



REVIEW OF *SUCCESSFUL, SAFE, AND HEALTHY STUDENTS*

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Summary of Review

The research summary “Successful, Safe, and Healthy Students” presents the research background for the Obama administration’s proposals for comprehensive, community-wide services in high-poverty neighborhoods, extended learning time, family engagement and safe schools. While these policies have broad and common-sense appeal, the research supporting the particular policies proposed by the administration is weak and poorly presented in the research summary. As promising as community-wide services may be, a broad research base does not yet exist concerning how to make them successful. The research on extended learning time is also inconclusive. Family involvement is crucial to education, but the evidence for a causal link between student achievement and the type of parent involvement discussed is ambiguous and suspect. The proposals for safe schools boil down to increased local flexibility and increased gathering of survey data, neither of which can be expected to improve outcomes. Together, the **administration’s** proposals would require an extensive financial commitment in order to be fully implemented, but the scope and source of these funds is not explained. Overall, the evidence provided is not sufficiently strong to justify the programs they champion. While the research summary adequately documents problems, a wiser course for public policy would be a carefully structured set of pilot studies to sharply and accurately identify solutions.

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I. Introduction

In March 2010, the Obama administration released a *Blueprint* outlining its proposals for reauthorizing the Elementary and Secondary Education Act (ESEA).¹ In May 2010 the U.S. Department of Education (USDOE) followed with a set of six documents, offered as “research summaries” supporting the administration’s plans.²

The fifth of these six research summaries, titled “Successful, Safe and Healthy Students” (SSHS), is the focus of this review.³ The SSHS research summary cites some of the large body of research pointing to the importance for educational outcomes of community poverty.⁴ Responding to related needs, the administration lays out a set of proposals divided into three sections, containing four categories of proposed policies: (1) Promise Neighborhoods (comprehensive services); (2) 21st Century learning communities (extended learning time); and “Successful, safe and healthy students,” which includes both (3) family engagement and (4) increased flexibility and use of data to target health and safety needs. This review considers each of these four in turn, examining the strength of the research cited, how well that research represents the larger research base, and how well it supports the *Blueprint’s* policy proposals.

Clear patterns emerge across the four categories of proposals: the research summary adequately documents problems but generally stops far short of presenting meaningful research in support of the proposed policy; the evidence cited to support the administration’s proposals does not always actually support them; and unfavorable evidence tends to be omitted. These and other issues are described in greater detail in this review.

II. Promise Neighborhoods

Policies are proposed to provide comprehensive services (including parenting education, preschool, health, and social and community development services) to children and families in very poor neighborhoods, with the goal of reducing achievement gaps between races, ethnic groups, and social classes. The most prominent proposal is the creation of Promise Neighborhoods nationwide based on the Harlem Children’s Zone (HCZ) model. Unfortunately, while SSHS cites some important research suggesting the benefits of HCZ, it fails to consider

adequately information on costs or the evidence that this approach may not be as effective as suggested by media reports.

The only rigorous study of HCZ to date has failed to find any evidence linking improved achievement directly to its community and neighborhood components.⁵ This research is best understood as preliminary and ongoing, but it provides little support for a major scaling up of **the HCZ approach. Even the HCZ’s early-parenting and preschool programs** were found to be ineffective in isolation (apart from combined effects with other interventions), although abundant evidence exists elsewhere demonstrating that early-parenting and preschool programs have been found to raise achievement compared to the status quo.⁶

The only positive academic effects found were associated with attendance at the HCZ “Promise Academy” charter schools—whose students also received the full panoply of comprehensive services. Therefore, the researchers could not attribute these results to the schools in isolation.

Significantly, the cost of the HCZ (and other proposals) is not discussed in SSHS. Other research, however, has suggested that the costs of comprehensive services similar to those

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proposed can add considerably to the cost of education even when they are not particularly effective.⁷ The benefits of such programs might well outweigh the cost (relative to investing in other policy options), but there is no way of knowing without an accurate cost estimate.

Notwithstanding these evidentiary limitations, the administration’s Promise Neighborhood proposal seems to be based entirely on the sparse HCZ evidence. In fact, a broader review of the research evidence raises important questions. While ample research points to the educational (and broader) importance of addressing out-of-school factors that affect children and families,⁸ research on programs that move families from neighborhoods of concentrated poverty, for example, casts doubt on remedies focused on a direct causal link to concentrated poverty.⁹ Similar doubts are raised by studies of preschool programs providing comprehensive services to young children.¹⁰ This relative weakness in the research base does not necessarily argue against **expanding and studying pilot “Promise Neighborhoods” efforts**, but it is an argument for sound, complete examinations of research in advance of policy formation.

A more balanced review than that presented in the SSHS research summary might also highlight the value of high-quality early childhood education programs. Such programs have been found to promote increased achievement and school success for disadvantaged children,¹¹ yet they receive no mention in SSHS.

III. Expanded Learning Time

The SSHS summary presents evidence in support of increasing the amount of time for instruction by adding hours to the school day or days to the school year. The stated approach **also “[g]ives priority to programs that comprehensively redesign and expand the school day or year to increase time for academics or enrichment activities and provide time for teachers to collaborate and improve their practice.” In support of this recommendation, a variety of findings from empirical studies are briefly mentioned:**

1. U.S. students attend school fewer days per year than those in nations with which the U.S. competes economically and that outscore the U.S. on the Trends in International Mathematics and Science Study (TIMSS) test.¹²
2. **“Programs that lengthen the school day and year are a promising approach to improving student outcomes.”**¹³
3. **“Preliminary research indicates that expanded learning time can increase student achievement.”**¹⁴

There is less here in empirical support for the policy than meets the eye. International comparisons are an unsound basis for making judgments about schooling in the U.S. Many influences other than the length of the school year differ among the countries. Further, surveys such as TIMSS have been justifiably criticized by researchers on the grounds of language differences (even in math and science questions), inconsistencies in sampling students across nations, and differing ages of students tested among nations.¹⁵ These are just a few of the reasons why experts believe that heavy reliance on TIMSS and other international comparisons in determining U.S. education policy is a seriously flawed strategy.¹⁶

Regarding the second point, the cited Traphagen and Johnson-Staub report¹⁷ offers only weak evidence for policy formulation. It is anecdotal, providing no data on the purported benefits of expanded learning time and student outcomes.

On point three, the Farbman and Hoxby, Murarka, and Kang reports are the only meaningful sources cited for the claims about student achievement and expanded learning time. Farbman wrote, **“we determined a statistically significant ($p < .01$) moderate correlation for Grades 7 and 10 in both math ($r = .29$ and $r = .41$, respectively) and ELA ($r = .31$ and $r = .43$, respectively). No significant differences were found for other grades.”**¹⁸ These tepid findings were all but retracted when the author subsequently **noted that “Analyses do not suggest a causal relationship and do not control for any other school- or student-related factors (e.g., demographics)” (p. 25).**

Hoxby and her associates performed a survey of charter schools in New York City in which they related the lengths of the school day and the school year to student achievement. In their regression analysis, a longer school year was associated with higher achievement.¹⁹ A longer school day may also be important, but since the schools in the database with longer days also tended to be those with longer years, the longer day does not show up as a predictor when both variables are included in the regression equation (p. V-3). Interestingly, this day-versus-year distinction emerges in other research and thus is probably more than an artifact of the Hoxby data. Research on longer days has shown few if any benefits for student achievement. Research

on longer years has shown that while *small* increases in the number of days students attend school reveal no benefits for student learning, *large* increases in the length of the school year—for example, extensive summer school attendance—are more promising.²⁰ Moreover, Hoxby and her colleagues, like Farbman, expressly state that they make no claims about causes.

The other source offered in support of expanded instructional time is a brief write-up of the Massachusetts Expanded Learning Time Initiative. This citation is curious. The research summary says that six of the seven middle schools in the study more closely approached the

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state average in math (that is, they narrowed the existing gap), and five of the seven schools did so in language arts. Yet such promising statements of findings seem at odds with the conclusions of the evaluation of the **program's** second year (2007-2008):

As is common in the early stages of new school-level initiatives, analyses revealed few effects of ELT [Expanded Learning Time] on student outcomes for the first cohort of ELT schools. For six of the eight Massachusetts Comprehensive Assessment System (MCAS) outcomes, there was no statistically significant effect of ELT on student MCAS performance in the first or second year of ELT implementation, controlling for student demographics, prior performance, and matched comparison schools' performance.²¹

The weak support for the administration's preferred policies given by the cited research is amplified by the omission in the SSHS research summary of key studies that should have been considered. For instance, the summary overlooked a meta-analysis by Levin and Glass on expanded instruction time that carried out a thorough cost-effectiveness analysis. Levin and Glass found that increasing the length of the school day for instruction was the second-least cost-effective of four interventions aimed at increasing student achievement (the others being cross-age tutoring, computer-assisted instruction, and reducing class size).²² Increasing time for instruction was superior only to class-size reduction. Other evidence is mixed regarding the effects of a longer school day and after-school programs on academic achievement.²³ Moreover, a broader review of this evidence might have prompted consideration of a wider range of schooling outcomes—including habits, attitudes, social behavior, delinquency, and crime.²⁴

IV. Family Involvement

SSHS claims that **“Many researchers agree that family engagement in their child's education is positively associated with increases in student academic achievement.”**²⁵ SSHS further claims that **“school-sponsored family engagement programs may indirectly improve student achievement by increasing families' expectations or improving parenting skills related to education.”**²⁶

Notwithstanding the careful wording of these claims, the evidentiary basis supporting the administration's policy proposals regarding family involvement is remarkably weak, and the **research summary's use of this evidence is partial and misleading**. Of four recent major reviews of the literature on **the effects of parent involvement on students' academic progress (three meta-analyses and one narrative review)**, two are cited in the research summary²⁷ and two are not.²⁸ Following a narrative review of the literature, Mattingly et al. (2002)—not cited by the USDOE—concluded that no influence of parent involvement on student achievement could be

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found. Fan and Chen (2001), which is cited but poorly explained, found the literature on parent-involvement effects to have **“considerable inconsistencies” (p. 1)** and **concluded that “parental aspiration/expectation for children's education achievement has the strongest relationship [to measured achievement], whereas parental home supervision has the weakest relationship” (p. 1)**. This finding is echoed in the research discussed below and is particularly noteworthy since it provides a clue to the potential invalidity of the **research summary's** implicit claims about parent involvement producing student achievement.

The most extensive reviews of research on parent involvement are two meta-analyses: Jeynes's examinations of parent involvement at the elementary school level (2005) and the secondary school level (2007). The earlier, elementary study is cited; the later, secondary study is not. Although these meta-analyses led Jeynes to conclude that parent involvement increased student achievement, when viewed more critically they actually bring the validity of that conclusion into question. With one exception, all **90 studies integrated in Jeynes's meta-analyses** are non-experimental, one-shot *ex post facto* surveys in which questionnaire data from parents are correlated with student achievement measures. All that is actually revealed is a correlation of **parent involvement (usually “aspirations” or “expectations for achievement”)** with students' measured achievement. Yet parents' **expectations** may be **influenced by their children's** achievement to at least as great an extent as their expectations cause increased achievement. This is suggested by stronger correlations between aspirations/expectations and achievement than **between students' achievement and actions taken by the parents in the home.**²⁹ With just one quasi-experiment identified by Jeynes in which parent involvement is a manipulated variable, the evidence base seems insufficient to support a major policy initiative.

The USDOE also cites the study by **D'Agostino** and his colleagues³⁰ in support of the beneficial impact of school-sponsored family-engagement programs. After controlling for **“pre-reading achievement,”** **the** largest portion of variance in reading achievement was accounted for by **“parents' aspirations for the child.”** Once again, this finding raises the issue of ambiguous direction of causality. Do parental aspirations cause high achievement or does high achievement cause high parental expectations (or are they mutually reinforcing)? In addition, these researchers found no relationship between reading performance and any of the parent-involvement variables of **“school-based collaboration,” “participation in school governance,”** or

“volunteering at school.” **This study is consistent with the larger body of research in this area:** the non-experimental evidence for a causal link between parent involvement and student achievement is ambiguous and suspect, and the experimental evidence is essentially non-existent.

V. Data to Improve Health and Safety

The SSHS research summary also calls for “using data to improve students’ safety, health, and well-being, and increasing the capacity of states, districts, and schools to create safe, healthy, and drug-free environments.” **The core idea**—using data to inform policy—is undoubtedly sensible, but the particular proposals and the research support provided are more troubling.

The USDOE argues in favor of three policies: (1) approaches that comprehensively improve school climate by improving safety and health; (2) the use of school-climate surveys to collect **“needs assessment data” to guide the allocation of resources and the implementation of effective programs**; and (3) the use of a grant application system allowing states and districts to apply for **a single “Successful, Safe, and Healthy Students” grant** and then target funds based on unique local needs. The core reform recommendation is based on an expansion of the collection and use of survey data. **The administration’s budget proposal seeks \$410 million to implement “climate measurement systems” to be used to target resources to (a) improve school safety and strengthen family and student engagement, and (b) improve students’ physical and mental health.**³¹

The research summary implicitly limits the focus of “Successful, Safe, and Healthy Students” to only three key issues: school violence, the use of drugs (including alcohol), and obesity.³² The summary cites some research providing statistics to demonstrate that these problems exist, and the brief presentation is sufficient to justify these issues as deserving of a policy response. We note, however, two omissions. First, nothing is provided in the research summary concerning school violence trends. And in fact, rates of most categories of violence in school are generally continuing a long decline.³³ We also note that lesbian, gay, bisexual, and transgendered (LGBT) students are particularly at risk for school violence,³⁴ but the research summary never mentions this, **nor does the administration’s *Blueprint* policy focus on the problem.**

Further, as noted in the research summary itself, use of marijuana and tobacco are both trending downward, and there are successful interventions currently being used. So while the research summary makes the case that violence and the use of drugs and alcohol are widespread, it falls short of making an evidence-based case documenting the key policy questions: which programs are currently in use, how effective are they, and is policy in need of change. The change argument relies on a 2007 USDOE report concluding that the current grant structure does not adequately target the neediest schools. That may indeed be the case, but given the positive trends, more justification for change would be helpful—particularly for the specific changes called for in the *Blueprint*.

In contrast, the trends as well as the rates of obesity are clearly troubling. What is missing from the research support provided by the USDOE is evidence supporting the *Blueprint’s* focus on a

school-specific response, rather than a broader program to address obesity at a societal level. No research citations are provided concerning school-level programs. Undoubtedly, schools can play a positive role through education, the provision of healthy breakfasts and lunches, the elimination of junk food sales and advertising on campus, and the provision of good opportunities for exercise during the school day.³⁵ However, it is not at all clear from the *Blueprint* that grant money would be spent in ways supported by research. Moreover, the obesity problem and solutions to it go far beyond the role of schools.³⁶ Again, none of this evidence is presented in the research summary, and none of these issues is addressed.

The central idea behind school-climate surveys seems to be that these state-level and district-level surveys can determine (a) the nature of specific needs, and (b) where to allocate resources. The survey data could also be used longitudinally to determine the effectiveness of resource allocation and of particular programs. The use of school-climate-survey data for these purposes has not been validated in any of the sources cited, however. While the primary survey instrument contemplated for this use has apparently been found to yield useful information

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about key school-climate constructs,³⁷ the question here is **one of “use validity”**: on what specific bases will policy be grounded, and will the survey results provide the information needed to make sound policy decisions? Neither the *Blueprint* nor the research summary indicate that the surveys have been, or will be, piloted and studied prior to being scaled up. Instead, the approach appears to be widespread adoption prior to study. This places a great deal of faith in a survey instrument that has apparently not been used for high-stakes purposes or been validated for such uses. **It also raises the concern, generally expressed as Campbell’s Law, that the high stakes themselves will corrupt the survey.**³⁸ The question arises as to why multiple tools and measures—beyond the survey—are apparently not being considered.³⁹

Overall, while safety and health issues are important and while better policy approaches should be sought, the case is never made that increased local flexibility informed by survey data will result in more successful policies and programs.

VI. Conclusion and Recommendations for Policy

The research summary proposes laudable ideals by viewing schools as integral parts of the community and by advocating increasing family involvement in schools, extending learning time, and promoting health and safety. Few would disagree with these ideals, and the summary does an adequate job of raising concerns in these areas.

Unfortunately, the research presented by the administration in support of its proposals generally stops far short of offering meaningful findings or illustrating useful policy directions.

The evidence cited to support the administration’s proposals does not in fact always support them, and unfavorable evidence tends to be omitted.

Extended learning time, for example, is a notion with far greater nuance than is presented. Greater school safety is a laudable goal, but the research summary never makes the case for how added local flexibility or the collection of additional data will improve the situation. Family involvement is certainly vital to social well-being, but little definition is provided of what that involvement entails. Nor is an explanation provided of how the changes would be accomplished, and there is no explanation of the true nature and limitations of the research evidence. Early education is well-established in the literature, and research also points to enormous out-of-school needs in low-income communities; however, specific wrap-around support mechanisms such as HCZ are not as well studied and lack the same sort of research support. Furthermore, the cost of many of these proposals is significant, and there is no indication of how funding would be provided.

The mismatch between **the administration’s** avowed—and sometimes demonstrated—commitment to evidence-based policymaking and the *Blueprint* policymaking process raises the question of whether a broader and more balanced review of the SSHS evidence might lead the administration to a different set of policy proposals. Such a balanced review should, we think, drive a carefully evaluated test of multiple promising approaches to addressing the identified needs.

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