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ABSTRACT

Public Sector Wage Gaps over the Long-Run: Evidence from Panel Administrative Data

With the increase in national debts, pay freezes are imposed for several years in the public sector of some countries, at the risk of decreasing the quality of public services. Since public wage setting policies should account for relevant comparisons with the private sector, we provide novel evidence on the public sector wage gap throughout the wage distribution in France, taking a long-term perspective. We exploit a long administrative panel dataset (1988-2013) and suggest methodological innovations. We estimate the public sector premia/penalties on the unconditional wage distribution while accounting for quantile-specific fixed effects and a jackknife correction for the potential incidental parameter bias. We find that the public wage gap is broadly negative in France, with larger penalties at the top, which contribute to a compression of the wage distribution by the public sector. We show that this compression effect is partly concealed by the incidental parameter bias. Time changes in the wage gap over 25 years are consistently explained by a mix of political and business cycles. The unobserved skill gap between sectors reveals the extent of positive selection into public jobs. It tends to decline in the 1990s, a period characterized by the growth of public employment and a move towards less selective recruitment schemes. More critically, it totally disappears among top earners in the recent period, suggesting the detrimental effect of nominal wage freeze and the absence of performance-based remuneration among public sector executives.

JEL Classification: J31, C14

Keywords: public wage gap, unconditional quantile regression, fixed effects, incidental parameter bias, jackknife

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1 Introduction

Many countries have been strongly pressured to consolidate their public finances in the wake of the Great Recession. As a result, the productivity and wage levels of civil servants are under scrutiny. Nominal or even real wage cuts in the public sector have been observed in several countries (see Depalo et al., 2015). In France, in particular, public sector wages have been nominally frozen since 2010 (except for a small rise in 2017). Arguably, excessive levels of public wages can distort market competition and increase public deficits. Yet, large cuts in public wages may also be detrimental, posing a threat to the quality of public services by making it difficult to retain skilled workers. Hence, it is urgent to provide comprehensive measures of wage differentials between sectors, with the aim of disentangling the true pay differences from structural differences in workers' observed and unobserved skills across sectors at different points of the distribution. Such measures and decompositions represent the relevant basis that should ideally be used for public wage setting policies.[1]

Economists have already provided much evidence about public wage gaps in many countries, controlling for observed characteristics of public versus private workers. Many studies examine the conditional wage gap at the mean or throughout the wage distribution using various techniques like quantile regressions.[2] Yet, it is still unclear how accurate these measures of pay differentials are. Two important limitations characterize the bulk of the literature on sectoral wage gaps and motivate our study. First, the presence of unobservables that affect both wage levels and selection into a particular sector require specific treatments. The IV approach applied to quantile regression shows some issues and faces the usual difficulty of finding exogenous instruments.[3] Panel estimations with

[1]In France and many other countries, wages are not settled in reference to private sector pay (as this is the case for instance in the UK). The level of the multiplier applied to the pay indices on the single integrated pay structure is decided unilaterally by the governments (the outcome of the bargaining process is not legally binding, cf. Guillotin and Meurs, 1999). Public pay levels thus reflect mostly public budgetary conditions and political cycles.

[2]Among the many studies considering average wage gaps, the oldest ones are probably Hartog and Oosterbeek (1993) and van Ophem (1993) for the Netherland and Dustmann and Van Soest (1997) for Germany. Distributional analyses started with Disney and Gosling (1998) for the UK and Mueller (1998) for Canada; see the survey of Disney (2007). More recent studies using quantile regressions include Melly (2005) for Germany, Lucifora and Meurs (2006) for Italy, France and the UK, or Depalo et al. (2015) for euro-area countries, among others.

[3]Several application in the literature are based on the extension of quantile estimations to selection correction using IV, following Buchinsky (1998). Yet, Huber and Melly (2015) show that his method correctly works only if the coefficients of quantile regressions are constant across quantiles. Clearly, then, this method cannot be used to study heterogeneous effects. Other methods have been suggested by Abadie et al. (2002) and Chernozhukov and Hansen (2005). Regarding the difficulty to find instruments,

fixed effects seem to be a promising approach, in particular their extension to distributional analyses.[4] However, a well-known issue is the potential incidental parameter bias affecting short panel estimations of nonlinear models with fixed effects (cf. Arellano and Weidner, 2015), such as fixed effects quantile regressions (Koenker, 2004, Canay, 2011).

Another difficulty pertains to the fact that distributional analyses most often rely on *conditional* quantile estimations, which bear very specific interpretations. In particular, with fixed effects in panel estimations, estimated wage gaps at different quantiles only reflect time variation in pay (since individual variation in time-invariant unobservables is controlled for). Recently, several contributions have suggested ways to estimate unconditional quantiles (Firpo et al., 2009, Chernozukhov et al., 2013). To our knowledge, there is hardly any application of these methods to characterize sectoral pay gaps at different points of the unconditional wage distribution.[5] Moreover, these approaches could include fixed effects in panel wage estimations without changing the interpretation of the public wage premia/penalties. Thus, it would become possible to assess the public wage gap at different points of the raw wage distribution when unobserved characteristics are taken into account and when they are not: the difference between these two estimates captures the 'unobservable skill gap', i.e. the degree of positive selection into a particular sector.

In this paper, we suggest novel evidence for France while addressing the different concerns above. We conduct a comprehensive assessment of the public sector wage gaps on average and throughout the distribution. To better characterize recent evolutions, we place them in a long-term perspective by estimating wage gaps over 25 years and interpreting time variation in terms of political and business cycles. To do so, we exploit a unique administrative panel dataset, registering 1/25 of all French salary workers since 1988. We make several methodological innovations to address the previous concerns. First, we estimate unconditional quantile effects à la Chernozukhov et al. (2013) while accounting for fixed effects in panel estimations. Second, our estimator is flexible, in the sense that fixed effects are quantile-specific, but remains tractable in presence of large data and bootstrapped standard errors. Third, the exceptionally long duration of the panel (1988-2013) tends to reduce the incidental parameter bias. More than this, we explicitly address this bias –

note that some studies identify the sector choice using parental background, relying on the fact that workers with civil servant fathers are more likely to belong to the public sector (Bey, 2009, Maczulski, 2011). Yet it is hard to assume that these variables do not also affect potential wages.

[4]See applications in Campos and Centeno (2012) for 10 European countries using the ECHP data, Hospido and Moral-Benito (2016) for Spain, Siminski (2013) for Autralia, or an early version of the present study using the French Labor Force Survey in Bargain and Melly (2008).

[5]An important exception to our statement is the study of Hospido and Moral-Benito (2016) who do estimate fixed effects unconditional quantile regressions using a panel for the years 2005-2012 in Spain. This paper does not address the incidental parameter bias.

one of the fundamental issues in the econometrics of nonlinear models – in the context of quantile estimations. We develop a jackknife correction inspired by Dhaene and Jochmans (2011) that can be applied to the estimation of quantile effects.

Results point to negative public wage gaps all along the distribution in France. Public wage penalties are close to zero at low quantiles and increase with wage levels, i.e. the public sector has a compressing effect on the wage distribution. Most studies find that this effect is due to unobserved heterogeneity – namely larger positive selection at the bottom of the distribution – since it usually disappears with the introduction of fixed effects. Quite differently, we do restore the compression effect when the jackknife correction is applied, indicating that the incidental parameter bias overstates the role of individual effects in panel quantile estimations. Time changes since 1988 are consistently explained by a mix of political and business cycles. In particular, large public wage penalties in the late 1980s have been compensated by catch-up policies in the 1990s. A fall in the public wage gap is observed in the recent period, especially due to the public wage freeze in the wake of the Great Recession.

We also analyze how the 'quality' of civil servants, as measured by the unobservable skill gap, changes over time and is possibly affected by various factors including the public pay gap itself. Over the entire period, we find a significant degree of positive selection into the public sector – both on observed and unobserved skills – at all wage levels. This reflects the fact that the French public sector has managed to attract 'good' workers through a selective recruitment process, despite lower financial returns, and possibly because of compensating differentials including job security and the intrinsic motivation attached to the sense of public service. However, the share of public wage gap explained by individual heterogeneity has strongly declined in the 1990s due to an harmonization of education levels across sectors and the diversification to less selective recruitment schemes in the public sector. More recently, positive selection has totally vanished in the upper part of the wage distribution, suggesting the detrimental effect of the wage freeze and the absence of performance-based remuneration among public sector executives.

This paper is organized as follows. Section 2 presents the data and raw wage trends. Section 3 explains the empirical strategy and the estimators. Results and extensive robustness checks are discussed in section 4 while section 5 concludes.

2 Data

2.1 Datasets

We use detailed administrative data, the *Panel tous salariés* (PTS), recently provided through secured access by the French national statistic institute INSEE. The dataset is based on annual compulsory records of employees (DADS) that are filled each years by all the French firms. It is completed by wage records from the public sector. Compared to traditional survey data on wages, like the French Labor Force Survey (FLFS), registered data basically avoids response errors. With a sampling rate of 1/25, it is also highly representative of all French salary workers (in comparison, the FLFS is sampled at 1/300). It contains around 2.4 million individuals and about 20 million panel observations for the period 1988-2013. A third advantage is the panel dimension, which allows following French employees over 12 years on average and at best over the 25 years.[6]

A possible drawback often encountered with administrative data is the limited set of relevant variables. We avail of information on age, gender and occupation type in the PTS. To the extent that workers' other characteristics are broadly time-invariant (like education levels), they should be picked by fixed effects. Nonetheless, our main estimations will be based on a random subset of the PTS that is combined with the *Echantillon Démographique Permanent* (EDP), i.e. a demographic dataset drawing from civil state registers (for birth, marriage, etc.), Census data (1982, 1990, 1999, then yearly since 2004) and other administrative information. This combined dataset (PTS+EDP) is a panel containing information on education (highest diploma obtained), the number of children and the marital status of French salary workers in addition to the PTS data. As can be seen in Table 1 – which we discuss below – both PTS and PTS+EDP samples are very large so that there is hardly any visible difference in mean values of the key variables present in both datasets. We will also verify that both of them yield similar estimation results. Finally, an advantage of using the smaller PTS+EDP is computational time.[7]

[6]The sample comprises information on all French workers born in October of each even-numbered year, for the period under study. An exception is the year 1990 when no data was collected for DADS due to the workload caused by the collection of the 1990 Census to national statistics services, and 1994 when a large number of errors on individual identification numbers occurred.

[7]Jackknife estimations and bootstraps for confidence intervals take a huge amount of time when run on the PTS data set via the INSEE secured server. We have nevertheless run the main estimations on both datasets and found almost identical results.

2.2 Sample Selection, Wage and Statistics

Sample Selection and Background information. Our selection goes as follows. The data focuses on employees only, which means that self-employed and farmers are not included. This is not a concern for our purpose since our wage gap measures should homogeneously concentrate on salary workers in public versus private sectors. Additionally, we drop all the persons not counted in the active population or not in work. We keep workers aged 18-60 to avoid very specific positions (apprenticeship, for the youth, and early retirement in the public sector). We also drop workers present less than four times in the panel.[8] Note that our baseline includes salary workers of all types of civil services: State/national (*fonction publique d'Etat*, FPE), regional/local (*fonction publique territoriale*, FPT) and health services (*fonction publique hospitalière*, FPH). The large majority, 73%, is composed of tenured servants, with unlimited contract and job protection.[9] The private sector is dual and essentially composed of permanent contracts ("CDI") and fixed-term contracts ("CDD"). Maybe surprisingly, the evolution of the public sector has led, by the end of the studied period, to a higher rate of short-term contracts (16%) than in the private sector (12%), as highlighted in Le Barbanchon and Malherbet (2013).[10]

Hourly Wage Computations. We construct hourly wages as follows. We focus on the main job (we ignore secondary activities and side-jobs). Net annual earnings from the main job include wages, extra-hour payments, all bonuses, profit-sharing, taxable in-kind benefits and professional expenses – taken net of social security contributions. We focus on full-time jobs and impute contract work hours to calculate hourly wage rates. This choice is motivated by the fact that for the State civil service, annual work duration is recorded in days and not in hours. Thus, annual net earnings are divided by the recorded number of annual working weeks and then by the statutory work duration per week for a full-time job.[11] Full-time work is highly regulated in France: it was traditionally set

[8]These individuals are mainly found near the base and end years. We have checked that this selection step does not affect the representativeness of our sample. Those present only once in the panel are anyway dropped in fixed effect estimations of the public wage gap.

[9]The rest is recruited as non-tenured employees on permanent or fixed-term contracts (17.2% overall and 15.8%, 19.4% and 16.5% in State, regional and health services respectively) or comprises the military (5.5%), specific contracts (for education and health specialists from the private sector) and subsidized contracts.

[10]Since contract types are correlated with wages and, by composition effect, with the type of sector, we will present sensitivity analyses where we additionally control for short-term contract versus permanent/tenured positions. Unfortunately, this information is available only for the period 2005-2013.

[11]We could impute statutory hours for part-time jobs but they vary quite substantially. We will provide estimations for regional and health public services, for which annual work hours are available, to check the implication of excluding part-timers. Note that the proportion of part-time work differs a little across

	Panel Tous Salariés (PTS)						PTS + EDP					
	Base year 1988		End year 2013		All years		Base year 1988		End year 2013		All years	
	Public	Private	Public	Private	Public	Private	Public	Private	Public	Private	Public	Private
Log wage rate (1)	2.38	2.29	2.63	2.58	2.52	2.43	2.38	2.29	2.63	2.58	2.52	2.43
	(0.34)	(0.45)	(0.36)	(0.45)	(0.44)	(0.50)	(0.34)	(0.45)	(0.36)	(0.46)	(0.44)	(0.50)
Demographics												
Age	37.4	34.5	43.5	40.5	41.1	37.6	37.2	34.4	43.6	40.5	41.0	37.5
Female	53%	33%	59%	36%	58%	35%	53%	33%	59%	36%	58%	35%
Married	-	-	-	-	-	-	47%	42%	45%	40%	47%	40%
Married with children	-	-	-	-	-	-	39%	34%	38%	34%	39%	33%
Experience and Occupation												
Executive (2)	14%	9%	21%	17%	21%	13%	14%	8%	21%	17%	20%	14%
Intermediate professions (3)	38%	21%	30%	21%	32%	23%	38%	21%	30%	21%	32%	23%
Employees and workers (4)	48%	70%	49%	62%	46%	63%	48%	70%	49%	62%	46%	63%
Education												
Secondary education	-	-	-	-	-	-	21%	32%	11%	17%	16%	23%
Vocational training	-	-	-	-	-	-	30%	43%	29%	37%	28%	39%
High school	-	-	-	-	-	-	19%	12%	17%	19%	16%	16%
University first degree	-	-	-	-	-	-	17%	8%	19%	16%	19%	13%
University upper degrees	-	-	-	-	-	-	13%	6%	25%	12%	20%	10%
# panel observations					16,338,134						2,170,706	
# workers					1,308,622						171,347	
Share of public sector workers					28%						28%	
Average # of periods observed by worker					12.5						12.7	
% of individuals with at least one sector move overall					6.9%						7.1%	
Average % of workers moving per year*					0.9%						0.9%	
Average % moving from public to private per period					0.4%						0.4%	
Average % moving from private to public per period					0.5%						0.5%	

Statistics in this table describe our selection of salary workers in the private and public sectors (aged 18-60, present in at least 4 waves of the 1988-2013 panel). PTS is the "Panel Tous Salariés" (registered data on employees), EDP is the "Echantillon Démographique Permanent" (additional demographic information).

(1) PTS and PTS+EDP data on French salary workers' wages in 2002 euros, include bonuses and premium; standard errors in brackets.

(2) Includes administrative, commercial or technical executives, professors and 'higher intellectual professions'.

(3) Includes intermediary positions in commercial, technical and administrative sectors, health seervices, teachers, technicians.

(4) Commercial, technical and administrative employees and clerks.

* movers transit mostly to/from regional public services (40% of the moves each way) or to/from state public services (34% of the moves each way) while stayers are to be found mainly in the state public services (49%).

Table 1: Descriptive Statistics

at 39 hours per week and changed to 35 hours in 1998-2002. This transition took place gradually over this period in the private sector and in 2002 in the public sector. The reform was fully compensated in terms of earnings – annual wages did not decrease – so that hourly wages sharply rose over 1998-2002 in the private sector, but only in 2002 in the public sector.[12] Given this transition pattern, we expect a temporary drop of the public sector wage gap during 1998-2002.

sectors (30% in the public sector versus 25% in the private sector over all years).

[12]In the private sector, the new law ("loi de Robien") imposed a different timing depending on firm characteristics but in practice, firms were allowed to choose the transition date between 1998 and 2002. From our data, we were able to infer this date from the discontinuous change in average worked hours in each firm in the PTS.

Workers' Characteristics. After selection, our sample based on PTS data contains around 1.3 million individuals and about 16.3 million panel observations. The PTS-EDP matched sample represents around a eighth of it. Table 1 provides an overview of the characteristics of public and private sector workers in both samples. The first row allows comparing log wages across sectors overall or for base and end year data. We find a raw wage gap of around 9% overall, which tends to decrease during 1988-2013 (see also the first row of Appendix Table A.1). Table 1 shows that PTS and PTS+EDP give very identical log wage values.

The rest of the table focuses on workers' characteristics. Public sector workers are on average 3.5 years older. The main explanation pertains to the fact that the majority of civil servants are tenured through a system of competitive examination at the national level (*concours de la fonction publique*). Transiting from the private sector to a tenured civil servant position depends on the candidate's performance that usually improves with experience. Table 1 also shows that more than a half (a third) of the public (private) sector is composed of women. The gender difference is consistent with the occupational distribution within each sector.[13] Job types recorded in the table correspond to three hierarchical positions. We observe a relatively larger share of executives and intermediate positions in the public sector, partly related to the age differences (public sector workers being older and with more work experience).

PTS and PTS+EDP data show very similar figures regarding age, gender and job compositions of both sectors. The PTS+EDP additionally provides information about education and family status. Marriage is more frequent among public sector workers, which is mechanically related to the age difference. As expected, the public sector is characterized by much higher education levels. This pertains to the type of jobs often encountered in the administration (education and health professionals, civil engineering, etc.) and is consistent with the lower rate of blue collars. It is also related to the selective national examination system to enter the tenured civil sector, as indicated above. Eligibility is granted on the basis of minimum requirements regarding high school diploma and university degrees. We can notice a dramatic increase in education levels over time, which tends to reduce the gap across sectors (the differential in the secondary education rate decreases from 11 to 6 points while the rate of high school graduates becomes larger in the private sector, for instance).

The lower part of Table 1 shows information about data structure and moves across sectors. Public sector employees represent around a quarter of our samples. The average

[13] In particular, the national education system comprises a majority of female workers while health and social services also account for a large part of the gender orientation of the public sector.

panel duration is similar in both PTS and PTS+EDP. Importantly for fixed effects estimations, which rely on transitions across sectors, we find a substantial number of movers in the data. Around 7% of all the individuals present in the panel – around $90,000$ persons in the PTS – have transited across sectors at least once. The fraction of moves from private to public sectors is larger than the reverse transition, yet both are represented and contribute to identify public sector wage gaps. Note that we will check how estimates vary (or not) depending on the move direction.

Raw Wage Gaps. The upper graph of Figure 1 depicts the time change in hourly wage levels in both sectors and at different percentiles of the pooled wage distribution (10th, 50th and 90th). We use our baseline hourly wage measures relying on statutory weekly work duration, as described above. The average raw gap seems in favor of the public sector at all points in time at the median and at the 10th decile. At the top of the distribution, hourly wages are fairly similar in both sectors and the gap turns negative in most of the years. The wage trends are relatively parallel (across sectors and across wage levels), even if they seem a little steeper at the 90th quantile.

The lower graph represents the raw difference in log wages across sectors. The baseline (solid lines) confirms previous observations: a positive gap in the first half of the distribution, which oscillates between around 10% and 20%, and a negative gap at the top, which varies between -10% and just above zero. The evolution first consists of a rising gap at all wage levels until the mid-1990s. It is consistent with the public wage policies of the period and, particularly, with three policy plans (1990, 1993 and 1995) that aimed to boost public sector remunerations. We come back to this later since better correlations with actual policy measures are expected to be seen once wage gaps are cleaned from essential workers' differences across sectors.

Then, the 1998-2002 period is characterized by the '35-hour workweek' reform and different timings of transition across sectors, as explained. Recall that hourly wages increased because the reduction in the legal working time was fully compensated. Since the reform was first implemented in the private sector, gradually during 1998-2002, the public hourly wage gap mechanically dropped until 2002 (year when the public sector then also switched to the 35-hour workweek). To assess the raw wage gaps in absence of the reform effect, we calculate hourly wages using a counterfactual 35-hour workweek at all years over 1988-2013 (dashed line). Ignoring the 35-hour transition, the raw gap is similar to the baseline except for the years 1998-2002.[14] The no-reform counterfactual indicates an

[14]Note that a small drop is also visible at the end of the 1990s under the no-reform counterfactual scenario, especially for top wages. It is driven by a relative decrease in the annual work duration due to new hirings (additional statistics available from the authors). It adds up tot the workweek transition

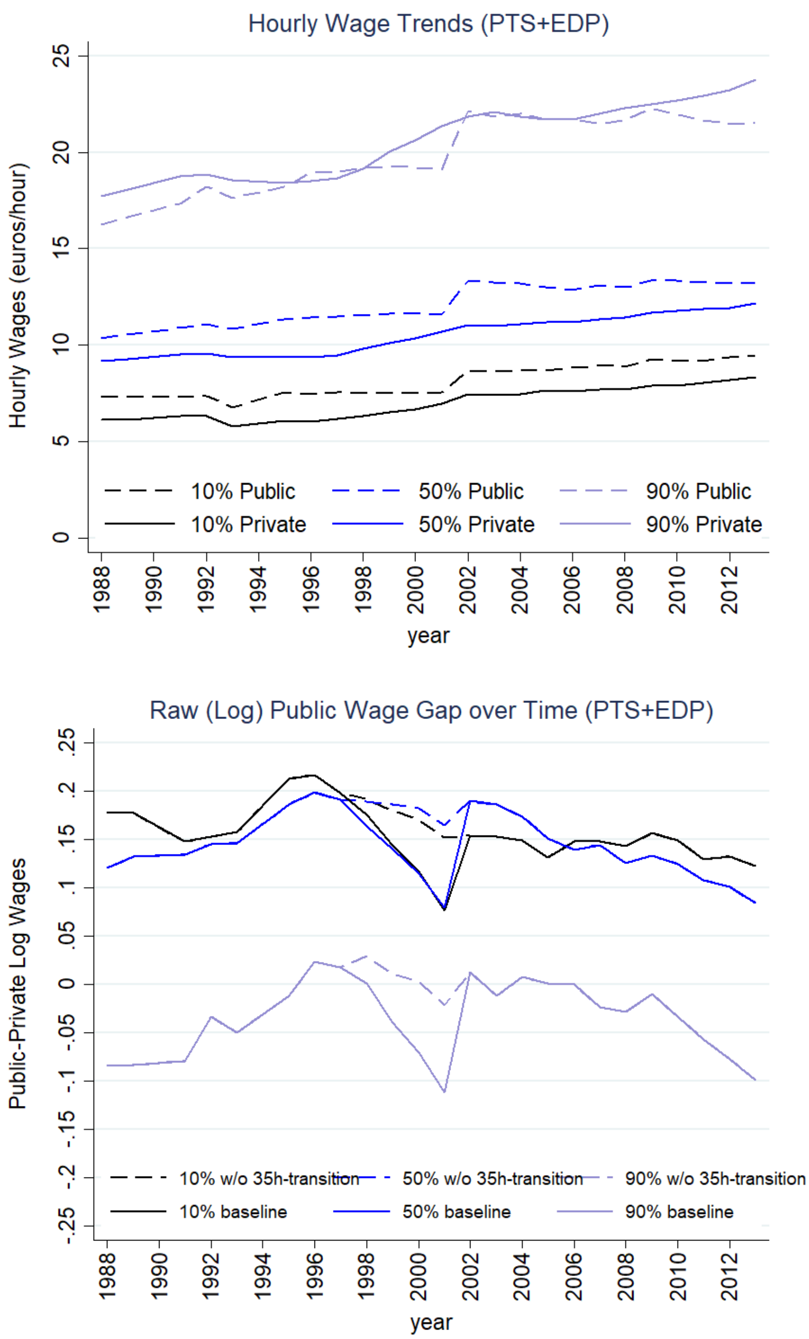


Figure 1: Evolution of the Raw Public Wage Gap by Quantile

almost continuous decline in the public wage gap over the long period, started in the late 90s and until 2013. This decline seems more marked at the bottom of the distribution.

3 Econometric Methods

3.1 Fixed Effects Quantile Regression

We address the potential endogeneity problem of the employment sector using a fixed effects approach. Since we are interested in the difference between the whole public and private sector wage distributions, we use quantile regression methods. Recently, there has been an active literature about the estimation of quantile models in the presence of fixed effects. We succinctly summarize the different approaches and explain our empirical strategy.

One of the main issue of fixed effects quantile regression is the incidental parameter problem. This is a general problem of nonlinear models estimated on panel data, first discussed in Neyman and Scott (1948). In general, there is no transformation of the data that can remove the dependence on the fixed effects in such models – first-differencing (or time de-meaning) works in linear mean regression, due to the linearity of the expectation operator, but not with nonlinear models like quantile regressions. Thus, any estimator will be a function of the estimated fixed effects, which cannot be estimated consistently when the number of periods is finite. This is a serious issue because, in most cases, the number of periods is limited while the number of units is large. In our application, with more than 1 million individuals observed on average during 12 periods (with a maximum of 25 periods), we must and can take this potential bias into account.

Let Y_{it} denote the outcome (log wage) for observations $i \in \{1, 2, ..., n\}$ in period $t \in \{1, 2, ..., T\}$. We also observe a vector of regressors X_{it} and the public sector indicator variable S_{it}. Several fixed effects quantile models and estimators have been suggested. Koenker (2004) assumes that the individual fixed effects α_i only shift the conditional distribution of the outcome without changing its shape:

$$Q_{Y_{it}}\left(\theta | X_{i1}, ..., X_{iT}, S_{i1}, ..., S_{iT}, \alpha_i\right) = X_{it}'\beta\left(\theta\right) + S_{it} \cdot \gamma\left(\theta\right) + \alpha_i, \tag{1}$$

where $Q_Y\left(\theta | X\right)$ is the θ^{th} conditional quantile of Y given X for some $0 < \theta < 1$. This is a linear quantile regression model as introduced by Koenker and Bassett Jr (1978) with individual fixed effects as additional regressors. In model (1), the fixed effects are treated differently from the other regressors: they are constrained to be the same at all quantiles. This may seem to be an unnatural assumption in a quantile regression setting

effect to yield the pronounced U-shape pattern of 1998-2002 in our baseline.

where the goal is precisely to analyze the heterogeneity of the effects. On the other hand, it considerably reduces the dimension of the problem since we have to estimate only one fixed effect for all quantiles instead of a whole distribution of fixed effects per individual.

Koenker (2004) suggests to impose the cross-quantile restrictions on the fixed effects by estimating jointly several quantile regressions.[15] This very large problem can nevertheless been solved in a reasonable amount of time by exploiting the sparse structure of the matrix of regressors. Canay (2011) also suggests an alternative 2-step estimator. He notes that assuming (1) for all $\theta \in (0,1)$ implies :

$$\begin{aligned} E\left[Y_{it}|X_{i1},...,X_{iT},S_{i1},...,S_{iT},\alpha_i\right] &= \int_0^1 \left(X_{it}'\beta\left(\theta\right)+S_{it}\cdot\gamma\left(\theta\right)+\alpha_i\right)d\theta \\ &= X_{it}'\int_0^1\beta\left(\theta\right)d\theta+S_{it}\cdot\int_0^1\gamma\left(\theta\right)d\theta+\alpha_i \\ &\equiv X_{it}'\bar{\beta}+S_{it}\cdot\bar{\gamma}+\alpha_i \end{aligned}$$

which is a linear fixed effect model for the mean. Thus, he suggests to first compute the traditional within-estimate of the fixed effects $\hat{\alpha}_i$ using linear regression. In a second step, each quantile function can be estimated by a standard linear quantile regression because

$$Q_{Y_{it}-\alpha_i}\left(\theta|X_{i1},...,X_{iT},S_{i1},...,S_{iT},\alpha_i\right)=X_{it}'\beta\left(\theta\right)+S_{it}\cdot\gamma\left(\theta\right).$$

This simplifies the computation of the estimates compared to the joint estimator of Koenker (2004). However, this estimator is also inconsistent with a finite number of periods because $\hat{\alpha}_i$ suffers from the incidental parameter bias.

Kato et al. (2012) and Kato and Galvao (2016) consider quantile regression models with individual quantile-specific fixed effects:

$$Q_{Y_{it}}\left(\theta|X_{i1},...,X_{iT},S_{i1},...,S_{iT},\alpha_i\right)=X_{it}'\beta\left(\theta\right)+S_{it}\cdot\gamma\left(\theta\right)+\alpha_i\left(\theta\right). \tag{2}$$

The individual effects are only allowed to shift the distribution in (1) while in (2), they can affect the whole distribution of the outcome. While this extra flexibility may certainly be useful to accommodate more complex patterns in the data, it comes at a cost, i.e. the necessity to estimate a whole function for each individual. In practice, the authors estimate the parameters by running separate quantile regressions for each quantile of interest. Obviously, even without covariates, only an approximation of $\alpha_i\left(\cdot\right)$ consisting of T different values can be estimated.

[15] He also considers shrinking the individual effects toward a common value to reduce the dimensionality of the problem and the variance of the estimates. We do not pursue this approach here and prioritize the reduction of the bias resulting from endogenous sector choice.

In our application, this approach is not computationally feasible due to the large number of observations, the large number of quantile regressions needed to obtain the unconditional effects as explained in Section 3.2, and the number of bootstrap replications needed to estimate the variance. In addition, our Monte Carlo simulations show a relatively large incidental parameter bias for this estimator, which is probably due to the functional individual effects $\alpha_i(\cdot)$.[16] On the other hand, the location shift model for the individual effects in (1) seems not natural and is rejected by the data in our application.

For this reason, we suggest an intermediate model with interacted fixed effects:

$$Q_{Y_{it}}(\theta|X_{i1},...,X_{iT},S_{i1},...,S_{iT},\alpha_i) = X'_{it}\beta(\theta) + S_{it}\cdot\gamma(\theta) + \alpha_i\cdot\delta(\theta). \tag{3}$$

This model treats the observed (X_{it} and S_{it}) and unobserved (α_i) regressors symmetrically by keeping them constant over the distribution but allowing them to have a different effect at each quantile. We could imagine further extensions of this model by including several individual fixed effects with different coefficients at each quantile. Ultimately, with T different individual fixed effects, we would be back to the completely flexible model (2). For computational reasons we use the simple interacted model (3) in our application. Assuming (3) for all $\theta \in (0,1)$ implies

$$\begin{aligned} E[Y_{it}|X_{i1},...,X_{iT},S_{i1},...,S_{iT},\alpha_i] &= \int_0^1 \left(X'_{it}\beta(\theta) + S_{it}\cdot\gamma(\theta) + \alpha_i\cdot\delta(\theta)\right)d\theta \\ &= X'_{it}\int_0^1 \beta(\theta)\,d\theta + S_{it}\cdot\int_0^1 \gamma(\theta)\,d\theta + \alpha_i\cdot\int_0^1 \delta(\theta)\,d\theta \\ &\equiv X'_{it}\bar{\beta} + S_{it}\cdot\bar{\gamma} + \alpha_i\bar{\delta} \end{aligned}$$

which is a linear fixed effect model for the mean. Without loss of generality, we normalize $\bar{\delta} = 1$. Similarly to Canay (2011), we compute in the first step the traditional within-estimate of the fixed effects $\hat{\alpha}_i$. In the second step, we regress Y_{it} on X_{it}, S_{it}, and $\hat{\alpha}_i$ via traditional quantile regression. The coefficients on $\hat{\alpha}_i$ allow us to test the location shift model (1). Of course, since $\hat{\alpha}_i$ is consistent for α_i only at the $\sqrt{T}$ rate, this estimator will also suffer from the incidental parameter bias. As explained in Section 3.3 below, we reduce the bias using the jackknife.

3.2 Unconditional Quantile Effects

The results of conditional quantile regression models with fixed effects must be interpreted carefully. It is tempting to interpret the results at low quantiles as the effect for low

[16] Monte Carlo simulations mentioned throughout the paper are not reported but are available from the authors upon request.

earners and the effect at high quantiles as the effect for high earners. This is only correct conditionally on the covariates, among which there are individual fixed effects. This means that, for high (low) values of θ, $\gamma(\theta)$ provides the effect of being employed in the public sector during the periods with high (low) wages. In other words, the inter-personal differences are captured by the fixed effects while the variation of γ over the distribution captures differences over time. This is a very specific piece of information. Besides, policy makers are certainly interested in knowing the public sector effect on the *unconditional* wage distribution. It is clearly much easier to interpret and also more policy relevant. Issues about income inequality, for instance, are always stated in absolute terms and not conditionally on individual unobserved ability.

For this reason, we shall estimate the public sector effect on the unconditional wage distribution. We follow the procedure suggested by Chernozhukov et al. (2013) with the difference that one of the regressor (the individual fixed effect) has been previously estimated. Let us now describe the algorithm that we use:

Algorithm 1

1. *Using a standard fixed effects estimators for the mean, we obtain the estimated individual fixed effects* $\hat{\alpha}_i$.

2. *We estimate 100 quantile regressions of* Y_{it} *on* X_{it}, S_{it} *and* $\hat{\alpha}_i$ *on a regular grid of 100* θ_q *quantiles. For* $q = 1, ..., 100$, *we obtain the estimates* $\hat{\beta}(\theta_q)$, $\hat{\gamma}(\theta_q)$ *and* $\hat{\delta}(\theta_q)$.

3. *The estimated counterfactual unconditional distributions in the private and public sector take respectively the following forms:*

$$\begin{aligned} \hat{F}_{Y(0)}(y) &= \frac{1}{100 \cdot n} \sum_{i=1}^{n} \sum_{q=1}^{100} 1\left(X'_{iT}\hat{\beta}(\theta_q) + \hat{\alpha}_i \cdot \hat{\delta}(\theta_q) \leq y \right) \\ \hat{F}_{Y(1)}(y) &= \frac{1}{100 \cdot n} \sum_{i=1}^{n} \sum_{q=1}^{100} 1\left(X'_{iT}\hat{\beta}(\theta_q) + \hat{\gamma}(\theta_q) + \hat{\alpha}_i \cdot \hat{\delta}(\theta_q) \leq y \right) \end{aligned}$$

4. *We report the unconditional quantile public sector effects*

$$\hat{\Delta}(\tau) = \hat{F}^{-1}_{Y(1)}(\tau) - \hat{F}^{-1}_{Y(0)}(\tau)$$

   for a grid of quantiles τ.

The estimated parameter is the difference between the τ quantile of the unconditional distribution that we would observe during the last period if everybody was employed in the public sector and the τ quantile of the distribution that we would observe if everyone was employed in the private sector. These unconditional distributions are obtained by integrating the conditional distributions over the distribution of covariates, including the estimated fixed effects, during the last period. The conditional distribution functions are approximated using 100 quantile regressions defined in Section 3.2. Given the multi-step procedure, standard errors are obtained by bootstrap with 100 replications. More replications would have been computationally too long and confidence intervals are already sufficiently narrow, as we shall see in the result section, to draw clear-cut conclusions.

3.3 Incidental Parameter Bias Correction

All the estimators discussed in Sections 3.1 and 3.2 suffer from the incidental parameter bias. Even if the number of individuals is very large, these estimators will be biased when the number of periods is finite. Arellano and Weidner (2015) characterize the bias of the estimator of model (2). They show that when the number of periods is moderate, the fixed effects estimators will underestimate the heterogeneity along the distribution by averaging the quantile coefficients around the quantile of interest. In the extreme case when $T = 2$, the estimated coefficients will be constant as a function of the quantile index θ. Thus, naively applying fixed effects quantile regression to short panels may give the impression that unobserved heterogeneity is explaining all of the variation along the distribution while this is only the consequence of the incidental parameter bias.

We adapt the half-panel jackknife correction suggested by Dhaene and Jochmans (2015) to the present context. Suppose that the number of periods T is even. Let $\hat{\gamma}(\theta)$ be the estimate based on the whole panel. We also compute the estimates based on the first $T/2$ periods and the last $T/2$ periods, which we respectively denote by $\hat{\gamma}_1(\theta)$ and $\hat{\gamma}_2(\theta)$. The bias-corrected estimator is given by

$$\begin{aligned}\hat{\gamma}_{BC}(\theta) &= \hat{\gamma}(\theta) - [0.5 \cdot (\hat{\gamma}_1(\theta) + \hat{\gamma}_2(\theta)) - \hat{\gamma}(\theta)] \\ &= 2 \cdot \hat{\gamma}(\theta) - 0.5 \cdot (\hat{\gamma}_1(\theta) + \hat{\gamma}_2(\theta)).\end{aligned}$$

The intuition is very simple: Since the incidental parameter bias is proportional to $\frac{1}{T}$, the bias of $0.5 \cdot (\hat{\gamma}_1(\theta) + \hat{\gamma}_2(\theta))$ is twice as large as the bias of $\hat{\gamma}(\theta)$. Thus, the difference between these estimates provides an estimate of the bias, which we substract from the original coefficient estimate.

We did numerous simulations that confirm the theoretical results and show a very significant reduction of the bias, yet at the price of seriously increasing the variance of the estimator. We could reduce the variance of the jackknife bias correction by incorporating the information about the mean coefficients. We know that the traditional fixed effect estimator, denoted by $\hat{\gamma}$, is unbiased even when T is as low as 2. At the same time, model (3) for all $\theta \in (0, 1)$ implies that $\gamma = \int_0^1 \gamma(\theta)\, d\theta$. Thus, our final estimator of $\gamma(\theta)$ is the recentered bias corrected estimator

$$\hat{\gamma}_{RBC}(\theta) = \hat{\gamma}_{BC}(\theta) + \hat{\gamma} - \int_0^1 \hat{\gamma}_{BC}(\theta)\, d\theta.$$

In simulations, the variance of this estimator is much lower than the variance of $\hat{\gamma}_{BC}(\theta)$ and only marginally larger than the variance of the uncorrected estimator $\hat{\gamma}(\theta)$.

We also use the half-panel bias correction for the estimator of the unconditional effects defined above. We correct both the first-stage quantile regression coefficients $\hat{\beta}(\theta), \hat{\gamma}(\theta)$ and $\hat{\delta}(\theta)$ and the second stage counterfactual quantile functions $\hat{F}^{-1}_{Y(1)}(\tau)$ and $\hat{F}^{-1}_{Y(0)}(\tau)$.

4 Results

We first present overall public quantile effects, then discuss the evolution of average wage gaps over time, and finally the time changes at different points of the distribution. Empirical results are reported in graphic form throughout this section while the Appendix Table A.1 gathers the main estimates with standard errors. The rest of the present section suggests an extensive robustness analysis and more heterogeneous effects.

4.1 Public Wage Gaps Along the Distribution

Raw Gap and Unconditional Quantile Effects. We first focus on the public sector wage gap across the whole wage distribution using pooled panel years.[17] The left hand side graph of Figure 2 summarizes our results, comparing raw wage differentials, unconditional quantile effects (UQE hereafter) and fixed effects quantile effects (FE-UQE). As seen before, raw wage differentials are fairly large in the first three-quarter of the distribution and becomes null at the top (they are 15%, 13% and -2.5% at the 10th, 50th and 90th percentiles respectively). In contrast, the UQE are much smaller: the pay gap once workers' observed characteristics are controlled for is only 5%, 0% and -2.5% at the 10th, 50th and 90th percentiles. The difference with raw gaps essentially conveys that civil servants have 'better' observables than private sector counterparts, all along the distribution except at the top. We have indeed described above their higher potential experience and education levels (due notably to the public sector selection process based on minimum degree level requirements).[18]

[17] Note that the unconditional quantile effects are evaluated at the characteristics of this pooled sample.

[18] Differences could also reflect gender compositions if there is less gender discrimination in the public sector where women represent a larger share of the workforce. This is not the case and we will show that estimates are very similar for men and women.

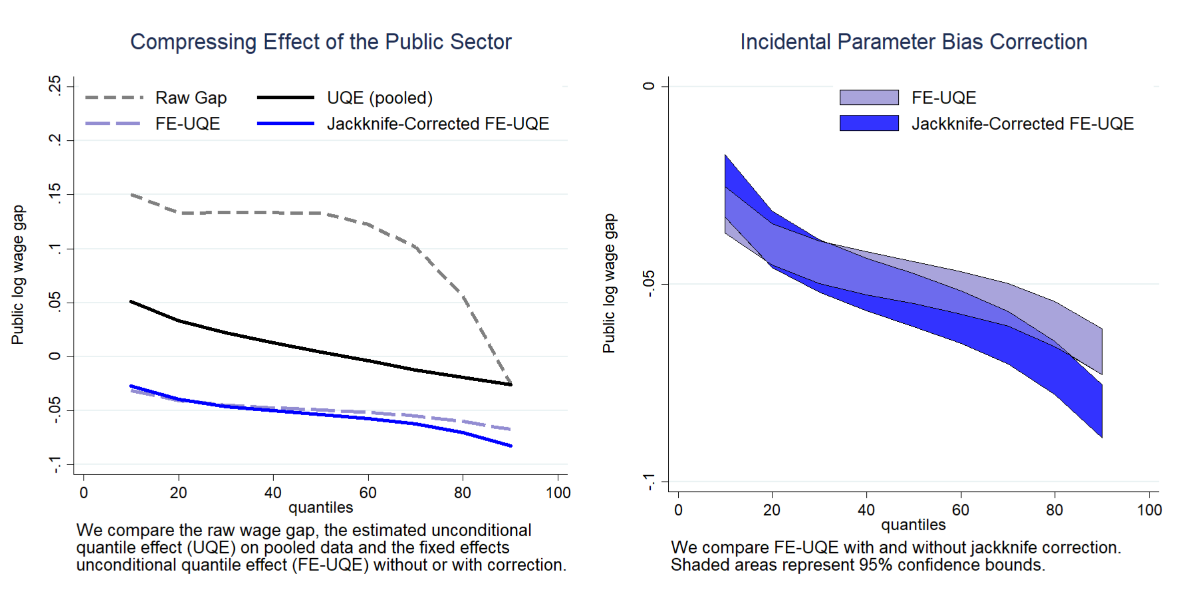


Figure 2: Public Wage Gap Estimation

Note that UQE estimates point to positive wage gaps at the bottom and negative ones at the top, i.e. a *compressing effect* of the public sector on the pooled wage distribution. This effect is found in many studies that do not account for fixed effects (for instance in Mueller, 1998, Melly, 2005, or Lucifora and Meurs, 2006). That the public sector tends to reduce wage inequalities compared to private wage setting seems reasonable. Governments are indeed supposed to ensure a good pay for lower-skilled workers while the public opinion may constrain them to moderate earnings of high-skilled civil servants.

Fixed Effects and Positive Selection in the Public Sector. Turning to FE-UQE, Figure 2 (left) shows that an additional part of the raw wage gap is explained by the positive selection of workers with better (time-invariant) unobservable characteristics in the public sector. This time, this contribution is verified at all quantiles including the top. Taking into account the differences in unobservable skills unveils the presence of public wage penalties at all points of the distribution (from -3% at the 10th percentile to -7% at the 90th). This is a relatively different picture compared to the bulk of the literature showing public sector premium at lower quantiles and penalties at the top (for instance Lucifora and Meurs, 2006, and other references in the introduction). A likely explanation is that past evidence mainly relies on quantile estimates without fixed effects (as illustrated by the difference between UQE and FE-UQE here).

To summarize, our results indicate that over the long run, the French public sector pays less – the penalty increasing with the wage level – but succeeds in attracting workers with ‘good’ unobserved characteristics all along the distribution. Despite lower financial returns, this attraction may pertain to specific amenities (including job protection) and elements of intrinsic motivation (a traditional sense of public service that relates to the public sector ‘mission’ described in Besley and Ghatak, 2005, or public service motivation as defined in Francois, 2000). On the demand side, positive selection also reflects the relative efficiency of the national examination process in selecting talented workers in the pool of applicants.

Jackknife Corrections and the Public Sector Wage Compression. We observe that part of the compressing effect of the public sector is reduced when fixed effects are introduced (long dash line in Figure 2). This flattening is typically observed in the studies based on panel data estimations of conditional quantile effects (for instance Siminski, 2013, Bargain and Melly, 2008, or Campos and Centeno, 2011, table 5.5, for a wide range of countries) or unconditional quantile effects (Hospido and Moral-Benito, 2016). These studies hence explain the compression effect by a positive selection in the public sector that prevails in the lower part of the wage distribution. Yet, as indicated in Section 3.2, applying fixed effects quantile regressions to short panels may attribute an excessive role to unobserved heterogeneity. The incidental parameter bias is likely to explain most of the flattening when fixed effects are introduced.

An important contribution of the present paper is to suggest a jackknife approach to reduce this bias. Figure 2 (left) reports Jackknife-corrected FE-UQE, showing that the correction restores most of the compression effect that we found with UQE on pooled data. Figure 2 (right) zooms on this comparison and reports the 95% confidence interval. It highlights a significant effect of the correction at the top. Simple tests convey that the difference between the 10th and 90th percentiles – a convenient summary measure of the compression effect – is significantly increased with the correction. This is true in the early period and overall (see the last row of Appendix Table A.1). In conclusion, the hypothesis of a compressed wage profile due to non-competitive wage settlements in the public sector cannot be completely overruled. It also means that a substantial positive selection into public jobs is also present at the top over the long period, an apparently good news for the sake of quality management in the administration. Unfortunately, as we shall see, this positive selection tends to disappear in the recent years.

4.2 Time Trends in the Public Sector Wage Gap

Few studies look at the evolution of the public wage differential over time, especially when unobserved skills are taken into account. Yet, as noticed by Disney and Gosling (2008), the time-varying public wage differential has relevant policy implications. Our panel is sufficiently long to interpret trends in the wage gap over the long run. We also study how the unobserved skill gap between public and private sector employees varies over time and relates to changes in the public wage gap itself and, more generally, to public sector policies and the selectivity of recruitment schemes.

Evolution of the Average Gap: General Conclusions. Using year dummy interaction, we estimate the average wage gap at different points in time while controlling for basic covariates (OLS) and, additionally, for fixed effects (FE). Figure 3 compares these estimates to the trend in the raw wage gap. We find that the average public wage gap becomes null when observables are controlled for: OLS estimates oscillate aroud zero over the long run.[19] It becomes negative when unobservables enter the picture. We conclude again that a large part of the apparent wage gap is explained by simple covariates as potential experience and education level. A smaller part – between a quarter and a half depending on the period – is due to better unobservable skills among civil servants. The difference between raw gaps and OLS estimates tend to decrease over time, and so is the difference between OLS and FE estimates: the positive selection into the public sector based on both observed and unobserved skills seems to gradually shrink.

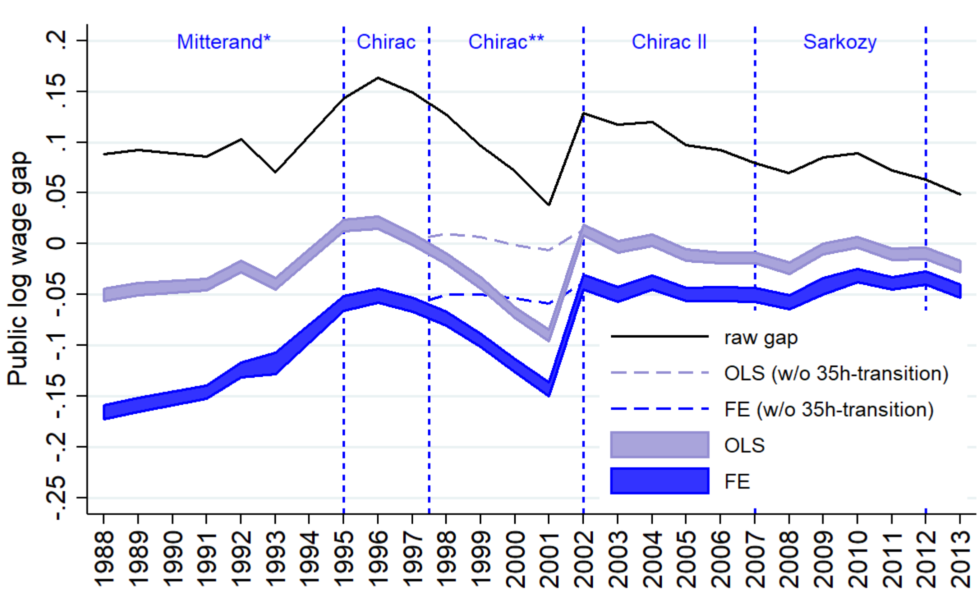
Interpreting the Trends in Public Wage Gaps: Policy and Business Cycles. We now interpret the trends in the estimated public wage gap, explained by a combination of policy and business cycles, taking FE estimates as our best average measure. Policy cycles can be proxied by presidential terms, as indicated at the top of Figure 3.[20] Indeed, the party of the president in power is a good indicator of the general policy governing the public sector in France, with some exceptions as indicated.[21]

The large public sector penalty characterizing the late 1980s reflects the turn to rigor after 1983 in France, accompanied by wage restraint (de-indexation of wages on prices) and the prioritizing of public-debt reduction (implying a decrease in public wage costs). Better conditions under Mitterand's second term have allowed the socialists to conduct

[19] Note that these results at the mean are consistently with the median estimates over all years from the previous section. Precisely, corrected FE-UQE conveyed a public wage penalty of around 5% at the median while FE estimates oscillate between -15% and -3% over 1988-2013.

[20] Mitterand from 1988 to 1995, Chirac I from 1995 to 2002, Chirac II from 2002 to 2007 (note the switch to 5 rather than 7-year terms), Sarkozy from 2007 to 2012.

[21] The Fifth Republic regime in France has given a much prominent role to the president, legitimated by the fact that he/she is elected by direct popular vote. Exceptions include periods of so-called "cohabitation", during which the president has lost majority at the parliament and must nominate a prime minister from the opposition. The duality of the executive power has been experienced by Mitterand (socialist) with PM Balladur (conservative) in 1993-1995 and by Chirac (conservative) with PM Jospin (socialist) in 1997-2002. These exceptions do not invalidate our interpretations over the long period.



Shaded areas represent 95% confidence bounds of the public wage gaps estimated by OLS or FE. Dashed lines show counterfactuals ignoring the 35-hour workweek reform effect. Upper labels indicate periods by presidential mandates.
* Socialist president had to lead jointly with PM from conservative opposition (Balladur) in 1993-95
** Conservative president had lead jointly with PM from socialist opposition (Jospin) in 1997-2002

Figure 3: Average Wage Gap over Time

a compensating policy. Indeed, the 1988-1995 period is marked by measures precisely allowing for catch-up pay increases in the public sector (*plans de réformes catégorielles*).[22] The first half of the nineties is also a period of slack in the labor market with moderate increases in private sector wages. These factors totally rationalize the upward trend, observed both with raw wage gaps and FE estimations in Figure 3, until the mid-1990s.

The following period is easily explained as well since the temporary drop is caused by the 35-hour workweek transition in 1998-2002, as commented before. Abstracting from this reform, we can see that the relative wage progression in the public sector stops: the counterfactual scenario without 35-hour workweek transition (dash lines) shows a relative stagnation. This is possibly the result of two opposite forces: better economic conditions in the late 1990s (improving private wages) and the continuation of the *réformes catégorielles* until 2000.

The slight decline after 2002 is explained by the economic upturn of the period and by Chirac's conservative program including a relative control of public wages. It is however less marked than the strong decline in the raw gap, indicating that the period is also characterized by the continuous decay of the positive selection into public careers. A temporary bounce in the wake of the Great Recession reflects the fact that the crisis has

[22]These policies have started in 1989 (Jospin), 1990 (Durafour) and 1993 (Lang).

primarily affected private wages in 2009-10. From 2010 onwards, the declining trend has resumed and is mainly explained by the policy of Sarkozy's conservative government to balance the public budget by freezing public wages (assorted to a relative decline in public employment as a proportion of the total population from 2007 onwards).

Evolution of the Wage Gap along the Wage Distribution. Figure 4 reports estimates of the jackknife-corrected FE-UQE, taken as our best distributional measure of the public wage gap at different points in time. Confidence intervals show that thanks to the large sample size, estimates are very precise, even at specific quantile and specific periods. Time trends are very similar across unconditional quantiles, with some exceptions. Over 1988-95, the *réformes catégorielles* seem to generate the same relative progression of public sector wages all along the distribution.[23] The last period is also conform to the average trend described above, with a generalized (but slight) decline after 2002. The bounce in 2009 due to cuts in private wages is, as expected, stronger but shorter-lived in the upper half of the distribution, also more affected by the freeze in public salaries after 2010. Note that the compression effect is verified in most years: public wage penalties are larger at higher unconditional quantiles. However, while the compression in the lower half of the distribution oscillates but remains significant, the compression in the upper half (90th/50th percentiles) gradually disappears over the entire period under study.

Evolution of the Positive Selection into the Public Sector. We can directly examine the difference between the raw wage gaps and UQE (or OLS) estimates as a contribution of the observables (the 'observed skill gap') to the raw pay gap. The difference between UQE and FE-UQE (or between OLS and FE estimates) captures the contribution of unobserved skills (the 'unobserved skill gap'). Both are reported in Figure 5. The observed gap is largely positive at the lowest quantiles and at the median but not at the top. As previously discussed, this pattern mainly reflects the selectivity of the public sector recruitment scheme based on minimum degree requirements – and the fact that it matters for those just entering the public sector or at an early career stage, but not for at executive level. The observed skill gap tends to decrease since the mid-1990s, which is consistent with the trend previously observed on average (i.e. the decreasing differential between raw gaps and OLS estimates in Figure 3). It is partly explained by the gradual increase in education levels (recall the relative catch-up of the private sector in terms of

[23] During the 35-hour workweek reform period, however, the underlying trends captured by the no-reform counterfactual (dash line) indicate a relative decline among low earner and a relative progression at the top. This is mainly due to the short-term bounce of the public wage gap during the 2001-02 economic slowdown, which affected private sector workers and more so in the upper part of the distribution given the nature of the downturn (e-bubble crisis).

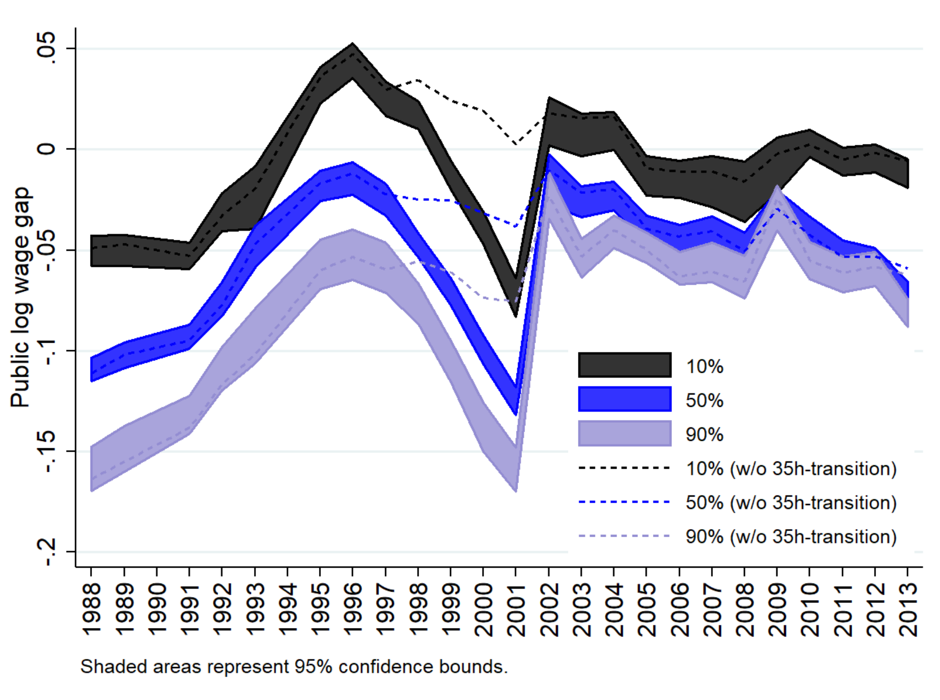


Figure 4: Unconditional Quantile Effects over Time (Jackknife-corrected FE-UQE)

human capital in Table 1).

In Figure 5 (right), the unobserved skill gap is positive on average and very similar across quantiles. It shows a steadily decrease in the 1990s followed by a plateau and a late fall among top earners. Overall, the positive selection into the public sector tends to fade away, which corroborates the average trend previously observed (i.e. a decreasing differential between OLS and FE estimates in Figure 3). In the early period, until the late 1990s, it cannot be explained in financial terms. Indeed, this decade is characterized by a catch-up of public sector salaries, as documented before. Three other factors may come into play. First, the public sector has considerably increased between 1990 and 1996 due to a recruitment policy aimed at anticipating the increase in retirements (Pouget, 2003). It is possible that new entrants were less concerned by the sense of public service. Second, in the same line of reasoning, entries in the public sector were diversified with the opening of non-tenured positions and alternative pathways to the standard competitive exams (Daussin-Bénichou, 2015) – hence an overall move towards less selective recruitment.[24] Third, increased education levels overall make that there is less correlation between ob-

[24]This includes hirings without national competitive examinations ("*Protocole Durafour*" in 1990), 'reserved' examinations ("*Plan Perben*" in 1996), specific pathways to position in the regional and health administration ('Pacte') and later on, specific hiring schemes for low-skill workers ("*Plan Sapin*") and the creation of a special entry system for private sector workers based on experience rather than degree requirement ('*3e concours*') – see Bounakhla (2013).

served degrees and unobserved skills. As a result, the minimum degree requirements to access public jobs are less effective in screening those with better unobservable skills.

According to Figure 5, this trend is broadly the same at all wage levels until 2007. If we now consider the period starting with Sarkozy's conservative government (2007-2012), we observe little movements at low or intermediate quantiles but a sharp decline at the top. Positive selection almost completely disappears at higher quantiles. Two tentative explanations can be given. First, intrinsic motivation may not resist to a continuous decline in the public pay gap over the period, characterized by sharp wage cuts in real terms (-6.5% during this presidential term) and a symbolic nominal wage freeze started in 2010 (and continuing under the following socialist government).[25] Second, this effect would prevail at the top because the rise in unemployment following the Great Recession would maintain some of the public sector attractivity in the lower part of the distribution (for top earners, unemployment rate remains below 5% at every point in time during the studied period).

More research is needed to elicit the extent to which intrinsic motivation (public service motivation as discussed in Besley and Ghatak, 2005, or Francois, 2000) may act as a compensating differential. Yet, recent studies tend to show that higher wages may actually attract better job applicants in the public sector (Dal Bo et al., 2013). In that sense, the explanations above are plausible: in the top of the distribution, a continuous degradation of real-term public wages may have eventually discouraged those with better observed characteristics (left graph) and also better unobserved abilities (right graph).

4.3 Robustness Checks and Additional Results: A Summary

We suggest several robustness checks aimed at validating our results against several types of concerns. The detailed analysis is presented in the Online Appendix and summarized here. A basic requirement for fixed effect estimations of the public wage gap is that there is enough transitions across sectors: we document large rates of transitions at all wage levels and throughout the 25 years under study (see Figure A.1 and comments in the Appendix).

Another concern pertains to whether transitions are endogenous to omitted variables that are not well controlled by fixed effects, either because they are sector-specific or because they vary over time.[26] We test these different possible cases of endogenous mobility using

[25] Also, the year 2007 also coincides with new measures that aimed to reduce the size of the public sector in France with the nonrenewal of half the positions of retiring civil servants (Bezes and Jeannot, 2011). Yet, these measures would take time to pervade to wage gaps through selection effects.

[26] Another potential issue in fixed effects estimations is the possible attenuation bias due to misreported

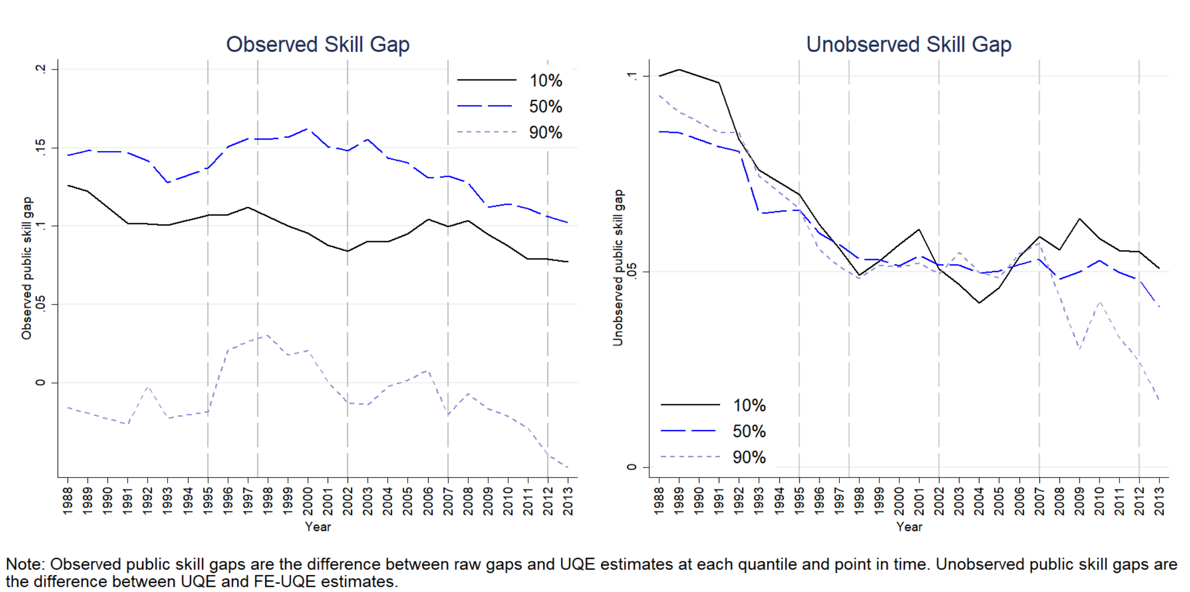


Figure 5: Observed and Unobserved Skill Gaps

the approaches suggested in Card et al. (2013). First, we find no evidence of worker-sector specific match by examining wage changes around sector transitions (Figure A.2). We also find no substantial differences when estimating wage gaps on the basis of public-to-private transitions rather than the opposite direction (Figure A.3). Wage penalties are only slightly smaller in the former case, possibly because moves to the public sector, motivated job security, may be associated with lower financial losses given the correlation between wage levels and having a tenured position within the civil sector. Third, digging into the potential reasons for time-varying unobserved confounders or transitory shocks, we see no sign of a relation between mobility patterns and pre-move wage fluctuations, which would otherwise indicate the presence of learning or cycle effects. Fourth, movers and stayers display common wage trends before the former transits to the other sector, i.e. a general 'parallel trend' verification (see Figure A.4).

Further checks are suggested that deal with the main limitation of our data and the generality of our results. First, we check whether using statutory weekly work hours to calculate hourly wages is a valid approximation. On a subsample for which annual work hours are available (i.e. years 1994-2013 for private and regional/health civil sectors), we find hardly any difference between our baseline and wage gaps estimated on the basis of the accurate hourly wage measure (Figure A.5). Second, on that same subsample, we check and find that our conclusions are not affected by the exclusion of part-time workers

public/private sector status. In the present work, sector information is drawn directly from administrative records and should be exempt from measurement errors.

(Figure A.6).

Finally, a few additional results are obtained. First, on a shorter panel, we avail of information on contract types and find that public wage penalties are smaller (and even turned to premia at low quantiles) when focusing on those holding a tenured or long-term contract (Figure A.7). Second, we replicate our estimations on more homogeneous groups, namely focusing on the (private and public) health sector or when excluding the (rather public) education sector. Estimations are less precise but our conclusions are not radically changed (Figure A.8). Third, we find very little difference between gender, with only traces of a glass-ceiling effect in the private sector at higher quantiles; there is also no major differences in public wage gaps across age groups (Figure A.9).

5 Conclusion

This paper contributes to the literature on sector wage gaps, suggesting four main contribution. First, we suggest unconditional quantile effects that are readily interpreted and useful for policy analysts and policy makers. Second, we suggest a flexible but tractable way to incorporate (quantile-specific) fixed effects in quantile estimations. Third, fixed effects quantile estimations on short panel suffer from the incidental parameter bias, which leads to wrong conclusions about the role of unobserved heterogeneity versus actual sector pay gaps. We suggest a simple jackknife correction to this problem and exploit a very long panel of workers to do so. Fourth, we apply these tools to estimate the public sector wage gap in France over the long period (1988-2013), exploiting a large administrative panel data. We provide interpretations of the results in terms of policy and business cycles. In the context of unconditional quantile effects, comparing estimates with or without individual effects also provides an assessment of the unobserved skill gaps between sectors – a measure of selection on unobservables – and how they vary over time.

Our results first show that overall, monetary returns are lower in the public sector in France, possibly justified by job security and compensating differential in terms of intrinsic motivation. Penalties are significantly larger at the top of the distribution. We show that this compression effect of the public sector is considerably attenuated by the incidental parameter bias – it is tempting to interpret this as larger positive selection into the public sector at lower quantiles when, in fact, the compression effect is restored by the jackknife correction. We also observe a positive selection – on both observables and unobservables – into the public sector over the long run, reflecting the efficiency of national recruitment schemes at selecting skilled workers.

Time trends show that the public wage penalties have been reduced by policy measures in the 1990s. Yet they tend to increase again more recently, particularly on account of

Sarkozy's policy measures from 2007 onwards, supported by austerity measures in the wake of the Great Recession. Both observed and unobserved skill gaps have significantly diminished over time, partly due to an expansion of the public sector in the 1990s, less selective forms of recruitment and an overall increase in education levels. The unobserved skill gap at the top of the distribution has almost vanished in the recent years: this trend may derive from the combined effect of increasing public wage penalties (including repeated wage freezes since 2010) and the fact that job security in the public sector is not an attraction for top earners. These results put in question the future quality of public services in France at the managerial level and call for more performance-based remuneration systems in the administration.

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A Online Appendix

A.1 Main Estimation Results

	All years					Base year					End year				
	Mean	10%	50%	90%	Compression effect as p90%-p10%	Mean	10%	50%	90%	Compression effect as p90%-p10%	Mean	10%	50%	90%	Compression effect as p90%-p10%
Raw wage gap	0.09	0.153	0.142	-0.035		0.089	0.177	0.120	-0.085		0.049	0.122	0.084	-0.099	
OLS and UQE	-0.009	0.051	0.004	-0.026	-0.077 ***	-0.050	0.051	-0.025	-0.069	-0.120 ***	-0.022	0.045	-0.018	-0.045	-0.090 ***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
FE and FE-UQE	-0.067	-0.031	-0.049	-0.067	-0.036 ***	-0.166	-0.055	-0.108	-0.143	-0.088 ***	-0.047	-0.027	-0.066	-0.075	-0.048 ***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)
FE and corrected FE-UQE	-0.067	-0.027	-0.054	-0.082	-0.055 ***	-0.166	-0.049	-0.111	-0.164	-0.115 ***	-0.047	-0.006	-0.059	-0.062	-0.056 ***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)
Effect of the correction in restoring the compression effect (a):					-0.019 ***					-0.027 ***					-0.008
					(0.006)					(0.009)					(0.006)

Estimates based on PTS+EDP data for 1988-2013: linear estimations (OLS or with fixed effects FE), unconditional quantile effects (UQE), fixed effects unconditional quantile effects (FE-UQE) with or without jackknife correction for the incidental parameter bias. Base and end year estimates obtained by interacting the treatment variable (public sector dummy) with time dummies.

(a) difference between corrected and uncorrected FE-UQE compression effects.

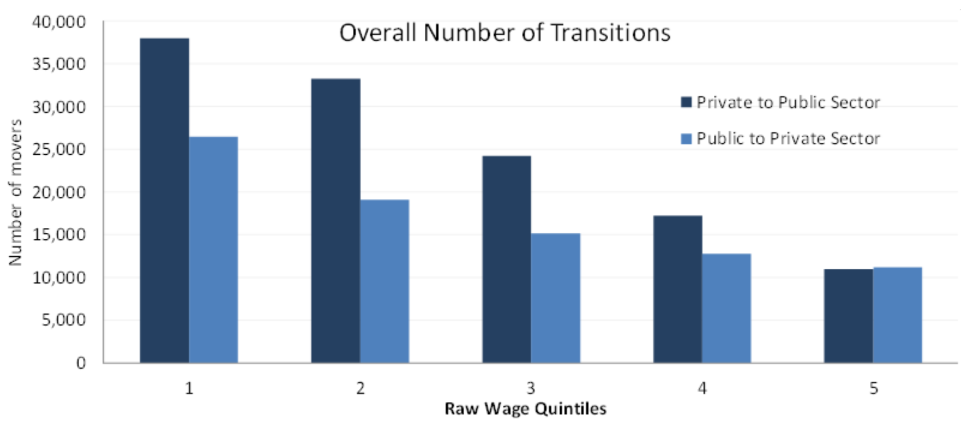
Table A.1: Main Estimates

A.2 Robustness Checks

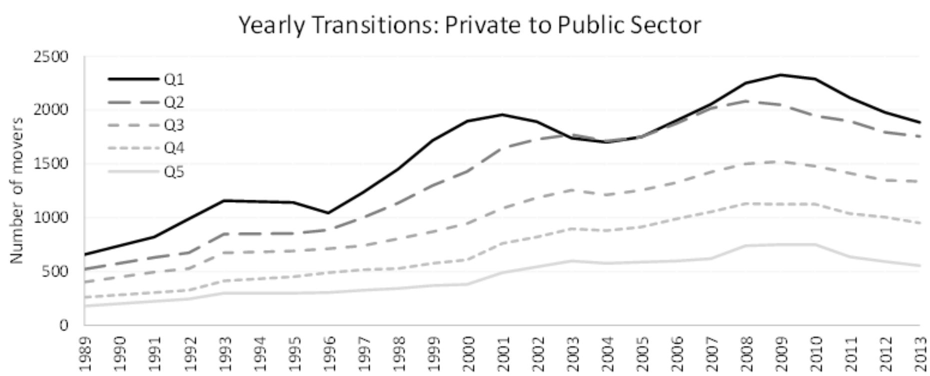
Frequency of Transitions. The identification of the public wage gap in panel estimations requires that there is enough transitions across sectors. Using administrative data has the advantage of dealing with a very large number of observations, so that the wage gap can be precisely estimated, even if the movers represent only a fraction of the workforce. In the data section (Table 1), we have provided general statistics showing that movers comprise 0.9% of the workforce per year on average and 7% of all workers over the course of the panel. We complete this information with additional statistics. The first graph of Figure A.1 presents the total number of transitions across sectors by wage quintile of destination, over the years 1988-2013. It conveys that there is a large number of moves – more than 10,000 one-way transitions – at each quintile of the wage distribution. As expected, there are more frequent transitions at lower wage levels (workers with more experience or longer-term positions are less likely to transit). In the lower part of the distribution, there are also more frequent transitions from private to public sector than in the other direction. These two features partly reflect the fact that some workers attempt to access tenured public sector jobs at an early stage of the career – but clearly, they do not represent all of the transitions across sectors.

In addition, the second and third graphs of Figure A.1 show the number of transitions per year and per quintile. It turns out that there is a substantial number of moves per year at different points of the distribution for a reasonable identification of our most detailed estimates (FE-UQE with time interactions), i.e. between around 500 and 2500 transitions per year and quintile. Again, the frequency of transitions decreases with wage levels (this is verified in both directions) and there are more transitions towards the public sector at low quintiles. Time trends are relatively comparable for both directions of the moves, with a long-term increase in the number of transitions. Such a similarity conveys that trends are not directly due to public sector reforms, which would mainly affect transitions towards the public sector. Specific effects go as follows. Transitions towards the private sector increase during economic downturns and with declining unemployment, which is for instance the case in the late 1990s. This period also coincides with a boost in vacancies in the public sector. After 2007-08, the number of transitions decreases both ways: the slowdown in public sector recruitment (in times of austerity measures) limits the transition in this direction; inversely, rising unemployment incites civil servants to stay in a more protected sector and not to risk their chance in better-paid private jobs.[27]

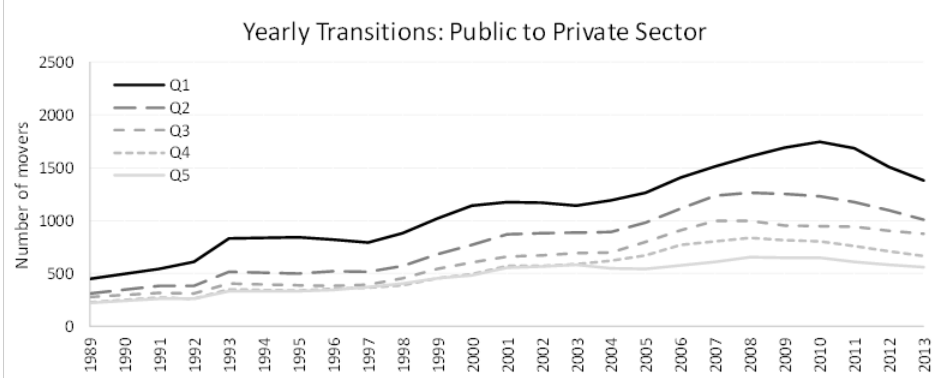
[27]While this is true for Q2-Q5, transitions continue to increase in Q1 after the onset of the Great Recession. This counter-intuitive trend can be explained by a lower renewal rate of short-term contracts among low-paid public workers in these years.



We count 208,437 movers/transitions across sectors over the period 1988-2013. This graph shows their breakdown by quintiles of the destination sector's wage distribution.



This graph show the yearly number of transitions across sector per wage quintile of destination.



This graph show the yearly number of transitions across sector per wage quintile of destination.

Figure A.1: Statistics on Transitions across Sectors

Worker-Sector Specific Matches and Symmetry Checks. Another concern about sector movers, when working with panel estimations, is the potential endogeneity of the transitions. Our estimates of the public wage gap are obtained under the assumption that conditional on individual heterogeneity (fixed effects), moves across sectors are random. We test different possible cases of endogenous mobility following Card et al. (2013). They suggest different situations where unobservable components may be correlated simultaneously with wages (the dependent variable) and with the moves on which their parameter of interest is identified.

The first series of check pertains to the violation of the exogenous mobility assumption due to sorting based on the value of a worker-sector match component. Assume that in addition to individual fixed effects, the wage equation includes such a sector-specific individual effect. The move decision not only depends on wage differential across sectors but also on the individual's difference across sectors in match components. In this mobility frame, the estimated sector wage gap depends on a worker-sector match and is no longer common across all employees. In particular, different wage changes may be observed depending on the direction of the transition. Following Card et al. (2013), we extract panel observations of workers before and after experiencing a transition and represent wage levels by quintile and sector around the time of the move. Results in Figure A.2 indicate a relative symmetry between wage increases for movers from high quintiles in sector A to low quintiles in sector B and wage decreases for movers in the opposite direction. This pattern suggests that there is no endogenous mobility based on unobserved matching components.[28]

A related check pertains to potential differences in the public wage gap estimates depending on the move direction used to identify the effect. Public-to-private sector movers can possibly be specific and exhibit different characteristics than private-to-public sector movers, which should be picked up by fixed effects unless sector-specific individual effects matter, as discussed above. Hence, in addition to the previous checks, we verify that there is no drastically different estimates of the wage gap depending on direction of the move. For instance, some of the moves to the public sector – motivated by compensating differentials like job security – may be associated with lower financial losses given the correlation, within the civil sector, between wages and having a tenured position. For this reason, we would expect larger public pay gaps when using private-to-public sector

[28]The two only cases where we do not observe a perfectly symmetric transitions in wage variation are moves from Q3 and Q4 (Private) to Q1 (Public): the wage loss in these situations is not as large as the wage gain in the reverse direction. Effectively, these workers probably leave the private sector for lower-paid tenured positions in the administration, which offer better returns than non-tenured positions. Workers transiting in the opposite direction may leave both non-tenured and low-earning position in the public sector for better pay job in the private sector.

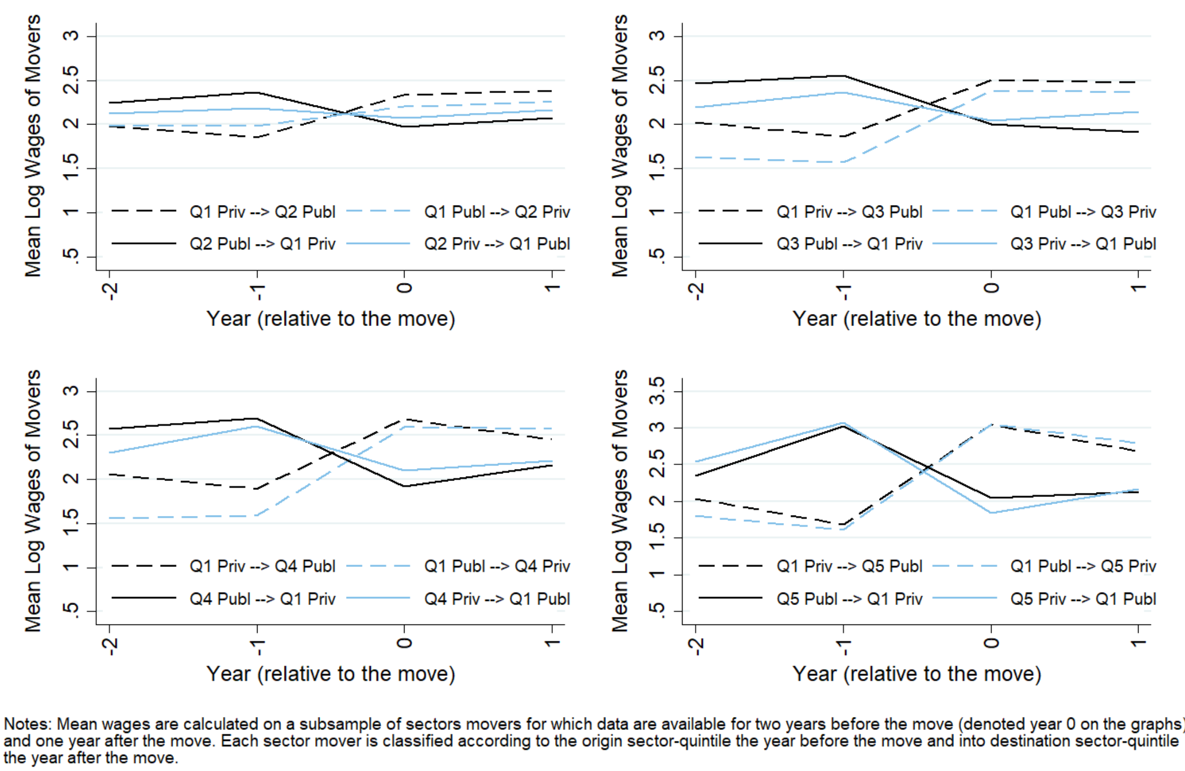
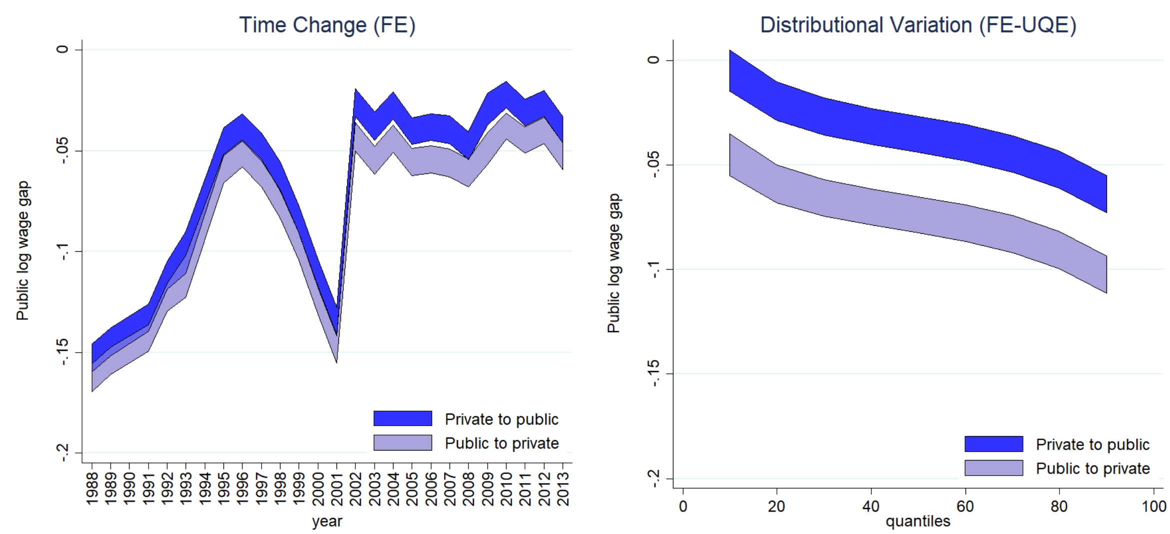


Figure A.2: Checking for Asymmetry: Changes in Mean Wages Around the Transitions



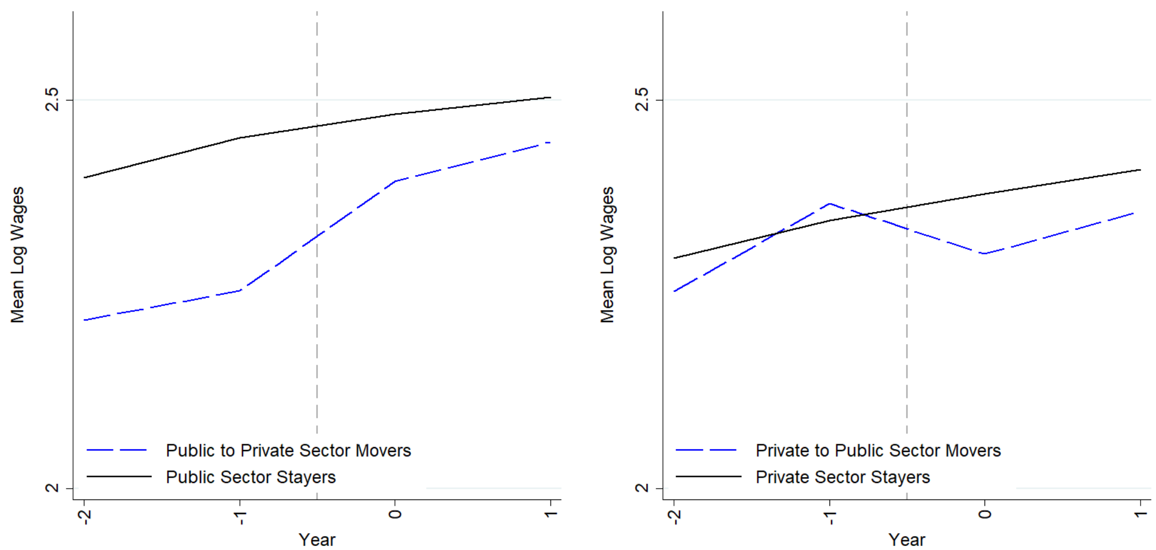
Estimations using our baseline sample restricted to stayers plus those moving either from private to public sector or in the opposite direction, as indicated.

Figure A.3: Checking for Asymmetry: Public Wage Gap based on Alternative Transitions

transitions.[29] We replicate our estimations using stayers plus movers in only one direction at a time. Results are presented in Figure A.3. Reassuringly, estimated wage penalties are only slightly smaller when identification is based on private-to-public transitions. FE estimates indicate that such differences are small and not always significant throughout the period (first graph). On average over time, we find a significant average difference at all quantiles (second graph), yet it is fairly small: less than 2 percentage point above (below) the baseline when using transitions from the private to the public sector (from the public to the private sector).

Time-varying Unobservables and Placebo Checks. Another possible endogeneity of the moves may be due to wage levels and transitions simultaneously depending on time-varying unobserved components or transitory shocks. Card et al. (2013) discuss the possible nature of these time-varying omitted variables. A "learning effect" may occur when workers benefit from a wage growth that signals their abilities and increase their probability of being hired in higher-paid positions (Gibbons et al., 2005). On Figure A.3, we observe no hint of a systematic switch to higher (lower) pay jobs at t=0 following wage gains (losses) between t=-2 and t=-1. Card et al. (2013) also suggest that a "cycle effect" may associate sector changes and the variation in a transitory error component, for instance if workers do cycle between high-paid jobs in the sector that is more sensitive

[29]Another reason for it would be if private sector employees had better returns to their private experience than the returns that public workers gain from their public experience (see evidence for Norway in Rattso and Stokke, 2017).



Notes: For each year in the panel, we calculate mean annual wages of workers for subsamples of movers observed two years before and two years after the move (year of the move denoted 0) and stayers observed exactly four consecutive years over the same period, then averaging all the 4-year series over all the panel years.

Figure A.4: Parallel Trend Checks: Wage Trends of Sector Movers and Stayers

to economic conditions and low-wage jobs that are more stable. In this scenario, workers who have recently experienced a positive (negative) transitory wage shock will be more likely to move to higher (lower) wage jobs in the private (public) sector, leading to an attenuation in the estimated wage gap. Again, Figure A.3 shows no sign of such a relation between mobility patterns and pre-move wage fluctuations.

Finally, our fixed effects model can be interpreted as a difference-in-difference estimation whereby movers are treated and stayers are the control group. In this interpretation, the potential bias due to time-varying unobservables requires a general placebo check (parallel trend) before the move. In other words, a minimum requirement for the absence of a bias is that movers and stayers display common wage trends before the former transits to the other sector. Thus, in Figure A.4, we compare wage dynamics of movers and stayers. It is produced by extracting, for each year of the panel, the movers of that particular year (with observations two years before and after) as well as the concomitant stayers (over the same four years) and by averaging over all four-year series.[30] Results show that wage trends are relatively similar between the two groups before the transitions, which gives some confidence that movers' specific characteristics are not time-varying and are captured by fixed effects.

[30]Note that the four-year graph could be shown for specific years (see also Rattso and Stokke, 2017). Also remark that in order to avoid the repetition of the same stayers across the different series, we keep only stayers observed exactly four years.

Recorded versus Statutory Hours. Recall that our hourly wage measures are calculated using statutory workweek duration (39 hours then 35 hours after the 1998-2002 reform). It is possible to check the validity of this approximation – and notably the fact that we do not account for overtime when using statutory weekly hours. In fact, the PTS contains the total worked hours over the year but this information is available since 1994 only, and for the private sector and the regional/local and health civil service only. For this subsample, however, we recalculate hourly wages and, hence, the raw wage gap based on recorded hours. The comparison with our baseline (based on statutory hours) is presented in the top graph of Figure A.5. The discrepancy is marginal in most years and small in some years (no larger than 5 percentage points). The time trends and differences across quantiles are very similar. Discrepancies are largest for top earners and correspond to an under-estimation of the raw gap with our baseline: indeed, overtime is more frequent in the private sector (so that we overstate private wages when ignoring it). In the lower graphs, we replicate wage gap estimations for this subsample using both approaches. Discrepancies go in the same way: our baseline (using statutory hours) overstates public wage gaps compared to estimates based on recorded hours. Yet the discrepancies are again very small and do not change our conclusions.[31]

Full-Time versus Part-Time and Short versus Long-Term Contracts. Focusing on full-time jobs, as in our baseline, may give a partial characterization of the public wage gap. On the subsample just described, for which we avail of annual worked hours, we now replicate our estimations with and without inclusion of part-timers. Results reported in Figure A.6 show very little difference – only a slightly decrease in the public wage penalty – when accounting for part-time workers. It conveys that our baseline is not very different from a complete picture that would account for all types of work duration contracts.

The type of job contract is also a characteristic likely to influence the public wage gap. Given the great disparity in wages between tenured and short-term contracts within the public sector, it is possible that part of the public wage penalties previously estimated are driven by short-term workers. A composition effect may also come into play since the rate of short-term contracts is larger in the public sector (following the expansion of new employment forms and subsidized jobs in this sector). An issue is data availability: contract types are not registered before 2005. For this shorter panel 2005-2013, we replicate our baseline estimation (all contracts) and conduct an estimation focusing on those holding a long-term contract (tenured job in the public sector or a permanent, non-statutory

[31]Note that the baseline estimates on these graphs is different from our previous baseline since the sample is not only shorter (i.e. restricted to years 1995-2013) but also based on a subset of the public sector (i.e. regional/health public sector only).

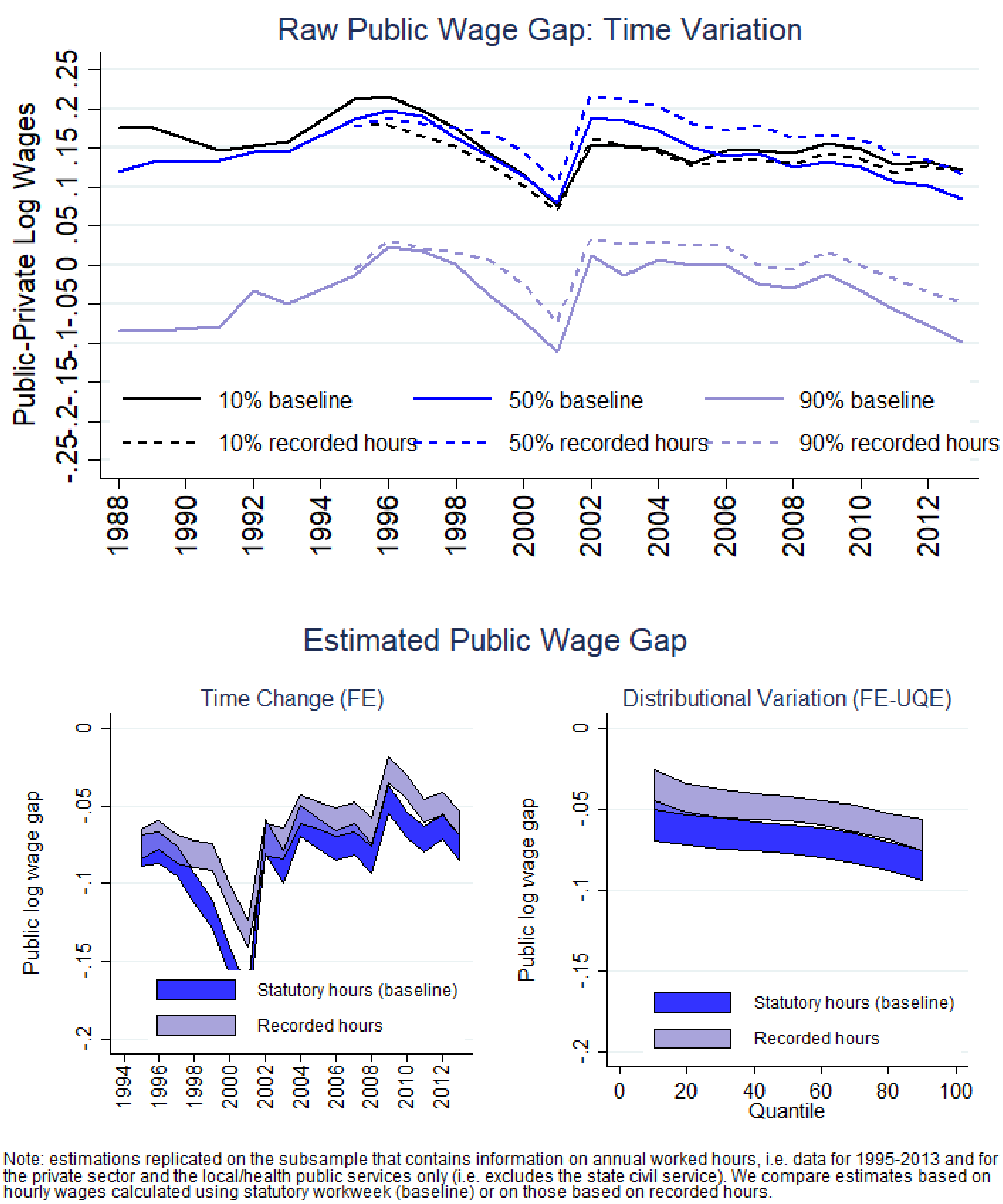
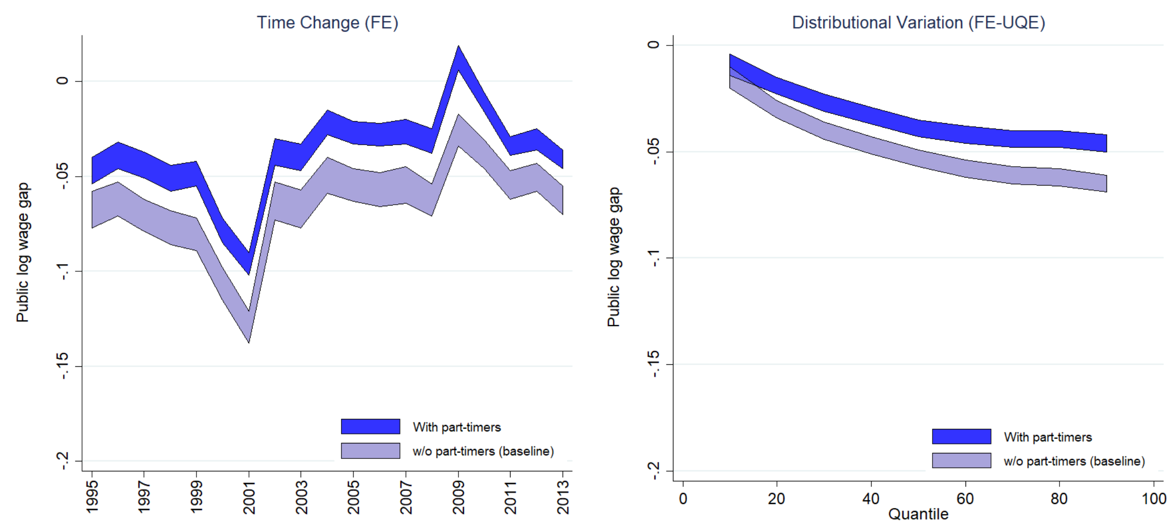


Figure A.5: Raw and Estimated Public Wage Gap: Statutory vs Recorded Weekly Hours



Note: estimations replicated on the subsample that contains information on annual worked hours, namely years 1995-2013 for private sector and the local/health public services only (i.e. excludes the state civil service). Using this subsample, we compare estimates obtained when dropping part-timers to those including part-timers.

Figure A.6: Estimations With or Without Part-Timers

contract 'CDI' in public or private sectors). Note that there are slightly more employees with a long-term job in the public sector (84%) than in the private sector (79%). Results are reported in Figure A.7. FE-UQE obtained on a shorter panel have to be taken with caution but FE estimations remain valid. As conjectured, those with long-term contracts hold relatively better paid job in the public sector: the public wage gap oscillates around zero over the (shorter) period and is positive at the bottom of the distribution.

Sectors. It is interesting also to replicate our estimations on an homogeneous industry that comprises both public and private jobs with more comparable occupational structures. This is the case of the health sector, with for instance public nurses and private nurses (see a similar exercise in Disney and Gosling, 2008, who precisely focus on nurses and midwives). Another variant would consider the whole sample except industries that are very specific to a particular sector. This is the case of education, since education services are mainly found in the public sector in France and are relatively specific (they gather a majority of women and concentrate a large share of high-skilled civil servants). In Figure A.8, we compare our baseline estimates with the estimated public wage gap in the health sector (identified for 1995 onwards) and in a sample without the education sector. In the latter case, there is not much difference with the baseline. In the former, focusing on the health sector yields estimates that are unfortunately not very precise but the overall trends are similar to the baseline. The compression effect becomes insignificant, possibly because of a shorter panel combined to low returns to skills in public health

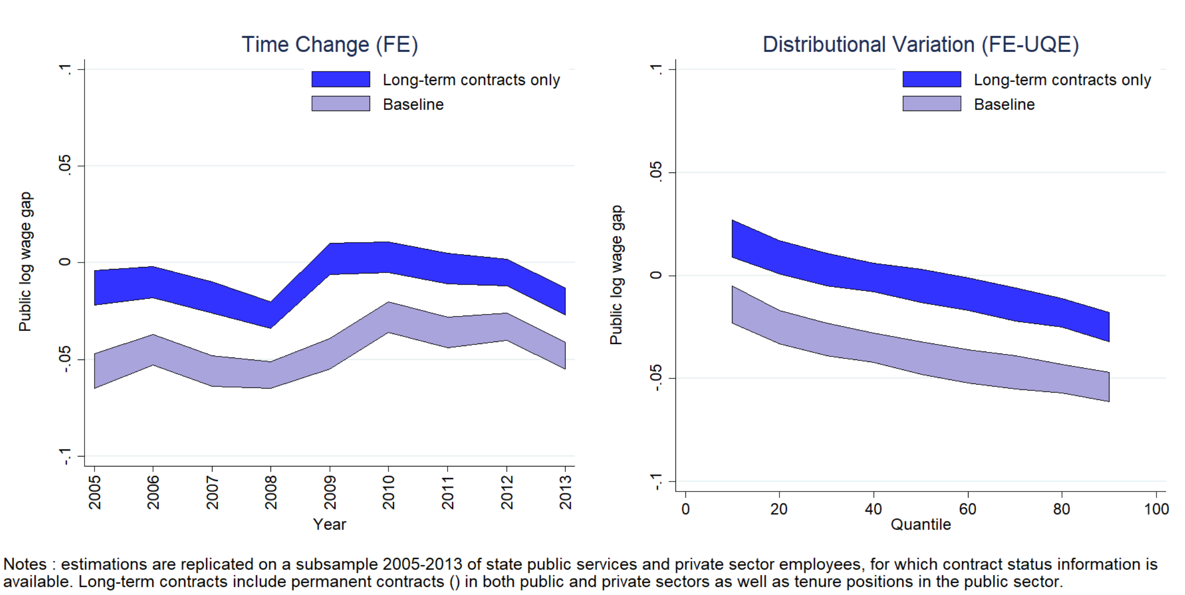


Figure A.7: Estimations With or Without Short-Term Contracts

services, especially at low wage levels.[32]

Heterogeneity in Gender and Age. Women are disproportionately working in the public sector. While gender is controlled for in our baseline estimations (directly or through fixed effects), it is nonetheless interesting to replicate our estimations for men and women separately. The public sector recruitment system is expected to be less discriminatory (the written part of the national examination scheme is anonymous) and so is the pay grid, which is more systematically based on seniority than on subjective evaluation factors. However, an extensive analysis of Gobillon et al. (2017) shows that the gender difference in position within a hierarchy at work is similar between public and private sector in France, with the exception of the top of the distribution where a glass-ceiling prevails in the private sector. The top graphs of Figure A.9 confirm these points: there is very little difference between gender, with only slightly smaller public wage penalties among women in the early years and a reflection of the glassceiling effect at higher quantiles.[33]

It is also interesting to study heterogeneity in age. We have shown that movers are not

[32]Larger penalties at low wage levels is likely a consequence of the particularly rapid expansion of non-tenured, short-term contracts in public hospitals over the past decade of our sampled years (around 2.5 faster than in other public services, cf. Duval and Baradji, 2013).

[33]Smaller public wage penalties at the top indeed confirm indirectly a larger gender pay gap in the private sector as the estimations already control for possibly different characteristics betwen men and women including education, potential experience, occupation and unobserved skills.

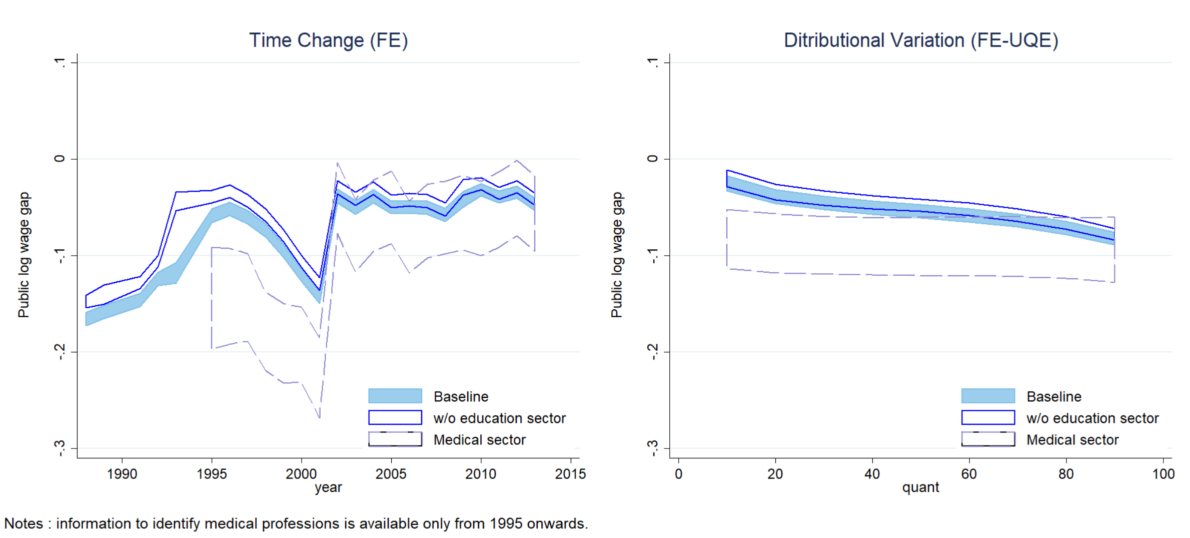


Figure A.8: Estimations with more Homogeneous Sectors

only those at an early career stage but we could anyhow expect some differences in the nature of the moves – as well as in the magnitude of the public pay gap – between older and younger workers. Another aspect is the fact that the fall in observable and unobservable skill gaps over time may relate to cohort effects (in addition to explicit policy measures taken during the studied period, as discussed). While we already account for cohorts via individual fixed effects, our baseline does not let the public wage gap estimates vary with factors like age or cohort. Results in the lower graphs of Figure A.9 reveal very little difference between those above and below 40 years of age, except slightly smaller average penalties for the older public workers in the recent years. This last feature may be due to increased age discrimination (and more concave returns to experience) in the private sector in times of rising unemployment.

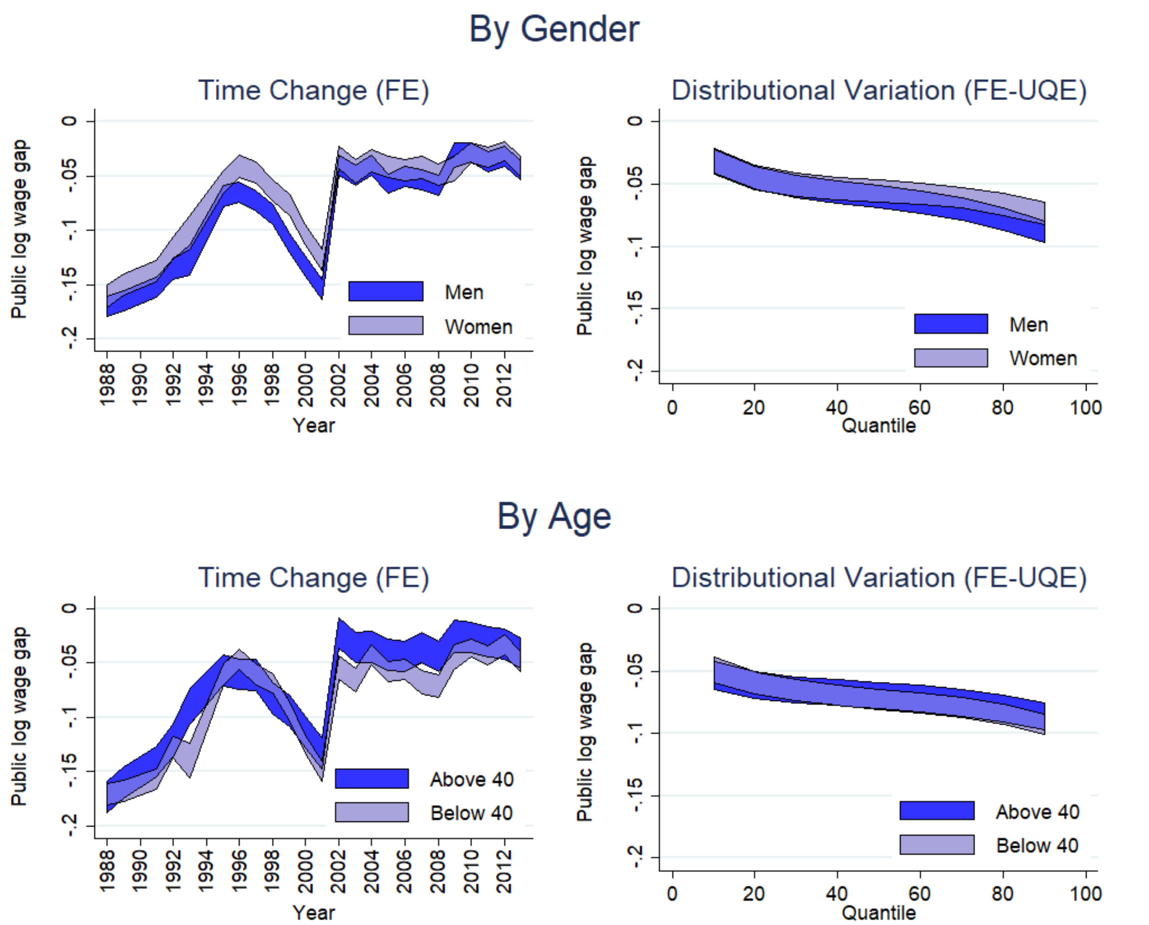


Figure A.9: Estimations by Gender and Age