For Online Publication: Appendix for Deleting a Signal: Evidence from Pre-Employment Credit Checks

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In this appendix, we present several extensions of the model and empirical application, and we provide more detailed discussions of our data, empirical methods, and results. Appendix Section A discusses our theoretical model in more detail, and Appendix Section B likewise develops the quantitative model. Next, we provide more information on our empirical application in Appendix Section C, where we provide an alternative way of interpreting the magnitudes in our estimates, discuss the robustness of our results to alternative empirical approaches, presents results from placebo PECC bans, and present estimates of the effect of PECC bans on additional outcomes such as wages and part-time employment. In Appendix Section D, we discuss our data sources and sample construction in detail. Finally, in Appendix Section E we discuss the connection of our approach and results to the wider literature in more detail.

A   Model Details

This appendix section presents further details on the model from Section 2.1. At the heart of the model is a signal extraction problem across information sources, or signals, indexed by k. We decompose the noise in each signal in a standard way: for sender i, the realization of signal k is,

$$s_{i,k} = \mu_i + \delta_k + \epsilon_{i,k} \tag{A.1}$$

where μ_i is the individual's true match quality, δ_k is a potential mean bias in signal k, and $\epsilon_{i,k}$ is signal noise that has mean zero across individuals. We begin with the case of unbiased signals, and we generalize to the case of $\delta_k \neq 0$ later in this appendix.

Signals differ in their precision h_k, or the inverse variance of their signal noise: $h_k \equiv 1/\text{Var}(\epsilon_{i,k})$. We suppose individuals are members of different groups g and signal precisions potentially differ across groups, $h_{g,k} \neq h_{g',k}$.

Following the normal-normal parametrization described in the text, the distribution of match qualities and signal realizations can then be written in terms of $h_{g,k}$, group-specific mean match qualities $\mu_{g,0}$ and inverse variance of match qualities $h_{g,0}$,

$$\mu_i \sim \mathcal{N}\left(\mu_{g(i),0}, 1/h_{g(i),0}\right) \tag{A.2}$$

$$s_{i,k} \sim \mathcal{N}\left(\mu_i, 1/h_{g(i),k}\right) \tag{A.3}$$

with the noise in each signal having been partialled out from the noise in other signals, such that signal noise can without loss of generality be treated as i.i.d. within group g. In such a setting, a lender's Bayesian posterior m_i about the unobserved μ_i after observing the realization of one or more signals $s_{i,k}$ for individual i is,

$$m_i \sim \mathcal{N}\left(\frac{h_{g(i),0} \times \mu_{g(i),0} + \sum_k h_{g(i),k} \times s_{ik}}{h_{g(i),0} + \sum_k h_{g(i),k}}, \frac{1}{h_{g(i),0} + \sum_k h_{g(i),k}}\right) \tag{A.4}$$

With risk-neutral receivers, it will be sufficient to keep track of the mean of this posterior distribution for each individual, which we denote $\bar{m}_i$. After integrating over signal noise realizations, the distribution of these expected match qualities across individuals in group g is then,

$$\bar{m}_{i|g(i)=g} \sim \mathcal{N}\left(\mu_{g,0}, \frac{\sum_k h_{g,k}}{h_{g,0}\left(h_{g,0} + \sum_k h_{g,k}\right)}\right) \tag{A.5}$$

This general framework makes it straightforward to study a banned signal and a baseline signal. The baseline signal can be expressed as a composite of all other available signals, with its precision given by the sum of the precisions of all other signals besides the banned signal,

$$h_{g,\text{baseline}} = \sum_k h_{g,k} - h_{g,\text{banned}} \tag{A.6}$$

If we normalize the number of senders in group g' to 1 and let m_g be the mass of senders in group g, then the quality threshold κ in the absence of a signal ban is defined implicitly by,

$$M = \lambda_{g'} + m_g \lambda_g \tag{A.7}$$

$$\lambda_g = 1 - \Phi\left[\frac{\kappa - \mu_{g,0}}{\left(\left(h_{g,\text{baseline}} + h_{g,\text{banned}}\right) \Big/ \left(h_{g,0}\left(h_{g,0} + h_{g,\text{baseline}} + h_{g,\text{banned}}\right)\right)\right)^{1/2}}\right] \tag{A.8}$$

$$\lambda_{g'} = 1 - \Phi\left[\frac{\kappa - \mu_{g',0}}{\left(\left(h_{g',\text{baseline}} + h_{g',\text{banned}}\right) \Big/ \left(h_{g',0}\left(h_{g',0} + h_{g',\text{baseline}} + h_{g',\text{banned}}\right)\right)\right)^{1/2}}\right] \tag{A.9}$$

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where $\lambda_{g'}$ and λ_g are group-specific success rates. Note that the expressions for $\lambda_{g'}$ and λ_g follow immediately from the distribution of posterior means in A.4, from the definition of $h_{g,\text{baseline}}$ in A.6, and from firms' cutoff strategies in terms of κ.

To study the effect of the banned signal, we parameterize the availability of the banned signal using $\alpha \in [0,1]$, and we replace the noise of the banned signal $h_{g,\text{banned}}$ with the term $\alpha h_{g,\text{banned}}$ in expressions A.8 and A.9 above. The case of a complete ban corresponds to $\alpha = 0$, and the case where the banned signal is available corresponds to $\alpha = 1$. We are then interested in the total derivative $d\lambda_g / d\alpha$ evaluated at various values of α. By differentiating A.7 with respect to α and isolating an expression for $\partial \kappa / \partial \alpha$ that we substitute into that total derivative, we obtain

$$\frac{d\lambda_g}{d\alpha} = \frac{\partial \lambda_g}{\partial \alpha} + \frac{\partial \lambda_g}{\partial \kappa} \left[\frac{-\frac{\partial \lambda_{g'}}{\partial \alpha} - m_g \frac{\partial \lambda_g}{\partial \alpha}}{\frac{\partial \lambda_{g'}}{\partial \kappa} + m_g \frac{\partial \lambda_g}{\partial \kappa}} \right] \tag{A.10}$$

After differentiation, some manipulation, and the introduction of a common denominator $\frac{\partial \lambda_{g'}}{\partial \kappa} + m_g \frac{\partial \lambda_g}{\partial \kappa}$ which we note to be negative, the resulting expression can be evaluated at $\alpha = 0$, yielding a result that implies our two main propositions,

$$\frac{d\lambda_g}{d\alpha} > 0 \iff \frac{h_{g,\text{banned}}}{h_{g',\text{banned}}} > \frac{h_{g,\text{baseline}}}{h_{g',\text{baseline}}} \left(\frac{h_{g,0} + h_{g,\text{baseline}}}{h_{g',0} + h_{g',\text{baseline}}} \right) \left(\frac{h_{g',0}}{h_{g,0}} \right) \left(1 + \frac{\Delta\mu}{\kappa - \mu_{g,0}} \right) \tag{A.11}$$

where we define $\Delta\mu = \mu_{g,0} - \mu_{g',0}$. Propositions 1 and 2 follow from expression A.11 by substituting the terms of relative advantage in baseline signals, ω_g, as defined in the text, and, to derive Proposition 1, by particularizing to the case where $h_{g',0} = h_{g,0}$ and $\Delta\mu = 0$.

Next we further generalize the model to include the potential for (1) taste-based discrimination, (2) biased priors, or (3) biased signals.[1] We model each of these as a shift, denoted δ_g, that is added to relevant mean parameters for group g. Concretely, to model (1) we say δ_g is added to priors $\mu_{g,0}$, to signal realizations $s_{g,k}$, and to receivers' post-match perception of actual match qualities; that is to say, taste-based discrimination implies a shift in perceived match quality at all stages of the interaction between senders and receivers. We model (2) as an δ_g-shift in $\mu_{g,0}$ only: that is, priors are biased by δ_g, but signal realizations and post-match perceptions are both unbiased. We model (3) as an δ_g-shift in signal realizations $s_{g,k}$ only: that is, priors and post-match perceptions are both unbiased, but signal realizations are biased.[2]

We show how each of these three types of discrimination or biases δ_g would introduce one additional term for each group in our general main result, Proposition 2. The additional term would take the form $f_g(\delta_g, h)$, where f's argument h contains the precisions of priors and signals across different groups. The rightmost term in Proposition 2, $\left(1 + \frac{\Delta\mu}{\kappa - \mu_{g,0}} \right)$, would then be

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[1] Bohren et al. (2019) provides evidence in favor of the existence of biased priors in a labor market settings and Bohren et al. (2020) argues that some research may mistake these biased priors, or inaccurate statistical discrimination in their terminology, for taste-based discrimination.

[2] A fourth case would be that signals are biased, but receivers know of this bias and adjust for it in forming posteriors. In this case, posteriors and hence success rates would be the same as in the unbiased case.

updated to include f as follows,

$$\left(1 + \frac{\Delta\mu + f_g(\delta_g, h) - f_{g'}(\delta_{g'}, h)}{\kappa - \mu_{g,0} - f_g(\delta_g, h)}\right) \tag{A.12}$$

For the first type of bias, (1) taste-based discrimination, the added term is simply,

$$f_g(\delta_g, h) = \delta_g \tag{A.13}$$

In other words, taste-based discrimination is observationally equivalent in terms of modeled labor market outcomes to an actual δ_g-shift in underlying match qualities. And so, as in our discussion of the term $\Delta\mu$ after Proposition 2, taste-based discrimination can reinforce the effects of relative disadvantage across information sources.

Meanwhile, (2) biased priors and (3) biased signals both imply that the influence of bias δ_g is attenuated by a factor that depends on various signal precisions h. Taking derivatives as in the steps between expressions A.9 and A.11 introduces additional terms in h as well, reflecting how the effect of the availability of the banned signal depends on the amount of bias present. After simplification, the form in expression A.12 holds, and f can be shown to have the following form for a biased baseline signal,

$$f_g(\delta_g, h) = \frac{h_{g,\text{baseline}} + h_{g,\text{banned}}h_{g,\text{baseline}}}{h_{g,0} + h_{g,\text{baseline}}} \tag{A.14}$$

or the following form for a biased prior,

$$f_g(\delta_g, h) = \frac{h_{g,0} + h_{g,\text{banned}}h_{g,0}}{h_{g,0} + h_{g,\text{baseline}}} \tag{A.15}$$

In both cases, the benefit from the availability of the banned signal accrues more to the group that has *relative* advantage in precision, holding other parameters constant.

Through these more general cases, we also see that the availability of the banned signal is more likely to benefit groups that face a relative disadvantage in bias terms, rather than just in precision terms. While we view the key conclusion as similar in both cases – i.e. *relative* advantage is what matters – we can use the quantitative model detailed in the next section to explore whether the bias channel or the relative-precision channel would explain our empirical results better in the context of PECC bans.

B   Quantitative Model Details

In this section we detail the quantitative version of the model introduced in Section 5.5, starting with the case where we assume priors and signals are unbiased. We allow the mean of baseline match qualities to differ across groups. We restrict $h_0^g = h_0^H = h_0^W$ however, as we are aware of no evidence suggesting such dispersion differences are present "before" the use of differently precise screening tools. We emphasize, as in the discussion above, that any differences in mean

baseline match qualities could simply be the result of taste-based discrimination rather than actual difference.

In estimating the model, we identify parameters that generate observed hiring rates and separation rates in the cases with and without PECC bans in place, which correspond to the model cases where $t = 1$ and $t = 0$ respectively. Quantitatively, the key step in this process is to estimate the hiring threshold κ in each case.

To estimate κ, we first calibrate each group's population share m_r using estimates of the unemployed population from Table 1. We calibrate the number of positions to be filled, $M \in [0, 1]$, using Table 2 to estimate monthly flows of new hires from among the unemployed as a share of the unemployed population. We do not change these calibrations as we search across different parameter values.

With those calibrations in hand, we then solve for κ numerically given any putative set of model parameters. For an arbitrary number of groups g, the hiring threshold κ in the more general case is defined implicitly by

$$M = \sum_g m_g \lambda^g \tag{B.1}$$

$$\lambda_g = 1 - \Phi \left[\frac{\kappa - \mu_{g,0}}{\left((h_{g,\text{baseline}} + th_{g,\text{banned}}) \middle/ (h_{g,0} (h_{g,0} + h_{g,\text{baseline}} + th_{g,\text{banned}})) \right)^{1/2}} \right] \quad \forall g \tag{B.2}$$

making it straightforward to solve for κ via simulation.[3]

Model moments are generated as follows. Job-finding rates for each group are simply the share of that group's $m_g N$ posteriors that fall above the estimated κ. Subsequent separation rates are generated by modeling the firm's firing decision as a tradeoff between a given hire's match quality, the expected match quality of making another new hire, and the firing cost $c_{\text{pre-ban}}$ or $c_{\text{post-ban}}$. Specifically, all new hires are fired if their true match quality falls below the quantity,

$$\sum_g m_g \mathbb{E}[\mu_i | \bar{m}_i > \kappa] - c \tag{B.3}$$

where the first term is the expected match quality of a new hire averaged over all possible groups, and c is the firing cost a firm incurs for firing the currently hired worker, equal to either $c_{\text{pre-ban}}$ or $c_{\text{post-ban}}$ depending on the availability of PECCs.[4] The expectation in the first term is likewise solved for numerically using the $m_g N$ draws from each group.

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[3]In particular, to solve for κ, we simulate $m_g N$ draws from each group's match quality distribution, and then simulate signal realizations for each of these draws. We then find the $N \cdot M$ highest values of Bayesian posteriors about these draws' match quality, across all groups g, where posteriors are calculated as in expression A.4 or the analogous expression for the case when PECCs are available. The implicit hiring cutoff κ is then defined by the minimum of these M highest posteriors.

[4]As an alternative to our view that $c_{\text{post-ban}}$ may represent compliance costs after a PECC ban, this cost could also reflect the cost of substitution to an alternative signal. The change in the dispersion in posterior expectations from before to after a PECC ban would then be net of the contribution of this new signal.

The simulated firing and job-finding rates provide a total of twelve model moments, where the twelve is for three groups, two cases with and without PECCs, and two rates. These twelve moments are compared to the twelve counterparts from our empirical work as described in the main body of the text. We search across parameter values to minimize the unweighted sum of squared differences between these twelve moments.

One technical note is relevant. In principle the implementation of a PECC ban will change the composition of match qualities in the applicant pool as firms endogenously change their hiring and firing strategies, and so there will in principle be a gradual rather than immediate convergence path to a new, post-PECC-ban equilibrium. While it is possible to solve for this convergence path in the same simulations above, we believe this extra complexity detracts from the core focus of the quantitative model. Thus we assume that the composition of match qualities in the pool of job-seekers is unchanged under a PECC ban in our model simulations. Equivalently, this can be seen as an assumption that flows into and out of unemployment that are affected by the availability of PECCs are small relative to other flows determining the characteristics of the applicant pool.

Figure 5 reports the model parameter estimates that we particularly highlight in our theoretical framework. Readers may also be interested in the full set of estimated model parameters; for completeness we report those here. We emphasize that the group-specific match qualities reflect any effects of taste-based discrimination as well as institutional discrimination such as inequalities in educational access. When we target our controlled estimates of job-finding and job separation using state-level variation, we estimate,

$$\begin{bmatrix} \mu_{B,0} & h_{B,\text{baseline}} & h_{B,\text{banned}} & h_0 \\ \mu_{H,0} & h_{H,\text{baseline}} & h_{H,\text{banned}} & c_{\text{post}} \\ \cdot & h_{W,\text{baseline}} & h_{W,\text{banned}} & c_{\text{pre}} \end{bmatrix} = \begin{bmatrix} 0.0490 & 0.5243 & 1.0022 & 0.9677 \\ 0.1489 & 1.2216 & 0.7141 & 1.0482 \\ \cdot & 1.3846 & 1.0408 & 0.8955 \end{bmatrix} \quad \text{(B.4)}$$

Meanwhile when we target our controlled-specification coefficients (column (3) of Table 2 Panel A, column (6) of Table 3), we estimate,

$$\begin{bmatrix} \mu_{B,0} & h_{B,\text{baseline}} & h_{B,\text{banned}} & h_0 \\ \mu_{H,0} & h_{H,\text{baseline}} & h_{H,\text{banned}} & c_{\text{post}} \\ \cdot & h_{W,\text{baseline}} & h_{W,\text{banned}} & c_{\text{pre}} \end{bmatrix} = \begin{bmatrix} 0.0810 & 0.4233 & 1.2936 & 0.7771 \\ 0.1745 & 1.2890 & 0.6731 & 1.0671 \\ \cdot & 1.3897 & 1.2110 & 0.9290 \end{bmatrix} \quad \text{(B.5)}$$

We also examine robustness to allowing for biased signals, in the sense of Autor and Scarborough (2008), in addition to differences in signal precision. Given that our estimates in Figure 5 highlight the baseline signal as a source of differences across groups, we introduce a twelfth parameter that allows the baseline signal to be biased by some amount δ, as described above in Appendix Section A, for minority applicants.[5] When we re-estimate the model allowing for this baseline signal bias, we find that our core results on relative advantage in PECC signal precision are largely unchanged, and in fact become slightly stronger in the sense of differences across groups; overall though, these changes are modest, as the magnitudes of estimated precisions

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[5]We obtain very similar results when the bias applies only for Black applicants, rather than all minority applicants

move by less than 11% of their baseline values. While further work is needed to develop empirical strategies for distinguishing the effects of differential noise separately from the effects of bias, this robustness exercise suggests PECCs are one important empirical context where differential signal noise, rather than signal biases in the sense of Autor and Scarborough (2008), are drivers of disparities across groups.

C   Further Empirical Results

In this section, we present additional discussion related to the magnitude of our estimates, and we provide more detail on the robustness of our results to alternative identifying assumptions, different specifications, and alternative datasets that we discussed briefly in Section 5.3. We also present the results from a placebo PECCs ban excercise, as well as evidence on the effects of PECC bans on additional outcomes, including wages and part-time employment.

C.1   Robustness to Treatment Effect Heterogeneity

Two-way fixed effects (TWFE) estimators can be confounded by heterogeneity in treatment effects across treated units when treatment is staggered over time, even when the parallel trends assumption holds (see e.g., Goodman-Bacon (2021), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), Borusyak et al. (2021)). Goodman-Bacon (2021) shows that this bias arises from two sources. First, TWFE models incorporate comparisons of treated units whose treatment starts in one time period to treated units whose treatment starts in a later time period, and this use of treated units as implicit controls can introduce bias of ambiguous sign. However, this bias is negligible in our setting. Using the decomposition of Goodman-Bacon (2021), which quantifies the weight that a TWFE estimator assigns to each possible pairwise comparison in the data (e.g., a comparison between treated unit A and treated unit B), we find that that a linear TWFE estimator in our setting places only 3% of its total weights on pairwise comparisons between treated units. In the phrasing of Baker et al. (2022), this means that the vast majority of our pairwise comparisons come from "clean" pairs.

As a result of this predominance of "clean pairs," we do not expect estimators that correct for the biases above to have a quantitatively important impact on our results. Nevertheless, for completeness, in Table 3 we re-estimate our key regression specifications from sections 4.1 and 4.2 using the robust estimator in Sun and Abraham (2021). Some of these results are discussed in Section 5, but we discuss them again and some further results here for completeness. Columns (1)-(3) report the robustness of our job-finding results, while Columns (4) and (5) report the robustness of our separations results. Panel A reports results using state level variation in exposure to PECC bans, while Panel B reports results using job-level variation in exposure to PECC bans.

We note that Sun and Abraham (2021), and many of the related recent tools developed to solve this challenge, use linear estimators, so in our job-finding survival models we first aggregate the data to use a linear model as in Appendix Section D.4 in Column (2), and we then show robustness to the Sun and Abraham (2021) estimator with that linear model in Column (3). We

compare both of these estimates to our base specification results shown in Column (1). Two patterns stand out regarding these estimates. First, the estimates are qualitatively all quite similar, with all estimates being of the same sign and general magnitude. Second, to the extent that there are differences, these differences are generated by the need to aggregate the data (i.e. in the move from specification (1) to (2)), rather than from using the Sun and Abraham (2021) estimator.

Moving to the job separation outcomes in Columns (4) and (5), we see that using the Sun and Abraham (2021) estimator has a very small effect on the estimates impact of PECC bans. Combined, these results confirm our expectation that biases due to heterogeneous treatment effects and staggered treatment onset are not consequential in our setting.

C.2 Interpreting Magnitudes

In the main text, we compare the estimated effect of PECC bans to demand-side factors, where we estimate the effects of PECC bans are equivalent to the employment declines resulting from a 4.6 percent increase in wages for Black workers. Here we attempt to gauge how these magnitudes compare to those of supply- rather than demand-side policies studied in the literature. Meyer (1990)'s study of unemployment insurance finds that a 10 percent increase in the size of unemployment benefits results in a roughly 8.8 percent decline in the job-finding hazard. Interpreting our results in light of this finding, the effect of PECC bans on Black job-finding rates are equivalent to about a 15 percent reduction in unemployment benefits using our estimates from the CPS and a 4 percent reduction in unemployment benefits using our estimates from the LEHD J2J. In more recent work using data from the Austrian unemployment insurance system, Card et al. (2007) find that eligibility for two months of severance pay results in a roughly 12 percent reduction in the job-finding rate, while extending unemployment insurance benefits from 20 to 30 weeks reduces job-finding hazards by 6 to 9 percent. These estimates are similar in magnitude to our estimates of the impact of PECC bans.

As we discuss in the main text, we prefer to estimate the long-run equilibrium effect on employment rates using the effect on job-finding and separation rates rather than directly estimating the effect on employment levels because it may take some time for equilibrium employment rates to converge to their steady state value after a change in job-finding and separation rates. However, for completeness, Appendix Table 4 reports estimates of the effect of PECC bans on employment rates. In our baseline specification using state variation in PECC bans without demographic or state policy and economic controls, reported in Column (1), we estimate that PECC bans reduce Black employment rates by about 0.8 percentage points, while having a positive effect on Hispanic employment rates and small effects on White employment rates. This estimated effect on Black employment levels is roughly half of the long-run equilibrium effect implied by the estimated effects on job-finding and separations. This smaller magnitude is consistent with incomplete convergence to the new steady state, reflecting the medium run time-horizon of our data.

C.3   LEHD J2J Data Results

We begin by studying job-finding in our supplementary dataset, the LEHD J2J data described earlier in Section D.6. Although the LEHD J2J does not provide the rich individual-level demographics or information on spell length that the CPS does, the fact that it is aggregated from administrative data on the near-universe of state unemployment-insurance records makes it a valuable source of additional information. We follow the empirical strategy detailed in Appendix Section D.4, where we estimate a difference-in-differences model for the complementary-log-log of observed job-finding rates for unemployed job-seekers at the state-race/ethnicity-time level.

Figure 3 reports estimates from event-time versions of Equations 4.3 and 4.6 using the LEHD J2J, where we plug in observed job-finding rates $b_{s,r,t}(\tau)$ for the outcome $\lambda^d_{s,r,t}(\tau)$ and estimate via OLS. Econometric properties of this plug-in estimator are addressed in more detail in Appendix Section D.4. After PECC bans went into effect, we see that the decline in Black job-finding rates accelerated, whereas job-finding rates for both Hispanic and white job-seekers rose for a period and then fell slightly.

We explore the robustness of these results to alternative identifying assumptions. To begin, Panel A, Column (1) in Appendix Table 6 shows coefficient estimates that correspond to the LEHD J2J event-time plots in Appendix Figure 3. Column (2) adds controls for linear trends at the state-race/ethnicity level to explore sensitivity to the baseline difference-in-difference estimator's parallel trends assumption. Column (3) implements the Sun and Abraham (2021) estimator. Column (4) adds state-time fixed effects to Column (1), creating a more-demanding triple-difference estimator as in Equation 4.2. Finally, Column (5) adds state-race/ethnicity linear trends to Column (4). Recall that the triple-difference estimator estimates the effect of PECC bans on the *difference* between Black and white (or between Hispanic and white) job-finding rates. These triple-difference specifications require the weaker identifying assumption that the difference between Black and white (or between Hispanic and white) job-finding rates would have exhibited common trends between states banning and not banning PECCs, in the absence of PECC bans.

The results in Column (2) of the table change relative to Column (1), all becoming more positive relative to the estimates in Column (1), with the sign for Black job seekers actually flipping and becoming positive (although imprecisely estimated). In Column (3) we again show that adjusting for staggered treatment adoption using the Sun and Abraham (2021) estimator has no material effect on our estimates. When we add state-time fixed effects in Column (4), the estimates become more similar to our baseline estimates in Column (1), with the estimated effect for Black job-seekers being around -1.5 log-points, although this estimate is imprecise. Finally, when we add state-race/ethnicity linear trends in Column (5), the point-estimates for Black job-seekers roughly doubles in magnitude relative to Column (4), while the Hispanic estimate also declines to near zero. Columns (6)-(9) reproduce Columns (1)-(2) and (4)-(5), adding the controls for changes in state policy and economic conditions discussed in Section D.1. Adding these controls does not have a clear pattern of effects on the coefficients, increasing some estimates but decreasing others. In general, the overall pattern is similar, with estimates for Black job-seekers showing a moderate, negative effect on job-finding rates.

Overall, these results lend additional credence to our baseline estimates that PECC bans

have a significant negative effect on job-finding rates for Black job-seekers. Seven of the nine estimates of the effect of PECC on Black job-finding in the LEHD J2J are negative, although they are smaller in magnitude than our baseline estimates from the CPS, varying from one-half to one-sixth of our CPS estimates. Furthermore, the most specification adjusting for staggered treatment adoption in Column (3) is both economically large and precisely estimated.

Why are the LEHD-J2J smaller in magnitude (although still generally negative) than the CPS estimates? One possibility is that the differences reflect differences in the types of unemployment spells that are included in the two datasets. The CPS sample that we use in our main analysis differs from the LEHD-J2J data in two ways. First, the CPS data include unemployment spells of all lengths ranging from 1 week to several years (124 weeks). Conversely, the LEHD-J2J data only includes adjacent quarter flows, i.e. people who leave employment at a given firm in quarter t and become re-employed at a different employer in $t + 1$.[6] This excludes both job-seekers who lose a job and find a new one within the same quarter, as well as those who have longer unemployment spells and don't find a new job until after $t + 1$. Second, the CPS data include information on all states (and the District of Columbia) while the LEHD-J2J data exclude 7 states either because the LEHD-J2J data are not available or do not start early enough or because a state's minority population was not large enough, as discussed in Appendix Section D.4.

In Appendix Table 7 we investigate whether the CPS results look more similar to the LEHD-J2J results once we limit the sample to unemployment spells of the same length as those in the LEHD-J2J. Panel A reports results in the LEHD (duplicated from Appendix Table 6. Panel B, Column (1) reports the base specifications using the CPS for comparison. Panel B, Column (2) then reports results where we restrict the CPS sample to more closely match the LEHD-J2J sample. Specifically, we restrict the sample to the same 44 states and the LEHD-J2J data and to unemployment spells that match the type of spells picked up in the LEHD-J2J (i.e., spells where the person starts a quarter unemployed and ends the quarter employed). We see that making this restriction reduces the CPS estimate of the effect of PECC bans on Black job-finding rates from -10.2 percent to -9.1 percent, roughly 20% closer to the LEHD-J2J estimate of -4.6 percent. In Column (3), when we add the state policy and economic conditions controls, the estimate for Black job-seekers is near zero, actually slightly less in magnitude than the -3.4 percent estimate in the LEHD-J2J reducing the absolute magnitude of the gap by about 60%. As in our previous tables, Column (4) shows that the results are not sensitive to using the Sun and Abraham (2021) estimator. Combined, these results suggests that some of the divergence between the CPS and LEHD-J2J is explained by the particular sample where we measure labor market transitions in the LEHD-J2J.

C.4   CPS Data Robustness Results

We also explore the robustness of our results in the full CPS data not restricted to the LEHD-J2J-like subsample. These results are shown in Appendix Table 8. Column (1) repeats our baseline analysis of CPS job-finding from Column (1) of Table 2. Column (2) then adds, as in our LEHD

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[6]There is also a within quarter transition measure in the LEHD-J2J. However, this variable includes both transitions with and without an unemployment spell in the same quarter, so it is not appropriate for our analysis of the effect of PECC bans on job-finding rates of unemployed workers.

J2J results, state-race/ethnicity linear trends to our baseline CPS specification in Equation 4.6. We see that adding state-race/ethnicity linear trends increases the magnitude of the Black and Hispanic estimates of the effect of PECC bans on job-finding, although it also substantially increases the standard errors. Moving to Column (5), where we add state-time fixed effects to the specification from Column (1), turning the specification into a triple-difference estimator, we see that the estimated effect of PECC bans on Black job-seekers is reduced and made noisier, but is still economically significant and is statistically indistinguishable from the estimate in column (1). The estimated effect for Hispanic job-seekers is indistinguishable from the estimate in Column (1). Finally, in Column (6) we add state-race/ethnicity specific linear trends to the triple-difference specification in Column (5). This change roughly triples the estimated effect for Black job-seekers to over 32 log-points, while the estimate for Hispanic job-seekers is now smaller (although imprecise). The patterns in this table are broadly similar to those we observe in the LEHD J2J, further validating our headline CPS results on job-finding. Section C.8 below further discusses the remaining columns in this table.

In Appendix Table 9 we perform the same robustness analysis for the effect of PECC bans on separation rates for new hires. This analysis is particularly important in light of the pre-trends evident for Hispanic new hires in Figure 3. Beginning with Column (2), we see that adding state-race/ethnicity linear trends nearly doubles the magnitude of the point estimates for Black new hires, while the estimated magnitudes for Hispanic and white new hires are sharply reduced, suggesting that the estimates for them are driven by pre-existing trends. This finding is confirmed when we turn to Column (5), which shows that adding state-time effects to Column (1) (thus generating a triple-difference estimator) increases the estimate for Black new hires considerably compared to our baseline specification in Column (1), while it decreases the estimate for Hispanic new hires to about half of its previous size. Adding state-race/ethnicity linear trends in Column (6) further reduces the absolute magnitude of the Hispanic estimate, while increasing the estimate for Black new hires by another fifty percent. Overall, Appendix Table 9 confirms our finding that PECC bans are associated with increases in separation rates for Black new hires, while casting some doubt on there being any particular relationship between PECC bans and separation rates for white and Hispanic new hires. This is the same conclusion reached in our earlier analysis of a placebo sample and of job-level variation, in Section 5.2. Section C.8 below further discusses the remaining columns in this table.

C.5   Race-specific baseline hazards

The model we estimate in our main job-finding specifications, shown in Equation 4.6, imposes the assumption of proportional baseline hazards. Our estimates may be inconsistent if this assumption is violated. In Appendix Table 15, we relax this assumption by allowing baseline hazards to differ flexibly across race. This change has almost no affect on the estimated parameters, suggesting that this specific issue does not have a material impact on our results.

C.6   Restricting to Months 1-4 of CPS

Krueger et al. (2017) provide evidence that in some circumstances attrition during the four month break between CPS survey months 1-4 and 5-8 can lead to attrition bias. We explore the extent to which this issue impacts our estimates in Appendix Table 13, which reports our main job-finding (in Columns (1) and (2)) and separations specifications (in Columns (3) and (4)) using both state-level variation (Panel A) and job-level (Panel B) variation in exposure to PECC bans. The results in this table are qualitatively similar to those in our main specifications. Quantitatively, the job-finding results tend to be larger than our baseline specifications using all of the CPS months suggesting that, if anything, attrition bias due to the rotation group structure is causing us to understate the impact of PECC bans on job-finding. The separations results tend to be smaller in magnitude and less precise than our baseline results.

C.7   Part-Time Employment Results

PECC bans may have different impacts on part-time job finding than overall job-finding for several reasons. First, if PECC bans worsen match quality and part-time jobs indicate poor match quality, PECC bans may increase part-time job finding. Second, if the type of people who tend to get part-time jobs find signaling their productivity particularly difficult, PECC bans may decrease part time job finding disproportionately. Appendix Table 14 estimates hazard models of the effect of PECC bans on the probability of finding a part-time job. The estimates are noisier than our baseline specification, but generally are similar or higher in magnitude to our baseline estimates. This is suggestive evidence that PECC bans may particularly impact those most likely to find part-time work, although the estimates are sufficiently noisy that strong inferences cannot be made.

C.8   Job-Specific Trends

A potential concern with the parallel trends assumptions underlying our difference-in-differences and triple-difference specifications could be whether particular segments of the labor market, which different race or ethnic groups may be differently exposed to, may be experiencing different secular rates of job growth or job losses. We examine whether this is the case and whether this has a material effect on our results in Tables 8 and 9, which present robustness in our job-finding and job-separation results respectively (other aspects of these tables are discussed separately above). The two tables are structured similarly: first, column (1) in both tables repeats our column (1) difference-in-differences analysis from Tables 2 and 3 in the main body, and column (2) then shows robustness to adding state-by-race/ethnicity-specific linear trends; column (4) then repeats our difference-in-difference analysis from Tables 2 and 3 using *job*-level variation, adding state-by-job linear, quadratic, and cubic trends, while column (3) shows robustness to adding state-by-job linear trends along (without the higher-order polynomial); column (5) shows the triple-difference specification discussed in Section C.4 above, and columns (6) and (7) then shows robustness to adding, respectively, state-by-race/ethnicity-specific linear trends and state-by-job linear trends. As these linear trends introduce hundreds or thousands of additional parameters to be estimated,

they often lead to higher standard errors on our primary coefficients of interest. However, we are reassured that these robustness checks either lead to similar or larger (and same-signed) coefficients as in our specifications without these trends.

C.9 Wage Results

We focus on separation rates for new hires as our main measure of the effect of PECC bans on match quality. However, wages are another important measure of match quality. Additionally, general equilibrium effects of PECC bans on non-directly affected workers may also appear in wages. We investigate these potential wage impacts of PECC bans in this section, expanding upon our brief discussion in Section 5.2. Appendix Table 10 reports estimates of the effect of PECC bans on the hourly wages of newly hired workers following PECC bans. These estimates parallel the results in Column (1) in Table 3 and Columns (1), (5), and (6) in Appendix Table 9. Starting with Column (1), we see that the estimated overall effect of PECC bans on hourly wages is small, only 0.2 log-points, and is similarly small when the results are broken down by race or ethnic group, with estimates less than 1 log-point for all three groups. However, it must be noted that the estimates are quite noisy, with large standard errors that make it difficult to rule out economically large results for both Black and Hispanic workers. This imprecision is highlighted by the results in Columns (2) and (3), which add state policy and economic controls and then state-race/ethnicity linear trends, resulting in large changes from specification to specification, without any particular pattern. This inconsistency of the results combined with their imprecision highlights that these data may not be well-suited to study the wage effects of PECC bans on wages.

PECC bans may also affect hourly wages through general equilibrium effects on labor markets. We examine this possibility by looking at the relationship between PECC bans and hourly wages among long-tenure workers in Appendix Table 11. Similar to the results in Appendix Table 10, the results are in general near zero and quite noisy, with the possible exception of Hispanic workers, for whom the estimated is negative, moderate in magnitude, and statistically significant in the first specification. However, the magnitude, significance, and sign of these Hispanic results are inconsistent across the four specifications, making it difficult to interpret the results. Broadly, we view these results as too noisy to use for reliable inference.

One potential concern with the results reported in Appendix Tables 10 and 11 is that hourly wages may be measured with error, particularly for workers who are not paid by the hour. Consequently, the estimates may be attenuated towards zero. In results available upon request, we explore this possibility by estimating the effect of PECC bans on weekly wages and hourly wages for the subset of workers paid by the hour. Although the exact estimates differ, the broad pattern of small, noisy, and inconsistent results we found in 10 and 11 is the same for these two additional sets of analyses. Definitive analysis of the effect of PECC bans on wages will have to wait for different empirical strategies or datasets.

C.10 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 specification 4.1 where we now add interactions with categories of education or experience: having a college degree, having six years or more of potential experience, or having high predicted wages.[7]

Table 12 Panel A reports estimates on overall job-finding (i.e. not interacted with 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. Turning to Columns (5) and (6), we see that PECCs bans have negative estimated effects overall on job-finding for low predicted-wage workers and positive or near zero estimated effects for low predicted-wage workers, but the differences between these two groups are not precisely estimated.

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.

Finally, in Columns (5) and (6) we see that, like in the case of education, the effects of PECCs bans are more negative for job-seekers with low predicted wages, particularly for Black job-seekers. This finding is consistent with workers with higher predicted wages having relative advantage in the precision of baseline non-PECC signals or in the precision of PECCs. Alternatively (or in addition), this pattern could reflect the potential ability of high-wage workers

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[7]We predict wages by estimating a Mincer-style wage regression, regressing log hourly wages on education, potential experience, years of education interacted with potential experience, potential experience squared, years of education squared, and two-digit industry and occupation dummies and interactions between industry and occupation dummies. We classify workers having above median predicted wages as having "high" predicted wages and workers having below median predicted wages as having "low" predicted wages.

to substitute towards applying to lower-wage jobs in response to PECC bans, while lower wage workers have less ability to make such adjustments. However, we emphasize that, although the differences between high and low-predicted wage workers are economically large for all Black and Hispanic job-seekers, the difference is not precisely estimated for any racial or ethnic group.

C.11   Placebo PECC Ban Treatments

In this section, we report results of placebo treatment analyses where we randomly assign treatment status to different states, jobs, and time periods and assess where our actual treatment effect estimates fall in the overall distribution of potential treatment effect estimates. Specifically, we repeatedly randomly assigned treatment status to 10 states (the overall number treated in our balanced sample), to a share of jobs approximately equal to the average share of jobs covered by an actual PECC ban (making these only approximately equal is necessary because of the discreteness of job groups), and randomly assigned treatment timing for each of these states (within the range of treatment starts dates observed in our data). We then estimated our main difference-in-differences models using both state-level and job-level variation for both our job-finding and separations outcomes following our main specifications in Tables 2 and 3. These regressions correspond to Column (4) in Panels A and B of Table 2, and to Columns (3) and (6) of Table 3. We performed 500 such simulations for each outcome and estimator.

We report the distribution of treatment effect estimates in Appendix Figures 5, 6, 7, and 8. In each of these figures, we show the distribution of estimates for Black, Hispanic, and White job-seekers in Panels A, B, and C respectively. For comparison, we show our treatment effect estimate in our actual sample with a dotted red line. In all four figures, we see that the simulated treatment effect estimates of the effect of PECC bans on job-finding for all three races are clustered around 0, with most of the mass lying between -.2 and .2 for job-finding rates, or between -.05 and .05 for job-separation rates.

Looking more closely at Figures 5 and 6, which respectively show simulated state-level and job-level variation, we see that our actual point estimates of -.129 and -.183 for Black job-seekers' job finding rates using state-level and job-level variation are smaller than most of the mass; for state-variation the two-sided p-value is .168, and when using job-level variation the two-sided p-value is .058. Conversely, and consistent with our baseline findings for these groups, our actual point estimates for white job-seekers and Hispanic job-seekers are closer to the center of the mass and have p-values of .998, .632, .364, and .198 across the two groups and two levels of variation.

Appendix Figures 7 and 8 then report the same placebo analysis for our job separations specifications. Here we again find that our point estimate for Black households is mostly outside the main mass of the simulations, especially for job-level variation where our simulations imply a two-sided p-value of .004. For Hispanic and white separation rates, our estimates are again within the main mass of the simulations, though the modest decrease we estimate in white separation rates has a p-value of .110 in the job-level variation (and .102 in the state-level variation). Broadly, this placebo exercise suggests that our results, particularly for Black job-seekers and using job-level variation, would be unlikely to be observed due to chance if there were no actual treatment effect of PECC bans.

D   Data Appendix

Here we note a few additional features of interest regarding state policy and economic controls sample selection, finite-sample properties, variance-covariance matrices, and weighting.

D.1   Controlling for State Policy and Economic Confounders

This subsection presents further details on the controls for state-time policy and economic shocks introduced in Section 5.1 of the text. These controls include three measures of changes in state labor market policy, including indicators for at least one MSA in the state adopting a Ban-the-Box policy (Doleac and Hansen, 2020), an indicator for whether the state expanded Medicaid as part of the Affordable Care Act (ACA), and a measure of state Unemployment Insurance generosity during the Great Recession (Hsu et al., 2018). The period covered in our data, from 2002 to 2018, was a time of significant labor market change, during which the US economy experienced a large housing boom and bust, the Great Recession, a 30 percent decline in manufacturing, a sizable oil and gas boom, and high immigration rates. To control for state exposure to these time-varying economic shocks, we include controls for the baseline manufacturing share of employment in each state multiplied by year-dummies, the interaction between elasticity of housing supply calculated by Saiz (2010) and year-dummies, as a control for exposure to the housing boom and bust, a measure of fracking activity in state s in time t (Bartik et al., 2019), and baseline share of the population that was Hispanic and foreign-born in 2000 interacted with year dummies. All of these state policy and economic conditions controls are included interacted with race and ethnicity dummies. All tables present results from uncontrolled regressions as well.

D.2   Sample Restrictions

Because PECC-ban states implemented their bans at different times, we balance the number of pre-ban years and post-ban years across all PECC-ban states, using the maximum number of balanced years available in our data.[8] For the CPS data this results in using three pre-ban years and four-post ban years: no more than three pre-years are available, because Washington enacted its ban in 2007 and a CPS redesign in 2003 presents considerable challenges in using earlier survey years;[9] no more than four post-years are available for many states, because the majority of state PECC bans were enacted in the years 2010 to 2014, and in our analysis we use CPS data through February 2018. Similar data constraints result in using six pre-ban years and two post-ban years and one quarter in the LEHD J2J data, as the LEHD J2J data were only available through the first quarter of 2017.

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[8]We restrict the sample to a balanced panel of years. In the absence of heterogeneous treatment effects, the unbalanced sample would also provide an unbiased estimate of the average treatment effect. However, because there may be heterogeneous treatment effects we conservatively restrict our sample to the balanced set of event-years. Although there have been 13 state and local PECC bans, in practice we only study ten; the remaining three are either a) city bans for which our data are limited or b) 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.

[9]Specifically, the CPS classification system for industries and occupations changed in 2003, which makes it impossible to define a consistent set of job groups to use both before and after 2003 for our job-level analyses.

We also note that, because our measures of job-level variation among the unemployed rely on knowing job-seekers' most recent jobs, we exclude from our analysis any job-seekers who do not report a most recent job (new labor market entrants). For sake of consistency across specifications, we impose the same sample restriction when using state-level variation. Including these new entrants in our state-level analyses generally tends to attenuate, although not undo, our results. This restriction has the added benefit of making our CPS sample more similar to our LEHD J2J sample, which only includes individuals who recently separated from a job.

We use one additional sample restriction when we estimate models using job-level variation, as described in Section 4.2. Because we have been unable to find reliable evidence on which jobs were covered or exempted by Washington's PECC ban, we exclude data from Washington for all results at the job level.

D.3 Encoding job-level variation in PECCs coverage

As discussed in Section 3.3 and illustrated in Table 1, PECC bans typically include a substantial number of job-specific exemptions. In this section, we provide more detail on how we categorize which individuals' 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. These 4-digit codes represent a relatively fine partition of industries and occupations: for example, industry code 9070 is for "drycleaning and laundry services," while occupation code 4420 is for "ushers, lobby attendants, and ticket takers." We then encode each of these occupations and industries as either covered by or exempted from each PECC ban, based on statute texts, 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)). For example, Gordon and Kauffman (2010) analyze the Illinois PECC ban and conclude the law should be interpreted as exempting occupations including "senior executives, in-house attorneys, human resources professionals, most finance department and information technology employees, and managers with money-handling responsibilities." Finally, consistent with the PECC ban statutes, we code a job as exempt whenever either its industry or occupation is coded as exempt.

In some cases the correct encoding of industries' and occupations' exempt status is clear. For example, we encode occupation code 3850, "police and sheriff's patrol officers," as exempt in states that grant an exemption for law enforcement occupations. Other cases are more ambiguous, particularly when exemptions are granted to specific job features (e.g., "unsupervised access to marketable assets"). In these cases we use our judgment and explore robustness to alternative classification schemes. In general, if we misclassify jobs in any of these ambiguous cases, our empirical estimates of PECC bans' effects will be biased toward finding no effect (i.e., attenuation bias)

Finally, to describe the "expected job" exposure measure of Section 3.3 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.

D.4   Hazard Model Estimation on Aggregate Data

We consider the problem of how to estimate the parameters of discrete-time hazard models such as Equation 4.6 when only population-average job-finding rates are observed, as in our LEHD J2J data, or when it is desirable to specify a linear regression, as in some of our robustness exercises using the Sun and Abraham (2021) estimator. Let $B_{s,r,t}(\tau)$ be the observed number of individuals finding a job, and $N_{s,r,t}(\tau)$ be the number of job-seekers, at unemployment duration τ for group r in state s time period t. Likewise define $b_{s,r,t}(\tau) = B_{s,r,t}(\tau)/N_{s,r,t}(\tau)$.

We begin by noting that $B_{s,r,t}(\tau) \sim \text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau))$, where $\lambda^d_{s,r,t}(\tau)$ is as specified in Equation 4.6 and "Bin" refers to the Binomial distribution. For large $N_{s,r,t}(\tau)$, we therefore know $b_{s,r,t}(\tau) \xrightarrow{p} \lambda^d_{s,r,t}(\tau)$. So by the continuous mapping theorem, which shows that functions of random variables limit to the function of the random variable's limit, we can consistently estimate models such as Equation 4.6 on aggregate data, simply by plugging in $b_{s,r,t}(\tau)$ for $\lambda^d_{s,r,t}(\tau)$.

However in practice the number of unemployed individuals, $N_{s,r,t}(\tau)$, is of course finite. Because we model a nonlinear function of $\lambda^d_{s,r,t}(\tau)$ on the left-hand-side of Equation 4.6, our OLS estimator will exhibit some finite sample bias when we plug in $b_{s,r,t}(\tau)$ for $\lambda^d_{s,r,t}(\tau)$.

We use numerical integration to investigate the size of this bias. Specifically, we calculate as a function of $N_{s,r,t}(\tau)$ and $\lambda^d_{s,r,t}(\tau)$ the size of the expected bias ϵ in the dependent variable[10]:

$$\epsilon = \mathbb{E}\left[\ln(-\ln(1-b)) \right] - \lambda^d_{s,r,t}(\tau) \tag{D.1}$$

$$b \sim \text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau)) \tag{D.2}$$

We find that ϵ is less than 0.1 percent of $\lambda^d_{s,r,t}(\tau)$ when $N_{s,r,t}(\tau) > 500$. So in practice when we estimate the parameters of models such as Equation 4.6 on the LEHD J2J, we exclude states in which any race or ethnic group ever has fewer than 500 unemployed individuals at the unemployment duration that we study. This leads to the exclusion of data points from Idaho, Wyoming, Montana, North Dakota, and South Dakota.

For clarity we also reiterate that, as discussed in Section D.6, we only estimate job-finding models in the LEHD J2J on a single length of unemployment duration τ, the intermediate category of "adjacent-quarter flows." Because the data are available at a quarterly frequency

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[10]Where Bin refers to the Binomial distribution.

and all individuals in this duration category are newly unemployed in the past quarter, this is consistent with $B_{s,r,t}(\tau)$ being distributed $\text{Bin}(N_{s,r,t}(\tau), \lambda_{s,r,t}^d(\tau))$.

D.5   CPS Data Appendix

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. The panel has a rotating structure so that roughly one-eighth of the sample is in each of the eight months.

We adjust the panel structure of the raw CPS micro-data only slightly. We correct household identifiers for occasional erroneous matches between months.[11] Due to recent improvements in the CPS micro-data (see Drew et al. (2014) for a discussion) this procedure does not rely on the more intricate matching process often used on CPS data from the 1990s and earlier (Madrian and Lefgen (1999)). We also remove military members and any children aged eighteen or younger from our panel. In order to have more clearly interpretable flow estimates, we remove individuals on temporary layoff from the population we refer to as "unemployed" (Katz and Meyer (1990)). Finally, as illustrated in Figure 1, the number of pre and post-ban years varies between treatment states. Consequently, for states implementing PECC bans, we restrict the sample to a balanced set of pre and post-ban years common to all states, which is 3 years before the bans' implementation and 4 years afterwards.

We define job-finding in the CPS by whether an individual transitions into employment in a given month, conditional on having been unemployed ("U") in the prior month. We define involuntary separations for newly non-employed individuals using the CPS question about an individual's most recent job, "Did you lose [that job], quit that job, or was it a temporary job that ended?" Individuals whose response is encoded as "lost job" as opposed to "quit job" or "temporary job ended" are coded as having involuntary separated. For individuals who appear with multiple spells (e.g., two transitions from unemployment to employment), we include all such spells in our analysis. Our clustering of standard errors at the state level allows arbitrary error correlation across these multiple spells within individual level. (Given that the CPS sampling frame is address/geography-based, individuals are always observed within the same state.)

When measuring an individual's race or ethnicity, we make the ad hoc classification choice that multi-racial individuals are "Black" whenever they identify partly as Black, and individuals otherwise are "Hispanic" whenever they identify as Hispanic. We group all other groups into a non-minority category that we refer to as "white." Thus we reach three mutually exclusive categories.

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[11]More precisely, when one household is replaced by another due to mid-panel attrition, we generate new identifiers for the replacement households in cases where the new and old identifiers coincide. This affects less than 0.07% of households in the data.

D.6 LEHD J2J Data Appendix

The CPS provides rich longitudinal information on individual job-finding hazards and separation rates. However, our estimates, although reasonably precise, are somewhat noisy. Furthermore, data in the CPS is 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 (2015)). This is a publicly available administrative data aggregated from Unemployment Insurance (UI) records from all 51 states and Washington, DC.[12] 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 14 quarters post-treatment and 17 quarters pre-treatment.[13] Figure 1 illustrates LHED-J2J availability for the treated states. We refer to these data as the LEHD J2J data.

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). Because of the coarse nature of the quarterly data none of these three categories contains exclusively voluntary or involuntary job changes. In our analysis we focus on the intermediate category, adjacent-quarter flows. We note that this category includes spells of involuntary unemployment but also short voluntary breaks between jobs (Hyatt et al. (2015)). The choice to use 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.[14]

The Census releases the LEHD J2J data separately by worker race and ethnicity categories. The census generates these data by merging the Unemployment Insurance data using Social Security Numbers (SSNs) with the Decennial Census short form, which contains detailed race and ethnicity information, and the Social Security Administration (SSA) Personal Characteristics File (PCF), which contains more limited information on race.[15] Roughly 95% of observations in the LEHD J2J are matched to either the Census short form or the SSA PCF file. Race and ethnicity is imputed for the remaining 5% of observations (Abowd and McKinney (2009)). Following our procedure in the CPS data, we then aggregate race and ethnicity into three major

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[12]Note that although the LEHD compiles data from all 51 states, as described in more detail below, We exclude some states due to data limitations. Particularly, we exclude Vermont, Washington, and Connecticut (three treated states) because their are insufficient numbers of pre and post-treatment years given our balanced sample restriction. Additionally, we also exclude Idaho, Wyoming, Montana, North Dakota, and South Dakota because of the small sample issues discussed in 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.

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

[14]These duration dependence problems arise because the same individual appears in the unemployed pool in multiple quarters, yet the length to-date of the spell in each case is unobserved. See Section 4 for more discussion of how we account for duration dependence in the CPS data.

[15]Most importantly, the PCF does not contain information on Hispanic origin.

categories: Black, Hispanic, and a non-minority category referred to as "white." The Census releases both raw and seasonally adjusted versions of the LEHD J2J. We use the seasonally adjusted time-series.

Appendix Table 2 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.

D.7   Inference and Weighting

Following Bertrand et al. (2004), to account for correlated shocks within states over time, and also to account for instances in which the same individual has multiple unemployment spells in our CPS panel, all of our standard errors are clustered at the state level or the state-race/ethnicity level.[16]

Finally, we note that throughout our CPS analysis we use sample weights as suggested by CPS documentation (i.e., longitudinal weights when estimating flows, and cross-sectional weights otherwise). In the LEHD J2J, all specifications are weighted by the newly unemployed population in the given state-race/ethnicity-year.

E   Extended Literature Discussion

Above, we estimated that PECC bans reduced steady-state Black employment by 1.5 percentage points in states banning PECCs. 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.[17] All three of these papers suggest that regulation

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[16]To be precise, all standard errors are clustered at the state level, except for the standard errors displayed as confidence intervals in Figure ??, which needed to be clustered at the state-race/ethnicity level due to computational constraints.

[17]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

of information used in the hiring process can have economically large impacts on employment outcomes.[18]

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.[19]

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.[20]

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

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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.

[18]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.

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

[20]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.

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.[21] 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. [22] 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).[23]

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

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[21]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.

[22]The higher precision of signals for white job candidates may reflect firms' having already paid the fixed costs of creating a job-screening process that allows white candidates to send precise signals. We do not model or provide evidence on how firms choose which screening tools to invest in. Exploring these choices is a rich area for future research to understand institutional discrimination.

[23]We conjecture, based on our own results and our reading of the combined evidence, that more subjective or 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 8 for discussion of other signals not covered by these two broad signal types.

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.

F   Appendix Figures

Appendix Figure 1: PECC Bans Timeline and Data Availability by State
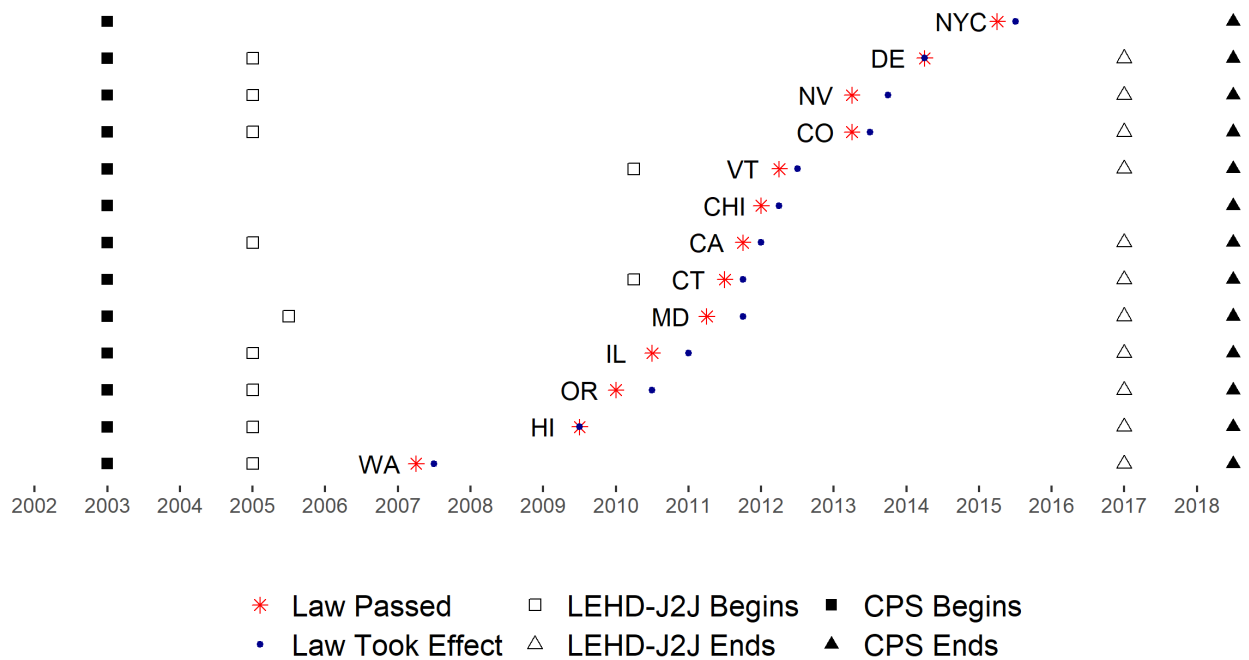
Notes: Oregon's PECC ban originally had an implementation date of 7/10/2010, but implementation was accelerated to 3/29/2010 (Friedberg et al., 2016). In Delaware, the law only banned credit screening for public jobs and before the first interview; see also Section 3.1.

Appendix Figure 2: Event-Time Analysis of the Effect of PECC Bans on Job-Finding Using Job-Variation

State-Job-Race/Ethnicity FE, Time-Race/Ethnicity FE
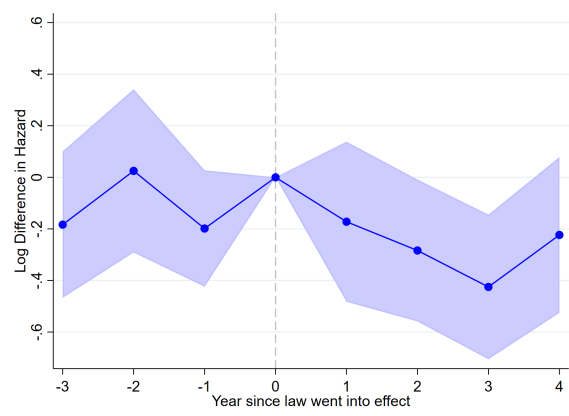
(a) Black

(b) Hispanic
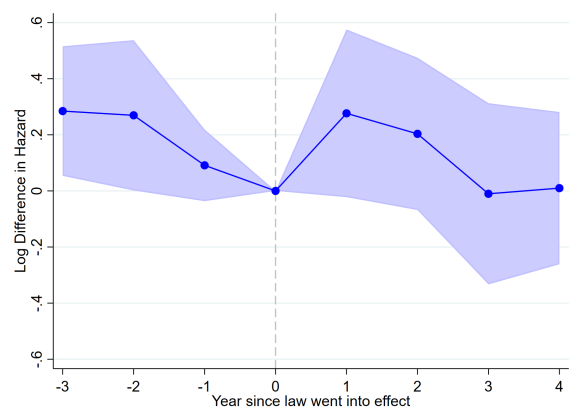
(c) White

Notes: This figure shows the results of an event-time analysis of the difference in job-finding for unemployed individuals between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. Each panel shows results for a different race or ethnic group. The reported coefficients come from estimating with MLE a version of the proportional hazards model in Equation 4.6, where we use state-job-race/ethnicity fixed effects in lieu of state-race/ethnicity fixed effects, and where we interact an indicator for being covered by a PECC ban with the expected probability of being in a PECC covered job (given the unemployed workers previous job $j(i)$), $D_{s(i),t} \times p_{j(i),s(i)}$, 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 job-time-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% confidence intervals generated from standard errors clustered at the state level.

Appendix Figure 3: Event-Time Analysis of the Effect of PECC Bans on Job-Finding: LEHD J2J

State-Race/Ethnicity FE, Time-Race/Ethnicity FE
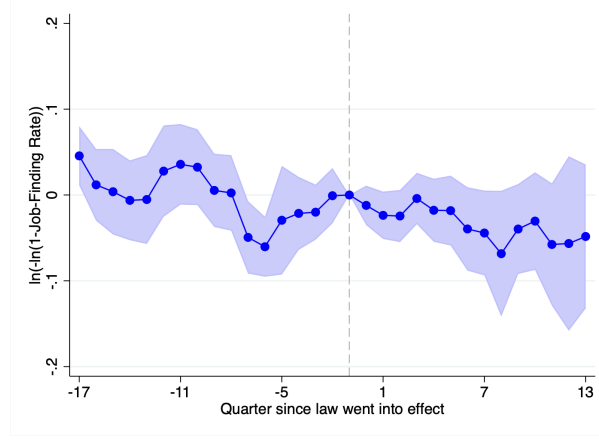
(a) Black

(b) Hispanic
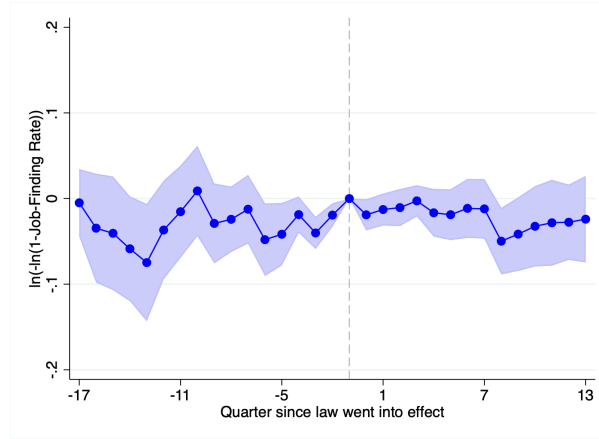
(c) White
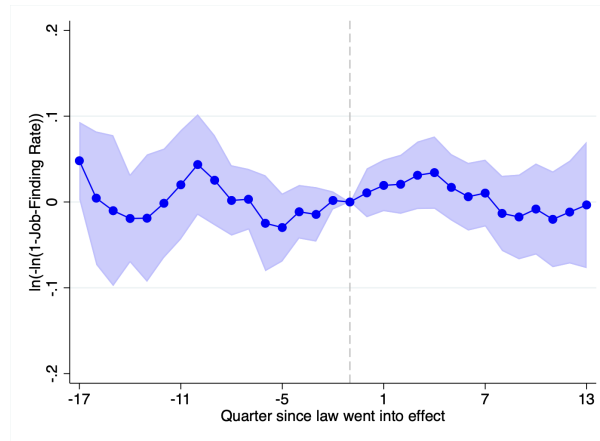
Notes: This figure shows the results of an event-time analysis of the difference in the complementary log-log of the average job-finding rate (i.e., $\ln(-\ln(1 - \text{job-finding rate}))$) between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. The reported coefficients come from estimating a version of Equation 4.1 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-quarter, t, minus the year-quarter that a PECC ban took effect in state s. 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. Regressions are weighted by the number of individuals of a given group who separated from their jobs in state s in year-quarter t. The sample is restricted to balanced event years common to all PECC-ban states. Data on job-finding rates for workers who separate from their main jobs come from the Longitudinal Employer-Household Dynamics Job-to-Job Flows data (LEHD J2J) (US Census Bureau (2015)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.

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Appendix Figure 4: Event-Time Analysis of the Effect of PECC Bans on Separations Using Job-Variation: New Hires

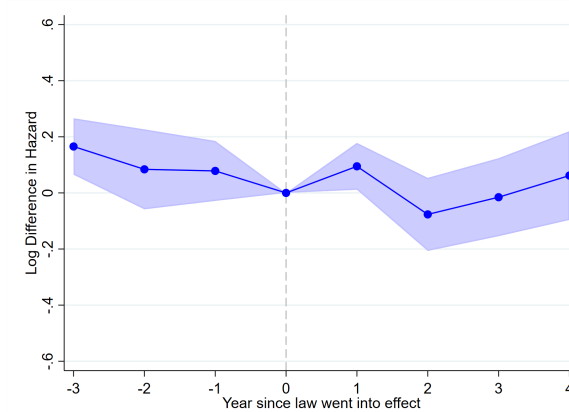State-Job-Race/Ethnicity FE, Time-Race/Ethnicity FE
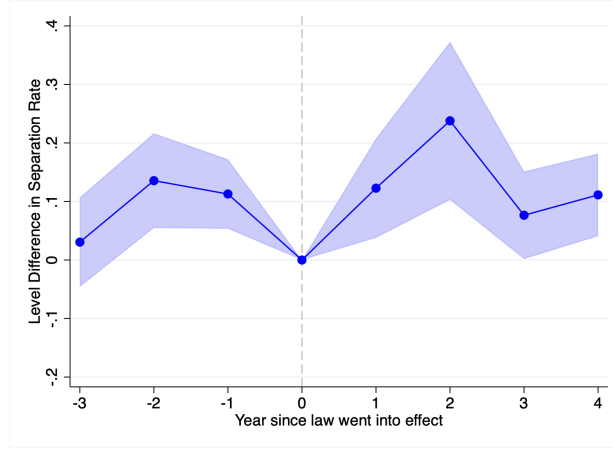
(a) Black

(b) Hispanic
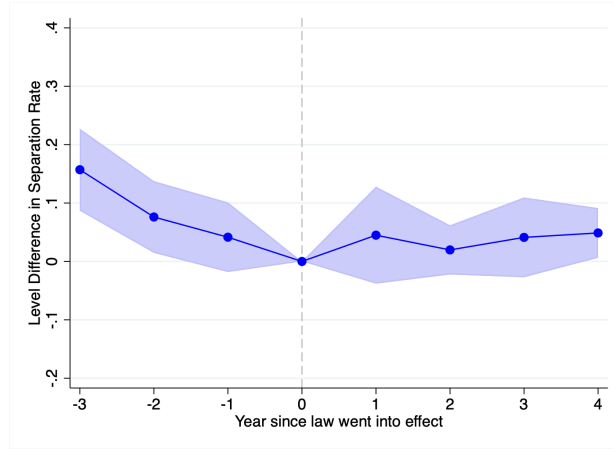
(c) White
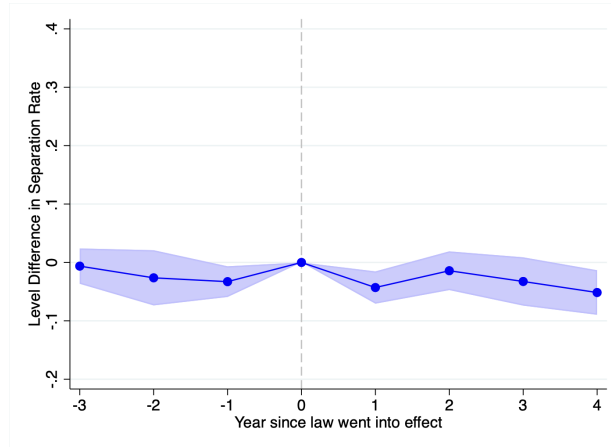
Notes: This figure shows the results of an event-time analysis of the difference in involuntary separation rates for workers newly hired out of unemployment between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. Each panel shows results for a different race or ethnic group. The reported coefficients come from estimating a modified linear probability model of equation 4.4 where we interact an indicator for being covered by a PECC ban in job $j(i)$ at the time of hiring, $D_{s(i)} \times \times T^C_{j(i),s(i)}$, 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 job-time-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 PECCs-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% confidence intervals generated from standard errors clustered at the state level.

Appendix Figure 5: Placebo Analysis of the Effect of PECC bans on Job-Finding Using State Variation
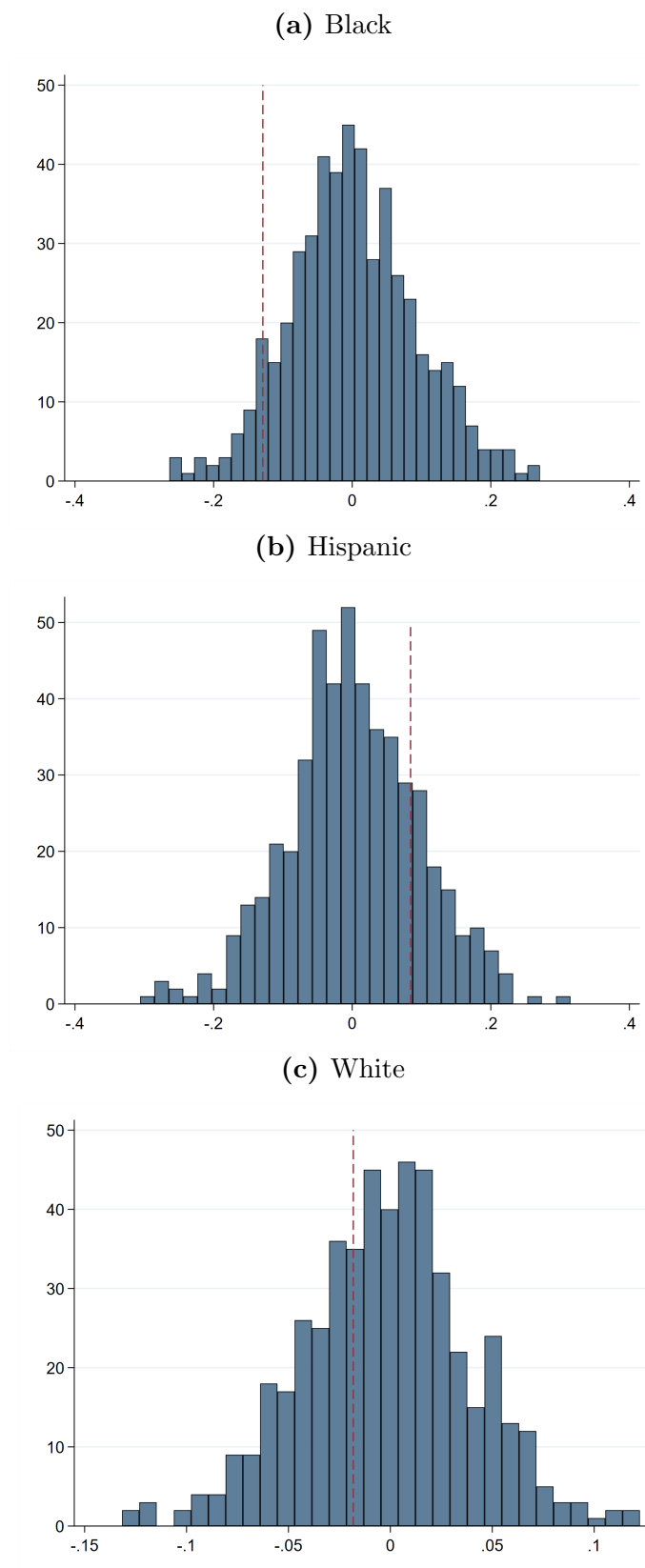
(a) Black

(b) Hispanic

(c) White

Notes: This figure shows results of placebo treatment analyses where we randomly assign treatment status to states and dates, following the share of treated states in our actual sample. We then estimate with MLE a version of the proportional hazards model in Equation 4.6 using state-level variation. The three panels of the figure show the distribution of estimated placebo treatment effects for Black, Hispanic, and white individuals. The vertical line in each figure shows our actual estimate.

Appendix Figure 6: Placebo Analysis of the Effect of PECC bans on Job-Finding Using Job Variation
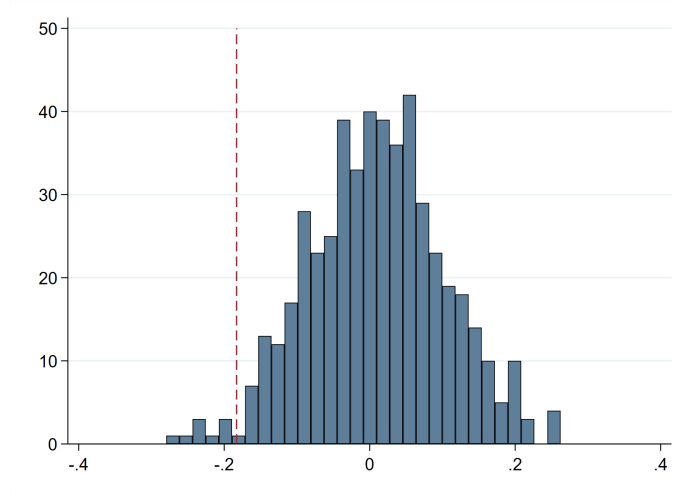
(a) Black

(b) Hispanic
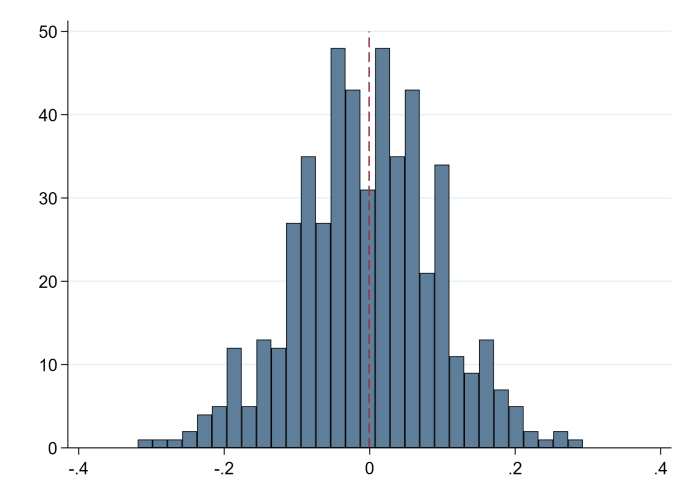
(c) White
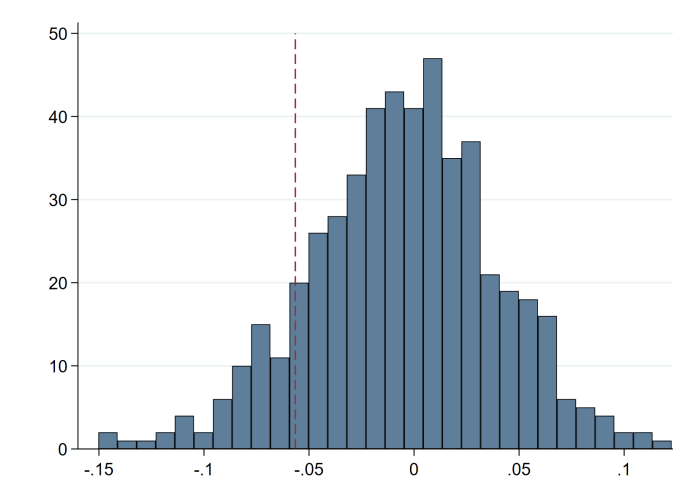
Notes: This figure shows results of placebo treatment analyses where we randomly assign treatment status to states, jobs, and dates, following the share of treated states and jobs in our actual sample. We then estimate with MLE a version of the proportional hazards model in Equation 4.6 using job-level variation. The three panels of the figure show the distribution of estimated placebo treatment effects for Black, Hispanic, and white individuals. The vertical line in each figure shows our actual estimate.

Appendix Figure 7: Placebo Analysis of the Effect of PECC bans on Separations for New Hires using State Variation
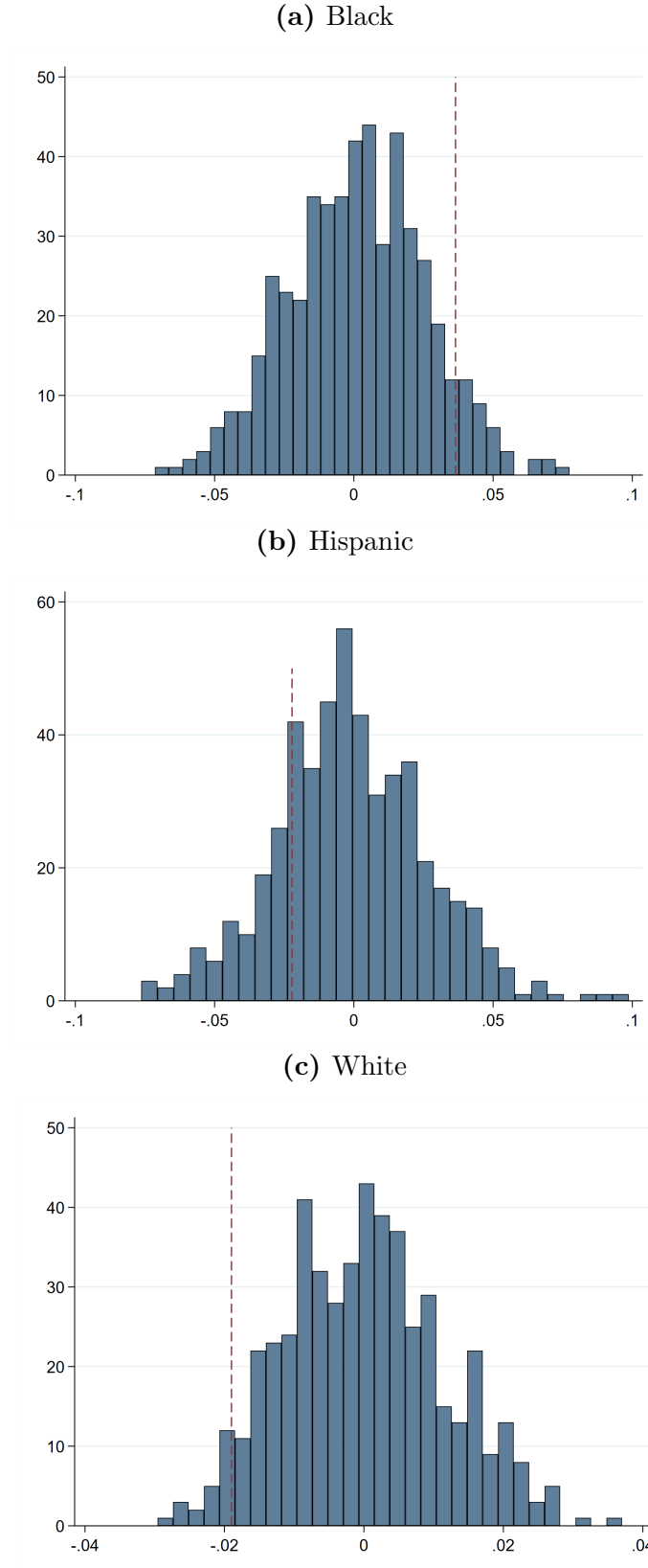
(a) Black

(b) Hispanic

(c) White

Notes: This figure shows results of placebo treatment analyses where we randomly assign treatment status to states and dates, following the share of treated states in our actual sample. We then estimate the linear probability model of involuntary separation rates for new hires in equation 4.4, using state-level variation. The three panels of the figure show the distribution of estimated placebo treatment effects for Black, Hispanic, and white individuals. The vertical line in each figure shows our actual estimate.

Appendix Figure 8: Placebo Analysis of the Effect of PECC bans on Separations for New Hires using Job Variation
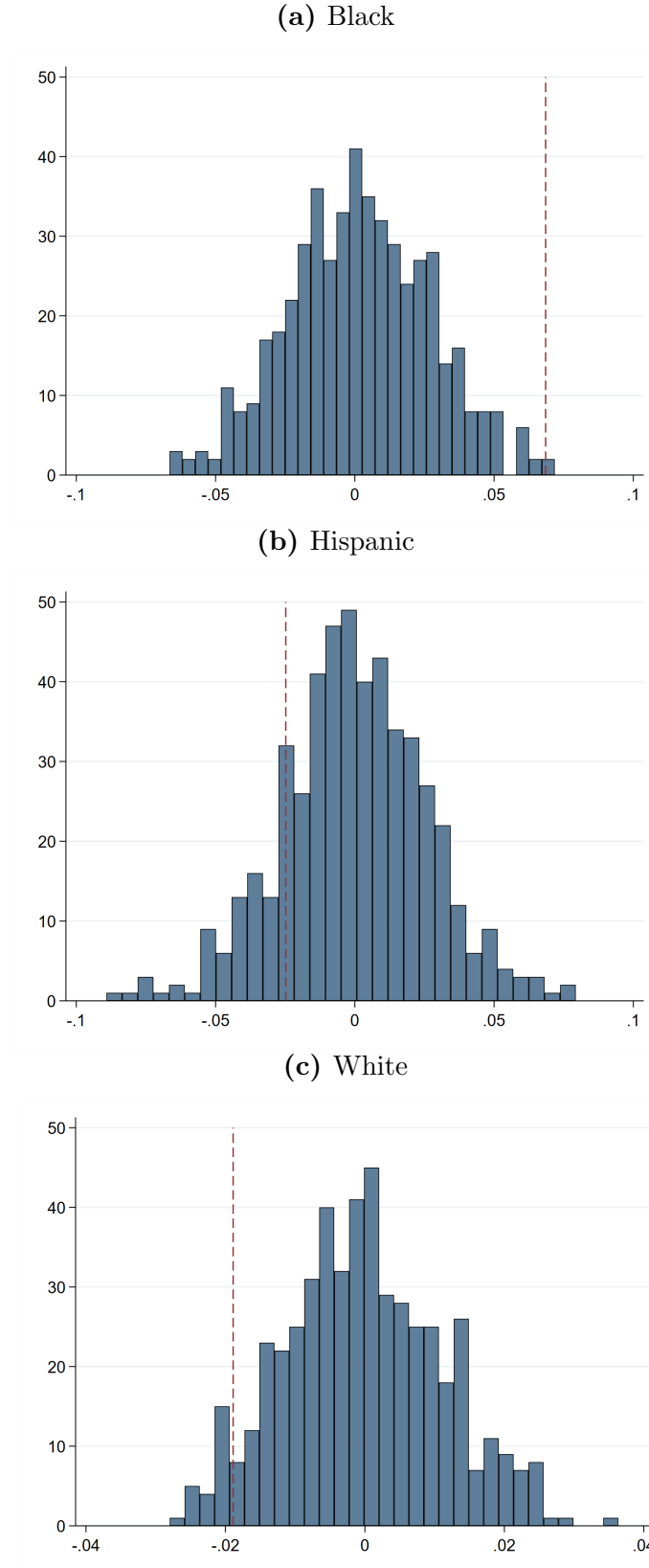
(a) Black

(b) Hispanic

(c) White

Notes: This figure shows results of placebo treatment analyses where we randomly assign treatment status to states, jobs, and dates, following the share of treated states and jobs in our actual sample. We then estimate the linear probability model of involuntary separation rates for new hires in equation 4.4, using job-level variation. The three panels of the figure show the distribution of estimated placebo treatment effects for Black, Hispanic, and white individuals. The vertical line in each figure shows our actual estimate.

G Appendix Tables

Appendix 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	
	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										
Legal	Law enforcement		X	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.

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Appendix Table 2: 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	0.193	0.150
Involuntary Separation Rate, New Hires (CPS)	0.107	0.090	0.109	0.109
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.024	0.019	0.023	0.024
Separation Rate (LEHD)	0.096	0.100		
Adjacent Quarter Job-Finding Rate (LEHD)	0.219	0.240		
Panel B: Hispanics				
Job-Finding Rate out of Unemployment (CPS)	0.183	0.229	0.259	0.198
Involuntary Separation Rate, New Hires (CPS)	0.097	0.076	0.093	0.126
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.020	0.015	0.021	0.018
Separation Rate (LEHD)	0.083	0.099		
Adjacent Quarter Job-Finding Rate (LEHD)	0.214	0.225		
Panel C: Whites				
Job-Finding Rate out of Unemployment (CPS)	0.153	0.192	0.211	0.168
Involuntary Separation Rate, New Hires (CPS)	0.084	0.068	0.076	0.110
Involuntary Sep. Rate, Long-Tenure Workers (CPS)	0.015	0.011	0.015	0.014
Separation Rate (LEHD)	0.063	0.069		
Adjacent Quarter Job-Finding Rate (LEHD)	0.210	0.224		

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.

Appendix Table 3: Adjusting for Staggered Treatment Adoption

Outcome:	Job-Finding			Separations	
Data-structure:	Individual	Aggregate		Individual	
	(1)	(2)	(3)	(4)	(5)
Panel A. State-level Variation					
Panel A1. Effect separately by race/ethnicity					
1(Black)*1(Treated by Ban)	-0.102**	-0.0883**	-0.0843**	0.0213	0.0231**
	-0.0418	(0.0436)	(0.0352)	(0.0131)	(0.0103)
1(Hispanic)*1(Treated by Ban)	0.0792	0.0272	0.0310	-0.0545***	-0.0531***
	(0.0488)	(0.0570)	(0.0448)	(0.0149)	(0.0139)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0137	-0.00325	-0.0088	-0.0215***	-0.0213***
	(0.0266)	(0.0295)	(0.0212)	(0.00749)	(0.0043)
Panel A2. Overall Effect					
1(Treated by Ban)	-0.000860	-0.0101	-0.0124	-0.0246***	-0.0243***
	(0.0270)	(0.0306)	(0.0170)	(0.00699)	(0.0063)
N	343,262	6,535	6,535	54,389	54389
States	51	51	51	51	51
Ban States	10	10	10	10	10
Panel B: Job-Level Variation					
Panel B1. Effects separately by race/ethnicity					
1(Black)*1(Treated by Ban)	-0.141***	-0.0906**	-0.0831**	0.0406**	0.0419***
	(0.0548)	(0.0427)	(0.0372)	(0.0160)	(0.0139)
1(Hispanic)*1(Treated by Ban)	0.0515	-0.00503	0.0117	-0.0545***	-0.0529***
	(0.0559)	(0.0636)	(0.0499)	(0.0168)	(0.0153)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0296	-0.0400	-0.0401*	-0.0207***	-0.0207***
	(0.0430)	(0.0411)	(0.0237)	(0.00630)	(0.0055)
Panel B2. Overall Effect					
1(Treated by Ban)	-0.0198	-0.0404	-0.0339	-0.0236***	-0.0231***
	(0.0402)	(0.0417)	(0.0208)	(0.00751)	(0.0074)
N	331,942	6,535	6,582	52,407	52407
States	50	50	50	50	50
Ban States	9	9	9	9	9
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	N	N
State-New Job-Race/Ethnicity Fixed Effects	N	N	N	Y	Y
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y	Y	N	N
Aggregate Level Regression	N	Y	Y	N	N
Sun & Abraham (2021)	N	N	Y	N	Y

Notes: This table investigates the impact that staggered treatment adoption has on our differences-in-differences estimates of the effect of PECC bans on job-finding (Columns (1) through (3)) and separation rates for new hires (Columns (4)-(5)). In Panel A, a job-seeker's exposure to a PECC ban is determined by whether or not she 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 Appendix Section D.3) is covered by or exempted from a PECC ban. Column (1) reports MLE estimates of hazard models that include the state-race/ethnicity and time-race/ethnicity fixed effects that implement difference-in-differences. Column (3) presents estimates models using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. This approach can only be implemented with linear models, so we aggregate job-finding rates to the year-by-quarters-unemployed-by-treatment level, where the treatment-level is the state in Panel A and state-by-job-treatment-status in Panel B. We then estimate OLS models for the complementary-log-log of race/ethnicity-specific job-finding rate (i.e., ln(-ln(1-job-finding))). To understand the extent to which any differences in the estimates are driven by the required aggregation versus using the Sun and Abraham (2021) estimator, Column (2) reports results using traditional difference-in-differences models on the aggregated data. Column (4) reports linear probability model estimates of (race/ethnicity-specific) differences in separation rates for newly hired workers following a PECC ban using the standard difference-in-differences fixed effects. Column (5) then implements Sun and Abraham (2021)'s estimator for these separations specifications. Standard errors clustered at the state level are shown in parentheses.

Appendix Table 4: Impact of PECC Bans on Employment Levels

	(1)	(2)	(3)	(4)
Panel A: Effect separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.00833***	-0.00764**	-0.00897***	-0.00664
	(0.00294)	(0.00380)	(0.00327)	(0.00425)
1(Hispanic)*1(Treated by Ban)	0.00877**	0.00761	0.0111***	0.0121**
	(0.00339)	(0.00542)	(0.00348)	(0.00516)
1(Non-Hispanic white)*1(Treated by Ban)	0.00163	0.00215	0.00438*	0.00486*
	(0.00169)	(0.00252)	(0.00232)	(0.00242)
Panel B: Overall effect				
1(Treated by Ban)	0.00202	0.00181	0.00471*	0.00496*
	(0.00207)	(0.00283)	(0.00259)	(0.00281)
N	10,599,769	10,599,769	7,505,047	7,505,047
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 and overall) differences in employment rates for working age individuals 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 controls for state economic and policy variables are: Saiz's price elasticity of housing multiplied by year indicators (Saiz (2010)), an indicator for whether the state had geological potential for fracking in the given year (Bartik et al. (2019)), the share of manufacturing jobs in 2000 multiplied by year indicators (ACS 2000), an indicator for whether the state had any Ban-the-Box policy, an indicator for whether the state had expanded Medicaid in the given year, year 2000 state Hispanic and foreign born share interacted with year indicators, and a measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). Standard errors clustered at the state level are shown in parentheses. All controls (individual and state policy/economic) are interacted by race-ethnicity dummies.

Appendix Table 5: Impact of PECC Bans on Involuntary Separation Rates for Long-Tenure Employees

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect separately by race/ethnicity						
1(Black)*1(Treated by Ban)	-0.00311	-0.00428	-0.000992	-0.00428	-0.00450***	-0.00215
	(0.00207)	(0.00373)	(0.00190)	(0.00373)	(0.00140)	(0.00375)
1(Hispanic)*1(Treated by Ban)	-0.00302	-0.00378*	-0.00239	-0.00378*	-0.00356**	-0.00327
	(0.00180)	(0.00221)	(0.00240)	(0.00221)	(0.00151)	(0.00271)
1(Non-Hispanic white)*1(Treated by Ban)	-0.00251***	-0.00330***	-0.00133	-0.00330***	-0.00329***	-0.00201**
	(0.000720)	(0.000871)	(0.000911)	(0.000871)	(0.00051)	(0.000979)
Panel B: Overall effect						
1(Treated by Ban)	-0.00265***	-0.00267***	-0.00142*	-0.00351***	-0.00349***	-0.00225**
	(0.000700)	(0.00054)	(0.000803)	(0.000851)	(0.00066)	(0.000915)
N	4,716,320	4,716,320	4,716,320	4,590,155	4,590,155	4,590,155
States	51	51	51	50	50	50
Ban States	10	10	10	9	9	9
Treatment Level	State	State	State	Current Job	Current Job	Current Job
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	N	N	N
State-New Job-Race/Ethnicity Fixed Effects	N	N	N	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	N	Y	N	N	Y
State Policy/Economic controls (-Race/Ethnicity)	N	N	Y	N	N	Y
Sun & Abraham (2021)	N	Y	N	N	Y	N

Notes: This table re-estimates the difference-in-differences models from Table 3 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 3, Columns (1)-(2) and (4)-(5) 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 (4) through (6) due to uncertainty about which jobs are exempted from Washington's ban. Columns (3) and (6) report estimates using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. 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.

Appendix Table 6: Robustness of Impact of PECC Bans on Job-Finding: LEHD-J2J

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Separately by race/ethnicity									
1(Black)*1(Treated by Ban)	-0.046	0.013	-0.045***	-0.015	-0.025*	-0.034	0.008	-0.024	-0.033*
	(0.031)	(0.011)	(0.014)	(0.010)	(0.015)	(0.027)	(0.013)	(0.017)	(0.017)
1(Hispanic)*1(Treated by Ban)	0.020	0.060***	0.021	0.002	0.015	0.007	0.008	-0.008	-0.023**
	(0.023)	(0.022)	(0.022)	(0.007)	(0.016)	(0.012)	(0.013)	(0.007)	(0.011)
1(Non-Hispanic white)*1(Treated by Ban)	-0.013	0.031*	-0.012			0.0044	0.0345**		
	(0.018)	(0.018)	(0.014)			(0.01936)	(0.01501)		
Panel A. Overall Effect									
1(Treated by Ban)	-0.010	0.035**	-0.009			0.000	0.027**		
	(0.019)	(0.017)	(0.015)			(0.019)	(0.013)		
N	5,700	5,700	5,700	5,700	5,700	5,700	5,700	5,700	5,700
States	44	44	44	44	44	44	44	44	44
Ban States	7	7	7	7	7	7	7	7	7
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-Time Fixed Effects	N	N	N	Y	Y	N	N	Y	Y
State-Race/Ethnicity Linear Trends	N	N	Y	N	Y	N	Y	N	Y
Demographic Controls (-Race/Ethnicity)	N	N	N	N	N	N	N	N	N
State Policy/Economic controls (-Race/Ethnicity)	N	N	N	N	N	Y	Y	Y	Y
Sun & Abraham (2021)	N	N	Y	N	N	N	N	N	N

Notes: This table investigates the robustness of the estimates of the effect of PECC-bans on job finding rates using an alternative data-set. Specifically, the table reports OLS estimates of difference-in-differences and triple-difference models for the complementary-log-log of race/ethnicity-specific aggregate job-finding rates (i.e., ln(-ln(1-job-finding))) using the LEHD J2J data. Panel A reports results separately by race while Panel B reports the overall effect of PECC bans. Column (1) includes the state(-race/ethnicity) and time(-race/ethnicity) fixed effects that implement difference-in-differences (Equation 6.11 in the text), while Column (2) adds controls for linear trends at the state-race/ethnicity level. Column (3) reports estimates using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. Column (4) adds state-time effects to the specification from Column (1), implementing a triple-difference estimator. Column (5) then augments the triple-difference model with linear trends at the state-race/ethnicity level. Column (6) through (9) add to the specifications in columns (1) , (2), (4), and (5) extra controls at the state level. Three treated states (Vermont, Washington, Connecticut) are not included in this table due to data limitations of the LEHD-J2J. Standard errors clustered at the state level are shown in parentheses. The set of extra controls for state economic and policy variables included in Column (6)-(9) 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 and economy) are interacted by race-ethnicity dummies.

Appendix Table 7: Robustness of Impact of PECC Bans on Job-Finding: LEHD-J2J and CPS Comparison Subsample

	(1)	(2)	(3)	(4)
Panel A. LEHD J2J				
Panel A1. Separately by race/ethnicity				
1(Black)*1(Treated by Ban)		-0.046	-0.034	-0.045***
		(0.031)	(0.027)	(0.014)
1(Hispanic)*1(Treated by Ban)		0.020	0.007	0.021
		(0.023)	(0.012)	(0.022)
1(Non-Hispanic white)*1(Treated by Ban)		-0.013	0.004	-0.012
		(0.018)	(0.019)	(0.014)
Panel A2. Overall Effect				
1(Treated by Ban)		-0.010	0.000	-0.009
		(0.019)	(0.019)	(0.015)
N		5,700	5,700	5,700
States		44	44	44
Ban States		7	7	7
Panel B. CPS				
Panel B1. Separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.102**	-0.0911	-0.0101	-0.144
	-0.0418	(0.148)	(0.150)	(0.175)
1(Hispanic)*1(Treated by Ban)	0.0792	0.120	0.0623	0.049
	(0.0488)	(0.0806)	(0.181)	(0.066)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0137	-0.0850	-0.103	-0.082
	(0.0266)	(0.0563)	(0.0664)	(0.049)
Panel B2. Overall Effect				
1(Treated by Ban)	-0.000860	-0.0273	-0.0593	-0.0619
	(0.0270)	(0.0495)	(0.0615)	(0.0408)
N	343,262	45,041	45,041	1,998
States	51	44	44	44
Ban States	10	7	7	7
Adjacent Quarter Separations Sample	N	Y	Y	Y
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Time Fixed Effects	N	N	N	N
State-Race/Ethnicity Linear Trends	N	N	N	N
Demographic Controls (-Race/Ethnicity)	N	N	N	N
State Policy/Economic controls (-Race/Ethnicity)	N	N	Y	N
Sun & Abraham (2021)	N	N	N	Y

Notes: This table investigates the robustness of the estimates of the effect of PECC-bans on job finding rates using alternative datasets and empirical strategies. Panel A reports OLS estimates of difference-in-differences models for the complementary-log-log of race/ethnicity-specific aggregate job-finding rates (i.e., ln(-ln(1-job-finding))) using the LEHD J2J data. Panel B reports MLE estimates of race/ethnicity-specific log differences in job-finding hazard rates following a PECC ban, using a variety of difference-in-differences models and data from the CPS for years 2003-2018. Panel B Columns (2) through (4) restrict the CPS sample to adjacent quarter transitions that mimic the transitions observed in the LEHD-J2J. For reference, column (1) reports CPS estimates in the unrestricted sample that we focus on in much of the paper. In both Panels, Column (2) includes the state(-race/ethnicity) and time(-race/ethnicity) fixed effects that implement difference-in-differences (Equation 6.11 in the text). Column (3) adds extra controls for local policy and economic changes at the state level. Column (4) reports estimates using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. In Panel A (LEHD-J2J) and Panel B columns (2) through (4), three treated states (Vermont, Washington, Connecticut) are not included in this table due to data limitations of the LEHD-J2J. Standard errors clustered at the state level are shown in parentheses. The set of extra controls for state economic and policy variables included in Column (3) 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 and economy) are interacted by race-ethnicity dummies.

Appendix Table 8: Robustness of Impact of PECC Bans on Job-Finding: CPS

	DD				DDD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Separately by race/ethnicity							
1(Black)*1(Treated by Ban)	-0.102**	-0.197	-0.0741	-0.0732	-0.0628	-0.337*	-0.0749
	(0.0418)	(0.186)	(0.0730)	(0.0722)	(0.0637)	(0.195)	(0.0599)
1(Hispanic)*1(Treated by Ban)	0.0792	0.121*	0.0429	0.0400	0.0754	0.0529	0.0708
	(0.0488)	(0.0682)	(0.0591)	(0.0574)	(0.0507)	(0.0980)	(0.0505)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0137	0.0865**	-0.0211	-0.0222			
	(0.0266)	(0.0406)	(0.0369)	(0.0356)			
Panel B. Overall Effect							
1(Treated by Ban)	-0.000860	0.0601	-0.0140	-0.0153			
	(0.0270)	(0.0247)	(0.0447)	(0.0432)			
N	343,262	343,262	15,853	15,853	342,023	342,023	341,218
States	51	51	50	50	51	51	51
Ban States	10	10	9	9	10	10	10
State Level Treatment	Y	Y	N	N	Y	Y	Y
Job-State Level Treatment	N	N	Y	Y	N	N	N
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y
State-Past Job-Race/Ethnicity Fixed Effects	N	N	Y	Y	N	N	N
State-Past Job-Linear Trends	N	N	N	N	N	N	Y
State-Race/Ethnicity Linear Trends	N	Y	N	N	N	Y	N
Past Job-Race/Ethnicity Fixed Effects-Linear Trends	N	N	N	Y	N	N	N
Past Job-Race/Ethnicity Fixed Effects-Quadratic Trends	N	N	N	Y	N	N	N
Past Job-Race/Ethnicity Fixed Effects-Cubic Trends	N	N	N	Y	N	N	N
State-Time Fixed Effects	N	N	N	N	Y	Y	Y
Yearly-Spell Length-State-Job Level Aggregate Regression	N	N	Y	Y	N	N	N

Notes: This table investigates the robustness of the estimates of the effect of PECC-bans on job finding rates using alternative empirical strategies in the CPS. The table reports MLE estimates of race/ethnicity-specific log differences in job-finding hazard rates following a PECC ban, using a variety of difference-in-differences and triple-difference models and data from the CPS for years 2003-2018. Column (1) repeats column (1) from Table 2 Panel A for reference. Column (2) adds state-race/ethnicity-specific linear trends. Columns (3) and (4) explore robustness of our estimates using job-level variation in Table 2 Panel B, by adding a flexible third-order polynomial in time at the state-job level. MLE estimation of this specification does not converge, so in column (4) we estimate this specification via OLS on collapsed job finding rates. To understand the extent to which any differences in the estimates relative to those in Table 2 are driven by the required aggregation, column (3) repeats the specification of Table 2 Panel B column (1) on the aggregated data. Columns (5) through (7) estimate triple-difference models based on equation 4.2. Column (5) adds state-time fixed effects to the column (1) specification from Table 2 Panel A in order to implement a triple-difference estimator. Column (6) then augments this triple-difference model with linear trends at the state-race/ethnicity level, while column (7) adds linear trends at the state-job level.

Appendix Table 9: Robustness of Impact of PECC Bans on Separations: CPS

	DD				DDD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effect separately by race/ethnicity							
1(Black)*1(Treated by Ban)	0.0213	0.0676**	0.0424**	0.0423**	0.0286**	0.0826***	0.0273*
	(0.0131)	(0.0264)	(0.0161)	(0.0166)	(0.0135)	(0.0275)	(0.0137)
1(Hispanic)*1(Treated by Ban)	-0.0545***	-0.00836	-0.0531***	-0.0537***	-0.0281	0.0223	-0.0260
	(0.0149)	(0.0338)	(0.0178)	(0.0186)	(0.0181)	(0.0493)	(0.0188)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0215***	-0.00610	-0.0211***	-0.0212***			
	(0.00749)	(0.0134)	(0.00641)	(0.00692)			
Panel B: Overall effect							
1(Treated by Ban)	-0.0246***	0.00275	-0.0231***	-0.0232***			
	(0.00699)	(0.0120)	(0.00812)	(0.00859)			
N	54,389	54,389	52,407	52,407	53,959	53,959	53,735
States	51	51	50	50	51	51	51
Ban States	10	10	9	9	10	10	10
State Level Treatment	Y	Y	N	N	Y	Y	Y
Job-State Level Treatment	N	N	Y	Y	N	N	N
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y	Y	Y	Y
State-Past Job-Race/Ethnicity Fixed Effects	N	N	Y	Y	N	N	N
State-Past Job-Linear Trends	N	N	N	N	N	N	Y
State-Race/Ethnicity Linear Trends	N	Y	N	N	N	Y	N
Job-Race/Ethnicity Fixed Effects-Linear Trends	N	N	Y	Y	N	N	N
Job-Race/Ethnicity Fixed Effects-Quadratic Trends	N	N	N	Y	N	N	N
Job-Race/Ethnicity Fixed Effects-Cubic Trends	N	N	N	Y	N	N	N
State-Time Fixed Effects	N	N	N	N	Y	Y	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 a variety of difference-in-differences and triple-difference strategies. Data are from the CPS for years 2003-2018. For convenience, Column (1) is a repeat of the state-time difference-in-differences results in Column (1) of Table 3. Column (2) adds state-job- state-race/ethnicity-specific linear trends. Columns (3) and (4) explore robustness of our estimates using job-level variation in Table 3, by adding a flexible third-order polynomial in time at the state-job level (in column 4) and a linear trend at the state-job level (in column 3). Columns (5) through (7) estimate triple-difference models based on equation 4.2. Column (5) adds state-time fixed effects to the column (1) specification from Table 3 in order to implement a triple-difference estimator. Column (6) then augments this triple-difference model with linear trends at the state-race/ethnicity level, while column (7) adds linear trends at the state-job level.

Appendix Table 10: Impact of PECC Bans on Hourly Wages: New Hires

	(1)	(2)	(3)
Panel A. Effect Separately by race/ethnicity			
1(Black)*1(Treated by Ban)	0.00770	0.0109	0.0290
	(0.0296)	(0.0310)	(0.0769)
1(Hispanic)*1(Treated by Ban)	-0.00278	0.0549**	-0.0223
	(0.0199)	(0.0209)	(0.0310)
1(Non-Hispanic white)*1(Treated by Ban)	0.00263	0.00331	-0.0351
	(0.0112)	(0.0137)	(0.0224)
Panel B. Overall Effect			
Treated by Ban	0.00188	0.0128	-0.0238
	(0.00802)	(0.0110)	(0.0144)
N	176,644	176,644	176,644
States	51	51	51
Ban States	10	10	10
Time-Race/Ethnicity Fixed Effects	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y
State-Time Fixed Effects	N	N	Y
State-Past Job-Race/Ethnicity Fixed Effects-Linear Trends	N	N	Y
Linear Trends-Past Job	N	N	N
Demographic Controls (-Race/Ethnicity)	N	N	N
State Policy/Economic controls (-Race/Ethnicity)	N	Y	N

Notes: This table reports linear model estimates of race/ethnicity-specific differences in (log) hourly wages for workers who did not lose their jobs following a PECC ban, using a variety of difference-in-differences and triple-difference strategies. Data are from the CPS for years 2003-2018. Column (1) presents results when controlling for state(-Race/Ethnicity) and year(-Race/Ethnicity) fixed effects. Column (2) adds extra controls at the state level. Column (3) adds state(-Race/Ethnicity) linear trends additionally to the controls in column (2). Standard errors clustered at the state level are shown in parentheses. 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. 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 and economy) are interacted by race-ethnicity dummies.

Appendix Table 11: Impact of PECC Bans on Hourly Wages: Long-Term Employees

	DD		DDD	
	(1)	(2)	(3)	(4)
Panel B. Effect Separately by race/ethnicity				
1(Black)*1(Treated by Ban)	0.0128	-0.0142	0.00360	-0.0216
	(0.0289)	(0.0336)	(0.0244)	(0.0357)
1(Hispanic)*1(Treated by Ban)	-0.0198**	-0.00193	-0.0110*	0.00417
	(0.00845)	(0.00816)	(0.00550)	(0.00765)
1(Non-Hispanic white)*1(Treated by Ban)	0.00357	0.0000317		
	(0.00775)	(0.00484)		
Panel B: Overall Effect				
Treated by Ban	-2.81e-05	-0.00160		
	(0.00840)	(0.00343)		
N	1,605,184	1,605,184	1,605,184	1,605,184
States	51	51	51	51
Ban States	10	10	10	10
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Past Job-Race/Ethnicity Fixed Effects-Linear Trends	N	N	Y	Y
Linear Trends-Past Job	N	N	N	N
State-Race/Ethnicity Linear Trends	N	Y	N	Y
Demographic Controls (-Race/Ethnicity)	N	N	N	N
State Policy/Economic controls (-Race/Ethnicity)	N	N	N	N

Notes: This table reports linear model estimates of race/ethnicity-specific differences in log(hourly wages) for workers who did not lose their jobs following a PECC ban, using a variety of difference-in-differences and triple-difference strategies. Data are from the CPS for years 2003-2018. Column (1) presents results when controlling for state(-Race/Ethnicity) and year(-Race/Ethnicity) fixed effects. Column (2) adds extra controls at the state level. Column (3) adds state(-Race/Ethnicity) linear trends additionally to the controls in column (2). Standard errors clustered at the state level are shown in parentheses. 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. 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 and economy) are interacted by race-ethnicity dummies.

Appendix Table 12: Impact of PECC Bans on Job-Finding by Other Observable Subgroups

Subgroup:	High Education		High Experience		High Expected Wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: by Subgroup ("Grp.")						
1(Grp=0)*1(Treated)	-0.0242	-0.0452	0.0129	-0.0108	-0.0311	-0.0497
	(0.0262)	(0.0386)	(0.0362)	(0.0512)	(0.0274)	(0.0452)
1(Grp=l)*1(Treated)	0.0630	0.0608	-0.00407	-0.0194	0.0262	0.0110
	(0.0478)	(0.0526)	(0.0330)	(0.0376)	(0.0390)	(0.0392)
p-value of differences:	0.05	0.03	0.72	0.85	0.85	0.17
Panel B: by Subgroup ("Grp.") * Race/Ethnicity						
1(Grp=0)*1(Treated)*1(Black)	-0.132**	-0.169**	-0.0700	-0.101	-0.131**	-0.165**
	(0.0556)	(0.0722)	(0.118)	(0.120)	(0.0617)	(0.0765)
1(Grp=l)*(Treated)*1(Black)	0.0485	0.0513	-0.113**	-0.139**	-0.0693*	-0.0836
	(0.111)	(0.0958)	(0.0453)	(0.0568)	(0.0419)	(0.0525)
p-value of differences:	0.21	0.10	0.75	0.75	0.37	0.25
1(Grp=0)*1(Treated)*1(Hisp.)	0.0621	0.0658	0.0891	0.0789	0.0381	0.0479
	(0.0509)	(0.0724)	(0.0578)	(0.0758)	(0.0526)	(0.0779)
1(Grp=l)*1(Treated)*1(Hisp.)	0.255	0.256	0.0761	0.0855	0.152**	0.148*
	(0.0744)	(0.0942)	(0.0528)	(0.0740)	(0.0744)	(0.0821)
p-value of differences:	0.01	0.02	0.82	0.91	0.14	0.18
1(Grp=0)*1(Treated)*1(White)	-0.0397	-0.0430	0.000186	-0.0100	-0.0390	-0.0420
	(0.0241)	(0.0406)	(0.0465)	(0.0621)	(0.0303)	(0.0430)
1(Grp=l)*1(Treated)*1(White)	0.0286	0.0339	-0.0162	-0.0205	0.00250	-0.00184
	(0.0469)	(0.0552)	(0.0279)	(0.0358)	(0.0289)	(0.0395)
p-value of differences:	0.13	0.12	0.73	0.83	0.13	0.13
N	343,262	343,262	343,262	343,262	343,262	343,262
States	51	51	51	51	51	51
Ban States	10	10	10	10	10	10
Treatment Level	State	State	State	State	State	State
Time-Race/Ethnicity FEs	Y	Y	Y	Y	Y	Y
State-Race/Ethnicity FEs	Y	Y	Y	Y	Y	Y
State-New Job-Race FEs	N	N	N	N	N	N
Demographic Controls	N	Y	N	Y	N	Y
State Policy/Econ. Controls	N	Y	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), (3), and (5) 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), (4), and (6) 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. High predicted wage is defined as having an above median predicted wage. We predict wages by estimating a Mincer-style wage regression, regressing log hourly wages on education, potential experience, years of education interacted with potential experience, potential experience squared, years of education squared, and two-digit industry and occupation dummies and interactions between industry and occupation dummies. 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.

Appendix Table 13: Impact of PECC Bans: CPS Interview Months 1-4

	(1)	(2)	(3)	(4)
	Job finding		Separations	
Panel A. State-level Variation				
Panel A1. Effect separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.136	-0.153	0.0118	0.00607
	(0.104)	(0.103)	(0.0135)	(0.0176)
1(Hispanic)*1(Treated by Ban)	0.0815	0.161	-0.0136	-0.00792
	(0.0552)	(0.112)	(0.00909)	(0.0183)
1(Non-Hispanic white)*1(Treated by Ban)	0.0285	0.0186	-0.0143***	-0.0160***
	(0.0386)	(0.0395)	(0.00323)	(0.00406)
Panel A2. Overall Effect				
1(Treated by Ban)	0.0163	0.0172	-0.0114***	-0.0122**
	(0.0306)	(0.0329)	(0.00365)	(0.00501)
N	180,994	180,994	65,640	65,640
States	51	51	51	51
Ban States	10	10	10	10
Panel B: Job-Level Variation				
Panel B1. Effects separately by race/ethnicity				
1(Black)*1(Treated by Ban)	-0.232*	-0.254*	0.0184	0.00822
	(0.134)	(0.132)	(0.0189)	(0.0251)
1(Hispanic)*1(Treated by Ban)	0.0197	0.0324	-0.0104	-0.00345
	(0.0703)	(0.0783)	(0.0109)	(0.0159)
1(Non-Hispanic white)*1(Treated by Ban)	0.0177	0.0192	-0.0177***	-0.0189***
	(0.0590)	(0.0621)	(0.00436)	(0.00591)
Panel B2. Overall Effect				
1(Treated by Ban)	-0.0142	-0.0124	-0.0120***	-0.0126***
	(0.0438)	(0.0493)	(0.00383)	(0.00460)
N	173,950	173,950	63,492	63,492
States	50	50	50	50
Ban States	9	9	9	9
Time-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	Y	N	Y
State Policy/Economic Controls (-Race/Ethnicity)	N	N	N	N
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y	Y	Y
Aggregate Level Regression	N	N	N	N
Sun & Abraham (2021)	N	N	N	N

Notes: This table reports estimates of the effects of PECC bans on job-finding and job separations restricting the sample to CPS interview months 1-4. Columns (1) and (2) report results for job-finding, while Columns (3) and (4) present results for separations for new-hires. Panel A reports estimates using state-level variation in exposure to PECC bans while Panel B reports estimates using job-level variation in exposure to PECC bans. Columns (1) and (3) report estimates using our baseline specification, while Columns (2) and (4) add controls for individual demographic characteristics. Column (1) and (2) report MLE estimates of hazard models of the effect of PECC bans on job-finding, while Columns (3) and (4) report linear probability models of the effect of PECC bans on separations for new hires. Data are from the CPS for years 2003 to 2018. Standard errors clustered at the state level are shown in parentheses. All controls are interacted by race-ethnicity indicators.

Appendix Table 14: Impact of PECC Bans on Part-Time Job-Finding

	(1) State Level	(2) Job-Level Variation
Panel A. State-level Variation		
Panel A1. Effect separately by race/ethnicity		
1(Black)*1(Treated by Ban)	-0.138	-0.183
	(0.0914)	(0.135)
1(Hispanic)*1(Treated by Ban)	0.152**	0.115
	(0.0683)	(0.0788)
1(Non-Hispanic white)*1(Treated by Ban)	-0.00110	-0.00854
	(0.0363)	(0.0507)
Panel A2. Overall Effect		
1(Treated by Ban)	0.0193	0.00445
	(0.0329)	(0.0415)
N	309,284	296,181
States	51	50
Ban States	10	9
Time-Race/Ethnicity Fixed Effects	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y
Demographic Controls (-Race/Ethnicity)	N	N
State Policy/Economic Controls (-Race/Ethnicity)	N	N
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y
Aggregate Level Regression	N	N
Sun & Abraham (2021)	N	N

Notes: This table reports estimates of the effects of PECC bans on finding part-time jobs using the same proportional hazards specification as in Table 2, but replacing the outcome of finding any job with the outcome of finding a part-time job. As in Table 2, all all columns report MLE estimates of hazard models that include the state-race/ethnicity and time-race/ethnicity fixed effects that implement difference-in-differences. Column (1) reports estimates using state-level variable in exposure to PECC bans, while Column (2) reports results using job-level variation in exposure to PECC bans (state-job-race/ethnicity fixed effects are included in this specification). Job-level exposure is measured using individual's expected job. Data are from the CPS for years 2003 to 2018. Standard errors clustered at the state level are shown in parentheses. All controls are interacted by race-ethnicity indicators.

Appendix Table 15: Impact of PECC Bans on Job-Finding: Different Baseline Hazards by Race

	(1)	(2)	(3)
Panel A. State-level Variation			
Panel A1. Effect separately by race/ethnicity			
1(Black)*1(Treated by Ban)	-0.102**	-0.0833**	-0.105**
	(0.0414)	(0.0348)	-0.0419
1(Hispanic)*1(Treated by Ban)	0.0795	0.0315	0.0838*
	(0.0490)	(0.0447)	(0.0495)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0129	-0.0083	-0.0107
	(0.0265)	(0.0211)	(0.0279)
Panel A2. Overall Effect			
1(Treated by Ban)	-0.000370	-0.0117	0.00162
	(0.0271)	(0.0171)	(0.0289)
N	343,239	6,535	343,239
States	51	51	51
Ban States	10	10	10
Time-Race/Ethnicity Fixed Effects	Y	Y	Y
State-Race/Ethnicity Fixed Effects	Y	Y	Y
Demographic Controls (-Race/Ethnicity)	N	N	Y
State Policy/Economic Controls (-Race/Ethnicity)	N	N	N
State-Past Job-Race/Ethnicity Fixed Effects	Y	Y	Y
Aggregate Level Regression	N	Y	N
Sun & Abraham (2021)	N	Y	N

Notes: This table reports estimates relaxing the proportional hazards assumption used in Table 2 by allowing for baseline hazards to vary by race/ethnicity. Data are from the CPS for years 2003 to 2018. Column (1) reports MLE estimates of hazard models that include the state-race/ethnicity and time-race/ethnicity fixed effects that implement difference-in-differences. Column (2) presents estimates models using Sun and Abraham (2021)'s interaction-weighted differences-in-differences estimator. This approach can only be implemented with linear models, so we aggregate job-finding rates to the year-by-quarters-unemployed-by-treatment level. We then estimate OLS models for the complementary-log-log of race/ethnicity-specific job-finding rate (i.e., ln(-ln(1-job-finding))). Column (3) returns to the MLE hazard specification estimated on individual level data and adds demographic controls fully interacted with race or ethnicity group to the specification in Column (1), including binned education, binned age, gender, and marital status, urbanicity, and interactions between month-of-year and Census division. Column (4) adds controls for state economic and policy variables. The controls for state economic and policy variables are: Saiz's price elasticity of housing multiplied by year indicators (Saiz (2010)), an indicator for whether the state had geological potential for fracking in the given year (Bartik et al. (2019)), the share of manufacturing jobs in 2000 multiplied by year indicators (ACS 2000), an indicator for whether the state had any Ban-the-Box policy, an indicator for whether the state had expanded Medicaid in the given year, year 2000 state Hispanic and foreign born share interacted with year indicators, and a measurement for unemployment insurance extensions during the Great Recession (Hsu et al. (2018)). Standard errors clustered at the state level are shown in parentheses. All controls are interacted by race-ethnicity indicators.

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