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The Long-Run Effects of Recessions on Education and Income[†]

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This paper examines the long-run effects of the 1980–1982 recession on education and income. Using confidential census data, I estimate difference-in-difference regressions that exploit variation across counties in recession severity and across cohorts in age at the time of the recession. For individuals age 0–10 in 1979, a 10 percent decrease in earnings per capita in their county of birth reduces four-year college degree attainment by 15 percent and earnings in adulthood by 5 percent. Simple calculations suggest that in aggregate, the 1980–1982 recession led to 1.3–2.8 million fewer college graduates and $66–$139 billion less earned income per year. (*JEL* E32, I21, I26, J24, J31)

Recessions receive tremendous attention from economists, policymakers, and the public. Most of this attention focuses on the short-run effects of recessions on workers' labor market outcomes and firms' investment and employment decisions. In addition to these short-run effects, recessions could have consequential long-run effects, especially if they affect human capital attainment. Influential theoretical and empirical work emphasizes the role of opportunity cost: by reducing labor market opportunities, recessions might increase educational attainment (Mincer 1958; Becker 1962; Black, McKinnish, and Sanders 2005; Atkin 2016; Charles, Hurst, and Notowidigdo 2018; Cascio and Narayan 2019).

The opportunity cost channel is most relevant for individuals making high school and college enrollment decisions during the recession. However, recessions could affect younger individuals, for whom mechanisms besides opportunity cost, such as a decrease in human capital investments during childhood, might dominate.

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Consequently, the size and even sign of long-run effects are an empirical question. This paper estimates the long-run effects of recessions on education and income for individuals who were children, adolescents, and young adults when the recession began.

I focus on the 1980–1982 double-dip recession in the United States, which followed large increases in interest rates and the price of oil. The recession was concentrated in certain industries, like durable goods manufacturing and wood products, and counties with preexisting specialization in these industries experienced a more severe recession. The 1980–1982 recession is a valuable setting because its timing permits the study of counties' prerecession economic conditions and individuals' long-run outcomes. I show that the recession led to a persistent relative decrease in earnings per capita and median family income in negatively affected counties. This persistent decrease in local economic activity is central in considering the long-run effects on individuals.

I estimate the long-run effects of the recession using newly available confidential data. I link the 2000 census and 2001–2013 American Community Surveys (ACSs) to the Social Security Administration (SSA) Numident file, which allows me to observe outcomes in adulthood and county of birth for 23 million individuals born from 1950 to 1979. With these data, I estimate generalized difference-in-difference regressions that compare the education and income in adulthood of individuals born in counties with a more versus less severe recession (first difference) and individuals who were younger versus older when the recession began (second difference). I isolate the effect of local labor demand shifts that emerged during the recession by instrumenting for the 1979–1982 change in log real earnings per capita with the log employment change predicted by the interaction of a county's preexisting industrial structure and aggregate employment changes. This empirical strategy identifies the overall effect of the recession on long-run outcomes, inclusive of any mitigating actions by parents and communities.[1]

I find that the 1980–1982 recession led to sizable long-run reductions in education and income. For individuals age 0–10 in 1979, a 10 percent decrease in real earnings per capita in their county of birth, which is around 1 standard deviation, leads to a 4.4 percentage point (14.6 percent) decrease in four-year college degree attainment. The negative effects on college graduation are most severe and essentially constant for individuals age 0–13 in 1979. This age profile suggests that the underlying mechanisms are a decline in childhood human capital or a long-term decline in parental resources to pay for college. Because I estimate small and statistically insignificant effects on college graduation for individuals age 14–19 in 1979, short-term credit constraints in paying for college appear less important. I estimate smaller negative impacts on college attendance and find no evidence of an effect on high school graduation. For individuals age 0–10 in 1979, a 10 percent decrease in real earnings per capita leads to a $2,100 (5.2 percent) decrease in annual earned income, a $1,900 (4.5 percent) decrease in total income, and a 1.6 percentage point

[1] For example, negative effects from a decline in earnings could be offset by positive long-run effects from a reduction in pollution (Chay and Greenstone 2003; Isen, Rossin-Slater, and Walker 2017; Voorheis 2017). My sample only includes individuals born before the recession, so effects on in utero development are not operative.

(13.0 percent) increase in the probability of living in poverty from 2000 to 2013. Simple calculations suggest that much of the long-run effects on income stem from the effects on education.

An important question is whether government policies mitigated these long-run effects. To study this, I characterize several relevant dimensions of state transfer systems as of 1970. I examine transfer generosity using total transfers and state appropriations to higher education, conditional on standard economic and demographic variables. In addition, I measure transfer progressivity as the degree to which states transferred higher amounts to poorer counties, conditional on standard variables. I find little evidence that states with more generous or more progressive transfer systems mitigated the long-run effects.

Several pieces of evidence support the validity of my empirical strategy. First, I show that earnings per capita evolved similarly before 1980 in counties with a more versus less severe recession. Second, I find no evidence of a relationship between the severity of the recession and the evolution of infant mortality—a marker of early life health and human capital—from 1950 to 1979. Most importantly, I conduct falsification tests by estimating the effect of the recession on education for individuals age 23–28 in 1979, using 29-year-olds as a comparison group. I find no evidence of an effect for 23–28-year-olds, who largely completed their schooling before the recession. These findings strongly support my research design.

The magnitude of my estimates and the large number of affected individuals suggest that the 1980–1982 recession depresses aggregate economic output today. To gauge the aggregate effects, I scale my estimates by the 70 million individuals born in the United States from 1960 to 1979. Depending on the assumed evolution of earnings per capita in the absence of the recession, back-of-the-envelope calculations suggest that the 1980–1982 recession led to 1.3–2.8 million fewer four-year college graduates, $66–$139 billion less earned income per year (as of 2000–2013), and 0.4–0.9 million more adults living in poverty each year. These numbers represent 2–4 percent of the stock of college-educated adults, 0.4–0.8 percent of GDP, and 1–2 percent of the number of individuals in poverty in 2015. While these calculations could understate or overstate the true aggregate effects, as I discuss in detail, the long-run effects that I document are important channels through which recessions affect welfare and economic growth.

This paper shows that the 1980–1982 recession persistently decreased earnings per capita in negatively affected counties and that children and adolescents born in these counties have less education and income as adults. In addition, I show that every US recession since 1973 has persistently decreased earnings per capita in negatively affected counties. This novel stylized fact indicates that the 1980–1982 recession is not unique in its persistent effects on county-level earnings per capita, which suggests that it might not be unique in its long-run effects on human capital attainment. Similar long-run effects could arise from other shocks leading to persistent declines in local economic activity, such as Chinese import competition (Autor, Dorn, and Hanson 2013) and the North American Free Trade Agreement (Hakobyan and McLaren 2016).

This paper contributes to four distinct literatures. First is the literature on the effect of labor market opportunities on contemporaneous schooling enrollment, with

a focus on the role of opportunity cost (Black, McKinnish, and Sanders 2005; Atkin 2016; Charles, Hurst, and Notowidigdo 2018; Cascio and Narayan 2019). I complement this literature by estimating effects on a wider age range, including children and adolescents, which is possible because of newly available data.[2]

Second is the literature on the role of early life conditions in shaping human capital and productivity (for recent reviews, see Currie and Almond 2011; Heckman and Mosso 2014; Almond, Currie, and Duque 2018). This literature provides very little direct evidence on the long-run effects of recessions on education and income.[3] Furthermore, Chetty and Hendren (2018b) find little relationship between local economic conditions and the effect of a place on children's long-run outcomes. As a result, considerable uncertainty exists about the magnitude and sign of recessions' long-run effects. Evidence on the size of these effects is necessary for weighing the potential benefits of mitigating policies and for assessing whether these long-run effects should be incorporated into theoretical and empirical models of recessions and the economy. My primary contribution is estimating the long-run effects of a recession on education and income. I find that the long-run effects on children's and adolescents' income are more severe than the effects on individuals entering or already in the labor market at the time of the recession.[4]

This paper also contributes to work on the welfare costs of recessions. Most of this literature focuses on the costs of intertemporal substitution in consumption (see the review in Lucas 2003). In contrast, I study costs that stem from long-run declines in children's and adolescents' human capital attainment. My results imply that recessions are costlier than previously thought and point to new possible targets for social insurance and economic stabilization policies.[5] Finally, this paper contributes to the literature studying how recessions affect subsequent economic activity. An influential strand of this literature examines recessions' cleansing effects, which increase productivity as less productive firms exit and resources are reallocated to remaining firms (e.g., Schumpeter 1939, 1942; Caballero and Hammour 1994; Davis, Haltiwanger, and Schuh 1998; Foster, Grim, and Haltiwanger 2016). My results demonstrate that recessions also affect productivity in a markedly different way by reducing children's and adolescents' human capital.

[2] Work examining the short-run effects of labor market opportunities and parental job loss on children includes Rege, Telle, and Votruba (2011); Stevens and Schaller (2011); Ananat et al. (2013); Schaller and Stevens (2015); and Schaller and Zerpa (2019).

[3] Most closely related is recent work by Cutler, Huang, and Lleras-Muney (2016) and Rao (2016), who examine the long-run effects of detrended GDP and unemployment rate fluctuations that occur during childhood. These fluctuations equal zero on average, and below-trend fluctuations tend to be followed by above-trend fluctuations, so their estimates do not speak directly to the effects of a recession. Studies on the long-run effects of parental job displacement (Page, Stevens, and Lindo 2007; Bratberg, Nilsen, and Vaage 2008; Oreopoulos, Page, and Stevens 2008; Coelli 2011; Hilger 2016) also are related but do not directly assess the effects of recessions, which might operate through additional channels besides parental job loss, such as schools, neighborhoods, and peers. Similarly, studies on the effects of parental income (e.g., Løken 2010; Løken, Mogstad, and Wiswall 2012) do not directly assess the effects of recessions.

[4] Recent studies on the effect of entry labor market conditions on workers include Kahn (2010); Hershbein (2012); Oreopoulos, von Wachter, and Heisz (2012); Altonji, Kahn, and Speer (2016); and Speer (2016).

[5] This paper does not analyze the desirability of social insurance or economic stabilization policies, but such an analysis depends in part on the long-run effects documented in this paper.

I. Background: The 1980–1982 Recession

The 1980–1982 double-dip recession had sizable short-run effects on the US economy.[6] The recession followed large increases in interest rates and the price of oil, as Paul Volcker and the Federal Reserve fought inflation and the Iranian Revolution curbed oil production and created panic in energy markets. The unemployment rate was stable from 1978 to 1980, then increased from 6.3 percent in January 1980 to 10.8 percent in November 1982. As seen in Table 1, employment declines were concentrated in certain industries. The manufacturing sector lost 1.7 million jobs from 1979 to 1982, and the construction sector lost nearly 600,000 jobs. Within manufacturing, two-thirds of the job loss occurred in six industries, including transportation equipment, primary metal (which includes steel mills) and fabricated metal, lumber and wood products, and textile products. Not all industries experienced employment declines, with notable growth in the mining sector (which includes oil and gas extraction) and the service sector.[7]

To measure the effect of the recession on local economic activity, I use Bureau of Economic Analysis (BEA) data on earnings per capita. Total earnings of a county's residents, available starting in 1969, primarily comes from administrative unemployment insurance and tax data. The earnings concept is comprehensive, including income from the labor market and asset ownership, and the denominator of earnings per capita comes from census annual population estimates. Throughout, I use the Consumer Price Index for All Urban Consumers to express all monetary variables in 2014 dollars. For each county, I measure the severity of the recession as the 1979–1982 decrease in log real earnings per capita. This variable captures several ways a recession might affect a county's residents, such as extensive margin employment changes, replacement of full-time jobs with part-time jobs, replacement of high-wage jobs with low-wage jobs, and decreasing wages or hours within a job.

Figure 1 shows that the severity of the recession varied considerably across counties. Categories on the map correspond to deciles, with darker shades of red indicating a more severe recession. Twenty percent of counties experienced a decline in earnings per capita of 13.4 percent or more, while 20 percent saw earnings growth.[8] The map displays regional patterns in recession severity that mirror patterns in industrial specialization. Oil-exporting states, like Kansas, Oklahoma, and Texas, benefited from high oil prices, and states specializing in durable goods manufacturing, like Indiana, Michigan, and Ohio, saw particularly large declines. New England, with more high-tech and defense manufacturing, fared relatively well, while the Pacific Northwest, which specialized in logging, fared poorly. Parts of the agricultural Upper Midwest also fared poorly, in conjunction with the "farm crisis" (Barnett 2000).

[6] The NBER recession dates are January to July 1980 and July 1981 to November 1982. I treat these as a single episode, using 1979 as the prerecession year.

[7] Table 1 is based on annual census County Business Patterns (CBP) data. While total employment in these data grew by 467,000 jobs during this period, monthly data from the Bureau of Labor Statistics Current Employment Statistics show large employment decreases in the first half of 1980 and from 1981 to 1982.

[8] The unweighted average decrease in log earnings per capita is 6.4 percent, and the standard deviation is 10.6 percent. Using 1979 population weights, the average decrease is 4.8 percent, and the standard deviation is 6.5 percent.

TABLE 1—AGGREGATE EMPLOYMENT CHANGES FROM 1979–1982, BY INDUSTRY

	Share of total 1979 employment (1)	Log employment change (2)	Employment change (3)
Panel A. Overall and one-digit industries			
All industries	1.000	0.006	467,093
Manufacturing	0.284	−0.084	−1,684,627
Construction	0.061	−0.139	−575,107
Agriculture, forestry, and fisheries	0.004	0.131	40,200
Transportation and public utilities	0.062	0.013	59,944
Wholesale trade	0.071	0.016	83,149
Mining	0.013	0.229	239,923
Retail trade	0.203	0.017	257,134
Finance, insurance, and real estate	0.070	0.060	316,449
Services	0.224	0.120	2,090,186
Panel B. Two-digit industries with largest employment decrease			
Transportation equipment (manufacturing)	0.024	−0.180	−279,883
General contractors (construction)	0.018	−0.223	−250,039
Special trade contractors (construction)	0.034	−0.108	−242,341
Auto dealers (retail trade)	0.027	−0.131	−232,032
Primary metal (manufacturing)	0.016	−0.222	−229,097
Fabricated metal products (manufacturing)	0.024	−0.117	−188,465
Lumber and wood products (manufacturing)	0.011	−0.276	−181,995
Trucking and warehousing (transportation)	0.019	−0.116	−146,868
Apparel and other textile products (manufacturing)	0.019	−0.106	−129,917
Textile mill products (manufacturing)	0.012	−0.161	−126,208
Panel C. Two-digit industries with largest employment increase			
Legal services (services)	0.007	0.200	109,155
Educational services (services)	0.016	0.095	114,630
Hotels (services)	0.014	0.110	116,017
Depository institutions (finance)	0.020	0.093	140,356
Food stores (retail trade)	0.031	0.087	196,198
Miscellaneous services (services)	0.012	0.251	242,976
Oil and gas extraction (mining)	0.005	0.506	249,725
Eating and drinking places (retail trade)	0.060	0.065	283,524
Business services (services)	0.041	0.126	382,400
Health services (services)	0.071	0.157	845,784

Notes: I construct this table by aggregating county-level data for the continental United States. Because employment is often suppressed at the county level, I impute employment using the number of establishments and nationwide information on average employment by establishment size, as described in Appendix C.

Source: Census CBP

Although the regional patterns are visually striking, 96 percent of the variation in the severity of the recession is within region, and 81 percent is within state.

Earnings per capita declined suddenly in counties where the recession was more severe, and the relative earnings decline between more- versus less-severe-recession counties has persisted. To show this simply, Figure 2 plots population-weighted mean earnings per capita for counties with a recession more and less severe than the nationwide (1979 population-weighted) median. I shift the less-severe-recession line down by $2,179, so that the 2 lines are equal in 1979, to focus on the evolution of earnings per capita over time. Mean earnings per capita evolve identically in more- versus less-severe-recession counties from 1969 to 1979, then diverge with the onset of the recession. From 1979 to 1982, mean earnings per capita fall by $2,288 (8.9 percent) in more-severe-recession counties while increasing by

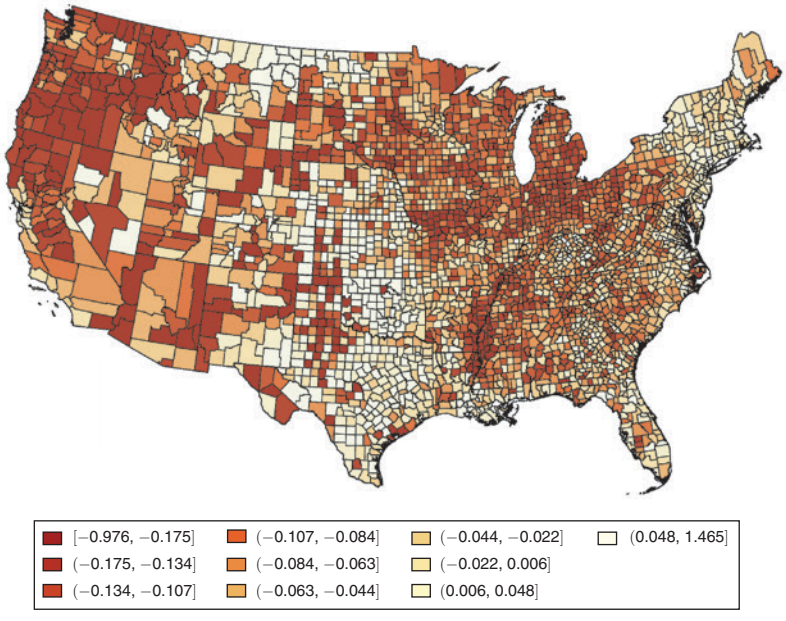

Figure 1. Change in Log Real Earnings per Capita, 1979–1982

Notes: Figure displays the county-level change in log real earnings per capita from 1979 to 1982, which I use to measure the severity of the 1980–1982 recession. Categories correspond to unweighted deciles, with darker shades of red representing a larger earnings decrease.

Source: BEA regional economic accounts

$76 (0.3 percent) in less-severe-recession counties. After 1982, mean earnings per capita evolve similarly in both sets of counties, including during later recessions, leaving severe recession counties with a persistent relative decline.[9] In the conclusion, I establish the novel stylized fact that every recession in the United States since 1973 generates a pattern similar to Figure 2. The employment-population ratio displays a similar pattern (online Appendix Figure A.1).

The persistence of the relative decline in Figure 2 might seem surprising given the conventional wisdom, based largely on Blanchard and Katz (1992), that local wages and employment rates converge after negative labor demand shocks. However, several studies find lasting wage and employment rate reductions (Bartik 1991, 1993; Bound and Holzer 2000; Greenstone and Looney 2010; Autor, Dorn, and Hanson 2013; Dix-Carneiro and Kovak 2017; Yagan 2019), and economic forces can rationalize this finding. For example, a recession could catalyze a lasting reduction in economic activity by inducing employers to pay fixed adjustment costs and shut down or move to other areas (Foote 1998).[10] Greater out-migration rates or lower in-migration rates of high-income workers following a decrease in local labor demand also could contribute to a persistent decrease in earnings per capita

[9] Greenstone and Looney (2010) document a similar finding for the 1980–1982 recession.

[10] Hershbein and Kahn (2018) and Jaimovich and Siu (2020) discuss factors that could lead to persistent declines in local economic activity, although they focus on more recent recessions.

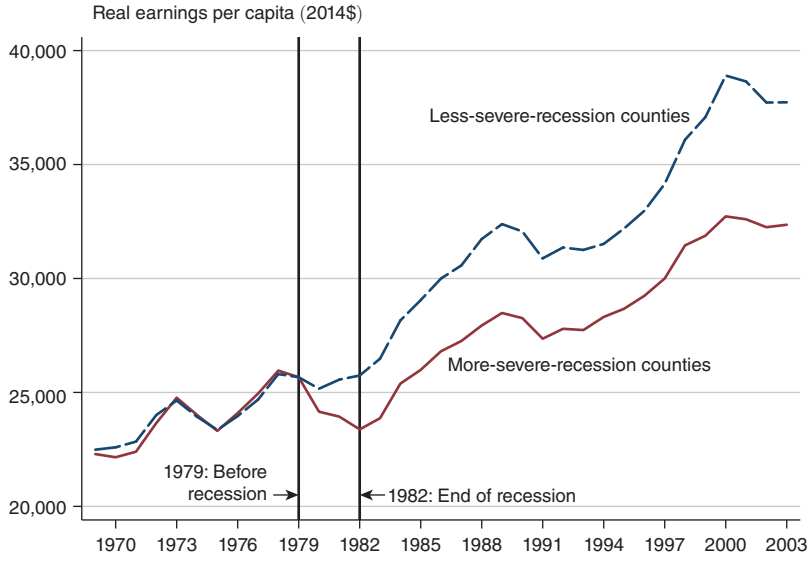

FIGURE 2. NORMALIZED MEAN REAL EARNINGS PER CAPITA, BY COUNTY-LEVEL SEVERITY OF THE 1980–1982 RECESSION

Notes: Figure displays population-weighted mean real earnings per capita among counties with a below and above median 1979–1982 decrease in log real earnings per capita. I calculate the median using 1979 population weights. I adjust the less severe recession line to equal the more severe recession line in 1979, which amounts to a downward shift of $2,179. Sample contains 3,076 counties in the continental United States.

Source: BEA regional economic accounts

(Topel 1986, Bound and Holzer 2000, Notowidigdo 2020). Quantifying the sources of the persistent relative decline is beyond the scope of this paper. Instead, I focus on the long-run effects of the recession on children and adolescents.

II. Possible Long-Run Effects of a Recession on Education and Income

This section draws on previous theoretical and empirical work to describe the possible long-run effects of a recession on education and income. Economic theory does not provide a sharp prediction about the magnitude or even sign of long-run effects, but it does highlight potential channels.

A recession could affect educational attainment and lifetime income by increasing or decreasing human capital obtained during childhood. The stock of childhood human capital depends on material and time investments from parents; community investments from schools, neighborhoods, and peers; and an initial human capital endowment (Currie and Almond 2011, Heckman and Mosso 2014). A recession-induced decrease in the local wage could produce income and substitution effects. The income effect predicts a decrease in parents' material investments.[11]

[11] An income effect also could arise from a decline in house prices and parental wealth. Some studies find that children's long-run outcomes are sensitive to changes in family resources (Aizer et al. 2016; Hoynes, Schanzenbach, and Almond 2016), while others do not (Jacob, Kapustin, and Ludwig 2015; Bleakley and Ferrie 2016). Bulman et al. (2021) find that college attendance is sensitive only to very large increases in income.

The substitution effect, due to a decrease in the cost of spending time with children, predicts an increase in parents' time investments.[12] Community investments could fall due to a reduction in government expenditures or the quality of schools, neighborhoods, or peers.[13] I focus on individuals born before 1980, for whom the recession does not affect initial human capital endowments.

A recession could influence high school or college degree attainment independently of any effects on childhood human capital. In choosing their desired level of schooling, individuals trade off higher lifetime earnings against the opportunity cost of forgone earnings and the cost of tuition (Mincer 1958, Becker 1962, Ben-Porath 1967). A recession-induced decrease in the earnings of less educated workers could reduce the opportunity cost, leading to long-run increases in education and income.[14] However, in the presence of credit constraints, a recession might decrease parents' ability to pay for college, leading to long-run decreases in education and income.[15]

This conceptual framework informs the unit of geography that I use to measure recession exposure in the empirical analysis. A recession's long-run effects could arise from mediating effects on parents, schools, neighborhoods, peers, and the local labor market. Counties, which are the most detailed unit of geography in my data, do not map exactly to school districts, neighborhoods, or peer groups, but they resemble these sources of local community investments more closely than broader geographic definitions. Moreover, BEA data report earnings by county of residence, so they reflect commuting patterns throughout the local labor market and could be especially relevant for perceived labor market opportunities.

III. Data and Empirical Strategy

A. *Data on Long-Run Outcomes and County of Birth*

To estimate the long-run effects of the 1980–1982 recession, I use newly available confidential data consisting of the 2000 census and 2001–2013 ACSs linked to the SSA Numident file. The linked data contain outcomes in adulthood (measured from 2000–2013) and county of birth. My sample consists of individuals born in the continental United States from 1950 to 1979 who were 25–64 years old at the

[12] Aguiar, Hurst, and Karabarbounis (2013) find that parents spent more time with children during the Great Recession, and Del Boca, Flinn, and Wiswall (2014) find that parental time produces cognitive skills in children. Additional time might have limited benefits, or even be harmful, if the recession increases parental stress (McLoyd et al. 1994; Leininger and Kalil 2014; Brand 2015; Akee et al. 2018).

[13] Existing work documents long-run effects of both disruptive peers (Carrell, Hoekstra, and Kuka 2018) and neighborhoods (Chetty, Hendren, and Katz 2016; Chetty and Hendren 2018a; Chyn 2018).

[14] Some empirical work finds an important role for opportunity cost in educational attainment (Black, McKinnish, and Sanders 2005; Atkin 2016; Charles, Hurst, and Notowidigdo 2018; Cascio and Narayan 2019). Bound and Holzer (2000) and Hoynes, Miller, and Schaller (2012) show that the 1980–1982 recession especially reduced the wages and employment of less educated workers.

[15] Several studies conclude that short-term credit constraints are relatively unimportant for college attendance or graduation (Cameron and Heckman 2001, Cameron and Taber 2004, Stinebrickner and Stinebrickner 2008, Bulman et al. 2021), but some evidence from college decisions made in the early 2000s suggests a larger role for credit constraints (Belley and Lochner 2007, Bailey and Dynarski 2011, Lovenheim 2011). Charles, Hurst, and Notowidigdo (2018) find that the early 2000s housing boom decreased college enrollment, consistent with opportunity cost being more important than parental resources in their setting.

time of the survey. I exclude individuals living in group quarters, who are not in the 2001–2005 ACS data, and individuals with allocated age, gender, race, or state of birth. I also exclude individuals with allocated dependent variables, leading to three nested samples. My first sample contains 23.5 million individuals with nonallocated years of education. My second sample contains 18.4 million individuals that also have nonallocated labor market outcome variables, and my third sample contains 15.6 million individuals that also have positive personal income, earned income, family income, and hourly wages.[16] I limit these samples to the 89 percent of individuals with a unique Protected Identification Key (PIK), which is an anonymized identifier, and unique birth county.[17]

B. *Generalized Difference-in-Difference Specification*

I estimate the recession's long-run effects with a generalized difference-in-difference specification that compares the education and income in adulthood of individuals born in counties with a more versus less severe recession (first difference) and individuals who were younger versus older when the recession began (second difference). In particular, consider the individual-level regression

$$(1) \qquad y_{i,a,c,t} = R_c^{79\text{–}82}\pi_a + x_{i,a,c,t}\beta + \gamma_c + \theta_{a,s(c)} + \delta_t + \varepsilon_{i,a,c,t},$$

where $y_{i,a,c,t}$ is a measure of educational attainment or income in adulthood of individual i, who was age a in 1979, born in county c, and observed in survey year t. The explanatory variable of interest is $R_c^{79\text{–}82}$, which measures recession severity as the decrease in log real earnings per capita from 1979 to 1982 in county c. The vector $x_{i,a,c,t}$ includes gender-by-age and race indicators plus interactions between indicators for age in 1979 and several covariates measured in individuals' birth county: the 1950–1960, 1960–1970, and 1970–1980 change in log real median family income and log population and the 1960 level of log population, log population density, percent urban, percent Black, percent foreign, percent with a high school degree, and percent of families with income below $3,000. These variables flexibly control for the fact that counties with a more severe recession saw greater income growth from 1950 to 1970 (see online Appendix B) and a variety of other

[16] These samples balance the goals of using as much information as possible given nontrivial allocation rates for labor market outcomes and limiting the number of samples to ensure that no confidential information is disclosed. In publicly available 2000–2013 census/ACS data, for people born in the United States from 1950 to 1979 who are age 25–64 at the time of the survey, 8.1 percent of individuals have allocated age, gender, race, or state of birth. Among individuals with no allocations in these variables, 1.8 percent are in group quarters. A further 1.1 percent of individuals have allocated years of education, and 21.3 percent have allocated education or labor market variables (any of the 7 individual income variables, total family income, weeks worked, hours worked, marital status, and labor force status).

[17] The Census Bureau assigns PIKs to individuals in the census and ACS using information on respondents' name, address, date of birth, and gender. Sometimes a PIK is assigned to more than one respondent in a survey year. While technically possible (e.g., if an individual receives a survey at multiple residences), this outcome likely reflects an error in PIK assignment. An individual may be assigned to multiple birth counties if the 12-character string from the Numident does not identify a single county. For example, there are two towns named Arcadia in North Carolina, and a respondent who writes "Arcadia" could be matched to two counties. Online Appendix A describes the algorithm that identifies county of birth using the 12-character place-of-birth string in the Numident.

possible determinants of long-run outcomes. Birth county fixed effects, γ_c, absorb cross-county differences in initial human capital endowments plus fixed characteristics of parents and communities. Age-in-1979-by-birth-state fixed effects, $\theta_{a,s(c)}$, control flexibly for changes over time in state-level higher education access, transfer programs, and other factors.[18] Survey year fixed effects are given by δ_t.

The parameter of interest, π_a, measures the effect of the recession on individuals who were age a in 1979. This parameter reflects the persistent relative decline in local economic activity in counties where the recession was more severe. I allow π_a to vary flexibly with age in 1979 because the operative mechanisms and sensitivity to the recession might vary with age.

Earnings per capita might have decreased from 1979 to 1982 in a county because of a recession-induced decrease in labor demand or an unrelated change in the composition of a county's residents, such as an increase in the relative number of low-income workers. To isolate the role of the recession, I construct an instrumental variable that predicts the 1979–1982 log employment change using a county's 1976 industrial structure and aggregate employment changes,

$$D_c^{79\text{–}82} = \sum_j \eta_{c,j,1976}\left(e_{-s(c),j,1982} - e_{-s(c),j,1979}\right). \tag{2}$$

In equation (2), $\eta_{c,j,1976}$ is the share of county c's employment in two-digit industry j in 1976, and $\left(e_{-s(c),j,1982} - e_{-s(c),j,1979}\right)$ is the log employment change from 1979 to 1982 for industry j in all states in the same region besides the state of county c.[19] This instrumental variable strategy is common in studies of local labor markets (e.g., Bartik 1991, Blanchard and Katz 1992, Bound and Holzer 2000, Diamond 2016, Notowidigdo 2020). It exploits the fact that the recession was more severe in counties that specialized in industries, like durable goods manufacturing or wood products, that were more sensitive to fluctuations in interest rates, oil prices, and the business cycle. I estimate equation (1) with two-stage least squares (2SLS), where the predicted log employment change, $D_c^{79\text{–}82}$, is an instrument for the decrease in log earnings per capita, $R_c^{79\text{–}82}$.

To reduce computational burden, I collapse census and ACS individual-level data into cells defined by age in 1979, birth county, survey year, race, and gender, and I estimate grouped regressions with weights equal to the number of observations in each cell. This grouped regression produces point estimates that are identical to

[18] Recessions could affect higher education access and transfer program expenditures, but there are many other determinants of these programs. Given the conceptual and practical challenges of controlling for changes due to factors besides the recession, I prefer to include age in 1979-by-birth state fixed effects.

[19] I use the predicted log employment change because earnings data are not available at a sufficiently detailed industry level. I exclude the contribution from a county's own state to remove a mechanical relationship between the actual and predicted change in economic activity. Using other states in the same region, as opposed to all other states, allows industry-level employment changes to differ by region. I construct $D_c^{79\text{–}82}$ using Census CBP data. CBP data frequently suppress employment for county-by-industry cells to protect respondent confidentiality but never suppress the number of establishments within establishment size categories. I impute CBP employment using the number of establishments and nationwide information on employment by establishment size, as described in online Appendix C.

those from an individual-level regression. I cluster standard errors by birth state to allow for arbitrary serial and within-state spatial correlation.

The key assumption underlying this empirical strategy is that cross-cohort differences in long-run outcomes between individuals born in counties where the 1980–1982 recession was more or less severe are driven by the recession as opposed to some confounding variable. For example, this assumption would be violated if there were a relative decline in infant health from 1950 to 1979 in counties with a larger predicted log employment decrease from 1979 to 1982. My baseline specification includes several control variables in $x_{i,a,c,t}$ to limit the possible influence of confounding variation, and Section IVC discusses additional results that establish the robustness of my findings.

Importantly, the empirical strategy also offers a falsification test. To facilitate the inclusion of birth county fixed effects, I normalize the parameter $\pi_{29} = 0$. As a result, the identified parameters are the effects on individuals age a minus the effect on 29-year-olds, $\pi_a - \pi_{29}$. For education outcomes, 29-year-olds provide a useful comparison group because they largely completed their schooling before the recession. Individuals between the ages of 23 and 28 also largely completed their schooling before the recession, which yields a falsification test of whether $\pi_a = 0$ for $a = 23, \ldots, 28$.[20] The results of these falsification tests support my key identifying assumption.

There are two main concerns with the resulting estimates. First, using 29-year-olds as a comparison group could bias the estimated effects on income upward if 29-year-olds experience a long-run decrease in income (i.e., $\pi_{29} < 0$), possibly because of job loss (Jacobson, LaLonde, and Sullivan 1993) or a decline in local job quality (Hagedorn and Manovskii 2013, Haltiwanger et al. 2018). This suggests that any negative estimated effects on income are too conservative, which should be kept in mind.[21] Second, county of birth is an imperfect proxy for individuals' county of residence in 1979, which is the ideal variable for measuring recession exposure. In online Appendix D, I show that this measurement error attenuates estimates of π_a and describe how to adjust for it using the relationship between recession severity in individuals' county of residence and county of birth. I estimate an auxiliary measurement error model using individuals born from 1970 to 2013 who I observe in 2000–2013. Using data from the Panel Study of Income Dynamics, I also show that migration patterns during childhood have been remarkably stable over time, which supports the validity of this approach. Adjusting for measurement error has larger proportional effects on older cohorts, for whom county of residence is less closely related to county of birth, but the qualitative patterns are similar. I calculate standard errors of the adjusted estimates using the delta method.

[20] For individuals born from 1957 to 1964, about 75 percent of four-year college degree attainment is completed by age 25, and 85 percent is completed by age 29 (online Appendix Figure A.3).

[21] The potential impacts of the recession on older individuals also means that there is no falsification test for income.

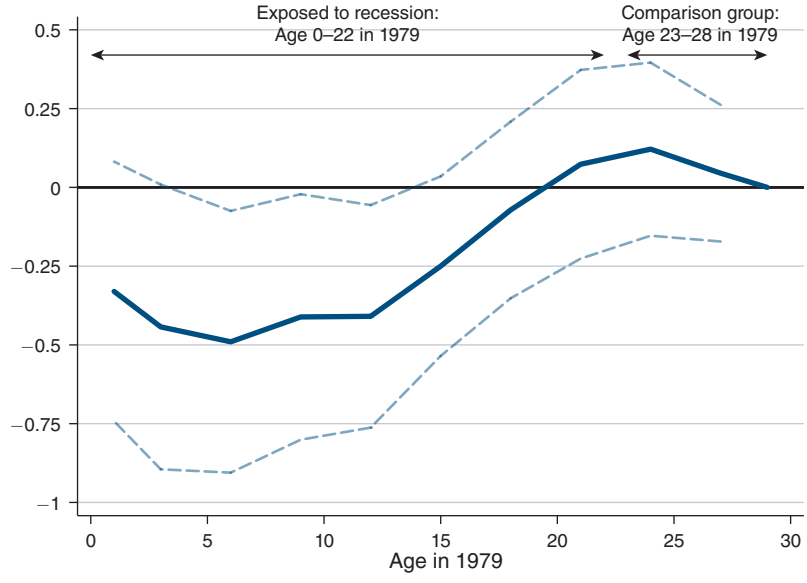

FIGURE 3. THE LONG-RUN EFFECTS OF THE 1980–1982 RECESSION ON FOUR-YEAR COLLEGE DEGREE ATTAINMENT

Notes: Figure plots estimates of the interaction between the 1979–1982 decrease in log real earnings per capita in individuals' county of birth and indicators for age in 1979. The interaction for individuals age 29 is normalized to equal 0. The dependent variable is an indicator for four-year college degree attainment. See notes to Table 2 for details on sample and specification. To increase precision, I combine ages 0–1, 2–4, 5–7, 8–10, 11–13, 14–16, 17–19, 20–22, 23–25, and 26–28. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by state.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file

IV. Results: The Long-Run Effects of the 1980–1982 Recession

This section presents my main results. I first show that the recession led to sizable long-run reductions in education and income. I then discuss evidence supporting the validity of my empirical strategy and the robustness of my results. Finally, I examine mechanisms and policies that might have mitigated the recession's long-run effects.

A. *Long-Run Effects on Education*

Figure 3 shows that the 1980–1982 recession reduced children's four-year college degree attainment. The figure displays 2SLS estimates of equation (1), where I use three-year age bins to estimate π_a more precisely.[22] Estimates for individuals who were 23–28 years old in 1979 are small and indistinguishable from 0, indicating that the severity of the recession is not related to trends in college degree attainment for this group. Because college degree attainment is largely completed by age 23, this

[22] The bins are for 1979 ages 0–1, 2–4, 5–7, 8–10, 11–13, 14–16, 17–19, 20–22, 23–25, and 26–28.

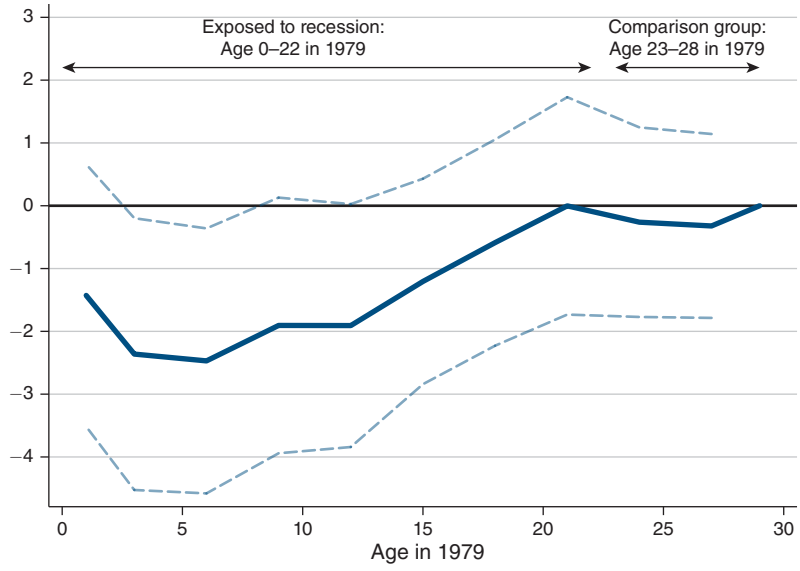

FIGURE 4. THE LONG-RUN EFFECTS OF THE 1980–1982 RECESSION ON YEARS OF SCHOOLING

Notes: Figure plots estimates of the interaction between the 1979–1982 decrease in log real earnings per capita in individuals' county of birth and indicators for age in 1979. The interaction for individuals age 29 is normalized to equal 0. The dependent variable is years of schooling. See notes to Table 2 and Figure 3 for details on sample and specification.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file

finding supports the validity of my empirical strategy. Negative effects emerge for individuals who were younger when the recession began, with these effects being most severe and nearly constant for individuals age 0–13 in 1979. For this group, the point estimates imply that a 10 percent decrease in earnings per capita from 1979 to 1982, approximately equal to 1 standard deviation, reduces four-year college degree attainment by about 4 percentage points, or around 13 percent of mean attainment.

Figure 4 shows analogous impacts on total years of education. Estimates for individuals age 23–28 in 1979 are again small and indistinguishable from 0, providing further support for the empirical strategy. The coefficients follow a similar pattern as those in Figure 3 and are almost five times as large, providing initial evidence that most of the effects on years of education come from four-year degree attainment.[23]

Table 2 reports estimates of long-run effects on several measures of educational attainment, grouping together individuals ages 0–10 and 11–19 in 1979 while normalizing the age 20–29 coefficient to equal 0.[24] There is no evidence of an effect on high school diploma or General Educational Development (GED) certificate attainment. The point estimates for college attendance are negative but indistinguishable

[23] Online Appendix Figures A.8–A.11 present results for other measures of educational attainment.

[24] Online Appendix E discusses the first-stage, reduced-form, and OLS estimates. First-stage *F*-statistics range from 43 to 54.

Table 2—The Long-Run Effects of the 1980–1982 Recession on Education

	Dependent variable:					
	HS/GED attainment (1)	Any college attendance (2)	Any college degree attainment (3)	Four-year college degree attainment (4)	Two-year college degree attainment (5)	Years of schooling (6)
Panel A. Interaction between 1979–1982 decrease in log real earnings per capita and age in 1979						
0–10	−0.020	−0.269	−0.444	−0.470	0.025	−1.967
	(0.047)	(0.158)	(0.174)	(0.179)	(0.059)	(0.868)
11–19	0.055	−0.141	−0.297	−0.286	−0.010	−1.065
	(0.046)	(0.122)	(0.107)	(0.107)	(0.043)	(0.573)
Panel B. Average value of dependent variable in years 2000–2013, by age in 1979						
0–10	0.936	0.588	0.414	0.321	0.093	13.57
11–19	0.932	0.537	0.380	0.288	0.093	13.39

Notes: Panel A reports estimates of the interaction between the 1979–1982 decrease in log real earnings per capita in individuals' birth county and indicators for age in 1979. The interaction for individuals age 20–29 is normalized to equal 0. Regressions include indicators for gender by age at time of survey, race, birth county, age-in-1979-by-birth-state, and survey year, plus indicators for age in 1979 interacted with several covariates measured in individuals' birth county: the 1950–1960, 1960–1970, and 1970–1980 changes in log real median family income and log population and the 1960 level of log population, log population density, percent urban, percent Black, percent foreign, percent with a high school degree, and percent of families with income below $3,000. Regressions are estimated by 2SLS using the predicted log employment change from 1979 to 1982 as an instrumental variable. Standard errors in parentheses are clustered by birth state. The sample in panel A contains 23.5 million individuals born in the continental United States from 1950 to 1979 with a unique birth county, unique PIK, and nonallocated variables. Panel B reports average values of the dependent variables for a comparable sample from publicly available 2000 census and 2001–2013 ACS data.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file, publicly available 2000–2013 census/ACS data from Ruggles et al. (2015)

from zero at conventional levels. Column 3 shows sizable and statistically significant negative effects on any college degree attainment. Columns 4 and 5 separate college degree attainment into two mutually exclusive categories: four- and two-year degrees.[25] There is evidence of a decrease in four-year degree attainment, as shown in Figure 3, and no effect on two-year degree attainment. The estimates in column 6 on total years of schooling are negative; as shown in columns 1–5, these are driven by a reduction in college attendance and four-year degree attainment. For individuals age 0–10 in 1979, a 10 percent decrease in earnings per capita from 1979 to 1982 leads to a 2.7 percentage point (4.6 percent) decrease in college attendance, a 4.4 percentage point (10.7 percent) decrease in any college degree attainment, and a 4.7 percentage point (14.6 percent) decrease in four-year degree attainment.

These results imply that in the absence of the recession, individuals born in counties where the recession was more severe would have been more likely to attend college and obtain a four-year degree. The larger effect on college degree attainment than on attendance suggests that part of the decline in degree attainment stems from individuals not completing college because of the recession. A negative impact on college persistence could arise from a reduction in college readiness or the quality

[25] Census/ACS data measure the highest degree completed, so an individual with a two- and four-year degree is recorded as having a four-year degree.

of colleges at which individuals enroll (e.g., Bound, Lovenheim, and Turner 2010). More generally, unobserved substitution patterns underlie these results. For example, individuals who would have obtained a four-year degree in the absence of the recession might have instead received a two-year degree, attended college without obtaining a degree, or not attended college. As another example, the lack of a net effect on two-year degree attainment could arise from equal-sized shifts of individuals from four-year degrees to two-year degrees (increasing two-year degree attainment) and from two-year degrees to no degree (decreasing two-year degree attainment). Economic theory and prior empirical work do not place sharp bounds on the possible adjustment margins.

Previously studied policies provide a useful comparison for understanding the size of these effects. A 10 percent decrease in earnings per capita from 1979 to 1982 for individuals age 0–10 in 1979 has an effect on any college degree attainment almost 3 times as large as the Tennessee Student/Teacher Achievement Ratio (STAR) experiment, which randomly reduced kindergarten to third-grade class sizes by roughly 30 percent and increased college degree attainment by 1.6 percentage points (Dynarski, Hyman, and Schanzenbach 2013). The effect of the recession on four-year college degree attainment is also larger than the STAR experiment, which increased four-year degree attainment by 0.9 percentage points (Dynarski, Hyman, and Schanzenbach 2013), and the effect of statewide scholarship programs that fully covered tuition and fees for qualified students, which increased four-year degree attainment by 2.5 percentage points (Dynarski 2008).

Another way to quantify these impacts is by comparing them to the cross-sectional relationship between recession severity and long-run outcomes. Online Appendix Table A.6 shows "first difference" estimates from a specification that does not include county fixed effects and so does not difference by age. The difference-in-difference estimates are smaller in magnitude than the first difference estimates. For four-year degree attainment, the estimated effects in Table 2 for individuals age zero to ten in 1979 are about half as large as the first difference estimates. The results also display the value of using cross-cohort variation, which facilitates the inclusion of birth county fixed effects; a purely cross-sectional research design would overstate the negative impacts of the recession on long-run outcomes.

Online Appendix Table A.7 presents results separately by gender and race. The impacts on educational attainment are largely similar for men and women, although there is evidence that the decline in college attendance was larger for men. The long-run effects on men and women reflect differences in the opportunity cost of education, contemporaneous labor market opportunities, and the responsiveness of boys versus girls to early life shocks (e.g., Autor et al. 2019). In terms of race, the negative impacts are concentrated among Whites. The point estimates on educational attainment are positive for non-Whites but generally insignificant. One possible explanation is that White families experienced a greater decline in wealth because they were more likely to own homes and house values declined after the recession. Another possible explanation is the opportunity cost channel, as non-White workers experienced greater reductions in employment and wages during the recession (Bound and Holzer 2000; Hoynes, Miller, and Schaller 2012), and this might have persisted.

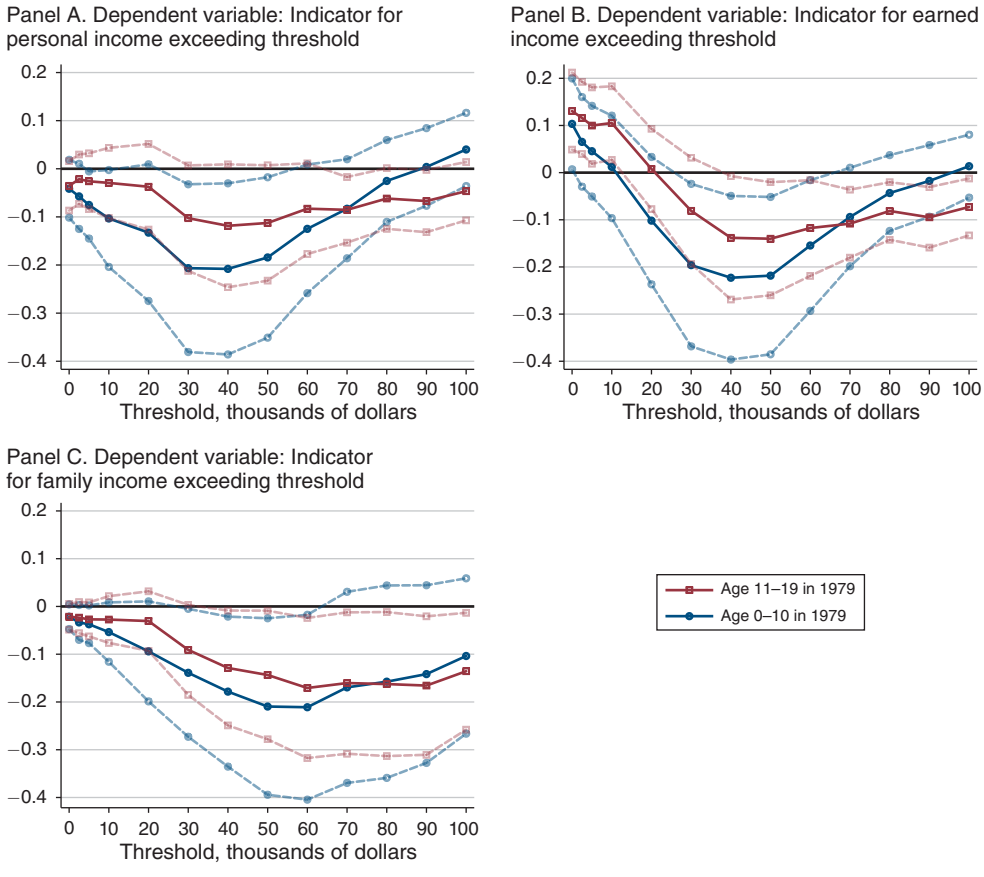

FIGURE 5. THE LONG-RUN EFFECTS OF THE 1980–1982 RECESSION ON INCOME THRESHOLDS

Notes: The figure reports estimates of the interaction between the 1979–1982 decrease in log real earnings per capita in individuals' birth county and indicators for age in 1979. The interaction for individuals age 20–29 is normalized to equal 0. The dependent variables are indicators for income exceeding thresholds \$0, \$2,500, \$5,000, \$10,000, \$20,000, . . ., \$100,000. See notes to Table 3 for details on sample and specification.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file

B. *Long-Run Effects on Income, Wages, and Poverty*

Figure 5 shows the estimated effects of the recession on indicators for income exceeding several thresholds: \$0, \$2,500, \$5,000, \$10,000, \$20,000, . . ., \$100,000. These dependent variables (measured from 2000 to 2013) yield evidence on impacts throughout the income distribution. Panel A shows results for total personal income. There is little evidence that the recession affected the extensive margin, which is not surprising given the wide range of income sources included in this measure. Instead, the effects of the recession are most severe in the middle of the income distribution.[26]

[26] Online Appendix Figure A.12 reports the share of individuals with income exceeding each threshold.

For example, a 10 percent decrease in earnings per capita during the recession reduces the probability of earning at least $30,000 by 2 percentage points for individuals who were age 0–10 in 1979 and 1 percentage point for those age 11–19. The point estimates are more negative for the younger group than the older group at all income thresholds up to $70,000. Moving further up the income distribution, there is little effect on whether the younger group earns $80,000 or more, but there is a negative effect for the older group. A natural explanation for this pattern is that (i) the larger reduction in income for individuals age zero to ten in 1979 reflects the larger reduction in education and (ii) life cycle income growth causes more of the older group to be "at risk" of a recession-induced decrease in income in the upper part of the distribution.

Panel B presents results for earned income. The pattern is similar, except the estimates are positive at low levels of income. The extensive margin effects are relatively small: a 10 percent decrease in earnings per capita leads to a 1 percentage point increase in the probability of having positive earned income for individuals who were age 0–10 in 1979. To interpret these results, recall that the identified effects are relative to those on individuals who were 20–29 years old in 1979, i.e., $(\pi_{0–10} - \pi_{20–29})$ and $(\pi_{11–19} - \pi_{20–29})$. If the recession led to a long-run earnings reduction of older individuals (so that $\pi_{20–29} < 0$, consistent with the results in online Appendix Figures A.13–A.17), then the estimated effects for younger individuals are biased upward. The similarity of the upward shift for individuals age 0–10 and 11–19 in 1979 in panel B suggests that the positive coefficients reflect a *decrease* in the probability that individuals who were age 20–29 in 1979 have positive or low levels of earnings. Because the recession led to lasting decreases in local economic activity (online Appendix B), this could arise as older individuals drop out of the labor force. Many individuals with zero earnings have positive total income (including from transfers), which explains why this pattern does not appear in panel A. This interpretation accords with summary statistics in online Appendix Figure A.12, which show that individuals who were 20–29 years old in 1979 are less likely to have positive earnings from 2000 to 2013 and equally likely to have positive personal and family income.

The results for family income in panel C resemble those for personal income in panel A. The effects extend further up the income distribution, which is not surprising, because family income exceeds personal income. The results for individuals who were ages 0–10 and 11–19 in 1979 are more similar, reflecting the additional heterogeneity that arises when considering families.

The estimates in Figure 5 show that the recession reduced the probability that individuals earned middle-class incomes. The ability to see these distributional effects comes at the cost of reduced precision and conciseness. To summarize the effects of the recession, I use log income and earnings as dependent variables going forward.

Table 3 shows that the recession led to long-run decreases in income and wages and increases in poverty, where these outcomes are measured from 2000 to 2013. For individuals age 0–10 in 1979, the estimates imply that a 10 percent decrease in earnings per capita from 1979 to 1982 reduces personal income by 4.5 percent ($1,900), earned income by 5.2 percent ($2,100), and hourly wages

Table 3—The Long-Run Effects of the 1980–1982 Recession on Income, Wages, and Poverty

	Dependent variable:				
	Log personal income (1)	Log earned income (2)	Log hourly wage (3)	Log family income (4)	In poverty (5)
Panel A. Interaction between 1979–1982 decrease in log real earnings per capita and age in 1979					
0–10	−0.447	−0.519	−0.303	−0.419	0.158
	(0.158)	(0.166)	(0.125)	(0.173)	(0.062)
11–19	−0.295	−0.333	−0.326	−0.332	0.070
	(0.120)	(0.119)	(0.105)	(0.118)	(0.040)
Panel B. Average value of dependent variable in years 2000–2013, by age in 1979, in levels					
0–10	42,728	41,004	25.53	80,971	0.122
11–19	51,325	48,484	29.80	94,026	0.103

Notes: See notes to Table 2. The sample in columns 1–4 contains 15.6 million individuals born from 1950–1979 in the continental US with a unique birth county, unique PIK, nonallocated variables, and positive values of family income, earned income, personal income, and wage. The sample in column 5 contains 18.4 million individuals born from 1950–1979 in the continental US with a unique birth county, unique PIK, and nonallocated variables. All monetary variables are in 2014 dollars.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file, publicly available 2000–2013 census/ACS data from Ruggles et al. (2015)

by 3.0 percent ($0.77).[27] For the same group, total family income falls by 4.2 percent ($3,400), and the probability of living in poverty increases by 13.0 percent (1.6 percentage points).[28] Individuals age 11–19 in 1979 experience a smaller decrease in income, a similar decrease in wages, and a smaller increase in poverty. The long-run effects of the recession on income, wages, and poverty are more severe for individuals who were children and adolescents when the recession began than for individuals who were young adults and already in the labor market. This points to the importance of human capital attainment in explaining these results. As the recession might have negatively affected individuals who were 20–29 years old in 1979, these estimates could be biased upward (i.e., too conservative).

Recent work by Chetty and Hendren (2018a, b) provides a valuable comparison for understanding the size of these effects. They study what happens when children grow up in worse or better places, where neighborhood (i.e., commuting zone (CZ) or county) quality is fixed over time and measured by the income in adulthood of permanent residents. As a result, Chetty and Hendren's (2018a, b) neighborhood quality measure reflects many determinants of long-run outcomes besides local economic conditions. Nonetheless, the effects of the recession are sizable relative to Chetty and Hendren's (2018a, b) measure: for individuals age 0–10

[27] Earned income is wage and salary plus business and farm income. Personal income is the sum of earned, welfare, social security, supplementary security, investment, retirement, and other income. To limit the influence of potential outliers in the self-reported income data, for each income category, I replace values above the 99.5th percentile in each survey year by state of residence cell with the average among those above the 99.5th percentile. I construct earned and personal income as the sum of the nonallocated, top-code-adjusted components.

[28] In constructing family income and poverty rates, I separate extended families living in the same household (see Hoynes, Page, and Stevens 2006). I create family interrelationship variables in the confidential census/ACS data that mirror those constructed by Ruggles et al. (2015) in Integrated Public Use Microdata Series data.

in 1979, a 1 standard deviation increase in recession severity has similar effects on family income as a 0.5 standard deviation decrease in their county quality measure throughout childhood.[29]

The age profiles in Table 3 are similar to those in Table 2, as the effects are generally more severe for individuals age zero to ten in 1979.[30] The impacts on labor market outcomes for the younger group could be attenuated by life cycle bias (Haider and Solon 2006). In particular, the effect of the recession on income and wages early in an individual's career could be biased upward relative to the lifetime effect. As a result, I likely understate the size of the lifetime effect for individuals who were younger when the recession began.[31]

To explore the role of education in these results, I calculate the implied effects of the recession on income and wages based on the estimated effects on education and OLS estimates of the returns to high school diploma/GED attainment, college attendance, two-year degree attainment, and four-year degree attainment.[32] Column 1 of Table 4 reports the estimates from Table 3 for reference. The estimates in column 2 show that the implied effects via education can account for much of the estimated magnitudes: the predicted effects are 41–85 percent as large as the actual effects. This evidence is simply suggestive, as the OLS estimates might differ from the causal returns to education for this population because of omitted variables bias or heterogeneity in the returns to schooling.

The negative effects on income and wages also might arise from individuals' tendency to live and work near their place of birth, which experienced a persistent decrease in local economic activity following the 1980–1982 recession. To examine this, I estimate regressions in which the dependent variable is the mean log income or wage of all workers age 25–64 who live in the individual's 2000–2013 CZ of residence. The estimates are in column 3 of Table 4. Interestingly, the pattern of coefficients is similar to the estimated impacts on education and income. This implies that individuals age 0–10 in 1979 live in CZs with lower mean income than individuals age 11–19 in 1979. The estimated impacts on mean income in individuals' CZ of residence are 44–67 percent as large as the impacts on individuals' income. Consequently, location also appears to be an important mediating factor. In interpreting these results, it is important to note that individuals' location depends

[29] A 1 standard deviation increase in the severity of the recession amounts to a 10.6 percent decrease in earnings per capita from 1979 to 1982, which leads to a 4.2 percent decrease in family income. Chetty and Hendren (2018b) find that spending 20 years in a 1 standard deviation worse county leads to a 10.4 percent decrease in earnings for children of parents at the twenty-fifth percentile of the income distribution, and a 6.4 percent decrease in earnings for children of parents at the seventy-fifth percentile. Taking the average of these yields an 8.4 percent decrease in earnings.

[30] Online Appendix Figures A.13–A.17 present estimates for income, wages, and poverty using finer age categories.

[31] Because the recession reduces college degree attainment, the effect of the recession on early career income is likely less negative than the effect on lifetime income. The effect on late career income (e.g., for individuals age 29 in 1979) could be more negative than the effect on lifetime income, in part because early career income is earned before the recession. Both considerations lead to an upward bias in the difference-in-difference estimates, with this bias being more severe for younger individuals. Haider and Solon (2006) find substantial life cycle bias up to age 30. Because the 2000 census has more observations than the 2001–2013 ACS samples, my estimates place higher weight on earlier ages, which suggests that life cycle bias could be quantitatively important.

[32] I use 2000–2013 census/ACS data to regress log income and log wages on indicators for high school/GED attainment, college attendance, two-year degree attainment, and four-year degree attainment, plus indicators for gender-by-age, race, and year.

Table 4—The Long-Run Effects of the 1980–1982 Recession on Income and Wages, Examining the Role of Education and Commuting Zone of Residence

	Estimated effect on dependent variable (1)	Implied by effects on education (2)	Estimated effect on mean in CZ of residence (3)	Relative effect: Education (4)	Relative effect: CZ of residence (5)
Interaction between 1979–1982 decrease in log real earnings per capita and age in 1979					
Panel A. Dependent variable: Log personal income					
0–10	−0.447	−0.317	−0.254	0.710	0.568
	(0.158)	(0.164)	(0.084)		
11–19	−0.295	−0.153	−0.149	0.520	0.504
	(0.120)	(0.115)	(0.053)		
Panel B. Dependent variable: Log hourly wage					
0–10	−0.303	−0.259	−0.204	0.853	0.672
	(0.125)	(0.124)	(0.073)		
11–19	−0.326	−0.138	−0.145	0.424	0.444
	(0.105)	(0.084)	(0.047)		
Panel C. Dependent variable: Log family income					
0–10	−0.419	−0.289	−0.250	0.689	0.596
	(0.173)	(0.150)	(0.085)		
11–19	−0.332	−0.136	−0.157	0.411	0.472
	(0.118)	(0.106)	(0.052)		

Notes: Column 1 reports the estimated effect on the dependent variable from Table 3. Column 2 reports the implied effect based on the estimates in Table 2 (columns 1, 2, 4, and 5) and OLS regressions of income and wages on these measures of education plus indicators for gender by age, race, and year. In column 3, the dependent variable is mean log income or wage of all workers age 25–64 who live in individuals' CZ of residence. Column 4 equals the ratio of column 2 to column 1, and column 5 equals the ratio of column 3 to column 1.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file, publicly available 2000–2013 census/ACS data from Ruggles et al. (2015)

partly on their educational attainment (Wozniak 2010, Malamud and Wozniak 2012).

Online Appendix Table A.8 displays the cross-sectional relationship between recession severity and income in adulthood. For earned income, the difference-in-difference estimates in Table 3 for zero-to-ten-year-olds are about one-third as large as the first difference estimates. Although we cannot interpret the first difference estimates on 20–29-year-olds as causal (e.g., because of the education differentials apparent in Table A.6), it is reassuring that the difference-in-difference estimates are of a reasonable order of magnitude. Online Appendix Table A.9 shows heterogeneity in the impacts by gender and race; these are largely as expected given the heterogeneity in the impacts on education.

C. *Evidence Supporting the Empirical Strategy and Robustness*

Several pieces of evidence suggest that these long-run effects are due to the recession as opposed to some confounding variable. Most importantly, the falsification tests in Figures 3 and 4 show a lack of pretrends in educational attainment. Furthermore, my empirical strategy exploits sudden changes in local economic activity driven by

the interaction of preexisting industrial specialization and aggregate employment changes that emerged during the 1980–1982 recession. This research design mitigates many potential concerns about selective migration or fertility before 1980. As discussed in online Appendix B, the industrial specialization that led to severe earnings and employment losses during the 1980–1982 recession is uncorrelated with counties' median family income growth from 1970 to 1980 and correlated with higher income growth from 1950 to 1970. My regressions control flexibly for prerecession changes in log median family income and other county-level covariates. Furthermore, there is little correlation between the predicted log employment change from 1979 to 1982 and the severity of other recessions from 1973 to 2009, Japanese import competition from 1970 to 1990, or Chinese import competition from 1991 to 2007.[33]

Online Appendix F describes results from birth certificate data that provide additional evidence on the validity of my empirical strategy. Most importantly, I find no evidence that the evolution of infant mortality from 1950 to 1979 is correlated with the severity of the 1980–1982 recession. There is also no evidence that my results are driven by a decrease in maternal education or infant birth weight. These results strongly rule out the possibility that my estimates simply reflect changes in infant health. Online Appendix G describes additional robustness tests, all of which support my conclusions about the long-run effects of the 1980–1982 recession.

D. *Mechanisms*

The age profile in Figure 3 informs the mechanisms underlying these long-run effects. As discussed in Section II, negative long-run effects on children could stem from a decrease in childhood human capital development or a decrease in parental resources to finance college in the presence of credit constraints. In principle, a decrease in parental resources to finance college also could affect teenagers and individuals already enrolled in college. However, I estimate small and statistically insignificant effects for individuals age 14–22 in 1979, which suggests that the short-term decrease in parental resources plays a minor role. Table 2 shows that the negative effects of the recession are concentrated at higher levels of educational attainment, for which childhood human capital and parental resources seem to have the largest impacts (Belley and Lochner 2007, Bailey and Dynarski 2011).

Both human capital development during childhood and parental resources to finance college could fall because of a decline in parental earnings. While my data do not contain information on parents' labor market outcomes, estimates of the long-run effects of parental job displacement provide a benchmark. If job loss generated all of the county-level decrease in earnings per capita from 1979 to 1982 and the recession only affected children whose parents lost a job, then previous

[33] Online Appendix Table A.10 shows small within-state correlations between the change in log earnings per capita from 1979 to 1982 and during other recessions as well as small within-state correlations between the predicted log employment change from 1979 to 1982 and the log earnings per capita change during other recessions. There is little correlation between the log earnings per capita change or predicted log employment change from 1979 to 1982 and Japanese import competition as measured by Batistich and Bond (2019) or Chinese import competition as measured by Autor, Dorn, and Hanson (2013).

work suggests that a 10 percent decrease in earnings per capita would decrease college attendance by 0.5 percentage points (Hilger 2016), 1.5 percentage points (Page, Stevens, and Lindo 2007), or 10 percentage points (Coelli 2011).[34] These studies typically focus on children who are teenagers at the time of job loss. For this group, Table 2 implies that a 10 percent decrease in earnings per capita leads to an imprecisely estimated 1.4 percentage point decrease in college attendance. This point estimate lies within the wide range predicted by past work, suggesting that parental job loss could explain the effects of the recession on college attendance. A similar conclusion holds when looking at the long-run effects of the recession on income.[35] However, these conclusions are tempered by the wide range of estimates from the parental job displacement literature.

Human capital development during childhood could fall because of a decline in community investments. While I cannot measure most forms of community investment, like neighborhood or school quality, data on local government expenditures are available. As described in online Appendix H, expenditures per capita fell starting in the early 1990s in counties with a more severe recession, but there is little evidence of a change before then. The decline in expenditures is driven by spending on welfare and health, and not education. These results imply that in principle, the decrease in expenditures could contribute to the effects of the recession on individuals born in the 1970s but not the effects on individuals born in the 1960s.

Other mechanisms could shape the long-run effects. The opportunity cost channel predicts positive effects of the recession on educational attainment. Given the persistence of the decline in local economic activity, this channel could influence children and adolescents in addition to older individuals making contemporaneous enrollment decisions. For children and adolescents, my results indicate that opportunity cost is dominated by the mechanisms that reduce educational attainment.[36]

Previous studies also find that individuals who graduate from college during a recession experience a lasting decline in earnings and wages relative to graduates who enter a stronger labor market, partly due to working at lower-paying employers (Kahn 2010; Oreopoulos, von Wachter, and Heisz 2012; Altonji, Kahn, and Speer 2016). This channel could explain some of the decrease in income and wages.[37]

[34] These papers find that job displacement leads to long-run reductions in family income of around 10 percent. They do not estimate the effect of parental job displacement on college degree attainment.

[35] Previous studies suggest that if job loss generated all of the county-level decrease in earnings per capita from 1979 to 1982 and the recession only affected children whose parents lost a job, then a 10 percent decrease in earnings per capita would decrease earned income by 0–9 percent (Page, Stevens, and Lindo 2007; Bratberg, Nilsen, and Vaage 2008; Oreopoulos, Page, and Stevens 2008; Hilger 2016). For individuals age 11–19 in 1979, Table 3 implies that a 10 percent decrease in earnings per capita leads to a 3 percent decrease in earned income.

[36] As shown in online Appendix Figures A.8 and A.9, I do not find significant long-run effects on high school degree attainment or college attendance for older cohorts. Previous research on the relationship between economic conditions and high school and college enrollment yields mixed results. Betts and McFarland (1995) and Clark (2011) find significant positive relationships between enrollment and unemployment rates. Conclusions are similar from studies of specific shocks by Black, McKinnish, and Sanders (2005); Charles, Hurst, and Notowidigdo (2018), and Cascio and Narayan (2019). Card and Lemieux (2001), Christian (2007), and Barr and Turner (2013) generally do not find statistically significant relationships, although Barr and Turner (2013) find a stronger positive relationship between unemployment and enrollment around the Great Recession.

[37] Online Appendix Figures A.14 and A.16 imply that a 10 percent decrease in earnings per capita leads to a statistically insignificant 3.6 percent decrease in earned income and hourly wages for individuals age 22 in 1979. This is consistent with some of the estimates in Kahn (2010) and with the estimates in Oreopoulos, von Wachter, and Heisz (2012) and Altonji, Kahn, and Speer (2016), which imply that college graduates' earnings and wages

E. *Potentially Mitigating Policies*

Finally, I examine policies that might have mitigated the recession's long-run effects. To do so, I estimate interactions between the severity of the recession and features of individuals' birth states. This augments my baseline specification in equation (1), which includes birth-state-by-birth year fixed effects. I focus on four-year college degree attainment because of its importance for individuals and the economy.

States might have mitigated the recession's long-run effects with more generous transfer programs that insured families and communities against earnings declines. In measuring states' transfer generosity, I control for economic and demographic characteristics that could relate mechanically to higher transfers by regressing, at the state level, log transfers per capita in 1970 on log median family income in 1969 and the share of the 1970 population that is Black, female, foreign born, urban, a high school graduate, a college graduate, and ages 5–19, 20–64, and 65 and above.[38] Columns 1 and 2 of Table 5 divide the sample into states with less and more generous transfers per capita using the residuals from this regression. The point estimates suggest that the effects of the recession are more severe in states with more generous transfers but the estimates are not statistically distinguishable ($p = 0.52$). There is little evidence that states with more generous transfer programs mitigated the recession's effects.

To examine whether overall transfer generosity masks effects of more targeted transfers, I use Grapevine data to divide the sample into states with higher versus lower per capita appropriations to higher education in 1970, conditional on the same economic and demographic characteristics. The point estimates in columns 3 and 4 suggest that the effect of the recession on four-year degree attainment of individuals age 0–10 in 1979 is over 50 percent smaller in states with more generous higher education transfers. However, the estimates again are not statistically distinguishable ($p = 0.20$).[39]

Another possibility is that states that transfer more money to poor counties mitigated the long-run effects of the recession. To characterize states' transfer progressivity, I regress log transfers per capita in 1970 on log median family income in 1969, state fixed effects, and the previously listed control variables, with the dependent and explanatory variables measured at the county level. Columns 5 and 6 present results from dividing states into two groups using the state-specific slope coefficient on log median family income.[40] The effects of the recession are

recover after 10 or 15 years. Prior work does not find persistent effects of labor market conditions at entry on workers with less than a college degree (Hershbein 2012, Speer 2016).

[38] In constructing the dependent variable, I focus on transfers over which states have some statutory or administrative control: retirement and disability insurance (excluding social security), Medicare, public assistance medical care benefits (primarily Medicaid), income maintenance benefits (including food stamps and Aid to Families with Dependent Children), unemployment insurance compensation, and education and training assistance. Online Appendix Table A.18 characterizes the generosity and progressivity of state transfer systems.

[39] Appropriations in 1970 have the advantage of being determined before the recession, but they imperfectly measure higher education funding when individuals made their schooling decision. Postrecession appropriations are difficult to interpret because they might have been affected by the recession. The same is true of overall transfers and transfer progressivity.

[40] Card and Payne (2002) use a similar approach to characterize state-level school aid systems.

Table 5—The Long-Run Effects of the 1980–1982 Recession on Four-Year College Degree Attainment, Potentially Mitigating Policies

Policy:	State transfer generosity		State higher education funding		State transfer progressivity	
	Less generous (1)	More generous (2)	Less generous (3)	More generous (4)	Less progressive (5)	More progressive (6)
Interaction between 1979–1982 decrease in log real earnings per capita and age in 1979						
0–10	−0.382	−0.536	−0.678	−0.312	−0.360	−0.541
	(0.174)	(0.283)	(0.280)	(0.177)	(0.207)	(0.231)
11–19	−0.185	−0.366	−0.335	−0.244	−0.161	−0.372
	(0.098)	(0.175)	(0.173)	(0.121)	(0.166)	(0.132)
p-value, equal effects	0.516		0.204		0.529	

Notes: See notes to Table 2. Each column reports the results of a separate 2SLS regression. The *p*-value is for the null hypothesis that the effects of the recession are equal across columns. States with less generous transfers are those with below-median log transfers per capita in 1970, conditional on log median family income in 1969 and the share of the 1970 population that is Black, female, foreign born, urban, a high school graduate, a college graduate, and ages 5–19, 20–64, and 65 and above. States with less generous higher education funding are those with below-median log higher education appropriations per capita in 1970, conditional on the same demographic and economic covariates. States with less progressive transfers are those with an above-median slope coefficient from a county-level regression of log transfers per capita on log median family income in 1970 and the same demographic and economic covariates.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file, Census County Data Book, Grapevine appropriations data

considerably less severe in states with more progressive transfers. However, the estimates are not statistically distinguishable ($p = 0.53$), providing little evidence that states with more progressive transfers mitigated the recession's effects. Ultimately, more research is needed to understand whether any policies mitigated the recession's long-run effects.

A separate possible mitigating factor is the postrecession recovery. To examine this, I estimate regressions that include the decrease in log real earnings per capita from 1979 to 1982 and 1982 to 1992. As instrumental variables, I use the predicted log employment change from 1979 to 1982 and 1982 to 1992 based on a county's 1976 industrial structure. The results in online Appendix Table A.19 show that, conditional on the extent of postrecession recovery, there are sizable negative effects of the earnings decrease during the recession (similar to the estimates in Table 2).[41] At the same time, a relative decrease in earnings per capita after the recession also leads to reductions in education; this implies that stronger postrecession growth had the potential to mitigate some of the recession's effects. Consequently, policies that promote local economic growth or encourage families to migrate to stronger local labor markets could be effective at mitigating the recession's long-run effects.

[41] This indicates that the evolution of earnings per capita from 1982 to 1992 is not a confounding omitted variable in my main specification, consistent with a relatively small correlation between the severity of the recession and the postrecession evolution of economic activity (e.g., see Figure 2).

V. Conclusion: The Long-Run Effects of Recessions

This paper provides new evidence on the long-run effects of recessions on education and income. Using confidential census data linked to county of birth and a generalized difference-in-difference framework, I estimate the long-run effects of the 1980–1982 recession on individuals who were children, adolescents, and young adults when the recession began. I find that the recession generated sizable long-run reductions in education and income. For individuals age 0–10 in 1979, a 10 percent decrease in earnings per capita from 1979 to 1982 in their county of birth leads to a 4.7 percentage point (14.6 percent) decrease in four-year college degree attainment, a $2,100 (5.2 percent) decrease in earned income, and a 1.6 percentage point (13.0 percent) increase in the probability of living in poverty as of 2000–2013. The negative effects on college graduation are most severe and essentially constant for individuals age 0–13 in 1979 and are small and statistically insignificant for individuals age 14–22, which suggests that the underlying mechanism is a decline in childhood human capital or a long-term decline in parental resources to pay for college.

The magnitude of these estimates and the large number of affected individuals suggest that the 1980–1982 recession depresses aggregate economic output today. Table 6 reports back-of-the-envelope calculations that scale my difference-in-difference estimates by the 70 million individuals born in the United States from 1960 to 1979. In particular, I calculate the aggregate effect of the recession for individuals who were age a in 1979 as $\sum_c N_{a,c}\left(R_c^{79–82} - R_c^{CF}\right)\hat{\pi}_a^*$, where $N_{a,c}$ is the number of individuals born in county c who would have been age a in 1979, $R_c^{79–82}$ is the observed decrease in log real earnings per capita from 1979 to 1982, R_c^{CF} is the counterfactual decrease, and $\hat{\pi}_a^*$ is the difference-in-difference estimate from equation (1) after adjusting for measurement error. If I assume that all counties would have experienced no change in real earnings per capita in the absence of the recession, these calculations imply that the recession led to 1.3 million fewer four-year college graduates, $66 billion less earned income per year, and 403,000 more adults living in poverty each year. If I instead assume that all counties would have experienced the average growth in real earnings per capita from 1969 to 1979 of 1.8 percent per year, these calculations suggest that the recession led to 2.8 million fewer four-year college graduates, $139 billion less earned income per year, and 850,000 more adults living in poverty each year. These numbers amount to 2–4 percent of the stock of college-educated adults in 2015, 0.4–0.8 percent of GDP in 2015 and 0.7–1.5 percent of GDP in 1979, and 1–2 percent of the number of individuals in poverty in 2015.[42] While these simple calculations could understate or overstate the true aggregate effects, they suggest that the 1980–1982 recession considerably reduces aggregate economic output and welfare today.[43]

[42] In 2015, there were 69 million individuals with a four-year college degree (Ryan and Bauman 2016) and 43 million individuals living in poverty (Proctor, Semega, and Kollar 2016). In 2014 dollars, US GDP was $9.0 trillion in 1979 and $18.2 trillion in 2015.

[43] These simple calculations do not capture cohort-wide effects or general equilibrium adjustments. The resulting bias from not capturing cohort-wide effects is unclear, as these effects could be positive or negative, while general equilibrium adjustments suggest that these calculations might overstate the aggregate effects. For example, increasing the college degree attainment of individuals born in one county might decrease the attainment of individuals born in other counties due to less than perfect elasticity of supply of college education (Bound

Table 6—Back-of-the-Envelope Calculations of the Aggregate Long-Run Effects of the 1980–1982 Recession

		Counterfactual 1: No real earnings per capita growth, 1979–1982			Counterfactual 2: Trend real earnings per capita growth, 1979–1982		
	Number of births, millions	Four-year college graduates	Earned income, billions $	Adults living in poverty	Four-year college graduates	Earned income, billions $	Adults living in poverty
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age in 1979							
0–10	36.0	−844,000	−38.2	284,000	−1,779,000	−80.6	598,000
11–19	34.1	−489,000	−27.6	120,000	−1,029,000	−58.1	252,000
0–19	70.2	−1,333,000	−65.8	403,000	−2,808,000	−138.6	850,000

Notes: Table displays back-of-the-envelope calculations of the aggregate long-run effects of the 1980–1982 recession. For individuals who were a years old in 1979, I calculate these as $\sum_c N_{a,c}\left(R_c^{79\text{–}82} - R_c^{CF}\right)\hat{\pi}_a^*$, where $N_{a,c}$ is the number of births of individuals residing in county c net of infant mortality, $R_c^{79\text{–}82}$ is the observed decrease in log real earnings per capita from 1979 to 1982 in county c, R_c^{CF} is the counterfactual decrease in log real earnings per capita, and $\hat{\pi}_a^*$ is the difference-in-difference estimate after adjusting for measurement error. In counterfactual 1, I set $R_c^{CF} = 0$, and in counterfactual 2, $R_c^{CF} = -0.055$, which corresponds to the average annual growth in earnings per capita from 1969 to 1979 of 1.84 percent. Column 1 reports the total number of births for each age group, net of infant mortality $\left(\sum_c N_{a,c}\right)$. Columns 2 and 5 use difference-in-difference estimates from column 4 of Table 2. Columns 3 and 6 use estimates from column 2 of Table 3. Columns 4 and 7 use estimates from column 5 of Table 3. Numbers are rounded.

Sources: BEA regional economic accounts, Census CBP, confidential 2000–2013 census/ACS data linked to the SSA Numident file, birth and infant mortality data from Bailey et al. (2018)

This paper shows that the 1980–1982 recession persistently decreased earnings per capita in negatively affected counties and that children and adolescents born in these counties have less education and income as adults. While I have not examined whether other recessions have similar long-run effects, Figure 6 provides reason for concern: every US recession since 1973 has led to a persistent relative decrease in earnings per capita in negatively affected counties.[44] This novel stylized fact indicates that the 1980–1982 recession is not unique in its persistent effects on county-level earnings per capita, which suggests that it might not be unique in its long-run effects on human capital.

I find little evidence that states with more generous or more progressive transfer systems mitigated the 1980–1982 recession's long-run effects. An open question left for future work is which policies, if any, temper the declines in human capital attainment. The importance of understanding these effects is underlined by the possibility that similar long-run effects could arise from other shocks leading to persistent declines in local economic activity, such as Chinese import competition (Autor, Dorn, and Hanson 2013).

and Turner 2007). These calculations could understate the true aggregate effects because they only include individuals born from 1960 to 1979, and the recession could have negative effects on earnings of people born before 1960 and on education and earnings of people born after 1979.

[44] The figure plots the percent difference in earnings per capita between counties with a more versus less severe recession, normalized to equal zero at the onset of the recession. The counties with a more versus less severe recession are defined separately for each recession.

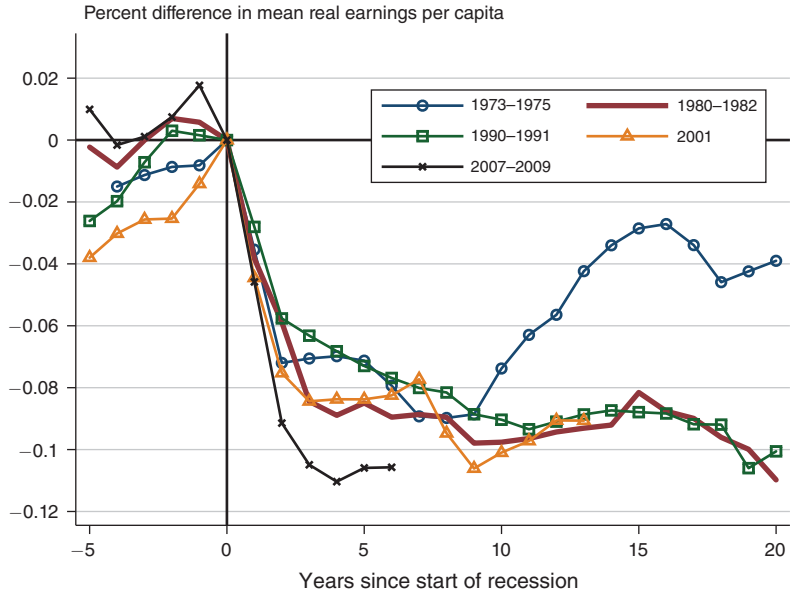

FIGURE 6. PERCENT DIFFERENCE IN MEAN REAL EARNINGS PER CAPITA BETWEEN COUNTIES WITH MORE VERSUS LESS SEVERE RECESSION

Notes: Figure displays the difference in population-weighted mean real earnings per capita between counties with a more versus less severe recession as a share of the less severe recession mean. Separately for each recession, I define counties with a more severe recession as those with an above median decrease in log real earnings per capita from 1973 to 1975, 1979 to 1982, 1989 to 1991, 2000 to 2002, and 2007 to 2010. I calculate the medians using population weights in the prerecession years. Each line is normalized to equal zero in the prerecession year via a parallel shift upward or downward. Sample contains 3,076 counties in the continental United States.

Source: BEA regional economic accounts

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