Nominal Wage Dynamics in China

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Abstract

This study examines nominal wage flexibility in China's labor market using individual-level data from the China Household Finance Survey from 2011 to 2019. The analysis documents weak downward nominal wage rigidity by international standards and a substantial incidence of nominal wage cuts among job stayers. These wage cuts are not confined to a single period and remain substantial after accounting for working-hour adjustments and survey-heaping in wage reporting. We find that wage cuts are much less persistent at the individual level than in the aggregate distribution: only a small share of workers experience nominal wage cuts in two consecutive observed wage-change intervals despite a substantial incidence of wage cuts overall. The paper then examines whether exposure to nominal wage cuts differs across worker and job characteristics. Education provides the clearest evidence: workers with secondary or higher education are less likely to experience nominal wage cuts than workers with compulsory education. Employer type, industry, and marital status also matter, while gender and tenure show weaker or less directly interpretable patterns. These findings suggest that nominal wage flexibility in China coexists with unequal exposure to nominal wage cuts. Potential mechanisms include performance-related wage components, decentralized wage bargaining, weaker outside options, and uneven enforcement of labor protection across employers and sectors.

Keywords: Downward Nominal Wage Rigidity, Nominal Wage Cuts, Bargaining Position.

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1 Introduction

Many efforts have been made to develop a more complete understanding of China's labor market, including studies of the effect of education on labor-market performance (Asadullah and Xiao, 2019; Knight et al., 2017) and the impact of the Hukou system on labor mobility (Wang et al., 2020; Meng, 2012). While this literature has examined many distinctive features of China's labor market, one relatively unexplored feature is the comparative lack of downward nominal wage rigidity (henceforth DNWR). Existing studies have mainly documented that nominal wages in China appear unusually flexible downward, but have paid less attention to what this flexibility implies for workers' exposure to nominal wage cuts. This paper, therefore, aims to document worker-level heterogeneity in exposure to nominal wage cuts and to examine whether this heterogeneity is systematically related to workers' bargaining positions.

The dynamics of nominal wages, especially DNWR,[1] have substantial implications for labor-market adjustment and the business cycle (Goette et al., 2007). Although both the determinants and the measurement of DNWR remain under debate, researchers have reached broad agreement on its prevalence in the United States (Barattieri et al., 2014; Gottschalk, 2005) and in European countries (Babecký et al., 2010; Goette et al., 2007; Knoppik and Beissinger, 2003). Depending on the country and sector under study, roughly 10% to 30% of workers are estimated to be affected by DNWR. China, however, appears to be an exception. Existing estimates suggest that the level of DNWR in China is about $\frac{1}{6}$ of that in the United States and about $\frac{1}{20}$ of that in Germany (Xu et al., 2012). One possible explanation could be that the rapid economic growth generates broadly rising nominal wages and therefore mechanically lowers measured DNWR.[2] Yet this interpretation is difficult to reconcile with the fact that a substantial share of job stayers experience nominal wage cuts between adjacent waves. Moreover, because these wage cuts appear across multiple survey waves rather than only around an isolated downturn, they are unlikely to be explained solely by temporary macroeconomic shocks, which hence motivates worker-level explanations. While some studies have documented the gap in DNWR between China and other countries (Xu et al., 2012), far fewer examine what weak DNWR implies for the distribution of nominal wage cuts across individuals. In particular, it remains unclear whether nominal wage cuts are broadly shared across individuals or concentrated among certain worker groups. This gap reflects two features of the existing literature: the mainstream DNWR literature has focused primarily on rigidity rather than on wage cuts themselves,[3] and many available data sources are collected at the firm or sector level, limiting the scope for individual-level analysis.[4] Because DNWR and nominal wage cuts are closely related manifestations of nominal wage adjustment, understanding why wage cuts occur requires examining the factors that weaken workers' resistance to downward wage adjustment. A central candidate mechanism at the individual level is workers' bargaining position: workers with stronger outside options or greater institutional protection should be better able to resist nominal wage cuts, while workers with weaker outside options may be more exposed to downward wage adjustments.

This paper contributes to the literature in three ways. First, it extends the literature on downward nominal wage rigidity in China from estimating the degree of rigidity to examining the worker-level exposure implied by weak rigidity. Second, the paper contributes to the literature on nominal wage cuts by jointly examining their incidence, aggregate recurrence, and individual-level persistence. By separating these margins, we provide a more precise description of nominal wage flexibility than can be obtained from the incidence of wage cuts alone. Third, the paper examines which groups of workers are more exposed to nominal wage cuts. Rather than treating nominal

[1] According to the OECD, downward nominal wage rigidity is commonly defined based on the frequency of nominal wage freezes (Babecký et al., 2010).

[2] If nearly all workers experience nominal wage growth in a rapidly expanding economy, the observed level of DNWR may appear low even in the absence of substantial wage flexibility. The sizable incidence of nominal wage cuts in China suggests that this explanation is incomplete.

[3] Kurmann and McEntarfer (2019) is an exception, although it does not focus on explaining the underlying sources of nominal wage cuts.

[4] Administrative payroll data for the United States have only recently become available and are much less exposed to the measurement problems common in survey data.

wage flexibility as a uniform aggregate phenomenon, the analysis documents how exposure to downward wage adjustment differs across observable worker and job characteristics, including education, gender, tenure, employer type, and industry. This descriptive and econometric evidence provides a basis for interpreting bargaining power and outside options, thereby connecting nominal wage dynamics to the broader literature on bargaining position and labor-market protection.

This paper develops these arguments in the context of China's institutional setting and examines them using the China Household Finance Survey (CHFS). The central empirical task is to document the extent of nominal wage flexibility in China and to examine whether exposure to nominal wage cuts differs systematically across worker groups. The CHFS is useful for this purpose because it provides individual-level information on wages, working hours, employment characteristics, and demographic characteristics over multiple survey waves, allowing the analysis to focus on job stayers and to construct within-worker nominal wage changes. We address two measurement concerns before moving to further analysis: whether hourly wage cuts are mechanically generated by changes in working time, and whether reported wages are affected by rounding or heaping. The empirical analysis proceeds in three steps. It first documents the level of DNWR and the incidence of nominal wage cuts in the CHFS, then examines whether these patterns are robust to measurement-error concerns arising from reporting heaping in survey wages, and finally estimates panel models relating nominal wage cuts to workers' individual and employment characteristics.

The results show that nominal wage cuts are common among job stayers in China and that DNWR is low by international standards. These wage cuts recur at the aggregate level and remain evident after accounting for measurement error. At the same time, evidence across observed wage-change intervals shows that nominal wage cuts are much less persistent at the individual level: only a small share of workers experience wage cuts in two consecutive observed intervals. The regression results further indicate that exposure to nominal wage cuts is not distributed uniformly across the workforce. Education provides the clearest pattern: workers with secondary or higher education are less likely to experience nominal wage cuts than workers with compulsory education. Gender, tenure, employer type, and industry also reveal systematic differences in wage-cut exposure, although their interpretation is less direct. Overall, these findings suggest that nominal wage flexibility in China is not simply an aggregate feature of wage adjustment. It is also associated with heterogeneous exposure across worker groups. In the implication section, we interpret these patterns as consistent with the view that workers' ability to resist downward wage adjustment may differ with outside options, workplace attachment, and institutional protection, while emphasizing that the empirical analysis remains primarily descriptive rather than causal.

The remainder of the paper is organized as follows. Section 2 reviews the literature on DNWR and nominal wage cuts. Section 3 discusses China's institutional setting. Section 4 describes the CHFS data, including its advantages and limitations. Section 5 presents the methods used to estimate DNWR and compares China's DNWR with that of other countries. Section 6 addresses measurement error in the data and the methods used to mitigate it. Section 7 presents the descriptive and econometric analysis of worker-level exposure to nominal wage cuts. Section 8 discusses the implications of the findings, including their possible interpretation through the lens of bargaining position and institutional protection. Section 9 concludes and outlines the study's limitations.

2 Literature Review

Treating nominal wages as prices paid to workers, one might expect them to adjust in response to market conditions like other prices. In practice, however, nominal wages are often much less flexible, especially downward. While nominal wage increases are generally accepted, nominal wage cuts frequently encounter resistance from workers and may also create costs for firms by damaging morale, effort, and retention. Downward nominal wage rigidity (DNWR) can therefore be understood as reflecting the tension between workers' resistance to reductions in earnings and firms' incentives to maintain stable employment relationships. Nominal wage cuts and DNWR describe two closely related aspects of the same adjustment process: when downward rigidity is strong, nominal wage cuts should be less frequent; when nominal wage cuts are common, downward rigidity is likely to be weak.[5]

A first step in understanding nominal wage dynamics is to clarify how DNWR should be measured. The measurement of nominal wage cuts is relatively straightforward: most studies define it as the share of job stayers who experience a nominal wage decline between two consecutive observations. Measuring DNWR is more difficult because of its counterfactual nature. DNWR refers to resistance to downward nominal adjustment, so the object of interest is the share of workers whose wages would have fallen in the absence of rigidity. Broadly speaking, the literature has adopted two approaches. The first measures DNWR directly from the observed frequency of wage freezes. The second infers rigidity from the shape of the wage-change distribution by comparing the observed distribution with a counterfactual distribution that would prevail without DNWR. Despite differences across measures, the literature has reached a broad consensus that DNWR is prevalent in many labor markets and that nominal wage cuts are relatively limited under normal conditions.

Some researchers have argued, however, that measurement error may distort this conclusion (Dickens et al., 2007). Aggregate-level data are particularly vulnerable to the replacement effect because they rely heavily on average wages to characterize nominal wage dynamics (Schmitt-Grohé and Uribe, 2013; Fortin, 2013; Holden and Wulfsberg, 2008; Kimura and Ueda, 2001). If high-wage workers leave employment and are replaced by lower-wage workers, average wages may decline even when no incumbent worker experiences an actual wage cut. Individual-level survey data avoid this particular problem when wages are observed for the same workers over time, but they introduce other concerns, such as rounding, recall error, and reluctance to report wage cuts accurately (Xu et al., 2012; Smith, 2000). Several methods have therefore been proposed to mitigate measurement error, although some rely on restrictive identifying assumptions and may themselves introduce bias. More recently, administrative payroll and tax records have provided additional evidence on DNWR. These studies confirm that nominal wage cuts do occur in countries such as the United States and several European economies, but they also show that such episodes are often concentrated in periods of severe shocks, including the Great Recession and the COVID-19 downturn, and tend to be transitory (Schaefer and Singleton, 2023; Grigsby et al., 2021). Recent China-specific firm-level evidence also points to the importance of institutional and adjustment margins. Ding and Zhou (2025) study Chinese firms' labor-cost adjustment during the global financial crisis and show that features of downward nominal wage rigidity coincided with the implementation of the Labor Contract Law, while firms with higher shares of casual workers adjusted labor costs more easily. Related evidence from the COVID-19 period shows that Chinese firms responded to lockdown shocks mainly by reducing employment rather than cutting wages (Chen et al., 2024). This evidence provides a useful benchmark for interpreting the magnitude and persistence of nominal wage cuts in settings where comparable administrative data are unavailable.

Once measurement concerns have been addressed, a further question concerns how nominal wage cuts are distributed across workers. Much of the DNWR literature focuses on aggregate rigidity or on the shape of the wage-change distribution. This focus is natural given the macroeconomic relevance of nominal wage rigidity, but it leaves less room to examine whether downward nominal adjustment is evenly distributed across workers. If nominal wage cuts are common, then an important empirical question is whether they are widely shared across the workforce or concentrated among workers with particular demographic, employment, or sectoral characteristics. This distinction matters because the same aggregate incidence of wage cuts can imply very different labor-market processes depending on whether wage cuts are randomly distributed or concentrated among more vulnerable groups.

A useful way to organize this heterogeneity is to distinguish between individual characteristics, on-the-job

[5] One exception is when nearly every individual is having nominal wage increases, where there are no nominal wage cuts to be prevented.

experiences, and institutional exposure. Individual characteristics such as education may be related to productivity, outside options, and labor-market mobility. Workers with higher education are often viewed as more productive or scarcer in the labor market, which may improve the quality of their outside options (Caldwell and Danieli, 2024; Attanasio and Kaufmann, 2017). Similarly, marital status may matter because it can constrain mobility and therefore limit the set of feasible alternatives (Meekes and Hassink, 2022; Carta and de Philippis, 2018). Married workers may be less willing to move across regions or cities and may therefore face a narrower opportunity set, weakening their bargaining position relative to otherwise similar unmarried workers. On-the-job experiences, reflected for example in tenure, may also matter, although their interpretation is ambiguous. Longer tenure may increase firm-specific human capital and make wage cuts less likely, but it may also reduce mobility if workers become more attached to their current employer. Employer type and industry capture a different margin: they reflect the institutional and sectoral environment in which wage setting takes place. Some sectors may be more exposed to demand fluctuations, while some employer types may operate under stronger employment-stability norms or more formalized wage-setting arrangements.

In the Chinese context, these sources of heterogeneity may be especially important because formal labor regulation coexists with decentralized wage setting and uneven enforcement. Compared with many OECD countries, where wage bargaining often takes place at the sectoral as well as the firm level, wage negotiation in China is more decentralized and more frequently conducted at the individual level (Bhuller et al., 2022; Friedman, 2017; Liu and Kuruvilla, 2016). Limited regulatory enforcement may further affect how formal labor protections translate into actual wage-setting outcomes (Duan, 2022). These institutional features suggest that worker-level exposure to nominal wage cuts may depend not only on aggregate labor-market conditions but also on the type of employer, the sector of employment, and the worker's position within the labor market.

Beyond their immediate relevance for wage-setting behavior, nominal wage dynamics also matter for macroeconomic adjustment. One implication of DNWR is wage compression: when nominal wages are difficult to cut, firms may become more cautious in granting wage increases, thereby compressing wage differentials within firms (Schaefer and Singleton, 2023; Stüber and Beissinger, 2012; Elsby, 2009). More broadly, DNWR reduces labor-market flexibility and may amplify unemployment during downturns by slowing wage adjustment (Bewley, 1999). In the Chinese context, by contrast, greater nominal wage flexibility may permit faster wage adjustment in response to adverse conditions. This possibility is one reason why the Chinese case is of broader interest for understanding the relationship between wage-setting institutions and macroeconomic adjustment (Xu et al., 2012; Lardy and Subramanian, 2011).

In summary, the literature suggests that DNWR is a common feature of many labor markets, while nominal wage cuts are usually limited under normal conditions and often concentrated around major shocks. China stands out because nominal wage cuts appear relatively common and recurrent at the aggregate level. At the same time, most of the literature on nominal wage cuts has focused on their measurement, macroeconomic consequences, or relationship with unemployment, rather than on their distribution across workers (Bauer et al., 2007). This leaves room for a closer examination of worker-level vulnerability to nominal wage cuts in China. By documenting how wage-cut exposure differs across education, gender, tenure, employer type, and industry, the paper provides evidence on the distributional dimension of nominal wage flexibility and, in the implications section, discusses how these patterns may relate to workers' outside options and institutional protection.

3 Institutional Setting and Empirical Motivation

This section discusses the institutional evolution of China's labor market and uses it to motivate the paper's empirical focus on heterogeneous exposure to nominal wage cuts. The central point is not that a single institutional mechanism fully explains nominal wage adjustment, but rather that China's labor-market institutions may create uneven protection across workers, employers, and sectors. This unevenness suggests that nominal wage cuts may not be distributed uniformly across the workforce. Worker exposure to downward wage adjustment may depend on employment protection, employer type, industry, labor-market attachment, and individual characteristics associated with outside opportunities. The historical development of labor regulation and enterprise reform, therefore, matters not only as background, but also because it helps explain why some groups of workers may be more vulnerable to nominal wage cuts than others. Broadly speaking, this evolution can be divided into three periods: before 1995, from 1995 to 2008, and from 2008 onwards.

Before 1995: Transition period

During this period, China's transition from a planned economy to a market-oriented economy exposed major gaps in worker protection. The emergence of privately owned enterprises (POEs) was concentrated in labor-intensive industries that relied heavily on rural surplus labor. This contributed to low wages and weakly regulated working conditions, which were further intensified by the fragmented policy-making process. Although broad policy directions were issued by the central government, implementation depended heavily on lower levels of government and often lacked binding force in practice, leaving workers more exposed to poor working conditions. Work intensity during this period was also substantial. Research conducted in Beijing in 1993 indicated that 12% of employees worked 10 to 12 hours per day, 5% worked 12 to 14 hours per day, and 2% worked more than 14 hours per day (Ngok, 2008).

For the purpose of this paper, the key implication is that employment protection was highly uneven across sectors and employer types. During this period, the distinction between state-owned enterprises (SOEs) and non-SOEs was particularly pronounced. Because several labor protections applied primarily to SOEs rather than to POEs or foreign-funded enterprises (FFEs), workers in SOEs generally enjoyed stronger protection under state regulation. A similar pattern applied to civil servants, whose employment conditions were more closely governed by formal rules. This institutional divide suggests that workers located in more protected parts of the labor market may have faced different exposure to unfavorable wage adjustments than workers in more market-exposed firms.

1995–2008: Introduction of China's Labor Law

In an effort to improve working conditions, the 1995 Labor Law introduced maximum working hours, overtime pay, written contracts, and workplace-safety requirements. It also established a three-step procedure for labor disputes, beginning with mediation between workers and firms, followed by arbitration, with the courts serving as a final step. In principle, these reforms should have strengthened workers' formal protection by clarifying labor rights and creating channels through which workers could contest adverse employment decisions.

In practice, however, employers often found ways to circumvent these regulations. Many contracts remained short-term, limiting employees' job security, and social-insurance costs were frequently shifted onto workers. Labor-dispute procedures also offered limited practical protection, since only more serious violations could be brought to court, and adjudication often took considerable time. As a result, the formal expansion of labor protections did not necessarily translate into uniform effective protection across workers. This gap between legal protection and enforcement in practice is important for understanding why nominal wage cuts could remain common even after labor regulation became more developed.

2008–Present: Introduction of the Labor Contract Law

The Labor Contract Law, issued by the National People's Congress in 2008, was designed to reinforce existing protections concerning working hours and contracting. Among other provisions, firms were required to give advance notice for layoffs involving more than 20 workers, dismissed workers were to receive priority in re-employment (Meng, 2017), minimum compensation for interns was increased, and the law clarified the circumstances under which non-fixed-term contracts should be provided. These reforms again moved in the direction of strengthening formal employment protection.

Even so, important limitations remained. The conditions for obtaining a permanent contract were stringent, enforcement was imperfect, and penalties for non-compliance were often too weak to deter violations effectively. In some cases, firms may have found it less costly to violate labor regulations than to comply with them fully. At the same time, the long-run decline of SOEs and the expansion of more market-oriented firms altered the institutional environment of wage setting. Whereas SOEs had once dominated urban employment, their share fell sharply over time, weakening the reach of employment arrangements traditionally associated with greater wage stability. Taken together, these developments suggest that China's labor market has become one in which formal protections coexist with substantial heterogeneity in workers' effective protection and exposure to wage adjustment.

This institutional history motivates the paper's empirical focus on worker-level vulnerability to nominal wage cuts. Employer type captures one dimension of this heterogeneity. Workers in civil-service positions, SOEs, private-owned enterprises, foreign-funded enterprises, and other employer types may face different wage-setting rules, employment-stability norms, and degrees of exposure to market competition. Industry captures a second dimension. Sectors differ in demand volatility, compensation practices, labor intensity, and the degree to which wage adjustment can substitute for employment adjustment. These differences may affect the likelihood that workers experience nominal wage cuts even among job stayers.

Individual characteristics may also be related to exposure to nominal wage cuts. Education is relevant because it is associated with human capital, productivity, and outside employment opportunities. Tenure captures labor-market attachment and accumulated match-specific capital. Longer tenure may protect workers if it reflects firm-specific human capital or stronger attachment to the employer. Rather, they are observable characteristics that help identify whether downward nominal wage adjustment is concentrated among particular groups of workers.

The institutional setting, therefore, suggests a descriptive empirical question: are nominal wage cuts evenly distributed across job stayers, or are specific groups of workers systematically more exposed to them? The empirical analysis addresses this question by examining differences in wage-cut exposure across education, gender, tenure, employer type, industry, and related controls.

4 Data

To facilitate cross-country comparisons with Western countries and the United States, this paper defines nominal wage change as the percentage increase or decrease in nominal wages relative to the previous survey wave. We use individual-level data from the China Household Finance Survey (CHFS) for empirical analysis, a nationally representative biennial survey conducted by Southwestern University of Finance and Economics (Gan et al., 2014). The CHFS covers most Chinese provinces and collects detailed household- and individual-level information on employment, income, working time, demographic characteristics, and employer characteristics. The survey began in 2011 and subsequently conducted follow-up waves in 2013, 2015, 2017, 2019, and 2021. To avoid the potential influence of the COVID-19 period and to maintain consistency with the main pre-pandemic analysis, this paper uses the 2011, 2013, 2015, 2017, and 2019 waves. Since wage changes are constructed between adjacent survey waves, the resulting wage-change observations are dated 2013, 2015, 2017, and 2019.

To facilitate comparison with the literature on downward nominal wage rigidity, the main analysis focuses on job stayers in paid employment. A worker is classified as a job stayer if the respondent is employed by the same firm in two adjacent waves, is not self-employed or working as a farmer, and reports valid wage and working-time information.[6] Restricting the sample to job stayers helps ensure that measured wage changes primarily reflect within-job nominal wage dynamics rather than wage changes associated with job mobility.

The main wage measure is the change in log hourly nominal wages between adjacent survey waves.[7] Hourly wages are constructed from reported regular salary and working-time information. A nominal wage cut is defined as a negative change in log hourly nominal wages between adjacent waves, while a wage freeze is defined as an exact zero change in hourly nominal wages.

Table 1: Summary statistics for nominal wage dynamics from CHFS

Year	Mean	SD	Median	Nominal wage cut	Obs.	Wage freeze
2013	0.359	0.730	0.256	0.274	954	0.037
2015	0.287	0.622	0.214	0.299	3138	0.002
2017	0.402	0.802	0.223	0.272	4356	0.002
2019	0.176	0.577	0.105	0.374	3800	0.045
Overall	0.299	0.694	0.182	0.311	12248	0.018

Note: Nominal wage cut denotes the share of job stayers who experienced an hourly nominal wage cut between two adjacent survey waves. Wage freeze denotes the share of job stayers whose hourly nominal wage remained exactly unchanged between two adjacent survey waves. The last row reports the overall summary based on the pooled job-stayer sample.
Period: 2013–2019, biennially. Wage changes are constructed from adjacent CHFS waves.
Data: China Household Finance Survey (CHFS).

Table 1 reports summary statistics for hourly nominal wage changes among job stayers. The number of observations is consistent across waves, except in 2013 due to fewer observations from 2011, which served as a test round for the whole survey. The mean and median changes in log hourly nominal wages are positive in every wave, indicating that nominal wage growth remains the dominant outcome for the average worker. At the same time, the mean is more volatile than the median. The mean ranges from 0.176 in 2019 to 0.402 in 2017, while the median ranges from 0.105 in 2019 to 0.256 in 2013. This difference suggests that the mean is more sensitive to large wage increases, whereas the median provides a more stable description of the central tendency of wage changes.

Despite positive average wage growth, nominal wage cuts are common. In the pooled job-stayer sample, 31.1% of observations involve an hourly nominal wage cut between adjacent waves. The incidence of nominal wage cuts is also substantial in each wave: 27.4% in 2013, 29.9% in 2015, 27.2% in 2017, and 37.4% in 2019. These figures show that downward nominal wage adjustment is not confined to a single survey interval. Instead, nominal wage cuts appear repeatedly across the sample period.

Wage freezes are much less common than wage cuts. Only 1.8% of pooled observations report exactly unchanged hourly nominal wages, and the wave-specific freeze rate is close to zero in 2015 and 2017. The highest freeze rates occurred in 2013 and 2019, at 3.7% and 4.5%, respectively. This low incidence of wage freezes, combined with the high incidence of nominal wage cuts, provides the first descriptive indication of weak downward nominal wage rigidity in the CHFS. Taken together, Table 1 suggests that nominal wages in China are flexible downward even among job stayers, while the extent of this flexibility varies across survey intervals.

[6] Operationally, observations with missing or invalid wage, working-hour, working-day, employment-status, or job-continuity information are excluded. The exclusion of farmers refers to self-employed agricultural work; employees in agricultural industries are retained when they satisfy the job-stayer definition.

[7] Throughout the paper, unless otherwise stated, nominal wage changes and nominal wage cuts refer to changes in log hourly nominal wages. Because hourly wages are constructed from reported earnings and working-time information, Section 6 also reports robustness checks using total nominal wages to assess whether the main findings are driven by changes in working hours.

5 Estimation of Downward Nominal Wage Rigidity

5.1 Estimation methods

The preceding sections suggest that nominal wages in China may be substantially more flexible compared to those in the United States or Europe. To examine this issue more formally, this subsection applies four commonly used measures of downward nominal wage rigidity (DNWR) and reports the corresponding incidence of nominal wage cuts. The purpose is threefold. First, the estimates provide direct evidence on the degree of nominal wage rigidity in China. Second, they allow the CHFS evidence to be compared with estimates from other countries using methods that are standard in the literature. Third, it adds robustness to our results.

There are two main approaches to estimating DNWR. The first is direct and measures rigidity from the observed frequency of nominal wage freezes. The second is distribution-based and relies on assumptions about the shape of the counterfactual wage-change distribution.[8] Under the second approach, the observed wage-change distribution is compared with a benchmark distribution that would prevail in the absence of DNWR. Since downward nominal rigidity is typically more relevant than upward nominal rigidity, these methods generally assume that the upper tail of the observed distribution is less affected by rigidity and can therefore be used to infer the missing lower tail.

The first direct measure is the Smith.2000 measure, which defines DNWR as the ratio of workers experiencing nominal wage freezes to the total number of workers (Smith, 2000). The second direct measure is the Dickens.2007 measure, which defines DNWR as the ratio of workers experiencing nominal wage freezes to the sum of workers with nominal wage freezes and nominal wage cuts (Dickens et al., 2007). This measure is based on the idea that, in the absence of DNWR, some workers observed with unchanged nominal wages would instead have experienced nominal wage cuts. The denominator is therefore restricted to workers whose observed outcomes are most likely to be directly affected by downward rigidity.[9]

The two distribution-based measures are Akerlof.1996 and Goette.2007. The Akerlof.1996 measure infers DNWR from the difference between the mass of wage changes below zero and the mass above twice the median wage change (Akerlof et al., 1996). The Goette.2007 measure assumes that the wage-change distribution can be approximated by a two-sided Weibull distribution and infers DNWR by comparing the predicted incidence of nominal wage cuts in the counterfactual distribution with the observed incidence (Goette et al., 2007). To make the Goette.2007 approach more flexible, this paper estimates not only the standard parameters of the two-sided Weibull distribution but also an additional location parameter, l, for each survey wave. This parameter captures the center of the wage-change distribution and is allowed to vary over time. Estimating it separately helps reduce the risk that changing macroeconomic conditions are incorrectly attributed to DNWR (Fallick et al., 2022). Details on the cumulative distribution function, probability density function, and quantile function are provided in Appendix A.1.

5.2 Downward nominal wage rigidity in the CHFS

Table 2 reports the estimated level of DNWR in the CHFS using the four measures described above. The estimates are based on changes in log hourly nominal wages among job stayers. The table also reports the observed incidence of nominal wage cuts, which provides the empirical counterpart to the rigidity estimates. This joint presentation is important because low measured DNWR can arise either because downward wage adjustment is genuinely frequent or because wage growth is so strong that few workers are at risk of a cut. In the CHFS, the first interpretation is more relevant: wage freezes are rare, while nominal wage cuts are common.

Across the four waves, the direct measures imply very low DNWR. The Smith.2000 measure ranges from

[8] While much of the existing literature assumes a symmetric wage-change distribution, some recent contributions allow for asymmetry when identifying the counterfactual distribution; see, for example, (Fallick et al., 2022).

[9] Hereafter, this measure is referred to as the “Dickens.2007” method.

0.002 in 2015 and 2017 to 0.045 in 2019. The Dickens.2007 measure is mechanically larger because it restricts the denominator to workers with either wage freezes or wage cuts, but it remains low in absolute terms, ranging from 0.006 in 2017 to 0.118 in 2013. These low values reflect the small mass of exact wage freezes in the hourly wage-change distribution. At the same time, the incidence of nominal wage cuts is substantial in every wave, ranging from 0.27 in 2013 and 2017 to 0.37 in 2019. Thus, the low level of DNWR in the CHFS is not driven by the absence of workers facing downward wage pressure. Rather, it reflects the fact that many such workers experience actual nominal wage cuts instead of being held at unchanged nominal wages.

Table 2: Estimates of downward nominal wage rigidity in the CHFS

	Direct measures		Distribution-based measures		Nominal wage cuts	Obs.
Year	Smith.2000	Dickens.2007	Akerlof.1996	Goette.2007		
2013	0.037 (2)	0.118 (1)	0.097 (2)	0.008 (2)	0.27	954
2015	0.002 (3)	0.007 (3)	0.060 (3)	-0.032 (3)	0.30	3138
2017	0.002 (4)	0.006 (4)	0.189 (1)	0.051 (1)	0.27	4356
2019	0.045 (1)	0.108 (2)	0.037 (4)	-0.054 (4)	0.37	3800

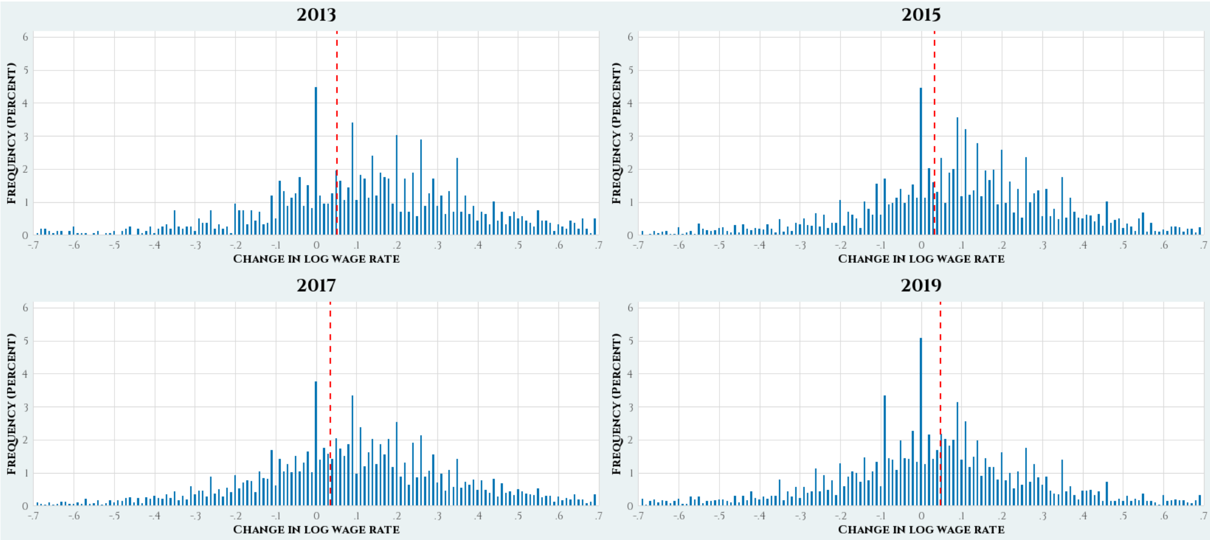
Notes: Numbers in parentheses rank the four waves from highest to lowest within each measure. Smith.2000 is the share of workers experiencing nominal wage freezes among all workers (Smith, 2000). Dickens.2007 is the share of workers experiencing nominal wage freezes among workers with either nominal wage freezes or nominal wage cuts (Dickens et al., 2007). Akerlof.1996 measures DNWR using the difference between the mass of wage changes below zero and the mass above twice the median wage change (Akerlof et al., 1996). Goette.2007 infers DNWR from the counterfactual incidence of nominal wage cuts under a two-sided Weibull distribution (Goette et al., 2007). Nominal wage cuts denote the share of workers experiencing nominal wage cuts between adjacent survey waves. Obs. reports the number of worker-level wage-change observations in each wave.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS).

Although the levels differ across methods, the rankings in Table 2 contain useful information. The two direct measures identify 2013 and 2019 as the waves with relatively higher rigidity and 2015 and 2017 as the waves with very limited rigidity. Their exact ordering differs because the two measures use different denominators. Smith.2000 is driven only by the unconditional frequency of exact wage freezes and therefore ranks 2019 highest. Dickens.2007 scales freezes by the number of workers with either freezes or cuts and therefore ranks 2013 slightly higher than 2019, because the 2013 freeze mass is large relative to the number of workers experiencing either a freeze or a cut. This difference in ordering is therefore mechanical rather than substantive. Both direct measures point to the same conclusion: exact wage freezes are rare in the CHFS.

The two distribution-based measures are more sensitive to the shape of the wage-change distribution. Akerlof.1996 and Goette.2007 give the same ranking across waves: 2017, 2013, 2015, and 2019. This similarity suggests that the distribution-based approaches are responding to common features of the wage-change distribution, even though their estimated levels differ. The 2017 wave receives the highest distribution-based estimate, while 2019 receives the lowest. This pattern contrasts with the direct measures because the distribution-based methods are not driven only by exact wage freezes. They also depend on the distribution of wage changes around zero and on the part of the distribution used to infer the counterfactual lower tail.

The negative Goette.2007 estimates in 2015 and 2019 deserve particular caution. In principle, the Goette.2007 measure estimates the missing mass of nominal wage cuts by comparing the observed incidence of wage cuts with the incidence predicted by a fitted counterfactual distribution. A negative value arises when the fitted counterfactual distribution predicts fewer nominal wage cuts than are actually observed. This does not imply that wages are "negatively rigid." Instead, it indicates that the estimated two-sided Weibull counterfactual does not identify a positive missing mass below zero in those waves. This outcome is plausible in the CHFS because exact wage freezes are scarce and the observed lower tail is already substantial. With little mass at zero to anchor the missing-mass interpretation, the Goette.2007 estimate becomes less precise and more sensitive to the parametric fit of the counterfactual distribution. For this reason, negative estimates are retained as diagnostic information but interpreted as evidence of no detectable DNWR under the Goette.2007 specification.

Figure 1: Distribution of hourly log wage changes in the CHFS



Note: Each panel plots the distribution of changes in log hourly nominal wages between adjacent survey waves for job stayers. Bars report the percentage of observations in each wage-change bin. Exact zero wage changes are retained in the sample, so that the mass at zero captures nominal wage freezes. The plotted range is restricted to wage changes between −0.7 and 0.7 to improve comparability across waves and to reduce the influence of extreme observations. The red dashed line in each subplot is the cumulative inflation rate for the past two years. Inflation rates are reported in Appendix Table A.2.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS), 12,248 observations in total, and the World Bank for the inflation rate.

Figure 1 provides a visual counterpart to Table 2, where the red dashed line represents the inflation rate[10]. The figure shows that the wage-change distribution has substantial mass below zero in every wave, confirming that nominal wage cuts are not confined to a single survey interval. At the same time, the spike at exactly zero is small, especially in 2015 and 2017. This visual pattern is consistent with the direct estimates in Table 2: wage freezes are uncommon, while wage cuts are frequent. The figure also helps explain why distribution-based measures are less stable than direct measures. The lower tail is empirically important, but the shape of the distribution around zero varies across waves, making counterfactual-distribution estimates sensitive to the chosen functional form.

In summary, Table 2 and Figure 1 support three conclusions. First, the direct measures provide clear evidence of weak DNWR in the CHFS because exact nominal wage freezes are rare. Second, nominal wage cuts are common among job stayers in all waves, implying that low measured rigidity reflects actual downward nominal wage adjustment rather than simply rapid nominal wage growth. Third, the distribution-based measures provide complementary but less precise evidence, especially when the fitted counterfactual distribution predicts no positive missing mass below zero. For comparability with the international literature and for transparency in interpretation, the discussion below focuses primarily on Smith.2000, while treating the other measures as robustness and diagnostic evidence.

[10] Although this paper focuses on nominal wages in this paper, the less spike at the inflation rate indicates less real wage rigidity for China.

5.3 Cross-country comparison of DNWR

6 Measurement Error and Robustness Checks

Before turning to the analysis of worker-level exposure to nominal wage cuts, it is important to address two measurement and robustness concerns. The first concerns shifts in total working hours, which serve as the denominator in constructing the hourly wage. The main analysis defines nominal wage changes using log hourly wages, constructed from reported nominal wages and total working hours. In the Chinese labor market, however, working schedules can change substantially across survey waves. If total working hours increase while reported earnings remain unchanged, the constructed hourly wage mechanically declines. In that case, some measured hourly wage cuts could reflect changes in working time rather than genuine downward adjustment in nominal earnings. To assess this possibility, this section repeats the main descriptive and DNWR exercises using total wages. If nominal wage cuts remain common when wages are measured by total earnings rather than hourly wages, then changes in working hours are unlikely to be the primary reason for China's weak DNWR and high incidence of nominal wage cuts.

The second concern is reporting errors in survey wages. Survey respondents may report approximate wages rather than exact amounts, generating heaping at round numbers and potentially artificial wage changes between adjacent waves. If such reporting behavior creates spurious wage cuts, the observed incidence of nominal wage cuts may overstate the true extent of downward nominal wage adjustment. To assess this possibility, this section follows the rounding-based approach in Xu et al. (2012) and examines whether reporting heaping in the CHFS can account for the observed incidence of nominal wage cuts.

6.1 Working-time variation and total-wage robustness

The first robustness exercise examines whether the main findings are sensitive to the use of hourly wages. This issue is especially relevant because hourly wages are not reported directly in the CHFS but are constructed by dividing reported nominal wages by reported total working hours. As a result, changes in reported working days or working hours may affect measured hourly wage growth even when total earnings do not fall. If the high incidence of hourly nominal wage cuts were mainly driven by increases in working time, then wage cuts should become much less frequent when nominal wage changes are measured using total wages. The purpose of this subsection is therefore not to provide a separate analysis of total-wage dynamics, but to examine whether the central findings from the hourly-wage analysis survive once the working-time denominator is removed.

Appendix Table A.4 further illustrates its relevance. Hourly nominal wage cuts are more frequent when reported total working hours increase: 56.96% of observations with increased hours experience an hourly wage cut, compared with 31.96% when hours are unchanged and 21.89% when hours decline. This pattern confirms that changes in working time affect the measured hourly wage. At the same time, hourly wage cuts are not confined to observations with rising hours. A non-negligible share of workers still experience hourly wage cuts when total working hours are unchanged or decline. This motivates the total-wage robustness check below: if the prevalence of hourly wage cuts were only a denominator effect, it should largely disappear when nominal wage changes are measured using total wages.

Table 3 shows that the main conclusion from the hourly-wage analysis is not driven primarily by changes in reported working time. The incidence of total-wage cuts is slightly lower than the corresponding incidence of hourly-wage cuts, as expected if part of the hourly-wage variation reflects changes in total working hours. However, the difference is not substantial enough to overturn the observed pattern. In the pooled sample, 27.1% of job-stayer observations experience a decline in total nominal wages, compared with 31.1% in the hourly-wage analysis. Across survey waves, total-wage cuts remain substantial in every interval, ranging from 21.3% in 2013 to 32.0% in 2019.

Table 3: Total-wage robustness for nominal wage dynamics from CHFS

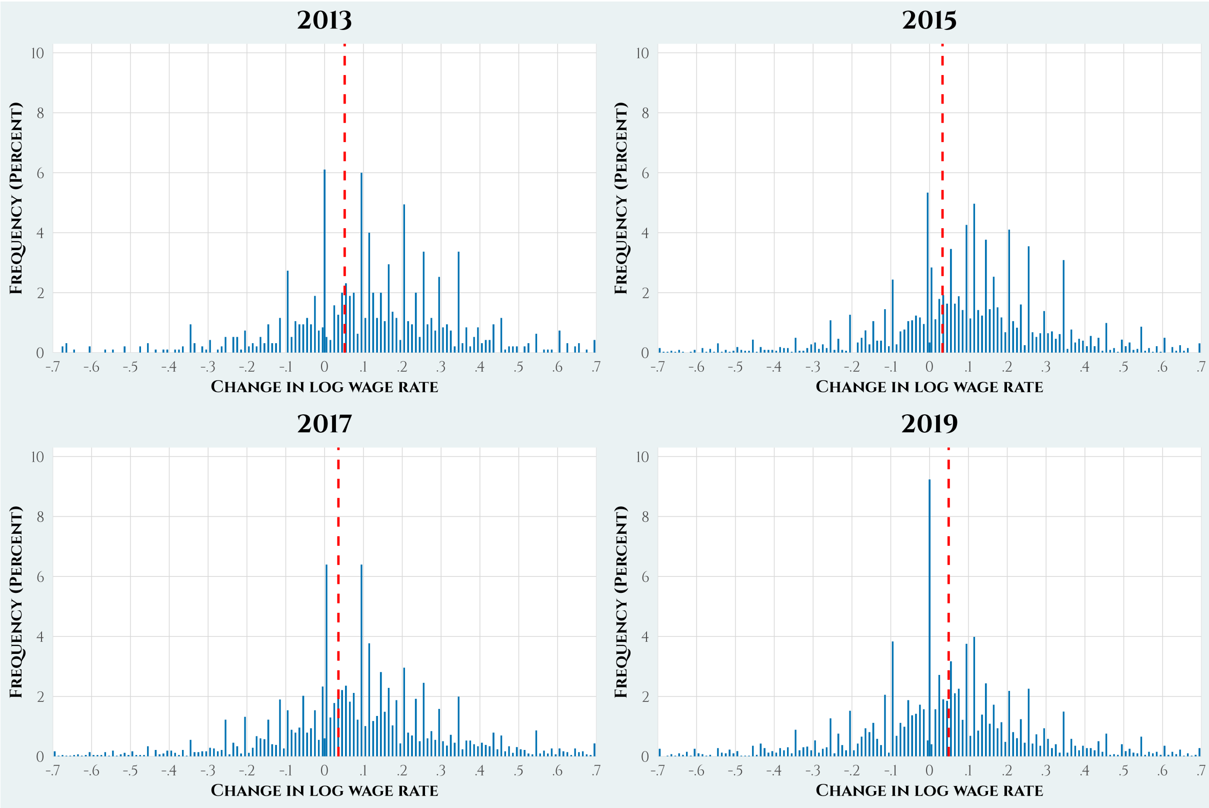
Panel A. Summary statistics for log total nominal wage changes						
Year	Mean	SD	Median	Wage freeze	Wage cuts	Obs.
2013	0.342	0.610	0.237	0.059	0.213	984
2015	0.278	0.568	0.223	0.003	0.267	3309
2017	0.376	0.755	0.182	0.006	0.244	4482
2019	0.179	0.543	0.105	0.091	0.320	4004
Overall	0.286	0.641	0.182	0.036	0.271	12779

Panel B. Alternative DNWR measures						
Year	Smith.2000	Dickens.2007	Akerlof.1996	Goette.2007	Wage cuts	Obs.
2013	0.059 (2)	0.216 (2)	0.278 (2)	0.167 (1)	0.21	984
2015	0.003 (4)	0.012 (4)	-0.001 (4)	-0.014 (4)	0.27	3309
2017	0.006 (3)	0.022 (3)	0.294 (1)	0.128 (2)	0.24	4482
2019	0.091 (1)	0.221 (1)	0.154 (3)	0.049 (3)	0.32	4004

Notes: Panel A reports summary statistics for changes in log total nominal wages among job stayers. Wage cuts denote the share of job stayers whose total log nominal wage declined between two adjacent survey waves. Wage freeze denotes the share of job stayers whose total nominal wage remained exactly unchanged between two adjacent survey waves. Panel B reports alternative measures of downward nominal wage rigidity calculated using total nominal wages. Numbers in parentheses rank the four waves from highest to lowest within each measure. Smith.2000 is the share of exact wage freezes. Dickens.2007 is the share of exact freezes among observations with either wage freezes or wage cuts. Akerlof.1996 and Goette.2007 are distribution-based measures.
Period: 2013–2019, biennially. Wage changes are constructed from adjacent CHFS waves.
Data: China Household Finance Survey (CHFS).

Thus, removing the working-time denominator reduces the measured incidence of nominal wage cuts, but it does not make wage cuts rare.

Figure 2: Distribution of total log wage changes in the CHFS



Note: Each panel plots the distribution of changes in log total nominal wages between adjacent survey waves for job stayers. Bars report the percentage of observations in each wage-change bin. Exact zero wage changes are retained in the sample, so that the mass at zero captures total nominal wage freezes. The plotted range is restricted to wage changes between -0.7 and 0.7 to improve comparability across waves and reduce the influence of extreme observations. The number of observations by wave is 984 in 2013, 3,309 in 2015, 4,482 in 2017, and 4,004 in 2019. The red dashed line in each subplot is the cumulative inflation rate for the past two years. Inflation rates are reported in Table A.2. *Period*: 2013–2019, biennially. *Data*: China Household Finance Survey (CHFS), 12,779 observations in total, and the World Bank for the inflation rate.

The comparison of wage freezes leads to the same interpretation. Total-wage freezes are somewhat more common than hourly-wage freezes, with a pooled freeze rate of 3.6%. This is consistent with the idea that some hourly-wage changes are generated by variation in working time. Nevertheless, exact freezes remain much less common than wage cuts under the total-wage measure and are still at a relatively low level internationally. The direct DNWR measures, therefore, continue to imply weak downward nominal wage rigidity. In other words, the total-wage specification shifts some observations away from hourly wage cuts and toward unchanged total earnings, but the dominant feature of the data remains a sizeable lower tail with a low spike at zero.

The alternative DNWR measures in Table 3 Panel B reinforce this point. Compared with the hourly-wage estimates, the total-wage estimates generally show somewhat more rigidity in years with a larger mass of exact total-wage freezes, especially 2013 and 2019. However, the main takeaway message of the evidence remains similar to Table 2: wage cuts are common, exact freezes are limited, and no measure suggests that nominal wage cuts disappear or significantly drop once total wages are used. The total-wage results should therefore be read as a robustness check on the hourly-wage findings rather than as a competing wage concept.

Figure 2 provides the visual counterpart to this comparison. As in the hourly-wage distribution, the total-wage distribution contains substantial mass below zero in all four survey intervals. This is the key robustness result. Since total wages are not mechanically reduced when reported working time increases, the negative mass in Figure 2 cannot be attributed to the change of total working hours.

Taken together, Table 3 and Figure 2 suggest that total working hours changes are not the primary reason why the CHFS shows weak DNWR and frequent nominal wage cuts. The total-wage measure reduces the incidence of wage cuts relative to the hourly-wage measure, suggesting that variation in working hours matters for the exact magnitude of measured hourly wage changes. However, the persistence of a large lower tail of total wages shows that this channel cannot explain the lower DNWR and prevalence of hourly nominal wage cuts. The evidence is therefore more consistent with genuine downward adjustment in reported nominal wages, although the CHFS does not allow us to distinguish reductions in base pay from changes in bonuses, subsidies, or other variable wage components.

6.2 Reporting heaping in survey wages

The second robustness exercise examines reporting heaping in survey wages. Measurement errors can be divided into structural-level and individual-level errors. Structural-level errors are difficult to eliminate when nominal wage changes are constructed from firm-level or aggregate data, because such data often rely on average wages and are therefore vulnerable to the replacement effect. For example, if high-wage workers leave a firm and are replaced by lower-wage workers, the average wage may fall even though no incumbent worker experiences an actual wage cut. By contrast, the CHFS records wages at the individual level and allows wage changes to be constructed for the same workers across adjacent waves. This avoids the replacement-effect problem that arises in aggregate wage data.

Individual-level measurement error is more subtle. Survey respondents may report approximate wages rather than exact amounts, generating artificial spikes at rounded values and potentially distorting estimates of wage rigidity (Gottschalk, 2005). For simplicity, the remainder of this subsection uses "measurement error" to refer to this individual-level reporting problem. Several approaches have been proposed in the literature, each of which relies on different identifying assumptions.

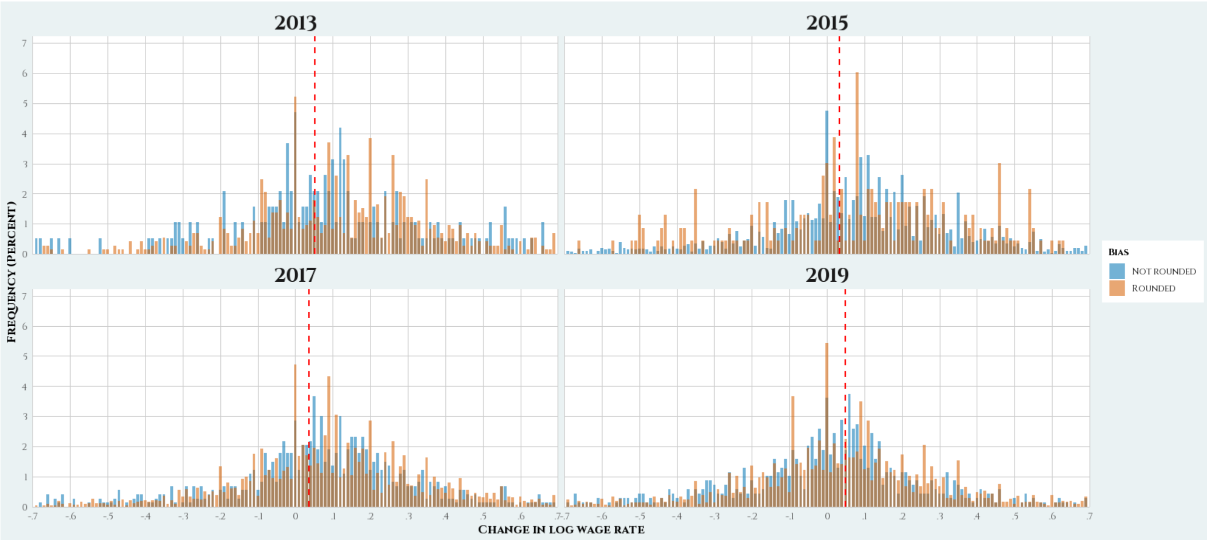
Altonji and Devereux (2000), for example, model the reported wage as the sum of the true wage and a measurement-error term and identify the latter through a mixed distribution.[11] This approach is elegant but relies on relatively strong parametric assumptions, which are less attractive in the present setting. Dickens et al. (2007) instead assume that reporting errors are correlated with previously reported wages and use a multiple structural-change model to recover the underlying wage-change distribution. That approach relaxes some distributional assumptions, but it is better suited to longer panels with many wage observations per individual. Given the shorter panel dimension of the CHFS, it is less well matched to the present analysis.[12]

This paper adopts a rounding-based approach. The basic idea is that respondents who report total nominal wages divisible by round numbers are more likely to be rounded, potentially leading to biased records. For a given threshold—for example, 100, 500, or 1000—the sample is divided into two groups: observations whose reported wages are exactly divisible by the threshold and observations whose wages are not. The first group is treated as more likely to reflect rounded wage reporting (hence, more likely to be biased), while the second group is treated as less likely to do so (hence, more likely to be non-biased). This approach is attractive here because it is conservative and does not require strong parametric assumptions about the measurement-error process.

[11] Their framework assumes that the error term equals zero with probability p and otherwise follows a normal distribution.

[12] A further concern is that strong identifying assumptions may overstate the extent of measurement error and, in turn, understate the prevalence of genuine wage cuts.

Figure 3: Distribution of hourly log wage changes by reported-wage rounding (1000)



Note: Each panel plots the distribution of changes in log hourly nominal wages between adjacent survey waves, separately for observations classified as rounded and not. Rounding status is defined by whether the reported nominal wage is exactly divisible by the rounding threshold, which is 1000 here. Following the reporting-rounding interpretation in the wage-rigidity literature, divisible observations are treated as more likely to reflect rounded wage reporting. Bars report frequency percentages for each rounding-status group. Exact zero wage changes are retained in the sample, so that the mass at zero captures nominal wage freezes. The plotted range is restricted to wage changes between -0.7 and 0.7 to improve comparability across waves and reduce the influence of extreme observations. The number of observations by wave is 954 in 2013, 3,138 in 2015, 4,356 in 2017, and 3,800 in 2019. The red dashed line in each subplot is the cumulative inflation rate for the past two years. Inflation rates have been stated in detail in A.2.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS), 12,248 observations in total, and the World Bank for the inflation rate.

Figure 3 first illustrates the wave-specific wage-change distributions when the rounding threshold is set to 1000. This threshold classifies 61.4% of observations as rounded and potentially biased, producing two groups that remain large enough for comparison. Across waves, the rounded and non-rounded distributions overlap substantially. Both groups exhibit a sizable lower tail, indicating that negative hourly wage changes are not confined to potential bias from rounded reporting. The figure also shows that the percentage of wage freezes remains minor relative to the mass of nominal wage cuts. This visual evidence suggests that potential bias has a limited impact on the distribution of hourly log wage changes, and hence, rounding in reported wages does not eliminate the central fact that nominal wage cuts are prevalent.

Table 4 reports the corresponding comparison across several more rounding thresholds. The first pattern is that a large share of reported wages is divisible by round numbers, especially at lower thresholds. For example, 75.6% of observations are divisible by 10, 74.3% by 100, and 61.4% by 1000. This confirms that reporting heaping is relevant in the CHFS and should not be ignored.

The second pattern is that nominal wage cuts are even more prevalent among observations classified as less likely to be rounded. Across all thresholds, the non-divisible group has a wage-cut rate of 36.4%-37.6%. These rates are not only substantial; they are also slightly higher than the corresponding rates for divisible observations. For example, at the 1000 threshold, the wage-cut rate is 34.1% among divisible observations and 37.6% among non-divisible observations. At the 5000 threshold, the corresponding rates are 33.5% and 36.5%. Thus, it further verifies that removing or separating observations with potential bias does not reduce the incidence of nominal wage cuts to a negligible level.

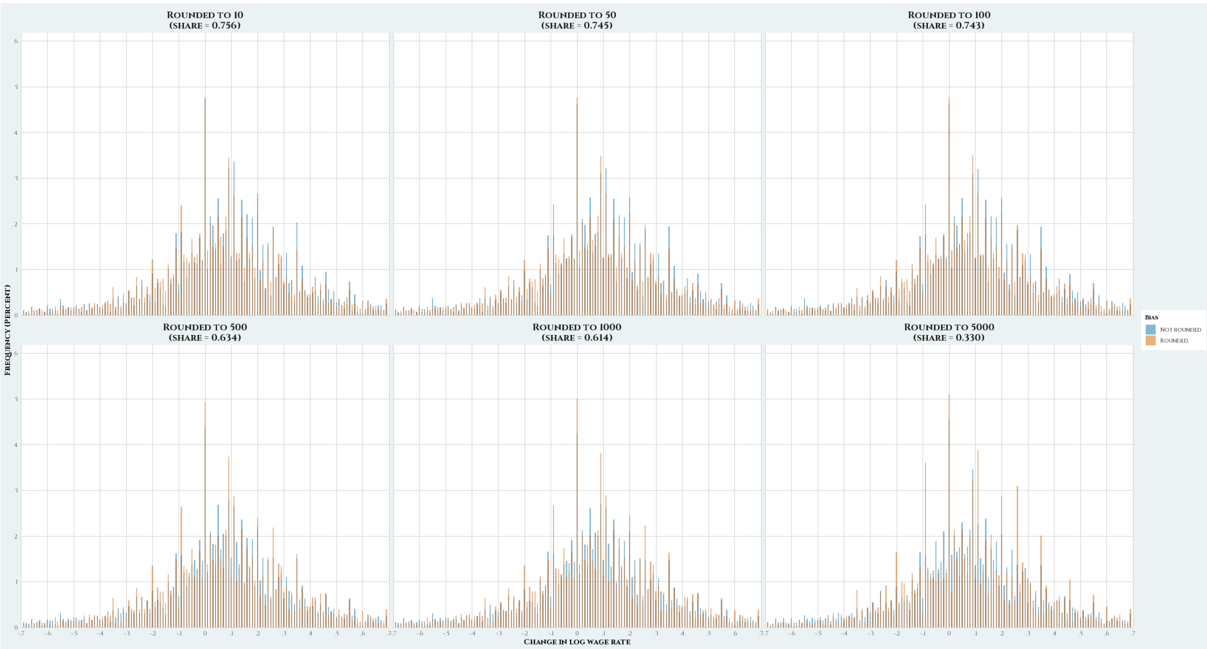
The third pattern concerns wage freezes. Exact wage freezes are rare in both groups, but they are especially rare among non-divisible observations. At thresholds of 10, 50, and 100, the wage-freeze rate among non-divisible

Table 4: Nominal wage cuts and ratio of biased/unbiased samples for CHFS

Num	Divisible	Mean of Wage Change		Wage Cuts (per.)		Wage Rigidity (per.)		T-stat	Sign.
		Divisible	Not	Divisible	Not	Divisible	Not		
10	0.756	0.093	0.038	0.352	0.364	0.022	0.000	1.251	
50	0.745	0.095	0.036	0.351	0.367	0.023	0.000	1.719	*
100	0.743	0.096	0.033	0.350	0.369	0.023	0.000	1.950	*
500	0.634	0.100	0.045	0.349	0.365	0.023	0.006	1.822	*
1000	0.614	0.109	0.033	0.341	0.376	0.024	0.005	4.072	***
5000	0.330	0.123	0.058	0.335	0.365	0.023	0.014	3.322	***

Note: The first column reports the rounding threshold used to classify whether nominal wages are exactly divisible by that number. The second column reports the share of observations whose reported wages are divisible by the listed threshold. Following the reporting-rounding interpretation in the wage-rigidity literature, divisible observations are treated as more likely to reflect rounded reporting and hence potentially biased. Columns 3–4 report mean wage growth for divisible and non-divisible observations, respectively. Columns 6–7 report the proportion of nominal wage cuts in the two groups. Columns 9–10 report the corresponding proportion of exact wage freezes, that is, the Smith.2000 measure of Downward Nominal Wage Rigidity, in the two groups. Column 11 reports the t-statistic for the difference in nominal wage-cut rates between the two groups, and Column 12 reports the associated significance level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS), 12,248 observations in total.

Figure 4: Distribution of hourly log wage changes by reporting-rounding status in the CHFS



Note: Each panel plots the pooled distribution of changes in log hourly nominal wages between adjacent survey waves, separately for observations classified as rounded and not rounded. Rounding status is defined by whether the reported nominal wage is exactly divisible by the threshold shown in the panel title. The share reported in each panel title gives the fraction of observations classified as rounded under that threshold. Following the reporting-rounding interpretation in the wage-rigidity literature, divisible observations are treated as more likely to reflect rounded wage reporting. Bars report frequency percentages for each rounding-status group. Exact zero wage changes are retained in the sample, so that the mass at zero captures nominal wage freezes. The plotted range is restricted to wage changes between −0.7 and 0.7 to improve comparability across thresholds and reduce the influence of extreme observations.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS), 12,248 observations in total.

observations is essentially zero. This suggests that some exact wage freezes may indeed be related to rounded reporting. However, this reinforces rather than weakens the main conclusion on DNWR: if exact freezes are partly generated by reporting heaping, then the already low Smith.2000 estimates should be interpreted cautiously as possible upper bounds on true wage rigidity. By contrast, the high incidence of wage cuts among non-divisible observations indicates that nominal wage cuts themselves cannot be explained away by rounding.

Figure 4 provides a broader robustness check by plotting pooled wage-change distributions for divisible and non-divisible observations under several rounding thresholds. The purpose of this figure is not to select one preferred threshold, but to examine whether the role of reporting rounding changes materially when different definitions of rounded wages are applied. The same pattern appears across thresholds: the rounded and non-rounded distributions overlap substantially, and both retain a clear mass of negative wage growth. The figure therefore confirms the message from Table 4.

In summary, Figure 3, Figure 4, and Table 4 lead to a clear conclusion. Reporting rounding is present in the CHFS and may affect the precise measurement of wage freezes. In particular, exact wage freezes are more common among observations divisible by round-number thresholds, suggesting that some measured freezes may partly reflect reporting heaping. However, this concern does not explain the prevalence of nominal wage cuts. Across thresholds, observations less likely to be rounded continue to show high wage-cut rates and substantial mass below zero in the wage-change distribution. The evidence, therefore, suggests that nominal wage cuts in China are not merely an artifact of survey misreporting.

7 Empirical Analysis

7.1 Descriptive analysis

After showing that nominal wage cuts remain substantial when using total wages and after accounting for reporting heaping, this section first compares the level of DMWR internationally before examining how nominal wage-change distributions differ across workers. The purpose of the descriptive analysis is not to identify causal effects, but to provide initial evidence on whether exposure to nominal wage cuts differs across observable worker and job characteristics. This analysis focuses on four dimensions that are also used in the subsequent regression analysis: gender, education, marital status, and firm type.

Following the same approach as Xu et al. (2012),[13] Table 5 compares China with other countries using the Smith.2000 measure. The ranking is based on the mean level of nominal wage rigidity. Since most international estimates are based on hourly wages, the hourly-wage estimate for China is the most directly comparable benchmark. The total-wage estimate is reported as an additional comparison because it uses a broader wage concept and helps assess whether the conclusion of low DNWR is sensitive to the wage definition.

The comparison shows that China's labor market exhibits low wage rigidity compared with both European countries and the United States, consistent with earlier evidence. Using hourly wages, China has an average DNWR of 1.8 %, placing it at the bottom of the cross-country distribution reported in Table 5. Using total wages, China's average DNWR rises to 3.6 %, but it remains below the United States and most European countries in the comparison, except for Ireland on average. Put differently, even the broader total-wage measure places China well below countries such as Germany, Belgium, Italy, the Netherlands, and the United States. These findings point to substantially greater downward nominal wage flexibility in China than in most Western economies and suggest a weaker impact of working hours on the level of hourly DNWR, which we would further explore in the next section. We are aware that Table 5 should be treated as a benchmarking exercise rather than as a strict international ranking. Cross-country estimates differ in survey design, sample selection, wage concepts, observation intervals, and institutional context. The comparison is nevertheless informative because both the hourly- and total-wage estimates for China imply low DNWR by international standards.

Figure 5 compares pooled distributions of changes in log hourly nominal wages by gender, education, firm type, and marital status. Three patterns are worth noting. First, all distributions retain substantial mass below zero, confirming that nominal wage cuts are present across the groups shown in the figure. Second, the distributions are

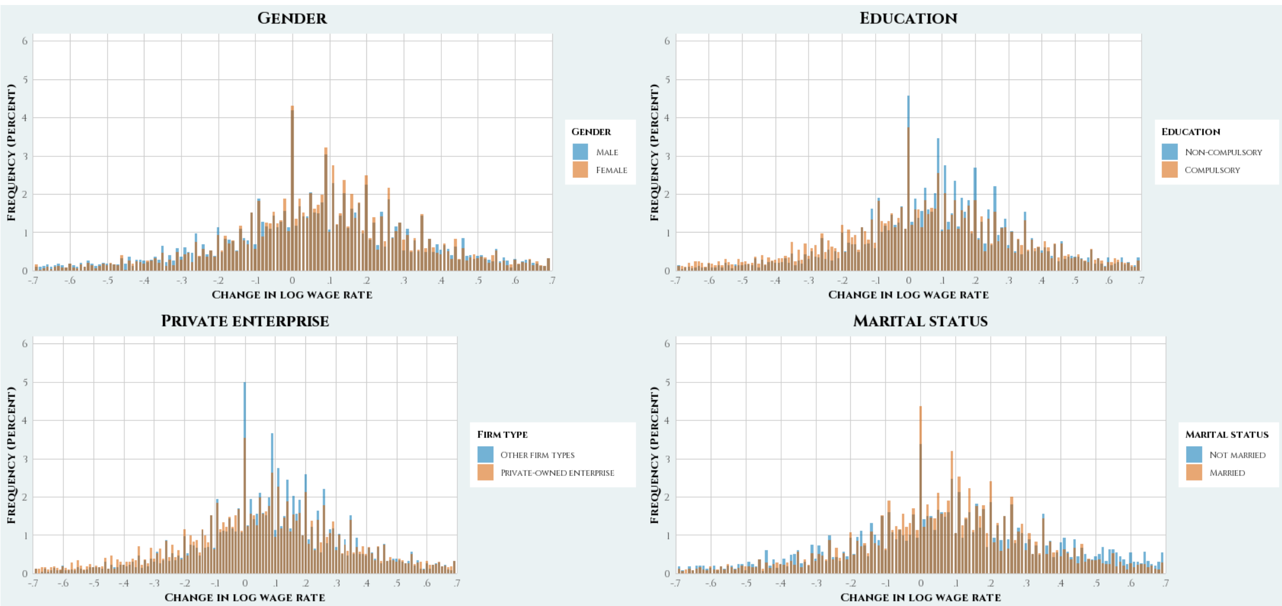
[13] The data source for each country is listed in detail in Table A.1.

Table 5: Downward nominal wage rigidity across countries

Rank	Country	Nominal wage rigidity		
		Mean (%)	Max (%)	Min (%)
1	Germany	36.1	46.0	26.3
2	Belgium	33.7	36.7	30.7
3	Italy	27.7	28.8	26.6
4	Luxembourg	27.1	32.4	21.8
5	Denmark	20.2	22.0	18.5
6	Portugal	15.9	19.3	12.4
7	Netherlands	15.4	16.8	14.0
8	Greece	12.0	12.7	11.4
9	United States	11.4	17.1	5.7
10	Sweden	10.0	15.0	5.0
11	France	9.9	10.4	9.4
12	United Kingdom	7.7	15.1	0.3
13	Spain	4.2	4.4	4.0
14	China (total)	3.6	9.1	0.3
15	Ireland	2.3	2.4	2.2
16	China	1.8	4.5	0.0

Notes: DNWR is measured using the Smith.2000 method, defined as the share of workers experiencing nominal wage freezes. Countries are ranked by the mean level of nominal wage rigidity. China is reported separately using hourly wages and total wages. The hourly-wage estimate is more directly comparable with the international literature, while the total-wage estimate is included as a robustness check.

Figure 5: Distribution of hourly log wage changes by worker characteristics



Note: Subplots report pooled distributions of changes in log hourly nominal wages by the worker characteristics indicated in the panel titles. Bars report frequency percentages within each group. Wage changes are constructed between adjacent survey waves, and exact zero wage changes are retained in the sample.
Period: 2013–2019, biennially. Wage changes are constructed using adjacent CHFS waves.
Data: China Household Finance Survey (CHFS), 12,248 observations in total.

relatively smooth and display a small spike at exactly zero, especially when compared with other countries, where they normally exhibit 10% to 30% wage freezes. This is consistent with the earlier evidence that exact nominal wage freezes are rare in the CHFS. Third, the extent of separation differs across characteristics. The education and private-enterprise panels show clearer group differences: Non-compulsory and non-POE employees have visibly more mass on the right-hand side of the distribution, while the others have relatively more mass on the left-hand side. This pattern suggests that the two groups differ in the overall location of the wage-change distribution, and that non-compulsory and non-POE employees have a generally lower probability of experiencing nominal wage cuts. By contrast, the distributions of gender and marital status overlap more closely, indicating weaker visual separation between male and female workers. These descriptive patterns motivate the panel regressions below, where the same characteristics are examined jointly with additional controls.

However, the recurrence of nominal wage cuts in the aggregate distribution does not imply that the same workers repeatedly experience wage cuts. Table 6 reports a dynamic matrix across observed wage-change intervals at the individual level. The table is constructed from workers who are observed for at least three survey waves and therefore have at least two wage-change intervals. The unit of observation is a person-interval pair, so workers observed in more than two wage-change intervals may contribute more than one transition pair.

Table 6: Dynamics of nominal wage changes in the CHFS at the individual level

	Next observed interval		
Previous observed interval	Wage increase	Wage cut	**Total**
Wage increase	1,196 (38.9%)	909 (29.6%)	2,105 (68.5%)
Wage cut	798 (26.0%)	168 (5.5%)	966 (31.5%)
Total	1,994 (64.9%)	1,077 (35.1%)	3,071 (100.0%)

Notes: The table reports transitions between wage-change outcomes across observed wage-change intervals. Rows indicate the outcome in the previous observed interval, while columns indicate the outcome in the next observed interval. The unit of observation is a person-interval pair. Workers observed in more than two wage-change intervals may contribute more than one transition pair. Percentages in all cells are expressed as shares of the full transition-pair sample. Conditional probabilities by previous wage-change outcome are discussed in the text. The final row and the rightmost column report the distribution of next-interval outcomes across all transition pairs.
Period: 2011–2019, biennially.
Data: China Household Finance Survey (CHFS), 3,071 transition pairs.

Table 6 contains 3,071 transition pairs. In the next observed interval, 1,077 transition pairs, or 35.1%, involve a nominal wage cut. Among workers who experienced a wage increase in the previous observed interval, 909 out of 2,105 transition pairs are followed by a nominal wage cut, corresponding to 43.2%. Among workers who experienced a nominal wage cut in the previous observed interval, 168 out of 966 transition pairs are followed by another nominal wage cut, corresponding to 17.4%, which is significantly lower compared to 35.1%, the percentage of nominal wage cuts if they are independent across waves. Thus, despite wage cuts being consistent at the aggregate level, they are not concentrated among workers who had already experienced a wage cut in the previous observed interval. In the full matched transition sample, only 168 out of 3,071 transition pairs, or 5.5%, involve nominal wage cuts in both the previous and next observed intervals. This distinction is important: nominal wage cuts are prevailing in the pooled distribution, but individual-level persistence is limited. As a robustness check, Appendix table A.3 states a similar pattern, but with total log wage.

7.2 Panel regressions

To examine worker-level exposure to nominal wage cuts in a more stringent setting, this subsection estimates panel regressions in which the dependent variable equals one if a worker experiences an hourly nominal wage cut between two adjacent survey waves and zero otherwise. The empirical objective is descriptive: the regressions examine whether nominal wage cuts are systematically related to observable worker and job characteristics, rather than testing a specific structural model. Table 7 reports descriptive statistics for the variables used in the regression analysis. Since the regressors enter the model as indicator variables, the reported means can be read as category shares in the estimation sample.

Table 7: Descriptive statistics for variables of interest

Status	Category	Share
Gender	Male	0.598
	Female	0.402
Age	Below 25	0.006
	25–34	0.168
	35–44	0.366
	45–54	0.357
	55+	0.104
Education level	Compulsory	0.340
	Secondary	0.408
	Higher	0.252
Marital status	Cohabited	0.002
	Divorced	0.019
	Married	0.876
	Separated	0.001
	Single	0.095
	Widowed	0.008
Employer type	Civil servant	0.285
	Foreign-funded enterprises	0.037
	Other	0.031
	Private-owned enterprises	0.434
	State-owned enterprises	0.214
Industry	Agriculture, forestry, animal husbandry, and fishery	0.023
	Mining and manufacturing	0.255
	Construction	0.098
	Electricity, heat, gas, and water production and supply	0.037
	Wholesale and retail	0.042
	Transport, storage, and postal services	0.087
	Hospitality and food service	0.034
	Information transmission, computer services, and software	0.025
	Finance	0.049
	Real estate	0.009
	Science, education, culture, and health	0.234
	Domestic residential services and other services	0.070
	Public administration and social organization	0.029
	Other	0.008
Residence	Rural	0.123
	Urban	0.877
Tenure	Below 1 year	0.067
	1–2 years	0.044
	2–5 years	0.149
	5–10 years	0.200
	Above 10 years	0.540

Notes: The table reports category shares for the variables used in the CHFS panel regressions. Each entry is the mean of the corresponding indicator variable. Shares are calculated within each variable and may not sum exactly to one because of rounding.
Period: 2013–2019, biennially.
Data: China Household Finance Survey (CHFS).

Table 7 shows that the estimation sample is 59.8% male and 40.2% female. In terms of education, 34.0% of observations have compulsory education, 40.8% have secondary education, and 25.2% have higher education. Private-owned enterprises account for the largest employer-type category, at 43.4%, followed by civil servants at 28.5% and state-owned enterprises at 21.4%. The two largest industry groups are mining and manufacturing, at 25.5%, and science, education, culture, and health, at 23.4%. The sample is predominantly urban, with 87.7% of observations classified as urban. Tenure is also highly concentrated at the upper end of the distribution: 54.0% of observations have tenure above 10 years.

Given the unbalanced panel structure of the CHFS, the choice of estimator requires some care. Table 8 reports both individual fixed-effects and random-effects linear probability models. The fixed-effects estimates use within-worker variation and therefore absorb time-invariant worker characteristics, including gender. The random-effects estimates preserve both within- and between-worker variation and are therefore more informative for characteristics that change slowly over time, such as education, employer type, and industry. By contrast, a fixed-effects specification is less suitable for the main job-stayer sample because the combination of a relatively small and unbalanced panel with categorical covariates leaves too little within-person variation for some variables, especially given the unbalanced structure of the dataset (Maddala and Mount, 1973; Baltagi and Song, 2006). Hence, we keep it as a robustness check for the result. Standard errors are heteroskedasticity-robust and clustered at the individual level.

The estimated model is

$$\text{Nominal wage cut}_{it} = \beta^{\intercal} \cdot \mathbf{X}_{it} + \alpha_i + u_{it}, \qquad t = 13, 15, 17, 19 \tag{1}$$

where Nominal wage cutit is an indicator that equals one if worker i experiences an hourly nominal wage cut between two adjacent survey waves and zero otherwise; $\vec{X}it$ contains the explanatory variables listed in Table 7; α_i denotes the worker-specific component; and u_{it} is the idiosyncratic error term. Because the dependent variable is binary, the coefficients can be interpreted as changes in the probability of experiencing a nominal wage cut.

Table 8 reports four specifications. Column (1) includes marital status, education, and tenure, together with the auxiliary controls described in the table notes. Column (2) adds employer-type indicators. Column (3) adds industry indicators. Column (4) includes all reported groups jointly. Panel A reports fixed-effects estimates, while Panel B reports random-effects estimates.

Table 8: Panel regressions of nominal wage-cut indicators from CHFS

	Dependent variable: Nominal wage cut (= 1)											
	Base			**+ Firm type**			**+ Industry**			**All**		
	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.
Panel A: Random effects estimates												
Gender												
Female	-0.022	0.007	***	-0.017	0.007	*	-0.014	0.007	*	-0.012	0.007	.
Wald χ^2: Gender	10.751	[0.001]	***	5.902	[0.015]	*	3.862	[0.049]	*	2.786	[0.095]	.
Marital status (ref. Married)												
Single	-0.035	0.014	*	-0.034	0.014	*	-0.041	0.014	**	-0.040	0.015	**
Cohabited	-0.038	0.086		-0.039	0.086		-0.037	0.085		-0.036	0.086	
Separated	-0.148	0.095		-0.134	0.098		-0.145	0.092		-0.133	0.097	
Divorced	-0.009	0.024		-0.025	0.025		-0.007	0.025		-0.024	0.025	
Widowed	-0.035	0.037		-0.021	0.039		-0.042	0.036		-0.027	0.039	
Wald χ^2: Marital status	9.697	[0.084]	.	8.685	[0.122]		11.937	[0.036]	*	10.452	[0.063]	.
Education (ref. Compulsory education)												
Higher education	-0.081	0.010	***	-0.053	0.011	***	-0.063	0.011	***	-0.047	0.012	***
Secondary education	-0.051	0.008	***	-0.035	0.009	***	-0.042	0.008	***	-0.032	0.009	***
Wald χ^2: Education	72.148	[0.000]	***	25.858	[0.000]	***	37.364	[0.000]	***	18.886	[0.000]	***
Tenure (ref. less than 2 years)												
2–5 years	-0.027	0.015	.	-0.026	0.015	.	-0.027	0.015	.	-0.026	0.015	.
5–10 years	-0.010	0.015		-0.010	0.015		-0.012	0.015		-0.012	0.015	
Above 10 years	0.009	0.014		0.013	0.015		0.010	0.014		0.012	0.015	
Wald χ^2: Tenure	14.418	[0.002]	**	16.408	[0.001]	***	15.718	[0.001]	***	15.242	[0.002]	**
Firm type (ref. Civil servant)												
State-owned enterprise				0.018	0.010	.				0.006	0.012	
Private-owned enterprise				0.044	0.009	***				0.027	0.012	*
Foreign-funded enterprise				0.011	0.020					-0.014	0.021	
Other firm type				0.036	0.021	.				0.030	0.022	

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Table 8: Panel regressions of nominal wage-cut indicators from CHFS *(continued)*

	Dependent variable: Nominal wage cut (= 1)											
	Base			**+ Firm type**			**+ Industry**			**All**		
	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.
Wald χ^2*: Firm type*				23.522	[0.000]	***				10.085	[0.039]	*
Industry (ref. Agriculture, forestry, animal husbandry, and fishery)												
Mining and manufacturing							0.037	0.024		0.022	0.025	
Construction							0.069	0.025	**	0.041	0.027	
Electricity, heat, gas, and water							0.020	0.028		0.007	0.029	
Wholesale and retail							0.043	0.027		0.027	0.028	
Transport, storage, and postal services							0.056	0.026	*	0.047	0.027	.
Hospitality and food service							0.072	0.029	*	0.059	0.030	.
Information, computer services, and software							0.013	0.031		-0.001	0.032	
Finance							0.013	0.028		0.000	0.029	
Real estate							0.024	0.036		0.002	0.037	
Science, education, culture, and health							0.009	0.024		0.004	0.025	
Residential and other services							0.023	0.025		0.015	0.026	
Public administration and social organization							-0.027	0.025		-0.027	0.026	
Other industry							0.043	0.027		0.028	0.028	
Wald χ^2*: Industry*							49.450	[0.000]	***	27.168	[0.012]	*
Within R^2		0.014			0.014			0.016			0.016	
Model F		256.559			247.734			296.090			268.580	
p-value		0.000			0.000			0.000			0.000	
Panel B: Fixed-effects estimates												
Marital status (ref. Married)												
Single	-0.108	0.117		-0.103	0.120		-0.135	0.134		-0.126	0.132	
Cohabited	0.081	0.185		0.109	0.190		0.094	0.186		0.127	0.192	
Separated	-0.430	0.215	*	-0.430	0.217	*	-0.428	0.215	*	-0.425	0.217	*
Divorced	-0.052	0.106		-0.030	0.108		0.013	0.121		0.046	0.121	
Widowed	0.155	0.178		0.286	0.176		0.148	0.177		0.278	0.175	
Wald χ^2*: Marital status*	5.936	[0.313]		7.831	[0.166]		6.358	[0.273]		8.579	[0.127]	
Education (ref. Compulsory education)												
Higher education	-0.090	0.071		-0.094	0.072		-0.061	0.073		-0.058	0.074	
Secondary education	-0.088	0.054	.	-0.088	0.055		-0.060	0.055		-0.057	0.055	
Wald χ^2*: Education*	2.738	[0.254]		2.628	[0.269]		1.214	[0.545]		1.051	[0.591]	
Tenure (ref. less than 2 years)												
2–5 years	-0.005	0.049		-0.011	0.050		0.005	0.050		0.001	0.052	
5–10 years	-0.019	0.051		-0.017	0.052		-0.012	0.053		-0.012	0.054	
Above 10 years	-0.053	0.052		-0.049	0.053		-0.045	0.054		-0.045	0.055	
Wald χ^2*: Tenure*	2.381	[0.497]		1.782	[0.619]		2.231	[0.526]		1.903	[0.593]	
Firm type (ref. Civil servant)												
State-owned enterprise				-0.018	0.043					-0.023	0.044	
Private-owned enterprise				-0.005	0.043					-0.013	0.044	
Foreign-funded enterprise				-0.152	0.092	.				-0.185	0.093	*
Other firm type				0.019	0.050					0.021	0.051	
Wald χ^2*: Firm type*				3.544	[0.471]					4.938	[0.294]	
Industry (ref. Agriculture, forestry, animal husbandry, and fishery)												
Mining and manufacturing							0.022	0.083		0.006	0.083	
Construction							0.073	0.096		0.062	0.097	
Electricity, heat, gas, and water							-0.036	0.098		-0.053	0.098	
Wholesale and retail							0.012	0.101		-0.004	0.101	
Transport, storage, and postal services							0.058	0.092		0.049	0.094	
Hospitality and food service							-0.062	0.112		-0.072	0.113	
Information, computer services, and software							0.005	0.122		0.016	0.123	
Finance							-0.124	0.113		-0.133	0.114	
Real estate							0.019	0.119		-0.015	0.123	
Science, education, culture, and health							-0.041	0.085		-0.057	0.086	
Residential and other services							0.054	0.082		0.044	0.083	
Public administration and social organization							-0.015	0.079		-0.028	0.080	
Other industry							0.026	0.080		0.010	0.081	
Wald χ^2*: Industry*							11.142	[0.599]		11.008	[0.610]	
Within R^2		0.011			0.012			0.013			0.015	
Model F		2.591			2.160			1.674			1.591	
p-value		0.000			0.002			0.012			0.016	

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Table 8: Panel regressions of nominal wage-cut indicators from CHFS *(continued)*

	Dependent variable: Nominal wage cut (= 1)											
	Base			**+ Firm type**			**+ Industry**			**All**		
	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.	Coef.	SE	Sign.

Source: China Household Finance Survey (CHFS).
Period: Employees observed in the CHFS panel between 2013 and 2019. The dependent variable equals one if the worker experiences an hourly nominal wage cut between two adjacent survey waves.
Estimation method: Panel A reports random-effects estimates, which are preferred by the Hausman test. Panel B reports individual fixed-effects estimates. Standard errors are heteroskedasticity-robust and clustered at the individual level.
Notes: All specifications control for age category, year indicators, and rural/urban status. Marital status and tenure estimates are reported explicitly. The random-effects specifications in Panel A additionally control for region dummies. These auxiliary controls, together with the intercept in the random-effects specifications, are not reported coefficient-by-coefficient. The female coefficient is reported only in Panel A because it is absorbed by individual fixed effects in Panel B. Column (1) includes marital status, education, and tenure controls; column (2) adds firm-type controls; column (3) adds industry controls; column (4) includes all controls jointly. Wald-test rows report chi-square statistics, with corresponding p-values in brackets. Reference categories are the omitted categories used in the underlying regressions.
Sign. level: ., *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

The random-effects estimates in Panel A show stronger and more stable associations. The coefficient on female workers is negative in all specifications, declining in magnitude from −0.022 in the baseline specification to −0.012 in the full specification. The estimate remains statistically significant or marginally significant across columns. Marital status is jointly significant or marginally significant in several specifications, with single workers having a lower estimated probability of a nominal wage cut than married workers. In the full specification, the coefficient on single workers is −0.040 with a standard error of 0.015.

Education displays the most stable pattern in Panel A. Relative to workers with compulsory education, both higher and secondary education are negatively associated with the probability of experiencing a nominal wage cut across all specifications. In the full specification, the coefficient is −0.047 for higher education and −0.032 for secondary education, both statistically significant at the 0.1 percent level. The joint Wald tests for education are also significant at the 0.1 percent level in all specifications.

Marital status also shows some association with nominal wage-cut exposure in the random-effects estimates, although the pattern is less stable than for education. Relative to married workers, single workers have a consistently lower estimated probability of experiencing a nominal wage cut. The coefficient on single workers ranges from −0.034 to −0.041 across specifications and remains statistically significant in all columns. In the full specification, the coefficient is −0.040 with a standard error of 0.015. The other marital-status categories are not individually significant, and their estimated coefficients are less stable. The joint Wald tests for marital status are statistically significant or marginally significant in three of the four random-effects specifications, but marital status is not jointly significant in the fixed-effects specifications. Overall, marital status is associated with wage-cut exposure in the random-effects models, but the evidence is weaker than that for education and appears to be driven mainly by differences between single and married workers.

Tenure is jointly significant in all random-effects specifications, although the individual coefficients are concentrated in the 2–5 year category. Relative to workers with tenure below two years, the coefficient for 2–5 years of tenure is approximately −0.026 to −0.027 and is marginally significant across columns. The coefficients for 5–10 years and above 10 years are smaller and individually not statistically significant, indicating that the joint tenure effect is not driven by a monotonic nominal wage cut across all tenure categories.

Employer type is jointly significant in the random-effects specifications. Relative to civil servants, private-owned enterprise employment is positively associated with the probability of a nominal wage cut. The coefficient is 0.044 in Column (2) and 0.027 in the full specification. The estimate remains statistically significant after adding industry controls. State-owned enterprises and foreign-funded enterprises do not show statistically significant coefficients in the full specification, while the broader employer-type group remains jointly significant.

Industry is also jointly significant in Panel A. In the specification with industry controls but without employer type, construction, transport, storage, and postal services, and hospitality and food service are positively associated with the probability of a nominal wage cut relative to agriculture, forestry, animal husbandry, and fishery. In the full specification, the coefficients for transport, storage, and postal services and hospitality and food service remain marginally significant, while the construction coefficient becomes smaller and statistically insignificant. The joint Wald test for industry remains statistically significant in the full specification.

The fixed-effects estimates in Panel B are generally less precise than the random-effects estimates in Panel A and hence serve as a robustness check. For marital status, the coefficient on separated workers is negative and statistically significant across specifications, but the joint Wald tests for marital status are not statistically significant. Education categories are negative throughout Panel B, but the joint tests for education are insignificant across all specifications. Tenure coefficients are also mostly negative, especially for workers with more than 10 years of tenure, but the tenure group is not jointly significant. Among employer types, the coefficient on foreign-funded enterprises is negative and becomes statistically significant in the full specification, at -0.185 with a standard error of 0.093. However, the joint Wald tests for employer type are not significant in Panel B. Industry indicators are also jointly insignificant in the fixed-effects specifications. The lower within-individual variation across waves might be the primary reason for this lack of significance, as job-stayers are less likely to change their education levels, industries, or firm types unless they receive on-the-job training or the firm changes its field. This further strengthens our decision to present random-effect estimates as the primary results.

Overall, Table 8 shows that the estimated associations are stronger in the random-effects specifications than in the fixed-effects specifications. In Panel A, gender, marital status, education, tenure, employer type, and industry show statistically significant or marginally significant associations with nominal wage-cut exposure. In Panel B, most variable groups are no longer jointly significant after including worker fixed effects. The interpretation of these associations is detailed in Section 8. For the purposes of this section, the main empirical conclusion is that nominal wage cuts are not distributed evenly across observable worker and job characteristics.

8 Implications for China's labor market

The results presented above have implications beyond the measurement of downward nominal wage rigidity. The central finding is not simply that DNWR is low in China, but that downward nominal wage adjustment appears to be a regular margin through which China's labor market reallocates economic pressure. Across CHFS waves, nominal wage cuts are common among job stayers, while exact nominal wage freezes remain rare. Using the Smith.2000 measure, China ranks near the bottom of the cross-country distribution of wage rigidity when hourly wages are used (Smith, 2000; Xu et al., 2012). This suggests that China's labor market differs from many Western economies not only in the level of measured DNWR, but also in the way adverse conditions are absorbed. In economies with stronger DNWR, downward pressure on labor costs is more likely to appear through employment adjustment, reduced hiring, or delayed wage growth (Bewley, 1999; Goette et al., 2007). In China, by contrast, part of this pressure appears to pass directly into nominal wage changes.

Although they cannot be tested empirically in this paper due to data limitations, several potential mechanisms may underlie nominal wage flexibility in China. The first interpretation concerns the wage fluctuation generated by performance-based pay. Performance-based pay links earnings to individual or team output, making wages more responsive to firm revenue, economic conditions, and individual performance (Lazear, 2018). The widespread adoption of performance-based pay schemes in China, such as piece rates and tournaments, may introduce a distinctive source of nominal wage volatility. With firms legally permitted to base a large share of pay on performance, workers may assume substantial income risk and become more exposed to nominal wage cuts during economic downturns or personal performance slumps (Merchant et al., 2011; Qian, 2016). Up to 58.3% of workers have performance components in their contracts, with 16.5% of workers fully paid on performanceMerchant et al. (2011). This pay structure may thus increase the likelihood of nominal wage cuts, particularly as firms adjust compensation to align with fluctuating revenues and economic cycles. Unfortunately, in the current CHFS analysis, we cannot test this channel directly because the dataset does not provide the wage-component information. However, the mechanism nevertheless remains relevant for interpreting the evidence: recurrent nominal wage cuts with limited persistence at the individual level are consistent with a wage-setting environment in which part of the nominal wage fluctuates with performance-related components rather than through permanent reductions in base wages.

A second interpretation concerns workers' bargaining position, which can be understood at both the aggregate and the individual levels. At the aggregate level, workers in many OECD and Scandinavian countries are protected by wage-setting institutions in which bargaining occurs not only at the individual level, but also at the firm, sectoral, or national level (Bhuller et al., 2022). In China, wage negotiation is more decentralized and is more frequently conducted at the individual level (Friedman, 2017; Liu and Kuruvilla, 2016). Workers are therefore needed to negotiate for themselves, which weakens their ability to resist downward nominal adjustment. This institutional feature is reinforced by uneven enforcement of labor regulation and by the long-run transformation of China's employment structure from SOEs toward POEs and FFEs (Duan, 2022; Meng, 2017). Since protection is generally less comprehensive in POEs, workers in more market-exposed firms may face a lower bargaining position at the aggregate level and greater exposure to nominal wage cuts. This is consistent with the regression result from Table 8, where employment in POEs is associated with a higher probability of experiencing nominal wage cuts compared to other firms.

At the individual level, bargaining position is closely tied to outside options. Workers with relatively poor outside options may be more inclined to accept nominal wage cuts because they cannot substantially improve their situation by leaving their current job. They may prefer accepting a wage cut if the reduction is less detrimental than the costs associated with job search, unemployment, or mobility. Conversely, workers with higher-quality outside options should be less tolerant of nominal wage cuts, because they can credibly switch to alternative employment (Caldwell and Danieli, 2024). The CHFS results are consistent with this interpretation. Education provides the clearest evidence: workers with secondary or higher education are less likely to experience nominal wage cuts than workers with compulsory education. This pattern may reflect higher productivity, stronger human capital, better outside options, and hence a stronger bargaining position (Attanasio and Kaufmann, 2017). Marital status also provides additional evidence. It impacts outside options through constraining the pool of alternative job opportunities. Married workers are less flexible about moving long distances and, thereby, restrict their outside options to a narrower set, leading to lower-quality outside options.

A third interpretation concerns the trade-off between nominal wage cuts, dismissal, and firm distress. When firms face adverse shocks, they can adjust labor costs through several margins: reducing wages, reducing employment, cutting hours, delaying hiring, or, in more severe cases, exiting the market. If nominal wages are rigid downward, firms may be more likely to rely on layoffs or non-renewal of contracts when labor costs must fall. If nominal wages are more flexible, part of the adjustment can instead take the form of wage cuts among job-stayers. From the worker's perspective, accepting a nominal wage cut may be preferable to dismissal if the outside option involves unemployment, costly job search, geographic mobility, or a lower-quality job match. From the firm's perspective, cutting wages may preserve existing employment relationships and firm-specific human capital. Recent evidence supports the relevance of this margin: Davis and Krolikowski (2025) show that many workers would accept pay cuts to save their jobs, while Bertheau et al. (2025) show that wage cuts are not rare among firms experiencing revenue reductions, even though layoffs remain more prevalent. Relatedly, Yokoyama and Obara (2017) model and estimate the trade-off between wage cuts and layoffs, showing that wage cuts can serve as a substitute for layoffs, and therefore can operate as a device for reducing layoffs.

Apart from these interpretations, the prevalence of nominal wage cuts also has implications for the business cycle. DNWR has long been treated as an important channel through which shocks are propagated over the business cycle (Goette et al., 2007; Daly and Hobijn, 2014). If nominal wages cannot fall, negative demand shocks may lead to unemployment because firms cannot easily reduce labor costs by cutting wages. Strong DNWR can therefore reduce labor-market flexibility, increase unemployment during downturns, and slow recovery after these shocks (Bewley, 1999; **?**). In China, by contrast, greater nominal wage flexibility may allow firms to adjust wages downward more quickly in response to external shocks, such as financial crises or pandemics. This may help explain why China's economy displays a less volatile business cycle and is also consistent with the broader argument that China's macroeconomic adjustment after adverse shocks has relied partly on flexible labor-market and wage-setting arrangements (Lardy and Subramanian, 2011; Xu et al., 2012).

However, greater nominal wage flexibility should not be interpreted as purely beneficial. It may reduce pressure on the employment margin, but it also increases workers' exposure to income risk. If adverse shocks are absorbed through nominal wage cuts rather than layoffs, the macroeconomic cost may appear less clearly in unemployment statistics but more clearly in household income volatility, consumption risk, and unequal exposure across worker groups. This is particularly important in the Chinese context because the burden of downward wage adjustment is not evenly distributed. Workers with lower education, weaker outside options, and employment in more market-exposed firms or sectors may bear a disproportionate share of wage flexibility. Thus, the Chinese case suggests a different form of labor-market adjustment: one in which macroeconomic flexibility is achieved partly through worker-level exposure to downward nominal wage risk.

9 Conclusions

This paper documents a distinctive form of nominal wage adjustment in China's labor market. Using individual-level data from the China Household Finance Survey (CHFS), it shows that DNWR is weak by international standards and that nominal wage cuts are common among job stayers. They recur across adjacent survey waves, remain visible when total wages are used instead of hourly wages, and persist after separating observations more and less likely to reflect rounded wage reporting. Hence, these wage cuts are not primarily driven by a single downturn, such as a financial crisis or pandemic, changes in total working hours, or survey-heaping in wage reporting. Despite its persistence at the aggregate level, nominal wage cuts are less consistent at the individual level, with only 5.5% of workers experiencing them for two consecutive waves. At the same time, nominal wage flexibility is not evenly distributed across workers. Workers with compulsory education, those employed in POEs, and married workers appear more vulnerable to nominal wage cuts, whereas workers with higher education, employment in more protected settings, or who are single are less exposed.

The contribution of the paper is threefold. First, it extends the literature on downward nominal wage rigidity in China by placing greater emphasis on the mechanisms that may lead to low DNWR. The existing evidence has identified China as an outlier in terms of its low DNWR and a persistently high incidence of nominal wage cuts, which serve as a regular margin of labor-market adjustment rather than an exceptional response to major downturns. We discuss potential underlying mechanisms: the prevalence of performance-based pay introduces fluctuations in the nominal wage, and the institutional setting of the decentralized bargaining process weakens collective bargaining power in China, leading to less resistance to nominal wage cuts. Second, the paper discusses the implications of China's distinctive nominal wage dynamics in relation to broader economic features. Since nominal wage cuts partially substitute for other adjustment margins, such as layoffs, the high percentage of nominal wage cuts may reduce firms' reliance on layoffs as an alternative margin of labor-cost adjustment. Third, the paper adds a distributional dimension to the DNWR literature by examining which workers are more exposed to nominal wage cuts. The evidence aligns with the interpretation of the outside-option interpretation: workers with better outside options, such as highly educated or single workers, are less likely to accept nominal wage cuts. Although the paper does not test these mechanisms causally due to data limitations, it shows why they are central to understanding the economic implications of China's high wage flexibility.

The policy implication is not that China should mechanically increase nominal wage rigidity. The evidence instead points to a need to distinguish productive nominal wage flexibility from excessive worker-level exposure to wage risk. Some downward wage adjustment may help firms respond to changes in demand, performance, and revenue without relying immediately on layoffs. However, when variable wage components are opaque, wage bargaining is highly decentralized, and labor protection is unevenly enforced, nominal wage flexibility can transfer firm-level shocks to workers with weaker outside options. A balanced policy response should therefore focus less on eliminating all wage-adjustment margins and more on improving the institutional conditions under which wage adjustment occurs. Clearer separation between base wages, bonuses, subsidies, and performance-related components, greater transparency in wage contracts, stronger enforcement of existing labor protections, and better income insurance for workers exposed to wage cuts would help preserve legitimate adjustment while reducing excessive income risk. In this sense, China's low DNWR may facilitate macroeconomic adjustment, but it does so partly by reallocating adjustment costs within the labor market rather than by removing them altogether.

The paper has several limitations. First, the analysis relies on survey-reported wages, which may be affected by rounding, recall error, and imprecise reporting of working time. The robustness checks suggest that neither working-time variation nor reporting heaping overturns the main finding that nominal wage cuts are common, but survey data remain less precise than administrative payroll records. Second, although the total-wage results show that wage cuts also appear in reported earnings themselves, the CHFS does not allow us to distinguish reductions in base wages from changes in bonuses, subsidies, allowances, or other variable wage components. Third, the CHFS panel is relatively short and unbalanced, which limits the ability to study long-run wage trajectories and repeated wage cuts over many consecutive periods. Fourth, the empirical analysis is primarily descriptive. The regressions document systematic associations between nominal wage-cut exposure and observable worker or job characteristics, but they do not identify the causal effect.

These limitations point to a clear agenda for future research. Administrative payroll data or matched worker-firm data would enable distinguishing permanent base-wage cuts from temporary adjustments in bonuses, subsidies, allowances, or other variable wage components. Such data could also test whether performance-based pay is an important source of nominal wage volatility and whether workers with a larger variable-pay share are more likely to experience nominal wage cuts. Matched worker-firm data would further allow researchers to examine whether wage cuts are concentrated in firms facing revenue declines, liquidity pressure, or employment reductions, thereby clarifying whether nominal wage cuts substitute for layoffs or occur alongside other adjustment margins. Future work could also measure outside options and bargaining environments more directly, for example by linking wage changes to local labor-market conditions, job mobility opportunities, contract type, union presence, or enforcement of labor regulation. These extensions would help distinguish between several possible explanations for China's high nominal wage-cut rate: wage volatility generated by variable pay, weak worker bargaining positions, firm-side adjustment to adverse shocks, and institutional differences in labor protection. A fuller account of these mechanisms would clarify not only why nominal wage cuts are common in China, but also which workers bear the risks created by a highly flexible nominal wage-setting environment.

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A Appendix

A.1 Two-sided Weibull distribution

Following Chen and Gerlach (2013), we simplify the distribution by assuming the restrictions that $\frac{\lambda_1}{k_1} = \frac{\lambda_2}{k_2} = \frac{1}{2}$ and $k_1 = k_2$. Therefore, the pdf for a random variable $X \sim STW(\lambda, l)$ is:

$$f(x|\lambda, l) = \begin{cases} b_p(-\frac{-b_p(x-l)}{\lambda})^{2\lambda-1} \cdot e^{-(\frac{-b_p(x-l)}{\lambda})^{2\lambda}}, \text{ for } x < l \\ b_p(\frac{b_p(x-l)}{\lambda})^{2\lambda-1} \cdot e^{-(\frac{b_p(x-l)}{\lambda})^{2\lambda}}, \text{ for } x \geq l \end{cases} \tag{2}$$

where b_p is defined as:

$$b_p = \lambda \cdot \sqrt{\Gamma(1 + \frac{1}{\lambda})}$$

The corresponding cdf is:

$$F(x|\lambda, l) = \begin{cases} \frac{1}{2}e^{-(\frac{b_p(x-l)}{\lambda})^{2\lambda}}, \text{ for } x < l \\ 1 - \frac{1}{2}e^{-(\frac{b_p(x-l)}{\lambda})^{2\lambda}}, \text{ for } x \geq l \end{cases} \tag{3}$$

The inverse cdf or quantile function of an STW is:

$$F^{-1}(\alpha|\lambda, l) = \begin{cases} l - \frac{\lambda}{b_p}\left[-log(2\alpha)\right]^{\frac{1}{2\lambda}}, \text{ for } 0 \leq \alpha < \frac{1}{2} \\ l + \frac{\lambda}{b_p}\left[-log(2(1-\alpha))\right]^{\frac{1}{2\lambda}}, \text{ for } \frac{1}{2} \leq \alpha < 1 \end{cases} \tag{4}$$

Note that the STW we mentioned here is a derivation from Chen and Gerlach (2013) that, with a shift l.

A.2 Source of literature mentioned in comparison

Table A.1: Summary table for wage rigidity within other literature

Countries	Literature	Data source and duration
Germany, Belgium, Luxembourg, Italy, Denmark, Portugal, Netherlands, United Kingdom, Greece, France, Spain, Ireland	Dessy (2002)	European Community Household Panel, 1994–1996
United States	Card and Hyslop (1997)	Panel Study of Income Dynamics, 1976–1979 and 1985–1988; Current Population Survey, 1979–1993
Switzerland	Fehr and Goette (2005)	Swiss Labor Force Survey; Social Insurance Files, 1990–1997
United States	Elsby (2009)	Current Population Survey, 1971–2002
United Kingdom	Smith (2000)	British Household Panel Study, 2001–2006
United Kingdom	Nickell and Quintini (2003)	British New Earnings Survey Panel Data, 1976–1998
United Kingdom	Elsby (2009)	British New Earnings Survey Panel Data, 1976–2001
China	This paper	China Household Financial Survey, 2011–2019, biennally

Note: The papers listed above apply comparable approaches to measuring downward nominal wage rigidity. Elsby (2009) is used in place of the earlier working-paper version of Elsby because it is the published journal version.

A.3 Annual inflation rates in China

Table A.2: Annual inflation rates in China

Year	Inflation rate (%)
2011	5.55
2012	2.62
2013	2.62
2014	1.92
2015	1.44
2016	2.00
2017	1.59
2018	2.07
2019	2.90

Notes: The table reports annual inflation rates for China, expressed in percentages. These values are used to construct the inflation benchmark in the comparison with biennial nominal wage changes. Since the wage-change outcomes are measured between adjacent survey waves, the relevant two-year inflation benchmark can be obtained by compounding the annual rates over the corresponding two-year interval.
Data: World Bank.

A.4 Dynamic log total wage change

Table A.3: Dynamics of nominal wage changes in the CHFS at the individual level

	Next observed interval		
Previous observed interval	Wage increase	Wage cut	**Total**
Wage increase	1,492 (46.6%)	797 (24.9%)	2,289 (71.4%)
Wage cut	767 (23.9%)	148 (4.6%)	915 (28.6%)
Total	2,259 (70.5%)	945 (29.5%)	3,204 (100.0%)

Notes: The table reports transitions between wage-change outcomes across observed wage-change intervals. Rows indicate the outcome in the previous observed interval, while columns indicate the outcome in the next observed interval. The unit of observation is a person-interval pair. Workers observed in more than two wage-change intervals may contribute more than one transition pair. Percentages in all cells are expressed as shares of the full transition-pair sample. Conditional probabilities by previous wage-change outcome are discussed in the text. The final row and the rightmost column report the distribution of next-interval outcomes across all transition pairs.
Period: 2011–2019, biennially.
Data: China Household Finance Survey (CHFS), 3,204 transition pairs.

A.5 Robustness check for total wage change

Table A.4: Changes in working time and hourly nominal wage dynamics

Panel A: Joint distribution of total-hours changes and hourly wage changes				
Total hours / hourly wage change	Wage ↑	Wage =	Wage ↓	Row total
Hours ↑	13.22	0.18	18.64	32.04
Hours =	24.21	1.38	10.70	36.29
Hours ↓	25.40	0.12	6.14	31.67
Column total	62.83	1.69	35.48	100.00
Panel B: Hourly wage-cut rates by changes in working days and hours				
Hours / days change	Days ↑	Days =	Days ↓	Row average
Hours ↑	75.07	55.13	39.54	56.96
Hours =	55.83	29.40	18.69	31.96
Hours ↓	36.75	19.29	16.56	21.89
Column average	57.13	32.87	22.79	31.12

Notes: Panel A reports the joint distribution of changes in total working hours and changes in hourly nominal wages between adjacent survey waves. Entries are percentages of the estimation sample. Panel B reports the percentage of observations experiencing an hourly nominal wage cut within each combination of changes in working hours and working days. Arrows indicate increases, no change, and decreases between adjacent survey waves. Shares may not sum exactly to totals because of rounding.
Data: China Household Finance Survey (CHFS).