

# Institutional reforms that really matter: OECD institutional indicators vs. Dutch reform history

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# Institutional reforms that really matter: OECD institutional indicators vs. Dutch reform history

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October 27, 2008

## Abstract

This paper analyses the impact of institutional reforms in the Netherlands on the social security network. We distinguish between disability, sickness, unemployment, employment protection legislation and active labor market policy reforms over the period 1980-2005 using both qualitative and econometric analyses. Our findings are compared to OECD institutional indicators constructed by OECD, following Blanchard and Wolfers (2000) and Nickell et al. (2001,2005). We show that these indicators are far too rough and do not correctly pick up the reforms and developments that took place over time.

JEL codes: J65, J33, J81

Keywords: Policy Reforms, OECD Indicators, Labor Market, Social Security.

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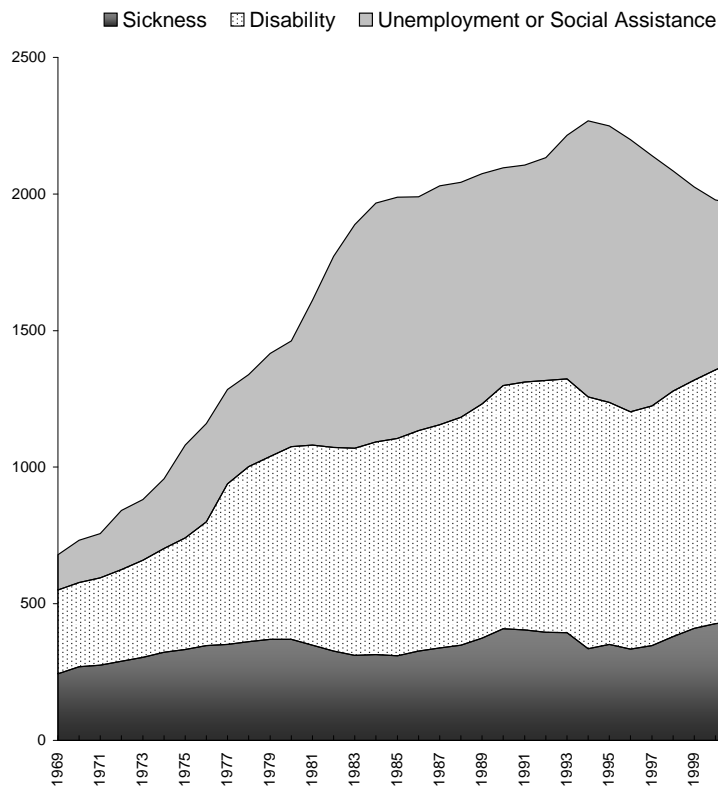
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# 1 Introduction

In 1977 The Economist supplied the Netherlands with the (in this case notorious) honor of having an economic phenomenon named after the country: The Dutch Disease.<sup>1</sup> A massive growth of the social security system occurred throughout the 1970s period of Dutch Disease, as is evident in Figure 1 below from a huge surge in benefit recipients between 1969 and the early eighties.<sup>2</sup> To counter this development the Dutch government embarked on a reform path that would give the Netherlands a frontrunner position within the European Union with regard to the intensity and outreach of institutional reforms. It would also repair the honor of the Netherlands with regard to the country-name based economic phenomena: With the Dutch Miracle and the Dutch notion of ‘Poldermodel’ the score was more than settled.<sup>3</sup>

**Figure 1:** Social security benefit recipients



Based on CPB data from Macro-economic Outlook 2007

<sup>1</sup>The Dutch Disease refers to the phenomenon of declining manufacturing industry caused by appreciation of the exchange rate when natural resources are found.

<sup>2</sup>Population growth was not markedly large in this period so the surge in benefit recipients is not mitigated by population growth.

<sup>3</sup>The Dutch Miracle refers to the period of high economic growth and low unemployment in the second half of the 1990s. The Poldermodel refers to consensus based policy making by government, unions and employers. An important outcome of the Poldermodel was wage moderation, which was a main driving force behind the Dutch Miracle. See (Muysken (2003))

Nickell and van Ours (2000: 173) acknowledge the Dutch Miracle and conclude that the Netherlands have seen a “*major decline in equilibrium unemployment and non-employment [sickness and disability] rates since the early 1980s.*” Using institutional data from amongst others Nickell and Layard (1999), Scarpetta (1996) and Elmeskov et al. (1998), they point to a positive relation between institutional reforms and a lowered equilibrium unemployment rate. This is exemplary for the widespread use, re-use and modification of a relatively limited data source originally dating back to the OECD Jobs Study of 1994. In this paper we intend to assess whether these OECD institutional indicators correctly reflect actual developments in Dutch reform history. To accomplish this we analyze the effectiveness of the reform efforts undertaken by the Dutch government to reduce the dependency and the expenditures on the social security system after the increase of benefit claimants throughout the seventies and early eighties (see Figure 1).<sup>4</sup>

We start in section 2 explaining our method of ranking reforms. In section 3 we discuss the incidence of reforms, looking at the policy areas they target and the main instrument(s) used in implementing the policy. Our research in section 3 also identifies which reforms, reform areas and reform instruments we hold to be of considerably large impact. We use an econometric analysis in section 4 to verify the results obtained in the preceding section and to show in more detail which reforms were of large impact. In section 5 we apply our findings to check whether the widely used OECD institutional indicators correctly pick up the reforms and developments that took place over time. Finally we draw some overall conclusions in section 6.

## **2 Ranking the reforms: our method**

In order to distinguish reforms that have been of large impact from reforms that were insufficiently substantial we started with an extensive literature survey taking into account a wide variety of sources ranging from academic journals to working papers from private and government research organizations. We collected all labor market reform information available in Brandt et al (2005), the labor market reforms database of the Fondazione Rodolfo DeBenedetti (FRDB), the LABREF database of the European Union, and the OECD economic surveys of the Netherlands (OECD (2000)). This was necessary because although all of these databases are supposed to include all important reforms, there are huge discrepancies between them. An added wider literature survey on Dutch labor market reforms was necessary to provide additional background information. The literature on Dutch (labor market) reforms usually distinguishes between sickness (ZW), disability (WAO) and unemployment (WW) reforms. These are mostly reforms that directly aim at reducing the amount of sickness, disability

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<sup>4</sup>For a brief description of the Dutch social security system before the start of the reforms see Bovenberg (2000).

or unemployment benefit claimants. In addition to that the international literature differentiates also between active labor market policies (ALMP) and employment protection legislation (EPL) - e.g. (Nickell et al. (2005)). These last two reform areas are often used to classify the more broadly targeted reforms that try to activate persons to participate in the labor market or that try to optimize the functioning of the labor market. To get a comprehensive picture of the Dutch reform history we thus take into account sickness, disability, unemployment, ALMP and EPL as the five main reform areas.

In the next step of our reform ranking exercise we developed a formal ranking system that allowed us to distinguish between reforms that have been of large impact and reforms that were insufficiently substantial, using a methodology that is based on a qualitative assessment. Our ranking scheme, summarized in Annex I, benefits from the discrepancies in the databases to determine a score for each reform. The underlying assumption here is that the more important a reform is, the more likely it is that it shows up in the reform databases. Thus, if a reform is not mentioned in Brandt et al (2005), FRDB, or LABREF in the relevant time frame, we reduced its score by 1 for each database it is not mentioned in.

If the reform was encountered in our additional background literature review, was defined as ‘Structural reform’ in FRDB, or if it was mentioned in OECD economic surveys, we augmented its score by 1 each time. To account for the evaluation of the reform’s effect in the OECD surveys or the background literature and to leave room for our own opinion regarding a reform’s effectiveness we created the option to reduce or augment the score with a maximum of 2 points. We only sparsely used our own opinion (termed ‘theoretical consideration’ throughout this paper) to modify a score of a reform and have consistently pointed out in the discussion of the indicators in section 3 whenever we did so (see Annex 1.A. in Gerards, Müllers and Muysken (2008) for a case by case overview).

The weakness of this approach is that all reform databases treat different time frames. The FRDB database covers 1987 - 2005, Brandt et al. cover 1994 - 2004, and LABREF covers 2000 - 2006. Thus, for 1980 - 1986, we could only rely on the OECD economic surveys and on our literature review. Given that the OECD surveys contain a lot of information on these reforms, we are convinced that the ranking of those reforms is reliable. Therefore, the ranking scheme in those years works a bit different. The main strength of the ranking scheme is that it relies on many academic sources. However, to avert issues of sensitivity the only relevant distinction we make is between reforms of large impact and reforms that were insufficiently substantial.<sup>5</sup> An overview of the reforms and the ranking scheme can be found in Annex 1.C in Gerards et al. (2008).

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<sup>5</sup>For the purpose of this paper we don’t distinguish anymore between reforms with rank higher than 0 post-1986 or 1 pre-1986, as these are all in the category of large impact. The reforms with a rank lower than 0 post-1986 or 1 pre-1986 make up the rest of the reforms and are considered to be insufficiently substantial.

### 3 The incidence of reforms in the Netherlands

As described in section 2 we have constructed a reform database and developed a formal ranking scheme that enables us to distinguish between reforms that had a considerable impact and reforms that were not substantial. A first visualization of our reform database is shown in Figure 2, which depicts the number of reforms<sup>6</sup> over time and over government, distinguishing also between considerably large impact reforms and the total number of reforms (thus including the insufficiently substantial reforms).<sup>7</sup>

An interesting observation is that there are four main waves of reforms; 1986/1987, 1995/1996, 1998/1999 and 2004.<sup>8</sup> One sees from Figure 2 that these waves occur each time just after the installation of a new government. The concentration of reforms is significantly lower towards the end of a political business cycle, which usually lasts four years. The often unpopular nature of social security reforms, which is reasonably expected to be an influence on chances for re-election, is very likely to be a factor in this timing pattern. Another factor likely to be of influence here is the time needed to see if a certain wave of reforms actually produces the desired results. Especially after 1986/1987 (in which a massive system wide reform was implemented) we see a fairly long ‘waiting period’ of relatively moderate reform activity. From a point of policy implementation this makes sense, since implementing reform upon reform might result in uncontrolled accumulation of effects, possibly leading to unwanted side effects.

Figure 2 does not show a clear pattern with regard to the relation between the total number of reforms in a specific period and the number of reforms of considerable impact that would result from these. Periods of high total reform activity do not necessarily produce relatively more reforms of considerable impact, nor do they produce significantly more insufficiently substantial reforms.

Moving into more detail Table 1 shows the amount of reforms initiated by year and by reform area.<sup>9</sup> The left part of the table counts only the reforms that we found to be of large impact. The right part of the table in addition includes the reforms that we found to be insufficiently substantial. The distribution of reforms over time shows that unemployment has been on the mind of policy makers continuously, with (large impact) reforms occurring throughout the entire period. Examples of unemployment reforms with large impact include the 1982 Wassenaar agreement, which was centered on unions and industry agreeing to lower working hours in order to create additional jobs. The

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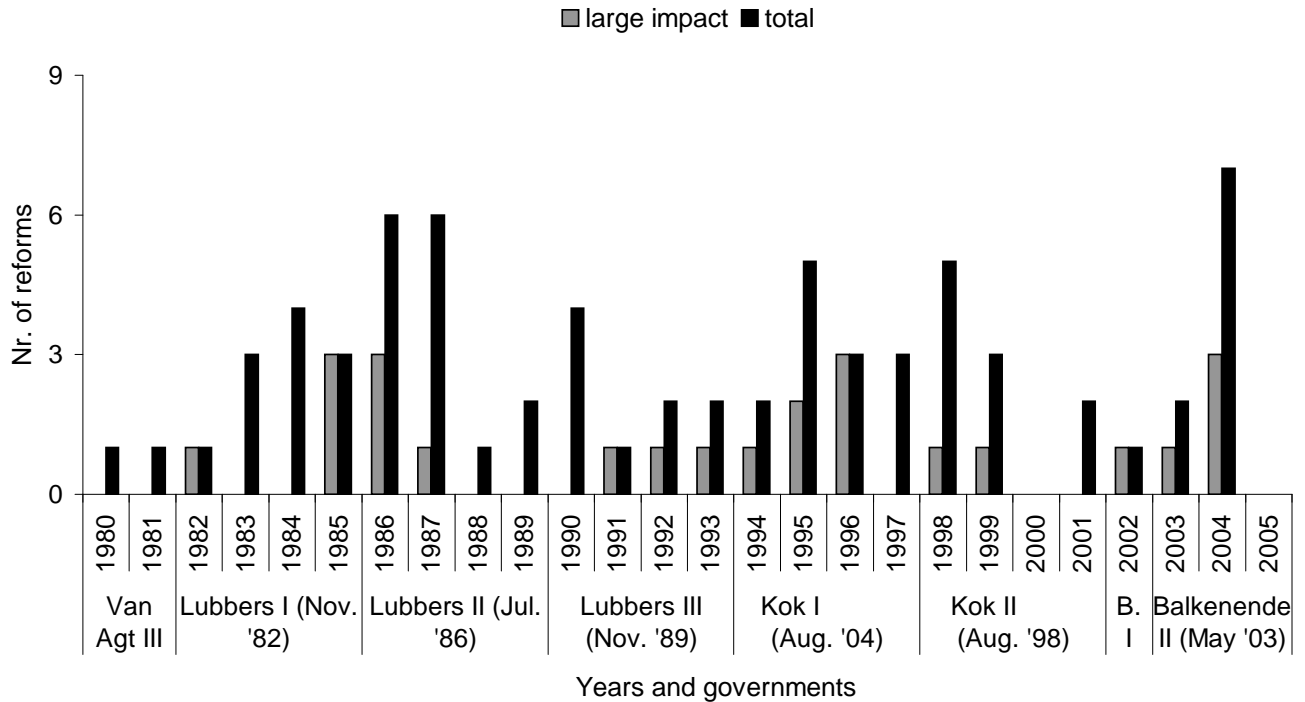
<sup>6</sup>The total of unemployment, disability, sickness, EPL and ALMP reforms.

<sup>7</sup>The number of reforms is taken from the data of Table 1 below, so counting the reforms by area.

<sup>8</sup>There was also a fifth reform wave in 1982/1983 when the Wassenaar agreement was signed. Compared to the other waves this wave is not as apparent from Figure 2 since the Wassenaar agreement is counted as one reform, while actually consisting of numerous reforms.

<sup>9</sup>Incidentally a reform was targeted at more than one area and we chose to include those in our reforms list in all of the relevant areas.

**Figure 2:** Number of reforms by impact, years and government



Source: Table 1

Wassenaar agreement is commonly regarded as one of the earliest manifestations of the Poldermodel, which is at the basis of the period of substantial job growth in the 1990s often referred to as the Dutch Miracle (Visser and Hemerijck (1997)). Another example is the 1991 PES (Public Employment Services) law which introduces the obligation for the PES to inform the institutions in charge of unemployment benefits of insufficient job search efforts by benefit claimants and increased sanctions for refusing a suitable job offer. Abbring et al. (2005) find that, using 1992-1993 data, “*re-employment rates are significantly and substantially raised by imposition of a sanction.*”

For disability reforms we observe that except for a few insufficiently substantial reforms in the early 1980s (that mainly involved changes in the taxation aspects of disability payments), and a two-stage reduction in disability replacement rates in 1985/1986, the focal period has been 1992 to present. An example of a large impact reform was the 1992 Act on Reducing Disability Volume (TAV), which entailed some measures such as a bonus-malus system and wage subsidies to reduce the number of disabled. An employer could be eligible for a bonus and subsidy for hiring a disabled worker, whereas he could be liable for a fine (malus) if one of his employees would become disabled. By estimating a model for changes in WAO inflow, Stegeman (2005) found a significant negative effect of TAV on WAO inflow. Another example is the 1993 Act on Reducing Disability Claims (TBA) that consisted of several measures amongst which were a new definition of disability and a corresponding reassessment of a large number of current WAO claimants. Several authors such as van Oorschot and Boos (2001)

**Table 1:** Reform areas and reform impact.

	Large impact					Total, including insufficiently substantial				
	sickness	disability	almp	epl	unemployment	sickness	disability	almp	epl	unemployment
1980							1			
1981								1		
1982					1					1
1983								1	1	1
1984							1	1		2
1985	1	1			1	1	1			1
1986	1	1			1	1	1	3		1
1987					1		1	3	1	1
1988								1		
1989								2		
1990						1		3		
1991					1					1
1992		1					1	1		
1993		1					1	1		
1994	1					1		1		
1995		1	1				2	2	1	
1996	1		1		1	1		1		1
1997								2		1
1998		1					2	2	1	
1999				1			1		2	
2000										
2001									1	1
2002		1					1			
2003					1				1	1
2004	1	1			1	1	3	1		2

Source: Annex 1 in Gerards et al. (2008)

and Aarts et al. (2002) point to a significant positive effect of the TBA.

Sickness reforms and their timing are closely related to their disability siblings, although the number of pure sickness reforms is notably smaller. Reforms that seem to have had a large impact include the 1985/1986 reduction in sickness replacement rate from 80% to 70% and the 1994 TZ/Arbo. An important element of this last reform was the obligation for employers to continue a sick employee's wage payment during the first weeks of sickness. Van Oorschot and Boos (2001) conclude that <sup>10</sup> “*The privatization of the first weeks of sickness benefit had an immediate and significant effect*”.

Active labor market policies and employment protection legislation have been popular areas of reform predominantly from the late 1980s onwards, albeit with very limited verified success. For instance, De Koning et al. (2003) state with respect to ALMP reforms: “*There is little we know about the effectiveness of the measures. Evidently, evaluation is more complex when a large number of institutions are involved.*” The so called ‘Melkert jobs’ of the mid 1990s which provided subsidized jobs for long term unemployed that would otherwise have a very low chance of obtaining work have been among one of the more successful ALMP policies. Jansen (2001) finds that of those participants

<sup>10</sup>Based on data published by CTSV (a social security supervisory board).



that have completed the program 73% eventually find regular paid employment. With regard to EPL the 1999 ‘Flexwet’ (Flexibility and Security Law) is worth mentioning. This law attempts to introduce more flexibility in working contracts while giving more protection to workers on such contracts. The OECD Economic survey for the Netherlands (2000) refers to mixed experiences with the Flexwet amongst both employers and employees.

Another method of looking at the reforms of Table 1 is by classifying them according to reform instrument. This is elaborated in Table 2 where we classified the reforms into five instrument categories.<sup>11</sup> Again we also make the distinction between large impact reforms and total reforms. With respect to the nature of the instruments, *level* reforms change real money levels of income from benefits. An obvious example of a level reform would be a reduction in replacement rate. *Responsibility* refers to reforms that shift responsibilities from government to private sector or change responsibilities between employers and employees.<sup>12</sup> An example is the 1996 WULBZ which states that employers became responsible for continuing wage payment the first few weeks an employee became ill. *Access* reforms change which specific groups of people are eligible for a certain benefit scheme. For example part of a 1983 unemployment reform was that 16 and 17 year olds would no longer have access to unemployment benefits. *Duration* reforms change the length of benefit entitlement and *stimuli* refers to reforms that provide financial or other incentives to employers and/or employees to seek out, create or accept work.

Most obvious from Table 2 is the concentration of level reforms in the early to mid 1980s. Among the ones with a large impact are the two-stage cuts in replacement rates from 80% to 70% in 1985/1986. Combined with a number of access and duration reforms, these level reforms aimed to reduce the costs of social security. Overall the table shows extensive reform activity in 1987 which was the already mentioned system wide reform that affected all areas of social security. Although this was a system wide reform, we will not elaborate further on its specific details. In part because the expected effects of the reform were not observed in the immediate years thereafter (for example Aarts et al. (2002)).

In the first half of the 1990s several reforms were implemented to reduce the volume of benefit claimants which include the abovementioned TAV, TBA and TZ/Arbo. These reforms combined stimuli (e.g. the bonus/malus aspect of the TAV) with access aspects (e.g. aforementioned reassessment of WAO claimants under TBA). The second half of the 1990s is characterized strongly by shifts in respon-

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<sup>11</sup>Some reforms contain multiple instruments and are therefore counted in all instrument categories. This explains for example why in Table 1 in 1987 there is only one reform of significant impact but in Table 2 there are three. This particular unemployment reform thus consisted of a combination of three instruments.

<sup>12</sup>Responsibility reforms mostly entail reforms that change who bears a certain cost or which actor is the first to take a certain action. Responsibility reforms do not refer to reforms that only change the administrative nature or bureaucratic characteristics of policy.

**Table 2:** Reform instruments and reform impact.

	Large impact					Total, including insufficiently substantial				
	level	responsibility	access	duration	stimuli	level	responsibility	access	duration	stimuli
1980						1				
1981										1
1982		1					1			
1983						1		2	1	1
1984						2		1		1
1985	1					1				
1986	1					1				4
1987	1		1	1		2	1	1	1	3
1988										1
1989										2
1990							1			3
1991		1			1		1			1
1992					1					2
1993			1					1		1
1994		1					1			1
1995					2		2			4
1996		1	1		2		1	1		2
1997							1			2
1998		1	1				2	1		3
1999		1			1		2	1		1
2000										
2001							1			1
2002		1					1			
2003					1		1			1
2004		1	1		1	1	2	1		3

Source: Annex 1 in Gerards et al (2008).

sibilities, where for example the WULBZ signaled the first stage in privatizing the sickness insurance scheme by making employers responsible for the continued wage payments in the first few weeks of sickness of an employee. This privatization and corresponding shift in responsibility from government to market was continued in the new millennium with the full abandonment of the sickness insurance scheme, being replaced by a privatized version in which the employer is since 2004 responsible for paying up to the first 2 years of sickness. Also dominant in the late 1990s (but not a specific topic in this paper) were the changes in the governing and administrative structure of the social security system, aimed at increasing the overall system efficiency. A final interesting observation from Table 2 is that pure duration reforms that change the length of benefit entitlements have sparsely been used.

Combining some of the insights from Figure 2 and Tables 1 and 2 leads to the observation that level reforms were used in the early 1980s to reduce the high costs associated with the large numbers of WW, WAO and ZW claimants. The late 1980 were relatively quiet in terms of reforms, likely a waiting period in anticipation of the first results of the extensive 1987 system wide reform. Especially with regard to WAO and ZW these results did not show and as a consequence these became hot reform

areas and the 1990s became riddled first with volume reforms targeted at WAO and ZW and after those with reforms shifting responsibilities through privatization and reorganization of administrative bodies. Unemployment has been under careful attention from policymakers continuously resulting in a number of reforms with considerable impact. In the 1990s when unemployment numbers were going down steadily and there were less specific targeted unemployment (WW) reforms, a shift in instrumentation to ALMP and EPL, and therewith from passive to active policies, can be observed. ALMP and EPL have tried to supplement the more specific targeted unemployment, disability and sickness reforms but their significance is difficult to establish. After reaching the lowest point in unemployment since decades in 2001 unemployment started to increase again. This led to a renewed attention to specific targeted unemployment reforms, with reforms implemented in 2001, 2003 and 2004.

An important conclusion from both Figure 2 and Tables 1 and 2 is that the overall amount of reforms that had a considerable impact is substantially smaller than the overall total amount of reforms. This is illustrated in Table 3 below which depicts the ratios of large impact reforms to total reforms. EPL and ALMP seem to be policy areas in which most unfruitful reforms are conducted<sup>13</sup>

**Table 3:** Reform impact ratios by area and instrument.

<b>Reform impact ratios by area</b>					
	sickness	disability	almp	epl	unemployment
ratio	5/6	8/16	2/26	1/8	8/14
%	83%	50%	8%	13%	57%

<b>Reform impact ratios by instrument</b>					
	level	responsibility	access	duration	stimuli
ratio	3/9	8/18	5/9	1/2	9/38
%	33%	44%	56%	50%	24%

Source: Tables 1 and 2

whereas the relatively few sickness reforms are considered to have been quite effective. This result is confirmed by the low score for stimuli instruments, of which a large number of ALMP reforms consist. Responsibility does not score very high either. This is in line with research by TNS-NIPO (2003) who find for example that when asked who is primarily responsible for preventing sickness and disability, actors tend to shift responsibilities to other actors. The study finds that employers think that employees are primarily responsible, while employees (whether healthy, sick or disabled) think that

<sup>13</sup>Measurement problems in evaluation studies of EPL and ALMP reforms and the relative lack of evaluation studies might bias this outcome, although the difference is still striking.

employers are primarily responsible). Level reforms also score low. This is not unthinkable given that quite some reforms that attempt to change the benefit level of the social security system are countered by collective bargaining agreements. See for example Hartog (1999) who writes that: “... *legislated changes in the mandatory components are often countered by adjustments in collective bargaining (e.g. supplementing legal benefit entitlements)*...”. These conclusions about stimuli, responsibility and level reforms are supportive to the notion that only reform policies restricting access and duration did achieve a large impact.

## 4 Econometric evidence

To support our findings from section 3 we want to test whether the reforms we found to be of considerable impact based on our formal ranking scheme also turn out to be significant using econometric analysis. To that end we estimate the effect of reforms in the areas of unemployment, sickness and disability.<sup>14</sup> In section 3 we identified the policy reforms that were of considerable impact and which will be the dummy variables in our regression analysis. In order to avoid spurious regressions we use a two-stage error correction model (Engle and Granger (2001)).<sup>15</sup>

The first stage consists of the estimation of a long run equation using macroeconomic data on the relevant reform areas and a number of covariates and reform dummies that are relevant in long run analysis. There we estimate:

$$\gamma_t = \alpha + \beta c_t + \delta d_t + \epsilon_t \quad (1)$$

where  $\gamma_t$  stands for either the unemployment rate, the sickness absence rate, or the disability rate (all of them as percentage of the labor force),  $c_t$  is a vector of covariates, and  $d_t$  is a vector of policy dummies. The second stage of the error correction model identifies the sensitivity of the variables to the business cycle and determines the adjustment speed to the long run. Thus we estimate in the second stage:

$$\Delta\gamma_t = \alpha + \beta\Delta c_t + \delta\Delta d_t + \psi\Delta\hat{\epsilon}_{t-1} + \mu_t \quad (2)$$

where  $\hat{\epsilon}$  represents the estimated error of the long run equation.<sup>16</sup> The data used in the regressions comes mainly from Statistics Netherlands (CBS) or the CPB. The starting and ending times of the dummies (representing the reforms) come from the Dutch reform database that we constructed, as discussed in section 2. Using dummy variables the effect of reforms that were implemented in

<sup>14</sup>The areas EPL and ALMP are left out because of data limitations. Moreover, the majority of EPL and ALMP reforms were found to be insufficiently substantial in our ranking - and the relevant literature.

<sup>15</sup>Using a similar procedure Stegeman (2005) shows that there is a positive relation between economic upswings and sickness leave in the Netherlands in the period 1980-2003.

<sup>16</sup>Thus the first stage looks at ‘levels’ ( $\gamma_t, \beta c_t$ , etc) while the second stage looks at ‘changes’ ( $\Delta\gamma_t, \beta\Delta c_t$ , etc).

**Table 4:** Estimation results for unemployment - 1980 to 2006.

Variables	levels (long run)	changes (short run)	changes (short run)	changes (short run)
Dependent variable	SR unempl. rate	SR unempl. rate	SR unempl. rate	SR unempl. rate
PA Ratio	0.644***	1.186***	0.795***	0.795***
Exemption 57+ and Wassenaar	-2.751***	-1.960***	-1.033*	-1.033***
1987 reform	-1.311 **	0.864	-0.015	-0.015
PES reform	-1.731 **	-1.457 **	-0.881	-0.881 **
nABW	-4.459***	-0.615	-0.850	-0.850
Bonus for employment	-0.860	-0.622	-0.650	-0.650***
Red. of benefit duration	-2.576*	-1.211	-0.034	-0.034
Flexwet	-1.367*	-0.690	-0.157	-0.157
Business cycle indicator			-0.483***	-0.483***
$R^2$	0.846	0.709	0.841	0.841
N	27	27	27	27
Durbin Watson D statistic	1.843			
Breusch Godfrey P-Value	0.923	0.577	0.334	-HR-
Heteroskedasticity test	0.035	0.114	0.011	-HR-
Ramsey Reset P-value	0.345	0.104	0.706	0.706
Estimation number	1	1.1	1.2	1.3

\*\*\* = significant at 1%, \*\* = significant at 5%, \* = significant at 10%

subsequent years is difficult to measure, as it is hard to distinguish between the impacts of dummies that are close to each other in time. Thus in order to reduce the density of dummies in the sample some dummies represent multiple reforms. For example the ‘Exemption 57+’ dummy is also measuring the effects of the Wassenaar agreement and the ‘1987 reform’ dummy also includes effects of the reductions in unemployment benefits in 1985 and 1986.<sup>17</sup>

We present the estimation results for unemployment in Table 4.<sup>18,19</sup> We focus here on short-term unemployment because it reacts faster to reforms and variations in the business cycle than long term or total unemployment. The first column presents the long run (first stage) estimation results. Columns 2, 3 and 4 present the short run (second stage) estimations. Although we chose to report only those estimations that give the strongest evidence for our case, our results appear to be robust over a number of specifications. In both the long run and the short run estimations the coefficients have the expected sign. In the long run estimation all variables are significant, except for the Bonus for employment. This makes sense as this bonus was only applied in one year (2003) and as the first stage regression

<sup>17</sup>Likewise, the ‘1998 package’ in the disability estimation corresponds to the introduction of both PEMBA and REA; the ‘2004 package’ represents the introduction of the WSW, the reform of the WAO and the introduction of the WIA.

<sup>18</sup>The last row of our tables contains ‘estimation numbers’ which we refer to in the text to indicate which column/estimation we are discussing.

<sup>19</sup>Regression 1.3 was generated using a heteroskedasticity-robust estimator. This is why we can not provide the Breusch-Godfrey P-value. As the estimator is heteroskedasticity-robust, we are also unable to report a specific value for the heteroskedasticity test.

is mainly estimating the long run impact of the variables. In the short run estimations the PA ratio (the share of part time work in full-time employment) and the ‘exemption 57+’/Wassenaar reforms are consistently significant over all estimations, whereas the 1987 reform, nABW and the reduction of benefit duration always turn out to be insignificant.

In regression 1.2 we introduce a business cycle indicator (the business cycle indicator as published by the CPB). In the regressions on sickness and disability reported in Tables 5 and 6 below, the number of vacancies will be used as proxy for the business cycle. By using the CPB business cycle indicator in the unemployment regressions, we tried to get around possible endogeneity problems which we could have if we would use the number of vacancies when estimating effects on unemployment.

It is striking that the introduction of the business cycle indicator in estimation 1.2 reduces the strength of almost all other variables in the model. However, this estimation suffers from heteroskedasticity. If we introduce heteroskedasticity-robust standard errors, see estimation 1.3, the business cycle indicator becomes somewhat less significant and the PES reform as well as the bonus for employment gain in significance. Such a strong reaction to a business cycle indicator suggests that unemployment is, at least in the short term and in this framework, heavily dictated by developments in the business cycle. The negative coefficient for the business cycle indicator indicates that the relation to short term unemployment is countercyclical.

The results from these long run and short run estimations confirm the results of the qualitative analysis in section 3 of this paper, where the PES reform and the Wassenaar agreement were identified as some of the reforms which had a considerable impact on unemployment. The mixed experiences with the Flexwet which were reported in section 3 are also consistent with a (weakly) significant coefficient in our long run estimation.

In Table 5 we present our estimation results for the sickness absence rate. In the long run estimation (estimation number 2) four of the nine variables are significant. At first glance this might seem a meager result, but one should take into account the following points. The introduction of the TBA coincided with a break in the sickness data series (Stegeman (2005)), the WVP came into effect in 2002 and thus is only observed for four years (hardly long run) and the main consequence of the TZ/Arbo was that employers would now be responsible for continuing an employee’s wage payment during the first weeks of sickness (thus a predominantly short run reform). In the short run (estimation 2.1) all variables except the replacement rate and WULBZ turn out to be significant. This points towards a low influence of the replacement rate on sickness in the short term. However, in this estimation the Durbin-Watson D-statistic is too low, which indicates non-stationarity in the residuals - see also the very low Breusch Godfrey P-value. With the introduction of the business cycle indicator this problem

**Table 5:** Estimation results for sickness - 1980 to 2006.

Variables	levels (long run)	changes (short run)	levels (short run)
Dependent variable	Sickness rate	Sickness rate	Sickness rate
PA Ratio	-0.402***	-0.244 * *	-0.108
Female participation	6.737	21.385 * *	16.617 * *
TZ/Arbo	-1.440	-1.372***	-1.692***
TAV	-0.972*	-0.898 * *	-0.325
WULBZ	0.036	-0.351	-0.288
Replacement rate	10.141 * *	1.594	5.726*
TBA	1.413	1.688 * *	2.106***
WVP	0.404	0.734 * *	1.057***
Extension sickness payments	-0.773***	-0.690 * *	-0.635***
Indicator for vacancies			0.577***
$R^2$	0.963	0.778	0.884
N	27	27	27
Durbin Watson D statistic	1.368		
Breusch Godfrey P-Value	0.144	0.002	0.563
Heteroskedasticity test	0.627	0.498	0.826
Ramsey Reset P-value	0.057	0.945	0.171
Estimation number	2	2.1	2.2

\*\*\* = significant at 1%, \*\* = significant at 5%, \* = significant at 10%

disappears as can be seen in the results of estimation 2.2. At the same time the explanatory power ( $R^2$  and adjusted  $R^2$ ) (the latter not reported in the table) of the short term model rises substantially. In addition, the replacement rate coefficient rises both in level and significance. The former indicates that, as also observed in Table 5 for unemployment, the developments in the sickness absence rate are influenced to a substantial degree by the developments in the business cycle, though this time the positive coefficient points to a pro-cyclical relation. Still, especially compared to the short run estimation results for unemployment in Table 5, estimation 2.2 shows that sickness is more susceptible to reforms in the short run.<sup>20</sup>

Comparing the results from these estimations with our qualitative results for sickness in the third section of this paper, we find substantiation for the considerable impact we found earlier for TZ/Arbo and the replacement rate reductions.

Table 6 presents the estimation results for the disability rate. In the long run estimation all variables with the exception of the 1998 reform package turn out to be significant. In the short run estimation the TBA/TAV coefficient becomes insignificant whereas the 1998 reform package turns out to be highly significant and positive, although very small. The introduction of the vacancies indicator as proxy for the business cycle mainly affects the AMBER and the TBA/TAV coefficients,

<sup>20</sup>The results of our sickness analysis go into the same direction as the one's of Stegeman (2005), the main difference being that our estimation of the effect of the sickness replacement rate is much bigger than his estimations.

**Table 6:** Estimation results for disability - 1980 to 2006.

Variables	levels (long run)	changes (short run)	changes (short run)
Dependent variable	Disability rate	Disability rate	Disability rate
Female participation	-7.257 * *	-23.933***	-23.473***
PA Ratio	0.514***	0.435***	0.536***
TBA/TAV	-1.652***	-0.546	-0.266
AMBER	-1.284***	-1.899***	-2.290***
1998 package	0.342	0.450***	0.404*
Poortwachter	-0.848***	-0.806***	-0.821***
2004 package	-1.251***	-1.307***	-1.508***
Indicator for vacancies			0.302*
$R^2$	0.918	0.862	0.889
N	27	27	27
Durbin Watson D statistic	1.788		
Breusch Godfrey P-Value	0.586	0.106	0.125
Heteroskedasticity test	0.218	0.182	0.314
Ramsey Reset P-value	0.316	0.904	0.478
Estimation number	3	3.1	3.2

\*\*\* = significant at 1%, \*\* = significant at 5%, \* = significant at 10%

rendering the TBA/TAV coefficient weaker and the AMBER coefficient stronger than in the previous estimation. As the  $R^2$  has benefited from the introduction of the business cycle proxy, estimation 3.2 seems to perform better than estimation 3.1. The positive coefficient of the vacancies indicator points to a pro-cyclical relation with the disability rate. The qualitative result we found in section 3 of a considerable impact of the TBA and TAV is confirmed by our long run estimation.

The results of the econometric analysis in this section confirm that reforms that were determined to be of large impact in section 3 indeed have been relevant. In addition this section shows strong empirical evidence for the role of the business cycle in short run adjustments in unemployment, sickness and disability. The former responds countercyclical and the latter two respond pro-cyclical. A factor we did not take into account is the possibility of spill-over effects and interactions between unemployment, sickness and disability and their respective reforms. Although the pro-cyclical nature of sickness and disability paired with the countercyclical nature of unemployment hints at the existence of these spill-overs, further research will be needed in this area.



## 5 Do OECD indicators correctly reflect our findings?

As mentioned in our introduction we compare the findings of our research as presented in the sections above with the development in OECD indicators for the Netherlands on institutional changes. We analyze this by reform area, thus making the same distinction as in Table 1. However, the reform areas distinguished by the OECD are different from those in Table 1. The OECD distinguishes ALMP, EPL and unemployment benefit replacement rates.

Table 7 shows OECD data with respect to EPL. The OECD numbers range between zero and six where a higher value represents stricter employment protection. There only exist data entries for three years. The table also presents an alternative indicator developed by Allard (2005) which also increases when EPL is stricter.

**Table 7:** OECD EPL indicators and Allard's EPL indicator

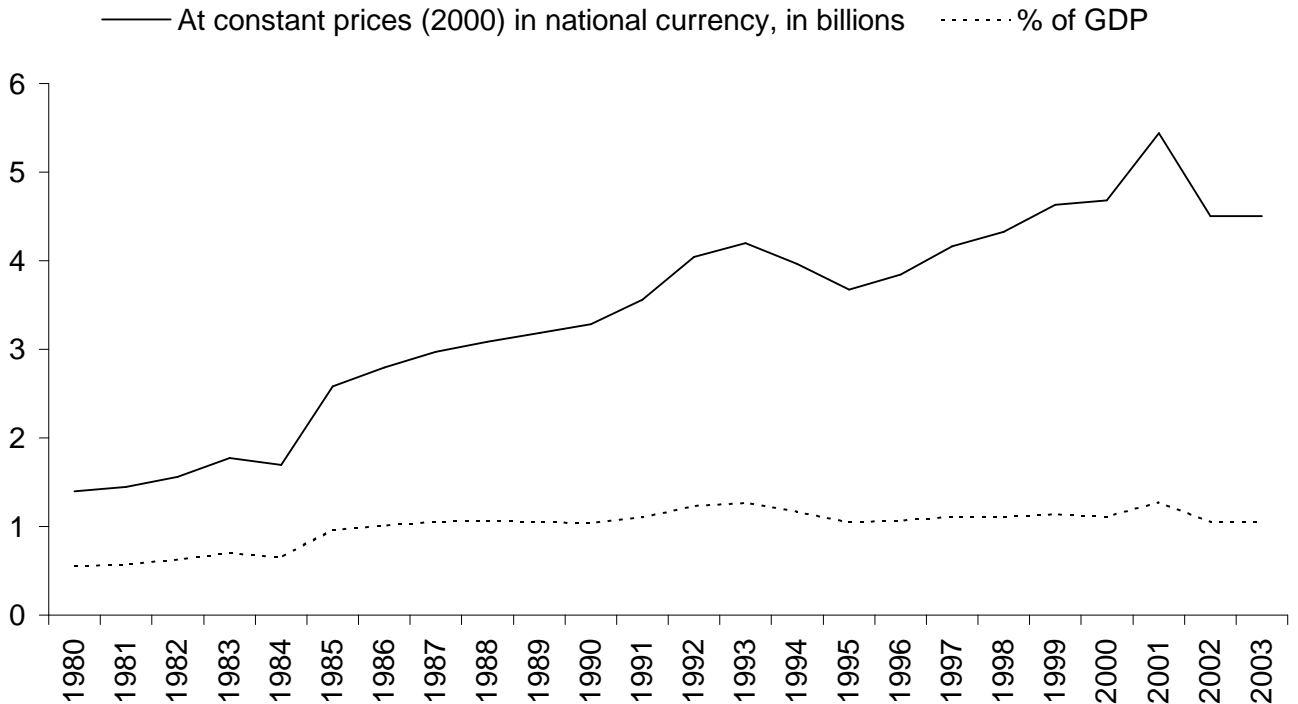
	OECD *				Allard 2005
	Overall strictness of protection against (individual) dismissals in regular jobs	Overall strictness of regulation on temporary employment	Overall EPL strictness version 1	Overall EPL strictness version 2	EPL score
<b>1980-1982</b>					2.2
<b>1983-1987</b>					2.6
<b>1988</b>					2.1
<b>1989</b>					2.1
<b>1990</b>	3.08	2.38	2.73		2.1
<b>1991-1994</b>					2.1
<b>1995</b>					2.3
<b>1996</b>					2.1
<b>1997</b>					2.1
<b>1998</b>	3.05	1.19	2.12	2.27	2.4
<b>1999-2002</b>					2.4
<b>2003</b>	3.05	1.19	2.12	2.27	2.4

\* Retrieved from OECD statistics website 5 Nov. 2007

Our research only shows one EPL reform that was of considerable impact, i.e. the Flexwet of 1999. This law provided more flexibility (variation) in working contracts and more security for temporary employees. This considerable change in EPL warrants in our opinion an upward change in overall EPL indicators and in EPL indicators that focus on temporary employment. However none of the OECD indicators show any change between 1998 and 2003. The only change that occurs is a drop in indicator values between the 1990 and the 1998 data entries. These drops in indicator values imply that EPL has become less strict between these points in time. However we found only two minor changes in EPL between 1990 and 1998 that do not justify the drastic changes that the OECD indicators show, especially not the huge drop from 2,38 to 1,19 in the second column of Table 7. Apparently the OECD

indicators fail to pick up changes introduced by the Flexwet and on top of that they display a large drop between 1990 and 1998 which we cannot justify on the basis of our research. The doubt this sheds on the accuracy of the OECD indicators is reinforced by recent promising work on EPL indicators by Allard (2005), which resulted in an indicator that shows less unexplainable volatility.

**Figure 3:** Total public spending on Active labour market programs



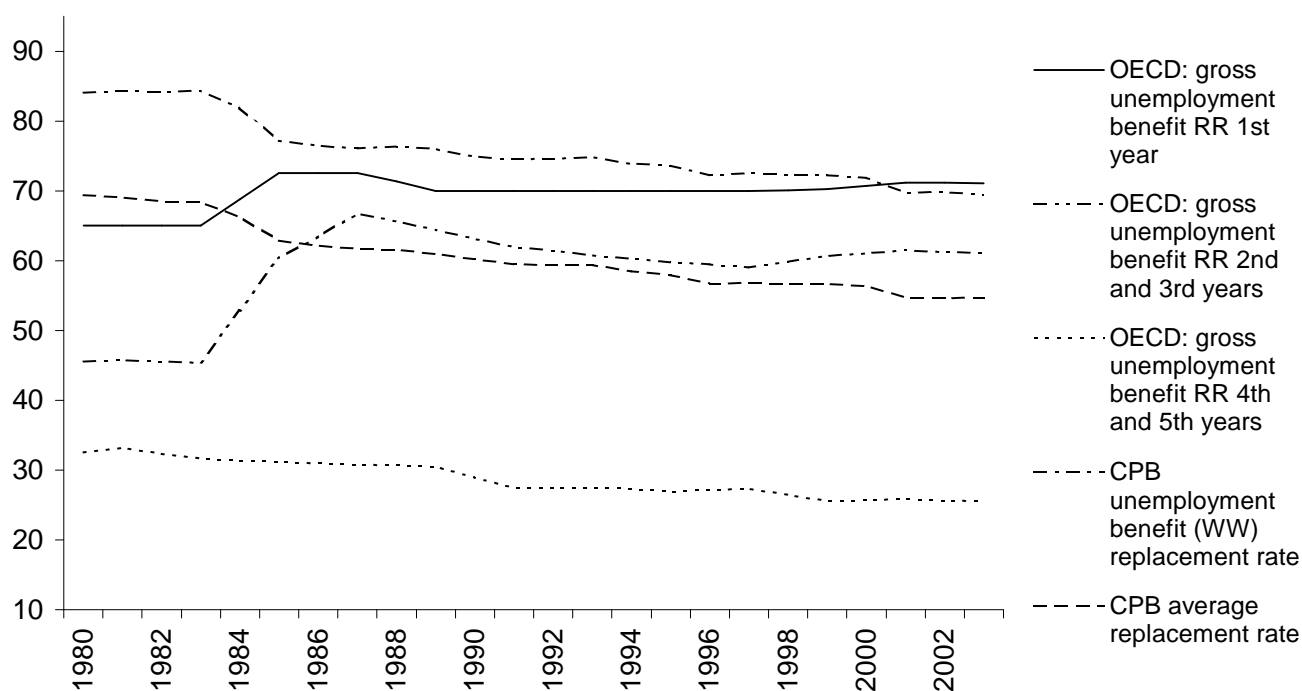
Source: Based on data from the OECD Social Expenditures Database

Figure 3 shows the OECD definition of ‘total public spending on active labor market programs’ at constant prices and as percentage of GDP. It makes sense to compare this OECD indicator to the right hand side of Table 1 which includes all ALMP reforms, also the ones that turned out to be insufficiently substantial. The reason is that whatever their impact has been, any new ALMP reform usually implies that more money is spent because ALMP most often involves subsidies and other financial incentives.

Both lines in Figure 3 show an upward trend from 1980 to 1993 with a jump in 1985/1986. The trend seems to be consistent with our findings from Table 1, but the jump is difficult to justify on the basis of our findings. There is a concentration of ALMP reforms in 1986 but this is one year after the jump. The continuously high density of ALMP reforms in the late 1980s and early 1990s explains the upward trend that follows the intriguing 1986 jump. The years 1993 to 1995 are relatively quiet in terms of ALMP reforms and the OECD indicator seems to pick this up as it shows decreasing spending on ALMP in those years. Implementation of the ‘Melkert 2’ and ‘Melkert 3’ subsidized job

programs in 1995 signals the beginning of a new period of high reform density that lasts until 1998. The OECD indicator shows a new upward trend as well from 1995 on and thus seems to reflect the actual changes in this period. A peak in the OECD indicator is reached in 2001 after which it declines again. Although this peak is preceded again by a jump that is difficult to explain on the basis of our research, the decline that follows looks consistent with the absence of ALMP reforms between 1999 and 2004. The OECD indicator thus picks up trends in ALMP reforms adequately, except for the two jumps in 1985/86 and 2001 in the upper indicator which is in terms of money spent. The jumps are however not so sharp when expressed in percentage of GDP so they may represent short periods of increased expenditure on certain existing ALMP programs.

**Figure 4:** OECD Unemployment replacement rate indicators



Source: Based on OECD and CPB data

Moving on to the next area of reform, Figure 4 shows three OECD indicators for unemployment benefit replacement rates and two indicators based on data of the CPB (Netherlands Bureau for Economic Policy Analysis). Logically the replacement rate becomes lower the longer an unemployment episode lasts and thus the OECD lines showing the replacement rate in the second and third year and fourth and fifth year are on subsequently lower levels than the OECD line that shows the replacement rate in the first year. To assess the validity of these OECD replacement rate indicators an in depth look at our reforms tables presented earlier in this paper is barely required. It is a well known fact that starting with the Wassenaar agreement in 1982 spending cuts have been prevailing and the increase the upper

two OECD indicators show are certainly contrary to what you would find if you'd have asked any Dutch unemployed person or family at that time. Official replacement rate reductions followed in 1985 and 1986 but the upper two OECD indicators do not start their decline until 1988. In part the official reductions in replacement rates were compensated by collective labor agreements but this still does not justify how the OECD indicator for the second and third year replacement rate can show an increase. The two CPB indicators however do correctly reflect all these reforms and hence they show the expected downward trend. The lowest OECD indicator (for the replacement rate in the fourth and fifth year of an unemployment episode) declines constantly, therefore reflecting at least somewhat better the true changes over time. This however cannot compensate for the very questionable performance of the other two indicators which do not convey accurately the true developments.

The last two reform areas, disability and sickness, are quite specific to the Netherlands and are hardly discussed in the OECD literature. Therefore we could not find OECD indicators for disability and sickness that we can test against the results we presented above in our qualitative analysis. In conclusion this section of the paper shows that OECD indicators on EPL and unemployment benefit replacement rates fail to reflect important reforms and even deviate from the actual trends in these reforms. The OECD indicator on ALMP performs reasonably well, although this was more or less expected due to the definition of the indicator.

## 6 Conclusion

In this paper we discuss the past 25 years of Dutch reform history in the areas of unemployment, disability, sickness, employment protection legislation and active labor market policy. We start an extensive literature survey to determine the impact of the reforms on their respective policy areas. As expected we find that not all reforms are important to the same extent. Most notably the reforms in EPL and ALMP seem to have been relatively unsuccessful compared to the reforms in sickness that were fewer in number but considerable in impact. In unemployment and disability there have also been a number of reforms with large impact. We also conduct an econometric analysis, to corroborate our findings. Most of the important reforms we identified earlier turned out to be significant in most of our estimations.

Over time policymakers have also exhibited changing preferences for certain types of reform. The early to mid 1980s were the prime era of spending cuts and in an attempt to reduce the high costs associated with the large number of social security claimants (as seen in Figure 1 in the introduction) this period witnessed a concentration of reforms that lowered benefit levels. Volume policies aimed directly at reducing the number of claimants characterized the early 1990s, whereas the later 1990s

saw an increase in reforms that shifted responsibilities through privatization and reorganization of administrative structures. In addition, we showed that the political business cycle also left a mark on the reform history, generally showing higher reform activity in the first two years of new governments and lower reform activity in the years leading up to elections.

Finally, we compare the results of our analysis with the OECD indicators on welfare reforms with respect to EPL, ALMP and unemployment benefit replacement rates. We concluded that these OECD indicators generally fail to reflect important reforms and sometimes even deviate from the actual trend direction. Only the OECD indicator on ALMP performs reasonably well, although this was more or less expected due to the definition of the indicator. Finally the results of this paper emphasize once more how important disability and sickness reforms are in Dutch reform history. These two areas are not covered by the OECD indicators thus when studying OECD indicators one should realize that these do not take notice of these two very important reform areas.

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## 7 Annex I: The ranking scheme table

	Mentioned	
	yes	no
Brandt et al (2005)	0	-1
FRDB (2000-2006)	0	-1
LABREF (1994-2004)	0	-1
FRDB calls the reform structural (1987-2005)	+1	0
OECD country surveys (1979-2007)	+1	0
General literature review	+1	0
Evaluation literature/OECD Theoretical considerations	} -2 to +2	