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## Robustness and Real Consequences of Nominal Wage Rigidity

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# CESifo *Working Paper Series*

## ROBUSTNESS AND REAL CONSEQUENCES OF NOMINAL WAGE RIGIDITY

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## ROBUSTNESS AND REAL CONSEQUENCES OF NOMINAL WAGE RIGIDITY

### Abstract

Recent studies found evidence for nominal wage rigidity during periods of relatively high nominal GDP growth. It has been argued, however, that in an environment with low nominal GDP growth, when nominal wage cuts become customary, workers' opposition to nominal cuts would erode and, hence, firms would no longer hesitate to reduce nominal pay. If this argument is valid nominal wage rigidities are largely irrelevant because in a high-growth environment there is little need to cut nominal pay while in a low-growth environment the necessary cuts would occur. To examine this argument we use data from Switzerland where nominal GDP growth has been very low for many years in the 1990s. *We find that the rigidity of nominal wages is a robust phenomenon that does not vanish in a low-growth environment.* In addition, it constitutes a *considerable obstacle to real wage adjustments.* In the absence of downward nominal rigidity, real wages would indeed be quite responsive to unemployment. Moreover, *the wage sweep-ups caused by nominal rigidity are strongly correlated with unemployment* suggesting that downward rigidity of nominal wages indeed contributes to unemployment.

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## 1. Introduction

The extent and the nature of downward nominal wage rigidity is likely to have strong implications for the functioning of the labor market and for questions of monetary policy. There are several reasons why firms may be reluctant to cut nominal wages. Firms may be constrained by efficient nominal wage contracts (MacLeod and Malcomson 1993, Holden 1999), by the existence of nominal loss aversion (Kahneman and Tversky 1979, Genesove and Mayer 1998) or by nominal fairness standards (Kahneman, Knetsch and Thaler 1986, Campbell and Kamlani 1997, Bewley 1999).

In this paper we examine two important unresolved questions in the empirical literature on nominal wage rigidity. First, there is, to our knowledge, no information regarding the rigidity of nominal wages in an *environment of low nominal GDP growth*. This question is important because in an environment with high average nominal growth there is little need to cut nominal wages and, hence, nominal wage rigidity – if it exists – has probably no big real effects. In contrast, in a low-growth environment wage rigidity may well be a binding constraint on wage setting for large segments of the work force. Hence, non-negligible real effects of nominally rigid wages are much more likely in an environment with low nominal GDP growth. However, little is known about the behavior of wages in this situation.

Second, there is little empirical support for the claim that nominal wage rigidity affects the real side of the economy. Yet, such knowledge is important because even if nominal wage cuts are frequently inhibited by nominal rigidity, it cannot be taken for granted that this causes real effects. The reason is that many labor relations are long-term so that the employer could, in principle, smooth the time path of individual wages without affecting the expected marginal costs of labor. For example, in a long-run employment relation a worker could pay for the absence of wage cuts in this year by lower wage increases in future years such that the present value of his labor costs would remain unaffected. From applications of the theory of repeated games to long run labor relations it is, however, known that these relations are characterized by infinitely many equilibria (MacLeod and Malcomson 1989). Therefore, it is far from obvious that the equilibria with wage smoothing are the relevant ones. Ultimately, it is thus an empirical question whether widespread nominal wage rigidity will be associated with real effects.

Due to the lack of data previous studies were forced to examine the existence of nominal wage rigidity in an environment with quite large average growth rates of nominal GDP. The

early studies by McLaughlin (1994) and Lebow, Stockton, and Washer (1995) found little evidence. Further studies by Akerlof, Dickens, and Perry (1996), Card and Hyslop (1996) and Kahn (1997) report more favorable evidence and two recent papers found quite strong evidence for downward rigidity (Altonji and Devereux 1999, Lebow, Saks and Wilson 1999). However, since all these studies used US data from the last four decades and since nominal GDP growth has been quite high during this time period it is difficult, if not impossible, to draw reliable inferences about the behavior of nominal wages in a low-growth environment from these studies. For example, between 1965 and 1998 there are only 3 years with a nominal GDP growth of less than 5 percent in the US. Gordon (1996) and Mankiw (1996) have forcefully argued that it is very problematic to infer from the presence of nominal wage rigidity in a high-growth environment that wages will also exhibit nominal rigidity in a low-growth environment. The reason is that the microeconomic behavior of workers and firms may well change in response to the change in the macroeconomic environment. "The ... attempt, to reason from evidence on nominal wage rigidity in an environment of rapid *positive* average nominal wage change to a hypothetical situation of *zero* average nominal wage change is subject to the Lucas critique. If the macroeconomic environment were different, microeconomic behavior would be different. Nominal wage reductions would no longer be seen as unusual if the average nominal wage was not growing. Workers would not see them as unfair, and firms would not shy away from imposing them." (Gordon, 1996, p. 62). If this argument is valid there would be little reason to be concerned about nominal wage rigidity because in a high-growth environment it is likely to have little impact on employment while in a low-growth environment nominal rigidity will be absent.

The empirical results presented in this paper challenge, however, the above argument. We provide evidence for the presence of strong nominal wage rigidity in an environment with sustained low nominal growth. Our study is based on the Swiss experience between 1991 and 1997. During this period Switzerland experienced inflation rates and real GDP growth rates close to zero in several consecutive years and in three years real growth was even negative. Between 1992 and 1997 nominal GDP growth was always below 2.6 percent. Thus, there was plenty of time for individual agents to adjust their behavior to this macroeconomic environment. Yet, our results indicate that for the whole time period the frequency of true wage cuts is very low and there is a strong and persistent reluctance to cut nominal wages. Moreover, instead of a decrease in the relevance of nominal wage rigidity we even observe an increase over time. For example, in 1991, when nominal GDP growth was still 5.2 percent, nominal rigidity prevented wage cuts for one third of the job stayers and the average

*prevented* wage decrease for these workers was 2.7 percent. In contrast, in 1997, after 5 years of very low nominal growth, the fraction of job stayers who did not receive wage cuts due to nominal rigidity was 62 percent and the average prevented wage decrease for these workers was 11.4 percent. These results leave little doubt that the rigidity of nominal wages is very persistent in these years. Moreover, our results also show that in the absence of nominal wage rigidity real wages would be quite flexible. This indicates that nominal wage rigidity is an important determinant of real wages in an environment with low nominal GDP-growth.

In view of this result it is interesting to ask whether nominal wage rigidity is associated with important real effects. Previous research has either not dealt explicitly with this question or has found no strong effects. At the micro-level Altonji and Devereux (1999) found evidence that workers who are protected by a nominal wage floor are less likely to quit. Whether nominal rigidity also affects layoffs, promotions, and relative wage growth remains, according to these authors, an open question. For the macro-level there seems to be even less evidence. To our knowledge, so far there exists no evidence suggesting that nominal wage rigidity is associated with higher unemployment. The recent paper by Lebow, Saks and Wilson (1999) even poses a so-called micro-macro puzzle. These authors found that despite the large wage sweep-ups caused by nominal wage rigidity in the US in the 1980s the unemployment rate even decreased in this period. Moreover, the paper reports that the measure of nominal rigidity is insignificant in Phillips curve estimates suggesting that nominal rigidity may be unimportant at the macro-level. However, in view of our arguments above it could also be the case that nominal wage rigidity has only small effects in an environment with relatively high nominal growth while it may well cause important real effects in a low-growth environment.

To examine whether nominal wage rigidity is associated with unemployment we have computed the average wage sweep-up caused by nominal rigidity for every canton<sup>1</sup> and every industry in Switzerland in each year between 1991 and 1997. This enables us to see whether the wage increasing effect of nominal rigidity is related to the unemployment rates in the different cantons and industries. Our analysis yields a striking result: In every single canton and in most industries we observe a positive relation between the unemployment rate and the average wage sweep-up caused by nominal rigidity. A plausible interpretation of this result is

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<sup>1</sup> Switzerland is a highly decentralized federation that consists of 26 cantons. The cantons are the primary political units comparable to the federal states in the US.

that the wage sweep-ups indeed represent sweep-ups in labor costs, which induce firms to lay off workers.

The remainder of the paper is structured as follows: Section 2 discusses the characteristics of the Swiss labor market. Section 3 provides descriptive evidence on wage rigidity from personnel files and Section 4 shows descriptive evidence from representative random samples. Section 5 discusses the empirical model of wage changes applied in our paper. Section 6 shows to what extent nominal rigidity persists in our low growth environment and discusses the real consequences on unemployment. Section 7 concludes the paper.

## **2. Characteristics of the Swiss Labor Market**

The Swiss labor market is one of the least regulated and least unionized labor markets in Europe. In Switzerland employers have, for example, the legal possibility to enforce wage cuts by proposing a lower nominal wage to incumbent workers. If a worker refuses to accept the new wage, the law allows the employer to fire the worker. Due to these characteristics the Swiss labor market is perhaps closer to the US labor market than to the labor markets in most other European countries. Despite the employers' opportunities of firing individual workers nominal wage rigidity may nevertheless occur if behavioral forces like nominal fairness standards and nominal loss aversion are sufficiently strong. For our purposes, the most important feature of the Swiss situation is that both inflation and real GDP growth was very low in the period under consideration. Between 1991 and 1993 real GDP growth was *even negative* and between 1994 and 1996 real growth was *always less than 0.5 percent*. Low real GDP growth implies that average real wage growth is moderate. Therefore, structural changes in the economy are likely to be associated with the necessity to cut the real wages of many workers. This downward pressure on the real wages of many workers is translated into downward pressure on nominal wages if inflation rates are low. In Switzerland the rate of inflation was *never above* 1.6 percent between 1993 and 1997. This is a very good environment for the examination of nominal wage rigidity. The downward pressure on the nominal wages of many workers means that firms face a strong temptation to cut the nominal wages of these workers, and, consequently, nominal wage cuts should become more customary. This, in turn, is the ideal situation to examine whether nominal wage rigidity indeed erodes. When, if not in this situation, can we expect an erosion of nominal wage rigidity? On the other hand, if nominal rigidity persists, this is the ideal environment for the

study of the real consequences of nominal rigidity because nominal rigidity prevents many real wage cuts.

It is instructive to compare the macro-environment in this study with the macro-environment in previous studies of nominal wage rigidity (see Table 1). In our study the median nominal GDP growth is 2.2 percent during the sample years while in the other studies it varies between 5.7 percent and 11.3 percent. Moreover, to study the persistence of nominal rigidity in a low nominal growth environment it is necessary that nominal growth rates are low in several consecutive years. It is unlikely that nominal rigidity erodes just because nominal GDP growth drops below, say, 3 percent in a single year. Table 1 shows that previous studies could not address this question because – except for the study by Akerlof, Dickens and Perry (1996) - nominal growth was *never* below 5.2 percent in two or more consecutive years. In contrast, in our sample period it was *always* below 5.2 percent. Likewise, in all studies, including the one by Akerlof, Dickens and Perry (1996), nominal growth was *never below* 2.6 percent in two or more consecutive years while in our study this was the case in 6 consecutive years.<sup>2</sup>

### 3. Descriptive Evidence from Personnel Files

The ideal data set for examining nominal wage rigidity would be a representative sample of firms' personnel files including precise information on wages, individuals' productivity and other individual characteristics. Unfortunately, to our knowledge there is no study with such a data set. Although less informative it is still useful to examine non-representative firm-level information.<sup>3</sup> We obtained personnel records from a large and a medium-sized Swiss firm. Firm A is a large firm in the service industry with approximately 10,000 employees. The available personnel records cover the period from 1993 to 1999. For both firms wages are calculated as total compensation divided by hours in the contract. Average wage growth in Firm A was 3.8 percent (s.d.: 5.3 percent). Firm B is a medium-sized firm in the service industry with a declining activity in manufacturing. The records of Firm B start in 1984 and end in 1999. In this firm employment drops from about 2000 in the 1980s to 1000 in 1998,

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<sup>2</sup> In the Akerlof et al. study nominal GDP growth was 3.9 percent in 1960 and in 1961. Then it rose to 7.5 percent. Thus this is also not the kind of environment where one would expect nominal rigidities to erode. The lowest nominal growth rate in the US between 1960 and 1998 was 3.2 percent in 1991.

<sup>3</sup> Interesting evidence from the personnel files of a large firm is reported in Baker, Gibbs and Holmström (1994) and Wilson (1999).



from where it started to rise again<sup>4</sup>. Wages grew on average by 5.7 percent (s.d.: 5 percent) in Firm B.

Figure 1 displays the distribution of wage changes (measured in log wage differences) in the two firms for the periods 1993 – 1999 and 1984 – 1998, respectively. The striking feature of both distributions is, that there are *almost no wage cuts*. In Firm A (N=35,779), only 1.7 percent of all observations are wage cuts. In Firm B (N=20,236), the fraction is even lower (0.4 percent). Both distributions exhibit a discontinuity at zero that could hardly be more pronounced. If we restrict our attention to the years with low nominal GDP growth the picture is essentially the same. Between 1993 and 1997 average nominal wage growth was also 3.8 percent in Firm A and the percentage of negative wage changes was 1.5. Firm B experienced 4.2 percent average nominal wage growth in this period and the percentage of wage cuts was again 0.4 percent. Therefore, irrespective of the period considered nominal wage cuts are extremely rare in these firms. These data are, thus, certainly consistent with the view that employers are reluctant to cut nominal wages. Yet, it is unclear to what extent the wage change regularities in these firms are representative for the whole economy.

#### 4. Descriptive Evidence from Representative Samples

To get representative information on the extent of nominal rigidity we examine two large data sets. The first data source is the Swiss Labor Force Survey (SLFS) for the years 1991 – 1998. The SLFS is a rotating panel that follows individuals for five years. In total, the SLFS provides 21,144 wage change observations. The second data source is a large random sample from the Social Insurance Files (SIF). The SIF contains information about all employees in Switzerland. This sample gives us 140,628 observations of wage changes and covers essentially the same time period as the SLFS-data<sup>5</sup>. The major advantage of examining both data sets is that this provides a very useful robustness check of our results. Below we will show that both data sources have their specific advantages and disadvantages. Hence, if both

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<sup>4</sup> The reason is that Firm B closed its manufacturing plants, which was accompanied with a large employment decrease, many of which were dismissals.

<sup>5</sup> The Social Insurance Files are December to December data, while the SLFS is conducted in May. Hence, referring to wage changes in e.g. 1993, we mean wage changes between May 1993 and May 1994 for the SLFS and wage changes between December 1992 and December 1993 for the SIF.

data sources nevertheless lead to similar results we can be more confident that the results are robust.

In both data sources we consider non-self employed individuals who stayed with the same firm for at least one year. We call these individuals “job stayers”. We trimmed both samples by excluding all observations with an absolute wage change above 50 percent. This is motivated by the concern that for job stayers wage changes exceeding 50 percent are utterly implausible. In both data sets we lose approximately 3 percent of the observations when we apply this criterion. However all our conclusions remain qualitatively identical and quantitatively very similar if we use the whole sample for our estimates<sup>6</sup>. For the SLFS-data our measure of wages is total compensation (net of social security contributions) divided by hours specified in the labor contract. For the SIF-sample we use a different measure of wages as discussed below.

The advantage of the SLFS is that it provides extensive information on the characteristics of individuals like, e.g., tenure, labor market experience, education levels, gender, age, nationality, etc. The disadvantage is that surveys are likely to be distorted by reporting errors. The advantage of the SIF-data is that all financial transactions between firms and workers are recorded in the Social Insurance Files. Hence, reporting error is not an issue. The earnings information obtained from the SIF is accurate. In addition, the SIF-sample is comfortably large. Since the SIF data covers the same period of time as the SLFS-data, we can replicate the empirical analysis we conduct with the SLFS. We should also mention that the SIF-data have three problems. First, it is impossible to identify job stayers with absolute certainty. We only consider those workers in the SIF-sample who were insured by the same local social insurance agency in two consecutive years since these are most likely to be job stayers. However, if a worker moves to another employer, but both employers are associated with the same local agency, the individual may still be included in our sample. Thus, we may wrongly include job movers in our SIF-sample, which could understate the true degree of nominal wage rigidity. Second, we have precise information on total compensation per year but not on hours worked. Our measure of observed wage changes in the SIF-sample is, therefore, given by the changes in total compensation per year. Hence, temporary variations in hours, which arise, e.g., through different overtime in two years, look like a ‘wage change’ in our sample.

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<sup>6</sup> We conducted all estimates with the full sample, too. If anything, nominal rigidities are more pronounced when we use this sample. These estimates are available on request.

As we will illustrate below, this can generate a substantial number of observations that look like a wage cut but which are indeed reductions in actual hours worked. This is particularly important for the time period considered because in a recession firms may use working time reductions as an alternative to nominal cuts. Third, the available worker characteristics in the SIF-sample are not the same as in the SLFS. They include age, nationality, gender, details on the agency that recorded the payment and the period of time to which it applies.

Figure 2 summarizes the distribution of nominal wage changes (measured in log wage differences) for job stayers in Switzerland between 1991 and 1997. Consider first the figure on the left which displays the histogram obtained from the SLFS. This histogram exhibits the following properties:

1. There is a *spike at zero*: The largest bin is the one containing no and small, but positive nominal wage changes (between zero and 2 percent).<sup>7</sup>
2. There is an *asymmetry* in the distribution of wage changes. Negative wage changes are observed less frequently than positive wage changes.
3. Despite the asymmetry there is a *considerable fraction of negative wage changes*.

Compare this to the right panel of Figure 1a, which is based on the SIF data using identical bins. Three features deserve to be mentioned here:

1. The SIF distribution exhibits less dispersion, i.e., it is more centered around zero than the SLFS distribution. While, e.g., 59 percent of all observations in the SIF are between zero and 10 percent, the corresponding figure for the SLFS is only 45 percent.
2. The asymmetry between positive and negative wage changes is much more pronounced in the SIF sample. There is a striking discontinuity around zero and the pile-up of observations just above zero is very pronounced.
3. The fraction of negative wage changes is considerably smaller in the SIF-sample.

Table 2 provides additional information on wage changes in our two data sources together with the inflation rate (measured by CPI changes) and real GDP growth. The table shows that the sharp decrease in the rate of inflation at the beginning of the period considered is associated with more observed wage cuts and more zero wage changes in the SLFS. The

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<sup>7</sup> For the exact fraction of zero wage changes see Table 2.

fraction of job stayers with a zero nominal wage change rises from 5 percent in 1991 to 15 percent in 1997. The fraction who reported wages that implied wage cuts is, in general, quite high. It also rises from 20 percent in 1991 to 33 percent in 1997. Interestingly, however, the fraction of workers with wage cuts is always lower in the SIF-sample than in the SLFS-sample. This suggests that reporting error is important in the labor force survey: Imagine that the distribution of true wage changes has no, or only a few, negative entries. Assume further that reporting error is important. Then, as the distribution moves closer to zero over time, reporting error creates a larger number of negative observations. Therefore, we observe more wage cuts in the SLFS sample. Note that the fact that we cannot control for hours variation in the SIF sample only strengthens this argument because it is likely to produce false negatives in this sample, too, a point to which we return below.

Figure 3 shows the evolution of the distribution of log wage differences over time, using the SIF sample. The sequence of distributions conveys the impression that the decline in inflation is associated with a rise in downward rigidity. Consider, first, the three panels for 1991, 1992, and 1993. In these years the distribution is – except for the small spike at zero - relatively symmetric around its median. The bins to the left and to the right of the median are of similar size. Compare this to the distribution of wage changes in the low inflation years 1995 to 1997, where the median is much closer to zero. In these years there is a sharp discontinuity at zero and the distribution also exhibits a pronounced asymmetry around zero. Note also that there is only a relatively small increase in the frequency of negative wage changes during these years.

The upshot of the descriptive evidence in Table 2 and Figures 2 and 3 can be summarized as follows: The asymmetry in the distribution of wage changes and the spike at zero may be interpreted as an indication of nominal wage rigidity. Support for this interpretation is also provided by the fact that the asymmetry becomes much more pronounced over the years. However, the relatively large fraction of observed wage cuts in the SLFS and the SIF provide much less convincing evidence for nominal wage rigidity than the descriptive evidence from the personnel files. This raises the question whether the non-negligible number of observed wage cuts represent true wage cuts or whether they are mainly the result of reporting error (in the SLFS) or of unobserved hours variation (in the SIF). The much smaller number of observed wage cuts and the generally smaller dispersion of wage changes in the SIF suggests

that reporting error is a serious problem at least in the SFLS.<sup>8</sup> Thus, many of the observed wage cuts in the SLFS might be spurious. In addition, the absence of a direct measure for working time in the SIF may pollute the SIF data in a similar way as reporting error pollutes the SLFS data.

In order to gain some insights into the potential role of unobserved variations in working time we take advantage of the fact that the personnel file of Firm B provides precise information on overtime payments for each individual. Thus, we can compute the distribution of wage changes in Firm B in the presence and in the absence of controlling for overtime payments. The results are presented in Figure 4. The first panel reproduces the true distribution of wage changes in Firm B, i.e., overtime payments are not included, for the period 1993 to 1998. We constrain the sample, because information on overtime payments is only available for this period. In the second panel, we deliberately add overtime payments to the compensation to calculate 'polluted' wage changes as we would observe them in the SIF. The distribution of 'wage' changes in the second panel now contains a sizeable fraction of spurious wage cuts (7.6 percent) and is less centered around zero compared to the true distribution. While this exercise does not replicate the moments of the SIF-sample perfectly, it suggests that unobserved working time variations may well cause a sizable fraction of spurious wage cuts in the SIF-sample. Note also that average wage growth is relatively high in Firm B, hence unobserved hours variation would generate even more false negatives in a low-growth firm.

## 5. An Empirical Model of Wage Changes

The upshot of the previous discussion is that we need an econometric model that explicitly allows for the presence of measurement error so that one can separate true wage changes from wage changes that merely reflect reporting error or reductions in actual hours worked. The approach by Altonji and Devereux (1999) neatly accomplishes this. The general idea behind their approach is that there may be reasons – e.g., efficient nominal wage contracts, nominal fairness standards and nominal loss aversion – that render nominal wage cuts costly for the firms. Therefore, firms will not implement all desired wage cuts and, as a consequence, there

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<sup>8</sup> The fact that the distribution of *positive* wage changes in the two firms above is much less dispersed than the distribution in the SLFS is also compatible with this conjecture. In both firms 89 percent of all observations are between zero and ten percent while in the SLFS only 45 percent of the observations are in this range.

will be a difference between the desired or “notional” wage cuts and actually implemented wage cuts. However, the larger the notional wage cut the more likely it is that the benefits will outweigh the costs. Hence, there may exist a threshold value  $\alpha$  for the implementation of actual wage cuts: If the notional cut is below  $\alpha$  the firm will not implement the cut but if the notional cut is above  $\alpha$  the pay reduction will be implemented. Our main focus is to estimate the parameter  $\alpha$ . Nominal inertia may, however, not only prevent wage cuts but it may also reduce the actual cut relative to the notional cut. The model below also allows for this possibility. It is a model of wage changes that incorporates the determinants of wages in the absence of rigidities and allows for measurement error in the data.

The general structure of the estimated model is as follows:

$$\Delta y_{it} = \begin{cases} x_{it}'b + e_{it} + m_{it} & \text{if } x_{it}'b + e_{it} \geq 0 \\ m_{it} & \text{if } -\alpha \leq x_{it}'b + e_{it} < 0 \\ x_{it}'b + \lambda + e_{it} + m_{it} & \text{if } x_{it}'b + e_{it} < -\alpha \end{cases} \quad (1)$$

where  $\Delta y_{it}$  is the *observed* log nominal wage change of individual  $i$  in period  $t$ ,  $x_{it}'b + e_{it}$  is the *notional* nominal wage change that would be implemented in the absence of downward nominal wage rigidity,  $x_{it}$  is a set of observable variables that are likely to affect wage growth,  $e_{it}$  represents the usual error term, and  $m_{it}$  denotes the measurement error, which can be interpreted as reporting error in the SLFS and unobserved hours variation in the SIF.  $\lambda$  measures the extent to which wage cuts, if implemented, are constrained. As modeled above,  $\lambda$  is a constant reduction of the wage cut relative to the notional cut. In the absence of nominal inertia and measurement error ( $\alpha = \lambda = m_{it} = 0$ ) wage changes are solely given by  $x_{it}'b + e_{it}$ . In our empirical estimates below  $x_{it}$  contains variables like labor market experience, age, tenure and observable skills of worker  $i$ . The inclusion of these variables into our wage growth equation is suggested by many papers (e.g., Topel 1991). It is generally recognized that the experience-earnings profile and the tenure-earnings profile is concave, i.e. wages grow at a decreasing rate with experience and tenure. Likewise, several studies indicate that wage growth is different for different categories of workers (e.g. Baker, Gibbs and Holmström 1994). In addition to the above variables, we also included the change in the regional unemployment rate, year dummies, industry dummies and a dummy that captures the nationality status of  $i$ . The change in the regional unemployment rate is included because a rise in unemployment is likely to constrain wage increases. We also experimented with the level of the unemployment rate in our wage growth equation. As an additional control we also

included the firm size.<sup>9</sup> In our estimates with the SIF sample we use a worker's age as a proxy for experience. In addition, a foreigner dummy variable, as well as an interaction term with log age, captures the systematic differences in experience and job status between Swiss employees and employees from other countries.

In the presence of nominal inertia and measurement error observed wage growth is not only determined by  $x_{it}'b + e_{it}$ . In addition,  $\alpha$ ,  $\lambda$  and the standard deviation of  $m_{it}$  are important. Therefore, observed wage changes can, in principle, fall into one of the following three regimes:

(i) If the notional wage change  $x_{it}'b + e_{it}$  is positive there are no forces that inhibit this wage change, i. e., we observe  $x_{it}'b + e_{it} + m_{it}$  in the data and the likelihood of this occurring is

$$f_{e+m}(\Delta y_{it} - x_{it}'b | x_{it}'b + e_{it} > 0)$$

where  $f_{e+m}(\cdot)$  is the density of the sum of  $e$  and  $m$ .

(ii) If  $x_{it}'b + e_{it}$  lies between  $-\alpha$  and zero, the firm will not cut the worker's wage but give him a pay freeze instead. The observed 'wage change' is then entirely due to unobserved variation. Hence the likelihood of falling in this regime only depends on the distribution of  $m$  and is given by

$$f_m(\Delta y_{it} - x_{it}'b | -\alpha < x_{it}'b + e_{it} < 0)$$

Note that we do *not* assume that sufficiently small notional wage cuts result in a pay freeze. Whether a notional wage cut is executed or not depends on the value of  $\alpha$  which is jointly estimated with all other parameters of the model. Only if the estimate of  $\alpha$  is positive, small notional wage cuts will be identified as measurement error.

(iii) If the notional wage cut is larger than  $\alpha$ , the firm will implement the wage cut although the size of the cut may be smaller than the notional cut by the magnitude  $\lambda$ . The conditional density for this event is

$$f_{e+m}(\Delta y_{it} - x_{it}'b - \lambda | x_{it}'b + e_{it} < -\alpha)$$

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<sup>9</sup> A recent study by Winter-Ebmer and Zweimüller (1999) reports firm-size effects for Switzerland that are comparable in size to those in the US.

Since it cannot be observed which regime generated a particular observation, the likelihood of an observation sums up to

$$\begin{aligned}
l_{it} &= f_{e+m}(\Delta y_{it} - x_{it}'b \mid x_{it}'b + e_{it} > 0) \cdot \Pr(x_{it}'b + e_{it} > 0) \\
&+ f_m(\Delta y_{it} - x_{it}'b \mid -\alpha < x_{it}'b + e_{it} < 0) \cdot \Pr(-\alpha < x_{it}'b + e_{it} < 0) \\
&+ f_{e+m}(\Delta y_{it} - x_{it}'b - \lambda \mid x_{it}'b + e_{it} < -\alpha) \cdot \Pr(x_{it}'b + e_{it} < -\alpha)
\end{aligned} \tag{2}$$

We assume that  $e$  and  $m$  are i.i.d. normal and estimate the parameters by maximum likelihood.<sup>10</sup>

One attractive feature of this approach is that it nests both the case of perfect wage flexibility and the case of perfect wage rigidity. As  $\alpha \rightarrow 0$  (and  $\lambda \rightarrow 0$ ) there is no downward wage rigidity. In this case the model collapses to a simple OLS regression of  $\Delta y_{it}$  where only the sum of  $e$  and  $m$  is identified. If, at the other extreme,  $\alpha \rightarrow \infty$ , there are no true wage cuts and the third regime drops out. Hence, the model nests both extreme cases, and any intermediate one. It provides joint estimates of the threshold value  $\alpha$ , the variance of the distribution of  $e$  and  $m$ , and of  $\lambda$ . If we estimate an  $\alpha$  that is close to zero, most observed wage cuts represent true wage cuts. However, if we estimate large values of  $\alpha$ , many observed wage cuts do not represent true wage cuts, that is, measurement error is more pervasive.

A second feature of the model is that we can estimate the determinants of  $\alpha$ . Instead of imposing the restriction (as in equation (1)) that  $\alpha$  is the same for all workers in all years we can allow for year-specific  $\alpha$ 's or for different  $\alpha$ 's for different groups of workers. In particular, by estimating year-specific  $\alpha$ 's we can observe whether  $\alpha$  is lower in low-inflation years, which would provide direct evidence for the validity of the conjecture put forward by Gordon (1996) and Mankiw (1996). Finally, by allowing variations of  $\alpha$  across different categories of workers we can also examine, e.g., whether  $\alpha$  is different for full-time and part-time workers or for job stayers and job movers.

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<sup>10</sup> In an appendix, that is available on request, we derive the explicit expression for (2), that can be directly used for estimation purposes.



## 6. Results

In this section, we discuss the results obtained by estimating the above model. We first present the overall tests for the presence of downward nominal wage rigidity. We then evaluate the stability of these estimates as inflation becomes very low. Next, we assess the implications of the model for different types of workers and the extent to which downward wage rigidity prevents real wage cuts. Finally, we examine the consequences of nominal wage rigidity for regional and industry-specific unemployment.

We estimate the basic model (1) in two versions. In the ‘continuous’ model, we assume that measurement error is normally distributed. That is, everybody is assumed to make mistakes when reporting earnings to the labor force survey, and, in the SIF-sample, all workers are assumed to have unobserved variation in hours. In the ‘mixed’ model we allow for the case that, in every year, a fraction  $p$  (that will be estimated) of the individual data has no measurement error, but that the rest of the sample draws a normally distributed error. This amounts to saying that in the SLFS, a fraction  $p$  of all respondents states the correct income, but the rest makes normally distributed errors. In analogy, in the SIF sample, a fraction  $p$  of all individuals has no variation in hours between the previous and the current year.

The key difference between the continuous and the mixed model is the way the observations with  $\Delta y_{it} = 0$  are treated. The mixed model is more appropriate for the SIF sample since we know from several other data sources (e.g. the SLFS) that a non-negligible fraction of the people did not work overtime in two consecutive years. Thus, for this fraction of the people in the SIF sample an observed earnings change of zero represents a true wage freeze. However, the mixed model could be more problematic to use with the SLFS, if rounding by respondents causes many of the observations with a zero wage change, as claimed e.g. in Smith (1999). The mixed model will treat a fraction of these observations as true wage freezes, whereas they could also be small wage increases which are subject to a rounding error. Hence, the mixed model will tend to overstate the extent of nominal rigidities in the SLFS if rounding is a problem. This is not the case in the continuous model, which treats all observations around zero alike. If rounding is symmetric, the continuous model will not be biased in a particular direction. Hence, by comparing the estimates across both methods and both data sets, we gain insights into how important rounding errors are and by how much they affect our conclusions.

## 6.1 Are Wages Flexible?

The basic results for both samples are displayed in Table 3. For both samples we estimated the mixed model with and without  $\lambda$ . Consider first the estimates from the SLFS on the left side of Table 3. We find strong evidence for nominal wage rigidity in both, the continuous and the mixed, model specifications.  $\alpha$  is large and estimated with a tiny standard error.<sup>11</sup> In both models, the notional wage cut must be substantial – at least 22 percent - in order to induce the firm to actually cut an employee's wage. The third SLFS-regression indicates that nominal wage rigidity does not only prevent wage cuts but that it also reduces the size of the cuts actually occurring. This follows from the positive and highly significant value of  $\lambda$ . If a wage cut occurs nominal rigidity reduces the cut by 7.8 percent.

What are our estimates for the determinants of the notional wage changes? We find that a rise in experience lowers wage growth.<sup>12</sup> The estimated coefficient is negative and highly significant. Increasing labor market experience from one to ten years decreases wage growth by 2.7 percent. Table 3 also shows that a rise in tenure decreases wage growth. The tenure effect is roughly half the size of the experience effect. Note that these estimates of the tenure and the experience effect control for the potentially confounding impact of nominal wage rigidity because they take the truncation of wage changes below nominal zero (and above  $-\alpha$ ) into account. Since our estimates of  $\alpha$  and  $\lambda$  indicate a substantial amount of nominal inertia, estimates of the tenure and the experience effect that do not control for nominal inertia are likely to be confounded.<sup>13</sup> Table 3 also indicates that the position of workers in the firm's hierarchy is important for wage growth. If the individual is a superior, wage growth is higher while if the individual is a member of higher management wage growth is lower.<sup>14</sup>

We also find evidence that the change in the regional unemployment rate causes a substantial reduction in nominal wage growth. Our estimates imply that a one percentage point increase in unemployment growth reduces wage growth by at least 0.6 percentage points. Hence wages would indeed be quite flexible in the absence of downward wage rigidity. We also

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<sup>11</sup> All standard errors are adjusted for clustering on cantons and years.

<sup>12</sup> In order to avoid awkward polynomials we only included log experience and log tenure to capture the curvature.

<sup>13</sup> If we conduct OLS regressions not controlling for the presence of nominal wage rigidity, the tenure and the experience profiles are, in general, flatter.

<sup>14</sup> A superior is defined as an employee who has the power to direct the activities of several other employees without being a member of higher management.

experimented with the level of the regional unemployment rate in our regressions. However, while the *change* in the unemployment rate has a sizable and significant impact on wage growth, the coefficient of the unemployment rate is always rather small and insignificant. We would like to emphasize here that, if one does not control for the presence of downward rigidity, the effect of unemployment changes on wage changes is not significant: In OLS-regressions, that disregard the potential truncation of wage changes below nominal zero (and above  $-\alpha$ ), the coefficient of unemployment changes is not significant, i.e., one is led to the wrong conclusion that unemployment growth does not affect wage growth. This shows the importance of taking into account the presence of nominal rigidity.

The extent of measurement error in our survey data is substantial although it is lower than expected. Our estimate of the standard deviation of the measurement error in the sample ( $\sigma_m$ ) is roughly 7 percent. This is low compared to what validation studies of labor force surveys found for the US (see Angrist and Krueger, 1999 for a survey). The standard errors obtained from validation studies for the US are never below 10 percent, and sometimes considerably larger.

Notice also that the two different specifications of measurement error do not alter the qualitative conclusions with respect to the extent of nominal wage rigidities. Previous studies (McLaughlin, 1994; Smith, 1999) have argued that the apparent rigidity of wages is largely due to rounding by individuals. We find weak evidence for this claim. The estimated  $\alpha$  is 0.214 and 0.268 for the continuous and the mixed model, respectively. The small increase in  $\alpha$  in the mixed model is compatible with the view that rounding in labor force surveys may somewhat overstate wage rigidity. Yet, this effect is small relative to the overall extent of nominal rigidity.

We now turn to the SIF sample. To preserve comparability with the SLFS, we do not use the whole SIF sample for the estimates in Table 3. The reason is that we only know either the residence or the industry affiliation of individuals in the SIF. However, due to the large number of different social insurance agencies in Switzerland, splitting the sample in an appropriate way can solve this problem. Some agencies only enroll individuals from a particular canton. Individuals enrolled in these agencies form the 'cantonal' sample (N=58,297). Other agencies only enroll employees from a particular industry (the 'industry' sample, N=55,884). The remaining observations in the SIF stem from agencies that cannot be matched to any canton or industry and they are, therefore, not considered in the empirical analysis. Since we used the change in the cantonal unemployment rate for the SLFS-estimates

comparability induced us to also use this regressor for the SIF-estimates. Therefore, we used only the SIF-data from the cantonal sample for the estimates presented in Table 3. However, our estimates of  $\alpha$  and  $\lambda$  are almost the same for the industry sample.<sup>15</sup>

Table 3 shows that the point estimates for  $\alpha$  with the cantonal SIF-sample are again quite large and have only a very small standard error. The estimates range from 0.183 for the continuous model to 0.268 for the mixed model when we included  $\lambda$ . While the estimated values of  $\alpha$  are now slightly smaller the estimate of  $\lambda$  is slightly larger in the SIF-sample. As in the SLFS, the estimated  $\alpha$  is larger in the mixed model than in the continuous model, but the difference is not large. This time, however, rounding error in the data is not an issue, and the estimates of the mixed model can be taken at face value. Thus, taken together, the SIF-data also reveal a considerable amount of nominal rigidity.

The effect of unemployment on wages in the SIF-sample is very similar to the SFLS sample. Again, the point estimate of the coefficient of the (percentage point) change in regional unemployment is negative and highly significant. This implies that, in the absence of nominal wage rigidities, a rise in the unemployment rate by one percentage point would reduce wage growth by at least 0.7 percent. This further corroborates our previous conclusion based on the survey data, that wages would be quite flexible and responsive to unemployment in the absence of nominal wage rigidities. With regard to the other determinants of the notional wage changes we find that wage growth strongly declines with age as indicated by the negative and highly significant coefficient on log age. Table 3 also shows that wage growth is smaller for foreign workers which reflects systematic differences in job status between Swiss and Non-Swiss employees. In addition, the positive coefficient on the interaction term indicates that wage growth declines less for Non-Swiss employees. Finally, Table 3 indicates that the unobserved variation in hours has a standard deviation  $\sigma_m$  of roughly 4 percent.

The results in Table 3 also have direct implications for the frequency of true wage cuts and the fraction of workers who did not receive wage cuts although the notional wage change was negative. According to the SLFS-estimates between 2.3 and 5.2 percent of all wage change

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<sup>15</sup> The estimates for the industry sample of the SIF are presented and discussed in the appendix, where we show that all conclusions also hold for this sample.

observations are true wage cuts (see row 3 in Table 3).<sup>16</sup> For the SIF sample between 4.7 and 6.4 percent of the observations represent true wage cuts. In contrast to the relatively small number of true wage cuts the fraction of workers who did not receive wage cuts although their notional wage change was negative is very large. It varies between 44.7 and 59 percent (see row 4 in Table 3). These numbers also provide strong evidence for nominal wage rigidity.

## 6.2 Are Nominal Rigidities Easily Malleable?

This section examines whether nominal wage rigidity tends to vanish in the course of a period with persistently low nominal growth. A natural way to test for this is to estimate year-specific  $\alpha$ 's. In addition, we calculate the frequency of true wage cuts and the fraction of workers affected by nominal rigidity for each year. Remember that inflation declined from roughly five percent in 1991 to zero percent in 1997. Real growth was slightly negative between 1991 and 1993 and slightly positive between 1994 and 1996. If nominal rigidity becomes weaker over time we should observe a declining  $\alpha$ . Table 4 provides our estimates of year-specific  $\alpha$ 's relative to the value of  $\alpha$  in 1991. Panels (a) and (b) in Figure 5 present the corresponding graphs.<sup>17</sup> It is remarkable that all estimates of  $\alpha$  are highly significant. In contrast to the prediction that  $\alpha$  declines over time the decrease in inflation between 1991 and 1993 is associated with a significant increase in  $\alpha$ . Moreover, between 1993 and 1997  $\alpha$  is also weakly increasing in both samples. Thus, there is no evidence that nominal rigidity becomes less important in the course of a relatively long period of low nominal growth. This is also confirmed by panels (c) and (d) in Figure 5, which plot the fraction of workers affected by nominal rigidity together with the estimated frequency of actual wage cuts. Irrespective of the data source, we get the same picture: There is essentially no or only a minor increase in the frequency of true wage cuts during the sample period. Yet, in contrast to the prediction, that nominal rigidity becomes less important over time, the fraction of workers who did not receive wage cuts due to nominal wage rigidity rises sharply in both samples. Thus, the long period of low nominal growth exacerbates the relevance of downward wage rigidity.

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<sup>16</sup> The frequency of wage cuts is obtained by calculating the probability that an individual received a wage cut and averaging over individuals. The probability that individual  $i$  received a wage cut in year  $t$  is given by  $\Pr(\text{wage cut}) = \Pr(x_{it}'b + e_{it} < -\alpha) = \Phi[-(x_{it}'b + \alpha)/\sigma_e]$ , where  $\Phi(\cdot)$  is the c.d.f. of the standard normal distribution and  $\sigma_e$  is the estimated standard deviation of  $e$  as indicated in Table 3.

<sup>17</sup> Recall that the SLFS is based on May-to-May data. Hence, we use May-to-May changes in the CPI measure of inflation. Analogously, we use December-to-December CPI changes whenever we use the SIF data. Therefore, inflation rates differ somewhat between panel (a) and (b) of Figure 5.

### 6.3 Who is most affected?

There are various reasons why nominal rigidity is likely to be different for different categories of workers. First, fairness standards that render nominal wage cuts costly are likely to arise through a history of repeated interactions between the worker and the firm. In the absence of such a history employers are less likely to feel constrained by fairness standards. Therefore, it seems much easier to impose pay cuts on job movers than on job stayers. Second, for a firm the loyalty and work morale of full-time workers is, in general, more important than the loyalty and work morale of part-time workers. Moreover, the relevance of fairness standards is likely to be more important for workers with a greater attachment to the firm. Therefore, one would expect more wage rigidity among full-time workers. A third reason is related to the theory of efficient nominal wage contracts (MacLeod and Malcomson 1993). These contracts serve the purpose to protect the relation-specific investments of firms and workers efficiently. They are therefore more important for those workers who have more firm-specific human capital. Job stayers have, by definition, more firm-specific human capital than job movers. In addition, it seems likely that full-time workers have more specific human capital than part time workers so that efficient nominal wage contracts are more important for full-time workers. Therefore, the theory of efficient nominal wage contracts also suggests that nominal wage rigidity is more important for job stayers and for full-time workers.

The results regarding the differences between full-time and part time job stayers are displayed in Table 5. As argued above we find large differences between the two groups of employees. For part-time job stayers, the estimated  $\alpha$  is between 17 and 20.5 percent, depending on the specification of the measurement error. A comparison of the estimates for full-time and part-time stayers indicates that a much higher notional wage cut is needed to induce firms to cut the wages of full-time stayers. The point estimate for full-time stayers is between 24 and 30 percent, and the difference to the part-time stayers is significant. This difference is also reflected in the estimated frequency of wage cuts. Both models predict that full-time job stayers experience true wage cuts in less than 4 percent of the cases while part-time job stayers have to accept wage cuts more frequently: their wages are reduced in 7.3 to 10.5 percent of the cases. Despite the higher frequency of wage cuts among part-time stayers this group is also quite strongly affected by downward rigidity. As Table 5 shows between 44 and 50 percent of the part-time stayers did not get wage cuts due to nominal rigidity. For full-time stayers the fraction of affected workers is even higher.

The estimates in Table 5 are based on observations about job stayers only. In a further step we added job movers to the sample to evaluate whether nominal rigidities are confined to job stayers only. Since we are also interested in the impact of different reasons for movements between jobs several waves of the SLFS cannot be used because the relevant information is only contained in the last three waves. Therefore our sample shrinks to 10,708 observations. Our estimates regarding the differences between movers and stayers are presented in Table 6.

Overall, job movers are much more likely to experience a wage cut than job stayers. While the threshold value for wage cuts is between 30 and 40 percent for full-time stayers it is slightly above 10 percent for job movers (see row 3-5 in Table 6). Moreover, while at most 3.3 percent of the full-time stayers experience wage cuts (see row 7 in Table 6) the frequency of estimated wage cuts is much higher for job movers. Between 8 (in the mixed model) and 13 percent (in the continuous model) of those job movers voluntarily quitting their job experience wage cuts. Dismissed job movers have to accept wage cuts even more frequently. Between 16.6 and 18.1 percent of dismissed movers experience wage cuts. A final observation is that the difference between full-time and part-time workers, which is rather big for job stayers, disappears for job movers (see row 6 in Table 6). For job movers it does not matter whether they work part-time or full-time because the fact that they move between jobs is already associated with much more flexible wages. Thus, taken together, the evidence in this section is consistent with the above arguments that predict differences in nominal rigidity across these groups of workers. This lends support to the view that fairness standards and efficient nominal wage contracts are relevant factors behind the rigidity of nominal wages.

#### 6.4 The Consequences of Downward Nominal Wage Rigidity

Our estimates provide two further pieces of information. First, we can calculate the average notional wage cut  $x'b + e$  that did not occur because  $-\alpha < x'b + e \leq 0$  holds. For brevity, we call this the average prevented wage cut and denote it by  $E(\Delta w_{it}^* | -\alpha < \Delta w_{it}^* \leq 0)$ , where  $\Delta w_{it}^* \equiv x'b + e$ . Second, we can compute a measure of the average wage sweep-up due to downward wage rigidity  $E(\Delta w_{it} - \Delta w_{it}^*)$  where  $\Delta w_{it}$  is the true wage change. The average wage sweep-up can be interpreted as the increase in average labor costs due to downward rigidity of nominal wages. If this interpretation is correct a rise in the average wage sweep-up should be associated with a rise in unemployment or a decline in employment in the different industries and cantons.

Panels (e) and (f) in Figure 5 exhibit the evolution of  $E(\Delta w_{it}^* | -\alpha < \Delta w_{it}^* \leq 0)$  for the job stayers. The panels show that downward nominal wage rigidity has less impact at the beginning of the period considered when inflation was still relatively high. At this time the prevented wage cut was between 3 percent (in the SIF sample) and 5 percent (in the SLFS). This changes substantially in years where inflation rates are closer to zero. From 1993 onwards, the prevented wage reductions are, on the average, 10 percent or more. This shows again that nominal rigidity became increasingly important during the period of low nominal growth.

We now turn to the question whether downward nominal wage rigidity has consequences for the real side of the economy. For this purpose we compute the average wage sweep up  $E(\Delta w_{it} - \Delta w_{it}^*)$  for every canton and every industry and relate them to the unemployment *rate* in the cantons and the industries.<sup>18</sup> Since there are large variations in the level and in the changes of unemployment across cantons and across industries it is interesting to examine to what extent variations in the wage sweep-up can explain these variations in unemployment. Note that in our estimate of the wage sweep-up the *rate* of unemployment is *not* an explanatory variable. This is important because otherwise there would be a relation between wage sweep-up and unemployment by construction.<sup>19</sup>

In Figure 6a we plotted the relation between average wage sweep-up and unemployment rate separately for each canton with more than 1 percent of the labor force<sup>20</sup>. The figure conveys a striking message: In each canton we can observe an unambiguous positive relation between the wage sweep-up and the unemployment rate. In addition to Figure 6a we also ran the following regression:

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<sup>18</sup> In the following presentation (which is based on the SIF-sample) we concentrate on the relation between average wage sweep-up and the *unemployment rate*. However, the changes in the unemployment rate in our sample are almost exclusively driven by the changes in the employment level because labor supply was roughly constant. Therefore our examination also provides direct insights into the relation between employment and average wage sweep-ups across cantons and industries.

<sup>19</sup> Remember (from section 6.1) that the *level* of the unemployment rate does not affect notional wage changes. Instead, notional changes are affected by labor market experience, tenure, unemployment *growth*, age, etc. The differences in these variables across cantons and industries determine, together with our estimate of  $\alpha$ , the different wage sweep-ups in cantons and industries. Note also that the correlation between cantonal (industry) unemployment *rates* and cantonal (industry) unemployment *growth* is negligible (-0.01 for the cantons and 0.13 for the industries). Hence, the cantonal (industry) wage sweep-ups can be used as an independent variable in the explanation of cantonal (industry) unemployment rates.

<sup>20</sup> For the other cantons we have too few data to get useful estimates. In total we lose less than 2 percent of all observations by excluding the small cantons.



$$u_{jt} = a_j + bE_{jt}(\Delta w_{ijt} - \Delta w_{ijt}^*) + e_{jt}$$

where  $u_{jt}$  is the rate of unemployment in canton  $j$  and year  $t$ ,  $a_j$  represents cantonal fixed effects and  $E_{jt}(\cdot)$  denotes the average wage sweep up in canton  $j$  and year  $t$ . The OLS estimate of this model yields a highly significant and positive point estimate of  $b$  of 0.82 (s.e. = 0.03) and a within- $R^2$  of 0.82 (N=119 canton and year cells). If we add the real GDP in the different cantons and the inflation rate as control variables to the above regression, the impact of the average wage sweep-up remains highly significant and sizable ( $b = 0.48$ , s.e. = 0.12).

In Figure 6b we plotted the relation between unemployment rate and wage sweep-up for each industry. Again a striking picture emerges. In almost every industry the increase in the wage sweep-up is associated with an increase in the unemployment rate. Interestingly, however, the steepness of this relation varies considerably across industries. While, e.g., the relation is very steep in Tourism and Personal Services it is less steep in Transportation and the Health sector. Analogously to the regression for the cantons above we also conducted a regression for the industries. The result is a point estimate of  $b$  of 1.05 (s.e.= 0.1) and a within- $R^2$  of 0.48 (N=133 industry and year cells).

Thus, Figures 6a and 6b and the associated regressions show that variations in unemployment rates across cantons and industries are strongly related to the corresponding variations in wage sweep-ups caused by nominal rigidity. This represents strong evidence that in the low growth-low inflation environment which characterized the Swiss economy in the 1990s nominal wage rigidity had negative employment effects.

## 7. Concluding Remarks

It has been argued that in a macro-environment with persistently low nominal GDP growth the downward rigidity of nominal wages will vanish. Workers will become accustomed to more frequent nominal wage cuts and employers will, therefore, not shy away from cutting nominal pay. If this argument is valid nominal wage rigidity would be largely irrelevant because in an environment with high nominal growth rates there is little need to cut nominal pay to achieve real wage adjustments while in a low-growth environment nominal rigidity would be absent.

This paper uses three different data sources to examine this conjecture for the Swiss situation between 1991 and 1997. During this time Switzerland went through a unique macro economic

phase with negative or very low real GDP growth and a rapidly declining rate of inflation. All three data sources used in our paper show that nominal wage rigidity also persists in periods of low nominal growth. According to the personnel files of two firms wage cuts almost never occur. The data from the Swiss Labor Force Survey indicate that at most 5 percent of the job stayers receive wage cuts while nominal rigidity prevents wage cuts for 50 or more percent of the job stayers. Similar results are obtained from the Social insurance data. Our estimates also show that the extent of nominal rigidity does not decline in this period of sustained low nominal GDP growth. The threshold level that has to be passed to induce firms to actually cut the pay of their employees even increases over time. The fraction of workers whose wages are *not* cut because of nominal rigidity increases considerably over time while the frequency of true wage cuts is roughly constant. This indicates that, although the downward pressure on nominal wages increased over time, the downward rigidity of nominal wages remained a binding constraint for many employees. Thus, if anything, our estimates imply that nominal rigidity became more important during the period under consideration. Moreover, the relatively large coefficient on the unemployment change in our wage equation suggests that in the absence of nominal rigidity wages would be quite flexible.

Theories of nominal wage rigidity that are based on the existence of efficient nominal contracts or on nominal fairness standards in repeated work relations predict that the wages of job movers show less rigidity than the wages of job changers. These theories also suggest that the wages of part-time workers exhibit less rigidity than the wages of full-time workers. Our results confirm these predictions and lend thus support to these theories.

Our examination also suggests that nominal wage rigidity has important macro economic effects in an environment with low real growth and low inflation. In most industries and in all cantons the wage sweep-up due to nominally rigid wages is visibly related to the rate of unemployment: The higher the wage sweep-up the higher is the unemployment rate. This is compatible with the view that the downward rigidity of nominal wages is sufficiently strong to cause an increase in real labor costs and a decrease in employment.

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TABLE 1: NOMINAL GDP GROWTH DURING THE SAMPLE YEARS

|                                     | YEARS<br>CONSIDERED | MEDIAN | NUMBER OF <i>CONSECUTIVE</i> YEARS<br>BELOW |             |
|-------------------------------------|---------------------|--------|---|-------------|
|                                     |                     |        | 5.2 PERCENT <sup>a</sup>                    | 2.6 PERCENT |
| A. PREVIOUS STUDIES (UNITED STATES) |                     |        |   |             |
| Card and Hyslop (1996)              | 1976 – 1991         | 8.1%   | 0   | 0           |
| McLaughlin (1994)                   | 1976 – 1986         | 11.3%  | 0   | 0           |
| Kahn (1997)                         | 1971 – 1988         | 8.9%   | 0   | 0           |
| Akerlof, Dickens, and Perry (1996)  | 1959 – 1995         | 7.6%   | 2   | 0           |
| Altonji and Devereux (1999)         | 1972 – 1992         | 7.7%   | 0   | 0           |
| Lebow, Saks and Wilson (1999)       | 1981 - 1998         | 5.7%   | 0   | 0           |
| B. THIS STUDY (SWITZERLAND)         |                     |        |   |             |
|                                     | 1991 – 1997         | 2.2%   | All Years                                   | 6           |

Sources: Economic Report of the President 2000, Table B-3; Swiss National Bank Monthly Bulletin; own calculations.

Notes: a. The highest nominal GDP growth rate in Switzerland was 5.2 percent in 1991.

TABLE 2: DESCRIPTIVE STATISTICS OF WAGE FREEZES AND WAGE CUTS

| YEAR | RATE OF<br>INFLATION<br>(CPI) | REAL GDP<br>GROWTH | SOURCE: SWISS LABOR FORCE SURVEY             |   |       | SOURCE: SOCIAL INSURANCE FILES              |  |        |
|------|-------------------------------|--------------------|--|---|-------|---|--|--------|
|      |                               |                    | FRACTION WITH<br>ZERO NOMINAL<br>WAGE CHANGE | FRACTION WITH<br>NOMINAL WAGE<br>DECREASE | N     | FRACTION WITH<br>ZERO NOMINAL<br>PAY CHANGE | FRACTION WITH<br>NOMINAL PAY<br>DECREASE | N      |
| 1991 | 4.7%                          | -0.8%              | 0.05   | 0.20                                      | 2,941 | 0.02  | 0.11                                     | 18,450 |
| 1992 | 3.7%                          | -0.1%              | 0.08   | 0.29                                      | 3,337 | 0.02  | 0.15                                     | 20,087 |
| 1993 | 1.1%                          | -0.5%              | 0.09   | 0.31                                      | 3,476 | 0.03  | 0.20                                     | 20,870 |
| 1994 | 1.6%                          | 0.5%               | 0.06   | 0.31                                      | 3,379 | 0.05  | 0.21                                     | 20,699 |
| 1995 | 0.9%                          | 0.5%               | 0.06   | 0.31                                      | 2,606 | 0.04  | 0.25                                     | 19,556 |
| 1996 | 0.6%                          | 0.3%               | 0.14   | 0.38                                      | 2,742 | 0.05  | 0.26                                     | 20,285 |
| 1997 | 0%                            | 1.7%               | 0.15   | 0.33                                      | 2,754 | 0.09  | 0.31                                     | 20,681 |

Sources: Federal Office of Statistics, Swiss Labour Force Survey 1991 – 1998, Social Insurance Files 1990 - 1997; own calculations.

TABLE 3: THE EXTENT OF NOMINAL WAGE RIGIDITIES - ML ESTIMATES

|  | SWISS LABOR FORCE SURVEY |                          |                         | SOCIAL INSURANCE FILES   |                          |                          |
|--|--------------------------|--------------------------|-------------------------|--------------------------|--------------------------|--------------------------|
|  | CONT.<br>ERRORS          | MIXED<br>ERRORS          | MIXED<br>ERRORS         | CONT.<br>ERRORS          | MIXED<br>ERRORS          | MIXED<br>ERRORS          |
| <b>Threshold<br/>Wage Cut <math>\alpha</math></b>                      | <b>.214**<br/>(.005)</b> | <b>.268**<br/>(.006)</b> | <b>.312**<br/>(.01)</b> | <b>.183**<br/>(.002)</b> | <b>.208**<br/>(.002)</b> | <b>.268**<br/>(.004)</b> |
| <b>Reduction of<br/>Wage Cut <math>\lambda</math></b>                  | -                        | -                        | .078**<br>(.013)        | -                        | -                        | .11**<br>(.004)          |
| <b>Implied Frequency of<br/>Nominal Wage Cuts</b>                      | .052                     | .023                     | .028                    | .064                     | .047                     | .052                     |
| <b>Fraction of Workers<br/>affected by Nominal<br/>Wage Rigidities</b> | .491                     | .556                     | .59                     | .447                     | .48                      | .537                     |
| <b>Determinants of<br/>Notional Wage Changes</b>                       |                          |                          |                         |                          |                          |                          |
| Log Experience   | -.009**<br>(.001)        | -.012**<br>(.002)        | -.013**<br>(.002)       | -                        | -                        | -                        |
| Log Tenure   | -.004**<br>(.002)        | -.005**<br>(.002)        | -.006**<br>(.002)       | -                        | -                        | -                        |
| Individual is a Superior<br>(dummy variable)                           | .025**<br>(.005)         | .029**<br>(.006)         | .032**<br>(.007)        | -                        | -                        | -                        |
| Individual is member<br>of higher management<br>(dummy variable)       | -.009**<br>(.003)        | -.01**<br>(.003)         | -.011**<br>(.003)       | -                        | -                        | -                        |
| Change in regional<br>Unemployment Rate                                | -.006*<br>(.004)         | -.007*<br>(.004)         | -.008*<br>(.004)        | -.007**<br>(.002)        | -.008**<br>(.003)        | -.009**<br>(.004)        |
| Log Age  | -                        | -                        | -                       | -.078**<br>(.002)        | -.085**<br>(.003)        | -.097**<br>(.003)        |
| Foreigner (dummy<br>variable)  | -                        | -                        | -                       | -.067**<br>(.018)        | -.069**<br>(.019)        | -.078**<br>(.022)        |
| Foreigner*Log(Age)   | -                        | -                        | -                       | .016**<br>(.005)         | .017**<br>(.005)         | .019**<br>(.006)         |

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TABLE 3, CONT.

|                        |        |        |        |        |        |        |
|------------------------|--------|--------|--------|--------|--------|--------|
| $\sigma_e$             | .121   | .118   | .137   | .113   | .115   | .139   |
| $\sigma_m$             | .068   | .073   | .068   | .043   | .041   | .037   |
| $p$                    | -      | .394   | .38    | -      | .33    | .31    |
| Year Effects           | Yes    | Yes    | Yes    | Yes    | Yes    | Yes    |
| Firm-Size Effect       | Yes    | Yes    | Yes    | -      | -      | -      |
| Number of Observations | 21,144 | 21,144 | 21,144 | 58,297 | 58,297 | 58,297 |
| Log likelihood         | 16,962 | 8,330  | 8,341  | 60,105 | 43,221 | 43,656 |

Notes: a. standard errors in parenthesis, adjusted for clustering on cantons and years. \*, \*\* denotes significance at the 5 percent and 1 percent level respectively.

b.  $\sigma_e$  and  $\sigma_m$  denote the standard deviation of  $e_{it}$  and  $m_{it}$ , respectively.



TABLE 4: THRESHOLD WAGE CUT OVER TIME

## ML ESTIMATES

|   | SWISS LABOR FORCE<br>SURVEY | SOCIAL INSURANCE<br>FILES |
|---|-----------------------------|---------------------------|
|   | MIXED MODEL                 | MIXED MODEL               |
| <b><i>Threshold Wage Cut</i></b>                          |                             |                           |
| Constant Term (1991)                                      | .24**<br>(.02)              | .216**<br>(.006)          |
| <b>Year Dummy Variables:</b><br>Changes relative to 1991: |                             |                           |
| 1992  | .039**<br>(.014)            | .014*<br>(.006)           |
| 1993  | .072**<br>(.014)            | .047**<br>(.006)          |
| 1994  | .073**<br>(.015)            | .052**<br>(.006)          |
| 1995  | .044**<br>(.015)            | .06**<br>(.006)           |
| 1996  | .070**<br>(.015)            | .057**<br>(.006)          |
| 1997  | .092**<br>(.015)            | .058**<br>(.006)          |
| $\sigma_e$  | .133                        | .161                      |
| $\sigma_m$  | .069                        | .038                      |
| Number of Observations                                    | 21,144                      | 58,297                    |
| Log likelihood  | 8,365                       | 43,751                    |

Notes: a. standard errors in parenthesis, adjusted for clustering on cantons and years.  
 \*, \*\* denotes significance at the 5 percent and 1 percent level respectively.

b. Same determinants of notional wage changes as in Table 3. Both specifications include  $\lambda$ .

TABLE 5: NOMINAL RIGIDITIES FOR DIFFERENT GROUPS OF WORKERS

ML ESTIMATES FROM SWISS LABOR FORCE SURVEY, 1991 - 1997

|   | CONTINUOUS MODEL | MIXED MODEL      |
|---|------------------|------------------|
| <b>Threshold Wage Cut <math>\alpha</math></b>           |                  |                  |
| Full-Time Job Stayer                                    | .237**<br>(.006) | .3**<br>(0.007)  |
| Part-time Job Stayer                                    | .17**<br>(.005)  | .205**<br>(.009) |
| <b>Frequency of Wage Cuts</b>                           |                  |                  |
| Full-Time Job Stayer                                    | .038             | .014             |
| Part-time Job Stayer                                    | .105             | .073             |
| <b>Fraction Affected by<br/>Nominal Wage Rigidities</b> |                  |                  |
| Full-Time Job Stayer                                    | .509             | .57              |
| Part-time Job Stayer                                    | .436             | .504             |
| $\sigma_e$  | .122             | 0.12             |
| $\sigma_m$  | .068             | 0.072            |
| Number of Observations                                  | 21,144           | 21,144           |
| Log likelihood  | 17,033           | 8,408            |

Notes: a. standard errors in parenthesis, adjusted for clustering on cantons and years.  
 \*, \*\* denotes significance at the 5 percent and 1 percent level respectively.

b. Same determinants of notional wage changes as in Table 3.

TABLE 6: NOMINAL RIGIDITIES FOR MOVERS AND STAYERS

ML ESTIMATES FROM SWISS LABOR FORCE SURVEY, 1995 – 1997

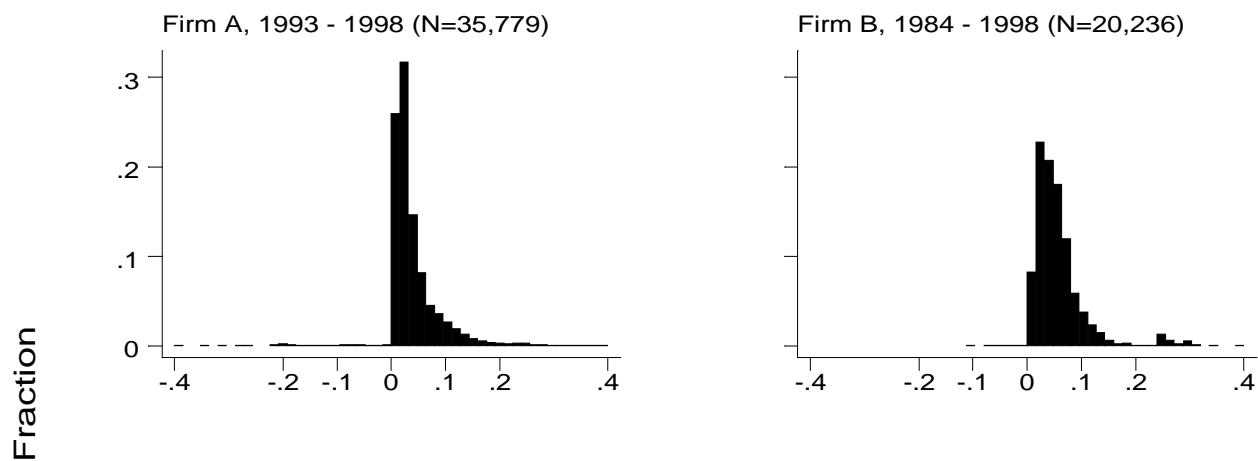
|  | CONTINUOUS MODEL | MIXED MODEL      |
|--|------------------|------------------|
| <b>Threshold Wage Cut <math>\alpha</math></b>      |                  |                  |
| <i>Job Stayers</i>                                 |                  |                  |
| Full-Time  | .309**<br>(.009) | .413**<br>(.011) |
| Part-time  | .203**<br>(.007) | .246**<br>(.008) |
| <i>Job Movers</i>                                  |                  |                  |
| Job quits  | .119**<br>(.026) | .138**<br>(.027) |
| Dismissals   | .103**<br>(.026) | .106**<br>(.026) |
| Other reasons                                      | .13**<br>(.027)  | .13**<br>(.03)   |
| $\alpha$ -increase if job<br>movers work full-time | .019<br>(.021)   | 0.035<br>(.022)  |
| <b>Frequency of Wage Cuts</b>                      |                  |                  |
| <i>Job Stayers</i>                                 |                  |                  |
| Full-time  | .033             | .01              |
| Part-time  | .139             | .114             |
| <i>Job Movers</i>                                  |                  |                  |
| Job quits  | .129             | .081             |
| Dismissals   | .181             | .166             |
| Other reasons                                      | .144             | .134             |

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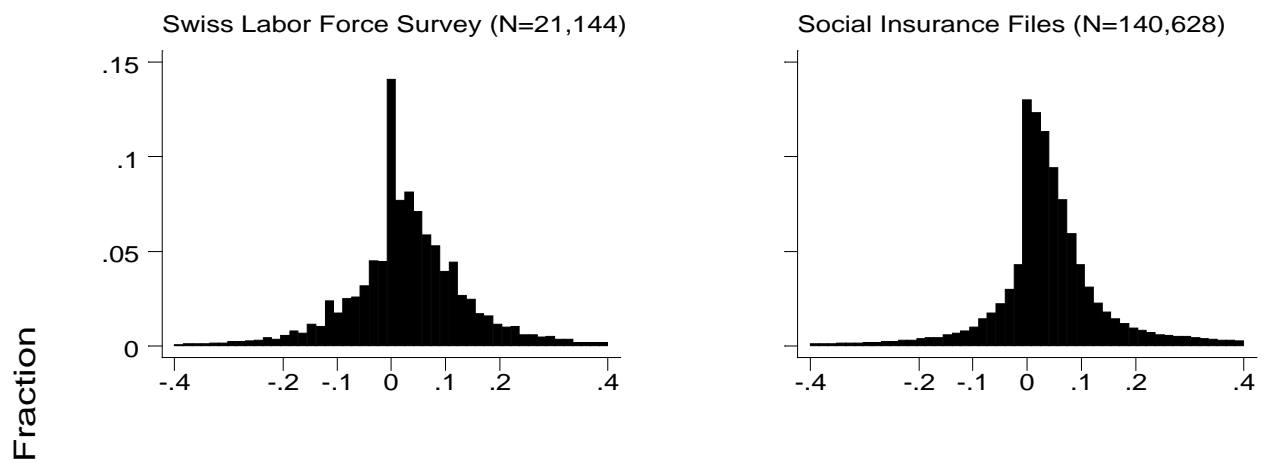
TABLE 6, CONT.

|                        |        |        |
|------------------------|--------|--------|
| $\sigma_e$             | .141   | .144   |
| $\sigma_m$             | .074   | .079   |
| $p$                    | -      | .334   |
| Number of Observations | 10,708 | 10,708 |
| Log likelihood         | 7,668  | 2,696  |

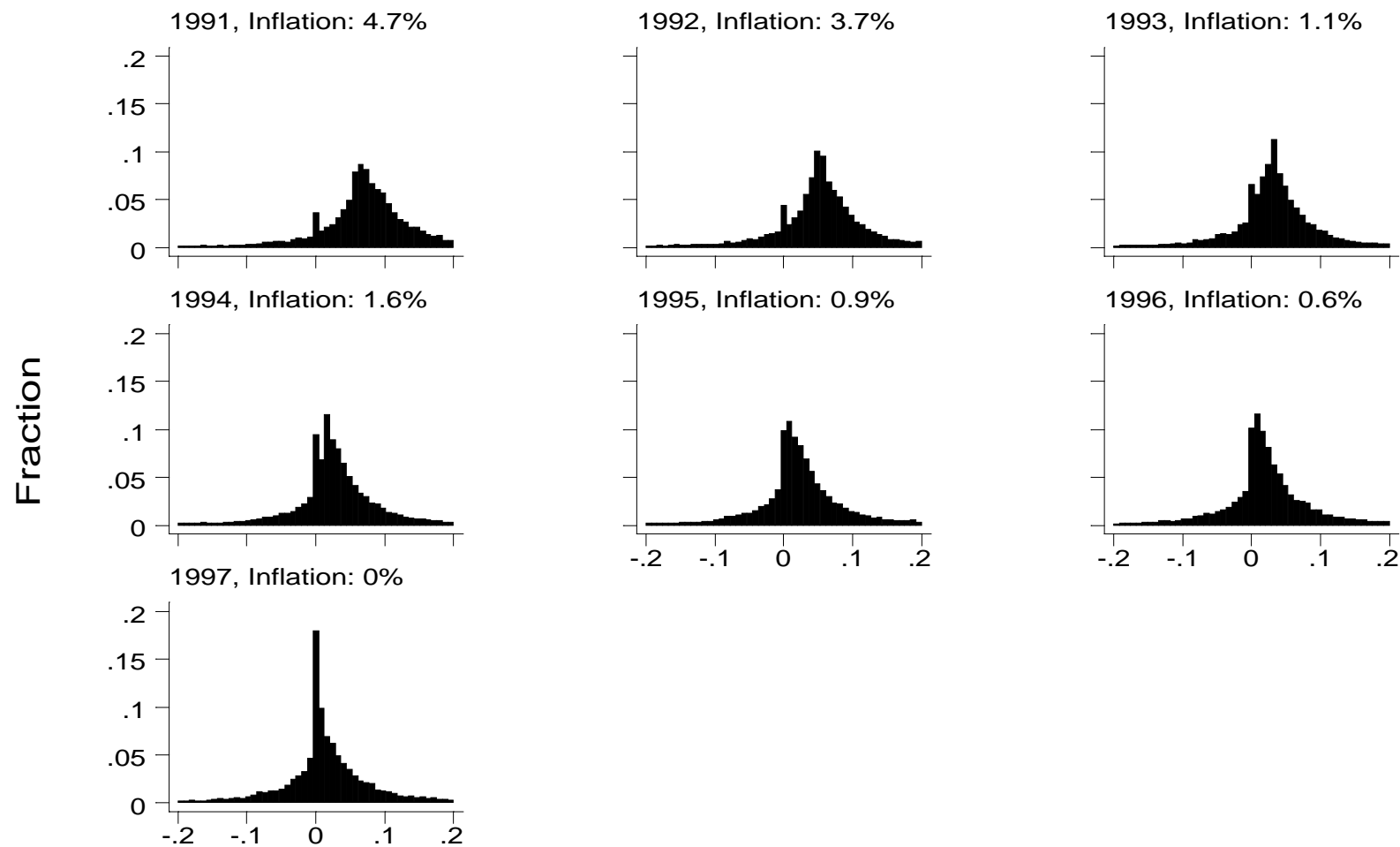
- Notes:
- a. standard errors in parenthesis, adjusted for clustering on cantons and years.  
\*, \*\* denotes significance at the 5 percent and 1 percent level respectively.
  - b. Same determinants of notional wage changes as in Table 3, but data is available only for 1995, 1996 and 1997.



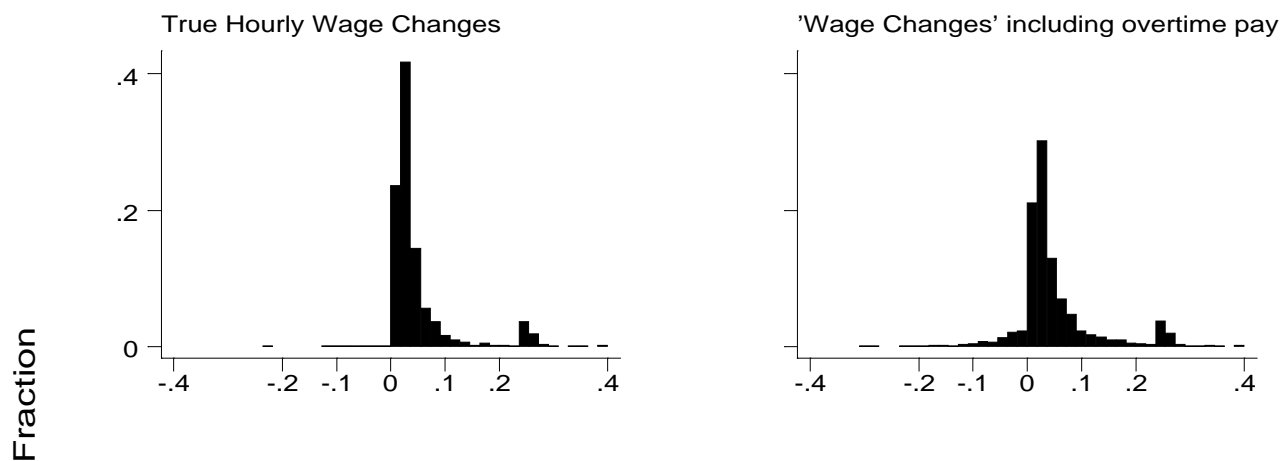
Evidence from Personnel Files  
**Figure 1: Distribution of Nominal Wage Changes**



Evidence from Representative Samples, Switzerland 1991 - 1997  
**Figure 2: Distribution of Nominal Wage Changes**



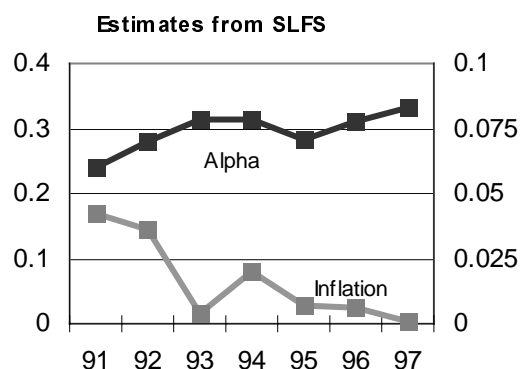
Evidence from Social Insurance Files  
**Figure 3: Distribution of Wage Changes over Time**



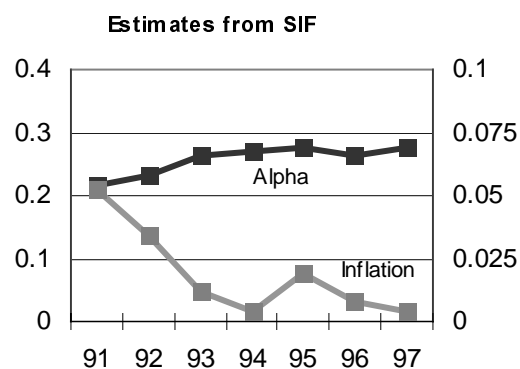
Evidence from Personnel Files, Firm B, 1993 - 1998  
**Figure 4: True and Polluted Wage Changes**



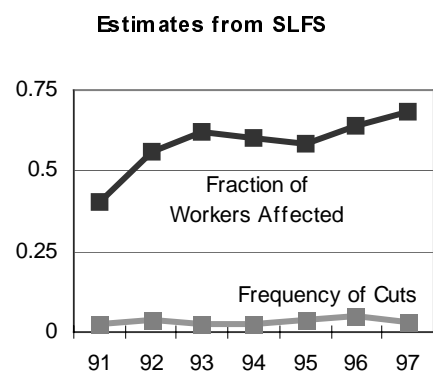
**Panel (a): Inflation Rate and Alpha**



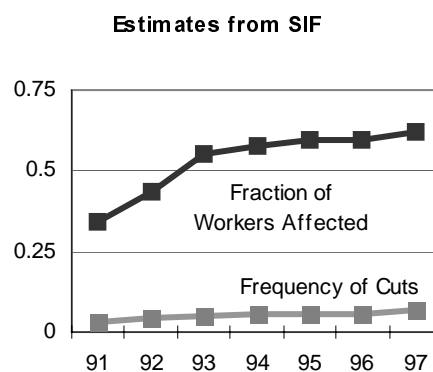
**Panel (b): Inflation Rate and Alpha**



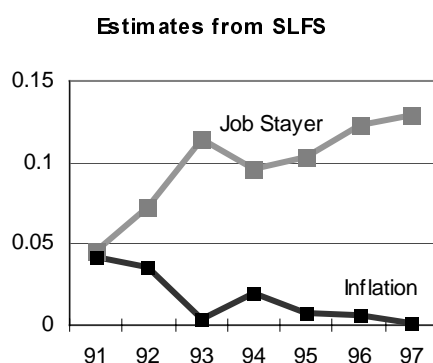
**Panel (c): The Extent of Nominal Wage Rigidities over Time**



**Panel (d): The Extent of Nominal Wage Rigidities over Time**



**Panel (e): Average Prevented Real Wage Cut**



**Panel (f): Average Prevented Real Wage Cut**



**FIGURE 5: ARE NOMINAL RIGIDITIES FADING?**

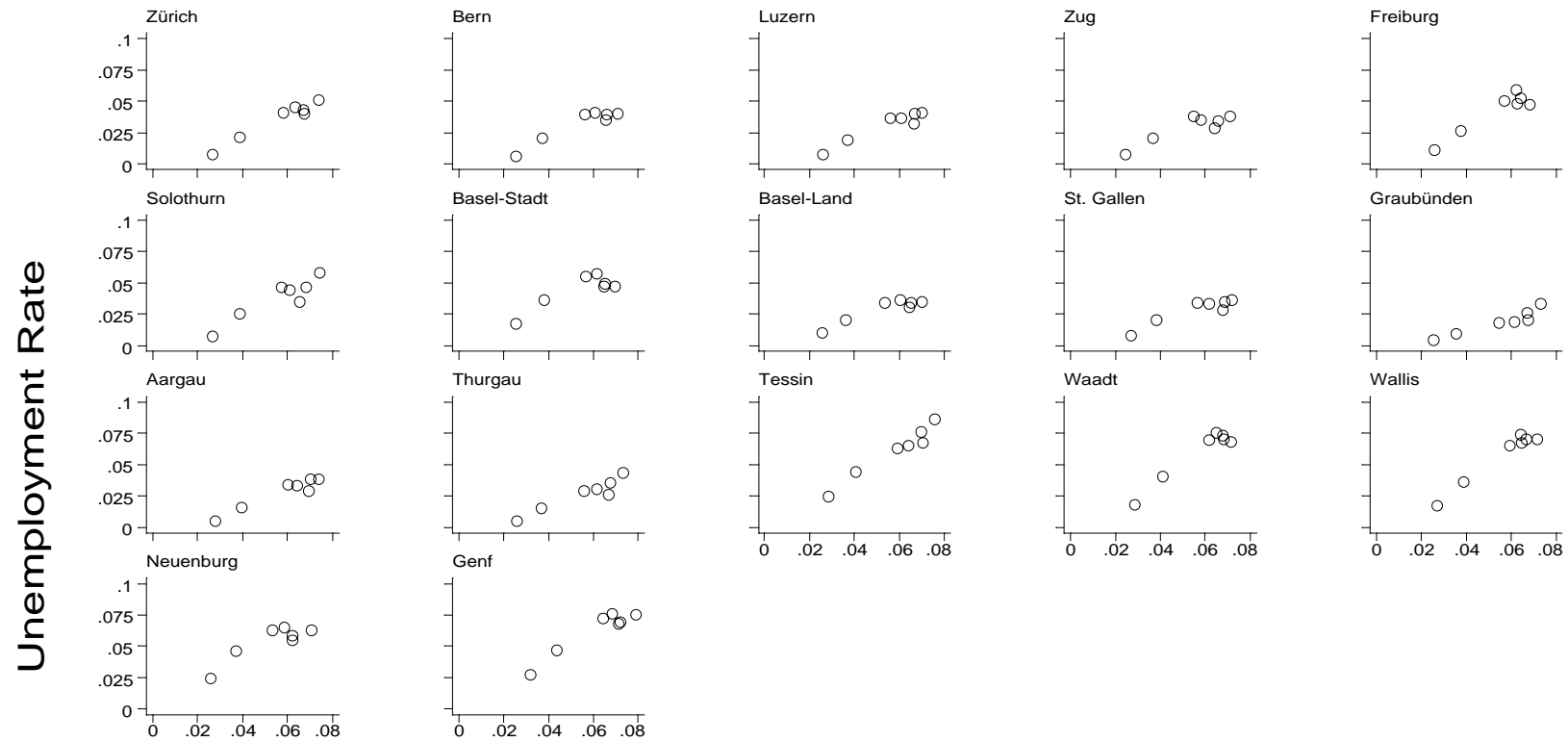


Figure 6a: Unemployment Rate and Wage Sweep-Ups across Cantons

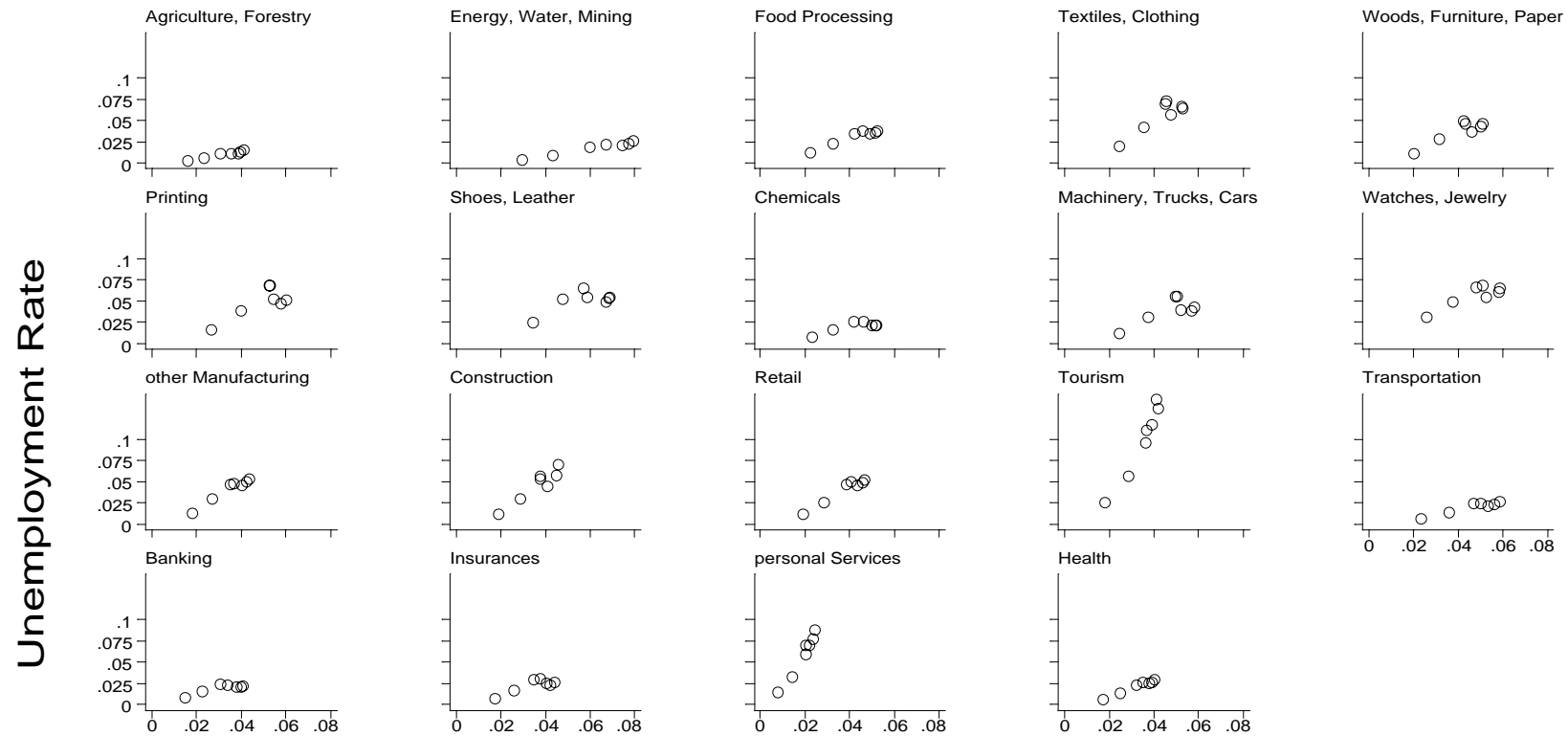


Figure 6b: Unemployment Rate and Wage Sweep-Ups across Industries

## Appendix 1

### Nominal Wage Rigidity in the SIF Industry Sample

In this appendix, we discuss the estimation results we obtained from the SIF industry sample. The first three columns of Table A1 display the results for the baseline specifications presented in Table 3 in the main text. The estimates in Table A1 are very similar to the estimates in Table 3. If anything, they indicate that nominal rigidity is even slightly stronger in the industry sample. The estimates imply that at most 5.1 percent of all job stayers receive wage cuts, while approximately 50 percent receive wage freezes instead.

The coefficients of the determinants of notional wage changes are also very similar to the coefficients found in the cantonal sample (see Table 3 in the main text). One notable feature is the quantitatively large and highly significant coefficient on the change in the industry-specific unemployment rate. An increase in the industry unemployment rate lowers wage growth by 0.8 to 1.1 percentage points. In the fourth column of Table A1 we include industry dummy variables. Hence the coefficient on the change in unemployment only reflects the within-industry variation of unemployment growth. The coefficient is still significant and about two thirds of its original size. This shows that unemployment growth curbs wage growth significantly, even if one only considers its variation within an industry. Thus, again we find substantial flexibility of the notional wage changes.

Finally, Table A2 displays the year-specific coefficients for  $\alpha$ . Again, we find a higher, not lower,  $\alpha$  at the end of the sample period. The implied frequencies of wage cuts are shown in Figure A1. They are very similar to the ones we show in Figure 4 in the main text.

TABLE A1: SIF INDUSTRY SAMPLE: BASELINE SPECIFICATIONS

## ML ESTIMATES

|  | CONT.<br>ERRORS          | MIXED<br>ERRORS          | MIXED<br>ERRORS          | MIXED<br>ERRORS          |
|--|--------------------------|--------------------------|--------------------------|--------------------------|
| <b>Threshold<br/>Wage Cut <math>\alpha</math></b>                      | <b>.196**<br/>(.002)</b> | <b>.212**<br/>(.002)</b> | <b>.266**<br/>(.004)</b> | <b>.212**<br/>(.002)</b> |
| <b>Reduction in<br/>Wage Cut <math>\lambda</math></b>                  | -                        | -                        | .104**<br>(.004)         | -                        |
| <b>Implied Frequency of<br/>Nominal Wage Cuts</b>                      | .051                     | .041                     | .045                     | .04                      |
| <b>Fraction of Workers<br/>affected by Nominal<br/>Wage Rigidities</b> | .457                     | .477                     | .531                     | .469                     |
| <b>Determinants of<br/>Notional Wage Change</b>                        |                          |                          |                          |                          |
| Change in industry<br>Unemployment Rate                                | -.008**<br>(.002)        | -.009**<br>(.002)        | -.011**<br>(.002)        | -.006*<br>(.002)         |
| Log Age  | -.106**<br>(.002)        | -.111**<br>(.002)        | -.125**<br>(.003)        | -.11**<br>(.002)         |
| Foreigner (dummy<br>variable)  | -.103**<br>(.017)        | -.11**<br>(.017)         | -.118**<br>(.02)         | -.105**<br>(.017)        |
| Foreigner*Log(Age)   | .026**<br>(.005)         | .028**<br>(.005)         | .03**<br>(.005)          | .027**<br>(.005)         |
| $\sigma_e$   | .112                     | .112                     | .133                     | .112                     |
| $\sigma_m$   | .045                     | .039                     | .036                     | .039                     |
| $p$  | -                        | .263                     | .247                     | .264                     |

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TABLE A1, CONT.

|                        |        |        |        |        |
|------------------------|--------|--------|--------|--------|
| Year Effects           | Yes    | Yes    | Yes    | Yes    |
| Industry Effects       | No     | No     | No     | Yes    |
| Number of Observations | 55,884 | 55,884 | 55,884 | 55,884 |
| Log likelihood         | 58,604 | 47,974 | 48,324 | 48,043 |

Notes: robust standard errors in parenthesis, adjusted for clustering on industries and years. \*, \*\* denotes significance at the 5 percent and 1 percent level respectively.  $\sigma_e$  and  $\sigma_m$  denote the standard deviation of  $e_{it}$  and  $m_{it}$ , respectively.

TABLE A2: THRESHOLD WAGE CUT OVER TIME

SIF INDUSTRY SAMPLE (ML ESTIMATES)

|   | MIXED MODEL      |
|---|------------------|
| <b><i>Threshold Wage Cut</i></b>                          |                  |
| Constant Term (1991)                                      | .157**<br>(.004) |
| <b>Year Dummy Variables:</b><br>Changes relative to 1991: |                  |
| 1992  | .03**<br>(.006)  |
| 1993  | .051**<br>(.005) |
| 1994  | .072**<br>(.006) |
| 1995  | .068**<br>(.006) |
| 1996  | .063**<br>(.006) |
| 1997  | .067**<br>(.006) |
| $\sigma_e$  | .112             |
| $\sigma_m$  | .039             |
| Number of Observations                                    | 55,884           |
| Log likelihood  | 48,092           |

Notes: a. robust standard errors in parenthesis, adjusted for clustering on industries and years. \*, \*\* denotes significance at the 5 percent and 1 percent level respectively.

b. same determinants of notional wage changes as in Table A1.

**Figure A1: The Extent of  
Nominal Wage Rigidities over Time**  
Estimates from SIF Industry Sample

