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WAGE DYNAMICS NETWORK

WORKING PAPER SERIES

NO 1269 / DECEMBER 2010

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DOWNWARD WAGE RIGIDITY AND AUTOMATIC WAGE INDEXATION

EVIDENCE FROM MONTHLY MICRO WAGE DATA

by Patrick Lünnemann
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In 2010 all ECB publications feature a motif taken from the €500 banknote.

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Wage Dynamics Network

This paper contains research conducted within the Wage Dynamics Network (WDN). The WDN is a research network consisting of economists from the European Central Bank (ECB) and the national central banks (NCBs) of the EU countries. The WDN aims at studying in depth the features and sources of wage and labour cost dynamics and their implications for monetary policy. The specific objectives of the network are: i) identifying the sources and features of wage and labour cost dynamics that are most relevant for monetary policy and ii) clarifying the relationship between wages, labour costs and prices both at the firm and macro-economic level.

The WDN is chaired by Frank Smets (ECB). Giuseppe Bertola (Università di Torino) and Julián Messina (World Bank and University of Girona) act as external consultants and Ana Lamo (ECB) as Secretary.

The refereeing process of this paper has been co-ordinated by a team composed of Gabriel Fagan (ECB, chairperson), Philip Vermeulen (ECB), Giuseppe Bertola, Julián Messina, Jan Babecký (CNB), Hervé Le Bihan (Banque de France) and Thomas Mathä (Banque centrale du Luxembourg).

The paper is released in order to make the results of WDN research generally available, in preliminary form, to encourage comments and suggestions prior to final publication. The views expressed in the paper are the author's own and do not necessarily reflect those of the ESCB.

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CONTENTS

Abstract

This paper assesses the degree of downward wage rigidity in Luxembourg using an administrative monthly data set on individual wages covering the entire economy over the period from January 2001 to January 2007. After limiting for measurement error, which would otherwise bias downwards the estimates of wage rigidity, we conclude that nearly all workers in Luxembourg are potentially subject to downward real wage rigidity. Our results are robust to different procedures to adjust for measurement error and methods for estimation of downward wage rigidity. We report relatively small differences in the frequency of nominal wage cuts across occupational groups and sectors. In addition, the observed rigidity does not seem to be driven predominantly by the absence of negative shocks. We show that the *real* wage rigidity is related to the automatic wage indexation, while additional factors might be necessary to explain the high degree of *downward* wage rigidity.

JEL Code: J31.
Keywords: downward wage rigidity, wage indexation.

Non-technical summary

Wage rigidity captures growing interest of both policymakers and economists. On theoretical grounds, it causes unemployment by limiting the adjustment of wages and is often put forward as one of the explanations of high and persistent unemployment in Europe when compared to the US (Grubb et al. (1983)). Wage rigidity has also important implications for the design and effectiveness of monetary policy. Since moderate inflation allows for reductions in real wages, the optimal level of inflation is higher when nominal wages are rigid, all else equal (Akerlof et al. (1996)).

While economic theory is unambiguous with regard to the role of wage rigidities, an empirical assessment of their strength remains an open question. Macroeconomic studies typically estimate the sensitivity of wages to shocks or changes in unemployment and/or productivity. The microeconomic line of research rests on the idea that wage rigidity causes asymmetries in the wage change distribution. For instance a spike in the wage change distribution at zero (close to the expected inflation) is considered as an indicator of downward nominal (real) wage rigidity.

Empirical estimates of the degree of downward wage rigidity vary not only across countries and approaches but also between different datasets referring to the same country (such as employment registers, household or employer surveys). First, labour market institutions were put forward as a potential source of wage rigidity. For instance Holden and Wulfsberg (2009) show that the frequency of real wage cuts is lower in countries with strict employment protection legislation and high unionization. Second, different estimates of wage rigidity obtained in datasets referring to one country can be due to measurement errors (Gottschalk (2005)). As a result, several approaches to deal with measurement errors were proposed in the literature.

This paper was conducted within the Eurosystem's *Wage Dynamics Network*. It provides estimates of downward nominal and real wage rigidity for Luxembourg. We use an administrative dataset with monthly frequency covering the entire economy over the period from January 2001 to January 2007. While Lünnemann and Wintr (2009) provide evidence on the flexibility of wages in Luxembourg by calculating the frequency of wage change, this paper directly estimates downward wage rigidity in Luxembourg and compares the results to other countries. We adopt two sets of rigidity estimates developed in the framework of the *International Wage Flexibility Project* (IWFP hereafter), see Dickens et al. (2007) and Dickens et al. (2008). We extend the literature in several ways. First, we provide estimates of downward rigidity for Luxembourg, which not only has not participated in the IWFP so far, but also features a number of distinctive institutional labour market characteristics (such as the automatic wage indexation). Second, in order to assess the robustness of our estimates, a cross-check is provided using alternative measures of wage rigidity and approaches to identify measurement error. Third, while the IWFP procedure is typically used to derive estimates of downward rigidity for the aggregate economy (Dickens et al. (2008)) or selected sectors (Messina et al. (2010)), in addition, we investigate whether the degree of downward wage rigidity differs across occupational groups, sectors and firms.

Our main findings can be summarized as follows. First, the dataset is subject to two main types of measurement error that can substantially bias downward the measured degree of downward wage rigidity. While the raw data suggest that nominal wage cuts are not uncommon, we find few nominal wage cuts once adjusting for measurement error.

Second, we find an overall very high degree of downward real wage rigidity. We observe a strong concentration of wage changes in the 2–3% interval (which includes the 2.5% wage increase stipulated by the automatic wage indexation). At the same time, wage changes below 2% are rare. As a consequence, our estimates suggest that nearly all workers in Luxembourg are potentially subject to downward real wage rigidity, a higher level than in any other country participating in the IWFP.

Third, these results are robust to the procedure used to adjust for measurement error and two different methods for estimation of downward wage rigidity that we adopt.

Fourth, we observe only relatively small differences in the frequency of wage cuts across occupational groups and sectors which have to be considered against the low frequency of wage cuts in Luxembourg. In addition, we show that in spite of very different dynamics of employment and wage bill at the company level, which suggests that the observed downward real wage rigidity is due to factors covering essentially the whole economy, such as labour market institutions, and is not driven predominantly by the absence of negative shocks. We show that the automatic indexation has significant implications on the distribution of wage changes and downward *real* wage rigidity.

1 Introduction

Wage rigidity captures growing interest of both policymakers and economists. It causes unemployment by limiting the adjustment of wages and thus inflicts substantial social costs. This is especially relevant in a monetary union in which alternative channels of adjustment might be more costly (De Grauwe (2007)). Wage rigidity was put forward as one of the explanations of high and persistent unemployment in Europe when compared to the US (Grubb et al. (1983)). It has also important implications for the design and effectiveness of monetary policy. The optimal level of inflation is ceteris paribus higher when nominal wages are rigid because moderate inflation allows for reductions in real wages ("grease" effect, see e.g. Akerlof et al. (1996)). Real wage rigidity introduces a trade-off between stabilizing inflation and the output gap in the New Keynesian model and induces persistence in inflation and output gap fluctuations that are observed in the data (Blanchard and Galí (2007)). It was suggested that wage rigidity can be one of the causes of price stickiness, which in turn exacerbates output volatility in response to shocks and requires larger interest rate changes to influence inflation (Altissimo et al. (2006)).

While economic theory is unambiguous with regard to the role of wage rigidities, an empirical assessment of their strength remains an open question. Macroeconomic studies typically estimate the sensitivity of wages to shocks or changes in unemployment and/or productivity. The microeconomic line of research rests on the idea that wage rigidity causes asymmetries in the wage change distribution. Dickens et al. (2008) interpret the presence of spikes at zero wage growth and expected wage increases as indicators of downward nominal and real wage rigidity, respectively, assuming that in the absence of downward wage rigidity the distribution of wage changes is symmetric and that the effects of rigidity can be seen only under the median. Kahn (1997) assumes that in the absence of rigidity, certain properties of the distribution of wage changes remain constant over time. The measure of wage rigidity is based on a comparison of the height of bins at a given distance from the median in years when they refer to wage cuts and in years when rigidity is not binding (wage increases). Another strand of the microeconomic estimates of wage rigidity is based on the assumption that there is a hypothetical distribution of wage changes that would prevail if there was no rigidity (notional distribution). Measures of wage rigidity can then be obtained from the comparison of the notional and observed distribution as in Dickens et al. (2007) or Altonji and Devereux (1999).

Empirical work estimating the degree of downward wage rigidity has provided vast but sometimes inconclusive results so far. The reported degree of downward wage rigidity varies not only across different approaches and countries but also between different datasets referring to the same country (such as employment registers, household or employer surveys). According to Gottschalk (2005), the results can be due to the presence of measurement error. He assumes that the measurement error-free base wage trajectory is a step function and identifies true wage changes as structural breaks in the reported base wage series. Alternative approaches assume a specific functional form of the measurement error distribution or make other distributional assumptions (Altonji and Devereux (1999)). Other papers (e.g. Card and Hyslop (1996) and Akerlof et al. (1996)) rely on validation studies conducted by Bound and Krueger (1991) which provide estimates of the ratio of the variance of the error component to the variance of reported wages. Dickens et al. (2007) and Dickens et al. (2008) remove autocorrelation in wage changes based on the assumption that measurement error introduces negative autocorrelation in wage changes.

This paper provides estimates of downward nominal and real wage rigidity for Luxembourg based on an administrative dataset with monthly frequency covering the period between January 2001 and January 2007. We provide estimates of both nominal and real downward wage rigidity. However, we focus on the latter because previous research suggested that it might be especially relevant in a country with full automatic wage indexation. While Lünnemann and Wintr (2009) provide evidence on the flexibility of wages in Luxembourg by calculating the frequency of wage change, this paper directly estimates downward wage rigidity in Luxembourg and compares the results to other countries. The frequency of wage change might provide a misleading indicator of wage rigidity because it depends, among others, on inflation and its expectations. We adopt two sets of rigidity estimates that attempt to limit these shortcomings and provide comparable results over time and across countries. More specifically, we follow the methodology developed by the *International Wage Flexibility Project* (IWFP hereafter), see Dickens et al. (2007) and Dickens et al. (2008).[1]

We extend the literature in several ways. First, we provide estimates of downward rigidity for Luxembourg, which not only has not participated in the IWFP so far, but also features a number of distinctive institutional labour market characteristics. Labour market institutions have been put forward as a potential source of wage rigidity (Holden and Wulfsberg (2009), Dickens et al. (2008)). Second, in order to assess the robustness of our estimates, a cross-check is provided based on two alternative measures to identify measurement error which, contrary to the IWFP procedure, make full use of the monthly frequency of the dataset. Third, while the IWFP procedure is typically used to derive estimates of downward rigidity for the aggregate economy or selected sectors (Messina et al. (2010) and Dickens et al. (2008)), in addition, we investigate whether the degree of downward wage rigidity differs across occupational groups, sectors and firms. We concentrate on job stayers and our ideal measure of wage is hourly base wage in order to facilitate international comparison.

Our main findings can be summarized as follows. First, the dataset is subject to two main types of measurement error that can substantially bias downward the measured degree of downward wage rigidity. While the raw data suggest that nominal wage cuts are not uncommon (overall roughly 7.7% of all y-o-y wage changes in the raw data set), we find few nominal wage cuts once adjusting for measurement error (2.0–3.6% of all y-o-y wage changes).

[1] It is impossible to follow with our dataset the line of research started by Kahn (1997), recently extended by Christofides and Nearchou (2007), because it requires substantial variation in inflation rate during the sample period. The same holds for the approach of Holden and Wulfsberg (2007b). NICP inflation in Luxembourg ranged from 2% to 2.7% between 2001 and 2007.

Second, we find an overall very high degree of downward real wage rigidity (DRWR), with 2.3–5.4% of all y-o-y wage changes being below 2% wage increases.[2] Our estimates suggest that nearly all workers in Luxembourg are potentially subject to downward real wage rigidity. The degree of DRWR in Luxembourg seems to be substantially higher than in all other countries studied within the IWFP so far. Even though Holden and Wulfsberg (2007b) obtain a much lower estimate of DRWR for Luxembourg, their estimate too, is the highest among the 19 OECD countries included in their study. As a mirror image of the strong DRWR, downward nominal wage rigidity does not play important role in Luxembourg.

Third, these results are robust to the procedure used to adjust for measurement error and the two different methods for estimation of downward wage rigidity that we adopt.

Fourth, we observe only relatively small differences in the frequency of nominal wage cuts across occupational groups and sectors that are not significant in most cases or can be related to specific deficiencies of a particular method. In addition, we show that in spite of very different dynamics at the company level, there is no robust relationship between the fraction of (real and nominal) wage cuts and firm's growth (as proxied by the growth rate in total wage bill and head count). This suggests that the observed downward real wage rigidity is not driven predominantly by the absence of negative shocks.

Our results suggest that the high degree of downward real wage rigidity observed in Luxembourg is primarily due to factors covering essentially the whole economy, such as labour market institutions. We show that the spike in the wage change histogram in the interval 2% to 3% is related to the automatic wage indexation which in effect contributes to the downward real wage rigidity. We cannot exclude that additional factors might be necessary to explain the high degree of downward nominal wage rigidity. Preliminary evidence from a recent survey among Luxembourg firms suggests that labour regulation/collective agreements are among the most important reasons preventing wage cuts in Luxembourg.

The paper is organised in four sections. Section 2 discusses the institutional characteristics of the Luxembourg labour market with a special focus on the automatic wage indexation mechanism and the statutory minimum wage. Section 3 describes the dataset, discusses two main types of measurement error and defines the relevant wage measure. In section 4, we propose three measures to reduce measurement error and describe the two sets of downward rigidity estimates developed by the IWFP. Our results with regard to the degree of downward wage rigidity are presented in Section 5 including findings for the entire economy, occupational groups, sectors and across firms. Section 6 concludes.

[2] The automatic wage indexation stipulates 2.5% wage increases.

2 The Luxembourg labour market in a nutshell

Recent research has demonstrated the role of the institutional settings of labour markets on wage rigidities (Dickens et al. (2007)). While the Luxembourg labour market shares certain characteristics with a number of other countries, it does feature some distinctive properties with potentially important implications for the presence/strength of downward wage rigidity. For example, Du Caju et al. (2008) and OECD (2009) report a high trade union density in Luxembourg relative to western European countries and a strongly decentralized wage bargaining process. More importantly, however, coordination of wage bargaining in Luxembourg is economy-wide predominantly achieved through full-automatic state-imposed wage indexation and statutory minimum wages, a combination rarely seen in other industrialised economies.[3] Details of the wage indexation mechanism and the statutory minimum wage are provided in sections 2.1 and 2.2, respectively.

2.1 Automatic wage indexation

Du Caju et al. (2008) report that some form of wage indexation exists in various European countries (e.g. Belgium, Cyprus, the Czech Republic and Slovenia), but their scope is rarely as broad as in the case of Luxembourg where wage indexation is the dominant coordination mechanism in the wage bargaining process. In Luxembourg, wage indexation is state-imposed, applies to all wages and pensions and, for most of the period under study (namely from 2001 to mid-2006), had been applied entirely automatically. The state-imposed wage indexation mechanism being entirely backward-looking, it stipulates an increase in all wages and pensions the month after the 6-month moving average of the national index of consumer prices ($NICP$ hereafter) increased by 2.5% relative to its level at the time of the preceding wage indexation event.[4] Importantly, the wage indexation mechanism makes no distinction with regard to the sources of inflation. Regardless of the sources underlying, increases in the general price level are fully passed through to wages in the event of wage indexation.[5] During the period under study, wage indexation kicked in six times, with one occurence

[3]For more details on the Luxembourg labour market from an institutional perspective see Lünnemann and Wintr (2009).

[4]Changes in a small number of prices included in the $NICP$, however, are not taken into account within the framework of the wage indexation mechanism.

[5]In 2006, the Government decided to no longer apply the wage indexation mechanism in its purely mechanical form over the period 2006-2009 and introduced an amendment (*modulation* hereafter) governing the implementation of automatic wage indexation over this period. The amendmend reflected concerns of (shrinking) competitiveness against the background of rapid increases in oil prices. In an effort to fix a ceiling to the frequency of wage indexation events over the period 2006-2009, when introduced in 2006, the *modulation* excluded any further wage indexation prior to December 2006 regardless of actual developments in the price level. In December 2006, wages were indexed by 2.5%, i.e. a rate inferior to the increase in the general price level observed since the preceding event of wage indexation (i.e. October 2005). While the *modulation* has not affected the rate by which wages are changed in the event of indexation, given the fairly high inflation rates observed in 2006, it might have had an impact on wage negotiations and wage setting behaviour in more general. However, the amendment has been introduced in mid-2006 only, probably after standard collective wage agreements had been concluded.

per calendar year and a 12-14 months duration between two consecutive indexation events.[6]

While the event of automatic wage indexation is exogenous to the individual firm, agents know the size of the wage adjustments due to indexation and they can broadly anticipate their timing. The actual level of the $NICP$ is regularly published by the national statistical institute *Statec*. In addition, both *Statec* and the *Banque centrale du Luxembourg* publish inflation forecasts twice a year. Moreover, the wage indexation mechanism often being considered a key element of the *Luxembourg model*, the prospects for an upcoming round of wage indexation are widely discussed in the national press. While the impact of wage indexation on wages is obvious, survey evidence suggests a strong link with prices. For example, Lünnemann and Mathä (2006) report that firms in the services sector consider wage indexation the second most important reason for price increases. According to their 2008 survey among Luxembourg firms (Lünnemann and Mathä (2010)), 42% of all firms confirm a direct link between wage changes due to indexation and price increases.[7]

2.2 Statutory minimum wage

The minimum wage is another corner stone of wage coordination in Luxembourg. Similar to the automatic wage indexation mechanism, the scope of the minimum wage in Luxembourg is particularly broad and relies on a strong legal framework. Though some form of minimum wages is in application in various industrialised countries (for an overview see, for example, Du Caju et al. (2008)), in Luxembourg the minimum wage is legally binding and extends to all sectors of the economy. The statutory minimum wage level imposed is identical for all sectors and, in general, there is no distinction according to tenure. However, a 20% (25%) hair cut is applied to the wages of young workers in the age of 17 (age of 15 and 16) and a 20% premium is applied in the case of qualified workers. As reported in Du Caju et al. (2008) and EIRO (2009), the minimum wage level in Luxembourg is fairly high and so is the proportion of workers covered by the minimum wage regime (between 10 and 20%).[8]

The minimum wage is defined in terms of an hourly wage rate. At the end of the sample period, the minimum wage was approximately EUR 9.1 for unskilled workers aged 18 or more. Similar to all other salaries and pensions, the minimum wage is subject to automatic wage indexation. However, changes to the minimum wage not only result from wage indexation, but also from the

[6]Wage indexations occured in April 2001, June 2002, August 2003, October 2004, October 2005 and December 2006.

[7]While the majority of the firms rejected the idea of automatic wage indexation directly leading to price increases, the share of firms confirming a direct link in the case of wage increase due to indexation is substantially higher than in the case of wage increases stipulated by collective wage agreements (22%) or due to other promotions (21%). A higher acceptance ratio with regard to a direct link to the firm's price is obtained for rising commodity prices only (52%).

[8]According to Du Caju et al. (2008), the proportion of workers subject to minimum wage enforcement is 10% or lower in most countries operating statutory minimum wages.

Government's bi-annual assessment of productivity gains and developments in real salaries over the past two years. During the sample period, the Government raised the statutory minimum wage (for reasons other than indexation) on four occasions, i.e. in January 2001 (+3.1%), in January 2003 (+3.5%), in January 2005 (+2.1%) and in January 2007 (+1.9%).

2.3 Other coordination mechanisms

While there are coordination mechanisms other than wage indexation and the statutory minimum wage, they are not dominant in the case of Luxembourg and/or not materially different from the mechanisms in operation in other economies and already dealt with in other studies. According to Du Caju et al. (2008), coordination is mainly achieved at the "intra-associational" level.[9] By contrast, they find that "inter-associational coordination"[10] and pattern bargaining do not apply in Luxembourg. Du Caju et al. (2008) report an average duration of collective wage agreements of roughly two years. Similar to many other countries, there is a strong seasonal pattern in the timing of such collective wage agreements in Luxembourg. Wage negotiations typically start around the turn of the year and agreements are reached within the first quarter of the following year. However, some sectors may exhibit different seasonal patterns.[11] Pre-expiry renegotiations are not uncommon in *Industry*, *Market services* and *Non-market services* sectors, in particular when facing periods of slower growth and rising concerns about competitiveness.

3 Dataset and variables

3.1 Baseline dataset

This paper uses data on individual salaries and hours worked as reported by employers on behalf of their employees and compiled by the Luxembourg social security authority (*Inspection Générale de la Sécurité Sociale*, *IGSS* hereafter). With more than 22 million observations, the dataset reports monthly salaries for more than 475 000 employees affiliated with more than 46 000 employers representing more than 845 000 occupations covering all sectors of the economy over the period from January 2001–January 2007.[12] In 2006, the aggregate volume of base salary reported in the dataset was roughly 11.4 bn EUR, an amount equivalent to more than 33% of GDP.

[9] I.e. wage bargaining is primarily undertaken at the level of umbrella organisations of employers and trade unions. According to Du Caju et al. (2008), "intra-associational coordination" is the dominant form of coordination in a number of countries.

[10] I.e. wage bargaining is undertaken at the national level or via cross-sector agreements.

[11] The actual timing of the wage bargaining process may lag behind the above seasonal pattern in case an agreement is particularly difficult to find. This applies in particular to the sectors *Industry*, *Market services* and *Non-market services*. In such cases, agreements are often applied retrospectively and/or include one-off payments.

[12] The dataset does not provide information on officials affiliated with the local EU institutions, such as the European Court of Justice, the European Court of Auditors, the European Commission and the European Investment Bank.

Even though our analysis primarily uses data on the monthly salary and the number of hours worked, we also make use of auxiliary information included in the dataset, such as the occupational group (blue-collar workers, white-collar workers and civil servants), the age of the employee, the NACE sector of the employer, the total number of employees per firm, etc. For the purpose of this paper, we remove observations that either cannot be used (e.g. due to missing information) or are clearly not representative of the Luxembourg labour market. For example:

1. We remove observations with missing information on the monthly salary, the sector, the number of hours worked, the occupational group, the age etc. Overall, this leads to a mere 0.2% reduction in the number of observations. The impact is largest for blue-collar workers (roughly 0.3%) and smallest for civil servants (less than 0.1%);

2. We remove observations on the basis of specific firm characteristics, i.e. firms belonging to the sectors *Domestic activities* and *Extraterritorial bodies*. This yields a 2.9% reduction in the overall number of observations. The structure of employment in these sectors not being representative of the weight of the three occupational groups in the total workforce, the reduction in the number of observations is relatively high for blue-collar workers (-6.0%) while the number of observations remains unchanged in the case of civil servants;

3. We remove observations on the basis of specific characteristics of the employee, i.e. observations referring to very young (i.e. below 18 years) or elderly people (aged 60 or more). This leads to a 1.4% reduction in the total number of observations. The largest reduction is observed for civil servants (-2.0%). For blue-collar workers and white-collar workers the impact is of similar magnitude (-1.4% and -1.3%, respectively);

4. We remove observations on the basis of specific properties of the job spell[13], e.g. in case the number of hours worked per month is less than 40 hours or whenever the implied hourly wage rate falls below the minimum wage. In addition, we remove individual observations in case the monthly salary is likely to include overtime compensation.[14] We cannot exclude the presence of overtime compensation whenever the number of hours worked reported exceeds the number of norm hours, whether fixed (173 hours per month for full-time staff) or variable (i.e. number of workdays

[13]Hereafter, a job spell is defined as a unique relationship between any given employee i and any given employer j. Whenever a relationship between employee i and employer j is interrupted for one month (or longer) and resumed later on, an additional job spell is generated. The term *wage trajectory* is to describe the series of monthly wage observations characterising a unique job spell.

[14]Empirical work on wage rigidities typically focuses on the base wage. Considering overtime remuneration is likely to understate the true degree of wage flexibility (for an illustration see, for example, Section 3.2.2 below). Variation in the number of overtime hours may lead not only to changes in the monthly salary for hourly workers, but also to a different average hourly wage rate in case overtime hours are remunerated at rates other than those applied to normal work hours (e.g. Sundays, night work).

times eight for full-time staff).[15] [16] In addition, we remove observations referring to simultaneous job spells (i.e. an employee maintaining two or more jobs with the same company at the same time) as these jobs are not reliably distinguishable from the information in the dataset. The job spell is entirely removed whenever its duration (in-sample) is shorter than 25 months. These measures have important implications for the size and the structure of the dataset, implying a reduction of the number of observations by half. While we observe a substantial reduction in the case of blue-collar workers (-76.3%, the impact remains moderate in the case of civil servants (-4.0%).

While in general, the impact of the above measures remains moderate, two single measures bear particularly important implications for the size and the structure of the dataset. The strongest reduction in the number of observations and the largest change in the structure of the dataset by status is due to the 25-months duration minimum requirement for job spells.[17] Roughly 2.7 million observations are dropped by eliminating observations obviously subject to overtime work.[18] In spite of their substantial impact, we consider these measures important, clearly improving our estimates of wage rigidity. First, observations including overtime overtime work may lead to more mean reversion in both the monthly salary and the hourly wage rate and therefore understate the degree of wage rigidity. Second, job spells lasting less than 25 months would yield a single observation for the year-on-year wage change, which is the key variable when assessing the degree of wage rigidity within the framework of the IWFP.

In sum, the cleaned dataset provides more than 10 million monthly observations covering more than 190 000 jobs (i.e. -77% compared to the initial dataset) maintained between almost 185 000 employees (i.e. -61%) and almost 15 000 firms (i.e. -68%). The impact of the cleaning procedure on the structure of the dataset of annual wage changes is shown in Table 4 in the Appendix. In particular, the cleaning procedure leads to an over-representation of white-collar workers and civil servants, public sector companies, companies with less than 15 employees and in general sectors with a lower proportion of blue-collar workers (such as *Manufacturing* and *Construction*). The number of annual January-to-January wage changes drops by approximately 46%.

3.2 Measurement error

Microeconomic studies are typically plagued by problems related to measurement error. This is well documented for survey data (e.g. Gottschalk (2005))

[15]On the definition of norm hours, see Section 3.2.1.

[16]While the cut-off levels are obvious in the case of full-time staff, a similar rule-of-thumb distinction between overtime hours and regular work time is hardly possible in the case of part-time staff. For part-time staff we therefore rely on the methods proposed to limit measurement error (see section 4.2).

[17]This measure leads to a reduction of almost 4.9 million observations, with more than 25% of observations lost in the case of blue-collar workers and less than 4% lost in the case of civil servants.

[18]Again, the change is particularly important for the category of blue-collar workers (almost 22% reduction in the number of observations) whereas in the case of civil servants the number of observations is hardly affected (approximately 0.1% reduction).

Figure 1: Simulated impact of measurement error due to norm hours

Note: The black distribution (without measurement error due to misreported norm hours) is calibrated on the empirical histogram of changes in salary of white-collar workers and civil servants. The simulation assumes that 25% of workers report time-varying norm hours which imply an increase of norm hours from 168 to 176 (red distribution). The distribution is rescaled and shifted accordingly.

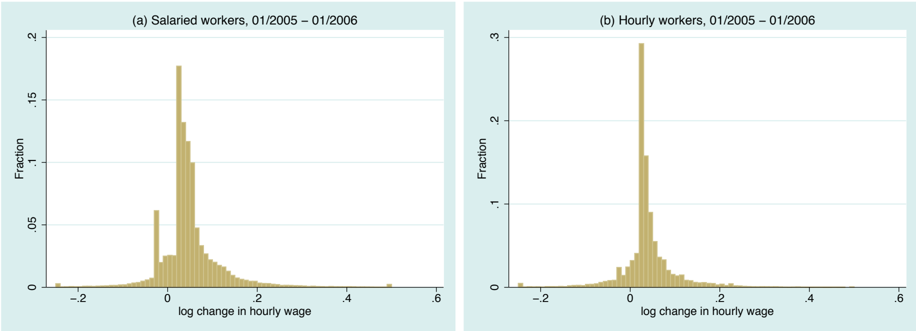
but it can be the case also in administrative datasets. The dataset used in this paper is no exception, featuring two main types of measurement error with important implications for the assessment of downward wage rigidity, namely unreported overtime hours and misreported hours worked.

3.2.1 Misreported (standard) work hours

While the dataset does provide information on the number of hours worked, there is no (unique) convention on how to report the number of ordinary work hours (i.e. excluding overtime hours). While some firms report the actual number of hours worked in a given month, others report some form of *norm hours*. The number of norm hours reported, in turn, can be fixed (i.e. 173 hours per month for full-time staff) or time-varying (i.e. the number of workdays in a given month times eight, which in turn is the regular daily work time in the case of full-time staff).

In case of hourly workers (and in the absence of overtime hours or short-time work), the actual hours worked and time-varying norm hours coincide with the remunerated number of hours and hence both can be used as a denominator in deriving the hourly base wage from the monthly base pay (excluding bonuses). However, if the hours reported are constant norm hours, the base wage would fluctuate from month to month whenever the number of work days changes between to subsequent months. In case of salaried workers (without remunerated overtime work), the hourly base wage can be derived correctly by dividing the base salary by the constant norm hours. If the hours reported reflect the actual number of hours worked (without overtime) or the time-varying norm hours,

Figure 2: Empirical distributions of *hourly* base wage changes



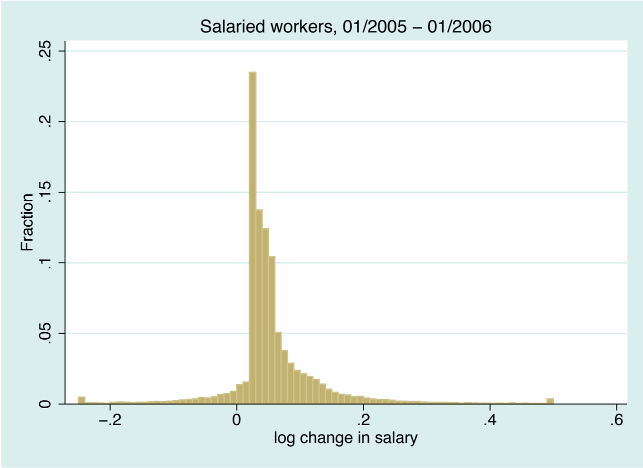
Note: The number of norm hours in January 2005 and January 2006 were 168 and 176, respectively.

both would lead to additional monthly wage changes in the ratio of base salary to reported hours that do not reflect true flexibility of the base wage.

Depending on the proportion of workers with misreported hours, this form of measurement error can have a substantial impact on the distribution of annual wage changes. In case of time-varying norm hours being reported for salaried workers or constant hours being reported for hourly workers, we can actually predict the bias in the annual wage change histogram because the change in norm hours is identical for all workers. The situation is illustrated in Figure 1. First, we assume that the distribution of the change in the hourly base wage for salaried workers reporting constant norm hours is a left-truncated Weibull distribution (depicted by the black line in Figure 1).[19] The plot depicts the annual (January-to-January) distribution of hourly base wage changes and the spike at 2.5% is due to automatic wage indexation. We assume that 25% of the population of salaried workers report time-varying norm hours and that there is a difference of one work day between January in time t and January in the previous year (e.g. the number of workdays increases from 22 to 23, as it was the case from 2005 to 2006). For this sub-set of workers, the hourly wage will be lower by approximately 5% reflecting the increase in the hours reported. This implies that the histogram of hourly base wage changes of salaried workers subject to this type of measurement error will have the same shape as the histogram of the remaining workers but it will be be shifted to the left (by approximately 5 percentage points in this example). The red distribution of wage changes in Figure 1 depicts this situation. The histogram of the change in the hourly base wage for all salaried workers can be obtained by vertically summing the two distributions in Figure 1.In fact, the resulting distribution remarkably resembles the empirical (observed) histogram of the change in the hourly base wage of salaried workers depicted in Figure 2(a). Figure 3 showing changes in base salary demonstrates that the spike in the interval $(-3\%; -2\%]$ in Figure 2(a) is a mere artefact of measurement error due to misreported hours.

[19]The reason for this particular choice of the distribution will become apparent later (see section 4.3.2). However, the argument is general to any distribution.

Figure 3: Empirical distribution of changes in base salary



In analogy, for hourly workers who report constant norm hours instead of the time-varying norm hours (which, in turn, would be the remunerated number of hours in the absence of overtime hours or short-time work), an increase in the number of work days would shift the distribution of the hourly wage rate to the right. Using the same numerical example as above for salaried workers, we would expect the spike due to wage indexation to appear in the interval 7%–8%. However, the empirical distribution of hourly base wage changes for hourly workers depicted in Figure 2(b) does not reveal any pronounced spike in this interval even though one can notice that the bin is slightly higher than one would expect in a smooth distribution. This suggests that the situation in which hourly workers report constant norm hours is much less common than the case of salaried workers reporting time-varying norm hours.

3.2.2 Unreported overtime hours

While the monthly base salary (i.e. excluding bonuses) is reliably recorded in the dataset and, where applicable, includes overtime compensation, the number of hours reported do not always incorporate the actual number of overtime hours. Hence, some of the measured volatility observed in the hourly base wage can be ascribed to the absence of reliable information on overtime hours worked. As the number of overtime hours is fluctuating around their mean, unreported overtime hours tend to understate the true degree of wage rigidity and lead to a more than proportional increase in the number of wage cuts (assuming that wage cuts are less frequent than wage increases in the true distribution). Combined with the effect of wage indexation, this would imply that the true frequency of wage changes below the $(-3\%; -2\%]$ interval is even lower than suggested by the empirical histograms in Figure 2(b) or Figure 3.

3.3 Defining wage and wage changes

The focus of the analysis is on the base wage (rather than total wage). While including bonuses in the definition of wages would likely increase the measured degree of wage flexibility, we do not consider bonuses for several reasons. First, one of our objectives is to provide an estimate of wage rigidity for Luxembourg and compare it to results obtained for other countries. The relevant literature focuses on the base wage in order to eliminate potential differences in wage rigidity stemming from cross-country differences in the use of bonuses.[20] Second, bonuses are not always a form of flexibility from the point of view of the firm. They can be stipulated in the collective agreement in the form of the 13^{th} salary or fixed on the basis of individual or firm performance. Lastly, survey evidence for Luxembourg suggest that only 35% of firms reduced bonus payments when they needed to lower labour costs (Lünnemann and Mathä (2010)).

Moreover, this paper assesses the degree of downward wage rigidity of ongoing employment relationships, i.e. we only consider "job stayers". The distinction between job stayers and job switchers can bear important implications for the measured degree of wage rigidity. Fehr and Goette (2005) found that job switchers have more flexible wages than job stayers. By not considering job switchers, we remove wage changes resulting from job changes (which, in turn, are likely to coincide with a change in productivity). To the extent that firms use changes in staff composition as another means of adjustment[21], our estimates provide an upper bound of wage rigidity.

In order to consider wage rigidity in isolation from changes in labour input, our preferred measure of wage would be the hourly wage rate. However, as illustrated in Section 3.2.1, the measurement error in hours worked reported in the dataset has profound implications for the shape of the hourly base wage change distribution for salaried workers (cf. Figure 2(a) and Figure 3). For this reason we disregard the information on hours worked for salaried workers and consider the base salary as the variable of interest. Overall, the error that we might commit by focusing on the base salary rather than on the hourly base wage in case of salaried workers appears to be rather minor compared to the extent of measurement error in the hours reported in the dataset.[22] In case of hourly workers our measure of wage is the implied hourly base wage. In what follows we refer to this variable (consisting of the base salary for salaried workers and the hourly base wage for hourly workers) simply as the "wage".

Even though the database does include information about the occupational status of the employee (blue-collar worker, white-collar worker, civil servant), whether a given person is a salaried worker or an hourly worker remains undefined. As a general observation, white-collar workers and civil servants tend to

[20] According to Babecký et al. (2009), the share of bonuses in the total wage bill is roughly 3.5% in Spain but almost 50% in Portugal.

[21] Lünnemann and Mathä (2010) report that 28% of firms in Luxembourg considered offering a lower wage to new employees when they needed to reduce labour costs.

[22] For instance a change from full-time to part-time status of a salaried worker will appear as a change in her salary. However, such changes will be likely removed as outliers.

be salaried while blue-collar workers are mostly remunerated on an hourly basis. Nevertheless, thorough inspection of the data suggests that this distinction is too simplistic. Assuming that the true wage trajectory exhibits a step-wise pattern, certain indications on the remuneration pattern (monthly basis versus hourly basis) can be derived from the dataset. Salaried employees earn an agreed monthly base salary which typically is not affected by fluctuations in the number of working days/hours per month (i.e. monthly salary is akin to a step function). In the case of hourly workers, by contrast, the monthly salary would reflect changes in the number of working days/hours while the hourly wage rate typically does not (i.e. hourly wage is a step function).[23] In this paper, a worker is considered salaried if her monthly base salary exhibits smaller volatility (in terms of the coefficient of variation) than her hourly base wage. Otherwise, the worker is considered an hourly worker.[24]

Even though the dataset has monthly frequency, our analysis of wage change histograms (and measures of wage rigidity) focuses on January-to-January wage changes because the y-o-y wage change is the key variable of interest within the IWFP framework. In line with the IWFP procedure, we remove outliers as wage changes below 35% and above 60%.

4 Methodology

4.1 Defining downward wage rigidity

Microeconomic estimates of downward wage rigidity typically rely on the hypothesis that the presence of wage rigidity will affect the distribution of wage changes. For instance, all else equal, downward wage rigidity will reduce the observed number of wage cuts and augment the number of wage freezes (constant wages in nominal or real terms). The extent of wage rigidity can then be assessed from a comparison with an ideal (*notional*) distribution that would prevail in a situation without wage rigidities. However, as illustrated in Section 3.2, our observed (*empirical*) distribution of wage changes will be distorted by measurement error.

In a first step, we consider three different ways that transform the empirical distribution of wage changes into an estimate of the *true* distribution without measurement error. Section 4.2.1 describes the key assumptions of the IWFP protocol. The IWFP protocol has been widely used to assess the degree of wage rigidity on the basis of individual wage data. It has proven to be sufficiently flexible to handle issues related to various types of measurement error arising with different types of datasets covering a vast range of countries. Complementary

[23] In case of staff remunerated on the basis of a piece-rate, both the monthly salary and the hourly wage can fluctuate over time, reflecting changes in output. We do not consider this case as we cannot identify whether a person is remunerated according to a piece-rate or not. According to recent survey evidence, remuneration on the basis of a piece-rate is particularly uncommon in Luxembourg (Lünnemann and Mathä (2010)).

[24] Alternatively, one could identify the relevant wage measure as the measure that is indexed in the case of wage indexation. This approach has been followed in Lünnemann and Wintr (2009).

to the IWFP procedure, in sections 4.2.2 and 4.2.3 we propose two procedures to reduce measurement error. While the main objective of these two procedures is to merely assess the robustness of the IWFP procedure with regard to the identification of measurement error, they are also considered a yardstick for assessing the precision of the IWFP identification scheme itself. Due to a lack of higher frequency data on wages and earnings in many countries studied within the framework of the IWFP, an important mainstay of the IWFP procedure is to consider only year-on-year wage change observations. Given that our data set discloses the full trajectory of the monthly salary earned by employee i affiliated with firm j at monthly frequency, this additional of information could potentially lead to a more complete assessment of downward wage rigidity.

4.2 Limiting measurement error

4.2.1 IWFP procedure

Similar to the method discussed in Section 4.2.2, the IWFP approach to limit measurement error is based on the assumption that if errors are not correlated from one period to another, then a wage increase due to measurement error will likely be followed by a fall in the reported wage in the next period and *vice versa*. In other words, measurement error will introduce negative autocorrelation in wage changes and this autocorrelation can be related directly to the frequency and variance of errors. This assumption is in line with the finding for the US reported in Abowd and Card (1989) and Gottschalk (2005).

The IWFP procedure considers wage changes from year to year and estimates the fraction of observations in each cell of the discrete empirical wage change histogram that can be expected to be found in the true distribution. The observed wage changes are generated through the following model

$$w_{it} = w_{it-1} + e_{it} \tag{1}$$

$$w_{it}^o = w_{it} + \eta'_{it} \quad \text{where} \quad \eta'_{it} = \begin{cases} 0 & \text{if } \mu_{it} > 0 \text{ or } \tau_i > 0 \\ \eta_{it} & \text{else.} \end{cases} \tag{2}$$

According to equation (1), the (log) true wage w_{it} of an individual i at time t follows a random walk which implies zero autocorrelation of wage changes (e_{it} is *i.i.d.* process). The (log) observed wage w_{it}^o consists of the true wage and an error η'_{it}. It is assumed that a fraction of the population p never reports errors, while the remaining individuals report errors with probability c. This specification is particularly appealing in the case of our dataset because most wage trajectories of white-collar workers and civil servants are reported correctly while many wage trajectories of blue-collar workers appear to be affected by measurement error in hours worked. Formally, the random variable η_{it} is *i.i.d.* from a two-sided Weibull distribution with zero mean; μ_{it} is an *i.i.d.* random variable with a uniform distribution on the interval $[-c, (1-c)]$; and τ_i is assumed to be an *i.i.d.* random variable with uniform distribution over the interval $[p-1,p]$.[25] Following the IWFP approach, we estimated the proportion of the population that is prone to make errors $(1-p)$ to be 38%. The probability that someone who is likely to make a mistake makes one (c) is estimated at one.

4.2.2 AR treatment

As illustrated in Sections 3.2.1 and 3.2.2, the two main types of measurement error in the dataset tend to lead to more frequent mean reversion of wages. While the true hourly wage rate is supposedly well-behaved and reveals the typical upward-sloping, step-like pattern, the hourly wage rate implied by the information from the dataset can be very volatile. Even in the absence of true cuts in the (unreported) base wage, unreported overtime hours and/or reported norm hours suggest a substantial number of downward wage changes. To the extent that firms do not disclose overtime hours and/or report norm hours (rather than the actual number of hours worked), changes in the hourly wage rate tend to exhibit negative auto-correlation (for an illustration, see Figure 4).

In order to obtain an assessment of the true degree of downward wage rigidity, for the purpose of cross-checking, we drop all wage trajectories from the dataset for which wage changes reveal significant negative auto-correlation.[26] For each job spell l involving employee i and firm j, the presence of negative auto-correlation in wage changes is tested according to the following specification:

$$dw_{l,t} = c_l^1 + c_l^2 * dw_{l,t-1} + c_l^3 * (dw_{l,t-2} - dw_{l,t-1}) + \epsilon_{l,t}, \tag{3}$$

where $dw_{l,t}$ denotes the log change in the wage observed for job spell l from period *t-1* to period t. In order to avoid the estimate of c_l^2 essentially being driven by a few very large wage changes, we apply weighted regressions with the data being weighted by *Cook's D* influence statistic. Equation (3) then becomes:

$$\begin{aligned} dw_{l,t} * \sqrt{D_{l,t}} &= c_l^1 * \sqrt{D_{l,t}} + c_l^2 * dw_{l,t-1} * \sqrt{D_{l,t}} \\ &+ c_l^3 * (dw_{l,t-2} - dw_{l,t-1}) * \sqrt{D_{l,t}} + \epsilon_{l,l} * \sqrt{D_{l,t}}, \end{aligned} \tag{4}$$

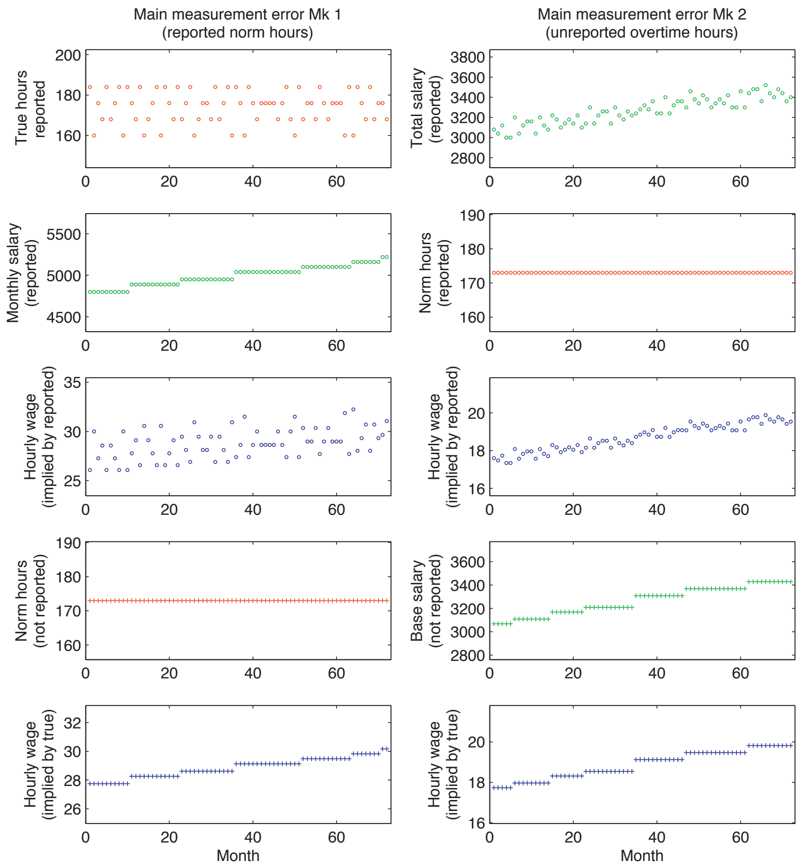
with:

$$D_{l,t} = \frac{\hat{e}_{l,t}^2 * (se_{l,t}^p / se_{l,t}^r)^2}{k_l}, \tag{5}$$

[25]Technical details on the estimation of the true distribution using method of moments can be found in Dickens and Goette (2006).

[26]Both the IWFP procedure and the AR treatment rely on the assumption that true wage trajectories can typically be characterised by a step-like function. However, by considering only y-o-y changes in wages/earnings at an annual frequency, the IWFP procedure implicitly assumes the duration of "no wage change" (i.e. the depth of the step) to fall into a certain interval. The AR treatment, by contrast, does not pre-suppose any specific duration of no wage change.

Figure 4: Main types of measurement error and their implications for the measured degree of wage rigidity by period



Note: For illustrative purposes only. Plots with *o* markers (+ markers) refer to reported data (unreported data) or numbers implied by reported data (unreported data), such as the hourly wage. Red markers refer to hours worked, green markers to monthly salaries and blue markers refer to the hourly wage.

where $\hat{e}$ represents the standardised residual, $se^{p}_{l,t}$ is the standard error of the prediction, $se^{r}_{l,t}$ is the standard error of the residual and k_l refers to the number of observations.

Contrary to the other two measures undertaken to limit measurement error, the AR treatment leads to observations being dropped from the dataset. We expect the AR treatment to have a substantial impact on the size and the structure of the dataset. This is in particular as we cannot exclude that unreported overtime hours are a common source of measurement error in the case of blue-collar workers. Overall, for almost one out of two job spells we find significant auto-correlation at the 5% significance level. Removing these job spells from the dataset also affects the structure of the dataset with regard to the occupational groups and sectors. While the share of observations referring to blue-collar workers shrinks to less than 14% (down from almost 22% in the cleaned dataset), the fraction of observations referring to white-collar workers rises to 70% (up from 61% in the cleaned dataset). The proportion of observations referring to civil servants remains almost unchanged (between 16% and 17% in both datasets). In terms of sectors, the AR treatment leads to smaller proportions of observations referring to *Public administration and defence; compulsory social security* (−2 percentage points) and *Health and social work* (−4 percentage points). By contrast, the AR treatment leads to a higher proportion of observations referring to *Real estate, renting and business activities* (+5 percentage points) and *Wholesale and retail trade* (+2 percentage points).[27]

4.2.3 Break-point test

As a second cross-check of the shape of the true distribution of wage changes, we follow Gottschalk (2005) and assume that the measurement error-free base wage trajectory is a step function and that true base wage changes can be identified as structural breaks in the wage series. Wage changes that do not coincide with structural breaks are considered reflecting measurement error.

More specifically, we apply the multiple break point test proposed by Bai and Perron (1998) which provides a least squares estimator of the number of break points, their timing, as well as the corresponding wage levels based on a recursive algorithm. In simple terms, the procedure identifies the first structural break as the one leading to the lowest sum of squared residuals over the two corresponding subsamples. Next, the null hypothesis of no break is tested against the alternative of a single break. If one break is identified, the sample is split at that break point and the test is applied again to divide the samples. In this second round, the null hypothesis of one break is tested against the alternative of two breaks. In practice the regression model is the same at each stage but the critical values for identifying additional breaks become stricter with each successive test. More details on the procedure and its application to

[27] For one out of two sectors, the deviations are smaller than 1 percentage point.

Luxembourg micro wage data can be found in Lünnemann and Wintr (2009).

The test will identify breakpoints in the wage series whenever the change in the mean wage between two subsequent segments is sufficiently large relative to the variation of the wage around the mean within the segment. The rejection of the null hypothesis of no structural change suggests a *genuine* wage change. By contrast, wage changes that do not create sufficiently large variation are considered reflecting measurement error. Bai and Perron (1998) show that the asymptotic properties of their multiple break point test hold for a very general form of measurement error and that the test can be used when the data are trending.

The maximum number of structural breaks that the Bai-Perron test can identify is determined by the trajectory length and the critical values tabulated by Bai and Perron (1998). This implies a maximum of nine breaks for a wage trajectory consisting of 73 observations. This upper bound might be too restrictive given there were already six events of wage indexation during the sample period. For this reason, before running the breakpoint test, we removed from the dataset the effects of institutional wage changes [28] by deducting the respective wage increases as stipulated by regulation for the periods and individuals concerned. At the same time, we removed outliers from the monthly dataset defined as wage changes below 35% and above 50%.[29] Once the structural breaks are identified, we add the institutional wage changes removed in first place. This provides an upper bound on the true number of wage changes as some of the wage changes due to wage indexation or change in statutory minimum wage might not have been identified as structural breaks if the procedure allowed for higher maximum number of structural breaks.[30]

4.3 Estimating wage rigidity

Our estimates of downward wage rigidity are based on the IWFP methodology using the true distribution of wage changes described in Section 4.2.1. The IWFP protocol distinguishes two sets of estimates, i.e. "simple measures" (discussed in Section 4.3.1) and "model-based estimates" (see Section 4.3.2).[31]

4.3.1 Simple measures

The simple measure of DNWR is based on the assumption that everyone who had a nominal wage freeze (i.e. unchanged nominal wage from the previous

[28]Hereafter, institutional wage changes are defined as wage changes due to automatic wage indexation or changes in the statutory minimum wage.

[29]The treshholds are identical to those applied in the IWFP procedure. Contrary to the IWFP procedure, however, here they refer to month-on-month wage changes.

[30]In spite of the *pre-adjustment* of the wage spells, the number of breaks identified might still be constrained by the maximum number of break dates allowed which, in turn, is restricted by the length of the job spell and the critical values reported in Bai and Perron (1998). Overall, the maximum number of breaks is hit for 16% of job spells. We cannot exclude that for a fraction of these spells the true number of wage changes is higher.

[31]Hereafter, DNWR (DRWR) is used as an abbreviation of downward nominal wage rigidity (downward real wage rigidity).

period) would have had a nominal wage cut in the absence of DNWR, regardless of the reason for the wage cut.[32] Based on the true distribution, we divide the fraction of workers with nominal wage freezes[33] (f_n) by the fraction of workers with either nominal wage cuts (c_n) or nominal freezes. The aim is to avoid direct measures such as the fraction of workers with nominal wage freezes because they are likely to vary over time, in line with the expected inflation rate.

The simple measure of DRWR is based on a similar idea, i.e. it is defined as a fraction of workers with real wage freezes (f_r) over the sum of workers with either real freezes or real wage cuts (c_r). The situation is complicated by the fact that firms and individuals might have different inflation expectations, which implies that the spike in the wage change distribution due to DRWR will have a spread. Details on the derivation of the simple measure of DRWR in this situation can be found in Dickens et al. (2008).

4.3.2 Model based measures

Dickens et al. (2007) propose alternative estimates of DNWR and DRWR based on a model of wage changes (again applied to the true wage change distribution). It is assumed that in case there were no rigidities, the distribution of log wage changes (i.e. the notional distribution) follows a symmetric two-sided Weibull distribution.[34] Hence, the notional wage change d_{it}^n of worker i at time t is a draw from the notional distribution in the respective period t. A fraction of the workers n_t is potentially subject to DNWR. If such a worker receives a negative notional wage change, and he/she is not subject to DRWR, he/she will receive a wage freeze instead of a wage cut. Another proportion of the workers ρ_t is assumed to be potentially subject to DRWR. If their notional wage change falls below their expected rate of inflation, they will receive a wage change equal to expected inflation instead of the notional wage change. Formally, we define the notional real adjusted wage change d_{it}^a as

$$d_{it}^a = \begin{cases} d_{it}^n & \text{if } \epsilon_{it}^r > \rho_t \\ \max(d_{it}^n, \pi_{it}^e) & \text{else,} \end{cases} \tag{6}$$

where $\pi_{it}^e \sim N(\pi_t^e, \sigma_t^\pi)$, ϵ_{it}^r is an i.i.d. random variable from $U(0,1)$ and ρ_t is the probability that an individual is subject to DRWR. The true wage change d_i^t is then determined by

[32] The resistance of workers to wage cuts can be a result of implicit or explicit agreements, labour norms etc.

[33] We consider tiny wage changes in the order of euro cent as exact wage freezes. To be precise, as suggested by the IWFP protocol, the zero log-wage change bin has a width of 0.034%.

[34] This choice is based on the observation that the distribution of wage changes is typically more peaked and has fatter tails than the normal distribution. The upper half of the distribution seems to be well proxied by a Weibull distribution. In addition, they show that in countries not affected by wage rigidities, the lower tail seems to be a mirror image of the upper one. Dickens et al. (2008) provide an example of a wage formation process (similar to the tournament model of Rosen (1986)) that leads to a wage change distribution that can be approximated by a Weibull distribution.

$$d_{it} = \begin{cases} 0 & \text{if } d^a_{it} \leq 0 \text{ and } \epsilon^n_{it} < n_t \\ d^a_{it} & \text{else,} \end{cases} \tag{7}$$

where ϵ^n_{it} is an i.i.d. random variable from $U(0,1)$ and n_t is the probability that an individual is subject to DNWR. The model is estimated with method of moments, details can be again found in Dickens and Goette (2006).

5 Results

Given the role of measurement error in assessing the true degree of wage rigidity, Section 5.1 focuses on the analysis of the true distribution of wage change implied by the three different techniques described in Section 4.2.[35] Estimates of downward nominal and real wage rigidity for the entire economy, by occupational groups and by sectors are provided in Sections 5.2, 5.3 and 5.4, respectively. Section 5.5 assesses simple estimates of downward wage rigidity at the firm level.

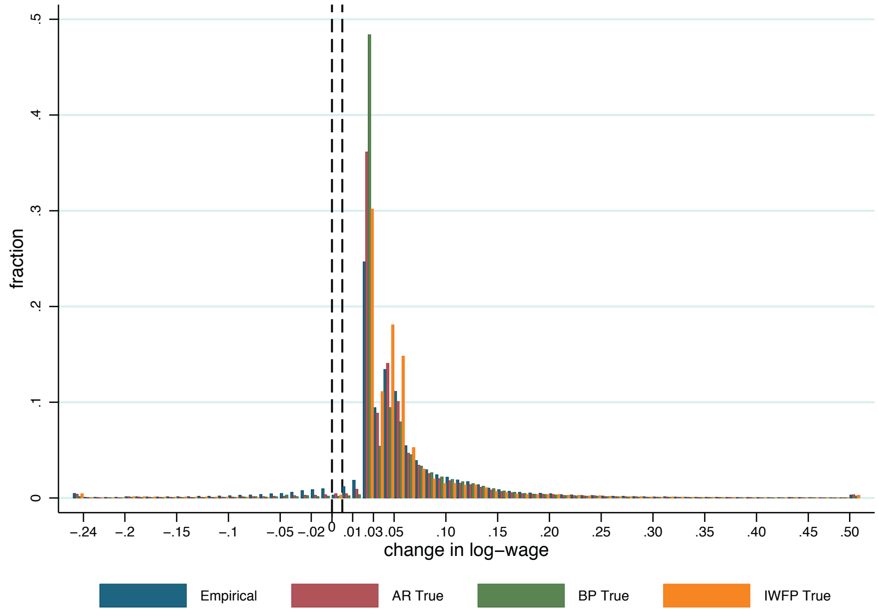
5.1 Estimates of the true wage change distribution

Similar to other studies analysing the degree of downward wage rigidity based on micro wage data, we find that measurement error indeed leads to a substantial bias. By replacing (i.e. break-point test), disregarding (i.e. AR treatment) or correcting for erroneous observations, all three measures to reduce measurement error lead to a substantial adjustment in the distribution of wage changes (see Figure 5) with important implications for the overall variation, the (a)symmetry of the distribution, and the concentration of wage changes around certain mass points. Figure 5 and Table 1 allow us the following observations on certain aspects of the wage change distributions that might reflect the presence (or lack of) downward wage rigidity.

First, based on their multi-country study, Dickens et al. (2008) highlight the presence of a large spike at zero (i.e. no wage change) in the wage change histogram in countries with substantial DNWR (e.g. the spike accounts for more than 15% of wage change observations in the US in 1987). In the case of Luxembourg, by contrast, there is no apparent spike at zero in any of the wage change distributions. The low proportion of wage changes in the zero bin suggests a relatively low fraction of workers with nominal wage freezes. This applies to both the initial dataset, for which we observe a mere 0.3% of wage changes in the zero bin, as well as after limiting for measurement error when the share of wage changes falling into the zero bin ranges from 0.2% (according to the breakpoint test) to 0.5% (according to the AR treatment).

[35] In order to allow for a direct comparison with international evidence from the IWFP, the bins shown in the histograms below are defined according to the IWFP procedure, see also the note under Figure 5.

Figure 5: The 'average' annual wage change distributions, 01/2001–01/2007



Note: January-to-January log wage changes were aggregated over the sample period. The broken lines delimit the zero bin with width of 0.034% and centered at zero. Bins have a width of 1 percent except for those neighbouring the zero bin. Outliers below -35% and above 60% were removed. For the definition of wage, see Section 3.3.

Second, the wage change distributions appear to be asymmetric with fewer observations below the zero bin than in the symmetric upper tail of the distribution. For example, in the empirical distribution, the share of nominal wage cuts in total wage changes is approximately 7.7% only while the corresponding symmetric upper tail of the distribution of y-o-y wage changes (i.e. between twice the median and 35% plus twice the median) covers nearly 18% of observations. Under the assumption that the distribution of wage changes would be symmetric in a situation without wage rigidity, one would infer that the relatively few nominal wage cuts are a manifestation of downward wage rigidity. It is also notable that the methods to limit measurement error suggest that more than half of the nominal wage cuts reported in the original datset are due to measurement error with the IWFP procedure signaling that only 25% of the nominal wage cuts reported are genuine wage cuts. The impact of measurement error is highly asymmetric because in the range of wage changes between 5% and 40% measurement error accounts only for 7.5% of the observed wage changes according to the IWFP procedure.

Third, the asymmetry of the wage change distribution goes beyond the zero bin and extends to the 2–3% bin. In the empirical distribution, roughly one in ten wage changes is smaller than 2%. After limiting measurement error, the fraction of wage changes below 2% varies from 2.3% (IWFP) to 5.4% (AR treatment) and we obtain a quasi-left-truncated distribution. This suggests that a

Table 1: Selected statistics of the wage change distribution

	Percentage of log-wage changes			
	below 0-bin	below 2%	in 0-bin	in 2–3% bin
Empirical	7.65	11.00	0.31	24.7
AR true	3.56	5.39	0.46	36.1
BP true	2.53	3.29	0.18	48.4
IWFP true	1.98	2.30	0.32	30.2

Notes: The zero bin ranges from -0.017% to 0.017%. Outliers excluded.

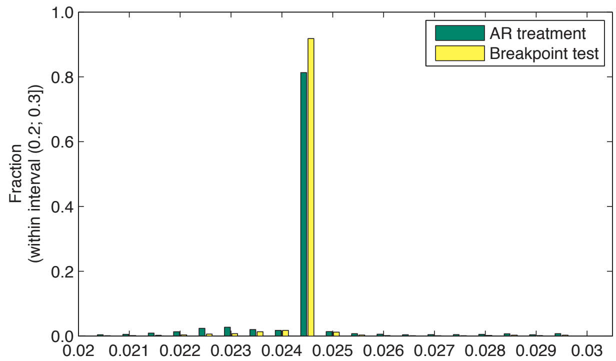
strategy to circumvent wage indexation by combining the obligatory wage increase with a nominal wage cut is not common. The result might be linked to the presence of DRWR.

Fourth, the most common interval for wage changes is from 2% to 3%. This result, which applies to all years under study and both before and after limiting measurement error, is not surprising as the automatic wage indexation falls into this interval and during the sample period there was one wage indexation in each calendar year. All the approaches limiting measurement error suggest an even stronger concentration of wage changes than in the original dataset. The break point test indicates that nearly half of all wage changes (48%) fall into this interval. The 2–3% bin still being relatively wide, Figure 6 further splits this interval and shows that, on average, more than 80% (more than 90%) of all wage changes in the 2–3% bin refer to the narrow interval from 2.45% to 2.50% according to the AR treatment (multiple break point test), i.e. very close to the rate of wage increase stipulated by the automatic wage indexation mechanism.[36] Given that the annual inflation rate based on the national index of consumer prices ranged from 2% (2002) to 2.7% (2005) over the sample period, the evidence of a large spike in the wage change histogram around the expected inflation rate and lack of mass below might suggest the case of DRWR.

Fifth, the results are fairly robust not only with regard to the approach used to delimit measurement error, but also over time (see Figure 11 in the Appendix). One striking difference among the histograms is the presence of the second spike in some of the years. Close scrutiny of both wage level and wage change data at the micro level reveals that the second spike can be due to two factors. First, wage changes of public sector employees are particularly concentrated in two or three wage change intervals. For instance, between January 2001 and January 2002, more than 60% of wage changes of civil servants fell into the interval 4-6%. This appears to be the sole reason for the second spike in the wage change histogram in panel (a) in Figure 11. The histogram of price changes of private sector employees in the same period reveals only one spike in the 2-3% interval. The same argument applies for the period between January 2003 and January 2004 (panel (c) in Figure 11).

[36]The 2.5% wage increase stipulated by the automatic wage indexation mechanism translates into an approximately 0.0247 log change.

Figure 6: Distribution of wage change in the 2% to 3% interval



Note: January-to-January log wage changes aggregated over the sample period.

Another reason for the additional spike in the wage change distribution is the bi-annual increase in the statutory minimum wage. Over the sample period, changes to the statutory minimum wage had been implemented in January 2001, January 2003, January 2005 and January 2007 (see Section 2.2). As the adjustments to the statutory minimum wage varied over the sample period, contrary to the case of automatic wage indexation, their impact does not necessarily show up at a unique bin of the histogram. As indicated in Section 2.2, during the sample period, the bi-annual increase in the statutory minimum wage was between 1.9% (January 2007) and 3.5% (January 2003). Together with the one automatic wage indexation per calendar year over the sample period, this can explain a spike in the $4-5\%$ bin ($5-6\%$) in 2007 (2003), see Figure 11. Moreover, the impact of adjustments to the statutory minimum wage on the distribution of wage changes is not limited to a single bin either. Rather, workers may be subject to wage increases which are smaller than the full increase in the minimum wage if their salary/wage is between the old minimum wage level and the new minimum wage level.[37]

5.2 Estimates of downward wage rigidity

In this section, we discuss the estimates of downward wage rigidity for Luxembourg and place them in an international perspective. The true distributions of wage changes that were estimated in the previous section point to a very low fraction of negative wage changes or wage changes below 2 percent. At the same time, none of the true distributions exhibits a noticeable spike at zero, implying that DNWR should not be a major issue in Luxembourg. The simple measure of DNWR based on the IWFP true distribution directly related to the fraction of workers with nominal wage freezes leads to an average estimate of DNWR

[37]The impact described so far is the direct impact of the minimum wage regulation. In practice, an indirect impact on the distribution of wage changes could arise if minimum wage adjustments were to result in a shift in the entire pay scale, an assessment which is clearly beyond the scope of this paper.

Table 2: Estimates of downward real and nominal wage rigidity

	Blue-collar workers*		White-collar workers and civil servants*		All	
	DNWR	DRWR	DNWR	DRWR	DNWR	DRWR
Simple (true)	0.204	1.067	0.117	1.167	0.138	1.129
	(0.109)	(0.085)	(0.049)	(0.036)	(0.059)	(0.035)
Model-based	N/A	0.998	N/A	0.961	N/A	0.998
		(0.295)		(0.061)		(0.078)

Notes: Averages include only results with estimated confidence bands that do not span the entire [0;1] interval. Hence, the average of model-based measures is based on 2 years in case of white-collar workers and civil servants and 3 years in each of the remaining two categories. The average of simple measures spans all 6 years in each category. Estimates of the simple measures are based on the IWFP true wage change distribution. Simple average of standard errors in parentheses.
* Lower bound on Weibull parameter a raised to 0.015 (from default 0.01).

of 0.14 (Table 2). On the other hand, all of the true wage distributions appear to be quasi-left-truncated at the wage change bin of 2 to 3 percent. This might suggest a substantial degree of DRWR in case the wage bargaining focal point of a substantial proportion of workers falls into this range. This is confirmed in Table 2 showing that the simple estimate of DRWR based on the IWFP true distribution is on average 1.13.[38] In international comparison, the estimates obtained for Luxembourg lie at the extremes. According to Dickens et al. (2008) the simple measure of DNWR is lower only in Ireland and is at a comparable level in Denmark, France and Belgium. In contrast, the simple estimates of DRWR are lower than 0.7 in all the 16 countries studied in Dickens et al. (2008).

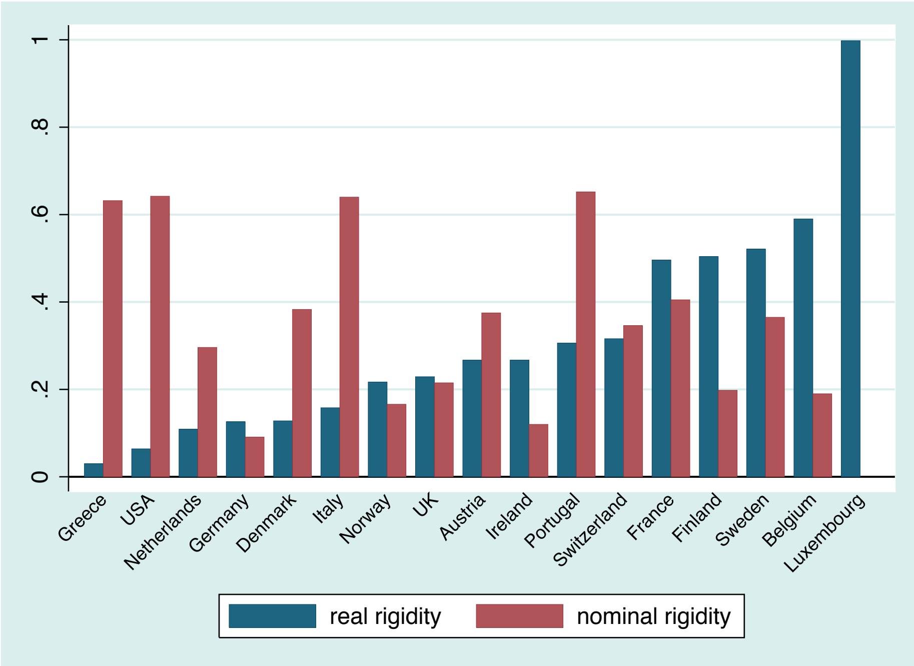
The model-based estimates of wage rigidity confirm the conclusion obtained from the simple measures. The average model-based estimate of DRWR presented in Table 2 is 0.998, which is only marginally below the upper bound of one and substantially higher than the figures provided in Dickens et al. (2007), see Figure 7.[39] Again, the finding of very strong downward real wage rigidity in Luxembourg is robust over time, with ρ being in the interval (0.996, 0.999) in all of the years studied. This suggests that nearly every worker in Luxembourg is potentially subject to DRWR. As a mirror image of the strong DRWR, we did not obtain sensible estimates of nominal rigidity in any of the years.[40] Figure 7 provides the model-based estimates of downward nominal and real wage rigidity reported by Dickens et al. (2007) and includes the corresponding values that we obtained for Luxembourg. Dickens et al. (2007) report model-based estimates of DRWR well below 0.6 for all 16 countries considered. The three highest measures of downward real wage rigidity are found for France, Finland and Sweden (all roughly 0.5). As in the case of simple measures, the extent of

[38] Note that according to equation (**??**), the estimate of r is effectively bound to lie between zero and two while the model based estimates are delimited by zero and one.

[39] The yearly estimates of DRWR hit the upper bound of one in three years which did not allow us to obtain an estimate of its standard error and hence we excluded those years from the average.

[40] The estimate of DNWR hit the boundary in all years except for one year. However, the standard error in that year exceeded 200 and so the observation was discarded from the average presented in Table 2.

Figure 7: Model-based estimates of wage rigidity in international comparison



Source: Dickens et al. (2007), Du Caju et al. (2007) and our calculations.

DRWR in Luxembourg is exceptionally high in international comparison and cannot be simply ascribed to differences in methodology or data.[41]

To our best knowledge, there is only one set of alternative estimates of downward nominal and real wage rigidity for Luxembourg provided by Holden and Wulfsberg (2007a) and Holden and Wulfsberg (2007b), respectively. Both papers use industry level data on the percentage growth of gross hourly earnings for manual workers in manufacturing, mining, energy and construction covering 19 OECD countries over the period 1973-1999. Both papers estimate the notional wage change distribution by pooling country-year samples with high nominal and real wage growth assuming that those distributions will not be significantly affected by wage rigidity. Consistent with our results, Holden and Wulfsberg (2007b) conclude that Luxembourg has the highest degree of real wage rigidity within the set of countries studied, even though their estimate (0.13) is much lower than ours. On the other hand, downward nominal wage rigidity in Luxembourg according to Holden and Wulfsberg (2007a) is significantly different from zero and there are eight countries with lower DNWR than

[41] Even though we showed in Section 5.1 that the IWFP error correction technique leads to very similar results when compared to two alternative approaches in Luxembourg, we cannot generalize our conclusion to the case of other countries or datasets which might suffer from a different type or extent of measurement error.

their estimate for Luxembourg (0.27).[42]

In Section 5, the estimates for the full economy suggested the *quasi-absence* of wage cuts (whether nominal or real) in Luxembourg over the entire sample period. While there is strong evidence of a high degree of downward real wage rigidity at the level of the entire economy, the overall result might nevertheless hide significant differences across occupational groups, sectors and/or firms. First, differences with regard to the degree of downward real rigidity across occupational groups and sectors might result from different institutional settings governing the wage setting process other than automatic wage indexation and the statutory minimum wage. Second, similar to other empirical work on rigidities (whether wages or prices) the low frequency of wage cuts observed might not only signal strong downward wage rigidity, but also result from weak incentives for firms to actually reduce labour costs in the absence of negative shocks. On average, the 6-year sample period is characterised by moderate to strong economic growth (average annual growth in GDP between 2001 and 2007: 4.6%) and moderate to high inflation rates (average annual inflation rate between 2001 and 2007: 2.3%).[43] At the same time, the Luxembourg economy witnessed a strong increase in employment (average annual growth in the number of jobs: 3.5%) and in the total wage bill (average annual growth rate in base wages and bonuses combined: 6.4%). However, even in the absence of sizeable negative shocks at the aggregate level, firm-specific shocks can vary substantially. To the extent that firms face different firm-specific shocks, including negative shocks for a subset of firms, the absence of significant differences with regard to the firm-specific degree of wage rigidity might then suggest a genuinely high degree of wage rigidity.[44]

5.3 Estimates of downward wage rigidity by occupational group

The literature on wage rigidity provides several reasons why the extent of downward wage rigidity might differ across occupational groups. The efficiency wage theories (e.g. Shapiro and Stiglitz (1984)) and adverse selection models (Weiss (1980)) predict that firms will be more reluctant to cut wages of employees whose effort is more difficult to monitor or those with high hiring and training costs in order to avoid that these workers reduce their effort or leave the firm. In both cases, cutting wages of white-collar workers can be potentially more costly for the firm. Campbell (1997) reported that wages of skilled and white-collar workers in the US are less-responsive to changes in unemployment but at the

[42]The differences to our results with regard to the level of downward wage rigidity in Luxembourg could be ascribed to several factors. First, our estimates are based on individual data and avoid potential aggregation and compositional effects. In addition, our dataset has economy-wide coverage. Several studies found that blue-collar workers are subject to lower degree of wage rigidity (e.g. Campbell (1997)). Similarly, Du Caju et al. (2009) find substantial differences in wage rigidity across sectors in Belgium.

[43]While the growth dynamics changed quite substantially, the annual growth rate of aggregate GDP never fell below 1.5% during the 6-year sample period. Annual GDP growth was highest in 2007 (6.4%), 2006 (5.6%) and 2005 (5.4%).

[44]By contrast, if our estimates of wage rigidity were driven by the absence of negative shocks we would expect the firm-specific estimates of wage rigidity to vary according to firm characteristics.

same time adjust more rapidly to inflation. Using the IWFP methodology, Du Caju et al. (2007) found that the degree of both downward nominal and real wage rigidity is higher for white-collar workers in Belgium. The difference is at least 25% in the case of the model-based measures. Babecký et al. (2009) using survey data for 15 EU countries report that firms employing a larger proportion of high-skilled white-collar workers are more likely to be subject to both downward nominal and real wage rigidity.

Table 2 above reports estimates of downward rigidity for the two largest occupational groups, i.e. blue-collar workers and white-collar workers. Our results, however, do not suggest significantly different degrees of DRWR.For blue-collar workers we obtain, on average, a ρ of 0.998. The average ρ for white-collar workers and civil servants is 0.961.[45]

However, the difference between the two categories does not seem to be significant given the relatively large standard errors obtained for blue-collar workers. The homogeneity of the estimates of wage rigidity is confirmed by the simple measures and holds both for nominal and real wage rigidity.[46] These results are in line with the evidence presented in Lünnemann and Wintr (2009) showing substantial differences in the frequency of wage change among occupational groups in Luxembourg but only minor differences in the frequency of wage cuts. Altogether, the results suggest that the effect of labour market institutions, especially wage indexation, is particularly strong in Luxembourg, thereby overriding any potential underlying differences in wage rigidity that might be related to worker characteristics.

5.4 Estimates of downward wage rigidity by sector

The extent of wage rigidity can also differ across sectors of economic activity. Campbell (1989) and Campbell (1991) show that the estimates of wage flexibility for the US, Canada and France based on the responsiveness of sector-level wages to the aggregate unemployment and sectoral product demand can be related to the proportions of blue-collar and white-collar workers and capital intensity in the respective sector. Du Caju et al. (2009) document that sectoral differences in wage rigidity in Belgium persist even after controlling for compositional effects. More rigid wages were found in sectors with centralised wage bargaining and in more competitive and labour intensive sectors.

Our estimates of DRWR for the major sectors in Luxembourg are presented in Table 3 below.[47] It is true that more than in the case of occupational groups the estimates of downward wage rigidity vary across sectors and results tend to be more sensitive to the method used to limit measurement error. However,

[45]We also derived results for white-collar workers and civil servants separately. Certain aspects of the distribution of wage changes for civil servants, however, suggest that the estimates of ρ for civil servants are misleading. For further discussion of this issue, please refer to Section 5.4 below.

[46]Reflecting the very high degree of downward real wage rigidity, the IWFP protocol does not provide any model-based estimates of downward nominal wage rigidity though.

[47]Owing to their small size, we do not consider the sectors *Agriculture* and *Energy*.

Table 3: Estimates of downward real wage rigidity across sectors

	Pct (Δ log-wage $< 2\%$)		Simple true	Model-based	
Sector	AR	BP	DRWR	DRWR	St. error
Industry	5.08	3.86	1.216	0.970	(.109)
Construction	5.61	3.74	0.718	0.994	(.072)
Trade*	6.18	4.03	0.931	0.986	(.12)
Market services	5.15	3.39	1.135	0.994	(.09)
Financial services	6.90	4.19	1.034	0.871	(.043)
Other services	4.37	2.30	1.275	0.618	(.045)

Notes: Averages include only results with estimated confidence bands that do not span the entire [0;1] interval. The average of model-based measures is based on at least 4 years in each sector. Estimates of the simple measure are based on the IWFP true wage change distribution and span all 6 years in each sector. Simple average of standard errors in parentheses.
* Lower bound on Weibull parameter a raised to 0.015 (from default 0.01).

these differences must be considered against the overall very low fraction of wage cuts. The fraction of wage changes below 2% is relatively small in all sectors according to both the AR treatment and the breakpoint test.

The model-based estimates of DRWR are very close to one in all sectors considered with the exception of *Financial services* and *Other services* (comprising *Public administration and defence; compulsory social security*, *Education* and *Other community, social and personal service activities*). For *Financial services* the lower degree of real wage rigidity according to the IWFP model-based measure might reflect the highest fraction of wage changes below 2%, see Table 3.

This is in clear contrast to *Other services*, for which the lowest estimate of ρ is obtained in spite of the lowest fraction of real wage cuts among the sectors considered.[48] However, the lower estimate of ρ for a sector populated predominantly by civil servants does not reflect a lower degree of real wage rigidity but rather results from the specificities of both the IWFP protocol and the distribution of annual wage changes for public sector employment. As illustrated in Figure 12 in the Appendix, the distribution of annual wage changes for the *Other services* sector differs from those typically obtained for the entire economy in that the mode of the distribution is not around the 2.5% stipulated by automatic wage indexation, but rather in the 4–5% or 5–6% bin. In the model-based estimate, this translates into higher estimate of the "wage bargaining focal point" (i.e. intended to capture wage increases due to expected inflation, productivity growth, collective bargaining etc.) close to this modal bin. As discussed in Section 4, the absense of wage changes below the "wage bargaining focal point" indicates DRWR. However, in Figure 12 all the observations in the 2–3% bin fall below the "wage bargaining focal point" and hence lower the model-based estimate of DRWR. Note that the simple measure of DRWR is not affected becasue in that case the "wage bargaining focal point" is simply approximated by (backward-looking) expected inflation. For civil servants and sectors with predominantly public sector employment, thus, the mere (very low) fraction of real

[48] In analogy, we obtained a lower estimate of ρ for civil servants compared to white-collar workers and blue-collar workers (see Section 5.3).

wage cuts might be a more reliable proxy of DRWR than the IWFP model-based estimate. In sum, both the simple and model-based measures tend to confirm a high degree of downward real wage rigidity in large parts of the economy (in particular, when compared to the international IWFP evidence).

5.5 Downward wage rigidity at the firm level

As already suggested, the low frequency of wage cuts (both in real and in nominal terms) might be a result of the overall moderate to strong growth dynamics observed over the sample period with relatively little need for firms to cut wages (rather than genuine downward wage rigidity). If indeed, our aggregate estimates of substantial downward wage rigidity were essentially driven by the lack of negative shocks at the aggregate level, we would expect weaker signs of downward wage rigidity in those parts of the economy being subject to sluggish (or negative) growth.

This section provides estimates of downward wage rigidity at the firm level. As the impact of negative shocks on public institutions is unclear, it refers to non-agricultural and non-energy private firms only.[49] Moreover to ensure a minimum number of observations of annual changes in the wagebill and staff, we focus on firms with a minimum duration of 37 months (in-sample).

Since we have no information about the output at the firm level, hereafter, growth rates in employment and total wage bill are considered (admittedly poor) proxies for company performance. The dataset suggests strong heterogeneity in growth dynamics among Luxembourg non-agricultural and non-energy private firms over the sample period. Figure 8 illustrates the distribution of the firm-specific average annual growth rate of the total wage bill and the headcount, respectively. Among the roughly 22 800 private non-agricultural and non-energy private firms that prevailed in the dataset for at least 37 months, the average growth rate in the nominal wage bill (headcount) was 5.9% (2.1%). Almost 5 000 firms (i.e. 22% of the firms) reveal a negative average annual growth rate in the total wage bill while for almost 7 900 firms (i.e. 35% of the firms) the average annual growth rate is below 2.5% (see Figure 8(a)).[50] Almost 4 000 firms (i.e. 17% of the firms considered), on average, reveal a reduction in their headcount (see Figure 8(b)).

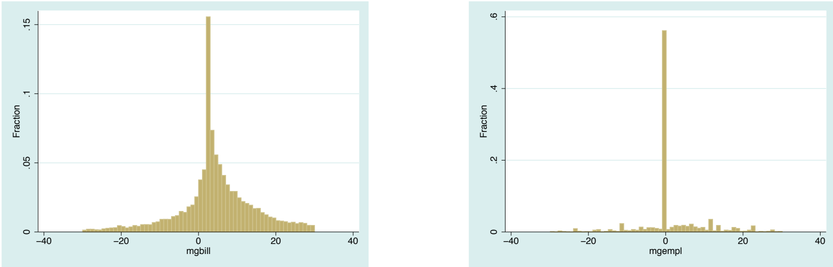
Figure 9 illustrates that the fraction of wage cuts (whether nominal or real) varies among companies. According to the breakpoint test procedure, for approximately 90% of the non-agricultural and non-energy private firms that were active for at least 37 months (in-sample), the fraction of nominal wage cuts in all wage changes was 10% or lower (see Figure 9(a)). For 85% of the persistent

[49] While the dataset does not provide explicit information on the ownership of the company, we consider a company public if and only if the dataset reports at least one civil servant among the staff. We cannot exclude, however, this being a too narrow definition of public institutions.

[50] These figures refer to the total wage bill, i.e. they include bonuses and one-off payments. The corresponding shares for annual growth rates in base wages are 22% and 34%, respectively.

Figure 8: Average annual growth in the total wage bill and headcount

(a) Total wage bill (b) Headcount



Note: Distribution over all non-agricultural and non-energy private firms with a minimum duration of 37 months (averaged across years). While the dispersed distribution of changes in the total wage bill across firms, at first glance, appears difficult to reconcile with the high degree of wage rigidity reported before, it is important to note that *(i)* Figure 8(a) illustrates the distribution of the annual change in the total wage bill (i.e. including bonuses, overtime hour compensation, etc.) while our analysis of the degree of downward wage rigidity focuses on the base wage after applying measures to limit for measurement error; *(ii)* The distribution of the annual change in headcounts across firms must be considered against the (mode) size of firms in the Luxembourg economy. Almost nine out of ten firms with a constant number of staff (see the spike in Figure 8(b)) employs no more than two staff members. For such small companies, variations in the total wage bill will typically result from a change in the remuneration (rather than from a change in their headcount); *(iii)* While the analysis of DRWR focuses on changes in the wage of a given employee, the annual change in the total wage bill reported in Figure 8(a) is obtained by aggregating all wage components across all staff members. To the extent that individual wages are not perfectly synchronised, frequent changes in the total wage bill may hide very infrequent wage adjustments at the level of the individual staff member; *(iv)* Changes in the firm's wage bill might reflect changes in the number of ordinary work hours when staff members are remunerated at an (identical) hourly wage rate.

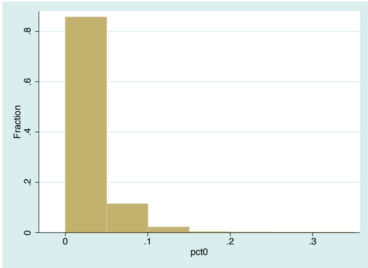
firms the share of real wage cuts (i.e. wage changes below 2.5%) in all wage changes was no more than 10% (see Figure 9(b)). According to the AR treatment, the corresponding fractions are more than 85% and close to 80% (Figures 9(c) and 9(d), respectively). According to the breakpoint test, the average share of nominal wage cuts in all wage changes across firms is 2.9% and the firm average share of wage changes lower than the rate stipulated by automatic wage indexation is 4.9%. According to the AR treatment, the firm average shares are 3.8% and 6.9%, respectively.

How can one characterise the (relatively few) non-agricultural and non-energy private firms revealing a higher than average frequency of wage cuts? Figure 13 in the Appendix reports no obvious relationship between the fraction of wage cuts and the growth rate of the total wage bill (left hand side panels) or of headcount (right hand side panels) at the firm level. Both the breakpoint test and the AR treatment suggest that on average the fraction of wage cuts is higher for firms suffering from negative growth in headcount and total wage bill.[51] Nevertheless, wage cuts can be found for both growing firms and shrinking firms. In addition, simple least squares estimates provide no robust evidence suggesting a significant impact of firm growth (as proxied by the growth rate in

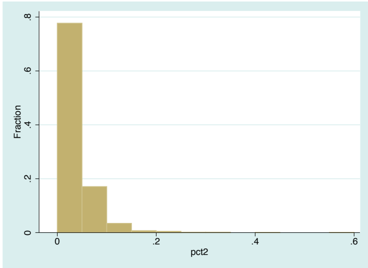
[51] According to the breakpoint test, the average fraction of wage cuts at the firm level is 2.9% but rises to 3.3% (3.8%) for firms with shrinking headcount (wage bill). This is confirmed by the AR treatment, where the corresponding proportions of wage cuts are 3.7%, 3.9% and 4.6%, respectively.

Figure 9: Fraction of wage cuts in firms' wage changes; Distribution over firms

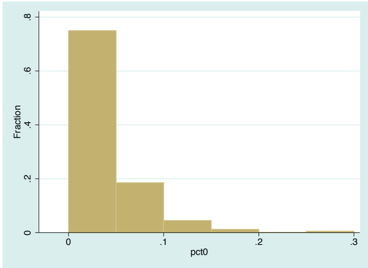
(a) Nominal wage cuts (breakpoint test)



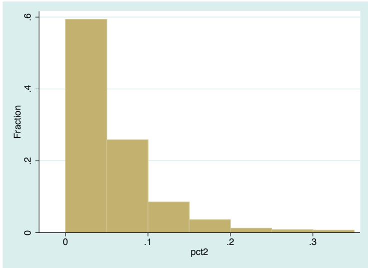
(b) Real wage cuts (breakpoint test)



(c) Nominal wage cuts (AR treatment)



(d) Real wage cuts (AR treatment)



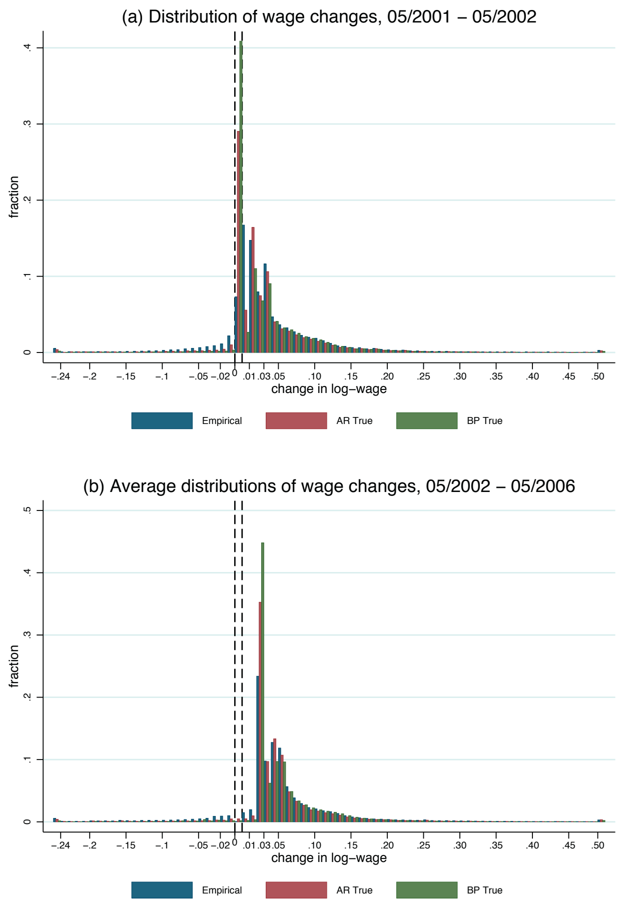
Note: Horizontal axis depicts the percentage of wage cuts (nominal or real) and the bars indicate the fraction of non-agricultural and non-energy private firms with the average share of wage cuts falling into each interval. Distribution over firms (averaged across years).

total wage bill and headcount) on the fraction of wage cuts once accounting for firm size and sector of activity. This applies to both nominal and real wage cuts.

In sum, in spite of (minor) differences with regard to the estimated degree of downward wage rigidity across firms, our results consistently suggest a high degree of downward wage rigidity by international standards for the vast majority of firms in all sectors of the Luxembourg economy. Given the strong heterogeneity in the growth pattern across firms over the sample period, we therefore consider our results not to be markedly driven by the absence of negative shocks. In a competitive, profit-oriented environment firms face pressure to cut costs at all times, not only when hit by an explicit negative shock. While negative shocks might strengthen the incentive to implement wage cuts, they do not overcome the overall downward wage rigidity *in the economy.*[52]

[52] If large number of firms was hit by a negative shock at the same time, it is more likely that they could overcome downward wage rigidity by some coordination mechanism.

Figure 10: Wage change distributions



Note: May-to-May log wage changes. The broken lines delimit the zero bin with width of 0.034% and centered at zero. Bins have a width of 1 percent except for those neighbouring the zero bin. Outliers below -35% and above 60% were removed. For the definition of wage, see Section 3.3.

5.6 The role of wage indexation

Since we found only minor differences in DRWR across sectors, occupational groups and firms despite very different dynamics at the sector and the firm level, we conclude that the main driving factor of the high degree of DRWR in Luxembourg covers basically the entire economy. To evaluate the potential role of the automatic wage indexation mechanism as the main source of DRWR in Luxembourg, we analyzed the distribution of wage changes in periods of 12 consecutive months during which wage indexation did not occur.[53]

Figure 10(a) shows the histogram of wage changes in one of these periods, May 2001 to May 2002, and compares it to the average of May to May wage

[53]These include 05/2001–05/2002, 07/2002–07/2003, 09/2003–09/2004 and 11/2005–11/2006.

change distributions between 05/2002 and 05/2006 (panel (b) in Figure 10). About 30–40% of wages did not change between May 2001 and May 2002 (results adjusted for measurement error) and nominal wage cuts appear rare. The wage change distribution for the remaining periods resembles the one which we obtained for January to January wage changes (cf. Figure 5) with a marked spike in the wage change interval 2–3%. Wage changes smaller than 2% are as uncommon as nominal wage cuts during the period between May 2001 and May 2002. The spike in the wage change histogram in the interval 2–3% is hence related to the automatic wage indexation and in effect contributes to the downward real wage rigidity that we observe. Additional factors might be necessary to explain the high degree of downward nominal wage rigidity. For example, high union power, fairness considerations or efficiency wages can all explain in theory why employers do not lower nominal wages. Preliminary evidence from a survey among Luxembourg firms conducted in the aftermath of the most recent crisis period (i.e. when constraints preventing wage cuts might have become more binding than in ordinary times) suggests that labour regulation/collective agreements are among the most important reasons preventing nominal wages from being cut in Luxembourg (Lünnemann and Mathä (2010) consider nine obstacles to wage cuts).[54] Conclusive evidence on the sources of downward wage rigidity in Luxembourg will be the aim of future research based on micro wage data incorporating the most recent crisis period.

6 Conclusion

This paper provides comprehensive microeconomic evidence on the degree of downward wage rigidity in Luxembourg based on a monthly data set covering all firms and employees between January 2001 and January 2007. We provided estimates of both nominal and real downward wage rigidity, however, we focused on the latter because it appears especially relevant in a country with full automatic wage indexation (Lünnemann and Wintr (2009)). In order to assess the robustness of our estimates, we adopted two sets of rigidity measures proposed in the framework of the *International Wage Flexibility Project*. We also provided a cross-check based on two alternative measures to identify measurement error, which, contrary to the IWFP procedure, make full use of the monthly frequency of the dataset.

We show that the dataset is subject to two main types of measurement error that would substantially lower the measured degree of downward wage rigidity. While the raw data suggest that nominal wage cuts are not uncommon (overall roughly 7.7% of all y-o-y wage changes in the baseline data set), we find few nominal wage cuts once adjusting for measurement errors (2.0–3.5% of all y-o-y wage changes). Contrary to the evidence for many other countries, in the case of Luxembourg, there is no apparent spike at zero in any of the wage change

[54] Labour regulation/collective agreements are the most important reason(s) preventing wage cuts according to the mean and median score. In addition, roughly six out of ten firms considered labour regulation/collective agreements very relevant in preventing wage cuts. Other important obstacles to wage cuts during the recent crisis refer to their potential impact on employees' morale and effort.

distributions. The distribution of wage changes is strongly asymmetric, and the asymmetry is extending well beyond the zero bin. The most common interval for wage changes is the 2–3% bin, which spans the 2.5% wage increase stipulated by automatic wage indexation. In the empirical distribution, approximately one in ten wage changes is smaller than 2%. After limiting measurement error, the fraction of wage changes below 2% varies from 2.3% to 5.3% and we obtain a quasi-left-truncated distribution. All three approaches limiting measurement error suggest an even stronger concentration of wage changes on the 2% to 3% interval (e.g. nearly half of all wage changes according to the break point test).

The evidence of a large spike in the wage change histogram around the expected inflation rate and lack of mass below suggests the case of DRWR. This is confirmed by the simple and model-based estimates. The latter conclude that nearly every worker in Luxembourg is potentially subject to DRWR, a particularly high level compared to the international evidence from the IWFP. In addition, our results show that a strategy to circumvent wage indexation by combining the obligatory wage increase with a nominal wage cut is not common. These findings are robust to the procedure used to adjust for measurement error, to the method used for estimating wage rigidity and over the years in the sample period.

We assessed whether the high degree of DRWR could be due to weak incentives for firms to actually reduce labour costs in the absence of negative shocks. In spite of very different dynamics at the company level, there is no robust relationship between the fraction of (real and nominal) wage cuts and firm's growth (as proxied by the growth rate in total wage bill and head count). This suggests that the observed downward real wage rigidity is not predominantly driven by the absence of negative shocks.

The differences in the frequency of wage cuts and estimates of downward wage rigidity across occupational groups and sectors are relatively small and in most cases not significant or can be related to specific deficiencies of a particular method. This suggests that the main driving factor of the high degree of DRWR in Luxembourg covers basically the entire economy. We showed that the spike in the wage change histogram in the interval 2% to 3% is related to the automatic wage indexation which in effect contributes to the downward real wage rigidity. We cannot exclude that additional factors might be necessary to explain the high degree of downward nominal wage rigidity. Preliminary evidence from a recent survey among Luxembourg firms suggests that labour regulation/collective agreements are among the most important reasons preventing nominal wage cuts in Luxembourg. Conclusive evidence on the sources of downward wage rigidity in Luxembourg will be the aim of future research based on micro wage data incorporating the most recent crisis period.

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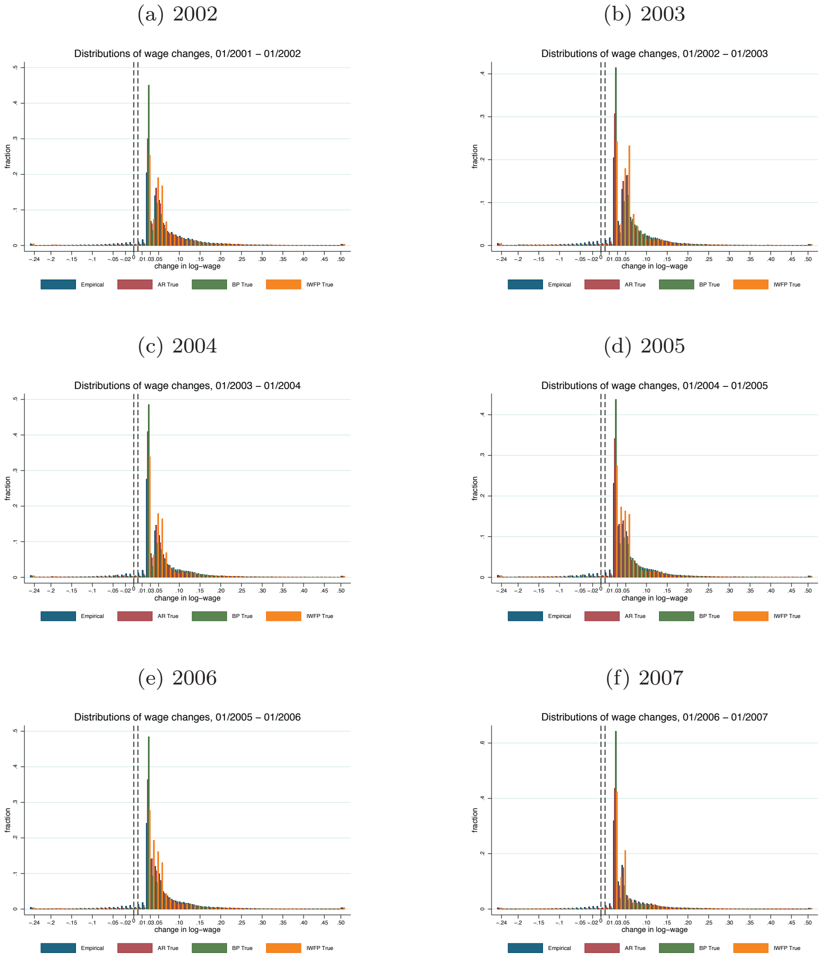
Appendix

Table 4: Structure of the dataset, January-to-January wage changes

	Baseline		Cleaned		AR Treatment	
	Obs.	Pct.	Obs.	Pct.	Obs.	Pct.
Blue-collars	592176	42.8	162073	21.7	50275	13.8
White-collars & Civil servants	790980	57.2	583246	78.3	312813	86.2
Private	1147700	83.0	538486	72.2	281463	77.5
Public	235456	17.0	206833	27.8	81625	22.5
0–14 empl.	269608	19.5	173487	23.3	142064	39.1
15–149 empl.	444357	32.1	215528	28.9	111730	30.8
$\geq$ 150 empl.	669191	48.4	356304	47.8	109294	30.1
A+B	6124	0.4	2335	0.3	1387	0.4
C	1710	0.1	506	0.1	298	0.1
D	176419	12.8	66550	8.9	26583	7.3
E	5638	0.4	2635	0.4	861	0.2
F	146509	10.6	39559	5.3	20513	5.6
G	171513	12.4	84340	11.3	46531	12.8
H	45732	3.3	16168	2.2	8330	2.3
I	122950	8.9	69539	9.3	29521	8.1
J	177845	12.9	121904	16.4	64636	17.8
K	165444	12.0	91456	12.3	63650	17.5
L	184802	13.4	153475	20.6	66026	18.2
M	7848	0.6	5125	0.7	3012	0.8
N	92772	6.7	72936	9.8	20752	5.7
O	35844	2.6	18791	2.5	10988	3.0
2002	209331	15.1	96522	13.0	46562	12.8
2003	220118	15.9	125840	16.9	61439	16.9
2004	228569	16.5	134096	18.0	64476	17.8
2005	234924	17.0	137231	18.4	65512	18.0
2006	240229	17.4	136251	18.3	68114	18.8
2007	249985	18.1	115379	15.5	56985	15.7
Total	1383156	100	745319	100	363088	100

Notes: All data refer to January-to-January wage changes for job stayers. Branches are defined as follows: A: Agriculture, hunting and forestry; B: Fishing; C: Mining and quarrying; D: Manufacturing; E: Electricity, gas and water supply; F: Construction; G: Wholesale and retail trade; H: Hotels and restaurants; I: Transport, storage and communication; J: Financial intermediation; K: Real estate, renting and business activities; L: Public administration and defence; compulsory social security; M: Education; N: Health and social work; O: Other community, social and personal service activities.

Figure 11: Distributions of wage changes



Note: See explanation below Figure 5.

Figure 12: Distribution of wage changes, Other services, 01/2002–01/2003

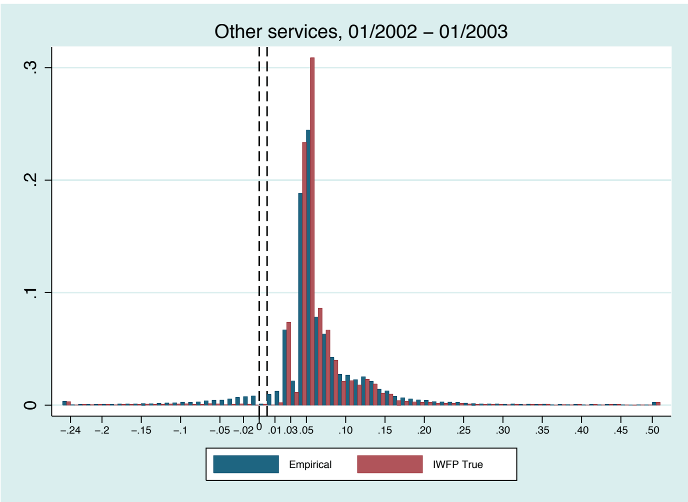
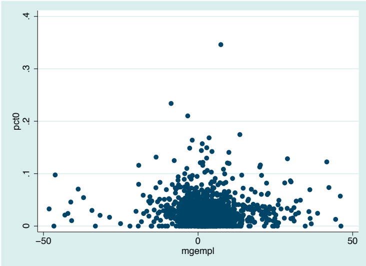
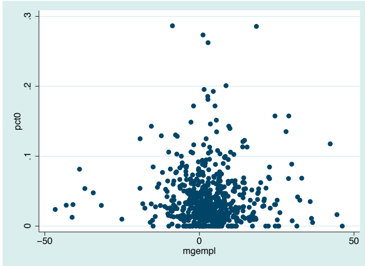


Figure 13: Fraction of wage cuts and firm growth

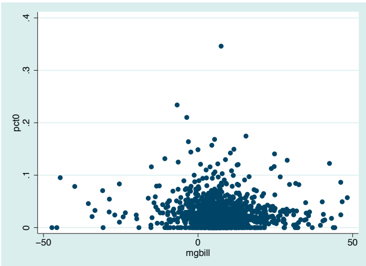
(a) Nominal wage cuts (BP), total wage bill



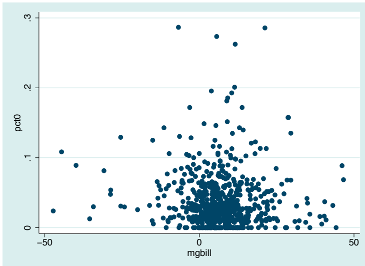
(b) Nominal wage cuts (AR), total wage bill



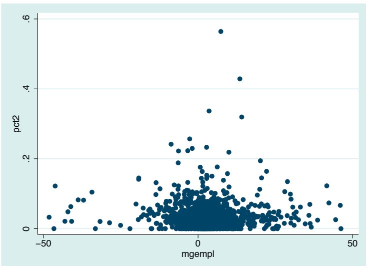
(c) Nominal wage cuts (BP), headcount



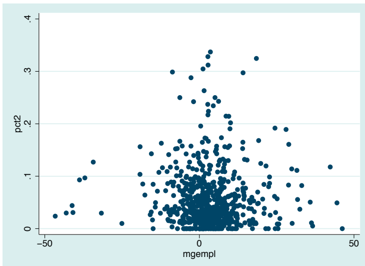
(d) Nominal wage cuts (AR), headcount



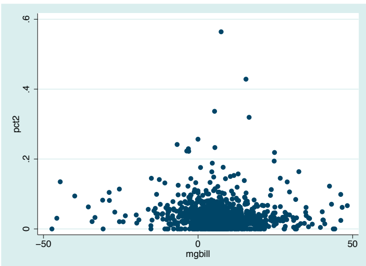
(e) Real wage cuts (BP), total wage bill



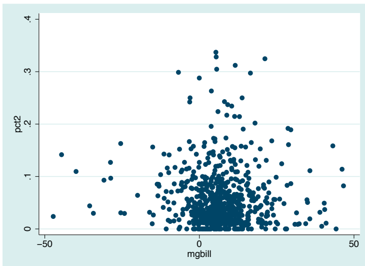
(f) Real wage cuts (AR), total wage bill



(g) Real wage cuts (BP), headcount



(h) Real wage cuts (AR), headcount



Note: BP (AR) refers to results obtained after limiting measurement error using the breakpoint test (AR treatment). *mgempl* and *mgbill* denote the mean annual growth rate of headcount and total wage bill per firm, respectively. The vertical axis captures the fraction of wage cuts. Figures include only non-agricultural and non-energy private firms with a minimum duration of 37 months in-sample and a sufficiently high number of wage changes (equivalent to a minimum of ten employees and one wage change per year and employee on average).