Essays on prices and frictions

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ABSTRACT

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This dissertation consists of three essays on prices and frictions. The first chapter documents cyclical properties of distributions of labor factor prices, wages, in the United States from 1979 to 2016. The second chapter investigates which theory of nominal wage frictions in the existing literature has consistent implications with empirical regularities documented in the first chapter. The third chapter estimates the impact of e-commerce, a recent technology innovation reducing information frictions and trade costs, on prices and welfare in Japan.

In Chapter 1, I construct distributions of individual workers' year-over-year changes in nominal hourly wages across time and across US states from two nationally representative household surveys, the Current Population Survey (1979-2017) and the Survey of Income and Program Participation (1984-2013). The novel result is that the share of workers with no wage changes, which accounts for the large spike at zero in nominal wage change distribution, is more countercyclical than the share of workers with wage cuts. A strand of related literature interpreted the empirical finding that US states with larger decreases in employment are also the states with lower average wage increases as a sign of wage flexibility. This paper overturns this interpretation by showing that the states with larger employment declines are also the states with greater increases in the share of workers with a zero wage change, suggesting wage rigidity instead.

In Chapter 2, I ask which type of nominal wage rigidity model in the existing literature can match empirical regularities documented in Chapter 1. This chapter builds

heterogeneous agent models with five alternative wage-setting schemes—perfectly flexible, Calvo, long-term contracts, menu costs, and downward nominal wage rigidity. The models feature not only idiosyncratic uncertainty but also aggregate uncertainty. Using a numerical method, I show among alternative wage setting schemes, the model with downward nominal wage rigidity has the most consistent implications with the empirical findings, regarding the shape and cyclicality of wage change distributions.

In Chapter 3, joint work with Misaki Matsumura and David Weinstein, we estimate the impact of e-commerce on Japanese prices and welfare. We find that goods sold intensively online have always had lower relative rates of price increase than goods sold mainly in physical stores, but the gap in inflation rates rose after the advent of e-commerce. This happened in part because goods sold offline began experiencing faster rates of price increase. Second, we compute the welfare gains generated by e-commerce by reducing intercity price differentials and by increasing available varieties. While we show the national gains were substantial, we also find that welfare rose much more for residents of high-income cities with highly educated populations and may have fallen for residents of other cities.

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To my beloved family,

Sungtae Jo, Sunmi Park, and Joonheum Jo

Chapter 1

Downward nominal wage rigidity in the United States

1.1 Introduction

Downward nominal wage rigidity is the resistance of nominal wages to adjusting downwards. While the existence of downward nominal wage rigidity has been studied in the literature,[1] it remains controversial whether downward nominal wage rigidity could have consequences for employment. Recent studies have theorized that downward nominal wage rigidity led to massive unemployment in peripheral Europe and in the United States during the Great Recession (Schmitt-Grohé and Uribe (2016); Schmitt-Grohé and Uribe (2017)). During periods of high inflation, real wages can fall even when nominal wages cannot adjust downwards. However, because inflation stayed low during the Great Recession, it is believed that downward nominal wage rigidity also prevented real wages from falling, resulting in greater unemployment. However, empirical evidence on the

[1] Kahn (1997); Card and Hyslop (1996); Lebow, Sacks, and Anne (2003); Daly, Hobijn, and Lucking (2012); Barattieri, Basu, and Gottschalk (2014); Daly and Hobijn (2014); Elsby, Shin, and Solon (2016); Fallick, Lettau, and Wascher (2016)

relationship between downward nominal wage rigidity, inflation, and employment is still lacking.

This chapter uses two nationally representative household surveys in the US, the Current Population Survey (CPS, 1979 - 2017) and the Survey of Income and Program Participation (SIPP, 1984 - 2013), to determine if the empirical patterns of wage change distributions of individual workers have impact on employment. While a number of other studies have investigated the relationship between the degree of nominal wage rigidity and employment, their findings are still contradictory, making the role of downward nominal wage rigidity during recessions a controversial topic.[2] To shed light on this discussion, I first examine the cyclical properties of the nominal wage change distribution in relation to employment and inflation. In the chapter 2, I show that the empirical patterns are not only consistent with theories of downward nominal wage rigidity, but also among five heterogeneous-agent models with alternative wage-setting schemes, only the model with downward nominal wage rigidity is able to match all the empirical patterns.

The CPS and the SIPP provide a number of advantages for the present analysis. First, the panel structure of both data sets allows one to measure individual year-over-year hourly wage growth rates, thus accounting for level differences in individual-specific wages. In addition, both data sets contain population weights, which allow for the aggregation of data to the national level. The two data sets are also complementary. The CPS, unlike the SIPP, is composed of rotating panels, allowing one to study a long time series containing multiple recessions. On the other hand, the SIPP contains an employer ID for each job of each respondent, allowing one to compare the wage change distributions of job stayers versus that of job switchers.

As the first step of the analysis, I examine the nominal wage change distribution for

[2] Daly and Hobijn (2014) argue that the downward nominal wage rigidity is more binding in the recession, however Elsby, Shin, and Solon (2016) argue that the downward nominal wage rigidity does not respond to the business cycle.

each year from 1979 to 2017 for the nation as a whole. Consistent with the findings of previous authors, I find that each year's distribution has a large spike at zero. That is, a large share of workers do not experience hourly wage changes from one year to another. Furthermore, these distributions are distinctively asymmetric; nominal wages changes are composed of many fewer wage cuts than raises. An analysis for each state in U.S. also confirms that the general shape of wage change distributions holds not only at the national level but also at the state level.

While it is apparent that nominal wages are more often moving upwards than downwards, this empirical fact alone is not compelling evidence of the existence of downward nominal wage rigidity, as it could be due to other factors such as labor productivity growth or inflation. Hence, I examine how the nominal wage change distribution changes over business cycles, and whether these changes are related to employment and inflation.

My analysis mainly focuses on three statistics from the nominal wage change distribution: the share of workers with no wage changes (which corresponds to the spike at zero), the share with cuts, and the share with raises. The theory of downward nominal wage rigidity suggests that downward nominal wage rigidity would have little effect on employment during periods of high inflation, but could adversely affect employment during periods of low inflation. Indeed, I find that the three statistics have statistically significant relationships with employment only when controlling for inflation. In particular, the size of the spike at zero has a negative correlation with employment when controlling for inflation. This is consistent with the prediction that in years when downward nominal wage rigidity is more binding, as indicated by the greater share of workers with no wage changes, employment decreases more. This finding is also consistent with that of Daly and Hobijn (2014), who focus on a period of relatively low inflation, namely the years 1986 - 2014, and find that the fraction of workers with no wage changes appears countercyclical.

Furthermore, I document a novel empirical finding, namely that the share of workers with no wage changes has greater countercyclical fluctuations compared to the share of workers with wage cuts. With downward nominal wage rigidity, because the movement of wages is restricted downwards, it is plausible that the share of workers wage cuts would vary little over time, while the share of workers with no wage changes would fluctuate more along the business cycle.

With the national level data, I first show that, unsurprisingly, both employment and the share of workers with raises decline during recessions: a one percentage point decline in employment is associated with a 0.9 percentage point decline in the share of workers with raises, controlling for inflation. Mechanically, this decline in the share of workers with raises corresponds to the sum of the increases in the share of workers with no wage changes and in the share with wage cuts. I then examine which of these two shares shows a larger co-movement with employment, controlling for inflation. I find that a one percentage point decline in employment is associated with a 0.6 percentage point increase in the share of workers with no wage changes and a 0.3 percentage point increase of workers with a wage cut. That is, as employment falls during recessions,the increase in the share of workers with no wage changes is double of the increase in the share of workers with wage cuts.

This pattern I identify at the national level across time also holds in the cross-sectional analysis of the data at the US state-level: controlling for state and time fixed effects, declines in state-level employment still show greater association with the increase in the share of workers with no wage changes compared to that of workers with wage cuts.

At first sight, this appears to contradict the recent finding by Beraja, Hurst, and Ospina (2016), which shows a positive correlation between state-level changes in nominal wages and employment during the Great Recession. Based on this finding, these authors argue wages were "fairly flexible", as lower employment growth was associated with lower wage growth. However, also using the state-level data for the same time period, I show that lower employment growth was also associated with larger increases in the share of workers with no wage changes. That is, in the states with low employment growth, the overall nominal wage growth may be lower due to declines in the share of workers with raises, but the distribution of wage changes contains a substantial increase in the size of the spike at zero. I therefore argue that Beraja, Hurst, and Ospina (2016)'s finding is still consistent with downward nominal wage rigidity. I conclude, contrary to Beraja, Hurst, and Ospina (2016), that nominal wages were "fairly rigid" during the Great Recession.

Empirical analysis suggests that the shape and cyclical properties of the nominal wage change distribution are consistent with downward nominal wage rigidity. The findings are established using both the CPS and the SIPP data, both at the national and state level. The main analysis includes both job stayers and job switchers, and while the patterns that suggest downward nominal wage rigidity are starker for job stayers (who comprise a large majority of the sample), the patterns hold for job switchers also. In summary, my empirical analysis presents three stylized facts about inflation, employment, and the nominal wage change distributions. Namely, controlling for inflation, the share of workers with zero wage changes increases as employment falls, the share of workers with wage cuts also increases as employment falls, and most importantly, the relative change in the former is nearly twice as large as that of the latter.

The remainder of the paper is organized as follows. Section 2.2 discusses the related literature. Section 1.3 describes the data sets: the CPS and the SIPP. Section 1.4 discusses the shape of nominal year-over-year hourly wage change distributions. Section 1.5 examines the cyclical properties of the nominal wage change distribution: as employment declines, the share of workers with no wage changes increases more than the share with wage cuts. The state-level analysis of this finding is presented in section 1.6. Section 2.5 concludes.

1.2 Related literature

This chapter is related to various branches of the empirical literature on nominal wage rigidity. Early studies use individual-level panel data for the period of high inflation, 1970-1993, and document a relationship between nominal wage change distribution and inflation rather than the former and employment. Kahn (1997) use data from the Panel Study of Income Dynamics (PSID) from 1970 to 1988 to show that nominal wage change distributions are asymmetric with a spike at zero. However, this author does not find a statistically significant relationship between the share of workers with no wage changes, the spike at zero, and employment. My conjecture is that this is because in her sample period, the average inflation was very high at 6.1 percent per year. Card and Hyslop (1996) use both PSID and CPS data from 1979 to 1993, a period during which the average inflation rate was about 5.3 percent per year. They argue that inflation can grease the wheels of the labor market by showing that the share of workers with no wage changes is significantly negatively correlated with inflation: fewer workers experience zero wage changes when inflation is high. Like Kahn (1997), these authors do not find a statistically significant relationship between the spike at zero and employment.

A recent paper by Daly and Hobijn (2014) studies the period of low inflation, 1986 - 2014, when the average inflation was 2.7 percent. These researchers find that the spike at zero is countercyclical: the share of workers with no wage changes increases when employment declines. The spike at zero from Daly and Hobijn (2014) is available from the Wage Rigidity Meter, published by the Federal Reserve Bank of San Francisco.[3] In contrast to Daly and Hobijn (2014), Elsby, Shin, and Solon (2016) argue that the spike at zero has been acyclical since 1998. Elsby, Shin, and Solon (2016) use the CPS data with

[3]The Wage Rigidity Meter shows the percentage of workers with no wage change within the subgroups of the labor force by type of pay, education, and industry using the CPS, which is available from here.

Atlanta Fed's Wage Growth Tracker (here) also reports the percent of individuals with zero wage changes.

biannual job-tenure supplements from 1980 to 2017. They show that the spike at zero has increased since 1998. They argue that the increase in the spike at zero is secular rather than cyclical in nature and is the consequence of a secular decline in inflation.

Contrary to Elsby et al. (2016), I find that the spike at zero is countercyclical using the CPS data with the longest time period, 1979-2012, controlling for inflation. Furthermore, I investigate not only the cyclicality of the spike at zero but also the cyclicality of the fraction of workers with wage cuts, which gives us a better understanding of the cyclicality of nominal wage change distribution.

In the studies mentioned above, wage change is defined to equal zero only when data show an exact zero, that is, when a worker reports the exact same hourly wage rate in the interviews one year apart. Reported wages suffer from measurement error, which can over- or understate the size of the spike at zero wage changes. Barattieri, Basu, and Gottschalk (2014) use the SIPP panel data for the period from 1996 to 2000 to estimate the constant frequency of no wage changes taking into account measurement error. They argue that correcting for measurement error leads to a larger estimate of the size of the spike at zero and a decline in the estimate of the share of workers experiencing a wage cut.

Furthermore, Fallick, Lettau, and Wascher (2016) use data from the Employment Cost Index for the period from 1982 to 2014. This BLS survey includes information on the annual costs for specific job descriptions and the annual hours that workers are supposed to work (contracted hours) to obtain their annual compensation. One advantage of employer-reported wage data is that they are free of measurement errors as they are recorded systematically. A disadvantage of this data is that it does not allow controlling for individual fixed effects since the base unit of observation is a job rather than an individual. They find mixed results on the extent of downward nominal wage rigidity during the Great Recession, and conclude that they cannot reject the hypothesis that the labor market distress during the Great Recession lowered nominal wage rigidity.

Unlike the previous studies mentioned thus far, Beraja, Hurst, and Ospina (2016) use state variations of wages and employment to argue that wages were fairly flexible during the Great Recession. They use nominal wage data from the 2007-2010 American Community Survey (ACS), which does not have a panel structure. To avoid composition bias, they use the residual wages, taking out variations in wages depending on observable worker characteristics. They argue that wages were "fairly flexible", since they find a positive correlation between state-level changes in nominal wages and employment during the Great Recession. However, as described in detail in section 1.6.3, I argue that their finding still can be consistent with the existence of downward nominal wage rigidity since I find a negative association between the share of workers with zero wage changes and employment at the state level.

Kurmann and McEntarfer (2017) uses data of Washington state from Longitudinal Employer-Household Dynamics and they argue that the increased incidence of wage cuts during the downturn suggest that downward nominal wage rigidity may not be a binding constraint. However, this chapter shows there are larger increases in the spike at zero compared to the share of workers with wage cuts during downturns.

A recent paper by Grigsby, Hurst, and Yildirmaz (2019) find the share of workers with wage cuts is 8.7 percent per year for 2008-2016 for both hourly and salaried workers, combining both job stayers and job switchers. Considering their sample period is during the Great Recession, they show downward adjustment of nominal wages are rare. They use payroll data from ADP, free of measurement error while it is a potential concern using household survey data. However, their sample consists of firms with more than 50 employees, leading to sample selection bias, whereas household survey publishes population weight to make the sample nationally representative. Rather than focusing on the Great Recession, this chapter uses a longer sample period from 1979 to 2016, including multiple business cycles.

1.3 Data

This chapter uses two nationally representative household panel data sets, the CPS and the SIPP, in the United States, which have individual-level wage data. It is important to use disaggregated data to avoid the composition bias embedded in aggregate time series of wages. Solon, Barsky, and Parker (1994) show that the composition of employed workers changes over the business cycle, which gives more weight to low-skilled workers during booms compared to recessions. Because the wages of low-skilled workers tend to be lower than those of high-skilled workers, such cyclical changes in the composition of the workers can lead to aggregate wages appearing not to fall during recessions, spuriously suggesting wage rigidity. To avoid this composition bias, the present paper uses panel data.

1.3.1 Current Population Survey

The Current Population Survey (CPS)[4] is jointly collected by the United States Census Bureau and the Bureau of Labor Statistics (BLS). The purpose of this survey is mainly to construct nationally representative labor force related statistics, such as unemployment rates and median weekly earnings in the United States. Almost 60,000 households are interviewed monthly. The sample period starts in 1979 and ends in 2017.

The CPS has a special sampling design. Each household in the sample is asked about their labor force status 8 times but not in a continuous way. After the first four months of the interview, households are out of the sample for 8 months and are interviewed 4 times again in the following 4 months. Table 1.1 shows the sampling design of the CPS. Among the 8 interviews, only when households are in the Outgoing Rotation Group (Earner Study) - the fourth and eighth interview of the survey - do they respond to earnings-related questions: usual earnings, hours worked last week, union coverage, and so on. Thus, each individual

[4]CPS monthly microdata are available from http://www.nber.org/data/cps_basic.html .

in the survey reports wages at most two times in a year apart, in the month in sample (MIS) in 4 and 8.

Table 1.1: CPS sampling design

Calendar Month	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4
Month in Sample (MIS)	1	2	3	4		——— Break ———							5	6	7	8
Labor force status	✓	✓	✓	✓									✓	✓	✓	✓
Outgoing Roation group				✓												✓

Notes. This table is from Daly, Hobijn, and Wiles (2011)

Knowing the special sampling design of the CPS, the monthly CPS could be exploited as panel data. However, CPS microdata do not provide unique individual identifiers within the households. Instead, Integrated Public Use Microdata Series - CPS (IPUMS-CPS)[5] provides the unique individual identifiers to link individuals across monthly CPS based on Drew, Flood, and Warren (2014).[6] To take advantage of the longitudinal features of the CPS data, this chapter uses the unique individual identifiers from IPUMS-CPS.

The main focus of this chapter is hourly workers who directly report hourly pay rates both in the previous year and the current year.[7] For nonhourly workers, hourly wages can be obtained by dividing the usual weekly earnings by the usual hours worked per week. However, the imputed hourly rates for salaried workers in this manner can be excessively volatile, as it is sensitive to any reporting errors on the number of hours worked, which is known as the division bias. To remove errors caused by imputing the hourly pay rates, the main results are shown only for hourly-rated workers. In the United States, about 58

[5]IPUMS-CPS data are available from .https://cps.ipums.org/cps/.

[6]Based on a method suggested by Madrian and Lefgren (1999) for matching the monthly CPS by exploiting differential basic demographic features within the households such as age, gender, race, and education level.

[7]When respondents are in the Outgoing Rotation Group (MIS4 or MIS8), they report their earnings in the easiest way: hourly, weekly, annually, or some other basis. Those who reported that the easiest way to report their wage is hourly are considered hourly workers. While some workers report that the easiest way to report their earnings is not hourly, they could have been rated as hourly. Therefore, for those who indicated that the easiest way to report their wages is some way other than hourly, they are asked again whether they are paid on hourly basis ,and if so, their hourly pay rate.

percent of workers are hourly-rated in 2014.[8] Workers paid hourly both in the previous and the current year represent about 50 percent of all workers.

Wages, the most important variable in this chapter, are often imputed in the CPS for missing values. On average, 34 percent of the hourly wages of hourly rated workers have been imputed since 1996.[9] Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) show that including imputed wages in the analysis may cause bias due to imperfect matching of donors with nonrespondents. Therefore, it is essential to exclude imputed wages. Although IPUMS-CPS provides individually linked CPS data, the IPUMS-CPS does not provide allocation flags for wage variables, that indicate whether wage variables are imputed or not. Therefore, I merge the IPUMS-CPS data with the monthly CPS, merged with the Outgoing Rotation Group. In this way, this chapter exploits the longitudinal feature of the CPS after excluding imputed wages.

This chapter focuses on base wages for hourly workers, which excludes other types of benefits from earnings - paid leave, overtime payment, nonproduction bonuses, insurance, retirement savings, and so on. In December 2018, BLS report on Employer Costs for Employee Compensation[10] says on 1.8 percent of total compensation can be contributed to nonproduction bonuses on average, while it is 1.4 percent of total compensation.[11] This suggests nonproduction bonuses are small and not cyclical. Also, Grigsby, Hurst, and Yildirmaz (2019) use payroll data from ADP and show median hourly workers earn 2.2 percent of annual gross earnings other than base wages from 2009 to 2016. This suggests base wages are the main source of earnings for hourly workers. In addition, they find the size and the frequency of bonus are acyclical.

One disadvantage of the CPS is that it is difficult to define job stayers and job switchers.

[8] https://www.bls.gov/opub/reports/minimum-wage/archive/characteristics-of-minimum-wage-workers-2014.pdf.

[9] Table A1 in the appendix shows the imputation ratio for usual weekly earning and hourly wage.

[10] https://www.bls.gov/news.release/archives/ecec_03192019.pdf

[11] https://www.bls.gov/news.release/archives/ecec_03142012.pdf

Although the CPS provides the variable to inform whether the respondent is employed by the same employer from the last month since 1994, this variable is missing in the MIS5 after 8 months break of the interview. Thus, it is difficult to define job stayers in the CPS. For example, if the respondent has switched jobs during the 8-month break period, for example in the calendar month 5, and stayed at the same job since then, he/she would respond as being employed by the same employer for MIS6-8. This respondent is likely to be identified as a job stayer from MIS4 to MIS8, although he/she is a job switcher. Therefore, this chapter does not distinguish job stayers from job switchers for the empirical analysis using the CPS.

This chapter considers only workers above the age of 16. Self-employed workers and workers whose earnings are top-coded or imputed are also dropped. The average number of observations is 15,418 per year. The time series number of observations is available in the appendix Table A2.

1.3.2 Survey of Income and Program Participation

The SIPP[12] is a U.S. household survey conducted by the U.S. Census Bureau. Each panel consists of approximately 14,000 to 52,000 households, and the interview is conducted every 4 months over 3 or 4 years. Longitudinal weights provided by the SIPP are used to aggregate data at the national level. This chapter uses thirteen panels: 1984, 1985, 1986, 1987, 1988, 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008. The sample period is from 1984 to 2012.

The main objective is the annual hourly wage growth rate for each hourly rated worker. Although wages for each worker are available from the SIPP at a monthly frequency[13],

[12]Data can be downloaded from http://www.nber.org/data/survey-of-income-and-program-participation-sipp-data.html .

[13]Each individual is required to provide monthly wages for the prior 4 months at the time of the interview; therefore, monthly wages are available. However, due to seam bias, this chapter uses wages only from the

this chapter studies the annual hourly wage growth rate since the hazard of a nominal wage change is highest at 12 months after a wage change (Barattieri, Basu, and Gottschalk (2014)). Similar to the CPS, this chapter focuses on hourly rated workers who report the hourly rate directly to the survey in order to eliminate errors from the imputation of the hourly pay rate for salaried workers.[14]

There are advantages of using the SIPP. First, the SIPP provides the unique individual identifiers so we can match individuals across waves without an additional process. Second, the SIPP keeps track of movers, while the address-based CPS does not follow movers in the sample. Third, the SIPP provides the unique and consistent job IDs across waves for each job that the respondent had, whereas the CPS does not offer them. Since job IDs are allocated based on a respondent's employer information in the SIPP, I define job stayers as employer stayers.[15] Job switchers are the ones who reported to work for the different employers in any given year, regardless of jobless spell between employer switching. One disadvantage of SIPP data is that the time series data are discontinuous because of gaps between the panels. Thus, state-level analysis is more reliable than the aggregate time series analysis in the SIPP.

The average number of observations in the SIPP is 13,937 per year, which is smaller but comparable to the CPS sample size.[16] In the SIPP, 55 percent of workers are hourly rated.

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reference month.

[14]The SIPP uses a specific questionnaire to ask whether survey respondents are paid by the hour for the main jobs. For workers who are paid by the hour, the SIPP questions for the regular hourly pay rate at that job from the specific employer. SIPP has introduced the dependent interviewing procedure to improve data quality since 2004 (Moore (2006)). That is, if respondents indicated the hourly wage is "the same as the last interview", the hourly wage at the current interview is filled by the one from the last interview.

[15]After the major revision of survey design in 1996, if the respondent was not employed for the entire 4 months for the reference period of the interview, then job ID will be renewed at the next interview. Thus, even if this respondent works for the same employer after the jobless spell, the job ID can be different. This issue is raised by Fujita and Moscarini (2017) and I corrected this problem using the method followed by Fujita and Moscarini (2017). For the panel 1990 - 1993, I used the revised job IDs.

[16]The original sample size of the CPS is much larger than that of the SIP; however, the CPS collects only 2 wage data for individuals for the whole interview. Therefore, the sample size of the SIPP is comparable to that of the CPS.

On average, 71 percent of them are job stayers. The time series number of observations is available from Table A13, and the number of job stayers and job switchers are available from Table A14 in the appendix.

1.4 Asymmetric nominal wage change distribution

This section examines year-over-year nominal hourly wage change distribution for each year from 1979 to 2017 using the CPS (section 1.4.1) and from 1984 to 2013 using the SIPP (section 1.4.2). Nominal wage change distributions show a large spike at zero, that is, a large share of workers experience exact zero wage changes in a given year. In addition, these distributions are highly asymmetric: there are fewer wage cuts than raises. This is consistent with the findings in a strand of earlier literature that argues for the existence of downward nominal wage rigidity; Kahn (1997); Card and Hyslop (1996); Lebow, Sacks, and Anne (2003); Barattieri, Basu, and Gottschalk (2014); Elsby, Shin, and Solon (2016); Fallick, Lettau, and Wascher (2016).

1.4.1 Nominal wage change distribution: CPS

I plot the distribution of log nominal hourly wage changes of hourly rated workers for each year from 1979 to 2017 using the CPS data. The following characteristics appear common to all nominal wage change distributions: 1) there is a large spike at zero, and 2) there are fewer wage cuts than raises. As an example, Figure 1.1 shows the distribution for the year, 2009-2010. We can clearly observe an apparent spike at zero, which is shown in red, defined as the percentage of hourly rated workers whose annual hourly wage growth rate is exactly zero. In other words, the spike at zero represents the share of hourly workers who report the exact same hourly wages in interviews one year apart. The width of all the blue bins is 0.02, except for the two bins at the very ends. From the smaller sizes of the blue bins to the left of zero, it is clear that the distribution contains fewer wage cuts than raises.

I provide some context for Figure 1.1. In 2010, the unemployment rate was highest at 9.7 percent after the onset of the Great Recession, and the inflation rate was 1.6 percent. Even with massive excess labor supply in the economy, 21.1 percent of the hourly rated workers experienced zero wage changes from 2009 to 2010, represented as the large spike at zero. The median hourly wage growth rate was 1.7 percent, and more than half of the hourly rated workers had raises higher than the inflation rate. Overall, 54.2 percent of hourly rated workers had raises, and only 24.6 percent of the hourly rated workers had wage cuts; that is, there were many more raises than wage cuts in 2010 despite high unemployment and low inflation. Many researchers have interpreted the asymmetry and the spike of zero in the wage change distribution as suggestive of downward nominal wage rigidity. Notably, focusing on the two bins right next to the spike at zero, one observes a discontinuous drop in density approaching from the left compared to approaching from the right. Kahn (1997) interpreted the spike at zero as a "pile-up" of workers, who without downward nominal wage rigidity, would have had negative nominal wage changes. Similarly, Card and Hyslop (1996) stated that the spike at zero is mostly from "swept-up" workers, who would have been part of the bins to the left of zero if not for downward nominal wage rigidity. Hence the drop in density to the left of zero has been also interpreted as being consistent with the existence of downward nominal wage rigidity.

Figure A1 and A2 in the appendix show similar distributions for each year from 1979 to 2017. Similarly to the figure for 2010, all nominal wage change distributions have large spikes at zero and more raises than cuts for the entire sample period. This suggests that nominal wage change distributions are consistent with existence of downward nominal wage rigidity for the entire sample period, 1979 - 2017.

To further exploit cyclical properties of nominal wage change distributions, I focus on

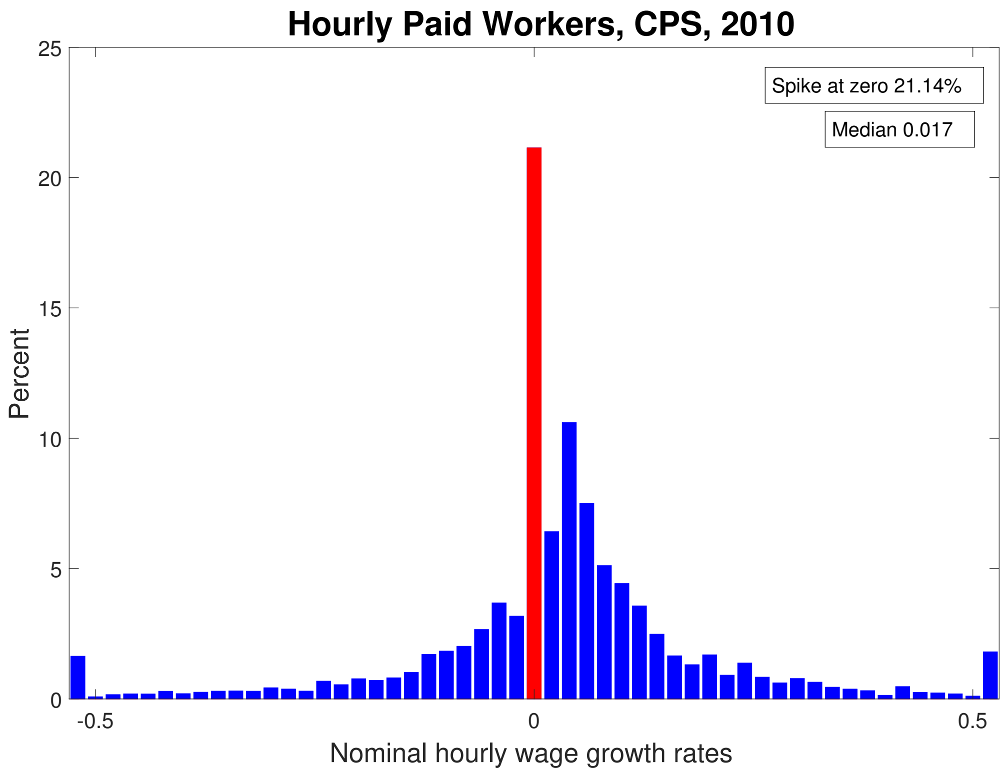


Figure 1.1: Year-over-year nominal hourly wage growth rates in 2010

Data source: CPS and author's calculation. The bin size is 0.02. The red bin shows the spike at zero, which represents the percentage of workers whose year-over-year nominal hourly wage growth rate is exactly zero from 2009 to 2010. The bin to the right of the zero represents the share of workers whose log nominal hourly wage differences are strictly greater than zero and lower than 0.02, and so on. The bin to the very right includes all the workers whose log nominal hourly wage differences are greater than 0.5, and the bin to the very left includes all the workers whose hourly wage growth rates are less than -0.5. The size of the spike at zero in 2010 is 21 percent and the median nominal hourly wage growth rate in 2010 is 1.7 percent. 24.6 percent of hourly workers had wage cuts and 54.2 percent of workers had raises.

three statistics from the distributions: the spike at zero (the share of workers with no wage changes), the share with wage cuts, and the share with raises. Table 1.2 reports the averages of these three statistics across the sample years. On average, 15 percent of hourly workers had exact zero hourly wage changes, 21 percent of them had wage cuts, and 64 percent had raises. Excluding minimum wage workers[17] only has a marginal effect on these average estimates.

[17] Workers whose hourly wages are lower than the state's minimum wage in either previous or current year are dropped. Vaghul and Zipperer (2016) document the monthly state-level minimum wage from 1973 to 2016. To extend the data set to 2017, I use https://www.dol.gov/whd/state/stateMinWageHis.htm.

Table 1.2: Descriptive statistics by worker charcteristic, CPS

	% of all workers	% of hourly workers	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
Hourly paid workers			15.25	21.13	63.63
Exc. Minimum wage workers			15.10	20.64	64.26
Male	52.17	49.25	15.17	22.15	62.69
Female	47.83	50.75	15.32	20.09	64.59
16 <= age <40	47.39	53.13	13.95	20.83	65.22
40 <= age <64	49.01	42.98	15.94	21.68	62.38
White	84.48	85.13	15.36	20.57	64.07
Non-white	15.52	14.87	14.62	24.39	60.99
High School or less	44.24	58.50	15.75	21.49	62.76
College or more	55.76	41.50	14.46	20.65	64.88
No union coverage	81.72	80.31	16.84	21.42	61.74
Union coverage	18.28	19.69	11.73	22.19	66.07

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). This table shows the sample average of spike at zero and the fraction of workers with wage cuts and raises over time by worker characteristics.

Nominal hourly wage change distributions do not show significant heterogeneity by worker characteristics. Table 1.2 reports descriptive statistics by worker characteristics. As I only focus on hourly workers, there is some sample selection: female workers, young workers, and less educated workers are overrepresented. However, calculating the averages of the three statistics for different subsets of workers results in similar estimates.

Table 1.3: Nominal hourly wage change distribution, CPS, by hourly wage quartiles

Hourly wage Quartiles	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
25th below	20.85	31.70	47.45
25th to Med	15.48	20.77	63.75
Med to 75th	13.29	18.09	68.62
75th and above	12.83	16.65	70.52

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). This table shows the sample average of the spike at zero and the fraction of workers with wage cuts and raises over time by hourly wage quartiles.

On the contrary, nominal hourly wage change distributions exhibits heterogeneity by hourly wage level and industry. Table 1.3 reports the averages for the same three statistics for the subsets of workers at different hourly wage quartiles. Workers in a lower hourly wage quartile tend to show a larger spike at zero and a larger share with wage cuts, compared to those in a higher hourly wage quartile. Table A3 in the appendix reports the averages calculated separately for the workers in each 2-digit NAICS industry code. The rows are sorted by the average size the spike at zero. The average size of the spike at zero varies from 11 percent to 23 percent. The biggest industry in terms of the number of hourly workers is manufacturing, and the average size of the spike at zero for manufacturing is around 14 percent, which is comparable to the national average.

1.4.2 Nominal wage change distribution: SIPP

Conducting the above analysis with the SIPP data from 1984 to 2013 results in very similar findings. Figure A4 in the appendix shows nominal hourly wage change distributions for hourly workers for each year in the sample period.[18] All the distributions are asymmetric with a large spike at zero.

Table 1.4 is similar to Table 1.2, reporting sample averages for the fractions of workers with zero wage changes, wage cuts, and raises. Again, these estimates do not show heterogeneity by worker characteristics such as gender and education - common to both the CPS and the SIPP.

In particular, the SIPP data allows me to compare nominal wage change distributions between job stayers and job switchers. I find that the empirical patterns suggestive of downward nominal wage rigidity - asymmetry and the spike at zero - are more pronounced for job stayers, but also hold for job switchers. Figure 1.2 displays nominal hourly wage change distributions in 2010 for job stayers (left) and job switchers (right). Both

[18]Note that the years 1990, 1996, 2001, 2004, and 2008 are missing from the sample due to the SIPP having gaps between panels

distributions display large spikes at zero, although the spike for job stayers is much larger than the other. [19]

Table 1.4: Descriptive statistics by worker characteristics, SIPP

	% fo hourly workers	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
Hourly paid workers		24.00	17.42	58.58
Exc. Minimum wage workers		23.99	16.68	59.33
Job stayers	71.08	28.89	12.32	58.79
Job switchers	28.92	12.52	29.86	57.62
Male	49.31	24.45	18.25	57.30
Female	50.69	23.58	16.59	59.83
White	83.27	23.92	17.00	59.08
Non-white	16.73	24.31	19.62	56.07
High School or less	54.92	25.19	17.51	57.30
College or more	45.08	22.54	17.30	60.15
No union coverage	89.55	25.02	14.75	60.24
Union coverage	10.45	24.39	16.14	59.47

Data source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008). This table shows the sample average of the spike at zero and the fraction of workers with wage cuts and raises over time by worker characteristics.

Similarly, Table 1.4 shows that for job stayers, the average size of the spike is larger, whereas the average share of workers with wage cuts is smaller.[20] The median size of wage growth rates for job switchers is also much larger than that for job stayers, as shown in Table 1.5.[21] These comparisons between job stayers and switchers appear overall consistent with the findings by Bils (1985) and Shin (1994), who argue that wages are more flexible for job

[19] Table A15 in the appendix shows the average of the spike at zero and the share of wage cuts and raises by reasons why hourly workers switched their employer in a given year. Contingent workers or temporary employed workers, workers on layoff, and injured or ill workers show the high average spike at zero among job switchers.

[20] In fact, the spike at zero for job stayers is always higher than that for job switchers and the share of workers with wage cuts for job stayers is always lower than that for job switchers. Table A14 shows time series spike at zero, the share of wage cuts and increases for both job stayers and job switchers.

[21] Nominal hourly wage change distributions for job stayers and job switchers for the entire sample period is available in Figure A5 and Figure A6. In addition, Table A12 shows that for both job stayers and job switchers, workers from a lower hourly wage quartile are more likely to have no wage changes or wage cuts than workers from a higher wage quartile.

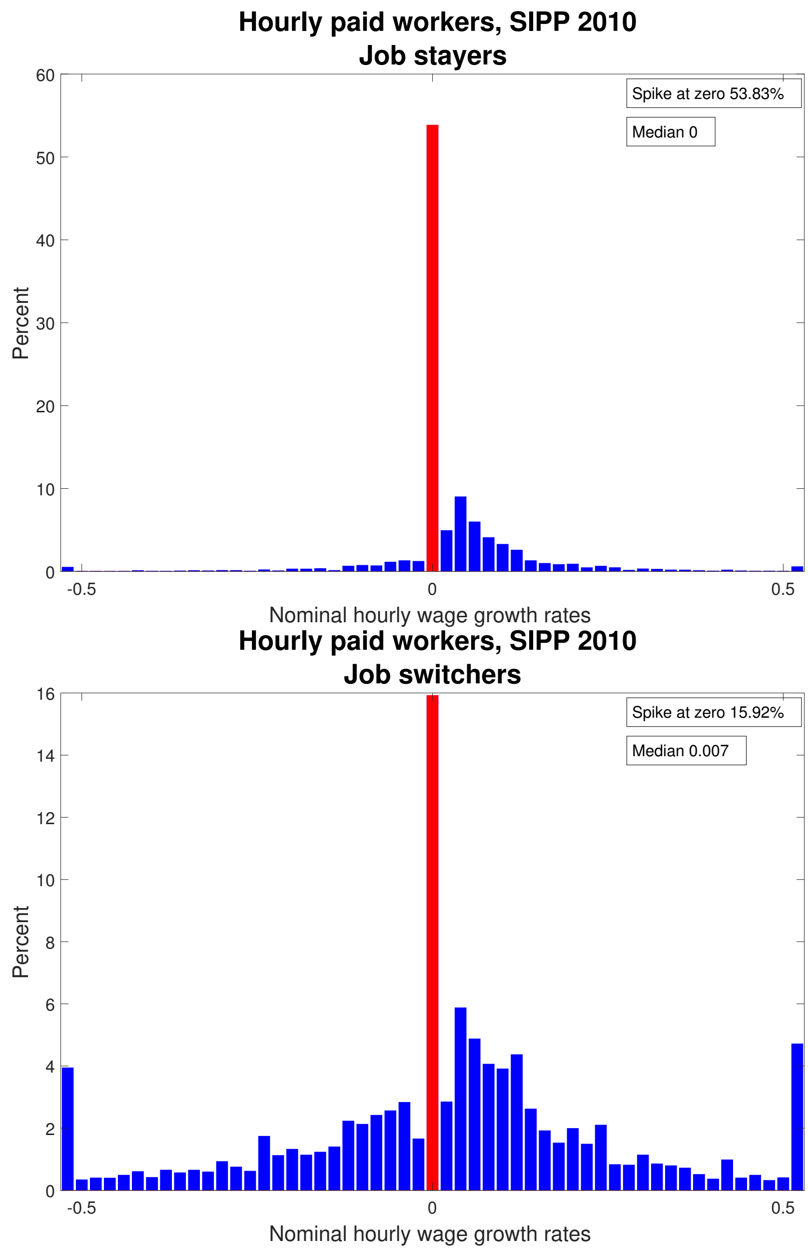


Figure 1.2: Nominal hourly wage distribution in 2010: job stayers vs. job switchers

Data source: SIPP and author's calculation. The figure shows nominal hourly wage change distribution for job stayers (left) and that for job switchers (right). The red bin shows the spike at zero, which represents the percentage of workers whose hourly wage growth rate is precisely zero from 2009 to 2010. Other than the red bin, bin size is 0.02. The spike at zero for job stayers is 54 percent and the spike at zero for job switchers is 16 percent.

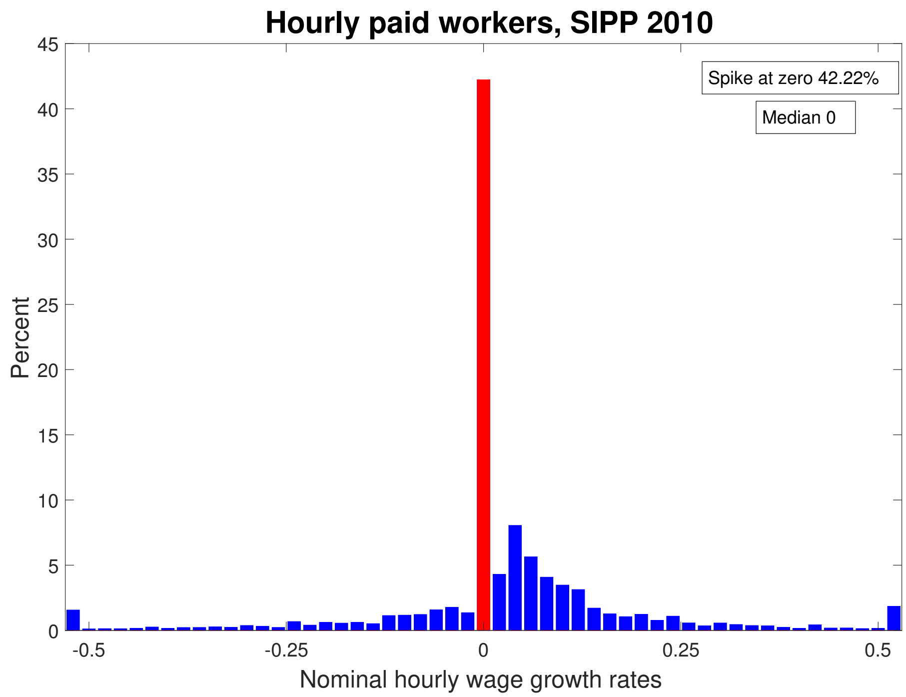


Figure 1.3: Nominal hourly wage growth rate distribution in 2010

Data source: SIPP and author's calculation. The red bin shows the spike at zero, which represents the percentage of workers whose hourly wage growth rate is exactly zero from 2009 to 2010. Other than red bin, bin size is 0.02. The spike at zero in 2010 is 42.2 percent and the median nominal hourly wage growth rate in 2010 is 0 percent. 16 percents of hourly workers had wage cuts and 41 percent of workers had raises.

switchers than job stayers. However, my findings suggest job switchers' wages may still be downwardly rigid, albeit to a lesser extent.

Because about 71 percent of hourly workers are job stayers in the SIPP, and because nominal hourly wage change distributions for job switchers still exhibit asymmetry and the spike at zero - although to a lesser extent - the distributions using all workers such as Figure 1.3 exhibit strong asymmetry and a large spike at zero. This is also comparable to Figure 1.1, nominal hourly wage change distributions in 2010 using the CPS, which also includes both job stayers and job switchers, with the former being a large share.

Table 1.5: Median size of wage change, SIPP

	Median size of ΔW given $\Delta W < 0$	Median size of ΔW given $\Delta W > 0$
Job stayers	-7.07	6.76
Job switchers	-16.29	16.20

Source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008).

1.5 The cyclicality of the aggregate nominal wage change distributions

This section contains the main empirical results of the paper, namely that the spike at zero shows greater countercyclical fluctuations compared to the share of workers with wage cuts. Section 1.5.1 documents this pattern in the CPS data for the period 1979 to 2017 and section 1.5.2 in the SIPP data for the period 1984 to 2013. I focus on the three aggregate time series: the share of workers with zero wage changes (the spike at zero), the fraction of workers with wage cuts, and the fraction of workers with raises, constructed in section 1.4 above. Table A2 of appendix A.1 reports these time series along with the number of observations of individual hourly workers that went into constructing these summary statistics of the nominal wage change distributions for a given year.

1.5.1 Aggregate analysis: CPS

To explore the cyclicality of the nominal wage change distributions, we could think about the following three regression equations:

$$\begin{aligned}
\text{[Spike at zero]}_t &= \alpha_s + \beta_s(1-e_t) + \epsilon_{st} \\
\text{[Fraction of wage cuts]}_t &= \alpha_n + \beta_n(1-e_t) + \epsilon_{nt} \ , \\
\text{[Fraction of raises]}_t &= \alpha_p + \beta_p(1-e_t) + \epsilon_{pt}
\end{aligned} \tag{1.1}$$

where e_t denotes the employment to population ratio in year t. Adding the above three equations will give us

$$1 = \alpha_s + \alpha_n + \alpha_p + (\beta_s + \beta_n + \beta_p)(1 - e_t) + \epsilon_{st} + \epsilon_{nt} + \epsilon_{pt},$$

as the sum of the three shares equals 1 by definition. Since the left-hand side of this equation is a constant, we know that

$$\beta_s + \beta_n + \beta_p = 0.$$

Thus, β_p – the change in the share of workers with raises associated with the change in $1 - e_t$ can be decomposed into two parts: either β_s – the change in the spike at zero – or β_n – the change in the share of workers with wage cuts.

This framework allows us to study the changes in nominal wage change distributions more comprehensively, unlike most of the earlier studies that only focused on the cyclicality of the spike at zero.

Table 1.6: The spike at zero, the fraction of wage cuts, and raises along the business cycles

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero $\Delta W = 0$	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop ratio	0.433	0.200	-0.632	0.616***	0.305*	-0.921***
$(1 - e_t)$	(0.299)	(0.221)	(0.498)	(0.161)	(0.156)	(0.281)
Inflation rate, π_t				-1.181***	-0.674***	1.855***
				(0.122)	(0.145)	(0.218)
				0.616/0.920 = 0.67		
Observations	37	37	37	37	37	37
Adjusted R^2	0.0419	-0.00492	0.0313	0.727	0.331	0.703

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). Inflation rate is calculated from CPI-U. This table shows regression results from regressing the spike at zero, the fraction of workers with wage cuts, and raises on 1-epop ratio without and with controlling for inflation. Controlling for inflation, the spike at zero exhibits greater fluctuations compared to the share of workers with wage cuts.

Table 1.6 shows regression results based on the regression equation (1.1) without and with controlling for inflation. During periods of high inflation, nominal wage rigidity would have a limited impact on real wage rigidity and thus on employment. On the other hand, during periods of low inflation, nominal wage rigidity could potentially have a substantial effect on employment. During my sample period, 1979 - 2017, inflation varies from negative rates (e.g., -0.4 percent in 2009) to high rates (e.g., 12.7 percent in 1980). Hence not controlling for inflation could understate the relationship between employment and nominal wage changes. Indeed, in the first three columns of Table 1.6 where I do not control for inflation, I do not find a statistically significant relationships between the dependent variables and employment.

By contrast, when I control for inflation, I find a statistically significant relationships between the dependent variables and employment. In particular, column (4) shows that the spike at zero increases when employment declines. The negative correlation between the spike at zero and employment, controlling for inflation, is consistent with the findings by Kahn (1997); Card and Hyslop (1996) and Daly and Hobijn (2014). [22] The countercyclicality of the spike at zero can also be seen from the figure 1.4, which plots the spike at zero against $1 - e_t$. We observe that the spike at zero has a countercyclical movement in the period of low inflation. Furthermore, the spike at zero shows greater countercyclical fluctuations compared to the share of workers with wage cuts. I find that a 1 percentage point decline in employment is associated with 1) a 0.6 percentage point increase in the spike at zero; 2) a 0.3 percentage point increase in the share with wage cuts; and 3) a 0.9 percentage point decrease in the share with raises. In other words, when there is a 1 percentage point decrease in employment, the share of workers with raises declines by 0.9 percentage points, and mechanically, the share of workers with wage cuts or no wage

[22] Card and Hyslop (1996) use the sample period of high inflation from 1979 to 1993 and conclude that the spike at zero is negatively correlated with inflation, leading them to conclude that inflation can grease the wheels of the labor market. Daly and Hobijn (2014) use the sample period of low inflation from 1986 to 2014 and argue that the spike at zero is positively related to the unemployment rate. Different from the previous literature, this chapter explores the cyclicality of the spike at zero as well as the share of workers with wage cuts and raises.

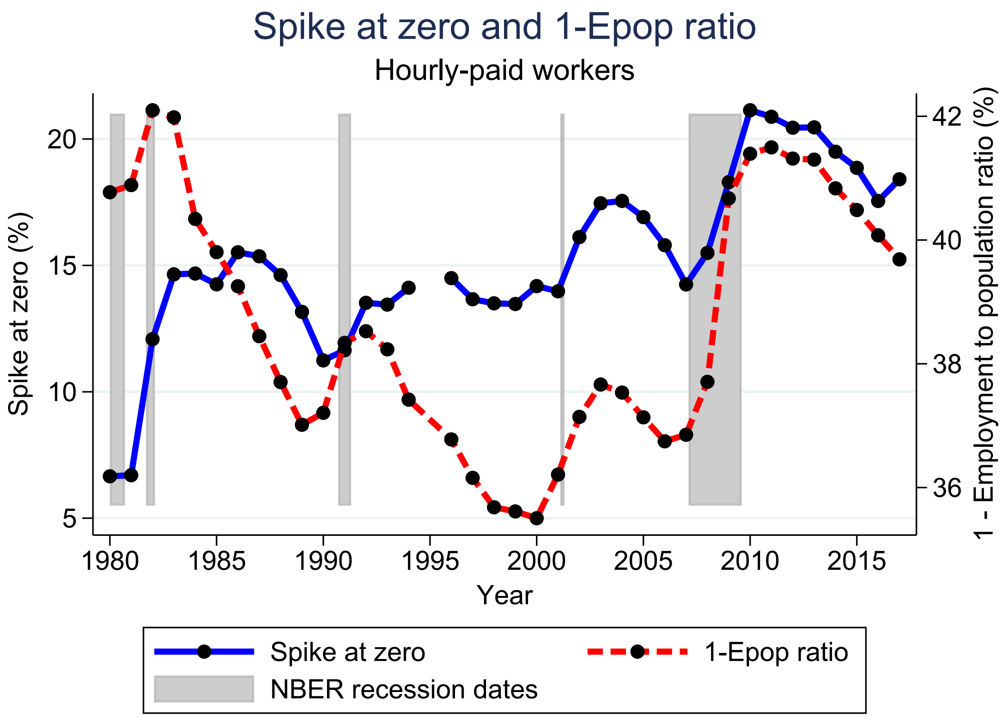


Figure 1.4: Time series of the spike at zero with 1-Epop ratio

Data source: CPS and author's calculation. Sample period: 1979 - 2017. This figure shows the spike at zero for each year (left axis) and the 1- employment to population ratio (right axis).

changes would increase by 0.9 percentage points. In fact, 67 percent (= 0.6/0.9) of such increase is attributable to the share of workers with no wage changes. That is, the increase in the spike at zero is much greater than the increase in the share that have wage cuts. [23]

This pattern seems plausible given downward nominal wage rigidity. During recessions with low inflation, the workers who may have experienced wage cuts if not for downward nominal wage rigidity, instead would experience zero wage changes, since nominal (and real) wages are restricted from adjusting downwards. This could lead to a larger change in the share of workers with no wage changes associated with a decline in employment. When employment increases and more workers experience wage increases, because a large

[23] Section A.1.2 from the appendix shows that there are no asymmetric responses of nominal hourly wage change distributions to employment increases compared to decreases.

number of workers are "piled up" at zero, the decrease in the spike at zero could be larger than the decrease in the share of workers with wage cuts. In conclusion, I find that the spike at zero exhibits greater countercyclicality compared to the share of workers with wage cuts, and interpret this to be consistent with the implication of downward nominal wage rigidity.

Regarding the regressions above, one may be concerned about error of self-reported hourly wages (Bound and Krueger (1991)); however, measurement error on the dependent variables, orthogonal to independent variables, would not bias the coefficient estimates. For hourly wages, we can expect largely two types of measurement errors. First, when respondents report their hourly wages, they may report their true wages with some error. This type of measurement error would understate the wage rigidity, the spike at zero. Second, workers may report rounded hourly wages, and this would overstate the spike at zero. However, these measurement errors do not vary with employment. In addition, the fraction of imputed wages, which is available from the last column of Table A1, can be a proxy for the degree of measurement error, and it does not exhibit cyclicality. As measurement errors do not have a cyclical component, we can argue that measurement errors on hourly wages do not add bias on the cyclicality of the spike at zero, the share of workers with raises, and cuts.

In addition, my primary findings are robust to using the nominal hourly wage change distributions of salaried workers, instead of hourly wage workers. For salaried workers, we can compute hourly wages by dividing the usual weekly earnings by the usual weekly hours worked.[24] Table 1.7 shows regression results using imputed hourly wages for salaried workers. We can still see that the spike at zero is negatively associated with inflation and employment, jointly. The spike at zero shows greater association with employment than

[24]This imputed hourly wage can be more volatile than the actual hourly wage due to measurement error in hours worked for salaried workers. The average of the spike at zero for salaried workers is 7.0 percent, the average of the share of workers with wage cut for salaried workers is 34.3 percent, and the average of the share of workers with wage increases for salaried workers is 58.8 percent.

Table 1.7: The spike at zero, the share of wage cuts, and raises for salaried workers along business cycles

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero $\Delta W = 0$	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop	0.429***	-0.0646	-0.364	0.471***	0.0535	-0.524**
$(1 - e_t)$	(0.0805)	(0.240)	(0.308)	(0.0539)	(0.165)	(0.196)
Inflation rate, π_t				-0.278***	-0.782***	1.060***
				(0.0322)	(0.122)	(0.132)
				0.472/0.524 = 0.9		
Observations	36	36	36	36	36	36
Adjusted R^2	0.416	-0.0269	0.0224	0.656	0.430	0.601

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1994, 1995). Inflation rate is calculated from CPI-U. Hourly rate is calculated from usual weekly earning/usual hours worked per week. Controlling for inflation, the spike at zero exhibits countercylical fluctuations in employment while the share of workers with wage cuts does not respond to employment.

the share of workers with wage cuts, and in fact, the share of salaried workers with wage cuts is not significantly associated with employment.

The primary results are also robust to looking at subgroups of workers by worker characteristics such as gender, age, race, and education. These robustness checks are available in section A.1.2 of the appendix. For example, low-paid young workers, who are less likely to be in a long-term contract, also show the main empirical findings on the cyclicality of nominal wage change distribution. I define low-paid young workers as hourly workers whose ages are less than 30 and hourly pay rates are less than the 25th percentile of hourly wages for each year and greater than the minimum wage. These workers constitute about 6 percent of the overall sample. They exhibit a sizable, and in fact, a greater spike at zero than the overall sample and also show a higher share of workers with wage cuts.[25] Table 1.8 shows that low-paid young workers still show a similar cyclical pattern of nominal wage change distribution as the overall sample. Controlling for inflation, I find that a

[25]The average spike at zero for low-paid young workers is 18.7 percent, and the average share of workers with wage cuts is 32.3 percent over the period from 1979 to 2017. Both of them are greater than the overall sample averages, 15.2 percent, and 21.1 percent, respectively.

1 percentage point decline in employment is associated with 1) a 0.9 percentage point increase in the spike at zero; 2) a 0.8 percentage point increase in the share of workers with wage cuts; and 3) a 1.7 percentage point decrease in the share of workers with raises. This can be suggestive evidence that nominal wages are also rigid for those workers without a long-term contract.

Table 1.8: The spike at zero, the fraction of wage cuts, and raises for low-paid young workers along the business cycles

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero $\Delta W = 0$	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop ratio	0.693*	0.772*	-1.465**	0.899***	0.844*	-1.743***
$(1 - e_t)$	(0.324)	(0.373)	(0.526)	(0.188)	(0.363)	(0.402)
Inflation rate, π_t				-1.325***	-0.468	1.794**
				(0.101)	(0.466)	(0.517)
				0.899/1.743 = 0.5		
Observations	37	37	37	37	37	37
Adjusted R^2	0.104	0.0892	0.159	0.739	0.121	0.516

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). Inflation rate is calculated from CPI-U. The spike at zero, the share of workers with raises and cuts come from the annual nominal hourly wage growth distribution of low-paid young workers, who are younger than the age of 30 and earn less than equal to the 25 percentile of hourly wages for each year and greater than the minimum wages.

1.5.2 Aggregate analysis: SIPP

To analyze the cyclicality of nominal wage change distributions using the SIPP data, I construct the same three aggregate time series using three different samples: all workers, only job stayers and only job switchers. Table A13 in the appendix reports the spike at zero and the fraction of workers with wage cuts and raises for all hourly workers for each year. From this aggregate time series, we can see a sudden increase in the level of the spike at zero in 2005 and accordingly sudden decreases in the share of workers with wage cuts and raises. This is due to the introduction of the new survey design to 2004 panel and after – the dependent interviewing procedure. That is, if hourly workers mention that s/he is paid by the same as the last interview, the hourly pay rate at the current interview is automatically filled by the one from the last interview. Table A14 reports the time series of the three statistics for job stayers and job switchers. Similarly, there is also a sudden jump in the level of the spike at zero for job stayers in 2005 for the same reason.

I replicate the analysis using the regression specification (1.1). Unlike the CPS, the SIPP does not have rotating panels and there are discontinuities between panels. To control for heterogeneity across panels, for instance, the change in the survey design, panel fixed effects are included.[26] In Table 1.9, the first three columns report results for all hourly workers, column (4) ~ (6) are for job stayers, and the last three columns are for job switchers.

The results from the first three columns of Table 1.9 show that the spike at zero increases when employment declines and the spike at zero fluctuates more than the fraction with wage cuts, which is consistent with the results using the CPS.

The spike at zero of job stayers appears to respond to employment more than the spike at zero of job switchers does. However, I still find that the spike at zero of job switchers have countercyclical fluctuations. This implies that the cyclical property of nominal wage change distributions for all hourly workers are not solely driven by job stayers. If the greater association between the spike at zero and employment, compared to that of the share with wage cuts and employment, is due to downward nominal wage rigidity, then this analysis with the SIPP suggests that nominal wages are still rigid for job switchers, and more rigid for job stayers.

This contrasts with some of the findings in previous literature. I compare my method with those in the earlier studies, and discuss the potential reasons for the differences in

[26] Overall, 5 panel fixed effects are included. One for every panel before 1996 panel and dummies for 1996, 2001, 2004, and 2008 panel. There are 24 observations but 8 regressors.

Table 1.9: The spike at zero, the fraction of wage cuts and raises - job stayers vs. job switchers, SIPP

	All hourly paid workers		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
1 - Epop	1.794***	-0.437	-1.357***
$(1 - e_t)$	(0.386)	(0.270)	(0.438)
Inflation rate, π_t	0.0405	-0.753***	0.713*
	(0.312)	(0.213)	(0.391)
Panel Fixed Effect	Yes	Yes	Yes
	1.794/1.357=1.32		
Observations	24	24	24
Adjusted R^2	0.982	0.762	0.970

	Job stayers			Job switchers		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1 - Epop	2.186***	-0.369	-1.817***	1.234*	-0.383	-0.851
$(1 - e_t)$	(0.720)	(0.353)	(0.550)	(0.590)	(0.629)	(0.678)
Inflation rate, π_t	0.288	-0.856***	0.568	-0.218	-0.677	0.895*
	(0.357)	(0.220)	(0.447)	(0.351)	(0.574)	(0.499)
Panel Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
	2.186/1.817=1.20			1.234/0.851 = 1.45		
Observations	24	24	24	24	24	24
Adjusted R^2	0.985	0.877	0.975	0.644	0.567	0.810

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008). Sample includes hourly paid workers in the United Sates. The three columns in the first panel include all hourly paid workers. The first three columns in the second panel include only job stayers, and last 3 columns in the second panel include only job switchers. The spike at zero shows greater association with employment than the share of workers with wage cuts for both job stayers and job switchers.

findings in section A.1.3 of appendix.

1.6 The cyclicality of state-level nominal wage change distributions

In this section, I validate the above results using the state-level data. This allows me to use more observations to examine the relationship between employment, inflation and nominal wage changes distribution, controlling for state and year fixed effects. To explore the cyclicality of state-level nominal hourly wage change distributions, I now construct the following statistics for each state: the share of workers with zero year-over-year changes in hourly wages (the spike at zero), the share of workers with wage cuts and the share of workers with raises. The state-level data analysis leads to similar findings as the aggregate data analysis. I interpret these results to be consistent with downward nominal wage rigidity, and contrast them with the arguments from a recent study by Beraja, Hurst, and Ospina (2016).

1.6.1 State-level analysis of the cyclicality of nominal wage change distribution: CPS

Similarly to the regression equations (1.1) in the aggregate analysis, we can think of the following state-level regression equations:

$$\begin{aligned} \text{[Spike at zero]}_{it} &= \alpha_{i,s} + \gamma_{t,s} + \beta_s(1 - e_{it}) + \epsilon_{it,s} \\ \text{[Fraction of wage cuts]}_{it} &= \alpha_{i,n} + \gamma_{t,n} + \beta_n(1 - e_{it}) + \epsilon_{it,n} \ , \\ \text{[Fraction of raises]}_{it} &= \alpha_{i,p} + \gamma_{t,p} + \beta_p(1 - e_{it}) + \epsilon_{it,p} \end{aligned} \tag{1.2}$$

where e_{it} is the employment to population ratio for each state i ($i = 1, \cdots, 48$) and time t. α_i ($\alpha_{i,s}$, $\alpha_{i,n}$, and $\alpha_{i,p}$) capture state fixed effects, γ_t ($\gamma_{t,s}$, $\gamma_{t,n}$, and $\gamma_{t,p}$) absorb time fixed effects. State fixed effects control for state-specific differential time trends. Time fixed effects control for the factors that are common across states for each year such as monetary policy or aggregate inflation. As shown in section 1.5, controlling for inflation is important for obtaining a statistically significant relationship between employment and the share of workers with zero year-over-year wage changes. I estimate these equations using data from 50 states for the years 1979-2017 (except 1985, 1986, 1995, and 1996).[27]

Table 1.10: The spike at zero, the fraction of wage cuts and raises across states

	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
1 - Epop	0.383***	0.292***	-0.675***
$(1 - e_{it})$	(0.0792)	(0.0642)	(0.0865)
State fixed Effect, α_i	Yes	Yes	Yes
Time Fixed Effect, γ_i	Yes	Yes	Yes
	0.383/0.674 = 0.57		
Observations	1700	1700	1700
Adjusted R^2	0.606	0.537	0.712

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1980-2017 (except 1985, 1986, 1995, and 1996 due to small sample sizes). The sample consists of 50 states over 34 years. The state-level spike at zero, the share of workers with wage cuts and raies are regressed on the state-level 1-epop ratio with both state and time fixed effects.

Table 1.10 shows the regression results using the regression specification (1.1), exploiting state-level variations. It shows that a 1 percentage point decrease in employment

[27] These 4 years are dropped due to small sample size.

is associated with 1) an increase in the spike at zero by 0.38 percentage point, 2) an increase in the share of workers with a wage cut by 0.29 percentage point, and mechanically 3) a decrease in the share of workers with raises by 0.67 percentage point. In other words, when employment declines by 1 percentage point, the share of workers with raises also declines, and 57 percent (=0.38/0.67) of this change is attributed to the change in the share of workers with zero wage changes. The higher responsiveness of the spike at zero compared to the fraction of workers with wage cuts in the cross-section of U.S. states implies that state-level cyclical variations in nominal wage change distributions are still consistent with the results obtained in section 1.5 for time variations in data for the U.S. as a whole.

The point estimate of the excess responsiveness of the spike at zero compared to that of the share of workers with wage cuts is slightly smaller, in the state-level analysis than in the aggregate analysis. This is likely because time fixed effects absorb all aggregate variations and the state-level analysis only exploits the deviations from state-specific averages and time-specific aggregate averages.

1.6.2 State-level analysis: job stayers versus job switchers

Table 1.11 shows regression results based on the equation (1.2) using the SIPP, controlling for both time and state fixed effects. Time fixed effects control for aggregate factors common across states for each year such as the change in the survey design in 2004. The first three columns include all hourly workers, the next three columns include only job stayers, and the last three columns are for job switchers. State-level regression results using all hourly workers in the SIPP also show higher responsiveness of the spike at zero than the share of workers with wage cuts.

The pattern - greater countercyclicality of the spike at zero than the share of workers with wage cuts - holds for both job stayers and job switchers. Job stayers show higher

Table 1.11: The spike at zero, the fraction of wage cuts and raises - job-stayers vs. job-switchers across states, SIPP

	All hourly paid workers		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
1 - Epop	0.407***	0.0989	-0.506***
$(1 - e_{it})$	(0.101)	(0.0767)	(0.111)
State fixed effect	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes
	0.407/0.506=0.80		
Observations	855	855	855
Adjusted R^2	0.842	0.341	0.783

	Job stayers			Job switchers		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1 - Epop	0.489***	0.121	-0.610***	0.348***	0.124	-0.471**
$(1 - e_{it})$	(0.123)	(0.0789)	(0.121)	(0.101)	(0.176)	(0.182)
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
	0.489/0.610=0.80			0.348/0.471= 0.74		
Observations	855	855	855	855	855	855
Adjusted R^2	0.871	0.499	0.814	0.171	0.0608	0.148

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008). Geographical unit: States in US. Several small states are dropped due to small sample sizes. Overall 43 states. 36 states for 21 years. 7 states for 20 years. The three columns in the first panel include all hourly paid workers. The first three columns in the second panel include only job stayers, and last 3 columns in the second panel include only job switchers. The spike at zero shows greater association with employment than the share of workers with wage cuts for both job stayers and job switchers.

responsiveness of the spike at zero than job switchers, but the pattern still holds for job switchers as well. This again shows that job stayers are not the sole ones driving the results in the aggregate analysis, but the wages of job switchers also exhibit patterns consistent with downward nominal wage rigidity.

1.6.3 The Great Recession of 2007 - 2010

In a recent study, Beraja, Hurst, and Ospina (2016) (BHO, hereafter) argue that wages were "fairly flexible" during the Great Recession. These authors show that nominal wage growth rates were strongly and positively correlated with employment growth rates across states during the Great Recession. This finding is represented in the top panel of Figure 1.5, which plots the percentage change in the median nominal wage growth rate against the percentage change in employment from 2007 to 2010 for each state. This figure uses CPS data to replicate Figure 3 of of BHO. The difference between the wage data used in the study of BHO and my study is these authors compute the composition adjusted average nominal wage for each state every year using the American Community Survey (ACS), as the ACS does not have a panel structure.[28] The figure shows that a state with a higher fall in employment also has a lower wage growth rate. Based on this, BHO argue that wages were fairly flexible since nominal wage growth rates were responding to changes in employment.

In the bottom panel of Figure 1.5, I present a similar plot, but using the spike at zero on the y-axis instead. That is, I plot the percentage changes in the spike at zero against the percentage changes in employment from 2007 to 2010 for each state. This plot shows that the changes in the spike at zero are negatively correlated with changes in employment for the same time period. In other words, a state with a higher fall in employment had a higher increase in the spike at zero; more workers experienced downwardly rigid wages in

[28] The sample consists of men between the ages of 21 and 55 with a strong attachment to the labor market only.

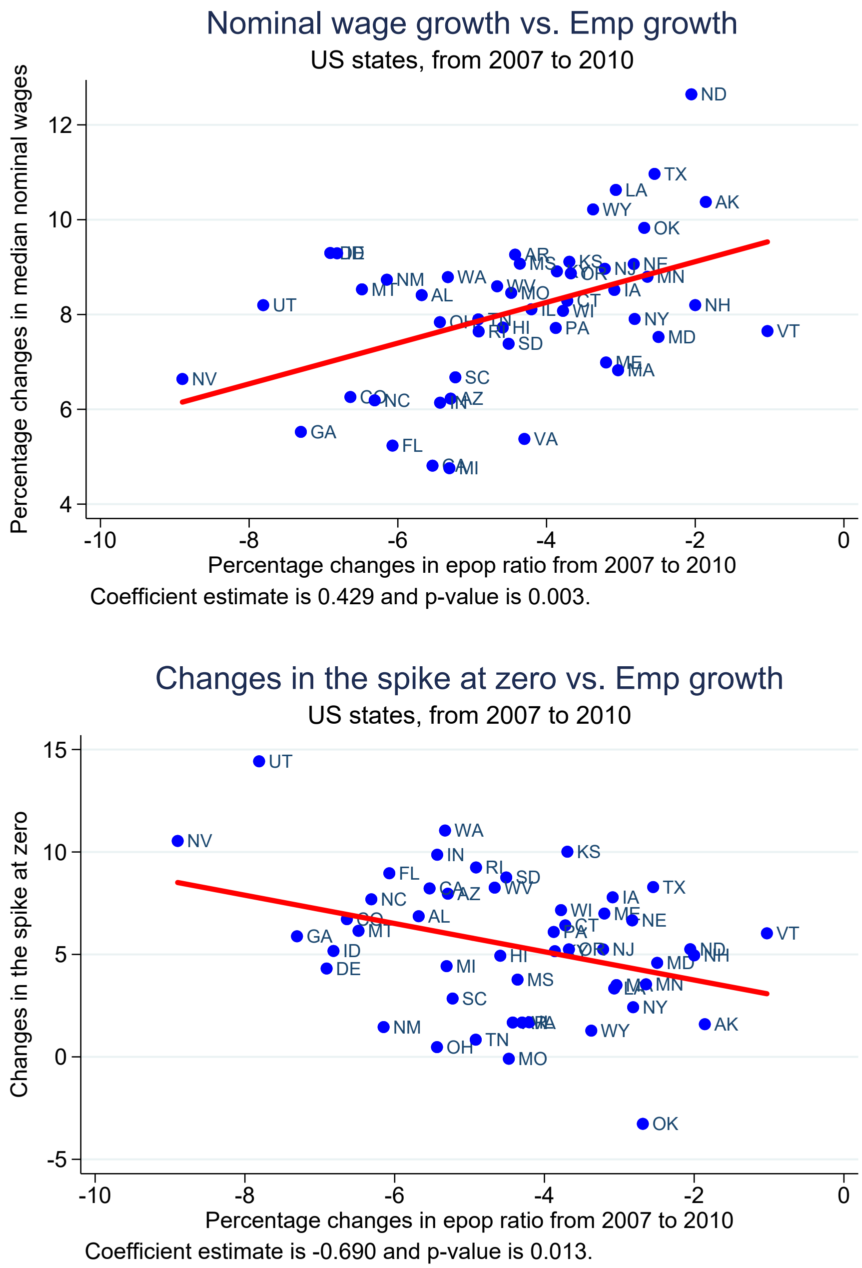


Figure 1.5: Nominal wage growth rates and changes in the spike at zero vs. employment growth from 2007 to 2010

Data source: CPS and author's calculation. The top panel shows the median nominal wage growth versus employment growth rates from 2007 to 2010 across states. The bottom panel shows the changes in the spike at zero versus employment growth from 2007 to 2010 across states. From 2007 to 2010, the annualized inflation rate was 1.7 percent, and the cumulative inflation was 5 percent.

Table 1.12: Changes in nominal wage distribution from 2007 to 2010 across states

	(1) Changes in Spike at zero $\Delta W = 0$	(2) Changes in Fraction of $\Delta W < 0$	(3) Changes in Fraction of $\Delta W > 0$	(4) $\ln \frac{W_{s2010}}{W_{s2007}}$
Percentage change in the employment	-0.690**	-0.215	0.904**	0.429***
	(0.269)	(0.321)	(0.397)	(0.136)
	0.690/0.904 = 0.76			
Observations	50	50	50	50
Adjusted R^2	0.103	-0.0103	0.0695	0.186

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 2007 - 2010. This table shows changes in nominal wage change distributions along with employment for each state from 2007 - 2010.

the states that had greater declines in employment.

I corroborate this finding by estimating the following regression equations for 2007-2010:

$$\begin{aligned} \Delta[\text{Spike at zero}]_i &= \alpha_s + \beta_s \Delta e_i + \epsilon_{i,s} \\ \Delta[\text{Fraction of wage cuts}]_i &= \alpha_n + \beta_n \Delta e_i + \epsilon_{i,n} \\ \Delta[\text{Fraction of raises}]_i &= \alpha_p + \beta_p \Delta e_i + \epsilon_{i,p} \\ \ln W_{i2010} - \ln W_{i2007} &= \alpha + \beta \Delta e_i + \epsilon_i \end{aligned} , \tag{1.3}$$

where Δe_i is the difference in the employment to population ratio from 2007 to 2010 in a state i. Table 1.12 shows regression results based on the equation (1.3). A 1 percentage point decrease in employment in a state is associated with 1) an increase in the size of spike at zero by 0.7 percentage points, 2) an increase in the share of workers with wage cuts by 0.2 percentage points, and 3) a decrease in the fraction with raises by 0.9 percentage points. We again see that the responsiveness of the spike at zero is larger than the responsiveness of the share with wage cuts, which is consistent with the findings reported earlier in table

1.6 for time series data and table 1.10 for cross-sectional data.

This result is still compatible with BHO's empirical finding, shown in the last column of Table 1.12: the positive correlation with nominal wage growth rates and changes in employment. This is because a state with a larger decline in employment is likely to also have a higher increase in the share of workers with wage cuts, leading to a overall drop in nominal wage growth rates. However, this is also accompanied by a much larger increase in the spike at zero. Thus, I argue that the finding by BHO does not contradict the existence of downward nominal wage rigidity.

1.6.4 The recession of 1979 - 1982

Table 1.13: Changes in nominal wage distribution from 1979 to 1982 across states

	(1) Changes in Spike at zero $\Delta W = 0$	(2) Changes in Fraction of $\Delta W < 0$	(3) Changes in Fraction of $\Delta W > 0$	(4) $\ln \frac{W_{s1982}}{W_{s1979}}$
Percentage changes in the employment	-0.374 (0.487)	0.163 (0.333)	0.211 (0.678)	0.607^{**} (0.281)
Observations	50	50	50	50
Adjusted R^2	0.00407	-0.0148	-0.0166	0.0715

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979 - 1982. This table shows changes in nominal wage change distributions along with employment for each state from 1979 - 1982.

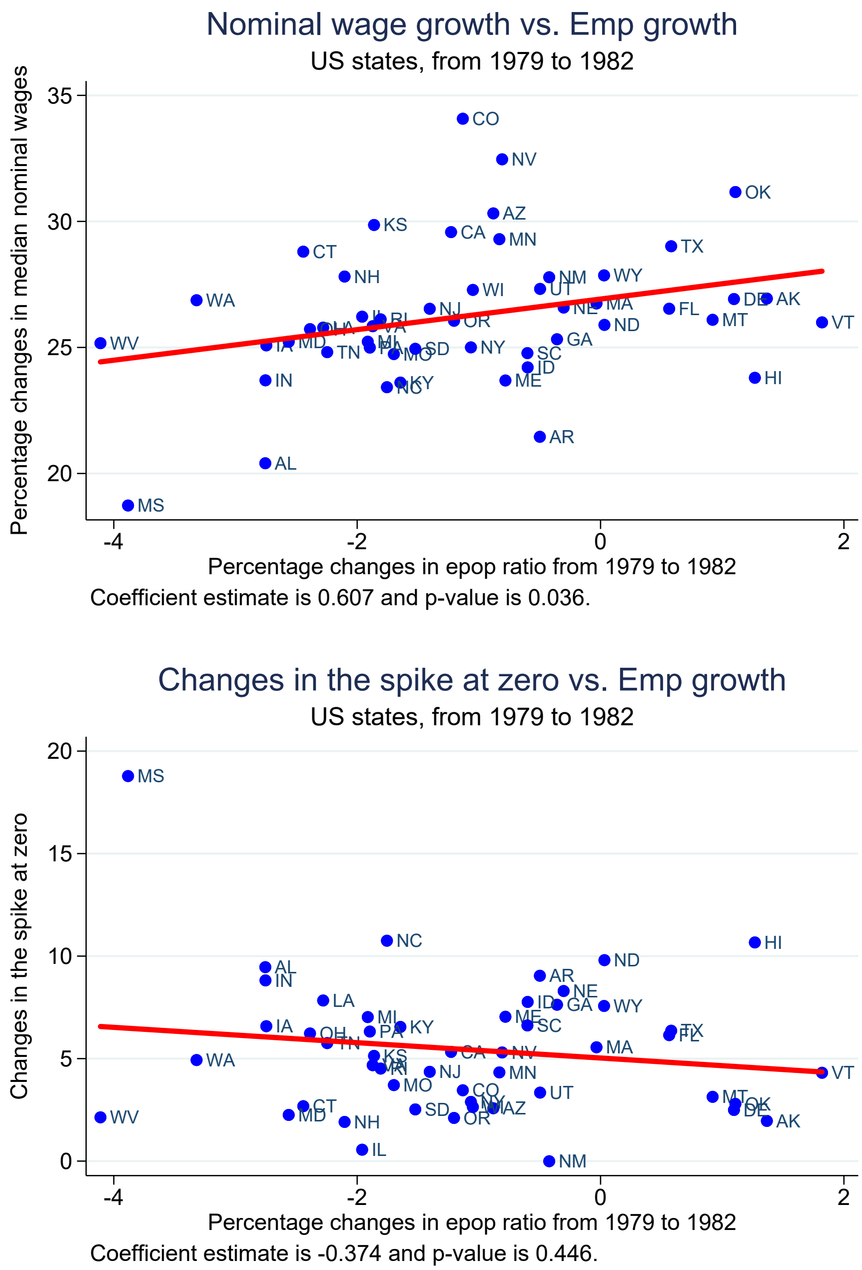


Figure 1.6: Nominal wage growth and changes in the spike at zero vs. employment growth from 1979 to 1982

Data source: CPS and author's calcuation. The top panel shows the median nominal wage growth with respect to employment growth rates from 1979 to 1982 across states. The bottom panel shows the change in the spike at zero with respect to employment growth from 1979 to 1982 across states. From 1979 to 1982, the average of annualized inflation rate was 9.5 percent and the cumulative inflation was 28.5 percent.

The Great Recession 2007 - 2010, was a period of relatively low inflation. Thus, it is a period in which downward nominal wage rigidity resulted in downward real wage rigidity, and hence reallocative effects on employment. One way to check whether nominal wages, as opposed to real wages, are downwardly rigid is to perform the same analysis just performed for the low inflation recession of 2007 - 2010 for a high inflation recession. In what follows I will consider the recession of 1979 - 1982,[29] because it was a deep recession – similar in size to the 2007 - 2010 recession, and inflation was high – the aggregate price level grew by 29 percent between 1979 and 1982. What we should see then under the hypothesis that nominal wages, as opposed to real wages, are downwardly rigid, in that there is no significant relationship in the cross-section of US states between employment changes and changes in the share of workers getting a zero wage change.

The top panel of Figure 1.6 shows state-level median nominal wage growth rates with respect to changes in employment across states from 1979 to 1982, and the bottom panel of Figure 1.6 shows changes in the spike at zero versus employment growth rates across states for the same period. Although median nominal wage growth rates show strong positive relationship with employment growth rates shown in the top panel of Figure 1.6, we cannot find the distinctive relationships between the changes in the spike at zero and changes in employment. Table 1.13 shows the regression results of changes in nominal wage change distributions on employment, confirming what we have seen from Figure 1.6, when the average inflation rate is high. This shows rigid nominal wages do not matter for the employment during the period of high inflation; it is about nominal wage rigidity, not real wage rigidity.

[29]Based on NBER recession dates, there were two recessions: January 1980 - July 1980 and July 1981 - November 1982.

1.7 Conclusion

I document the cyclical properties of nominal wage change distributions of individual workers. To construct nominal wage change distributions of individual workers, I use two nationally representative US household survey, the current population survey (CPS, 1979 – 2017) and the survey of income and program participation (SIPP, 1984 – 2014), which includes multiple business cycles.

As both data sets have panel features, I can plot the distribution of individual worker's year-over-year changes in nominal hourly wages for hourly workers for each year. One notable thing from the distributions is a large spike at exact zero, meaning that a huge fraction of workers has no wage changes from one year to another, and many more raises than cuts. This asymmetry of the nominal wage distributions with a spike at zero has been suggested in the literature to argue the existence of downward nominal wage rigidity.

This chapter contributes to the literature by documenting cyclical properties of nominal wage change distributions in a relation to employment and inflation. In a recession, when employment decreases, 1) the share of workers with no wage changes increases, 2) the share of workers with wage cuts also increases, and 3) the former is much larger than the latter, which is new empirical finding in the literature. In other words, in a recession, the increase in the share of workers with no wage change is double the increase in the share of workers with wage cuts. So in a recession, among those workers not having wage increases, two thirds of them would pile up at zero and a few of them have wage cuts.

Chapter 2

Theories of nominal wage rigidity

2.1 Introduction

The sluggish adjustment of nominal variables is key to understanding how demand shocks propagate in the economy, such as monetary shocks. In terms of product price rigidity, there is a large body of literature documenting the frequency and size of price changes using micro-level price data and theory incorporating microdata evidence. However, when it comes to wage rigidity, the theory of wage setting is still ad-hoc due to the lack of well-established facts regarding wage rigidity. Thus, this chapter asks which models with wage-setting schemes are able to match stylized facts documented in chapter 1 using micro-level wage data.

In this chapter, I build heterogeneous agent models with 5 alternative wage-setting schemes widely discussed in the literature - perfectly flexible, Calvo, long-term contracts, menu costs, and downward nominal wage rigidity. The models present not only idiosyncratic labor productivity uncertainty but also aggregate uncertainty. Using numerical methods, I characterize the year-over-year wage change distributions of individual workers implied by each model and study how they change with aggregate employment.

I find that, except for the perfectly flexible model, all the other models can predict a stationary wage change distribution with a spike at zero. However, the time-dependent models – Calvo and long-term contracts – fail to generate the countercyclical movement of the spike at zero. The Calvo model assumes to have the size of the spike at zero stay constant over the business cycle. Similarly, long-term contract model followed by Basu and House (2016) predicts constant spike at zero along the business cycle. A long-term contract is designed in a way that if a worker signs a long-term contract, then this worker is paid by the same contracted wage over the contract. Thus, the share of workers in an ongoing long-term contract determines the size of spike at zero, which induces the constant spike at zero. This contradicts the empirical finding documented in chapter 1 – the spike at zero increases in a recession.

On the other hand, the state-dependent models – both menu costs and downward nominal wage rigidity – can generate the countercyclical spike at zero. However, according to the menu cost model, the share of workers with wage cuts shows greater responsiveness than the share of workers with no wage changes, which contradicts the last empirical regularity, shown in chapter 1. In a recession, the optimal wage changes distribution in the absence of wage rigidity shifts to the left, more workers want to lower their wages optimally, due to an aggregate negative shock. This leads to an increase in the share of workers with wage cuts. In the menu costs model, only for those whose optimal wage change is close to zero decide not to change their wage due to a fixed cost, which becomes the part of the spike at zero. Thus, there is more increase in workers with wage cuts compared to workers having no wage change when employment declines, which is not consistent with the empirical finding , namely, the spike at zero fluctuates more than the share of workers with wage cuts.

Thus, among these alternative wage-setting schemes, I conclude the model with downward nominal wage rigidity is the most consistent with my new stylized fact - in a recession, the number of workers with zero wage changes increases more than the number of workers with wage cuts. In a recession, there are more workers who want to lower their wages optimally due to a negative aggregate shock. In the model with downward nominal wage rigidity, among those workers who want to lower their wages optimally, only a constant fraction of workers can lower their wage, and the other fraction of workers are not able to lower their wages. This leads to more increase in the spike at zero than the share of workers with wage cuts.

The remainder of the paper is organized as follows. Section 2.3 builds heterogeneous agent models with 5 alternative wage-setting schemes, equipped with both aggregate and idiosyncratic shocks. Section 2.4 compares numerical predictions from 5 those wage-setting schemes to the empirical findings. Section 2.5 concludes and discusses future work.

2.2 Related literature

This chapter is related to the theoretical literature on nominal wage rigidity. Schmitt-Grohé and Uribe (2016) build a representative agent model with downward nominal wage rigidity. In this model, nominal wages cannot decrease by more than a fixed fraction. This model predicts the spike at that fixed negative wage growth rate during the recession and no spike during the boom. Although only predicting discrete effect of downward nominal wage rigidity, this model implies that downward nominal wage rigidity is more binding during the recession.

Fagan and Messina (2009) use a heterogeneous agent model with downward nominal wage rigidity and show that the implied stationary wage change distribution is similar to the empirical nominal wage change distribution: a spike at zero and fewer wage cuts than wage increases. Their model has only idiosyncratic shocks. To generate the stationary distribution similar to the empirical distribution, they impose 3 different menu-costs: one for raises, one for cuts, and one for when wage growth rate is smaller than inflation.

Daly and Hobijn (2014) build a heterogeneous agent model with either perfectly flexible wages or downward nominal wage rigidity, and they compare the stationary distributions implied by the two models. After a one-time negative aggregate shock, they also find the spike at zero increases for the model with downward nominal wage rigidity. However, they do not consider the share of workers with wage cuts but only focus on the size of the spike at zero. Mineyama (2018) presents a heterogeneous agent model with downward nominal wage rigidity, equipped with both idiosyncratic and aggregate shocks. The model by Mineyama (2018) generates the countercyclical spike at zero; however, this paper also does not consider the changes in the share of wage cuts. Mineyama (2018) argues that downward nominal wage rigidity is helpful for explaining the observed flattening of the Philips curve during the Great Recession.

My theoretical analysis contributes to this literature by building models with all of the following components: (1) heterogeneous agents; (2) both idiosyncratic uncertainty and aggregate uncertainty; (3) 5 alternative wage-setting schemes - perfectly flexible, Calvo, long-term contracts, menu costs, and downward nominal wage rigidity. I compare the predictions of these models not only for the cyclical movement of the spike at zero but also for the share of workers with wage cuts, in order to provide a comprehensive analysis.

2.3 Five alternative models of wage rigidity with heterogeneous agents

In this section, I build heterogeneous agent models with both idiosyncratic and aggregate shocks, imposing 5 alternative wage-setting schemes - perfectly flexible, Calvo, long-term contracts, menu-costs, and downward wage rigidity model. A representative firm uses aggregate labor to produce output. The firm's profit maximization problem gives the labor demand function for each differenitated labor. Households supply heterogeneous labor determined by idiosyncratic labor productivity, and set nominal wages subject to labor demand and wage-setting constraints. The basic set up of the model is derived from Erceg, Henderson, and Levin (2000). Daly and Hobijn (2014); Mineyama (2018) introduce heterogeneous disutility of labor supply, and Fagan and Messina (2009) adds idiosyncratic labor productivity shocks to the basic model of Erceg, Henderson, and Levin (2000). The basic wage-setting mechanism of heterogeneous labor in this chapter is derived from Fagan and Messina (2009).

2.3.1 Firm

There is a representative firm, which produces consumption goods using aggregate labor. The firm has a constant returns to scale production function in aggregate labor, which is,

$$Y_t = L_t,$$

where L_t represents the aggregate labor. The profit function of the firm is

$$\Pi_t = P_t Y_t - W_t L_t,$$

where P_t is the price of goods and W_t is the aggregate nominal wage in the economy. There is no product price rigidity, and the firm's profit will be redistributed to households. The firm's problem to maximize profits is equivalent to minimize the cost of labor. Hence, the firm chooses differentiated labor $l_t(i)$, indexed by $i \in [0, 1]$, to minimize the total production cost

$$\min_{l_t(i)} \int W_t(i) l_t(i) di \quad \text{(s.t.)} \quad L_t = (\int_0^1 (q_t(i) l_t(i))^{\frac{\theta-1}{\theta}} di),$$

given $W_t(i)$ is nominal wage for each individual i and $q_t(i)$ is idiosyncratic productivity for i. The problem of minimizing the cost of labor gives the labor demand function by the firm,

$$l_t^d(i) = q_t(i)^{\theta-1}(\frac{W_t(i)}{W_t})^{-\theta}L_t, \quad \theta > 1,$$

where θ governs the elasticity of substitution across differentiated labor. The quantity of labor demand increases in the level of productivity and decreases in the relative wage. The aggregate wage W_t is given by the Dixit-Stiglitz aggregate wage index,

$$W_t = \left[\int \left[\frac{W_t(i)}{q_t(i)}\right]^{1-\theta} di\right]^{\frac{1}{1-\theta}}.$$

2.3.2 Households

There is a continuum of households, indexed by $i \in [0,1]$, and each household chooses the consumption, saving, nominal wage, and labor supply to maximize life-time utility subject to intertemporal budget constraint, the labor demand function, and a wage-setting constraint. Assume households have an additively separable preference between consumption and labor supply, similar to Erceg, Henderson, and Levin (2000).

Each household chooses the $\{C_t(i), B_{t+1}(i), W_t(i), l_t(i)\}$ to maximize

$$\max_{\{C_t(i),B_{t+1}(i),W_t(i),l_t(i)\}} \mathbb{E}_t\Sigma_{t=0}^{\infty}\beta^t\left[\frac{C_t(i)^{1-\gamma}}{1-\gamma} - \frac{1}{1+\psi}l_t(i)^{1+\psi}\right]$$

subject to

$$\begin{aligned} P_tC_t(i) + Q_{t+1}B_{t+1}(i) \leq & B_t(i) + W_t(i)l_t(i) + \Pi_t \\ l_t^d(i) = & q_t(i)^{\theta-1}(\frac{W_t(i)}{W_t})^{-\theta}L_t, \end{aligned}$$

Wage setting constraint

given with $\{P_t, Q_{t+1}, \Pi_t, B_0(i), L_t\}$. P_t is the price level of consumption goods. Each household saves by $B_{t+1}(i)$ and Q_{t+1} represents the risk-free price of 1unit of good for the next period. γ is the relative risk aversion parameter and ψ is the inverse Frisch elasticity parameter. There are complete contingent asset markets so that idiosyncratic labor income is fully insured and the household consumes the exactly same amount. However, the amount of leisure is not insured so that the level of utility is lower for those who worked more.

The Lagrangian of the households problem is given by

$$\mathcal{L} = \mathbb{E}_t \Sigma_{t=0}^{\infty} \beta^t \left\{ \begin{array}{l} \frac{C_t(i)^{1-\gamma}}{1-\gamma} - \frac{\omega}{\psi+1} l_t(i)^{1+\psi} \\ +\lambda_t(i)[B_t(i) + W_t(i)l_t(i) + \Pi_t - P_t C_t(i) - Q_{t+1}B_{t+1}(i)] \\ +\mu_t(i)[q_t(i)^{\theta-1}(\frac{W_t(i)}{W_t})^{-\theta} L_t - l_t(i)] \\ +\theta_t(i)[\text{Wage-setting constraint}] \end{array} \right\} \tag{2.1}$$

The first-order conditions with respect to $C_t(i)$ and B_{t+1}(i) are

$$C_t(i)^{-\gamma} = \lambda_t(i)P_t,$$

$$\lambda_t(i)Q_{t+1} = \beta \mathbb{E}_t \lambda_{t+1}(i),$$

respectively. As consumption risks are fully insured by complete state contingent asset markets, we can rewrite the first order conditions as follows.

$$\lambda_t(i) = \lambda_t = \frac{C_t^{-\gamma}}{P_t}$$

$$Q_{t+1} = \beta \mathbb{E}_t \left[\frac{P_t}{P_{t+1}} \left(\frac{C_{t+1}}{C_t} \right)^{-\gamma} \right]$$

2.3.3 Five wage-setting restrictions

As the household utility is additively separable, we can isolate the wage relevant part of the Lagrangian (2.1) and households choose the wage $W_t(i)$ and labor supply $l_t(i)$ to maximize

$$\max_{\{W_t(i),l_t(i)\}} \mathbb{E}_t\Sigma_{t=0}^{\infty}\beta^t\Big\{\lambda_t(i)W_t(i)l_t(i) - \omega\frac{l_t(i)^{1+\psi}}{1+\psi}\Big\} \quad \text{(s.t.)} \quad \underbrace{l_t^d(i) = q_t(i)^{\theta-1}(\frac{W_t(i)}{W_t})^{-\theta}L_t}_{\text{Wage-setting constraint}} \tag{2.2}$$

this chapter introduces five alternative wage-setting schemes.The first is that a perfectly flexible case in which there is no wage-setting constraint.

Second, consider Calvo wage rigidity, assuming only a constant fraction of workers can optimize wages. This is the most commonly used wage-setting mechanism for nominal rigidity.[1] Followed by Calvo (1983), wage setters cannot optimize their wages with the constant probability of μ^{Calvo}, regardless of the state of the economy. The Calvo wage-setting constraint can be rewritten as following,

$$W_t(i) = \begin{cases} W_{t-1}(i) & \text{, with the prob } \mu^{\text{Calvo}} \\ W_t^*(i) & \text{, with the prob } (1-\mu^{\text{Calvo}}) \end{cases}$$

, where $W_t^*(i)$ is the optimal wage, nominal wage that maximizes the equation (2.2) in the absence of wage-setting constraint in a period t.

Third, consider a long-term contract model. As workers are often in a long-term contract with the firm, the present discounted value of expected nominal wages over the contract is important to determine employment rather than the remitted wages or observed wages in each point of time. This is often called Barro's critique (Barro (1977)) or efficiency-wage theory. To address this concern by Barro (1977), Basu and House (2016) introduced long-term contracts in a New Keyensian model in which firms pay the same nominal wages (remitted wages) over the contract. In this model, there are two notions

[1] Erceg, Henderson, and Levin (2000); Christiano, Eichenbaum, and Evans (2005); Smets and Wouters (2007), and so on

of wages: allocative wages and remitted wages. Allocative wages determine the level of employment and remitted wages are the one that the firm actually remits to the workers. Firms calculate allocative wages under the perfectly flexible case and find the remitted wages of which present discounted value is the same as the present discounted value of allocative wages over the contract. Following by Basu and House (2016), the remitted wages for each i type of labor, $x_t(i)$ can be determined as follows.

$$\mathbb{E}_t[\Sigma_{j=0}^{\infty}[\beta(1-s)]^j \frac{\lambda_{t+j}}{\lambda_t} w_{t+j}(i)] = \mathbb{E}_t[\Sigma_{j=0}^{\infty}[\beta(1-s)]^j \frac{\lambda_{t+j}}{\lambda_t} x_t(i)]$$

$$x_t(i) = \frac{\mathbb{E}_t[\Sigma_{j=0}^{\infty}[\beta(1-s)]^j \frac{\lambda_{t+j}}{\lambda_t} w_{t+j}(i)]}{\mathbb{E}_t[\Sigma_{j=0}^{\infty}[\beta(1-s)]^j \frac{\lambda_{t+j}}{\lambda_t}]},$$

where s is the probability of renewing the contract.

Fourth, consider the menu-costs model of wage rigidity, motivated by the empirical evidence that changes in nominal wage change distribution is state-dependent. In the context of wage-setting model, we may imagine the cost involved in changes in wages. For example, whenever the wage setters want to change their wages, they have to pay an additional cost of bargaining to bring them to the bargaining table. Wage setters must pay menu-costs to change their wage with the probability of μ^{Menu}. With the other probability of $1-\mu^{\text{Menu}}$, wage setters can freely change their wage. The model with random menu-cost in the price rigidity literature (Alvarez, Le Bihan, and Lippi (2016)) to explain small changes in prices. This can be summarized as follows.

$$W_t(i) = \begin{cases} \begin{cases} W_t^*(i) & \text{if } W_t^*(i) \neq W_{t-1}(i)\text{, pays cost } K \\ W_{t-1}(i) & \text{No cost} \end{cases} & \text{,with the prob of } \mu^{\text{Menu}} \\ W_t^*(i) & \text{,with the prob of (1-}\mu^{\text{Menu}}\text{)} \end{cases}$$

The fifth wage-setting scheme is the downward nominal wage rigidity model. If the optimal wage in a period t, $W_t^*(i)$, maximizing the equation (2.2) in the absence of wage-setting constraint in a period t, is higher than the previous wage, $W_{t-1}(i)$, then the current wage can be the optimal wage, $W_t(i) = W_t^*(i)$. There is no explicit restriction to raise the current nominal wage. However, if the optimal wage in a period t , $W_t^*(i)$, is lower than the previous wage, $W_{t-1}(i)$, then wage setter cannot lower wage with the probability of μ^{DNWR}. With the other probability of $(1-\mu^{\text{DNWR}})$, wage setters can lower wages optimally. This wage-setting restriction can be summarized, as follows.

$$\text{if } W_t^*(i) \geq W_{t-1}(i) \begin{cases} W_t(i) = W_t^*(i) \end{cases}$$

$$\text{if } W_t^*(i) < W_{t-1}(i) \begin{cases} W_t(i) = W_{t-1}(i) & \text{,with the prob } \mu^{\text{DNWR}} \\ W_t(i) = W_t^*(i) & \text{,with the prob } (1-\mu^{\text{DNWR}}) \end{cases}$$

Although there is no explicit restriction on raising nominal wages, there is an implicit restriction on raising nominal wages, as the wage setters solve the intertemporal problem. When wage setters find the optimal to increase their wage, they do not increase as much as they want to maximize current utility because they understand that they cannot lower their wages with the probability of μ^{DNWR} in the future. This is pointed out by Elsby (2009) and Mineyama (2018).

2.3.4 Closing the market

The goods market clearing condition is

$$Y_t = C_t.$$

In the economy, nominal output equals to the total wage payment in the economy, which is the same as total money supply in the economy, as follows.

$$P_t Y_t = P_t C_t = W_t L_t = M_t,$$

where M_t is the aggregate money supply. Monetary authority uses nominal output growth rate targeting rule, given by

$$\ln(M_{t+1}) = \mu + \ln(M_t) + \eta_{t+1} \quad \eta_{t+1} \sim \mathbb{N}(0, \sigma_\eta^2), \tag{2.3}$$

where μ is the average growth of nominal output. Idiosyncratic productivity shock follows AR(1) process as following:

$$\ln(q_{t+1}(i)) = \rho_q \ln(q_t(i)) + \epsilon_{t+1}(i), \quad \epsilon_{t+1}(i) \sim \mathbb{N}(0, \sigma_\epsilon^2).$$

2.3.5 Value function

We can write down households' wage-setting problem in a recursive way. Note that the value function is a function of the relative wage rather than both individual wage and aggregate wage, which allows us to reduce one dimension of the problem, followed by Nakamura and Steinsson (2008).

Under Calvo wage rigidity, wage setters can optimize their wage with probability $(1 - \mu^{\text{Calvo}})$ regardless of the sign of wage change. To introduce randomness, one more state variable, x_t, a binary variable, is added. Once x_t equals 1 with the probability of $(1-\mu^{\text{Calvo}})$, wage setters can reoptimze their wage. The recursive problem under the Calvo rigidity can be written as follows:

$$
\begin{aligned}
&V(q_t(i), L_t, \tfrac{W_{t-1}(i)}{W_t}, x_t) \\
&\quad = \max\nolimits_{W_t(i)} \left[H(q_t(i), L_t, \tfrac{W_t(i)}{W_t}) + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_t(i)}{W_{t+1}}, x_{t+1})) \right] \mathbb{I}(x_t = 1) \\
&\qquad + \max\nolimits_{W_t(i)} \Bigg[H(q_t(i), L_t, \tfrac{W_{t-1}(i)}{W_t}) - C \times \mathbb{I}(W_t(i) \neq W_{t-1}(i)) \\
&\qquad\qquad + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_{t-1}(i)}{W_{t+1}}, x_{t+1})) \Bigg] \mathbb{I}(x_t = 0)
\end{aligned}
$$

, where $C > \infty$ and

$$H(q_t(i), L_t, \frac{w_t(i)}{W_t}) = q_t(i)^{\theta-1} (\frac{w_t(i)}{W_t})^{1-\theta} L_t^{(1-\gamma)} - \omega \frac{[q_t(i)^{\theta-1} (\frac{w_t(i)}{W_t})^{-\theta} L_t]^{1+\psi}}{1+\psi},$$

which can be derived from substituting labor demand into the current objective function in the equation, (2.2). When x_t is one, wage setters adjust nominal wages freely, whereas wage setters must pay infinite cost of wage adjustment when x_t equals to zero.

For the menu-costs model, wage setters have to pay an additional fixed cost, K, to adjust their wage with the probability of μ^{Menu}, when x_t equals to zero. With the other probability of $(1-\mu^{\text{Menu}})$, wage setters can adjust wages without any cost. The recursive problem with menu costs can be written as follows:

$$\begin{aligned}
&V(q_t(i), L_t, \tfrac{W_{t-1}(i)}{W_t}, x_t) \\
&\quad = \max_{W_t(i)} \left[H(q_t(i), L_t, \tfrac{W_t(i)}{W_t}) + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_t(i)}{W_{t+1}}, x_{t+1})) \right] \mathbb{I}(x_t = 1) \\
&\qquad + \max_{W_t(i)} \left[H(q_t(i), L_t, \tfrac{W_t(i)}{W_t}) - K\mathbb{I}(W_t(i) \neq W_{t-1}(i)) \right. \\
&\qquad\qquad \left. + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_t(i)}{W_{t+1}}, x_{t+1})) \right] \mathbb{I}(x_t = 0).
\end{aligned}$$

Under the downward nominal wage rigidity, wage setter's problem is

$$\begin{aligned}
&V(q_t(i), L_t, \tfrac{W_{t-1}(i)}{W_t}, x_t) \\
&\quad = \max_{W_t(i)} \left[H(q_t(i), L_t, \tfrac{W_t(i)}{W_t}) \mathbb{I}(\tfrac{W_t(i)}{W_t} > \tfrac{W_{t-1}(i)}{W_t}) + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_t(i)}{W_{t+1}}, x_{t+1})) \right] \\
&\qquad + \max_{W_t(i)} \left[H(q_t(i), L_t, \tfrac{W_t(i)}{W_t}) \right. \\
&\qquad\qquad \left. + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_t(i)}{W_{t+1}}, x_{t+1})) \right] \mathbb{I}(\tfrac{W_t(i)}{W_t} \leq \tfrac{W_{t-1}(i)}{W_t}) \mathbb{I}(x_t = 1) \\
&\qquad + \left[H(q_t(i), L_t, \tfrac{W_{t-1}(i)}{W_t}) + \beta \mathbb{E}(V(q_{t+1}(i), L_{t+1}, \tfrac{W_{t-1}(i)}{W_{t+1}}, x_{t+1})) \right] \mathbb{I}(\tfrac{W_t(i)}{W_t} \leq \tfrac{W_{t-1}(i)}{W_t}) \mathbb{I}(x_t = 0).
\end{aligned}$$

If the current optimal wage is higher than the previous wage, wage setters can raise the nominal wages. However, if the current optimal wage is lower than the previous wage, wage setters can adjust downwardly only if x_t equals to 1, with the probability of $(1-\mu^{\text{DNWR}})$. So, if x_t equals to zero with probability of μ^{DNWR}, current wage ($W_t(i)$) is set to be previous wage ($W_{t-1}(i)$).

2.4 Numerical results

As the model has both idiosyncratic shock and aggregate shock, I solve the model numerically. This sections starts to explain calibrated parameters and solution methods. This section shows the stationary nominal wage change distribution and cyclical properties of nominal wage change distributions from five alternative wage-setting schemes. This chapter shows only downward nominal wage rigidity model exhibits consistent implications with empirical distributions. Finally, this chapter compares data moments to moments predicted by the model.

2.4.1 Calibration

Table 2.1 shows calibrated parameters. Parameters in the top panel show parameters related to preference. The relative risk aversion parameter, γ, is 1, which implies the intertemporal elasticity of substitution as 1. The discount rate β is 0.97, which implies a steady-state annual real interest rate is 3 percent. ψ = 0.5 is the inverse of Frisch elasticity, which is in a permissible range of macro literature shown in Chetty, Guren, Manoli, and Weber (2011). Different from earlier parameters, there is no consensus regarding the wage elasticity of labor demand, θ. θ varies from 1.67 to 21 from the previous theory literature.[2] This chapter sets θ to be 3, which implies steady state markup 1.5, followed by Smets and Wouters (2007). Recent paper by De Loecker and Eeckhout (2017) mention that the average markup in 1980 was 1.18 and started to rise and it becomes 1.67 in 2014.

The second panel of Table 2.1 shows the parameters governing shock processes in the economy. Since the nominal output is total wage payment in the model, this chapter uses

[2] Erceg et al. (2000) set θ at 4. Christiano et al. (2005) set θ at 21. Smets and Wouters (2007) set wage markup at 1.5, which implies θ being 3. Daly and Hobijn (2014) set θ at 2.5. The model from the Daly and Hobijn (2014) has homogeneous differentiated labor but households have different disutility from the labor supply. Fagan and Messina (2009) used $\theta = \frac{11}{12}$. Mineyama (2018) used θ at 9, which makes the steady state wage mark up 12.5 percent

total wage payment[3] to estimate the aggregate shock process, given by the equation (2.3). I estimated the constant growth rate (μ) and the standard deviation from the growth rate of the total wage payment. Parameters related to idiosyncratic productivity are from the Guvenen (2009). Guvenen (2009) decompose individual labor earnings into nonstationary and stationary components using more than 20 years of individual labor earnings data from PSID. For the individual labor productivity shock in this chapter, I use the stationary process of labor earnings from Guvenen (2009), allowing heterogeneity growth rate of income.[4]

The last panel of Table 2.1 shows parameters governing the degree of wage rigidity. The probability that workers constrained not to adjust their wages downwardly, μ^{DNWR}, comes from Table 1.6, aggregate evidence using the CPS. Among households whose optimal wages are lower than the previous wages, only 33 percent of them can lower current wages at the optimal level. Other 67 percent of workers cannot lower wages if the optimal wages are below the previous wages. Therefore, μ sets to be 0.67. Other than downward nominal wage rigidity wage-setting, $\mu^{\text{Calvo}}$from Calvo model, s from long-term contracts model, and μ^{Menu} and K from menu costs model, are set to have the same size of the spike at zero at the steady-state level of the spike at zero under the downward nominal wage rigidity.

2.4.2 Solution methods

This chapter solves the recursive problem using the policy function iteration over the discretized state space. Wage setter's problem is infinite dimensional as they have to take into account the entire wage and productivity distribution. Followed by Krusell and Smith (1998), this chapter assumes agents use only partial information, the first and second

[3]The total wage payment is defined as the median weekly earning (Series ID: LEU0252881500) times the number of people at work (CPS series LNU02005053). Source: https://www.bls.gov/data

[4]Table 1 row(4) from Guvenen (2009). HIP (heterogeneity income process) after assuming $\sigma_\beta \neq 0$

Table 2.1: Calibrated Parameters

Parameters	Value	Description	Target/Source
γ	1	Relative Risk Aversion	
β	0.971	Discount rate	Annual interest rate, 3%
ψ	0.5	Inverse of Frisch elasticity	
θ	3	Elasticity of substitution	
μ	0.044	Mean level of aggregate shock	Total wage payment
σ_m	0.021	Standard deviation of aggregate shock	
ρ_q	0.821	Persistence of idiosyncratic shock	Guvenen (2009)
σ_q	0.17	Standard deviation of idiosyncratic shock	Guvenen (2009)
μ^{DNWR}	0.67	The probability of DNWR	The cyclicality of DNWR
μ^{Calvo}	0.22	The frequency of no wage change	Matching the spike at zero, implied by DNWR model
$\mu^{\text{Menu cost}}$	0.8	The probability of facing menu cost	
K	0.002	Menu cost	
s	0.23	The probability of continuing contract	

Time unit is a year.

moments of the distribution, to predict the law of motion of the aggregate wage growth. I choose the simple parametric function for the aggregate wage growth rate, as follows.

$$\begin{aligned} W_{t+1} &= H(W_t, M_{t+1}) \\ \ln(\frac{W_{t+1}}{W_t}) &= H(\ln(\frac{M_{t+1}}{W_t})) = \gamma_0 + \gamma_1 \ln \frac{M_{t+1}}{W_t} + \gamma_2(\ln \frac{M_{t+1}}{W_t})^2 \end{aligned} \tag{2.4}$$

Parameters, γ_0, γ_1, and γ_2, are estimated by regressing the realized wage inflation on the aggregate state variables. Starting from the initial guess, the algorithm is iterated until the predicted wage inflation gets close enough to the realized wage inflation. The detailed algorithm is followed by Heer and Maussner (2009), which is available in the appendix B.1. Krusell and Smith (1998) reported R^2 to check the accuracy of the predicted law of motion and Den Haan (2010) argue that the maximum forecast error should be reported. R^2 is higher than 0.98 [5] and the maximum forecast error is less than 0.1 percent.

[5] $R^{2,\text{Flex}} = 0.99$, $R^{2,\text{Calvo}} = 0.98$, $R^{2,\text{Menu}} = 0.99$, and $R^{2,\text{DNWR}} = 0.98$.

2.4.3 Stationary wage change distribution

Figure 2.1 shows the stationary nominal wage change distributions generated from 5 alternative wage-setting schemes. The red bar represents the fraction of workers with exact zero wage changes and the width of the blue bar is 0.01. The top left panel shows the stationary wage change distribution under the perfectly flexible case. It is symmetric around the median and there is no spike at zero.

The Calvo model generates the spike at zero but the symmetric stationary wage change distribution. The second left panel of Figure 2.1 shows the stationary wage change distribution generated by Calvo model. We can observe the spike at zero, which is shown as the red bar. The frequency of wage adjustment from the Calvo model is assumed to be constant over the business cycle, so does the frequency of no wage change. However, we cannot find the asymmetry of nominal wage distribution - lack of wage cuts compared to raises. Instead, the stationary distribution is symmetric around the median, excluding the spike at zero. We can imagine one variant of the Calvo model in which the frequency of wage adjustment is stochastic, responding to the business cycle. In this way, we may be able to generate the countercyclical spike at zero, but we cannot generate the asymmetric wage distribution: fewer wage cuts than raises.

The long-term contract wage-setting generates the spike at zero but symmetric stationary wage change distribution. The second right panel of Figure 2.1 shows the remitted wage change distribution from the long-term contract under the perfect foresight. Allocated wages come from the perfectly flexible model, so its implications on employment should be the same as the perfectly flexible model. However, the stationary wage distribution has the spike at zero and is symmetric around the median, which is similar to the one from the Calvo model, which is again inconsistent with empirical findings.

Menu-costs of wage adjustment generates the spike at zero, but there is no

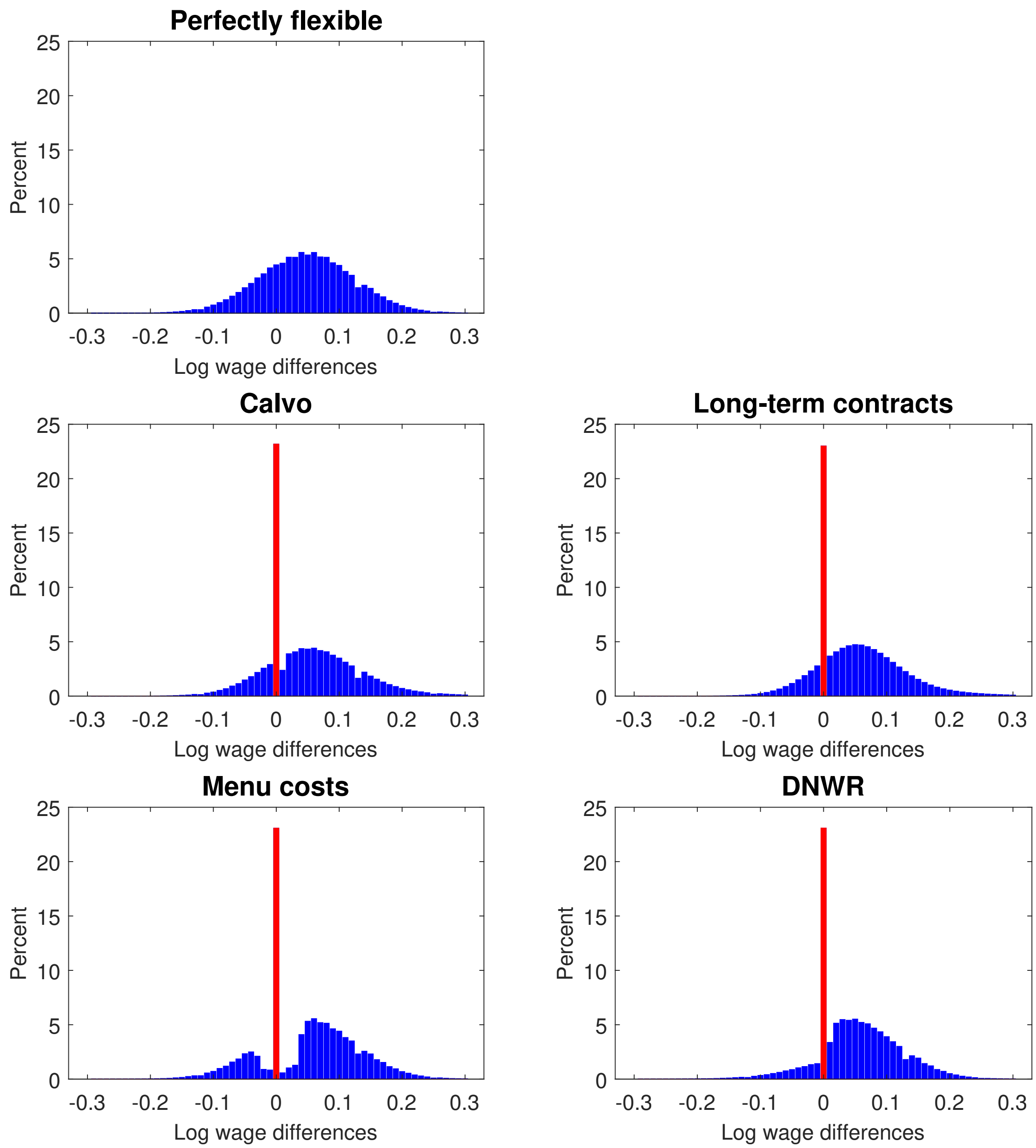


Figure 2.1: Staionary wage change distribution from 5 different wage-setting schemes

Stationary distribution generated by 5 alternative wage-setting schemes are drawn. The red bar represents the percentage of workers with no wage change and the size of the blue bin is 0.01. The top left panel is from a perfectly flexible case. The second row is from the Calvo model (left) and long-term contracts model (right). The bottom panel is from the menu-costs model (left) and downward nominal wage rigidity (right).

discontinuous drop in the stationary distribution approaching to zero from the left compared to approaching from the right. The stationary wage change distribution from the menu-costs model is shown at the bottom left panel of Figure 2.1. As wage setters must pay an additional fixed cost for any changes in wages, wage setters decide to change their wages only when the current wages are significantly different from the optimal wages. Hence, the size of wage change is big and there are not many small wage changes compared to the Calvo model. Under the positive inflation, the optimal nominal wage change distribution has always higher densities above zero than below zero. Therefore, more portion of the spike at zero comes from the right to the zero rather than the left to the zero, which leads to the lack of raises compared to wage cuts. This is inconsistent with empirical nominal wage change distribution, shown in the section 1.4.

The downward nominal wage rigidity wage restriction generates a spike at zero and a sudden drop in below zero compared to above zero from the stationary nominal wage change distribution. The bottom right panel of Figure 2.1 displays nominal wage change distribution under the downward nominal wage rigidity model. We can observe the spike at zero. Furthermore, it is asymmetric - fewer wage cuts than raises, and there is a sudden drop in the below zero compared to the above zero. Therefore, we can conclude that only model with downward nominal wage rigidity among 5 wage-setting schemes generates the stationary distribution, consistent with empirical findings.

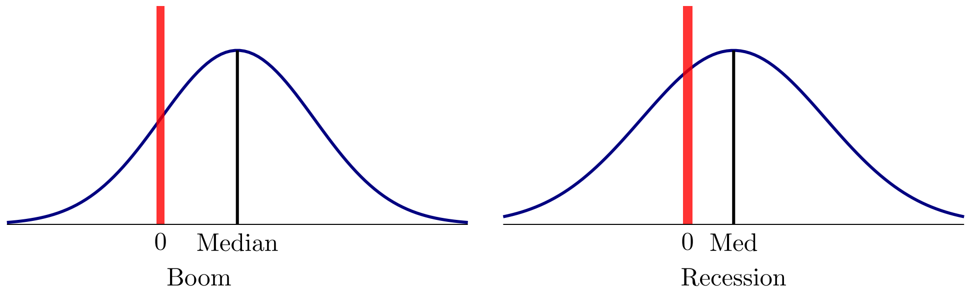
2.4.4 The cyclicality of wage change distribution

This section runs the main regression (1.1) using simulated data from 5 alternative wage-setting schemes to see which model has consistent implications on cyclicality patterns of nominal wage change distributions: 1) the spike at zero increases when employment declines and 2) the increase in the spike at zero is higher than the increase in the fraction

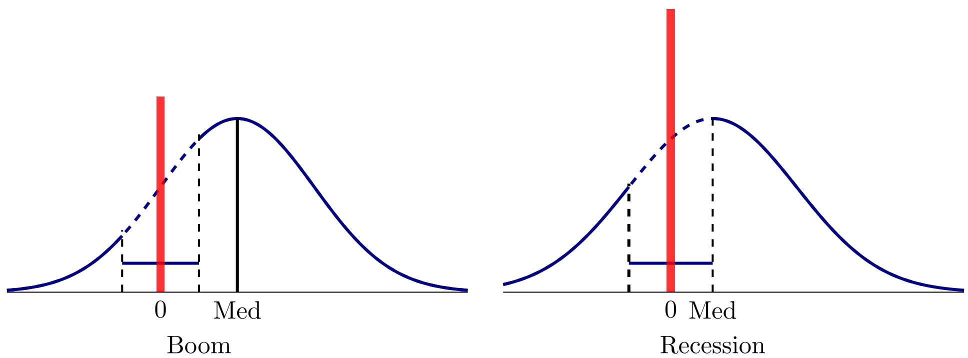
Table 2.2: The spike at zero, the fraction of wage cuts, and raises along business cycles

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
	Data		
Employment	-0.616	-0.305	0.921
Inflation	-1.181	-0.674	1.855
	Perfectly flexible		
Employment	-0.042	-0.414	0.456
Inflation	-0.042	-4.476	4.519
	Calvo		
Employment	0.089	-0.553	0.465
Inflation	-0.192	-3.919	4.111
	Long-term contracts		
Employment	0.005	-0.424	0.419
Inflation	-0.018	-4.207	4.225
	Menu costs		
Employment	-0.187	-0.329	0.516
Inflation	-1.623	-3.452	5.074
	DNWR		
Employment	-0.712	-0.329	1.041
Inflation	-3.699	-1.772	5.470

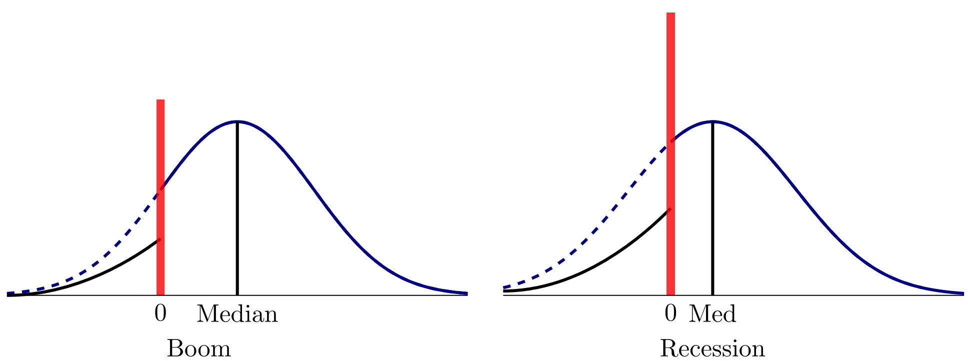
Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). The inflation rate is calculated from CPI-U. The first panel is from data, last three columns of table 1.6. This table (from the second panel to the last one) shows the regression results based on the equation (1.1) using simulated data series under 5 alternative wage-setting schemes.



Conceptual wage change distribution from the Calvo model



Conceptual wage change distribution from the menu costs model



Conceptual wage change distribution from the DNWR model

Figure 2.2: Conceptual wage change distribution from alternative wage-setting schemes

This figure shows conceptual nominal wage change distributions under Calvo, menu costs, and DNWR wage-setting restriction. Upon the business cycle, nominal wage change distribution in the absence of rigidity shifts right or left in a boom or a recession, respectively. Calvo rigidity implies the constant spike at zero along the business cycle. Menu costs model implies the countercyclical spike at zero, but more fraction of the spike at zero comes from workers otherwise would have positive wage growth. DNWR implies the countercyclical spike at zero and the increase in the spike at zero is higher than the increase in the fraction of workers with wage cuts when employment declines.

of wage cuts when employment declines. Table 2.2 illustrates the regression results from the data and the models. The first panel of the table shows the cyclicality of nominal wage change distributions from national level analysis, which is shown at the last three columns of Table 1.6 from the section 1.5.1.

Nominal wage change distributions in the model shift left or right along the business cycle under a perfectly flexible wage model. The second panel of Table 1.6 shows regression results using simulated data series under the perfectly flexible wage setting. After controlling inflation, we can see that the increase in the fraction of workers with wage cuts is almost the same as the decrease in the fraction of workers with raises when employment declines without changing the spike at zero, which is inconsistent with the empirical findings.

The Calvo model presents the constant spike at zero along the business cycle. The third panel of Table 1.6 shows regression results using simulated data under the Calvo model. The spike at zero barely responds to employment because the Calvo wage adjustment assumes the spike at zero, the frequency of no wage change, does not respond to the business cycle. Thus, we can observe a small coefficient of the spike at zero on employment. The conceptual diagram of changes in wage distributions under the Calvo model is shown at the first panel of Figure 2.2. Along the business cycle, the optimal nominal wage changes distribution shifts left or right. When employment declines, nominal wage change distribution shifts to the left and the fraction of workers with raises declines, leading to the increase in the fraction of workers with wage cuts to the same extent without any impact on the spike at zero. This is inconsistent with empirical findint that the spike at zero is countercyclical and the greater responsiveness of the spike at zero than the share of workers with wage cuts.

The long-term contracts model also shows the constant spike at zero along the business cycle similar to the Calvo model. The fourth panel of Table 1.6 shows regression results using simulated data implied by the long-term contracts model. The decrease in the fraction of workers with raises leads to the increase in the fraction of workers with wage cuts by the same magnitude when employment declines. This is again inconsistent with empirical findings.

The spike at zero implied by menu costs model responds to the employment, as the menu costs model is state-dependent. The fifth panel of Table 1.6 shows regression results using simulated data under the menu costs model. The spike at zero rises when employment declines. Intuitively, nominal wage distribution in the absence of rigidity will shift to the left in the recession, shown at the second panel of Figure 2.2. Then, there are more densities around the zero, that is, there are more densities in the inaction region, and this will increase the size of the spike at zero since fixed menu costs will be incurred to any changes in nominal wage with the probability of μ^{Menu}. While the whole optimal wage change distribution shifts to the left during a recession, only a certain fraction of worker's wages in the inaction region, whose optimal wages are close enough to the previous wages, do not change, which adds the size of the spike at zero. This leads to higher responsiveness of the share of workers with wage cuts compared to the spike at zero, which is inconsistent with empirical evidence.[6]

The downward nominal wage rigidity model implies the spike at zero rises and the increase in the spike at zero is higher than the increase in the fraction of workers with wage cuts when employment declines, consistent with the empirical finding. The last panel of Table 1.6 shows regression results using simulated data under the downward nominal wage rigidity model. In the downward nominal wage rigidity model, when there is a decrease

[6] In the menu cost model, two parameters, μ^{Menu} and the fixed cost,κ, are calibrated to match the average spike at zero implied by DNWR model. Thus, we cannot uniquely pin down these parameters. Holding the average spike at zero fixed, Table B1 in the Appendix B.2.1 shows that menu cost model implies higher responsiveness of the share of workers with wage cuts than the spike at zero by varying μ^{Menu} from 0.3 to 1. As μ^{Menu} increases, the fixed cost, κ, decreases, so does inaction region. In the random menu cost model, the spike at zero is the proportion of the inaction region. The proportion is determined by μ^{Menu} and the size of inaction region is determined by κ.

in employment by 1 percentage point, there is a decrease in the fraction of workers with raises by 1 percentage point. Out of 1 percentage point, 0.7 percentage point of workers have no wage change, and the other 0.3 percentage point of workers have wage cuts, which is comparable to the first panel of Table 1.6. In the recession, nominal wage change distribution in the absence of wage rigidity shifts to the left as shown in the third panel of Figure 2.2. Under the downward nominal wage rigidity wage-setting constraint, 67 percent ($= \mu^{\text{DNWR}}$) of workers whose optimal wages are lower than the previous wages experience no wage changes, and the other 37 percent of worker cut their wages. In the recession, there are more workers whose optimal wages are lower than the previous wages, and this leads to an increase in the spike at zero larger than the increase in the fraction of workers with wage cuts.

2.4.5 Data moments

Table 2.3 shows empirical moments and moments from 5 alternative wage-setting schemes. To compare moments across the model, wage rigidity parameters are calibrated to have the similar level of the spike at zero, the frequency of no wage change. Sluggish adjustment in nominal wages results in real effects of monetary policy on employment, which can be measured by the standard deviation of employment growth rates.

Let's compare moments generated by the Calvo model to the long-term contracts model and menu costs-model, shown in the third, fourth, and the fifth panel of Table 2.3. The average spike at zero and the fraction of wage cuts and raises are comparable, and it is designed to be comparable by calibration. However, their implications on the standard deviation of employment growth rates are different.

The volatility of the employment from the Calvo model, the degree of monetary nonneutrality, is almost double of the long-term contracts or menu-costs model. The

Table 2.3: Data and model generated moments

	Wage growth rates	Employment growth rates	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
			Data		
Mean	4.102	0.020	15.484	21.318	63.198
SD	1.539	0.792	3.059	2.436	4.686
Skewness	1.032	-1.492			
			Perfectly flexible		
Mean	4.374	0.000	1.822	27.013	71.165
SD	2.068	0.476	3.220	9.710	9.790
Skewness	0.094	-0.000	-	-	-
			Calvo		
Mean	4.378	0.000	23.171	17.626	59.203
SD	1.529	1.051	1.703	6.663	6.905
Skewness	0.006	0.032	-	-	-
			Long-term contracts		
Mean	4.363	0.001	22.994	15.944	61.062
SD	1.403	0.476	0.603	6.128	6.151
Skewness	0.051	-0.003	-	-	-
			Menu costs		
Mean	4.374	0.000	23.085	17.332	59.583
SD	2.069	0.503	3.625	7.351	10.616
Skewness	0.073	-0.019	-	-	-
			DNWR		
Mean	4.382	0.000	23.025	10.530	66.445
SD	1.645	0.812	6.820	3.219	9.901
Skewness	0.320	-0.061	-	-	-

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). Wage growth rate is average of the median hourly wage growth rate for hourly paid workers from 1979-2017. The model generated moments are calculated from the simulated data under 5 different wage setting schemes.

standard deviation of employment growth rates from long-term contracts model is much smaller than the one from the Calvo model because allocative wages from perfectly flexible model determine employment, but not remitted wages.

Even if the fraction of wage adjustments from the menu-costs model is similar to the one from the Calvo model, the standard deviation of employment growth from menu costs model is smaller than the one from the Calvo model due to selection effects, noted by Caplin and Spulber (1987) and Golosov and Lucas (2007). For the menu costs model, only those workers whose current wages are far away from the optimal wages would want to change their wages after paying an additional fixed cost incurred to change in wages. Workers willing to pay a fixed cost to change their wages, they would want to change their wages by a large amount, which leads to a smaller effect on employment from aggregate uncertainty.

The spike at zero from the downward nominal wage rigidity model is similar to the other rigidity model. However, the fraction of wage cut is smaller and the fraction of raises is higher than other rigidity model as a result of the downward nominal wage rigidity restriction. The standard deviation from the downward nominal wage rigidity model is in between that the once from the Calvo and menu costs model. Compared to the Calvo model, the standard deviation of the downward nominal wage rigidity model is lower because downward nominal wage rigidity has restrictions only to lower wages but not to raise. However, the downward nominal wage rigidity model shows many small wage changes below zero, which makes the standard deviation higher than the menu cost model. As wage adjustment is asymmetric in the downward nominal wage rigidity model, it has an asymmetric implication on employment. Although the downward nominal wage rigidity model does not explain the entire left skewness of employment growth rate, only the downward nominal wage rigidity model can explain left skewness of employment growth, consistent with Dupraz, Nakamura, and Steinsson (2017).

2.5 Conclusion

In the second chapter of my dissertation, I examine which models with wage-setting schemes are able to match these stylized facts documented in chapter 1. I build heterogeneous agent models with 5 alternative wage-setting schemes widely discussed in the literature - perfectly flexible, Calvo, long-term contracts, menu costs, and downward nominal wage rigidity. The models feature not only idiosyncratic shocks but also aggregate shocks. Using numerical methods, I show the year-over-year wage change distributions of individual workers implied by each model and study cyclical properties of them.

Among 5 widely used wage-setting schemes, I conclude the model with downward nominal wage rigidity has the most consistent implications with empirical findings on cyclical properties nominal wage change distributions. This can be suggestive evidence of allocative consequences of downward nominal wage rigidity for employment.

The model with downward nominal wage rigidity predicts a distribution of annual employment growth that is skewed to the left, which is consistent with data, whereas the standard model predicts a symmetric distribution. This has important implications for monetary policy since there is a potential welfare gain in pursuing high inflation targets to relax the downward nominal wage rigidity constraint.

Chapter 3

The impact of e-commerce on urban prices and welfare

with Misaki Matsumura and David Wenstein

3.1 Introduction

How has e-commerce affected prices and welfare? One of the challenges in answering this question is that researchers typically only have short time series that do not allow them to compare pricing dynamics before and after the advent of e-commerce. Thus, while we can observe how the pricing dynamics of goods sold intensively online differs from those not sold online, it is difficult to assess whether any differences arise due to the advent of e-commerce or because of inherent differences in the pricing behavior of the goods themselves. This issue is particularly relevant because the types of products sold intensively online—books, clothing, electronics, and hardware—are also the types of goods that used to be sold intensively through catalogs. Thus, evidence about different pricing dynamics for these types of products is not necessarily evidence that e-commerce *caused* these pricing dynamics.

In order to resolve these issues, this chapter makes use of a unique Japanese data set covering price quotes for the set of goods that make up the Japanese consumer price index over the period 1991 to 2016 to examine the impact of the internet on Japanese prices and welfare. We merge these data with Japanese government survey data documenting the share of consumption expenditures occurring through each retail channel—catalog, e-commerce, and physical store—for each of these product categories. The long time series enables us to control for important pre-trends in the pricing dynamics of the types of goods available from online merchants. Second, we are also able to correct for an important endogeneity bias arising from the fact that e-commerce firms tend to enter sectors where they anticipate high markups by using historical catalog sales as an instrument for e-commerce sales.

Following pioneering work by Goolsbee and Klenow (2018), we also find that goods sold intensively online have significantly lower rates of price increase than goods not sold much online. However, we differ in that we also show that this pattern was also true before e-commerce firms entered the Japanese market. Moreover, we show that while e-commerce appears to have increased the relative difference in goods price inflation between the two sets of goods, an important reason for the increased differential is that the rate of price increase of goods not sold on e-commerce platforms rose. This may reflect a mechanism in which e-commerce eliminated low-cost physical stores, which reduced the amount of competition faced by stores that do not compete directly with online merchants.

Second, we document that e-commerce had important impacts on rates of intercity price differentials. Following Cavallo (2018), we argue that e-commerce is a technology that promotes uniform pricing across locations. As such, we should expect to see the rate of intercity price arbitrage rise for goods sold intensively online but not for goods sold principally in physical stores. This is exactly what we observe in the data. While we find that prior to e-commerce intercity price differentials dissipated a similar rates for the sets of goods that would eventually be sold online compared to goods not available online, after the advent of the internet, we find that intercity price differentials dissipated rapidly for goods available online but not for goods sold mostly in physical stores.

Based on our estimates of how e-commerce differentially affected the ability of merchants to price discriminate across cities, we compute the impact of e-commerce on Japanese welfare using the model developed in Jensen (2007). We estimate the welfare gains due to e-commerce to be 2.9 percent of consumption expenditure in 2014. While price arbitrage necessarily produces aggregate welfare gains in this setup, the model also predicts that consumers in low-priced cities lose while consumers in high-priced cities gain. This result is consistent with DellaVigna and Gentzkow (2017) who argue that uniform regional pricing by chain stores is likely to benefit high-income locations, where demand is likely to be more inelastic and hurt poorer consumers who benefit from the lower prices associated with price discrimination. In Japan, we find that residents of high-income cities benefited substantially from e-commerce, but we find negative effects for people in low-income cities. Thus, our results support the possibility that the digital divide is an important factor in understanding the costs and benefits of new technology.

The Jensen (2007) model has a number of potential shortcomings that we address in the final section of the paper. First, it is a partial equilibrium model that does not take into account how e-commerce might have affected wages and other components of marginal cost. Second, the approach is suitable for modeling the impact of e-commerce on relative prices, but it does not provide a framework that enables us to understand the impact of e-commerce on welfare that might arise because it enables consumers to access new varieties of products. As Brynjolfsson et al. (2003) and Einav et al. (2017) have argued, these variety channels are likely to be quite important. In order to address these concerns, we also adopt the approach developed in Arkolakis et al. (2012) to compute the general equilibrium gains due to varieties that would have occurred if e-commerce acted like a trade technology that reduced the cost of purchasing products at a distance and thereby allowed consumers purchase more varieties.

Our results for this exercise suggest that the general equilibrium gains that arise from the main class of new trade theory models (e.g., Melitz (2003) or Eaton and Kortum (2002)) are substantially smaller than those estimated using the Jensen (2007) approach: about a 0.6 percent welfare gain. These smaller gains are surprising given that calibrated new-trade models are hard-wired to produce welfare gains as long as e-commerce shares are non-zero. Interestingly, despite the lower aggregate gain, we also find evidence of the digital divide in this setup as well. Since e-commerce expenditure shares are highly correlated in Japan with college education, we estimate that the gains due to e-commerce in a new-trade theory setup are four times higher in cities with highly educated populations like Tokyo than in cities with low shares of college-educated people like Akita.

3.1.1 Related Literature

Our results are related to a number of papers related to how information technology has affected pricing and welfare. A large literature has demonstrated that information technology serves to reduce price dispersion and promote trade. Freund and Weinhold (2004) show that countries with more web hosts export more to each other. Jensen (2007), Aker (2010), and Allen (2014) examine the impact of the introduction of mobile phones on fish or agricultural markets in India, Niger, and the Philippines, and Steinwender (2018) examines the impact of the transatlantic telegraph cables on 19th century textile prices and exports. Our work is complementary to these papers in that we also show that e-retail serves to reduce price dispersion. However, our work differs in focus and scope—our study examines the role played by e-commerce in an advanced, modern economy on the prices of hundreds of goods in physical retailers. The paper also relates to the literature on internet pricing. In particular, Cavallo (2017) shows that online prices and prices in physical stores are quite similar. This fact helps motivate our assumption that local retailers with high prices should face stiff competition from online retailers.

Our paper is also related to studies of the impact of e-commerce on welfare. Many of these studies have focused on the gains from variety that arise as consumers can purchase products that are not available in local stores. For example, Brynjolfsson et al. (2003) compute the variety gains from internet book sales; Fan et al. (2018) examine the relative variety gains in large and small Chinese cities associated with internet usage; and Einav et al. (2017) estimate the gains from e-retail due to shopping convenience and and new varieties. An important difference between these studies and ours is that we make use of household survey data to measure e-commerce sales shares and control for pre-trends and historical catalog sales.

Our paper also relates to studies of how the internet affects local markets. Goldmanis et al. (2010) examine regional patterns in online purchase behavior change the market structure in bookstores, travel agencies and car dealers. Goyal (2010) finds that the introduction of internet kiosks raised soy prices in rural India. Couture et al. (2018) conduct a randomized control trial in eight rural Chinese counties and find little effect of the introduction of e-commerce on the local economy. Brown and Goolsbee (2002) show that the creation of online insurance sales systems reduced the variance of insurance pricing. Our work differs from these studies in terms of scope (the large number of different sectors considered), the link to physical retail prices across an entire economy, and identification strategy (the ability to examine differential rates of price convergence before and after the advent of e-commerce).

Finally, our paper is also related to the large literature on PPP convergence regressions. Parsley and Wei (1996) were the first to document that differences in convergence coefficients across cities was linked to trade costs, an insight that we build upon in this chapter. We estimate that intercity convergence rates for Japan pre-Rakuten are higher than those obtained in Parsley and Wei (1996) and Cecchetti et al. (2002). These studies found no price convergence across U.S. cities once one controlled for city fixed effects. In contrast, we find that prior to the advent of e-commerce, the half-lives for price differentials across Japanese cities were only 4.5 years. Our ability to better detect intercity price convergence probably arises from the fact that Japanese CPI data is based on the sampling of identical or extremely similar goods across cities, whereas U.S. price data is based on similar but non-identical sets of goods across cities. Moreover, we find that after the entry of e-commerce firms the half lives of goods sold intensively online collapsed to just a few months whereas goods not sold much online experienced no similar change. Our approach also builds off Bergin, Glick, and Wu (2017), who employ a similar triple difference strategy to show that rates of price convergence across European countries increased after joining the euro area.

The remainder of the paper is organized as follows. Section 3.2 introduces the the estimation strategy and provides the theory for the welfare calculation. Section 1.3 presents the data and provides some stylized facts about e-commerce suitability. Section 3.4.1 presents our results on national prices. We present our main estimates for the impact of Rakuten on price convergence and welfare in Sections 3.4.2, 3.4.2.1, and 3.4.2.2. Section 3.4.3 presents our calibration of the new trade theory models, and Section 2.5 concludes.

3.2 Theory

In Section 3.2.1, we model the impact that e-commerce has had on interregional price differentials and show how the decline in these differentials raises welfare in Section 3.2.2. Estimating the impact of e-commerce on average prices and in New Trade Theory is very straightforward following Arkolakis et al. (2012), so we will skip the theoretical discussion of how to do this and just deal with the estimation issues in Sections 3.4.1 and 3.4.3.

3.2.1 Estimating the Impact of the E-Retail on Price Arbitrage

We begin by defining some notation. Let $p_{ict} \equiv \ln P_{ict}$ be the log price of item i in city c in time t. Define the Δ^k operator as $\Delta^k p_{ict} \equiv p_{ict} - p_{ic,t-k}$; thus, if we set $k = 1$, we can examine annual changes, but we can also examine longer differences by setting k equal to a whole number larger than one. Let $x_i \in [0, 1]$ be the "e-commerce sales intensity" of a good, where zero indicates it is not suitable for e-commerce and one indicates that it is the most suitable good for e-retail. Let D_t be an indicator variable that is one if e-commerce are positive in period t and zero otherwise. We assume that the change in the price of any item in a city c can be written as a standard purchasing price parity specification in which we introduce a modification that allows the rate of price converge for goods available online to change, i.e.,

$$\Delta^k p_{ict} = \alpha_{it} + \beta_{ct} + (\gamma + \delta_1 x_i + \delta_2 D_t x_i)\, p_{ic,t-k} + \epsilon_{ict}, \tag{3.1}$$

where α_{it} is a item-time fixed effect; β_{ct} is a city-time fixed effect; γ is a parameter that captures the rate of intercity price convergence for goods not available online; δ_1 is a parameter that captures the rate of price convergence for goods available online prior to the entry of e-commerce firms; δ_2 captures the increase in rate of price convergence for online goods after the entry of e-commerce firms; and ϵ_{ict} is an iid error term. We think of this error as price shocks arising from period t local supply-and-demand conditions for an item in a city that are not shared by all items in the city and are uncorrelated with past prices.

In this specification, a critical parameter is the rate of convergence given by $(\gamma + \delta_1 x_i + \delta_2 D_t x_i)$, which we expect to be between -1 and 0. A value of -1 means that equation (3.1) collapses to $p_{ict} = \alpha_{it} + \beta_{ct} + \epsilon_{ict}$, and therefore the price of any item can be decomposed into its national price (α_{it}), a common local market premium (β_{ct}), and an iid error term that is not persistent. In this case, any idiosyncratic price shock to a good in a city (ϵ_{ict}) has no impact on prices in the next period. Hence, price convergence occurs in one period, and prices always equal their conditional mean of ($\alpha_{it} + \beta_{ct}$) plus a random iid shock. At the other extreme, we have the case of where $(\gamma + \delta_1 x_i + \delta_2 D_t x_i) = 0$, which implies that the price of that good i in city c follows a random walk with a drift term given by $(\alpha_{it} + \beta_{ct})$. In intermediate cases where $(\gamma + \delta_1 x_i + \delta_2 D_t x_i) \in (-1, 0)$, price differences across cities can persist for more than k years.

In our setup, we can write the approximate half-life[1] of any price deviation from the steady-state price (measured in intervals of length k) as[2]

$$H_t \equiv \frac{\ln(0.5)}{\ln\left(1 + \hat{\gamma} + \hat{\delta}_1 x_i + \hat{\delta}_2 D_t x_i\right)}. \tag{3.2}$$

As one can see from this formula, the change in the rate of convergence depends on all of the estimated convergence parameters, therefore there is not a simple mapping from changes in δ_t into rates of convergence. Thus, the impact of e-commerce on the rate of convergence for any good i can be written as:

$$\Delta H_t \equiv \frac{\ln(0.5)}{\ln\left(1 + \hat{\gamma} + (\hat{\delta}_1 + \hat{\delta}_2) x_i\right)} - \frac{\ln(0.5)}{\ln\left(1 + \hat{\gamma} + \hat{\delta}_1 x_i\right)}. \tag{3.3}$$

3.2.2 Welfare

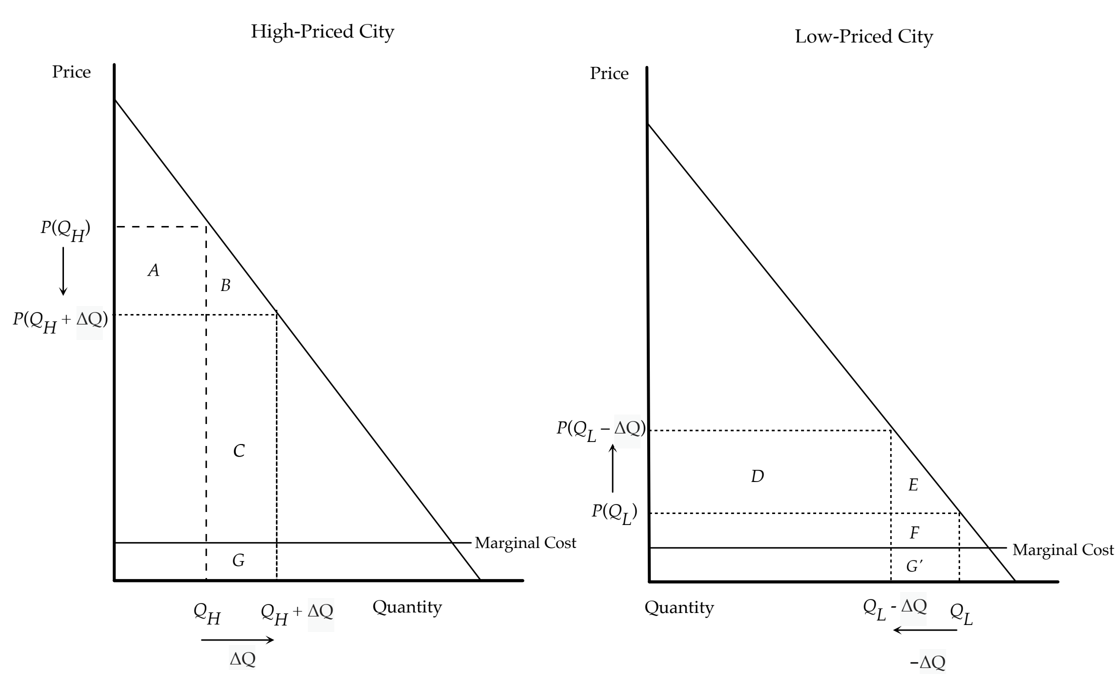
We can use these estimates to inform us about the welfare gains from e-commerce by using the framework developed in Jensen (2007). Jensen considered a technological

[1] As Goldberg and Verboven (2005) note, this formula is only correct for AR1 processes.

[2] The steady state price is given by the price at which setting $\Delta p_{ict} = 0$. This price equals $p_{ic} = -(\alpha_{it} + \beta_{ct}) / (\gamma + \delta_1 x_i + \delta_2 D_t x_i)$.

change that enabled arbitrage between a high-priced region (H) and a low-priced region (L). This framework can easily be applied to the e-commerce context since e-commerce firms provide a platform that enables consumers in any city to purchase goods from a large number of retailers spread across Japan. If e-commerce enables local retailers in the low-priced region to make ΔQ units of sales to the high-priced region, we should expect the price in region H to fall and the price in L to rise as shown in Figure 3.1. Consumers in H will gain $(A + B)$, and sellers will gain $(C - A)$, yielding a net gain of $(B + C)$. Similarly, in region L, consumers will *lose* $(D + E)$ and sellers will gain $(D - F)$, yielding a net loss of $(E + F)$. Overall, the welfare gain is $(B + C) - (E + F)$, which will necessarily be positive in the case of linear demands with equal slopes as long as the price in H is at least as large as the price in the region L after arbitrage (i.e., $P(Q_H + \Delta Q) \geq P(Q_L - \Delta Q)$). One can also see this condition holds in the figure because both trapezoids $(B + C)$ and $(E + F)$ have identical bases and differ only in the heights of their parallel sides.

Figure 3.1: Welfare Gains from Arbitrage in the Jensen Model



Jensen (2007) considered a case in which the marginal cost of supplying a market is zero, which enabled him to compute the lengths of the parallel sides of the quasi-trapezoids by just using the prices. When thinking about production more generally, however, marginal costs are likely to be positive, so technically we should subtract marginal costs from prices when computing the lengths of the parallel sides of the quasi-trapezoids. However, as one can see from Figure3.1, if we assume constant and equal marginal costs of production, then $G = G'$, and we can still compute the welfare gain as $(B + C + G) - \left(E + F + G'\right) = (B + C) - (E + F)$.[3]

In order to compute the welfare gain, we need to compute the price change associated with the arbitrage opportunity associated with e-commerce. We begin by writing the change in welfare due to the price change of good i in city c over a year as

$$\Delta W_{ict} = \frac{1}{2}\left(2P_{ic,t-1} + \Delta P_{ict}\right)\Delta Q_{ic,t} - m\Delta Q_{ic,t}, \tag{3.4}$$

where m is the marginal cost of producing the good. Without loss of generality we can decompose prices and quantities into two components: a national component that captures national movements in the price of good i ($\Delta P^N_{ict} \equiv \left(P_{ic,t-1}/P_{i,t-1}\right)\Delta P_{it}$), a city-specific component that captures relative movements in prices in that city (ΔP^R_{ict}):

$$\Delta P_{ict} = \Delta P^N_{ict} + \Delta P^R_{ict}. \tag{3.5}$$

Let the total quantity demanded of item i in time t be denoted by $Q_{it} \equiv \sum_c Q_{ict}$. We can now decompose quantity movements (ΔQ_{ict}) into a national component ($\Delta Q^N_{ict} \equiv \left(Q_{ic,t-1}/Q_{i,t-1}\right)\Delta Q_{it}$), which tells us how consumption in the city would have moved if it followed the national trend, and a city-specific ($\Delta Q^R_{ict} \equiv \Delta Q_{ict} - \Delta Q^N_{ict}$) component. This lets us rewrite equation (3.4) as

[3]The assumption of equal marginal costs is probably not extreme for Japan given the small physical size of the country (most major cities are within a few hours drive of Tokyo), which means that transport costs are unlikely to produce large price differences across cities.

$$\Delta W_{ict} = \frac{1}{2}\left(2P_{ic,t-1} + \Delta P^N_{ict} + \Delta P^R_{ict}\right)\left(\Delta Q^N_{ict} + \Delta Q^R_{ict}\right) - m\Delta Q_{ic,t-1} \tag{3.6}$$

Rearranging terms produces

$$\begin{aligned}
\Delta W_{ict} &= \frac{\left(2P_{ic,t-1} + \Delta P^N_{ict} + \Delta P^R_{ict}\right)\Delta Q^N_{ict}}{2} + \frac{\left(2P_{ic,t-1} + \Delta P^N_{ict} + \Delta P^R_{ict}\right)\Delta Q^R_{ict}}{2} - m\Delta Q_{ict} \\
&= \frac{\left(2P_{ic,t-1} + \Delta P^N_{ict}\right)\Delta Q^N_{ict}}{2} + \frac{\left(2P_{ic,t-1} + \Delta P^R_{ict}\right)\Delta Q^R_{ict}}{2} \\
&\quad + \frac{\Delta P^R_{ict}\Delta Q^N_{ict}}{2} + \frac{\Delta P^N_{ict}\Delta Q^R_{ict}}{2} - m\Delta Q_{ict} \\
&= \left(\Delta W^N_{ict} - m\Delta Q^N_{ict}\right) + \left(\Delta W^R_{ict} - m\Delta Q^R_{ict}\right) + \frac{\Delta P^R_{ict}\Delta Q^N_{ict}}{2} + \frac{\Delta P^N_{ict}\Delta Q^R_{ict}}{2}
\end{aligned} \tag{3.7}$$

In other words, the change in welfare in a city can be decomposed into a term that depends on how the national change in prices and quantities affected welfare, a second term that depends on how price movements in that city relative to the national average affected welfare, and two second-order terms that capture the fact that a relative price decline matters more for welfare if it occurs for a good that is on average growing in consumption and one that captures the fact that a national drop in prices raises welfare more if local demand is also rising. Our focus will be on the aggregate gains arising from the second term ($\sum_c \Delta W^R_{ict}$=$\sum_c \Delta W^R_{ict} - m\sum_c \Delta Q^R_{ict}$), gains due to arbitrage, which captures the first-order impact of relative price movements on welfare.

Defining the terms this way lets us write $\Delta Q^R_{ict}/Q_{ic,t-1} = \Delta Q_{ict}/Q_{ic,t-1} - \Delta Q_{it}/Q_{i,t-1}$, which is more convenient to write as a log approximation given by $\Delta q^R_{ict} \equiv \Delta q_{ict} - \Delta q_{it}$. If we assume that there is no regional variation in demand elasticities ($\eta_c = \eta\ \forall c$), we then have

$$\Delta q^R_{ict} \equiv \Delta q_{ict} - \Delta q_{it} = -\eta\left(\Delta p_{ict} - \Delta p_{it}\right). \tag{3.8}$$

If we multiply this expression by $Q_{ic,t-1}$ and sum across all cities we obtain an expression for the aggregate change in quantity due to relative price movements across the cities:

$$\sum_c \Delta Q_{ict}^R \equiv \sum_c Q_{ic,t-1} \Delta q_{ict}^R = -\eta \left(\sum_c Q_{ic,t-1} \Delta p_{ict} - \sum_c Q_{ic,t-1} \Delta p_{it} \right) = 0, \tag{3.9}$$

where the last equality follows from our assumption (which is the same as that of Jensen (2007)) that relative price changes arising from new arbitrage opportunities do not affect aggregate demand for the good. This expression lets us solve for the expression for national prices:

$$\Delta p_{it} = \frac{\sum_c Q_{ic,t-1} \Delta p_{ict}}{\sum_c Q_{ic,t-1}} \tag{3.10}$$

In other words, the log-change in national price index is just a quantity-weighted average of the log price change in each city. We now can conduct our counterfactual welfare analysis. Based on equation (3.1), we can write the best estimate of price change:

$$\widehat{\Delta p}_{ict} = \hat{\alpha}_{it} + \hat{\beta}_{ct} + \left(\hat{\gamma} + \hat{\delta}_1 x_i + \hat{\delta}_2 D_t x_i \right) p_{ic,t-1}. \tag{3.11}$$

Therefore, the aggregate price change for any good is

$$\widehat{\Delta p}_{it} = \hat{\alpha}_{it} + \sum_c \frac{Q_{ic,t-1} \hat{\beta}_{ct}}{\sum_c Q_{ic,t-1}} + \left(\hat{\gamma} + \hat{\delta}_1 x_i + \hat{\delta}_2 D_t x_i \right) \sum_c \frac{Q_{ic,t-1} p_{ic,t-1}}{\sum_c Q_{ic,t-1}}, \tag{3.12}$$

The evolution of prices in each city relative to the national price increase can be expressed as the difference between equations (3.11) and (3.12):

$$\widehat{\Delta p}_{ict}^R(D_t) = \left[\hat{\beta}_{ct} - \sum_c \frac{Q_{ic,t-1} \hat{\beta}_{ct}}{\sum_c Q_{ic,t-1}} \right] + \left(\hat{\gamma} + \hat{\delta}_1 x_i + \hat{\delta}_2 D_t x_i \right) \left[p_{ic,t-1} - \sum_c \frac{Q_{ic,t-1} p_{ic,t-1}}{\sum_c Q_{ic,t-1}} \right]. \tag{3.13}$$

We now can write the impact of a price change on welfare in a city as

$$\widehat{\Delta W}_{ict}^R(D_t) = \frac{-\eta}{2} \left[2P_{ic,t-1} + P_{ic,t-1} \widehat{\Delta p}_{ict}^R(D_t) \right] Q_{ic,t-1} \widehat{\Delta p}_{ict}^R(D_t) \tag{3.14}$$

The welfare gains due to the enhanced price arbitrage from e-commerce can be written as

$$\begin{aligned}
\widehat{\Delta W}^{E}_{ict} &\equiv \widehat{W}^{R}_{ict}(D_t=1)-\widehat{W}^{R}_{ict}(D_t=0)\\
&= [\widehat{\Delta W}^{R}_{ict}(D_t=1)-\widehat{\Delta W}^{R}_{ict}(D_t=0)]+[\widehat{W}^{R}_{ict}(D_t=1)-\widehat{W}^{R}_{ict}(D_t=0)]+\\
&= \textstyle\sum_{\tau=1997}^{t}[\widehat{\Delta W}^{R}_{ic\tau}(D_t=1)-\widehat{\Delta W}^{R}_{ic\tau}(D_t=0)]+\hat{W}^{R}_{ic1996}(D_{1996}=1)-\hat{W}^{R}_{ic1996}(D_{1996}=0)\\
&= \textstyle\sum_{\tau=1997}^{t}[\widehat{\Delta W}^{R}_{ic\tau}(D_t=1)-\widehat{\Delta W}^{R}_{ic\tau}(D_t=0)]
\end{aligned}$$

, where in the last line we assume that e-commerce provided no gains to consumers before the entry of Rakuten, i.e., $\left[\hat{W}^{R}_{ic1996}(D_{1996}=1)-\hat{W}^{R}_{ic1996}(D_{1996}=0)\right]=0$. The estimated welfare gains due to arbitrage from e-commerce can therefore be written as

$$\widehat{\Delta W}^{E}_{t}=\Sigma_i\Sigma_c\widehat{\Delta W}^{E}_{ict} \tag{3.15}$$

3.3 Data

A major advantage of using Japanese data is that one can obtain measures of consumer expenditures by product and type of sales merchant. The National Survey of Family Income and Expenditures (NSFIE) is a representative survey of households with two or more members that records expenditures by product from each major retail outlet store type: small retail, supermarket, convenience, department, club, discount, catalog, internet, and "other". Starting in 2004, the NSFIE also began a quinquennial recording the expenditure share of each product from online merchants. One of the problems with the NSFIE data is that it tends to under-report aggregate internet sales due to questions about which retail outlet they used. Fortunately, the Ministry of Economy Trade and Industry (METI) reports very reliable aggregate estimates of sales by e-commerce and other retailers by surveying sales to consumers by retail merchants. We therefore scale the NSFIE data by the ratio of aggregate sales in the METI data relative to the NSFIE data in order to obtain the same value for aggregate e-commerce sales in the two datasets.

In order to make sure that sampling problems are not driving our results, we also conduct a robustness check for all of our main results using data from Rakuten, the largest e-retail company in Japan, who provided us with 2010 internet sales data (aggregated across buyers and merchants) for each of approximately 40,000 product categories or "genres." In that year, Rakuten had a 30 percent market share of all Japanese e-commerce.[4] We then matched these genres to the expenditure categories in the 2010 Japanese Family Income and Expenditure Survey (FIES) that are used in to construct the Japanese consumer price index. This generated a matched sample in which we have 312 tradable goods in a typical year, which we use in our main specifications.

We construct e-commerce intensity of an expenditure category by comparing the average household total expenditure on that category with the average household's online expenditure on it. We measure total expenditure share e_i on category i by using national average expenditures per household in 2009 taken from the Family Income and Expenditure Survey ("FIES", which forms the basis of the Japanese CPI and is distinct from the NSFIE). We denote online expenditure share in category i from NSFIE by s_i. We then define e-commerce intensity x_i of category i by taking the ratio of the online to total expenditures share, normalized by the maximum value of this ratio:

$$x_i = \frac{s_i}{e_i} / \max_j \left(\frac{s_j}{e_j} \right).$$

In order to see how e-commerce intensity varies across products, we aggregated the FIES codes into some broader categories in Table 3.1 so that we could display the data in a compact form. As most of services are not available online, we will focus on e-commerce's impact on goods prices for all of our main results. The rows are ordered by a category's share of Japanese expenditures on goods. The first column of Table 3.1 reports the percentage of expenditures in category ℓ among goods in 2009 as reported in the FIES

[4]Rakuten, Inc. (2010) *Annual Report.*

($E_\ell \equiv \sum_{i \in \Omega^\ell} e_i / \sum_j e_j \times 100$), where Ω^ℓ is the set of items in some more aggregated category ℓ. In the second column, we report the percentage of online expenditure in 2009 that corresponds to that category (($S_\ell \equiv \sum_{i \in \Omega^\ell} s_i / \sum_j s_j \times 100$), where s_i is online expenditure from NSFIE). The third column reports the "e-commerce intensity" in 2009, which we define to be the ratio of the two previous columns divided it by the maximum value of S_ℓ / E_ℓ (i.e., $x_i \equiv S_i/E_i / \left[\max_j \{S_j/E_j\}\right]$). Thus, our measure of e-commerce intensity takes on a value of zero if there are no transactions involving an expenditure category and a value of 1 if the online expenditure relative to those in the economy is the highest among all categories of goods. Expressing e-commerce intensity this way makes our e-commerce intensity (x_i) invariant to the size of sector i.

Table 3.1: E-Commerce intensity of consumer expenditure on goods

Category	Share of Total Expenditure 2009	Share of E-Commerce Expenditure 2009	E-Commerce Intensity 2004	E-Commerce Intensity 2009	E-Commerce Intensity 2014	Catalog Intensity 1999	E-Commerce Intensity Rakuten 2010
Fruits and vegetables	10.24	1.76	0.01	0.03	0.05	0.06	0.03
Household consumables	10.19	18.00	0.15	0.36	0.28	0.34	0.58
Clothing	9.61	13.45	0.11	0.28	0.22	0.41	0.42
Store-bought cooked food	7.62	1.10	0.03	0.03	0.04	0.04	0.03
Cereal	6.21	1.54	0.02	0.05	0.05	0.06	0.07
Fish and shellfish	6.13	1.40	0.02	0.05	0.05	0.05	0.04
Cakes and candies	5.72	1.62	0.03	0.06	0.04	0.05	0.08
Meat	5.55	0.73	0.01	0.03	0.04	0.02	0.03
Recreational goods	4.65	12.71	0.30	0.55	0.47	0.22	0.93
Household applicances	4.05	6.32	0.21	0.31	0.36	0.17	0.35
Electronics	3.88	19.32	1.00	1.00	1.00	0.41	0.53
Alcoholic beverages	3.36	1.32	0.05	0.08	0.10	0.06	0.26
Medicine and nutritional supplements	3.35	4.85	0.23	0.29	0.31	1.00	0.23
Non-alcoholic beverages	3.17	2.20	0.09	0.14	0.15	0.27	0.16
Oils, fats and seasonings	3.11	0.73	0.02	0.05	0.07	0.09	0.05
Newspapers and magazines	2.96	0.00	0.00	0.00	0.00	0.00	0.00
Dairy products and eggs	2.81	0.29	0.01	0.02	0.04	0.02	0.01
Transportation equiment	2.14	3.01	0.23	0.28	0.18	0.40	0.58
Domestic utensils	2.06	4.04	0.14	0.39	0.49	0.41	0.53
Furniture and furnishings	1.78	3.45	0.33	0.39	0.51	0.56	1.00
Footwear	1.40	2.13	0.14	0.30	0.28	0.33	0.92
Total/Mean	100.00	100.00	0.15	0.22	0.23	0.24	0.32

Data source: FIES, NSFIE, Rakuten, and authors' calculation. Notes: The first column is from FIES. Column 2- 6 are from NSFIE and the last column is from Rakuten. Notes: Shares are expressed as percentages. This table shows the share of consumption expenditure, e-commerce expenditure, and e-commerce sales intensity, and catalog intensity for goods. E-Commerce intensity is calculated as $x_i = \frac{s_i}{e_i}/max_j(\frac{s_i}{e_i})$.

Table 3.1 makes clear some basic stylized facts of our data. First, within goods categories we see that there are no zeros except newspapers and magazines in the table indicating that at this level of aggregation all categories of goods were available online in 2009. Second, there is enormous variation in the e-commerce intensity. Some of this reflects the fact that highly perishable, non-standardized items (e.g. fresh foods), restricted/time-sensitive items (e.g., medicine and physical newspapers), and high weight-to-value items (non-perishable groceries) are not sold much online. At the other end of the spectrum, we see that more standardized goods—e.g., electronics, books, clothing, footwear, and furniture and furnishings—are sold very intensively online. Interestingly, we see that domestic utensils, household consumables (which includes non-durable household supplies like paper products and cleaning agents), and recreational goods (which includes items like sports equipment and gardening supplies) are sold very intensively online as well.

As one can also see from the table, there is a lot of similarity between goods that are sold intensively online and goods that were sold intensively by catalogs in 1999. In that year, e-commerce firms in Japan were still in their infancy: Amazon had not yet entered the Japanese market and Rakuten only had 5.5 million dollars worth of sales on its platform (Olsen 2012). Thus, we can be fairly confident that Japanese catalog sales were probably not much influenced by e-commerce sales. Nevertheless, it is interesting to note that goods sold intensively online tend have characteristics that are similar to those goods historically available in catalogs—i.e., goods that are non-perishable, low weight-to-value, standardized, and storable.

Although e-commerce was small in Japan in 1999, the situation changed radically over the next two years. By April of 2000, when Rakuten announced its initial public offering and a year before the entry of Amazon into Japan, Rakuten had grown to be a platform in which consumers had access to goods available from 2,300 merchants, and the Rakuten website was getting 95 million hits per month—almost one hit for every man, woman, and child in Japan.[5] The following year sales on the Rakuten platform exceeded ¥52 billion (about $430 million). Thus, within five years, Japanese consumers in any city went from only being able to buy locally or from catalogs to being able to purchase goods from thousands of merchants located across Japan. Rakuten's growth was part of a broader e-commerce boom in Japan. By 2017, e-commerce firms accounted for 5.8 percent of Japanese retail sales or about ¥16.5 trillion (about $149 billion). Despite the explosive growth, as one can see in the last column of Table 3.1, the set of goods selling well on the Rakuten platform remained remarkably similar to those that sold well in catalogs. Moreover, the Rakuten sales intensities are highly correlated ($\rho = 0.57$) with the e-commerce sales intensities we obtained from the NSFIE data, which suggests that these datasets are in broad agreement as to what goods sell well online.

In addition to the retail sales data that we have been discussing, we also make use of the fact that the Japan Statistical Bureau (JSB), which produces the Japanese CPI, provides detailed information on representative prices of the products in the FIES categories. These prices are sampled in all cities that are either a prefectural government or have population of 150,000 or more, which gives us the ability to not only tracking product prices across time but also across space. This information typically identifies the brand of an item or a detailed description (e.g., "Big-eyed tuna, sliced (for sashimi), lean, 100g"). While the data is not sufficiently detailed to always pin down the exact barcode, the data leaves limited scope for unobserved quality differences to affect intercity price differentials. For example, Imai and Watanabe (2015) find that it is sufficiently detailed to rule out approximately 85 percent of all bar codes in a CPI product category. Moreover, since the objective of the JSB sampling is to make meaningful intercity price comparisons, there is a tendency to select the same

[5] Phred Dvorak, "Japan's Highly Popular Rakuten Plans IPO Despite Shaky Market," *Wall Street Journal*, April 18, 2000.

products by, for example always picking the largest selling item within a sampling frame if available. Thus, while US CPI data typically is based on different baskets of goods in different cities, the JSB's "purposive" sampling generate samples in which the same good or very similar goods are sampled in different cities. Therefore, it is reasonable to believe that intercity prices are informative about true price differences across locations.[6] One problem in the data is that we have periodic product substitutions that arise as goods are added to or dropped from the CPI sample. Fortunately, we have official quality-adjusted price quotes for Tokyo computed by the JSB[7], which we use to adjust the prices in other cities. This procedure is equivalent to assuming that the quality change associated with a product substitution in the CPI is identical across cities.

[6]In order to further clean the data, we drop all observations in which the item only appears in one city. We also trimmed 3 smallest and 3 largest price quotes within an item-year observation. Finally, we dropped the bottom and top 1% of log price changes.

[7]http://www.e-stat.go.jp/SG1/estat/List.do?bid=000001033703&cycode=0, accessed on April 5th, 2017.

Table 3.2: Summary Statistics for the Sample of Goods

	Mean	Standard Deviation	Min	p10	p50	p90	Max
Period: 1991 to 1996							
$\Delta^1 p_{ict}$	-0.004	0.099	-0.858	-0.101	0.000	0.090	0.953
$x_{i(t=2009)}$	0.057	0.073	0.000	0.004	0.022	0.157	0.456
$x_{i(t=2009)} \times p_{ic(t-1)}$	0.456	0.679	0.000	0.021	0.142	1.340	4.107
$xcat_{i(t=1999)}$	0.078	0.119	0.000	0.004	0.035	0.207	0.995
$xcat_{i(t=1999)} \times p_{ic(t-1)}$	0.592	1.022	0.000	0.019	0.219	1.662	9.725
Observations	74,732						
Period: 1996 to 2001							
$\Delta^1 p_{ict}$	-0.008	0.106	-1.124	-0.117	-0.001	0.084	1.165
$x_{i(t=2009)}$	0.053	0.070	0.000	0.004	0.022	0.151	0.456
$x_{i(t=2009)} \times p_{ic(t-1)}$	0.417	0.646	0.000	0.021	0.125	1.215	4.075
$xcat_{i(t=1999)}$	0.073	0.112	0.000	0.004	0.034	0.170	0.995
$xcat_{i(t=1999)} \times p_{ic(t-1)}$	0.541	0.934	0.000	0.022	0.212	1.376	9.826
Observations	109,486						
Period: 2001 to 2006							
$\Delta^1 p_{ict}$	-0.009	0.114	-1.798	-0.127	-0.003	0.104	1.679
$x_{i(t=2009)}$	0.052	0.074	0.000	0.004	0.022	0.146	1.000
$x_{i(t=2009)} \times p_{ic(t-1)}$	0.400	0.690	0.000	0.021	0.115	1.169	11.690
$xcat_{i(t=1999)}$	0.071	0.107	0.000	0.004	0.033	0.170	0.995
$xcat_{i(t=1999)} \times p_{ic(t-1)}$	0.508	0.873	0.000	0.020	0.202	1.243	9.703
Observations	163,473						
Period: 2006 to 2011							
$\Delta^1 p_{ict}$	-0.001	0.124	-1.695	-0.127	0.000	0.126	1.556
$x_{i(t=2009)}$	0.053	0.081	0.000	0.004	0.022	0.146	1.000
$x_{i(t=2009)} \times p_{ic(t-1)}$	0.411	0.751	0.000	0.020	0.114	1.197	10.684
$xcat_{i(t=1999)}$	0.070	0.104	0.000	0.004	0.032	0.162	0.995
$xcat_{i(t=1999)} \times p_{ic(t-1)}$	0.497	0.845	0.000	0.019	0.197	1.235	9.411
Observations	164,029						
Period: 2011 to 2016							
$\Delta^1 p_{ict}$	0.014	0.100	-1.276	-0.084	0.010	0.122	1.092
$x_{i(t=2009)}$	0.052	0.080	0.000	0.004	0.020	0.143	1.000
$x_{i(t=2009)} \times p_{ic(t-1)}$	0.398	0.725	0.000	0.020	0.109	1.175	10.300
$xcat_{i(t=1999)}$	0.068	0.104	0.000	0.004	0.029	0.162	0.995
$xcat_{i(t=1999)} \times p_{ic(t-1)}$	0.485	0.838	0.000	0.016	0.165	1.215	9.081
Observations	168,241						

Data source: RPS, NSFIE, and authors' calculation. Notes: This table shows summary statistics of of price changes, e-commerce intensity, and catalog intensity for five five-year-periods from 1991 to 2016. Prices are in natural log. $\Delta^1 p_{ict}$ is the one-year log difference in prices; $x_{i(t=2009)} = \frac{s_i/e_i}{\max_j(s_j/e_j)}$ shows e-commerce intensity in 2009 using NSFIE and $xcat_{i(t=1999)}$ indicates catalog sales intensity in 1999.

One obvious concern with these data is that they are not as good as barcode data. However, we can test how problematic they are by computing some simple sample statistics. Hottman, Redding, and Weinstein (2016) show that the correlation between price and quality in bar-code data is 0.9, so we should expect sampling problems to produce high levels of price dispersion in our sample. Thus, if there is substantial quality variation within the goods used in the Japanese CPI sample, we should expect to see a lot of intercity price dispersion for the same items. In order to check for this, we compute the price of each good in each city less the average price of that good across all cities and then taking the standard deviation of this difference. When we do this, we find that the standard deviation of intercity price differences for the same good in Japan is about 15 percent. By contrast, Broda and Weinstein (2008) find the standard deviation in intercity prices of bar-coded goods is 22 percent in the US and 19 percent for Canadian provinces. The fact that intercity price dispersion of goods in the Japanese CPI is lower than that for bar-coded goods in the US and Canada suggests that the JSB item definitions probably do not include goods that differ substantially in quality in different cities and therefore that quality variation across cities for the same product is unlikely to be a major problem in our data.

Table 3.2 reports the sample statistics for our data. As one can see from the table, we have more than 100,000 price quotes in each of our five-year periods since e-retail has become available in Japan in 1997. The first line of the table shows the average annual rate of inflation across the sample period. As one can see, on average goods prices fell before 2011, which reflects the deflation that can be observed in Japan over this time period. The second line reports information on the e-commerce intensity of the goods in our sample (x_i). The values of x_i across goods tell us about the relative importance of online sales. Here we see that goods in the the upper 90^{th} percentile of the distribution have an e-commerce sales intensity of 0.146 over the full sample period, which is more than six times higher than a good with the median intensity. Moreover, at the upper tail of the distribution, we observe goods with an e-commerce intensity that is more than 45 times higher than that of the median good. These summary statistics reflect the skewness in the distribution of e-retail sales intensity that we saw in Table 3.1. Some goods are sold very intensively online, but most goods are purchased predominantly in physical stores.

3.4 Estimation

In Section 3.4.2, we present plots to show that price convergence is a central tendency in the data and that the internet appears to have changed the rate of convergence for goods available online but not for other goods. This provides some *prima facie* evidence that our focus on relative intercity price movements of goods sold by e-retailers as opposed to absolute price declines of online goods is in line with the data. We next estimate the impact of e-retail on the rate of price convergence in Section 3.4.2.1. Finally, in Section 3.4.2.2, we present our estimates of the welfare gain from e-retail.

3.4.1 E-commerce and National Prices

Goolsbee and Klenow (2018) find that goods traded online have inflation rates that were about one percentage point lower than goods not available online. Here, we extend this work to show that these differential rates of price increase were present long before the entry of e-commerce firms, became more pronounced after the entry of e-commerce merchants, and arose in part because the rate of price increase of goods not available online rose.

In order to examine this in the data, we regress annual log price changes of goods (Δp_{ict}) on good (α_i) and city (β_c) fixed effects along with an indicator variable, D_t, that is one starting in 1997 (the year Rakuten opened) and zero before as well as the e-commerce intensity of the good interacted with this dummy ($x_i D_t$):

$$\Delta p_{ict} = \alpha_i + \beta_c + \phi D_t + \theta x_i D_t + \epsilon_{ict}, \tag{3.16}$$

where α_i is a parameter to capture any pre-trends in the data that might arise if goods available online exhibit have different price increase trends than goods not available online. The coefficient on D_t (ϕ) tells us whether there was any differential trend in price inflation for goods available online after the entrance of e-commerce firms and θ, the coefficient on the e-commerce intensity interaction term ($x_i D_t$) tells us about the differential rate of price change for goods traded online after the entry of e-commerce firms. We do this for two time periods (1992-2001) and (1992-2016) to see if there is any difference in the results we obtain by looking at the period immediately after the entry of e-commerce firms versus the full time period.

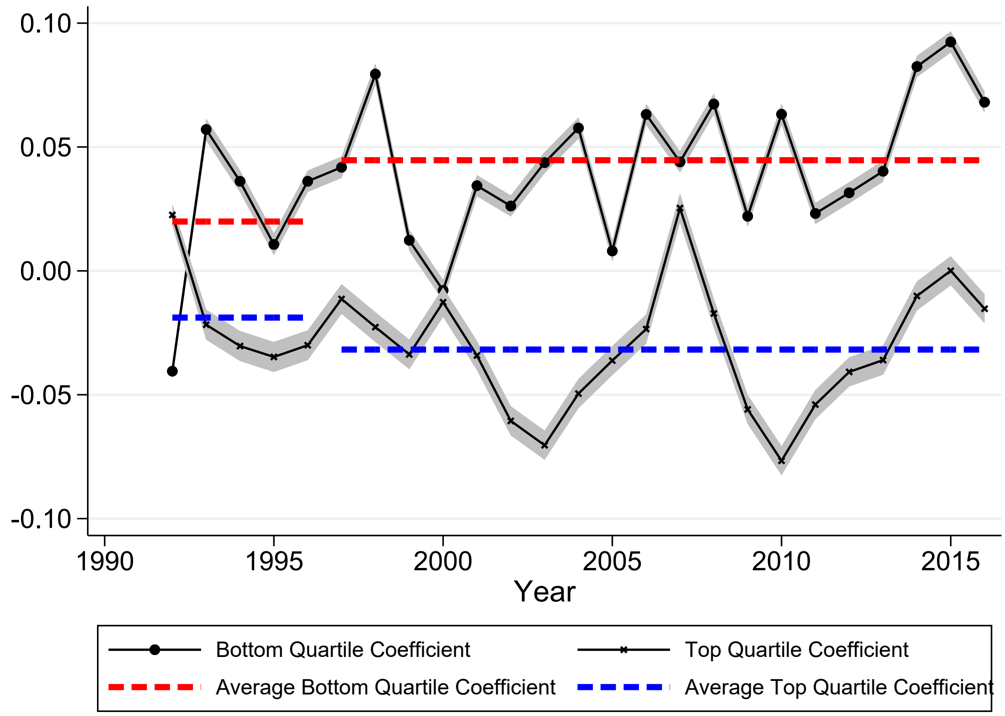
One of the advantages of our specification is that we can eliminate any good-specific pre-trends (α_i) that might confound specifications that compare growth rates of goods available online with those not sold online. In order to understand to understand whether controlling for these pre-trends is likely to be important, we split the sample into two groups by e-commerce sales intensity. The first sample of goods (X_B) consists of products that have an e-commerce sales intensity (x_i) in the bottom quartile, and second sample is composed of goods with an e-commerce sales intensity in the top quartile (X_T). We then computed the average rate of price increase for the two sets of goods by running the following regression separately for each sample:

$$\Delta p_{ict} = \theta_t + \epsilon_{ict}, \tag{3.17}$$

where the estimates of the time fixed effect θ_t in each sample tell us the average rate of price increase for the goods in each sample.

We plot these estimates and the 95-percent confidence bands in Figure 3.2. As the figure

Figure 3.2: Price Growth of of Goods With High and Low E-Commerce Intensity



Data source: RPS, NSFIE, and authors' calculation. Notes: This black line shows time fixed effect $\hat{\theta}_t$ from equation (3.17), which tells the average rate of price increase for the goods in two groups: products with bottom quartile e-commerce sales intensity (black line with dot) and products with top quartile e-commerce intensity (black line with symbol x). The red dashed line shows the average rate of price increase before and after the entry of Rakuten for goods with bottom quartile e-commerce sales intensity and the blue dashed line shows that for goods with top quartile e-commerce sales intensity.

makes clear, there are unmistakable pre-trends in the data. Before the entry of the Rakuten in 1997, the average rate of price increase for the types of goods that would ultimately be sold on e-commerce platforms was -1.9 percent per year, while the average annual rate of price increase for goods that not sold much on these platforms was 2.0 percent per year. Thus, there was a 3.9 percentage point gap between the relative inflation rates of goods would be sold intensively online relative to those would not be sold intensively online even in the early 1990s. These differences in inflation rates may reflect the fact that the production of standardized, non perishable goods, which tend to dominate e-commerce platforms, may benefit more from the cost reductions associated with modern manufacturing techniques.

It is also interesting to see what happened to this gap in inflation rates after the entry of e-commerce firms. While we do not see much change in pricing behavior in the first five years after the entry of Rakuten, by 2002, we see that the differences in the price growth rates between the two sets of goods widened significantly. The relative inflation rate for goods sold heavily online relative to goods not sold much online fell substantially. Goods in the top quartile of e-commerce sales intensity had an average rate of price growth from 1997 to 2016 of -3.2 percent per year: a 1.3 percent per year fall in the rate of price growth. By contrast, the rate of price growth for goods in the bottom quartile of e-commerce sales *rose* to 4.5 percent per year: an increase of 2.5 percent per year.[8]

[8]The cause of this price increase is not clear, but the higher prices charged by merchants for goods not sold intensively online may be related to the impact that e-commerce firms have had on large national retailers. DellaVigna and Gentzkow (2017) argue that chain stores typically offer uniform pricing across cities and this works to depress regional price dispersion. This implies that a negative shock to chain stores that sell standardized products might be associated with a rise in prices in stores selling products not available online. For example, suppose mass-merchandisers traditionally sell groceries and electronics. If e-commerce firms eliminate mass-merchandise stores, local grocery stores may find themselves with fewer physical competitors and more able to raise prices.

Table 3.3: Relative Price Changes

	(1) $\Delta^1 p_{ict}$	(2) $\Delta^1 p_{ict}$	(3) $\Delta^1 p_{ict}$	(4) $\Delta^1 p_{ict}$
D_t	-0.0001	0.0097***	-0.0001	0.0142***
	(0.0021)	(0.0022)	(0.0038)	(0.0030)
E-Commerce Intensity	-0.0069	-0.0906***	-0.0049	-0.1636***
$\times D_t$	(0.0246)	(0.0200)	(0.0668)	(0.0411)
Sample	Goods	Goods	Goods	Goods
Fixed Effects	Product	Product	Product	Product
Estimation Period	1992-2001	1992-2016	1992-2001	1992-2016
Observations	152,416	393,246	150,417	387,918
R^2	0.04	0.03		
E-Commerce Intensity Year	2009	2009	2009	2009
First-Stage F-Stat			57.33	58.26
Estimation Method	OLS	OLS	IV	IV

Data source: RPS, NSFIE, and author's calculation. Notes: Table shows relative price changes for goods sold online intensively relative to goods not sold online intensively before and after the entry of e-commerce firms. Column 1 and 3 are for 1992-2001 and column 2 and 4 are for 1992-2016. For the first two columns, OLS estimates are shown with e-commerce sales intensity and the second two columns use catalog sales intensity as IV.

Turning to our differences-in-differences specification, we present the results from estimating equation (3.16) in Table 3.3. The first column present the results from estimating equation (3.16) over the period 1992 to 2001. Consistent with what we observed in Figure 3.2, we do not find much of an effect from e-commerce in the first few years after the entry of Rakuten. However, as one can see in columns 2 and 3, we do see a significant decline in the *relative* prices of goods available online as evidenced by the coefficient of -0.09 on the post-e-commerce e-commerce intensity interaction ($x_i D_t$) term. The coefficient implies that a good at the 90^{th} percentile of internet intensity experienced a 1.3 percent per year drop in its rate of annual price growth relative to goods not available online.

As we have argued earlier, one possible challenge to our identification strategy is that e-commerce firms are not likely to have chosen which sectors they are likely to have entered randomly. In order to deal with this endogeneity, we construct a variable, catalog intensity, which is constructed analogously to e-commerce intensity except that we use catalog sales instead of e-commerce sales. Unfortunately, the earliest year for which we have catalog sales data is 1999, but since Rakuten was only two-years old in 1999 and still a small company and major players in the e-commerce market like Amazon Japan had not even entered the Japanese market, we think it plausible to argue that the distribution household catalog purchases in 1999 were unlikely very different than those before the entry of Rakuten.

Table 3.4 reports the results of our instrumental variables estimation. As one can see from the F-statistic reported in the first two columns of the table, catalog sales intensity in 1999 is a strong instrument for e-commerce sales intensity in 2009. Sectors that on average were major channels for catalog sales also became major channels of e-commerce firms. In the third, column we simply regress the e-commerce intensity of sectors in 2009 on catalog intensity in 1999 to show that the relationship holds in the cross-section. This establishes that as long as historical catalog sales were not being driven by the anticipation of e-commerce, we have a powerful instrument for e-commerce sales intensity.

Table 3.4: Relative Price Changes

	(1) E-Commerce Intensity $\times D_t$	(2) E-Commerce Intensity $\times D_t$	(3) E-Commerce Intensity
Catalog Intensity $\times D_t$	0.7363*** (0.0972)	0.7304*** (0.0957)	
D_t	0.0237*** (0.0043)	0.0241*** (0.0043)	
Catalog Intensity			0.7457*** (0.0745)
Constant			0.0269*** (0.0051)
Sample	Goods	Goods	Goods
Fixed Effects	Product	Product	None
Estimation Period	1992-2001	1992-2016	
Observations	150,417	387,918	306
R^2	0.26	0.26	0.25
E-Commerce Intensity Year	2009	2009	2009
First-Stage F-Stat	57.33	58.26	
Estimation Method	IV-First Stage	IV-First Stage	OLS

Data source: NSFIE and author's calculation. Notes: Table shows the first stage regression results.

We report the results from our instrumental variables (IV) estimation in columns 3-4 of Table 3.3. As before, we do not see much of an effect of e-commerce on national pricing in the first few years after the entry of Rakuten and the other e-commerce firms, but we do see strong effects in subsequent years. Overall, our IV estimate of the impact of e-commerce intensity ($x_i D_t$) on price increases doubles in magnitude in the full sample estimates (columns 2 and 4). The fact that the OLS estimates are attenuated implies that e-commerce firms tended to enter sectors where prices were rising, perhaps because these markets were likely to be more profitable. This pattern of behavior would explain why estimates that do not control for the endogeneity of market entry are likely to underestimate the the relative impact of e-commerce on pricing. In terms of economic significance, the results in column 4 imply that a good at the 90^{th} percentile of internet sales intensity had rates of price increase that were 2.4 percentage points per year lower than goods not sold online after the entry of e-commerce firms.

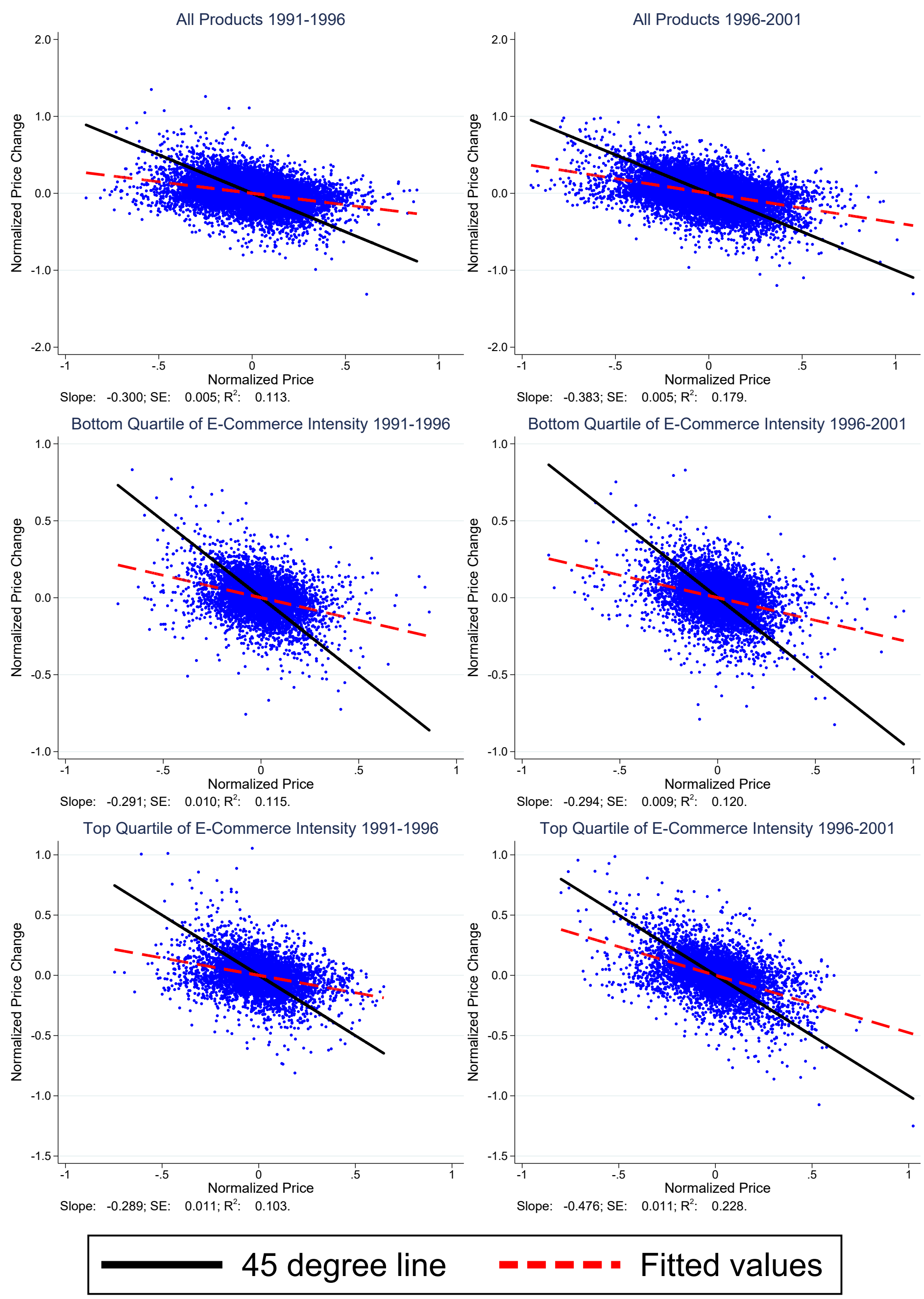
3.4.2 Gains Due to Price Arbitrage

As the last section made clear, while there is strong evidence that the rise of e-commerce caused the relative prices of goods sold online to decline in Japan, there is little evidence that this caused the overall price level to fall because the the lower relative rate of price increase for goods sold intensively by e-commerce firms was in part due to higher rates of price increase for goods sold principally by physical merchants. However, there is an alternative mechanism through which e-commerce might affect prices along the lines suggested by Jensen (2007) and DellaVigna and Gentzkow (2017): namely e-commerce might force retailers to adopt uniform pricing across regions. This effect would be manifest in our data by an acceleration of price arbitrage across cities.

In order to visualize whether this is likely to be important, we first consider two five-year periods. The first five-year period (1991-1996), pre-dates the formation of e-commerce by at least a year, so we can call this period the "pre-e-commerce period." We start the second period in 1996 because we assume that in 1996, the distribution of prices was reflective of a world without e-commerce but by 2001, Rakuten was already a prominent, listed company, with tens of millions of hits and thousands of stores selling on its platform.

It is difficult to compare price changes across goods and cities in their raw form because different goods exhibit different average price changes in different years. We therefore normalized the data by regressing Δp_{ict} and p_{ict} on product and city fixed effects and construct normalized price changes ($\Delta^5 p_{ict} - \hat{\alpha}_{it} - \hat{\beta}_{ct}$) and normalized price levels ($p_{ic,t-5} - \hat{\alpha}'_{it} - \hat{\beta}'_{ct}$), where $\hat{\alpha}_{it}$ ($\hat{\alpha}'_{it}$) and $\hat{\beta}_{ct}$ ($\hat{\beta}'_{ct}$) are the estimated fixed effects from the regression of Δp_{ict} (p_{ict}) on product and city fixed effects. Thus, these normalized prices

Figure 3.3: Normalized Price Change vs. Normalized Price



Data source: RPS, NSFIE, and authors' calculation. Notes: This graph plots normalized price changes against normalized price levels. The left panel shows normalized price changes before the entry of e-commerce and the right panel shows them after the entry of e-commerce. The first panel plots for all goods, the second panel plots for goods with e-commerce intensity lower than the bottom quartile, and the third panel shows for goods with e-commerce intensity higher than the top quartile.

remove the effect of any common price movements at the product or city level. Figure 3.3 presents plots of normalized five-year change in prices ($\Delta^5 p_{ict} - \hat{\alpha}_{it} - \hat{\beta}_{ct}$) against the normalized five-year lag of prices in each city ($p_{ic,t-5} - \hat{\alpha}'_{it} - \hat{\beta}'_{ct}$).

The first panel shows how normalized price changes vary with normalized prices before and after the entry of e-commerce. There is a clear negative relationship between initial urban price deviations and future price growth, which indicates that goods that had high prices in cities tended to have lower rates of inflation than goods with low relative prices. This mean reversion is likely the product of price arbitrage. As one can see from these two plots, 30 percent of any relative price difference tends to be eliminated within five years before the advent of e-commerce and this number rose to 38 percent in the five years after e-commerce firms entered. These plots also speak to the relatively high quality of the Japanese data. For example, studies using U.S. data (c.f., Parsley and Wei (1996)) find no evidence of price convergence once one controlled for city fixed effects.[9]

The next two pictures show what was driving this increase in the intercity rate of price convergence. Here, we divide the sample into the set of goods with an internet sales intensity in the lowest first quartile of the distribution in 2009 ($x_i < 0.076$) and the set of goods in the highest quartile of the distribution ($x_i > 0.13$). As one can see from the second panel in Figure 3.3, there was almost no change in the rate of convergence for goods not sold on the e-commerce. The slope of the line for goods not sold intensively online in the early period is -0.29, which is almost identical to the slope in the pre-e-commerce period (-0.30). In other words, the entry of e-commerce firms seems not to have affected the speed at which intercity price differentials converged for goods not sold much online. However, we see a very different pattern for goods with an e-commerce intensity in the upper quartile of the distribution. The slope steepens by 66 percent, rising in magnitude

[9]One plausible reason for the weaker evidence of price convergence in the U.S. is that that the data used in Parsley and Wei (1996) is not based on purposive sampling, so price changes in cities are based on a changing mix goods of different qualities across locations (as shown in Handbury and Weinstein (2015)).

from -0.29 to -0.48. Thus, enabling consumers to shop online seems to have significantly reduced the ability of merchants to charge different prices in different cities for the same good. We now turn to exploring this result rigorously in Section 3.2.1.

3.4.2.1 Estimating Convergence Rates

Following Obstfeld and Rogoff (1996) , we can test for whether we observe absolute price convergence or relative price convergence by estimating equation (3.1) and testing whether the estimated city-time fixed effects are jointly zero. If they are, then the data suggests that the prices of goods are converging to the same price across cities. Otherwise, it implies that the prices of goods converge to different levels in different cities. We can use an F-test to reject the hypothesis that the city-year fixed effects are zero, which suggests that absolute price convergence fails, so average price levels of goods do not converge to exactly the same level in all cities. We therefore city-time fixed effects in our main specifications. We also report results without city-year fixed effects in an online appendix as a robustness check to show that their inclusion does not matter qualitatively for our results.

Table 3.5 presents the results of estimating equation (3.1) for five- and one-year intervals using 1999 catalog sales intensity as an instrument for e-commerce sales intensity. In the first two columns, we present separate regressions for 1996 and 2001 where we let the convergence rates vary across the two time periods as we did in the earlier plots. Comparing the first rows of columns 1 and 2 reveals the convergence rates for goods not suitable for e-commerce (i.e., those where $x_i = 0$) were almost identical before and after the entry of e-commerce, which is the result that we saw in Figure 3.3. The coefficient on e-commerce intensity interacted with lagged prices ($x_i p_{ic,t-5}$) in column 1 indicates that the rate of convergence for goods suitable for e-commerce sales was not significantly different than the convergence rate of other goods prior to to the entry of e-commerce. However, the negative and significant coefficient on the interaction term ($D_t x_i p_{ic,t-5}$) in the post-e-commerce sample (where we dropped the $x_i p_{ic,t-5}$ term from the specification because we do not have any pre-e-commerce observations) indicates significantly faster convergence rates for goods available online after the entry of e-commerce firms also confirming the result we saw in we saw in Figure 3.3.

Table 3.5: Estimates Over Period 1991-2001

Dependent Variable	(1) Δp^5_{ict}	(2) Δp^5_{ict}	(3) Δp^5_{ict}	(4) Δp^1_{ict}
Lagged Price	-0.292***	-0.324***	-0.309***	-0.129***
	(0.032)	(0.037)	(0.031)	(0.013)
E-Commerce Intensity	-0.170		-0.039	0.032
(t=2009) $\times$ Lagged Price	(0.413)		(0.410)	(0.151)
E-Commerce Intensity		-1.145*	-1.292***	-0.509***
$\times$ Lagged Price $\times$ Post Rakuten		(0.592)	(0.315)	(0.130)
t	{1996}	{2001}	{1996,2001}	Annual 1992-2001
Observations	25,848	27,407	51,012	152,416
E-Commerce Intensity Year	2009	2009	2009	2009
First-Stage F-Stat	29.82	33.98	17.81	16.42
Estimation Method	2SLS	2SLS	2SLS	2SLS

Source: RPS, NSFIE, and authors' calculation. Notes: Table shows regression results of equation (3.1) using 2SLS: e-commerce sales intensity in 2009 is instrumented using 1999 catalog sales intensity. The first column uses the five-year log differences in prices from 1991 - 1996 and the second column uses that from 1996 - 2001. The third column uses two five-year periods, 1991 - 1996 and 1996 - 2001. The OLS regression results are available first three columns OLS regression results are available in Appendix C.1 from Table C1.

In column 3, we estimate our baseline differences-in-differences specification of equation (3.1) using a five-year difference by letting t take on two values: 1996 and 2001. The most important result for our purposes is the estimate of the coefficient on the interaction term on the e-commerce intensity coefficient. As one can see from the table, the coefficient is negative and precisely measured. Not surprisingly, the estimated coefficient on $p_{ic,t-k}$, $\hat{\gamma}$, does not change much, and we continue to get a negative and significant coefficient on the e-commerce intensity interaction term ($\hat{\delta}_2 = -1.292$). Interestingly, the estimate of δ_1, the differential rate of price convergence for e-commerce intensive goods remains close to zero, indicating that e-commerce appears to have no impact on intercity price convergence rates of goods not sold intensively online.

Table 3.6: Estimates Over Alternative Periods

Dependent Variable	(1) $\Delta^5 p_{ict}$	(2) $\Delta^5 p_{ict}$	(3) $\Delta^5 p_{ict}$	(4) $\Delta^5 p_{ict}$	(5) $\Delta^1 p_{ict}$
Lagged Price	-0.378*** (0.028)	-0.450*** (0.032)	-0.373*** (0.030)	-0.373*** (0.026)	-0.144*** (0.013)
E-Commerce Intensity (t=2009) $\times$ Lagged Price	0.676 (0.415)	1.337*** (0.439)	0.518 (0.423)	0.519 (0.403)	0.172 (0.165)
E-Commerce Intensity $\times$ Lagged Price $\times$ Post Rakuten	-1.846*** (0.354)	-3.168*** (0.316)	-1.943*** (0.373)	-1.741*** (0.247)	-0.775*** (0.102)
t	{1996,2006}	{1996,2011}	{1996,2016}	{1996,2001, 2006,2016}	Annual 1992-2016
Observations	51,845	43,256	42,555	87,515	393,246
E-Commerce Intensity Year	2009	2009	2009	2009	2009
First-Stage F-Stat	15.03	23.61	19.13	18.02	27.17
Estimation Method	2SLS	2SLS	2SLS	2SLS	2SLS

Source: RPS, NSFIE, and authors' calculation. Notes: Table shows regression results of equation (3.1) using 1999 catalog sales intensity as an instrument for e-commerce sales intensity. First three columns compare the five-year period of log price differences before (1991-1996) and after the entry of e-commerce firms (2001-2006, 2006-2011, and 2011-2016). The last column uses the annual frequency of log price changes. The OLS regression results are available in Appendix C.1 Table C2.

There are a number of potential problems with the evidence that we have just presented. A first concern is that these results may understate the impact of e-commerce because e-commerce firms were relatively small before 2001. In order to deal with this concern, Table 3.6 presents results in which we use alternative time periods. In the first three columns, we do a differences in differences based comparing the five years prior to the entry of e-commerce firms (1991-1996) with three alternative non-overlapping periods: 2001-2006, 2006-2011, and 2011-2016. These results confirm what we saw in Table 3.3; the estimated effects of e-commerce on pricing are stronger after e-commerce firms has a chance to expand operations. The coefficient on the e-commerce triple interaction ($\hat{\delta}_2$) approximately doubles if we compare periods ten or more years after the entry of Rakuten with the period before. We obtain a similar result when we repeat the estimation over the full period (1992-2016) at the annual frequency.

Our results are economically significant as well. If we use the estimates in column 5 of Table 3.6 as a benchmark, we find that the half life for a relative price difference for a good not traded online is 4.5 years. By contrast, the half life for a good with maximal internet sales intensity is six months. Similarly, goods at the 90^{th} percentile of e-commerce sales intensity have a half life of price dispersion of 2.6 years. Thus, our estimates imply that the advent of e-commerce seems to have significantly altered the ability of retailers to charge different prices in different cities.

The second concern that one might have with the the results is that we may have a data measurement problem that is influencing the results. In order to make sure that some idiosyncratic component of the NSFIE survey method is not driving our results, we replicate our result using measures of e-commerce intensity based on Rakuten sales data instead. We report the results from this exercise in Appendix Table **??**, which shows that we obtain very similar results regardless of whether we measure internet sales intensity using consumer expenditure data or Rakuten e-commerce sales data.

3.4.2.2 Welfare Gain

Aggregate consumer gains due to faster price convergence can be calculated from the equation (3.15). One of the interesting features of these equations is that the welfare gain is proportional to the choice of demand elasticity. Since this elasticity has been estimated in other papers, we calibrate a demand elasticity of -6 and simply note that the welfare gain using any other elasticity (η) equals the welfare gain in Table 3.7 multiplied by $\eta/6$. In all cases, we base our estimates of the impact of e-retail on the rate of convergence on

Table 3.6 column 5. All of the data has been converted into 2014 yen. The first columns shows the estimated welfare gains due to price convergence in 2014 and the second column gives the counterfactual welfare gain that would have occurred if price convergence for goods available online had remained at the pre-e-commerce rate (i.e., $(D_t = 0)$) as given by equation (3.14). We see that price convergence across regions during this time period led to a welfare gain of 4,315 billion yen in 2014 (about 38 billion US dollars) for residents of our sampled cities. In the second column, we compute the counterfactual gain that would have occurred if the speed of convergence had remained at the pre-e-commerce rate. This gain is lower: 2,917 billion yen in 2014. Thus, the difference between these two columns—1,218 billion yen— (approximately 11 billion US dollars) constitutes the annual welfare gain for consumers in our sample of cities in 2014.

Table 3.7: Counterfactual Welfare Gain

Year	$\hat{W}^R_{2014}(D_t = 1)$	$\hat{W}^R_{2014}(D_t = 0)$	$\Delta\hat{W}^E_{2014}$	Total Expenditure	Expenditure on Goods
2014	4,135	2,917	1,218	42,262	26,991

Notes: Unit is in billions of yen. The first three columns show welfare gains due to price arbitrage in 2014 with and without e-commerce firms, and their difference.

This number is difficult to interpret because it is only computed for residents in our sampled cities. To obtain a sense of how much this matters for welfare, we deflate the number by the total amount of expenditures of our sampled households, which is reported in the fourth column of Table 3.7. We obtain an estimate of the welfare gain which equals 2.9 percent of consumer expenditures. [10]

One feature of the Jensen (2007) approach is that it is possible that certain locations that had on average low prices might actually lose as a result of more uniform pricing. We

[10] To get some sense of how large this is, we can compare the gain to Brynjolfsson et al. (2003) estimate of the gains due to Amazon's entry into U.S. book market. That paper estimated a gain of less than 1 billion dollars in 2000—only 0.015 percent of U.S. personal consumption expenditures in that year. In other words, our estimate is about eight times as large.

Figure 3.4: Counterfactual Welfare Gain Per Household vs. Log of Population

../Rakuten/Paper/figures/welfare_vs_population_1997_2016_

Data source: NSFIE, JSB, and authors' calculation. Notes: This plot plots the city-level welfare gain in 2014 per household due to enhanced arbitrage against log population in 2014. The number of observations is 48.

explore this in Figure 3.4, where we plot the per household welfare gain in each Japanese city against the population of the city. There are two striking features of the plot. First, there are substantial welfare gains for the largest Japanese cities. The six largest cities in Japan—Tokyo, Yokohama, Osaka, Nagoya, Sapporo, and Kobe—all experienced welfare gains as a result of e-commerce's effect on price arbitrage. However, more than half of the cities in our sample experienced losses and most of these losses accrued in small cities.

While we do not have data on the share of college graduates or average household income by city for Japan, we do have it by prefecture, which lets us understand how these welfare gains are distributed across regions. For prefectures with two or more cities in them, we assume that the welfare gain is equal to the population weighted average of the welfare gain in each city. In Figures 3.5 and 3.6, we plot the welfare gains per household in each prefecture against the share of college-educated people or the average household income in that prefecture. The data make clear a very strong positive correlation between our estimated welfare gains and income per household and a less strong, but also positive correlation between gains per household and the share of the prefecture with college education. These results suggest that the e-commerce can create winners and loserthrough pricing effects because new technologies like e-commerce benefit high-income, highly educated consumers, but it may raise costsfor low-income, less educated households. These

Figure 3.5: Counterfactual Welfare Gain Per Household vs. Share of College Education

../Rakuten/Paper/figures/welfare_vs_education_1997_2016_

Data source: NSFIE, JSB, and authors' calculation. Notes: This plot plots the city-level welfare gain in 2014 per household due to enhanced arbitrage against the share of college education. The number of observations is 47.

Figure 3.6: Counterfactual Welfare Gain Per Household vs. Household Income

../Rakuten/Paper/figures/welfare_vs_income_1997_2016_v15

Data source: NSFIE, JSB, and authors' calculation. Notes: This plot plots the city-level welfare gain in 2014 per household due to enhanced arbitrage against the household income in 2014. The number of observations is 47.

differences are economically quite significant. Households in Tokyo had gains of ¥200,000 per household (around $1,800), but low-income, low-education prefectures like Miyazaki (located in the southern tip of the main archipelago) or Akita (located in the north of Japan's main island) actually lost comparable amounts as a result of e-commerce.[11]

3.4.3 Gains in "New Trade Models"

An alternative channel through which e-commerce might affect welfare is by enabling consumers to access new varieties as in Brynjolfsson et al. (2003). One of the challenges of estimating the gains from new varieties is that our data does not enable us to see which

[11] The results are consistent with those of DellaVigna and Gentzkow (2017) who argue that uniform pricing eliminates the ability of merchants to charge high prices in high-income cities and low prices in low-income cities.

varieties became available. Fortunately, we do observe sufficient statistics that enable us to compute the welfare gain even in a world in which we do not see the underlying varieties. In order to do this, we adopt the framework of Arkolakis et al. (2012). Suppose that the correct model of how retail operates is described by a Krugman (1980) or Melitz (2003) model in which firms in their model correspond to retail merchants who sell locally through physical stores or at a distance through catalogs or e-commerce operations. In this simple extension of Melitz (2003), we assume that manufactured goods are produced locally using only labor using a constant returns-to-scale technology, so the cost of producing a manufactured good in region j is $w_j a$, where w_j corresponds the wage in j and a is the unit labor requirement. Manufacturers sell to local retailers who then can sell these goods locally or at at a distance. In order for a retailer to sell locally, it needs to pay a fixed cost and incurs a marginal cost of sales equal to $(a/\varphi)\, w_j$, where φ is the productivity of the retailer. Similarly, in order to sell in a different location, a retailer needs to pay an addition "export" fixed cost and an iceberg transportation cost between regions i and j of τ_{ij}. It is immediately obvious that this cost structure is exactly that of Melitz (2003). Moreover, we can think of e-commerce and catalog sales as a technology that reduces the cost of trade between regions (τ_{ij}). [12]

If wethink about e-commerce as a trade-facilitating technology, we can use the result in Arkolakis et al. (2012) to write the log change in welfare following a trade liberalization $\Delta W_t = \frac{1}{\epsilon} \ln\left(\lambda_t/\lambda_{t-k}\right)$ where $\lambda_t \in (0, 1]$ is the share of consumer of expenditures on sales from retailers other than e-commerce firms in period t, and ϵ equals the "trade elasticity." To understand how this formula works, imagine that in the initial period $(t-k)$ consumers only purchase products locally, so $\lambda_{t-k} = 1$, but after the advent of e-commerce, consumers purchase ten percent of their goods online, so $\lambda_t = 0.9$. If we use a standard estimate of the

[12] In order for the counterfactual to be exact, we also need to assume that the advent of e-commerce does not cause labor to move across regions. However, given the abundant evidence of sluggish migration across regions even in the presence of large shocks and the likely small impact of e-commerce on relative wages, we think that this assumption is a reasonable approximation.

trade elasticity of -5, we will obtain a welfare gain of 2.1 percent (=$-\ln(0.9)/5$). Since we observe e-commerce sales at the prefectural level in Japan, we can also use regional e-commerce sales shares to compute gains for each prefecture. Moreover, the formula can easily be adapted to account for catalog sales. In order to account for catalog sales, we simply define λ_t to be the share of expenditures on products not sold by catalogs or e-commerce firms, i.e., local physical stores and use the same formula. This is a very simple way to incorporate the fact that e-commerce gains may higher or lower once we take into account catalog sales.

In order to implement these calculations, we need to first adjust the data to take into account that not all consumer expenditures occur through retailers. Based on the NSFIE data, we know that the share of household expenditures purchased from all retailers (χ) was 0.62 in 2014, with the remaining expenditures covering utilities, education, and other expenditure items that we will assume are not affected by e-commerce's entry into the goods sectors. In 2014, e-commerce expenditures on goods as a share of all retail expenditures, which we denote by s, was 0.0437. The share of household expenditures from non-e-commerce firms in 2014 is therefore $\lambda = (1-s)\chi + 1 - \chi = 0.97$. Assuming a trade elasticity of -5, his gives us an estimate of the welfare gain from e-commerce in Japan in 2014 of 0.5 percent. We report this number in the first column of Table 3.8 along with a number of alternative estimates based on different plausible estimates of the trade elasticity. These welfare gains range from 0.4 percent to 1.0 percent in 2014 and from 0.5 percent to 1.2 percent in 2017. The higher numbers in later years reflect the fact that e-commerce sales have continued to expand rapidly in Japan.

Table 3.8: National Welfare Change

Epsilon	ΔW_{14}	ΔW_{17}	ΔW^c_{14}	ΔW^c_{17}
-3	0.009	0.012	0.010	0.013
-5	0.005	0.007	0.006	0.008
-7	0.004	0.005	0.004	0.006

Data source: NSFIE, JSB, MIETI and authors' calculation. Notes: The first two column shows welfare gain due to new varieties from e-commerce in 2014 and 2017 along with plausible trade elasticities. The last two column shows welfare gain due to the increased variety from e-commerce and catalog sales in 2014 and 2017.

The second two columns make define local sales as total expenditures less expenditures on products sold over the internet or through catalogs. Interestingly, we see that aggregate welfare gains appear to be higher when we allow for the fact that consumers purchased and continue to purchase goods through catalogs. The mechanical reason for this result is that the share of consumer expenditures through catalogs actually grew slightly between 1996 and 2014. While one might have thought that the growth of e-commerce would have led to lower catalog sales because e-commerce is a good substitute for catalogs, there are a number of reasons why they may have grown together. First, the remarkable reduction in the costs of data transmission through the internet also occurred at a time when it became significantly cheaper to obtain and use phones. Thus, reductions in telecommunications costs may have benefited both catalog and e-commerce merchants. Second, e-retailers often advertise their wares in catalogs, and as e-commerce firms grew, they may have expanded catalog mailings, which may have caused both to rise. Nevertheless, the rise in catalog sales was quite small, so adjusting for it raises the welfare gains by around 0.2 percentage points.

This number is substantially larger than the structural approach we applied in Section 3.4.2.2 and reflects the impact of different modeling assumptions. A major advantage of the Arkolakis et al. (2012) approach is that it corresponds exactly to the gains implied

Figure 3.7: ΔW_{pt}^{c} : $\epsilon = -5$ vs. Share of College Education

../Rakuten/Paper/figures/acr_welfare_c14_college_edu_v15

Data source: NSFIE, JSB, MIETI and authors' calculation. Notes: Figure plots the welfare gain due to the increased variety including catalog sales in 2014 against the share of college education. The number of observations is 47.

by "core" trade models and the calculation takes into account general equilibrium effects. A disadvantage of this approach is that these models impose a number of assumptions that may not exactly fit the data: iceberg transportation costs, balanced trade, no labor mobility, monopolistic or perfect competition, aggregate profits being a constant share of revenues, etcetera. A second potential problem with variety-based approaches to modeling the internet is that they are "hard-wired" to produce welfare gains as long as e-commerce shares (or e-commerce and catalog shares) rise everywhere. The major advantage of the Jensen (2007) approach used in Section 3.4.2.2 is that it not based on all of these identifying assumptions and as a result produces different estimates and allows for the possibility that e-commerce may not benefit everyone, but the main disadvantage is that it does not take into account general equilibrium forces that might also matter for welfare. Since it is difficult to say which approach is most plausible, we simply note that reasonable estimates of the percentage welfare gains from e-commerce in 2014 range from 0.5 percent to 2.9 percent.

As in Section 3.4.2.2, it is also interesting to see how these gains have affected individual prefectures. In order to do this, we computed the welfare gains for each prefecture. Since there are 47 prefectures in Japan (which are similar in size to U.S. counties), we do not present for prefectures individually, but instead look for patterns in the data. One of the strongest patterns arises from the fact that in Japan, the share of online purchases is strongly associated with college education, which means that the share of prefectural expenditures on e-commerce (λ_{p14}) has a strong negative correlation with the share of college educated people in a prefecture. This produces a strong positive correlation (0.68) between the welfare gain in a prefecture and the share of college-educated people in the prefecture. We can see this clearly in Figure 3.7, where we plot the welfare gain (including catalog sales in our definition of non-local sales). This result suggests that our earlier result about a digital divide in which regions with a large share of highly educated people benefit more than regions with fewer highly educated people is present even if we shift our methodology of computing welfare gains.[13]

Table 3.9: Prefecture Welfare Change

	(1) ΔW_{14}	(2) ΔW_{14}	(3) ΔW_{14}	(4) ΔW_{14}	(5) ΔW_{14}	(6) ΔW_{14}
Share of College Educated	0.0195***	0.0167***	0.0214***	0.0180***	0.0123**	0.0144***
	(0.0032)	(0.0041)	(0.0039)	(0.0041)	(0.0047)	(0.0052)
Population		0.0000				0.0000
		(0.0000)				(0.0000)
Income per Capita			-0.0000			-0.0000
			(0.0000)			(0.0000)
Average Age				-0.0001		0.0000
				(0.0001)		(0.0001)
Share of Secondary Educated					-0.0087**	-0.0084
					(0.0043)	(0.0052)
Constant	-0.0001	0.0004	0.0005	0.0034	0.0053*	0.0052
	(0.0008)	(0.0009)	(0.0011)	(0.0060)	(0.0028)	(0.0061)
Observations	47	47	47	47	47	47
R^2	0.460	0.474	0.469	0.464	0.506	0.528

Data source: NSFIE, JSB, MIETI and authors' calculation. The table shows how prefectural welfare gains due to increased variety relate to characteristics of prefecture - share of the college education, population, income per capita, and share of secondary education.

[13] Whether we include or exclude catalog sales does not matter substantively for our prefectural results. If we define λ_{p14} without counting catalog sales as non-local expenditures, we obtain the same correlation up to two significant digits, and the plot looks very similar.

One obvious concern with these results is that the share of college educated people might be correlated with other factors that matter for internet purchases. For example, Einav et al. (2017) document that e-commerce in the U.S. is positively associated with city size. Alternatively, it may be the case that income or age may be associated with e-commerce intensity. In order to understand the importance of these factors, we regressed the welfare gain on population (which is a proxy for urban vs. rural prefectures), prefectural income per capita, and average age and report the results in Table 3.9. We find that none of these variables are significant once we control for the share of college educated people in a prefecture. When we include the share of secondary-school graduates, we find that it is significant in one specification, but it has a negative sign, which reinforces our earlier point that it is highly educated people that are the main users of e-commerce. In fact, most of the coefficients are precisely estimated zeros.[14] These differences are economically significant. The gain for Tokyo (the prefecture with the highest share of college-educated people) is four times that of Aomori, which has the lowest gain and has a share of college-educated people that is half that of Tokyo.

3.5 Conclusion

This chapter makes use of a unique Japanese data set covering hundreds of products over close to three decades to examine the impact of the internet on Japanese prices and welfare. While we find that at the national level the price increases for goods sold intensively online are lower than those sold principally in physical stores, we show that this result was present even before the advent of e-commerce. Nevertheless, the entry of e-commerce firms is associated with a widening of this gap which is consistent with e-commerce affecting

[14] This may explain why Fan et al. (2018) find no link between education and internet sales intensity. Chinese education levels are much lower than in Japan, which means that very few people have gone to college in their sample. The average number of years of education in Fan et al. (2018) is only 8.8 years whereas the average in our sample of Japanese cities is 11.9 years.

relative price increases. However, part of the reason for the increasingly different price trends is due to the fact that the rate of price increase for goods sold intensively in physical storesonline rose, which underscores the difficulty in interpreting the relatively low rates of price increase for goods sold online as providing information about how e-commerce affects aggregate inflation.

At the local level, we find strong evidence that the rate at which intercity price differences disappear rose significantly for goods sold intensively online after e-commerce sales became common in Japan. Analyzing the impact of this faster rate of price convergence through the lens of the Jensen (2007) model indicates that the welfare gains due to e-commerce were sizable: Japanese welfare in 2014 was 2.9 percent higher as a result of e-commerce. However, the data also reveals an important digital divide. By lowering prices in high-income cities with high average relative prices for goods sold intensively online and raising them in low-income cities which tend to have low prices, the uniform pricing associated with e-commerce appears to have generated substantial gains to the richest Japanese cities while reducing the welfare of poorer cities.

When we examine the robustness of these results by calibrating new-trade theory models which control for general equilibrium forces and consider welfare gains through variety expansion, we find smaller overall gains from e-commerce—a welfare rise of 0.5 percent—but we find the same pattern of highly educated regions benefiting more than less educated regions. Although a feature of new-trade theorymodels is that no location can be made worse off as a result of trade liberalization, the estimated welfare gains in relatively rich cities like Tokyo are four times higher than in small cities. This result arises from the fact that higher-educated consumers buy substantially more online than less-educated consumers. Thus, while the level of the gains varies depending on the modeling framework adopted, the core result that e-commerce has a differentially positive effect for cities with a large share of high-income or highly-educated people remains.

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Appendix A

Appendix to chapter 1

A.1 CPS

Table A1 shows the unweighted number of population for age greater than 16 and the unweighted number of employed workers among the population greater than age 16. Table A1 also shows the imputation ratio for usual weekly earning and the hourly wage. Since the major revision in the CPS in 1994, about 34 percent of hourly wages are imputed by the CPS. The CPS imputes unreported data items to fill in based on the demographic characteristics and residential address.[1] Including imputed wages may amplify measurement error, so this paper drops imputed wages. Although IPUMS-CPS provides with the individual identifiers, they do not offer imputation flags for wage variables. Thus, this paper merges IPUMS - CPS data into CPS data to exclude imputed wages.

Table A2 shows the number of observations for hourly workers whose hourly wage growth rate is available. The spike at zero and the fraction of hourly workers with wage cuts and raises are also shown in Table A2.

[1] https://www.census.gov/programs-surveys/cps/technical-documentation/methodology/imputation-of-unreported-data-items.html

Table A1: The unweighted number of observation in the CPS and the imputation ratio

Year	Age $\geq$ 16	Employed	Usual weekly earning Including Imputation	Usual weekly earning Excluding Imputation	Usual weekly earning Imputation ratio	Hourly wage Including Imputation	Hourly wage Excluding Imputation	Hourly wage Imputation ratio
1979	1,314,693	787,170	171,595	142,839	16.8	101,392	86,323	14.9
1980	1,546,827	918,046	199,290	167,183	16.1	116,941	100,699	13.9
1981	1,456,261	861,395	186,766	157,760	15.5	109,545	95,055	13.2
1982	1,404,030	813,120	175,643	151,075	14.0	102,475	90,129	12.0
1983	1,394,390	808,514	173,763	149,358	14.0	102,126	89,857	12.0
1984	1,374,456	819,764	176,724	150,317	14.9	104,287	90,780	13.0
1985	1,375,158	828,675	179,671	153,633	14.5	106,174	92,556	12.8
1986	1,353,321	821,067	178,586	159,172	10.9	105,861	96,029	9.3
1987	1,348,579	828,009	180,272	155,604	13.7	108,033	95,385	11.7
1988	1,286,466	797,107	172,931	147,658	14.6	104,079	90,836	12.7
1989	1,301,108	814,698	176,411	169,438	4.0	106,594	104,732	1.7
1990	1,355,294	846,099	185,022	176,278	4.7	110,916	110,425	0.4
1991	1,341,040	822,621	179,555	170,083	5.3	108,088	107,590	0.5
1992	1,320,939	808,261	176,833	167,846	5.1	106,996	106,608	0.4
1993	1,302,955	798,202	174,587	164,720	5.7	105,595	105,188	0.4
1994	1,271,347	790,130	160,223	-	-	104,915	82,776	21.1
1995	1,251,928	784,129	159,344	39,798	75.0	104,976	25,991	75.2
1996	1,108,899	699,605	141,204	109,604	22.4	93,986	71,087	24.4
1997	1,114,451	708,705	143,999	111,214	22.8	95,571	72,226	24.4
1998	1,116,813	717,245	145,863	111,979	23.2	96,018	71,190	25.9
1999	1,123,666	723,156	147,726	107,929	26.9	96,545	67,801	29.8
2000	1,120,585	723,930	150,128	105,889	29.5	97,335	65,899	32.3
2001	1,236,870	793,912	157,460	110,480	29.8	102,410	68,712	32.9
2002	1,312,304	832,519	171,218	119,592	30.2	110,766	74,092	33.1
2003	1,302,483	818,795	167,393	114,282	31.7	108,915	70,976	34.8
2004	1,283,683	809,185	164,286	112,821	31.3	107,440	70,276	34.6
2005	1,279,052	810,893	165,522	114,632	30.7	108,662	71,531	34.2
2006	1,271,693	810,582	165,913	114,399	31.0	107,615	70,545	34.4
2007	1,260,380	801,226	165,246	115,224	30.3	104,945	70,299	33.0
2008	1,257,619	790,341	163,481	113,608	30.5	103,028	68,438	33.6
2009	1,273,634	766,660	158,331	110,588	30.2	100,010	66,815	33.2
2010	1,277,199	759,458	156,774	104,822	33.1	99,623	63,812	35.9
2011	1,265,607	749,778	155,636	102,360	34.2	98,885	62,345	37.0
2012	1,258,730	749,477	155,224	103,294	33.5	98,333	62,489	36.5
2013	1,253,663	745,840	155,474	99,965	35.7	97,570	60,185	38.3
2014	1,261,811	751,675	156,940	98,865	37.0	98,310	59,167	39.8
2015	1,245,862	739,222	155,734	94,674	39.2	97,108	56,410	41.9
2016	1,244,166	740,071	156,416	95,959	38.7	97,585	57,406	41.2
2017	1,227,127	731,896	154,809	94,638	38.9	95,955	56,385	41.2

Source: CPS and author's calculation. Sample period: 1979 - 2017
This table shows the unweighted number of observation. The second column shows the unweighted number of individuals greater or equal to 16 for each year in the CPS. The third column shows the unweighted number of employed workers, greater or equal to age 16. Column 4-5 show the unweighted number of workers whose usual weekly earning is available including imputation (column 4), excluding imputation (column 5). Column 6 shows the imputation ratio for usual weekly earning. Column 7-8 show the unweighted number of workers whose hourly wages are available, including imputation (column 7), excluding imputation (column 8). Column 9 shows the imputation ratio for the hourly wage.

Table A2: Time series spike at zero, the share of wage cuts and raises for hourly workers in the CPS

	Unweighted count of		Spike at zero (%)		Fraction of	Fraction of
year	Δw	$\Delta w = 0$	Unweighted	Weighted	$\Delta W < 0$	$\Delta W > 0$
1980	21,029	1,403	6.67	6.66	14.24	79.11
1981	23,641	1,605	6.79	6.70	14.32	78.98
1982	23,211	2,839	12.23	12.08	18.90	69.01
1983	22,869	3,397	14.85	14.65	20.64	64.71
1984	22,840	3,398	14.88	14.68	20.21	65.11
1985	11,115	1,608	14.47	14.25	20.65	65.10
1986	6,202	956	15.41	15.52	21.48	63.00
1987	24,569	3,807	15.50	15.36	21.41	63.23
1988	23,302	3,414	14.65	14.62	20.38	65.01
1989	24,648	3,293	13.36	13.16	21.26	65.58
1990	29,434	3,327	11.30	11.24	23.58	65.17
1991	30,034	3,549	11.82	11.64	24.91	63.44
1992	29,816	4,057	13.61	13.52	25.52	60.96
1993	29,751	3,989	13.41	13.45	26.42	60.13
1994	22,974	3,255	14.17	14.12	23.89	62.00
1995						
1996	6,085	887	14.58	14.50	19.89	65.62
1997	18,058	2,533	14.03	13.66	19.56	66.78
1998	17,866	2,458	13.76	13.50	18.30	68.20
1999	16,880	2,348	13.91	13.47	18.95	67.58
2000	15,796	2,251	14.25	14.18	18.24	67.58
2001	14,721	2,062	14.01	13.98	18.65	67.38
2002	15,789	2,558	16.20	16.12	20.12	63.76
2003	17,336	2,932	16.91	17.46	21.09	61.45
2004	16,243	2,791	17.18	17.55	21.36	61.09
2005	14,991	2,466	16.45	16.91	20.63	62.46
2006	16,374	2,513	15.35	15.80	20.87	63.33
2007	16,249	2,310	14.22	14.25	20.43	65.32
2008	16,437	2,492	15.16	15.49	20.55	63.96
2009	16,077	2,906	18.08	18.30	23.59	58.11
2010	15,620	3,272	20.95	21.14	24.61	54.25
2011	14,776	3,030	20.51	20.88	24.30	54.82
2012	14,463	2,947	20.38	20.45	24.73	54.82
2013	14,467	2,897	20.02	20.46	23.07	56.47
2014	13,342	2,538	19.02	19.50	22.15	58.35
2015	10,758	1,975	18.36	18.86	21.58	59.56
2016	12,125	2,155	17.77	17.55	20.95	61.50
2017	12,676	2,322	18.32	18.41	20.26	61.33

Source: CPS and author's calculation. Sample period: 1979 - 2017
This table shows the number of observation and the spike at zero, the fraction of workers with wage cuts and raises for all hourly paid workers. Household identifiers were scrambles in 1995 so there were no observations available in 1995, and it leads to small observations in 1996.

Figure A1 and A2 show the nominal year-to-year hourly wage change distribution for each year from 1980-2017. Nominal hourly wage change distribution is highly asymmetric: there is an apparent spike at zero and fewer wage cuts compared to raises.

Table A3: The average of the spike at zero, the share of wage cuts and raises by industry, CPS

	% hourly workers	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
Agriculture, Forestry, Fishing and Hunting	1.04	23.74	21.00	55.25
Other Services (except Public Administration)	3.69	22.07	22.04	55.90
Administrative, Support, Waste Management, and Remediation Services	1.59	20.65	23.33	56.03
Real Estate and Rental and Leasing	0.95	18.29	20.33	61.38
Arts, Entertainment, and Recreation	1.86	18.21	22.87	58.92
Accommodation and Food Services	7.65	18.15	26.32	55.54
Professional, Scientific, and Technical Services	3.25	17.67	17.63	64.70
Construction	6.43	17.66	21.11	61.23
Wholesale Trade	3.09	16.31	19.68	64.02
Retail Trade	14.51	15.82	20.53	63.65
Educational Services	5.18	14.68	21.73	63.60
Mining, Quarrying, and Oil and Gas Extraction	0.71	14.45	24.05	61.50
Manufacturing	20.91	13.65	20.83	65.52
Transportation and Warehousing	4.53	13.61	22.83	63.57
Health Care and Social Assistance	15.03	13.24	19.57	67.19
Finance and Insurance	2.66	12.72	18.74	68.55
Information	1.43	11.97	20.55	67.48
Utilities	1.69	11.54	20.07	68.39
Public Administration	3.81	11.15	19.93	68.92

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). This table shows the average of the spike at zero and the fraction of workers with wage cuts and raises over time by 2017 2 digit NAICS industry classification.

A.1.1 Time series spike at zero, fraction of wage cuts and raises

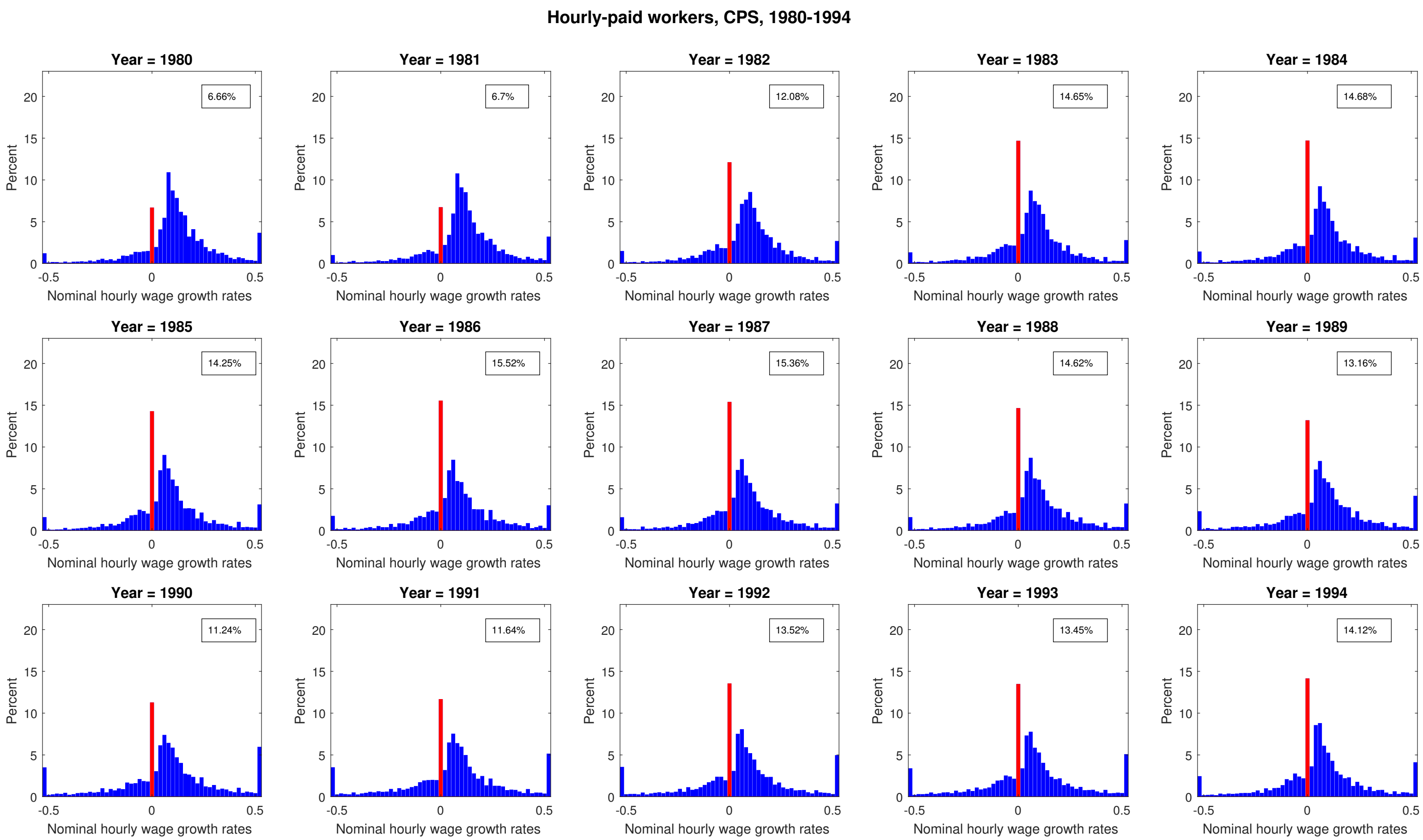

Figure A1: Nominal hourly wage growth rates distributions from 1980 to 1994

Data source: CPS and author's calculation. The red bin shows the spike at zero, the percentage of workers whose hourly wage growth rate is exactly zero. The width of blue bin is 0.02.

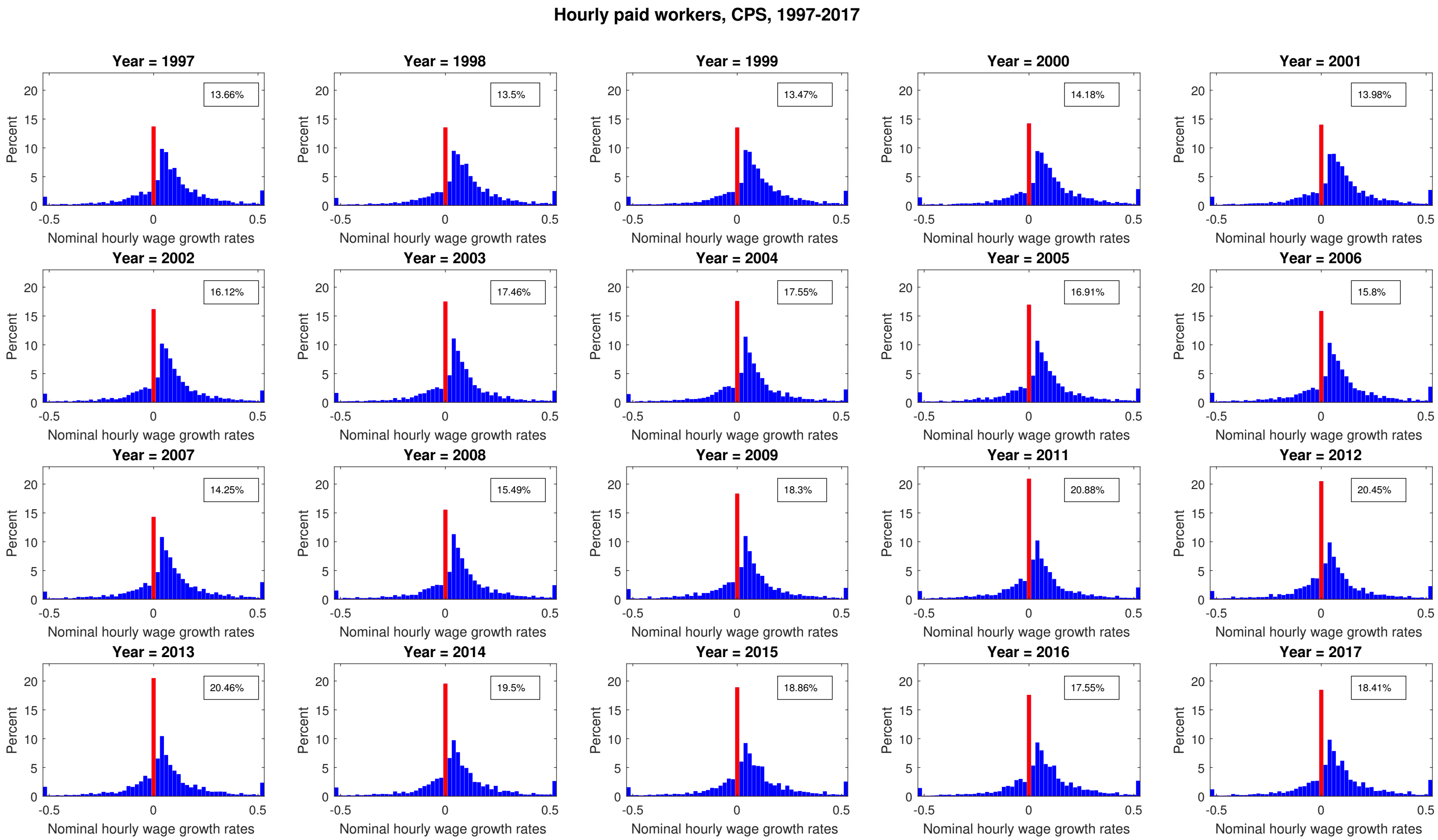

Figure A2: Nominal hourly wage growth rates distributions from 1997 to 2017

Data source: CPS and author's calculation. The red bin shows the spike at zero, the percentage of workers whose hourly wage growth rate is exactly zero. The width of blue bin is 0.02.

A.1.2 Robustness checks for aggregate time series evidence

Table A4 shows regression results based on (1.1), excluding minimum wage workers. Table A5 shows regression results based on (1.1) using only working age population from 16 to 64. Main results are robust even if we exclude minimum wage workers and we use only working age population.

Table A6 shows regression results based on (1.1) by varying the level of education. Table A7, A8, A9, A10 show regression results based on the level of age, gender, race, and hourly wage quartiles.Main results: the spike at zero increases when employment declines, controlling for inflation and the increase in the spike at zero is higher than the increase in the share of wage cuts when employment declines also hold for different worker characteristics.

Table A4: Exluding minimum wage workers, the spike at zero, the fraction of wage cuts, and raises

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Size of peak $\Delta W = 0$	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop	0.363	0.197	-0.559	0.555***	0.302*	-0.857**
	(0.336)	(0.222)	(0.532)	(0.201)	(0.156)	(0.316)
Inflation rate				-1.237***	-0.678***	1.915***
				(0.133)	(0.141)	(0.195)
				0.555/0.857 = 0.648		
Observations	37	37	37	37	37	37
Adjusted R^2	0.0150	-0.00620	0.0152	0.675	0.325	0.683

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1980-2017 (except 1995). Inflation rate is calculated from CPI-U.

There is no asymmetric response of nominal hourly wage change distribution to employment. Consider the specification, taking into account an asymmetric response of nominal wage change distribution to the employment, meaning that the response to the declining employment is different from the response to inclining employment. From the regression specification (A.1), γ captures asymmetric response to declining employment.

Table A5: The spike at zero, the fraction of wage cuts, and raises among prime-aged hourly workers along the business cycles

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero $\Delta W = 0$	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop ratio	0.283	0.105	-0.388	0.507***	0.237*	-0.743***
	(0.270)	(0.210)	(0.463)	(0.145)	(0.140)	(0.253)
Inflation rate				-1.168***	-0.688***	1.856***
				(0.124)	(0.145)	(0.214)
				0.507/0.743 = 0.68		
Observations	37	37	37	37	37	37
Adjusted R^2	0.0184	-0.0192	0.00542	0.717	0.318	0.684

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). Inflation rate is calculated from CPI-U. The spike at zero, the share of wage cuts and raises are constructed among prime-aged hourly paid workers.

Table A6: The spike at zero, the fraction of wage cuts and raises by education

	High School or less			College or more		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1 - Epop	0.551***	0.300	-0.851***	0.663***	0.323*	-0.986***
	(0.156)	(0.187)	(0.254)	(0.159)	(0.180)	(0.249)
Inflation	-1.189***	-0.721***	1.910***	-1.232***	-0.628***	1.860***
	(0.134)	(0.161)	(0.219)	(0.137)	(0.156)	(0.215)
	0.551/0.851=0.65			0.663/0.986 =0.67		
Observations	37	37	37	37	37	37
Adjusted R^2	0.695	0.346	0.687	0.709	0.305	0.691

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: CPS and author's calculation. Sample period: 1979-2017 (except 1995).

Table A7: The spike at zero, the fraction of wage cuts and raises by age

	$16\leq$ age < 40			$40\leq$ age < 64		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop	0.581***	0.247	-0.828***	0.614***	0.359	-0.973***
	(0.131)	(0.167)	(0.245)	(0.150)	(0.223)	(0.249)
Inflation	-1.093***	-0.699***	1.792***	-1.178***	-0.613***	1.791***
	(0.113)	(0.144)	(0.212)	(0.129)	(0.192)	(0.215)
	0.552/0.851=0.65			0.664/0.986 =0.67		
Observations	37	37	37	37	37	37
Adjusted R^2	0.737	0.383	0.675	0.713	0.209	0.676

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: CPS and author's calculation. Sample period: 1979-2017 (except 1995).

Table A8: The spike at zero, the fraction of wage cuts and raises by gender

	Male			Female		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop	0.516***	0.345*	-0.861***	0.714***	0.251	-0.964***
	(0.153)	(0.202)	(0.251)	(0.147)	(0.182)	(0.256)
Inflation	-1.104***	-0.510***	1.614***	-1.262***	-0.876***	2.139***
	(0.132)	(0.174)	(0.217)	(0.126)	(0.157)	(0.221)
	0.515/0.861=0.60			0.714/0.964=0.74		
Observations	37	37	37	37	37	37
Adjusted R^2	0.671	0.188	0.622	0.754	0.451	0.731

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: CPS and author's calculation. Sample period: 1979-2017 (except 1995).

Table A9: The spike at zero, the fraction of wage cuts and raises by race

	White			Non-White		
	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$	(4) Spike at zero	(5) Fraction of $\Delta W < 0$	(6) Fraction of $\Delta W > 0$
1-Epop	0.630***	0.333*	-0.964***	0.554***	0.0862	-0.641**
	(0.144)	(0.174)	(0.242)	(0.171)	(0.239)	(0.250)
Inflation	-1.199***	-0.678***	1.877***	-1.079***	-0.598***	1.677***
	(0.124)	(0.150)	(0.208)	(0.148)	(0.206)	(0.215)
	0.630/0.964=0.66			0.556/0.641 =0.87		
Observations	37	37	37	37	37	37
Adjusted R^2	0.736	0.359	0.707	0.611	0.152	0.629

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: CPS and author's calculation. Sample period: 1979-2017.

Table A10: The spike at zero, the share of wage cuts and raises by hourly wage quantiles

	25th below			From 25th to Median		
	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
1-Epop	0.972***	0.220	-1.192**	0.624***	0.131	-0.756**
	(0.272)	(0.271)	(0.448)	(0.204)	(0.247)	(0.339)
Inflation	-1.250***	-0.938***	2.188***	-1.218***	-0.689***	1.907***
	(0.235)	(0.234)	(0.387)	(0.176)	(0.213)	(0.292)
Observations	37	37	37	37	37	37
Adjusted R^2	0.491	0.282	0.483	0.584	0.191	0.541
	Median to 75th			Above 75th		
	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
1-Epop	0.429**	0.386**	-0.814***	0.547***	0.439**	-0.986***
	(0.200)	(0.177)	(0.283)	(0.163)	(0.164)	(0.234)
Inflation	-1.115***	-0.405**	1.521***	-1.144***	-0.703***	1.847***
	(0.173)	(0.152)	(0.244)	(0.141)	(0.141)	(0.202)
Observations	37	37	37	37	37	37
Adjusted R^2	0.535	0.191	0.532	0.659	0.427	0.716

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). This table shows the cyclicality of the spike at zero, the share of wage cuts and raises by hourly wage quantiles.

However, from Table A11, we can see γ is not statistically different from zero, implying that there is no asymmetric response of nominal wage change distribution to employment.

$$\begin{aligned}
\text{[Spike at zero]}_t &= \alpha_1 + \beta_1(1-e_t) + \gamma_1(1-e_t)\cdot \mathbb{I}[\Delta(1-e_t)>0] + \epsilon_{1t} \\
\text{[Fraction of wage cuts]}_t &= \alpha_2 + \beta_2(1-e_t) + \gamma_2(1-e_t)\cdot \mathbb{I}[\Delta(1-e_t)>0] + \epsilon_{2t} \\
\text{[Fraction of raises]}_t &= \alpha_3 + \beta_3(1-e_t) + \gamma_3(1-e_t)\cdot \mathbb{I}[\Delta(1-e_t)>0] + \epsilon_{3t}
\end{aligned} \tag{A.1}$$

Table A11: The spike at zero, the fraction of wage cuts and raises along the business cycle

	(1) Spike at zero	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
1-Epop	0.624***	0.280*	-0.904***
	(0.159)	(0.156)	(0.274)
(1-Epop)$_t \cdot \mathbb{I}(\Delta$(1-Epop)$_t > 0)$	-0.00792	0.0235	-0.0156
	(0.0170)	(0.0203)	(0.0271)
Inflation rate	-1.175***	-0.691***	1.866***
	(0.115)	(0.143)	(0.227)
Observations	37	37	37
Adjusted R^2	0.721	0.341	0.697

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: CPS and author's calculation. Sample Period: 1979-2017

A.1.3 Comparisons to the previous literature: CPS

Figure A3 compares the spike at zero from the previous literature using the CPS and the one that this paper constructed. When this paper constructs the spike at zero from nominal wage change distributions using the CPS, this paper includes all hourly workers including both job stayers and job switchers, while the previous literature focuses only on job stayers.

Card and Hyslop (1996) use the CPS of the sample period from 1979 to 1993 to construct the share of workers with no wage change among hourly rated job stayers. Elsby, Shin, and Solon (2016) use the CPS from 1980 to 2012 and job tenure supplements to construct the share of workers with no wage change among hourly rated workers whose job tenure is more than one year. The San Francisco Federal Reserve Bank publishes the Wage Rigidity Meter using the CPS from 1980 to 2017 with some gaps, which shows the fraction of works with a zero wage change among workers who have not changed their jobs.[2]

Based on the description, the spike at zero from Card and Hyslop (1996), Elsby, Shin, and Solon (2016), and the Wage Rigidity Meter should be similar; however, this is not the case. Although they are highly correlated with each other, there are differences in the level of the spike at zero. The spike at zero by Card and Hyslop (1996) is higher than the one from Elsby, Shin, and Solon (2016) and the Wage Rigidity Meter. Instead, the spike at zero from Elsby, Shin, and Solon (2016) and the Wage Rigidity Meter closely follows the spike at zero from this paper, which includes both job stayers and job switchers in the CPS. However, we know that the spike at zero for job stayers is higher than the spike at zero for job switchers from the SIPP. This may imply that the spike at zero from Elsby, Shin, and Solon (2016) the Wage Rigidity Meter do not solely come from job stayers.

[2]For the fair comparison, I used the percent of hourly rated job stayers with a wage change of zero from SF - Wage Rigidity Meter (here). Other than hourly workers, non-hourly workers and all workers' (including both hourly and non-hourly workers) Wage Rigidity Meter is also available. Atlanta Fed's Wage Growth Tracker (here) also reports the percent of individuals with zero wage changes. However, when they count individuals with zero wage changes, they include individuals with hourly wage growth rates from -0.5 percent to 0.5 percent, while this paper and SF - Wage Rigidity Meter count only workers with exact zero wage changes. Also, Atlanta Fed's wage growth tracker includes both hourly workers and non-hourly workers, while this paper considers only hourly rated workers. They impute hourly wages for non-hourly workers by dividing usual weekly earnings by usual weekly hours worked or actual hours worked. However, hourly wages calculated in this way tend to suffer from excess volatility, known as the division bias (Borjas (1980)).

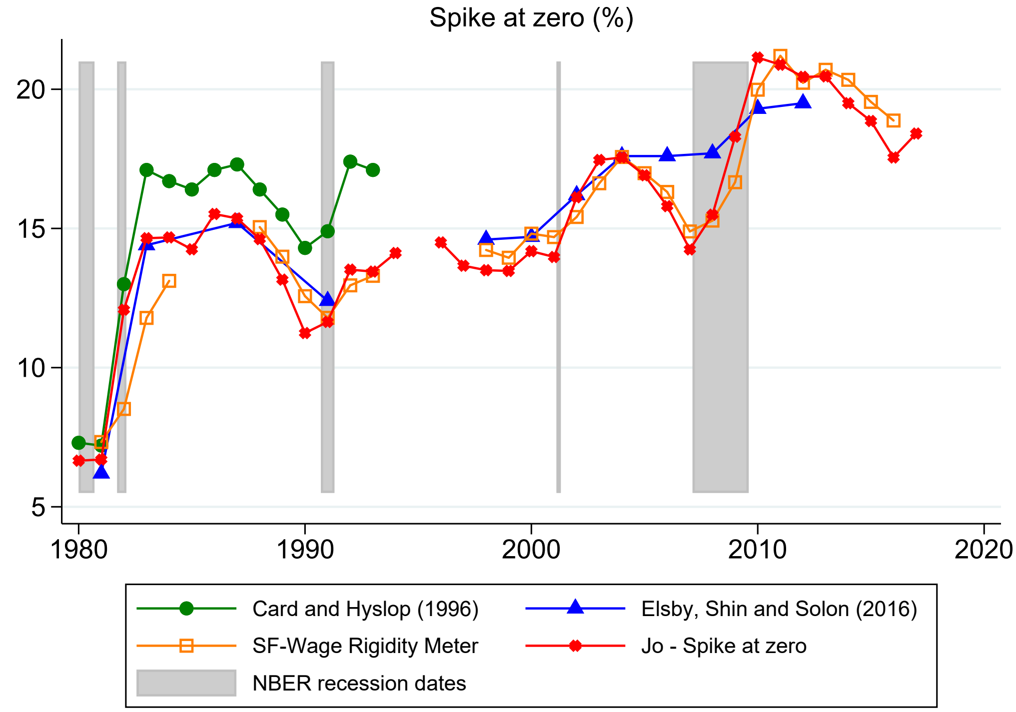

Figure A3: Comparisons of the spike at zero from the previous literature

Notes: Card and Hyslop (1996) - Data: CPS, Sample Period: 1979 - 1993, Job stayers only
Elsby, Shin and Solon (2016) - Data: CPS, Sample Period: 1980 - 2012 (biannual), Job stayers only
SF Wage Rigidity Meter - Data: CPS, Sample Period: 1980 - 2017, Job stayers only
Jo (2018) - Data: CPS, Sample Period: 1980 - 2017, Both job stayers and job switchers

A.2 SIPP

Table A13 shows the unweighted count of observations of hourly workers whose hourly wage growth rate is available for each year and the time series of the spike at zero, the share of wage cuts and raises. Table A14 divides hourly workers into two - job stayer and jobs switchers - and shows the unweighted count of observations, the spike at zero, the share of wage cuts and raises, respectively.

Figure A4 shows year-over-year hourly wage change distribution for hourly workers including both job stayers and job switchers for each year from 1985-2013 with some gaps. The red bar presents the spike at zero, the share of workers with no wage change and the size of blue bin is 0.02. Figure A5 shows year-over-year hourly wage change distribution for hourly job stayers and Figure A6 shows one for job switchers.

Table A12: The spike at zero, fraction of wage cuts and raises (%), SIPP, by hourly wage quartiles

	Hourly wage Quartiles	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
Job-stayer	25th below	36.11	15.45	48.44
	25th to Median	28.11	11.21	60.68
	Med to 75th	25.83	11.33	62.84
	75th and above	24.86	11.10	64.04
Job-switcher	25th below	18.11	45.20	36.69
	25th to Med	11.71	29.69	58.60
	Med to 75th	9.53	23.08	67.39
	75th and above	9.77	19.42	70.81

Data source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008). This table shows the sample average of the spike at zero and the fraction of workers with wage cuts and raises over time by hourly wage quartiles.

A.2.1 Time series spike at zero, fraction of wage cuts and raises

Table A13: The spike at zero, the share of wage cuts, and raises in the SIPP

Year	Obs Δw	Spike at zero $\Delta w = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
1985	9,827	16.75	18.76	64.50
1986	13,490	17.26	19.36	63.38
1987	11,171	17.92	20.11	61.97
1988	10,508	14.95	18.12	66.93
1989	10,930	14.63	17.92	67.44
1990				
1991	11,820	14.30	18.74	66.96
1992	17,241	17.31	19.32	63.37
1993	16,318	18.58	20.29	61.14
1994	19,430	18.28	20.66	61.07
1995	9,347	18.31	18.58	63.12
1996				
1997	16,951	14.02	18.68	67.30
1998	15,877	14.31	16.33	69.37
1999	14,939	16.98	16.91	66.11
2000	5,408	17.52	15.29	67.20
2001				
2002	13,727	16.12	21.85	62.04
2003	12,287	19.27	19.51	61.21
2004				
2005	20,055	30.13	17.31	52.57
2006	17,621	30.05	14.19	55.76
2007	7,922	31.48	13.64	54.88
2008				
2009	13,909	39.85	16.85	43.29
2010	16,080	42.22	16.00	41.77
2011	14,228	45.59	13.24	41.17
2012	13,242	43.84	13.72	42.44
2013	11,943	46.46	12.61	40.93

Source: SIPP and author's calculation. Sample period: 1984 - 2013 except 1990, 1996, 2001, and 2008
This table shows the unweighted number of observation and the size of peak, the fraction of workers with wage cuts and raises for hourly paid workers.

Table A14: The spike at zero, the share of wage cuts, and raises in the SIPP by job stayers and job switchers

	Job stayers				Job switchers			
Year	Obs Δw	Spike at zero $\Delta w = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$	Obs Δw	Spike at zero $\Delta w = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
1985	7,724	16.95	16.08	66.97	2,103	15.99	28.52	55.49
1986	9,735	18.58	16.14	65.28	3,755	13.50	28.50	58.00
1987	8,489	19.46	16.80	63.74	2,682	12.88	30.96	56.16
1988	7,593	16.70	14.00	69.30	2,915	10.35	28.92	60.73
1989	7,949	16.45	14.09	69.46	2,981	9.66	28.44	61.90
1990								
1991	8,699	16.41	13.70	69.89	3,121	8.43	32.78	58.79
1992	13,226	19.30	15.02	65.67	4,015	10.70	33.52	55.77
1993	12,514	20.97	16.34	62.69	3,804	10.66	33.36	55.98
1994	14,422	20.64	16.54	62.82	5,008	11.54	32.39	56.07
1995	6,935	20.56	14.92	64.52	2,412	11.86	29.03	59.11
1996								
1997	11,184	16.20	14.84	68.96	5,767	9.86	26.04	64.11
1998	10,290	17.05	12.05	70.91	5,587	9.30	24.16	66.55
1999	9,851	19.71	12.38	67.91	5,088	11.73	25.61	62.66
2000	3,938	20.00	11.54	68.45	1,470	10.93	25.20	63.87
2001								
2002	8,926	18.92	16.34	64.74	4,801	10.91	32.06	57.03
2003	8,491	22.17	14.25	63.57	3,796	12.81	31.25	55.94
2004								
2005	13,282	38.87	10.14	50.99	6,773	13.29	31.10	55.61
2006	11,937	38.60	7.42	53.98	5,684	12.75	27.90	59.35
2007	5,339	40.88	6.81	52.31	2,583	12.04	27.78	60.18
2008								
2009	10,194	49.10	10.21	40.69	3,715	15.44	34.41	50.16
2010	11,292	53.83	8.44	37.73	4,788	15.92	33.15	50.93
2011	10,076	57.39	6.46	36.15	4,152	18.01	29.08	52.92
2012	9,333	56.21	6.21	37.58	3,909	15.84	30.73	53.43
2013	8,695	58.39	5.07	36.54	3,248	16.18	31.75	52.08

Source: SIPP and author's calculation. Sample period: 1984 - 2013 except 1990, 1996, 2001, and 2008
This table shows the number of observation and the spike at zero, the fraction of workers with wage cuts and raises for hourly paid job stayers and job switchers.

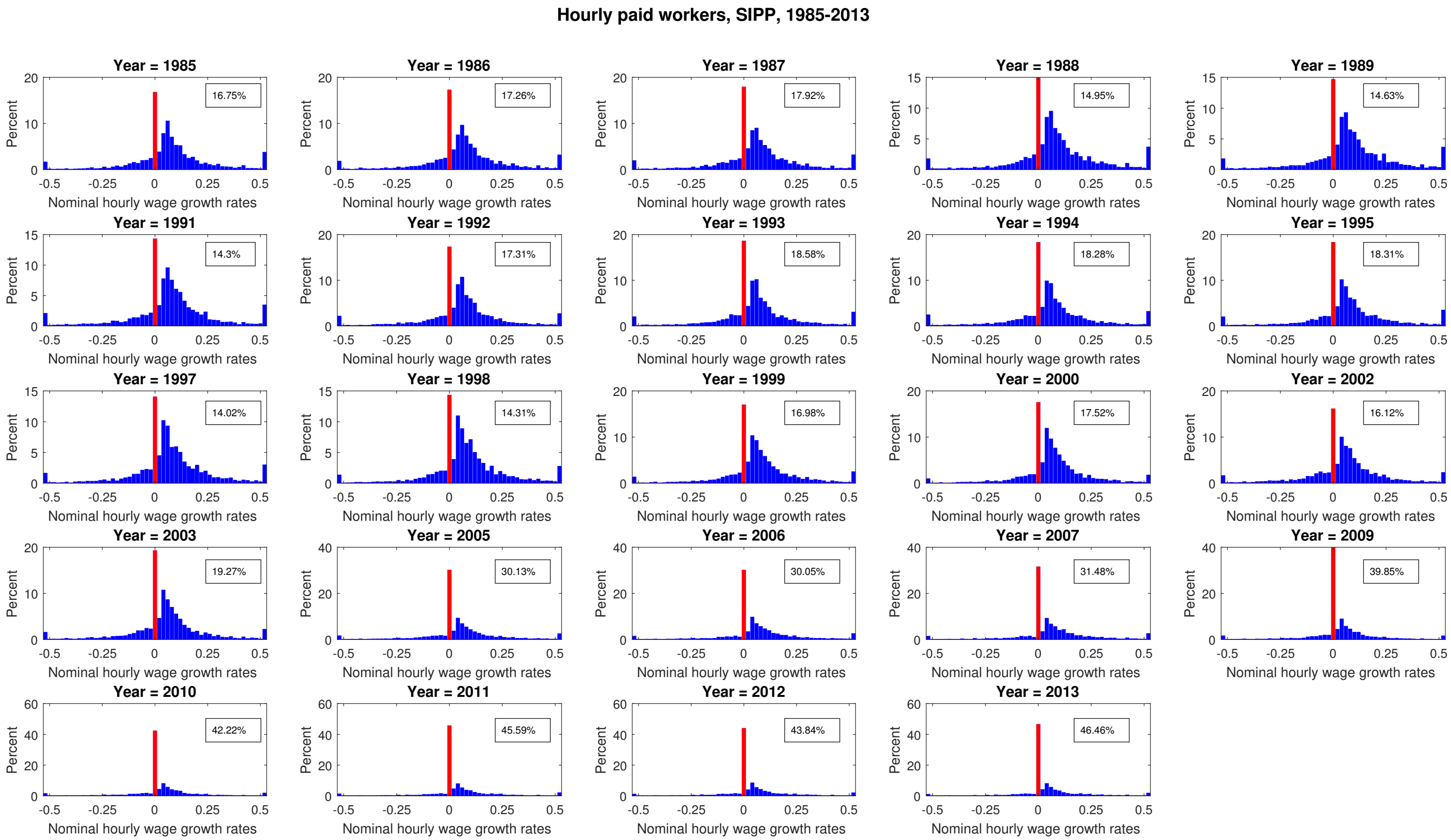

Figure A4: Nominal hourly wage growth rates 1985-2013

Data source: SIPP and author's calculation. The red bin shows the spike at zero, the percentage of workers whose hourly wage growth rate is exactly zero. Other than red bin, the width of the bin is 0.02.

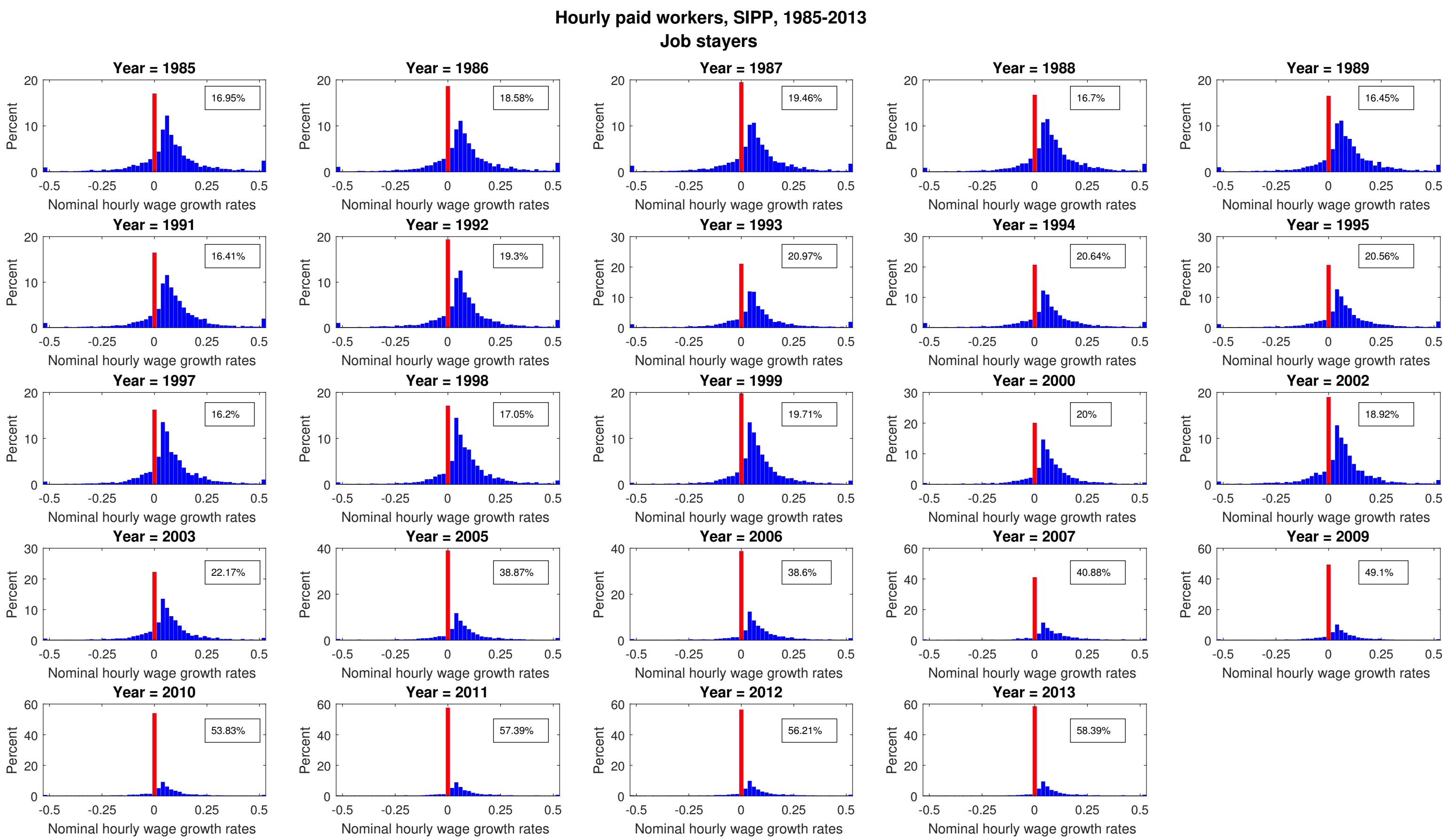

Figure A5: Nominal hourly wage growth rates 1985-2013 for job stayers

Data source: SIPP and author's calculation. For hourly rated job stayers. The red bin shows the spike at zero, the percentage of workers whose hourly wage growth rate is exactly zero. Other than red bin, the width of blue bin is 0.02.

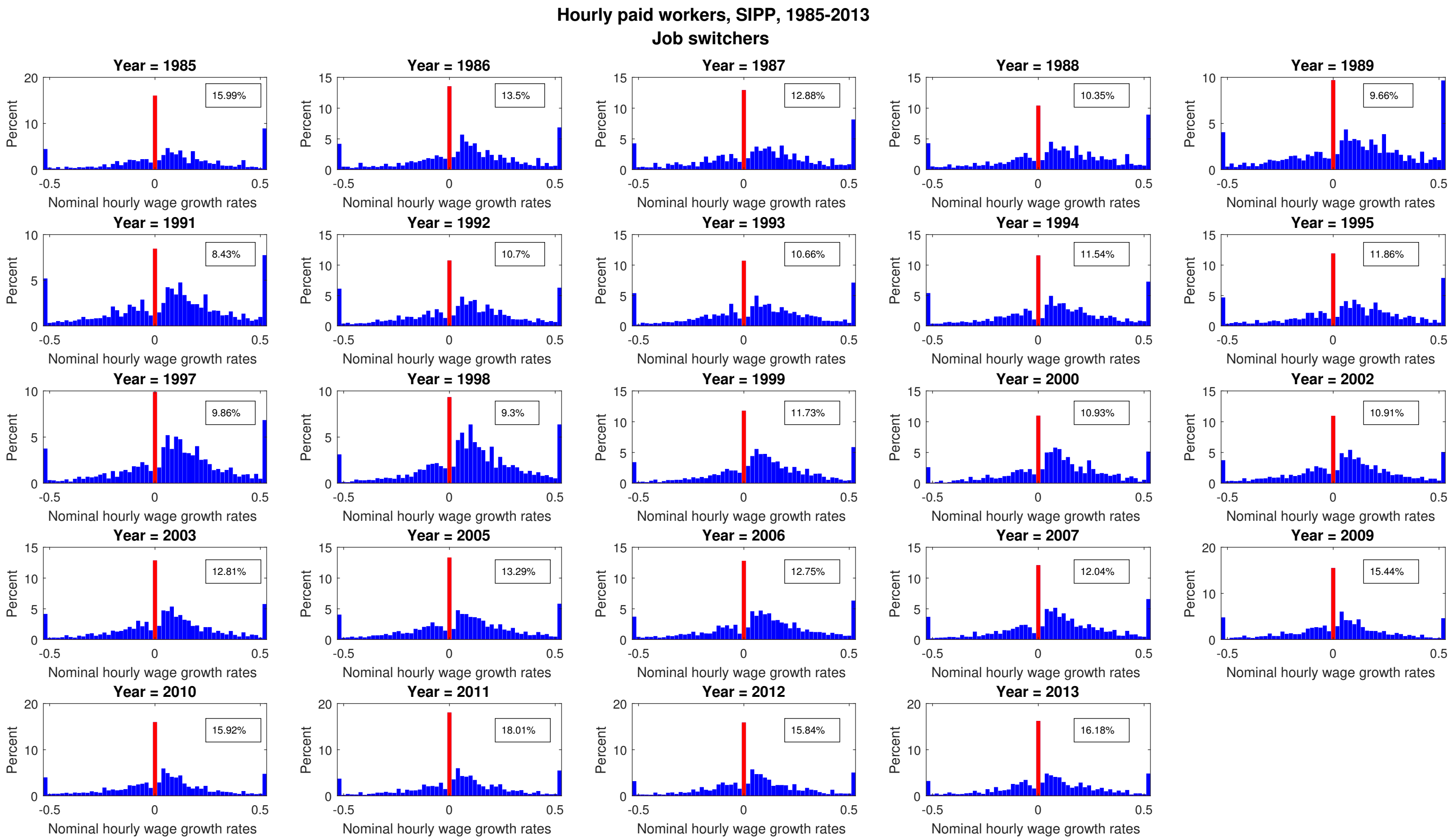

Figure A6: Nominal hourly wage growth rates 1985-2013 for job switchers

Data source: SIPP and author's calculation. For hourly rated job switchers. The red bin shows the spike at zero, the percentage of workers whose hourly wage growth rate is exactly zero. Other than red bin, the width of blue bin is 0.02.

A.2.2 The nominal wage change distribution for job switchers by reasons of job switching

This section reports the average spike at zero, the share of wage cuts and increases for job switchers by reasons of job switching. SIPP asks the reasons why respondents have stopped working for the previous employer. About 50 percent of job switchers do not respond to this question. Among the other 50 percent, workers on layoff, or injured, or temporary workers record the higher spike at zero.

Fired/Discharged workers presents the similar level of the spike at zero compared to workers who quit the job to take another jobs. However, workers who quit the job to take the another job tend to have higher fraction of raises and the less share of cuts. Fired or discharged workers tend to show the higher share of wage cuts. A15

Table A15: The spike at zero, the fraction of wage cuts, and raises for job-switchers by reasons of switching, SIPP

	% of job switchers	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
On layoff	11.53	14.06	37.05	48.89
Fired/Discharged	2.35	9.96	43.98	46.07
Quit to take another job	8.27	9.33	22.89	67.78
Contingent worker/temporary employed	4.22	14.38	29.97	55.65
Illness/Injury	1.26	14.26	38.69	47.05
Others	19.54	12.17	32.79	55.04
Missing	52.82	12.23	27.79	59.98

Data source: SIPP and author's calculation. Sample Period: 1984-2013 (except 1990, 1996, 2001, 2004, 2008). This table shows the sample average of the spike at zero and the fraction of workers with wage cuts and raises over time by reasons of job switching. The category others include attending schools, childcare problems, family/personal obligations, unsatisfactory work arrangements, retirement and so on.

A.3 Counterfactual analysis: Missing mass

Lack of nominal wage cuts compared to nominal wage increases is often suggested as the existence of DNWR. To measure how absent of nominal wage cuts in the nominal wage

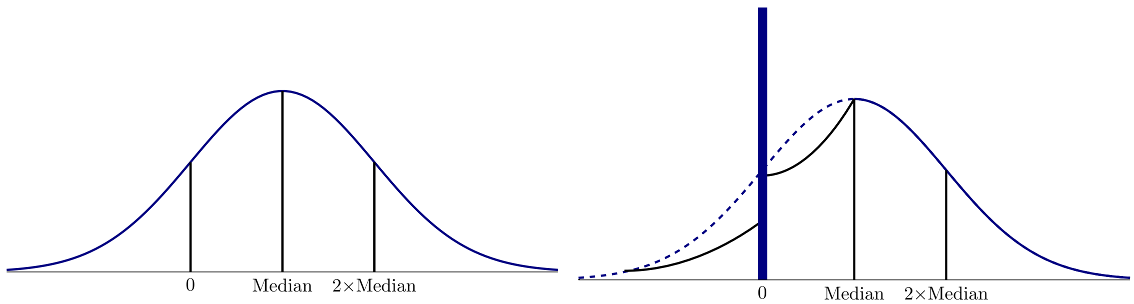

Figure A7: Conceptual diagram of nominal wage distribution

Left panel shows the nominal wage change distribution under the assumption in the absence of wage rigidity and the right panel shows how nominal wage change distribution looks like

growth distribution, this paper introduces the concept of missing mass. This concept is often used to show the asymmetry of wage change distribution in the previous literature, Card and Hyslop (1996), Lebow et al. (2003), and Kurmann and McEntarfer (2017).

To define missing mass, let us assume that nominal wage growth rate distribution is symmetric around the median without any types of wage rigidity, which is shown as the left panel of Figure A7. However, instead of symmetric distribution around the median, what we can observe in the data is that an apparent peak at zero wage change and shortages of wage growth rates around the zero compared to nominal wage change distribution above median, displayed at the right panel of Figure A7. An apparent peak at zero, referred as the spike at zero in this paper, can be decomposed into two: one is the share of workers with no wage change who would have otherwise wage cut without wage rigidity and the other is the share of workers with zero wage change who would have positive wage growth rate in the absence of wage rigidity. The red colored area left to the zero in Figure A8 shows the missing share of wage cuts due to wage rigidity, which becomes the part of the spike at zero. The blue colored area right to the zero in Figure A8 represents the lack of share of raises due to wage rigidity, which becomes part of the spike at zero. From now on, this paper refers the red shaded area as the missing mass left to the zero and the blue shaded

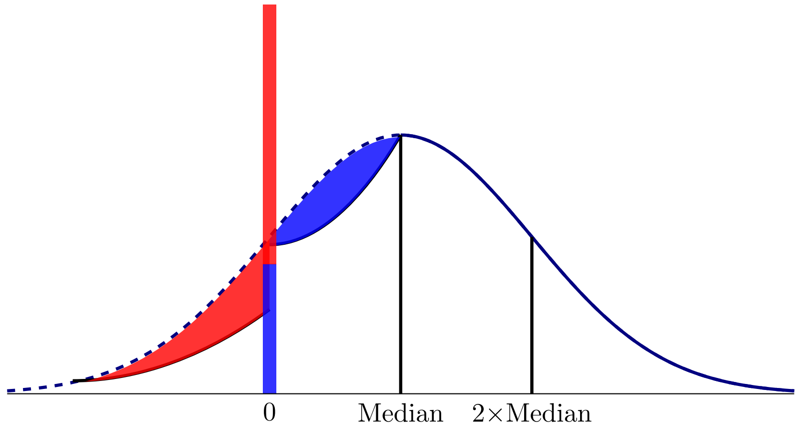

Figure A8: Missing mass left to the zero vs. missing mass right to the zero

area as the missing mass right to the zero.

Formally, we can write the missing mass left to the zero as

$$\frac{\Sigma_i 1(\Delta w > 2 \times \text{Med}) - \Sigma_i 1(\Delta w < 0)}{N} \tag{A.2}$$

and the missing mass right to the zero can be written as

$$\frac{\Sigma_i 1(\text{Med} < \Delta w \leq 2 \times \text{Med}) - \Sigma_i 1(0 < \Delta w \leq \text{Med})}{N}. \tag{A.3}$$

Table A16 shows missing masses calculated using the equation A.2 and A.3. We can clearly see the most of missing mass comes from the left using the CPS and the SIPP. In the CPS, 85 percent of the spike at zero comes from the left to the zero. In the SIPP, 90 percent of the spike at zero for job stayers comes from the left to the zero and 87 percent of the spike at zero comes from the left to the zero for job switchers.

Table A16: Missing mass from left to the zero vs. right to the zero

	CPS		
	Spike at zero	Missing mass from left to zero	Missing mass from right to zero
Hourly workers	15.25	12.97	2.15
	SIPP		
	Spike at zero	Missing mass from left to zero	Missing mass from right to zero
Job-stayer	23.74	21.25	2.49
Job-switcher	12.19	10.58	1.61

Data source: CPS, SIPP, ans author's calculation. Sample period for CPS: 1979 - 2017. Sample period for SIPP: 1984-2013 (except 1990, 1996, 2001, 2004, and 2008)

Appendix B

Appendix to chapter 2

B.1 Solution Algorithm

- Step 1: Guess a parameterized functional form of H and choose the initial parameter, γ_0, γ_1, and γ_2.

$$
\begin{aligned}
W_{t+1} &= H(W_t, M_{t+1}) \\
\ln(\frac{W_{t+1}}{W_t}) &= H(\ln(\frac{M_{t+1}}{W_t})) = \gamma_0 + \gamma_1 \ln \frac{M_{t+1}}{W_t} + \gamma_2(\ln \frac{M_{t+1}}{W_t})^2
\end{aligned}
$$

- Step 2 : Solve the wage setter's optimization problem $V_t(q_t(i), L_t, \frac{w_{t-1}(i)}{W_t}, x_t)$, given the law of motion H.

- Step 3 : Simulate the dynamics of the cross-sectional distribution for finite households for T periods using the policy function obtained by step 2.

- Step 4 : Construct a time series for wage inflation. Burn first initial periods and estimate the parameters γ_0, γ_1, and γ_2.

– Calculate simulated wage inflation, $\ln(\frac{W_{t+1}^S}{W_t})$,

$$\frac{W_{t+1}}{W_t}^S = \frac{\left\{\int \left[\frac{w_{t+1}(i)}{q_{t+1}(i)}\right]^{1-\theta} dj\right\}^{\frac{1}{1-\theta}}}{\left\{\int \left[\frac{w_t(i)}{q_t(i)}\right]^{1-\theta} dj\right\}^{\frac{1}{1-\theta}}}$$

$$\approx \left[\frac{\Sigma_j \left[\frac{w_{t+1}(i)/W_{t+1}}{q_{t+1}(i)}\right]^{1-\theta}}{\Sigma_j \left[\frac{w_t(i)/W_{t+1}}{q_t(i)}\right]^{1-\theta}}\right]^{\frac{1}{1-\theta}}$$

– Estimate parameters using the OLS

$$\ln(\frac{W_{t+1}}{W_t}^S) = H(\ln(\frac{M_{t+1}}{W_t})) = \gamma_0 + \gamma_1 \ln\frac{M_{t+1}}{W_t} + \gamma_2(\ln\frac{M_{t+1}}{W_t})^2$$

- Step 5: Update γ_0, γ_1, and γ_2 using the OLS. Iterate from Step 2 to Step 5 until the parameters converge.

- Step 6: Test the goodness of fit for H using R^2.

B.2 Sensitiveness

B.2.1 Menu cost model

In the menu cost model, two parameters, the probability of facing the menu-cost to change their wage (μ^{Menu}) and the fixed cost (κ) , are calibrated to match the average spike at zero. To keep the average spike at zero fixed, as μ^{Menu} increases, the fixed cost, κ, decreases, so does inaction region. In the random menu cost model, the spike at zero is the proportion of the inaction region. Table B1 shows that menu cost model implies greater responsiveness of the share of workers with wage cuts by varying μ^{Menu} from 0.3 to 1.

Table B1: The spike at zero, the fraction of wage cuts, and raises along the business cycles by varing menu cost, K, and μ^{Menu}

			The responsiveness to employment		
		The average	(1)	(2)	(3)
		Spike at zero	Spike at zero	Fraction of	Fraction of
μ^{Menu}	K	(%)	$\Delta W = 0$	$\Delta W < 0$	$\Delta W > 0$
1	0.0010	23.200	-0.120	-0.336	0.456
0.9	0.0012	23.035	-0.165	-0.333	0.498
0.8	0.0015	23.085	-0.187	-0.329	0.516
0.7	0.0020	23.205	-0.210	-0.358	0.568
0.6	0.0003	23.100	-0.210	-0.292	0.502
0.5	0.0004	23.000	-0.142	-0.353	0.495
0.4	0.0075	23.100	-0.164	-0.391	0.555
0.3	0.0190	23.164	-0.037	-0.469	0.506

This table shows the responsiveness of the spike at zero, the share of workers with wage cuts, and raises by varing parameters of menu-cost model, μ^{Menu} and K.

B.2.2 DNWR model

As the parameter governing the degree of DNWR(μ^{DNWR}) increases, model predicts the higher degree of DNWR. When employment declines, the optimal nominal wage change distributions shift to the left. For those workers whose optimal wages are lower than the previous wages, μ^{DNWR} fraction of workers cannot change their wages and the other $(1 - \mu^{\text{DNWR}})$ fraction of workers would experience wage cuts. Thus, we can expect that as μ^{DNWR} increases, the average spike at zero increases and the average share of wage cuts decreases, which is shown at Table B3 and Figure B1. Similarly, the degree countercylicality of the spike at zero increases as μ^{DNWR} increases, which is shown at Table B2.

Lowering the persistence of idiosyncratic shock to $\rho_q = 0.3$ does not make changes in the average wage change distribution. The second panel of Table B5 shows the similar level of the average spike at zero and the share of workers with wage cuts and raises. On the contrary, increasing σ_q raises the level of spike at zero and the share of wage cuts, shown at Table B5. Table B4 shows that as long as μ^{DNWR} is the same, the degree of higher responsiveness of the spike at zero compared to the share of wage cut is the same, the ratio of two coefficients from the regression of the spike at zero on employment to the that of the share of wage cuts on employment.

Table B2: The spike at zero, the fraction of wage cuts, and raises along the business cycle by varying μ^{DNWR}

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
	Data		
Employment	-0.616	-0.305	0.921
Inflation	-1.181	-0.674	1.855
	DNWR ($\mu = 0.3$) model		
Employment	-0.194	-0.429	0.623
Inflation	-1.467	-3.365	4.832
	DNWR ($\mu = 0.5$) model		
Employment	-0.440	-0.373	0.813
Inflation	-2.658	-2.517	5.176
	DNWR ($\mu = 0.67$) model		
Employment	-0.712	-0.329	1.041
Inflation	-3.699	-1.772	5.470
	DNWR($\mu = 0.9$) model		
Employment	-1.456	-0.144	1.600
Inflation	-5.124	-0.574	5.698

Data source: CPS and author's calculation. Sample Period: 1979-2017 (except 1995). Inflation rate is calculated from CPI-U.

Table B3: Data and model generated moments, varying μ^{DNWR}

	Wage growth rates	Employment growth rates	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
		DNWR ($\mu = 0.3$) model			
Mean	4.373	0.000	10.092	20.290	69.618
SD	1.931	0.677	3.350	6.789	9.729
Skewness	0.204	0.021	-	-	-
		DNWR ($\mu = 0.5$) model			
Mean	4.401	0.000	16.681	15.120	68.199
SD	1.769	0.766	5.204	4.757	9.749
Skewness	0.203	-0.017	-	-	-
		DNWR ($\mu = 0.67$) model			
Mean	4.381	0.000	23.026	10.531	66.443
SD	1.645	0.812	6.820	3.219	9.902
Skewness	0.320	-0.061	-	-	-
		DNWR ($\mu = 0.9$) model			
Mean	4.345	0.000	32.994	3.495	63.510
SD	1.510	1.045	9.303	1.052	10.310
Skewness	0.448	-0.077	-	-	-

Data source: CPS and author's calculation. Sample Period: 1980-2017 (except 1995). Wage growth rate is average of the median hourly wage growth rate for hourly paid workers from 1980 - 2017.
model generated moments are from stat.m

Table B4: The spike at zero, the fraction of wage cuts, and raises along the business cycle by varying idiosyncratic shock

	(1) Spike at zero $\Delta W = 0$	(2) Fraction of $\Delta W < 0$	(3) Fraction of $\Delta W > 0$
DNWR ($\mu = 0.67, \rho_q = 0.821, \sigma_q = 0.17$) model			
Employment	-0.712	-0.329	1.041
Inflation	-3.699	-1.772	5.470
DNWR ($\mu = 0.67, \rho_q = 0.3, \sigma_q = 0.17$) model			
Employment	-1.605	-0.680	2.285
Inflation	-3.319	-1.637	4.956
DNWR ($\mu = 0.67, \rho_q = 0.821, \sigma_q = 0.254$) model			
Employment	-0.447	-0.200	0.647
Inflation	-2.740	-1.339	4.079

Data source: CPS and author's calculation. Sample Period: 1980-2017 (except 1995). Inflation rate is calculated from CPI-U.

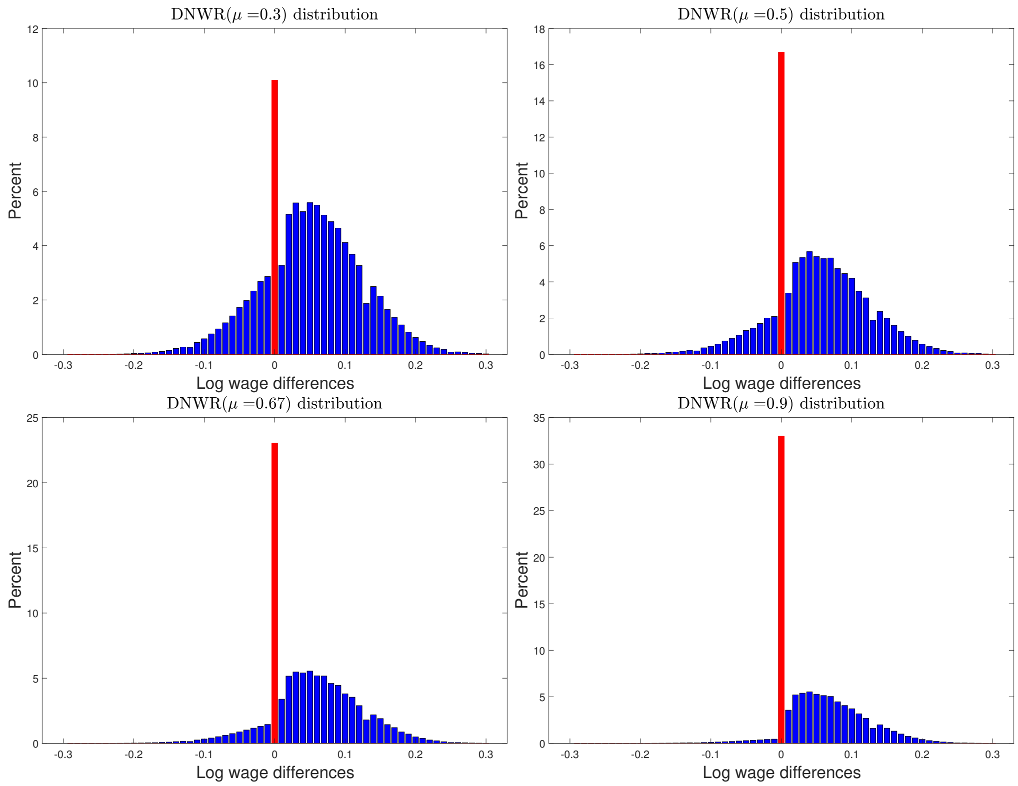

Figure B1: Staionary wage change distribution by varying μ^{DNWR}

Table B5: Data and model generated moments by varying idiosyncratic shock

	Wage growth rates	Employment growth rates	Spike at zero $\Delta W = 0$	Fraction of $\Delta W < 0$	Fraction of $\Delta W > 0$
DNWR ($\mu = 0.67, \rho_q = 0.821, \sigma_q = 0.17$) model					
Mean	4.381	0.000	23.026	10.531	66.443
SD	1.645	0.812	6.820	3.219	9.902
Skewness	0.320	-0.061	-	-	-
DNWR ($\mu = 0.67, \rho_q = 0.3, \sigma_q = 0.17$) model					
Mean	4.380	0.000	23.762	11.166	65.073
SD	1.633	0.920	6.331	3.079	9.364
Skewness	0.288	0.023	-	-	-
DNWR ($\mu = 0.67, \rho_q = 0.821, \sigma_q = 0.254$) model					
Mean	4.382	0.000	29.305	13.693	57.002
SD	1.576	1.119	4.934	2.370	7.153
Skewness	0.230	-0.038	-	-	-

Data source: CPS and author's calculation. Sample Period: 1980-2017 (except 1995). Wage growth rate is average of the median hourly wage growth rate for hourly paid workers from 1980 - 2017.
model generated moments are from stat.m

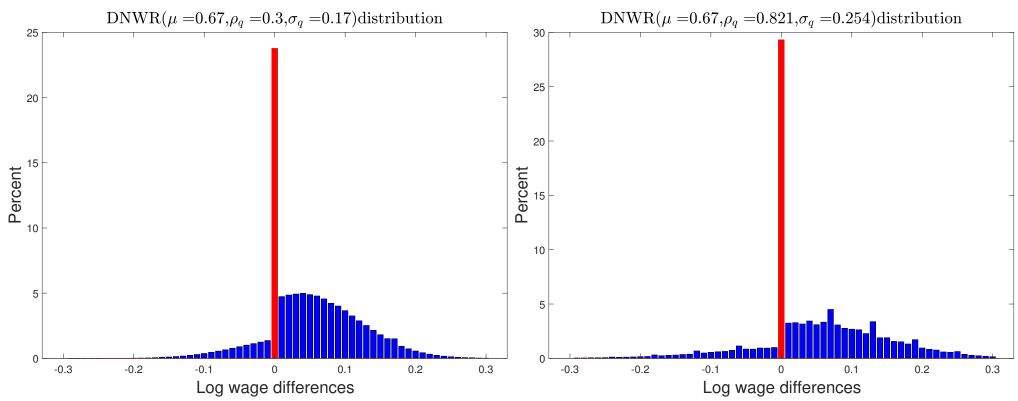

Figure B2: Staionary wage change distribution by varying idiosyncratic productivity shock process

Appendix C

Appendix to chapter 3

C.1 OLS regression results

Table C1: OLS Estimates Over Period 1991-2001

Dependent Variable	(1) Δp_{ict}	(2) Δp_{ict}	(3) Δp_{ict}	(4) Δp_{ict}
Lagged Price	-0.292***	-0.325***	-0.311***	-0.127***
	(0.026)	(0.027)	(0.024)	(0.011)
E-Commerce Intensity	-0.179		-0.033	-0.040
(t=2009) × Lagged Price	(0.269)		(0.251)	(0.114)
E-Commerce Intensity		-1.126***	-1.222***	-0.413***
× Lagged Price × Post Rakuten		(0.321)	(0.329)	(0.101)
t	{1996}	{2001}	{1996,2001}	Annual 1992-2001
Observations	25,848	27,407	51,012	152,416
R^2	0.52	0.52	0.52	0.51
E-Commerce Intensity Year	2009	2009	2009	2009

Source: RPS, NSFIE, and authors' calculation. Notes: Table shows regression results of equation (3.1) using OLS estimation method. The first column uses the five-year log differences in prices from 1991 - 1996 and the second column uses that from 1996 - 2001. The third column uses two-five year period 1991 - 1996 and 1996 - 2001. The 2SLS regression results are available from Table 3.5.

Table C2: OLS Estimates Over Alternative Periods

Dependent Variable	(1) Δp_{ict}	(2) Δp_{ict}	(3) Δp_{ict}	(4) Δp_{ict}	(5) Δp_{ict}
Lagged Price	-0.391***	-0.460***	-0.389***	-0.390***	-0.156***
	(0.020)	(0.026)	(0.019)	(0.016)	(0.009)
E-Commerce Intensity	0.638**	1.076***	0.477**	0.485**	0.171
(t=2009) $\times$ Lagged Price	(0.275)	(0.277)	(0.233)	(0.213)	(0.109)
E-Commerce Intensity	-1.319***	-2.458***	-1.380***	-1.297***	-0.553***
$\times$ Lagged Price $\times$ Post Rakuten	(0.250)	(0.270)	(0.321)	(0.211)	(0.086)
t	{1996,2006}	{1996,2011}	{1996,2016}	{1996,2001, 2006,2016}	Annual 1992-2016
k	5	5	5	5	
Observations	51,845	43,256	42,555	87,515	393,246
R^2	0.53	0.58	0.63	0.60	0.46
E-Commerce Intensity Year	2009	2009	2009	2009	2009

Source: RPS, NSFIE, and authors' calculation. Notes: Table shows regression results of equation (3.1) using OLS estimation method. First three columns compare the five-year period of log price differences before (1991-1996) and after the entry of e-commerce firms (2001-2006, 2006-2011, and 2011-2016). The last column uses the annual frequency of log price changes. The 2SLS regression results are available from Table C2.