

Give Us This Day Our Daily Breadth

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As with any discipline, the field of child development progresses by both deepening and broadening its conceptual and empirical perspective. The rewards to refinement are impressive, but there is little need for encouragement in this area, since existing disciplines, universities, and funding agencies reward depth. The current study makes the case for breadth: for combining insights from different disciplines and methods in synergistic ways. Examples include influences of family poverty on children, inequality and child development, and methods for assessing impacts of policies. Drawing together disparate ideas from different research traditions can be not only time consuming and frustrating but also deeply rewarding, both scientifically and personally. The study closes with thoughts about how departments, universities, funding agencies, and Society for Research in Child Development itself might promote interdisciplinary inquiry.

Half a century ago, the Nobel laureate economist Frederick von Hayek wrote: “The economist who is only an economist is likely to become a nuisance if not a positive danger” (von Hayek, 1956, p. 463). Many economists have taken this to heart. Some have partnered with Daniel Kahneman and Amos Tversky to bring principles of cognitive psychology into the economics of decision making (e.g., Thaler & Sunstein, 2003). Others have shown how neuroscience enriches behavioral economics with its use of functional neuroimaging and other methods to understand what is going on in the brain as we make decisions involving risk and present–future trade-offs (Camerer, Loewenstein, & Prelec, 2005).

In contrast to these interdisciplinary successes, few insights from developmental psychology have penetrated the conceptual models or empirical methods that economists and many sociologists use in their numerous studies of child well-being. Until Cunha and Heckman (2007), economic models typically assumed an undifferentiated single period of childhood in which optimizing-investment decisions are made by parents or governments on behalf of children (e.g., Becker, 1991). When adoles-

cents are studied, they are typically assumed to be as fully rational in their decision making as adults. Psychometric considerations are typically ignored, and mediational models are rarely seen.

Worse yet, all child capabilities other than IQ and achievement are often lumped into a single category and termed, of all things, “noncognitive” (e.g., Heckman & Rubinstein, 2001). As a result, economists’ empirical studies lump attention problems, antisocial behavior, mental health, motivation, self-esteem, locus of control, and whatever other measures happen to be available in the data set at hand into a single index of “noncognitive skills.”

There are, of course, a number of important exceptions to this broad-brush tarring of my economist and sociologist colleagues, some of which I will mention in the following, but the point remains that developmental psychology is generally invisible within these disciplines. As a frequent reviewer of proposals and manuscripts submitted by economists and sociologists regarding child well-being, I find myself repeatedly encouraging authors to incorporate at least the simplest insights from developmental psychology into their conceptual models or empirical methods.

And what about the reverse? To what extent does developmental science open itself up to the models and methods of other disciplines? One testament to Society for Research in Child Development’s

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(SRCD) interdisciplinary character is the requirement that every third president come from a field outside of developmental psychology—an affirmative action policy that led to my election. But the vast majority of SRCD members are developmental psychologists. As a frequent reviewer of proposals and manuscripts submitted by developmental psychologists, I find myself repeatedly urging authors to incorporate even the simplest insights from the models and methods of economists and sociologists. Enough of this disciplinary balkanization!

In this article, I attempt to make the case for breadth, for combining insights from different disciplines and methods in new and imaginative ways. Referring to evolution, the French biologist and Nobel laureate Francois Jacob observed that “to create is to recombine” (Jacob, 1977, p. 1163). So it can be across the social, behavioral, and biomedical sciences.

Let me be clear that by “breadth” I mean true integration of disciplines at the levels of concepts, assumptions, theories, methods, and interpretation. In a wonderful essay on interdisciplinary scholarship, Aletha Huston (in press) invokes Mildred Parten’s descriptions of young children’s peer interactions (Parten, 1932). The first stage is *parallel* play—side-by-side but essentially independent activities, with a research example being a conference in which scholars from different disciplines present papers on a common general topic. *Associative* play consists of independent activities using a common set of materials, as with children pursuing their own projects in a sandbox or, in research, mixed-method studies if the research teams using the different methods are not working closely with one another.

Cooperative play involves truly interdependent interactions. Here, Huston (in press) uses the example of children working together to build one structure, with each child placing blocks on those added by others. In the case of research, this conception of “breadth” means interactions among disciplines in which each builds on, and is responsive to, the others. Lee Cronbach describes the benefits and nature of cooperative play as follows: “We rarely see a topic in proper perspective if our inquiry employs resources from only one discipline. . . . With regard to problems I have worked on, someone with a disciplinary base far from my own has often supplied a relevant framework my analysis had missed. . . . Interdisciplinary contacts will have their greatest benefit when each participant individually makes colleagues’ idioms part of his or her own thinking” (Cronbach, 1986, pp. 98–99).

I, too, have found that drawing together disparate ideas about development and methods from different research traditions can be not only time consuming and frustrating but also deeply rewarding—personally and perhaps also for the promotion of science, policy, and practice.

I will make my case for breadth using examples from research on the effects of poverty on children’s development. I first argue that poverty research needs a genuine life-span perspective, beginning in utero and stretching well into adulthood, and it needs to embrace early biology as a key dimension of development.

I then show that the *macroeconomic* dimension of poverty and affluence—in other words income inequality—appears to be highly consequential for child development. I look next at innovative methods of estimating causal impacts of income policies on child outcomes. All of these issues are best addressed through interdisciplinary collaboration. I close with some thoughts about how we might promote this kind of “cooperative play.”

Toward a More Comprehensive Model of Poverty and Child Development

Nearly one fourth of America’s children have family incomes below the poverty line, currently around \$17,000 for a family of three. The standard developmental model of family poverty posits a dual pathway in which growing up poor hurts children (Figure 1). First is the “what money can buy” pathway. Higher income parents can buy more books, horizon-broadening travel, higher quality child care, better schools, and safer neighborhoods than their poor counterparts.

Second is the maternal stress pathway, which emphasizes how the day-to-day stresses of poverty can compromise parents’ mental health and the

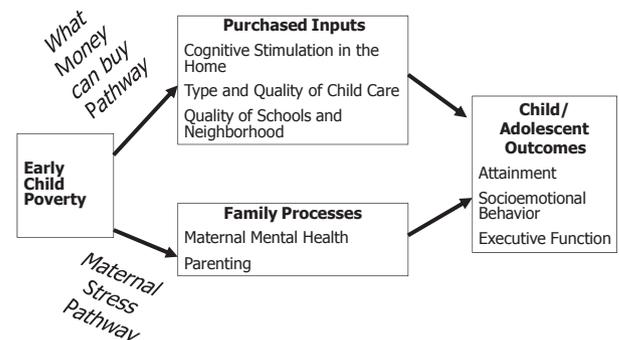


Figure 1. A mediated model of family poverty.

quality of interactions with their children. Taken together, these two pathways are thought to translate poverty into lower achievement and worse behavior when poor children enter school, and these forces may overwhelm the compensatory efforts of schools to level the playing field by the time children transition from adolescence into adulthood.

Before concentrating on the shortcomings of this model, let me praise its utility and durability. Vonnie McLoyd's (1990) work on the maternal stress pathway helped to stimulate voluminous research on the consequences for children of maternal depression and harsh parenting. *The consequences of Growing Up Poor* (Duncan & Brooks-Gunn, 1997) provided considerable support for the "what money can buy" pathway and also pointed to early childhood as a particularly sensitive period for impacts of economic deprivation on child well-being.

To gauge the size of the income effects, let us use a yardstick of a \$3,000 increase in annual income, which is well within the scope of U.S. income support policies. After controlling for differences in an extensive set of parent characteristics, and for income in later childhood stages, Duncan, Brooks-Gunn, Yeung, and Smith (1998) showed that for children living in low-income families, a \$3,000 increase in annual income (in 1993 dollars) between birth and age 5 was associated with a highly significant eight tenths of a year increase in completed schooling. In contrast, the estimated effects of increases in income during middle childhood and adolescence were smaller and not statistically significant. Even eight tenths of a year of additional completed schooling is important, as that effect size has been found to lead, on average, to tens of thousands of dollars in higher career earnings (Card, 1999), less unemployment (Mincer, 1991), and better health (Cutler & Lleras-Muney, 2010).

These estimates by Duncan, Brooks-Gunn, and colleagues were based on 20 years of data from the Panel Study of Income Dynamics project. Data collection on the children in this study has continued. Selecting children born into the study in its early years—1968–1975—we can now estimate associations between poverty as early as the prenatal year and a host of adult outcomes, measured as late as age 37.

Using procedures similar to those followed in the Duncan et al. (1998) article, Duncan, Ziol-Guest, and Kalil (2010) found that for low-income children, a \$3,000 annual increase in family income (in 2005 dollars) between the prenatal year and age 5

was associated with a 17% gain in earnings between ages 25 and 37. If sustained throughout an individual's career, this would translate into nearly \$200,000 in higher career earnings. As with completed schooling, income later in childhood and in adolescence appeared to be far less important than income earlier in life.

These links between poverty early in childhood and earnings three decades later are truly remarkable. Eventual labor market success is an unconventional although important outcome for child development research; higher earnings provide obvious benefits for workers and their families. And for economists and policy makers, they signal the worker productivity that fuels a nation's prosperity.

What is behind these associations? Testing the "what money can buy" and "maternal stress" pathways was not possible with Panel Study of Income Dynamic (PSID) data drawn from the project's early years. But the PSID data cover a range of adult outcomes that provide clues regarding process (Table 1). The first row confirms, as we have already seen, that increments to family income early in childhood have a statistically and substantively important association with earnings later in life.

Suppose we take the PSID and estimate an identical model of income effects but substitute for adult earnings two behavioral outcomes—arrests and out-of-wedlock childbearing. Income early in life is *not* a significant predictor of either of these outcomes, which suggests that the positive effect of early childhood income on adult earnings is unlikely to be operating through some kind of problem behavior pathway.

Let us keep expanding our list of adult outcomes. Early childhood income *is* strongly linked to adult work hours. Something about avoiding poverty in the early years of life is enabling children to sustain full-time working careers in adulthood. Remarkably enough, our work has shown that income in the prenatal and birth years reduces the

Table 1
Regression-Adjusted Associations With Early Family Income

Adult earnings (+)	$p < .05$
Arrests	<i>ns</i>
Nonmarital birth	<i>ns</i>
Adult work hours (+)	$p < .01$
Adult obesity (–)	$p < .05$
Adult hypertension (–)	$p < .05$
Adult arthritis (–)	$p < .05$

Source: Duncan, Ziol-Guest, and Kalil (2010) and Ziol-Guest, Duncan, and Kalil (2011).

likelihood of obesity in adulthood (Ziol-Guest, Duncan, & Kalil, 2009). And preliminary work appears to show reductions in adult hypertension and arthritis as well (Ziol-Guest, Duncan, & Kalil, 2011). Perhaps research on childhood poverty should be spending as much time thinking about pathways related to health as pathways related to achievement and behavior.

Doing so would open up exciting new opportunities for interdisciplinary collaborations. Such collaborations would connect us to rich literatures spanning psychology, neuroscience, and epidemiology, which point to detrimental consequences of in utero and very early life stressors on cognitive, social, emotional, and health outcomes. We have long known about the harm caused by in utero exposure to teratogens such as alcohol (Lanphear, Wright, & Dietrich, 2005). But stressors ranging from earthquakes to partner violence have also been shown to affect the length of gestation and neonatal health. Gary Evans and his collaborators have highlighted associations between childhood poverty and allostatic load, a biological index of the cumulative wear and tear on the body, measured in the teenage years (Evans, Kim, Ting, Tesher, & Shannis, 2007). Andrea Danese and others are showing links between child stressors, such as maltreatment, and dysregulation of the immune system (Danese, Pariante, Caspi, Taylor, & Poulton, 2007).

These pathways challenge our well-worn model of poverty effects. Instead of the “what money can buy” and “maternal stress” pathways linking childhood poverty to child and adolescent outcomes, we need to start earlier, end later, and include more boxes (Figure 2). Poverty effects may begin prenatally and extend well into adulthood. The body’s stress and immune systems become important pathways linking poverty with later outcomes. And the outcomes themselves do not fit neatly into attainment and behavioral categories but instead relate to an individual’s ability to sustain a healthy and productive adult life.

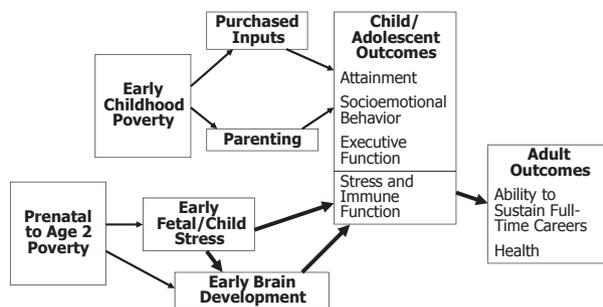


Figure 2. Revised model of family poverty.

SRCD members are among the leaders in the development of this expanded multifactor conceptualization, but their work has not been embraced by many developmental researchers and is largely unknown to the majority of economists and sociologists. The payoff to interdisciplinary collaborations using this expanded model of poverty effects could be quite high.

Child Development and the Macroeconomy

For my second example of the virtues of interdisciplinarity, let me explore the outer reaches of Bronfenbrenner’s (1979) ecological model. His macrosystem includes political and cultural influences and, with little fanfare, the macroeconomic and policy influences. They are so distant from the child, in his child-centric model, that it is tempting to follow the lead of astronomers in dealing with Pluto and classify them as Kuiper Belt Objects rather than as worthy planets. But we do so at our peril.

Let us think big about economics and child development. When sociologist Glen Elder delivered his SRCD presidential address 14 years ago, he featured the connections between economic hardship and child development detailed in his landmark book *Children of the Great Depression* (Elder, 1998).

But an even larger, and more important, set of macroeconomic forces are at work. As documented by Goldin and Katz (2008), technological change rewards and, eventually, influences the acquisition of skills from an evolving workforce. In the first three quarters of the twentieth century—until about 1975—technology improved the productivity and pay of both higher and lower skilled workers. Since then, these rewards have shifted and fueled a massive increase in inequality with large but little-noticed consequences for child development.

Figure 3 shows changes in family income over the past half century, with all incomes expressed in 2008 dollars. In 1947, the income threshold separating the poorest 20% of families from everyone else was a little over \$13,000. Between 1947 and 1977, the economic fortunes of poor families improved markedly, with incomes doubling to about \$26,000. Between the mid-1970s and today, technological change has favored only the most educated, as can be seen in the painfully slow growth—to only \$28,000—in the incomes of low-income families.

What about the richest one fifth of families? Between World War II and the late 1970s, their incomes doubled as well—from about \$40,000 to

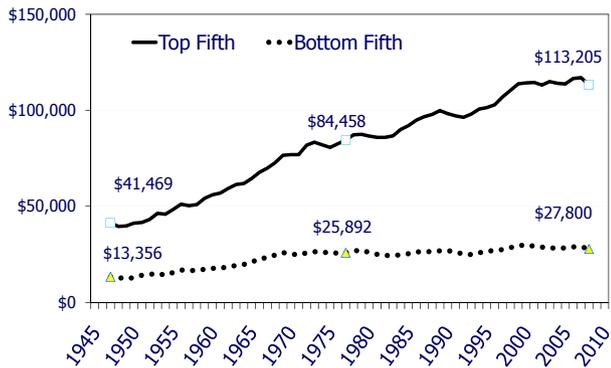


Figure 3. High and low family incomes, 1947–2008.
Source: Author calculations based on data from the U.S. Bureau of the Census.

\$84,000. But in contrast to low-income families, the incomes of affluent families have continued to increase. Of course, *really* high-income families did even better. By choosing the top 20%, I am including 15 million American children and providing a broader population view of child well-being.

A key question for developmentalists is whether the powerful forces fueling inequality have affected the developmental fortunes of children. Sean Reardon (2011) examines this question using achievement test scores, which are one of the few developmental indicators that have been gathered consistently over the past 40 years (Figure 4). To get our bearings, let us look first at familiar trends in the Black–White test score gap. Figure 4 presents smoothed data based on the National Assessment of Educational Progress. Among children born in the early 1950s, shortly before *Brown versus Board of Education*, Black children scored 1.25 *SD* below White children when tested in the ninth grade. Over the next two decades, as the quality of schools attended by Black children improved, these gaps narrowed—by a remarkable .5 *SD*—but have changed little since then.

But now look at the achievement gaps defined by income, based in this case on various national surveys. As with the race gaps, we have smoothed the line in order to focus on trends. The specific comparisons here are test score gaps between children at the 10th and 90th percentiles of the family income distribution.

Among children born around 1950, test scores of low-income children lagged behind those of their better-off peers by a little over .5 *SD*. For children born 50 years later, this gap was twice as large! This is hugely important. Fifty years ago, the race gap was much larger than the income gap. Now,

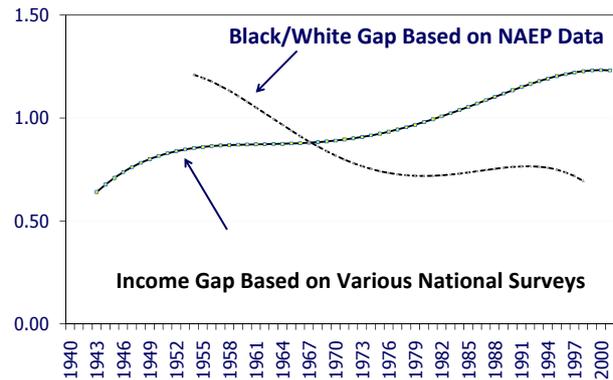


Figure 4. Achievement gaps by race and income, by birth year.
Source: Reardon (2011).

the reverse is true. Not surprisingly, the income gap in completed schooling has grown as well (Duncan & Murnane, 2011a). All told, this inequality story means that the “economic” planet in Bronfenbrenner’s solar system is looking more like Jupiter than Pluto. And yet developmental journals almost never publish articles focused on the consequences of macrolevel trends in inequality or other economic conditions.

Importing New Methods

An examination of how increasing inequality affects children’s attainment, and what might be done about it, could fill a book. In fact, it already has. Richard Murnane and I have edited a volume with Sean Reardon’s analysis, as well as the work of many others, describing the ways in which inequality works through the labor market, neighborhoods, families, and schooling to affect children’s educational attainments (Duncan & Murnane, 2011b).

I joined the book project midstream, after its structure of 25 chapters and 53 authors and coauthors had already been determined. It is instructive to look at the disciplines of the 53 contributors. They include numerous sociologists and economists, two neuroscientists who help to explain the role of experience in the brain’s development, and several interdisciplinary scholars—but no mainline developmental psychologists. This is symptomatic of the fact that policy-focused studies of family, neighborhood, school, labor market, and policy contexts published in developmental journals have little visibility outside of the developmental sciences.

In fact, there are very few developmental articles focused on the effects of systematic variations in policy. Policy implications abound. But they are inferred, often with leaps of logic, from analyses of the effects of conditions such as child-care quality or parenting that might be affected by policy changes.

To be sure, developmental researchers have an enviable *experimental* tradition with intervention studies that are widely known throughout the social and behavioral sciences. And, as Aletha Huston observed in her 2007 SRCD presidential address, experimental studies of, for example, early childhood education programs are viewed as highly credible in Congress and other policy arenas (Huston, 2008).

But the scope and expense of random-assignment policy experiments limit their use. Lacking genuine experiments, how can we study policy effects? Economists and some sociologists and epidemiologists have developed sophisticated quasi-experimental approaches for this task. But here again, the absence of interdisciplinary collaboration with developmental psychologists means that these economic and sociological studies are quite unsophisticated with regard to developmental theory and measurement.

In my view, more use of natural experiments holds the greatest promise for a new generation of policy-relevant development studies. Urie Bronfenbrenner (1979) and past SRCD president Sir Michael Rutter (2009) are among the advocates of natural experiments, which work by taking advantage of variation in key independent variables that are beyond the control of the family or child being analyzed. As illustrated by the use of dizygotic and monozygotic twin births to estimate heritability, natural experiments have a long tradition in developmental research. But not for understanding the impacts of policy.

Let me give an example. The U.S. Earned Income Tax Credit (EITC) provides income supplements to low-income working families that can amount to as much as \$5,600 per year—a huge boost to the incomes of families supported by minimum wage jobs. Suppose that we are interested in understanding whether income from the EITC promotes child development but lack the resources to mount a randomized trial. As we believe that increased family income improves child outcomes, we would expect to find that children are better off with the EITC policy in place than they would otherwise be. And as we believe that one of the pathways to improved child well-being may involve reduced maternal

stress, it would be valuable to be able to test for that as well.

What data and tools can be used to answer this question? Between 1993 and 1996, the generosity of the EITC increased sharply. For a single mother with two children and earnings of about \$10,000, the credit was more than \$2,000 higher in 1996 than in 1993. Moreover, and here is the good part, the increase of \$2,000 in the credit for families with two or more children was more than 3 times as large as the increase for families with just one child. So, if income matters for child and maternal outcomes, we should see a bigger improvement for children and mothers in two-child low-socioeconomic-status (SES) families than in single-child low-SES families (Figure 5).

Gordon Dahl and Lance Lochner (in press) used data on children’s test scores gathered before and after the jump in EITC benefits. They estimated that benefit increases of \$3,000 produced a .20 *SD* increase in the test scores of children most likely to receive EITC benefits (Figure 6).

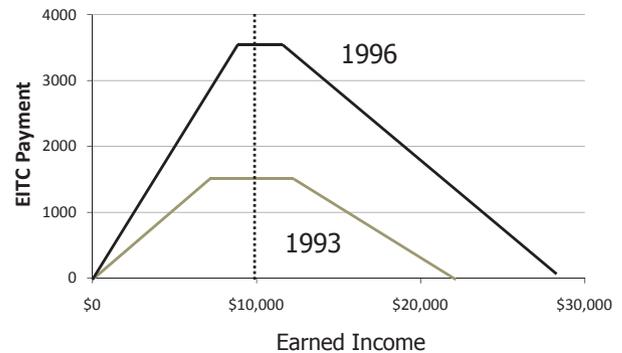


Figure 5. Earned Income Tax Credit (EITC) payments for family with 2+ children.

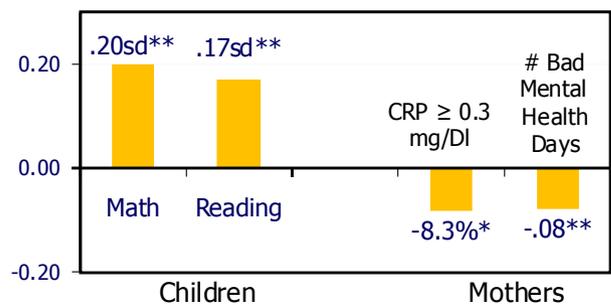


Figure 6. Effects of Earned Income Tax Credit (EITC) expansion on children’s test scores and mothers’ health. Source: Dahl and Lochner (in press) and Evans and Garthwaite (2009). **p* < .05. ***p* < .01.

How to test the maternal stress pathway? Evans and Garthwaite (2010) examined data from the National Health Examination and Nutrition Survey gathered before and after the time of the EITC expansions. Remarkably, they found that low-SES mothers with two or more children, when compared with mothers with just one child, experienced larger reductions in risky biomarkers such as diastolic blood pressure and C-reactive protein and self-reported better mental health.

The statistics behind these studies are quite simple. They require no fancy multilevel, latent-growth or structural-equation modeling—only a few interaction terms to isolate the key policy variation across groups and time. I would argue that natural experiments and other tools for sharpening causal inference are at least as important in our empirical work as tools for psychometric or multilevel modeling.

The Case for Breadth

Let us step back and ask the biggest question: How can we best accelerate discoveries in the field of child development? As with any discipline, the field of child development progresses by both deepening and broadening its conceptual and empirical endeavors. The rewards to depth are impressive, and in no way do I want to minimize their importance.

As shown by the examples I have presented, however, there is a clear case to be made for breadth—for combining insights from very different disciplines and methods in new and creative ways. When economist Gary Becker introduced the idea of human capital into economics in the early 1960s, it was dismissed by many outside of the discipline (Becker, 1964). But within 20 years, research based on the human capital paradigm had made great progress in the field of economics and begun to spread to other disciplines. Sociologist James Coleman (1988) then took the concept of “capital”—something of value built up through costly investments—and applied it to social relationships. He called it “social capital” and launched a highly productive research tradition within sociology that has given us, among other things, the idea of a neighborhood’s “collective efficacy” (Sampson, Raudenbush, & Earls, 1997).

I am most familiar with the promise of interdisciplinary links between developmental psychology and other social sciences. Neuroscience is another obviously fertile field. Silvia Bunge writes the synergies in the field of cognitive neuroscience. In

her view, although cognitive neuroscience “is deemed overly reductionistic by psychologists and overly superficial by cellular and systems neuroscientists . . . its intermediate position lends itself to integrative research that spans several levels of analysis.” Indeed, she goes on to argue, “Cognitive neuroscience is the *right level* at which to begin to understand how cognitive developmental trajectories are influenced by such important factors as, . . . hormonal changes during puberty, . . . and socio-economic and cultural contexts” (Bunge & Munro, 2011).

One of my favorite science writers is Matt Ridley. His 2010 book *The Rational Optimist* tackles the daunting task of explaining the dramatic rise in the world’s living standards. For all but the most recent two centuries of the last 2,000 years, the average income of the world’s population hovered around \$500 per person per year (Figure 7). But, around 1800, economic well-being began to grow exponentially and continues to do so today. Why the change? For Ridley, the answer to this monumental question lies more in breadth than depth.

As illustrated by Henry Ford’s assembly lines, *depth* in the form of division of labor played a very significant role in this progress. But increasing specialization surely cannot account for the exponential growth we have enjoyed in the last two centuries. Ridley instead points to a very different mechanism, as he puts it “ideas having sex with other ideas. . . . The history of the modern world is a history of ideas meeting, mixing, mating and mutating. . . . [S]o long as it can hop from country to country and from industry to industry, discovery is a fast-breeder chain reaction; innovation is a feedback loop; invention is a self-fulfilling prophecy.”

As illustrated by Arnold Sameroff’s (2010) SRCD presidential address in Denver and, I hope, my

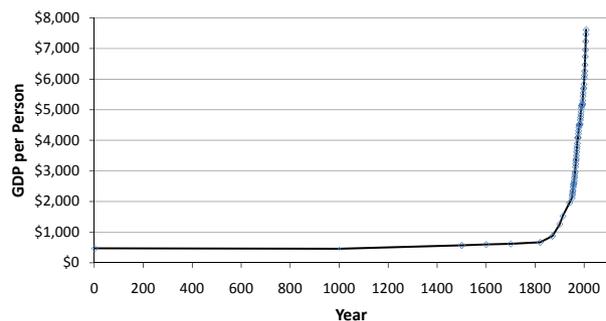


Figure 7. World living standards, 1-2008 AD.

Source: James Maddison, <http://www.ggdc.net/MADDISON/oriindex.htm>

own, mixing, mating, and mutating can also characterize our approaches to understanding the process of child development. Developmental science informed by psychology, economics, sociology, anthropology, history, and biology has much to contribute and to learn.

Broadening the Field of Child Development

Progress in the science of child development must come from depth and breadth. There is little need to worry about depth—the gravitational forces within our traditional social and behavioral disciplines and proposal review panels are robust and likely to stay that way.

How to promote cooperative play among the disciplines so that they combine ideas to yield new and exciting ways of understanding development and behavior? Learning about the key ideas and methods of other disciplines is a costly investment and the rewards are highly uncertain. It is not for everyone, and that is fine.

But how to encourage more of it? One way is interdisciplinary research networks. NICHD's Network on Child and Family Well-Being enabled Jeanne Brooks-Gunn and me to launch our program of poverty research. The MacArthur Foundation has an exceptionally long history of supporting various social and behavioral research networks, and I was fortunate enough to serve on three of them. Jacque Eccles's Network on middle childhood provided the opportunity for Tom Weisner, Aletha Huston, and me to integrate disciplines and methods in the New Hope work support intervention (Duncan, Huston, & Weisner, 2007).

The National Institutes of Health provide grants for program projects—clusters of conventional proposals tied together by a common theme. A stated purpose of NICHD program projects is to encourage multidisciplinary approaches to the investigation of complex problems. NIH-funded population study centers such as Northwestern University's Cells to Society center promote interdisciplinary research among developmental psychologists, biological anthropologists, and more traditional population-related disciplines. And some large data collection projects, such as Fragile Families (<http://www.fragilefamilies.princeton.edu/>) and even the experimental Moving to Opportunity evaluation (<http://www.nber.org/mtopublic/>), engage researchers in cooperative play.

Schools and departments can also take steps to promote interdisciplinarity. Are graduate students

best served by working within one laboratory throughout their graduate school careers or by breadth-promoting rotation among laboratories? Should not we encourage them to take elective courses well outside their particular areas of interest? I believe that graduate students benefit most if they understand how psychologists think about development, neuroscientists think about the brain, economists think about policy and methods, and anthropologists think about culture.

Given the narrow, disciplinary standards for awarding tenure, the years immediately following the attainment of tenure probably provide the most freedom for broadening one's research agenda. Master lectures at professional meetings, seminars in other departments, professional meetings and journals focused on complementary research areas, books by good science writers, and service on interdisciplinary review panels or research committees all provide opportunities for learning about ideas that might mix, mutate, or mate with your areas of expertise. In the end, though, there is no good substitute for a wholehearted interdisciplinary research collaboration.

What about SRCD itself? A key goal in SRCD's strategic plan is to promote interdisciplinary research. For the past 3 years, SRCD has funded proposals for workshops and conferences to promote interdisciplinary and other goals in our strategic plan. And we are starting a set of themed meetings in the years between our biennial meetings. The first three will be in 2012, with themed meetings likely every other year after that. All will be decided through a competitive process. For 2012, one will focus on developmental methodology, a second on positive development among minority children, and a third on emerging adulthood.

But there is only so much that organizations such as SRCD and NICHD, or foundations can do. In the end, cooperative play requires the collective efforts of many individuals willing to put up with the inevitable frustrations of interdisciplinary work for a chance at its rewards. I remember my first set of interdisciplinary network meetings, when it seemed like hours before I could begin to understand what people were talking about. Gradually, I realized that "moderation" really meant "interaction"; that a "causal model" was a description of process rather than a tightly formulated, mathematical model with explicit assumptions; that "fixed effects" means something very different in HLM and in econometric models; that it is measurement errors rather than coefficient bias that keeps developmentalists up at night; that Uri Bronfenbrenner may have been on to

something; and that we might eventually be able to link neurons to neighborhoods. Most of all, though, I came to appreciate the rich rewards of framing questions within the context of the very dynamic processes of development.

I hope that my many collaborators believe that they have learned something from me as well. But it is not so much about individual learning, but rather about the collective construction of new insights and methods. This is where we will find exciting possibilities for genuine breakthroughs in developmental science—in other words: for creation through recombination.

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