

Do Financial Incentives for Firms Promote Employment of Disabled Workers? A Regression Discontinuity Approach

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Abstract

This paper studies the impact of employment quota on firms' demand for disabled workers. The policy obliges firms to provide at least one job to disabled workers upon passing a threshold firm size of 25 non-disabled workers, a rule which is strictly enforced by non-compliance taxation. Adopting a regression discontinuity design we find that employment quota lead to excess employment in firms just above the quota threshold; that the flat rate nature of the non-compliance creates stronger employment effect among low wage firms; that the employment effect of quota differs by industry; and that excess employment increases most for workers who have already worked within the firm when becoming disabled and to a lesser extent for workers who have worked for other firms or who were unemployed when becoming disabled.

JEL classification: J15, J20, J71, J78

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1 Introduction

Many countries aim to encourage employment of disabled workers for at least three reasons. First, a strikingly high number of about 14 % of working age individuals living in OECD member countries report having a health problem that is limiting activities of daily living in general (OECD 2003). Second, employment matters tremendously for the economic well-being of disabled individuals. The work incomes of disabled individuals with a job are nearly as high (86 %) as the work incomes of individuals without disabilities. In contrast, the financial resources available to disabled individuals without a job are considerably lower (54 %) than the disposable income of disabled individuals holding a job. Third, employment rates of disabled individuals are rather low: Whereas 70.8 % of non-disabled individuals are employed, only 43.9 % among the disabled individuals are.

This paper contributes to understanding the role of employment quota, an important tool in many countries that aims to improve the integration of disabled individuals into the labor market. While employment quota for disabled individuals have been in place since years in a number of OECD countries, empirical evidence estimating the causal effect of quotas on employment prospects of disabled individuals is almost non-existent. This paper contributes to closing this gap by evaluating the role of employment quota in Austria. Specifically, Austria obliges firms to hire at least one disabled individual as soon as they pass a threshold firm size of 25 non-disabled employees. Firms that do not comply with this obligation are subject to a flat-rate tax of about € 200 per month (or 6.6 percent of the average wage of Austrian employees) for each place not filled. We exploit the discontinuous change in financial obligations created by the employment quota to identify the causal effect of the non-compliance tax on the number of severely disabled workers employed by firms on the margin of crossing the threshold firm size. In particular, the employment quota implies that a firm with 25 non-disabled workers faces larger costs of not providing employment for a severely disabled worker than a firm with 24 non-disabled workers. In contrast, threshold firms are almost identical with respect to firm size, one of the most important determinants of the number of jobs offered to disabled individuals. The key idea of approach is to learn about the causal effect of the employment quota on threshold firms by using information on firms just below the threshold to learn about the counterfactual employment of disabled workers. Comparing the employment patterns of the two groups of firms, we can learn about the effect of employment quotas on the allocation of severely disabled workers across firms.

Our key identifying assumption is that firms do not endogenously select below the threshold to avoid becoming subject to the employment quota. We discuss the validity of this assumption by studying four indicators that would help detecting such self-selection. First, endogenous self-selection is expected to result in a discontinuity in the firm size

distribution. Our empirical evidence indicates that there is no such discontinuity. Second, if firms wanted to avoid becoming subject to the employment obligation, firms below the quota threshold grow slower than firms at the threshold. We find no such pattern in the data. Third, the populations of firms below and above the threshold are identical in terms of a range of observable characteristics. Finally, endogenous self-selection of firms prone to refrain from employing disabled workers would result in an dip in terms of the number of disabled workers employed just below the quota threshold. We do not see such a dip. Thus, a comprehensive set of indicators is consistent with the central identifying assumptions that firms just below the threshold provide valid information on the employment decisions of threshold firms without the quota system. This suggests that the employment quota creates a sharp regression discontinuity design.¹ The causal impact of employment quota can therefore be identified by comparing employment decisions of threshold firms to firms just below the threshold.

Our empirical analysis uncovers four important results. Our first result is that firms facing the financial obligation to employ severely disabled workers in fact do employ more severely disabled workers than they would without financial obligation. About 1 in 20 firms have a disabled worker on the payroll whom they would have not hired if the employment quota were not present. Second, the flat rate nature of the non-compliance tax is expected to generate a stronger financial incentive for firms that pay low wages than for firms that pay high wages. Consistent with this hypothesis, we find that firms' response to the flat rate non-compliance tax decreases monotonically with a firm's position in the firm-wage distribution. Third, our empirical analysis detects important differences of the impact of the non-compliance tax with respect to industry. Whereas firms in the services sector and in the construction sector display below-average responses to quota, firms in manufacturing react more strongly than average. This is consistent with the hypothesis that technology plays an important role for the extent to which firms can accommodate disabled workers. Finally, we also provide evidence on the type of individuals benefitting from additional employment. We find that about 50 % of the employment gain can be attributed to workers that had been employed by the firm already at the time of becoming severely disabled. About 42 % of excess employment can be attributed to workers who had been employed by other firms at the time of becoming severely disabled. The remaining 8 % of excess employment goes to individuals who had not been employed at the time of becoming severely disabled.

There are basically two different legislative approaches to promote employment of disabled individuals: anti-discrimination legislation and employment quotas. Anti-discrimination

¹The RDD has been used in a number of studies to measure causal effects. See Angrist and Pischke (1999), DiNardo and Lee (2004), Imbens and Lemieux (2008), Lalive (2008), etc. for studies assessing the causal effects of unions, social assistance, unemployment benefits on labor market outcomes.

legislation prohibits discrimination against disabled individuals in all aspects of employment and the employment process. The alternative approach, employment quota specify that employers have to offer a specific proportion of their jobs to disabled individuals. One of the most striking differences between these two approaches is, arguably, that the burden of proof of employment discrimination is put on disabled individuals themselves with anti-discrimination legislation, whereas employment quota provide incentives to employers to employ disabled workers.

A rapidly growing literature studies the effects of anti-discrimination legislation for disabled individuals. Using state-by-state variation in the timing of passage of the the Americans with Disabilities Act (ADA), DeLeire (2000), Acemoglu and Angrist (2001), and Beegle and Stock (2003) find that the ADA has not improved employment of disabled individuals in the U.S. and may, in some cases, even have reduced employment chances of disabled workers. Kruse and Schur (2003) challenge this finding arguing that the data used in the earlier studies may not have provided precise information on disability status. Jolls and Prescott (2004) and Jolls (2004) argue that the ADA increased education participation by individuals whose employment prospects are thought to have been improved by the ADA and argue that increased education participation is the result of an increase in the return to further education. Bell and Heitmueller (2005) study the effects of the Disability Discrimination Act (DDA) in the U.K. Their results confirm earlier findings for the ADA that the DDA has neither improved nor worsened employment prospects for disabled individuals in the U.K. ² The existing literature on the effects of employment quota is rather sparse. The only empirical study that we know about the role of employment quota is Wagner *et al.* (2001).³ The paper assesses the impact of the employment quota in Germany on job dynamics in 400 small firms finding no effect of the quota threshold.

This paper contributes to the literature in at least three important ways. *First*, the evaluation is based on very high quality data covering a universe of almost 26,000 firms. We use the same data that Austrian Authorities use to determine compliance with employment quota the Austrian Social Security Data (ASSD). We combine this data with information from the Austrian Federal Welfare Office (FWO) on the disability status of each Austrian individual. The former data set is important, since it allows to link firm and worker data and provides exact information on the firm size of each single Austrian firm. The latter data set is crucial in the present context, since only individuals who are recognized as disabled by the FWO are counted for the fulfillment of the employment quota. Our analysis is thus based on precise and sharp information on the size of the firm

²See also Lechner and Vazquez-Alvarez (2003) and Verick (2004) study the effects of German anti-discrimination legislation.

³See Welch (1976) for an early theoretical attempt to characterizing the effects of quota on the labor market.

as well as on whether an individual is covered by legislation. *Second*, we provide a rich picture of the implications of employment quota along several dimensions and focuses on understanding the mechanisms behind the effect of employment quota on employment of disabled workers. *Third*, while there is a considerable literature on the effects of anti-discrimination legislation, we are not aware of studies that provide clean and convincing causal evidence of employment quota systems. Our paper makes a first attempt to understanding employment quota and, in doing so, complements existing evidence on anti-discrimination legislation. This is important as the assessment of alternative policy options is absolutely central for designing optimal policies to integrate disabled individuals into the labor market.

The paper is organized as follows. Section 2 provides a detailed description of institutional situation in Austria in regard of the Disabled Persons Employment Act and formulates the hypotheses with respect to firms' hiring decisions regarding severely disabled individuals to derived from it. Section 3 describes the data. Section 4 outlines the empirical strategy. Our main results are presented in section 3. Section 6 concludes.

2 Background

This section describes the institutional background on Austria's employment quota system, discusses the incentive effects created by this system and derives hypotheses that will be tested in the empirical analysis below.

2.1 The Disabled Persons Employment Act in Austria

In 1969 Austria implemented the Disabled Persons Employment Act (DPEA) which forms the legal basis of the Austrian employment quota system. The DPEA defines the process by which individuals acquire the status of being severely disabled; regulates the employment obligations for firms and the financial sanctions associated with non-compliance of these obligations; specifies rules how to pay out subsidies to firms employing disabled workers; and introduces employment protection rules for disabled workers. In what follows, we discuss these elements in more detail.

The Austrian employment quota obliges firms to hire one "severely disabled" worker per 25 non-disabled workers leading to a quota of 4 %.⁴ Firms that do not comply with this obligation are subject to a non-compliance tax. The non-compliance tax steadily increased from € 118 in 1990 to € 150 in the first half of 2001. The tax currently stands at € 213 which amounts to roughly 6.6 % of workers' average monthly salary or 0.8 %

⁴The Austrian quota is lower than the German (5 %), French (6 %), Polish (6 %), and Italian (7%) quota; but it is higher than the Belgian, Korean, and Spanish quota (2 %).

of firms' average monthly pay-roll in the Austrian private sector in 2006.⁵ Note that whereas there is an change in labor regulation at firm size 15 (that makes works councils mandatory), there is no further change in any labor regulation legislation at the threshold firm size of 25.

The process by which individuals acquire the registered "severely-disabled" status goes as follows. In order to get entitled, disabled individuals have to file an application to the Austrian Federal Welfare Office (FWO) for a legal declaratory procedure. Eventually, they are approved only if a medical expert of the FWO assesses a degree of physical, mental, intellectual or sensuous disorder, which reduces the individual's work capacity by at least 50 percent. The registered status can not be recalled unless a new expertise declares that the reduction in work capacity is less than 50 percent. Employers have little possibility to interfere with the process of obtaining the registered status for at least two reasons. First, workplace or other accidents that lead to a sudden change in work capacity almost automatically trigger a start of the process of assessing the reduction in work capacity. Second, employers would have to persuade individuals who are *de facto* severely disabled to wait until their firm has crossed the quota threshold. This seems hard since these individuals would forego substantial advantages in terms of employment protection. Thus, firms can not simply ask an individual who is *de facto* disabled to get registered disabled once they cross the quota threshold, i.e. a simple *relabeling* effect of employment quota is unlikely to be quantitatively important.

The group of registered disabled individuals comprises a non-negligible proportion of the Austrian work force. In 1999, the time period of our analysis, about 80,000 individuals or 2 % of aggregate employment were registered severely disabled.⁶ Recall that the employment quota specifies that 4 % of each firm's workforce be registered disabled. This means that the group of registered disabled individuals is too small to achieve full compliance with the employment quota. The non-compliance tax is a de-facto tax on firms who employ at least 25 non-disabled workers.

The employment obligation is directly enforced by the FWO. The procedure is as follows. First, the firm size of each firm is checked on the first day of each month. Only non-disabled workers are taken into account for the firm-size computation. Second, the number of registered disabled workers per firm is counted, in order to assess to what extent a firm complies with the employment quota. There is some double-weighting, i.e. particular

⁵On July 1, 2001 the non-compliance tax increased by € 46.- to € 196.- and was again gradually increased henceforth. In addition, there was another important policy change. Before 1999, firms that employed more disabled individuals than demanded by the DPEA were granted a premium in the amount of the non-compliance tax per month for each excessive disabled employee. However, this rule was abolished on January 1, 1999.

⁶See Humer *et al.* (2007) for more information on the number of individuals who are registered disabled in Austria.

groups of disabled workers are equivalent to two disabled workers, which include the (i) blind, (ii) disabled individuals at the age less than 19 years, (iii) disabled apprentices, (iv) disabled individuals at the age of more than 50 years and a degree of disability of at least 70 percent, (v) disabled individuals at the age of more than 55 years and (vi) individuals in a wheelchair. Finally, the Austrian Federal Welfare Office levies a non-compliance tax on firms who do not fulfill the employment quota..

The DPEA also defines how the revenues collected through non-compliance taxes are spent. The main beneficiaries are firms (and their disabled employees) who actually offer employment to disabled workers. These subsidies, either in form of allowances or loans, support those firms, which employ at least one disabled worker. In particular, they are granted for adequate workplace accommodation, wage subsidies, work assistance, occupational retraining or professional development. Basically, this represents a reallocation of resources from firms that do not comply at all with the employment quota to firms that employ at least one disabled worker in order to compensate the latter for their effort in employing disabled workers. Note, however, that the subsidies are available to all firms, not just the firms that are subject to the employment obligation. In 2005, the granted subsidies amounted to roughly € 65 Mio.

The DPEA provides increased employment protection for registered disabled workers. Increased job protection comprehends an increased dismissal protection and protection of remuneration. The increased protection against dismissal is twofold. On the one hand, it requires that the termination of the contract takes effect only after a probation period of at least 4 weeks. On the other hand, the dismissal is only valid if a special FWO committee agrees to it. Any dismissals without the consent of this committee are unlawful. However, the increased dismissal protection comes into effect not until a probation period of six months has elapsed. Originally, the probation period amounted to one month and was extended in two steps, at first from one to three months on January 1, 1999 and finally on July 1, 2001 from three to currently six months. The protection of remuneration ensures that the wage of disabled workers must not be reduced due to their disability.

2.2 Hypotheses

How do employment quota and the associated financial sanctions for non-compliance affect firms' incentive to employ of severely disabled workers? Recall that work capacity must be reduced by at least 50% in order to qualify for the status 'registered disabled'. This means that productivity of disabled workers is, arguably, much lower than the productivity of non-disabled workers. Thus, without a force counterbalancing this reduced productivity, we would not expect any firm to be willing to hire disabled workers. This force takes the form of a cost advantage introduced by DPEA along two dimensions. On one hand,

complying firms do not pay the non-compliance tax which is on the order of 6 % of a the average salary. On the other hand, firms willing to offer jobs to severely disabled workers can ask for subsidies to pay for workplace adjustments but also for hiring (and training) subsidies. These subsidies can improve the cost advantage of disabled workers considerably.

Moreover, one would expect that the effects of the non-compliance fee to depend on a firm's position in the wage distribution as well as on technology. With regard to wages, the flat rate nature of the fee implies that the incentive to employ severely disabled workers is stronger for firms in the lower end of the wage distribution than for firms in the upper end of the wage distribution. We therefore assess the effect of the non-compliance fee for firms grouped by the quartiles of the firm wage distribution. With regard to technology, the non-compliance fee will be more effective in promoting employment in firms where productivity is less sensitive to disability related work impediments. Using industry as a proxy for technology, we therefore assess the effects of the non-compliance tax by industry.

From the firm's point of view, one of the key pieces of information when hiring a disabled worker is the worker's productivity. Such information is, arguably, more readily available to firms considering keeping a former employee on the payroll than for firms who are offering employment to an individual who has not previously worked for the firm. One would expect, therefore, that the bulk of the employment effect of a quota would protect employment of former employees of the firm rather than create employment for individuals who have not been employed with the firm before. The empirical analysis below will provide information on the effect of employment quota by employment status of the individual at the time when the registered disability status has been granted to the worker. In particular, we will decompose the overall effect into the effect on employment of disabled employees who worked with their current firm at the data of acquiring the registered disability status as well as into the effect on employment provided to individuals who have not worked for the firm when acquiring the registered status.

3 Data

To assess the impact of the employment quota on the firms' hiring decisions with respect to disabled workers, we use register data from two different sources: (i) the Austrian Social security database (ASSD) which contains detailed information on the individuals' employment history and characteristics since 1972 on a daily basis together with an unambiguous firm identifier as well as firms' industry affiliation and location and (ii) personal data from the Federal Welfare Office (FWO) in Austria which records the disability status, type, and degree of all registered disabled individuals in the context of the Disabled

Persons Employment Act (DPEA). Since the disability status is objectively assessed by a medical procedure (rather than self-reported by firms or workers) means that we can provide clean evidence on the impact of disability on employment prospects. The absence of measurement error is a particular advantage of our data set.⁷

These two data sources can be linked on the basis of an anonymized person identifier. This allows the accurate calculation of two variables which are crucial for our empirical analysis: the number of the non-disabled workers employed in each firm; and the number of disabled workers employed in each firm. The former variables determines whether a firm is obliged to hire a disabled worker and the latter represents how many disabled workers each firm actually employs. In summary, we can precisely determine whether and to which extent a firm complies with the employment quota. The Federal Welfare Office checks firms' compliancy with the employment quota on the first day each month. We account for this administrative *modus operandi* by creating a data set with monthly reference dates, which all refer to the first day of each month.⁸

Since the 1990s, two major changes of the DPEA occurred, which were relevant for the employment quota: (i) on January 1, 1999 the over-compliance premium in the amount of the non-compliance tax was abandoned and (ii) on July 1, 2001 the non-compliance tax was extraordinarily increased by one third to about € 200.⁹ Our sample starts in January 1999 and ends in June 2000, hence it is unaffected by these major policy changes. We restrict the analysis to firms in the private sector – who are likely to pursue a clear profit maximizing objective. In particular we focus on firms operating in the services sector, or in manufacturing, construction or the tourism industry.

We focus on firms' employment decisions with respect to disabled workers just above and just below threshold $T = 25$, above which firms are obliged to hire a disabled worker (i.e. treated firms). Firms below this threshold are not subject to the employment quota (i.e. non-treated firms). This sample restriction implies that we study the effect of the employment quota on firms' that are on the margin of entering the employment quota system.¹⁰ Furthermore, we restrict the sample to firm sizes $S \in [T-12, T+12]$. Restricting

⁷For instance, studies evaluating the Americans with Disabilities Act (ADA) often use subjective disability measures that does not necessarily capture ADA coverage. Kruse and Schur (2003) show that these measurement problems can result in misleading results.

⁸Note that it is not feasible for firms to simply hire a disabled worker for only one day (i.e. the first day of each month) a month in order to comply with the employment quota. This behavior is ruled out by the regulatory restriction imposed by the DPEA. It even turns out that the average number of working days – including weekends – are slightly higher for disabled than for non-disabled workers. The average number amounts to 30.12 (1.86) days for disabled and 29.93 (1.39) for non-disabled workers.

⁹The non-compliance tax is yearly adjusted by means of a matching factor determined by the general social security act (*Allgemeines Sozialversicherungsgesetz*).

¹⁰Note that the Austrian employment quota system introduces further thresholds at multiples of 25, i.e. at 50, 75, 100, etc. We focus on studying the effects of the employment quota at a firm size of 25 for two reasons. First, firms at higher order threshold are already covered by the quota system. Studying the first threshold allows analyzing the effects of being covered versus not being covered. Second, there

the analysis to observations that are close to the quota threshold is important since, not surprisingly, firm size turns out to be an important predictor of disabled employment. Note, as we discuss in section 4, we will not only control for the effect of firm size by sample restriction but we will also use regression techniques to further account for the effects of firm size.

Table 1 reports key background statistics on firms which are located around the threshold $T = 25$. Panel A of table 1 shows the two key indicators. The first line provides information on the average number of jobs provided to registered severely disabled individuals in treated and control firms. We calculate the number of disabled workers per firm in the same way as the FWO, i.e. it is calculated by double weighting particular groups of disabled individuals (see section 2). Control firms provide on average 0.19 work places to disabled individuals. This means that about 1 in 5 firms provides employment to a disadvantage worker even in the absence of an employment obligation. In contrast, treated firms which have passed the quota threshold employ about 0.42 disabled workers. This is twice as many as non-treated firms do. Thus, *prima facie* evidence is consistent with an employment promoting effect of the employment obligation.

Table 1

Row 2 of panel A of Table 1 clearly indicates that treated and control firms are quite different and hence comparing the raw differential does not provide strong causal evidence on the effects of employment quota. Treated firms are, by definition, much larger than control firms. Whereas control firms employ on average 17.10 non-disabled workers, treated firms employ almost 30.13 non-disabled workers – again almost twice as many as control firms. To the extent that firm size is important in offering jobs to disabled workers, *prima facie* evidence is likely to be biased.

Panel B of table 1 displays information on firm size dynamics. The indicator "unchanged workforce since last month" measures stability of firm size between month t and month $t - 1$. The indicator "expanded since 6 months" measures whether firm size in month t is strictly larger than firm size in month $t - 6$. The indicator "contracted since 6 months" measures whether firm size in t is strictly smaller than in month $t - 6$. Results indicate that 41 % of control firms have not seen any change to their workforce since half a year, whereas this is true only for 25 % of the firms above the quota threshold. In terms of firm growth, results indicate that 45 % of control firms and 47 % of the treated firms have expanded their employment during that past 6 months. In contrast, 39 % of treated firms have downsized within the last six months compared to 34 % of control firms. This

are much fewer firms at higher order thresholds than at the first threshold. This means that the first threshold is the most relevant threshold in terms of the number of firms concerned by the quota.

suggests that treated and control firms differ strongly in terms of stability of the workforce and in terms of downsizing but less so in terms of firm growth.

Panel D of table 1 provides further background information on firms. Firm age measures the number of years the firm number has been observed in ASSD since 1972 – the year ASSD started.¹¹ Results indicate that the average control firm has been founded 15.9 years before the current date, whereas treated firms have been created almost exactly 1 year earlier. Panel B of Table 1 also reports information on a firms' workforce structure, pay, age, tenure, and apprentices. Note that all of these measures are based on non-disabled workers employed by the firm in month t . In terms of firms' workforce structure, results indicate that the average control firm employs 44 % white-collar workers which is also true for treated firms. In contrast, female employment is substantially higher among control firms (41 % women) than among treated firms (38 % women). This difference is, arguably, due to the fact that control firms are considerably smaller than treated firms; it should thus disappear once the differences in firm size are reduced. Control firms' employees are on average 35.4 years old whereas treated firms employ individuals who are slightly older (35.7 years). In order to measure the level of pay for each firm, we also report information on the median daily wage paid by each firm to its employees on the first day of month t . Results focus on the median wage since ASSD wage information is censored from above for about 20 % of employees covered by ASSD. Results indicate that control firms pay their employees about 59.8 € per day whereas treated firms pay almost 3 € per day more (62.6 €). Control firm employees have been working for their current employer on average for 5.3 years whereas treated workers have been with their employer slightly longer (5.6 years). Finally, treated firms provide about 2 jobs to apprentices which is considerable more than control firms (1.4 jobs).

Panel D of table 1 contains information on firms that should not be affected by the treatment, i.e. variables that are pre-determined with respect to the DPEA: location, and industry. Whereas there are no strong differences in terms of firm location, there are moderate differences in terms of industry. Whereas 30 % of treated firms are in manufacturing, the corresponding figure is only 28 % for control firms. In contrast, whereas 11 % of control firms are in the tourism industry, only 9 % of the treated firms are.

The figures of Panel B and C of table 1 reveal that there are some striking differences between treated and non-treated firms in variables that directly relate to the firm size

¹¹Note that this implies that firm age is left censored. Left censoring is not problematic in this application because the focus of this paper is to measure the effects of employment quota on employment of disabled workers. This means that information on firm age is merely used to control for differences between treated firms and control firms. Moreover, firm age will turn out to be balanced between threshold firms. This implies that left censoring of firm age is unlikely to bias estimates of the effect of employment quota on employment of disabled workers.

(and thus to the treatment status) and to pre-determined covariates. This means that simply contrasting treated and control firms does not provide information on the causal effect of employment quota on the employment decisions of Austrian firms. The following section discusses the more refined framework we use in the empirical analysis.

4 Identification and estimation

Our empirical strategy is based on the fact that the DPEA discontinuously changes the financial incentives of employing severely disabled workers. The DPEA obliges firms to hire a disabled worker if the size of the firm (as measured by the number of non-disabled workers) S_i is greater than or equal to the quota threshold $T = 25$. Firms that do not comply are subject to a non-compliance tax. This creates financial incentives for firms to hire disabled workers as firms face a trade-off between hiring a disabled worker or buying off this obligation.

To identify the causal effect of employment quota on employment of disabled workers we adopt a regression discontinuity (RD) design. The RD design allows identification of the causal effect of the employment quota for firms located at the quota threshold T . The RD design in the DPEA context is sharp, i.e. the probability of having to pay the fine for not providing a job to a disabled worker changes from zero to one at the threshold. This means that the discontinuity at the assignment threshold reflects the average causal effect for threshold firms. The key identifying assumption for this result is that the expected number of disabled workers in absence of the employment quota is continuous at the threshold (Hahn *et al.* (2001)). In terms of the potential outcomes approach, the validity of our empirical strategy hinges on the assumption that the potential outcome for non-treated firms is independent of firm size at the threshold (i.e. a local exclusion restriction).

The following linear regression allows to identify the discontinuity in the average number of disabled workers per firm at treatment assignment thresholds $T = 25$:

$$Y_i = \alpha_0 + \alpha_1 \cdot D_i + \beta_0 \cdot (S_i - T) + \beta_1 \cdot D_i \cdot (S_i - T) + \epsilon_i,$$

where Y_i denotes the number of disabled workers, D_i indicates whether a firm is treated or not, and S_i denotes firm size. The inclusion of S_i controls for the direct effect of firm size on the number of disabled workers. This is crucial since we in general expect that larger firms are more likely to employ disabled workers. The key parameter is α_1 . This parameter measures the average causal effect of DPEA on the number of disabled workers for firms at the quota threshold T . α_0 measures the average number of disabled workers

for firms just below the assignment threshold T . The parameters β_0 and β_1 capture the correlation between the assignment variable S_i on the average number of disabled workers per firm.

Note that assignment variable S_i inherently has discrete support. This implies that we have to choose a particular functional form for the relationship between Y_i and S_i . The baseline model assumes a linear functional form, but we also add quadratic terms in S_i as a sensitivity analysis. Moreover, we only consider firms within a window of size 12 below and above the threshold, thus $S_i \in [T - 12, T + 12]$. This sample restriction is intended to down-play the importance of the functional form between the mean number of disabled workers and non-disabled firm size.

Unknown functional form also affects variance-covariance matrix estimates. Lee and Card (2008) propose to use cluster-consistent standard errors (clustered on the distinct values of S_i) to account for the uncertainty related to the choice of the functional form. Furthermore, remember that we use pooled cross-section data for the econometric analysis. Observations of the same firm cannot be considered to be independent from each other. Thus, we do not only need to cluster on S_i but also on firms (note that this is non-nested). Cameron *et al.* (2006) propose a new variance estimator for OLS that provide cluster-robust inference when there is two-way clustering that is non-nested. As a consequence, we report two types of robust standard errors in our regression outputs: standard errors that are (i) clustered on S_i and (ii) such that are clustered on S_i and firms.

Recall that table 1 indicates that means of important firm characteristics differ between firms below and above the threshold. This imbalance invalidates a strategy of simply comparing differences in means. It may, however, not invalidate our empirical strategy unless these differences are due to endogenous selection of firms at the quota threshold. We investigate three pieces of evidence that allow discussing endogenous selection.

First, we shed light on whether the firms' choice of size, i.e. the employment decision behavior with respect to non-disabled workers, is affected by the DPEA. Firms might try to stay just below the threshold in order to prevent becoming subject to the non-compliance tax. Yet, there is no reason for firms to cross the threshold. Thus, if this endogenous sorting behavior related to the DPEA is present, we would expect a spike in the firm size distribution just below the threshold. Figure 1 reports the firm size distribution around the quota threshold. Visual inspection suggests that no spike is visible. We also formally test for the presence of a discontinuity in the firm size distribution (see McCrary (2008)). We run a regression in the flavor of the baseline model extended with quadratic terms in S (which is in regard to figure 1 appropriate), where we used the firm size density as a dependent variable. A firm size bandwidth $S_i \in [T - 12, T + 12]$ is used, yielding 25 observations. It turns out that the firm size density is -0.42 percentage points

lower at the quota threshold than would be expected from information on firms below the threshold. This difference is statistically significantly different from zero at the 5 %-level of significance (p-value of 0.03). While this looks like a violation of the identifying assumption, this is not the case.

Figure 1 about here

To see this, we run the same regression for a set of placebo thresholds $\ddot{T} \in \{13, 14, \dots, 23, 24, 26, 27, \dots, 37\}$ using the density for firm sizes ranging from 1 to 49. Figure 2A plots the discontinuities on these alternative (=placebo) thresholds. Just like at the true threshold $T = 25$, the model detects discontinuous changes in the firm size density at almost all placebo thresholds. Moreover, these discontinuities tend to be somewhat larger at lower firm sizes and then level off as firm size increases. It is important to note that, in line with our identification strategy, that the discontinuity at threshold 25 is not an outlier in any sense. Figure 2B displays the empirical density of the estimated discontinuities for firm sizes between 13 and 37. The discontinuity at $T = 25$ is well within the range of the discontinuities estimated for the placebo thresholds (and is smaller in absolute size than the average discontinuity of the placebo thresholds). Taken together, these results suggest that the discontinuity in the firms size density estimated at the quota threshold is a statistical artefact rather than due to purposeful self-selection of firms.

Figure 2

As a second test for self-selection of firms below the threshold $T = 25$, we look at the firms' employment fluctuation and employment growth. The idea is that firms just below the threshold avoid to grow whereas no such restriction on firm growth exists at other firm sizes. For the same reason we expect higher employment stability just below the threshold. Again, we run regressions in the flavor of the baseline model and use the following three dependent variables instead: (i) i.e. whether a firm has had any changes in its workforce as compared to the previous month, (ii) whether a firm has expanded as compared to six months ago, and (iii) whether a firm has contracted as compared to six months ago. The results are summarized in table 2. (In all regression, the dependent variable is a dummy variable). Linear probability model results indicate that the only variable exhibiting a statistically significant discontinuity at either the 1 % or 5 % level represents the one indicating whether a firm has grown. The estimate suggests that threshold firms expanded 1.7 percentage points less likely than would be expected using information on firms just below the quota threshold. However, this result is neither economically significant nor very robust. Adding a second order term in firm size leads to an insignificant threshold effect on firm growth (Table 2 column (2)).

Table 2 about here

Finally, we investigate whether the pre-determined variables firm age, industry, and firm location exhibit any discontinuity at the threshold. The baseline model with a linear fit in S is used for this purpose. Figure 3 shows the discontinuities together with the corresponding 95% confidence intervals (with standard errors clustered on firm size and firm) at the threshold $T = 25$. With the exception of one region (Vorarlberg), none of the firm characteristics display significantly different means for quota threshold firms compared firms just below the quota threshold. We nonetheless investigate the sensitivity of our results with respect to adding these firm characteristics. Including information on the firms in our sample can also lead to a reduction in the noise and potentially produce more precise estimates.

Figure 3 about here

5 Econometric Results

This section presents the econometric estimates of the effects of the DPEA on the number of disabled workers per firm. Figure 4 reports the number of disabled workers per firm by firm size for sizes ranging from 13 to 37. The evidence is based on 442,788 firm-month observations providing information on the employment decisions of 25,687 firms. Descriptive evidence indicates that the average number of disabled workers employed by firms below the quota threshold is lower than the the number of disabled workers employed by firms facing the quota. Specifically, firms who employ 13 non-disabled workers offer about 0.14 workplaces to disabled workers – 1 out of 7 firms provides employment to disabled workers. In contrast, firms who employ 24 non-disabled workers provide 0.3 jobs to disabled workers. Figure 4 also suggests an approximately linear increase in the mean number of jobs provided to disabled workers as firm size increases. Strikingly, quota threshold firms with 25 non-disabled workers appear to offer 0.35 jobs to disabled workers, an unexpected increase given behavior of firms who are not subject to the employment obligation. Again, the number of jobs provided to disabled workers increases in an almost linear fashion from firm size 25 to firm size 37 with 2 of the largest firms employing one disabled worker.

Figure 4 about here

Figure 4 thus presents evidence of an unexpected change in the average number of jobs provided to disabled individuals. This change can be measured by superimposing the fit of the baseline model. Doing so yields a discontinuity at the quota threshold of 0.0521

with a standard error (clustered on firm size) amounting to 0.0078. This discontinuity is statistically significant at the 1%-level. The effects appears to be rather small. However, the mean number of disabled workers per firm around the first threshold is 0.25, meaning that the discontinuity constitutes a 20.6% increase in the number of disabled workers per firm. Put differently, roughly one in 20 firms employs one disabled worker more due to the DPEA.

Moreover, Figure 4 also provides supplementary evidence concerning endogenous self-selection of firms below the threshold. Arguably, firms who face strong difficulties of accommodating a disabled worker self-selecting below the quota threshold would tend to decrease the mean number of jobs provided to displaced workers just below the quota threshold, i.e. this would show up in an unexpected dip in the number of disabled jobs just below the quota threshold. In contrast, results in Figure 4 suggest that firms just failing the quota threshold are employing slightly more disabled individuals than would be expected from a linear regression modeling the behavior of firms below the quota threshold. This evidence is not consistent with self-selection of firms in terms of propensities to provide employment to disabled workers taking place.

Table 3 shows our main results for the effect of the employment quota on jobs provided to disabled workers. Note at first that whether we cluster on the firm size or on the firm size and firm does not affect the statistical significance of our results in any of the 5 columns. Column 1 simply repeats the results from figure 4. The validity of RD estimates hinges on the appropriate specification of the relationship between the assignment variable and the outcome. If this relationship is misspecified, what looks like a jump at the threshold might simply be a marked nonlinearity. Column 2 adds quadratic terms in $S_i - 25$. The result indicates that the effect is with 0.0351 somewhat weaker, but remains statistically significant at the 1%-level. In Column 3 we add all covariates displayed in Panel B-D of table 1 and time-dummies as controls and re-estimate the discontinuity. Results indicate that controlling for these covariates does not change size and significance of the treatment effect. Another issue is the sensitivity to the choice of bandwidth. In the first three columns in table 3 we use the baseline bandwidth, i.e. we use firm-month observations with firm size $S \in [13, 37]$. We narrow the firm size bandwidth by one fourth (thereby losing one third of all firm-month observations) in column 4 and by one half (thereby losing almost two thirds of all firm-month observations) in column 5. From column 4, we infer that considering only firm-month observations with firm sizes $S_i \in [16, 34]$ the magnitude of the effect reduces from 0.0521 to 0.0434. Considering only firm sizes $S \in [19, 31]$ in column 5 further decreases the effect to 0.0381. Yet, both effects remain statistically highly significant at the 1%-level.

Table 3 about here

Estimates of the causal effect of employment quota on employment of disabled workers are somewhat sensitive to functional form and bandwidth choice. The statistical reason for this sensitivity is due to the fact that threshold quota firms with 25 non-disabled workers provide lower employment to disabled workers than would be expected from the behavior of firms with 26 to 37 non-disabled workers. Introducing higher order terms in firm size or restricting bandwidth results in putting more weight on threshold firms thus leading to a lower importance of employment quota. One potentially important behavioral reason for unexpectedly low disabled employment for threshold firms could be due to lags in fulfilling the obligation. The idea is that firms who end up employing at least 25 non-disabled workers may not be fully informed of the employment obligation. This implies that even firms take time to adjust their employment pattern. We investigate the empirical relevance of this explanation for low disabled employment at the threshold by omitting all firm-month observations observed for the first time above the threshold.¹²

Table 4 reports estimates identical to the results in Table 3 except that all first firm months above the threshold have been removed from the data. Estimates in column 1 suggest that employment quota increase employment of severely disabled workers by 0.061 slots per firm, an estimate that is slightly higher than the baseline estimate in Table 3. Adding a quadratic term (column (2)) reduces the baseline estimate from 0.061 to 0.046 workplaces provided to disabled workers. Adding control variables (column (3)) slightly reduces the estimate from 0.061 to 0.057. Reducing bandwidth by 3 non-disabled workers (column (4)) also leads to a reduction of the estimate from 0.061 to 0.053, and reducing the bandwidth by an additional 3 non-disabled workers (column (5)) leads to small further reduction of the estimate from 0.053 to 0.049.

Table 4 about here

The overall pattern of the results in Table 3 and 4 is well within a 95 % confidence interval around the baseline result that employment quota lead to excess employment of 0.052 workplaces for disabled workers. We therefore decide to adopt the model producing results in Table 3 column (1) as our baseline model.

To further assess the validity of our RD setup, we estimated discontinuities in the number of disabled workers per firm at firm sizes where there should be no discontinuities. For this purpose, we focus on observations with firm sizes lower than 24, i.e. we only considered firms that are not affected by the employment quota. We used the baseline model, but narrowed the firm size bandwidth by one half (i.e. $S_i \in [\bar{T} - 6, \bar{T}]$, with placebo thresholds $\bar{T} \in \{7, 8, \dots, 18\}$). Figure 5 reports the results. Figure 5A (plot of the discontinuities and

¹²Note that this sample restriction only removes 4778 (1 %) of data on firm months from above the threshold. About 46 % of the datapoints removed are located at a firm size of 25 non-disabled workers with the share of data removed monotonically decreasing to a firm size of 37 non-disabled individuals.

corresponding 95% confidence interval) shows that the discontinuities at the 12 placebo thresholds fluctuate between -0.01 and 0.01. Moreover, only the discontinuities at firm sizes 12 and 13 are statistically significant at the 5%-level. They amount to 0.0086 and 0.0136, respectively. However, compared to the discontinuity in the number of disabled workers per firm of 0.0381 at the first threshold derived from the same specification (see table 3, panel A, column 5), this looks rather small. Figure 5B illustrates this. It shows the density of discontinuities at placebo thresholds and also indicates the discontinuity at the true threshold. This suggests that the finding for the true threshold is not a statistical artefact.

Figure 5 about here

Next, we turn to discussing heterogeneity of the treatment effect. Table 5 reports the causal effect of employment quota for firms in different parts of the firm wage distribution. We group firms according to the median daily wage paid to their workers in the period 1999 to 2001. The median daily wage paid by firms to their workers is our measure of the firm wage. We then allocate each firm month observation to four approximately equal sized groups based on the quartiles of the firm wage distribution. This grouping ensures that the relative size of the non-compliance tax decreases strongly. Whereas the average firm in the first quartile face a tax of 12.9 % of its firm wage, firms in the top quartile only face a tax of 5.5 % of the firm wage (bottom row in Table 5).

Table 5 about here

Results indicate that employment quota produce a strong increase in the workplaces available to displaced workers among firms located in the first quartile of the wage distribution. Quota firms provide 0.089 workplaces to disabled workers which would not be there without the employment quota (column (1)). The employment effect of the employment obligation is similarly strong among firms located in the second quartile of the firm wage distribution. These firms generate excess employment on the order of 0.072 workplaces provided to registered disabled workers (column (2)). High-wage firms located above the median of the firm wage distribution respond considerable less than firms below the median. Threshold firms in the third quartile provide employment in excess of what would be expected from firms just below the threshold of 0.042 workplaces (column (3)) – or about half the workplaces created by firms in the first quartile of the firm wage distribution. Interestingly, firms in the top quartile quartile of the firm wage distribution do not appear to respond to the employment obligation (the estimated effect in column (4) is 0.0099). Note that the pattern of causal effects of employment quota are very much in line with the pattern of relative impact generated by the flat rate tax. This is consistent

with a notion that the financial incentives are important in shaping employment patterns for disabled workers.

We further look at effect heterogeneity with respect to industry affiliation and firm location. Figure 6 displays the discontinuities in the number of disabled workers per firm for each industry and province. The y-axis on the left hand side measures the discontinuities and the corresponding 95% confidence intervals (clustered on firm size) using the specification of column 1 in table 3. The y-axis on the right hand side measures the number of firm-month observations used for the calculations. Figure 6 reveals that the treatment effect is only somewhat heterogenous across industries at the threshold. The manufacturing industry reacts the most to the financial incentives created by the DPEA. Treated firms belonging to this industry employ 0.0812 disabled workers more than non-treated firms. The second-largest effect is estimated for the tourism industry with a discontinuity of 0.0509, then the construction industry with 0.0436, and finally the service industry with the lowest, however still important, effect of 0.0375. All calculated discontinuities at the first threshold are statistically significant at the 5%-level.

Figure 6 about here

In table 3 above, we have shown that treated firms employ 0.0521 disabled workers more than non-treated firms at the first threshold. Table 6 discusses how threshold firms provide employment to disabled workers. To do this, we decompose employment provided to disabled workers who had been employed with the same firm at the date of registering as disabled (*own former employees*), who had been employed with another firm at the date of registering as disabled (*other former employees*), and who had not been employed at the time of registering as disabled (*non-employees*).

Providing information on the effects for these three groups of workers is important. Own former employees have been employed with the current employer already before acquiring the status of being severely disabled. This means that the current employer is, arguably, quite well informed about the on-the-job productivity of the disabled individual. Moreover, seeing an increase in workplaces provided to individuals formerly employed by the current firm allow discussing the effects of DPEA on retention, i.e. the effect of keeping workers on the payroll that would not have been kept without the employment obligation. Other former employees have not been working for the current employer. Thus, the current employer does not have as good information regarding on-the-job skills as for own former employees. Moreover, information on other former employees allows speaking about the role of DPEA in reallocating jobs between the current firm and other different firms. Non-employees have not been working at the date of registering. The current employer has, therefore, as poor information regarding on-the-job productivity

of the person hired as for an individual hired from another firm. Yet, in contrast to results for other former employees, non-employees are not poached from other employers. Employment effects for non-employees therefore speak to the role of DPEA in increasing hiring from the non-employment pool.

Table 6 provides information on the separate effects of DPEA on workers of different type. Column 1 in table 6 displays the baseline effect at the quota threshold (we repeat the estimate in column 1 of Table 3 for ease of comparison). Columns 2 to 4 provide separate estimates for workers who had been employed by the current firm (column 2), workers who had been employment by a different firm (column 3), and workers who had not been employed when becoming registered disabled (column 4). Results in column 2 suggest that quota threshold firms employ 0.026 more disabled workers who had already been working for the firm before becoming recognized severely disabled. This means that about 50 % of the baseline treatment effect at the quota threshold goes to workers whose productivity is, arguably, quite well known to the current employer. The resulting excess employment speaks to the role of DPEA in increasing retention of existing employees. Results in column 3 indicate that quota firms tend to have 0.021 more employees on their payrolls who had been employed in different firms when becoming recognized disabled. This means that up to 42 % of the treatment effect goes to workers whose productivity is less well known to the current employer. Moreover, the resulting excess employment effect is likely to reflect reallocation of jobs from non-quota firms to quota firms. Results in column 4 indicate that 0.004 jobs in quota firms go to individuals who were not employed when entering the recognized disabled status. This means that about 8 % of the excess employment gain reflects employment gains going to individuals whose productivity cannot easily be inferred. In sum, about 50 % of excess employment at quota threshold firms is directed to workers whose productivity is well known to the current employer. This is in line with a priori expectations concerning the role of information on productivity. Moreover, more than half of excess employment reflects retention and pure job creation for the non-employed. This means that DPEA indeed promotes employment of disabled workers.

Table 6 about here

6 Conclusion

This paper analyzes the effect of employment quota that aim to promote employment for severely disabled workers. While there is a considerable literature on the effects of anti-discrimination legislation, convincing causal evidence of employment quota systems is almost non-existent. Our paper makes a first attempt to understanding employment quota

and, in doing so, complements existing evidence on anti-discrimination legislation. The identification strategy relies on the sharp discontinuity in the relative costs of employing severely disabled workers created in a quota system combined with taxes raised on firms that do not comply with legal employment requirements.

Our empirical results indicate that quota do promote the employment of severely disabled workers in firms located above the quota threshold as compared to firms just below the quota threshold. Quota lead to excess employment of one severely disabled worker per 20 firms. We also detect important interactions between wages and industry. Firms in the lower end of the firm wage distribution tend to be incited to provide most of the excess employment to disabled workers. In terms of industry, the employment gain tends to be concentrated in capital intensive manufacturing rather than labor intensive sectors such as services, tourism or construction. We also find that quota boost employment primarily among former employees of the firm. They are, to a lower extent the result of poaching workers from other firms, or result due to employing individuals that were not formerly employed.

We conclude that employment quota systems when rigorously enforced by financial non-compliance sanctions on firms are a potentially effective policy tool. Future research should put more emphasis on evaluating this policy instrument in other contexts and compare the relative effectiveness of quota with antidiscrimination legislation.

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A Tables

Table 1: Descriptive Statistics

| | firm size 13-24 | | firm size 25-37 | |
|---|-----------------|-------------|-----------------|-------------|
| | mean | (std. dev.) | mean | (std. dev.) |
| <i>Panel A: Outcome and treatment assignment variable</i> | | | | |
| number of disabled | 0.19 | (0.54) | 0.42 | (0.86) |
| firm size | 17.10 | (3.33) | 30.13 | (3.71) |
| <i>Panel B: Firms' fluctuation and growth w.r.t. non-disabled workforce</i> | | | | |
| unchanged workforce since last month* | 0.41 | (0.49) | 0.25 | (0.43) |
| expanded since 6 months* | 0.45 | (0.50) | 0.47 | (0.50) |
| contracted since 6 months* | 0.34 | (0.47) | 0.39 | (0.49) |
| <i>Panel C: Other covariates</i> | | | | |
| share of white-collar workers* | 0.44 | (0.34) | 0.44 | (0.34) |
| share of women* | 0.41 | (0.29) | 0.38 | (0.28) |
| worker's age* (in years) | 35.42 | (4.96) | 35.68 | (4.56) |
| median daily wage* (€) | 59.82 | (19.93) | 62.55 | (19.91) |
| tenure* (in years) | 5.32 | (3.67) | 5.59 | (3.66) |
| number of apprentices* | 1.37 | (2.23) | 2.04 | (3.22) |
| <i>Panel D: Pre-determined covariates</i> | | | | |
| age of firm (in years) | 15.91 | (10.24) | 16.92 | (10.18) |
| Vienna | 0.22 | (0.41) | 0.23 | (0.42) |
| Lower Austria | 0.17 | (0.38) | 0.18 | (0.38) |
| Burgenland | 0.03 | (0.17) | 0.03 | (0.18) |
| Upper Austria | 0.17 | (0.37) | 0.18 | (0.38) |
| Styria | 0.11 | (0.31) | 0.10 | (0.30) |
| Carinthia | 0.06 | (0.24) | 0.06 | (0.25) |
| Salzburg | 0.09 | (0.28) | 0.08 | (0.27) |
| Tyrol | 0.10 | (0.30) | 0.09 | (0.29) |
| Vorarlberg | 0.05 | (0.22) | 0.05 | (0.22) |
| services | 0.45 | (0.50) | 0.44 | (0.50) |
| manufacturing | 0.28 | (0.45) | 0.30 | (0.46) |
| construction | 0.17 | (0.37) | 0.17 | (0.38) |
| tourism | 0.11 | (0.31) | 0.09 | (0.29) |
| Number of firm-month observations** | 328,020 | | 114,768 | |

* denotes that variable bases only on non-disabled workers.

** 25,687 distinct firms give rise to this number.

Source: Own calculations, based on ASSD and FWO.

Table 2: Discontinuities in Firms' Turnover and Growth

| | (1) | (2) |
|--------------------------------------|------------|----------|
| stable workforce since last month | 0.0118 | 0.0019 |
| (cluster: firm size) | (0.0069) | (0.0068) |
| (cluster: firm size <i>and</i> firm) | (0.0073) | (0.0069) |
| [R ²] | [0.0286] | [0.0287] |
| expanded since 6 months | -0.0170 | 0.0043 |
| (cluster: firm size) | (0.0080)** | (0.0040) |
| (cluster: firm size <i>and</i> firm) | (0.0087) | (0.0054) |
| [R ²] | [0.0010] | [0.0012] |
| contracted since 6 months | 0.0050 | -0.0005 |
| (cluster: firm size) | (0.0049) | (0.0049) |
| (cluster: firm size <i>and</i> firm) | (0.0059) | (0.0060) |
| [R ²] | [0.0025] | [0.0025] |
| Polynomial order | 1 | 2 |
| Number of observations | | 442,788 |

Notes: ***, ** denotes significance at the 1% , 5% level respectively.

Robust standard errors in parentheses.

Source: Own calculations, based on ASSD and FWO.

Table 3: The Effect of the Employment Quota on the Number of Disabled Workers per Firm – Full Sample

| | (1) | (2) | (3) | (4) | (5) |
|--------------------------------|-------------|-------------|-------------|-------------|-------------|
| Treatment effect | 0.0521 | 0.0351 | 0.0523 | 0.0434 | 0.0381 |
| cluster: S | (0.0078)*** | (0.0093)*** | (0.0084)*** | (0.0085)*** | (0.0088)*** |
| cluster: S , firm | (0.0140)*** | (0.0106)*** | (0.0143)*** | (0.0131)*** | (0.0093)*** |
| $S \in 25 \pm s$ | $s = 12$ | $s = 12$ | $s = 12$ | $s = 9$ | $s = 6$ |
| Polynomial order in $S_i - 25$ | 1 | 2 | 1 | 1 | 1 |
| Controls | No | No | Yes | No | No |
| mean (dep. var.) | 0.2526 | 0.2526 | 0.2526 | 0.2833 | 0.3074 |
| s.d. (dep. var.) | 0.6459 | 0.6459 | 0.6459 | 0.6907 | 0.7261 |
| N | 442,788 | 442,788 | 442,788 | 290,232 | 180,957 |
| R ² | 0.0286 | 0.0286 | 0.0567 | 0.0186 | 0.0105 |
| Adjusted R ² | 0.0286 | 0.0286 | 0.0566 | 0.0186 | 0.0105 |

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.

Robust standard errors in parentheses.

Source: Own Calculations, based on ASSD and FWO

Table 4: The Effect of the Employment Quota on the Number of Disabled Workers per Firm – Only Firm-Month Observations Included that Are At Least Since Two Months Above the First Threshold

| | (1) | (2) | (3) | (4) | (5) |
|--------------------------------|-------------|-------------|-------------|-------------|-------------|
| Treatment effect | 0.0612 | 0.0463 | 0.0570 | 0.0532 | 0.0489 |
| cluster: S | (0.0071)*** | (0.0093)*** | (0.0081)*** | (0.0079)*** | (0.0085)*** |
| cluster: S , firm | (0.0144)*** | (0.0116)*** | (0.0148)*** | (0.0135)*** | (0.0100)*** |
| $S \in 25 \pm s$ | $s = 12$ | $s = 12$ | $s = 12$ | $s = 9$ | $s = 6$ |
| Polynomial order in $S_i - 25$ | 1 | 2 | 1 | 1 | 1 |
| Controls | No | No | Yes | No | No |
| mean (dep. var.) | 0.2524 | 0.2524 | 0.2524 | 0.2835 | 0.3082 |
| s.d. (dep. var.) | 0.6453 | 0.6453 | 0.6453 | 0.6906 | 0.7268 |
| N | 438,010 | 438,010 | 438,010 | 285,612 | 176,575 |
| R ² | 0.0293 | 0.0293 | 0.0571 | 0.0194 | 0.0112 |
| Adjusted R ² | 0.0293 | 0.0293 | 0.0570 | 0.0194 | 0.0112 |

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.

Robust standard errors in parentheses.

Source: Own Calculations, based on ASSD and FWO

Table 5: The Effect of the Employment Quota on the Number of Disabled Workers per Firm by Firm Median Daily Wage (Quartiles) – Full Sample

| | 1 st Quartile | 2 nd Quartile | 3 rd Quartile | 4 th Quartile |
|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Treatment effect | 0.0892*** (0.0209) | 0.0724*** (0.0173) | 0.0422*** (0.0104) | 0.0099 (0.0106) |
| mean (dep. var.) | 0.2199 | 0.2591 | 0.2573 | 0.2796 |
| s.d. (dep. var.) | 0.6007 | 0.6706 | 0.6399 | 0.6731 |
| Number of Obs. | 119,274 | 118,762 | 107,425 | 97,327 |
| R ² | 0.0315 | 0.0276 | 0.0295 | 0.0246 |
| Adjusted R ² | 0.0315 | 0.0275 | 0.0295 | 0.0246 |
| p-value (F-statistic) | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| tax as % of monthly wage | 12.9% | 9.0% | 7.5% | 5.5% |

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.

Robust standard errors in parentheses.

All regressions are specified with linear fit and without control variables.

Source: Own Calculations, based on ASSD and FWO

Table 6: Decomposing the Treatment Effect by Employment Status at Date of Registering as Severely Disabled

| | Number of disabled workers | | | |
|--------------------------------|----------------------------|----------------------|------------------------|---------------|
| | baseline | own former employees | other former employees | non-employees |
| Treatment Effect | 0.0521 | 0.0259 | 0.0218 | 0.0044 |
| cluster: S | (0.0078)*** | (0.0050)*** | (0.0048)*** | (0.0019)** |
| cluster: S , firm | (0.0140)*** | (0.0096)*** | (0.0070)*** | (0.0043) |
| mean (dep. var.) | 0.2526 | 0.1360 | 0.0753 | 0.0413 |
| s.d. (dep. var.) | 0.6459 | 0.4574 | 0.2918 | 0.2281 |
| N | 442,788 | 442,788 | 442,788 | 442,788 |
| R ² | 0.0286 | 0.0177 | 0.0124 | 0.0050 |
| Adjusted R ² | 0.0286 | 0.0176 | 0.0124 | 0.0050 |
| Percentage w.r.t. total effect | 100 | 50 | 42 | 8 |

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.

Own former employees are individuals who had been employed with same employer at date of registering as severely disabled.

Other former employees are workers who had been employed with another employer at date of registering as severely disabled.

Non-employees are workers who had not been employed at date of registering as severely disabled.

Robust standard errors in parentheses.

All regressions are specified with linear fit and without control variables (baseline model).

Source: Own Calculations, based on ASSD and FWO

B Figures

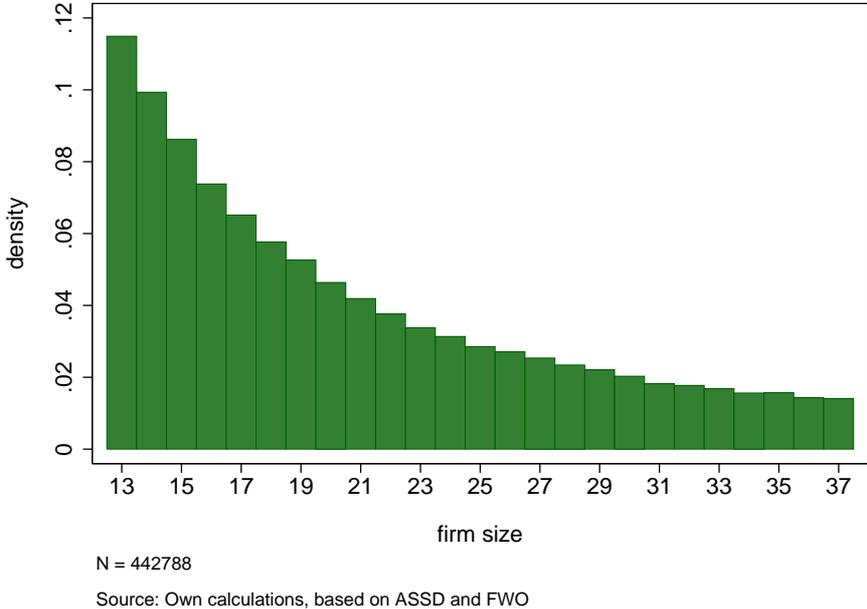
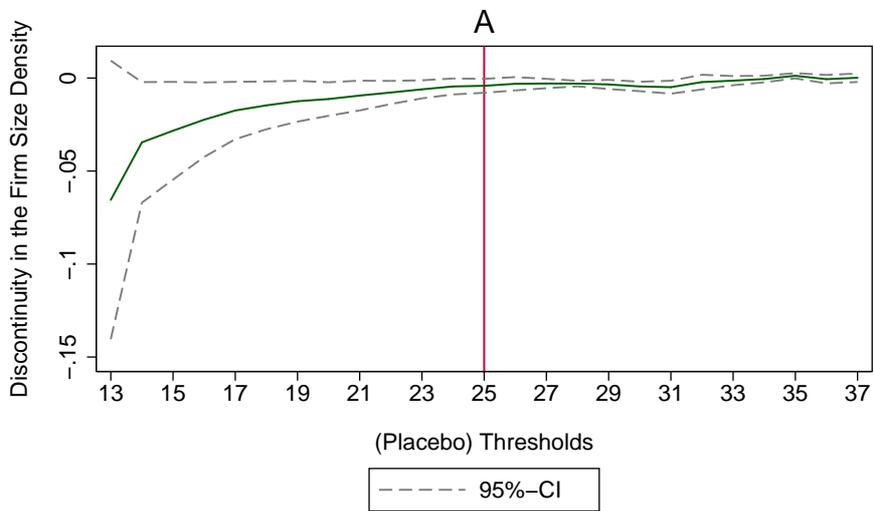


Figure 1: Firm Size Distribution



Discontinuities calculated by model 1 (quadratic fit).
 95%-CI based on standard errors clustered on firm size.

Source: Own calculations, based on ASSD

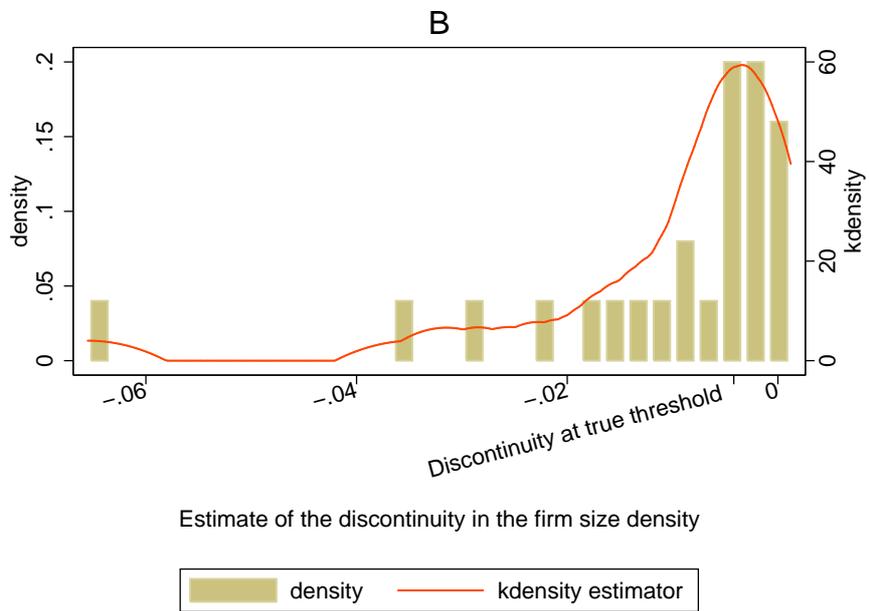


Figure 2: Discontinuities in the Firm Size Distribution (A) and Density of the Discontinuities in the Firm Size Distribution (B)

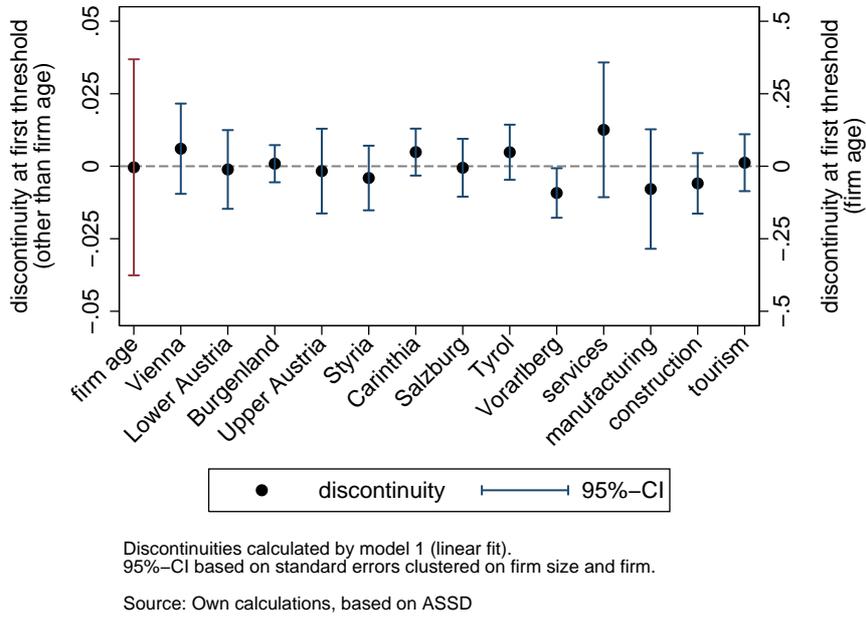


Figure 3: Discontinuities of Pre-Determined Covariates

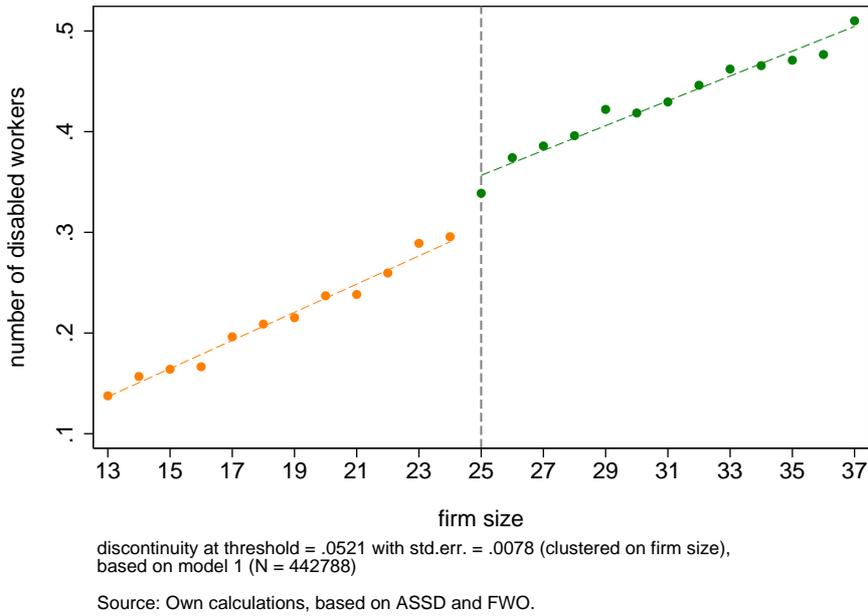
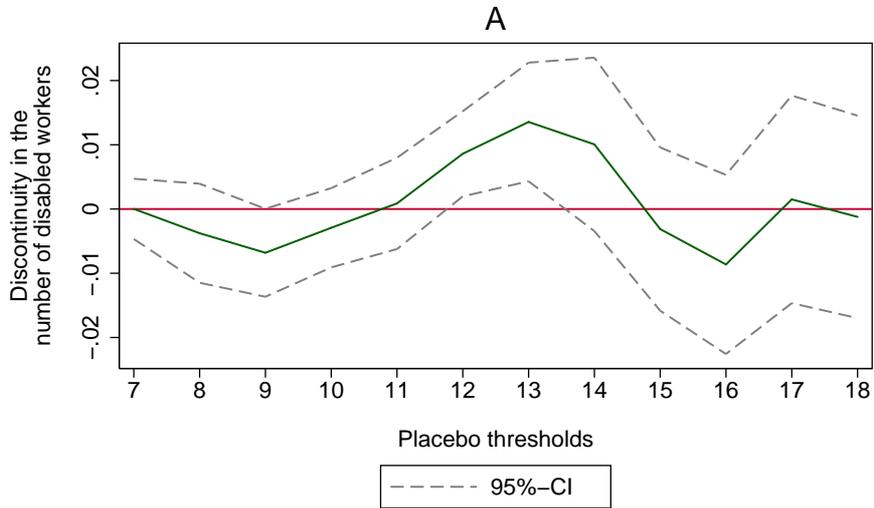


Figure 4: The Effect of the DPEA on the Number of Disabled Workers



Discontinuities calculated by model 1 (linear fit).
 95%–CI based on standard errors clustered on firm size and firm.

Source: Own calculations, based on ASSD

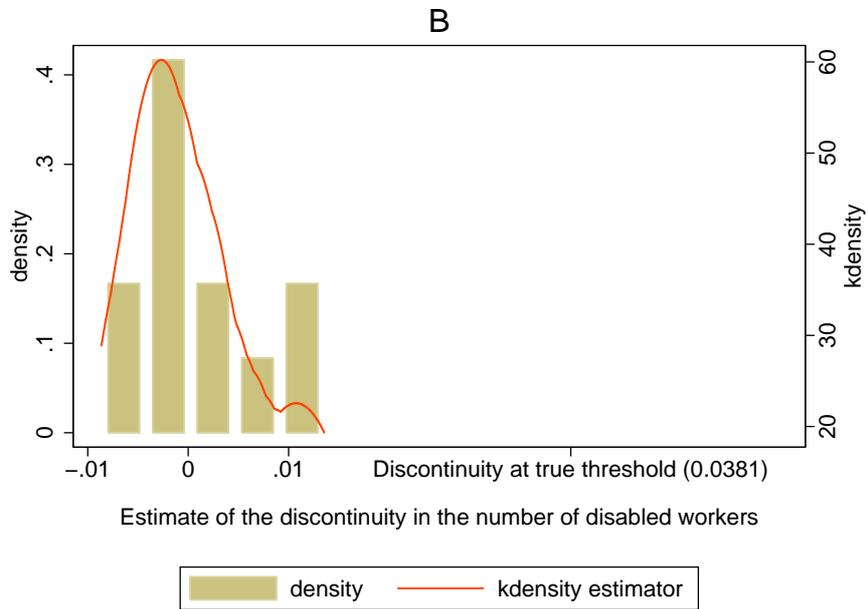
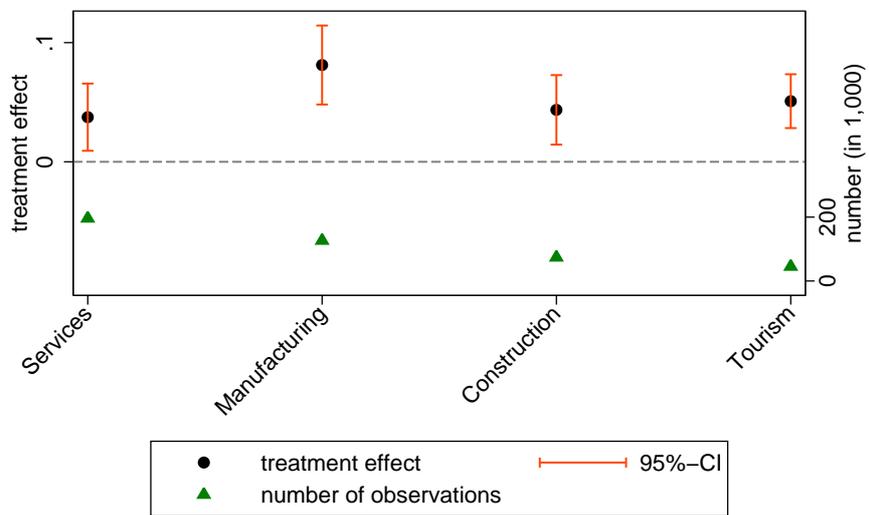


Figure 5: Discontinuities at Placebo Thresholds (A) and Density of Discontinuities at Placebo Thresholds (B)



Discontinuities calculated by model 1 (linear fit).
 95%-CI based on standard errors clustered on firm size.

Source: Own calculations, based on ASSD

Figure 6: Employment Effects by Industry