

Proposed educational innovations to reduce income-based achievement gaps

Heather Schwartz
RAND Corporation
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Introduction

Schooling remains among most important public policy tools we have in the United States for raising children out of poverty in the United States.¹ Given the ongoing shift in the U.S. economy toward jobs that require more formal education, it is more important than ever to graduate from high school. Likewise, the returns to a college degree have increased over the past 40 years (see Figure 1.6 in Baum, Ma, & Payea, 2013).

But there is also a limit on the ability of schools to improve student achievement and attainment. Individual and family characteristics account for a greater proportion of student achievement than schools (Coleman et al., 1966 in Rothstein, 2010). According to their family socioeconomic status, children enter the earliest grades with large gaps in their early math skills, attention, and antisocial behavior that grows modestly, rather than shrinks, as children progress through grade levels (Duncan & Magnuson, 2011). By the time students reach the upper grades, the characteristics of high school are estimated to account for up to 12 percent of the variation in students' high school graduation rates (Altonji and Mansfield, 2011).

Even so, schools are still important. A growing number of longitudinal studies have identified a causal relationship between characteristics of schooling, like model preschool programs, smaller class sizes, and teacher effectiveness, on long-term outcomes like adult earnings and well-being (e.g., Chetty et al., 2011; Heckman et al., 2010). Even the relatively modest contributions of high-quality versus low-quality high schools matter because, as Altonji and Mansfield (2011) point out, it can mean the difference between graduating or not for those students on the margin of dropping out. Plus, from a public policy perspective, schools offer a more direct means to reach children compared to those important, but difficult to influence, arenas such as family practices.

The need for schooling to play an equalizing role in raising low-income students' achievement and later earnings has never been greater. As the income gap in the U.S. has grown over the last 50 years, so has the achievement gap between children of high- and low-income families. It is currently about twice the size of the gap between black and white children (see Figure 1 in the Appendix, Reardon (2011)). The growth of the income-based achievement gap would be less troubling if there was a high rate of intergenerational mobility within the U.S. But several studies show that the prevailing national rate of intergenerational mobility is not that high compared to other countries (Duncan & Murnane, 2011; Chetty et al., 2014).²

¹ This was especially true as free public schooling became universal throughout the twentieth century. Seventy-five percent of children graduated from high school by 1975 compared to 6 percent in 1900 (Goldin and Katz, 2008 in Duncan and Murnane, 2011).

² A silver lining to this depressing national statistic is the surprising degree of regional variation in intergenerational mobility (Chetty et al., 2014). The absolute upward mobility rate—i.e., the average income rank of children with parents in the bottom half of the income distribution—is most highly correlated (either positively or negatively) with: (1) the fraction of households living who commute less than 15 minutes to work (measure of sprawl); (2) the Gini coefficient (a value capturing the distribution of income across a commuting zone population—0 would mean all households in the commuting zone have equal incomes, and 1 would be perfect inequality where 1 household earned 100% of the total income in that commuting zone and the rest of the population earned \$0); (3) average test scores of public schools (after controlling for income); (4) the public high school drop-out rate (after controlling for income); (5) teenage birth rates; (6) the social capital index (an index of civic mindedness derived from voter turnout rates, fraction of people returning census forms, and participation in community organizations); (7) fraction of households who are religious; (8) fraction of households who are single moms; (9) fraction of households who are divorced; (10) fraction of households who are married.

Reardon (2013) attributes recent growth in the income achievement gap not to stagnating or declining performance by lowest income children, but to a more rapid growth in performance among the highest income children. The rich now outperform the middle class to the same degree that the middle class outperform the poor (ibid). This finding, of course, raises the question of what services and education upper class children receive. Research points to large gaps in family investments in time and money (for money, see Duncan & Murnane, 2011; for time, see Risley and Hart, 1995). Time diary data suggest that, by age six, children from high-income families have approximately 1,300 more hours—the equivalent of about 8 weeks—in “novel contexts” that are not at home, school, or with a caretaker than children from low-income families (Phillips, 2011). The supposition is that non-routine contexts are stimulating and create more background knowledge, which is in turn, predictive of later reading skills. And, in a more direct connection to literacy, upper income children will have spent about 400 more hours—approximately 2.5 weeks—in literacy activities by age six relative to low-income peers (ibid). These findings comport with the much publicized findings by Hart and Risley (1995) of the large gap in the number and type of words spoken by professional versus low-income parents at home to their children. These gaps in investments of time and resources emerge even before children enroll in kindergarten and that appear to have lasting impact.

Given the importance of out-of-school time to children’s school performance, the question then becomes exactly what can a federal Department of Education do to both improve schools *and* best reach those important out-of-school factors? With federal rather than local policy making in mind, I propose four areas as the most promising from the perspective of reducing poverty. I assign major investment to two sets of school-based reforms where the Department of Education arguably has greater influence, where they have already invested substantial resources and development momentum, and where the preponderance of experimental evidence is available. In light of the importance of out-of-school factors to alleviating poverty for children, I direct minor investment into two family-focused topics that lack sufficient evidence to justify significant investment of federal resources. Note that in restricting myself to reforms the federal department of education could theoretically undertake, I do not propose policies such as income supplements, tax credits, or housing subsidies that could arguably be the most effective public policy tools to alleviate poverty. The four areas are:

1. *Major investment:* Revise the next generation of federal *Race to the Top - Early Learning Challenge* (ELC) grants to place significant focus on how best to measure early childhood education quality. This recommendation pivots from the ongoing efforts to expand early childhood education (which is important) to identifying what measures of quality matter before states’ early care Quality Improvement Systems become entrenched.
2. *Major investment:* Retool federal *School Improvement Grants* (SIG) that are given to lowest performing public schools by dropping the current menu of turnaround models. Instead, restructure the grants to reward longevity of implementation while also providing greater flexibility to districts to select and justify research-based comprehensive reforms. In other words, structure SIG grants to (a) encourage applicants to propose significant school reforms for which there is evidence, and (b) stick with it for a minimum of five years and preferably more like ten.
3. *Minor investment:* Following in the footsteps of *Investing in Innovation* grants (i3s), invest federal resources to test pilot programs that increase poor families’ access to high performing schools via informational coaching. This coaching could occur through a range of programs like offering free

school information counseling at key junctures in families' selection of schooling, including during residential moves.

4. *Minor investment*: With the same type of i3 grant structure, create a federal competition for grantees to engage parents in their children's schooling. For example, these pilots could range from public education and awareness campaigns, to home visits, to technological means of engaging parents in their children's schooling. The idea is to develop (possibly low impact, but on a per person basis low cost) sustainable means to educate parents to educate children.

Why invest in these areas and not others

Given the national focus of this conference, these recommendations pertain to federal rather than state and local policy making for education. Keeping in mind the formally decentralized structure of schooling in the U.S.³, these proposals copy the competitive funding structures that the Obama administration has created, but for a poverty-centered agenda. This is because of the success the Obama administration's competitive grants have had to effect substantial change in state school legislation in the administration's desired areas. Take the example of teacher performance evaluation. Until 2009, the large majority of public school teachers were compensated based on their credentials, and informal observations by principals would form the basis of annual performance reviews. This system of teacher performance review has substantially changed due largely to federal initiatives such as the *Race to the Top* competition, *Improving Teacher Quality Grants*, the *Teacher Incentive Fund*, and the *Investing in Innovation (i3) Fund*, in concert with foundation-funded initiatives such as from Bill and Melinda Gates Foundation. Between 2009 and 2013, the majority of states have made major policy changes related to teacher evaluation. As of September 2013, 35 states and the District of Columbia require that teacher evaluations include objective evidence on student learning, with 27 states requiring that teacher evaluations be based on multiple measures of student growth and achievement and 44 states requiring classroom observation to be incorporated into teacher evaluations (Doherty and Jacobs, 2013).

A core assumption that underlies each of these recommendations is that poverty as experienced by individuals and when concentrated in places or institutions like schools or neighborhoods is inherently destabilizing. Thus anti-poverty policies—whether in health, housing, education—should share the goal of increasing stability as a necessary but not sufficient condition for improvement. For example, the concentration of poverty in schools not only challenges educators to catch up students who already perform behind their upper-income peers, but it poses institutional challenges like heightened rates of school leader and teacher turnover, negative peer effects, heightened student absenteeism, heightened crime, higher rates of school failure that trigger accountability-based changes like curricular change and staff change in a fragmented, revolving, symbolic manner (Schwartz, 2012; Raudenbush et al., 2001; Hess, 1998; Boyd et al., 2001; Kirk & Sampson, 2011). Constant flux inhibits schools to get over the "implementation dip" (Fullan, 2001), and to develop the expertise, the institutional knowledge and memory, and the trust required for a change to take hold over a period of years (Hess, 1998; Bryk & Schneider, 2005).

For reasons mentioned in the introduction, three out of four of these recommendations attempt to reach out-of-school factors—for example, by expanding schooling into earlier years and by attempting to engage parents more effectively in their children's schooling. But, as important as out-of-school

³ Although the federal role in education has grown since the 1960s, the federal reach has been confined historically to redistributive programs like Title I and the encouragement of reforms through incentives rather than mandating the use of any single curriculum, standard, hiring policy, or accountability formula (Manna, 2006).

factors are, we cannot ignore the glaring needs of high-poverty, low-performing schools. Given the geographic concentration of poverty, it is not practical to simply or only encourage students and families to enroll elsewhere. Therefore the primary goal of the SIG proposal focusing on high-poverty schools is to allow districts to choose evidence-based reforms and to implement them for a longer period of time.

The size of investment accorded to each of the four proposed focus areas is based chiefly on the amount of validated evidence about their effectiveness. (See Table 1 of the Appendix for counts of experimental-design studies with positive effects.) Admittedly, the reliance on experimental evidence to dictate the amount or the topic area in which to invest is highly imperfect. This is because randomized controlled trials or other quasi-experimental methods are not feasible for all types of interventions, and their measurement of long-term effects (that are arguably the most meaningful) is rare. Also, the creation of a research base—an expensive, time-consuming process—is a selective process that often reflects funders' political priorities rather than an agnostic sampling across all possible types of educational interventions. Nevertheless, the prevalence of ineffective or minorly effective education policies should engender caution on the part of the federal Department of Education. So I propose smaller investments to expand beyond school-based programming into family-focused topics as way to minimize financial and political risk from any one failed pilot and in recognition of the current lack of evidence. A secondary reason for the selection of early childhood education quality measurement and SIG for major investments is to build on the momentum derived from the large, recent investments by the federal Department of Education in these areas.

There are some notable absences from this list of policy recommendations. For example, none directly address teacher and principal quality, even though teachers and school leaders are arguably among the most important within-school factors influencing student achievement and attainment. This is because I believe there is insufficient evidence for or against emerging policies for improving educator quality (like new teacher evaluation systems premised largely on student achievement and formal classroom observation scores or like teacher peer review programs or specific professional development programs) to legislate any of them.⁴ Other reforms that are currently popular such as vocational training in high schools, blended learning, charter school expansion, and extended school day or year also have merit. But, for the reasons I state below, from the perspective of reducing poverty, I favor reforms that focus on early years, that reach as far as is tenable into out-of-school factors, and that increase to the degree possible the stability of high-poverty environments.

In summary, the intentions of these proposed policies are to:

- combat instability in low-income families and schools by intervening at key junctures and time periods—namely, a child's earliest years, and at times of school selection
- direct a majority of dollars into school-based programming where: Department of Education has greatest influence; where there is already a track record of substantial investment and momentum; and where the research base is most developed
- direct a minority of dollars into family-focused policies to expand the evidence base in light of the importance of out-of-school factors to children's academic achievement and attainment
- appreciate how easily and often reforms have either no lasting positive effect or have unintended negative consequences by funding lower-risk pilot programs in areas where evidence is too thin to justify major expenditures, and

⁴ Intuitively, I subscribe to the theory that improving workplace conditions is the single most important means by which to attract and retain high quality teachers and school leaders in high-poverty schools. But this is also an insufficiently defined and tested theory to legitimate specific federal policy making.

- leverage one of the strengths (in my view) of the federal Department of Education—namely, offering well-funded competitions to encourage state and district innovation in specific topic areas that we conceptually know perpetuate poverty but where programs have had little success historically.

1. Major investment: The next generation of Early Learning Challenge (ELC) Grants should focus on advancing the measurement of early childhood education and care quality.

Due to rapid brain development in early years and importance of the timing of environmental exposures to that development, “early experience has the potential for greater impact on adult outcomes than later experience” (Nelson & Sheridan, 2011, p. 1381). As Duncan and Magnuson (2011) explain, in the preschool years of age 3-5 it is important to develop a set of early skills that can, in turn, set the stage for subsequent feedback loops that promote or deter subsequent school success and learning. For example, the early development of reading or math skills can mean placement with other high skilled children (and greater peer effects), greater enjoyment and positive affirmation, and more participation in the activity at home and in school. Duncan and Magnuson (2011) categorize the set of important early skills as (a) academic skills—e.g., identifying letters of the alphabet, associating sounds with letters that appear at the beginning and end of words (aka, decoding), recognizing numbers and shapes and comparing sizes of objects—, (b) attention skills like the ability to sit still and to concentrate, and (c) behavior and mental health, which include pro-social behaviors like the ability to get along with others.

Experimental evidence from several model preschool interventions for at-risk children show short- and long-term academic achievement gains, reduction in special education services, reduced grade retention, reduced dropping out of high school, reduced criminal activity, and increased adult educational attainment (Blau & Currie, 2006). Of course, the effectiveness of small model programs like Perry Preschool does not necessarily mean the same will hold for scaled-up programs that offer less intensive services. Studies of preschool programs that operate at scale (like Head Start, early Head Start, and state programs) have yielded primarily positive results for participants, but the results do not necessarily hold for all groups, some are measured over the very short run, and some effects have faded out rapidly for certain groups (see Ladd, Muskin, & Dodge (2014) for a recent review this literature). There is also evidence that early language and social skill interventions at ages like two and five and six can prevent future, expensive remediations such as through special education (Nelson & Sheridan, 2011). A recent study adds to this literature by examining the medium-run effects of large-scale early programs on the broader community—not just participants—and finds positive effects equivalent to 2-4 months of instruction for third graders from two North Carolina early childhood policy initiatives (Ladd, Muskin, & Dodge, 2014). There is also the suggestion of so-called “sleeper effects” from preschool—initial effects that seem to fade and the re-emerge later, such as from Head Start in terms of future reductions in crime (Deming, 2009).

With these primarily positive but still mixed results in mind, the question of how to ensure that the newly expanding state programs are of high quality takes on additional importance. The importance of quality seems obvious—it receives significant attention in Senate and House legislation as well as Obama’s preschool proposal—but there is no consensus in the research on the links between particular measures of program quality and children’s development. In fact, non-experimental and quasi-experimental research has found weak or no relationship between structural measures of quality (e.g., of inputs like group size or staff education and training) and children’s outcomes (Sabol et al., 2013;

Auger et. al, 2012; Burchinal, 2010; Burchinal, Kainz, & Cai, 2011). And there are conflicting findings, although mostly positive, that process measures of instruction like caregiver-child interactions are more closely associated with children's cognitive gains (for positive results, see: Sabol et al., 2013; Burchinal et. al., 2010, 2011. For null results, see Weiland et al., 2013). Given the lack of consensus about which measures matter and then how best to promote them, it remains possible that "the current measures of quality may simply not be strong measures of the classroom quality factors that improve children's academic outcomes" (Weiland et al., 2013, p. 207).

The infusion of funds into the expansion and improvement of early childhood education and care makes this the moment to examine the basics about measuring quality before state Quality Rating and Improvement Systems (QRIS) —a summary measure akin to a school report card grade that is based on a multipoint scale – and licensure requirements become entrenched. In other words, this is a key moment at which to examine what to measure, how to measure it, how often, for whom (e.g., small family child cares up to large commercial centers), at what cost, and where to regulate or offer incentives for increased quality.

Working from knowledge gained thus far in validation studies, the next generation of ELC grants can go one step further by adding priority points to grantees not just for having and validating the bundle of standards within QRIS (as in prior rounds of ELCs), but in working with other state grantees (such as through the formation of state consortia) to fund the deeper investigation of which practices promote quality and how. For example, process-measures related to use of curricula, child assessments, and caregiver-child interactions are thought to be more closely aligned with desired child outcomes like early academic skills or greater attention. Do multiple validation studies find such a link, how are these measured (e.g., with which Environment Rating Scales), and how should these measures, if important, be validated in state quality rating systems? Likewise, there is much work to be done related to quality measures for student subgroups, such as for children with disabilities. Also, the consolidation of measures could help streamline complex quality improvement systems and thereby reduce the costs associated with their implementation. As the early childhood education field expands, this is a key stage at which ELC funding priorities could influence the direction of its expansion. In short, investigation of how best to structure state quality improvement systems and an understanding of the trade-offs inherent to the adoption of any measures of quality could influence the fundamentals of early childhood education.

2. Major investment: Retool SIG to focus on longevity of implementation.

Just as poverty is geographically concentrated in some neighborhoods rather than evenly dispersed, so too is poverty concentrated within schools. As of the 2010-2011 school year, almost one in five public schools had student bodies where 75 percent or more qualified for a free or reduced price meal (Aud et al., 2010). Given the high correlation between poverty and low performance, it is hardly surprising that the average poverty rate is 78 percent in School Improvement Grant (SIG) schools—a grant targeting the bottom performing five percent of schools (US Department of Education, 2011). As described above, the concentration of poverty poses a host of problems for schools, including the sorting of more highly qualified teachers away from poor schools, higher student absenteeism, greater rates of administrator and teacher turnover, embattled school environments, fewer economic resources, lower rates of parental involvement, and fewer positive peer effects. Duncan & Magnuson (2011) found that high-poverty classrooms have four times the concentration of academic, attention, and behavior problems as low-poverty classrooms (pg. 2071).

Primarily due to the disproportionate contribution of the lowest-performing high schools to national dropout rates, the Obama administration initiated School Improvement Grants (SIG) in 2009. SIG are three-year grants to states that state education agencies then competitively grant to its school districts to substantially improve its lowest performing schools. Since inception, SIG has provided grants to approximately 1,500 low-performing schools, and these grants have ranged up to \$2 million per school. Schools receiving a SIG grant must commit to adopt one of four prescribed turnaround models:

- transformation: at least five required elements, the most significant being the replacement of the principal, implementing a comprehensive school reform, and rewarding teachers for performance.
- turnaround: nine required elements, the most significant being replacing the principal, provide the principal operational flexibility over budget, schedule, staffing; rehire no more than 50 percent of the original staff; implementing a comprehensive school reform; and expanding learning time.
- restart: close the school and reopen under new management, where new management could be charter school or a non-profit or for-profit management organization.
- school closure.

Districts have chosen the transformation option, the least invasive model, for approximately three-quarters of its SIG schools (US Department of Education, 2011). The evidence base for the effectiveness of these four turnaround strategies, per se, is generally weak (Herman et al., 2008, Table 2). But there is experimental evidence of effective comprehensive⁵ school reform models (Borman et al., 2003; Tuttle et al., 2013).

Even though research has found positive effects on student achievement from some comprehensive school reform models—especially ones that have focused on curriculum and instruction—they generally take three to five years for effects instead of the one to three-year turnaround envisioned in SIG (Borman et al., 2003; De la Torre et al., 2012). This comports with prior qualitative research documenting a so-called “implementation dip” and the rough estimate that change requires three to five years of implementation before taking hold (Fullan, 2001).

SIG could improve by lengthening considerably the three year grant period to encourage longer implementation of a given comprehensive school reform over, say, five to ten years. And it could do this more effectively by recommending districts (while allowing some flexibility for inevitable contextual variation) release a large year one grant to schools to cover the large, up-front one-time costs of adopting a comprehensive school reform. Then, to create an incentive for continuity, after a drop in grant funds in year two, annual school grant funds could escalate thereafter until the end of the grant period. Payment of the graduated annual allocation would be conditioned on: (a) continued implementation of the original comprehensive school reform; (b) meeting of interim benchmarks that measure the fidelity of the reform; and, (c) the direct linkage of proposed alterations to evidence from interim benchmarks.

⁵ Comprehensive in this context means a school improvement program that makes every effort to “integrate the research-based methods into a comprehensive design with aligned components that has the support of teachers, administrators, and staff at the school, and that includes measureable goals and benchmarks for student achievement” (Rowan, Camburn, and Barnes, 2004, p. 10).

The idea is to create an incentive for schools to adopt a reform, develop expertise in its implementation, adapt it based on evidence of learning, and be rewarded for sticking with it even across the inevitable changes in school leadership and staffing. As John Easton, the former director of the Institute of Education Sciences, stated in a speech about education research in 2014, “low performing schools don’t become high performing schools by implementing proven interventions. They become great schools by becoming learning organizations that chose carefully, monitor, discuss, analyze, adapt and refine.” Rather than expecting one to three year fixes, SIG grants should be realistic about the need for time. In fact, SIG grants should provide explicitly the frame to encourage schools to learn and refine. Within turbulent high-poverty environments, relatively long grant periods could potentially help to increase continuity and to form a more coherent, stable school culture.

Given the complexity and inevitable specificity of school contexts, a cookie-cutter approach to reform is bound to fail. So a secondary suggestion beyond adopting long grant terms is that SIG drop the mandate to choose one of the four turnaround categories (of which one is the most used anyway) and thereby provide districts greater flexibility to select reforms that fit their specific context. For example, the replacement of a principal may make sense in most cases, but not in all. SIG grant-making should, however, retain its general intent, which is requiring the adoption of a research-based comprehensive school reform for which there is strong, positive evidence. Acceptable proposals must be comprehensive, but they could range from adoption of comprehensive school reforms, curricula, and add provision of wrap-around services like health care, professional development, a changed teacher workforce, altered student assignment procedures, or likely some combination of several of these elements.

Importantly, with a longer grant period, SIG grants could provide the structure for and thus legitimately require that grantees simplify their reforms and not attempt to do everything at once, right away. In his study of school improvement efforts, Mintrop (2004) found that school-improvement plans identify an average of 50 discrete activities each year. Educators and education research has bemoaned incoherent, vague and overly complex plans for school reforms (Rowan, 2011; Schmoker, 2014). The intent of SIG grants should be for schools to keep it relatively simple, require a focus on the instructional core (while also allowing for non-school services), and then work outward over years to augment, adapt, refine, and expand. Hopefully, SIG thus restructured could encourage longevity of curriculum, of staff, of student assessment practices, and allow struggling schools to get good at their reform. While longer grant periods will most certainly mean fewer but bigger grants for low-performing schools, the sustained focus on fewer schools will also hopefully better match districts’ constrained capacity to engage and oversee turnaround schools.

3. Minor investment: School information should be better connected to parents during periods of choice.

In urban districts especially, the link between one’s place of residence and public school assignment is becoming more attenuated. This is largely due to the growth of publicly-funded school choice since the 1990s, when charter schools⁶ were first created. And charter schools are just one form of publicly

⁶ Charters are publicly funded schools that hold a contract (i.e., a charter) with a state or a jurisdiction. The terms of the contract must be renewed (usually every 3-5 years) depending on whether the school meets accountability standards. In exchange for this accountability, charters are provided increased flexibility in terms of hiring and firing teachers, selecting themes, school year, and budgetary flexibility. About five percent of all public school students nationally are enrolled in charter schools, although that percentage varies widely by district and is likely

funded choice; there is also open enrollment, managed or controlled choice plans, magnet schools, tuition tax credits, and school vouchers.

Depending on the extent of school choice, parents can have the unenviable task of sifting through an array of information and requirements that are not aligned across schools. Selecting schools gets substantially more complicated when siblings are enrolled in multiple public schools. For example, schools calendars, application deadlines, application forms, and acceptance deadlines are not necessarily aligned, which can create an arduous school search for even the most motivated of parents.

There is mounting evidence that parents are often unable to navigate what can be a confusing, disjointed system. First, parents may not have sufficient information about schools to make an informed choice; Neild (2005) and Lubienski (2008) argue that the tools parents use to make school choices are not detailed or high-quality enough to be effective. Second, parents may have difficulty navigating school application cycles and admissions processes across choice options. Third, research in behavioral economics and psychology suggest that having too many choices can induce “choice fatigue” (Reutskaja and Hogarth, 2009).

For lower-income parents to take advantage of their choices, they need information that is both readily available and makes sense of their *effective* set of choices—i.e., schools within reasonable geographic reach, serving the right grade levels, and with desired characteristics like theme, academic performance, and/or desired enrichment activities such as band or sports. Using an experimental design, Hastings, Van Weelden and Weinstein (2007) found that when parents receive clear, simplified information, particularly about school test scores, they are more likely to choose higher-performing schools for their children. But not all presentations of school choice information are equal; in a case study of school choice in Washington, D.C., Buckley and Schneider (2007) found that a comprehensive website established to allow parents to compare public school options cost \$200,000 but was hardly used. The simple provision of information (such as via online school report cards or a paper directory of schools) does not make the information easy to consume.

I am aware of one published study about the effectiveness of informational school choice coaching to parents of PK-12 children (Daugherty et al., 2013). In light of its potential, I propose a smaller financial focus on this topic to develop the evidence base. This could be done via allocation of priority points within a vehicle like *Investing in Innovation* (i3) grants for proposals to offer parents information about choices at any of the following key junctures: (a) selection of preschools; (b) structural moves into or from elementary, middle, or high schools; (c) selection of post-secondary schools; or (d) residential moves.

For example, pilot programs might hire retired teachers and principals to serve as coaches who could offer a combination of coaching via calls and/or home visits to parents who sign up for the service. Other pilot programs might consider technological means for offering parents information and nudges as application dates come due. School choice counseling might be an arm of the Nurse Family Partnership. Depending on the level of schooling targeted, school choice counseling services might be piloted directly with teens. Public housing authorities might consider piloting school information (that is compiled by educators) as a part of its housing counseling for housing choice voucher tenants. A notable low-cost variant on this theme is the potential to inject specific school information into online for-sale or

to increase since many states recently lifted caps on the number of charter schools allowed for eligibility for federal Race to the Top grants.

rental listings—i.e., information about the residentially-assigned public schools and or most proximate schools of choice added to the same page where individual homes are listed.⁷

Eligible grantees could include a wide range of potential providers such as school districts, offices of child care licensing, consortia of schools, public housing authorities, universities, or local community based organizations. The only restriction is that programs must not steer parents or children to or from a particular set of schools based on non-objective criteria—e.g., school districts must not restrict counseling to only district-run public schools, for example. In so funding pilot programs, the hope is that this grant making could build a base of evidence for subsequent expansion of programming if effective at least on a small scale. Ideally, such an approach complements the turnaround of lowest performing schools by also directing investments to encourage parents to choose the best possible schools available to them.

4. Minor investment: Educate parents to educate children.

Families invest time and money in children, which influences children's outcomes (Becker, 1991). And recent evidence described above suggests that the chief source of advantage for children who come from upper income families derives from the child's first five years. These children enter and remain ahead of their lower-income peers. This suggests that policies intended to equalize children's opportunities to learn should influence lower-income parents' involvement, expectations, and actions, especially during their children's earliest years.

However, a review of experimentally tested programs that have aimed to alter family educational practices offers a dim view on the potential of such programs to substantially alter student achievement and school readiness (Furstenberg, 2011). In fact, Furstenberg concludes that "we may be overestimating our capacity to alter family practices in ways that will reduce the achievement gap between advantaged and disadvantaged children" (pg. 11758).

Even with this serious limitation in mind, I nevertheless propose an additional priority area within a grant vehicle like i3 for parent education campaigns, just as I did for informational coaching about schools. Even if campaigns to alter family practices were low impact, so long as those impacts were widespread and the cost of implementation relatively low they could well be cost-effective and thus sustainable. By soliciting pilot programs from a wide range of potential applicants—offices of child care licensing, school districts, school consortia, district consortia, chambers of commerce, public housing authorities, health care providers, charter management organizations—the hope is to test a wide variety of high- to low-intensity programs that educate parents about themes like the "countdown to kindergarten—activity of the week," about the importance of schooling to children's future outcomes, about how to promote children's readiness for school, and about the availability of services.

These programs could test a variety of media, a variety of ways to reduce the barriers parents experience that prevent them from promoting their children's education, and they could test new technological applications. They could include high-cost variants like home visits or case management, medium-cost variants like hospital-based delivery of school information paired with mandatory wellness visits, or low-cost variants like text messaging. For example, were schools to couple educational information with needed services in "parent academies," they could both teach parents how to access school services and offer free services such as arranging for local doctors, counselors, or volunteer services. By testing pilot ideas on the small scale, the risk from any one failed study is minimized. While

⁷ This idea comes from a colleague, Peter Bergman.

the evidence base from experimental designed research is admittedly thin, the bottom line is parents are too important to ignore when seriously considering how to reduce the income-based achievement gap.

Conclusion

I have proposed four ideas intended to reduce the negative effects of poverty on children's academic performance and achievement. To increase the likelihood of their success, I argue that these ideas should re-use federal competitive grant making structures, rather than create new mechanisms for distribution of funding from the federal to local grantees. The limited success of anti-poverty school-based programs to narrow the achievement gap thus far should provide caution. For this reason, I try to direct the majority of the hypothetical federal funding to school-based programming for which there is the most research evidence and to build on the momentum of ongoing large investment by the federal Department of Education. But the importance of out-of-school influences combined with the depressing absence of proven programming to alter family educational practices demands that educators pilot new ideas. The importance of schooling combined with the large and growing income-based gap in educational achievement underline the need for action.

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Appendix

Figure 1. Income Achievement Gap

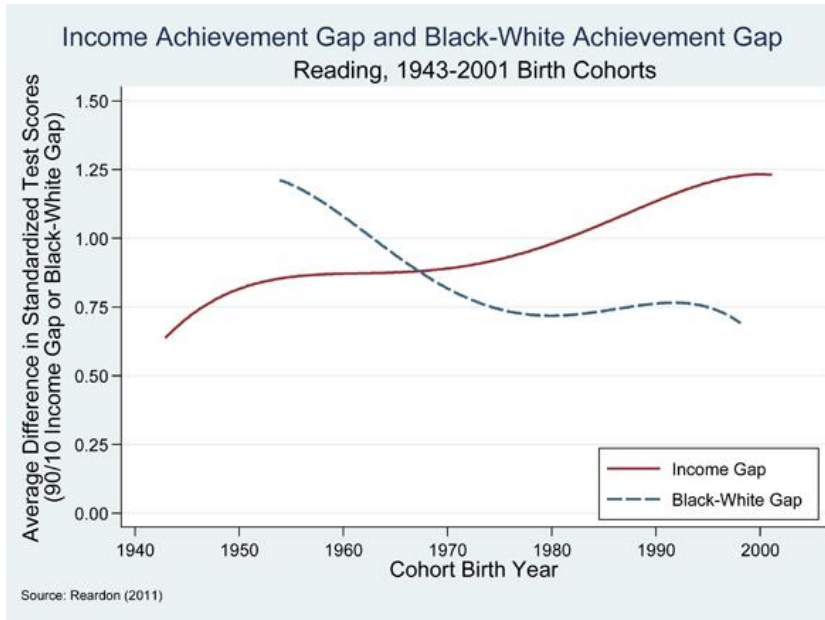


Table 1. Proven education programs for which there is high quality evidence from studies with experimental designs

Topic area	What Works Clearinghouse (Department of Education)	Coalition for Evidence-Based Policy	Best Evidence Encyclopedia (Center for Data Driven Reform in Education, Johns Hopkins University)
Dropout prevention	2		
Preschool / early childhood interventions	4		6
Literacy curricula	11		11
Math curricula	7		7
Social & emotional behaviors	5		
Charter schools	2		
Professional development for teachers	2		
School turnaround models		1	3
Parental education programs		1	

Website ies.ed.gov/ncee/wwc/ toptierevidence.org bestevidence.org

Notes: For What Works Clearinghouse, the number within each cell represents the unique programs that have rigorous research evidence, statistically significant positive findings, and where the extent of the evidence is "medium to large." Blank cells indicate that the website did not include these categories, which may or may not mean there are zero proven programs within that category.