Labor Market Effects of Deleting Delinquencies*

Gonzalo Maturana† Jordan Nickerson‡ Santiago Truffa§

September, 2020

We develop a simple model to examine the implications of prohibiting the use of credit histories in hiring practices. We empirically test the model using a recent law implemented in Chile. This law extended periods of unemployment for low-income workers, consistent with the pooling equilibrium in the market for talent predicted by our model. Moreover, these effects are particularly large for younger workers and female workers. While laws that ban credit checks for hiring purposes continue to gain traction, our paper highlights that these laws may not benefit all low-income workers and may instead lead to cross-subsidization within this group.

JEL classification: J01, J60, M51, D80

keywords: unemployment, hiring, credit reports, information

*We thank Felipe Aldunate, Mark Egan, Andrew Ellul, William Grieser, Sean Higgins, Cristobal Huneeus, Andres Liberman, Song Ma, as well as seminar and conference participants at Texas Christian University, Universidad de los Andes Chile, Universidad Diego Portales, the 2019 Finance UC International Conference, and the 2019 Kelley Junior Finance Conference for helpful comments. We also thank Cangyuan Li for excellent research assistance. Supplementary results can be found in an Internet Appendix at the authors' websites.

†Goizueta Business School, Emory University. Email: gonzalo.maturana@emory.edu.

‡Sloan School of Management, MIT. Email: jordo@mit.edu.

§ESE Business School, Universidad de los Andes Chile. Email: struffa.ese@uandes.cl.

Labor Market Effects of Deleting Delinquencies

September, 2020

Abstract – We develop a simple model to examine the implications of prohibiting the use of credit histories in hiring practices. We empirically test the model using a recent law implemented in Chile. This law extended periods of unemployment for low-income workers, consistent with the pooling equilibrium in the market for talent predicted by our model. Moreover, these effects are particularly large for younger workers and female workers. While laws that ban credit checks for hiring purposes continue to gain traction, our paper highlights that these laws may not benefit all low-income workers and may instead lead to cross-subsidization within this group.

Should the information set considered by economic agents be regulated? If so, what are the consequences of such restrictions? This question is becoming ever more relevant as the volume and accessibility of information rapidly increases, giving rise to terms such as *big data*. As analog and digital footprints expand, it is natural for rational agents to gather and incorporate these signals into their decision-making process (Berg et al. (2019)). Labor markets serve as a prominent example of this practice. Employers no longer evaluate candidates solely by their resumes but instead gather information from additional sources such as general online job boards and social media profiles. In this paper, we study the labor market implications associated with restrictions placed on the use of one such signal: an individual's credit report.

The use of credit reports in the hiring process has not been exempt from controversy. Advocates for this practice argue that existing signals (e.g., resumes) lack sufficient precision and that information embedded in a credit report can help protect a firm and other workers from low-quality employees.[1] Challengers to this view argue that there is insufficient evidence to support the notion that credit history correlates with worker quality. Moreover, this practice is likely to be discriminatory in nature, since it has a disparate impact on many subsets of the workforce (Demos (2016)). As a result, several countries, including many states in the U.S., have enacted bans on employer credit checks.[2] More recently, on January 29, 2020, the U.S. House of Representatives passed an amendment to the Fair Credit Reporting Act by way of the Comprehensive CREDIT Act of 2020. The amendment prohibits the use of credit information for most hiring decisions. While yet to be considered by the Senate, this step highlights the attention currently being given to the topic by policymakers.

To analyze the implications that restricting the use of financial information poses for hiring decisions, we develop a model of information frictions in the market for talent in the

[1]For example, Experian states that credit checks help make better hiring decisions because "[c]redit information provides insight into an applicant's integrity and responsibility towards his or her financial obligations" (`http://www.experian.com/consumer-information/employment-credit-checks.html`).

[2]As of February 2020, 11 U.S. states and the District of Columbia have passed laws limiting the use of credit reports in hiring decisions. Other countries that have implemented similar policies include Canada, Germany, and the United Kingdom.

spirit of Hermalin and Weisbach (1998). This model provides intuition for the restriction's effect on labor outcomes such as job finding rates and wages. We focus on the role of hidden information. The model illustrates that banning the use of financial information increases the uncertainty regarding a worker's hidden productivity, thereby increasing the duration of job-seeking spells. The critical mechanism highlighted by the model is that the exclusion of credit report information leads to a noisier labor market from the employer's perspective. This uncertainty leads to tighter hiring rules, which decreases the unconditional probability of a job candidate receiving an offer. On the other hand, from the firm's perspective, this uncertainty also increases the option value of finding a qualified worker. Thus, equilibrium wages increase when the information set is restricted.

We investigate the implications of this model by exploiting a nationwide policy change implemented in Chile. In October 2010, the Chilean Congress approved Law 20,453 (hereafter, "the law"), which significantly limited employers' ability to use an applicant's credit information for hiring purposes. After the law, employers found it more difficult to distinguish applicants who have poor credit histories from applicants with no negative credit events.

Using detailed employment data from a sample of more than 137,000 workers, we examine the effect of this policy change on labor market outcomes. Unfortunately, our data do not contain disaggregated credit histories for the workers we observe. Given this limitation, we instead focus on the heterogeneous effects of the policy change across workers. Arguably, the policy change had a disproportionate effect on low-income workers, who are more likely to have poor credit histories (Comision Para el Mercado Financiero (2019)). As such, a credit history likely serves as a more precise signal for a low-income worker, making it more likely to be used in the hiring process.[3] This assertion is consistent with Cortes, Glover, and Tasci (2019), who find stronger effects of credit report bans on job vacancy rates in counties with high concentrations of subprime households. Consistent with our model, we begin by

[3]Anecdotal evidence gained through conversations with Chilean employers is consistent with increased usage of credit histories when hiring workers for low-income positions.

using a difference-in-differences framework to demonstrate that after the law, low-income unemployed workers require 11 more days to find a new job relative to their high-income counterparts. This effect is equivalent to a 7% increase in the average unemployment spell in our sample (i.e., 5.1 months). We examine the robustness of this result in two ways. First, we verify that this effect is not being driven by a change in a particular economic sector. Second, to alleviate concerns regarding the use of a difference-in-differences approach to compare low- and high-income workers, we repeat our analysis using an adaptation of the synthetic control method (Abadie and Gardeazabal (2003); Abadie, Diamond, and Hainmueller (2010)). Our inferences remain unchanged.

A useful feature of our data is the ability to observe detailed worker characteristics. Thus, we also investigate the law's effect on particular subpopulations of workers. First, we examine the effects of the law when portioning workers by age. We find that the law has a stronger effect on younger workers, especially for young workers who are old enough to have credit histories. Second, we examine subsamples of workers split by gender. Strikingly, we find that the effect of the law is 2.8 times greater for female workers relative to their male counterparts. Finally, we find that the law had the greatest impact on low-income workers who reside in lower income areas. Overall, these last two results are consistent with the statistical discrimination of female workers and workers who reside in lower-income areas.

Next, we investigate the effect of the law on the new wage that an unemployed worker earns conditional on finding another job. Consistent with the model, we find that low-income unemployed workers experience a wage increase of 3.2% relative to high-income workers. Taken together, the results on unemployment spells and equilibrium wages are consistent with the theoretical predictions derived from our model of restricting the information set used by employers.

Finally, we investigate whether restricting the use of credit histories by employers leads to less efficient matches along other dimensions. Intuitively, impacted workers may trade-off other non-monetary features in the set of prospective job openings to partially offset the increased difficulty in securing reemployment. We find no distinguishable effects of the law on (a) the likelihood of workers finding jobs that require longer commutes or on (b) the likelihood of workers switching to another industry.

Taken together, these results are consistent with the theoretical predictions derived from our model, in which job applicants are pre-screened according to their credit histories. Yet, we cannot rule out the possibility of an omitted variable correlated with the job prospects of low-income workers. For instance, it is possible that the temporary concealment of prior delinquencies also eases the credit constraints of low-income workers, permitting a greater degree of selectivity over potential job opportunities. However, this contradicts prior literature that documents an overall *reduction* in credit when the consideration of past credit defaults is prohibited in lending practices (Liberman et al. (2018)). Additionally, it is plausible that the enactment of the law coincided with a shift in immigration into the region, thereby increasing the supply of potential workers relative to job vacancies. However, data from the Chilean Immigration Department shows that immigration remained stable and represented a negligible fraction of the workforce during the years surrounding the law.

Our paper relates to a growing literature that studies recent bans on the use of credit reports to screen job applicants. Interestingly, the existing literature finds conflicting evidence on how these bans affect various subpopulations. Bartik and Nelson (2018) show that state-level bans reduce job-finding rates and increase subsequent separation rates for Black workers. In contrast, Clifford and Shoag (2016) show that these bans are associated with employment gains in geographic areas that have lower average credit scores, with these benefits mostly coming from mid- to high-wage jobs. In the prior study most closely related to our paper, Friedberg, Hynes, and Pattison (2017) examine the heterogeneous effect of bans on unemployed workers who likely differ in their recent credit performance. In contrast to our results, they find an increase in job-findings rates among workers who are likely to have bad credit histories. There are at least two potential explanations for this contradictory result. First, while our paper examines the effect of omitting recent credit performance from hiring considerations, Friedberg et al. focus on the removal of credit information altogether. Second, they focus on the precise group most likely to benefit from the policy: individuals who have trouble meeting recent expenses. In contrast, we study a broader class of workers for whom the screening is most likely to take place. However, our group of interest includes both workers likely to benefit from the policy and other workers who are potentially harmed (i.e., credit-worthy workers). This allows us to examine the potential pooling effect of the policy. Moreover, we complement this work by examining the law's effects on other related labor market outcomes (e.g., wages).

Our paper also relates to a recent literature that studies the role of credit information on the employment outcomes of individuals who have poor credit. Dobbie and Song (2015) show that workers who receive more generous bankruptcy protection have better outcomes. Bos, Breza, and Liberman (2018) study pawn-loan borrowers in Sweden and they find that negative credit information leads to lower employment levels and lower wages. This difference in their findings is possibly explained by a key difference between the settings they study. While the institutional shift in Sweden modified the duration that negative credit events remained on the credit reports of some workers, it is less likely that employers changed their hiring practices, given the small percentage of impacted workers. In contrast, the widespread implementation of the law in our study is more likely to induce a response from firms regarding their employment practices. Overall, our combined inferences suggest that restricting the use of credit information benefits unemployed workers who have worse credit at the expense of those with better credit. This inference is consistent with the pooling equilibrium predicted by our theoretical model, and it is similar to the effects found in other settings (e.g., in Agan and Starr (2018) and Doleac and Hansen (2020), who study bans on the consideration of criminal records in employment screening). Thus, while there are likely social benefits of implementing policies such as the Restricting Use of Credit Checks for Employment Decisions Act, our paper highlights the ensuing cross-subsidization of such policies.

Finally, our paper relates to the literature that studies the effect of credit information on other outcomes. Bos and Nakamura (2014) show that the deletion of negative credit information leads to a tightening of lending standards by banks, while Liberman et al. (2018) show that such a deletion leads to lower consumer borrowing, especially among lower income individuals. Dobbie et al. (2019) show that bankruptcy flag deletion leads to increases in credit limits and borrowing, while Herkenhoff, Phillips, and Cohen-Cole (2017) show that such a deletion increases entrepreneurship rates.

The rest of the paper is organized as follows. Section 1 develops the conceptual framework to help us consider the implications of credit information deletion for labor markets. Section 2 discusses Law 20,453. Section 3 describes the empirical strategy that we use to evaluate the effect of the law. Section 4 discusses the data and sample. Section 5 presents the empirical results. Section 6 concludes.

1 Conceptual Framework

While the law almost certainly had a profound effect on multiple aspects of the Chilean economy, we restrict our focus to its impact on labor market dynamics surrounding job losses. More specifically, we now present a parsimonious model that can generate a set of empirical predictions related to the re-employment of a worker after job separation.

One potential approach is to model the general equilibrium effects the law had on the posting of jobs and the search intensity of workers. While this is an interesting avenue, we elect instead to model the actions of agents in the "interview stage" after a firm chooses to post a job and after the arrival of the job candidate (i.e., the *worker*).

The labor market consists of (a) a continuum of workers who have heterogeneous abilities and (b) ex ante homogeneous firms. We assume that a worker knows her ability, β, and we assume that a hired worker yields an expected output, x, equal to her ability: $E(x) = \beta$. Moreover, β is drawn from the following distribution known to both the firm and the worker:

$$\beta \sim N(B, 1/\tau). \tag{1}$$

Importantly, this distribution does not characterize all workers in the economy; instead, it describes the set of workers who are pre-selected for a specific job opening. Thus, B represents the average quality of pre-selected workers, while τ represents the precision of the distribution of workers who are pre-screened for a particular type of job within a firm. Intuitively, a large τ corresponds to a firm that has excellent information on all workers' types. This facilitates the selection of candidates to be interviewed from the entire labor pool. In contrast, a small τ implies that an employer is less able to perform a directed search for job candidates, and instead selects candidates from a broader set of job applicants.

The hiring firm knows the distribution of workers described in Equation (1). After interviewing a candidate, the firm gains an additional signal, y, regarding the candidate's quality, where

$$y \sim N(\beta, 1/\rho). \tag{2}$$

Thus, the hiring firm observes an unbiased, but imperfect, signal of a worker's type. The precision of the signal is given by ρ, where a larger value implies a greater ability to infer a worker's type during the interview. We assume that markets are competitive, and a worker's ability translates directly to her output. Therefore, if a firm makes a job offer, market clearing implies that this offer will be accompanied by a proposed wage that is equal to the candidate's expected quality.

Let us define $\hat{\beta}$ as the firm's posterior for a candidate's quality after observing a signal of y:

$$\hat{\beta} = E(x|y). \tag{3}$$

Under the normal distribution, this yields:

$$\hat{\beta} = \frac{\tau B + \rho y}{\tau + \rho}. \tag{4}$$

In addition to the job candidate, we assume that the hiring firm also possesses an outside option. This option entails hiring a reservation worker with a normalized expected return of zero. Thus, the employer will hire the job candidate as long as $\hat{\beta} > 0$. This implies that the firm adopts the following hiring rule:

$$y \geq -\frac{\tau B}{\rho} = C. \tag{5}$$

That is, a firm will make an offer to all workers for whom $y > C$; otherwise, the firm will hire a reservation worker, who has a value of zero.

With this optimal hiring rule, we can characterize the ex ante probability that a firm hires a worker. And, conditional on being employed, we can also characterize the expected wage received by that worker. Since the firm hires a worker whenever it observes a private signal above C, the hiring probability is given by

$$Pr(hire) = Pr(y \geq C) = 1 - \Phi_y(C), \tag{6}$$

where Φ_y is the cumulative distribution function of y. Note that y follows a normal distribution, with $E(y) = B$ and $Var(y - B) = 1/\rho + 1/\tau = \frac{\rho+\tau}{\rho\tau} \equiv 1/H$.

Finally, we can specify the expected wage of workers in this economy, V, as the expected value of their productivity conditional on being hired. This will be the expected value of the maximum between zero (if a worker is not hired) and $\hat{\beta}$, which is a worker's inferred productivity. Since this is conditional on the private signal, which is a random variable that is distributed $y \sim N(B, 1/H)$, we obtain the following expression for the expected wage:

$$V = \int_{-\infty}^{\infty} max(0, \hat{\beta}) \sqrt{\frac{H}{2\pi}} exp(-\frac{H}{2}[y - B]^2) dy. \tag{7}$$

We can evaluate how the previous expressions change when employers face a noisier environment (i.e., a lower τ). This can occur when crucial information related to worker type is deleted or withheld.

PROPOSITION 1. In a labor market that features more noise regarding worker types, workers are less likely to find a job.

Proof. See Appendix A ■

The intuition behind this result is straightforward: More noise implies a higher hiring cut-off. Therefore, if workers cannot respond by increasing the intensity of their job search, then the fraction of workers who find jobs over a set time interval is lower, as is the probability of an employer hiring a worker.

PROPOSITION 2. In a labor market that features more noise regarding worker types, the expected value of hiring a worker is higher, as is her expected salary.

Proof. See Appendix A ■

This result stems from the fact that the option value of hiring a worker increases with more noise. Employers are stricter in their hiring standards, but a worker has higher expected productivity if she is hired.

2 Law 20,453

Chile has made significant efforts to protect consumer privacy and prevent discriminatory practices associated with the use of consumer credit information in a variety of settings. Dating back to the late 1990s, a strict policy has governed the usage of consumer financial and commercial information. Moreover, the Chilean constitution guarantees freedom of work and prohibits any discrimination aside from considerations of personal capacity or suitability for a particular job. Activists have argued that an employer's screening of applicants based on past credit performance violates the spirit of these laws (Biblioteca del Congreso Nacional de Chile (2010)). In response to these concerns, the Chilean Congress passed Law 20,453, "the law," in October 2010. This law sought to remove negative credit events from an unemployed worker's credit report under the assumption that removing such information helps unemployed workers avoid discrimination based on their credit history when searching for a new job.

The law is implemented as follows. Following a newly unemployed worker's application for unemployment insurance, the Unemployment Fund Administrator (UFA) instructs the credit registry to temporarily modify the credit report of the person in question. Specifically, the credit registry is instructed to temporarily delete all information related to missing payments incurred by the worker from one year before the application for unemployment insurance to the end of the worker's time of unemployment.[4] All omitted information remains unobservable while the unemployed worker receives insurance payments, typically a period between one and six months. Finally, the UFA instructs the credit registry to restore an unemployed worker's history upon re-employment, or the exhaustion of insurance benefits, whichever comes first. The replacement of missing payment information after a worker's benefits are exhausted offers a potentially interesting feature to exploit; however, we do not restrict ourselves to evaluating this feature of the law. Importantly, a hiring firm is unlikely to know the status of a job candidate's insurance benefits, so the firm cannot distinguish a candidate with a good credit history from a candidate who has a bad credit history but is still receiving unemployment insurance benefits.

3 Empirical Strategy

Ultimately, the enactment of the law restricted the information set of hiring firms in the Chilean labor market. The model we present in Section 1 offers up multiple predictions regarding the effect of this change on labor market outcomes. Importantly, the model predicts

[4]Information related to debts incurred more than one year before the unemployment insurance application date is not modified.

a larger effect on the hiring probability and wages of a worker as the precision of τ decreases. That is, the model predicts a more pronounced effect of the law when credit histories are more likely to be used as a pre-screening device, or alternatively, when such credit histories contain more information relevant to a worker's quality. To this end, we use a difference-in-differences method to exploit the heterogeneous effect of the law across worker types.

Lower income consumers are more likely to have poor credit histories (Comision Para el Mercado Financiero (2019)). This plausibly increases the precision of a credit report signal for this group, thereby increasing their usage in the hiring process for low-income workers. This is consistent with the findings of Cortes, Glover, and Tasci (2019) as well as anecdotal evidence from conversations with Chilean employers. Thus, our treatment group consists of workers in the lowest quartile of the income distribution, while workers in the highest quartile comprise the control group.[5] With this, we estimate regression specifications in the form

$$Y_i = \alpha + \beta_1 Treated \times Post_i + \beta_2 Treated_i + X_i^{'}\Gamma + \epsilon_i, \tag{8}$$

where Y_i is the outcome of interest for worker i (e.g., the length of time until re-employment), $Treated \times Post_i$ is the interaction of $Treated_i$ (a dummy variable that equals 1 if worker i's average salary throughout the sample is in the lowest quartile), and $Post_i$ (a dummy variable that equals 1 if worker i became unemployed after the implementation of the law (i.e., October 2010)). The vector X_i represents worker-level characteristics (including age, gender, civil status, any secondary education, and months of unemployment insurance coverage). We include year–month of job separation and home postal code fixed effects to control for the local economic environment during the worker's job search.

Importantly, our preferred specification also includes fixed effects for the interaction of year–month of job separation with the worker's economic sector. This inclusion accounts for

[5]In the Internet Appendix, we show that our results are robust to defining both groups based on median income and other quartiles.

differences in economic and labor conditions across economic sectors at different points in time. The coefficient of interest is β_1, which captures the average difference in the outcome variable among bottom-quartile workers (i.e., the treatment group) before and after the passage of the law minus the average difference in the outcome among top-quartile workers (i.e., the control group) before and after the passage of the law.

The key identifying assumption for this difference-in-difference approach is that, absent the treatment (i.e., before the introduction of the law change), there no relevant difference in outcome of interest between the treatment and control groups in the post-law period. While this cannot be explicitly tested, we do examine the presence of parallel trends before the treatment period. Moreover, to alleviate possible concerns regarding the comparability of low- and high-income workers, we also consider an alternative empirical approach inspired by the synthetic control method, which we discuss below.

4 Data and Sample Selection

The data for this study come from the Chilean Unemployment Insurance Program, which provides unemployment benefits to eligible workers. More specifically, since 2002, unemployment insurance coverage is mandatory for workers governed by Chile's labor code, who represent 70% of the labor force.[6] The government entity that manages the insurance program publicly discloses detailed employment data for a random sample of 12% of the workers in the system.

Our primary analysis relies on this random sample, which consists of four files. The first file contains the employment history of each worker. This file provides a monthly time-series of salaries, as well as the employer's identity and other employer-related information. Appendix B provides more details about the variables. The second file contains a snapshot of worker characteristics which includes gender, date of birth, education, civil status, and home area postal code. The third file contains information related to unemployment insur-

[6] Workers younger than 18 years old, pensioners, and independent workers are not covered by the program.

ance claims, which we use to identify unemployment spells. This file provides the date the worker was separated from her previous employer and her available benefits, among other information. The final file contains information about the insurance payments the worker received.

We focus on workers who have job separation dates between July 2009 and January 2012.[7] We drop individuals who are reported to have more than two employers at the same time, and we drop individuals who are not re-employed within 24 months of job separation to avoid capturing workers who may be seeking non-traditional employment (e.g., self-employed workers) after losing their jobs. We also drop individuals younger than 18 years old and older than 65 years old, as well as individuals who have a monthly average salary below CLP 63,500.[8] Finally, we drop individuals whose average salary throughout the sample is in the second and third quartiles of the average salary distribution. Note that we focus on a worker's average salary over the entire sample rather than her salary before job separation because one variable of interest we consider is the change in wages. This choice helps eliminate concerns about reversion to the mean. The final sample consists of 252,832 unemployment spells involving 137,448 individuals.

Table **1** describes the final sample by worker type (i.e., low-income versus high-income workers) for different subperiods (i.e., full, pre-law, and post-law). Low-income workers tend to be slightly younger, less likely to be male, more likely to be single, and are less educated than high-income workers. Low-income workers also tend to have longer unemployment spells, smaller salary increases, and are slightly more likely to become re-employed in the same economic sector.

[Table **1** about here]

[7]We do not consider separation dates beyond January 2012 because in February 2012, the Chilean government implemented Law 20,575, which forced credit bureaus to stop reporting the debt defaults of a large subset of the population. See Liberman et al. (2018) for details on this law.

[8]Based on the currency exchange rate prevailing in October 1, 2010, CLP 63,500 corresponds to USD 132.07. These individuals are roughly in the lowest 1% of the income distribution in the sample.

5 The Effects of Law 20,453

We now turn to the empirical tests motivated by the theoretical predictions presented in Section 1. First, we examine the primary effects of the law on the length of unemployment. Second, we focus on cross-sectional analyses. Third, we examine the effects of the law on the change in wages conditional on re-employment. Finally, we consider alternative measures of worker–firm match quality.

5.1. Unemployment spell length

We begin by analyzing the change in the duration of unemployment spells for low- and high-income workers after the passage of the law. A first-order prediction generated by the model is that when firms are less able to pre-screen workers and face more uncertainty in worker quality among the individuals selected to interview, the probability of an offer being extended is decreased. From the standpoint of a worker, this would translate into an increase in the expected number of interviews required to secure re-employment, and thus the duration of unemployment spells will likely increase. Importantly, this effect should be greater for low-income workers, for whom it is more likely that an applicant's prior credit history will be used in the pre-screening process.

Before turning to the first empirical test, we visually examine the difference in unemployment spells between low- and high-income workers around the passage of the law. Figure 1 plots the mean unemployment spell separately for workers in the lowest and highest quartile of average wages at a quarterly frequency for five quarters around the passage of the law. Three patterns emerge from the figure. First, unemployment spells exhibit a high degree of seasonality for both low- and high-income workers, peaking in the first quarter of the year. Second, while the baseline unemployment spell length differs for the two groups (with low-income spells lasting longer, on average), the two series appear to track each other in the quarters leading up to the law change. Finally, this gap appears to widen between low-and high-income workers after the passage of the law, as predicted by our model. While suggestive, this figure does not constitute statistical evidence of a relative extension of unemployment periods for low-income workers. Instead, we now turn to the reduced-form framework outlined in Section 3.

[Figure **1** about here]

Table **2** presents the results of OLS regressions where the outcome being examined is the number of months required to find employment after a job separation. Recall that we exclude from the sample any individual who does not secure re-employment within 24 months, in order to avoid capturing workers who seek informal re-employment (e.g., self-employment) for some period of time after a job loss. We begin with a sparsely populated empirical specification that omits all controls and fixed effects. The coefficient of 0.375 (t-stat = 3.08) on the interaction term indicates an increase of roughly 11.3 days (30×0.375) in the unemployment spell of a worker in the bottom quartile of income relative to a worker in the top quartile of income following the passage of the law.[9] While this may not seem like an economically significant length of time, the average unemployment spell in our dataset is 5.1 months. Thus, this effect represents a relative increase of 7.4% above the baseline value. The coefficient on $1(\textit{low income})$ indicates that low-income workers take more time to find re-employment in general. When including individual-level controls in the second specification, the point estimate on $1(\textit{low income}) \times 1(\textit{post law})$ decreases slightly to 0.374 (t-stat = 3.33). Perhaps unsurprisingly, workers with greater unemployment benefits take longer to find re-employment. The coefficient on the variable of interest remains relatively unchanged in the third and fourth specifications when including fixed effects for the worker's month of separation and home postal code to account for variation in employment prospects across time and geographic areas. Finally, the point estimate on $1(\textit{low income}) \times 1(\textit{post law})$

[9]Throughout this paper, we double-cluster standard errors by home postal code and by economic sector×year—month of separation.

is 0.357 (t-stat = 4.51) when replacing the month-of-separation fixed effects with fixed effects for the month of separation by economic sector.

[Table 2 about here]

In the Internet Appendix, we show additional robustness for the results shown in Table 2. Specifically, Table IA.1 shows little economic effect of the law on all but two of the control variables used in the regressions. Table IA.2 shows that the results are robust to dropping the first quarter of 2012 (i.e., the quarter with the largest difference in unemployment spells between the two income groups, as shown in Figure 1) from the sample. Additionally, although the financial sector represents only 1.9% of our sample, Table IA.3 shows that the results are not driven by this sector, where employers may have an informational advantage regarding credit profiles. Note that our model simply suggests longer unemployment durations for workers where screening ability is likely to be less precise. Table IA.4 considers an alternative treatment classification based on educational levels (i.e., high school versus post-secondary education), and we find consistent results.

We also examine the sensitivity of the results shown in Table 2 to alternative thresholds used to classify workers into the low- and high-income groups. Specifically, we re-estimate the final specification of Table 2 when segmenting our sample into two (median) and 10 (decile) quantiles to test for a post-law difference in unemployment spell duration between the lowest and highest quantiles in each iteration. Figure 2 reports the regression coefficient with 95% standard errors for each partitioning scheme. This figure demonstrates an increase in the relative difference in unemployment spells following the passage of the law as the sample is broken into finer quantiles and stabilizes when contrasting quantiles that are more granular than a quintile.

[Figure 2 about here]

Finally, we consider the possibility that the passage of the law coincided with an increase in immigration into the region. If immigrants are more likely to compete for jobs in the lower-income segment, then an increase in the flow of immigration would likely increase the unemployment spell length for low-income workers, thus driving our results. Figure IA.1 plots data from the Chilean Immigration Department. We find that immigration remains stable and represents a negligible fraction of the workforce during the years surrounding the law. This is inconsistent with the alternative explanation that immigration drives our results.

5.2. Synthetic controls

The results in Table 2 are consistent with an extension of the unemployment spells of low-income workers when employers are unable to screen on an applicant's credit history. However, the seasonality in unemployment spell lengths makes it difficult to gauge the degree to which the treatment and control groups exhibit similar changes in the pre-law period. For this reason, to account for any possible pre-treatment deviations in spell lengths between low- and high-income workers we turn to an adaptation of the synthetic control method implemented in Abadie and Gardeazabal (2003) and in Abadie, Diamond, and Hainmueller (2010). Intuitively, rather than rely on all high-income workers to serve as the control group for low-income workers, this method constructs a synthetic control observation for each treated observation by forming a convex combination of non-treated observations (i.e., high-income workers) that most closely resembles the treated observation in the pre-treatment period.

While there are many dimensions over which one may attempt to minimize the distance between the synthetic control observations and the treated observations, a natural choice is the outcome variable (e.g., unemployment spell length) in the months before the policy intervention. In fact, this is the precise example described in Imbens and Wooldridge (2009), who note that the applications of the method "... are very promising" (p.72).

However, the original synthetic control method requires that the outcome of interest is observable for each observation in every period. Intuitively, after establishing the appropriate combination of control observations in the pre-law period, the weighted outcomes of these control observations can be compared to those of the treated observations in the post-law period. In contrast, the nature of the question we study renders this impossible, as a given worker may experience a job separation more than once in our sample. Therefore, it is not feasible to compare the outcomes of the same workers in the pre- and post-periods. To overcome this limitation, we instead classify individual workers into groups based on observable characteristics, and we estimate the method using group-level outcomes over time.

More precisely, we first partition our sample into groups based on gender, previous sector of employment, income quartile, and 5-year bins of age. Then, for each group and month of separation, we compute the mean unemployment spell length. Using this panel of monthly group-averages, for each low-income group, this method estimates the convex combination of high-income groups that most appropriately matches the average unemployment spell of the low-income group across all separation months in the pre-period.

Before presenting our empirical results, we reproduce Figure 1 after replacing the high-income time series with the synthetic controls constructed using this method. Figure 3 displays the results of this change, from which two observations emerge. First, the average unemployment spell of low-income workers and the synthetic control composed of high-income workers match each other quite well in the pre-period. Second, this relationship does not persist after the enactment of the law. Instead, low-income workers appear to exhibit longer unemployment spells relative to their high-income counterparts in the post-period. We now formally test this second observation in a regression framework.

[Figure 3 about here]

Table 3 presents the results of weighted regressions that differ from those in Table 2 in one key manner. While the previous approach included all control (high-income) workers, giving them equal weight, Table 3 instead utilizes the estimated weights from the synthetic controls method.[10] The first specification omits all additional controls and includes only the three covariates needed to estimate a difference-in-differences regression. The coefficient of 0.614 (t-stat = 3.23) on the interaction term indicates an increase of more than 0.5 months in unemployment spell lengths for workers in the lowest quartile of income relative to those in the highest quartile following the law's passage. Note that this point estimate is somewhat larger in magnitude when contrasted against the corresponding specification in Table **2** (0.375). The point estimate remains relatively stable following the inclusion of additional controls (Column 2), separation month fixed effects (Column 3), postal code fixed effects (Column 4), and separation month by economic sector fixed effects (Column 5). In the final specification, the point estimate of 0.507 indicates that low-income workers have a greater relative 0.5-month increase in the length of unemployment after the passage of the law. This represents a 9.9% increase relative to the mean unemployment spell length observed in our sample (i.e., 5.1 months). Given this increase in magnitude, we briefly examine whether this effect is being driven by the re-weighting of the sample according to the synthetic control method, or simply by the exclusion of some observations that are never selected as an appropriate control (thus having a final weight of 0). Table IA.**5** performs un-weighted OLS regressions after eliminating all workers that receive a zero weight, yielding results similar to the un-weighted regressions of Table **2**. This suggests that the increase in magnitude is due to a change in the intensive margin (re-weighting) rather than the extensive margin (dropping observations) of the sample considered.

[Table **3** about here]

[10]Note that for each treated group, the method produces a set of *weights* for each control group. We take two steps to transform these group-level weights to individual-level weights. First, we spread the group-level weight evenly over all workers in the control group by dividing the weight by the number of workers in the group. The result is an appropriate worker-level weight to use in comparison to a single treated worker. Thus, we also scale the weight by the number of workers in the treated group. This results in control weights that sum to exactly the number of treated observations in each period.

5.3. Cross-sectional analyses

The results presented thus far are consistent with an increase in unemployment spell lengths for low-income workers when employers cannot screen applicants based on their credit score. However, before moving forward, we will briefly consider the variation we observe in these results across economic sectors. Specifically, Panel A of Table 4 repeats the final specification of Table 2 for the four largest economic sectors in our sample. The coefficient on $1(\textit{low income}) \times 1(\textit{post law})$ is positive and statistically significant in three of the four sectors, as well as for the union of all smaller sectors. Interestingly, the law is particularly detrimental for low-income workers in the commercial and agricultural sectors. One possible explanation for this finding is the potential ability of a credit history to serve as a signal for a worker's likelihood of stealing from her firm (Gross-Schaefer et al. (2000)), which is likely a more serious concern in the commercial and agricultural sectors (Society For Human Resource Management (2010)).

[Table 4 about here]

We repeat the previous analysis in Panel B of Table 4 while considering the synthetic control approach.[11] As we follow this alternative approach, we see a general increase in standard errors, with two of the previously statistically significant sectors losing their statistical significance. However, it is unclear whether this decrease in the precision of our point estimate is due to a smaller true effect or an inability of synthetic controls to construct a proper counterfactual for some sectors.

Next, we consider cross-sectional heterogeneity across other dimensions. Importantly, our model predicts that the passage of the law should have a stronger effect for (a) workers who exhibit more variation in their abilities or (b) workers who are more difficult to pre-screen based on other observable characteristics. A convenient feature of our data is the ability to

[11] Figure IA.2 plots the mean unemployment spell length for low- and high-income workers over time for each sector. The figure reports both the un-weighted and the weighted (synthetic control) cases.

observe detailed worker characteristics, which is useful for exploring this additional theoretical prediction. We begin by examining the heterogeneity of the law's effect across different subsamples of workers split by age. Younger workers are less likely to have previously obtained substantial work experience that is useful in the pre-screening of applicants. This is consistent with a less precise initial signal. Thus, our model would predict an outsized effect of the law on unemployment spell duration.[12]

Panel A of Table **5** reports the results of OLS regressions similar to the final specification of Table **2** for different partitions based on a worker's age. We first bisect our sample into workers older than 25 for the first specification and workers 25 or younger for the second specification. The point estimates indicate that the relative effect of the law on low-income workers is 1.3 times larger for workers under 25 compared to their elder counterparts. This is consistent with shorter work histories among younger workers, making it more difficult for firms to pre-screen young applicants. At the same time, restricting a firm's ability to screen on prior delinquencies is only valuable to the extent that a credit history can feasibly exist.[13]

In contrast, it is unlikely that young workers (e.g., 19-year-olds) will have accumulated a meaningful credit history. The third specification considers this possibility by restricting the sample of young workers to workers between the ages of 21 and 25, and we find a larger effect (0.521, t-stat = 4.10) relative to either of the previous subsamples. Thus, the results indicate that the law was especially detrimental for those workers who are old enough to have meaningful credit histories, but do not have substantial working experience.

[Table **5** about here]

Next, we consider an additional channel (albeit one outside the scope of our model) that could cause heterogeneity in the effect of the law across subpopulations. The enact-

[12]In the case of younger workers, the signal about worker skill, ρ, is noisier. This translates into employers featuring priors about the skill distribution of younger workers that are more dispersed, which in turn extends their unemployment spells.

[13]More precisely, as firm's are unable to observe on-time payments, the value of a credit history signal is a function of the firm's belief that sufficient time has passed for delinquencies to accumulate for low-ability workers.

ment of the law served to reduce the precision of one signal available to employers to screen and evaluate job candidates. As a result, firms likely responded by increasing the weight of other signals used in their hiring decisions. However, prior literature has demonstrated the discriminatory nature of some signals used in the labor market. For example, extant research shows that employers are biased against women (e.g., Reuben, Sapienza, and Zingales (2014);Moss-Racusin et al. (2012)) and minorities (e.g., Bertrand and Mullainathan (2004);Quillian et al. (2017)). If low-income workers have fewer "hard" signals of ability to screen on, or alternatively, if firms are more likely to discriminate in the hiring of low-income workers, then it is plausible that the law increased the weight being placed on such signals. We consider two such margins of potential discrimination: gender and residential location.

When considering male workers in the fourth specification of the panel, the positive coefficient of 0.228 (t-stat = 3.06) on the interaction term indicates an increase of seven days in the unemployment spell of low-income relative to high-income workers following the passage of the law. In contrast, the corresponding point estimate on $1(\textit{low income}) \times 1(\textit{post law})$ increases substantially to 0.638 (t-stat = 4.94) in the fifth specification when considering the relative effect on female workers. Economically, this is equivalent to a 19-day increase in the unemployment spell for females. This effect is 2.8 times larger than their male counterparts, on average. Finally, the last two columns of the panel partition the sample on the median value of the average income of the worker's home postal code. The socioeconomic status conveyed by an applicant's home address may represent another dimension in which discriminatory hiring practices persist, and it may carry more weight when other signals are excluded. While the law had a detrimental effect on low-income workers in both areas, its effects are 1.7 times larger for workers who reside in low-income areas. Panel B of Table **5** repeats the previous analysis using the synthetic control method. The results from Panel A remain unchanged.[14]

[14] Figure IA.**3** plots the time-series of mean unemployment spell lengths for low- and high-income workers, respectively, for each subgroup considered in Table **5**.

5.4. Discussion

Our results show that restricting the use of credit information in hiring decisions has detrimental effects on the labor market outcomes of low-income workers. On the other hand, Bos, Breza, and Liberman (2018) show that this restriction is beneficial for workers who have negative credit histories. The combined inferences from these two findings suggest that restricting the use of credit information precipitates a redistribution of labor market benefits to low-income workers with poor credit at the expense of low-income workers with good credit. Moreover, as the majority of workers do not have negative credit events, at least in our setting, the unconditional effect across all workers is negative. Importantly, in our setting, this *cross-subsidization* occurs within the sample of low-income workers (i.e., the treated group) rather than between low- and high-income workers.

Moreover, the negative implications of information deletion are more prevalent for female workers and younger workers. This result is consistent with employers statistically discriminating against these two groups. Overall, our results show effects similar to deleting other types of information, such as the existence of a criminal record (i.e., "ban the box" policies). Specifically, Agan and Starr (2018) and Doleac and Hansen (2020) show that when a worker's criminal history is unavailable to employers, they may statistically discriminate against demographic groups that are more likely to have a criminal record.

Our paper has important implications for policymakers. Many states in the U.S. have recently enacted bans on employer credit checks. On January 29, 2020, the U.S. House of Representatives passed an amendment to the Fair Credit Reporting Act by way of the Comprehensive CREDIT Act of 2020, which will prohibit the use of credit information for most hiring decisions. While there are likely societal gains of implementing these policies, our paper highlights the cross-subsidization that entails when implementing policy to protect workers with worse credit histories.

5.5. Wages

Next, we seek to empirically test the second, and possibly more interesting, prediction generated from the model. Specifically, our model predicts that, conditional on receiving a job offer, the associated wage the firm offers will be larger when the hiring firm faces more uncertainty over candidate quality for those who pass the pre-screening. To conduct this test, we begin by constructing a variable $Prev.Wage$, which is set to the maximum of a worker's last two monthly salaries at her previous employer. We consider the last two wage payments, as the worker may suffer a job loss in the middle of her final month, resulting in a smaller final payment. In a similar fashion, we define $NextWage$ as the maximum of the first two monthly wages received by the worker upon re-employment. From these two variables, we construct the outcome of interest, $WageChange$, equal to $ln(Prev.Wage) - ln(NextWage)$.

Table **6** examines the effect of the law on $WageChange$, which represents the relative change in a worker's wages between old and new employers. Panel A begins by presenting the results using un-weighted OLS regressions, where the empirical specifications are identical to those of Table **2**. In the first specification, which omits all controls and fixed effects, the coefficient of 0.052 (t-stat = 3.20) on $1(low - income) \times 1(post\ law)$ indicates that low-income workers experience a 5.2% increase in their wages relative to high-income workers following the law's enactment. Importantly, we bisect the sample into low- and high-wage workers based on an individual's average salary over the entire sample to avoid concerns of reversion to the mean.[15] In contrast, the coefficient on $1(low\ income)$ suggests that before the law, low-income workers faced a relative decrease in wages upon re-employment. When including individual-level controls in the second speciation, the coefficient of interest decreases to 0.032. In the final three specifications, the point estimate remains virtually unchanged when controlling for local economic conditions and the economic sector.

[Table **6** about here]

[15] In untabulated robustness tests, we consider an alternate classification based on the average wages across all employees of a worker's firm. We find similar results under this alternative classification scheme.

Panel B of Table 6 repeats the previous analysis using the synthetic control method. Following this change, point estimates continue to be positive and have magnitudes roughly similar to their corresponding values in Panel A. However, these estimates become statistically insignificant at traditional levels. For this reason, we are careful to avoid inferring too much from this analysis.

Taken together, the results from the preceding analysis and, to a lesser extent, the results shown in Table 6 are consistent with the theoretical model presented above. These empirical findings are consistent with a reduced ability of firms to pre-screen job candidates based on their past credit performance, thus increasing uncertainty in the pool of labor market candidates vying for job postings.

5.6. Alternative measures of worker–firm match quality

As a final set of tests, we turn to other outcomes that are plausibly related to a worker's preferences regarding different re-employment prospects. Outside this model, if a worker faces more uncertainty and is less likely to be hired, then it is plausible that she will expand her job search to include potential employers that are less desirable along non-monetary dimensions. The first such dimension we consider is the change in commuting distance between a worker's former and new employers. Based on revealed preferences, one would expect a worker to prefer a new employer in the same locale as her previous firm. If there is a decrease in the quality of matches in the labor market, we would expect that the distance between former and new employers increases for low-income workers relative to high-income workers following the law change.

Columns (1) and (2) of Table 7 present the results of OLS regressions where the outcome of interest is the log distance between a worker's past and future employers. Both specifications indicate that, on average, a low-income worker experiences an increase in the distance between her old and new employer relative to a high-income worker following the passage of the law, but the effect is statistically indistinguishable from zero.

[Table 7 about here]

A second dimension we consider is the likelihood that a worker switches to a different economic sector in her new job. A reduction in the screening ability of firms may lead a worker to venture across industries more frequently after the law is implemented. Columns (3) and (4) of Table 7 examine this hypothesis, where the outcome of interest is an indicator that takes a value of 1 if the worker switched sectors following re-employment. Both specifications yield statistically insignificant point estimates.

6 Conclusion

The model we have developed shows that banning the use of financial information in employment screening increases the uncertainty regarding a worker's imperfectly-observed productivity, and this uncertainty leads to a noisier labor market from an employer's perspective. This uncertainty also translates into tighter hiring rules, thus decreasing the unconditional probability of a job candidate receiving an offer and thereby increasing the duration of job-seeking spells. This uncertainty also increases the option value of a qualified worker. Thus, equilibrium wages increase when the information set is restricted.

We empirically investigate the implications of the model by exploiting a nationwide policy change implemented in Chile, which significantly limits an employer's ability to use an applicant's credit information for hiring purposes. We employ a difference-in-differences framework that exploits the heterogeneous effects of this policy across workers.

Arguably, this policy change disproportionately affects low-income workers, who are more likely to have poor credit histories and for whom credit reports are more likely to be used in the hiring process. We find that after the law, low-income unemployed workers require an additional 11 days (i.e., 7% longer) to find a new job relative to high-income workers. This effect is particularly prevalent among female workers and younger workers, which is consistent with statistical discrimination against these two groups. In addition, we find that when low-income workers are re-employed, their wages increase by 3.2% relative to high-income workers.

Unfortunately, our data do not contain disaggregated credit histories for the workers we observe, so we cannot examine the direct effect that the removal of a bad credit signal has on the affected worker in question. However, prior research shows that negative credit information leads to lower employment and wages (Bos, Breza, and Liberman (2018)). An inference based on this prior research and our findings suggests that restricting the use of credit information benefits unemployed workers who have worse credit performance, but at the expense of those with better credit. This trade-off is important, as it suggests that policies such as those in the Comprehensive CREDIT Act of 2020 (which is currently being considered by federal policy-makers in the US), may not benefit all low-income workers, as some have argued, and such policies can have unintended consequences for workers with better credit histories.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, 2010, Synthetic control methods for comparative case studies: Estimating the effect of california's tobacco control program, *Journal of the American statistical Association* 105, 493–505.

Abadie, Alberto, and Javier Gardeazabal, 2003, The economic costs of conflict: A case study of the Basque Country, *American Economic Review* 93, 113–132.

Agan, Amanda, and Sonja Starr, 2018, Ban the box, criminal records, and racial discrimination: A field experiment, *The Quarterly Journal of Economics* 133, 191–235.

Bartik, Alexander, and Scott Nelson, 2018, Credit reports as resumes: The incidence of pre-employment credit screening, Working paper.

Berg, Tobias, Valentin Burg, Ana Gombović, and Manju Puri, 2019, On the rise of fintechs–credit scoring using digital footprints, *Review of Financial Studies* Forthcoming.

Bertrand, Marianne, and Sendhil Mullainathan, 2004, Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination, *American economic review* 94, 991–1013.

Biblioteca del Congreso Nacional de Chile, 2010, *Historia de la ley No 20.463*.

Bos, Marieke, Emily Breza, and Andres Liberman, 2018, The labor market effects of credit market information, *Review of Financial Studies* 31, 2005–2037.

Bos, Marieke, and Leonard Nakamura, 2014, Should defaults be forgotten? Evidence from variation in removal of negative consumer credit information, Working paper.

Clifford, Robert, and Daniel Shoag, 2016, 'no more credit score': Employer credit check bans and signal substitution, Working paper.

Comision Para el Mercado Financiero, 2019, *Informe de endeudamiento financiero*.

Cortes, Kristle Romero, Andrew S Glover, and Murat Tasci, 2019, The unintended consequences of employer credit check bans for labor markets, Working paper.

Demos, 2016, *Bad credit shouldn't block employment: How to make state bans on employment credit checks more effective*.

Dobbie, Will, Paul Goldsmith-Pinkham, Neale Mahoney, and Jae Song, 2019, Bad credit, no problem? Credit and labor market consequences of bad credit reports, Working paper.

Dobbie, Will, and Jae Song, 2015, Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection, *American Economic Review* 105, 1272–1311.

Doleac, Jennifer L, and Benjamin Hansen, 2020, The unintended consequences of "ban the box": Statistical discrimination and employment outcomes when criminal histories are hidden, *Journal of Labor Economics* 38, 321–374.

Friedberg, Leora, Richard M Hynes, and Nathaniel Pattison, 2017, Who benefits from bans on employer credit checks?, Working paper.

Gross-Schaefer, Arthur, Jeff Trigilio, Jamie Negus, and Ceng-Si Ro, 2000, Ethics education in the workplace: An effective tool to combat employee theft, *Journal of Business Ethics* 89–100.

Herkenhoff, Kyle, Gordon Phillips, and Ethan Cohen-Cole, 2017, The impact of consumer credit access on employment, earnings and entrepreneurship, Working paper.

Hermalin, Benjamin, and Michael Weisbach, 1998, Endogenously chosen boards of directors and their monitoring of the ceo, *American Economic Review* 96–118.

Imbens, Guido W., and Jeffrey M. Wooldridge, 2009, Recent developments in the econometrics of program evaluation, *Journal of Economic Literature* 47, 5–86.

Liberman, Andres, Christopher Neilson, Luis Opazo, and Seth Zimmerman, 2018, The equilibrium effects of information deletion: Evidence from consumer credit markets, Working paper.

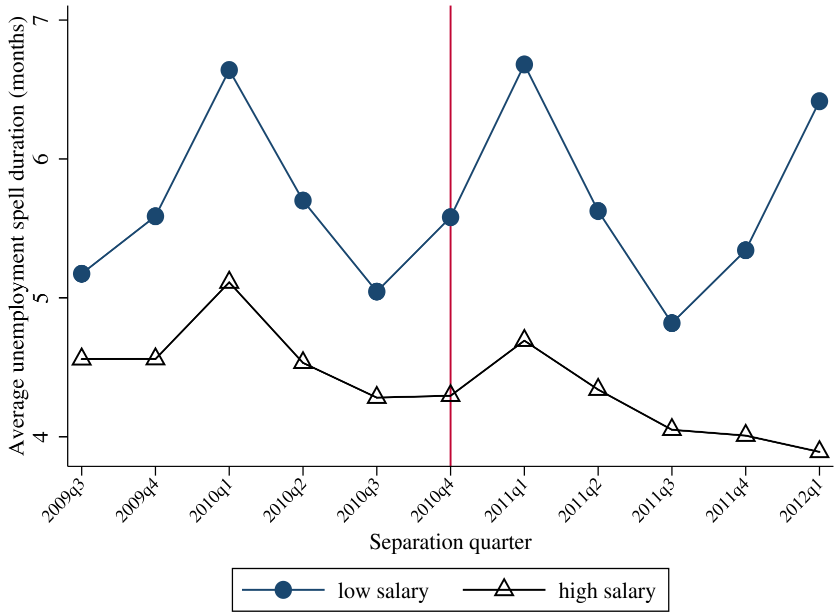
Moss-Racusin, Corinne A, John F Dovidio, Victoria L Brescoll, Mark J Graham, and Jo Handelsman, 2012, Science faculty's subtle gender biases favor male students, *Proceedings of the national academy of sciences* 109, 16474–16479.

Quillian, Lincoln, Devah Pager, Ole Hexel, and Arnfinn H Midtbøen, 2017, Meta-analysis of field experiments shows no change in racial discrimination in hiring over time, *Proceedings of the National Academy of Sciences* 114, 10870–10875.

Reuben, Ernesto, Paola Sapienza, and Luigi Zingales, 2014, How stereotypes impair women's careers in science, *Proceedings of the National Academy of Sciences* 111, 4403–4408.

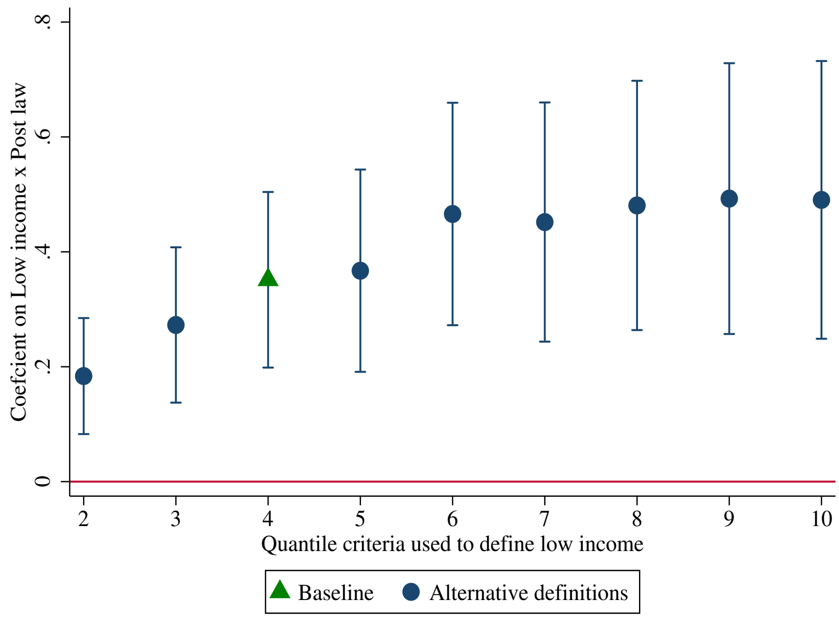
Society For Human Resource Management, 2010, *SHRM Research Spotlight: Credit Background Checks*.

Figure 1. Length of unemployment for low-income and high-income workers by quarter



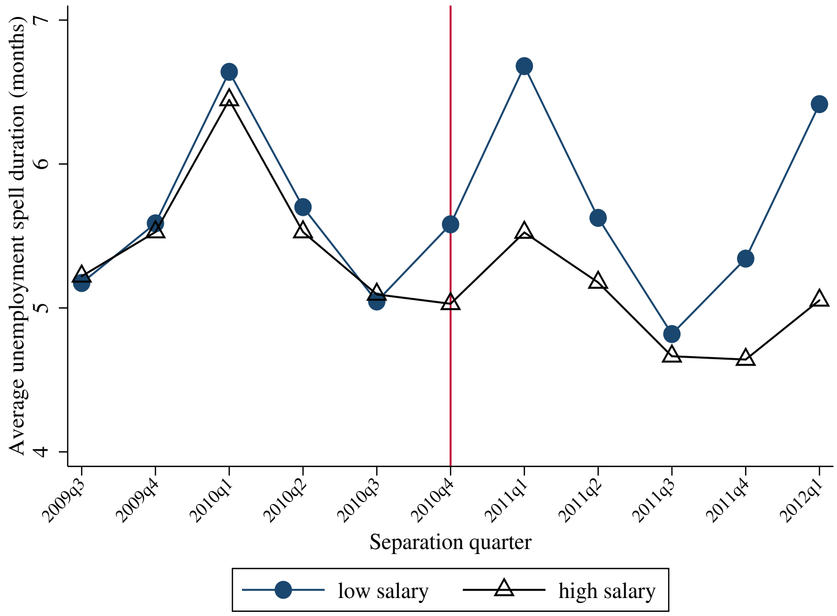
This figure shows the average length of unemployment for workers in the lowest and highest quartiles of average wages at a quarterly frequency for five quarters around the passage of the law.

Figure 2. Robustness of main results to definitions of low-income worker



This figure shows the coefficient associated with $1(low\ income) \times 1(post\ law)$ from estimating the specification in Column (5) of Table **2** for variants of $1(low\ income)$ based on different quantiles of the average salary distribution. For example, a quantile equal to 2 means that $1(low\ income)$ is defined based on the median average salary. A quantile equal to 10 means that $1(low\ income)$ is defined based on the lowest and highest deciles of the average salary. Note that the sample size shrinks as the quantile criteria increase. The vertical lines denote the corresponding 95% confidence intervals based on doubled-clustered standard errors by worker postal code and by job separation month $\times$ economic sector.

Figure 3. **Length of unemployment for low-income and high-income workers by quarter using the synthetic control method**



This figure shows the average length of unemployment for workers in the lowest and highest quartiles of average wages at a quarterly frequency for five quarters around the passage of the law when implementing the synthetic control method.

Table 1. Descriptive statistics

	Full period Jul. 2009 -Jan. 2012		Before law Jul. 2009-Sep. 2010		After law Oct. 2010-Jan. 2012	
	Low income	High income	Low income	High income	Low income	High income
Labor outcome						
Unemployment spell duration (months)	5.73	4.45	5.72	4.63	5.74	4.27
Log change in salary (%)	1.12	4.68	0.01	6.28	2.05	3.08
Former–new employer distance	123.2	183.1	121.9	180.5	124.3	185.7
1(different sector)	0.49	0.51	0.478	0.51	0.5	0.509
Individual characteristics						
Age	33.9	35.6	34.4	35.0	33.6	36.1
1(male)	0.53	0.78	0.54	0.79	0.526	0.775
1(single)	0.59	0.54	0.57	0.53	0.613	0.540
1(secondary education)	0.06	0.23	0.05	0.23	0.1	0.2
Benefits remaining (months)	0.23	0.76	0.26	0.72	0.20	0.80
Monthly salary (USD)	301.1	1,347.1	300.3	1,352.2	301.7	1,342.1
N	126,420	126,412	57,630	62,958	68,790	63,454

This table describes the sample of unemployment spells by worker type (i.e., low income and high income) for different subperiods (i.e., full, pre-law, and post-law). Workers are categorized as *low income* if their average salary throughout the sample is in the lowest quartile. Workers are categorized as *high income* if their average salary throughout the sample is in the highest quartile. $1(\cdot)$ denotes dummy variables. Detailed variable difinitions are available in Appendix B.

Table 2. Effect of the law on unemployment

	(1)	(2)	(3)	(4)	(5)
1(low income) × 1(post law)	0.375***	0.374***	0.390***	0.388***	0.351***
	(3.08)	(3.33)	(3.95)	(4.24)	(4.51)
1(low income)	1.098***	1.179***	1.143***	1.192***	1.156***
	(10.00)	(12.73)	(13.55)	(17.46)	(18.58)
1(post law)	-0.355***	-0.356***			
	(-3.49)	(-4.87)			
Benefits remaining		0.440***	0.449***	0.442***	0.434***
		(38.53)	(39.25)	(39.61)	(38.94)
1(male)		-0.801***	-0.767***	-0.730***	-0.593***
		(-16.65)	(-16.62)	(-15.23)	(-13.48)
1(single)		-0.394***	-0.381***	-0.326***	-0.315***
		(-7.59)	(-7.56)	(-9.37)	(-8.92)
ln(age)		-1.193***	-1.134***	-1.076***	-0.987***
		(-10.43)	(-10.65)	(-12.27)	(-11.15)
1(secondary education)		0.593***	0.578***	0.451***	0.405***
		(9.78)	(9.87)	(7.06)	(7.02)
Separation month FE	no	no	yes	yes	no
Postal code FE	no	no	no	yes	yes
Separation month × Sector FE	no	no	no	no	yes
N	252,832	252,832	252,832	252,829	252,817
R^2	0.016	0.037	0.044	0.052	0.060
Mean of dependent variable	5.1	5.1	5.1	5.1	5.1

This table shows OLS regressions of different variants of Equation (8). The dependent variable is the number of months required to find employment following a job separation. The main independent variable of interest is 1(*low income*) × 1(*post law*), the interaction of a dummy variable that equals 1 if the worker's average salary is in the lowest quartile and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as job separation month, home area postal code, and job separation month × economic sector fixed effects are included as reported. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Table 3. Synthetic control regressions

	(1)	(2)	(3)	(4)	(5)
1(low income) × 1(post law)	0.614***	0.643***	0.645***	0.619***	0.507***
	(3.23)	(3.52)	(3.71)	(3.62)	(3.36)
1(low income)	0.083	0.428***	0.417***	0.428***	0.477***
	(0.53)	(2.82)	(2.93)	(3.38)	(4.49)
1(post law)	-0.594***	-0.621***			
	(-3.31)	(-3.66)			
Benefits remaining		0.457***	0.460***	0.458***	0.458***
		(11.62)	(12.02)	(12.16)	(12.93)
1(male)		-0.677***	-0.655***	-0.644***	-0.529***
		(-8.71)	(-8.56)	(-9.56)	(-8.43)
1(single)		-0.602***	-0.575***	-0.505***	-0.538***
		(-6.47)	(-6.22)	(-6.78)	(-7.91)
ln(age)		-1.536***	-1.482***	-1.420***	-1.302***
		(-10.56)	(-11.00)	(-11.99)	(-11.74)
1(secondary education)		0.373***	0.327**	0.230*	0.220**
		(2.83)	(2.51)	(1.85)	(2.04)
Separation month FE	no	no	yes	yes	no
Postal code FE	no	no	no	yes	yes
Separation month × Sector FE	no	no	no	no	yes
N	246,298	246,298	246,298	246,295	246,282
R^2	0.003	0.024	0.034	0.050	0.075
Mean of dependent variable	5.1	5.1	5.1	5.1	5.1

This table shows weighted regressions of different variants of Equation (8) using the synthetic control method. The dependent variable is the number of months required to find employment following a job separation. The main independent variable of interest is 1(*low income*) × 1(*post law*), the interaction of a dummy variable that equals 1 if the worker's average salary is in the lowest quartile and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as job separation month, home area postal code, and job separation month × economic sector fixed effects are included as reported. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Table 4. Effect of the law on unemployment by economic sector

Panel A: Baseline regressions

	Construction (1)	Real Estate (2)	Agriculture (3)	Commerce (4)	Other (5)
1(low income) × 1(post law)	0.046	0.380**	0.525**	0.549**	0.396***
	(0.53)	(2.14)	(2.71)	(2.42)	(3.64)
1(low income)	1.755***	0.983***	0.886***	0.959***	0.996***
	(27.56)	(8.88)	(6.24)	(6.84)	(13.00)
Control variables	yes	yes	yes	yes	yes
Postal code FE	yes	yes	yes	yes	yes
Separation month × Sector FE	yes	yes	yes	yes	yes
N	53,082	44,571	34,421	30,635	90,089
R^2	0.056	0.051	0.089	0.056	0.059
Mean of dependent variable	4.2	5.2	5.5	5.6	5.2
Frequency in sample (%)	21.0	17.6	13.6	12.1	35.7

Panel B: Synthetic control regressions

	Construction (1)	Real Estate (2)	Agriculture (3)	Commerce (4)	Other (5)
1(low income) × 1(post law)	-0.211	0.213	0.584**	0.338	0.638***
	(-0.47)	(0.61)	(2.12)	(0.58)	(2.89)
1(low income)	0.973***	0.282**	0.829***	0.118	0.282
	(4.57)	(2.29)	(5.23)	(0.22)	(1.62)
Control variables	yes	yes	yes	yes	yes
Postal code FE	yes	yes	yes	yes	yes
Separation month × Sector FE	yes	yes	yes	yes	yes
N	138,451	141,649	149,089	36,675	72,498
R^2	0.066	0.085	0.104	0.098	0.067
Mean of dependent variable	4.5	4.6	4.7	5.5	5.5

This table shows regressions identical to Column (5) of Table **2** (in Panel A) and Column (5) of Table **3** (in Panel B), by economic sector. The largest four economic sectors are included. The dependent variable is the number of months required to find employment following a job separation. The main independent variable of interest is 1(*low income*) × 1(*post law*), the interaction of a dummy variable that equals 1 if the worker's average salary is in the lowest quartile and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as home area postal code and job separation month × economic sector fixed effects are also included. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Table 5. Cross-sectional splits

Panel A: Baseline regressions

	Age			Gender		Area income	
	Older than 25 (1)	25 or younger (2)	Between 21 and 25 (3)	Male (4)	Female (5)	High (6)	Low (7)
1(low income) × 1(post law)	0.349***	0.456***	0.521***	0.228***	0.638***	0.296***	0.500***
	(3.99)	(3.88)	(4.10)	(3.06)	(4.94)	(3.53)	(5.20)
1(low income)	1.100***	0.925***	0.738***	1.334***	0.646***	1.309***	1.057***
	(15.77)	(10.65)	(7.20)	(20.85)	(5.93)	(16.59)	(14.51)
Control variables	yes	yes	yes	yes	yes	yes	yes
Postal code FE	yes	yes	yes	yes	yes	yes	yes
Separation month × Sector FE	yes	yes	yes	yes	yes	yes	yes
N	190,811	61,985	36,137	166,106	86,686	126,502	126,307
R^2	0.055	0.079	0.081	0.055	0.056	0.060	0.059
Mean of dependent variable	4.8	5.9	5.5	4.7	5.9	5.0	5.2

This table shows regressions identical to Column (5) of Table **2** (in Panel A) and Column (5) of Table **3** (in Panel B), for different subsamples based on age, gender, and the average income of the worker's home postal code. The dependent variable is the number of months needed to find employment following a job separation. The main independent variable of interest is $1(\textit{low income}) \times 1(\textit{post law})$, the interaction of a dummy variable that equals 1 if the worker's average salary is in the lowest quartile and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as home area postal code and job separation month × economic sector fixed effects are also included. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Panel B: Synthetic control regressions

	Age			Gender		Area income	
	Older than 25	25 or younger	Between 21 and 25	Male	Female	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(low income) × 1(post law)	0.422***	0.550***	0.629***	0.295**	0.614***	0.328**	0.614***
	(2.90)	(2.67)	(3.32)	(2.17)	(3.28)	(2.03)	(3.76)
1(low income)	0.508***	-0.013	0.372**	0.440***	0.527***	0.620***	0.211
	(4.99)	(-0.09)	(2.50)	(4.57)	(3.93)	(4.44)	(1.48)
Control variables	yes	yes	yes	yes	yes	yes	yes
Postal code FE	yes	yes	yes	yes	yes	yes	yes
Separation month × Sector FE	yes	yes	yes	yes	yes	yes	yes
N	204,682	157,635	158,160	187,296	178,844	167,784	198,355
R^2	0.060	0.091	0.048	0.062	0.078	0.080	0.064
Mean of dependent variable	4.8	4.9	5.6	4.8	5.0	4.8	4.9

Table 6. Effect of the law on wages

Panel A: Baseline regressions

	(1)	(2)	(3)	(4)	(5)
1(low income) × 1(post law)	0.052***	0.032*	0.033**	0.032**	0.033**
	(3.20)	(1.86)	(1.97)	(1.97)	(2.11)
1(low income)	-0.063***	-0.116***	-0.116***	-0.130***	-0.160***
	(-5.47)	(-9.87)	(-9.69)	(-11.29)	(-14.62)
1(post law)	-0.032**	-0.020			
	(-2.55)	(-1.64)			
Control variables	no	yes	yes	yes	yes
Separation month FE	no	no	yes	yes	no
Postal code FE	no	no	no	yes	yes
Separation month × Sector FE	no	no	no	no	yes
N	252,832	252,832	252,832	252,829	252,817
R^2	0.000	0.014	0.016	0.018	0.027
Mean of dependent variable	0.029	0.029	0.029	0.029	0.029
930					

Panel B: Synthetic control regressions

	(1)	(2)	(3)	(4)	(5)
1(low income) × 1(post law)	0.044	0.025	0.025	0.025	0.024
	(1.60)	(0.92)	(0.94)	(0.98)	(1.01)
1(low income)	-0.020	-0.100***	-0.100***	-0.119***	-0.180***
	(-1.16)	(-5.49)	(-5.60)	(-6.95)	(-11.45)
1(post law)	-0.024	-0.013			
	(-0.91)	(-0.53)			
Control variables	no	yes	yes	yes	yes
Separation month FE	no	no	yes	yes	no
Postal code FE	no	no	no	yes	yes
Separation month × Sector FE	no	no	no	no	yes
N	246,298	246,298	246,298	246,295	246,282
R^2	0.000	0.017	0.022	0.032	0.062
Mean of dependent variable	0.030	0.030	0.030	0.030	0.030

This table shows OLS regressions (in Panel A) and weighted regressions (in Panel B) of different variants of Equation (8). The dependent variable is the relative change in a worker's wages between old and new employers. The main independent variable of interest is $1(\textit{low income}) \times 1(\textit{post law})$, the interaction of a dummy variable that equals 1 if the worker's average salary is below-median and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as home area postal code and job separation month × economic sector fixed effects are also included. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Table 7. Effect of the law on other employment outcomes

	Commuting distance		1(different sector)	
	(1)	(2)	(3)	(4)
1(low income) × 1(post law)	0.061*	0.028	0.013	0.008
	(1.90)	(0.86)	(0.77)	(0.85)
1(low income)	-0.239***	-0.237***	-0.008	-0.014*
	(-7.05)	(-6.70)	(-0.70)	(-1.74)
Control variables	yes	yes	yes	yes
Separation month FE	yes	no	yes	no
Postal code FE	yes	yes	yes	yes
Separation month × Sector FE	no	yes	no	yes
N	250,442	250,430	252,829	252,817
R^2	0.063	0.071	0.036	0.085
Mean of dependent variable	9.4	9.4	0.50	0.50

This table shows OLS regressions of different variants of Equation (8). In Columns (1) and (2), the dependent variable is the log distance between a worker's past and future employers. In Columns (3) and (4), the dependent variable is a dummy variable that equals 1 if the worker moved to a different sector following re-employment. The main independent variable of interest is 1(*low income*) × 1(*post law*), the interaction of a dummy variable that equals 1 if the worker's average salary is below median and a dummy variable that equals 1 if the worker became unemployed after October 2010. Worker-level controls, as well as home area postal code and job separation month × economic sector fixed effects are also included. t-statistics (in parentheses) are heteroscedasticity-robust and double-clustered by worker postal code and by job separation month × economic sector. ***p <0.01, **p <0.05, *p <0.1.

Appendix A

Proof of Proposition 1

Let $Z \equiv \sqrt{H}(y-B) \Rightarrow dy = dZ/\sqrt{H}$.

Then

$$V = \int_{-\infty}^{\infty} max(0, B + \frac{Z\sqrt{H}}{\tau})\sqrt{\frac{1}{2\pi}} exp(-\frac{1}{2}[Z]^2)dZ, \tag{A.1}$$

where $\phi(Z) = \sqrt{\frac{1}{2\pi}} exp(-\frac{1}{2}[Z]^2)$ is the probability density function of a standard normal distribution. Note that $B + \frac{Z\sqrt{H}}{\tau} > 0$ when $Z \geq \frac{-B\tau}{\sqrt{H}}$; therefore, we can represent V as

$$V = \int_{-\frac{B\tau}{\sqrt{H}}}^{\infty} (B + \frac{Z\sqrt{H}}{\tau})\phi(Z)dZ. \tag{A.2}$$

Furthermore, $-\frac{B\tau}{\sqrt{H}} = -B\frac{\tau+\rho}{\tau+\rho}\frac{\tau}{\sqrt{H}} = (C-B)\sqrt{H}$

and

$$V = \int_{(C-B)\sqrt{H}}^{\infty} (B + \frac{Z\sqrt{H}}{\tau})\phi(Z)dZ = B[1 - \Phi((C-B)\sqrt{H})] + \frac{\sqrt{H}}{\tau}\phi((C-B)\sqrt{H}). \tag{A.3}$$

Now, we must evaluate

$$\frac{\partial Pr(hire)}{\partial \tau} = \frac{\partial Pr(hire)}{\partial 1/H}\frac{\partial 1/H}{\partial \tau}, \tag{A.4}$$

where $\frac{\partial 1/H}{\partial \tau} = \frac{\rho^2}{(\rho+\tau)^2}$.

Therefore,

$$\frac{\partial Pr(hire)}{\partial \tau} = \frac{\rho^2}{(\rho+\tau)^2}\frac{\partial}{\partial 1/H}[1 - \int_{-\infty}^{C} \phi_y dx]. \tag{A.5}$$

Using the Leibniz rule and the properties of the normal distribution, we obtain

$$\frac{\partial Pr(hire)}{\partial \tau} = -\frac{\rho^2}{(\rho+\tau)^2}[\phi_y(C)\frac{\partial C}{\partial 1/H} + -(\frac{C-B}{H^2})\phi_y(\frac{C-B}{H})] \tag{A.6}$$

and

$$\frac{\partial Pr(hire)}{\partial \tau} = \frac{\rho^2}{(\rho+\tau)^2}[\phi_y(\frac{HB}{H-\rho})\frac{\rho B}{(H-\rho)^2} + (\frac{\rho B}{(H-\rho)H^2})\phi_y(\frac{-\rho B}{(H-\rho)H})] > 0. \tag{A.7}$$

Thus, more noise (i.e. a lower τ) implies a lower unconditional probability of hiring.

Proof of Proposition 2

We begin by considering

$$\frac{\partial V}{\partial \tau} = (\frac{\rho^2}{2\tau(\rho+\tau)^2\sqrt{H}} - \frac{\sqrt{H}}{\tau^2})\phi((C-B)\sqrt{H}) < 0, \tag{A.8}$$

which indicates a higher value of hiring in more uncertain markets (i.e., markets with a lower τ).

Appendix B

Variable	Description
Labor outcomes	
Unemployment spell duration	Number of months required to find employment following a job separation.
Log change in salary	Natural log change in a worker's wages between old and new employers.
Former–new employee commuting distance	Natural log distance between a worker's past and future employers.
1(different sector)	Dummy variable that equals 1 if the worker moves to a different sector following re-employment. The sample consists of 19 economic sectors.
Individual characteristics	
Benefits remaining	Number of months left before the exhaustion of unemployment benefits.
1(male)	Dummy variable that equals 1 if the worker is male.
1(single)	Dummy variable that equals 1 if the worker is single.
ln(age)	Natural log of a worker's age.
1(secondary education)	Dummy variable that equals 1 if the worker has at least one year of secondary education.