Table 1: How much of the differences in later outcomes (test scores, educational attainment, earnings) can be explained by early childhood factors?

Study and Data	Inputs/Intermediate Variables	Outcomes	Results
STUDIES USING NATIONAL LONGITUDINAL SURVEY OF YOUTH-CHILD SAMPLE DATA			
Childhood Emotional and Behavioral Problems and Educational Attainment (McLeod and Kaiser (2004)) NLSY-Child Sample data on children 6-8 in 1986. Five waves through 2000 (when children 20-22) n=424.	Emotional and behavioral problems at age 6-8 measured by BPI, mother emotional problems and delinquency, poverty status, mother's AFQT score, mother's education, mother's marital status and age, child's age, sex, and race, dummy for LBW	Dummy for graduating high school by 2000, dummy for enrolling in college by 2000	**R-squared for predicting HS graduation:** Only child emotional and behavioral problems: 0.046 Add child and mother demographics: 0.111 Add mother's emotional problems and delinquency: 0.124 **R-squared for predicting college enrollment:** Only child emotional and behavioral problems: 0.017 add child and mother demographics: 0.093 Add mother's emotional problems and delinquency: 0.112.
Formulating, Identifying, and Estimating the Technology of Cognitive and Noncognitive Skill Formation (Cunha and Heckman (2008)) NLSY-Child Sample data on white males. n=1,053.	Home environment measured by HOME score. Measures of parental investments: number of child's books, whether the child has a musical instrument, whether the family receives a daily newspaper, whether the child receives special lessons, how often the child goes to museums, and how often the child goes to the theater. Measures of child cognitive skills: PIAT test scores at various ages. Measures of child's non-cognitive skills: BPI at various ages.	Log earnings and likelihood of graduation from high school. Estimated a dynamic factor model that exploites cross-equation restrictions for identification. Estimated effects of cognitive and non-cognitive skills as well as parental investments at different stages of the child's lifecycle on log earnings and likelihood of graduation from high school.	A 10% increase in parental investments at ages 6-7 increases earnings by 24.9% (12.5% through cognitive skills, 12.4% through non-cognitive skills), and increases likelihood of graduating high school by 64.4% (54.8% through cognitive skills, 9.6% through non-cognitive skills).

Estimating the Technology of Cognitive and Non-Cognitive Skill Formation (Cunha, Heckman, and Schennach (2010)). NLSY-Child sample first-born white children. N=2,207.	Examples of measures of child cognitive skills: Motor-Social Development at birth; PIAT Reading-Comprehension at ages 5-6; PIAT Math at ages 13-14. Examples of measures of child non-cognitive skills: BPI at various ages; "friendliness at birth". Examples of measures of parental investments: "How often child goes on outings during year of birth"; "How often mom reads to child during year of birth"; "Child has a CD player, ages 3-4"; How often child is praised, ages 13-14".	Main outcome variable: completed years of education by age 19. Multi-stage production functions estimated in which the productivity of later investments depends on early investments and on the stock of cognitive and non-cognitive skills and investments are endogenous. Identification based on nonlinear factor models with endogenous inputs. In preferred specification, used maximum-likelihood methods to estimate the production technology.	34% of variation in educational attainment is explained by measures of cognitive and non-cognitive capabilities. 16% is due to adolescent cognitive capabilities; 12% is due to adolescent non-cognitive capabilities. Parental endowments/investments account for 15% of variation in educational attainment.
STUDIES USING NATIONAL CHILD DEVELOPMENT STUDY (1958 BRITISH BIRTH COHORT) DATA			
Ability, family, education, and earnings in Britain (Dearden (1998)) Focus on individuals who participated in waves 4 and 5 of the survey in 1981 and 1991, who were employees in 1991. n = 2597 males, 2362 females	Math and verbal ability (age 7), type of school, family characteristics -- teacher's assessment of interest shown by parents in child's education at 7; type of school attended at 16 family's financial status at 11 and 16; region dummies; father's SES; parents' education levels	Years of full-time education; Earnings at age 33	**R-squared for education as outcome:** Including all explanatory variables: 0.33-0.34 **R-squared for earnings as outcome:** Baseline earnings equation including only years education: 0.15 (males), 0.25 (females). Add reading and math scores at age 7, school type and regional dummies: 0.26 (males), 0.31 (females). Including all explanatory variables: .029 (males), 0.41 for females.
Early test scores, socioeconomic status, and future outcomes (Currie and Thomas (1999)) Full sample size (based on responses at 7): n = 14,022	Reading and math test scores at age 7, mother and father's SES and education, birth weight, other child background variables at age 7	Number of O-level passes of exams by age 16; employed at age 23, 33; log wage at age 23, 33	**R-squared for predicting age 16 exam passes:** Reading and math scores only: 0.21-0.22 Add other background variables: 0.31-0.32. **R-squared for predicting employment at 33:** Reading and math test scores only: 0.01 Add other background variables: 0.04-0.05. **R-squared for predicting log wage at 33:** Reading and math test scores only: 0.08-0.09 Add other background variables: 0.18-0.20.
The lasting impact of childhood health and circumstance (Case, Fertig, and Paxson (2005)) n = 14,325 (7016 men, 7039 women)	Mother's and father's education and SES, LBW, indicators for moderate, heavy, and varied maternal smoking during pregnancy, number of chronic conditions at age 7 and 16.	Number of O-level passes of exams by age 16; adult health status at age 42; part-time or full employment at age 42.	**R-squared for predicting age 16 exam passes/adult health/employment at 42.** Mother's education and SES: 0.062/0.082/0.076 Father's education and SES: 0.241/0.189/0.173 LBW and maternal smoking: 0.024/0.086/0.052

Explaining intergenerational income persistence: non-cognitive skills, ability, and education (Blanden, Gregg, and Macmillan (2006)) **A.** NCDS: 1958 cohort Focus on 2163 males. **B.** British Cohort Study: 1970 cohort. Focus on 3340 males.	**A.** Family income at age 16, reading and math test scores at age 11, scores for "behavioral syndromes" at age 11, O-level exam scores at age 16 **B.** Years of preschool education, birth weight, height at 5 and 10, emotional/behavioral scores at ages 5, 10, & 16, family income at ages 10 and 16, reading and math test scores at age 10, IQ at age 10, dummy for HS degree, exam scores at age 16.	**A.** Earnings at age 33 **B.** Earnings at age 30	**R-squared Model A.** Birth weight, childhood health, and age 11 test scores only: 0.116 Including "behavioral syndromes" at age 11: 0.151 All variables: 0.263 **R-squared Model B.** Birth weight, childhood health, and age 10 test scores only: 0. 075 Including emotional/behavioral characteristics at age 10: 0.087 All variables: 0.222

Table 2: Estimated Effect of Birth Weight on Parental Investments Within Twin Pairs, Estimates from the Early Childhood Longitudinal Study.

9 month survey			
Outcome:	**All Twins**	**Same Sex Twins**	**Identical Twins**
1 if child was ever breastfed	0.0183	0.0187	0.0031
	[0.0238]	[0.0277]	[0.0355]
	1550	1000	350
1 if child is now being breastfed	0.0038	-0.0039	-0.0007
	[0.0126]	[0.0152]	[0.001]
	1550	1000	350
How long child was breastfed in months	-0.0753	-0.2165	-0.343
given breastfed	[0.1752]	[0.204]	[0.3182]
	800	500	150
Age solid food was introduced in months,	-0.1802	-0.2478	-0.6660*
given introduced	[0.1523]	[0.1906]	[0.2914]
	1550	1000	350
Number of well-baby visits	0.283	0.3803	0.5797
	[0.1883]	[0.2414]	[0.5253]
	1550	1000	350
Number of well-baby visits	0.1956	0.2329	0.2668
only children in excellent of very good health	[0.1624]	[0.1944]	[0.3799]
	1500	950	300
1 if caregiver praises child	-0.0015	-0.051	0.096
	[0.0941]	[0.1189]	[0.2089]
	1250	800	250
1 if caregiver avoids negative comments	-0.0051	-0.0077	0
	[0.0055]	[0.0084]	[.]
	1250	800	250
1 if somewhat difficult or difficult to raise	-0.0181	-0.0772	-0.0946
(caregiver report)	[0.0583]	[0.0712]	[0.1395]
	1550	1000	350
1 if not at all difficult or not very difficult to raise	0.1065	0.153	0.2237
(caregiver report)	[0.0707]	[0.0812]	[0.1195]
	1550	1000	350
2-year survey			
1 if Caress/kiss/hug child	0.0228	0.0055	0.0021
	[0.0266]	[0.0254]	[0.0049]
	1350	850	300
1 if Spank/slap child	-0.0195	-0.0095	-0.0048
	[0.0249]	[0.0192]	[0.0316]
	1350	850	300
1 if tme spent calming child >1 hr usually	0.0317	-0.024	0.0719
	[0.0646]	[0.0759]	[0.093]
	1450	950	300
1 if somewhat difficult or difficult to raise	-0.0432	-0.0901	-0.1412
(caregiver report)	[0.0555]	[0.0621]	[0.086]
	1450	950	300

1 if not at all difficult or not very difficult to raise (caregiver report)	-0.0031 [0.0757] 1450	0.068 [0.0869] 950	0.0527 [0.1258] 300
Age when stopped feeding formula in months	-0.1903 [0.255] 1150	-0.4504 [0.3204] 750	-0.5903 [0.7844] 250
Age when stopped breastfeeding in months	-0.1492 [0.5981] 100	-0.0267 [0.044] 50	-0.0422 [0.069] 50
Preschool Survey			
1 if parent expects child to enter kindergarten early	-0.0082 [0.012] 1300	-0.0071 [0.0102] 800	0 [0] 250
1 if parents concerned about child's kindergarten readiness	-0.1435** [0.0554] 1300	-0.1299* [0.0636] 850	-0.1099 [0.1253] 250
1 ifexpect child to get >= 4 yrs of college	-0.0073 [0.0272] 1350	0.0069 [0.0327] 850	0.0228 [0.0264] 300
Number of servings of milk in the past 7 days	-0.0598 [0.2074] 1350	-0.0577 [0.2278] 850	0.0819 [0.2489] 300
Number of servings of vegetables past 7 days	0.0632 [0.2634] 1350	0.2131 [0.3027] 850	0.0871 [0.4091] 300

Notes: Standard errors clustered on the mother are shown in in brackets with sample sizes below. Twin pairs in which a child had a congenital anomaly are omitted. Birth weight measured in kilograms. Each entry is from a separate regression of the dependent variable on birth weight and a mother fixed effect. Models in column 1 also control for child gender. Sample sizes are rounded to the nearest multiple of 50.

Table 3: Sample Power Calculations

Given a true population effect size, what is the power of a size alpha = .05 test against the null hypothesis that there is no effect for different sample sizes?

Basis Study	Assumptions	Sample Size	Power
Black, Devereux, and	True model:	100	0.097
Salvanes (2007)	Prob(HSGRAD) = 0.7 + 0.1*ln(birthweight) + error	300	0.167
		500	0.263
Key result: a 1% increase	Calculation of error variance and SD:	600	0.298
in birth weight increases	Let y = Prob(HSGRAD), x = ln(birthweight), e = error	700	0.351
the probabililty of high	Var(y) = 0.44^2 = 0.19	800	0.376
school completion by	Var(x) = 0.26^2 = 0.07 (where SD(x)=0.26, according	900	0.409
0.09 percentage points.	to the distribution of ln(birthweight))	1000	0.446
	If y = 0.7 + 0.1x + e, and x and e are independent,	1250	0.531
Birth weight sample	Var(e) = Var(y) - (0.1^2)*Var(x)	1500	0.617
summary stats (twins):	= 0.19-(0.1^2)*(0.07) = 0.19	1620	0.660
mean = 2598g, SD = 612g		2000	0.744
	So, SD(e) = sqrt(Var(e)) = 0.44	2200	0.750
Probability of HS grad		2500	0.825
sample summary stats:	Therefore, assume: birthweight ~ N(2598, 612), and	3000	0.892
mean = 0.73, SD = 0.44	take the natural log of birthweight.	3500	0.928
	error ~ N(0, 0.44)	4000	0.962
		4500	0.975
		5000	0.982
		5500	0.993
		6000	0.994
		6500	0.996
		7000	0.999

Given a sample size, how large would the true effect size have to be in order to be able to detect it with reliable power using a test of size alpha = .05?

Basis Study	Assumptions	True B1	Power
Conley, Pfeiffer and	Model: y = B0 + B1*x + error	0.005	0.046
Velez (2006)		0.01	0.047
	Assume: z ~ N(2598, 612), x = ln(z)	0.02	0.077
Sibling sample from	error ~ N(0, 0.44)	0.03	0.092
PSID (n=1,360)	sample size = 1500	0.04	0.146
		0.05	0.198
		0.06	0.274
		0.07	0.354
		0.08	0.461
		0.09	0.525
		0.1	0.631
		0.12	0.769
		0.15	0.926
		0.17	0.975
		0.2	0.99

Notes: Power calculations are based on Monte Carlo simulations with 1000 replications.

Table 4: Prenatal Effects on Later Child and Adult Outcomes

Study and Data	Study Design	Results
EFFECTS OF MATERNAL HEALTH		
Is the Impact of Health Shocks Cushioned by Economic Status? The Case of Low Birth Weight. (Currie and Hyson (1999)) NCDS 1958 cohort. N=11,609 at age 20, 10,267 at age 23, 9,402 at age 33.	Multivariate regression with numerous background and demographic controls (including maternal grandfather's SES, birth order, and maternal smoking during pregnancy). Key explanatory variables are indicators for LBW, SES, and the interactions. Outcomes measured are the number of O-level passes at age 16 (transcripts collected at age 20), employment, wages and health status at ages 23 and 33. SES assigned using father's social class in 1958 (or mother's SES if father is missing).	LBW children are 38-44% less likely to pass Math O-level. LBW females are 25% less likely to pass English O-level tests. LBW females are 16% less likely to be employed full-time at age 23, LBW males are 9% less likely to be employed full-time at age 33. LBW females are 54% more likely to have fair/poor health at age 23, LBW males are 43% more likely to have fair/poor health at age 33. Few significant differences by SES.
Returns to Birthweight (Behrman and Rosenzweig (2004)). Monozygotic female twins born 1936-1955 from the Minnesota Twins Registry for 1994 and the birth weights of their children. N=804 twins, and 608 twin-mother pairs.	Twin fixed effects estimates compared to OLS. To explore generalizability of findings from twins sample, weighted the sample using the U.S. singleton distribution of fetal growth rates.	**Results from twins sample**: 1 oz. per week of pregnancy increase in fetal growth leads to increases of 5%, in schooling attainment; 2% in height; 8% hourly wages. **Results from twins sample weighted using singleton distribution:** 1 oz. per week of pregnancy increase in fetal growth leads to increase of 5% in schooling attainment; 1.7% in height; no significant effect on wages. OLS underestimates effects of birth weight by 50%.
The Costs of Low Birth Weight (Almond, Chay, and Lee (2005)). Linked birth and infant death files for U.S. for 1983-85, 1989-91 and 1995-97. Hospital costs from Healthcare Cost and Utilization Project State Inpatient Database for 1995-2000 in New York and New Jersey. NCHS N=189,036 twins, 497,139 singletons. HCUP N=44,410.	Twin fixed effects to estimate effect of low birth weight (LBW) on hospital costs, health at birth, and infant mortality. Also estimated impact of maternal smoking during pregnancy on health among singleton births, controlling for numerous background and demographic characteristics using OLS and propensity score matching.	**Results using twin fixed effects:** 1 SD increase in birth weight leads to: 0.08 SD decrease in hospital costs for delivery and initial care 0.03 SD decrease in infant mortality rates 0.03 SD increase in Apgar scores 0.01 SD decrease in use of assisted ventilator after birth (OLS estimates w/out twin fixed effects are 0.51 SD, 0.41 SD, 0.51 SD, 0.25 SD). **Results from OLS on effects of maternal smoking:** Maternal smoking reduces birth weight by 200g (6%); increases likelihood that infant is LBW (<2500g) by more than 100% (mean=.061) No statistically significant effects on Apgar score, infant mortality rates, or use of assisted ventilator at birth.
The 1918 influenza pandemic and subsequent health outcomes (Almond and Mazumder (2005)). Data from SIPP for 1984-1996. N=25,169.	Compare cohorts in utero before, during and after Oct. 1918 flu pandemic. Regressions estimate cohort effects including survey year dummies and quadratic in age interacted with survey year.	Individuals born in 1919 are 10% more likely to be in fair or poor health, also increases of 19%, 35%, 13%, 17% in trouble hearing, speaking, lifting and walking.

Estimating the Impact of Large Cigarette Tax Hikes: The Case of Maternal Smoking and Infant Birth Weight (Lien and Evans (2005)). 1990-1997 U.S. Natality files. Data on cigarette taxes from the Tax Burden on Tobacco, various years.	IV using tax hikes in four states as instruments for maternal smoking during pregnancy. Controlled for state and month of conception and background characteristics.	Maternal smoking during pregnancy reduces birth weight by 5.4% and increases likelihood of low birth weight by ~100%.
Long term effects of in utero exposure to the 1918 influenza pandemic in the post-1940 US population (Almond (2006)) . 1960-1980 U.S. Census data. 1917-1919 Vital Statistics data on mortality. For 1960 n=114,031, for 1970 n=308,785, for 1980 n=471,803.	Estimated effects of in utero influenza exposure by comparing cohorts born immeditely before, during, and after the 1918 pandemic and by employing the idiosyncratic geographic variation in intensity of exposure to conduct within- cohort analysis. Exposed cohort = those born in 1919. Surrounding cohorts = those born in 1918 and 1920. Used multivarate regression with dummy for birth cohort = 1919, and a quadratic cohort trend to measure departures in the 1919 birth cohort outcomes from the trend. For geographic comparison, used data on virus strength by week as well as data on epidemic timing by census division to yield a measure of average pandemic virulence by division. Then estimated multivariate regression including the virulence measure, state and year of birth fixed effects, the infant mortality rate in state and year of birth, and the attrition of birth cohort in the census data.	**Estimation results comparing birth cohorts:** The 1919 birth cohort, compared with the cohort trend: was 4-5% (13-15% among treated) less likely to complete high school received 0.6-1.6% fewer years of education had 1-3% less total income (for males only, 2005 dollars) had 1-2% lower socioeconomic status index (Duncan index) was 1-2% more likely to have a disability that limits work (for males only) had 12% higher average welfare payment (for women) Estimates are slightly larger for nonwhite subgroup. **Estimation results using geographic variation and state fixed effects:** (For the 1919 birth cohort, used average maternal infection rate = 1/3) Maternal infection: reduces schooling by 2.2% reduces probability of high school graduation by 0.05% decreases annual income by 6% reduces socioeconomic status index by 2-3%
Explaining sibling differences in achievement and behavioral outcomes: the importance of within- and between-family factors (Conley, Pfeiffer, and Velez (2007)). Data from Child Development Supplement of PSID. N=1,360.	Sibling fixed effects to examine effects of birth weight, birth order, and gender on later outcomes. Cognitive outcomes measured by the Woodcock- Johnson Revised Tests of Achievement. Behavioral outcomes measured by the Behavioral Problems Index (BPI). Control for family- and child-specific characteristics.	No statistically significant effects of birth weight on BPI or on cognitive assessments for whole sample. For blacks, positive effect of birth weight on cognitive assessments.

Twin differences in birth weight: the effects of genotype and prenatal environment on neonatal and postnatal mortality (Conley, Strully, and Bennett (2006)). Data on twin births from the 1995-97 Matched Multiple Birth Database. N=258,823.	Twin fixed effects models of effects of birth weight on mortality for same-sex and mixed-sex pairs.	1 lb increase in birth weight leads to: 9% (10%) reduction in infant mortality for mixed-sex (same-sex) 7% (8%) reduction in neonatal mortality for mixed-sex (same-sex) 2% reduction in post-neonatal mortality for mixed-sex and same-sex For full-term twins, mixed-sex effects much larger than same-sex effects.
Biology as destiny? Short- and long-run determinants of intergenerational transmission of birth weight (Currie and Moretti (2007)). California natality data for children born between 1989 and 2001 and their mothers (if born in CA) born between 1970 and 1974. Mothers who are sisters are matched using grandmother's name. n=638,497 births.	Examine effect of mother's birth weight on child's birth weight in models with grandmother fixed effects. Examine interactions of mother's birth weight and grandmother's SES at time of mother's birth as proxied by income in zip code of mother's birth. Examine effect of maternal low birth weight on mother's SES at time she gives birth.	Mother's low birth weight increases likelihood that child is low birth weight by about 50%. The incidence of child low birth weight is 7% higher if mother was born into high poverty zip code than into low poverty zip code. Children born into poor households are 0.7% more likely to be low birth weight if their mothers were low birth weight; children born into nonpoor households are .0.4% more likely to be low birth weight if their mothers were low birth weight (so, poverty raises the probability of transmission of low birth weight by 88%). Being low birth weight is associated with a loss of $110 in future income, on average, on a baseline income of $10,096 (in 1970 dollars). Being low birth weight increases the probability of living in a high-poverty neighborhood by 3% relative to the baseline. Being low birth weight reduces future educational attainment by 0.1 years.
From the cradle to the labor market: the effect of birth weight on adult outcomes (Black, Devereux, and Salvanes (2007)). Birth records from Norway for 1967-1997. Dropped congenital defects. Matched to registry data on education and labor market outcomes and to military records. n=33,366 twin pairs.	Twin fixed effects, controlling for mother and birth-specific variables. Log(birth weight) is primary independent variable.	10% increase in birth weight: reduces 1-year mortality by 13% increases 5 min APGAR score by 0.3% increases probability of high school completion by 1.2% increases full-time earnings by 1% **Male outcomes at age 18-20:** 10% increase in birth weight: increases height by 0.3% increases BMI by 0.5% increases IQ by 1.1% (scale of 1-9) **Effects of mother's birth weight on child's birth weight:** 10% increase in mother's birth weight: increases child's birth weight by 1.5%

The influence of early-life events events on human capital, health status and labor market outcomes over the life course (Johnson and Schoeni (2007)) . PSID. Adult sample born between 1951 and 1975. N=5,160 people and 1,655 families. Child sample 0-12 years old in 1997. N=1,127 mothers and 2,239 children.	Sibling fixed effects to estimate impact of maternal smoking during pregnancy on birth outcomes. Also estimated model using OLS, controlling for grandparent smoking in adolescence, maternal birth weight, and paternal smoking, among other background characteristics.	Note: means are not reported, so relative effects cannot be calculated. **Effects of income and mother's birth weight on child's birth weight:** An increase in income of $10,000 raises child's birth weight by 0.12 lbs if mother was low birth weight (by 0.02 lbs if mother was not low birth weight) for a family with income of $7,500 (1997 dollars). Having no health insurance increases the probability of low birth weight by 10 pp. **Effects on child health outcomes:** (health index 1-100) Low birth weight siblings have a 1.67 point lower health index Private health insurance increases health index by 1.02 points A $10,000 increase in income for families with $15,000-50,000 income increases health index by 0.53 pp. **Effects on adult outcomes:** Low birth weight siblings: 4.7 pp more likely to be high school dropouts, have a 3.7 point lower health index (1-100 scale) Low birth weight brothers (sample of males only): are 4.3 pp more likely to have no positive earnings (sig at 10% level) have $2,966 less annual earnings (sig at 10% level).
Maternal Smoking During Pregnancy and Early Child Outcomes (Tominey (2007)). Children of NCDS mothers born between 1973 and 2000. n=2,799 mothers and 6,291 sibling children.	Sibling fixed effects to estimate impact of maternal smoking during pregnancy on birth outcomes. Also estimated model using OLS, controlling for grandparent smoking in adolescence, maternal birth weight, and paternal smoking, among other background characteristics.	Note: only reporting results from sibling fixed effects regression here. Maternal smoking during pregnancy reduces birth weight by 1.7%. No statistically significant effect of maternal smoking during pregnancy on probability of having a low birth weight child, pre-term gestation, or weeks of gestation. No statistically significant effects of maternal smoking among mothers who quit by month 5 of pregnancy. Larger effects of maternal smoking on birth weight among low educated women.
Can a pint per day affect your child's pay? The effect of prenatal alcohol exposure on adult outcomes (Nilsson (2008)). Data from Swedish LOUISE database on first-born individuals born between 1964 and 1972. n=353,742.	Swedish natural experiment in which alcohol availability in 2 treatment regions increased sharply as regular grocery stores were allowed to market strong beer for 6 months during 1967 with the minimum age for purchase being 16 (instead of 21). Difference-in-difference-in-difference comparing under-21 mothers with older mothers in treatment and control regions pre-, during, and post- experiment. Baseline estimations focused on children conceived prior to the experiment, but exposed in utero to the experiment. Controlled for quarter and county of birth fixed effects.	**DDD results:** Years of schooling: decreased by 0.27 (2.1%) years for whole sample; by 0.47 years for males; no statistically significant effect for females HS graduation: decreased by 0.4% for whole sample; by 10% for males; no stat. significant effect for females Graduation from higher education: decreased by 16% for whole sample; by 35% for males; no stat. significant effect for females Earnings at age 32: decreased by 24.1% for whole sample; by 22.8% for males; by 17.7% for females Probability no income at age 32: increased by 74% whole sample Proportion on welfare: increased by 90% for whole sample; by 5.1pp for males

Short-, medium- and long-term consequences of poor infant health (Oreopoulos et al. (2008)). Data from the Manitoba Center for Health Policy matching provincial health insurance claims with birth records, educational records, and social assistance records. Includes all children born in Manitoba from 1979 to 1985. n=54,123 siblings and 1,742 twins.	Sibling and twin fixed effects. Used three measures of infant health : birth weight, 5-min Apgar score, and gestational length in weeks; used dummies for different categories of the variables to estimate nonlinear effects. Also estimated models using OLS for the whole sample, for the siblings sample, and for the twins sample without family fixed effects.	Note: only reporting results from regressions that included family fixed effects here. BW = birth weight. Relative to Apgar score=10, BW>3500g, gestation 40-41 weeks lower values **Effects on infant mortality:** increase infant morality. E.g. in Sibling Sample (infant mortality = 0.011) Apgar score <6 increases probability of infant mortality by 31.9 pp BW<1000g increases probability of infant mortality by 87.2 pp gestation<36 weeks increases probability of infant mortality by 11.9 pp **Effect on Language Arts score (taken in grade 12):** Apgar score <6 decreases test score by 0.1 of SD (sig. at 10% level) BW 2501-3000g decreases test score by 0.04 of SD **Effect on probability of reaching grade 12 by age 17:** Apgar score <6 decreases grade 12 probability by 4.1 pp (sig. at 10%) BW 1001-1500g decreases grade 12 probability by 14.1 pp gestation<36 weeks decreases grade 12 probability by 4.0 pp **Effect on social assistance take-up during ages 18-21.25:** BW<1000 decreases probability of take-up by 21.5 pp.
Birth Cohort and the Black-White Achievement Gap: The Role of Health Soon After Birth (Chay, Guruyan and Mazumder (2009)). Data from the National Assessment of Educational Progress Long-term Trends for 1971-2004. AFQT data from U.S. military for 1976-2001 for male applicants 17-20. n=2,649,573 white males and n=1,103,748 black males. Hospital discharge rates from National Health Interview Survey.	Regression of test scores on year, age, and subject fixed effects (separately for blacks and whites) using NAEP-LTT and AFQT test score data. In AFQT test score data, correct for selection bias using inverse probability weighting from Natality and Census data. Estimated regressions separately for blacks and whites and for the North, the South, the Rustbelt, the deep South, and individual states within the South and North. Also estimated difference-in-difference-in-difference (DDD) models, comparing black and white test scores between cohorts born in 1960-62 and 1970-72 in the South relative to the Rustbelt, controlling for region-specific, race-specific age-by-time effects and race-by-region-by-time effects. Used post neonatal mortality rate (PNMR) as a proxy for infant health environment to assess impact of infant health on the test score gap.	**NAEP-LTT data:** The black-white test score gap declined from 1 SD to 0.6 of SD between 1971 and 2004. The convergence was primarily due to large increases in black test scores in the 1980s. Regression results indicate that the convergence was due to cohort effects, rather than time effects. **AFQT data:** The black-white gap in AFQT test scores declined by about 19% between the 1962-63 and 1972 birth cohorts. The decline in the gap is about 0.3SDs greater in the South relative to the Rustbelt between 1960-62 and 1970-72 cohorts. Convergence in PNMR explains 52% of the variation across states in AFQT convergence. **Effects of access to hospitals:** A 30pp increase in black hospital birth rates from 1962-64 to 1968-70 increases cohort AFQT scores by 7.5 percentile points. A black child who gained admission to a hospital before age 4 had a 0.7-1 SD gain in AFQT score at age 17-18 relative to a black child who did not.

Separated at girth: US twin estimates of the effects of birth weight (Royer (2009)). California birth records. n=3,028 same-sex female twin births 1960-1982. Data from Early Childhood Longitudinal Study-Birth Cohort. n=1,496 twin births.	Twin fixed effects, allowing non-linear effects of birth weight.	**Twin fixed-effect results:** 250g increase in birth weight leads to: 8.3% decrease in probability of infant mortality 0.82 day decrease in stay in hospital post-birth 0.02-0.04 of SD increase in mental/motor test score 0.2% increase in educ. attainment of mother at childbirth 0.5% increase in child's birth weight 11% decrease in pregnancy complications No statistically significant effects of birth weight on adult health outcomes such as hypertension, anemia, and diabetes. No statistically significant effects of birth weight on income-related measures. 200g increase in birth weight leads to a 0.5 pp increase in probability of being observed in adulthood -- small selection bias. Found no statistically significant evidence of compensating or reinforcing parental investments when considering early medical care and breastfeeding.
The long-term economic impact of in utero and postnatal exposure to malaria (Barreca (2009)). Malaria mortality from U.S. mortality statistics 1900-1936. n=1,147 state-year observations. Climate data n=1,813 obs. Adult outcomes from 1960 Census. All data merged at state/year of birth level.	IV using fraction of days in "malaria- ideal" temperature range as IV for malaria deaths in state and year.	Note: Only results from the IV regression are reported here. Exposure to 10 additional malaria deaths per 100,000 inhabitants causes: 3.4% less years of schooling Exposure to malaria can account for approximately 25% of the difference in years of schooling between cohorts born in high and low malaria states.
Long-run longevity effects of a nutrition shock in early life: The Dutch Famine of 1846-47. (Berg, Lindeboom, Portrait (2009)). Historical Sample of the Netherlands. Exposed cohort born 9/1/1846-6/1/1848. Non-exposed born 9/1/1848-9/1/1855 and 9/1/1837-9/1/1944.	Key independent variable is exposure to famine at birth. Instrument access to food with variations in yearly average real market prices of rye and potatoes for three different regions. Controlled for macroeconomic conditions, infant mortality rates, and individual demographic and socio-economic characteristics.	**Results from nonparametric regression:** Residual life expectancy at age 50 is 3.1 years shorter for exposed men than for men born after the famine; 1.4 years shorter than for men born before famine. Kolmogorov-Smirnov test suggests that the survival curves after age 50 differ significantly for exposed and control men. Max difference in distribution = 0.15 at age 56. **Results from parametric survival models**: Exposure to famine at birth for men reduces residual life expectancy at age 50 by 4.2 years. No statistically significant results for women.

Maternal stress and child well-being: evidence from siblings (Aizer, Stroud, and Buka (2009)). National Collaborative Perinatal Project. Births 1959-65 in Providence and Boston. N=1,103 births to 915 women. 163 siblings in sample.	Sibling fixed effects. Estimated effect of deviations from mean cortisol levels (cortisol measures maternal stress) during pregnancy on outcomes.	No significant effects on birth weight or maternal postnatal investments. Exposure to top quartile of cortisol level distribution (relative to the middle) leads to 47% of SD decrease in verbal IQ at age 7 (sign. at 10% level). 1 SD deviation in cortisol levels leads to 26% of SD decrease in educational attainment. Exposure to top quartile of cortisol level distribution (relative to the middle) leads to 51% of SD decrease in educational attainment.
The scourge of Asian flu: the physical and cognitive development of a cohort of British children in utero during the Asian Influenza Pandemic of 1957, from birth until age 11 (Kelly (2009)). NCDS 1958 cohort. N=16,765 at birth, 14,358 at 7, 14,069 at 11.	Effect of flu on each cohort member identified using variation in incidence of epidemic by local authority of birth. Epidemic peaked when cohort members were 17-23 weeks gestation. 1/3 of women of chlid-bearing age were infected, hence true treatment effect can be estimated by multiplying results by 3.	No statistically significant evidence of reinforcing or compensating parental investments. 1 SD increase in epidemic intensity decreases birth weight by .03-.363 SD; decreases test scores by 0.067 of SD at age 7, by 0.043 of SD at age 11; increases detrimental effect of mother preeclampsia from -0.075 to -0.11 SD on birth weight; increases detrimental effect of mother smoking by 0.03 of SD on birth weight; increases detrimental effect of mother under 8 stone weight from -0.54 to -0.61 of SD on birth weight.
Do lower birth weight babies have lower grades? Twin fixed effect and instrumental variables evidence from Taiwan (Lin and Liu (2009)). Birth Certificate data for Taiwan. High school entrance exam results from Committee of Basic Competence Test. N=118,658. Twin sample n=7,772.	Twin fixed effects and IV using the public health budget and number of doctors in county where child was born as instruments for child's birth weight.	Increase in birth weight: 100g/100g (IV for <9 yrs ed and <25 years) Increase Chinese score: 0.5%/8.2% Increase English score: 0.3%/ Increase Math score: 0.7%/12.6% Increase Natural Science score: 0.8%/11.8% Increase Social Science score: 0.4%/
Poor, Hungry and Stupid: Numeracy and the Impact of High Food Prices in Industrializing Britain, 1750-1850 (Baten, Crayen and Voth (2007)). Data on age heaping from 1951 to 1881 British Census. Information on poor relief from Boyer (1990).	Age heaping is rounding age to nearest 5 or 10. Whipple index (WI) = number of ages that are multiples relative to expected number given uniform age distribution. Regressions use WI as dependent variable. Wheat prices and poor relief measures are independent variables.	During the Revolutionary and Napoleonic Wars, the price of wheat almost doubled -- and the number of erroneously reported ages at multiples of 5 doubled from 4% to 8%. Men and women born in decades with higher WI sorted into jobs that had lower intelligence requirements. 1 SD increase in the Whipple Index associated with a 2.8% (relative to median earnings) decrease in earnings.

<table>
<tr><td colspan="2">IMPACTS OF ECONOMIC SHOCKS</td><td></td></tr>
<tr><td>Birth weight and income: interactions across generations (Conley and Bennett (2001)). Data from PSID. Two samples: 1) children born 1986-1992, n=1,654; 2) reached 19 by 1992, n=1,388 individuals in 766 families.</td><td>Sibling fixed effects.</td><td>Low birth weight child who spent 6 years at poverty line is less likely to graduate high school than normal birth weight child who spent 6 years at poverty line. Low birth weight child who spent 6 years with income 5 times the poverty line is as likely to graduate high school as normal birth weight child of same income (significant at 10% level).</td></tr>
<tr><td>Economic conditions early in life and individual mortality
(Berg, Lindeboom, and Portrait (2006)). Data from Historical Sample of the Netherlands on individuals born 1812-1903 with date of death observed by 2000. n=9,276. Merged with historical macro time series data.</td><td>Compared individuals born in booms with those born in the subsequent recessions. Controlled for wars and epidemics.</td><td>Comparing those born in boom of 1872-1876 with those born in recession of 1877-1881:
Boom T|T>2 = 66.0 years, Recession T|T>2 = 62.5 years
Boom T|T>5 = 70.8 years, Recession T|T>5 = 67.5 years
Regression results:
Boom T|T>2 is 1.58 years greater than Recession T|T>2
Notation: T|T>2 = average lifetime given survival past age 2
T|T>5 = average lifetime given survival past age 5</td></tr>
<tr><td>Evidence on early-life income and late-life health from America's Dustbowl era
(Cutler, Miller, and Norton (2007)). The Health and Retirement Study. N=8,739 people born between 1929 and 1941. Agricultural data from National Agricultural Statistics Service. Income data from Bureau of Economic Analysis.</td><td>Measured economic conditions in utero using income and yield from the same calendar year for those born in 3rd or 4th quarter of the year and from the previous calendar year for those born in 1st or 2nd quarter. Regressions include the in utero economic condition measure (log income, log yield), dummy for whether respondent's father was a farmer and interaction of the dummy with the economic conditions. Controlled for region and year of birth fixed effects, region-specific linear time trends, and region-year infant death and birth rates, and other demographic characteristics.</td><td>No statistically significant relationship between poor early-life economic conditions during the Dustbowl and late-life health outcomes such as heart conditions, stroke, diabetes, hypertension, arthritis, psychiatric conditions, etc.</td></tr>
<tr><td>Long-run health impacts of income shocks: wine and Phylloxera in 19th century France
(Banerjee et al. (2007)). See paper references for source of data on wine production, number of births, and infant mortality. Department level data on heights from military records 1872-1912. n=3,485 year-departments.</td><td>Regional variation in a large negative income shock caused by Phylloxera attacks on French vineyards between 1863 and 1890. Shock dummy equal to 0 pre Phylloxera and after 1890 (when grafting solution found). Dummy equal 1 when wine production<80% of pre-level. Difference-in-difference comparing children born in affected and unaffected areas before and after.</td><td>Decrease in wine production was not compensated by an increase in other agricultural production (e.g. wheat), suggesting that this was truly a large negative income shock.
Main outcomes:
3-5% decline in height at age 20 for those born in wine- growing families during the year that their region was affected by Phylloxera
Those born in Phylloxera-affected year are 0.35-0.38 pp more likely to be shorter than 1.56 cm.
No statistically significant effects on other measures of health or life expectancy.</td></tr>
</table>

IMPACTS OF ENVIRONMENTAL SHOCKS		
Lifespan depends on the month of birth (Doblhammer and Vaupel (2001))	Used t tests to perform pairwise comparisons between mean age at death by quarter of birth. To test whether the seasonal difference in the risk of death accounts for differences in adult life span by month of birth, calculated monthly deviations from annual death rates and used weighted least squares regression with dummies for month of birth, current month, age since last birthday in months, sex,and birth cohort. To test whether selective survival or debilitation during the 1st year of life explains differences in life expectancy at age 50, calculated monthly death rates during 1st year of life and monthly deviations from annual death rates during 1st year of life; then used a multivariate regression without controls for sex and birth cohort.	Note: Autumn-Spring = average difference in age at death between people born in Autumn (Oct-Dec) and in the Spring (Apr-Jun) **Denmark:** mean remaining life expectancy at age 50 = 27.52 years 0.19 years shorter lifespans for those born in 2nd quarter 0.12 years longer lifespans for those born in 4th quarter. Correlation between infant mortality at time of birth and adult mortality after age 50=0.87. **Austria:** average age at death = 77.70 years 0.28 years shorter lifespans for those born in 2nd quarter **Austria:** average age at death = 77.70 years 0.28 years shorter lifespans for those born in 2nd quarter 0.32 years longer lifespans for those born in 4th quarter. Additional results shown for specific causes of death. **Australia:** mean age of death = 78.00 years for those born in 2nd quarter; mean age of death = 77.65 years for those born in 4th quarter British immigrants born Nov.-Jan. have age of death .36 years higher than natives. Those immgrants born Mar-May have age of death .26 years lower than Australian natives.
Air Quality, Infant Mortality, and the Clean Air Act of 1970 (Chay and Greenstone (2003)). County-level mortality and natality data 1969-1974. Annual monitor level data on total suspended particles from EPA. N=501 county-years.	Clean Air Act imposed regulations on polluters in counties with TSP concentrations exceeding federal ceilings. Used nonattainment status as an instrument for changes in TSP.Also used regression discontinuity methods to examine effect of TSP on infant mortality.	1% decline in TSP pollution results in 0.5% decline in infant mortality rate.Most effects driven by reduction of deaths occuring within 1 month of birth.
Air Pollution and Infant Health: What Can We Learn from California's Recent Experience? (Currie and Neidell (2005)). Data on pollution from California EPA. Individual-level infant mortality data from California vital statistics 1989-2000. n=206,353.	Estimated linear models that approximate hazard models, where the risk of death is defined over weeks of life, and length of life is controlled for with a flexible nonparametric spline. Controlled for prenatal and postnatal pollution exposure, weather, child's age, and numerous other child and family characteristics, as well as month, year, and zip code fixed effects. Examined effects of ozone (O3), carbon monoxide (CO), and particulate matter (PM10) on infant mortality.	1.1 unit reduction in postnatal exposure to CO (the actual reduction that occurred in CA in the 1990s) saved 991 infant lives - 4.6% decrease in infant mortality rate. No statistically significant effects of prenatal exposure to any of the pollutants.

Prenatal exposure to radioactive fallout (from Chernobyl) and school outcomes in Sweden (Almond, Edlund, and Palme (2007)). All Swedes born 1983-1988, n=562,637. Data on radiation from Swedish Geological Survey at parish level.	Three empirical strategies: 1) Cohort comparisons - Compared cohorts in utero before, during, and after Chernobyl with particular focus on cohort 8-25 weekspost conception. 2) Within-cohort comparisons- Use geographic variation in levelsof exposure. Define 4 regions, R0 (least exposure), R1, R2, R3. 3) Diff-in-diff sibling comparison -Compared those exposed to radiation in utero 8-25 to siblings who were not.	**Results from 1):** Probability of qualifying for high school reduced by 0.2% for cohort in utero during radiation; by 0.6% for cohort in utero 8-25 Grades reduced by 0.4% for cohort in utero during radiation;by 0.6% for cohort in utero 8-25. **Results from 2):** (R3 relative to R0) Probability of qualifying for high school reduced by 3.6% for cohort in utero during radiation in R3; by 4% for cohort in utero 8-25 in R3. Grades reduced by 3% for cohort in utero during radiation in R3; by 5.2% for cohort in utero 8-25 in R3. **Results from 3):** (R3 relative to R0) Difference in probability of qualifying for high school between siblings increased by 6% in R3. Difference in grades between siblings increased by 8% in R3.
Air Pollution and Infant Health: Lessons from New Jersey (Currie, Neidell, and Schmieder (2009)). Pollution data from EPA. Individual-level data on infant births and deaths from the New Jersey Department of Health 1989-2003. n=283,393 for mother fixed effects models.	Mother fixed effects to estimate impact of exposure to pollution (during and after birth) on infant health outcomes.Air pollution measured from air quality monitors and assigned to each childbased on home address of mother. Also included interactions b/n variable for pollution exposure and maternal smoking as well as other maternal characteristics. Controlled for weather, pollution monitor locations, time trends, seasonal effects, and other background characteristics.	1 unit change in mean CO exposure during last trimester ofpregnancy decreases average birth weight by 0.5%, increases likelihood of low birth weight by 8%, and decreases gestation by 0.2%. 1 unit change in mean CO exposure in first 2 weeks after birth increases likelihood of infant mortality by 2.5%. Estimated that a 1 unit decrease in mean CO exposure in first 2 weeks after birth would save 17.6 per 100,000 lives. Effects of CO exposure on infant health at birth are 2-6 times larger for smokers and mothers who are over age 35. Effects of PM10 (particulate matter) and ozone are not consistently significant across the specifications.
Fetal Exposures to Toxic Releases and Infant Health (Currie and Schmieder (2009)). Toxic Release Inventory data at county-year level matched to county-year level natality and infant mortality data. N=5,279.	Multivariate regression of infant health outcomes on amount of toxic releases in each county and year, controlling for demographic and socio-economic characteristics, mother drinking or smoking during pregnancy, county employment, and county and year fixed effects. Compared effects of developmental toxins to other toxins, and "fugitive" air releases to "stack" air releases (since emissions that go up a smoke stack are more likely to be treated in some way, and hence will affect those in the vicinity less).	An additional thousand pounds per square mile of all toxic releases leads to: 0.02% decrease in length of gestation 0.04% decrease in birth weight 0.1% increase in probability of low birth weight 0.8% increase in probability of very low birth weight 1.3% increase in infant mortality Larger effects for developmental toxins and for fugitive air releases than for other toxins or for stack air releases. A 2-SD increase in lead releases decreases gestation by 0.02% and decreases birth weight by 0.05%. A 2-SD increase in cadmium releases decreases gestation by 0.03%, decreases birth weight by 0.07%, increases probability of low birth weight by 1.2%, increases probability of very low birth weight by 1.4%, and increases infant mortality by 5%. Similar results for toluene, epichlorohydrin. Reductions in releases over 1988-1999 can account for 3.9% of the reduction in infant mortality over the same time period.

Traffic Congestion and Infant Health: Evidence From E-ZPass (Currie and Walker (2009)). Birth records for Pennsylvania and New Jersey, 1994-2003. Data on housing prices for New Jersey. N=727,954 for diff-in-diff. N=232,399 for mother fixed effects.	Identification due to introduction of electronic toll collection (E-ZPass), which reduced traffic congestion and motor vehicle emissions in the vicinity of highway toll plazas. Difference-in-difference, comparing infants born to mothers living near toll plazas to infantsborn to mothers living near busy roadways (but away from toll plazas), before and after introduction of E-ZPass. Also estimated impacts of exposureto E-ZPass on infant health using mother fixed effects methods. Controlled for various background characteristics, year and month fixed effects, and plaza-specific time trends. Also estimated impact of E-ZPass introduction on housing prices near the toll plaza.	**Results from Difference-in-Difference Method:** Reductions in traffic congestion generated by introduction of E-ZPass reduced incidence of premature birth by 10.8% and low birth weight by 11.8% for children of mothers living w/in 2km of a toll plaza. For those living w/in 3km of a toll plaza, effects are 7.3% for premature births and 8.4% for low birth weight. Similar results using mother fixed effects. No effects of E-ZPass introduction on housing prices or demographic composition of mothers living near toll plazas.
Caution, Drivers! Children Present. Traffic, Pollution, andInfant Health. (Knittel, Miller, and Sanders (2009)). Traffic data from the Freeway Performance Measurement System. Pollution data from EPA. Birth data from California Dept. of Public Health 2002-2006. N=373,800.	IV using traffic shocks (due to accidents or road closures) to instrument for air pollution. Preferred specifications include traffic flow, delays, and interactions between traffic and weather.	Note: only results from the IV regression are reported here. 1 unit decrease in exposure to particulate matter (PM10) leads to 5% decrease in infant mortality rate (saves 14 lives per 100,000 births).

Note: Unless otherwise noted, only results significant at 5% level are reported.

Table 5: Impacts of Early Childhood Shocks on Later Outcomes

Study and Data	Study Design	Results
CHILDHOOD PHYSICAL AND MENTAL HEALTH		
Mental Health in Childhood and Human Capital (Currie and Stabile (2006)) Data from NLSCY and NLSY. Canadian NLSCY: data on children aged 4-11 in 1994. Mental health screening in 1994; outcomes measured in 2002. n= 5604. US NLSY: data on children aged 4-11 in 1994. Mental health score averaged over 1990-1994. n=3758.	Sibling fixed effects. Hyperactivity and agression scores based on"screener" questions asked of all children. Estimate effect of hyperactivity on grade repetition, reading and math scores, special education, and delinquency. Controlled for individual background characteristics. Estimated same model omitting children with other learning disabilities besides those in the main explanatory variable.	**1 unit change in hyperactivity score:** increases probability of grade retention by 10-12% in both US and Canada decreases math scores by 0.04-0.07 SD in both US and Canada increases probability of being in special ed by 11% in US decreases reading scores by 0.05 SD in US **1 unit change in conduct disorder score (in the US only):** increases probability of grade retention by 10% decreases math scores by 0.02 SD decreases reading scores by 0.03 SD **1 unit change in aggression score (in Canada only)** decreases probability that a youth aged 16-19 is in school by 4% High depression scores increase probability of grade retention by 10% in both US and Canada. No significant effects of interaction between mental health scores and income or maternal education.
Disease and Development: Evidence from Hookworm Eradication in the American South (Bleakley (2007)). Data on hookworm infection rates from the Rockefeller Sanitary Commission surveys for 1910-1914. Census data from IPUMS for 1880-1990. n=115.	Identification due to different pre-eradication hookworm infection rates in different states. Compare individuals born before and after eradication campains funded by the Rockefeller Sanitary Commission (RSC)..Active years of RSC were 1910-1915. Considered contemporaneous effects on children as well as long-term effects on adult wages and educational attainment. Controlled for geographic and year fixed effects, as well as some individual characteristics. In regressions for contemporaneous effects, geographic units are state economic areas. In regressions for long-term effects, geographic units are states of birth.	**Contemporaneous Effects of Infections on Children:** 1 SD increase in lagged hookworm infection associated with: 0.18-0.25 SD decrease in school enrollment. 0.21-0.28 SD decrease in full-time school enrollment 0.1 SD decrease in literacy Results robust to inclusion of state-year fixed effects, controlling for mean-reversion in schooling, and using state-level infection rates. Larger effects for blacks than for whites. No contemporaneous effects on adults. **Long-Term Effects of Infections in Childhood on Adults:** Being infected with hookworm in one's childhood leads to a reduction in wages of 43% and a decrease in returns to schooling by 5%. 80% reduction in wages due to hookworm infections explained by reduced returns to schooling. Being infected with hookworm in one's childhood leads to a reduction in occupational income score by 23% and a decrease in Duncan's Socio-Economic Index by 42%.

Adult Health and Childhood Disease (Bozzoli, Deaton,and Quintana-Domeque (2009)) Data on height from the European Community Household Panel, the Health Survey of England, and the National Health Interview Survey in the US for individiuals born in 1950-1980. Data on post-neonatal mortality from the World Health Organization. n=316. Also used data on women's heights from the international system of Demographic and Health Surveys on women aged 15-49 in more than 40 countries in the late 1990s-2000s. Data on infant mortality from the United Nations population division. n=1514.	Analyzed relationship b/n post-neonatalmortality (PNM, death after 28 days and before first birthday) and adult height. PNM is a measure of childhood health environment. OLS regression with adult height as outcome variable, controlling for country and year fixed effects and a time trend. Also controlled for neonatal mortality rates and GDP in year and country of birth. Considered mortality from pneumonia, intestinal disease, congenital anomalies, and other causes separately.	Survivors are expected to be positively selected relative to those who died, but may still be stunted by illness. In poor countries, the selection effect dominates, whereas in rich countries (with low mortality rates) the stunting effect dominates. Overall correlation b/n PNM and average adult height of the same cohort in a given country = -0.79. In the US, PNM was 3x larger than in Sweden in 1970. This diff accounts for 20-30% of the 2-cm diff in average height b/n 30-yr-old Americans and Swedes in 2000. After controlling for PNM, no relationship b/n adult height and GDP in the year and country of birth. Biggest determinant of differences in PNM rates across countries is mortality from intestinal disease, followed by mortality from pneumonia. Out of the 4 determinants of PNM, only mortality from pneumonia has a significant negative effect on adult height.
Stature and Status: Height, Ability and Labor Market Outcomes (Case and Paxson (2008a)). Data from NCDS (on cohort of individuals born in week of 3/3/1958 in Britain), BCS, NLSY (on siblings in the US), and Fragile Families and Child Well-Being Study (on young children in the US). NCDS: n=9155; BCS: n=9003; NLSY: n=13884 (total, not siblings); Fragile Families: n=2150.	Analyzed relationship b/n adult height and cognitive ability as a potential explanation for the significant and positive relationship b/n adult height and earnings that is observed. Adult height is a proxy for early childhood health conditions. OLS regression of cognitive test scores on height-for-age (HFA) z-scores,controlling for background and demographic variables (using NCDS, BCS and Fragile Families data). Same regression with mother fixed effects using NLSY79 data. OLS regression of log hourly earnings on adult height, controlling for cognitive test scores at ages 7, 10, and 11, and other background and demographic variables.	**Child's height and cognitive test scores at age 3**: 1 SD increase in HFA z-score linked to a 0.05-0.1 SD increase in PPVT score. **Child's height and cognitive test scores at ages 5-10:** (reporting only mother fixed effects results) 1 SD increase in HFA z-score linked to increased PIAT math score, PIAT reading recognition score, and PIAT reading comprehension score by 0.03 SD. Height does not explain differences in test scores across racial groups. **Adult height, earnings, and cognitive test scores:** Inclusion of cognitive test scores at ages 7, 10, and 11 makes the coefficient on adult height insignificant for predicting hourly wages. Height difference b/n men and women does not account for the difference in earnings.

Height, Health and Cognitive Function at Older Ages (Case and Paxson (2008b)). Data from HRS on men and women aged 50 and older from 1996 to 2004. n=72,258.	OLS regression of health and cognition measures on adult height, controlling for age, survey wave, race and sex. Adult height is a proxy for early childhood health and nutrition.Also estimated models including controls for childhood health (self-reported), completed education, and a dummy for employment in a white-collar occupation.	1 in increase in height associated with: 0.9% increase in delayed word recall score 0.3% increase in probability of being able to count backwards 0.3% increase in probability of knowing the date 1.4% decrease in depression score 0.4% decrease in health status scale (1 = excellent, 5 = poor) Childhood health, completed education, and employment in a white-collar occupation all positively related to adult cognitive function and health. Inclusion of these controls makes the coefficients on adult height insignificant except for the cases of delayed word recall and depression.
The Role of Childhood Health for the Intergenerational Transmission of Human Capital: Evidence from Administrative Data (Salm and Schunk (2008)). Administrative data from the departmentof health services in the city of Osnabrueck, Germany, collected during official school entrance medical examinations on children aged 6 between 2002-2005. Sibling sample: n=947. 321 children had at least one parent w/ college degree.	Sibling fixed effects to estimate impact of health conditions on cognitive and verbal ability at school entrance. Controlled for individual background characteristics. Used the Oaxaca (1973) decomposition and a nonparametric decomposition to estimate how much of the gap b/n children of college-educated parents and children of less educated parents can be explained by chronic childhood health conditions.	Mental health conditions reduce cognitive ability by 10% for the whole sample, by 9% for college-educated sample, by 11% for less-educated sample. Asthma reduces cognitive ability by 8% for less-educated sample only. Mental health conditions reduce verbal ability by 11% for the whole sample, and by 13% for the less-educated sample. Health conditions explain 18% of the gap in cognitive ability and 65% of the gap in language ability b/n children of college-educatedand less-educated parents.
Long-Term Economic Costs of Psychological Problems During Childhood (Smith and Smith (2008)) Data from PSID on siblings (with a special supplement designed by the authors in the 2007 wave, asking retrospective questions about childhood health). Sample consists of sibling children of the original participants who were at least 16 in 1968. Sibling children were at least 25 in 2005.	Sibling fixed effects to estimate the impact of reporting having had childhood psychological problems (measured by depression, drug or alcohol abuse or other psychological problems before age 17) on later life socio-economic outcomes. Controlled for individual childhood physical illnesses (asthma, diabetes, allergic conditions, and many others) as well as family background characteristics.	Having had psychological problems during childhood leads to: 20% reduction in adult earnings ($10,400 less per year; $17,534 less family assets) reduction in number of weeks worked by 5.76 weeks per year 11pp reduction in likelihood of getting married 33.5pp reduction in educational attainment (means not reported, so can't calculate relative effect sizes) **Costs of childhood psychological problems:** Lifetime cost: $300,000 loss in family income $3.2 trillion cost for all those affected

Early Life Health and Cognitive Function in Old Age (Case and Paxson (2009)) Region-level historical data on mortality from infectious diseases, as well as total infant mortality. Data for 1900-1936 from Grant Miller's data archive on NBER website. Data for 1937-1950 from Vital Statistics documents. Data on later life outcomes from Health and Retirement Study for 1996-2004 on men and women aged 50-90. n=60,000	Identification due to variation in mortality rates across time and regions. OLS regression of later life outcomes on log mortality rates from various infectious diseases in year of birth and in 2nd year of life in region of birth. Controlled for age, sex, race, and current census region of residence.	Decrease in infant mortality by half during 2nd year of life associated w/ an increase in delayed word recall score by 0.1 SD. Significant negative impacts of typhoid, influenza, and diarrhea mortality in 2nd year of life on delayed word recall score. (Means not reported so can't calculate relative effect sizes). Weaker associations b/n disease mortality and ability to count backwards. No significant impacts of disease mortality and overall mortality during year of birth, once mortality in 2nd year of life is included, on either of the later life outcomes. Results not robust to adding census region-specific time trends.
Child Health and Young Adult Outcomes (Currie et al (2009)) Administrative data on public health insurance records from the Canadian province of Manitoba on children born b/n 1979 and 1987, followed until 2006. n=50,000.	Sibling fixed effects of the long term effects of health problems at various child ages controlling for health at birth (with birth weight and congenital anomalies). Key explanatory variables examined are: asthma, major injuries, ADHD, conduct disorders, and other major health problems at ages 0-3, 4-8, 9-13, and 14-18. Key outcome variables: achievement on standardized test on language arts in grade 12, whether child took college-preparatory math courses in high school,whether child is in grade 12 by age 17, and welfare participation after becoming eligible at 18.	**Results reported below are only those that control for health problems at all age groups:** An additional major health condition at ages 0-3/4-8/14-18 increases the probability of being on welfare by 10%/9%/31%. Effects of major health conditions at younger ages on educational outcomes not significant when controlling for health at ages 14-18. **ADHD/conduct disorders diagnosis:** at ages 0-3/4-8/9-13/14-18 decreases probability of being in grade 12 by age 17 by 4%/10%/17%/19%; at ages 4-8/9-13/14-18 increases probability of being on welfare by 38%/44%/109%; at ages 4-8/9-13/14-18 decreases probability of taking a college prep math class by 11%/25%/35%; at ages 4-8/9-13/14-18 decreases literacy score by 0.15 SD/0.23 SD/0.27 SD. Effects of asthma at younger ages not statistically significant once health at ages 14-18 is controlled for. **Major injury:** at ages 0-3/14-18 increases probability of being on welfare by 7%/9%; at ages 9-13/14-18 decreases probability of being in grade 12 by age 17 by 2%/2%; at ages 9-13/14-18 decreases probability of taking a college prep math class by 6%/8%; at ages 9-13/14-18 decreases literacy score by 0.03 SD/0.03 SD. Children who have a major physical health condition and then recover do not have significant adverse outcomes. Children with mental health conditions, children with major health conditions at ages 14-18, and those with conditions that persist for multiple age periods suffer worse outcomes.

The Impact of Childhood Health on Adult Labor Market Outcomes (Smith (2009)) Data on siblings from PSID. Childhood health measured as a self-reportedretrospective health index regardinghealth at ages younger than 17. Adult outcomes measured in 1999. n=2,248.	Sibling fixed effects to estimate impact of self-reported retrospective childhood health status (on a 5-point scale) before age 16 on adult earnings, employment, education, marital status, etc., controlling for demographics and family background.	Better health during childhood increases adult family income by 24%, increases adult family wealth by 200% (relative to mean=$2000), and increases adult earnings by 25%. Better health during childhood increases probability of having worked the year before by 5.4pp (mean not reported). About 2/3 of overall impact of poor childhood health on adult family income is present at age 25; the remaining 1/3 is due to a slower growth path after age 25 due to poor childhood health. About 1/2 of overall impact of poor childhood health on individual earnings is present at age 25; the remaining 1/2 is due to a slower growth path after age 25 due to poor childhood health. No statistically significant impacts of childhood health on educational attainment.
The Effect of Childhood Conduct Disorder on Human Capital (Vujic et al. (2009)) Data from the Australian Twin Register on twins born between 1964 and 1971. Data collected in 1989-1990 and 1996-2000 n=5322 twins; 2250 identical twins	Twin fixed effects to estimate impact of childhood conduct disorder (measured by various indicators based on diagnostic criteria from psychiatry on educational attainment and criminal behavior in adulthood. Controlled for birth weight, timing of the onset of conduct disorders, and other family and individual background characteristics. Conducted separate analyses for all twins and identical twins.	Childhood conduct disorders lead to: 5-16% decrease in likelihood of high school graduation 100-228% increase in likelihood of being arrested (mean=.07) 6-68% increase in likelihood of grade retention 20-60% increase in likelihood of having 3+ job quits (not significant in identical twin sample) 50-325% increase in likelihood of telling lies (mean=.04) 37-526% increase in likelihood of going to jail (mean=.019) Earlier occurrence of conduct disorder has larger negative effects than later occurrence.
Causes and Consequences of Early Life Health (Case and Paxson (2010a)) Data from NCDS, BCS, PSID, Whitehall II Study (longitudinal study of British civil servants b/n ages 34 and 71, collected from 1985 to 2001), HRS, and NLSY79. See Case and Paxson (2008a) and (2008b) for more info on the data sets. n=11,648 (NCDS), 11,181 (BCS), 63,995 (PSID), 29,774 (Whitehall II), 66,269 (HRS), n=3,200-46,000 (NLSY79)	Multivariate regression of educational attainment, employment, log earnings, and self-reported health status and cognitive function on adult height in inches (proxy for early childhood health) using 5 longitudinal data sets. Controlled for age, ethnicity, sex, and survey wave. Used sibling fixed effects in NLSY79 to understand what aspects of early life health adult height captures - regressed children's test scores, grade level, self-perception in school, and childhood health outcomes on children's HAZ.	Results from NCDS, BCS, PSID, Whitehall II, and HRS: One inch of height is associated with: 0.05-0.16 more years of schooling; 0.2-0.6 pp increase in likelihood of employment; 0.012-0.028 increase in average hourly earnings for men; 0.007-0.027 increase in average hourly earnings for women. A 4-in increase in height leads to: 8% decrease in probability of long-standing illness; 40% decrease in probability of disability; 4% of SD decrease in depression score. Results from NLSY79 (reporting sibling fixed effects only) indicate that 1 pt. increase in HAZ leads to: 0.3% increase in PIAT math score; 0.1% increase in PIAT reading recognition score; 0.2% increase in PIAT reading comprehension score; 0.9% increase in digital span test score; 0.4% increase in PPVT score. 2% decrease in likelihood that child is in appropriate grade level for age (ages 6-14). 1% increase in child doing school work quickly; 1% increase in child remembering things easily; 0.9% increase in Total Scholastic Competence score. 8% decrease in likelihood that child has limiting emotional/neurological condition; 0.105 increase in birth weight z-score (mean not reported).

The Long Reach of Childhood Health and Circumstance: Evidence from the Whitehall II Study (Case and Paxson (2010b)) Data from the Whitehall II Study (see above for description). n=10,308	Since most Whitehall II study participants belong to the highest occupational classes, estimated the selection effect using data from NCDS and BCS for comparison. OLS regressions of initial placement and promotion in Whitehall on height, family background characteristics, educational attainment, and other individual characteristics. Also estimated individual fixed effects and first-differenced regressions of future promotions on self-reported health, and of future health on grade in Whitehall.	Selectivity of Whitehall II study attenuates the impact of father's social class on various outcomes relative estimates that would be obtained in the full population. OLS regressions indicate significant correlations between family background (father's social class), initial placement and likelihood of promotion. Significant relationship between initial placement and promotion, height, and self-reported health. Although some of the effects of health on placement and promotion are mediated by educational attainment, there is an independent effect of health. Individual fixed effects estimates indicate that cohort members who report "excellent" or "very good" health are 13% more likely to be promoted (relative to lowest grade). No effect of grade in Whitehall on future health.
HOME ENVIRONMENT		
The Impact of Maternal Alcohol and Illicit Drug Use on Children's Behavior Problems: Evidence from the Children of the National Longitudinal Survey of Youth (Chatterji and Markowitz (2000)). Data from the 1988, 1992, and 1994 waves of NLSY-Child surveys on children who were 4-14 yrs old during the survey years. n=6194 children in total sample; n=2498 mothers who have siblings in sample; n=7546 obs for children who have 2 or 3 observations in sample.	Estimated impact of maternal substance abuse on children's behavior problems using OLS, IV, child-specific & maternal family fixed effects models. In IV, used alcohol and illicit drug prices as instruments for maternal substance abuse. Controlled for numerous demographic and background variables. Behavior problems measured by the Behavior Problems Index (higher BPI means more problems).	No statistically significant relationship between maternal substance abuse and child behavior in IV regressions, but 1st stage relationship is weak. Fixed effects suggest that 1 more drink consumed by mother in past month increases child BPI by 1%. Maternal marijuana use in past month increases child BPI by 7-8%. Maternal cocaine use in past month increases child BPI by 19%.
Parental Employment and Child Cognitive Development (Ruhm (2004)). Data from the Children of NLSY79 for 1986-1996 (mothers aged 29-38 at the end of 1995). n=3,042.	OLS to estimate the impact of maternal work in the year prior to child's birth, and in the first 4 years of child's life on child cognitive test scores at ages 3-4 and 5-6. Used a rich set of controls for maternal background, demographic, and socio-economic characteristics, as well as assessment year fixed effects. Maternal work measured both by hours and weeks worked during the year. Controlled for family income.	20 more hrs worked each week by mother during child's 1st year: decrease PPVT Score by 0.06-0.10 SD (ages 3-4) 20 more hrs worked each week by mother during child's age 2-3: decrease PIAT-Reading score by 0.06-0.08 SD (ages 5-6) decrease PIAT-Math score by 0.05-0.06 SD (ages 5-6) Results robust to alternative specifications (measuring employment by weeks worked or part-time vs. no employment). More negative effects on PPVT and PIAT-Math scores for boys than girls. More negative effects on PIAT-Reading scores for girls than boys. More negative effects on PPVT an PIAT-Reading scores for whites than blacks. More negative effects on PIAT-Math scores for blacks than whites.

Maternity Leave, Early Maternal Employment and Child Health and Development in the US (Berger, Hill, and Waldfogel (2005)) Data from the Children of NLSY79 on children born b/n 1988 and 1996. n=1,678.	OLS and propensity score matching to estimate impact of maternal return to work within 12 weeks of birth on child health and developmental outcomes. Models control for numerous background characteristics as well as state and year of birth fixed effects. Sample limited to mothers who worked pre-birth.	Mother returning to work within 12 weeks of giving birth leads to: 2.5% decrease in likelihood of well-baby visit 12.8% decrease in likelihood of any breastfeeding 40.4% decrease in number of weeks breastfed 4.3% decrease in child getting all DPT/Polio immunizations Results from OLS consistent with results from propensity score matching methods. Larger negative effects of returning to work full-time within 12 weeks of giving birth compared to any work at all.
Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development (Baker and Milligan (2009)) Data from the NLSCY in Canada on children up to age 29 months and their mothers in 1998-2003.	Identification due to large expansions in parental leave policies in Canada on Dec. 31, 2000 (from 25 to 50 weeks of leave). OLS regression. Key explanatory variable is a dummy for birth after Dec. 31, 2000. Outcomes include child development indicators, maternal employment, and use of child care. Controlled for various individual and family background characteristics as well as labor market variables. Observations from Quebec omitted due to changes in childcare policies over the same time period. Observations from single-parent families omitted b/c of expansions in benefits to those families. Subsample analysis for women who returned to work within 1 year of child's birth.	Expansions in parental leave policies led to a 48-58% increase in months of maternal care during the 1st year of life and a 25-29pp decrease in non-parental care. No statistically significant effects of parental leave expansions on child development indicators (motor/social development, behavior, physical ability, cognitive development).

The Effect of Expansions in Maternity Leave Coverage on Children's Long-Term Outcomes (Dustmann and Schonberg (2009)). Administrative data on students in Germany attending public schools in Hesse, Bavaria, and Schleswig-Holstein (3 states in Germany) for 2002-03 to 2005-06. n=101,257. Administrative data on social securityrecords for German individuals born b/n July 1977 and June 1980. n=140,387	Identification due to 3 policy reforms in Germany that expanded unpaid and paid maternity leave to estimate impact on adult wages, employment, and educational outcomes. First reform in 1979 increased paid leave from 2 to 6 months. Second reform in 1986 increased paid leave from 6 to 10 months.Third reform in 1992 increased unpaid leave from 18 to 36 months. OLS regression, comparing children born one month before and after each reform,controlling for background variables and state fixed effects. Also difference-in-difference, comparing children born before and after leave expansions with children born in the same months the year before (hence not affected by expansions).	Despite significant delays in maternal return to work due to policy reforms, there are no statistically significant impacts of expansions in maternity leave policies (paid or unpaid) on any long-run outcomes (wages, employment, selective high school attendance, grade retention and grade attendance).
Child Protection and Adult Crime: Using Investigator Assignment to Estimate Causal Effects of Foster Care (Doyle (2008)). Data from Computerized Criminal History System from the Illinois State Police on arrests and imprisonment up to age 31 for 2000-2005, linked with child abuse investigation data on individuals who are 18-35 in 2005. n = 23,254.	IV models of the effect of foster care placement on measures of criminal activity. Identification due to the fact that child protection cases are randomized to investigators, and investigators influence whether a child is placed in foster care or remains at home. Model allows for treatment effect heterogeneity -- used random coefficient model. IV where the instrument is the investigator's probability of using foster placement relative to the other investigators.	Children on the margin for placement into foster care are 1.5 times more likely to be arrested, 2.68 times more likely to have a sentence of guilty/withheld, 3.41 times more likely to be sentenced to prison if they are placed into foster care. Children on the margin are likely to include African-Americans, girls, and young adolescents.

Long-term Consequences of Child Abuse and Neglect on Adult Economic Well-Being (Currie and Widom (2009)). Sample of abused/neglected children based on court-substantiated cases of childhood physical and sexual abuse in 1967-1971 in one Midwestern metropolitan county. Maltreated children matched to controls on the basis of sex, race, and elementary school class/hospital of birth. Cases restricted to children 11 yrs or younger. Adult outcomes measured at mean age 41. Used info collected in 1989-95 and 2003-04 interviews. n=1195 in 1989-95; n=807 in 2003-04. Matched sample (both members of matched pair interviewed during the 2 waves): n=358.	Multivariate regression with key explanatory variable being a dummy for having been maltreated. Controlled for demographic and family background characteristics, as well as quarter of year at time of interview. Also estimated models separately for males and females and for the subsample of participants whose families received food stamps or welfare when they were children.	**Individuals who were maltreated as children:** complete 4.3% less years of schooling (1989-95) score 5.3% lower on IQ test (1989-95) have 24% lower imputed earnings (2003-04) are 0.52 times as likely to have a skilled job (1989-95) are 0.46 times as likely to be employed (2003-04) are 0.58 times as likely to own a vehicle (2003-04) For males, the only significant effects of maltreatment are for years of schooling and having a skilled job. For females, significant negative effects of maltreatment on years of schooling, IQ testscores, imputed earnings, being employed, owning a bank account,owning a stock, owning a vehicle, and owning a home. Effects larger for females than for males.
Child Maltreatmentand Crime (Currie and Tekin (2006)) Data from the National Longitudinal Study of Adolescent Health (AddHealth). First wave: 1994-95; last wave: 2001-2002. n=13,509 (full sample), n=928 (twins sample).	OLS, propensity score matching, and twin fixed effects methods. Estimate the impact of maltreatment on criminal activity. Controlled for numerous demographic and background characteristics as well as state fixed effects. Also controlled for parental reports of chlid being bad-tempered or having a learning disability.	Only results that are significant in all 3 main specifications (OLS, propensity scores, and twin fixed effects) are reported. Children who experienced any maltreatment are: 99-134% more likely to commit any non-drug related crime (mean=0.109) 288-489% more likely to commit a burglary (mean=0.009) 113-181% more likely to damage property (mean=0.052) 106-131% more likely to commit an assault (mean=0.049) 183-222% more likely to commit a theft>$50 (mean=0.018) 76-101% more likely to commit any hard-drug related crime (mean=0.085) 96-103% more likely to be a crime victim (mean=0.077) Probability of crime increases if a person suffers multiple forms of maltreatment. Being a victim of maltreatment doubles the probability that an individual is convicted as a juvenile. Cost-Benefit: Estimated costs of crime induced by abuse=$8.8-68.6 billion/year Estimated costs of home visiting programs (that have been shown to reduce cases of maltreatment by 50%)=$14 billion/year

The Effect of Maternal Depression and Substance Abuse on Child Human Capital Development (Frank and Meara (2009)) Data from Children of NLSY79 on children aged 1-5 in 1987. n=1587.	OLS regression of the impact of maternal depression and substance abuse on maternal inputs and children's outcomes in grades 1-5 and 6-9. Controlled for a rich set of demographic and background variables including family measures of parent and sibling behavior and health. For robustness, estimated models using sibling fixed effects (comparing siblings born to the same mother) and propensity scores.	**Effects on maternal inputs:** Maternal depression leads to a 0.2 SD decrease in emotional stimulation sub-component of the HOME score (ages 7-10, 11-14) Maternal substance abuse leads to a 0.23 SD decrease in emotional stimulation sub-component of the HOME score (ages 11-14) **Effects on child outcomes:** Maternal depression leads to a 0.46 SD increase in behavioral problems index (ages 7-10, 11-14). Maternal alcohol abuse leads to a 0.31 SD decrease in child's PIAT math score (ages 11-14); 0.29 SD increase in behavioral problems index (ages 7-10); 377% decrease in likelihood of ever being suspended or expelled at any age (mean=0.22).
ENVIRONMENTAL SHOCKS		
Environmental Policy as Social Policy? The Impact of Childhood Lead Exposure on Crime (Reyes (2007)). Data on lead in gasoline for 1950-1990 on a state-by-state level from Yearly Report of Gasoline Sales by State, Petroleum Marketing Annual, and Petroleum Products Survey. Data on air lead exposure from the EPA's Aerometric Information Retrieval System. Data on per capita crime rates from the Uniform Crime Reports compiled by the Federal Bureau of Investigation for 1985-2002. Data on Data on individual blood lead levels from NHANES for 1976-1980 only.	Used state-by-state variation in the decline of lead exposure from gasoline between 1975 and 1985 due to enforcement of the Clean Air Act to identify the impacts of childhood lead exposure on violent crime in young adulthood. Also estimated the impact of lead exposure on levels of lead in blood during childhood for robustness. Primary measure of lead exposure is the grams of lead per gallon of gasoline in automobile sources. Calculated air lead exposure (measure of average concentration of lead in the air in each state and year). Calculated effective lead exposure relative to each crime in each state and year as the weighted average of lead exposure b/n ages 0 and 3 for all cohorts of arrestees. Regression of per capita crime rate on effective lead exposure, controlling for state and year fixed effects and other state-year characteristics that could affect crime rates.	Elasticity of violent crime with respect to lead exposure = 0.8. Changes in childhood lead exposure in the 1970s are responsible for a 56% decrease in violent crime in the 1990s (while abortion is responsible for a 29% drop in violent crime). Weak evidence for link b/n lead exposure and murder or property crimes. Cost-Benefit Analysis: Cumulative social cost of switching to unleaded gasoline: $15-65 billion. Cumulative social benefit of reduced crime from reduced lead exposure: $1.2 trillion. Note, due to data limitations, it is not possible to make a direct connection between blood lead levels, lead in gasoline, and criminal activity at the individual level.

The Long-Term Effects of Early Childhood Lead Exposure: Evidence from Sharp Changes in Local Air Lead Levels Induced by the Phase-Out of Leaded Gasoline (Nilsson (2009)). Data on air lead exposure from Swedish Environmental Protection Agency thatuses a nationwide grid of moss samples collected in 1000 locations evenly spread across Sweden for average lead exposure levels for 1972-74, 1977-79, and 1982-84. Individual outcome data from the Educational database at the Institute for Labor Market Policy Evaluation in Uppsala, Sweden on those born in 1972-74, 1977-79, and 1982-84. n=797,889	Identification due to geographical variation in childhood lead exposure due to a policy in Sweden that induced a phase-out of leaded gasoline between 1973 and 1981. Difference-in-difference comparing children born in municipalities that experienced large reductions in lead exposure to those born in municipalities with relatively no change, before and after policy went into effect, to estimate impact of childhood lead exposure on later life outcomes. Also used mother fixed effect methods, comparing siblings who experienced different levels of lead exposure in childhood. Controlled for various background characteristics as well as year of birth and municipality of birth fixed effects.	A decrease in blood lead levels by 30 mg/Kg leads to: 3% decrease in likelihood of being in the lower end of the GPA distribution 0.6pp increase in average IQ for males 0.05 yr increase in schooling attainment 0.7pp increase in likelihood of graduating high school 0.5pp decrease in likelihood of receiving welfare in adulthood Effects of lead are stronger at lower end of ability/skills distribution and for children from poorer households. Effects of lead are stronger when exposure is at ages 0-2 than when exposure is at ages 5-7. No statistically significant difference in effects by gender. Nonlinear effects - significant and negative effects of lead exposure above 75th percentile (>50mg/kg). No statistically significant effects of exposure to lead below 75th percentile levels. Estimated that there exists a blood lead threshold of 60 mg/L below which reductions in lead exposure have no effect on outcomes. Similar results using mother fixed effects. Note: means not reported, so relative effect sizes can't be calculated.

Note: Unless otherwise noted, only results significant at the 5% level are reported. Percent changes reported relative to the mean.

Table 6: Effects of Income on Birth and Early Childhood Outcomes: Evidence from the US and Around the World

Study and Data	Study Design	Results
The link between AFDC participation and birthweight (Currie and Cole (1993)). Data from NLSY merged with state andcounty level data. Data on children born between 1979-1988. (n=5,000).	IV: using parameters of AFDC, FSP and Medicaid as instruments, controlling for child characteristics and region fixed effects. Separate regressions for poor black and poor white women. Sibling FE: comparing sibs where mother on AFDC during pregnancy to others.	OLS results suggest that AFDC has negative effects. Sibling fixed effects indicate no significant effects. IV results suggest that AFDC during pregnancy causes large increases in mean birth weight among poor whites only. Hence, evidence suggests that AFDC has neutral or positive rather than negative effects. Negative estimates driven by selection.
Does Money Really Matter? Estimating Impacts of Family Income on Children's Achievement with Data from Random-Assignment Experiments (Morris, Duncan, and Rodrigues (2004)). Data from four studies that evaluated 8 welfare and anti-poverty programs with randomized designs: Connecticut's Jobs First, the Los Angeles Jobs First GAIN, the New Brunswick and British Columbia sites of the Canadian Self-Sufficiency Project, and the Atlanta, GA, Grand Rapids, MI, and Riverside, CA sites of the National Evaluation of Welfare to Work Strategies. n=18,471 children aged 2-15 at the time of random assignment.	IV models use random assignment as instruments for income, welfare receipt, and employment to estimate impact of income on children's cognitive achievement. Included dummies for sites in both stages of the regression analysis. Cognitive achievement measured with test scores and parent/teacher reports. Controlled for various baseline background family characteristics.	A $1000 increase in annual income raises cognitive achievement by 0.06 of SD for children aged 2-5. No statistically significant effects of income on children aged 6-9 or 10-15. A 3-SD increase in the proportion of quarters that welfare is received leads to a 1.5 SD decrease in cognitive achievement among 10-15 year-olds.
Who Benefits from Child Benefit? (Blow, Walker and Zhu (2005)). Data from UK Family Expenditure Survey 1980-2000. n = 9811 two-parent households; n = 2920 one-parent households.	Identification from variation in real value of Child Benefit (CB) 1980-2000 due to inflation and policy changes. Calculated anticipated and unanticipated changes in CB payments. Examine the propensity to spend CB on child goods (children's clothing) and adult goods (alcohol, tobacco and adult clothing).	**Spending out of unanticipated vs. anticipated change in CB:** 15x greater for alcohol in 2-parent households; 10x greater for adult clothing in lone-parent households. Results suggest that parents fully insure children against income shocks so that unanticipated changes only affect spending on adult goods.
Effects of EITC on birth and infant health outcomes. (Baker (2008)). U.S. Vital Statistics Natality data file 1989-96. (n=781,535). Exclude observations from 1994. Used data from 1997 March CPS to compute proportion of treatment group that is eligible for EITC.	Difference-in-Difference-in-Differences (DDD). Exploit the large expansion of EITC in 1993 (phased in 1994-96) that increased benefits to families with 2 or more children."Treatment" group=mothers giving birth to 3rd or higher child. Control 1=mothers giving birth to 1st or 2nd child. Control 2=mothers giving birth to 1st child. Used mothers with less than High school as a proxy for EITC eligibility. Effect of interest is interaction between <HS*treatment*after EITC expansion.	Birth weight: Increased by 0.4% for all women Incidence of low birth weight: Decreased by 3.7% for all No statistically significant effects on # prenatal visits or maternal smoking during pregnancy.

The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit (Dahl and Lochner (2008)). Data from NLSY on children and their mothers for 1988-2000. n = 4720 children (2527 mothers).	Identification based on EITC expansions in 1987-1993 and 1993-1997 that increased benefits for low- and middle- income.Used simulated instrumental variables (IV): IV is predicted change in EITC income. Controlled for family background and demographic variables and for time-varying state-specific policies that might affect child outcomes. Examine contemporaneous and lagged income. Also estimate OLS and child fixed effects models.	A $1000 increase in income raises combined math and reading scores by .06 SD. Larger gains from contemporaneous income for children aged 5 to 10 than for those aged 11-15. Larger gains for children from disadvantaged families.
Do Child Tax Benefits Affect the Well-being of Children? Evidence from Canadian Child Benefit Expansions (Milligan and Stabile (2008)). Data from Survey of Labor and Income Dynamics on families with children for 1999-2004 for benefit simulation. Data from Canadian National Longitudinal Study of Children & Youth for 1994-2005 for children 10 and under. (n=56,000).	Instrumental variables. Used variation in child benefits across time, provinces, and number of children per family to develop a measure of benefit income as IV for child benefits in regressions of the effect of child benefits on child outcomes. Controlled for province-year fixed effects. Also reduced forms for simulated benefits' effect on child outcomes.	An increase in $1000 in simulated benefits leads to: 1.5% reduction in likelihood of learning disability (if mom < HS); 3.6% decline conduct disorder/aggression score 4-10; 4.3% of SD decline maternal depression; 11.6% SD decline in maternal depression (if mom < HS); 1.1% decline in child ever experiencing hunger (if mom < HS) Larger positive impacts on education and physical health for boys than girls; larger positive impacts on mental health for girls. Effects on math and vocabulary scores, behavioral outcomes, maternal depression and likelihood of ever experiencing hunger persist at least 4 years.
South African Child Support Grant (CSG): Unconditional cash transfer program with payments made to child's primary caregiver. Intended to cover the country's poorest 30% of children. Study of effects on child nutrition (Aguero et al. (2009)). Data from KwaZulu-Natal Income Dynamics Study. Main outcome measure: Height-for-Age z-score (HAZ). T=245, C=886.	Exploit varying lengths of exposure to CSG among children aged 0-3 due to the timing of the rollout of CSG. Fit a quadratic OLS model to application delay (number of days between program creation and child's enrollment) using data for children born <=2 years prior to survey date. Calculated an expected application delay variable for each child conditional on birth date, location and family characteristics, defined as % deviation in application delay from expected delay. Conditional on family characteristics, deviation in delay, and observables, the extent of CSG treatment should be random.Use generalized propensity scores, MLE.	No gains in HAZ for treatments covering <=20% of the 0-3 window. HAZ 0.20 higher for treatment covering 2/3 of window than for a child receiving treatment covering 1% of window. **Benefit-cost ratio of CSG:** Between 1.06 and 1.48 (estimating lifetime earnings gains from gains in HAZ, using annual 5% discount rate, and assuming unemployed 33% of time). Results robust to checking for age cohort effects and location/spacial effects.
Conditional Cash Transfer Programs (CCT)		

Mexican "Progresa" program (now "Oportunidades"): Conditional cash transfers of 20-30% of household income every 2 months. Families must take young children to health clinics, immunizations, get adequate prenatal care, & receive nutrition supplement. (Families also to keep older children in school) (Gertler (2004)). Data from survey collected from experimental villages. Sample sizes: 7,703 children underage 3 at baseline; 1,501 newborns; Height analysis: T=1,049, C=503; Anemia analysis: T=1,404, C=608.	Program phased in. 320 treatment villages and 185 control villages randomly selected. Control villages received benefits 2 years after start of program. They did not know this would be the case. Investigate 0/1 treatment dummy as well as different lengths of program exposure.	**Treatment Effects:** Newborns 25.3% less likely to be ill in past month. Children 0-3 22.3% less likely to be ill in past month. Height (12-36 mos) 1.2% higher. Probability of being anemic (12-48 mos) 25.5% less.
"Attencion a Crisis" in rural Nicaragua. Conditional cash transfer ~ 15% average per capita expenditures. Women receive payments every 2 months if preschool children taken for regular visits to health clinics. (Older children must also be enrolled in school) (Macours et al. (2008)). Sample sizes: T=3,002, C=1,019. Additional data from the 2001 Nicaragua Living Standards Measurement Study.	Randomized experimental design. 4 groups: CCT, CCT + vocational training,CCT + productive investment grant, control. Subgroup analysis by gender and age. Analyzed various transmission mechanisms by estimating expenditures on different foods (to measure nutrient intake), and differences in indicators for early childhood development.	**Treatment Effects:** Developmental screener (DDST social-personal): increase by 0.13 SDs DDST language: increase by 0.17 SDs Receptive vocabulary (TVIP): increase by 0.22 SDs. (Children made up approx. 1.5 months of delay on TVIP) **Effects by age:** DDST language: up 0.06 SDs 0-35 mos., 0.20 SDs 60-83 mos. TVIP: up 0.05 SDs 36-59 mos., up 0.36 SDs 60-83 mos. (Oldest children made up 2.4 months of delay on TVIP) No statistically significant differences by gender. Transmission mechanisms: Better diet; more likely to have books, paper, pencils;more likely to be read to; more likely to have checkup, vitamins,de-worming drugs; more likely to be enrolled; more likely to see doctor when ill.

Note: Unless otherwise noted, only results significant at 5% level are reported.
Percent changes (denoted by % instead of "pp") are reported relative to the mean, if means were reported in the paper.
T' = Treatment group, 'C' = Control group
Outcomes written as 'T>C' mean that treatment outcome greater than control outcome at 5% significance level.
Outcomes written as 'T=C' mean that there is no statistically significant difference in outcomes between 2 groups at 5% level.

Table 7: Impacts of Food Stamps on Birth and Early Childhood Outcomes

Study	**Study Design**	**Results**
Consumption responses to in-kind transfers: evidence from the introduction of the Food Stamp program (Hoynes and Schanzenbach (2009)). Data from PSID for 1968-78 and from the 1960, 1970, and 1980 decennial censuses. n=39,623 person-year obs.	Difference-in-difference using variation in county-level implementation of FSP to estimate impact of FSP on food consumption and labor supply. Controlled for county and year fixed effects as well as state linear time trends. Included trends interacted w/ pre-treatment characteristics and three measures of annual per capita county transfer payments.	Introduction of FSP is associated with: 18% increase in total food expenditures (whole sample); 26-28% increase in total food expenditures for female-headed HHs; 6-13% increase in total food exp. for non-white female-headed HHs. No significant effect on meals out and cash expenditures on food at home. Elasticity of food spending with respect to income = 0.30. MPC for food out of cash income = 0.09 (for whole sample); MPC for food out of cash income = 0.111 (<$25,000 income); MPC for food out of FSP income = 0.16 (for whole sample); MPC for food out of FSP income = 0.238 (<$25,000 income). Decrease in whether the HH head reports any work by 21%.
There's no such thing as a free lunch: Altruistic parents and the response of household food expenditures to nutrition program reforms (Bingley and Walker (2007)). Data from UK Family Expenditure Surveys for 1981-1992. n = 29,222.	Analyzed 3 nutrition programs in the UK: free school lunch for children from poor HHs, free milk to poor HHs w/ pre-school children, and free milk at day care for pre-schoolers in attendance regardless of income. For identification, exploited 1988 reform that ended eligibility for poor HHs with working parents. Difference-in-difference (DD). Also did DD using the fact that free school lunches available only during term time, and summer holidays begin earlier in Scotland.	Free school lunch reduces food expenditure by 15% of the purchase price of the lunch. Free pint of milk reduces milk expenditure by 80%.
Impact of Food Stamp Program (FSP) on birthweight, neonatal mortality, and fertility (Almond et al. (2009)). Birth and death micro data from the National Center for Health Statistics merged with county-level data for 1968-77. FSP data from USDA. County characteristics from 1960 City and County Data Book. Data on government transfers and per-capita income from REIS. Participation rates calculated using CPS. n=97,785 whites; 27,274 blacks.	Difference-in-difference, using the fact that FSP was introduced to different counties at different times due to available funding and policy changes. Key policy/treatment variable is the month and year that each county implemented FSP. Estimated the impact of FSP on county-level birth outcomes, using county and time fixed effects. Main outcomes concerned with availability of FSP during 3rd trimester of pregnancy.	**IIntroduction of FSP in 3rd trimester led to:** 0.06-0.08% (0.1-0.2%) increase in birth weight for whites (blacks); a 1% (0.7-1.5%) decrease in fraction of low birth weight for whites (blacks). Insignificant impacts of exposure to FSP during earlier trimesters. Results robust to adding fixed-effect and other controls, as well as various time trends to the analysis. Results robust to conducting an event study analysis.

Effects of FSP benefits on weight gained by expectant mothers (Baum (2008)). Data from the NLSY. Limited to low-income black and Hispanic women w/ pregnancy information in the surveys. n=1477 pregnancy-level obs.	Random effects models using Heckman and Singer method to model unobserved heterogeneity. Dependent variable is whether women gain correct amount of weight during pregnancy based on pre-pregnancy BMI. Assume state variation in FSP eligibility rules, and program administration affects FSP takeup but not weight gain. Control for gestation, pre-pregnancy FSP, WIC.	Increasing average monthly FSP benefits from $0 to $100 decreases probability of gaining too little weight by 11.8-13.7%. No effect on probability of gaining too much weight. No statistically significant difference in effects of FSP on weight gain between first-time and non-first-time mothers.
Impact of FSP on birth outcomes in California (Currie and Moretti (2008)). Data on FSP from state records and REIS. Data from birth records in CA for 1960-74. Aggregated data into cells defined using county, race, year of birth, maternal age group, parity, and the third of the year. n=38,475 cells.	Difference-in-difference using county-level variation in timing of FSP introduction. FSP measured using dummy (=1 if FSP introduced), log expenditures,or log participation. FSP dummy refers to 9 months prior to birth. County fixed effects and county-specific time trends included. Examined teenage mothers and LA county separately.	FSP introduction led to a 10% increase in number of first births to white teen mothers (only in Los Angeles); a 24% increase in number of first births to black teen mothers; a 12% increase in number of first births to all blacks; a 0.1% increase in probability infant 1500-2000g survives for whites; a 4% decrease in probability infant <3000g survives for blacks; a 4% increase in probability of low birth weight among white teens.

Note: Unless otherwise noted, all reported results are statistically significant at 5% level.
Percent changes (denoted by % instead of "pp") are reported relative to the mean.

Table 8: Effects of Housing and Neighborhoods on Child Outcomes

Study and Data	Study Design	Results
Housing and Child Outcomes		
Are Public Housing Projects Good for Kids? (Currie and Yelowitz (2000) Data from SIPP for 1992-1993, March CPS for 1990-1995, and US Census for 1990 on families w/ 2 children (under 18) and income <$50,000. n=279,129	Two-sample instrumental variable (TSIV) of the effects of living in public housing projects on child's education, housing conditions. Outcome variables are in SIPP and Census. Endogenous regressor is in CPS. Instrument is a dummy for siblings being of different sexes since families with different sex children get larger apartments in public housing than families with same sex children. Controlled for per-capita availability of projects, vouchers, Section 8 subsidies, as well as other neighborhood and family background characteristics.	First stage results**:** Having siblings of different sex increases likelihood of family living inproject by 24%. Second stage results**:** Families who live in projects are 16pp (mean = .04)l ess likely to be overcrowded; and 12pp (mean =.02) less likely to live in high-density housing. Children who live in projects are 111% less likely to have been held back in school (larger effects for boys than for girls). Black children who live in projects are 19% less likely to have been held back in school. No statistically significant effect for white children.
The Long-Term Effects of Public Housing on Self-Sufficiency (Newman and Harkness (2002)). Data from PSID - Assisted Housing Database on cohorts born b/n 1957 and 1967. Sample limited to individuals whose families were eligible for public housing b/n ages 10 and 16. n=1183. Adult outcomes measured at ages 20-27.	Amemiya generalized least squares regression where the instrument was the vector of residuals from a regression for the number of public housing units per eligible family in the area on demographic characteristics of the area. Instrumented for whether child lived in public housing to estimate impact on various adult outcomes. Controlled for numerous background characteristics and state and year fixed effects.	Living in public housing as a child leads to an increase in the probability of any employment b/n ages 25 and 27 of 7.8%; an increase in annual earnings b/n ages 20 and 27 of 14.3%; an increase in the number of years not on welfare b/n ages 20 and 27 of 11.3%. No statistically sigificant effect on household earnings relative to the poverty line. Note: above results significant at 6% level.
Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago (Jacob (2004)). Administrative data from the Chicago Housing Authority and Chicago Public Schools for 1991-2002 on students who lived in high-rise public housing for at least one semester. n=10,556	Instrumental variables regression of the effect of living in a public housing project on student outcomes. Identification from variation in timing of housing demolitions in Chicago in the 1990s. Instrumented for living in public housing with a dummy for whether the student lived in a unit scheduled for demolition at the time of the closure announcement. Controls for background characteristics and project and year fixed effects.	No statistically significant effect of living in high-rise projects on student outcomes. Living in a building that was demolished leads to an 8.2% increase in probability that students>14 yrs drop out of school within 3 years relative to those that lived in buildings scheduled for demolition that had not yet been demolished (12% more likely for girls; 4% more likely for boys).

Public Housing, Health and Health Behaviors: Is There a Connection? (Fertig and Reingold (2007)). Data from the Fragile Families and Child Well-Being Studyon children born in 20 U.S cities between 1998 and 2000. Sample 1: T=422, C=2,055; Sample 2: T=323, C=1,999; Sample 3: T=150, C=919.	Instrumental variables regression.Three instruments: gender composition of household, the supply of public housing in each city, and length of waiting list. Three subsamples. "Control" group is mothers who have below 80% of median income in their area but are not in public housing. "Treatment 1" is mothers who live in public housing at time of first interview immediately after childbirth. "Treatment 2" is mothers who have moved into public housing between childbirth and second interview at child's age 1 ("move-in" subsample)."Treatment 3" is the "move-in" subsample limited to mothers with two or three children at second interview. Controlled for family, background, neighborhood, and other demographic and socio-economic characteristics.	Note: only reporting IV estimates with complete combination of instruments here. **Effects of moving into public housing between childbirth and one-year interview:** 1.63 point increase in mother's health index (5 point scale, mean not reported, can't calculate effect size) 55pp decrease in likelihood that mother has limiting health condition (mean=.08) 41pp decrease in likelihood that mother is underweight (mean=.07) 18% increase in mother's BMI Decrease in domestic violence (imprecise coefficient estimate) No statistically significant effects on child's birth weight or PPVT scores.
Neighborhood Characteristics and Child Outcomes		
The Long-Run Consequences of Living in a Poor Neighborhood (Oreopoulos (2003)). Data from the Intergenerational Income Data base for 1978-1999 (taken from income tax files) on individuals living in Toronto, born b/n 1963 and 1970. n=4,060	Among families who apply for housing projects, assignment to a particular project is approximately random - based on 1st available unit. Compared housing-project means of various adult outcomes across neighborhood quality (measured by density of housing, total size of project, proportion of the census tract below the low-income cutoff, and whether the project is all high-rises).	The quality of housing project or neighborhood has no statistically significant impact on total income, annual earnings, or number of years on welfare in adulthood. Family characteristics account for up to 30% of total variation in adult outcomes.

Neighborhood Effects on Crime for Female and MaleYouth: Evidence from a Randomized Housing Voucher Experiment (Kling, Ludwig and Katz (2005)) Data from Moving to Opportunities (MTO) experiment. Families living in public housing projects in Boston, Los Angeles, NewYork City, Baltimore, and Chicago were randomly chosen to stay in their current home, to receive a section 8 housing voucher, or to receive a section 8 housing voucher restricted to use in neighborhoods w/ poverty rate less than 10%. Surveys conducted in 2002. Data on youths aged 15-25. Data on arrest records for MTO states. n=4,475.	OLS regression of crime outcomes on dummy for whether family used a voucher (instrumented by whether family was assigned to treatment group) or on voucher type, and on an age-treatment interaction variable. Instrument necessary since not all families who were assigned a voucher chose to use it - some remained in their initial housing.	Assignment to a restricted voucher leads to a 31% decrease in violent crime arrests for female youths. Use of restricted voucher leads to a 76% reduction in violent crime arrests among female youths. No statistically significant effects on males for violent crime arrests. Assignment to a restricted voucher leads to a 33% decrease in property crime arrests for female youths. Use of restricted voucher leads to a 85% decrease in property crime arrests forfemale youths. Assignment to a restricted voucher leads to a 32% increase in property crime arrests for male youths. Use of restricted voucher leads to a 77% increase in property crime arrests for male youths.
Neighborhoods and Academic Achievement (Sanbonmatsu et al. (2006)) Data from MTO experiment surveys for 2002 (see above) on youths aged 6-20. n=5,074	Same regression as Kling, Ludwig, and Katz (2005) for educational outcomes.	Children in families assigned to a restricted voucher attend better quality schools: the mean rank of schools on state exams is 30% greater than the control group mean, and the mean proportion of school-lunch-eligible children is 8% lower. No statistically significant effects of either voucher on child educational outcomes (school attendance, whether child does homework, child's behavior in class, whether child attended class for gifted students or class for special help, and Woodcock-Johnson Revised reading and math test scores).

Close Neighborhoods Matter: Neighborhood Effects on Early Performance at School (Goux and Maurin (2005)) Data from the French Labor Force Survey for 1991-2002 on 16-year-old youths. n=13,116.	Authors assume that a child's birthdate will impact his/her own educational outcomes, but not the outcomes of his/her neighbors. (Proportion of 15-yr-olds held back a grade is 15pp higher among those born at the end of the year relative to those born at the beginning of the year). IV regression, where first stages are: regression of being held back at age 15 on timing of birthday and regression of proportion of neighborhood youths held back at age 15 on their birthdays. Second stage is regression for being held back at age 16 on having been held back at age 15 and proportion of neighborhood youths held back at age 15. Controlled for year fixed effects, gender, and nationality.	1 SD increase in the proportion of neighboring youth that were held back in school at age 15 increases the probability of being held back at age 16 by 0.2 SD.
Experimental Analysis of Neighborhood Effects (Kling, Liebman, and Katz (2007)). Data from MTO experiment surveys for 2002 (see above) on youthsaged 15-20. n=1,807	Same regression as Kling, Ludwig, and Katz (2005) for health and behavior outcomes.	Assignment to an unrestricted voucher leads to a decrease in likelihood of experiencing anxiety symptoms by 62%/91% among females/males; a decrease in marijuana use by 44% among females; an increase in the likelihood of non-sports injury by 130% among males; an increase in incidence of smoking by 121% among males. Assignment to restricted voucher leads to a decrease in likelihood of experiencing psychological distress by 100% among females; a decrease in likelihood of experiencing anxiety by 57% among females; a decrease in marijuana use by 50% among females; an increase in likelihood of non-sports injury by 140% among males; an increase in incidence of smoking by 82% among males.

Note: Unless otherwise noted, only results significant at 5% level are reported here.

Table 9: Randomized Trials of Home Visiting Programs

Study/Program Name	**Data, Program Description, and Study Design**	**Results**
The Comprehensive Child Development Program (CCDP) (St. Pierre and Layzer (1999))	Biweekly visits starting 0-1, ending at 5 years. Population Served: 43% African-American, 26% Hispanic; all below poverty. Background of Home Visitors: paraprofessionals Sample Sizes: T=2,213, C=2,197 Evaluation Sites: 21 sites throughout the US Age of children at last follow-up: 5 years old	Developmental Checklist: T=57.93, C=57.51 Found no significant effects on wide range of outcomes including: Development and Behavior scores, medical care, mortality, HOME scores, maternal depression, welfare use, maternal income, education or employment, maternal substance use. Total cost per participant: $37,488 Total benefit per participant: $91. Net present value **= -$37,397.**
Healthy Start (Duggan, McFarlane, Windham, et al. (1999)) (Duggan et al. (2004)) (Harding et al. (2007)) (DuMont et al. (2006))	Weekly visits, fading to quarterly Age of Participation: Birth to 5 years. Population Served: Low-income, at-risk families of newborns recruited through an HSP screening and assessment protocol. All English-speaking. Background of Home Visitors: paraprofessionals. Sample Sizes: Alaska: T=179, C=185; Hawaii: T=373, C=270, 6 sites; Virginia: T=422, C=197, 2 sites. 19 additional sites discussed in Harding et al. (2007). Age of children at last follow-up: 2 years old (3 in San Diego)	Some positive effects on parenting practices and negative effects on domestic violence in some sites. E.g. Hawaii partner violence: T=16%, C=24%. Less corporal/verbal punishment T<C (odds ratio .59). Health effects in some sites but not others, e.g. VA: birth complications T=.2, C=.48; New York: low birth weight T=3.3%, C=8.3%; maternal depression T=23%, C=38%. Increases in child Bayley Scale for Infant Development in San Diego, Arkansas. Increases in maternal education and decreases in serious child abuse only in New York (T 12.5% less than C). All sites tested for a wide range of possible effects, with generally insignificant effects on other measures of child well being and child abuse.
The Nurse-Family Partnership Program (Olds, Henderson, Cole et al. (1998))	Weekly visits, fading to monthly, prenatal to 2 yrs. Population Served: disadvantaged first-time mothers less than 30 weeks pregnant (62% unmarried, 47% teenage, 23% poor, unmarried and teenage). Background of Home Visitors: Nurses. Sample Sizes: C1=90, C2=94, T3=100, T4=116 (see below for description of groups) Evaluation Site: Elmira, New York. C1 = screening; C2 = screening & transportation; T3 = screening, transportation, & prenatal home visits; T4 = screening, transportation, prenatal and postnatal home visits. Prenatal analysis: T=T3+T4, C=C1+C2; Postnatal analysis: T=T4, C=C1+C2 Age of children at last follow-up: 15 years old.	Preterm births for women who smoked more than 4 cigarettes per day: T=2.08%, C=9.81% (mothers also less likely to smoke during pregnancy, better nutrition during pregnancy, more likely to use WIC). For children: fewer emergency room visits at 0-12, 12-24 months. AT 15-YR FOLLOW-UP Mother's number of months receiving AFDC: T4=60.4, C=90.3; Mother's substance use impairments: T4=0.41, C=0.73; Mother's arrests: T4=0.18, C=0.58; Convictions: T4=0.06, C=0.28; Substantiated reports of child abuse and neglect, 0 to 15 yrs: T4=0.29, C=0.54; Child's incidence of arrests: T4=0.20, C=0.45; Child's convictions and probation violations: T4=0.09, C=0.47; Child's number of sex partners: T4=0.92, C=2.48; Child's number of days drank alcohol: T4=1.09, C=2.49. COST-BENEFIT ANALYSIS Total Cost per child: $10,300 ; Total Benefit per child: $30,000. **Net Present Value = +$19,700.** Most benefits due to reduced crime on part of the child and reductions in child abuse on part of parent.

The Nurse-Family Partnership Program (Olds, Kitzman, Hanks et al. (2007))	Weekly visits, fading to monthly, prenatal to 2 yrs. Population Served: first-time mothers less than 29 weeks pregnant and at least 2 of the following: unmarried, less than 12 yrs of education, or unemployed 92% African-American, 98% unmarried, 64% 18 yrs of age or younger, 85% at or below poverty level. Background of Home Visitors: nurses. Sample Sizes: C1=166, C2=515, T3=230, T4=228 (see below for description of groups). Evaluation Sites: Memphis, Tennessee. C1 = transportation; C2 = screening & transportation; T3 = screening, transportation, prenatal home visits, one visit postpartum in hospital, one postpartum visit at home; T4 = T3 plus home visits through child's 2nd birthday. Prenatal analysis: T=T3+T4, C=C1+C2; Postnatal analysis: T=T4, C=C2. Age of children at last follow-up: 9 years old.	During first 2 years of child's life: Number of health encounters for injuries/ingestions: T=0.43, C=0.56 Number of outpatient visits for injuries/ingestions: T=0.11, C=0.20 Number of days hospitalized for injuries/ingestions: T=0.04, C=0.18 Mother attempted breast-feeding: T=26%, C=16% Subsequent live births: T=22%, C=31% **AT 9-YR FOLLOW-UP** Child GPA (low-resource only): T=2.68, C=2.44 Reading and Math Achievement (low-resource only): T=44.89, C=35.72 Mother's # months with current partner: T=51.89, C=44.48 Number of months on AFDC/TANF per year: T=5.21, C=5.92 Number of months on food stamps per year: T=6.98, C=7.80 Maternal mastery: T=101.03, C=99.50 No. of months with employed partner: T=46.04, C=48.43 No significant effects on ER visits for injuries in first 2 years, health at birth, use medical care, maternal health.
Early Start (Fegusson et al. (2005))	Weekly visits for 1st month, then varying Age of Participation: prenatal to 3 years. Population Served: families recruited through the same screening process as in Hawaii Healthy Start. Background of Home Visitors: "family support workers" with nursing or social work qualifications. Sample Sizes: T=220, C=223. 1 site in New Zealand. Age of children at last follow-up: 3 years old.	Average number of doctor's visits 0-36 mo: T>C by 0.24 SD. Percentage of up-to-date well-child checks 0-36 mo: T>C by 0.25 SD. Percentage enrolled w/ dentist 0-36 mo: T>C by 0.20 SD. Percentage attended hospital for accident/injury or accidental poisoning 0-36 mo: T<C by 0.22 SD. Mean duration of early childhood education: T>C by 0.22 SD. Mean number of community service contacts: T>C by 0.31 SD. Positive parenting attitudes score: T>C by 0.26 SD. Nonpunitive parenting attitudes score: T>C by 0.22 SD. Overall parenting score: T>C by 0.27 SD. Percentage of parental report of severe physical assault: T<C by 0.26 SD. Child internalizing (negative) behavior score*: T<C by 0.26 SD. Child total negative behavior score*: T<C by 0.24 SD.

Effectiveness of Home Visitation by Public Health Nurses in Prevention of the Recurrence of Child Physical Abuse and Neglect: A Randomized Controlled Trial (MacMillan et al. (2005))	Weekly visits for first 6 months, then biweekly Age of Participation: Entry: 0-13 yrs old, program lasted 3 years. Population Served: parents who have a reported episode of physical abuse or neglect in the 3 months prior to joining program. Background of Home Visitors: public health nurses. Sample Sizes: T=73, C=66. 1 site in Hamilton, Canada. Note: Control group received standard services from the child protection agency CPA). Treatment group also received the standard services in addition to the home visiting. Age of children at last follow-up: Varied (3-year follow-up).	Recurrence of physical abuse or neglect based on hospital records: T=24%, C=11% Also tested, but found no statistically significant effects on: Recurrence of abuse or neglect based on CPA records; HOME score; child's behavioral, anxiety, attention problems, aggression or conduct disorder scores; parenting behavior scores or CAP score
Economic Evaluation of an Intensive Home Visiting Programme for Vulnerable Families: A Cost- Effectiveness Analysis of a Public Health Intervention (McIntosh et al (2009))	Weekly visits Age of Participation: prenatal to 18 months old. Population Served: Pregnant women identified by community midwives as being at risk for child abuse or neglect. Background of Home Visitors: paraprofessionals who received program training . Sample Sizes: T=67, C=64. 1 site in the UK. Age of Children at Last Follow-up: 1 year old	Maternal sensitivity (CARE Index): T>C by 13% Infant cooperativeness (CARE Index): T>C by 18% Likelihood infant placed on child protection register: T>C (1.35 times more likely) Proportion of children removed from home: T=6%, C=0%

Notes: Some cost-benefit analysis from Aos *et al.* (2004), Technical Appendix.

T' refers to treatment group, 'C' refers to control group. 'T=C' means no discernable difference between groups at 5% significance level.

Table 10: Selected Studies of Special Supplemental Feeding Progr

Study	Study Design	Results
IMPACT ON BIRTH OUTCOMES		
Bitler and Currie (2005). Data from Pregnancy Risk Assessment Monitoring System (PRAMS), (n=60,731).	Compared WIC participants and non-participants in the sample of women whose deliveries were paid by Medicaid. Addressed selection bias by comparing a broad range of observable characteristics between eligible WIC participants and non-participants.	WIC mothers are negatively selected into the program relative to all Medicaid recipients. WIC participants are 1.4-1.5 times more likely to have had prenatal care in 1st trimester; 0.7 times as likely to give birth to low birth weight infant; 0.9 times as likely to give birth to infants who are below the 25th percentile of birth weight given gestational age; 0.9 times as likely to have their infant admitted to the Intensive Care Unit. WIC associated with increases in maternal weight gain, gestation, and birth weight. Larger impact for more disadvantaged women (such as those who received public assistance, high school drop-outs, teen mothers, single mothers).
Figlio *et al.* (2009). Matched data on births in Florida during 1997-2001 with school records of older siblings of the infants to identify whether the older child receives free lunch or reduced-price lunch though the NSLP (those on NSLP are WIC eligible). Marginally Eligible: n=2,530; Marginally Ineligible: n=1,744	Instrumental variables comparing marginally eligible and ineligible WIC women. Marginally ineligible for WIC=families not participting in NLSP during pregnancy, but participating during either the year before or after. Marginally eligible=families that received NSLP during the pregnancy, but did not receive the lunches at least one adjacent year. A federal policy change that increased income reporting requirements for WIC eligibility in September 1999, made it more difficult for eligibles to obtain WIC. The instrument for WIC participation is an interaction between an indicator for the policy change and eligibility.	WIC participation reduces the likelihood of low birth weight by 12.9pp (imprecise estimate). No statistically significant effect of WIC on gestational age or likelihood of prematurity.
Gueorguieva, Morse, and Roth (2009). Data on mother-infant pairs from birth files on all singleton births in Florida hospitals between 1996 and 2004. Merged with Medicaid eligibility and WIC participation data from the Florida Department of Health. (n=369,535)	Adjusted for selection using propensity scores. "Treatment" is percent days on WIC during pregnancy. Main outcome is SGA ("small for gestational age"). Separate analyses for full-term, late preterm, very preterm, and extremely preterm births.	A 10% increase in percent of time during pregnancy in WIC associated with a 2.5% decrease in probability of a full-term and SGA infant; 2.0% decrease in probability of a late preterm and SGA infant; 3.7% decrease in probability of a very preterm and SGA infant.

Joyce, Gibson, and Colman (2004). Data from birth certificate files in New York City between 1988 and 2001 on women who were on Medicaid and/or WIC during pregnancy. (n=35,415 in 1988-1990; n=50,659 in 1994-1996; n=52,608 in 1999-2001)	Multivariate regression with dummies for WIC participation, interacted with year of birth. To address selection bias, limited analysis to women with first births who initiated prenatal care in first 4 months of pregnancy. Estimated separate models by race, ethnicity, nativity, and parity. Estimated same model comparing twin births.	Among US-born blacks, WIC participants are 2.4pp less likely to experience a low birth weight birth than non-WIC participants in 1988-1991. No statistically significant effects of WIC participation for foreign-born Hispanic women. In twin analysis, for US-born blacks, WIC participation is associated with a 55 g increase in birthweight adjusted for gestation, and a 3.9pp decrease in SGA rates. Effects biggest for US-born black women under age 25. (Note, means for subgroups not reported, so effect sizes can't be calculated).
Joyce, Yunzal-Butler and Racine (2008). Pregancy Nutrition Surveilance System (PNSS) data (n=2,870,031). Included all women who were enrolled in WIC during pregnancy and re-enrolled postpartum. Comparison group is women who enrolled in WIC after delivery but were not exposed to WIC during pregnancy.	Multivariate regression with dummies for WIC in each trimester. To deal with selection bias, estimated separate models by race/ethnicity. Also analyzed subgroups whose pre-pregnancy characteristics put women at high risk for anemia, low weight gain, and intrauterine growth retardation. Finally, analyzed subgroup of first-births.	Conditional on gestational age, mean birth weight is 40g greater among prenatal enrollees than post-partum enrollees. Rates of SGA are 1.7pp (14%) less, and rates of term low birth weight are 0.7pp (30%) less. Difference between 1st and 3rd trimester enrollees in mean birth weight is 13.5g.
Kowaleski-Jones and Duncan (2002). NLSY Mother-Child data. 2,000 children 1990-96. 104 sibling pairs, 71 pairs in which one child participated and one didn't.	Sibling fixed effects.	Increase of 7 ounces in mean birthweight. Positive effect on temperament score. No effect on social or motor skills test scores.
Hoynes, Page, and Stevens (2009). Data on county-level WIC availability in 1971-1975 and 1978-1982 from lists of local agencies that provided WIC services. Data on birth weight from vital statistics records. Data on population of women by county-year from the CANCER-SEER population data. Data on various control variables from 1970 IPUMS. (n=18,517 county-year cells)	Difference-in-difference, using variation in the timing of WIC implementation across different counties in 1972-1979, to estimate impact of WIC availability on birth weight. Controlled for three measures of per capita government transfers, an indicator for Food Stamp program availability, other demographics, and county and year fixed effects, and state-year fixed effects. Scaled estimates by WIC participation rates. Also conducted sub-group analysis in county-year-maternal education level cells.	If WIC is available in a county by the third trimester, average birth weight increases by 0.1% (estimate not scaled by participation rate). Among women with low levels of education, WIC increases average birth weight by 10% (esimate scaled by participation rate) and reduces the fraction of births classified as low birth weight by 11% (estimate scaled by participation rate). No effects of WIC on fertility - so results not driven by selection into birth.
IMPACT ON BREAST FEEDING AND INFANT FEEDING PRACTICES		
Chatterji *et al.* (2002). NLSY Mother-Child file. 1,282 children born 1991-95. 970 siblings born 1989-95.	IV with WIC state program characteristics as instruments. Sibling fixed effects.	OLS and IV indicate WIC reduces breastfeeding initiation, but no effect on duration. Fixed effect suggests reductions in length breastfeeding.

IMPACT ON NUTRITION AND HEALTH OUTCOMES OF CHILDREN		
Black *et al.* (2004). Data from surveys administered by The Children's Sentinel Nutritional Assessment Program in a multi-site study at urban medical centers in 5 states and Washington, DC in 1998-2001. (n=5,923: 5,395 received WIC assistance, 528 did not).	Compared WIC-eligible families who participated in WIC with those who did not due to self-reported access problems. Multivariate regression including a participation dummy, controlling for relevant background characteristics.	Compared to infants who received WIC assistance, those who did not receive WIC assistance were more likely to be underweight (weight-for-age z-score = -0.23 vs. 0.009), short (length-for-age z-score = -0.23 vs. -0.02), and perceived as having fair or poor health (adjusted odds ratio = 1.92). No statistically significant differences in rates of food insecurity.
Lee and Mackey- Bilaver (2007). Data from IL Integrated Database on Children's Services. Includes info on Medicaid, FSP, WIC enrollment. All IL children born between 1990 and 1996 who entered Medicaid within first month. Tracked to 2001. Total sample=252,246. sibling fe sample=36,277.	Multivariate regression with sibling fixed-effects, using participation dummies for FSP, WIC, and FSP-WIC jointly. (Note, sibling FE only make sense for WIC, effects of FSP estimated using OLS. Also, most families on WIC also receive FSP).	Effect of WIC: Abuse or neglect rates decrease by 84% (mean=.10). Incidence of anemia decrease by 41% (mean =.195). Failure to thrive decrease by 78% (mean=.128). Nutritional deficiency decrease by 115% (mean=.038).

Note: Unless otherwise noted, only results significant at 5% level are reported here.
Percent changes (denoted by % instead of "pp") are reported relative to the mean.

Table 11: Selected Recent Evaluations of Early Childhood Programs with Randomized Designs

Study/Program Name[a]	Data, Program Description, and Study Design	Results
hb	Preschoolers: full-day child care School age: parent program Sample sizes: Initial: T=57, C=54 Age 8: T=48, C=42 Age 15: T=48, C=44 Age 21: T=53, C=51 Age of participation in program: Entry: 6 weeks to 3 months old Exit: 5 to 8 years	**Follow-up at 21 years of age:** Grade retention: T = 34%, C = 65%, age 21 Special education: T = 31%, C = 49%, age 21 High school dropout: T = 33%, C = 49%, age 21 College attendance: T = 36%, C = 13%, age 21 Crime rate: T=C, age 21 Employment status: T=C at age 21 Average age first child born: T>C at age 21 **Cost/Benefit Analysis:** (using 5% discount rate, $2002) Net cost per child = $34,599 Net Benefit of program = $72,591 per participant
Infant Health and Development Project Follow-up at 18 years of age. (McCormick et al (2006)).	Home visits, full-day child care Sample sizes: Initial: T=377, C=608 Followup at age 8: T=336, C=538 Followup at age 18: T=254, C=381 (divided in 2 groups: lighter low birth weight (LLBW) and heavier low birth weight (HLBW)) Entry: birth (home visits), 1 year (care). Exit: 3 years.	Math achievement: T>C by 6.8%, age 18 HLBW Reading achievement: T>C by 5.6%, age 18 HLBW Risky behaviors: T>C by 23.3%, age 18 HLBW IQ: T=C, age 18 HLBW *Note: For all outcomes: T=C, age 18 LLBW
A Reevaluation of Early Childhood Intervention -- Abecedarian, Perry Preschool and Early Training Project -- With Emphasis on Gender Differences and Multiple Inference. (Anderson 2008).	Abecedarian: T=57, C=54 Perry: T=58, C=65 ETP: T=44, C=21 Ages of entry: Abecedarian/Perry/ETP: 4.4 mo./3 yrs./3-4 yrs.	Outcomes include: IQ, grade repetition, special ed., high school, college attendance, employment, earnings, receipt transfers, arrests, convictions, durg use, tenn pregnancy, marriage. Summary index pools multiple outcomes for a single test. Separate tests by gender. Effects on summary index for girls 5-12: ABC/Perry: increase by 0.45/0.54 SDs Effects on summary index for girls 13-19: ABC/Perry/ETP: increase by 0.42/.061/0.46 SDs Effects on summary index for women over 21-40: ABC/Perry: increase by 0.45/0.36 SDs. No statistically significant effects on males of any age

High/Scope Perry Preschool Project Follow-up and cost-benefit analysis at 40 years of age (Barnett et al (2006))	Home visits, preschool program Sample sizes: Initial: T=58, C=65 Age 40: T=56, C=63 Age of participation in program: Entry: 3 to 4 years, Exit: 5 years.	Arrests: T=32%, C=48%; In jail: T=6%, C=17%. Report of stopping work for health reasons: T=43%, C=55% Hard drug use: T=22%, C=29%; Abortions: T=17%, C=32%. **Cost-Benefit Analysis:** Main result: Benefit of $12.90 for each $1 cost Most benefits due to reduced crime rates for males. Cost: $15,827 ($2000) per student. Total Net Private Benefit = $17,730 per participant. Total Net Public Benefit = $180,455 per participant.
National Evaluation of Early Head Start (Administration on Children, Youth and Families (2002) & Love et al. (2005)) Cost-Benefit (Aos et al. (2004)).	17 Early Head Start sites selected to reflect EHS programs funded in 1995-96. Random assignment within site (possible given wait lists). Sample: T=1513, C=1488. Age of participation in program: Entry at 0-1 year, exit 3 years.	Mental Development Index (MDI): T>C by 0.12 SD PPVT-III Vocabulary score: T>C by 0.13 SD Percentage with PPVT score <85 pts: T<C by 0.12 SD Aggressive behavior: T<C by 0.11 SD Supportiveness during parent-child play: T>C by 0.15 SD HOME score: T>C by 0.11 SD Index of severity of discipline: T<C by 0.11 SD No statistically significant effects on parental mental or physical health or on measures of family functioning. **Cost-Benefit Analysis:** Total Cost per child: $20,972 Total Benefit per child: $4,768 **NPV: -$16,203**
Head Start Impact Study (U.S. Department of Health and Human Services (2010))	Congressionally-mandated study of Head Start. Children from wait lists randomly assigned to one of 383 randomly selected Head Start centers across 23 different states. Baseline data collected in fall 2002; annual spring follow-ups through spring 2006. Sample Sizes: T=2783, C=1884. Entry: 3-4 years old; Exit: 4-5 years old.	Summary of Effects for 4-year-old entry cohort at end of 1st grade: PPVT: T>C by 0.09 SD; Withdrawn behavior: T<C by 0.13 SD; Shy/socially reticent: T>C by 0.19 SD; Problems with teacher interaction: T>C by 0.13 SD. No statistically significant impacts at age 4, kindergarten, or 1st grade on: math scores, Spanish language tests, oral comprehension, and several parent- and teacher-reported measures of emotional and behavioral outcomes. No statistically significant impacts at kindergarten or 1st grade on: school accomplishments, promotion, language and literacy ability, math ability, and social studies and science ability. Summary of Effects for 3-year-old entry cohort as of 1st grade (selected results): Oral comprehension: T>C by 0.08 SD. No significant effect on other outcomes.

The Rates of Return to the High/Scope Perry Preschool Program (Heckman et al. (2009))	See Barnett et al. (2006) entry for sample sizes and information about the Perry Preschool Program. Randomization was compromised because of reassignment after initial randomization. Standard errors on the rate of return estimates adjusted for failure of randomization using bootstrapping and Monte Carlo methods. Also, adjusted for the deadweight loss due to taxation (assuming 0%, 50%, and 100% deadweight losses). Used a wide variety of methods for within-sample imputation of missing earnings. Used local data on costs of education, welfare participation, and crime instead of national data, wherever possible. Used several methods to extrapolate benefits and costs beyond age 40 (after last follow-up). Used several measures of the statistical cost of life to estimate costs of murder.	Estimated social rates of return to the Perry Preschool Program are 7-10%. Estimated benefit-cost ratio = 2.2 to 31.5 (depends on discount rate used, and the measure of cost of murder).

Notes:

[a]Programs are grouped such that those enrolling children younger than three years old appear first, followed by those enrolling children after age three.
Throughout the table, 'T' refers to treatment group and 'C' refers to control or comparison group.
Outcomes listed as T>C or C>T were statistically significant at the 5% level.

Table 12: Selected Studies of Large-Scale Public Early Childhood Programs

Study/Program Name and Data	Study Design	Results
EVALUATIONS OF HEAD START		
Does Head Start Make a Difference? Does Head Start Help Hispanic Children (Currie & Thomas (1995, 1999)). NLSCM. Sample size: T=896, C=911 Hispanic study: T=182, C=568 Entry: 3 to 5 years; Exit: 5 to 6 years.	Estimate sibling fixed effects models of effects of Head Start and other preschool attendance on various outcomes. Examine differences between siblings that might potentially explain differential attendance by siblings.	Achievement tests: T>C (1/3 SD whites and Hispanics only) Grade retention: T<C (~50% whites and Hispanics only) Immunization rates: T>C (8-11%) Child height-for-age: T=C
Long Term Effects of Head Start (Garces et al (2002)). PSID, Sample size: T=583, C=3502 Entry: 3 to 4 years; Exit 5 to 6 years.	Compared Head Start participants to their own siblings who did not participant. Outcomes measured between ages 18 and 31. Retrospective reports on Head Start participation.	High school graduation: T>C (~25% for whites only) Arrests T<C (~50% for African-Americans only) College T>C (~25% for Whites) Teen pregnancy T=C Welfare T=C
Effect of Head Start on health and schooling (Ludwig and Miller (2005)) Vital Statistics Compressed Mortality Files 1973-83; Individual data from NELS, where T = 649, C = 674.	Regression discontinuity around cutoff at which counties were eligible for assistance in applying for Head Start in 1965. T = 300 poorest counties in 1965 C = 301-600th poorest counties 80% of treatment counties received funding vs. 43% of all counties nationwide.	Effects of Participation in Head Start: Mortality, age 5-9: T<C by 35-79% High school completion rates: T>C by 5.2-8.5% Some college+: 16.2-22.4% for oldest cohort only
Head Start Participation and Childhood Obesity (Frisvold (2006)). PSID Child Development Supplement. Sample size=1332.	Estimated the effect of Head Start on likelihood of a child being overweight or obese. Assume that # of spaces available in a community is a valid instrument for Head Start participation..	Head Start reduces probability of obesity at ages 5-10 among blacks. No effect in full sample of children or in chidlren over 10. Estimates are large relative to sample means implying ~100% reductions in overweight/obesity.

Evidence from Head Start On Lifecycle Skill Development (Deming (2009)). Data from NLSY Children for cohort enrolled in Head Start between 1984 and 1990. Children in study at least 5 years old in 1990. Sample size: 3415 total.	Sibling fixed effects estimates of benefits of Head Start.	Test scores: T>C by 0.145 SD ages 5-6, by 0.133 SD ages 7-10, by 0.055 SD ages 11-14. Noncognitive school-age outcomes index: T>C by 0.265 SD. Long-term effect on index* of young adult outcomes: T>C by 0.228 SD. Large fade-out in test scores of African Americans, none for whites or Hispanics. No effects on criminal activity. Summary: Head Start increases index of long term outcomes by 0.23 SD (~1/3 of gap attendees and others). Projecting wages implies that benefits (~$1,500 in greater earnings per year), exceed program costs of ~$6,000. * index includes: graduate high school, complete 1 yr college idle (no job, not in school), poor health.
Preventing Behavior Problems in Childhood and Adolescence: Evidence from Head Start (Carneiro and Ginja (2008)). NLSY Children data. Sample size = 1786 males. Behavior problems, grade rep. and obesity at 12-13. Depression, crime, and obesity at 16-17. Oldest in data -- born in 1974; youngest in data -- born in 1992.	Head Start eligibility rules create discontinuities in income eligibility. Compare families above and below the cutoff. Identification strategy requires that families are not able to strategically locate above or below cutoff.	**USING REDUCED FORM ESTIMATION** **Head Start participation impacts on 12-13 year-old males:** behavioral problems index decreases by 38% probability of grade retention decreases by 33.3% probability of obesity decreases by ~100% for blacks only **Head Start participation impacts on 16-17 year-old males:** Depression (CESD) decreases by 23.4% probability of obesity decreases by 57.9% probability of being sentenced decreases by >100% for blacks only **USING STRUCTURAL EQUATIONS** **Head Start participation impacts on 12-13 year-old males:** probability of grade retention decreases by >100% **Head Start participation impacts on 16-17 year-old males:** probability of being sentenced decreases by >100% probability of obesity decreases by >100% probability of being sentenced decreases by >100% for blacks only probability of obesity decreases by >100% for blacks only Note: baseline means are for sample of children with incomes between 5% and 195% of Head Start eligibility cut-off.

Investing in Health: The Long-Term Impact of Head Start on Smoking (Anderson, Foster, and Frisvold (2009)). Data from the PSID. Used smoking data from 1999 and 2003 on individuals aged 21-36 in 1999. n = 922 in 1999; n = 1005 in 2003.	Compared smoking of siblings who did and did not attend Head Start or any preschool using sibling fixed effects. Controlled for family background characteristics specific to the age children were eligible for Head Start. Examined sibling differences that might be predictive of Head Start attendance. Examined spillover effects by including interactions between Head Start and birth order.	**Results from 1999 data:** Head Start participants are 58% less likely to smoke than siblings **Results from 2003 data:** Head Start participants are 65% less likely to smoke than siblings. Including control for educational attainment makes results statistically insignificant. **Cost-Benefit:** PV reduction in smoking is $9,967 per participant (using 3% discount rate, accounting for medical expenses and productivity losses) Average cost per Head Start participant in 2003 is $7,092. Depending on discount rate used, the value of reduction in smoking is associated with 36-141% of program costs.
Expanding Exposure: Can Increasing the Daily Exposure to Head Start Reduce Childhood Obesity? (Frisvold and Lumeng (2009)) Administrative data from a Michigan Head Start for 2002-2006. n = 1833 obs. (from 1532 children, since some attend for multiple years) Full-day class sample=424 obs. Half-day class sample=1409 obs.	Estimated the effect of full-day vs. half-day Head Start on obesity at end of school year. Identification via elimination of a grant which led to a decrease in the # of full- day classes from 16 classrooms in 2002 to 4 classrooms in 2003. (IV=% full-day funded slots). Controls for observable family characteristics.	**First Stage Results:** 10pp increase in percentage of full-day slots increases likelihood of full-day attendance by 85% (relative to baseline=11% enrollment in full-day slots in 2003). **Second Stage Results:** Full-day enrollment in Head Start decreases likelihood of obesity by 143%. This implies that children who attended full-day classes would have been almost 3 lbs heavier had they attended half-day classes. Simulation results suggest that the 143% change in the likelihood of obesity can be explained by a change in caloric intake of 75 calories per day with no change in physical activity.

STUDIES OF PUBLIC PRE-K/K AND CHILD CARE PROGRAMS		
Impact of Early Childhood Care and Education on Children's Preschool Cognitive Development: Canadian Results from a Large Quasi-Experiment (Lefebvre, Merrigan, and Verstraete (2006)) Data from Canada's NLSCY on 4- and 5-yr old children from 5 consecutive cycles. n = 15,546	Identification from changes in Quebec's child care subsidies. On Sep. 1, 1997, child care facilities offered $5-per-full-day services to children who were 4 yrs old by Sep. 30th. In the following years, age cutoffs decreased and the number of spaces increased. No similar policies in other provinces of Canada in 1994-2003. Difference-in-difference (DD) and Difference-in-difference-in-difference (DDD) designs, comparing Quebec's preschool children with children of similar ages from other provinces using the fact that different cohorts of children were exposed to different numbers of treatment years.	Subsidies increase the number of hours in child care by 5.5-7.4 hours per week for children aged 4-5. Effect larger for mothers in highest educational group. No effects on 4-year-old children's PPVT scores. Decrease in PPVT scores of 0.33 SD for 5-year-old children.
Promoting School Readiness in Oklahoma: An Evaluation of Tulsa's Pre-K Program (Gormley, Jr. and Gayer (2006a). Data from Tulsa Public Schools (TPS) on test scores of Pre-K and Kindergarten children from test administered in Aug. 2001. T=1112, C=1284 (T = children who just completed Pre-K, C = children who are about to begin Pre-K). Entry: 4 years old; Exit: 5 years old.	Regression discontinuity design arising from cutoff of Sept. 1 to enroll in Pre-K in a given year. Compare kindergarden children who just completed Pre-K with slightly younger children who were ineligible to attend. Used quadratic polynomial to fit underlying age/test score relationship.	Cognitive/knowledge score: T>C by 0.39 SD Motor skills score: T>C by 0.24 SD Language score: T>C by 0.38 SD Largest impacts for Hispanics, followed by blacks, little impact for whites. Children who qualify for free school lunch have larger impacts than other children.

Does Prekindergarten Improve School Preparation and Performance? (Magnuson, Ruhm, and Waldfogel (2007)) Data from ECLS-K. n = 10,224.	Primary method is a multivariate regression to estimate the impact of Pre-K attendance on various outcomes. Robustness checks using teacher fixed effects, propensity score matching, and instrumental variables (IV). IV is different measures of access to pre-K in a given state. Dependent variables are measured in the fall of kindergarten and in the spring of first grade to assess any lasting impacts of Pre-K.	Pre-K attendance: increases reading scores at school entry by 0.86 SD (IV); increases aggression at school entry by 0.69 SD (IV). No effect on math scores or self control in IV. Effect sizes for all outcomes are larger for Pre-K than for other forms of child care, but Pre-K children have different characteristics than other children. Among children attending Pre-K in the same public school as their kindergarten, higher reading scores are not accompanied by increased behavioral problems. For disadvantaged children, cognitive gains are more lasting than in the whole sample. Effect sizes for cognitive outcomes much lower in spring of 1st grade than at school entry. Effect sizes for behavioral outcomes are the same.
The Effects of Oklahoma's Pre-K Program on Hispanic Children (Gormley (2008)). Tests administered by Tulsa public schools in Aug. 2006. T = 194, C=295. (T= children who just completed Pre-K, C=children who are about to begin Pre-K) Entry: 4 years old; Exit: 5 years old.	See Gormley, Jr. and Gayer (2006) entry.	Letter-Word Identification Test score: T>C by 0.846 SD Spelling score: T>C by 0.52 SD Applied Problems score: T>C by 0.38 SD Significant effects only for Hispanic students whose primary language at home is Spanish.
Universal Child Care, Maternal Labor Supply, and Family Well-Being (Baker, Gruber, Milligan (2008)). Canadian NLSCY (1994-2003) includes only married women and their children. Average of 2000 children at each age per yr. Primary sample ages 0-4, robustness checks ages 8-11.	Compare outcomes in Quebec, which began $5 per day daycare for 4 year olds in 1997, extended program to 3 year olds in 1998, 2 year olds in 1999, and all children <2 in 2000, to rest of Canada. Difference in differences.	Increase in use of any child care/institutional care/mothers working by 35%/>100%/14.5%. Crowding out of other informal child care. Increase in emotional disorder anxiety score (physical aggression and opposition) by 12% (9%) for 2-3 yr olds. Decrease in standardized motor and social development score by 1.7%. Increase in mother depression score by 10%. 40% of the cost of the child care subsidy are offset by increased taxes on extra labor supply.

Impacts of New Mexico Pre-K on Children's School Readiness at Kindergarten Entry: Results from the Second Year of a Growing Initiative (Hustedt et al (2008)). Data on children who participated in the 2nd year of the Pre-K program during 2006-2007 and entered kindergarten in Fall 2007. T = 405, C = 519 (T= children who just completed Pre-K, C = children who are about to begin Pre-K). Entry: 4 years old; Exit: 5 years old.	Regression discontinuity design due to a birthday eligibility cut-off of Aug. 31st to enroll in Pre-K in a given year. Compared "young" kindergarten children who just completed Pre-K with slightly younger children who are about to being Pre-K. Used linear model for vocabulary score as dependent variable, quadratic model for early literacy score as dependent variable, cubic model for math score as dependent variable.	Vocabulary (PPVT) score: T>C by 0.25 SD Math score: T>C by 0.50 SD Early literacy score: T>C by 0.59 SD No statistically significant difference in effects between Pre-K programs funded by the Public Education Department and those funded by the Children, Youth and Families Department.
An Effectiveness-Based Evaluation of Five State Pre-Kindergarten Programs (Wong et al (2008)). Data on test scores from fall 2004 from Michigan, New Jersey, Oklahoma, South Caroline and West Virginia. Sample sizes: T=485, C=386 (MI); T=1177, C=895 (NJ); T=431, C=407 (OK); T=353, C=424 (SC); T=379, C=341 (WV). (T=children who just completed Pre-K, C=children who are about to begin Pre-K) . Entry: 4 years old; Exit: 5 years old.	Regression discontinuity design due to a strict birthday eligibility cut-off. Looked at effect size differences due to programmatic variation between states. Used a polynomial approximation to the continuous function on the assignment variable in the regressions.	**Intent-to-Treat Results:** MI: T>C by 0.53 SD for math; 1.09 SD for Print Awareness. NJ: T>C by 0.36 SD for PPVT, 0.23 SD for math, 0.32 SD for Print Awareness. OK: T>C by 0.28 SD for PPVT, 0.78 SD for Print Awareness. WV: T>C by 0.92 SD for Print Awareness. **Treatment-on-Treated Results:** MI: T>C by 0.47 SD for math, 0.96 SD for Print Awareness. NJ: T>C by 0.36 SD for PPVT, 0.23 SD for math, 0.50 SD for Print Awareness. OK: T>C by 0.29 SD for PPVT. SC: T>C by 0.79 SD for Print Awareness. No statistically significant results for WV. No clear relationship between state funding for Pre-K programs and effect sizes. State Pre-K programs have larger effect sizes than Head Start.

Do Investments in Universal Early Education Pay Off? Long-term Effects of Introducing Kindergartens into Public Schools (Cascio (2009)). Data from 4 Decennial censuses for 1970, 1980, and 2000 from the Public Use Microdata Samples. n=840 whites, 425 blacks. Data from PSID on Head Start enrollment: n=174 whites, 126 blacks.	Analyzed effect of expansion of public kindergarten on long-term outcomes. Identification from the variation in the timing of funding initiatives among treated states. Event study model, comparing individuals aged 5 before and after the initiatives were implemented. Included dummies for cohorts interacted with dummies for 3 different groups of treated states defined on the basis of average education expenditure per pupil in the early 1960s. Also controlled for cohort-by- region-of-birth fixed effects and state fixed effects. Units of observation are cohort-state cells.	White children aged 5 after the typical state reform are 2.5% less likely to be high school drop-outs and 22% less likely to be institutionalized as adults. No significant effects on grade retention, college attendance, employment, or earnings. No significant effects for blacks, despite comparable increases in enrollment in public kindergartens post reform. Potential explanation is that state funding for public kindergartens reduced the likelihood that a black 5-year-old attended Head Start by ~100%. Reduction in Head Start attendance may account for 16% of the 1.13 pp increase in the black-white gap in high school drop-out rates. Difference in effects on educational attainment between whites and blacks are driven by females.

No Child Left Behind: Universal Child Care and Children's Long-Run Outcomes (Havnes and Mogstad (2009)). Data from Statistics Norway on individuals from 1967 to 2006. Household information from the Central Population Register. Administrative data on child care institutions and their locations for 1972-1996. Restricted sample to individuals whose mothers were married at the end of 1975. n = 499,026 children; 318,367 families. Adult outcomes measured between ages 30 and 33.	Difference-in-differences. Exploited a child care reform in 1975 in Norway, which assigned responsibility for child care to local governments, and thus resulted in great variation in child care coverage for children aged 3-6 both between cohorts and across municipalities. T = municipalities where child care expanded a lot; C = municipalities where child care did not change much. Compared changes in outcomes for treatment and control adults who were 3-6 years old before and after the reform. Also investigated heterogeneity of effects. Controlled for various child and family-specific characteristics, as well as municipality fixed effects. Robustness checks: included a time trend, checked for placebo effect by comparing the two groups before the reform, and used sibling fixed effects comparing siblings exposed to the reform to those who were not.	**TT Effects (ITT effects in parentheses)** One more child care place: increases educational attainment by 2.7% (0.4%) decreases probability of dropping out of HS by 22.8% (3.8%) increases probability of college enrollment by 17% (2.5%) decreases probability of having little or no earnings by 23.3% (3.9%) increases probability of having average earnings by 7.4% (1.3%) decreases probability of being on welfare by 31.9% (5.6%) decreases probability of parenthood by 10.4% (1.8%) increases probability of being single with no children by 23.3% (4%) Almost all of the reduction in probability of being a low earner is driven by females. Women more likely to delay child bearing and family formation than men as a result of increased child care. Most benefits of universal child care are for children of low-educated mothers. Subsidized formal child care crowds out informal child care with almost no net increase in total child care use or maternal employment. No impact of child care reform on maternal education. **Cost-Benefit Analysis:** Cost: Annual budgetary cost per child care place = $5400 Benefit: Increase in 0.35 yrs of education implies an increase in $27,000 in lifetime earnings.

Notes: See Barnett and Karoly et al. for more information about some of the studies described in this table. Unless otherwise noted, none of these evaluations was rand
T' refers to the treatment, and 'C' refers to the control or comparison group. T>C means that the difference was significant at the 5% level.

[a]A very similar study by Gormley Jr., Gayer, Phillips, and Dawson (2005) evaluates the effects of Oklahoma's Universal Pre-K program on school readiness using the same regression discontinuity design, but measuring outcomes with the Woodcock-Johnson Achievement Test. They find a 0.79 SD increase in the Letter-Word Identification score, a 0.64 SD increase in the Spelling score, and a 0.38 SD increase in the Applied Problems score.

Table 13: Effects of Medicaid and other Public Health Insurance on Birth and Early Childhood Outcomes

Study and Data	Study Design	Results
EFFECTS ON BIRTH WEIGHT AND HEALTH AT BIRTH		
The efficacy and cost of changes in the Medicaid eligibility of pregnant women (Currie and Gruber (1996)). Note: Authors also conduct an analysis of Medicaid take-up, which is not included in the results here. Data from CPS and Vital Statistics. Data on Medicaid expenditures from the Health Care Financing Administration. Data on the use of medical services by pregnant women from the NLSY. Simulated model for targeted changes estimated for 1979-1992. Simulated model for broad changes estimated for 1987-1992. (n=600).	Exploited variation between states in the timing of expansions of Medicaid eligibility. Use a fixed sample to simulate the fraction eligible under different state rules. Distinguish "targeted" changes affecting very low income women from "broad" changes to women further up the income distribution. Instrument the actual fraction of women eligible in each state and year with the simulated eligibility measure. Controlled for state fixed effects and time varying state characteristics.	The percentage of 15-44 yr old women eligible for Medicaid (had they become pregnant) rose from 12.4% to 43.3% b/n 1979 and 1991. A 30% increase in eligibility leads to a 1.9% decrease in incidence of low birth weight (sig. at 10% level) and a 8.5% decrease in infant mortality rate. For targeted program changes, a 30% increase in eligibility decreases low birth weight (infant mortality) by 7.8% (11.5%). For broad program changes a 30% increase in eligibility decreases low birth weight (infant mortality) by 0.2% (2.9%).
Canadian National Health Insurance and Infant Health (Hanratty (1996)). County-level panel data on infant mortality from 10 provinces in 1960- 1975 from the Census of Canada and from Canada's Division of Vital Statistics. (n = 204 counties). Data on birth weight from a sample of all birth records in Canada from 1960 to 1974.	Used variation in timing of implementation of national health insurance across provinces in Canada over 1962-72. Logit of outcomes on a dummy for having national health insurance in a particular county-year, controlling for demographic and socio-economic factors, a time trend, and year fixed effects.	Introduction of national health insurance leads to declines of 4% in infant mortality rates; 1.3% in low birth weight (whole sample); 8.9% in low birth weight (single parents). No impact on birth weight among married women.
Changes in Prenatal Care Timing and Low Birth Weight by Race and Socioeconomic Status: Implications for the Medicaid Expansions for Pregnant Women (Dubay et al. (2001)). Data on births from the 1980, 1986, and 1993 Natality Files. n = 8,100,000 births.	Difference-in-difference, subtracting difference in obstetrical outcomes (rates of late initiation of prenatal care and rates of low birth weight) b/n 1980 and 1986 from difference in outcomes b/n 1986 and 1990, within socioeconomic (SES) groups. Also compared changes in obstetrical outcomes in 1986-93 b/n women of low and high SES (since high SES women were not affected by Medicaid expansions). SES defined by marital status and years of schooling. Medicaid expansions occurred in 1986-93. Controlled for year, age of mother, parity, and age-parity interactions.	**Results from Diff-in-Diff Within SES:** Medicaid expansions associated with decreases of: 12-21% prenatal care initiation after 1st trimester for white women; 3-5% in low birth weight among white women w <12 years education; 10-13% in prenatal care initiation after 1st trimester for black women w <12 years education; 13-27% in prenatal care initiation after 1st trimester for black women w 12-15 years of education; 12-35% in prenatal care initiation after 1st trimester for black women >15 years of education. Association with a 3% increase in likelihood of low birth weight for unmarried black women with <12 years education Similar results using diff-in-diff across SES for 1986-93.
Effects of Medicaid expansions and welfare contractions on prenatal care and infant health (Currie and Grogger (2002)). Data on birth outcomes from VSDN files 1990-1996. Data on fetal deaths from vital statistics fetal deaths detailed records. Data on Medicaid administrative reforms from National Governor's Association Maternal and Child Health newsletters. Data on welfare caseloads from the US Department of Health and Human Services. n=3,985,968 whites; 4,014,935 blacks.	Logit regression that includes dummies for state-level Medicaid administrative reforms; income eligibility cutoffs; rates of welfare participation; unemployment rates; and maternal observable characteristics. Also estimated auxiliary regressions that examine the effect of policy variables on aggregate Medicaid caseloads.	Medicaid caseload increases by 0.233 for each 1% increase in welfare rate. Medicaid caseload increases by 0.664 for each 100% increase in income cut-off (for those not receiving cash benefits). Increase in income cutoff from 100% to 200% of poverty line increases probability of adequate prenatal care by 0.4% for whites. 2pp increase in welfare rate associated w/ 1.1% increase in probability that prenatal care was initiated in 1st trimester; 0.7% increase in probability of adequate care for whites; 2% increase for both for blacks. Increase in income cutoff from 100% to 200% of poverty line associated w/ a decrease of 1720 fetal deaths per year among blacks 2% increase in welfare associated w/ 10% decrease in fetal deaths per year among blacks. Most administrative reforms have no effect. But using mail-in forms (instead of in-person interviews) increases probability that prenatal care was initiated in 1st trimester by 0.7% for blacks and horter forms increase probability that prenatal care was initiated in 1st trimester by 3% for whites. Using mail-in forms decreases probability of low birth weight by 26%; of very low birth weight by 38% for whites.

Using Discontinuous Eligibility Rules to Identify the Effects of the Federal Medicaid Expansions on Low-Income Children (Card and Shore-Sheppard (2004)). Data from SIPP for 1990-93, March CPS for 1990-96, and Health Interview Survey for 1992-1996 on children under 18 years old. n= 10,268 to 16,196 across the different years in SIPP.	Two sources of identification: "The 133% expansion" (children under age 6 living in families with incomes below 133% of poverty line became covered in 1989) and "the 100% expansion" (children born after September 30, 1983 in families with incomes below the poverty line became covered). Difference-in-difference design comparing age-6 and age-5 children in families with incomes between 100% and 133% of the poverty line for the 133% expansion, and comparing children born before and after Sep. 30 , 1983 for the "100% expansion". Regression of Medicaid enrollment on dummy for being below poverty level, dummy for being born after 9/30/1983, their interaction, dummy for age <6 years old, interaction between dummy for age <6 years old and dummy for being between 100% and 133% of poverty line, a flexible function of age and family income, and other background characteristics as well as year fixed effects.	The 100% expansion led to 7-11% take-up rates, while the 133% expansion had <5% take-up rates. No evidence for other insurance crowd-out in SIPP data. Results from CPS data suggest that the 133% expansion led to decline in other health insurance coverage by approximately the same amount as the take-up in Medicaid. Similar results using Health Interview Survey data.
Effects of Medicaid managed care on prenatal care and birth outcomes (Aizer, Currie, Moretti (2007)). Data on birth outcomes from the California Birth Statistical Master File 1990-2000 and Birth Cohort files for the same time period. Hospital-level data from Vital Statistics records. n = 55,000 births.	Exploited the county-by-county variation in implementation of Medicaid managed care, which resulted from a phase-in policy in California that required women enrolled in Medicaid to switch to managed care plans. Some counties switched to COHS plan, others switched to 2 or more plan system. Multivariate regression with county fixed effects, and mother fixed effects, as well as other observable characteristics. Robustness checks: Estimating same model for married women only (unlikely to be on welfare, hence unlikely to be subject to MMC); controlling for mobility b/n counties; regression discontinuity design to eliminate time trend effects; adding interaction terms b/n time trend and several dummies; "intent- to-treat" models to control for the MMC adoption being not exogenous to counties.	Probability of starting prenatal care in 1st trimester: decreased by 9-10% in both COHS and 2-plan counties. Use of induction/stimulation of labor: increased by 43.8% in COHS counties. Use of fetal monitors: increased by 25.9% in COHS counties. Incidence of low birth weight: increased by 15% in both COHS and 2-plan counties. Incidence of short gestation: increased by 15% in both COHS and 2-plan counties. Incidence of neonatal death: increased by 50% in 2-plan counties.

EFFECTS ON LATER CHILD OUTCOMES		
Medicaid Eligibility and the Incidence of Ambulatory Care Sensitive Hospitalizations for Children (Kaestner, Joyce and Racine (2001)). Data from the Nation-wide Inpatient Sample of the Healthcare Cost and Utilization Project for 1988 and 1992. n = 36,000.	Difference-in-difference, comparing the change in ambulatory care sensitive (ACS) hospitalizations before and after Medicaid expansions b/n poor and non-poor children. Poverty status determined by median family income in child's zip code of birth. Two treatment groups: income<$25,000 and $25,000<income<$35,000. Control group: income>$35,000. Separate estimates for children aged 2-6, 7-9. Controlled for hospital-specific, year, and individual factors. Incidence of ACS hospitalizations calculated using both non-ACS hospitalizations and total births in the denominator.	For children aged 2-6 in families with <$25,000 income, incidence of ACS hospitalizations due to dehydration, convulsions and non-asthma illnesses declined by 10-20%. Estimated effect sizes of 40-80% for those affected by Medicaid expansions. For children aged 2-6 in families with $25,000<income< $35,000, incidence of ACS hospitalizations due to non-asthma illnesses and pneumonia declined by 10-14% (only when denominator is total births). For children aged 7-9 in families with $25,000<income< $35,000, incidence of ACS hospitalizations due to asthma declined by 22-30%; hospitalizations due to eye, nose and throat illnesses declined by >100%. No significant effects on children aged 2-6 in $25,000< income<$35,000 group or on children aged 7-9 in <$25,000 income group.
Low Take-Up in Medicaid: Does Outreach Matter and for Whom? (Aizer (2003)). Data on monthly Medicaid enrollment for children aged 0-15 in CA from 1996 to 2000 linked with information on the number of community-based application assistants in each zip code in the month of application by age and race. n = 324,331 for analysis of take-up. n = 121,806 for analysis of hospitalizations.	Examined effects of CA outreach campaign launched in June 1998, which consisted of community-based application assistants and a media campaign to raise awareness about Medicaid, on Medicaid enrollment. Multivariate regression with key explanatory variable being the number of community-based application assistants in each zip code in the month of application by age and race. Included zip code and month fixed effects as well as controls for changes in business cycle and the demographic compositions of the state. Also examined effect of early Medicaid enrollment on ACS hospitalizations by instrumenting Medicaid enrollment with outreach measures.	Access to bilingual application assistants increases new monthly Medicaid enrollment by 4.6% among Hispanic children, and by 6% among Asian children (relative to other children in same neighborhood). A 1,000-child increase in Medicaid enrollments decreases ACS hospitalizations by 3.26 hospitalizations (mean not reported, so can't calculate relative effect size).
Public Insurance and Child Hospitalizations: Access and Efficiency Effects (Dafny and Gruber (2005)). Data from NHDS on child discharges for 1983 to 1996. Cells defined for 4 age categories (<1, 1-5, 6-10, 11-15) for each state and year. n = 2308 cells. Used age-state-year populations from the Census Bureau to calculate hospitalization rates for each cell.	Investigated impact of Medicaid expansions on hospitalizations using the variation in Medicaid eligibility b/n states, over time, and by age. Key explanatory variable is the eligibility rate measured by the fraction of children eligible for Medicaid in each age- state-year cell. Controlled for age, state, and year fixed effects as well as state-year interactions. Used the fraction eligible calculated using a fixed sample to instrument for actual eligibility in each state, year, and age group. Also examined effect of Medicaid eligibility on length of stay in hospital and the number of procedures performed.	A 10pp increase in Medicaid eligibility leads to a 8.4% increase in total hospitalizations, an 8.1% increase in unavoidable hospitalizations, and no statistically significant impact on avoidable hospitalizations. Assuming access to hospital care increases the likelihood of all kinds of hospitalizations equally, these results imply that increased use of primary care engendered by Medicaid expansions mitigated the increase in total hospitalizations by reducing the increase in avoidable hospitalizations that would have otherwise occurred. A 10pp increase in eligiblity leads to a 3.1% decrease in the length of hospital stay, a 5% increase in the # of procedures performed, and a 6.6% increase in the likelihood of having any procedure performed, i.e. leads to more aggressive care.
Public Health Insurance, Program Take-Up and Child Health (Aizer (2007)). See Aizer (2003) entry for description of data.	See Aizer (2003) entry. Additional analysis: Examined heterogeneity in effects on take-up by age. Examined nonlinear effects on take-up. Examined effects of English and Spanish language advertisements on take-up.	Proximity to an additional bilingual application assistant increases new monthly Medicaid enrollment by 7-9% among Hispanics and by 27-36% among Asians. Smallest effects for infants who were already largely eligible; largest effects for ages 6-15. Effect is linear for Hispanics; slightly concave for Asians. English language advertisements increase Medicaid enrollment in the following month by 4.7% among all children, and Spanish language advertisements increase Medicaid enrollment in the following month by an additional 2.5% among Hispanic children. Increasing the # of children w/ Medicaid by 10% results in a 2-3% decline in avoidable hospitalizations.

Has Public Health Insurance for Older Children Reduced Disparities in Access to Care and Health Outcomes? (Currie, Decker, and Lin (2008)). Data from the National Health Interview Surveys for 1986-2005. n = 474,164 children <18 yrs old. Instrument data from the CPS.	Identification due to the fact that expansions in Medicaid/SCHIP eligibility for older children relative to younger children happened at different times in different states. Instrumented for individual Medicaid/SCHIP eligibility using an index of generosity of the state's public health insurance programs calculated using a fixed group of children. Then, estimated impact of public health insurance eligibility on the relationship b/n family income and child health status and on the relationship b/n family income and doctor visits in the previous year. Also tested whether the relationship b/n income and outcomes changed over time within age groups by running separate regressions for 4 age groups: 0-3, 4-8, 9-12, 13-17. Finally, estimated relationship b/n health outcomes and the fraction of children eligible in child's birth cohort in the child's current state of residence for children aged 9-17. Controlled for family background variables and year and state fixed effects.	For children aged 9-12, the coefficient on income in a model of health status declined by 20% over 2000-2005. For children aged 13-17, it declined by 18% over 1996-2000 and by 25% over 2000-05. For children 0-3/4-8/9-12 the coefficient on income in a model of doctor visits declined by 64%/62%/50% between 2000-2005. For children aged 13-17 there was no significant change in the income coefficient. Significant declines in the income coefficient over 1991-1995 and 1996-2000 for children aged 0-3 and 4-8 as well. No statistically significant impact of contemporaneous health insurance eligibility on child health status. A 100 pp increase in the fraction eligible at age 3 would reduce the probability that the average child aged 9-17 is in less than excellent health by 11%. A 100 pp increase in the fraction eligible at ages 1 and 2 would reduce the probability that the average child aged 9-17 had no doctor visit in the past year by 41%.
The Impact of Children's Public Health Insurance Expansions on Educational Outcomes (Levine and Schanzenbach (2009)). Data on state-level average scaled test scores from the National Assessment of Educational Progress for 1990 to 2003. (n=431). Data for simulated instruments from March CPS. Data on child health from Vital Statistics for 1984-2003 (n=1020).	Examine the impact of public health insurance at birth on 4th and 8th grade reading and math test scores. Difference-in-difference-in-difference using cross-state variation over time and across ages in children's health insurance eligibility due to Medicaid and SCHIP expansions. Instrumented for public health insurance eligibility using the simulated fraction eligible (as in Currie and Gruber (1996) see above). To test whether changes in educational outcomes are due to improvements in health status or to additional household income generated if the availability of public health insurance crowds out private health insurance, they estimated the direct impact of health at birth on educational outcomes. Controlled for state and year fixed effects and state-specific time trends.	A 50 pp increase in eligibility at birth increased reading test scores by 0.09 SD relative to 4th and 8th grade combined mean scaled score. No effect on math test scores. Expansions to public health insurance eligibility at birth associated w/ 1.6% reduction in low birth weight rate for whole sample and 6.7% reduction in infant mortality rates among women with at least a high school degree. A 50% increase in low birth weight (infant mortality) rate would decrease reading test scores by 0.12 SD (.07SD).
Public Insurance, Crowd- Out and Health Care Utilization (Koch (2009)). Data from the Medical Expenditure Panel Survey. Focus on subsample of children <18 yrs whose families have incomes b/n 50% and 400% of poverty line. (n=32,609). Data on public insurance reimbursement rates from the American Academy of Pediatrics for 1998-2001.	Regression discontinuity design using family income cut-offs that determine eligibility for Medicaid and SCHIP. To distinguish b/n children who are eligible for public insurance w/out access to private insurance and those whose private insurance may be crowded-out due to expansions in public health insurance, constructed a measure of private-insurance "offer" (=1 if any of the following holds: child is privately insured at the time of the eligibility measurement or a family member is privately employed at the time and either has insurance through the job or turns down the insurance at the job). Estimated impacts of public health insurance eligibility on measures of care utilization (doctor's visits, etc.) as well as on private insurance take-up. Also estimated the differential effect of eligibility on those w/ and w/out private health insurance by interacting the eligibility variable with the "offer" variable. Controlled for background characteristics as well as state and year fixed effects. Also looked at impacts of differential reimbursement rates across states and years on access to care for the publicly insured.	Public insurance eligibility increases public insurance coverage by 21% and decreases private insurance coverage by 24%. So overall decrease in insurance coverage of about 4.8%. Eligibility decreases the number of office visits/self-reported health status/total expenditures by 12%/87%/23%. Increases the number of ER visits by 18%. Eligibility decreases the number of office visits by 18% for those with outside private health insurance. No statistically significant effects on ER visits, prescriptions, or refills. Children just eligible for public health insurance are 4% less likely to have a usual source of care and 4% more likely to go without care. No statistically significant effects on hospital or ER visits. Eligibility increases children's BMI by 2%. Parents of just eligible children are 5% less likely to be given advice about their child's eating healthfully. No significant effects on asthma diagnosis. Conditional on taking medication for asthma, eligibility increases probability of taking inhaled steroids (gold standard treatment) by 32%. A $4 increase in reimbursement for office visits leads to a 5% increase in the number of office visits.

Note: Percent changes (denoted by % instead of "pp") are reported relative to the mean.