Deleting a Signal: Evidence from Pre-Employment Credit Checks

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Abstract

We study the removal of information from a market, such as a job-applicant screening tool. We characterize how removal harms groups with relative advantage in that information: typically those for whom the banned information is most precise relative to alternative signals. We illustrate this using recent bans on employers' use of credit report data. Bans decrease job-finding rates for Black job-seekers by 3 percentage points and increase involuntary separations for Black new hires by 4 percentage points, primarily because other screening tools, such as interviews, have around 60% higher standard deviation of signal noise for Black relative to white job-seekers.

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1 Introduction

We are in the midst of an information boom: employers now check job-seekers' Facebook friends; lenders evaluate loan applicants' SAT scores; and other examples abound.[1] With this boom have come contentious debates. Should policy restrict access to some of this new information? When are different individuals helped or hurt by such restrictions?

We help answer such questions theoretically and empirically. Theoretically, we characterize which groups of individuals benefit from information restrictions using a novel measure of relative advantage in information. Relative advantage depends on an intuitive ratio between various information sources' precision as signals of unobserved quality. The group with the lowest (highest) relative advantage typically benefits from an information source being banned (available).

Empirically, we illustrate this result by studying recent restrictions on the use of credit report data in labor markets. We ask whether these restrictions have achieved one of their primary stated goals, to protect the labor market opportunities of minority job-seekers. We estimate these restrictions have sizable, negative effects on labor market outcomes for Black job-seekers: a 13-percent decline in Black job-finding hazards and a 3.7 percentage-point increase in Black workers' probability of involuntary separation shortly after hiring. Population average job-finding and white job-finding show little change after credit check bans, and our estimates for Hispanic job-seekers are usually statistically indistinguishable from those for white job-seekers.

Our theoretical work helps explain these results. In the case of credit check bans, Black job-seekers will not necessarily benefit on average from recent restrictions even though Black individuals are disproportionately likely to have low credit scores,[2] and even though Black individuals may have moderately noisier credit report information.[3] This is because the effect of restrictions also depends on the informativeness of credit report data relative to other existing screening tools. In a quantitative version of our theoretical model, we estimate this measure of relative advantage is

[1] See Jayakumar (2019) and Hughes (2013) for discussion.

[2] In the mid-2000s for example, over 50 percent of Black individuals were in the bottom quintile of the credit score distribution (Avery et al. (2009)).

[3] We quantify this in Section 5.5; for related evidence, see also Blattner and Nelson (2020).

particularly high for Black job applicants, largely because the standard deviation of noise in *other* screening tools such as job interviews and referrals is 70% higher for Black job-seekers than for white job-seekers.

Our application to credit check bans is motivated by the prevalence of such credit checks and by the rich variation available in recent regulations. Pre-employment credit checks, which we shorten as PECCs, have been a popular screening tool among employers, used by perhaps 60 percent of large firms to screen job applicants in 2010 (Society for Human Resource Management (2012)). However, often citing concerns about minorities' disadvantage in credit report information,[4] policy-makers have restricted PECCs in eleven states, New York City, and Chicago. These restrictions include varied exemptions for certain occupations and industries, providing us with rich variation to study.

We conduct our empirical work in two datasets - the Current Population Survey (CPS) and administrative data aggregated from state unemployment insurance records. We begin with a standard, state-time difference-in-differences analysis of job-finding and involuntary separation rates after PECC bans, and then corroborate our results using rich job-level variation in which occupations and industries are covered by or exempted from PECC bans. Results from demanding triple-difference models, from tests of our estimators' various parallel-trends assumption, and from permutation tests, all further corroborate these results. Results using the Sun and Abraham (2021) estimator and from the diagnostics proposed in Goodman-Bacon (2021) suggest that biases due to staggered treatment timing do not influence our results. We also investigate PECC bans' effects across other dimensions of heterogeneity for which non-PECC screening tools likely differ, such as different levels of educational attainment. We find that the negative effect of PECC bans is particularly pronounced for Black job-seekers without a college degree, which, assuming education provides a signal of match quality, is further consistent with our theoretical results.

[4]US Sen. Elizabeth Warren, for example, has claimed that "credit reports in the hiring process are disproportionately used to disqualify people of color from open positions" (Office of Senator Elizabeth Warren (2013)). The EEOC has pursued Civil Rights Act suits against employers asserting that PECCs "tend[s] to impact more adversely on females and minorities" (Crawford (2010)).

Our paper makes three contributions to the literature. First, we show theoretically how a ban on an information source has differential effects across groups depending on a novel measure of groups' relative advantage in information. Whereas some models of labor market screening (and discrimination) have emphasized group differences in average match quality, signal precision, or both (Autor and Scarborough, 2008; Phelps, 1972; Aigner and Cain, 1977), our characterization of how relative advantage in signal precision affects the incidence of banning a signal across groups is new. We view our theoretical result as relevant in a range of information-intensive markets, such as labor and finance, where regulation contemplates bans of various information sources, and where extant information restrictions have been found to generally worsen Black individuals' outcomes (Agan and Starr, 2018; Wozniak, 2015; Doleac and Hansen, 2020).

Second, focusing empirically on the example of PECC bans, we provide the first evidence that PECC bans have negative effects on average labor market outcomes for Black job-seekers, despite Black individuals' lower average credit scores; we are also the first to estimate PECC bans' effects on new-hire match quality. These findings show that restricting PECCs has large labor market impacts: the estimated overall effect of PECC bans on Black job-seekers' job-finding is as large in magnitude as a 5.9 percent rise in wages (Lichter et al. (2014)). A contemporaneous paper, Ballance et al. (2017), studies the impacts of PECC bans on census tracts with different average credit scores. Friedberg et al. (2016) study the direct effects of PECC bans on individuals with poor credit health, and Cortes et al. (2018) investigate the supply-side responses to PECC bans.[5]

Third, we are the first to provide quantitative estimates of divergent signal noise across race and ethnic groups for standard job-screening tools such as interviews and referrals. This finding

[5] A related literature has studied the correlation of credit histories with measures of worker personality traits (Bernerth et al. (2012)), employee ratings (Bryan and Palmer (2012)), and match quality (Weaver (2015)). A more detailed portrait of how firms use PECCs during the hiring process is provided by the sociology literature on PECCs, which has studied how HR professionals interpret credit report information (Kiviat (2017)) and respond to adverse or positive credit information from different types of workers (O'Brien and Kiviat (2018)). Corbae and Glover (2018) theoretically study PECC bans in an equilibrium search framework where credit report data, due to the interaction between credit constraints and human capital, are used as a proxy for imperfectly observed education.

provides empirical support for models in which employers have noisier information about Black job-seekers than other groups (Aigner and Cain, 1977; Cornell and Welch, 1996; Morgan and Vardy, 2009; Bjerk, 2008) and help explain known results in other settings beyond information regulation, including the higher return for Black individuals to other labor market signals such as occupational licenses (Blair and Chung (2018)), referral letters (Heller and Kessler (2021)), and veteran status (De Tray (1982)), the positive relationship between firm size and Black share (Holzer (1998), and the persistent impacts of temporary affirmative action programs (Miller (2017)).[6] More broadly, our results show how differences in the precision of signals received by employers, given the social environment and commonly used screening tools, may represent a form of institutional discrimination (Small and Pager (2020)) that generates racial disparities in labor market outcomes.[7]

The remainder of this paper is organized as follows. We develop our theoretical results in Section 2. Section 3 provides background on the use of PECCs and introduces our data, and Section 4 describes our empirical strategy. We present our results in Section 5, where we also apply our theoretical results to our empirical estimates. Section 6 concludes.

[6]Recent research has explored the implications of the usage of particular types of information in labor markets, including more specific types of credit information (Bos et al. (2018), Herkenhoff et al. (2016), and Dobbie et al. (2019)), drug testing (Wozniak (2015)), criminal records (Holzer et al. (2006), Finlay (2009), Shoag and Veuger (2016), Agan and Starr (2018), Craigie (Forthcoming), and Doleac and Hansen (2020)), unemployment duration (Kroft et al. (2013) and Jarosch and Pilossoph (Forthcoming)) and job-testing (Autor and Scarborough (2008) and Hoffman et al. (2018)). Liberman et al. (2018) study the effects of removing credit history information on credit market outcomes in Chile.

[7]A range of evidence from sociology and economics points to signal precision playing a central role in labor markets in particular, for reasons including the importance of social networks and referrals in job search (Braddock II and McPartland (1987), Neckerman and Kirschenman (1991), Waldinger (1997), Smith (2005), Bayer et al. (2008), Hellerstein et al. (2011)), interpersonal or cultural barriers to interpreting interview information (Lang (1986)), or a hiring manager's lower ability to screen applicants from races and ethnicities other than their own (Giuliano et al. (2009), Benson et al. (2019)). Employers and job-seekers also directly report disparities in signal precision: Wozniak (2011) finds that 23% of employers say that they find it easier to determine who is a good hire for white than Black applicants and that, for both employers and Black job-seekers, the most common specific suggestion for improving Black-male employment outcomes is creating ways for Black job-seekers to provide additional information to employers.

2 Conceptual Framework

This section analyzes theoretically the effects of banning an information source. We develop a measure of relative advantage in information, and we show our measure has a straightforward closed form in a range of settings.

2.1 A Model of Relative Advantage in Information

We focus on markets with information asymmetries where agents on one side of the market – the senders – compete to signal the other side of the market – a receiver – that their quality is above some (potentially endogenous) threshold. Examples include labor market settings with fixed wages, or credit market settings similar to US mortgage market, where loan approval often depends on threshold rules.[8] We characterize the effects of banning an information source when that source as well as other information sources perform differently for senders from different groups, and when these groups differ in terms of their underlying match qualities for the receiver. We also allow for receivers, or their priors, or particular signals, to be biased against particular groups.

The model highlights three mechanisms that drive the effects of information restrictions in screening. First, adding an information source or increasing the precision of an existing informa-

[8]See Keys et al. (2010) for evidence on the use of threshold rules in mortgage approvals. In a labor market setting, the assumption of fixed wages could be motivated by a binding minimum wage, firms having all the bargaining power and paying workers their (common) outside option, or firm commitment to paying posted wages. Hall and Krueger (2012) find that only about one-third of new hires reported bargaining over their wage. Of the two-thirds that did not report bargaining over their wage, about half reported knowing their exact wage prior to being interviewed for the job. Wage bargaining is more common among highly educated workers and workers making job-to-job transitions. Broadly, we interpret this evidence to suggest that modeling firms as committing to posted wages is a reasonable assumption for firm behavior in many cases, particularly among unemployed job-seekers as in our empirical setting. We also discuss in Section 5.2 some evidence that PECC bans have not had a detectable effect on wages. At the same time, our model may not be appropriate for analyzing labor market segments where wage-bargaining dominates, or for understanding information sources more commonly used during wage-bargaining rather than screening, such as salary histories (Bessen and Denk, 2020; Hansen and McNichols, 2020).

tion source – that is, a refinement of the available signal technologies – will spread out, or add variance to, the receiver's expectations about the senders' qualities. Second, such an increase in variance is good for any group in partial equilibrium, so long as the senders benefit from conveying any information at all – that is, so long as a sender who sends no information does not get selected by the receiver. However, and third, in general equilibrium the threshold for being selected is endogenous, so the effects of adding or subtracting signals depends on a sense of *relative* advantage over other groups in the market.

We now specify the details of the model. The distribution of match qualities within group g has mean $\mu_{g,0}$ and inverse variance $h_{g,0}$. Receivers are risk neutral and desire to transact with a fixed number of senders M, and receiver payoffs are increasing in match quality. Match qualities are unobserved, but various information sources indexed by k send signals about match quality with noise that is characterized by inverse variance $h_{g,k}$ for group g. Receivers form posterior beliefs about match quality based on these signals and make matching decisions based on these posteriors. We suppose receivers have rational priors based on group membership, consistent with a model in which receivers statistically discriminate, though we extend to more general (and potentially biased) priors in Appendix Section A.[9] We focus on one signal that is banned and we represent all other available information sources as a single composite "other" signal, where this composite is defined formally in Appendix Section A. Following a long literature and especially Autor and Scarborough (2008), we suppose both match qualities and signal noise are normally distributed, which makes manipulating receivers' posteriors tractable.

[9]Whereas statistical and taste-based discrimination are illegal in labor market contexts under Title VII of the Civil Rights Acts and in credit markets under the Equal Credit Opportunity Act, including such behavior in the model is consistent both with our empirical results on PECC bans' effects, and broader evidence in labor markets on how employers discriminate based on race and ethnicity in the hiring process (for example see Bertrand and Mullainathan (2004)) and in credit markets on how lenders discriminate based on race and ethnicity in underwriting decisions (Bartlett et al. (2019)). When receivers cannot use group membership in decision-making, our model is also consistent with receivers observing how signal precisions vary across groups, for example when employers are aware of how much statistical noise there is in a given referral or or educational background.

Given risk-neutrality, it is straightforward to see that receivers' optimal strategies are to transact with all senders for whom the receivers expect match quality to be above some cutoff κ. Therefore to study senders' outcomes, we are interested in what share of posterior means are above κ for each group g, given the distributions of posteriors that arise from a given set of signaling tools. We refer to the share of group g's posteriors above this cutoff as λ_g, or a success rate for group g.[10] We assume the desired number of successes, M, is small enough that receivers select matches from the right tail of perceived match qualities; this is consistent with, for example, an employer hiring less than half of the applicants whom it sees.

We now examine how the availability of the second signal affects these success rates. While the availability of the second signal unambiguously make the tails of the distribution of posteriors thicker,[11] the quality threshold κ must also increase in response, so there are two counteracting forces that could either increase or decrease a given group's success rate. The result that senders can benefit from a refinement in signals that shifts some posteriors to a "good" region for senders is familiar to the literature on Bayesian persuasion, for example in Proposition 2 of Kamenica and Gentzkow (2011), and we characterize implications of this in a setting where an endogenous quality threshold can generate counteracting effects across groups in equilibrium.[12]

To characterize the banned signal's net effect, we parameterize the availability of the second signal with $\alpha \in [0,1]$, and replace the noise of the banned signal $h_{g,\text{banned}}$ with the term $\alpha h_{g,\text{banned}}$, such that $\alpha = 0$ corresponds to a total ban and $\alpha = 1$ corresponds to no ban at all. We then evaluate the effect of varying α on group-specific success rates for two groups g and g'. Details are presented in Appendix A. Our main result is that success rates for a group are increasing

[10]The desired number of successes, M, and the resulting threshold κ, may vary across contexts. We abstract from these differences in this section. Our empirical approach is robust to rich differences between firms, states, and across time, as long as those differences are not correlated with PECC bans.

[11]That is to say, posteriors about each sender become more precise, and therefore the *population* distribution of posterior means becomes more diffuse. This can be seen by examining expression A.5 in Appendix Section A.

[12]This literature has also studied strategic interaction among senders, for example in Gentzkow and Kamenica (2017), Board and Lu (2018), and Au and Kawai (2020). We study a setting where senders take the signal technology as given, and a planner regulates the availability of signals with an eye to equilibrium incidence across groups.

in the availability of the banned signal if and only if that group's relative advantage in the banned signal precision, $h_{g,\text{banned}}/h_{g',\text{banned}}$, is greater than the group's relative advantage in other available screening tools, $\omega_g/\omega_{g'}$. Relative advantage takes an especially simple form when underlying match qualities are identically distributed across groups with the same variance $1/h_0$:

Proposition 1. *With identically distributed match qualities across groups, the success rate λ_g of group g is increasing in the availability α of the banned signal as,*

$$\frac{d\lambda_g}{d\alpha} > 0 \ \text{ if and only if } \ \frac{h_{g,\text{banned}}}{h_{g',\text{banned}}} > \frac{\omega_g}{\omega_{g'}} \tag{2.1}$$

where the final term, which captures relative advantage in baseline signals, is characterized by

$$\omega_g = h_{g,\text{baseline}}\left(h_0 + h_{g,\text{baseline}}\right) \tag{2.2}$$

Intuitively, the proposition shows that a group, labeled g, can benefit from the availability of a signal even without having absolute advantage vis-a-vis that signal (i.e., even if the ratio $h_{g,banned}/h_{g',banned}$ is less than 1), if and only if the same group is more disadvantaged in terms of other signals (i.e., if the ratio $\omega_g/\omega_{g'}$ is yet lower). Figure 1 illustrates this result graphically. In Figure 1 Panel A, we show equilibrium success rates after a signal ban: one group, labeled "blue," has noisier signals under baseline screening tools and therefore less diffuse posteriors than another group, labeled "green." Correspondingly the blue group has a lower success rate, given the equilibrium quality threshold κ. Figure 1 Panel B then illustrates how posterior means of match qualities shift when the banned signal is instead available, in the case where the banned signal provides more precise signals for the blue group. Finally, Figure 1 Panel C illustrates how the quality threshold κ must then shift in order for markets to clear in response to the new information provided by the additional signal, and how this shift affects success rates for each group. We see that the group for which the banned signal provides relatively precise signals is indeed the group that benefits from the availability of the signal.

When groups have differently distributed match qualities, relative advantage takes a more general but still intuitive form:

Proposition 2. *When match quality distributions differ across groups, the success rate λ_g of some group g is increasing in the availability α of the banned signal as,*

$$\frac{d\lambda_g}{d\alpha} > 0 \text{ if and only if } \frac{h_{g,banned}}{h_{g',banned}} > \frac{\omega_g}{\omega_{g'}} \left(1 + \frac{\Delta\mu}{\kappa - \mu_{g,0}}\right) \tag{2.3}$$

where $\Delta\mu = \mu_{g,0} - \mu_{g',0}$, and where relative advantage in baseline signals is characterized by,

$$\omega_g = h_{g,baseline}\left(h_{g,0} + h_{g,baseline}\right)/h_{g,0} \tag{2.4}$$

The more general expression in Proposition 2 nests the simpler expression in Proposition 1 by adding two multiplicative factors, each of which relates to differences in match quality distributions across groups. The factor newly included in expression 2.4 captures the effect of differences in the dispersion of match qualities $h_{g,0}$. The factor newly included in expression 2.3 captures the effect of differences in mean match qualities $\Delta\mu$. Proposition 2 provides a more general characterization of the role of signal precision and bias in determining the effects of changing available signaling tools than has been shown in prior work. Aigner and Cain (1977), for example, study signal precision without characterizing changes in available signals, and Autor and Scarborough (2008) assume signals differ only in their biases, not their precisions.

Differences in mean match qualities $\Delta\mu$ may reflect the effects of institutional discrimination across groups (Small and Pager, 2020), including, for example, unequal access to education, but these differences may also reflect taste-based discrimination or more overt bias against particular groups that affect the receiver's "perceived" match quality. The result in Proposition 2 shows that a group is more likely to have relative advantage in a banned signal whenever that group is the target of such discrimination.[13] The same conclusion also holds for other types of bias that we explore in Appendix A, including biased priors or stereotypes (Bordalo et al., 2016; Bohren et al., 2019,

[13]This holds given our assumption that matches are made from the upper half of the perceived match quality distribution, such that the denominator $\kappa - \mu_{g,0}$ is positive.

2020; Charles and Guryan, 2008; Darity and Mason, 1998)), and biased signals where the receiver remains unaware of the bias in the signal (Autor and Scarborough, 2008).

3 Empirical Setting: Background and Data

In this section we provide background on our empirical application and describe our data. Our primary dataset is the Current Population Survey (CPS), which we use to measure job-finding, involuntary separation rates, and employment. We discuss additional details in Appendix D.

3.1 Background and Institutional Details

While evidence on employer use of PECCs is limited, an industry survey suggests that perhaps 60 percent of firms used PECCs to screen job applicants in 2010; roughly a quarter of these firms used PECCs for all job applicants (Society for Human Resource Management (2012)). For over half of these firms, the primary reason for using PECCs was to prevent theft, and correspondingly these firms report using PECCs for nearly all jobs (91 percent) that involve handling cash or other fiduciary responsibility (Society for Human Resource Management (2012)).

PECCs have a non-trivial effect on hiring decisions. Household surveys suggest that 10 percent of low- and middle-income job-seekers recall being told they were denied a job on the basis of information in their credit report (Traub (2013a)). The true PECC-related rejection rate may be higher is some applicants are unaware of the reason for their rejection (Traub, 2013b).

Thirteen new PECC bans have been enacted to date, while more than a dozen other states have seen related legislation proposed but not enacted (Morton (2014)). Washington was the first state to enact a PECC ban, in April 2007. Hawaii, Oregon, and Illinois then followed in 2009-2010, and six other states followed in 2011-2013. Delaware restricted PECCs for public employers in 2014. Chicago and New York City enacted city-level restrictions on PECCs in 2012 and 2015. Appendix Figure 1 shows the states and large cities that have enacted PECC bans, along with the dates these laws were signed and went into effect.

These bans vary in strength because of the exemptions they grant to certain jobs, for example jobs that involve access to payroll information, jobs in high-level management, jobs that involve supervising other staff, and jobs in dozens of other industries or categories such as law enforcement, gaming, space research, banking, or insurance.[14] In Table 1 we summarize the full breadth of this heterogeneity in PECC bans' exemptions. We collected this heterogeneity by referring to statute texts, various state agencies' interpretations of statutory terms such as "banking activities," and guidance from human-resources law firm Littler Mendelson that summarizes relevant case law (discussed further in Appendix D). We then translate each law's exemptions into the Census industry and occupation codes that will classify jobs in our data, a process that we describe in more detail in Section 3.3 below. Although there have been 13 state and local PECC bans, in practice we only study ten; the remaining three are either city bans which were enacted after the state had already banned PECCs (Chicago), enacted towards the end of our sample (New York City), or the Delaware ban, which only covered the public sector and continued to allow PECCs after an initial interview, which according to evidence in Society for Human Resource Management (2012) likely makes the restriction non-binding. See Appendix Section D for more details.

The results of this process indicate how PECC bans' coverage varies across states. Among jobs ever covered by a PECC ban, we estimate that 49 percent are granted exemptions from a PECC ban in at least one state. And among states that enact bans, the share of workers covered by a ban

[14] The bans also differ in their enforcement mechanisms. In Illinois, for example, enforcement relies on private litigation by job applicants; in contrast the Connecticut law tasks the state Department of Labor with enforcement. These differing enforcement mechanisms also raise the question of how vigorously different regulators or plaintiff bars have chosen to enforce these laws. From our conversations with state regulators and reading of the professional literature in human resources, we conclude that enforcement has not been particularly vigorous in most states, but that some employers have nonetheless been eager to comply with bans to avoid being in non-compliance. Indeed, Phillips and Schein (2015) reported that, as of their writing, state courts had seen no cases on the state-level bans enacted by 2012, which could be consistent with strong compliance with these laws. Furthermore, other evidence is consistent with at least a large share of firms complying with the bans: Ballance et al. (2017) find, using Equifax credit report data, that employer-related credit checks per unemployed person decline 7 to 11 percent in the three years after credit bans are passed (see Figure 3 in Ballance et al. (2017)).

ranges from 42 percent in Connecticut, to 80 percent, in Hawaii.

3.2 CPS

We use the panel dimension of the 2003-2018 Current Population Survey's (CPS) micro-data (US Census Bureau (2019)). The Bureau of Labor Statistics uses the CPS to measure cross-sectional unemployment and labor-force participation, while the panel dimension is used for estimating gross flows in and out of unemployment, employment, and non-participation. Monthly sample sizes are about 100,000 adults, each of whom stays in the sample for four consecutive months, then leaves for eight months, and then re-enters for a final four months. We restrict the sample to civilians over the age of eighteen who are not on a temporary layoff. As illustrated in Appendix Figure 1, the number of pre- and post-ban years varies between treatment states. Consequently, for states implementing PECC bans, we restrict the sample to a balanced set of pre- and post-ban years common to all states, which is 3 years before the bans' implementation and 4 years afterwards.[15]

Table 1 presents summary statistics related to PECC bans using the CPS data. Columns (1) and (2) respectively show labor market statistics for states that do and do not ban PECCs. Columns (3) and (4) then focus on states with PECC bans, and respectively show statistics for jobs covered by and exempted from those bans. Statistics are presented separately for Black, Hispanic, and white workers in panels A1 through A3. We see that labor market characteristics such as employment rates are broadly similar within race and ethnicity groups across states, although employment rates are slightly higher in states without PECC bans. Wages are also higher in PECC ban states, as is the overall share of workers with a four-year college degree.

The CPS provides rich longitudinal information on individual job-finding hazards and involuntary separation rates. However, our estimates, although reasonably precise, are somewhat noisy. Furthermore, data in the CPS are self-reported and this may result in further uncertainty. We

[15]As recommended by Schmidheiny and Siegloch (2020) (see their Remark 2), we note that this implies our estimates are not informative about PECC bans' effects at horizons longer than four years; both our regression estimates and our graphical analyses reflect this balanced-data restriction.

address these concerns by analyzing the Job-to-Job (J2J) Flows data released as part of the Longitudinal Employer-Household Dynamics (LEHD) program (US Census Bureau (2018)). We discuss these data more in Appendix Section D.6 and present results using these data as robustness checks in Appendix Section C.3.

3.3 Encoding Job-Level Variation

As we introduced in Section 3.1 and Table 1, PECC bans typically include a substantial number of job-specific exemptions. In order to use this job-level variation to complement our state-level analyses, we categorize which jobs in our data are covered by or exempted from each law.

We identify jobs in our data using US Census 4-digit industry codes and 4-digit occupation codes, the most precise classifiers available in the CPS. We then encode each of these occupations and industries as either covered by or exempted from each PECC ban, based on the legal sources detailed in Section 3.1 and, when necessary, our judgment. Finally, consistent with the PECC ban statutes, we code a job as exempt whenever either its industry or occupation is coded as exempt. More detail on this classification procedure is presented in Appendix Section D.3.

We next use this classification of jobs' exempt status to measure individuals' exposure to PECC bans. For employed individuals this is straightforward: in a PECC-ban state after the enactment of a ban, an individual is exposed to the ban whenever his current job is not exempt. For unemployed individuals, we use two measures of job-level exposure. Our first measure, which we refer to as "past job" exposure, uses an unemployed individual's most recent job. Our second measure, which we refer to as "expected job" exposure, uses an estimate of each unemployed individual's probability of searching for work in a non-exempt job, conditional on her most recent job. We construct this measure by assuming these search probabilities are proportional to observed job-to-job transition rates (via unemployment) in the absence of PECC bans, and then using our ban-specific measures of jobs' exempt status. Appendix Section D.3 describes the construction of our "expected job" exposure more formally. For all three of these measures of individuals' exposure to PECC bans, i.e. current (C), past (P), and expected (E) job exposure, let $T^{l}_{j(i),s(i)}$ stand for the exposure of individual i in job j and state s after the enactment of state s's PECC ban, for $l \in \{C, P, E\}$.

4 Empirical Strategy

4.1 State-level Variation

Our baseline specifications are state-time difference-in-differences models using the staggered adoption of PECC bans across geography and time. Let y_{it} be a labor market outcome for an individual i living in state $s(i)$ in time t, $D_{s(i)}$ be an indicator for state $s(i)$ having ever put into effect a PECC ban, and $P_{s(i),t}$ be an indicator for whether a state had implemented a PECC ban by time t. We estimate:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_r \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \varepsilon_{it} \tag{4.1}$$

where all right-hand-side variables are fully interacted with race or ethnicity r.[16]

In other specifications, we also estimate triple-differences models of the form:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \xi_{s(i),t} + \sum_{r \neq W} \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \varepsilon_{it} \tag{4.2}$$

These models modify equation (4.1) by adding state-time fixed effects while suppressing the $r = W$ (white job-seeker) interaction terms. The coefficients δ_r therefore test for PECC bans' effects among group r relative to white job-seekers.

We also estimate event-time models where we fully interact our treatment dummies with event time, i.e., the number of time periods since a given ban took effect. Formally, let t_0^s be the time period when PECCs are banned in state s and define $\kappa_{st} = t - t_0^s + 1$. The event-time specification

[16] We define three mutually exclusive race or ethnicity categories, $R = \{\text{white}, \text{Black}, \text{Hispanic}\}$.

corresponding to equation 4.1 is then:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_k \sum_r \delta_r^k 1_{r=r(i)} \times D_{s(i)} \times 1_{k=\kappa_{st}} + \varepsilon_{it} \tag{4.3}$$

with other event-time specifications defined analogously.

4.2 Job-level Variation

The specifications in Section 4.1 above do not exploit the substantial job-level variation available in different states' PECC bans. To leverage this job-level variation we use the treatment measure $T^l_{j(i),s(i)}$, as we constructed in Section 3.3. Recall that $T^l_{j(i),s(i)}$ is a measure of how state s's PECC ban covers an individual i with job j, where we use the notation $l \in \{C, P, E\}$ to stand for a PECC ban's coverage of either a current job (C), past job (P), or expected job (E). Our baseline specification relying on job variation is then:

$$y_{it} = \alpha_{s(i),j(i),r(i)} + \gamma_{t,r(i)} + \sum_r \delta_r D_{s(i)} \times P_{s(i),t} \times T^l_{j(i),s(i)} + \varepsilon_{it} \tag{4.4}$$

Note that this specification may produce high-variance estimates in datasets of moderate size, given the large number of state-job fixed effects $\alpha_{s(i),j(i)}$ to be estimated. In practice we therefore form groups of jobs such that all jobs in a given group are either all treated or all not treated by a PECC ban in any given state at any given time – these are the standard fixed effects to include given the level of variation in the data. There are 43 such job groups in our setting. Throughout our empirical work we therefore allow $j(i)$ to stand for job group rather than job.

As in Section 4.1 above, we also estimate event-time versions of our job-level specifications to determine the validity of our parallel trends assumptions and to explore the path of treatment effects over time.

4.3 Duration Dependence

One of our outcomes of interest y_{it} is unemployed individuals' job-finding rates, i.e., probabilities of transitioning from unemployment to employment in adjacent months. It is well known that job-finding rates exhibit duration dependence, so we formally estimate a hazard model to account for how a PECC ban may affect both the probability of job-finding at a given unemployment duration and the composition of durations among the pool of unemployed. In particular, we specify a semi-parametric proportional hazards model of job-finding as in Han and Hausman (1990) or Meyer (1990), and show how it can incorporate our difference-in-differences strategy.

To begin, we model $\lambda_{i,t}(\tau)$, the probability of finding a job for unemployed person i, at time t, after being unemployed for a length of time τ, given individual characteristics X_i and an arbitrary set of fixed effects W_i, as:

$$\lambda_{i,t}(\tau) = \lambda_0(\tau)\exp\left(W_i + \sum_r \beta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i'\beta_{x,r(i)}\right) \tag{4.5}$$

Note that the λ_0 term is fully non-parametric in τ, as in Cox (1972), while the proportional hazards assumption appears through the exponentiated term's non-dependence on τ.

We transform this continuous time hazard, $\lambda_{i,t}(\tau)$, into a discrete time hazard, $\lambda^d_{i,t}(\tau)$, defined as the probability of job-finding between $\tau - 1$ and τ conditional on being unemployed at time $\tau - 1$. Following Han and Hausman (1990) and Meyer (1990), we can work from 4.5 to write the discrete time hazard in complementary-log-log form as:

$$\ln(-\ln(1-\lambda^d_{i,t}(\tau))) = \alpha_\tau + W_i + \sum_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i'\beta_{x,r(i)} \tag{4.6}$$

$$\alpha_\tau = \ln \int_{\tau-1}^{\tau} \lambda_0(s)ds \tag{4.7}$$

If we replace the arbitrary fixed effects W_i with those from the difference-in-differences specifications described in Sections 4.1 and 4.2, this specification inherits difference-in-differences' parallel trends identifying assumption. In particular, we assume parallel trends in the complementary-log-log of discrete-time hazards. It can be shown in the derivation of equation 4.6 that this assumption is equivalent to the assumption of parallel trends in log *continuous* time hazards. Given the nonnegativity of hazard rates, we view this log form as the most natural parallel trends assumption to make. We also interact our treatment dummies with dummies for event time, i.e. κ_{st}, to generate event-time versions of any of these difference-in-differences hazard model specifications, analogous to equation 4.3.

We take two approaches to estimation depending on the aggregation level of our data. For individual-level data like the CPS, the parameters of equation 4.6 can be estimated via maximum-likelihood (Meyer (1990)). For aggregated data like the LEHD-J2J or cases where we need to estimate linear models, we use OLS where we plug in population-average job-finding rates for $\lambda_{i,t}^{d}(\tau)$ on the left-hand-side of equation 4.6. We discuss finite-sample properties of this nonlinear plug-in estimator in Appendix Section D.4, where we show that any finite-sample bias due to nonlinearity on the left-hand-side is negligible so long as we exclude LEHD-J2J data from five states with few minority job-seekers: Idaho, Wyoming, Montana, South Dakota, and North Dakota.

4.4 Staggered Treatment

Recent literature on difference-in-differences in settings where treatment is staggered over time (e.g., Goodman-Bacon (2021), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Borusyak et al. (2021)) shows that two-way fixed-effects estimators like the ones we use can be confounded by heterogeneity in treatment effects across treated units when treatment is staggered over time. We re-estimate our key regression specifications from sections 4.1 and 4.2 using the robust estimator in Sun and Abraham (2021) to explore whether these biases influence our results. We note that Sun and Abraham (2021) and related recent tools use linear estimators, so in our job-finding survival models we first aggregate the data to use a linear model as in Appendix Section D.4, and we then show robustness to the Sun and Abraham (2021) estimator within that linear model. We discuss these results in detail throughout section 5 below; broadly we find that these biases due to staggered treatment adoption have almost no impact on our results.

To understand why staggered treatment adoption has little quantitative influence on the estimates in our setting, we adapt a diagnostic strategy suggested in Goodman-Bacon (2021) to assess two underlying sources of staggered-treatment bias. We discuss this analysis extensively in Appendix C.1. One important source of bias is a two-way fixed-effects estimator's implicit use of some treated units as controls for other treated units. We find using Goodman-Bacon (2021) weights that essentially all (97%) of the identifying variation for a linear two-way fixed effects estimator in our setting does *not* use these potentially confounding treated-vs.-treated comparisons; this reflects the fact that most states never enact a PECC ban. The second source of bias is that, among treated-vs.-never-treated comparisons, correlation between treatment timing and treatment effect heterogeneity can lead a two-way fixed effects estimate to differ from the average treatment effect (ATE). In assessing this second source of bias, we find that there is essentially no correlation between individual states' treatment timing and those states' treatment-effect estimates, and furthermore that most state-specific estimates are clustered closely to our overall estimated treatment effect.

5 Results

5.1 Job-Finding Rates

We start with estimating PECC bans' effects on job-finding. Panel A1 of Table 2 reports our baseline estimates in CPS data of δ_r from equation 4.6, with the three table rows corresponding to Black, Hispanic, and white job-seekers. Column (1) presents results from a specification without demographic or state-level policy and economic controls. Column (2) then reports estimates using the Sun and Abraham (2021) interaction-weighted estimator that aggregates treatment cohort-specific treatment-effect estimates, using the linear specification on aggregated data discussed in Section 4.4. Column (3) then returns to the column (1) specification but adds our set of demographic covariates: education groups; age groups; gender; and marital status; urbanicity; and interactions between month-of-year and Census division (to capture possible seasonal effects).[17]

For these first three columns, potential confounders, such as the Great Recession, will only bias our estimates if they cause state-specific changes in labor market outcomes by race or ethnicity, and if these state-specific changes are correlated with the implementation of PECC bans. To further show robustness to such potential confounders, our preferred specification in column (4) then adds controls for state-time varying policy and economic shocks, including controls for policy choices in state s at time t (such as Ban-the-Box policy, expanding Medicaid, or extending unemployment insurance) and exposure to local economic shocks (such as manufacturing decline, the housing boom and bust, immigration, and fracking).[18] All of these state-year controls are interacted with race or ethnicity dummies.

Our estimate for Black job-seekers in Panel A1 column (1) of Table 2 is roughly -10 percent. And while the 95 percent confidence interval for this estimate is somewhat wide, we reject a null of no effect of PECC bans for Black job-seekers. The estimate in column (2), which implements the Sun and Abraham (2021) estimator, is about -8 percent. This similar difference relative to Column (1) confirms that any biases due to staggered adoption of treatment are negligible, as also discussed in Section 4.4. We further explore these estimates in Appendix Section C.1 and find that the modest differences between Columns (1) and (2) are driven by our need to use aggregated data in column (2), rather than by staggered treatment timing per se.

This estimate is essentially unchanged after adding demographic covariates in column (3).

[17]Specifically, our age categories are 18-29, 30-39, 40-49, 50-61, and 62+; education categories are less than high school, GED, high school diploma (not GED), some college, and college or more; marital status is an indicator for married; and the definition of urbanicity is taken from the CPS documentation.

[18]The full set of controls include: Saiz's price elasticity of housing multiplied by year dummies (Saiz (2010)), a dummy variable that equals 1 if the state had geological potential for fracking in a given year (Bartik et al. (2019)), the share of manufacturing jobs in 2000 multiplied by year dummies (ACS 2000), a dummy variable that equals 1 if the state had any Ban-the-Box policy in a given year, a dummy variable that equals 1 if the state had expanded Medicaid by a given year, year 2000 state Hispanic and foreign born share interacted with year dummies, and a measure of unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). All controls are interacted by race-ethnicity dummies. These controls are further described in Appendix Section D.1.

Adding controls for time varying effects of economic conditions or changes in state policy in column (4) increases the magnitude of the point estimate to about -13 percent, although we cannot reject the previous estimates. Our estimated effects for white job-seekers are small and statistically indistinguishable from zero. For white job-seekers we can reject a positive effect on job-finding of 5 percent or more. For Hispanic job-seekers, we estimate a marginally significant increase in job-finding rates after PECC bans are passed, which we discuss more below.

Panel A2 then reports the overall effect of being treated by a PECC ban for all workers. In all five specifications, the overall effect is small in magnitude and negative, with coefficients ranging from -1.7 to 0.1 percent. The standard errors are small enough that we can rule out large positive or negative effects of PECC bans, although small to moderate positive or negative effects are consistent with the data.

To illustrate the data patterns that drive these estimates, Figure 2 shows corresponding event study plots, with three panels for Black, Hispanic, and white job-seekers. Like our preferred specification in column (4) of of Table 2, these figures all include controls for demographics, other state-by-time policy variation, and economic shocks.

In Panel A of the figure, we see that Black job-finding hazards in PECC-ban states are on mostly parallel trends with non-ban states in the three years prior to implementing PECC bans. This helps validate our difference-in-differences strategy's identifying assumption. Then immediately after the implementation of a PECC ban, Black job-finding hazards fall by about 15 percent. They fall by an additional 10 percent of initial levels in the second year after a PECC ban's implementation and then fall further to about 30 percent lower in the third year after implementation before rebounding somewhat in the fourth year.

Panels B and C of Figure 2 then show analogous estimates for Hispanic and white job-seekers. These plots also exhibit parallel trends in the three years prior to the implementation of PECC bans. On net we see little evidence of an effect in the post-PECC ban years for white job-seekers. For Hispanic job-seekers, there is some evidence of an uptick in job-finding in the first two years after the PECC bans' implementation, though this attenuates in subsequent years.

We next turn to using job-level variation to estimate the effects of PECC bans on job-finding. This serves as a validation of our state-level results: if the effects we documented in Panel A of Table 2 are indeed attributable to PECC bans, then we should see these effects in particular for jobs exposed to these bans. To do this, we extend the baseline proportional hazards model in equation 4.6 to use our measures $T^{l}_{j(i),s(i)}$ of job-specific exposure to PECC bans, as defined in Section 3.3. Results for estimating several versions of this specification are shown in Panel B of Table 2, and corresponding event-time plots are shown in Appendix Figure 2.

We measure job-specific exposure using expected-job exposure, $T^{E}_{j(i),s(i)}$. Column (1) of Table 2 Panel B starts with our baseline version with no covariates other than the appropriate job-level fixed effects, W_i. Column (2) then reports estimates using the Sun and Abraham (2021) estimator. The results are qualitatively similar to the estimates in Column (1), although moderately smaller in magnitude. As with the results in Panel A, we find in Appendix Section C.1 that the differences between Columns (1) and (2) in Panel B are driven by aggregating the data rather than the adjustment for staggered adoption itself.

Column (3) then adds individual-level covariates X_i, and Column (4) adds state-policy and economic controls. Estimates for Black job-seekers are substantially larger than our state-level estimates from Panel A1 in three of the four columns, indicating that the patterns observed in our state-level analysis are indeed driven by jobs covered by PECC bans. If one scales up the point estimates from our state-level analysis in Panel A, Column (4) by the share of unemployed Black individuals who previously worked in covered jobs in PECC ban states (as reported in the summary statistics in Table 1), the resulting estimate is .189, strikingly close to our job-level estimate of .183 in Column (4) of Panel B1 of Table 2. While estimates using job-level variation have the potential to be attenuated by spillovers from treated to exempt jobs, this similarity provides further evidence that our results are driven by individual exposure to PECC bans rather than other explanations.

In specifications using job-level variation, the sometimes marginally significant positive effect for Hispanic job-seekers that we found in Panel A1 is slightly reduced in magnitude and no longer statistically significant, and in subsequent columns that include our individual-level and state-level controls, these estimates are reduced to be essentially zero (a 1-percent, not percentage point, change in job-finding hazards). We thus see three pieces of evidence suggesting a null effect of PECC bans on Hispanic job-finding: the initial increase seen in Figure 2 is reduced to nearly zero in subsequent years; the positive effect estimated in the first year after PECC bans is not stronger, but is in fact weaker, using variation in which jobs were covered by PECC bans; and the effect is reduced to zero in job-level regressions after adding controls. We conclude that while our findings for Black job-finding are robust, the actual effect of PECC bans on Hispanic job-finding are most similar to the near-zero estimates we find for white job-seekers.

5.2 Job Separation Rates

In this section, we study how a PECC ban affects newly hired workers' rates of subsequent involuntary separation, which is a readily available measure of new hires' match quality. Our dependent variable in this section is an indicator for any subsequent involuntary separation after being newly hired. Given the rotating-panel structure of our CPS data, we observe involuntary separation for new hires at horizons ranging from 1 to 14 months, making this a short- to medium-run measure of separation. Individual observations are assigned to a time period t based on their *hire* date, and each newly hired individual only appears once in our estimating sample. Accordingly, our empirical strategy does not need to account for any dependence of involuntary separation rates on the duration of employment, and we use linear probability models estimated via OLS.

Similar to our job-finding specifications, we begin our analysis using difference-in-differences models fully interacted with race/ethnicity. The first column of Table 3, Panel A reports estimates of equation 4.1: we find a precisely estimated 2.1 percentage-point rise in involuntary separation rates for Black new hires, and a similar-in-magnitude decline in involuntary separation rates for white new hires. As before, using the Sun and Abraham (2021) estimator has little effect on the point estimates in Column (2). Column (3) shows that adding our standard, individual-level covariates X_i and state-policy and economic controls increases the estimated effect for Black new hires to 3.7 percentage points, while almost entirely eliminating the estimated impact of PECC bans on Hispanic new hires and reducing the magnitude of the estimate for white new hires.

In Columns (4) through (6) of Panel A of the table, we then take advantage of job-level variation based on whether new hires' jobs are covered by or exempted from their states' PECC bans. We estimate our baseline job-level difference-in-differences model, equation 4.4, using our current-job exposure measure $T^{C}_{j(i),s(i)}$ (i.e, the job into which new hires are newly hired). As with our earlier job-finding results, the use of job-level variation also increases the magnitude of the estimated impact on Black involuntary separation rates, which, for specifications without individual and state-policy controls, rises from 2.1 percentage points in Columns (1) to 4.1 in Column (4) and, for specifications with these controls, rises from 3.7 percentage points in Column (3) to 6.9 in Column (6). However, also note that the use of job-level variation leads to little quantitative change in the estimated coefficient for white and Hispanic new hires, and in Column (6), adding individual and state-policy controls makes the estimated impact on involuntary separation rates for Hispanic new hires substantially smaller and statistically indistinguishable from zero. Column (5) again duplicates our previous finding that implementing the Sun and Abraham (2021) estimator has little qualitative or quantitative impact on our estimates.

We now turn to Panel B, which reports the overall effect of PECC bans on involuntary separation rates. In the specifications without state policy and economic controls, Columns (1) and (4), PECC bans are estimated to have a precise $-.024$ to $-.025$ percentage point effect on involuntary separation rates; these estimates are attenuated somewhat by the inclusion of state policy and economic controls in Columns (3) and (6). We discuss these results further below.

Figure 3 shows event-study plots corresponding to our baseline estimates in column (3) of Table 3, Panel A. Starting with the event-time analysis for Black new hires, we see that involuntary separation rates for new hires fluctuated around 0 prior to the enactment of PECC bans but were on a downward trend in the year immediately preceding the ban. Black involuntary separation rates then increase immediately after PECC bans go into effect. The increase in Black new hires' involuntary separation rates stands in contrast to the patterns seen for Hispanic and white new hires in Panels B and C of the figure. Hispanic new hires exhibit a negative pre-trend. Meanwhile we see a slight but precisely estimated decrease in white new hires' involuntary separation rates in Panel C: after exhibiting parallel trends in pre-years, involuntary separation rates fall by roughly 2 percentage points in the first two years after a PECC ban's implementation, and then by an additional 1 percentage point in the fourth year. Appendix Figure 4 shows similar patterns in event-study plots using job-level variation, corresponding to column (6) of Table 3, Panel A.

To better understand our separation results for new hires, we re-estimate the four involuntary separation regressions from Table 3 on a placebo sample: long-tenure employees. The CPS does not report employment tenure, so we define this "long-tenure" sample as all individuals in our panel who are never observed as unemployed in any preceding sample month. As compared to the sample of new hires, this sample is less likely to have been hired when a PECC ban was in effect, but arguably is equally exposed to broader labor-market disruptions that could confound our results, such as plant closings and sectoral change. Appendix Table 5 reports results of these regressions. All but one of our placebo estimates for Black workers are insignificant and all have a negative sign, in contrast with the significant positive effects on Black involuntary separations in our new-hire sample. Placebo results for white and Hispanic workers are similarly negative; all three subgroups' placebo estimates are statistically indistinguishable from each other. Meanwhile when all three subgroups are pooled together, placebo estimates are significantly, modestly negative in the population overall. Overall, these placebo results further corroborate that our finding for Black separation rates is attributable to the effect of PECC bans rather than confounding labor market trends.

In summary, we see two key conclusions from our analysis of separation rates. First, there is robust evidence that PECC bans have decreased new Black hires' match quality. In addition to the evidence in our baseline regressions and event-time plots, we find that the estimated effect on Black new hires' involuntary separation rates in Table 3 is higher when we use job-level variation than when we use state-level variation. This suggests that the state-level results are indeed driven by jobs covered by PECC bans. Furthermore we find that these effects for Black new hires are not present in a placebo sample of long-tenure Black employees, who were presumably screened before PECC bans took effect.

Second, we find that there was a modest negative change in overall separation rates after PECC bans. This effect is significant for white new hires, while effects for Hispanic new hires are often statistically indistinguishable from those for white new hires. Importantly, we also find a small negative effect in the placebo sample, and we find that this effect is *not* more pronounced when using job-level variation than when using state-time variation. Accordingly, the evidence suggests that PECC bans coincided with other shifts that made it slightly more costly to fire employees in general, regardless of racial or ethnic group or whether PECC bans were in effect at the time of initial hiring. While our data are not well positioned to speak to what drives these changes, two possibilities are that PECC bans could lead to direct changes in hiring or firing costs due to costs of compliance, or that the hiring process becomes more expensive due to employers' substitution to alternative, potentially more costly, signals.

An alternative measure of match quality is wages. In Appendix Table 10, we report estimates of equation 4.1 with hourly wages for new hires as the outcome variable. The estimates are generally quantitatively small: the estimates in Column (1) using our base specifications are 0.8, -0.3, and 0.3 percent for Black, Hispanic, and white workers respectively and .2 percent for all race and ethnicity groups combined. However, the estimates are also quite imprecise, and we are unable to rule out substantively large negative or positive effects. Given this imprecision, exploration of the effect of PECC bans on wages will likely need to wait for alternative empirical approaches or datasets. Appendix Section C.9 discusses the wage results in more detail.

5.3 Additional Robustness

We explore the robustness of our job-finding and job-separation results to other identifying assumptions and modeling choices; to alternative definitions of exposure to PECC bans; to changes to the sample definition; to different outcome measures; to permutation tests using randomized treatment assignment across jobs, states, and time; and, in the case of job-finding, estimation on a supplementary dataset built from administrative data. These results include the triple-difference specification from equation 4.2: both an uncontrolled specification, and our preferred specification with the same controls as in our difference-in-differences results. Full details are presented in Appendix Section C. Figure 4 summarizes a subset of these results. Panel A of the figure summarizes how our estimates of the estimated relationship between PECC bans and job-finding vary with alternative modeling choices, controls, and datasets. Panels B and C explore the robustness of the CPS job-finding results using job-variation, and of our involuntary job-separation results. The results suggest that the relatively large effect of PECC bans on the job-finding rates of Black job-seekers is consistent across specifications, although the magnitude of the effects does vary some across specifications and datasets. Permutation tests also produce p-values consistent with our baseline estimates; for example, we find that the probability of estimating an equal or greater effect size as our actual estimate of PECC bans' effects on Black job-finding probabilities is less than 2.5% in simulations with randomly assigned treatment across states, jobs, and time.

5.4 Magnitudes

In our specification using state-time variation in the CPS data, equation 4.6, with demographic and state policy and economic shock controls we estimate that PECC bans reduced the job-finding hazard for Black job-seekers by 13 percent. To get a sense for the magnitude of these effects in absolute terms, we compare them to the baseline job-finding rates for Black job-seekers in the LEHD-J2J data, as reported in Appendix Table 2. This comparison implies our estimate translates into a 2.9 percentage point reduction in the probability of Black job-seekers finding a new job within a quarter of job-loss.

Given that PECC bans primarily affect labor demand, we gauge the magnitude of our estimated effect of PECC bans by calculating how big an increase in wages would be required to reduce Black hiring and employment the same amount. To do so, we need to convert our estimates of the effects of PECC bans on job-finding hazards to estimates of the effect of PECC bans on Black *employment rates*. We perform a back of the envelope calculation and assume that the baseline hazard was equal to the mean job-finding rate for PECC-ban states in the sample. We can then combine that figure with the mean job-separation rate for a given race or ethnicity group to compute the change in the steady-state employment rate for the given group caused by PECC bans.[19] Using this approach, we estimate that PECC bans reduced the steady-state Black employment rate by 1.2 percentage points in states banning PECCs. Combining these calculations with the elasticity of labor demand estimate of $-.246$ from Lichter et al. (2014)'s meta-analysis, the effects of PECC bans are equivalent to the employment declines resulting from a 4.8 percent increase in wages.[20]

5.5 Mechanisms: Noise in PECCs and Other Screening Tools

This section estimates a quantitative version of the theoretical model introduced in Section 2 in order to explore mechanisms behind our empirical results above. We examine several key determinants of PECC bans' effects: the precision of PECCs as a screening tool and the precision of other available screening tools such as job interviews and referrals, both for minority and for white job-seekers. Intuitively, to identify these precisions, this section asks what amount of noise in PECCs and in other, non-PECC screening tools would be consistent with the changes we observe in both job-finding rates and involuntary separation rates after PECC bans. We also assess whether allowing for other differences across signals and groups, such as biases in signal realizations or in employers' priors, would substantially affect our estimates of precision differences.

The quantitative model follows the same setup as in Section 2.1: each group g has normally distributed, unobservable match qualities with mean $\mu_{g,0}$ and a common inverse variance h_0; non-PECC screening tools provide unbiased signals of these match qualities with normally distributed noise that has inverse variance $h_{g,\text{baseline}}$; PECC signals likewise have normally distributed, mean-

[19] The steady-state employment rate is equal to $lf_r \times \frac{f_r}{f_r+s_r}$ where f_r is the job-finding rate for group r, s_r is the job-separation rate for group r, and lf_r is the labor-force participation rate for group r.

[20] We prefer this approach rather than directly estimating the effect on employment levels because it takes time for employment levels to converge to their steady state equilibrium value after a change in job-finding and separation rates. In Appendix Section C.2 we discuss Appendix Table 4, which reports estimates of the effect of PECC bans on employment levels. The qualitative patterns are similar to the estimates of the new steady state equilibrium, but magnitudes are smaller, consistent with incomplete convergence to the new steady state.

zero noise with inverse variance $h_{g,\text{banned}}$. Meanwhile, risk-neutral firms seek to fill an exogenous number of positions $M \in [0,1]$ and hire the M job applicants that have the highest expected match qualities based on their signal realizations. After the hiring decision, firms learn the true match quality of all new hires; firms then fire any of the new hires whose match quality is lower than the expected match quality of making another new hire (from any group), less some firing cost c. Given our evidence that firing rates change after PECC bans even for long-tenure employees whose screening was not affected by the ban (see Section 5.2), we also allow a PECC ban to coincide with a change in firing cost c, perhaps related to regulatory compliance costs involved in hiring a fired worker's replacement. Appendix Section B gives a formal treatment of model parameters, the firm's problem, and the hiring and involuntary separation process.

We estimate these terms for the same three groups from our empirical results: Black, Hispanic, and white. Besides the dispersion in match qualities h_0, there are three terms to identify per group, plus the pre- and post-ban firing costs c_{pre} and c_{post}. We normalize one of the μ_0^r terms to zero. This leaves eleven parameters to be estimated:

$$\theta = \begin{bmatrix} \mu_{B,0} & h_{B,\text{baseline}} & h_{B,\text{banned}} & h_0 \\ \mu_{H,0} & h_{H,\text{baseline}} & h_{H,\text{banned}} & c_{\text{post}} \\ \cdot & h_{W,\text{baseline}} & h_{W,\text{banned}} & c_{\text{pre}} \end{bmatrix} \tag{5.1}$$

We identify these eleven parameters by simulating model moments and matching these to corresponding moments from our empirical work: hiring and firing decisions for each of the three groups, both with and without PECCs. Specifically, for the "with PECCs" case we match the empirical moments in the first two rows of each panel in Appendix Table 2, column (1). For the "without PECCs" case we use estimates of PECC bans' effects on job-finding rates and new hires' involuntary separation rates in Panel A of Tables 2 and 3. Further details on the model simulations are presented in Appendix Section B, where we also explore robustness to using other estimates of PECC bans' effects, using job-level variation and additional controls.

Figure 5 presents estimates of some key parameters for each group: PECCs' signal precisions

$h_{g,\text{banned}}$ and the precision of non-PECC screening tools $h_{g,\text{baseline}}$. We find that the precision of non-PECC screening tools differs sharply across groups. For example in the top panel, which shows parameters identified off our state-level regression estimates with controls, these precisions range from 0.52 for Black job-seekers to 1.38 for white job-seekers.[21] Meanwhile, the precision of PECCs as a screening tool is closer to equal for white and Black job-seekers – equal to 1.04 for white job-seekers and 1.02 for Black job-seekers. Interestingly, the precision of PECCs for Hispanic job-seekers is markedly lower, equal to 0.71, which may relate to the elevated share of Hispanic adults with thin or no credit files that has been noted elsewhere (Brevoort et al. (2015)). The bottom panel of the figure shows parameters identified off our job-level regressions with controls; these estimates are broadly similar, with Black and white PECC precisions again particularly close to each other, at 1.29 and 1.21 respectively.

These parameter estimates illustrate one of the main points of our earlier theoretical work, that a group – in this case, Black job-seekers – can benefit from a new information source such as PECCs even if that group does not have an absolute advantage vis-a-vis that new information. We estimate that the availability of PECCs improves Black labor market outcomes not because PECCs provide more precise signals about match qualities for Black job-seekers than other groups, but because other, non-PECC screening tools contain relatively more noise for Black job-seekers.

In Appendix Section B, we also briefly explore robustness of these empirical estimates to allowing for signal biases across groups, in the sense of Autor and Scarborough (2008). We find that our core results on relative advantage in PECC signal precision are unchanged, and in fact are modestly strengthened by, allowing for biases in signals – suggesting that PECC bans are a prominent empirical setting where relative advantage is determined more by signal precision than by signal bias. As discussed in Section 2.1, our estimates of relative advantage are also robust to the presence of taste-based discrimination, related racial animus or biases, and also structural dis-

[21] The units on these precision estimates are in terms of the inverse variance of the distribution of match qualities, relative to a standard normal distribution. Because we do not translate our estimates of match qualities into economic terms such as dollars of marginal product, these estimates are best understood in relative terms to each other, for example comparing the precision of posteriors to the precision of PECCs signals.

crimination that affects human capital accumulation, as all of these are captured in the $\mu_{r,0}$ terms that measure mean differences in employers' perceived match quality. Overall, we conclude our results on the importance of relative advantage in PECC signal precision are generally robust to considering potential biases among employers and in labor market signals.

We conduct some basic counterfactual exercises to help illustrate our model estimates and their usefulness. Table 4 presents these counterfactuals across three panels, where each panel shows predicted job-finding and involuntary separation rates for each of the three groups, in the two cases where PECCs are and are not available. For reference, Panel A shows the case with our baseline parameters and no counterfactual, using the parameter values reported in the top half of Figure 5. Panel B shows job-finding and separation rates when we counterfactually equalize the precision of PECCs across the three groups, by changing the precision of PECCs for Black and Hispanic job-seekers so that it equals PECCs' precision for white job-seekers. Panel C, in contrast, has the same PECC precisions as in the baseline case, but here we counterfactually set the precision of baseline signals for Black and Hispanic job-seekers to equal that of white job-seekers.

As can be seen across the three panels, equalizing the precision of PECCs (Panel B) does little to reverse the patterns seen in the baseline case: the availability of PECCs still substantially improves labor market outcomes for Black job-seekers, while other groups are affected less. Meanwhile, equalizing baseline signal precisions across the three groups while leaving the precision of PECCs unchanged (Panel C) markedly changes these patterns. Reflecting how our estimates of PECCs' precision are relatively close to equal across all three groups, no group experiences a pronounced change in its job-finding rates when PECCs are made available.

6 Conclusions

We study "banning a signal," or removing access to an information source in a market. We characterize the effects of information removal theoretically using a novel measure of relative information advantage, and we apply these results empirically to the use of credit report data in labor markets.

Our main theoretical result emphasizes that relative advantage in information determines incidence across groups, and relative advantage depends on that information's precision scaled appropriately by the precision of other available information sources. Hence a group can benefit on average from an information source being available even if the information appears to disfavor that group in absolute terms – for example because of lower average realizations or greater noise.

We use these findings in an empirical study of bans on pre-employment credit checks, or PECCs. Using state-, time-, and job-level variation in PECC bans' coverage, we provide the first evidence of PECC bans' adverse effects on Black job-seekers' hiring rates, and the first evidence of PECC bans' effects on match quality among new hires. We then illustrate how reduced-form findings such as these identify differences in the precision of various information sources across groups. We find that PECC bans hurt Black job-seekers not because PECC signals are especially precise for Black job-seekers, but rather because non-PECC screening tools, such as referrals or job interviews, provide noisy signals about match quality for Black relative to white job-seekers. Concretely we estimate that the standard deviation of noise in non-PECC screening tools is 70% higher for Black job-seekers than for white job-seekers. This could be due to a number of reasons, such as segregation in American society (Logan and Parman (2017), Boddie and Parker (2018)).

Our model and estimates suggest two broad conclusions about the relationship between information and inequality. First, in the context of labor markets, our results suggest that introducing new screening tools can improve Black labor market outcomes, even if those screening tools are less precise for or generally have low realizations for Black job-seekers; this emerges from a general result that new signals will tend to benefit groups for whom baseline screening tools are particularly imprecise, as well as groups that face biases such as taste-based discrimination or racial animus. Second, research and policy should work to understand and remedy the institutional and social factors underlying inequality in screening tools in a variety of markets, for example in finance as well as labor, where the sources of and characteristics of information used by decision-makers play an important role in determining economic outcomes.

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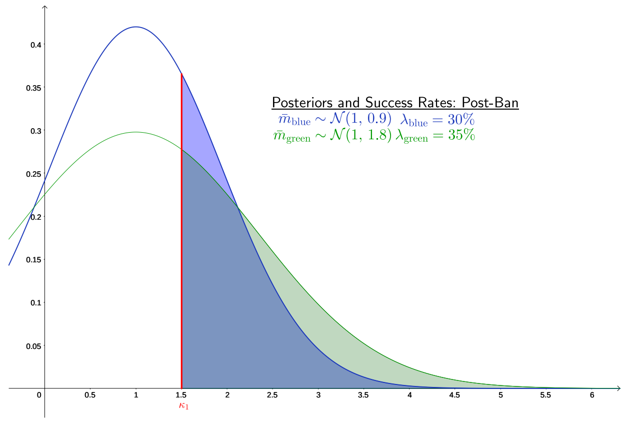
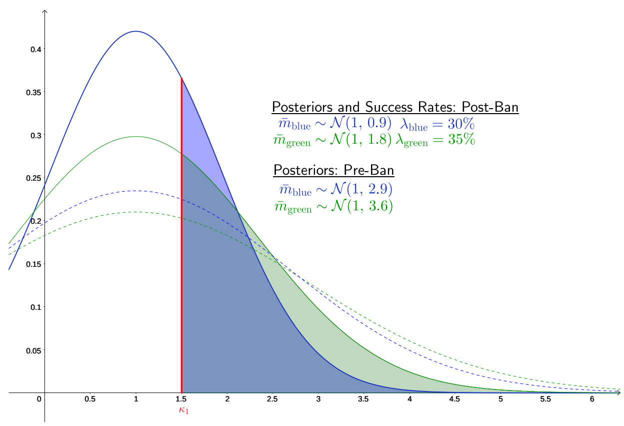
7 Figures

Figure 1: Illustrating the Effects of a Signal Ban

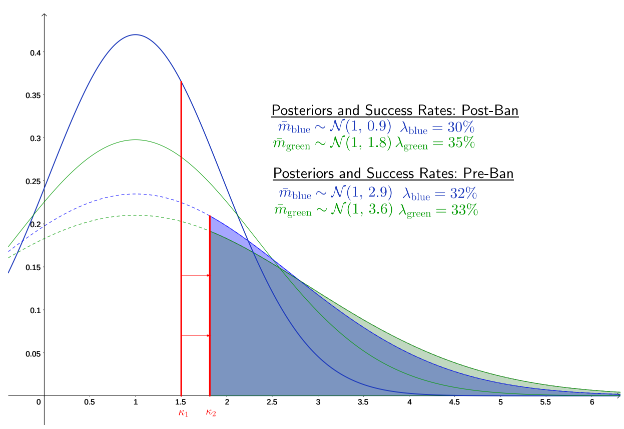
(a) Success Rates under a Signal Ban



(b) Change in Receivers' Posteriors when Banned Signal is Available



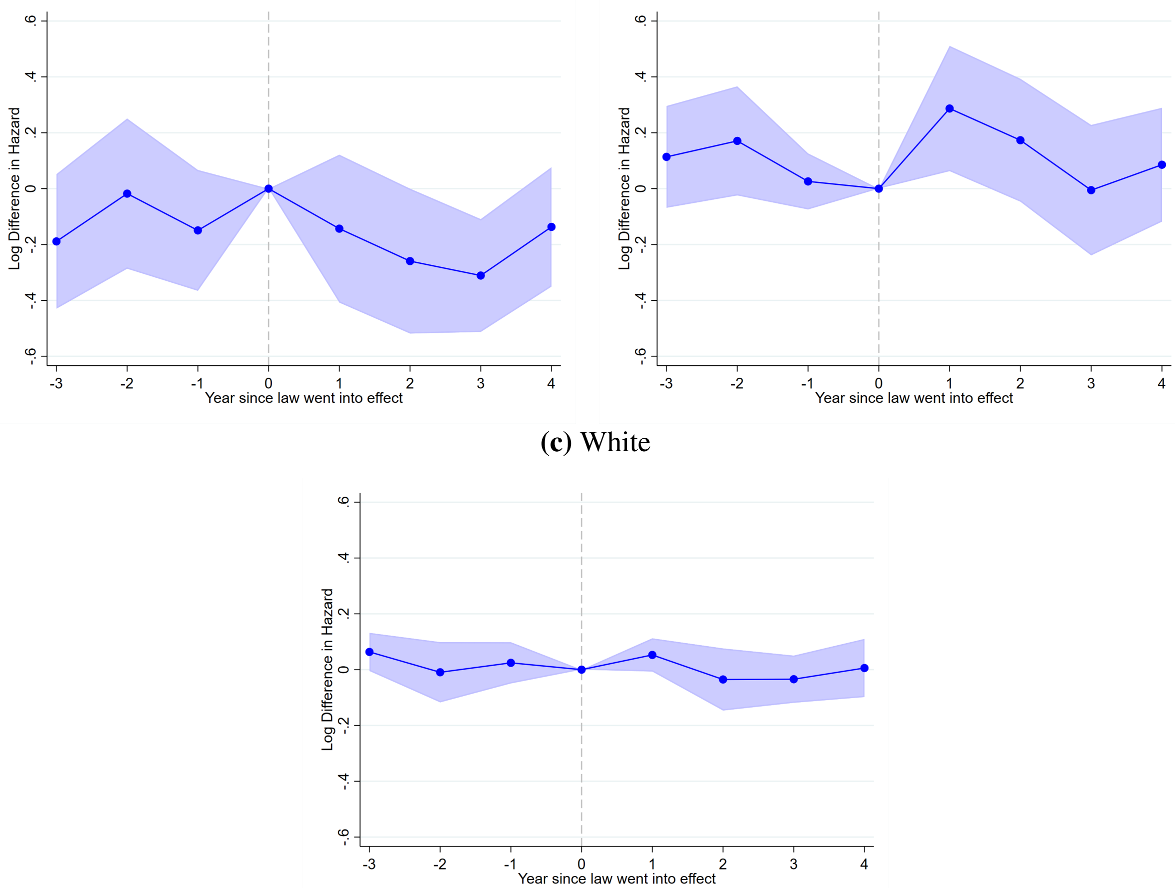
(c) Change in Success Rates when Banned Signal is Available



Notes: This figure graphically illustrates the model developed in Section 2.1. We apply the model to the parameter values reported in the figure for two groups of senders: "blue" and "green," who have identically distributed match qualities but who differ in how precisely they are screened by available information sources. For more details see Section 2.1.

Figure 2: Event-Time Analysis of the Effect of PECC Bans on Job-Finding: CPS
State-Race/Ethnicity FE, Time-Race/Ethnicity FE

(a) Black **(b)** Hispanic **(c)** White

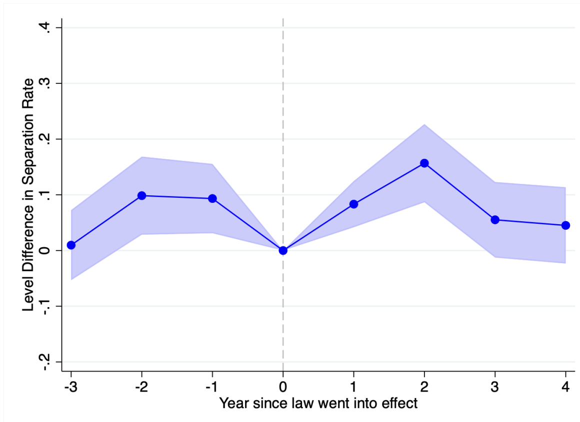


Notes: This figure shows the results of an event-time analysis of the difference in job-finding for unemployed individuals between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. For details see Section 5.1.

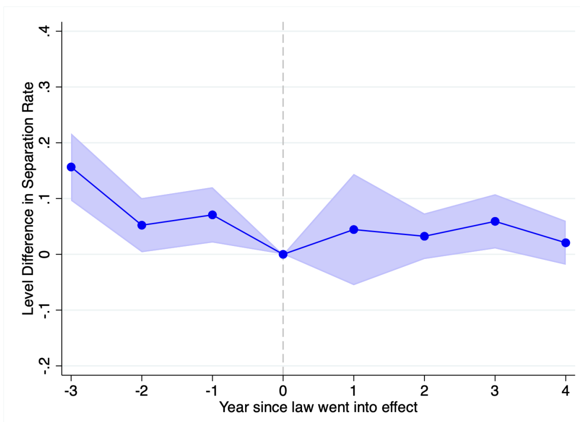
Figure 3: Event-Time Analysis of the Effect of PECC Bans on Involuntary Separations: New Hires

State-Race/Ethnicity FE, Time-Race/Ethnicity FE

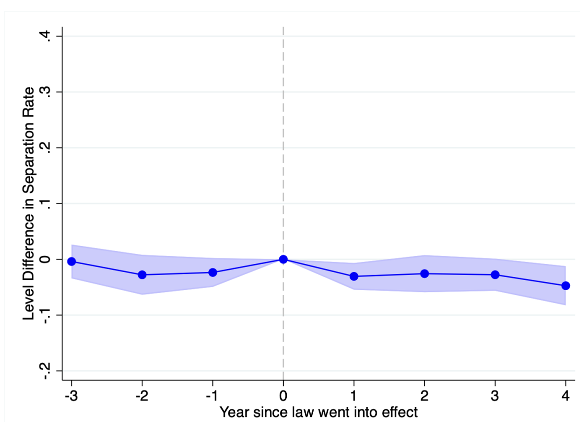
(a) Black



(b) Hispanic



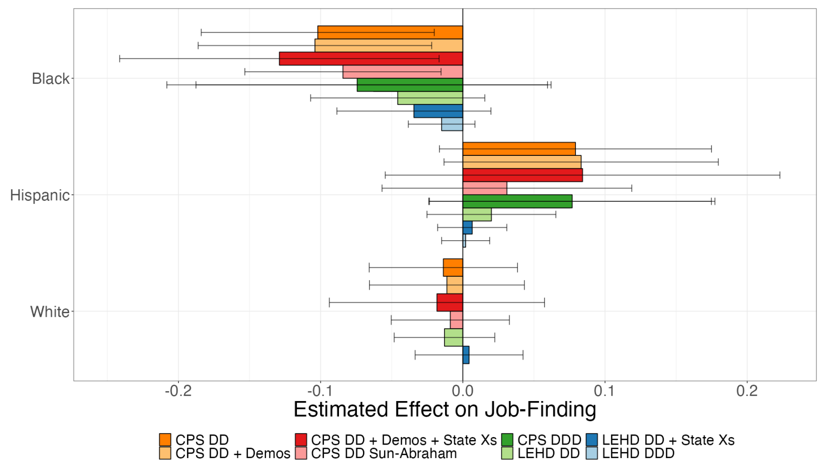
(c) White



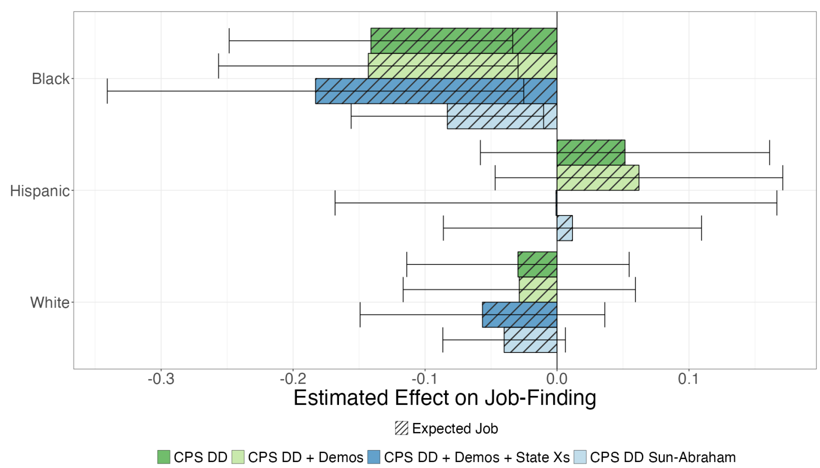
Notes: This figure shows the results of an event-time analysis of the difference in involuntary separation rates for workers newly hired out of unemployment between states banning and not banning PECCs before and after the PECC bans went into effect. For details see Section 5.2.

Figure 4: Robustness of Estimated Effects of PECC Bans

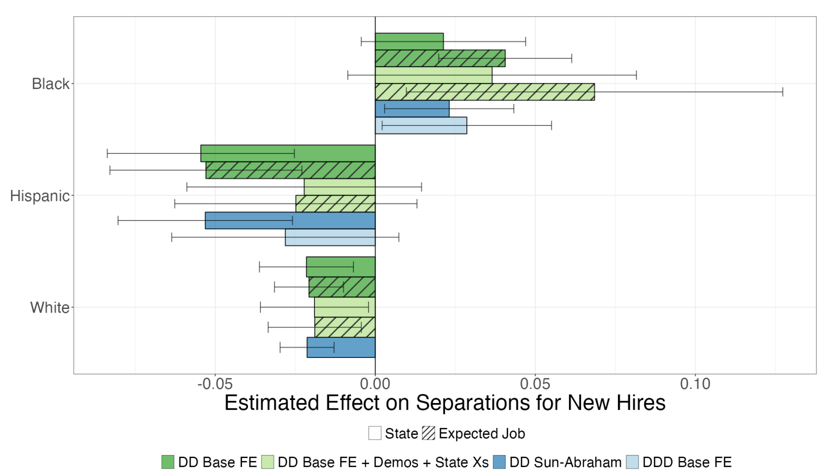
(a) Job-Finding using State-Level Variation



(b) Job-Finding using Job-Level Variation

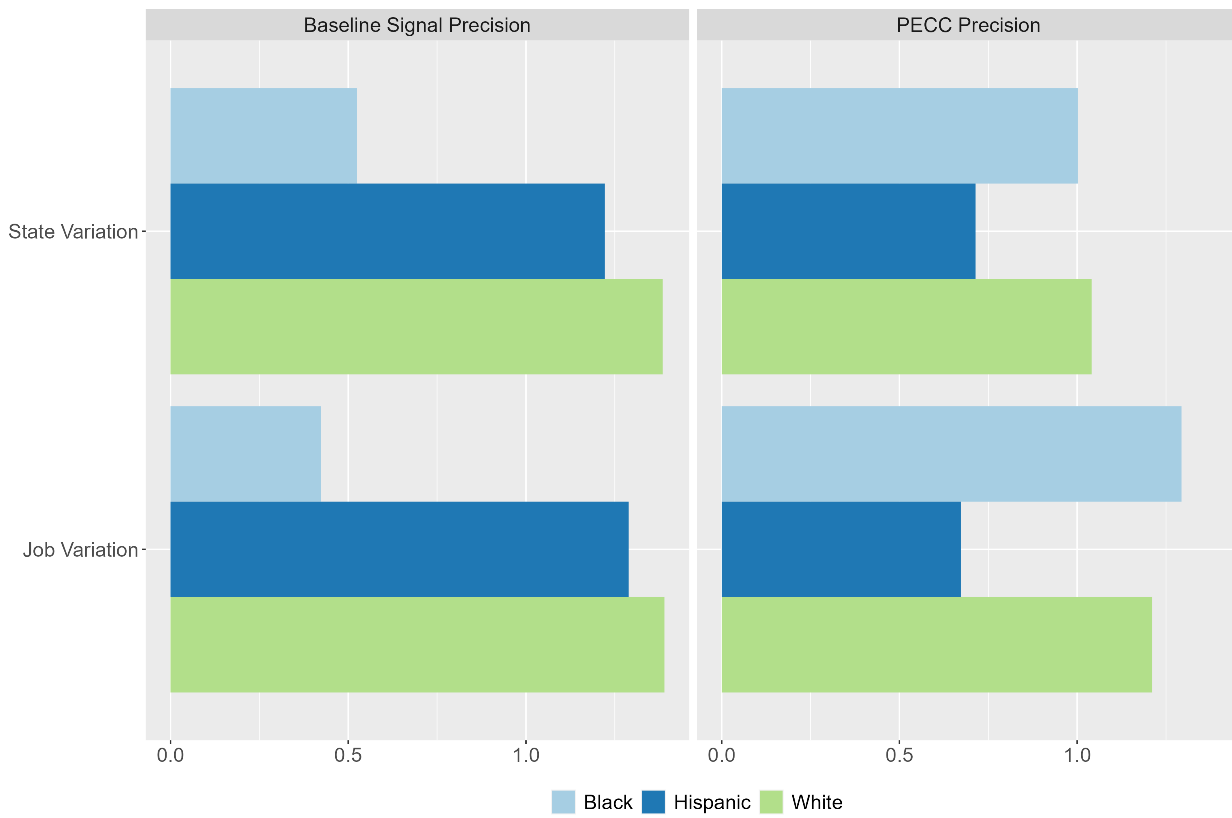


(c) Involuntary Job Separations



Notes: This figure reports the robustness of our estimates of the effect of bans of PECCs to alternative modeling choices and data. Error bars show 95 percent confidence intervals generated from standard errors clustered at the state level.

Figure 5: PECC Precision and Baseline Signal Precision



Notes: This figure shows estimates of model parameters h_ε^r (the precision of PECC signals for group r) and h_s^r (the precision of baseline screening tools for group r), as described in Section 5.5.

8 Tables

Table 1: Characteristics of PECC and non-PECC Ban States and Jobs

	PECC-Ban States	Non-PECC-Ban states	Covered Jobs (Within PECC-ban states)	Exempted Jobs
	(1)	(2)	(3)	(4)
Panel A. Labor Market Characteristics by Race/Ethnicity				
Panel A1. Blacks				
Black Share of State Adult Population	9%	14%		
Black Employment Rate	87%	90%		
Share of Black Unemployed Covered by Ban	68%	0%		
Average Weekly Wage ($)	$776	$923	$664	$923
Share of Workers with 4-Year College Degree	31%	24%	21%	43%
Panel A2. Hispanics				
Hisp. Share of State Adult Population	21%	11%		
Hisp. Employment Rate	90%	93%		
Share of Hisp. Unemployed Covered by Ban	79%	0%		
Average Weekly Wage ($)	$645	$634	$571	$847
Share of Workers with 4-year College Degree	14%	17%	9%	26%
Panel A1. Whites				
White Share of State Adult Population	70%	75%		
White Employment Rate	94%	95%		
Share of White Unemployed Covered by Ban	65%	0%		
Average Weekly Wage ($)	$990	$866	$850	$1,158
Share of Workers with 4-Year College Degree	45%	37%	36%	56%
Panel B. State economic and policy variables				
Saiz Housing Supply Elasticity	1.367	2.367		
Share of manufacturing jobs	12%	14%		
Maximum total unemployment benefit (thousands of $)	$22	$17		
Share of states with fracking activity	10%	22%		
Share of states passing "Ban-the-Box" policies	80%	59%		
Share of states expanding Medicaid under the ACA	80%	54%		

Notes: This table shows how the characteristics of workers, state economic conditions, and policy vary between-PECC banning and non-PECC-banning states, and between jobs covered by PECC bans and not covered by PECC bans. Panel A reports summary statistics by race or ethnicity from the CPS for years 2003 to 2018. Panel B reports average economic characteristics and state-policy variables. Columns (1) and (2) respectively show statistics for PECC-ban states and non-ban states. Columns (3) and (4) then compare covered vs. exempted jobs within PECC-ban states. The share of unemployed workers covered by a PECC ban is determined by whether an unemployed worker's most recent job was covered by or exempted from her home state's PECC ban.

Table 2: Impact of PECC Bans on Job-Finding: State and Job-Level Variation

	(1)	(2)	(3)	(4)
Panel A. State-level Variation				
Panel A1. Effect separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.102**	-0.0843**	`-0.105**	`-0.129**
	-0.0418	(0.0352)	(0.0419)	(0.0573)
1(Hispanic)*1(Treated by Ban)	0.0792	0.0310	0.0832*	0.0842
	(0.0488)	(0.0448)	(0.0492)	(0.0708)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0137	-0.0088	-0.0112	-0.0182
	(0.0266)	(0.0212)	(0.0278)	(0.0386)
Panel A2. Overall Effect				
1(Treated by Ban)	-0.000860	-0.0124	0.00111	-0.0174
	(0.0270)	(0.0170)	(0.0288)	(0.0362)
N	343,262	6,535	343,262	343,262
States	51	51	51	51
Ban States	10	10	10	10
Panel B: Job-Level Variation				
Panel B1. Effects separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.141***	-0.0831**	-0.143**	-0.183**
	(0.0548)	(0.0372)	(0.0579)	(0.0805)
1(Hispanic)*1(Treated by Ban)	0.0515	0.0117	0.0621	-0.000790
	(0.0559)	(0.0499)	(0.0556)	(0.0854)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0296	-0.0401*	-0.0286	-0.0565
	(0.0430)	(0.0237)	(0.0449)	(0.0473)
Panel B2. Overall Effect				
1(Treated by Ban)	-0.0198	-0.0339	-0.0164	-0.0664
	(0.0402)	(0.0208)	(0.0438)	(0.0413)
N	331,942	6,535	331,942	331,942
States	50	50	50	50
Ban States	9	9	9	9
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	N	Y	Y
State Policy/Economic Controls (-Race/Ethnicity)	N	N	N	Y
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
Aggregate Level Regression	N	Y	N	N
Sun & Abraham (2021)	N	Y	N	N

Notes: This table reports estimates of race/ethnicity-specific log differences in job-finding hazard rates following a PECC ban using both a state-time difference-in-differences strategy and a state-job-time difference-in-differences strategy. Data are from the CPS for years 2003 to 2018. Column (1) reports MLE estimates of hazard models that include the state-race/ethnicity and time-race/ethnicity fixed effects that implement difference-in-differences. Column (2) presents estimates models using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. Column (3) adds demographic controls fully interacted with race or ethnicity group to the specification in Column (1). Column (4) adds controls for state economic and policy variables. Standard errors clustered at the state level are shown in parentheses. For more details see Section 5.1

Table 3: Impact of PECC Bans on Involuntary Separation Rates for New Hires

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Effect Separately by Race/Ethnicity						
1(Black)*1(Treated by Ban)	0.0213	0.0231**	0.0365	0.0406**	0.0419***	0.0685**
	(0.0131)	(0.0103)	(0.0230)	(0.0160)	(0.0139)	(0.0300)
1(Hispanic)*1(Treated by Ban)	-0.0545***	-0.0531***	-0.0222	-0.0545***	-0.0529***	-0.0248
	(0.0149)	(0.0139)	(0.0187)	(0.0168)	(0.0153)	(0.0193)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0215***	-0.0213***	-0.0190**	-0.0207***	-0.0207***	-0.0189**
	(0.00749)	(0.0043)	(0.00862)	(0.00630)	(0.0055)	(0.00742)
Panel B. Overall Effect						
1(Treated by Ban)	-0.0246***	-0.0243***	-0.0112	-0.0236***	-0.0231***	-0.00709
	(0.00699)	(0.0063)	(0.00939)	(0.00751)	(0.0074)	(0.00749)
N	54,389	54389	54,389	52,407	52407	52,407
States	51	51	51	50	50	50
Ban States	10	10	10	9	9	9
Treatment Level	State	State	State	New Job	New Job	New Job
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	N	N	N
State-New Job-Race/Ethnicity Fixed Effects	N	N	N	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	N	Y	N	N	Y
State Policy/Economic controls (-Race/Ethnicity)	N	N	Y	N	N	Y
Sun & Abraham (2021)	N	Y	N	N	Y	N

Notes: This table reports linear probability model estimates of (race/ethnicity-specific) differences in separation rates for newly hired workers following a PECC ban, using various difference-in-differences strategies. Columns (1) through (3) use state-time difference-in-differences, while Columns (4) through (6) use state-job-time difference-in-differences. Data are from the CPS for years 2003 to 2018. Columns (1), (3), (4), and (6) include the state-(job)(-race/ethnicity) and time-race/ethnicity fixed effects that implement difference-in-differences, while Columns (3) and (6) add demographic controls (fully interacted with race or ethnic group), which include binned education, binned age, gender, and marital status, urbanicity, and interactions between month-of-year and Census division, and a set of state-year policy and economic controls. Columns (2) and (5) report results using Sun and Abraham (2021)'s interaction-weighted difference-in-differences estimator. The controls for state economic and policy variables are described in Section 5.1. Standard errors clustered at the state level are shown in parentheses. All controls (individual and state policy/economic) are interacted by race-ethnicity dummies.

Table 4: Counterfactual Effects of PECC Bans under Alternative Signal Precisions

	Black		Hispanic		Non-Hispanic White	
	Hiring Rate	Firing Rate	Hiring Rate	Firing Rate	Hiring Rate	Firing Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Case / No Counterfactual						
With PECCs	0.1421	0.1129	0.1825	0.0917	0.1473	0.0797
After PECCs Ban	0.106	0.1352	0.1952	0.0762	0.1522	0.0718
Panel B: No Heterogeneity in PECCs Precision						
With PECCs	0.1424	0.1116	0.1874	0.081	0.1465	0.0803
After PECCs Ban	0.106	0.1352	0.1952	0.0762	0.1522	0.0718
Panel C: No Heterogeneity in Baseline Precision						
With PECCs	0.1573	0.0811	0.1823	0.0875	0.1445	0.0812
After PECCs Ban	0.1579	0.0732	0.1907	0.0709	0.1432	0.0744

Notes: This table shows simulated job-finding rates and involuntary separation rates in the quantitative model of Section 5.5 under various counterfactual parameter values. Panel A shows the baseline case with no counterfactual. Panel B counterfactually sets PECCs' precision for all groups equal to our estimate of PECCs' precision for white job-seekers. Panel C does not vary PECCs' precision from the baseline case but instead sets baseline signal precisions to be equal across groups.