

CRISES AND INTERGENERATIONAL MOBILITY*

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Abstract

I study the impact of the Great Depression on the educational attainment and intergenerational education mobility of U.S. males. I collect local data on youth unemployment and school quality during the Depression and study their effect on secondary education. Using Census data on males and their fathers, my difference-in-differences strategy attempts to explain the variation in schooling and mobility across cohorts. I find that worsening labor markets for youth significantly increased the school attendance of youth from poor, but not wealthy, families while public spending cuts decreased it. Overall, I find that the Depression increased intergenerational education mobility.

JEL Classification: N3, I2, H4

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1 Introduction

Education is one mechanism through which younger generations climb the socioeconomic ladder and sever intergenerational links in economic outcomes. Yet, investment in education - both at the policy and individual level - varies with macroeconomic conditions that alter the available resources and opportunity cost of schooling. In a decentralized public education system, an economic crisis can lower education quality (e.g., fewer teachers per student) and the opportunity cost of youth labor (e.g., fewer low-skill jobs) differently across regions and time. When a crisis hits, do these differences at the local level result in varying levels of educational attainment and, ultimately, intergenerational mobility? If so, who are the winners and losers of crises? Understanding the interaction between macroeconomic shocks and microeconomic mechanisms driving changes in human capital investment is essential for guiding education and labor policy.

For example, consider a two-generational, low-educated, and poor household in financial distress. The household head may decide that the opportunity cost of education is too high for their child: parents may pull youth out of school and push them into the labor market or household production, solidifying the parent-child link in educational attainment at the lower end of the socioeconomic spectrum. At the same time, wealthy households have no such constraints: during a crisis, children of highly educated parents continue going to school, reinforcing the intergenerational link at the upper levels of schooling. In this example, macroeconomic busts reduce educational mobility. More broadly, the changes in intergenerational mobility depend on the labor market opportunities for youth and household constraints.

This paper studies the schooling behavior and intergenerational mobility of young, urban males in the U.S. during the Great Depression. Combining full-count Census records with school district data and local youth unemployment rates, I quantify the effect of the change in local youth labor markets and school quality during the Great Depression on the educational attainment and mobility of males in the 1908-1918 birth cohorts. I compare outcomes in 1940 for cohorts that graduated high school right before the Depression started (1926 – 1929) with those attending high school during it (1930-1937).¹

The historical context provides several empirical advantages. First, a decentralized public funding system created local variation in education quality in the 1930s, which does not exist - to the same extent - under the current state-level equalization schemes and federal support for K-12 education in the U.S. Likewise, the effect of the Depression was unevenly felt across the country,

¹State-level evidence that the Depression shut off employment opportunities for youth and drove them back into schooling was first introduced by Claudia Goldin and Lawrence Katz (e.g., Goldin and Katz (1999)).

creating considerable variation in youth opportunities and economic deprivation across local labor markets. Second, most states in the U.S. today have laws preventing youth from entering the formal labor market, making studying this relationship impractical with modern survey data. In contrast, youth labor was much more common in the first half of the 20th century. Lastly, the availability of microeconomic Census records of the whole population during and after the high-school movement provides measurable short- and long-run outcomes and permits a holistic analysis of heterogeneous effects.

I create my dataset by merging multiple archival sources. The outcome variables come from the 1940 Census, the first federal Census to ask about each respondent's education level. Fortunately, in the scope of intergenerational mobility studies, education is less sensitive to well-known problems with measurement error and lifecycle bias afflicting measures of income (Solon (1992), Mazumder and Acosta (2015)).² I create my primary sample of linked (1930 – 1940) urban males using 100 percent count U.S. Census records and linking crosswalks provided by Abramitzky et al. (2020).

I combine my sample of young males with unemployment-by-occupation-by-age data from the Special Unemployment Census of 1931. Since the Unemployment Census canvassed only 18 regionally dispersed cities and three boroughs of New York City, I estimate youth unemployment for all other cities by taking a weighted average of regional youth unemployment-by-occupation rates, using 1930 occupation-by-city shares aggregated from the 1930 Census full count census as weights.³ To the best of my knowledge, mine is the first attempt to quantify locally disaggregated and age-specific unemployment rates during the Great Depression in the U.S. context.⁴ Finally, to obtain education measures, I digitize biennial records from the Census of Education from the U.S. Office of Education on revenues and expenditures at the city level from 1922 to 1938. I follow the economics of education literature and proxy quality with the change in the total real spending per pupil. I show that expenditure is closely related to the student-teacher ratio, average real teacher wages, and term length.

My empirical strategy attempts to explain the within-city variation in high school grad-

²One well-known issue in intergenerational studies using U.S. Census data is that parent-child links are only identified when parents and children cohabit. Because the proportion of children living with their parents drops from 80 percent to 60 percent once the children reach their 20s (Online Figure A.1), I link the younger cohorts in my sample to their 1930 households and the older cohorts to their 1920 households to obtain parent and household characteristics.

³I show that this measure is a good predictor of the actual unemployment rate in the Census sample and predicts youth welfare enrollment in 1934.

⁴Numerous efforts have been made to compute accurate unemployment rates at a higher level of aggregation, notably Sundstrom (1992), Darby (1975), Margo (1991) and Wallis (1989).

uation rates across cohorts using a difference-in-differences design using across-city variation in unemployment and public education spending cuts. Notably, the variation in either is not systematically related to changes in the graduation rate before 1930, providing evidence that the underlying parallel-trends assumption is not prohibitively strong. The experiment thus compares the education outcomes of individuals on the cusp of making secondary schooling decisions during the Great Depression with those who graduated before the Great Depression within the same city, conditional on national trends and static city determinants of educational attainment. After quantifying the average effect, I conduct heterogeneity analyses based on household characteristics such as parental occupation and family composition, county and state-level characteristics regarding New Deal spending, and youth labor regulation. Lastly, using the same pooled difference-in-differences strategy with a sample of linked father-son pairs, I explore the impact of the Depression on inter-generational education mobility.

Beginning with education attainment outcomes, I find that increases in the youth unemployment rate in 1931 significantly increased secondary school graduation rates: one standard deviation (4 percent) increase in unemployment caused a 1.2-1.6 percentage point increase in the graduation rate for the Depression-era cohorts and 0.9 – 0.12 more years of schooling. On the other hand, cuts in education spending decreased graduation rates by about a quarter of this effect. To put these numbers in perspective, I find that youth unemployment can account for 18 – 21 percent of the total increase in high school graduation during the 1930s, while education spending cuts attenuated this effect by 4 – 5 percent.

I then examine the heterogeneity in the response based on parental occupations and find an influx of students from primarily blue-collared families into high schools drives the average result: sons of laborers, craftsmen, service workers, and salesmen. Conversely, educational attainment was unchanged for the sons of professionals, managers, and clerks. Grouping occupations by their 1950 median incomes, I find that the effect is mainly driven by the first through third quartiles of the parental income distribution. Regarding household composition, I observe more significant results for youth with siblings and those living in non-multigenerational (no grand-parent) households. Finally, I show that the Depression specifically impacted entry into secondary schools: I find a sharp jump in the distribution of 8th through 10th-grade finishers, with some attenuation during the last two years of schooling.

This paper contributes to three strands of literature. First is the literature that studies the elasticity of schooling choices with respect to changes in labor markets. Researchers have shown

that local labor market conditions affect education attainment in both developed (e.g., Betts and McFarland (1995); Charles et al. (2015)) and developing economies (e.g., Shah and Steinberg (2017); Bau et al. (2020); Atkin (2016)). Most of this body of work uses trade or industry-specific labor-demand shocks (e.g., natural resources as in Black et al. (2005) and Cascio and Narayan (2015)) and finds that youth discontinue schooling when opportunities *increase* and the skill premium is low. I extend this literature by studying the elasticity during macroeconomic *downturns* from a non-trade perspective and measuring the effect’s attenuation due to pro-cyclical (declining) education quality. The closest paper is the one by Feigenbaum (2015), who focuses on the effect of the Great Depression on intergenerational mobility in the U.S. using approximately 5,000 males from the Bureau of Labor Statistics Cost of Living Survey. While his focus is on income, not education, mobility, he does not find a causal effect of the Great Depression on education attainment in his sample father-son pairs. The differences in our findings is not surprising given that we use different samples (BLS sample vs. linked population records) as well as measures of Depression severity (change in retail sales vs. youth unemployment).

The second piece of literature my work builds on is the research studying the consequences of U.S. educational investments in the first half of the 20th century, specifically the high school movement (Goldin and Katz (1997); Schmick and Shertzer (2019); Card et al. (2018)). For example, Goldin and Katz (1997) finds that graduation rates increased in states with the largest increases in unemployment during the Depression, and Shanahan et al. (1997) finds that Depression-era cohorts in Stanford-Terman Study of Gifted Children also obtained more schooling. My work also provides evidence for one possible mechanism for the observed increase in intergenerational mobility during the first half of the 20th century (Jácome et al. (2021)). I add to this literature by introducing new local measures of youth unemployment and school quality that cover a substantial portion of the urban male population and by quantifying and disentangling the effects of the Depression using a quasi-experimental framework.⁵

The third is the literature on the public sector during the Great Depression in the U.S., primarily focused on federal programs stemming from the New Deal, such as the Federal Emergency Relief Administration and the Works Progress Administration (Fishback and Wallis (2012)). This literature has found that federal programs had a positive impact on retail consumption (Fishback et al. (2005)), migration (Fishback et al. (2006)), and crime reduction (Fishback et al. (2010)). At

⁵Other papers that study the determinants of educational attainment during this period are Baker et al. (2020) (boll weevil), Baran et al. (2020) (Great Migration), and Karger (2021) (public libraries), and Stephens Jr and Yang (2014) (compulsory schooling laws).

the local level, research has found large reductions in public good provision during this time period (Janas (2021); Siodla (2020)). To my knowledge, this paper is one of the first to study how local school districts responded and the consequences on schooling choices.

The rest of the paper is organized as follows. Section 2 providing motivating macroeconomic evidence for the unemployment channel. Section 3 provides an overview of the linked sample of urban males, youth unemployment in 1931, and school expenditure shocks during the Depression. Section 4 discusses the pooled difference-in-differences empirical design while Section 5 presents the results and explores heterogeneous effects. Finally, Section 6 concludes by discussing intergenerational mobility.

2 Education attainment in the 1930s

To motivate, I begin by presenting four macroeconomic facts about trends in high school graduation during the first half of the 20th century - the 1930s in particular - in Panels (A) through (D) of Figure 1. This period of U.S. economic history, typically referred to as the “high school movement,” was characterized by a marked increase in the number of youth completing at least 12 years of education (Goldin and Katz (1997)).

(Figure 1 around here)

In the aggregate, trends in high school graduation rates in the U.S. suggest that schooling decisions in the 1930s were, in fact, unique. Using data from the U.S. Department of Education in Panel A, I plot the ratio of high school graduates to 17-year-olds decennially between 1910 and 1930 and annually after. In 1910, this ratio stood at just 8.8%. While the graduation rate more than tripled to 29% by the eve of the Great Depression in 1929, there was an evident gain relative to the trend beginning in the early 1930s. This uptick did not subside until the U.S. entered the Second World War in the early 1940s.

But did the Depression have anything to do with this acceleration? Panel B offers suggestive evidence that Depression severity at the local level was positively associated with increased educational attainment. I combine a linked sample of males in the 1930 and 1940 Censuses with the change in county-level retail spending between 1929 and 1933 (Fishback et al. (2003)). I restrict attention to persons in this sample from the 1906-1922 birth cohorts who reported living in a city in 1930. I then compute the share of the cohort that reported finishing at least 12 years of education in the 1940 Census, separately by whether or not the individual was living in a county in the top or bottom tercile of the change in retail sales. Panel B shows a slight difference in high school

attainment by cohort across the two groups of counties from 1926 until the 1930 graduating cohort. However, beginning with those who turned 14 during and after the Depression started and thus at the point of making high school-going decisions, the graduation rate for those in counties with substantial adverse economic shocks surpassed that of those from the same birth cohort in counties with milder shocks. By 1936, the average rate rose by 10.0% in worse-off counties and only 7% in better-off counties.

This relative gain in worse-off counties differed across households by socioeconomic status. Taking the same sample of linked males as in Panel B, I split the cohorts by local Depression severity and the individual's parent's occupation in 1930 (blue-collar vs. white-collar)⁶. In Panel C, I plot the cohort share that reported finishing at least 12 years of education in the 1940 Census separately by local economic condition and family background. I observe no meaningful difference between individuals with white-collar parents across Depression severity. The main driver of the aggregate increase was, in fact, individuals from blue-collar families, especially those in places more hit by the Depression.

Because blue-collar workers have lower educational attainment and their kids were the ones staying in school during the 1930s, it is thus not surprising that the Depression increased inter-generational educational mobility during this decade, as shown in Panel D. Using linked father-son pairs and novel local youth unemployment in 1931, I plot the share of upwardly mobile sons - those who completed at least 12 years of education and whose father did not - separately in areas of low and high youth unemployment. Sons in high-unemployment areas before the Depression were slightly less mobile before the Depression with only 34 percent of them out-performing their fathers, roughly 1.50 percent less than those sons in low unemployment cities in 1929. However, after the onset of the Depression, the pattern reversed: the sons in the former group were 2 percent *more likely* more likely than those in the latter group to obtain at least a high school diploma, conditional on having a father with less than a high school diploma.⁷

The patterns in Figure 1 indicate a boost in the high school graduation rate for individuals during the Depression and raise the possibility that the recession pushed youth into schooling. I present a conceptual model that portrays the trade-off between higher local unemployment and school quality. In the model, households invest in an additional year of schooling if the marginal

⁶As I discuss in greater detail in Section 3, linking parents to their kids for older cohorts in 1930 requires linking to the 1920 Census.

⁷This jump is due to the difference in sons' schooling choices, not the fathers'. There was no differential trend in fathers' high school completion rates based on the quartile of youth unemployment in 1931, as I show in Online Figure A.4

return to education is larger than the opportunity cost, conditional on the perceived returns to education based on observable quality measures. While an economic downturn reduces the opportunity cost for everyone and entices some to opt-in to secondary school, this influx of youth is attenuated as schools shift from high to low quality, and the greater cost of attending low-quality institutions outweighs the return.

3 Data Construction and Sample Selection

This section describes the construction of city-level youth unemployment rates, school quality measures, and the linked sample of sons and fathers. In section 3.1, I present my method of measuring opportunity cost, which I proxy by the youth unemployment rate. Locally dis-aggregated unemployment data during the Depression for youth (or adults) is not available systematically. Therefore, I use three sources of information to estimate unemployment rates: city-level occupation reports within the state-level publications of the 1930 decennial Census, the Special Unemployment Census of 1931, and the full count records of the 1930 Census publicly available on IPUMS (Ruggles et al. (2020)). In Section 3.2, I discuss school district expenditure, enrollments, teacher wages, number of teachers, and term lengths, which I obtain from the *Biennial Survey of Education*. Finally, I describe the linking procedure between 1930 and 1940 Censuses of young males and their fathers in Section 3.3.

3.1 Local youth unemployment rates

Summary of sources to construct youth unemployment rates

	Name	Variable	Level	Scope	Source
(1)	Unemployment Rate - Numerator	Class A and Class B Unemployed - Male	Occupation-city	21 cities*	1931 Special Census of Unemployment (U.S. Census Bureau)
(2)	Unemployment Rate - Denominator	Number of Employed - Male	Occupation-city	same as (1)	1930 Population Census (U.S. Census Bureau)
(3)	Regional occupation-unemployment rates	Average{(1) / ((1) + (2))} across cities within region	Occupation-region	4 regions	Author calculation
(4)	Occupational share	Number of employed in occupation/number in all occupations	Occupation-city	981 cities	1930 Census 100% count records (IPUMS)
(5)	Youth unemployment estimate	$\Sigma(3) \times (4)$	City	981 cities	Author calculation

3.1.1 Unemployment rate (numerator): Special Census of Unemployment 1931

Amid deteriorating labor market conditions in January 1931, Congress authorized the Census Bureau to conduct a special Census of Unemployment in 21 urban areas - 18 cities and three boroughs of New York City. The Bureau used the same schedule form and enumerators (to the extent possible) as in the April 1930 Census to make the returns comparable. Since the Bureau's focus was on the characteristics of those unemployed, the reported statistics break down occupational unemployment by sex, age, occupation, marriage status, race, and nativity. Enumerators were instructed to visit each household and ask whether any household member who ordinarily worked at a gainful occupation was unemployed on the preceding day and, if so, ask the specified questions and make detailed entries.

Most of the unemployed fall under two classes, and I collect data on both. Class A contains persons out of a job, able to work, and looking for a job. Across the 1931 census, 20.4 percent of gainful workers from 1930 were classified as Class A unemployed. Class B includes persons having jobs but on lay-off without pay, excluding those sick or voluntarily idle. This class constituted another 3.9 percent of all gainful workers in 1930.

As stated in the introduction to the Special Census statistics, the Bureau published the data in 1931 such that comparisons to 1930 figures would accurately reveal the extent of unemployment in the labor market. Therefore, the age and occupation distribution in these tables was made to conform as closely as possible with the age and occupation distribution of the gainful workers as presented in the 1930 Census. Likewise, the occupations in 1931 were the ones used in 1930.

To obtain the number of unemployed by age group and occupation in each of these cities by 1931, I digitize Table 12 of the Special Unemployment Census of 1931. For example, I observe that 167 deliverymen in the 10-19 age group in Birmingham, Alabama, are Class A unemployed, and 12 are Class B unemployed. In total, I collect data on 21 cities spanning the special enumeration area of the 1931 Census: Boston, Buffalo, New York - Bronx, New York - Brooklyn, New York - Manhattan, Philadelphia, Pittsburgh, Cleveland, Dayton, Chicago, Detroit, Duluth, Minneapolis, St. Louis, Birmingham, New Orleans, Houston, Denver, Seattle, Los Angeles, and San Francisco.

3.1.2 Unemployment rate (denominator): Census 1930

The total number employed by occupation comes from the April 1930 Census. The employed are "gainful workers" that include everyone ten years old and over who regularly work in an

occupation for pay. It does not include women doing housework in their own homes, without wages, and having no other employment, nor does it include children working at home or at odd times on other work. The detailed occupation classification for gainful workers consists of 534 occupations. In the tabulation of the unemployment returns, this list was consolidated to 330.

Employment by occupation for different age groups in 1930 comes from Table 12 in the state-level reports from the 1930 Census. Specifically, this table reports the number of employed persons by occupation in cities of 100,000 or more. Continuing with the example from (1), I observe 458 delivery men enumerated by the Census in the 10-19 age group in Birmingham, AL, in 1930. I collect occupation-city data for the same 21 urban areas enumerated by the special census of unemployment in 1931 in (1)⁸.

3.1.3 Constructing regional occupation-unemployment rates

For each occupation in cities reported in both the 1930 and 1931 censuses, I define the youth unemployment rate as:

$$unemp_{ij} = \frac{ClassA_{1931,ij} + ClassB_{1931,ij}}{ClassA_{1931,ij} + ClassB_{1931,ij} + Employed_{1930,ij}} \quad (3.1)$$

where i denotes the occupation, and j denotes the city, and all measures are for the age group 10-19. I then compute the average unemployment rate by occupation for each region by calculating the average occupation unemployment for all cities within the region, weighted by total males aged 10-19 in the labor force as of 1930.

3.1.4 Occupational shares

I obtain youth occupational shares for all cities to extrapolate unemployment rates to all Census-enumerated cities by aggregating person-level records from the 100 percent count 1930 Census returns available on IPUMS. My sample includes all 10-19 year-olds reporting an occupation in 1930. The occupation variable in the 100 percent count records was standardized to reflect the 1950 occupational definitions, varying slightly from those published in Census reports in 1930 and 1931. I create a crosswalk between 1930/31 and 1950 occupations and discuss potential mismeasurement in more detail in Online Appendix B.

⁸The age brackets are: 10-17, 18-19, 20-24, 25-34, 35-44, 45-54, 55-64, 65-74, 75 and over. The Census reports combined brackets 10-17 and 18-19 in 1931.

3.1.5 Youth unemployment estimates

Finally, using occupational shares from (4) and the regional rates in (3) I compute average city-level youth unemployment rates:

$$unemp_{j(k)} = \sum_{\forall i} \omega_{i,j} \times unemp_{i,k} \quad (3.2)$$

where $\omega_{i,j}$ denotes the youth occupational share of occupation i in city j and $unemp_{i,k}$ is the unemployment rate of occupation i in region k .

Table 1 shows significant variation in 1931 unemployment of 10-19 year-olds in the cities enumerated by the Census. Consistent with the literature showing regional patterns of the Depression across the U.S. (Rosenbloom and Sundstrom (1999)). I find that the estimated unemployment was above 40% in industrialized cities specializing in durable goods manufacturing (Buffalo, Detroit, Cleveland) and relatively low (25%) in cities specialized in trade and services (San Francisco, Seattle, Manhattan).

(Table 1 around here)

The occupational distribution of youth in these cities drives the variation in total rates. For example, consider the difference between Detroit and San Francisco. The largest share (11.5%) of the youth labor force in Detroit was employed as laborers in the iron and steel industry and experienced a staggering 53% unemployment rate. On the other side of the country, youth in San Francisco primarily worked in low-skill white-collar clerical work, which experienced a much milder Depression of 10.5% youth unemployment.

(Figure 2 around here)

Figure 2 plots $unemp_{j(k)}$ for the full sample of cities. Not surprisingly, this figure shows strong regional clustering with relatively high rates in the Midwest and Northeast and low rates throughout the South and West. To help explain the causes of this clustering, Table 2 presents the most common youth occupations by region. The column “# Cities” reports the number of cities in which the occupation is the most common, and the “Weight” column reports the share of the youth labor force in that occupation. In both the Midwest and Northeast, operatives and laborers in manufacturing constitute much more of the youth labor force - with a higher estimated unemployment rate of between 30 and 40 percent - than in Southern cities. Additionally, the weight placed on these occupations in the computation of the total unemployment rate is considerable, between 20 and 30 percent. On the other hand, the South youth labor force is dominated by

servants and retail workers, who saw lower unemployment rates. In only two cities manufacturing laborers make up the largest share in the South, and the weight is below 15 percent. I flexibly control for region-by-year fixed effects in my empirical methods to account for regional clustering.

(Table 2 around here)

3.2 Public education quality

This section describes data on K-12 education quality at the city level during the 1920s and 1930s in the United States. I collect statistics from the *Biennial Survey of Education*, a U.S. Office of Education publication. The *Survey* contains information on state-run school systems, city school systems, universities, colleges, professional schools, teachers colleges, and private high schools and academies. Initially, the statistics were assembled by contacting the roughly 31 thousand school systems in the U.S. at the time. I use the public city-school system data in each report between 1922 and 1938, which had a high (98-99 percent) survey response rate throughout this period.

The scope of the city data is vast - over 118 variables regarding enrollments, expenditures, and revenues - and I use only a sample of these variables in this paper. While my primary variable of interest is total education spending per pupil, I also collect data on enrollments and teachers and show, in Online Appendix A, that reductions in expenditure had a direct impact on more direct measures of school quality: student-teacher ratios, average teacher wages, and term lengths. In all, I collect expenditures in 1930 and 1934 for all cities with a population of above 10,000 as of 1930 (564 cities). For panel analysis, I collect all other data for cities above 30,000 in population (220 cities)⁹.

(Figure 3 around here)

Figure 3 shows the extent to which public school quality in the United States suffered during the Depression. Panel A shows that the student-teacher ratio increased, on average, from around 33.5 to 37.5 from 1930 to 1934 for secondary schools, with no significant change in elementary schools. This increase is driven by higher enrollments into secondary schools and teacher dismissals. Nominal teacher wages, as indexed to other non-teaching wages measured in 1940, Panel B, show that salaries decreased by around 12 percent from 1930 and 1934, with a complete rebound by 1938. Real total per-pupil expenditure, Panel C, decreased significantly during the Depression after continually growing during the 1920s, and the school term was reduced by approximately

⁹I combine data for cities reporting multiple districts: Aurora, IL, Evanston IL, Beaumont TX, Berwyn IL, Dearborn MI, Pueblo CO, Saginaw MI, Troy NY, Waterloo IA, Wheeling WV, Clarksburg WV, Corning NY, Berwyn IL, Manchester CT, Clinton IA

four days on average. My primary measure of school quality shock is the log difference in total real per-pupil expenditure between the 1934 (closest to the Depression trough) and 1930 (closest to pre-Depression peak) school years:

$$\Delta exp_j = \log(exp_{j,1934}) - \log(exp_{j,1930}) \quad (3.3)$$

3.3 Linked Census records of sons and fathers, 1930 - 1940

The primary outcome variable for sons and fathers - education attainment – comes from the 1940 U.S. Census, the first time questions regarding education appeared in the Decennial Census. However, to obtain father-son linkages, I need to link the 1940 Census records with the 1930 (or 1920) Census.

The well-known obstacle in studying intergenerational mobility using U.S. Census data is that intergenerational links are identified only if members of both generations live in the same household. This requirement presents an issue to empirical studies in the U.S. because most youth leave the household by their 22nd birthday, and linking an older individual to their parents requires a link to a Census taken during their childhood.¹⁰

This paper focuses on youth for a short period (the 1930s) and the Census records in 1930 provide good coverage for the treated sample of 11-17 year-olds in 1930. These are the cohorts making high-school-going decisions during the Depression and graduating high school before 1940, when I can observe education outcomes. However, comparing treated cohorts with their older counterparts with intergenerational links – those that finished their high-school education before the Depression and still lived with a parent in 1930 - is problematic due to self-selection. Thus, I construct my sample by linking the older cohorts that have moved out by 1930 to their 1920 households, when they were 8-12 years old.

Starting with the entire population of males between the ages of 11 and 22 in 1930 (16.1 million), I imposed several restrictions to arrive at my primary analysis sample. First, I keep those living in a census enumerated city and drop those living in non-households (e.g., orphanages). Second, for the 10-17-year-olds, I keep only those reporting to be a child of the head of the household, effectively dropping grandsons and nephews and those living with older siblings as heads of households. Third, For 18-22-year-olds in 1930, I keep only those reporting to be either a

¹⁰In Online Figure A.1, I plot the share of each age cohort that lives with a parent in the 1930 Census. While the majority (75 to 95 percent) between 0 to 17 live with a parent, this share drops to 68 percent for 19 and 36 percent for 23-year-olds.

child or a head-of-household. For 18-22-year-old heads, I link back to 1920 to find their household characteristics and parental information. For 18-22-year-old children, I use household data from 1930. Finally, I drop records from any city without unemployment or education quality measures. The final sample size before linking to 1940 is approximately 4 million.

I use the crosswalks provided by the Census Linking Project (Abramitzky et al. (2020)) and IPUMS publicly available data (Ruggles et al. (2020)) to link records over time using the ABE procedure.¹¹ Figure 4 shows the proportion of each cohort from my final sample linked between 1930 and 1940 (no marker), that lived with a father in 1930 (hallow circle marker), that lived with a father in 1930 *or* 1920 and was linked to 1920 (x marker), and finally, those that had either father *or* mother info in either 1920 or 1930 (solid circle marker). The line with the triangle marker denotes the smaller sample of linked sons with linked (to 1940) fathers, which I use to study intergenerational mobility. Overall, I obtain outcome variables in 1940 for 31 – 34 percent of the final sample of sons in each cohort, both in the pre-Depression and during-Depression groups.

(Figure 4 around here)

3.4 Summary statistics

Table 3 presents the summary statistics. In Panel A, the unit of observation is the person, and in Panel B, it is the occupation-city (e.g., carpenters in Chicago). All dollar amounts here and throughout the paper are converted to real 1967 dollars using the Consumer Price Index.

(Table 3 around here)

In panel A, I present summary statistics of my person-level sample of linked Census records of U.S. males. The sample consists of 1.4 million records of men aged 10-22 in 1930. By 1940, the average person had completed 10.8 years of education (median 11 years). The sample is predominantly white (96 percent) and consists mainly of non-immigrants (65 percent) living in large metro areas (75 percent). These men come from dual-parent households (92 percent) with 2.6 siblings on average. In terms of local economic and school quality conditions, the average unemployment was 20 percent, and the average drop in (real dollar) total school spending between 1930 and 1940 of 7 percent.

Panel B summarizes the data used to construct youth unemployment rate estimates for 1931. Starting with 981 enumerated cities in the Census of 1930, I drop all cities for which 1931 occupation data covers less than 50 percent of youth workers in 1930, which leaves me with 925

¹¹In my main analysis, I use records matched using any of the linking methods provided.

total cities. There are 67 distinct occupation categories of youth on average, and data on 36 of them exist in 1931. The average weight of a single occupation in the total youth unemployment rate computation is around 2.8 percent. The average regional unemployment rate for an occupation is 25.5 percent, while the median is 24.2 percent.

I explore the representativeness of my main sample by comparing the characteristics between linked and unlinked individuals and the time-varying linking probabilities across treatment variables. Online Table A2 shows small but statistically significant differences between linked and unlinked males. Notably, linked are 6 percent more likely to be a non-immigrant, 5 percent more likely to come from a homeownership household, and 2 percent more likely to have a parent in a white-collar occupation. I do not find evidence that the linking probability varies by main treatment variables over time. In Figure 4, I plot estimated coefficients on cohort fixed effects interacted with the treatment variables (unemployment and school quality shocks) when the outcome is a binary taking the value of 1 if a record is linked. The effect of the treatment variables in my main sample - linked sons with father or mother occ using 1920 or 1930 (yellow circle) – is quantitatively small and statistically insignificant.

4 Empirical Framework

4.1 Identification Strategy

I estimate the dual effect of youth unemployment and public education spending on education attainment and intergenerational mobility using a pooled difference-in-differences research design that compares outcomes of cohorts in cities before and after the onset of the Great Depression. The fundamental identifying assumption is that, in the absence of the Depression, these household choices across cities would have evolved in parallel. As described in Section 5 below, I present direct evidence to support the validity of the parallel trends assumption: the trend in the probability that individuals graduated high school in low youth-unemployment cities versus that in high youth-unemployment cities did not diverge during the period preceding the Great Depression.

This fact is both reassuring and plausible. In the short period considered in this paper, there is no reason to expect that the trends of school-going should vary significantly across cities unevenly hit by the Depression *before* the 1930s. Indeed, the factors that contribute to different levels of educational attainment in regular times, such as the skill-premium, cultural norms, or the availability and proximity of schools, evolved over the preceding three decades, not years.

Conversely, the sharp turn of the economy starting at the end of 1929 was an unexpected and severe shock for households, and choices about a child’s investment in education had to be made in the short run.

Furthermore, the lack of collinearity between education spending and youth unemployment renders stable estimates of both effects. I do not find evidence that collinearity is prohibitive in my setting. Using standard diagnostic tests, I find that the variance inflation factors for both are less than 1.03, and the condition number is 8.4, which are smaller than the standard thresholds of 5 and 10, respectively. Moreover, the bivariate correlation between these two shocks (0.09) is not significant.¹² This lack of correlation is not surprising: at the time, education funding came primarily from local property taxes while most youth worked in industries (retail, manufacturing operatives) that were not directly affected by the housing market crash. Collinearity would be an issue if youth worked in construction: worse housing market conditions would decrease the local tax base (and thus education funding) and youth employment opportunities. However, this was not the case.

4.2 Estimation

My baseline econometric model is a pooled difference-in-differences regression at the person level. Specifically, I estimate regressions of the following form:

$$S_{ijk} = \alpha_j + \beta_k + \sum_{z=1926}^{1936} (Unemp_j T_z) \cdot \gamma_1 + \sum_{z=1926}^{1936} (\Delta Exp_j T_z) \cdot \gamma_2 + \sum_{z=1926}^{1936} (C_j T_z) \cdot \delta_1 + \Omega_i + \epsilon_{ijk} \quad (4.1)$$

where S_{ijk} is an outcome for person i who reported city of residence j in 1930 and turns 18 (cohort) in year k . $Unemp_j$ is the standardized (mean zero, standard deviation of one) estimate of youth unemployment in 1931, where T_z is a year dummy taking the value of 1 if $z = k$ or $k+1$ and 0 otherwise. The vector β_k contains cohort fixed effects, α_j includes the city of residence in 1930 fixed effects, ΔExp_j is the standardized 1934 - 1930 change in log per-pupil expenditure, C_j is a vector of city and county control variables, and Ω_i is a vector of person-specific controls.¹³

The primary outcomes are whether the individual finished at least 12 years of education (binary) and the number of years completed. The coefficients of interest are the vectors γ_1 and

¹²In Online Figure A.2, I plot the relationship between the two treatment variables for the 564 cities in my sample.

¹³The person (household) level controls are the following categorical variables: the presence of a multigenerational household, number of families within the household, ownership of house dwelling, living in a metro area, number of persons in the household, number of siblings, and family size.

γ_2 , which measure the differential change in outcomes for cohorts before and during the Great Depression, holding constant person characteristics and aggregate differences in outcomes across cities and over time. To account for serial correlation and city-specific random shocks, I cluster the standard errors at the cohort-city level in all specifications.

The 1926 and 1927 cohorts serve as the omitted interaction, and all reported coefficients are relative to that year. These coefficients are informative about the timing of the effect of youth unemployment and expenditure cuts on education attainment and the validity of the parallel trends assumption. If cities exposed unevenly to the Depression have common pre-trends, I should not be able to reject the hypothesis of null effects for cohorts before 1930.

I control for several observable and plausibly confounding variables in all specifications above, but I also report estimated coefficients before and after inclusion. First, due to the regionally clustered nature of the youth unemployment measure, I include region-by-year fixed effects, such that the results rely only on *within* region variation. Second, to address potential bias arising from city-level omitted time-varying variables, I include interaction terms between baseline public education expenditure, county unemployment, and youth labor share with cohort dummies. For example, variables that are potentially correlated with baselines - such as teacher recruitment, attitudes toward public spending and education, or labor market conditions before the Depression that could amplify the impact of a recession - would also differently change the incentives of youth to exit school over the Depression are controlled for to the extent possible with the available data.¹⁴

5 The Effect of the Great Depression on Educational Attainment

In this section, I present the paper’s main result: despite a small negative effect of education spending cuts, young males from blue-collar families significantly increased their education investment during the Depression. In turn, the intergenerational education mobility of young males increased during this economic crisis.

To quantify the causal effects of interest, I present a series of formal difference-in-differences estimates for high school completion and educational attainment. I further examine the heterogeneity in the response of youth based on parental occupation – both occupational category and occupational income – and find that the average effect is driven by an influx of students from

¹⁴For example, firms in cities with large youth labor shares in the labor force before the Depression could potentially react to the Depression by setting wage and labor supply policies that would also change a youth’s incentive to pursue education, regardless of the youth unemployment rate. By including the youth labor share by cohort controls, the effects on different cohorts in my preferred specification are above and beyond time-varying local policies that correlate with pre-Depression youth labor shares.

middle-class and blue-collar families: their fathers are laborers, salesmen, or craftsmen in the middle two quartiles in the occupational income distribution. Finally, I conclude the cohort analysis by showing that the effects are largest in cities in states without youth labor regulation as of 1931 but are not driven by county-level New Deal spending.¹⁵

5.1 Cohort estimates

Table 4 presents estimates from the pooled difference-in-differences specification given by equation 4.1 using high school completion as the outcome variable in columns (1) – (4) and school years completed (0 to 17) in columns (5) – (8). The first column reports estimates from a baseline specification that includes only the main shock variables interacted with cohort indicators (grouped every 2 years), fixed effects for both the cohort and city of residence in 1930 and household controls. The second (fifth) and third (sixth) column add region-by-year fixed effects, baseline expenditure, youth labor share, and county unemployment rate in 1930 interacted with cohort indicators. In columns (4) and (9), I add the father’s unemployment rate based the regional adult unemployment rate for his occupation in 1931. Since not all occupations are covered in the 1931 Census, the sample shrinks to 983 thousand. Finally, in columns (5) and (10), I add household fixed effects, effectively comparing outcomes across siblings within the same household. The omitted cohorts are 1926-1927, and the 1928-1929 cohort serves as the pre-period control group. To ease interpretation, I standardize youth unemployment and education spending cuts to have a mean of zero and a standard deviation of one.

(Table 4 around here)

Across all specifications, the results indicate that youth unemployment in 1931 did not impact high school completion in the pre-period, non-Depression cohorts. The coefficient estimate for the 1928-1929 by unemployment term is quantitatively small and statistically indistinguishable from zero. However, the effects become significant starting with the 1930 and 1931 cohorts and grow steadily until the 1934-1935 cohorts, after which they decrease slightly for the 1936-37 cohorts. The presence of more significant effects for later cohorts corresponds well with the Depression explanation: longer (earlier) exposure of these cohorts to the deteriorating labor market pushed relatively more of them towards secondary education.

¹⁵In the Online Appendix, I provide additional evidence on the role of education spending on quality. I examine the impact on teacher wages, student-teacher ratio, and term length and find that reductions in wages and term length and increases in the student-teacher ratio are all significantly related to expenditure cuts, affirming the assumption that quality - and not just spending - decreased in cities across cohorts during the Depression.

In my preferred specification (3), I find that one standard deviation increase in youth unemployment led to a 0.7 to 1.6 percentage point increase in the likelihood that a young male in a Depression-era cohort graduated high school. This estimate accounts for roughly 15 percent of the cross-sectional standard deviation (10.2 percent) of high school graduation rates across cities in 1930.

The coefficient estimates on education spending, on the other hand, imply much smaller (if any) adverse effects on high school completion. According to the estimates in (3), after the onset of the Depression, cohorts that lived in cities with one standard deviation lower spending graduated at a 0.4 percentage point lower rate compared to their city peers before the Depression, approximately one-quarter of the youth unemployment estimate.

To put these numbers in historical perspective, the graduation rate of cohorts in the sample increased from approximately 43 percent in 1927 to 54.5 percent by 1936, or roughly 1.1 percentage points per year - a remarkable increase even in the context of the High School Movement. Thus, the 1934-1935 cohorts were, on average, 7.7 – 8.8 percent more likely to graduate high school than their 1927 counterparts. Therefore, the estimates here show that one standard deviation in youth unemployment accounted for 18 – 21 percent of this increase. On the other hand, education spending cuts slowed the High School Movement by roughly 4 to 5 percent. In total, the net impact of the dual shocks of the Depression resulted in a 15 percent increase in high school completion for males during the 1930s in the U.S.

The education attainment results in columns (6) – (10) should be interpreted more cautiously. In specifications (6) – (10), I observe a significant pre-trend for the 1928-29 cohorts relative to the 1926-27 cohorts on the order of 0.05 more years. This estimate roughly doubles for the later cohorts to 0.12 more years completed. Only in the sample of siblings does the effect quadruple from a statistically insignificant 0.04 for the 1928-29 cohorts to 0.16 years for the 1934-35 cohorts, as in specification (8). These results, combined with those in columns (1) – (4), indicate that the *timing* of school dropout changed for cohorts during the Depression. That is, even though youth in cities hit harder by the Depression completed more schooling even before the recession started, the average increase in attainment shifted towards the last years of secondary school. I explore the effect on the entire distribution function of education in the next section.

5.2 Who benefited the most from the Depression?

Even though around 68 percent of the sons in my sample come from a family with a father working in a blue-collar occupation, only 54 percent of high school graduates came from a blue-collar family in the 1930 cohort. This gap suggests that students from lower socio-economic households drop out of school earlier than their peers from higher socioeconomic status households. On the one hand, a recession can negatively impact poorer students disproportionately – they may be less likely to stay in school and enter the labor force full-time to support the family, either through the formal labor market or through household production. On the other hand, if a recession lowers the opportunity cost for youth to stay in school, and if poor students are the ones for whom this cost is salient, then a recession may entice them to stay in school. Did the Depression widen or narrow the high school attainment gap?

Consistent with the opportunity cost explanation, I find that the factors leading to higher education attainment during the Depression impacted students differently across socio-economic strata. In this section, I partition the main sample of sons based on parental occupation. Using coarse parental occupation categories and occupational income scores, I re-estimate the model in Equation 4.1. Tables 5 and 6 report the results for high school completion and educational attainment, respectively. In Table 7, I report them based on the quartile of the occupational income score – a continuous measure of the median total income of all persons in that occupation in 1950. Each column in Tables 5 and 6 corresponds to the sample of sons whose father reported being a professional (1), farmer (2), manager (3), clerk (4), salesman (5), craftsman (6), operative (7), service worker (8), farm laborer (9), or non-farm (common) laborer (10).

(Tables 5 and 6 around here)

I do not find any effect of youth unemployment or education spending cuts on high school completion or educational attainment for sons of professional workers. In fact, the average results from Table 4 are driven primarily by sons of craftsmen, service workers, laborers, and salesmen.

The estimates for sons of common laborers are particularly striking. One standard deviation in youth unemployment for these sons, who make up 15.3 percent of the sample, results in a 1.7 percentage point increase for the Depression-era cohorts, an astonishing 6 percent increase from the mean (28 percent). Across all categories, the sons of laborers have the lowest high school completion percentage. The result shows that they benefited the most from worse labor market opportunities for youth during the Depression. In terms of educational attainment, the Depression-era sons gained 0.155 more years, representing a 1.6 percent increase from the mean.

Occupational categories as described above include both high- and low-earners. To verify the interpretation of the previous results, I further split the sample by the occupational income score. These results, as shown in Table 7 paint a similar picture as with occupational categories – sons of fathers with highest quartile occupations were not affected, and those with bottom quartile occupations were impacted most.

(Table 7 around here)

The main takeaways from the preceding heterogeneity analysis are consistent with the opportunity cost model. Students from more privileged backgrounds typically continue to secondary school, regardless of the local labor market for youth, so we should not see pronounced effects on this group. The results should, in fact, load on the youth who typically did not finish secondary education and were likely to drop out in the first place - in the context of the model, the high κ_c individuals - which is precisely what the results indicate.

5.3 Did the Depression also increase college-going?

Even though high school completion was the most critical milestone for cohorts in the 1930s in terms of labor-market outcomes, post-secondary levels of schooling would also have important implications for the labor supply of higher-educated and higher-earning professions. To investigate, I trace the effects of the Depression at all levels of education by estimating Equation 4.1 across the cumulative distribution function of school years completed. I estimate the model using 17 outcome variables (separately) where each outcome variable S_{ijk}^p is a binary that takes the value of one if the individual completed at least p years of education and zero otherwise.

I plot the estimated interaction terms between youth unemployment and cohorts in Figure 5. Several facts stand out. First, I observe that the pre-trends for cohorts (1928-1929) were quantitatively small, and most, but not all, were significantly different from zero across education levels. Notably, one standard deviation in 1931 youth unemployment was associated with a roughly 1 percent higher likelihood of completing at least nine years for pre-period cohorts. Second, the shape of the figures indicates that youth unemployment resulted in significantly higher rates of 9th-grade entry with some attrition between grades 10 through 12. For example, comparing the first panel with the last one, I observe a jump in the estimates during the last three years of secondary school. Third, for cohorts before 1934, I do not find a substantial impact on college-going – the estimates for college entry and completion are small. For younger cohorts, in the last two panels, I do find some evidence that the Depression pushed youth into college. Primarily, however, the

Depression increased average schooling through entry and completion of secondary schools.

(Figure 5 around here)

5.4 New Deal work programs and state-level youth labor regulation

I conclude my discussion of the impact of youth unemployment and education spending on schooling by presenting a set of results regarding its interaction with two sets of policies: state-level youth labor regulations and Federal work program spending of the New Deal.

Before the National Industrial Recovery Act set a federal age minimum (16) for most occupations in 1934, each state was responsible for regulating the minimum age and schooling requirements of youth entering the labor force. Therefore, I collect data on the minimum age, grade requirements, and maximum daily and weekly hours for employment in factories and stores as of 1931 for each state from the U.S. Department of Labor Children’s Bureau (United States Department of Labor (1933)). In general, most states set a minimum age of 14 to work in factories and limited the daily and weekly work hours to 8 and 48, respectively. Typically, states had different laws for 15-16 years olds than for 16-18-year-olds before the NRA codes took effect in 1934. For time-series comparability, I focus on the restrictions for 16-18 year-olds, which were not affected by the federal legislation. I split my sample of young males into those who lived in states with (662 thousand) and without (735 thousand) restrictions for 16-18-year-old factory workers.

Federally funded work programs can potentially increase the opportunity cost of schooling and counteract the push-in effect of youth unemployment on education. However, minors did not qualify to participate in all programs; in fact, the most extensive work-relief program for young men - Civilian Conservation Corps – was not supposed to employ men below the age of 17. To proxy for New Deal spending on work-relief programs, I use the county-level data on total Works Progress Administration grants from Fishback et al. (2003). Again, I split the sample based on cohorts in cities in counties that received above and below median per-capita grants (\$55.57).

(Table 8 around here)

Table 8 reports the estimates in these subsamples. In columns (1) and (2), I check whether the effect was more pronounced for cohorts in cities in states which had stricter pre-Depression youth labor regulation statutes. I find that it is concentrated in no-regulation states - column (1) - and statistically different (p-value 0.05) from the effect in states with pre-Depression regulation - column (2). My interpretation of this result is that the opportunities available to cohorts in regulated states did not change much because of the Depression because they were limited *ex ante*.

On the other hand, the change in opportunity cost for cohorts in non-regulated states was plausibly higher because the cost for youth to drop out before the Depression in these states was higher. In columns (3) and (4), I investigate whether work relief programs instituted by New Deal legislation attenuated the effect of Depression shocks. I do not find significant differences for cohorts before 1934 – before the New Deal programs were in full effect. For the cohorts that turned 18 in 1934 – 1937, I find that the effect of unemployment was larger in places where federal spending was greater: more youth stayed in school and completed a high school degree.

6 Conclusion: the Great Depression Increased Intergenerational Education Mobility

Whether an economic crisis strengthens or weakens intergenerational bonds is ambiguous. I conclude by presenting direct evidence that, at least in the context of the Great Depression in the United States, a crisis increased educational mobility for young males.

I restrict my primary sample to linked 1930-1940 sons whose fathers can also be linked to 1940, resulting in 675 thousand son-father pairs. I collect each father’s educational attainment in 1940 and use both the son’s and father’s educational attainment to investigate mobility. Before 1930, roughly 35 percent of young males with a father who did not finish high school would go on to complete high school. Only seven years later, over 44 percent of them did. The 1930s was a decade of accelerating investments in human capital in the population, but it was also a time of upward educational mobility for young males in the United States.

More formally, I define upward mobility indicator variables $im-i$ for i in 7 to 13 that take the value of 1 if the son finishes at least i years of school and his father finishes less than i , and zero otherwise. I re-estimate the model in Equation 4.1 with these upward mobility outcomes variables $im-i$. That is, I compare the probability that a son becomes more educated than his father across cities with different Depression severity, accounting for static city-level determinants of mobility and national trends.

Table 9 reports the results. The effect of the Depression was increasingly important for mobility in grades 8 to 12, with no effect across cohorts for grade 7 completion. I find that the 1934-35 cohort was 1.5 - 2 percent more likely to be upwardly-mobile in terms of high school entry and exit than their peers in lower unemployment areas. In aggregate, the 1934-35 cohort was on average 9 percent more mobile than the 1926-1927 cohort – one standard deviation in youth unemployment can explain 22 percent of this increase, a significant and economically meaningful

share.

(Table 9 around here)

This paper used new data sources on youth unemployment and school quality during the Depression and found that children from blue-collared families obtained more education due to deteriorating labor market conditions than they otherwise would. It further showed that expenditure cuts in the publicly financed education sector attenuated this effect by 25 percent - fewer students stayed in lower-quality schools compared to higher-quality schools, suggesting an essential dual role of labor market opportunity costs and education expenditure on human capital investment decisions. Finally, while this project focused on short-term outcomes, it suggests important follow-up questions regarding how, and for whom, the Depression changed lifetime earnings and outcomes in adulthood.

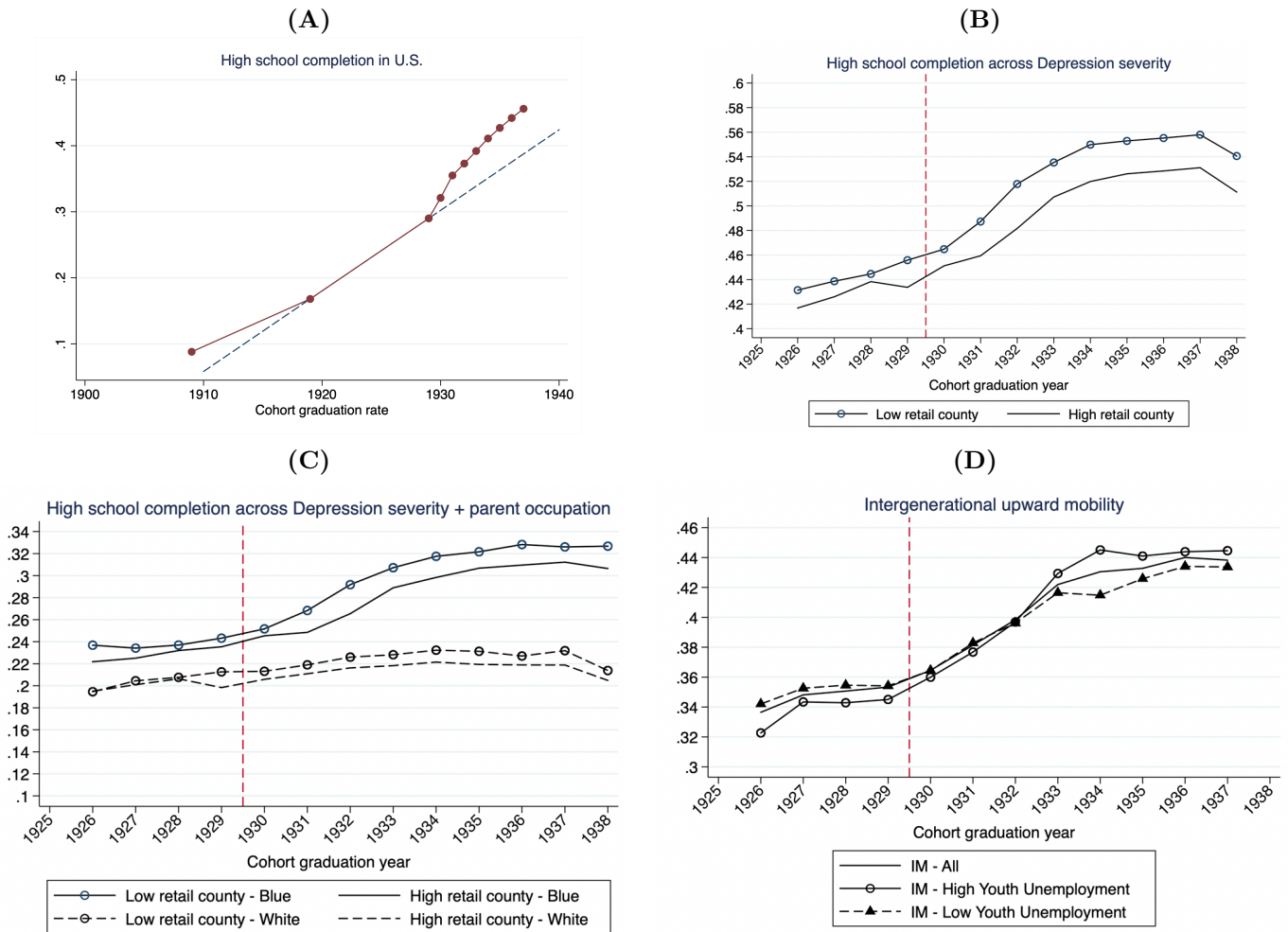
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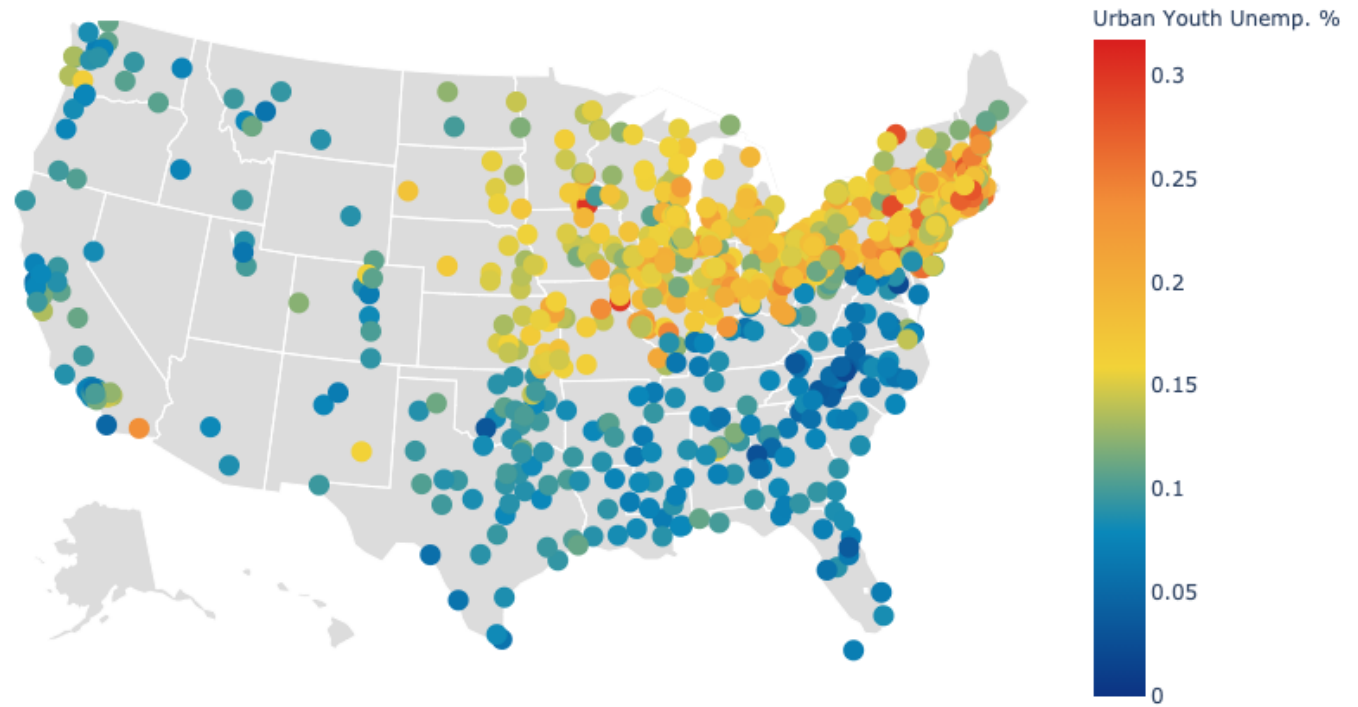
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Figure 1: High School Movement, Great Depression, and Intergenerational Upward Mobility of U.S. Males



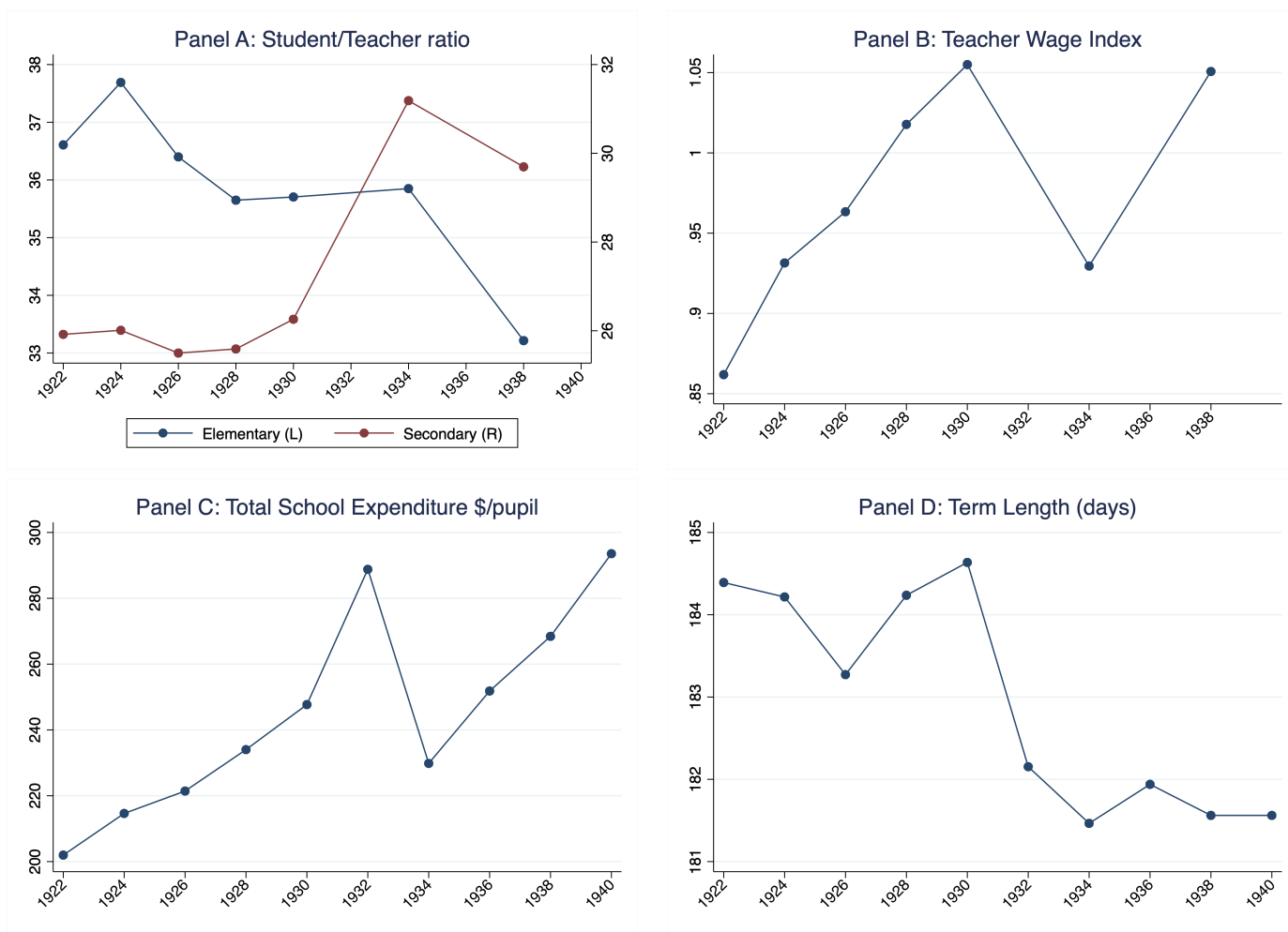
Notes: Panel (A) plots the number of high school graduates as a proportion of 17-year-olds in the United States for the years 1910, 1920, and 1930-1938. The dashed line denotes the average 1920-1930 growth rate extrapolated to earlier and later decades. The source of the data is U.S. Department of Commerce, Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1970* as reproduced in Table 19 of *120 Years of American Education: A statistical portrait* published by the National Center for Education Statistics. Panel (B) plots the proportion of high school graduates in the main sample of Census-linked young males used in this paper. Cohort graduation year is when the respondent turns 18 years old. "Low retail county" denotes all counties in the lowest tercile of retail sales growth between 1929 and 1933 and "high retail county" denotes all cohorts in the top tercile. In Panel (C), I plot the proportion of high school graduates in the linked sample based on parental occupation in 1930: Blue (blue-collar) and White (white-collar) denote the father's - or mother's, if father not present in the household - occupation category. In Panel (D), upward mobility takes the value of 1 if the son reported at least 12 years or more of schooling in 1940 while his father reported less than 12 years, and 0 otherwise. All education outcomes come from the 1940 Census for both son and father. Youth unemployment is extrapolated to all Census enumerated cities using occupation by age data from the 1931 Special Census of Unemployment and occupation shares from the 1930 Census population records. See Section 3.1 for details.

Figure 2: Map of urban youth unemployment estimates



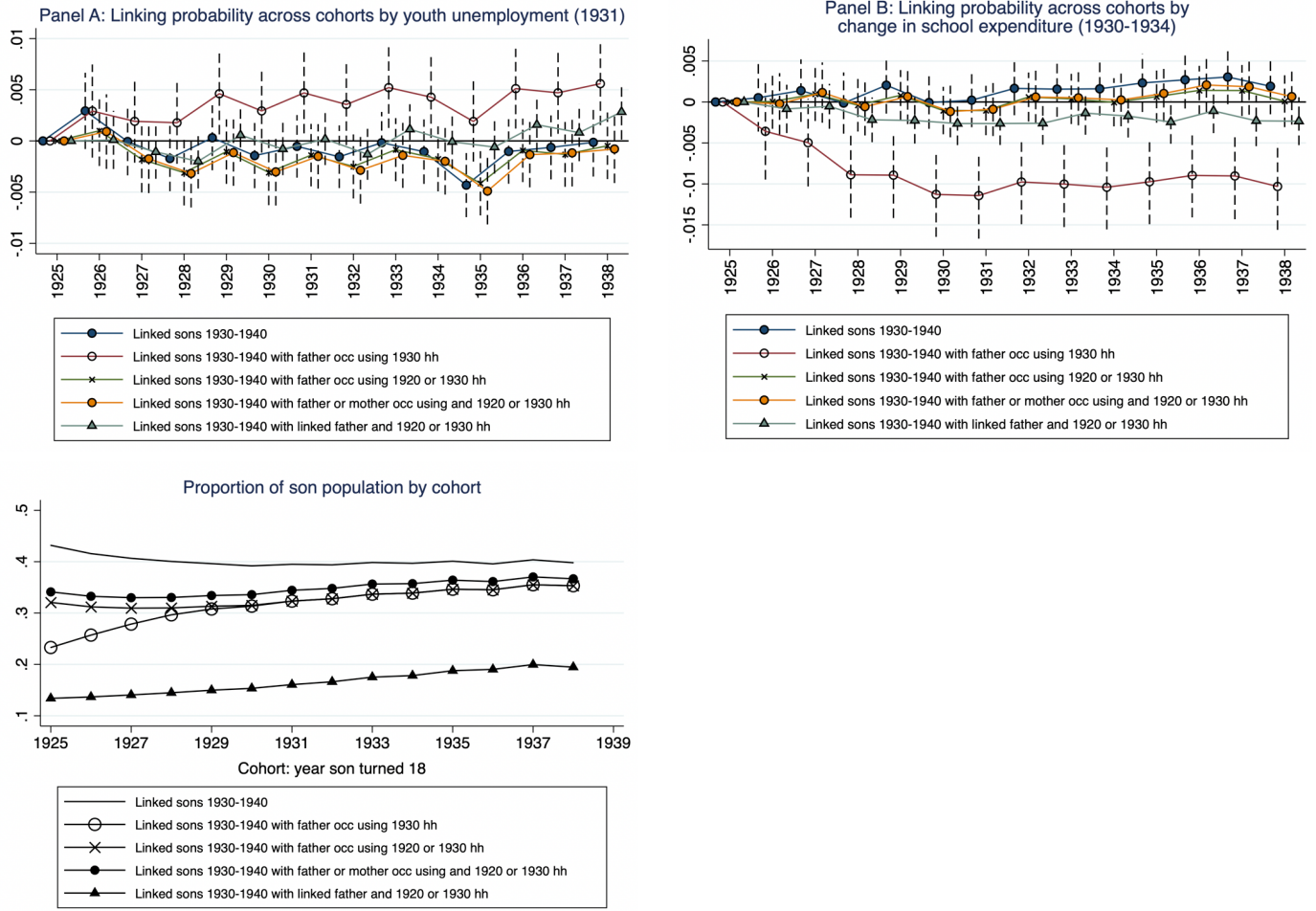
Notes: This figure plots the geographical distribution of youth unemployment in 1931. Youth unemployment is estimated using city-level occupation shares and regional occupational unemployment shares as computed from the Special Unemployment Census of 1931. Section 3.1 describes the construction in more detail.

Figure 3: Average quality of public schools over time



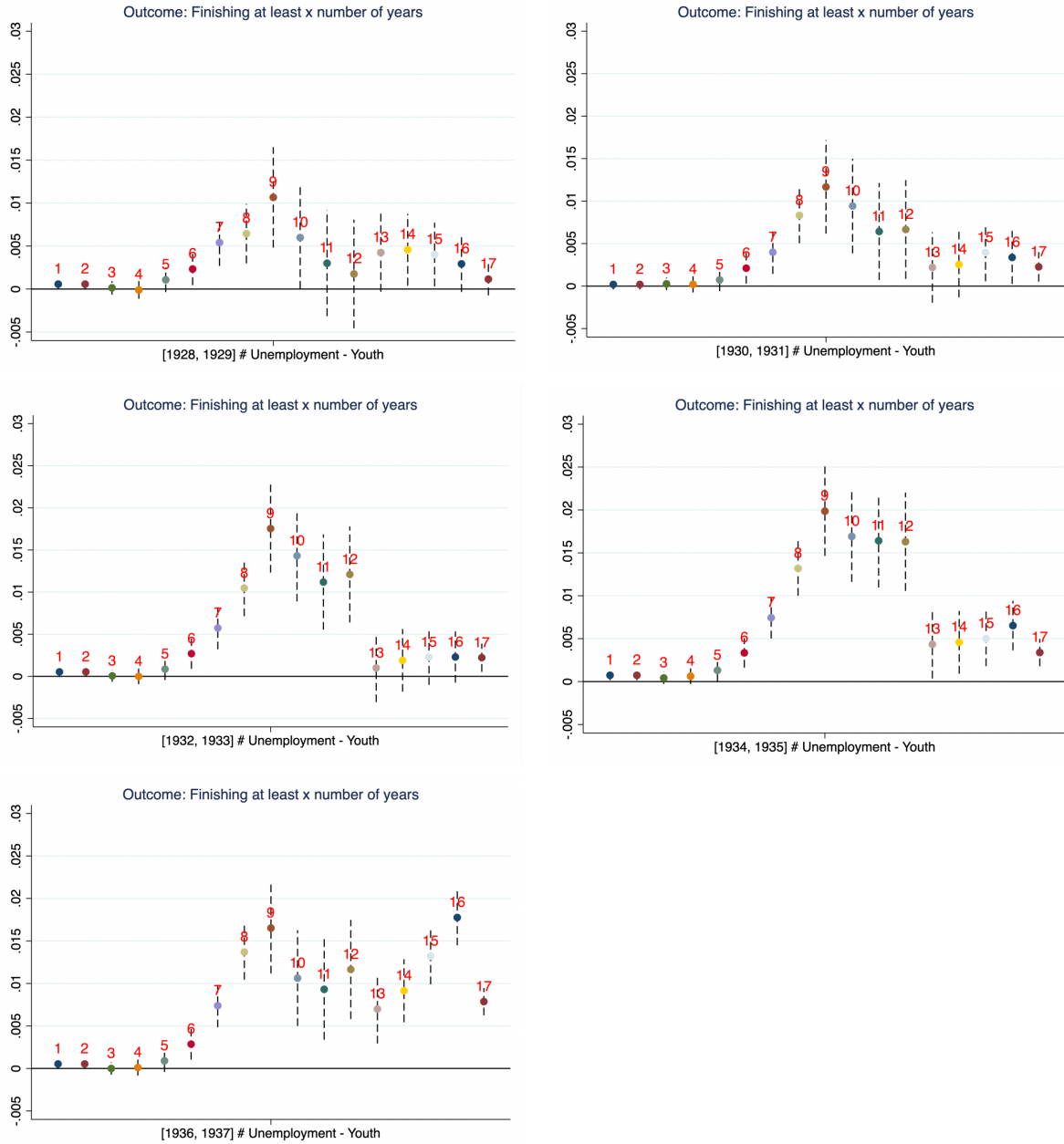
Notes: This figure plots measures of average school quality for 220 large U.S. cities between 1922 and 1938. The data comes from the *Biennial Census of Education* of the U.S. Office of Education. Panel A: Student teacher ratio is total enrolled students over total number of teachers for elementary (typically grades 1-8) and secondary (typically grades 9 - 12) schools. Panel B: Teacher wage index is average city-level teacher wages deflated by the CPI over average city-level wages in 1940. To compute average city-level wages in 1940, I average reported earnings of all white, 22-65 year old, non-teachers reporting at least 13 years of education in the full count Census records . Panel C: Total school expenditure per pupil, deflated by the CPI to 1967 dollars. Panel D: Number of school days in term.

Figure 4: Census linking probabilities across cohorts and Depression shocks



Notes: Panels (A) and (B) report the marginal probability of linking a male aged 11-22 between the 1930 and 1940 Censuses across Depression severity - youth unemployment and school expenditure cuts - using crosswalks provided by Abramitzky et al. (2020). Each line denotes the estimated coefficients of an OLS regression of a binary variable taking the value of 1 if a link to 1940 is present on the interaction terms between cohort and Depression shocks. The samples are the following: all males 11-22, all males with an employed father in 1930, all males with an employed father in either 1920 (for 18-22 year-olds) or 1930 (for 11-17 year-olds), all males with either father or mother occupation, and finally all males with non-missing father information in the 1940 Census. Bottom panel shows the proportion of total cohort captured at various steps that lead to my main sample of sons (solid circle) and father-son pairs (solid triangle).

Figure 5: Cumulative distribution function of education attainment



Notes: This figure shows the effect of youth unemployment on the cumulative distribution function of education attainment. In each panel, I present estimates of γ_2 - interaction between cohort graduating year and youth unemployment - of seventeen pooled difference-in-differences specifications of Equation 4.1. The outcome variable for each specification is an indicator variable taking the value of 1 if the individual reported at least X or more years of schooling in the 1940 Census where X is denoted in text above the estimate. Each panel presents the interaction terms for the two year cohort specified in the x-axis. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. All standard errors are clustered at the city-cohort level. Ninety-five percent confidence intervals are shown with dashed lines.

Table 1: Unemployment rates for youth in 1931 in select occupations and cities

City	Rate	Weight	Occupation
Duluth	68.1%	6.2%	Laborers : Iron and steel industries
Seattle	59.4%	1.1%	Operatives : Iron and steel industries
Chicago	55.9%	2.0%	Operatives : Paper, printing, and allied industries
Chicago	54.6%	1.6%	Operatives : Food and allied industries
Denver	54.1%	1.0%	Operatives : Iron and steel industries
Detroit	53.9%	11.5%	Laborers : Iron and steel industries
Philadelphia	53.7%	1.7%	Operatives : Electrical machinery and supply factories
Buffalo	53.2%	1.1%	Laborers : Food and allied industries
San Francisco	53.0%	1.1%	Laborers : Building construction, laborers, and helpers
Detroit	52.8%	2.0%	Mechanics
Philadelphia	52.3%	1.1%	Operatives : Leather industries
Boston	51.5%	5.0%	Operatives : Leather industries
Duluth	51.4%	3.5%	Laborers : Building construction, laborers, and helpers
Detroit	50.8%	9.0%	Operatives : Iron and steel industries
Boston	50.7%	1.1%	Operatives : Clothing industries
Buffalo	50.4%	1.1%	Operatives : Leather industries
New Orleans	50.0%	1.4%	Porters (except in stores)
Chicago	50.0%	1.0%	Laborers : Building construction, laborers, and helpers
Buffalo	49.5%	3.3%	Laborers : Building construction, laborers, and helpers
Seattle	49.1%	3.3%	Laborers : Building construction, laborers, and helpers
Boston	48.9%	2.4%	Laborers : Building construction, laborers, and helpers
Chicago	48.9%	1.8%	Mechanics
Buffalo	48.8%	1.5%	Operatives : Textile industries

Birmingham	2.8%	1.3%	Engineers (stationary), cranimen, hoistmen, etc
San Francisco	4.3%	7.9%	Servants (except cooks)
Denver	4.9%	3.5%	Bookkeepers, cashiers, and accountants
San Francisco	5.2%	2.3%	Waiters
San Francisco	5.4%	8.8%	Sailors, deck hands, boatmen, etc.
Denver	5.4%	2.1%	Retail dealers
San Francisco	7.1%	3.3%	Bookkeepers, cashiers, and accountants
Minneapolis	7.6%	3.9%	Bookkeepers, cashiers, and accountants
St. Louis	8.0%	2.8%	Bookkeepers, cashiers, and accountants
Seattle	8.1%	2.3%	Bookkeepers, cashiers, and accountants
San Francisco	8.5%	2.2%	Retail dealers
Denver	9.0%	27.6%	Clerks (except "clerks" in stores)
Seattle	9.4%	8.5%	Servants (except cooks)
Los Angeles	9.8%	3.1%	Bookkeepers, cashiers, and accountants
San Francisco	10.5%	31.0%	Clerks (except "clerks" in stores)
Seattle	10.6%	17.5%	Clerks (except "clerks" in stores)
Manhattan	11.1%	1.5%	Retail dealers
San Francisco	11.1%	2.0%	Machinists, millwrights, and toolmakers
Los Angeles	11.2%	19.9%	Clerks (except "clerks" in stores)
Manhattan	11.8%	6.5%	Salesmen and saleswomen
New Orleans	11.8%	2.7%	Bookkeepers, cashiers, and accountants
Denver	11.9%	3.2%	Laborers : Iron and steel industries
Seattle	11.9%	6.8%	Sailors, deck hands, boatmen, etc.

Notes: This table lists the largest and smallest youth unemployment rates as defined in Section 3.1 across cities in the 1931 Special Census of Unemployment. Column "Weight" refers to share of the youth labor force at the occupation-city level, as of 1930.

Table 2: Most common youth occupations and unemployment rates by region

Rank	Modal Occupation	Unemployment Rate	# Cities	Weight
Midwest				
1	Retail workers	7%	176	16%
2	Servants (except cooks)	16%	50	24%
3	Operatives: Leather industries	43%	15	24%
4	Clerks (except "clerks" in stores)	15%	15	21%
5	Laborers: Iron and steel industries	50%	6	17%
6	Operatives: Clothing industries	44%	4	19%
7	Laborers: Food and allied industries	45%	3	30%
8	Farm laborers (wageworkers)	33%	3	17%
9	Operatives: Metal industries (except iron and steel)	33%	3	17%
10	Operatives: Iron and steel industries	46%	2	17%
11	Laborers: Metal industries	34%	1	19%
12	Operatives: Clay, glass, and stone industries	31%	1	15%
Northeast				
1	Retail workers	5%	91	15%
2	Operatives: Textile industries	42%	72	31%
3	Clerks (except "clerks" in stores)	13%	61	20%
4	Servants (except cooks)	12%	40	23%
5	Operatives: Leather industries	47%	29	29%
6	Laborers: Iron and steel industries	49%	20	24%
7	Operatives: Clothing industries	44%	16	20%
8	Stenographers and typists	18%	8	14%
9	Operatives: Cigar and tobacco factories	27%	5	15%
10	Laborers: Clay, glass, and stone industries	38%	4	21%
11	Operatives : Metal industries (except iron and steel)	29%	3	14%
12	Public service - non-laborers	11%	3	38%
13	Operatives: Rubber factories	33%	2	29%
14	Farm laborers (wageworkers)	29%	1	13%
15	Operatives: Electrical machinery and supply factories	29%	1	13%
South				
1	Retail workers	5%	78	15%
2	Servants (except cooks)	8%	69	18%
3	Clerks (except "clerks" in stores)	11%	2	13%
4	Laborers : Food and allied industries	26%	1	26%
5	Stenographers and typists	15%	1	15%
6	Laborers : Iron and steel industries	42%	1	13%
7	Waiters	15%	1	15%
8	Laborers : Clay, glass, and stone industries	43%	1	15%
West				
1	Retail workers	4%	56	16%
2	Servants (except cooks)	6%	10	24%
3	Farm laborers (wageworkers)	39%	10	23%
4	Laborers : Lumber and furniture industries	29%	5	24%
5	Clerks (except "clerks" in stores)	9%	5	16%
6	Oil and gas well operatives	29%	1	20%
7	Operatives : Food and allied industries	17%	1	14%
8	Fishermen and oystermen	40%	1	12%
Total			878	

Notes: This table shows the most common occupations reported by urban youth and their estimated unemployment rates in 1931 from city-level data obtained from the Special Census of Unemployment. The column “# Cities” reports the number of cities in which the occupation listed is the most common occupation within the city. The “Weight” column reports the share of youth that hold the occupation as a proportion of all city youth workers. Midwest includes the states: IA, IL, IN, KS, MI, MN, MO, ND, NE, OH, SD, WI. Northeast includes the states: CT, MA, ME, NH, NJ, NY, PA, RI, VT. South includes the states: AL, AR, DC, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV. West includes the states: AZ, CA, CO, ID, MT, NM, NV, OR, UT, WA, WY.

Table 3: Summary Statistics

Panel A: Census data

	N	Mean	SD	Median	25 pct	75 pct
School years completed (1940)	1,600,933	10.839	2.88	11.00	8.00	12.00
I(school years completed: 8+)	1,600,933	0.917	0.28	1.00	1.00	1.00
I(school years completed: 12+)	1,600,933	0.480	0.50	0.00	0.00	1.00
I(school years completed: 13+)	1,600,933	0.189	0.39	0.00	0.00	0.00
I(mom in labor force)	1,600,933	0.113	0.32	0.00	0.00	0.00
I(dad in labor force)	1,600,933	0.862	0.35	1.00	1.00	1.00
I(blue collar occ)	1,600,933	0.598	0.49	1.00	0.00	1.00
I(white collar occ)	1,600,933	0.275	0.45	0.00	0.00	1.00
Professional	1,600,933	0.041	0.20	0.00	0.00	0.00
Farmer	1,600,933	0.008	0.09	0.00	0.00	0.00
Manager	1,600,933	0.132	0.34	0.00	0.00	0.00
Clerk	1,600,933	0.045	0.21	0.00	0.00	0.00
Sales	1,600,933	0.084	0.28	0.00	0.00	0.00
Crafts	1,600,933	0.234	0.42	0.00	0.00	0.00
Operative	1,600,933	0.169	0.37	0.00	0.00	0.00
Service	1,600,933	0.086	0.28	0.00	0.00	0.00
Farm Laborer	1,600,933	0.004	0.07	0.00	0.00	0.00
Laborer	1,600,933	0.125	0.33	0.00	0.00	0.00
Age (1930)	1,600,933	16.161	3.39	16.00	13.00	19.00
I(native)	1,600,933	0.642	0.48	1.00	0.00	1.00
I(white)	1,600,933	0.962	0.19	1.00	1.00	1.00
Siblings	1,600,933	2.677	2.01	2.00	1.00	4.00
Family size	1,600,933	5.767	2.16	5.00	4.00	7.00
Household size	1,600,933	5.933	2.19	6.00	4.00	7.00
I(homeowning household)	1,600,933	0.525	0.50	1.00	0.00	1.00
I(3+ generations in hh)	1,600,933	0.088	0.28	0.00	0.00	0.00
I(multiple family hh)	1,600,933	0.104	0.30	0.00	0.00	0.00
I(metro, not central city)	1,600,933	0.108	0.31	0.00	0.00	0.00
I(metro, in central city)	1,600,933	0.755	0.43	1.00	1.00	1.00
I(not in metro)	1,600,933	0.130	0.34	0.00	0.00	0.00
I(mother only hh)	1,600,933	0.114	0.32	0.00	0.00	0.00
I(father only hh)	1,600,933	0.034	0.18	0.00	0.00	0.00
I(mother and father present)	1,600,933	0.853	0.35	1.00	1.00	1.00
Unemployment - Youth - City Average	1,600,933	0.200	0.04	0.21	0.18	0.23
Unemployment - Father - Regional Occupation	983,106	0.249	0.15	0.26	0.11	0.37
Δ Total School Spend	1,600,933	-0.073	0.20	-0.06	-0.16	0.02
Δ Edu. Spend	1,600,933	0.043	0.13	0.06	-0.04	0.15
Ln(School expenditure) (1930)	1,600,933	5.541	0.30	5.61	5.36	5.71
Youth labor share (1930)	1,600,933	0.179	0.05	0.18	0.14	0.22
County Unemployment (1930)	1,600,933	0.199	0.09	0.20	0.15	0.22

Panel B: City-Occupations in 1930 and 1931

	N	Mean	SD	Median	25 pct	75 pct
Total under 20 workers [city, 1930]	925	1537.0	8271.5	459.0	288.0	948.0
Youth occupation categories [city, 1930]	925	67.1	25.1	59.0	49.0	79.0
Youth occupation categories w/rates [city, 1931]	925	35.8	13.3	33.0	26.0	43.0
%Youth covered by occupation categories w/rates [city, 1931]	925	73.4	10.5	74.0	65.6	82.3
%Weight per occupation [city x occ, 1930]	33,071	2.8	5.4	0.9	0.4	2.5
%Regional unemployment rate [city x occ, 1931]	33,071	25.5	11.7	24.2	16.6	33.9

Notes: Panel A presents the summary statistics of U.S. Decennial Census variables of 1930 - 1940 linked males between between the ages of 11 and 22 in 1930. Sample includes only males living in Census enumerated cities for which education spending and youth unemployment rates could be obtained (534 cities). For 18-22 year olds, household and parent characteristics come from the 1920 Census (if head-of-household in 1930) or 1930 (if child in household in 1930). Youth living in any non-households (institutions) were dropped from the sample, as were youth with unclassified parent occupations. For more details, see text. Records linked using crosswalks obtained from Abramitzky et al. (2020). Panel B shows the summary statistics of the main variables used in the construction of the youth unemployment estimate from the 1931 Special Census of Unemployment. Total under 20 workers reports the size of the under-20 labor force. Regional unemployment rate denotes the 1931 unemployment estimates for each city-occupation. Youth occupation categories is the number of occupations reported in the 1931 Census.

Table 4: Pooled difference-in-differences estimates: average effects

	Outcome: I(completed 12 years)					School years completed				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
[1928, 1929] × Δ Edu Spend	0.002 (0.002)	0.002 (0.002)	0.003 (0.002)	0.001 (0.002)	0.005 (0.003)	0.002 (0.002)	0.015 (0.011)	0.018* (0.011)	0.018 (0.014)	-0.002 (0.018)
[1930, 1931] × Δ Edu Spend	-0.000 (0.002)	0.001 (0.002)	0.002 (0.002)	0.001 (0.002)	0.008*** (0.003)	-0.000 (0.002)	0.013 (0.010)	0.015 (0.010)	0.020 (0.013)	0.018 (0.018)
[1932, 1933] × Δ Edu Spend	-0.000 (0.002)	0.001 (0.002)	0.003 (0.002)	0.004 (0.002)	0.006* (0.003)	-0.000 (0.002)	0.022** (0.010)	0.021* (0.011)	0.031** (0.013)	0.030* (0.017)
[1934, 1935] × Δ Edu Spend	0.001 (0.002)	0.002 (0.002)	0.004* (0.002)	0.005** (0.002)	0.008** (0.003)	0.001 (0.002)	0.024** (0.010)	0.023** (0.010)	0.036*** (0.012)	0.041** (0.019)
[1936, 1937] × Δ Edu Spend	0.003 (0.002)	0.002 (0.002)	0.004* (0.002)	0.004* (0.002)	0.006* (0.003)	0.003 (0.002)	0.025** (0.010)	0.025** (0.011)	0.042*** (0.013)	0.042** (0.019)
[1928, 1929] × Unemployment - Youth	0.003 (0.002)	0.002 (0.002)	0.001 (0.003)	0.002 (0.002)	-0.002 (0.005)	0.003 (0.002)	0.046*** (0.015)	0.048*** (0.017)	0.057*** (0.019)	0.037 (0.025)
[1930, 1931] × Unemployment - Youth	0.003 (0.002)	0.006*** (0.002)	0.007*** (0.003)	0.007** (0.003)	0.013*** (0.005)	0.003 (0.002)	0.060*** (0.013)	0.069*** (0.015)	0.067*** (0.018)	0.089*** (0.025)
[1932, 1933] × Unemployment - Youth	0.007*** (0.002)	0.009*** (0.002)	0.012*** (0.003)	0.012*** (0.003)	0.021*** (0.005)	0.007*** (0.002)	0.068*** (0.014)	0.086*** (0.015)	0.079*** (0.018)	0.133*** (0.025)
[1934, 1935] × Unemployment - Youth	0.013*** (0.002)	0.015*** (0.002)	0.016*** (0.003)	0.015*** (0.003)	0.023*** (0.005)	0.013*** (0.002)	0.106*** (0.013)	0.121*** (0.014)	0.106*** (0.017)	0.162*** (0.026)
[1936, 1937] × Unemployment - Youth	0.012*** (0.002)	0.010*** (0.002)	0.013*** (0.003)	0.011*** (0.003)	0.020*** (0.005)	0.012*** (0.002)	0.126*** (0.014)	0.135*** (0.015)	0.093*** (0.017)	0.164*** (0.027)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE					✓					✓
Region x Year		✓	✓	✓	✓		✓	✓	✓	✓
School Expenditure (1930) x Year			✓	✓	✓			✓	✓	✓
Youth Labor Share (1930) x Year			✓	✓	✓			✓	✓	✓
County Unemployment (1930) x Year			✓	✓	✓			✓	✓	✓
Father Unemployment (Region-Occupation) x Year				✓					✓	
E[y]	0.48	0.48	0.48	0.48	0.48	10.84	10.84	10.84	10.84	10.84
	0.10	0.10	0.10	0.12	0.69	0.10	0.11	0.11	0.14	0.72
R-sq	1,600,933	1,600,933	1,600,933	983,106	519,735	1,600,933	1,600,933	1,600,933	983,106	519,735

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation results of the pooled difference-in-differences model of Equation 4.1. The unit of observation is an individual in the sample of linked males between 1930-1940. The variable [year, year + 1] takes the value of 1 if the individual turned 18 in either year or year plus 1. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in columns (1) - (4) is an indicator variable taking the value of 1 if the individual reported 12 or more years of schooling in the 1940 Census. In columns (5) - (8), the outcome variable is the number of school years completed, again from the 1940 Census. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Column (1) controls for city fixed effects, cohort fixed effects, and household controls: number of individuals per household, household type, house ownership, metro-area, number of families in household, number of siblings, family size, and the presence of more than 2 generations within the household. Column (2) adds region by year controls and column (3) adds baseline school expenditure, county youth employment share, and unemployment rate by year fixed effects. Finally, column (4) adds household fixed effects, effectively dropping children without linked siblings from the sample. Columns (5) through (8) are structured analogously. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 5: Effect of youth unemployment and spending by parent occupation: high school completion

	Outcome: I(completed 12 years)									
	Prof.	Farmer	Manager	Clerk	Sales	Craft	Operative	Service	Farm Labor	Labor
[1928, 1929] × Δ Edu Spend	0.011 (0.007)	0.003 (0.010)	0.015*** (0.005)	0.007 (0.010)	-0.010 (0.007)	-0.000 (0.005)	-0.002 (0.005)	0.002 (0.006)	0.017 (0.015)	0.011** (0.005)
[1930, 1931] × Δ Edu Spend	0.002 (0.007)	-0.004 (0.014)	0.007 (0.005)	0.013 (0.009)	-0.000 (0.007)	-0.002 (0.005)	-0.004 (0.005)	0.005 (0.006)	0.003 (0.015)	0.003 (0.005)
[1932, 1933] × Δ Edu Spend	0.001 (0.007)	-0.006 (0.014)	0.010** (0.005)	0.011 (0.009)	-0.002 (0.007)	-0.001 (0.005)	0.007 (0.005)	0.008 (0.006)	0.005 (0.016)	0.005 (0.005)
[1934, 1935] × Δ Edu Spend	0.009 (0.007)	-0.028* (0.015)	0.011** (0.005)	0.014 (0.009)	-0.002 (0.006)	0.002 (0.005)	0.003 (0.005)	0.005 (0.007)	0.017 (0.016)	0.007 (0.005)
[1936, 1937] × Δ Edu Spend	0.006 (0.007)	-0.028** (0.014)	0.011** (0.005)	0.016* (0.009)	-0.004 (0.007)	0.004 (0.005)	0.003 (0.005)	0.008 (0.007)	0.016 (0.014)	0.003 (0.005)
[1928, 1929] × Unemployment - Youth	0.019 (0.012)	-0.011 (0.018)	-0.004 (0.009)	0.000 (0.016)	0.037*** (0.012)	0.005 (0.008)	0.001 (0.008)	0.001 (0.010)	-0.065** (0.027)	-0.002 (0.008)
[1930, 1931] × Unemployment - Youth	0.017 (0.012)	0.023 (0.024)	0.001 (0.009)	0.002 (0.015)	0.030*** (0.012)	0.011 (0.008)	0.002 (0.007)	0.007 (0.010)	-0.036 (0.028)	0.008 (0.007)
[1932, 1933] × Unemployment - Youth	0.011 (0.011)	0.004 (0.023)	0.005 (0.008)	0.015 (0.015)	0.027** (0.011)	0.014* (0.007)	0.003 (0.007)	0.020** (0.010)	-0.027 (0.028)	0.017** (0.007)
[1934, 1935] × Unemployment - Youth	0.009 (0.011)	0.018 (0.025)	0.011 (0.009)	0.021 (0.014)	0.040*** (0.011)	0.019** (0.007)	0.006 (0.007)	0.019* (0.010)	-0.038 (0.028)	0.017** (0.008)
[1936, 1937] × Unemployment - Youth	0.019* (0.011)	-0.017 (0.026)	0.005 (0.008)	0.022 (0.014)	0.036*** (0.011)	0.010 (0.008)	0.000 (0.007)	0.013 (0.010)	-0.037 (0.026)	0.010 (0.007)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	0.80	0.37	0.66	0.64	0.65	0.47	0.40	0.40	0.28	0.28
R-sq	0.05	0.13	0.07	0.08	0.07	0.07	0.07	0.07	0.09	0.05
N	65,407	12,550	210,999	71,661	133,845	375,159	270,203	138,129	6,833	199,625

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation results of the pooled difference-in-differences model of Equation 4.1. The unit of observation is an individual in the sample of linked males between 1930-1940. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in all columns is a binary taking the value of 1 if the individual reported at least 12 years of education in the 1940 Census, zero otherwise. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. The specification includes all controls as in column (3) of Table 4. Each column reports the estimated coefficients using a subsample of the son population based on the occupation of the father - or mother, if father not present in the household - in either 1930 or 1920 (see text for a detailed explanation). The occupational categories are: professional, farmer, manager, clerk, salesman, craftsman, operative, service, farm laborer, and non-farm laborer as reported by the occ1950 Census variable. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 6: Effect of youth unemployment and spending by parent occupation: school years completed

	Outcome: School years completed									
	Prof.	Farmer	Manager	Clerk	Sales	Craft	Operative	Service	Farm Labor	Labor
[1928, 1929] × Δ Edu Spend	0.001 (0.056)	0.079 (0.076)	0.084** (0.033)	0.011 (0.060)	-0.029 (0.045)	0.029 (0.030)	-0.008 (0.034)	0.045 (0.045)	0.184 (0.127)	0.046 (0.039)
[1930, 1931] × Δ Edu Spend	-0.023 (0.055)	-0.149* (0.086)	0.082*** (0.031)	0.069 (0.056)	0.014 (0.043)	0.029 (0.030)	-0.021 (0.033)	0.011 (0.044)	0.093 (0.105)	0.013 (0.039)
[1932, 1933] × Δ Edu Spend	-0.022 (0.056)	0.022 (0.092)	0.086*** (0.032)	0.015 (0.058)	0.014 (0.043)	0.031 (0.027)	0.022 (0.032)	0.056 (0.044)	0.102 (0.109)	0.017 (0.037)
[1934, 1935] × Δ Edu Spend	0.015 (0.052)	-0.028 (0.087)	0.103*** (0.032)	0.030 (0.054)	0.031 (0.041)	0.040 (0.026)	0.011 (0.031)	0.046 (0.045)	0.127 (0.120)	0.024 (0.037)
[1936, 1937] × Δ Edu Spend	-0.001 (0.050)	-0.203** (0.082)	0.086*** (0.029)	0.056 (0.053)	0.021 (0.040)	0.052** (0.026)	0.017 (0.032)	0.071 (0.044)	0.065 (0.105)	0.031 (0.038)
[1928, 1929] × Unemployment - Youth	0.071 (0.092)	-0.028 (0.125)	0.026 (0.054)	0.015 (0.092)	0.295*** (0.072)	0.059 (0.043)	0.038 (0.049)	0.010 (0.063)	-0.339* (0.200)	0.073 (0.052)
[1930, 1931] × Unemployment - Youth	0.082 (0.089)	0.020 (0.153)	0.056 (0.052)	-0.014 (0.088)	0.202*** (0.070)	0.067 (0.044)	0.035 (0.048)	0.019 (0.062)	-0.143 (0.190)	0.111** (0.049)
[1932, 1933] × Unemployment - Youth	0.004 (0.085)	-0.043 (0.131)	0.032 (0.050)	0.032 (0.085)	0.207*** (0.068)	0.091** (0.040)	0.015 (0.045)	0.079 (0.059)	-0.023 (0.181)	0.150*** (0.049)
[1934, 1935] × Unemployment - Youth	-0.023 (0.080)	0.013 (0.156)	0.102** (0.050)	0.039 (0.081)	0.229*** (0.066)	0.110*** (0.039)	0.051 (0.043)	0.107* (0.059)	-0.108 (0.172)	0.160*** (0.048)
[1936, 1937] × Unemployment - Youth	0.099 (0.076)	-0.056 (0.137)	0.070 (0.046)	0.062 (0.077)	0.218*** (0.065)	0.088** (0.038)	0.039 (0.044)	0.077 (0.058)	-0.088 (0.173)	0.122*** (0.047)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	13.09	9.93	12.01	11.68	11.78	10.74	10.32	10.22	9.27	9.52
R-sq	0.07	0.18	0.08	0.07	0.07	0.08	0.08	0.11	0.17	0.08
N	65,407	12,550	210,999	71,661	133,845	375,159	270,203	138,129	6,833	199,625

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table presents the estimation results of the pooled difference-in-differences model of Equation 4.1. The unit of observation is an individual in the sample of linked males between 1930-1940. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in all columns is the number of years completed as reported in the 1940 Census. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. The specification includes all controls as in column (3) of Table 4. Each column reports the estimated coefficients using a subsample of the son population based on the occupation of the father - or mother, if father not present in the household - in either 1930 or 1920 (see text for a detailed explanation). The occupational categories are: professional, farmer, manager, clerk, salesman, craftsman, operative, service, farm laborer, and non-farm laborer as reported by the occ1950 Census variable. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 7: Education attainment and parent occupation income score (1950) quartiles

	Outcome: I(completed 12 years)				School years completed			
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
[1928, 1929] × Δ Edu Spend	0.006* (0.003)	-0.004 (0.004)	-0.002 (0.004)	0.008** (0.003)	0.031* (0.018)	-0.002 (0.027)	0.014 (0.021)	0.051** (0.022)
[1930, 1931] × Δ Edu Spend	0.003 (0.003)	-0.005 (0.004)	0.002 (0.004)	0.005 (0.003)	0.023 (0.018)	-0.006 (0.027)	0.026 (0.020)	0.045** (0.021)
[1932, 1933] × Δ Edu Spend	0.005* (0.003)	-0.002 (0.004)	0.004 (0.004)	0.006* (0.003)	0.028 (0.018)	0.017 (0.026)	0.050*** (0.019)	0.032 (0.021)
[1934, 1935] × Δ Edu Spend	0.008*** (0.003)	-0.005 (0.004)	0.004 (0.004)	0.008** (0.003)	0.041** (0.017)	-0.014 (0.026)	0.048** (0.019)	0.049** (0.021)
[1936, 1937] × Δ Edu Spend	0.006* (0.003)	-0.001 (0.004)	0.005 (0.004)	0.005 (0.003)	0.043** (0.018)	0.008 (0.026)	0.043** (0.018)	0.032 (0.020)
[1928, 1929] × Unemployment - Youth	-0.002 (0.004)	0.016** (0.007)	0.006 (0.006)	-0.001 (0.005)	0.048* (0.025)	0.143*** (0.042)	0.070** (0.034)	0.026 (0.033)
[1930, 1931] × Unemployment - Youth	0.004 (0.004)	0.015** (0.007)	0.010* (0.006)	0.006 (0.005)	0.067*** (0.023)	0.094** (0.041)	0.067** (0.031)	0.046 (0.031)
[1932, 1933] × Unemployment - Youth	0.012*** (0.004)	0.018*** (0.007)	0.017*** (0.005)	0.007 (0.005)	0.086*** (0.023)	0.127*** (0.040)	0.109*** (0.029)	0.028 (0.030)
[1934, 1935] × Unemployment - Youth	0.012*** (0.004)	0.023*** (0.007)	0.025*** (0.005)	0.014*** (0.005)	0.106*** (0.022)	0.155*** (0.038)	0.146*** (0.027)	0.077*** (0.029)
[1936, 1937] × Unemployment - Youth	0.004 (0.004)	0.028*** (0.007)	0.013** (0.006)	0.011** (0.005)	0.083*** (0.022)	0.192*** (0.040)	0.110*** (0.028)	0.101*** (0.029)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	0.36	0.52	0.51	0.62	10.06	10.98	10.96	11.74
R-sq	0.08	0.10	0.09	0.12	0.10	0.11	0.09	0.14
N	467,017	217,901	349,513	362,862	467,017	217,901	349,513	362,862

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table presents the estimation results of the pooled difference-in-differences model of Equation 4.1. The unit of observation is an individual in the sample of linked males between 1930-1940. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in columns (1) - (4) is an indicator variable taking the value of 1 if the individual reported 12 or more years of schooling in the 1940 Census. In columns (5) - (8), the outcome variable is the number of school years completed, again from the 1940 Census. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Each column reports the estimated coefficients using a subsample of the son population based on the quartile of the father's - or mother's, if father not present in the household - occupational income score (1950) in either 1930 or 1920 as reported by the *occscore* Census variable (see text for a detailed explanation). Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 8: Depression shocks, New Deal spending, and state-level youth labor regulation

	Outcome: I(completed 12 years)				School years completed			
	Regulation:		WPA:		Regulation:		WPA:	
	No	Yes	Below Median	Above Median	No	Yes	Below Median	Above Median
[1928, 1929] × Δ Edu Spend	0.006** (0.003)	-0.001 (0.004)	0.002 (0.003)	0.004 (0.005)	0.035** (0.015)	-0.001 (0.021)	0.032** (0.016)	0.016 (0.027)
[1930, 1931] × Δ Edu Spend	0.003 (0.003)	-0.000 (0.004)	0.001 (0.003)	0.001 (0.005)	0.033** (0.014)	0.001 (0.020)	0.027* (0.015)	0.009 (0.026)
[1932, 1933] × Δ Edu Spend	0.008*** (0.003)	-0.001 (0.004)	0.004 (0.003)	0.008* (0.005)	0.039*** (0.014)	0.018 (0.020)	0.041*** (0.016)	0.045* (0.026)
[1934, 1935] × Δ Edu Spend	0.006** (0.003)	0.002 (0.004)	0.003 (0.003)	0.012** (0.005)	0.024* (0.013)	0.039** (0.019)	0.033** (0.015)	0.072*** (0.025)
[1936, 1937] × Δ Edu Spend	0.007*** (0.003)	-0.001 (0.004)	0.005** (0.003)	0.012** (0.005)	0.030** (0.014)	0.027 (0.020)	0.046*** (0.015)	0.068** (0.027)
[1928, 1929] × Unemployment - Youth	-0.001 (0.004)	0.005 (0.004)	0.003 (0.005)	0.005 (0.006)	0.034 (0.026)	0.077*** (0.026)	0.055** (0.026)	0.084** (0.032)
[1930, 1931] × Unemployment - Youth	0.010** (0.004)	0.005 (0.004)	0.007 (0.004)	0.011** (0.005)	0.092*** (0.024)	0.051** (0.024)	0.062** (0.025)	0.091*** (0.029)
[1932, 1933] × Unemployment - Youth	0.019*** (0.004)	0.011*** (0.004)	0.015*** (0.004)	0.012** (0.005)	0.115*** (0.024)	0.094*** (0.022)	0.110*** (0.026)	0.093*** (0.027)
[1934, 1935] × Unemployment - Youth	0.023*** (0.004)	0.012*** (0.004)	0.014*** (0.004)	0.026*** (0.005)	0.147*** (0.023)	0.121*** (0.022)	0.119*** (0.024)	0.161*** (0.026)
[1936, 1937] × Unemployment - Youth	0.023*** (0.004)	0.005 (0.004)	0.011*** (0.004)	0.020*** (0.005)	0.163*** (0.023)	0.128*** (0.022)	0.140*** (0.024)	0.152*** (0.029)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	0.46	0.51	0.45	0.50	10.69	11.08	10.60	10.94
R-sq	0.09	0.10	0.11	0.08	0.11	0.10	0.12	0.09
N	735,190	662,103	628,786	631,434	735,190	662,103	628,786	631,434

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation of the pooled difference-in-differences specification of Equation 4.1 using subsamples of the full sample of linked sons. The variable [year, year + 1] takes the value of 1 if the individual turned 18 in either year or year plus 1. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in columns (1) - (4) is an indicator variable taking the value of 1 if the individual reported 12 or more years of schooling in the 1940 Census. In columns (5) - (8), the outcome variable is the number of school years completed, again from the 1940 Census. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Each column presents the results using a subsample of the main sample: individuals who reported living in a state without restrictions on male youth employment in factories as of 1931, those who reported living in a state with such restrictions, those living in counties with below-median Works Progress Administration per-capita spending, and finally those living in counties with above median WPA spending. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 9: Intergenerational education mobility across Depression severity using linked father-son pairs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	up_im_7	up_im_8	up_im_9	up_im_10	up_im_11	up_im_12	up_im_13
[1928, 1929] × Δ Edu Spend	0.003 (0.003)	0.004 (0.003)	-0.002 (0.003)	-0.001 (0.003)	0.000 (0.003)	-0.001 (0.003)	-0.000 (0.002)
[1930, 1931] × Δ Edu Spend	0.001 (0.003)	0.002 (0.003)	0.001 (0.003)	0.001 (0.003)	0.002 (0.003)	0.000 (0.003)	0.003 (0.002)
[1932, 1933] × Δ Edu Spend	0.002 (0.002)	0.001 (0.003)	0.000 (0.003)	-0.002 (0.003)	-0.000 (0.003)	-0.001 (0.003)	0.004* (0.002)
[1934, 1935] × Δ Edu Spend	-0.000 (0.003)	0.001 (0.003)	0.002 (0.003)	0.000 (0.003)	0.002 (0.003)	0.002 (0.003)	0.004** (0.002)
[1936, 1937] × Δ Edu Spend	0.001 (0.002)	0.004 (0.003)	0.005* (0.003)	0.004 (0.003)	0.004 (0.003)	0.002 (0.003)	0.002 (0.002)
[1928, 1929] × Unemployment - Youth	0.002 (0.004)	0.000 (0.004)	0.005 (0.005)	0.002 (0.005)	0.002 (0.004)	-0.000 (0.004)	0.007** (0.003)
[1930, 1931] × Unemployment - Youth	0.006 (0.004)	0.005 (0.004)	0.011** (0.004)	0.010** (0.004)	0.007* (0.004)	0.007* (0.004)	0.006** (0.003)
[1932, 1933] × Unemployment - Youth	0.005 (0.004)	0.007* (0.004)	0.015*** (0.004)	0.014*** (0.004)	0.012*** (0.004)	0.013*** (0.004)	0.004 (0.003)
[1934, 1935] × Unemployment - Youth	0.005 (0.004)	0.008** (0.004)	0.016*** (0.004)	0.018*** (0.004)	0.020*** (0.004)	0.019*** (0.004)	0.007*** (0.003)
[1936, 1937] × Unemployment - Youth	-0.001 (0.003)	0.005 (0.004)	0.014*** (0.004)	0.010** (0.004)	0.010*** (0.004)	0.013*** (0.004)	0.009*** (0.003)
City FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓
E[y]	0.31	0.35	0.56	0.53	0.46	0.40	0.16
R-sq	0.06	0.05	0.03	0.03	0.04	0.04	0.04
N	675,187	675,187	675,187	675,187	675,187	675,187	675,187

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation of the pooled difference-in-differences specification of Equation 4.1 using the sample of 1930 - 1940 linked father-son pairs. The variable [year, year + 1] takes the value of 1 if the son turned 18 in either year or year plus 1. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable in column (1) is an indicator variable taking the value of 1 if the individual reported 7 or more years of schooling in the 1940 Census and his father reported fewer than 7 years of schooling. The outcome variables in Columns (2) through (7) are defined analogously for the number of years denoted by the column header. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Standard errors shown in parentheses and are clustered at the city-cohort level.

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A Education spending cuts predict lowers education quality

Using panel data on school districts, I show that decreases in education spending were concentrated on expenses related to instruction and led to a worse learning environment. I report the results of a difference-in-differences design when education quality measures are outcomes in Table [A1](#). In column (1), the outcome is average log teacher wages defined by total teacher payroll divided by the number of teachers. In column (2), the outcome is the student-teacher ratio in secondary schools. In column (3), it is the number of school days in a term.

The estimates in the first row all show a significant correlation between spending and quality. One standard deviation in spending cuts resulted in a 6.7 log point decrease in teacher wages, a 0.85 increase in the student/teacher ratio, and a 1.1 day decrease in the school year. Considering that average real teacher wages decreased by 12 percent between 1930 and 1934, the 6.7 log point decrease is significant. This is the expected result if school districts cut spending evenly across the various expenditure categories, as over 50 percent of a school district budget was dedicated to teacher wages (Figure [A.6](#)). In addition, the baseline student-teacher ratio for secondary schools in 1929 was approximately 33.5, meaning that this ratio increased only about 2.5 percent (annually) for Depression cohorts in cities with one standard deviation larger spending cuts. Likewise, the average term length in 1929 was 184 days, and the 1.1 day decrease resulting from cuts represents only a 0.6 percent reduction. Turning to the second row of Table [A1](#), I report the coefficient estimate of unemployment and find no significant impact on teacher wages or term length (columns (1) and

Table A1: Difference-in-differences estimates: school quality measures

	Teacher Wages (1)	S/T Ratio (2)	Term Days (3)
Post=1 × Δ Edu. Spend	0.067*** (0.017)	-0.852** (0.405)	1.122*** (0.357)
Post=1 × Unemployment - Youth	-0.025 (0.024)	0.852* (0.516)	-0.178 (0.659)
City FE	✓	✓	✓
Cohort FE	✓	✓	✓
Region x Year	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓
Pre period	1927-29	1927-29	1927-29
Post period	1934-38	1934-38	1934-38
R-sq	0.45	0.57	0.22
N	1,778	1,778	1,838

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation of the difference-in-difference specification of Equation 4.1. Cohorts of size 100 or more included. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. The outcomes in columns (1) - (3) are: mean teacher wages, the student-teacher ratio, and the length of the school year in days, respectively. The outcome variable in Columns (4) - (6) is ratio of high school (or more) graduates. Standard errors shown in parentheses and are clustered at the city level. Regressions are weighed by log number of of school aged children in county as of 1930.

(3)) but do find an impact on the student-teacher ratio (column (2)). To sum up, I find that education quality was directly related to expenditure cuts during the Depression.

B Data limitations and measurement validation

Admittedly, the estimated youth unemployment rate using the extrapolation procedure discussed above is an imperfect representation of local labor market opportunities for youth. In the literature, opportunity costs are typically proxied by youth/low skill (no high school diploma) wages. However, to my knowledge, wages for youth during the Depression across many cities do not exist. Conceptually, unemployment rates are the closest proxy for opportunity and, empirically, the best available systematic data.

Three sources of potential mismeasurement could impact my empirical work: (1) incomplete occupation data for cities in 1931, (2) regional, not city-level, average occupation unemployment rates, and (3) employment shares derived from aggregating 1930 census records with imperfect occupation categorization.

I take several steps to quantify the possible mismeasurement induced by (1) - (3) and find that the extrapolation procedure produces highly predictive estimates of actual values in a subsample of cities and youth welfare enrollments later in the Depression. Using the sample of 21 urban places for which I have actual unemployment rates in 1931, I estimate the true measurement error at each step (1) - (3). The scatter plot of actual vs. estimated rates for these cities, along with a 45-degree line that denotes a perfect fit, is plotted in Figure A.3. Panel (A) plots actual (total) youth unemployment rates vs. estimates computed using a weighted average of occupation rates. The difference between the two comes from missing, but relatively unimportant, data on occupations in 1931. The correlation between the two is very high (0.9), meaning the omitted occupations in 1931 do not significantly alter the variation in city-level youth rates.

Next, in Panel (B), I show the scatter plot of actual rates versus weighted averages using *regional* (versus city-level as in the previous step) unemployment rates. The weights assigned to each regional rate are actual employment shares reported by the 1930 Census publications. Though the fit worsens, there is still a robust correlation between the true and estimated rates (0.8) in the sample of 21 cities in the 1931 Unemployment Census.

Finally, in Panel (C), I plot the actual rates versus weighted averages using regional rates and aggregated employment shares from the 100 percent Census records instead of the reported employment shares from the 1930 publications. The difference between the two is that occupational categories in the 1930 complete count census were standardized to 1950 occupational categories. Even though the fit becomes worse (correlation coefficient of 0.5), it is still high considering the

small size of the sample.

As a second validation exercise, I find that 1931 youth unemployment rates predict youth welfare enrollment by 1934 in a larger sample of cities. The data from 1934 comes from the *Survey of Urban Workers on Relief in May 1934* produced by the Works Progress Administration (Wood and Palmer (1936)). Specifically, I collect data on the number of people receiving welfare payments from the WPA in 59 cities by age group. Next, I construct a measure of the relief rate (16-19-year-old males on relief rolls over the total number of 16–19-year-old males in the 1930 Census) and regress this rate on the estimated youth unemployment rates, controlling for the average relief from the WPA in the city (total relief over total population). Using ordinary least squares, I find that the youth unemployment estimate is a strong predictor (R^2 of 0.60, p-value of 0.06 with robust standard errors) of the 1934 relief rate. The residualized scatter plot of the two is shown in [Figure A.5](#).

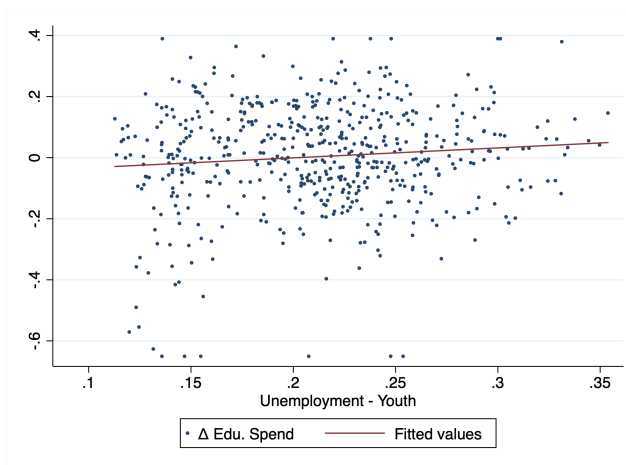
C Supplementary Figures

Figure A.1: Proportion of males living with a father in the 1930 Census



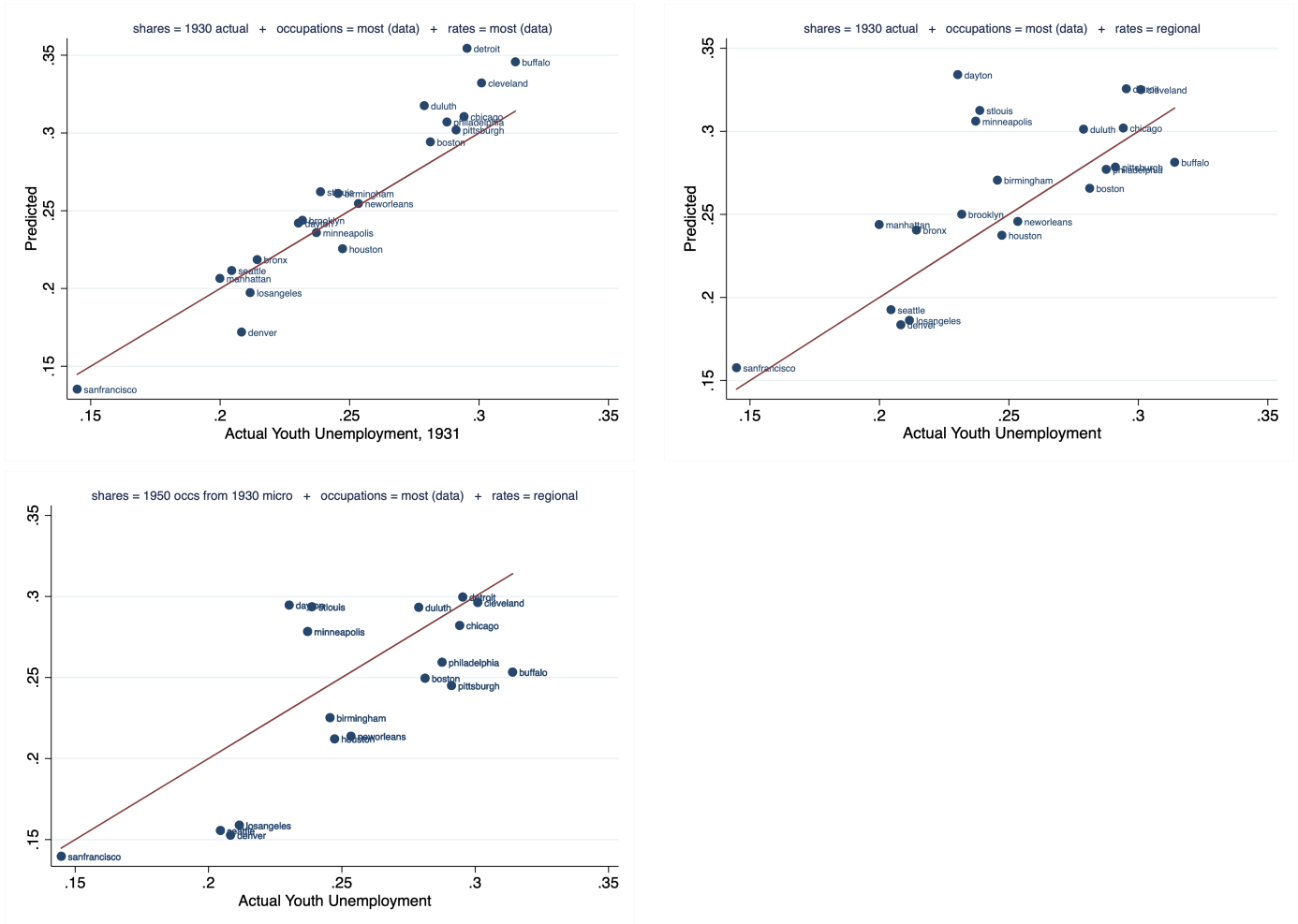
Source: Aggregation of 100 percent count records of the 1930 Decennial Census, available on IPUMS.

Figure A.2: Youth unemployment and education spending



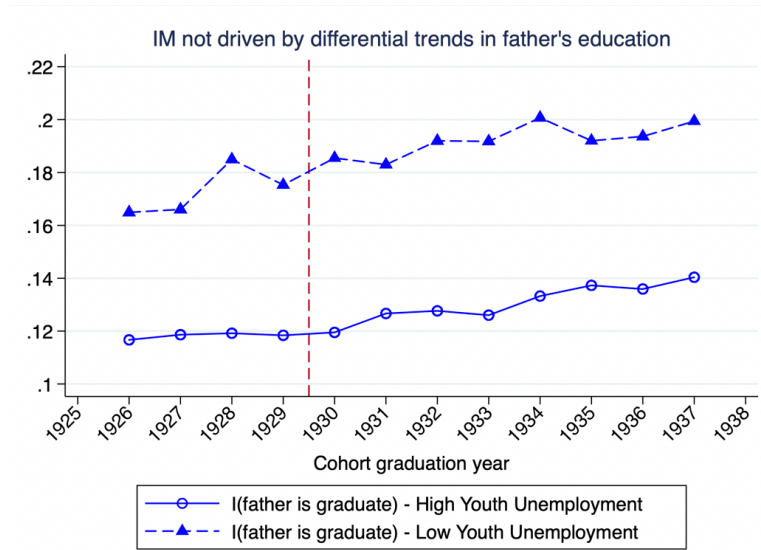
Notes: This figure shows a scatterplot between unemployment and change in education spending for 555 cities (correlation = 0.09). The cohort graduation rate is defined as the number of respondents reporting at least 12 years of schooling in the 1940 Census divided by total cohort size in a 1930-1940 linked sample. The youth unemployment rate is estimated using city-level occupation shares and the Special Unemployment Census of 1931. Section 3.1 describes the construction in more detail. Grade completion in 1936 is the average between 1935-37 and in 1930 it is the average between the 1929-1931 cohorts to minimize year selection bias. All measures were trimmed at 2-98 levels to avoid influence of outliers.

Figure A.3: Unemployment Validation



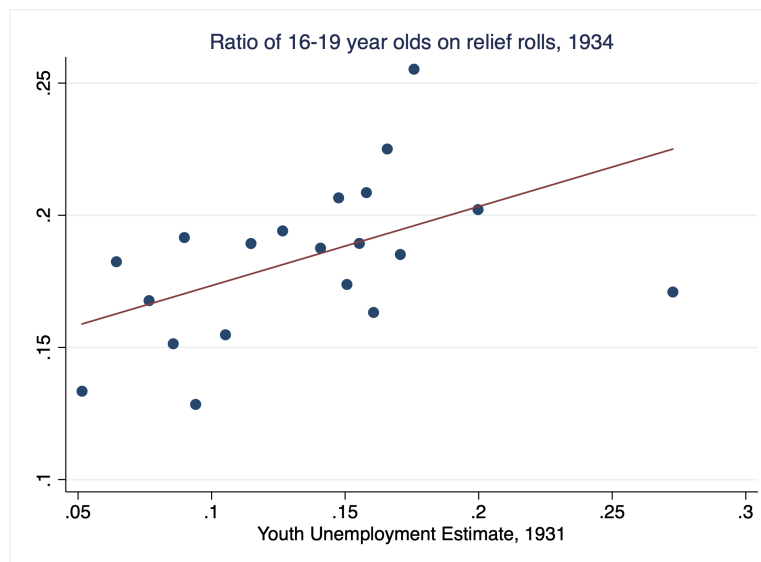
Notes: This figure plots the actual and estimated youth unemployment rates for 21 cities appearing in the 1931 Census. Title denotes how each estimated rate was constructed. See Section 3.1 for details regarding the differences between the panels.

Figure A.4: Education of fathers



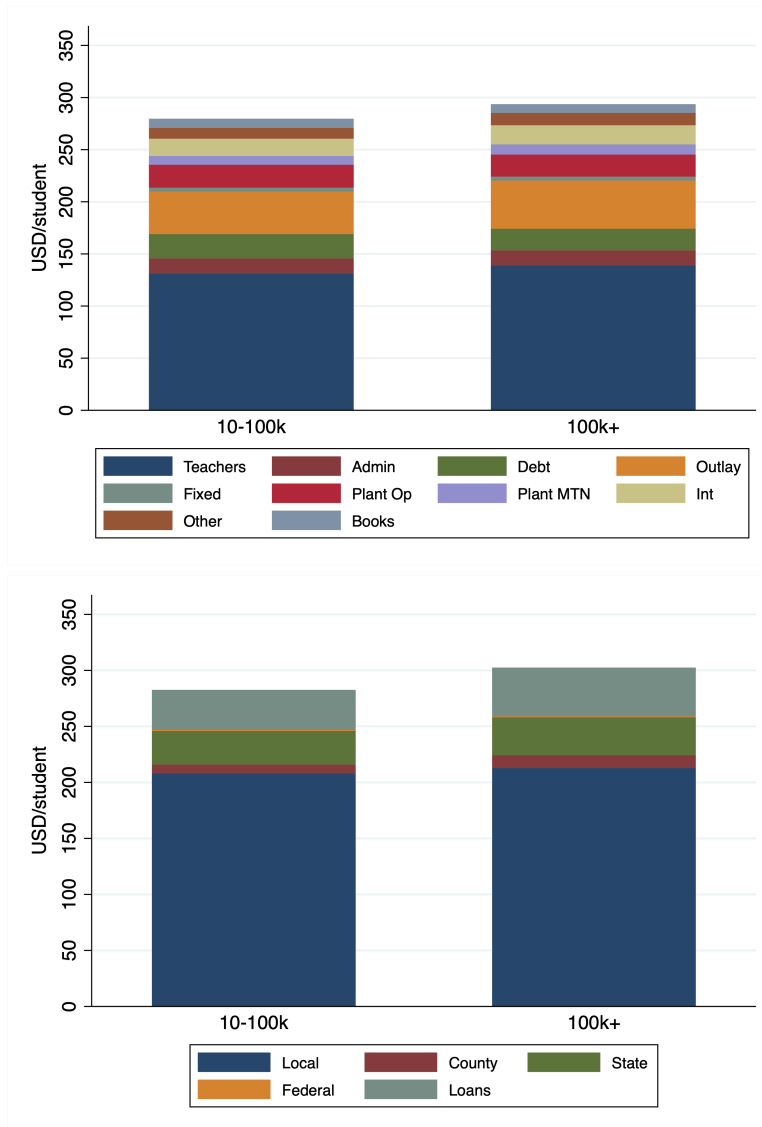
Notes: This figure plots the proportion of father high school graduates in the linked father-son sample across youth-unemployment terciles.

Figure A.5: Youth unemployment and youth workers on relief during the Depression



Notes: This figure shows a binned scatterplot of estimated youth unemployment in 1931 and the ratio of 16-19 year olds on relief rolls in 1934 at the city level. Both measures are residualized to account for the total relief rate in city. The data for 59 cities in 1934 come from a Works Progress Administration study titled *Urban Workers on Relief* (Wood and Palmer (1936)). The study reports the number of people in each age group who were, in 1934, receiving aid from the WPA. To compute relief rates, I divide the relief in each age group by total in age group as reported by city in the 1930 Census. The youth unemployment rate is estimated using city-level occupation shares and the Special Unemployment Census of 1931. Section 3.1 describes the construction in more detail.

Figure A.6: School district expenditure and revenue, 1930



Source: Biennial Survey of Education.

D Supplementary Tables

Table A2: Balance tests across linked and unlinked samples of young males aged 11-22 in 1930

	All	Linked 1930-1940	Not Linked	Difference
I(mom in labor force)	0.13 (0.34)	0.13 (0.33)	0.14 (0.35)	0.01** (0.00)
I(dad in labor force)	0.94 (0.23)	0.95 (0.22)	0.94 (0.24)	-0.01** (0.00)
I(blue collar occ)	0.70 (0.46)	0.68 (0.46)	0.71 (0.45)	0.02** (0.00)
I(white collar occ)	0.30 (0.46)	0.32 (0.46)	0.29 (0.45)	-0.02** (0.00)
Professional	0.04 (0.21)	0.05 (0.21)	0.04 (0.20)	-0.00** (0.00)
Farmer	0.01 (0.09)	0.01 (0.09)	0.01 (0.08)	-0.00** (0.00)
Manager	0.15 (0.35)	0.15 (0.36)	0.14 (0.35)	-0.01** (0.00)
Clerk	0.05 (0.21)	0.05 (0.22)	0.05 (0.21)	-0.01** (0.00)
Sales	0.09 (0.29)	0.10 (0.29)	0.09 (0.29)	-0.00** (0.00)
Crafts	0.26 (0.44)	0.27 (0.44)	0.25 (0.44)	-0.01** (0.00)
Operative	0.20 (0.40)	0.19 (0.39)	0.20 (0.40)	0.01** (0.00)
Service	0.11 (0.31)	0.10 (0.30)	0.11 (0.31)	0.01** (0.00)
Farm Laborer	0.01 (0.07)	0.00 (0.07)	0.01 (0.07)	0.00** (0.00)
Laborer	0.16 (0.36)	0.14 (0.35)	0.17 (0.37)	0.03** (0.00)
Age (1930)	16.00 (3.34)	16.02 (3.36)	15.98 (3.33)	-0.03** (0.00)
I(native)	0.61 (0.49)	0.65 (0.48)	0.58 (0.49)	-0.06** (0.00)
I(white)	0.95 (0.22)	0.96 (0.19)	0.94 (0.23)	-0.02** (0.00)
Siblings	2.70 (2.03)	2.69 (2.02)	2.72 (2.03)	0.03** (0.00)
Family size	5.84 (2.17)	5.83 (2.15)	5.84 (2.18)	0.01** (0.00)
Household size	6.00 (2.21)	5.99 (2.19)	6.01 (2.22)	0.03** (0.00)
I(homeowning household)	0.50 (0.50)	0.53 (0.50)	0.48 (0.50)	-0.05** (0.00)
I(3+ generations in hh)	0.08 (0.28)	0.09 (0.28)	0.08 (0.27)	-0.00** (0.00)
I(multiple family hh)	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)	0.00** (0.00)
I(metro, not central city)	0.11 (0.31)	0.11 (0.31)	0.11 (0.31)	-0.00 (0.23)
I(metro, in central city)	0.76 (0.42)	0.75 (0.43)	0.77 (0.42)	0.02** (0.00)
I(not in metro)	0.12 (0.33)	0.13 (0.34)	0.12 (0.32)	-0.01** (0.00)
I(mother only hh)	0.05 (0.23)	0.05 (0.22)	0.06 (0.23)	0.01** (0.00)
I(father only hh)	0.04 (0.19)	0.03 (0.18)	0.04 (0.19)	0.00** (0.00)
I(mother and father present)	0.91 (0.29)	0.92 (0.28)	0.90 (0.29)	-0.01** (0.00)
Observations	3486598	1398640	2087958	3486598

This table presents the average values of 1930 Census variables of all 11-22 urban males (as specified in Section 3) who can and cannot be linked to the 1940 Census. The last column reports the t-test between the linked and unlinked samples.

Table A3: Interaction between household composition and the impact of the Depression on high school completion

	Average	Number of families within hh		Siblings		Generations within hh	
		1	2+	0	1+	2	3+
[1928, 1929] × Δ Edu Spend	0.003 (0.002)	0.002 (0.002)	0.009* (0.005)	-0.004 (0.005)	0.004* (0.002)	0.003 (0.002)	0.003 (0.005)
[1930, 1931] × Δ Edu Spend	0.001 (0.002)	0.001 (0.002)	0.008 (0.005)	-0.008 (0.005)	0.003 (0.002)	0.002 (0.002)	-0.005 (0.005)
[1932, 1933] × Δ Edu Spend	0.004* (0.002)	0.003 (0.002)	0.008 (0.005)	-0.007 (0.004)	0.006** (0.002)	0.005** (0.002)	-0.009* (0.005)
[1934, 1935] × Δ Edu Spend	0.004** (0.002)	0.004* (0.002)	0.008 (0.005)	-0.006 (0.005)	0.006*** (0.002)	0.005** (0.002)	-0.001 (0.005)
[1936, 1937] × Δ Edu Spend	0.004* (0.002)	0.004* (0.002)	0.006 (0.005)	-0.002 (0.005)	0.005** (0.002)	0.004* (0.002)	0.004 (0.005)
[1928, 1929] × Unemployment - Youth	0.002 (0.003)	0.001 (0.003)	0.005 (0.008)	-0.004 (0.007)	0.003 (0.003)	0.002 (0.003)	0.002 (0.008)
[1930, 1931] × Unemployment - Youth	0.007** (0.003)	0.006* (0.003)	0.017** (0.008)	0.009 (0.007)	0.007** (0.003)	0.008*** (0.003)	-0.004 (0.008)
[1932, 1933] × Unemployment - Youth	0.012*** (0.003)	0.011*** (0.003)	0.024*** (0.008)	0.007 (0.007)	0.014*** (0.003)	0.013*** (0.003)	-0.002 (0.008)
[1934, 1935] × Unemployment - Youth	0.016*** (0.003)	0.015*** (0.003)	0.023*** (0.008)	0.017** (0.007)	0.018*** (0.003)	0.017*** (0.003)	0.011 (0.008)
[1936, 1937] × Unemployment - Youth	0.012*** (0.003)	0.011*** (0.003)	0.020** (0.008)	0.022*** (0.007)	0.012*** (0.003)	0.013*** (0.003)	-0.001 (0.007)
City FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓
E[y]	0.49	0.48	0.54	0.60	0.48	0.49	0.49
R-sq	0.10	0.09	0.11	0.07	0.09	0.10	0.12
N	1,397,293	1,259,071	138,222	158,957	1,238,336	1,277,110	120,183

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation of the difference-in-difference specification of Equation 4.1. The variable [year, year + 1] takes the value of 1 if the individual turned 18 in either year or year plus 1. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable is high school completion across all columns. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Each column reports the estimated coefficients using a subsample of the son population based on household composition: number of families in the household (2, 3), number of siblings (4, 5) and number of own-family generations within household (6, 7). Standard errors shown in parentheses and are clustered at the city-cohort level.

Table A4: Interaction between household composition and the impact of the Depression on school years completed

	Average	Number of families within hh		Siblings		Generations within hh	
		1	2+	0	1+	2	3+
[1928, 1929] × Δ Edu Spend	0.022* (0.012)	0.018 (0.013)	0.053 (0.037)	-0.001 (0.030)	0.026** (0.012)	0.025** (0.013)	0.002 (0.033)
[1930, 1931] × Δ Edu Spend	0.020* (0.011)	0.016 (0.012)	0.048 (0.035)	-0.048* (0.029)	0.033*** (0.012)	0.023** (0.012)	-0.003 (0.031)
[1932, 1933] × Δ Edu Spend	0.028** (0.011)	0.022* (0.012)	0.067* (0.035)	-0.039 (0.028)	0.040*** (0.012)	0.036*** (0.012)	-0.040 (0.032)
[1934, 1935] × Δ Edu Spend	0.030*** (0.011)	0.025** (0.012)	0.072** (0.034)	-0.046 (0.028)	0.043*** (0.011)	0.033*** (0.011)	0.007 (0.030)
[1936, 1937] × Δ Edu Spend	0.029*** (0.011)	0.026** (0.012)	0.068** (0.035)	-0.006 (0.029)	0.039*** (0.012)	0.032*** (0.012)	0.024 (0.029)
[1928, 1929] × Unemployment - Youth	0.055*** (0.018)	0.051*** (0.019)	0.073 (0.053)	0.047 (0.044)	0.058*** (0.018)	0.054*** (0.019)	0.059 (0.048)
[1930, 1931] × Unemployment - Youth	0.064*** (0.016)	0.056*** (0.017)	0.138*** (0.051)	0.059 (0.042)	0.070*** (0.017)	0.069*** (0.017)	0.030 (0.048)
[1932, 1933] × Unemployment - Youth	0.086*** (0.016)	0.077*** (0.017)	0.146*** (0.049)	0.005 (0.041)	0.101*** (0.017)	0.094*** (0.017)	-0.005 (0.046)
[1934, 1935] × Unemployment - Youth	0.121*** (0.015)	0.115*** (0.016)	0.155*** (0.048)	0.065 (0.042)	0.134*** (0.016)	0.122*** (0.016)	0.119*** (0.044)
[1936, 1937] × Unemployment - Youth	0.129*** (0.016)	0.123*** (0.017)	0.181*** (0.047)	0.143*** (0.041)	0.135*** (0.016)	0.136*** (0.017)	0.058 (0.043)
City FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓
Region × Year	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) × Year	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) × Year	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) × Year	✓	✓	✓	✓	✓	✓	✓
E[y]	10.86	10.82	11.26	11.56	10.77	10.86	10.85
R-sq	0.11	0.11	0.14	0.07	0.11	0.11	0.14
N	1,397,293	1,259,071	138,222	158,957	1,238,336	1,277,110	120,183

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation of the pooled difference-in-differences specification of Equation 4.1. The variable [year, year + 1] takes the value of 1 if the individual turned 18 in either year or year plus 1. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. The outcome variable is total number of school years completed across all columns. Δ Edu Spend is the standardized measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Each column reports the estimated coefficients using a subsample of the son population based on household composition: number of families in the household (2, 3), number of siblings (4, 5) and number of own-family generations within household (6, 7). Standard errors shown in parentheses and are clustered at the city-cohort level.