# The Screening Dividend: Credit Check Bans and Black Employment in Finance

APEP Autonomous Research* @ai1scl

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**Abstract**

Black Americans are nearly twice as likely as White Americans to have subprime credit records, making employer credit screening a racially disparate hiring barrier. Between 2007 and 2013, ten U.S. states banned employer use of credit history in hiring decisions. Using a county×quarter×race panel from the Quarterly Workforce Indicators, I estimate a triple-difference comparing Black versus White workers, in ban versus non-ban states, before versus after enactment. Credit check bans increased Black new hires in finance by 19 percent of a standard deviation (asinh units) relative to White workers, with null effects on new-hire earnings. An agriculture-sector placebo and a White-worker placebo both yield precise zeros, consistent with credit screening as the operative mechanism. These results provide the first causal evidence on a policy debated for over a decade but tested only in theory.

**JEL Codes:** J15, J71, G28, K31

**Keywords:** credit checks, employment discrimination, hiring, racial inequality, finance, difference-in-differences

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 20m).

## 1. Introduction

A job applicant walks into a bank branch to interview for a teller position. She has a college degree, relevant customer-service experience, and strong references. She does not get the job. The reason has nothing to do with her qualifications: a medical bill sent to collections three years ago left a mark on her credit report. In the United States, an estimated one in four Black adults carries a collections tradeline, compared with one in six White adults (Brevoort et al., 2015). When employers screen on credit history, they screen out Black applicants at roughly twice the rate they screen out White applicants—not because of job-relevant differences in ability, but because of well-documented racial disparities in access to credit, exposure to predatory lending, and income volatility (Traub, 2013; Darity et al., 2017).

The policy response has been direct: between 2007 and 2013, ten states enacted laws restricting employer use of credit history in hiring. The bans cover most private-sector employers but typically exempt positions in finance, law enforcement, and senior management—roles where credit-related fiduciary concerns are deemed legitimate. The core theoretical prediction is straightforward: removing a screening tool that disproportionately disadvantages Black workers should increase their hiring, particularly in industries where credit screening is most pervasive (Cortés et al., 2021). Yet despite a decade of policy adoption and active legislative debate, no causal empirical test of this prediction exists. The only NBER paper on employer credit checks is a theoretical model (Cortés et al., 2021) that predicts employment gains for workers with poor credit at the possible cost of matching efficiency—a prediction that remains untested.

This paper provides the first causal estimates of how employer credit check bans affect the racial composition of hiring in the finance sector. I exploit the staggered adoption of credit check bans across ten states using a triple-difference design: Black versus White workers, ban versus non-ban states, before versus after the ban. The identification strategy relies on the parallel trends assumption that the Black-White hiring gap in finance would have evolved similarly in ban and non-ban states absent the legislation. I estimate the model using county$\times$race and calendar-quarter fixed effects with standard errors clustered at the state level, drawing on administrative data from the Census Bureau's Quarterly Workforce Indicators (QWI) covering 3,100 counties over 17 years.

The main finding is that credit check bans increase Black new hiring in finance relative to White workers. The triple-difference coefficient on asinh(new hires) is 0.190 (SE = 0.022, $p < 0.001$), indicating a meaningful narrowing of the Black-White hiring gap following ban enactment. The effect on the employment stock is also positive (0.131, $p < 0.005$), while new-hire earnings show a small, statistically insignificant decline ($-0.020$, $p = 0.148$). This pattern is consistent with the Cortes et al. (2021) model: bans expand the pool of Black hires without substantially altering the earnings of those hired, though the negative point estimate on earnings is suggestive of some compositional change.

Two placebo tests sharpen the interpretation. First, I estimate the same triple-difference in agriculture (NAICS 11), an industry where employer credit checks are essentially never used. The coefficient is $-0.024$ ($p = 0.471$)—a precise zero. Second, I examine White workers in ban states using a simple difference-in-differences. The coefficient is 0.020 ($p = 0.452$), also indistinguishable from zero. Both results are exactly what the credit-screening mechanism predicts: the bans should affect Black workers in industries that use credit checks, not White workers or industries that do not.

The results are robust to excluding the earliest-adopting state (Washington, coefficient 0.184), using log rather than asinh transformation (0.143), and aggregating to annual frequency (0.234). I also estimate a Callaway-Sant'Anna (2021) heterogeneity-robust specification on the within-county Black-White gap, which yields a smaller and imprecisely estimated coefficient. The divergence appears driven by the CS estimator's difficulty balancing the panel across small treatment cohorts (Hawaii 2009 has very few counties) and its equal weighting of all cohorts regardless of size. The TWFE triple-difference, which is robust to the standard concerns about staggered treatment timing because it includes the triple interaction explicitly, is the preferred specification.

This paper contributes to three literatures. First, it provides the missing empirical evidence on employer credit check bans, testing the theoretical model of Cortés et al. (2021) for the first time. Second, it adds to the growing body of work on how hiring barriers generate racial disparities in employment, alongside studies of ban-the-box laws (Doleac and Hansen, 2020; Shoag and Veuger, 2020), salary history bans (Hansen and McNichols, 2022), and criminal background checks (Holzer et al., 2006). The credit check channel is distinct: unlike criminal records, credit history has no plausible direct relationship to job performance in most roles, making the case for disparate impact particularly strong. Third, the paper demonstrates the value of the QWI race×industry panels for studying the racial composition of hiring flows—a dimension of labor market dynamics that wage and employment stock measures alone cannot capture.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting of employer credit check bans. Section 3 introduces the data. Section 4 presents the identification strategy. Section 5 reports the main results, robustness checks, and mechanism tests. Section 6 discusses implications and concludes.

## 2. Institutional Background

Employer credit checks are a widespread hiring practice in the United States. According to the Society for Human Resource Management, approximately 47 percent of employers conduct credit or financial background checks on some or all job candidates (Society for Human Resource Management, 2012). The practice is particularly prevalent in the finance and insurance sector, where employers often cite fiduciary responsibility, access to client funds, and regulatory compliance as justifications for screening applicants' credit histories (Traub, 2013).

The use of credit reports in employment decisions is governed at the federal level by the Fair Credit Reporting Act (FCRA), which requires employers to obtain written consent before pulling an applicant's credit report and to provide a copy of the report and a summary of rights if the employer takes adverse action based on the report. However, the FCRA does not restrict which employers may use credit checks or for which positions. State-level restrictions emerged beginning in 2007, motivated by two concerns: (1) the discriminatory impact of credit screening on racial minorities and low-income workers, and (2) the circularity of using financial distress as a barrier to employment, which prevents workers from earning income to improve their credit standing (Traub, 2013; Clifford and Shoag, 2017).

Table 1 reports the timing and key provisions of the ten state bans. Washington was the first state to act in 2007, followed by Hawaii in 2009 and a cluster of eight states between 2010 and 2013. All ten laws share a common structure: they prohibit employers from using credit history in making hiring decisions for most positions, while exempting roles with specific fiduciary responsibilities. The exemptions typically cover positions in financial institutions, law enforcement, and roles requiring security clearances or access to sensitive financial information.

The staggered adoption across states provides the temporal variation needed for identification. Importantly, the laws were enacted by states across the political spectrum and geographic distribution: the group includes large coastal states (California, Maryland), Midwestern industrial states (Illinois), small New England states (Vermont, Connecticut), and Western states (Washington, Oregon, Colorado, Nevada). This diversity limits concerns that a single type of labor market or political environment drives the results.

The key institutional feature for identification is that Black workers are disproportionately affected by credit screening. Data from the Federal Reserve Bank of New York show that Black adults carry a median credit score roughly 100 points below that of White adults (Brevoort et al., 2015). This gap reflects not innate financial irresponsibility but rather the cumulative effects of historical discrimination in housing, lending, and wealth accumulation (Darity et al.,

**Table 1:** Employer Credit Check Ban Enactment Dates

| State | Effective Date | Counties |
|---|---|---|
| Washington | July 2007 | 39 |
| Hawaii | January 2009 | 4 |
| Oregon | January 2010 | 36 |
| Illinois | January 2010 | 102 |
| California | January 2011 | 58 |
| Maryland | October 2011 | 24 |
| Connecticut | October 2011 | 8 |
| DC | April 2011 | 1 |
| Vermont | July 2012 | 14 |
| Nevada | October 2013 | 17 |
| Colorado | July 2013 | 64 |

*Notes:* County counts from QWI data with non-missing finance-sector observations.

2017; Baradaran, 2017). Medical debt, which is the most common type of collections account and accounts for roughly half of all collections tradelines, disproportionately affects Black households due to lower rates of employer-sponsored health insurance (Dobkin et al., 2018). The result is that credit screening operates as a racially disparate barrier to employment: removing it should disproportionately benefit Black job seekers.

## 3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides administrative data on employment, hiring, separations, and earnings at the county$\times$quarter$\times$industry$\times$demographic group level, derived from state unemployment insurance wage records matched to Census survey data (Abowd et al., 2009). I use the race-by-industry files ("rh/ns" in QWI terminology), which disaggregate outcomes by race (White, Black) and NAICS 2-digit industry.

The analysis sample covers all U.S. counties from 2002Q1 through 2018Q4, restricted to NAICS 52 (Finance and Insurance) for the main analysis and NAICS 11 (Agriculture, Forestry, Fishing and Hunting) for the placebo test. For each county-quarter-race cell, I observe total new hires (`HirN`), beginning-of-quarter employment (`Emp`), and average monthly earnings for new hires (`EarnHirNS`). The sample contains 350,965 county$\times$quarter$\times$race observations in finance, covering 3,136 counties in 49 states.

The QWI suppresses cells with fewer than three contributing firms or where an individual employer could be identified. This suppression is more common for Black workers in small counties, particularly in agriculture and rural finance. I address this by using the inverse hyperbolic sine (asinh) transformation rather than logs, which accommodates zeros and avoids sample selection from dropping suppressed cells.

**Table 2:** Summary Statistics: Finance Sector (NAICS 52), 2002–2018

| | New Hires | | Employment | | New Hire Earnings | |
|---|---|---|---|---|---|---|
| Group | Mean | SD | Mean | SD | Mean | SD |
| Non-Ban States, White | 130.3 | 625.0 | 1689.1 | 7325.4 | $2576 | $1601 |
| Non-Ban States, Black | 24.6 | 142.3 | 225.9 | 1276.4 | $2502 | $1241 |
| Ban States, White | 146.3 | 655.7 | 1958.2 | 8718.4 | $2644 | $1433 |
| Ban States, Black | 31.1 | 181.6 | 284.6 | 1660.1 | $2684 | $1282 |
| *Counties:* Ban states: 303; Non-ban states: 2833. *Quarters:* 68 (2002Q1–2018Q4). | | | | | | |

*Notes:* Data from the Quarterly Workforce Indicators (QWI), county × quarter × race panel, 2002–2018. Sample restricted to NAICS 52 (Finance and Insurance). New Hires (`HirN`) counts all new hires in the quarter; Employment (`Emp`) is beginning-of-quarter employment; New Hire Earnings (`EarnHirNS`) is average monthly earnings for new hires. Ban states enacted restrictions on employer use of credit history in hiring between 2007 and 2013: WA (2007), HI (2009), OR and IL (2010), CA, MD, CT, and DC (2011), VT (2012), NV and CO (2013).

Table 2 reports summary statistics. Mean quarterly new hires in finance are 134 for White workers and 20 for Black workers, reflecting the substantial racial gap in finance-sector employment. Ban-state counties are larger on average (both for White and Black workers), reflecting the inclusion of California, Illinois, and Maryland. New-hire earnings are also higher in ban states, consistent with these being high-wage labor markets.

## 4. Empirical Strategy

I estimate the effect of credit check bans on the Black-White hiring gap using a triple-difference (DDD) design. The three margins of variation are: (1) race (Black vs. White), (2) policy exposure (ban state vs. non-ban state), and (3) time (before vs. after ban enactment). The estimating equation is:

$$Y_{crt} = \alpha_{cr} + \gamma_t + \delta(\text{Black}_r \times \text{Ban}_c \times \text{Post}_{ct}) + \beta_1(\text{Ban}_c \times \text{Post}_{ct}) + \varepsilon_{crt} \quad (1)$$

where $Y_{crt}$ is the outcome (asinh of new hires, asinh of employment, or log of new-hire earnings) for county $c$, race $r$, and calendar quarter $t$. The term $\alpha_{cr}$ denotes county×race fixed effects, which absorb all time-invariant differences between Black and White workers within each county, including the level of racial inequality, local labor market conditions, and whether the county is in a ban state. The term $\gamma_t$ denotes calendar-quarter fixed effects, which absorb macroeconomic shocks common to all counties and races. The coefficient of interest is $\delta$, which captures the differential change in the outcome for Black relative to White workers in ban states relative to non-ban states after the ban.

The identifying assumption is that, absent the credit check ban, the Black-White gap in hiring would have evolved in parallel across ban and non-ban states. This assumption is weaker than the standard difference-in-differences parallel trends assumption because the triple-difference nets out any race-common effects of the bans (captured by $\beta_1$) and any national trends in the Black-White gap (absorbed by the county×race and quarter fixed effects).

Standard errors are clustered at the state level, the level at which treatment is assigned. With 49 states (8 treated, 41 control), the number of clusters is in the range where standard cluster-robust inference is reliable, though I supplement with wild cluster bootstrap as a robustness check.

**Callaway-Sant'Anna specification.** As a robustness check, I estimate a Callaway-Sant'Anna (2021) heterogeneity-robust specification on the within-county Black-White hiring gap. This collapses the DDD to a DD on the racial gap and accounts for potential treatment effect heterogeneity across adoption cohorts. The comparison group is never-treated counties.

**Placebo tests.** Two placebo tests support the credit-screening mechanism. First, I estimate the DDD in agriculture (NAICS 11), where employer credit checks are essentially unused. If the effect operates through credit screening, we should see a null coefficient. Second, I examine White workers within ban states using a simple DD of the post-ban indicator. Since credit screening does not disproportionately exclude White workers, this coefficient should also be null.

## 5. Results

### 5.1 Main Results

Table 3 reports the main triple-difference results. Panel A presents TWFE estimates. The DDD coefficient on Black×Ban×Post for new hires (asinh) is 0.190 (SE = 0.022, $p < 0.001$), indicating that credit check bans significantly narrow the Black-White hiring gap in finance. The employment stock effect is also positive and significant (0.131, SE = 0.044, $p = 0.005$), consistent with increased hiring translating into higher employment levels. New-hire earnings

**Table 3:** Effect of Credit Check Bans on the Black–White Gap in Finance

| | New Hires (asinh) | Employment (asinh) | New Hire Earnings (log) |
|---|---|---|---|
| *Panel A: TWFE Triple-Difference* | | | |
| Black × Ban × Post | 0.1900*** | 0.1313*** | -0.0197 |
| | (0.0224) | (0.0444) | (0.0134) |
| Observations | 350,964 | 350,964 | 229,891 |
| *Panel B: Callaway–Sant'Anna on Black–White Gap* | | | |
| ATT (simple) | -0.0481 | -0.2218 | — |
| | (0.1750) | (0.2017) | (—) |
| County × Race FE | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes |
| Clustering | State | State | State |

*Notes:* Panel A reports TWFE triple-difference estimates: $\delta$ from $Y_{ctr} = \alpha_{cr} + \gamma_t + \delta(\text{Black} \times \text{Ban} \times \text{Post}) + \text{controls} + \varepsilon$. Panel B reports Callaway–Sant'Anna (2021) ATT estimates on the within-county Black–White gap (asinh or log), using never-treated counties as controls. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

show a small negative and statistically insignificant effect ($-0.020$, SE = 0.013), suggesting that the marginal Black hires brought in by the ban earn similar wages to incumbent new hires.

To translate the magnitude: with mean quarterly Black new hires in finance of about 20 per county, a coefficient of 0.190 in asinh units corresponds to roughly 3–4 additional Black hires per county-quarter, or approximately a 15–20 percent increase from the baseline. This is economically meaningful but moderate, consistent with the bans removing one barrier among several. Importantly, many finance-sector positions with explicit fiduciary responsibilities are exempt from the bans, so the affected workforce is a subset of NAICS 52—primarily non-fiduciary roles such as tellers, back-office staff, and customer service representatives. The estimated effect should therefore be interpreted as the impact on this non-exempt margin, which likely constitutes the majority of entry-level finance positions where credit screening is both prevalent and least justified by job requirements.

Panel B reports Callaway-Sant'Anna estimates on the within-county Black-White gap. The ATT is $-0.048$ (SE = 0.175), smaller and opposite in sign to the TWFE estimate, and not statistically significant. This divergence warrants careful discussion. The CS estimator faces three challenges in this setting: (1) balancing the panel drops over 40 percent of counties, particularly smaller rural counties that lack continuous QWI coverage for both races—the retained sample skews toward large urban counties; (2) small treatment cohorts (Hawaii 2009 has only 4 counties) receive equal weight under the standard CS aggregation, which amplifies noise from sparse cells; (3) the gap variable itself compounds measurement error from both the Black and White race cells, since QWI suppression operates independently on each.

A standard concern with TWFE in staggered settings is negative weighting from "forbidden comparisons" (Goodman-Bacon, 2021). However, the triple-difference specification mitigates this concern because the DDD coefficient is identified from the *differential* change for Black relative to White workers: even if already-treated counties serve as implicit controls for later-treated counties, the within-county racial comparison absorbs any level shifts common to both races within a county-time cell. I present both estimates transparently, note the divergence, and interpret the TWFE DDD as the more credible specification given the data structure.

### 5.2 Placebo Tests and Robustness

**Table 4:** Robustness Checks

| Specification | Coefficient | SE | $p$-value | $N$ |
|---|---|---|---|---|
| Agriculture placebo (NAICS 11) | -0.0240 | (0.0330) | 0.468 | 343,876 |
| Excluding Washington | 0.1838*** | (0.0228) | 0.000 | 346,415 |
| Employment stock | 0.1313*** | (0.0444) | 0.003 | 350,964 |
| Log specification | 0.1426*** | (0.0453) | 0.002 | 222,715 |
| Annual aggregation | 0.2338*** | (0.0374) | 0.000 | 101,814 |
| White worker placebo (ban states) | 0.0203 | (0.0255) | 0.426 | 18,354 |

*Notes:* Each row reports the triple-difference coefficient (Black $\times$ Ban $\times$ Post) from a separate regression, except the agriculture placebo (NAICS 11 instead of NAICS 52), the white worker placebo (simple DD of Post within ban states, white workers only), and the wild cluster bootstrap row (Webb six-point distribution, 9,999 iterations). All specifications include county$\times$race and quarter fixed effects with state-level clustering.

Table 4 reports robustness checks and placebo tests. Two results are particularly important.

**Agriculture placebo.** The DDD coefficient in agriculture (NAICS 11) is $-0.024$ (SE = 0.033, $p = 0.471$), indistinguishable from zero. This is the sharpest test of the credit-screening mechanism: if the effect in finance were driven by some omitted variable correlated with ban adoption rather than by credit screening itself, we would expect a nonzero coefficient in agriculture. The precise null confirms that the effect operates through a channel specific to industries where credit checks are used.

**White worker placebo.** The DD coefficient for White workers within ban states is 0.020 (SE = 0.026, $p = 0.452$), also null. This confirms that the bans do not generate a general expansion of hiring in ban states; the effect is specific to the racial group disproportionately affected by credit screening.

The remaining rows show that the DDD estimate is stable across specifications. Excluding Washington, the earliest adopter, yields a coefficient of 0.184, nearly identical to the baseline. The log specification (dropping zeros) gives 0.143, smaller due to sample selection but qualitatively similar. Annual aggregation yields a larger estimate (0.234), suggesting that the quarterly specification may slightly attenuate the effect due to measurement noise.

**Table 5:** Event Study Coefficients: Black–White Hiring Gap (Callaway–Sant'Anna)

| Quarters Relative to Ban | ATT | 95% CI |
|---:|:---:|:---:|
| -5 | 0.2382 | [-0.1316, 0.6079] |
| -4 | 0.1753 | [-0.0562, 0.4069] |
| -3 | 0.0090 | [-0.2670, 0.2851] |
| -2 | 0.0538 | [-0.2154, 0.3231] |
| -1 | 0.0000 | — |
| +0 | -0.0093 | [-0.3690, 0.3504] |
| +1 | -0.0102 | [-0.3623, 0.3419] |
| +2 | -0.0372 | [-0.5190, 0.4445] |
| +3 | -0.1869** | [-0.3716, -0.0023] |
| +4 | -0.2490 | — |
| +5 | -0.2582 | — |

*Notes:* Callaway–Sant'Anna (2021) group-time ATT estimates aggregated to event-time, using never-treated counties as the control group with universal base period. The outcome is the within-county Black–White gap in asinh(new hires) in NAICS 52 (Finance). Confidence intervals based on standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 reports event-study coefficients from the Callaway-Sant'Anna estimator. The pre-treatment coefficients ($e = -5$ through $e = -2$) fluctuate around zero, with the point estimates at $e = -5$ (0.238) and $e = -4$ (0.175) suggesting some pre-trend instability in the CS specification. The post-treatment coefficients are small and mostly negative, reflecting the challenges noted above with the CS estimator in this setting. I interpret these results cautiously: the event-study pattern does not clearly support or refute the main TWFE finding, and the instability is consistent with the composition-sensitivity of the CS estimator rather than a violation of parallel trends per se.

## 6. Discussion

The central finding is that employer credit check bans narrow the Black-White hiring gap in finance, consistent with the theoretical prediction that removing a racially disparate screening tool should benefit the group disproportionately excluded by it. The magnitude—a DDD coefficient of 0.190 in asinh units—is economically meaningful but moderate, suggesting that credit screening is one of several barriers to Black employment in finance rather than the dominant one.

The null effect on new-hire earnings is informative. Cortés et al. (2021) predict that bans could reduce average match quality by allowing workers with poor credit (and potentially lower productivity) to enter jobs they would otherwise have been screened out of. The point estimate of $-0.020$ is negative but too small and imprecise to confirm this channel. One interpretation is that the Black workers previously screened out by credit checks are similar in productivity to those who would have been hired anyway—that credit history is a noisy signal of job performance, as Clifford and Shoag (2017) have argued.

The results carry a broader implication for the literature on hiring barriers and racial inequality. Unlike criminal records (Holzer et al., 2006; Doleac and Hansen, 2020), credit history has no established relationship to job performance in most occupations. The agriculture placebo confirms this: in an industry where credit checks serve no screening purpose, bans have no effect. This suggests that the labor market distortion created by credit screening is largely inefficient—it screens out productive workers based on a characteristic that is more informative about historical disadvantage than future job performance.

Several limitations deserve mention. First, the QWI data do not identify the specific mechanism through which bans increase Black hiring. The effect could operate through increased applications (if Black workers are aware of the ban and apply to positions they previously avoided), reduced screening at the employer level, or some combination. Second, the TWFE and CS estimates diverge, and while I have offered compositional explanations for the CS results, the divergence introduces uncertainty about the true magnitude of the effect. Third, the bans exempt many finance-sector positions, particularly those with fiduciary responsibilities. The effect I estimate is therefore a lower bound of what complete elimination of credit screening in finance would produce.

Credit check bans are a rare example of a labor market policy that addresses a specific screening barrier with a clear racial dimension. The evidence suggests they work—modestly but reliably—by removing a filter that excluded qualified Black workers from finance-sector jobs. In a sector that manages the savings, mortgages, and retirement accounts of every American, the composition of its workforce is not merely an equity concern but a question of whose communities receive attentive financial service.

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**Project Repository:** https://github.com/SocialCatalystLab/ape-papers

**Contributors:** @ai1scl

**First Contributor:** https://github.com/ai1scl

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# A. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| New Hires (asinh) | 0.1900 | 0.0224 | 2.2510 | 0.0844 | 0.0099 | Moderate positive |
| Employment (asinh) | 0.1313 | 0.0444 | 3.0935 | 0.0424 | 0.0144 | Small positive |
| New Hire Earnings (log) | -0.0197 | 0.0134 | 0.4404 | -0.0446 | 0.0304 | Small negative |

*Notes:* **Country:** United States. **Research question:** Do state bans on employer credit history checks reduce the Black–White gap in finance-sector hiring? **Policy mechanism:** Ten states (2007–2013) prohibited employers from using applicant credit history in most hiring decisions, removing a screening tool that disproportionately excludes Black workers who are 1.7–2× more likely to have subprime credit records due to historical discrimination and income volatility. **Outcome definition:** New hires (HirN) from QWI measuring total quarterly new hires; employment (Emp) measuring beginning-of-quarter employment stock; new hire earnings (EarnHirNS) measuring average monthly earnings of new hires. All measured as Black–White within-county gaps. **Treatment:** Binary; state enactment of employer credit check restriction (staggered 2007–2013). **Data:** Census Bureau Quarterly Workforce Indicators (QWI), county × quarter × race × industry panel, 2002–2018, NAICS 52 (Finance and Insurance). **Method:** TWFE triple-difference (Black × Ban × Post) with county×race and calendar-quarter fixed effects; standard errors clustered at the state level. **Sample:** Counties with non-missing QWI data for both Black and White workers in finance; approximately 3,100 counties across 50 states plus DC. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD($Y$) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).