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Large-Scale Public Hiring, Wages, and Community Outcomes: Evidence from the Works Progress Administration

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Abstract

Using discontinuities in the wage schedule of the Works Progress Administration (WPA), I document that higher WPA wages led to greater WPA employment but did not significantly reduce unemployment levels. The estimated impact on private wages is positive but imprecise. Higher WPA wages did not influence the characteristics of WPA employees. However, they led to lower school dropout rates of 15–16-year-old boys in WPA households.

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I. Introduction

The Works Progress Administration (WPA) was a large-scale public hiring program established in 1935 as a key component of President Franklin D. Roosevelt's New Deal during the Great Depression. Under the WPA, the federal government directly employed millions of unemployed individuals to carry out public works projects across the U.S. Research shows that the WPA and other New Deal programs were instrumental in alleviating hardship by increasing consumption activity, reducing crime rates, and lowering several types of mortality (see Fishback 2017 for a review). In this paper, I study how wages, one of the key design considerations for the WPA and for contemporary large-scale public hiring proposals (Howard 1943, p. 158; Nunn et al. 2018), influenced outcomes for workers and their communities.

WPA wages were determined by schedules of monthly earnings that were based on geographical region, degree of urbanization, and skill level (Burns and Kerr 1941). To study the impact of WPA wages, I rely on discontinuities in the wage schedule based on degree of urbanization, which jumped as the 1930 population of the largest city in a county crossed the 5,000, 25,000, and 100,000 thresholds. I focus my analysis on the 5,000 threshold because of the multitude of counties near that threshold whose data I can use when implementing a regression discontinuity methodology to understand the impact of higher wages.

To measure WPA wages, I rely on the 1940 complete count Census, which allows me to construct weekly wages for individuals at work on public emergency projects such as those under the WPA (Ruggles et al. 2024). Using a local linear regression discontinuity methodology that controls for demographic and geographic characteristics to help with precision, I find that WPA weekly wages increased by 6 percent across the 5,000 city population threshold, in line

with the posted wage schedule. This finding is robust across different specifications. I then estimate elasticities of various outcomes with respect to WPA wages.

I begin by examining whether WPA employment responded to higher WPA wages. I find that WPA employment increased, with a finite and elastic labor supply elasticity of roughly 3. Higher WPA wages did not change the composition of WPA workers based on observable characteristics. The WPA did not retain or attract workers with more age (and thus work experience) or education when paying higher wages.

I next assess whether the increases in WPA employment were associated with reductions in unemployment and whether the increases in WPA wages were associated with increases in private wages. Although the populations of WPA employed and unemployed were roughly the same in rural counties, my unemployment elasticity estimates rule out reductions in unemployment fully corresponding with the increases in WPA employment. The increase in WPA employment could have come partially from private employment, as has been found in the existing literature (see Fishback 2017 for a review), but my results are too imprecise to definitively determine if that were the case. My estimates of higher WPA wages on private sector wages are positive but also imprecise. The estimates rule out equivalent increases in private wages but allow for smaller spillovers. This finding remains when I examine private sector wages with the same occupational makeup of the WPA or for those with no high school education.

After examining how the increase in WPA wages affected WPA workers and the broader economy, I study how WPA wages might have impacted future economic outcomes by assessing the effect of WPA wages on children in WPA households. I examine whether teens with a WPA worker in their household were more likely to stay in school when the WPA worker earns more. I

find that teenage boys ages 15-16, which were ages of frequent early dropout during this time, were more likely to stay in school as pressure for them to earn income decreased. I do not observe a similar effect for teenage girls, who would face less pressure to earn for their household, or for younger boys or older boys who would not be at high risk for early dropout in this period. Relatively few boys 14 and under left school in 1940, and boys 17 and over had frequently left school already.

Altogether, my results show that higher WPA wages benefited WPA workers and their families but had limited effect on unemployment. The paper makes two main contributions. First, it contributes to the economics literature examining the functioning of the labor market, both more generally as well as specifically in relation to the WPA and similar New Deal programs. The primary finding of an increase in WPA employment provides a historical estimate of the labor supply elasticity faced by employers of about 3. This estimate falls in the middle of the range of labor supply elasticities faced by employers found in the broader literature, which spans from almost zero to nearly 10 (e.g., Dal Bó et al. 2013; Dube et al. 2020; Emanuel and Harrington 2022; Ransom and Sims 2010; Staiger et al. 2010; Webber 2015).

Although the labor supply elasticity is finite, consistent with an imperfectly competitive monopsonistic labor market, it is not as inelastic as many of the estimates in the literature. Monopsony power is theoretically heightened when the labor market is slack and workers have fewer outside options (Cumming 2022). The rather elastic estimate uncovered in my study, which examined a period of extremely slack labor market conditions, suggests that factors outside of economic conditions may instead matter more for the monopsony power employers can exercise. For example, Cumming (2022) suggests that monopsony power is also influenced

by the transferability of skills used for the job, geographic mobility, job search frictions related to information and hiring processes, worker voice, and household obligations like childcare.

These characteristics are also all relevant to my WPA analysis setting. Most of the WPA jobs analyzed were skill accessible. The analysis period coincided with the Great Migration and Dust Bowl migration. The WPA hiring policies were shared with the public by the government and were somewhat onerous but not to the extent of modern hiring requiring background checks, multiple interview rounds, and lengthy applications. Workers had the right to organize and strike. And lastly, my sample is limited to men accustomed to working and thus not likely faced with sizeable childcare obligations. The high elasticity I uncover suggests that these factors, in some combination, could outweigh the effect of labor market slack in determining monopsony power.

Additionally, the paper contributes to the literature on efficiency wages, which theorizes that higher wages may help employers attract or retain workers with more work experience or skill (Katz 1986; Stiglitz 1976; Weiss 1980). The paper shows that at least in the setting of the WPA, higher wages did not change the composition of the workforce. It is possible the result stems from extremely slack labor conditions and WPA eligibility requirements that led the average WPA worker to look similar to the marginal worker induced by wage increases.

Although it is difficult to isolate the precise mechanism, the study presents a series of circumstances under which certain aspects of efficiency wage theory are less likely to hold.

The paper also adds to a labor economics literature about the WPA and related programs. Research specific to programs like the WPA has found that these programs had limited impact on unemployment, with some studies uncovering reductions in private employment associated with the programs (Aizer et al. 2024; Benjamin and Matthews 1992; Fishback 2017; Fleck 1999; Liu and Fishback 2019; Neumann et al. 2010). The findings in this paper align with that literature,

likewise uncovering a limited reduction in unemployment. In addition to corroborating existing results, the paper extends the literature on the WPA's labor market effects by examining the specific impact of higher wages, an important component of the program which had been previously unexplored.

Wages are a key design parameter of large-scale public hiring policies, with concerns about private sector crowd out a major consideration (Nunn et al. 2018). This study suggests that higher wages do not significantly reduce unemployment, highlighting a trade-off between income support and unemployment reduction in public hiring programs. The paper supports new research suggesting that much of the unemployment reduction of public hiring programs occurs when the government focuses its hiring on individuals with the fewest outside options, with a much smaller effect for those with more outside options (Bee and Narayan 2025). Higher wages likely induced employment among those with better outside options who otherwise would not have sought WPA employment.¹ It is therefore consistent with Bee and Narayan (2025) that increased WPA employment induced by higher wages did not correspond with significantly lower unemployment. Moreover, this study expands upon Bee and Narayan (2025) by examining a setting when the labor market was rather slack and public hiring wages quite low, suggesting that the role of outside options matters even in extreme contexts.

In addition to contributing to a labor economics literature, my paper adds to a literature on the educational impacts both of New Deal programs in particular as well as income shocks more broadly. Existing work finds that children who were born in areas with more New Deal

¹ The existing literature observing limited reduction in unemployment by WPA and related employment using variation in voter turnout and political leaning, shift shares instruments, panel vector autoregressions, and ordinary least squares with controls likely also identifies effects for the average or upper margin of public hiring workers, but it is less obvious from the study designs compared with wages what the outside options are for individuals captured in the estimated treatment effects (Aizer et al. 2024; Benjamin and Matthews 1992; Fishback 2017; Fleck 1999; Liu and Fishback 2019; Neumann et al. 2010).

spending or employment had greater educational attainment (Arthi et al. 2018; Modrek et al. 2022). However, despite using a series of controls, these studies cannot completely isolate the effects of New Deal spending or employment from underlying economic, social, and political trends that occurred areas with greater New Deal involvement during the 1930s and early 1940s. Analysis using individual level variation in time spent in the Civilian Conservation Corps, a New Deal program similar to the WPA for youth, finds that additional time led to increased educational attainment (Aizer et al. 2024). Although Aizer et al. (2024) can include a wider set of geographic and individual controls than earlier work, the setting does not allow for the researchers to completely control for factors that influence individuals' time spent in the Civilian Conservation Corps.

This study adds to the New Deal literature on educational impacts by showing that higher WPA wages lowered the likelihood of school dropout for teenage boys in WPA households on the margin of early dropout. It builds on the existing research in two ways. First, it uses a regression discontinuity research design that helps reduce omitted variable bias. Because geographic and individual factors should be smooth across the discontinuity, the method helps isolate the effects of the New Deal program more clearly than existing studies. Second, this study's methodology allows for a more careful understanding of the mechanisms behind the result of increased educational attainment. New Deal spending could have affected educational outcomes in various ways in existing research, such as by building schools and related public facilities, by facilitating social connections that encouraged schooling, or by increasing household income. By examining wages for household members and finding effects concentrated among boys in WPA households on the verge of dropout, this analysis suggests that one mechanism through which the WPA and related programs affected educational outcomes was via

greater household income that reduced opportunity cost of schooling for boys.² Other mechanisms would have likely affected girls and boys in similar ways or non-WPA households.

More generally, the paper contributes to a literature examining the educational response to greater household income. This literature often finds increases in educational attainment in response to parental income shocks, but the result is mixed. For example, some research finds that large lottery winnings increase college attendance and that Earned Income Tax Credit receipt just prior to high school graduation increases college enrollment (Bulman et al. 2021; Manoli and Turner 2018). Other research, however, studying parental layoffs or family receipt of basic income finds limited impact on educational attainment (Hill 2016; Krause et al. 2025). Some research seeks to reconcile the diversity of estimates in the literature, building theories of how the timing of parental income matters for children's outcomes (e.g., Carneiro et al. 2021; Caucutt and Lochner 2020). This study adds to the literature by supporting the hypothesis that additional income is helpful for individuals during a time when they are making attainment decisions, based on the result for teenage boys on the margin of early high school dropout. However, the null result for girls suggests that this income is less useful if the direct cost of schooling is low and the opportunity cost is relatively unaffected, as it would be for high school girls in my setting. The results from the paper relying on the WPA's historical context thus help add additional data points to an active debate in the literature.

Altogether, the study makes contributions to various literatures related to labor markets and education. The paper proceeds as follows. Section II provides an overview of WPA policies. Section III outlines my empirical strategy and evaluates its assumptions. Section IV presents the main empirical results. Section V concludes the paper and discusses its policy implications.

² The WPA was unlikely to have raised the opportunity cost of schooling for younger teens, as discussed later in the paper.

II. WPA Policy

II.A. Overview of the WPA

A very detailed overview of the WPA can be found in Howard (1943); I describe the WPA features from the book and related sources most relevant to this study. The WPA was created in 1935 to provide employment on socially useful projects for as many needy unemployed workers meeting certain eligibility criteria as could be given jobs with funds appropriated by Congress (Howard 1943, p. 105). It was part of President Franklin D. Roosevelt's New Deal seeking to provide relief for households struggling during the Great Depression, which had been raging for roughly five years at that point (Howard 1943, p. 105).

WPA projects were developed and operated at the local level and covered a wide range of infrastructure, construction, and conservation efforts, including building roads, bridges, schools, parks, and public buildings, along with art, literacy, and historical initiatives (Howard 1943, pp. 130 and 144). Construction and engineering projects provided the vast majority of WPA employment (Howard 1943, p. 130). Projects had to be publicly useful, could not displace regular employees, had to be capable of being executed by the available supply of eligible workers, could not have costs of material be excessive in comparison to the costs of labor, had to be able to be completed by a certain time, and needed to occur on public property (Howard 1943, p. 141). Local government sponsors initiated projects, contributed to some of the costs, and provided some supervision (Howard 1943, p. 144).

Eligibility was determined by Congress (Howard 1943, p. 269). The minimum age for employment was raised from 16 to 18 years in 1936, though it was difficult to enforce given a lack of birth records in many parts of the country (Howard 1943, p. 271). People over the age of

65 could be employed by the WPA but were rarely deemed eligible when they could qualify for old age assistance (Howard 1943, p. 272). Both women and men were eligible for WPA employment, but women could only be employed by the WPA if they were the head of their household without an employable husband who had a history as a wage earner (Howard 1943, p. 278). Discrimination on the bases of race, color, and creed was prohibited but difficult to enforce (Howard 1943, p. 285). WPA employment was restricted to U.S. citizens, Native Americans, and others owing allegiance to the U.S. since 1939, with affidavits rather than formal documents serving as proof (Howard 1943, p. 303). Starting in 1939, the WPA required workers who had been employed by the WPA for 18 months or longer to be discharged and ineligible for 30 days (Howard 1943, p. 520).

The WPA required workers to demonstrate need to secure employment (Howard 1943, p. 351). They had to show they were unemployed and in need of wages to secure the necessities of life (Howard 1943, p. 351). Need was calculated on a family basis except for unattached individuals living alone (Howard 1943, p. 381). Typically, only the family head was eligible for WPA employment (Howard 1943, p. 341). In addition to demonstrating need, WPA workers had to demonstrate employability (Howard 1943, p. 448).

Employability was based on the ability of the person to perform work in a satisfactory manner on available WPA projects (Howard 1943, p. 449). White-collar workers who could not perform manual labor would be deemed unemployable if there were insufficient white-collar projects initiated in their area (Howard 1943, p. 449). The WPA usually limited eligibility to workers who had recent and regular private employment, restricting its employment of those who had been unemployed or out of the labor force out of long-term (Howard 1943, p. 452). Individuals with physical and mental disabilities that would affect their own or others' safety on

the job were not eligible for WPA employment (Howard 1943, p. 457). Performance on the job impacted WPA workers' ability to keep their jobs (Howard 1943, p. 514). Workers could be disqualified for repeatedly failing to satisfactorily perform required duties (Howard 1943, p. 514). Some offenses, like bodily harm, bribery, malicious destruction of government property, and fraud resulted in permanent dismissals (Howard 1943, p. 515).

WPA employment was thus not available for many individuals in need. The relief provided by WPA employment was also limited by other factors. Local leaders' reluctance to initiate projects restricted the WPA's ability to hire certain workers in need, such as Black workers or workers from specific skill backgrounds (Howard 1943, p. 292). Limited federal appropriations also impacted the availability of WPA projects (Howard 1943, p. 559).

Preliminary estimates of need were calculated up to 15 months before workers would be employed on a project (Howard 1943, p. 567). Local economic conditions could change drastically during this period. Tensions between the Roosevelt administration, Congress, and interest groups also contributed to a smaller budget, and political considerations affected the disbursement of funds across space and time (Fleck 1999).

At its peak in 1938, the WPA employed over 3 million workers (Howard 1943, p. 532). Some evidence suggests that WPA employment was inadequate in its coverage (Howard 1943, p. 605). Only a third of responses for cities between 100,000 and 1,000,000 were affirmative in response to the question: "Have the WPA Programs Covered the Field of the Needy Employables?" (Howard 1943, p. 607). Roughly half of cities with populations less than 100,000 answered affirmatively to this question (Howard 1943, p. 607).

Others argued that the WPA more than adequately met the need of local communities (Howard 1943, p. 605). A variety of data points support this claim, at least for individuals with

good outside options. First, data on labor turnover show that voluntary separations from the WPA were high, even in extremely slack labor markets (United States House Committee on Appropriations, p. 1377). Most workers who separated from the WPA were subsequently supported by income from private employment (United States Works Progress Administration 1938, p. XI).

Second, policymaker concerns about displacement have been validated by more recent research, which shows that WPA and related New Deal employment and spending provided limited reduction in unemployment (Aizer et al. 2024; Benjamin and Matthews 1992; Fishback 2017; Fleck 1999; Liu and Fishback 2019; Neumann et al. 2010).

Neumann et al. (2010) use panel vector autoregressions with differencing and controls for serial correlation and find that after 1935, each additional WPA job-month was associated with 0.66 fewer private job-months. Using shift share instruments and controls, Benjamin and Matthews (1992) find that each additional New Deal relief job during the Second New Deal, the era studied in this paper, crowded out nine-tenths of a private job. Liu and Fishback (2017) control for a broad range of correlates and find that greater public works spending between 1933 and 1939 led to limited changes in unemployment. Point estimates for private employment are negative but imprecise. Fleck (1999) instruments for relief jobs using voter turnout measures of loyalty to Democratic Presidential Candidates and observes that additional relief hiring would not have substantially reduced the number of jobless individuals, though the results cannot indicate the magnitude of the effect on private employment. Aizer et al. (2024) find limited effect of longer service in the Civilian Conservation Corps on 1940 employment using multiple methods to address selection into service lengths. Fleck (1999) and Fishback (2017) review additional related literature with similar findings.

The data on churn between the WPA and the private sector coupled with research showing private sector displacement of the WPA suggest a close connection between private employment and WPA employment. Higher WPA wages in theory thus could raise WPA employment, reduce private employment, and increase private sector wages. However, gaps in the existing literature leave these relationships and their magnitude an open question.

Because WPA eligibility relied on workers being both able to work and needing work, the treatment effects estimated by existing studies, either on average or the margin, could include impacts for not only those who were less needy but also those who were less capable. The existing research also often relies on more rudimentary datasets due to the only recent availability of the complete count 1940 Census, and it often uses methodologies that could more likely be contaminated by omitted variable bias. For example, it is possible that local economic shocks, such as droughts or factory closures, both increasing unemployment and generating demand for WPA employment are not fully captured by local area controls or panel methods, and the shocks or the local response to the shocks could be correlated with shift share or vote share instruments as well. Such shocks may be less likely to affect estimates from a regression discontinuity methodology. Moreover, only recent studies can rely on person-level data to refine their methodologies to study or compare sub-populations, while most older studies must use county-level aggregates. Person-level data are necessary to carry out a study like this one which examines wages for WPA workers and compares WPA and non-WPA workers with the general demographic characteristics of WPA workers. It is therefore not obvious whether the effects observed in the literature would be similar when looking at the treatment effects estimated for higher WPA wages, which likely affected workers with better outside options, using modern regression discontinuity methods and person-level data.

Building specific understanding about the effects of higher wages is also useful for other reasons, as discussed in the introduction. It sheds light on an important policy parameter in public hiring policies, is helpful for learning about the determinants of labor supply, and can enhance our understanding of how income influences children's outcomes like their educational attainment. The next section describes the WPA wage policy as relevant to this study in more detail.

II.B. WPA Wage Policy

Policymakers faced a key trade-off when designing WPA wage policy: they wanted wages to be high enough to support families and encourage work but low enough to minimize private employment impacts (Howard 1943, p. 165). Among those employed, WPA scheduled monthly wages were meant to provide a minimal security wage (Howard 1943, p. 159). Wages were deliberately set below levels in private employment to encourage workers to return to private employment as quickly as possible (Howard 1943, p. 165). It was recognized that posted wage rates may be insufficient for recruiting skilled essential personnel, so state administrators were allowed to make exemptions under select circumstances (Howard 1943, p. 358).

Prior to 1939, WPA wages were based on prevailing wages, with the hours worked making up differences between the posted monthly pay schedule and local conditions (Howard 1943, p. 214). The prevailing wage policy was abandoned in 1939 in order to improve efficiency with more regular hours across occupations (Howard 1943, p. 214). Skilled trade unions also complained about the prevailing wage policy as skilled WPA workers were competing with those in private employment (United States Federal Works Agency 1947, p. 25). Starting in 1939, the WPA required that employees generally work 130 hours a month (Howard 1943, p. 214). Hours

of work could not typically exceed 8 per day or 40 per week (Howard 1943, p. 213). The 1939 WPA wages were often well below private wages, and strikes ensued in response to the wage policy change (Howard 1943, p. 223). There is some evidence that the policy change made it difficult to hire certain WPA workers, such as experienced supervisors (Butcher 1990).

WPA monthly wages for unskilled workers were less than emergency standards in all cities and states for which Howard collected information (Howard 1943, p. 178). WPA wages were typically not supplemented with outside earnings (Howard 1943, p. 199). Over three-quarters of WPA workers reported no outside earnings in their quarterly statements (Howard 1943, p. 209).

Despite paying less than private sector jobs on average, WPA jobs had some advantages over some private sector jobs (Howard 1943, p. 166). In some areas, such as the rural South, WPA wages could be larger than private sector wages (Burns and Kerr 1941). The WPA provided a guaranteed security wage for work done, meaning that individuals in occupations with irregular work hours could earn as much or more working for the WPA than they would in the private sector (Howard 1943, p. 167). In addition, the working conditions at WPA jobs could exceed those in the private sector, leading the WPA jobs to be better for some workers even if the pay was lower. For example, WPA workers could organize, present grievances, and make up time lost due to a variety of reasons ranging from weather conditions to family illness (Howard 1943, pp. 217 and 218).

WPA wages were determined by a county's geographical region and degree of urbanization coupled with the worker's skill level (Burns and Kerr 1941). The wage schedule changed frequently over the duration of the WPA (Howard 1943, p. 158). Some of these decisions had implications for private employment, such as the move to a uniform number of

hours in 1939, as noted above. There were also hours increases and corresponding earnings increases during the wartime influenced by national defense concerns; state WPA administrators were allowed to increase hours and corresponding earnings if they were unable to use other means, such as hiring additional workers or using multiple shifts, to carry out projects (United States Federal Works Agency 1947, p. 25). Other changes were motivated by rising costs of living and a desire to reduce wage differentials across skills or regions (United States Federal Works Agency 1947, p. 23).

Table 1 provides the 1939 monthly pay schedule, the schedule most relevant for my analysis, from Burns and Kerr (1941). The wage regions that correspond with the table are provided in Figure 1. Wages were lowest in the southern U.S. and slightly higher in the western U.S. compared to the northern U.S. Wage regions changed over time, with the South consistently paid less than northern counterparts (Howard 1943, p. 159). The wage schedule also varied by the degree of urbanization, with changes in pay as the 1930 population of the largest city in a county crossed the 5,000, 25,000, and 100,000 thresholds (Howard 1943, p. 159). Earlier schedules also contained a threshold at 50,000 (Burns and Kerr 1937).

Workers were not eligible for WPA certification if they moved across political subdivisions for the sole purpose of obtaining employment on WPA projects or of taking advantage of higher wage rates (Howard 1943, p. 333). In general, workers were not certified for WPA work until they had been in residence for a certain period of time, and WPA officials would often need to individually approve transfers across county or state borders (Howard 1943, p. 333).

III. Measurement of WPA Wage Discontinuity

III.A. Foundation of Empirical Strategy

The discontinuous jumps in pay at the 1930 population thresholds form the basis of my empirical strategy for estimating the impact of higher WPA wages. I focus on this source of variation in WPA pay rather than, for example, variation across regions, as it is fairly consistent over time and unlikely to be correlated with other state or federal policies. To obtain the population of the 1930 largest city population for a county, I rely on a field from the 1930 complete count Census in IPUMS that provides the city population for all identifiable cities rounded to the nearest 100 (Ruggles et al. 2024). I focus my analysis on the discontinuity at the 5,000 population threshold, where there is a larger mass of counties for a regression discontinuity methodology. Figure 2 presents histograms showing the number of counties with their largest cities having a given population in 1930. There are many counties whose populations are close to 5,000 in Panel A, some whose populations are close to 25,000 in Panel B, and very few whose populations are close to 100,000. Appendix Table A1 shows that the regression discontinuity results, using the estimation strategy described in Equation 1 below, are unstable for the 25,000 and 100,000 thresholds, as might be expected given the relatively small number of counties with cities of those sizes in the U.S. in 1930. I therefore do not analyze other outcomes at those thresholds.

To assess the impact of higher WPA pay, I combine the 1930 population count of the largest city in each county with data from the complete count 1940 Census (Ruggles et al. 2024). Unless otherwise noted, I restrict my 1940 sample to household heads ages 18 to 64, in line with Liu and Fishback (2019) and the details of WPA employment provided in section II.A. Table 2 provides summary statistics for this sample for counties with a 1930 largest city population less than 10,000. As controls, I construct from this main analysis sample the county average of age

and age squared, share with no high school education, share with high school education, share with any college education, share White, and share Black. I also include state indicators. These controls help increase the precision of my regression discontinuity methodology.

The main reduced form specification for my regression discontinuity strategy using local linear regression can be characterized by the following equation:

$$(1) \quad wage_c = \gamma_0 + \gamma_1 \mathbf{1}\{pop_c \geq T\} + \gamma_2 \{pop_c - T\} + \gamma_3 \{pop_c - T\} \times \mathbf{1}\{pop_c \geq T\} + vX_c + \mu_s + v_c$$

Here, γ_1 represents my parameter of interest, which is the impact of crossing the wage schedule threshold of $T = 5,000$. pop_c is the running 1930 largest city population variable.

$\mathbf{1}\{pop_c \geq T\}$ is an indicator for whether the population exceeds $T = 5,000$. X_c are the controls mentioned above, and μ_s are the state indicators. v_c represents my error term. $wage_c$ will be my primary first-stage outcome of interest, but I will also use this strategy to assess 1930 baseline characteristics. Unless otherwise noted, I take the natural log of my outcome variables so that the results can be more easily interpreted as percents or elasticities. My unit of observation is county, consistent with the unit of wage policy variation I study. I present the point estimate and 95 percent confidence interval on my parameter of interest in figures, with additional information provided in appendix tables.

Given that the 1930 population counts I use are generated from IPUMS and could differ from those used by WPA administrators, I rely on a uniform kernel to avoid weighting observations close to the threshold more highly than those far away. I use the default bandwidth selection procedure based on the `mserd` one common MSE-optimal bandwidth selector with local linear regression for estimation ($p=1$) and local quadratic regression for bias correction ($q=2$), as suggested by Calonico et al. (2017). The MSE-optimal bandwidth is a data-driven window

around the cutoff that balances bias and variance to minimize mean squared error loss. It yields an asymptotically optimal local average treatment effect (Imbens and Kalyanaraman 2012). Because the bandwidth selection procedure introduces bias into the treatment effect estimation, I report the bias-corrected estimates and robust standard errors from the estimation in the figures and provide conventional results in appendix tables (Calonico et al. 2014; Calonico et al. 2017). I also explore alternative bandwidths that are either optimal in terms of coverage error or are intuitive round numbers.

To assess the validity of my approach, I begin by examining whether there is any bunching at the 5,000 threshold which might indicate manipulation of some sort. Although manipulation seems unlikely given that the 5,000 threshold for the 1930 largest city size was not used for other government policies to my knowledge, it is worthwhile to check for bunching. Figure 3 presents a density plot testing for manipulation using a uniform kernel based on Cattaneo et al. (2018). The associated p-value for the manipulation test is 0.211, failing to reject the null.

I next examine whether there are any discontinuities at the $T = 5,000$ threshold for 1930 economic and other characteristics in Figure 4 Panel A. I observe small and statistically insignificant results for most of the outcomes. The economic outcomes I examine are the 1930 unemployment rate, manufacturing wages, wholesale wages, value of manufacturing, wholesale net sales, and the share of land in farms. The other outcomes I examine are the 1930 total population, percent White, number illiterate, number of radios, and infant mortality. I take the natural log of all variables for ease of interpretation. The estimates are all statistically insignificant and are generally small in magnitude. For example, crossing the $T = 5,000$ threshold

is only associated with a 1 percent change in unemployment or a 4 percent change in the share White, though some of the economic outcomes are noisier with point estimates in the teens.

In the last estimate of the panel, I assess whether there were changes in migration leading up to my study period. The migration measure I examine is the share of individuals in a county who lived in the same county five years ago based on the 1940 complete count Census (Ruggles et al. 2024). I also observe no significant increase in cross-county migration from five years prior to 1940 at the $T = 5,000$ threshold. This result is consistent with the WPA migration policy described in Section II, which limited migration for the purpose of WPA employment. The confidence interval would rule out very small increases of more than 1 percent.

In Panel B, I further test the validity of my approach by examining whether there were discontinuities at the $T = 5,000$ threshold in spending for other New Deal programs. The New Deal spending data I use cover the years 1933 to 1939 and were compiled by Fishback et al. (2003) from the U.S. Office of Government Reports. I find that none of the other New Deal grant programs with more than \$500 million in spending, namely the Federal Emergency Relief Administration (FERA), the Agricultural Adjustment Administration (AAA), the Public Roads Administration (PRA), the Public Works Administration (PWA), the Civil Works Administration (CWA) or Social Security public assistance, saw significant changes in spending. Neither did any of the New Deal loan or insurance programs of more than \$500 million, which were the Reconstruction Finance Corporation (RFC), Home Owners Loan Corporation (HOLC), Farm Credit Administration (FCA), Public Works Administration (PWA), and Farm Housing Administration. Only WPA spending discontinuously rose at the $T = 5,000$ threshold, consistent with the longstanding WPA wage policy and estimates elsewhere in this paper. The results from Figures 3 and 4 support the internal validity of the proposed regression discontinuity method.

III.B. First-Stage Impact on Wages

I use Equation 1 to assess whether WPA workers' wages rose in accordance with the wage schedule. Although the strikes in response to the 1939 wage policy suggest that WPA administrators followed national policy, there are limited data on compliance. To observe county-level WPA wages, I use data from the 1940 complete count Census (Ruggles et al. 2024). The 1940 Census identifies whether individuals are currently working in a public emergency program like the WPA or Civilian Conservation Corps, how long they've been at work in a public emergency job or unemployed, the number of equivalent full-time weeks they were employed the previous year, and their wage and salary income for the previous year. To construct my primary WPA wage variable, I take the previous year's wage and salary income divided by the number of equivalent full-time weeks worked for individuals currently working in a public emergency program who have been at work in a public emergency program or unemployed for at least 65 weeks, following Liu and Fishback (2019). My results are presented using the natural log of weekly WPA wages for ease of interpretation.

The restriction to the roughly 40 percent of public emergency program workers who have been at work in a public emergency program or unemployed for more than 65 weeks helps ensure I am capturing only WPA wages. The variable identifying whether an individual is currently working in a public emergency program is based on the individual's employment status as of March 24-30, 1940. Wage and salary income is for the calendar year 1939. The 65-week restriction allows me to exclude conventionally employed individuals in 1939. The use of workers in public emergency programs is likely a good proxy for WPA workers. Even though the Census does not specify whether individuals working in public emergency programs are working

for the WPA or an alternative program, the WPA was by far the largest of these programs. In March 1940, the WPA employed over 2.3 million workers, while the Civilian Conservation Corps employed about 260,000 (Government Printing Office 1940a; Government Printing Office 1940b). Still, there is some noise in the construction of this WPA wage variable. I trim the top and bottom 5 percent of observations where the wages are clearly above or below what would be expected from the posted wage schedule to make sure I am capturing WPA wages. I examine whether my results are sensitive to these choices in robustness checks.

Figures 5 and 6 present the results for Equation 1 using the WPA wage definition noted above. Figure 5 depicts the result for this primary specification in a detailed regression discontinuity plot, while Figure 6 summarizes the estimates for my primary specification along with various robustness checks. The figures show that my main specification uncovers a roughly 6 percent jump in wages at the $T = 5,000$ threshold. A 6 percent increase in wages at the threshold would be consistent with the wage schedule shown in Table 1. Appendix Figure A1 shows that the discontinuity observed at the 5,000 population threshold represents the most significant increase in wages associated with a rounded cutoff under 10,000. The figure presents the Z statistics for estimations of Equation 1 with thresholds in increments of 500 from 1,000 to 10,000 in the largest city population size. Only one other threshold has a similar magnitude Z statistic, which could be due to chance.

The subsequent estimates in Figure 6 examine the robustness of this result to alternative regression specifications. Rows 2 and 3 explore alternative bandwidths to that chosen using the default mserd one common MSE-optimal bandwidth selector. Row 2 uses a narrower bandwidth chosen with another bandwidth selector which is optimal in terms of coverage error, the cerrd one common CER-optimal bandwidth selector. Row 3 uses a wider arbitrary bandwidth of 2,500.

The estimates are slightly smaller than those from the baseline specification with both alternative bandwidth choices but remain statistically significant. Row 4 uses a triangular kernel rather than a uniform one, and the estimate remains significant showing an increase of about 5 percent. Row 5 drops counties where the largest city had a population of 4,900, 5,000, or 5,100 given uncertainties in my population measure. The estimate remains similar to that at the baseline. Dropping controls in Row 6 has no impact on the estimate but lowers its precision, as might be expected. Row 7 and 8 explore alternative wage definitions. Row 7 uses the baseline definition but only trims the top and bottom 1 percent. Row 8 uses the baseline definition but rather than restricting to individuals unemployed for more than 65 weeks includes all WPA workers in 1940. The point estimates change slightly but remain statistically significant. The last two rows show the estimates when clustering standard errors at the state level and using a local quadratic regression rather than a linear one. In both cases, the estimates and their significance change little.

IV. Impact of Higher Wages on Outcomes

I use the increase in observed WPA wages to estimate the impact of higher WPA wages on WPA employment, the broader economy, and family member outcomes with an instrumental variables methodology implemented with a fuzzy regression discontinuity design. My estimation strategy uses the $T = 5,000$ discontinuity in the 1930 largest city population to predict WPA wages and then uses those predicted WPA wages to understand the impact of higher WPA wages on my outcomes of interest. This research design helps me isolate the effect of WPA wages from other factors that could be correlated with WPA wages.

More formally, the method takes the estimated wage from Equation 1 to estimate Equation 2 below.

$$(2) \quad outcome_c = \beta_0 + \beta_1 \widehat{wage}_c + \beta_2 \{pop_c - T\} + \beta_3 \{pop_c - T\} \times \mathbf{1}\{pop_c \geq T\} + \zeta X_c + \eta_s + \varepsilon_c$$

The regression is analogous to Equation 1, with β_1 representing my parameter of interest. β_1 provides the local average treatment effect (LATE) of higher WPA wages at the $T = 5,000$ threshold. Because I take the natural log of wages and the natural log of my outcome variables, the coefficient can be interpreted as an elasticity, unless otherwise noted. The estimation of the fuzzy regression discontinuity is akin to the reduced form estimation described above, where I use the same controls, a uniform kernel, and the default bandwidth selection procedure based on the msrd one common MSE-optimal bandwidth selector with local linear regression for estimation ($p=1$) and local quadratic regression for bias correction ($q=2$). I continue to report the bias-corrected estimates and robust standard errors from the estimation (Calonico et al. 2017). I still present the point estimate and 95 percent confidence interval on my parameter of interest in figures, with additional information provided in appendix tables. I report the underlying Z statistic associated with the first stage estimation for each outcome as this varies across outcomes due to changes stemming from the optimal bandwidth selector. Since I only have one instrument, the Z statistic directly corresponds to the square root of the F statistic useful for understanding the strength of the instrument. The appendix tables also provide the reduced form estimates that correspond with the estimates from Equation 2, and Appendix Figure A2 presents reduced form regression discontinuity plots for key outcomes of interest. Where reasonable, I present results

for WPA workers and non-WPA workers separately. I do not have the statistical power to explore further heterogeneity, such as by race, in my analysis.³

IV.A. Impacts on WPA Employment

I begin by estimating the impact of pay on WPA employment using the methodology outlined in Equations 1 and 2. In line with the rest of the paper, I define WPA employment as male household heads ages 18 to 64 working in a public emergency program. Although working in a public emergency program does not perfectly capture WPA employment as noted when describing WPA wages, it is a very good proxy for WPA employment. Figure 7 Panel A presents the results of this regression, showing an elasticity of roughly 3 for WPA employment with respect to WPA wages in the first estimate. Row 2 uses a slightly different definition of WPA employment in line with the population restrictions for WPA wages, considering only those who had been at work in a public emergency program or unemployed for at least 65 weeks. The estimate for that WPA employment definition is slightly larger and less precise but in the same ballpark as the main estimate. Row 3 relies on county level aggregates from the 1937 unemployment census, which was mailed to every household in the U.S. Individuals unemployed or in a public emergency program were expected to respond. I continue to observe an elasticity of about 3 for data from 1937. The last two rows explore robustness to the baseline estimate by using wages for all WPA employees rather than restricting to those unemployed for at least 65 weeks and by using wages where only the top and bottom 1 percent of wages are excluded. The

³ As an example, see Appendix Table A15 for various results disaggregated for Black and White individuals. Although it appears as though the effects on WPA employment and schooling for 15- to 16-year-old boys are smaller for Black individuals, it's also the case that the methodology is unable to detect an increase in wages at the population threshold for the smaller subset of counties for which I observe Black WPA employment. It is therefore difficult to assess whether the impacts were indeed smaller or whether the methodology is ill suited for detecting effects among Black individuals.

estimates remain similar under these alternate specifications. It is worthwhile to note that the elasticities I present reflect the bias-corrected estimates from Calonico et al. (2017). The conventional instrumental variables regression discontinuity estimates still suggest a positive labor supply response to higher wages and fall within the confidence intervals suggested by my primary specification, but they are smaller and lose statistical significance, as seen in Appendix Table A5.

The labor supply elasticity estimates I uncover may have been influenced by barriers to additional WPA hiring faced by administrators. As described in Section II.A., imperfect federal forecasting and political processes led to budgetary shortfalls, and local sponsors' objectives sometimes differed from those of WPA officials (Howard 1943, pp. 292 and 567). These factors could have hindered the labor supply response to higher WPA wages, even though the WPA was often meeting needs in rural areas (Howard 1943, p. 607). Monopsony power and associated labor market frictions too could be at play, as has been observed in other settings with finite labor supply elasticities (e.g., Dal Bó et al. 2013; Dube et al. 2020; Emanuel and Harrington 2022; Ransom and Sims 2010; Staiger et al. 2010; Webber 2015). The labor supply elasticities faced by employers range from basically zero to nearly 10 in the literature, with estimates of 0.1, 0.1, 1.08, 2.15, 3.7, and 9.6. The elasticity I uncover falls in the middle of that range. As described in my introduction, this finding in consideration with the context of the WPA has potential implications for understanding when employers have wage setting power. For example, it appears as though slack labor market conditions, a defining characteristic of the New Deal era, might not matter as much as other factors for determining the labor supply elasticity faced by employers.

Figure 7 Panel B explores whether the composition of WPA employment responded to higher wages in addition to the count of WPA employment. In theory, higher wages could help the WPA attract or retain workers who are older and thus with more work experience or who have more education and thus skill (Katz 1986; Stiglitz 1976; Weiss 1980). However, the point estimates are small and statistically insignificant for all WPA worker characteristics in Figure 7, which are presented as their raw and not naturally logged values. A 100 percent increase in wages would only increase the average age of workers by four years and reduce the share with no high school education by less than 3 percentage points. I also observe limited impact on racial makeup of WPA workers in response to a wage increase. It thus appears that although higher wages increased WPA employment, they did not change the type of workers employed by the WPA.

IV.B. Impacts on Broader Economy

In Figure 8 Panel A, I study whether the increases in WPA employment were associated with declines in unemployment. Akin to my analysis on WPA employment, I examine three different unemployment outcome variables: 18- to 64-year-old males who were the heads of their household who were unemployed as of March 1940, those who were at work in a public emergency program or unemployed for at least 65 weeks and thus more likely to have been unemployed in 1939, and the unemployed population from the 1937 Census. For all three outcomes, the confidence intervals rule out large negative effects on unemployment that would coincide with the estimates for WPA employment. Because the unemployed and WPA employed populations were roughly similar in size in rural areas, as demonstrated in Table 2, these findings suggest that the estimated increase in WPA employment did not fully come out of the

unemployed population. This result is consistent with the existing literature on the WPA and similar New Deal programs which finds no reduction in unemployment (Benjamin and Matthews 1992; Fishback 2017; Fleck 1999; Liu and Fishback 2019; Neumann et al. 2010). It is also in line with the eligibility criteria outlined in section II generally limiting WPA employment from those who did not have a recent employment and may have been unemployed long term.

Prior research finds that the increases in WPA employment instead came from private employment (Benjamin and Matthews 1992; Neumann et al. 2010). This finding is consistent with the close link between private and WPA employment described in the background section. Unfortunately, my analysis is too underpowered to definitively determine whether WPA wages affected private employment. The last two estimates of Figure 8 Panel A examine the impacts of higher WPA wages on private employment overall and occupation weighted to align with WPA employment. Because the private employment level shown in Table 2 is more than six times as large as WPA employment, the wide confidence intervals in Figure 8 do not rule out reductions in private employment driving increases in WPA employment, but they also do not definitively suggest a decline in private employment. It is possible the increase in employment came from those out of the labor force, but this seems less likely given that my sample includes males ages 18-64 who were heads of households.

Figure 8 Panel B assesses whether higher WPA wages had spillovers onto private wages. WPA employment equated to about 15 percent of private employment, as shown in Table 2, making it a substantial force in the labor market that could have spillovers to private workers. At the same time the program was in response to an incredibly slack labor market, and WPA wages were well below private wages, also shown in Table 2, potentially dampening its impact. WPA wages were about 25 percent lower than wages in similar occupations, 30 percent lower than

wages for individuals with no high school degree, and 40 percent lower than wages overall. Studies examining settings that share certain similarities to the WPA have a range of findings related to wage spillovers. Wallis and Benjamin (1981) study a precursor to the WPA, the Federal Emergency Relief Administration (FERA), and find a null effect of FERA spending on private wages. Similarly, a paper studying the modern retail setting, which is relatively low paying and somewhat fixed in employment levels like the WPA, finds little evidence of wage spillovers (Derenoncourt and Weil 2024). On the other hand, Liu and Fishback (2017) find evidence of New Deal public works spending increasing earnings. Research studying another large-scale public hiring scheme in modern day India as well as research looking at another labor market force in the twentieth century, unions, has also found evidence of spillovers (Azam 2012; Berg et al. 2012; Budd 1992; Farber 2005; Imbert and Papp 2015).

The findings from Figure 8 Panel B are positive but imprecise, and they only rule out impacts larger than an equivalent increase in private wages for WPA wages. I focus my analysis on weekly wages, where the weeks are equivalent full-time weeks, and I trim the private wage variables similarly to the WPA wage variables to remove the top and bottom 5 percent of wages. Row 1 looks at all private sector weekly wages. Row 2 examines private sector weekly for those with no high school education. Row 3 examines weekly private wages that are weighted to mimic the occupational distribution of WPA workers. In all cases, the results are positive but statistically insignificant, with point estimates ranging from elasticities of 0.148 to 0.326, which would correspond with some spillovers. Still, the confidence intervals all exclude 1, ruling out large effects of equivalent increases in private wages for WPA wages.

IV.C. Impacts on Household Members

Even though I find limited evidence of higher WPA wages affecting broader labor economic outcomes, WPA wages could have still affected the economy through other means, such as by affecting WPA household members. In Figure 9, I assess how higher wages for WPA workers influenced the children in their households. I examine the educational choices of teens in households with WPA workers, with WPA workers defined as they are elsewhere in the paper. Increased WPA wages for household heads could have decreased the opportunity cost of schooling for teens and led to higher enrollment rates, especially for teenage boys on the verge of dropout who would feel otherwise compelled to join the labor market (Becker 1964). Higher WPA wages could have also increased the opportunity cost of schooling or reduced the returns to schooling and encouraged teens to drop out to obtain higher WPA wages. Other economic shocks that have favored less-skilled workers have been associated with declines in schooling when they potentially increase the opportunity cost of schooling or decrease the returns to it (Cascio and Narayan 2022; Charles et al. 2018). However, an increased opportunity cost seems unlikely in the setting of the WPA for many teens given that teens under the age of 18 were technically ineligible for WPA employment and typically only the head of the family could be employed by the WPA. In addition, it seems unlikely that the WPA wages affected the perceived longer-term returns to schooling since the WPA was a New Deal program tied to unemployment and not a longstanding labor market policy. To identify which teens would be most at risk of dropout, Appendix Table A16 shows school attendance rates by age and sex in 1940. During this time, most early dropout decisions for males were happening at the ages of 15 and 16. By age 16, about one-third of all teens were no longer enrolled in school.

In Figure 9, I find that this is the age group with the largest increase in school attendance, with an attendance elasticity of about 1.5. Teenage boys of other ages in WPA households did not

have a statistically significant change in enrollment. However, the point estimate for teenage boys ages 17 and 18 is sizable and negative, pointing to a potential increase in dropout for teens who may have had greater access to WPA employment. Teenage girls' schooling did not respond to higher WPA wages, as might be expected, and neither teenage boys nor teenage girls in households without any WPA workers experienced a change in their schooling outcomes. The increase in schooling for teenage boys on the margin of dropout in response to greater household income provides insights into when educational attainment responds to income shocks, as described in the introduction. It adds to the literature with varied findings by supporting the hypothesis that additional income is helpful when reducing opportunity costs for individuals during a time when they are making attainment decisions (Bulman et al. 2021; Carneiro et al. 2021; Caucutt and Lochner 2020; Hill 2016; Krause et al. 2025; Manoli and Turner 2018).

V. Conclusion

In this paper, I examine the impact of higher wages in the WPA, a historic large-scale direct public hiring program, by using a discontinuity in its wage schedule. Relying on data from the complete count Census, I find that higher wages for WPA workers led to greater employment in the WPA, but no change in the composition of that employment. I uncover no significant reductions in unemployment, and my results rule out equivalent increases in private wages, though smaller positive wage impacts may have been possible. However, I find evidence that higher wages may have had a longer-term impact on the economy. Higher WPA wages impacted teens in WPA households, increasing school enrollment for teen boys on the cusp of early dropout.

These results build economic insights that may have potential implications for modern public policy proposals (e.g., Center for American Progress 2018; Paul et al. 2018; Shakesprere and Nightingale 2019; Tcherneva 2018). By studying the effects of higher wages, the paper sheds light on an important design parameter of large-scale direct public hiring policies by empirically demonstrating the economic intuition underlying the trade-offs in setting this parameter. Higher wages likely induce those with better outside options to take part in the program. New research suggests that workers' outside options can influence the level of crowd out by public hiring programs, with those with more outside options experiencing more crowd out (Bee and Narayan 2025). Consistent with this economic intuition, I observe no significant reductions in unemployment in response to greater WPA employment induced by higher WPA wages. However, higher wages also allow workers to better support their families, as evidenced by reduced early school dropout among boys in WPA households. The study thus empirically illuminates the key trade-off faced by both WPA policymakers and potential future policymakers in determining how to set wages in public hiring programs: higher wages are less likely to reduce unemployment but also more likely to help workers support their families.

The study shows that this trade-off exists even in a setting with extremely high unemployment, a time when economic theory might suggest that firm wage setting power is greater due to workers having fewer outside options (Cumming 2022). Other factors that defined the WPA era labor market, such as high geographic mobility, may instead matter more for determining employer monopsony power. Still, this paper shows that even under extremely slack labor market conditions, the labor supply elasticity faced by employers, including the government, can be rather elastic. Policymakers seeking to reduce unemployment may instead need to consider additional policy parameters when designing large-scale public hiring policies

that have sufficiently high wages for supporting families. For example, WPA eligibility criteria included a requirement for need. In the case of the WPA, this requirement was likely insufficient for preventing crowd out, but modern policymakers may have other tools for targeting workers at their disposal. The results from this study suggest that such tools may be important when designing large-scale public hiring programs.

Additional research is needed to determine how large-scale public hiring policies can be best designed across various parameters and contexts. This paper examines the role of higher wages in the WPA, which was a program created in the 1930s during the Great Depression, hired millions of workers for public works projects, had fairly strict eligibility criteria, and paid workers relatively low wages. The effects of other large-scale public hiring policies and their wages might differ from what I observe based on their policy design. More research examining both historical and modern public hiring programs would further build understanding of large-scale public hiring policies.

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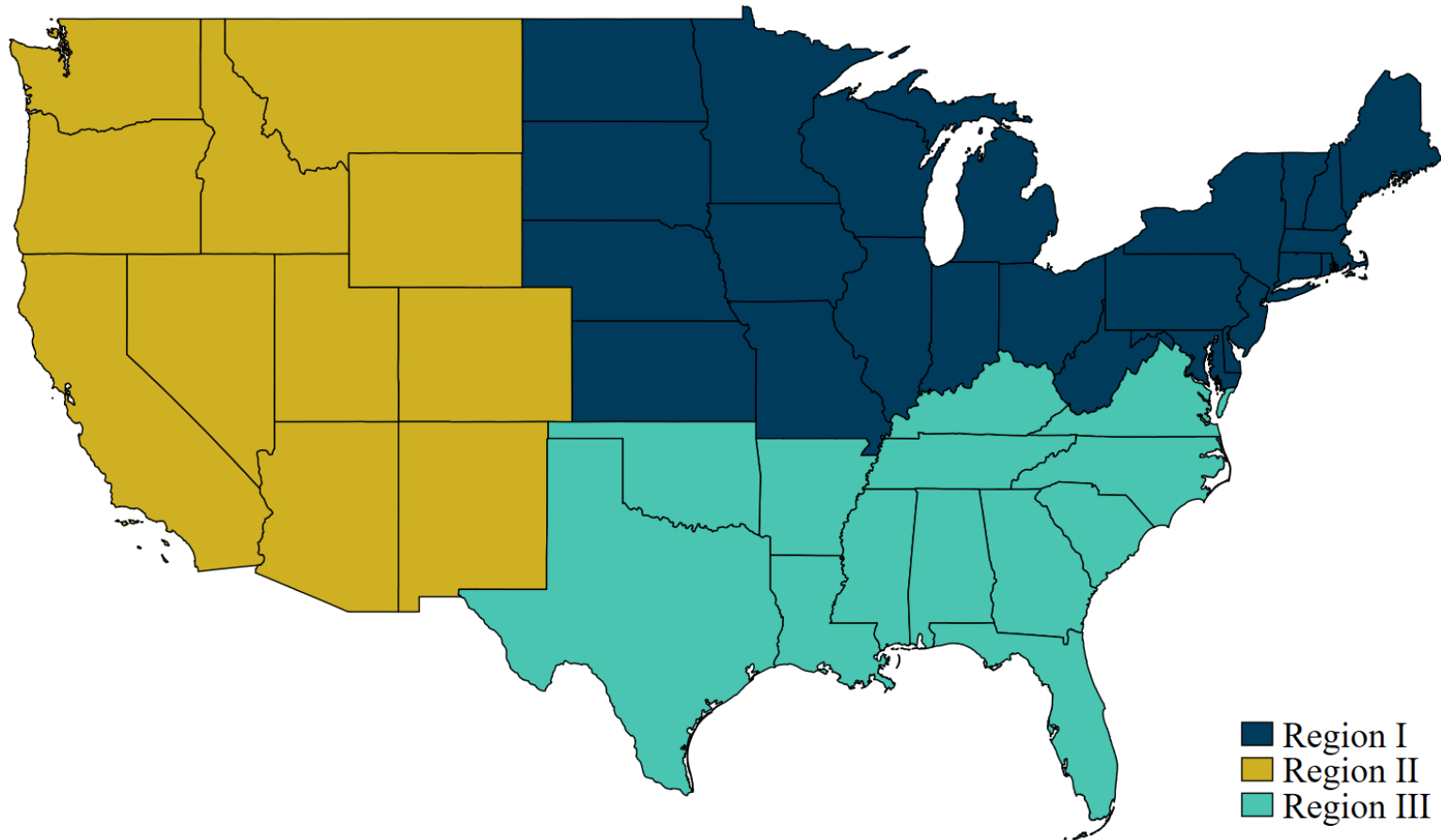
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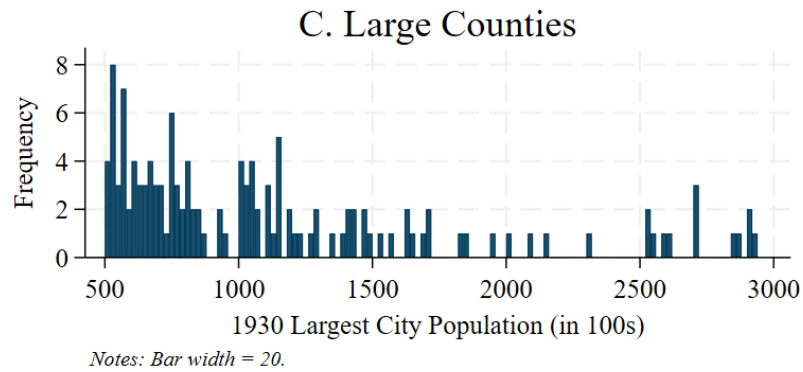
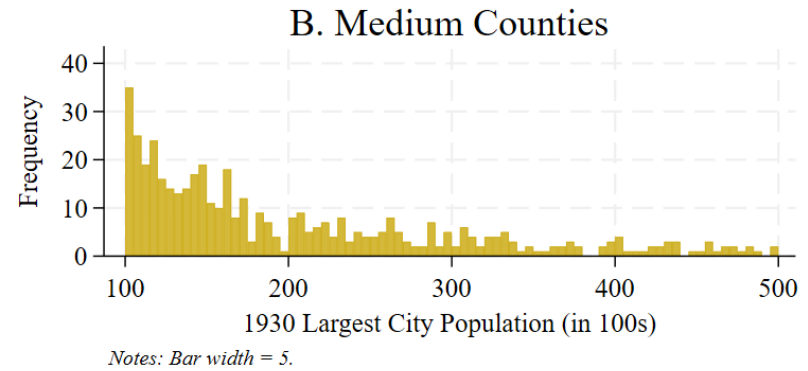
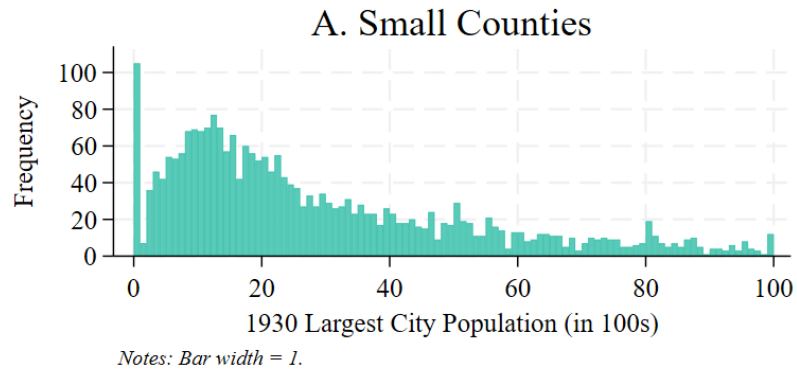
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Figure 1. WPA Wage Regions for the 1939 Wage Schedule



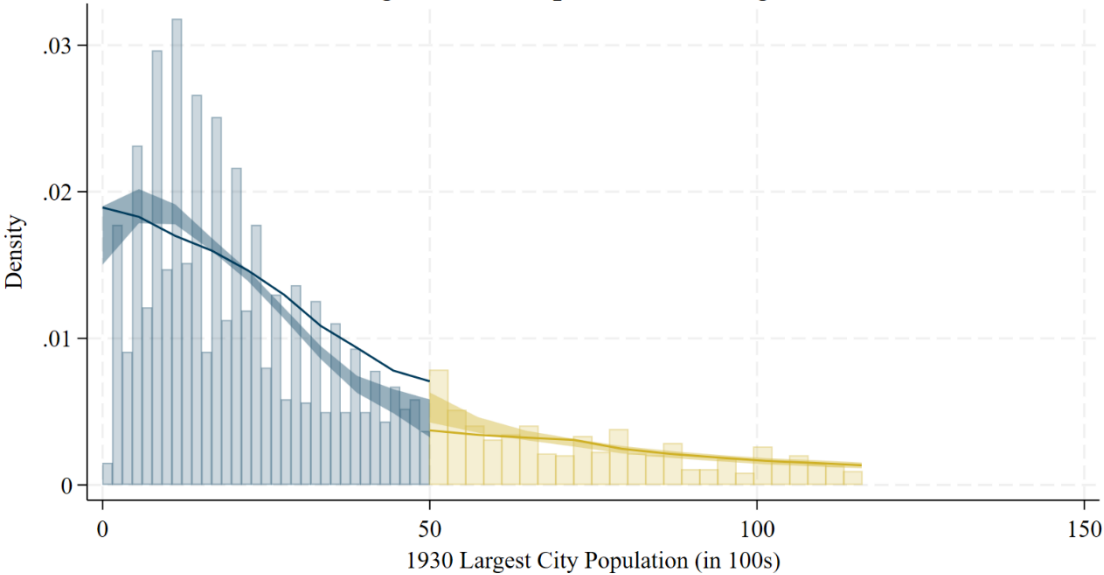
Sources: 1939 WPA regions are from Burns and Kerr (1941).

Figure 2. Histograms of Counties by Population of Largest City in 1930



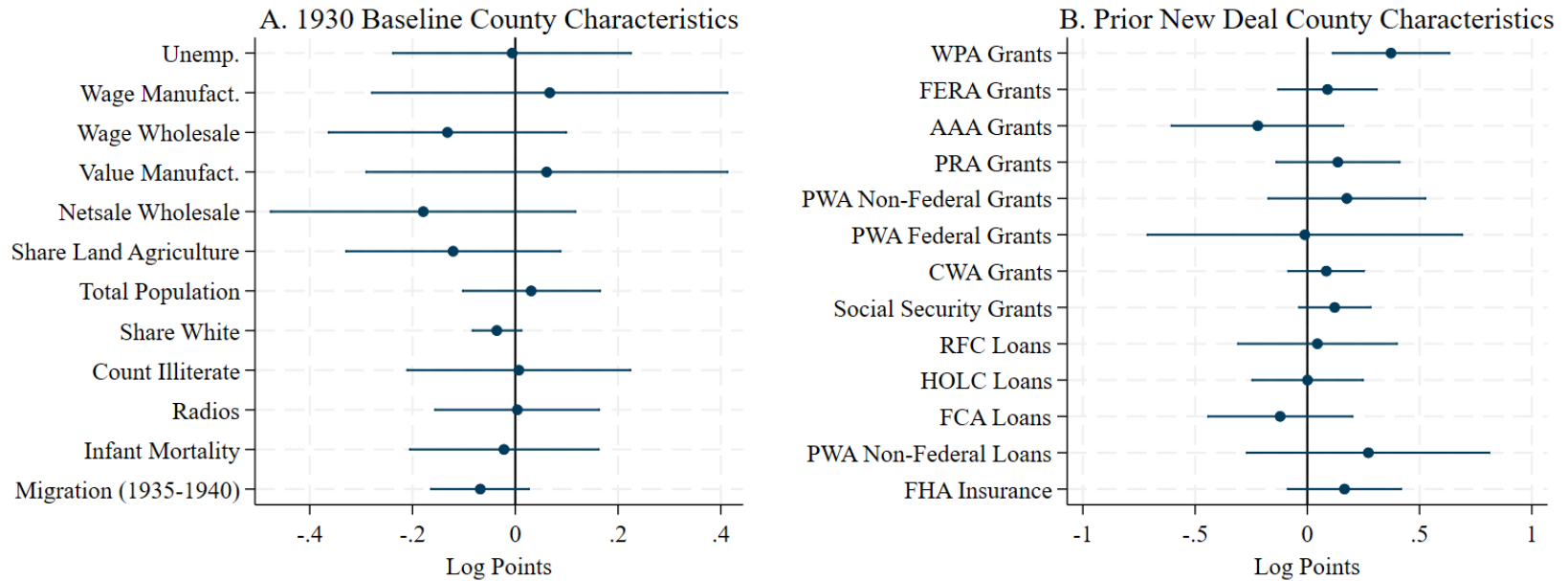
Sources: Data are from the 1930 complete count Census (Ruggles et al. 2024).
Notes: Figure excludes 29 counties where the largest city is larger than 300,000.

Figure 3. Manipulation Testing Plot



Sources: Data are from the 1930 complete count Census (Ruggles et al. 2024).

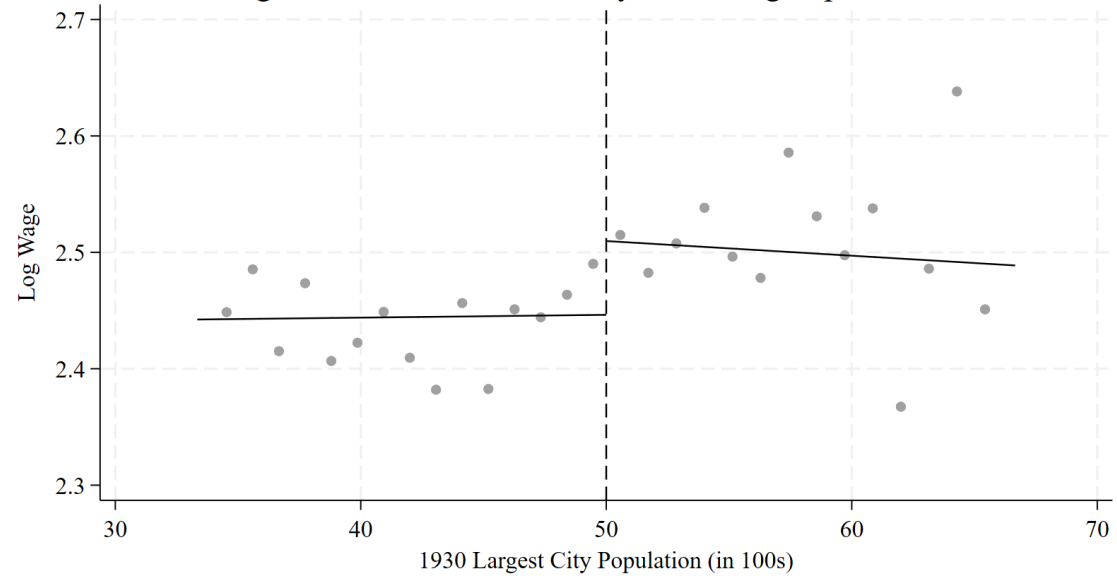
Figure 4. Regression Estimates of Discontinuity on Baseline Characteristics



Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024), from Fishback, Kantor, and Wallis (2003), and from 1930 Census estimates.

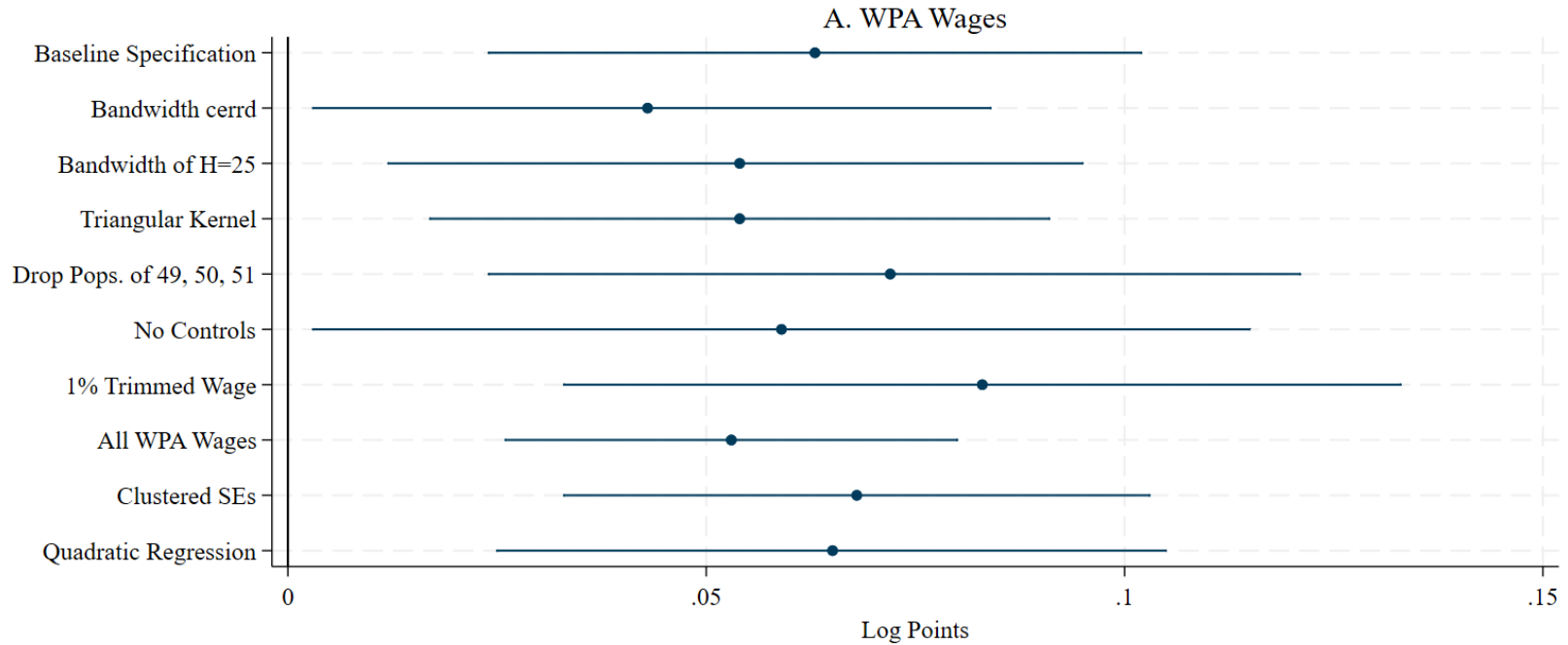
Notes: Unit of observation is county. Outcomes are logged. Baseline 1930 data are from published 1930 Census estimates except for migration, which is for the years 1935-1940 and is derived from the 1940 complete count Census. Data for New Deal spending are for the years 1933-1939 and come from Fishback, Kantor, and Wallis (2003). Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators in the 1940 sample for consistency. Running variable is 1930 largest city population in 100s.

Figure 5. RD Plot for Primary WPA Wage Specification



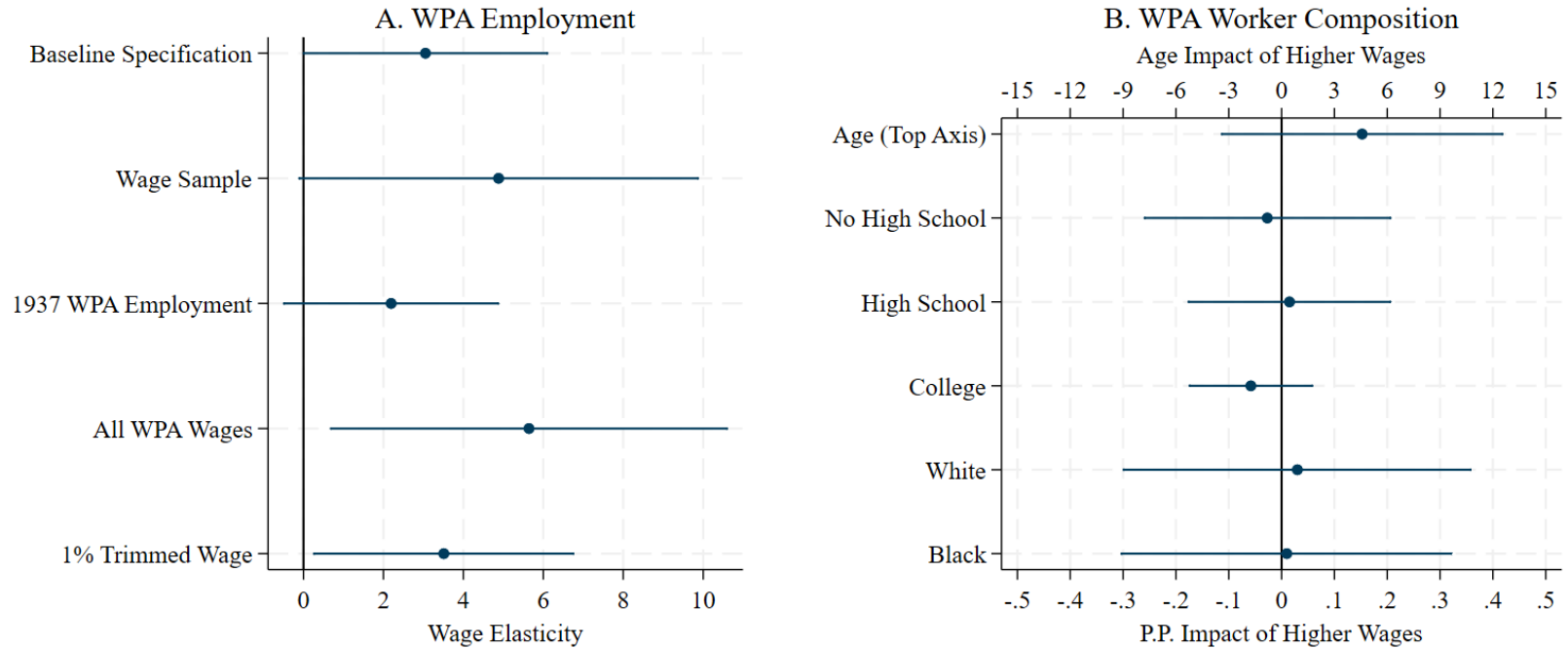
Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Figure 6. Regression Estimates of Discontinuity on WPA Wage



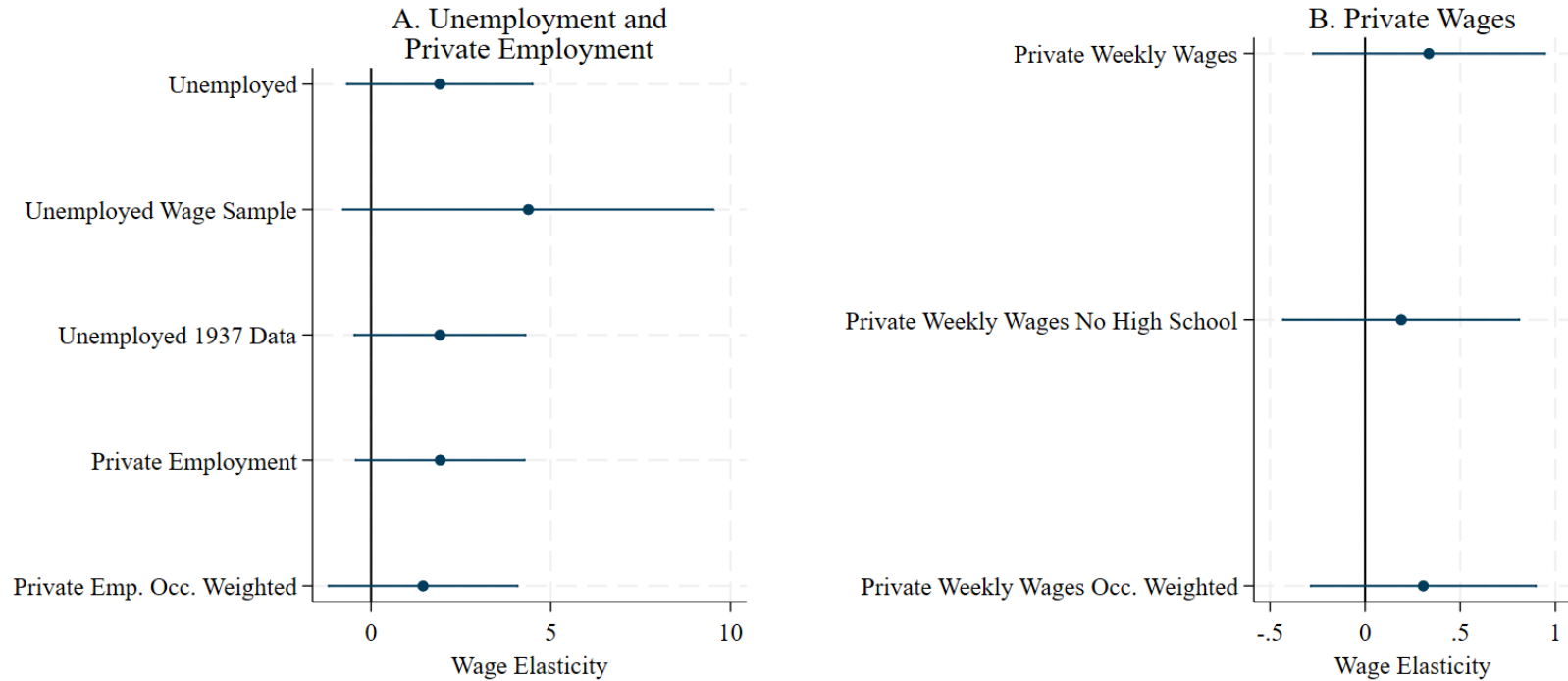
Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).
 Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$) (unless otherwise noted). Regressions use a uniform kernel (unless otherwise noted), and the bandwidth selection procedure uses the `mserd` one common MSE-optimal bandwidth selector (unless otherwise noted). Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators (unless otherwise noted). Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Figure 7. Regression Estimates of WPA Wages on WPA Employment



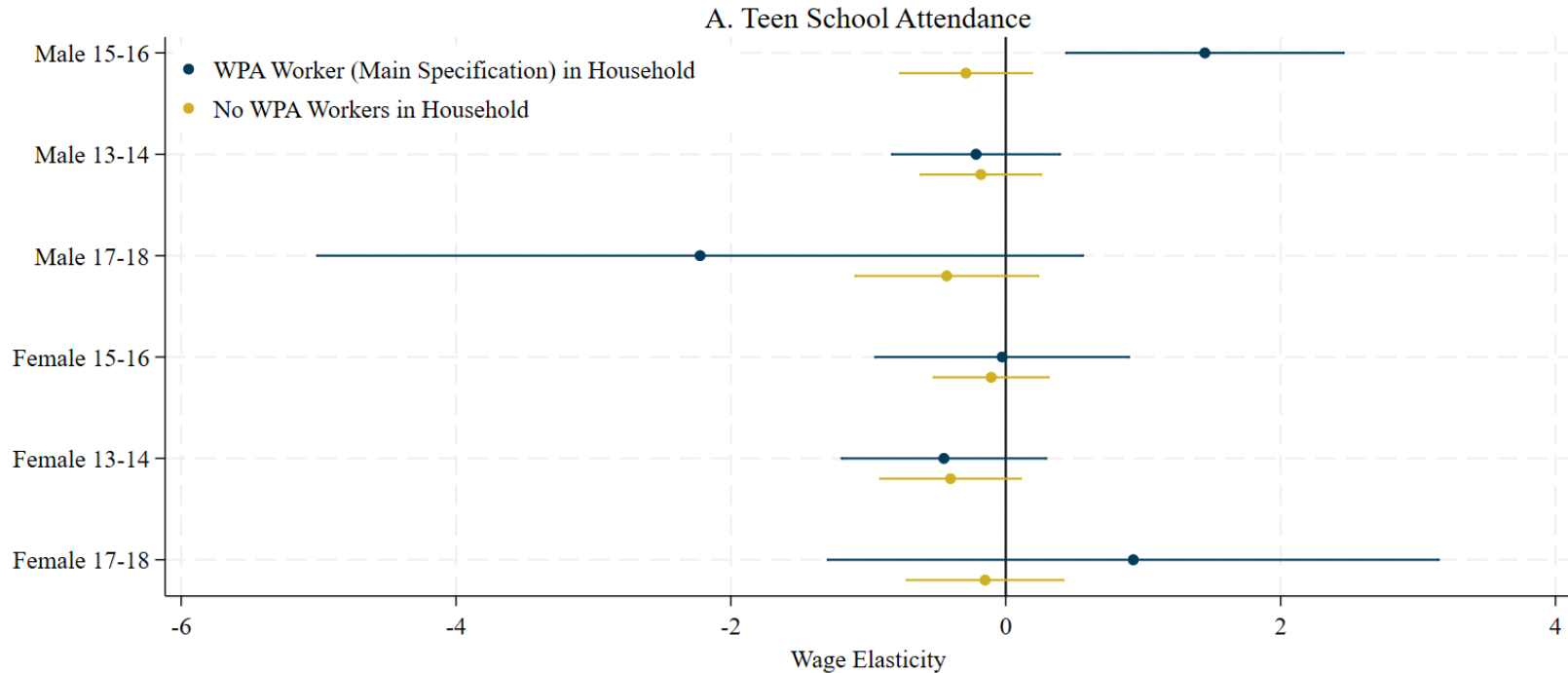
Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024) and the 1937 unemployment Census. Notes: Unit of observation is county. Outcome variables are logged except for characteristic variables. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Figure 8. Regression Estimates of WPA Wages on Other Employment



Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024) and the 1937 unemployment Census. Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Figure 9. Regression Estimates of WPA Wages on Children in WPA Households



Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Table 1 -
1939 WPA Monthly Pay Schedule

| 1930 Population of Largest Municipality in County | Wage Class | | | | |
|---|---------------|---------------|--------------|---------|-------------------------------|
| | Unskilled "B" | Unskilled "A" | Intermediate | Skilled | Professional and Technical |
| <u>Wage Region I (North)</u> | | | | | |
| 100,000 or more | 52.00 | 57.20 | 68.90 | 89.70 | 94.90 |
| 25,000 to 100,000 | 48.10 | 52.00 | 62.40 | 81.90 | 84.50 |
| 5,000 to 25,000 | 42.90 | 48.10 | 57.20 | 74.10 | 76.70 |
| Fewer than 5,000 | 39.00 | 42.90 | 52.00 | 67.60 | 68.90 |
| <u>Wage Region II (West)</u> | | | | | |
| 100,000 or more | 52.00 | 57.20 | 68.90 | 89.70 | 94.90 |
| 25,000 to 100,000 | 48.10 | 52.00 | 62.40 | 81.90 | 84.50 |
| 5,000 to 25,000 | 46.80 | 50.70 | 61.10 | 79.30 | 81.90 |
| Fewer than 5,000 | 44.20 | 49.40 | 59.80 | 76.70 | 78.00 |
| <u>Wage Region III (South)</u> | | | | | |
| 100,000 or more | 46.80 | 50.70 | 61.10 | 79.30 | 81.90 |
| 25,000 to 100,000 | 42.90 | 48.10 | 57.20 | 74.10 | 75.40 |
| 5,000 to 25,000 | 36.40 | 40.30 | 48.10 | 62.40 | 65.00 |
| Fewer than 5,000 | 31.20 | 35.10 | 42.90 | 54.60 | 55.90 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Exception to the schedule above are certain metropolitan districts near large municipalities where the schedule for 100,000 or more was applied for the whole area. Source is Burns and Kerr (1939).

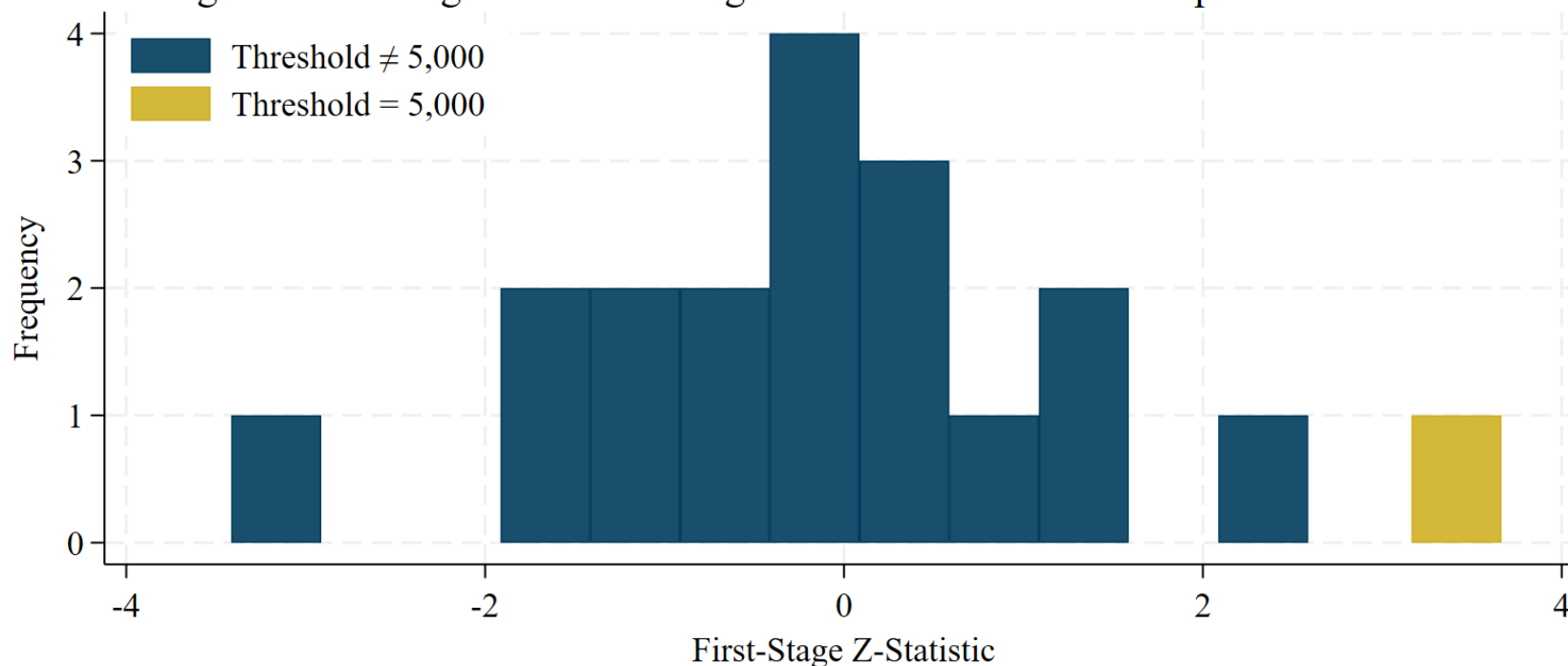
Table 2 -
Summary Statistics for 1940 Sample

| | Mean | Std. Dev. |
|--------------------------------------|--------|-----------|
| | (1) | (2) |
| WPA weekly wage | 11.823 | 2.175 |
| Private weekly wage | 20.003 | 4.808 |
| Private weekly wage (occ. weighted) | 15.967 | 4.213 |
| Private weekly wage (no high school) | 16.949 | 5.008 |
| WPA employment | 194 | 197 |
| Unemployment | 144 | 182 |
| Private employment | 1200 | 1363 |
| Share no high school | 0.669 | 0.136 |
| Share high school | 0.233 | 0.094 |
| Share college | 0.074 | 0.037 |
| Age | 41.9 | 1.7 |
| Share White | 0.891 | 0.176 |
| Share Black | 0.102 | 0.174 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Data are restricted to counties where the 1930 largest city size is less than 10,000. Underlying sample is males ages 18 to 64 who are heads of household.

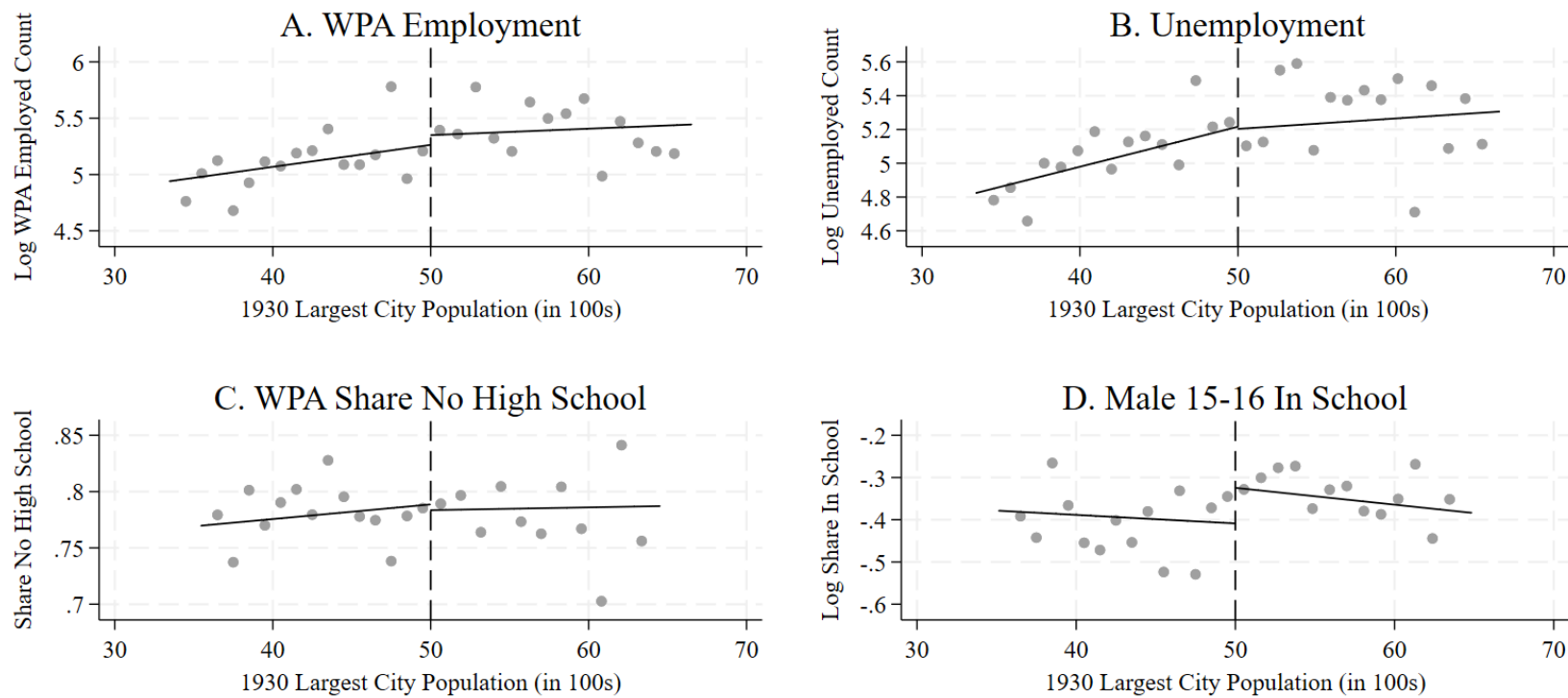
Figure A1. Histogram of First-Stage Z-Statistics at Various Population Thresholds



Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Z-statistics are derived from estimates of Equation 1 with population thresholds in 500 increments ranging from 1,000 to 10,000 using WPA wages as the outcome. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the `mserd` one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population

Figure A2. Reduced Form RD Plots of Key Outcome Variables



Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators. Running variable is 1930 largest city population in 100s.

Table A1 -
Impact of Discontinuity on WPA Wages at Various Thresholds

| | 5,000 Pop. Discontinuity, No Controls | 5,000 Pop. Discontinuity, Controls | 25,000 Pop. Discontinuity, No Controls | 25,000 Pop. Discontinuity, Controls | 100,000 Pop. Discontinuity, No Controls | 100,000 Pop. Discontinuity, Controls |
|------------------------------|---|--|--|---|---|--|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-corrected estimate | 0.059 | 0.063 | 0.065 | 0.007 | 0.007 | 0.171 |
| Robust standard error | 0.028 | 0.020 | 0.036 | 0.015 | 0.056 | 0.186 |
| Robust 95% conf. interval | [.003 ; .115] | [.024 ; .102] | [-.006 ; .137] | [-.023 ; .037] | [-.103 ; .118] | [-.194 ; .536] |
| Conventional estimate | 0.063 | 0.063 | 0.061 | 0.008 | 0.013 | 0.171 |
| Conventional standard error | 0.024 | 0.017 | 0.032 | 0.015 | 0.051 | 0.186 |
| Effective observations left | 337 | 307 | 111 | 24 | 26 | 3 |
| Effective observations right | 234 | 229 | 73 | 25 | 27 | 14 |
| BW loc. poly. (<i>h</i>) | 17.489 | 16.659 | 88.651 | 24.237 | 286.003 | 110.392 |
| BW bias (<i>b</i>) | 30.483 | 29.693 | 164.225 | 156.065 | 613.203 | 590.067 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A2 -
Discontinuities in Baseline County Characteristics

| | A. 1930 Economic Characteristics | | | | | |
|------------------------------|----------------------------------|-------------------|-------------------|--------------------|----------------------|---------------------------|
| | Unemp. | Wage Manufact. | Wage Wholesale | Value Manufact. | Netsale Wholesale | Share Land Agriculture |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-corrected estimate | -0.006 | 0.067 | -0.132 | 0.061 | -0.179 | -0.121 |
| Robust standard error | 0.118 | 0.176 | 0.118 | 0.179 | 0.151 | 0.106 |
| Robust 95% conf. interval | [-.238 ; .225] | [-.279 ; .413] | [-.363 ; .099] | [-.29 ; .413] | [-.476 ; .117] | [-.329 ; .088] |
| Conventional estimate | -0.022 | 0.030 | -0.091 | 0.036 | -0.139 | -0.093 |
| Conventional standard error | 0.102 | 0.154 | 0.103 | 0.156 | 0.129 | 0.096 |
| Effective observations left | 318.000 | 222.000 | 312.000 | 222.000 | 312.000 | 204.000 |
| Effective observations right | 232.000 | 187.000 | 229.000 | 187.000 | 229.000 | 177.000 |
| BW loc. poly. (<i>h</i>) | 16.424 | 13.876 | 16.695 | 13.735 | 16.654 | 11.624 |
| BW bias (<i>b</i>) | 30.558 | 24.219 | 30.614 | 24.828 | 28.274 | 23.716 |
| | B. 1930 Other Characteristics | | | | | |
| | Total Population | Share White | Count Illiterate | Radios | Infant Mortality | Migration (1935-1940) |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| Bias-corrected estimate | 0.031 | -0.036 | 0.007 | 0.004 | -0.022 | -0.068 |
| Robust standard error | 0.068 | 0.024 | 0.111 | 0.081 | 0.094 | 0.049 |
| Robust 95% conf. interval | [-.102 ; .165] | [-.083 ; .012] | [-.21 ; .224] | [-.156 ; .163] | [-.205 ; .162] | [-.164 ; .027] |
| Conventional estimate | 0.025 | -0.032 | 0.001 | -0.004 | -0.014 | -0.056 |
| Conventional standard error | 0.059 | 0.021 | 0.098 | 0.069 | 0.081 | 0.043 |
| Effective observations left | 295.000 | 349.000 | 295.000 | 295.000 | 287.000 | 318.000 |
| Effective observations right | 220.000 | 237.000 | 221.000 | 221.000 | 206.000 | 232.000 |
| BW loc. poly. (<i>h</i>) | 15.063 | 17.217 | 15.973 | 15.015 | 16.655 | 16.841 |
| BW bias (<i>b</i>) | 28.237 | 30.074 | 29.145 | 26.125 | 29.237 | 31.622 |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and 1930 Census estimates.

Notes: Unit of observation is county. Outcomes are logged. All outcome data are from published 1930 Census estimates except for migration, which is for the years 1935-1940 and is derived from the 1940 complete count Census. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators in the 1940 sample for consistency. Running variable is 1930 largest city population in 100s.

Table A3 -
Discontinuities in Prior New Deal County Characteristics

| | A. Largest New Deal Grant Programs | | | | | | | |
|---|------------------------------------|----------------|----------------|----------------|----------------|----------------|----------------|-----------------|
| | WPA | FERA | AAA | PRA | PWA | Non- | CWA | Social Security |
| | | | | | Federal | PWA Federal | | |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | |
| Bias-corrected estimate | 0.373 | 0.090 | -0.221 | 0.136 | 0.176 | -0.011 | 0.085 | 0.122 |
| Robust standard error | 0.133 | 0.112 | 0.196 | 0.140 | 0.179 | 0.358 | 0.087 | 0.081 |
| Robust 95% conf. interval | [.113 ; .633] | [-.13 ; .31] | [-.605 ; .163] | [-.138 ; .411] | [-.174 ; .526] | [-.712 ; .690] | [-.085 ; .254] | [-.037 ; .282] |
| Conventional estimate | 0.341 | 0.077 | -0.140 | 0.159 | 0.184 | -0.130 | 0.061 | 0.102 |
| Conventional standard error | 0.118 | 0.098 | 0.174 | 0.118 | 0.152 | 0.291 | 0.076 | 0.071 |
| Effective observations left | 314.000 | 291.000 | 200.000 | 345.000 | 374.000 | 216.000 | 344.000 | 314.000 |
| Effective observations right | 228.000 | 217.000 | 177.000 | 233.000 | 227.000 | 101.000 | 232.000 | 228.000 |
| BW loc. poly. (<i>h</i>) | 16.958 | 15.856 | 11.297 | 17.552 | 19.081 | 28.662 | 17.929 | 16.481 |
| BW bias (<i>b</i>) | 32.278 | 29.967 | 22.361 | 29.449 | 32.787 | 43.273 | 32.663 | 30.161 |
| B. Largest New Deal Loan and Insurance Programs | | | | | | | | |
| | RFC | HOLC | FCA | PWA | Non- | | | |
| | | | | Federal | FHA Insurance | | | |
| | (9) | (10) | (11) | (12) | (13) | | | |
| Bias-corrected estimate | 0.045 | 0.001 | -0.121 | 0.272 | 0.166 | | | |
| Robust standard error | 0.181 | 0.126 | 0.164 | 0.276 | 0.129 | | | |
| Robust 95% conf. interval | [-.309 ; .399] | [-.245 ; .248] | [-.442 ; .201] | [-.27 ; .814] | [-.087 ; .42] | | | |
| Conventional estimate | 0.071 | 0.041 | -0.083 | 0.293 | 0.119 | | | |
| Conventional standard error | 0.159 | 0.108 | 0.145 | 0.237 | 0.118 | | | |
| Effective observations left | 324.000 | 262.000 | 176.000 | 192.000 | 263.000 | | | |
| Effective observations right | 223.000 | 207.000 | 169.000 | 127.000 | 207.000 | | | |
| BW loc. poly. (<i>h</i>) | 17.934 | 14.378 | 10.519 | 19.874 | 14.280 | | | |
| BW bias (<i>b</i>) | 33.731 | 25.863 | 20.863 | 33.886 | 30.832 | | | |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and from Fishback, Kantor, and Wallis (2003).

Notes: Unit of observation is county. Outcomes are logged. Data for New Deal spending are for the years 1933-1939 and come from Fishback, Kantor, and Wallis (2003). Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, share no high school, high school, college, White, and Black as well as state indicators in the 1940 sample for consistency. Running variable is 1930 largest city population in 100s.

Table A4 -
Impact of Discontinuity on WPA Wages

| | Baseline Specification | Bandwidth cerrd | Bandwidth of H=25 | Triangular Kernel | Drop Pops. of 49, 50, 51 |
|------------------------------|---------------------------|--------------------|----------------------|----------------------|-----------------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Bias-corrected estimate | 0.063 | 0.043 | 0.054 | 0.054 | 0.072 |
| Robust standard error | 0.020 | 0.021 | 0.021 | 0.019 | 0.025 |
| Robust 95% conf. interval | [.024 ; .102] | [.003 ; .084] | [.012 ; .095] | [.017 ; .091] | [.024 ; .121] |
| Conventional estimate | 0.063 | 0.044 | 0.061 | 0.057 | 0.069 |
| Conventional standard error | 0.017 | 0.020 | 0.014 | 0.016 | 0.021 |
| Effective observations left | 307 | 198 | 570 | 451 | 291 |
| Effective observations right | 229 | 175 | 298 | 262 | 181 |
| BW loc. poly. (h) | 16.659 | 11.183 | 25.000 | 21.685 | 16.524 |
| BW bias (b) | 29.693 | 29.693 | 25.000 | 33.673 | 32.014 |
| | No Controls | 1% Trimmed Wage | All WPA Wages | Clustered SEs | Quadratic Regression |
| | (6) | (7) | (8) | (9) | (10) |
| Bias-corrected estimate | 0.059 | 0.083 | 0.053 | 0.068 | 0.0651 |
| Robust standard error | 0.028 | 0.026 | 0.014 | 0.018 | 0.0205 |
| Robust 95% conf. interval | [.003 ; .115] | [.033 ; .133] | [.026 ; .08] | [.033 ; .103] | [.025 ; .105] |
| Conventional estimate | 0.063 | 0.080 | 0.054 | 0.068 | 0.0666 |
| Conventional standard error | 0.024 | 0.023 | 0.012 | 0.016 | 0.0192 |
| Effective observations left | 337 | 237 | 318 | 284 | 849 |
| Effective observations right | 234 | 196 | 232 | 219 | 350 |
| BW loc. poly. (h) | 17.489 | 13.887 | 16.221 | 15.658 | 31.92 |
| BW bias (b) | 30.483 | 28.310 | 29.935 | 30.862 | 69254 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$) (unless otherwise noted). Regressions use a uniform kernel (unless otherwise noted), and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector (unless otherwise noted). Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators (unless otherwise noted). Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A5 -
Impact of WPA Wages on WPA Employment

| | Baseline Specification | Wage Sample | 1937 WPA Employment | All WPA Wages | 1% Trimmed Wage |
|------------------------------|---------------------------|-----------------|------------------------|------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Bias-corrected estimate | 3.052 | 4.882 | 2.194 | 5.643 | 3.511 |
| Robust standard error | 1.555 | 2.544 | 1.368 | 2.527 | 1.656 |
| Robust 95% conf. interval | [.005 ; 6.099] | [-.104 ; 9.868] | [-.486 ; 4.875] | [.691 ; 10.595] | [.265 ; 6.757] |
| Conventional estimate | 1.596 | 3.621 | 0.749 | 4.095 | 2.736 |
| Conventional standard error | 1.361 | 2.259 | 1.215 | 2.430 | 1.511 |
| Effective observations left | 748 | 702 | 747 | 431 | 454 |
| Effective observations right | 320 | 313 | 316 | 257 | 264 |
| BW loc. poly. (h) | 29.191 | 28.003 | 29.684 | 20.156 | 21.989 |
| BW bias (b) | 73.392 | 79.046 | 108.177 | 200.727 | 48.081 |
| Robust 1st stage Z-score | 3.965 | 4.011 | 4.175 | 4.016 | 3.692 |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and the 1937 unemployment census.

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Table A6 -
Impact of WPA Wages on WPA Worker Composition

| | Age | No High School | High School | College | White | Black |
|------------------------------|-----------------|----------------|----------------|----------------|----------------|----------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-corrected estimate | 4.575 | -0.027 | 0.015 | -0.058 | 0.030 | 0.010 |
| Robust standard error | 4.064 | 0.119 | 0.098 | 0.059 | 0.167 | 0.160 |
| Robust 95% conf. interval | [-3.39 ; 12.54] | [-.259 ; .206] | [-.176 ; .206] | [-.174 ; .058] | [-.299 ; .358] | [-.303 ; .322] |
| Conventional estimate | 2.123 | -0.062 | 0.041 | -0.046 | -0.016 | 0.023 |
| Conventional standard error | 3.624 | 0.108 | 0.088 | 0.054 | 0.149 | 0.143 |
| Effective observations left | 534 | 477 | 509 | 509 | 534 | 477 |
| Effective observations right | 289 | 270 | 280 | 280 | 289 | 270 |
| BW loc. poly. (h) | 24.361 | 22.981 | 23.839 | 23.608 | 24.828 | 22.308 |
| BW bias (b) | 49.125 | 48.753 | 47.698 | 50.487 | 47.211 | 44.236 |
| Robust 1st stage Z-score | 3.887 | 3.945 | 3.798 | 3.761 | 3.772 | 3.674 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are not logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Table A7 -
Impact of WPA Wages on Unemployment and Private Employment

| | Unemployed | Unemployed Wage Sample | Unemployed 1937 Data | Private Employment | Private Emp. Occ. Weighted |
|------------------------------|-----------------|---------------------------|-------------------------|-----------------------|-------------------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Bias-corrected estimate | 1.913 | 4.377 | 1.915 | 1.923 | 1.448 |
| Robust standard error | 1.315 | 2.631 | 1.213 | 1.198 | 1.341 |
| Robust 95% conf. interval | [-.664 ; 4.491] | [-.78 ; 9.535] | [-.462 ; 4.292] | [-.424 ; 4.27] | [-1.18 ; 4.077] |
| Conventional estimate | 1.263 | 3.471 | 1.240 | 1.027 | 0.311 |
| Conventional standard error | 1.218 | 2.353 | 1.140 | 1.094 | 1.274 |
| Effective observations left | 451 | 412 | 476 | 389 | 389 |
| Effective observations right | 262 | 251 | 267 | 245 | 245 |
| BW loc. poly. (<i>h</i>) | 21.846 | 20.914 | 22.306 | 19.032 | 19.442 |
| BW bias (<i>b</i>) | 50.244 | 40.643 | 79.901 | 41.750 | 54.253 |
| Robust 1st stage Z-score | 3.861 | 3.298 | 3.997 | 3.187 | 3.356 |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and the 1937 unemployment census.

*Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.*

Table A8 -
Impact of WPA Wages on Private Wages

| | Private Weekly Wages | Private Weekly Wages No High School | Private Weekly Wages Occ. Weighted |
|------------------------------|----------------------------|--|---|
| | (1) | (2) | (3) |
| Bias-corrected estimate | 0.335 | 0.190 | 0.306 |
| Robust standard error | 0.311 | 0.317 | 0.302 |
| Robust 95% conf. interval | [-.274 ; .945] | [-.431 ; .810] | [-.285 ; .897] |
| Conventional estimate | 0.219 | 0.204 | 0.235 |
| Conventional standard error | 0.273 | 0.281 | 0.256 |
| Effective observations left | 534 | 647 | 570 |
| Effective observations right | 289 | 307 | 298 |
| BW loc. poly. (h) | 24.425 | 27.907 | 25.198 |
| BW bias (b) | 46.395 | 61.807 | 44.747 |
| Robust 1st stage Z-score | 3.635 | 4.436 | 3.616 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Table A9 -
Impact of WPA Wages on Teen School Attendance

| | A. WPA Worker (as in Main Specification) in Household | | | | | |
|------------------------------|---|----------------|-----------------|-----------------|-----------------|-----------------|
| | Male 15-16 | Male 13-14 | Male 17-18 | Female 15-16 | Female 13-14 | Female 17-18 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-Corrected Estimate | 1.449 | -0.216 | -2.224 | -0.026 | -0.450 | 0.928 |
| Robust Standard Error | 0.515 | 0.312 | 1.422 | 0.471 | 0.380 | 1.134 |
| Robust 95% Conf. Interval | [.44 ; 2.458] | [-.827 ; .394] | [-5.011 ; .563] | [-.950 ; .898] | [-1.194 ; .294] | [-1.295 ; 3.15] |
| Conventional Estimate | 1.260 | -0.417 | -1.522 | -0.221 | -0.339 | 0.986 |
| Conventional Standard Error | 0.462 | 0.292 | 1.222 | 0.405 | 0.324 | 0.993 |
| Effective Observations Left | 528 | 336 | 344 | 696 | 387 | 291 |
| Effective Observations Right | 289 | 234 | 235 | 313 | 245 | 222 |
| BW Loc. Poly. (<i>h</i>) | 24.186 | 17.472 | 18.521 | 28.897 | 19.358 | 16.910 |
| BW Bias (<i>b</i>) | 55.700 | 48.788 | 32.300 | 71.354 | 34.697 | 30.789 |
| Robust 1st Stage Z-Score | 4.135 | 3.225 | 2.852 | 4.449 | 3.022 | 3.499 |
| | B. No WPA Workers in Household | | | | | |
| | Male 15-16 | Male 13-14 | Male 17-18 | Female 15-16 | Female 13-14 | Female 17-18 |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| Bias-corrected estimate | -0.290 | -0.181 | -0.429 | -0.106 | -0.402 | -0.150 |
| Robust standard error | 0.245 | 0.224 | 0.340 | 0.213 | 0.261 | 0.291 |
| Robust 95% conf. interval | [-.77 ; .191] | [-.62 ; .258] | [-1.094 ; .237] | [-.525 ; .312] | [-.914 ; .11] | [-.721 ; .421] |
| Conventional estimate | -0.352 | -0.280 | -0.545 | -0.219 | -0.496 | -0.204 |
| Conventional standard error | 0.226 | 0.215 | 0.303 | 0.194 | 0.251 | 0.271 |
| Effective observations left | 451 | 307 | 606 | 509 | 337 | 451 |
| Effective observations right | 262 | 229 | 303 | 280 | 234 | 262 |
| BW loc. poly. (<i>h</i>) | 21.110 | 16.186 | 26.472 | 23.976 | 17.035 | 21.883 |
| BW bias (<i>b</i>) | 51.022 | 53.051 | 65.936 | 63.370 | 61.640 | 51.676 |
| Robust 1st stage Z-score | 3.872 | 3.538 | 4.299 | 3.868 | 3.206 | 3.881 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the msrd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. WPA wages are instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable.

Table A10 -
Impact of (Reduced Form) Discontinuity on WPA Employment

| | Baseline | 1937 WPA | |
|------------------------------|----------------|---------------|----------------|
| | Specification | Wage Sample | Employment |
| | (1) | (2) | (3) |
| Bias-corrected estimate | 0.094 | 0.396 | 0.176 |
| Robust standard error | 0.134 | 0.192 | 0.119 |
| Robust 95% conf. interval | [-.168 ; .357] | [.019 ; .773] | [-.057 ; .409] |
| Conventional estimate | 0.086 | 0.364 | 0.152 |
| Conventional standard error | 0.115 | 0.166 | 0.105 |
| Effective observations left | 318.000 | 366.000 | 293.000 |
| Effective observations right | 232.000 | 245.000 | 219.000 |
| BW loc. poly. (h) | 16.556 | 18.728 | 15.770 |
| BW bias (b) | 28.302 | 33.644 | 29.291 |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and the 1937 unemployment census.

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A11 -
Impact of (Reduced Form) Discontinuity on WPA Worker Composition

| | Age | No High School | High School | College | White | Black |
|------------------------------|----------------|----------------|----------------|----------------|---------------|----------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-corrected estimate | 0.046 | -0.007 | 0.006 | 0.002 | 0.007 | -0.010 |
| Robust standard error | 0.321 | 0.012 | 0.008 | 0.007 | 0.012 | 0.012 |
| Robust 95% conf. interval | [-.583 ; .675] | [-.03 ; .015] | [-.009 ; .021] | [-.011 ; .016] | [-.016 ; .03] | [-.033 ; .013] |
| Conventional estimate | 0.034 | -0.005 | 0.006 | 0.000 | 0.008 | -0.010 |
| Conventional standard error | 0.278 | 0.010 | 0.006 | 0.006 | 0.010 | 0.010 |
| Effective observations left | 295 | 267 | 295 | 267 | 267 | 267 |
| Effective observations right | 221 | 210 | 221 | 210 | 210 | 210 |
| BW loc. poly. (h) | 15.846 | 14.550 | 15.449 | 14.286 | 14.938 | 14.338 |
| BW bias (b) | 28.592 | 27.809 | 27.392 | 28.428 | 27.071 | 25.427 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are not logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A12 -
Impact of (Reduced Form) Discontinuity on Unemployment and Private Employment

| | Unemployed | Unemployed Wage Sample | Unemployed 1937 Data | Private Employment | Private Emp. Occ. Weighted |
|------------------------------|----------------|---------------------------|-------------------------|-----------------------|-------------------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Bias-corrected estimate | -0.010 | 0.292 | 0.123 | 0.075 | 0.063 |
| Robust standard error | 0.104 | 0.170 | 0.093 | 0.082 | 0.092 |
| Robust 95% conf. interval | [-.214 ; .195] | [-.041 ; .626] | [-.058 ; .304] | [-.086 ; .236] | [-.116 ; .243] |
| Conventional estimate | -0.013 | 0.282 | 0.102 | 0.061 | 0.041 |
| Conventional standard error | 0.090 | 0.145 | 0.083 | 0.073 | 0.082 |
| Effective observations left | 318 | 311 | 293 | 244 | 267 |
| Effective observations right | 232 | 230 | 219 | 198 | 210 |
| BW loc. poly. (<i>h</i>) | 16.59 | 16.528 | 15.512 | 13.951 | 14.107 |
| BW bias (<i>b</i>) | 28.473 | 28.760 | 30.686 | 26.496 | 29.953 |

Sources: Data are from the 1930 and 1940 complete count Censuses via Ruggles et al. (2024) and the 1937 unemployment census.

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A13 -
Impact of (Reduced Form) Discontinuity on Private Wages

| | Private Weekly Wages | Private Weekly Wages No High School | Private Weekly Wages Occ. Weighted |
|------------------------------|----------------------------|--|---|
| | (1) | (2) | (3) |
| Bias-corrected estimate | 0.026 | 0.037 | 0.034 |
| Robust standard error | 0.023 | 0.028 | 0.023 |
| Robust 95% conf. interval | [-.019 ; .071] | [-.018 ; .092] | [-.01 ; .078] |
| Conventional estimate | 0.022 | 0.029 | 0.029 |
| Conventional standard error | 0.020 | 0.025 | 0.020 |
| Effective observations left | 267.000 | 267.000 | 295.000 |
| Effective observations right | 210.000 | 210.000 | 221.000 |
| BW loc. poly. (h) | 14.902 | 14.558 | 15.476 |
| BW bias (b) | 27.473 | 27.115 | 28.011 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Underlying sample is males ages 18 to 64 who are heads of household. Running variable is 1930 largest city population in 100s.

Table A14 -
Impact of (Reduced Form) Discontinuity on Teen School Attendance

| | A. WPA Worker (as in Main Specification) in Household | | | | | |
|------------------------------|---|----------------|----------------|----------------|---------------|----------------|
| | Male | Male | Male | Female | Female | Female |
| | 15-16 | 13-14 | 17-18 | 15-16 | 13-14 | 17-18 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Bias-corrected estimate | 0.084 | -0.027 | -0.133 | 0.028 | -0.006 | 0.032 |
| Robust standard error | 0.038 | 0.019 | 0.073 | 0.040 | 0.023 | 0.077 |
| Robust 95% conf. interval | [.011 ; .158] | [-.065 ; .011] | [-.276 ; .01] | [-.051 ; .107] | [-.05 ; .038] | [-.118 ; .183] |
| Conventional estimate | 0.084 | -0.025 | -0.109 | 0.022 | -0.009 | 0.034 |
| Conventional standard error | 0.032 | 0.016 | 0.065 | 0.035 | 0.020 | 0.067 |
| Effective observations left | 261.000 | 219.000 | 276.000 | 311.000 | 262.000 | 328.000 |
| Effective observations right | 209.000 | 185.000 | 214.000 | 231.000 | 209.000 | 229.000 |
| BW loc. poly. (<i>h</i>) | 14.872 | 12.347 | 15.605 | 16.741 | 14.591 | 17.514 |
| BW bias (<i>b</i>) | 25.754 | 22.694 | 31.577 | 30.710 | 25.596 | 31.794 |
| | B. No WPA Workers in Household | | | | | |
| | Male | Male | Male | Female | Female | Female |
| | 15-16 | 13-14 | 17-18 | 15-16 | 13-14 | 17-18 |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| Bias-corrected estimate | -0.017 | -0.019 | -0.041 | -0.024 | -0.025 | -0.027 |
| Robust standard error | 0.018 | 0.015 | 0.028 | 0.015 | 0.013 | 0.024 |
| Robust 95% conf. interval | [-.052 ; .017] | [-.048 ; .01] | [-.095 ; .013] | [-.054 ; .006] | [-.05 ; 0] | [-.074 ; .02] |
| Conventional estimate | -0.016 | -0.018 | -0.036 | -0.021 | -0.023 | -0.024 |
| Conventional standard error | 0.015 | 0.013 | 0.023 | 0.014 | 0.011 | 0.020 |
| Effective observations left | 318.000 | 295.000 | 318.000 | 267.000 | 267.000 | 267.000 |
| Effective observations right | 232.000 | 221.000 | 232.000 | 210.000 | 210.000 | 210.000 |
| BW loc. poly. (<i>h</i>) | 16.097 | 15.063 | 16.251 | 14.981 | 14.356 | 14.224 |
| BW bias (<i>b</i>) | 28.271 | 24.047 | 28.118 | 27.141 | 24.776 | 25.015 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the *mserd* one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. Running variable is 1930 largest city population in 100s.

Table A15 -
Impact of WPA Wages Discontinuity on Outcomes by Race

| | A. White | | | |
|------------------------------|----------------|----------------|------------------|----------------------|
| | WPA Wages | WPA Employment | WPA Unemployment | Male 15-16 Schooling |
| | (1) | (2) | (3) | (4) |
| Bias-corrected estimate | 0.065 | 3.436 | 1.937 | 1.131 |
| Robust standard error | 0.037 | 1.602 | 1.581 | 0.583 |
| Robust 95% conf. interval | [-.008 ; .138] | [.297 ; 6.575] | [-1.162 ; 5.036] | [-.012 ; 2.275] |
| Conventional estimate | 0.068 | 1.853 | 1.021 | 0.959 |
| Conventional standard error | 0.031 | 1.410 | 1.507 | 0.503 |
| Effective observations left | 281.000 | 702.000 | 337.000 | 557.000 |
| Effective observations right | 219.000 | 313.000 | 234.000 | 298.000 |
| BW loc. poly. (<i>h</i>) | 15.149 | 28.811 | 17.820 | 25.392 |
| BW bias (<i>b</i>) | 26.378 | 69.350 | 49.483 | 48.942 |
| Robust 1st stage Z-score | | 4.008 | 3.242 | 4.397 |

| | B. Black | | | |
|------------------------------|---------------|-----------------|------------------|----------------------|
| | WPA Wages | WPA Employment | WPA Unemployment | Male 15-16 Schooling |
| | (5) | (6) | (7) | (8) |
| Bias-corrected estimate | -0.292 | -1.992 | -1.976 | -0.857 |
| Robust standard error | 0.213 | 3.996 | 4.100 | 2.734 |
| Robust 95% conf. interval | [-.71 ; .125] | [-9.825 ; 5.84] | [-10.011 ; 6.06] | [-6.216 ; 4.502] |
| Conventional estimate | -0.196 | -1.522 | -1.593 | 0.350 |
| Conventional standard error | 0.183 | 3.284 | 3.440 | 2.170 |
| Effective observations left | 203.000 | 218.000 | 237.000 | 267.000 |
| Effective observations right | 134.000 | 157.000 | 167.000 | 152.000 |
| BW loc. poly. (<i>h</i>) | 20.812 | 17.307 | 18.622 | 30.068 |
| BW bias (<i>b</i>) | 35.041 | 28.313 | 31.113 | 44.896 |
| Robust 1st stage Z-score | | 2.274 | 1.943 | 2.004 |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is county. Outcomes are logged. Estimates are from local linear regressions ($p=1$) with a local quadratic regression for bias correction ($q=2$). Regressions use a uniform kernel, and the bandwidth selection procedure uses the mserd one common MSE-optimal bandwidth selector. Covariates include age, age squared, and share no high school, high school, college, White, and Black as well as state indicators. WPA wages instrumented via a fuzzy regression discontinuity design with the 1930 largest city population in 100s as the running variable are not race specific.

Table A16 -
 Schooling by Age and Sex in 1940

| | Male | Female |
|--------|-----------|-----------|
| | In School | In School |
| | (1) | (2) |
| Age 13 | 90% | 91% |
| Age 14 | 86% | 87% |
| Age 15 | 78% | 80% |
| Age 16 | 65% | 69% |
| Age 17 | 51% | 55% |
| Age 18 | 33% | 33% |
| Age 19 | 20% | 18% |
| Age 20 | 12% | 10% |
| Age 21 | 8% | 6% |
| Age 22 | 5% | 3% |

Sources: Data are from the 1930 and 1940 complete count Censuses (Ruggles et al. 2024).

Notes: Unit of observation is person. Data are restricted to counties where the largest 1930 city size is less than 10,000.