The Effect of Employment Protection on Firms' Worker Selection

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Introduction

1. Introduction

- How does strictness of employment protection legislation (EPL) affect employment?
- Anticipated firing costs reduce hiring but also firing (Bertola, 1999) $\rightarrow$ ambiguous effect on employment levels (OECD, 2013)
- Not much is known about how EPL affects which types of workers are hired
  - Distributional effects?
  - Hiring standards?
- Theoretical predictions: risky workers vs. hiring standards
  - Risky workers (Lazear, 1995):
    - Less stringent EPL allows firms to hire from lower and upper end of ability distribution
    - Implies option value for high-productivity workers $\rightarrow$ private information
  - Pries and Rogerson (2005):
    - Stricter EPL implies a higher threshold $\rightarrow$ raise hiring standard

1. Introduction

Empirical evidence on strictness of EPL and firms' hiring (strategies)

- Marinescu (2009): increasing job security for workers with 1-2y tenure (UK)
  - also unaffected workers (<1y) have lower firing hazard → improved match quality?
  - no direct evidence but consistent with the idea that more stringent EPL makes firms more selective in their hiring
- Bjuggren and Skedinger (2018): 2001 LIFO reform in Sweden
  - firms with less stringent EPL → more likely to hire from unemployed individuals and from participants of active labour market policies
  - argument that unemployed individuals are harder to screen → reform makes treated firms screen less
- Effects on labor turnover
  - Kugler and Pica (2008): higher firing costs → lower worker turnover (13-15%)
  - von Below and Skogman Thoursie (2010; LIFO 2001): reform increased number of hirings and separations (5%) – no employment effects

1. Introduction: This paper

- Research question: does employment protection legislation (EPL) affect hiring standards?
  - We exploit the 2001 last-in-first-out (LIFO – *turordningsregler*) reform in Sweden
  - Reform granted more exemptions for small firms
  - Did small firms change their hiring behavior, compared to larger firms?
  - Firm size threshold used for difference-in-differences framework
- We are not the first to study the 2001 reform:
  - Firm-level outcomes: firm productivity $\uparrow$ (Bjuggren, 2018); labor turnover $\uparrow$ employment $\rightarrow$ (von Below and Thoursie, 2010); firm growth $\nearrow$ (Bornhäll et al., 2017)
  - Worker-level outcomes: sickness absence $\downarrow$ (Lindbeck et al., 2006; Olsson, 2009); parental leave take up among fathers $\downarrow$ (Olsson, 2017)

1. Introduction: This paper

- Preview of our findings
  - Less stringent EPL $\Rightarrow$ establishments decrease their hiring standard
  - Hiring standard: minimum worker ability that is hired
  - Changes in screening activities likely underlying mechanism
- Contribution to the literature
  - Little direct evidence on how EPL affects *who* is hired and *why* this is $\rightarrow$ findings consistent with related papers (Marinescu, 2009; Bjuggren and Skedinger, 2018)
  - More evidence on the determinants of selectivity in worker-firm matching (e.g., Balsvik and Haller, 2015: foreign ownership; Bender et al., 2018: management quality; Hensvik and Skans, 2016: networks)
- And why is this important?
  - Efficiency perspective: firms' selectiveness affects allocation of workers
  - Distributional implications: disadvantaged groups increasingly hired?
  - How should we optimally design EPL policies? $\rightarrow$ current policy discussion in Sweden regarding labor market policies (LAS)

Remainder of the presentation

Institutional background

Empirical strategy and data

Main results

Threats to identification

Conclusion and discussion

Institutional background

2. Institutional background: The 2001 Last-in-first-out rule reform

- Swedish EPL (*Lagen om anställningsskydd, LAS*) is relatively strict: Dismissals allowed in case of
  - misconduct
  - economic necessity $\rightarrow$ Last-in-first-out (LIFO) rule is applied
- LIFO first introduced in 1974
- We study the 2001 LIFO reform: introduction of exemption for small firms
  - Pre-reform (< 2001): LIFO applied to all firms $\rightarrow$ last hire is laid off first
  - Post-reform(≥ 2001): small firms can choose between three most recent hires
  - How are small firms defined? ≤ 10 employees

2. Institutional background: The 2001 Last-in-first-out rule reform

- To which degree are firms affected by the reform?

firm size	Pre-reform		Post-reform		Share protected
	Protected	Unprotected	Protected	Unprotected	becoming unp.
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%

2. Institutional background: The 2001 Last-in-first-out rule reform

- Implementation of the 2001 reform:
  - Feb 2000: first proposal in parliament
  - Oct 2000:
    - Written into law
    - "Unlikely coalition": liberal-conservatives and greens against then-governing social democrats
  - Jan 2001: introduction
- Possible anticipation effects in 2000?
  - Expectation that hirings can be reversed

Empirical strategy and data

3. Empirical strategy and data

Aim: estimate the effect of the 2001 relaxation of Sweden's LIFO for small establishment on hiring standards

- Difference-in-differences approach

  $y_{jt} = \alpha + \beta TR * POST_{jt} + \gamma_t + \delta TR_j + \epsilon_{jt}$.

  - y_{jt}: hiring standard
  - TR:
    - dummy for small firms ($\leq$ 10 employees)
  - $POST$:
    - 0=before (1993-1999)
    - 1=after (2001-2004)
      (Possible anticipation effects in 2000)
  - β: estimate of interest

3. Empirical strategy and data

- Key assumption 1: absent of reform treatment and control firms would have followed similar trend ($\mathbb{E}\,\epsilon_{jt}|\,TR_j * POST_t = 0$)
- Key assumption 2: no major reforms at same time *and* with same treatment definition
  - 1994: temporary exemption from LIFO $\rightarrow$ diff. year; no size threshold
  - 1997: reform of temporary contracts $\rightarrow$ diff. year; no size threshold
  - 2001: gender equality act $\rightarrow$ same year and similar threshold
- Treatment definition:
  - firm had $\leq$ 10 employees in 1999
  - assumption that reform was not anticipated in 1999 (first draft 02/2000) – cf. Bjuggren (2018)
- What do we estimate?
  1. Estimates intention-to-treat effect (ITT) – time-invariant firm size (1999)
  2. Endogenous sorting of firms $\rightarrow$ additionally estimate LATE with 1999 size as instrument for current firm size

3. Empirical strategy and data

- How do we measure minimum hire quality $y_{jt} = min_{jt}\{ability_i\}$?
  1. Main measure: estimated AKM worker fixed effects
     - Years 1986-1992 used for estimation ▸ details
     - Birth cohorts 1922 to 1976
  2. Military draft cognitive and psychological test scores ▸ details
     - Birth cohorts 1951 to 1991
  3. High school GPA age 15
     - Birth cohorts 1973 to 1982
- Why focus on AKM?
  - Male *and* female hires
  - Relatively long coverage

3. Empirical strategy and data

Individual-level Swedish register data (Statistics Sweden: SCB)

- Employment spell information (JOBB): timing of formation/dissolution of worker-firm matches, industry, location, firm age, public/private sector, firm size
  - Estimation of AKM worker fixed effects (1986-1992)
  - Information on hires (1993-2004)
- Education information (LISA)
- Grade 9 GPA (årskurs-9)
- Armed forces' database: military test scores

3. Empirical strategy and data

- Sample size and definitions
  - 1986-1992: sample used only for AKM estimation
  - 1993-2004
    - 1993-1999: pre-period
    - 2000 excluded: potential anticipation effects (excluded)
    - 2001-2004: post-period
- Firm level restrictions
  - Firm growth: exclude outliers with extreme firm growth (.5%)
  - Firms: keep only firms that exist in 1999 ($N = 275,731$)
- Firm size: keep only firms of size 2-15. ($N = 129,187$)

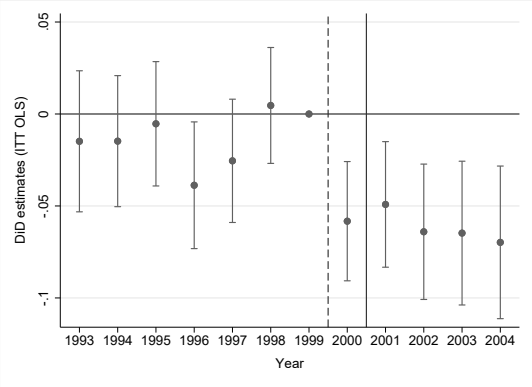
Firm characteristics by treatment status

A: Continuous characteristics	Treated		Control	
	mean	sd	mean	sd
Head count (rounded)	4.857	2.453	12.688	1.388
Firm age (years)	8.171	4.548	8.859	4.407
Mean worker age (years)	40.158	9.311	38.644	8.532
Female worker share	0.409	0.297	0.373	0.267
Mean worker years of schooling	11.418	1.450	11.356	1.222
Mean worker monthly wage (100 SEK 1980)	45.080	29.996	45.506	29.199
Mean worker AKM person effect (std, 1986-92)	-0.007	0.998	-0.022	0.817
Estimated AKM firm effect (std, 1986-92)	-0.007	0.992	0.038	0.995
Observations	87,441		13,947	

Main results

4. Main results

Dynamic EPL effect on minimum hire quality (AKM)



- Minimum hiring quality consistently lower in the post-period
- Less stringent EPL → significantly lower hiring standard
- 2000: likely anticipation effect (excluded from regressions)
- Results similar for military test scores and GPA ▸ details

4. Main results

- Quantify the effect of loosening EPL for different outcomes (standardized)

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

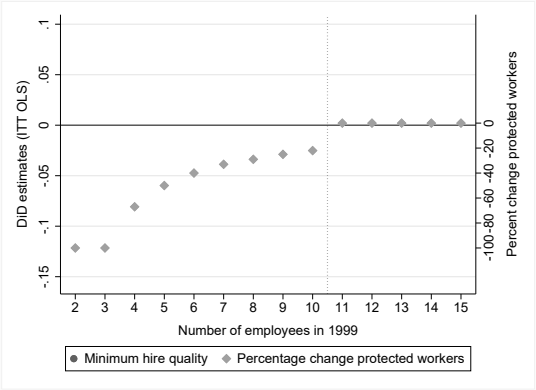
- Consistent results across outcome measures
- Similar results when including controls and firm FE ▸ details

4. Main results: heterogeneous effects

- Heterogeneous effects: effects by firm size (reform bite)
  - Smallest treatment group firms (2-3 workers): 100% of protected workers become unprotected
  - Largest treatment group firms (10 employees): 22% of protected workers become unprotected
- Smallest firms should be affected the strongest

4. Main results: heterogeneous effects

EPL effect heterogeneity by firm size (AKM)



- Light gray dots show change in share of protected workers becoming unprotected
- Dark gray dots show firm size-specific DiD estimates
- Results show that effects are much stronger for smallest (=most affected) firms

4. Main results: analysis of mechanisms

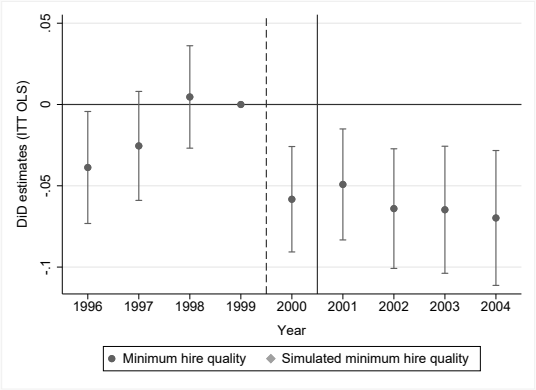
- Main result:
  - Affected firms react to the reform by hiring more workers with lower ability
  - *Why* do firms adjust their hiring standard?
- What are potential mechanisms behind this result?
  - Mechanism 1: mechanical labor turnover effect. Increased labor turnover implies more draws from the pool of applicants $\rightarrow$ this mechanically increases variation in hired individuals.
  - Mechanism 2: screening effect. When laying off is easier (cheaper) $\rightarrow$ firms can afford to reduce their screening intensity.
  - Mechanism 3: risky hiring. Increased flexibility in firing allows firms to take more risks in learning about individuals productivity $\rightarrow$ implies greater variance in hiring.

4. Main results: analysis of mechanisms

- Mechanism 1: number of hires effect
  - Reform has been shown to increase labor turnover (von Below and Skogman Thoursie 2010)
  - Firms make more draws from the pool of potential hires
  - Simulation exercise:
    - Assume that hiring standard has *not* changed and that only the number of hires has changed
    - Randomly reshuffle hires within firm size groups (1996-2004)
    - If purely mechanical $\rightarrow$ results based on simulated data should fully explain our findings

4. Main results: analysis of mechanisms

EPL effect and random hiring simulation (1996-2004)



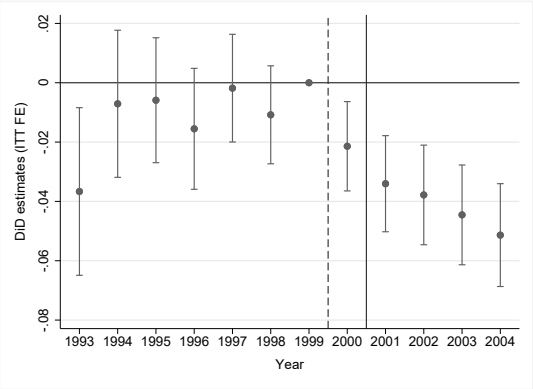
- Actual effects (dark dots)
- Simulated effects (light gray)
- Simulated effects more attenuated
- Actual effects about twice the size of simulated effects (-.048*** vs -.025***)
- Suggests that labor turnover does not fully explain our main finding

4. Main results: analysis of mechanisms

- Mechanism 2: Screening effect $\rightarrow$ firms decrease their screening efforts
- Pries & Rogerson (2005): less severe consequences makes firms more likely to hire somebody with lower quality
- How can we measure screening intensity of firms?
  - Nordström Skans and Hensvik (2016) have provided evidence that firms' use for network based hiring for screening purposes
  - We use a measure of the links between new hires and incumbent workers as an indication of firm's screening intensity
  - Intuition: if firms screen less $\rightarrow$ reliable signals of ability less important (such as through networks)

4. Main results: analysis of mechanisms

EPL effect on network-based hiring



- Outcome variable: Decrease in network-based hiring for treated firms
- Result suggests that treated firms decreased their screening activities
- Caveat:
  - Without fixed effects, pre-trends *not* parallel
  - Outlier 1993

4. Main results: analysis of mechanisms

- Mechanism 3: risky hires channel
  - Lazear (1995): theory of risky workers
    - Productivity of individuals at both ends of the ability distribution is more difficult to assess
    - Creates an option value for the firm $\rightarrow$ cheaper when firing costs are low
  - Did the maximum hire quality also increase with the reform?

4. Main results: analysis of mechanisms

Actual and simulated estimates

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

- Yes, maximum hire quality was also affected → risky hires?
- But: driven by mechanical effects (→ Mechanism 1)

4. Main results: summary of findings

- Summary main results
  - Firms react to change in EPL by adjusting hiring standard
  - Results partially explained by purely mechanical effects (labor turnover)
  - Suggestive evidence for screening explanation

Threats to identification

5. Threats to identification

- Gender equality act: coinciding reform with similar size threshold ▸ details
- Demand vs. supply: sorting of workers into treatment and control firms ▸ details
- Non-classical measurement error: estimation error in AKM estimation and sorting of workers into treatment and control firms ▸ details
- Selective worker ability measurement: availability of AKM estimate related to firms' treatment status ▸ details
- Time-invariant firm heterogeneity: changes in firm composition over time ▸ details
- Endogenous firm size and overcorrection: IV estimation to account for changes in firm size ▸ details
- Non-parallel trends: placebo tests ▸ details

Conclusion and discussion

6. Conclusion and discussion

- We find that less stringent EPL reduces firms' hiring standards
  - Mechanical effects are important
  - Importance of mechanical effects caused by labor turnover
  - Evidence for a screening-based mechanism
- Robustness tests suggest that these effects can be interpreted as causal

6. Conclusion and discussion

- How do our results relate to previous research?
  - Bjuggren and Skedinger (2018): same reform increased hiring from previously unemployed individuals → consistent with our finding that *lower* end of the hiring standard is affected
  - Bjuggren (2018): reform increased firms' labour productivity
    - Inconsistent with our results of hiring more lower-ability individuals?
    - No! Reform also increased labor turnover (von Below and Skogman Thoursie 2010) → labor market churning explanation (Hopenhayn and Rogerson, 1993)?
    - Firms hire more widely and may retain the best workers
- Current policy debate in Sweden
  - Following 2018 GE: plans on reforming employment protection (*LAS*)
  - Expanding exemption rules of *turordningsregler*
  - Our findings suggest that disadvantaged groups could indeed benefit

Measurement: AKM worker fixed effects

- We follow Abowd et al. (1999) and Card et al. (2013) and estimate two-way fixed-effects regression:

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

  - $\ln(w_{ijt})$ is the natural logarithm of individual i's hourly wage at firm j in year t
  - Additive fixed effects for individuals (α_i) and firms (ψ_j)
  - Year dummies (γ_t)
  - Vector of time-varying individual-level controls (x_{it})
    - Age squared and age cubed
    - Education categories interacted with the year dummies, age squared and age cubed

Measurement: AKM worker fixed effects

back

- Use individual-level spell data on employment from JOBB
- Obtain full time-equivalent (FTE) monthly wages from the Wage Survey Statistics (WSS – survey based)
  - CPI deflated
  - Winsorize at 0.5% and 99.5% of the annual real monthly FTE wage distribution
  - WSS survey based: covers only stratified random sample of smaller firms
- Sample definition (1986 to 1992)
  - Individuals aged 18-65 with a November spell with FTE wage data
    - 45% of person-year observations have FTE information
    - 64% of all individuals have at last one FTE observation
  - Education information available from 1990 $\rightarrow$ imputed from first year available

Measurement: military draft test scores

back

Military test scores

- Individual measurement: nearly all men at age 18-19.
- 9-point scale.
- Approximately normally distributed.
- Hiring standard: use minimum hire ability at firm-year level.

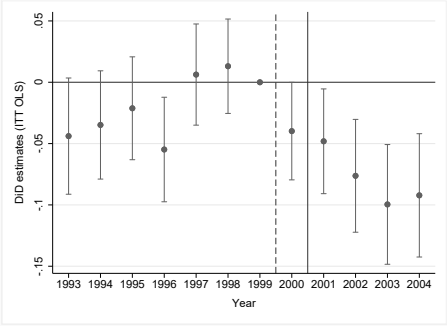
Measurement: military draft test scores

- Cognitive ability: 4 sub-scores from written tests
  - Inductive skill/reasoning
  - Verbal comprehension
  - Spatial ability
  - Technical understanding.
- Non-cognitive ability: 4 sub-scores from behavioral interview with psychologist
  - Social maturity
  - Psychological energy (focus and perseverance)
  - Intensity (e.g., activation without pressure)
  - Emotional stability (e.g., tolerance to stress).

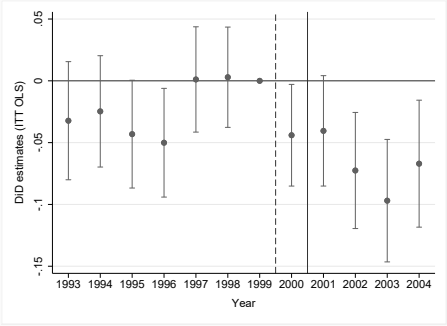
DiD estimates for other outcomes

- Dynamic effects for (1) cognitive test scores (military); (2) psychological test scores (military); (3) GPA test scores age 15

(1) cognitive test scores (military)



(2) psychological test scores (military)

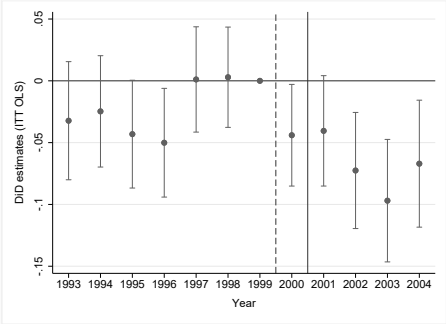


DiD estimates for other outcomes

back

- Dynamic effects for (1) cognitive test scores (military); (2) psychological test scores (military); (3) GPA test scores age 15

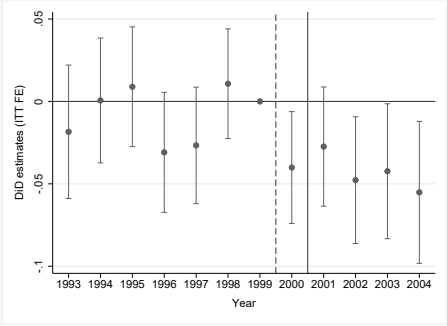
(3) GPA test score age 15



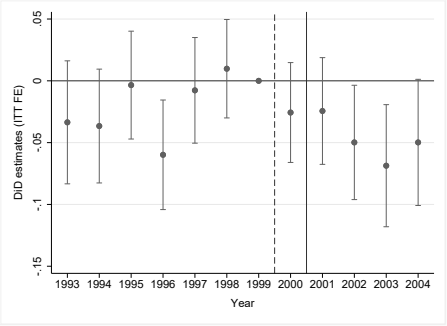
DiD estimates for other outcomes (with FE)

- Dynamic effects for (1) AKM; (2) cognitive test scores (military); (3) psychological test scores (military); (4) GPA test scores age 15

(1) AKM



(2) cognitive test scores (military)

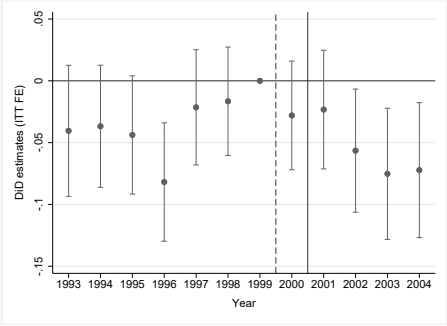


DiD estimates for other outcomes (with FE)

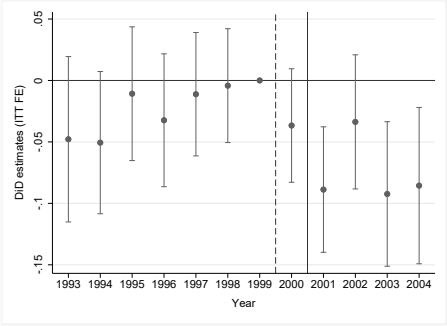
back

- Dynamic effects for (1) cognitive test scores (military); (2) psychological test scores (military); (3) GPA test scores age 15

(3) psychological test scores (military)



(4) GPA test score age 15



Threat to identification: Gender equality act

back

- Reform Gender Equality Act (GEA) effective 2001 (SFS, 2000)
  - GEA introduced in 1992 (SFS, 1991): firms with ≥ 10 employees are required to publish document specifying measure to promote gender equality
  - Reform made wording more concrete, but no sanctions or penalties
  - No econometric evaluation of the reform available
  - GEA-treatment group (10-15) $\approx$ LIFO control group (11-15)
- Hypothesis: GEA treatment group $\rightarrow$ favoring women to "comply" with GEA
  - LIFO control group hires more women
  - Labor market for LIFO treatment group tighter $\rightarrow \beta$ biased towards zero

	Women	Men
DiD estimate (Treated*Post)=1	-0.0353***	-0.0522***
	(0.0128)	(0.0122)
Observations (firms)	180,194 (76,453)	192,387 (78,853)
Adjusted R^2	0.0012	0.0019

Threat to identification: Demand vs. supply

back

- Implicit assumption that self-selection of workers into treatment / control firms not affected by the reform
- Ambiguous effects
  - Short-run: *not* most recent hire needs to be laid off in treated firms
  - Medium-run: seniority capital lower since lower protection in treated firms
- Information on applicants? No
- Voluntary job-job transitions $\rightarrow$ sorting patterns systematic to the reform?

	E-E hires
DiD estimate (Treated*Post)=1	0.0109
	(0.0084)
Observations	314,144
Firms	101,388
Adjusted R^2	0.0022

Threat to identification: Non-classical measurement error

back

- Measurement error in estimated worker fixed effects (AKM)
  - AKMs are estimated using period *before* pre-period starts
  - As long as treated and control firms are equally affected by measurement error $\rightarrow$ will be differenced out
  - But: women facing wage discrimination $\rightarrow$ AKMs understate their ability
    - What if women are increasingly hired in treated firms?
    - Lowering hiring threshold could be due to underestimation of ability

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

Threat to identification: Selective worker ability measurement

back

- AKM worker fixed effects cannot be estimated for the entire population (55% of all new hires)
- Problematic?
  - Yes if AKM measurement is *positively* related to the reform
  - This can create mechanical effects

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

Threat to identification: Time-invariant firm heterogeneity

back

- Does firm composition change over time?
- Example: business cycles creating many new small firms (but not large ones)
- Problematic?
  - We condition on firms having existed in 1999 (when we measure firm size)
  - Firm fixed effects estimation (Figures: details)

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

Threat to identification: Endogenous firm size and overcorrection

back

- Local average treatment effect (LATE) estimation to allow for time-varying firm size
- Instrument: 1999 firm size

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

Threat to identification: Non-parallel trends

back

- Would treated firms changed hiring behavior also in absence of reform?
- Placebo treatment at actual threshold +20, 30 and 40

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