Directing Job Search in Practice: Mandating Pay Information in Job Ads

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Abstract

Using a policy reform that mandated firms to provide pay information in job ads, I study how workers adapt their search behavior and firms their hiring decisions, including actual wages. At the time of the reform, only one fifth of job ads contained pay information, strongly varying across firms and occupations. Linking online job board data to administrative social security data, I find that wages of newly hired workers increased, by about 3%, particularly within complying firms that were induced by the reform to show pay information. Job ads posted by these firms received more clicks and applications from job-seekers but I do not find evidence that the wage increase was driven by a positive selection of employees: previous wages of newly hired workers at their prior job did not increase, and additionally, applicants had on average lower pay expectations and characteristics less fitting job ads requirements. Furthermore, I do not find evidence of closing the gender pay gap. Adapting a simple model of partially directed job search, I illustrate how to interpret these responses.

Keywords: job search, pay transparency, directed search.
JEL: D83, J31, J63, J64, J68

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1 Introduction

Workers want jobs with good wages. When searching for an ideal job, workers would like to have perfect information about all relevant jobs – and especially information about wages that employers are willing to offer. In reality, most firms do not reveal pay information until an offer is made at the end of the hiring process. What are consequences of this lack of pay transparency? The obfuscation of pay might (i) contribute to persisting gender wage gap (Biasi and Sarsons, 2022; Roussille, 2022), (ii) result in monopsony power (Jäger et al., 2021; Robinson, 1933) and (iii) hinder labor mobility and wage arbitrage for observationally similar workers, especially in negatively affected industries or local markets (Autor, Dorn and Hanson, 2021). Therefore it should not be surprising that the question of mandating pay transparency has recently resonated strongly both in the academic literature as well as in the broader public debate[1].

How would a change in pay transparency affect behavior of firms and workers in a standard search and matching framework? On the one hand, competitive (directed) search models (e.g. Acemoglu and Shimer 1999; Moen 1997) assume that job seekers possess full information, or that it can be costlessly inferred by them. Firms post full contracts and workers will earn exactly the wages that are specified by contracts. On the other hand, random search models (e.g. models in Diamond-Mortensen-Pissarides tradition but also Burdett and Mortensen 1998) assume the polar opposite: search occurs before wage setting and offers arrive randomly, leaving little allocative or signalling role for wages. Interestingly, when taken literally, both directed and random job search models make the same prediction for the effect of providing wage information on realized wages: any new wage information should not matter.

In this paper, I test this prediction using a specific transparency reform in Slovakia. Since May 2018, firms have to include wage information in any job ad they post. Firms became bound from below by these wage offers as they could not legally hire workers at a lower rate than what they stated in the job ad, facing steep fines for violations. However, they could still pay workers more in form of bonuses and other extra pay. The intended goal of this policy change was to improve the position of workers in the hiring process, making them more aware of what wage they can ask for in a job interview. Because transparency is mandated at the job ad level, this reform differs in two major ways from previously studied pay transparency policies that typically mandate firms to provide aggregated pay reports. First, pay information directly in job ads is very salient compared to periodic pay reports that require effort to find and

[1] During the last decade, some form of pay transparency policies have been adopted in Austria, Denmark, Iceland, Finland, Germany, Norway, Sweden, the UK as well as in the US states of Colorado and New York. Most of these policies were explicitly motivated by existing gender pay gaps. I discuss empirical evidence from some of these reforms later in the paper.

are much less granular. Second, since the reform bounds firm's offers only from below and the mandate does not require to inform about ex-post realized wages, the offers are going to be less informative for incumbent workers, highlighting a different channel than those emphasized in the literature.

To estimate the effects of this reform, I bring together three novel datasets. I base my analysis on data from the dominant online job board platform in Slovakia. This dataset allows me to observe the universe of job ads placed on the platform around the reform date as well as profiles and applications of job seekers who are actively using the platform. I combine this dataset with monthly administrative employer-employee data and annual firm financial data. Since all datasets share the same firm identifier, I can measure from the job board data how often firms included pay information in job ads before the reform, and study the impact on wages and hiring at the firm level. I make several novel findings regarding firm's and job seeker's search behavior as well as the overall impact of this non-typical pay transparency reform on wages and hiring.

My first finding regarding firms' responses is that firms do comply with the mandate by providing wage information and change their behavior almost instantly. Despite the possibility for firms to avoid constraining themselves by advertising artificially low wages in combination with advertising extra pay and bonuses, posted pay does actually increase after the reform. This finding indicates that firms find it more profitable to partially constrain themselves than to appear unattractive and face the risk of attracting fewer applicants. However, for jobs that included pay information already before the reform, I find a significant decrease in posted pay, which works in the opposite direction but in smaller magnitude. At the same time, the reform did not affect the number of ads posted on the job board and the sectoral composition of job ads is largely unaffected as well.

Second, turning to the job seekers' behavior, I find that, as a result of the reform, job seekers turn their attention to job ads posted by firms that previously avoided providing pay information. Job ads of firms that have to provide pay information after the reform register higher interest in terms of clicks on ads as well as actual received applications. The pool of applicants changes, however. Job seekers applying to firms complying with the reform have lower self-reported reservation wages and their primary job title from their profile is less likely to match job title specified in the job ad. However, in terms of required education, they do not seem to match job educational requirements any worse.

Third, focusing on the outcome of the matching between firms and workers, I estimate the effect of the reform on wages of newly hired workers. I find that the reform increased wages, particularly by increasing wages in firms that previously did not use pay information. I do not find any differential effect for men and women. I also esti-

mate the effect of the reform on incumbent workers, finding only a small increase, in line with the assertion that the new information brought by the reform is relatively uninformative for incumbent workers and therefore unlikely to trigger renegotiation. I do not detect any changes in the number of newly hired workers or in the size of firms.

To reconcile these findings with the search and matching framework I adopt a model by Wu (2020), which nests random and directed search as special cases. In this general search framework, workers face different costs of directing search for two types of firms. I examine the model's qualitative predictions when the cost exogenously decreases, corresponding to the transparency reform that effectively decreased wage uncertainty in certain firms. The comparative statics of the model imply that as a result of the reform, more job seekers will start applying to productive firms, that previously did not include pay information and queues become longer. As the cost of directing search decreases for productive firms they lose some of their monopsony power, which leads to wage increases. At the same time, the effect of receiving more applicants compresses wages. These predictions are broadly in line with my empirical findings. I also argue that the observed wage increases are not consistent with alternative explanations such as an increase in bargaining power or in matching efficiency.

Related literature. First, I contribute to the literature on the empirical assessment of random and directed job search models. Until recently, there was little evidence whether workers actually redirect search according to wage information, which is the core feature separating directed and random search. Despite the simplicity of the question, observational datasets do not allow for establishing a causal relationship between wages and applications, because wages tend to be correlated with other features of job ads as I also document later. Yet, Marinescu and Wolthoff (2020) attempt to establish a causal relationship by conditioning on narrowly defined job titles, which have strong predictive power on posted wages. They also show that failing to control for job titles leads to an estimate of a negative application supply elasticity. Banfi and Villena-Roldán (2019) confirm this finding and also show that job ad information is predictive even when posted wages are unobservable to job seekers. Some studies, mostly in development contexts,[2] have experimentally varied wages for jobs within a single firm or a public organization and found a positive application supply elasticity as well as improvements in applicants' quality or in their motivation, consistent with directed search. He, Neumark and Weng (2021) experimentally vary posted wages for different occupations within a single firm on a Chinese online job board.[3]

[2] See Abebe, Caria and Ortiz-Ospina (2021); Dal Bó, Finan and Rossi (2013); Deserranno (2019); Hedblom, Hickman and List (2019).

[3] They also argue that a positive application supply elasticity without accounting for reservation wages is insufficient as an evidence for directed search. If the offered wage is raised above the reservation wage it would lead to more applicants, even if job seekers search randomly among offers above

A disadvantage of these research designs is that they are by design limited to a single firm. Belot, Kircher and Muller (2022) overcome this limitation using a "reverse" version of the Bertrand and Mullainathan (2004) audit study, by varying wages (and additional job characteristics) in duplicated, artificial job ads that are being shown to job seekers. They estimate an application intention elasticity of 0.7-0.9, very similar to Dal Bó, Finan and Rossi (2013) and He, Neumark and Weng (2021) as well as to non-experimental estimates of Marinescu and Wolthoff (2020).

My paper differs substantially from these tests: I do not study changing *levels* of posted wages but changing *presence* of posted wages. The studies previously discussed are implicitly limited by studying only a part of the labor market that features wage offers (only about a quarter of all job offers, see e.g. Brenčič, 2012) and therefore exclude the majority of job ads.[4] My paper fills this gap in the literature. Furthermore, rather than being restricted to a single firm as is common in almost all previous work, I study a market-wide reform that affects multiple firms in different sectors. In addition, the reform could have triggered general equilibrium effects that would not be captured otherwise.

Second, this paper speaks to the emerging literature on the role of workers' beliefs, biases and the role of information. Several papers show that the unemployed are overly optimistic, as their reservation wages strongly depend on their previous pay and they update reservation wages only slowly as their job search progresses (see Mueller, Spinnewijn and Topa 2021 for a literature review). From this strand of literature, the most related work is Jäger et al. (2021). Using a special module in the German socio-economic panel survey, they show that to form beliefs about the pay in their outside option job, workers simply use their current pay. They then estimate actual outside options for observationally similar workers, showing that relatively underpaid workers are therefore overly pessimistic, while workers that are relatively overpaid are overly optimistic. They argue that the existence of some low-paid jobs is currently viable only because of incorrect beliefs about outside options. I show that the pay transparency reform I study did lead to an increase in wages but without a detectable decrease in vacancies or an increase in unemployment.

Active labor market policies (ALMP), such as counselling programs for the unemployed, also often involve provision of information to change beliefs. ALMP have been found on average to have a positive effect on employment (Card, Kluve and Weber,

the reservation wage. Therefore, they also conduct the test with only those job ads that are above reservation wages, strengthening the evidence for directed search.

[4] On page 47, Belot, Kircher and Muller (2022) write "A particularly fruitful application could investigate those vacancies that do not post wage offers. Our study is silent on those, as it focuses on variations of wages for those vacancies that already use wage offers. For those that do not, it might be interesting to explore what happens if one randomly introduces wage offers."

2018) but their evaluation typically does not differentiate between provision of specific advice and enforcing search. More recently, Altmann et al. (2018) show that providing German job-seekers with a brochure on general information about job search and unemployment has a positive impact on finding a job. There is also evidence that job-seekers redirect their search to specific jobs in response to information about new job openings (Skandalis, 2018) and in response to receiving advice about occupations where market tightness is currently favorable and skill requirements make the transition feasible (Belot, Kircher and Muller, 2019). I show that pay information can turn job seekers' attention to certain firms and increase wages among the affected firms.

Third, I contribute to the literature on information treatments regarding wages. These treatments are either informative about the wages of co-workers from the same firm or organization (Card et al., 2012; Cullen and Perez-Truglia, 2022, 2018) or about wages in the larger labor market. The latter case includes primarily evaluations of pay transparency laws. These are of particular interest as they share some similarities with the reform studied in this paper. Cullen and Pakzad-Hurson (2021) summarize results of these recently adopted policies in a meta-analysis and propose a model that can explain some of the heterogeneous findings across different labor markets.[5] In general, these studies are primarily focused on wage compression and gender gaps, and typically find that transparency laws lead to wage increases for women and slower wage growth for men, leading to a decrease in gender pay gap. The model of Cullen and Pakzad-Hurson (2021) implies that these effects are muted when wage setting is dominated by unions and sectoral agreements as opposed to cases, where individual bargaining is more common. Lastly, Roussille (2022) studies the gender pay gap on the job board Hired.com that uses an unusual hiring procedure: first, job seekers set their ask salary, which is visible to employers, then employers react with offering bid salary and if the person is hired, final salary is recorded. Roussille (2022) finds that almost all the unexplained gender pay gap can be attributed to differences in the ask wage. Furthermore, when this field becomes automatically pre-filled for a subset of San Francisco software engineering jobs, the gender gaps in ask, bid and final salary drop towards zero.

Relative to other transparency laws, the reform studied in my paper does not involve transparency about final wages but about wages in job ads. This seemingly innocuous difference has, however, several consequences. Since final wage remains unobserved, it is less likely that the reform would lead to wage renegotiations or quits among incumbent workers stemming from comparison with co-workers. Similarly, firms do

[5] The meta-analysis includes following studies: Baker et al. (2022); Bennedsen et al. (2022); Blundell (2020); Böheim and Gust (2021); Duchini, Simion and Turrell (2020); Gulyas, Seitz and Sinha (2022); Mas (2017); Obloj and Zenger (2022).

not have to fear ramifications of revealing the true within-firm wage inequality. The effect on closing the gender pay gap is less clear: on the one hand, there is evidence that women feel less informed about pay and are less likely to engage in negotiations (Biasi and Sarsons, 2022; Cullen and Perez-Truglia, 2018; Flory, Leibbrandt and List, 2015) and therefore they could benefit from the wage transparency similarly to findings in Roussille (2022). On the other hand, the reform mandates only a lower bound on the wage information and further bargaining remains possible.

On-going work by Frimmel et al. (2022) and Arnold, Quach and Taska (2022) studies analogous transparency reforms in Austria and Colorado, respectively. Frimmel et al. (2022) use public job board data combined with the ASSD database (Zweimüller et al., 2009) and find no effect on wages overall. Arnold, Quach and Taska (2022) use Burning Glass data to study the introduction of 2021 Colorado law that required job postings to contain expected salary information. Unfortunately, job ads cannot be linked to actual wages or to application data in their setting. Therefore, they study only compliance and changes in posted wages for firms that already provided pay information. I discuss in detail how my findings relate to these two papers in Section 7.

2 Institutional setting

I study a pay transparency reform in Slovakia, an OECD high-income economy in Central Europe with population of roughly 5.4 million people. Slovakia has been a member of the European Union since 2004 and of the Eurozone since 2009 and as such its economy and labor market is roughly comparable to other Eastern EU members that it borders (Poland, Czechia and Hungary). Automotive, electrical and engineering are the dominant industries. These industries, and the economy as a whole, are highly export-oriented, with Germany as its primary export destination.

The first proposal to amend Act No. 5/2004 on Employment Services by mandating employers to publish pay information in any job ads was discussed in the parliament but rejected in May 2017. Several months later, the governing coalition introduced an almost identical proposal, which was eventually adopted as an Act No. 63/2018 on March 8, 2018 and became colloquially known as Pay Transparency Act. As its main result, the Employment Services Act was amended with the following statement: "*When publishing a job offer, the employer is obliged to state the amount of the fixed component of pay*". The fixed component refers to the part that is explicitly agreed on and cannot be changed without changing the working contract.

However, the bargaining between an employer and an employee remained possible through the variable component of pay: pay for overtime, work outside usual hours or on holidays, performance or seniority-based bonuses or other type of rewards. How is

the overall pay contractually split between fixed and variable component in a job offer is left for the employer to decide. In principle, it is compatible with the law to simply shift a part of the fixed pay to variable pay under a new contract, without actually changing the employer's overall labor costs. The offered pay itself can be stated either per hour or, as it is more common in practice, per month.

The Pay Transparency Act also amended the Labor Code by prohibiting employers from hiring employees with "*the fixed component of pay lower than fixed component of pay than* [*the employer's*] *published job offer*". Hiring at a pay below the pay stated in the corresponding job ad can lead to fines up to 100 000 EUR for the employer. The amendments also introduce a sanction mechanism for the case when an employer would fail to provide pay information in a job ad. The National Labour Inspectorate can impose fines that can reach 33 193 EUR.

The changes that Pay Transparency Act introduced came into effect on May 1, 2018. It should be noted that pay transparency introduced in Slovakia differs rather dramatically from seemingly similar pay transparency reforms in other countries. First, the transparency involves pay in job ads not the actual pay. Therefore, the concerns about equity and satisfaction of workers, that was documented to be a potent mechanism in other settings, is not of the primary concern here, because the actual pay (i.e. what the Slovak labor law considers the combined fixed and variable components) remains unobservable. Therefore, employers do not have to fear the backlash of employees raised by within-firm wage inequalities.

Second, the transparency reform effectively requires to provide only a lower bound on the pay that an employer is willing to offer as it allows further bargaining. Therefore if some groups differ in their proclivity to bargain, as has been shown e.g. for men and women, they might still end up with substantial pay gaps. Of course, even the partial fixed component of pay in a job ad can be informative about the actual pay. However, the reform does not prevent employers to tailor pay at a worker level nor does it prohibit workers from bargaining despite knowing the fixed pay component from the job ad. It would be incorrect to interpret the reform as a switch from following wage bargaining protocol to pure wage posting in the entire labor market. The actual change that the reform induced was more subtle: it made easier for workers to infer their potential future wages from any job ad that a worker sees but it did not eliminate wage bargaining between employers and employees.

3 Data

The dataset used in the empirical analysis resulted from merging three data sources. I am unaware of any prior use of these datasets in academic research. First, I use propri-

etary data from *Profesia.sk*, a major online job search platform in Slovakia, which is the most popular website for posting vacancies by firms and searching for jobs by workers. The time range from January 2017 to June 2019, i.e. covering more than a year before the reform on May 1, 2018 and more than a year after it, allows me to study over 3.3 million job applications from over 270 thousand unique profiles. On the firm side, these job applications were sent to 658 thousand job ads from 38.5 thousand firms, or about 722 new job ads per day.

My second data source is administrative records from Slovak social security administration. The data contain information about monthly pay for the universe of legally employed persons in Slovakia. Apart from monthly pay, it also contains basic demographic variables (age, gender) and notably a firm identifier, that allows creating matched employer-employee dataset. Furthermore, this identifier can be used to match the proprietary job ads and applications data to administrative data. However, the datasets do not share the same worker or vacancy identifiers. Therefore, in the empirical analysis with respect to pay I will rely on firm-level outcomes rather than on worker-level outcomes.

Finally, I enrich firm information from the previous two datasets with the data from the Register of Financial Statements (RFS), that provides annual financial information on the universe of Slovak firms. RFS data are publicly accessible and searchable on the website `www.registeruz.sk` and accessible via an API. The supposed role of the RFS is to "improve and simplify the business environment and reduce the administrative burden on business". This dataset contains information on sales, assets and their detailed breakdown, accounting profits, number of employees and other information on economic and financial performance of firms. Since the financial statements are filled annually, this third dataset does not constitute a solid basis for reduced-form empirical analysis, however it provides a rich set of variables to provide descriptive information about firms in the pre-reform period.

4 Pay information in job ads

I start with providing key descriptive statistics on the online platform that I study. This job ad portal is the largest platform by online traffic and is widely known among jobseekers and employees. According to the survey conducted by the company itself, 91% of working-age population are familiar with their online platform. In 2021, 292 838 job ads were placed on the website. I focus on the period from January 2017 to June 2019, allowing me to observe more than a year before the reform and enough time to study its impact after. The data is reported on daily basis. Importantly, the job ads data include a string variable that employers can use to inform job seekers about pay that is being

offered. Figures A.6 and A.7 in the Appendix show how job ads are displayed to job seekers. Typically, before the reform the field is left empty or contains vague statements such as "by agreement" or "depending on experience and qualifications".

First, I define a variable whether any numerical value is provided at all. This will be important for understanding how the pay information availability changes with the reform. Second, as sometimes a range is provided, or some extra pay is mentioned, I attempt to unify these different reporting standards to be able to form a meaningful measure of offered pay that could be compared to actual pay in administrative records. For this standardization I assume 40 working hours per week and 4.35 weeks per month. If a range or multiple values are provided, I use the lower bound and the first of several values (sometimes additional pay or bonuses are mentioned in parentheses), respectively. However, not all jobs with relatively low posted pay can be credibly attributed to wages posted per hour or per week - these are likely to be part-time contracts that unfortunately cannot be identified so for the lack of information about hours. I also exclude job ads that indicate currency other than Euro, implying the employment contract is realized outside Slovakia (1.83% of all job ads).

In 23% of job ads some numerical information is provided already before the reform, consistent with the presence of wage posting among a small subset of jobs, e.g. Hall and Krueger (2012) find that 28% of surveyed U.S. workers knew exactly how much they are going to be paid at the time of interview and did not bargain. Marinescu and Wolthoff (2020) use data from CareerBuilder.com platform, where pay information is present among 20% of job ads, which is very similar to the setting studied in this paper. Of course, the presence of pay information does not necessarily mean that any bargaining is impossible and reversely, lack of pay information does not preclude existence of very rigid pay schedules within firms. Therefore it would not be correct to dichotomize jobs into wage posting and wage bargaining just based on the presence of pay information. In fact, I will argue that the reform improves information about the pay being offered to workers rather than forcing firms to switch to wage posting from wage bargaining.

Pre-reform descriptive analysis in Table 1 reveals that firms that resort to the use of wage information (which indicates wage posting) typically aim at workers with lower education levels and young workers without experience, mostly in manufacturing and retail and pay, on average lower wages. These job offers are filled quicker, while receiving similar number of applications from job seekers. However, even within a specific sector, firms vary in their use of wage posting. Table A.1 shows that inclusion of wage information is very common in the restaurant sector (39%) and in job agencies in manufacturing (41%), while being relatively rare in the IT industry (4.5%) or banking (8%). These descriptive findings are generally in line with the previous literature (Brenzel, Gartner and Schnabel, 2014; Marinescu and Wolthoff, 2020).

			Before, info	Before, no info	Difference
Education	(% of ads)	Less than HS	66.62	29.77	36.71***
		High school completion	28.40	45.19	-16.68**
		Post HS diploma	0.61	2.01	-1.42***
		College or more	4.38	23.04	-18.62***
			100	100	
Wage	(mean)		895.99	.	
	(% of ads)	Less than 600 EUR	5.34	.	
		600 EUR - 800 EUR	33.01	.	
		800 EUR - 1000 EUR	19.56	.	
		1000 EUR - 1200 EUR	9.78	.	
		1200 EUR - 1400 EUR	5.02	.	
		More than 1400 EUR	11.34	.	
			100	.	
Sector	(% of ads)	Job agency	51.16	21.43	29.73*
		Retail	11.02	10.72	.30
		IT	1.51	9.45	-7.94***
		Banking	1.76	5.88	-4.12*
		Automotive	3.12	5.79	-2.66*
		Engineering	1.57	3.82	-2.25***
		Transport	3.25	3.18	.07
		Other sector	34.29	52.36	-18.06*
			100	100	
Entry job	(% of ads)		55.22	29.52	25.44***
Full-time job	(% of ads)		67.77	80.20	-12.58***
Duration	(mean)		17.49	22.30	-4.85*
	(% of ads)	A week or less	36.32	17.07	19.53
		1 to 2 weeks	16.76	19.16	-2.57*
		More than 2 weeks	46.92	63.78	-16.96*
			100	100	
Views	(mean)		669.29	500.06	168.13
Applications	(mean)		14.41	14.17	.21
	(%)	None	11.52	11.17	.48
		1 to 2	19.14	17.40	1.90
		3 to 5	17.51	17.90	-.43
		6 to 10	16.68	17.94	-1.36
		11 to 20	14.74	15.65	-1.01
		21 to 30	7.24	7.32	-.12
		31 to 50	6.37	6.08	.29
		51 or more	5.73	5.40	.33
			100	100	
Observations			79 653	269 922	

Table 1: Summary statistics of job ads, by availability of pay information before the reform

Notes: The table shows means and shares of job ads posted before the reform, separately for ads with and without pay information. The difference column reports the coefficient from a regression of each characteristic on an indicator for the ad containing pay information. Standard errors are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Unlike other comparable data sources, the job board data also allow me to examine detailed descriptives of applicants, including their socio-economic characteristics, education and detailed skill sets. From Table 2 it can be seen that the platform is used broadly across different demographics. Both college educated as well as workers with little formal education are well represented on the platform. Similarly, the job board is not limited only to younger workers, though here the difference to overall population is more prominent as a typical job seeker is almost 10 years younger than an employee

in administrative data.

			Male	Female	Total
Age	(mean)		30.28	29.43	29.85
	(% of applicants)	Less than 18	6.03	8.27	7.18
		18-24	28.69	28.91	28.80
		25-34	32.46	31.32	31.87
		35-44	19.56	19.42	19.49
		45 or more	13.27	12.07	12.65
			100	100	100
Education	(% of applicants)	Less than HS	31.75	23.85	27.74
		High school diploma	44.02	45.81	44.93
		Post HS specialization	2.17	2.41	2.29
		College or more	22.05	27.93	25.04
			100	100	100
Reservation wage	(mean)		960.57	730.55	846.47
	(% of applicants)	Less than 600 EUR	7.71	17.23	12.44
		600 EUR - 800 EUR	21.10	28.28	24.67
		800 EUR - 1000 EUR	21.00	18.50	19.76
		1000 EUR - 1200 EUR	14.79	8.58	11.71
		1200 EUR - 1400 EUR	9.12	4.72	6.94
		More than 1400 EUR	15.86	5.18	10.56
			100	100	100
Applications sent	(mean)		12.05	11.70	11.87
Observations			130 503	138 679	269 182

Table 2: Summary statistics of job applicants

I find that substantial variation in wage information is across firms rather than within firms, therefore I further characterize firms according to the use of wage information before the reform. Figure 1 illustrates the intuitive idea of this definition. This motivates my division of firms into two categories based on their pre-reform behavior for some of the subsequent analysis: (i) firms that mostly did not provide pay information (share of job ads with wage lower than 50%), (ii) firms that mostly did provide pay information (share of job ads with wage higher than or equal to 50%). Throughout the paper, I refer to the first group as *complying firms*, since the reform forced them to change their behavior and comply with the mandate, and to the second group as *always-taking firms*, since they provided pay information regardless of the reform.

4.1 Impact of the 2018 reform on posted pay information

Figure 2 shows that the reform worked in forcing firms to include pay information in job ads. The share of job ads posted on the online board Profesia.sk almost quadrupled. Specifically, the share of job ads with pay information increased from 22.8% to 83.2%. Three observations are worth pointing out. First, there is an uptick in provision of pay information already in April 2018, the month before the reform became binding. The likely explanation is that some firms did not want to risk a fine for job ads that were posted in April but were advertised to start only in May as the interpretation of the

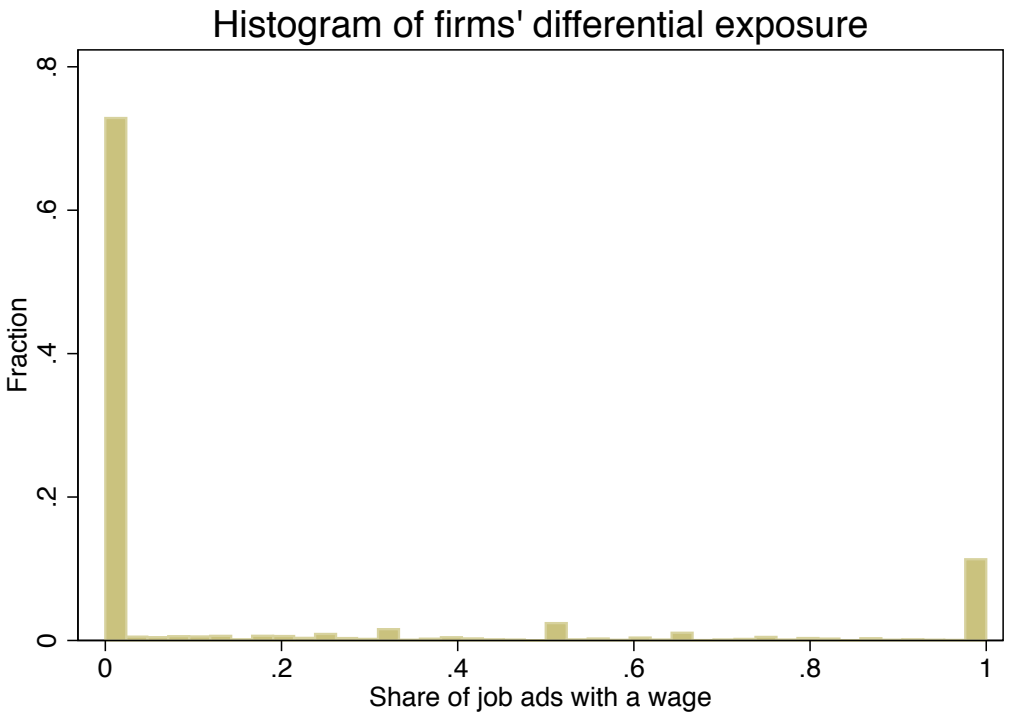


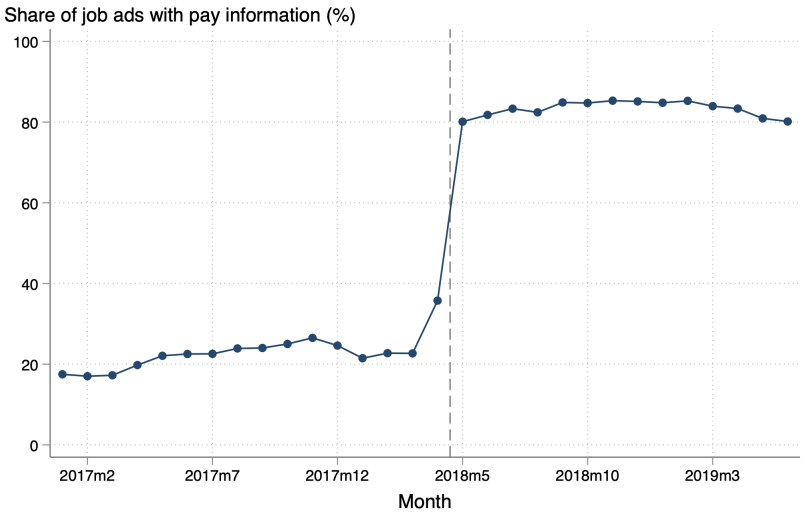
Figure 1: Firm's exposure to the reform

law was not perfectly clear. Second, the share of job ads with pay information does not reach 100% after May 2018, rather stays only above 80%. This appears to be primarily a data issue: the law does not require to provide pay information in a standardized way, just forces firm to provide the information in some, unspecified way. Third, both before and after the reform there is no apparent trend so it seems unlikely that provision of pay information would depend importantly on external factors, making interpretation of the reform more straightforward as it allows us to cleanly separate the period before and the period after the reform.

In practice, most of the "missing" pay information can be reached by following a link to the company website, away from the job board. In some other cases, companies use templates that put pay information in the body of the text or even in pictures, while leaving the text field provided by the job board empty. In these cases, the wage is not observable as the dataset does not contain entire job ad texts. While in principle such a behavior could be strategic, after cross-checking some of these job ads manually it appears unlikely that the goal would be to hinder pay transparency as the information was typically quite apparent after clicking on the job ad. After all, the reform left open the option for firms to artificially deflate base pay while not changing the overall pay.

To investigate whether this behavior was indeed happening, in Figure 3 I plot the distribution of posted wages, separately before and after the reform. As the portal is

Figure 2: The percentage share of job ads with pay information, by month



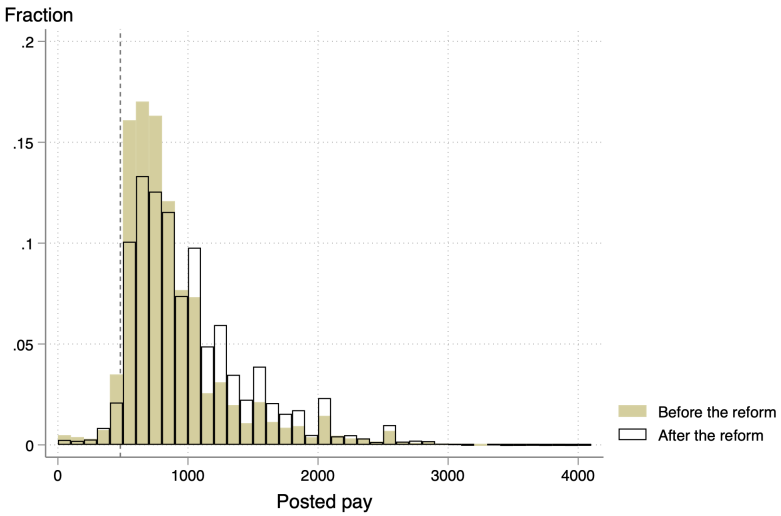
Notes: The figure shows the monthly evolution of the average share of job ads that contain pay information. The dashed line indicates the time of reform (May 2018).

used broadly across multiple sectors, distribution of posted wages is clearly not limited to low wage jobs only. In Slovakia, minimum wages are set at national level and updated annually. The monthly minimum wage was set to 435 EUR in 2017, 480 EUR in 2018 and 510 EUR in 2019.[6] The dashed line denotes the minimum wage at the time of the reform. The spikes at the minimum wage in respective years are not particularly pronounced. For example, in 2018 before the reform, there is some bunching exactly at 480 EUR (0.79% of job ads with pay information), however the posted pay of nicely rounded up 500 EUR is more than 2 times as frequent. Only about 4% of job ads with pay information during the same period have offers between 450 EUR and 500 EUR. After the reform, the bunching increases slightly to 1.04% at the exact minimum wage.

From the comparison it is clear that posted pay increased, particularly by revealing the right part of the pay distribution. It is interesting that posted pay does not extensively bunch close to the minimum wage. Therefore, firms provide information that is likely to be not too far from the actual offers and avoid artificially deflating the base pay all the way to the minimum wage. Such a behavior would in a directed search model come at a cost to firms as their job ads would appear less attractive to job seekers and receive fewer applications. This competition for workers is therefore likely to lead to unravelling and firms would end up posting offers that tend to be somewhat realistic.

[6] The Slovak minimum wage law defines both minimum monthly wage as well as minimum hourly wage, which was 2.759 EUR for jobs with 40 hours work week, hence the assumptions used for standardization of non-monthly information in job ads used above.

Figure 3: Histogram of posted pay, before and after the reform



Notes: The figure shows two histograms of posted pay from job ads. If a range was provided, the lower bound was taken. Both histograms are capped at 4000 EUR. Dashed line shows the monthly minimum wage at the time of the reform (480 EUR). Kolmogorov-Smirnov test for equality of distributions rejects the null of equality at p-value < 0.001.

On the other hand, the reform also bans firms from paying workers less than what they state in a job ad. Therefore, firms could react by decreasing their wage offers to avoid such a situation, and thus muting the overall increase that is apparent in Figure 3. Of course, this comparison is possible only for jobs that included pay both before and after the reform. To be able to test this hypothesis, it is necessary to make sure that the jobs in the comparison are as similar as possible. Therefore, I estimate the following regression equation:

$$PostedPay_{it} = \alpha_0 + \alpha_1 Post_t + \gamma_t + \delta_{j(i)} + \epsilon_{it} \tag{1}$$

where the outcome variable $PostedPay_{it}$ is the pay information in the job ad i at time t, $Post_t$ is a dummy variable equal to one in the post-reform period and zero otherwise. The term γ_t refers to year and month fixed effects, $\delta_{j(i)}$ to job fixed effects and ϵ_{it} is the error term. The key coefficient of interest is α_1. The standard errors are clustered at the firm level.

Specifications in Table 3 vary by the definition of the job fixed effects term $\delta_{j(i)}$. Even though the inclusion of month and year FE implies that pay must have been included before the reform, the lack of controls for job characteristics would be clearly inadequate (Marinescu and Wolthoff, 2020). Therefore, I start in column (1) with using job title fixed

effects. There are over 600 distinct job titles that an employer specifies when setting up a job ad on the online board, and it can also use multiple different ones at the same time. For my purposes, I use only the first job title in case there is more than one job title stated in a job ad. In column (2) I use combined job title-firm fixed effects to capture $\delta_{j(i)}$. Finally, in the third column I further interact these fixed effects with counties (i.e. 79 geographic units within Slovakia; I amalgamate counties Bratislava I-V and Košice I-IV, located within the two largest cities, into 2 counties). In this preferred specification the inclusion of $\delta_{j(i)}$ limits variation in postings to narrow firm-job title-county cells. Regardless of the exact definition of job fixed effects $\delta_{j(i)}$, the estimates show a decrease in posted pay by 7 to 9 percent, indicating that while the distribution of posted pay after the reform is shifted to the right, this happens despite a decrease in posted wages in jobs that included pay information already before the reform.

The identifying assumption is that in absence of the reform, the posted pay would evolve similarly as before the reform. In Figure A.1 in the Appendix, I modify the preferred specification to allow event study estimates by month. The results show a large decrease in posted pay realized immediately after the reform and afterwards posted pay picks up again. It seems unlikely that this sudden decrease could be attributed to a different event, since there was no other policy change taking place at the same time as pay transparency law came into effect.

Table 3: Effect of the reform on posted pay

	(1) log(Pay)	(2) log(Pay)	(3) log(Pay)
Post	-0.09***	-0.08***	-0.07***
	(0.01)	(0.01)	(0.01)
Month FE	yes	yes	yes
Year FE	yes	yes	yes
Job title FE	yes	no	no
Job title × firm FE	no	yes	no
Job title × firm × county FE	no	no	yes
Adj. R2	0.39	0.74	0.79
Avg. Outcome	6.69	6.69	6.69
N	330972	303491	242195

Notes: Fixed effects included in the specifications are indicated at the bottom of the table. Standard errors are clustered at the firm level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A potential problem for the interpretation of the reform could be caused by employers switching to different methods of advertising jobs, therefore leading to selection bias in the job board data. However, this is not the case, as can be seen in from results in Table 4. Here, I regress the number of ads on a dummy variable for the post-reform

period, using the same specification as in Table 3. The outcome variable is first aggregated at the level of the fixed effects used in the regression, for example in column (3), the outcome is the monthly count of job ads within each job title. The estimates are small relative to the pre-reform averages of the outcome variable and not statistically significant except for the last estimate that uses the richest fixed effects with the average number of ads per cell before the reform being only 1.64. In Figure A.4 I show the time series of the number of monthly job ads, i.e. the outcome variable from the specification (1). Apart from a certain degree of seasonality (a strong drop in job ads posted each December) the graph looks flat, without any apparent break after the reform. Therefore, it seems unlikely that employers would react by leaving the online job board for alternative methods of advertising jobs. This is reassuring as it simplifies interpretation of the results of the reform.

Table 4: Effect of the reform on the number of ads

	(1) Ads	(2) Ads	(3) Ads	(4) Ads	(5) Ads
Post	425.93	0.10	1.07	0.03	0.11***
	(813.11)	(0.16)	(1.32)	(0.07)	(0.03)
Month FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Firm FE	no	yes	no	no	no
Job title FE	no	no	yes	no	no
Job title × firm FE	no	no	no	yes	no
Job title × firm × county FE	no	no	no	no	yes
Adj. R2	0.84	0.91	0.96	0.10	0.10
Avg. Outcome	21848.44	4.35	49.78	1.93	1.64
N	30	139556	13258	326132	296981

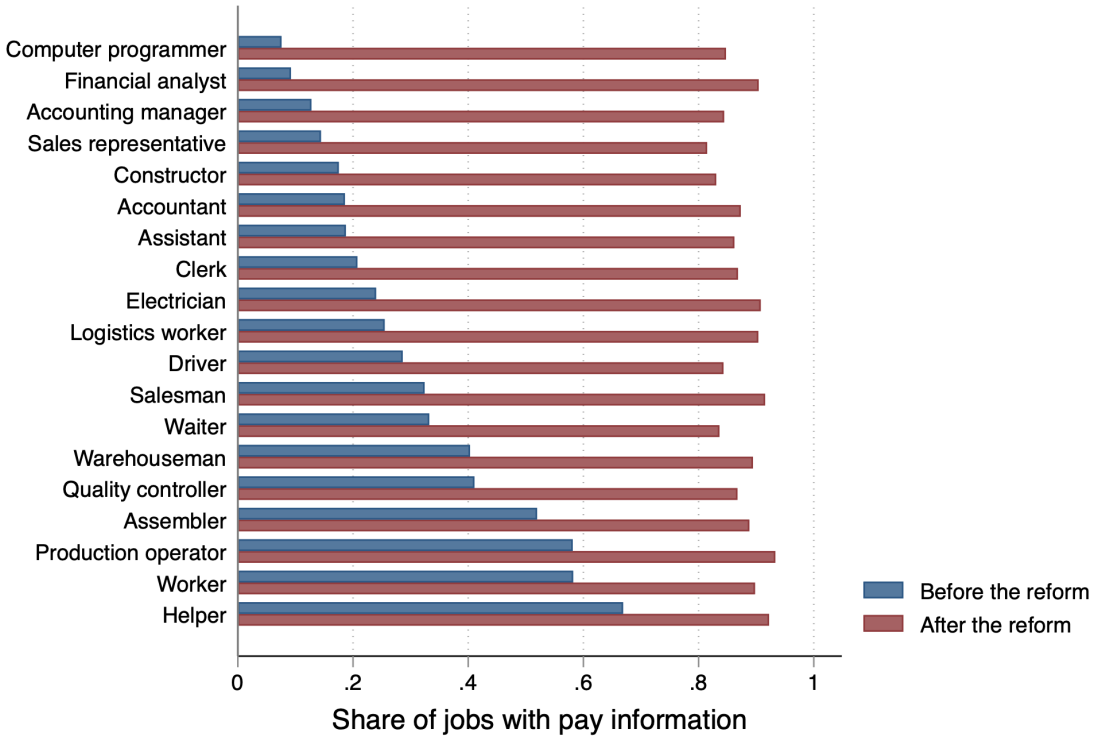
Notes: Fixed effects included in the specifications are indicated at the bottom of the table. The outcome variable, the number of ads, is in each specification aggregated at the same level as included fixed effects. Standard errors are clustered at the firm level in specification (2), (4) and (5) and at job title level in the specification (3). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.2 Heterogeneity by firm and job characteristics

The overall impact of the reform on job ads in the previous section might mask important heterogeneity at the firm and job level. In particular, not all jobs or firms might have been equally affected since some firms differed in their job posting strategies already before May 2018. For example, the survey in Hall and Krueger (2012) shows that there is variation in bargaining and wage posting for observationally similar workers, even after conditioning on a rich set of controls. This finding also motivated a small number of theoretical models that allow for heterogeneous wage-setting strategies such

as in Michelacci and Suarez (2006), Flinn and Mullins (2021) and Cheremukhin and Restrepo-Echavarria (2021). My job board data allows to explore this heterogeneity before the reform across multiple dimensions and thus construct a measure of "exposure" to the reform: Firms that always resorted to providing pay information on the platform (and committed to the advertised pay) would not be affected directly by the reform at all. Firms that never provide pay information have to change their behavior for any job ad they post.

Figure 4: Share of jobs with pay information by job title, before and after the reform



Notes: The figure shows the share of jobs with pay information for 20 most frequent job titles.

In Figure 4 I show that the presence of pay information is strongly dependent on the job title. For clarity I plot only the 20 most common job titles used by job ads, ranked from the lowest to the highest share of job ads. In some job titles, such as programmer or financial analyst, pay information is relatively rare, with share of job ads with pay information lower than 10%. In others, about two thirds of job ads may explicitly mention pay. The share of job ads with pay information jumps after the reform to very similar levels above 90% across different job titles, which indicates that the missing job ads with pay information in the data are more likely to be a result of improperly entered data rather than a non-compliance. It is also notable, that there is a clear rela-

tionship between share of job ads with pay information and the nature of the job title: pay information is relatively common in low-skilled manual jobs, somewhat common in routine non-manual jobs and rare in abstract jobs. This ranking translates also to average pay that is being advertised. In Figure A.5 I plot averages of posted pay for 200 most common job titles by their share of pay information, separately for pre-reform and post-reform period. There is a clear negative relationship between posted pay and presence of pay information both before and after the reform. In fact, visually there is little evidence that within job titles the average posted pay would change as a result of the reform. This appears to be the case regardless of the prevalence of pay information before the reform, even though overall the number of job ads with pay information quadrupled.

4.3 The effect on other job ad characteristics

I examine if the reform changed the composition of job ads or their characteristics. Specifically, first in Figure A.8 I look at differences in sectoral composition of job ads of firms, before and after reform. Only one of the 16 estimates is statistically significant and all are within 2 percentage points difference, suggesting little difference in composition. In Figure A.8 I also report changes in requirements of job ads. For complying firms, it become more common to mark the job ad as an entry job and require no specific education, and less common to mark it as full-time job. The opposite is true for always-taking firms. This suggests that pay transparency mandate made the job ads more alike than before the reform.

5 Job Search and Pay Information

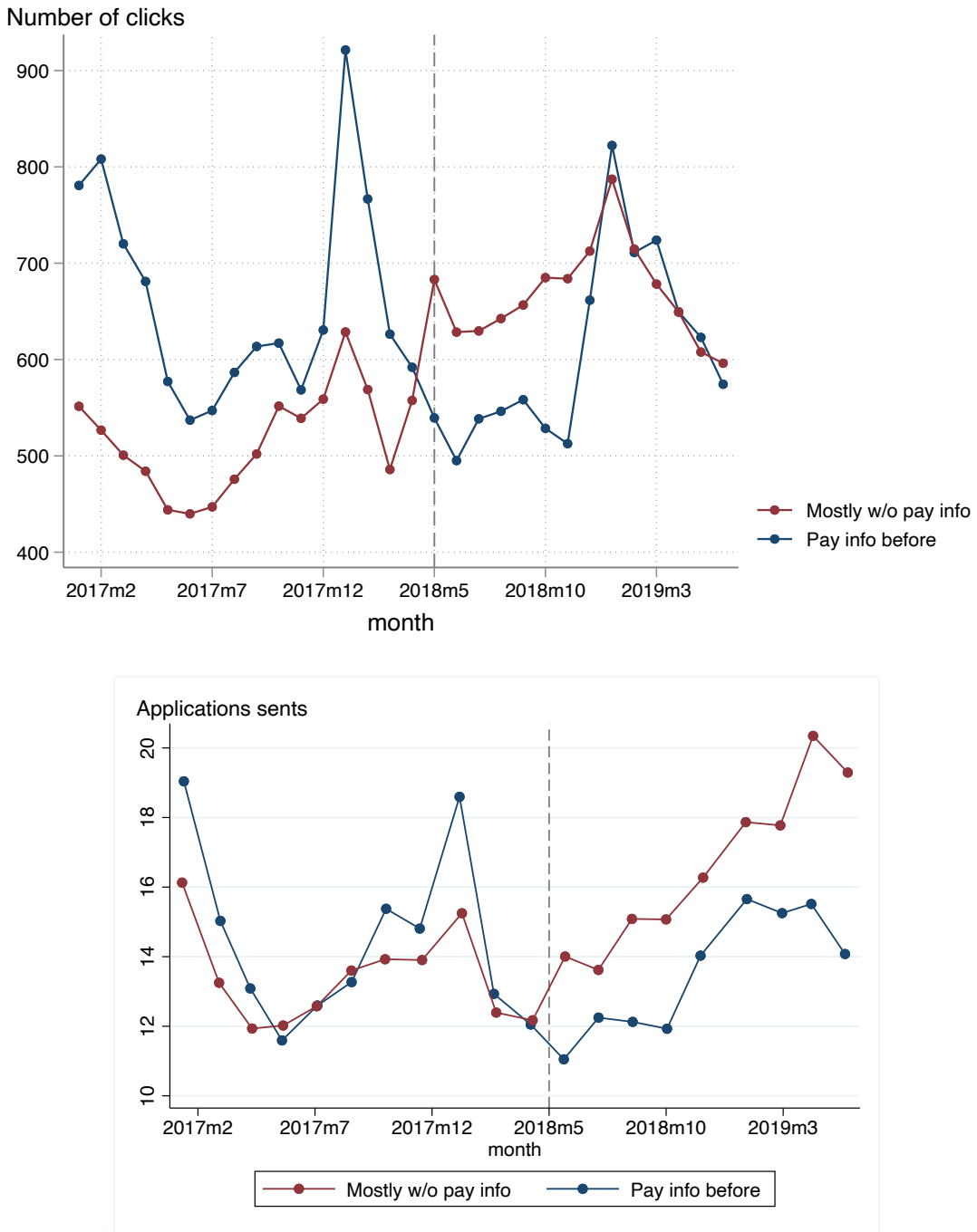
So far I have been describing only behavior of firms. Now I turn to job seekers and how the transparency reform affected their search behavior. For this purpose I will adopt the definition of firms motivated by Figure 1.

I find that the reform changed the relative attractiveness of jobs advertised by these two groups of firms. Before the reform, firms that were using pay information more frequently were receiving, on average, more ad views and applications. With the mandate, this relationship reverts.

Figure 5 shows the monthly evolution of ad views and applications for both groups of firms around the reform date.

Furthermore, I investigate how the pool of applicants changed with respect to the reform. I find that applicants to firms that had to include pay information have lower self-reported reservation wages (see Figure A.9) and are less likely to match exactly the

Figure 5: Interest in job ads by firms with and without pay information



Notes: The upper panel shows the evolution of the number of clicks (i.e. views) of job ads for firms that before the reform mostly did not include pay information and for firms that did. The lower panel shows the number of applications sent. The dashed line indicates the time of reform (May 2018).

job title specified by the job ad (see Figure A.10). However, there does not seem to be discrepancy in required level of education and self-reported education of an applicant. Together, this indicates that the reform made discovery cheaper for some job seekers as they apply to jobs they would previously ignore.

6 The Effect of the Reform on Wages and Hiring

Finally, I focus on hiring decisions and wage setting that is determined jointly by firms and workers. First, I start by defining the sample of new hires. Since renegotiating of contracts takes time and reform is not very informative about final wages, I do not expect an effect on impact for incumbent workers. Instead I focus on new hires, which I define as workers that moved to the firm either from unemployment or a different firm and stayed at the new firm at least 3 months. I use wages in the second month of employment, since the data is collected monthly and the first (or the third) month of employment could be otherwise incomplete. I then estimate the effect of the reform by comparing wages of new hires at firms *before* and *after* the reform, using an interrupted time series design. Specifically, for the impact on wages I estimate the equation

$$\log(wage_{it}) = \alpha + \beta_0 After_t + \beta_1 PayInfo_{j(i)} + \beta_2 PayInfo_{j(i)} \times After_t + \gamma_{y(t)} + \delta_{m(t)} + \varepsilon_{it}, \tag{2}$$

where $After_t$ is a dummy variable equal to 1 after the reform (May 2018) and 0 otherwise, $PayInfo_{j(i)}$ is an indicator for firms that mostly included pay information in job ads before the reform, and $\gamma_{y(t)}$ and $\delta_{m(t)}$ are year and calendar month fixed effects. In the baseline specification I omit the $PayInfo$ terms. I cluster standard errors at the firm level, to allow for correlation over time. My coefficient of interest is β_0, capturing the change in log wages after the reform; in specifications with the $PayInfo$ terms, β_0 captures the effect for complying firms and β_2 the difference for always-taking firms. Figures 6 and 7 additionally show monthly averages with separate linear fits before and after the reform. Analogously I estimate the effect of the reform on other outcomes, though my focus is primarily on wages of new hires. I find a statistically significant increase in wages of new hires by about 3% (Table 5, column 1), which is also demonstrated in Figure 6. Figure 8 splits firms by the number of new hires. The effect is concentrated in firms that previously did not include wage information (i.e. 77%), for firms that did include the wage information the estimated effect is close to zero ($0.032 - 0.038 \approx -0.006$) and the difference relative to complying firms is not statistically significant (see column 2 of Table 5 and Figure 7).

To validate my design I also perform a placebo check with jobs in education and

Table 5: Effect of the reform on log wages of new hires

	(1) All	(2) All	(3) Educ. & Gvt.	(4) # hires <10	(5) # hires ≥10
After	0.028*	0.032*	-0.062	0.013*	0.052*
	(0.012)	(0.013)	(0.043)	(0.006)	(0.025)
PayInfo		-0.264***	-2.098***	-0.230***	-0.298***
		(0.042)	(0.438)	(0.018)	(0.075)
PayInfo × After		-0.038	-0.083	0.027*	-0.113
		(0.029)	(0.127)	(0.013)	(0.067)
Avg. Outcome	6.62	6.62			
Year and month FE	yes	yes	yes	yes	yes
Adj. R2	0.01	0.03	0.42	0.03	0.03
N	691594	691594	25129	344928	346666

Notes: The outcome is the log wage of new hires in their second month at the new firm. *After* is an indicator for the post-reform period, *PayInfo* an indicator for firms that mostly included pay information in job ads before the reform. Columns (1), (2), (4) and (5) exclude education and government; column (3) is restricted to them. Columns (4) and (5) split firms by the number of new hires. All specifications include year and month fixed effects. Standard errors, clustered at the firm level, in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

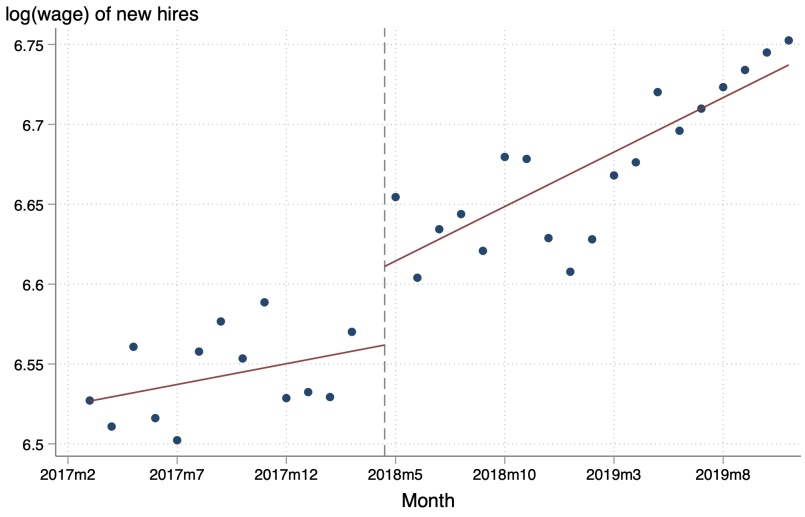


Figure 6: The effect of the reform on wages of new hires

government that have fixed pay scheme and therefore no scope for adjusting wages. I find small, statistically insignificant decrease in wages (column 3 of Table 5 and Figure A.2 in the Appendix). Similarly I perform placebo checks by moving the date of the reform to different, artificial dates. The results of these checks are more mixed: some placebo dates, particularly in 2017, also yield positive estimates of similar magnitude, so the size of the estimated effect should be interpreted with some caution.

In line with my predictions, I find only a small increase in wages among incumbent workers of about 0.8%. I also do not observe differential effects on wage increases for men and women (see Figure 9). This is notable as some literature (e.g. Roussille 2022) indicates that lack of wage information combined with different proclivity to bargain could contribute to persistent gender wage gap differences.

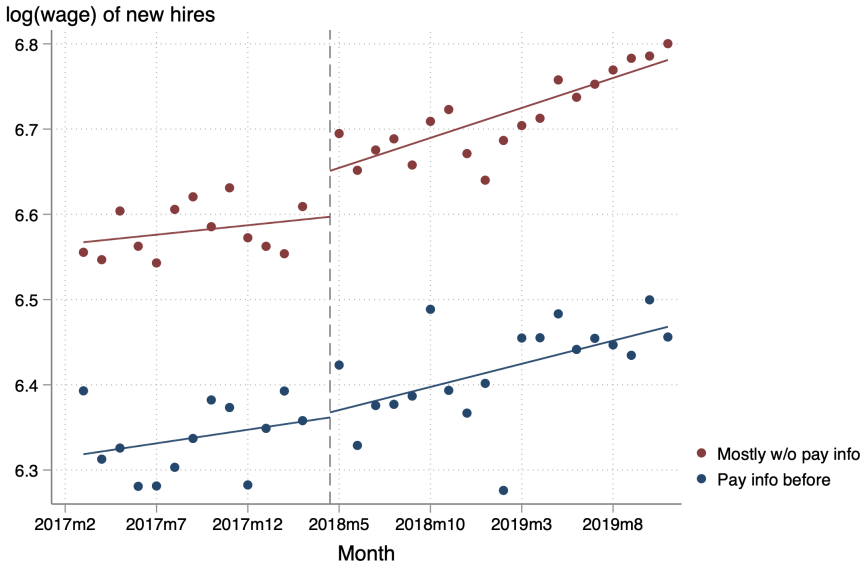


Figure 7: The effect of the reform on wages of new hires, by pre-reform use of pay information

I also examine whether sorting of workers changes after wage information becomes available. I find that job seekers start to apply more broadly, more often sending their applications to firms in different sectors or to ads with different job titles. However, I do not find that these applicants would match educational or skill requirements worse than before the reform. However, I find that workers that actually end up being hired (and with wages higher than in the absence of the reform) were not themselves paid higher in their previous employment (see Figure A.3 in the Appendix). In other words, the evidence suggests that the increase in wages is not an effect of sorting but rather affects bargaining process itself.

Interpreted through the lens of a search model, it is hard to rationalize these empir-

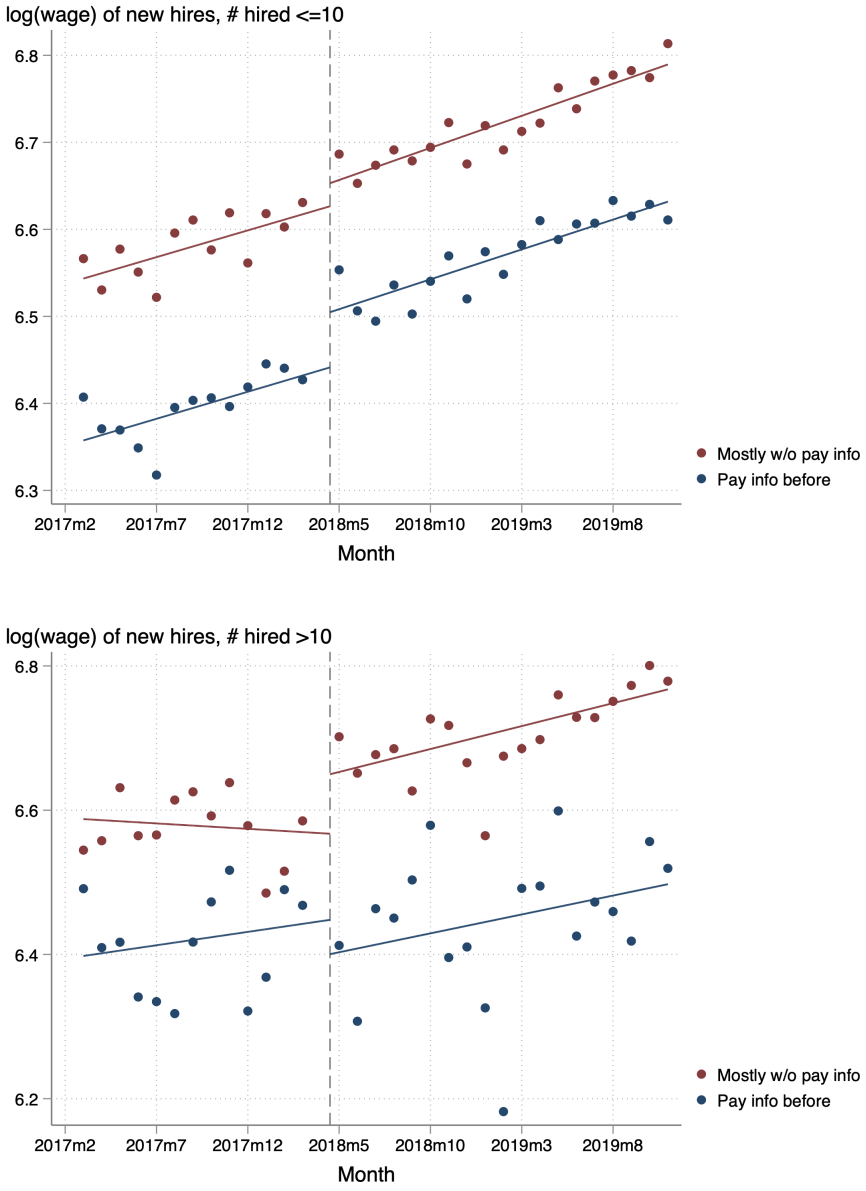


Figure 8: The effect of the reform on wages of new hires, by number of hires

Notes: The upper panel restricts the sample to firms with at most 10 new hires, the lower panel to firms with more than 10 new hires. The dashed line indicates the time of reform (May 2018).

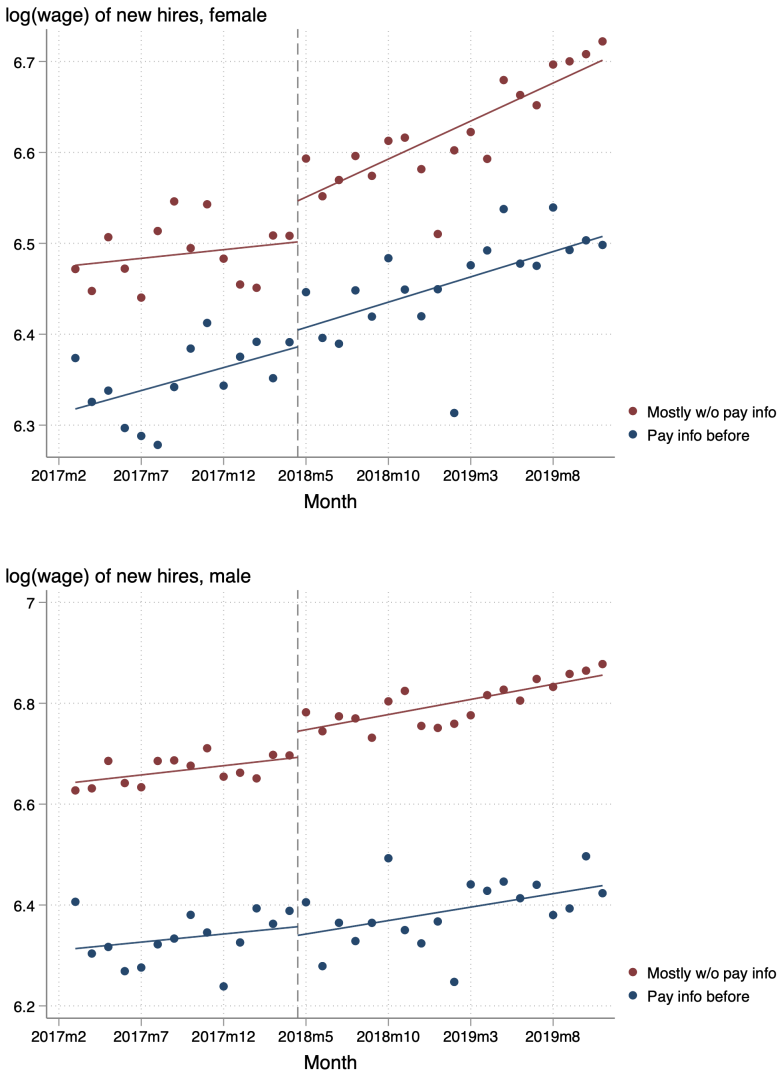


Figure 9: The effect of the reform on wages of new hires, by gender

Notes: The upper panel shows log wages of newly hired women, the lower panel of newly hired men, separately for firms that before the reform mostly did not include pay information and for firms that did. The dashed line indicates the time of reform (May 2018).

ical findings as they do not easily fit comparative statics corresponding to an increase in matching efficiency or just an increase in bargaining power parameter or an outside option.

7 Discussion of results from Austria and Colorado

As mentioned in the introduction, Frimmel et al. (2022) find no effect of the Austrian reform on wages overall. However, there is a small but statistically significant decrease in the gender gap driven by an increase in female wages and a decrease in male wages. The effect on female wages becomes stronger and statistically significant for subset of vacancies that indicate early starting date of the vacancy. Arnold, Quach and Taska (2022) use Burning Glass data to study the introduction of 2021 Colorado law that required job postings to contain expected salary information. Unfortunately, job ads from Burning Glass are not linked to actual wages and therefore allow only to study changes in posted wages for firms that already provided pay information. They find an increase in posted wages, but also document substantial non-compliance with the transparency law.

My paper differs from this work as my focus is not on gender wage gap nor change in posted wages *per se* but more generally on the role of pay information in hiring and I interpret the findings through the lens of the search and matching framework. For this purpose I use job board data on applications and applicants profiles as well as data on the firms' side, including their financial information. Similar information is not available in the case of Austria or Colorado. However, the findings complement each other in several meaningful ways. The difference in institutional setting between Slovak and Austrian labor market could account for quantitatively different findings. Unlike Austria, Slovakia's labor market is not characterized by high degree of unionization or dominated by collective industry-level agreements and therefore should give rise to a higher degree of individual wage bargaining. Furthermore, Frimmel et al. (2022) use data from a public job board data that may over-represent public administration jobs with rigid pay schedules. Together this could account for the overall seemingly unchanged wages in Austria. Additionally, the gender pay gap for women without children is considerably lower in Slovakia than in Austria, which could explain why the reform in Austria seemed to operate to some extent through increases in female wages rather than male wages, while in Slovakia I do not find differences by gender for the effect of the policy. With respect to Arnold, Quach and Taska (2022), it is notable that compliance was much lower than in Slovakia. This appears to be attributable to different sanction mechanisms used in Colorado and Slovakia: in Colorado individuals first have to inform the Colorado Department of Labor about non-compliance before

the authorities ask the non-complying employers to rectify the issue. Employers can therefore easily avoid fines, which are also an order of magnitude smaller in comparison to Slovakia.

8 Interpreting empirical findings

In the previous sections I have provided reduced form empirical analysis that already yield multiple interesting results that shed new light but also raise questions about our understanding of job search and matching. The goal of this section is to use this empirical evidence from an unique policy change to inform theoretical labor search models. As I further argue, the observed empirical patterns cannot be easily reconciled with baseline labor search models.

Of course, in case of fully directed (competitive) search models, this is a consequence of the central role that wage information is assumed to play. These models feature wage-posting and therefore all job seekers are assumed to have full information about firm's posted contracts and the ability to choose among them. In contrast, in random search models the role of wage information is extremely limited because search occurs before wage-setting. Offered wages therefore cannot influence patterns of search as long as the search is worthwhile for the unemployed (i.e. the expected value of search is above their reservation wage).

Recently, there has been a growing interest in modelling search behavior that is only partially directed (Cheremukhin and Restrepo-Echavarria, 2021; Menzio, 2007; Moen and Lentz, 2017; Pilossoph, 2012) , blending elements from random and directed search frameworks. However, in these studies wages are still either set exogenously or isolated from search behavior. One important exception of a partial directed search, where wages directly depend on search behavior is Wu (2020). In this section, I will adapt a modified version of his model that allows for a natural interpretation of the reform that sheds more light on my key empirical findings. For easiness of exposition, I will adapt a simplified version of the model with 2 firms and 2 workers. The main insights remain unchanged in a full model with atomistic firms and workers.

8.1 Model setup

There are two workers ($i = 1, 2$) and two firms ($j = l, h$) trying to match a single worker to a single vacancy. Firms differ in productivity ($z_l < z_h$). A firm with a filled vacancy produces z_j and pays the wage w_j to the worker. Otherwise, an unmatched worker receives outside option b and an unmatched vacancy does not produce any output. The timing is as follows:

(i) both firms post vacancies with wages w_l and w_h,

(ii) workers, seeing the posted wages, choose probabilities q_i^j of applying to each firm, such that $q_i^l + q_i^h = 1$,

(iii) firms receive 0, 1 or 2 applications, and either the vacancy stays vacant, the single applicant is offered the job offer, or one of the two applicants is randomly offered the job, respectively for each case,

(iv) the decision whether to accept the firm's offer is made by workers.

The innovation of Wu (2020) is that when workers choose the probabilities of applying to each firm they face a cost of directing search. If the worker does not direct her search, she will apply to each firm with the probability $q_l = q_h = 1/2$. But the worker can distort these probabilities by directing the search towards a specific firm, however, at a cost. The cost function captures that the more the worker departs from equal probabilities, the higher cost she faces. Furthermore, Wu (2020) shows that the game with observable posted wages with the cost of directing search is equivalent to a game, where wages are not fully observed but workers can pay a cost of learning more accurately about payoffs of applying to different firms. The bigger is the decrease in uncertainty, the bigger is the cost. Therefore, the setting can be restated as a setting with rational inattention, which makes the model very elegant and grants a natural interpretation to the cost of directing search.

To capture the cost of distorting the search, the original setting uses Kullback-Leibler divergence to measure the difference between the chosen probabilities (q_l, q_h) and $(1/2, 1/2)$, which corresponds to random search. However, this measure maintains symmetric marginal costs for all firms. Departing from the original model, I will use a modified cost function, which in contrast to Wu (2020), differentiates parameters c_l and c_h to capture that the two firms have different marginal costs of directing search. Additionally, there is a fixed cost of applying, C. This cost function allows me to treat the transparency reform as decrease in marginal cost of directing search towards only one type of firms, rather than a general improvement in search technology. Therefore, the new cost function takes the form:

$$\text{Cost of directing search} = c_l q_l \log \frac{q_l}{1/2} + c_h q_h \log \frac{q_h}{1/2} + C. \tag{3}$$

Together with the microfounded interpretation of directing search as a discovery of firm's wages, a decrease in c_h can be interpreted as decreasing uncertainty about wages of firms with productivity z_h, while leaving uncertainty about wages paid by firms with productivity z_l unchanged. This model with partial search can therefore allow for a type of wage transparency policies, such as the one studied in this paper,

that are completely outside of scope of both random and directed search models. The equilibrium is defined identically as in the original model, i.e.

Definition 1 (Equilibrium in the 2×2 Game)**.** *An equilibrium is* $\left(q_j^i\left(w_l, w_h\right), w_l^e, w_h^e\right)$ *such that:*

1. *(Firm)* w_j^e *maximizes profit of the firm* j *, given application probabilities* $q_j^i\left(w_l, w_h\right)$ *and wages set by the other firm* $w^e{}_{-j}$*.*
2. *(Worker) Application probability* $q_j^i\left(w_l, w_h\right)$ *maximizes payoff of the worker* i*, given posted wages* $\left(w_l, w_h\right)$ *and application probability of the other worker* $q_j^{-i}\left(w_l, w_h\right)$*.*
3. *(Symmetry)* $q_j^1\left(w_l, w_h\right) = q_j^2\left(w_l, w_h\right)$*, such that workers adopt symmetric, non-coordinated strategies.*

The equilibrium definition rules out non-symmetric equilibria to avoid strategies where workers' application strategies would be coordinating among themselves and eliminate search frictions. The worker maximizes expected payoff, taking into account posted wages $\left(w_l, w_h\right)$, by choosing application probabilities q_l and q_h to solve the following problem:

$$\max_{q_l, q_h \in [0,1]} q_l \left(1 - q_l^{-i} + \frac{q_l^{-i}}{2}\right) \max\left\{w_l - b, 0\right\} + q_h \left(1 - q_h^{-i} + \frac{q_h^{-i}}{2}\right) \max\left\{w_h - b, 0\right\}$$

$$-c_l q_l \log \frac{q_l}{1/2} - c_h q_h \log \frac{q_h}{1/2} - C,$$

s.t.

$$q_l + q_h = 1.$$

The probability of receiving the offer from firm with the productivity z_l is the probability of applying to the firm times the probability of the other worker not applying $q_l \times (1 - q_l^{-i})$ plus the probability of both workers applying and being selected at random $q_l \frac{q_l^{-i}}{2}$. If selected, the worker can either accept the offer w_l or decline it and take the outside option b. Analogously for firm with productivity z_h. Taking the first order condition, applying symmetry of workers ($q_j^1 = q_j^2$) and further rearranging yields:

$$\log \frac{(q_l^i)^{c_l}}{(q_h^i)^{c_h}} + c_h - c_l - \log 2^{c_h - c_l} = \left(q_h + \frac{q_l}{2}\right)(w_l - b)^+ - \left(q_l + \frac{q_h}{2}\right)(w_h - b)^+$$

$$q_l^i + q_h^i = 1.$$

This equation characterizes subgame Nash equilibrium in the second stage. At the optimum, workers equalize the marginal benefit of applying to firm l to the marginal benefit of applying to firm h. The role of cost of directing search should be highlighted: the model nests both random search and directed search as special cases. With both c_l and c_h tending towards zero, the worker will apply only to the more lucrative one, i.e. the search will be directed, and with c_l and c_h tending towards infinity the search becomes random. However, decoupling of c_l and c_h allows to consider also more general combinations.

Finally, this modification of the cost function does not affect existence of an unique subgame perfect equilibrium as the first stage is unchanged relative to the original model. Therefore in equilibrium, the wage w_j^e will maximize profit of the firm j given the w_{-j}^e if and only if

$$w_j^e = \arg\max_w \left[1 - (1 - q_j)^2\right] (z_j - w),$$

s.t.

$$q_j = Q\left(w; w_{-j}^e\right),$$
$$w \geq b.$$

and $\left(q_l^e, q_h^e\right)$ is the outcome of the subgame equilibrium given $\left(w_l^e, w_h^e\right)$:

$$q_j^e = Q\left(w_j^e, w_{-j}^e\right).$$

8.2 Comparative statics

An analytical solution cannot in general be obtained, therefore I focus on the qualitative comparative statics corresponding to the studied reform. In my empirical setting the reform effectively reduces this cost for one type of firms but not for others as it becomes cheaper for workers to learn the relevant wage offers. Empirically, firms that pay higher wages and have, on average, higher AKM fixed effects are less likely to include pay information in the job ad. I will consider them to be the more productive firms in the model, represented by the firm h with $z_h > z_l$. Some existing literature (Flinn and Mullins, 2021; Michelacci and Suarez, 2006) attempts to understand why certain firms self-select into wage-bargaining as opposed to wage-posting. I will treat the fact that these firms are pursuing less transparent policies as a fact exogenous to the model since this question is beyond the scope of the paper. Moreover, in the opposite case the reform would have to be interpreted as directly manipulating an equilibrium object rather changing the equilibrium by manipulating deeper exogenous parameters. Therefore, I will examine below how does the 2×2 economy react to a decrease in c_h from an initial higher level to a level of c_l.

First, the reform makes directing the search to more productive firms cheaper for workers. More job seekers will start applying to the more productive firms. The queues become more unequal. This is in line with my findings as the relative number of applicants as well as ad views increases among complying firms after the reform.

Second, the decrease in c_h translates into more elastic application supply curve. This puts upward pressure on wages in productive firms. Decoupling c_l and c_h as considered above means that the change happens only for one type of firms, while for the other type application supply elasticity will remain the same. Indeed, I estimate a sizeable increase in log wages for complying firms.

Third, the decrease in the length of queues among less productive firms puts also upward pressure on wages in these firms. The competitive effects means that to lure the same number of applicants, less productive firms have to also increase wages. Conversely, for more productive firms, the effect is opposite. Despite the fact that the overall effect on low skill firms is theoretically positive, I find only limited evidence of positive wage spillovers among these firms. In contrast, wages in complying firms increase significantly. Together, the evidence points toward relatively small role of competitive effects as opposed to search friction effects discussed in the previous paragraph.

Fourth, the model abstracts from worker heterogeneity, therefore calibrations are silent on the quality of applicants. While I find a change in composition of pool of applicants, remarkably I find that actual hires do not seem to differ from hires before the reform. Therefore, the worker heterogeneity might indeed play only a limited role in reality. Similarly, the model does not feature unemployment (that would be different from queue length) nor firm entry but empirically the reform does not appear to affect either: turnover of employees did not change and the number of ads also remained rather constant.

8.3 Alternative explanations

While DMP-style models have been found to be extremely useful in capturing many aspects of labor search, they give no role to wage information and therefore the transparency reform considered in this paper cannot be easily related to comparative statics within a standard DMP model. However, it is worthwhile to think about other variables in the model that could capture the effect of the reform.

Most prominently, one could think of the reform as a change in bargaining power. If the reform affected market power of firms such that workers can effectively extract a larger share from the joint surplus it would be captured exactly by the bargaining power as a primitive for market power. The mechanism would work as follows. First, wages would mechanically increase. Second, firms would post fewer vacancies since

they would become less profitable to firms. Third, fewer workers would be matched, leading to an increase in unemployment. Therefore filling rates would increase as well. While the wages have increased, the other facts cannot be easily explained. Unemployment and vacancy postings were effectively unaffected by the reform. Furthermore, a simple DMP model cannot discriminate between bargaining power among complying and always-taking firms. In practice, the wage increase was much stronger among complying firms.

An alternative explanation could entail an increase in the outside option or in matching efficiency. For the case of the outside option, the mechanisms are very similar to the previous change. The main difference is that rather than a decrease in the firm's share of joint surplus, the joint surplus itself shrinks. With an increase in matching efficiency, the wages would increase but so would the vacancy postings. Unemployment would decrease and job filling rate increase. Again, the estimated changes do not support this interpretation.

9 Conclusion

In this paper I have evaluated a specific form of pay transparency reform that mandates firms to post wages in job ads. Contrary to other transparency policies, I find a sizeable effect of the policy on wages of new hires, with no differences between male and female workers. Armed with rich data on job search I document how the patterns of job search changed with respect to the reform and how firms reacted with their vacancy postings. I argue that these responses cannot be easily reconciled with standard job search models and instead I illustrate that a simple model of partially directed job search can qualitatively account for the empirical findings.

The reform was effective: the share of job ads with pay information rose from about one fifth to more than four fifths almost immediately, without any decline in the number of ads posted on the job board. Job seekers responded by turning their attention to complying firms, whose ads received more views and applications after the reform. These new applicants had lower reservation wages and were less likely to match the job title of the ad. Yet, the wage gains among new hires do not appear to be driven by positive selection, since newly hired workers were not better paid in their previous jobs. In the model, these patterns correspond to a decrease in the cost of directing search towards complying firms: job seekers can more easily discover their offers, which erodes the firms' monopsony power and puts upward pressure on wages. By contrast, an increase in bargaining power, in the outside option or in matching efficiency would also imply changes in vacancy postings or unemployment that I do not observe.

These findings have implications for the design of pay transparency policies. Man-

dating pay information in job ads operates through a different channel than pay reports on realized wages: it informs job seekers rather than incumbent workers, and it raised wages of new hires without detectable costs in terms of vacancies. At the same time, because the mandate sets only a lower bound on pay and leaves room for bargaining, it does not by itself narrow the gender pay gap.

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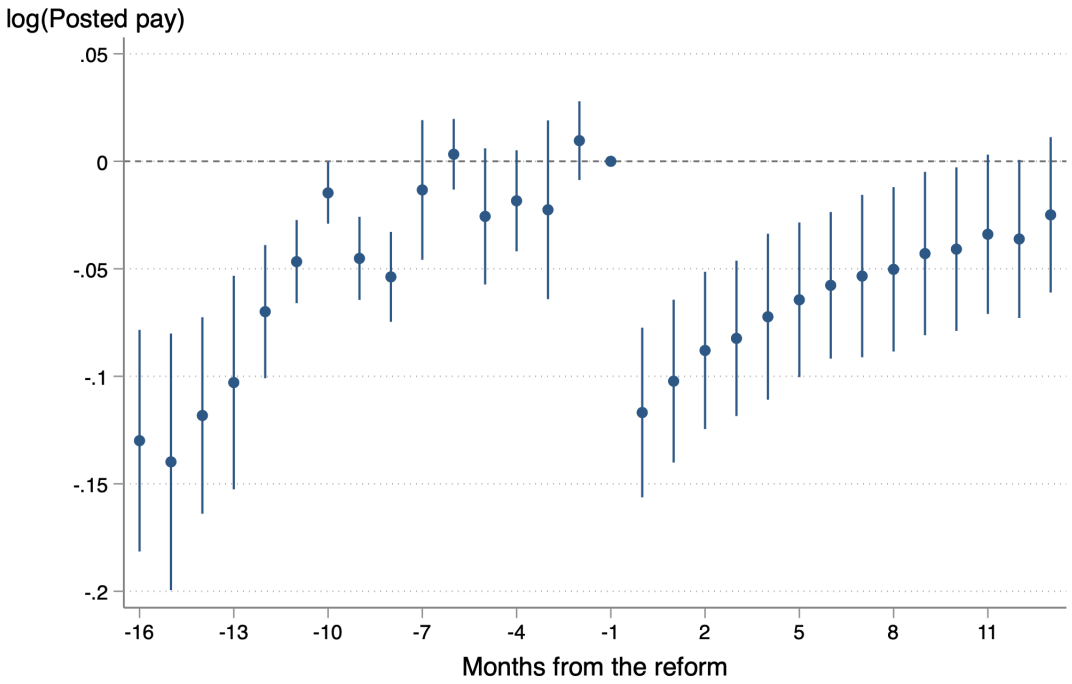
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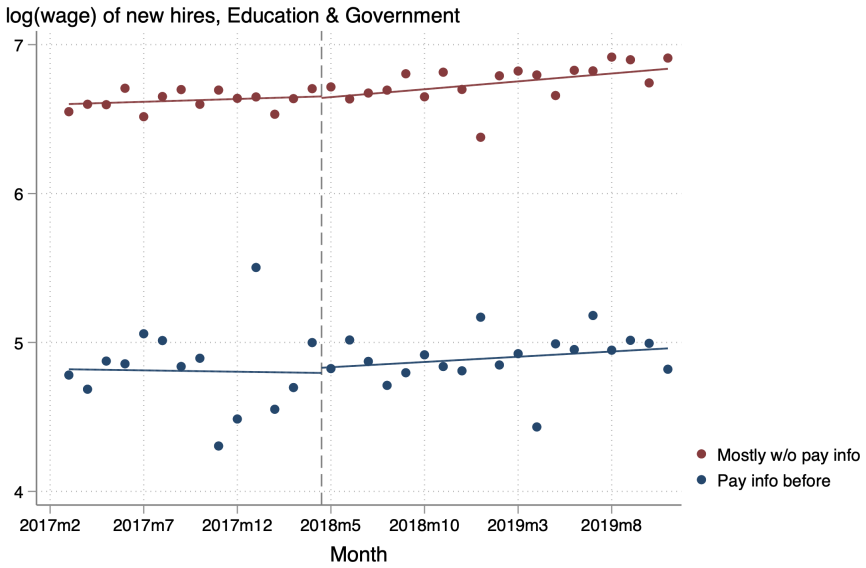
A Supplementary Figures and Tables

Figure A.1: Event study: Effect of the reform on posted pay



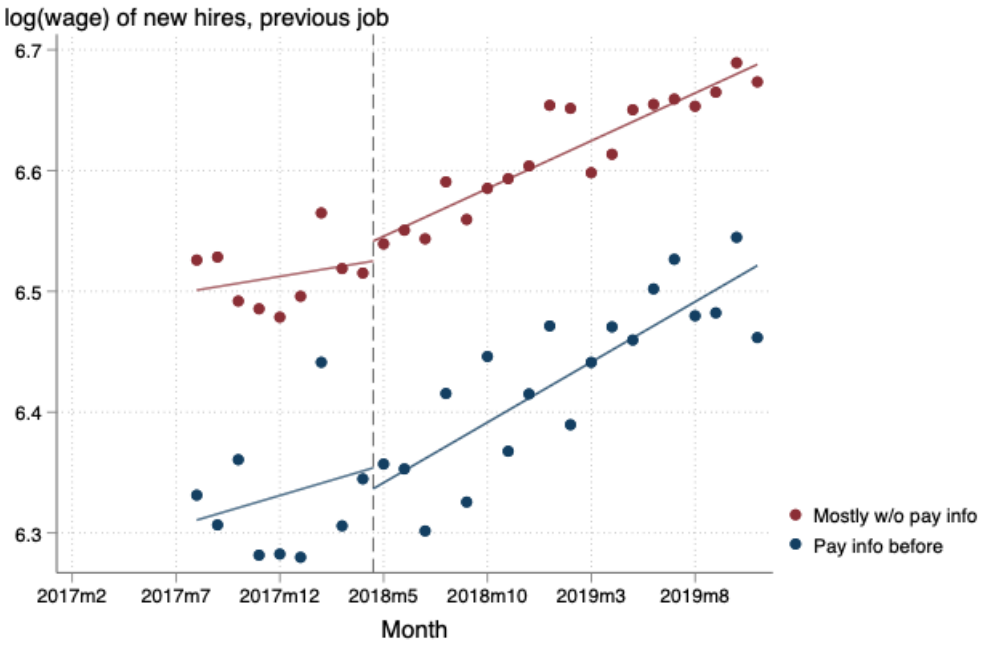
Notes: The graph plots event study coefficients from a regression of posted on the *month* and *year* fixed effects and *firm* $\times$ *job title* $\times$ *county* fixed effects. The omitted category is one month before the reform to show changes in log posted pay relative to the last pre-reform month. The standard errors are clustered at the firm level.

Figure A.2: Placebo: wages of new hires in education and government



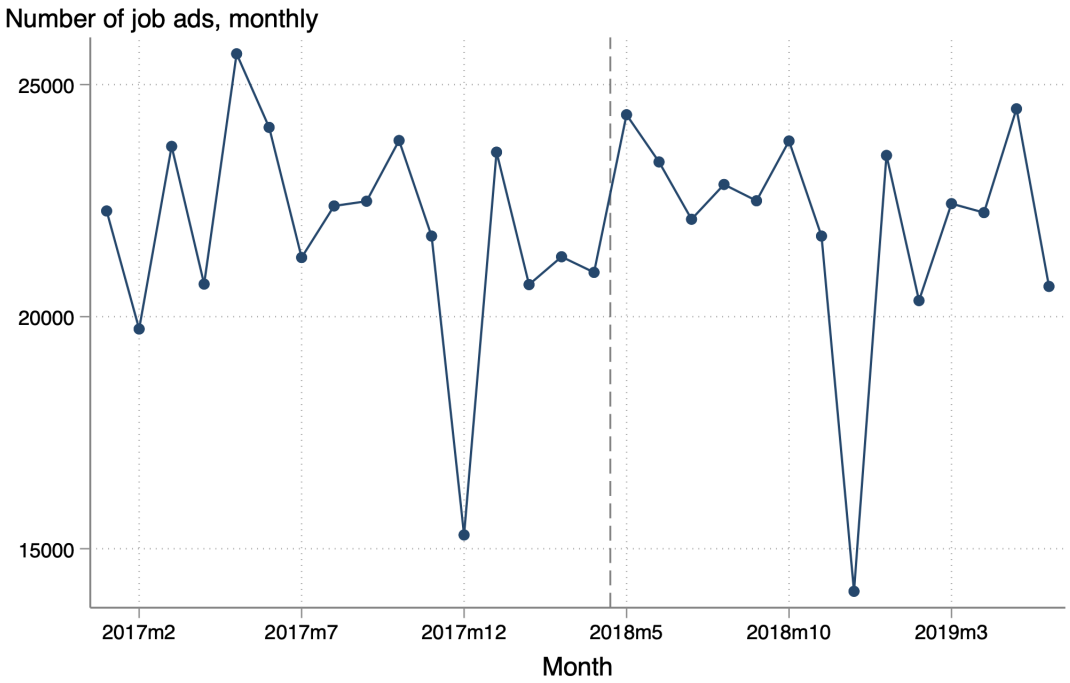
Notes: The graph shows log wages of new hires in education and government, where pay follows fixed pay scales, separately for firms that before the reform mostly did not include pay information and for firms that did. The dashed line indicates the time of reform (May 2018).

Figure A.3: Wages of new hires at their previous job



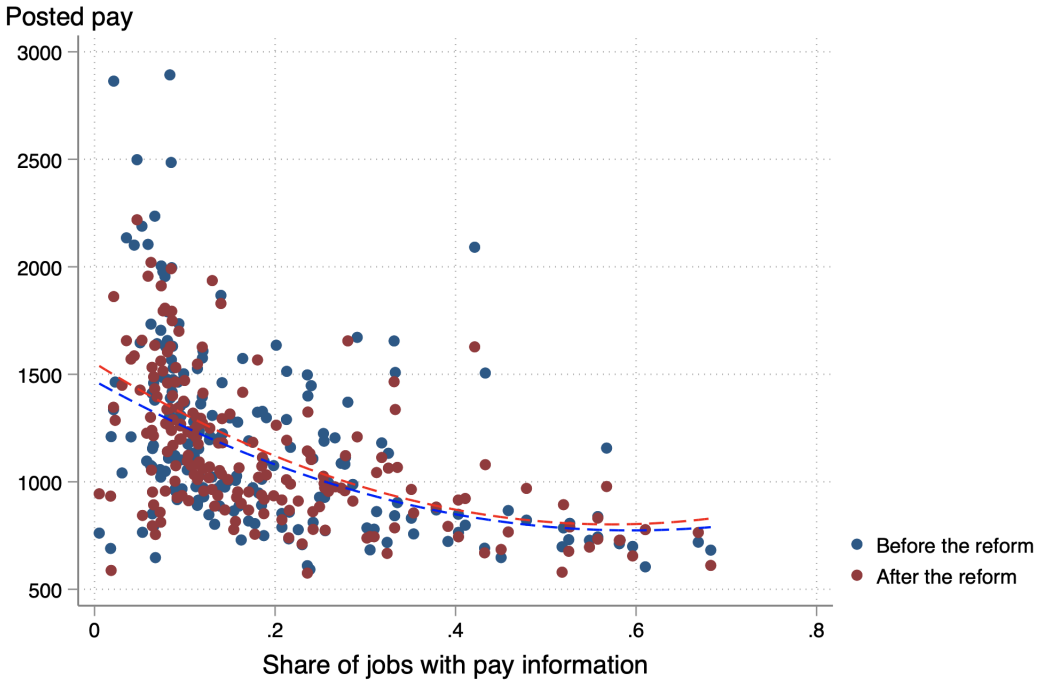
Notes: The graph shows log wages that newly hired workers earned at their previous job, by month of hiring, separately for firms that before the reform mostly did not include pay information and for firms that did. Because the administrative data start in 2017, previous wages are observed only for workers hired from mid-2017 onwards. The dashed line indicates the time of reform (May 2018).

Figure A.4: Monthly count of job ads



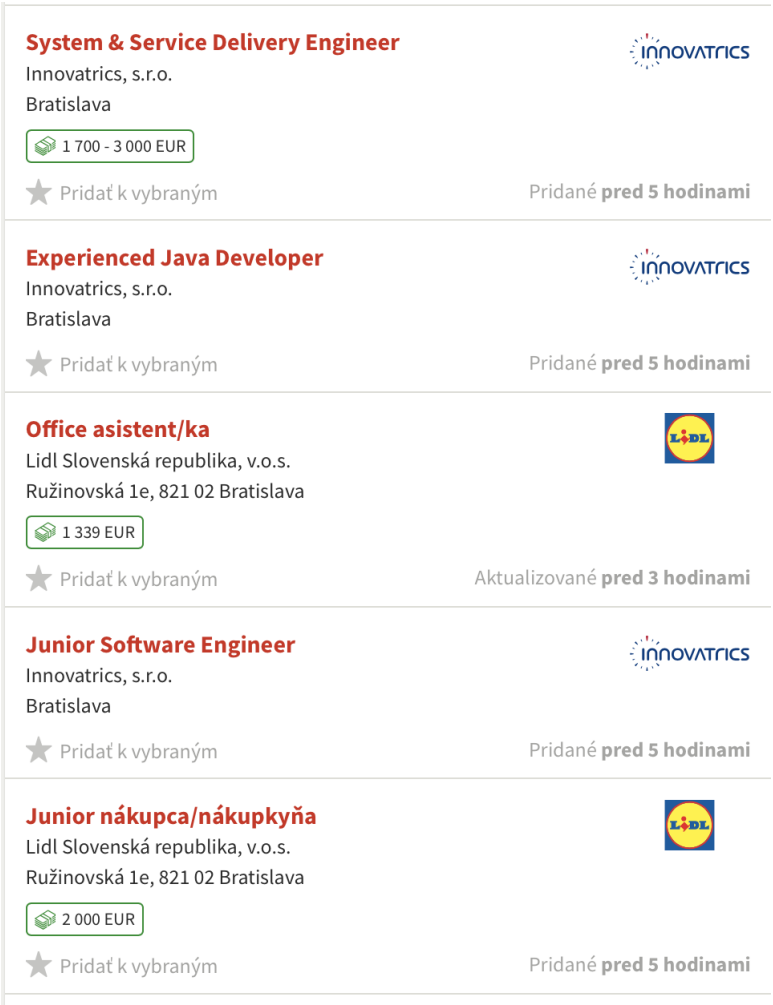
Notes: The graph plots raw count of job ads per month. The dashed lines shows the time of reform.

Figure A.5: Posted pay and prevalence of pay information by job title



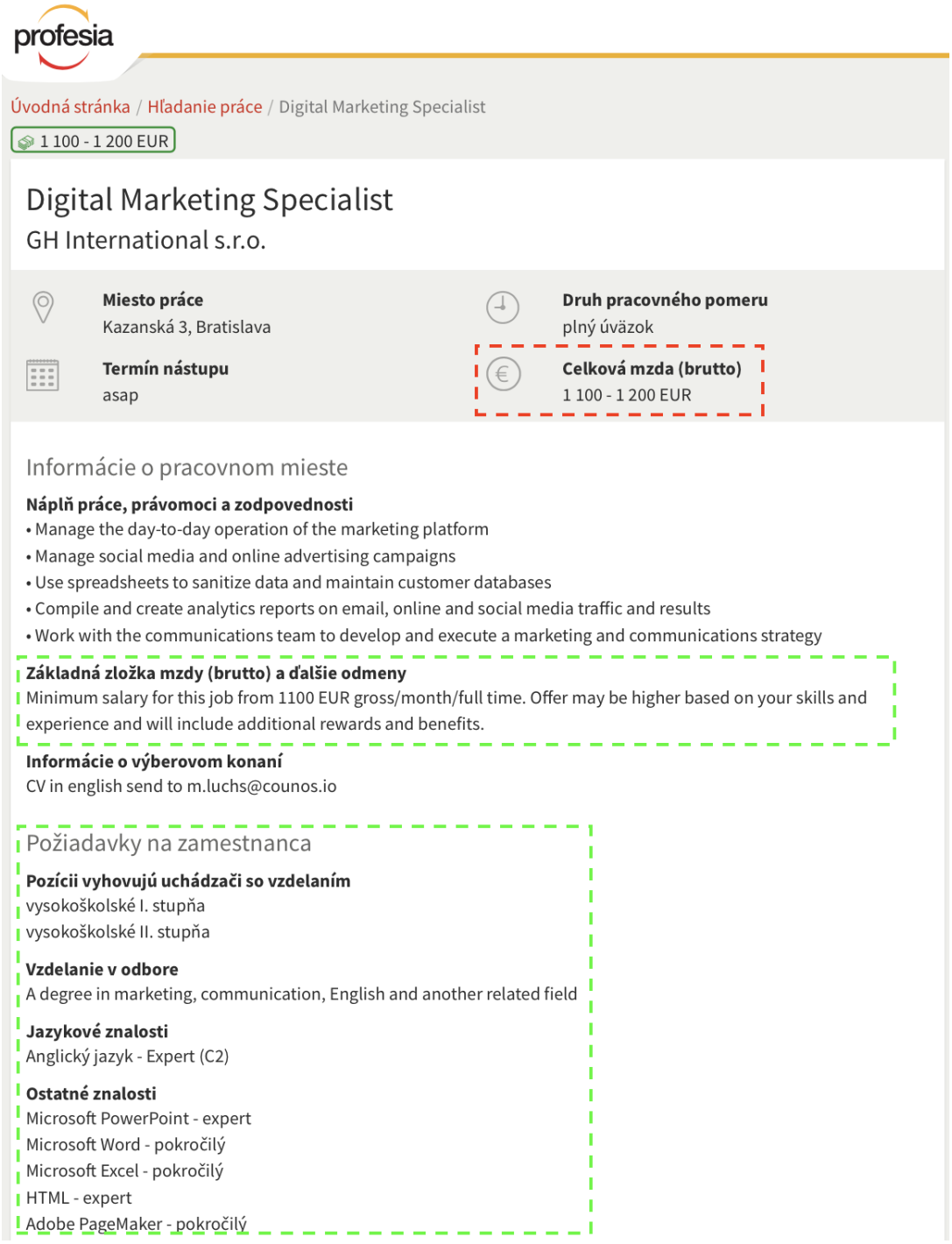
Notes: The graph shows average posted pay for 200 most common job titles by the prevalence of pay information in job ads, both before and after the reform. The dashed lines use linear regression with quadratic specification for the best fit, separately for the pre-reform and post-reform posted wages.

Figure A.6: Job board - list view



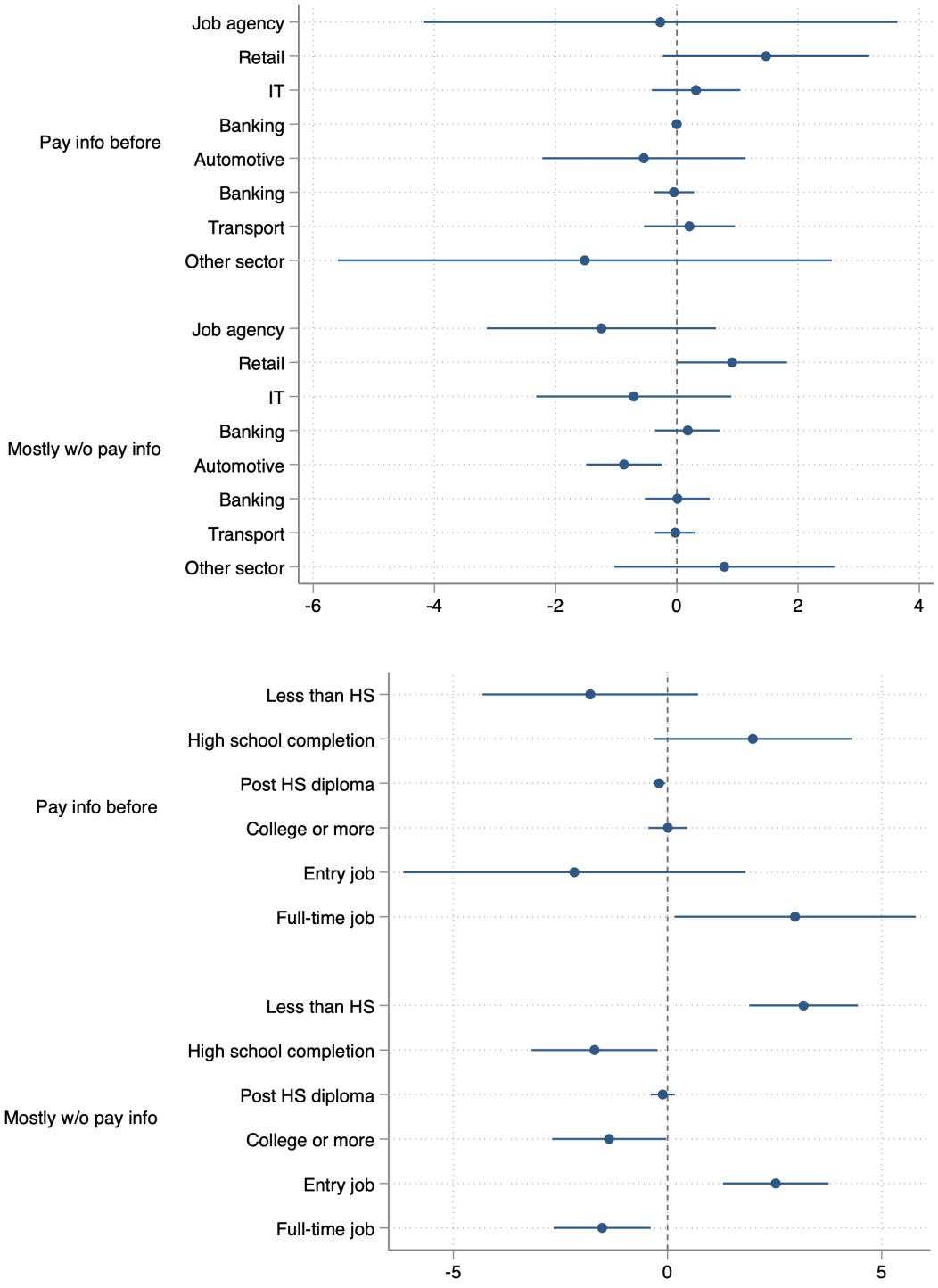
Notes: The figure shows a screenshot from the website with a list of job ads that are shown to job seekers. Before the reform, only some of them have pay information.

Figure A.7: Job board - ad view



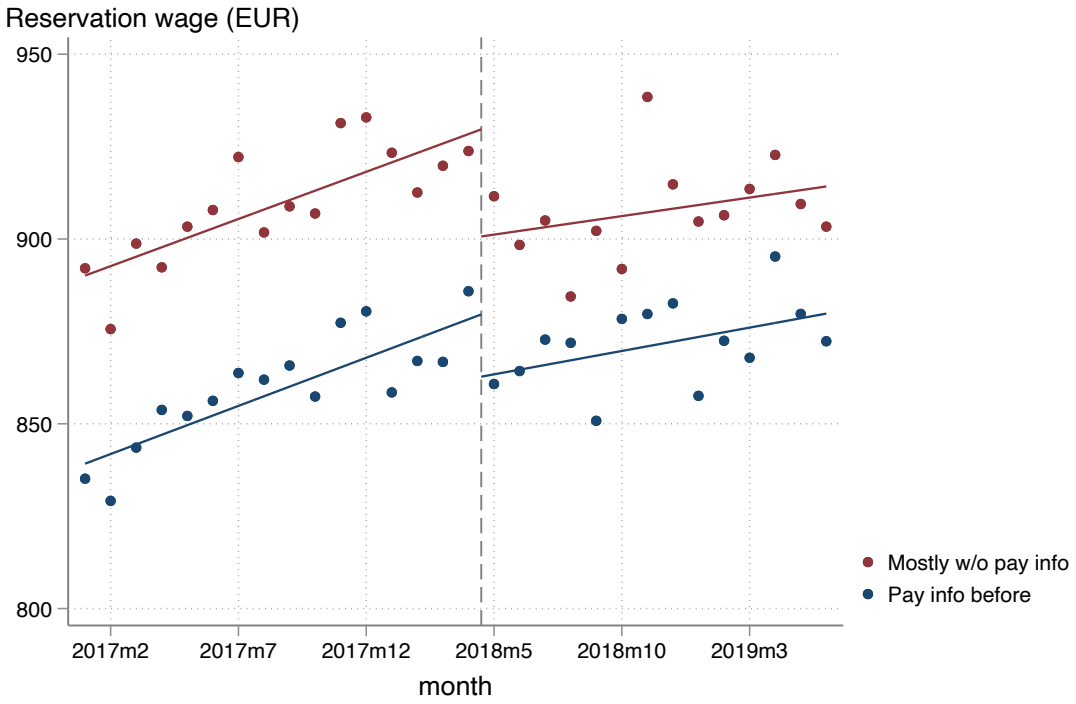
Notes: The figure shows a single job ad view with standard description on the job board. The green rectangle highlights education, language and skill requirements. The red rectangle shows pay information.

Figure A.8: Difference in job ads characteristics



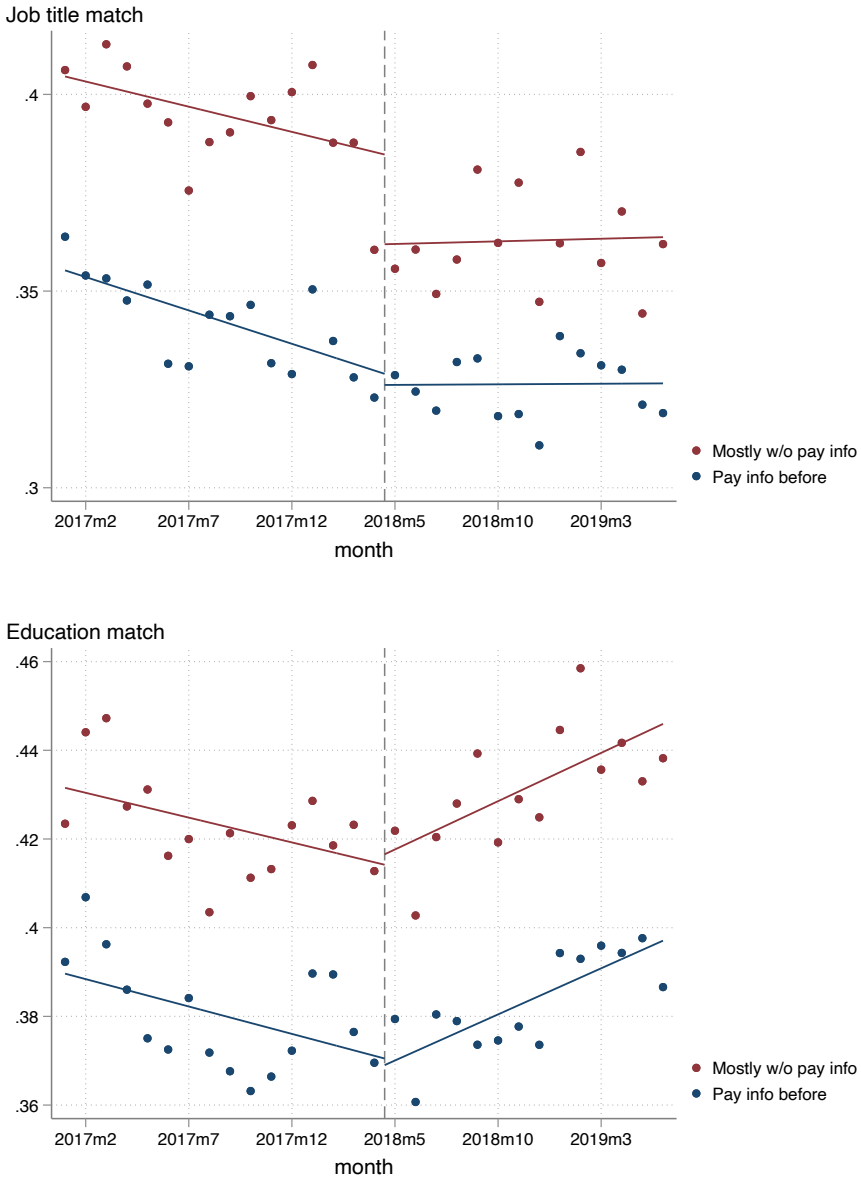
Notes: The figure shows differences in share of specific characteristics and sectors among job ads, relative to pre-reform period.

Figure A.9: Reservation wage of applicants



Notes: The dashed lines use linear regression with quadratic specification for the best fit, separately for the pre-reform and post-reform posted wages.

Figure A.10: Match of applicants to job ads



Notes: The dashed lines use linear regression with quadratic specification for the best fit, separately for the pre-reform and post-reform posted wages.

Table A.1: Top 10 sectors and bottom 10 sectors by prevalence of wage information before the reform

Top 10	%	Bottom 10	%
Veterinary medicine	51.5	Education	1.1
Job agencies	41.4	Energy	1.4
Restaurants	39.3	IT	4.5
Travel agencies	37.2	Insurance	5.0
Mining	34.2	Utilities	5.7
Repairshops	33.7	Accounting	6.2
Social care	31.2	Publishing	7.8
Manufacturing of paper products	30.7	Electrical eng.	7.8
Financial advice, insurance	28.9	Banking	8.2
Arts & culture	26.6	Chem. & pharma	8.2