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Labor Market Returns to an Early Childhood Stimulation Intervention in Jamaica

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Abstract

A substantial literature shows that U.S. early childhood interventions have significant long-term economic benefits. 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 re-interviewed 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 non-stunted comparison group identified at baseline (65 out of 84 participants).

Keywords

early childhood development; stunting; randomized trial

1 Introduction

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

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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 skill. Programs that boost skills are likely to have greater benefits in developing countries because skills are less abundant there. For example, the returns to schooling are typically higher in developing countries (31).

This paper reports estimates of the causal effects on earnings of an intervention that gave two 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 a non-stunted 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.

2 The Jamaican Study

The Jamaican Study enrolled 129 growth stunted children age 9–24 months that lived in Kingston, Jamaica, in 1986–1987 (35). Section A of the Supplementary Online Materials (SOM) 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: (1) psychosocial stimulation (N=32), (2) nutritional supplementation (N=32), (3) both psychosocial stimulation and nutritional supplementation (N=32), and (4) a control group that received neither intervention (N=33). The Jamaican study also surveyed a comparison group of 84 non-stunted children who lived nearby. All subjects were given access to free health care.

The stimulation intervention (groups 1 and 3) consisted of two years of weekly one-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, schooling attainment and reduced participation in violent crimes (36).

The nutritional intervention (groups 2 and 3) consisted of giving one kilogram 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 SOM 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.

3 Methods

We resurveyed both the stunted and non-stunted samples in 2007–08, some 20 years after the original intervention when the participants were approximately 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 (SOM 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 non-attrition groups (SOM 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. In this paper, 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 non-stunted 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 SOM section B for the analysis of baseline balance, attrition, and the details of implementing IPW.

In order to better understand the external validity of our catch-up analysis, we compare the non-stunted group to the general population using data on individuals 21–23 years old living in the greater Kingston area from the 2008 Jamaican Labor Force Survey (JLF) survey. By age 22, the non-stunted group attained comparable levels of skills as those of persons the same age who were living in the Kingston Area interviewed in the JLF (SOM 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 non-parametric combining statistic. Section C of the SOM gives details.

4 Parental Investment and Migration

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 (SOM Table S.1). At the end of the 2-year intervention the HOME inventory of the stunted treatment group was 16%, greater than the 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 afterwards. 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 7 or later at age 11.

While 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 would have thought. In fact, significant differences in schooling attainment appear at age 17 (36). By age 22, the treatment group had 0.6 (p=.08) more years of schooling attainment than the control group. The proportion of the treatment group still enrolled in full time school (0.22) was more than 5 times larger than in the control group (0.04) (p <=.01).

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-value = .09) implying that treatment is associated with migration.

5 Earnings

5.1 Measurement

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 CPI and are then transformed into logs. Migrants' earnings are first deflated to 2005 using the CPI of residence and were then converted to Jamaican dollars using PPP adjusted exchange rates. In Section B.3 of SOM we report the results of all analyses separately for earnings from the first job, last job and current job. See Section E of SOM 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 non-students 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 non-temporary 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 non-temporary 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. Over-representation 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 9 missing migrants. We replace missing values with predicted log earnings from an OLS regression on treatment, gender and migration status. Imputing the missing observations re-weights the data so that the treatment and control groups of migrants are no longer under- or over-represented in the sample. In a sensitivity analysis, we delete migrants and still find strong and statistically significant effects of the program on earnings (see SOM section D.4).

5.2 Results

We begin by examining the impact of the intervention on densities of log earnings at age 22. Panel A of Figure 1 presents Epanechnikov kernel density estimates of the treatment and control groups estimated using bandwidths that minimize mean integrated squared error for Gaussian data. The figures 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 non-temporary 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 (non-temporary) jobs.

The results of the catch-up analysis, presented in Table 2, show that the stunted treatment group caught up with the non-stunted comparison group, while the control group remained behind. The differences in log-earnings between the non-stunted group and the stunted treatment group are never statistically significant and average around zero. The graphs in Panel B in Figure 1 generally show little difference between the earnings densities for the two groups. In contrast, the stunted control group remains behind. The non-stunted comparison group consistently earns significantly more than the stunted control group 2.

SOM Section D 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 re-estimating the effects of treatment on earnings without the IPW weights.

6 Conclusions

This is the first study to experimentally evaluate 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 non-stunted 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 US–based interventions, suggesting that ECD interventions may be an especially effective strategy for improving long-term outcomes of disadvantaged children in developing countries.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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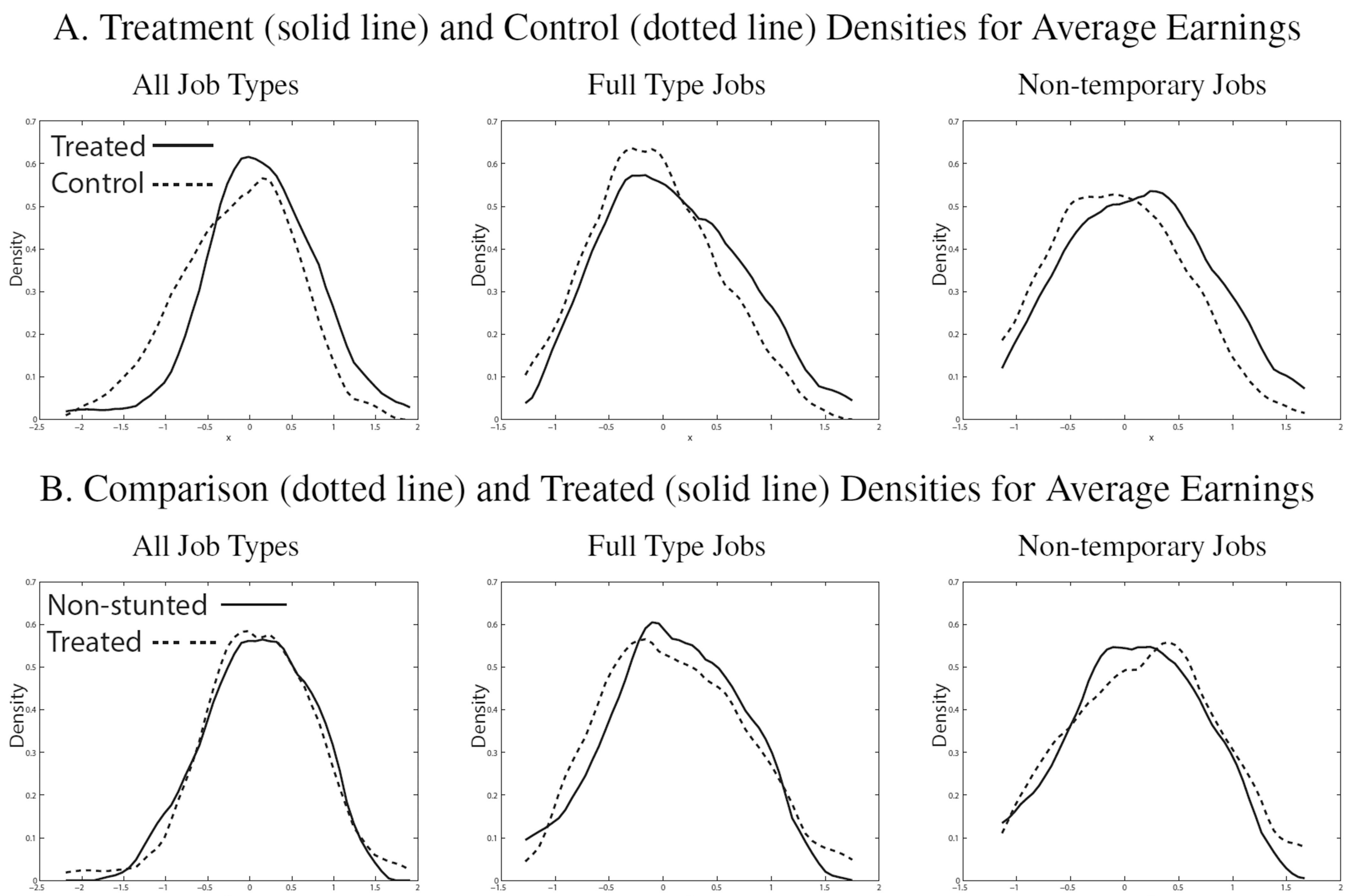

Figure 1.

Impact of Stimulation Treatment and Catch-up on the Densities of Average Earnings at Age 22

Notes: Panel A presents the log earnings densities for the treatment and control groups using data where earnings of migrant workers who were lost to follow-up were imputed. The control density is the dotted line and the treatment density the solid one. Panel B presents the log earnings densities for the non-stunted comparison and stunted treatment groups, where where earnings of migrant workers who were lost to follow-up were imputed. The treatment group density is the dotted line and the non-stunted group density the solid one. 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 used was used for the corresponding control group.

Table 1

Treatment Effect on Average Log Earnings at Age 22
(Statistically Significant Results in Bold)

Job Type	All Job Types	Full Time Job	Non-Temporary Job	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

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). Estimates are not reported for the current job for non-temporary workers because the non-missing sample size is less than 40% of the total sample. 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 parenthesis) 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 a single-hypothesis inference using the average rank across variables as a test statistic. See SOM section for details.

Table 2

Catch Up - Comparison of Average Earning at Age 22 of the non-stunted and stunted treatment and control samples
(Statistically Significant Results in Bold)

Panel (I) Non-stunted - treatment				
Job Type	**All Job Types**	**Full Time Job**	**Non-Temporary Job**	**Combined (Rank Mean)**
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
Panel (II) Non-stunted - control				
Job Type	All Job Types	Full Time Job	Non-Temporary Job	Combined (Rank Mean)
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

The table presents estimates of the difference in the means of log earnings between respectively (I) the weighted non-stunted comparison group and the stunted cognitive stimulation group and (II) the weighted non-stunted 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 SOM section for details.