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Do Financial Incentives Affect Firms' Demand For Disabled Workers?

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Abstract

A number of OECD countries aim to encourage work integration of disabled persons using quota policies. For instance, Austrian firms must provide at least one job to a disabled worker per 25 non-disabled workers. Non-complying firms pay a tax for each job-month missed. We study the role of this employment quota on firms' demand for disabled workers. Specifically, we compare firms that employ 25 non-disabled workers and are subject to the non-compliance tax to firms that employ 24 non-disabled workers and are not subject to the tax. Our results indicate that firms with 25 non-disabled workers employ about 0.04 (or 12 percent) more disabled workers than would be expected from smaller firms, employment effects are stronger in low-wage firms than in in high-wage firms, and the quota generates excess disabled employment on the order of 0.07 among firms located at non-disabled firm size 50 and higher. Two reforms of the system also suggest that increasing the non-compliance tax increases excess disabled employment, whereas paying a bonus to over-complying firms slightly dampens the employment effects of the non-compliance tax. These results are only valid if we rule out strategic behavior in firms' choice of non-disabled employment that is related to the tax. We show in a simple behavioral framework that firms may indeed manipulate non-disabled employment and that this manipulation can lead to either upward or downward biases. Based on empirical estimates for manipulation we provide a lower and upper bound for the causal effect of the quota on firms' demand for disabled employment.

JEL classification: J15, J20, J71, J78

Keywords: disabled workers, employment quota, financial incentives

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1 Introduction

Integrating disabled workers is a key challenge of employment policy. One out 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 to increase the demand for disabled workers. Understanding the effects of 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 been explored much less. 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. 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.; see table 1 for an overview). While these regulations have a core component in the form of a mandatory employment quota in common, they differ in terms of the quota amount (ranging from 7% in Italy to 2% in Korea and Spain), in terms of the target firms, and in terms of the salience of non–compliance sanctions (ranging from 0.25% of the monthly pay-roll for firms in Germany to 4% in Italy).

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. Austrian firms have to hire at least one disabled individual per 25 non-disabled employees leading to quota thresholds at firm size 25, 50, etc. Firms that fail to comply are subject to a tax of currently 223 € per month. The tax revenues are used to subsidize firms that provide employment to disabled workers (regardless of whether they are subject to the employment quota or not). As similar quota rules are in place in many other OECD countries, it is important to investigate how the quota rule impacts employment.

In order to comprehensively capture the employment effects of the quota rule, it is crucial to understand how quota affect firms' employment decisions of both disabled and non-disabled workers. Our empirical strategy exploits the discontinuous change in the relative cost of employ-

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 fulfillment (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)

ing disabled and non-disabled workers and provides us with a "threshold design". We compare (i) the number of *disabled* workers in firms just below and just above the quota threshold; and (ii) the density of the firms size distribution – measured by *non-disabled* employment – just below and just above the threshold. The basic idea is that when firms' demand for disabled workers is affected by these sharp changes in financial incentives at the quota thresholds, we should see a discontinuity in disabled employment at the threshold. In this respect our threshold design is closely related to the regression discontinuity design (RDD).[1] However, the employment quota may also affect employment of non-disabled workers since firms may manipulate employment to avoid the non-compliance tax. This latter issue is key for the interpretation of our threshold discontinuity results for disabled workers.

We explicitly address the question how manipulating employment of non-disabled workers may bias the estimated quota effect on non-disabled employment (see McCrary, 2008). We develop a simple behavioral framework that shows that manipulation can lead either to an upward or a downward bias of the estimated threshold effect for disabled workers. The sign of the bias depends on whether or not the productivity of disabled workers exceeds their wage.

The model is helpful as it lets us estimate bounds for the estimated quota effect. We first check for employment manipulation. We show that, while the two populations of firms below and above thresholds are very similar in terms of a range of observable characteristics, there is, in fact, a significant (albeit small) discontinuity in the firm size distribution at the threshold. Based on our model we use the estimated of firm-size discontinuity effect to provide a lower and upper bound for the causal effect of the quota on firms' demand for disabled employment. We

[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), and Lalive (2008), for studies assessing the causal effects of unions, social assistance, or unemployment benefits on labor market outcomes.

conclude that, while manipulation bias cannot be ignored, even the lower bound of the quota effect remains positive.

The empirical analysis documents four 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 4 in 100 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 employment quota. The average effect at higher order thresholds (50, 75, ...) is roughly twice as large but imprecisely estimated. Both estimates suggest that firms are quite responsive to the tax (the elasticity of substitution is around 1.60 for firms employing 25 non-disabled workers and around 1.58 for firms hiring 100 non-disabled workers). These results allow us to assess the extent to which firms incur losses from hiring a disabled worker. We find that about 35 % of all threshold firms incur no or only small losses from hiring disabled workers – with 31 % being indifferent between hiring an extra disabled worker or an extra non-disabled worker and 4 % incurring losses that are smaller than the non-compliance tax. The remaining 65 % of all firms do not comply with the regulation because the losses from doing so exceed the tax. *Second*, we document important heterogeneity of the effects of employment quota with respect to wages. We find that firms' response to the per-head non-compliance tax decreases monotonically with a firm's position in the wage distribution. *Third*, 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 64 % of the employment effect can be attributed to workers already employed by the firm on the date of acquiring formal disability status. About 34 % of excess employment can be attributed to workers who were employed by other firms at the time of acquiring disability status. The remaining 2 % of excess employment goes to individuals who were not employed at the time of acquiring disability status. *Fourth*, two reforms of the system suggest that increasing the non-compliance tax increases excess disabled employment, whereas paying a bonus to over-complying firms slightly dampens the employment effects of the non-compliance tax.

The existing literature has extensively studied the effects of anti-discrimination legislation for disabled individuals. DeLeire (2000), Acemoglu and Angrist (2001), and Beegle and Stock (2003) find that the Americans with Disabilities Act has not improved employment of disabled individuals in the U.S.. Kruse and Schur (2003) challenge this finding, pointing to imprecise information on disability status. Jolls and Prescott (2004) and Jolls (2004) argue that the Act increased education participation. Bell and Heitmueller (2009) find that the U.K. Disability Discrimination Act did not have a significant impact on employment prospects for disabled individuals. Wagner *et al.* (2001) find that employment quota did not generate a quota effect

for disabled workers nor an impact on job dynamics in Germany.[2] Wuellrich (2010) finds that an increase in the non-compliance tax in Austria generated positive employment effects for disabled workers.[3]

This paper contributes to the literature in several ways. *First*, it adds to the literature by studying the impact of quota rules. 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 evaluate the effect of quota on employment of disabled workers. *Second,* we provide a simple theoretical framework that is helpful in interpreting the estimated quota threshold effect in the presence of employment manipulation ("bunching") by firms. *Third*, our evaluation is based on high-quality data from Austrian private firms and their (disabled and non-disabled) workers. We use the same data base that Austrian social welfare authorities use to determine compliance with employment quota: the Austrian Social Security Data (ASSD) linked with data from the Austrian Federal Welfare Office (FWO). The former data set contains the universe of Austrian firms and workers. The latter data set allows us to assess the formal disability status of each worker. The data cover all of 46,467 Austrian private sector firms with employment close to the quota threshold during 1996–2003(see Zweimüller *et al.*, 2009, for a description of the ASSD).

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

2 Institutional Background

The Austrian Disabled Persons Employment Act (DPEA), implemented in 1970, forms the legal basis of the Austrian employment quota system. It defines the process by which individuals acquire the formal status "severely disabled". It regulates the employment obligations for firms and the financial sanctions associated with non-compliance; specifies subsidies to firms employing disabled workers; and grants employment protection for disabled workers. Here we briefly discuss the legal background that was in effect during January 1999 to June 2001 and we also discuss two important reforms to this system that took place before and after this period.[4]

[2]See also Lechner and Vazquez-Alvarez (2009) and Verick (2004) who study the effects of German anti-discrimination legislation. Moreover, two strands of the literature study (i) the role of employment protection for worker effort (Ichino and Riphahn, 2005), and (ii) the role of general employment protection provisions on firm dynamics and firm size (Bertrand and Kramarz, 2002; Borgarello *et al.*, 2004). See also Welch (1976) for an early theoretical attempt to characterize the effects of quota on the labor market.

[3]See also Humer *et al.* (2007) for an overview of the Austrian system and a descriptive account of disabled workers' career patterns.

[4]The DPEA regulates employment of severely disabled workers. Another important program in Austria that targets disabled individuals is the disability insurance program (DI) which grants income transfers for disabled individuals who have dropped out of the labor force permanently (see e.g. Staubli, 2010, for an investigation of the impact of tightening the eligibility criteria for DI on labor force participation). Only 12 % of individuals who

Employment quota The quota rule obliges firms to hire one disabled worker per 25 non-disabled workers. Firms that do not comply with this obligation have to pay a non-compliance tax of currently € 223 (2010) and stood at 150 € in 1999, roughly 8 % of a worker's average monthly salary. Enforcement of non-compliance is close to 100 %. The FWO checks firms' employment obligation every month taking into account particular disabilities. (Blind individuals, disabled individuals below age 19, above age 50 with a degree of disability of at least 70 percent and above age 55 and individuals in a wheelchair are double-counted.) Disabled workers have to be hired on the same type of contracts offered to non-disabled workers. The non-compliance tax for disabled worker is the only labor market regulation that kicks in at a firm size of 25 non-disabled workers (and multiples thereof). This means that contrasting firms with 25 employees to firms with fewer than 25 workers really informs on the non-compliance tax rather than on other labor market regulations.[5]

Acquisition of disability status To acquire the formal status of a "severely disabled" individual a worker has to file an application with the FWO. The application is approved once a medical expert assesses a physical, mental, intellectual, or sensory disorder which reduces the individual's work capacity by at least 50 percent. This procedure aims to rule out that the formal status of a disabled individual can be obtained by fraud. While it cannot be ruled out that the procedure is only triggered after a firm has crossed the threshold, pure relabeling of existing workers is unlikely given the joint incentives of workers and firms. Workers gain in terms of increased job protection. Firms gain in getting access to refunds for workplace accommodation and wage subsidies (see below). In 2009, almost 95,000 individuals or 2.2 % of total employment were registered as disabled according to the law.

Workplace accommodation and (wage) subsidies The DPEA also defines how non-compliance tax revenues are to be spent. In 2009, these revenues amounted to € 88.2 millions. Beneficiaries are both the firms employing disabled workers and the disabled workers themselves.

Firms employing disabled workers can get allowances for: (i) workplace accommodation (up to € 25,000 and at most 50% of total costs involved, only firms with 50 or less non-disabled employees are eligible); (ii) wage subsidies (at most € 700 a month for new entrants and up to two years; at most € 650 or 50 percent of the wage for long-term employees with a major reduction in work capacity; at most € 400 a month for apprentices; at most € 1,000 or 50% of the wage for employees when the firm can credibly show that it needs to layoff the worker

are eligible for DPEA also receive a DI pension. Yet even though few individuals are covered by both programs, the mere existence of DI might affect the employment effects of DPEA via its effect on reservation wages. Notice, however, that DI in Austria is predominantly an early retirement program. (See Staubli, 2010, for an analysis of DI incentives on employment of older workers.) DI take-up among younger individuals is comparably low.

[5]There is a discontinuity in labor regulations at firm size of 15 employees. Firms above this employment threshold have to establish a works council.

without the subsidy); and (iii) work assistance (such as counseling the firm regarding the efficient integration of disabled workers, a service provided free of charge by the FWO). These allowances represent a reallocation of resources from firms that fail to comply with the quota rule to those firms that employ at least one disabled worker. The reallocation is used to compensate the latter for their effort in employing disabled workers. Allowances are available to all firms, including small firms not subject to the quota rule.[6]

Disabled workers are eligible to allowances for the following purposes: vocational (re)training, professional development, work assistance (counseling service), mobility enhancing measures (e.g. provision of a guide dog), and formation of a subsistence securing self-employment (up to € 60,000).

Employment protection 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. First, it stipulates that a contract may only be terminated after a notice period of at least four weeks. Second, 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 comes into effect only after a probationary period of three months has elapsed.

Policy Changes Two important reforms to the DPEA took place since the late 1990s. First, on July 1, 2001 the non-compliance tax was increased from € 150 to € 196. Second, on January 1, 1999 a bonus for over-compliance was abolished. Before 1999, firms employing more disabled workers than required by the quota rule were granted a bonus of € 150 for each excess disabled worker. The 1999 reform and the 2001 reform also changed the probation period for disabled workers, from 1 to 3 months in the first reform and from 3 to 6 month in the second reform. Our empirical analysis (see section 5) will focus on the period January 1999 to June 2001. However, we will provide additional results also for the preceding and subsequent periods to check the robustness of our results.

3 A Simple Behavioral Framework

This section studies a simple model in which a quota threshold affects a firm's decision to employ disabled and non-disabled workers. We want to shed light on the issue of bunching and look at firms *around the threshold*. Firms have to solve two discrete choice problems: (i) whether to

[6] Dyk *et al.* (2002) investigate take-up of allowances by firms. Almost 40% of firms employing disabled workers receive an allowance with wage subsidies being the most important allowance. 86% of firms with an allowance obtain a wage subsidy and about 6% obtain an allowance for providing adequate access to the premises and workplace accommodation. 60% of all firms assess the existence of allowances as essential for hiring disabled workers.

hire non-disabled workers below or at (or above) the threshold; and (ii) whether or not to hire a disabled worker.[7] We assume that non-disabled workers have productivity P and disabled workers have productivity $p < P$. Anti-discrimination legislation ensures the same wage w for both types of workers.[8] We assume $P > w$ for all firms but allow for $w < p$ in some firms and $w > p$ in others (i.e. firms differ in p). The quota threshold is defined on a *discrete* number of employees, so it is reasonable to assume that labor is indivisible. However, product demand Z is *continuous* (so firms either hire too few workers and ration Z; or they hire more effective labor units than needed to produce Z).

No quota rule. We first look at employment decisions in the absence of a quota rule. A firm that hires L^N non-disabled and L^D disabled workers makes profit

$$\pi_0(L^N, L^D) = \min(PL^N + pL^D, Z) - w(L^N + L^D).$$

Consider first optimal employment of non-disabled workers of the firm with product demand Z (assuming firms do not hire disabled workers). Define "residual demand" as product demand Z minus output produced by L^N non-disabled workers, so $R(Z, L^N) = Z - L^N P$. A firm with $0 < R(Z, L^N) < P$ will hire at least L^N non-disabled workers and at most $L^N + 1$ non-disabled workers. Optimal non-disabled employment is characterized by a threshold rule: hire $L^N + 1$ non-disabled workers if residual demand exceeds the wage rate $R(Z, L^N) > w$.

Now consider optimal hiring of a disabled worker. When $p < w$ productivity falls short of the wage rate and the firm is not willing to hire a disabled worker. However, when $p > w$ some firms are willing to hire. When residual demand is in the range $R(Z, L^N) \in (w, p)$ the firm is indifferent between hiring a disabled or a non-disabled worker. Both types of workers are sufficiently productive to satisfy residual demand $R(Z, L^N)$. No disabled worker will be hired when residual demand is outside the range $R(Z, L^N) \in (w, p)$. A firm with low residual demand, $0 < R(Z, L^N) < w$ would make a loss. A firm with high residual demand, $p < R(Z, L^N) < P$ would sacrifice profit since hiring a disabled worker generates less additional revenue than hiring a non-disabled worker.

Employment quota with non-compliance taxes. Now assume a system in which firms with non-disabled employment $L^N \geq T$ need to hire one disabled worker whereas firms with $L^N < T$ do not face such an obligation. Non-compliance with the quota rule leads to a tax

[7] The discussion focuses on the first threshold, i.e whether to hire 25 workers (or more) or 24 workers (or less). However, the general logic generalizes to higher thresholds. For instance, at the second thresholds the firm has to decide whether or not to hire 50 workers or more or 49 workers or less and whether to hire 0,1, or 2 disabled workers, etc.

[8] Note that the assumption of fixed wages does not preclude substitution between disabled and non-disabled workers since the non-compliance tax acts like an increase in the cost of non-disabled workers compared to disabled workers.

τ. Throughout we assume $\tau < w$ and $\tau < P - p$.[9] A firm that hires L^N non-disabled and $L^D \in \{0, 1\}$ disabled workers makes profit

$$\pi_1(L^N, L^D) = \min\left[PL^N + pL^D, Z\right] - w(L^N + L^D) + \min\left[L^D - 1(L^N \geq T), 0\right] \cdot \tau$$

where $1(L^N \geq T)$ is an indicator taking value 1 if $L^N \geq T$ and zero otherwise.

How does the quota rule affect employment decisions? It turns out that the answer depends crucially on whether a disabled worker's productivity p is larger or smaller that the wage w. We discuss the two cases in turn.

Case 1: $p < w$. Such a firm is not willing to hire a disabled worker even under a quota system. However, the quota system will affect employment of non-disabled workers. Consider the decision of a firm whether to hire T or $T-1$ workers of firms with residual demand $0 < R(Z, T-1) < P$. In the absence of quota this firm will hire T workers when $R(Z, T-1) > w$. With employment quota a firm with residual demand $R(Z, T-1) \in \{w, w+\tau\}$ employs only $T-1$ non-disabled workers. This "bunching" occurs because the marginal cost of the Tth non-disabled workers equals the wage w *plus* the cost of crossing the quota threshold τ; and because residual demand $R(Z, T-1) \in \{w, w+\tau\}$ is lower than this marginal cost. The firm is better off setting employment at $T-1$ and avoid the tax. Notice also that the quota system affects only firms that would hire exactly T workers (or multiples of T) while the employment decisions of all other firms remain unaffected (though profits of firms with $L^N \geq T$ are lower because of the tax).

Case 2: $p > w$**.** When disabled workers' productivity exceeds the wage, $p > w$, it depends on residual demand whether or not the firm is willing to hire a disabled worker. Consider again the decision of a firm whether to hire T or $T-1$ non-disabled workers. Firms with low residual demand $0 < R(Z, T-1) < w$ are not affected by the quota rule. Hiring an additional (disabled or non-disabled) worker does not generate the necessary revenue. Firms with residual demand in the range $w < R(Z, T-1) < p$, strictly prefer to hire one disabled worker and $T-1$ non-disabled workers as hiring T non-disabled worker and no disabled worker would make them subject to the tax. (Without a quota system, firms in this range are indifferent whether the Tth worker is disabled or non-disabled.) Firms with residual demand $p < R(Z) < p + \tau$ are also affected by the quota system. They hire one disabled and $T-1$ non-disabled workers (and would have hired no disabled and T non-disabled workers without a quota). Finally, firms with residual demand $p + \tau < R(Z, T-1) < P$ always hire T non-disabled workers. While a quota system forces them

[9] Both assumptions are well in line with the Austrian system where the tax is on the order of 8 % of monthly earnings. Moreover, productivity of a disabled worker is likely to be substantially lower than productivity of a non-disabled worker since disabled individuals have lost at least 50 % of work capacity.

to pay the tax a non-disabled workers generates higher profits.

Finally, let us look at the decision of firms whether to hire T or $T+1$ workers.[10] Firms with low residual demand $0 < R(Z,T) < w - \tau$ are unaffected by the quota and hire T non-disabled and no disabled worker. Firms with residual demand $w - \tau < R(Z,T) < w$ hire T non-disabled workers but are now incentivized to hire one disabled worker. As the quota rule applies, the marginal cost of hiring a disabled worker is $w - \tau$, smaller than residual demand. (Notice that this incentive does not exist when choosing between $T-1$ and T). For residual demand $R(Z,T) > w$ the choice between T and $T+1$ is similar to the one between $T-1$ and T.

Figures 1a and 1b summarize how firms adjust employment of disabled and non-disabled workers in the presence of the quota rule. When $p < w$ (see Figure 1a) firms do no hire a disabled worker despite the quota. When $p > w$ (see Figure 1b) and residual demand is in range E firms will not hire a disabled worker in the absence of a quota rule. In the presence of quota, however, they will hire one disabled worker, provided their optimal employment of non-disabled workers is at or above the threshold ($L^N \geq T$). Similarly, firms with residual demand in range G do not have an incentive to hire a disabled worker without a quota rule, but will hire one disabled worker with a quota – provided their optimal employment of non-disabled workers is just below the threshold or larger ($L^N \geq T-1$). Finally, firms with residual demand in range F and optimal non-disabled employment $L^N \geq T-1$ strictly prefer hiring a disabled worker under a quota system while they are indifferent between a disabled and a non-disabled worker in the absence of a quota system.

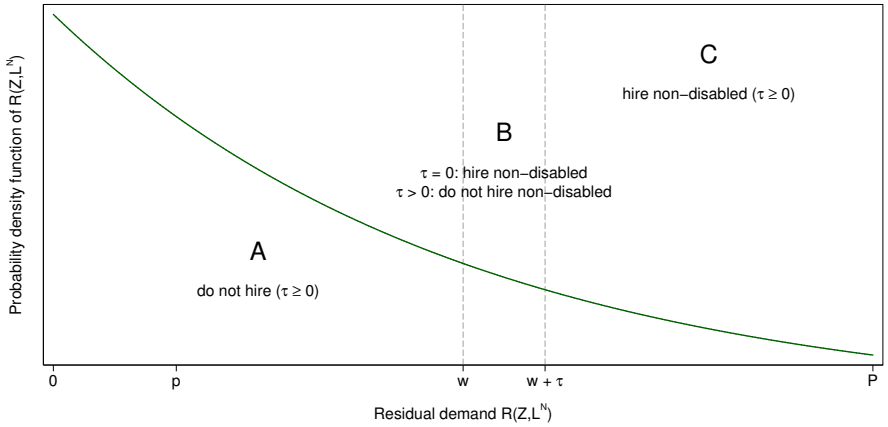
The figures also show which firms will manipulate employment of non-disabled workers in reaction to the quota rule. *Manipulators* – firms that locate below the threshold under a quota system but above in the absence of such a system – include firms with residual demand in ranges B and G. Firms in range B set non-disabled employment below the threshold to avoid the tax and do not hire a disabled worker. Firms in range G also set non-disabled employment below the threshold but they are better off employing a disabled worker as this worker adds to profits. All other firms do not manipulate.

Consequences of employment manipulation. We have seen that the presence of a quota rule induces some employers to choose a firm size just below the quota threshold. The key question is whether and to what extent such manipulation biases the contrast of mean employment of disabled workers between threshold firms and firms just below the threshold.

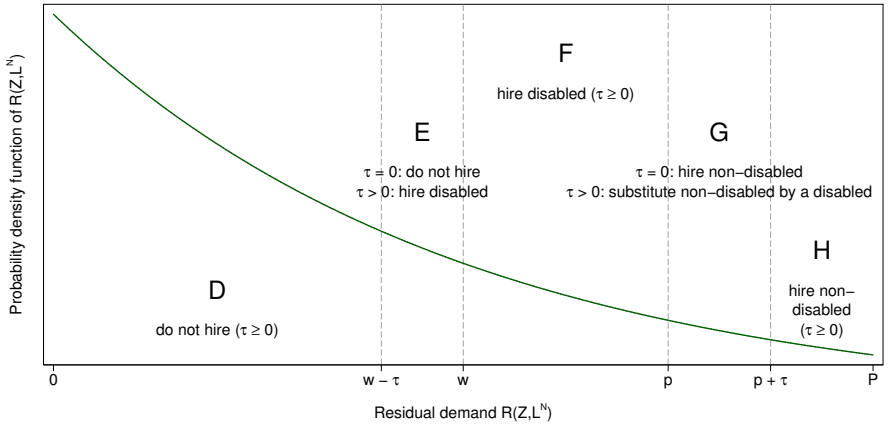
Consider firms hiring at the threshold with observed firm size $L^N = T$ under a quota system

[10]The same logic that applies the decision to employ T or $T+1$ non-disabled workers, applies for the decision to employ $T+1$ or $T+2$ non-disabled workers, $T+2$ or $T+3$ non-disabled workers, and so on. This is because all involved employment level are subject to the same quota rule. Notice further that the choice of firms to hire $T-1$ versus $T-2$ non-disabled workers is identical to the in the absence of the quote system, see the discussion above.

Figure 1: Employment choices derived from the model



(a) Case 1: $p < w$



(b) Case 2: $p > w$

in place. Among those firms are manipulators (with residual demand in ranges B and G) and non-manipulators (all other firms). It is important to note that manipulation due to a quota system leaves composition of firms at $L^N = T$ with respect to employment of disabled workers essentially unchanged. While there are firms that would have optimally chosen T in the absence of a quota system, now chose $T - 1$ as a result of the quota system, there are also firms that would have optimally chosen $T + 1$ in the absence of quota system now chose T in the presence of the system. This means that the composition of threshold remains essentially unchanged. A substantial bias in the average number of disabled workers hired by threshold firms can be ruled out.

Now consider firms hiring just below the threshold with observed firm size $L^N = T - 1$. These firms serve to identify the counterfactual disabled employment decisions without a quota rule. This set of firms also consists of manipulators and non-manipulators. The composition of firms just below the threshold changes as a result of the quota rule. Firms in range B and in range G are induced by the quota to bunch at $T - 1$ but would have chosen T in the absence of

quota. How does bunching affect average employment of disabled workers for firms observed at $L^N = T - 1$? Notice that B-firms are not willing to hire disabled workers, whereas G-firms are actually hiring disabled workers. Hence two types of bunching create biases in opposite directions that tend to offset each other. A bias arises when the number of bunching firms is very large *and* when the percentage G-firms in all bunching firms (B- plus G-firms) deviates substantially from the average number of disabled workers below the threshold in the absence of quota. If the total number of bunching firms is small – or if the $B/(B+D)$-ratio is similar to the percentage of $(T-1)$-firms hiring disabled workers in the absence of a quota, the contrast of firms observed at the threshold to firms just below the threshold will not be strongly biased and will be informative on the impact of the quota rule on employment of disabled workers.

In our empirical analysis below we will estimate the fraction of bunching firms. Using this estimate, we will be able to bound the estimated threshold-effect for disabled workers assuming that bunching arises either entirely from B-firms (which would lead to an upward bias) or entirely from G-firms (which would lead to a downward bias).

4 Empirical Strategy

The first part of this section provides the essential background regarding the data for the empirical analysis. The second part of this section discusses identification and estimation of the causal effect of the non-compliance tax on disabled employment.

4.1 Data

To assess the impact of the employment quota on 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 1972–2003 on a daily basis together with an unambiguous firm identifier, as well as firms' industry affiliation and location (see Zweimüller *et al.*, 2009) and (ii) personal data from the Austrian Federal Welfare Office (FWO) from 1970–2003, which reports disability status, disability type, and disability degree for all individuals who are disabled in the context of the DPEA. One advantage of this type of information is that a medical procedure (rather than self-reported by firms or workers) objectively assesses the disability status. The ASSD and FWO data can be linked on the basis of an anonymized person identifier. This allows us to calculate accurately (i) the number of the non-disabled workers and (ii) the number of disabled workers employed by each firm. The former variables determines whether a firm is subject to the quota rule and the latter variable shows whether the firm complies. 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.[11]

Our main empirical analysis focuses on the period January 1999 to June 2001. The FWO data set includes all individuals who acquired the disability status in 1970 or later. This means that the stock of disabled workers might be incompletely captured in the early years but does not cause a major problem here as our analysis focuses on the above period. To check the robustness of our results we use also data for the periods July 1996 to December 1998 and July 2001 to December 2003 when slightly different rules were in place (see section 2). We further restrict the analysis to firms in the private sector – those who are likely to pursue a profit-maximizing objective. In particular, we look at firms operating in the services, manufacturing, construction, and tourism industries.

Table 2 characterizes disabled individuals in terms of the DPEA (column 1) and contrasts them to their disabled counterparts in terms of the disability insurance program (DI) (column 2) and to non-disabled individuals (column 3). The sample consists of the universe of working-age (age 20–65) individuals in Austria (August 2000). Table 2 reveals that the mean degree of disability (i.e. the reduction in work capacity) for DPEA-disabled amounts to 61.50 % and that the average duration of the disability is 7.93 years. DPEA-disabled are somewhat more female than the DI-disabled but have roughly the same years of schooling. DPEA-disabled are substantially younger than DI-disabled (44.33 vs 57.22 years). The main reason is that DI is mainly an early retirement program, whereas DPEA tries to integrate severely disabled into the labor market. Among those who work DPEA-disabled earn substantially more than DI-disabled (€ 66.89 vs € 45.45) and are almost twice as likely white collar.[12]

4.2 Identification

Our empirical strategy is based on the discontinuously changes in financial incentives for firms to employ disabled workers. The DPEA requires firms to hire a disabled worker if employment of non-disabled workers L_i^N is greater than or equal to the quota threshold $T \in \{25, 50, 75, ...\}$. Our aim is to identify the causal effect of the non-compliance quota tax on threshold firms' disabled employment. In order to understand whether the quota rule has an impact, it is crucial to understand how firms' employment decisions of both disabled *and* non-disabled workers are affected.

The quota rules provide us with a "threshold design".[13] Our threshold design contrasts em-

[11] Firms cannot simply hire disabled workers for one day in order to fulfill the quota. In fact, disabled workers have, on average, a substantially higher tenure than non-disabled workers. Mean (std. dev.) tenure amounts to 10.3 (8.7) years for disabled and to 6.1 (6.9) years for non-disabled workers, the difference being statistically highly significant.

[12] Public sector workers and the self-employed are excluded since we do not observe their wages nor the color of their collar.

[13] While our threshold design is closely related to a regression discontinuity design (RDD), our set-up differs

Table 2: Characterization of disabled (DPEA), disabled (DI), and non-disabled workers

	Disabled (DPEA)	Disabled (DI)	Non-disabled
Disability			
Degree of disability	61.50	n/a	–
Years registered as disabled	7.93	n/a	–
Personal characteristics			
Female	35.92	28.59	47.82
Schooling (in years)	11.43	11.91	12.42
Age (in years)	44.33	57.22	40.08
age < 35	19.66	1.62	37.70
$35 \leq$ age < 50	45.44	10.50	39.67
$50 \leq$ age < 55	20.93	11.44	9.28
age ≥ 55	13.97	76.44	13.34
Labor market status (in percent)			
Employment	71.52	3.51	82.80
Unemployed	9.16	0.26	4.33
Retirement (disability insurance)	12.03	96.22	–
Retirement (old-age)	1.55	–	7.32
Sick	3.19	–	0.66
Out-of-labor-force	2.55	–	4.90
Number of observations	74,843	232,824	3,695,244
Job-related characteristics (excluding public sector workers and self-employed)			
Daily wage (in Euro)	66.89	42.45	65.45
White-collar	52.40	28.20	54.32
Number of observations	45,187	1,493	2,468,399

Notes: The sample consists of the universe of working-age (age 20–65) individuals in Austria (August 2000). Source: Own Calculations, based on ASSD

ployment of disabled workers just below and just above quota thresholds. The identification strategy builds on the key behavioral assumption that both disabled labor demand and supply are continuous in non-disabled employment at the threshold. Labor supply of disabled workers is clearly continuous in firm size because none of the DPEA provisions – except the quota rule – is conditional on firm size. Moreover, we assume that labor demand would be continuous in the absence of a quota rule. Since there are no rules – other than the disability quota – that kick in when firms change employment around thresholds, this is a reasonable assumption. However, the behavioral framework we develop in section 3 suggests that, in the presence of disability quota, firms may indeed manipulate employment of non-disabled workers and that manipulation can lead to either a downward or upward bias in the estimated threshold effect. Our empirical analysis will therefore report estimates of both the "threshold effect" (effect of the quota rule on disabled employment) and the "bunching effect" (effect of the quota rule on the density of non-disabled employment across firms). The bunching effect indicates the maximum number of firms that manipulate their non-disabled employment. Using this estimate, we will

from a RDD since the forcing variable is endogenous in our framework.

be able to bound the threshold effect.

4.3 Estimation

The basic econometric model for the identification of the threshold effect at threshold $T = 25$ is as follows:

$$L_{it}^D = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{L_{it}^N} + \beta_1 \cdot D_{it} \cdot \tilde{L_{it}^N} + \epsilon_{it},$$

where L_{it}^D denotes the number of disabled workers, $D_{it} = 1(L_{it}^N \geq T)$ indicates whether a firm is treated or not, and $\tilde{L_{it}^N} = L_{it}^N - T \in [-12, 12]$ denotes the difference between current non-disabled employment L_{it}^N and threshold $T = 25$ of firm i at date t. Including $\tilde{L_{it}^N}$ is important since non-disabled employment will turn out to be strongly correlated with disabled employment. 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. The parameter α_0 measures the average number of disabled workers for firms just below the threshold T and parameters β_0 and β_1 capture the correlation between firm size L_{it}^N and the average number of disabled workers per firm. We will also use a 'long' version of our basic model that includes a vector of covariates measuring (i) firm size dynamics, (ii) characteristics of firms' workforce, (iii) firms' industry affiliation, (iv) firms' geographical location (at the state–level), and (v) time fixed effects (see table 3 for a detailed list of covariates). In addition, we interact (i)–(v) with $\tilde{L_{it}^N}$ in order to allow for different effects of the forcing variable $\tilde{L_{it}^N}$ for different types of firms. Inclusion of these covariates should not affect the estimated discontinuity, if manipulation of L_{it}^N is small.

The discrete support of the assignment variable $\tilde{L_{it}^N}$ implies that we need to extrapolate in order to predict the counterfactual for threshold firms, i.e. we need to extrapolate the number of disabled workers threshold firms employ in the absence of the non-compliance tax.[14] The above econometric model assumes a linear functional form which could be mis-specified. The empirical analysis below will follow Lee and Lemieux (2010) who suggests two approaches to assess sensitivity to functional form: adding higher order polynomials to the basic model and local linear regression (i.e. keep the linear functional form but reduce the bandwidth).

For the investigation of the higher thresholds $T > 25$, we pool all higher order thresholds and extend the basic model with a set of threshold dummies G_{it} that indicate the threshold that is closest to firm i at date t to control for differences in non-disabled employment across normalized thresholds (note that, for pooled quota thresholds $T > 25$, $\tilde{L^N}_{it}$ denotes the difference between

[14] Discrete support of the assignment variable also affects the variance-covariance matrix estimates. Lee and Card (2008) suggest using cluster-consistent standard errors (clustered on the distinct values of L_{it}^N) 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 L_{it}^N but also on firms (note that this is non-nested). Miller *et al.* (2009) 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 L_{it}^N and (ii) those that are clustered on L_{it}^N *and* firms.

current non-disabled employment S_{it} and threshold T_{it} that is closest to firm i at date t.).[15] In addition, to allow for effect heterogeneity across thresholds, we include interactions between the threshold dummies G_{it} and (i) the treatment indicator D_{it}, (ii) the normalized firm size $\tilde{L_{it}^N}$, and (iii) the interaction between D_{it} and $\tilde{L_{it}^N}$. Thus, the treatment effect α_1 in this model can be interpreted as an inverse variance weighted average of the threshold specific treatment effects (see Angrist, 1998).

4.4 Manipulation Checks

In this subsection we consider whether the quota rule induces firms to manipulate employment of non-disabled workers. Following Lee and Lemieux (2010) we undertake two manipulation checks at the quota threshold: (i) inspection of the firm size density and (ii) inspection of baseline covariates. The intuition behind the former test is that manipulation should be reflected in a discontinuity in the firm size distribution at the threshold. Our behavioral framework predicts two types of manipulation both of which lead to "bunching" below the threshold. Thus we expect a negative discontinuity in the firm size density at the threshold and report the "bunching effect" along with the "threshold effect" when presenting our main results in section 5.

The intuition behind inspection of covariates is that manipulation should lead to local unbalancedness of baseline covariates around the threshold. Table 3 reports key background statistics on firms located around the threshold $T = 25$. The first line provides information on firm size – the number of jobs provided to non-disabled workers – for firms above (= treated firms) and below (= control firms) the threshold $T = 25$. Table 3 indicates that treated firms differ from control firms. Treated firms are, by construction, larger than control firms. Table 3 also displays information on firm size dynamics. The firm characteristic "employment stability" indicates whether the work force in month t was subject to any changes since month $t-1$. The characteristic "expanded (contracted) since 6 months" measures whether firm size in month t is strictly larger (smaller) than firm size in month $t-6$. It turns out that the workforce in treated firms is less stable than in control firms; both the fraction of firms that experienced employment growth and the fraction of firms that experienced a fall in employment are significantly larger among treated firms. Table 3 also reveals that there are slight differences in pay, tenure, and the percentage women, the number of employed apprentices, workers' age and the age of the firm. There are only negligible differences in industry composition between treated and control firms.

To shed more light on how treated and control firms differ, Figure 2 plots mean employment stability (a), mean wage (b), and mean firm age (c) as a function of firm size. Figure 2 clearly indicates that employment stability is a strongly decreasing monotone function of firm size. Vi-

[15] Let $G_{it} = floor((L_{it}^N + 12)/25)$ indicate a firm's threshold group, i.e. $G_{it} = 1$ for firms located around the threshold at firm size 50, $G_{it} = 2$ for firms located around the threshold at firm size 75, etc.

Table 3: Descriptive statistics around quota threshold $T = 25$ (time period: January 1999 - June 2001)

	below threshold mean	above threshold mean	difference	discontinuity[b,c]
Firm size	17.1139	30.1447	13.0308***	n/a
Firm size dynamics				
employment stability[a]	0.4092	0.2555	−0.1537***	0.0121
expanded since 6 months[a]	0.4473	0.4696	0.0223***	−0.0150
contracted since 6 months[a]	0.3408	0.3881	0.0473***	0.0038
Characteristics of firms' workforce				
log. of median daily wage[a] (in €)	4.0551	4.1028	0.0477***	0.0032
tenure[a] (in years)	5.3589	5.6305	0.2716***	0.0255
fraction women[a]	0.4083	0.3743	−0.0341***	0.0110
fraction white-collar[a]	0.4496	0.4458	−0.0038	0.0151
number of apprentices[a]	1.3625	2.0429	0.6804***	−0.0494
workers' age[a]	35.4687	35.7083	0.2396***	0.0376
age of firm (in years)	16.0121	17.0554	1.0433***	0.0793
Industry				
services	0.4556	0.4486	−0.0070	0.0107
manufacturing	0.2764	0.2935	0.0171**	−0.0091
construction	0.1676	0.1718	0.0043	−0.0045
tourism	0.1004	0.0861	−0.0143***	0.0030
Number of firm–month observations	330,427	117,729	448,156	448,156
Number of firms	22,311	9,058		
Total number of firms	25,755			

Notes: [a] denotes that variable bases on characteristics of non-disabled workers only. [b] The estimated discontinuity is based on the following model: $x_{it} = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{L^N}_{it} + \beta_1 \cdot D_{it} \cdot \tilde{L^N}_{it} + \epsilon_{it}$, where the coefficient α_1 detects discontinuities in the mean of characteristics x_{it}. [c] The p-values for the estimated discontinuities are adjusted for multiple testing according to Holm (1979). ***, **, * denotes significance at the 1%, 5%, and 10% level respectively (standard errors are adjusted for clustering on firm size). Source: Own calculations, based on ASSD and FWO

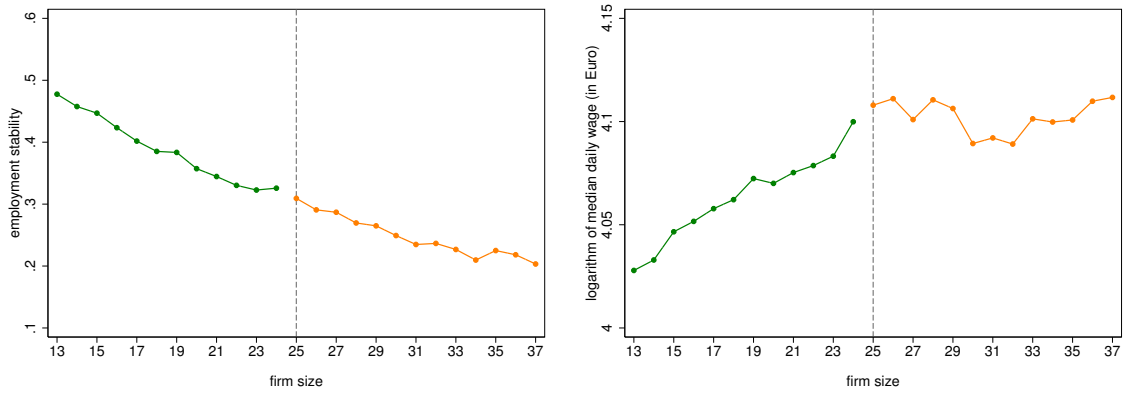
sual inspection suggests that there is no discontinuity in employment stability at the threshold. Similar patterns are observed for wage and firm age. We formally test for discontinuities in baseline covariates implementing our basic econometric model for each of the background characteristics. Column (4) of Table 1 presents the estimate of parameter α_1. Strikingly, results indicate that firms on either side of the $T = 25$ threshold are perfectly balanced with respect to observed covariates.

This means that all of the differences in baseline covariates shown in column (3) of table 3 are not due to purposeful self-selection of firms but rather due to underlying differences in firm size. Thus, the inspection of baseline covariates does not indicate any manipulation of the firm size suggesting that the key identifying assumption of our threshold design holds.

5 Econometric Results

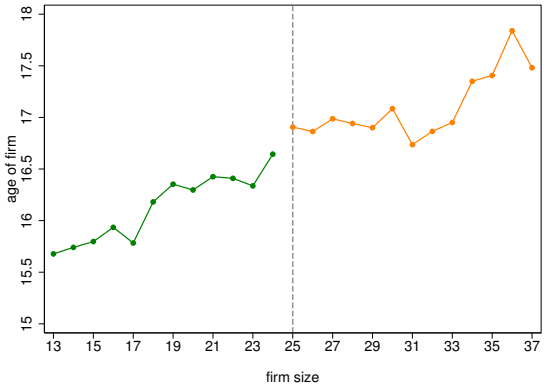
In this section we discuss our main results. We start by analyzing the threshold and the bunching effect at the first threshold ($T = 25$). We then check robustness on the basis of placebo

Figure 2: Selected controls (mean) vs firm size



(a) employment stability

(b) median wage



(c) firm age

Notes: Figure plots the mean of selected controls vs firm size. We selected three controls with means that vary strongly with firm size. The means of these selected controls but also of those we do not report are continuous in firm size at the threshold. The same result holds for pooled thresholds. Source: Own Calculations, based on ASSD and FWO

regressions; look for effect heterogeneity; look for the respective effects at higher thresholds ($T = 50, 75, ...$); and provide robustness checks on the basis of earlier and later sample periods.

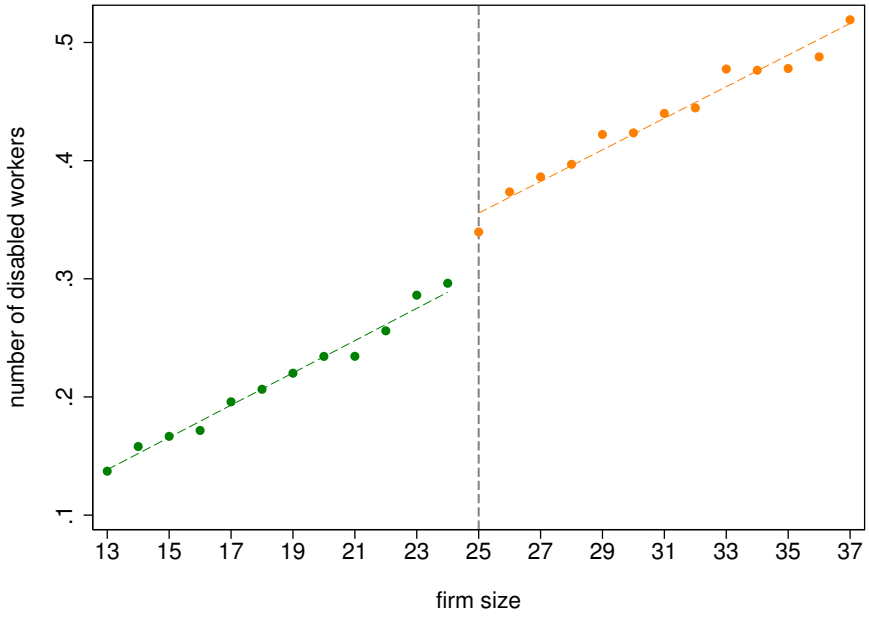
5.1 Results for Quota Threshold $T = 25$

This section presents the econometric estimates of the effects of quota rule for the first threshold $T = 25$.[16] Figure 3 reports the number of disabled workers per firm by firm size for sizes ranging from 13 to 37. The evidence is based on 448,156 firm-month observations, providing information on the employment decisions of 25,755 firms. The average number of disabled workers employed by firms increases with firm size. Firms that employ 13 non-disabled workers offer on average 0.14 workplaces to disabled workers whereas firms that employ 37 non-disabled workers provide

[16]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 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 subject to the quota.

on average 0.50 jobs to disabled workers. Figure 3 also suggests an approximately linear relationship. Strikingly, there is a jump in employment of disabled workers at the quota threshold. While firms with 24 non-disabled workers employ on average 0.31 disabled workers, firms with 25 non-disabled workers offer on average 0.34 jobs to disabled workers.

Figure 3: The effect of the DPEA on the number of disabled workers at quota threshold $T = 25$



Notes: Discontinuity at threshold = 0.0533 with (i) standard error = 0.0075 (adjusted for clustering on firm size) and (ii) standard error = 0.0133 (adjusted for clustering on firm size and firm), based on the baseline model (number of observations = 448,156). Source: Own calculations, based on ASSD and FWO.

Table 4 presents the econometric results. Panel A shows our main results for the threshold effect, i.e. how employment quota affects jobs provided to disabled workers. Column (1) of table 4 shows results for the basic econometric model. The estimated discontinuity at the quota threshold $T = 25$ is 0.0533 (the standard error adjusted for clustering on firm size is 0.0075; the standard error adjusted for clustering on firm size and firm is 0.0133; see footnote 14 for the justification of the use of different standard errors). This discontinuity is statistically significant at the 1%-level. Note 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 four columns.

Column (2) of Panel A shows results for a model extended by the full set of controls, time fixed-effects, and their respective interaction with firm size. The threshold effect only slightly changes from 0.0533 to 0.0545. This is not surprising given the local balancedness of these covariates around threshold $T = 25$, which we assessed in section 4.4. Again, this is evidence against the presence of substantial manipulation of the firm size related to the employment quota. Column (3) narrows the bandwidth to $\tilde{L^N}_{it} \in [-6, 6]$. The estimated discontinuity becomes smaller,

amounting to 0.0373 with this smaller bandwidth. The inclusion of second order polynomials in $\tilde{L^N}_{it}$ in column (4) to the specification in column (2) leads to the same effect as narrowing the bandwidth. The estimated discontinuity amounts to 0.0366 being almost identical to that of column (3). Thus, the basic model is sensitive to changes in functional form. The sensitivity analyzes in column (3) and (4) are, however, quite consistent regarding the causal effect of the non-compliance tax. We therefore adopt the model in column (3) as the *baseline model* for the remainder of the paper. Note that results are not sensitive to adopting the model in column (4).

Table 4: The effect of the employment quota on the number of disabled workers per firm at quota threshold $T = 25$ (time period: January 1999 - June 2001)

Panel A: Threshold effect (dep. var.: number of disabled workers)				
Mean	0.2550	0.2550	0.3081	0.2550
Standard deviation	0.6390	0.6390	0.7064	0.6390
Threshold effect	0.0533	0.0545	0.0373	0.0366
Cluster: L^N	$(0.0075)^{***}$	$(0.0081)^{***}$	$(0.0075)^{***}$	$(0.0084)^{***}$
Cluster: L^N, firm	$(0.0133)^{***}$	$(0.0136)^{***}$	$(0.0091)^{***}$	$(0.0098)^{***}$
$L^N \in 25 \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 12$
Polynomial order in $(L^N - 25)$	1	1	1	2
Controls	No	Yes	Yes	Yes
Controls $\cdot$ $(L^N - 25)$	No	Yes	Yes	Yes
Time fixed–effects	No	Yes	Yes	Yes
Time fixed–effects $\cdot$ $(L^N - 25)$	No	Yes	Yes	Yes
Number of observations	448,156	448,156	183,678	448,156
R^2	0.0304	0.0697	0.0517	0.0698
Adjusted R^2	0.0304	0.0695	0.0512	0.0695
Panel B: Bunching effect (dep. var.: firm size density in percent)				
Mean	4.0000	4.0000	7.6923	7.6923
Standard deviation	2.7926	2.7926	2.5369	2.5369
Bunching effect	-0.4195^{**}	0.1464	-0.4646^{***}	-0.2178^{**}
	(0.1577)	(0.1264)	(0.0644)	(0.0820)
$L^N \in 25 \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	2	3	2	3
Number of observations	25	25	13	13
R^2	0.9986	0.9997	0.9998	0.9999
Adjusted R^2	0.9982	0.9995	0.9996	0.9998

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

Is the threshold 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 estimate of column (3) of the threshold effect suggests that the quota leads to 0.0373 more disabled workers holding a job in threshold firms – an increase of about 12 % of disabled workforce of 0.31 disabled workers in firms just below the quota threshold. This change in disabled worker employment is triggered by a non-compliance tax on the order of 8 % of the median non-disabled worker wage (€ 150 in fine per month relative to about € 1,850 in wages per month).

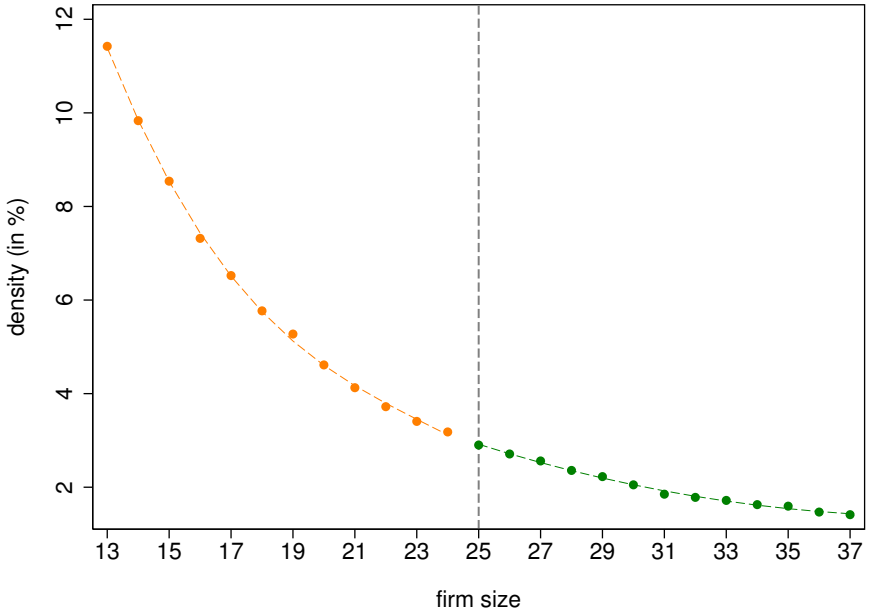
The elasticity of substitution between disabled workers and non-disabled workers is therefore at least 1.60.[17] As Acemoglu and Angrist (2001) conjecture, disabled and non-disabled workers are quite strong substitutes. Indeed, small changes in the price lead to large changes in demand suggesting that a quota policy would not be necessary if wages could be adjusted to account for differences in productivity. Yet anti-discrimination legislation does not permit firms to adjust wages for disability status.

We can use these result to interpret these results on the basis of our behavioral framework. Suppose we can disregard any bias induced by bunching. The fact that the average number of disabled workers hired even without the tax is 0.31 suggests that 31 % of all firms are indifferent between hiring a disabled worker and a non-disabled worker (firms populating area F). This is a sizable proportion of the overall population of firms which can employ disabled workers productively (i.e. their productivity p exceeds the wage rate w). Moreover, our findings indicate that roughly 4 out of 100 firms comply with the quota. This means that about 4 percent of all firms incur a loss from hiring a disabled worker that is no larger than the tax. Taken together, we find that about 35 % of all threshold firms incur small (or no) losses from hiring disabled workers. The remaining 65 % of all firms do not comply with the regulation presumably because the losses from doing so are substantial.

Let us now turn to the bunching effect. Figure 4 reveals no important discontinuity in the density of non-disabled employment around the quota threshold $T = 25$. We test for the presence of a discontinuity in the firm size distribution formally (see McCrary, 2008). Panel B of table 4 shows the bunching effect in more detail and investigates its sensitivity with respect to the functional form in L^N and to the bandwidth h. Column (2) of Panel B uses polynomial order 3 and a bandwidth $h = 12$. This specification repeats the result already displayed in Figure 4. Column (1) reduces the polynomial order of 3 to 2 in L^N and detects a significant bunching effect of -0.42. Columns (3) and (4) reduce the bandwidth to $h = 6$. Again, a significant bunching effect is detected, amounting to -0.46 with a polynomial order in L^N of 2 and to -0.22 with a polynomial order in L^N of 3. Hence columns (1), (3) and (4) of Panel B of Table 4 suggest that firms indeed manipulate their non-disabled employment due to the tax, though this evidence is not completely robust to changes in the specification (see column (2)). We take column (3) as our benchmark specification. This is the largest statistically significant and thus most conservative estimate of the bunching effect.

[17] Recall that the elasticity of substitution is the negative of the percentage change disabled to non-disabled employment caused by a percentage change in the relative disabled to non-disabled wage. Consider the first threshold. Disabled to non-disabled employment increases by 0.15 % (effect of 0.0373 divided by threshold firm size of 25) because the tax reduced the relative wage of a disabled worker by about 7.5 percent (regular monthly earnings are € 1,850; this means that the tax decreases disabled to non-disabled relative earnings from 1,850 / 1,850 to 1,850 / 2,000). The relative disabled and non-disabled wage is 1 whereas the disabled to non-disabled employment level stands at 0.31/25. Thus the elasticity of substitution of threshold firms stands at about 1.60 $(= -(0.0373/25)/(1850/2000 - 1) \cdot 1/(0.31/25))$.

Figure 4: Firm size distribution at quota threshold $T = 25$



Notes: Estimated discontinuity = 0.1464 with standard error = 0.1264 (adjusted for clustering on firm size), based our basic model with a cubic trend in $\tilde{L^N}$ using the density of the firm size distribution (in %) as outcome variable (number of observations = 25). Source: Own calculations, based on ASSD and FWO.

What does the bunching effect of -0.46 imply for our estimated threshold effect? Clearly, the key identifying assumption of our threshold design does not hold in light of this strategic behavior of firms. The above behavioral framework suggests that employment manipulation may lead either to an upward or a downward bias of the threshold effect. Using the estimated bunching effect in Column (3) of Panel A of Table 4, we are able to provide bounds, assuming that bunching arises either entirely from B-firms ($p < w$) or entirely from G-firms ($p > w$). Note first that the bunching effect is informative on the absolute number of manipulators. It suggests that 0.46 percent of the 183,678 firms in the firm size bracket 19–31 manipulate employment. In other words, there are 427 ($= (0.0046 \cdot 183,678)/2$) employment manipulators.[18] To get an *upper* bound of the threshold effect assume that in all firms disabled workers have a productivity larger than their wage ($p > w$). In that case, manipulation of non-disabled employment arises because firms substitute a non-disabled worker with a disabled worker. This kind of manipulation leads to an upward bias in the mean number of disabled workers just below the threshold and thus to a downward bias in the estimated threshold effect. Moving all 427 potential manipulators just above the threshold thereby retaining their substituted disabled worker and recalculating the difference in mean disabled employment yields a threshold effect of 0.0861. The raw difference

[18]Imagine that there are 50 firms on each side of the threshold. Assume now that five firms manipulate their firm size and thus sort below the threshold. There are now 55 firms to the left and 45 firms to the right of the threshold. The resulting discontinuity in the number of firms amounts to 10 firms - twice the number of manipulating firms. This is the reasoning for the division by two.

in the mean number of disabled workers between firms with non-disabled employment $T = 24$ and $T = 25$ is 0.0434, so the bias in the threshold effect amounts to $0.0434 - 0.0861 = -0.0427$. To get a *lower* bound of the threshold effect we assume that manipulation arises *only* because the wage of all disabled workers exceeds their productivity ($p < w$). In that case bunching arises from firms that sort below the threshold to avoid the non-compliance tax. Reassigning all 427 potential manipulators from $T = 24$ to $T = 25$ (manipulators would still hire zero disabled workers) we recalculate the difference in mean disabled employment and obtain a value of 0.0235. Consequently, the bias in the threshold effect amounts to $0.0434 - 0.0235 = 0.0199$.

In sum, our estimated threshold effect is potentially confounded by firm bunching, but the resulting bias is rather small. Our preferred estimate in Column (3) of Panel A of Table 3 amounts to 0.0373. Using our bounding exercise the upward bias is at most 0.0199 and the downward bias is at most 0.0427. Hence our simple thought experiment suggests that the threshold effect is bounded by $[0.0138, 0.0800]$. We conclude that, taking into employment manipulation, still leads to a positive threshold effect. In other words, our estimates suggest that quota rules induce firms to increase employment of disabled workers. Evaluated at average disabled employed of firms just below the threshold 0.31 this increase is between 4.5 % (= 0.0138/0.31) and 25.8 % (= 0.08/0.31). While employment manipulation biases the threshold effect, even our most conservative estimate suggests a positive impact of quota on disabled employment.

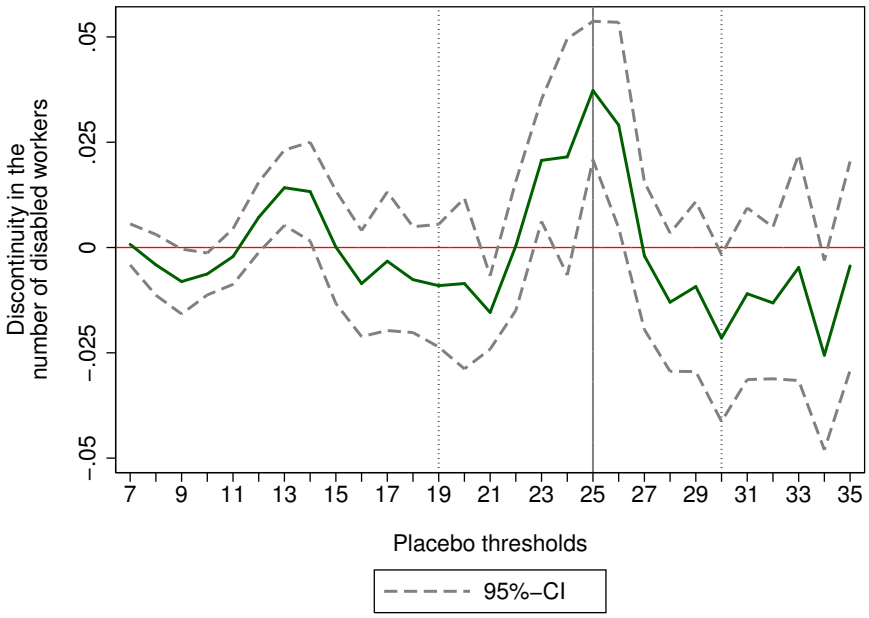
5.2 Placebo Regressions

To further assess the validity of the threshold design, we estimate discontinuities in the number of disabled workers per firm at firm sizes where there should be no discontinuities. Figure 5 shows the estimated discontinuities (according to our baseline model) for firm sizes 7–35 (including the true threshold at firm size 25). The pattern is striking. There is a clear-cut peak at the true threshold, which already begins to grow at around firm size 21 and then flattens out beginning at firm size 26. Note that this is not surprising – given that there is a true discontinuity at firm size 25. The bandwidth of our baseline model is 6, thus the discontinuities calculated at 'placebo'-thresholds 19–24 already consider treated firms.

The estimated discontinuities for the placebo thresholds at firm sizes 7–18 and 31–35, for which either only treated or only control firm are considered, are in all but five instances (at firm sizes 9, 10, 13, 14, and 34) statistically not different from zero at the 5%-level. Note, however, that we test a large number of coefficients at once. Therefore we need to adjust the p-values for the issue of multiple testing. It turns out that once we adjust the p-values for multiple testing according to Holm (1979), only the estimated discontinuity of the true threshold at firm size 25

remains statistically significant at the 5%-level.[19] This strongly supports the credibility of our estimated discontinuity at firm size 25.

Figure 5: Testing continuity of mean disabled employment



Notes: Figure plots the parameter α_1 in regression $L_{it}^D = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{L^N}_{it} + \beta_1 \cdot D_{it} \cdot \tilde{L^N}_{it} + \epsilon_{it}$ where L_{it}^D is number of disabled and $\tilde{L^N}_{it}$ is normalized firm size, i.e. firm size minus threshold, adopting a half-width $h = 6$. This parameter measures the difference in actual mean disabled employment at the threshold compared to mean disabled employment expected from data below the threshold for each threshold between firm size 7 to firm size 35. Note that all thresholds except threshold 25 are placebo thresholds. Parameter estimates within the vertical dashed lines can be affected by the factual discontinuity at firm size 25. Parameter estimates outside the vertical dashed lines can not be affected by the discontinuity. Source: Own Calculations, based on ASSD and FWO

5.3 Effects by low-wage and high-wage firms

Next, we turn to discussing heterogeneity of the treatment effect. Panel A of Table 5 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. These groups differ strongly in (relative) financial incentives. Whereas the average firm in the first quartile face a tax of 12.6 % of its firm wage, firms in the top quartile only face a tax of 5.6 %

[19] We chose the Holm Method, which controls the *family-wise error rate* (FWE). As pointed out by Romano *et al.* (2008), this is the standard approach to account for multiple testing. Romano *et al.* (2008) argue that this criterion can be too strict when the number of hypotheses under consideration is very large and propose to apply the procedure of *false discovery proportion* (FDP). However, we only test 29 hypotheses at once and therefore stick to the standard approach. The procedure of the Holm Method is as follows. The p-value of each estimated discontinuity is ranked from the smallest to the largest. The first p-value is multiplied by the number of investigated (placebo) threshold (29 in our case). The other p-values are consecutively – according to their rank – multiplied by the number of investigated (placebo) thresholds less the number of already adjusted p-values.

of the firm wage (bottom row in table 5).

Table 5: The effect of the employment quota on the number of disabled workers per firm by firms' median daily wage (quartiles) at quota threshold $T = 25$ (time period: January 1999 - June 2001)

	1^{st} Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile
Panel A: Threshold effect (dep. var.: number of disabled workers)				
Mean	0.2868	0.3144	0.3033	0.3283
Standard deviation	0.6947	0.7148	0.6882	0.7261
Threshold effect	0.0758	0.0418	0.0261	0.0015
Cluster: L^N	$(0.0241)^{***}$	$(0.0185)^{**}$	$(0.0104)^{**}$	(0.0076)
Cluster: L^N, firm	$(0.0261)^{***}$	$(0.0216)^{*}$	$(0.0148)^{*}$	(0.0095)
$L^N \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls $\cdot$ $(L^N - 25)$	Yes	Yes	Yes	Yes
Time fixed–effects	Yes	Yes	Yes	Yes
Time fixed–effects $\cdot$ $(L^N - 25)$	Yes	Yes	Yes	Yes
Number of observations	47,575	46,519	43,818	45,766
R^2	0.0606	0.0506	0.0642	0.0814
Adjusted R^2	0.0585	0.0485	0.0620	0.0793
Tax as % of monthly wage	12.6%	9.0%	7.5%	5.6%
Panel B: Bunching effect (dep. var.: firm size density in percent)				
Mean	7.6923	7.6923	7.6923	7.6923
Standard deviation	2.8414	2.9787	2.4155	1.9653
Bunching effect	-0.4248^{**}	-0.6161^{**}	0.1268	-0.9181^{**}
	(0.1393)	(0.2263)	(0.3989)	(0.3167)
$L^N \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	2	2	2	2
Number of observations	13	13	13	13
R^2	0.9984	0.9983	0.9969	0.9955
Adjusted R^2	0.9973	0.9972	0.9947	0.9923
Bounds for threshold effect	$[0.0589, 0.1193]$	$[0.0129, 0.0991]$	n/a	$[-0.0356, 0.0781]$

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

Results indicate that the employment quota produces a strong increase in disabled employment in the first quartile of the wage distribution. Firms in the first quartile provide 0.0758 workplaces for disabled workers which would not be there without the quota (column 1). The estimated discontinuity decreases continuously with the wage level of the firm and amounts to 0.0418 for the second quartile (column 2), 0.0261 for the third quartile (column 3) and (insignificant) 0.0015 for the fourth quartile.

Panel B of Table 5 reports the estimates for the bunching effect. Using again the most conservative specification with bandwidth $h = 6$ and polynomial order in L^N of 2 in terms of magnitude, bunching is detected for all but the third wage quartile (though the effect is not very robust with respect to the bandwidth nor the polynomial order). The magnitude of the bunching

effect seems to be larger the higher we move up in the wage distribution (column 3 being an outlier). Panel B of Table 5 further displays the bounds for the threshold effect based on the same thought experiment as above. This suggests that the threshold effect remains positive for firms in the first and second wage quartile (column 1 and 2).

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.[20] Moreover, about 3 out of 10 firms hire disabled workers even when they do not face the employment obligation and this does not vary much across the wage distribution. This is consistent with the productivity loss due to disability being roughly proportional to the wage rate (or to maximum productivity of an extra worker). Yet, there appear to be more firms incurring only a small loss from hiring a disabled worker rather than a non-disabled worker among low wage firms than among high wage firms. This suggests that the distribution of losses from hiring a disabled worker are more concentrated for low wage firms and more dispersed for high wage firms. Introducing the same nominal tax therefore leads to a weaker employment response for high wage firms than for low wage firms.[21]

5.4 Results for Pooled Quota Thresholds $T > 25$

The above results were based on firms around the first threshold $T = 25$. We now investigate the threshold effect by pooling firms at higher thresholds $T > 25$. The employment quota may act differently for large firms than for small firms. On the 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 6 shows the results for pooled higher thresholds ($T = 50, 75, ...$). Here we assign treatment status according to the deviation from thresholds $\tilde{L_{it}^N} \equiv (L_{it}^N - T_{it})$, where T_{it} represents the nearest threshold L_{it}^N is associated with. Firms are treated if $\tilde{L_{it}^N} \geq 0$ and non-treated if $\tilde{L_{it}^N} < 0$. Remember from section 4.2 that all specifications in table (6) include threshold dummies G_{it}, as well as their interaction with the treatment indicator D_{it} and normalized firm size $\tilde{L_{it}^N}$ and $D_{it} \cdot \tilde{L_{it}^N}$. Thus, the estimated discontinuity can be interpreted as an inverse variance weighted average of the threshold specific treatment effects. Column 1 shows that the effect amounts to 0.1387 if no additional covariates are included. The effect falls to 0.1071 (column 2) when controls, time fixed-effects, and their interactions with $\tilde{L_{it}^N}$ are added. The specification of column 3 uses the smaller bandwidth and the effect vanishes (0.0636). If higher order polynomial

[20] The implied elasticity is larger for firms that pay low wages than for firms paying high wages (2.1 for firms in the 1^{st} quartile, 1.5 for firms in the 2^{nd} quartile, 0.9 for firms in the 3^{rd} quartile, and 0 for firms in the 4^{th} quartile).

[21] Note that it is important to be careful with this interpretation. There are many other aspects that differ between firms that pay high and low wages, for instance industry or market environment, that could also be driving the differences between high and low wage firms.

are added instead, the effect becomes smaller (0.0775), but is statistically significant at least at the 10%-level if standard errors adjusted for clustering on the deviation from the threshold $\tilde{L^N}$ are considered.[22] Panel B of Table (6) shows that no bunching takes place at higher thresholds.

Table 6: The effect of the employment quota on the number of disabled workers per firm at pooled quota thresholds $T > 25$ (time period: January 1999 - June 2001)

Panel A: Threshold effect (dep. var.: number of disabled workers)				
Mean	2.3044	2.3044	2.3833	2.3044
Standard deviation	5.3183	5.3183	5.4524	5.3183
Threshold effect	0.1387	0.1071	0.0636	0.0775
Cluster: $\tilde{L^N}$	(0.0504)**	(0.0394)**	(0.0425)	(0.0419)*
Cluster: $\tilde{L^N}$, firm	(0.0580)**	(0.0524)**	(0.0490)	(0.0706)
$L^N \in T \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 12$
Polynomial order in $\tilde{L^N}$	1	1	1	2
Controls	No	Yes	Yes	Yes
Controls $\cdot$ $\tilde{L^N}$	No	Yes	Yes	Yes
Time fixed–effects	No	Yes	Yes	Yes
Time fixed–effects $\cdot$ $\tilde{L^N}$	No	Yes	Yes	Yes
Threshold dummies	Yes	Yes	Yes	Yes
Threshold dummies $\cdot$ D^a	Yes	Yes	Yes	Yes
Threshold dummies $\cdot$ $\tilde{L^N}$	Yes	Yes	Yes	Yes
Threshold dummies $\cdot$ $\tilde{L^N} \cdot D^a$	Yes	Yes	Yes	Yes
Number of observations	220,187	220,187	111,746	220,187
R^2	0.3859	0.4574	0.4651	0.4574
Adjusted R^2	0.3854	0.4568	0.4639	0.4568
Panel B: Bunching effect (dep. var.: firm size density in percent)				
Mean	4.0000	4.0000	7.6923	7.6923
Standard deviation	0.7929	0.7929	0.7705	0.7705
Bunching effect	−0.0468	−0.0568	−0.1088	−0.0529
	(0.0714)	(0.0717)	(0.1529)	(0.1996)
$L^N \in T \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 6$
Polynomial order in $\tilde{L^N}$	2	3	2	3
Number of observations	25	25	13	13
R^2	0.9959	0.9960	0.9796	0.9796
Adjusted R^2	0.9948	0.9943	0.9650	0.9512

Notes: [a] The interaction term between the threshold dummies and the treatment indicator D is calculated with threshold dummies demeaned by $E[\text{Threshold dummy}_j | D = 1]$. ***, **, * denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO

5.5 Effects by employment status before becoming disabled

This subsection provides separate estimates by the initial state before acquiring the disability status. We consider disabled workers who, at the date of registration as disabled, were employed (i) with the same firm (*own former employees*); (ii) with another firm (*other former employees*);

[22] The elasticity for the threshold firm with 100 non-disabled workers is 1.58 ($= -(0.0636/100)/(2000/2150 - 1) \cdot 1/((2.3044/4)/100)$). Note that average disabled employment is adjusted to reflect that the firm with 100 workers has already passed three thresholds and regular monthly earnings are € 2,000 rather than € 1,850 in small firms (see footnote 17).

or (iii) were not employed (*non-employees*). Providing separate effects for these three groups sheds light on the mechanism that generates excess disabled employment. Threshold firms may retain their own former employees, poach employees from other firms, or create new jobs for workers who were not employed. Encouraging retention is clearly one of the main objectives of the DPEA (see section 2 above). Retention is also likely to conserve firm specific human capital more than generating excess employment through hiring from the non-employment pool or from other firms.[23]

Column 1 in table 7 displays the baseline effect at the quota threshold $T = 25$ (we repeat the estimate in column 3 of table 4 for ease of comparison). Results in column 2 suggest that threshold firms employ 0.0239 more disabled workers who had already been working for the firm when they acquired the disability status. This means that about 64 % of the baseline treatment effect at the quota threshold goes to workers who were already employed by their current employer. The resulting excess employment likely reflects the role of DPEA in increasing retention of existing employees. Whether the retention effect represents an increase in total employment is not clear. Firms may be relabeling existing workers. However, we think that the retention effect is a true employment effect rather than pure relabeling. First, the process of acquiring the status of a severely disabled worker is an involved process with a detailed medical assessment of a workers' work capacity. Hence relabeling a non-disabled worker as disabled is unlikely to happen. Second, since acquiring the disability status comes with substantial benefits to both firms and workers, it is unlikely that an effectively disabled worker postpones acquiring the legal disability status to a date when the firm passes the quota threshold.

Results in column 3 indicate that quota firms tend to have 0.0127 more employees on their payrolls who had been employed in different firms when becoming recognized as disabled. This means that up to 34 % of the treatment effect is generated by reallocating workers from other firms to firms at the quota threshold. Results in column 4 indicate that threshold firms' excess employment is not generated by increased hiring from the non-employment pool.

5.6 Policy Changes

The above results are based on the period January 1999 to June 2001. To further check the robustness of our results, we also provide evidence for preceding and subsequent time periods. During the preceding period, July 1996 to December 1998, firms got a bonus for over-compliance with the quota of € 52–76 per additional disabled worker, whereas during the subsequent period the non-compliance tax was substantially larger. Column 2 of Table 8 repeats the result for the baseline period January 1999 to June 2001 for the quota threshold $T = 25$. Columns 1

[23]Note, however, that these results do not speak about effects on total employment. Retained workers might have found work elsewhere, and workers who used to work at other firms may trigger new hiring at these other firms.

Table 7: Decomposing the treatment effect by employment status at date of registering as severely disabled at quota threshold $T = 25$ (time period: January 1999 - June 2001)

	Number of disabled workers			
	baseline	own former employees	other former employees	non-employees
Mean	0.3081	0.1688	0.0940	0.0453
Standard deviation	0.7064	0.5069	0.3253	0.2377
Threshold effect	0.0373	0.0239	0.0127	0.0007
Cluster: L^N	$(0.0075)^{***}$	$(0.0050)^{***}$	$(0.0018)^{***}$	(0.0022)
Cluster: L^N, firm	$(0.0091)^{***}$	$(0.0055)^{***}$	$(0.0026)^{***}$	(0.0031)
$L^N \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls $\cdot$ $(L^N - 25)$	Yes	Yes	Yes	Yes
Time fixed–effects	Yes	Yes	Yes	Yes
Time fixed–effects $\cdot$ $(L^N - 25)$	Yes	Yes	Yes	Yes
Number of observations	183,678	183,678	183,678	183,678
R^2	0.0517	0.0588	0.0328	0.0126
Adjusted R^2	0.0512	0.0583	0.0323	0.0121
Percentage w.r.t. total effect	100	64	34	2

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

and 3 shows the corresponding results for the preceding and subsequent period. The results are supportive for our basic findings. The estimated discontinuity in column is with 0.0244, roughly two-thirds as big as in the baseline period. This can be explained by the bonus for over-compliance creates incentives for firms that are below the quota threshold to hire a disabled but left incentives of firms above the threshold unaffected. Column 3 of table 8 shows that the threshold effect in the subsequent period is substantially larger than that of the baseline period, consistent with incentives from the higher non-compliance tax during that period. Column 4 pools the entire time period from July 1996 to December 2003 with full interactions with a dummy for period 1 and period 3. Results indicate that the difference between period 2 and 3 is statistically significant at least at the 10%-level (irrespectively of the type of standard error). This is in line with Wuellrich (2010) who finds that the increase in the non-compliance tax had a positive impact on firms' demand for disabled workers. The difference between baseline and preceding periods (columns 1 and 2), however, is not significant, if simultaneous clustering on firm and firm size is taken into account.

Panel B of table 8 indicates that the bunching effect in period 1 and 2 is very similar in magnitude. Surprisingly, the bunching effect in period 3 becomes statistically insignificant despite of the higher non-compliance tax. Again, the bunching results are not very robust to changes

Table 8: The effect of the employment quota on the number of disabled workers per firm for different time periods

	Number of disabled workers			
	Jul 1996 – Dec 1998	Jan 1999 – Jun 2001	Jul 2001 – Dec 2003	Jul 1996 – Dec 2003
Panel A: Threshold effect (dep. var.: number of disabled workers)				
Mean	0.2735	0.3081	0.3373	0.3070
Standard deviation	0.6435	0.7064	0.7701	0.7105
Threshold effect	0.0244	0.0373	0.0607	0.0373
Cluster: L^N	$(0.0054)^{***}$	$(0.0075)^{***}$	$(0.0094)^{***}$	$(0.0075)^{***}$
Cluster: L^N, firm	$(0.0061)^{***}$	$(0.0091)^{***}$	$(0.0103)^{***}$	$(0.0091)^{***}$
Threshold effect · Period 1				-0.0129
Cluster: L^N				$(0.0072)^{*}$
Cluster: L^N, firm				(0.0106)
Threshold effect · Period 3				0.0234
Cluster: L^N				$(0.0105)^{**}$
Cluster: L^N, firm				$(0.0139)^{*}$
$L^N \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls · $(L^N - 25)$	Yes	Yes	Yes	Yes
Time fixed–effects	Yes	Yes	Yes	Yes
Time fixed–effects · $(L^N - 25)$	Yes	Yes	Yes	Yes
Number of observations	179,968	183,678	192,953	556,599
R^2	0.0500	0.0517	0.0465	0.0504
Adjusted R^2	0.0495	0.0512	0.0460	0.0499
Amount of non-compliance tax (in €)	142 – 146	148 – 150	196	–
Bonus for over-complying (in €)	52 – 76	n/a	n/a	–
Probationary period (in months)	1	3	6	–
Panel B: Bunching effect (dep. var.: firm size density in percent)				
Mean	7.6923	7.6923	7.6923	7.6923
Standard deviation	2.5520	2.5369	2.4997	2.4622
Bunching effect	-0.3776^{**}	-0.4646^{***}	-0.1179	-0.4646^{***}
	(0.1432)	(0.0644)	(0.1018)	(0.0662)
Bunching effect · Period 1				0.0870
				(0.1226)
Bunching effect · Period 3				0.3466^{**}
				(0.1495)
$L^N \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(L^N - 25)$	2	2	2	2
Number of observations	13	13	13	39
R^2	0.9991	0.9998	0.9994	0.9994
Adjusted R^2	0.9985	0.9996	0.9990	0.9990
Bounds for threshold effect	$[0.0102, 0.0610]$	$[0.0138, 0.0800]$	n/a	–

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

in the specification. No bunching is detected in either period if we use e.g. a specification with a larger bandwidth and higher polynomials. However, even the bounds derived from the most conservative estimates of the bunching effect displayed in panel B of table 8 show that our estimated threshold effects remain qualitatively valid.

5.7 Wage Determination for Disabled Workers

The evidence so far suggests that threshold firms do react to financial incentives. A key element of shaping demand for disabled workers is the wage paid to disabled workers. The DPEA states that disabled workers must be offered the same contract as non-disabled workers. This implies that firms cannot set wages for job entrants differently for disabled and non-disabled workers. But firms may affect wage growth within firms through promotion decisions. We carried out an in-depth analysis of the determinants of the wages of disabled workers. This section briefly summarizes the results.[24] Among white-collar workers, the disabled earn 14.8 percent less than the non-disabled. Among blue-collar workers the wage gap between disabled and non-disabled workers is twice as large (28.6 percent). The wage gap among white-collar workers does not result from lower starting wages (we do not find any differences in the starting wage), but rather accumulates over time as disabled are less remunerated for each year of tenure. This suggests that disabled white-collar workers are either less promoted or sort into jobs with worse prospect. In contrast, the wage gap among blue-collar workers is not only the result of different returns to tenure but also to gaps in the starting wage (the gap in the starting wage is 12 percent). We conclude that there are substantial wage differentials, but they are most likely not large enough to compensate the productivity differences. This creates the necessary margin for the employment quota to affect firms' demand for disabled workers in the first place (if the wage gap was bigger than the productivity difference all firms would comply).

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 role of employment quota for disabled workers in shaping the marginal firms' demand for disabled employment. This analysis complements existing evidence on anti-discrimination legislation.

The identification strategy relies on the sharp discontinuity in the relative costs of employing disabled and non-disabled workers created in a quota system combined with taxes raised on firms that do not comply with legal employment requirements. We adopt a threshold design, which – in contrast to a RDD – accounts for the fact that the forcing variable (firm size) may be endogenous. Firms may adjust their non-disabled workforce when faced with the financial sanctions stipulated by the quota rule. We derive from a simple behavioral framework that firms

[24] In an online appendix we provide the details of this in-depth analysis of the determinants of the wages of disabled workers.

may indeed manipulate their non-disabled workforce due to the tax, which is a violation of the key identifying assumption of our threshold design. The simple behavioral framework further suggests that this manipulation can either lead to an downward or upward bias in the estimated threshold effect, depending on whether disabled workers are productive or not. Based on these predictions as well as on estimates about the extent to which firms manipulate their non-disabled workforce we are able to provide an lower and upper bound for the threshold effect. It turns out that these bounds are still informative in the sense that our results hold in qualitative terms. We conclude that the application of a threshold design is sufficiently valid in our set-up.

Our results indicate that the quota promotes the employment of disabled workers in firms located at quota thresholds. The quota leads to excess employment of 0.04 or loosely speaking to the employment of one more disabled worker per 25 threshold firms around threshold $T = 25$. We also detect important interactions between wages and firm size. Firms in the lower tail of the firm wage distribution tend to provide most of the excess employment to disabled workers. The employment quota leads to twice as much excess employment among larger firms (the effect is imprecisely estimated though). We also find that the quota boosts employment primarily among former employees of the firm, which suggests that retention of disabled workers, one of the DPEA's main goals, is achieved. The quota also encourages firms to poach workers from other firms but does not lead to hirings from non-employment. In addition, two reforms suggest that raising the non-compliance tax increases excess disabled employment, while paying a bonus to over-complying firms dampens the employment effects of the non-compliance tax.

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 also displace non-disabled workers leading to ambiguous effects on 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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