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Large-Scale Public Hiring and Labor Market Outcomes

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Abstract:

We analyze the impacts of large-scale public hiring on labor market outcomes by studying individuals hired to assist with the 2010 Decennial Census. Compared to similar applicants who were not hired, individuals who received Census jobs were more likely to be working in 2010 across the distribution, with larger effects for those with marginal scores on the Census hiring test. The effects persisted for individuals with marginal scores, who had higher employment likelihoods of about 25 percentage points over the next 15 years. Male hires accounted for the entirety of the long-term employment effects for marginal applicants.

*Contact Adam Bee (U.S. Census Bureau) at charles.adam.bee@census.gov or Ayushi Narayan (Minneapolis Fed) at ayushi.narayan@mpls.frb.org. We thank Ryan Nunn for his thoughtful early conversations on the topic, colleagues at the U.S. Census Bureau for helpful conversations about Decennial Census hiring processes, and Ryan Nunn and others for comments on the paper. All views are our own and do not necessarily reflect the views of the Federal Reserve Bank of Minneapolis or the Federal Reserve System. Any opinions and conclusions expressed herein are those of the authors and do not reflect the views of the U.S. Census Bureau. All comparative statements in this report have undergone statistical testing, and, unless otherwise noted, all comparisons are statistically significant at the 95 percent confidence level. The U.S. Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product (Data Management System number P-7503840 (P-7518691), Disclosure Review Board approval number CBDRB-FY25-0430).

I. Introduction

What are the short- and long-term impacts of providing government jobs to individuals on a large scale? In the United States, policymakers, scholars, and advocates have argued for policies such as job guarantees that provide government jobs to individuals who want a job (e.g., Center for American Progress 2018; Omar 2023; Paul et al. 2018; Pressley 2021; Shakesprere and Nightingale 2019; Tcherneva 2018). In comparison to directly giving income to individuals, government-provided jobs may have benefits associated with keeping individuals attached to the labor market, providing counter-cyclical economic stabilization, and carrying out publicly beneficial projects. However, these jobs can also have drawbacks associated with the cost to administer them, market distortions, and worker incentives (Bartik 2001; Nunn et al. 2018). In part due to these trade-offs, key large-scale public hiring programs that have existed thus far, such as the National Rural Employment Guarantee Act (NREGA) in India or the Works Progress Administration (WPA) in the U.S. during the New Deal, have been temporary in nature (Howard 1943; Imbert and Papp 2015). Some academics have argued that future large-scale public hiring policies should also provide only short-term jobs (Bartik 2001).

Our research seeks to understand how a temporary government-provided job can influence the labor market trajectories of individuals in the modern-day U.S., informing current policy debates on public hiring. We use the case study of the 2010 Decennial Census, which employed over 850,000 individuals to assist with enumeration and related activities (Seebold 2011). We combine internal Census data on Decennial Census applicants with Census survey and IRS tax records to carry out our analyses. To compare similar applicants who were and were not hired by the Census, we rely on scores from the hiring test taken by individuals at the beginning of the application process (see Appendix C for the example test provided to applicants).

Using a regression discontinuity methodology, we examine marginal applicants with test scores that barely did or did not exceed the minimum test score threshold of 70 generally required for

obtaining a Decennial Census job. This score corresponds with knowing about four of the 30 questions correctly on the test and randomly guessing on the remainder, with only 6 percent of applicants failing to meet the threshold. The analysis thus provides insight into the potential impacts of public hiring for individuals who may find it more difficult to find jobs elsewhere, in line with the motivations for many of the public hiring proposals listed above.

We find that receiving a Census job for these applicants with marginal test scores is associated with a 61-percentage-point increase in the likelihood of working for an employer that issues a W-2 in 2010, indicating that most affected hires would not have otherwise received a W-2 from an employer other than the Census Bureau. The increase in the likelihood of W-2 employment persists over our analysis period through 2023. Applicants with marginal test scores who were hired for the Decennial Census were 28 percentage points more likely to receive a W-2 in 2023 than those who were not hired. These effects were concentrated among males. Conditional on working, there are no statistically significant changes in W-2 wages after 2010, suggesting that the jobs individuals held after working for the Census were comparable to those that were held by those who were not hired by the Census. On net, a higher likelihood of working and similar wage rates translates to nearly $6,000 in higher W-2 wages per year after 2010. Over 20 years, this would translate to over $8,600 in additional discounted tax revenue, assuming a 10 percent tax rate and a 3 percent discount rate. This represents about $6,300 more than, or nearly four times as much as, the $2,300 that Census paid its workers with low test scores on average.

Do these results hold for applicants with higher test scores who have higher baseline levels of work? To examine the impacts of Census hiring more broadly, we compare applicants with the same scores who were available for the same job opening (in terms of the position, location, days, hours, and transportation required) but were randomly more or less likely to receive one of the jobs in that opening. Census hiring policy is to create a selection sheet that ranks eligible applicants for each set of

job openings based on their test score and veteran status. Applicants with the same test score and veteran status are randomly ranked. Interviews are then supposed to occur sequentially from top to bottom on the selection sheet. Focusing on non-veterans with the lowest test score given a job offer on a selection sheet, individuals closer to the top of the selection sheet are thus randomly more likely to receive a Census job than those closer to the bottom.

Using this variation, we find across the test score distribution as a whole, applicants who receive a job are only 22 percentage points more likely to be working for a W-2 employer in 2010, and this effect dissipates by 2011. There are likewise no persistent effects on W-2 wages. When we stratify the sample by test score, we observe that the higher the score, the smaller the impact on the likelihood of work in 2010.

Taken together, these results suggest that a temporary government job can help workers stay attached to the labor market, with larger and more persistent effects for those most on the margin of receiving that job. Hiring individuals with lower test scores can thus have outsized benefits for workers and taxpayers. However, we also find evidence of outsized costs for this population. Low-scoring Census workers were more than twice as likely to be terminated for misconduct or unsatisfactory performance compared to higher-scoring Census workers. Whether or not the costs of early termination and associated management time, replacement workers, and reduced work quality exceed the $6,300 in additional benefits of hiring workers with lower test scores is not observable in the data we have on hand.

Policy makers considering large-scale public hiring policies may need to assess these trade-offs when considering whether to hire and whom to target for hiring. It is also not obvious from our setting whether the results would hold in the absence of Census hiring mostly high-scoring applicants. One reason for the persistent positive labor market impacts for low-scoring workers could be the signal of passing a background check and carrying out a job that requires high levels of trust and is usually

completed by individuals with higher skill levels. Indeed, we find the labor market trajectories of Census hires who worked for less than 40 total hours to be comparable to those who worked for more than 40 hours. Labor market trajectories were also comparable among those who were and were not dismissed for misconduct or unsatisfactory performance. We cannot observe whether the impacts would be similar in absence of these characteristics of the Decennial Census setting. Even so, our results suggest that when certain policy design criteria are met, public hiring can provide key benefits to workers on the margin of employment.

Our results contribute to three primary sets of literature in the field of economics. First, we contribute to a literature on large-scale public hiring, which has largely focused on the developing or historical context. This research, examining public hiring programs largely under the NREGA and WPA, usually finds substantial private sector displacement (Azam 2012; Benjamin and Matthews 1992; Fishback 2017; Fleck 1999; Imbert and Papp 2015; Liu and Fishback 2019; Neumann et al. 2010; Wallis and Benjamin 1981; Zimmerman 2023). Our results are generally consistent with this finding, where for workers on average, we observe only a 22-percentage-point increase in the likelihood of working during 2010, meaning that most Census workers would have been working anyway. However, we show that this displacement is much smaller for those with lower test scores, who are a full 61 percentage points more likely to be working. The existing literature largely looks at the effects of public hiring programs either on average or for those on the margin between public hiring jobs and better outside options. We demonstrate that the impacts of public hiring for individuals on the margin of employment, who likely have worse outside options, differ dramatically from the rest of the population.

The paper contributes to a literature on more targeted public hiring as well. A body of work studies programs that provide jobs to specific disadvantaged groups, such as those receiving welfare payments. One study finds that temporary-help job placements do not improve outcomes for low-

skilled participants in the Temporary Assistance for Needy Families (TANF) program (Autor and Houseman 2010). However, another set of studies examine the Supported Work Program, which provided temporary jobs with graduated stress to targeted groups including those receiving Aid to Families with Dependent Children (AFDC, the precursor to TANF), and find that the program increased earnings both in the short and medium terms (Couch 1992; Hollister et al. 1984). More descriptive research that traces the labor market trajectories of temporary workers finds positive long-term impacts of targeted public hiring for less-skilled workers (e.g., Andersson et al. 2009; Ferber and Waldfogel 1998; Lane et al. 2003; Heinrich et al. 2007). The variation in results across studies examining targeted public hiring suggests that differences in the types of temporary jobs provided and the types of individuals served by them could influence program efficacy. Our results contribute to this work by highlighting that males on the margin of Census hiring may particularly benefit from the type of work provided by Census jobs, and other groups may see less long-term benefit from this type of public hiring.

Our study also contributes to a broader literature on job ladders and job scarring. Studies show that job loss can have lasting negative impacts across the distribution, including for lower-income workers (e.g., Jacobson et al. 1993; Rose and Shem-Tov 2023). We show that job gain, even if temporary, can have lasting positive impacts for part of the distribution, namely workers with low test scores. We also demonstrate that a temporary skill accessible job has no negative impact on worker outcomes, contrary to some existing research suggesting that underemployment can be more detrimental than unemployment (Farber et al. 2017; Nunley et al. 2017). Our finding here may be influenced by the timing of 2010 Decennial Census hiring, which occurred during and in the immediate aftermath of the Great Recession when unemployment rates were high. Suggestive research finds that other signals, such as unemployment duration, may be more muted when the labor market is weak (Abraham et al. 2019). We cannot assess whether being hired for the Census would have a more

negative effect in a stronger labor market. Still, our results suggest that temporary public hiring can keep workers attached to the labor market without any longer-term negative employment effects and potentially positive longer-term effects when the economy is poor.

Lastly, our paper adds to a body of work studying the decline in labor force participation, particularly among men. This research has hypothesized that among other factors, a drop in the demand for low- and middle-skill occupations may have dampened participation in the labor market for men (Council of Economic Advisers 2016). Our analysis supports this hypothesis, showing that access to a Census job can bolster rates of work, both in the short and long terms, particularly for men at the margin of being hired. The study demonstrates that public hiring may be an effective policy tool in encouraging labor force participation, at least for some groups of individuals.

Our paper proceeds as follows. Section II describes the 2010 Decennial Census hiring process. Section III describes our methodologies for assessing the impacts of Census hiring, both for workers on the margin of employment with lower test scores and for workers on average. Section IV assesses and presents results for the workers with lower test scores, and Section V assesses and presents results for workers on average. Section VI discusses the results and concludes.

II. Decennial Census Hiring Process

The Decennial Census is a mandatory, nationwide count of the U.S. population conducted every 10 years by the Census Bureau. The data collected in the Decennial Census determine congressional representation and aid in the allocation of federal funds and in other aspects of public policy. In order to successfully complete 2010 Decennial Census operations, the Census Bureau recruited 3.9 million applicants for over 850,000 temporary positions between June 2008 and May 2010 (Seebold 2011). These positions included enumerators, office clerks, crew leaders, supervisors, recruiting assistants, and

related roles. Peak hiring occurred in 2010 for the non-response follow-up operation, but many individuals were also hired in 2009 for address canvassing.

Appendix Figure A1, copied from the 2010 Decennial Applicant, Personnel, and Payroll System (DAPPS) Assessment Report, describes the Census hiring process as it pertains to the DAPPS data we use for our analysis (Seebold 2012). Additional information is available in the 2010 Census Recruiting and Hiring Assessment (Seebold 2011). The recruiting process began with an application, which was available in paper at job service centers, libraries, and community-based organizations as well as online. A copy of this application is provided in Appendix B. Applicants then took an employment test at libraries and other community organizations where applications were available. There were two types of tests: one for non-supervisory positions and another for supervisory and management positions. We focus our attention in this paper on non-supervisory positions and the associated testing. A copy of a sample test provided to applicants is available in Appendix C. The test was 30 minutes long, with testing sessions lasting about an hour including the job application and other paperwork.

Applicants were required to achieve a converted test score of 70 to be considered for employment. This converted score generally corresponded with getting 10 questions correct on the test. Because there was no penalization for guessing and four choices per question, 10 questions correct would on average correspond with the applicant correctly knowing four of the 30 questions and randomly guessing on the remainder. Only about 6 percent of applicants failed to meet this threshold. Figure 1 shows the distribution of test scores in our sample. After completing the application and testing, applicants underwent a background name check. Those who passed were then added to selection sheets as deemed fit.

Each set of job openings – job openings are defined by their position, location, days, hours, and transportation required – had its own selection sheet. A given selection sheet could thus correspond to one opening or many. A selection sheet contained up to 100 eligible applicants, that is, up to 50 new

applicants and 50 experienced ones who had worked for the Census in the previous year, who fit the requirements of the job opening. Candidates were listed in rank order by their converted test score, which was based solely on the employment test score and veteran status, from the highest to lowest score with veterans listed first for a given score. Applicants with the same test score and veteran status were ranked randomly. However, this random ranking could be correlated across selection sheets. For example, one randomization algorithm suggested by Census in its guidance to administrative staff was to assign each candidate a number based on the last digits of their SSN and then rank order the candidates based on a number 0 through 9 that varied each day. Unless the SSNs were perfectly evenly spaced out, this algorithm would lead to correlations over time in the ordering of individuals. Once an applicant was added to a selection sheet, they couldn't be added to another selection sheet for the same position until that sheet had been closed. It was recommended that selection sheets be open no longer than three or four days (Department of Commerce 2009). Applicants hired for a job opening, declining a job offer three times, or failing an interview by, for example, using excessive profanity would no longer be a candidate for future selection sheets. There were about 2.3 million applicants that showed up on at least one selection sheet. Conditional on being on at least one selection sheet, applicants appeared on an average of nine selection sheets.

Applicants on the selection sheet were interviewed from top to bottom, separately for new and experienced applicants, until the hiring manager was able to make as many offers as needed. The interview mostly assessed a candidate's interest and availability in the job, with offers given to those who were interested, available, and qualified for the position. More offers were made than open positions existed due to expected attrition before individuals began working. After applicants received offers, they completed training, were fingerprinted and went through another background check, and had their employment eligibility verified. In our data, we consider hired individuals to be those for whom we observe hours and pay.

III. Data and Methodology

We rely on restricted DAPPS, Census, and Internal Revenue Service (IRS) data to understand the impact of Census hiring on worker outcomes. The DAPPS data, used in an earlier analysis of Census workers by Eggleston et al. (2020), contain information on who applied for a Decennial Census job, their test scores, certain other characteristics from their job application such as their age and education level, the selection sheets in which they were listed, the date the selection sheets were created, their position in those selection sheets, whether they received an offer for a given job opening, whether they were ultimately hired and paid by the Census, and how they left their position. The DAPPS data do not directly contain a veteran status variable that we can observe, but we can infer it based on the test score we observe in comparison to the raw score. We exclude veterans from our analyses throughout the paper due to the non-random hiring preference they receive.

The DAPPS datasets have various anonymized applicant and employee identifiers that allow us to link across datasets, as well as Protected Identification Keys (PIKs) that allow external linkage. We are able to merge over 99 percent of applicants to their hiring, Census, and IRS outcomes in a one-to-one manner with these identifiers. We randomly select an individual if there are multiple observations for a given identifier in a dataset or label variables as missing for the handful of observations where identifiers are slightly different across datasets. We merge the DAPPS data with Census and IRS data to understand how Decennial Census hiring relates to individuals' employment outcomes before and after 2010. The Census data we use come from the 2000 Decennial Census and the 2010 Decennial Census short form. The Census data allow us to obtain demographic information about applicants. We can observe sex in both the 2010 and 2000 Census data, using the 2010 data where available and filling in with 2000 sex if 2010 is missing for an individual. For race and ethnicity, we rely only on 2010

information due to changes in how Census collected race and ethnicity information between the two years.

The IRS data span from 2005 to 2023 and are constructed from the Census Bureau's Income History dataset originally created by Jonathan Rothbaum from a universe of 2000 and 2010 Decennial Census respondents (e.g., Borgschulte et al. 2025). Our primary outcomes of interest come from Form W-2 data reported to the IRS, focusing on wages, tips, and other compensation reported in Box 1. With these data, we examine whether an individual had a positive W-2, the log value of annual wages in the W-2 conditional on having a positive W-2, and the total W-2 wages inclusive of zero. We winsorize W-2 earnings at $100,000, consistent with Chetty et al. (2011), affecting less than 1 percent of observations. We deflate the income measures to 2024 dollars using the CPI-U-RS.

To estimate the impact of Census hiring on our outcomes of interest, we use a regression discontinuity methodology around the test score of 70 which is generally required for employment by the Census. We estimate reduced form regressions that can be characterized by the following equation:

$$(1) \quad outcome_i = \gamma_0 + \gamma_1 \mathbf{1}\{score_i \geq 70\} + \gamma_2 \{score_i - 70\} + \gamma_3 \mathbf{1}\{score_i \geq 70\} \times \{score_i - 70\} + \eta_i$$

Here, γ_1 represents our parameter of interest, the impact of crossing the test score threshold of 70. $\mathbf{1}\{score_i \geq 70\}$ is an indicator for whether the test score exceeds 70, and $score_i$ is the running test score variable. η_i represents our error term. We calculate robust standard errors for our analysis. We estimate the regression with a bandwidth of 5, consistent with a data-driven MSE-optimal bandwidth that balances bias and variance to minimize mean squared error loss, as estimated with the "MSERD" common MSE-optimal bandwidth selector from Calonico et al. (2017), for the outcome of being hired by the Census. A bandwidth of 5 is also the minimum required to have more than two points below the threshold. We use a uniform kernel for ease of a conventional local linear regression discontinuity estimation. In specification checks, we present bias-corrected estimates with robust standard errors

implemented as suggested by Calonico et al. (2017), and we also explore robustness to alternate bandwidths.

(2) $$outcome_i = \beta_0 + \beta_1 hired_i + \beta_2\{score_i - 70\} + \beta_3 \mathbf{1}\{score_i \geq 70\} \times \{score_i - 70\} + \varepsilon_i$$

To calculate a Local Average Treatment Effect (LATE), we estimate $hired_i$ using Equation (1) and then estimate Equation (2) via two-stage least squares. This instrumental variables (IV) regression specification is otherwise analogous to Equation (1), and we carry out the same specification tests in later appendix tables. β_1 represents our coefficient of interest that provides the LATE in this fuzzy regression discontinuity design. We are able to capture the LATE with this specification because our treatment is binary, our instrument is binary, there is monotonicity by design of the rank order hiring process, and our covariates of the running variable are correctly specified by the assumptions in our regression discontinuity design. Mogstad and Torgovitsky (2024) outline how each of these features of our data are required to obtain a LATE. We opt not to include additional covariates that could improve precision given the potential biases they could impose on the LATE we wish to uncover (Mogstad and Torgovitsky 2024).

Table 1 provides summary statistics and hiring likelihoods by test score to better illustrate the population to which this LATE applies. Applicants with test scores around 70 were about 14 percentage points less likely to work in 2005 than individuals with the highest scores, based on W-2 filings. Conditional on working, they earned about 70 percent as much as high-scoring individuals. When combining both their lower likelihoods of work and their lower earnings, we see that low-scoring applicants had W-2 incomes that were about 56 percent as high as their higher-scoring counterparts. Applicants with scores around 70 were also older by about five years, with an average age of roughly 44. They were about 10 percentage points less likely to report being registered in the selective service, which is required for males ages 18 to 50, but qualitatively just as likely to be male. Most notably, they had about a 50-percentage-point lower likelihood of having completed an Associate or Bachelor's

college degree. Only 20 percent of applicants with a score of 70 reported having a college degree, in comparison with nearly 70 percent of applicants with a score of 100.

We validate our regression discontinuity methodology in a handful of ways. First, we assess whether there is bunching around our threshold of 70. Visually, it is clear in Figure 1 and Table 1 that application counts are continuing to rise at this part of the test score distribution, with no obvious jump around 70. Appendix Figure 2 shows the results of a more formal manipulation testing procedure from Cattaneo et al. (2018) with a linear estimator consistent with our baseline strategy. The testing procedure delivers high T-statistics, suggesting manipulation. However, the direction differs between the conventional and robust estimators, with the conventional estimator having a T-stat of 185 and the robust estimator having a T-stat of -84. When one inspects Appendix Figure 2, it appears that the testing procedure appears to poorly fit the pattern of the data. At the bottom range of test scores, we see exponential growth in application counts as test scores rise, followed by more linear growth in counts once scores are somewhat higher. The manipulation testing may therefore be poorly suited for assessing bunching near our cutoff of 70.

In Table 2, we instead examine a more direct form of test score manipulation, namely test retakes. Our baseline model uses the maximum test score for each individual. It's conceivable that individuals retook the test in order to get over the threshold, even though it's unlikely they would know how their performance corresponded with this threshold. Column 1 of Table 2 presents the estimates from Equation (1) for an indicator on whether an applicant retook the test as the outcome. The estimate is small and negative, at -1.6 percentage points, suggesting, if anything, that individuals with maximum scores just above the 70 threshold were less likely to have retaken the test. The estimate is precise, with a 95 percent confidence interval failing to include any increase in test retaking above the threshold.

The next three columns in Table 2 examine whether we can detect any large differences in applicant characteristics that we can observe from their application information. We see that applicants

with test scores just higher than 70 are only slightly younger by less than half a year than applicants with scores just below 70, though this difference is statistically significant. Columns 3 and 4 likewise show that applicants above and below the 70 threshold are not different in terms of the share who have a college degree and the share registered in the selective service. The point estimates for both of these variables suggest qualitatively insignificant differences of less than 1 percentage point. On net, it appears as though applicants scoring just below and above 70 on the hiring test are generally comparable based on observable characteristics and that it is unlikely that applicants are manipulating their test scores to make sure they exceed a score of 70.

To more directly observe whether demographics change at the threshold, we use data from the Decennial Census. Our measure for sex includes information from both the 2000 and 2010 Censuses, while the race and ethnicity variables include information from only the 2010 Census. Column 5 shows that we observe a statistically significant 3-percentage-point decrease in the likelihood an applicant is male at the threshold, even though Table 1 shows the share of applicants who are male hovers just around 40 percent for individuals of all test scores. Columns 6 and 7 likewise find jumps in the share White and share Hispanic, though the magnitudes are small, at 1 to 2 percentage points. It is worthwhile to note that 2010 response rates for the Decennial Census variables could have been influenced by applicants' experiences of being interviewed and hired for the 2010 Decennial Census. Indeed, in Column 8, we find that the likelihood of non-response significantly decreases at the threshold. We therefore interpret the results deriving from the 2010 Census here with caution.

The last column of Table 2 examines whether the regression discontinuity methodology provides a strong first stage for predicting Census hiring. Consistent with the means in Table 1, the point estimate in Column 9 suggests a 6-percentage-point increase in hiring when crossing the test score threshold of 70. The T-stat for this estimate is 80, indicating that our instrument for hiring is indeed very strong. Our final validation of the methodology is shown in the subsequent tables and

figures that describe the results associated with this method, where we show that prior to Decennial Census hiring that started in 2009, applicants with test scores just above and below 70 had similar outcomes.

IV. Labor Market Impacts of Census Hiring for Marginal Applicants

Table 3 presents the reduced form and IV results using Equations (1) and (2) to estimate the impacts of Census hiring for marginal applicants. Figure 2 presents a sample of reduced form regression discontinuity plots for the outcome of having a positive W-2. Figure 3 presents the IV results for this variable and the two other W-2 variables graphically. The table and figures examine whether hiring had an impact on the share of applicants with positive W-2s in a given year, the log of those W-2 wages, and the total reported W-2 income in a year. As noted above, we see that prior to 2009, applicants with test scores just above and below 70 had values of these outcome variables that did not significantly differ.

Starting in 2009 as hiring for canvassing began, we see that the share of applicants with positive W-2s jumps just above the 70 threshold. The effect peaks in 2010 when nearly all individuals hired by the Decennial Census had a positive W-2, showing a point estimate of about 61 percentage points in the IV specification. The effect of Census hiring on the share of people with a positive W-2 persists even after 2010 when Decennial Census hiring ended. The IV estimates suggest that marginal applicants hired by the Census were about 25 percentage points more likely to be working in a job that issued a W-2 through our last data year of 2023. These results are replicated in Appendix Table A1 when using an alternate bandwidth of 10 or the robust bias-corrected regression discontinuity estimator provided by Calonico et al. (2017).

Our results for log wages suggest no lasting change in how much these working individuals earn, however, though these estimates are somewhat imprecise. We do observe a significant dip in 2010

as hired individuals receive relatively small W-2s for part-year and often part-time Census work, but the estimates are insignificant thereafter. Still, the relatively flat wages when combined with a higher likelihood of working do contribute to higher total W-2 wages in the long run. In 2010, the IV estimates indicate that total W-2s were an insignificant roughly $3,000 higher for hired applicants. This number grows over time to an average increase of nearly $6,000 between 2011 and 2023. Over 20 years, assuming a tax rate of 10 percent, a discount rate of 3 percent, and an average annual increase of $5,855, each marginal hire would contribute an additional $8,600 in taxes, well exceeding the $2,300 received in payment for their work on the Decennial Census.

Figure 4 explores heterogeneity in applicant characteristics for the impacts of Census hiring. It presents the IV results for having a W-2 job by age and education, two key characteristics available in the application. The first panel by age shows that individuals above age 50 had more persistent elevated W-2 receipt levels following being hired by the Census, though their baseline levels were also somewhat higher. Those under 50 had more variable impacts. The effect of Census hiring was positive but diminishing through the late 2010s, after which the effect started to rise. By the end of our sample in 2023, both age groups saw about a 20-percentage-point increase in their likelihood of receiving a W-2. The last two panels examine heterogeneity by education. Because there are relatively few individuals with any college degree with low test scores, the estimates for that group are noisier. Comparing just the point estimates, we see that the estimates are slightly larger for those with no college degree, though our standard errors cannot distinguish between the two. Appendix Table A2 provides the underlying estimates for the figure.

Figure 5 and Appendix Table A3 show heterogeneity by demographic variables derived from the Decennial Census. Panels A and B show considerable heterogeneity by sex, with males accounting for nearly the entire observed impact. Both White and non-White applicants saw increases in their likelihood of having a positive W-2. The long-term impact on the likelihood of W-2 work for Hispanic

individuals was smaller in magnitude and statistically indistinguishable from zero, though the estimates are more imprecise. The results for non-Hispanic individuals show persistent increases in the likelihood of W-2 employment.

Appendix Table A4 examines heterogeneity by potential policy-relevant geographic characteristics. The first two columns explore whether effects differed for states that did and did not exempt Census income from Medicaid eligibility. Thirty-four states excluded Census income, and 17 states did not exclude Census income. In theory, there could be both positive and negative effects of this inclusion on work outcomes. If staying enrolled in Medicaid allowed individuals to receive the care they needed to be productive citizens, then the exemption could lead to larger or more persistent labor market impacts. Conversely, losing access to Medicaid could incentivize individuals to seek employment that could provide healthcare coverage. The patterns we observe in Columns 1 and 2 are indistinguishable across both groups of states, suggesting that the impacts of Medicaid eligibility were either small or counteracting. Columns 3 and 4 carry out an analogous exercise looking at states that excluded the income of temporary Census employees from eligibility for the Supplemental Nutrition Assistance Program (SNAP). Thirty-eight states excluded Census income from SNAP eligibility, with theoretical effects similar to those outlined for Medicaid. As with Medicaid eligibility, we observe limited heterogeneity based on SNAP eligibility.

The final two columns of Appendix Table A4 explore whether results differ based on local labor market conditions. Column 5 restricts the sample to the 24 states that experienced high levels of unemployment above 9 percent in 2010, while Column 6 examines the remaining states with lower unemployment rates. Overall, the results look parallel across these groups of states.

V. Labor Market Impacts of Census Hiring for Other Applicants

We next seek to understand whether our results for applicants at the margin who have low test scores translate to applicants more generally who may have higher test scores. To do this, we implement a second methodology that compares applicants who are similar based on observables. More specifically, we compare applicants with the same scores who were available for the same job opening.

We limit our sample to observations where we can use the randomized selection sheet ordering to compare individuals who were more or less likely to be interviewed and thus hired for the job. To do this, we leave out a small number of observations (4 percent) that have selection sheets with fewer than five individuals where the sequential hiring is less obvious, as this occurs only after the third individual. Importantly, we restrict our sample to applicants with scores equal to the lowest test score that received a job offer on their selection sheets. To clearly define the lowest test score receiving an offer, we further restrict our sample to the vast majority (roughly 75 percent) of selection sheet observations where test scores either monotonically decrease or increase only once where the experienced hire section of the list begins. We exclude these experienced hires from our analysis. We also drop applicants after the first time we see them on a selection sheet due to the correlated ordering of individuals across selection sheets.

At the lowest score, we can assume that individuals randomly located lower down on the selection sheet were less likely to be interviewed, while those higher up were more likely to be interviewed. Because the last individual to receive an offer and likely complete the interviews is the type of applicant who the hiring manager thinks would make a good hire, we cannot directly compare individuals who are located higher than this individual to those who are located lower. In a finite sample, the group that likely received an interview is biased to the types of individuals who would receive an offer.

We therefore calculate the total number of offers O given at the lowest test score to non-veterans and construct an instrument that equals one if an individual is among the first O non-veteran applicants on the selection sheet at the lowest score and zero if an individual is at the lowest score but later on the selection sheet. Because individuals are randomly ranked if they have the same test score and veteran status, this instrument is similar in spirit to the "initial offer" instrument used in randomized waiting list methodologies (De Chaisemartin and Beghagel 2020). As a robustness check, we implement the doubly reweighted ever-offer estimator by De Chaisemartin and Beghagel (2020), which may be more efficient than the "initial offer" estimator but in our setting requires calculating the impact of an offer rather than a hire which may yield a blunter estimate. Our primary specification can be characterized by the following reduced form equation:

(3) $$outcome_i = \delta_0 + \delta_1 \mathbf{1}\{initial\ offer_i\} + \psi_i + \upsilon_i$$

Here, δ_1 is our coefficient of interest, showing the impact of likely receiving an interview on our outcomes for non-veterans with the lowest test score the first time we observe them. $\mathbf{1}\{initial\ offer_i\}$ is an indicator for whether an individual is in the first O applicants on the selection sheet among non-veterans with the lowest test score. ψ_i are selection sheet fixed effects. υ_i represents our error term. We cluster our standard errors by selection sheet.

To construct LATEs with this specification, we estimate the following analogous IV regression using two-stage least squares, where *hired* is predicted with Equation (3), and θ_1 becomes our outcome of interest. As with Equation (2), we are able to uncover the LATE with this specification because our treatment is binary, our instrument is binary, there is monotonicity by design of the rank order hiring process, and our covariates of specification sheet fixed effects are rich (Mogstad and Torgovitsky 2024).

(4) $$outcome_i = \theta_0 + \theta_1 hired_i + \tau_i + \nu_i$$

The LATE estimated here differs from the LATE estimated with the regression discontinuity design at the test score of 70, which captures individuals at the very bottom of the test skill distribution. The modal lowest score was 97, with the vast majority of selection sheets stopping at a score of 90 or higher. Figure 1 shows the cumulative distribution function of applicants, hires, and selection sheet lowest scores. The lowest test score of offers in our sample of selection sheets roughly aligns with the overall distribution of hires, though there are slightly more selection sheet low scores at the bottom of the distribution and slightly fewer at the top, as would be expected given that individuals with scores higher than the lowest score would also receive offers and be hired.

We start by assessing the validity of our within-score methodology. The first eight columns of the last two rows of Table 2 present the same balance statistics we examined for the regression discontinuity methodology. They show that applicants randomly higher up on the selection sheet who would be theoretically more likely to receive offers with our "initial offer" instrument had similar test retaking shares, ages, college degree shares, selective service shares, male shares, White shares, Hispanic shares, and shares not responding to the 2010 Decennial Census. All are statistically insignificant, except for age, which as with the regression discontinuity estimate has a small but precisely estimated difference. Column 9 examines whether the "initial offer" instrument predicts hiring. The effect is indeed positive, suggesting that those who receive a hypothetical "initial offer" are about 10 percentage points more likely to be eventually hired by the Census. The result is highly significant, with a T-stat of around 18.

Figure 6 graphically presents the IV estimates of Census hiring using the "initial offer" instrument on our labor market outcomes of interest. Appendix Table A5 presents the reduced form and IV estimates underlying this figure. When examining the applicant pool as a whole, we continue to observe significant employment increases, measured by the share of positive W-2s, in 2009 and 2010. The IV estimates in Panel A of Figure 6 and in Column 2 of Appendix Table A5 suggest a peak increase

in positive W-2s of 22 percentage points in 2010. However, this effect size is much smaller than the 61 points estimated in the regression discontinuity methodology.

It thus appears that Census hiring has a greater impact on the likelihood of W-2 employment for more marginal applicants with lower test scores than for applicants on the whole with higher test scores. This result is apparent as well in Appendix Table A6, which examines the impacts of Census hiring on the share of individuals with a positive W-2 when segmenting the selection sheets by those with scores of 70-80, 82-90, and 92-100. In Column 2, we see the IV estimates of hiring suggest an effect size of 46 percentage points in 2010 for those with scores of 70-80, which is larger than the effect size of 23 estimated for applicants with test scores of 82-90 in Column 4, which is larger than the effect size of 16 for the score range of 92-100 in Column 5.

In addition to having smaller increases in the likelihood of W-2 employment in 2010, non-marginal applicants also do not see the same increases in employment that marginal applicants do after 2010. Panel A of Figure 6 shows that we fail to detect a persistent positive effect on employment after 2010 for non-marginal hires. It is also noteworthy that we fail to detect persistent effects for applicants with scores in the 70-80 test score range in Columns 1 and 2 of Appendix Table A6. Although the point estimates suggest a somewhat noisily estimated 15- to 20-percentage-point increase in W-2 employment in the early 2010s for low scoring hires, the effect drops to pre-2010 trends fairly quickly. Test scores of 80 represent the 30th percentile of applicants, while those with scores of 70 fall in the bottom 9 percent of applicants, as shown in Figure 1. The 70-80 results and regression discontinuity results thus apply to somewhat distinct populations, and it seems as though the persistent employment effects we uncover for marginal hires hold long term only among those with the lowest test scores.

Our null result means that we fail to detect a persistent negative effect as well. Theoretically, there could also be a negative effect of Census hiring on future employment for non-marginal applicants due to the scarring effects of underemployment found in other settings (Farber et al. 2017;

Nunley et al. 2017). Our estimates are sufficiently precise to rule out large declines greater than 10 percentage points in W-2 employment following Census hiring. We observe a similar pattern in Columns 5 and 6 of Appendix Table A1, which provides estimates from the doubly reweighted ever-offer estimator by De Chaisemartin and Beghagel (2020). The results are more precise, so we do observe statistically significant declines in W-2 employment after 2010, but the estimates are small and qualitatively similar to trends before 2010. Moreover, when we restrict our analysis to those with the highest scores in Appendix Table A6, we also do not observe a statistically significant decline in W-2 employment likelihoods after 2010, although the estimates are less precise. As a whole, these findings help allay concerns about the longer-term effects of underemployment at least in the setting we examine.

The remaining panels of Figure 6 and columns of Appendix Table 5 examine the impacts of Census hiring with our within-score method on log W-2 wages and on total W-2 wages. We find no significant effects on these variables for applicants in general, either in the reduced form or IV specifications.

VI. Discussion and Conclusion

We find that Decennial Census hiring can help individuals stay attached to the labor market in the short term. In the long term, we observe limited negative repercussions for non-marginal workers and large persistent positive effects for those on the margin, especially males. Our results are specific to the Decennial Census setting, which hired during an exceptionally weak labor market, hired many highly skilled individuals, provided temporary employment, paid relatively well for the skill level required, and required individuals to go through an application process that involved testing, interviews, and background checks. Whether our results would apply to other public hiring policies,

such as those discussed in recent proposals mentioned at the beginning of this paper, may depend on the details of the policies and the mechanisms that drive our results.

Although we cannot disentangle which aspects of Census hiring are most relevant for our results, we can shed some light on the mechanisms that drive the persistent employment results for workers on the margin of employment. There are two key candidate mechanisms we consider. First it could be the training and work experience provided by Census that enable marginal hires to increase their job finding after leaving Census. Second, it could be the signal of passing a background check and carrying out a job that requires high levels of trust and is usually completed by individuals with higher skill levels. We cannot formally disentangle these two factors with the data we have available, but we can make hypotheses based on the work trajectories of different types of marginal Census hires.

Figure 7 shows the likelihood of having a positive W-2 in a given year for Census hires with scores of 70-75. The solid navy line traces out the likelihood of W-2 employment for marginal Census hires who were terminated for misconduct and unsatisfactory performance, and the dotted line shows the likelihood of employment for workers who were not terminated for those issues. The solid gold line represents marginal hires who worked for fewer than 40 hours total, including training, and the dotted line represents those who worked for 40 hours or more. All four lines are basically equivalent, suggesting that the trajectories for the likelihood of W-2 employment were qualitatively similar for all types of marginal Census hires.

If training and experience were driving the effects we observe, we might have expected to see lower employment likelihoods in later years for individuals terminated for misconduct or unsatisfactory performance or for individuals working fewer than 40 hours. It thus appears as though the signal of Census employment could be driving the long-term effects we observe for marginal applicants. However, it is difficult to know the counterfactual for the different types of applicants. We do not know whether the treatment effects of Census hiring differ for different types of applicants. For example, 15

hours of work could be uniquely useful for the type of Census hire who only worked for 15 hours total. There could also be other mechanisms driving the result, such as Census hiring boosting workers' self-confidence or increasing their attachment to the labor market through some other way. We therefore consider this analysis only suggestive.

Our analysis thus far has only considered the labor market effects of Census hiring, but there are other costs and benefits associated with hiring, particularly those related to job performance. Figure 8 shows the share of hires by test score who were terminated for misconduct or unsatisfactory performance. The figure shows that more than 1 in 10 hires with test scores below 75 were terminated for these reasons. Meanwhile roughly 1 in 20 hires with test scores above 95 were terminated for misconduct or unsatisfactory performance. The lowest-scoring hires were more than twice as likely to be terminated for these reasons. Low-scoring hires may therefore have greater costs associated with hiring replacement workers, and they also may require more supervision and have lower work quality, which could impose additional costs.

Our results highlight key trade-offs in worker targeting faced by policymakers considering large-scale public hiring policies. The workers closest to the margin of employment may experience the largest benefits from such policies, but they also may impose the largest costs. The gains experienced by workers at the margin of employment may also depend on the presence of more highly skilled workers. Additional research is needed to understand the costs and benefits of public hiring policies and how they can be optimally designed. Still, our research highlights that when certain criteria are met, public hiring can provide important long-term benefits to workers on the margin of employment while having no longer-term repercussions for non-marginal workers.

The results show that there are key benefits to large-scale direct public hiring a policymaker might consider that a private employer may not. The government can select types of workers who may see unique benefits to being hired whom the private sector may not choose. The results from this study

suggest that direct public hiring may thus have certain advantages over indirect public policies to promote hiring, such as wage subsidies or other tax incentives, which are often found to provide limited benefit (see Neumark 2018 for a review). The paper also might support alternative public policies that encourage employers to hire more risky employees they may not otherwise hire, as the results show how marginal hires can see lasting benefits from such hiring, even if the job is temporary.

The findings from our study may have broader implications outside the policy context as well. First, we show that hiring workers on the margin of employment can have lasting long-term labor market effects on those workers. This result could influence how private employers seeking a stronger labor market recruit workers, provide apprenticeships, hire interns, and invest in those marginally attached to the labor market. Second, we show that Census hiring has limited long-term negative effects for non-marginal workers. This result could influence how advisers guide individuals when they are unemployed or need to temporarily pull back from their careers.

Overall, we show that a temporary Census-type government job can provide benefits for a wide range of workers in the short term and in the longer term, especially for males with lower skill levels on the margin of employment. Future research can help shed light on how economic context, demographic characteristics, and policy design influence the efficacy of large-scale public hiring.

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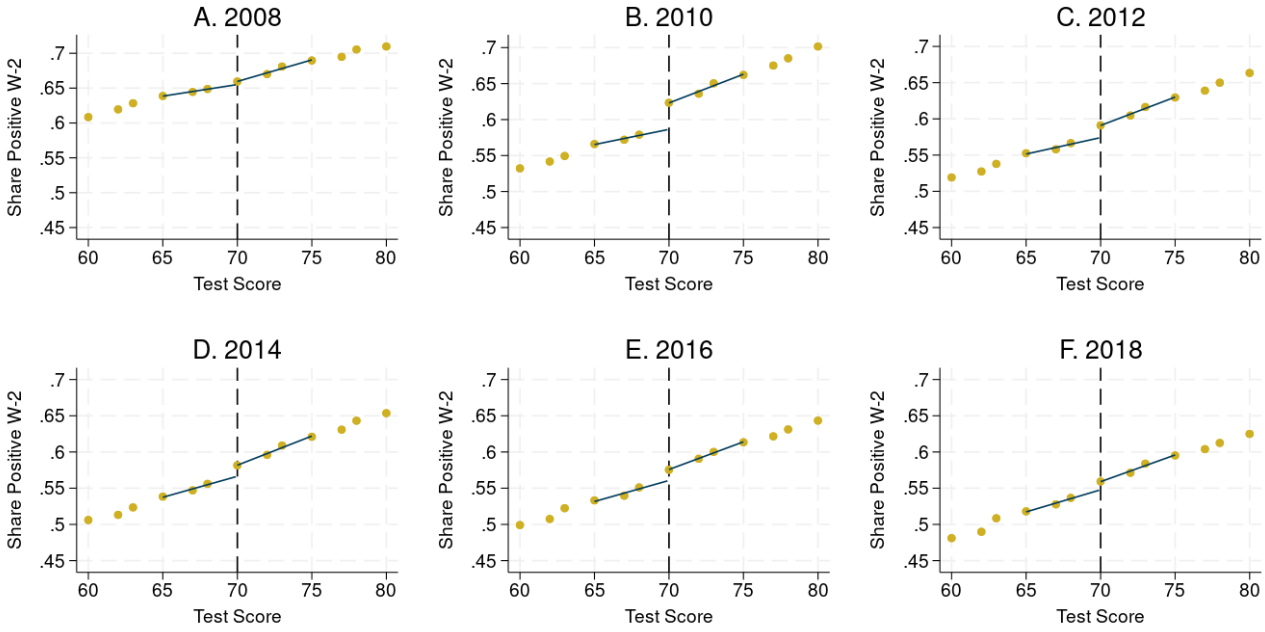
Figures and Tables

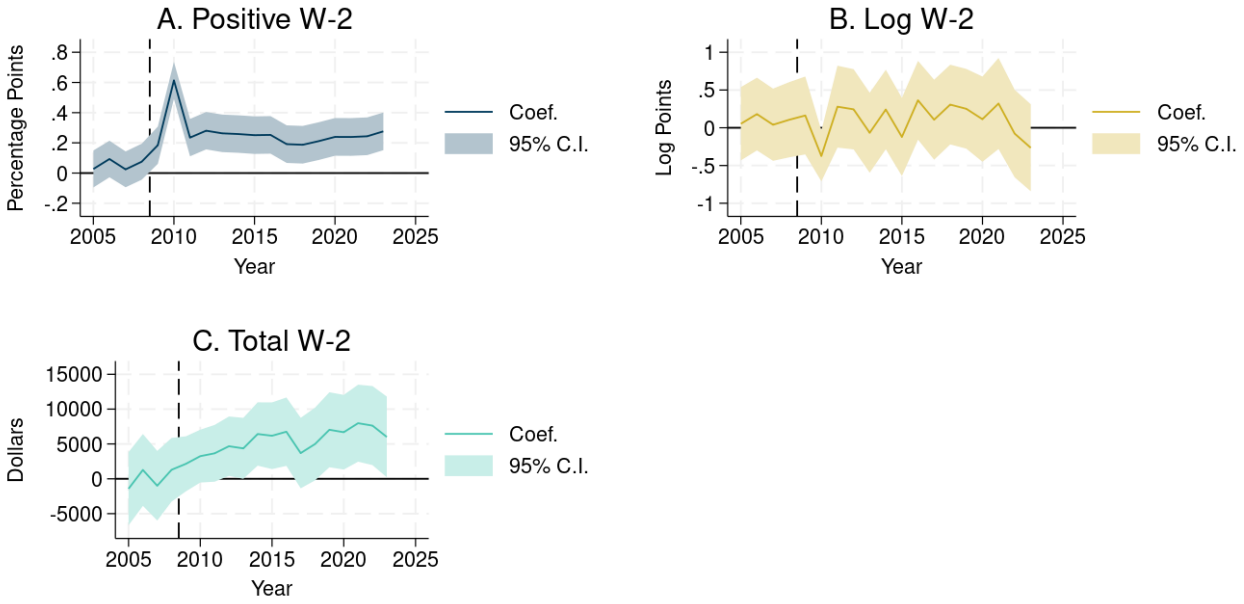
Source: 2010 Decennial Applicant, Personnel, and Payroll System.
Note: Figure excludes veterans and applicants with missing scores for non-supervisory positions.

Figure 2. RD Plots of Marginal Census Hiring on W-2 Employment



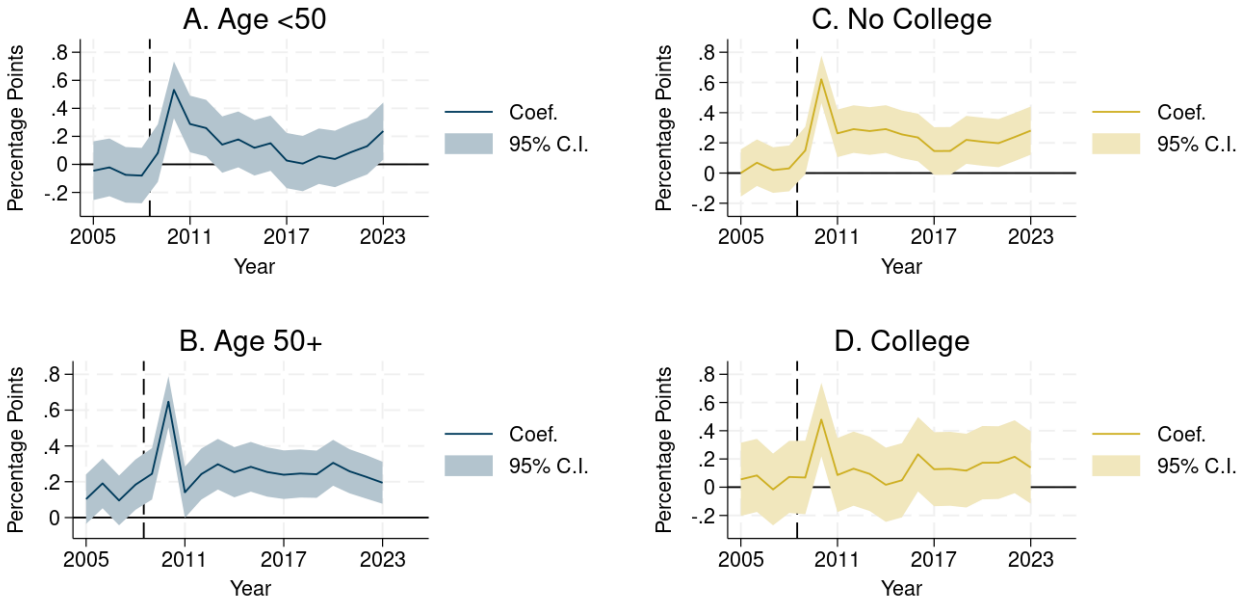
Source: 2010 Decennial Applicant, Personnel, and Payroll System.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel.

Figure 3. IV Estimates of Marginal Census Hiring on Labor Market Outcomes



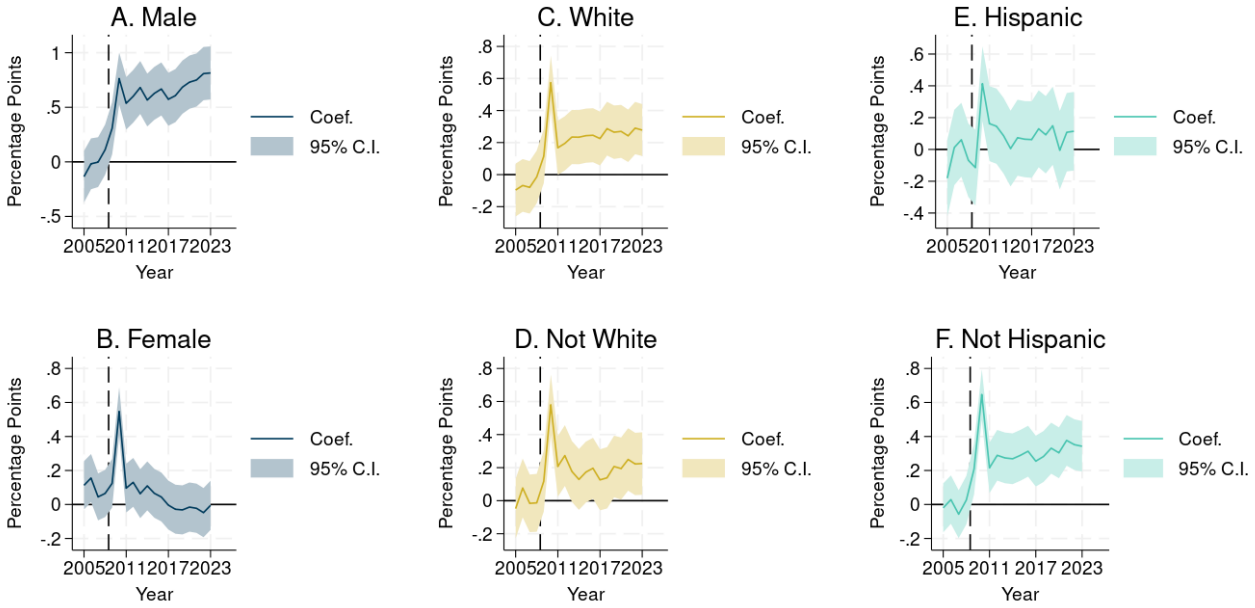
Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to Form W-2 records.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Figure 4. Heterogeneity in Positive W-2 Impacts by Applicant Characteristic



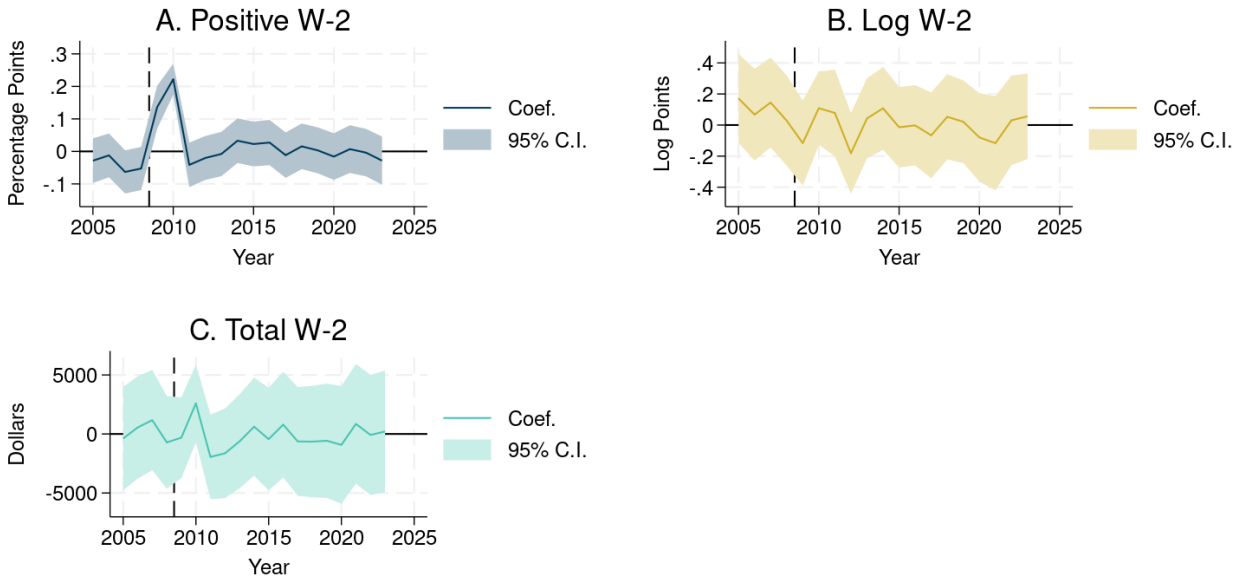
Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to Form W-2 records.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Figure 5. Heterogeneity in Positive W-2 Impacts by Census Demographic



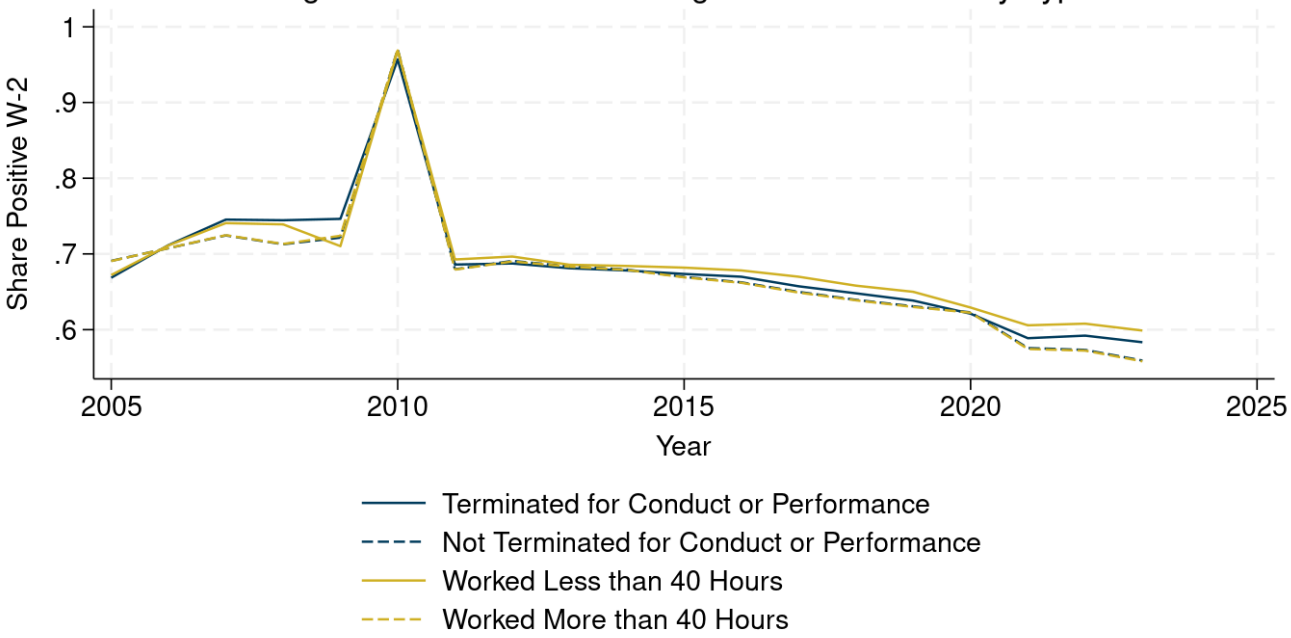
Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to Form W-2 records.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Figure 6. IV Estimates of General Census Hiring on Labor Market Outcomes



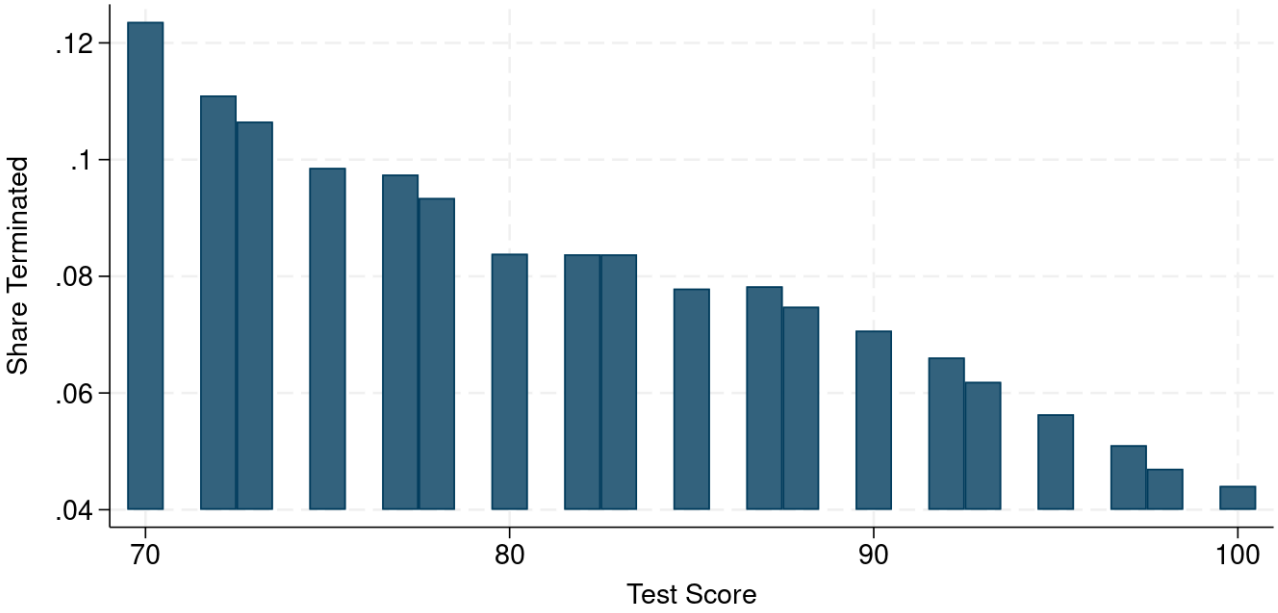
Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to Form W-2 records.
Note: Regressions are estimated for individuals with the lowest score on a selection sheet using an 'initial offer' instrument and selection sheet fixed effects. Standard errors clustered on selection sheet are provided. All regressions exclude veterans.

Figure 7. Work Paths of Marginal Census Hires by Type



Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to Form W-2 records.
Note: Sample includes all non-veteran hires with scores of 70-75.

Figure 8. Termination for Misconduct or Unsatisfactory Performance by Test Score



Source: 2010 Decennial Applicant, Personnel, and Payroll System.
Note: Figure excludes veterans and applicants with missing scores for non-supervisory positions.

Table 1 -

Summary Statistics by Test Score

Test Score	Count	Share Hired	Share Test Retake	Share Positive 2005 W-2	Average 2005 W-2 Value	Average Non-Zero 2005 W-2 Value	Average Age	Share with College Degree	Share in Selective Service	Share Male	Share White	Share Hispanic	Share Missing Race in 2010
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
55	2,400	0	0.01424	0.5396	13,150	24,380	56.21	0.1528	0.1127	0.4189	0.5028	0.3249	0.1877
57	3,800	0	0.01363	0.5497	12,820	23,320	51.72	0.1337	0.1365	0.3848	0.4326	0.3599	0.2178
58	6,900	-	0.02280	0.5692	13,130	23,060	49.17	0.1269	0.1725	0.3851	0.3957	0.3302	0.2222
60	12,000	-	0.03001	0.5764	13,160	22,830	47.68	0.1259	0.1814	0.3740	0.3764	0.3180	0.2227
62	19,500	0.001075	0.03715	0.5826	13,330	22,870	46.95	0.1313	0.1999	0.3843	0.3726	0.2838	0.2337
63	29,500	0.001086	0.05004	0.5851	13,540	23,140	46.30	0.1402	0.2116	0.3867	0.3665	0.2721	0.2364
65	41,000	0.0009040	0.06543	0.5916	13,850	23,420	45.62	0.1464	0.2183	0.3906	0.3661	0.2574	0.2341
67	53,000	0.001733	0.07922	0.5959	14,110	23,680	45.35	0.1610	0.2277	0.3976	0.3768	0.2510	0.2322
68	62,500	0.002055	0.09035	0.6005	14,480	24,110	45.00	0.1767	0.2326	0.4006	0.3884	0.2379	0.2290
70	90,500	0.06357	0.08911	0.6068	14,650	24,150	44.04	0.1957	0.2431	0.3787	0.3815	0.2383	0.2193
72	99,000	0.06701	0.08960	0.6147	15,320	24,920	43.78	0.2179	0.2453	0.3796	0.4008	0.2284	0.2144
73	111,000	0.07392	0.09033	0.6230	15,850	25,450	43.22	0.2334	0.2542	0.3812	0.4144	0.2215	0.2093
75	120,000	0.07967	0.08711	0.6281	16,160	25,730	42.87	0.2517	0.2601	0.3832	0.4308	0.2138	0.2026
77	132,000	0.08892	0.08576	0.6334	16,690	26,350	42.41	0.2647	0.2652	0.3834	0.4525	0.2082	0.1974
78	141,000	0.09691	0.08637	0.6407	17,170	26,790	42.05	0.2806	0.2700	0.3861	0.4689	0.2007	0.1907
80	153,000	0.1103	0.08395	0.6451	17,650	27,360	41.78	0.2968	0.2728	0.3834	0.4896	0.1955	0.1857
82	161,000	0.1219	0.08389	0.6538	18,120	27,720	41.45	0.3090	0.2796	0.3869	0.5107	0.1881	0.1799
83	175,000	0.1353	0.08423	0.6577	18,700	28,430	41.20	0.3264	0.2843	0.3903	0.5356	0.1796	0.1716
85	182,000	0.1534	0.08320	0.6662	19,150	28,740	40.99	0.3423	0.2886	0.3908	0.5607	0.1722	0.1650
87	199,000	0.1713	0.08374	0.6748	19,740	29,250	40.80	0.3628	0.2939	0.3951	0.5900	0.1628	0.1585
88	213,000	0.1917	0.08364	0.6818	20,330	29,820	40.58	0.3835	0.3030	0.4025	0.6202	0.1530	0.1510
90	232,000	0.2181	0.08347	0.6894	20,950	30,390	40.35	0.4093	0.3066	0.4041	0.6508	0.1442	0.1432
92	249,000	0.2467	0.08024	0.7002	21,830	31,180	40.15	0.4397	0.3140	0.4079	0.6840	0.1322	0.1324
93	268,000	0.2855	0.07719	0.7085	22,690	32,020	39.94	0.4800	0.3224	0.4129	0.7165	0.1177	0.1237
95	279,000	0.3292	0.07409	0.7196	23,480	32,630	39.68	0.5237	0.3297	0.4166	0.7469	0.1060	0.1147
97	270,000	0.3825	0.07000	0.7309	24,470	33,480	39.46	0.5770	0.3354	0.4175	0.7796	0.08952	0.1070
98	220,000	0.4356	0.06600	0.7438	25,460	34,230	39.19	0.6376	0.3410	0.4195	0.8106	0.07505	0.09670
100	123,000	0.4950	0.08148	0.7511	26,020	34,640	38.85	0.6928	0.3438	0.4146	0.8278	0.06269	0.08869

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records and 2010 Decennial Census.

Note: Table excludes veterans and applicants with missing scores for non-supervisory positions.

Table 2 -
Balance Tests

	Share Retake	Age	Share with College Degree	Share in Selective Service	Share Male	Share White	Share Hispanic	Share Missing Race in 2010	Share Hired
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RD 70 Estimate	-0.01568	-0.4358	0.001888	-0.0005725	-0.02901	-0.01947	0.01014	-0.006354	0.05974
RD 70 Standard Error	(0.002091)	(0.1234)	(0.00305)	(0.003284)	(0.003717)	(0.004206)	(0.003717)	(0.003195)	(0.0007479)
Within Score Estimate	-0.0006058	0.2508	0.004713	-0.00131	0.00207	0.0009248	-0.003327	-0.002341	0.1054
Within Score Standard Error	(0.003062)	(0.159)	(0.005729)	(0.005282)	(0.005386)	(0.004526)	(0.003285)	(0.003313)	(0.005944)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2010 Decennial Census.
Note: The RD 70 regression is estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided. There are approximately 589000 observations in the RD 70 regressions, with fewer in the college degree, selective service, and 2010 decennial outcomes due to missing variables. The within score regression is estimated for individuals with the lowest score on a selection sheet using an “initial offer” instrument and selection sheet fixed effects. Standard errors clustered on selection sheet are provided. All regressions exclude veterans.

Table 3 -

Labor Market Impacts of Census Hiring using RD 70 Method

	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Log W-2 Reduced Form	Log W-2 Instrumental Variables	Total W-2 Reduced Form	Total W-2 Instrumental Variables
	(1)	(2)	(3)	(4)	(5)	(6)
2005	0.001604	0.02712	0.00321	0.05312	-85.52	-1426
	(0.003727)	(0.06238)	(0.01508)	(0.2475)	(159.9)	(2676)
2006	0.005561	0.09321	0.01095	0.1816	76.07	1271
	(0.003688)	(0.06175)	(0.01471)	(0.2456)	(157.0)	(2628)
2007	0.001473	0.02465	0.002422	0.04006	-59.04	-996.2
	(0.003613)	(0.06049)	(0.01427)	(0.2437)	(152.2)	(2547)
2008	0.004482	0.07518	0.006175	0.1062	77.01	1296
	(0.003632)	(0.06080)	(0.01474)	(0.254)	(139.1)	(2328)
2009	0.011	0.1845	0.009926	0.1626	127.4	2151
	(0.003753)	(0.06286)	(0.01616)	(0.2630)	(120.1)	(2011)
2010	0.03661	0.6138	-0.03464	-0.3740	193.2	3253
	(0.003748)	(0.06265)	(0.01588)	(0.1712)	(115.4)	(1932)
2011	0.01399	0.2348	0.01589	0.2794	217.8	3657
	(0.003763)	(0.06308)	(0.01568)	(0.2766)	(124.0)	(2076)
2012	0.01673	0.2806	0.01396	0.2444	278.9	4679
	(0.003767)	(0.06318)	(0.01541)	(0.2709)	(129.7)	(2172)
2013	0.0157	0.2631	-0.003592	-0.06588	260.8	4369
	(0.003776)	(0.06332)	(0.01516)	(0.2700)	(133.2)	(2232)
2014	0.01542	0.2585	0.01342	0.2418	383.7	6432
	(0.003777)	(0.06334)	(0.01486)	(0.2684)	(138.2)	(2316)
2015	0.01498	0.2511	-0.006636	-0.1213	368.4	6179
	(0.003780)	(0.06339)	(0.01452)	(0.2640)	(145.6)	(2439)
2016	0.01510	0.2527	0.02000	0.3640	403.3	6761
	(0.003782)	(0.06341)	(0.01465)	(0.2660)	(149.3)	(2501)
2017	0.01144	0.1916	0.005757	0.1069	219.8	3694
	(0.003788)	(0.06348)	(0.01463)	(0.2702)	(154.0)	(2579)
2018	0.01119	0.1875	0.01666	0.3071	297.5	4995
	(0.003791)	(0.06354)	(0.01461)	(0.2691)	(158.9)	(2661)
2019	0.01266	0.2118	0.01346	0.2514	419.9	7037
	(0.003797)	(0.06364)	(0.01449)	(0.2692)	(163.7)	(2743)
2020	0.01432	0.2396	0.006198	0.1139	399.7	6688
	(0.003801)	(0.06372)	(0.01553)	(0.2880)	(163.1)	(2732)
2021	0.01428	0.2390	0.01690	0.3213	476.8	7981
	(0.003796)	(0.06366)	(0.0161)	(0.3073)	(168.2)	(2818)
2022	0.01452	0.2438	-0.003647	-0.07671	455.9	7638
	(0.003797)	(0.06369)	(0.01542)	(0.2957)	(172.5)	(2891)
2023	0.01648	0.2762	-0.01384	-0.2650	358.8	6007
	(0.003790)	(0.06359)	(0.01551)	(0.2938)	(176.3)	(2953)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Appendix A. Additional Figures and Tables

Figure A1. Employee Lifecycle Chart from Seebold (2012)

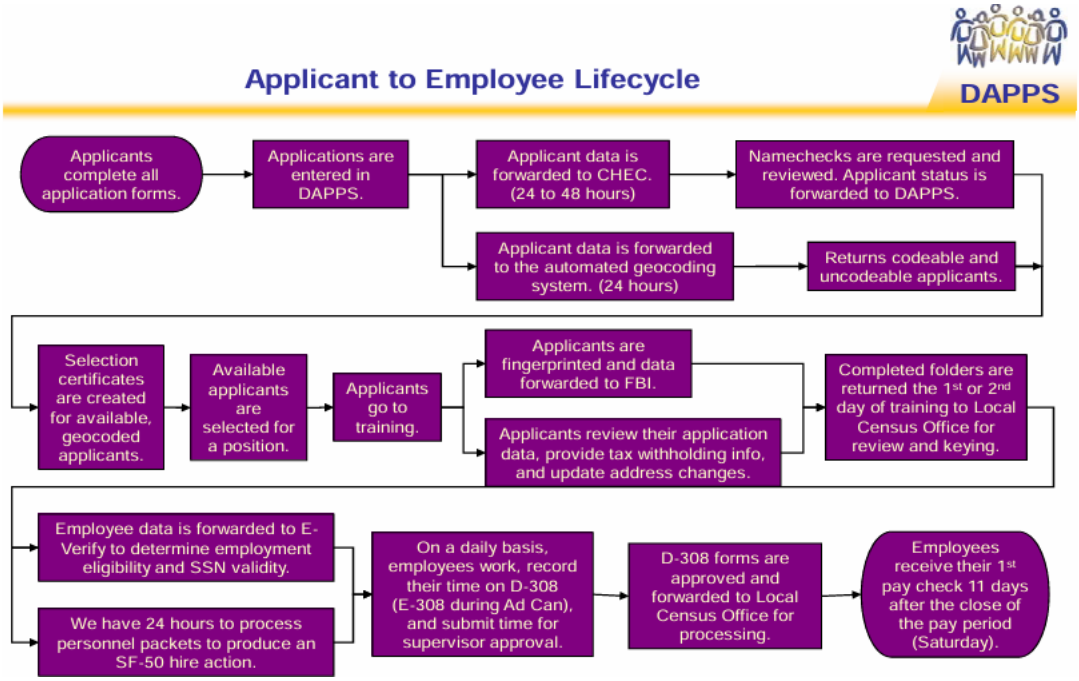
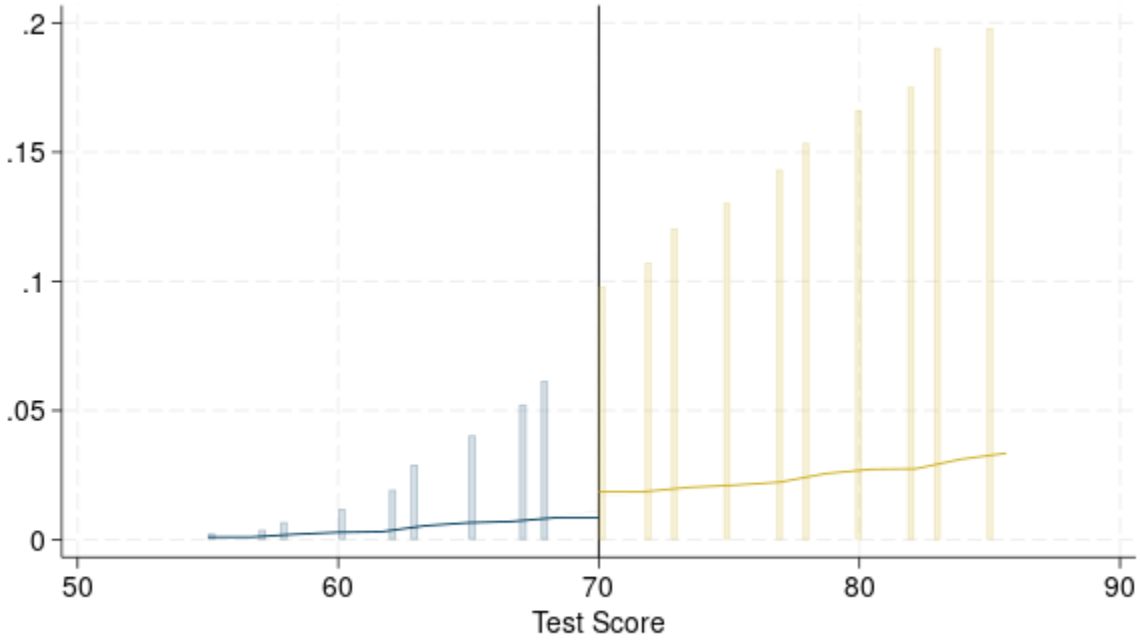


Figure A2. Manipulation Testing Plot



Source: 2010 Decennial Applicant, Personnel, and Payroll System.
Note: Figure excludes veterans and applicants with missing scores for non-supervisory positions.

Table A1 -

Labor Market Impacts of Census Hiring Robustness Checks

	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Positive W-2 ITT	Positive W-2 LATE
	RD 70 Robust RD		RD 70 Bandwidth of 10		Within Score DREO	
	(1)	(2)	(3)	(4)	(5)	(6)
2005	0.0006872	0.01195	0.002847	0.05029	-0.01174	-0.01976
	(0.006630)	(0.1110)	(0.002426)	(0.04274)	(0.003744)	(0.006298)
2006	0.007579	0.1269	0.004908	0.08665	-0.008545	-0.01438
	(0.006565)	(0.1099)	(0.002402)	(0.04231)	(0.003665)	(0.006166)
2007	0.003244	0.05444	0.002039	0.03600	-0.01674	-0.02817
	(0.006441)	(0.1078)	(0.002353)	(0.04145)	(0.003572)	(0.006005)
2008	0.008618	0.1444	0.003114	0.05494	-0.01783	-0.03000
	(0.006472)	(0.1083)	(0.002366)	(0.04168)	(0.003628)	(0.006103)
2009	0.01309	0.2191	0.01104	0.1947	0.01644	0.02766
	(0.006680)	(0.1119)	(0.002442)	(0.04307)	(0.00376)	(0.006329)
2010	0.04152	0.6945	0.03233	0.5704	0.05384	0.09061
	(0.006679)	(0.1117)	(0.002437)	(0.04283)	(0.002837)	(0.004739)
2011	0.0182	0.3047	0.01007	0.1778	-0.01166	-0.01962
	(0.006698)	(0.1122)	(0.002450)	(0.04320)	(0.003711)	(0.006242)
2012	0.02074	0.3472	0.01427	0.2520	-0.01111	-0.01869
	(0.006705)	(0.1123)	(0.002451)	(0.04328)	(0.003738)	(0.006288)
2013	0.01654	0.2763	0.01649	0.2905	-0.008286	-0.01394
	(0.006720)	(0.1126)	(0.002457)	(0.04341)	(0.003586)	(0.006033)
2014	0.01778	0.2972	0.01562	0.2757	-0.006774	-0.01140
	(0.006722)	(0.1126)	(0.002457)	(0.04341)	(0.003663)	(0.006163)
2015	0.01761	0.2945	0.01471	0.2594	-0.009899	-0.01666
	(0.006727)	(0.1127)	(0.002459)	(0.04344)	(0.003725)	(0.006268)
2016	0.01608	0.2681	0.01594	0.2809	-0.01080	-0.01818
	(0.006728)	(0.1127)	(0.002461)	(0.04347)	(0.003672)	(0.006178)
2017	0.01428	0.2385	0.01259	0.2218	-0.01443	-0.02429
	(0.006738)	(0.1129)	(0.002465)	(0.04351)	(0.003775)	(0.006349)
2018	0.01448	0.2418	0.01118	0.1971	-0.01335	-0.02247
	(0.006742)	(0.1129)	(0.002467)	(0.04353)	(0.003861)	(0.006494)
2019	0.01811	0.3023	0.01153	0.2029	-0.009987	-0.01681
	(0.006749)	(0.1131)	(0.002470)	(0.04359)	(0.003915)	(0.006584)
2020	0.02155	0.3600	0.01312	0.2311	-0.01469	-0.02473
	(0.006750)	(0.1131)	(0.002472)	(0.04365)	(0.003925)	(0.006605)
2021	0.02050	0.3424	0.01214	0.2138	-0.01327	-0.02233
	(0.006738)	(0.1129)	(0.002470)	(0.04360)	(0.004058)	(0.006826)
2022	0.01840	0.3082	0.01314	0.2318	-0.01104	-0.01858
	(0.006740)	(0.1129)	(0.002470)	(0.04362)	(0.004016)	(0.006757)
2023	0.01958	0.3275	0.01434	0.2527	-0.01362	-0.02291
	(0.006726)	(0.1127)	(0.002466)	(0.04356)	(0.004012)	(0.00675)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.

Note: RD regressions are estimated using a local linear regression and a uniform kernel. Within score regressions are estimated for individuals with the lowest score on a selection sheet. All regressions exclude veterans.

Table A2 -

Heterogeneity in Positive W-2 Impacts by Applicant Characteristic

	Age <50	Age 50+	No College	College
	(1)	(2)	(3)	(4)
2005	-0.04541	0.1031	0.0007307	0.05573
	(0.1069)	(0.07047)	(0.07960)	(0.1322)
2006	-0.02150	0.1909	0.06869	0.08345
	(0.1051)	(0.07079)	(0.07857)	(0.1319)
2007	-0.07491	0.09555	0.01965	-0.01588
	(0.1012)	(0.07073)	(0.07696)	(0.1296)
2008	-0.08009	0.1840	0.03086	0.07282
	(0.1008)	(0.07170)	(0.07730)	(0.1302)
2009	0.08048	0.2442	0.1495	0.06865
	(0.1047)	(0.07314)	(0.08008)	(0.1328)
2010	0.5315	0.6468	0.6207	0.4789
	(0.1032)	(0.07266)	(0.07975)	(0.1327)
2011	0.2880	0.1414	0.2629	0.08612
	(0.1032)	(0.07287)	(0.08025)	(0.1337)
2012	0.2591	0.2430	0.2911	0.1313
	(0.1027)	(0.07265)	(0.08030)	(0.1339)
2013	0.1412	0.2979	0.2785	0.0943
	(0.1025)	(0.07209)	(0.08047)	(0.1340)
2014	0.1782	0.2531	0.2914	0.01716
	(0.1017)	(0.07147)	(0.08048)	(0.1340)
2015	0.1182	0.2834	0.2562	0.04908
	(0.1010)	(0.07079)	(0.08047)	(0.1340)
2016	0.1509	0.2543	0.2356	0.2332
	(0.1005)	(0.07020)	(0.08046)	(0.1342)
2017	0.02715	0.2393	0.1460	0.1274
	(0.1005)	(0.06917)	(0.08056)	(0.1338)
2018	0.005582	0.2460	0.1465	0.1304
	(0.1005)	(0.06843)	(0.08065)	(0.1337)
2019	0.05812	0.2420	0.2200	0.1174
	(0.1009)	(0.06717)	(0.08090)	(0.1333)
2020	0.03864	0.3057	0.2075	0.1735
	(0.1028)	(0.0653)	(0.08114)	(0.1327)
2021	0.08728	0.2582	0.1981	0.1741
	(0.1037)	(0.06268)	(0.08119)	(0.1314)
2022	0.1299	0.2275	0.2384	0.2159
	(0.1030)	(0.06189)	(0.08123)	(0.1316)
2023	0.2375	0.1945	0.2812	0.1395
	(0.1039)	(0.05984)	(0.08124)	(0.1305)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.

Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Table A3 -

Heterogeneity in Positive W-2 Impacts by Census Demographic

	Male	Female	White	Not White	Hispanic	Not Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)
2005	-0.1352	0.1126	-0.09620	-0.04797	-0.1806	-0.02004
	(0.1203)	(0.07258)	(0.08416)	(0.09215)	(0.1229)	(0.0733)
2006	-0.01763	0.1556	-0.06755	0.07689	0.01243	0.02851
	(0.1188)	(0.07192)	(0.08376)	(0.09043)	(0.1209)	(0.07250)
2007	-0.002503	0.04468	-0.07944	-0.01609	0.06079	-0.05721
	(0.1164)	(0.07044)	(0.08347)	(0.08875)	(0.1185)	(0.07185)
2008	0.1112	0.06481	-0.01449	-0.01301	-0.06825	0.02625
	(0.1172)	(0.07073)	(0.08379)	(0.08933)	(0.1176)	(0.07252)
2009	0.3074	0.1240	0.1149	0.1175	-0.1144	0.2086
	(0.1219)	(0.07288)	(0.08585)	(0.09366)	(0.1215)	(0.07536)
2010	0.7632	0.5471	0.5739	0.5800	0.4138	0.6467
	(0.1216)	(0.07261)	(0.0855)	(0.09325)	(0.1194)	(0.07524)
2011	0.5367	0.09605	0.1677	0.2072	0.1622	0.2157
	(0.1226)	(0.07312)	(0.08621)	(0.09363)	(0.1202)	(0.07574)
2012	0.6007	0.1302	0.1950	0.2730	0.1461	0.2893
	(0.1230)	(0.07318)	(0.08632)	(0.09372)	(0.1207)	(0.07592)
2013	0.6812	0.06349	0.2347	0.1769	0.08660	0.2739
	(0.1236)	(0.07332)	(0.08632)	(0.09382)	(0.1213)	(0.07607)
2014	0.5669	0.1093	0.2337	0.1285	0.005433	0.2688
	(0.1234)	(0.07333)	(0.08621)	(0.09376)	(0.1215)	(0.07613)
2015	0.6276	0.06576	0.2431	0.1733	0.07388	0.2889
	(0.1238)	(0.07334)	(0.08614)	(0.09391)	(0.1219)	(0.07621)
2016	0.6680	0.04397	0.2458	0.1963	0.06462	0.3133
	(0.1240)	(0.07332)	(0.08596)	(0.09390)	(0.1221)	(0.07628)
2017	0.5738	-0.003796	0.2251	0.1261	0.06206	0.2549
	(0.1239)	(0.07344)	(0.08565)	(0.09425)	(0.1228)	(0.07629)
2018	0.6069	-0.02803	0.2869	0.1384	0.1298	0.2820
	(0.1241)	(0.07350)	(0.08548)	(0.0945)	(0.1234)	(0.07635)
2019	0.6845	-0.03206	0.2647	0.2067	0.09218	0.3310
	(0.1244)	(0.07363)	(0.08506)	(0.09501)	(0.1240)	(0.07641)
2020	0.7304	-0.01531	0.2699	0.1945	0.1490	0.3039
	(0.1244)	(0.07381)	(0.08425)	(0.09575)	(0.1252)	(0.07621)
2021	0.7500	-0.02231	0.2421	0.2496	-0.00497	0.3769
	(0.1242)	(0.07381)	(0.08335)	(0.09626)	(0.1257)	(0.07600)
2022	0.8088	-0.04809	0.2905	0.2221	0.1082	0.3519
	(0.1244)	(0.07382)	(0.08329)	(0.09617)	(0.1258)	(0.07599)
2023	0.8147	-0.005049	0.2780	0.2249	0.1144	0.3424
	(0.1239)	(0.07377)	(0.08251)	(0.09635)	(0.1261)	(0.07569)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records and 2010 Decennial Census.

Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided.

Table A4 -

Heterogeneity in Positive W-2 Impacts by State

	Medicaid Unaffected	Medicaid Affected	SNAP Unaffected	SNAP Affected	High UR	Low UR
	(1)	(2)	(3)	(4)	(5)	(6)
2005	-0.003291	0.03289	0.002356	0.06448	-0.07368	0.1379
	(0.07484)	(0.1101)	(0.06907)	(0.1418)	(0.09190)	(0.08316)
2006	0.03881	0.1400	0.1073	-0.02672	0.007554	0.1870
	(0.07374)	(0.1096)	(0.06818)	(0.1416)	(0.09057)	(0.08276)
2007	0.05721	-0.05882	0.02303	0.008597	0.002536	0.04562
	(0.07286)	(0.1069)	(0.06730)	(0.1373)	(0.08965)	(0.08028)
2008	0.004872	0.1728	0.03507	0.2075	0.006269	0.1489
	(0.07328)	(0.1074)	(0.06763)	(0.1382)	(0.09046)	(0.08029)
2009	0.2106	0.1262	0.1811	0.1769	0.09199	0.2849
	(0.07651)	(0.1096)	(0.07011)	(0.1413)	(0.09389)	(0.08252)
2010	0.6140	0.6012	0.5984	0.6533	0.5752	0.6550
	(0.07625)	(0.1092)	(0.06989)	(0.1407)	(0.09358)	(0.08221)
2011	0.2224	0.2482	0.2382	0.2042	0.1278	0.3512
	(0.07684)	(0.1099)	(0.07043)	(0.1415)	(0.09397)	(0.08319)
2012	0.2841	0.2706	0.2508	0.3799	0.2100	0.3567
	(0.07709)	(0.1098)	(0.07052)	(0.1419)	(0.09411)	(0.08330)
2013	0.2752	0.2399	0.2468	0.3105	0.2128	0.3167
	(0.07732)	(0.1100)	(0.07073)	(0.1419)	(0.09433)	(0.08344)
2014	0.3042	0.1791	0.2553	0.2574	0.1405	0.3868
	(0.0774)	(0.1099)	(0.07079)	(0.1417)	(0.09432)	(0.08359)
2015	0.2774	0.2054	0.2813	0.1186	0.2067	0.2984
	(0.07745)	(0.1100)	(0.07088)	(0.1416)	(0.09439)	(0.08359)
2016	0.2885	0.1953	0.2530	0.2384	0.1862	0.3245
	(0.07753)	(0.1099)	(0.07088)	(0.1418)	(0.09442)	(0.08364)
2017	0.2360	0.1221	0.1972	0.1591	0.1293	0.2579
	(0.07763)	(0.1100)	(0.07099)	(0.1418)	(0.09454)	(0.0837)
2018	0.2334	0.1171	0.2100	0.09215	0.1527	0.2227
	(0.07769)	(0.1101)	(0.07108)	(0.1417)	(0.09467)	(0.08369)
2019	0.2871	0.09086	0.2407	0.09204	0.1457	0.2821
	(0.07784)	(0.1103)	(0.07121)	(0.1418)	(0.09481)	(0.08387)
2020	0.3375	0.08019	0.2576	0.1623	0.1707	0.3133
	(0.07791)	(0.1105)	(0.07132)	(0.1419)	(0.09496)	(0.08394)
2021	0.2854	0.1712	0.2606	0.1501	0.2271	0.2493
	(0.07771)	(0.1106)	(0.07125)	(0.1418)	(0.09494)	(0.08374)
2022	0.2959	0.1714	0.2741	0.1247	0.2702	0.2143
	(0.07774)	(0.1106)	(0.07128)	(0.1418)	(0.09500)	(0.08373)
2023	0.3088	0.2354	0.2866	0.2307	0.2963	0.2520
	(0.07754)	(0.1106)	(0.07117)	(0.1417)	(0.09486)	(0.08362)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.
Note: Regressions are estimated using a local linear regression with a bandwidth of 5 and a uniform kernel. Robust standard errors are provided. Medicaid states include AZ, AR, CO, FL, GA, HI, ID, IL, IA, KY, LA, MD, MA, MI, MT, NV, NH, NM, NC, ND, OH, OK, OR, PA, SC, SD, TN, TX, UT, VT, VA, WA, WI, and WY. SNAP states include AK, AZ, CA, CT, DE, DC, FL, GA, HI, ID, IL, KS, KY, LA, ME, MD, MA, MS, MO, NY, NC, ND, OH, OK, OR, PA, RI, SC, SD, TX, UT, VT, VA, WA, WV, WI, and WY. High UR states include AL, AZ, CA, CO, CT, DC, FL, GA, ID, IL, IN, KY, MI, MS, MO, NV, NJ, NC, OH, OR, RI, SC, TN, and WA.

Table A5 -

Labor Market Impacts of Census Hiring using Within Score Method

	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Log W-2 Reduced Form	Log W-2 Instrumental Variables	Total W-2 Reduced Form	Total W-2 Instrumental Variables
	(1)	(2)	(3)	(4)	(5)	(6)
2005	-0.003117	-0.02840	0.01930	0.1716	-35.00	-382.7
	(0.005007)	(0.03501)	(0.02232)	(0.1462)	(318.5)	(2230)
2006	-0.001419	-0.01225	0.008134	0.0674	64.83	554.8
	(0.004909)	(0.03437)	(0.02180)	(0.1504)	(315.4)	(2210)
2007	-0.00668	-0.06313	0.01505	0.1449	126.3	1171
	(0.004846)	(0.03385)	(0.02112)	(0.1475)	(308.8)	(2166)
2008	-0.005824	-0.05260	0.003981	0.02868	-75.64	-713.3
	(0.004855)	(0.03387)	(0.02151)	(0.1481)	(285.4)	(1996)
2009	0.01426	0.1363	-0.01287	-0.1164	-36.14	-310.2
	(0.004729)	(0.03359)	(0.02083)	(0.1397)	(249.4)	(1745)
2010	0.02351	0.2221	0.01087	0.1087	276.3	2592
	(0.003664)	(0.02442)	(0.01654)	(0.1204)	(236.7)	(1671)
2011	-0.004519	-0.04143	0.008355	0.07821	-210.2	-1941
	(0.005007)	(0.03495)	(0.02084)	(0.1421)	(262.1)	(1826)
2012	-0.00207	-0.02013	-0.01913	-0.1817	-175.9	-1636
	(0.004933)	(0.03448)	(0.01969)	(0.1321)	(277.5)	(1934)
2013	-0.0009258	-0.007874	0.004519	0.04291	-64.95	-626.0
	(0.004970)	(0.03476)	(0.01943)	(0.1315)	(289.9)	(2027)
2014	0.003393	0.03295	0.01086	0.1078	66.41	618.6
	(0.004982)	(0.03499)	(0.01949)	(0.1359)	(301.8)	(2115)
2015	0.002408	0.02283	-0.0004546	-0.01387	-38.11	-439.6
	(0.005009)	(0.03515)	(0.01906)	(0.1320)	(317.1)	(2218)
2016	0.002939	0.02724	0.0005964	-0.001967	89.89	797.3
	(0.005064)	(0.03554)	(0.01929)	(0.1318)	(325.2)	(2280)
2017	-0.001236	-0.01153	-0.005758	-0.06621	-60.65	-631.8
	(0.005094)	(0.03564)	(0.01981)	(0.1408)	(335.7)	(2348)
2018	0.001586	0.01565	0.005398	0.05315	-68.71	-646.7
	(0.005110)	(0.03579)	(0.01949)	(0.1399)	(344.1)	(2407)
2019	0.0003877	0.003308	0.001420	0.02135	-55.11	-573.4
	(0.005147)	(0.03605)	(0.01953)	(0.1358)	(352.9)	(2469)
2020	-0.001637	-0.01588	-0.00895	-0.07962	-100.2	-925.1
	(0.005220)	(0.03653)	(0.02152)	(0.1452)	(363.3)	(2540)
2021	0.0007643	0.007119	-0.01248	-0.1167	86.62	856.8
	(0.005345)	(0.03744)	(0.02232)	(0.1544)	(368.6)	(2582)
2022	-0.0004125	-0.003685	0.002931	0.03091	-14.51	-79.65
	(0.005324)	(0.03727)	(0.02179)	(0.1470)	(370.8)	(2595)
2023	-0.003049	-0.02826	0.006489	0.05642	16.31	209.8
	(0.005386)	(0.03764)	(0.02155)	(0.1404)	(375.3)	(2628)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.
Note: Regressions are estimated for individuals with the lowest score on a selection sheet using an “initial offer” instrument and selection sheet fixed effects. Standard errors clustered on selection sheet are provided. All regressions exclude veterans.

Table A6 -

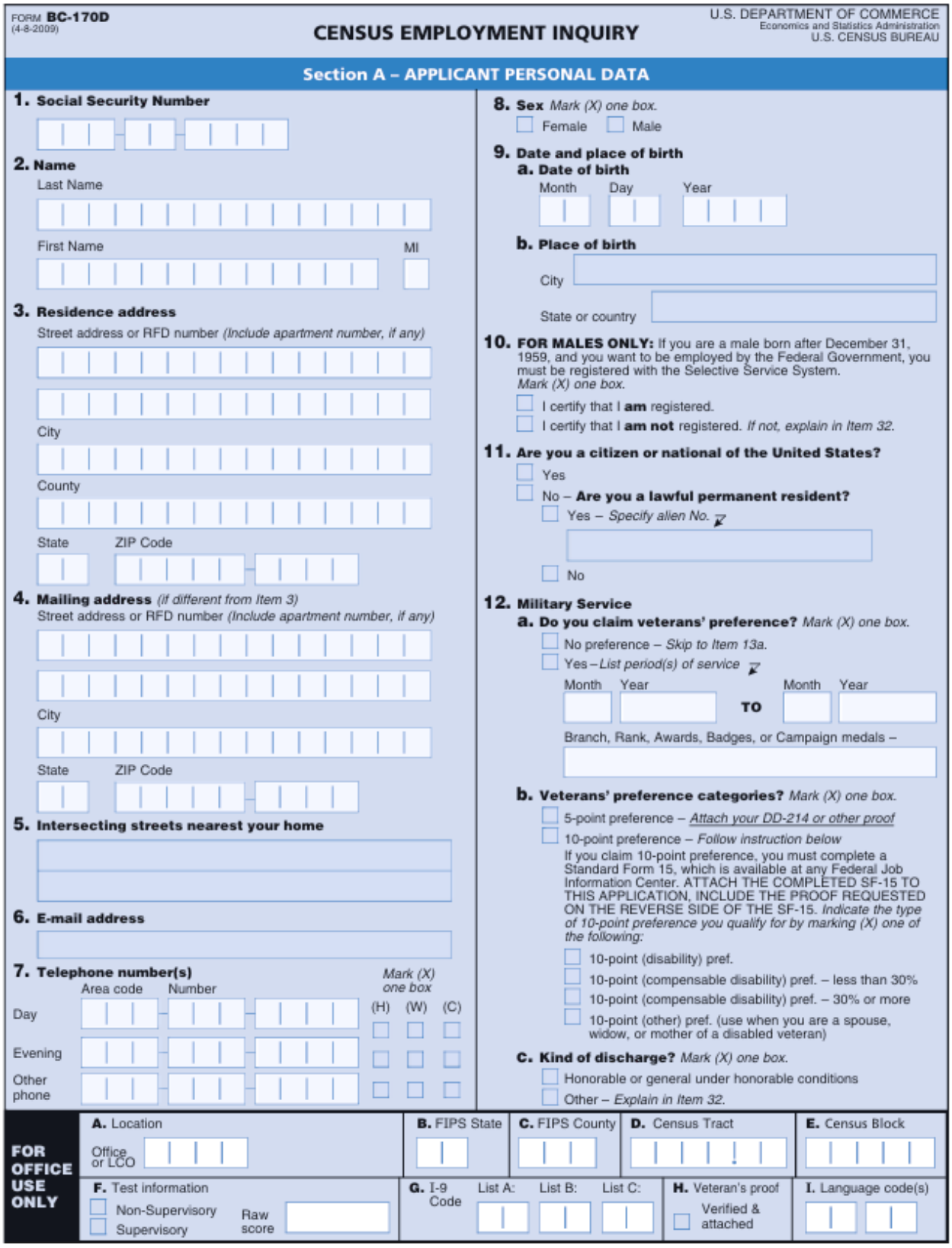
Heterogeneity in Positive W-2 Impacts by Test Score Bin

	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables	Positive W-2 Reduced Form	Positive W-2 Instrumental Variables
	Score 70-80		Score 82-90		Score 92-100	
	(1)	(2)	(3)	(4)	(5)	(6)
2005	0.01436	0.1054	-0.01178	-0.07891	-0.002645	-0.02854
	(0.01819)	(0.08507)	(0.01079)	(0.05222)	(0.005942)	(0.05325)
2006	0.01373	0.09617	-0.007198	-0.04762	-0.001581	-0.01532
	(0.01793)	(0.08408)	(0.01043)	(0.05021)	(0.005851)	(0.05259)
2007	0.01815	0.1262	-0.01162	-0.07565	-0.008631	-0.1014
	(0.01812)	(0.08568)	(0.01037)	(0.0497)	(0.005738)	(0.0515)
2008	0.004646	0.03028	-0.02004	-0.1302	-0.00221	-0.02146
	(0.01839)	(0.08588)	(0.0102)	(0.04903)	(0.005774)	(0.0518)
2009	0.03327	0.2456	-0.001852	-0.01283	0.01717	0.2064
	(0.01885)	(0.09153)	(0.01032)	(0.04957)	(0.005479)	(0.05085)
2010	0.06419	0.4621	0.03408	0.2250	0.01319	0.1569
	(0.01651)	(0.07208)	(0.008084)	(0.03651)	(0.004092)	(0.03543)
2011	0.02555	0.1830	-0.01139	-0.07655	-0.007008	-0.07891
	(0.01848)	(0.08902)	(0.01046)	(0.05013)	(0.005997)	(0.0537)
2012	0.01955	0.1426	-0.009897	-0.06624	-0.002891	-0.03467
	(0.01884)	(0.08968)	(0.01024)	(0.0492)	(0.005894)	(0.05287)
2013	0.01669	0.1244	-0.008387	-0.05713	-0.0008748	-0.008081
	(0.01897)	(0.08985)	(0.01048)	(0.05038)	(0.005902)	(0.053)
2014	0.02611	0.1923	-0.005175	-0.03326	0.002989	0.03615
	(0.01899)	(0.09147)	(0.01043)	(0.05008)	(0.005934)	(0.05357)
2015	0.01881	0.1407	-0.004956	-0.03322	0.002566	0.03012
	(0.01922)	(0.09099)	(0.0104)	(0.05)	(0.005976)	(0.05395)
2016	0.01318	0.09693	-0.008668	-0.0560	0.005445	0.06281
	(0.0192)	(0.09014)	(0.01065)	(0.0512)	(0.006028)	(0.05464)
2017	0.006477	0.04793	-0.008226	-0.05368	-0.00001761	-0.0003006
	(0.01933)	(0.09021)	(0.01062)	(0.05101)	(0.006085)	(0.05472)
2018	0.01359	0.1005	-0.0001012	-0.0002424	0.0002873	0.003988
	(0.01919)	(0.0901)	(0.01055)	(0.0506)	(0.006137)	(0.05516)
2019	0.01112	0.08617	0.005944	0.03851	-0.003428	-0.04247
	(0.01955)	(0.09119)	(0.01055)	(0.05089)	(0.006182)	(0.0555)
2020	0.005391	0.04137	-0.001299	-0.008468	-0.00285	-0.03524
	(0.01954)	(0.09086)	(0.01073)	(0.05158)	(0.006279)	(0.0564)
2021	0.01533	0.1173	-0.004947	-0.03197	0.0002931	0.001459
	(0.02004)	(0.09422)	(0.01095)	(0.05262)	(0.006433)	(0.05784)
2022	0.0129	0.1019	-0.00344	-0.02251	-0.001646	-0.02126
	(0.01967)	(0.0921)	(0.01093)	(0.05253)	(0.006419)	(0.05764)
2023	0.009068	0.07238	-0.005536	-0.03574	-0.004542	-0.05495
	(0.01987)	(0.09271)	(0.01097)	(0.05264)	(0.006515)	(0.05842)

Source: 2010 Decennial Applicant, Personnel, and Payroll System linked to 2005-2023 Form W-2 records.

Note: Regressions are estimated for individuals with the lowest score on a selection sheet using an "initial offer" instrument and selection sheet fixed effects. Standard errors clustered on selection sheet are provided. All regressions exclude veterans.

Appendix B. Application



FORM **BC-170D**
(4-8-2009)

CENSUS EMPLOYMENT INQUIRY

U.S. DEPARTMENT OF COMMERCE
Economics and Statistics Administration
U.S. CENSUS BUREAU

Section A – APPLICANT PERSONAL DATA

1. Social Security Number

2. Name
Last Name
First Name MI

3. Residence address
Street address or RFD number *(Include apartment number, if any)*
City
County
State ZIP Code

4. Mailing address *(if different from Item 3)*
Street address or RFD number *(Include apartment number, if any)*
City
State ZIP Code

5. Intersecting streets nearest your home

6. E-mail address

7. Telephone number(s)
Area code Number *Mark (X) one box* (H) (W) (C)
Day
Evening
Other phone

8. Sex *Mark (X) one box.*
☐ Female ☐ Male

9. Date and place of birth
a. Date of birth
Month Day Year
b. Place of birth
City
State or country

10. FOR MALES ONLY: If you are a male born after December 31, 1959, and you want to be employed by the Federal Government, you must be registered with the Selective Service System.
Mark (X) one box.
☐ I certify that I **am** registered.
☐ I certify that I **am not** registered. *If not, explain in Item 32.*

11. Are you a citizen or national of the United States?
☐ Yes
☐ No – **Are you a lawful permanent resident?**
☐ Yes – *Specify alien No.*
☐ No

12. Military Service
a. Do you claim veterans' preference? *Mark (X) one box.*
☐ No preference – *Skip to Item 13a.*
☐ Yes – *List period(s) of service*
Month Year **TO** Month Year
Branch, Rank, Awards, Badges, or Campaign medals –

b. Veterans' preference categories? *Mark (X) one box.*
☐ 5-point preference – *Attach your DD-214 or other proof*
☐ 10-point preference – *Follow instruction below*
If you claim 10-point preference, you must complete a Standard Form 15, which is available at any Federal Job Information Center. ATTACH THE COMPLETED SF-15 TO THIS APPLICATION, INCLUDE THE PROOF REQUESTED ON THE REVERSE SIDE OF THE SF-15. *Indicate the type of 10-point preference you qualify for by marking (X) one of the following:*
☐ 10-point (disability) pref.
☐ 10-point (compensable disability) pref. – less than 30%
☐ 10-point (compensable disability) pref. – 30% or more
☐ 10-point (other) pref. (use when you are a spouse, widow, or mother of a disabled veteran)

c. Kind of discharge? *Mark (X) one box.*
☐ Honorable or general under honorable conditions
☐ Other – *Explain in Item 32.*

FOR OFFICE USE ONLY

A. Location	B. FIPS State	C. FIPS County	D. Census Tract	E. Census Block
Office or LCO				

F. Test information	G. I-9 Code	List A:	List B:	List C:	H. Veteran's proof	I. Language code(s)
☐ Non-Supervisory ☐ Supervisory Raw score					☐ Verified & attached	

Section A – APPLICANT PERSONAL DATA – Con.

13a. Education – *Mark (X) highest education level.*

- ☐ Some high school – Did not graduate
- ☐ High school diploma/GED
- ☐ Technical degree/Trade school degree or certificate
- ☐ Associate's degree
- ☐ Bachelor's degree
- ☐ Master's degree
- ☐ Doctoral degree

b. Complete when a Technical/Trade school program, an Associate's, Bachelor's, Master's or Doctoral degree is selected in 13a.

		Degree (if any)	Year received
Name of institution			
City			
State or country			

Section B – RECRUITING SOURCES

14. How did you hear about census job opportunities? *Mark (X) one box* ***only.***

- ☐ Poster/flyer
- ☐ Community or organization newsletter/newspaper/leader
- ☐ Federal, state, tribal employment office/ Job service and information center
- ☐ Speech/presentation by Census employee
- ☐ Friend or relative working for Census
- ☐ Toll-free Census number or job line
- ☐ Census job mailing or postcard
- ☐ Friend or relative not working for Census
- ☐ Job Fair
- ☐ Newspaper – advertisement
- ☐ Newspaper – article
- ☐ Radio
- ☐ Internet/E-Mail
- ☐ School or college
- ☐ Business/private company
- ☐ TV
- ☐ Other – *Specify*

Section C – APPLICATION DATA

Most field positions require dealing with the public and knocking on doors to collect personal information, which may not be discussed or shared with anyone except for sworn Census Bureau employees.

15. Are you willing to work in the field, verifying household address listings and knocking on doors to collect information?

- ☐ Yes
- ☐ No

Most office positions involve working with computers.

16. Are you willing to work in the office? (There are a limited number of these jobs available.)

- ☐ Yes
- ☐ No

17. Do you have personal computer experience?

- ☐ Yes
- ☐ No

18. Are you now employed by a federal, state, local or tribal government? – *If yes, indicate date of hire and current grade level in Item 32.*

- ☐ Yes – *Name of agency* →
- ☐ No

19. Are you now employed by a law enforcement agency?

- ☐ Yes – *Name of agency* →
- ☐ No

20. Do you have supervisory experience?

☐ Yes – *Describe in Item 32.* ☐ No

21. Do you receive retirement or have you ever applied for retirement pay, pension, or other pay based on federal civilian or District of Columbia Government service?

☐ Yes – *Explain in Item 32.* ☐ No

22. Do any of your relatives work for the Census Bureau? *Include – Father, mother, husband, wife, son, daughter, brother, sister, uncle, aunt, first cousin, nephew, niece, father-in-law, mother-in-law, son-in-law, daughter-in-law, stepfather, stepmother, stepson, stepdaughter, stepbrother, stepsister, half brother, and half sister.*

- ☐ Yes – *Provide location (city and state) and position.*
- ☐ No

23. Hiring may be based in part on the hours you are available to work. *Indicate your availability by placing "X" in the appropriate box(es) for each day.*

	Sun	Mon	Tues	Wed	Thurs	Fri	Sat
a. Any hours	☐	☐	☐	☐	☐	☐	☐
b. Morning (8 a.m. – 12 noon)	☐	☐	☐	☐	☐	☐	☐
c. Afternoon (12 noon – 4 p.m.)	☐	☐	☐	☐	☐	☐	☐
d. Evening (4 p.m. – 9 p.m.)	☐	☐	☐	☐	☐	☐	☐

24. Total hours per week you are willing to work. *You may only work up to 40 hours per week.*

Section D – LANGUAGE SKILLS

25. Some census jobs require census employees to conduct the census interview by reading and recording responses to questions in a language other than English. The census employee must also be able to convince individuals who speak no English to respond to the interview by explaining the purpose and importance of the census. **Employees will receive Census training, but not language training.**

Are you fluent enough in any specific language to hold a conversation, to easily read and record responses, as well as to respond to questions in that language with individuals who speak **no** English? *If so, please list the language(s) below and mark (X) to all that apply.*

Language(s)	Speak	Read	Write
	☐	☐	☐
	☐	☐	☐

Section E – TRANSPORTATION

26a. Indicate the type(s) of transportation available for your use – *Mark (X)* ***ALL*** *that apply.*

- ☐ Automobile
- ☐ 4-Wheel Drive
- ☐ Airplane
- ☐ Boat
- ☐ ATV (All terrain vehicle)
- ☐ Other – *Describe* →
- ☐ None

b. Do you have a valid driver's license?

☐ Yes ☐ No

Section F – BACKGROUND INFORMATION

Answer questions 27 through 31 below. Read each statement carefully before responding. *Mark (X) one*

27. During the past **5 years**, have you been **fired from any job** for any reason, did you **quit after being told that you would be fired**, or did you leave any job by mutual agreement because of specific problems, or were you debarred from Federal employment by the Office of Personnel Management or any other Federal agency? *If "YES," use **Item 32** (below) to write for each job a) the name and address of the employer; b) the approximate date you left the job, and c) an explanation of the problem and the reason for leaving.* ☐ Yes ☐ No

When answering questions 28 through 31 you may omit: 1) traffic fines of $300 or less; 2) any violation of law committed before your 16th birthday, 3) any violation of law committed before your 18th birthday if finally decided in juvenile court or under a Youth Offender law, 4) any conviction set aside under the Federal Youth Corrections Act or similar state law, and 5) any other conviction for which the record was expunged under Federal or state law. ***NOTE:*** You must include convictions resulting from a plea of nolo contendere (no contest).

Important note about questions 28 through 31. We will consider the date, facts, and circumstances of each event you list. In most cases you can still be considered for Federal jobs. However, if you fail to tell the truth or fail to list all relevant events, this failure may be grounds for not hiring you, for firing you after you begin work, or for criminal prosecution [18 U.S.C. 1001]. *If "YES," use Item 32 to provide the date, explanation of the violation, place of occurrence, and the name and address of the police department or court involved.*

28. During the last 10 years, have you been convicted, been imprisoned, been on probation, or been on parole? *(Includes felonies, firearms or explosives violations, misdemeanors, and all other offenses. If "YES," use Item 32 to provide the date, explanation of the violation, place of occurrence, and the name and address of the police department or court involved.)* ☐ Yes ☐ No

29. Are you **now** under charges for **any** violation of law? *If "YES," use Item 32 to provide the date, explanation of the violation, place of occurrence, and the name and address of the police department or court involved.* ☐ Yes ☐ No

30. Have you been convicted by a **military court-martial in the past 10 years?** *If no military service, answer "NO." If "YES," use Item 32 to provide the date, explanation of the violation, place of occurrence, and the name and address of the military authority or court involved.* ☐ Yes ☐ No

31. Are you **delinquent** on any Federal debt? *(Include delinquencies arising from Federal taxes, loans, overpayment of benefits, and other debts to the U.S. Government **plus** defaults on Federally guaranteed or insured loans such as student and home mortgage loans.) If "YES," use Item 32 to provide the type, length, and amount of the delinquency or default, and steps that you are taking to correct the error or repay the debt.* ☐ Yes ☐ No

32. EXPLANATIONS OR ANSWERS TO QUESTIONS 1 THROUGH 31 – *Attach additional listing if needed.*

Section G – PRIVACY ACT STATEMENT

Solicitation of this information is authorized by section 23 of title 13, U.S. Code, which authorizes temporary appointments in the Census Bureau. The information will be used primarily to determine your qualifications for employment and may be used also to identify you to other sources asked to comment on your qualifications, e.g., educational institutions, former employers, and law enforcement agencies, or to a court during legal proceedings.

We must have your Social Security Number (SSN) to keep our records straight because other people may have the same name and birth date. The SSN has been used to keep records since 1943, when Executive Order 9397 asked agencies to do so. The furnishing of all the information is voluntary, but failure to provide any part or all of the data requested will result in your receiving no further consideration for employment.

Public reporting burden for this collection of information is estimated to average 15 minutes per response, including the time for reviewing instructions, searching existing data sources, gathering and maintaining the data needed, and completing and reviewing the collection of information. Send comments regarding this burden estimate or any other aspect of this collection of information, including suggestions for reducing this burden, to: Paperwork Project 0607-0139, U.S. Census Bureau, 4600 Silver Hill Road, Washington, DC 20233-1500. You may E-mail comments to Paperwork@census.gov; use "Paperwork Project 0607-0139" as the subject.

The eight digit OMB control number on the first page of this form confirms our authority to collect this information.

Section H – SIGNATURE, CERTIFICATION, AND RELEASE OF INFORMATION

YOU MUST SIGN THIS APPLICATION IN DARK INK. Read the following carefully before you sign. A false statement on any part of your application may be grounds for not hiring you, or for firing you after you begin work. Also, you may be punished by fine or imprisonment (U.S. Code, Title 18, Section 1001).

I understand that any information I give may be investigated as allowed by law or Presidential order. I consent to the release of information about my ability and fitness for Federal employment by employers, schools, law enforcement agencies and other individuals and organizations, to investigators, personnel staffing specialists, and other authorized employees of the Federal Government. I certify that, to the best of my knowledge and belief, all of my statements are true, correct, complete, and made in good faith.

Signature	Date signed
Print name	

Appendix C. Sample Test

Part I – CLERICAL SKILLS

This section is designed to test your clerical skills. Clerical skills include such things as alphabetizing, serializing, and matching.

In questions 1 and 2, file folders are labeled as shown. Alphabetize these folders for filing. Choose the answer that shows the correct order.

1. (1) FOS
(2) AOS
(3) OOS
(4) FOA
(5) DOM
(6) OOA

A (2) – (5) – (1) – (4) – (6) – (3)
B (2) – (5) – (4) – (1) – (3) – (6)
C (2) – (5) – (4) – (1) – (6) – (3)
D (2) – (4) – (1) – (5) – (6) – (3)

2. Alphabetize the following names of people. Last names (family names) are given last:

(1) Linda Jameson
(2) James Alberts
(3) Allan Jameson
(4) Alfred Johnson
(5) Lydia Moreno

A (2) – (1) – (3) – (4) – (5)
B (3) – (4) – (2) – (1) – (5)
C (2) – (3) – (1) – (4) – (5)
D (4) – (3) – (2) – (1) – (5)

3. Which set of numbers is **NOT** in numerical order from the smallest to the largest number?

A 0095, 0101, 0011, 0111
B 0063, 0120, 0200, 0210
C 1096, 1099, 1118, 1181
D 0106, 0160, 0601, 0610

4. Place the following dates in order, from the earliest to the latest. Choose the answer that shows the correct order.

(1) 3/8/76
(2) 3/9/76
(3) 8/14/75
(4) 12/31/76
(5) 1/1/76

A (3) – (5) – (1) – (2) – (4)
B (3) – (1) – (2) – (4) – (5)
C (4) – (2) – (1) – (5) – (3)
D (5) – (4) – (2) – (1) – (3)

5. Group the records in the following way:

(1) by sex with females first
(2) by social security number from lowest to highest

Record	Social Security Number	Sex
1	301–98–1888	M
2	105–99–9999	F
3	309–11–6633	M
4	601–33–9123	M
5	302–89–1666	F

then choose the answer which shows the correct order.

A 2, 5, 1, 3, 4
B 2, 1, 5, 3, 4
C 1, 3, 4, 2, 5
D 5, 2, 4, 3, 1

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Part I – CLERICAL SKILLS – Continued

Use the following table in order to answer question 6.

ZIP Code	No. of units	Address No.	Block No.	Type of form	Serial No.	Date Questionnaire checked in	No. of persons
00011	3	0056	107	B	0500	9/6	6
00011	3	0056	107	A	0501	9/16	6
00011	3	0056	107	B	0502	9/6	6
00011	1	0057	107	C	0503	9/9	9
00011	1	0157	117	A	0504	9/19	9
00011	1	0158	117	B	0505	9/16	1
00011	1	0159	117	A	0506	9/6	3
00011	2	0160	117	B	0507	9/9	3
00011	2	0160	117	C	0508	9/16	4
00111	1	1160	177	A	0509	9/16	0
00111	2	1161	177	B	0510	9/6	7
00111	2	1161	177	A	0511	9/16	6
00111	1	1162	177	B	0512	9/9	0

6. Which of the following correctly matches a row in the table above?

A 00111-1-1160-177-A-0509-9/6-0
B 00011-1-0157-107-A-0504-9/19-9
C 00011-3-0056-107-A-0501-9/6-6
D 00111-2-1161-177-A-0511-9/16-6

Part II – READING

This section is designed to test how well you read. The items test your vocabulary, comprehension, and ability to apply what you read.

Choose the one answer which BEST fits the meaning of the word in capital letters.

7. Do not permit any **UNAUTHORIZED** person to see census information.

- **A** uniformed
- **B** unidentified
- **C** unofficial
- **D** illegal

8. TRANSCRIBE –

- **A** to transport
- **B** to copy
- **C** to repeat
- **D** to exchange

Read the paragraph regarding census work. Answer the question which follows it based on the information in the paragraph. Select the **BEST** answer.

9. The Field Operations Supervisor (FOS) will issue one identification card to each Crew Leader. Crew Leaders will issue one identification card to each enumerator. Clerks will be issued identification cards by the FOS only if necessary for them to work outside the office on special tasks and in contact with the general public. All identification cards issued to Crew Leaders and enumerators must be turned in to the Field Operations Supervisor upon completion of work, separation, termination, or resignation. The FOS is responsible for seeing that all returned cards are destroyed.

Which of the following is **NOT** a true statement?

- **A** The overall responsibility for identification cards rests with the Field Operations Supervisor.
- **B** Clerks may not always be provided with identification cards.
- **C** Enumerators who resign turn their identification cards in to their supervisor, the Crew Leader, who destroys them.
- **D** A Crew Leader's identification card is destroyed when he/she resigns.

A definition of a census term is given below. Read the definition and then answer the question that follows it based on the definition.

10. Group Quarters: Any living quarters occupied by ten or more unrelated persons is called a group quarters. Examples of a group quarters are worker's dormitories, boarding houses, halfway houses, convents, etc. In addition, college dormitories, fraternity houses, or nurse's dormitories are **always** considered to be a group quarters, regardless of the number of students who live there.

Which of the following is a group quarters?

- **A** A college dormitory in which six students live
- **B** A house in which a family of six and three boarders live
- **C** A convent occupied by five nuns
- **D** A medical office building with eleven doctors' offices

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Part II – READING – Continued

The answer to question 11 is based on the information below.

A census taker is told to count only the following as living in a particular household:

- Family members living here, including babies still in the hospital
- Relatives living here
- Lodgers or boarders or hired hands living here
- Domestic employees or hired hands living here
- Other persons living here
- College students who stay here while attending college, even if their parents live elsewhere
- Persons who usually live here, but are temporarily away (including children in boarding school below the college level, but excluding family members serving in the armed forces)
- Persons with a house elsewhere, but who stay here most of the week while working

In item 11, select the person the census taker would count as living in that household.

11.

A A son who is away in the armed forces

B A family member traveling in Europe for a couple of weeks

C Mr. Smith, a boarder, who stays here during the weekend and returns to his home during the week

D A son who is living in a college dormitory while attending college out-of-town

12. Census takers who visit homes to collect census information are called enumerators. They are told to use any person who owns or rents the housing unit as the "reference person" and then to list the relationship of every household member to the "reference person." When an enumerator interviews at 1105 Low Street, he talks to Mr. Sanchez. Mr. Sanchez says that both Mr. and Ms. Morales own the house and all three are household members. Who should the census taker list as the reference person?

A Either Mr. Morales or Ms. Morales

B Mr. Morales

C Ms. Morales

D Mr. Sanchez

Part III – NUMBER SKILLS

Some of the items in this section are designed to test your ability to perform arithmetic computations involving addition, subtraction, and multiplication. Others involve no computation at all, but an understanding of numerical concepts.

Solve the problems in items 13 through 18. Select the correct answer from the given choices.

13. .41 + 21.4 + 6.3 + 280 =

A 48.49
B 59.8
C 308.11
D 450

14. Your new cell phone battery needs to be charged for three hours and 45 minutes before using it. If you plugged the battery into the charger at 8:20 a.m., you should wait until what time before using it?

A 11:05 a.m.
B 11:20 a.m.
C 12:00 p.m.
D 12:05 p.m.

15. 40 X 17.2

A 6.88
B 680.8
C 68.8
D None of the above

16. Mr. Hernandez had to interview a total of 63 households in his assignment. He has already finished 28. What percentage of the households in his assignments has he finished? (Round your answer to the nearest tenth of a percent.)

A 4.4%
B 22.5%
C 44.4%
D 2.5%

17. 2610.0 miles – 2554.8 miles

A 55.2 miles
B 56.2 miles
C 165.2 miles
D 552 miles

18. Place each of the following ten numbers in the correct column of the tables below them. Then choose the answer (A, B, C, or D) that matches your entries in columns a-f.

70067, 99992, 33336, 24689, 3330, 603, 22221, 28, 87059, 67000

Odd Numbers Only		
1–33333 (a)	33335–66665 (b)	66667–99999 (c)

Even Numbers Only		
2–33332 (d)	33334–66666 (e)	66668–99998 (f)

A (a) 28, 3330
(b) 33336
(c) 67000, 99992
(d) 603, 22221, 24689
(f) 70067, 87059

B (a) 603, 22221, 24689
(c) 70067, 87059
(d) 28, 3330
(e) 33336
(f) 67000, 99992

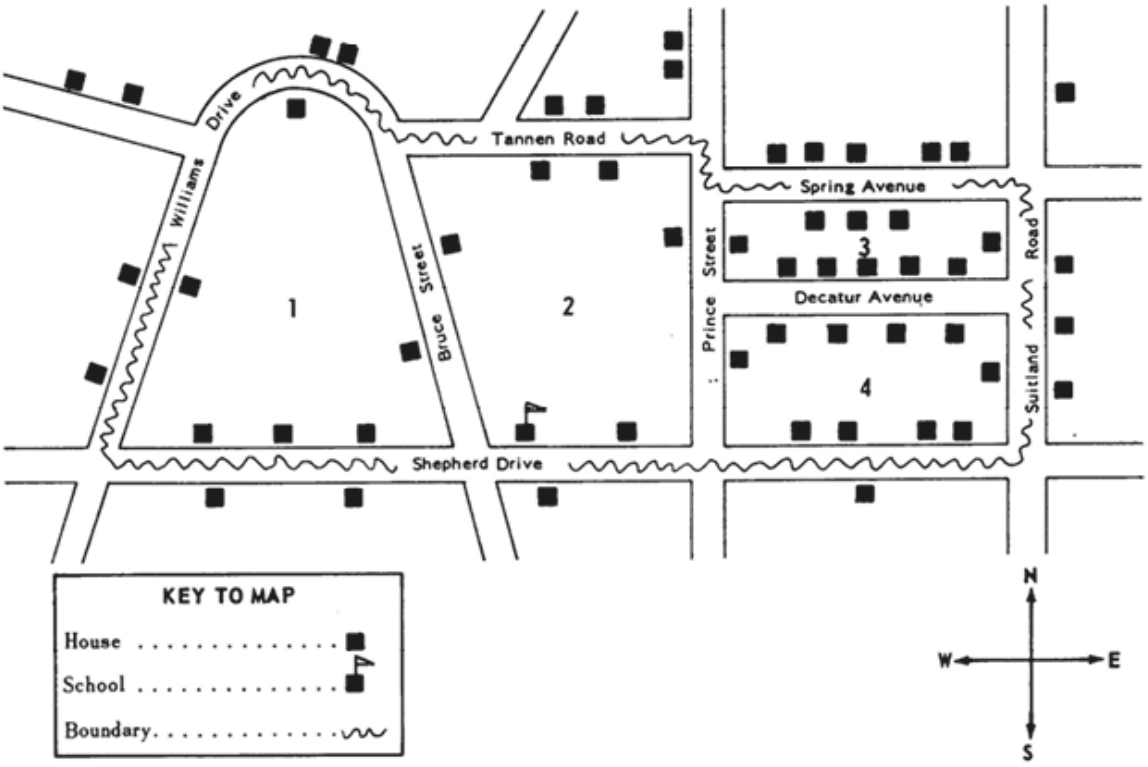
C (a) 603, 22221, 24689
(b) 33336
(c) 67000, 760067, 87059
(d) 28, 3330
(e) 99992

D None of these

Part IV – INTERPRETING INFORMATION and EVALUATING ALTERNATIVES

This section is designed to test your ability to use good judgment in interpreting information in order to determine the best of several possible alternatives.

You have been assigned to enumerate inside the boundaries outlined on the map below. Where the boundary is a road, the boundary line runs down the center of it. Using the map choose the best answer to questions 19, 20, and 21.



19. How many houses on Decatur Avenue are included in block 3?

A 10
B 4
C 5
D 6
E 7

20. How many houses would you visit on Shepherd Drive?

A 13
B 12
C 9
D 8
E 4

21. If you went from the corner of Bruce Street and Tannen Road to the corner of Spring Avenue and Suitland Road by the shortest way, how many houses in your assignment would you pass?

A 5
B 6
C 10
D 12
E 4

Part IV – INTERPRETING INFORMATION and EVALUATING ALTERNATIVES – Continued

Refer to the following outline of a chapter from a census procedures manual in order to answer questions 22 and 23.

Furnishing and Operating the Local Census Office

2.A. Space
2.A1 Space for the Local Census Office
2.A2 Training Space

2.B. Bills
2.B1 Identification
2.B2 Certification
2.B3 Submission for Payment
2.B4 Special Instructions
2.B5 Administrative Operations Codes

2.C. Services
2.C1 Telephones
2.C2 Utilities

2.D. Supplies and Equipment
2.D1 Receiving Shipments
2.D2 Organizing Supplies
2.D3 Storing Small Items
2.D4 Repair of Office Machines
2.D5 Requests for Supplies
2.D6 Purchase of Supplies and Equipment

2.E. Rental of Office Equipment
2.E1 Pre-Arranged Rental
2.E2 Local Census Office Rentals
2.E3 Delivery Acceptances
2.E4 Payment of Rental Bills

2.F. Rental of Automobiles
2.F1 Report Days Rented
2.F2 Forward Trip Tickets

2.H. Local Census Office Layout
2.H1 Administrative Area
2.H2 Supply Area
2.H3 Processing Area
2.H4 Field Operations Area
2.H5 Computer Terminal Room

22. Which section would tell you about the payment procedure for rental office equipment?

A 2.B3
B 2.D6
C 2.E2
D 2.E4

23. Which section tells how supplies such as pencils, erasers, and paper clips should be stored?

A 2.D2
B 2.D3
C 2.D5
D 2.H2

Refer to the table below to answer question 24.

Log of Census Operations

Questionnaire number	Review operations 1		2		3	
	S	C	S	C	S	C
0015	3/14	3/15	3/16	3/18		
0016	3/13	3/14	3/15	3/15	3/17	3/18
0116	3/12	3/12	3/14	3/16		
0118	3/17					
0119	3/11					

S=Start C=Completion

24. On what date was the second review completed for Questionnaire 0116?

A 3/13
B 3/14
C 3/15
D 3/16

Part V – ORGANIZATIONAL SKILLS

This section is designed to determine your ability to use logical reasoning in order to analyze, summarize, and/or organize information to solve a problem or follow an instruction.

25. Ms. Jones can't remember the year of her birth but she knows that she was born in the month of September. If it is now May 2010, and Ms. Jones tells you she is 78 years old, in what year was she born?

- **A** 1931
- **B** 1932
- **C** 1930
- **D** None of these

Read the following paragraph and answer the question below it.

26. Each county is divided into a number of census tracts. The tracts are, in turn, subdivided into blocks. Groups of blocks within a tract are then combined into Assignment Areas (AA).

Which of the following shows the four geographic units discussed above, in order of size, from largest to smallest?

- **A** county, tract, AA, block
- **B** county, block, tract, AA
- **C** county, AA, tract, block
- **D** AA, block, tract, county

The paragraph below may be confusing as written. Choose the answer that explains its meaning in the simplest, most complete way.

27. The number of questions to be asked in the 2010 Census approximates the number asked in 2000 and 1990, and is less than was asked in 1970 and 1980. The Census questionnaires contain questions relating to housing characteristics and questions relating to population characteristics.

Choose the statement that explains the above paragraph in the simplest, most complete way.

- **A** There will be fewer questions asked in 2010 than 30 years ago, and they relate to population and housing characteristics.
- **B** The number of questions has not changed in over 40 years, and all questionnaires have both population and housing questions.
- **C** Questionnaires in 2010 will have the same number of questions relating to population as in 1990 and 2000.
- **D** Questionnaires in 2010 will have more questions than in 1970, but fewer than in 2000.

Part V – ORGANIZATIONAL SKILLS – Continued

Answer question 28 by referring to the following samples and the paragraph of coding rules which follows them.

ADDRESS LABEL

D.O.	A1.	A2.	A3.	A4.	A5.	A6.
3011	101	23		117	B	172
811 Main Street						
Anytown, USA						
00000 **B**						

CODING SHEET

A2. Unit number	A3. Building address number	A4. Block number	A6. Serial number
● 0 0	0 0 0	0 0	0 0 0 0
1 1 1	1 1 1	1 1 1	1 1 1 1
2 ● 2	2 2 2	2 2 2	2 2 2 2
3 3 ●	3 3 3	3 3 3	3 3 3 3
4 4 4	4 4 4	4 4 4	4 4 4 4
5 5 5	5 5 5	5 5 5	5 5 5 5
6 6 6	6 6 6	6 6 6	6 6 6 6
7 7 7	7 7 7	7 7 7	7 7 7 7
8 8 8	8 8 8	8 8 8	8 8 8 8
9 9 9	9 9 9	9 9 9	9 9 9 9

The corresponding areas of an address label and a coding sheet are identified by a letter and number, such as A1, A2, etc. One records a number from the address label onto the corresponding area of the coding sheet by marking the appropriate digits, placing one digit in each column. When there are more columns on the coding sheet than there are digits in the numbers on the address label, the recorded numbers must be preceded by zeroes. See the example above for A2, Unit number.

28. How would you record the "Serial Number" on the coding sheet?

A	B	C	D
0 ● 0 0	0 0 0 0	0 0 0 0	● 0 0 0
1 1 ● ●	● 1 1 1	1 ● 1 1	1 ● 1 1
2 2 2 2	2 2 ● 2	2 2 2 ●	2 2 2 ●
● 3 3 3	3 3 3 3	3 3 3 3	3 3 3 3
4 4 4 4	4 4 4 4	4 4 4 4	4 4 4 4
5 5 5 5	5 5 5 5	5 5 5 5	5 5 5 5
6 6 6 6	6 6 6 6	6 6 6 6	6 6 6 6
7 7 7 7	7 ● 7 7	7 7 ● 7	7 7 ● 7
8 8 8 8	8 8 8 8	8 8 8 8	8 8 8 8
9 9 9 9	9 9 9 9	9 9 9 9	9 9 9 9