REPORTS

CHILDHOOD DEVELOPMENT

Labor market returns to an early childhood stimulation intervention in Jamaica

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A substantial literature shows that U.S. early childhood interventions have important long-term economic benefits. However, there is little evidence on this question for developing countries. We report substantial effects on the earnings of participants in a randomized intervention conducted in 1986–1987 that gave psychosocial stimulation to growth-stunted Jamaican toddlers. The intervention consisted of weekly visits from community health workers over a 2-year period that taught parenting skills and encouraged mothers and children to interact in ways that develop cognitive and socioemotional skills. The authors reinterviewed 105 out of 129 study participants 20 years later and found that the intervention increased earnings by 25%, enough for them to catch up to the earnings of a nonstunted comparison group identified at baseline (65 out of 84 participants).

Early childhood, when brain plasticity and neurogenesis are very high, is an important period for cognitive and psychosocial skill development (*1–3*). Investments and experiences during this period create the foundations for lifetime success (*4–13*). A large body of evidence demonstrates substantial positive impacts of early childhood development (ECD) interventions aimed at skill development (*14*, *15*). ECD interventions are estimated to have substantially higher rates of return than most remedial later-life skill investments (*6*, *8*, *13*, *16*).

More than 200 million children under the age of 5 currently living in developing countries are at risk of not reaching their full developmental potential, with most living in extreme poverty (*17*, *18*). These children start disadvantaged, receive lower levels of parental investment, and throughout their lives fall further behind the advantaged (*15*, *19*, *20*).

The evidence of substantial long-term economic benefits from ECD is primarily based on U.S. data (*21–30*). There are reasons to suspect that these benefits may be higher in developing countries. Children there typically live in homes where the environment is less stimulating than in developed countries. As a result, they enter ECD programs with lower levels of skills. Programs that boost skills are likely to have greater benefits in developing countries because skills are less abundant there. For example, the returns to investment in schooling are typically higher in developing countries (*31*).

We report estimates of the causal effects on earnings of an intervention that gave 2 years of psychosocial stimulation to growth-stunted toddlers living in poverty in Jamaica (*32*). To our knowledge, this is the first experimental evaluation of the impact of an ECD psychosocial stimulation intervention on long-term economic outcomes in a developing country (*33*).

Unlike many other early childhood interventions with treatment effects that fade out over time (*8*, *13*, *15*), the Jamaican intervention had large impacts on cognitive development 20 years later (*34*). We show that the intervention had large positive effects on earnings, enough for stunted participants to completely catch up with

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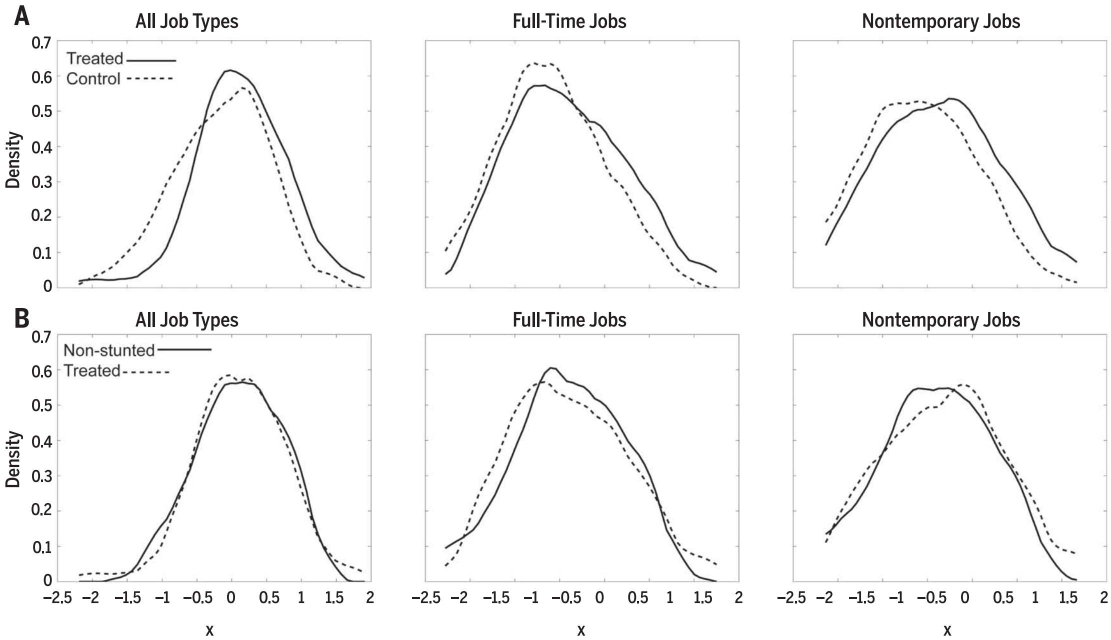

Fig. 1. Impact of stimulation treatment and catch-up on the densities of average earnings at age 22. (**A**) Treated (solid line) and control (dotted line) densities for average earnings. Panel presents the log earnings densities for the treatment (solid line) and control (dotted line) groups using data where earnings of migrant workers who were lost to follow-up were imputed. (**B**) Comparison (dotted line) and treated (solid line) densities for average earnings. Panel presents the log earnings densities for the nonstunted comparison (solid line) and stunted treatment (dotted line) groups, where earnings of migrant workers who were lost to follow-up were imputed. The densities are estimated using Epanechnikov kernels. The treatment densities were estimated with an optimal bandwidth defined as the width that would minimize the mean integrated squared error under the assumption that the data are Gaussian. For purposes of comparability, the same bandwidth was used for the corresponding control group.

a nonstunted comparison group. The intervention compensated for early developmental delays and reduced later-life inequality. The Jamaican intervention had substantially larger effects on earnings than any of the U.S. programs, suggesting that ECD programs may be an effective strategy for improving long-term outcomes of disadvantaged children in developing countries.

The Jamaican Study enrolled 129 growth-stunted children age 9 to 24 months who lived in Kingston, Jamaica, in 1986–1987 (*35*). Section A of the supplementary materials gives a detailed description of the intervention and original study design. The children were stratified by age and sex. Within each stratum, children were randomly assigned to one of four groups: (i) psychosocial stimulation (N = 32); (ii) nutritional supplementation (N = 32); (iii) both psychosocial stimulation and nutritional supplementation (N = 32); and (iv) a control group that received neither intervention (N = 33). The Jamaican Study also surveyed a comparison group of 84 nonstunted children who lived nearby. All participants were given access to free health care.

The stimulation intervention (groups 1 and 3) consisted of 2 years of weekly 1-hour play sessions at home with trained community health aides designed to develop child cognitive, language, and psychosocial skills. The stimulation arms of the Jamaica Study showed significant long-term cognitive benefits through age 22 (*36*, *37*). Moreover, stimulation had positive impacts on psychosocial skills and schooling attainment and reduced participation in violent crimes (*36*).

The nutritional intervention (groups 2 and 3) consisted of giving 1 kg of formula containing 66% of daily-recommended energy (calories), protein, and micronutrients provided weekly for 24 months. The nutrition-only arm, however, had no long-term effect on any measured outcome (*36*, *38*). In addition, there were no statistically significant differences in effects between the stimulation and stimulation-nutrition arms on any long-term outcome, although the arm with both interventions had somewhat stronger outcomes (see supplementary materials, section D). Hence, we combine the two psychosocial stimulation arms into a single "stimulation" treatment group and combine the nutritional supplementation–only group with the pure control group into a single "control" group, understating the benefits of the joint intervention.

We resurveyed both the stunted and nonstunted samples in 2007–2008, some 20 years after the original intervention when the participants were ~22 years old. We found and interviewed 105 out of the original 129 stunted study participants. This sample was balanced. We only observe statistically significant differences in 3 out of 23 variables at baseline (table S.1). In addition, there is no evidence of selective attrition. We also found and interviewed 65 out of the 84 children of the original comparison sample. For that sample there are significant differences in the baseline characteristics of the attrition and nonattrition groups (table S.3).

We estimate the impact of the stimulation intervention on earnings by comparing the earnings of the stunted treatment group to those of the stunted-comparison group. We control for potential bias from baseline imbalances using inverse propensity weighting (IPW) (*39*). We then assess the degree to which the intervention enabled the stunted treatment group to catch up to the nonstunted comparison group by comparing the earnings of the treatment group to those of the comparison group. In the catch-up analysis, we correct for potential attrition bias using IPW weighting. See supplementary methods, section B, for the analysis of baseline balance, attrition, and the details of implementing IPW.

To better understand the external validity of our catch-up analysis, we compare the nonstunted group to the general population using data on individuals 21 to 23 years old living in the greater Kingston area from the 2008 Jamaican Labor Force Survey (JLF) survey. By age 22, the nonstunted group attained levels of skills comparable to those of persons the same age who were living in the Kingston area interviewed in the JLF (table S.4). The two samples are equally likely to still be in school and achieve the same educational level in terms of the highest grade of schooling attained and passing national comprehensive matriculation exams.

Statistical inference is complicated by small sample size and multiple outcomes. We address the problem of small sample size by using exact permutation tests as implemented in (*21*). We correct for the danger of arbitrarily selecting statistically significant treatment effects in the presence of multiple outcomes by performing multiple hypothesis testing based on the stepdown algorithm proposed in (*40*). In addition, we aggregate over outcomes using a nonparametric combining statistic. Section C of the supplementary methods gives details.

The stimulation intervention was designed to improve maternal-child interactions and the quality of parenting. Using the infant-toddler HOME score (*41*, *42*), we examine whether treatment resulted in more maternal investment in stimulation activities at home during the experimental period. The HOME score captures the quality of parental interaction and investment in children by observing the home environment and maternal activities with her child.

The intervention increased the HOME inventory during the intervention period. At baseline, there was no difference in parenting between treatment and control groups (table S.1). At the end of the 2-year intervention, the HOME inventory of the stunted treatment group was 16%, greater than that of the control group (P = 0.01). However, the effect of the intervention on home environment and maternal activities with her child appears to have declined afterward. Using a series of HOME-like questions designed to capture stimulation activities in mid-to-late childhood (*43*), there was no difference between the treatment and control groups at age 7 or later at age 11.

Although most of the direct parental stimulation encouraged by the intervention seems to have occurred during the treatment period, the intervention may have also affected other types of parental investments later in life that, in turn, also contributed to improved earnings. As children exited the intervention period with higher skills, parents may have realized that investments, such as schooling, had higher returns than they might otherwise have thought. Indeed, significant differences in schooling attainment appear at age 17 (*36*). By age 22, the treatment group had 0.6 (P = 0.08) more years of schooling attainment than the control group. The proportion of the treatment group still enrolled in school full-time (0.22) was more than five times larger than in the control group (0.04) ($P \leq 0.01$).

Table 1. Treatment effect on average log earnings at age 22 (statistically significant results in bold). This table reports the estimated impacts of treatment on log monthly earnings for the observed sample with imputations for the earnings of missing migrants (9 observations imputed). The treatment effects are interpreted as the differences in the means of log earnings between the stunted treatment and stunted control groups conditional on baseline values of child age, gender, weight-for-height z-score, maternal employment, and maternal education. Our P-values are for one-sided block permutation tests of the null hypothesis of no treatment effect (single P-value, in parentheses) and multiple hypotheses (stepdown P-value, in brackets) of no treatment. Permutation blocks are based on the conditioning variables used in the treatment effect regressions. The last column uses a combined statistic that summarizes the participant's outcomes. Specifically, we perform a single-hypothesis inference using the average rank across variables as a test statistic. See section C of the supplementary materials for details.

Job type	All job types	Full-time jobs	Nontemporary jobs	Combined (rank mean)
Treatment effect	**0.30**	**0.22**	**0.39**	**0.09**
Single P-value	**(0.01)**	**(0.04)**	**(0.01)**	**(0.04)**
Stepdown P-value	**[0.02]**	**[0.04]**	**[0.02]**	–
Control mean	9.40	9.59	9.67	0.36
Sample size	109	105	82	109

The stimulation treatment may have improved children's skills enough so that families were encouraged to move overseas to take advantage of better education and labor market opportunities. The overall migration rate of the treatment group (0.22) was significantly higher than that of the control group (0.12) ($P = 0.09$), implying that treatment is associated with migration.

We examine the impact of the stimulation intervention on average monthly earnings, which are calcuated as total earnings through the date of the survey divided by the number of months worked to that date. Earnings are expressed in 2005 dollars using the Jamaican consumer price index (CPI) and are then transformed into logarithms. Migrants' earnings are first deflated to 2005 using the CPI of residence and were then converted to Jamaican dollars using purchasing power parity (PPP) adjusted exchange rates. In section B.3 of the supplementary materials we report the results of all analyses separately for earnings from the first job, last job, and current job. See section E of the supplementary materials for more details on the construction of these variables.

One issue is that in the treatment group, there are more individuals who both work and attend school full-time than in the control group. Working, full-time students are likely to have lower earnings than nonstudents with the same education. Hence, observed average earnings likely understate the long-run earnings of the treatment group more than the control group, implying that we underestimate the long-run effects of treatment on earnings. We address this issue by restricting the sample to earnings in full-time jobs (at least 20 days per month), which excludes those who had part-time jobs while primarily attending school. We additionally examine a sample restricted to nontemporary permanent jobs (8 months a year or more) in order to omit students working in summer jobs that may have been full-time. Of the 105 individuals in the sample, 103 had participated in the labor force, 99 had a full-time job, and 75 had a nontemporary full-time job.

Another issue is the selective attrition of the migrants. We were able to locate and interview 14 out of the 23 migrants. Among those 14 migrants, we found a significantly larger share of the treatment migrants than of the control migrants. Overrepresentation of treatment migrants can be a source of bias as migrant workers earn substantially more than those who stay in Jamaica. We address potential bias by imputing earnings for the nine missing migrants. We replace missing values with predicted log earnings from an ordinary least-squares regression on treatment, gender, and migration status. Imputing the missing observations reweights the data so that the treatment and control groups of migrants are no longer under- or overrepresented in the sample. In a sensitivity analysis, we omit migrants and still find strong and statistically significant effects of the program on earnings (see section D.4 of the supplementary materials).

We begin by examining the impact of the intervention on densities of log earnings at age 22. Figure 1A presents Epanechnikov kernel density estimates of the treatment and control groups estimated using bandwidths that minimize mean integrated squared error for Gaussian data. The panels show that for all comparisons, the densities of log earnings for the treatment group are shifted everywhere to the right of the control group densities. The differences are greater when we restrict the sample to full-time workers and even greater when we restrict the sample further to nontemporary workers.

The estimated impacts on log earnings, reported in Table 1, show that the intervention had a large and statistically significant effect on earnings. Average earnings from full-time jobs are 25% higher for the treatment group than for the control group, where the percent difference is estimated by $\exp(\beta) - 1$ and β denotes the treatment effect estimate from Table 1. The impact is substantially larger for full-time permanent (nontemporary) jobs.

The results of the catch-up analysis, presented in Table 2, show that the stunted treatment group caught up with the nonstunted comparison group, whereas the control group remained behind. The differences in log earnings between the nonstunted group and the stunted treatment group are not statistically significant and average around zero. The graphs in Fig. 1B generally show little difference between the earnings densities for the two groups. In contrast, the stunted control group remains behind. The nonstunted comparison group consistently earns significantly more than the stunted control group (Table 2).

Section D of the supplementary materials presents the results of a range of specification tests that corroborate the robustness of the estimates presented in Table 1. Specifically, we first examine treatment effects separately for the pure stimulation intervention and for the combined stimulation/supplemental intervention and test whether we can pool the two arms. Second, we test the hypothesis that there is no effect of nutritional supplementation on log earnings and whether we can pool the supplementation and pure control groups. Third, we examine the extent to which the estimates may be affected by censoring that arises because we only observe the earnings of those employed who are in the labor force. Fourth, we examine the extent to which the imputation of the earnings of missing migrants influences the estimates. Finally, we assess the extent to which the IPW correction for baseline imbalance affected the estimates by reestimating the effects of treatment on earnings without the IPW weights.

Table 2. Catch-up—comparison of average earning at age 22 of the nonstunted and stunted treatment and control samples (statistically significant results in bold). The table presents estimates of the difference in the means of log earnings between, respectively, (I) the weighted nonstunted comparison group and the stunted cognitive stimulation group and (II) the weighted nonstunted comparison group and the stunted control group. Our *P*-values are for one-sided block permutation tests of the null hypothesis of complete catch-up on each outcome (single *P*-value, in parentheses) and accounting for multiple hypotheses (stepdown *P*-values, in brackets). Permutation blocks are based on gender only, but do not control for differences in baseline values, because the aim is to test for catch-up despite the initial disadvantage. The last column uses a combined statistic that summarizes the participant's outcomes. Specifically, we perform a a single-hypothesis inference using the average rank across variables as a test statistic. See section C of the supplementary materials for details.

Job type	All job types	Full-time jobs	Nontemporary jobs	Combined (rank mean)
			(I) Nonstunted—treatment	
Treatment effect	–0.06	–0.08	–0.24	–0.01
Single *P*-value	(0.68)	(0.75)	(0.94)	(0.59)
Stepdown *P*-value	[0.78]	[0.79]	[0.94]	–
Control mean	9.90	9.97	10.11	0.47
Sample size	120	116	97	120
			(II) Nonstunted—control	
Treatment effect	0.21	0.13	0.10	0.07
Single *P*-value	**(0.05)**	(0.15)	(0.24)	**(0.09)**
Stepdown *P*-value	**[0.08]**	[0.18]	[0.24]	–
Control mean	9.63	9.76	9.77	0.44
Sample size	121	119	101	121

This study experimentally evaluates the long-term impact of an early childhood psychosocial stimulation intervention on earnings in a low-income country. Twenty years after the intervention was conducted, we find that the earnings of the stimulation group are 25% higher than those of the control group and caught up to the earnings of a nonstunted comparison group. These findings show that a simple psychosocial stimulation intervention in early childhood for disadvantaged children can have a substantial effect on labor market outcomes and can compensate for developmental delays. The estimated impacts are substantially larger than the impacts reported for the U.S.-based interventions, suggesting that ECD interventions may be an especially effective strategy for improving long-term outcomes of disadvantaged children in developing countries.

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SUPPLEMENTARY MATERIALS

www.sciencemag.org/content/344/6187/998/suppl/DC1
Materials and Methods
Figs. S1 and S2
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SOLAR CELLS

Coherent ultrafast charge transfer in an organic photovoltaic blend

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Blends of conjugated polymers and fullerene derivatives are prototype systems for organic photovoltaic devices. The primary charge-generation mechanism involves a light-induced ultrafast electron transfer from the light-absorbing and electron-donating polymer to the fullerene electron acceptor. Here, we elucidate the initial quantum dynamics of this process. Experimentally, we observed coherent vibrational motion of the fullerene moiety after impulsive optical excitation of the polymer donor. Comparison with first-principle theoretical simulations evidences coherent electron transfer between donor and acceptor and oscillations of the transferred charge with a 25-femtosecond period matching that of the observed vibrational modes. Our results show that coherent vibronic coupling between electronic and nuclear degrees of freedom is of key importance in triggering charge delocalization and transfer in a noncovalently bound reference system.

The currently accepted model for the basic working principle of a bulk-heterojunction organic solar cell (*1*, *2*), comprising a conjugated polymer donor and an electron acceptor material, relies on four elementary steps: (i) photon absorption, creating a spatially localized, Coulomb-bound electron-hole pair (exciton) in the donor phase; (ii) exciton diffusion to the donor/acceptor interface; (iii) exciton dissociation at the interface leading to the formation of a charge-separated state (*3*, *4*), often called charge-transfer exciton or polaron pair; and (iv) dissociation of the polaron pair into free charges and their transport to the electrodes.

In this work, we focused on the dynamics of the primary light-induced steps, (i) and (iii), which lead to a charge-separated state in organic photovoltaic (OPV) materials and represent the key process in OPV cells. Over the past years, charge photogeneration has been investigated in several technologically relevant materials, such as blends of polyphenylene-vinylene (*5*, *6*), polythiophene (*7*, *8*), or low band gap polymers (*9*, *10*) with fullerene derivatives. In all of these systems, it is now accepted that charge separation is an ultrafast process occurring on a sub-100-fs time scale. So far the experimental studies on charge photogeneration in OPV materials have mainly been described within the framework of an incoherent transfer model (*11*, *12*), giving a rate constant for the transfer process. These rate constants may be enhanced by hot exciton dissociation