Intergenerational Mobility during the Great Depression*

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Abstract

Do severe economic downturns increase intergenerational economic mobility by breaking links between generations, or do they instead reduce mobility by limiting opportunity for the young? To answer this question, I estimate rates of intergenerational mobility during the Great Depression in American cities that experienced downturns of varying severity. I create two new historical samples, digitizing and transcribing archival data on individual earnings and linking fathers to sons before and after the Depression. To build these longitudinal samples, I develop a machine learning approach to census matching that enables me to link individuals accurately and efficiently between censuses in the absence of unique identification numbers. I find that the Great Depression lowered intergenerational mobility for sons growing up in cities hit by large downturns. These results are not driven by place-specific mobility differences: for the generation before the Depression, mobility between 1900 and 1920 is unrelated to future downturn intensity. Differential directed migration is a key mechanism to explain my results. Although sons fled distressed cities at similar rates, the sons of richer fathers migrated to locations that had suffered less severe Depression effects. The differences in rates of intergenerational mobility for sons in the most and least Depression-affected cities are comparable to the differences between the United States and Sweden today.

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1 Introduction

Do economic downturns expand or contract intergenerational economic mobility? The effects of macroeconomic conditions on the stakes of the lottery of birth are unclear. A significant disruption of the economy may diminish or even render irrelevant inequities of opportunity bestowed by the previous generation, decoupling the fates of children from their parents. Alternatively, poorer families may be less able to endure a downturn, and children who might have climbed the income ladder in normal times—perhaps with more education or by making savvier migration choices—would instead emerge from a crisis no better off than their parents. To answer this question, I estimate the effects on intergenerational mobility of the largest economic cataclysm in American history, the Great Depression. The Depression presents an ideal natural experiment for studying the impact of a downturn on the transmission of economic status for three primary reasons. First, with a quarter of the labor force unemployed, the magnitude of the Depression dwarfs the recessions of the postwar era; if downturns do alter mobility rates, this should be most observable in the largest one. Second, fortunes large and small were destroyed in the 1929 stock market crash as well as in many local real estate crises and bank failures across the United States, creating geographic variation that enables me to compare intergenerational mobility across cities affected unevenly by the Depression. Third, the passage of time allows me to observe the downturn-exposed children as adults in 1940, which is not yet possible for the children of the recent Great Recession.

I measure mobility in two historical datasets that I construct, linking parents before the Depression to their children as young adults in 1940. Historical microdata reporting earnings as well as names and ages—necessary to link records between sources—are exceedingly rare before 1940.[1] I add to the stock of these datasets by digitizing and transcribing the 1918-1919 Bureau of Labor Statistics (BLS) cost of living survey that includes names, ages, addresses, income, and occupation for 12,817 families in 99 cities. I link the parents in the survey ahead to their children in 1940, creating a new dataset of intergenerational earnings. I supplement this data by building a second linked sample of parents and children containing individual occupations and average occupational

[1] In the United States, there are two such sources: the 1915 Iowa State Census, which includes records for only the state of Iowa (Goldin and Katz 2000), and published lists of personal income tax payers in New York City in 1923 and 1924 (Marcin 2014). The 1940 Federal Census is the first census to include data on earnings and completed years of education. The original manuscripts with names and addresses were released in 2012 following the standard 72-year privacy period.

earnings, drawn from the 1920 and 1940 US Censuses of Population.

To create these intergenerational linked samples, I develop a machine learning approach to record matching across historical microdata.[2] Linking historical data—without unique identification numbers—is difficult and imprecise, relying on demographic information like name, age, and place of birth. Manual linking by a trained researcher yields accurate and comprehensive matches, but at the cost of time and replicability. Algorithmic approaches have been developed in the historical literature, but their rigid rules are often quite inefficient—many records go unmatched—and inaccurate in the face of messy historical data. My technique uses supervised learning to train an algorithm to replicate the process of manually matching individual records across sources. I am thus able to increase the speed, accuracy, and consistency of creating historical linked samples.

I find that economic mobility was lower in cities with more severe downturns during the Depression: both the 1940 earnings and occupations of sons growing up in these cities are more closely linked to their father's outcomes than are those of sons in less negatively affected cities. I measure relative intergenerational mobility in three ways: the elasticity of the son's earnings with respect to the father's earnings; the coefficient from a regression of the son's position in the earnings distribution in 1940 on the father's position in 1920; and the elasticity of the son's occupation score to the father's occupation score.[3] All three measures show similar reductions in mobility caused by the Great Depression. The effects are of similar magnitude for all sons in my sample, regardless of whether they were in grade school in 1929, of high school age, or already in the labor force when the Depression hit.[4]

To explore whether the Depression effects on mobility are causal—and not driven by pre-existing city-level differences in mobility that happen to correlate with Depression severity—I perform a parallel analysis on intergenerational mobility from 1900 to 1920. Implicitly, this is a differences-in-differences framework, comparing sons in cities before and after the Depression that experienced Depression downturns of varying magnitudes. For this earlier generation, I find no *ex ante* dif-

[2]The use of machine learning techniques in the economics literature has increased recently both in traditional prediction tasks (Kleinberg et al. 2015) and in causal policy evaluation (Athey 2015).

[3]I focus on relative intergenerational mobility, estimated via the slope of a regression of the sons' outcomes on their fathers' outcomes, rather than absolute mobility, which indicates the expected position of a child born to a parent at a given place in the earnings distribution. In Section 4, I document the effects of variation in Depression severity on absolute mobility as well.

[4]I estimate the relative effect of the Great Depression, exploiting variation in severity across cities. I refer to this local, differential cross-city impact of the Depression as the Great Depression effect. However, I am unable to directly assess the aggregate effect of the Depression on intergenerational mobility.

ferences in mobility between cities that later experienced larger or smaller Depression downturns. This result suggests that it is unlikely that low levels of mobility drove local Depression severity, or that mobility and Depression severity are correlated outside the Depression generation. I also show that Depression severity does not predict intergenerational mobility in the late 20th century.

How did the Depression decrease mobility in cities that experienced more severe shocks? Migration is a key mechanism. Local Depression severity drove out-migration, as sons fled distressed cities for better opportunities elsewhere. However, not all sons migrated at the same rates or to the same places. Rather, migration varied by father's earnings: the sons of richer fathers were able to make better migration decisions, moving to cities and regions that had suffered less severe Depression downturns, reducing estimated economic mobility. Formal education plays no role in explaining my estimated effects; while sons from cities with more severe downturns did accumulate more years of schooling than sons from other cities, the Depression did not affect the link between a father's earnings and a son's education. I also find that the Depression increased income inequality in cities, a change that may have made climbing the ladder of economic status more difficult. Finally, I show that local variation in New Deal spending did not affect mobility.

I motivate my empirical analysis with a model of intergenerational mobility based on Solon (2004) and in the spirit of Becker and Tomes (1979), but augmented with migration. Parents invest in their children's human capital, and parents with higher income invest more heavily. Parents with higher income also endow their children with larger social networks, enabling them to migrate to regions with higher match-specific returns. Both education and migration could explain why a large macroeconomic downturn with spatial variation could decrease intergenerational mobility, but my empirical results suggest that migration is the primary mechanism.

The estimated effects of the Depression on mobility are economically large. The differences in intergenerational mobility rates between sons in cities with more severe and less severe downturns are comparable to the differences in mobility between the United States and Sweden today, the least and most economically mobile countries in the OECD (Corak 2013). The differences I find are also comparable to the differences in mobility between Charlotte, NC and Salt Lake City, UT, two American cities that are currently at opposite ends of the mobility spectrum (Chetty et al. 2014a). The Depression calcified the mobility ladder for the generation of children unlucky enough to be born in cities with especially severe downturns.

This historical study offers a lens on intergenerational mobility today and in the future. Mobility appears to have remained stable for the last three decades (Chetty et al. 2014b; Lee and Solon 2009). Has the Great Recession upset that stability? The children of the Great Recession are not yet old enough for researchers to observe their lifetime earnings or occupation choices. Both the Great Recession and Great Depression were periods of large economic and financial market disruption and uncertainty, accompanied by large increases in unemployment. The results of my study, that mobility decreased in cities hardest hit by the Depression, may predict that scholars will find less mobility in the years to come, particularly in those regions that suffered the worst downturns during the Great Recession.

By comparing mobility across cities, I am able to overcome three challenges to measuring historical intergenerational mobility. These potential issues could bias overall mobility estimates, but because they do not vary across the cities in my sample or correlate with local Depression severity—claims for which I will give evidence throughout the paper—they will not determine my estimates of the Depression effect on mobility. First, a pair of historical challenges. I observe only a single year of earnings or occupations for fathers and sons. In addition, any intergenerational matching procedure across different data sources, mine included, will introduce some share of false matches into the sample. Second, the sample of fathers drawn from the BLS survey is not entirely nationally representative. The BLS targeted married families in 99 urban areas, most of whose male heads worked in industrial occupations, and all families are drawn from the middle of the earnings distribution, missing the very rich and the very poor. Third, I rely on the 1940 Federal Census because it is the first and only national survey with both earnings data and the necessary biographical details to enable intergenerational linking, but there are some concerns with measuring mobility using the 1940 census. Only labor earnings were enumerated in the census, not capital or self-employment income. Earnings and occupations are observed as of 1939; while the economy had recovered somewhat from the Depression and the 1937 Recession, the unemployment rate did not return to pre-1929 levels until at least 1941. The sons in my sample are still relatively young in 1940, and samples with sons who have not yet reached their permanent earnings levels tend to produce artificially high rates of mobility (Corak 2006; Grawe 2004; Mazumder 2015). However, as I document, none of these challenges varies across cities in my sample and will be differenced out when I examine the effects of relative Depression severity across cities.

The paper proceeds as follows. In Section 2, I detail the historical data I collect, digitize, and link. I also document the machine learning based census matching procedure I develop to facilitate linking large samples. In Section 3, I motivate my analysis with a model of intergenerational mobility in the face of large and spatially varied macroeconomic downturns, allowing for investment in human capital and endogenous geographic mobility. I present my main results in Section 4: for the sons growing up in cities with the most severe Depression downturns, intergenerational mobility was greatly reduced. I present evidence that these Depression effects are specific to the 1920 to 1940 period, showing no relationship between local Depression severity and mobility in the generation preceding the Depression (1900 to 1920) or in the current period (1980-82 to 2011-12). In Section 5, I explore several possible mechanisms that might drive the main results, showing that differential directed geographic migration best explains the decrease in intergenerational mobility in Depression-affected cities. I also show that the Great Depression increased earnings inequality. I explore the effects of New Deal on mobility in Section 6. Section 7 concludes.

2 Data and Census Record Linking

I draw on several historical data sources, including new archival microdata I collected for this project. I have developed a matching algorithm to facilitate linking parents and children across censuses and other data sources, applying insights from the machine learning literature. The algorithm learns the implicit rules that a careful and well-trained researcher uses to match records across historical samples and replicates these decisions for the full dataset, increasing the speed, accuracy, and consistency of the process. The generated matched samples are both large and unbiased. I combine these data with measures of local variation in Great Depression severity based on retail sales from Fishback et al. (2003).

2.1 Earnings Data

I combine two data sources to measure the earnings of fathers before the Great Depression and their sons after it. For the fathers, the data come from a 1918-1919 Bureau of Labor Statistics (BLS) survey that provides one of the earliest national samples to include both earnings and—crucially for record linkage—names and ages. I digitize and transcribe the names for the complete sample from the original surveys stored at the National Archives in College Park, MD. For the sons, I rely on the complete count 1940 census.

In 1918 and 1919, the BLS conducted one of the first cost of living surveys in the United States.[5] The BLS surveyed 12,817 families in 99 industrial and mining cities between July 31, 1918 and February 28, 1919. Though the primary focus of this survey was estimating the cost of living across urban areas during the First World War, the BLS also collected detailed data on earnings and labor supply.[6] These economic variables are instrumental to my study, as are the names and addresses of the original survey respondents. I have collected and digitized the information from each original BLS survey response deposited at the National Archives and linked the names and addresses to the rest of the survey data.[7] After linking the BLS respondents to their sons in 1940, this detailed data enables me to construct estimates of intergenerational mobility of earnings and occupation.

In Figure 1, I map the cities included in the BLS survey. The largest cities in the sample are New York (516 observations), Boston (405), and Chicago (348). However, many smaller urban areas were also included, for example Bisbee, AZ (population 9,205 in 1920 with 80 observations in the BLS sample) and Calumet, MI (population 2,390, 73 observations). The sample is 93% white. The African American sample is concentrated in a few cities, and only Baltimore, New Orleans, and St. Louis have more than 75 sampled black families. I restrict my analysis to white families.[8]

What types of respondents were targeted by the survey? The BLS sampled only married couples with both spouses living in the same house, and all families had to include at least one child. The surveys were conducted in respondents' homes by local female enumerators, and the sample was limited to English speakers who had resided in the "community" for more than one year. The BLS intended to sample only wage earners of any annual income and salaried workers earning less than $2000 per year. Of course, enumerators did not know respondents' incomes before asking, so the

[5]The survey has been used by economic historians, including Dora Costa (1997, 2000, 1999), Shawn Kantor and Price Fishback (1996), Carolyn Moehling (2001, 2005), Martha Olney (1998), and Evan Roberts (2003).

[6]Most of the variables originally collected by the BLS, including earnings and occupation, were transcribed and digitized by the Interuniversity Consortium for Political and Social Research (ICPSR) in the 1980s. Those data are available via the ICPSR dataverse at `http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/8299`. However, the ICPSR files do not contain transcribed names or addresses of the respondents. According to Peter Granda, an associate director of ICPSR, these names may have been transcribed when the survey was first digitized in the early 1980s, but those files were never used for privacy reasons and have since been lost.

[7]I traveled to the National Archives to access the original survey responses and photographed the first page of each survey, capturing the names, addresses, and other demographic information of each respondent family. Each survey included an identification number that made linking between my transcriptions and the original ICPSR data exact. The original survey responses are deposited in 93 boxes at the National Archives in RG 257, accession NN-373-183.

[8]In future work, I hope to exploit different datasets, including linked census samples and World War II enlistment surveys, to explore intergenerational mobility among African Americans, especially in light of the high levels of geographic mobility during the Great Migration. Unfortunately, the BLS sample is not well suited to studying African Americans or other racial, ethnic, or religious minorities.

sample was selected imperfectly; some people with higher or lower incomes are included. This fact is apparent in the distribution of income graphed in Figure 2. The families sampled by the BLS were concentrated between the 30th and 70th percentile of the full 1920 earnings distribution.[9] In 94% of families, the husband worked more than 40 weeks a year; only 2% of wives worked more than 40 weeks, and only 8.5% had positive annual earnings. The richer fathers in the sample often worked for the railroads as machinists, foremen, or carpenters; other rich fathers were foremen in machine shops, foundries, and mines. The poorest fathers were mainly factory and mill laborers.[10]

The BLS survey comprises a representative but slightly younger sample of white middle income families living in large and small urban areas around 1920. I compare the demographics of the BLS sample with an urban sample of the 1920 census, drawn from the IPUMS 1% census extract, in Table 1. I restrict the census sample to white families in cities with married heads of household, both spouses present, and at least one child, replicating the BLS demographic sampling frame. The families sampled by the BLS tended to be younger: both the fathers and the mothers sampled were about five years younger than the comparable census sample. The children in the BLS sample are also three to four years younger on average. In terms of family size, however, the two samples are similar. The average family in both samples had 2.5 children. Because I can match only sons and not daughters forward into the future censuses, I focus on them. On average, there are 1.25 sons in each family in the BLS sample and 1.29 in the census sample.[11]

I convert earnings in the BLS sample to 1920 dollars to account for changes in the price level during the months of the survey using monthly urban consumer CPI data from the BLS. The respondents in my data were surveyed at different points over an eight month period, from July 1918 to February 1919. Earnings were reported to enumerators relative to the twelve months preceding the survey. These years were a period of rapid price changes, surely part of the impetus

[9] A complete earnings distribution for 1920 does not exist. I estimate the distribution by drawing on the 1940 earnings distribution from the complete 100% sample of the 1940 census, weighting observations by occupation and industry shares from the 1920 census and converting 1940 dollars to 1920 dollars using the CPI. I detail the construction of the 1920 distribution in Appendix C. I also compare the families surveyed by the BLS in Davenport, IA and Des Moines, IA to the earnings distributions in those cities according to the 1915 Iowa State Census in Figure C.2. I use the CPI to convert the distribution from 1940 to 1920 rather than mean nominal earnings because I also convert the BLS earnings—collected across 8 months from 1918 to 1919—to 1920 dollars and need a monthly deflator. In practice, the choice of deflators does not change Figure 2 significantly.

[10] I present a word cloud illustrating common occupation strings by earnings in Figure A.4.

[11] Of the families with at least one son, the average number of sons is 1.7, with a maximum of 8, in the BLS sample. In the census sample, the conditional average is 1.7 as well. In Figure A.6, I plot histograms of the father's age in 1920 and the number of sons in each family in the BLS sample.

for the BLS to collect cost of living data in the first place. In Figure A.5 in the Appendix, I plot monthly inflation in these years.

After transcribing the BLS survey, I match families to the 1920 census to recover full census information on the sons: full name, state of birth, and year of birth. This facilitates matching the sons into the 1940 census. In 1940, I observe annual earnings, weeks and hours worked, and occupation, as well as years of education, for each son in my sample. The earnings data in 1940 is restricted to labor earnings; business and self-employment income, as well as other capital income like farm-owner earnings, are not collected. Given that my sample is drawn exclusively from city families, the missing farmer income data is not relevant. However, some of the sons in my sample may have had business or capital earnings in 1940. Though the census did not record the exact amount, enumerators did ask whether or not the respondent earned $50 or more in non-wage or salary income during the year.[12] Only 12% of the sons in my sample reported such income in 1940, a reasonable share given that their fathers were all wage and salary earners in 1920.[13]

2.2 Occupation Score Data

In addition to the newly digitized BLS survey, I also create a matched sample of fathers and sons from the IPUMS 1% sample of the 1920 census to the 1940 census.[14] While this additional sample lacks data on fathers' incomes before the Depression because income was not reported in any US Census of Population until 1940, it enables me to calculate occupation score mobility and assess my main results on a much larger sample.[15]

I ensure comparability between the BLS sample and the IPUMS sample in two ways. First, I limit the sample to the sons of married fathers living in the same household in 1920. Second, I focus on sons living in one of the 99 cities sampled by the BLS.

My identification strategy requires that individuals in cities hit with larger Great Depression shocks were no less (or more) economically mobile prior to the Depression. To assess this claim, I

[12] In the 1950 census, capital and labor earnings were reported. I impute capital income in 1940 for my sample, using the 1950 data and age, education, occupation codes, industry codes, and geographic location. See Appendix D. My main results are all robust to using imputed data for capital income earners.

[13] The earnings in the 1940 census are also top-coded at $5000. However, this restriction is not relevant for me, as only 16 of the sons in my sample report earning $5000 or more. I code earnings as $5000 in 1940 for these 16 sons.

[14] I use the IPUMS sample because it includes coded versions of important covariates—like occupation—which have not been digitized in the complete 1920 sample from Ancestry.com.

[15] Occupation scores are commonly used in historical research with data that lacks income information (Abramitzky et al. 2014). The occupation scores are the median earnings of all workers in a given occupation in 1950. In Figure A.7, I show that occupation scores and earnings correlate strongly in my samples in both 1920 and 1940.

also construct a linked sample beginning with the 1900 IPUMS 6% sample, matching fathers and sons in 1900 ahead to 1920.[16] By observing mobility *before* the Depression, I can test whether or not some locations are inherently less mobile.[17] In addition, I draw on data on recent intergenerational mobility from Chetty et al. (2014a) to show that mobility in the 1990s and 2000s is neither correlated with Great Depression severity, nor with my measures of mobility from 1920 to 1940.[18]

2.3 Linking Census Microdata with a Machine Learning Approach

Without a linked intergenerational sample, it is difficult to estimate economic mobility accurately. Constructing historical linked samples, however, requires matching across censuses and other sources without the use of unique identification codes such as tax payer IDs or social security numbers. I have developed an automated process for census matching that increases the speed, accuracy, and consistency of matching historical samples, and eliminates the need for hiring (costly) research assistants.[19] Starting with a small sample of training data with records that I have identified as matches or non-matches, the algorithm learns what features predict matches and then generates large and accurately matched samples.[20]

I begin the linking procedure with a list of sons from the 1920 census. I observe first and last names, years of birth, and states of birth. I then merge the data to the complete 1940 census, limiting the set of possible matches for any given son in 1920 to be the men in 1940 who meet the following criteria: born in the same state, born in the same year ±3 years, and have a Jaro-Winkler

[16] Technically there is no 6% sample in IPUMS but a 1% sample of 1900 and a 5% sample of 1900, which do not overlap. I use an aggregation of both samples and refer to it throughout as a 6% sample.

[17] Implicitly, this sets up a difference-in-differences framework, where I compare mobility before and after the Depression, across cities with more severe and less severe Depression shocks.

[18] This test rules out a very long-run persistent effect of the Great Depression on mobility through the late 20th century. A natural question is how long the Depression's effect on mobility persisted. As more longitudinal data on mobility in the mid-century are constructed (after future census releases), these answers will come into better focus.

[19] In Feigenbaum (2015), I provide more details on the procedure, as well as tests of its accuracy and efficiency performed with cross validation on a test set. I highlight a comparison of my matching algorithm with methods that have been used previously by economists linking historical records. I show that my method yields a higher match rate overall. In addition, when comparing methods against data manually linked by a trained researcher, I show that my method identifies 90% of the links a researcher makes (efficiency), and that 90% of the links made by the algorithm were made by a researcher (accuracy); these rates are all evaluated on the test set, not the training set used to fit the model. In comparison, the popular soundex-based method will make only 60% of the links made manually, and only 75% of the links made by the soundex-based method would have been made manually. Exact matching fares far worse on efficiency (20% to 30% depending on the age rules) but without a significant decrease in accuracy (86%).

[20] The procedure I use to train my algorithm makes this an application of supervised learning (Kuhn and Johnson 2013). The idea is straightforward: the researcher uses a labeled dataset and fits various models to predict the known labels. However, a portion of that labeled dataset is held back as the test set. To avoid overfitting, the candidate models are tested on the test set and a variety of out-of-sample prediction metrics can be compared. For more on machine learning and econometrics, see Varian (2014); Athey (2015); Kleinberg et al. (2015).

string distance in first and last names of less than 0.25.[21] This yields a very large set of possible matches; on average each son in 1920 has 240 possible matches. John Malone, born in 1914 in New York, has 1346 possible matches, the maximum in my dataset. He has both a common first name and a common last name, both are similar to other common first and last names, and he was born in the largest state.

I then build a training data set of matches, manually identifying which—if any—of the possible matches are correct links for each son. I build this training data using only 1500 sons in 1920; across a variety of different census linking examples, the algorithm reaches peak precision and accuracy very quickly, with only 15% of the full data (Feigenbaum 2015). I then use this training data to fit a probit model which generates a score, π_{ij}, indicating how likely it is that a given record i in the original dataset matches with a given record j in the target dataset.[22] However, the scores do not account for the fact that each original record can match only (and at most) once. Thus, I require each match to be (1) the best match for a given son, (2) a sufficiently good match ($\pi_{ij} > \gamma_1$), and (3) a sufficiently better match than the next best match for the son $\frac{\pi_{ij}}{\pi_{ij'}} > \gamma_2 \ \forall j'$.[23] With the model and selection rules trained on a subsample of my data, it is straightforward to construct a full matched sample on the entire data.

My linking procedure is very accurate. Across a variety of different samples and settings, the algorithm has type I and type II error rates of approximately 10% (Feigenbaum 2015). Thus, the algorithm can identify at least 90% of the matches that would be made in a careful manual linking process by a trained researcher, and at least 90% of the matches made by the algorithm

[21]The Jaro-Winkler algorithm is used to compute the distance between two strings. The Jaro-Winkler distance between Feigenbaum and Fiegenbaum is 0.03, while the distance between Feigenbaum and Teigenbaum is 0.07, an example of two transcription errors I have seen for my own name in census records that could be possible matches. Meanwhile Feinstein, which has a Jaro-Winkler distance of 0.27 from Feigenbaum, would not be in the set of possible matches because the string distance is too large. The Jaro-Winkler algorithm is popular in computing distances between proper nouns because it penalizes differing letters early in a string more heavily than letters later in the string. Jaro-Winkler has been used for record linkage in Mill (2012) and Nix and Qian (2015). For more on the string comparison algorithm, see Winkler (2006).

[22]I model links with a probit rather than any other machine learning algorithms for two reasons. First, unlike random forests or support vector matrices, probits are a standard empirical tool in the social sciences. Second, while other methods (in particular random forests) outperform the probit on the training dataset, the probit's fit on the testing data (the data that was held back from the initial fitting) is as good or better. I compare my method to other possible machine learning algorithms in Feigenbaum (2015) on the basis of the true positive rate (sensitivity) and the positive predictive value (precision).

[23]The parameters γ_1 and γ_2 are chosen to maximize the efficiency (defined as the true positive rate, the share of true matches identified by the algorithm) and the accuracy (defined as positive predictive value, the share of matches identified that are correct) on the training set via cross validation. The true positive rate corresponds to type I errors (rejecting good matches) and the positive predictive value corresponds to type II errors (accepting bad matches).

would have been made by the researcher. Still, some links between fathers and sons will be missed, and other links will be inaccurate. How will imperfect matching affect my results? Random mismatches will downwards bias estimated intergenerational mobility. If the outcome of interest were a persistence parameter on its own, one might imagine scaling up the estimated coefficient by the matching error rate.[24] However, in this paper, my main question depends not on the level of the persistence parameter, but rather on the variation of the parameter against with other covariates; in particular, how mobility changes with local Depression severity. If, as I document in the next section, inaccurate matches or biases in the linking procedure do not correlate with these other covariates of interest, then my results are unlikely to be driven by selection bias induced by the matching process.

2.4 Matched Samples

The match rates for my two main samples—the sons from the BLS survey and the sons of the IPUMS 1920 sample—using my automated linking procedure are quite high, resulting in large samples to analyze intergenerational mobility.

I match 56% of the sons in the BLS sample, in line with or better than past record linkages in the early 20th century.[25] I begin with the 12,871 family observations in the BLS survey. Some of the records have been lost in the National Archives and others are not legible, while another 3,277 of the families surveyed did not have a son. I also exclude the 871 non-white fathers. That leaves me with 6,685 fathers linked from the BLS to the 1920 census, with 11,195 sons for which to search.[26] My matching algorithm links 6,269 sons ahead to 1940, accounting for 4,385 fathers,

[24]Such a procedure may be overly conservative. Incorrect matches are not likely to be random because they will be to false sons with names (and ages and states of birth) very similar to the correct sons. Given the strong evidence for the socio-economic content of names, both in the contemporary period (Fryer and Levitt 2004) and the early 20th century (Olivetti and Paserman 2015) the mismatches may be relatively good proxies for the true outcomes of sons. As I argue in Feigenbaum (2014), a better way to compare persistence parameters estimated with noisy, possibly mismatched historical data with current data may be to increase the noise and mismatches in the modern estimates to reflect biases in the historical data, similar to the approach Romer (1986) takes to industrial production data.

[25]Other record linking attempts in this period include Parman (2011), who links fathers and sons within the Iowa 1915 census with a 50% match rate. Boustan et al. (2012) link the 1920 and 1930 censuses to study migration in response to natural disasters and match 24% of individuals. Collins and Wanamaker (2014) match men from the 1910 to 1930 censuses with a 21% match rate. Hilger (2015), aiming to estimate intergenerational mobility of education, links children aged 10 to 17 from the 1930 census to the 1940 census, using complete samples of each; however, as he requires exact and unique matches on first name, last name, year of birth, state of birth, race, and sex, his match rate is only 14%. Mill and Stein (2012) report a range of match rates that vary with the strictness of their matching procedure, ranging from 11% to 34%. The Parman (2011) match rate is comparable to mine because that sample was linked manually, the standard my machine learning approach replicates. The other rates are lower because they rely on more restrictive linking techniques that are less efficient.

[26]The average father in the full sample has 1.3 sons; conditional on having at least one son, the average father has

resulting in a 56% match rate.

The match rate for my urban sons sample from IPUMS is also high, and the sample size is much larger. I begin with the 1% sample of the 1920 census and limit it to sons living in urban locations in 1920 with married fathers present in their household to replicate the sampling frame used in the BLS survey. This yields 110,339 sons to search for, with 64,078 unique fathers. I find 51,699 sons using my matching algorithm, a match rate of 46.9%. From this larger sample of urban sons in 1920, I can create a subsample limited to just the cities included in the BLS survey. This subsample includes 45,698 sons in 1920 and I find 20,283 of them in 1940, a match rate of 44.4%.[27]

2.5 Biases in the Linking Procedure

Which sons do I match into the 1940 census? While my matching procedure is not able to link every son in my sample, I show in this subsection that the matches are not a selected subsample, so my results will not be biased by the data construction.

There are two main reasons sons in 1920 should not be found in 1940: death or emigration. However, mortality risk should be low for my sample, as I observe sons in childhood and search for them in adulthood before middle age.[28] In addition, I search for the sons in 1940, a year before Pearl Harbor, America's entry into World War II, and any combat fatalities.[29] It is difficult for the sons to move out of the sample because I am matching into the entire 1940 Federal Census, and emigration rates are quite low in this period.

Transcription errors are the most likely cause of an unmatched son from 1920 to 1940. These transcription errors could be caused by the historical census enumerators misrecording a respon-

1.67 sons. In the analysis that follows, I will cluster at least at the family level to account for multiple sons with the same father. Most clustering will be more conservative (at the city level) when I compare across cities with different Great Depression severity.

[27]The IPUMS match rate is approximately 10 percentage points lower than the BLS match rate. The primary explanation is lower quality transcription of names in the IPUMS sample. The name transcription I undertook in the BLS surveys was done with the explicit goal of accurately capturing the respondent names, while names were just one of many variables transcribed in the IPUMS data collection process. It is possible names were the least important variable collected by IPUMS, relative to demographically and economically vital variables like race, age, sex, and occupation. In addition, the sample frame of the BLS survey—stable community members, neither on public assistance, nor too rich—could create a list of sons who are more likely to be matched successfully.

[28]While under-1 mortality in 1920 was 98.1 per 1000 for white males, childhood mortality was only 2.7 per 1000 for white males aged 5 to 14. Mortality rates for white males aged 1 to 4 were slightly higher at 9.8. The low rates for the cohort in my sample persisted from 1920 to 1940 (Linder and Grove 1947).

[29]Mortality is also a potential problem for any matching procedure: individuals who die between the collection of the initial records and the second census wave should not be matched. With my algorithm, the rates of ghost-matching are very low. For a sample of individuals I know to be deceased by the 1940 census, only 5% are "located" in the 1940 census (Feigenbaum 2015).

dent's name or by today's data entry workers incorrectly digitizing the enumerator's original entry. The errors could occur in either the 1920 or the 1940 data. I quantify transcription quality that might make records more difficult to ultimately match in three ways: name string characteristics, enumerator effects, and family effects.

First, I use name commonness and length. More common names are likely easier to transcribe, but at the same time harder to match because common names will have more possible close matches. I measure commonness by counting the number of men in the 1% 1920 IPUMS sample with the same first (last) name. In addition, longer names have more letters, presenting more opportunities for transcription error.

Second, I make use of variation in enumerator quality. The BLS survey was originally recorded by 360 agents working in each of the 99 cities. Each agent enumerated an average of 30 to 40 surveys and worked in 4 to 5 different cities. These agents not only asked the survey respondents questions, but they also filled out the 11-page survey forms by hand. The quality of handwriting is extremely variable from enumerator to enumerator. While some printed in clear block print, most wrote in cursive with varying degrees of legibility, leading to variation in match rates from 30% to 80% between the best and worst enumerators.[30] To assess how important enumerators are to the matching process, I calculate the leave-one-out match rates for each record, which indicates the probability of matching the other people surveyed by that same enumerator.[31]

Third, I make use of the 3,088 sets of brothers included in my sample (including 1,889 pairs). Within families (sets of brothers), I calculate the leave-one-out match rate for each record, quantifying the probability a given son's brothers will be matched.

Which features of name strings predict a successful match from the BLS data into the 1940 census? I present these results in Table 2. Sons with more common names, both first and last, are less likely to be matched. Even with information on state of birth and year of birth, there will be far more possible matches for a common name like "James Smith" that—to avoid false

[30] In Figure A.8 of the Appendix, I present the signatures of four enumerators, two with match rates of approximately 80% and two with match rates of only 35%. Though not obvious in their signatures, the enumerators with high match rates tended to write in block print or very clean, simple cursive; the enumerators with very low match rates instead used sweeping, ornamental cursive, making the letters much more difficult to decipher for the researcher. The BLS enumerators are not the only agents whose handwriting might determine the match rates. Both the 1920 and 1940 censuses were, of course, recorded by enumerators, also with large variation in penmanship. However, because the Federal censuses were such large undertakings, very few of my sample records were enumerated by the same census agent, and I do not analyze match rates based on census enumerator.

[31] In Table A.3, I show that my main results are robust to cutting the data based on enumerator match rates.

positives—the matching algorithm will not identify as a match. Records with longer first names are more likely to be matched, perhaps because this correlates with commonness. I also find that the BLS survey enumerator, likely driven by the clarity of her handwriting, is a strong predictor of which records are ultimately matched; the coefficient in column (3) suggests that shifting from the enumerator with the lowest match rate—Marcia G. Brown (30%)—to the enumerator with the best—Lucille Henry (81%)—increases the probability of matching a son from the BLS sample to the 1940 census by 16 percentage points. In columns (4) and (5), I restrict to the sample of sons with brothers. Sons with a matched brother are 16.9 points more likely to be matched themselves, 15.5 when conditioning on name features and enumerator effects.

The matching rates in my sample are unrelated to any of the important economic variables I consider in my study. In Figure 3, I present binned scatter plots comparing match rates with father's age, father's income, local Great Depression severity measured by the decline in retail sales, and finally the interaction of father's income and Depression severity. In all plots, I control for first and last name string characteristics.[32] In none of the cases do the match rates vary systematically across covariates. In regressions corresponding to each sub-figure, I cannot reject that the variable of interest has no effect on the probability of making a match. This suggests that the matching procedure, while imperfect, is not introducing bias into the linked census samples.

2.6 Great Depression Severity

To assess the effects of the Great Depression on mobility—the father-son measures just described—I require variation in Great Depression severity across cities. To measure local Depression severity in this paper, I use the decline in retail sales per capita from 1929 to 1933. The Great Depression was felt throughout the country, but there was local variation in its severity. For example, per capita retail sales fell in Davenport, IA by nearly 50%, while the decline in Des Moines, IA was only 25%.

The path of the unemployment rate in the early twentieth century underscores the massive disruption of the Great Depression. Between 1900 and 1930, unemployment exceeded 10% only once (1921). From 1931 to 1940, unemployment never fell below 14%, hitting 25% in 1933 at the nadir of the Depression (Bordo et al. 1998).[33] While national unemployment statistics are available

[32] Specifically, I include controls for name commonness, length, and enumerator leave-one-out match rates. In the plot of match rates against the interaction term, I directly control for father's income and Depression severity as well.

[33] Real GNP fell by more than one-third during the Depression (Bordo et al. 1998). The Dow Jones lost 80% of its value from 1929 to 1932, and corporate profits were $2.7 billion in the red (Tyack et al. 1984).

throughout the Great Depression period, local rates, either at the county or city-level, are not. Instead, I use retail sales as an alternative, local measure of Depression severity.

I draw data at the county level from the Census of Retail Sales, originally digitized by Fishback et al. (2005). These censuses, taken biennially, allow me to measure retail sales per capita during the Depression era. Retail sales have been used frequently to measure local Depression severity and variation (Fishback et al. 2005, 2007, 2010); the measures were also some of the only indicators of economic conditions available to New Deal policymakers and administrators during the 1930s (Fishback et al. 2005). As my main measure of Depression downturn, I calculate the log difference of retail sales per capita in 1933, at the nadir, and in 1929, before the Depression.[34] I merge the counties in the retail sales data to the cities in my linked intergenerational mobility samples.

The economic downturn in specific cities in my sample paints a vivid picture of the severity and variation of the Great Depression. In Figure A.1, I map severity in each city in my sample according to the change in retail sales per capita. Portland, OR is the median city, with per capita retail sales declining by 0.41 log points from 1929 to 1933. Everett, WA suffered from a local downturn one standard deviation worse than Portland; retail sales fell in Everett by 0.55 log points. The downturn in Manchester, NH was relatively small, one standard deviation better than Portland, and yet per capita retail sales still fell by 0.29 log points.

In Appendix B, I explore several determinants of local Great Depression severity and review the literature on the Depression's geographic variation. I find cities specializing in durable manufacturing and in the extraction of raw materials used in construction and manufacturing to have suffered especially large downturns. In addition, the Depression was milder in the South.[35]

3 A Model of Mobility in a Depression

How do large macroeconomic downturns affect intergenerational mobility? To fix ideas about what factors may drive mobility, I consider a model based on Solon (2004), augmented with endogenous

[34] Specifically, I define Depression severity as $\log\left(\frac{retailsales_{1933}}{population_{1933}}\right) - \log\left(\frac{retailsales_{1929}}{population_{1929}}\right)$.

[35] The southern United States has especially low mobility, both historically (Olivetti and Paserman 2015) and today (Chetty et al. 2014a). Given the region-wide mild downturn, this could explain a spurious finding of Depression severity leading to more mobility. However, I find the opposite result. Further, I make use of city fixed effects, which subsume regional fixed effects. My main results are also robust to including interactions of father's income and census region indicators, a control that allows for different mobility elasticities in different regions. For these robustness checks, see Figure 7.

migration and macroeconomic shocks.[36] The model suggests that downturns will tend to lower intergenerational mobility, but could do so through two key channels: human capital and geographic migration.

Consider fathers and sons who live for two periods with each family indexed by i. In the first period, $t-1$, fathers supply labor inelastically and each earn incomes $Y_{i,t-1}$. Fathers can spend income on either consumption, $C_{i,t-1}$, or investment in the child's human capital, $I_{i,t-1}$. The sons work in the second period and have income Y_{it}. Families are liquidity constrained: a father cannot borrow against his son's future earnings in the first period to finance additional consumption or investment. The budget constraint for fathers is:

$$Y_{i,t-1} = C_{i,t-1} + I_{i,t-1} \tag{1}$$

Investments in human capital are transformed into the son's human capital, h_{it}, through a simple log production function:

$$h_{it} = \theta \log(I_{i,t-1}) + e_{it} \tag{2}$$

where e_{it} is the child's (genetic or cultural) endowment. The parameter θ governs the efficiency of human capital production. The endowment is inherited from the father:

$$e_{it} = \delta + \lambda e_{i,t-1} + v_{it} \tag{3}$$

where the heritability factor is $\lambda \in (0,1)$ and v_{it} is white-noise error. The earnings function is a standard Mincerian return to human capital, augmented by a son-city match specific return:

$$\log(Y_{it}) = \mu + p h_{it} + m_{it} \tag{4}$$

with p as the earnings return to human capital. m_{it} is the son-city match return, representing a good (or bad) economic fit between the child and his city of residence in adulthood. It is difficult for a son to predict whether he will be a good fit for a given city or not. Historically, connections like kin and ethnic networks were vital to job search and placement (Lebergott 1964; Rosenbloom 2002). Sons with richer fathers are more likely to have more connections in more cities and thus be able to learn more about city options. As the number of cities about which the son has information increases, the expected match quality will increase: in the limit, a son who could sample the entire

[36] I abstract away from government policy in the form of taxation or public human capital investment. The Solon model is a modification of the classic Becker and Tomes (1979) model of intergenerational mobility.

distribution of cities will know which city match is the best match for him. Richer sons may also be able to afford to migrate multiple times, trying out more cities while searching for the best match. In addition, sons with higher earning fathers receive greater human capital investment (as the model will predict) and more education might make a migrant better at finding city matches. Motivated by these links between parental income and match quality, I model the match return as a function of parental income in a reduced-form way:

$$m_{it} = \omega \log(Y_{i,t-1}) \tag{5}$$

where ω corresponds to the match quality return to parent's earnings.[37] Fathers have a standard Cobb-Douglas utility function with preferences for both consumption and child's income, weighted by the father's degree of altruism, α:

$$U_i = (1-\alpha)\log(C_{i,t-1}) + \alpha \log(Y_{it}) \tag{6}$$

Plugging the budget constraint in equation (1), the human capital production function in equation (2), the location match in equation (5), and the income function in equation (4) into the father's utility, the parent chooses the amount of investment in his child, $I_{i,t-1}$, to maximize:

$$U_i = (1-\alpha)\log(Y_{i,t-1} - I_{i,t-1}) + \alpha\mu + \alpha p\theta \log(I_{i,t-1}) + \alpha p e_{it} + \alpha\omega \log(Y_{i,t-1}) \tag{7}$$

The optimal amount of investment in the child is

$$I^*_{i,t-1} = Y_{i,t-1} \times \frac{\alpha p\theta}{1-\alpha(1-p\theta)} \tag{8}$$

increasing in parental resources, altruism, and the returns to human capital. Plugging the optimal choice into the human capital and income functions yields a function that suggests a regression of the son's income on the father's income (in logs):

$$\log(Y_{it}) = \mu^* + (p\theta + \omega)\log(Y_{i,t-1}) + p e_{it} \tag{9}$$

where $\mu^* = \mu + p\theta \log(\frac{\alpha p\theta}{1-\alpha(1-p\theta)})$. The coefficient on $\log(Y_{i,t-1})$, which I denote as β, is a persistence parameter; $1-\beta$ indicates the amount of mobility. A larger β suggests a stronger link between the

[37] I model the city match as a function of father's income directly, but it may be that match quality is also a function of human capital investment. Wozniak (2010) shows that the highly educated are more mobile in response to labor market shocks, and Malamud and Wozniak (2014) provide positive causal estimates of the effect of college education on migration. If these factors held during the Depression generation, that suggests that investment by fathers in sons could also increase expected city match quality. As I will show in equation (8), optimal investment is increasing in parental income and the model predictions are not affected by modeling city match as a function of income, investment, or both.

income of children and parents and thus less mobility. However, the true population β will not be exactly $p\theta+\omega$ because the "error" term is correlated with $\log(Y_{i,t-1})$. In fact, as pointed out by Solon (2004), this is the regression of an autoregressive variable (income) with an error term (the endowment) that is autoregressive as well. Working out the covariance and variance terms, the estimated β is

$$\hat{\beta} = \frac{p\theta+\omega+\lambda}{1+(p\theta+\omega)\lambda} \tag{10}$$

The four model parameters affect the degree of intergenerational mobility in the following ways: the persistence parameter, β, will be higher, which implies less intergenerational mobility, when (1) inheritance is stronger (λ); (2) the returns to human capital are higher (p); (3) the human capital investment efficiency is greater (θ); and (4) the location-specific match is greater in parent income (ω). These terms help direct my exploration of possible mechanisms that might be affected by the Great Depression and ultimately determine the degree of intergenerational mobility across my sample. Although it seems unlikely that the Depression changed the persistence of inherited traits, it may have changed the returns to human capital or the productivity of investment in human capital, as well as the ability of parents to help their children make good geographic matches.

The framework of the model also enables me to show that my results are not driven by measurement error. The Great Depression may induce measurement error in income, either father's or son's. With my historical data, I only observe a single year of income.[38] In cities with more severe Depression shocks, that single year of observed income may be a worse proxy for permanent income, the real Y_i in the model above.[39] When I introduce measurement error in either the father's or the son's income into the framework above, I find that the familiar attenuation bias from classical errors in variables applies, even though income in each generation is autoregressive, as is the endowment error term.[40] The probability limit of β is decreasing in the variance of measurement error. If the Depression increased the difficulty of observing accurate income data for affected fathers or sons, this would tend to overstate mobility (understating persistence), the opposite of what I find.

[38]Historical single-year income data make estimating precise permanent persistence parameters more difficult. However, to the extent that I am concerned with *differences* in mobility estimates across cities and all the mobility estimates are based on the same imperfect data sources, these issues should not affect my relative results.

[39]Recall that I observe father's income in 1918 or 1919, a decade before the Depression. Though this income measure will not be changed by the Depression, if the Depression significantly changes the father's permanent income, this measure a decade earlier will be less accurate in the cities with more severe downturns.

[40]See Appendix E. The attenuation bias is somewhat complicated by the λ term.

Differential variance in income between cities most and least affected by the Great Depression does not explain my results either. To show this, I consider the Corak (2004) decomposition of the persistence parameter β. Let ρ be the correlation between the log of father's income and the log of son's income. Let σ_{t-1} and σ_t be the standard deviation of log fathers' and log sons' income. Omitting, family subscripts i, it is straightforward to rewrite

$$\beta = \frac{Cov(\log(Y_{t-1}), \log(Y_t))}{Var(\log(Y_{t-1}))} = \rho \frac{\sigma_t}{\sigma_{t-1}}$$

This implies that even if the correlation between father's and son's outcomes is the same across cities, the persistence parameters may still be different. This variation in persistence could be driven solely by changes in the relative standard deviations of income in the two generations. In particular, if the Great Depression induced additional variation in income for sons in cities where the shocks were especially bad, β will be higher in those cities, implying less mobility. It does not appear that this mechanical relationship explains my results of less mobility in cities hit with more severe downturns, because neither the variance in log earnings of sons nor the variance in log earnings of fathers varies across cities with above or below median Depression shocks.[41]

4 Empirical Strategy and Results

In this section, I describe my empirical strategy to measure mobility and then present the main results of the paper: that Great Depression severity reduced intergenerational economic mobility. I also argue for the causal nature of my findings by documenting that mobility and future Depression severity by city were not related in the generation before the Depression.

4.1 How to Measure Mobility

Intergenerational mobility is the relationship between the outcomes in one generation and the outcomes of the following generation. In this paper, I focus on relative mobility in earnings and occupation, estimating persistence parameters that correspond to the slope coefficients in a regression of sons' outcomes on fathers' outcomes.

Let Y_i be the outcome of interest: log earnings, rank, or occupation score. Outcomes for fathers

[41] The variance in 1940 log earnings of sons is 0.6, both for sons growing up in cities hit with above median Depression downturns and below median downturns. The variance in 1920 log earnings of fathers is 0.1 across cities. The variance is much lower for fathers than sons due to the BLS sample construction.

are Y^f, outcomes for sons Y^s. The intergenerational persistence parameter is the β estimated by

$$Y_i^s = \alpha + \beta \cdot Y_i^f + \epsilon_i \tag{11}$$

Larger estimates of β mean a tighter link between father and son and thus less mobility. A society with no relationship between parents' and children's outcomes would have complete mobility and a persistence parameter of $\beta = 0$. Conversely, a perfectly immobile society, with relative income preserved across generations, would have a persistence parameter of $\beta = 1$.[42]

When focusing on income mobility, there are two traditional measures in the literature. The first, known as Intergenerational Elasticity (IGE), is estimated by logging both the father's income and the son's income. The IGE allows for a straightforward interpretation of intergenerational persistence: a 1 percent increase in the father's income is expected to increase the son's income by β. With logged incomes, $1-\beta$ captures regression to the mean in percentage terms (Mulligan 1997) and can be used to compute how many generations a rich or poor family would take to converge to the average (Mazumder 2015). However, the log-log relationship between father's and son's income is just one functional form. In practice, the implied linear relationship between logs of income does not always hold (Chetty et al. 2014a).[43] A second specification of mobility considers instead the relative income rank of both fathers and sons, relative to their respective cohorts (Dahl and DeLeire 2008; Chetty et al. 2014a). Thus, Y in equation (11) is the income percentile, and β is a rank parameter or the rank-rank coefficient. In my sample, this entails calculating the sons' earnings ranks in the national 1940 earnings distribution and the fathers' earnings ranks in the 1920 earnings distribution.[44]

Occupation score mobility is an alternative measure of economic status that can be used to estimate intergenerational mobility. In my sample, I observe occupations for both generations in the census and in the BLS survey. This allows me to compute occupational score mobility, where the occupation score is the median earnings by occupation. While occupation scores crudely ignore any variation within occupations, they may be better suited to deal with life-cycle bias in

[42] In theory, β is not bounded between 0 and 1. However, such extremes are unlikely. $\beta < 0$ only if relatively richer parents had relatively poorer children and $\beta > 1$ only when incomes "regress away from the mean" (Mulligan 1997).

[43] Another limitation of the log-log method generally is that the IGE parameter is affected by both the intergenerational correlation of income and the variances of income within each generation (Pfeffer and Killewald 2015).

[44] Mazumder (2015) highlights one reason why researchers might prefer rank parameters to IGE estimates when comparing mobility across cities, counties, or commuting zones: the IGE in a given city signals the regression to the mean in that city, while rank-rank coefficients are based on national income distributions.

mobility estimates. If occupations with higher status or lifetime earnings feature steeper earnings trajectories, observing a younger sample will lead to biased overestimates of mobility. However, such life-cycle induced bias will not be a problem for occupation score based mobility measures. Using occupation scores may also smooth out noise in annual earnings data and uncover a more accurate measure of economic standing when working with historical data.

There are three other potential measures of mobility that I do not focus on in this paper. First, the intergenerational correlation of education between generations could also describe mobility.[45] However, I am limited by the data available on completed years of schooling. I observe education only in 1940; thus, I know schooling for only the sons in my sample and the possibly selected subset of fathers found in 1940.[46] Second, wealth mobility would also be a valuable metric, but I do not observe summary measures of wealth for either generation.[47] Third, Mulligan (1997) focuses on consumption as a primary measure of intergenerational mobility. Though the fathers in my sample are drawn from a BLS cost of living survey with incredibly rich detail on consumption, the census data in 1940 are not well suited for measuring the consumption of the sample sons.[48]

4.2 Intergenerational Mobility in the Early Twentieth Century

How much intergenerational mobility was there in the early twentieth century? Across a variety of mobility measures and in both of my samples, I find more mobility historically than is typically found today. I also document that the relationship between father's and son's outcomes in my

[45] I calculate education mobility for the fathers and sons of Iowa from 1915 to 1940 (Feigenbaum 2014); Hilger (2015) uses education to calculate mobility from 1940 to the present.

[46] In addition, without years of schooling for the 1900 to 1920 sample, I would be unable to calculate educational mobility for the pre-Great Depression period.

[47] I observe housing value (for homeowners) and rent (for renters) in the 1920 and 1940 censuses. It is unclear how well these variables proxy for wealth historically, particularly for some sons who are not household heads in 1940.

[48] In the historical intergenerational mobility literature, two other non-regression alternatives to mobility measurement have been used: Altham statistics (Altham and Ferrie 2007; Long and Ferrie 2015, 2007) and rare surnames (Clark 2014). Altham statistics are useful when data on income are unavailable. Rather than forcing cardinal comparisons of occupational data, the Altham statistics are measures based on occupational transition tables that indicate how many sons are likely to enter occupations different from their fathers. However, these statistics require slotting occupations into a few categories: often the four groups are farmers, white-collar workers, skilled laborers, and unskilled laborers. For my sample of urban fathers and sons, the farmer category is nearly empty, and accurately assigning many of the other occupations is difficult. Further, as the number of categories in an occupation transition table increases, the measure becomes less informative, as almost all sons will be in different categories from their fathers. I take advantage of the income and detailed occupation measures in my data instead of using transition tables. Rather than link one generation to the next, Clark (2014) uses rare surnames and documents high levels of persistence in socio-economic status. Throughout a variety of periods and countries, Clark (2014) finds that rare surnames with high status in the initial period tend to be relatively overrepresented in later periods in high income or high status professions. While this technique yields interesting results for very long run mobility, it is not well suited to the task of comparing cities across the country in a one-generation period.

sample is linear in log earnings, earnings rank, and log occupation score.[49]

However, the claim that my samples show more mobility historically than prevails today should be tempered somewhat by five limitations of my historical data. First, my sample is built by linking fathers and sons across censuses, possibly imperfectly, which may downwards bias estimated coefficients. Second, I observe income and occupation in only one year for fathers and sons. To the extent that a single year of income or occupation is a noisy signal of permanent status, the estimated persistence parameters will be biased towards zero. Third, the sons in my sample range in age from 20 to 40, but many are in their late 20s. Mobility can appear spuriously high in samples with especially young sons and old fathers (Corak 2006; Grawe 2004; Mazumder 2015). Fourth, my BLS sample was restricted by the original enumerators to collect data only on families in the middle of the income distribution in 1920. Solon (1989) argues that homogeneous samples will also bias the persistence parameters downwards, and Chetty et al. (2014b) show how estimated mobility rates vary depending on the income distribution of the parents in the sample. Finally, my sample is restricted to white families in urban areas in 1920 and only father-son links, so any estimates of mobility are specific to that demographic group.

The measures of mobility in my two samples are approximately linear, as documented by Figure 4. In each binned scatter plot, I pool fathers into 20 bins, each representing five percent of the data, ranked by the father's 1920 outcome. The linearity is at least partially driven by the original BLS plans to survey only middle-income wage and salary workers in urban areas. However, it enables me to represent relative mobility or persistence with one simple term, the β from equation (11).

In Table 3, I present estimates of several mobility measures across my two main linked samples. In all cases I regress son's outcomes on father's outcomes, controlling for quartic polynomials in father's age and son's age following Lee and Solon (2009). I cluster my standard errors at the family level because brothers in the sample will have the same father with the same data. I use state fixed effects and then city fixed effects, based on the son's city of residence in the BLS sample in 1918-1919, to control for any state-level or city-level common shocks to long run outcomes.

Consider first the IGE estimates in columns (1) and (2), calculated by regressing the son's log earnings in 1940 on the father's log earnings in 1918. The IGE parameter is 0.275 with state fixed

[49] On intergenerational mobility today, see Chetty et al. (2014a,b), Solon (1999), Corak (2006). On mobility in the early twentieth century, see Parman (2011). In my own work, I document higher rates of income mobility in Iowa in the early twentieth century than today (Feigenbaum 2014).

effects or 0.272 with city fixed effects. In both cases, the results imply much greater mobility than is often found in contemporary American data, where common IGE estimates are often greater than 0.4 or 0.45 (Corak 2006).[50]

As pointed out by Chetty et al. (2014a), the IGE estimates assume a linear relationship between log incomes across generations. That assumption is not always satisfied in contemporary data (Nybom and Stuhler 2014). While the linear approximation fits my data well, I supplement my IGE analysis with mobility estimates based on income ranks. Calculating each son's position in the national income distribution in 1940 is straightforward with the full 1940 census with earnings data. Calculating ranks for the fathers in 1918 is more difficult because the full income distribution is unknown. I calculate ranks in three ways, each yielding similar results. In each case I start with the national 1940 income distribution, based on the 1% IPUMS sample of the 1940 census. First, I adjust for changes in the price level between 1918 and 1940. Second, I adjust for inflation and changing occupation shares by weighting each observation by the share of men with that occupation in the 1920 census. Third, I adjust for inflation and changing occupation and industry shares by weighting each observation by the share of men with that occupation and industry in the 1920 census. In columns (3) and (4) of Table 3, I present results using the first method.[51] Similar to the IGE parameters, I find significantly more mobility for my historic sample than is found in current data using the rank-rank method (Chetty et al. 2014a,b; Mazumder 2015).

Occupation score mobility, presented in the final four columns of Table 3, provides an alternative to income mobility measures. Occupation scores are calculated as the median earnings within an occupation code. While the scores eliminate variation in outcomes within an occupation, there are reasons to prefer the scores as a measure of mobility. For one, a single year of occupation is likely a much less noisy proxy for lifetime average earnings (or for socio-economic class or status) than a single year of income. Further, occupations should suffer from a smaller life-cycle bias than contemporaneous income, a complication that is known to drive down income mobility estimates when

[50]Both Mazumder (2015) and Mazumder (2005), meanwhile, find much higher IGEs, greater than 0.6. Chetty et al. (2014a) also estimate an IGE of 0.45, but argue that the parameter is highly sensitive to the treatment of children or parents with very small or very large incomes. They also suggest that in contemporary data the conditional relationship between parent's log income and child's log income is non-linear. However, for the historical BLS sample I consider in this paper—a sample drawn from a set of fathers with a condensed distribution of income—I cannot reject linearity, as seen in Figure 4 (a).

[51]See Appendix C for more detail on all three methods of calculating the father's earnings rank, including a plot of the empirical cumulative distribution functions.

sons are observed early in their working lives (Corak 2006). Finally, occupations may be generally more accurate measures of socio-economic status than income. More vital to my study, occupation scores are necessary to estimate mobility in the IPUMS matched sample as the 1920 census lacks income data. I present occupation score elasticities in columns (5) through (8), regressing the log of son's occupation score in 1940 on the father's occupation score in 1920, for the BLS sample and then the IPUMS sample. Occupation score persistence in the BLS sample is slightly larger than occupation score mobility in the IPUMS sample, but both rates are comparable.

The father and son ages at which income or occupations are observed often complicate the intergenerational mobility estimates. For example, many early IGE studies have been criticized for observing sons who were too young and fathers who were too old, which biases parameter estimates down and implies spuriously large levels of mobility (Corak 2006; Grawe 2004; Mazumder 2015). I show in Appendix Figure A.9 that my mobility estimates are all relatively stable across sons ages.

The results in Table 3 suggest that mobility was higher in the early twentieth century than it is today; in the next section, I explore what role the Great Depression played in driving this result.

4.3 The Great Depression Decreased Economic Mobility

At the nadir of the Depression in 1933, more than one-quarter of the American work force was unemployed, and others saw their hours or wages fall and their life savings and home values erode. Did this disruption break the economic links between fathers and sons, driving mobility higher in cities that experienced more severe downturns? Or did the Depression underscore differences in outcomes for children in different parts of the income distribution, reducing upward mobility for sons with poorer parents more than it reduced mobility for middle class sons?

To summarize the results in this section, I graph two binned scatter plots of father's earnings against son's earnings in my matched BLS sample in Figure 5. In (a), I plot log earnings, and in (b), earnings rank. Each point represents one-twentieth of the data in each category, plotted at the mean for father's and son's outcomes within the bin; these binned scatter plots present the raw earnings data, without controlling for father or son ages or any fixed effects. The relationship between father's and son's earnings is steeper in cities where the downturn was larger than the median.[52] A steeper curve implies a stronger link between father's and son's outcomes and thus

[52] Cutting Depression severity at the median produces a convenient graphical presentation, but as I show in Table 4, the results hold with a more continuous measure of Depression severity.

less mobility: the sons growing up in cities where the Great Depression was worse have much less mobility than sons in cities with relatively mild downturns.

Figure 5 also indicates a surprising fact about the effect of the Depression on absolute mobility, defined by Chetty et al. (2014a) as the expected adult outcome for a son born to a father at a given rank. For both the IGE and the rank-rank, Figure 5 shows that the best fit lines overlap around the middle of father's distribution, suggesting that a son born to a father in the middle of the distribution has the same expected earnings in 1940 whether he grew up in a city hit by a severe or mild downturn. Absolute mobility, however, does respond to the Great Depression for the sons of both richer and poorer fathers. The typical son in the bottom half of the father's earnings distribution did worse in cities with more severe downturns, but absolute mobility increases for sons from the top half of the distribution. The best fit lines in Figure 5 provide a simple measure of absolute mobility. For example, a son born to a father at the 25th percentile of earnings could expect to be in the 41st percentile in 1940 if he grew up in a city with a downturn less severe than the median, but in the 38th percentile in a city with a more severe Depression. For sons born at the 75th percentile, the expected rankings are 48th percentile from the less severe cities and 54th percentile from the more severe cities. In Section 5, I will explore why it might be the case that a locally severe downturn is so harmful to the sons of poorer fathers and beneficial to the sons of richer fathers.

The graphical results in Figure 5 are only illustrative, and thus I turn to a fuller regression analysis, controlling for covariates at both the father-son and city-level. To determine the direction of the Great Depression effect on mobility, I run regressions of the form:

$$Y_{i,son} = \beta_0 + \beta_1 \times Y_{i,father} + \beta_2 \times Y_{i,father} \times GD_{city} + \gamma_{city} + \epsilon_i \tag{12}$$

where $Y_{i,son}$ is the son's outcome in 1940 and $Y_{i,father}$ is the father's outcome in the 1918-19 BLS sample or the 1920 IPUMS sample. The outcomes are either log earnings, position in the earnings distribution, or occupation score. GD_{city} is the severity of the Great Depression in the son's city of residence in childhood. I cluster standard errors at the city level, to reflect that each son growing up in a city is subject to common city-level shocks and the same observed city-level Depression severity measure.

Two different parameterizations of severity are used, both based on my underlying measure of the decline in per capita retail sales between 1929 and 1933. In the first, I normalize the sales growth, subtracting the mean in my sample and dividing by the standard deviation. In the second, I generate an indicator variable that takes a one in cities with a downturn worse than median and a zero in cities with a downturn more mild than median. In both cases, the β_1 parameter can be easily interpreted. When the severity is normalized, β_1 is the persistence parameter in a city with a mean Great Depression downturn; when severity is measured with an indicator variable, β_1 is the persistence in a city with a more mild than median downturn. The key variable of interest, however, is β_2, the interaction term. When severity is normalized, β_2 will imply the addition or reduction in persistence with a one standard deviation worse downturn. This is the difference between the downturn in Portland, OR (retail sales fell by 0.41 log points from 1929 to 1933) and Everett, WA (retail sales fell by 0.55 log points). When severity is an indicator for worse than median local conditions, β_2 will imply the change in persistence if the downturn was worse than median.

I find that in cities with more severe Depression downturns, intergenerational mobility is lower, as presented in Tables 4 and 5.[53] In the first table, I examine mobility measured by the IGE and the rank-rank coefficient. Consider the estimated IGE parameter: it is 0.280 in column (2) with city fixed effects. A city with a downturn one standard deviation worse than average, however, is expected to have a persistence parameter of 0.388, indicating significantly less mobility. In columns (3) and (4), I use the coarser parameterization of Depression severity, identifying cities with downturns worse than the median city, Portland, OR. The point estimates suggest similarly dramatic effects of a worse than median Depression on mobility. For cities hit with severe downturns, the predicted persistence is 0.184 to 0.197 points higher, roughly the difference between the United States and Sweden in contemporary IGE estimates (Corak 2013). The rank-rank results in columns (5) through (8) depict similarly dramatic effects of the Depression on mobility.[54] Today, the difference in rank-rank mobility between one of the least mobile cities in America (Charlotte, NC) and one

[53]In both tables, the odd columns include state fixed effects and direct controls for city-level Great Depression severity, while the even columns replicate the previous column with the addition of city fixed effects.

[54]The samples included vary slightly between the total number of sons matched from 1920 to 1940, as well as between the IGE and the rank measures of mobility. I drop all sons with $0 in earnings in the 1940 census from the IGE estimates because I cannot log 0. In the rank-rank estimates, sons with no earnings are included because I am able to calculate their position in the earnings distribution. However, in both samples I exclude the sons with no reported labor earnings in 1940 but who do report capital or self-employment earnings, as their ranking, based on no earnings, would be very misleading. In Appendix D, I show that my results are robust to imputing capital income for sons with no labor earnings, using the 1950 census. Finally, I drop 516 sons with illegible or mistranscribed earnings in the 1940 census.

of the most (Salt Lake City, UT) is 0.133 (Chetty et al. 2014a). This difference is less than the difference in rank-rank mobility between a city hit with a one standard deviation more severe local Depression and a one standard deviation more mild shock, or the difference between a city hit with an above median severity downturn versus not.[55]

Based on results from both my BLS sample and the larger IPUMS-based sample, the Great Depression also decreased occupational mobility. The correlation between occupation scores in two generations is a coarser measure of economic status, as it varies only across occupations and not within them. In columns (1) through (4) of Table 5, I measure occupation score mobility in the sample of fathers and sons matched between the BLS survey and the 1940 census. Though the results are somewhat less precise, I find similarly that cities with more severe Great Depression downturns have much less intergenerational mobility. Turning to my larger sample of fathers and sons living in the BLS cities in 1920, based on the IPUMS 1920 sample, in columns (5) through (8) of Table 5, I find large Great Depression effects on mobility as well.[56]

Comparing the Depression effects to the overall persistence parameter illustrates the magnitude of the effects. In columns (1) and (2), as well as columns (5) and (6), I show that the intergenerational occupation score elasticity is roughly one-third larger in a city with a one standard deviation worse Depression decline. Similarly, when I measure severity as worse than the median downturn or not, as in columns (3) and (4) and columns (7) and (8), I find the occupation score elasticity increases by almost 50 percent.

The Great Depression effects on mobility do not vary across age ranges, as I demonstrate in Figure 6. Here, I replicate the specification from column (2) of Table 4 and column (6) of Table 5, but allow the Depression interaction to vary by son's age in 1940. In the BLS sample, the point estimates are consistently between 0.101 and 0.125, with 95% confidence intervals from roughly 0.014 to 0.212. Though the confidence intervals are slightly smaller in the second panel, based on data from the IPUMS 1920 to 1940 sample, they do overlap with zero for several of the son's ages. Nonetheless, the point estimates are similarly stable.

[55] In Table 4, I measure earnings without adjusting for differences in local prices. In Table A.1, I show that my main results are robust to accounting for variation in local price levels. I use median city rents in 1920 to calculate fathers' real earnings and median county rents in 1940 to calculate sons' real earnings.

[56] The Depression effect on occupational mobility is not particular to the cities sampled by the BLS. Using all urban sons in the 1920 IPUMS census sample matched ahead to 1940, I show in Table A.2 similar declines in mobility in cities with more severe Depression downturns.

Although there are five key limitations of my data that make it difficult to compare overall historical mobility rates to estimates for recent decades, none of these drawbacks will bias my estimates of the Depression effects on mobility. First, as I documented in Section 2.5, imperfections in the matching procedure do not vary systematically across families or cities in ways that could affect the estimated Depression effect. In addition, I show in Table A.3 that my results are robust to various sample restrictions based on enumerator quality, one of the stronger predictors of linking records from 1920 to 1940. Second, I only observe income or occupations in one year for both the fathers and sons in my sample. Overall, this might downwards bias the IGE or rank-rank parameters, suggesting more mobility than there actually was. However, I only observe one year of income for all fathers and sons in my data. This downward bias will affect mobility for all cities, whether the Depression downturn was severe or not. Third, the ages of my sons are somewhat younger relative to other mobility samples, but this does not drive the differential effects of the Depression, as I show in Figure 6. Fourth, while the BLS sample was restricted to the middle of the earnings distribution, these restrictions were consistent across cities. And finally, while my conclusions are necessarily limited to the demographic group I observe in the BLS—white families living in cities—I argue that this sample is particularly relevant for understanding the effects of the Great Depression. 1920 marked the first year America was a majority urban nation.

4.4 Falsification: Cities with Severe Downturns Do Not Always Have Lower Mobility

In the previous section, I showed that the intergenerational links between fathers and sons were stronger in cities that suffered larger Great Depression declines. However, Depression shocks are not randomly assigned throughout the country. In this section, I turn to two additional data sources on mobility—a linked sample from 1900 to 1920, as well as data on intergenerational mobility in the late 20th century—to show that Depression severity does not predict mobility for either the generation before the Depression or for the contemporary period.

By studying the generation before the Great Depression, I can uncover city variation in mobility that is unrelated to large macroeconomic downturns. If mobility in this period were related, either positively or negatively, with Depression shocks, that would suggest that the finding presented in the main section does not reveal the effects of the Depression on mobility. One concern would be that places with especially low levels of mobility in all periods would be more likely to be hit by Great Depression shocks. This could be because low mobility causes regional severity in the downturn or simply that the two variables are correlated. For example, certain industries, which may be regionally concentrated, could reduce economic mobility and could have been particularly susceptible to employment loss in the Depression. Implicitly, this is a differences-in-differences framework, comparing cities before and after the Depression with larger and smaller Depression downturns. The identifying assumption, as in a diff-and-diff, is that no other determinants of mobility are changing over this period differentially across cities in a way that is correlated with the downturn.[57]

To estimate mobility before the Depression, I create an additional matched sample that links fathers from the 1900 census to their sons in 1920. I begin with the IPUMS 1900 6% sample, limiting to only families living in one of the cities in the BLS survey with both a father and at least one son present in the household. There are 70,561 such families, with a total of 125,078 sons. I then search for these sons in the complete 1920 census, following the linking procedure used throughout this paper. I locate 58,600 sons for a match rate of 46.9%. However, neither the 1900 nor the 1920 census includes income or earnings information. Instead, I make use of occupation scores as a measure of economic status.[58] In the previous section, I found that the generation coming of age during and after the Great Depression had less intergenerational mobility in cities hit by more severe downturns. Does this same pattern emerge in the previous generation?

I present the "effects" of the Great Depression on mobility between 1900 and 1920 in Table 6. Mobility is generally high in this sample, which I document in the first row of the table. The estimated elasticity of a son's occupation score in 1920 with respect to his father's occupation score in 1900 ranges from 0.110 to 0.115, lower than the corresponding estimates in Table 3.[59] However,

[57]New Deal spending might be one obvious candidate. However, as I document in Section 6, particularly in Table A.5, the New Deal had no significant effects on mobility. If anything, certain forms of New Deal aid might be expected to increase mobility and, given that the spending was targeted at cities with the most severe downturns, this would bias me against finding the Depression and mobility effects I observe in Table 3.

[58]For the fathers in 1900, I use the occupation scores based on occupation codes created by IPUMS. However, for the sons in 1920, standardized occupation codes are not available in the complete count data. Instead, I draw on the raw occupation strings entered by census enumerators and transcribed by Ancestry.com and manually link these strings to occupation codes and strings, basing my links on the occupation coding in the 1% IPUMS sample of the 1920 census. Due to missing or unclear occupation strings, I only observe an occupation score for 42,393 matched father and son pairs, 72% of the matched pairs. I use this sample in Table 6.

[59]It is not surprising that there was more occupational mobility from 1900 to 1920 than I found between 1920 and 1940. Long and Ferrie (2007) show higher levels of mobility for a linked sample of fathers and sons between 1860 and 1880 than between 1880 and 1900, suggesting that mobility rates decreased in the nineteenth and early twentieth century. The contemporary pattern of stable mobility rates documented by Lee and Solon (2009) and Chetty et al. (2014b) does not begin until the 1970s.

mobility is no higher or lower for sons born in cities that would, thirty years later, experience more severe contractions during the Great Depression. In the third and fourth columns, I include an interaction of Great Depression severity and the log of the father's occupation score; in the fifth and sixth columns, the interaction is between an indicator for a worse than median downturn and the log of the father's occupation score. But in all cases, with or without city fixed effects, the interaction coefficients are small and statistically insignificant. Further, the corresponding estimated actual Great Depression effects from Table 4 for both the BLS sample of sons or the IPUMS sample are well above the confidence intervals for each of the interaction terms in Table 6.

Table 6 suggests that there are no pre-treatment differences in economic mobility rates across cities with varying levels of Depression downturns. This result buttresses my main claim that the Depression drove lower levels of mobility.

I also find that the Great Depression does not have a lasting effect on mobility in the current period.[60] Drawing on county-level data on mobility from Chetty et al. (2014b,a), which measures mobility for the cohort of children born between 1980 and 1982 and their parents, I regress mobility against Great Depression severity. If the Depression did influence contemporary mobility, that might prompt concern that the observed severity effect on mobility in the previous section was driven by other fixed local factors that determined both mobility and severity. However, in Table 7, I show that there is no relationship between Great Depression severity and rates of economic mobility today, measured either by the slope of the rank-rank regressions or the expected adult outcomes for children born in the 25th percentile.[61] These results further strengthen my claim that the cities shocked by the Great Depression do not have fundamentally different rates of mobility from cities with milder downturns.[62]

The results in this section have underscored the causal argument I offered in the previous sections: cities with more severe downturns during the Great Depression had less mobility in that

[60] I describe this exercise in full detail in Appendix F.

[61] I merge my county-level measures of Great Depression severity to the county level measures of recent mobility. Cities comprised of multiple counties like New York and St. Louis enter the sample multiple times.

[62] The results also rule out very long run persistence of the Great Depression on mobility many generations later. While individual economic outcomes are likely the result of many generations of family inputs (Long and Ferrie 2015), given the high levels of geographic mobility in the US during the last century, these null results are not surprising and not the ideal test of long run economic shocks on multi-generation mobility. A better test would link people today to their grandfathers or great-grandfathers during the Depression and to use the location of the grandfather rather than the location of the child to measure Depression severity. However, due to privacy restrictions on census data, such multigenerational matches ending with final generations in the contemporary period are not yet possible.

period, but not before or much later. It is unlikely, therefore, that the Depression effects on mobility are merely correlated with some underlying geographic fixed driver of mobility. The Depression reduced mobility for the unlucky sons growing up in the worst hit cities.

4.5 Robustness: City Heterogeneity Does Not Reduce the Depression Mobility Effect

In the previous section, I showed that Great Depression severity does not predict mobility for the generation before the Depression. However, it could be the case that other city-level heterogeneity, which correlates with the Great Depression, drove mobility between 1920 and 1940. In this section, I consider several such sources of variation—industry mix, regional fixed effects, age variation, population size, historical growth rates, and inequality—and show that my central finding is robust.

I use the same empirical framework throughout this section. Let $Z_{i,city}$ be some candidate father- or city-level covariate that may be correlated with the Great Depression and driving my results. I run regressions of the form:

$$Y_{i,son} = \beta_0 + \beta_1 \times Y_{i,father} + \beta_2 \times Y_{i,father} \times GD_{city} + \beta_3 \times Y_{i,father} \times Z_{city} + \gamma_{city} + \epsilon_i \tag{13}$$

with variables as in equation (12).[63] The β_3 term allows mobility to differ according to $Z_{i,city}$. When the candidate covariate is scalar—population in 1920, 1920s growth rate—I recenter the variable around zero to simplify the interpretation of β_2, the effect of the Depression on intergenerational mobility. When the candidate covariate is a set of indicators—father's industry, census region or division, son's age—I saturate the regression and estimate a mobility rate for each indicator, dropping β_1. In both cases, the variable of interest remains β_2: the decrease in mobility due to a more severe Depression downturn. I plot β_2 for each candidate covariate in Figure 7, measuring mobility with both the intergenerational elasticity of income and the rank-rank parameter. The samples and controls in the baseline (first row) match exactly columns (2) and (6) of Table 4.

Industry-mix at the city-level was one of the strongest predictors of Depression severity, which I document in Appendix B. Industry may also determine the degree of intergenerational mobility. Certain fields or professions may be extremely rigid: miners' sons become miners and remain relatively poor, while the sons of mine electricians and carpenters may move on to more lucrative

[63] $Y_{i,son}$ is the son's outcome in 1940 and $Y_{i,father}$ is the father's outcome in the 1918-19 BLS sample. The outcomes are log earnings, position in the earnings distribution, or occupation score. GD_{city} is the severity of the Great Depression in the son's childhood city of residence. I cluster standard errors at the city level because sons in the same city are subject to common city-level shocks and the same observed city-level Depression severity.

employment. Did cities with industries that were less mobile also have more severe Depression downturns, and could this explain my main finding? As I show in the second and third rows of Figure 7, it is unlikely. I first include fixed effects for each father's industry, allowing the son's outcome to differ based on father's industry. I then include interactions of these fixed effects with father's income, allowing mobility to vary by industry as well. In both cases, the Depression effect on mobility remains stable.[64]

The Depression exhibited some degree of regional variation (Rosenbloom and Sundstrom 1999). Intergenerational mobility has a strong regional component, both historically and in the current period. Studying mobility in the early twentieth century using socioeconomic patterns in given names, Olivetti and Paserman (2015) find low mobility in the South and high mobility in the Midwest. These patterns persist today: Chetty et al. (2014a) also document low mobility in the South and high mobility in the plains states. If the Depression had struck regions that had lower rates of mobility, either coincidentally or because low mobility drove Depression severity, that could explain my results. In the previous section, I showed that this is not likely to be the case, because, at the city level, mobility from 1900 to 1920 is unrelated to Depression severity. Here, I push the analysis a step further. I include in my main specification an interaction of father's log earnings or earnings rank with census region fixed effects.[65] This allows there to be a different rate of mobility in each of the four census regions separate from the Depression affected mobility rate. As shown in the fourth row of Figure 7, the main results are generally unaffected by this control. Allowing mobility to vary at the nine census division levels does not affect the main results either (fifth row).[66] Finally, when I allow both industry-specific and census division specific mobility rates, the main results remain stable (sixth row).

One critique of many estimates of intergenerational mobility is life-cycle bias. Samples with either older fathers or younger sons tend to exhibit more mobility (Corak 2006). The BLS city samples may have different age profiles, or the Great Depression may have affected the trajectory of earnings for sons over time. In Figure 6, I documented the stability of the Depression interaction effect across sons' ages in my sample. I confirm that finding in Figure 7 (seventh row), showing

[64] I code industries that were initially recorded as strings in both the 1920 and 1940 censuses, as well as in the BLS sample using the IPUMS 1950 standardized industry list; I aggregate the industries to 14 broad categories, roughly corresponding to SIC 2-digit codes.

[65] The census divides the 50 states into four census regions and nine census divisions.

[66] I do not include census region or division fixed effects directly because they are subsumed by city fixed effects.

that the overall Depression effect does not vary when allowing for age-specific mobility rates.

Several other city-level covariates could influence mobility. The cities sampled by the BLS range in size dramatically from New York, NY to Calumet, MI. Smaller cities may have less mobility and may have been more susceptible to Depression downturns because they were less industrially diversified or because local labor markets were too small. There is also some evidence that the Roaring 20s, the boom preceding the Depression, may have exacerbated the downturn.[67] Inequality could also have been correlated with the Depression, and there is a well known relationship between inequality and mobility today (Krueger 2012; Corak 2013). Unfortunately, inequality measures are not abundant in the early 20th century; instead, I turn to city-level data from the Census of Manufactures and calculate a worker-skill premium. For each city, I know the wages paid and the number of workers in three manufacturing skill categories: operators, clerks, and officers. I calculate the skill premium in each city as the ratio of wages per operator against wages per clerk and officer. As I show in the final three rows of Figure 7, allowing mobility to vary across any of these city-level dimensions does not affect the main finding.

Variations in military spending as the United States prepared to enter WWII could be another potential driver of mobility patterns. The wartime labor market could have compressed earnings variation, which might affect mobility overall. Alternatively, local defense spending could have affected sons' earnings in different ways across different cities. If these war effects were correlated with Depression severity, perhaps because spending was targeted to distressed areas, that could threaten my interpretation of the Depression effects. However, I find this to be unlikely. To begin with, I observe the earnings and occupations of sons in the 1940 census which refers to 1939 outcomes. Germany did not invade Poland until September 1939, and the US did not enter the war until after Japan attacked Pearl Harbor, on December 7, 1941. US defense spending remained at around 2% of GDP in 1939 and 1940, its average share throughout the 1930s. Cullen and Fishback (2006) argue that the economic mobilization for the war "started gradually"; monthly munitions output at the end of 1940 was at barely 2% of the peak in late 1943 (Krug 1945). The massive production of the instruments of war did not begin until well after I observe the sons in my sample.

In the previous sections, I have shown that Great Depression severity reduced intergenerational

[67] I explore determinants of local Depression severity in Appendix B and find a weak connection between growth in the 1920s and the downturn, at least for the cities in the BLS sample.

mobility for sons growing up in cities with severe downturns. In the next section, I ask why.

5 Mechanisms: How Did the Depression Lower Mobility?

Sons growing up in cities with worse downturns during the Great Depression were "locked" into their fathers' outcomes. Guided by my model of intergenerational mobility, I explore several possible mechanisms: migration, education, and inequality. Ultimately, I find that differential geographic mobility by sons of rich and poor fathers offers the best explanation for my findings.

5.1 Geographic Mobility

Since de Tocqueville (1839), observers have highlighted the high rates of geographic mobility in America. The sons in my sample are highly mobile, and this mobility differs by both father's income and Great Depression severity. In particular, I find that increased local Great Depression severity prompts all sons to migrate, but that the sons of richer fathers are more likely to make *better* migration choices, moving to cities and counties with less severe Depression downturns.

Historically, the young are among the most geographically mobile population groups. Counties with larger population shares between 10 and 19 years of age in 1930—a group comprising half of the sons in my sample—had more out-migration during the 1930s (Fishback et al. 2006). The sons in my sample are no different, moving an average of 165 miles. Of those sons who leave their county of residence in 1918-19, they move an average of 340 miles. In my sample, 21% of the sons move to a different state between 1918-19 and 1940. Of those remaining in-state, 33% move counties.

Local Great Depression severity drove this migration. Overall, geographic mobility was lower during the Depression decade than during previous decades, but this decline in mobility is common during recessions.[68] However, across the country, I find that the sons in my sample growing up in cities suffering more severe Depression declines were more likely to migrate. Figure 8 shows that both the probability of moving to a new state and the distance moved from 1920 to 1940 increase for sons living in cities with larger downturns. Sons in cities with a Depression downturn one standard deviation worse than the mean were 3% more likely to move out of state and moved 40% farther.[69] In Figure 8, I also explore whether this mobility effect differed by father's earnings.

[68] See for example, the index of internal redistribution of population in Figure 1.2 of Rosenbloom (2002). There was less mobility in the 1930s than during the 1920s or 1940s, but low levels of migration during the 1890s and 1910s, decades with multiple macroeconomic contractions. During the first American depression in 1819, migration to the frontier declined (Lebergott 1964).

[69] The Depression pushed sons to move, but where did they go? In the appendix, I map the locations of all the

Splitting the sample of sons at the median of father's earning ($1300 in 1920 dollars), it appears that sons with richer fathers may be somewhat more mobile in response to increased Depression severity, but these differences are not statistically significant.[70]

However, the sons of richer fathers and the sons of poorer fathers did not move to the same sorts of places. In Figure 9, I show that sons from richer families made much better migration choices. I measure the quality of a migration by the difference in Great Depression downturn severity between the home county in 1920 and the destination county in 1940: a negative change in severity implies a move to a region with a less severe Depression. Among the sons moving out of state, the quality of the sons' moves increased with father's earnings in 1918-1919, measured either as the log of earnings or the father's ranking in the earnings distribution.[71]

The differential in directed mobility can explain my main finding that the Great Depression decreased intergenerational economic mobility. To see this, I split my sample into movers and stayers and estimate intergenerational mobility on these selected samples in Table A.4. For both the IGE and rank-rank parameters, I find the Depression effect is concentrated within the migrant sample rather than the non-migrant sample.

In cities with more severe downturns, more sons migrated and thus had to make a decision about where to live. The sons with richer fathers were able to make better choices, moving to areas that experienced a less severe Depression shock. This explains the increase in their average outcomes, relative to the sons of richer fathers in cities with less severe downturns, in Figure 5. Even if the richer sons in mild Depression cities were just as likely or as able to make good migration choices, fewer of them did so, as the milder Depression compelled fewer to migrate in the first place. Conversely, Figure 9 shows that the sons of poorer fathers migrated to areas with more severe local Depression downturns, a choice that was likely to lower their eventual economic outcomes. Because the sons in cities with more severe Depression downturns were more likely to migrate, more of the

sons in my sample in Figure A.2. Among the sons moving out of state, 19% of the sample ended up in California and another 7% in New York. Others moved to the industrial midwest, as 4 to 5% of sons went to Illinois, Michigan, Pennsylvania, Missouri, Ohio, and Indiana.

[70]In the regressions that correspond to the plots in Figure 8 (a), the semi-elasticity of distance moved with respect to Depression severity is 0.446 for sons with fathers earning above the median in 1920 and 0.300 for sons with fathers earnings below the median, but with overlapping confidence intervals. Similarly, for Figure 8 (b), the two slopes are 0.041 and 0.016, but are not statistically significantly different.

[71]Expanding the sample to include all sons, not just those migrating between 1920 and 1940, reduces the estimated slopes in Figure 9, but does not change the main point that the sons of richer fathers were more likely to move to locations with a milder Depression downturn. In addition, controlling for 1920 severity or including 1920 city fixed effects does not break the relationship.

poorer sons in these cities moved to cities with even worse local economic conditions. During the Great Recession, Yagan (2014) finds that richer workers bear a lower incidence rate from a local downturn thanks to migration. The same pattern played out in the Great Depression, with more geographic mobility and smarter geographic mobility protecting the sons of richer fathers in cities with worse downturns.

5.2 Years of Education

My model suggests that the Great Depression could have determined intergenerational mobility through a differential change in educational attainment. I find that in all cities the sons of richer fathers get more years of education, but that this education gap does not vary according to Great Depression severity. Therefore, education is not the mechanism through which the Depression affected mobility.

In cities with worse downturns, sons of poorer fathers may have been more likely to drop out of school earlier to enter the labor force, with income and employment reduced for their parents.[72] Had they remained in school, some of those sons would have climbed the education and income ladder: these Depression-induced drop outs would thus decrease local mobility relative to a city with a more mild downturn. Sociologists have speculated that, for the sons of poorer fathers, educational opportunities beyond high school may have been especially sensitive to the limits on resources during the Great Depression (Elder 1999, p. 154).

However, the relationship between the Great Depression and education is much more complex. As I document in Figure 10, for sons growing up in cities with worse downturns, both total years of education and high school graduation rates actually increase. These changes are driven by a shift in the distribution of completed schooling from 8 to 12 years, as is clear in the histogram in Figure 10 (a). The high school graduation rate was only 45% in cities with mild Depressions, but was 51% in cities with downturns worse than median. These results echo the findings in Goldin and Katz (1997) and Goldin (1998) on the positive effects of the Great Depression on education. Drawing on data at the state level, Goldin and Katz (1997) find that high school graduation rates increased the most in states with the largest increases in unemployment. In addition, states with

[72] In a history of the working class in Depression era Chicago, Cohen (1991) recounts several cases of children serving as primary earners while parents were unemployed. Such stories are common in oral histories of the Depression generation (Terkel 2013; Watkins 2000).

large manufacturing bases also saw large increases in graduation rates during the Depression, likely because the New Deal era National Industry Recovery Act codes banned the employment of youths in manufacturing. Shanahan et al. (1997), analyzing different cohorts from the Stanford-Terman Study of Gifted Children, also find that the Depression increased educational attainment.

Did the Great Depression *cause* the increase in education suggested in Figure 10? Without comprehensive data on educational attainment before the Depression, that is difficult to prove, but the variation of the Depression effect within my sample is suggestive. In Figure 11, I plot coefficients from a pair of regressions of educational outcomes in my sample of sons on 1920 city-level Depression severity interacted with age in 1929; in (a) the outcome is completed years of education in 1940, and in (b) it is an indicator for high school graduation. It seems unlikely that sons who had already graduated or dropped out of school when the Depression hit in 1929 would be responsive to its severity. On the other hand, for sons at the ages on the margin of working or staying in school, the Depression could have a large effect: if jobs were more scarce in cities with more severe downturns, then those sons would have a lower opportunity cost of education and be more likely to remain in school. The patterns in Figure 11 support these possibilities: for sons between 15 and 19 in 1929, a one standard deviation increase in Depression severity increases total years of education by 0.25 to 0.5 years and increases the probability of graduating from high school by 5 to 10 percentage points. However, sons at other ages, either younger or older, are no more likely to increase their educational attainment in response to Depression severity.

If the Depression-induced increase in education is differential according to father's income, that could explain my main finding. To test this, I regress measures of the sons' educational outcomes on the father's log income in 1920, an interaction of income and Depression severity, as well as age quartics and city fixed effects. In Table 8, I show that while sons of richer fathers have more years of education and are more likely to graduate from high school and college, there is no effect of Depression severity on these links. The relationship between father's income and son's education is stable regardless of Great Depression severity. However, education could still play an important role in my findings through its protective effects against downturns. In Figure 9, I showed the sons from more advantaged families moving to cities with less severe Depression downturns. These sons had more education (Table 8). Even if education differentials do not change with Depression severity, education could be one mechanism to explain differences in migration quality. More education might allow sons to find a better city match by opening up connections and information about different labor markets.

5.3 Returns to Education

The model of intergenerational mobility presented in section 3 makes clear the role of the return to skill in determining mobility rates. As p, the return to human capital parameter, increases, persistence rises and so mobility falls.[73] For the cities in my sample, the Great Depression does not appear to have increased the return to skills or human capital, suggesting that changes in the return to education do not explain the Great Depression effects on mobility. However, this analysis is necessarily limited by the fact that I cannot observe the returns to education *before* the Depression across cities.

To test whether the Great Depression increased the return to skills in the US, I turn to the 1940 100% census. I observe annual earnings in 1939, completed years of education, age, and place of residence in 1940. Separately for each of the 3,071 counties in the country, I run a simple Mincerian returns to education regression, regressing log earnings on years of education and age dummies for all full-time employed white men between the ages of 16 and 65 in the county. The coefficient on education in each regression is the observed return to education; I map the returns in Figure A.3. Obviously, these estimated returns to education are descriptive and not causal. But they suggest which counties had relatively higher returns for skill and education.[74]

Nationwide, it does appear that Depression downturn severity, measured as usual by the decline in retail sales per capita from 1929 to 1933, have higher returns to education in 1940. The top panel of Figure 12 shows the correlation in a binned scatter plot. Each point represents one percentile of the Depression severity measure, comprising approximately 30 counties. However, this includes many small, rural counties. When I narrow my focus to the cities sampled by the BLS, the

[73] This prediction is borne out empirically in recent data as well. Comparing intergenerational mobility across counties today, Corak (2013) shows a strong positive correlation between the college premium and mobility. Aaronson and Mazumder (2008) document a correlation between the college premium and intergenerational mobility from 1940 to 2000 in the US. The recent increase or convexification in the return to education has likely driven part of the increase in inequality as well (Autor et al. 2008).

[74] The returns to education correlate negatively with county population: an increase in county population by 1% correlates with a 1% smaller estimated return. It is also apparent from the map that the human capital returns were higher in the plains states—the Dakotas, Nebraska, and Kansas—as well as the in mid-South. The returns to education were extremely low in New England and New York, as well as in the industrial Midwest—Ohio, Illinois, Michigan, and Indiana—and on the West Coast.

relationship disappears, as is evident in the bottom panel of Figure 12.[75] I find little evidence that the Great Depression was more (or less) severe in cities with high returns to education in 1940, suggesting that higher returns to schooling are unlikely to be the mechanism driving lower levels of mobility.[76] However, without good estimates of the return to education in these cities before the Depression, this conclusion is more speculative.

5.4 Inequality

Intergenerational mobility and inequality are linked, both conceptually and empirically. Immobile societies are often highly unequal. I have shown here that the Depression hardened the link between parents and children's economic positions, but did it also change the relative economic distribution? I find that earnings inequality did increase in the cities hit hardest by the Depression, but that this increase in inequality is not nearly large enough to explain my mobility findings.

Data on inequality at the county or city level in the years before and after the Depression are rare. I use data from the only such contemporaneous survey I know of: David Wickens' Financial Survey of Urban Housing, which collected data on the income distribution in 1929 and 1933 for 33 American cities.[77] I recollect these data and calculate measures of inequality before (1929) and during (1933) the Great Depression. Cities range in size from Cleveland (1.2 million) to Boise (22,000). Only 21 of the 33 cities are in both the Wickens sample and BLS sample in 1918-1919.

Results from this small sample of cities suggest that the Depression increased inequality. In Figure 13, I plot the changes in Gini coefficients and in the difference between 90th and 10th percentile earnings against Depression severity at the city level: the worse the downturn, the larger the increase in inequality. The results hold for other inequality measures, including the 90-50 and 50-10 differences. In all cases, Depression severity is uncorrelated with inequality in 1929 and positively correlated in 1933. If the Depression increased inequality and decreased mobility across cities, that follows the Great Gatsby curve identified across countries (Krueger 2012; Corak 2013) as well as contemporary patterns in mobility and inequality across US counties (Chetty et al. 2014a).[78]

[75] There is nothing particular about the BLS-sampled cities. The correlation between the estimated return to education and Depression severity is flat across all counties with populations over 25,000, for example.

[76] This finding tracks the time series evidence for inequality. Goldin and Margo (1992) document lower inequality in the decades following the Great Depression, a period of declining returns to education (Goldin and Katz 2008).

[77] This survey has been used in previous work on historical inequality by Grayson (2012) and Mendershausen (1946).

[78] The proposed causal effect of inequality on mobility within the US may not exist. Bloome (2015) exploits state and cohort variation within the US and finds "very little support for the hypothesis that inequality shapes mobility."

The magnitude of the effect of the Depression on inequality, however, is too small to explain my mobility results. Chetty et al. (2014a) regress intergenerational elasticities of income on Gini coefficients across countries, estimating a slope of 0.72. I estimate a 0.10 increase in IGE and a 0.02 increase in the Gini coefficient for a one standard deviation worse Depression shock. With the strong assumption that the Gatsby Curve is causal—and not inflated due to spurious correlation driven by public policy or social norms—and that the contemporary Gatsby Curve is relevant to the inequality-mobility relationship in the early 20th century, widening inequality only explains about 14% of the change of mobility.[79]

6 The New Deal and Mobility

Franklin D. Roosevelt took office in March 1933 amidst the depths of the Depression. During and after his famed first hundred days, Roosevelt attacked the Depression from a number of angles. A bank holiday staved off a string of bank runs; leaving the interwar Gold Standard freed monetary policy from fixed-exchange shackles; and the Federal government began a new era of involvement in the economy, spending and loaning huge amounts of money through various programs, grants, and agencies. Did these New Deal programs—aimed at alleviating the economic misery of the Depression—also have an effect on intergenerational mobility?

I measure per capita New Deal spending at the county level with data drawn from Fishback et al. (2003). In addition to observing overall expenditures on grants and loans, I follow Fishback et al. (2003) in categorizing spending into three broad categories.[80] Relief spending comprised the first category, including Federal Emergency Relief Administration grants, Civil Works Administration grants, Works Progress Administration grants, and Public Assistance Grants via the Social Security Act. Spending was directed to areas most in need of relief, targeting cities and counties with the most severe downturns (Fishback et al. 2003). In the cities in my sample, spending in the relief category accounts for roughly 43% of New Deal outlays in each city. In the second cate-

[79] The IGE in a city suffering a Depression downturn one standard deviation worse than the mean would be expected to increase by 0.10; the increase in the Gini coefficient would be 0.02, which when multiplied by the slope of the Gatsby Curve yields an expected IGE increase of $0.02 \times 0.72 = 0.0144$, or roughly 14% of the 0.10 direct effect of the Depression on the IGE.

[80] A fourth category of New Deal grants and loans was spending directed to farmers and rural areas. However, while these New Deal outlays are important to the economic history of the Great Depression and New Deal, they are not relevant for my urban sample of fathers and sons. Only a handful of sons live in counties which received more than 10% of New Deal outlays in the form of farm assistance, and this spending was likely directed at the non-urban core of the counties. These programs include the AAA, FCA, FSA grants and loans, and the REA.

gory is public works spending via Public Works Administration grants and loans, as well as grants from the Public Roads Administration and the Public Buildings Administration. The infrastructure programs endeavored to build useful projects, rather than immediately boosting employment during the Depression. In most of my sample cities, public works spending accounts for less than 20% of total New Deal grants and loans. Finally, a number of New Deal programs targeted the housing market with loans or the insurance of mortgages. This spending included loans from the Reconstruction Finance Corporation, from the Home Owners Loan Corporation, and grants and loans from the US Housing Administration.

New Deal spending did not alter the relationship between the downturn and intergenerational mobility, as I show in Table A.5. In Panel A, I supplement my baseline IGE specification with interactions of normalized per capita New Deal spending and father's log income in 1920 in columns (2) and (3). Though the point estimates are negative—suggesting that New Deal spending may have in part counteracted the deleterious effects of the Depression on economic mobility—the results are imprecise. When I decompose New Deal spending into three constituent parts—relief spending, public works, and housing market assistance—I find weak evidence that one aspect of the New Deal, housing market support, may have reduced the negative mobility effects of the Depression. However, these results are only marginally significant and are more speculative. Further, when I examine the effects of the New Deal on rank-rank mobility in Panel B of Table A.5, I find no effects of the New Deal on mobility.

New Deal spending had long term positive effects in the cities and counties targeted: Fishback et al. (2005) show that a dollar of spending increased retail sales in 1939 by 44 cents. But the programs were slow to begin and did not ameliorate economic misery immediately. By the time the economic recovery began—fueled in part by New Deal programs—sons hit hardest by the downturn were likely already locked in to their fathers' relative economic position.

7 Conclusion

In this paper, I have explored the effects of the Great Depression on intergenerational mobility. Comparing across sons, I find that the Depression was bad for mobility. Despite—or perhaps because of—massive economic disruption, the economic slate was not wiped clean. Instead, my results suggest that sons growing up in cities with more severe downturns during the Great Depression had less economic mobility than sons from cities with milder shocks. These effects are not driven by pre-existing place-specific differences in mobility, as the mobility rates for the generations before and long after the Depression are unaffected by local downturn severity.

Differential directed migration is the primary mechanism to explain my results. The Depression pushed all sons to look elsewhere for better economic opportunities: geographic mobility increased with Depression downturn severity across my entire sample. However, the sons of richer fathers were able to make better choices, migrating to cities which had suffered far less severe Depression downturns. Because the city-specific Depression effects were long lasting—cities with more severe downturns still had lower wages and higher unemployment in 1940—these directed movements were hugely beneficial to the sons making them.

I have shown the mobility effects of variation in Great Depression severity, but my empirical specification cannot directly address a different question: what would intergenerational mobility have been had the Great Depression not struck? Speculatively, my results may shed some light on this more dramatic counterfactual. My local findings may be an underestimate of the aggregate effects of downturns on mobility. The sons of richer fathers in my sample were able to migrate to "better" locations than their poorer peers, but their choices were constrained by the Depression: no American cities had robust wage growth or low levels of unemployment during the Depression-era. If such places existed, the effects of directed migration on son's outcomes may have been even more dramatic, which would make the mobility effect of the Depression that much stronger. However, the historical relationship between migration and macroeconomic growth complicates this interpretation. Overall migration rates during the Depression era were lower, as is usually true during periods of slow growth in American history (Rosenbloom 2002; Lebergott 1964). The lower geographic mobility rates in aggregate could have increased intergenerational economic mobility by reducing the share of sons with richer fathers benefiting from the gains of directed migration, pushing against the local effects I find. In this case, in contrast to my finding that local Depression severity reduced mobility, in aggregate it may have increased mobility.[81]

To create the longitudinal sample of fathers and sons necessary for my analysis, I developed a

[81] It may also be possible that migration is more directed during downturns than during periods of growth, a claim that my census linking procedure could enable me to test by constructing longitudinal samples of fathers and sons across many different eras. A further complication is the unknown distributional effect of aggregate economic conditions on migration across richer and poorer sons.

machine learning approach to census linking. With my method, I am able to quickly, accurately, and efficiently match large samples of people across censuses or into the census from other sources, like the BLS cost of living survey. Combining this method with many recently digitized historical microdata sets, it is possible to extend our understanding of the determinants of intergenerational economic mobility. The Depression reduced mobility, but what of the other major economic, social, and political events of the early twentieth century? Did the first wave of the Great Migration break intergenerational bonds among African American children born in this period—both for those who migrated and for those who remained in the South? What effect did the dramatic expansion of public schooling during the High School movement have on economic mobility? How did mobility change as the American population shifted from majority rural to majority urban?

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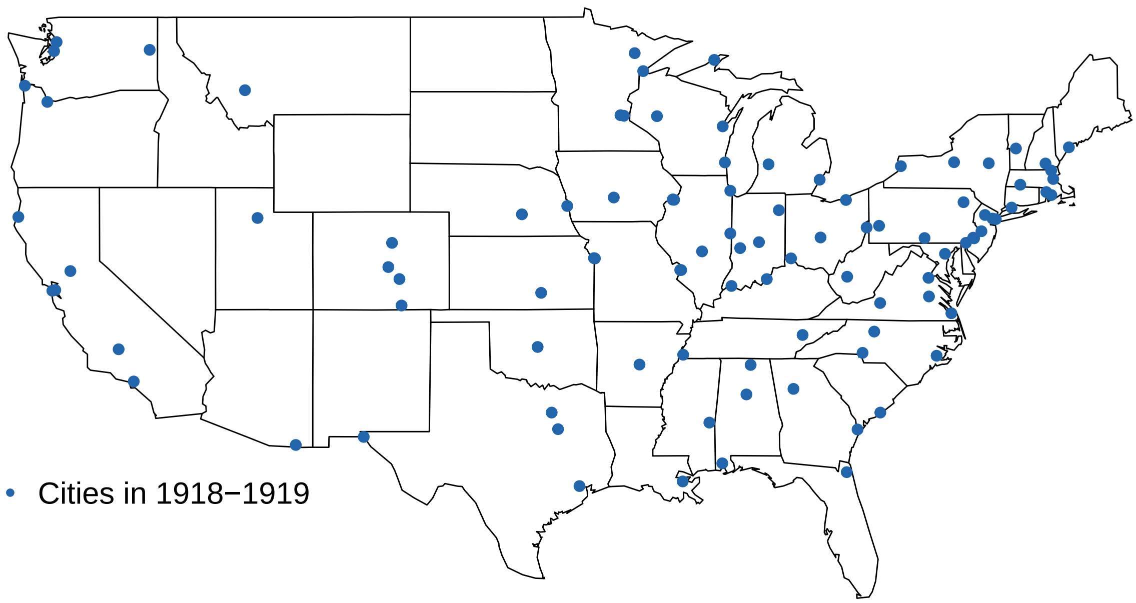
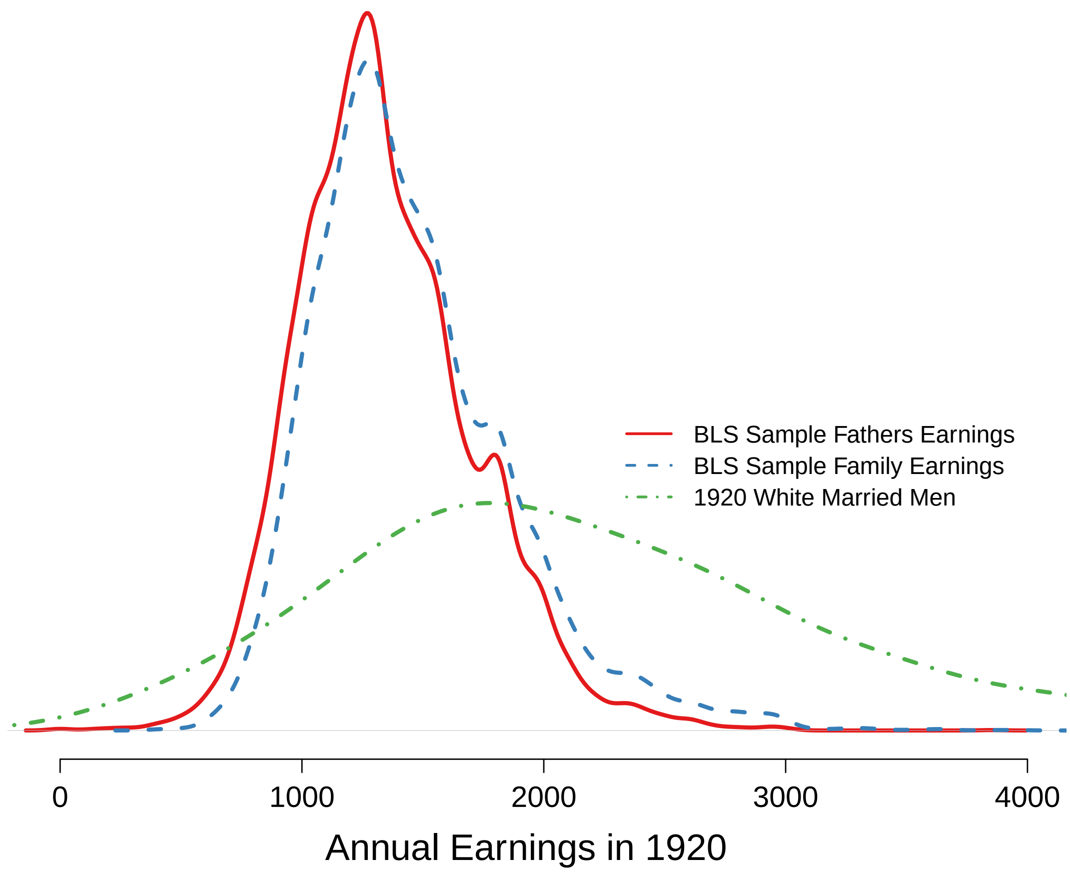
Figure 1: 99 Industrial Cities included in the BLS Sample, 1918-1919



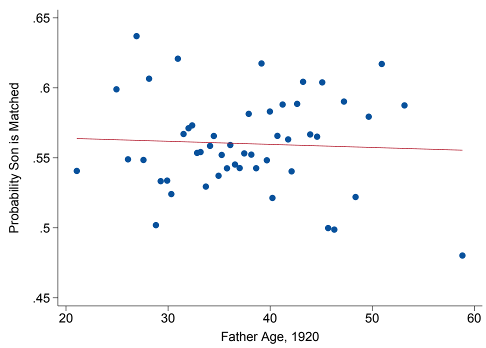
The cities range in population from New York City (5.6 million in 1920) to Calumet, MI (2,390 in 1920).

Figure 2: Distribution of Earnings in the BLS Sample

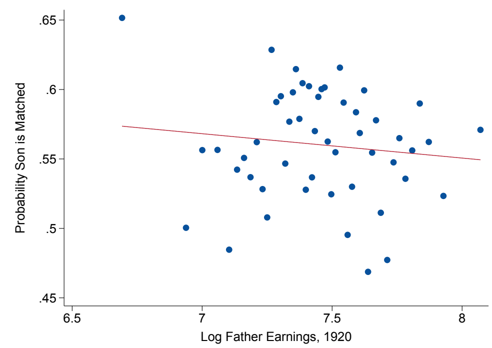


The BLS survey designers intended to sample only families earning less than $2000 per year, but excluding families on relief or charity; 80.8% of the sample earned between $1000 and $2000 annually, 91.8% between $800 and $2000. All earnings are measured in 1920 dollars. The complete earnings distribution for 1920 is calculated using the 1940 earnings distribution, weighted by occupation and industry shares from the 1920 census.

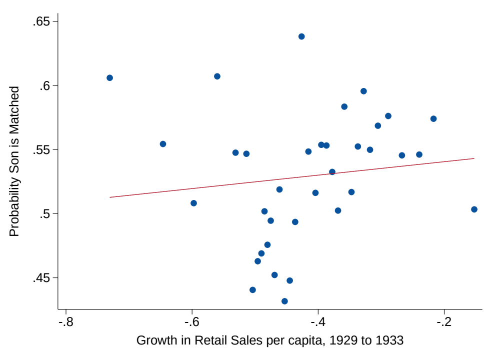
Figure 3: Match rates for BLS sample sons into the 1940 census do not correlate with important covariates



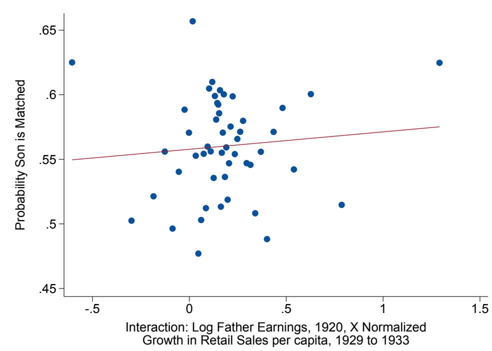
(a) Father's Age in 1918-1919



(b) Log Father's Income in 1918-1919



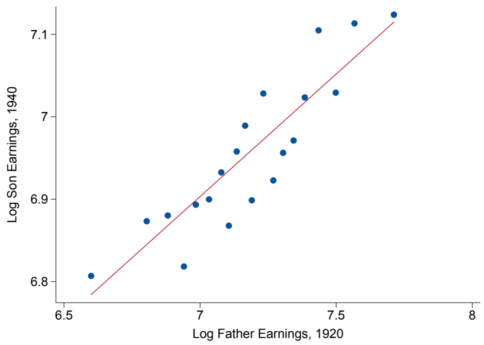
(c) Decline in retail sales



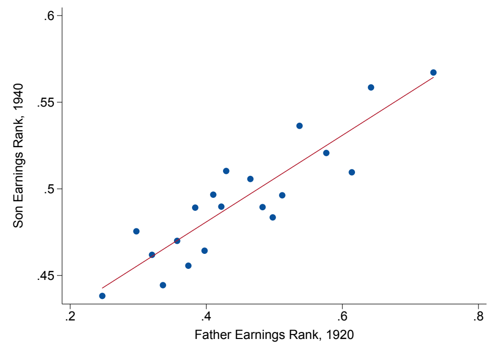
(d) Interaction

Match rates for sons do not correlate with father's age (a) or income (b). In addition, sons experiencing more severe Great Depression downturns in their city of origin are no more or less likely to be found in the 1940 census (c). Finally, and most importantly for my study, the match rates do not correlate with the interaction of Great Depression severity and father's income (d). All binned scatter plots control for first and last name commonness, length, and enumerator leave-one-out match rates. In the plot in (d), direct controls for father's income and Depression severity are included. The p-values for the main (x-axis) variable coefficients are 0.723, 0.343, 0.470, and 0.450 in separate regressions.

Figure 4: Mobility measures are approximately linear in my samples

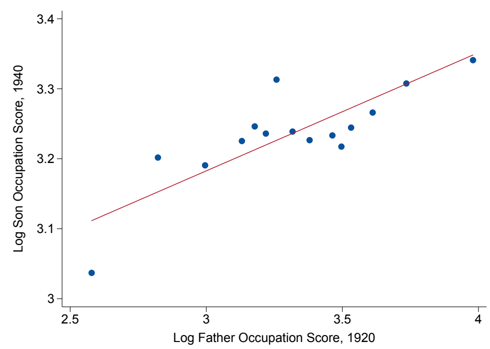


(a) Intergenerational Elasticity of Earnings, BLS Sample



(b) Rank-Rank Parameter, BLS Sample

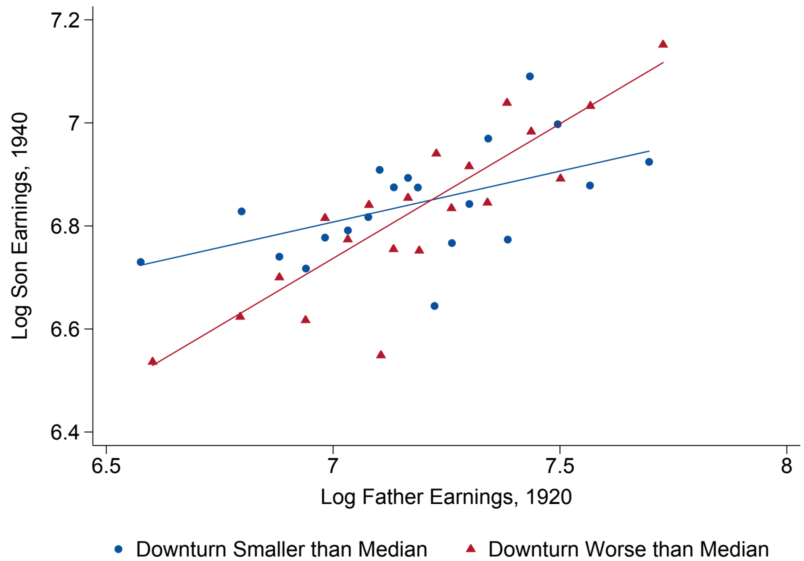
(c) Occupation Score Elasticity, BLS Sample



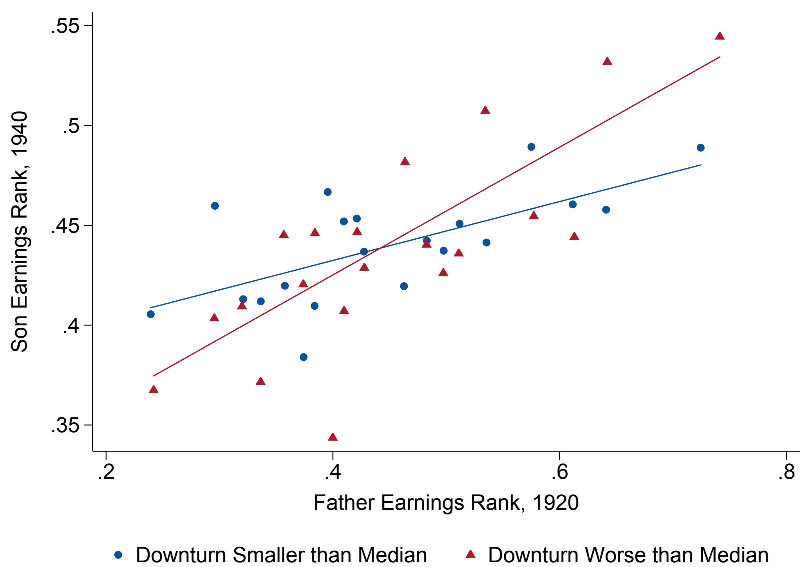
(d) Occupation Score Elasticity, IPUMS Sample

Binned scatter plots of mobility in my samples. In all cases, these relationships are approximately linear. I measure mobility in three ways in the BLS sample: (a) intergenerational elasticity (log of son's earnings regressed on log of father's earnings), (b) rank-rank (position in the national earnings distribution in 1940 of the son regressed on the father's earnings position in the national 1920 distribution), and (c) occupation score elasticity (log of the son's occupation score on the log of the father's occupation score). I also measure mobility in the larger IPUMS sample that links the 1920 census to the 1940 census in (d), relying on occupation score elasticities, as earnings and income were not collected in the 1920 census. The binned scatter plot pools fathers into 20 bins, each representing five percent of the data, ranked by the father's 1920 outcome. Points are plotted at the mean for father's and son's outcomes within each bin.

Figure 5: Intergenerational mobility of earnings is lower in cities with more severe Great Depression downturns



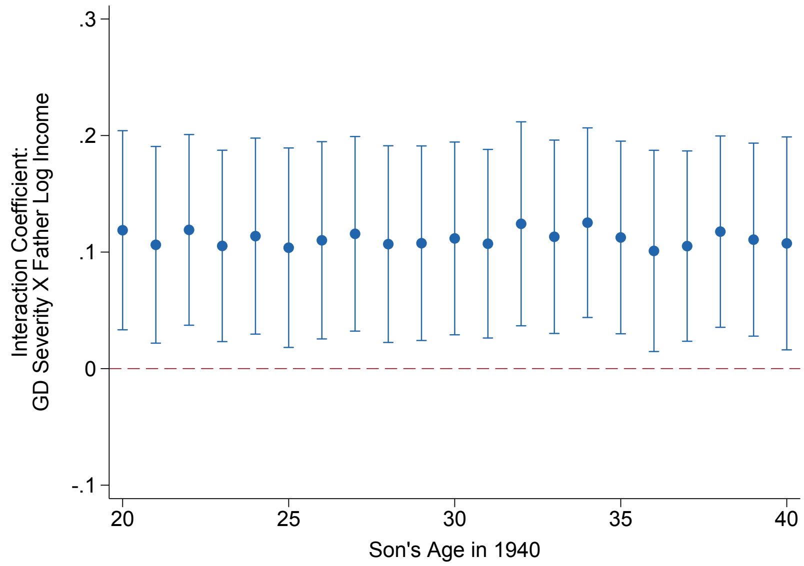
(a) Intergenerational Elasticity of Earnings



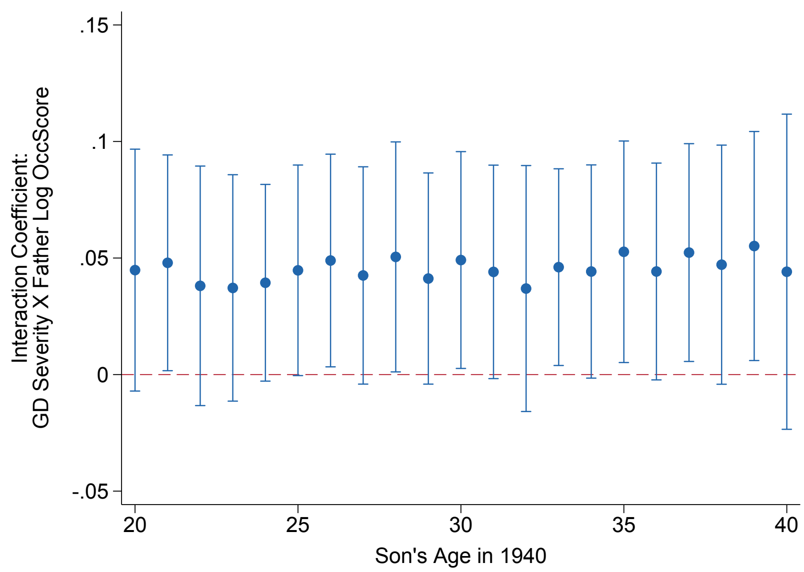
(b) Rank-Rank Mobility

The steeper slope between the father's log earnings and son's log earnings in cities with downturns more severe than the median implies less mobility. Cities are split into groups with above or below median Great Depression severity, as measured by the decline in per capita retail sales between 1929 and 1933. The binned scatter plot pools fathers into 20 bins, each representing five percent of the data. Points are plotted at the mean for father's and son's log earnings within each bin.

Figure 6: The effect of Great Depression severity on intergenerational mobility is stable across sons by age



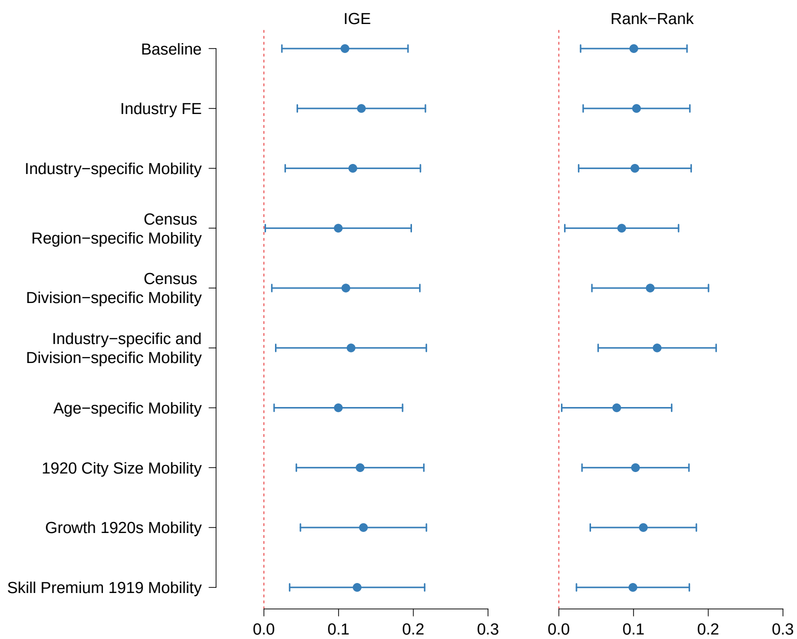
(a) BLS Sample



(b) IPUMS 1920-1940 Sample

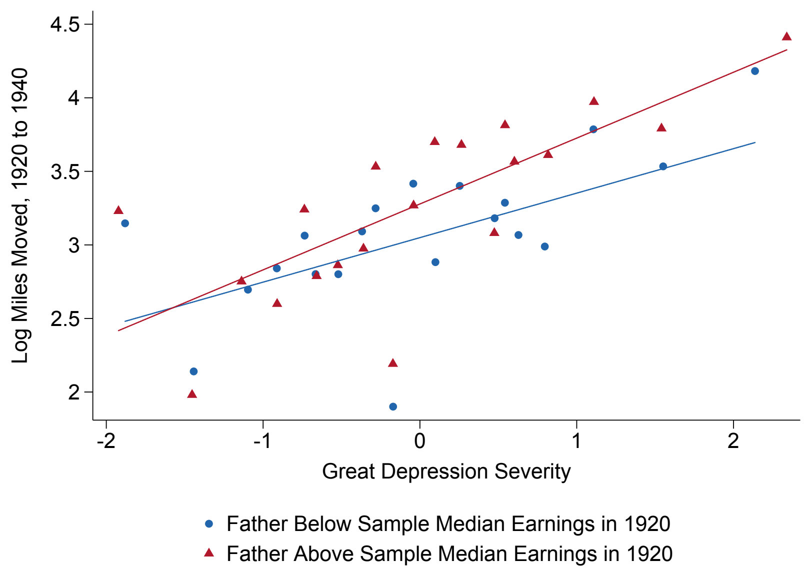
In the BLS sample, the effect varies between 0.11 and 0.14, roughly half the estimated intergenerational mobility elasticity in a city with an average Great Depression downturn. This implies that sons in a city with a local Depression one standard deviation more severe than average had mobility rates of 0.37 to 0.41. In the IPUMS sample, I calculate mobility rates using occupation scores and find similarly stable effects between 0.04 and 0.06, roughly one-third the overall estimated occupation score elasticity. The IPUMS results are slightly less precise, likely reflecting the noise in measuring occupations and assigning occupation scores. Great Depression severity is measured with normalized retail sales growth per capita from 1929 to 1933, where higher severity implies less growth.

Figure 7: Estimated Depression effect on mobility, allowing mobility to vary with other city and individual covariates

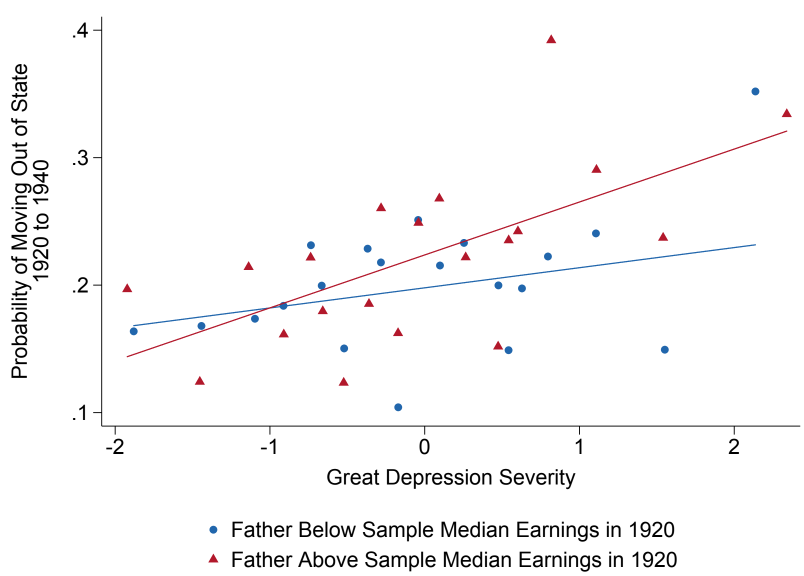


The reduction in mobility from a more severe Depression downturn is robust to a variety of control specifications. I run my main specification—the son's earnings in 1940 on father's earnings, Depression severity, and the interaction—and include additional terms that allow mobility to vary along other dimensions. Above, I plot the primary coefficient of interest, the interaction of Great Depression severity with father's earnings (either logged or his rank in the overall distribution). In each row, I include a different robustness control, described along the y-axis. The samples and controls match columns (2) and (6) of Table 4, including city fixed effects.

Figure 8: Sons in cities with more severe Depression downturns were more likely to be move out of state and to move farther



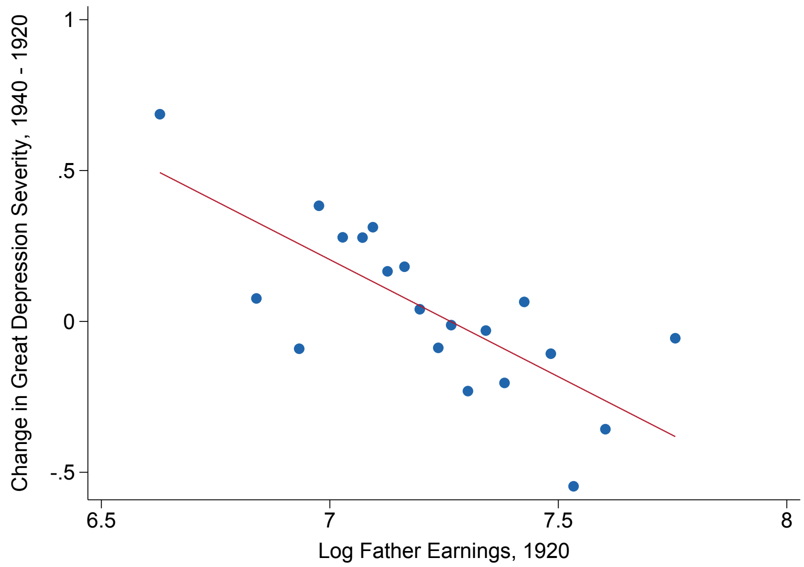
(a) Distance Moved



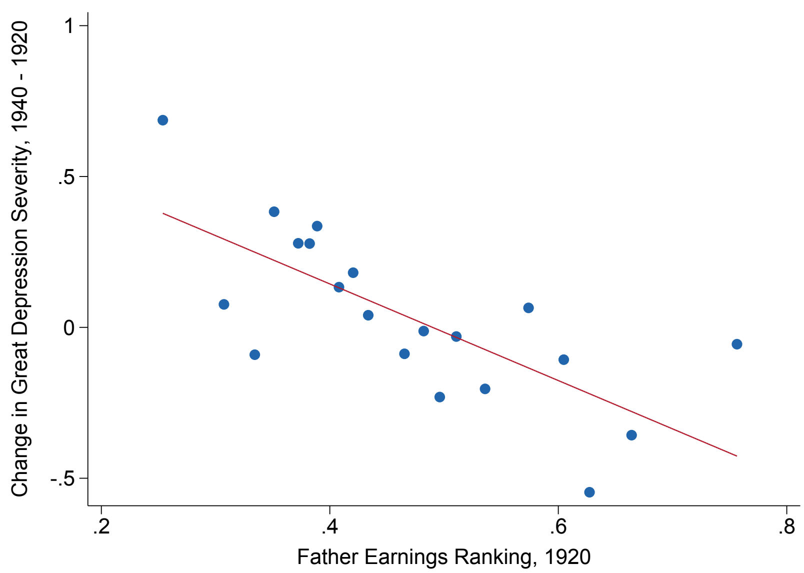
(b) Moving Out of State

Relative Great Depression severity spurred out-migration of the sons in my sample between 1920 and 1940. Each figure presents a binned scatter plot, grouping the sons in the sample in Depression severity percentiles and plotting (a) the average log miles moved or (b) the share moving between states within each group. Sons are split in each plot by the earnings of their father in 1920; median earnings for fathers in the sample was $1300 in 1920 dollars. Sons of richer fathers appear to be slightly more likely to move or to move farther in response to Depression severity, but these differences are not statistically significant.

Figure 9: Sons of richer fathers moved to cities with less severe Depression downturns



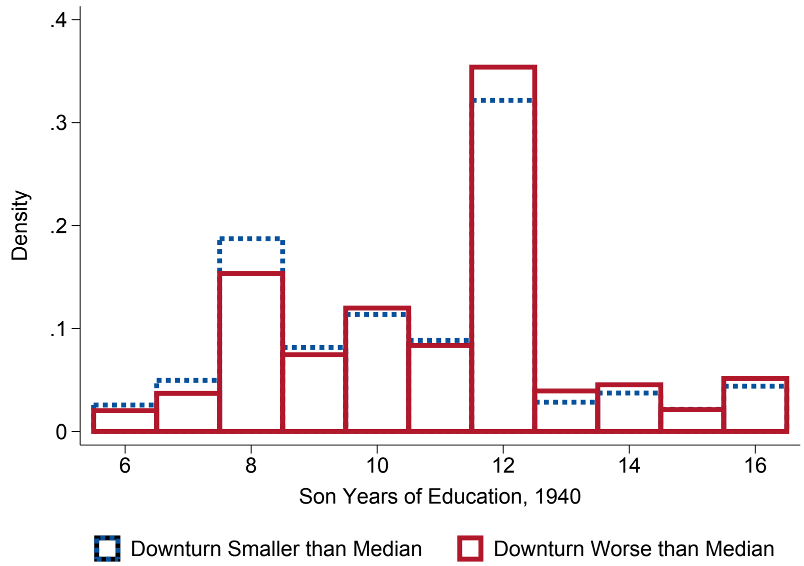
(a) Log Father Earnings



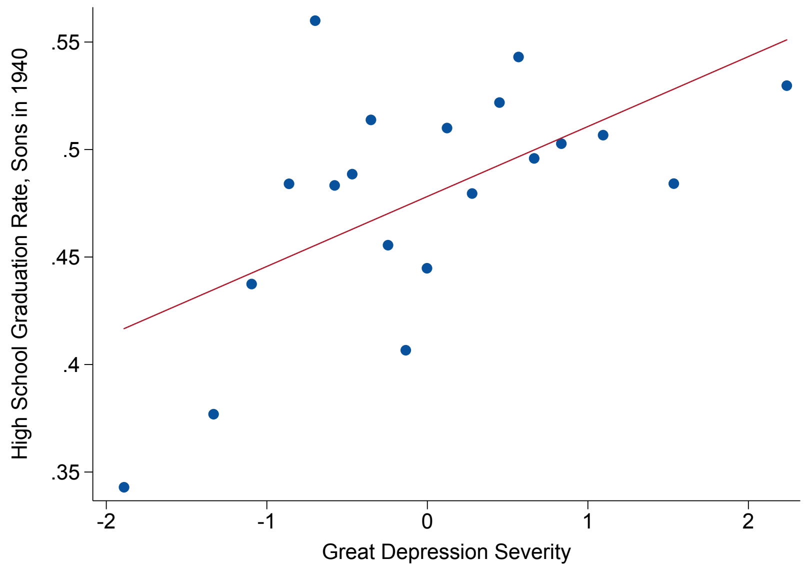
(b) Father Earnings Rank

The sons of richer fathers were more likely to make better migration choices, measured by the difference in Depression downturn severity in sending and receiving counties. Migration quality is measured by the difference in growth of retail sales per capita between 1929 and 1933 in the county of residence in 1940 and the county of residence in 1920. A negative value implies a move from a city with a higher level of Great Depression severity to a city with a lower level of Depression severity. Only sons moving out of state are included in this figure. Each figure presents a binned scatter plot, grouping the sons in the sample into fathers' earnings or ranking percentiles.

Figure 10: Sons growing up in cities with more severe Depression downturns had more education in 1940



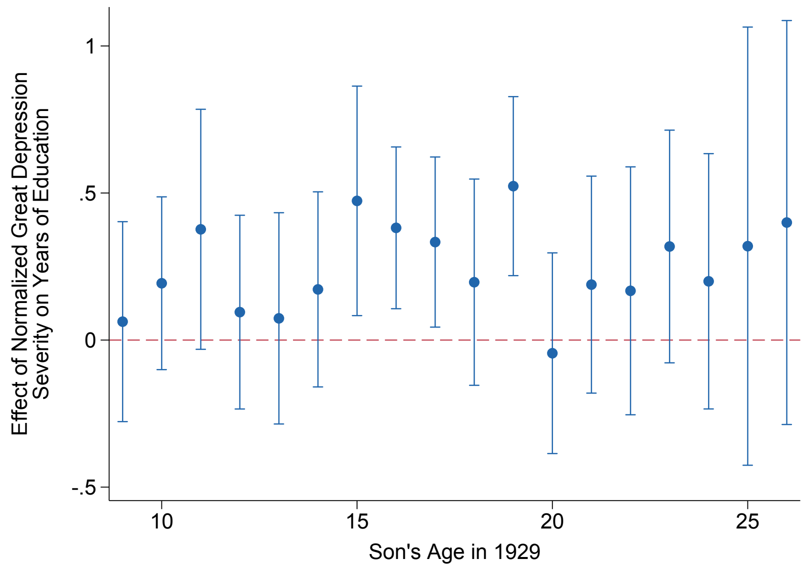
(a) Son's Years of Education in 1940



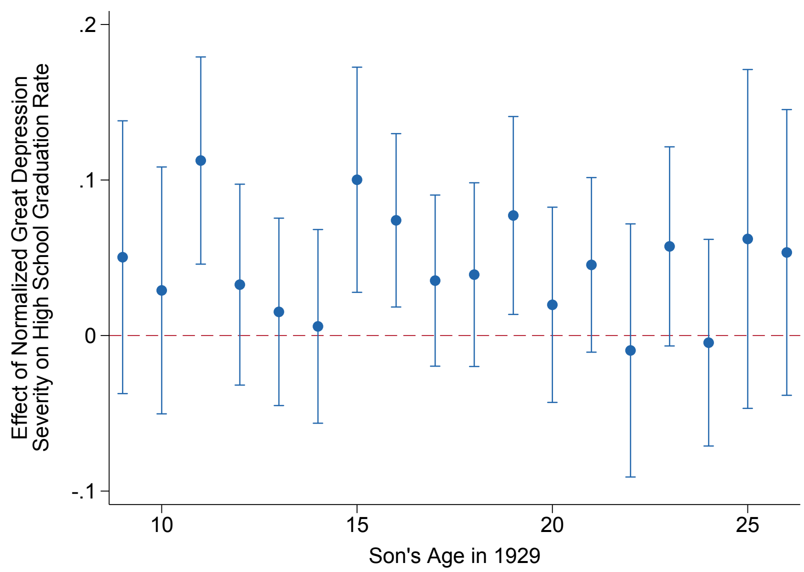
(b) Son's High School Graduation Rate in 1940

The Great Depression shifted sons from grade school graduates to high school graduates. In (a), I present a histogram of the completed years of schooling by Depression severity. Cities with downturns smaller than median are displayed in dashed blue bars; cities with downturns worse than median are in solid red bars. I omit the 3% of sons with 5 or fewer years of education from the histogram. In (b) the binned scatter plot demonstrates that high school graduation rates among sons in my matched sample increased in normalized Great Depression severity: the more severe the downturn, the higher the graduation rate.

Figure 11: Great Depression Severity Increased Education among Sons aged 15 to 19 in 1929



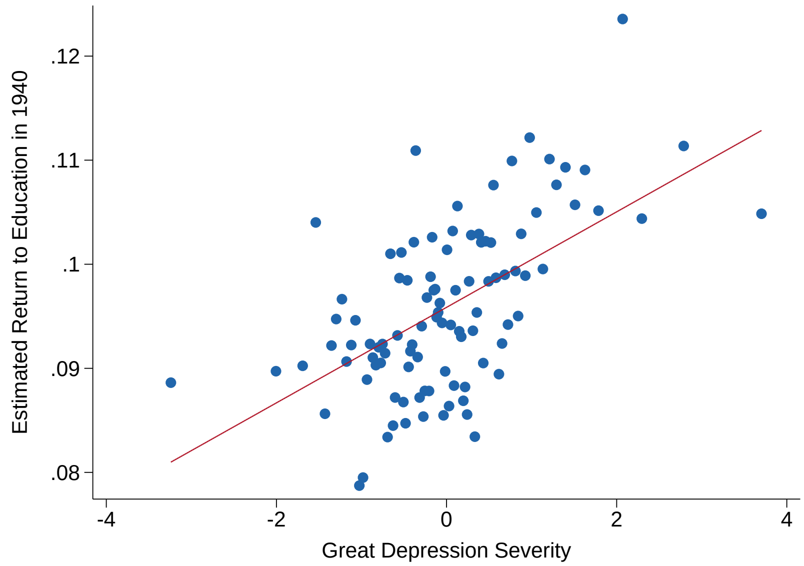
(a) Son's Years of Education in 1940



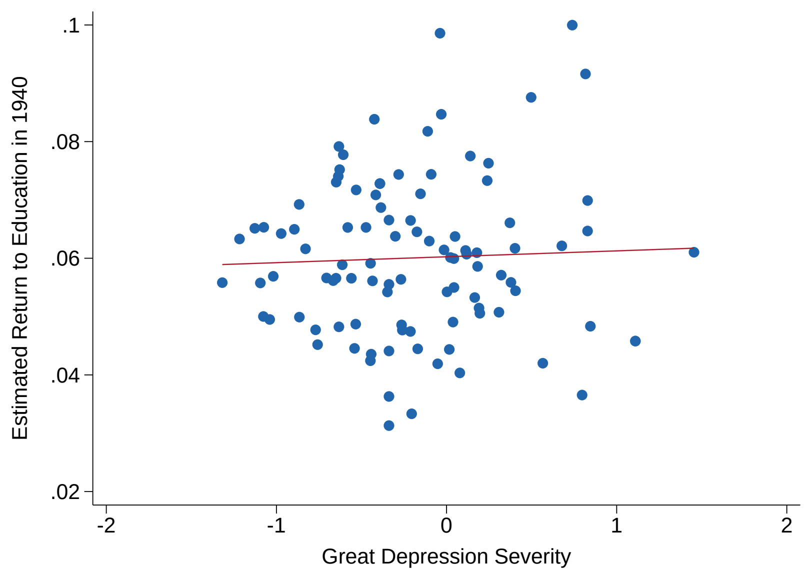
(b) Son's High School Graduation Rate in 1940

Both plots present coefficients from single regressions of (a) completed years of education in 1940 or (b) an indicator for high school graduation on the interaction of Depression severity with son's age in 1929. Standard errors are clustered at the city level. Controls include son's age fixed effects and a quartic in father's age.

Figure 12: Great Depression severity did not correlate with higher returns to education in 1940



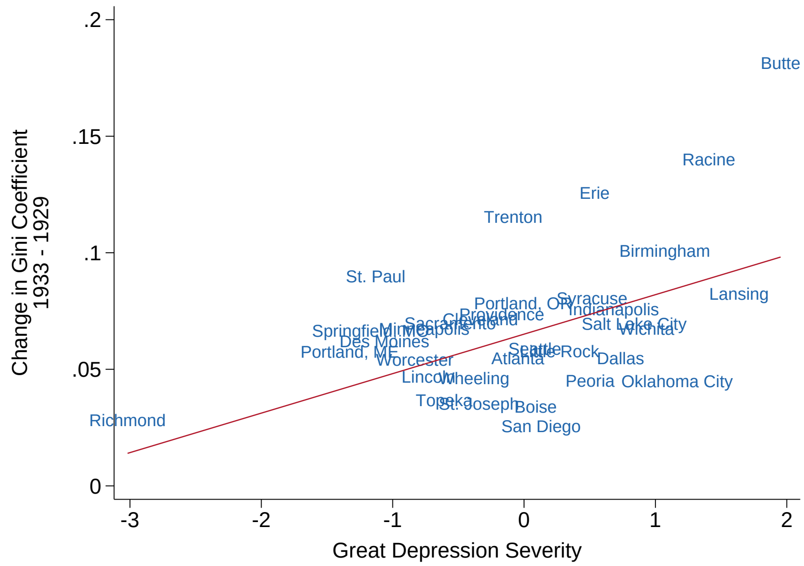
(a) All Counties



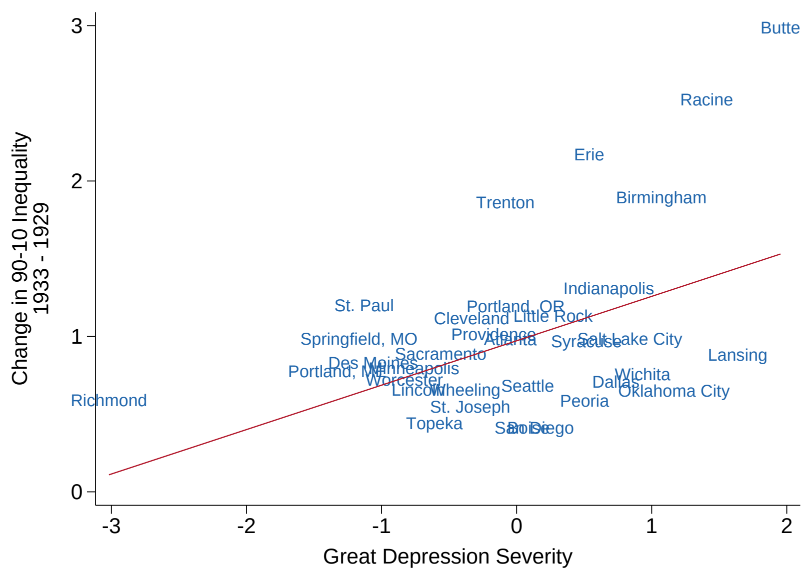
(b) BLS Sample Cities

The top panel shows a binned scatter plot, comparing Great Depression severity from 1929 to 1933 and the estimated return to education in 1940 across all counties. Counties hit by more severe downturns had substantially higher returns to an additional year of schooling in 1940. However, this relationship is driven by smaller, rural counties. The bottom panel limits the sample to the 99 industrial cities included in the BLS survey and shows no correlation between Depression severity and the return to education. Education returns are measured with county level regressions in the 1940 complete count census, regressing log annual earnings on years of education and complete age dummies for full time employed white men between 16 and 65. Great Depression severity is normalized retail sales growth per capita from 1929 to 1933, where higher severity implies less growth.

Figure 13: The Great Depression increased earnings inequality in cities between 1929 and 1933.



(a) Gini Coefficient



(b) 90th - 10th Percentile Earnings

Across the 33 cities included in the *Financial Survey of Urban Housing*, which collected data on the income distribution in 1929 and 1933, the change in inequality was correlated with the severity of the Great Depression. The relationship between Depression severity and the Gini coefficient and the 90-10 differential are plotted above, but both the changes in the 90-50 and in the 50-10 differential also correlate with Depression severity. There is no relationship between inequality in 1929 (by any measure) and Depression severity. Great Depression severity is normalized retail sales growth per capita from 1929 to 1933, where higher severity implies less growth.

Table 1: The BLS Families are Younger but the Same Size as Families in the 1920 Census

	BLS Sample	IPUMS 1920 Sample	
		All Cities	BLS Sample Cities
Father's Age	36.94	42.09	41.89
	(8.52)	(11.78)	(11.54)
Mother's Age	33.32	38.04	37.92
	(7.86)	(11.23)	(11.04)
Number of Children	2.53	2.52	2.53
	(1.50)	(1.64)	(1.64)
Number of Sons	1.25	1.29	1.29
	(1.08)	(1.17)	(1.16)
Age of Oldest Child	9.27	13.42	13.29
	(6.06)	(9.25)	(9.18)
Age of Youngest Child	4.68	8.44	8.34
	(4.64)	(8.63)	(8.49)
Homeowner	0.27	0.40	0.34
	(0.44)	(0.49)	(0.48)
Observations	11946	67864	36416

The families sampled by the BLS are younger than families in the IPUMS 1920 sample, but the sizes of families are similar. They are less likely to own their own home, though this difference is smaller when comparing across the same set of cities. I restrict the census sample to white families in cities with married heads, both spouses present, and at least one child, replicating the BLS demographic sampling frame. The BLS sample is the complete BLS sample less 871 non-white families and 54 records missing from or illegible in the National Archives. The table presents means with standard deviations in parentheses.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample

Table 2: Probability of Matching a Son from the BLS Survey to the 1940 Federal Census

	(1)	(2)	(3)	(4)	(5)	(6)
Name commonness, first name	-0.014**			-0.005		-0.007
	(0.006)			(0.006)		(0.007)
Name commonness, last name	-0.096**			-0.089**		-0.073*
	(0.038)			(0.038)		(0.041)
String length, first name		0.013***		0.012***		0.010***
		(0.003)		(0.003)		(0.004)
String length, last name		0.006*		0.005*		0.004
		(0.003)		(0.003)		(0.003)
Enumerator Match Rate Leave Out Mean			0.319***	0.317***		0.219***
			(0.047)	(0.048)		(0.054)
Brothers Match Rate Leave Out Mean					0.169***	0.155***
					(0.018)	(0.019)
Match Rate	0.560	0.560	0.560	0.560	0.558	0.558
Observations	11195	11195	11195	11195	7912	7912
Clusters	6371	6371	6371	6371	3088	3088
Adjusted R^2	0.001	0.002	0.005	0.007	0.021	0.024

Linear probability model with an indicator variable for a successful match as the outcome. Standard errors are clustered by family in the BLS data. Results are consistent using a probit or logit model as well. Name commonness is measured as the share of 100 men in the 1920 IPUMS sample with the same first or last name. Name length is the number of characters in the first or last name.

Source: BLS Cost of Living Survey 1918-1918

Table 3: Estimated Rates of Intergenerational Mobility, 1920-1940

	IGE		Rank-Rank		OccScore (BLS)		OccScore (IPUMS)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Father Earnings 1920	0.275^{***}	0.272^{***}						
	(0.042)	(0.045)						
Father Earnings Rank 1920			0.217^{***}	0.214^{***}				
			(0.036)	(0.038)				
Log Father Occupation Score 1920					0.202^{***}	0.215^{***}	0.154^{***}	0.144^{***}
					(0.041)	(0.042)	(0.011)	(0.011)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4730	4730	4952	4952	4819	4819	13216	13216
Clusters	3572	3572	3708	3708	3437	3437	10509	10509
Adjusted R^2	0.180	0.187	0.150	0.152	0.048	0.056	0.072	0.076

All standard errors are clustered at the family level to account for multiple observations of the same father across brothers. Columns (1) and (2) give the IGE for the BLS sample where the dependent variable is log son earnings in 1940. Columns (3) and (4) present rank-rank mobility estimates following Dahl and DeLeire (2008) and Chetty et al. (2014a), where the dependent variable is son's earnings rank in 1940. Columns (5) and (6) present occupation score mobility in the BLS sample, where the dependent variable is the log son occupation score in 1940. Columns (7) and (8) present occupation score mobility in the IPUMS sample, where the dependent variable is the log son occupation score in 1940. All regressions include controls for quartics in father's age (in 1920) and son's age (in 1940). The odd columns add state fixed effects, based on the state where the father and son lived in 1920. The even columns substitute city fixed effects for state fixed effects, also based on 1920 residence. Sons with only capital or self-employment earnings, which were not recorded in the 1940 census, are excluded from both the IGE and rank-rank analysis. In addition, sons with no labor earnings in 1940 are excluded from the IGE analysis, but are included in the rank-rank analysis.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample; 1940 Complete Count Census.

Table 4: Great Depression Severity Decreases Intergenerational Mobility: IGE and Rank-Rank Measures

	IGE				Rank-Rank			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Father Earnings 1920	0.285^{***}	0.280^{***}	0.190^{***}	0.181^{***}				
	(0.041)	(0.043)	(0.054)	(0.056)				
Log Father Earnings 1920 X GD Normalized Severity	0.087^{**}	0.108^{**}						
	(0.039)	(0.042)						
Log Father Earnings 1920 X GD Above Median Severity			0.184^{**}	0.197^{**}				
			(0.081)	(0.082)				
Father Earnings Rank 1920					0.219^{***}	0.213^{***}	0.131^{***}	0.124^{**}
					(0.031)	(0.033)	(0.047)	(0.051)
Father Earnings Rank 1920 X GD Normalized Severity					0.078^{**}	0.100^{***}		
					(0.033)	(0.036)		
Father Earnings Rank 1920 X GD Above Median Severity							0.167^{**}	0.175^{***}
							(0.065)	(0.066)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4730	4730	4730	4730	4952	4952	4952	4952
Clusters	99	99	99	99	99	99	99	99
Adjusted R^2	0.181	0.188	0.181	0.188	0.151	0.153	0.151	0.153

Estimates of intergenerational mobility based on a linked sample from the BLS survey of urban families in 1918-1919 to the 1940 Federal census. Each column is a regression of the son's outcome in 1940 on the father's corresponding outcome in 1918-1919, a measure of Great Depression severity in the city of residence in 1918-1919, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. In the odd columns, I include state fixed effects and direct controls for Great Depression severity (normalized in columns 1 and 5, above or below median in columns 3 and 7) but omit the point estimates from the table. In the even columns, these controls are absorbed by the city fixed effects. All fixed effects are based on the city of residence in 1918-1919. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. IGE is the intergenerational elasticity of income, and the dependent variable is log son earnings in 1940. Rank-rank mobility compares the son's position in the earnings distribution in 1940 to the father's position in 1918-1919 and the dependent variable is son earnings rank in 1940.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample; 1940 Complete Count Census.

Table 5: Great Depression Severity Decreases Intergenerational Mobility: Occupation Score Elasticity Measures

	Son OccScore (BLS)				Son OccScore (IPUMS)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Father Occupation Score 1920	0.182^{***}	0.198^{***}	0.119^{**}	0.131^{***}	0.161^{***}	0.157^{***}	0.128^{***}	0.125^{***}
	(0.041)	(0.043)	(0.052)	(0.047)	(0.013)	(0.014)	(0.017)	(0.017)
Log Father Occupation Score 1920 X GD Normalized Severity	0.071^{**}	0.067^{*}			0.050^{**}	0.045^{*}		
	(0.029)	(0.034)			(0.020)	(0.023)		
Log Father Occupation Score 1920 X GD Above Median Severity			0.124	0.128			0.067^{**}	0.055^{*}
			(0.080)	(0.079)			(0.028)	(0.028)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4819	4819	4819	4819	11555	11555	11555	11555
Clusters	99	99	99	99	92	92	92	92
Adjusted R^2	0.049	0.057	0.048	0.057	0.075	0.081	0.079	0.081

Estimates of intergenerational mobility in columns (1) through (4) are based on a linked sample from the BLS survey of urban families in 1918-1919 to the 1940 Federal census. Mobility estimates in columns (5) through (8) are based on a linked sample from the IPUMS 1% sample of the 1920 census to the full 1940 census. Each column is a regression of the son's log occupation score in 1940 on the father's log occupation score in 1918-1919 or 1920, a measure of Great Depression severity in the city of residence in 1918-1919 or 1920, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. In the odd columns, I include state fixed effects and direct controls for Great Depression severity (normalized in columns 1 and 5, above or below median in columns 3 and 7) but omit the point estimates from the table. In the even columns, these controls are absorbed by the city fixed effects. All fixed effects are based on the city of residence in 1918-1919 or 1920. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. Occupation scores are calculated as the national median income for men in the occupation.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample; 1940 Complete Count Census.

Table 6: Great Depression Severity Does Not Affect Intergenerational Mobility 1900 to 1920

	Log Son Occupation Score					
	(1)	(2)	(3)	(4)	(5)	(6)
Log Father Occupation Score 1900	0.1117***	0.1101***	0.1117***	0.1101***	0.1153***	0.1144***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Log Father Occupation Score 1900 X GD Normalized Severity			0.0004	-0.0002		
			(0.008)	(0.008)		
Log Father Occupation Score 1900 X GD Median Severity					-0.0077	-0.0085
					(0.017)	(0.017)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	42,393	42,393	42,393	42,393	42,393	42,393
Clusters	89	89	89	89	89	89
Adjusted R^2	0.028	0.030	0.028	0.029	0.029	0.029

Intergenerational mobility estimated on a matched sample of fathers from the 1900 census to sons in the 1920 census. The outcome variable throughout is the son's logged occupation score in 1920. The sample consists of sons in the IPUMS 6% sample of the 1900 census living in a city later surveyed by the BLS. Standard errors are clustered by city. In the odd columns, I include state fixed effects based on the state of residence in 1900 and a direct control for Depression severity. In the even columns, I add city fixed effects which absorb both the state fixed effect and the Depression severity measure.

Source: IPUMS 1900 6% Census Sample; 1920 Complete Count Census; Census of Retail Sales

Table 7: Great Depression Severity Does Not Affect Intergenerational Mobility Today

	Rank-Rank Slope		Rank-Rank 25th Percentile	
	(1)	(2)	(3)	(4)
Great Depression Severity	-0.005	0.013	0.761	0.491
	(0.011)	(0.011)	(0.540)	(0.567)
Geographic Controls	No	Yes	No	Yes
Observations	107	107	107	107
Y Mean	0.35	0.35	40.52	40.52
Adjusted R^2	-0.007	0.220	0.004	0.202

Great Depression severity is the normalized growth (decline) in retail sales per capita from 1929 to 1933. County level data on Great Depression severity is matched to county level data on mobility in the contemporary period from Chetty et al. (2014b). There are 107 observations, rather than the 99 cities in my main sample because several cities include multiple counties in the contemporary data, including New York, St. Louis, and three independent cities in Virginia. In the first two columns, the outcome variable is the slope from a regression at the county level of children's rank in the income distribution on parent's rank in the income distribution, for children born between 1980 and 1982. In the second two columns, the outcome variable is the expected rank of children with parents at the 25th percentile of the national income distribution, calculated from a regression of children's rank on their parent's rank. The geographic controls row indicates the inclusion of linear controls for latitude and longitude.

Source: Census of Retail Sales; Chetty et al. (2014a)

Table 8: Fathers' Income and Sons' Educational Outcomes Across Depression Severity

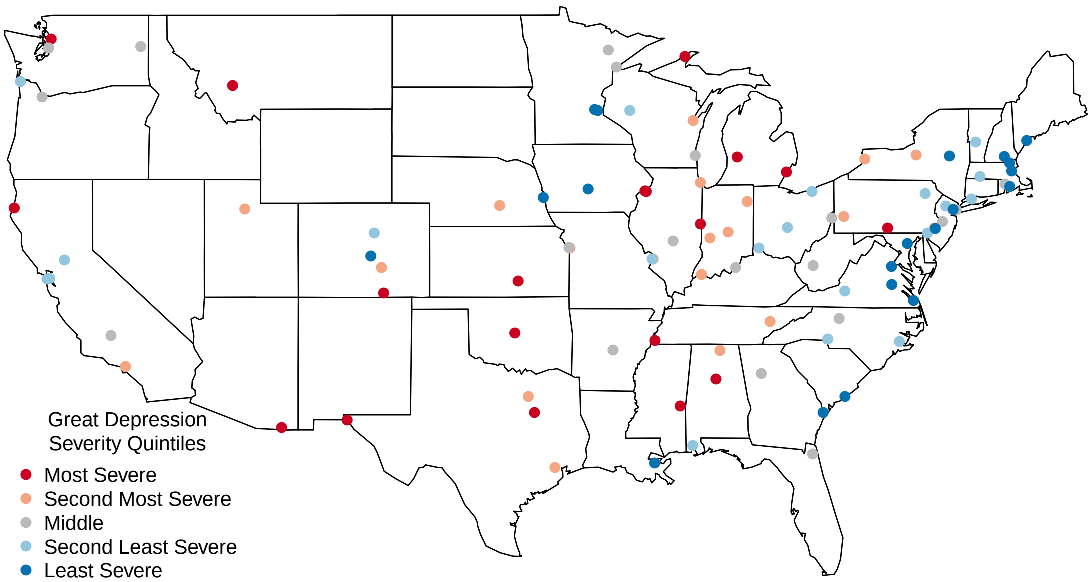
	Years of Education		High School Graduate		College Graduate	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Father Earnings, 1920	1.320^{***}	1.318^{***}	0.218^{***}	0.218^{***}	0.051^{***}	0.051^{***}
	(0.168)	(0.169)	(0.028)	(0.029)	(0.014)	(0.014)
Log Father Earnings 1920 X GD Normalized Severity		-0.034		-0.008		-0.003
		(0.125)		(0.023)		(0.012)
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6129	6129	6129	6129	6129	6129
City Clusters	99	99	99	99	99	99
Adjusted R^2	0.095	0.095	0.083	0.083	0.013	0.013

Standard errors clustered at the 1920 city level. The sons of higher earning fathers have more years of education and are more likely to graduate from high school and college. However, the relationship between father's earnings and son's education is not significantly affected by local Great Depression severity. Sons have an average of 10.7 years of education in my sample. 48% of the sons are high school graduates and only 3% are college graduates.

Source: BLS Cost of Living Survey 1918-1918; 1940 Complete Count Census; Census of Retail Sales

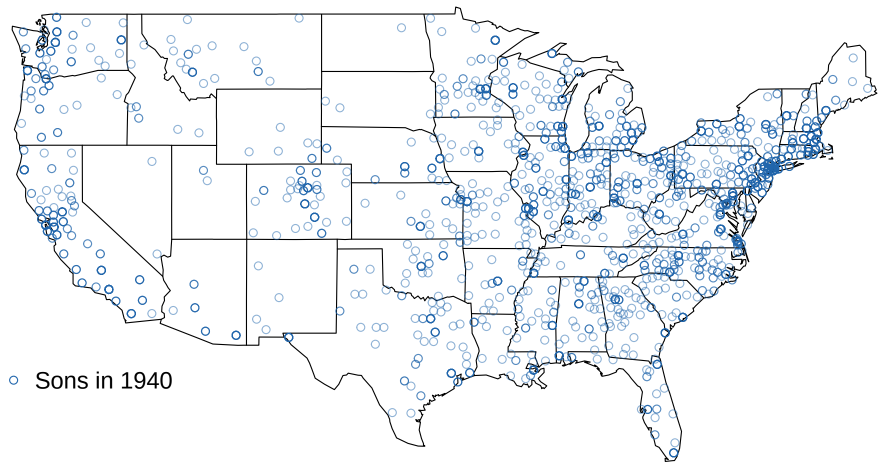
A Appendix

Figure A.1: City-level Great Depression Severity, measured using per capita growth in retail sales from 1929 to 1933 (Fishback et al. 2003).

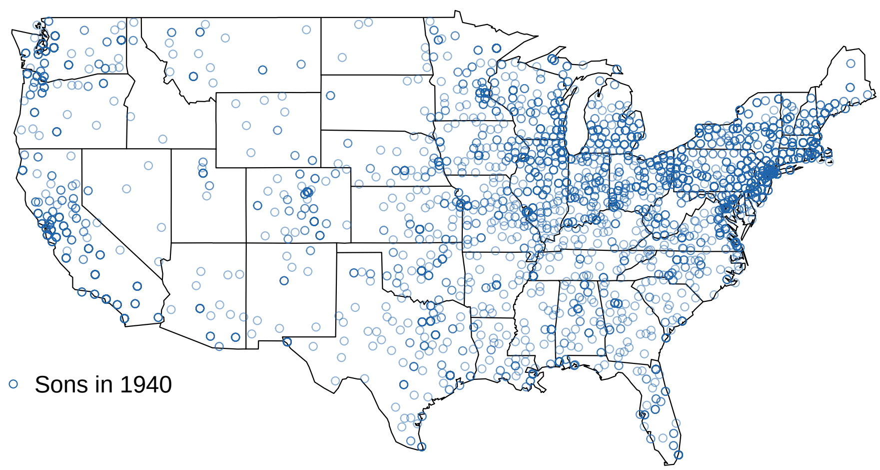


This map illustrates the variation in Great Depression downturn across the 99 American cities from the BLS Cost of Living Survey. Downturn severity is split into quintiles. Cities with the most severe downturns are in red, cities with the most mild downturns are in blue. Great Depression severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933.

Figure A.2: The 1940 location of sons in my sample. Sons are plotted in the county in which they reside.



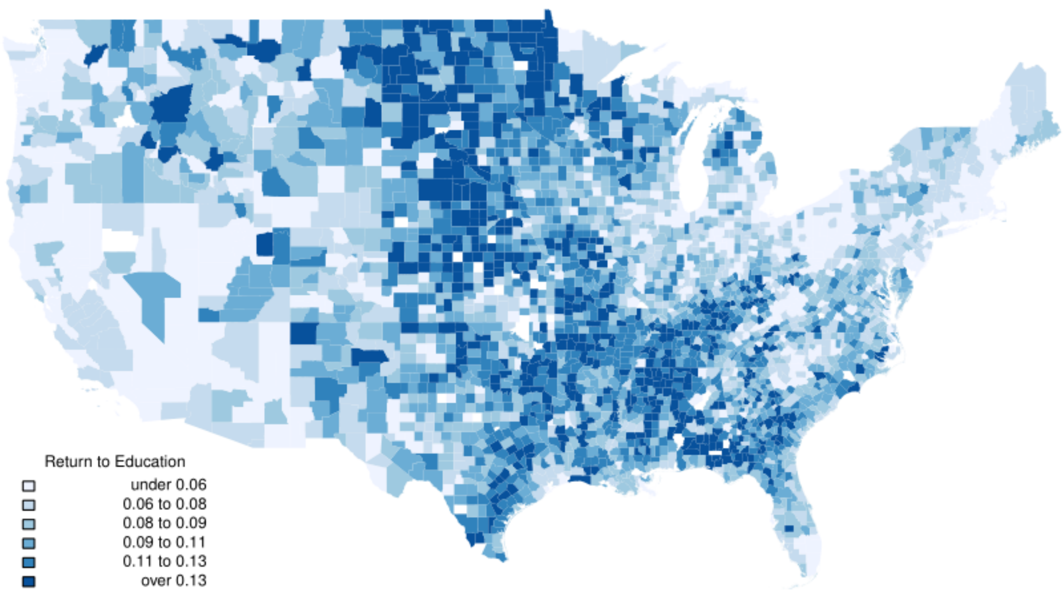
(a) Sons in 1940 from the BLS Sample



(b) Sons in 1940 from the full IPUMS Sample

These maps illustrate the locations in the 1940 census of the sons in my two intergenerational samples.

Figure A.3: The Estimated Return to Education from a Mincerian Earnings Regression by County in 1940



I estimate the return to a year of education in the complete 1940 census. I observe annual earnings in 1939, completed years of education, age, and place of residence in 1940. Separately for each of the 3,071 counties in the country, I run a simple Mincerian returns to education regression, regressing log earnings on years of education and age dummies for all full-time employed white men between the ages of 16 and 65 in the county. The coefficient on education in each regression is the observed return to education, mapped above. These estimated returns to education are descriptive and not causal. The returns to education correlate negatively with county population: an increase in county population by 1% correlates with a 1% smaller estimated return. It is also apparent from the map that the human capital returns were higher in the plains states—the Dakotas, Nebraska, and Kansas—as well as the in mid-South. The returns to education were extremely low in New England and New York, as well as in the industrial Midwest—Ohio, Illinois, Michigan, and Indiana—and on the West Coast.

Table A.1: Great Depression Severity Decreases Intergenerational Mobility: Local Cost of Living Adjusted Earnings

	IGE				Rank-Rank			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Father Earnings 1920	0.256***	0.281***	0.173***	0.182***				
	(0.042)	(0.044)	(0.055)	(0.058)				
Log Father Earnings 1920 X GD Normalized Severity	0.074*	0.109**						
	(0.043)	(0.046)						
Log Father Earnings 1920 X GD Above Median Severity			0.165**	0.196**				
			(0.082)	(0.085)				
Father Earnings Rank 1920					0.178***	0.186***	0.102**	0.101**
					(0.029)	(0.031)	(0.044)	(0.047)
Father Earnings Rank 1920 X GD Normalized Severity					0.074**	0.096***		
					(0.029)	(0.033)		
Father Earnings Rank 1920 X GD Above Median Severity							0.142**	0.162**
							(0.058)	(0.062)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4730	4730	4730	4730	4952	4952	4952	4952
Clusters	99	99	99	99	99	99	99	99
Adjusted R^2	0.165	0.175	0.165	0.175	0.137	0.142	0.137	0.141

Earnings of both fathers and sons are adjusted for variations in local prices. For fathers, I use median rents by city in 1920, drawing on data from the BLS Cost of Living Survey. For sons, I use median rents by county in 1940 from the 1940 Federal Census. Earnings are divided by the rental price index in the city of residence (for fathers based on 1920, for sons based on 1940). These yield estimates of real annual earnings. This table replicates, but with the local price adjustment, the results in Table 4. Estimates of intergenerational mobility based on a linked sample from the BLS survey of urban families in 1918-1919 to the 1940 Federal census. Each column is a regression of the son's outcome in 1940 on the father's corresponding outcome in 1918-1919, a measure of Great Depression severity in the city of residence in 1918-1919, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. In the odd columns, I include state fixed effects and direct controls for Great Depression severity (normalized in columns 1 and 5, above or below median in columns 3 and 7) but omit the point estimates from the table. In the even columns, these controls are absorbed by the city fixed effects. All fixed effects are based on the city of residence in 1918-1919. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. IGE is the intergenerational elasticity of income, and the dependent variable is log son earnings in 1940. Rank-rank mobility compares the son's position in the earnings distribution in 1940 to the father's position in 1918-1919 and the dependent variable is son earnings rank in 1940.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample; 1940 Complete Count Census.

Table A.2: Great Depression Severity Decreases Intergenerational Mobility in All Cities

	Son OccScore (IPUMS, All Cities)					
	(1)	(2)	(3)	(4)	(5)	(6)
Log Father Occupation Score 1920	0.261***	0.264***	0.266***	0.268***	0.244***	0.248***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.011)	(0.010)
Log Father Occupation Score 1920 X GD Normalized Severity			0.020**	0.019*		
			(0.010)	(0.010)		
Log Father Occupation Score 1920 X GD Above Median Severity					0.043***	0.037**
					(0.015)	(0.015)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No
City Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	35035	35035	33067	33067	33067	33067
Clusters	161	161	143	143	143	143
Adjusted R^2	0.120	0.116	0.122	0.118	0.122	0.118

Estimates of intergenerational mobility based on occupation scores for all cities. Mobility estimates in all columns are based on a linked sample from the IPUMS 1% sample of the 1920 census to the full 1940 census. Rather than restrict the sample to sons living in one of the 99 cities surveyed by the BLS in 1918-1919, I include all sons living in cities in 1920. Each column is a regression of the son's log occupation score in 1940 on the father's log occupation score in 1920. In the first two columns, I present an overall measure of mobility, comparable to the occupation score elasticities estimated in Table 3. In the four final columns, I include both a measure of Great Depression severity in the city of residence in 1920 and an interaction of severity and the father's occupation score. These columns are comparable to Table 5, showing the effect of Great Depression on occupation score based mobility. Controls include quartics in the son's and father's ages. In the odd columns, I include state fixed effects and direct controls for Great Depression severity (normalized in column 3, above or below median in column 5) but omit the point estimates from the table. In the even columns, these controls are absorbed by the city fixed effects. All fixed effects are based on the city of residence in 1920. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. Occupation scores are calculated as the national median income for men in the occupation.

Source: IPUMS 1920 1% Census Sample; 1940 Complete Count Census.

Table A.3: Matching Procedure Does Not Drive Results

	IGE			Rank-Rank		
	(1) Original	(2) Remove Best	(3) Remove Worst	(4) Original	(5) Remove Best	(6) Remove Worst
Log Father Earnings 1920	0.280***	0.305***	0.279***			
	(0.043)	(0.049)	(0.050)			
Log Father Earnings 1920 X GD Normalized Severity	0.108**	0.111**	0.121**			
	(0.042)	(0.047)	(0.050)			
Father Earnings Rank 1920				0.213***	0.216***	0.210***
				(0.033)	(0.037)	(0.037)
Father Earnings Rank 1920 X GD Normalized Severity				0.100***	0.089**	0.097**
				(0.036)	(0.041)	(0.037)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4730	3629	4131	4952	3788	4324
Clusters	99	99	99	99	99	99
Adjusted R^2	0.188	0.178	0.189	0.153	0.147	0.154

Estimates of intergenerational mobility based on a linked sample from the BLS survey of urban families from 1918-1919 to the 1940 Federal census. Each column is a regression of the son's outcome in 1940 on the father's corresponding outcome in 1918-1919, a measure of Great Depression severity in the city of residence in 1918-1919, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. In the second and fifth columns, I exclude observations enumerated by agents in the top quarter of match rates ($\geq$63%). In the third and sixth columns, I exclude observations enumerated by agents in the bottom quarter of match rates ($\leq$48%). All fixed effects are based on the city of residence in 1918-1919. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. IGE is the intergenerational elasticity of income. Rank-rank mobility compares the son's position in the earnings distribution in 1940 to the father's position in 1918-1919.

Source: BLS Cost of Living Survey 1918-1918; 1940 Complete Count Census; Census of Retail Sales

Table A.4: Depression Effects on Intergenerational Mobility Only for Migrants

	IGE			Rank-Rank		
	Full Sample (1)	Migrants (2)	Non-Migrants (3)	Full Sample (4)	Migrants (5)	Non-Migrants (6)
Log Father Earnings 1920	0.280***	0.352***	0.236***			
	(0.043)	(0.069)	(0.051)			
Log Father Earnings 1920 X GD Normalized Severity	0.108**	0.154**	0.063			
	(0.042)	(0.063)	(0.047)			
Father Earnings Rank 1920				0.213***	0.252***	0.183***
				(0.033)	(0.057)	(0.045)
Father Earnings Rank 1920 X GD Normalized Severity				0.100***	0.102**	0.074
				(0.036)	(0.050)	(0.048)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4730	2001	2729	4952	2090	2862
Clusters	99	99	99	99	99	99
Adjusted R^2	0.188	0.173	0.243	0.153	0.135	0.206

Estimates of intergenerational mobility based on a linked sample from the BLS survey of urban families from 1918-1919 to the 1940 Federal census. Each column is a regression of the son's outcome in 1940 on the father's corresponding outcome in 1918-1919, a measure of Great Depression severity in the city of residence in 1918-1919, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. The first and fourth columns replicate my main results from Table 4, columns (2) and (6). In the second and fifth columns, I restrict the sample to those sons migrating from their 1920 city of residence in 1940. In the third and sixth columns, I restrict the sample to those sons remaining in their 1920 city of residence. The results suggest that the Depression effected the mobility parameter through the migrating sons, not the sons remaining in their 1920 cities. All fixed effects are based on the city of residence in 1918-1919. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. IGE is the intergenerational elasticity of income. Rank-rank mobility compares the son's position in the earnings distribution in 1940 to the father's position in 1918-1919.

Source: BLS Cost of Living Survey 1918-1918; 1940 Complete Count Census; Census of Retail Sales

Table A.5: New Deal Spending Did Not Affect Intergenerational Mobility

	Panel A. Intergenerational Elasticity						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Father Earnings 1920	0.266***	0.264***	0.259***	0.267***	0.265***	0.265***	0.268***
	(0.040)	(0.040)	(0.040)	(0.039)	(0.040)	(0.039)	(0.038)
Log Father Earnings 1920 X GD Normalized Severity	0.105***	0.105***		0.105***	0.103***	0.104***	0.102***
	(0.040)	(0.038)		(0.040)	(0.038)	(0.037)	(0.035)
Log Father Earnings 1920 X Total New Deal Spending		-0.043	-0.044				
		(0.037)	(0.038)				
Log Father Earnings 1920 X New Deal Relief Spending				0.011			0.025
				(0.041)			(0.039)
Log Father Earnings 1920 X New Deal Public Works					-0.040		-0.040
					(0.047)		(0.048)
Log Father Earnings 1920 X New Deal Real Estate and Insurance						-0.075*	-0.076*
						(0.040)	(0.040)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4730	4730	4730	4730	4730	4730	4730
Clusters	99	99	99	99	99	99	99
Adjusted R^2	0.199	0.199	0.197	0.198	0.199	0.199	0.199
	Panel B. Rank-Rank Mobility						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Father Earnings Rank 1920	0.203***	0.203***	0.205***	0.205***	0.202***	0.204***	0.206***
	(0.033)	(0.033)	(0.034)	(0.033)	(0.033)	(0.032)	(0.032)
Father Earnings Rank 1920 X GD Normalized Severity	0.077**	0.077**		0.076**	0.077**	0.076**	0.074**
	(0.034)	(0.034)		(0.034)	(0.033)	(0.033)	(0.033)
Father Earnings Rank 1920 X Total New Deal Spending		-0.007	-0.005				
		(0.037)	(0.039)				
Father Earnings Rank 1920 X New Deal Relief Spending				0.022			0.026
				(0.035)			(0.035)
Father Earnings Rank 1920 X New Deal Public Works					0.015		0.013
					(0.040)		(0.040)
Father Earnings Rank 1920 X New Deal Real Estate and Insurance						-0.040	-0.043
						(0.030)	(0.029)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4952	4952	4952	4952	4952	4952	4952
Clusters	99	99	99	99	99	99	99
Adjusted R^2	0.151	0.151	0.150	0.151	0.151	0.151	0.151

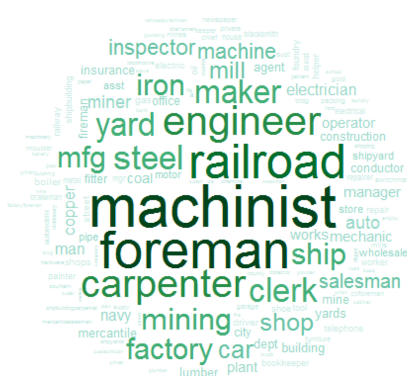
Great Depression severity is the normalized growth (decline) in retail sales per capita from 1929 to 1933. New Deal spending is drawn from Fishback et al. (2003). All spending measures are in per capita terms and normalized. Total New Deal spending includes spending on public works, relief, and loans, as well as other smaller programs.

Source: BLS Cost of Living Survey 1918-1918; IPUMS 1920 1% Census Sample; 1940 Complete Count Census; Census of Retail Sales

Figure A.4: Occupation titles in the BLS survey among fathers

(a) All BLS Fathers

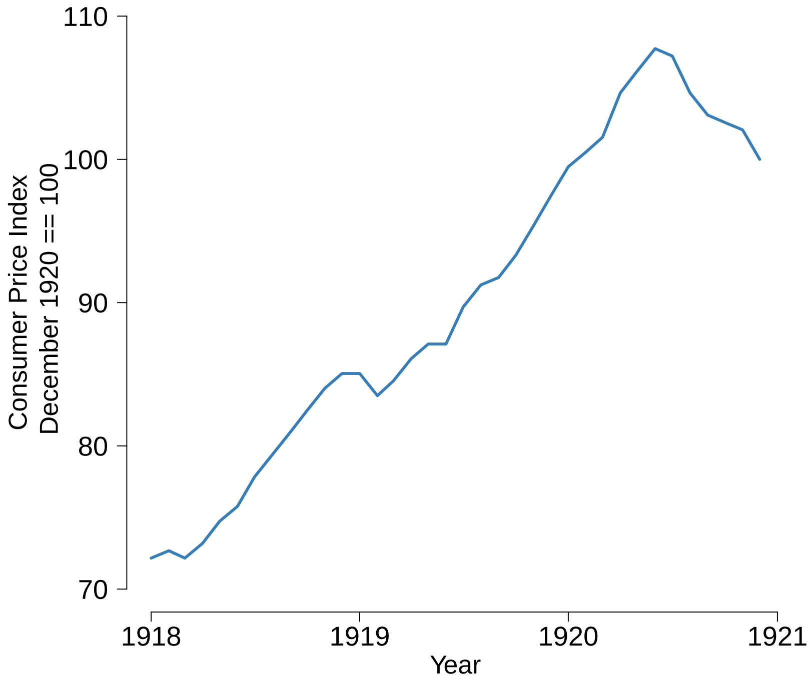
(b) Bottom Earnings Quartile



(c) Top Earnings Quartile

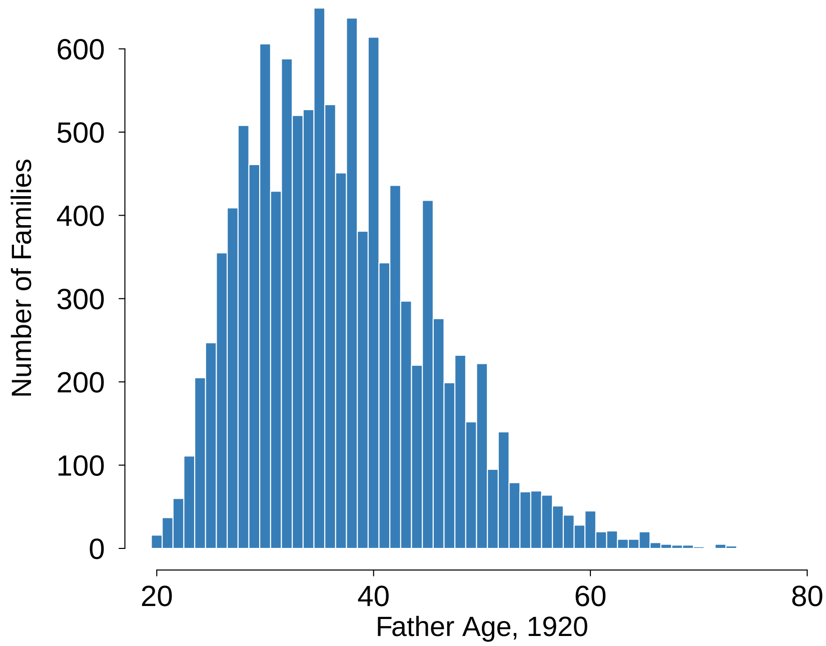
The word clouds represent the most frequent single word strings in occupation titles, scaled and colored by frequency. Among the fathers with the least earnings in 1918 or 1919, the most common occupation descriptions included the words laborer and factory. Among the fathers with the most earnings, however, descriptions like machinist, foreman, railroad, carpenter, and engineer were much more common.

Figure A.5: Consumer Price Index, 1918 to 1920

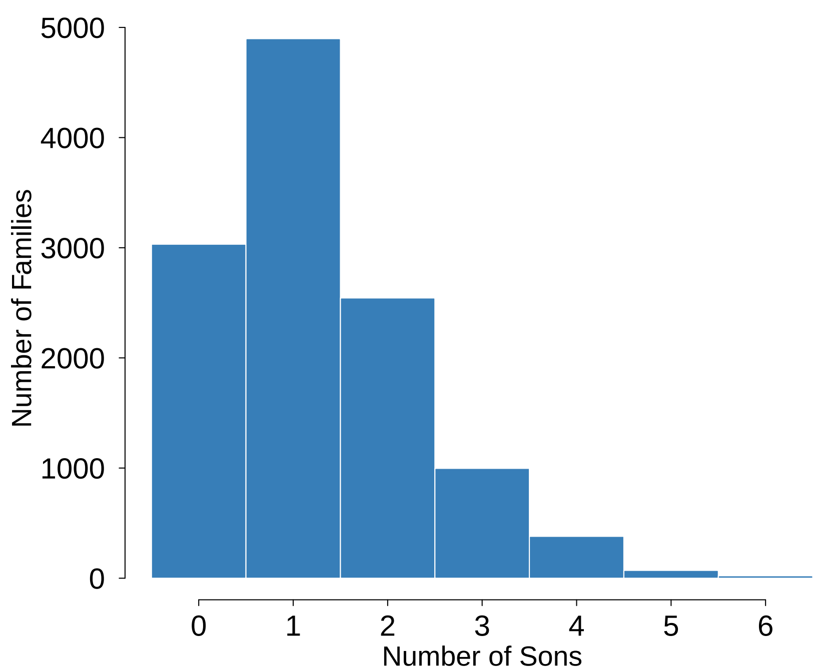


The final year of the First World War and the two following years of peace were a period of rapid price changes. The families surveyed by the BLS were asked about earnings in the previous 12 months, relative to survey dates from July 1918 to February 1919. I use the monthly CPI to standardize and normalize the earnings responses. CPI data for all urban consumers are drawn from a BLS series, stored at the Federal Reserve Bank of St. Louis archive, FRED: `https://research.stlouisfed.org/fred2/series/CPIAUCNS/`.

Figure A.6: Demographics of the BLS survey

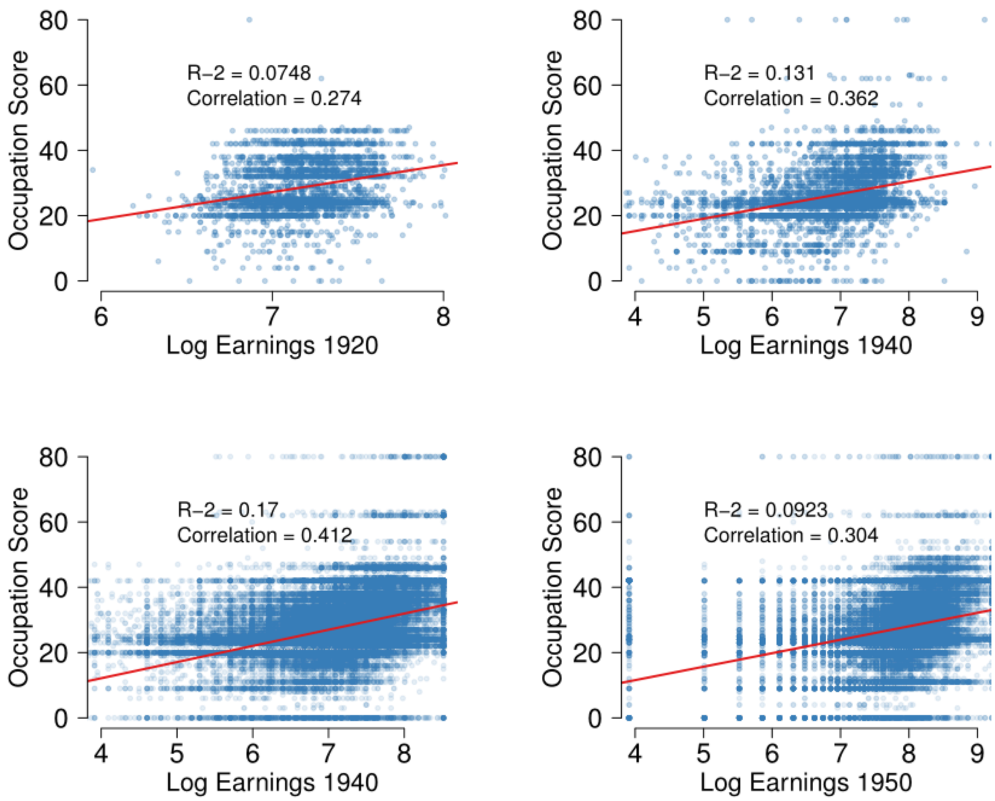


(a) Father's Age



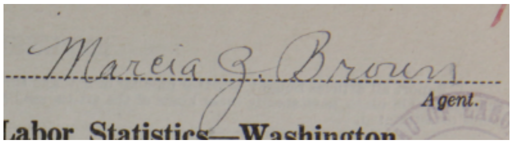
(b) Number of Sons

Figure A.7: The correlation of occupation score and log earnings

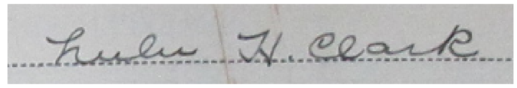


Occupation scores are calculated by IPUMS based on median earnings by occupation in 1950. Linking occupations across time is imprecise: some occupations may rise in earnings and prestige, while others fall, and other occupations may not still exist in the same form in 1950 as in 1920. However, generally, occupation score is a reasonable measure of earnings throughout my sample period. In the first plot, I graph log earnings in 1920 in my sample of fathers from the BLS survey against their occupation score. In the second plot, I graph log earnings in 1940 in my sample of sons from the BLS survey, matched into 1940, against their occupation score. In the bottom row, I show the same correlations for the 1940 IPUMS 1% and the 1950 IPUMS 1% sample.

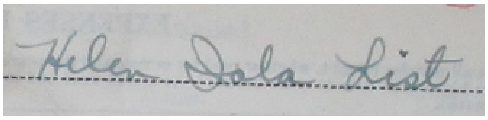
Figure A.8: Examples of Enumerator Signatures from the Original BLS Surveys



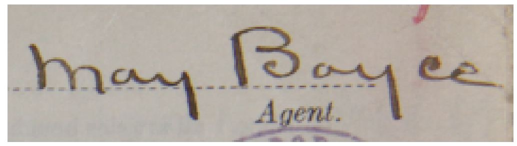
(a) Marcia G. Brown



(b) Lulu H. Clark



(c) Helen Iola List



(d) May Boyce

Records written up by the enumerators in the top row have very low match rates from the BLS sample to the 1940 census, while entries recorded by the enumerators in the bottom row have very high match rates. The quality and clarity of the enumeration of the original records is a strong determinant of the accuracy with which names and other biographical information can be transcribed.

B What Predicts Local Great Depression Severity?

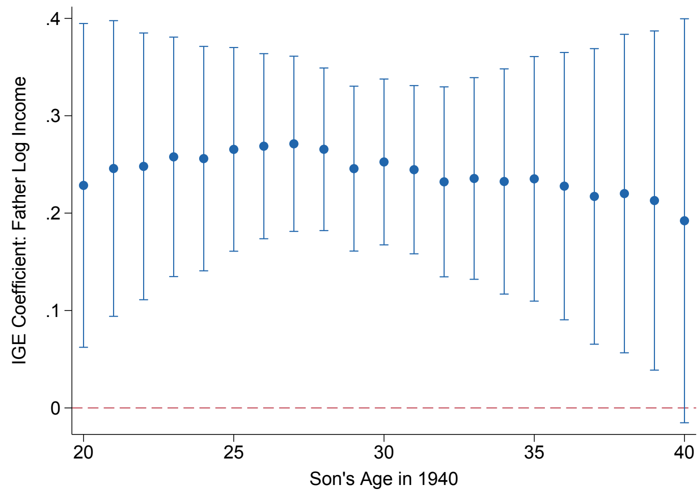
In this section, I review the literature on the Great Depression and its local variation across the United States and empirically test theories of local variation on the cities in my sample. The primary explanation of the Depression's regional variation is pre-1929 industry mix: cities and states specializing in durable manufacturing or extraction of raw materials used in durable manufacturing saw the largest downturns.

The consensus in the macroeconomic literature is that monetary factors caused the Great Depression (Friedman and Schwartz 1963; Bernanke 2000), particularly reliance on the gold standard (Bernanke 1995; Eichengreen 1992; Temin 1989). But while such factors may explain international variation, monetary policy did not vary within the US.[82]

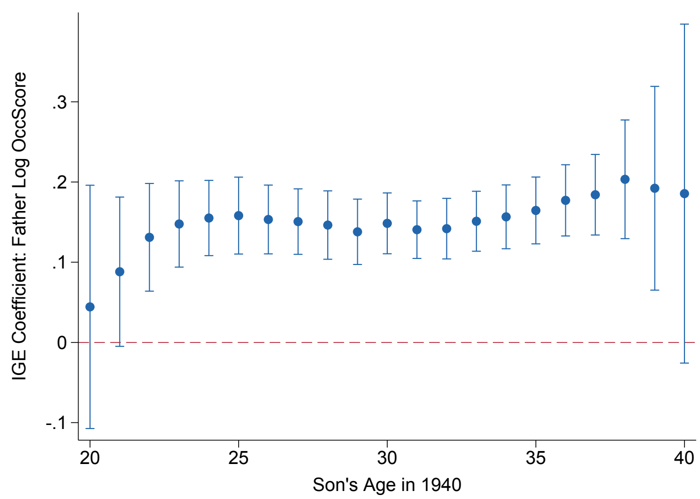
There was large regional variation in the severity of the Depression. As John Wallis writes, "neither the Great Crash nor the economic recovery that followed was evenly felt across the different regions of the country" (Wallis 1989). The downturn was worst in the mountain states but more mild in the upper south (Rosenbloom and Sundstrom 1999). I explore three possible explanations for this variation: industry mix, 1920 growth and subsequent bank failures, and state capital status.

[82]Financial regulatory policy did vary across Federal Reserve districts. As Richardson and Troost (2009) show, rates of bank failure did change dramatically at the border between activist and conservative Fed districts. Ziebarth (2013) extends these results to the real economy using a sample of plants from the Census of Manufactures. However, variation across Fed districts cannot explain why the downturn varied within districts: Pittsburgh endured a huge decline while Cleveland had a relatively mild recession.

Figure A.9: Estimates of intergenerational mobility are relatively stable across sons by age



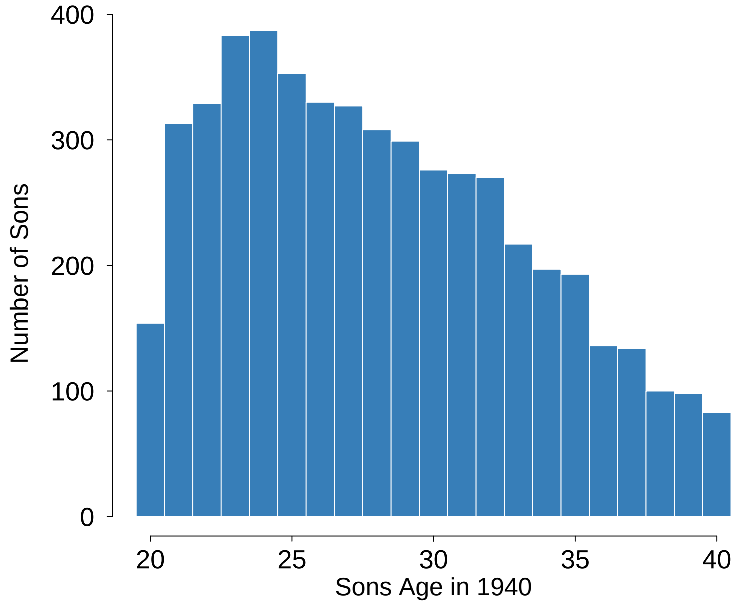
(a) BLS Sample



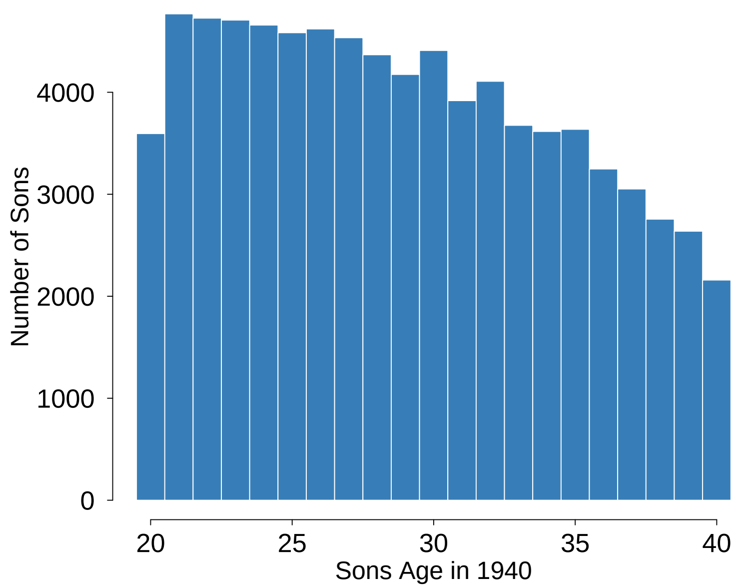
(b) IPUMS 1920-1940 Sample

Higher IGE coefficients implies a stronger link between fathers and sons and less mobility. The father and son ages at which income or occupations are observed often complicate the intergenerational mobility estimates. For example, many early IGE studies have been criticized for observing sons who were too young and fathers who were too old, which biases parameter estimates down and implies spuriously large levels of mobility (Corak 2006; Grawe 2004; Mazumder 2015). In this figure, I explore this potential issue in my sample. In the BLS sample, I regress son's log earnings in 1940 on father's log earnings in 1920 interacted with a set of son's age dummy variables, controlling for a quartic in father's age and city fixed effects. In the IPUMS sample, I calculate mobility rates using occupation scores, running a similar regression of son's log occupation score in 1940 on father's log occupation score in 1920 interacted with a set of son's age dummy variables, controlling for a quartic in father's age and city fixed effects. I find similarly stable estimates of the occupation score mobility after age 23. The IPUMS-based estimates are more precise than those based on my BLS sample, as the sample is nearly four times larger. In Figure A.10, I plot the sample sizes underlying these figures. Using contemporary administrative data, Chetty et al. (2014a) do not find stabilization mobility estimates until sons in their late 20s. However, given the differences in mean education between 1940 and recent decades, parameter stabilization does occur at similar points of labor market experience.

Figure A.10: Distribution of son's adult ages in 1940.



(a) BLS Sample



(b) IPUMS 1920-1940 Sample

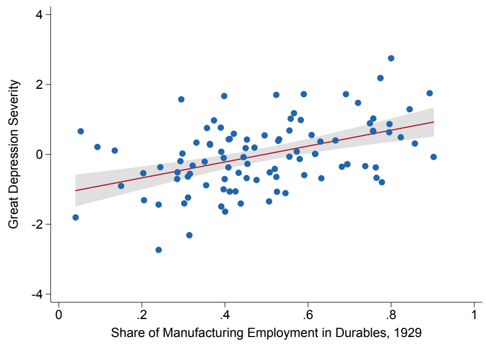
First, American cities all have different mixes of industries. At the onset of the Depression, for example, Detroit specialized in auto manufacturing, Pittsburgh in steel, and Lawrence, MA in textiles. Industries like mining, metal work, and timber declined much more during the Depression than others, so the cities with a higher share of employment or income from these industries would also decline more. At the state level, this also explains why the heavy-industry-focused midwest (particularly census region East North Central) and the extractive-natural-resources-focused mountain states (specializing in lumber and mining) suffered the worst Depression downturns (Wallis 1989). Inelastic demand for cigarettes and other tobacco products may explain the milder declines in the upper south (Heim 1998). I use two metrics to test these industry effects. First, drawing on Romer (1990), I measure the share of manufacturing employment in durables: durable consumption suffered a more immediate decline than nondurables in the first few years of the Depression. Following Boone and Wilse-Samson (2014), I calculate the share of manufacturing employment in durables in the IPUMS 1930 1% sample using the standardized IPUMS industry codes. Second, I construct a Bartik shock, compiling industry mix before the Depression and assuming that city-level employment in a given industry would grow (or shrink) at the same rates as the industry grew nationally.[83] In the top panel of Figure B.1, I show that the severity of the Depression is strongly correlated with both the share of manufacturing employment in durables (a) as well as the predicted decline based on industry mix (b).

Second, the booms of the 1910s and 1920s were also regionally varied. I measure the 1920s booms in each city with the growth rate of manufacturing employment from 1920 to 1929, as measured in the Census of Manufactures. I normalize these growth rates to be mean zero and standard deviation of one. In addition, I draw on bank failure data at the state level from FDIC Data Bank, ICPSR dataset 7. The bubble of the preceding decades made banks more susceptible to failure during the Depression, as they had invested in inflated assets like farm real estate (Calomiris and Mason 2003). Because interstate banking laws remained restrictive during this time period, the banks in a given city would be especially sensitive to local or state-wide bubbles. I count the number of banks failing between 1929 and 1933 and divide by the number of banks in each state in 1929.[84] In the bottom panel of Figure B.1, I show that growth rates during the 1920s as

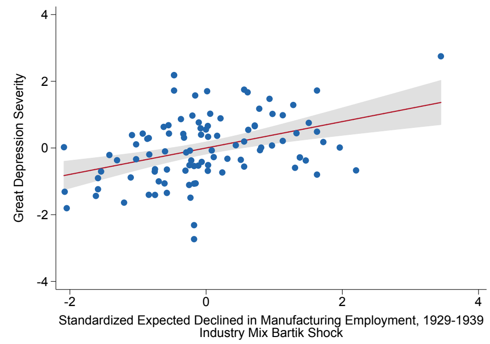
[83] I draw on the Census of Manufactures in 1929 to calculate industry mix before the Depression. For a handful of cities, industry-level data is not available, and I use state industry mix to proxy.

[84] Failure rates cannot be weighted by bank size, as assets data is only available in aggregate.

Figure B.1: Local correlates of Great Depression severity.

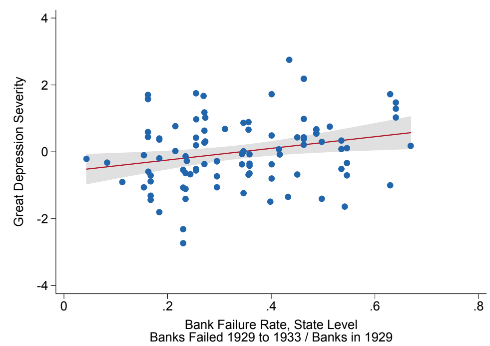


(a) Durable Share



(b) Industry Mix Bartik Shock

(c) 1920s Growth in Manufacturing Employment



(d) Bank Failures

well as state-level bank failure rates are positively correlated with Depression severity, though the boom-bust relationship is weaker.

Third, state capitals across the country appear to have suffered less severe downturns, perhaps because the response of government employment in the capital to the downturn was less elastic than private sector employment. Across all counties in the United States, I find that the growth rate of retail sales per capita from 1929 to 1933 in counties including a capital city was about 8 percentage points larger than the growth rate in all other counties: capitals declined by 0.39 log points, while non-capitals declined by 0.47 log points. 15 of the 99 cities in my sample are state capitals. When I regress Depression severity on capital status in my sample of BLS cities, I find that capitals declined 0.06 fewer log points than non-capitals.

While the bivariate correlations presented in Figure B.1 are suggestive, I turn to regressions to gauge the relative importance of local factors in determining city-level Depression severity. To facilitate the comparisons in Table B.1, I standardize every variable (except for the capital status

Table B.1: Correlates of Local Great Depression Severity

	Normalized Great Depression Severity			
	(1)	(2)	(3)	(4)
Share of Manufacturing Employment in Durables	0.268**	0.377***		0.283***
	(0.103)	(0.095)		(0.098)
Standardized Bartik Shock	0.227**		0.347***	0.133
	(0.107)		(0.096)	(0.100)
Employment Growth in 1920s	0.197*	0.186*	0.232**	0.165
	(0.103)	(0.104)	(0.103)	(0.108)
Bank Failures 1929 to 1933				
Share of Banks in 1929	0.116	0.135	0.156*	0.115
	(0.082)	(0.082)	(0.089)	(0.086)
Capital City of State	-0.257	-0.255	-0.351	-0.265
	(0.225)	(0.220)	(0.248)	(0.221)
Latitude				-0.025
				(0.022)
Longitude				-0.014**
				(0.006)
Log Population				-0.003
				(0.075)
Observations	99	99	99	99
Adjusted R^2	0.266	0.233	0.221	0.293

Source: Census of Retail Sales; Census of Manufactures; IPUMS 1930 1% sample.

indicator) to have mean zero and standard deviation one. I find that the industry mix variables, either the share of manufacturing workers in durables or a standardized Bartik shock, have the strongest effects on severity. The two measures are highly collinear, but, when included separately, a standard deviation increase in either increases Depression severity by more than a third of a standard deviation. The effects of a standard deviation increase in either bank failures or growth during the 1920s, meanwhile, increases Depression severity by between 0.15 and 0.25 of a standard deviation. Finally, the capital effect is the expected sign, as capital cities had less severe Depressions; however, this effect is statistically insignificant.[85]

Garrett and Wheelock (2006) conduct a similar study at the state level, exploring the decline in per capita state income from 1929 to 1933, and find that only industry structure and per capita income before the Depression correlate significantly with Depression severity: states with a higher concentration of industries declining during the Depression declined more, as did states with higher

[85] Recall that 15 of the 99 cities in my sample are capitals, meaning the indicator variable has a standard deviation of 0.36. Thus, multiplying the coefficient on capital by 0.36 allows me to interpret it in the same way as the other coefficients and yields a standardized effect of about 0.13.

incomes in 1929. However, the authors conclude that spatial spillovers and industrial composition fully explain the effect of initial income in 1929 on variation in Depression downturn.

C Ranking Income in 1920

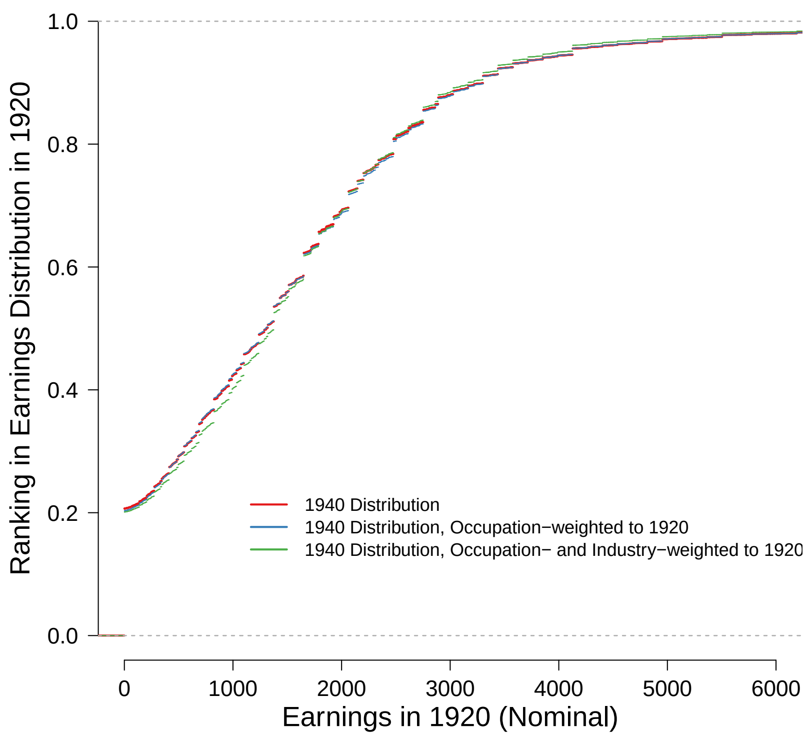
One measure of mobility I use in this paper is the rank-rank parameter. In this section, I describe the construction of the earnings ranking for my sample of fathers from 1918 to 1919, where the full earnings distribution is unknown. I construct three different ranking distributions, and my results are robust to choosing any of the three.

The rank-rank parameter is the coefficient from a regression of the son's ranking in the earnings distribution on his father's earnings distribution rank, as well as controls for the son's and father's age. Four measures are required to calculate the rank-rank parameter: (1) earnings of the father, (2) earnings of the son, (3) the relevant earnings distributions for fathers, and (4) the relevant earnings distribution for sons. I have the complete 1940 census, which includes labor earnings information for every person in the country. This data enables me to observe a son's earnings in 1940 as well as his ranking in the distribution. For the fathers in 1918-1919, I observe earnings from the BLS sample. However, this sample is one of the only sources for earnings information for this period. While it would be simple to calculate each father's position within the BLS sample, this position is a very misleading representation of the father's position overall. The BLS targeted middle income respondents, as the survey takers were hoping to study the costs of living faced by urban consumers who were wage workers or had salaries under $2000 per year. Instead, I construct an earnings distribution in 1920, leveraging the complete and known earnings distribution for 1940.

I reconstruct the earnings distribution in 1920 in three different ways in order to rank fathers in the earnings distribution. I base each ranking on the known 1940 earnings distribution. In the first, I use the 1940 distribution and simply convert earnings from 1940 dollars to 1920 nominal dollars using the CPI. In the second, I use the 1940 distribution, but reweight observations by the relative prevalence of occupations in the 1920 census and convert 1940 dollars to 1920 dollars. In the third, I use the 1940 distribution, but reweight observations by the relative prevalence of occupations and industries in the 1920 census and convert 1940 dollars to 1920 dollars.

In all three ranking constructions, I use the CPI to deflate the distribution from 1940 dollars to 1920 dollars. I choose the CPI rather than a measure of mean nominal earnings because I need

Figure C.1: Estimated 1920 Earnings Cumulative Distribution



I reconstruct the earnings distribution in 1920 in three different ways in order to rank fathers in the earnings distribution. In the first, I use the 1940 income distribution, only converting earnings from 1940 to 1920 nominal levels (plotted in red). In the second, I use the 1940 income distribution, but reweight observations by the relative prevalence of occupations in the 1920 census and convert 1940 dollars to 1920 dollars (plotted in blue). In the third, I use the 1940 income distribution, but reweight observations by the relative prevalence of occupations and industries in the 1920 census and convert 1940 dollars to 1920 dollars (plotted in green).

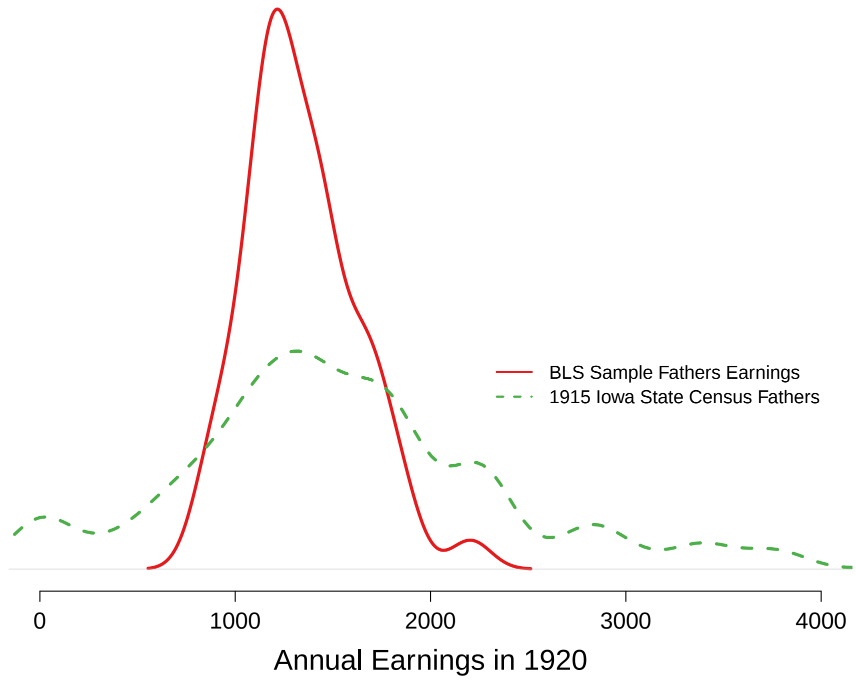
a monthly price deflator to convert the BLS earnings themselves. The BLS sample was collected over the course of eight months, from July 1918 to February 1919, during a period of significant price changes, as I document in Figure A.5. Earnings were reported relative to the 12 months prior to the survey data, and thus they vary across respondents.

To reweight by occupation and occupation-by-industry, I collect the counts of men in each occupation or occupation-by-industry cell in the 1920 IPUMS 1% sample. Occupations and industry are collected by census enumerators as free-form strings but standardized by IPUMS. Specifically, I rely on the occ1950 and ind1950 variables, which are occupation and industry codes that have been standardized both within and across census waves to 1950 definitions. There are 258 unique occupation codes in the 1920 IPUMS sample and 3662 unique occupation-by-industry codes. For the occupation reweighting, the weight for each observation in the 1940 earnings distribution is simply the share of men with that occupation in 1920 divided by the share of men with that occupation in 1940. For example, there were 3.5 times as many blacksmiths in 1920 as there were in 1940, yielding a higher weight on blacksmiths in the estimated 1920 distribution than in the actual 1940 distribution. Conversely, there were 30 times as many "Airplane pilots and navigators" in 1940 than in 1920, leading to a very small weight. The reweighting process is parallel for the occupation and industry version, but with smaller cells defined by both occupations and industries. I plot the CDFs of these three ranking functions in Figure C.1.

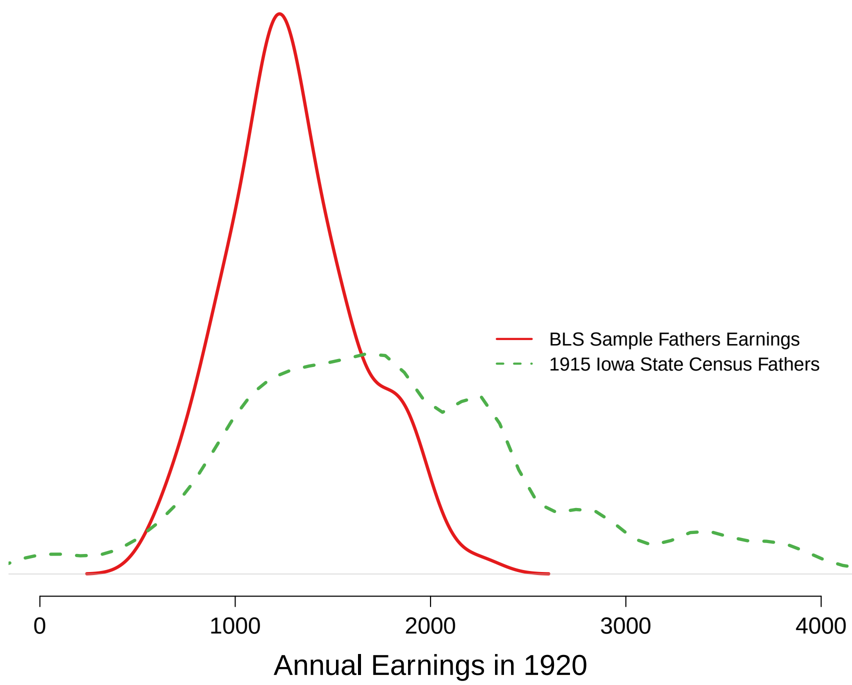
The correlation in earnings rankings of the fathers in my sample across the three possible methods is extremely high. The unweighted method correlates with the occupation weighted ranking at 0.99996 and with the occupation and industry weighted ranking at 0.99974. The occupation weighted and occupation and industry weighted rankings correlate at 0.99960. Given these high correlations, it is not surprising that my results are robust to using any of the rankings. Throughout the paper, I rely on the first, unweighted earnings distribution.

The 1940 census is not the only possible source with which to build an earnings distribution for 1920. The 1915 Iowa State Census also collected earnings data from respondents. Two Iowa cities, Davenport and Des Moines, are among the 99 cities surveyed by the BLS sample. I compare the distribution of earnings in those two Iowa cities from the BLS sample to the distribution of earnings in 1915, drawn from a sample of the state census, digitized originally by Goldin and Katz (2000), in Figure C.2.

Figure C.2: Distribution of Earnings in the BLS Sample in Iowa cities compared to the 1915 Iowa Stata Census



(a) Davenport, IA



(b) Des Moines, IA

The differences in the distributions of earnings in the BLS Sample in Davenport and Des Moines and the distributions of earnings in those two cities based on the 1915 Iowa State Census are comparable to the distributional differences shown in Figure 2, which compared the full BLS sample to the national 1920 earnings distribution. All earnings are measured in 1920 dollars. The complete earnings distribution for 1915 for the two Iowa cities is calculated using the 1915 Iowa state census.

D Imputed Business and Self-Employment Earnings in 1940

I use earnings data from the 1940 Federal Census to measure the adult outcomes of the sons in my intergenerational sample. However, the 1940 census includes detailed records only for earnings from wages and salaries; the exact amounts of other forms of income, including self-employment earnings, were not recorded—only an indicator for more than $50 in such earnings. In this section, I show that this data challenge cannot explain my results by imputing the income of sons using the 1950 Federal Census. My estimates of the effect of variation in Great Depression severity on intergenerational mobility are robust to supplementing my original data with imputed income for sons with non-wage or salary income.

Unlike the 1940 census, the 1950 census recorded earnings data for respondents from both wages and salary, as well as from business income. I draw on the IPUMS 1% sample of the 1950 census, which I limit to white men, aged 20 to 40, replicating my sample. Among the men in the 1950 sample with business earnings, I predict log earnings from business on age, years of education, state of residence, occupation code, and industry code.[86] Returning to my sample of sons in 1940, I calculate total earnings for each son, either by adding predicted business earnings to labor earnings for sons earning both labor and business income in 1940, or by using predicted business earnings directly for sons without any labor earnings.

My main results do not change significantly when supplementing my sample with imputed capital earnings for the sons reporting such earnings in Table D.1. Only 12% of the sons in my sample of 1940 report earning more than $50 in non-wage or salary income, and less than half of that group earned business income exclusively.

E Intergenerational Mobility Model and Measurement Error

I only observe father's income in one year—1918 or 1919—which may be a noisy proxy for permanent income. For the fathers living in cities with larger Great Depression downturns, this one year of income, before the Depression, may be a particularly noisy measure. In this section, I show that noise driven by the Depression cannot explain my results; in fact, if the Great Depression increased

[86] I convert the earnings in 1950 to 1940 dollars using the CPI. To improve the predictive fit, I use a multi-level model that shrinks estimated fixed effects towards the mean when the cell sample sizes are smaller (Gelman and Hill 2007). Both age and years of education enter the prediction non-parametrically. Cross-validation rejected any gains from including interactions of the predictors in the model.

Table D.1: Great Depression Severity Decreases Intergenerational Mobility, Including Imputed Business Earnings

	IGE				Rank-Rank			
	(1) Main	(2) Imputed	(3) Main	(4) Imputed	(5) Main	(6) Imputed	(7) Main	(8) Imputed
Log Father Earnings 1920	0.280***	0.252***	0.181***	0.162***				
	(0.043)	(0.042)	(0.056)	(0.053)				
Log Father Earnings 1920 X GD Normalized Severity	0.108**	0.098**						
	(0.042)	(0.039)						
Log Father Earnings 1920 X GD Above Median Severity			0.197**	0.175**				
			(0.082)	(0.082)				
Father Earnings Rank 1920					0.213***	0.225***	0.124**	0.168***
					(0.033)	(0.028)	(0.051)	(0.038)
Father Earnings Rank 1920 X GD Normalized Severity					0.100***	0.070**		
					(0.036)	(0.030)		
Father Earnings Rank 1920 X GD Above Median Severity							0.175***	0.112**
							(0.066)	(0.056)
Son Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Father Age Quartic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4730	5468	4730	5468	4952	5690	4952	5690
Clusters	99	99	99	99	99	99	99	99
Adjusted R^2	0.188	0.196	0.188	0.196	0.153	0.244	0.153	0.244

Estimates of intergenerational mobility based on a linked sample from the BLS survey of urban families from 1918-1919 to the 1940 Federal census. Each column is a regression of the son's outcome in 1940 on the father's corresponding outcome in 1918-1919, a measure of Great Depression severity in the city of residence in 1918-1919, and an interaction of severity and the father's outcome. Controls include quartics in the son's and father's ages. All fixed effects are based on the city of residence in 1918-1919. Great Depression Severity is measured using the decline in per capita retail sales at the county level from 1929 to 1933. The odd columns, titled "Main," are the standard results from Table 4 with city fixed effects. In the even columns, titled "Imputed," I impute capital earnings for the 12% of the sample reporting more than $50 in non-wage or salary income in 1940. The imputations use the IPUMS 1% sample of the 1950 census, which includes data on business income. Log business income is predicted using a multi-level model including age, years of education, state of residence, occupation code, and industry code. IGE is the intergenerational elasticity of income. Rank-rank mobility compares the son's position in the earnings distribution in 1940 to the father's position in 1918-1919.

Source: BLS Cost of Living Survey 1918-1918; 1940 Complete Count Census; Census of Retail Sales

measurement error, then it would tend to lower the estimated persistence parameter and thus increase the estimated mobility.

The true model is

$$\log(Y_{it}) = \mu^* + (p\theta + \omega)\log(Y_{i,t-1}) + pe_{it}$$

But suppose only noisy measures of log income are observed: $\log(\tilde{Y_{i,t-1}}) = \log(Y_{i,t-1}) + u_{i,t-1}$ and $\log(\tilde{Y_{it}}) = \log(Y_{it}) + u_{it}$. The probability limit of the estimated persistence parameter, β, will be

$$\text{plim}\beta = \frac{Cov\left(\log(\tilde{Y_{it}}), \log(\tilde{Y_{i,t-1}})\right)}{Var\left(\log(\tilde{Y_{i,t-1}})\right)}$$
$$\frac{Cov(\mu^* + (p\theta + \omega)\log(Y_{i,t-1}) + pe_{it} + u_{it}, \log(Y_{i,t-1}) + u_{i,t-1})}{Var(\log(Y_{i,t-1}) + u_{i,t-1})}$$

But μ^* is a constant, and $u_{i,t-1}$ and u_{it} are random noise and do not covary with the other terms.

$$\text{plim}\beta = \frac{Cov((p\theta + \omega)\log(Y_{i,t-1}) + pe_{it}, \log(Y_{i,t-1}))}{Var(\log(Y_{i,t-1}) + u_{i,t-1})}$$

Inspecting the denominator, I have $Var(\log(Y_{i,t-1}) + u_{i,t-1}) = Var(\log(Y_{i,t-1})) + Var(u_{i,t-1}) + 2Cov(\log(Y_{i,t-1}, u_{i,t-1})) = Var(\log(Y_{i,t-1})) + Var(u_{i,t-1})$.

$$\text{plim}\beta = \frac{(p\theta + \omega)Var(\log(Y_{i,t-1})) + pCov(e_{it}, \log(Y_{i,t-1}))}{Var(\log(Y_{i,t-1})) + Var(u_{i,t-1})}$$

Let $Var(u_{i,t-1}) = Var(u_{it}) = \sigma_u^2$. $Var(e_{i,t-1}) = Var(e_{it}) = Var(\delta + \lambda e_{i,t-1} + v) = \lambda^2 Var(e_{i,t-1}) + \sigma_v^2$. That implies that $Var(e_{i,t-1}) = \frac{\sigma_v^2}{1-\lambda^2}$.

$$\begin{aligned}
Var(\log(Y_{i,t-1})) &= Var(\log(Y_{it})) \\
&= Var(\mu^* + (p\theta + \omega)\log(Y_{i,t-1}) + pe_{it}) \\
&= (p\theta + \omega)^2 Var(\log(Y_{i,t-1})) + p^2 Var(e_{it}) + 2(p\theta + \omega)pCov(\log(Y_{i,t-1}), e_{it}).
\end{aligned}$$

That implies that $Var(\log(Y_{i,t-1})) = \frac{p^2 Var(e_{it}) + 2(p\theta+\omega)pCov(\log(Y_{i,t-1}),e_{it})}{1-(p\theta+\omega)^2}$.

$$\begin{aligned} Cov(\log(Y_{i,t-1}),e_{it}) &= Cov(\log(Y_{i,t-1}),\delta+\lambda e_{i,t-1}+v) \\ &= \lambda(p\theta+\omega)Cov(\log(Y_{i,t-1}),e_{it})+\lambda pVar(e_{it}) \end{aligned}$$

That implies that $Cov(\log(Y_{i,t-1}),e_{it}) = \frac{\lambda pVar(e_{it})}{(1-\lambda(p\theta+\omega))}$. Putting this together we have

$$\begin{aligned} p\lim\beta &= \frac{(p\theta+\omega)Var(\log(Y_{i,t-1}))+pCov(e_{it},\log(Y_{i,t-1}))}{Var(\log(Y_{i,t-1}))+Var(u_{i,t-1})} \\ &\quad \frac{p^2\sigma_v^2(p\theta+\omega+\lambda)}{\sigma_v^2 p^2((p\theta+\omega)\lambda+1)+\sigma_u^2(1-\lambda^2)(1-(p\theta+\omega)\lambda)(1-(p\theta+\omega)^2)} \end{aligned}$$

As σ_u^2 increases, the denominator increases, so the fraction decreases. Thus, the estimated persistence parameter is decreasing in measurement error, and mobility $(1-\beta)$ is increasing. If the Great Depression caused an increase in measurement error, that would bias the results towards finding more mobility in more severely shocked cities, not less.

F The Great Depression Does Not Predict Current Mobility

If local fixed factors influenced both Depression severity and economic intergenerational mobility, a relationship between the Depression and mobility might be evident in the contemporary mobility data. I find that this is not the case: mobility today is no different in the cities in my sample with the worst Depression downturns and in the cities with the mildest downturns.

Chetty et al. (2014b,a) measure local economic mobility for the recent period, using administrative tax records to generate economic mobility parameters. Their primary estimates of mobility are derived from rank-rank regressions, where the children's rank in the income distribution as an adult is regressed on the parent's rank in the income distribution. They focus on children born in the 1980 to 1982 cohorts and report results at the county level, assigning linked pairs of parents and children to the county of residence during childhood. Arguing that the rank-rank relationship is approximately linear throughout all samples, Chetty et al. (2014a) note that mobility can be fully described by two parameters: the slope of the rank-rank coefficient and the expected rank for children born at the 25th percentile.[87] Within the sample of cities in the BLS survey, Morris

[87] Given the linearity of the results, the slope and the intercept would also fully characterize mobility, but the 25th percentile measure has more rhetorical interest.

County, New Jersey (Newark) has the highest expected income rank for children born at the 25th percentile (49.5), and St. Louis has the lowest (32.2). Meanwhile, San Francisco has the weakest relationship between parents' and children's ranks (slope of 0.18), and Richmond, VA has the strongest (0.50).

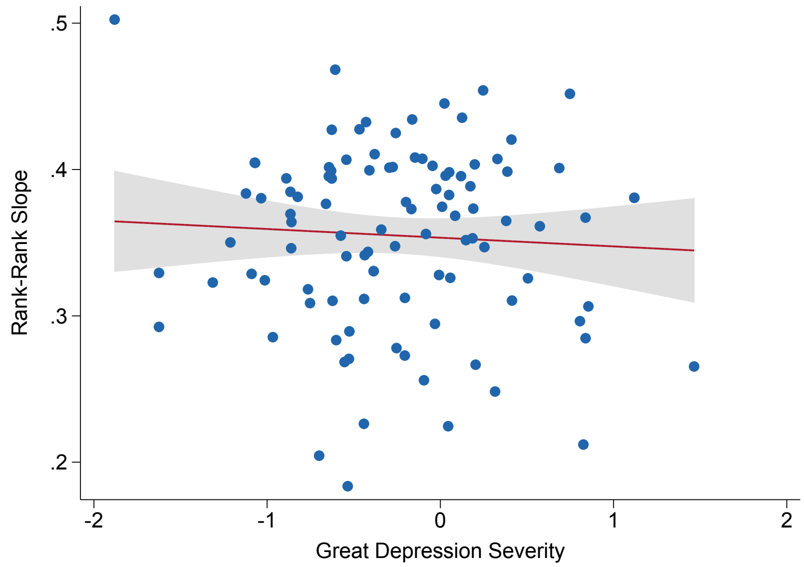
Neither of these measures of mobility today correlates with historical mobility or the Great Depression. If this were the case, again, that might prompt concern that the observed severity effect on mobility in the previous section was driven by other fixed local factors that determined mobility and severity. As I documented in Table 7 in the main text and present as well in Figure F.1 here, I show that there is no relationship between Great Depression severity and rates of economic mobility today, measured either by the slope of the rank-rank regressions or the 25th percentile expectation. I merge my county level measures of Great Depression severity to the county level measures of recent mobility.[88] These results further strengthen my claim that the cities shocked by the Great Depression do not have fundamentally different rates of mobility from cities with milder downturns.[89]

When the outcome is the rank-rank slope, the sign changes depending on the specification, and the effects are generally quite small. A positive relationship between Depression severity and the slope would imply that regions with more severe downturns have less mobility today, echoing my result that cities with more severe downturns had less mobility between 1920 and 1940. When the outcome is the expected adult rank of a child born into the 25th percentile, a positive coefficient implies a more severe Great Depression increases mobility as measured by the expected outcomes for poor children.

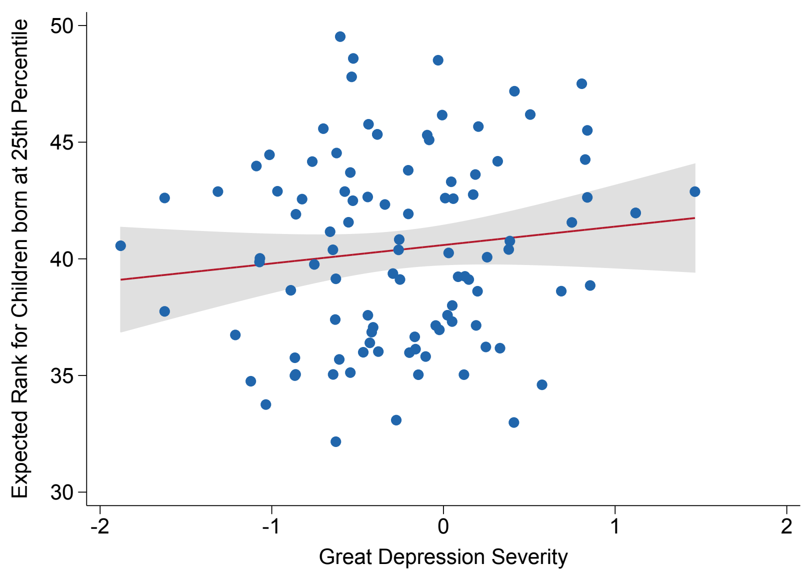
[88] Cities comprised of multiple counties like New York and St. Louis, enter the sample multiple times.

[89] The results also rule out any very long run persistence of the effect of the Great Depression on mobility many generations later. While economic outcomes are likely the result of many generations worth of family inputs (Long and Ferrie 2015), given the high levels of geographic mobility in the US during the last 80 years, these null results are not surprising and are not the ideal test of long run economic shocks on multi-generation mobility. A better test would link people today to their grandfathers or great-grandfathers during the Depression and use the location of the grandfather rather than the location of the child to measure Depression severity. However, due to privacy restrictions on census data, such multigenerational matches ending with final generations in the recent period are not yet possible.

Figure F.1: Great Depression Severity Does Not Correlate with Contemporary Measures of Mobility



(a) Rank-Rank Coefficient



(b) Expected Rank for Children born at 25th Percentile

Drawing on data from Chetty et al. (2014a), I show that there are no lasting effects today of the Great Depression on local intergenerational mobility. Chetty et al. (2014a) use administrative tax data to estimate both relative and absolute mobility between the generation born between 1980 and 1982 and their parents by county. I plot these estimates of mobility for each city included in my BLS sample against Great Depression severity, measured by the decline in per capita retail sales between 1929 and 1933. There is no clear relationship between the two measures, suggesting that places with high or low mobility today are no more or less likely to have suffered large downturns during the Great Depression.