Crises and Intergenerational Mobility*

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Abstract

Between 1910 and 1940, the high school graduation rate in the United States increased five-fold, setting the stage for human capital-led economic growth throughout the 20th century. In this paper, I study the impact of a macroeconomic shock - the Great Depression - on these shifting schooling choices for households across the socioeconomic spectrum. I use novel 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 intergenerational mobility across cohorts. I find that worsening labor markets for youth significantly increased the school attendance of youth from poor, but not wealthy, families. The Depression not only contributed significantly to the rise of secondary education in the U.S., but it also narrowed the achievement gap between the rich and the poor.

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1 Introduction

Intergenerational education mobility, a cornerstone of equitable socioeconomic progress, has emerged as a critical area of research within the broader scope of economics (Mazumder (2005), (Chetty et al. (2020)). This mobility offers insights into the dynamics of human capital transfer across generations and its subsequent impacts on long-term economic growth. However, during periods of economic crises, the mechanisms that underpin such mobility can be severely perturbed, potentially leading to entrenched inequalities and prolonged economic stagnation. Understanding the interplay between education mobility and economic downturns is thus essential, not only for policymakers aiming to bolster the recovery but also for economists seeking a complete view of human capital evolution. Using new data and comprehensive microeconomic records during the largest crisis in the 20th century, this paper aims to shed light on this intricate relationship.

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?[1]

This paper studies the schooling behavior and intergenerational mobility of young, urban males in the U.S. during the Great Depression. I find that the Depression created unlikely winners: non-wealthy, white, urban men who would have dropped out of school absent the Depression. These men entered, and stayed in, secondary schools when their labor market opportunity cost shrank. In the aggregate, over twenty percent of the mobility in these cohorts can be explained through the labor market channels of the Great Depression.

[1] 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.

Empirically, I combine full-count Census records with school district data and local youth unemployment rates to 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).[2]

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, 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)).[3] 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

[2]State-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)).

[3]One 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 (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.

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. 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 graduation 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 state-level dynamics, 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 intergenerational 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 percentage point in-

[4]Numerous 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).

crease in the graduation rate for the Depression-era cohorts and 0.1 more years of schooling. 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. On the other hand, I do not find significant impact on cuts in education spending on graduation rates.

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 (those with lowest *ex ante* education attainment), craftsmen, service workers, and salesmen. Conversely, educational attainment was unchanged for the sons of professionals and managers. 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 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. Consistent with the heterogeneous effects of the Depression found in the main sample, I find that the upward educational mobility in the sample of linked father-son pairs increased in areas with deteriorating youth labor markets. Overall, the Depression narrowed the achievement gap between the poor and the rich.

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 the 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 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 facts about education attainment in the United States during the Great Depression. 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

[5]Other 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).

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 change in the high school graduation rate separately by local economic condition and family background where the 1930 cohorts serve as the base. The figure reveals that individuals from both backgrounds completed high school at a higher rate in worse-off counties as compared to their peers in better counties. However, 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 intergenerational 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]

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

[7] This 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 Figure A.4

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 the 1920, 1930, and 1940 Census records of young males and their fathers in Section 3.3.

3.1 Local youth unemployment rates

Summary of sources to construct youth unmployment 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	∑(3) x (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, marital 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:

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

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

[8]The 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.

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 Appendix B.

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:

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

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 U.S. Office of Education publication called the*Biennial Survey of Education*. 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 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 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:

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

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. Household characteristics and parental information that I use to conduct heterogeneity analysis, however, come from either the 1920 or 1930 Census. This is because of the well-known obstacle in studying intergenerational mobility using U.S. Census data: intergenerational links are identified only if members of both generations live in the same (physical) household.[10] 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

[9] I 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

[10] An exception was college students, who were enumerated at their "usual place of abode" and not at their college.

individual to their parents requires a link to a Census taken during their childhood.[11]

This paper focuses on youth during 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 only their older counterparts with intergenerational links as of 1930– those that finished their high-school education before the Depression and still lived with a parent in 1930 - is problematic due to obvious self-selection. Namely, this sample misses all the youth that graduated and established their own households before the Depression. Thus, I complement my sample of older cohorts by linking those who 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, nephews, and those living with older siblings as heads of households, as opposed to a parent. Third, For 18-22-year-olds in 1930, I keep only those reporting to be either a child or a head-of-household. For 18-22-year-old head-of-households, I link back to their 1920 records to find their household characteristics and parental information. For 18-22-year-olds reporting to be a child, I use household data from 1930. Finally, I drop records from any city for which I could not construct youth 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.[12] 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

[11] In 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.

[12] In my main analysis, I use records matched using any of the linking methods provided. In Table A2, I restrict the sample to sons linked with exact name matching. The results are unchanged.

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)

The linked sample is not a perfectly representative sample of the urban youth population - children with white-collar fathers, those who lived outside the Southern states, and whites are over-represented. To address this issue, I use inverse probability weighing in my empirical analysis, where the weights are created after predicting the characteristics that are associated with a successful link (see Appendix D.1). Importantly, however, 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.

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 1.3 million records of men aged 11-22 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 (64 percent) living in large metro areas (75 percent). These men come from dual-parent households (85 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 1934 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 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.

4 Empirical Framework

4.1 Crises and human capital decisions

Conceptually, there are three distinct channels of how business cycles can affect human capital decisions. The first is the youth unemployment channel: as youth unemployment increases during a recession, the likelihood of finding a job and wages decrease, and the opportunity cost of education decreases. The second channel is the education quality channel, and it works in the opposite direction: as public finances deteriorate during a recession, school quality and the value of education decreases. The third channel works through the family budget constraint: as family income decreases (due to *adult* unemployment), the opportunity cost of schooling for a working-age youth increases. We would expect households to consider the trade-offs between these channels. The data described thus far can illuminate the relative strength of these channels during the Great Depression, both directly and through various heterogeneity analyses.

4.2 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.[13] 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)

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

and youth employment opportunities. However, this was not the case.

4.3 Estimation

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

$$S_{ijk} = \alpha_j + \beta_k + \sum_{z=1}^{4}(Unemp_j T_z) \cdot \gamma_1 + \sum_{z=1}^{4}(\Delta Exp_j T_z) \cdot \gamma_2 + \sum_{z=1}^{4}(C_j T_z) \cdot \delta_1 + \Omega_i + \epsilon_{ijk} \quad (4.1)$$

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_1 - T_4 are dummy variables taking the value of 1 if k = {1926-1927}, {1928 - 1929}, {1930 - 1933}, and {1934 - 1937}, respectively. I bin cohorts in this manner to reflect the average early-Depression and late-Depression impacts. 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: indicators for 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.

The primary outcomes are whether the individual finished at least 12 years of education (binary) and the number of schooling years completed. The coefficients of interest are the vectors γ_1 and 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 those cohorts. 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. As discussed above, all regressions are weighted by inverse propensity scores derived after predicting the characteristics that are associated with a successful link (see Appendix D.1).

4.4 Control variables

I control for several observable and plausibly confounding variables in all specifications above, but I also report estimated coefficients before and after their inclusion. First, I include state-by-year fixed effects to account for possible uneven youth unemployment dynamics at the state-level after 1931. These fixed effects also flexibly account for state-level policies that affected all youth within a state dynamically throughout the Great Depression. Thus, the identifying variation relies on locally-persistent *within* state differences, relative to the initial shock in 1931. Said differently, I assume that the 1931 unemployment and school funding shocks are valid estimates of the opportunity cost channel for the later cohorts (1934-1937) after conditioning on state-level dynamics in income and unemployment. Using an alternative data source in Appendix B, I find that 1931 youth unemployment rates predict youth welfare enrollment by 1934, which provides evidence that this assumption is not prohibitively strong.

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 (all in 1930) 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]

[14]For 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.

The last major concern with control variables is the impact of New Deal relief spending. Federally funded work programs can potentially increase the opportunity cost of schooling and counteract the push-in effect of youth unemployment on education. I explore the heterogeneous impact of youth unemployment and education spending in more detail in Section 5.7.

5 Great Depression and 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 the 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 middle-class and blue-collar families: their fathers are laborers, salesmen, or craftsmen in the first three quartiles in the occupational income distribution. I rule out other (local) Depression channels that could be proxied by the change in retail sales or the change in manufacturing output, and I find that the effects are stronger for sons with fathers who were more likely to stay employed during the Depression. 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 of the pooled difference-in-differences model given in Eu-ation 4.1 using high school completion (binary) as the outcome variable in columns (1) –

[15] In the 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.

(5) and school years completed (0 to 17) in columns (6) – (10). The first column reports estimates from a baseline specification that includes only the main education spending and youth unemployment variables interacted with cohort indicators, fixed effects for both the cohort and city of residence in 1930 and household controls. The second (fifth) and third (sixth) column add state-by-year fixed effects, baseline expenditure, youth labor share, and county unemployment rate (all in 1930) interacted with cohort indicators. In columns (4) and (9), I add a proxy for the father's unemployment rate based the regional adult unemployment rate for his occupation in 1931 using data from the Special Census of Unemployment. Since not all occupations are reported in the 1931 Census publications, the sample shrinks to 800 thousand in these specifications. Finally, in columns (5) and (10), I add household fixed effects, effectively comparing outcomes across siblings within the same household. The omitted cohorts are those who turned 18 in 1926 or1927, and the 1928-1929 cohorts serve 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 to 1933 cohorts and grow for the 1934-1937 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.

In my preferred specification (3), I find that one standard deviation increase in youth unemployment led to a 0.6 (early-Depression) to 1 (late-Depression) percentage point increase in the likelihood that a young male in a Depression-era cohort graduated high school, depending on his length of exposure to the crisis. This estimate accounts for roughly 6-10 percent of the cross-sectional standard deviation (10 percent) of high school graduation rates across cities in 1930.

The coefficient estimates on education spending, on the other hand, do not show any

adverse effects on high school completion. According to the estimates in column (3), after the onset of the Depression, cohorts that lived in cities with one standard deviation lower spending graduated at a 0.2-0.4 percentage point higher rate compared to their city peers before the Depression, albeit not statistically different from zero.

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 1.1 percentage points per year - a remarkable increase even in the context of the High School Movement. Thus, the 1934-1937 cohorts were, on average, 7.7 – 12 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 9 – 13 percent of this increase.

Turning to the results on the total number of school years completed in columns (6) – (10), I again do not find significant pre-trends in the pre-Depression era cohorts. Yet, there is a sharp jump in the estimates starting with the early-Depression cohorts: one standard deviation in youth unemployment corresponds to 0.05 more years of schooling, or, roughly 1.7 percent of the standard deviation (specification (8)). This estimate doubles for the later Depression cohorts, which again gives credence to the theory that the length of exposure to the Depression mattered. These results, combined with those in columns (1) – (5), indicate that the *timing* of school dropout changed for cohorts during the Depression: 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 Section 5.5.

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?

5.3 Heterogeneity with respect to father occupation

I find that the factors leading to higher education attainment during the Depression impacted students differently across socio-economic strata: the Great Depression effected those in the middle and lower-middle class the most. I partition the main sample of sons based on parental occupation using coarse parental occupation categories and occupational income scores and 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), manager (2), clerk (3), salesman (4), craftsman (5), operative (6), service worker (7), or non-farm (common) laborer (8).

(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 professionals or managers. 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 percentage point increase for the Depression-era cohorts, an astonishing 3.6 percent increase from the mean (27 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 0.09 more years, representing a 0.94 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 father's 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 less affected than those in bottom quartile occupations.

(Table 7 around here)

5.4 Heterogeneity with respect to birth order and household composition

Even though I cannot observe family budgets, I attempt to illuminate the budget constraint channel by utilizing the rich Census data on household composition the aforementioned extrapolated adult unemployment occupation rates. Briefly, the channel hypothesizes that as family income decreases, the opportunity cost of schooling for a working-age youth increases. I use five ways of proxying for the family budget constraint, as it related to the decision faced by each son in my sample: birth order and the presence of siblings, the number of families and generations within the household, and the regional unemployment rate for the father's occupation.

(Tables 8 and 9)

Tables 8 and 9 present the main results when these various subsamples are used. Starting with Table 8, I observe that local youth unemployment was marginally more impactful on older sons (columns (3) and (6)) and those without siblings (column (1)) in the later-Depression cohorts. Conditional on age, the oldest sons reacted sooner (starting in the earl-Depression cohort) and stronger than the youngest sons within a household: the marginal eldest sons graduated at a higher rate when his opportunity cost of his labor was lowered. Additionally, those without siblings were about twice as likely to stay in school than the average son when the crisis hit. Both of these facts are consistent with a model of household bargaining where schooling was prioritized for children closest to graduation when labor markets deteriorated.

Columns (5) and (6) in Table 9 report the estimates of the main specification using only sons with fathers in occupations with below and above median unemployment. These

columns show that, when the father was more likely to be employed and the household budget constraints did not bind as much, the marginal son graduated at a higher rate when his opportunity cost of his labor was lowered. This evidence is, again, consistent with a basic budget constraint model where sons in households with tight budget constraints were called upon to provide during the crisis.

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, which is precisely what the results indicate. Additionally, the birth order and family composition results indicate,that household bargaining in the presence of siblings and the family budget constraint were important determinants of how the crisis impacted human capital decisions.

5.5 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. 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 the three pooled 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. However, 12th grade completion was not different across youth unemployment in the pre-Depression cohorts. Second, the shape of the fig-

ures 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 0.5 - 1.5 pp 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 panel, 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)

5.6 Robustness to other channels: retail sales and manufacturing

The results I have described so far could be due to factors that merely correlate with youth unemployment measures but function through a different, non-opportunity cost, channel. In order to provide some evidence that youth unemployment, and not Great Depression severity in general, is the main channel, I perform two additional analyses.

(Figure 6 around here)

First, I include the change in per capita retail sales between 1933 and 1929 (county-level) by year fixed effects to the specification. Retail sales is one widely used and available proxy for Depression severity. The blue line in both panels of Figure 6 shows the cohort coefficient estimates on youth unemployment when retail sales is included in the specification. Once included, I observe no additional explanatory effect of retail sales, and the estimates on the interaction term between Depression-era cohorts and youth unemployment remain significant and large. Similarly, I next include the log county level change in manufacturing output between 1933 and 1929 by year fixed effects. The resulting coefficient estimates on youth unemployment are plotted in the red line of the same figure. Again, I see no discernible attenuation ones this local shock is included in the specification.

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

I culminate 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 and relief 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 (WPA, columns (3) and (4))) grants and the Federal Emergence Relief Act (FERA, columns (5) and (6)) from Fishback et al. (2003). I split the sample based on below and above median per-capita gspending.

(Table 10 around here)

Table 10 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 do not find strong evidence that the effect

was concentrated in particular regulation states. In columns (3) through (4), I investigate whether work relief programs instituted by New Deal legislation attenuated the effect of Depression shocks. Again, 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 marginally larger in places with below-median federal spending. Finally, in columns (5) and (6), I find larger estimates in counties with below-median FERA spending, though significant pre-trends in the above-median group make the comparison uninformative. In all, the evidence for a meaningful interaction between the New Deal and local unemployment shocks on human capital decisions of young urban males is mixed.

6 Conclusion: the Great Depression Increased Intergenerational Education Mobility

Whether an economic crisis strengthens or weakens intergenerational bonds is ambiguous because the costs and benefits of schooling can change differently for families in different socioeconomic strata. In this paper, I have so far shown that a crisis (the Great Depression) was a boon to poor urban males during an important period in the development of the American economy. I conclude by presenting direct evidence that the Depression increased educational mobility for young males by lowering their opportunity cost of schooling.

Starting with the original 1.3 million linked sons records, I restrict my samle to those whose fathers could also be linked to 1940, resulting in over 590 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 not only a decade of accelerating investments in human capital in the population, but it was also a time of upward educational mobility in the United States.

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 strictly 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 youth unemployment and public education spending cuts, accounting for static city-level determinants of mobility, state-level dynamics, household characteristics, and baseline city characteristics.

Table 11 reports the results. The effect of opportunity cost was increasingly important for mobility in grades 9 to 12, with no parallel effect across cohorts for grade 7 or 8 completion. I find that the 1930-1933 cohorts were 1.3 and 0.7 percent more likely to be upwardly-mobile in terms of high school entry and exit, respectively, than their peers in lower youth unemployment areas. Given longer exposure to the Depression, the 1934-1937 cohorts were even more mobile. The quantitative significance of the Depression on mobility was remarkable: taking the 1935 cohort as an example, a one standard deviation in youth unemployment can explain roughly 20 percent of the 9 percentage point increase in mobility of this cohort relative to the 1926-1927 cohorts.

(Table 11 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, suggesting an essential role of labor market opportunity costs on human capital investment decisions. While this project focused on short-term outcomes *during* the crisis itself, it opens up interesting and important follow-up questions regarding how, and for whom, the Depression changed *lifetime* earnings and outcomes in adulthood.

With new micro-level data from later Census as well as

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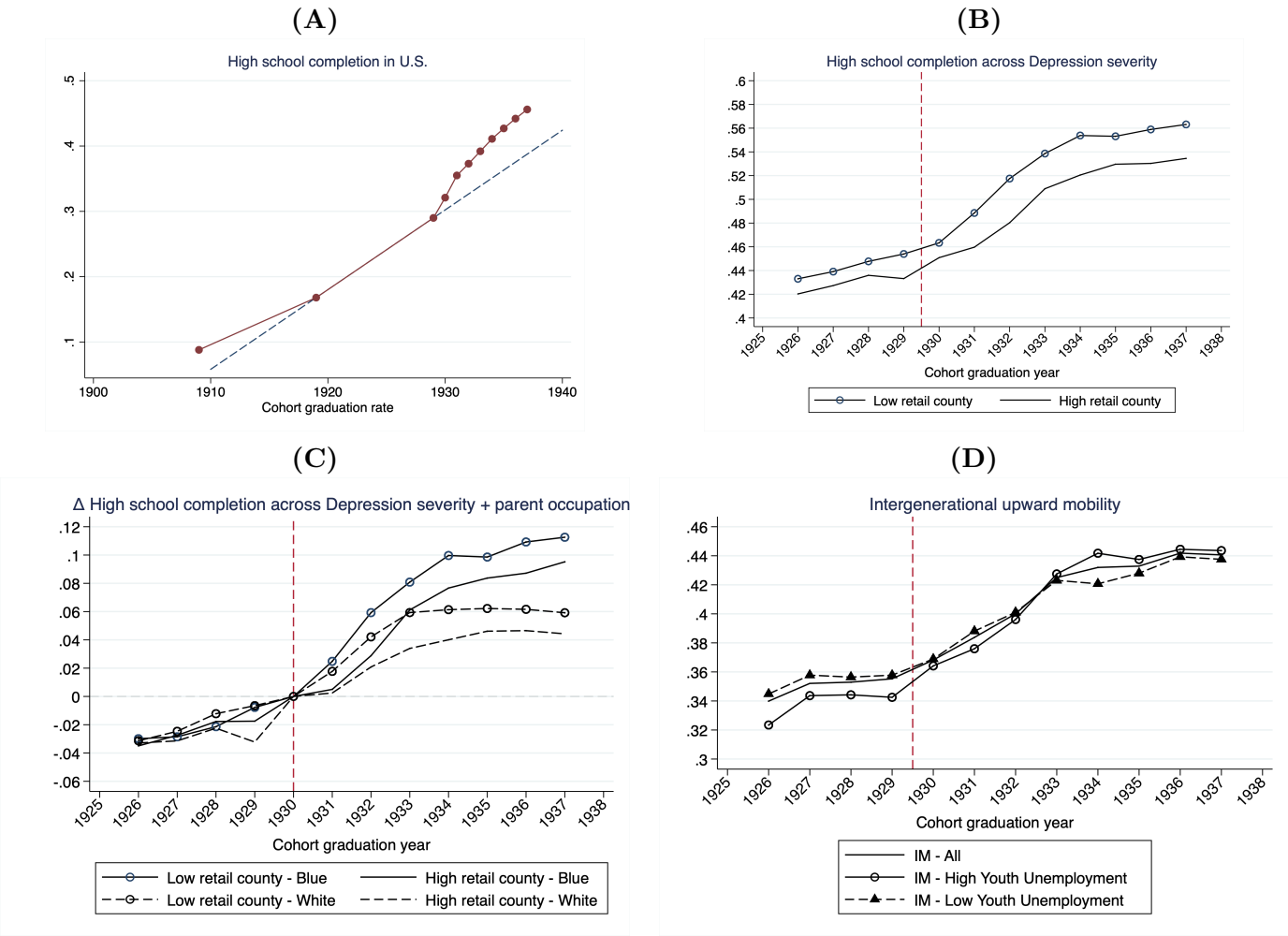
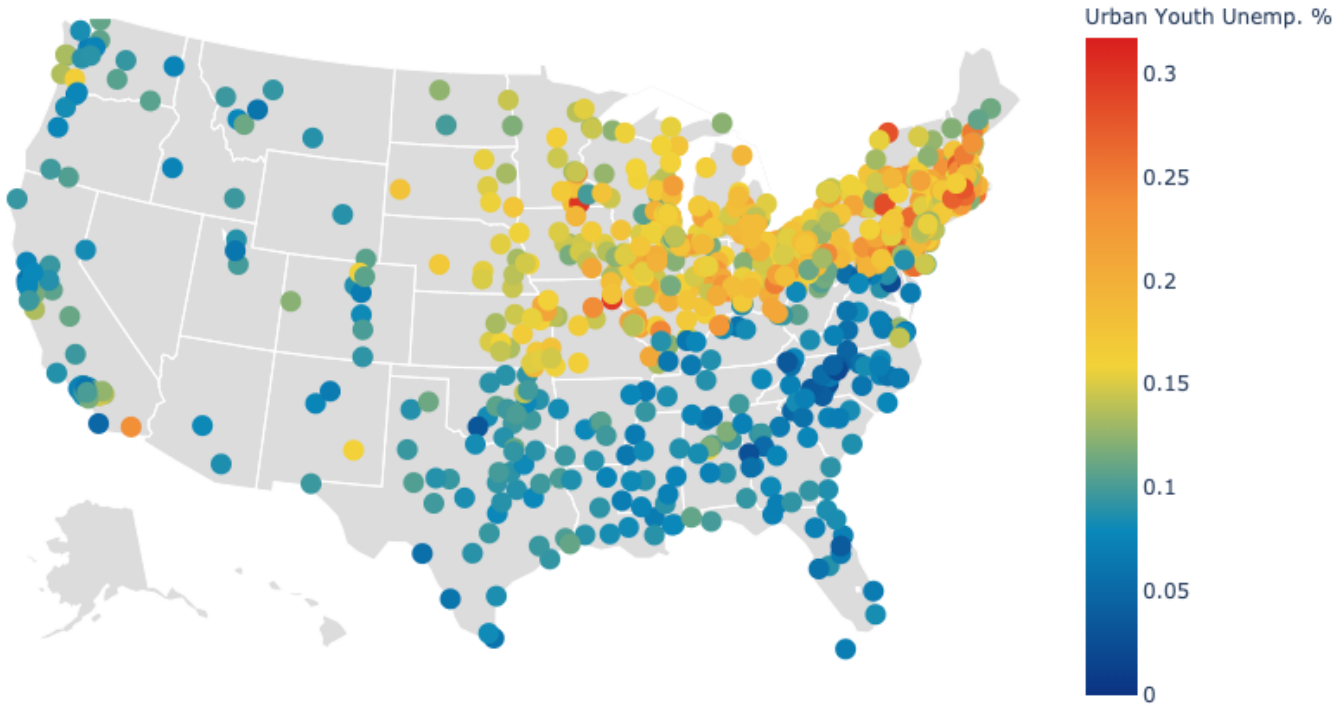
Figure 1: High School Movement, Great Depression, and Upward Mobility of U.S. Males



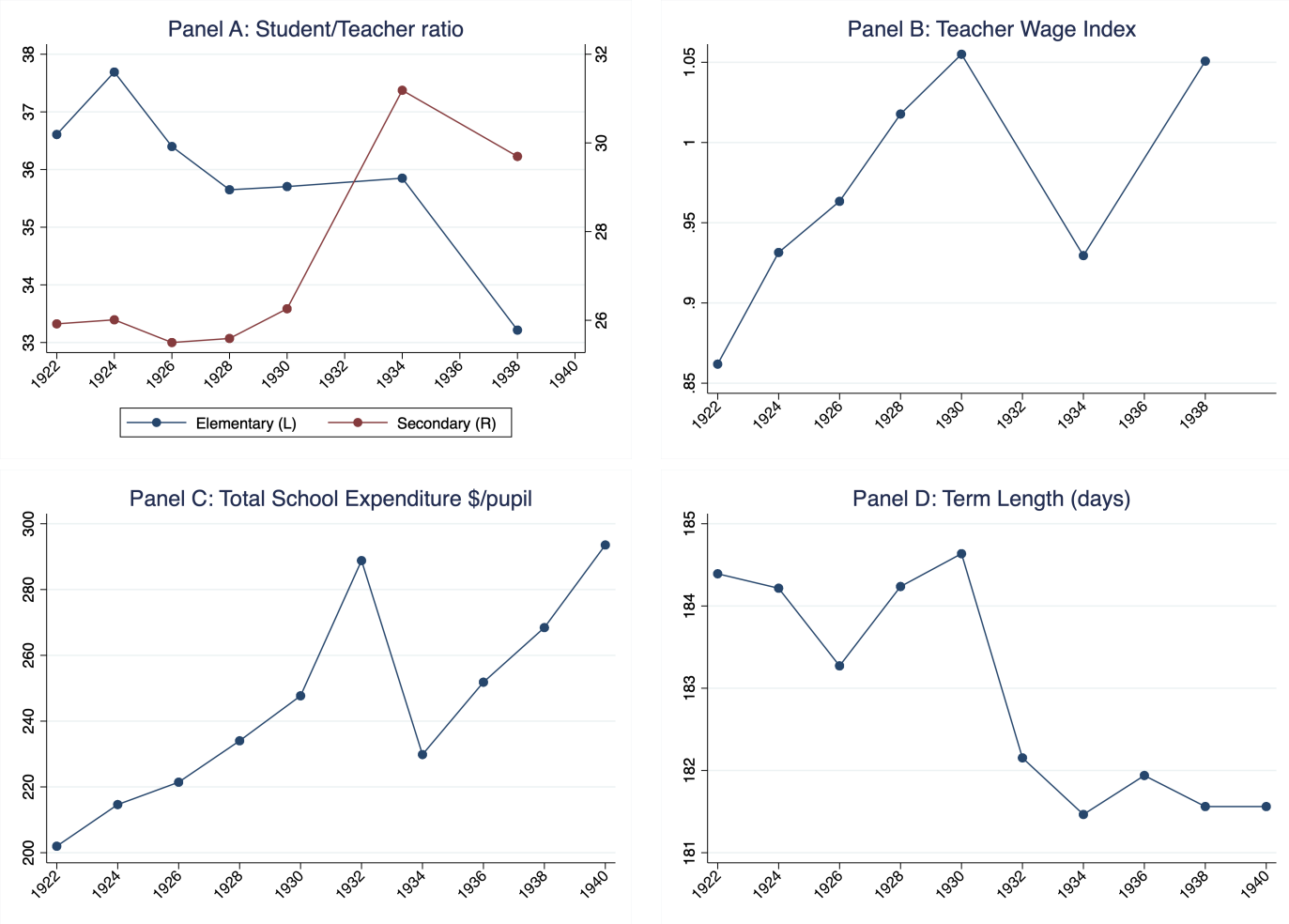
Notes: Panel (A) plots the number of high school graduates as a propotion of 17-year-olds in the United States for the years 1910, 1920, and 1930-1938. The dashed line denotes the average 1920-1930 growth rate extrapolated to earlier and later decades. The source of the data is U.S. Department of Commerce, 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 change from 1930 in the average high school graduation rates for 4 types of cohorts: those in "low retail" counties with a blue-collar (solid line, circle) and white-collar (dashed line, circle) parent and those in "high retail" counties with a blue-collar (solid line, no circle) and white-collar (dashed line, no circle) parent. The 1930 graduation rates (%) for those groups are: 37, 68, 36, 67, respectively. 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



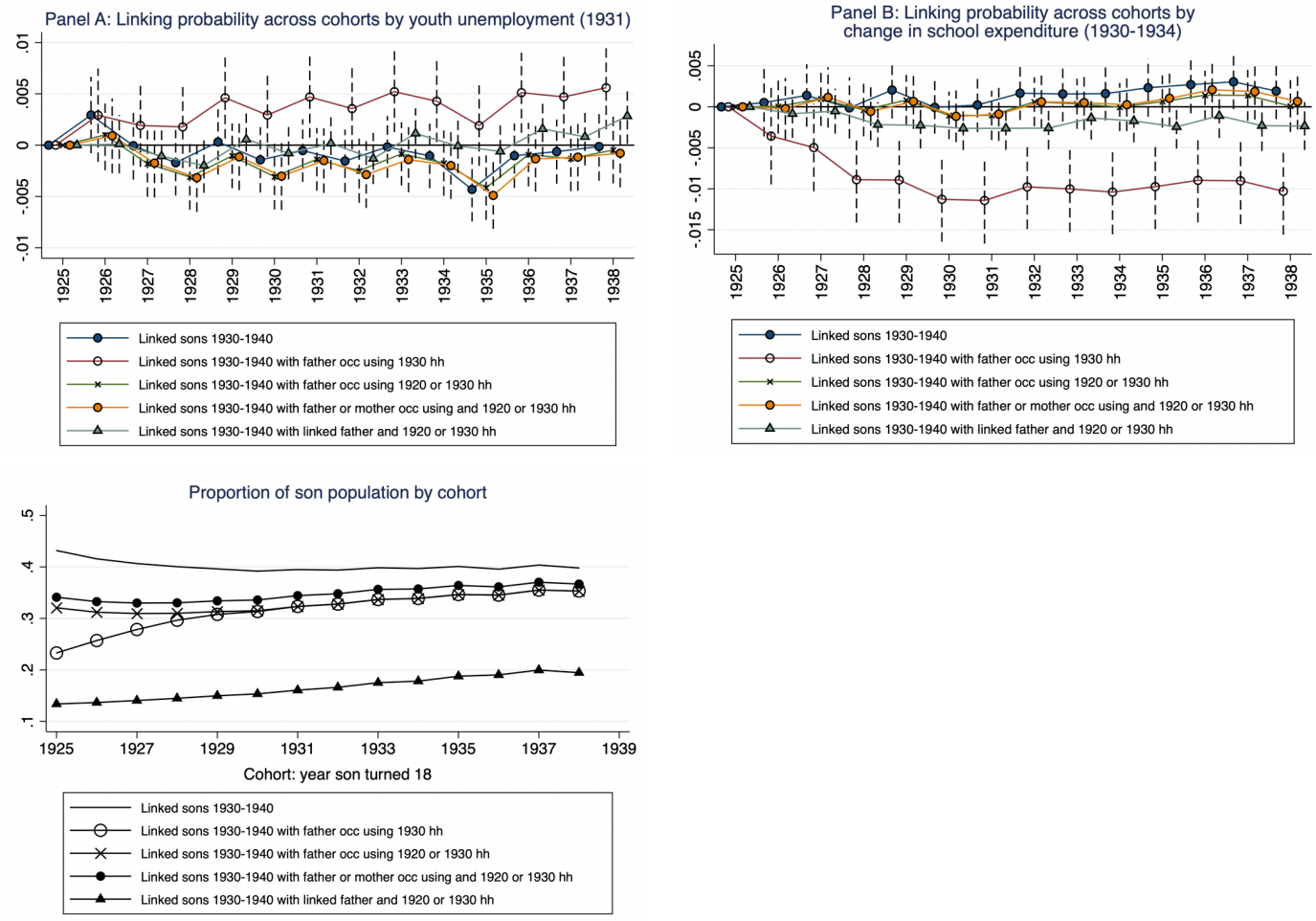
Notes: This figure plots the geographical distribution of youth unemployment in 1931. Youth unemployment is estimated using city-level occupation shares and regional occupational unemployment shares as computed from the Special Unemployment Census of 1931. Section 3.1 describes the construction in more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

Figure 3: Average quality of public schools over time



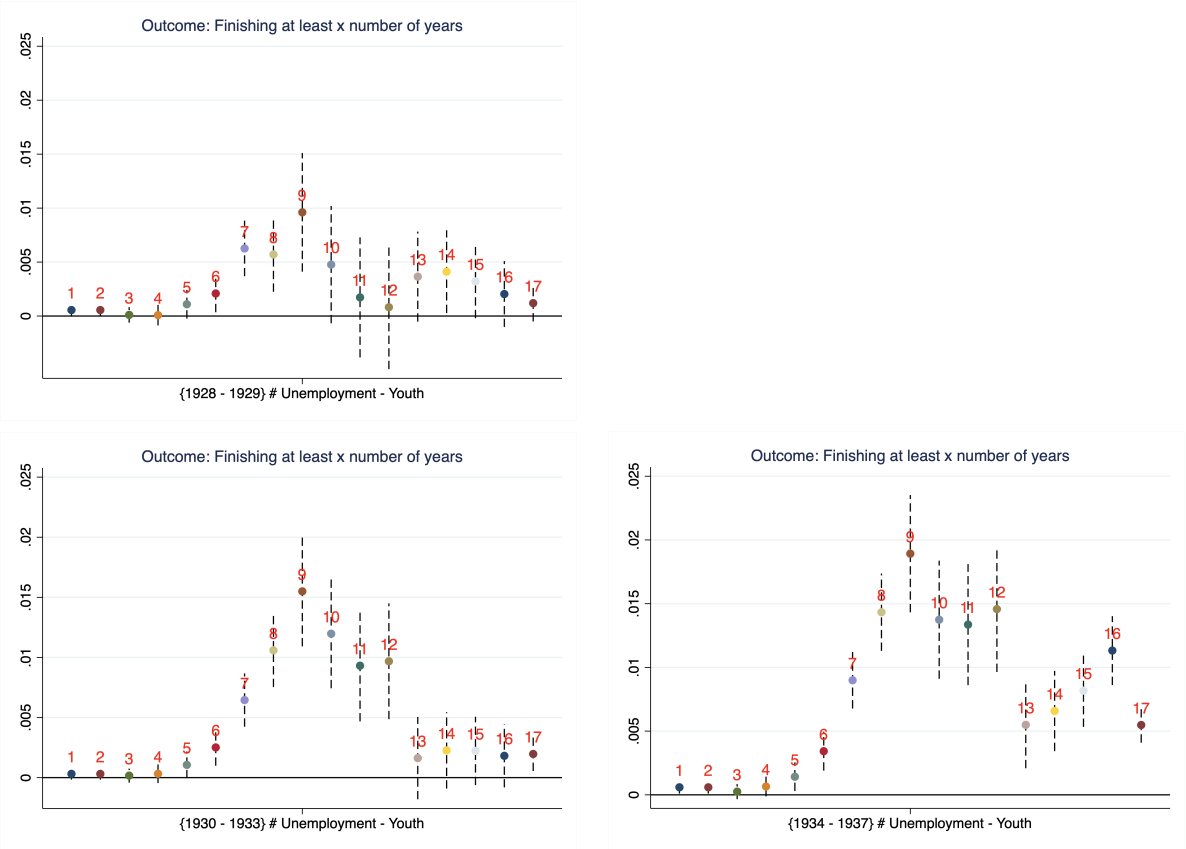
Notes: This figure plots measures of average school quality for 220 large U.S. cities between 1922 and 1938. The data comes from the *Biennial Census of Education* of the U.S. Office of Education. Panel A: Student teacher ratio is total enrolled students over total number of teachers for elementary (typically 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 divided by the average city-level wages of all workers with at least 2 years of post-secondary education 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



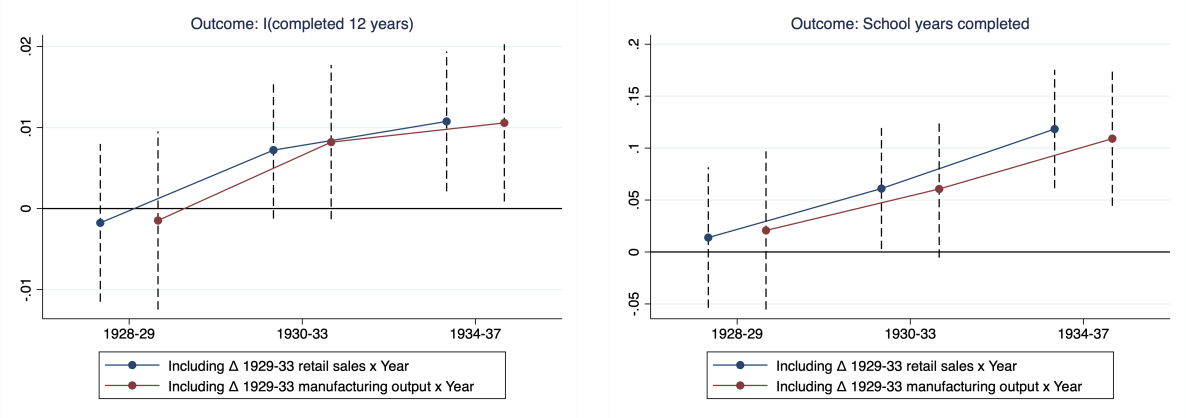
Notes: Panels (A) and (B) report the marginal probability of linking a male aged 11-22 between the 1930 and 1940 Censuses across Depression severity - youth unemployment and school expenditure cuts - using crosswalks provided by Abramitzky et al. (2020). Each line denotes the estimated coefficients of 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: Opportunity and the 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 pooled cohorts specified in the x-axis. Cohorts that turned 18 in 1926 or 1927 serve as the omitted group. Unemployment - Youth is the standardized (mean zero, standard deviation one) measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Standard errors are clustered at the city-cohort level and regressions are weighted for representativeness using inverse propensity scores. Ninety-five percent confidence intervals are shown with dashed lines.

Figure 6: Robustness to other local shocks



Notes: This figure plots the coefficient estimates of $Cohort$ x $Unemployment - Youth$ in the pooled difference-in-differences model of Equation 4.1 when two other local shock variables - change in lop per capita county retail sales and in county log manufacturing output between 1933 and 1929 - are included in the specification. Controls include city fixed effects, cohort fixed effects, state by year fixed effects, household controls, and baseline school expenditure (1930), county youth employment share (1930), and unemployment rate (1930) by year fixed effects. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

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), cranemen, 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 occupations with the largest and smallest youth (10-19 years old) 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 10-19 year olds 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)	1,306,262	10.885	2.86	11.00	9.00	12.00
I(school years completed: 8+)	1,306,262	0.920	0.27	1.00	1.00	1.00
I(school years completed: 12+)	1,306,262	0.486	0.50	0.00	0.00	1.00
I(school years completed: 13+)	1,306,262	0.192	0.39	0.00	0.00	0.00
I(blue collar occ)	1,306,262	0.596	0.49	1.00	0.00	1.00
I(white collar occ)	1,306,262	0.278	0.45	0.00	0.00	1.00
Professional	1,306,262	0.041	0.20	0.00	0.00	0.00
Manager	1,306,262	0.134	0.34	0.00	0.00	0.00
Clerk	1,306,262	0.044	0.21	0.00	0.00	0.00
Sales	1,306,262	0.084	0.28	0.00	0.00	0.00
Crafts	1,306,262	0.236	0.42	0.00	0.00	0.00
Operative	1,306,262	0.169	0.37	0.00	0.00	0.00
Service	1,306,262	0.083	0.28	0.00	0.00	0.00
Laborer	1,306,262	0.123	0.33	0.00	0.00	0.00
Age (1930)	1,306,262	16.177	3.38	16.00	13.00	19.00
I(native)	1,306,262	0.634	0.48	1.00	0.00	1.00
I(white)	1,306,262	0.969	0.17	1.00	1.00	1.00
Siblings	1,306,262	2.669	2.00	2.00	1.00	4.00
Family size	1,306,262	5.757	2.15	5.00	4.00	7.00
Household size	1,306,262	5.923	2.19	6.00	4.00	7.00
I(homeowning household)	1,306,262	0.528	0.50	1.00	0.00	1.00
I(not in metro)	1,306,262	0.133	0.34	0.00	0.00	0.00
I(mother and father present)	1,306,262	0.854	0.35	1.00	1.00	1.00
Unemployment - Youth - City Average	1,306,262	0.200	0.04	0.21	0.18	0.23
Unemployment - Father - Regional Occupation	800,373	0.250	0.15	0.26	0.11	0.37
Δ Total School Spend	1,306,262	-0.074	0.20	-0.06	-0.17	0.02
Δ Edu. Spend	1,306,262	0.041	0.13	0.06	-0.04	0.15
Ln(School expenditure) (1930)	1,306,262	5.538	0.30	5.61	5.35	5.71
Youth labor share (1930)	1,306,262	0.178	0.06	0.18	0.14	0.22
County Unemployment (1930)	1,306,262	0.197	0.08	0.20	0.14	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 (when available) if the individual reported as a head-of-household in in the 1930 Census. Otherwise, they come from the 1930 Census, Youth living in any non-households (institutions) were dropped from the sample, as were youth with parents in unclassified occupations. Census records were 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 Special Census of Unemployment.

Table 4: Pooled difference-in-differences estimates

	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	0.001	-0.000	-0.003	-0.000	0.002	0.001	-0.009	-0.014	-0.048*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.014)	(0.015)	(0.018)	(0.026)
1930 - 1933 × Δ Edu Spend	0.001	-0.000	-0.001	-0.001	0.004	0.001	-0.006	-0.018	-0.005	0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.012)	(0.013)	(0.015)	(0.023)
1934 - 1937 × Δ Edu Spend	0.003*	-0.001	-0.002	-0.002	0.004	0.003*	-0.008	-0.020	-0.009	0.017
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.012)	(0.013)	(0.015)	(0.025)
1928 - 1929 × Unemployment - Youth	0.003	0.001	-0.003	-0.003	-0.004	0.003	0.026	0.006	0.019	0.039
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.002)	(0.016)	(0.020)	(0.026)	(0.036)
1930 - 1933 × Unemployment - Youth	0.007***	0.010***	0.006**	0.009***	0.019***	0.007***	0.084***	0.054***	0.064***	0.120***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.002)	(0.013)	(0.017)	(0.021)	(0.033)
1934 - 1937 × Unemployment - Youth	0.016***	0.014***	0.010***	0.011***	0.025***	0.016***	0.143***	0.112***	0.093***	0.223***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.002)	(0.013)	(0.017)	(0.021)	(0.033)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓		✓	✓	✓	✓	
Household FE					✓					✓
State x Year		✓	✓	✓	✓		✓	✓	✓	✓
School Expenditure (1930) x Year			✓	✓	✓			✓	✓	✓
Youth Labor Share (1930) x Year			✓	✓	✓			✓	✓	✓
County Unemployment (1930) x Year			✓	✓	✓			✓	✓	✓
Father Unemployement (Region-Occupation) x Year				✓					✓	
E[y]	0.49	0.49	0.49	0.49	0.49	10.88	10.88	10.88	10.88	10.88
R-sq	0.10	0.10	0.10	0.12	0.70	0.10	0.12	0.12	0.15	0.74
N	1,306,262	1,306,262	1,306,262	800,373	380,867	1,306,262	1,306,262	1,306,262	800,373	380,867

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 columns (1) - (5) 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 (6) - (10), the outcome variable is the number of school years completed, again from the 1940 Census. Δ Edu Spend is the standardized (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized (mean zero, standard deviation one) 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 state by year fixed effects and column (3) adds baseline school expenditure (1930), county youth employment share (1930), and unemployment rate (1930) by year fixed effects. Column (4) adds the estimated regional unemployment rate of the father's occupation - taken from the Special Census of Unemployment in 1931 - by year fixed effects. Finally, column (5) adds household fixed effects, effectively dropping sons without linked siblings from the sample. Columns (6) through (10) are structured analogously. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 5: Youth unemployment and education by parent occupation: high school completion

	Outcome: I(completed 12 years)							
	(1) Prof.	(2) Manager	(3) Clerk	(4) Sales	(5) Craft	(6) Operative	(7) Service	(8) Labor
1928 - 1929 × Δ Edu Spend	0.010	0.014**	0.005	-0.006	-0.002	-0.006	0.000	0.009*
	(0.011)	(0.006)	(0.012)	(0.009)	(0.005)	(0.006)	(0.007)	(0.005)
1930 - 1933 × Δ Edu Spend	-0.001	0.010*	0.002	-0.002	-0.004	-0.001	0.001	0.001
	(0.010)	(0.005)	(0.010)	(0.008)	(0.004)	(0.005)	(0.006)	(0.005)
1934 - 1937 × Δ Edu Spend	0.007	0.011**	0.013	-0.005	-0.001	-0.003	0.007	-0.004
	(0.010)	(0.005)	(0.010)	(0.008)	(0.005)	(0.005)	(0.006)	(0.005)
1928 - 1929 × Unemployment - Youth	0.017	-0.012	-0.013	0.031**	0.004	0.004	-0.004	-0.009
	(0.017)	(0.010)	(0.019)	(0.014)	(0.008)	(0.008)	(0.012)	(0.008)
1930 - 1933 × Unemployment - Youth	0.018	-0.004	-0.001	0.017	0.011*	0.009	0.017	0.010
	(0.015)	(0.008)	(0.017)	(0.012)	(0.007)	(0.007)	(0.011)	(0.007)
1934 - 1937 × Unemployment - Youth	0.013	-0.000	0.022	0.024**	0.013*	0.008	0.012	0.010
	(0.015)	(0.008)	(0.016)	(0.012)	(0.007)	(0.007)	(0.011)	(0.007)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	0.81	0.67	0.64	0.65	0.47	0.40	0.41	0.27
R-sq	0.07	0.08	0.09	0.08	0.08	0.08	0.10	0.07
N	53,833	174,484	57,875	110,297	308,369	220,943	108,769	161,178

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 unit of observation is an individual in the sample of linked males between 1930-1940. Each column reports the estimated coefficients using a subsample of the son population based on the occupation of the father - or mother when father was not present. For individuals that were 18-22 years old in 1930 and listed as a head-of-household in the 1930 Census, the parental occupation Census variable was taken from the 1920 Census, when available. For everyone else, it was taken from the 1930 Census. The occupational categories are: professional, manager, clerk, salesman, craftsman, operative, service, and non-farm laborer as reported by the *occ*1950 Census variable. 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 (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the (mean zero, standard deviation one) standardized measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 6: Youth unemployment and education by parent occupation: school years completed

	School years completed							
	(1) Prof.	(2) Manager	(3) Clerk	(4) Sales	(5) Craft	(6) Operative	(7) Service	(8) Labor
1928 - 1929 × Δ Edu Spend	-0.019	0.101**	-0.050	-0.005	0.014	-0.044	0.007	0.003
	(0.092)	(0.039)	(0.077)	(0.064)	(0.034)	(0.042)	(0.060)	(0.053)
1930 - 1933 × Δ Edu Spend	-0.030	0.093***	-0.045	0.011	0.003	-0.050	-0.029	-0.009
	(0.081)	(0.033)	(0.067)	(0.054)	(0.029)	(0.037)	(0.054)	(0.046)
1934 - 1937 × Δ Edu Spend	0.063	0.095***	0.006	0.026	0.011	-0.050	0.027	-0.024
	(0.079)	(0.032)	(0.066)	(0.052)	(0.027)	(0.037)	(0.055)	(0.045)
1928 - 1929 × Unemployment - Youth	0.080	0.031	-0.047	0.252***	0.052	0.048	-0.112	-0.035
	(0.131)	(0.062)	(0.118)	(0.091)	(0.047)	(0.055)	(0.084)	(0.063)
1930 - 1933 × Unemployment - Youth	0.071	0.021	-0.052	0.137*	0.091**	0.038	-0.010	0.089*
	(0.111)	(0.052)	(0.104)	(0.076)	(0.041)	(0.047)	(0.075)	(0.054)
1934 - 1937 × Unemployment - Youth	0.053	0.086*	0.099	0.155**	0.112***	0.058	0.044	0.090*
	(0.107)	(0.050)	(0.099)	(0.074)	(0.039)	(0.045)	(0.074)	(0.055)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	13.27	12.11	11.76	11.87	10.77	10.33	10.31	9.52
R-sq	0.09	0.08	0.09	0.08	0.09	0.10	0.16	0.12
N	53,833	174,484	57,875	110,297	308,369	220,943	108,769	161,178

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 unit of observation is an individual in the sample of linked males between 1930-1940. Each column reports the estimated coefficients using a subsample of the son population based on the occupation of the father, or mother when father was not present. For individuals that were 18-22 years old in 1930 and listed as a head-of-household in the 1930 Census, the parental occupation Census variable was taken from the 1920 Census, when available. For everyone else, it was taken from the 1930 Census. The occupational categories are: professional, manager, clerk, salesman, craftsman, operative, service, and non-farm laborer as reported by the *occ*1950 Census variable. 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 (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized (mean zero, standard deviation one) measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Regressions are weighted for representativeness. 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			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
1928 - 1929 × Δ Edu Spend	0.001	-0.010**	-0.001	0.006	-0.030	-0.054*	0.030	0.043
	(0.003)	(0.005)	(0.005)	(0.004)	(0.026)	(0.031)	(0.029)	(0.030)
1930 - 1933 × Δ Edu Spend	-0.000	-0.003	-0.004	0.003	-0.023	-0.020	-0.005	0.013
	(0.003)	(0.004)	(0.004)	(0.003)	(0.020)	(0.027)	(0.024)	(0.026)
1934 - 1937 × Δ Edu Spend	-0.004	-0.002	-0.001	0.004	-0.042**	-0.022	0.003	0.032
	(0.003)	(0.004)	(0.004)	(0.004)	(0.020)	(0.027)	(0.023)	(0.026)
1928 - 1929 × Unemployment - Youth	-0.010**	0.007	-0.001	-0.001	-0.041	0.091**	0.017	0.025
	(0.005)	(0.006)	(0.007)	(0.006)	(0.032)	(0.041)	(0.042)	(0.040)
1930 - 1933 × Unemployment - Youth	0.005	0.013**	0.010*	0.004	0.070**	0.091**	0.068**	0.014
	(0.004)	(0.006)	(0.006)	(0.005)	(0.028)	(0.036)	(0.034)	(0.034)
1934 - 1937 × Unemployment - Youth	0.011**	0.011**	0.016***	0.006	0.125***	0.121***	0.109***	0.076**
	(0.004)	(0.006)	(0.006)	(0.005)	(0.027)	(0.036)	(0.033)	(0.033)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓
E[y]	0.37	0.48	0.48	0.63	10.16	10.84	10.79	11.84
R-sq	0.08	0.10	0.09	0.13	0.12	0.11	0.10	0.17
N	404,449	255,951	298,370	346,908	404,449	255,951	298,370	346,908

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 unit of observation is an individual in the sample of linked males between 1930-1940. Each column reports the estimated coefficients using a subsample of the son population based on the quartile of the father's or - mother's when father was not present - occupational income score (1950). For individuals that were 18-22 years old in 1930 and listed as a head-of-household in the 1930 Census, the parental *occscore* Census variable was taken from the 1920 Census, when available. For everyone else, it was taken from the 1930 Census. Cohorts that turned 18 in 1926 or 1927 serve as the omitted reference 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 (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the city school district level. Unemployment - Youth is the standardized (mean zero, standard deviation one) measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 8: Pooled difference-in-differences estimates: birth order

	Outcome: I(completed 12 years)			School years completed		
	Only child	Youngest	Oldest	Only child	Youngest	Oldest
	(1)	(2)	(3)	(4)	(5)	(6)
1928 - 1929 × Δ Edu Spend	-0.000	0.002	0.003	0.024	-0.017	0.014
	(0.004)	(0.007)	(0.003)	(0.031)	(0.046)	(0.024)
1930 - 1933 × Δ Edu Spend	-0.009**	0.004	0.002	-0.078***	-0.011	0.029
	(0.004)	(0.005)	(0.003)	(0.027)	(0.037)	(0.022)
1934 - 1937 × Δ Edu Spend	-0.001	0.002	0.002	-0.030	-0.036	0.031
	(0.004)	(0.005)	(0.003)	(0.029)	(0.035)	(0.021)
1928 - 1929 × Unemployment - Youth	-0.011*	0.008	-0.001	-0.056	0.122**	0.040
	(0.007)	(0.009)	(0.005)	(0.045)	(0.061)	(0.032)
1930 - 1933 × Unemployment - Youth	0.003	0.007	0.010**	-0.016	0.070	0.116***
	(0.006)	(0.008)	(0.004)	(0.038)	(0.050)	(0.028)
1934 - 1937 × Unemployment - Youth	0.020***	0.012	0.017***	0.071*	0.138***	0.164***
	(0.007)	(0.008)	(0.005)	(0.040)	(0.047)	(0.029)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓
E[y]	0.49	0.49	0.49	10.88	10.88	10.88
R-sq	0.13	0.07	0.11	0.16	0.08	0.13
N	172,963	233,915	384,150	172,963	233,915	384,150

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 columns (1) - (3) 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 (4) - (6), the outcome variable is the number of school years completed, again from the 1940 Census. Δ Edu Spend is the standardized (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the school district level. Unemployment - Youth is the standardized (mean zero, standard deviation one) measure of the youth unemployment rate in 1931 estimated using the 1931 Unemployment Census. The samples used in each specification - only-children, youngest, oldest - are indicated in the header. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 9: Interaction between household composition and the impact of the Depression on high school completion

	Average	Number of families within hh		Generations within hh	Father's unemployment	
		1	2	2	Below p50	Above p50
	(1)	(2)	(3)	(4)	(5)	(6)
1928 - 1929 × Δ Edu Spend	-0.000	-0.001	0.007	-0.001	-0.003	-0.002
	(0.002)	(0.002)	(0.006)	(0.002)	(0.005)	(0.005)
1930 - 1933 × Δ Edu Spend	-0.001	-0.003	0.012**	-0.001	0.004	-0.004
	(0.002)	(0.002)	(0.005)	(0.002)	(0.004)	(0.004)
1934 - 1937 × Δ Edu Spend	-0.002	-0.003	0.004	-0.002	0.003	-0.005
	(0.002)	(0.002)	(0.005)	(0.002)	(0.005)	(0.004)
1928 - 1929 × Unemployment - Youth	-0.003	-0.003	-0.007	-0.003	-0.000	-0.004
	(0.003)	(0.003)	(0.009)	(0.003)	(0.007)	(0.007)
1930 - 1933 × Unemployment - Youth	0.006**	0.005*	0.012	0.008***	0.010	0.008
	(0.003)	(0.003)	(0.008)	(0.003)	(0.006)	(0.006)
1934 - 1937 × Unemployment - Youth	0.010***	0.010***	0.011	0.012***	0.013**	0.007
	(0.003)	(0.003)	(0.008)	(0.003)	(0.006)	(0.006)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
State 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	0.49	0.55	0.37
R-sq	0.10	0.10	0.13	0.10	0.09	0.07
N	1,306,262	1,171,129	135,133	1,192,650	404,648	395,725

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 10: Heterogeneity based on youth labor regulation and New Deal spending

	Youth labor regulation		WPA		FERA	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	below p50	above p50	below p50	above p50
1928 - 1929 × Δ Edu Spend	0.004	-0.006	-0.002	0.001	-0.001	-0.007
	(0.003)	(0.004)	(0.003)	(0.006)	(0.003)	(0.006)
1930 - 1933 × Δ Edu Spend	0.001	-0.004	-0.000	0.003	0.001	-0.003
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.005)
1934 - 1937 × Δ Edu Spend	-0.001	-0.001	0.001	0.001	0.001	-0.004
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.005)
1928 - 1929 × Unemployment - Youth	-0.007	0.002	0.002	-0.009	0.005	-0.031***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.012)
1930 - 1933 × Unemployment - Youth	0.006	0.009**	0.009**	0.011*	0.008**	0.004
	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)	(0.010)
1934 - 1937 × Unemployment - Youth	0.011***	0.012***	0.011**	0.007	0.006	0.008
	(0.004)	(0.004)	(0.005)	(0.006)	(0.004)	(0.010)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓
E[y]	0.46	0.51	0.48	0.49	0.49	0.47
R-sq	0.10	0.10	0.11	0.09	0.11	0.09
N	686,930	619,332	589,757	587,750	593,227	584,280

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 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 reference group. The outcome variable across all columns is an indicator variable taking the value of 1 if the individual reported 12 or more years of schooling in the 1940 Census. Δ Edu Spend is the standardized (mean zero, standard deviation one) measure of the log change between 1930 and 1934 per-pupil education spending at the city school district level. Unemployment - Youth is the standardized (mean zero, standard deviation one) 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 (as of 1931) on the maximum working hours in a day or week for 17 and 18 year-old males employed in factories, those who reported living in a state with such restrictions, and those living in counties with below or above median per-capita WPA and FERA spending. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Table 11: Intergenerational education mobility and the Depression

	(1) up_im_7	(2) up_im_8	(3) up_im_9	(4) up_im_10	(5) up_im_11	(6) up_im_12	(7) up_im_13
1928 - 1929 × Δ Edu Spend	0.003	0.005	-0.006*	-0.007**	-0.006*	-0.007**	-0.005**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
1930 - 1933 × Δ Edu Spend	0.001	0.003	-0.003	-0.005	-0.004	-0.006**	-0.005**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
1934 - 1937 × Δ Edu Spend	0.000	0.004	-0.003	-0.004	-0.003	-0.004	-0.003
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
1928 - 1929 × Unemployment - Youth	0.011**	0.010**	0.001	-0.003	-0.005	-0.004	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
1930 - 1933 × Unemployment - Youth	0.011***	0.014***	0.013***	0.010**	0.006	0.007*	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
1934 - 1937 × Unemployment - Youth	0.006*	0.014***	0.016***	0.014***	0.013***	0.012***	0.007***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
City FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓
E[y]	0.32	0.35	0.55	0.52	0.46	0.40	0.17
R-sq	0.06	0.05	0.03	0.04	0.05	0.05	0.05
N	594,147	594,147	594,147	594,147	594,147	594,147	594,147

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. Regressions are weighted for representativeness. Standard errors shown in parentheses and are clustered at the city-cohort level.

Appendix

Table of Contents

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

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

	Teacher Wages	S/T Ratio	Term Days
	(1)	(2)	(3)
Post=1 × Δ Edu. Spend	0.067	-0.852	1.122
	(0.017)	(0.405)	(0.357)
Post=1 × Unemployment - Youth	-0.025	0.852	-0.178
	(0.024)	(0.516)	(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

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.

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 (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.

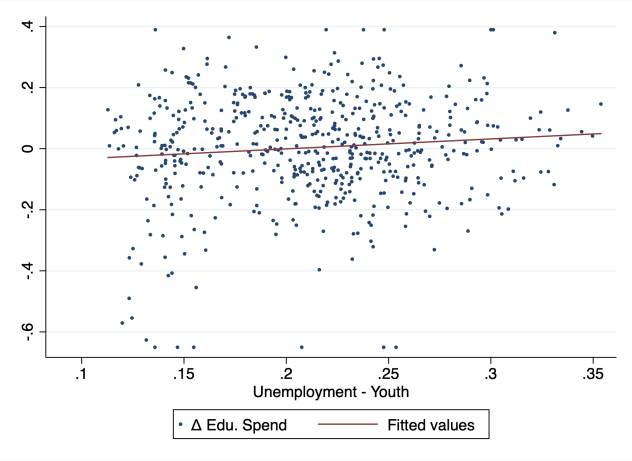
As a second validation exercise, I find that 1931 youth unemployment rates predict youth welfare enrollment by 1934 in a larger sample of cities. The data from 1934 comes from the *Survey of Urban Workers on Relief in May 1934* produced by the Works Progress Administration (Wood and Palmer (1936)). Specifically, 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 father in the 1930 Census

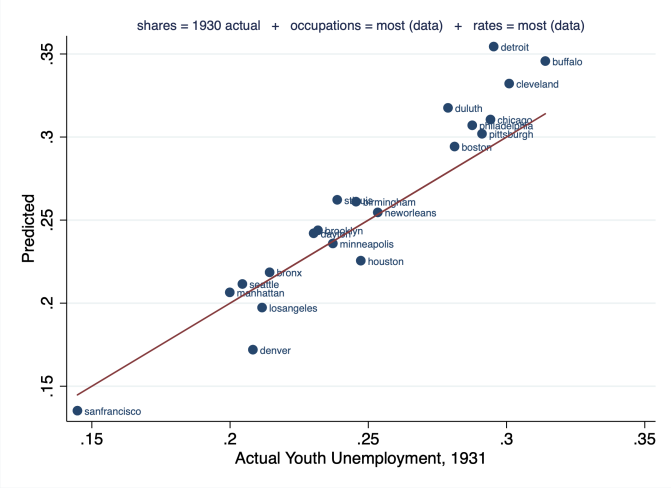
Source: Aggregation of 100 percent count records of the 1930 Decennial Census, available on IPUMS.

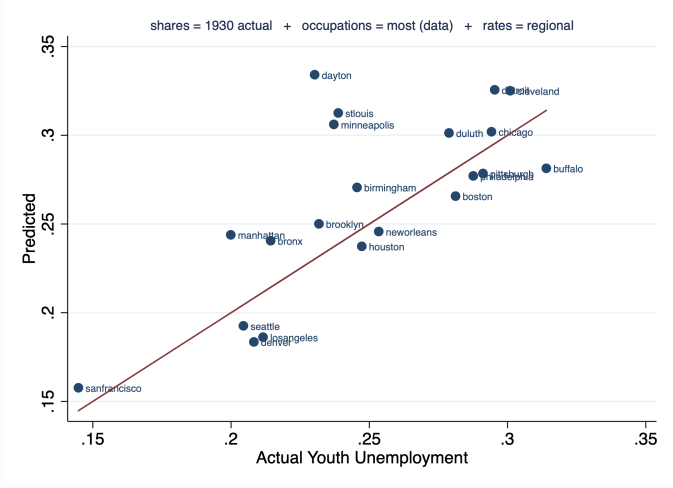
Figure A.2: Youth unemployment and education spending

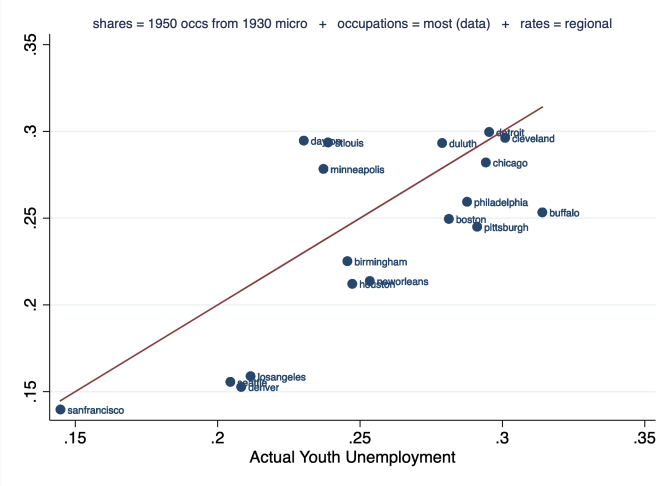


Notes: This figure shows a scatterplot between unemployment and change in education spending for 555 cities (correlation = 0.09). The cohort graduation rate is defined as the number of respondents reporting at least 12 years of schooling in the 1940 Census divided by total cohort size in a 1930-1940 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

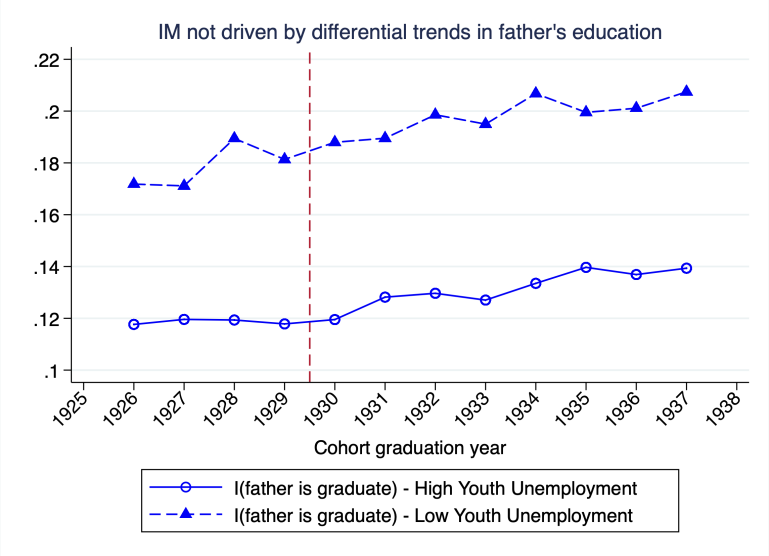






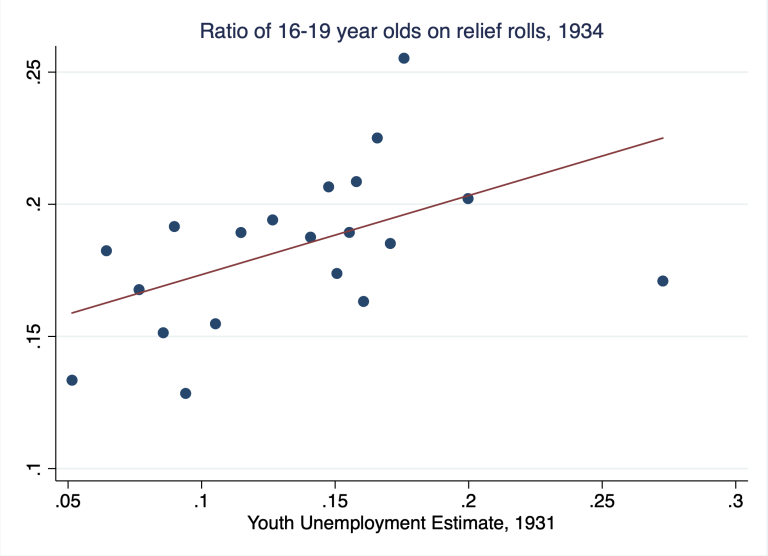
Notes: This figure plots the actual and estimated youth unemployment rates for 21 cities appearing in the 1931 Census. Title denotes how each estimated rate was constructed. See Section 3.1 for details regarding the differences between the panels.

Figure A.4: Education of fathers



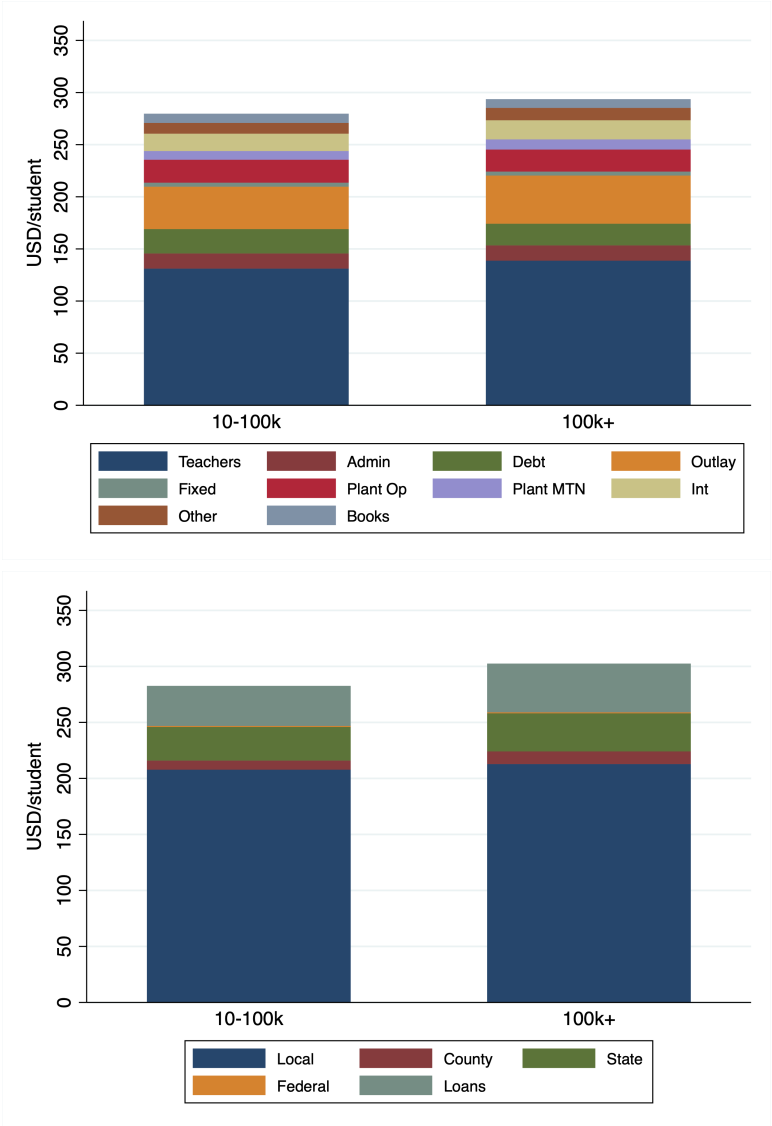
Notes: This figure plots the proportion of father high school graduates in the linked father-son sample across youth-unemployment terciles.

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: Pooled difference-in-differences estimates: average effects using conservative linking

	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	-0.003	-0.006*	-0.003	-0.002	-0.004	-0.010	-0.016	-0.035
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.002)	(0.018)	(0.019)	(0.022)	(0.033)
1930 - 1933 × Δ Edu Spend	-0.000	-0.002	-0.003	-0.002	0.001	-0.000	-0.002	-0.010	0.002	-0.008
	(0.002)	(0.003)	(0.003)	(0.003)	(0.006)	(0.002)	(0.015)	(0.016)	(0.018)	(0.029)
1934 - 1937 × Δ Edu Spend	0.003	-0.002	-0.003	-0.002	0.003	0.003	-0.012	-0.026	-0.014	0.018
	(0.002)	(0.003)	(0.003)	(0.003)	(0.006)	(0.002)	(0.015)	(0.016)	(0.018)	(0.030)
1928 - 1929 × Unemployment - Youth	0.001	0.001	-0.004	-0.005	0.002	0.001	0.039**	0.029	0.029	0.102**
	(0.003)	(0.003)	(0.004)	(0.005)	(0.009)	(0.003)	(0.020)	(0.025)	(0.032)	(0.048)
1930 - 1933 × Unemployment - Youth	0.007***	0.012***	0.006*	0.007*	0.019**	0.007***	0.093***	0.074***	0.068***	0.167***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.008)	(0.002)	(0.017)	(0.021)	(0.026)	(0.042)
1934 - 1937 × Unemployment - Youth	0.016***	0.015***	0.009***	0.006	0.026***	0.016***	0.167***	0.123***	0.087***	0.257***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.009)	(0.002)	(0.016)	(0.021)	(0.026)	(0.043)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓		✓	✓	✓	✓	
Household FE					✓					✓
State x Year		✓	✓	✓	✓		✓	✓	✓	✓
School Expenditure (1930) x Year			✓	✓	✓			✓	✓	✓
Youth Labor Share (1930) x Year			✓	✓	✓			✓	✓	✓
County Unemployment (1930) x Year			✓	✓	✓			✓	✓	✓
Father Unemployement (Region-Occupation) x Year				✓					✓	
E[y]	0.49	0.49	0.49	0.49	0.49	10.88	10.88	10.88	10.88	10.88
R-sq	0.11	0.11	0.11	0.14	0.72	0.11	0.13	0.13	0.17	0.77
N	749,145	749,145	749,145	453,227	184,820	749,145	749,145	749,145	453,227	184,820

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 using only the sample of sons with ABE exact (names) conservative linking between the 1930 and 1940 Censuses. For more details, see notes in Table 4.

Table A3: Interaction between household composition and the impact of the Depression on school years completed

	Average	Number of families within hh		Generations within hh	Father's unemployment	
		1	2	2	Below p50	Above p50
	(1)	(2)	(3)	(4)	(5)	(6)
1928 - 1929 × Δ Edu Spend	-0.009	-0.018	0.061	-0.010	-0.009	-0.004
	(0.015)	(0.016)	(0.048)	(0.017)	(0.037)	(0.034)
1930 - 1933 × Δ Edu Spend	-0.018	-0.029**	0.065	-0.017	0.008	-0.007
	(0.013)	(0.013)	(0.040)	(0.014)	(0.031)	(0.029)
1934 - 1937 × Δ Edu Spend	-0.020	-0.027**	0.048	-0.022	0.010	-0.021
	(0.013)	(0.013)	(0.038)	(0.014)	(0.031)	(0.029)
1928 - 1929 × Unemployment - Youth	0.006	0.007	-0.024	0.006	0.021	0.031
	(0.020)	(0.021)	(0.063)	(0.022)	(0.049)	(0.045)
1930 - 1933 × Unemployment - Youth	0.054***	0.047***	0.106**	0.064***	0.036	0.094**
	(0.017)	(0.018)	(0.053)	(0.018)	(0.041)	(0.038)
1934 - 1937 × Unemployment - Youth	0.112***	0.113***	0.101**	0.121***	0.095**	0.099***
	(0.017)	(0.017)	(0.051)	(0.018)	(0.040)	(0.038)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
State x Year	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓
E[y]	10.88	10.84	11.29	10.89	11.27	10.13
R-sq	0.12	0.12	0.16	0.12	0.10	0.10
N	1,306,262	1,171,129	135,133	1,192,650	404,648	395,725

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), precense of of siblings (4) and number of own-family generations within household (6, 7). Standard errors shown in parentheses and are clustered at the city-cohort level.

D.1 Weighing

Following Bailey et al. (2017), I construct inverse propensity weights to adjust for observable differences between linked and linked records in two steps. First, using the population of 11-22 year old males living in an Census-identified city in 1930, I estimate a probit regression of link status (whether an individual is matched) on the following variables: indicator for being white, indicator for father having a white-collar occupation, indicators for each Census region, age and age squared, and a constant. The results are shown in Table A4. I then compute the inverse propensity scores for each person as (1-p)/p times m/(1-m),

where (p) is the predicted likelihood of an individual being matched based on the estimated probit coefficients and (m) is the actual match rate (22.1 percent).

Table A4: Predicting characteristics of successful links using a probit regression

	(1) I(in sample)
I(White)	0.453***
	(0.026)
I(White Collar Father)	0.106***
	(0.012)
Middle Atlantic Division	-0.105***
	(0.040)
East North Central Div.	0.086**
	(0.042)
West North Central Div.	0.138**
	(0.056)
South Atlantic Division	-0.489**
	(0.195)
East South Central Div.	-0.223**
	(0.109)
West South Central Div.	-0.137*
	(0.076)
Mountain Division	0.115
	(0.080)
Pacific Division	0.029
	(0.081)
Age	0.106***
	(0.005)
Age Squared	-0.004***
	(0.000)
Constant	-1.801***
	(0.054)
N	5,910,651

Standard errors in parentheses

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