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

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Do Financial Incentives for Firms Promote

Employment of Disabled Workers?

A Regression Discontinuity Approach

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Abstract

We study the impact of employment quota on firms' demand for disabled workers. The Austrian Disabled Persons Employment Act (DPEA) requires firms to provide at least one job to a disabled worker per 25 non-disabled workers, a rule which is strictly enforced by non-compliance taxation. We find that, as a result of the discontinuous nature of the non-compliance tax, firms exactly at the quota threshold employ 0.05 (20 % in relative terms) more disabled workers than firms just below the threshold – an effect that is unlikely driven by purposeful selection below the threshold. The flat rate nature of the non-compliance tax generates strong employment effects for low-wage firms and weak effects for high-wage firms. We also find that growing firms passing the quota threshold react with a substantial time-lag but the magnitude of the long-run effect is similar to the one found in cross-section contrasts.

JEL classification: J15, J20, J71, J78

Keywords: disability, discrimination, employment, employment quota, regression discontinuity

1 Introduction

Integrating disabled workers is a key challenge in employment policy. One out of of seven individuals who live in OECD countries report a health problem that limits activities of daily life (OECD, 2003). Employment matters tremendously for disabled individuals' economic well-being. The work incomes of disabled individuals with a job are nearly as high as those of individuals without a disability. In contrast, the financial resources available to a disabled individual without a job are 46 % lower than the disposable income of an employed disabled individual. Even though work is of crucial importance for disabled individuals' material standard of living, their employment rates are substantially below those of the non-disabled.

This paper studies whether an employment quota for firms can help increase the demand for disabled workers. We study the case of Austria, where the Disabled Persons Employment Act (DPEA) defines specific employment targets, coupled with financial incentives for meeting these targets. Understanding the effects of the quota is important for several reasons. First, the two most important policies for encouraging employment of disabled workers among OECD member countries are anti-discrimination legislation and employment quotas. While the effects of anti-discrimination policies are quite well understood, the effects of employment quotas on firms' employment decisions have not yet been explored. Second, labor economists have long attempted to understand the importance of financial incentives in labor demand (Hamermesh, 1993). The employment quota policy allows studying firms' reaction to a sharp change in the relative cost of employing disabled and non-disabled workers. Arguably, this design provides important information on the extent to which firms are willing to substitute disabled workers for non-disabled workers. Third, legislation very similar to that in Austria is in force in many other OECD countries (or has been so until very recently, as in the U.K). (table 1 provides an overview). While these regulations have a core component in the form of a mandatory employment quota in common, they differ in the quota amount (ranging from 7% in Italy to 2% in Korea and Spain), in the targeted firms, and in the sanctions in case of non–compliance (ranging from 0.25% of the monthly pay-roll for firms in Germany to 4% in Italy). Hence, while the case of Austria may seem special prima facie, we believe that our results are also of high policy relevance beyond the Austrian borders.

<div align="center">Table 1 about here</div>

Under Austrian law, firms have to hire at least one disabled individual per 25 non-disabled employees. Firms that fail to comply with this obligation are subject to a tax for each unfilled quota slot. This tax is non-negligible and amounts to somewhat more than € 200 per month (or 6.3 percent of the average wage of Austrian employees). The tax revenues are used to subsidize firms that provide employment to disabled workers (regardless of whether they are subject to

<div align="center">2</div>

the employment quota). Our empirical strategy exploits the discontinuous change in financial incentives the employment quota creates. To identify the causal effect of the non-compliance tax on threshold firms employing 25 (or 50, 75, etc.) non-disabled workers, we compare the number of disabled workers in firms just below and just above the quota threshold. The central idea of this identification strategy is this: When a firm with 25 non-disabled workers decides not to hire a disabled worker, it has to pay the non-compliance tax. In contrast, when a firm with 24 non-disabled workers does not hire a disabled worker, it is not subject to this tax. We can therefore estimate the causal effect of the employment quota on the demand for disabled workers by comparing firms at the threshold to those just below it.

The key identifying assumption of this strategy is that firms do not endogenously self-select below thresholds to avoid becoming subject to the employment quota. We discuss the validity of this assumption by studying four indicators that would help detect such a 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 quota thresholds would grow more slowly than those at the threshold. We find no such pattern in the data. Third, the two populations of firms below and above thresholds are identical in terms of a range of observable characteristics. Finally, endogenous self-selection by firms which want to avoid employing disabled workers would result in an dip in terms of the number of disabled workers employed just below quota thresholds. We do not see such a dip. Thus, a comprehensive set of indicators is consistent with the central identifying assumption that firms just below thresholds 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 those of firms just below thresholds.

The empirical analysis documents five important results. *First*, firms facing the obligation to employ disabled workers do in fact employ more disabled workers than similar firms without this obligation. A comparison of firms just above the quota threshold to those just below the threshold shows that roughly 1 in 20 firms around the first threshold (25 non-disabled workers) have a disabled worker on the payroll whom they would have not hired in the absence of the employment quota. The average effect at higher order thresholds (50, 75, ...) is roughly twice as large. Both estimates suggest that firms are quite willing to substitute between disabled and non-disabled workers (with an elasticity of substitution of around 2.4 at the first threshold

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

and around 0.7 at higher order thresholds)[2]. *Second*, firms need a substantial amount of time to comply with employment quota rules. When a set of growing firms passes the first quota threshold (25 employees), only 1 in 170 firms around this threshold fulfills the employment obligation in the first month; this increases slowly to 1 in 33 firms after 2 years. This indicates substantial frictions in hiring disabled workers. *Third*, the per-head nature of the non-compliance tax is expected to generate a stronger financial incentive for firms that pay low wages than for those paying high wages. Consistent with these financial incentives, we find that firms' response to the per-head non-compliance tax decreases monotonically with a firm's position in the wage distribution. *Fourth*, our empirical analysis reveals differences in the impact of the non-compliance tax with respect to industry. While the quote effect is significant in all industries, firms in the services sector and construction firms display below-average responses to quota, while manufacturing firms react more strongly than average. This is consistent with the hypothesis that technology plays an important role in the extent to which firms can accommodate disabled workers. *Finally*, we explore the extent to which firms' employment decisions merely reflect poaching from other firms rather than creating or maintaining employment. We find that roughly 50 % of the employment effect can be attributed to workers already employed by the firm on the date of acquiring formal disability status. About 42 % of excess employment can be attributed to workers who were employed by other firms at the time of acquiring disability status. The remaining 8 % of excess employment goes to individuals who were not employed at the time of acquiring disability status.

The existing literature has extensively studied 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, have even reduced their employment chances. 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 those individuals for whom the ADA probably offered improved employment prospects, and argue that increased education participation is the result of an increase in the return on further education. Bell and Heitmueller (2005) study the effects of the Disability Discrimination Act in the U.K. Their results confirm that, as in the U.S., disability legislation did not have a significant impact on employment prospects for

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[2]We calculated this elasticity as follows: we find a discontinuous increase in the number of disabled workers of roughly 20.6% at the first threshold, where the non–compliance tax amounts to € 150, and the the average of the median monthly wage of a non-disabled worker employed at firms around the first threshold amounts to roughly € 1,800. Thus the elasticity of substitution is 2.4 ($= 20.6/[(150/1, 800) \cdot 100]$). The same calculation for higher order thresholds yields 0.7 ($= 5.3/[(150/1, 950) \cdot 100]$).

disabled individuals in the U.K. [3] The existing literature on the effects of employment quota is rather sparse. The only empirical study we know on the role of the employment quota is Wagner *et al.* (2001).[4] Their paper assesses the impact of the employment quota in Germany on job dynamics in 400 small firms and finds no effect of the quota threshold.

This paper contributes to the literature in at least three dimensions. *First*, this paper adds to the literature by adopting a regression discontinuity design to estimate the causal effect of the employment quota on the employment of disabled workers. While a large number of studies have looked at the effects of anti-discrimination legislation with respect to disabled workers, we are not aware of previous studies that attempt to evaluate the effect of quota rules on employment of disabled workers. *Second*, our evaluation is based on high-quality data from Austrian private firms and their (disabled and non-disabled) workers. In fact, we use the same data sources the Austrian social welfare authorities use to determine compliance with employment quota: the Austrian Social Security Data (ASSD) linked to data from the Austrian Federal Welfare Office (FWO). The former data set allows us to calculate the exact size of the labor force (divided into disabled and non-disabled workers) of every single Austrian firm. The latter data set allows us to assess the number of individuals with formal disability status within each firm. Since our data set covers all of the almost 240,000 Austrian private sector firms, we can provide informative contrasts of firms just below and just above the quota threshold to estimate the quota effect. *Third*, by providing evidence for low-wage and high-wage firms, for the employment dynamics of disabled workers within a firm, and for the employed disabled worker's disability/employment history, our study sheds light on potentially important mechanisms where quota rules have an impact on employment of disabled workers.

The paper is organized as follows. Section 2 provides a detailed description of institutional environment in Austria. Section 3 describes the data and Section 4 outlines the empirical strategy. Our main results are presented in section 5. Section 6 concludes.

2 Background

2.1 The Disabled Persons Employment Act

The Disabled Persons Employment Act (DPEA) was implemented in Austria in 1969; it forms the legal basis of the Austrian employment quota system. It defines the process by which individuals acquire the formal status of being "severely disabled", regulates the employment obligations for firms and the financial sanctions associated with non-compliance of these obligations, specifies rules on how to pay out subsidies to firms employing disabled workers, and

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

[4]See Welch (1976) for an early theoretical attempt to characterize the effects of quota on the labor market.

introduces employment protection rules for disabled workers. The quota rule obliges firms to hire one disabled worker per 25 non-disabled workers, leading to a quota of 4 %.[5] Firms that do not comply with this obligation are subject to a non-compliance tax. This tax is the same for each non-hired disabled worker and is independent of the wage bill paid in the particular firm. The non-compliance tax steadily increased from € 118 in 1990 to € 150 in the first half of 2001, was increased by € 46 to € 196 on July 1, 2001, and reached € 206 in 2006. This amounts to roughly 6.3 % of a worker's average monthly salary or 0.7 % of a firm's average monthly payroll in the Austrian private sector in 2006.[6]

The process by which individuals acquire the formal status of a disabled individual is as follows. In order to become entitled, disabled individuals have to file an application with the Austrian Federal Welfare Office (FWO). The application is approved once a FWO medical expert assesses a physical, mental, intellectual, or sensory disorder which reduces the individual's work capacity by at least 50 percent. Employers do not have a direct influence on the process of obtaining the formal disability status for at least two reasons. On the one hand, workplace or other accidents that lead to a sudden change in work capacity almost always trigger the disability application process automatically. On the other hand, one could argue that employers below the quota threshold might have an incentive to persuade a *de facto* disabled individual to abstain from acquiring the disability status. However, this is unlikely as these individuals would forego substantial advantages in terms of employment protection.

The group of individuals with the formal disability status 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 as disabled according to the law.[7] Recall that the employment quota specifies that 4 % of each firm's workforce be employees registered as 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 thus a de-facto tax on firms who employ at least 25 non-disabled workers.

The FWO directly enforces the employment obligation, by checking the size of each firm and the number of employed disabled workers on the first day of each month. The exact calculation of the employment quota takes the particular disabilities into account. There is some double-weighting, i.e. particular groups of disabled workers are equivalent to two disabled workers, which include the (i) blind, (ii) disabled individuals of age 19 years or younger, (iii) disabled

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[5]The Austrian quota is lower than that in Germany (5 %), France (6 %), Poland (6 %), and Italy (7%); but it is higher than that in Belgium, Korea, and Spain (2 %).

[6]In addition to the increase in the non–compliance tax increase of almost 30%, another important policy change took place on January 1, 1999. Before that, firms that employed more disabled individuals than the DPEA demanded 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.

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

apprentices, (iv) disabled individuals of age 50 or older with a degree of disability of at least 70 percent, (v) disabled individuals of 55 years or older, and (vi) individuals in a wheelchair. Finally, the FWO levies a non-compliance tax on firms that do not fulfill the employment quota.

The DPEA also defines how the revenues collected through non-compliance taxes are to be 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 fail to comply with the quota rule to firms that employ at least one disabled worker; the reallocation is used to compensate the latter for their effort in employing disabled workers. Note, however, that the subsidies are available to all firms, not just to those subject to the employment obligation. The subsidies granted in 2005 amounted to roughly € 65 Mio.

Finally, the DPEA provides increased employment protection for disabled workers, i.e. protection from dismissal and protection from wage cuts due to disability. The increased protection against dismissal is twofold. On the one hand, it stipulates that a contract may only be terminated after a probationary period of at least 4 weeks. Furthermore, dismissal is only valid if a special FWO committee agrees to it; dismissals without the consent of this committee are unlawful. However, the increased dismissal protection first comes into effect after a probationary period of six months has elapsed. The probationary period originally amounted to one month, and was extended in two steps, first from one to three months on January 1, 1999 and then from three to six months (which is currently in force) on July 1, 2001.

2.2 Hypotheses

How do quota rules and the associated financial sanctions affect firms' incentive to employ a 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 a disabled worker's productivity is arguably lower than that of a non-disabled worker. Since firms are required by law to pay disabled workers the same wages as non-disabled workers, there is a gap in terms of the marginal profit from employing a non-disabled worker compared to a disabled worker.

DPEA changes this situation in two respects. First, DPEA generally subsidizes employment of disabled workers by paying for workplace adjustments and also by covering direct wage and training subsidies. These subsidies serve to reduce the productivity gap between disabled and non-disabled workers. Second, the non-compliance tax generates a discontinuous increase on the order of 6.3 % in the relative cost of employing a non-disabled worker compared to employing a disabled worker at the quota threshold. Since subsidies are available to all firms *regardless of*

their non-disabled workforce, comparing threshold firms to those just below the threshold identifies substitution effects of small changes in the relative cost in a context with small productivity gaps between non-disabled and disabled workers. In this context, even small changes in cost can lead to large changes in firm's employment decisions.

Moreover, we expect that the effects of the non-compliance tax depends on a firm's position in the wage distribution and on technology. With regard to wages, the flat rate nature of the tax implies that the incentive to employ disabled workers will be stronger for firms at the lower end of the wage distribution than for firms at the upper end of the wage distribution. We therefore assess the effect of the non-compliance tax for firms grouped by the quartiles of the firm wage distribution. With regard to technology, the non-compliance tax 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.

The final key question examines the extent to which firms' employment decisions merely reflect poaching from other firms rather than creating or maintaining employment. We assess this important issue by analyzing three groups of disabled workers: workers who had already been with the firm when acquiring the protected status, workers who had been employed by other firms at that time, and individuals who were not-employed when applying for protection. Full reallocation of disabled workers can be detected by decomposing the quota employment effect. Full reallocation means that all excess employment in threshold firms goes to workers from other firms. Employment promoting effects of the quota show up for former employees (with threshold firms maintaining their jobs), or for individuals who were not employed when registering for protection (new employment).

3   Data and Empirical Strategy

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 from 1972–2002 on a daily basis together with an unambiguous firm identifier, as well as firms' industry affiliation and location and (ii) personal data from the Austrian Federal Welfare Office (FWO) which reports disability status, disability type, and disability degree for all individuals disabled in the sense of the Employment Act for Disabled Individuals (DPEA). One advantage of this type of information is that a medical procedure (rather than self-reported by firms or workers) objectively assesses disability status. Note, however, that the FWO data set is inflow-based; its records began in January 1970. This means that the stock of disabled

workers might be incompletely captured in the early stages of this data. This drawback gradually vanishes if a snapshot of the stock of disabled workers is taken at a later time. Accordingly, we will only use data from very recent years (see below for details). The ASSD and FWO data can be linked on the basis of a person identifier. This allows us to calculate two crucial pieces of information accurately: the number of the non-disabled workers and the number of disabled workers each firm employs. The former variables determines whether a firm is required to hire a disabled worker and the latter represents how many disabled workers each firm actually employs. Hence we can precisely determine whether and and the extent to which a firm complies with the employment quota. The FWO checks firms' compliance 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, all of which correspond to the first day of each month.[8]

Since the 1990s, two major changes relevant for the employment quota occurred in the DPEA: (i) an "over-compliance premium" (of the same amount as the non-compliance tax) was abandoned on January 1, 1999 and (ii) the non-compliance tax was increased to € 200 on July 1, 2001.[9] We restrict our sample to the time period between January 1999 and and June 2000 for two reasons. First, these policy changes do not affect this time span. Second, as the FWO data only provide information on the inflow into recognized disability starting in 1970, restricting the sample to dates after 1999 makes it very likely that our data capture the entire stock of disabled workers.[10] We further restrict the analysis to firms in the private sector – those likely to pursue a clear, profit maximizing objective. In particular, we look at firms operating in the services sector, manufacturing, construction, and the tourism industry.

We focus on firms' employment decisions with respect to disabled workers just above and just below quota thresholds $T \in \{25, 50, 75, ...\}$. This sample restriction effectively means that we study the effect of quota rules on firms (i) on the margin of entering the employment quota system when the quota threshold $T = 25$ is considered, and (ii) on the margin of being induced to hire an (additional) disabled worker when any quota threshold $T > 25$ is considered. We define just above and just below as firms with firm size $S \in [T - 12, T + 12]$. This bandwidth is the largest bandwidth that does not result in overlapping firm sizes across thresholds.[11] We

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[8]Note that simply hiring a disabled worker for only one day a month (i.e. the first day of each month) in order to comply with the employment quota is not feasible for firms. An imposed regulatory restriction of the DPEA rules out this behavior. 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 (s.d.) number amounts to 30.12 (1.86) days for disabled and 29.93 (1.39) for non-disabled workers.

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

[10]In 2000, the mean retirement was 59 years for men and 57 years for women. This means that our data does not provide information on disability status for those individuals who attained the recognized disabled status before they were 30 years old. Since less than one fourth of the disabled individuals attain disabled status before this age, we do not expect this drawback in our data to be of major significance.

[11]When analyzing the first threshold with this bandwidth, firm sizes up to 37 (= 25+12) are taken into account. The smallest firm size considered for the analysis at the second threshold amounts to 38 (= 50 − 12). Increasing the bandwidth would therefore lead to overlapping firm sizes across thresholds.

test the sensitivity of the results to the choice of bandwidth by further restricting the sample to $S \in [T-9, T+9]$ as well as $S \in [T-6, T+6]$. It turns out that our main findings are not sensitive to the choice of bandwidth. Restricting the analysis to observations 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 will also use regression techniques to further account for the effects of firm size. Finally, note that the number of firm–month observations is largely decreasing in firm size. There are therefore only very few firm–month observations for very large firms. In order to cover all possible firm size values around each threshold, we only keep firm–month observations associated with a threshold with a complete set of firm size values. As a result, we only consider firm-month observations around the $1^{st} - 37^{th}, 42^{nd},$ and 50th threshold. In the light of very few firm–month observations around thresholds $T > 25$, we decided to pool firm–month observations around these thresholds, i.e. we assign treatment status according to the deviation from thresholds $\tilde{S}_i \equiv (S_i - T)$, where T represents the nearest threshold S_i is associated with. Firms are treated if $\tilde{S}_i \geq 0$ and non-treated if $\tilde{S}_i < 0$.

Table 2 reports key background statistics on firms located around the threshold $T = 25$. Panel A of table 2 shows the two key indicators. The first line provides information on the average number of jobs provided to registered 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 employs a disabled 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, roughly twice as many as non-treated firms do. Thus, *prima facie* evidence is consistent with an employment promoting effect of the quota rule.

Table 2 about here

Row 2 of panel A of table 2 clearly indicates that treated and control firms are quite different and hence comparing the raw differential does not provide causal evidence on the effects of employment quota. Treated firms are, by construction, larger than control firms. Whereas control firms employ 17.10 non-disabled workers on average, 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 2 displays information on firm size dynamics. The indicator "unchanged workforce since last month" measures stability of workforce 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 did not see any change in their workforce over a month, 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 expanded their employment during that past 6 months. In contrast, 39 % of treated firms downsized within the last six months compared to 34 % of control firms. This 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 C of table 2 provides further background information on firms. It reports information on a firms' workforce structure, pay, age, tenure, and apprentices. Note that all of these measures only take non-disabled workers employed by the firm in month t into account. 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 each firm pays 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 (or about 1764 € per month), whereas treated firms pay almost 3 € per day more (62.6 € per day or 1878 € per month). Firms located around the first quota threshold face a non-compliance tax of € 150 – or about 8 % of the median wage they pay their non-disabled workforce. 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 considerably more than control firms (1.4 jobs).

Panel D of table 2 contains information on firms that the treatment should not affect, i.e. variables that are pre-determined with respect to the DPEA: firm age, location, and industry. Firm age measures the number of years the firm number has been observed in ASSD since 1972 – the year ASSD started.[12] Results indicate that the average control firm was founded 15.9 years

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[12]Note 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 the 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.

before the current date, whereas treated firms were established almost exactly 1 year earlier. While 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, 11 % of control firms are in the tourism industry, while only 9 % of the treated firms are.

The figures of Panel B and C of table 2 reveal that there are some differences between treated and non-treated firms in variables that directly relate to firm size (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. Descriptive statistics for the pooled higher thresholds are available in table 3 in the appendix.

4 Identification and Estimation

Our empirical strategy is based on the fact that the DPEA discontinuously changes the financial incentives for employing disabled workers. The DPEA requires that firms 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 $T \in \{25, 50, 75, ...\}$. 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 paying a compensation to be rid of 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 tax 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)).[13] 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 identification of the discontinuity in the average number

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[13]This basically means that the demand of firms for disabled workers is continuous at the threshold in absence of the employment quota. Note that this also applies for the labor supply of disabled workers, since all provisions provided by the DPEA (increased employment protection, wage subsidies, workplace accommodation, etc.) are available independently of the firm size.

of disabled workers per firm at treatment assignment thresholds $T \in \{25, 50, 75, ...\}$:

$$Y_{it} = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot (S_{it} - T) + \beta_1 \cdot D_{it} \cdot (S_{it} - T) + X_{it}'\gamma + \pi_t + \epsilon_{it}, \tag{1}$$

where Y_{it} denotes the number of disabled workers, D_{it} indicates whether a firm is treated or not, and S_{it} denotes firm size of firm i at date t. Including S_{it} is important since larger firms tend to provide more jobs to 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 firm size S_{it} and the average number of disabled workers per firm. In some specifications, we additionally control for X_{it}' (a vector containing all covariates displayed in Panel B-D of table 2 or table 3 respectively) and time fixed effects π_t (30 monthly dummies). X_{it}' allows improving the precision of the RDD estimates, and π_t controls for changes over time that potentially affect the hiring strategy of either disabled or non-disabled workers, such as economic conditions, for example. It turns out, however, that the results are neither sensitive to the inclusion of X_{it}' nor π_t.

There are three issues of particular relevance in the present context. First, note that firm size S_{it} has discrete support. Discrete support implies that we need to extrapolate in order to predict the counterfactual for threshold firms – the number of disabled workers threshold firms employ in the absence of the fine. The baseline model assumes a linear functional form, but we also add quadratic terms in S_{it} as a sensitivity analysis. Moreover, we only consider firms within a window of size 12 below and above the threshold, thus $S_{it} \in [T - 12, T + 12]$. This sample restriction is intended to downplay the importance of the functional form between the mean number of disabled workers and non-disabled firm size. Note that this is the largest bandwidth that ensures non-overlapping firm sizes across thresholds. We test the sensitivity of our results with respect to the choice of bandwidth by further restricting the sample to firms with firm sizes $S_i \in [T-9, T+9]$ and $S_i \in [T-6, T+6]$. It turns out that the choice of bandwidth does not drive our main findings. Discrete support also affects variance-covariance matrix estimates. Lee and Card (2008) suggest using 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 not only need to cluster on S_{it} but also on firms (note that this is non-nested). Cameron *et al.* (2006) propose a new variance estimator for OLS that provides 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_{it} and (ii) those that are clustered on S_{it} *and* firm identifiers.

Second, the baseline specification may be sensitive to unobserved firm heterogeneity, and it does not provide information on the timing of firms' employment decisions. Comparing threshold firms to those just below the threshold regardless of how long threshold firms have been subject to the quota, the baseline strategy provides an estimate of the long-run effect of employment quota on disabled employment. We assess sensitivity to unobserved heterogeneity and timing of firms' employment decisions in three ways. We begin by estimating the baseline specification by firm fixed effects to identify the employment quota effect; we use firms that cross the quota threshold within the sample period of 30 months. This provides a medium-run estimate of the employment quota on threshold firms which is unaffected by the presence of time invariant firm heterogeneity. We then add an interaction term between the treatment dummy D_{it} with the number of months a firm had been exposed to the quota in the baseline model in the past, i.e. $D_{it} \sum_{j=0}^{t-1} D_{ij}$, and re-estimate it by firm fixed effects. This allows assessing both the immediate effect of crossing the threshold as well as the dynamics of the treatment effect for firms that cross the threshold within the sample period. We ultimately provide estimates of the treatment effect in a series of difference models. The first difference specification of the baseline model – relating the month–to–month change in disabled employment to the month–to–month change in treatment status – identifies the immediate impact of crossing the threshold. We also provide estimates at higher order differences to assess how time since crossing the threshold affects disabled employment.

Third, we assess whether there is endogenous selection of firms at the quota threshold studying firm size density, firm dynamics, and pre-determined characteristics of firms. Regarding firm size, firms might stay just below the threshold in order to avoid becoming subject to the non-compliance tax. 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 $T = 25$. Visual inspection suggests that no important spike is present.[14]

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[14]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 appropriate with regard to figure 1), where we used the firm size density as a dependent variable. A firm size bandwidth $S_{it} \in [T - 12, T + 12]$ 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). However, when we run the same regression for a set of placebo thresholds $\ddot{T} \in \{13, 14, ..., 23, 24, 26, 27, ..., 37\}$ using the density for firm sizes ranging from 1 to 49, we also find discontinuities at these placebo thresholds which tend to be somewhat larger at lower firm sizes and then level off as firm size increases. Moreover, the discontinuity at the true threshold 25 is well in line with the discontinuities estimated at placebo thresholds. These results suggest that the discontinuity in the firms size density estimated at the quota threshold is a statistical artifact rather than due to purposeful self-selection by firms. Furthermore, pooled quota thresholds $T > 25$ figure 2, detailed in the appendix, also fail to show a spike. An application of the same formal test as for quota threshold $T = 25$ yields no discontinuity around quota thresholds $T > 25$ (using a linear as well as a quadratic functional form).

Figure 1 about here

With regard to firm dynamics, we look at the firms' employment fluctuation and employment growth. The idea is that firms just below the threshold avoid growing, 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 but use the following three dependent variables: (i) i.e. whether a firm has had any changes in its workforce from $t-1$ to t, (ii) whether a firm has expanded between $t-6$ and t, and (iii) whether a firm has contracted between $t-6$ and t. The results are summarized in table 4 (the dependent variable in all regressions 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 is that indicating whether a firm has grown. The estimate suggests that threshold firms expanded 1.7 percentage points less 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 4 column (2)).[15]

Table 4 about here

With regard to pre-determined variables, we study whether firm age, industry, and firm location exhibit any discontinuity at the threshold. The baseline model with a linear fit in firm size 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 quota threshold $T = 25$. With the exception of one region (Vorarlberg), none of the firm characteristics display significantly different means for quota threshold firms when compared to firms just below the quota threshold.[16] 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

Main Results for Quota Threshold $T = 25$**:**   This section presents the main econometric estimates of the effects of the DPEA on the number of disabled workers per firm at quota

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[15]For pooled quota thresholds $T > 25$ we conduct the same tests for self–selection as for quota threshold $T = 25$. Results are displayed in table 5 in the appendix. The coefficients are neither economically nor statistically significant.

[16]With the exception of two regions (Burgenland and again, Vorarlberg), non of the predetermined covariates turn out to have different means for pooled quota thresholds $T > 25$ (see figure 4 in the appendix).

threshold $T = 25$ (for the effect of the employment quota at thresholds higher than $T = 25$ see further below).[17] Figure 5 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 subject to the quota. Specifically, firms that 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 that employ 24 non-disabled workers provide 0.3 jobs to disabled workers. Figure 5 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 the behavior of firms 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.

<center>Figure 5 about here</center>

Figure 5 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 $T = 25$ of 0.0521 (standard error clustered on firm size is 0.0078; standard error clustered on firm size and firm is 0.0140). This discontinuity is statistically significant at the 1%-level, but the effect 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 ($= 0.0521/0.2526$). Put differently, roughly one out of 20 firms employs one additional disabled worker due to the DPEA.[18]

Table 6 shows our main results for the effect of the employment quota on jobs provided to disabled workers. Note first that the choice of clustering 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 5 and constitutes our baseline specification.

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[17]We put our main focus to this threshold for two reasons. First, firms at higher order thresholds are already subject to the quota system. Studying the first threshold allows analyzing the effects of being subject to or free of the quota system. Second, there are many 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 subject to the quota.

[18]Moreover, figure 5 also provides supplementary evidence concerning endogenous self-selection of firms below the threshold. Arguably, firms that face strong difficulties in accommodating a disabled worker and that self-select 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 as an unexpected dip in the number of disabled jobs just below the quota threshold. In contrast, results in figure 5 suggest that firms just failing the quota threshold employ 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 the self-selection of firms with respect to their likelihood of providing employment for disabled workers.

<center>16</center>

The validity of RD estimates hinges on the appropriate specification of the relationship between firm size and the number of disabled workers. If this relationship is misspecified, an apparent jump at the threshold might simply be a marked nonlinearity. Column 2 adds quadratic terms in excess firm size $S_{it} - 25$. The result indicates that the effect of 0.0351 is somewhat weaker, but remains statistically significant at the 1%-level. In Column 3, we add all covariates displayed in Panel B-D of table 2 as well as 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. This confirms our finding from section 4 that control variables are balanced at the quota threshold. Another issue is the sensitivity to the choice of bandwidth. In the first three columns in table 6, 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 (i.e. $S \in [16, 34]$, thereby losing one third of all firm-month observations) in column 4 and by one-half (i.e. $S \in [19, 31]$, 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. Both effects remain statistically highly significant at the 1%-level, however.[19]

Table 6 about here

Is this effect quantitatively large? A lower bound on the extent to which firms substitute disabled workers and non-disabled workers can be calculated as follows. The lowest estimate of the treatment effect suggests that the quota leads to 0.0351 more disabled workers holding a job in threshold firms – an increase of about 12 % compared to the recognized disabled workforce of 0.30 disabled workers at the quota threshold. This change in disabled worker employment is triggered by a non-compliance tax which stands on the order of 8 % of the median non-disabled worker wage (€ 150 in fine per month relative to about € 1800 in wages per month). The elasticity of substitution between disabled workers and non-disabled workers is therefore at least on the order of 1.4 ($= 12/[(150/1,800) \cdot 100]$). As Acemoglu and Angrist (2001) conjectures, disabled and non-disabled workers are quite strong substitutes.

Placebo and Robustness: 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

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[19]Estimates of the causal effect of the employment quota on the 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 fewer jobs for disabled workers than would be expected from a linear regression using firms with 26 to 37 non-disabled workers. Introducing higher order terms in firm size or restricting bandwidth means putting more weight on threshold firms. This reduces the estimate of the causal effect of the employment quota. We show below that this is consistent with lags in firms' employment decisions.

no discontinuities. For this purpose, we focus on observations with firm sizes lower than 24, i.e. we only considered firms 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 $\bar{T} \in \{7, 8, ..., 18\}$). Figure 6 reports the results. Figure 6A (plot of the discontinuities and 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, this is rather small in comparison 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 6, panel A, column 5) Figure 6B 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 artifact.

Figure 6 about here

Next, we assess whether the results are sensitive to the choice of the dependent variable. Instead of using the number of disabled workers per firm, we could also have defined the dependent variable as the percentage of disabled employees in each firm. One can argue that when the number of disabled workers per firm is expressed in percentages, the results may be less sensitive to misspecifications of the relationship between the number of disabled workers and the firm size. Table 7 displays the results. Note that it is organized in the same way as table 6. Column 1 shows the baseline effect. Firms employ 0.1819 percentage points more disabled workers than they would in the absence of the employment quota. For a firm just above threshold $T = 25$, this effect translates into 0.0455 disabled workers ($= (0.1819 \cdot 25)/100$). This is perfectly in line with our previous findings (as are the results in columns 2–5), which suggests that our results are not sensitive to how the dependent variable is defined (i.e. whether disabled workers are expressed in levels numbers or percentages). Evidence in table 7 again suggests that the elasticity of substitution between disabled and non-disabled workers is at least 1.3 ($= 11/[(150/1,800) \cdot 100]$). This estimate is somewhat lower than the baseline estimate because the number of disabled workers strongly increases with firm size, whereas the percentage disabled workers does not change with firm size.

Table 7 about here

Dynamics: Up until now, we have analyzed the data using a static RD approach (applied to repeated cross–sectional data). This allows estimating the long run effect of being subject to the non-compliance tax on the demand for disabled workers free of bias from the correlation of being treated and observed firm characteristics. In the real world, firms need time to adjust their

workforces. There are at least four reasons why excess employment would build up over time. First, threshold firms may not be fully informed about the employment obligation. Second, threshold firms may need time to search and/or to provide workplace accommodation to fill a job vacancy with a disabled worker. Third, threshold firms may only be able to learn over time whether they have only temporarily or permanently crossed the quota threshold. Fourth, excess employment may also build up over time due to retention. If the non-compliance tax drives a wedge between the separation rate of disabled workers in threshold firms compared to their colleagues in firms below the threshold, excess employment levels will diverge slowly over time. This section now investigates short– and medium–run effects (up to 24 months) at the quota threshold $T = 25$.

For the long–run effect, we used a sample that was restricted to firm–month observations in a firm size bracket of $S_{it} \in [13, 37]$. We extend this original sample to cover firms in the firm size bracket $S_{it} \in [1, 100]$ in order to comprehensively exploit the panel structure of the data, i.e. 5,176,476 firm–month observations (233,775 firms).[20] Table 8 reports the results.[21] Column 1 corresponds to the baseline specification (1). Results agree almost one–to–one with our main result of column 1 in table 6 with respect to the coefficient as well as standard error. This suggests that extending the sample selection does not affect estimates of the long-run effect of being subject to the employment quota. Column 2 adds firm fixed effects to the baseline regression. The treatment effect collapses to 0.0161, i.e. only to one–third of the baseline effect, but remains statistically significant at the 1 %–level. This suggest that either unobserved firm heterogeneity or timing of employment decisions or both may play an important role in driving the baseline results.

To study the timing of employment decisions, column 3 of table 8 adds an interaction term between the treatment indicator and the number of months a firm has been treated up to t. The main effect of being at the employment quota threshold is not statistically significant. Yet the interaction term stands at 0.0009 and is statistically significant at the 5 %–level. This means that firms do not immediately respond to the employment quota. However, the longer a firm is exposed to the quota, the more it expands employment of disabled workers. For instance, the excess demand for disabled workers amounts to $0.0301 (= 0.0085 + 24 \cdot 0.0009)$ after 24 months, or to about two–thirds of the long–run effect from the static RD approach (table 6). Column 4 provides results of a 24 month difference specification of the baseline model (1). The idea behind this regression is to examine employment of disabled individuals by firms up to 24 months after crossing the employment quota threshold $T = 25$. Results indicate that this effect amounts to

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[20]The results presented in this section are robust to this adjustment in the sample selection (none of the results change if we use only firms in the firm size bracket $S_{it} \in [13, 37]$).

[21]The regressions neither contain control variables nor time fixed–effects. Note that adding them does not change the results presented in this section.

0.0276 (statistically significant at the 1 %–level), which is consistent with our finding in column 3.

Table 8

Figure 7 shows the treatment effects (and the corresponding 95–% confidence interval) for lag–τ differences with $\tau \in \{1, 2, 3, ..., 24\}$.[22] We see that the effect for the first difference amounts to only 0.0059, but increases steadily with τ to 0.0276 at $\tau = 24$ (the treatment effects are statistically significant at the 5 %–level for all possible values of τ). Results in figure 7 provide evidence that firms respond sluggishly to the employment quota. This is consistent with findings from the fixed effects model. Taken together, dynamic results (columns 3 and 4 of table 8) suggest that sluggish adjustment to employment quota rather than unobserved heterogeneity in firms explains why fixed effects results (column 2 of table 8) are weaker than level results in (column 1 of table 8).

Figure 7 about here

Extensions: Next, we turn to discussing heterogeneity of the treatment effect. Table 9 reports the causal effect of the employment quota for firms in different parts of the firm wage distribution at quota threshold $T = 25$. We group firms according to the median daily wage paid to their workers in the period 1999 to 2001. 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 9).

Table 9 about here

Results indicate that the employment quota produces a strong increase in the workplaces available to disabled workers among firms located in the first quartile of the wage distribution. Quota firms provide 0.089 workplaces for 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 considerably less than firms below the median. Threshold firms in the third quartile

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[22]Note that the number of observations (almost linearly) decreases from originally 4,899,035 firm–month observations for the lag–1 regression to only 781,068 firm–month observations for the lag–24 difference regression, which explains why the confidence interval widens with τ.

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 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 the employment quota are very much in line with the pattern of relative impact generated by a flat rate tax. Moreover, except for the top quartile, results consistently suggest that the elasticity of substitution between disabled and non-disabled workers exceeds 2.

We further look at the effect of heterogeneity with respect to industry affiliation. Figure 8 displays the discontinuities in the number of disabled workers per firm for each industry at the quota threshold $T = 25$. 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 6. The y-axis on the right hand side measures the number of firm-month observations used for the calculations. Figure 8 reveals that the treatment effect is only somewhat heterogenous across industries at the threshold. The manufacturing industry reacts most strongly 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. There are several reasons why the quota generates slightly more employment in manufacturing than in the services or construction sector. On one hand, manufacturing is probably less labor intensive compared to construction and services. Thus, reduction in physical labor capacity are less likely to be relevant in manufacturing.

Figure 8 about here

The employment quota may also act differently for large firms than for small firms. On one hand, large firms pay higher wages, implying that financial incentives should have less bite than for small firms. On the other hand, existing evidence strongly suggests that firm size is positively related to employment of the disabled. This may be because large firms find it easier to accommodate disabled workers. Table 10 shows the results for pooled higher order thresholds (50, 75, ...). Here we assign treatment status according to the deviation from thresholds $\tilde{S}_{it} \equiv (S_{it} - T)$, where T represents the nearest threshold S_{it} is associated with. Firms are treated if $\tilde{S}_i \geq 0$ and non-treated if $\tilde{S}_i < 0$). Column 1 in table 10 displays the baseline effect at the quota threshold (we repeat the estimate in column 1 of table 6 for ease of comparison). Column 2 shows the effect of the employment quota on the number of disabled workers per firm at pooled

quota thresholds $T > 25$.[23] The effect of having to offer a job to at least one additional disabled worker is 0.1118 – more than twice as large as the baseline effect at quota threshold $T = 25$ and statistically significant at the 1 %–level. Put differently, roughly one in ten firms employs one additional disabled worker due to the DPEA. From column 3 we infer that the result in column 2 is not sensitive to adding controls and time fixed–effects. Column 4 uses the percentage of disabled workers per firm as dependent variable instead of the level. The effect amounts to 0.1218 percentage points and is statistically significant at the 1 %–level. This translates into an effect of 0.1486 ($= 0.001218 \cdot 122$) disabled workers for the average firm in the large firm sample. Thus, our result that the treatment effect is roughly twice as large as for firms around quota thresholds $T > 25$ than for firms around quota threshold $T = 25$ is quite robust. What is the substitution elasticity in large firms? Firms in the pooled sample pay about 1950 € – the non-compliance tax is about 7.7 % of the median non-disabled worker wage. The employment effect is on the order of 5.3 % of average disabled worker employment. The substitution elasticity is therefore on the order of 0.7 for large firms – somewhat lower than among small firms.

Table 10 about here

In table 6 above, we showed that treated firms employ 0.0521 disabled workers more than non-treated firms at threshold $T = 25$. Table 11 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 on the date of registration as disabled (*own former employees*), who had been employed with another firm on the date of registratoin as disabled (*other former employees*), and who had not been employed at the time of registration as disabled (*non-employees*).

Providing information on the effects for these three groups of workers is important. Own former employees were already employed with the current employer before attaining the status of being disabled. This means that the current employer is, arguably, quite well informed about the disabled individual's on-the-job productivity. 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 who would not have been kept without the employment obligation. Other former employees were not 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 an examination of the role of DPEA in reallocating jobs between the current firm and other different firms. Non-employees were not working on the date of they attained status

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[23]Column 2 neither includes control variables nor time fixed–effects but columns 2–4 do include dummies for the threshold closest to each firm-month observation to control for differences in disabled employment across thresholds.

as disabled. The current employer therefore has equally poor information regarding the hired person's on-the-job productivity 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 11 provides information on the separate effects of DPEA on workers of different types. Column 1 in table 11 displays the baseline effect at the quota threshold $T = 25$ (we repeat the estimate in column 1 of table 6 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 employed by a different firm (column 3), and workers who had not been employed at the time of registration as 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 as 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 reflects 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 as 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 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.

<center>Table 11 about here</center>

6   Conclusion

This paper analyzes the effect of an employment quota in promoting employment for 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 understand the employment quota for disabled workers and, in doing

<center>23</center>

so, complements existing evidence on anti-discrimination legislation. The identification strategy relies on the sharp discontinuity in the relative costs of employing 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 the quota promotes the employment of disabled workers in firms located at the quota threshold, in comparison to firms just below the quota threshold. The quota leads to excess employment of one disabled worker per 20 firms. Firms respond sluggishly to the quota, creating about one job per 50 firms in one year. We also detect important interactions between wages, industry, and firm size. Firms in the lower end of the firm wage distribution tend 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 in labor intensive sectors such as services, tourism, or construction. The employment quota leads to twice as much excess employment among large firms rather than among small firms. We also find that the quota boosts employment primarily among former employees of the firm. The quota also encourages firms to poach workers from other firms and to hire individuals who were not formerly employed.

We conclude that the financial sanctions accompanying the employment quota do indeed increase compliance with the quota. This is a first result that is necessary for the quota to promote overall employment for disabled workers. We also show that the quota employment effect is not entirely due to reallocation of disabled workers between firms. Taken together, these results suggest that overall disabled employment may increase due to the employment quota. However, the employment quota may displace non-disabled workers, thereby potentially decreasing overall employment. Further research should therefore put emphasis on evaluating this policy instrument in other contexts and compare the relative effectiveness of quota with anti-discrimination legislation.

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A Tables

Table 1: OECD Countries with Employment Quotas

Country	Quota	Targeted firms	Sanction
Austria	4%	private and public employers with over 25 employees	€ 200.- per month for each place not filled (0.4% of payroll)
Belgium	2–2.5%	only public employers	–
France	6%	public and private employers with over 19 employees	€ 150–250 per month (0.45–0.75% of payroll)
Germany	5%	public and private employers with over 19 employees	€ 100–250.- per month for each place not filled, depending on fulfilment (0.25–0.65% of payroll)
Italy	7%	public and private employers with over 50 workers, one/two places for 15–35/36–50 employees	€ 1,075.- per month for each place not filled (4% of payroll)
Korea	2%	public sector and private employers with over 300 employees	€ 324.- per month for each place not filled (0.5% of payroll)
Poland	6%	public sector and private employers with over 50 employees	40.65% of average wage per month for each place not filled (2.4% of payroll)
Spain	2%	public sector and private employers with over 50 employees	–

Source: OECD (2003)

Table 2: Descriptive Statistics around Quota Threshold $T = 25$

	firm size 13-24		firm size 25-37	
	mean	(std. dev.)	mean	(std. dev.)
Panel A: Outcome and treatment assignment variable				
number of disabled	0.19	(0.54)	0.42	(0.86)
firm size	17.10	(3.33)	30.13	(3.71)
Panel B: Firms' fluctuation and growth w.r.t. non-disabled workforce				
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)
Panel C: Other covariates				
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)
Panel D: Pre-determined covariates				
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	
Number of firms	22,368		8,908	
Total number of firms			25,687	

Notes: * denotes that variable bases on characteristics of non-disabled workers only.

Source: Own calculations, based on ASSD and FWO.

Table 3: Pooled Descriptive Statistics around Quota Thresholds $T > 25$
$(2^{nd} - 37^{th}, 42^{nd},$ and 50th threshold)

	firm size 13-24		firm size 25-37	
	mean	(std. dev.)	mean	(std. dev.)
Panel A: Outcome and treatment assignment variable				
firm size	102.31	(111.89)	130.15	(126.47)
number of disabled	1.81	(4.30)	2.44	(5.26)
Panel B: Firms' fluctuation and growth w.r.t non-disabled workforce				
unchanged workforce since last month*	0.11	(0.31)	0.07	(0.26)
expanded since 6 months*	0.50	(0.50)	0.51	(0.50)
contracted since 6 months*	0.41	(0.49)	0.42	(0.49)
Panel C: Other covariates				
share of white-collar workers*	0.45	(0.33)	0.45	(0.32)
share of women*	0.37	(0.27)	0.37	(0.27)
worker's age* (in years)	36.07	(4.04)	36.24	(3.93)
median daily wage* (€)	64.82	(20.56)	65.50	(20.80)
tenure* (in years)	6.09	(3.74)	6.33	(3.80)
number of apprentices*	4.27	(7.83)	5.22	(9.15)
Panel D: Pre-determined covariates				
age of firm (in years)	18.60	(10.08)	19.25	(10.01)
Vienna	0.22	(0.42)	0.22	(0.42)
Lower Austria	0.17	(0.38)	0.18	(0.38)
Burgenland	0.03	(0.16)	0.03	(0.16)
Upper Austria	0.19	(0.39)	0.19	(0.39)
Styria	0.10	(0.30)	0.10	(0.30)
Carinthia	0.06	(0.25)	0.07	(0.25)
Salzburg	0.08	(0.27)	0.08	(0.27)
Tyrol	0.09	(0.29)	0.08	(0.28)
Vorarlberg	0.05	(0.22)	0.05	(0.22)
services	0.45	(0.50)	0.45	(0.50)
manufacturing	0.35	(0.48)	0.36	(0.48)
construction	0.14	(0.35)	0.13	(0.34)
tourism	0.06	(0.24)	0.05	(0.22)
Number of firm-month observations	118,192		90,475	
Number of firms	9,117		6,746	
Total number of firms		9,475		

Notes: * denotes that variable bases on characteristics of non-disabled workers only.

Source: Own calculations based on ASSD and FWO.

Table 4: Discontinuities in Firms' Turnover and Growth at Quota Threshold $T = 25$

	(1)	(2)
stable workforce since last month	0.0118	0.0019
(cluster: firm size)	(0.0069)	(0.0068)
(cluster: firm size *and* 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 *and* 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 *and* 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 5: Discontinuities in Firms' Turnover and Growth at Pooled Quota Thresholds $T > 25$

	(1)	(2)
stable workforce since last month	0.0021	-0.0072
(cluster: firm size)	(0.0047)	(0.0069)
(cluster: firm size *and* firm)	(0.0050)	(0.0068)
[R^2]	[0.0485]	[0.0485]
expanded since 6 months	-0.0020	0.0065
(cluster: firm size)	(0.0045)	(0.0052)
(cluster: firm size *and* firm)	(0.0058)	(0.0057)
[R^2]	[0.0021]	[0.0021]
contracted since 6 months	0.0028	0.0006
(cluster: firm size)	(0.0033)	(0.0043)
(cluster: firm size *and* firm)	(0.0047)	(0.0045)
[R^2]	[0.0014]	[0.0014]
Threshold-Dummies	Yes	Yes
Polynomial order	1	2
Number of observations	208,667	

Notes: ***, ** denotes significance at the 1% , 5% level respectively.
Robust standard errors in parentheses.
Source: Own calculations, based on ASSD and FWO

Table 6: The Effect of the Employment Quota on the Number of Disabled Workers per Firm at Quota Threshold $T = 25$

	(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 and time fixed–effects	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 7: The Effect of the Employment Quota on the Percentage of Disabled Workers per Firm at Quota Threshold $T = 25$

	(1)	(2)	(3)	(4)	(5)
Treatment effect	0.1819	0.1372	0.1831	0.1525	0.1425
cluster: S	$(0.0301)^{***}$	$(0.0415)^{***}$	$(0.0328)^{***}$	$(0.0363)^{***}$	$(0.0357)^{***}$
cluster: S, firm	$(0.0556)^{***}$	$(0.0479)^{***}$	$(0.0566)^{***}$	$(0.0539)^{***}$	$(0.0369)^{***}$
$S \in 25 \pm s$	$s = 12$	$s = 12$	$s = 12$	$s = 9$	$s = 6$
Polynomial order in $S_i - 25$	1	2	1	1	1
Controls and time fixed–effects	No	No	Yes	No	No
mean (dep. var.)	1.1956	1.1956	1.1956	1.2342	1.2732
s.d. (dep. var.)	3.0902	3.0902	3.0902	3.0155	2.9936
N	442,788	442,788	442,788	290,232	180,957
R^2	0.0017	0.0017	0.0295	0.0017	0.0015
Adjusted R^2	0.0016	0.0016	0.0294	0.0017	0.0015

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 8: The Effect of the Employment Quota on the Number of Disabled Workers per Firm at Quota Threshold $T = 25$ – Short– and Medium–Run Effects

	(1)	(2)	(3)	(4)
Treatment effect	0.0492***	0.0161***	0.0085	0.0276***
	(0.0085)	(0.0056)	(0.0063)	(0.0065)
Treatment effect · # past treated months			0.0009**	
			(0.0004)	
Firm fixed–effects	No	Yes	Yes	Yes
Lag–24 difference (i.e. $Y_t - Y_{t-24}$)	No	No	No	Yes
mean (dep. var.)	0.0609	0.0609	0.0609	0.0033
s.d. (dep. var.)	0.3453	0.3453	0.3453	0.2716
Number of Obs.	5,176,476	5,176,476	5,176,476	781,068
R^2	0.1808	0.0106	0.0107	0.0115
Adjusted R^2	0.1808	0.0106	0.0107	0.0115

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.
Robust standard errors in parentheses.
Source: Own Calculations, based on ASSD and FWO

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Table 9: The Effect of the Employment Quota on the Number of Disabled Workers per Firm by Firm Median Daily Wage (Quartiles) at Quota Threshold $T = 25$

	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Treatment effect	0.0892***	0.0724***	0.0422***	0.0099
	(0.0209)	(0.0173)	(0.0104)	(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
N	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
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 and time fixed–effects.
Source: Own Calculations, based on ASSD and FWO

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Table 10: The Effect of the Employment Quota on the Number *or* Percentage of Disabled Workers per Firm at Pooled Quota Thresholds $T > 25$

	(1)	(2)	(3)	(4)
		Number of Disabled		Percentage of Disabled
Treatment effect	0.0521***	0.1118**	0.1000**	0.1218***
	(0.0078)	(0.0421)	(0.0359)	(0.0415)
Threshold	$T = 25$	$T > 25$	$T > 25$	$T > 25$
Controls and time fixed−effects	No	No	Yes	No
Threshold−dummies	No	Yes	Yes	Yes
mean (dep. var.)	0.2526	2.0822	2.0822	1.5985
s.d. (dep. var.)	0.6459	4.7513	4.7513	2.2240
Number of Obs.	442,788	208,667	208,667	208,667
R^2	0.0286	0.3533	0.4193	0.0143
Adjusted R^2	0.0286	0.3532	0.4191	0.0141

Notes: ***, **, * denotes significance at the 1%, 5%, and 10% level respectively.
Robust standard errors in parentheses.
All regressions with linear fit in $\tilde{S}_i$.
Source: Own Calculations, based on ASSD and FWO

Table 11: Decomposing the Treatment Effect by Employment Status at Date of Registering as Severely Disabled at Quota Threshold $T = 25$

		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 and time fixed–effects (baseline model).
Source: Own Calculations, based on ASSD and FWO

B   Figures

N = 442788

Source: Own calculations, based on ASSD and FWO
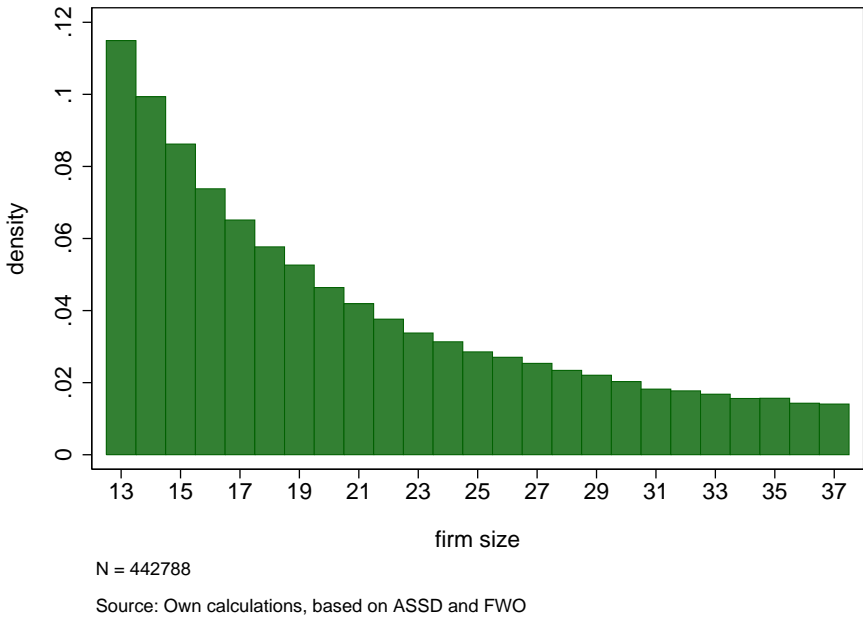
Figure 1: Firm Size Distribution at Quota Threshold $T = 25$

N = 208667

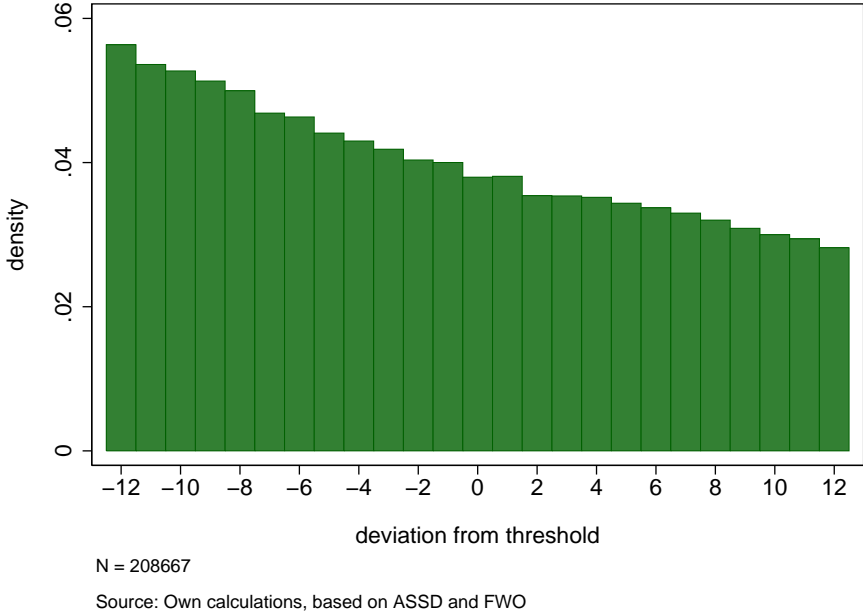Source: Own calculations, based on ASSD and FWO

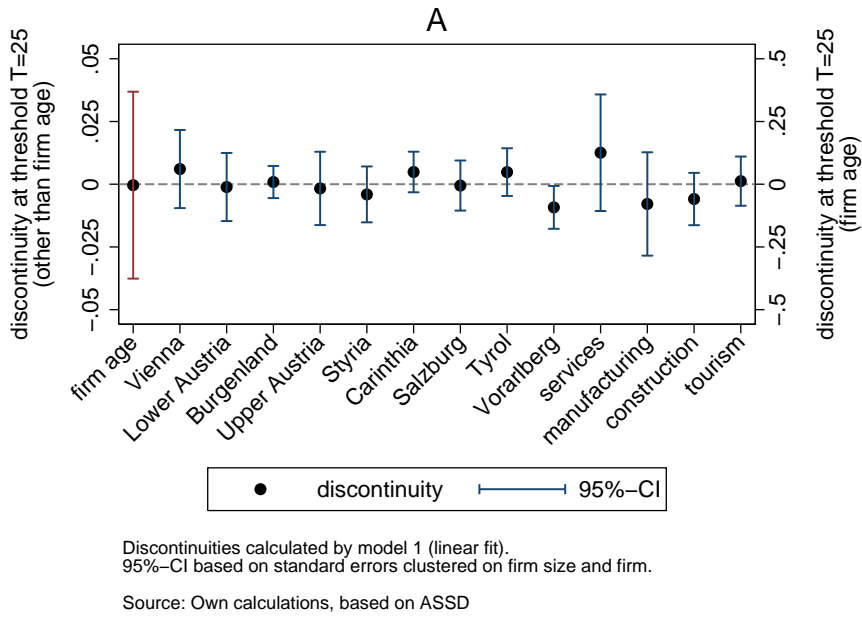Figure 2: Firm Size Distribution at pooled Quota Thresholds $T > 25$

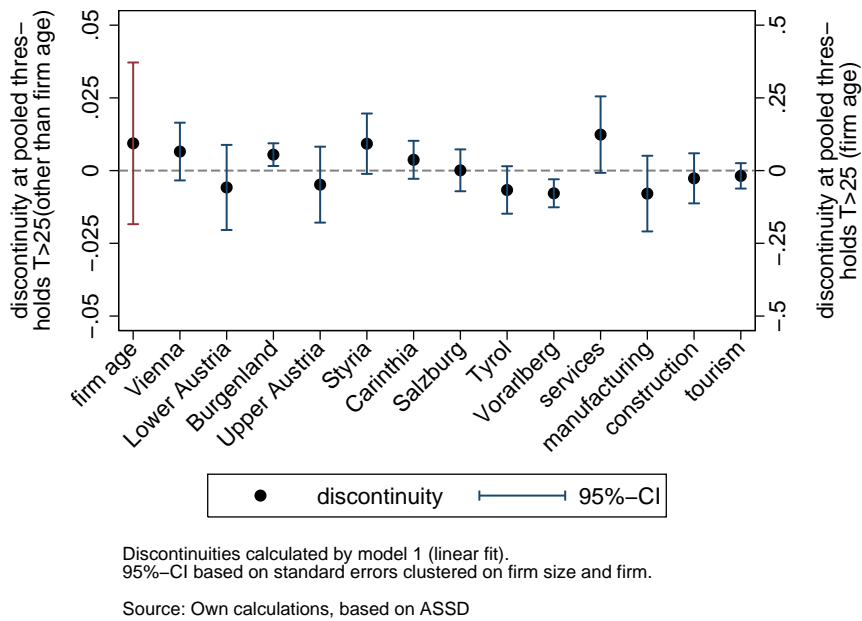Figure 3: Discontinuities of Pre-Determinded Covariates at Quota Threshold $T = 25$

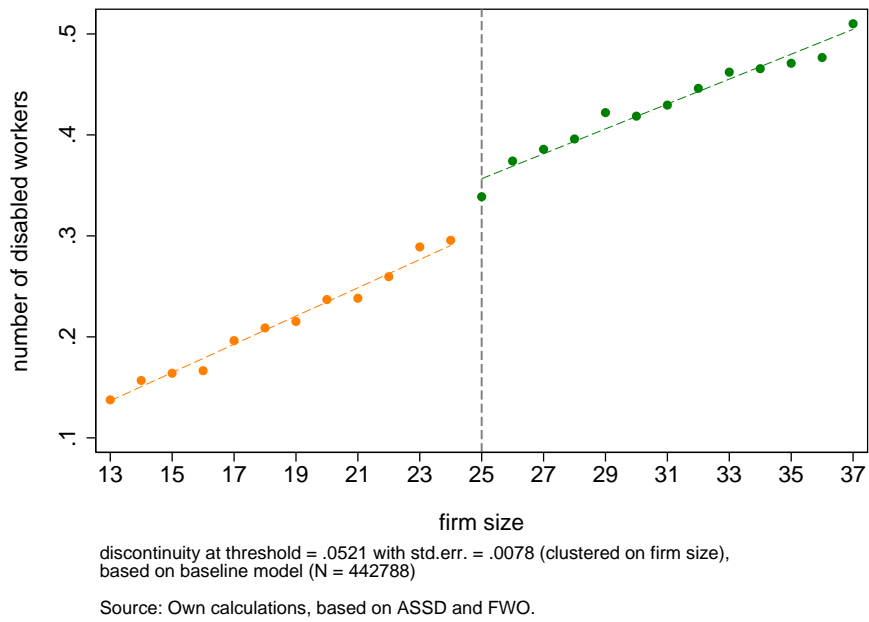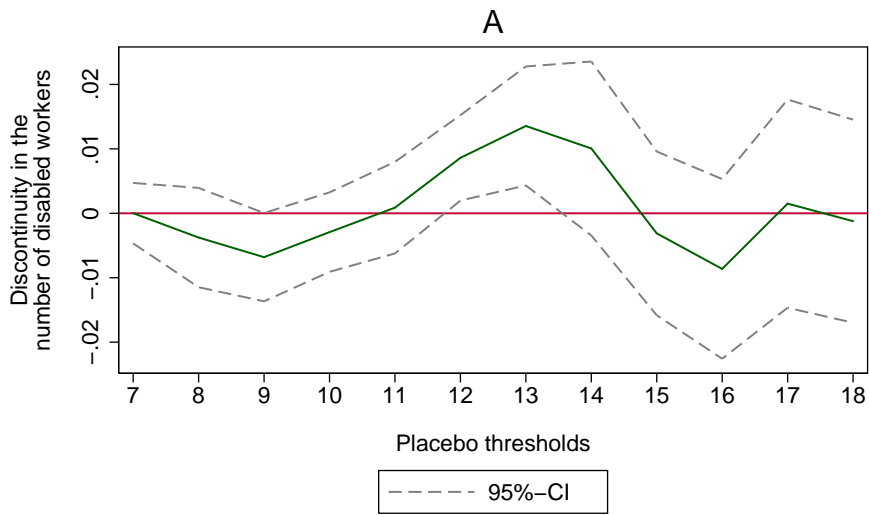Figure 4: Discontinuities of Pre-Determinded Covariates at Pooled Quota Thresholds $T > 25$

discontinuity at threshold = .0521 with std.err. = .0078 (clustered on firm size),
based on baseline model (N = 442788)

Source: Own calculations, based on ASSD and FWO.

Figure 5: The Effect of the DPEA on the Number of Disabled Workers at Quota Threshold $T = 25$
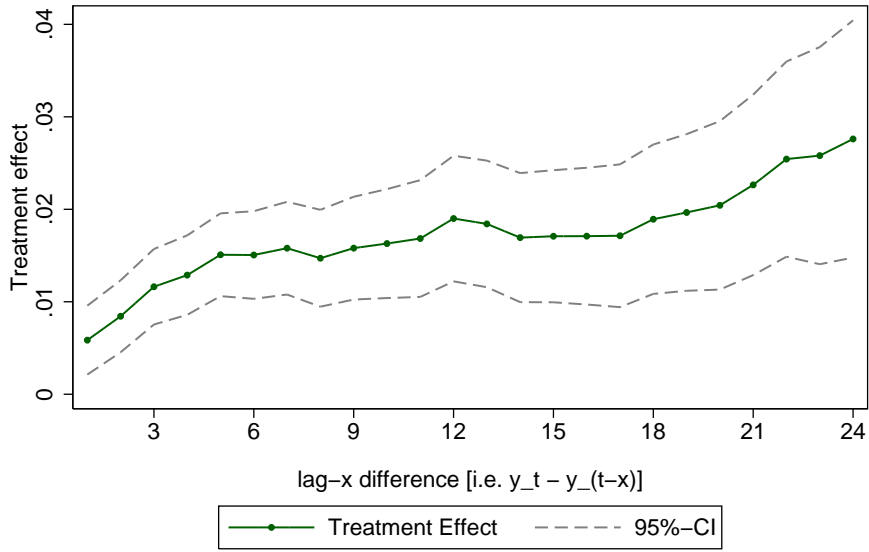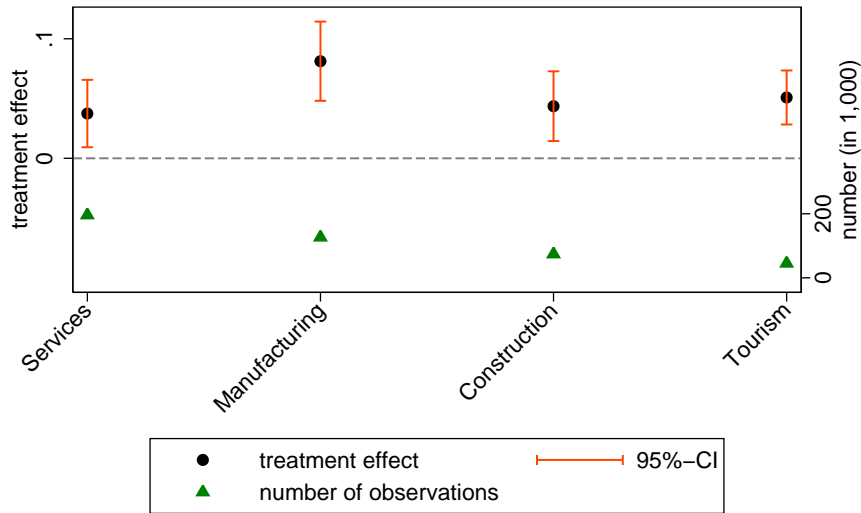
Figure 6: Discontinuities at Placebo Thresholds (A) and Density of Discontinuities at Placebo Thresholds (B)

95%–CI bases on robust standard errors.
Source: Own calculations, based on ASSD and FWO

Figure 7: The Effect of the DPEA on the Number of Disabled Workers at Quota Threshold $T = 25$ – Short–run to Medium–Run effects ($Y_t - Y_{t-\tau}$ for $\tau \in \{1, 2, ..., 24\}$)

Discontinuities calculated by model with linear fit.
95%–CI based on standard errors clustered on firm size.

Source: Own calculations, based on ASSD and FWO

Figure 8: Employment Effects by Industry at Quota Threshold $T = 25$