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ABSTRACT

The Effect of Employment Protection on Firms' Worker Selection*

To estimate the causal effect of employment protection on firms' worker selection, we study a policy change that reduced dismissal costs for the employers of over a tenth of Sweden's workforce. Our difference-in-differences analysis of firms' hiring uses individual ability measures including estimated worker fixed effects and cognitive test scores. We find that the reform reduced minimum hire quality by 5% of a standard deviation, half of which we can attribute to firms' hiring becoming more selective. Our results help discriminate between existing theories, supporting the prediction that firms shift their hiring standards in response to changes in dismissal costs.

JEL Classification: M51, D22, J24, J38

Keywords: worker selection, screening, hiring standard, employment protection, dismissal costs

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1 Introduction

Economists have long studied the effect of labor market regulation on *how many* workers are hired. Far less is known about whether regulation also affects *who* gets a job. Does labor market policy influence firms' worker selection? This question has consequences for the efficiency of labor market outcomes, as firms' selectiveness directly influences the allocation of workers. Moreover, the question has distributional implications: changes in firms' worker selection will alter the employment prospects of the most disadvantaged labor market participants, such as the long-term unemployed. Firms' worker selection may thus be an overlooked pathway from labor market regulation to labor productivity and to the employment dynamics of vulnerable workers.

Several theoretical papers on the hiring process agree that there should be an effect of labor market regulation on the selectiveness of firms' hiring. The existing models suggest different mechanisms for the link from regulation to firms' worker selection though. For example, the model in Lazear (1995) implies that tighter regulation makes hiring more selective as firms hire less "risky" workers, i.e., firms reduce the spread of their hires' ability distribution. By contrast, the matching model in Pries and Rogerson (2005) predicts that stricter regulation increases firm selectiveness by causing employers to no longer hire "bad" workers, i.e., to raise their hiring standard. Do firms reduce the variance of hires' ability at the bottom and the top or do firms primarily avoid the bottom tail of the ability distribution? These are, roughly, the testable predictions of the risky-hires framework formulated by Lazear (1995) and the matching model variant in Pries and Rogerson (2005). Empirically studying the effect of regulation on firms' selectiveness offers the prospect of enhancing our understanding of the hiring process by discriminating between these suggested mechanisms.

In this paper, we investigate *if* and *how* a relaxation of employment protection legislation (EPL) reduces firms' selectivity in hiring. To estimate the effect of weaker EPL on firms' worker selection we exploit a Swedish reform that made it easier for small firms to lay off workers. The policy change generated exogenous variation in dismissal costs across firms and over time, whose effects we analyze in a difference-in-differences (DiD) framework. Administrative data on the universe of Swedish workers and firms allow us to precisely measure firms' affectedness by the reform and to credibly proxy for the ability of new hires. We find that the reduction in dismissal costs directly reduces firms' selectiveness by about 2.5% of a standard deviation. We further

conclude that this is driven by an adjustment of hiring thresholds rather than by making riskier hires.

The main contribution of this paper is to show that employment protection legislation directly affects *whom* firms hire, and that this results from firms lowering their hiring threshold. Regarding the first point, we confirm and extend the findings of two other papers that provide evidence consistent with EPL affecting worker selection (Marinescu, 2009; Bjuggren and Skedinger, 2018). In this study we go further by establishing a causal effect on the *ability* of new hires and by providing direct evidence for a change in screening intensity. With respect to the second point, no evidence exists on *how* dismissal costs affect the selectiveness of firms' hiring. This paper is the first to study potential mechanisms underlying the link between EPL and firms' worker selection, enabling us to discriminate between the theoretical predictions of Lazear (1995) and Pries and Rogerson (2005).

Our first step in investigating whether EPL changes worker selection is to estimate the overall effect of Sweden's 2001 EPL reform on firms' minimum hire quality, i.e., the ability of the least productive hire a firm makes in a year. As we discuss below, this overall effect may contain indirect effects other than changes in firms' selectiveness. Our preferred proxy for worker productivity is an estimated person effect (AKM) from two-way-fixed effect log-wage regressions in the spirit of Abowd et al. (1999). Using a DiD specification that exploits the size cut-off in firms' eligibility for the EPL reduction, we estimate that the reform reduced firms' minimum hire quality by 5% of a standard deviation. Our alternative ability measures are military draft test scores for cognitive and psychological aptitude from age 18 and grade point averages (GPAs) from age 15. We prefer AKMs as the alternatives cover either only men or only young people. Nonetheless, the alternative ability proxies produce remarkably similar effect estimates.

We then isolate the direct effect of Sweden's EPL reform on firms' worker selection. To do so we quantify the role of a mechanism that may lower firms' minimum hire quality but *does not* go through the selectiveness of firms' hiring: as reduced dismissal costs make firms hire more workers, they may—by chance—employ lower-ability workers without actively adjusting their selectiveness.¹ We estimate the importance of this “number-of-hires” channel by simulating a counter-factual scenario designed to preclude changes in firm selectiveness. Our findings suggest the number-of-hires channel accounts for about half of the overall effect of the EPL reform on

¹Pries and Rogerson (2005) predict a direct effect of EPL on both firms' hiring threshold and on worker turnover. There is also empirical evidence on an effect of EPL on hiring rates both internationally and in Sweden (Kugler and Pica, 2008; von Below and Skogman Thoursie, 2010).

firms' minimum hire quality. This leads to our estimated direct effect of Sweden's EPL reform on firms' selectiveness of about 2.5% of a standard deviation.

Having established that lower dismissal costs reduce firms' selectiveness we proceed to investigate two mechanisms suggested by the theoretical literature. The first potential channel is based on the insight that firms may like hiring risky workers as long as workers are easy to fire: these hires provide option value as they may turn out more productive than expected (Lazear, 1995). If dismissal costs go down, firms should then intensify their hiring of risky workers. In terms of unobserved ability, the variance of new hires' ability distribution will go up as the firm now hires workers that will either turn out less able than earlier cohorts or prove to be more productive than expected. In our setting, this would imply that lower EPL both reduces minimum hire quality and increases maximum hire quality: under reduced dismissal costs, hires should be better than expected as often as they are worse than expected. Adjusting for the number-of-hires channel, we find no evidence that firms symmetrically increase the riskiness of their hiring. That is, our evidence does not support the mechanism suggested by the risky-hires framework in Lazear (1995).

The second suggested mechanism for the link between EPL and worker selection works through firms' hiring standards. The idea is that there is a productivity threshold below which a firm does not hire applicants. In their matching model with employer search, Pries and Rogerson (2005) show that dismissal costs shift this threshold, implying that a reduction in EPL should shift firms' hiring standards downward. For our setting this results in the prediction that the EPL relaxation should lower firms' minimum hire quality without increasing maximum hire quality. Consistent with this prediction, we find that the reform reduced firms' selectiveness only at the bottom of the hire ability distribution. An additional implication of firms lowering their hiring threshold in response to the EPL reform is that employers may relax their pre-hire screening of applicants. To obtain a direct measure of such screening, we build on the finding in Hensvik and Skans (2016) that firms use incumbent workers' networks of past co-workers to hire selectively. Using the number of co-worker links from new hires to incumbents as a proxy, we find that reduced EPL causes firms to rely less on such referral-based hiring. Both pieces of evidence suggest that firms' worker selection responded to the EPL relaxation by becoming less stringent with respect to low ability. This favors the hiring threshold mechanism of Pries and Rogerson (2005) as an explanation of the effect of dismissal costs on firms' worker selection.

Our paper is the first to provide empirical evidence on the mechanisms driving the effect of EPL on firms' worker selection. In showing that there is a link between EPL and firms'

selectiveness it extends scarce existing evidence. Three studies present evidence consistent with regulation affecting firms' worker selection. Marinescu (2009) studies the effect of an increase of EPL in the UK on job durations. Her main result is that shortening the qualification period for claiming unfair dismissal decreases the hazard of termination not only for directly affected workers, but also for unaffected workers hired after the policy change. Although her data do not allow her to provide direct evidence on the quality of hires, she shows that the pattern is consistent with EPL inducing more selective hiring. We confirm and complement her suggested explanations by making use of explicit information on worker ability.²

In a recent working paper written independently from and contemporaneously to ours, Bjuggren and Skedinger (2018) analyze the effect of the same 2001 EPL reform on the share of workers firms hire from unemployment and from active labor market programs. They find that the EPL reduction increases the share hired from these sources and argue that this is consistent with firms reducing their screening. Their paper and ours arrive at similar conclusions using different pieces of evidence: their argument is that unemployed workers are harder to screen, so that intensified hiring of unemployed workers is an indication that firms are happy to screen less. Our argument goes through hire ability and through firms' network-based hiring, both of which point to a relaxation of screening. The findings of Bjuggren and Skedinger (2018) and ours strengthen each other: they bring different evidence to bear on the question whether EPL affects firms' worker selection and come to consistent and complementary answers.

Focusing on a different type of labor market regulation, Butschek (2017) investigates the effect of the 2015 introduction of a statutory minimum wage in Germany on firms' worker selection. He finds that the exogenous wage shock at the bottom of the wage distribution increased employers' hiring thresholds as proxied by their minimum hire quality. In support of a screening-based interpretation he shows that the size of the minimum wage effect on worker selection is monotonically increasing in the importance of pre-hire screening to the firm's hiring process. Though the policy change exploited by Butschek (2017) tightened rather than relaxed regulation his results are consistent with ours.

Also relevant to our study are the findings of von Below and Skogman Thoursie (2010) and Bjuggren (2018), who study the effects of the EPL reduction on labour turnover and firm

²The finding in Kugler and Saint-Paul (2004) that stronger EPL decreases the chances of unemployed individuals is also similar to our conclusion. The mechanisms studied are quite different, however. In their setting EPL affects worker-firm matching through separations. As firing becomes easier, churn increases and the pool of unemployed workers becomes more negatively selected. It is a composition effect that leads to the deterioration of unemployed workers' employment prospects. In our setting, by contrast, the opportunities of low-ability workers change through adjustments in firms' selectiveness.

productivity, respectively. At first glance our finding that the 2001 EPL reform made firms' hiring less selective seems at odds with Bjuggren (2018), who finds positive effects of the same EPL relaxation on labor productivity. Looking at the theoretical motivation of his analysis, however, suggests that there does not need to be a contradiction: in standard models of reduced employment protection, it is not a change in who is hired in the first place that improves the allocation of labor, but increased labor market churning (Hopenhayn and Rogerson, 1993). Indeed, the reform has been shown to have increased both firms' hiring and their firing without a change in net employment (von Below and Skogman Thoursie, 2010). We show that the reform increased the dispersion of hire quality. It is possible that firms increased worker productivity by first hiring less selectively but then identifying and retaining the abler workers. This would be consistent with both higher churn and a larger ability range of workers entering.

By studying the effects of dismissal costs on worker-firm matching, this paper also expands the scope of the literature studying the effects of Sweden's 2001 EPL reform. Labor market outcomes considered by existing papers include sickness absence (Lindbeck et al., 2006; Olsson, 2009) and parental leave take-up (Olsson, 2017).³

At a more general level, this study contributes to the literature looking at determinants of selectivity in worker-firm matching. Examples include the analysis of foreign ownership and hiring selectivity (Balsvik and Haller, 2015), the relation between management quality and the quality of worker flows in and out of the establishment (Bender et al., 2018), the role of networks in hiring (Hensvik and Skans, 2016), and skill sorting across firms over time (Håkanson et al., 2015). By analyzing the effect of dismissal costs on worker selection, our paper not only looks at a very different determinant of worker-firm matching than these studies, but it also offers exogenous variation in the determinant of interest, allowing us to identify a causal relationship.

To support the causal interpretation of our results, we present evidence addressing a number of threats to causal identification. These include concurrent changes in labor market legislation, workers' differential self-selection into firms, non-classical measurement error in worker ability, selective attrition and time-invariant firm heterogeneity. Our results are robust to a broad range of tests.

³As there is practically no evidence on the effects of EPL on worker-firm matching internationally, our paper generally broadens the literature on the effects of employment protection legislation—see, e.g., Bastgen and Holzner (2017) for a recent review.

2 Institutional background

The 2001 reform of Sweden’s employment protection provisions created variation in firms’ recruitment incentives that we want to exploit. In this section we briefly describe the institutional setting of our quasi-experiment before discussing selected features of Swedish EPL and its 2001 reform that are relevant to our analysis.⁴

2.1 Employment protection legislation in Sweden

Swedish EPL is relatively strict: dismissals are only allowed in case of misconduct and redundancy. In the latter case, i.e., when a firm’s business is underperforming and it wants to lay off a worker, legislation from 1974 further dictates whom it may fire. A last-in-first-out (LIFO) rule stipulates that the establishment’s most recent hire has to be dismissed first. The LIFO rule constrains firms’ ability to lay off its least productive workers in a downturn, instead forcing them to dismiss those with the lowest seniority (SFS, 1982).

The 2001 reform of Sweden’s EPL did not change the basic idea of protecting higher-tenure workers but allowed small firms to exempt up to two of its workers from the LIFO rule. This meant that out of their three most recent hires, firms with ten or fewer workers became able to choose whom to lay off.⁵ The degree to which this relaxed employment protection varies by firm size (see below for details).

Given that small firms (with up to ten workers) make up a sizeable share of the workforce (11.2% in 1999), these changes constitute a substantial liberalization of labor market regulation. Notwithstanding its importance the 2001 EPL reform came about quickly and rather unexpectedly: two possible versions of it were proposed in February 2000 and the second, after some modification, was written into law in October 2000 to become effective on 1 January 2001. That the reform was unexpected is best illustrated by who originated it: an atypical coalition of opposition parties (Liberal-Conservatives and Greens) pressed the unwilling minority government to make a proposal, which the governing Social Democrats sought to derail even after presenting it (Lindbeck et al., 2006).

⁴See Lindbeck et al. (2006), von Below and Skogman Thoursie (2010) and Bjuggren (2018) for other information on the 2001 EPL reform. Skedinger (2008) and Böckerman et al. (2018) contain useful information on EPL legislation in Sweden generally.

⁵One implication is that on-the-job screening may have been made more feasible by the reform: in addition to the probation period affected firms now had time to fire a recent hire as long as there were no more than two more recent hires.

Most changes to labor market regulation in the 1990s did not coincide with the EPL reform in either timing or targeting. One was a temporary provision allowing *all* firms to exempt two workers from the LIFO tenure ranking, effective only during 1994 (SFS, 1993; Skedinger, 2008). Second, there was a 1997 reform that made it easier to hire workers under fixed-term contracts without a particular reason, though for at most 5 individuals and with individual employment on a temporary contract capped at 12 months in a three-years period (Sjöberg, 2009; Holmlund and Storrie, 2002). Unlike the LIFO reform of 2001 these policy changes applied to firms of all sizes equally. They also were not introduced at the same time as the change in EPL. The only exception is an amendment to the Gender Equality Act, which we discuss in detail in Section 6.

2.2 Institutional detail of the last-in-first-out rule

The change in EPL applied to small firms of all sectors except private households with employed persons, as the LIFO rules do not apply to these (SFS, 1982). Defining “small”, the reform law makes it clear that the threshold of ten workers is in heads (not full-time equivalents) and includes both part-time and full-time workers as well as those on permanent and temporary contracts (von Below and Skogman Thoursie, 2010). The head count, however, excludes owners and their family members, (leading) managers and workers on employment subsidies (SFS, 1982). What the law is less explicit on is the time frame during which LIFO-relevant firm size is measured (more in Section 3.3).

The policy change did not alter the fact that firms (both small and large) had to define a tenure ranking of eligible workers in case of dismissals.⁶ What did change was the proportion of workers in the tenure ranking who enjoyed LIFO protection: before the reform, a downsizing firm had to lay off the person at the bottom of the relevant tenure ranking. After the reform, the firm was able to choose between the bottom three workers. As a consequence, the bite of the 2001 EPL reform was largest for the smallest firms. For firms with 2 or 3 workers, employment protection was practically abolished. For firms with 4-10 workers, the share of protected workers fell by between 67% and 22%. For larger firms, EPL did not change (cf. Table 1).

Finally, there is a degree of fuzziness to the reform. Some firms too large to be eligible will, through other means, have managed to relax or avoid LIFO’s strict rules. For example, Böckerman et al. (2018) point out that LIFO-relevant tenure rankings are defined separately for each establishment, blue-collar and white-collar workers and may even be split up again by

⁶Both before and after the reform, the protection afforded by the LIFO rule did not cover workers on temporary contracts (even though these workers entered in the head count determining whether the firm was affected by the reform).

Table 1: Degree of EPL before and after the 2001 reform, by firm size

# workers	Pre-reform		Post-reform		Δ protected
	Protected	Unprotected	Protected	Unprotected	
2	1	1	0	2	-100%
3	2	1	0	3	-100%
4	3	1	1	3	-67%
5	4	1	2	3	-50%
6	5	1	3	3	-40%
7	6	1	4	3	-34%
8	7	1	5	3	-29%
9	8	1	6	3	-25%
10	9	1	7	3	-22%
11+	10+	1	10+	1	0%

Note: Calculated levels and changes in EPL ignore exemptions, such as workers on fixed-term contracts.

worker skill.⁷ Conversely, a certain fraction of eligible firms will have retained their previous EPL provisions, as employers and trade unions may negotiate modifications to the LIFO rules in local agreements. Nonetheless, the evidence on the LIFO reform’s effects suggests that the EPL provisions from 1974 are a binding constraint on firms’ decisions (e.g., von Below and Skogman Thoursie, 2010; Bjuggren, 2018).

3 Data and method

3.1 Data

To be able to analyze firms’ hiring behavior, we need to characterize firms’ new hires. For this we would like to capture the timing and nature of all inflows (not just the change in net employment between a given day one year and the same day the next year). To do so we use individual-level data at monthly frequency on the universe of workers from Statistics Sweden (SCB). We extract and infer information about when workers enter new firms and about worker characteristics, most importantly their ability. We also use the firm identifiers in the individual-level data to infer the existence and size of firms at any point in time. We then combine this information to obtain a yearly firm-level panel that tracks all Swedish firms of a certain size over time and characterizes their worker inflows.

⁷To avoid confusion, we reiterate the role of the *firm* and the *establishment* in Swedish EPL: it is firm size that determines whether or not an employer is eligible for the relaxation of EPL effected by the 2001 reform. The establishment is only relevant for determining the LIFO ranking, which must be respected in laying off individual workers. As will become clear below this implies that for us the firm is the relevant unit of analysis.

Data sources We construct information on worker flows from *Jobbregistret* (JOBB), a register containing the universe of individual employment spells in Sweden from 1986 onwards. The spell data allow us to both identify new hires and measure firm size at monthly resolution.⁸ In addition, we use JOBB to obtain information on firms’ industry, ownership (private/public), location and age. Finally, JOBB is our main source of information on individuals’ labor market history, including where and with whom they worked and how much they earned. We use this information to estimate individual ability (see Section 3.5 for detail on our estimation of AKM person effects) as well as co-worker links (see Section 5.3). Drawing on other registers, we obtain further individual characteristics, such as workers’ gender, age, highest education and GPA from grade 9. For a majority of male hires we can also match military draft test scores to our sample. Table B1 in the appendix provides more detail on variables and data sources used in this study.

Time period Our period of analysis is 1993-2004. This gives us seven years (1993-1999) before the EPL reform we use was discussed and could have been anticipated (in 2000). We then have four years (2001-2004) during which we can study the short- and medium-run effects of the change in EPL. We do not extend our window of observation further into the past than 1993 as we use the period from 1986-1992 to estimate individual ability (further detail in Section 3.5).

Sample restrictions In our main analyses we focus on small firms employing between two and 15 workers in 1999. To ensure we have the relevant firm size measure for all, we restrict our sample to firms that existed in 1999. There are 275,731 firms in 1999 and we keep the 129,187 firms that were between two and 15 in size that year (see Section 3.2).⁹ We then remove the 1 private household with employed persons, as LIFO rules do not apply to such “firms”. Finally, we drop extreme outliers in terms of employment growth, i.e., the 0.5% of firms that experienced the largest inflows of workers.¹⁰

3.2 Identification strategy

We want to estimate the effect of a change in EPL on the selectiveness of firms’ hiring. The 2001 reform made EPL less strict for small firms. Because until 1999 this policy change was

⁸LISA, a multi-purpose register containing similar information, would only have given us individuals’ employment in November of each year.

⁹Note that firm size may be below two or above 15 before or after 1999.

¹⁰We show in Table B2 in the appendix that this restriction does not drive our results.

unexpected, we view it as exogenous variation in EPL over time for small firms (our treated group). The empirical strategy we employ addresses two key requirements for identifying a causal effect: first, we find a control group of similar but unaffected firms to obtain an estimate of the counter-factual hiring behavior of the treated firms in absence of the reform. Second, we recognize that, from 2000 onwards, well-managed firms may have downsized strategically to benefit from the reform. To ensure that such selection is not captured by our effect estimates we assign firms to the treated and control groups before the reform and keep them in that group. For assignment we use the latest possible time pre-reform when we can be reasonably sure that firms could not have anticipated the reform. As Section 2 illustrates, such anticipation cannot be ruled out in 2000 but is highly unlikely in 1999. We therefore use 1999 firm size to assign firms to the treated or control groups.

More formally, to estimate the effect of the 2001 relaxation of Sweden’s LIFO rule on small firms’ hiring standards, we use the following difference-in-differences (DiD) specification:

$$y_{jt} = \alpha + \beta TR_j * POST_t + \gamma_t + \delta TR_j + \epsilon_{jt}, \quad (1)$$

where firm j is defined as treated ($TR_j = 1$) if it has 10 or fewer employees in 1999. Firms with 11 to 15 workers are used as the control group. Observations between 2001 and 2004 make up the post-reform period, $POST_t = 1$. Observations from 1993 through 1999 are defined as the pre-reform period. The year 2000, when anticipation behavior is plausible but the reform was not yet in effect, is dropped from this DiD specification. γ_t are year fixed effects. Our main estimates do not include control variables or firm fixed effects, though we show that controlling for time-invariant heterogeneity leaves results qualitatively unchanged.¹¹ We cluster standard errors at the firm level. The outcome y_{jt} is a measure for firms’ selectiveness in hiring. In our main analyses, it is the minimum ability of firm j ’s hires in year t (more detail in Section 3.4).

The DiD estimate of the reform’s effect on hiring standards is given by $\hat{\beta}$. As we use a pre-determined and time-invariant measure of firm size, our DiD specification yields an intention-to-treat (ITT) estimate of β . This has the benefit of immunity to endogenous selection of firms into treatment: firms remain classified as control (treated) if they employed up to (more than) ten workers in 1999, even if they strategically downsize (naively expand). A potential drawback of the ITT approach is that it may bias our estimates toward zero to the extent that firms non-selectively shrink into or grow out of the treatment group.

¹¹Not including fixed effects allows us to look at effect heterogeneity by firm size, which is time invariant in our empirical approach.

Our key identifying assumption is $\mathbb{E}[\epsilon_{jt}|TR_j * POST_t] = 0$: absent the EPL reform, treated firms' worker selection would have followed a trend parallel to that of control firms'. While it is impossible to test this assumption for the reform years, we can check the extent to which it holds in the pre-reform period. To do so, we consider an event-study framework of the following form:

$$y_{jt} = \alpha + \sum_t (\beta_t TR_j * \gamma_t) + \gamma_t + \delta TR_j + \epsilon_{jt}, \quad (2)$$

where the DiD coefficient β_t is year-specific and 1999 is used as the reference year. Parallel trends in the outcome variable during the pre-reform period would imply estimates of β_t that are indistinguishable from zero for the years 1993-1998. Non-zero $\hat{\beta}_{t=2000}$ would constitute evidence for anticipation effects. The yearly effect estimates for the years 2001-2004 make it possible to learn whether the reform's effects, if any, are sustained in the medium run or die out over time.

There are potential threats to our identifying assumption that are not addressed by comparing pre-reform trends. Our identifying assumption would be violated if trends in outcomes, while parallel in the pre-reform period, would have deviated from their parallel paths in the post-reform period even in the absence of the EPL reform. We return to this issue in Section 6.7.

3.3 EPL reform bite measure

We want to compare firms potentially affected by the EPL reform to unaffected firms. This makes 1999 firm size a central measure. A key issue in defining firm size is whom to include in the headcount that determines whether the firm is small enough to benefit from the LIFO relaxation. As described in Section 2, owners/managers are excluded from this headcount. We follow Bjuggren (2018) in arguing that each firm will have at least one owner/manager and subtract one from the raw headcount.¹²

¹²In principle, the data allow us to account for most qualifications detailed in Section 2: they identify owners/entrepreneurs and we can use family linkages to find out whether their parents, siblings or children also work at the firm. However, because in 85.4% of all firm-level observations for 1999, none of the individuals linked to a firm is categorized as either owner or manager, we ignore this partial information. Von Below and Skogman Thoursie (2010) choose a different approach, excluding owners/managers from the count where such information is available and reducing the head count by one where no manager/owner appears in the employment spell data under that firm ID. We prefer the Bjuggren (2018) approach because it requires fewer assumptions. Table B3 in the appendix shows that our main results are robust to the firm size definition suggested by von Below and Skogman Thoursie (2010).

There is another important issue in defining firm size: is LIFO-relevant firm size the average number of employees in a year or the snapshot headcount at the time the firm wants to lay off a worker? Here the law makes provides no explicit information. We argue that this ambiguity is relatively unproblematic because we are most interested in how the *expected* ease of firing affects firms' hiring. When forming expectations about what rules apply to them in present and future, firms, like us, will need to rely on some approximation of their headcount. We choose the average number of employees in a year as a simple proxy that also takes account of seasonal fluctuations. That is, we use the employment spell data to determine firms' monthly number of individuals in dependent employment and take the average for each year. We then round this average to the nearest integer.

3.4 Measuring the selectiveness of firms' hiring

As a proxy for the selectiveness of firms' hiring we use their minimum hire quality in a given year, $\min_{j,t}\{ability_i\}$. This allows us to capture changes in firms' worker selection driven by both the risky-hiring mechanism (Lazear, 1995), which predicts a change in hire quality spread, and the hiring-threshold channel (Pries and Rogerson, 2005), which hypothesizes a shifting of hiring standards at the bottom. Measuring firms' time-varying minimum hire quality requires individual-level ability proxies.

Worker ability measures Thanks to the wealth of Swedish register data there are three main options for measuring worker ability: first, average school grades at age 15 (GPA); second, military draft test scores at age 18 for cognitive ability (COG) and psychological aptitude (NON-COG) (Lindqvist and Vestman, 2011); and third, estimated person fixed effects (AKM) from a two-way fixed-effects log wage regression with person and firm effects (Abowd et al., 1999). GPA is dominated by the other two measures: it is available only for some of the young birth cohorts in our data (born between 1973 and 1982).¹³ While draft test scores are arguably more precise measures of individual ability than AKM estimates, the former were collected only for men. AKM, on the other hand, will capture individual ability only to the extent that it is reflected in people's gross monthly earnings. Some of Sweden's elaborate wage-setting institutions will no doubt cause individual wages to depart from a worker's marginal product of labor. Nonetheless, AKMs will contain information about those aspects of time-constant individual productivity that are remunerated in the labor market. The key advantage of AKMs is that they can be

¹³AKM estimates are available for birth cohorts 1922-1976 and draft test scores males born 1951 -1991.

obtained for both men and women. For this reason we use AKMs as our main ability measure and rely on draft test scores and GPA to explore the robustness of our results.¹⁴

3.5 Estimating AKM person effects

To estimate time-invariant individual productivity we use individual-level spell data on employment from JOBB. We obtain full time-equivalent (FTE) monthly wages from the Wage Survey Statistics (WSS). For the AKM estimation we use data on the period from 1986 (the beginning of records) through 1992 (the year before the beginning of our analysis period). We want our ability estimates to be pre-determined from the point of view of our analysis and so avoid an overlap with our analysis period.

In our AKM estimation we include individuals aged 18-65 with a November spell for whom we have some FTE wage data.¹⁵ We deflate wages using the CPI and winsorize at 0.5% and 99.5% of the annual real monthly FTE wage distribution. Information on years of schooling/education is available only from 1990; for earlier years, we impute it from the first year it is available.¹⁶

Following Abowd et al. (1999) and Card et al. (2013) we estimate a two-way fixed-effects regression:

$$\ln(w_{ijt}) = \alpha_i + \psi_j + \gamma_t + x'_{it}\beta + r_{ijt}, \quad (3)$$

where $\ln(w_{ijt})$ is the natural logarithm of individual i 's hourly wage at firm j in year t . Moreover, there are additive fixed effects for individuals (α_i) and firms (ψ_j) as well as a set of year dummies (γ_t) and a vector of time-varying individual-level controls (x_{it}). Controls include age squared and age cubed as well as education categories interacted with the year dummies, age squared and age cubed.¹⁷ We deviate from Card et al. (2013) by estimating the two-way fixed-effects regression for men and women together so $\hat{\alpha}_i$ is comparable across gender. We obtain individual fixed-effect estimates $\hat{\alpha}_i$ for 78.7% of the workers for whom we observe a monthly FTE wage and

¹⁴Butschek and Sauermann (2019b) compare different individual ability measures and find a correlation between estimated AKM person effects and cognitive test scores of around 29% and GPAs at age 15 and cognitive test scores of around 63%.

¹⁵We have a measure of FTE monthly wages for 45.4% of person-year observations, or at least one monthly FTE wage observation for 63.7% of individuals. Coverage is partial because WSS is a survey-based register that only covers a stratified random sample of smaller firms.

¹⁶We do not have reliable information on whether individuals are primarily studying. We therefore omit this information.

¹⁷Age squared and age cubed are included to avoid capturing the effect of experience on wages in $\hat{\alpha}_i$.

estimates of ψ_j for the 46.6% of firms that have multiple workers and are connected by worker mobility.¹⁸

3.6 Summary statistics

Table 2 provides summary statistics for our main estimation sample, separately for treated and control firms. Panel A summarizes continuous variables. The average treatment group firm employs 4.9 workers, compared to 12.7 workers for the average control group firm. Treated firms have existed for a bit shorter, employ older workers and slightly more women and they pay a little less. Means are virtually identical across groups for workers' years of schooling and estimated AKM person and firm fixed effects. Panel B shows means for dummy variables. These are very similar across groups, with two exceptions: treated firms are less likely to be manufacturing firms and are less likely to have expanded since the previous year.

Table 2: Firm characteristics by treatment status

<i>A: Continuous characteristics</i>				
	Treated		Control	
	mean	sd	mean	sd
Head count (rounded)	4.857	2.453	12.688	1.388
Firm age (years)	8.171	4.548	8.859	4.407
Mean worker age (years)	40.158	9.311	38.644	8.532
Female worker share	0.409	0.297	0.373	0.267
Mean worker years of schooling	11.418	1.450	11.356	1.222
Mean worker monthly wage (100 SEK 1980)	45.080	29.996	45.506	29.199
Mean worker AKM person effect (std, 1986-92)	-0.007	0.998	-0.022	0.817
Estimated AKM firm effect (std, 1986-92)	-0.007	0.992	0.038	0.995
Observations	87,441		13,947	
<i>B: Binary characteristics</i>				
	Treated		Control	
	Share Yes	Frequency	Share Yes	Frequency
Privately owned	0.991	86,651	0.986	13,750
Manufacturing firm	0.090	7,881	0.132	1,845
Expanding firm	0.430	35,124	0.581	7,786
Downsizing firm	0.286	23,316	0.287	3,847
Stockholm county	0.227	19,845	0.230	3,212
Malmö (Skåne county)	0.123	10,745	0.115	1,606
Göteborg (Västra county)	0.163	14,261	0.175	2,435
Observations	87,441		13,947	

Note: This table summarizes treatment and control firms' characteristics in 1999 for the main estimation sample. Panel A provides mean and standard deviation of continuous variables. Panel B gives means and frequencies for dummy variables.

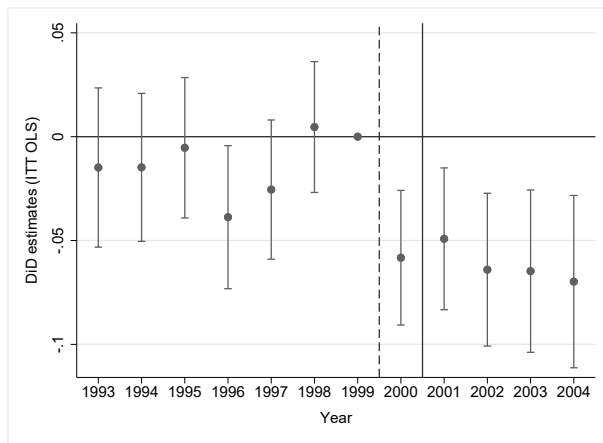
¹⁸To estimate Equation 3, we use `reghdfe` (Correia, 2016), which drops singletons (22.3% of individuals and 38.9% of firms).

4 Results

4.1 EPL effect on firms' minimum hire quality

We first consider the results from the event-study estimation framework described in Equation 2. Recall that the DiD estimates for 2001-2004 show the reform's (dynamic) effect on minimum hire quality $\min_{j,t}\{\hat{\phi}_i\}$, where $\hat{\phi}_i$ are estimated AKM person effects. DiD estimates for 1993-1998 provide a test of the parallel-trends assumption for the pre-reform period. Figure 1 displays these DiD estimates over time. With the exception of 1996 pre-reform DiD estimates are statistically indistinguishable from zero. While this is a deviation from parallel pre-reform trends in outcomes we argue that overall the assumption of parallel counter-factual trends is likely to hold. We present specific evidence supporting this claim in Section 6.7, where we perform placebo tests. For the years 2001-2004 Figure 1 shows a negative and persistent effect of the EPL reform on minimum hire quality. The event-study framework suggests that there was strong anticipation in 2000, the year that the LIFO reform was debated and passed in parliament. While the size of the anticipation effect may be somewhat surprising it is intuitive that, if they respond, firms may adjust their worker selection once they anticipate that firing workers becomes easier in the near future.¹⁹

Figure 1: Dynamic EPL effect on minimum hire quality



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS (ITT) specification without covariates other than year dummies. Hire quality is measured by AKM person effects estimated 1986-1992. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

¹⁹This is especially true given that the reform facilitates firing of recent hires only once they are no longer the most recent hire.

We next compare the EPL reform’s estimated effect on minimum hire quality across our four different worker ability measures. Table 3 presents estimated DiD coefficients for the specification in Equation 1. The difference between the columns is the worker ability proxy underlying firms’ yearly minimum hire quality: estimated AKM person effects (1), military draft cognitive test scores (2), draft psychological test scores (3) and GPA at age 15 (4). The outcome variables are standardized to make them comparable across scores. The DiD results are not only qualitatively consistent across measures but also quantitatively comparable. This impression is reinforced by looking at the event-study graphs for the alternative ability measures: Figures A1-A3 in the appendix reproduce the event-study DiD graphs for cognitive ability, psychological aptitude and GPA. They show similar dynamics as Figure 1. A somewhat more pronounced effect on minimum hire quality as measured by GPAs (available for younger cohorts) may hint at a relaxation of firms’ screening: it is plausible that relatively young hires, about whom firms may know little but their school grades, may benefit disproportionately from laxer hiring standards.

Table 3: EPL effect on different minimum hire quality measures

	(1)	(2)	(3)	(4)
	AKM	COG	NON-COG	GPA
DiD estimate (Treated*Post)=1	-0.0484*** (0.0096)	-0.0603*** (0.0126)	-0.0488*** (0.0124)	-0.0724*** (0.0150)
Observations	314,144	193,025	189,923	132,281
Firms	101,388	79,923	79,210	63,909
Adjusted R ²	0.0027	0.0062	0.0058	0.0071

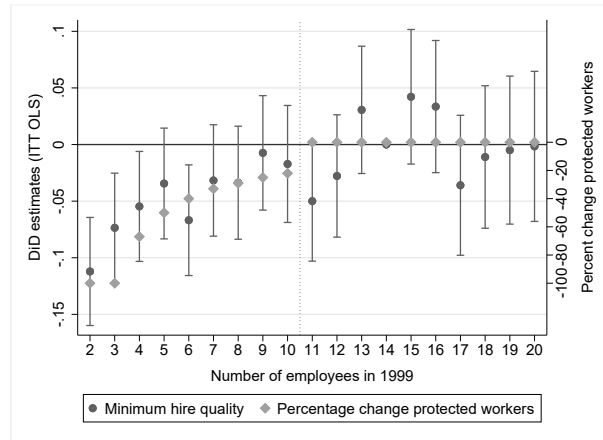
Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable is one of three minimum hire quality measures, based on: AKM person effects estimated 1986-1992 (1), military draft cognitive test scores (2), military draft psychological test scores (3), GPA at age 15 (4). Estimates are from OLS (ITT) specifications without covariates other than year dummies and are all based on the sample of firms from (1). The year 2000 is excluded to rule out anticipation effects.

4.2 Effect heterogeneity by reform bite

The EPL reform did not affect all treated firms equally. As we discuss in some detail in Section 2, firms with two and three relevant workers experienced a near abolition of employment protection, with the degree of EPL relaxation diminishing monotonically up to firms with ten workers. The light gray diamonds in Figure 2 illustrate this variation in reform bite. The figure also shows results from a DiD specification exploiting this heterogeneity. It splits up the overall DiD term into separate DiD terms for each firm size group (and uses firms with 14 workers as the reference

group). The results, shown in dark grey dots, are noisy, particularly further up the firm size distribution where there are fewer observations. Still, the results in Figure 2 strengthen the case that firms' minimum hire quality responded to the reform. The absolute magnitude of the DiD estimates is greatest for the smallest firms in our sample and near-monotonically falls with firm size.

Figure 2: EPL effect heterogeneity by firm size



Note: The dark gray dots show firm size-specific DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS (ITT) specification without covariates other than year dummies. The year 2000 is excluded to rule out anticipation effects. Hire quality is measured by AKM person effects estimated 1986-1992. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level. The light gray diamonds illustrate the bite of the reform in terms of the percentage change of workers protected by the seniority rule.

5 Mechanisms

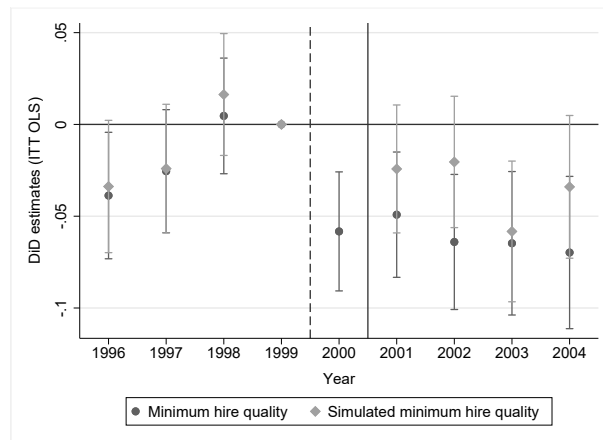
5.1 Number of hires channel

Von Below and Skogman Thoursie (2010) find that the 2001 EPL reform increased worker flows both at the hiring and the firing margin. Is the drop in minimum hire quality at treated firms driven by a reform-induced increase in the hiring rate, mechanically lowering minimum hire quality? To test this, we simulate a scenario where firms hire the same type of workers before and after the reform, not changing their selectiveness. We implement this by randomly re-shuffling new hires over time within firm size groups. For the re-shuffle we use the sub-period

from 1996-2004 and exclude the year 2000.²⁰ This ensures that the pre-and post-reform periods are of the same length so that there is approximately the same mass of pre- and post-reform hires in the simulation data.

In our simulation the type of hires is the same before and after the reform by design. If in reality, too, firms' selectiveness had been unchanged by the reform, the counter-factual exercise should leave the reform's estimated effect on minimum hire quality the same as the effect based on actual data. If, on the other hand, the estimated effect is attenuated by the simulation, this gives us an indication that firms hired systematically different workers after the reform. Figure 3 shows that in the counter-factual scenario, the reform's effect is less pronounced. Reassuringly, pre-reform coefficients are nearly identical but simulated reform effects are attenuated. Point estimates for the actual and counter-factual reform effects on minimum hire quality are given in Column (1) and (2) of Table 4. The estimated actual effect is nearly twice as large in absolute value as the counter-factual one. This suggests that a substantial part of the overall effect is driven not by firms hiring more workers but by firms otherwise changing their hiring behavior. We argue below that the likeliest candidate for a shift in firm hiring behavior is a change in firm selectiveness.

Figure 3: EPL effect and random hiring simulation



Note: The light gray diamonds are yearly DiD estimates for a counter-factual effect of the EPL reform on minimum hire quality from an OLS (ITT) specification. The data have been modified, leaving each firm's actual number of hires unchanged but randomly re-shuffling hired individuals across time within a firm size group. (The year 2000, when anticipation is possible, is omitted from the re-shuffle). For comparison, the DiD estimates for actual minimum hire quality are included as dark gray dots. Estimates are from OLS (ITT) specifications without covariates other than year dummies and are all based on the sample of firms from (1). Hire quality is measured by AKM person effects estimated 1986-1992. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

²⁰Potential anticipation effects make it difficult to unambiguously assign the year 2000 to before or after the reform.

5.2 Risky hires channel

Theoretically, changes in firm selectiveness may come in at least two guises. One is that firms are more willing to hire risky workers. That is, firms could refrain from screening out candidates whose ability is so difficult to judge a priori that it may be either so low the firm would not want to employ the worker or so high the worker would really boost productivity at the firm. Before the reform, Sweden's six-month probation period may not have been long enough to give them confidence they would learn the new hire's productivity in time to fire the worker without extra hiring costs. After the reform, however, treated firms may want to use their extra firing flexibility to harness the option value of risky workers explored by Lazear (1995). That is, these firms may want to hire workers whose ability is difficult to tell precisely because they can try to retain the workers that turn out to be unusually productive and lay off those that are revealed to be unproductive. One piece of evidence in favor of this mechanism is that the reform not only lowered minimum but also increased maximum hire quality at affected firms - see column (3) of Table 4. As above, though, this result may come about without a change in selectiveness as firms hire more workers, thus drawing larger samples from the ability distribution. We therefore use the same simulated data as above and estimate the reform's counter-factual effect on maximum hire quality in a scenario where, by design, firms cannot have changed their selectiveness. Column (4) of Table 4 shows that unlike the drop in minimum hire quality, the increase in maximum hire quality is entirely explained by the number of hires effect in our simulation. The evidence therefore does not support the interpretation that firms' intensified hiring of risky workers is the mechanism behind firms' reduced selectiveness.

Table 4: EPL effect mechanisms

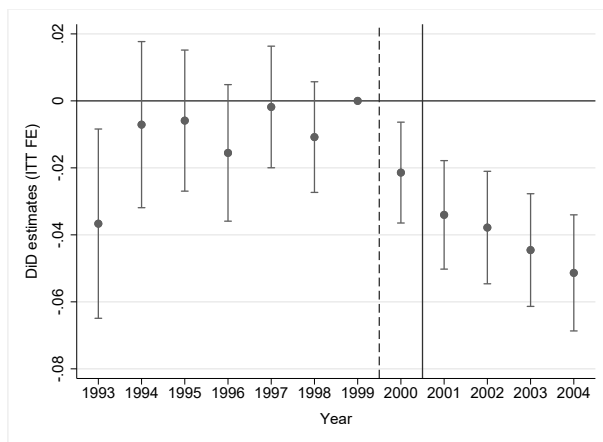
	(1)	(2)	(3)	(4)
	Minimum		Maximum	
	Actual	Simulated	Actual	Simulated
DiD estimate (Treated*Post)=1	-0.0478*** (0.0102)	-0.0253** (0.0099)	0.0309*** (0.0104)	0.0390*** (0.0104)
Observations	232,707	244,459	232,707	244,459
Firms	92,164	95,473	92,164	95,473
Adjusted R ²	0.0028	0.0017	0.0026	0.0023

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable: actual minimum hire quality (1), counter-factual minimum hire quality (2), actual maximum hire quality (3), counter-factual maximum hire quality (4). For the counter-factual effect estimate, the data have been modified, leaving each firm's actual number of hires unchanged but randomly re-shuffling hired individuals across time within a firm size group. Hire quality is measured by AKM person effects estimated 1986-1992. All estimates are from OLS (ITT) specifications without covariates other than year dummies. The year 2000 is excluded to rule out anticipation effects.

5.3 Lowered hiring threshold channel

An alternative mechanism, suggested by Pries and Rogerson (2005), is that loosened job protection makes firms more willing to give a chance to workers who at first do not appear very productive. The reform-induced drop in minimum hire quality may thus be driven by firms lowering their hiring standard. For more direct evidence of a lowering of hiring thresholds we look to firms' network-based hiring. Hensvik and Skans (2016) provide evidence that firms use their employees' past co-worker networks for screening purposes. We build on their result and use new hires' past links with incumbents as a screening proxy. The intuition is the following: if affected firms after the reform are happy to hire workers of potentially lower ability, it is less important for them to obtain reliable signals of new hires' productivity, e.g., through employee referrals. If so, we would expect to see the intensity of network-based hiring decrease at firms affected by the reform.²¹ As Figure 4 shows, this is indeed the case. The dynamics of the effect are very similar to those of the effect on minimum hire quality.²² We interpret this as evidence that relaxed screening in the form of a lowered hiring threshold is a key driver of the reform-induced drop in minimum hire quality.

Figure 4: EPL effect on network-based hiring



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on network-based hiring from a firm fixed effects specification without covariates other than year dummies. Network-based hiring is measured by new hires' average share of incumbents with whom they previously worked at another firm. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

²¹We follow Hensvik and Skans (2016) in our definition of new hires' co-worker links by counting the number of incumbents who previously worked at the same firm and establishment as the new hire. To ensure this measure is not mechanically related to firm size we divide new hires' average number of co-worker links to incumbents by the number of incumbents, giving us the share of incumbents the new hire is linked to.

²²Column (1) of Table B4 in the appendix shows that the effect size is also comparable to FE estimates of the reform's effect on minimum hire quality given in Table 7.

There are two caveats to this result. First, unlike our other results the estimates in Figure 4 are from a specification adding firm fixed effects to Equation 2. Without controlling for time-invariant firm heterogeneity in this way, pre-trends are not parallel, as Figure A4 in the appendix shows. Second, including firm fixed effects results in parallel pre-reform trends only back to 1994, as is clear from Figure 4. Barring a data issue we are unaware of this suggests that some specific event differentially affected the network-based hiring of treatment and control firms in 1993. Note, however, that trends are parallel again as we go back further in time, as Figure A5 in the appendix illustrates.

6 Threats to identification

6.1 The Gender Equality Act

As pointed out in Section 2 there was an amendment to the gender equality act (GEA) that roughly coincided with the 2001 LIFO reform. GEA, in force from 1992, called for firms of ten employees or more to annually publish two documents specifying measures to promote gender equality.²³ Effective in 2001, the wording of these requirements was made more concrete. Importantly, however, it did not fundamentally change their content (SFS, 1991, 2000).²⁴ In light of the modest legal changes it is unlikely that the GEA's effect on firm's hiring abruptly increased in 2001. This view is reinforced by the fact that compliance with the GEA's provisions was neither monitored nor enforced through sanctions. Still, the 2001 re-phrasing of the legal text may have reflected growing awareness of gender inequality and rising pressure to do something about it. In this case firms with 10 or more workers (mostly contained in our control group) may have felt more obliged to promote gender equality in their hiring choices. It appears likely that they would have responded by marginally favoring women in hiring, thus relaxing their hiring standard. In difference-in-difference terms, this is equivalent to a tightening of the hiring standard of the unaffected small firms (our treated group). As a consequence, if the 2001 strengthening of GEA had a perceptible effect on larger firms' hiring standards for women, it should bias our estimated EPL reform effects towards zero. To provide suggestive evidence

²³The mandate for an annual gender equality action plan was introduced in 1992 and called for, e.g., measures to promote women in male-dominated positions through training and recruitment. The mandate to publish an equal pay action plan was added in 1994 and asked firms to report on gender differences in pay and ways of removing them.

²⁴The requirement for an annual gender action plan was largely re-stated and the equal pay action plan mandate was re-worded to specify in more concrete terms what comparing women's and men's pay should entail (SFS, 1991, 2000).

whether this is the case we separately estimate the effect of the EPL reform on minimum female and male hire quality. Columns (1) and (2) of Table 5 are consistent with such an interpretation: the point estimate for women is less pronounced than that for men.

Table 5: EPL effect on minimum hire quality by gender and on firm attractiveness

	(1)	(2)	(3)
	Women	Men	E-E hires
DiD estimate (Treated*Post)=1	-0.0353*** (0.0128)	-0.0522*** (0.0122)	0.0109 (0.0084)
Observations	180,194	192,387	314,144
Firms	76,453	78,853	101,388
Adjusted R ²	0.0012	0.0019	0.0022

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable: female hires' minimum AKM person effect (1), male hires' minimum AKM person effect (2), share of hires transitioning directly from employment at a high-wage firm (above-median estimated AKM firm effect) (3). Estimates are from OLS (ITT) specifications without covariates other than year dummies and are based on the sample of firms from Column (1) in Table 3. The year 2000 is excluded to rule out anticipation effects.

6.2 Demand vs. supply

We have been using an equilibrium outcome (observed hires) to infer something about firms' (qualitative) labor demand. Implicit in this analysis is the assumption that labor supply remained constant, i.e., that workers' self-selection into firms was not systematically affected by the LIFO reform. To bias our results away from zero, the reform would have had to make affected firms less attractive employers. Theoretically the reform's value for new hires is ambiguous: in the short run it made treated firms more attractive as the lowest-tenure worker need no longer be laid off first. In the medium run, however, the reform reduced the value of seniority capital as second- and third-lowest tenure no longer afforded protection from layoffs. Beyond this, attractiveness was unchanged. A conclusive empirical answer to this question would require data on applicants, which we lack. We can, however, look at job changes that were likely voluntary and therefore contain information about firms' attractiveness as employers. As a proxy for voluntary job changes we use the share of new hires who worked at a high-wage firm right up to starting their new job.²⁵ As Column (3) of Table 5 shows, the LIFO reform has no significant effect on voluntary employment-to-employment transitions. We view this as suggestive evidence that

²⁵We define as high-wage firms those with a firm fixed effect estimate (from our AKM specification) above the median, irrespective of size.

the reform did not make treated firms less attractive to workers and that our assumption of unchanged (qualitative) labor supply is valid.

6.3 Non-classical measurement error in worker ability

It is likely that our estimated AKM person effects measure worker ability with substantial error. Those instances of such measurement error are unproblematic that affect our control and treated firms in the same way. As we estimate individual ability before our analysis period and in other firms, this is plausible in most cases. Take an example: to the extent that there is any wage discrimination in the labor market, women’s wages (and thus AKM) will systematically understate their ability. However, thanks to our DiD-setting, this will not be a problem unless we underestimate to a particularly large extent the ability of women hired into treated firms from 2001 on, which is unlikely.²⁶ However, a related problem would be if the reform increased the share of women hired: in this case, the share of underestimated ability would have increased and that might bias minimum hire quality downward. We test this possibility in Column (1) of Table 6. There is no detectable effect of the 2001 reform on the female hire share, allaying this concern.

Table 6: EPL effect on female hire share and AKM coverage

	(1)	(2)
	Share Women	Share AKM
DiD estimate (Treated*Post)=1	-0.0040 (0.0085)	-0.0196** (0.0089)
Observations	314,144	314,144
Firms	101,388	101,388
Adjusted R ²	0.0014	0.0292

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable: female hire share (1), share of hires for whom an estimated AKM person effect is observed (2). Estimates are from OLS (ITT) specifications without covariates other than year dummies and are based on the sample of firms from Column (1) in Table 3. The year 2000 is excluded to rule out anticipation effects.

6.4 Selective worker ability measurement

We do not observe worker ability for all new hires. In 1999, at the average firm in our estimation sample we have an estimated AKM person effect for 55.4% of new hires. Our DiD setting and

²⁶An analogous example would be if our AKM person effect estimates contained some experience or tenure effect even though our specifications include age polynomials. The reason this is likely to be unproblematic is that both treated control firms’ hire quality will be similarly affected by the mis-measurement this introduces.

ability measurement ensure that this is only problematic if the share of new hires whose ability we can measure is positively affected by the reform. Similarly to the number of hires effect described in Section 5.1, this may mechanically drive down minimum hire quality even if firms' selectiveness remains unchanged. To test for this in our sample, Column (2) of Table 6 reports results from using firms' share of new hires with an AKM person effect estimate as an outcome. It shows we do not observe the ability of relatively more hires at treated firms. In fact, the point estimate is *negative* and significant, implying that, if anything, selective ability measurement will bias our results towards zero.

6.5 Time-invariant firm heterogeneity

Consider a scenario in which the composition of small firms deteriorates over time, so that small firms observed after the reform are negatively selected as compared to those observed before. Suppose that the composition of large firms remains unchanged. A credit boom with lots of new entrants, for example, may produce such a pattern. This scenario could theoretically deliver similar results as our study in the absence of any EPL-induced change of selectiveness in hiring. The danger of such a confounding scenario is somewhat reduced by the fact that we condition on firms existing in 1999. However, it can be ruled out by controlling for unobserved time-invariant firm heterogeneity. Column (2) of Table 7 reports results from a fixed-effects version of our DiD specification. While this reduces the size of our estimated effect by roughly a quarter, the reform's negative effect on minimum hire quality remains strongly significant.²⁷

Table 7: EPL effect on minimum hire quality: FE and IV

	(1)	(2)	(3)
	OLS	FE	IV
DiD estimate (Treated*Post)=1	-0.0484*** (0.0096)	-0.0359*** (0.0104)	-0.1162*** (0.0207)
Observations	314,144	314,144	314,144
Firms	101,388	101,388	101,388
Adjusted R ²	0.0027	0.0010	.

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. (1) OLS (ITT); (2) FE (ITT) (3) 2SLS (LATE). All specifications include only year dummies as covariates. Dependent variable: minimum hire quality. Hire quality is measured by AKM person effects estimated 1986-1992. The year 2000 is excluded to rule out anticipation effects. The first-stage F statistic for (3) is 5563.93.

²⁷Figures A6-A9 in the appendix show that adding firm fixed effects to Equation 2 yields results that are qualitatively the same as OLS, though noisier.

6.6 Endogenous firm size and overcorrection

Column (3) of Table 7 reports an estimate of the reform’s local average treatment effect (LATE). The two-stage least squares specification uses size in 1999 as an instrument for time-varying actual firm size. It zooms in on the reform’s effect on those firms that, based on their 1999 size, were likely to also be small enough to benefit from the reform from 2001 onward. The LATE addresses the possibility that the ITT effect used in our main specifications is overly conservative as it classifies firms no longer benefiting from the reform as treated and vice versa. As expected, the LATE results are more pronounced than our main (ITT) results.

6.7 Non-parallel counter-factual trends: firm size

Our results may be spurious if minimum hire quality in different firm size groups had deviated from parallel trends even in the absence of the reform. While this is impossible to ascertain for the firm sizes actually affected by the reform, placebo estimates comparing firms further up the firm size distribution provide a sense of how likely non-parallel counter-factual trends in minimum hire quality are. To this end we define as placebo treated and placebo control those firms that are 10, 20 and 30 workers bigger than our actual treated and untreated firms. This gives us placebo firm size cut-offs at 20, 30 and 40 workers. Table 8 gives the results from estimating Equation 1 on these placebo treatments. Reassuringly, none of the DiD estimates of the placebo effects are statistically significant.

Table 8: EPL effect on minimum hire quality: placebo tests

	(1)	(2)	(3)
Placebo firm size cut-off	20	30	40
DiD estimate (Treated*Post)=1	0.0016 (0.0029)	-0.0061 (0.0040)	-0.0012 (0.0050)
Observations	109,735	54,044	33,130
Firms	22,331	9,063	4,902
Adjusted R ²	0.0051	0.0064	0.0088

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Placebo tests. (1) placebo TR: firms with 12-20 workers, placebo CT: 21-25. (2) placebo TR: 22-30, placebo CT: 31-35. (3) placebo TR: 32-40, placebo CT: 41-45. All specifications include only year dummies as covariates. Dependent variable: minimum hire quality. Hire quality is measured by AKM person effects estimated 1986-1992. The year 2000 is excluded to rule out anticipation effects.

7 Conclusion

In this paper we study the effect of reduced employment protection on firms' worker selection. By doing so, we provide empirical evidence on a less obvious way in which labor market regulation affects both efficiency (through the quality of worker-firm matches) and equity (through the opportunities of the least productive workers).

To obtain quasi-experimental variation in employment protection we use the 2001 relaxation of Sweden's seniority rule. This allows us to test *if* reduced dismissal costs make firms less stringent in their worker selection and if so, *how*. Using individual-level data for the universe of Swedish workers and several ability measures, we find that the EPL reform lowers minimum hire quality at affected firms by about 5% of a standard deviation. This result is robust to addressing various scenarios that threaten the validity of our identification strategy.

To isolate the portion of the EPL effect on minimum hire quality due to a change in firms' selectiveness we implement a counter-factual simulation. We find that about half of the overall EPL effect is explained by an increase in the hiring rate, which mechanically lowers minimum hire quality, whereas the other half represents a change in the selectiveness of firms' hiring. This is our first main finding: employment protection legislation directly affects *who* gets hired.

We devise tests to investigate two mechanisms that may be driving the effect on firms' hiring selectivity. The first possible mechanism is that firms hire riskier workers as dismissal costs fall (Lazear, 1995), implying a widening of the spread of hire quality both at the bottom and the top. To test for it, we look at the response of maximum hire quality to the EPL reform. We do not find evidence for this channel.

The second possible mechanism is that employers respond to looser employment protection by lowering their hiring standard, i.e., the productivity threshold above which they hire applicants (Pries and Rogerson, 2005). We build on the literature on network-based hiring, in particular, Hensvik and Skans (2016), to obtain direct evidence for a change in firms' screening of workers. Our analysis of new hires' past co-worker links with incumbents at their new firm suggests that the reform makes employers rely less on their employees' networks to screen potential recruits. We conclude that a lowering of firms' hiring standard drives the EPL effect on firms' selectiveness in hiring. Our second key result is thus that EPL affects firms' worker selection by shifting their hiring threshold as suggested by Pries and Rogerson (2005).

Our finding that employment protection directly affects firms' worker selection is consistent with the scarce evidence that already exists (Marinescu, 2009; Bjuggren and Skedinger, 2018).

We go beyond these papers by establishing a causal effect on the *ability* of new hires and by providing direct evidence for a change in screening intensity.

This is the first study to provide evidence on the mechanisms by which EPL influences firms' worker selection, i.e., *how* dismissal costs affect the selectiveness of firms' hiring. By empirically assessing the predictions of different theoretical models we hope to inform the modelling choices of future theoretical work on worker-firm matching.

Following the 2018 general election in Sweden, the Swedish government re-opened debate on a possible loosening of employment protection legislation that would exempt more firms from the LIFO rule. The implication of our results for this policy discussion is that a further reduction of employment protection may make it yet easier for the least productive job-seekers to get a foot in the door. This points to an intuitive trade-off in the design of labor market policy between safeguarding the job security of insiders and making work attainable for the weakest outsiders.

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Appendix

Table B1: Data sources

Data source	Period used	Description	Used for measurement of	Variables used/generated
JOBB	1986-2004	Employment spells	Worker flows	Timing and identity of hire
JOBB	1993-2004	Employment spells	Firm size	Monthly head count
JOBB	1993-2004	Employment spells	Other firm characteristics	Industry, age, location, ownership
JOBB	1986-2004	Employment spells	Worker labor market history	Co-workers
WSS	1986-1992	Wage survey statistics	Wages	Full-time equivalent monthly wages
LISA	1990-2004	Various information	Worker characteristics	Highest educational qualification
BAKGRUND	n.a.	Birth register	Worker characteristics	Year of birth, gender
KRIGSARKIVET	1970-2004	Military draft information	Worker ability	Cognitive, psychological test scores
ÅRSKURS9	1988-1997	Grade 9 school grades	Worker ability	GPA at age 15

Table B2: EPL effect on minimum hire quality: mechanisms, with outliers

	(1)	(2)	(3)	(4)
	Minimum		Maximum	
	Actual	Simulated	Actual	Simulated
DiD estimate (Treated*Post)=1	-0.0473*** (0.0101)	-0.0249** (0.0099)	0.0341*** (0.0104)	0.0375*** (0.0104)
Observations	235,187	245,211	235,187	245,211
Firms	92,774	95,689	92,774	95,689
Adjusted R ²	0.0027	0.0017	0.0026	0.0024

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, with standard errors clustered at the firm level. Data include worker inflow outliers. Dependent variable: actual minimum hire AKM (1), counter-factual minimum hire AKM (2), actual maximum hire AKM (3), counter-factual maximum hire AKM (4). For the counter-factual effect estimate, the data have been modified, leaving each firm's actual number of hires unchanged but randomly re-shuffling hired individuals across time within a firm size group. All estimates are from OLS (ITT) specifications without covariates other than year dummies. The year 2000 is excluded to rule out anticipation effects.

Table B3: EPL effect on minimum hire quality: firm size corrections

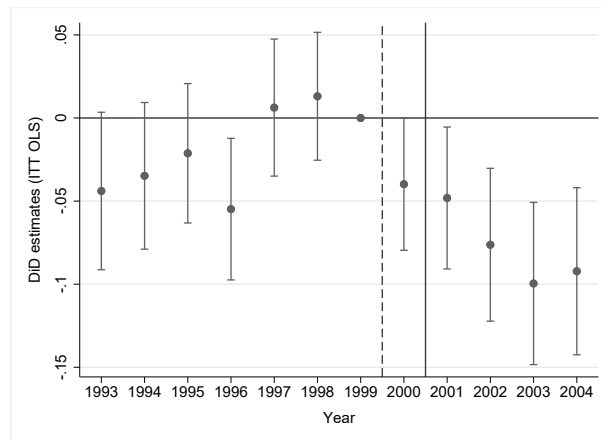
	(1)	(2)	(3)
DiD estimate (Treated*Post)=1	-0.0484*** (0.0096)	-0.0469*** (0.0096)	-0.0621*** (0.0089)
Observations	314,144	312,152	347,798
Firms	101,388	99,873	124,328
Adjusted R ²	0.0027	0.0027	0.0028

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Different versions of reform bite measure: (1) head count -1 all firms; (2) head count - managers and family where known, -1 otherwise; (3) unadjusted head count. All estimates are from OLS (ITT) specifications without covariates other than year dummies. Dependent variable: minimum hire quality. Hire quality is measured by AKM person effects estimated 1986-1992. The year 2000 is excluded to rule out anticipation effects.

Table B4: EPL effect on network-based hiring

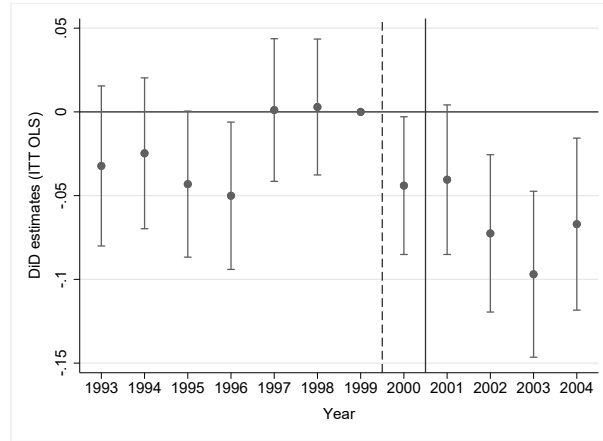
	(1)	(2)
DiD estimate (Treated*Post)=1	-0.0347*** (0.0059)	-0.0311*** (0.0059)
Observations	618,260	659,637
Firms	121,242	121,701
Adjusted R ²	0.0071	0.0078

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Estimates are from firm fixed-effects specifications without covariates other than year dummies. Dependent variable: network-based hiring, measured by new hires' average share of incumbents with whom they previously worked at another firm. (1) is for the period 1994-2004. (2) is for 1993-2004. The year 2000 is excluded to rule out anticipation effects.

Figure A1: EPL effect on minimum hire cognitive test score

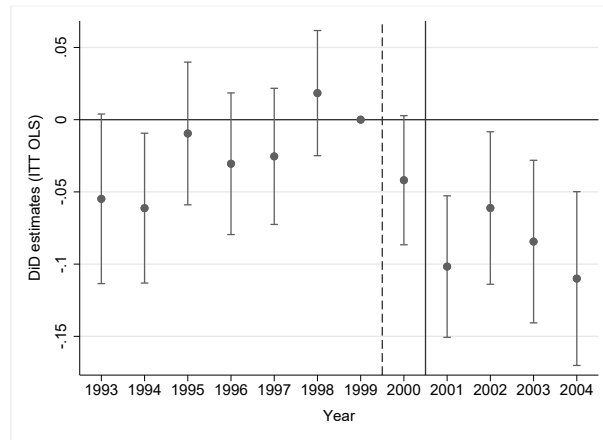
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS (ITT) specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by cognitive test scores from the military draft. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A2: EPL effect on minimum hire psychological test score



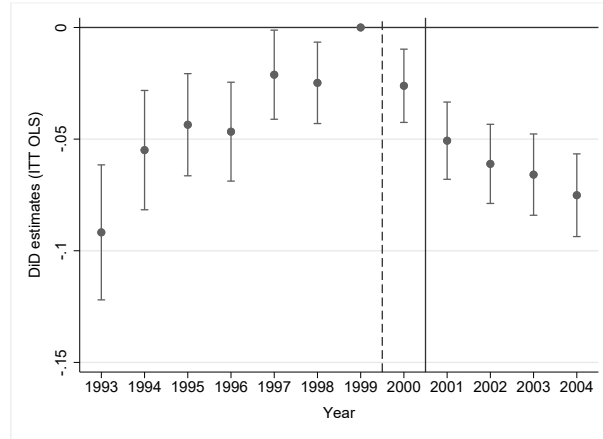
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS (ITT) specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by psychological test scores from the military draft. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A3: EPL effect on minimum hire grade point average



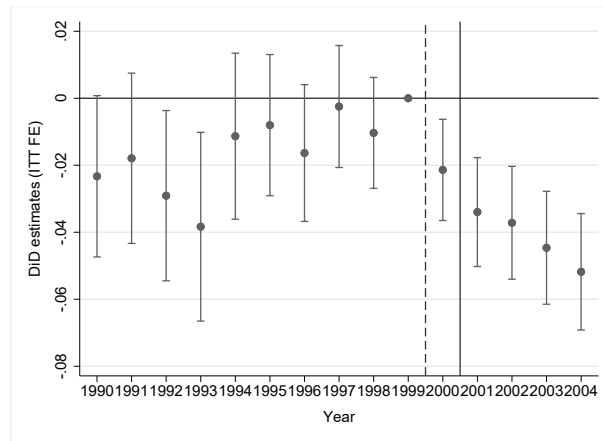
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS (ITT) specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by GPA at age 15. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A4: EPL effect on network-based hiring



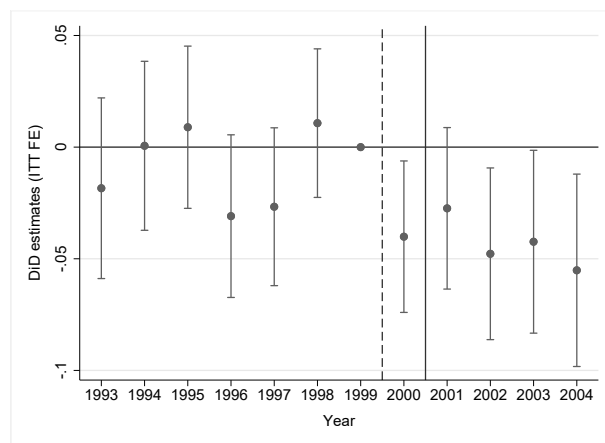
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on network-based hiring from an OLS specification without covariates other than year dummies. Network-based hiring is measured by new hires' average share of incumbents with whom they previously worked at another firm. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A5: EPL effect on network-based hiring (FE)



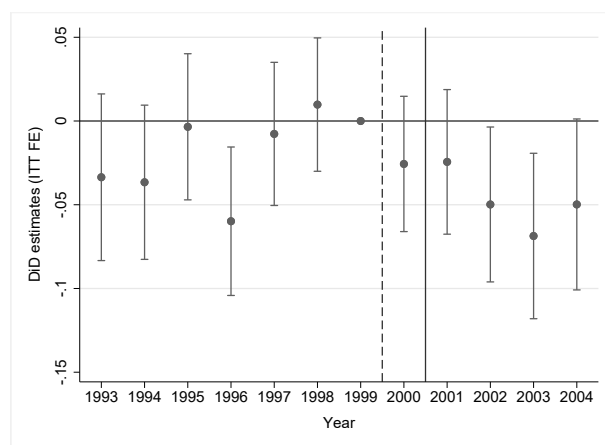
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on network-based hiring from a firm fixed-effects specification without covariates other than year dummies. Network-based hiring is measured by new hires' average share of incumbents with whom they previously worked at another firm. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A6: EPL effect on minimum hire quality (FE)



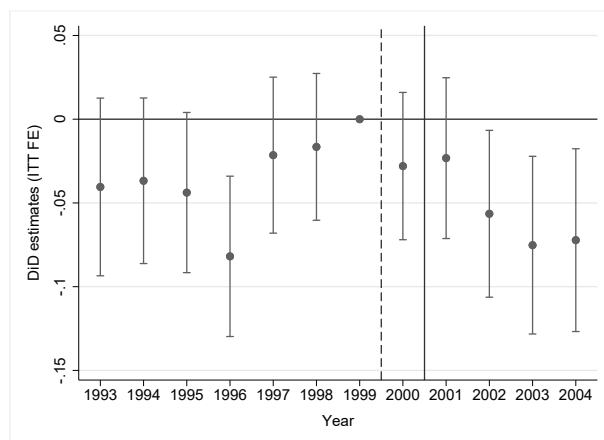
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from a firm fixed-effects specification without covariates other than year dummies. Hire quality is measured by AKM person effects estimated 1986-1992. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A7: EPL effect on minimum hire cognitive test score (FE)



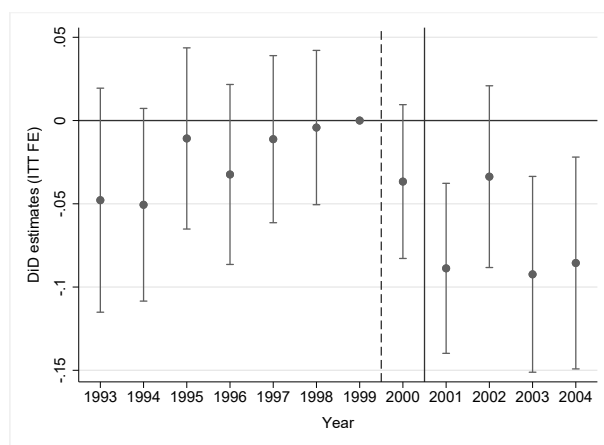
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from a firm fixed-effects specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by cognitive test scores from the military draft. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A8: EPL effect on minimum hire psychological test score (FE)



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from a firm fixed-effects specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by psychological test scores from the military draft. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure A9: EPL effect on minimum hire grade point average (FE)



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality from a firm fixed-effects specification without covariates other than year dummies. The sample of firms is the same as in Figure 1. Hire quality is measured by GPA at age 15. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.