The science of reading provides little or no support for the widespread claim that systematic phonics should be part of initial reading instruction: A response to Buckingham

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Abstract

It is widely claimed that the science of reading supports the conclusion that systematic phonics should be part of initial reading instruction. Bowers (2020) challenged this conclusion after reviewing all the main evidence, and Buckingham (2020a) provided a detailed response where she argues that the evidence does indeed support systematic phonics and criticizes an alternative form of instruction called "Structured Word Inquiry" or (SWI).   Here we show that every substantive criticism Buckingham makes is factually incorrect or reflects a fundamental mischaracterization. There is nothing in her article that challenges the conclusions that Bowers (2020) draws regarding systematic phonics, and nothing that challenges the claims we have made in the past regarding SWI. This should not be used to support whole language or balanced literacy, but it should motivate researchers to consider alternative methods that are well motivated on theoretical grounds, such as SWI.

We are pleased to receive a detailed response to the Bowers (2020) article entitled "Reconsidering the evidence that systematic phonics is more effective than alternative methods of reading instruction", but every substantive criticism Buckingham (2020a) makes is factually incorrect or reflects a fundamental mischaracterization (see Table 1 for a summary of some of the errors and mischaracterizations). We agree that researchers should be open to investigating new hypotheses regarding reading instruction, but it is also important for conclusions to be based on an accurate reporting of theoretical claims and empirical findings. The Buckingham response is problematic as it introduces and reinforces a wide range of falsehoods. Here we highlight her most important errors and encourage readers to read the following blogpost (https://jeffbowers.blogs.bristol.ac.uk/buckingham-2020/) where you can learn more about the history of this paper (why this paper is published in PsyArxiv rather than *The Educational and Developmental Psychologist*) and where there is an opportunity to respond to the points we have made here.

What is systematic phonics?

Before reviewing the evidence, it is important to address some points of agreement and disagreement regarding what systematic phonics entails. We agree that systematic phonics explicitly teaches the main GPCs (or sometimes onset-rimes or other letter-sound correspondences) in a planned sequence, and in addition, we agree that advocates of phonics claim that phonics should be embedded in a broader literacy curriculum. For instance, Bowers (2020) wrote: "The NRP (2000) emphasizes that systematic phonics should be integrated with other forms of instruction, including phonemic awareness, fluency, and comprehension strategies". (p. 683). That is, advocates of phonics adopt the position that phonics is necessary but not sufficient. As far as we are aware, no one has criticized the "strawman" position that phonics is sufficient for reading instruction.

However, there is disagreement regarding how phonics is combined with other forms of instruction. As Bowers and Bowers (2018a) note, a key motivation for phonics is that the meaning of written words is accessed via phonology, and based on this, it is claimed that reading instruction should focus on GPCs before focusing on the meaning of written words, what we called the "phonology first" hypothesis. Buckingham rejects the phonology first hypothesis and claims this mischaracterization of phonics led Bowers (2020) to draw some unjustified conclusions. It is worth quoting a lengthy passage from Buckingham as it is key for interpreting the empirical review that follows. She writes:

> "Bowers' definition of systematic phonics is a serious faultline in his article, with the result that his argument is based on a false premise. According to Bowers (2020): 'systematic phonics explicitly teaches children grapheme-phoneme corre- spondences prior to emphasizing the meanings of written words in text (as in whole language or balanced literacy instruction) or the meaning of written words in isolation (as in morphological instruction). (p. 3)'
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> This definition incorrectly implies that systematic phonics instruction mandates teaching the entire graphophonemic code before considering meaning and morphology. Systematic phonics instruction takes place alongside meaning-based instruction, including morphology, vocabulary and comprehension. Bowers' mischaracterization of systematic phonics permeates the article and perhaps explains why Bowers seems to believe that evidence for the positive effect of nonphonics instruction presents a challenge to the conclusion that systematic phonics is effective. It is a matter of and, not or.' (p. 106)

Table 1

Summary of errors and mischaracterizations in Buckingham (2020a)

Buckingham Claim	Accuracy	Comment
Buckingham claims phonics occurs alongside morphological instruction.	Misleading -Buckingham also writes: "…Logically, a child cannot read morphemes if they cannot read graphemes" (p. 106) and "…once children have mastered decoding, other aspects of reading instruction become stronger variables in their reading ability". (p. 108)	-We cite multiple authors who highlight how morphological instruction should follow phonics. -A key feature of SWI that distinguishes it from phonics is that GPCs are taught in the context of morphology, at the same time, because they interact.
Regarding NRP Buckingham writes: "…the effect sizes are moderate, including for synthetic phonics, and are certainly stronger than the evidence found for any other method, including whole language." (p. 107)	False: - The NRP only included 4 studies that compared synthetic phonics to whole language, and the effect sizes were: $d= 0.91$, $d = 0.12$ $d = 0.07$, and $d = -0.47$.	- The NRP also reported no evidence that phonics had a long-term impact on spelling, reading text, and comprehension, the evidence that systematic phonics should be introduced early was inconclusive.
Regarding Camilli (2006) meta-analysis, Buckingham claimed there was an advantage of systematic over unsystematic phonics.	Misleading -As noted by Buckingham herself, the effect size of $d = .123$ was not statistically significant. A nonsignificant effect is a failure to show an advantage.	-This $d = .123$ effect would be further reduced following a delay of 4-12 months, for children with reading difficulties, for spelling, reading text, and comprehension, and is inflated by the fact that there is evidence for publication bias in NRP.
Regarding Torgerson et al. (2006) meta-analysis, Buckingham claims that the word reading results were moderate and significant after an outlier study was removed (by the authors themselves).	False/Misleading. - In Torgerson et al.'s updated analysis the spelling effect was significance with a fixed effects model ($d = .20$ $p = 0.03$) and nonsignificant on a random effects model ($d = .21$, $p = 0.09$). Neither effect size is moderate.	-Torgerson et al. also highlighted limited quality of the studies in their meta-analysis, evidence of publication bias, and no significant effect for spelling or comprehension.
Regarding McArthur et al. (2012) meta-analysis, Buckingham claims that there is no reason to remove the Levy studies on the basis that the study and test words were designed to be high	False: -The effect size of an intervention will increase when children are trained to name words maximally similar to the test words. However, the meta-analysis was used to make claims about the effect size of phonics in general, not the special case of training to the test.	-To illustrate the problem, consider the PSC in England (now being introduced in Australia). Teachers are not informed what words are on the test so that they can't "cheat". If the teachers were given the test words performance on PSC would improve without improve reading outcomes in general.
Regarding Galuschka et al. (2014) meta-analysis, Buckingham clams the findings support the conclusion that phonics was the most effective method assessed.	False: -In order to conclude that phonics was the most effective the authors need to show an interaction, with phonics more effective than the other interventions. This was not tested, nor would it be significant given the findings.	-A valid conclusion would be something like: Similar effect sizes were obtained across multiple different forms of instruction, with only phonics significant. However, there was no evidence that phonics was more effective than other methods.
Buckingham claims that SWI does not teach GPCs	False: -We provided multiple quotes in the paper that explicitly emphasize the importance of teaching GPCs in SWI.	- J. Bowers directly communicated with Buckingham multiple times explaining that SWI teaches GPCs explicitly prior to this commentary.
Buckingham claims that the reading and writing SAT scores improved more than math and science scores between 2011-2015.	Misleading: -As shown in Figure 1, the increase is extremely similar.	-Equally important, all scores started increasing the year before the PSC took effect.
Buckingham claims that that Walker et al. (2015) analysis of SAT scores does not contradict her claim that reading and writing improved after introduction of PSC	Misleading -Bowers did not question that scores went up after introduction of PSC, but cited Walker et al. who noted that SAT scores started increasing prior to the introduction of PSC.	-The fact that reading, writing, math, and science SAT scores all went up at a similar rate, starting before the introduction of the PSC, undermines the claim that the PSC contributed to improved SAT scores.
Buckingham claims that the Devonshire et al. study that provided evidence for SWI used a control condition that included both phonics and whole word instruction	False/Misleading: -The control group were only taught phonics in school. All children, including children in SWI condition, were given homework that might be construed as whole word instruction.	-It is unclear why the homework would interfere with classroom phonics but not SWI. -This is a much stronger control group than most studies used in the meta-analyses..
Regarding SWI, Buckingham claims that there "is no evidence of effectiveness at all".	False -We report a number of studies that provide evidence in support of SWI, including in children 5-7 years old.	-We are clear that the evidence for SWI is preliminary and more research needs to be done. The main motivation of the Bowers (2020) critique was to motivate researchers to explore other approaches, including SWI.

Note, d or Cohen's d is a common effect size measure and refers to the difference between two means divided by a standard deviation for the data.

But the phonology first hypothesis is key to phonics, and it manifests itself in two main ways. First, despite the fact that English spellings represent both phonology and meaning (via morphology and etymology; see Bowers & Bowers, 2018b), phonics explicitly teaches GPCs before and independently of these meaning-based regularities. The centrality of this claim is not only highlighted by the many statements that morphological instruction should follow phonics (e.g., Adams, 1990; Castles, Rastle, & Nation, 2018; Ehri & McCormick, 1998; Frith, 1985; Rastle, 2019; Rastle & Taylor, 2018; for a challenge to these claims see Bowers & Bowers, 2018a), but also by the fact that not a single phonics intervention in all the meta-analyses described below (constituting well over 100 studies) taught children the interaction between morphology and phonology. An unfortunate consequence of this strong focus on phonology is that teachers know very little about morphology (Washburn, & Mulcahy, 2019), reflecting the fact that so little attention is given to the meaningful organization of spellings at either the initial or later stages of instruction. Second, phonics discourages children from using meaning that can be derived from a sentence context or picture cues to "guess" the pronunciation of a word. That is, neither the meaning of parts of words (morphology or etymology) nor the meaning of the sentence/context should be used prior to teaching GPCs.

Although Buckingham appears to reject the phonology first hypothesis when she claims that phonics is taught alongside morphology and comprehension strategies (see above quote), in other places she is clear that GPCs do come first in phonics. For example, she writes: "Systematic phonics teaching is supported by research showing that for beginning readers, meaning is activated via a phonological pathway in the brain" (p. 106), or "Logically, a child cannot read morphemes if they cannot read graphemes" (p. 106), or "…once children have mastered decoding, other aspects of reading instruction become stronger variables in their reading ability" (p. 108). We do agree that systematic phonics does commonly occur alongside oral vocabulary instruction at the very start, but with regards to <u>written</u> words, GPCs come first.

We thus reject the claim that Bowers (2020) defined phonics "based on a false premise" (p. 106). But in any case, the most basic point to note is that Bowers (2020) accepted the definitions of the authors of the 12 meta-analyses described below and the government report that led to the legal requirement to teach of systematic phonics in state schools in England (Rose, 2006). His conclusions cannot be attributed to a mischaracterization of systematic phonics unless these other authors have also mischaracterized systematic phonics.

This context should help clarify the question that Bowers (2020) posed, namely, whether phonics, as defined by all the authors of the meta-analyses and by Rose (2006), is more effective than common alternative methods used in schools. Bowers' review of the meta-analyses addressed the effectiveness of targeted phonics interventions, and his review of reading outcomes in England addressed the effectiveness of phonics in a broader reading curriculum. Importantly, the goal was not to pit phonics vs. whole language (the focus of the "reading wars"), but to highlight how little evidence there is for either approach. Our hope is that this realization will motivate researchers to explore alternative ways to teach GPCs and reading more generally.

Review of the meta-analyses taken to support systematic phonics.

Given the strong consensus in support of systematic phonics in the research community it should be easy to refute the Bowers (2020) claim that there is little or no evidence that systematic

phonics is more effective than common alternative approaches. In this section we consider Buckingham's characterization of the meta-analyses that she takes to support systematic phonics.

National Reading Panel (2000)

Bowers (2020) detailed a host of problems with using the NRP (2000) meta-analysis to make strong claims regarding the effectiveness of systematic phonics, including the fact there was no evidence that spelling, reading text, and reading comprehension improved following a delay of 4 to 12 months (the effects were not reported nor assessed), nor significant short- or long-term benefit for struggling readers above grade 1 with average or below average intelligence (that constitute at least half of struggling readers in grade 1). Furthermore, the quality of the studies include in the NRP meta-analyses were often problematic, with only 13 of the 38 comparisons using randomized controlled trials (more detail below on this). Buckingham simply ignored all this, writing:

> "However, the effect sizes are moderate, including for synthetic phonics, and are certainly stronger than the evidence found for any other method, including whole language." (p. 107)

But as detailed in Bowers (2020), there were only 4 studies that compared synthetic phonics to whole language, and the effect sizes were: $d = 0.91$, $d = 0.12$ $d = 0.07$, and $d = -0.47$.

Camilli et al. (2003, 2006)

A key insight of Camilli et al. (2003, 2006) was that the main analysis of the NRP compared systematic phonics to a control condition that included two different forms of studies, namely, interventions that employed no phonics and interventions that included some phonics but taught unsystematically. As noted by Camilli et al., this is problematic if researchers want to test the hypothesis that phonics should replace alternative approaches common in schools (such as whole language) given that the alternatives almost always include some degree of unsystematic phonics. In order to test this hypothesis, the control condition should exclude the intervention studies that included no phonics. This motivated Camilli et al. (2003, 2006) to carry out new meta-analyses that directly assessed the question of whether systematic phonics is more effective than unsystematic phonics. The key finding from Camilli et al. (2006) meta-analysis that included new covariates (including the degree to which phonics was taught systematically) was that systematic phonics was not significantly more effective than unsystematic phonics ($d = .123$, $p > .05$). Bowers (2020) noted that all subsequent meta-analyses also included the wrong control condition to test the hypothesis that systematic phonics should replace alternative forms of instruction common in schools.

Buckingham dismisses the distinction between systematic and unsystematic phonics, writing: "the latter category is so nebulous that it is difficult to see how unsystematic phonics and no phonics could be easily distinguished." (p. 107) However, the authors of the NRP (2000) themselves made this distinction writing:

> Whereas in the 1960s, it would have been easy to find a 1st grade reading program without any phonics instruction, in the 1980s and 1990s this would be rare. Whole language teachers typically provide some instruction in phonics, usually as part of

invented spelling activities or through the use of graphophonemic prompts during reading... However, their approach is to teach it <u>unsystematically</u> [underline added] and incidentally in context as the need arises. (p. 2-102).

It is not clear why Buckingham claims that it is difficult to identify interventions that included no phonics and exclude them (leaving studies in the control condition that had unsystematic phonics). Indeed, the McArthur et al. (2012) meta-analysis described below specifically compared systematic phonics to studies that included no phonics (because the control group got no reading instruction), and it is straightforward to remove studies that assessed "whole word" interventions that teach no phonics. If there was a problem with how Camilli et al. coded the degree to which interventions taught GPCs systematically this could have been explained.

Buckingham also claims that Bowers was incorrect in writing: "Camilli et al (2006) failed to show an advantage of systematic over unsystematic phonics" (p. 107), but that is exactly what they found (given the d = .123 was not significant). And even if the nonsignificant d = .123 is taken to support systematic phonics over unsystematic phonics, the effect was largely driven by short-term measures of decoding. Indeed, given this meta-analysis was largely composed of the same studies included in the NRP, we can expect much weaker effect sizes for reading fluency, reading text, spelling, and reading comprehension, and weaker effects still with children with reading difficulties, and weaker effects still following a 4-12-month delay.

Torgerson Brooks, & Hall (2006)

The meta-analysis by Torgerson et al. (2006) further undermined the conclusions that can be drawn from the NRP. The authors noted is that few of the studies included in the NRP were randomized control trial (RCT) studies, and further, the quality of these RCT studies was problematic. Here is a passage from this meta-analysis that specifically focused on 14 RCT studies that existed at that time (including one unpublished study):

"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". (pp. 33-34)

Despite these limitations, and despite the fact that Torgerson et al. (2006) failed to obtain a significant effect for spelling or comprehension, Buckingham (2020a) takes the significant word reading accuracy results (d = 0.27 to 0.38; depending on whether fixed or random effects models were used) as further evidence for systematic phonics. A problem with this conclusion is that the word reading results were inflated on the basis of a flawed study with an effect size of 2.69 that Torgerson et al. <u>themselves</u> removed in a re-analysis. In their updated analysis the spelling effect just reached

significance using a fixed effects model (d = .20, p = 0.03) and was nonsignificant using a random effects model (d = .21, p = 0.09). It is misleading for Buckingham to summarize the word reading results as moderate and significant after this study was removed.

In addition, Torgerson et al. reported evidence of publication bias in support of systematic phonics. This bias will have inflated the results from the NRP (2000) and the Camilli et al. (2003, 2006) meta-analyses, and the fact that Torgerson et al. (2006) included one additional unpublished study does not eliminate this problem in their own meta-analysis. And again, the design of the study did not even compare systematic to unsystematic phonics, and accordingly, the meta-analysis did not even test the hypothesis that systematic phonics is more effective than standard alternative forms of classroom instruction. Nevertheless, Buckingham approvingly quotes Torgerson et al. who wrote: "Systematic phonics instruction within a broad literacy curriculum appears to have a greater effect on progress in reading than whole language or whole word approaches". (p. 10)

McArthur et al. (2012)

The McArthur et al. meta-analysis was designed to assess the efficacy of systematic phonics with children, adolescents, and adults with reading difficulties. The authors reported significant effects of word reading accuracy and nonword reading accuracy, whereas no significant effects were obtained in word reading fluency, reading comprehension, spelling, and nonword reading fluency. However, Bowers (2020) noted that the significant word reading accuracy results depended on two studies by Levy and colleagues (Levy, Bourassa, & Horn, 1999; Levy, & Lysynchuk, 1997) that provided inflated estimates of the efficacy of systematic phonics. Once these studies are removed, the only remaining significant effect was nonword reading accuracy.

Buckingham criticizes the rejection of these two studies and suggests that Bowers (2020) removed them because they were especially effective. However, it was the design of the studies that led to their exclusion. Consider the Levy and Lysynchuk (1997) study. Children were trained to name 32 words following 15 days of one-on-one instruction (or until a child could name all the 32 words accurately) and then were tested on another set of words designed to be as similar as possible to the study words. For example, children were taught to name a set of words with an "an" rime (e.g., <can>, <ban>, <man>, <pan>) and then tested on a new word that shared this rime (e.g., <fan>). The fact that study and test words are so similar means that the results cannot be taken as evidence that systematic phonics improves word reading accuracy in general (the claim of the McArthur et al. meta-analysis). The same approach was taken in Levy et al., (1999). As an analogy, consider the administration of the Phonics Screening Check that assesses word and nonword naming for all Year 1 children in state schools in England. In order to determine whether systematic phonics has been effective teachers are not aware of the test words so they cannot "cheat" by training on the same or similar words. By contrast, the two Levy et al. studies were designed to ensure high overlap between study and test words. To be fair to Levy et al., the authors were comparing the effectiveness of various forms of instruction when the study and test words were matched across conditions, and the authors were not claiming that effect sizes they observed provided an estimate of the efficacy of systematic phonics more generally. Consequently, the inclusion of these two studies in a meta-analysis designed for just this purpose is misguided.

In addition, McArthur et al. compared small group phonics instruction to a control condition that included <u>no</u> instruction in reading. So again, the significant nonword naming results (in the face of nonsignificant effects for word reading accuracy, word reading fluency, reading comprehension, spelling, and nonword reading fluency) cannot be used to make any conclusions regarding the relative effectiveness of phonics to standard classroom instruction. The same fundamental point applies when you include the extra 3 trials in the updated Torgerson (2018) meta-analysis – the authors are still comparing systematic phonics to no phonics, and thus is irrelevant to the question of whether systematic phonics is more effective than standard alternative teaching methods. Any form of small group reading instruction may have produced similar small (mostly nonsignificant) effects compared to no instruction.

Galuschka et al. (2014)

Galuschka et al. carried out a meta-analysis that assessed the impact of various forms of instruction on children with reading difficulties. They reported an overall effect size for phonics ($g' = 0.32$) that was similar to the outcomes with phonemic awareness instruction ($g' = 0.28$), reading fluency training ($g' = 0.30$), auditory training ($g' = 0.39$), and color overlays ($g' = 0.32$). However, only the phonics result were significant, and based on this the authors concluded "…the systematic instruction of letter-sound correspondences and decoding strategies, and the application of these skills in reading and writing activities, is the <u>most </u>effective method for improving literacy skills of children and adolescents with reading disabilities". [underline added] (p. e89900)

Bowers (2020) noted that in order to conclude that systematic phonics is the most effective method, the authors need to report an interaction, with a greater effect size for systematic phonics compared to alternative methods. Not only was this not reported, some of the other effect sizes were numerically larger than phonics. Buckingham rejects the need for a significant interaction to draw this conclusion, and furthermore, criticizes my reasoning regarding the interaction, writing:

> "…it is inconsistent with his criticisms of Torgerson et al. (2006) and Suggate (2010), where he dismisses effects based on their lack of statistical significance. One might argue whether statistical significance is important or not, but not change position within one paper based on a preferred interpretation of the results". (p. 108)

However, Bowers (2020) applied a consistent criterion across studies: Galuschka et al. need to observe a significant interaction if they want to claim that systematic phonics is more effective than the other methods they reviewed, and Torgerson et al. (2006) and Suggate (2010) need to report significant effects for systematic phonics if they want to claim that systematic phonics was effective. More generally, if the claim is that the science of reading supports systematic phonics, then the importance of statistical significance should not be an issue worthy of debate.

In addition to these statistical issues, Galuschka et al. reported evidence for publication bias that inflated the effect size of systematic phonics, and when they corrected for the bias, the effect was reduced to $g' = 0.198$ (still significant). And once again, this meta-analysis did not even test the hypothesis that systematic phonics is more effective than the standard alternative methods that include some degree of phonics.

Suggate (2010, 2016)

Suggate (2010) reported evidence that the short-term impact of systematic phonics was most effective at the start of instruction, but Buckingham does not respond to my main critique of the study, namely, that the advantage of early phonics was very small (d = ~.1), there is no indication that this advantage was significant, and the study that showed the largest benefit of early phonics (d = 1.37) was carried out in pointed-Hebrew (a shallow orthography where GPCs are highly regular), a result that should not be used when making conclusions regarding the effectiveness of early systematic phonics in English. This is not an Anglo-centric position, it is a recognition of the fact that different orthographies are organized in different ways, and in contrast with pointed-Hebrew and other "shallow" orthographies that systematically map graphemes to sounds, English is a morphophonemic system where letters encode both phonology and meaning. Phonics may be more effective in shallow orthographies.

Suggate (2016) reported the first (and only) meta-analysis that focused on the long-term impact of various teaching methods, and it showed that effects of systematic phonics were short-lived (consistent with the NRP), and less effective than some alternative forms of instruction. This is from the abstract of the paper:

> "Overall, comprehension and phonemic awareness interventions showed good maintenance of effect that transferred to nontargeted skills, whereas phonics and fluency interventions, and those for preschool and kindergarten children, tended not to." (p. 77)

But Buckingham dismisses these results, writing: "Phonics interventions will have stronger effects on proximal decoding outcomes than on more distal comprehension assessments." (p. 108)

We do not understand how Buckingham can brush aside the poor long-tern outcomes of systematic phonics in recent meta-analysis that assessed the long-term effects across a range of methods. Systematic phonics is widely claimed to be an essential form of early instruction, and when there are no long-term effects of early systematic phonics, Buckingham claims it is to be expected. It seems the benefits of systematic phonics are unfalsifiable.

Remaining meta-analyses:

Finally, Buckingham dismisses the critique of the remaining meta-analyses and a recent review writing: "In each case, positive findings for systematic phonics are downplayed, irrespective of the actual findings and the conclusions of the authors." (p. 108) Not only does Buckingham fail to respond to any of the points raised by Bowers (2020), her claim that Bowers (2020) downplayed all positive effects is false. Indeed, Bowers criticized Hammill and Swanson (2006) for downplaying the importance of significant small effect sizes in the NRP (the problems with the NRP lie elsewhere, as detailed above).

More recently, in an article entitled "Evidence strongly favours systematic synthetic phonics", Buckingham (2020b) has published a summary of Buckingham (2020a) in which she includes a table that summarizes the results from 9 meta-analyses that she takes to strongly support systematic phonics. But this table includes errors, and it mischaracterizes the evidence. In Table 2 we reproduce the table

from Buckingham (2020b) in the first two columns (same identical information is provided), and in the third column we highlight multiple problems. Based on a review of the meta-analyses Bowers (2020) wrote: "There can be few areas in psychology in which the research community so consistently reaches a conclusion that is so at odds with available evidence." (p. 696) There is no reason to change this conclusion based on Buckingham's response.

Reading outcomes in England post 2007

A possible response to these null results is to note that the studies in the meta-analyses were not concerned with embedding systematic phonics in a broader literacy context. Buckingham makes a similar point, writing: "Alone, systematic phonics is not a foolproof guarantee of reading success; its effectiveness is mediated by the quality of the rest of the literacy program." (p. 106) Note, it is hard to reconcile this response with the fact that these same meta-analyses have been cited 1000s of times in support of systematic phonics, and in any case, this response only provides a possible explanation for why the meta-analyses provide little or no evidence in support of systematic phonics.

Given this limitation of the meta-analyses, the natural experiment in England is all the more interesting. Since 2007 all state schools are required by law to teach systematic synthetic phonics, with phonics embedded in a wider literacy context. Furthermore, since 2013, all state schools administer the Phonics Screening Check (PSC) that asks children in Year 1 (the equivalent to grade 1 in the USA) to name aloud a small set of regular words and nonwords in order to assess GPC knowledge. Since 2013 the scores appear to have gone up dramatically, from 58% students passing the check in 2012 to 82% in 2019, suggesting that the PSC has improved the teaching of phonics (see below for more details regarding the PSC and why this increase from 58% to 82% is misleading). An important question, then, is whether the introduction of systematic phonics in England has translated into better reading outcomes. Proponents of systematic phonics claim that reading outcomes have indeed improved, and Bowers (2020) challenged this conclusion. And once again, all of Buckingham's criticisms of the Bowers (2020) critique are either mistaken, mischaracterizations, or she simply does not address key points.

Machin, McNally and Viarengo (2018)

Machin, McNally and Viarengo (2018) assessed the impact of the early roll out of systematic phonics on reading scores from Standard Assessment Tests (SATs) at Key Stage 1 (when children were 7 years of age) and Key Stage 2 (when children were 11). These are standardized tests given to all students in state schools. Machin et al. claimed that there were long-term benefits of systematic phonics on the Key Stage 2 results, writing: "…this program helped narrow the gap between disadvantaged pupils and other groups". (p. 217)

Buckingham repeats this conclusion but does not address the Bowers (2020) critique of this study. That is, Machin et al. included two cohorts of students in their study (one cohort were given systematic phonics in the year 2005, the other cohort in 2006), and within each cohort they subdivided

Table 2

Errors and mischaracterizations from Buckingham (2020b)

Study	Effect size (Cohen's d or Hedges' g)	Comment
National Reading Panel (2000)/Ehri (2001)	Overall d = 0.67 (decoding regular words) d = 0.60 (decoding pseudowods) d = 0.40 (irregular words) d = 0.51 (reading comprehension) Type of phonics Synthetic phonics d = .45 (average for all measures) Analytic phonics d = 0.35 (average for all measures) Grade level Kindergarten = 0.56 (average for all measures) First grade d = .54 (average for all measures) Grade 2-6 d = 0.26 (average for all measures)	-These effects were all obtained immediately after instruction. Effects were greatly reduced following a delay of 4-12 months. - The overall short-term effects were all reported together in Table 1 of Ehri et al. (2001, p. 404), but Buckingham misreported the effect size for reading comprehension as d = 0.51 rather than d = 0.27. The former outcome was not an overall effect, but rather was only obtained in Kindergarten/First Grade, with other groups showed much reduced (or negative) effects. -As noted above, only 4 studies compared synthetic phonics to whole language, and the effect sizes were: 0.91, d = 0.12 d = 0.07, and d = − 0.47.
Camilli, Vargas & Y urecko (2003)	d = 0.24 (average for all measures)	-This is the overall effect size when comparing systematic to non-systematic phonics, and contrasts with .41 effect reported in the NRP.
Camilli, Wolfe & Smith (2006)	d = 0.123 *phonics only instruction	- This meta-analysis included the same set of studies as Camilli et al. (2003) and largely same set of studies as NRP. So not clear what "phonics only instruction" refers to. -d = 0.123 not significant.
Torgerson, Brooks, & Hall (2006)	d = 0.27/0.38 (fixed effects/random effects; word reading accuracy) d = 0.24/0.35 (fixed effects/random effects; reading comprehension).	-These d values reflect the analysis with the outlier study (d = 2.69) included. -The reading comprehension results were not significant.
Adesope, Lavin, Thompson, & Ungerleider (2018)	= 0.40 (average for all measures).	-This was a meta-analysis for second language learners. -From the abstract: "Collaborative reading interventions… produced larger effects than systematic phonics instruction".
Galuschka, Ise, Krick & Schulte-Korne (2014)	g= 0.322 (average for all measures).	-The effect for obtained for phonics was no larger than multiple other interventions. -g' = 0.198 after correcting for bias.
Suggate (2016)	Post-test d = 0.44 (average for all measures) d = 0.48 (pre-reading) d = 0.45 (reading skills) d = 0.48 (comprehension) follow-up d = 0.25 (average for all measures) d = 0.26 (pre-reading) d = 0.30 (reading skills) d = -0.03 (comprehension) *included unpublished and published studies **8 out of 22 phonics interventions were computer-based phonics training	-These d values are unweighted effect sizes reported in Table 3. Weighted estimated effect sizes also taken from Table 3 are as follows. Post-test: d = .29, .32, .26, .47, respectively. Follow-up: d = .07, .08, .07, -.10, respectively. -Main finding was that alternative interventions were often more effective. Key quote from abstract: "Overall, comprehension and phonemic awareness interventions showed good maintenance of effect that transferred to nontargeted skills, whereas phonics and fluency interventions, and those for preschool and kindergarten children, tended not to".
McArthur et al (2018)	d = 0.52 (mixed/regular word reading accuracy) d = 0.67 (nonword reading accuracy) d = 0.84 (irregular word reading accuracy) d = 0.45 (mixed/regular word reading fluency) d = 0.39 (nonword reading fluency) d = 0.28 (reading comprehension)	-all d values based on a comparison of systematic phonics to <u>no</u> reading instruction. -This updated meta-analysis McArthur (2012) includes the two Levy et al. studies that artificially inflated effect sizes. -Authors used the GRADE approach to assess the quality of the evidence, with the following outcomes reported in their Table 6: low, low, moderate, moderate, moderate, and low, respectively. They also reported low GRADE evidence for spelling, letter-sound knowledge and phonological output.

Table 1: Effect sizes for reading outcomes associated with systematic phonics instruction. Note, d and g are common measures of effect size. d refers to Cohen's and is the difference between two means divided by a standard deviation for the data. g refers to Hedges' g and is closely related to Cohen's d.

children in to eight groups (economically advantaged/disadvantaged children, native/non-native English speakers, and combinations of economic and language status). Neither cohort showed

significant long-term effects overall (consistent with the Suggate, 2016 meta-analysis), but there were some long-term effects for some of the subgroups: For the second cohort they observed significant gains for non-native speakers (d = .068, p < .05) and economically disadvantaged children (d = .062, p < .05), whereas for the first cohort, economically advantaged native English speakers tended to read more <u>poorly</u> after phonics (d = −0.061, p < 0.1), a result the authors could not explain. Importantly, there is no evidence that the long-term benefits were larger than the costs. It is also the case that more resources were provided for the schools in the phonics compared to the control conditions (for the first cohort, 18 local authorities each received £50,000 to support the appointment of a specialist consultant with a further £15,000 allocated to each school, and for the second cohort a further 32 local authorities each received £50,000 for a specialist consultant with a further £15,000 to each school). Accordingly, the short-term benefits that Machin et al. reported may reflect the additional resources rather than anything specific about systematic phonics instruction per se.

The Program for International Student Assessment (PISA) outcomes:

PISA assesses 15-year-old students on mathematics, science, and reading every 3 years since 2000 across multiple countries, and critically, the 2018 version is the first to assess the reading outcomes of children in England who were taught systematic phonics in year 1 as legally required in 2007. Buckingham agrees that the PISA results do not provide much evidence for systematic phonics, but it is interesting to note her explanation, namely, the early implementations of phonics instruction starting in 2007 in England was variable, and the cohort of 15-year-old students who completed the 2018 PISA tests would have only been exposed to poor quality phonics. This may well be true, but it is difficult to reconcile with her claim that systematic phonics had long-term effects in the Machin et al. (2018) study where phonics instruction was even less well established.

Progress in International Reading Literacy Study (PIRLS) outcomes:

PIRLS assesses reading comprehension in fourth graders across a wide range of countries every 5 years: 35 countries participated in 2001, 38 in 2006, 48 in 2011, and 50 in 2016. Buckingham accepts that the PIRLS results are more relevant to assessing the success of requiring systematic phonics in English state schools compared to the PISA 2018 results, but she disagrees with the Bowers (2020) analysis of the findings.

The key finding taken in support of the efficacy of systematic phonics is that England has moved up in the PISA league tables since 2006. In 2006, the year before systematic phonics was required in 2007, England was in 15th position (with a score of 539) and then moved up to 10th position in 2011 (score 552) and 8th in 2016 (score 559). Sir Jim Rose, author of the Rose (2006) report, took "the spectacular success of England shown in the latest PIRLS data" as further evidence in support of systematic synthetic phonics" (Rose 2017).

What is often ignored when making this argument is that PIRLS results in England were also excellent in 2001 (scoring 553), and indeed similar to the outcome in 2016. Buckingham argues that the 2001 results may have been inflated, quoting Hilton (2006) who wrote "the sampling and the test itself to have been advantageously organised" (p. 110) and quoting McGrane et al. (2017) who wrote that there was "relatively large error for the average score in 2001". (p. 110) However, there is no evidence that the sampling process changed in later years, and there was no error in the 2001 scores that

explains the strong outcomes. J. Bowers contacted Joshua McGrane by email and asked if he had identified an error in the 2001 results and he responded:

> "No, not claiming that at all. Just that there is greater uncertainty for the 2001 estimate, which would impact the significance test with 2016" (personal communication, May 7th, 2020; he is happy for us to quote him).

Furthermore, in contrast with the widespread claim that PIRLS results improved from 2011 to 2016, Solity (2018) reported at the Society for the Scientific Study of Reading (SSSR) that England went down one place in the league tables from 2011 to 2016 (10th to 11th) when excluding performance from private schools (that are not required to teach systematic phonics). Buckingham commented:

> "While Bowers (2020) mentions a theory put by Jonathan Solity that there may have also been a sampling issue with the 2016 test, the reference he provides is not to a published source." (p. 110)

Apart from the fact that SSSR is the one of the top international conferences in reading research, this conclusion is not based on a theory, it is based on an analysis of the data presented on Table 5.13 on Page 96 of the document entitled "Progress in international reading literacy study (PIRLS): National report for England" (McGrane, Stiff, Baird, Lenkeit, & Hopfenbeck, 2017).

Another problem with the claim that the PIRLS results support the effectiveness of systematic phonics in England is that Northern Ireland did better than England in 2011 and 2016 without legally requiring systematic phonics. Buckingham argues that Northern Ireland does implement systematic phonics, quoting the Northern Ireland Department for Education (2011). However, she omitted the following passage from this document that immediately proceeds the passage she quoted:

> "A range of other strategies for developing early literacy should also be deployed as appropriate and pupils who have successfully developed their phonological awareness should not be required to undertake phonics work if the teacher does not think it necessary or beneficial". (p. 25)

It is also worth noting that Northern Ireland does not use the PSC, and according to Buckingham, the PSC played an important role in improving synthetic systematic phonics provision in England (see below). Nevertheless, Buckingham wants to credit the success of Northern Ireland to the excellent provision of systematic phonics? And if indeed, Northern Ireland does teach systematic phonics so effectively without the PSC, then it is unclear why she has been arguing (successfully) for the implementation of the PSC in Australia (Buckingham, 2016).

Standard Assessment Test (SAT) results:

Bowers (2020) argued that there is little or no evidence that reading scores from SATs Key Stage 1 (when children were 7 years of age) and reading scores from Key Stage 2 (when children were 11) improved in response to the legal requirement to teach systematic phonics in 2007 nor in response to the PSC that was introduced in 2012. Buckingham disagrees, and writes:

> "The graphs of [Key Stage 1] and [Key Stage 2] scores from 2006 to 2018 in Bowers
> (2020) clearly show an upward trend in reading and writing from 2011 to 2015 that
> is greater than the upward trend for maths and science…". (p. 11)

Figure 1 depicts the SAT results for Key Stage 1 (taken from Bowers, 2020) and ask the reader to judge for themselves whether this characterization of the graph is accurate (please also see the graph from Key Stage 2 from Bowers, 2020).
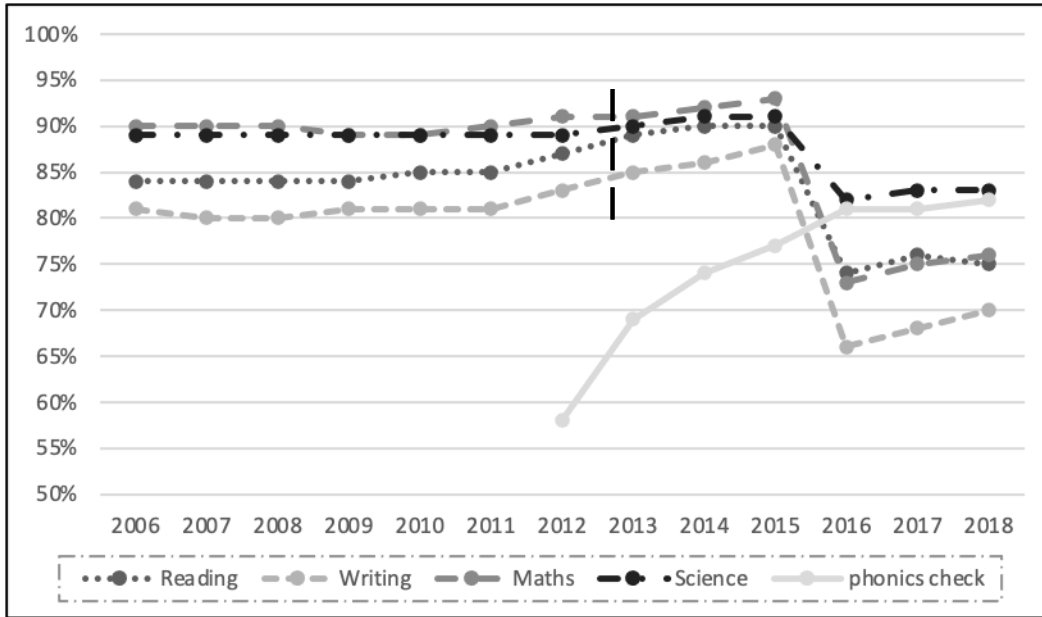
Figure 1

Figure 1. Results on Key Stage 1 SAT tests in reading, writing, maths, and science from 2006 to 2018 as well as the results of the phonics screening check from 2012 to 2018. SAT scores to the left of vertical dashed line were achieved without having completed the phonics screening check in year 1. The drop in performance across all measures in 2016 reflects an introduction of a more difficult SATs test. Taken from Bowers (2020).

In addition to the fact that math and science SAT scores improved at a similar rate to the reading and writing SAT scores, another reason to challenge the claim that the PSC played a role in the improving reading and writing SAT outcomes is that the scores started increasing the year before children were given the PSC during 2012. Bowers (2020) quoted from a government report on the PSC that made this very point

> "These analyses of national data therefore indicate small improvements in attainment
> at KS1, which were a feature before the introduction of the check and continued at a
> similar pace following the introduction of the check" (Walker, Sainsbury, Worth,
> Bamforth, & Betts, 2015, p. 27).

As noted by Bowers (2020), this is inconsistent with Buckingham (2016) who claimed that the increase in reading SAT scores from 85% in 2011 to 90% in 2015 followed the introduction of the PSC. Specifically, Buckingham wrote:

> "A five percentage point increase in the proportion of children achieving the target level perhaps understates the change. Put another way, the number of children who failed to achieve the target reading level fell by 33% over the four years since the Phonics Screening Check started". (p. 16)

In fact, the reading SAT scores increased from 87% to 90% after the introduction of the PSC (from 2012-2015). Or to put another way, 40% of the increase in SAT scores from 2011-2015 occurred prior to the PSC. Buckingham (2020a) claims that the Walker et al. (2015) analysis does not contradict Buckingham (2016) given they carried out different analyses and given that Buckingham (2016) considered an additional year of SAT results (extending the Walker et al. analysis from 2014 to 2015). But this misses the point. The Walker et al. (2015) analysis does falsify Buckingham's claim that

> "…the number of children who failed to achieve the target reading level fell by 33% over the four years since the Phonics Screening Check started".

Buckingham (2020a) also describe a study carried out by Double, McGrane, Stiff, and Hopfenbeck (2019) that observed PSC scores in Year 1 predicted SAT reading results in Year 2 as well as the PIRLS results for children age 11. Buckingham takes these findings to support systematic phonics and quotes the conclusion of the authors: "ameliorating early phonics difficulties predicts better reading comprehension up to 4 years later' (Double et al., 2019, p. 1232)".

However, this study provides no evidence that phonics instruction played a role in this correlation. Reading skills correlate to one another, with strong readers good at multiple literacy skills and struggling readers poor at multiple skills. Indeed, there is a large literature highlighting that morphological knowledge predicts literacy outcomes (e.g., Deacon, Kirby, & Casselman-Bell, 2009), as does vocabulary knowledge (Valentini, Ricketts, Pye, & Houston-Price, 2018), as does spelling (Ouellette, Martin-Chang, & Rossi, 2017), etc. None of these latter correlations support any given form of instruction, and neither does a correlation between decoding as measured on the PSC and reading comprehension. In order to conclude that systematic phonics has a long-term impact on comprehension it is necessary to carry out intervention studies, and as reviewed above, not a single meta-analysis nor the natural experiment in England supports this conclusion.

It is also worth noting that Stainthorp (2020), in the same issue as the Buckingham (2020a) commentary, published an article claiming that the SAT reading results have improved in England since the introduction of phonics. A key argument came in the form of a graph depicting increasing SAT scores. However, the graph did not depict the math and science results that improved at similar rates, the graph failed to indicate that all measures started increasing the year prior to the introduction of the PSC, and Stainthorp (2020) failed to cite Bowers (2020) that covered the same data and reached the opposite conclusion. The paper also failed to address any of the problems with the Machin et al., PISA and PIRLS results noted by Bowers (2020).

Phonics Screening Check (PSC) outcomes:

One finding that Bowers (2020) did not consider in detail is the impressive improvement in the PSC scores since its introduction in 2012 as can be seen in Figure 1. The PSC requires children to decode regular words and pseudowords (e.g., fape, blan, geck). As noted above, performance increased from 58% pass rate in 2012 (correctly naming 80% of all items) to 82% in 2019. Although this improvement looks and is often described as dramatic, it is much less so when you consider the mean improvement rather than the pass rate. These numbers are not easy to find, so we computed the means from the "phonics tables" from 2012 and 2019 from the GOV.UK website https://www.gov.uk/government/collections/statistics-key-stage-1. The mean scores improved from 29.5/40 to 33.8/40.

The PSC has proved controversial. Although the test was designed to assess GPC knowledge, there is good evidence that teachers have organized their teaching in order to pass the test, including spending more time teaching pseudoword naming (Carter, 2020). Not only do many teachers report disliking the PSC (Carter, 2020), the pressure to pass the test appears to have led to cheating by teachers given a disproportionate number of children scored exactly at the pass level. Bishop (2012) wrote:

> "As one who is basically in favour of phonics testing, I'm sorry to put another cat among the educational pigeons, but on the basis of this evidence, I do query whether these data can be trusted."

Buckingham does not consider any these problematic issues, let alone address the fundamental point that there is little or no evidence that PSC scores have translated into better reading more generally as reflected in SAT, PISA, or PIRLS scores as discussed above.

An interesting question for future work would be to assess what percentage of the increase in PSC is attributed to increased performance with words vs pseudowords (the data are not available). If the improvements are largely due to improved pseudoword naming, it would provide further evidence that indeed teachers are teaching to the test (teaching children to read pseudowords), and it would help explain why raising PSC scores are not associated with better reading outcomes on other measures.

Structured Word Inquiry:

In addition to challenging the Bowers (2020) critique of systematic phonics, Buckingham's response article has a section criticizing "Structured Word Inquiry" or SWI. In fact, the Bowers critique barely mentions SWI, and the conclusion was that the poor evidence for systematic phonics should motivate teachers and researchers to consider alternative approaches, including SWI. But in any case, just as with her analysis of systematic phonics, all her substantive points regarding SWI are either mistaken or mischaracterizations.

Buckingham first point is that SWI does not teach GPCs, based on the following reasoning:

> "Students are encouraged to 'spell words out' using letter names rather than sound them out using GPCs. This contradicts any claim that SWI teaches GPCs, given that GPCs by definition involve the speech sounds associated with the letters, not their names". (p. 109)

This is a logically flawed argument given that SWI could (and does) teach multiple aspects of literacy, with children learning to 'spell words out' as well as learning GPCs. It is also an extraordinary claim given that we have emphasized repeatedly GPCs are an essential aspect of SWI. For example, in Bowers and Bowers (2017) we wrote:

> "SWI emphasizes that English spellings are organized around the interrelation of morphology, etymology, and phonology and that it is not possible to accurately characterize grapheme–phoneme correspondences in isolation of these other sublexical constraints (p. 124) … "We have no doubt that learning grapheme–phoneme correspondences is essential" (p. 133).

And in Bowers and Bowers (2018b) we wrote:

> "To avoid any confusion, it is important to emphasize that the explicit instruction of orthographic phonology — how grapheme-phoneme correspondences work—is a core feature of SWI. However, unlike phonics, SWI considers grapheme-phonemes within the context of morphology and etymology." (pp. 409-410)

We have also made this point multiple times directly to Buckingham in personal communications.

Furthermore, the article that introduced the phrase SWI (Bowers & Kirby, 2010) made it clear that SWI teaches children to spell out <u>word sums</u> rather than words (both in writing and orally) in order to emphasize morphemic and graphemic units in a word, as well as suffixing conventions. For example, Bowers and Kirby illustrated how the word sum for <pleasure> is spelled out: ''p–l–e–a–s–e–plus-u-r-e is rewritten as-p–l–e–a–s–no e—u-r-e''. The dash length represents pause length between saying letter names, with the <ea> string named together quickly to highlight its structure as a digraph and the "no e" reflecting a suffixing convention. In this example, the child learns that the <ea> digraph links to the phoneme /i:/ in "please" and /ɛ/ in "pleasure." That is, students are encouraged to 'spell words out' in order to teach GPCs in the context of morphology. Since that 2010 study, the second author has provided more detail on this process as can be found in a resource for educators at this link (https://tinyurl.com/spelling-out-loud).

Second, Buckingham also criticizes the fact that there are no illustrations of how to teach SWI from the start of instruction, writing: "Videos of SWI do not show how it teaches children 'from the start', when children have little or no knowledge of the alphabetic principle or concept of print". (p. 109) But Bowers and Bowers (2017, 2018b) provided links to videos of SWI being carried out in Grade 1(http://tinyurl.com/zlr27pn), in kindergarten (https://goo.gl/XbZS9d), and in pre-school (https://vimeo.com/189070725). It is true that these videos do not capture instruction on day 1 of instruction, but some of the students in the pre-school video are not yet readers and are learning GPCs in this morphological context. It is also worth emphasizing that there is background research that motivates teaching SWI from the early stages of reading, including the fact children already have substantial morphological knowledge prior to any reading instruction (Berko, 1958), all meta-analyses and reviews of morphological instruction show that morphological instruction is more effective with younger readers (Bowers et al. 2010; Goodwin and Ahn, 2010, 2013; Reed, 2008), and, morphological

awareness training delivered in preschool produced long-term effects on reading comprehension (Lyster, Lervåg, & Hulme, 2016).

Buckingham's final criticism is that there is no good empirical evidence for SWI, writing:

"The problem with positing SWI as a superior alternative to systematic phonics is first that there is insufficient information to assess whether it is an effective method for teaching beginning readers, and particularly for the acquisition for essential knowledge about GPCs." (p. 109)

There are two problems with this criticism. First, and most importantly, Bowers (2020) never claimed that there is strong empirical evidence in support of SWI. Rather, the lack of evidence in support of systematic phonics was taken as a strong motivation to conduct more research into alternative approaches, including SWI. Indeed, Bowers (2020) ends with:

"...the first step in motivating more research into alternative forms of instruction is to realize that there is a problem with the current approach". (p. 703)

Similarly, in Bowers and Bowers (2017) we wrote:

"This is our goal: to motivate future empirical studies of SWI in order to assess whether indeed this method is more effective than phonics that is currently failing too many children". (p. 138)

And in Bowers and Bowers (2018b) we concluded:

"We do not want to make too much of this empirical evidence given so few studies have been carried out thus far. But in combination with the strong pedagogical considerations, we would argue that SWI is a highly promising approach that deserves more attention". (p. 411)

It is Buckingham and the research community in general that have dramatically mischaracterized the strength of evidence for systematic phonics rather than us mischaracterizing the strength of empirical evidence in support of SWI.

Second, Buckingham mischaracterizes the evidence for SWI. One example concerns the Devonshire et al. study that compared SWI to systematic phonics in early reading instruction (children ages 5-7) in a randomized controlled study. The study found SWI improved performance on spelling and naming, but Buckingham criticized it writing:

"The comparison condition was a 'business as usual' combination of letter-sound teaching and a whole word reading scheme." (p. 109)

In fact, children in this study were given systematic phonics instruction in school without combining it with whole word reading schemes. The standard practice of the school was to give all children (including children in the SWI and control conditions) some homework that Devonshire et al. characterized as "whole word" type work, and Buckingham might want to argue that this homework selectively impaired learning in the phonics group (why this might be is unclear). But there are no

grounds to dismiss the study as flawed due to the absence of systematic phonics in the control group (for more detail see Bowers and Bowers, 2018a). And as noted above, Buckingham endorses the conclusions of meta-analyses that compared systematic phonics to a control condition that included no reading instruction at all (McArthur et al., 2012, 2018). Buckingham has an inconsistent standard of evidence when evaluating systematic phonics and SWI.

Buckingham also describes an unpublished intervention study by Colenbrander et al. (2018; currently under review) that compared SWI to an alternative approach called "Motivated Reading" in Year 3 and 5 children who had poor reading and spelling skills (conditions in which there is little evidence that phonics is beneficial, as reviewed above). Buckingham quotes slide 42 from a slide presentation:

> "No evidence that Structured Word Inquiry is more effective than Motivated
> Reading for improving reading, spelling, vocabulary or reading comprehension'; and
> 'Motivated Reading instruction led to greater reading gains than Structured Word
> Inquiry for the weakest readers (also true for Year 5 spelling)' (Colenbrander et al.,
> 2018, Slide 42). (p. 109)

In fact, SWI was more successful than motivated reading in some conditions, but overall, it would be fair to say that the two approaches were equally successful. But Buckingham omitted a key finding from the study, namely, that the teaching assistants who carried out the intervention were not confident at teaching SWI, and this may have impacted the results. Indeed, Buckingham failed to list some of the other conclusion listed on the same Slide 42 quoted above, including:

> "Lower levels of TA knowledge and confidence in SWI may have reduced
> effectiveness"; "Future studies could explore effects of increasing TA training and
> tailoring to ability levels".

Note, all other SWI intervention studies (Bowers & Kirby, 2010; Devonshire et al., 2010, 2013; Georgiou, Savage, Dunn, Bowers, & Parrila, 2020) have reported significant benefits, and all were carried out by instructors who were more skilled at SWI compared to the TAs in the Colenbrander et al. (2018) study. The pattern of results from Colenbrander et al. (2018) do highlight a challenge for SWI, namely, teachers need to understand the logic of the English writing system, and we did underestimate the challenge for the teaching assistants in this regard. But these findings do not challenge the effectiveness of SWI when taught by someone who is confident in SWI instruction. Again, we agree there is currently little empirical evidence for SWI because there has been so little investigation of this approach. At the same time, we do think there are strong theoretical and pedagogical reasons consider SWI (Bowers and Bowers, 2017, 2018b; Bowers & Kirby, 2010).

Conclusion

In sum, every substantive point that Buckingham makes regarding the Bowers (2020) critique of systematic phonics, and every substantive point that Buckingham makes regarding SWI as described in numerous paper (Bowers & Bowers, 2017, 2018a,b; Bowers & Kirby, 2010; Devonshire et al., 2013), is factually incorrect or a mischaracterization. We hope that that this response will help make teachers and researchers aware that there is no justification for the widespread claim that the evidence for systematic

phonics is strong, and that it will inspire teachers and researchers to consider alternative approaches that explicitly teach GPCs along with other regularities in the English writing system. Everyone should agree that it is important to have an accurate assessment of the efficacy of systematic phonics, and we hope that Buckingham and others will acknowledge the lack of empirical support for systematic phonics or respond to the challenges we have raised here. Claiming that the science of reading strongly supports phonics, without at least citing contrary arguments, is poor scientific practice.

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