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Better Protected, Better Paid: Evidence on How Employment Protection Affects Wages

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ABSTRACT

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This paper empirically establishes the effect of the employer's term of notice on the wage level of employees. The term of notice is defined as the period an employer has to notify workers in advance of their upcoming dismissal. The wages paid during this period are an important element of firing costs and hence employment protection. To find a causal effect, I exploit the exogenous change in the term of notice that resulted from the introduction of a new Dutch law in 1999. Strong evidence is found that a longer 'dormant' term of notice leads to higher wages. In my sample, an additional month of notice increases wages by three percent, ceteris paribus.

JEL Classification: C23, J31, J38, J63

Keywords: employment protection, term of notice, wages

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1 Introduction

Employment protection legislation (EPL) has been on the (European) political agenda for several decades and continues to be widely debated. On the one hand employers complain that they are incapable of adapting to economic circumstances because of the high cost of firing employees and on the other hand vulnerable groups of employees complain that the firing risk they face is too large. Policy-makers in the meantime have to decide upon an optimal level of protection. In order to make such decisions politicians need to know how different types of employment protection affect labor market outcomes. In order to provide such answers, a large economic literature on employment protection has been developed.

The rationale for installing employment protection legislation is clearly not to manipulate wages. In practice EPL might however have a considerable impact on them. In fact, this impact is a central issue in the academic EPL literature as it is closely related to the effect of employment protection on employment levels[1]. How these two labor market outcomes are theoretically related depends on the type of employment protection (EP) under investigation, on the relevant labor market institutions and on which further assumptions the respective researcher makes. When one for example considers a competitive economy with individual wage-setting as in Lazear (1990), transferable firing costs such as a severance payment will be shifted to the worker at the onset of an employment contract. In Lazear's model, wages will adjust downwards to take the future firing costs into account and employment will not be affected. Alternatively, one could consider central wage bargaining by a union who cares for incumbent workers only and a general turnover cost associated with dismissals as in Bertola (1990). According to insider/outsider theory, employment protection will then induce unions to bargain for higher than competitive wages. As a result, firms will fire (and hire) fewer workers.

Empirically, only suggestive evidence of the true relationship between employment protection and wages exists. This is partly because most empirical research has analyzed macro-data and composite employment protection indices, which introduces comparability issues and confounding factors into the estimations. This paper seeks to use the Dutch Socio-Economic Panel (SEP) dataset to empirically establish the causal effect of a specific type of employment protection, namely the employer's term of notice (ToN), on the wage level of employees. The term of notice is defined here as the amount of time an employer has to notify an employee in advance of her upcoming

[1] I do not discuss the empirical effect of employment protection on employment in this paper. In order to do a similar fixed effects analysis for employment rather than wages, one would need a dataset containing a substantial number of individuals that are observed over time as being both fired and non-fired. The Dutch socio-economic panel unfortunately features too little of these observations.

dismissal. Specific groups, notably older workers, are often protected by a longer term of notice. The term of notice is usually ignored in the empirical employment protection literature. This is unfortunate as a long term of notice corresponds to a large number of obligatory wage payments and hence is a substantial firing cost to the employer.

The Dutch labor market has two relevant features that makes it suitable as a research ground for this paper. First, for each worker, it is possible to calculate the 'dormant' term of notice. I define 'dormant' here as the notice period that would apply in case the employer would soon want to fire the worker. The term of notice can be calculated because the law sets out a formula to do so, which generates variation in the degree of employment protection for workers of different ages and tenure. Second, in 1999 the legal formula to calculate the term of notice was changed. In short, low-tenured workers of all ages benefited from the introduction of the law on flexibility and security as their employer's term of notice increased while older high-tenured workers experienced a shorter term of notice after the 1st of January 1999. This paper will exploit this exogenous policy change to answer the causality question and to separate the age, tenure and term of notice effects on wages.

The remainder of this paper presents evidence of a strong positive causal effect of the term of notice on wages. Each additional month increases wages by three percent. It is also demonstrated that regressions that ignore the multi-collinearity issues involved can overestimate this effect. The theoretical literature provides two plausible reasons for a positive effect of employment protection on wages. First, insider/outsider theory suggests that the bargaining position of insiders is enhanced by better protection when unions only represent incumbent workers. In wage negotiations this could then drive wages up. Second, employment protection creates more incentives for firms and workers to invest in match-specific human capital. These investments will then lead to higher productivity and higher wages. Longitudinal data reflecting individual productivity in all types of jobs would be necessary to ultimately distinguish between the alternative explanations. Unfortunately, this is not available in The Netherlands. Using the alternative data at hand, I do present suggestive evidence that invalidates the investment argument.

This paper continues as follows. Section 2 discusses the most important findings in the employment protection literature on wages. Section 3 then lays out the term of notice regulations in The Netherlands and the changes introduced in the law of flexibility and security ('Flexwet'). The empirical strategy is explained in Section 4. The utilized data are discussed in Section 5 and results will be presented in Section 6. Section 7 concludes.

2 Literature

2.1 Theoretical literature

Employment protection, i.e. firing costs, comprises of two elements: *taxes* to be paid outside the job-worker pair and *transfers* from the firm to the worker (Garibaldi and Violante, 2005, p.799). Taxes are broadly defined here: any kind of legal costs associated with layoffs fall under the first type. The term of notice and the severance pay however fall under the second type of employment protection. The length of the term of notice, or the number of additional wage payments, partly determines the size of the transfer firing costs. An extensive theoretical literature looks into the effects of employment protection or firing costs on the labor market. Concerning wages, the predictions of what employment protection actually does vary greatly. Many of these differences result from considering different types of employment protection and different types of labor market institutions. Some of these views are explained in this section, although I do not claim to be exhaustive.

In the employment protection literature (e.g. Oi, 1962, and Bentolila and Bertola, 1990), whether a profit-maximizing firm would like to fire an individual worker primarily depends on the wage of the worker and her match-specific productivity[2]. When the difference between these entities is negative, an employer loses money and she will consider firing the employee. It is costly however to adjust the number of employees downward because of associated firing costs. When wages are fixed and whenever there is a possibility that in the nearby future the worker's productivity could increase, higher firing costs such as a longer term of notice then lower the propensity to fire (and hire) a worker.

Wages are exogenous in the greatest part of this literature. Notable exceptions to this will be discussed here. Bertola (1990) draws from the insider/outsider literature. See Lindbeck and Snower (2001) for an overview. In short, the theory divides the labor market into insiders - incumbent workers who benefit from employment protection - and outsiders - those who do not benefit such as temporary workers and the unemployed. Because it is difficult to get rid of them, insiders have some bargaining power in the wage process and hence demand higher than competitive wages (for a formal model, see Lindbeck and Snower, 1986).

Bertola looks at how employment protection affects wages under different wage setting institutions. First, he investigates labor demand and endogenous wages when wage negotiations take place at the individual level. He finds that under certain assumptions total received wages might not be affected, although outsiders might offer to work for a very low wage in order to become an insider and insiders might afterwards rise wage demands above

[2] This paper considers individual lay-offs that have a financial firing rationale.

the competitive level. Second, Bertola assesses wages when there is a wage setting union that cares for everyone in the labor market. He concludes that in this set-up lifetime wages would also remain unaffected. Only in the instance of unions who solely represent working members does employment protection increase total labor income for insiders indefinitely.

Garibaldi and Violante (2005) exploit the idea that a country's wage-setting institutions influence the effect of employment protection on wages in a search and matching framework. The authors built a model with endogenous wage setting behavior by a monopolistic union. Garibaldi and Violante stress that in such a setting the introduction of an exogenous firing cost has two opposing effects on the workers' desired wage level: workers would like to have a higher wage (*the income effect*) but do not enjoy the accompanying higher probability to get fired (*the job security effect*). Whenever the elasticity of the firm's firing probability to wages is low enough, workers will demand higher wages when they are better protected.

Lazear (1990) wrote an influential paper on employment protection, arguing that firing costs do not necessarily affect hirings and firings. He reasons that in a flexible labor market, in the absence of contract and market restrictions, *transfer* employment protection such as the term of notice could be undone by efficient wage setting behavior between workers and firms. He predicts that in a competitive economy with decentralized wage setting, firing costs drive wages down, up to the point where the severance pay and the wages paid during the term of notice can be seen as a delayed payment. Note that Lazear predicts wages to go down at the onset of an employment contract. Pissarides (2001) also suggests a negative effect of employment protection on wages, but argues from the workers point of view (like Bertola and Rogerson, 1997). In his search and matching model the term of notice is endogenous and generates lower wages because risk averse workers accept a lower income during the productive period of a job, in order to receive a higher income during unproductive times.

Because most of the relevant literature thinks about workers as having fixed or at least exogenous productivity, it often ignores another possible route through which employment protection positively affects wages. More employment protection namely enhances the incentives for a firm to invest in a worker and for a worker to invest in firm-specific human capital. Nickell and Layard (1999) briefly describe this mechanism. These human capital investments could pay off in terms of higher productivity and higher wages. Arulampalam, Booth and Bryan (2004) present some indirect empirical evidence that employment protection does increase training of employees. Using a European dataset, they find that those on fixed term contracts take up less training than those on permanent contracts.

While certain theories thus suggest the term of notice could increase wages, others argue it could decrease wages. The most appropriate theories for this empirical study are the ones involving some market imperfections, moderate centralized wage-setting and exogenous employment protection. Lazear's argument is hence not likely to hold in the Dutch economy, in which employers organizations and a small number of labor unions negotiate over wages per industry. For more information on wage setting in The Netherlands see Wallerstein, Golden and Lange (1997).

2.2 Empirical literature

The empirical literature has experienced difficulties in establishing a clear relationship between firing costs and wages. Some of the papers discussed in the previous subsection do attempt to present empirical evidence of their models. However, the authors typically only provide suggestive evidence of their theories, mainly because most of them use macro-data and aggregate indices of employment protection that are hard to compare. The estimates are furthermore troubled by confounding factors. Contrary to what his theoretical model predicts, Bertola (1990) for example presents some evidence that the productivity wage gap is actually lower in countries with stricter employment protection. The empirical wage setting literature in its turn often ignores employment protection as it is so hard to quantify. See for example the establishment-level study by Blanchflower, Oswald and Garrett (1990) and the cross-country study of industry wage differentials by Holmlund and Zetterberg (1991). Both papers do suggest substantial insider wage gains.

An interesting firm-level study by Autor, Kerr and Kugler (2007) includes employment protection - i.e. *tax* employment protection - explicitly. The paper exploits U.S. state variation in the adoption of wrongful-discharge protections in order to study firm-level productivity differences. The authors find that the introduction of these laws coincided with a rise in capital investment, non-production worker employment and hence measured a labor productivity increase. Another firm-level analysis was published by Martins (2009), who analyzes a Portuguese policy change in *tax* employment protection that favored firms with twenty or less workers. In 1989 the strict Portuguese rules and regulations considering layoffs were considerably softened, and more so for the smaller firms. Using a large administrative dataset that links employers to employees he finds, among other things, that after the policy change average wages in the smaller firms fell more than in the larger firms. This suggests that in a highly regulated labor market the better protected workers earn more.

The use of micro-data in the empirical employment protection literature is limited. This is unfortunate as micro-data is often a prerequisite for identifying causality and as even in highly regulated economies wages are very heterogeneous across workers of different ages and tenure in the same firms. I am aware of only one micro study on employment protection and wages, which is a paper by Leonardi and Pica (2007). The authors empirically analyze the effect of severance payments on male wages by exploiting an Italian policy change that introduced severance payments for unjust dismissals for firms with less than fifteen employees. This policy change is explained in more detail in Kugler and Pica (2008). Their paper, like mine, thus analyzes *transfer* employment protection rather than legal protection such as in Martins (2009). Leonardi and Pica apply a regression discontinuity design, with the discontinuity being the number of employees, to study entry wages and the tenure wage profile. They use individual wage information from an administrative employers dataset from the Veneto region in Northern Italy. Contrary to their theoretical predictions, the authors find no causal effect of severance payments on entry wages. They do find that the average returns to tenure of previously dismissed workers declined by three percent in the smaller firms, relative to larger firms that did not experience an increase in employment protection. Leonardi and Pica interpret this as partial evidence for Lazear's argument that government-mandated employment protection can be shifted to employees if any employment contract is allowed. Using a Dutch dataset of individuals of all tenures and backgrounds, I find an opposite average effect of employment protection on wages, namely a strongly significant positive one. Additional research should teach us which specific characteristics of the analyzed environments generate these differences.

3 Term of notice

There are two paths to dismissal in The Netherlands, and the legal term of notice only applies to the labor office path. This is a relatively slow route that does not require severance payments. The labor office has the discretion to refuse an application, but only does so in a small percent of the cases (i.e. five percent in 2002). Mainly individual lay-offs in small- and medium sized firms and collective lay-offs are dealt with by the labor office, but the path is open to all employers. The other route, through court, is faster and involves substantial severance payments[3]. The cantonal court judge in principle always allows a lay-off but adapts the required severance payment to the specific firing rationale (i.e. a worker receives more if the employer could have done more to prevent the lay-off)[4]. An employer is free to choose a dismissal path and the labor office is thus a credible threat to all workers. Furthermore, nothing changed in the court procedure over the analyzed

[3] Figure A-1 in the appendix shows the ratio of lay-offs through the labor office over the total number of lay-offs. A constant percentage of about fifty percent of lay-offs are handled by the labor office. A small increase in the ratio can be seen in 1999, the year the legal term of notice was changed. It can be argued that the labor office procedure became more profitable that year because the term of notice went down for the majority of workers.

[4] In a limited amount of cases a cantonal court judge refuses to end the labor contract, for example when a sick worker is dismissed because of her illness.

period. This means that the employer's term of notice and the associated policy change are relevant to all employees on a permanent contract.

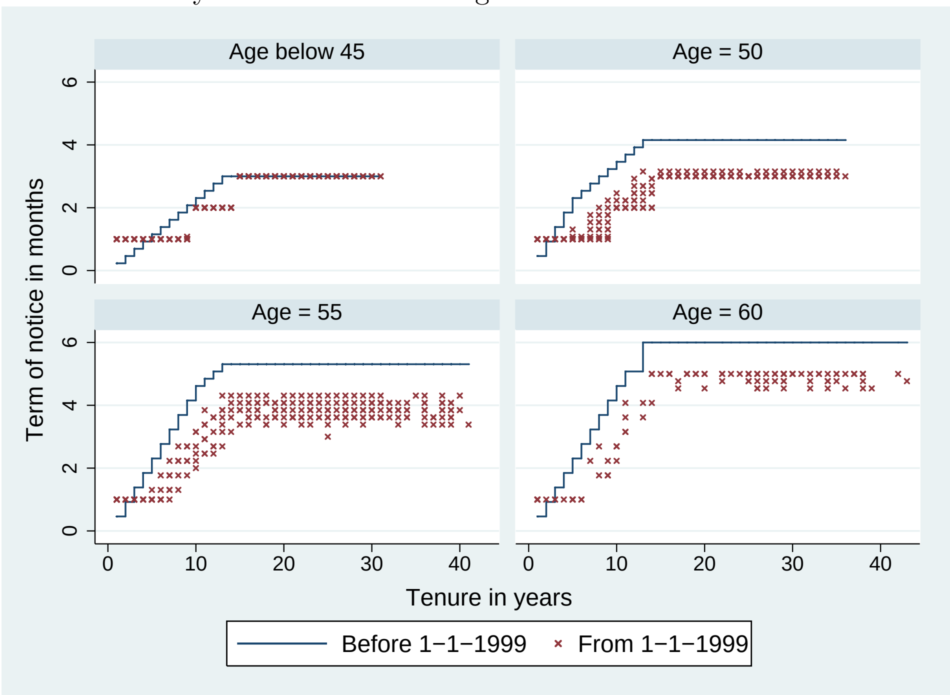
The policy change that will be exploited in the fixed effects method below is the introduction of the law on flexibility and security (the 'Flexwet') in The Netherlands on January 1st, 1999. This law intended to diminish differences in the labor market between temporary and permanent workers. On the one hand, employees with temporary contracts received better legal protection than before. On the other hand, employees with permanent contracts lost some rights. The main change for tenured employees was the adaptation and simplification of the legal formula for the term of notice. See Heerma van Voss (1998) and Smitskam and Kronenburg-Willems (2000) for a detailed description of the new law. The calculation of the term of notice before the 1st of January 1999 (old-ToN) was as follows. For every year of tenure an employer had to add a week to the notice period, with a maximum of thirteen weeks. On top of this, workers received an extra week of notice for each year they had worked while being forty-five or older, also with a maximum of thirteen weeks. Note that tenure - which determines the term of notice - is legally defined as the amount of time that has passed since one started working for a certain employer.

The law introduced on the 1st of January 1999 consists of two elements: a new term of notice formula for newly-hired employees and a transitory arrangement for workers that were already employed. The new formula does not depend on age[5] and accommodates fewer term of notice possibilities. For workers employed in between zero and four years employers face a term of notice of one month. When a worker is employed between five and nine years, her employer will have to notify her two months in advance. If a worker's tenure is between ten and fourteen years, her employer will have to notify her three months in advance. Any tenure longer than fourteen years results in a term of notice of four months.

The 'Flexwet' included an important transitory arrangement for those workers that were already employed by the 1st of January 1999. For these employees, the employer had to calculate both the old term of notice for the employee's tenure and age on the 1st of January 1999 and the new term of notice for the employee's tenure and age at the time of firing. The longest notice period of the two applied. This transitory arrangement was agreed upon so that no workers would suffer a large sudden decline in EP. Another feature of the new scheme was that for both newly hired workers and those in the transitory scheme, the law allows employers to deduct one month of the notice period if they have waited to end the labor agreement until after they have received permission from the labor office. The minimum term of notice should however remain one month, so that the deduction only applies

[5]All age related components of Dutch employment laws are supposed to be phased out in order to satisfy European anti-discrimination treaties.

Figure 1: Employer's Term of Notice before and after the introduction of the 'Flexwet' by tenure for selected ages of workers



to workers with a tenure above four years. This subtraction is granted in almost all relevant cases (i.e. ninety-six percent in 2002 according to the Ministry of Social Affairs and Employment). In my 'dormant' term of notice calculations I assume that this permission is always granted. Note that I define 'dormant' here as the notice period that would apply in case the employer would soon want to fire the worker. Hence, the introduction of the 'Flexwet' generated a discrete change in EP for many workers at the beginning of 1999, and the transitory scheme ensured a further gradual move towards the new scheme.

The relationship between the old and new term of notice thus depends on age and tenure of the worker and on calendar time. More specifically, besides on tenure and time, it depends on tenure obtained from the age of 45 onwards. Figure 1 shows the old- and new-ToN for the respondents in the entire SEP dataset over tenure and selected ages. As the term of notice will be identical for all employees under 45, there is one panel representing them. Additionally, three panels display the relationship between tenure and the term of notice for all 50-, 55-, and 60-year old workers.

As can be seen in the below-45-panel, the old term of notice went up by a week each tenure year and had a maximum of thirteen weeks. How the policy change affected different workers depends on their respective tenure.

The new term of notice turns out to be longer than the old term of notice for those employees with a tenure below five years. Young workers with a tenure from six to fourteen years experience a new term of notice that is shorter than the old-ToN. For young workers with a tenure of both five and a tenure above fourteen years the old and new term of notice are exactly the same.

A different picture emerges for older workers. As can be seen in the panels for the 50-, 55- and 60-year old workers, the old term of notice went up by two weeks each tenure year for at least some of the years and the maximum term of notice was longer than thirteen weeks. The difference between the three panels is determined by how much older one is than 44 as this explains the number of two-week-years in the graph. For employees with a tenure below three years the new term of notice is longer than before. Those who have been working for their employer for exactly three years face the same term of notice before and after the introduction of the 'Flexwet'. The term of notice of workers with a tenure of four years and over is shortened in the new scheme. Wherever there are multiple terms of notice visible for an age-tenure combination this is because of the transition scheme. This arrangement ensures that the term of notice moves closer to the new calculation as the evaluation date is further from the 1st of January 1999. So, the employers of two workers with identical tenures (say fifteen years) and age (say sixty years) but fired at different times after the policy change (say at the 1st of February 1999 and at the 1st of February 2002) will face different terms of notice (in this case five months and four-and-a-half months).

Although the length of the term of notice that applies in the labor office path is set out by law, the 'Flexwet' made it possible to agree upon a different term in a collective wage agreement. For the analysis in this paper it is important to know to what extent this occurred[6]. Combining information from the Ministry of Social Affairs and Employment and my own calculations I find that after the 1st of January 1999 the legal term of notice applied to eighty percent of workers[7]. If the deviation from the labor

[6] Smits (2000) and Smits and Samadhan (2002) report that the one month deduction, with a minimum term of notice of one month, prevailed in practically all agreements settled after the 1st of January 1999.

[7] About twenty percent of the workers do not fall under any sort of collective agreement and hence the legal term of notice will always apply to them. The other eighty percent of workers do fall under a collective agreement. I checked 112 (of about 200) collective wage agreements in alphabetical order that were declared to hold for all workers in the relevant sector between 1999 and 2002 and found that roughly seventy percent of these featured the term of notice set out in the 'Flexwet'. In sixteen percent of the cases a term of notice calculation that is related to age and tenure still existed. In fourteen percent of the analyzed collective wage agreements I found a constant term of notice. If I assume that the distribution of workers is equal across the different agreements it can be concluded that eighty percent of workers on a permanent contract face the legal term of notice after January 1st 1999.

law, i.e. the measurement error in the real term of notice, is independent of wages, the measurement error leads to an attenuation bias which will drive the coefficient of the term of notice down. Any significant results will hence be underestimating the true causal effect of the term of notice. In the Smits and Samadhan report the distribution of deviations over sectors seems rather equal. Therefore, and because it is hard to know which agreement specifically applies to which worker, I abstract from the deviation in the collective wage agreements and assume that the legal term of notice applies to everyone. In the empirical specification I do control for industry categories.

For older, long tenured employees the introduction of the law on flexibility and security thus resulted in a lower term of notice, but for shorter tenured workers of all ages the term of notice increased. This variation in the direction of the policy change will prove useful in identifying the causal effect of the term of notice on wages in Section 6.

4 Empirical strategy

The dependent variable in my analysis is the log of the real gross hourly wage rate. Several econometric difficulties trouble the identification of a causal effect on this rate of the term of notice. Everything arises from the fact that for the vast majority of employees in The Netherlands the term of notice is determined by a deterministic function of the total number of years an employee has worked for her employer and the number of years she has done so while being 45 or older. The only other variable influencing the employer's term of notice is calendar time.

$$ToN_{it} = f(\text{Tenure}_{it}, \text{Tenure after } 45_{it}, \text{Time}_t)$$

Note that calendar time both determines under which legal framework a worker is fired and also plays a role in the transition arrangement from the old towards the new scheme. The analyzed wage model can be seen in equation 1, where X_{it} refers to personal characteristics, Z_{it} to employer and job characteristics, υ_i to an individual-specific time-invariant error term and ε_{it} to an i.i.d error term.

$$Wages_{it} = h(ToN_{it}(\text{Tenure}_{it}, \text{Tenure after } 45_{it}, \text{Time}_t), X_{it}, Z_{it}) + \upsilon_i + \varepsilon_{it} \tag{1}$$

The first econometric problem that arises is that of strong multicollinearity between the term of notice and the important covariates tenure and age. When one would analyze periods in which the term of notice formula doesn't change, its marginal effect on wages cannot be determined ceteris paribus as changes in the notice period from one year to the next would coincide with changes in tenure and age. To nevertheless answer the research question, one can exploit an exogenous change in the term of notice that does not coincide with a change in tenure and age. To identify a causal relationship between the term of notice and wages this paper therefore uses the exogenous policy change described in Section 3: the introduction of the law on flexibility and security in The Netherlands. It is not necessary to apply any specific type of policy evaluation method however. For reasons explained below, I will estimate a fixed effects regression model which has the additional trait that - through the subtracted average - it automatically incorporates the exogenous variation in the term of notice.

Even when including the exogenous policy change, it is still crucial to correctly control for tenure and tenure beyond the age of 45 so that an estimated term of notice coefficient does not capture any linear or non-linear relationship between these variables and wages. Without imposing any structure on the relationship beforehand I therefore include a full set of all relevant tenure times tenure after 45 dummies. These dummies control for the effect of tenure on wages, for the effect of tenure in older ages on wages and for the interaction effect of the two on wages in the most flexible way. I observe 43 different tenures and 19 different tenures experienced over the age of 45. This leaves me with a total of 513 tenure times tenure beyond 45 dummies[8]. Naturally, other interactions between age and tenure, such as tenure obtained over the age of 25, could potentially also influence wages. However, omitting such interactions will not bias the coefficient of interest as these interactions do not enter the term of notice calculations.

Solving the multi-collinearity and non-linearity problems alone does not result in unbiased estimates of the term of notice coefficient. This is because tenure and subsequently the term of notice suffer from an endogeneity problem. Tenure is an endogenous variable because unobservable characteristics such as work attitude and innate ability influence tenure as well as wages. As a result of the fixed formula for the notice period this endogeneity stains the term of notice variable as well. A permanent endogeneity problem can and will be addressed by applying a fixed effects type estimator that filters out any time-invariant individual components in the wage regression. Doing so however does not remove a potential non-permanent effect of the unobserved quality of a worker on tenure and wages induced by the 1999 policy change[9].

I will explain the non-permanent endogeneity problem by focusing on two types of workers: low-quality workers that earn a relatively low wage and high-quality workers that earn a relatively high wage. Here I assume that the type is unobserved by the econometrician. Of both types of workers, the relatively high-tenured ones (53 percent of the sample) will have experienced a decrease in their employer's term of notice on the first of Jan-

[8]The full set of tenure interactions amounts to less than 45*19=817 dummies as the tenure obtained beyond 45 will always be equal to or smaller than general tenure.

[9]I am grateful to an anonymous referee for pointing this out.

uary 1999. It is possible that this decrease had a different impact on the two types in terms of layoffs and job switches. Low-quality employees could have been fired sooner than their high-quality colleagues. These fired low-quality workers would then either leave the sample or re-enter the sample in a new job. This would lower the number of low-paid workers in the high term of notice group, resulting in an overestimation of the effect of the term of notice on wages. Note that underestimation is also a possibility, as the 1999 policy change made high-tenured jobs less attractive in terms of employment protection. Hence all workers could have become more likely to voluntarily quit a high-tenured job. High-quality workers potentially quitted their jobs more often than their low-quality colleagues as they receive better alternative job offers. This would increase the number of high-paid workers in the low term of notice group, resulting in an underestimation of the term of notice effect on wages. As it is unclear which job switches and which lay-offs would have taken place without the policy change, the described time-variant type of endogeneity cannot be fully controlled for.

The quantitative importance of the effect can be looked at in more detail. First, it can be checked whether the inclusion of those who get fired and those who change jobs voluntarily drives the term of notice results. To check the robustness of estimates I run a wage model on various restricted samples, such as a sample without anyone that is ever observed as being fired and a sample without anyone that is ever observed to voluntarily switch jobs. The results of these exercises are hopeful and can be found in the sensitivity analysis in Section 6. It seems that the term of notice results are robust to the inclusion or exclusion of various groups of mobile workers.

Second, it can be checked whether the number of firings and job switches before and after 1-1-1999 differed substantially. Table 1 shows what has happened to the workers in my sample in the year following their interview. It shows unconditional percentages for those whose term of notice would have or went down and for those whose term of notice would have or went up. The latter category also includes individuals whose term of notice remained unchanged. The first two columns therefore present the percentages for relatively high-tenured workers, with an average tenure of fourteen years, while the last two columns present the percentages for relatively low-tenured workers, with an average tenure of six years. Unsurprisingly, all job turnover percentages are lower for the high-tenured group than for the low-tenured group. Moreover, both groups experienced more dismissals and voluntary job quits after 1-1-1999. Overestimation of the effect of the term of notice on wages requires a substantial number of low-quality, high-tenured workers that are fired because of the policy change. Since only 0.2 percent more high-tenured workers were fired after 1-1-1999, it is unlikely that the non-permanent endogeneity of tenure drives the positive effect on wages found in Section 6.

Table 1 deserves more explanation. Although theory predicts that the

Table 1: What happens to employees in the year following inclusion in the sample, before and after 1-1-1999. Sample divided on the basis of tenure and tenure over 45.

	'Flexwet' decreased ToN (High average tenure)		'Flexwet' increased ToN or kept constant (Low average tenure)	
	Before	After	Before	After
Same job	93.4%	91.3%	90.0%	82.2%
Fired from job	2.5%	2.7%	5.0%	8.1%
Quitted - Better job	0.6%	0.9%	1.1%	1.6%
Quitted - Personal	3.4%	5.1%	3.9%	8.0%
Total known	3,518	4,601	2,721	4,201
Attrition - Unknown	351	728	324	770

group who 'suffered' from the policy change by being less protected should display larger increases in job turnover rates this is not what is happening. In fact, the group of individuals with more incentives to stay in their jobs (as their term of notice mostly increased) displayed larger increases in these rates. Apparently, other economic circumstances and policy changes that occurred between 1997 and 2001 affected the job flows of the two groups of workers in the opposite direction. If this is the case, wages might have seen an opposite development as well. Fortunately, it is possible to control for these differential time trends in wages as selection into the higher- or lower term of notice group depends on tenure and tenure beyond the age of 45 only. In the regressions in Section 6 differential wage trends are controlled for in a flexible way by adding interaction terms between a dummy for those surveys after 1-1-1999 and the full set of tenure times tenure after 45 dummies. While doing so, the transition arrangement that was part of the 'Flexwet' ensures that there is enough variation in the term of notice after 1999 for identification of its effect on wages. Table 2 in Section 6 shows that excluding the possibility of differential wage trends from the regressions overestimates the effect of the term of notice of wages.

The 1999 policy change does have one drawback following directly from the advantage laid out above. The separate effect of employment protection on wages of newly hired individuals cannot be analyzed. As Leonardi and Pica (2007) do, it would be interesting to look at this group separately as wages of outsiders (newly hired individuals) could respond differently to a change in employment protection than wages of insiders. As all new employees are under the same term of notice scheme from the 1st of January 1999 onwards, the effect of the change in the term of notice cannot be distinguished from other wage developments in this group. Table 2 does show estimates for newly hired individuals only to illustrate that a negative effect of employment protection on wages is unlikely.

Given the discussion above, the term of notice results will be referred to as 'causal' from now on. The causal effect I am interested in can thus be obtained as the marginal effect of the term of notice variable in a regression of wages on the term of notice and the full set of tenure times tenure beyond the age of 45 dummies (and interactions of this set with a dummy for all periods after 1-1-1999). To furthermore control for other determinants of wages I include a full set of age dummies, a full set of time dummies, the job characteristics hours worked and level of occupation and the employer characteristics company size and industry type. Equation 2 is then estimated as a fixed effects linear regression model using the log of the real gross hourly wage rate as the dependent variable. In this equation X_{it} refers to all personal covariates, Z_{it} to employer and job characteristics, $\sum_{t=1998}^{2001} \text{Time}_t$ to a set of time dummies, υ_i to an individual-specific time-invariant error term and ε_{it} to an i.i.d error term.

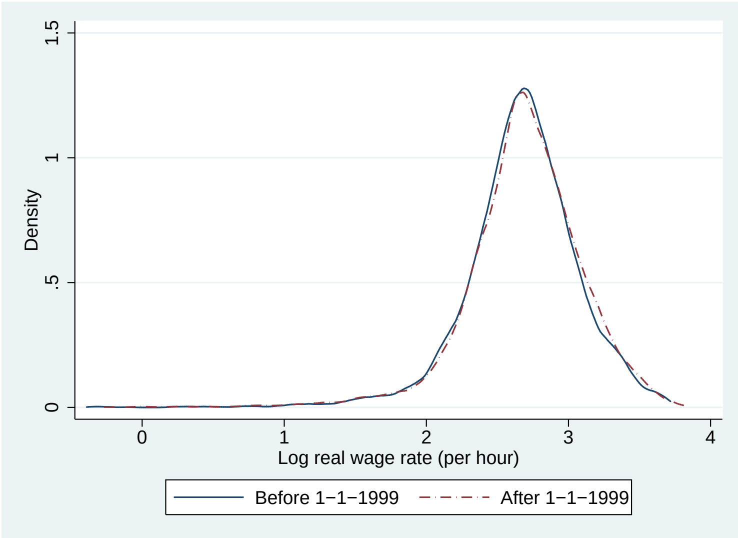
$$Wages_{it} = \alpha + \beta_{ToN} * ToN_{it} + \gamma' X_{it} + \delta' Z_{it} + \sum_{t=1998}^{2001} \zeta_t \text{Time}_t + \upsilon_i + \varepsilon_{it} \quad (2)$$

5 Data

Five waves of the Dutch Socio-Economic Panel (SEP), a household survey, are used for the empirical analysis of the research question (1997-2001). This longitudinal dataset has been collected annually around April by Statistics Netherlands from 1984 to 2002. I use all available waves before and after the 1999 policy change that contained information on the type of contract a worker was on and on the sector an individual was employed in. Note that a job in this paper refers to a contract between an employee and an employer and that internal promotions ore demotions thus do not play a role. Only employees with a permanent contract are included in the sample as employers only face a legal term of notice for these workers. This leaves me with a final sample of 17,214 observations. Although the same individuals are observed multiple times in the sample, it is not balanced. 79 percent of the individuals are observed both before and after the policy change, 7 percent only before and 15 percent only after.

The dependent variable in the wage regressions is the logarithm of the real gross hourly wage rate. Net wages are not directly observed. Over the analyzed period the income tax legislation did not change substantially. I use the consumer price index published by Statistics Netherlands to compute real wages (base year is 2005) and the conversion rate between the Dutch guilder and the euro that has been fixed to 2.20371 from the 31st of December 1998 onwards. For simplicity I use this exchange rate for the 1997 and 1998 waves as well. In each survey year, all observations below the 0.1th percentile and above the 99.9th wage percentile are dropped, because rates

Figure 2: Kernel density estimation of the hourly wage rate for the period before and after 1-1-1999 (N=17,214)

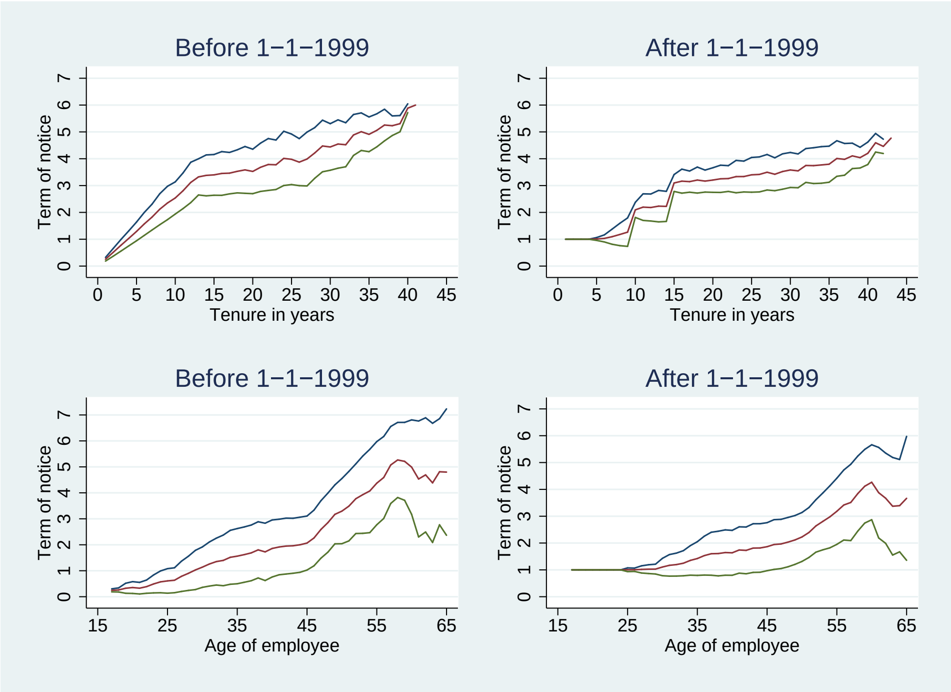


in these areas seem unlikely. Figure 2 shows a kernel density estimation of the cleaned variable both for the period before the introduction of the 'Flexwet' and the period after. The distribution in real wages is very similar across the two periods. The average wage rate before the policy change was (2005 real) €15.59 euro per hour (s.d. 5.83) and after the policy change it was €15.89 (s.d. 6.08). Those with a tenure above two years earn more (€16.63, s.d. 5.72) than those who are new to their employer (€13.41, s.d. 6.07). As expected, those over 45 also earn more on average (€17.76, s.d. 6.56) than their younger counterparts (€14.79, s.d. 5.42).

Figure 3 provides information on the average terms of notice in my sample before and after the policy change for different tenures and ages. Before 1999 the aggregate average term of notice was 2.1 months (s.d. 1.5), whereas it was 1.8 months (s.d. 1.1) after the introduction of the 'Flexwet'. Figure 3 also presents the ninety percent confidence intervals. It can be seen that the new law increased the minimum and decreased the maximum notice period and that the variation in employment protection across ages and tenures was diminished. In the new scheme, a more distinct stepwise pattern by tenure is also observed.

Figures A-2 and A-3 in the appendix show histograms of the ages and tenures of the workers in my sample both before and after the first of January 1999. Before 1999, the average age is 39.4 years (s.d. 9.4) and the average tenure is 10.2 years (s.d. 8.7). After the first of January 1999, the average age in the sample is 39.9 years (s.d. 9.8) and the average tenure is 9.9 years (s.d. 9.2). A relatively large group of employees are only shortly

Figure 3: Average employer's Term of Notice and its ninety percent confidence interval in the sample pre 1999 and post 1-1-1999 by tenure and age



employed at their employer, and this is more so in the period after 1-1-1999. In the sensitivity analysis in the next section, I check whether only looking at newly hired employees, or only at all individuals other than the newly hired affects the term of notice results. This is not the case.

The regressions presented in table 2 include the following control variables of which the descriptive statistics can be found in table A-1 in the appendix: all tenure times tenure over the age of 45 dummies, all age dummies, dummies for the years 1998 to 2001, hours worked, industry and size of the organization in which the worker was employed and level of her occupation. Table A-1 also includes information on a dummy representing whether someone took up training paid by the employer and several education dummies. These variables are used in the regressions presented in table 3.

6 Results

6.1 Impact of term of notice on wages

Table 2 presents the results of the empirical wage analysis. The table only displays the coefficients and standard errors of the variable of interest - the term of notice. This is done for expositional reasons as the inclusion of all age and tenure times tenure beyond 45 dummies in the regressions makes these difficult to interpret[10]. All specifications include observations of before and after the 1999 policy change. Hence, multi-collinearity problems can no longer bother the estimates. Note that in both table 2 and 3 all standard errors were clustered at the individual level. The regression diagnostics can be found in table A-1 in the appendix.

The coefficient and standard error in the first row refers to a fixed effects linear regression estimate of the log of real gross hourly wages on the term of notice and the described covariates. The model is preferred as it includes interactions between the dummies that determine the term of notice (tenure times tenure after the age of 45 cells) and a dummy for the period after the first of January 1999. As discussed in Section 4 this is done to make sure that the term of notice coefficient does not capture any differential wage developments over time other than the introduction of the 'Flexwet'. In the preferred model, the term of notice coefficient equals 0.0324 and is highly significant. This means that for each additional notice month the hourly wage rate of a worker goes up by three percent, ceteris paribus. The estimate in the second row is biased because the fact that wage trends could have been different for low and high-tenured workers is ignored. The result in row two suggests that an additional legal month of notice increases the wage rate, ceteris paribus, by 5.67 percent, which is a

[10] All results are however available upon request from the author.

Table 2: Term of notice coefficients and standard errors in wage models. Dependent: log of real hourly wage rate.

	Model	Sample	Coefficient	S.e.	N
1	Preferred	Full sample	0.0324***	(0.005)	17,214
2	No interact.	Full sample	0.0567***	(0.007)	17,214
3	OLS	Full sample	0.0077	(0.007)	17,214
4	Preferred	Without 1999	0.0324***	(0.005)	13,807
5	Preferred	Balanced panel	0.0267***	(0.007)	8,650
6	Preferred	Ind before/after	0.0390***	(0.005)	13,582
7	2000 interact.	Full sample	0.0351***	(0.005)	17,214
8	2001 interact.	Full sample	0.0375***	(0.005)	17,214
9	No interact.	Newly hired	0.2084	(0.305)	3,462
10	Preferred	Tenure >1	0.0296***	(0.007)	13,752
11	Preferred	No lay-offs	0.0307***	(0.005)	16,053
12	Preferred	No job switches	0.0356***	(0.006)	14,133
13	Preferred	No lay-offs/switches	0.0337***	(0.006)	13,333

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All regressions include all tenure times tenure after 45-dummies, all age dummies, year dummies, hours worked, industry type, size of company, level of occupation and a constant.

The preferred model includes interaction terms of the tenure cells and a dummy for after the policy change.

substantial overestimation of the impact of the term of notice. In the third row a coefficient and standard error are presented for a pooled OLS regression and the regression estimate therefore suffers from a time-independent endogeneity problem. The effect of the term of notice on wages is only 0.77 percent in this specification and not significantly different from zero. Not controlling for the time-invariant endogeneity of tenure can hence underestimate the effect of the term of notice on wages.

To check the robustness of the results in the preferred model it was estimated on several samples and the coefficients obtained in this sensitivity analysis can be found in rows four to thirteen. The analysis clearly confirms that the positive significant effect of EP on wages is robust. The model in the fourth row of table 2 does not use observations recorded between April 1998 and March 1999. This was done to ensure that the causal effect of the term of notice on wages is not the result of wage setting behavior anticipating the January 1999 policy change. Although I do not find such anticipation, note that such behavior would not contradict but rather emphasize a causal effect of the term of notice on wages. The fifth line displays the term of notice coefficient for the wage model on a balanced panel sample in which all individuals are observed five years in a row. The estimates are reassuring as the ninety-five percent confidence interval of the significant term of notice coefficient includes 0.0324. The model presented in row six only includes individuals that are observed both before and after the policy change (so that I loose twenty-one percent of the sample). The associated effect of the term of notice is even larger than that found in row one: a four percent wage increase per month. To be more prudent, my preferred model thus includes the individuals that are only observed at either side of the policy change.

Row seven and eight display term of notice coefficients for models in which the possibility for differential time trends with different cut-off years was included. The model in row seven includes interactions with a dummy for all periods after 1-1-2000. The model in row eight includes the same interactions but then with a dummy for all periods after 1-1-2001. It seems that the causal effect found in row one is not driven by my choice of interactions as the coefficients in row seven (0.0351) and eight (0.0375) are even larger. Row nine and ten present the estimates of the wage model on a sample of newly-hired individuals only and on a selected higher-tenured sample respectively. Note that the latter coefficient will be biased because differential wage trends cannot be taken into account and because selection into this group is highly selective. The selection problem also plagues the longer-tenured individuals estimate. However, the positive marginal effects that I find in both models (0.2084, not significant, and 0.0296, significant) are reassuring.

As mentioned in Section 4 including individual-specific fixed effects does not solve all endogeneity problems in the data. Although the time-variant type of endogeneity cannot fully be controlled for I can show that the inclusion of marginal groups does not severely affect the term of notice results. In the estimation for row eleven in table 2 all those individuals are excluded that are ever observed as being fired from 1996 to 2002. This decreases the sample size to 16,053. The inclusion of this group of people could potentially overestimate the term of notice effect. Indeed the term of notice coefficient in row eleven is lower than in the first row. However, it is precisely estimated at 3.07 percent, which is similar to the estimate in row one. Row twelve presents results for a sample in which those individuals that ever voluntarily switched jobs from 1996 to 2002 are excluded. The inclusion of these workers would perhaps underestimate the term of notice effect, as explained in Section 4. The result in row twelve does suggest this is the case. The estimated term of notice coefficient (0.0356) is closer to 0.04 than to 0.03. Row thirteen presents an estimate in which both groups, the ever fired and the ever job switchers, are excluded. This leaves me a sample of 13,333 individuals who either stay in their job over the sample period or who quit their job for personal reasons. Using this subgroup of the population, I estimate a significant effect of the term of notice on wages of around three percent per month (0.0337) as well.

I thus find evidence that the employer's term of notice has a strong positive causal effect on wages. A three percent higher wage rate for each additional month of notice is a relevant and substantial side-effect of this type of employment protection. For a prime-aged worker in my sample, aged 50 with 15 years of tenure in 1998, who experienced a drop in term of notice from 3.15 months to 2.15 months because of the 1999 policy change, this equals a loss in the hourly wage rate of three percent. On a yearly basis this employee, who worked 38 hours a week and earned 19.71 euros, therefore lost €1,262 because of the lower employer's term of notice.

6.2 Suggestive evidence of mechanism at play

As discussed in Section 2 there are two plausible reasons for a positive effect of employment protection on wages. First, insider/outsider theory suggests that the bargaining position of insiders is enhanced by better protection when unions only represent incumbent workers. In wage negotiations this could then drive wages up. Second, employment protection creates more incentives for firms to invest in workers and for workers to invest in firm-specific human capital. These investments will then lead to higher productivity and higher wages. Using the data at hand I can only provide suggestive evidence on which of the two mechanisms seems more important. For this purpose table 3 in included.

In rows one and two I present the term of notice coefficients of models estimating formal training on the same covariates as in row one of table 3. The dependent variable here is a dummy equal to one when the employee is enrolled or has been over the previous year in a course or training pro-

Table 3: Term of notice coefficients and standard errors. Dependent row 1-2: dummy for currently following formal training paid by the employer. Dependent row 3-5: log of real hourly wage rate.

	Topic	Model	Coefficient	S.e.	N
1	Training	FE OLS	-0.0075	(0.006)	17,434
2	Training	Probit	-0.0705**	(0.023)	15,475
3	Wages	Low educated	0.0574***	(0.022)	1,864
4	Wages	Middle educated	0.0318***	(0.009)	6,017
5	Wages	High educated	0.0277***	(0.008)	4,017

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All regressions include the same controls as in table 2.

gram that her employer is paying for. Although investment in a employee-employer match could involve more or other things than formal training, one would expect a positive coefficient of the term of notice on formal training take-up if the investment mechanism would play a significant role. Row one depicts results for a linear probability model and row two for a pooled probit estimation[11]. Both estimates are negative (-0.0075, not significantly, and -0.0705, significantly), suggesting that better protected individuals take up less training offered by their employer. Arulampalam et al. (2004) found a similar effect using the European Community Household Panel.

In rows three to five I present term of notice coefficient estimates for the preferred wage model on three different samples: the sample is divided into those with a low level of education (regular high school or lower), those with a middle level of education (higher level high school or vocational training) and those with a high level of education (higher tertiary education). This was done as I suspected a potential investment mechanism to have differential effects over the unobserved quality of workers. If I assume that there is a strong positive correlation between education and this quality and if I furthermore assume that the gains from investing in match-specific human capital would be higher for workers with higher abilities, the better-incentives-to-invest effect would ensure a higher term of notice coefficient for the better educated. Note that finding such a higher coefficient could also indicate that better able employees also excel at wage bargaining. However, rows three to five in table 3 paint the opposite picture. The term of notice coefficient for the low educated equals 5.74 percent, while for the middle and high educated it is around three percent. Low educated individuals thus benefit more from employment protection in terms of wages. If one indeed believes that higher educated individuals are better able to obtain match-specific human capital, this result also invalidates the investment ar-

[11]The large number of independent variables rendered estimating a fixed effects binary choice model impossible.

gument.

These pieces of evidence point in the direction of an improved bargaining position as the mechanism driving the positive effect of employment protection on wages. In the Dutch context, a longer term of notice seems to improve the position of employees in wage negotiations such that they are able to extract a larger part of the rent generated in their job. Note that this conclusion should be treated with caution as the evidence is only suggestive. More research will be necessary to come up with a decisive answer on what exactly explains the positive term of notice coefficients.

7 Conclusion

This paper establishes the causal effect of the employer's term of notice on the wage level of employees. The legal term of notice is defined as the amount of time a firm is required to notify a worker in advance of her upcoming dismissal. As such, the term of notice is an important component of firing costs and thus of employment protection.

In order to find a causal link, I have performed a fixed effects estimation exploiting an exogenous policy change in the term of notice. This procedure corrects for the time-invariant endogeneity of tenure and for the strong multi-collinearity between the term of notice, tenure and age. The latter problem arises because tenure and age, or more specifically tenure and tenure obtained while being 45 or over, are the only inputs in the legal formula that calculates the term of notice. The relevant policy change is the 1999 introduction of the law on flexibility and security ('Flexwet') in The Netherlands. This law altered the calculation of the legal term of notice such that the term of notice of low-tenured individuals of all ages went up and the term of notice of older high-tenured individuals went down. Five waves of the Dutch Socio-Economic Panel (SEP) were used for the empirical analysis (1997-2001) in which a fixed effects linear regression model is estimated using the logarithm of real gross hourly wages. A possible limitation of my approach is that tenure and the term of notice are also influenced by the policy change itself, thereby generating a time-variant endogeneity problem. Robustness analysis however suggests that this type of endogeneity is not driving the results.

The preferred econometric model unveils a very significant, positive causal effect of the 'dormant' term of notice on the real hourly gross wage rate. An increase in the term of notice of one month leads to three percent higher wages. The arguments presented in Section 2 describing a negative effect of employment protection on wages thus do not hold in the analyzed context. This was to be expected for Lazear's (1990) efficiency wage argument as the Dutch labor market institutions do not resemble his competitive model with individual wage-setting. The same could be said for Pissarides'

(2001) exposition about an endogenous term of notice as the term of notice is decided upon by policy-makers.

The theoretical literature provides two plausible reasons for a positive effect of employment protection on wages. First, insider/outsider theory suggests that the bargaining position of insiders is enhanced by better protection when unions only represent incumbent workers. In wage negotiations this could then drive wages up. Second, employment protection creates more incentives for firms to invest in workers and for workers to invest in firm-specific human capital. These investments will then lead to higher productivity and higher wages. More empirical research needs to be conducted to ultimately decide which of these theories is best describing reality. To do so, detailed information on individual employees such as longitudinal micro-data reflecting individual productivity is needed.

Using the available information instead, I present suggestive evidence that undermines the investment argument. First, better protected employees do not have a higher take-up rate of formal training paid by their employer. If anything, those with a longer term of notice participate less in such courses, which is not what one would expect if the investment mechanism was playing a large role. Second, the effect of the term of notice on wages is much stronger for lower-educated individuals than for middle- and high educated individuals. If one believes that higher educated individuals are better able to obtain match-specific human capital, this also suggests that the investment argument is least important. Hence, the bargaining argument wins the first round in the battle of the mechanisms. In the Dutch context, better employment protection probably improves employees' wage bargaining position such that a larger part of the profits flows to the employee rather than to the employer. Policy makers should be aware of this side-effect of the term of notice on wages. If the wage bargaining argument is indeed stronger, employment protection creates rents for incumbent employees which policy-makers will want to avoid.

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Appendix

Figure A-1: The ratio of lay-offs through the labor office over the total number of lay-offs (Source: Ministry of Social Affairs and Employment, 2003)

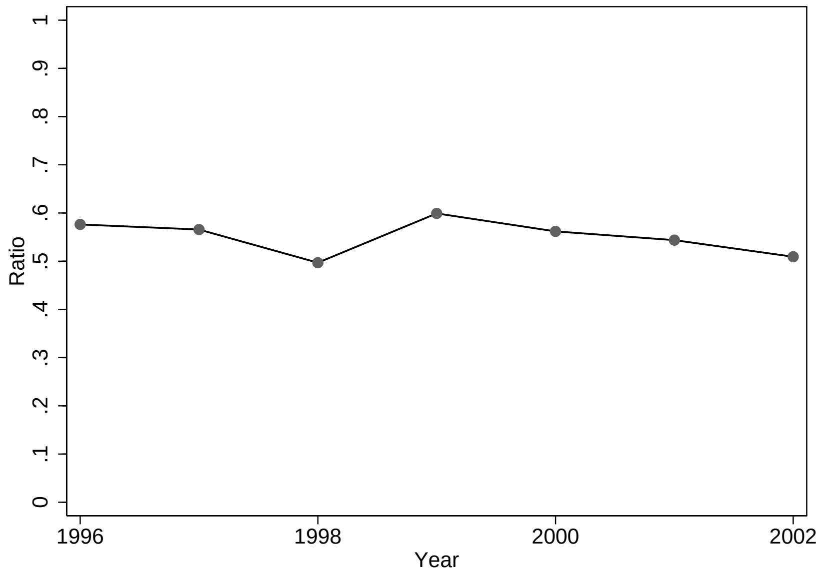

Figure A-2: Histogram of ages in sample

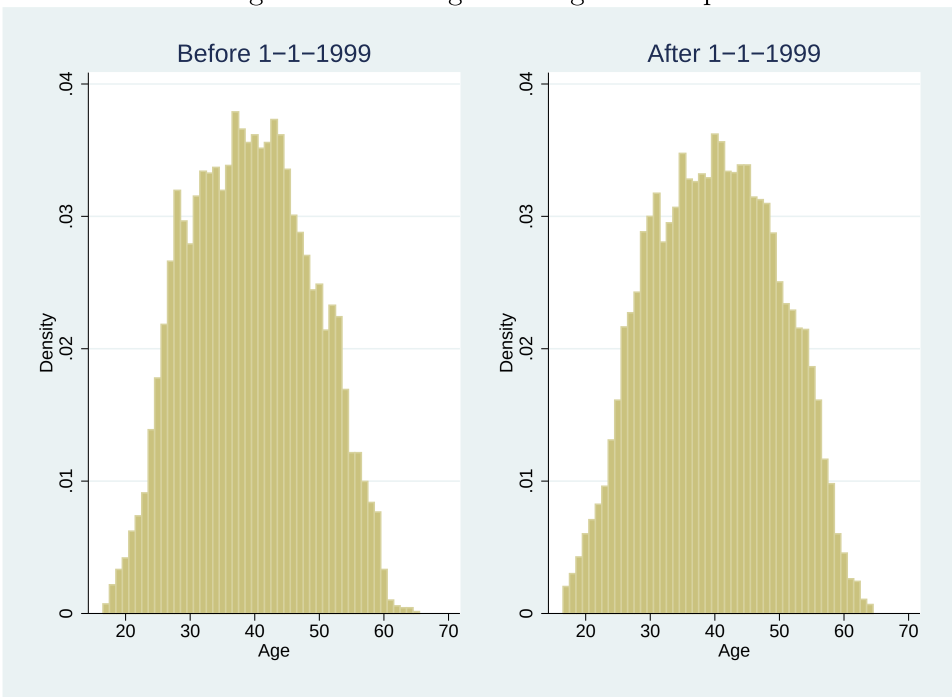

Figure A-3: Histogram of tenure in sample

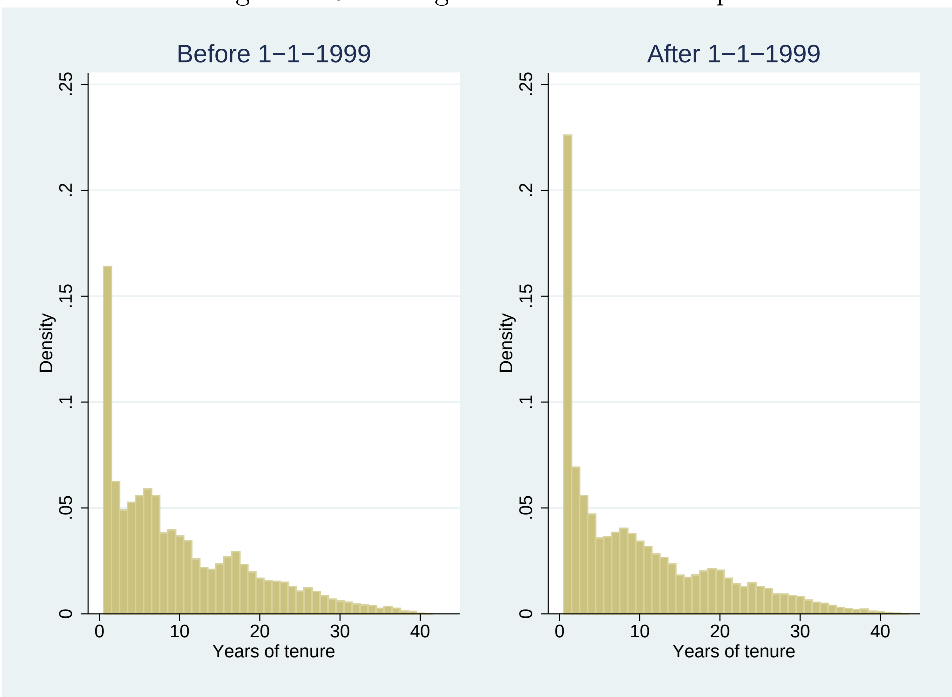

Table A-1: Descriptive statistics of all variables (N=17,214)

	Mean	Sd	Min	Max
Log gross wage rate (per hour)	3.473	0.409	0.387	4.529
Term of notice (months)	1.935	1.273	0.231	6.000
Age (years)	39.694	9.661	17	65
Tenure (years)	10.028	8.996	1	43
Tenure beyond 45	1.629	3.262	0	19
Dataset	1998.995	1.419	1997	2001
Hours worked (per week)	36.231	10.246	12	89
Agriculture	Omitted			
Fisheries	0.000	0.011	0	1
Mining	0.012	0.110	0	1
Industry	0.143	0.350	0	1
Trade	0.006	0.079	0	1
Construction	0.060	0.237	0	1
Retail	0.109	0.312	0	1
Hospitality	0.014	0.119	0	1
Transport	0.058	0.233	0	1
Financial institutions	0.043	0.203	0	1
Real estate	0.098	0.297	0	1
Public services	0.090	0.287	0	1
Education	0.081	0.273	0	1
Health care	0.158	0.364	0	1
Environmental services	0.026	0.158	0	1
Personal services	0.001	0.029	0	1
Other industry	0.099	0.299	0	1
Company 1-19 employees	0.173	0.378	0	1
Company 20-49 employees	0.125	0.330	0	1
Company 50-99 employees	0.100	0.300	0	1
Company >99 employees	Omitted			
Unknown level	0.065	0.246	0	1
Elementary occupations	0.053	0.223	0	1
Lower occupations	0.271	0.445	0	1
Middle occupations	0.342	0.474	0	1
Higher occupations	0.210	0.407	0	1
Academic occupations	Omitted			
Employer-paid training	0.079	0.270	0	1
Lower educated	0.1083	0.311	0	1
Middle educated	0.3495	0.477	0	1
Higher educated	0.2334	0.423	0	1

Table A-2: Regression diagnostics of the models in table 2 and 3.

	Model	Sample	Aic	Ind.	N
1	Preferred	Full sample	-21,228	5,522	17,214
2	No interact.	Full sample	-21,178	5,522	17,214
3	OLS	Full sample	8,445	5,522	17,214
4	Preferred	Without 1999	-17,993	5,396	13,807
5	Preferred	Balanced panel	-7,375	1,730	8,650
6	Preferred	Ind before/after	-14.261	3,189	13,582
7	2000 interact.	Full sample	-21,350	5,522	17,214
8	2001 interact.	Full sample	-21,432	5,522	17,214
9	No interact.	Newly hired	-3,004	2,102	3,462
10	Preferred	Tenure >1	-22,893	4,659	13,752
11	Preferred	No lay-offs	-20,649	5,078	16,053
12	Preferred	No job switches	-18,930	4,565	14,133
13	Preferred	No lay-offs/switches	-18,621	4,248	13,333
1	FE OLS	Training sample	-6,800	5,561	17,434
2	Probit	Training sample	8,964	5,408	15,475
3	Preferred	Low educated	-2,075	612	1,864
4	Preferred	Middle educated	-6,964	1,681	6,017
5	Preferred	High educated	-5,502	1,148	4,017

Akaike's information criterion in fourth column, number of individuals in fifth column.