# 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 strategies, and present estimates of the effect of PECC bans on wages. Finally in Appendix Section D, we discuss our data sources and sample construction in detail.

# A Model Details

This appendix section presents further details on the model from Section 2.2. 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 $\mu_i$ is the individual's true match quality, $\delta_k$ 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, $h_{g,k} \neq h_{g',k}$.

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Following the normal-normal paramterization 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 $\mu_i$ 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 $\kappa$ in the absence of a signal ban is defined implicitly by,

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

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

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

where $\lambda_{g'}$ and $\lambda_g$ are group-specific success rates. Note that the expressions for $\lambda_{g'}$ and $\lambda_g$ 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 $\kappa$.

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

$$\frac{d\lambda_g}{dt} = \frac{\partial\lambda_g}{\partial t} + \frac{\partial\lambda_g}{\partial\kappa}\left[\frac{-\frac{\partial\lambda_{g'}}{\partial t} - m_g\frac{\partial\lambda_g}{\partial t}}{\frac{\partial\lambda_{g'}}{\partial\kappa} + m_g\frac{\partial\lambda_g}{\partial\kappa}}\right] \tag{A.10}$$

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

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

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, $\omega_g$, 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 $\delta_g$, that is added to relevant mean parameters for group $g$. Concretely, to model (1) we say $\delta_g$ 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 $\delta_g$-shift in $\mu_{g,0}$ only: that is, priors are biased by $\delta_g$, but signal realizations and post-match perceptions are both unbiased. We model (3) as an $\delta_g$-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 $\delta_g$ 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 updated to include $f$ as follows,

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

[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.

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 $\delta_g$-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 $\delta_g$ 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 $\kappa$ in each case.

To estimate $\kappa$, we first calibrate each group's population share $m_r$ using estimates of the unemployed population from Table 2. We calibrate the number of positions to be filled, $M \in [0, 1]$, using Table 3 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 $\kappa$ numerically given any putative set of model parameters. For an arbitrary number of groups $g$, the hiring threshold $\kappa$ in the more general case is defined implicitly by

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

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

To solve for $\kappa$, 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 $\kappa$ is then defined by the minimum of these $M$ highest posteriors. The simulation sample size $N$ begins with $N = 10^6$ and adjusts upward to $N = 10^8$ as our search across possible parameter values gets closer to convergence, to aid with the precision of the simulated model moments.

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 $\kappa$. 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.[3] The expectation in the first term is likewise solved for numerically using the $m_g N$ draws from each group.

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

[3]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.

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.

We also explore robustness to using other estimates of PECC bans' effects, both using job-level variation (Panel B of Table 4, column (3) of Table 5) and using state-level and demographic controls (column (3) of Table 4, column (6) of Table 5). All of the results emphasized in the qualitative discussion of the model simulations in Section 5.5 are robust to using these alternative sets of empirical moments; magnitudes are also roughly unchanged and the relative sizes of parameter values remain the same. The primary differences across these sets of moments are that Black job-seekers' relative disadvantage in baseline signal precision becomes somewhat more pronounced when using moments based on job-level variation, and Hispanic job-seekers' relative disadvantage in PECC signal precision becomes more pronounced using both of these alternative sets of moments.

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 6 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 $\delta$, as described above in Appendix Section A, for minority applicants.[4] 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 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.

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

# C Further Empirical Results

In this section, we present additional discussion related to the magnitude of our estimates, results from various robustness checks, and evidence on the effects of PECC bans on wages.

## C.1 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 13 percent reduction in unemployment benefits using our estimates from the CPS and a 6 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.

## C.2 Robustness Discussion

This section discusses in more detail the results described in Section 5.3, where we explore robustness to alternative identifying assumptions, more demanding specifications, and alternative datasets. We begin by studying job-finding in our supplementary dataset, the LEHD J2J data described earlier in Section 3.3. 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 newly unemployed job-seekers at the state-race/ethnicity-time level.

Figure 2 reports estimates from event-time versions of Equation 4.11 using the LEHD J2J, where we plug in observed job-finding rates $b_{s,r,t}(\tau)$ for $\lambda^d_{s,r,t}(\tau)$ 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 1 shows coefficient estimates that correspond to the LEHD J2J event-time plots in Appendix Figure 2. 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) adds state-time fixed effects to Column (1), creating a more-demanding triple-difference estimator as in Equation 4.3. Finally, Column (4) adds state-race/ethnicity linear trends to Column (3). 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 and less precise relative to the estimates in Column (1). When we add state-time fixed effects in Column (3), 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 (4), the point-estimates for Black job-seekers roughly doubles in magnitude relative to Column (3), while the Hispanic estimate also increases in magnitude. Columns (5)-(8) reproduce Columns (1)-(4), 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.

We graphically validate the absence of pre-trends for Black job-seekers in the specification from Column (3), using event-time estimates corresponding to this triple-difference estimator. These estimates are shown in Appendix Figure 3. Looking at Panel A, which shows the evolution of Black job-finding hazards compared to white-job finding hazards in PECC-ban states compared to non-ban states, we confirm there was little discernible trend prior to PECC bans' implementation. After PECC bans are introduced, Black job-finding rates steadily decline relative to white job-finding rates. Black job finding rates seem to recover somewhat around 10 quarters after the PECC bans are passed. Turning to Panel B, we see that there may be a slight downward trend in job-finding rates for Hispanic job-seekers that decelerates slightly after PECC bans were introduced. This explains the sensitivity of the estimates for Hispanic job-seekers in Panel A of Appendix Table 2 to the inclusion of state-race/ethnicity linear trends.

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. Six of the eight estimates of the effect of PECC on Black job-finding in the LEHD J2J are negative, and five of the eight estimates are economically large in magnitude, 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 demanding specifications in Columns (4) and (8) are both economically significant 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 $t+1$.[5] 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 Panel B of Table 2 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 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 −10.6 percent to −8.2 percent, roughly 40% closer to the LEHD-J2J estimate of −4.6 percent. In Column (6), 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 and about 60% closer relative to the LEHD-J2J estimate. 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.

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 3. Column (1) repeats our baseline analysis of CPS job-finding from Column (1) of Table 4. Column (2) then adds, as in our LEHD J2J results, state-race/ethnicity linear trends to our baseline CPS specification in Equation 4.10. 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 (3), 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. The estimated effect for Hispanic job-seekers is indistinguishable from the estimate in Column (1). Appendix Figure 4 reports the event studies corresponding to Column (3), showing relatively little pre-trend for Black job-seekers and a steady decline in job-finding rates after PECC bans go into effect. Conversely, Hispanic job-finding rates jump up in the first two years after a PECC ban goes into effect, but then decline to around their previous levels. Finally, in Column (4) we add state-race/ethnicity specific linear trends to the triple-difference specification in Column (3). This change roughly triples the estimated effect

[5]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.

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. Columns (5)-(8) repeat columns (1)-(4) but add controls for state level covariates that vary over time and proxy for economic and policy shocks. Adding these covariates substantially increases the magnitude of the estimated effect for Black job-seekers, while having mixed effects for Hispanic job-seekers. Adding these controls roughly halves the point estimates for white job-seekers.

Finally, in Appendix Table 4 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 4. 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 (3), which shows that adding state-time effects to Column (1) 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 less than half its previous size. Adding state-race/ethnicity linear trends in Column (4) further reduces the Hispanic estimate, while increasing the estimate for Black new hires by another fifty percent. These changes should not be surprising given the substantial pre-trends visible for Hispanic new hires in the event-time version of Column (3), which is shown in Appendix Figure 5. Again, as in Appendix Table 2, adding controls for state-time covariates in Columns (5)-(8) substantially increases the magnitude of the estimated effects for Black new hires, confirming that our results are not driven by PECC bans being confounded with other state-level policy or economic shocks. Overall, Appendix Table 4 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.

## C.3 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 5 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 5 and Columns (1), (5), and (6) in Appendix Table 4. Starting with Column (1), we see that the estimated overall effect of PECC bans on hourly wages is small, only $-.4$ 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 6. Similar to the results in Appendix Table 5, 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 our main 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 5 and 6 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 5 and 6 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.

# 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.[6] 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;[7] 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.

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 we discussed in Section 3.4 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

[6]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.

[7]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.

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 the legal sources detailed in Section 3.1 and, when necessary, our judgment. Finally, consistent with the PECC ban statutes, we code a job as exempt whenever either its industry or occupation is coded as exempt.

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)

## 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 4.10 when only population-average job-finding rates are observed, as in our LEHD J2J data, rather than individual job-finding outcomes, as in the CPS. 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 $\tau$ 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 4.8 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 4.10 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 4.10, 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 $\epsilon$ in the dependent variable[8]:

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

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

We find that $\epsilon$ 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 4.10 on the LEHD J2J, we exclude states in which any race or ethnic group ever has fewer than 500 unemployed individuals at the

[8]Where Bin refers to the Binomial distribution.

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 3.3, we only estimate job-finding models in the LEHD J2J on a single length of unemployment duration $\tau$, the intermediate category of "adjacent-quarter flows." Because the data are available at a quarterly frequency 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 $\text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau))$.

## 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.[9] 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 2, the number of pre and post-ban years varies between treatment states. Consequently, for states implementing PECC bans, we restrict the sample to a balanced set of pre and post-ban years common to all states, which is 3 years before the bans' implementation and 4 years afterwards.

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.

## 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

[9] 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.

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.[10] 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.[11] Figure 2 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.[12]

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.[13] 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 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.

[10]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.

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

[12]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.

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

## 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.[14]

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.

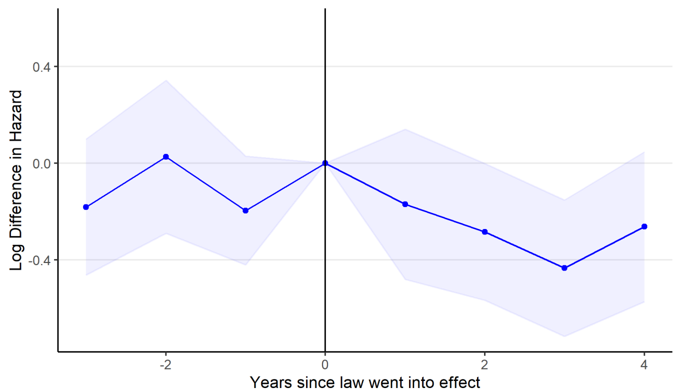
[14] To be precise, all standard errors are clustered at the state level, except for the standard errors displayed as confidence intervals in Figure 3, which needed to be clustered at the state-race/ethnicity level due to computational constraints.

# E Appendix Figures

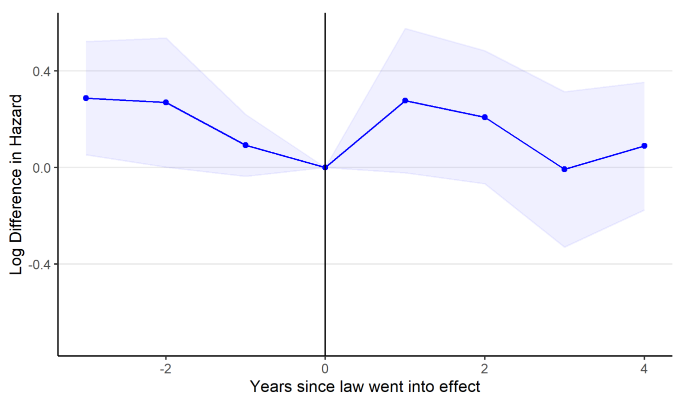
## Appendix Figure 1: Event-Time Analysis of the Effect of PECC Bans on Job-Finding Using Job-Variation

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

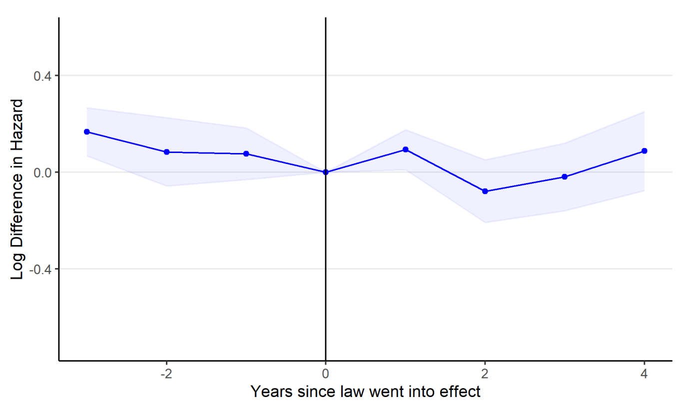
**(a)** Black




**(b)** Hispanic




**(c)** White

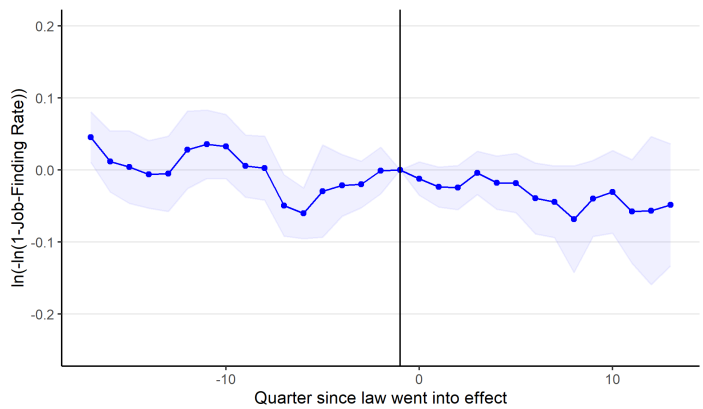



*Notes:* This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed individuals between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. Each panel shows results for a different race or ethnic group. The reported coefficients come from estimating with MLE a version of the proportional hazards model in Equation 4.11, 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, $\kappa_{st}$. 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.

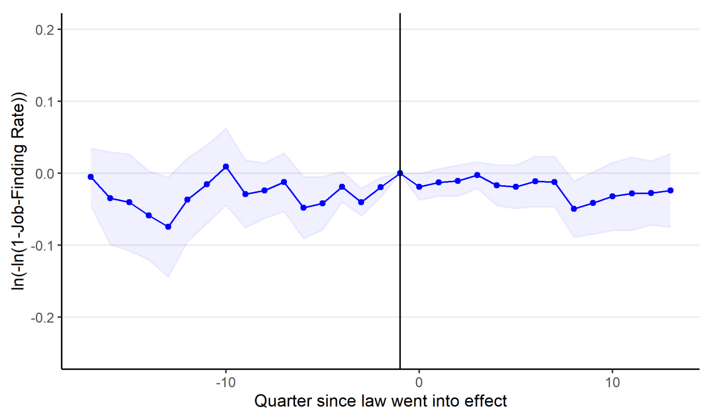
## Appendix Figure 2: Event-Time Analysis of the Effect of PECC Bans on Job-Finding: LEHD J2J

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

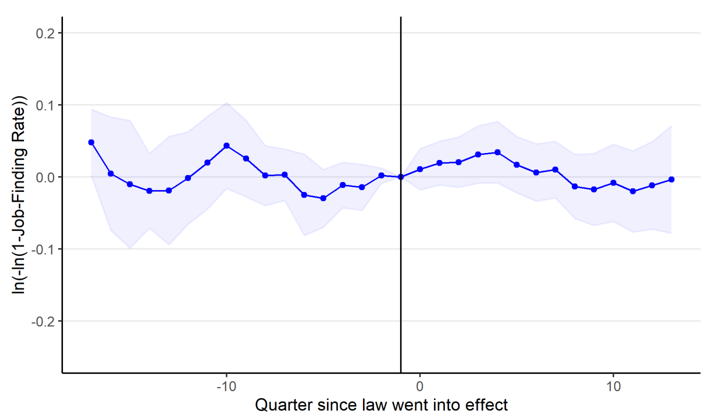
**(a)** Black




**(b)** Hispanic




**(c)** White

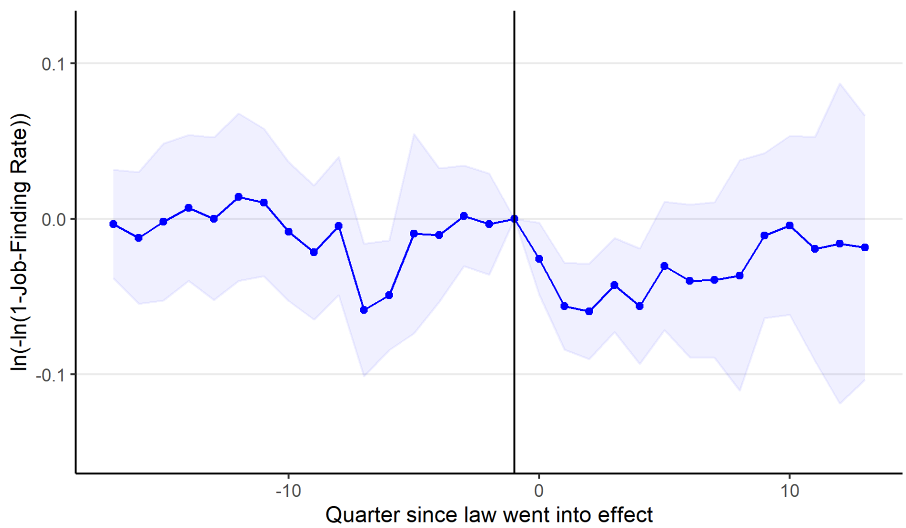



*Notes:* This figure shows the results of an event-time analysis of the difference in the complementary log-log of the average job-finding rate (i.e., $\ln(-\ln(1 - \text{job-finding rate}))$) between states banning and not banning Pre-Employment Credit Checks (PECCs) before and after the PECC bans went into effect. The reported coefficients come from estimating a version of Equation 4.2 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, $\kappa_{st}$. 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.

**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, Time-State FE

**(a)** Black



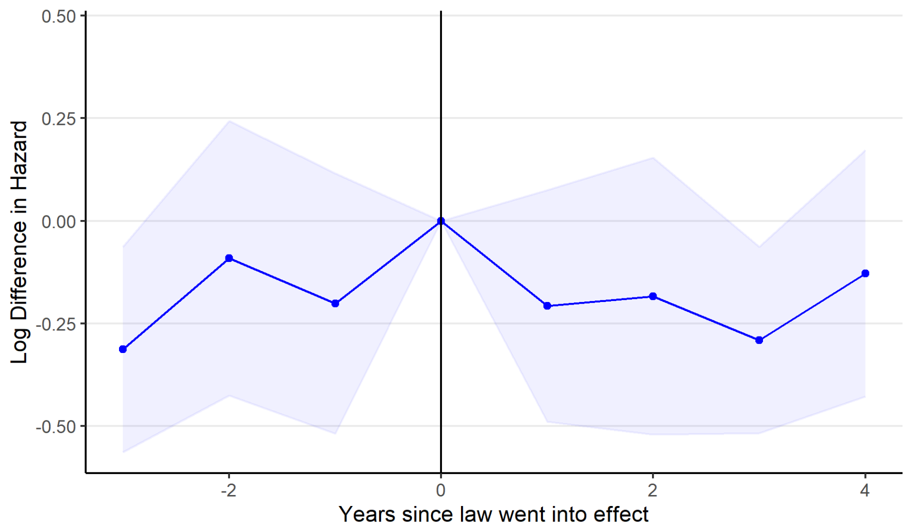

**(b)** Hispanic


*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 4.3 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, $\kappa_{st}$. 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, state policy and economic controls interacted with race-ethnicity dummies, and year-quarter-state fixed effects. The inclusion of state-quarter-year fixed effects means the reported coefficients should be interpreted as the difference between PECC-banning states and others states in the *difference* in the job-finding rate between Black or Hispanic job-seekers and white job-seekers. Regressions are 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-race/ethnicity level.

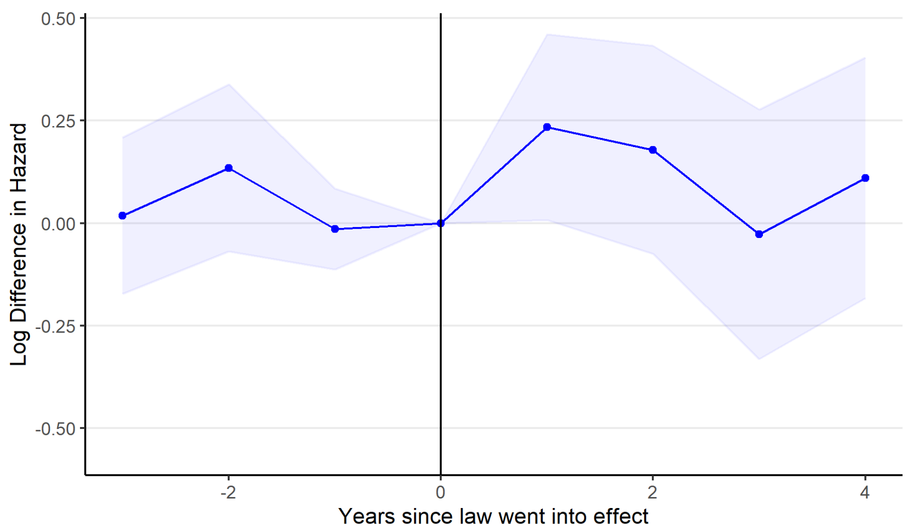
**Appendix Figure 4: Event-Time Analysis of the Effect of PECC Bans on Job-Finding: CPS**

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

**(a)** Black




**(b)** Hispanic



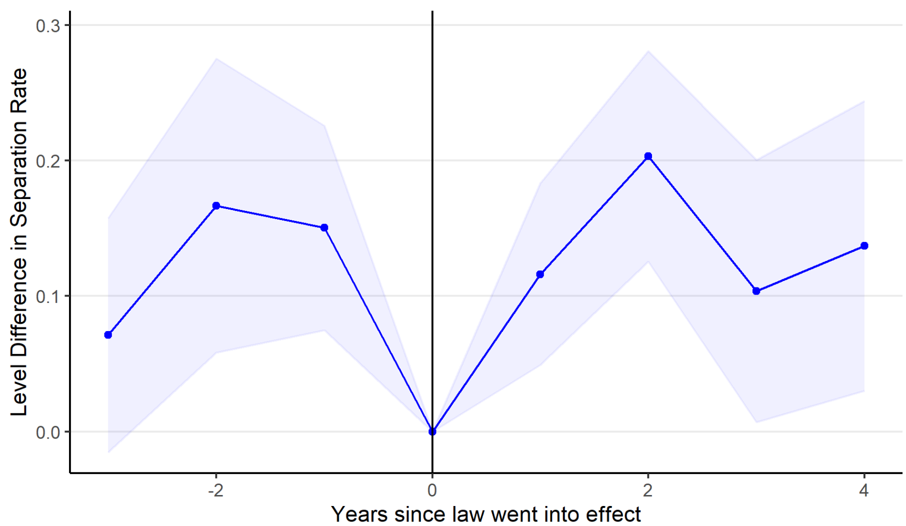

ZX

*Notes:* This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed Black and Hispanic 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 group. The reported coefficients come from estimating with MLE a version of the proportional hazards model in Equation 4.11 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, $\kappa_{st}$, and where we add state-time fixed effects to the original specification. 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. In addition to state-time fixed effects, the model also includes time-race/ethnicity, state-race/ethnicity fixed effects, individual demographic characteristics interacted with race-ethnicity dummies, and state policy and economic controls interacted with race-ethnicity dummies. The inclusion of state-time fixed effects means the reported coefficients should be interpreted as the difference between PECC-banning states and others states in the *difference* in the job-finding hazard between Black or Hispanic job-seekers and white job-seekers. 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.

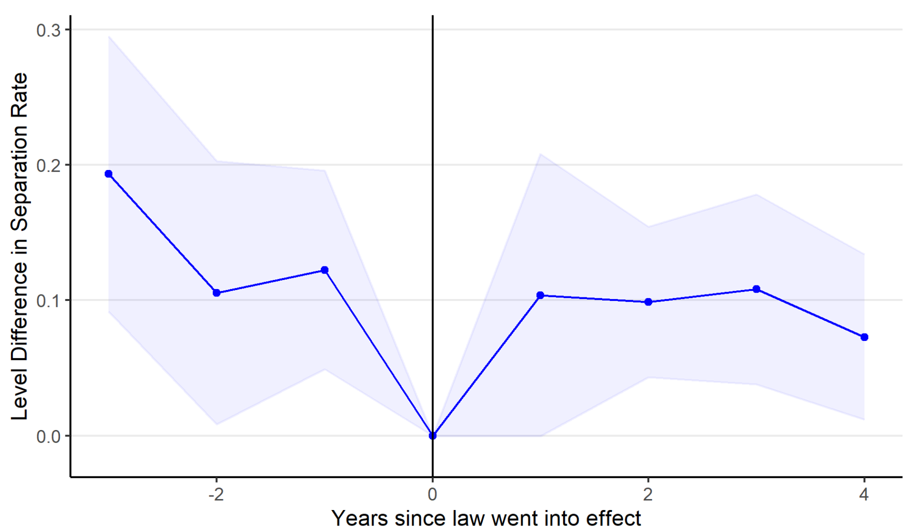
## Appendix Figure 5: Event-Time Analysis of the Effect of PECC Bans on Separations: New Hires

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

**(a)** Black




**(b)** Hispanic




*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 group. The reported coefficients come from estimating a modified linear probability model of Equation 4.3 where we interact an indicator for being covered by a PECC ban, $D_{s(i),t}$, with indicators for event time, $\kappa_{st}$. Event time is defined as the calendar year and month, $t$, minus the year and month that a PECC ban took effect in state $s$. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race/ethnicity, state-race/ethnicity fixed effects, individual demographic characteristics interacted with race-ethnicity dummies, state policy and economic controls interacted with race-ethnicity dummies, and state-time fixed effects. The inclusion of state-time fixed effects means the reported coefficients should be interpreted as the difference between PECC-banning states and others states in the *difference* in the separation rate between Black or Hispanic job-seekers and white job-seekers. 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.

# F Appendix Tables

**Appendix Table 1:** Robustness of Impact of PECC Bans on Job-Finding: LEHD-J2J

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Separately by race/ethnicity** | | | | | | | | |
| 1(Black)*1(Treated by Ban) | -0.0458* | 0.0126 | -0.0149 | -0.025* | -0.0344 | 0.0079 | -0.024 | -0.033* |
| | (0.03124) | (0.01062) | (0.01195) | (0.01858) | (0.02763) | (0.01342) | (0.02064) | (0.02151) |
| 1(Hispanic)*1(Treated by Ban) | 0.0201 | 0.0601*** | 0.002 | 0.0148 | 0.0066 | 0.0076 | -0.0079 | -0.023** |
| | (0.02312) | (0.02251) | (0.00862) | (0.01941) | (0.01241) | (0.01293) | (0.00926) | (0.01363) |
| 1(Non-Hispanic white)*1(Treated by Ban) | -0.0129 | 0.0313** | | | 0.0044 | 0.0345** | | |
| | (0.01805) | (0.018) | | | (0.01936) | (0.01501) | | |
| **Panel A. Overall Effect** | | | | | | | | |
| 1(Treated by Ban) | -0.0098 | 0.0352** | | | -0.0002 | 0.0267** | | |
| | (0.01911) | (0.01672) | | | (0.01871) | (0.01298) | | |
| N | 5700 | 5700 | 5700 | 5700 | 5700 | 5700 | 5700 | 5700 |
| States | 44 | 44 | 44 | 44 | 44 | 44 | 44 | 44 |
| Ban States | 7 | 7 | 7 | 7 | 7 | 7 | 7 | 7 |
| Time-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Time Fixed Effects | N | N | Y | Y | N | N | Y | Y |
| State-Race/Ethnicity Linear Trends | N | Y | N | Y | N | Y | N | Y |
| Demographic Controls (-Race/Ethnicity) | N | N | N | N | N | N | N | N |
| State Policy/Economic controls (-Race/Ethnicity) | N | N | N | N | Y | Y | Y | Y |

*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) 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 (5) , (6), (7), and (8) add to the specifications in columns (1) , (2), (3), and (4) 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 (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 2:** Robustness of Impact of PECC Bans on Job-Finding: LEHD-J2J and CPS Comparison Subsample

| | (1) | (2) | (3) |
|---|---|---|---|
| **Panel A. LEHD J2J** | | | |
| *Panel A1. Separately by race/ethnicity* | | | |
| 1(Black)*1(Treated by Ban) | | -0.0458* | -0.0344 |
| | | (0.03124) | (0.02763) |
| 1(Hispanic)*1(Treated by Ban) | | 0.0201 | 0.0066 |
| | | (0.02312) | (0.01241) |
| 1(Non-Hispanic white)*1(Treated by Ban) | | -0.0129 | 0.0044 |
| | | (0.01805) | (0.01936) |
| *Panel A2. Overall Effect* | | | |
| 1(Treated by Ban) | | -0.0098 | -0.0002 |
| | | (0.01911) | (0.01871) |
| N | | 5700 | 5700 |
| States | | 44 | 44 |
| Ban States | | 7 | 7 |
| **Panel B. CPS** | | | |
| *Panel B1. Separately by race/ethnicity* | | | |
| 1(Black)*1(Treated by Ban) | -0.106** | -0.0820 | 0.00386 |
| | (0.0416) | (0.157) | (0.154) |
| 1(Hispanic)*1(Treated by Ban) | 0.0887* | 0.124 | 0.0711 |
| | (0.0489) | (0.0782) | (0.175) |
| 1(Non-Hispanic white)*1(Treated by Ban) | -0.00649 | -0.0834 | -0.102 |
| | (0.0283) | (0.0616) | (0.0684) |
| *Panel B2. Overall Effect* | | | |
| 1(Treated by Ban) | 0.00537 | -0.0240 | -0.0551 |
| | (0.028) | (0.0546) | (0.0669) |
| N | 342,049 | 45,065 | 45,065 |
| States | 51 | 44 | 44 |
| Ban States | 10 | 7 | 7 |
| Adjascent Quarter Separations Sample | N | Y | Y |
| Time-Race/Ethnicity Fixed Effects | Y | Y | Y |
| State-Race/Ethnicity Fixed Effects | Y | Y | Y |
| State-Time Fixed Effects | N | N | N |
| State-Race/Ethnicity Linear Trends | N | N | N |
| Demographic Controls (-Race/Ethnicity) | N | N | N |
| State Policy/Economic controls (-Race/Ethnicity) | 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 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 B reports MLE estimates of race/ethnicity-specific log differences in job-finding hazard rates following a PECC ban, using a variety of difference-indifferences and triple-difference models and data from the CPS for years 2003-2018. Panel B Columns (2) and (3) restricts 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. In Panel A (LEHD-J2J) and Panel B columns (2) and (3), 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 3:** Robustness of Impact of PECC Bans on Job-Finding: CPS

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Separately by race/ethnicity** | | | | | | | | |
| 1(Black)*1(Treated by Ban) | -0.106** | -0.190 | -0.0743 | -0.322* | -0.126** | -0.189 | -0.0731 | -0.262 |
| | (0.0416) | (0.184) | (0.0683) | (0.190) | (0.0572) | (0.170) | (0.0800) | (0.198) |
| 1(Hispanic)*1(Treated by Ban) | 0.0887* | 0.110 | 0.0769 | 0.0534 | 0.0744 | 0.284** | 0.107 | 0.232 |
| | (0.0489) | (0.0687) | (0.0512) | (0.0987) | (0.0687) | (0.118) | (0.0736) | (0.150) |
| 1(Non-Hispanic white)*1(Treated by Ban) | -0.00649 | 0.0760** | | | -0.0154 | 0.0309 | | |
| | (0.0283) | (0.0387) | | | (0.0366) | (0.0537) | | |
| **Panel B. Overall Effect** | | | | | | | | |
| 1(Treated by Ban) | 0.00537 | 0.0516** | | | -0.0167 | 0.0423 | | |
| | (0.028) | (0.025) | | | (0.0331) | (0.0298) | | |
| N | 342,049 | 342,049 | 340,788 | 340,788 | 342,049 | 342,049 | 340,788 | 340,788 |
| States | 51 | 51 | 51 | 51 | 51 | 51 | 51 | 51 |
| Ban States | 10 | 10 | 10 | 10 | 10 | 10 | 10 | 10 |
| Time-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Time Fixed Effects | N | N | Y | Y | N | N | Y | Y |
| State-Race/Ethnicity Linear Trends | N | Y | N | Y | N | Y | N | Y |
| Demographic Controls (-Race/Ethnicity) | N | N | N | N | N | N | N | N |
| State Policy/Economic controls (-Race/Ethnicity) | N | N | N | N | Y | Y | Y | Y |

*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-indifferences and triple-difference models and data from the CPS for years 2003-2018. 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) 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 (5) , (6), (7), and (8) add to the specifications in columns (1) , (2), (3), and (4) extra controls at the state level. 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 4:** Robustness of Impact of PECC Bans on Separation: CPS

| | DD | | DDD | | DD | | DDD | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Panel A: Effect separately by race/ethnicity** | | | | | | | | |
| 1(Black)*1(Treated by Ban) | 0.0266* | 0.0542** | 0.0342** | 0.0749** | 0.0436* | 0.0636** | 0.0477* | 0.104** |
| | (0.0145) | (0.0260) | (0.0147) | (0.0283) | (0.0242) | (0.0262) | (0.0257) | (0.0388) |
| 1(Hispanic)*1(Treated by Ban) | -0.0453*** | -0.0104 | -0.0170 | 0.0269 | -0.0113 | 0.0566 | 0.00428 | 0.105** |
| | (0.0142) | (0.0359) | (0.0177) | (0.0532) | (0.0180) | (0.0471) | (0.0176) | (0.0498) |
| 1(Non-Hispanic white)*1(Treated by Ban) | -0.0235*** | -0.0142 | | | -0.0203** | -0.0224 | | |
| | (0.00807) | (0.0148) | | | (0.00855) | (0.0170) | | |
| **Panel B: Overall effect** | | | | | | | | |
| 1(Treated by Ban) | -0.0228*** | -0.00436 | | | -0.0102 | 0.00465 | | |
| | (0.00751) | (0.0124) | | | (0.0101) | (0.0173) | | |
| N | 54,160 | 54,160 | 53,726 | 53,726 | 54,160 | 54,160 | 53,726 | 53,726 |
| States | 51 | 51 | 51 | 51 | 51 | 51 | 51 | 51 |
| Ban States | 10 | 10 | 10 | 10 | 10 | 10 | 10 | 10 |
| Time-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Race/Ethnicity Fixed Effects | Y | Y | Y | Y | Y | Y | Y | Y |
| State-Time Fixed Effects | N | N | Y | Y | N | N | Y | Y |
| State-Race/Ethnicity Linear Trends | N | Y | N | Y | N | Y | N | Y |
| Demographic Controls (-Race/Ethnicity) | N | N | N | N | N | N | N | N |
| State Policy/Economic controls (-Race/Ethnicity) | N | N | N | N | Y | 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 8. Column (2) adds controls for linear trends at the state(-race/ethnicity) level. Column (3) adds state-time effects to the specification from Column (1), implementing a triple-difference estimator. Column (4) then augments the triple-difference model with linear trends at the state-race/ethnicity level. Column (5) , (6), (7), and (8) add to the specifications in columns (1) , (2), (3), and (4) extra controls at the state level. 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 5:** 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) | 0.00932 | 0.0213 | 0.00731 |
| | (0.0309) | (0.0300) | (0.0707) |
| 1(Hispanic)*1(Treated by Ban) | -0.00861 | 0.0408* | -0.0122 |
| | (0.0187) | (0.0211) | (0.0291) |
| 1(Non-Hispanic white)*1(Treated by Ban) | -0.00508 | 0.00432 | -0.0361 |
| | (0.00924) | (0.0110) | (0.0261) |
| **Panel B: Overall effect** | | | |
| 1(Treated by Ban) | -0.00431 | 0.0127 | -0.0259* |
| | (0.00877) | (0.0102) | (0.0148) |
| N | 268,864 | 268,864 | 268,864 |
| 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 | N |
| State-Race/Ethnicity Linear Trends | N | N | Y |
| Demographic Controls (-Race/Ethnicity) | N | N | N |
| State Policy/Economic controls (-Race/Ethnicity) | N | Y | Y |

*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 6:** Impact of PECC Bans on Hourly Wages: Long-Term Employees

| | DD | | DDD | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: Effect separately by race/ethnicity** | | | | |
| 1(Black)*1(Treated by Ban) | 0.0139 | -0.0144 | 0.00512 | -0.0227 |
| | (0.0287) | (0.0342) | (0.0250) | (0.0353) |
| 1(Hispanic)*1(Treated by Ban) | -0.0190** | -0.00265 | -0.00998* | 0.00327 |
| | (0.00849) | (0.00815) | (0.00555) | (0.00776) |
| 1(Non-Hispanic white)*1(Treated by Ban) | 0.00374 | 0.000412 | | |
| | (0.00760) | (0.00493) | | |
| **Panel B: Overall effect** | | | | |
| Treated by Ban | 0.000370 | -0.00148 | | |
| | (0.00821) | (0.00378) | | |
| N | 1,513,664 | 1,513,664 | 1,513,664 | 1,513,664 |
| 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-Time Fixed Effects | N | N | Y | Y |
| 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.

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