Cyclical Labor Costs Within Jobs[†]

Online Appendix

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Appendix A. Further description of the data and sample

In what follows we give some additional details regarding the datasets used and how we have constructed sub-samples thereof. All the relevant documentation and variable descriptions attached to these datasets are publicly available from the UK Data Service. The ONS has also published various documents concerning the data quality and consistency of the ASHE. We will publish the replication files for the analysis and sample construction.

We focus on methodological details through the period 1998-2016. Throughout this period, the ASHE should be a random sample of all employees, irrespective of occupation, size of employer etc. Given the legal obligation of employers to respond using payrolls, it has a high response rate and is believed to be accurate. There is no cumulative attrition from the panel, as any individual not included in the ASHE in any year, for whatever reason, remains in the sampling frame the following year. Conditional on a hundred percent response, the ASHE is a true one percent random sample of employees: all those with a National Insurance Number which has a numerical part ending in 14. However, there are two major sources of under-sampling, both occurring if individuals do not have a current tax record. This could happen for some individuals who have very recently moved job, or for those who earn very little (mostly part-time) and are not paying income tax or National Insurance in the period when their employers are looked up. From 2004, the ASHE aimed to sample some of those employees under-represented. It added supplementary responses for those without a PAYE reference, and also attempted to represent employees whose jobs changed between the determination of the sampling frame in January and the reference period in April. Since the ONS states that the biases that these amendments were introduced to address were small, we do not believe they could affect the results substantially. The ASHE also introduced some imputations, using similar matched 'donor' observations where responses were, for example, missing an entry of basic hours but had recorded pay. These imputations were added for weighting purposes, but throughout the analysis we ignore the weights in the ASHE, since they are designed to make the aggregate results population representative in terms of worker observables and are not firm level.

From 2005, a new questionnaire was also introduced, which was intended to reduce the latitude for respondents' own interpretations of what was being asked of them. From 2007, there were further notable changes. Before occupations were classified as follows: if the respondent stated an employee's job had not changed in the past year the previous year's occupational classification was applied - otherwise, it was manually coded. Afterwards

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an automatic coding, text recognition, tool was used: "The effect of using ACTR was to code more jobs into higher paying occupations. The jobs that tended to be recoded into these higher paying occupations generally had lower levels of pay than the jobs already coded to those occupations. Conversely, they tended to have higher levels of pay than the other jobs in the occupations that they were recoded out of. The impact of this was to lower the average pay of both the occupation group that they had moved from and that they had moved to." (ONS). From 2007, the sample size of the ASHE was reduced by 20 percent, with reductions targeted at those industries exhibiting the least variation in earnings patterns.

We use the ASHE annual cross-sections for each year from 1998 to 2016 and construct a panel as follows: first, we merge the two separate cross-sections for the year 2010, where one contains occupations coded in SOC2010 and the other in SOC2000. This is done to match occupations across classification schemes for the same individuals. In case of multiple jobs per individual, we exclude non-main jobs. In case of missing main job markers, we impute these based on the job with the highest working hours. In a next step we link employees across consecutive years based on their unique identifiers. This enables us to impute missing enterprise reference numbers (entrefs) backwards, since the ASHE contains a variable which indicates whether an employee is holding the same job as in the last reference period. Note that this "same job" variable alone does not allow between-firm and within-firm job changers to be distinguished. After linking two consecutive years in this way, we use local unit identifiers to impute missing entrefs across individuals within the same year (the ONS states that the local unit identifiers are not consistent across years, rather they are created to identify establishments within years). We continue to update missing entrefs in this way back to and including 1998. The number of observations with non-missing entrefs after imputation declines rapidly as we go further back in time. While for the years 2003-16 we are only adding a couple of missing entrefs per year, prior to 2003, and especially prior to 2000, we are imputing almost all entrefs. We could also impute entrefs for 1997, but this year does not include the marker that indicates whether an individual is working in the same job, which is vital to the sample selection strategy.

We keep only observations for individuals aged 16-64, and which have not been marked as having incurred a loss of pay in the reference period through absence, employment starting in the period, or short-time working, and which are marked as being on an adult rate of pay (i.e. dropping trainees and apprenticeships). This is practically the same filter applied for annual ONS published results on UK "Patterns of Pay" using the ASHE. We drop observations with missing basic hours, gross weekly earnings, or hourly wage rates. Basic hours are intended to be a record for an employee in a normal week, excluding overtime and meal breaks. Gross weekly pay is the main recorded value in the survey, and from this overtime records are subtracted. Hourly rates are then derived from dividing by basic hours worked. We drop observations with over a hundred or less than one basic hour worked, as these could reflect measurement error and the inclusion of overtime. Full-time is defined as working over thirty basic hours in a week. But there are a tiny number of discrepancies in some years, we believe relating to teaching contracts, where the definition applied by the ONS differs. We however recode these such that for all observations the thirty hours threshold applies. To further address some potential for measurement error, especially in the recorded basic hours, we drop observations whose derived hourly rates of pay, excluding overtime, are less than 80 percent of the applicable National Minimum Wage (NMW) each April, with allowance

for the different age-dependent rates of the NMW over time. We set the threshold lower to avoid dropping observations where employers have rounded figures about the NMW, where the degree of rounding could vary with the actual value of the NMW, a behavior which has been hypothesized by the ONS. Since the data are not top-coded, we drop the highest one percent of all weekly or hourly earners.

We define an entry or new hire into a firm as an individual with less than one year of tenure. For this we make use of the employment start date. The ASHE contains information on when an employee started working for an enterprise from 2002 onward. We drop a tiny number of unrealistic entry dates, where the start date lies either in the future or implies an employee started work aged fifteen or younger. Unfortunately, there are some inconsistencies across years in these records. First, an employee can be employed by the same company for three consecutive years, holding the same job, but the starting dates recorded in the first and third years, though identical, can vary from the second. In this case we update the "one-off" deviation with the value of the previous year. Second, if we observe an employee in a chain of consecutive years in the same firm, holding the same job, but the start date differs for some years, then we impute the earliest date available. This decision is based on a conservative interpretation of a "new hire": in case of previous employment within the same firm, we do not include an employee in the CH-firms sample of new hires if we are in any doubt. Given the main finding is that hiring earnings cyclicality is larger in absolute terms than that of job stayers, any expected bias would go in the opposite direction. Finally, we use the employment start dates to impute entrefs for employees backwards again. This enables us to no longer have to observe employees in a chain of consecutive years to make imputations. We then again use within-year local unit identifiers to update longitudinal entrefs within a year for other employees with missing entrefs. The ASHE contains the number of employees of an enterprise as listed in the Inter-Departmental Business Register (IDBR). A very small fraction of employees in the same enterprise and year have missing or varying values for this variable. We impute the same value for all employees within a year and an enterprise as the modal value for the firm.

For 1996-2001, occupations are classified using the three-digit ONS1990 Standard Occupational Classification (SOC). For 1998-2010, occupations are classified using the four-digit SOC2000, and for 2011-2016 with the SOC2010. We experimented using the ONS' publicly available cross-walk from 2010 and 2000 but discovered that this causes a large structural break in the distribution of occupations. It causes a substantial additional degree of polarization of work from 2002 onward. Therefore, we use our own cross-walk obtained from the ASHE cross-section 2010, as discussed above, to map SOC2010 into SOC2000 *within* an enterprise. However, some occupations for some firms are not observed in the year 2010, but are in the following years, for which we do not have dual coded data. To address this, we first convert SOC2010 to the 2008 International Standard Classification of Occupations (ISCO), obtained from the ONS website. Then we convert SOC2000 to ISCO1988, where we obtain conversion tables from the Cambridge Social Interaction and Stratification Scale (CAMSIS) project. Finally, we use the ISCO2008 to ISCO1988 cross-walk, available from the International Labour Organization. For the industry classification, we convert ONS Standard Industrial Classification (SIC) 2007 to 2003, using files made available by the UK Data Service. This conversion uses the 2008 Annual Respondents Dataset, where both classifications were applied, and where any 2007 code mapping to multiple 2003 codes is decided using whichever of the two bore a greater share of economic output.

Appendix B. Further robustness checks

Table B1 presents some further robustness checks of the main empirical results presented in Table 3. The first row repeats the baseline/main result for convenience. The robustness discussed here is with regards to the specification of the first step of the regression models: (1) & (3). The specification of the second step is unchanged compared with the baseline. The second row describes the estimated semi-elasticity of real wages and hours with respect to the unemployment rate when typical job-level measures of wages and hours are employee sample means, rather than median values. Qualitatively the results are unchanged: wages for hires and job stayers exhibit a sizable and significant cyclical response, as do hiring hours. We prefer the median as a measure of the typical wage because it is less sensitive to changes in the extent of sampling error within jobs over time, given the specific sample selection criteria for jobs. However, for robustness, rows 3 and 4 show estimates using the 25th and 75th percentiles of within job wages or hours as the first-step dependent variables, respectively. For wages, the responses are qualitatively unchanged, though the estimated response to the unemployment rate at the 75th percentile is slightly higher, reflecting that these hires were less constrained cyclically by the National Minimum Wage. The estimated hours response for job stayers remains small in magnitude at both percentiles within jobs, though it is statistically significant from zero at the 75th percentile. The hiring hours response was insignificant for hires with relatively low hours within jobs, i.e. part-time. However, at the 75th percentile within jobs, the hiring hours response to the business cycle is greater than at the median. The fifth row removes all controls for time-varying job characteristics from the first step. In doing so we would expect to underestimate the cyclical response of wages because of a procyclical composition bias along some observable characteristics. However, the results here show that those observables that we do control for at the job level, namely gender, union coverage, age and firm size, are collectively not important in this regard. The sixth row includes jobs from the public sector. The main findings are qualitatively unchanged. The hiring hours in public sector entry-level jobs were somewhat more responsive to the unemployment rate than in the private sector, potentially reflecting the squeeze on labor costs imposed by fiscal austerity. The seventh row simply illustrates the difference in results when we use an alternative price deflator. The RPI notably includes the cost of housing, including mortgage interest payments, whereas the CPI does not. Interest rates were cut during the Great Recession, and so the RPI is itself more cyclical than the CPI. Hence the measured real RPI-wage cyclicality is smaller, though still significant. We prefer the CPI because it is more internationally comparable and is the basis of the Bank of England's inflation target.

TABLE B1: Estimated semi-elasticity of real wages and hours with respect to the unemployment rate, 1998-2016: more robustness checks

	Wages		Hours	
	New hires (1)	Job stayers (2)	New hires (3)	Job stayers (4)
1. Baseline	-2.56^{***}	-2.60^{**}	-1.71^{***}	-0.10
	(0.94)	(1.13)	(0.47)	(0.18)
2. Job means	-2.65^{***}	-2.67^{**}	-1.21^{***}	-0.28
	(0.96)	(1.17)	(0.39)	(0.18)
3. 25th percentile	-1.98^{**}	-2.69^{**}	-0.21	-0.35
	(0.82)	(1.23)	(0.53)	(0.43)
4. 75th percentile	-3.27^{***}	-2.76^{**}	-2.24^{**}	-0.42^{***}
	(1.11)	(1.16)	(0.99)	(0.14)
5. Baseline, but without controls	-2.56^{***}	-2.61^{**}	-1.87^{***}	0.02
	(0.86)	(1.10)	(0.41)	(0.21)
6. Baseline, but including public sector	-2.32^{**}	-2.62^{**}	-2.07^{***}	-0.41^{***}
	(1.01)	(1.31)	(0.33)	(0.10)
7. RPI instead of CPI	-1.96^{***}	-1.97^{**}		
	(0.71)	(0.90)		

Notes.- second-step regression results of estimated period effects on the unemployment rate, $\hat{\gamma}$. First row is identical to Table 3, included here for comparison. Second row uses mean wages or hours in jobs as the dependent variable in the first step. Third row uses the 25th percentiles of wages and hours within jobs as the dependent variable in the first step. Fourth row uses the 75th percentiles of wages and hours within jobs as the dependent variable in the first step. Fifth row excludes all time-varying controls from the first step. Sixth row includes public sector firms in the analysis. Seventh row uses the Retail Price Index, instead of the Consumer Price Index, to deflate wages.

Newey-West standard error estimates robust to first-order serial correlation in parentheses.

*** Statistically significant at the 1% level; ** at the 5% level, two-sided tests.

TABLE B2: Estimated semi-elasticity of real wages and hours with respect to the unemployment rate, 1998-2016: sample selection robustness - varying the minimum number of employees per job-year required for inclusion in the CH-firms sample

	Wages		Hours	
Min. hires requirement	New hires (1)	Job stayers (2)	New hires (3)	Job stayers (4)
2 employees	-2.42^{***}	-2.66^{**}	-1.18^{***}	-0.26^{**}
	(0.83)	(0.94)	(0.35)	(0.11)
3 (baseline)	-2.56^{***}	-2.60^{**}	-1.71^{***}	-0.10
	(0.94)	(1.13)	(0.47)	(0.18)
4	-2.63^{***}	-2.46^{***}	-2.08^{***}	-0.11
	(0.83)	(0.91)	(0.54)	(0.15)
5	-2.84^{***}	-2.22^{**}	-1.97^{***}	0.01
	(0.80)	(0.89)	(0.55)	(0.17)
6	-2.88^{***}	-2.15^{***}	-2.21^{***}	-0.28
	(0.77)	(0.80)	(0.67)	(0.17)
7	-2.85^{***}	-2.25^{**}	-2.23^{***}	-0.38
	(0.78)	(0.88)	(0.58)	(0.21)
8	-2.72^{***}	-2.27^{**}	-2.04^{***}	-0.56^{***}
	(0.73)	(0.94)	(0.77)	(0.21)
9	-2.70^{***}	-2.41^{***}	-2.66^{***}	-0.55
	(0.70)	(0.93)	(0.90)	(0.23)
10	-2.81^{***}	-2.01^{***}	-2.61^{***}	-0.49
	(0.63)	(0.75)	(0.95)	(0.25)

Notes.- second-step regression results of estimated period effects on the unemployment rate, $\widehat{\gamma}$. Each row gives results varying the minimum number of employees per job-year required for selection into the analysis sample. "3 (baseline)" is identical to Table 3 row 1, included here for comparison.

Newey-West standard error estimates robust to first-order serial correlation in parentheses.

*** Statistically significant at the 1% level; ** at the 5% level, two-sided tests.

TABLE B3: Estimated semi-elasticity of the difference in $\widehat{\beta}_t$ series between new hires and job stayers with respect to the unemployment rate/labor productivity, 1998-2016

	Wages	Hours
Baseline sample	−0.35	−1.57***
	(0.31)	(0.43)
Table 3		
2. Including controls for share of full-time workers	−0.27	−0.30
	(0.32)	(0.21)
3. Job hires in at least 25% of years when the firm is observed	−0.02	−0.48
	(0.33)	(0.33)
4. All jobs observed in at least 2 years	0.09	−0.36
	(0.28)	(0.25)
5. Baseline sample, but weighted by number of employees per year	−0.59	−2.23**
	(0.39)	(0.98)
6. Including other pay and hours	−0.44	−1.28***
	(0.30)	(0.41)
7. 3-digit occupations (jobs)	0.04	−0.87***
	(0.35)	(0.31)
Table 5		
2. Baseline with quadratic trend	−0.37	−1.55***
	(0.21)	(0.35)
3. Baseline sample, but weighted by number of jobs per years	−1.00***	−1.20**
	(0.29)	(0.50)
4. First differences (OLS)	0.25	−0.04
	(0.36)	(0.61)
Table 6		
1. Whole economy	−0.12**	0.29***
	(0.06)	(0.10)
2. Services sector	−0.14**	0.31***
	(0.06)	(0.11)

Notes.- "second-step" regression results of estimated period effects on the unemployment rate/labor productivity, $\widehat{\gamma}$, as per (2), where the Dependent variable is instead the difference between hiring and job stayer composition-adjusted period means, $\left[\{\widehat{\beta}_t\}_{\text{New hires}} - \{\widehat{\beta}_t\}_{\text{Job stayers}}\right]$, obtained from associated first-step regressions described by the corresponding rows in Tables 3, 5 and 6.

Newey-West standard error estimates robust to first-order serial correlation in parentheses.

*** Statistically significant at the 1% level; ** at the 5% level, two-sided tests.

Appendix C. Additional tables

TABLE C1: Distribution of new hires over industries, all years 1998-2016

Industry (SIC2003)	Hires	Share
Retail Trade; Repair of Personal and Household Goods (52)	26,792	0.49
Hotels and Restaurants (55)	9,842	0.18
Financial Intermediation, exc. Insurance and Pension Funding (65)	3,821	0.06
Other Business Activities (74)	9,295	0.17
Other	5,467	0.10

Notes.- absolute and frequency distribution of new hires over industries. Shares might not sum to one due to rounding. Classification according to the ONS Standard Industrial Classification 2003.

TABLE C2: Distribution of new hires over occupations, all years 1998-2016

Occupation (ISCO88)	Hires	Share
Customer services clerks (41)	5,468	0.10
Personal and protective services workers (51)	6,561	0.12
Models, salespersons and demonstrators (52)	26,245	0.48
Sales and services elementary (91)	8,202	0.15
Labourers in mining, construction, manufacturing and transport (93)	2,734	0.05
Other	5,467	0.10

Notes.- absolute and frequency distribution of new hires over occupations. Shares might not sum to one due to rounding. Classification according to the ILO International Standard Classification of Occupations 1988.

TABLE C3: Estimated period-fixed effects for real hourly wages ($\widehat{\beta}_t$ from first-step regressions)

	CH-firms		ASHE	
	New hires	Job stayers	New hires	Job stayers
Year	(1)	(2)	(3)	(4)
1999	−0.093***	−0.091***	−0.086***	−0.103***
2000	−0.091***	−0.067***	−0.084***	−0.072***
2001	−0.049	−0.048***	−0.046***	−0.039***
2002	−0.012	−0.011	−0.019	−0.013***
2003	0.000	0.000	0.000	0.000
2004	0.007	0.003	0.008	0.010**
2005	0.025	0.026	0.028**	0.033***
2006	0.022	0.045	0.036***	0.042***
2007	0.036	0.045	0.041***	0.047***
2008	0.009	0.045	0.025**	0.047***
2009	0.031	0.048	0.026**	0.034***
2010	−0.001	0.017	−0.002	−0.001
2011	−0.043	−0.017	−0.027**	−0.022***
2012	−0.067**	−0.047	−0.053***	−0.050***
2013	−0.073**	−0.067	−0.065***	−0.064***
2014	−0.074**	−0.072	−0.077***	−0.067***
2015	−0.053	−0.049	−0.057***	−0.046***
2016	−0.004	−0.017	−0.004	−0.020***

Notes.- time series of period-fixed effects for different subsamples of the ASHE. Results based on the OLS estimations of Equations (1) and (3). Normalized to zero in 2003, linear trends removed. (1) Entry-level new hires, (2) job stayers in CH-firms (3) ASHE new hires, (4) ASHE job stayers. *** Statistically significant from zero at the 1% level; ** at the 5% level, two-sided tests, standard errors robust to clustering at the firm-level.

TABLE C4: Estimated period-fixed effects for basic weekly hours worked ($\widehat{\beta}_t$ from first-step regressions)

	CH-firms		ASHE	
	New hires	Job stayers	New hires	Job stayers
Year	(1)	(2)	(3)	(4)
1999	−0.003	0.007	−0.002	−0.005***
2000	0.040	0.011	0.001	−0.002
2001	0.018	0.005	0.007	−0.001
2002	0.021	−0.001	−0.002	−0.001
2003	0.000	0.000	0.000	0.000
2004	0.003	−0.002	0.023	0.001
2005	−0.010	−0.012	−0.017	−0.010**
2006	−0.027	−0.010	−0.013	−0.009**
2007	0.016	0.003	−0.012	−0.010**
2008	−0.052	0.009	−0.008	−0.008
2009	−0.053	0.002	−0.024	−0.012**
2010	−0.059	0.004	−0.037**	−0.009
2011	−0.131***	−0.004	−0.049***	−0.009
2012	−0.139***	−0.006	−0.052***	−0.009
2013	−0.154***	−0.002	−0.047***	−0.004
2014	−0.129***	0.008	−0.045***	−0.000
2015	−0.132***	0.006	−0.051***	0.003
2016	−0.119**	0.006	−0.062***	0.001

Notes.- see Table C3

*** Statistically significant from zero at the 1% level; ** at the 5% level, two-sided tests, standard errors robust to clustering at the firm-level.

TABLE C5: Time series of price deflators and business cycle indicators

				Labor productivity	
Year	CPI	RPI	SPPI	Whole economy	Services sector
1998	95.56	92.48	97.58	90.62	91.73
1999	97.04	93.96	96.08	93.00	94.26
2000	97.58	96.78	95.97	95.99	96.37
2001	98.66	98.52	98.27	98.26	98.36
2002	100.00	100.00	100.00	100.00	100.00
2003	101.48	103.10	101.61	102.60	101.94
2004	102.69	105.68	103.34	105.67	104.52
2005	104.57	109.06	104.72	106.59	105.27
2006	106.72	111.89	108.06	109.46	108.60
2007	109.68	116.93	110.94	110.76	110.12
2008	112.90	121.84	115.09	112.34	111.79
2009	115.59	120.38	113.59	110.38	110.29
2010	119.89	126.76	115.67	109.80	109.88
2011	123.92	133.35	116.94	111.32	110.68
2012	123.03	138.02	117.86	112.15	111.48
2013	132.12	142.02	118.78	110.99	111.35
2014	134.54	145.57	120.51	110.65	110.87
2015	134.27	146.88	120.97	111.65	111.90
2016	134.68	148.79	122.81	112.42	112.62

Notes.- "CPI" - Consumer Price Index; "RPI" - Retail Price Index; "SPPI" - Services Producer Price Index; "Labor prod. Whole economy" - chain volume measure of gross value added at basic prices in the UK; "Labor prod. Services sector" - chain volume measure of gross value added at basic prices in services industries.

TABLE C6: Estimated hiring-year-tenure-fixed effects for real wages and hours relative to their hiring levels: workers who stay in entry-level jobs

		Wages			Hours		
Cohort	Tenure:	1 year	2 years	3 years	1 year	2 years	3 years
2002		1.79***	4.40***	7.35***	1.60	2.78**	1.66
		(0.62)	(0.80)	(0.83)	(0.92)	(1.13)	(1.38)
2003		−0.09	2.63***	6.68***	0.80	2.62	2.09
		(0.74)	(0.91)	(1.01)	(1.21)	(1.49)	(1.69)
2004		2.07***	7.13***	8.61***	0.91	2.79**	0.95
		(0.61)	(0.74)	(0.89)	(1.10)	(1.26)	(1.48)
2005		4.86***	6.89***	8.78***	1.37	0.46	3.54**
		(0.50)	(0.67)	(0.71)	(1.07)	(1.38)	(1.53)
2006		2.26***	2.67***	4.53***	−0.52	2.63	1.67
		(0.49)	(0.65)	(0.79)	(1.17)	(1.54)	(1.95)
2007		2.44***	5.66***	4.27***	1.40	2.25	3.40
		(0.46)	(0.66)	(0.73)	(1.16)	(1.57)	(1.82)
2008		4.85***	4.00***	4.12***	−1.20	2.08	2.13
		(0.43)	(0.55)	(0.64)	(1.07)	(1.31)	(1.51)
2009		−0.20	0.28	−0.80	1.25	3.55***	4.72**
		(0.39)	(0.50)	(0.61)	(1.11)	(1.37)	(1.95)
2010		1.11**	−0.35	−0.37	0.98	1.06	3.20
		(0.47)	(0.61)	(0.78)	(1.40)	(1.94)	(2.01)
2011		−1.24**	−1.65**	−0.18	1.64	6.04***	9.88***
		(0.52)	(0.67)	(0.89)	(1.34)	(1.76)	(2.06)
2012		−0.39	1.17	5.79***	4.63***	8.28***	14.64***
		(0.47)	(0.65)	(0.86)	(1.20)	(1.69)	(2.05)
2013		3.04***	7.70***	14.30***	8.75***	16.35***	18.46***
		(0.53)	(0.65)	(1.00)	(1.22)	(1.72)	(2.31)

Notes.- This table reports the full results displayed in Figure 3, Section 5. "Cohort" refers to the year of hiring. Robust standard errors in parentheses.
*** Statistically significant from zero at the 1% level; ** at the 5% level, two-sided tests.

TABLE C7: Gross transition rates of job stayers by tenure between full-time and part-time work (percent of all job stayers), and associated mean change in hours worked

		PT to FT			FT to PT		
Cohort	Tenure:	1 year	2 years	3 years	1 year	2 years	3 years
2002	Pr(transition):	6.17	4.77	5.25	3.36	4.91	3.57
	Δ hours:	16.61	15.51	13.44	−15.72	−12.64	−12.27
2003		7.64	8.01	4.79	4.20	3.81	4.19
		18.04	12.75	11.87	−15.04	−14.74	−12.07
2004		8.38	6.34	3.33	3.97	4.43	3.59
		15.84	15.54	10.56	−17.27	−13.39	−12.13
2005		8.58	6.55	5.91	5.61	6.03	3.16
		16.89	13.52	15.26	−17.82	−16.28	−13.92
2006		6.69	7.75	5.71	5.94	3.01	6.29
		16.83	15.49	15.12	−20.35	−14.38	−16.43
2007		8.35	5.88	6.04	4.57	6.21	3.72
		15.20	16.45	12.16	−15.09	−16.17	−11.85
2008		6.10	5.57	6.69	6.82	4.38	3.52
		15.98	14.03	11.40	−15.63	−10.83	−13.57
2009		9.42	7.30	5.82	5.71	6.58	5.57
		14.84	16.02	14.60	−14.69	−12.60	−9.49
2010		9.25	9.57	5.84	6.98	6.64	6.49
		17.13	13.42	10.88	−20.50	−12.07	−10.78
2011		9.98	8.23	7.18	5.20	5.38	7.18
		14.69	12.95	12.46	−16.12	−11.99	−10.08
2012		9.58	9.17	6.48	4.45	4.81	8.45
		16.13	15.43	13.40	−14.76	−11.46	−12.15
2013		11.84	10.13	7.84	4.19	4.82	6.58
		15.35	15.77	13.50	−13.60	−11.79	−11.20

Notes.- "Cohort" refers to the year of hiring. "Pr(transition)" shows the transition probabilities from part-time to full-time status and vice versa, relative to all job stayers between two periods. "Δhours" shows the mean change in hours worked upon transition between statuses.

TABLE C8: Gross transition rates of job stayers by tenure between full-time and part-time work, and associated mean change in hours worked

		PT to FT			FT to PT		
Cohort	Tenure:	1 year	2 years	3 years	1 year	2 years	3 years
2002-07	Pr(transition):	6.84	5.79	4.43	4.19	4.29	3.45
	Δhours:	16.46	14.47	12.73	−16.69	−14.57	−13.34
2008-13		9.31	8.22	6.64	5.66	5.56	6.03
		15.55	14.86	12.64	−15.48	−11.89	−11.20

Notes.- see Table C7.

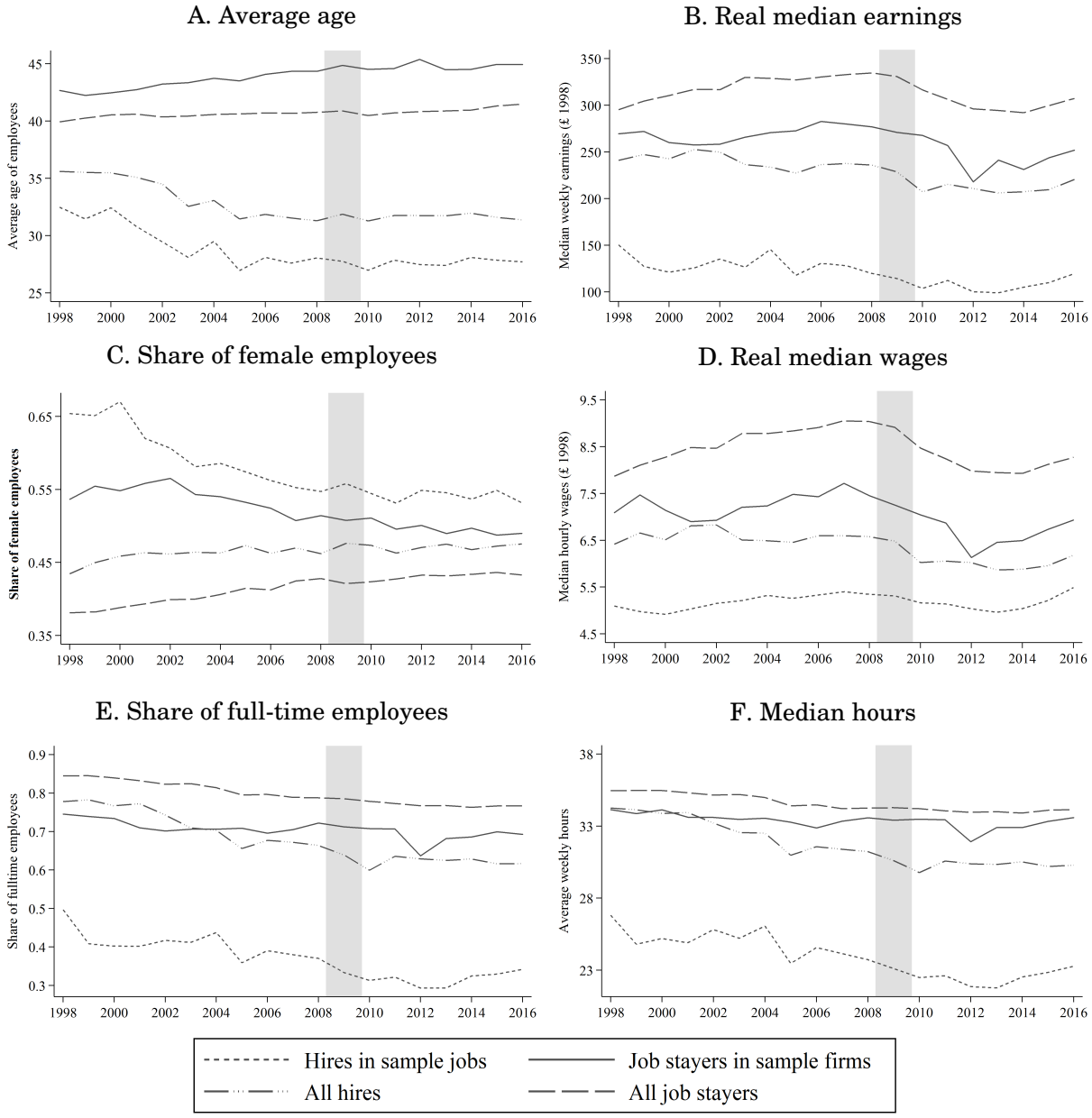
TABLE C9: Share of mean hours changes among job stayers by tenure accounted for by transitions between full and part-time status and vice versa

	Tenure		
Cohort	1 year	2 years	3 years
2002-07	0.66	0.54	0.79
2008-13	0.60	0.68	0.61

Notes.- "Cohort" refers to the year of hiring. The shares not shown are accounted for by hours changes within full or part-time work.

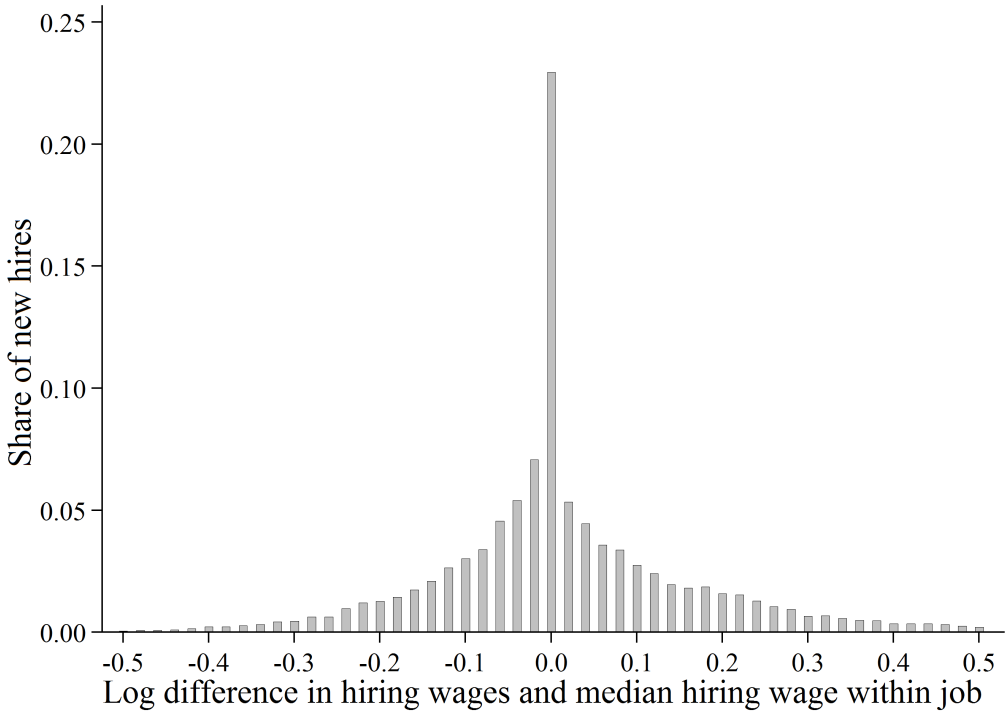
Appendix D. Additional figures

FIGURE D1: Characteristics of employees in the consistent-hiring-firms sample and whole ASHE: comparison of new hires in entry-level jobs vs. job stayers, 1998-2016



Notes.- shaded area marks official UK recession dates. "Hires in sample firms" refers to employees in entry-level jobs with less than twelve months tenure. "Job stayers in sample firms" are for jobs and employees who have more than 12 months tenure in the same job, and only for firms which are represented in the CH-firms sample. "All hires" and "All job stayers" show the corresponding series for new hires and job stayers in the ASHE, estimated as averages at the worker level. Ages 16-64 only.

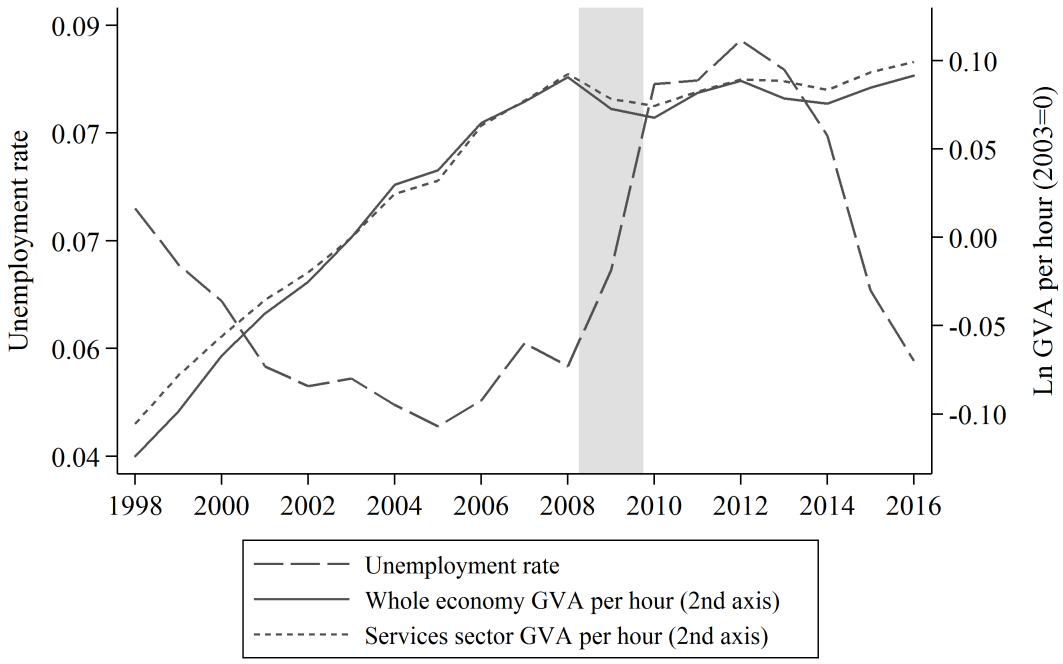
FIGURE D2: Distribution of differences between log real wages of new hires and their median values within entry-level jobs, 1998-2016



Notes.- within each entry-level job and year, the median hiring wage is subtracted from all hiring wages in that job and the resulting log differences are collected in bins with a width of two log points.

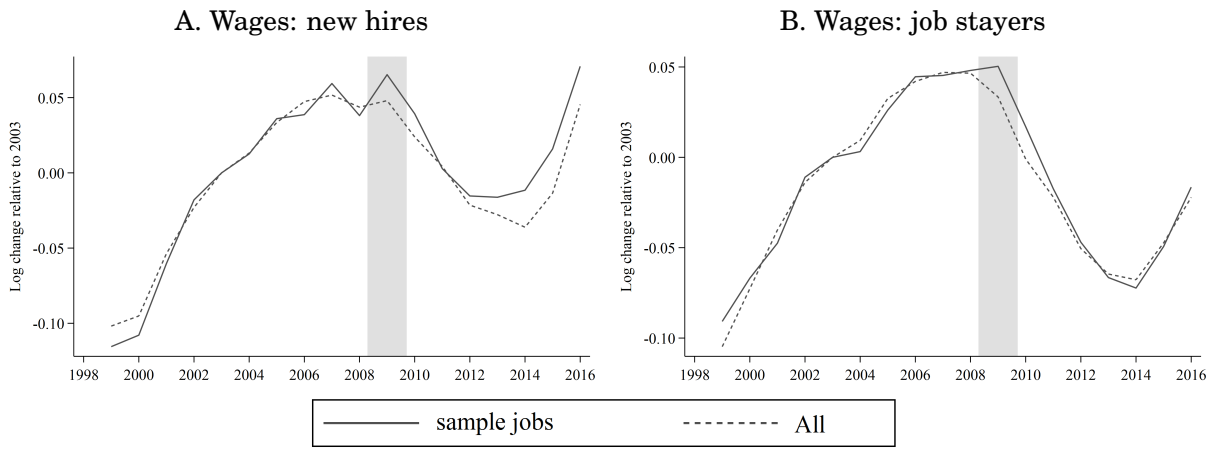
Figure D2 shows the distribution of employee hiring wages after subtracting their respective entry-level job median values. Some dispersion around the median hiring wages, indicated by zero, is visible here. We constructed similar figures for each year (omitted for brevity), and did not find evidence of systematic variation over the business cycle. In particular, the mass in the tails of the distribution does not change and the interquartile range is constant over the sample period.

FIGURE D3: Comparison of business cycle indicators, 1998-2016



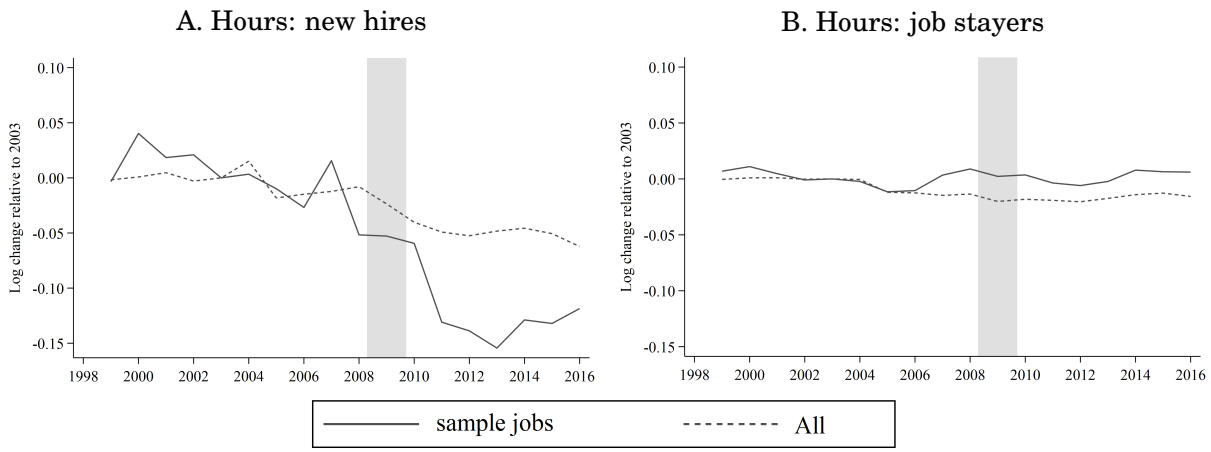
Notes.- see Section 2 and Table 6 for sources. Shaded area marks official UK recession dates.

FIGURE D4: Estimated period-fixed effects for real wages, 1998-2016: comparison of entry-level jobs, all new hires, job stayers in the CH-firms sample, and all job-stayers



Notes.- see Section 2 for further details of sample construction. "All" here refers to all firms and jobs represented in the ASHE. Shaded area marks official UK recession dates.

FIGURE D5: Estimated period-fixed effects for hours worked, 1998-2016: comparison of entry-level jobs, all new hires, job stayers in the CH-firms sample, and all job-stayers

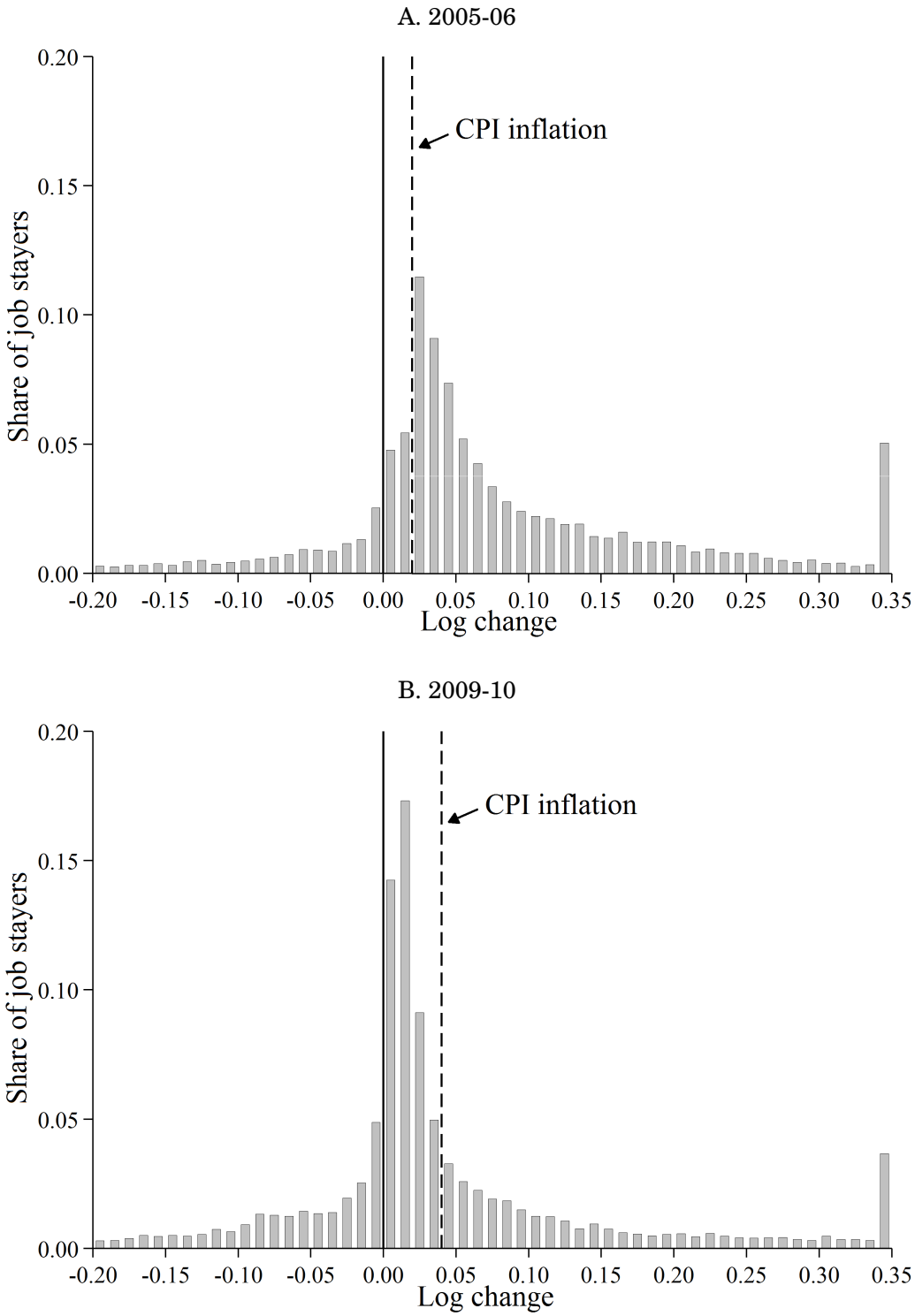


Notes.- see Section 2 for further details of sample construction. "All" here refers to all firms and jobs represented in the ASHE. Shaded area marks official UK recession dates.

Appendix E. Nominal wage changes of job stayers in CH-firms

The nominal wage changes of job stayers in the UK have been analyzed previously by Nickell and Quintini (2003) and most recently by Elsby et al. (2016). We briefly summarize results for year-to-year changes in the log nominal wages of job stayers in the baseline sample of consistent-hiring firms. Figure E1 shows the distributions of log changes for job stayers for the periods 2005-06 and 2009-10. These are representative of periods with relatively low (2005-06) and relatively high (2009-10) shares of job stayers with nominal wage cuts. Table E1 also displays summary statistics for all years in the sample. The dashed line marks the CPI rate of inflation in the histograms. Bars in the histograms exclude upper limits, so log wage changes of exactly zero are included in the bin to the right of the solid line. The spike at zero is relatively small during normal times, ranging from 0.5 percent to 2.1 percent in the period before the Great Recession, as Table E1 shows. The distribution in Figure E1A also suggests that most wages increase with the rate of inflation during normal times, thus keeping the real wage constant. Nevertheless, even during this period a notable share of job stayers, around 20 percent, appear to have experienced nominal wage cuts. This share increased during the recession to around 25 percent on average. Similarly, the share of job stayers with exactly zero nominal wage growth peaked at 5.7 percent between 2009-10. In particular, Figure E1B displays a relatively large share of nominal wage changes between zero and two percent for job stayers in CH-firms between 2009-10. However, the large increase in the share of job stayers which experienced negative changes in log *real* wages, as shown in the last column of Table E1, was mainly related to the rise in inflation. These findings suggest that zero is a significant threshold for nominal wage changes and limited the downward adjustment of nominal wages, as Nickell and Quintini (2003) argue. But on average more than 20 percent of year-to-year job stayers appear to experience nominal wage cuts in the UK, suggesting that there is a relatively high degree of nominal wage flexibility in the British labor market. Nevertheless, the increase in inflation during the Great Recession resulted in over two-thirds of job stayers seeing their real wages cut.

FIGURE E1: Distribution of year-to-year changes in log nominal hourly wages for job stayers in CH-firms, 2005-06 and 2009-10



Notes.- solid line marks zero, dashed line marks the log change in the Consumer Price Index. Bars show half-open intervals, excluding the upper limit. Ages 16-64, private sector only.

TABLE E1: Percentage of job stayers with year-to-year changes in log nominal hourly wages in given category, 1997-2016

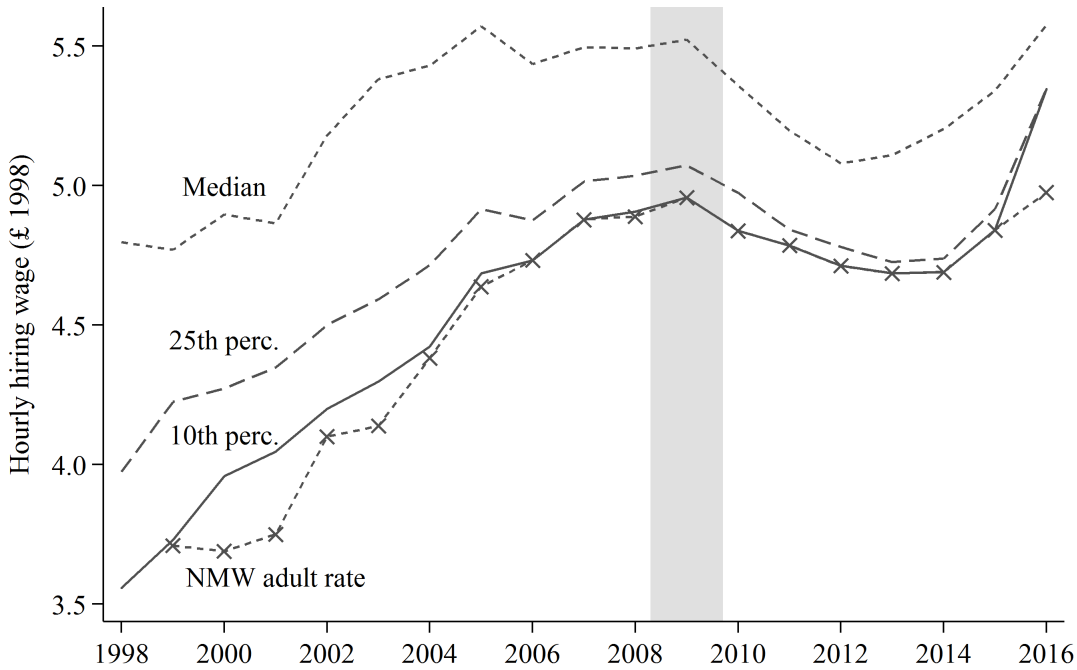
	Percentage of log nominal wage changes in given category					
Years	$[-.01;0[$	Exactly 0	$]0;\ .01]$	Nominal wage cut	Real wage cut	Inflation
1997-98	2.6	1.1	2.6	36.1	41.8	1.8
1998-99	1.7	0.9	2.2	22.8	27.2	1.5
1999-00	2.7	1.5	3.2	22.0	25.3	0.6
2000-01	2.5	1.4	2.8	18.0	22.5	1.1
2001-02	2.0	2.1	2.8	20.1	26.1	1.3
2002-03	2.9	0.7	3.5	23.7	29.2	1.5
2003-04	2.6	0.5	2.9	34.5	38.4	1.2
2004-05	2.6	0.8	3.3	20.0	27.5	1.8
2005-06	2.4	0.7	3.9	17.1	27.2	2.0
2006-07	2.4	1.7	3.2	21.7	37.2	2.7
2007-08	2.8	1.7	3.5	17.9	37.0	2.9
2008-09	3.4	2.9	4.2	20.1	34.3	2.4
2009-10	4.7	5.7	7.9	26.8	69.2	3.7
2010-11	3.8	3.7	5.7	24.5	61.2	3.3
2011-12	4.4	4.4	5.4	24.8	66.7	4.0
2012-13	3.8	4.3	5.3	24.7	55.8	2.4
2013-14	3.2	3.2	5.6	24.3	40.4	1.8
2014-15	2.7	1.5	4.7	18.9	18.0	-0.2
2015-16	3.2	2.4	4.3	25.9	29.6	0.3

Notes.- share of job stayers in CH-firms with log nominal wage changes in the indicated interval. Inflation is computed as the average of log changes in CPI over the previous four quarters.

Appendix F. The role of the National Minimum Wage

Our results suggest that the real wages of new hires are equally responsive to business cycle conditions as job stayers' wages. One potential explanation for this finding is the presence of a wage floor. This could constrain firms in how far they can reduce hiring wages. In 1999 such a floor was introduced in the UK in the form of the National Minimum Wage (NMW), with both adult and youth rates applying nationwide. These are usually uprated on an annual basis [Source: https://www.gov.uk/national-minimum-wage-rates; accessed 01/07/2017]. Collectively bargained wages can also limit a firm's flexibility in setting hiring wages. However, at the onset of the Great Recession, only six percent of new hires in the sample were covered by a national or industry-level collective agreement (affecting working conditions, not necessarily pay). Therefore, we consider the NMW to be the more likely limit on the responsiveness of hiring wages.

FIGURE F1: Real hourly wages of new hires and NMW adult rate, ages 22-64



Notes.- National Minimum Wage adult rate and 10th, 25th, and median percentile of job-level hourly hiring wages, ages 22-64. All monetary values are deflated to 1998 values using the CPI. Shaded area marks official UK recession.

Figure F1 displays the real NMW rate that applied to workers aged 21 and older, along with the 10th percentile, 25th percentile, and median real wages of new hires within the baseline entry-level jobs sample for each year. The adult rate age limit was decreased from 22 to 21 in 2010. These hiring wages are not adjusted for changes in sample composition and include only workers aged 22-64. Between 2006 and 2015, new hires at the 10th percentile of the wage distribution were paid the legal minimum, i.e. the real value of the adult rate. In 2016 the 10th percentile of new hires increased more than the adult rate, which followed the introduction of a higher NMW rate for workers aged 25 and over. We also observe a narrowing of the gaps between the minimum wage and both the 25th percentile and median of hiring wages over the sample period. In other words, the domain of the distribution of real hiring wages at the job level, for employees aged 22-64, became more restricted from below at the level of the real NMW adult rate

during the recent downturn. The wages of job stayers in CH-firms were less constrained by the minimum wage than hiring wages, since stayers are generally paid more than new hires (see Figure D1D).

To answer the question of how hiring wages might have responded to the Great Recession in the absence of a binding minimum wage, we use the kernel re-weighting method of DiNardo et al. (1996). A description of this method is provided in the next section below. In short, a partial equilibrium assumption underlies it: the number and composition of entry-level jobs is not affected by the NMW. This assumption is unlikely to hold in reality. Nevertheless, this method allows us to assess the impact of the NMW in a simple and transparent way. Here we briefly explain the intuition. For each year following 2004, we replace the density of job-level real hiring wages which was at or below the real value of the NMW in that year, with the corresponding section of the 2004 density, adjusted for differences in observable job characteristics. Then, we re-scale this counterfactual density so that the two sections integrate to one. We select 2004 as the base year because this was the last year when the real value of the NMW was below its lowest level in 2014 (see Figure F1), and the partial equilibrium assumption seems least restrictive. For this estimation we use the plug-in method of Sheather and Jones (1991) to select the optimal bandwidth, which ranges from 0.01 to 0.04 for the sample.

The most important parameter in this kernel re-weighting exercise is the assumed size of the spillover effect of the minimum wage, i.e. the highest value of the real hiring wage density which is affected by the NMW. The more spillover we assume, the more of this period's density - the section below the real value of the minimum wage plus any spillover - is replaced with the corresponding section of the 2004 density.

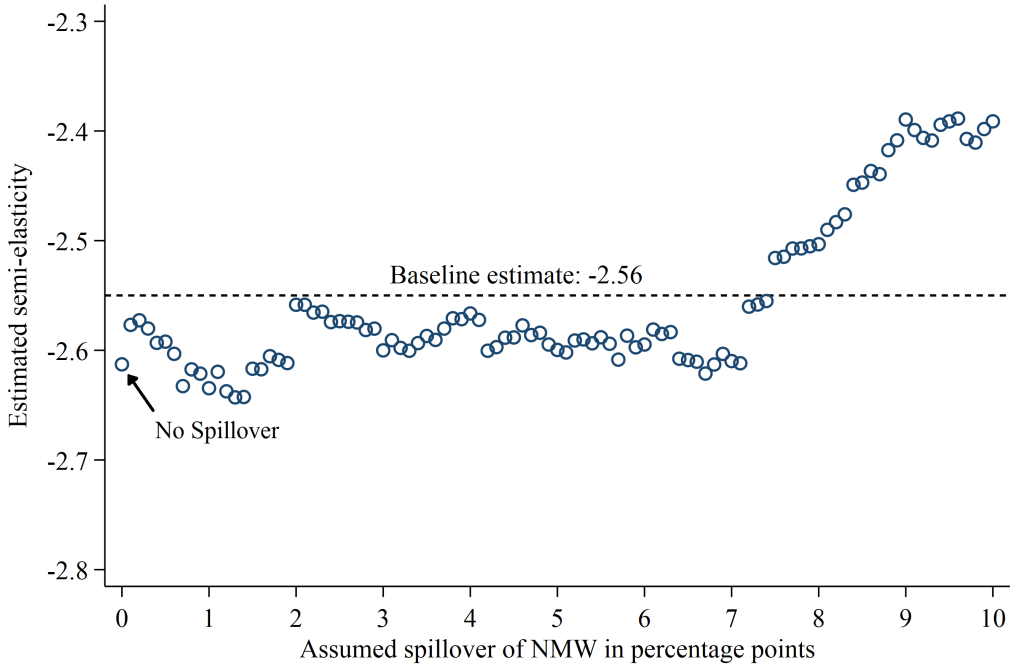
To the best of our knowledge, the extent to which the minimum wage is affecting real hiring wages in the UK has not been addressed. Varying estimates exist for the size of the spillover on UK wages in general, with estimates ranging from almost no spillover effects (Dickens and Manning, 2004) to relatively small effects up to the 5th percentile of wages above the NMW (Stewart, 2012), and up to 40 percent above the NMW (Butcher et al., 2012). Therefore, we estimate counterfactual real wage densities for new hires, assuming spillover effects ranging from 0 to 10 p.p. above the real NMW in a given year.

To compute hiring wages at the job level from counterfactual densities, we assume that the rank of a job in the distribution of hiring wages is preserved under different values of the NMW. For example, if under the counterfactual in 2013 ten percent of jobs hired at a wage below the NMW, then for the bottom decile of jobs ranked by actual hiring wages we would impute the hiring wages below the NMW from the counterfactual. Then we re-estimate regressions (1) and (2), using each of the counterfactual real hiring wage samples estimated with varying spillover parameter values. Figure F2 displays the point estimates of the counterfactual semi-elasticity of real hiring wages with respect to the unemployment rate across a range of assumed parameters of the spillover.

Assuming there is no spillover effect, the left-most circle shows that the responsiveness of real hiring wages to the unemployment rate increases from -2.56 to -2.64 percent. The standard errors are comparable to the baseline value (0.9) and lie outside the range of this figure. The semi-elasticity falls below -2.7 when the spillover effect increases to five p.p. If the spillover increases above seven p.p., then the responsiveness of real hiring wages to changes in the unemployment rate begins to revert towards the baseline estimate: the shape of the counterfactual density increasingly resembles the shape of the density

observed in 2004 when we assume larger spillover effects, and hence the variation of hiring wages over time in entry-level jobs declines. These results suggest that the NMW constrained firms in how far they could reduce the wages of new hires during the Great Recession.

FIGURE F2: Counterfactual estimates of the semi-elasticity of real hiring wages, 1998-2016: varying the assumed spillover effect of the NMW



Notes.- each circle represents an estimate of the semi-elasticity of real hiring wages with respect to the unemployment rate. Standard errors lie outside of the figure. The horizontal axis shows the assumed spillover effect in p.p.. Dashed line shows the baseline estimate of semi-elasticity. We use a Gaussian kernel, and the bandwidth is selected using the Sheather-Jones plug-in estimator.

F.1 Description of the kernel re-weighting method

The following describes the method of DiNardo et al. (1996), which we use to estimate counterfactual densities of real hiring wages: the exposition here follows closely their own. Let $f^i(w|x;m_i)$ be the density of real hiring wages in period i, conditional on observable attributes x and the real minimum wage m_i. The density of observed attributes in period i is $h(x|t=i)$. The observed densities of real hiring wages in two periods, say 2004 ($i=04$) and 2013 ($i=13$), are

$$g(w|t=04;m_{04}) = \int_{\Omega_x} f^{04}(w|x;m_{04})h(x|t=04)dx \ , \tag{5}$$

and

$$g(w|t=13;m_{13}) = \int_{\Omega_x} f^{13}(w|x;m_{13})h(x|t=13)dx \ , \tag{6}$$

where Ω_x is the domain of observed attributes. Differences in attributes at the job-level between the two periods are captured by the density functions $h(x|t=04)$ and $h(x|t=13)$. Differences in the "price" paid for these attributes are captured by differences in

$f^{04}(w|x;m_{04})$ and $f^{13}(w|x;m_{13})$, and these differences can depend on the real minimum wage. The counterfactual density of real hiring wages that would prevail if the level of the real minimum wage of 2004 was realized in 2013, *and* prices had remained at their 2013 level, is

$$g(w|t=13;m_{04}) = \int_{\Omega_x} f^{13}(w|x;m_{04})h(x|t=13)dx \ , \tag{7}$$

where we know $h(x|t=13)$, but the density of prices $f^{13}(w|x;m_{04})$, consisting of the real wage schedule of 2013 and the real minimum wage of 2004, is unobserved. We can partition the density in (7) into the part of real wages below the real value of the minimum wage in 2013 and the part of real wages above this threshold:

$$\begin{aligned} g(w|t=13;m_{04}) = &\int_{\Omega_x} [1-I(w \le m_{13})] f^{13}(w|x;m_{04})h(x|t=13)dx \\ &+ \int_{\Omega_x} I(w \le m_{13}) f^{13}(w|x;m_{04})h(x|t=13)dx \ , \end{aligned} \tag{8}$$

where $I(w \le m_{13})$ is an indicator function that equals one if the observed wage is at or below the level of the real minimum wage in 2013. We follow DiNardo et al. and make quite restrictive economic assumptions, but because of this restrictiveness, they are also transparent.

Assumption 1 *Between two periods $i = \{L,H\}$ with $m_L < m_H$, the conditional density of hiring wages above the real value of the minimum wage m_H is not affected by the minimum wage:*

$$[1-I(w \le m_H)] f^i(w|x;m_H) = [1-I(w \le m_H)] f^i(w|x;m_L) \ . \tag{9}$$

This is a conservative assumption, and we later conduct a sensitivity analysis where we allow the minimum wage to affect the wage density above its real value. The results vary with the size of this spillover effect, but not substantially.

Assumption 2 *The shape of the conditional density at or below the minimum wage depends only on the real value of the minimum wage. Thus, the conditional density in $i = L$ below m_H is proportional to the conditional density in $i = H$ below m_H:*

$$I(w \le m_H) f^H(w|x;m_L) = I(w \le m_H)\psi_w f^L(w|x;m_L) \ , \tag{10}$$

where the re-weighting function ψ_w will be defined below.

With this assumption we can regard the density in 2004 below the real value of the 2013 minimum wage as the latent hiring wage distribution, conditional on observable attributes x. Without a structural model, it is not possible to impute the wage schedule below the real minimum wage in 2013 without making strong assumptions like Assumption 2. But we think this assumption is at least relatively transparent. The last assumption necessary to derive a counterfactual hiring wage density is:

Assumption 3 *The level of the minimum wage can affect the number of new hires but has no effect on the number of entry-level jobs.*

This assumption is weaker than the corresponding one originally made by DiNardo et al. (1996), who assumed that the level of the minimum wage does not affect the level of employment.

We illustrate the method in Figure F3, which shows estimated densities of real hiring wages (we pool data over multiple periods in Figures F3 and F4 for data confidentiality reasons). The kernel density estimate in the Figure F3A uses pooled data from 2002-04, a period where the NMW was relatively less binding. The solid vertical line shows the real value of the NMW at its 2013 level. Figure F3B shows the corresponding density for 2012-14, where the real NMW remained nearly constant around its 2013 value. The counterfactual density displayed in Figure F3C is a simple combination of the part of the density to the left of the solid line in Figure F3A and to the right of the solid line in Figure F3B, scaled to integrate to one.

Increasing the assumed spillover of the NMW acts as if shifting the NMW in Figure F3A-C to the right: the area of the density below the new threshold, consisting of the NMW plus spillover, will increase. This means that a larger part of the 2012-14 density will be replaced by the 2002-04 density. In the extreme case that the NMW plus spillover exceeds the highest measured job-level hiring wage in 2012-14, the counterfactual density would fully consist of the estimated density in 2002-04.

Figure F4A plots the estimated (connected circles) and counterfactual (solid line) density for 2012-14 together. Most of this mass originates from jobs which are observed to hire slightly above the NMW, a result of the smoothing by the kernel estimator. For hiring wages which exceed the NMW substantially, the estimated and counterfactual density are, as expected, indistinguishable. Figure F4B displays the difference between the estimated and counterfactual wage densities shown in Figure F4A of this figure. The difference is negative for values around the value of the NMW in 2013 and positive for log hiring wages between ln(4) and ln(4.6).

Assumptions 1-3 allow us to write

$$\begin{aligned} g(w|t=13;m_{04}) = &[1-I(w \le m_{13})]f^{13}(w|x;m_{04})dx \\ &I(w \le m_{13})\psi_w f^{04}(w|x;m_{04})dx \ , \end{aligned} \tag{11}$$

with

$$\psi_w = \frac{\Pr(w \le m_{13}|x, f = f^{13})}{\Pr(w \le m_{13}|x, f = f^{04})} \ , \tag{12}$$

which ensures that the density integrates to one over the distribution of attributes. The counterfactual real hiring wage density is found by integrating over the observed distribution of attributes:

$$\begin{aligned} g(w|t=13;m_{04}) = &\int_{\Omega_x} [1-I(w \le m_{13})]f^{13}(w|x;m_{04})h(x|t=13)dx \\ &+ \int_{\Omega_x} I(w \le m_{13})\psi_w f^{04}(w|x;m_{04})h(x|t=13)dx \ . \end{aligned} \tag{13}$$

The key insight of DiNardo et al. is that the wage density that would result from combining the wage schedule in 2004, $f^{04}(w|x;m_{04})$, and the marginal distribution of attributes, $h(x|t=13)$, can be obtained by taking the observed density of attributes in 2004, $h(x|t=04)$, and re-weighting it to reflect differences between the two periods. Let

this re-weighting function be denoted θ, then

$$g(w|t=13;m_{04}) = \int_{\Omega_x} [1 - I(w \leq m_{13})] f^{13}(w|x;m_{04}) h(x|t=13) dx$$
$$+ \int_{\Omega_x} I(w \leq m_{13}) \psi_w f^{04}(w|x;m_{04}) \theta h(x|t=04) dx \ , \tag{14}$$

where the re-weighting function is

$$\theta = \frac{h(x|t=13)}{h(x|t=13)} = \frac{\Pr(t=13|x)}{\Pr(t=04|x)} \frac{\Pr(t=04)}{\Pr(t=13)} \ . \tag{15}$$

The last equality follows from Bayes' rule. We combine the two re-weighting functions to give

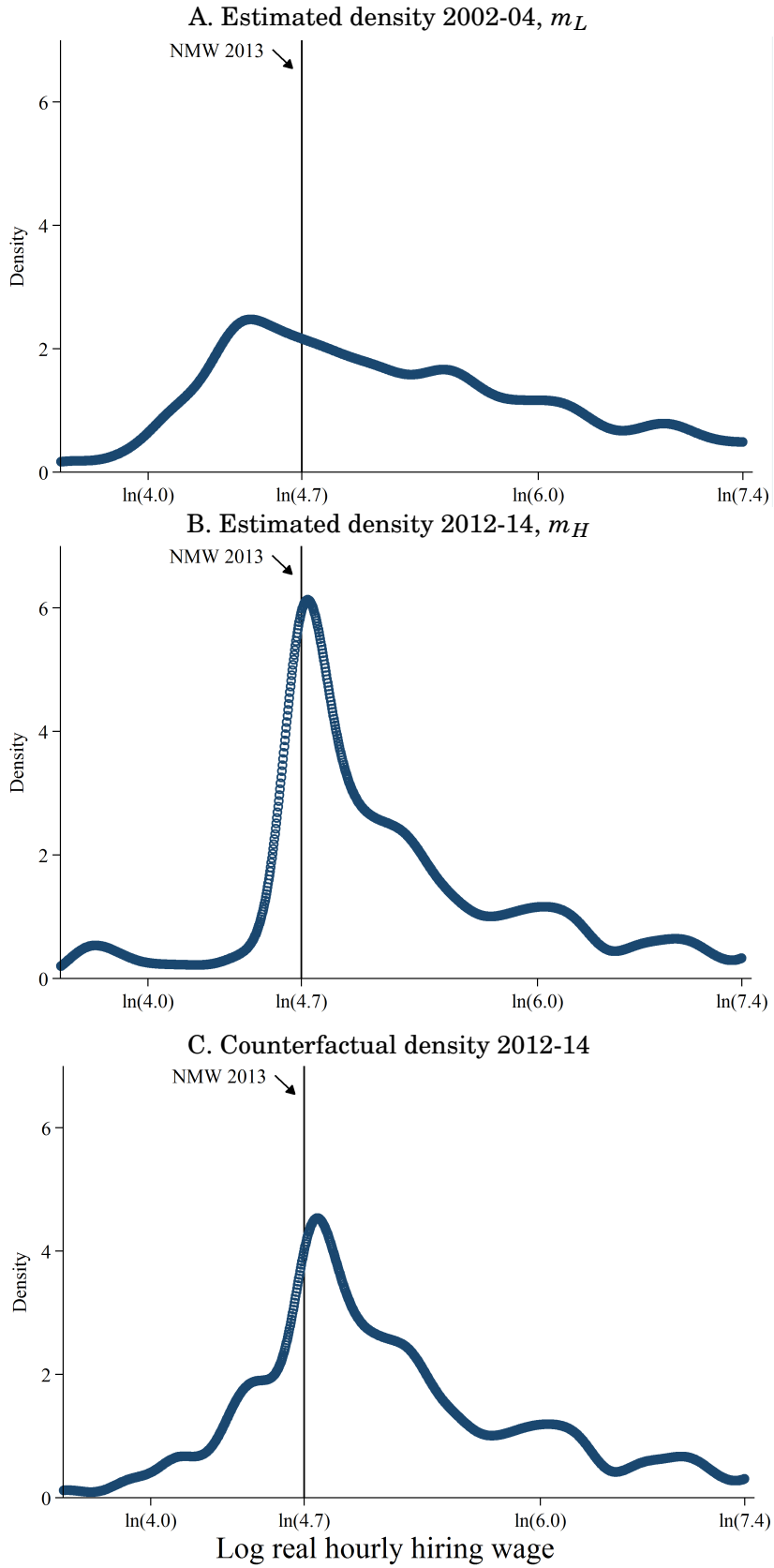
$$\psi = \theta \cdot \psi_w = \frac{\Pr(t=13|x, w \leq m_{13})}{\Pr(t=04|x, w \leq m_{13})} \frac{\Pr(t=04)}{\Pr(t=13)} \ . \tag{16}$$

We estimate the probability of a job being below the NMW in 2013, conditional on its observed attributes parametrically, using a logit model,

$$\Pr(t=13|x, w \leq m_{13}) = \Lambda(C(x)) \ , \tag{17}$$

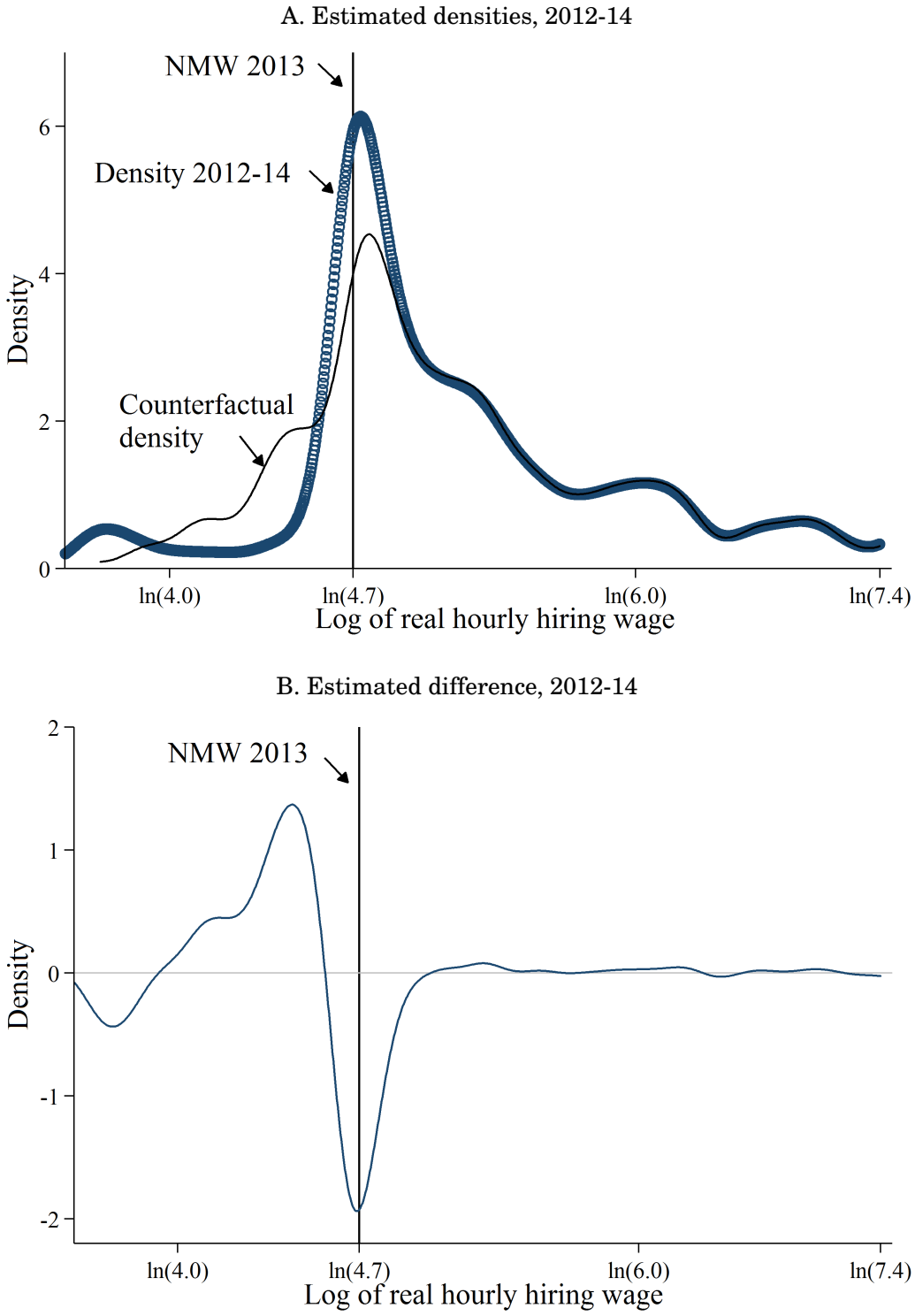
with $C(x)$ being a vector that is a function of the covariates x. The covariates are: one-digit industry dummies, a cubic in age and firm size, and the shares of workers that are female, full-time, permanent, and covered by a collectively bargaining agreement. We then compute estimates of $\widehat{\psi}$ for each observation and use these weights in the kernel density estimation to derive the counterfactual density of real hiring wages in 2013. The weight equals one if an observation is above the NMW in 2013, it equals zero if the observation is below the NMW in 2013, and the weight equals $\widehat{\psi}_j$ if an observation j in the pooled data is from 2004 and from the section of the density of real wages below the real value of the NMW in 2013.

FIGURE F3: Illustration of the re-weighting procedure for log real hourly hiring wages



Notes.- densities estimated using Gaussian kernel and bandwidth of 0.03 (A) and 0.02 (B-C). Monetary values deflated to 1998 values using the CPI. Solid lines show the real value of the adult rate minimum wage in 2013.

FIGURE F4: Estimated and counterfactual densities of real log hiring wages at the job-level, 2012-14



Notes.- densities estimated using Gaussian kernel and bandwidth of 0.02. Monetary values deflated to 1998 values using the CPI. Solid vertical line shows the real value of the adult-rate minimum wage.

Appendix G. Possible (UK-specific) explanations

Using essentially the same dataset but without firm identifiers, Elsby et al. (2016) show that UK real wages behaved very differently during the Great Recession when compared with previous recessions: during the 1980s and 1990s downturns the growth in real wages for British job stayers slowed, whereas it turned markedly negative in the most recent downturn. This matches the findings of Gregg et al. (2014), who document that UK wages became significantly more sensitive to changes in local unemployment rates sometime in the early 2000s. Both Elsby et al. (2016) and Gregg et al. (2014) emphasize that the decline in unionization in the UK since the 1970s could only account for a small part of these observed changes in the behavior of real wages. One argument for this is that US real wages remained relatively constant in the years following the 2008 financial crisis, while US employment fell sharply, despite the US seeing a greater decline and lower contemporary level of unionization than the UK.

A similar line of argument applies for the role inflation. In both the US and the UK, price inflation was historically low before and during the Great Recession. In Online Appendix E we further dispel the notion that price inflation could account for the high level of real wage flexibility in the Great Recession, by demonstrating that there is a lack of absolute nominal wage rigidity among UK job stayers. We extend the period of Elsby et al.'s account of UK nominal wage rigidity, and specifically consider year-to-year hourly wage changes among the job stayers in the baseline sample from our main analysis. As many as two-thirds of these employees experienced annual real wage cuts at the height of the downturn, while around a quarter also experienced nominal wage cuts. The incidence of exactly zero annual nominal wage changes increased from approximately 0-2 percent of employees before 2008-9 to 3-5 percent in the years after. Our main findings on the extent of UK real wage flexibility reflect the fact that large numbers of employees appear to experience yearly nominal wage cuts, almost independently of the economic cycle.

Blundell et al. (2014) argue that the UK's labor supply curve shifted to the right during the Great Recession. This was most likely caused by welfare reforms, which led to the addition, and stricter enforcement, of job search requirements for several groups of non-employed persons. For example, lone parents, who constitute approximately a quarter of all UK family households, were particularly affected. The age of the youngest child, at which lone parents are entitled to unconditional income support, was gradually reduced from sixteen to five years old between 2008 and 2012. If their youngest child was older than these lowered thresholds, then lone parents would have had to show evidence that they were searching for work in order to receive the same income support as they were entitled to previously without searching. It has been estimated that these policy changes led to an increase of almost ten percent in the employment rate among UK lone parents, despite this occurring throughout a major recession (Avram et al., 2018). It is plausible that increased competition for jobs, brought on by the cumulative and extensive changes in the UK's active labor market policy since the last major downturn in the early 1990s, resulted in large decreases in the real values of workers' reservation wages and outside options, and thus led to new hires and job stayers accepting large decreases in real wages.

Perhaps our most striking finding for the behavior of the UK labor market since 2008 is the extent to which hiring hours in jobs were reduced. How could this shift from full- to part-time recruitment be explained? Shifts in the labor supply curve, particularly for part-time work, are again potentially relevant. The UK has a system of tax credit

benefits for working families with children similar to the US earned income tax credits. Entitlement for the work-contingent component requires at least one adult to work for a minimum of sixteen hours per week. There is observable bunching in the distribution of employee hours worked around the thresholds in the UK tax credits system, which is unsurprising given the large differences in the amount of credits families receive around these levels (see Blundell et al. (2016) for a more detailed discussion). This part of the UK welfare system cushions workers from income loss when their working hours decline, as well as encouraging them to take part-time work more readily than they perhaps would otherwise. In fact, the number of people in the UK who said that they were working part-time because they could not find a full-time job in 2013 stood at the highest level on record: almost 1.5 million (6 percent of all employees), compared with 2.5 million unemployed, and compared with 0.7 million involuntary part-time employed in 2007 [Source: ONS Labour Market Statistics, October 2017, available at https://www.ons.gov.uk/; accessed 07/11/2017. See also Bell and Blanchflower (2013) for more details about so-called "Underemployment" in the UK].

Another possible cyclical feature of labor markets is the so-called "Added Worker Effect", whereby individual household members will increase their labor supply when the household experiences persistent income shocks, typically thought of as resulting from a partner's job loss. There is some aggregate evidence of this effect for the UK, based on individual-level labor force transition rate data (Razzu and Singleton, 2016). However, Bryan and Longhi (2018) have shown that while this effect seems to draw individuals into the UK unemployment pool, it does not significantly increase their likelihood of becoming employed. The added worker effect is therefore unlikely to be a large part of the overall story of why hiring hours were flexible since 2008.

Montgomery (1988) discusses the factors which determine firms' demand for part-time employees. If there are fixed costs of hiring and training new employees, then these costs are unlikely to vary between part- and full-time hires in the same job: the ratio of hours to fixed costs will often be lower for part-time hires. Firms require compensation for this lower return from part-time hiring, that is, the hourly wage per worker has to be lower. This firm-side compensating differential should be stronger for higher-skilled jobs, where hiring and training costs are typically greater. Montgomery (1988) provides evidence for these features of wage-setting and hiring behavior in the presence of fixed costs among US establishments. Moreover, if firms have to pay all workers in some job the same hourly rate, then firms are more likely to employ full-time employees when there are fixed hiring costs. However, fringe benefits (pension contribution, health care) function as quasi-fixed costs which might only be offered to full-time employees, and thus shift the demand from full- to part-time workers. To the extent that these fixed costs depend on the level of productivity, it is possible that they decline during recessions, and thus make part-time hiring more likely. The cyclical properties of fixed hiring costs in the UK is an interesting empirical question for future research.

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