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 70% higher standard deviation of signal noise for Black relative to white job-seekers.

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1 Introduction

Markets with information asymmetries are in the midst of an information boom: in labor markets, employers now check job-seekers' Facebook friends, not just résumés; in insurance markets, health insurers collect data from patients' home medical devices, not just from doctor visits; in credit markets, lenders evaluate borrowers' SAT scores, not just past loan repayment.[1] With this boom have come contentious debates. Should policy restrict access to some of this new information, and on what grounds? When are different individuals helped or hurt by such restrictions?

In this paper we provide theoretical and empirical results to address such questions. Theoretically, we characterize which groups of individuals gain or lose from information restrictions based on a novel measure of relative advantage in information. Relative advantage, we show, emerges as a simple and intuitive ratio between various information sources' precisions as signals of unobserved quality, and the group with the lowest (resp. highest) relative advantage benefits from an information source being banned (resp. available).

Empirically, we illustrate this general result in a recent setting that has both policy relevance and rich variation to study – the use of credit report data in labor markets. We evaluate how recent restrictions on such data have achieved one of their primary stated goals, to protect the labor market opportunities of minority job-seekers.

Our theoretical work provides guidance on the potential impact of such restrictions. We show that a group benefits from banning an information source when that source is less informative for that group relative to the precision of other available signals (for example, job referrals or interviews), where a simple ratio of signal precisions determines the correct sense of relative advantage. In contrast, neither average realizations of a given information source (for example, low credit scores) nor absolute measures of signal noise (for example, the frequency of credit report errors) is sufficient or necessary for determining incidence across groups. Hence in the case of credit check bans, minority job-seekers will not necessarily benefit on average from recent restrictions even though minorities are disproportionately likely to have low credit scores,[2] and even though minorities may have moderately noisier credit report information.[3] Rather, incidence hinges on the informativeness of credit report data relative to other existing screening tools.

These results may help explain known empirical patterns in a range of settings, such as restrictions on criminal record information or drug testing in labor markets (Agan and Starr (2018), Wozniak (2015)), or delinquency history in credit markets (Liberman et al. (2018)). Our application to credit check bans in particular is motivated by the prevalence of such credit checks and by the rich variation available in recent regulations. Pre-employment credit checks, or 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]

[1]See Allen (2018), Jayakumar (2019), and Hughes (2013) discussing these particular examples.

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

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

[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 a series of PECC-related Civil Rights Act suits against employers, asserting that PECCs

policy-makers have now restricted PECCs in eleven states, New York City, and Chicago since 2007. These restrictions include varied exemptions for certain occupations and industries across different states, providing us with rich variation to study.

We estimate that PECC bans have sizable, negative effects on labor market outcomes for Black job-seekers. Our estimates suggest that Black job-finding hazards declined by 14 percent after a PECC ban, while new Black hires became 4.4 percentage points more likely to experience involuntary separation shortly after being hired. Population average job-finding and white job-finding show little change after PECC bans, and our estimates for Hispanic job-seekers are usually statistically indistinguishable from those for white job-seekers. We then estimate an empirical version of our model to explain these results and conclude that differences in the incidence of PECC bans across groups result mainly from differences in the precision of *non*-PECC information. Concretely, the standard deviation of noise in non-PECC tools such as job interviews and referrals is 70% higher for Black job-seekers than for white job-seekers. We believe these are the first such quantitative estimates of divergent signal noise across race and ethnic groups for standard job-screening tools. These estimates are consistent with institutional discrimination making it harder for employers to screen Black job-seekers than white job-seekers, and likewise the estimates provide a salient example of the importance of assessing relative advantage in information when seeking to understand the incidence of regulating access to information.

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, exploiting temporal and geographic variation in recent PECC bans. We find that PECC bans decreased Black job-finding hazards on average by 13.5 percent. We then corroborate our results using rich job-level variation in which occupations and industries are covered by or exempted from PECC bans; here our estimate grows in magnitude, to 19.4 percent, an increase that is close to what would be predicted by the share of Black workers actually covered by rather than exempted from PECC bans. Results from demanding triple-difference models and tests of our estimators' various parallel-trends assumptions further corroborate these results on job-finding.

We also investigate the effects of PECC bans on new hires' match quality, as revealed by involuntary separation rates shortly after hiring. Consistent with PECCs being informative for Black job-seekers, we find that the share of new Black hires involuntarily separating from their jobs increases by 4.4 percentage points after a PECC ban.

We use our theoretical results to interpret these empirical patterns. We match our reduced-form moments of hiring and involuntary separation rates by group with and without PECCs to corresponding moments in a quantitative version of our theoretical model, in order to estimate model parameters that characterize signals across groups. We find that *non*-PECC screening tools are roughly three times as precise (in terms of the inverse variance of noise) for white as for Black job-seekers, with Hispanic job-seekers falling somewhere in between. Conversely, the precision of the PECC signal is similar for white and Black job-seekers. Combining these two findings, we see that PECC bans hurt Black labor market outcomes on average not because

"tend[s] to impact more adversely on females and minorities" (Crawford (2010)).

PECCs are particularly precise signals for Black job-seekers, but because the signals that employers receive at baseline are substantially less precise for Black than for white job-seekers. These results are consistent with evidence on the importance of social networks and referrals in job search (Neckerman and Kirschenman (1991), Bayer et al. (2008), Hellerstein et al. (2011)), which may provide fewer or less precise signals for Black workers in particular (Braddock II and McPartland (1987), Waldinger (1997), Smith (2005)), interpersonal or cultural barriers to interpreting interview information for minority applicants (Lang (1986)), or a hiring manager's lower ability to screen job-seekers from races and ethnicities other than their own (Giuliano et al. (2009), Benson et al. (2019)).

We also briefly investigate PECC bans' effects across other dimensions of heterogeneity, focusing on groups for which non-PECC screening tools likely differ, such as groups with 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, whereas any negative effects are negligible for Black job-seekers with higher educational attainment. Assuming that education helps provide informative signals of match quality, this result provides further evidence consistent with our theoretical results that PECC bans will tend to hurt groups for whom employers receive relatively imprecise signals in the absence of PECCs.

Our paper makes three contributions to the literature. First, we show theoretically how a ban on an information source has differential effects that depend on an (appropriately scaled) measure of that information's signal precision. We view our theoretical result as relevant in a range of information-intensive markets, such as in labor and finance, where regulation increasingly contemplates bans of various information sources. Whereas some models of statistical discrimination have emphasized group differences in average match quality, signal precision, or both (e.g., Autor and Scarborough (2008), Phelps (1972), Aigner and Cain (1977)), we believe our characterization of how relative advantage in signal precision affects the incidence of banning a signal across groups is a new and broadly applicable result in this literature.

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. While these studies find that PECC bans lead to improved labor market outcomes for some subgroups, our study instead emphasizes that not all groups with poor average credit necessarily benefit from PECC bans, and that these bans' average effects depend on the relative informativeness of PECCs as a signal compared to alternative signals across groups.[5]

[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

Third, we use our model quantitatively to shed light on how existing screening tools differ in precision across different groups, and how the availability of new information sources such as PECCs can therefore help equalize different groups' outcomes. Our estimates imply that PECC bans worsen labor market outcomes for Black job-seekers because, without PECC bans, employers have less precise estimates of Black applicants' match quality than for white or Hispanic applicants. In so doing, we potentially provide a unifying explanation for existing results from a range of contexts, including drug testing and criminal record checks, where recent information restrictions have been found to adversely affect minority groups (Agan and Starr (2018), Wozniak (2015), Doleac and Hansen (2020)). This finding also provides support for the idea that employers have noisier information about Black job-seekers than other groups, a key-underpinning of many statistical discrimination models (Aigner and Cain (1977), Cornell and Welch (1996), Morgan and Vardy (2009), and Bjerk (2008)). Our results can 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)) 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 current 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.

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 the data used in our empirical work. In Section 4, we flesh out our empirical strategy for estimating the effect of PECC bans. We present our results in Section 5, where we also apply our theoretical results to our empirical estimates in order to illustrate mechanisms and assess the precision of labor market screening tools. We provide some interpretation of our results in Section 6, and then Section 7 concludes.

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.

[6]Recent research has explored a wide variety of questions regarding 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)). In complementary work, Liberman et al. (2018) study the effects of removing credit history information on credit market outcomes in Chile. More broadly, our work relates to the extensive literatures on affirmative action (see Leonard (1984), Leonard (1990), Coate and Loury (1993), Holzer and Neumark (2000a), Holzer and Neumark (2000b), and Miller (2017)), statistical discrimination and employer learning (Phelps (1972), Aigner and Cain (1977), Lundberg and Startz (1983), and Altonji and Pierret (2001)), and racial wage-gaps (Bayer and Charles (2018)).

2 Conceptual Framework

This section analyzes theoretically the effects of banning an information source. We focus on markets with information asymmetries where agents on one side of the market – for example, job-seekers or loan applicants – compete to signal the other side of the market 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.[7] In such settings, we show that the effect of banning an information source depends on a measure of relative advantage in information across groups. We begin with a simple model that helps make the key role of relative advantage intuitive. We then derive our measure of relative advantage in a richer setting where the receiving side of the market faces a signal extraction problem across multiple information sources, and where groups of applicants may face biases or discrimination of various kinds, and we show our measure has a straightforward closed form in this more general setting. Relative to existing work, most prominently Autor and Scarborough (2008), we focus on a setting where there may be differences in the precision of signals for different groups. Finally, we discuss an empirical strategy for estimating the more general model empirically, which we later implement in Section 5.5.

2.1 Building Intuition: A Simple Model

To help build intuition, we illustrate our key mechanism in a stylized model of hiring. The stylized model features three key mechanisms that will also play a primary role in our more general model. First, adding an information source or increasing the precision of an existing information source – that is, a refinement of the available signal technologies – will spread out, or add variance to, the expectations of an employer about its applicants. Second, such an increase in variance is good for any group in partial equilibrium, so long as applicants benefit from conveying any information at all – that is, so long as an applicant who sends no information does not get hired. However, and third, in general equilibrium the threshold for getting hired is endogenous, so the effects of adding or subtracting signals depends on a sense of *relative* advantage over other groups in the market.

To see these three points, in this subsection's simple model we suppose an employer's job applicants, indexed by i, are drawn from two equal-sized groups, g and g', and both groups have

[7] 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, particularly knowledge industry 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. Specific to our setting, we 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 primarily used during wage-bargaining rather than screening. See Bessen and Denk (2020) and Hansen and McNichols (2020) for empirical evidence on bans of one such information source, salary histories.

a uniform distribution of potential match qualities μ on the unit interval,

$$\mu_{i,g} \sim U(0,1) \tag{2.1}$$

$$\mu_{i,g'} \sim U(0,1) \tag{2.2}$$

An employer is seeking to hire some share α of the applicants, and it hires those with the highest expected match quality. As discussed above in footnote 7, we assume that wages are fixed, so that firms do not prefer to hire low-quality matches at lower wages. To reveal their match quality, applicants have access to two signals indexed by k. These signals work simply: for an applicant from group g, signal k has a $p_{g,k}$ probability of perfectly revealing the applicant's type and a $1 - p_{g,k}$ probability of conveying no information at all. While we do not view this "all-or-nothing" signal technology as realistic, it is helpful in this simple model for illustrating the role of relative advantage in a straightforward way.

We are interested in how the two groups' outcomes – in this case hiring rates – change when one of the signals is banned. Suppose the banned signal, indexed by $k = 2$, has a higher probability of successfully conveying information for group g' than for group g, that is, $p_{g',2} > p_{g,2}$. Despite group g''s apparent advantage with this signal in absolute terms, we can show in this simple setup that the change in both groups' hiring rates depends on how the other available signal differs across groups.

To see this, note that the hiring rate for group g after a ban is,

$$\lambda_{g,1} = \underbrace{\left(\frac{2\alpha}{p_{g,1} + p_{g',1}}\right)}_{\substack{\text{Share of applicants whose } \mu_i \text{ is} \\ \text{above hiring threshold when} \\ \text{only signal 1 is available}}} \times \underbrace{p_{g,1}}_{\substack{\text{Probability signal 1} \\ \text{reveals match quality}}} \tag{2.3}$$

Intuitively, the first term in this expression shows what share of applicants' types are above the hiring threshold, where the share is larger when the available signal is less informative (i.e., when the the denominator $p_{g,1} + p_{g',1}$ is smaller, pushing the hiring threshold lower). Meanwhile the second term shows the probability that applicants whose types are above the threshold indeed have their match quality revealed. The product of these is then the hiring rate for group g.[8]

A similar intuition leads to an expression for group g's hiring rate prior to a ban, when both signal 1 and signal 2 are available,

$$\lambda_{g,2} = \underbrace{\left(\frac{2\alpha}{p_{g,1} + p_{g',1} + (1 - p_{g,1})p_{g,2} + (1 - p_{g',1})p_{g',2}}\right)}_{\substack{\text{Share of applicants whose } \mu_i \text{ is} \\ \text{above hiring threshold when} \\ \text{signal 1 and 2 are available}}} \times \underbrace{(p_{g,1} + (1 - p_{g,1})p_{g,2})}_{\substack{\text{Probability signal 1 or signal 2} \\ \text{reveals match quality}}} \tag{2.4}$$

Relative to the earlier expression 2.3, the change in the denominator in the first term on the right-

[8]This expression also relies on the assumption mentioned above that applicants benefit from conveying any information at all, or equivalently, that employers do not hire job-seekers who provide no information. Formally, this assumption can be written as $\frac{2\alpha}{p_{g,1}+p_{g',1}} < \frac{1}{2}$, which guarantees α is small enough relative to the share of successful signals that employers do not choose to hire applicants for whom no signal was successful.

hand side of expression 2.4 illustrates the effect of an endogenously higher hiring threshold when both signals are available, whereas the change in the second term illustrates the countervailing force that more applicants from group g reveal their type to be above that (higher) threshold. Subtracting expression 2.4 from expression 2.3 shows the change in hiring rates for group g when a ban is relaxed (or, alternatively, when the second signal becomes available). After some manipulation, this yields a sufficient and necessary condition for the availability of signal 2 to increase group g's hiring rate:

$$\frac{p_{g,2}}{p_{g',2}} > \frac{\left(\frac{p_{g,1}}{1-p_{g,1}}\right)}{\left(\frac{p_{g',1}}{1-p_{g',1}}\right)} \tag{2.5}$$

This expression shows that in order for the introduction of signal 2 to increase group g's hiring rate in this simple model, the ratio of signal 2's probabilities for g relative to g' must be greater than the corresponding ratio of odds for signal 1. This condition captures group g's *relative advantage* in signal 2: the condition can fail to hold even if $p_{g,2} < p_{g',2}$ as long as $p_{g,1}$ is sufficiently smaller than $p_{g',1}$. Consequently, if the baseline signals for a given group are very imprecise relative to other job applicants, then adding new signals will generally help that group even if those signals are themselves slightly less informative for that group. In the extreme case, if either $p_{g',1} = 1$ or $p_{g,1} = 0$, then making signal 2 available will help group g as long as it provides *any* information, i.e. $p_{g,2} > 0$. Note that, so long as employers observe whether a signal has revealed applicant match quality, this expression for hiring rates will hold whether or not firms' observe or use group status g in the hiring process.[9]

2.2 Relative Advantage: A Richer Model

This subsection extends our results from the simple model above to a richer setting with a more general signal extraction problem. We analyze settings where agents on one side of a market – the senders – each compete to convince the other side of the market – a receiver – that their quality is above some threshold, in order to transact with the receiver. 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. We derive an expression for a group's relative advantage in an information source that, as in the simple model above, depends on the precision of the banned signal for each group relative to baseline signals. This expression has a closed form under some standard parametric assumptions and determines which groups of senders benefit from an information ban.

In this richer model, receivers face senders who are each a member of some group, and the

[9]This feature helps resolve a question about statistical discrimination models raised by Darity (1998) in the context of racial discrimination by employers: given all of the other potential signals available to employers, why would employers focus on race or find race particularly useful? Our model illustrates suggests that, even if employers do not directly use race, differences in the precision of signals sent by different groups can generate racial inequality.

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 employers statistically discriminate, though we extend to more general (and potentially biased) priors in Appendix Section A.[10] 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; the normal distribution is self-conjugate so this makes manipulating receivers' posteriors tractable. We show these posteriors in Appendix Section A.

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.[11] Similar to the simple model of Section 2.1, 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,[12] 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

[10] 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 match quality distributions are identical across groups or receivers do not observe or use group membership in decision-making, our model is also consistent with receivers observing heterogeneous signal precision across groups, for example when employers are aware of how much statistical noise there is in a given referral, interview, résumé, or educational background.

[11] Obviously, the desired number of successes, M, and the resulting threshold κ, may very across receivers, contexts, and time periods. For expositional reasons, we abstract from these differences in this section. When we turn to focus on PECC bans, 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. Given that we lack data on firms, we are unable to explore firm-level variation in the number of positions and the hiring threshold empirically.

[12] 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.

quality threshold can generate counteracting effects across groups in equilibrium.[13]

To characterize the banned signal's net effect, we parameterize the availability of the second signal with $t \in [0, 1]$, where $t = 0$ corresponds to a total ban and $t = 1$ corresponds to no ban at all, and we then evaluate the effect of varying t 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 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 t of the banned signal as,*

$$\frac{d\lambda_g}{dt} > 0 \text{ if and only if } \frac{h_{g,\text{banned}}}{h_{g',\text{banned}}} > \frac{\omega_g}{\omega_{g'}} \tag{2.6}$$

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

$$\omega_g = h_{g,\text{baseline}} \left(h_0 + h_{g,\text{baseline}}\right) \tag{2.7}$$

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*

[13] 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), whereas we study a setting where senders take the signal technology as given, and a planner is regulating the availability of different signals with an eye to incidence across different groups in equilibrium.

some group g is increasing in the availability t of the banned signal as,

$$\frac{d\lambda_g}{dt} > 0 \text{ if and only if } \frac{h_{g,banned}}{h_{g',banned}} > \frac{\omega_g}{\omega_{g'}}\left(1 + \frac{\Delta\mu}{\kappa - \mu_{g,0}}\right) \tag{2.8}$$

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

$$\omega_g = h_{g,baseline}\left(h_{g,0} + h_{g,baseline}\right)/h_{g,0} \tag{2.9}$$

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 2.9 captures the effect of differences in the dispersion of match qualities $h_{g,0}$. The factor newly included in expression 2.8 captures the effect of differences in mean match qualities $\Delta\mu$.

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.[14] 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), and Bohren et al. (2020)), and biased signals where the receiver remains unaware of the bias in the signal (Autor and Scarborough, 2008).

Evidence suggests a substantial role for such biases, especially in the context of race (Darity and Mason (1998), Bertrand and Mullainathan (2004), Charles and Guryan (2008)). We still focus on the role of signal precision because we view it as understudied and because, as we show in Section 5.5, in our empirical setting we find it is quantitatively important, even when accounting for the potential role of biases by employers and in signals. Furthermore, 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)).[15] Finally, signal biases and stereotypes may be eroded over time by experience and by competitive pressure (Becker (1957), Darity and Mason (1998), Altonji and Pierret (2001)),

[14] 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. As a concrete example, this is true when an employer hires less than half of the applicants whom it sees.

[15] 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.

whereas inequality in signal precision may reflect and contribute to long-standing institutional forces (Small and Pager (2020)) that shape racial disparities in economic outcomes.

Overall, the richer model in this subsection helps confirm the intuition presented by the simple model in Section 2.1. Proposition 2 also 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. We find that in order to benefit from the availability of a banned information source, a group does not need to have strictly less noise in the signal than other groups, nor does it need to have higher average realizations of the signal; rather, the group's relative advantage in terms of signal noise for the banned information must be greater than its relative advantage in other available information sources, in the precise senses of Propositions 1 and 2.

2.3 Estimating Model Parameters

Our theoretical framework also suggests that empirical work can usefully quantify the parameters that determine relative advantage. Such parameter estimates could be used both to characterize heterogeneity across information sources and to understand, when regulating the availability of a given information source, the incidence of such regulation across various groups. This subsection outlines such an empirical strategy in general terms meant to be relevant for a range of applications. We later apply such a strategy specifically to the case of PECC bans and job applicant screening tools in Section 5.5.

In the model of Section 2.2, four parameters describe how signals are generated and a receiver's posterior beliefs are formed for group g: the mean and inverse variance of match qualities ($\mu_{g,0}$ and $h_{g,0}$); the inverse variance of baseline signal noise ($h_{g,\text{baseline}}$); and the inverse variance of the banned signal's noise ($h_{g,\text{banned}}$). In cross-sectional data these are likely not separately identified for two reasons: the baseline signal is taken, as in Section 2.2, to be a composite of all other available signals, some of which may be unobserved for the econometrician; and second, dispersion in signal realizations could result from either dispersion in actual match quality or dispersion in noise. However data from before and after an information ban make it possible to overcome this challenge, even when some baseline signals are unobservable.

In general, the key quantities to observe are success rates before and after an information ban together with at least one measure of match quality before and after the ban. These four moments per group identify the four parameters described above in an intuitive way. First, holding fixed the differences in average match quality ($\mu_{g,0}$) across groups, differences in success rates from when the banned signal is *unavailable* will identify differences in the dispersion of receiver posteriors across groups, where the dispersion in posteriors is a function of $h_{g,0}$ and $h_{g,\text{baseline}}$.[16] Differences in match quality when the banned signal is unavailable then identify how much that estimated level of posterior dispersion is due to signal precision ($h_{g,\text{baseline}}$) or due to underlying match quality dispersion ($h_{g,0}$) – or in more intuitive terms, what share of matching

[16]See expression A.5 in Appendix A for details on this relationship.

successes are due to signal noise.

Next, changes in match quality when the banned signal is instead *available* identify how much precision is added by the additional signal ($h_{g,\text{banned}}$). Lastly, changes in success rates identify how far in the tail of posteriors successful senders must be in order for a given change in precision to result in a given change in success rates, which pins down average match quality ($\mu_{g,0}$) across groups. In some empirical settings, including additional model parameters may be important in order to match empirical moments on match quality: for example in a labor market setting, if estimates of involuntary separation rates are used to identify match quality, then additionally estimating a cost parameter that governs an employer's firing decisions may be necessary. In this case the model is still identified, up to normalizing one group's average match quality ($\mu_{g,0}$) to zero, and the cost parameter is identified relative to the receiver's surplus from matching with an average sender from the normalized group.

This empirical strategy may prove useful across a broad range of applications, including the regulation of screening tools in labor markets (Wozniak (2015), Agan and Starr (2018), Doleac and Hansen (2020)), insurance markets with thresholds for approval and denial of new policy applications (Hendren (2013)), the removal or introduction of information in consumer credit reports (Foley et al. (2020), Liberman et al. (2018)), and in US mortgage markets, where underwriting decisions often rely on approval thresholds (Keys et al. (2010)).

In the empirical application that we develop in the next section, we estimate empirical moments in the context of labor markets and recent bans on PECCs. We later use these moments in an application of the above methodology, where we estimate differences in signal precision for PECCs and for other labor market screening tools across groups.

3 Empirical Setting: Background and Data

In this section we provide background about our empirical application, and we describe our data and the construction of several variables that play a central role in our empirical work. Our primary dataset is the Current Population Survey (CPS), which we use to measure job-finding, involuntary separation rates, and overall employment. We supplement our CPS measures of job-finding by using publicly available administrative data collected from the near-universe of state unemployment records: the Job-to-Job (J2J) Flows data from the US Census Bureau, which are compiled as part of the Longitudinal Employer-Household Dynamics (LEHD) program. 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 also have a non-trivial effect on hiring decisions. Household survey evidence suggests that 10 percent of low- and middle-income[17] job-seekers recall being told they were denied a job on the basis of information in their credit report (Traub (2013a)). As Traub (2013b) argues, the true PECC-related rejection rate may be higher if firms do not always comply with the Fair Credit Reporting Act's requirement of sending adverse-action letters that report the use of credit report data in adverse hiring decisions, or if applicants do not recall receiving these letters.

Restrictions on PECCs have typically been motivated by two concerns. First, PECCs are seen as having an unequal effect on traditionally disadvantaged job-seekers.[18] Second, PECCs are seen as contributing to labor market hysteresis, as PECCs may increase the persistence of unemployment shocks for individuals with poor credit histories.

On net, this policy debate has seen thirteen new PECC bans enacted over the past thirteen years, 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. Figure 2 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. The bans variously grant exemptions to 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.[19] 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 (Gordon and Kauffman (2010), Rubin and Nelson (2010), Rubin and Kim (2010), Fliegel and Mora (2011), Fliegel and Simmons (2011), Fliegel et al. (2011), Fliegel and Mora (2012), Fliegel et al. (2013)). To use these exemptions empirically, 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 below. Although there have been 13 state and local PECC bans, in practice we only study ten;

[17] The survey defined middle-income as up to 120 percent of county-level median income.

[18] See footnote 4 for related evidence.

[19] The bans also differ in their enforcement mechanisms. The enforcement mechanism in Illinois, for example, 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)). Because it is difficult to categorize which of these laws' enforcement mechanisms are stronger than others, our analysis focuses on the between- and within-state heterogeneity in PECC bans' exemptions.

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 48.8 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 ranges from 41.5 percent in Connecticut, to 79.7 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 (e.g., as in Shimer (2012)). 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 Figure 2, 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.

Table 2 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 three different panels, A through C. 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. For both Black and white individuals, wages are also higher in PECC ban states, as is the share of workers with a four-year college degree.

Table 3 reports summary statistics on our key dependent variables, job-finding and separation rates, using both the CPS and the LEHD-J2J. Summary statistics are calculated using observations from between the first quarter of 2005 and the first quarter of 2017 in the LEHD-J2J and between 2003 and 2018 in the CPS (the years for which we have data from all states in our sample). Columns (1) and (2) report averages for states that have banned and have not banned PECCs respectively. Columns (3) and (4) then report the CPS dependent variables separately for covered and exempted jobs within states banning PECCs (LEHD-2J2 data do not have the occupational detail required to break down outcomes by covered and exempt status). Panel A, B, and C report these averages separately for Black, Hispanic, and White individuals respectively.

3.3 LEHD-J2J Data

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 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)). These are publicly available administrative data aggregated from Unemployment Insurance (UI) records from all 50 states and Washington, DC.[20] As in the CPS, we restrict the sample to a balanced set of pre- and post-treatment time periods. In the case of the LEHD, this restriction limits us to three years post-treatment and four years pre-treatment.[21] Figure 2 illustrates LEHD-J2J availability for the treated states.

The LEHD-J2J reports three different measures of transitions to new jobs, depending on the duration of unemployment spells between jobs. These three measures correspond to spells that last two or more quarters ("transitions from persistent unemployment"), spells that last roughly one quarter ("adjacent-quarter transitions"), and spells that last less than one quarter (including spells of zero length, i.e. job changes without any time off from work). In our analysis we focus on the intermediate category, adjacent-quarter flows. This category includes spells of involuntary unemployment but also short voluntary breaks between jobs (Hyatt et al. (2015)). This category strikes a balance between trying to focus on involuntary unemployment, which would be impossible in the shortest-duration category, and avoiding duration-dependence problems that would arise in using the longest-duration category. However, this limits our focus in the LEHD to spells of unemployment spells of about one quarter, excluding shorter or longer spells.

As mentioned above, Table 3 reports summary statistics on our key dependent variables from both the CPS and LEHD-J2J. The LEHD-J2J outcome variables included are the separation rate and the adjacent quarter job-finding rate. We discuss these statistics in more detail when we turn to our analysis of these outcomes in Section 5.

3.4 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

[20]Note that although the LEHD compiles data from all 50 states, we exclude Vermont, Washington, and Connecticut (three treated states) because there are insufficient numbers of pre- and post-treatment years given our balanced sample restriction, and we exclude Montana, South Dakota, Wyoming, and Idaho because the number of workers of different races or ethnicities are very small, as discussed more in Appendix Section D.4. The data from these suppressed states is still included in the flows data for other states. For example, if a person separated from a job in New York and took a job in Connecticut, this would be recorded as a job-to-job flow for New York, even though Connecticut's own flows data are suppressed.

[21]This window differs slightly from that for the CPS because of differences in years for which data are available across states.

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.[22]

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 $l \in \{C, P, E\}$. For example, $T^E_{j(i),s(i)} \in [0, 1]$ stands for unemployed individual i's expected-job exposure when formerly employed in job j, i.e. $T^E_{j(i),s(i)} = p_{j,s} = \Pi_j t_s$, using the notation developed in footnote 22.

4 Empirical Strategy

In this section, we discuss the variation we exploit to study the effect of PECC bans and how we use this variation empirically.

The core of our empirical strategy is difference-in-differences. We first develop this strategy using between-state variation in the timing of PECC bans, and we then show how to extend this strategy to use job-level variation in which workers or job-seekers are exposed to each ban. We discuss and test, by means of event-time plots, the parallel-trends identifying assumptions underlying this strategy. We also estimate demanding triple-difference models of the effect of PECC bans to explore the robustness of our results to less restrictive identifying assumptions. Given our focus on how PECC bans affect minority labor market outcomes, we also allow all of our estimates of PECC bans' effects to vary by race or ethnicity.

[22] To describe our "expected job" exposure more formally, let t_s be a vector of job-specific treatment dummies indicating which jobs are treated by a PECC ban in state s (i.e., zeros in t_s correspond to exempted jobs). We estimate job-to-job transition probabilities (via unemployment) in all untreated states and months, collect these probabilities in the transition matrix Π, and pre-multiply t_s by Π to obtain a state-specific vector of job treatment probabilities p_s for the unemployed, $p_s = \Pi \times t_s$. Intuitively, each component j of p_s is a measure of the probability that an unemployed worker formerly employed in job j will transition into employment in a job that is treated in state s, conditional on transitioning into some employment. We then assume search probabilities are equal to these estimated transition probabilities. See also footnote 25.

4.1 State-level Variation

Our baseline specifications take advantage of geographic variation in which states enacted PECC bans and temporal variation in when those bans were enacted. Letting y_{ist} 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 a difference-in-differences model of the effect of PECC bans on labor market outcomes:

$$y_{it} = \alpha_{s(i)} + \gamma_t + \delta D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{4.1}$$

Given the policy concerns about PECCs' disparate impacts on minority job-seekers, we are interested in whether PECC bans differentially affect different race or ethnic groups. To capture these heterogeneous effects of PECC bans, we fully interact all of the right-hand-side variables in equation 4.1 with race or ethnicity,[23] leading to the workhorse specification we use in much of our analysis:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_r \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{4.2}$$

Equation 4.2 is equivalent to running equation 4.1 separately by group. The parameters of interest, the group-specific interactions δ_r, will identify the causal effect of PECC bans on labor market outcomes for each race or ethnic group r under the identifying assumption that different groups would have had, in the absence of a PECC ban, similar trends in states enacting PECC bans as in states not enacting PECC bans.

Finally, in our most demanding specifications, we add state-time fixed effects to equation 4.2, leading to the triple-differences specification:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \xi_{s(i),t} + \sum_{r \neq W} \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{4.3}$$

Note that we can no longer include all race/ethnicity-treatment interactions, because one is absorbed by the state-time fixed effect, so we instead sum over all non-white groups (i.e. $\sum_{r \neq W}$).

In an effort to investigate the validity of our parallel trends assumptions and explore how treatment effects change over time, 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 $\kappa_{st} = t - t_0^s + 1$. Fully interacting the vector of event-time indicators, κ, with a PECC ban indicator then leads to the event-time study specifications below. For example, the event-time specification corresponding to equation 4.2 is:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_k \sum_r \delta_r^k 1_{r=r(i)} \times D_{s(i)} \times 1_{k=\kappa_{st}} + \epsilon_{it} \tag{4.4}$$

[23] As discussed more in Appendix Section D.5, we define three mutually exclusive race or ethnicity categories, $R = \{\text{white}, \text{Black}, \text{Hispanic}\}$.

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 3.4. 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:

$$y_{it} = \alpha_{s(i),j(i)} + \gamma_t + \delta D_{s(i)} \times P_{s(i),t} \times T^l_{j(i),s(i)} + \epsilon_{it} \tag{4.5}$$

The identifying assumption in this baseline specification is that treated jobs (i.e., jobs covered by a law in a treated state) are on parallel trends with non-treated jobs (both exempted jobs within PECC-ban states and all jobs in non-PECC-ban states).

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 our CPS data, for example, there are 473,398 such fixed effects, i.e. 473,398 non-empty state × industry × occupation cells, to be estimated on 13,077,449 panel observations.) In practice we therefore form groups of jobs according to each job's treatment status. We choose the smallest possible number of groups 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.[24] These are simply the standard fixed effects to include in a difference-in-differences strategy, recognizing that our state-job variation is truly at the state and job-*group* level. Throughout our empirical work we therefore allow $j(i)$ to stand for job group rather than job.[25]

As in Section 4.1 above, we are interested in how PECC bans differentially impact different race or ethnic groups. Consequently, in practice we interact equation 4.5 with a full set of group dummies, leading to the empirical specification:

$$y_{it} = \alpha_{s(i),j(i),r(i)} + \gamma_{t,r(i)} + \sum_r \delta_r D_{s(i)} \times P_{s(i),t} \times T^l_{j(i),s(i)} + \epsilon_{it} \tag{4.6}$$

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. 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 unemploy-

[24]That is, our set of job groups is the meet of the (job) partitions generated by recent PECC bans.

[25]For clarity, we emphasize that this also is true of the $j(i)$ notation used in our estimation of the transition probabilities that determine $T^E_{j(i),s(i)}$; see also footnote 22.

ment 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 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:

$$\lambda_{i,t}(\tau) = \lambda_0(\tau) \exp\left(W_i + \sum_r \beta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i' \beta_{x,r(i)} \right) \tag{4.7}$$

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. The choice of the exponential functional form is standard, in order to model the hazard rate $\lambda_{i,t}(\tau)$ as nonnegative.

In order to bring this expression to the data we need to 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 4.7 to write the discrete time hazard in complementary-log-log form as:

$$\ln(-\ln(1 - \lambda^d_{i,t}(\tau))) = \alpha_\tau + W_i + \sum_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i' \beta_{x,r(i)} \tag{4.8}$$

$$\alpha_\tau = \ln \int_{\tau-1}^{\tau} \lambda_0(s) ds \tag{4.9}$$

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, we can then rely on our earlier identifying assumptions to identify the parameters of interest, β_r. For example, our workhorse state-time difference-in-differences model in equation 4.2 can be written in complementary-log-log form as

$$\begin{aligned} \ln(-\ln(1 - \lambda^d_{i,t}(\tau))) = &\alpha_\tau + \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + X_i' \beta_{x,r(i)} \\ &+ \sum_r \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} \end{aligned} \tag{4.10}$$

This equation inherits the state-time difference-in-differences strategy's basic identifying assumption of parallel trends between PECC-ban states and non-PECC-ban states. In particular, we assume parallel trends in the complementary-log-log of discrete-time hazards. It can be shown in the derivation of equation 4.8 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.

Note that we can 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 4.4. For example, the event-time version of equation 4.10 is,

$$\begin{aligned}\ln(-\ln(1-\lambda^d_{i,t}(\tau))) =& \alpha_\tau + \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + X_i'\beta_{x,r(i)} \\ &+ \sum_k \sum_r \delta^k_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} \times 1_{k=\kappa_{st}}\end{aligned} \tag{4.11}$$

Exponentiated event-time coefficients δ^k_r are then interpretable as event-time-specific hazard ratios.

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 4.10 can be estimated via maximum-likelihood (as detailed in Meyer (1990)). For aggregated data like the LEHD-J2J, we use OLS where we plug in population-average job-finding rates for $\lambda^d_{i,t}(\tau)$ on the left-hand-side of equation 4.10. We discuss finite-sample properties of this nonlinear plug-in estimator in Appendix Section D.4, where we demonstrate that any finite-sample bias due to nonlinearity on the left-hand-side is negligible, so long as we limit our estimation to states with sufficiently large populations of job-seekers in each group that we study. In practice this means that when we estimate equation 4.10 in the LEHD-J2J, we exclude data from states with few minority job-seekers: Idaho, Wyoming, Montana, South Dakota, and North Dakota.

5 Results

In this section, we present our main results on the labor market effect of PECC bans. We focus on the two outcomes that, based on the general empirical strategy in Section 2.3, will allow us to characterize relative advantage in PECCs and other labor market signals. These two are job-finding rates and new hires' involuntary separation rates, which we use to measure new hires' match quality.

5.1 Job-Finding Rates

Starting with CPS data and our proportional hazards model of job-finding, we estimate the event-time coefficients in equation 4.11 to illustrate key patterns of how job-finding varies with PECC bans. We estimate δ^k_r for event-time years $k = -3, -2, \ldots 2, 3, 4$, omitting $k = 0$ such that all estimates are relative to the 12 months immediately prior to the implementation of a PECC ban.

Figure 3 Panel A shows estimates of δ^k_r for Black job-seekers ($r = B$) in our preferred specifications, which include demographic and state-year policy and economic controls that we describe more below. 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 31 percent lower in the third year after implementation before rebounding somewhat in the fourth year.

Panels B and C of Figure 3 then show analogous estimates for Hispanic and white job-seekers, respectively. 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 three years post-PECC ban 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, although in subsequent years the difference in job-finding declines.

Having illustrated these patterns in our data, we now turn to our baseline estimates of δ_r in equation 4.10, which summarize average effects in post-ban years. Panel A1 of Table 4 reports these estimates in three rows for Black, Hispanic, and white job-seekers respectively. Column (1) presents results from a specification without demographic or state-level policy and economic controls. This specification allows for rich differences between states or across time in labor market outcomes by race and ethnicity. Column (2) then 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).[26] For both of these first two 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 (3) then adds controls for state-time 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).[27] 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 4 is roughly -11 percent. This estimate is essentially unchanged after adding demographic covariates in column (2). 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. Adding state-time controls for time varying effects of economic conditions or changes in state policy in Column (3) increases the magnitude of the point estimate to about -14 percent, although we cannot reject the previous estimates.

Our estimated effects for white job-seekers are small and statistically indistinguishable from zero, as expected given the patterns in the event-time plots. 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 three specifications, the overall effect is small in magnitude and negative, with coefficients ranging from -0.2 to 0.7 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

[26] 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.

[27] These controls are further described in Appendix Section D.1.

effects are consistent with the data.

With these baseline state-level estimates in hand, 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 4 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 4.8 to use our measures $T^l_{j(i),s(i)}$ of job-specific exposure to PECC bans, as defined in Section 3.4. Results for estimating several versions of this specification are shown in Panel B of Table 4, and corresponding event-time plots are shown in Appendix Figure 1.

We measure job-specific exposure using expected-job exposure, $T^E_{j(i),s(i)}$.[28] Column (1) of Table 4 Panel B starts with our baseline version with no covariates other than the appropriate job-level fixed effects, W_i. Column (2) then adds individual-level covariates X_i, and Column (3) adds state-policy and economic controls. Estimates for Black job-seekers are larger than our state-level estimates from Panel A1, indicating that the patterns observed in our state-level analysis are indeed driven by jobs covered by PECC bans. In fact, if one scales up the point estimates from our state-level analysis in Panel A 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 2), the resulting estimate is .199, strikingly close to our job-level estimate of .194 in Column (3) of Panel B1 of 4. This similarity provides further evidence that our results are driven by individual exposure to PECC bans rather than alternative explanations.

In specifications using job-level variation, the 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 3 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 explore 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 throughout this section is an indicator for any subsequent involuntary separation after being newly hired.[29] Given the rotating-panel structure of our CPS data (as discussed in Section 3.2), 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

[28] In results available upon request, we find similar results using our alternative job-specific measures.

[29] This choice may be particularly apt in the case of PECCs, as PECCs are seen as a screen for behaviors such as theft (Society for Human Resource Management (2012)) that are likely to lead to involuntary termination.

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, as in equation 4.2. Figure 4 reports event-time versions of this baseline model. Starting with the event-time analysis for Black new hires in Panel A, 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, which makes the near-zero point estimates in post years somewhat difficult to interpret; as we discuss below, we ultimately find imprecise results for Hispanic involuntary separation rates. 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.

The first column of Table 5, Panel A reports estimates of equation 4.2 corresponding to these event-time plots. The results confirm our visual inspection of Figure 4, with Column (1) showing a precisely estimated 2.7 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. Column (2) 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 4.4 percentage points,[30] 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. We discuss these white and Hispanic effects in more detail below.

In Columns (3) and (4) 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 4.6, 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 2.7 percentage points in Columns (1) to 4.9 in Column (3) and, for specifications with these controls, rises from 4.4 percentage points in Column (2) to 7.9 in Column (4). 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 (4), 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.

[30] In results available upon request, we show that this increase is driven entirely by the state and economic policy controls. Controlling for only individual covariates has little impact on the results.

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 (3), PECC bans are estimated to have a precise $-.019$ to $-.022$ percentage point effect on involuntary separation rates; however, the inclusion of state policy and economic controls in Columns (2) and (4) reduces the magnitude of both estimates, making both indistinguishable from zero.

To better understand these results, we re-estimate the four involuntary separation regressions from Table 5 on a placebo sample: long-tenure employees rather than new hires. 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.

Table 6 reports results of these placebo regressions. All placebo estimates for Black workers are insignificant and 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.

On net we see two key conclusions from these results. 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 5 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 5, we report estimates of equation 4.2 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 0.9, -0.8, and -0.5 percent for Black, Hispanic, and white workers respectively and $-.4$ 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.3 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, alternative definitions of exposure to PECC bans, and in the case of job-finding, estimation on a supplementary dataset built from administrative data. Full details are presented in Appendix Section C.2, and Figure 5 summarizes these results. Panel A of the figure summarizes how our estimates of the relationship between PECC bans and job-finding vary with alternative modeling choices, controls, and datasets. 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 somewhat across specifications and datasets. In particular, the estimates in the LEHD-J2J of the effect of PECC bans on Black job finding hazards, ranging from -3.4 to -4.6 percent, are smaller than the corresponding CPS estimates of -10.6 to -13.5 percent.

One possible explanation for the smaller estimates in the LEHD-J2J data relative to our CPS sample is that, as discussed in Section 3.3, we are only able to analyze adjacent-quarter, employment-to-unemployment-to-employment transitions in the LEHD-J2J; we do not analyze spells that start and end within the same quarter or that last for multiple quarters. In addition, the LEHD-J2J only includes 44 of 50 states, including only 7 of the 10 treatment states we study in the CPS. To study the effect of these differences, Panel B of Appendix Table 2 re-estimates the CPS models restricting the sample to spells that would have selected into the LEHD-J2J adjacent-quarter transition data and to the 44 LEHD-J2J states. We see that the CPS estimates become markedly more similar to the LEHD-J2J estimates when the sample mirrors the LEHD-J2J selection process, suggesting that different samples drive the divergence between the estimates using the two datasets.

Returning to Figure 5, Panels B and C explore the robustness of the CPS job-finding results using job-variation and involuntary job-separation results respectively. The full robustness analysis in the CPS is reported in Appendix Table 3 and Appendix Table 4. For both job-finding and involuntary separations in the CPS, we find that adding controls for time-varying state-level trends tends to increase the magnitude of the effect of PECC bans on Black job-seekers, while having little systematic impact on estimates for Hispanic or white job-seekers.

5.4 Heterogeneity by Other Observable Subgroups

In this section we explore whether PECC bans have different effects across other observable subgroups, both overall and within race or ethnicity groups. We focus on two central labor market observables that are likely to be related to how employers screen applicants: education level and potential experience. In view of the theoretical results in Section 2, we would expect any benefits of a ban to accrue to subgroups that have relative advantage in the precision of baseline, non-PECC signals, and relative disadvantage in the precision of PECCs, all else equal. We re-estimate versions of our baseline specifications 4.1 (which has no race/ethnicity interactions) and 4.2 (which does have race/ethnicity interactions) where we now add interactions with categories of education or experience: having a college degree, or having six years or more of potential experience.

Table 7 Panel A reports estimates without race or ethnicity. We see that PECC bans have a small, imprecisely estimated negative effect on job-finding rates for job-seekers without college degrees, and a positive effect for college-educated job-seekers; the difference between the two groups is marginally significant in column (1) and becomes more significant as we add state policy and economic controls in column (2).

We see fewer differences across experience levels in columns (3) and (4). One potential explanation for these contrasting patterns is that we may be measuring (relevant) experience imprecisely and hence attenuating any actual difference between these categories.

Panel B then investigates whether the effects we estimated previously by race and ethnicity also differ within group by education level or experience. For all three race or ethnicity groups, the effect of a PECC ban is between 6 and 16 percentage points less negative for job-seekers with a college degree or more. For Black job-seekers, the reduced effect of PECC bans on job-finding rates is large enough to completely counteract the negative effect of a PECC ban, leading to a slight rise in job-finding rates of around 2.1 percent for Black college graduates. In contrast, we do not find evidence that PECC bans significantly reduce job-finding rates for white or Hispanic job-seekers without college degrees, and, consistent with the results in Panel A, we find that college-educated white and Hispanic job-seekers have slightly higher job-finding rates after a PECC ban. Moving to Columns (3) and (4), we see that, as in Panel A, the effect of PECC bans does not seem to vary with our measure of potential experience.

5.5 Mechanisms: Noise in PECCs and Other Screening Tools

This section implements the quantitative model discussed in Section 2.3 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 2.2: 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-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 Table 3, column (2). 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 4 and 5. 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 6 presents estimates of some key parameters of interest for each group: PECCs' signal precisions $h_{g,\text{banned}}$ and the precision of non-PECC screening tools $h_{g,\text{baseline}}$. In the left half of the figure, 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 0.71 for Black job-seekers to 2.23 for white job-seekers.[31] Meanwhile in the right half of the figure, the precision of PECCs as a screening tool is closer to equal for white and Black job-seekers – equal to 1.16 for white job-seekers and 1.23 for Black job-seekers. Interestingly, the precision of PECCs for Hispanic job-seekers is markedly lower, equal to 0.65, 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 uncontrolled state-level regressions; these estimates are broadly similar, and in this panel the Black and white PECC precisions are even closer to equal, at 1.29.

[31] Given how the model abstracts from heterogeneity across employers and their applicant pools, we emphasize that our model estimates are intended to be illustrative of broad patterns and differences across groups and signals. 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.

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 the availability of a new information source such as PECCs even if that group does not have an absolute advantage vis-a-vis that new information. Rather, as emphasized by our main theoretical result in Proposition 1, what matters is how precise the new information source is relative to other available screening tools for a given group. We estimate that the availability of PECCs improves Black labor market outcomes not because PECCs provide meaningfully 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 2.2, our estimates of relative advantage are also robust to the presence of taste-based discrimination, related racial animus or biases, and also structural discrimination 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.[32]

We conduct some basic counterfactual exercises to help illustrate our model estimates and their usefulness. Table 8 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, where we use the parameter values reported in the top half of Figure 6. 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

[32] Interestingly, when biases alone are used to explain related empirical results in other contexts, the estimated biases can be dramatically large, potentially suggesting that signal precision differences could be an important omitted factor behind large estimates of bias. Estimates of employer biases in Agan and Starr (2018), for example, suggest that changes in Black job-finding rates after Ban-the-Box (BTB) policies imply employers' priors must over-estimate Black applicants' probability of having a felony conviction by a strikingly large amount, between 200% and 400% of actual rates.

pronounced change in its job-finding rates in Panel C when PECCs are made available. Note we also see that firing rates decrease slightly for all groups after a PECC ban in this counterfactual, given the estimated shift in firing costs.[33]

These results again underscore the key message of the parameter estimates above. We find that the availability of PECCs as a screening tool has a different effect on Black job-seekers not because PECCs per se provide different information for Black job-seekers, but rather because the other, non-PECC screening tools are disproportionately noisy for Black job-seekers.

6 Discussion

We have documented that PECC bans brought a marked decrease in job-finding hazards for Black job-seekers, and also a decrease in Black new hires' match quality as measured by subsequent involuntary separation rates. We found little evidence of an impact for white job-seekers, with generally similar conclusions for Hispanic job-seekers. We also used these results together with the empirical strategy of Section 2.3 in order to estimate differences across groups in the precision of PECCs and of other labor market screening tools. In this section, we interpret the economic magnitude of our estimates and discuss the implications of these findings in the context of the discrimination and racial inequality literatures.

In our specification using state-time variation in the CPS data, equation 4.10, with demographic controls we estimate that PECC bans reduced the job-finding hazard for Black job-seekers by 11 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 Table 3.[34] This comparison implies our estimate translates into a 2.8 percentage point reduction in the probability of Black job-seekers finding a new job within a quarter of job-loss.

Given that PECC bans may 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.[35] Using this approach, we estimate that PECC bans reduced the steady-state

[33] However, if we remove this channel by holding firing costs unchanged, we see that firing rates rise slightly – though by nearly zero – for all groups, consistent with PECCs being about equally informative across groups. Specifically, if firing costs are unchanged after a PECC-ban, then the post-ban firing rates in the Panel C counterfactual are 0.0878, 0.0801, and 0.0867 for Black, Hispanic, and white workers respectively.

[34] We estimate similar baseline job-finding rates in the CPS. However, given the CPS sample structure, estimating these baseline rates in the CPS requires formally estimating a hazard model, and *baseline* hazards are only estimated consistently in our specified model under strong conditions. Among these strong conditions is the unrealistic requirement that no relevant regressors are omitted from the model, even if those regressors are independent of our primary regressor of interest, the indicator for PECC ban coverage (Lancaster (1979)).

[35] 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.

Black employment rate by 1.5 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 5.9 percent increase in wages for Black workers.

Is it reasonable that restrictions on the use of information like PECCs in the hiring process can have such a large impact on job-finding rates? Other evidence from the literature suggests yes. Studying the effect of the usage of credit information in hiring in Sweden, Bos et al. (2018) find that the removal of information on past defaults from credit reports results in a 2 to 3 percentage point increase in employment rates for affected individuals in the year after the past-default information removal. Wozniak (2015) finds that laws discouraging or encouraging the use of drug-testing in the hiring process have a 7 to 30 percent effect of Black employment levels in affected industries. In work closely related to our empirical application, Doleac and Hansen (2020) find that Ban-the-Box (BTB) policies reduce the employment of low-skilled Black workers by 3.5 percentage points.[36] All three of these papers suggest that regulation of information used in the hiring process can have economically large impacts on employment outcomes.[37]

Our results also are complementary with other recent papers on PECC bans, though our focus differs from these papers. One contemporaneous paper, Ballance et al. (2017), and a more recent paper, Friedberg et al. (2016), study the direct effect of PECC bans on individuals who are especially likely to have poor credit: Friedberg et al. (2016) focus on Survey of Income and Program Participation (SIPP) respondents who report having recent trouble paying their bills, finding that job-finding hazards rose by 25 percent for these individuals after PECC bans; Ballance et al. (2017) focus on individuals living in census tracts with average credit scores below 620, finding that employment in these census tracts rose 6 percent after PECC bans. In contrast, we emphasize how PECC bans' effects for broader groups can still be negative overall. That is, even though Black job-seekers with particularly weak credit may benefit from PECC bans, we find that restricting access to credit information still harms Black job-seekers on average.[38]

[36] Agan and Starr (2018) combine a résumé-audit design with a difference-in-differences strategy exploiting New Jersey and New York's BTB policies to explore the mechanisms driving the effects of BTB policies and find that BTB increases job application callback rates among Black applicants with criminal records, but decreases them among Black applicants without criminal records. On net, the BTB policies reduce average callback rates of Black applicants, with the primary beneficiary of the policies being white applicants with criminal records. Using the Longitudinal Origin Destination Employment Series (LODES), Shoag and Veuger (2016) reach somewhat divergent findings, finding that employment actual rose by 4 percent for residents of neighborhoods with high crime-rates after BTB laws were passed. Using broader variation, Holzer et al. (2006) find that employers' use of criminal background checks predicts higher Black-male employment, despite higher levels of criminal history among Black males. Finlay (2009) finds that labor market outcomes worsened for ex-offenders once criminal records became available online.

[37] Craigie (Forthcoming) adds important nuance to this discussion by noting that the effects of these information restrictions may be different in public versus private labor markets. She investigates the effects of BTB policies on *public employer* hiring rates for Black workers with criminal records and finds large, positive effects, while finding no evidence for negative spillovers in public employment for Black workers without criminal records.

[38] Friedberg et al. (2016) and Ballance et al. (2017) briefly explore effects by race. Friedberg et al. (2016)'s point estimates for Black individuals are actually positive, but their standard errors large enough to be consistent with very large positive or negative effects. Ballance et al. (2017) use the ACS to study the effect of the bans on the overall Black employment rate, rather than transitions from unemployment to employment, and they do not investigate effects on white or Hispanic individuals. However, despite these differences, a back of the envelope calculation suggests that Ballance et al. (2017)'s estimate that PECC bans reduce employment by 1.9 percentage

Three recent studies on the removal of adverse information from credit reports are closely related to, and complementary to, our own. Bos et al. (2018) study an administrative change in Sweden that removed bankruptcy and default information from some borrowers' credit reports, and they find that this change led to higher employment rates for affected individuals. In two related studies, Herkenhoff et al. (2016) and Dobbie et al. (2019) study the effect of the removal of bankruptcy flags using labor market data linked with credit records. Consistent with our findings, they both find that removal of bankruptcy information from credit records modestly increases flows into employment, by 7 and 3.6 percent respectively.[39]

The settings and variation in these papers differ from our own. The identifying variation in Bos et al. (2018) affected both credit and labor markets, as it prevented lenders as well as employers from viewing past default information. Their study focuses on Swedish pawnshop borrowers who previously defaulted on their loans, which is both a different institutional context and likely a more credit-challenged group than the population of US job-seekers as a whole. Herkenhoff et al. (2016) and Dobbie et al. (2019) study the removal of bankruptcy flags, which usually occurs 7 to 10 years after bankruptcy. However, survey research suggests that only some employers check information as far back as seven years.[40] Furthermore, bankruptcy is only one type of adverse credit information, and the effects of availability of other types of adverse credit information may differ. We thus view these three studies as informative about different types of information in different populations than our own.

More broadly, we provide the first quantitative estimates of which we are aware that the precision of traditional labor market screening tools, such as interviews and referrals, differs across different race or ethnic groups. While interesting on their own, these results also offer a possible unifying explanation for a wide range of findings in labor economics. First, these results may help explain higher returns for Black individuals to other labor market signals, such as occupational licenses (Blair and Chung (2018)) and veteran status (De Tray (1982)), that could help compensate for higher noise in other screening tools. Second, our findings may also help in understanding the relationship between firm size and Black employee share (Holzer (1998)), and the long-run impacts on Black worker hiring of temporary affirmative action programs (Miller (2017)), as firms may face fixed costs to reducing baseline signal noise for minority applicants. Third, our results provide support for the frequent modeling assumption in the statistical discrimination literature that employers may have less precise estimates of the match quality of minority job candidates (Phelps (1972), Aigner and Cain (1977), Cornell and Welch (1996), Morgan and Vardy (2009)). Our evidence on the imprecision of signals generated by the current screening process for Black job-seekers suggests that the current screening tools used by firms may represent an important form of institutional discrimination, as discussed by Small and Pager (2020).[41]

points for Black individuals is quite similar to our estimate of 1.5 percentage points.

[39] Dobbie et al. (2019) briefly explore the effects of PECC bans on workers who filed for bankruptcy four to six years ago. However, they estimate effects on employment levels, rather than flows out of unemployment, and do not disaggregate these results by race.

[40] In the Society for Human Resource Management (2012) report, roughly 25 percent of firms say they look at credit report information from 7 years ago or later.

[41] We conjecture, based on our own results and our reading of the combined evidence, that more subjective or

As a result, policies that reduce segregation or shift firm screening practices may be promising policy responses. For example, Miller (2017) finds that firms temporarily required by federal contracts to follow affirmative action hiring guidelines permanently increase their hiring rates for Black workers, even after the federal contract lapses. He interprets these findings in a model of "screening capital", where firms respond to affirmative action by investing in screening tools that, because firms have less precise posteriors of the quality of minority workers as in our model, benefit minority workers. Policy may also seek to address other factors that prior work has identified as contributing to inequality in labor market screening tools, including differences in social networks (Neckerman and Kirschenman (1991), Bayer et al. (2008), Hellerstein et al. (2011)), which for racial and ethnic minorities may reflect longstanding structural inequality (Smith (2007)).

Beyond labor economics, our results relate to the burgeoning literature at the intersection of economics and computer science on the effects of including or excluding certain data features used in algorithmic decision-making, especially in settings where concerns about discrimination and equity are important (Kleinberg et al. (2018) and Rambachan and Roth (2020)). Much of this literature has focused on whether algorithms reduce or increase disparities in outcomes across groups, depending on how the baseline decision-maker affects the training data and how outcomes are measured in the training data. Our paper adds an additional dimension to this discussion by suggesting that the impact of new data or algorithms on policy-relevant disparities depends also on how the precision of available information for different groups is affected.

7 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 advantage in information, 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 application to recent 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 we also provide the first evidence of PECC bans' effects on match quality among new hires.

qualitative information sources such as referrals and interviews may be more precise for white than Black job seekers, while precision may be more similar across groups for third-party measures, and for more quantitative measures such as credit reports, job-screening tests, and criminal records. Consistent with this idea, evidence suggests that the precision of commonly used *quantitative* job-screening tests is similar for Black and white applicants (Hartigan and Wigdor (1989), Wigdor and Green (1991), and Jencks and Philips (1998)). This conjecture is also consistent with the results in Agan and Starr (2018), Doleac and Hansen (2020), and Wozniak (2015). However, see also footnote 7 for discussion of other signals not covered by these two broad signal types.

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 particularly 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, including the significant segregation in American society (Boustan (2010), 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 will generally 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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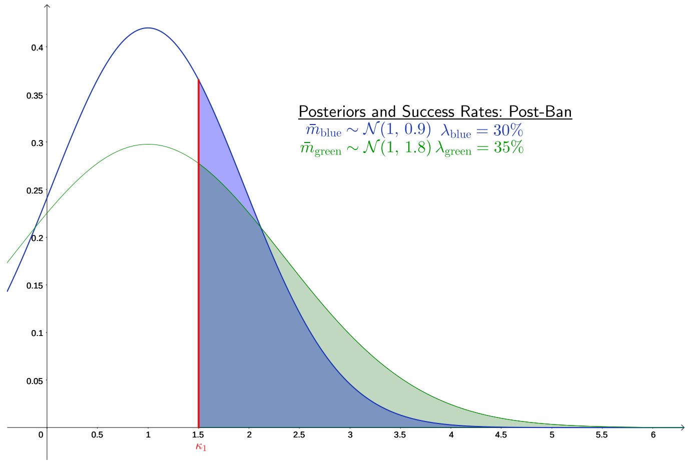
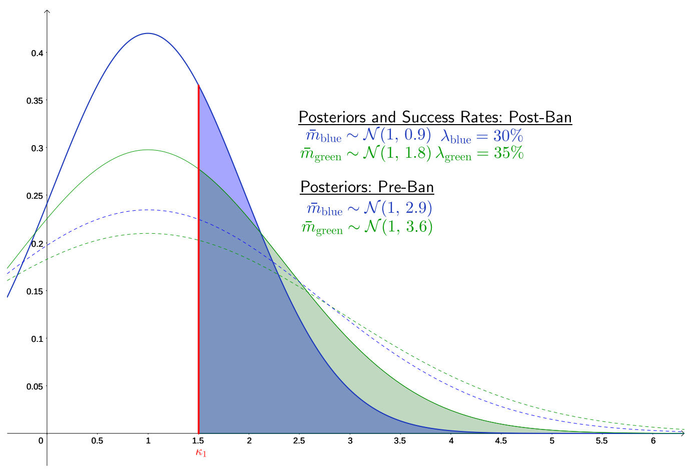
8 Figures

Figure 1: Illustrating the Effects of a Signal Ban

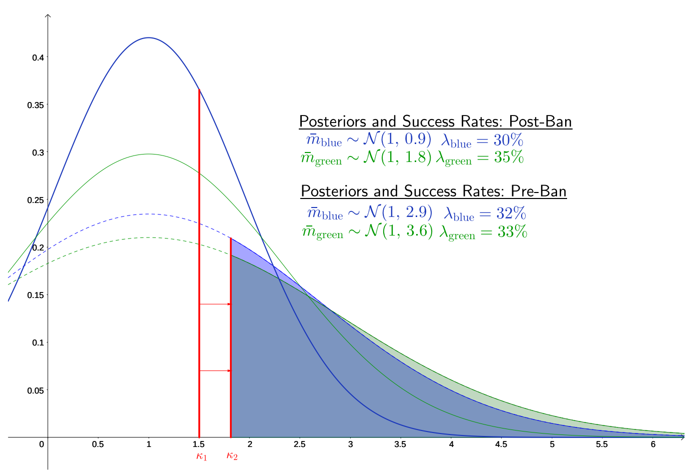
(a) Success Rates under a Signal Ban



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

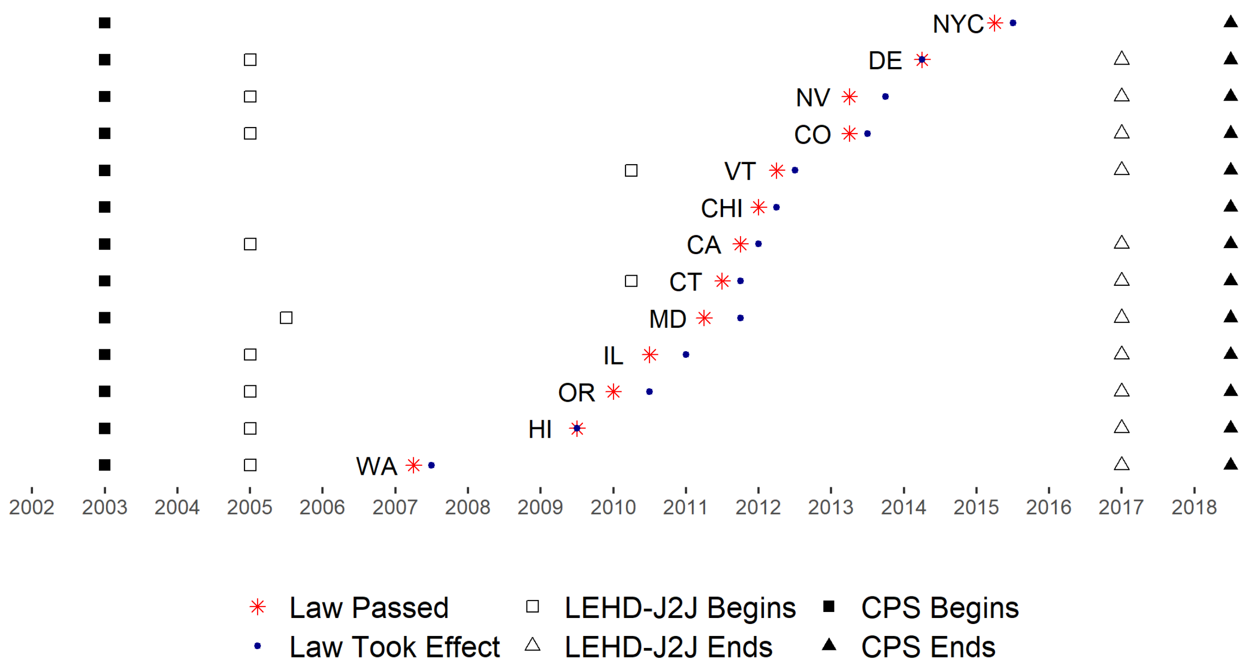


(c) Change in Success Rates when Banned Signal is Available



Notes: This figure graphically illustrates the model developed in Section 2.2. 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. Panel A shows the distribution of a receiver's posteriors for green and blue senders when they do not have access to the banned signal ("post-ban"), and the resulting hiring threshold κ_1. The greater variance of the population distribution of posteriors $\bar{m}$ for green relative to blue reflects the greater precision of baseline screening tools for this group. Panel B then shows the change in the population distribution of posteriors when a ban is not in effect. The greater increase in the variance of these posteriors for blue relative to green reflects that the banned signal is relatively precise for this group. Panel C then shows how the quality threshold shifts to ensure market-clearing for both groups after the banned signal is made available, and how this leads to an increase (resp. decrease) in the success rate for the group for which the banned information provided relatively precise (resp. imprecise) signals.

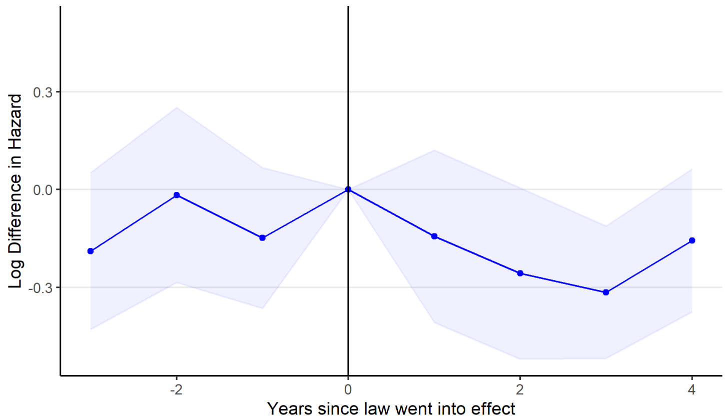
Figure 2: PECC Bans Timeline and Data Availability by State



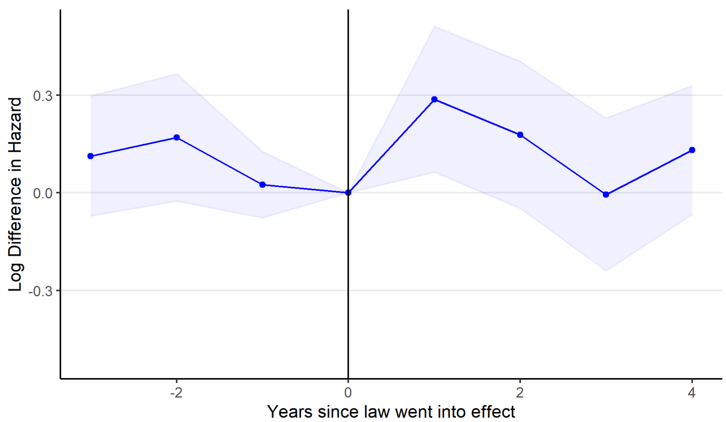
Notes: Oregon's PECC ban originally had an implementation date of 7/10/2010, but implementation was accelerated to 3/29/2010 Friedberg et al. (2016). In Delaware, the law only banned credit screening for public jobs and before the first interview; see also Section 3.1.

Figure 3: Event-Time Analysis of the Effect of PECC Bans on Job-Finding: CPS
State-Race/Ethnicity FE, Time-Race/Ethnicity FE

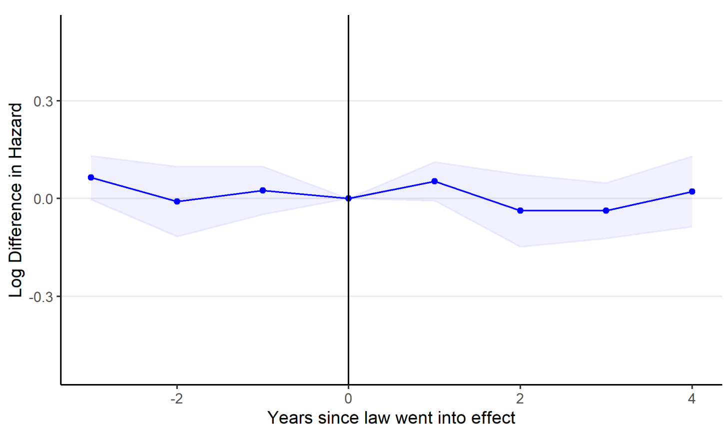
(a) Black



(b) Hispanic



(c) White

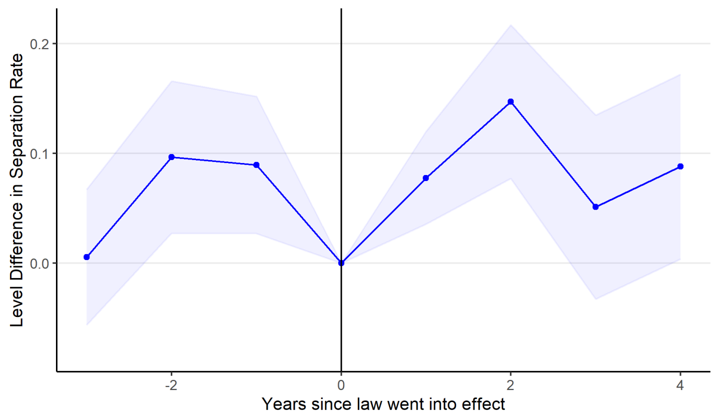


Notes: This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed individuals between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. Each panel shows results for a different race or ethnic group. The reported coefficients come from estimating via MLE a version of the proportional hazards model in equation 4.11 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECC ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race/ethnicity, state-race/ethnicity fixed effects, individual demographic characteristics interacted with race-ethnicity dummies, and state policy and economic controls interacted with race-ethnicity dummies. The sample is restricted to balanced event years common to all PECC-ban states. Microdata on individual unemployment and job-finding come from the Current Population Survey (US Census Bureau (2019)). Error bars show 95 percent confidence intervals generated from standard errors clustered at the state level.

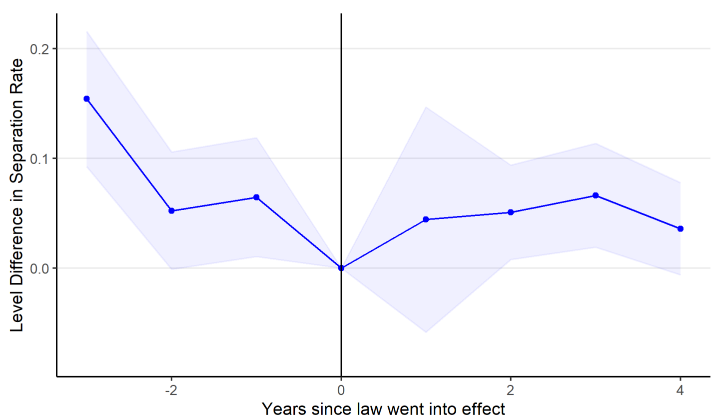
Figure 4: Event-Time Analysis of the Effect of PECC Bans on Involuntary Separations: New Hires

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

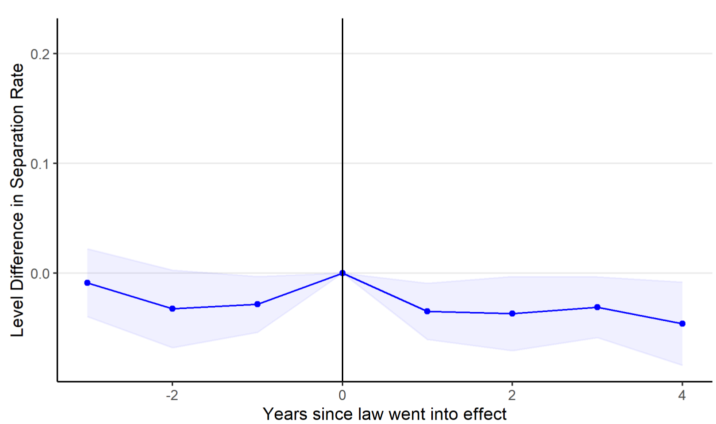
(a) Black



(b) Hispanic



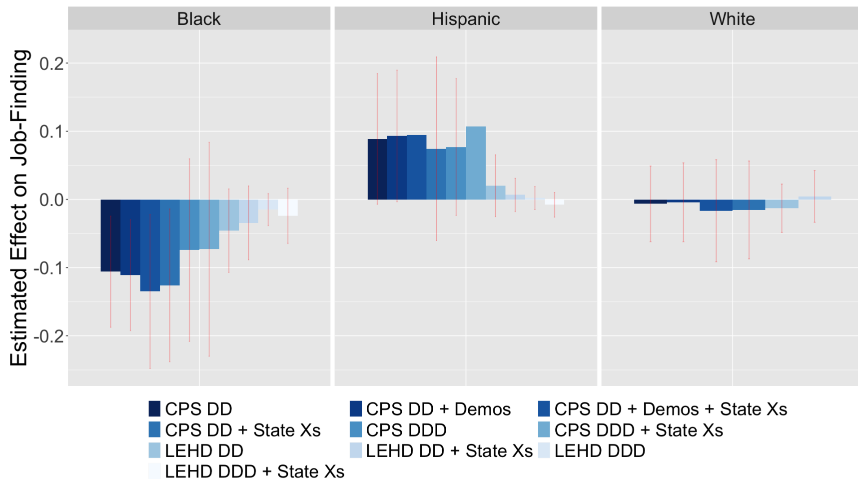
(c) White



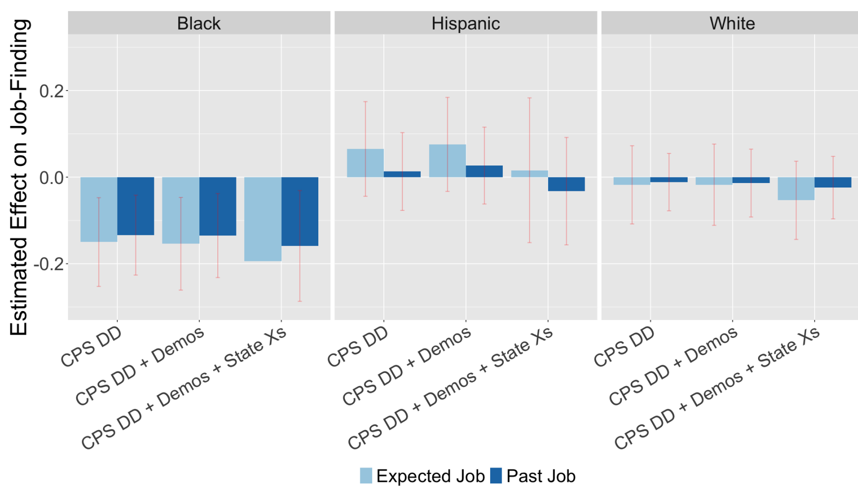
Notes: This figure shows the results of an event-time analysis of the difference in involuntary separation rates for workers newly hired out of unemployment between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. Each panel shows results for a different race or ethnic group. The reported coefficients come from estimating a modified linear probability model of equation 4.2 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECC ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race/ethnicity, state-race/ethnicity fixed effects, individual demographic characteristics interacted with race-ethnicity dummies, and state policy and economic controls interacted with race-ethnicity dummies. The sample is restricted to balanced event years common to all PECC-ban states. Microdata on individual unemployment and involuntary separation rates for new hires come from the Current Population Survey (US Census Bureau (2019)). Error bars show 95 percent confidence intervals generated from standard errors clustered at the state level.

Figure 5: Robustness of Estimated Effects of PECC Bans

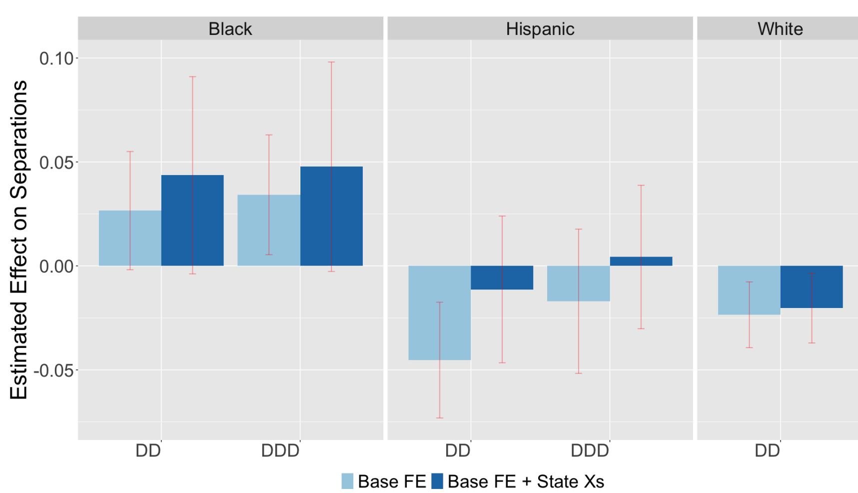
(a) Job-Finding using State-Level Variation



(b) Job-Finding using Job-Level Variation

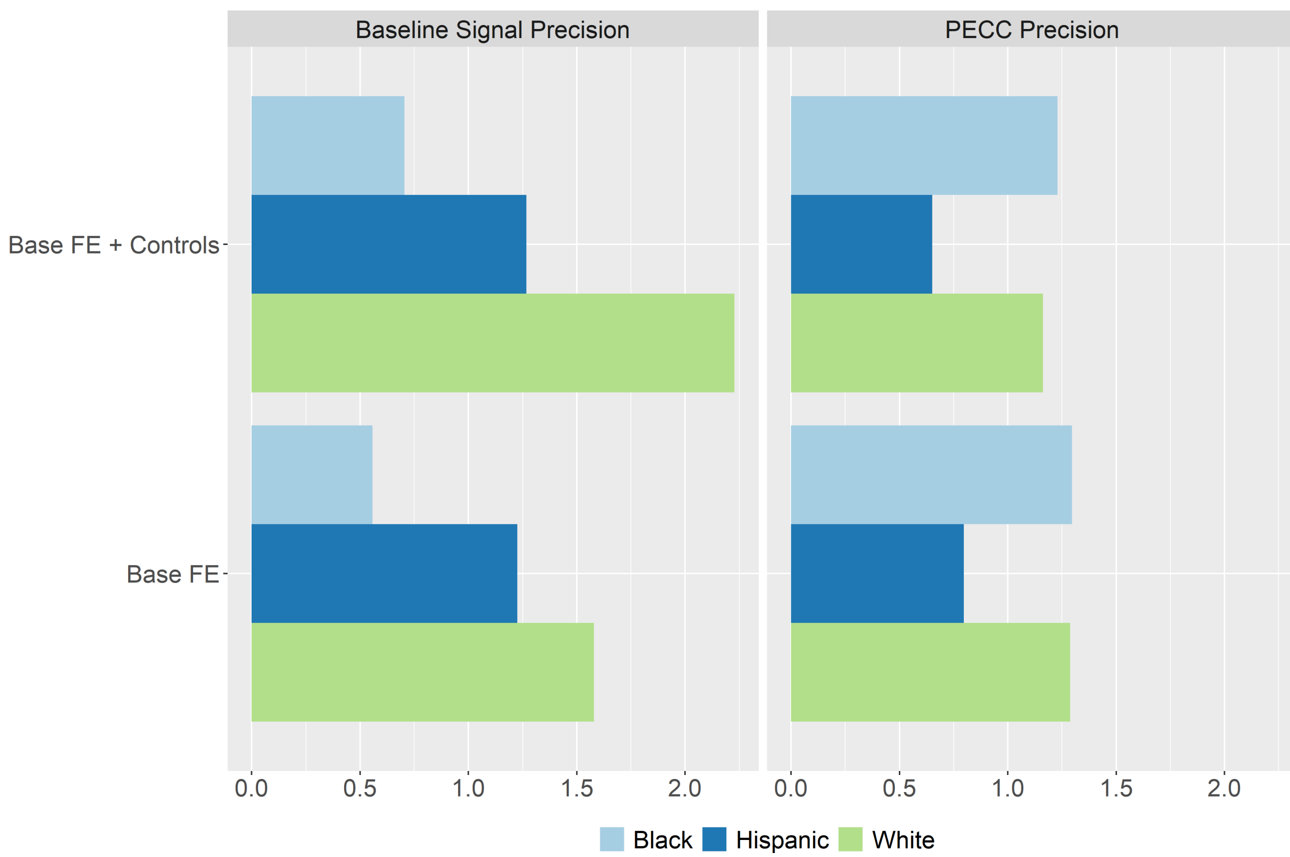


(c) Involuntary Job Separations



Notes: This figure reports the robustness of our estimates of the effect of bans of Pre-Employment Credit Checks (PECCs) to alternative modeling choices and data. Panel A shows estimates using state variation. The different bars report specifications that differ in what controls are included (demographic and/or state economic and policy controls), which specification is used (difference-in-differences as in equation 4.2 or triple-differences as in equation 4.3), and which dataset is used. Panel B reports the robustness of results using job-variation in exposure to PECC bans to different definitions of job-exposure (past vs. predicted job) and specification (DD vs. DDD). Panel C reports the robustness of our estimates of the effect of PECC bans on job separations to alternative controls and specification (DD vs. DDD). Error bars show 95 percent confidence intervals generated from standard errors clustered at the state level.

Figure 6: 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.

9 Tables

Table 1: PECC Bans: Exempted Jobs and Industries

		HI (1)	OR (2)	IL (3)	MD (4)	CT (5)	CA (6)	CHI (7)	VT (8)	CO (9)	NV (10)	NYC (11)
Panel A. Exempted Jobs / Job Duties												
Management	Set the direction of a business or business unit	X		X	X	X	X	X		X	X	
	Access to high-level trade secrets			X	X	X	X	X			X	X
	Access to corporate financial info			X								
	Access to payroll info					X			X	X		
	Provide administrative support for executives									X		
	Direct employees using independent judgment	X									X	
Legal	Law enforcement		X	X			X	X	X	X	X	X
Confidentiality	Access to clients' financial info (non-retail)		X	X	X	X	X	X	X	X	X	
	Access to clients' personal confidential info			X		X				X	X	
Fiduciary	Signatory power / custody of corporate accounts			X	X	X	X	X	X	X	X	X
	Unsupervised access to marketable assets			X		X		X				
	Unsupervised access to cash			X		X	X					
Miscellaneous	Control over digital security systems											X
	Airport security		X									
Panel B. Exempted Industries												
Finance	Banking and related activities	X	X	X	X	X	X	X	X	X	X	
	Savings institutions, including credit unions	X	X	X	X	X	X	X	X	X	X	
	Securities, commodities, funds, trusts, etc.				X	X	X			X		
	Insurance carriers and related activities			X		X	X	X		X		
Law Enforcement	Law Enforcement and Corrections			X				X		X		
	Department of Natural Resources			X								
Miscellaneous	Gaming										X	
	Space Research									X		
	National Security									X		
	Debt Collection			X				X				
	Other state and local agencies			X								

Notes: Marketable assets are e.g. museum/library collections, pharmaceuticals, and exclude furniture and equipment. Table excludes Washington, which passed similar legislation on 4/18/07, taking effect 7/22/07.

Table 2: Characteristics of PECC and non-PECC Ban States and Jobs

	PECC-Ban States (1)	Non-PECC-Ban states (2)	Covered Jobs (Within PECC-ban states) (3)	Exempted Jobs (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	$655	$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	$633	$571	$847
Share of Workers with 4-year College Degree	14%	17%	9%	26%
Panel A1. Whites				
White Share of State Adult Population	70%	75%		
White Employment Rate	94%	95%		
Share of White Unemployed Covered by Ban	65%	0%		
Average Weekly Wage ($)	$989	$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	60%	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 3: Dependent Variable Summary Statistics: CPS and LEHD-J2J Data

	PECC-Ban States	Non-PECC-Ban states	Covered Jobs	Exempted Jobs
			(With PECC-ban states)	
	(1)	(2)	(3)	(4)
Panel A: Blacks				
Job-Finding Rate out of Unemployment (CPS)	0.131	0.155	0.192	0.150
Involuntary Separation Rate, New Hires (CPS)	0.110	0.090	0.112	0.109
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.027	0.022	0.026	0.027
Separation Rate (LEHD)	0.098	0.098		
Adjacent Quarter Job-Finding Rate (LEHD)	0.234	0.252		
Panel B: Hispanics				
Job-Finding Rate out of Unemployment (CPS)	0.184	0.229	0.259	0.198
Involuntary Separation Rate, New Hires (CPS)	0.099	0.076	0.097	0.125
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.024	0.018	0.024	0.022
Separation Rate (LEHD)	0.084	0.096		
Adjacent Quarter Job-Finding Rate (LEHD)	0.226	0.234		
Panel C: Whites				
Job-Finding Rate out of Unemployment (CPS)	0.153	0.192	0.211	0.168
Involuntary Separation Rate, New Hires (CPS)	0.087	0.069	0.079	0.113
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.019	0.014	0.019	0.018
Separation Rate (LEHD)	0.067	0.068		
Adjacent Quarter Job-Finding Rate (LEHD)	0.218	0.229		

Notes: This table shows summary statistics for our outcome variables, job-finding and separation rates, in both the CPS and LEHD-J2J data. From the CPS, we report job-finding rates, separation rates for recent hires, and separation rates for long-tenure workers, by race or ethnicity for years 2003 to 2018. Recent hires are defined as individuals observed with previous unemployed-to-employed transitions in up to 15 months of history in the CPS panel. Long-tenure workers are defined as individuals observed as employed at all prior available dates in the CPS panel. From the LEHD-J2J data, we report separation rates and adjacent quarter job-finding rates for quarters from 2005Q1 until 2017Q1. The separation rate is computed as the number of separations divided by beginning-of-quarter employment and the adjacent quarter job-finding rate is computed as the number of people who separate to adjacent quarter employment divided by total separations. Three treated states (Vermont, Washington, Connecticut) are not included in the LEHD-J2J rows due to data limitations. For both data sources, columns (1) and (2) respectively show statistics for states with and without PECC bans. Columns (3) and (4) then compare covered vs. exempted jobs within PECC-ban states for the CPS dependent variables (occupation data are not available in the LEHD-2J2, preventing us from computing averages separately for covered and exempted jobs). Different panels report dependent variable means separately separately by race and ethnicity, with Panels A, B, and C showing averages for Black, Hispanic, and White workers and job-seekers respectively.

Table 4: Impact of PECC Bans on Job-Finding: State and Job-Level Variation

	(1)	(2)	(3)
Panel A. State-level Variation			
Panel A1. Effect separately by race/ethnicity			
1(Black)*1(Treated by Ban)	-0.106**	-0.111***	-0.135**
	(0.0416)	(0.0415)	(0.0576)
1(Hispanic)*1(Treated by Ban)	0.0887*	0.0931*	0.0946
	(0.0489)	(0.0492)	(0.0716)
1(Non-Hispanic white)*1(Treated by Ban)	-0.00649	-0.00417	-0.0166
	(0.0283)	(0.0295)	(0.0382)
Panel A2. Overall Effect			
1(Treated by Ban)	0.00537	0.00719	-0.0155
	(0.0280)	(0.0297)	(0.0357)
N	342,049	342,049	342,049
States	51	51	51
Ban States	10	10	10
Panel B: Job-Level Variation			
Panel B1. Effects separately by race/ethnicity			
1(Black)*1(Treated by Ban)	-0.150***	-0.154***	-0.194**
	(0.0523)	(0.0546)	(0.0805)
1(Hispanic)*1(Treated by Ban)	0.0651	0.0756	0.0159
	(0.0558)	(0.0554)	(0.0853)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0178	-0.0175	-0.0536
	(0.0461)	(0.0479)	(0.0461)
Panel B2. Overall Effect			
1(Treated by Ban)	-0.0104	-0.00767	-0.0632
	(0.0415)	(0.0450)	(0.0402)
N	330,744	330,744	330,744
States	50	50	50
Ban States	9	9	9
Time-Race/Ethnicity Fixed Effects	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	Y	Y
State Policy/Economic Controls (-Race/Ethnicity)	N	N	Y
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y	Y

Notes: This table reports MLE 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 (Equation 6.11 in the text). Data are from the CPS for years 2003 to 2018. Column (1) includes the state-race/ethnicity and time-race/ethnicity fixed effects that implement difference-in-differences, while Column (2) adds demographic controls fully interacted with race or ethnicity group, which include binned education, binned age, gender, and marital status, urbanicity, and interactions between month-of-year and Census division. Column (3) adds controls for state economic and policy variables. In Panel B, Columns (1)-(3) also include state-job-race/ethnicity fixed effects. In Panel A, a job-seeker's exposure to a PECC ban is determined by whether or not he lives in a state that implemented a PECC ban. In Panel B, a job-seeker's exposure to a PECC ban is determined by whether her expected next job (as defined in footnote 22 in the text) is covered by or exempted from a PECC ban. Standard errors clustered at the state level are shown in parentheses. The controls for state economic and policy variables are: 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 measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). All controls (individual and state policy/economic) are interacted by race-ethnicity dummies.

Table 5: Impact of PECC Bans on Involuntary Separation Rates for New Hires

	(1)	(2)	(3)	(4)
Panel A: Effect separately by race/ethnicity				
1(Black)*1(Treated by Ban)	0.0266*	0.0436*	0.0485***	0.0787***
	(0.0145)	(0.0242)	(0.0164)	(0.0273)
1(Hispanic)*1(Treated by Ban)	-0.0453***	-0.0113	-0.0448***	-0.0157
	(0.0142)	(0.0180)	(0.0159)	(0.0181)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0235***	-0.0203**	-0.0199**	-0.0162**
	(0.00807)	(0.00855)	(0.00756)	(0.00764)
Panel B: Overall effect				
1(Treated by Ban)	-0.0228***	-0.00910	-0.0191**	-0.00186
	(0.00751)	(0.0104)	(0.00822)	(0.00915)
N	54,160	54,160	52,177	52,177
States	51	51	50	50
Ban States	10	10	9	9
Treatment Level	State	State	New Job	New Job
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	N	N
State-New Job-Race/Ethnicity Fixed Effects	N	N	Y	Y
Demographic Controls (-Race/Ethnicity)	N	Y	N	Y
State Policy/Economic controls (-Race/Ethnicity)	N	Y	N	Y

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) and (2) use state-time difference-in-differences, while Columns (3) and (4) use state-job-time difference-in-differences. Data are from the CPS for years 2003 to 2018. Columns (1) and (3) include the state-(job)(-race/ethnicity) and time-race/ethnicity fixed effects that implement difference-in-differences, while Columns (2) and (4) 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. The set of extra controls for state economic and policy variables is: Saiz's price elasticity of housing multiplied by year dummies (Saiz (2010)), a dummy variable that equals 1 if the state was actively extracting oil with 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 (most states expanded in January of 2014), and a measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). In the state-job-time difference-in-differences model, a new hire's exposure to a PECC ban is determined by whether her new job is covered by or exempted from a PECC ban. New hires are defined as individuals observed with previous unemployed-to-employed transitions in up to 15 months of history in the CPS panel. 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 6: Impact of PECC Bans on Involuntary Separation Rates for Long-Tenure Employees

	(1)	(2)	(3)	(4)
Panel A: Effect separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.000967	0.000573	-0.000935	0.000569
	(0.00218)	(0.00227)	(0.00272)	(0.00307)
1(Hispanic)*1(Treated by Ban)	-0.00135	-0.000528	-0.00191	-0.00132
	(0.00206)	(0.00267)	(0.00245)	(0.00289)
1(Non-Hispanic white)*1(Treated by Ban)	-0.00150**	-0.000330	-0.00295***	-0.00197*
	(0.000708)	(0.000992)	(0.000995)	(0.00111)
Panel B: Overall effect				
1(Treated by Ban)	-0.00143*	-0.000267	-0.00251**	-0.00160
	(0.000766)	(0.000944)	(0.000997)	(0.00109)
N	4,432,444	4,432,444	4,310,058	4,310,058
States	51	51	50	50
Ban States	10	10	9	9
Treatment Level	State	State	Current Job	Current Job
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	N	N
State-New Job-Race/Ethnicity Fixed Effects	N	N	Y	Y
Demographic Controls (-Race/Ethnicity)	N	Y	N	Y
State Policy/Economic controls (-Race/Ethnicity)	N	Y	N	Y

Notes: This table re-estimates the difference-in-differences models from Table 5 on a placebo sample of long-tenure workers. Long-tenure workers are defined as individuals observed as employed at all prior available dates in the CPS panel. As in Table 5, Columns (1) and (3) include the state-(job)(-race/ethnicity) and time-race/ethnicity fixed effects that implement difference-in-differences, while Columns (2) and (4) add demographic controls and state-time policy and economic controls. Data from Washington are excluded from Columns (3) and (4) due to uncertainty about which jobs are exempted from Washington's ban. Standard errors clustered at the state level are shown in parentheses. The set of extra controls for state economic and policy variables is: Saiz's price elasticity of housing multiplied by year dummies (Saiz (2010)), a dummy variable that equals 1 if the state was actively extracting oil with 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 (most states expanded in January of 2014), and a measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). All controls (individual and state policy/economic) are interacted by race-ethnicity dummies.

Table 7: Impact of PECC Bans on Job-Finding by Other Observable Subgroups

Subgroup:	High Education		High Experience	
	(1)	(2)	(3)	(4)
Panel A: by Subgroup				
1(Subgroup = 0)*1(Treated by Ban)	-0.0139	-0.0387	0.0216	-0.00691
	(0.0274)	(0.0374)	(0.0356)	(0.0498)
1(Subgroup = l)*1(Treated by Ban)	0.0580	0.0507	0.00168	-0.0182
	(0.0462)	(0.0502)	(0.0345)	(0.0376)
p-value of differences:	0.08	0.04	0.67	0.81
Panel B: by Subgroup * Race/Ethnicity				
1(Subgroup = 0)*1(Treated by Ban)*1(Black)	-0.131**	-0.169**	-0.0845	-0.118
	(0.0563)	(0.0732)	(0.128)	(0.127)
1(Subgroup = l)*(Treated by Ban)*1(Black)	0.0206	0.0221	-0.113**	-0.142**
	(0.122)	(0.106)	(0.0439)	(0.0572)
p-value of differences:	0.33	0.19	0.84	0.85
1(Subgroup = 0)*1(Treated by Ban)*1(Hispanic)	0.0752	0.0802	0.0936*	0.0839
	(0.0512)	(0.0730)	(0.0566)	(0.0746)
1(Subgroup = l)*1(Treated by Ban)*1(Hispanic)	0.232***	0.234***	0.0868	0.0974
	(0.0699)	(0.0908)	(0.0531)	(0.0751)
p-value of differences:	0.03	0.04	0.90	0.81
1(Subgroup = 0)*1(Treated by Ban)*1(Non-Hispanic White)	-0.0292	-0.0380	0.0161	-3.38e-05
	(0.0245)	(0.0396)	(0.0462)	(0.0600)
1(Subgroup = l)*1(Treated by Ban)*1(Non-Hispanic White)	0.0300	0.0287	-0.0115	-0.0219
	(0.0472)	(0.0533)	(0.0300)	(0.0358)
p-value of differences:	0.16	0.27	0.55	0.35
N	342,049	342,049	342,049	342,049
States	51	51	51	51
Ban States	10	10	10	10
Treatment Level	State	State	State	State
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-New Job-Race/Ethnicity Fixed Effects	N	N	N	N
Demographic Controls (-Race/Ethnicity)	N	Y	N	Y
State Policy/Economic controls (-Race/Ethnicity)	N	Y	N	Y

Notes: This table reports MLE estimates of alternative-subgroup-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 (Equation 6.11 in the text). Data are from the CPS for years 2003 to 2018. Columns (1) and (3) include the state-subgroup and time-subgroup fixed effects that implement difference-in-differences, as well as demographic controls fully interacted with the subgroup for the given column, which include binned education, binned age, gender, and marital status, urbanicity, and interactions between month-of-year and Census division. Columns (2) and (4) include extra controls for state economic and policy variables interacted with the subgroup. Each subgroup is binned into two categories and we report both the interaction of those subgroups with an indicator for being treated by a PECC ban but also the p-value of the difference between the two-values of the subgroup. High education is defined as having a four-year college degree or more; high experience is defined as having six or more years of potential experience. Panel A includes the subgroup alone interacted with being exposed to a PECC ban. In Panel B, we fully interact the given subgroup with race/ethnicity and being exposed to a PECC ban. The set of extra controls for state economic and policy variables is: Saiz's price elasticity of housing multiplied by year dummies (Saiz (2010)), a dummy variable that equals 1 if the state was actively extracting oil with 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 (most states expanded in January of 2014), a measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)), and share of the 2000 population that was foreign born and Hispanic interacted with year dummies. All controls (individual and state policy/economic) are interacted with subgroup and race-ethnicity dummies.

Table 8: 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	0.163	0.1033	0.2397	0.0951	0.1876	0.0627
After PECCs Ban	0.1303	0.1375	0.2449	0.0852	0.1929	0.0546
Panel B: No Heterogeneity in PECCs Precision						
With PECCs	0.1612	0.1068	0.2458	0.0793	0.187	0.0633
After PECCs Ban	0.1303	0.1375	0.2449	0.0852	0.1929	0.0546
Panel C: No Heterogeneity in Baseline Precision						
With PECCs	0.1758	0.0648	0.2464	0.0698	0.1842	0.0649
After PECCs Ban	0.1743	0.0587	0.2525	0.0533	0.1836	0.0579

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.