

The effect of employment protection on firms' worker selection*

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September 13, 2021

Abstract

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, GPA at age 15, and military test scores. We find that the reform reduced minimum hire quality by around 2%. Our results show that firms both decrease their hiring thresholds and hire more workers. We find that firms increasingly hire young, foreign born and long-term non-employed individuals, suggesting potential welfare gains of the reform.

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

JEL Classification Numbers: M51, D22, J24, J38.

*We thank Adrian Adermon, Matthew Lindquist, Erik Lindqvist, Martin Nybom, Björn Öckert, Anna Sjögren, Dirk Sliwka, Josef Zweimüller, and workshop and seminar participants at the University of Cologne, Erasmus University Rotterdam, the Research Institute for Industrial Economics, the Institute for Evaluation of Labour Market and Education Policy and the IAB Nuremberg, and the EALE-SOLE-AASLE conference for helpful comments. We are grateful to Johan Egebark, Erik Sjödin and Erik Bihagen for their expertise on Swedish data and law and to Anders Forslund for making us aware of the 2001 employment protection reform in Sweden. The paper makes use of and benefited from the Stata modules `coefplot` for figures (Jann, 2014) and `esttab` for tables (Jann, 2007). Sebastian Butschek thanks the German Research Foundation for funding (DFG SSP 1764). Jan Sauermann thanks the Jan Wallanders och Tom Hedelius Stiftelse for financial support (Grant numbers I2011-0345:1 and P2014-0236:1).

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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 may 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.

Theoretical work on the hiring process suggests an effect of labor market regulation on the selectiveness of firms’ hiring. For example, the matching model in Pries and Rogerson (2005) predicts that looser regulation reduces firm selectiveness: as the cost of hiring (and firing) the marginal worker falls, firms lower their hiring standard and hire more workers. In another possible story, firms may respond by hiring “worse” workers for a given number of hires if the change in regulation makes it feasible to reduce screening costs.

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 policy change in Sweden that relaxed one aspect of EPL, namely seniority rules. Seniority rules dictate that the worker with the shortest tenure be laid off first.¹ The reform partially exempted small firms from seniority rules, making it easier for them to lay off workers. This 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. Our central result is that the relaxation of EPL reduced firms’ selectiveness by about 2.2% of a standard deviation; we also find that firms’ lowered hiring thresholds are tightly linked to increased hiring rates; and that the reform improved the hiring prospects of workers who were young, foreign-born or long-term non-employed.

The main contribution of this paper is to show that employment protection legislation affects firms’ hiring thresholds. This confirms and extends the findings of two other papers that

¹Seniority rules are stipulated by law in France, the Netherlands, Italy, and Mexico (amongst others); they are often part of collective agreements in the US and Finland. Their application is also widespread (though not legally enshrined) in the United Kingdom and Norway (von Below and Skogman Thoursie, 2010).

provide evidence consistent with EPL affecting worker selection (Marinescu, 2009; Bjuggren and Skedinger, 2018). We add new insights by showing: that the ability of low-skill hires measurably changes in response to the reform; that these changes are consistent with a mechanism proposed by theoretical work (Pries and Rogerson, 2005); and that various worker groups who are potentially discriminated against in the labor market seem to benefit from relaxed hiring standards.

Our first step in investigating whether EPL changes worker selection is to test if the reform represented an effective treatment, i.e., whether EPL was binding for firms in the first place. We present evidence suggesting that firms' behavior was indeed constrained by the seniority rules before the policy change. Having established the relevance of the reform, we estimate the 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. Our preferred proxy for worker productivity are estimated worker effects (AKM). 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 2.2% of a standard deviation. Our alternative ability measures are grade point average (GPA) at age 15, and military draft test scores for cognitive and psychological aptitude at age 18. Effect estimates are 6.8% of a standard deviation for GPA, 4.1% for psychological test scores and 2% for cognitive test scores. Estimating heterogeneous treatment effects for firms of different sizes, we conclude that the reform's effect on minimum hire quality is likely to be driven by small firms, for which the reform had the largest bite.

To support the causal interpretation of our results, we address a number of threats to identification. These include concurrent changes in labor market legislation, selective worker ability measurement and non-parallel counterfactual post-reform trends. Our results are robust to a range of tests, including bandwidth tests that vary the control group definition and placebo tests that compare unaffected firms of different sizes.

We then turn to discussing our main finding along three dimensions. First, we probe our interpretation of the drop in minimum hire quality as a demand-side phenomenon. We start by looking for direct evidence of a reduction in firm screening. Building on the finding in Hensvik and Skans (2016) that firms use incumbent workers' networks of past co-workers to hire selectively, we check if referral-based hiring is reduced in treated firms. As the results are inconclusive, we proceed to identify and try to rule out potential supply-side drivers of our result. We present suggestive evidence that treated firms do not become less attractive to

positively selected workers but cannot rule out that negatively selected workers may contribute to our main result by searching harder for jobs at treated firms.

Second, we explore two potential mechanisms behind our main result: (i) the Pries-Rogerson channel, where firms lower their hiring thresholds as they hire more workers; and (ii) a drop in firms' selectiveness for a given number of hires as greater ease of firing allows employers to save costs by reducing pre-hire screening and intensifying on-the-job screening. As the two mechanisms are not mutually exclusive, we cannot quantify their respective contributions to the overall effect. However, we find compelling evidence primarily in support of the first mechanism: we not only confirm the existing result that the reform increased hiring (von Below and Skogman Thoursie, 2010), but also use a simulation exercise to demonstrate that lowered hiring thresholds and increased hiring rates are not independent consequences of the reform but imply each other, as in Pries and Rogerson (2005).

Third, we study welfare implications by looking at various indicators of the reform's impact on the hiring prospects of disadvantaged workers. We use worker-level unconditional quantile regressions to check if the quality of the least productive hires overall deteriorated in response to the reform and if the reform lowered the age of the youngest hires. Returning to the firm level, we estimate the reform's effect on the hire share of 15-24 year olds and foreign-born workers and the hiring rate of long-term non-employed individuals. While the quantile-regression analyses do not indicate that the reform brought into employment people with lower ability than would otherwise have been hired, we do find gains for the young, foreign born and long-term non-employed.

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 and in exploring the welfare implications of relaxed hiring standards it extends scarce existing evidence. Three studies present evidence consistent with regulation affecting firms' worker selection. Marinescu (2009) shows that shortening the qualification period for claiming unfair dismissal in the UK decreased 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

²The finding in Kugler and Saint-Paul (2004) that stronger EPL decreases the chances of unemployed individuals is also similar to our conclusion. In their setting with adverse selection, unemployed workers are more negatively selected than employed workers because firms first lay off the lemons. When firing costs increase, firms rely on statistical discrimination to avoid unproductive hires—they shy away from hiring unemployed job-seekers. In our setting, by contrast, firms screen and directly identify less productive workers before hiring them.

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 as unemployed workers are harder to screen. The key distinction of our approach is that we rely on measures of individual ability (rather than on labor market status) to infer something about the selectiveness of hiring. We then also look at the hiring prospects of unemployed workers (and those of other groups), but we do so from a different perspective: given our evidence that firms hire less selectively, can we show that vulnerable labor market participants benefit from this response? These conceptual differences notwithstanding, Bjuggren and Skedinger (2018) and our paper come to consistent and complementary conclusions.

Also relevant to our study is the finding of Bjuggren (2018), who studies the effects of the EPL reduction on firm productivity. 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 need not be a contradiction: in standard models of employment protection, reduced EPL improves the allocation of labor not through who is hired in the first place but through increased labor market churning (Hopenhayn and Rogerson, 1993). Indeed, the reform has been shown to have increased both firms' hiring and their firing (von Below and Skogman Thoursie, 2010), a result that we confirm.³ It is possible that firms boosted worker productivity by first hiring less selectively but then identifying and retaining the abler workers. While we do not find evidence of increased selectiveness of firing, we also cannot rule it out.

By studying the effects of dismissal costs on worker-firm matching, this paper also expands the scope of the literature on the effects of Sweden's 2001 EPL reform on labor market outcomes more broadly. Existing studies have analyzed the reform's effect on sickness absence (Lindbeck et al., 2006; Olsson, 2009) and parental leave take-up (Olsson, 2017).⁴

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, 2019), the relation between management quality and the

³While von Below and Skogman Thoursie (2010) find no reform effect on net employment, Bornhäll et al. (2017) conclude that treated firms do grow slightly faster. For work on how EPL and the associated separation costs affect job creation and job destruction, see, for example, Bentolila and Bertola (1990), Abowd and Kramarz (2003), Garibaldi and Violante (2005), and more recently Cahuc et al. (2019) and Munoz and Micco (2019).

⁴As 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.

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., 2021). 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.

The remainder of this paper is structured as follows. In Section 2 we present the institutional background of the reform studied in this paper. Section 3 provides information on the data and method we use to estimate the effect of the reform. We present our main results in Section 4 and test their robustness in Section 5. Section 6 is devoted to discussion: we gauge the interpretation of our results as a labor demand phenomenon in Section 6.1, explore potential mechanisms in Section 6.2 and discuss potential welfare implications in Section 6.3. Section 7 concludes.

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.⁵

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

⁵See 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.

choose whom to lay off.⁶ 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).

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 5.1.

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 3.4).

⁶One 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.

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.⁷ 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).

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.

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, Bäckerman 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 worker skill.⁸ 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

⁷Both 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).

⁸To 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.

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 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.

Data sources We construct information on worker flows from *Jobbregistret* (JOBB), a register containing the universe of individual employment spells in Sweden from 1993 onwards. The spell data allow us to both identify new hires and measure firm size at monthly resolution.⁹ In addition, we use JOBB to obtain information on firms' industry, ownership (private/public), location and age, and on workers' non-employment spells and co-worker links (see Section 6.1). Drawing on other registers, we obtain further individual characteristics, such as workers' gender, age, highest education and GPA from the last grade of compulsory school (ninth grade). For a majority of male hires we can also match military draft test scores to our sample. Table C1 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.¹⁰

⁹LISA, a multi-purpose register containing similar information, would only have given us individuals' employment in November of each year.

¹⁰We could use a shorter pre-reform period. We show in Table C2 in the Appendix that this leaves our results virtually unchanged.

Sample restrictions In our main analyses we focus on small firms employing between two and 19 workers between 1993 and 2004. Firms are only included in our estimation sample in those years in which they meet this size restriction, so that we have at least one and at most twelve (yearly) observations per firm. For our analysis period this gives us 1,585,770 observations for 330,103 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.¹¹

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 unexpected, we view it as exogenous variation in EPL over time for small firms (our treated group). To identify a causal effect we compare the evolution of treated firms' hiring behavior over time with that of similar but unaffected firms, namely those a bit larger than the eligibility cut-off of 10 workers.

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:

$$y_{jt} = \alpha + \beta TR_{jt} * POST_t + \gamma TR_{jt} + \delta_0 X_{jt} + \delta_1 TR_{jt} * X_{jt} + \eta_j + \theta_t + \epsilon_{jt}, \quad (1)$$

where the outcome variable y_{jt} is a measure for firms' selectiveness in hiring. In our main analyses, it is defined as the minimum ability of firm j 's hires in year t (see Section 3.5). Firms j are defined as treated ($TR_{jt} = 1$) whenever they have 2 to 10 employees. Firms with 11 to 19 workers are used as the control group (see Section 3.6 for details on this choice). 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 the reform was not yet in effect but during part of which anticipation behavior is possible, is dropped from this DiD specification. X_{jt} are county-level gross regional product and unemployment rate and $TR_{jt} * X_{jt}$ is their interaction with the treatment dummy (addressing the possibility that macro shocks differentially affect very small and small firms). η_j are firm fixed effects (we also report results without firm fixed effects) and θ_t are year fixed effects. Standard errors are clustered at the firm level.

¹¹We show in Tables C3 and C4 in the Appendix that this restriction does not drive our results.

Our key identifying assumption is $\mathbb{E}[\epsilon_{jt}|TR_{jt} * POST_t] = 0$: 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:

$$y_{jt} = \alpha + \sum_t (\beta_t TR_{jt} * \theta_t) + \gamma TR_{jt} + \delta_0 X_{jt} + \delta_1 TR_{jt} * X_{jt} + \eta_j + \theta_t + \epsilon_{jt}, \quad (2)$$

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.

Where pre-reform trends are non-parallel but display a trend that looks approximately linear, we fit a linear trend to the pre-reform estimates of β_t and extrapolate this trend to the years 2000-2004. This allows us to estimate the effect of the reform by comparing post-reform estimates of β_t to the post-reform extrapolation of the pre-reform trend. The interpretation of these results require the assumption that the linear pre-trends would have continued after 1999 absent the reform.

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 5.3.¹²

Finally, our identifying assumption would also be violated if firms strategically self-selected into treatment after the reform. We address this threat in the next subsection.

3.3 Endogenous sorting into treatment

The DiD estimate of the reform's effect on hiring standards is given by $\hat{\beta}$. As we use a time-varying firm headcount for treatment assignment, one may worry that our DiD specification yields a biased estimate of β , that is, an estimate that is sensitive to the endogeneity of firm size: from 2000 onwards, well-managed firms may have downsized strategically to benefit from the

¹²Spillover effects of the reform to non-directly impacted firms represent another potential source of bias. This would be the case if affected firms' hiring response changed the availability to non-affected firms of certain types of labor. However, given the treated firms' share of the labor force (11.2%), this effect is likely to be small.

added flexibility in laying off workers. We address this concern in two ways. First, including firm fixed effects ensures that $\hat{\beta}$ is identified by treatment status stayers, i.e., those with $TR_{jt} = 1$ before and after and those with $TR_{jt} = 0$ before and after (unlike $\hat{\gamma}$, which is identified by treatment status changers). Second, we provide evidence against systematic downsizing around the eligibility cut-off in this subsection.

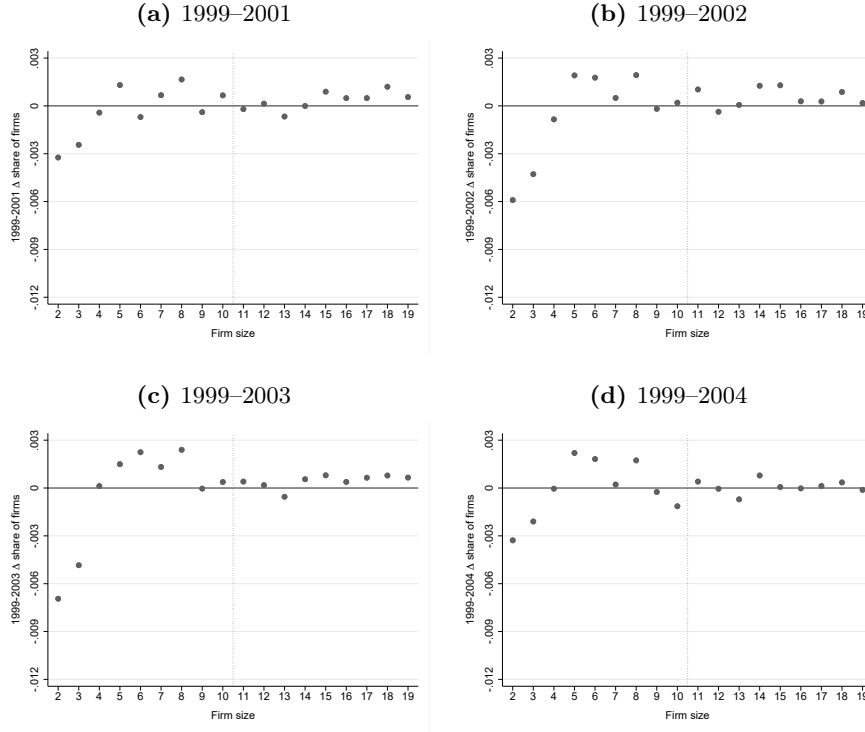
We explicitly choose a “naive” estimation strategy with a time-varying treatment indicator over an intention-to-treat (ITT) approach with a time-invariant treatment indicator defined in the pre-treatment period. While ITT confers immunity to endogenous selection into treatment, in our setting, it introduces another, more severe, source of bias. As we elaborate in a separate paper, the combination of a substantial share of firms that non-selectively change treatment status and a levels difference in the outcome variable between the treated and control groups biases $\hat{\beta}$ away from zero (Butschek and Sauermann, 2021).

With firm fixed effects (FE) included, Equations (1) and (2) yield estimates of β and β_t that are unaffected by potential self-selection of firms into (or out of) treatment. However, the OLS versions without firm fixed effects, which we also use, are sensitive to the endogeneity of firm size. That is, for instance, if firms with particularly un-selective hiring strategically downsize to benefit from the relaxation of employment protection for small firms, $\hat{\beta}$ may be downward biased (away from zero if the true effect is non-positive).

Whether or not firms actually downsize systematically can be tested empirically. To this end we look at how the firm size distribution changes over time. If there were strategic downsizing we would expect bunching to emerge after the reform: the share of firms just below the eligibility cut-off should increase and the share of firms just above should decrease. Figure D1 in the Appendix compares firm size histograms for 1999, the last full pre-reform year, with each of the post-reform years 2001-2004. There is no indication that mass is shifted from just above the eligibility cut-off (10 workers or less) to just below. Figure 1 zooms in on the within-firm size changes. For each firm size bin, it plots the change in that bin’s share among all firms in our estimation sample from 1999 to the post-reform years 2001-2004. The values are very close to zero and show no systematic pattern to either side of the cut-off; in particular, the change in shares close to the cut-off does not stand out from those further away.

The absence of bunching around the cut-off suggests that if there is strategic downsizing it is so rare that it unlikely introduces bias in our OLS results. We therefore view both the OLS and the FE versions as causal estimates; the key difference is that the OLS estimate captures changes in firm composition and the FE estimate does not.

Figure 1: Year-to-year changes in share of firms with respective size



Note: The gray dots plot the change in a size bin's share of firms between 1999 and each of the years 2001 (a) to 2004 (d). For example, a value of zero for firm size 14 in (a) means that the share of firms with headcount 14 among all firms of size 2-19 was the same in 2001 as in 1999.

3.4 EPL reform bite measure

We want to compare firms potentially affected by the EPL reform to unaffected firms; and we study the firm size distribution to explore how appropriate our empirical strategy is. Both endeavours make 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.¹³

¹³In 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 C5 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.

3.5 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, $\min_{j,t}\{ability_i\}$. Measuring firms' time-varying minimum hire quality requires individual-level ability proxies. Thanks to the wealth of Swedish register data there are three main options for proxying 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); and third, estimated worker effects (AKM) from a two-way fixed-effects log wage regression with worker and firm effects (Abowd et al., 1999); we provide details on our estimation of an AKM model in Section A in the Appendix.¹⁴ These productivity proxies differ along several dimensions, three of which we care about: quality, relevance/salience to the employer and coverage.

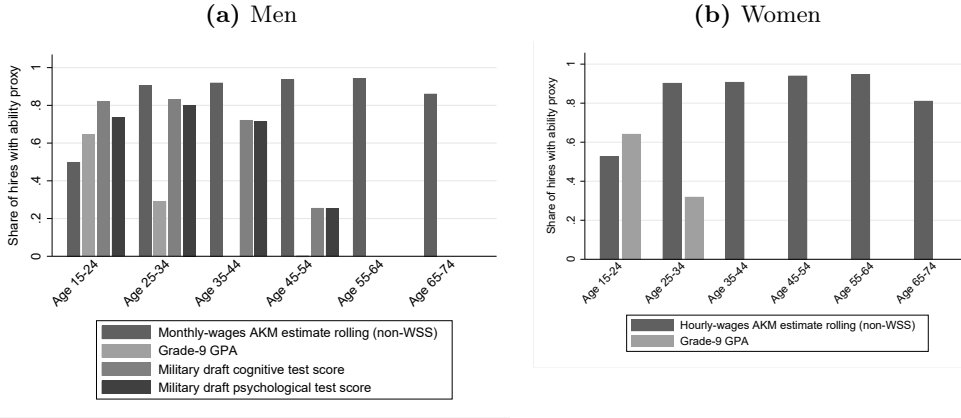
Quality considerations arguably favor the scores from standardized tests, i.e., draft test scores COG and NONCOG as well as GPA. There is clear evidence that COG and NONCOG predict labor market success (Lindqvist and Vestman, 2011); similarly, school grades have been shown to be positively correlated with wages, especially at the beginning of the career (Farber and Gibbons, 1996; Altonji and Pierret, 2001).¹⁵ AKM is the least direct of our productivity proxies; however, it is positively correlated with both COG and NONCOG (Butschek and Sauermann, 2019b).

¹⁴There are some potential drawbacks to using AKM. One is that they 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. A more fundamental concern is the theoretical point raised by Eeckhout and Kircher (2011) that in the presence of complementarities between firm and worker productivity as well as search frictions, wages may not be monotonic in worker ability.

¹⁵GPA at age 15 indirectly affects labor market outcomes, too, by constraining individuals' choices for upper secondary school programmes (Dahl et al., 2020).

Of our ability proxies, only GPA is likely to be directly observed by potential employers, especially for young people. While individuals can get information on their military draft test scores after the test, test scores cannot be verified by potential employers and are therefore not likely to be part of hiring assessments.¹⁶ However, firms could use cognitive ability tests of their own in their pre-hire screening, or try to gauge candidates' psychological aptitude for a given task or setting in an interview or a psychological assessment. The individual time-invariant pay premia captured by AKM worker effects should be *relevant* to employers, but they are probably least likely to be “observed”. While employers will look at applicants' employment histories, they are unlikely to know their past wages, let alone their pay relative to similar workers at the same firms.

Figure 2: Coverage of new hires' quality by age group and gender



Note: The figure shows the share of new hires with respective quality measures by age group and gender.

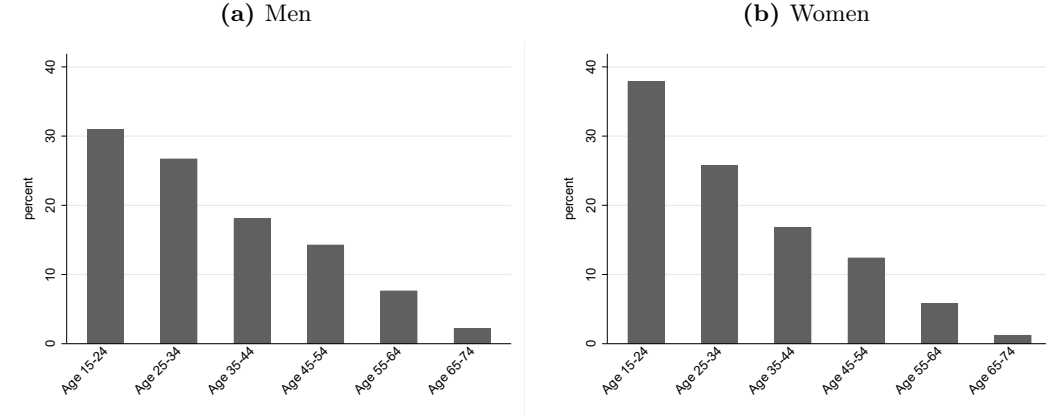
With respect to coverage, AKM dominates the other measures: as Figure 2 shows, we have worker effect estimates for more than 85% of new hires for both men and women in all age groups but the youngest, where coverage is around 50%.¹⁷ Poorer coverage among the youngest hires is a non-trivial drawback, as Figure 3 illustrates: more than a third of new hires fall into the 15-24 age bracket and this age gradient is even more pronounced for women. This is where GPA has an edge: while only available for the young birth cohorts in our data (born between 1973 and

¹⁶Indirectly, an individual's military rank and role may signal that they had a high cognitive and/or psychological test score (Grönqvist and Lindqvist, 2016), but this is less relevant at the lower hiring threshold we are interested in.

¹⁷Many workers aged 15-24 have not been observed in employment in at least two years prior to being hired, a pre-requisite for our estimating an AKM worker effect for them.

1982), its coverage of the 15-24 year-olds, the biggest age group among new hires, is above 60%. Military draft test scores have very good coverage for hires up to age 44, which includes the bulk of new hires; their main drawback is that COG and NONCOG are only available for men.

Figure 3: Age distribution of firm’s hires



Note: The figure shows the share of firms’ hires by age and gender.

In our reading, there is no unambiguous winner of this three-dimensional contest. Faced with a trade-off between quality and relevance/salience on the one hand and coverage on the other, we choose AKM worker effects as our main ability proxy and use the other measures to complement our analyses, in particular to address AKM’s weaknesses. For instance, the quality of AKM worker effects as a productivity proxy is likely poorer for younger workers, who have not been observed long enough and for whom wages have had less time to adjust to reflect their ability. It is therefore fortunate that GPA is available for young workers and both women and men.

3.6 Control group definition: upper size cutoff

Existing papers on Sweden’s 2001 converged on using firms of size 11-15 as a control group (Lindbeck et al., 2006; Olsson, 2009; von Below and Skogman Thoursie, 2010; Olsson, 2017; Bjuggren, 2018). They argue that this choice should result in a control group of firms with similar characteristics; and most papers present robustness analyses varying the control group definition. Still, our sense is that the choice remains ad-hoc.¹⁸

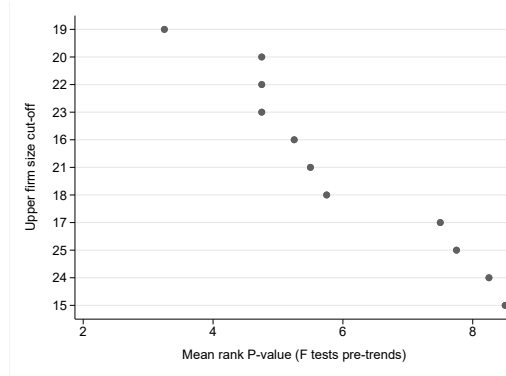
¹⁸We thank an anonymous referee for suggesting that we look for a way of “optimally” choosing a control group.

We attempt to formalize the choice of the control group by varying the upper firm size limit between 15 and 25 and picking the control group definition that results in the most parallel pre-trends when we estimate Equation (2). To identify the control group that gives the most similar pre-reform trend, we compare the F statistic of the joint significance of the year*TR interactions for the years 1993-1999 in the specification from Equation (2) with AKM, GPA, COG and NONCOG as outcomes. For this procedure, we always use all treatment group firms, i.e. firms with firm 2-10 employees. For the control group, we choose from 11 candidate groups with firms of size 11-15, 11-16, \dots , 11-25.¹⁹

For each outcome, we then rank the control group candidates by their average p value: the candidate with the highest p value (most similar pre trend) is ranked first and the candidate with the lowest p value (least similar pre trend) is ranked last (see Figure D2 in the Appendix). We aggregate across outcomes by forming the mean rank.

Figure 4 summarizes the result of this exercise. It shows control group candidate 11-19 in first place for the most similar pre trend across AKM, GPA, COG and NONCOG. We therefore choose firms of size 11-19 as our control group. In Section 5.4, we test the sensitivity of our findings to this “bandwidth” choice.

Figure 4: Ranking of potential control groups by joint significance of pre-reform DiD coefficients



Note: For each control group candidate of size 11-15, \dots , 11-25, this figure plots the mean rank across AKM, GPA, COG and NONCOG. Ranks are based on the mean p value across 50 random draws of an F test of joint significance of $\hat{\beta}_{1993}$, $\hat{\beta}_{1994}$, \dots , $\hat{\beta}_{1999}$ from Equation (2). The control group candidate with the lowest mean rank has the highest p value most often. The underlying p values for AKM, GPA, COG and NONCOG can be found in Figure D2 in the Appendix.

¹⁹If we use the full candidate control groups, F tests are more likely to be significant for the larger control groups, which afford more degrees of freedom. To avoid this we take 50 equal-sized random draws from each potential control group 11-15, 11-16, \dots , 11-25. The (constant) sample size of the random draw is smaller than each of the candidate control groups. We use the number of observations for firms of size 11-14. For each draw and outcome we perform an F test for joint significance of $\hat{\beta}_{1993}$, $\hat{\beta}_{1994}$, \dots , $\hat{\beta}_{1999}$. We record the F tests' average p value across the 50 draws for each control group candidate and outcome.

3.7 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 4.5 workers, compared to 14.1 workers for the average control group firm. Treated firms have existed for a bit shorter, employ older workers and slightly more women, pay a little less compared to control group firms, and employ workers with slightly less years of schooling. Panel B shows means for dummy variables. Despite differences for most variables are small in size, all differences are significantly different from zero. The differences of three variables stand out: treated firms are less likely to be manufacturing firms, are less likely to have expanded since the previous year, and more likely to have downsized relative to the previous year.

Table 2: Firm characteristics by treatment status

<i>A: Continuous characteristics</i>					
	Treated		Control		Difference
	Mean	SD	Mean	SD	Mean
Head count (rounded)	4.485	2.408	14.061	2.525	-9.576***
Firm age (years)	8.419	4.541	8.930	4.403	-0.511***
Mean worker age (years)	39.580	9.641	38.308	8.393	1.271***
Female worker share	0.407	0.297	0.369	0.266	0.038***
Mean worker years of schooling	11.274	1.436	11.340	1.221	-0.066***
Mean worker monthly wage (100 SEK 1980)	44.749	28.990	46.814	29.051	-2.065***
Estimated firm FE	0.009	1.014	-0.043	0.932	0.053***
Observations	114,227		21,013		135,240
<i>B: Binary characteristics</i>					
	Treated		Control		Difference
	Share Yes	Frequency	Share Yes	Frequency	Mean
Privately owned	0.991	113,167	0.983	20,660	0.008***
Manufacturing firm	0.098	11,140	0.146	3,064	-0.048***
Expanding firm	0.425	38,121	0.683	12,256	-0.258***
Downsizing firm	0.390	34,987	0.278	4,981	0.112***
Stockholm county	0.211	24,130	0.229	4,818	-0.018***
Malmo (Skåne county)	0.125	14,329	0.116	2,444	0.009***
Gothemburg (Vastra county)	0.167	19,047	0.174	3,647	-0.007**
Observations	114,227		21,013		135,240

Note: This table summarizes treatment and control firms' characteristics in 1999 for the main estimation sample, and shows the difference between treated and control firms. Panel A provides mean and standard deviation of continuous variables. Panel B gives means and frequencies for dummy variables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4 Results

4.1 Was pre-reform EPL binding?

Our empirical strategy exploits an exogenous change in employment protection legislation to learn about firms' hiring behaviour. Whether the relaxation of these firing constraints represents an effective treatment—that may have an effect on firm behaviour—hinges on whether the LIFO rules effectively constrained firms' firing in the first place.

To test how binding the LIFO rules were before the reform we briefly go to the individual level and study recently hired workers' relative probability of being fired. Recall that the 2001 reform stripped the second and third most recent hires of their employment protection by newly allowing the employer to pick from the three most recent hires when laying off a worker; before, they had to fire the most recent one. If pre-reform regulation constrained firing we would expect that in treated firms the involuntary separation probability of the second and third most recent hire increases relative to that of the most recent hire from 2001 onward.²⁰

To see if it did we restrict our sample to firms of size 10 and smaller, i.e., those firms for whom EPL was relaxed, and zoom in on the worker level. We then define all second and third most recent hires as the treated group and the most recent hires as control. Including firm fixed effects allows us to identify the effect of the reform on individual firing probabilities from within-firm changes in layoff decisions. Figure 5 plots the dynamic DiD estimates for this specification. It confirms that the second and third most recent hires became more likely to be laid off relative to the most recent hire from 2001 onward, suggesting that pre-reform EPL represented a binding constraint on firing (and thus potentially also on hiring).²¹

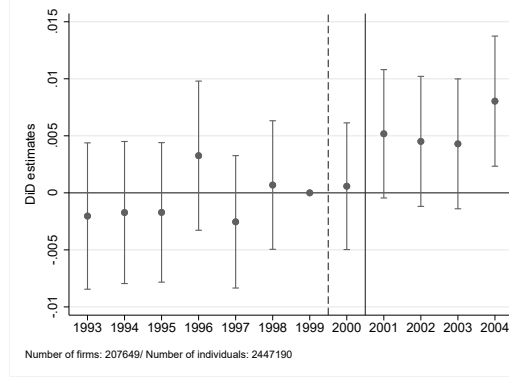
4.2 EPL effect on firms' minimum hire quality

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 $\min_{j,t}\{AK\hat{M}_i\}$, where $AK\hat{M}_i$ are estimated AKM person effects. DiD estimates for 1993-1998 provide a test of the parallel-trends assumption for the pre-reform period. Figure 6 displays these DiD estimates over time for specifications with and without firm fixed effects (both contain county-level macro controls interacted with the treatment dummy). With the exception

²⁰Our data does not contain information on whether an employee was laid off or left voluntarily. We therefore approximate involuntary separations as those who either have an employment gap of at least one month before their subsequent job or who have no gap but start their subsequent job at a lower wage.

²¹The results shown in Figure 5 are also consistent with the positive effect of the reform on involuntary separations in Figure D3 in the Appendix.

Figure 5: Involuntary separations for newly unprotected workers



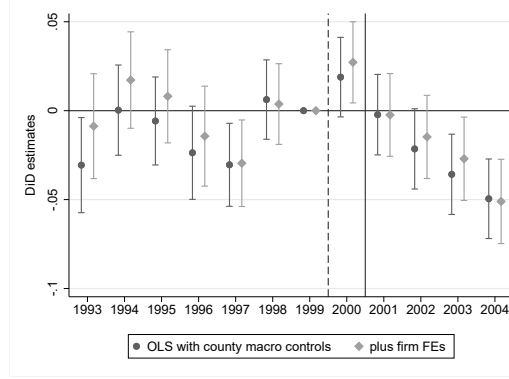
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on involuntary separations for the second and third last hire (“treated”), versus the last hire (“control”). Estimates are from a worker-level version of the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Involuntary separations are approximated with separations that are either followed by at least a month of non-employment or by a direct transition to a job that pays less. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

of 1997, 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 5.3, where we perform placebo tests. For the years 2002-2004 Figure 6 shows a negative effect of the EPL reform on minimum hire quality which gradually materializes, with an apparent trend break between 2000 and 2001. The event-study framework suggests no anticipation in 2000, the year that the LIFO reform was debated and passed in parliament. This is unsurprising given that the reform was written into law only in October.

We next compare the EPL reform’s estimated effect on minimum hire quality across different specifications of the estimating equation as well as across different worker ability measures. Table 3 presents estimated DiD coefficients for the effect on minimum hire AKM from three variations of the specification in Equation 1: Column (1) reports OLS without control variables other than year dummies; Column (2) reports OLS with county-level macro controls including interactions with the treatment; and Column (3) has firm fixed effects in addition to the county-level macro controls interacted with the treatment. The estimated effect is around 2% of a standard deviation in all three specifications, with the estimates statistically indistinguishable at conventional levels.²²

²²Table C6 shows corresponding results for other percentiles of the hiring distribution (P01, P05, P10, P15, P20). The results show that effects become smaller for higher percentiles. Note that for the P15- and P20-estimates for AKM worker effects, pre-reform trends are significantly different from zero.

Figure 6: EPL effect on minimum hire quality (AKM worker effects)



Note: Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Table 3: EPL effect on minimum hire quality (AKM)

	(1) OLS	(2) MACRO	(3) FE
DiD estimate: Treated*Post=1	-0.0162*** (0.0055)	-0.0231*** (0.0068)	-0.0216*** (0.0076)
Observations	911,981	876,832	876,832
Firms	287,436	279,459	279,459
Adjusted R ²	0.0175	0.0180	0.0126

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, with standard errors clustered at the firm level. Dependent variable is minimum hire quality. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Estimates are from different versions of the specification in Equation (1): OLS without controls (1), OLS with county-level macro controls interacted with the treatment (2), firm FEs with county-level macro controls interacted with the treatment (3). The year 2000 is excluded to rule out anticipation effects.

As discussed in Section 3.5, the quality as an ability proxy of AKM may generally be inferior to that of the other ability measures. More importantly, our analyses suggest that AKM estimates are of dubious quality for the youngest hires, aged 15-24. To compensate for this AKM shortcoming, we turn to GPA at age 15, military draft cognitive test scores (COG), and draft psychological test scores (NONCOG) as individual ability proxies. These have coverage issues of their own but offer a high-quality ability measure and good coverage for 15-24 year-old men and women in the case of GPA and for young men in the case of COG and NONCOG. The three panels of Table 4 present DiD estimates from the same specifications as Table 3 above, with Column (4) reproducing the effect on AKM for the sample of firms for which both an AKM and the respective alternative ability measure is observed for at least one new hire. The outcome variables are standardized to make them comparable across scores.

Table 4: EPL effect on different minimum hire quality measures

	(1) OLS	(2) MACRO	(3) FE	(4) FE
<i>Panel A: GPA at age 15</i>				
Quality measure:	GPA	GPA	GPA	AKM
DiD estimate: Treated*Post=1	-0.0241*** (0.0071)	-0.0337*** (0.0086)	-0.0678*** (0.0097)	-0.0497*** (0.0105)
Observations	514,469	494,273	494,273	448,903
Firms	215,947	209,895	209,895	198,365
Adjusted R ²	0.0103	0.0154	0.0089	0.0125
<i>Panel B: Cognitive test scores</i>				
Quality measure:	COG	COG	COG	AKM
DiD estimate: Treated*Post=1	0.0082 (0.0063)	0.0035 (0.0078)	-0.0195** (0.0085)	-0.0235*** (0.0091)
Observations	635,825	610,649	610,649	564,746
Firms	240,036	233,034	233,034	223,653
Adjusted R ²	0.0081	0.0174	0.0067	0.0126
<i>Panel C: Psychological test scores</i>				
Quality measure:	NONCOG	NONCOG	NONCOG	AKM
DiD estimate: Treated*Post=1	-0.0171*** (0.0063)	-0.0251*** (0.0078)	-0.0408*** (0.0091)	-0.0250*** (0.0092)
Observations	616,078	591,364	591,364	550,232
Firms	236,690	229,682	229,682	220,860
Adjusted R ²	0.0089	0.0104	0.0062	0.0124

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, with standard errors clustered at the firm level. In Columns (1) to (3), the dependent variable is one of three minimum hire quality measures, based on: GPA at age 15 (Panel A), military draft cognitive test scores (Panel B), military draft psychological test scores (Panel C). Estimates are from different versions of the specification in Equation (1): OLS without controls (1), OLS with county-level macro controls interacted with the treatment (2), firm FEs with county-level macro controls interacted with the treatment (3 and 4). As a benchmark, Column (4) reports the estimated effect on minimum hire AKM for the sample of firms used in the respective panel. The year 2000 is excluded to rule out anticipation effects.

The point estimates are more pronounced for GPA and somewhat bigger for NONCOG than for AKM but closer to zero for COG. This impression is reinforced by looking at the event-study graphs for the alternative ability measures: Figure D4 in the Appendix reproduces the event-study DiD graphs for GPA, COG and NONCOG. While the dynamics are similar for GPA and NONCOG as for AKM, the pattern is less clear for COG.

What may explain these differences? One possibility is observability to the employer. For young workers, who make up the bulk of new hires, it is likely that school grades are included in a written application. By contrast, employers *cannot* observe military draft test scores or AKM worker effects, though these may well be informative of skills relevant to employers.

This observability explanation is closely related to the finding of the employer learning literature that school grades are relatively more predictive of wages early in the career than AFQT scores, with the latter becoming more important as tenure grows (see, for example, Farber and Gibbons, 1996; Altonji and Pierret, 2001). That GPA more readily captures changes in hir-

ing behavior than our other ability measures that are unobserved by employers but potentially relevant to them would be consistent with this result.

As for the difference between our results using psychological and cognitive test scores, we do not know whether it is easier for firms to observe psychological aptitude than cognitive ability. However, we find it plausible that, for many jobs at firms' lower ability threshold for hiring, psychological attributes such as communication skills, emotional stability and dependability may be more important than cognitive ability.²³

A second explanation why the effect on GPA is stronger than for the other ability measures is that the selectiveness of firms' hiring went down more for young people and responded less strongly for older new hires. This is supported by the fact that the EPL reform's estimated effect on minimum hire AKM is more pronounced when we condition on the sample of firms with a non-missing minimum GPA measure, i.e., those firms with young hires. Why would firms reduce their hiring standards more for younger people? One potential reason is that for older hires, firms rely more on alternative productivity signals, which we do not observe and which are less strongly correlated with our ability proxies.

Finally, differences in quality across productivity proxies may explain some of the differences. In particular, AKM worker effect estimates are likely much noisier for young workers, who have only been observed for a short period. A resulting attenuation bias may be one reason why our estimated effect on minimum AKM is so much smaller than that for minimum GPA.

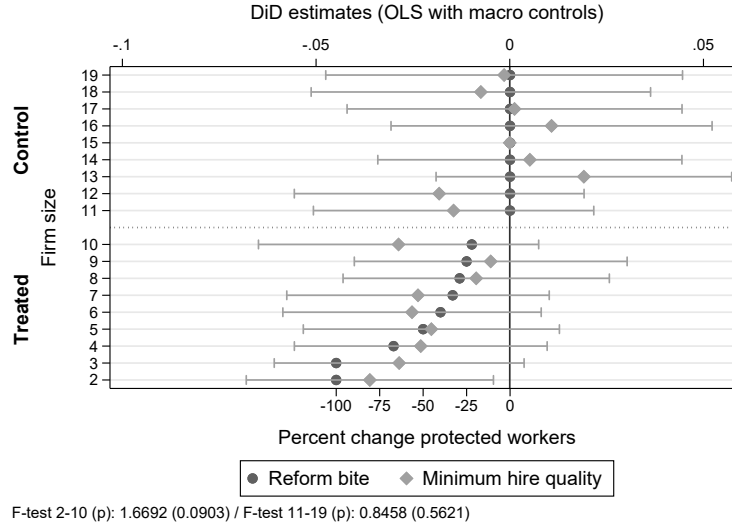
4.3 Effect heterogeneity by reform bite

The EPL reform did not affect all treated firms equally. As we discuss 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 dark grey dots in Figure 7 illustrate this variation in reform bite. The figure also shows results from a DiD specification that exploits this heterogeneity. It builds on the specification in Equation (1) but splits up the overall DiD term into separate DiD terms for each firm size bin. The specification leaves out firm fixed effects; including them would mean that firm size-specific DiD estimates are identified only from firms that remain in the exact same firm size bin, which make up only a small and very selective sub-set of the treated and control groups. The results without firm fixed effects, shown in light grey diamonds, are still noisy, particularly further up the firm size distribution

²³Edin et al. (2021) show that, conditional on the level of education, the returns to the psychological test score is higher than the returns to the cognitive test score, suggesting that firms are able to identify individuals performing well on the psychological test score and reward them accordingly.

where there are fewer observations. Still, Figure 7 strengthens 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. Consistent with our finding for different ability measures above, the pattern of heterogeneity is similar for GPA and psychological test scores as for AKM and hardly discernible for COG (see Figure D5 in the Appendix).

Figure 7: EPL effect heterogeneity by firm size



Note: The light gray diamonds show firm size-specific DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS specification (without firm fixed effects) with county-level macro controls interacted with the treatment and year dummies. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Horizontal bars denote 95% confidence intervals. Standard errors are clustered at the firm level. The dark gray dots illustrate the bite of the reform in terms of the percentage change of workers protected by the seniority rule. The reference firm size category of 15 is chosen so that it roughly coincides with the mean of estimated firm size-specific DiD coefficients for firms in the control group.

5 Robustness

5.1 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.²⁴ Effective in 2001, the wording of these requirements was made more concrete. Importantly, however, it did not fundamentally change their content (SFS, 1991, 2000).²⁵ 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 which is defined as firms with 11 or more workers) may have felt more obliged to promote gender equality in their hiring choices. To the extent that firms discriminated against women in hiring before the GEA amendment, we expect that those firms affected by the GEA amendment would have responded by reducing this discrimination, resulting in a relaxation of hiring standards for women relative to before. As firms affected by the amendment broadly coincide with our control firms, in difference-in-difference terms, this is equivalent to a tightening of the female 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 whether this is the case we separately estimate the effect of the EPL reform on minimum female and male hire quality. The point estimates in Columns (1) and (2) of Table 5 do not provide conclusive evidence—they are not statistically distinguishable at conventional levels. However, the point estimate for women is less pronounced than that for men. This would be consistent with the GEA slightly attenuating our estimate of the EPL’s effect on minimum hire quality (through those firms where a woman is the least productive new hire).

What is the effect of the GEA amendment and the EPL reform on women’s employment prospects at our treated firms? Again, to the extent that firms discriminated against women before, the GEA amendment may have increased their hire share in affected (our control) firms. By contrast, we do not have a prior on the effect of the EPL reform on women’s chances of being hired (by our treated firms). This combination suggests that if the GEA amendment increased women’s hiring chances, it would negatively bias our estimate of the EPL reform’s effect on the share of women hired. Column (3) of Table 5 reports the result from estimating Equation (1)

²⁴The 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.

²⁵The 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).

Table 5: EPL effect on minimum hire quality by gender and on female hire share

	(1)	(2)	(3)
Worker sub-group	Minimum hire quality of	Minimum hire quality of	Share
Sample of firms	Female hires	Male hires	Female hires
	with female hires	with male hires	with any hires
DiD estimate: Treated*Post=1	-0.0069	-0.0278***	0.0020
	(0.0113)	(0.0100)	(0.0021)
Observations	495,997	668,900	979,421
Firms	205,231	244,294	292,182
Adjusted R ²	0.0053	0.0097	0.0009

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable: female hires' minimum AKM (1), male hires' minimum AKM (2), female hire share (3). AKM worker effects are estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Estimates are from Equation (1) specifications with firm fixed effects, county-level macro controls interacted with treatment and year dummies. The year 2000 is excluded to rule out anticipation effects.

with the female hire share as an outcome. We find a precisely estimated zero that rules out a positive effect above 0.65 percentage points and a negative effect below -0.19 percentage points with 95% confidence. As this is not our focus we content ourselves with stressing that, if it has any effect, the GEA should push our estimate toward the lower bound, i.e., the EPL effect on women's hire share may in fact be slightly bigger than the precise zero we estimate.

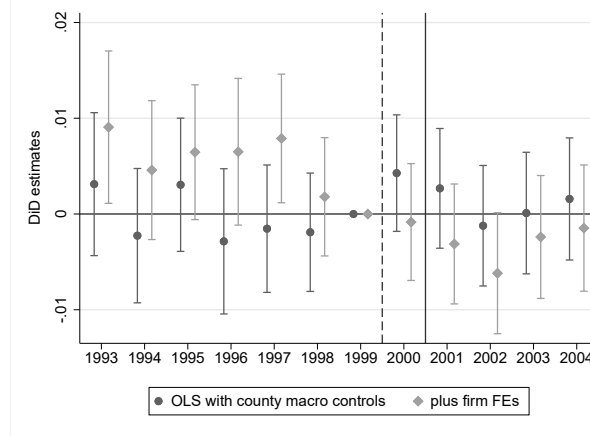
5.2 Selective worker ability measurement

We do not observe worker ability for all new hires. In 1999, at the average firm in our estimation sample we have AKM for 75.7% of new hires (see Section 3.5 for more information on the share of new hires for whom we observe the different ability measures). Our DiD setting ensures that incomplete ability measurement is only problematic if the share of new hires whose ability we can measure is affected by the reform. If the reform boosted (reduced) the share of hires for whom we observe AKM, this may mechanically decrease (increase) minimum hire quality even if firms' selectiveness remains unchanged.²⁶ To test for this in our sample, we estimate Equation (2) with the share of each firm's new hires for whom we observe AKM as an outcome. Figure 8 plots the yearly DiD coefficient estimates from this exercise. There appears to be a slight downward trend throughout the analysis period, but no trend break around the introduction of the reform. To drive our results, the EPL reform would have needed to increase the share of hires for whom we

²⁶A similar, more subtle selective measurement problem may arise if the reform increased the share of women hired *and* if AKM were systematically underestimated for women. In this case, the share of underestimated ability would have increased and that might bias minimum hire quality downward. As discussed in Section 5.1 and shown in Column (1) of Table 5, there was no increase in women's hire share in treated relative to control firms, allaying this concern.

observe an AKM worker effect in the treated firms; the evidence here suggests the reform did not affect AKM availability for new hires.

Figure 8: Share of hires with an AKM worker effect



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the share of new hires for whom we observe an estimated AKM worker effect. Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

5.3 Non-parallel counter-factual trends by 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.²⁷ To this end we define as placebo treated and placebo control those firms that are 10, 20, 30, 40, 50 and 60 workers bigger than our actual treated and untreated firms. This gives us placebo firm size cut-offs at 20, 30, 40, 50, 60 and 70 workers. Table 6 gives the results from estimating Equation (1) on these placebo treatments with AKM worker effects as an ability proxy.²⁸ While the placebo DiD estimate for the 20-worker cut-off is similar to our estimate for the actual EPL effect, the other placebo estimates are not statistically significant. We are

²⁷The interpretation of this exercise as a placebo test would be complicated by significant spill-over effects of the reform on the quality of labor supplied to non-treated firms. However, as we discuss in Section 3.2 above, the likelihood of this is small.

²⁸Note that the standard errors for the placebo effect estimates are around 2 to 6 times larger than those of the actual effect estimate. This is what we would expect, given that the number of firms (and hence the degrees of freedom) gets smaller as we move the placebo firm size threshold up.

inclined to ascribe this to random variation: first, only one out of six placebo tests for AKM gives a significant result and second, when we check the other ability measures, only 2 out of 18 come up significant and they do so at different placebo cut-offs. As Table C7 in the Appendix shows, there is one significantly negative placebo estimate for GPA at 30, one significantly negative one for COG at 60 and no significant placebo estimates for NONCOG. The signs of point estimates vary across placebo thresholds for all minimum hire quality measures. Our overall interpretation of this evidence is that trends in minimum hire quality may not always have been parallel but did not systematically diverge between groups of firms of different (but adjacent) size around 2001.

Table 6: Placebo thresholds

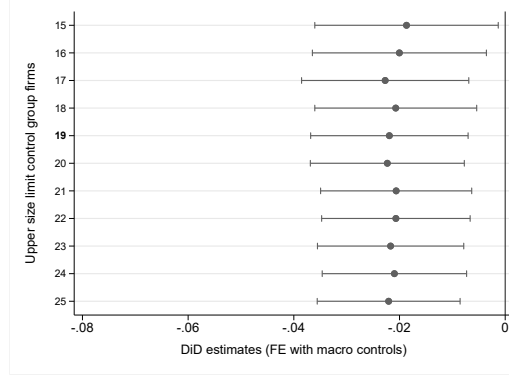
	(1) 12-20 vs 21-29	(2) 22-30 vs 31-39	(3) 32-40 vs 41-49	(4) 42-50 vs 51-59	(5) 52-60 vs 61-69	(6) 62-70 vs 71-79
DiD estimate: Treated*Post=1	-0.0311** (0.0130)	-0.0239 (0.0192)	0.0254 (0.0246)	0.0119 (0.0299)	0.0443 (0.0365)	-0.0629 (0.0413)
Observations	237,558	102,847	59,826	39,481	27,767	20,898
Firms	77,507	37,244	23,258	16,218	12,095	9,463
Adjusted R ²	0.0166	0.0214	0.0197	0.0215	0.0206	0.0167

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable is minimum hire AKM, with AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Placebo treated and control groups are defined around 6 placebo firm size cut-offs at 20, 30, 40, 50, 60 and 70 workers. Estimates are from Equation (1) specifications with firm fixed effects, county-level macro controls interacted with treatment and year dummies. The year 2000 is excluded to rule out anticipation effects.

5.4 Bandwidth test

We conclude this section by testing the sensitivity of our results to the choice of control group we made in Section 3.6: firms of size 11-19. To this end we check how the effect of the EPL reform on minimum hire quality varies as we vary the upper cut-off of the control group from 15-25. That is, we estimate Equation (1) with 11 different control groups: firms of size 11-15, 11-16, ..., 11-25. As Figure 9 shows, the choice does not make a big difference. There are no statistically significant differences between the effect estimates and the point estimates hover slightly above 2% of a standard deviation.

Figure 9: EPL effect on minimum hire quality with different control groups



Note: This figure shows estimated DiD coefficients for the effect of the EPL reform on minimum hire quality when varying the firm size categories that are included in the control group. Estimates are for β from Equation (1) with firm fixed effects, county-level macro controls interacted with treatment and year dummies. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . The year 2000 is excluded to rule out anticipation effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

6 Discussion

6.1 Demand vs. supply

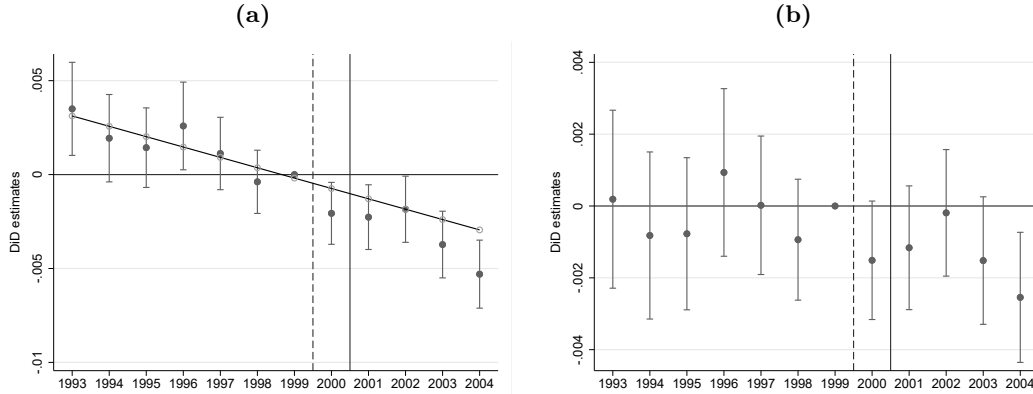
We have been using an equilibrium outcome (observed hires) to infer something about firms' (qualitative) labor demand. Implicit in this analysis is the assumption that labor supply remained constant, i.e., that workers' self-selection into firms was not systematically affected by the LIFO reform. In this subsection we first try to provide direct evidence of a change in labor demand and then discuss the plausibility of the assumption that labor supply quality remained constant. Lacking data on applicants who were not hired, our evidence on changes in the quality of labor supply remains suggestive.

We start by looking at firms' network-based hiring as a potential indicator for firms' demand for a certain quality of labor. 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 hire less selectively, it is less important for them to obtain reliable signals of new hires' productivity, e.g., through employee referrals. We would therefore interpret a decrease in the intensity of network-based hiring at firms affected by the reform as confirmation that the drop in minimum hire quality is a demand-side phenomenon.²⁹

²⁹We 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 with incumbents by the number of incumbents, giving us the share of incumbents the new hire is linked to.

Figure 10 presents the results of estimating Equation (2), with the share of incumbents new hires have past links with (averaged at the firm-year level) as an outcome (the treatment group mean in 1999 is 0.0375). As Panel (a) shows, the pre-reform trends are not parallel, complicating interpretation. Given their regularity, however, we estimate a linear trend for the pre-reform difference between treated and control firms and extrapolate it to the post-reform period. This allows us to compare treated firms' actual network-based hiring to what we would expect absent the reform if the linear pre trend had continued. We plot the deviation of treated firms' network-based hiring from the extrapolated pre trend in Panel (b). These de-trended effect estimates are insignificant and close to zero. While the point estimates go in the expected direction (they show a slight drop around the reform), they provide no clear evidence in favor of our labor demand interpretation. However, the results on network-based hiring may help us better understand the mechanisms through which a reduction of firm selectiveness may have operated, see Section 6.2 below.

Figure 10: EPL effect on network-based hiring



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on minimum hire quality. Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Network-based hiring is measured by new hires' average share of incumbents with whom they previously worked at another firm. The solid line in Panel (a) shows a linear trend for the pre-reform difference between treated and control firms, extrapolated to the post-reform period. In Panel (b), the dependent variable is de-trended using the linear pre-reform trend. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

We next discuss the plausibility of and potential threats to the assumption that the quality of labor supply remained roughly constant. Note that theoretically the reform's implication for new hires is ambiguous: in the short run, it made treated firms more attractive as the lowest-tenure worker needed 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.

One potential supply-side driver of our main result is that the reform made workers with particularly low productivity search harder for jobs at treated firms. As discussed above, treated firms might have become more attractive for them as long as they are the most recent hire, but less attractive while they are the second or third most recent. Alternatively, these workers might have anticipated that treated firms would hire more (and less selectively) and seen an improved opportunity to get hired at all. We cannot rule out that this contributed to the reduction in minimum hire quality we find; however, it would not have led to hires being made if employers had not also adjusted their hiring thresholds.

A related supply-side story is that the reform made treated firms *less* attractive employers, causing better workers to self-select away from them. To explore this possibility we look at a subset of employment-to-employment transitions. If voluntary job changers from well-paying firms became less likely to join the firms affected by the reform then this would suggest that the reform made affected firms less attractive. We therefore use as an outcome the share of new hires likely to be voluntary job changers³⁰ coming directly from a high-wage firm.³¹ As Figure D6 in the Appendix shows, the LIFO reform has no significant effect on voluntary employment-to-employment transitions from high-wage firms—certainly not a negative one. We view this as suggestive evidence that the reform did not make treated firms less attractive to better workers.

6.2 Mechanisms

The evidence presented so far shows that Sweden’s 2001 relaxation of EPL reduced minimum hire quality and suggests that this reflects a change in labor demand. This subsection explores two possible ways in which an employer response to reduced firing costs could have brought about that drop in minimum hire quality.

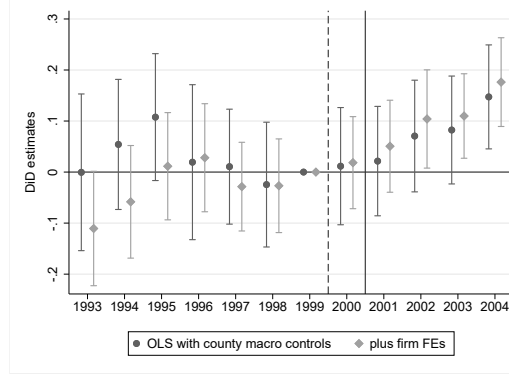
Hiring less selectively and hiring more The first is Pries and Rogerson (2005)’s prediction that as the cost of hiring the marginal worker falls, firms lower their hiring threshold. This should result in the least productive hires getting worse (our main result) *and* more workers being hired. Indeed, von Below and Skogman Thoursie (2010) find that the 2001 EPL reform

³⁰We use the analogous approximation of voluntary job changers as in Section 4.1: individuals without an employment gap who earn the same wage or more in the new job.

³¹We define as high-wage firms those with an AKM firm fixed effect estimate (estimated from the years $t - 7$ to $t - 1$ for any observation year t) above the median, irrespective of size.

increased worker inflows; as Figure 11 shows, we also find a clear positive reform effect on firms' hiring rate.

Figure 11: EPL effect on hiring rate



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the hiring rate (number of hires divided by previous-year average headcount). Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Vertical bars denote 95% confidence intervals.

We go one step further in trying to establish that the reduction in minimum hire quality and the increase in hiring are indeed linked, as in Pries and Rogerson (2005). Consider a thought experiment in which, instead of consciously picking them, firms randomly hire workers. If the Pries and Rogerson (2005) mechanism were not at play and the increase in the hiring rate were an independent effect of the reform, then stripping away firms' active choice of new hires should wipe out the reform's effect on minimum hire quality. By contrast, if an increase in hiring and the reduction of hiring thresholds necessitate and imply each other as in Pries and Rogerson (2005), then with randomly drawn hires an effect of the reform on minimum hire quality should persist.

We implement this thought experiment in a simulation that preserves the number of hires each firm makes in a year but takes away its choice of whom to hire. That is, we simulate counterfactual hiring outcomes by replacing firms' actual hires with randomly drawn individuals from the pre-reform pool of hires in the respective firm size category. Consider the following example as an illustration of our simulation approach: say a firm of size 7 made a hire in 1997 and another hire in 2002. From the pool of all individuals hired into firms of size 7 between 1993 and 1999 (the pre-reform period), we randomly draw a person to replace the 1997 hire with; we

also randomly draw a person to replace the 2002 hire with (potentially the same individual—our draws are with replacement).

Figure 12 presents the results of repeating our simulation exercise 200 times and estimating the effect of the reform for each counterfactual allocation of individuals to firms.³² The graph compares the reform’s estimated dynamic effect on the distribution of 200 simulated minimum hire qualities to the estimated effect on actual minimum hire quality. The whiskers denote the 2.5th and 97.5th percentiles of the simulated effect estimates (by analogy to a 95% confidence interval) and the boxes mark the interquartile range (split by the median). The distribution of simulated effects tracks the actual effect estimates closely—the median simulated effect falls within the 95% confidence interval of the actual effect in all years. That is, when firms are prevented from actively choosing workers, the effect on minimum hire quality persists.

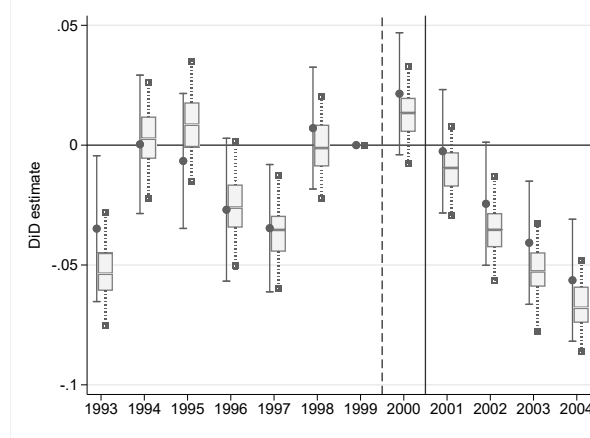
Importantly, we do not interpret the simulation results as quantifying this mechanism’s precise contribution to the overall effect; as it counterfactually and unrealistically assumes random hiring, our simulation is explicitly not able to tell us how much of the effect is due to the Pries and Rogerson (2005) mechanism, nor can it be used to rule out other channels. The simulation does, however, provide clear qualitative evidence that the hiring rate and hiring thresholds are closely linked and serves as confirmation that the Pries and Rogerson (2005) mechanism is a major driver of the effect on minimum hire quality that we find.

Hiring less selectively for a given number of hires A second possible mechanism is that even for a given number of hires, firms’ hiring starts to include worse workers. Employers may hire less selectively because looser EPL makes it preferable to spend fewer resources on pre-hire screening and instead intensify on-the-job screening of recent hires.³³ We do not have direct evidence of a reduction of ex-ante screening; the results on network-based hiring in Section 6.1 are insignificant (though the point estimates would be consistent with a small reduction). There are several other dimensions of pre-hire screening though, which we cannot measure but along which firms may have reduced their screening effort.

³²Note that we estimate Equation (2) without firm FEs here; the fact that firms’ hires are random draws should preclude systematic firm heterogeneity in hire quality.

³³Lazear (1995) proposes an alternative mechanism for why firms may be willing to hire worse workers for a given number of hires: lower firing costs make very bad matches less threatening. As a consequence, employers may become keener to hire risky workers whose (ex-ante) unknown ability is lower in expectation but has a higher variance—i.e., who are more likely to turn out to be either a disaster or a star. While we do not have a good way of identifying ex-post disasters and stars, Lazear’s risky hires mechanism is also consistent with less selective hiring for a given number of hires.

Figure 12: EPL effect and random hiring simulation



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on 200 simulated minimum hire qualities (box-and-whisker plot) and on actual minimum hire quality (black dots with vertical bars). Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . (For hires in t whom the simulation counterfactually replaced with a hire from t' , AKM worker effects estimated for the latter individual in years $t' - 7$ to $t' - 1$ are used.) Estimates are from the specification in Equation (2) with county-level macro controls interacted with the treatment and year dummies (no firm FEs). For the box-and-whisker plot, there are no confidence intervals. Instead, the whiskers denote the interval containing 95% of the 200 estimated EPL effects on simulated hire quality. The box spans the interquartile range and is split by the median. For the black dots, vertical bars denote 95% confidence intervals (with standard errors clustered at the firm level).

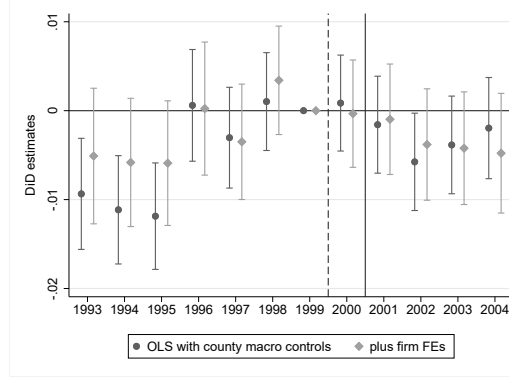
We also have ways of detecting changes in ex-post screening: our finding of an increased lay-off probability for newly unprotected workers in Section 4.1 provides a first indication that firms may have increased their reliance on it. A natural next step is to look at the effect of the reform on the selectiveness of firing. We do so by asking whether the within-firm ability rank of workers fired after the reform (normalized by the headcount) went up in treated firms, i.e., whether treated firms selected less productive workers for lay-offs. Figure 13 presents the results. It provides no evidence that the quality of laid-off workers changed systematically around the reform.

In summary, we have considered two potential mechanisms driving our overall effect: (i) the lowering of the hiring threshold associated with an increase in the number of hires (Pries and Rogerson, 2005) and (ii) a hiring-rate neutral reduction of selectiveness. The evidence presented in this subsection does not conclusively rule out the second but it suggests that the first is a major driver.

6.3 Implications for disadvantaged workers

We have shown that the 2001 EPL reform reduced minimum hire quality at affected firms (Sections 4 and 5) and have provided evidence supporting our interpretation that this is primarily

Figure 13: EPL effect on selectiveness of firing



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the average normalized ability rank of involuntarily separating workers. Normalized rank 1/headcount indicates the highest AKM, so a positive effect implies a deterioration in the productivity of separating workers. Ability for any individual observed in t is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$. Yearly DiD estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

driven by a change in firm behavior (Section 6.1). It appears plausible that this reduction of firms hiring thresholds benefited less productive workers, who might have found it easier to get hired. In this section, we attempt to test this hypothesis by looking at the effects of the EPL reform on the hiring prospects of disadvantaged labor market participants.

Least productive hires' productivity We start by switching from the firm to the worker level to ask whether the least productive new hires got worse in response to the reform. This would constitute a positive welfare effect in the sense that, thanks to the reform, less productive individuals found their way into treatment-firm employment than would otherwise have been the case.

To test whether productivity near the bottom of the overall hire quality distribution diverged between workers hired by treated and control firms we estimate Appendix Equation (5), an unconditional quantile regression version of Equation (1). We take all individuals hired by either treated or control firms in our analysis period (1993-2004) and look at unconditional quantile regression DiD estimates for the 1st, 5th, 10th, 15th and 20th percentiles of AKM worker effects. Negative coefficient estimates would tell us that—from the perspective of workers rather than firms—the reform lowered the ability of low-ability workers hired by treated firms and thus helped some of the least productive workers into employment.³⁴ Panel A of Table 7 suggests that this

³⁴We use the approach developed by Powell (2020). See Section B in the Appendix for a brief methodological discussion of (unconditional) quantile regression and details of the specification we estimate.

is not the case: while point estimates are negative for all quantiles considered, the coefficients are far from statistically significant. Estimated unconditional quantile DiD estimates are also insignificant for our other ability proxies, see Table C8 in the Appendix.

That the reform did not—from the worker perspective—allow “worse” workers to become treated firms’ low-ability hires stands in apparent contrast to our main result of firms’ lowered hiring thresholds. In our reading, this is not a contradiction but instead implies that the reform-induced reduction in minimum hire quality at the firm level is not driven by the lowest-productivity hires overall but is spread across new hires who *are* less productive than their firms’ previous hires but *are not* necessarily among the most unproductive compared to all other firms’ hires.³⁵

One can think of several ways of characterizing disadvantaged workers other than using ability proxies. One such alternative characteristic associated with disadvantage in the labor market is (young) age; fortunately, age also lends itself to a quantile-regression approach as it tends to be a continuous or ordered discrete variable. To find out whether the reform caused the youngest hires into treated firms to get younger, we repeat the unconditional quantile regression analysis from above and use age as an outcome. Panel B of Table 7 reports the results. The point estimates are again all negative; for the 1st and 20th percentile, they are also statistically significant. This points to some positive effect of the reform on the employment prospects of very young workers, which we will seek to verify in the next paragraph.³⁶

Disadvantaged worker groups We return to the firm level but continue to look at disadvantaged labor market participants as defined by characteristics that either do not measure ability (such as demographics) or are indirect ability proxies (such as unemployment).

First, we follow up on the quantile-regression evidence suggesting that the EPL reform improved employment prospects for the youngest job seekers. We focus on individuals aged 15-24, the age group used by the ILO’s and the OECD’s definitions of youth unemployment. Figure 14 reports yearly DiD estimates for the effect of the EPL reform on the share of hires aged 15-24. The OLS estimates are difficult to interpret due to non-parallel (and non-linear) pre-reform trends (though they do point to a break in the negative trend around the reform). However, the estimates with firm FEs, which net out sample composition effects, show parallel pre-reform

³⁵Our results in Sections 4.3 suggest that it is particularly hires into smaller firms that drive the firm-level effect.

³⁶Note that we have truncated the age of new hires below 15 and above 74, likely making these lower-bound estimates.

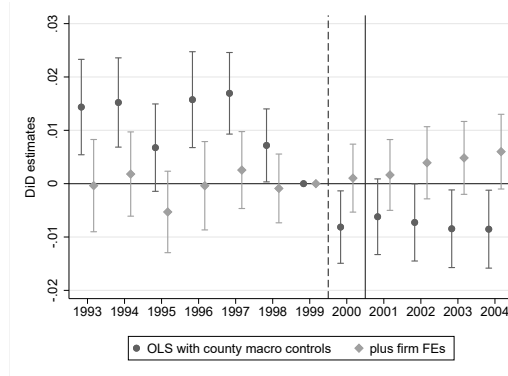
Table 7: Worker-level EPL effect at the bottom of the outcome distribution: quantile regression estimates

	(1)	(2)	(3)	(4)	(5)
Unconditional quantile	P01	P05	P10	P15	P20
<i>Panel A: AKM worker effects</i>					
DiD estimate: Treated*Post=1	-0.0886 (10.0123)	-0.0388 (0.4749)	-0.0220 (3.0680)	-0.0275 (0.4244)	-0.0212 (176.3391)
Observations	2915074	2915074	2915074	2915074	2915074
<i>Panel B: Age</i>					
DiD estimate: Treated*Post=1	-1.0000*** (0.2255)	-3.0000 (10.1218)	-0.6736 (0.5586)	-0.6122 (10.2311)	-1.9074* (1.0889)
Observations	3687092	3687092	3687092	3687092	3687092

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variable is estimated AKM worker effects (Panel A) or age in years (Panel B). Estimates are from Equation (5), an unconditional quantile-regression version (following Powell (2020)) of the specification in Equation (1), see Appendix Section B for details. The year 2000 is excluded to rule out anticipation effects.

trends. Consistent with the individual-level results, they suggest a small but positive effect on the youth hire share.

Figure 14: EPL effect on the share of hires aged 15-24



Note: Estimates are from the specification in Equation (2) with county-level macro controls interacted with the treatment and year dummies and, optionally, with firm fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

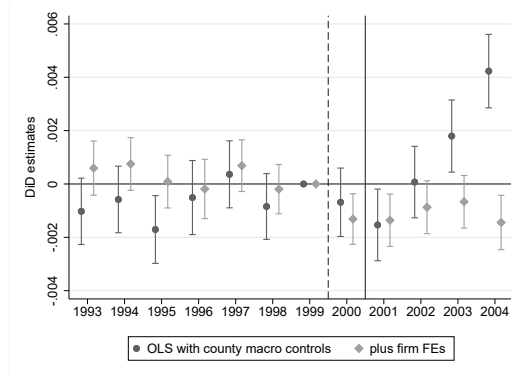
Second, we test whether the EPL reform benefited foreign-born workers. We again estimate Equation (2), now using as an outcome the share of hires born outside the Nordic countries or the EU15.³⁷ This narrow definition of foreign-born reflects the view that individuals born in countries that joined the EU later or in non-EU member states are more likely to be disadvantaged in the labor market than, say, other Scandinavian or German workers. Figure 15 reports the results.

³⁷The EU15 include Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Sweden, Spain and the United Kingdom.

The specifications with firm FEs provide no evidence that treated firms increased the share of foreign-born hires. Note, however, that there is an increase in the share of foreign-born workers hired by treated firms in the specification without firm FEs.

Given our findings of no endogenous selection into treatment in Section 3.3 and given the similarity of FE and non-FE estimates in the rest of our analyses, we interpret this disparity between the FE and OLS estimates as driven by a change in the composition of firms. That is, the reform seems to have increased market entry (or reduced exit) of firms that are more likely to hire foreign-born workers. From the worker perspective, an increased hire share of their group is good news regardless of whether it is driven by a change in the behaviour of incumbent firms or by a change in the composition of hiring firms (though this reasoning ignores potential differences in job quality). We therefore read the evidence here as pointing to some improvement in the employment prospects of foreign-born workers.

Figure 15: EPL effect on the share of foreign-born hires



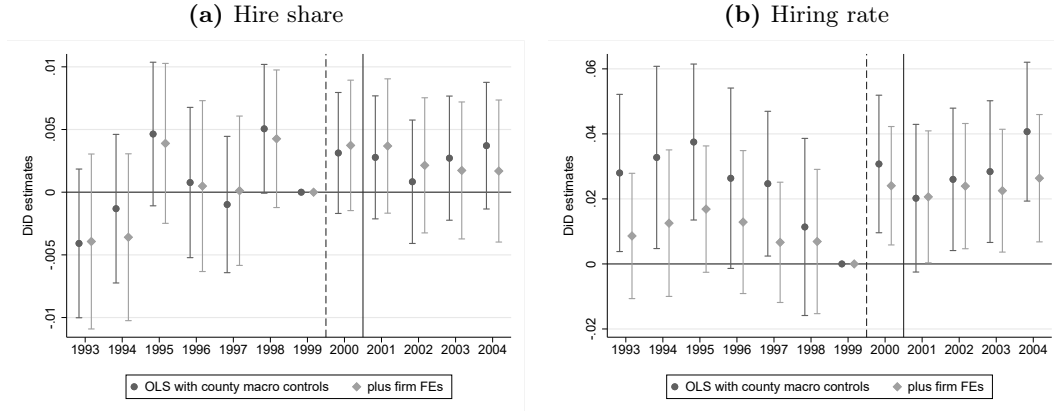
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the share of foreign-born workers among new hires. Estimates are from the specification in Equation (2) with county-level macro controls interacted with the treatment and year dummies and, optionally, with firm fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Third and finally, we check for evidence that the EPL reform improved the situation of long-term unemployed individuals. Bjuggren and Skedinger (2018) find that the reform increased the share of workers hired from active labor market programs and unemployment using information on whether an individual was registered as ALMP participant or as unemployed at some point in a given year. We try to leverage our detailed employment spell data by defining those as long-term non-employed who are *not* visible in our data for at least a year. Using this definition, we estimate the reform's effect on the hire share of long-term non-employed workers. As Panel

(a) of Figure 16 shows, the hire share of long-term non-employed remained relatively constant around the reform.

Note, however, that with increased hiring rates in treated firms, the long-term non-employed hire share need not go up for the hiring prospects of long-term non-employed individuals to improve. In fact, a constant hire share implies that the hiring rate of long-term non-employed workers might have gone up—and this is indeed what we find, as Panel (b) of Figure 16 illustrates.

Figure 16: EPL effect on hiring of long-term non-employed



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on (a) the share of hires coming from long-term non-employment (un-interrupted 12 months or more); or (b) the hiring rate of long-term non-employed workers (number of hires coming from at least 12 months of non-employment divided by the firm's previous-year headcount). Estimates are from the specification in Equation (2) with county-level macro controls interacted with the treatment and year dummies and, optionally, with firm fixed effects. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Taken together, the evidence in this section points to some welfare gains from the EPL reform in the form of improved hiring prospects for disadvantaged workers. These accrue to very young workers, foreign-born workers and long-term non-employed workers. By contrast, with our ability proxies, we do not find that—viewed from the bottom of new hires' ability distribution—the reform led to observably less productive workers being hired by treated firms.

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 first show that firms reacted to the new legislation and that previously protected hires became more likely to be fired. We then provide causal evidence that the EPL reform lowers minimum hire quality at affected firms by 2.2% of a standard deviation. The 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 discuss our main findings along three dimensions. We first explore demand-side explanations for our findings. Building on the idea that new hires’ past links with incumbents can be used as a proxy for screening intensity, we analyze whether referral-based hiring is affected by the reform. Finding inconclusive results, we then turn to supply-side explanations, testing whether the reform made treated firms less attractive employers by analyzing voluntary job-to-job transitions. We find suggestive evidence that firms did not become less attractive for workers.

Second, we explore potential mechanisms, namely the channel proposed by Pries and Rogerson (2005), i.e. that firms lower their hiring threshold when hiring more workers, and an alternative channel that treated firms became less selective as firing costs decrease and thereby effectively shift their screening efforts from pre-hiring screening to on-the-job screening. While it is not possible to discriminate between the two channels, our results show that the reform both lowered hiring thresholds *and* increased hiring rates, i.e. as in Pries and Rogerson (2005).

Third, we study the welfare implications of the reform. Using worker-level unconditional quantile regressions to test whether the quality and the age of the least productive hires was affected by the reform, and, using our firm-level approach, to test whether the share of young individuals, foreign-born workers and long-term non-employed individuals was affected by the reform. Our results show that young individuals, foreign born individuals and long-term non-employed individuals benefitted from the reform. This highlights a possible welfare effect of the reform: getting disadvantaged groups into employment can help to overcome market failures due to lack of information of workers’ abilities (Pallais, 2014).

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 and introduced a loosening of the LIFO rules for 2022. 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

A AKM person effects

A.1 Estimating AKM person effects

To estimate time-invariant individual productivity we use individual-level spell data on employment and monthly wages (unadjusted for working time) from JOBB.³⁸ To have sufficient AKM worker effect coverage of new hires for both the period before the introduction of the reform and after, we estimate the AKM model in rolling time periods, i.e. for individuals hired in t , we estimate their AKM worker effect using the employment history for the preceding 7 years (from $t - 7$ to $t - 1$). In our AKM estimation we include all employment information for individuals aged 18-65 for the years starting in 1986 (the beginning of records) through 2004.

We deflate wages using the CPI and winsorize at 0.5% and 99.5% of the annual real monthly wage distribution. We drop singletons, i.e., individuals that are only observed once in the respective 7-year estimation window. We residualize wages by age, gender and education using the following specification:

$$\begin{aligned} \ln(w_{it}) = & \alpha_i + \beta_1 \text{male}_i + \beta_2 \text{age}_{it} + \beta_3 \text{educ}_{it} \\ & + \beta_4 \text{male}_i * \text{age}_{it} + \beta_5 \text{male}_i * \text{educ}_{it} \\ & + \beta_6 \text{age}_{it} * \text{education}_{it} + \beta_7 \text{male}_i * \text{age}_{it} * \text{educ}_{it} + \epsilon, \end{aligned} \tag{3}$$

where gender is a dummy, age is in years and education is a categorical variable with 8 categories, one of which is missing education information.³⁹ Information on education is available only from 1990; for earlier years, we impute it from the first year it is available.⁴⁰

³⁸Unlike the Wage Statistics Survey (WSS), the JOBB register does not contain information on working hours (or at least full-time status). Using JOBB rather than WSS to estimate an AKM model comes at the cost of lowering the predictive power of AKM worker effects for, e.g., cognitive test scores (Butschek and Sauermann, 2019b). However, as WSS is available only for the employees of a changing sub-set of firms, using JOBB gives us an AKM worker effect estimate for a much larger share of new hires in our sample.

³⁹1: *Folkskola*; 2: *Grundskola*; 3: cat. 2 plus 1 or 2 years of *Gymnasieskola*; 4: cat. 2 plus 3 years of *Gymnasieskola*; 5: cat. 3 or cat. 4 plus less than 3 years of *Eftergymnasial utbildning*; 6: cat. 4 plus 3 or more years of *Eftergymnasial utbildning*; 7: cat. 6 plus PhD; 8: information on education is missing.

⁴⁰We do not have reliable information on whether individuals are primarily studying. We therefore omit this information.

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

$$\ln(w_{ijt}) = \alpha_i + \psi_j + \gamma_t + x'_{it}\beta + r_{ijt}, \quad (4)$$

where $\ln(w_{ijt})$ is the residualized natural logarithm of individual i 's monthly wage at firm j in year t .⁴¹ 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}). We deviate from Card et al. (2013) by including age-group specific year effects as individual-level controls (rather than age squared and age cubed as well as education categories interacted with the year dummies, age squared and age cubed). We use 10 age groups; this allows us to flexibly control for the differential effect of macro shocks on different age groups, while the residualization should take care of life-cycle and gender differences. We also proceed differently than 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.⁴²

One limitation of the AKM approach is that it ignores match quality between worker and firm. It has been shown that this match component makes up around 25% of AKM worker effects (Jackson, 2013; Woodcock, 2015). In this paper we rely on the following assumption: if new hires' AKM worker effects contain a match component from previous employment spells, the resulting error with which we measure their ability is not correlated with the treatment status of the firm that is hiring them.

A.2 Estimation results of AKM estimation

Table A1 provides information on the overall number of workers and firms in the population for each estimation window for which the AKM effects are separately estimated. On average we obtain worker fixed-effect estimates $\hat{\alpha}_i$ for 91.6% of the workers for whom we observe a monthly wage and estimates of ψ_j for the 85.0% of firms that have multiple workers and are connected by worker mobility. Our estimation procedure drops all singletons from the population (8.3% of all workers). Columns (7) to (10) show the share of movers by firm fixed effects quartile.

⁴¹We residualize rather than including age directly as age is perfectly collinear with the combination of individual effects and year dummies.

⁴²To estimate Equation (4), we use `reghdfe` (Correia, 2016). Singletons are not included in the estimation.

Table A1: Descriptive statistics AKM estimation

Sample frame	(1) Overall	(2) Workers Singletons	(3) Worker effects	(4) Overall	(5) Firms Firm effects	(6) Share largest connected set	(7) Share of movers Q1	(8) Q2	(9) by firm FE Q3	(10) quartile Q4
1986–1992	5,435,319	401,754	5,027,152	356,284	299,109	.9947	.6925	.4464	.5908	.5887
1987–1993	5,422,789	396,617	5,020,275	367,475	310,643	.9940	.7036	.4629	.5494	.5936
1988–1994	5,435,506	417,911	5,011,443	380,430	321,403	.9934	.6932	.5167	.5263	.6050
1989–1995	5,447,014	434,153	5,006,559	390,229	331,166	.9926	.6794	.556	.4825	.6153
1990–1996	5,445,220	469,624	4,968,963	395,067	337,528	.9916	.6688	.5738	.452	.5860
1991–1997	5,433,129	503,608	4,922,598	399,915	341,060	.9908	.6563	.5931	.4356	.5741
1992–1998	5,415,569	513,167	4,895,434	397,728	338,377	.9906	.6500	.5828	.4118	.5609
1993–1999	5,419,934	491,487	4,921,917	396,556	338,802	.9906	.6448	.5722	.4478	.5572
1994–2000	5,460,962	473,694	4,980,853	402,424	342,785	.9907	.6492	.5260	.4652	.5695
1995–2001	5,498,288	451,921	5,040,310	403,595	344,756	.9908	.6557	.5223	.4802	.5867
1996–2002	5,528,757	442,109	5,080,866	402,955	343,871	.9908	.6646	.5181	.5092	.5876
1997–2003	5,544,878	425,812	5,113,684	399,729	341,806	.9907	.6631	.5179	.5117	.5904

Note: the table shows descriptive statistics from the estimation of AKM worker and firm effects. The sample frame relates to the 7 years preceding t (from $t - 7$ to $t - 1$) that are used for the estimation. Columns (1) to (5) show the total number of workers (1) and firms (4) in the data, the number of singleton (workers) that are dropped for the estimation (2), and the number of estimated AKM worker and firm effects ((3) and (5)). Column (6) shows the share of workers within the largest connected set among workers with AKM worker effects. Columns (7) to (10) show the share of movers by firm effect quartile. Largest connected set and share of movers are identified by using `felsdvreg` (Cornelissen, 2008).

B Quantile-regression analysis

As compared to Ordinary Least Squares, quantile regression, introduced by Koenker and Bassett Jr (1978), makes it possible to study the association between a variable of interest and various positions in the conditional distribution (conditional quantiles) of the outcome variable (instead of the conditional mean). Rather than minimizing the sum of squared residuals, quantile regression minimizes the sum of residuals’ weighted absolute values. For instance, for quantile regression for P5, the weight is 0.05 when the residual is positive, i.e., when a data point is above the P5 quantile regression line; and the weight is 0.95 when the residual is negative (see, e.g., Koenker and Hallock, 2001; Safer et al., 2011, for an introduction to “traditional” (conditional) quantile regression).

More recent work has emphasized that, in the presence of covariates other than the regressor of interest (“treatment”), the interpretation of conditional quantile regression ceases to be straightforward (and, often, ceases to be useful) as it tells us about associations between the treatment and certain quantiles of the outcome variable not just conditional on the treatment, but also conditional on all other covariates (see, e.g. Wenz, 2019, for a discussion of interpretation pitfalls). Based on this insight, methods for estimating unconditional quantile regressions have been developed, which allow the researcher to return to a straightforward interpretation

of estimates as the changes at a given quantile of the outcome’s unconditional distribution (or of the distribution conditional only on the treatment—see, e.g., Firpo, 2007; Firpo et al., 2009; Powell, 2020). We follow the approach of Powell (2020), which distinguishes explicitly between treatments and additional covariates to provide estimates of quantile treatment effects that are conditional only on the treatment; this allows us to estimate a similar DiD specification as our main one in Equation (1).⁴³

Our unconditional quantile regression analysis in Section 6.3 estimates the following DiD specification:

$$y_{it} = \alpha + \beta TR_{ijt} * POST_t + \gamma_0 TR_{ijt} + \gamma_1 Post_t + \delta_0 X_{ijt} + \delta_1 TR_{ijt} * X_{ijt} + \epsilon_{ijt}, \quad (5)$$

where y_{it} is some, potentially time-varying individual-level outcome for a newly hired worker i in year t ; TR_{ijt} is one when firm j hiring i in year t has 2 to 10 employees at the time of hire and zero when it has 11 to 19 workers. 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 the reform was not yet in effect but during part of which anticipation behavior is possible, is dropped from this DiD specification. X_{ijt} are county-level gross regional product and unemployment rate (for the firm’s location) and $TR_{ijt} * X_{jt}$ is their interaction with the treatment dummy (addressing the possibility that macro shocks differentially affect very small and small firms). We use Baker (2016)’s Stata implementation `genqreg` of Powell (2020) to separately estimate Equation (5) for quantiles 0.01, 0.05, 0.1, 0.15, and 0.2.

⁴³Several other papers study unconditional quantile regression in difference-in-difference settings (Athey and Imbens, 2006; Melly and Santangelo, 2015; Callaway and Li, 2019).

C Tables

Table C1: Data sources

Data source	Period used	Description	Used for measurement of	Variables used/generated
JOB	1993-2004	Employment spells	Worker flows	Timing and identity of hire
JOB	1993-2004	Employment spells	Firm size	Monthly head count
JOB	1993-2004	Employment spells	Other firm characteristics	Industry, age, location, ownership
JOB	1993-2004	Employment spells	Worker labor market history	Co-workers
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 C2: EPL effect on minimum hire quality: analysis and AKM estimation periods

	(1)	(2)	(3)	(4)
Analysis period	1993-2004	1994-2004	1995-2004	1996-2004
DiD estimate: Treated*Post=1	-0.0216*** (0.0076)	-0.0209*** (0.0078)	-0.0195** (0.0080)	-0.0209** (0.0085)
Observations	876,832	813,258	742,301	669,632
Firms	279,459	268,333	255,826	242,487
Adjusted R ²	0.0126	0.0123	0.0113	0.0104

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable is minimum hire quality. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Estimates are from a specification with firm FEs and county-level macro controls interacted with the treatment. The year 2000 is excluded to rule out anticipation effects. Each column restricts the estimation sample to the period specified in the header.

Table C3: EPL effect on minimum hire quality with outliers included (non-winsorized)

	(1)	(2)	(3)
	OLS	MACRO	FE
DiD estimate: Treated*Post=1	-0.0195*** (0.0055)	-0.0268*** (0.0068)	-0.0239*** (0.0075)
Observations	915,545	880,254	880,254
Firms	288,873	280,835	280,835
Adjusted R ²	0.0182	0.0188	0.0128

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, with standard errors clustered at the firm level. Data include outliers, i.e., firms whose number of hires is above the 99.5th percentile. Dependent variable is minimum hire quality. Hire quality is measured by AKM worker effects estimated in years $t - 7$ to $t - 1$ for an individual hired in t . Estimates are from different versions of the specification in Equation (1): OLS without controls (1), OLS with county-level macro controls interacted with the treatment (2), firm FEs with county-level macro controls interacted with the treatment. The year 2000 is excluded to rule out anticipation effects.

Table C4: EPL effect on different minimum hire quality measures with outliers included (non-winsorized)

	(1)	(2)	(3)	(4)
	OLS	MACRO	FE	FE
<i>Panel A: GPA at age 15</i>				
Quality measure:	GPA	GPA	GPA	AKM
DiD estimate: Treated*Post=1	-0.0254*** (0.0070)	-0.0357*** (0.0085)	-0.0687*** (0.0096)	-0.0522*** (0.0104)
Observations	517,486	497,170	497,170	451,757
Firms	217,330	211,215	211,215	199,684
Adjusted R ²	0.0107	0.0158	0.0091	0.0127
<i>Panel B: Cognitive test scores</i>				
Quality measure:	COG	COG	COG	AKM
DiD estimate: Treated*Post=1	0.0062 (0.0063)	0.0012 (0.0077)	-0.0195** (0.0084)	-0.0261*** (0.0090)
Observations	639,052	613,744	613,744	567,795
Firms	241,442	234,377	234,377	224,995
Adjusted R ²	0.0085	0.0177	0.0069	0.0129
<i>Panel C: Psychological test scores</i>				
Quality measure:	NONCOG	NONCOG	NONCOG	AKM
DiD estimate: Treated*Post=1	-0.0202*** (0.0063)	-0.0286*** (0.0077)	-0.0422*** (0.0091)	-0.0277*** (0.0092)
Observations	619,288	594,442	594,442	553,266
Firms	238,094	231,023	231,023	222,200
Adjusted R ²	0.0095	0.0110	0.0063	0.0127

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: GPA at age 15 (Panel A), military draft cognitive test scores (Panel B), military draft psychological test scores (Panel C). Estimates are from different versions of the specification in Equation (1): OLS without controls (1), OLS with county-level macro controls interacted with the treatment (2), firm FEs with county-level macro controls interacted with the treatment (3 and 4). As a benchmark, Column (4) reports the estimated effect on minimum hire AKM for the sample of firms used in the respective panel. The year 2000 is excluded to rule out anticipation effects.

Table C5: EPL effect on minimum hire quality: firm size corrections

	(1)	(2)	(3)
DiD estimate: Treated*Post=1	-0.0216*** (0.0076)	-0.0219*** (0.0076)	-0.0225*** (0.0072)
Observations	876,832	865,181	1,021,154
Firms	279,459	273,161	344,822
Adjusted R ²	0.0126	0.0126	0.0119

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. The columns show different versions of the reform bite measure: (1) head count -1 all firms; (2) head count - managers and family where known, -1 otherwise; (3) unadjusted head count. Dependent variable is minimum hire quality. Hire quality is measured by AKM worker effects estimated in years $t-7$ to $t-1$ for an individual hired in t . Estimates are from a specification with firm FEs and county-level macro controls interacted with the treatment. The year 2000 is excluded to rule out anticipation effects. Each column restricts the estimation sample to the period specified in the header.

Table C6: EPL effect on minimum hire quality and other percentiles

Percentile	(1) P01	(2) P05	(3) P10	(4) P15	(5) P20
<i>Panel A: AKM worker effects</i>					
DiD estimate: Treated*Post=1	-0.0246*** (0.0086)	-0.0197** (0.0086)	0.0066 (0.0083)	0.0199** (0.0080)	0.0285*** (0.0076)
Observations	876,832	876,832	876,832	876,832	876,832
Firms	279,459	279,459	279,459	279,459	279,459
Adjusted R ²	0.0126	0.0123	0.0100	0.0069	0.0047
<i>Panel B: GPA at age 15</i>					
DiD estimate: Treated*Post=1	-0.0670*** (0.0096)	-0.0660*** (0.0096)	-0.0589*** (0.0095)	-0.0433*** (0.0094)	-0.0324*** (0.0092)
Observations	494,273	494,273	494,273	494,273	494,273
Firms	209,895	209,895	209,895	209,895	209,895
Adjusted R ²	0.0089	0.0089	0.0084	0.0074	0.0064
<i>Panel C: Cognitive test scores</i>					
DiD estimate: Treated*Post=1	-0.0192** (0.0083)	-0.0189** (0.0083)	-0.0119 (0.0083)	0.0002 (0.0082)	0.0095 (0.0080)
Observations	610,649	610,649	610,649	610,649	610,649
Firms	233,034	233,034	233,034	233,034	233,034
Adjusted R ²	0.0067	0.0067	0.0063	0.0053	0.0043
<i>Panel D: Psychological test scores</i>					
DiD estimate: Treated*Post=1	-0.0410*** (0.0092)	-0.0408*** (0.0092)	-0.0325*** (0.0091)	-0.0190** (0.0090)	-0.0089 (0.0088)
Observations	591,364	591,364	591,364	591,364	591,364
Firms	229,682	229,682	229,682	229,682	229,682
Adjusted R ²	0.0062	0.0061	0.0057	0.0048	0.0037

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the firm level. Dependent variable: minimum hire AKM (Panel A), minimum hire GPA (Panel B), minimum hire cognitive test score (Panel C), and minimum hire psychological test score (Panel D). Estimates are from Equation (1) specifications with firm fixed effects, county-level macro controls interacted with treatment and year dummies. The year 2000 is excluded to rule out anticipation effects.

Table C7: Placebo thresholds

	(1) 12-20 vs 21-29	(2) 22-30 vs 31-39	(3) 32-40 vs 41-49	(4) 42-50 vs 51-59	(5) 52-60 vs 61-69	(6) 62-70 vs 71-79
<i>Panel A: grade point average</i>						
DiD estimate: Treated*Post=1	-0.0237 (0.0149)	-0.0576*** (0.0205)	-0.0291 (0.0254)	0.0402 (0.0306)	-0.0306 (0.0363)	0.0386 (0.0398)
Observations	171,160	83,458	51,304	34,940	25,152	19,294
Firms	67,931	34,318	21,879	15,452	11,596	9,108
Adjusted R ²	0.0116	0.0175	0.0217	0.0245	0.0232	0.0273
<i>Panel B: cognitive test score</i>						
DiD estimate: Treated*Post=1	-0.0037 (0.0135)	-0.0173 (0.0190)	0.0037 (0.0247)	0.0120 (0.0299)	-0.0646* (0.0356)	0.0499 (0.0391)
Observations	197,727	93,002	56,001	37,660	26,827	20,345
Firms	72,216	35,878	22,670	15,928	11,911	9,360
Adjusted R ²	0.0096	0.0137	0.0130	0.0126	0.0154	0.0160
<i>Panel C: psychological test score</i>						
DiD estimate: Treated*Post=1	-0.0082 (0.0145)	-0.0132 (0.0207)	-0.0108 (0.0268)	-0.0248 (0.0325)	-0.0452 (0.0378)	0.0295 (0.0433)
Observations	194,010	91,849	55,497	37,396	26,683	20,280
Firms	71,721	35,751	22,619	15,883	11,877	9,354
Adjusted R ²	0.0096	0.0139	0.0138	0.0144	0.0152	0.0130

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: GPA at age 15 (Panel A), military draft cognitive test scores (Panel B), military draft psychological test scores (Panel C). Estimates are from Equation (1) specifications with firm fixed effects, county-level macro controls interacted with treatment and year dummies. The year 2000 is excluded to rule out anticipation effects.

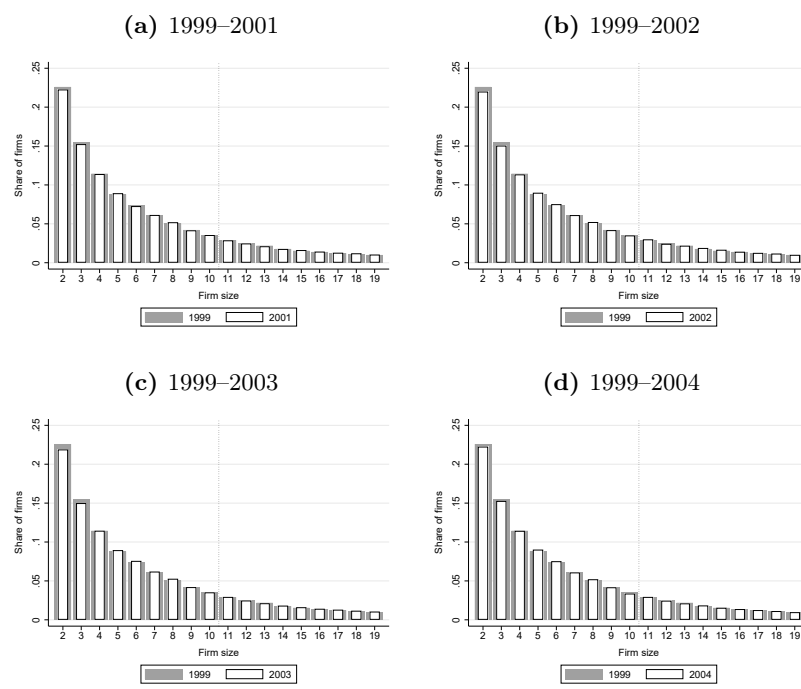
Table C8: Worker-level EPL effect at the bottom of the outcome distribution: quantile regression estimates

	(1) P01	(2) P05	(3) P10	(4) P15	(5) P20
<i>Panel A: GPA at age 15</i>					
DiD estimate: Treated*Post=1	-0.0000 (0.0006)	-0.0500 (0.0320)	-0.0000 (0.1267)	-0.0000 (0.3812)	0.2500 (0.5485)
Observations	1082195	1082195	1082195	1082195	1082195
<i>Panel B: cognitive test scores</i>					
DiD estimate: Treated*Post=1	0.0000 (0.1576)	0.0052 (0.3801)	0.0026 (0.0493)	-0.0024 (0.1344)	0.0017 (0.3307)
Observations	1459096	1459096	1459096	1459096	1459096
<i>Panel C: psychological test scores</i>					
DiD estimate: Treated*Post=1	-0.0073 (0.7181)	-0.0043 (0.2783)	-0.0227 (0.1395)	-0.0045 (313.7405)	-0.0129 (0.0801)
Observations	1376541	1376541	1376541	1376541	1376541

Note: *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is estimated GPA at age 15 (Panel A) or, from the military draft, either cognitive test scores (Panel B) or psychological test scores (Panel C). Estimates are from Equation (5), see Appendix Section B for details. The year 2000 is excluded to rule out anticipation effects.

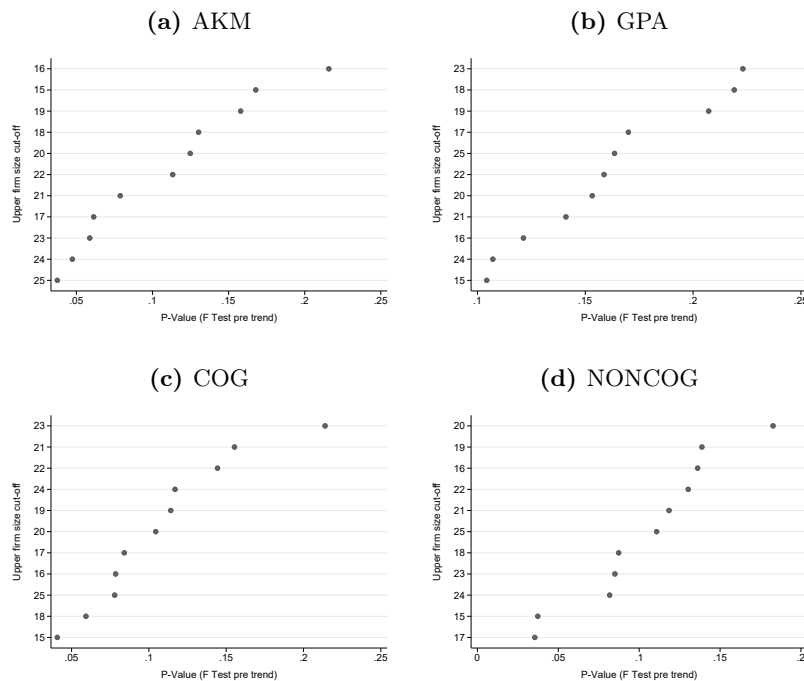
D Figures

Figure D1: Changes in the firm size distribution for different intervals



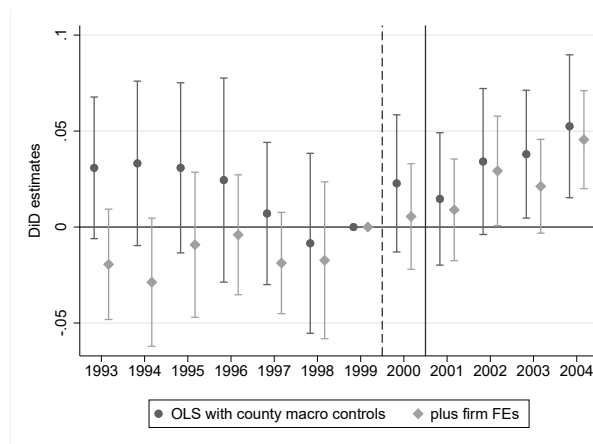
Note: The gray bars show the firm size distribution for firms in 1999. White bars show the corresponding distribution for the years 2001 (a) to 2004 (d).

Figure D2: p values of joint significance tests for potential control groups



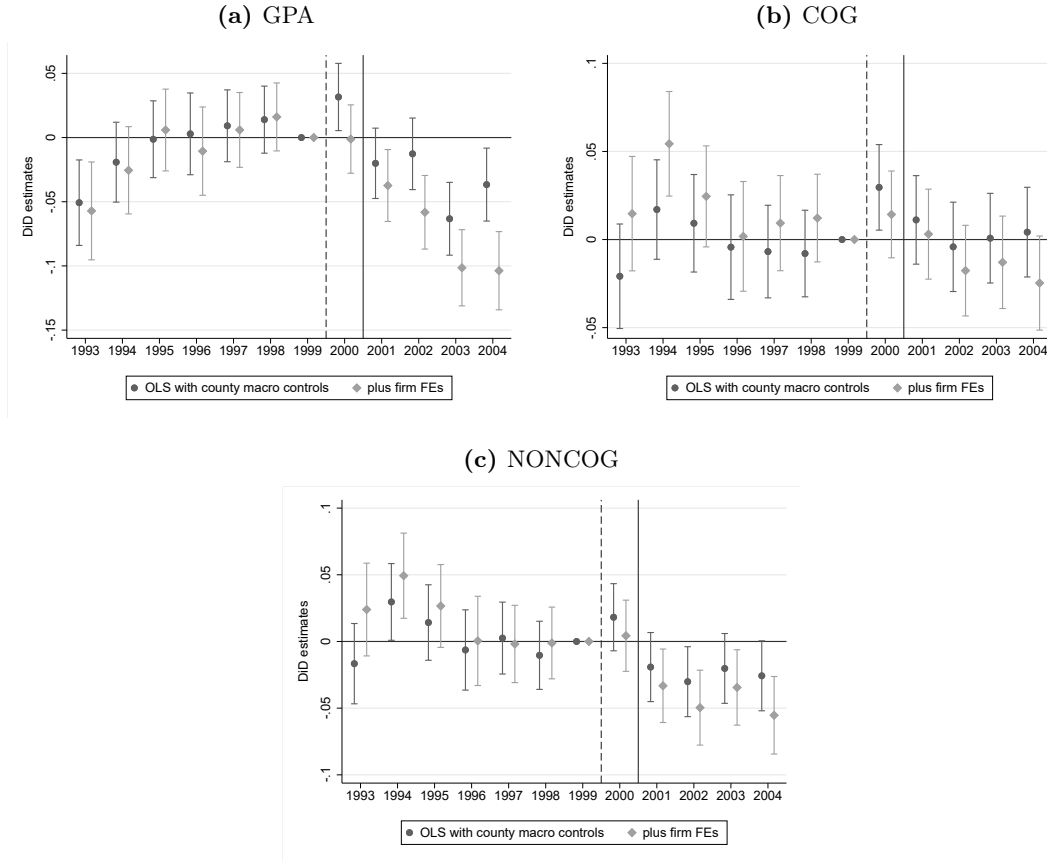
Note: For each control group candidate of size 11-15, ..., 11-25, the figures plot the average p value across 50 random draws of an F test of joint significance of $\hat{\beta}_{1993}, \hat{\beta}_{1994}, \dots, \hat{\beta}_{1999}$ from estimating Equation (2).

Figure D3: Effects on involuntary separations



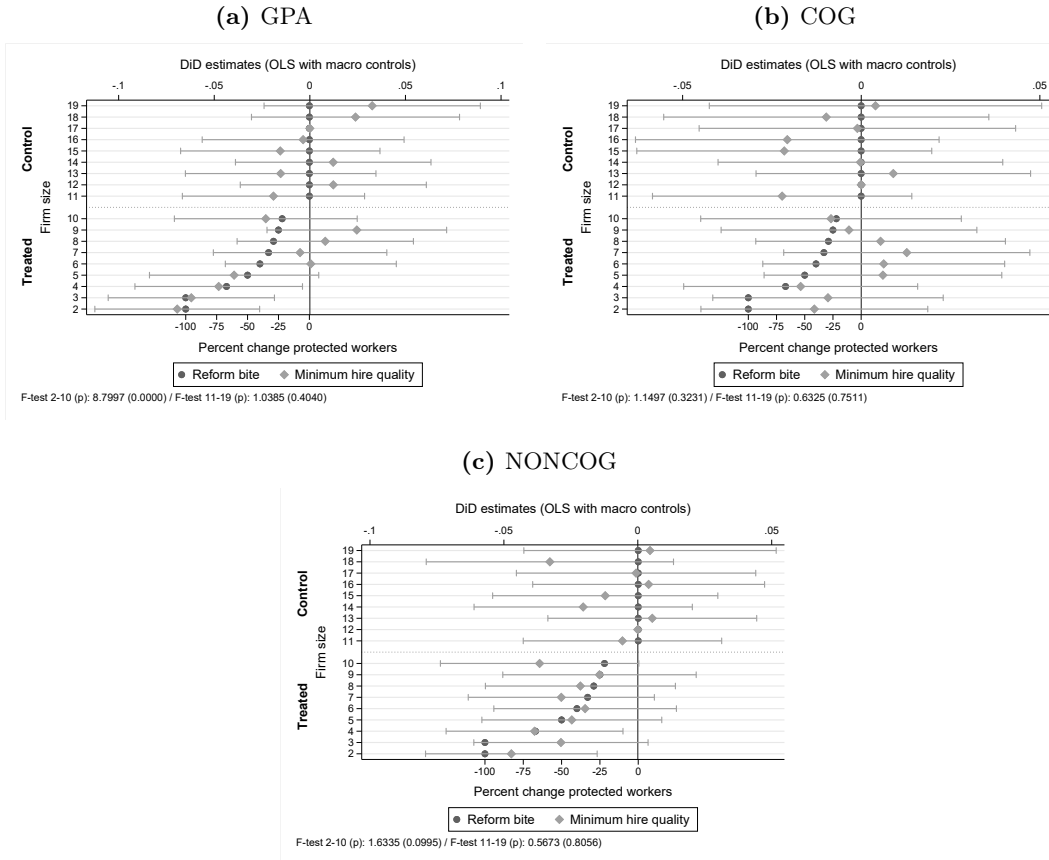
Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the involuntary separation rate (number of involuntary separations divided by previous-year average headcount). Involuntary separations are approximated with separations that are either followed by at least a month of non-employment or by a direct transition to a job that pays less. Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Vertical bars denote 95% confidence intervals.

Figure D4: EPL effect on minimum hire quality



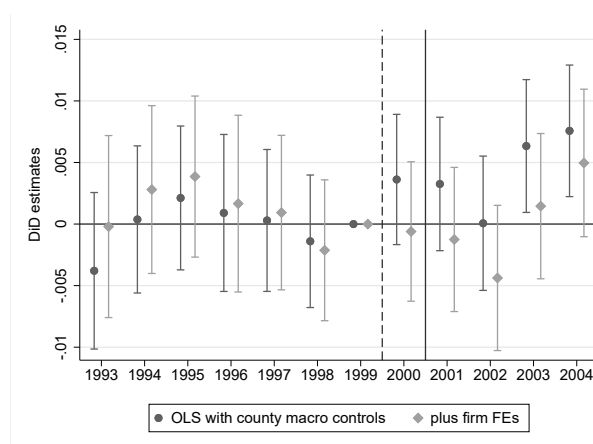
Note: Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Hire quality is measured by GPA at age 15 (a) and, from the military draft, cognitive test scores (b) as well as psychological test scores (c). Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.

Figure D5: EPL effect heterogeneity by firm size



Note: The light gray diamonds show firm size-specific DiD estimates for the effect of the EPL reform on minimum hire quality from an OLS specification (without firm fixed effects) with county-level macro controls interacted with the treatment and year dummies. Hire quality is measured by GPA at age 15 (a) and, from the military draft, cognitive test scores (b) as well as psychological test scores (c). Horizontal bars denote 95% confidence intervals. Standard errors are clustered at the firm level. The dark gray dots illustrate the bite of the reform in terms of the percentage change of workers protected by the seniority rule. The reference firm size category of 12 has been chosen so that it roughly coincides with the mean of estimated firm size-specific DiD coefficients for firms in the control group.

Figure D6: EPL effect on firm attractiveness



Note: This figure shows yearly DiD estimates for the effect of the EPL reform on the share of hires who are voluntary job changers coming directly from high-wage firms (estimated AKM firm effect for 1986-92 above the median). Estimates are from the specification in Equation (2) with firm fixed effects, county-level macro controls interacted with the treatment and year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the firm level.