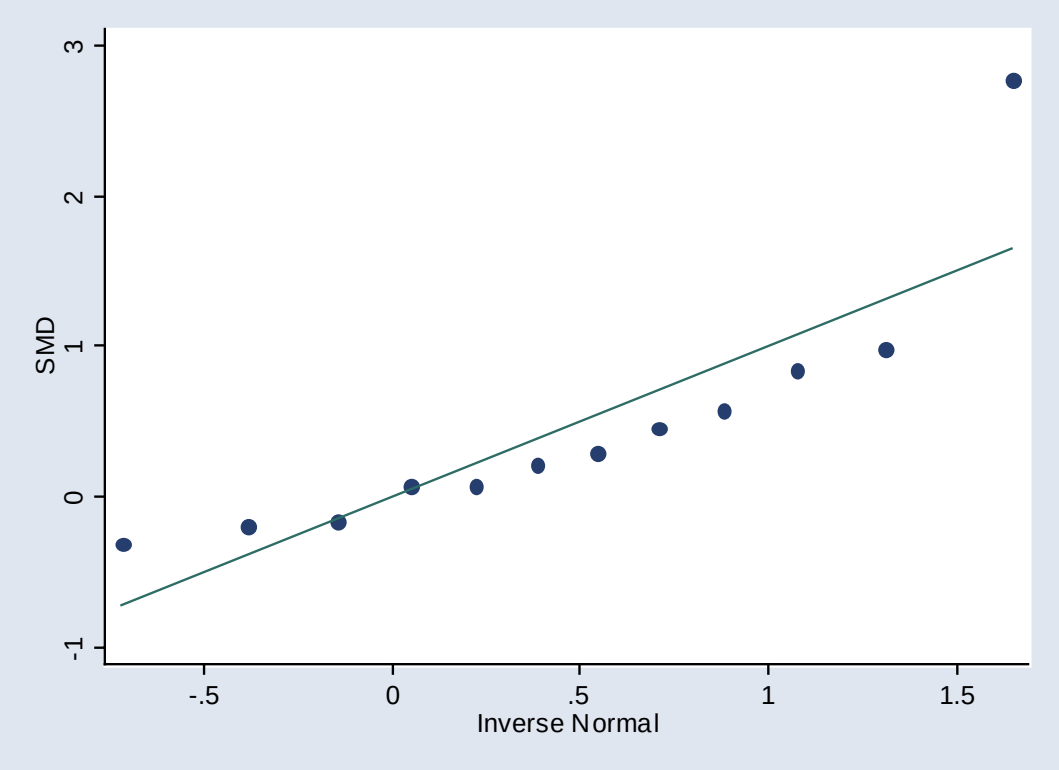
Figure 5: Funnel plot of effect sizes of the 12 individual RCTs in the current review



Statistical evidence for study heterogeneity

There was significant heterogeneity in the pooled data used for the main meta-analysis ('Q' statistic 46.30, $p<0.001$). In addition, the normal quantile plot (Figure 6) was also suggestive of at least two study populations because the studies did not uniformly fall on the diagonal and tended to form an 'S' shape.

Figure 6: Normal quantile plot of the 12 individual RCTs in the current review



APPENDIX D: Search strategies for each database

PsycInfo

For PsycInfo a search strategy was created using the three sets of search terms in the Ehri *et al.* paper (p. 399) combined using 'AND'. This strategy was run for the period 1970-2005. This retrieved 1079 records for the period 1970-2000 and 398 records for the period 2001-5. These records were then checked to see if they included any of the included studies. Of the 38 papers in the Ehri review 26 were retrieved by this search. The researchers therefore decided to use this search strategy for PsycInfo. For the period 1970-2000 the database was sorted by publication type and then included only unpublished records (103).

ERIC

The three sets of terms in the Ehri *et al.* review (p. 399) were combined: 'set 1 AND (set 2 OR set 3)'. The reviewers ran this for the period 1970-2000 and retrieved 22 papers from the original review. Therefore they decided to use this strategy for ERIC. For the period 1970-2000 the database was sorted by publication type and then included only unpublished records (4462).

ASSIA, BEI and SIGLE

For each of these three databases the three groups of search terms in the Ehri *et al*. (2001) review were combined: 'set 1 and (set 2 OR set 3)' for the period 1970-present for BEI and SIGLE and for the period 1987 – present for ASSIA.

APPENDIX E: Results of the searching and screening at first and second stages

Electronic database or method of retrieval	Initial 'hits' after de-duplication	No. included at first stage	Unobtainable or not received	No. of RCTs included at second stage
Ehri *et al.* (2001)	13	11	0	11
Contact	6	4	0	4
PsycInfo 1970-2000	103	4	0	0
PsycInfo 2000-2005	398	19	0	3 (in 2 papers)
ERIC 1970-2000	4462	37	1	1
ERIC 2000-2005	652	14	0	1
ASSIA	143	4	0	0
BEI	277	0	0	0
SIGLE	61	0	0	0
Total	**6114**	**101**	**1**	**20 (in 19 papers)**

APPENDIX F: Method of retrieval of the 20 included RCTs

Method of retrieval	Included
Ehri *et al.* (2001) meta-analysis	11 RCTs: Brown and Felton (1993); Greaney *et al.* (1997); Haskell *et al.* (1992); Leach and Siddall (1990); Lovett *et al.* (1989); Lovett *et al.* (1990); Lovett *et al.* (1997); Martinussen and Kirby (1998); Torgesen *et al.* (1999); Umbach *et al.* (1989)
Contact	4 RCTs: Fayne and Bryant (1981); Johnston and Watson (2004), Exp 2; Sullivan *et al.* (1971); Torgesen *et al.* (2001)
ERIC search 1970-2000	1 RCT: Skailand (1971)
ERIC search 2000-2005	1 RCT: Berninger *et al.* (2003)
PsycINFO 2000-2005	3 RCTs: O'Connor and Padeliadu (2000); Walton *et al.* (2001), Exp 1; Walton *et al.* (2001), Exp 2

APPENDIX G: Details of the 20 included RCTs

Author(s) & date	Synthetic phonics intervention group	Other interventions	Comparisons
Berninger *et al.* (2003)	Word recognition group. Segmentation and teacher modelling blending are mentioned, but children blending is not explicitly mentioned (pp.105-7)	1) Reading comprehension = whole language (p.107); 2) Word recognition and reading comprehension = phonics plus whole language (p.107)	Synthetic/ whole language
Brown & Felton (1990)	Lippincott Basic Reading programme group (pp.229-30)	Houghton Mifflin programme = look-and-say (p.229)	Synthetic/ look-and-say
Fayne & Bryant (1981)	Treatment 3, Letter-by-letter training (pp.618-9)	Other 4 groups were onset-rime &/or body-coda (pp.618-9)	Synthetic/ onset-rime using Treatment 2 (Final-final) as onset-rime
Greaney *et al.* (1997)		1) Rime analogy = onset-rime (p.647); 2) Item-specific training = look-and-say (p.648)	Onset-rime/ look-and-say
Haskell *et al.* (1992)	Phoneme group (pp.40, 42-3)	1) Onset-rime (pp.40, 43); 2) Whole word = look-and-say (pp.40, 43)	Synthetic/ onset-rime & synthetic/ look-and-say
Johnston & Watson (2004), exp.2	Synthetic (pp.344, 347-8)	1) No-letter training group = look-and-say (pp.344, 346); 2) Accelerated letter learning group = analytic (pp.344, 346-7)	Synthetic/ analytic; also synthetic/ look-and-say
Leach & Siddall (1990)	Direct Instruction (pp.349-50)	1) Hearing Reading = look-and-say (p.351); 2) Paired Reading = look-and-say (p.351); 3) Pause, Prompt and Praise = look-and-say (p.351)	Synthetic/ look-and-say
Lovett *et al.* (1989)	Decoding Skills Program group (p.95)	Oral and Written Language Stimulation Program = whole language (pp.95-6)	Synthetic/ whole-language & systematic vs no phonics
Lovett *et al.* (1990)	REG ≠ EXC group (pp.771-2)	REG = EXC group = look-and-say (pp.772)	Synthetic/ look-and-say
Lovett & Steinbach (1997)	Phonological analysis and blending/Direct Instruction group (pp.193-4)	Word identification strategy training program group = onset-rime (pp.194-5)	Synthetic/onset-rime
Lovett *et al.* (2000)	Double phonological analysis and blending/direct Instruction (PHAB/DI) group (pp.266-7, 269)	1) Double Word identification strategy training program (WIST) group = onset-rime (pp.267-8); 2) PHAB/DI + WIST (p.269); 3) WIST + PHAB/DI (p.269)	Synthetic/ onset-rime, using double WIST as onset-rime

Appendix G: Details of the 20 included RCTs, cont.

Author(s) & date	Synthetic phonics intervention group	Other interventions	Comparisons
Martinussen *et al.* (1998)	Successive-phonological group (pp.28-30)	Meaning group = whole language (p.30)	Synthetic/ whole-language
O'Connor & Padeliadu (2000)	Synthetic group (pp.165-7)	Whole word = look-and-say (p.167)	Synthetic/ look-and-say
Skailand (1971)	Grapheme/ phoneme group (p.5)	1) Whole word = look-and-say (p.5); 2) Similar spelling = analytic with rime families (p.5); 3) Contrastive spelling = analytic with onset families (p.5)	Synthetic/ analytic, using 'similar spelling' as analytic; also synthetic/ look-and-say
Sullivan *et al.* (1971)	Single-letter group (pp.228-32)	Letter-combination group = onset-rime (pp.228-32)	Synthetic/ onset-rime
Torgesen *et al.* (1999)	Phonological awareness plus synthetic phonics group (p.582), even though blending not explicitly mentioned	Embedded phonics = analytic (p.582)	Synthetic/ analytic
Torgesen *et al.* (2001)	Embedded Phonics (pp.39-40)	Auditory Discrimination in Depth = whole-word + phonemic awareness (pp.38-9)	Synthetic/ whole-word + phonemic awareness, = approx. whole language
Umbach *et al.* (1989)	Reading Mastery Series group (p.115)	Houghton Mifflin programme = look-and-say (p.116)	Synthetic/ look-and-say
Walton *et al.* (2001), exp. 1	Letter recoding group (pp.164-5)	Rime analogy = onset-rime (p.165)	Synthetic/ onset-rime
Walton *et al.* (2001), exp. 2	Letter recoding group (pp.164-5)	Rime analogy = onset-rime (p.165)	Synthetic/ onset-rime

Appendix H: Abbreviations for Table 1

BASWRT = British Ability Scales Word Reading Test

Burt NZ = Burt Word Reading Test, New Zealand Revision

GORT = Gray Oral Reading Test

GORT-III = Gray Oral Reading Test, 3rd edition

Neale = Neale Analysis of Reading Ability

PIAT = Peabody Individual Achievement Tests

SORT = Slosson Oral Reading Test

TOWRE/SWE = Test of Word Reading Efficiency, Sight Word Efficiency subtest

WRAT-R = Wide Range Achievement Tests, Revised

WRM = Woodcock Reading Mastery

WRM-R = Woodcock Reading Mastery, revised

WRMPCT = Woodcock Reading Mastery passage comprehension test

WRMPCT-R – Woodcock Reading Mastery passage comprehension test, revised

APPENDIX J: Data extraction tables for all studies included in the meta-analyses

Berninger, V.W. *et al.* (2003) Comparison of three approaches to supplementary reading instruction for low-achieving 2nd grade readers, *Language, Speech and Hearing Services in Schools*, 34(2): 101-16.	
Country of origin	USA
Setting	Second grade classrooms.
Objective	To evaluate the relative effectiveness of three instructional approaches to **supplementing** the regular reading program for second graders with low word reading and/or pseudo-word reading skills.
Study design	Cluster trial. "48 pairs of children were randomly assigned to the 4 experimental conditions in the following way. At each of the 8 schools, dyads were created based on number of children who met inclusion criteria at that school. Then, these child pairs were randomly assigned to each of 48 slots in the overall design (12 dyads in each of 4 conditions) (p.105)". Some control for teacher effects**. Supplemental reading instruction – children 'did not miss any work for the regular reading program' (p.105). 'Overall, in the regular reading program, the children appeared to receive background reading instruction that included both word recognition and reading comprehension' (p.108) – balanced reading instruction.** Raw data reported for only one out of three outcome measures.
Participants	Referred by teachers as **'poorest' students** (p.103). Inclusion criteria – scaled score of 6 or higher on WISC-III vocabulary subset and a score at or below 85 on WRM-R word identification or word attack. The other WRM-R had to be below population mean of 100. Number randomized = 96 (56 girls, 40 boys; 8% African American, 8% Asian American, 61.5% European American, 8% Hispanic, 1% Native American, 13.5% other/not reported). Age: Average age 7 yrs (girls), 8yrs 7 months (boys).
Intervention	1. Word recognition training only, n = 24. Explicit instruction in alphabetic principle in and out of word context and reflective activities such as classifying a spelling unit according to different pronunciations associated with it or generating words to illustrate different phonemes associated with the spelling unit. 2. Reading comprehension training only, n = 24. Engaged in reflective discussion using Connects, Organize, Reflect and Extend model to develop situational component of comprehension plus explicit language cueing at the word, sentence and text levels. 3. Combined word recognition/reading comprehension, n = 24. Explicit instruction in alphabetic principle plus explicit language cueing at word, sentence and text level.
Control	N = 24. Word play with oral language and repeated readings at their instructional level. Practised aural comprehension and word reading skills but did not receive explicit instruction in reading comprehension or word reading skills. **All groups – 2 x weekly sessions of 20 minutes each for a total of 24 lessons.**

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Brown, I.S. and Felton, R.H. (1990) Effects of instruction on beginning reading skills in children at risk for reading disability, *Reading and Writing: An Interdisciplinary Journal*, 2(3): 223-41.	
Country of origin	USA
Setting	Five schools in USA, kindergarten grades 1 and 2.
Objective	To investigate the impact of code-emphasis versus context-emphasis instruction on the acquisition of word identification and decoding skills in children identified as at risk for reading disability.
Study design	Cluster trial – although methods not very clear. 'Six groups of eight at risk children were placed into regular first grade classrooms in the five schools, and were randomly assigned to one of two instructional methods' (p.227). No further details provided. Cluster trial – only 6 clusters with 8 in each. 48 randomized but only 47 included in baseline characteristics. Also, very little information about randomization process.
Participants	**Included 'at risk' children. 'At risk' status required child to obtain, on at least three of the research measures, scores of one or more Standard Deviations below the group mean, or to be in the bottom 16th percentile for the sample.** 48 children randomized, however baseline characteristics based on n = 47. (48th child (in code approach group) no info given but is included in counting of 6 drop-outs during the study). Age/Grade: Mean (SD), range for context group 6.2 (0.53), 5.6-7.3 and for code group 6.1 (0.35), 5.5-6.8
Intervention	Code Approach: n = 23 at Grade 1 results and n = 19 at Grade 2 results. Lippincott Basic Reading Program (1981) used. Direct code method (synthetic phonics). Length of follow-up 2 years in total.
Control	Context Approach: n = 24 at Grade 1 results and n = 23 at Grade 2 results. Houghton Mifflin (1986) programme used for general reading instruction in local school system. Children taught to first attempt words by using context plus also employs phonics cues but no attempt is made to teach blending of phonics elements. Length of follow-up 2 years in total.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Greaney, K.T., Tunmer, W.E. and Chapman, J.W. (1997) Effects of rime-based orthographic analogy training on the word recognition skills of children with reading disability, *Journal of Educational Psychology*, 89(4): 645-51.	
Country of origin	New Zealand
Setting	Pupils were drawn from 36 primary schools in North Island, New Zealand.
Objective	The aim of this study was to determine whether meta-cognitive strategy training in the use of rime spelling units would be an effective intervention strategy for children with reading disability.
Study design	Individual RCT. Thirty-six **disabled readers** were randomly assigned to 1 of 2 training groups, a rime analogy training group or an item-specific training group (p.645) No further details provided.
Participants	Initial sample contained 57 disabled children. Children were from Years 3-6. All were native English speakers. No children were included who were receiving special education assistance in school or who were known to have a hearing, visual, language or intellectual impairment. Mean age of intervention group 8.23 years and control group 8.16 years. 36 of the disabled readers were randomly assigned to one of two treatment groups, the rime analogy treatment group or the item specific treatment group. One year after treatment follow-up data were obtained from the two treatment groups.
Intervention	**Subjects received 30 minutes of individual instruction three or four times per week for 11 weeks**. The study was carried out during the middle term of a three-term school year. The children were tested individually in a quiet withdrawal room in their school. Rime analogy treatment – the rime analogy training group received systematic training in the use of rime spelling units to identify words. Received 30 minutes of individual instruction 3 to 4 times per week for 11 weeks. Systematic training in the use of rime spelling units to identify words was incorporated in the thirty-minute lesson format. Rime analogy training did not generally exceed 5 minutes in duration in each 30-minute session.
Control	Item-specific training – systematic training in the use of context cues to identify unfamiliar words. Received 30 minutes of individual instruction 3 to 4 times per week for 11 weeks. Systematic training in the use of context cues to identify unfamiliar words was incorporated into the lesson and did not exceed 5 minutes in duration in each 30-minute session.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Haskell, D.W., Foorman, B.R. and Swank, P. (1992) Effects of three orthographic/phonological units on first-grade reading, *Remedial and Special Education*, 13(2): 40-49.	
Country of origin	USA
Setting	Within school during language arts instruction, suburban middle school in South Western United States.
Objective	To examine whether instruction at the onset-rime level facilitates first graders' accuracy in the word reading more than instruction at the whole word level or phoneme level. (p.40) Hypotheses: training at the onset-rime level will facilitate first graders' word reading more than training at the phoneme level. Either the phoneme level or onset-rime level training will be more facilitative than the whole-word level training or no training. (p.42)
Study design	Individual RCT with stratified randomization. Pupils scoring at the 98th percentile on the Gates-MacGintie Reading Test, Level A, Form 1 were excluded from the study. Students above and below the median raw score of 48 formed two pools from which students were alternately randomly selected for equal distribution into one of the four treatment groups: phoneme level training, onset-rime level training, whole word level training and untrained controls.
Participants	48 first graders from 4 of the 7 first grade classes in a suburban, predominantly middle class school in south west United States. **No pupils had English as a second language and only one had learning disabilities.** Attrition – one student from the untrained control group left during the study and was replaced by a spare subject. No gender figures are given.
Intervention	12 students in each intervention group were divided randomly into groups of 6 and received **15 x 20 minute sessions over a six-week period during language arts instruction time in school.** Intervention received was either phoneme or onset-rime.
Control	12 students in the whole word control group were divided randomly into groups of 6 and received **15x 20-minute sessions over a six week period during language arts instruction time in school.** The untrained control group received no extra intervention.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Johnston, R.S. and Watson, J.E. (2004) Accelerating the development of reading, spelling and phonemic awareness skills in initial readers, *Reading and Writing: An Interdisciplinary Journal*, 17(4): 327-57.	
Country of origin	UK (Scotland)
Setting	Extracted from usual classroom to form instructional groups.
Objective	To establish whether synthetic phonics is more effective than analytic phonics merely because letter sounds are taught at an accelerated pace.
Study design	Individual. "Participants matched into 3 groups on chronological age, sex, vocabulary knowledge, letter knowledge, emergent reading, phoneme segmentation and rhyme generation ability". (p344) Participants were then randomized into 3 groups – this is not stated in paper (personal communication to Carole Torgerson).
Participants	Drawn from 4 Primary 1 classes in two schools one week after school entry in early September. **No child had English as 2nd language.** N = 99, 7 dropouts therefore n = 92. 46 boys, 46 girls. Age/Grade: mean age 5.0 (SD 0.3)
Intervention	1. Synthetic phonics, n = 30. Accelerated learning and blending of the letter sounds in initial, middle and final positions of words. The letter making the sound for the day was shown, the children hearing the sound and repeating it. 2. Accelerated letter training group, n = 33. Accelerated learning of letters in the initial position of words. Letter sounds were specifically taught. Two letter sounds a week were introduced.
Control	No letter training group, n = 29. Received no additional letter training beyond their classroom teaching. Children were shown pictures and words in a book designed to teach a particular letter sound, although the letter sound was not taught explicitly. Therefore exposed to print vocabulary but a 'look-and-say' whole word approach adopted, and no teaching of the letter sound. Various games then played, involving matching pictures to words to reinforce teaching of these words. Work carried out individually, in pairs or in small groups.
	All groups – Treatment groups started 6 weeks after entering school. Seen in groups of 4 to 5. **Seen twice a week for 15 mins on 2 separate days, with two non-intervention days in between. Continued for 10 weeks, 19 sessions per child in total.** Same printed words used in all 3 conditions. All children continued in their normal class reading programmes. Two weeks after the start of the trial teachers began teaching the classroom analytic phonics programme, following the educational authorities guidelines. Continued for 26 weeks then children shown importance of letter sounds in all positions of words, using CVC words. None taught letter names. By time of last post-test all the children had learnt letter sounds in all positions of words in their classroom programmes. Follow-up – at end of intervention (10 weeks), 3 months after end of intervention and 9 months after end of intervention.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Leach, D.J. and Siddall, S.W. (1990) Parental involvement in the teaching of reading: A comparison of hearing reading, paired reading, pause, prompt, praise and direct instruction methods, *British Journal of Educational Psychology*, 60(3): 349-55.	
Country of origin	Australia
Setting	Parent training was carried out at school. All reading sessions were conducted by parents, in their own homes – **10-15 minutes per day, each weekday for 10 weeks.**
Objective	To compare the relative effectiveness of Paired Reading, Pause, Prompt, and Praise, Hearing Reading and Direct Instruction methods. It was hypothesized that Direct Instruction would increase beginning reading skills to a greater extent than the other methods. Hearing reading, it was hypothesized, would be the least effective.
Study design	Individual RCT. N = 40. Forty parents were randomly drawn, then randomly assigned to receive one of four tutoring methods already listed above. (p.350)
Participants	N = 40. Final sample was composed of 26 boys and 14 girls with chronological ages ranging from 5 yrs 3 mths to 6 yrs 4 mths (M = 5 yrs 7 mths). **No child was considered to have learning difficulties. 1 had mild speech impediment, 1 ESL pupil.** Children were all beginning readers in the middle of their first term at school. Each had made more than 16 errors on the first story of Neale Analysis of Reading Ability. They continued to receive instruction in reading according to the school's normal reading practices during the intervention period. Tutors = 3 fathers, 1 older sister, 36 mothers.
Intervention	10-15 minutes per weekday for 10 weeks. Direct Instruction: phonics programme – parents received 4 and half hours of training and all programme materials to use at home. Paired Reading – simultaneous parent/child reading of texts – parents received one and a half hours training and children took a reader home from school each day. Pause, Prompt, and Praise – method to teach self-correction responses to errors in reading using syntactic and semantic clues – parents received one and a half hours training and pupils took home a reader every day.
Control	Hearing Reading 'minimally guided hearing reading group'. Parents received no training, simply a sheet of guidelines. Pupils took home a reader every day.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Lovett, M.W. *et al.* (1989) Can dyslexia be treated? Treatment specific and generalised treatment effects in dyslexic children's response to remediation, *Brain and Language*, 37(1): 90-121.	
Country of origin	Canada
Setting	Special laboratory classrooms in a paediatric teaching hospital.
Objective	To compare the effects on reading levels of disabled readers, of a decoding skills programme versus an oral and written language stimulation programme, compared to control group.
Study design	Individual. "178 disabled readers were randomly assigned to one of the three experimental treatment conditions". (p.94). No further details.
Participants	**Children referred for remedial reading instruction, aged 8 to 13 years. Inclusion criteria: evidence of specific underachievement in reading in context of at least low average intelligence.** Exclusion criteria: English 2nd language, hyperactivity, hearing impairment, brain damage, chronic medical condition or serious emotional disturbance. Sample consisted of 178 disabled readers. 137 males/41 females. Average intelligence with underachievement in written language. 60% were accuracy disabled and also had problems in decoding accuracy. Scored at least 1.5 years below instructional level expectations on at least 4 different measures of word recognition accuracy. 71% from families in middle SES ranges. No mention of race. Age/Grade: mean age 10.8 years (SD 1.5)
Intervention	1. DS (decoding skills programme). Training in word recognition and spelling skills. Regular and exception words instructed upon; regular words in context of a word family and exception by sight methods alone. Training in phonetic analysis and blending, rapid word recognition, morphological analysis, written spelling. No explicit training in reading comprehension, listening comprehension or appreciation of sentence structure. 2. OWLS (oral and written language stimulation programme). Developed to remediate oral and written language simultaneously. Intensive work on oral language comprehension, reading and reading comprehension implemented over 4 day instructional cycles. Weekly topic themes. Parallel instruction concentrated in vocabulary, structural analysis and grammar and discourse comprehension. The programme was presented in the context of language structures larger than the single word.
Control	3. CSS (classroom survival skills programme). Received training in areas of social skills, classroom etiquette, life skills, organisational strategies, academic problem solving and self-help techniques. No direct instruction and no exposure to print were offered. **All groups: 40 treatment sessions. Seen in pairs. Each session 50 to 60 mins, 4 times per week for 10-week period**. Administered by special education teachers with each teacher implementing each treatment programme with approx one third of assigned cases. No attempt to control for other educational experiences of the children.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Lovett, M.W. *et al.* (1990) Training the word recognition skills of reading disabled children: Treatment and transfer effects, *Journal of Educational Psychology*, 82(4): 769-80.	
Country of origin	Canada
Setting	Special laboratory classrooms in a paediatric teaching hospital.
Objective	To compare the effectiveness of two experimental word recognition training programmes with a control programme, in disabled readers (instructed words and uninstructed words).
Study design	Individual. "Children were randomly assigned to a treatment condition and to a particular teacher" (p.771). "Subjects were randomly assigned to a treatment condition and to instructed word lists" (p.773). No further details provided.
Participants	54 **disabled readers**. Inclusion criteria – aged 7-13 years, evidence of specific underachievement in reading (had to score below 25th percentile, replicable on 4 of the 5 different measures). Exclusion criteria – English as a 2nd language, history of extreme hyperactivity, hearing impairment, brain damage, a chronic medical condition, serious emotional disturbance. 38 boys, 16 girls. Average intelligence in middle socio-economic ranges according to Blishen scales. Age/Grade: mean age 8.4 yrs (SD 1.6)
Intervention	1. REG ≠ EXC. Intensive systematic instruction in word recognition and spelling skills. Regular words taught by training the constituent letter-sound mappings. Exception words were introduced and rehearsed by whole-word methods alone. 35 sessions x 60 minutes, 4 times per week. 2. REG = EXC. Intensive systematic instruction in word recognition and spelling skills. Both regular and exception words were taught the 'exception word' way. 35 sessions x 60 minutes, 4 times per week.
Control	CSS (Classroom survival skills programme, revised version). Received training in areas of classroom etiquette, life skills, organisational strategies, academic problem solving and self-help techniques. No direct instruction and no exposure to print were offered.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Martinussen, R.L. and Kirby, J.R.. (1998) Instruction in successive and phonological processing to improve the reading acquisition skills of at-risk kindergarten children, *Developmental Disabilities Bulletin*, 26(2): 19-39.	
Country of origin	Canada
Setting	Separate lessons for intervention groups (in groups of 2 to 3). Controls stayed in usual classroom.
Objective	To examine the effects of instruction in successive and phonological processing upon the phonological and early reading skills of kindergarten children judged to be at-risk for later reading failure.
Study design	Individual. "The 43 subjects with parental consent to participate in the study were randomly assigned, by stratified randomization to three conditions" (p.24)
Participants	161 kindergarteners assessed on successive and phonological processing. **Low performers selected.** N = 43 subjects with parental consent. Two dropouts and mean age/gender is based on n = 41. 24 boys. Same number of males/females in each group. No child in any group able to read any words on word attack/word identification at pre-test. Age/Grade: Mean age 69 months.
Intervention	1. Successive phonological group, n = 15. Oriented to blending phonemes, rhyming words and sounds. Five main tasks: linear and non-linear matrices, joining shapes, window sequencing and sequential analysis. 2. Meaning oriented group, n = 15. Designed to be similar to regular activities in classroom. Focused on the meaning of the story and words within the stories. Words were not analyzed phonetically and no special attention was given to individual letters in words. Both groups had 8 week intervention. 2 to 3, 20 minute sessions per week (number of sessions varied from 17 to 20). Both received letter-identity and letter-sound instruction for nine letters and both read to by the instructors.
Control	Remained in class and received regular classroom instruction, n = 13.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

O'Connor, R.E. and Padeliadu, S. (2000) Blending versus whole word approaches in first grade remedial reading: Short-term and delayed effects on reading and spelling words, *Reading and Writing: An Interdisciplinary Journal*, 13(1-2): 159-82.	
Country of origin	USA
Setting	1:1 tutoring – taken out of class
Objective	Selected children with significant deficits in reading words in phonological awareness skills and in spelling toward end of 1st grade to test whether specific instruction in phonological decoding and blending or whole word reading, each combined with letter-sound instruction and spelling, could have an immediate and delayed effect on their learning a small core of words and transferring their learning to novel words.
Study design	Individual. "**Lowest skilled children** randomly assigned to the blending or whole word condition" (p.168).
Participants	12 children in four 1st grade classes in urban elementary school were selected. 2 girls, 10 boys. 4 African American, 8 European American. All read 5 or fewer words correctly on letter-word identification test of WRMT of Achievement, scored one or more SDs below the 1st grade mean score on the blending test, had full scale IQ scores >59, nominated by teachers as very poor readers. Included 4 children receiving special education services for mild disabilities (2 with serious emotional disturbance and 2 with learning disabilities). Age/Grade: 1st grade.
Intervention	Blending, n = 6. Phonological decoding and blending. Shown words one at a time and taught how to blend the first two letter sounds and then to add the final consonant.
Control	Whole word approach, n = 6. Cumulative introduction of whole words. Shown each word one at a time. Instructors read the word, then child read it. Both groups – 10 1:1 training sessions of 10 to 13 minutes each. Follow-up after last session and 8 days later.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Skailand, D.B. (1971) ***A comparison of four language units in teaching beginning reading,*** **Paper presented at Annual Meeting American Educational Research Association, New York, 7 February 1971**	
Country of origin	USA
Setting	Four kindergarten classes at an elementary school in Oakland, California.
Objective	To compare the effectiveness of four language units and beginning reading approaches: the grapheme phoneme (synthetic), the morpheme (similar spelling pattern), the morphophone morphographeme (contrastive spelling pattern) and the whole word (sight) approach. To teach kindergarten children to read a limited number of words and syllables.
Study design	Individual RCT. The research design was a modified experimental treatment/control group design. Placement in one of the four treatments was by random assignment after ranking within each of the four kindergarten classes according to pre-treatment scores on the Pintner-Cunningham Primary Test. (p.4)
Participants	86 kindergarten children in four classes at an elementary school in Oakland, California. Race stats given as: 76% Negro, 13% Spanish surname(?), 10% Other Caucasian, and 1% Oriental. Socio-economic level is given as 'breadwinners' employment' categories: approx two-fifths blue-collar, one-fourth each white collar and unemployed, and the remainder service or tradesmen. Four treatments were repeated four times on each teaching day, so that there were sixteen groups of approximately six children each receiving instruction for periods of fifteen minutes – twice weekly for ten weeks, commencing January 1970. All instruction was by the experimenter. Claims made that 'either spelling pattern treatment favoured girls' but no gender stats given.
Intervention	Grapheme/phoneme treatment involved the production of the sounds represented by each letter and then the blending or synthesis of the letters into the word or syllable. Morpheme - presented the words in pairs according to similarity of spelling patterns. Morphophone morphographeme– utilized contrastive predictable spelling patterns.
Control	Whole word treatment – graphic form was presented simultaneously with its oral counterparts.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

<table>
<tr><td colspan="2">Torgesen, J.K. et al. (1999) Preventing reading failure in young children with phonological processing disabilities: group and individual responses to instruction, Journal of Educational Psychology, 91(4): 579-93.</td></tr>
<tr><td>Country of origin</td><td>USA</td></tr>
<tr><td>Setting</td><td>Within school – 13 elementary schools.</td></tr>
<tr><td>Objective</td><td>To examine the effectiveness of several instructional procedures for a specific subset of children who are at risk of reading difficulty – delayed development of phonological skill on entering school.
4 conditions: no treatment control, regular classroom support, embedded phonics phonological awareness plus synthetic phonics.</td></tr>
<tr><td>Study design</td><td>Individual RCT.
Children were randomly assigned within school to one of four conditions.</td></tr>
<tr><td>Participants</td><td>180 children who obtained lowest combined scores on letter naming task and phoneme elision task (screening battery) and who had an estimated verbal intelligence score above 75.
<table>
<tr><td></td><td>NTC
M (SD)</td><td>RCS
M (SD)</td><td>PASP
M (SD)</td><td>EP
M (SD)</td></tr>
<tr><td>Age (months)</td><td>66.0 (3.6)</td><td>64.9 (3.3)</td><td>65.8 (3.8)</td><td>65.2 (3.2)</td></tr>
<tr><td>Gender</td><td>22M, 22F</td><td>26M, 19F</td><td>24M, 21F</td><td>23M, 22F</td></tr>
<tr><td>Race</td><td>24AF, 21W</td><td>23AF, 21W</td><td>22AF, 22W</td><td>25AF, 20W</td></tr>
</table></td></tr>
<tr><td>Intervention</td><td>Subjects in treatment conditions received four 20-minute sessions of one-to-one instruction per week for two and a half years, beginning second semester of kindergarten until end of 2nd grade.
RCS – regular classroom support.
EP – embedded phonics.
PASP – phonological awareness plus synthetic phonics – explicit instruction in phonemic awareness.</td></tr>
<tr><td>Control</td><td>Received no treatment (NTC).</td></tr>
</table>

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Torgesen, J.K. *et al.* (2001) Intensive remedial instruction for children with severe reading disabilities: Immediate and long-term outcomes from two instructional approaches, *Journal of Learning Disabilities*, 34(1): 33-58.	
Country of origin	USA
Setting	Room provided on school grounds.
Objective	To determine whether two approaches that both contain explicit instruction in word level skills but vary systematically in their depth of instruction in phonemic awareness and extent of practice in decontextualized phonemic decoding skills would affect specific reading skills in different ways.
Study design	Individual. "Children identified as eligible for the study were randomly assigned to one of two groups". (p.37)
Participants	60 children, aged 8-10 **previously identified as learning disabled.** Each year for 3 years 20 children were selected from LD classes in three elementary schools in the state of Florida Inclusion criteria: • Identified by teachers as having serious difficulty acquiring word level reading skills. • Average standard score on two measures of word-level reading was at least 1.5 SDs below age average. • Established verbal intelligence above 75. Performed below minimum required levels for their grade on a measure of phonological awareness. Exclusion criteria: • Adopted. • Evidence of acquired neurologic disease. • Experienced perinatal encephalopathic event. • Sensory deficits. • Evidence of chronic medical illness. • Showed some form of severe psychopathology. • English was second language. ADD group 22 male:8 female, 18 white:12 black. EP group 21 male:9 female, 21 white:9 black. Age/Grade: ADD 117.6 months (10.5), EP 117.6 months (12.6).
Intervention	Embedded Phonics (EP) n = 30. Stimulated phonemic awareness through writing and spelling activities, taught phonemic decoding strategies directly and spent a much greater percentage of instructional time in reading and writing connected text. Phonemic awareness was stimulated during spelling and writing activities, and word identification strategies were practised extensively while participants read the text.
Control	Auditory Discrimination in Depth Program (ADD) n = 30. Now called/revised to The Lindamood Phoneme Sequencing Program for Reading, Spelling and Speech. Stimulated phonemic awareness via articulatory cues and spent almost all the instructional time building phonemic/articulatory awareness and individual word-reading skills. Both groups: 1:1 basis in two 50-minute sessions each day of the week. Training provided over period of 8 to 9 weeks until 67.5 hours of instruction completed. Followed up 2 to 3 weeks immediately following end of intensive training period plus 1 and 2 year intervals.

APPENDIX J: Data extraction tables for all studies included in the meta-analyses, cont.

Umbach, B., Darch, C. and Halpin, G. (1989) Teaching reading to low performing first graders in rural schools: A comparison of two instructional approaches, *Journal of Instructional Psychology*, 16(3): 112-21	
Country of origin	USA
Setting	Usual classrooms, two 1st grade classrooms.
Objective	To determine if there was a difference between the reading performance of low-performing students taught by a traditional basal approach and a more structured direct instruction approach.
Study design	Individual. "These students were randomly assigned to either the experimental group or the comparison group". (p.114)
Outcome measures	WRMT: letter identification, word identification, word attack, word comprehension and passage comprehension. WRAT (post-test only)
Participants	31 students from two 1st grade classrooms in a rural community in the Southeast United States. Low income area. **Students nominated by regular teachers as students having difficulty with reading and needed extra help.** Direct instruction / Basal Male 10 / 9 Female 5 / 7 Black 15 / 15 White 0 / 1 Age/Grade: 1st grade.
Intervention	Direct Instruction (n = 15). From the Reading Mastery Series (1986). Structured scripted teacher presentation manuals. Students taught every required academic skill. Uses synthetic phonics approach – all skills broken down into small steps and opportunity for repeated practice provided. Students taught to blend sounds together before required to sound out simple words. Taught by 4 masters degree students.
Control	Basal Program (n = 16). Used Houghton-Mifflin reading series 1983. Was used in school system. Groups of 8 with regular teacher and university practica student. Teachers closely followed teachers guide and students presented with variety of activities in ever changing organizational structures. Correction procedures used in much less explicit manner. Both groups had approx 50 mins each day (8.30am to 9.20am) for entire school year.

Appendix K: Raw data (means, standard deviations and numbers): studies included in main analysis and secondary analysis

Mean accuracy scores

Study	IntN	IntM	IntSD	CntN	CntM	CntSD
Berninger	17	91.29	6.2	17	89.71	3.82
Brown	6	23.24	19.6	6	19.06	11.54
Greaney	18	24.25	6.89	18	22.25	6.56
Haskell	12	11.62	8.53	12	10.92	10.83
Johnston	30	5.4	0.3	29	5	0.5
Leach	10	7.11	0.47	10	6.76	0.36
Lovett89	60	61.3	6.96	61	59.75	7.35
Lovett90	18	43.2	30.33	18	49.55	33.52
Martinussen	13	1.4	3.83	15	0.2	0.54
O'Connor	6	17.5	4.9	6	14.5	5.6
Skailand	23	5.3	3.94	19	6.26	7.23
Torgesen99	45	0.98	1.65	45	0.87	1.46
Torgesen01	24	86.1	10.6	26	89.4	10.46
Umbach	15	63.96	9.93	16	32.28	12.69

Accuracy 1 scores

Study	IntN	IntM	IntSD	CntN	CntM	CntSD
Berninger	17	91.29	6.2	17	89.71	3.82
Brown	6	33.52	25.23	6	33.08	15.49
Greaney	18	29.11	6.69	18	26.5	6.48
Haskell	12	18.5	11.7	12	17.25	14.83
Johnston	30	5.4	0.3	29	5	0.5
Leach	10	7.11	0.47	10	6.76	0.36
Lovett89	60	81.9	6.2	61	80.8	6.25
Lovett90	18	49.5	35.2	18	58.4	38.4
Martinussen	13	1.8	4.9	15	0.1	0.3
O'Connor	6	17.5	4.9	6	14.5	5.6
Skailand	23	5.3	3.94	19	6.26	7.23
Torgesen99	45	0.76	1.7	45	0.14	0.53
Torgesen01	26	90.3	8.3	24	96.4	7
Umbach	15	30.43	9.75	16	17	10.36

Comprehension scores

Study	IntN	IntM	IntSD	CntN	CntM	CntSD
Leach	10	7.06	0.47	10	6.84	0.25
Lovett89	60	27.4	6.2	61	26.9	6.25
Torgesen01	26	92	19.8	24	91	9
Umbach	15	9.46	5.3	16	3.83	4.82

Spelling 1 scores

Study	IntN	IntM	IntSD	CntN	CntM	CntSD
Brown G1	6	5.61	4.57	6	2.83	2
Lovett89	60	78.8	6.2	61	78.8	6.25
Martinussen	13	4.5	3.9	15	3.5	2.4
O'Connor	6	4.7	1.5	6	5	2.2

Synthetic vs analytic scores

	IntN	IntM	IntSD	CntN	CntM	CntSD
Johnston	30	5.4	0.3	33	5	0.3
Skailand	23	5.3	3.94	23	12.13	8.37
Torgesen average	45	0.98	1.65	45	1.49	2.38
Torgesen99	45	0.76	1.7	45	0.28	1.3
Torgesen 99	45	1.2	1.6	45	2.7	3.1

KEY:
IntN = Intervention number
IntM = Intervention mean
IntSD = Intervention standard deviation
CntN = Control number
CntM = Control mean
CntSD = Control standard deviation

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