Published in final edited form as:
Am Psychol. 2012 November ; 67(8): 681–689. doi:10.1037/a0030085.

Low-Income Children's Self-Regulation in the Classroom: Scientific Inquiry for Social Change

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Abstract

Over 21% of children in the United States today are poor, and the income gap between our nation's richest and poorest children has widened dramatically over time. This article considers children's self-regulation as a key mediating mechanism through which poverty has deleterious consequences for their later life outcomes. Evidence from field experiments suggests that low-income children's self-regulation is modifiable by early educational intervention, offering a powerful policy option for reducing poverty's negative impact. The author discusses ways that scientific models of self-regulation can be expanded to include multiple developmental periods and real-world classroom contexts. Recommendations for advances in research design, measurement, and analysis are discussed, as are implications for policy formation and evaluation.

Keywords

poverty; self-regulation; prevention; emotion; executive function

Arguably one of the greatest social problems we now face in the United States is that of the widening income gap and educational inequality between affluent children and poor children. As recently as 2010, over 21% of children in the United States are poor: More startling, perhaps, is the reality that the income gap between our nation's richest and poorest children has widened by 40% to 50% over the last 25 years (Reardon, 2011). In addition, the consequences of this gap in income for students' opportunities for learning are very large: To put it in perspective, the gap in academic achievement between our nation's richest and poorest kindergarteners is now two to three times larger than the achievement gap between Black and White children (Reardon, 2011). The income gap has increased the likelihood of poor children's exposure to worsened school conditions, lower neighborhood safety, and lower family resources (Murnane & Duncan, 2011). In short, the evidence from the last 20 years suggests that poverty has grave consequences for the formation of human capital in the

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Author's Note. The content is solely the responsibility of the author and does not necessarily represent the official views of the Eunice Kennedy Shriver National Institute of Child Health and Human Development or the National Institutes of Health. The Chicago School Readiness Project (CSRP) is not associated with The Chicago School of Professional Psychology or any of its affiliates.

Editor's Note. C. Cybele Raver received the Award for Distinguished Contributions of Applications of Psychology to Education and Training. Award winners are invited to deliver an award address at the APA's annual convention. This article is based on the award address presented at the 120th annual meeting, held August 2–5, 2012, in Orlando, Florida. Articles based on award addresses are reviewed, but they differ from unsolicited articles in that they are expressions of the winners' reflections on their work and their views of the field.

next generation, increasing the risk of children's lower academic achievement and heightening their risk of serious behavioral difficulty. Recent reviews by a range of scholars across social science disciplines have underscored our confidence that poverty causes these negative outcomes (Yoshikawa, Aber, & Beardslee, 2012). The next pressing scientific question is not whether poverty is bad for children, but how poverty exerts such deleterious effects and what we can do to mitigate those effects.

When applied to this pressing social problem, developmental science has identified a wide range of likely mechanisms by which poverty takes such a negative toll. Multiple pathways of poverty's influence can be conceptualized primarily at three levels: at the individual level, at the level of corroding interpersonal relationships, and at the institutional level, where educational, health, and child care services for poor families may be of lower quality (see Gershoff, Aber, & Raver, 2003, and Yoshikawa et al., 2012, for more extensive discussion). In the following review, I focus on a key individual process—namely, child self-regulation—as one powerful potential mechanism by which poverty-related stressors shape low-income children's chances for positive educational outcomes. I then focus on ways that we as prevention scientists may leverage the other two types of pathways—namely, interpersonal relationships between teachers and children and the quality of preschools that young children attend—as policy solutions to the costs that poverty exerts on children's self-regulatory development. I next discuss new theoretical and methodological directions for the fields of developmental science, prevention science, and policy analysis in order for us to better understand the ways that we might structure educational opportunities for children facing economic disadvantage. I argue that social scientists can serve as powerful social actors in political economies, where educational policies for children can yield dramatic shifts in children's life course trajectories (though most of the changes in those policies are frustratingly accretive and glacially slow). But first, it is to a brief review of the empirical "lens" of self-regulation in classroom contexts that I now turn.

Self-Regulation as Key Hypothesized Mediator of the Impact of Poverty

Self-regulation is defined as the primarily volitional regulation of attention, emotion, and executive functions for the purposes of goal-directed actions (Blair & Ursache, 2011, p. 305). Disparate areas of research in executive function, effortful control, and emotion regulation all point to the ways in which children's exposure to the chronic ecological stressors associated with low income, in turn, has serious negative sequelae for their ability to modulate their emotions, their higher order cognitive function, and their behavior (Blair et al., 2011; Evans & English, 2002). Increasingly, neuroscientific evidence suggests support for the hypothesis that chronic stressors associated with poverty-related adversity exert their influence through neuroendocrine pathways (see Blair & Raver, 2012, and McEwen & Gianaros, 2010, for reviews). Children experiencing lower levels of executive function and more difficulty with behavioral and emotional self-control have been found, in turn, to face significantly greater risk for difficulty in educational contexts (Brock, Rimm-Kaufman, Nathanson, & Grimm, 2009; Li-Grining, Votruba-Drzal, Maldonado-Carreño, & Haas, 2010; Raver, 2002).

This stark empirical evidence of the cost of poverty and its related risks to children's self-regulation supports an important theoretical insight: that is, that children's cognitive and affective regulation at the behavioral and neurobiological levels is relatively malleable or plastic in the face of changing environmental conditions (see Blair, 2002; Bryck & Fisher, 2012; McEwen & Gianaros, 2010). That same theoretical model also implies the potential for environmental reversal of poverty's negative consequences (Blair, 2002; Blair & Raver, 2012; Bryck & Fisher, 2012). Working from similar models, many prevention scientists have recently underscored ways that children's environments offer pivotal opportunities for reversing the negative neurobiological and behavioral consequences of poverty through comprehensive intervention (Jones, Brown, & Aber, 2011; Shonkoff, 2011). Put simply, we can leverage the very large amount of time that many low-income children spend in out-of-home settings in ways to support their self-regulation, which may in turn substantially improve their chances of academic success.

Testing Developmental Models While Also Examining Feasibility for Social Change

Evidence From Randomized Controlled Trials

Correlational evidence amassed over the last three decades in developmental psychology was clearly supportive of the role of children's self-regulation in learning. However, that evidence was not sufficiently persuasive to overcome skepticism among many members of both the social science and policy communities (Duncan et al., 2007). A key problem was that the correlational findings of children's lower self-regulation predicting significantly lower academic achievement could be the result of a third set of unobserved child, family, or community characteristics that might underlie that observed association (Duncan, 2003).

It is within this framework that cluster-randomized efficacy trials of interventions targeting children's socioemotional skills have had a major impact on developmental science. First, randomized controlled trial (RCT) interventions offer a means of directly testing whether children's self-regulatory skills are environmentally modifiable over short periods of time. Second, randomizing some preschool programs and not others to services targeting children's self-regulation allows us to test the causal role of children's emotional and behavioral competence in their academic achievement (see Greenberg, 2006; Kellam, Ling, Merisca, Brown, & Ialongo, 1998; Raver, 2002).

To address these questions, our research team conducted a cluster-randomized trial with community-based Head Start programs in seven of Chicago's most economically disadvantaged neighborhoods. Head Start sites were randomly assigned to receive multicomponent intervention services (and therefore serve as the "treatment group") or to receive a lower intensity package of services (including a lower cost teacher's aide in the classroom one day a week). These latter sites were designated as "control group" classrooms. Our intervention staff marshaled several primary programmatic components to improve low-income preschool-age children's self-regulation. These programmatic components included 30 hours of teacher training in classroom management strategies (e.g., rewarding positive behavior, redirecting negative behavior) that were hypothesized to

provide children with more effective regulatory support (Webster-Stratton, Reid, & Hammond, 2001; Webster-Stratton, Reid, & Stoolmiller, 2008). Weekly "coaching" through classroom-based consultation was also provided to teachers, as were stress reduction workshops for teachers to help limit burnout. Classroom consultants also worked one on one with three to five children who exhibited the most challenging behavioral problems.

The Impacts of the Chicago School Readiness Project (CSRP)

As a preliminary test of whether our intervention was successfully implemented, we first examined whether CSRP made a difference in teachers' classroom management behaviors. To avoid the risk of measurement bias, we relied on independent observers' ratings of classroom quality using a new standardized measure. Our first set of analyses suggested that teachers in treatment-assigned Head Start sites were successfully able to provide children with significantly more emotionally and behaviorally supportive classroom environments than were teachers in control group-assigned Head Start sites (Raver et al., 2009).

Our second set of analyses suggested that this classroom-based intervention also led to clear reductions in children's emotional and behavioral difficulty (Raver et al., 2009). For example, children in the treatment group were reported by their teachers as having significantly fewer internalizing (or sad and withdrawn) behavior problems than their control group-enrolled counterparts by spring of the Head Start year. Children in the treatment group were also reported by their teachers to show significantly fewer externalizing (or aggressive, disruptive, and acting out) behaviors than were children in the control group in the spring of the Head Start year.

Most important, our third set of analyses provided clear evidence of CSRP's benefit for young children's self-regulation and opportunities for learning. These analyses confirmed that CSRP improved low-income children's executive function skills (as indexed by assessors' ratings of children's attention/impulse control as well as by direct assessments) from fall to spring of the Head Start year. Analyses also suggested significant benefits of CSRP for children's pre-academic skills, as measured by direct assessments of children's vocabulary, letter-naming, and math skills (Raver et al., 2011). The effect sizes of this intervention were substantial, ranging from .34 to .63. From a policy perspective, these findings provided clear support for specific steps that programs might take to improve school readiness for children through a comprehensive, classroom-based approach. From a scientific perspective, these findings also provided us with persuasive evidence that children's self-regulation is environmentally modifiable. When targeted through classroom-based intervention, changes in self-regulation also lead to socioemotional and academic gains for children facing high poverty-related risk.

These findings from CSRP are in keeping with several other recent trials suggesting the modifiability of children's self-regulation in classroom contexts. New evidence from several recently implemented preschool interventions is promising (see Diamond & Lee, 2011, for a review). For example, low-income preschool-age children receiving the comprehensive preschool REDI (or Research Based, Developmentally Informed) intervention designed to improve their socioemotional and preacademic skills were found to demonstrate stronger levels of self-regulation on a direct assessment of attention and impulsivity at posttest,

compared to low-income preschoolers in the control group (Bierman, Nix, Greenberg, Blair, & Domitrovich, 2008). What is particularly intriguing is that changes in interpersonal relationships and in the quality of settings may have biobehavioral benefits with health impact as well. Recent findings from a comprehensive home- and school-based intervention suggest that regulatory support provided by the important adults in children's lives not only reduces children's behavioral risk but also has a significant impact on neuroendocrine indicators of reactivity to stress (e.g., lower cortisol) and substantially reduces low-income children's later risk of obesity (Brotman et al., 2011, 2012).

Expanding Our Models of Self-Regulation to Include Adults

Working on CSRP from 2003 to 2006 also keenly sharpened our attention to the likely cost of poverty and its stressors to the cognitive and emotional self-regulation of adults as well as of children. In a series of subsequent articles, my colleagues and I considered ways that many preschool teachers, themselves earning incomes that placed their families just above the poverty line, faced high levels of stress both at home and at work (Li-Grining, Raver, et al., 2010). In giving teachers concrete strategies to manage children's negative and acting out behaviors, CSRP intervention services also significantly reduced teachers' feelings of stress on the job (Zhai, Raver, & Li-Grining, 2011). Widening our empirical lens to consider self-regulation processes among adults as well as among the children in the classroom yielded important insights for policy. Interventions such as CSRP might lower teacher turnover and might have significant benefits for teachers' emotional and psychological well-being. These findings also provided an important starting point from which to develop new theory regarding the ways that adult executive function, attention, and emotion regulation may be affected by negative interactions with others (in this case, children) in the heat of the moment. We look forward to continuing to pursue those lines of inquiry in future research.

Strengthening the Science That Undergirds Social Action: New Directions for Theory and Methods

The Need for New Theory

These new questions in our field call for the development and validation of new theoretical lenses through which to understand children's self-regulation in classroom contexts. The first of these questions concerns the interrelation between children's cognitive control, or executive function, and their emotional regulation. Emergent literature in developmental neuroscience clearly indicates support for top-down models in which prefrontal and parietal cortical systems work jointly to aid children in responding in a reflective rather than a reactive way to emotionally threatening stimuli (Zelazo, Qu, & Kesek, 2010). Yet the subcomponent processes of the ways in which children can marshal planning, inhibition, and strategic deployment of attention to aid in the modulation of emotion are still largely unexplored. In addition, recent work in social neuroscience suggests support for bottom-up as well as top-down processes, in which individuals' exposure to intense negative or threatening stimuli (as well as individuals' difficulty in modulating emotion) can also impede cognitive control (Blair & Dennis, 2010; Dennis, Chen, & McCandliss, 2008; Fox, Russo, & Dutton, 2002).

Recent work in developmental and cognitive neuroscience on children's processing of threatening stimuli is particularly relevant to poor children, who have a much higher likelihood of exposure to violent events in their homes and school neighborhoods. Reanalysis of CSRP data highlights both the tremendous scientific relevance and pressing policy relevance of this question. Results from that reanalysis suggest that the occurrence of homicide in close proximity to CSRP children's homes had clear negative consequences for their attention and impulsivity (Sharkey, Tirado-Strayer, Papachristos, & Raver, in press). Theoretical and empirical insights from developmental cognitive neuroscience are badly needed to help us understand the possible mechanisms that undergird findings such as these.

The second theoretical question we face is one of external validity, or generalizability of our field's strongest neuroscience findings to real-world contexts. That is, we have reached a critical juncture across fields of developmental science, cognitive neuroscience, and prevention science at which it is fair to ask whether innovative and empirically supported neurocognitive and affective models underlying children's self-regulation can "translate" from lab settings to the social and academic contexts of the classroom. Children's maintenance of cognitive control and accurate detection and appropriate response to affective stimuli may be increasingly critical as they move into later grades, where they face demands of test taking, more rigorous academic demands, and complex social situations involving acceptance versus rejection among their peers. Consider, as just two examples, recent work on ways that students' academic performance on standardized tests is so substantially lowered by affective priming of anxiety or feelings of threat (Inzlicht, McKay & Aronson, 2006). How can neurocognitive findings from laboratory contexts regarding the role of high negative affect in disrupting executive function be applied to understand this key domain of student achievement?

Similarly, additional work is needed on ways that children must repeatedly detect, correctly interpret, and respond to their own emotion as well as peers' social cues of anger or rejection on the playground or in the lunchroom in order to successfully navigate the social landscape of school. Major strides have been made in assessing the correspondence of physiological, behavioral, adult-report, and self-report measures in both executive function and emotion regulation, which allow for expanded opportunities to measure children's executive function and emotion regulation in settings such as schools (McCoy, Raver, Lowenstein, & Tirado-Strayer, 2011; Vasilev, Crowell, Beauchaine, Mead, & Gatzke-Kopp, 2009). Those measures can now be strategically deployed to better understand key developmental questions regarding, for example, ways that chronic exposure to negative emotional experiences such as chaotic classrooms, peer conflict, or bullying may shape neuroendocrine and neurocognitive function among children (see Ouellet-Morin et al., 2011; Watamura, Coe, Laudenslager, & Robertson, in press). Extending this line of inquiry into social action would logically lead us to test school-based interventions that aim to ameliorate both the neurocognitive and behavioral sequelae of early, chronic exposure to emotional stressors inside and outside classrooms.

A third question is how we conceptualize models of self-regulation over time. To date, there are exceptionally few studies of self-regulation from infancy through adolescence or young adulthood. Across areas of executive function and emotion regulation, investigators have

focused either on the preschool period or the early adolescent period, but the large majority of studies have been cross-sectional or have spanned several years within either developmental period but not across them (Herba & Phillips, 2004). Walter Mischel's studies on self-regulation and later life outcomes, Nancy Eisenberg and colleagues' longitudinal studies in effortful control, as well as Caspi and Moffitt's extensive analyses of the Dunedin Multidisciplinary Health and Development Study offer tremendous promise (Eigsti et al., 2006; Eisenberg et al., 2004; Moffitt et al., 2011). To date, analyses from these studies have yielded broader brush models of the role of adversity in self-regulation but do not yet offer a fine-grained model of self-regulatory component processes across executive function and emotion regulation in family, school, and neighborhood contexts.

More recent longitudinal studies, including the Environmental Risk (E-Risk) Longitudinal Twin Study (led by Moffitt), the Family Life Project (led by Vernon-Feagans, Greenberg, Cox, Blair, Burchinal, and colleagues), and smaller longitudinal studies by Nathan Fox and by Stephanie Carlson, are currently underway. Some of these studies provide an early suggestion of remarkable stability in self-regulation, where undercontrol at age 3 predicts difficulty with inhibitory control, as indicated by problem gambling, at age 32 (Slutske, Moffitt, Poulton, & Caspi, 2012). Other studies of cortisol from preschool through adolescence suggest that even the neuroendocrine processes that underlie cognitive and affective regulatory processes themselves undergo dramatic change and that patterns are not stable over time and require complex modeling to capture trajectories that are interpretable (Hankin, Badanes, Abela, & Watamura, 2010).

However, there are few theoretical guideposts suggesting what we might expect for the development of both the prefrontal cortical and subcortical neural systems (and the connectivity between them) that underlie self-regulation if children experience continuity versus discontinuity in poverty-related stressors across time (see Blair & Raver, 2012, and Raver et al., 2012, for additional discussion). Such theoretical models will be crucial if we are to test ways that trajectories of self-regulation can be supported versus adversely affected by higher versus lower quality relationships and in higher versus lower quality classroom contexts. In our own laboratory, my colleagues and I have had the chance to examine how direct assessments of executive function, effortful control, and emotion regulation in preschool predict teacher ratings of self-regulation in early elementary school and direct assessments of sensitivity to reward, executive function, and emotion regulation as children enter fourth or fifth grade (see Raver et al., 2012, for a fuller description of the measures). We are in the process of considering how those trajectories of self-regulation may be deflected for those children who experienced chronic exposure to lower school quality compared to other children in the CSRP sample who were able to attend higher quality schools from kindergarten through fifth grade, while we also account for early individual differences in self-regulatory skill in preschool. These data will be invaluable in helping to build new models of the independent and combined roles of poverty and exposure to positive versus negative school quality when predicting trajectories of cognitive, emotional, and behavioral regulation over time.

Advanced Quantitative Methods: New Tools in Building Scientific Evidence for Social Action

The Need for New Measurement

New methods allowing investigators to assess the neuroendocrine correlates of executive function and emotion regulation as well as new means for children to report on executive function and emotion regulation in vivo during the school day would undoubtedly offer important insights. Watamura's research (e.g., Watamura et al., in press) in capturing cortisol measures during children's experience of socially stressful contexts of child care offers a good example regarding the value of psychobiological data for these questions. With regard to behavioral measures and subjective self-report, a recent study (Tan et al., 2012) of clinically anxious 9- to 13-year-olds versus controls suggests that cell-phone ecological momentary assessments also demonstrate significant promise. The use of a new methodology to capture children's real-time emotion regulation in that study revealed some similarities and some differences from prior clinic- and lab-based findings: While clinically anxious youths reported higher intensity of negative emotion, they did not report using significantly fewer emotion regulation strategies such as avoidance, distraction, and problem solving than did controls during typically occurring stressful events during the week (Tan et al., 2012). To date, we have only begun to scratch the surface in understanding the cross-sectional and longitudinal linkages between children's neuroendocrine and neurocognitive functioning, their performance on direct behavioral assessments of self-regulation in the lab, and the ways that those processes unfold in real time in classroom contexts. In short, we have a tremendous need to develop and validate new measures if we are to successfully map out the likely processes that alternately support or constrain children's self-regulation in school settings.

Advances in Study Design and Analysis

In many ways, our field has increasingly embraced the value of experimental designs in real-world social settings as a way to inform both science and policy. But there may be productive means of further leveraging experimental designs in ways that expand both our scientific understanding and our policy priorities. Not only do experiments offer much stronger grounds on which to draw causal inferences, but they also provide opportunities for ways to think through and test policy options that have real-world relevance to the practitioners and professionals who are often our collaborators. For example, in designing CSRP, my colleagues and I were pressed to imagine scenarios in which we might obtain significant treatment effects that were not driven from key, predicted features of our intervention but might instead be due to covarying confounds. We imagined that one objection might be that our provision of classroom-based mental health consultancy simply added an extra pair of hands to the classroom. If we were fortunate enough to detect significant differences between treatment- and control-assigned classrooms, we wanted to rule out the skepticism that those treatment effects might be due to the altered ratio of students to staff that mental health consultancy might introduce. To address this concern, we decided against the use of a "business as usual" control group design, instead electing to

provide control group classrooms with teacher's aides as part of a low-intensity services dose so that both groups would be equivalent on staffing ratios.

We learned through experience that experimental designs in classroom contexts offer new opportunities to think carefully about our counterfactuals, where we can leverage the contrast between groups to brighten the line between competing, theoretically driven models of classroom and behavioral change. This type of a priori mental rotation of our favored ideas about intervention also forced us to consider the value of the counterfactual from a policy perspective. With limited resources, child care center administrators might actually prefer the lower cost option of adding a teacher's aide rather than a more expensive mental health consultant into classrooms that were noisy, chaotic, and emotionally negative. By testing our intervention against a feasible policy-relevant alternative, we effectively hedged our empirical bets: We increased our confidence that results of the study would be socially actionable, regardless of whether the evidence ended up favoring the intervention group or the control group.

It was important that we did not rely solely or too simply on experimental designs. Why not rely solely on them? Building and sustaining scientists' and policymakers' commitment to the problem of poverty and children's higher risk of school failure has required an integration of social science findings that span aims of description, prediction, and causal inference. A clear empirical description of the scope and depth of the problem of income poverty among families with young children in the United States provides key motivation and justification for the importance of this as a research question. Second, many of the most innovative and compelling intellectual sources for our ideas about mechanisms (including the prior research evidence that undergirded our plans for intervention) were predictive or correlational in design. Our own and others' randomized controlled trials were conducted to meet the third aim of making causal inferences regarding possible policies that might support children's self-regulation. Just as centrally, those studies offered an opportunity to experimentally test key questions of developmental mechanism that were basic to our models grounded in developmental science. It is critical that we maintain this threefold focus on description, prediction, and causal inference if we want to identify and fix social problems while also building and testing rigorous models within developmental science.

While experimental design will continue to be central to our field's success in rigorously testing our hypotheses, it is now time to recognize and address the additional complexities that experimental approaches raise. For example, it has long been recognized that the experimental design is of limited external validity in that findings cannot be generalized too extensively beyond the time and place in which the intervention was implemented (Shadish, Cook, & Campbell, 2002). From a longitudinal, trajectory-based perspective in developmental science, this represents a major drawback of RCT design. Specifically, long-term follow-up of children in an intervention will estimate the average causal effect of the intervention in early childhood over the large number of life events that occurred subsequent to exposure to that intervention. Yet children's encounters with those additional life events and settings are nonrandom and may be a result of the very "selection" characteristics that pose threats to causal inference in the first place. One solution proposed by Raudenbush (2001) is to remember that "treatment" (or participation in a given intervention for a given

period of time) may actually be best conceptualized as a set of treatments, the first of which is the only one that is randomized. The rest of the life events that follow are endogenous to that first randomized treatment; the set of treatments can best be conceptualized as time-varying (Raudenbush, 2001).

With this framework in mind, my colleagues and I examined the longer term impact of CSRP, considering children's nonrandom enrollment into higher versus lower quality kindergartens as a second treatment that needed to be statistically taken into account. Our findings were grave: CSRP's impact was found to be null, overall. However, when we took school quality into account, matching and comparing treatment-assigned children who nonrandomly "sorted" or enrolled into higher quality schools with similar children from the control group who had the same propensity to attend high-quality schools, we found robust and positive effects of CSRP on key academic outcomes (Zhai, Raver, & Jones, 2012). In short, the benefits of treatment were sustained only for those children who were able to find their way into higher quality kindergartens. For those children who subsequently attended lower quality schools, the effects of CSRP were lost, presumably as a function of children's subsequent exposure to kindergartens that were noisy, crowded, and underresourced in many other ways.

Propensity score matching is increasingly applied to policy analyses of the impact of preschool education, with evidence, for example, that high-quality prekindergarten investments have socioemotional benefits, including increased attentiveness among those children who are enrolled in the extra year of schooling (Gormley, Phillips, Adelstein, & Shaw, 2010). These analytic methods, as well as fixed effects methods for considering the role of change in environments on change in child self-regulation, hold tremendous promise for applied developmental science and intervention research. When applied to large, longitudinal data sets, these methods help to expand the external validity of our assertions regarding the effects of a range of environments for children's self-regulation. With these methods we can model development not only within the child and across time but also across many different types of children, levels of intervention quality, and types of environments.

In this way, our team finds itself iteratively focused on the three aims of description, prediction, and causal inference, described earlier, in order to understand how the children in our study are faring as they move through middle childhood and stand at the threshold of adolescence. The descriptive power of data on the quality of Chicago's schools was incontrovertible: Our preschool intervention represented a drop in the bucket when considering the larger class size, more variable quality, and higher number of stressors experienced within many of the city's public schools. Prediction continues to play a key role in our analyses as we consider the propensity of children to be exposed to new stressors, including violence in home and school neighborhoods as well as informal and formal school supports. We have had to find new ways of conceptualizing "treatment," such as exposure to more supportive adults and higher performing peers, and new analytic methods with which to estimate children's propensity to be exposed to those treatments as we follow this group of children forward.

Conclusions

This review has focused on ways to ameliorate the impact of poverty-related stressors on children's self-regulation through classroom-based intervention. Recent innovations in prevention science have highlighted that interpersonal relationships between children and the important adults in their lives can be leveraged to support young children's self-regulation. In so doing, committed, dedicated practitioners, families, and social scientists can and do improve children's academic and health outcomes in many ways. In addition, these intervention studies highlight the ways that the quality of institutions that serve young children can be substantially improved as feasible and affordable policy solutions to the toll that poverty takes on children's self-regulatory development. But a broader question may be what the impact of reducing poverty itself might be on children's self-regulation. As recently pointed out by Yoshikawa et al. (2012), robust estimates of the role of the Earned Income Tax Credit (EITC), the nation's largest antipoverty policy, demonstrate significant and meaningful benefits for children's academic gains. Yet it is unclear what the mechanisms are that underlie those academic gains and whether they included significant change for children's self-regulation at neurocognitive, affective, and behavioral levels. That question will not be answered until the next generation of experimental evaluations of antipoverty programs is conducted, with executive function and emotion regulation as excellent candidates for inclusion as key child outcomes.

Bold experiments in antipoverty policy and in education policy may be needed in order to significantly move the dial on the gap that was highlighted earlier. Consider the experiment of school desegregation in the early 1960s, enacted in this nation 50 years ago. That dramatic policy change yielded gains in human capital that have transformed our nation's social, political, and economic landscape (Johnson, 2011). Unfortunately, those bold social experiments are experienced by many of us in the social science and prevention communities as very few and too far between. What we need is a set of policy experiments that test the impact of multiple economic and educational investments designed to improve children's experiences both inside and outside the classroom to support their self-regulation. We need to evaluate those experiments in ways that yield compelling and persuasive answers to our pressing scientific questions regarding the plasticity of brain development and behavior across multiple developmental periods and multiple social contexts. We learned an enormous amount about employment, income, and child well-being from welfare reform experiments (Grogger & Karoley, 2005; Morris, Gennetian, & Duncan, 2005). Until we undertake such experiments in the area of comprehensive classroom-based intervention, we risk engaging in what Brooks-Gunn (2003) described as "magical thinking," where we hope that short-term and partial solutions implemented in a restricted set of out-of-home settings such as during a few months of preschool may undo the effects of poverty. Ten years ago, I argued in a Society for Research in Child Development *Social Policy Report* that "it may be unreasonable to expect … that a short-term program lasting a year or less will 'inoculate' a child from the debilitating consequences of a chronic, recurring set of material hardships such as deep poverty, inadequate housing, and violent surroundings" (Raver, 2002, p. 13). That concern is even more strongly underscored today, as the poverty rate among children in the United States has risen from just under 17% in 2002 to 21% in

2010 (Macartney, 2011; Proctor & Dalaker, 2003). Rather than relying on magical thinking, we must be honest about the neurobiological, behavioral, and academic costs of poverty. We must be creative and deeply committed in developing and testing new models of intervention. We must remain both sanguine about the limitations and hopeful about the benefits of classroom-based intervention in ameliorating poverty's costs in order to enact social change through scientific inquiry.

Acknowledgments

The project described was supported by Award R01HD 046160 from the Eunice Kennedy Shriver National Institute of Child Health and Human Development.

I thank the policy professionals, teachers, families, and CSRP intervention and research teams who made this work possible.

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