The effects of the EU equal-treatment legislation Directive for fixed-term workers: evidence from the UK.

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Non-Technical summary

In 2002, the United Kingdom implemented the EU directive mandating equal treatment of fixed-term and permanent workers. This paper uses data from 1997 to 2007 from the Labour Force Survey to assess whether the new legislation has led to a decrease in the average wage gap between fixed-term and permanent workers.

For men, the analysis finds that fixed-term workers suffered a wage penalty of around 4% compared to permanent worker with similar characteristics in the five years prior to the reform. This wage gap closed following the introduction of the equal-treatment legislation.

There are however several pieces of evidence that cast doubt on the extent to which this change can be interpreted as a direct consequence of the new legislation. In the first place, the differential between fixed-term and permanent workers appears to have been decreasing even before the new legislation. Secondly, there is evidence that the wages of agency workers increased relative to that of permanent workers over the same time period in spite of the fact that the equal-treatment legislation did not apply to them.

Given the common temporary nature of their employment, the paper also explores the possibility of using agency workers as a counterfactual for what would have happened to fixed-term workers in the absence of the new legislation. The evidence indicates that the wages of the two groups behaved in different ways before the reform suggesting that agency workers might not provide a good indication of what would happened to fixed-term wages after 2002 in the absence of the reform.

For females, in line with previous literature, this paper finds that women on temporary contracts tend to have characteristics which are associated with higher wages, but they are paid less than women with the same characteristics employed on permanent contracts. The pre-reform differential is similar to that found among men, but in this case there is no indication that the wage gap closed after the reform.

The effects of the EU equal-treatment Directive for fixed-term workers: evidence from the UK

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1st May 2014

Abstract

In 2002, the United Kingdom implemented the EU directive mandating equal treatment of fixed-term and permanent workers. This paper uses eleven years of data from the Labour Force Survey to assess whether the new legislation has led to a decrease in the average wage gap between fixed-term and permanent workers. For women, there is no evidence of that. For men, the wage gap appears to have closed after 2002. However, this gap was falling even before 2002 and some evidence of changes in the selection of workers after the implementation of the Directive cast doubts on the extent to which the closing of the gap can be ascribed to the new legislation.

Keywords: fixed-term contracts, wage differentials, equal-treatment legislation.

JEL classifications: J3, J7, J41.

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1 Introduction

A large body of literature has documented that across Europe (and beyond) workers on temporary contracts are paid less, receive less training and report lower satisfaction than workers with similar observable characteristics on permanent contracts (OECD, 2002; Booth et al., 2002; Arulampalam et al., 2004; Kahn, 2007; Arulampalam et al., 2004; Brunello et al., 2007). Such evidence fueled a policy debate which eventually led the European Union to adopt the Council Directive 1999/70/EC to prevent discrimination against fixed-term workers. All member states have now transposed the Directive into national legislation. To the best of my knowledge, there has been very little research into the effects of the Directive on fixed-term employment across Europe. This paper appears to be the first large-scale empirical study of the impact of the EU legislation on wage differentials in one of the Member States, the UK.

The UK passed the Fixed Term Employees' (Prevention of Less Favourable Treatment) Regulations in July 2002 which then came into effect in October of the same year. The new regulations mandate that fixed-term employees cannot be treated less favourably than comparable permanent employees in the same firm in terms of wages, benefits and training. Unequal treatment is accepted when can be "objectively justified[1]" and employers can balance a less favourable condition against a more favourable one[2].

This paper uses eleven years (1997-2007) of data from the Labour Force Survey to study the effects of the new legislation on fixed-term (FixT) workers' wages. The reform therefore falls in the middle of the interval studied and the results are unlikely to be affected by the Great Recession of 2008. Previous literature has already documented the existence of a conditional wage penalty for these workers in the UK. In particular, Booth et al. (2002) use data from the British Household Panel Survey between 1991 and 1997 and find that after controlling for observable characteristics the wage penalty was around 17% for men and 14% for women. In principle, there was therefore ample room for the new legislation to produce visible effects on the average wage differentials. However, these differentials might at least in part reflect differences across groups that researchers are not able to account for. If that was the case, the legislation could alter the market price of fixed-term contracts possibly leading to unintended and potentially perverse effects, for instance by making it more difficult for low-skilled workers to obtain fixed-term jobs due to increased cost of such contracts. As part of a wider empirical strategy to try and isolate the causal effect of the legislation on wages, I also present evidence on the possible unintended effects of the reform on the level and composition of temporary employment.

A first simple evaluation of the impact of the new legislation was conducted by Green (2008). He looks at the years immediately before and after 2002 and finds that the average wage of fixed-term workers increased more than that of permanent workers. The analysis of this paper attempts to address some of the potential limitations of such an approach. In the first place, a number of control variables covering an eleven-year period are included in the econometric specifications to account for possible changes in the characteristics of workers employed on different contracts. A careful and detailed descriptive analysis is certainly of interest in itself given the very limited amount of research on this topic, but this paper attempts to go further and investigates the credibility of the assumptions necessary for the identification of the causal effect of the reform. In particular, the analysis is cast in a difference-in-difference framework and two alternative control groups are considered, i.e. permanent workers (who have open-ended employment contracts with the firm) and temporary agency workers (TAW, who are employed for a limited time by the firm through a third agency). The fundamental identification assumption is that the treated and the control groups share a common time-trend, that is, that in the absence of the policy intervention the wages of the two groups would have followed the same pattern. The validity of such assumption is scrutinised using the evidence on the behaviour of the relevant wage differentials before the reform. In addition, changes in the composition of unobserved characteristics must also be ruled out as they would confound the effects of the new regulations. In the absence of data that allow to tackle this issue directly, a number of checks are performed to try and uncover any indirect evidence of changes in unobservables within contract groups.

The analysis finds that fixed-term workers of both genders suffered a conditional wage penalty of around 0.04 log points compared to permanent worker in the five years prior to the reform. For males only, there is evidence that the wage gap closed following the introduction of the equal-treatment legislation. However, we find evidence that the closing of the gap had begun even before 2002 and that after that year the wages of agency workers, who were not subject to the new legislation, also increased. These facts cast doubts on the extent to which the disappearance of the conditional wage penalty for male fixed-term workers can be ascribed to the equal treatment legislation.

2 Empirical strategy

The new regulations mandated equal treatment between fixed-term workers and permanent workers and did not apply to temporary agency workers. As will become clearer in the discussion below, the key to isolating the impact of the legislation is to identify a group of workers whose wages were behaving similarly to those of fixed-term workers before the reform was introduced. On the one hand, permanent workers are arguably likely to meet this requirement because fixed-term and permanent workers are both employees of the firm whereas temporary agency workers are employed through a third party (the agency). On the other hand, it could be argued that the market for fixed-term jobs is more closely related to that for agency jobs because of the common temporary nature of these jobs. Therefore this paper considers both candidate groups.

The analysis begins by estimating a simple log-linear regression model:

$$log(w_{it}) = \alpha + \gamma_1 FixT_{it} + \gamma_2 FixT_{it} * Post + \gamma_3 TAW_{it} + \gamma_4 TAW_{it} * Post + X'\beta + \sum_t \lambda_t + \sum_{q=1}^{4} d_{qt} + \varepsilon_{it} \tag{1}$$

where $FixT$ and TAW are dummies for fixed-term and agency workers respectively, λ_t and d_{qt} are year and quarter dummies respectively and $Post$ is a dummy for the post-reform period, taking value 1 from 2002 onwards. The coefficient γ_2 captures the change in the conditional wage differential between fixed-term and permanent workers following the introduction of the new regulations. The coefficient γ_4 measures the change in the differential between agency workers and permanent workers. These are effectively difference-in-difference estimators comparing the two groups of temporary workers to permanent workers. Since the new rules did not apply to agency workers and assuming that any other confounding variables affected both groups in the same way, no change in the wage of agency workers (relative to that of permanent workers) should be detected following the reform, implying $\gamma_4 = 0$. However, agency workers' wages might have changed due to (i) general equilibrium effects of the regulations (ii) other factors affecting agency workers only (including group-specific trends) (iii) common factors affecting both agency workers and fixed term workers.

Consider first the possibility of general equilibrium effects. Standard economic theory suggests that the increase in the wage of fixed-term workers relative to that of agency workers would result (i) in a decrease in the supply of and (ii) in an increase in the demand for agency workers. The increase in the wages generated by such forces would be picked up by a positive γ_4. To the extent that such effects take time to unfold, their empirical relevance can be assessed by considering different time intervals around 2002.

Consider now the scenario where agency workers' wages changed after 2002 as a consequence of unobservables affecting exclusively agency workers. There were no significant changes in the regulations affecting agency workers' working conditions in the UK over this period, but differences in pre-refom trends in the wages for different contracts could explain post-reform differences, leading to $\gamma_4 \neq 0$. Perhaps more importantly, the same concern can be raised with reference to γ_2 which is meant to isolate the effect of the new legislation on the differential between fixed-term and permanent workers. To try and purge the post-2002 estimates from the confounding effects of differences in underlying trends, contract-specific (quarterly) linear trends in wages can be added to equation 1 (Angrist and Pischke, 2009). In addition, differences in changes in wages over time could also be due to differences in the responsiveness of wages of different type of contracts to the economic cycle. Therefore, following Kugler et al. (2005), I also include group-specific cyclical effects as interactions between quarterly real GDP growth and the contract dummies.

Finally, factors other than the new legislation affecting both agency workers and fixed-term workers might result in $\gamma_4 \neq 0$. Under the assumption that the effect of these unobservables on fixed-term and agency workers is the same, one can hope to purge such confounding effects from the estimate of the policy effect by subtracting γ_4 from γ_2. An estimate of this difference and its standard error can be obtained by defining an additional dummy variable identifying both fixed-term and agency workers:

$$FixTOrTAW = \begin{Bmatrix} 1 \text{ if } TAW \text{ or } FixT \\ 0 \text{ otherwise} \end{Bmatrix}$$

and rewriting equation 1 as:

$$\begin{aligned} w_{it} = \alpha + \delta_1 TAW_{it} + \delta_2 TAW_{it} * Post + \delta_3 FixTOrTAW_{it} \\ + \delta_4 FixTOrTAW_{it} * Post + X'\beta + \lambda_t + \varepsilon_{it} \end{aligned} \tag{2}$$

In this equation, δ_1 provides a direct estimate of the differential between fixed-term and agency workers in the pre-2002 period and δ_2 picks up any changes in this differential following the introduction of the new regulations. δ_3 is the differential between fixed-term and permanent workers and δ_4 the post-reform change in it.

3 Data

The analysis uses data from 1997 to 2007 from the UK Labour Force Survey (LFS). This is a quarterly sample survey of households living at private addresses in the United Kingdom managed by the Office for National Statistics (ONS). Each individual can remain in the survey for up to five quarterly waves, but since questions on earnings are only asked in the first and fifth wave, they can appear in the subsample used in the analysis at most twice. To account for the possible serial correlation that this might generate, the standard errors are clustered at the individual level throughout the analysis and the substantive conclusions of the paper are unaffected if the analysis is conducted using only observations for wave 1.

The paper presents results obtained using the ONS-recommended measure of hourly wage, i.e. pay over hours usually worked. Results obtained using pay over hours worked in the reference week are substantively the same and are not reported here. Wages are deflated using the quarterly retail price index based on the year 2000 and outlying observations in the top and bottom 1% of the wage distribution in each year are excluded from the sample.

In the LFS, employees whose job is not permanent are asked whether their job falls within one of the following categories: done under contract for a fixed period/fixed task, agency temping, seasonal work, casual type of work, some other reason for not being permanent. Table 1 reports the share of employees on fixed-term contracts ($FixT$), temporary agency contracts (TAW), and permanent contracts ($Perm$). Those in casual, seasonal and "other temporary contracts" are grouped together under the label $OtherT$. The share of permanent employment grew more among women (from 91.7% to 94.2%) than men (from 93.3% to 95.3%) between 1997 and 2007. Conversely, the share of employees on fixed-term contracts decreased by more than a third for both genders[3], while that of agency workers remained stable around 1% for both genders (section 7 discusses the relevance of these changes for the analysis of this paper).

Throughtout the analysis, standard econometric specifications for the wage equations are used. In particular, they include controls for: age, education, health status, foreign nationals, presence of children in the household, marital status, region of residence (20 dummies), tenure, work experience, public sector, size of firm, part-time, occupational group (9 dummies), and industry (9 dummies)[4]. The first two panels in Tables 2 and 3 report the average value of some of these variables for fixed-term workers and the differential with permanent and agency workers before and after the reform. The bottom panel shows Wald tests for changes in such differentials across the two time periods considered. There appears to be some significant differences between the groups considered. In particular, for both genders, fixed-term workers are more commonly found in the public sector and among professional occupations. Agency workers are more represented among clerical staff. Women on fixed-term contracts appear to be more educated than women on other contracts. The bottom panels of the tables show that although most changes over time are arguably small in substantive terms, some are statistically significant. While changes in observables are accounted for in the analysis of this paper, this finding warrants caution in interepreting the results since changes in unobservables might also confound the effect of the new legislation. I return to this point is section 7.

4 Unconditional differentials and trends

Table 4 reports the average hourly wage for permanent workers and the differential with each group of temporary workers separately by gender and before and after the introduction of the new legislation. The first two columns report the real hourly wage (base 2000), while columns 3 and 4 present results in logs.

Between 1997 and 2001 male fixed-term workers suffered a wage penalty just under 0.4 GBP (column 1), amounting to about 4% of the average hourly wage for a male permanent worker (9.2 GBP) or -.053 log points (column 3). This is considerably smaller than the (unconditional) wage penalty of 16% found by Booth et al. (2002) using a different dataset (the BHPS) for an earlier period (1991 to 1997). Following the introduction of the equal-treatment legislation, male fixed-term workers benefitted from an average wage premium of 0.20 GBP (.007 log points), which is statistically significant at the 10% level. Other groups of temporary workers did not experience a similar change in fortune. The average wage for an agency worker remains more than 3 GBP below that of a permanent worker, or about .36 log points. While agency workers were not covered by the new equal-treatment legislation, other temporary workers, such as seasonal and casual workers, were. In spite of this, there is no sign that the very large negative differential of .50 log points between seasonal and permanent workers decreased at all. This might be explained by the fact that the seasonal and casual nature of these jobs make it less likely that there be comparable permanent jobs within the same firms, therefore making the new legislation effectively inapplicable. In light of this, and following the vast majority of the previous literature on temporary employment, we restrict attention to workers who work on fixed-term contracts in the rest of the paper.

For female workers, no unconditional wage penalty is detected for fixed-term workers even before 2002. In fact, female fixed-term workers on average receive a hourly wage which is more than 0.90 GBP higher than that of their permanent counterparts, a premium of about .11 log points. Such a figure is in line with the +13% premium found in BHPS data by Booth et al. (2002). Other groups of female temporary workers do suffer wage penalties although they are generally smaller than those found for males (but clearly all female employees earn less than male employees, as shown by the comparison between the two panels in table 4). Such penalties appear to have increased slightly after 2002. Agency workers in particular saw their average penalty increase from 0.55 GBP to around 0.92 GBP, bringing the log difference with a female permanent worker up from .07 to around .10.

Table 5 presents a decomposition of the change before and after 2002 in the wage differential into a component explained by observable characteristics and a residual one (Juhn et al. (1993), Jann (2008)). Each of these is then further decomposed into three terms accounting respectively for changes in quantities (of observable or unobservable characteristics), changes in their prices, and the interaction between changes in quantites and prices. The loss of some observations due to missing values on the $X's$ explains the small differences between the differentials in table 4 and table 5. The top panel of table 5 shows that the wage of male fixed-term workers increased by .065 log points relative to that of permanent workers and that two third of this change (.042 log points) is explained by observable characteristics (column 1). The quantity effect component of the change in the explained gap (in column 2) is given by differential changes in observable characteristics between fixed-term and permanent workers, holding the (permanent workers') market prices for these characteristics constant. Hence, the positive .028 log points reported in column 2 indicates that over time fixed-term workers saw an increase in the incidence of observable characteristics (relative to permanent workers) which command higher wages. The price effect (column 3), on the other hand, is the result of the change in the (permanent workers') market prices holding the differences in observable characteristics between the two groups constant. This is .023 log points for males indicating that changes in prices have favoured characteristics which were more common among fixed-term workers. The interaction term in column 4 accounts for simultaneous changes in prices and quantities.

Similarly, the unexplained part of the change in the differential is decomposed in a quantity, a price and an interaction term. The quantity effect is driven by changes in unobservable characteristics, while the price effect is driven by changes in the remuneration of such characteristics. For males, table 5 shows that the change in the unexplained component of the differential (.023 log points) is entirely driven by changes in unobservable quantities (0.024 log points).

For females, the bottom panel of table 5 shows that the small increase in the relative wage of fixed-term workers (.008 log points) is the result of two contrasting forces, as the change in the explained differential is a positive .01 while that in the residual differential is a negative -.002. The former is the sum of small positive quantity and price effects, while the latter is driven by a (small) negative quantity effect only partially offset by very small positive price and interaction effects.

These results provide a first indication that the relative wage of fixed-term workers did increase on average after the introduction of the equal-treatment legislation, and that at least for men this was mostly driven by changes in differences in observable and unobservable characteristics between fixed-term and permanent workers. To try and establish to what extent these observed changes can be attributed to the reform, I now turn to the difference-in-difference methodology explained in the previous section.

To provide an assessment of the credibility of the common trend assumption, figure 1 plots the (yearly and 3-quarter moving) average of log real wages for fixed-term, agency and permanent workers separately (and by gender). For males, the plots show that the three groups exhibit trends in the wages which are somewhat similar before 2002. Also, the plot of the quarterly averages show that the trends in wages of fixed-term and agency workers were very similar in the quarters right before the introduction of the new legislation. The post-2002 portion of the plot shows that following the introduction of the new legislation the average wage of a fixed-term worker has mostly been above that of permanent workers falling below it again in the last few quarters. For females, the figure tells a different story. The pre-2002 trend in fixed-term workers' wages appears now flatter than that of permanent or agency workers. Although after 2002 the wage differential between fixed-term and either of the other two groups has increased, those differences in the underlying trends make it difficult to quantify the effect of the new legislation.

5 Conditional differentials

Table 6 presents the OLS estimates of wage differentials between temporary and permanent workers by gender. Columns 1 and 5 show results for the difference-in-difference model with year and quarter dummies only, while the remaining columns add controls for demographic characteristics, differences in the responsiveness to the economic cycle, and differences in underlying trends in wages across groups of workers.

The first two columns of table 6 show that the wage penalty conditional on observable characteristics for male fixed-term workers before 2002 was around .04 log points. Controlling for cyclical effects reduces the point estimate and inflates the standard errors. The introduction of contract-specific time-trends in column 4 leads to a positive estimate which is not statistically significant. The Wald tests reported at the bottom of the table provide some weak support for the presence of different cyclical effects, but not for contract-specific time trends. The interaction between $Post$ and $FixT$ indicates consistently across columns that the conditional wage differential between permanent and fixed-term workers decreased after 2002, possibly closing the gap between the two types of employees. The estimate of the post-2002 change is statistically significant at least at the 5% level across columns and varies between .029 and .055 log points.

Agency workers suffered a larger wage penalty than fixed-term workers before 2002 and there is some evidence of a reduction in it after 2002. In particular, columns 2 and 3 show that agency workers' wages were about .16 log points lower than those of permanent workers and this gap reduced by about .045 points after 2002. Since the equal-treatment legislation did not apply to agency workers, this result casts doubt on the extent to which the aforementioned reduction in the wage penalty for fixed-term workers can be attributed to the new legislation. We return to this point below where we compare fixed-term and agency workers directly.

As for females, column 6 of table 6 shows that when differences in observable characteristics are accounted for, female fixed-term workers are found to suffer from a wage penalty as well. The size of the differential is -.037 log points. The estimate is larger (.049 log points) in the next column where cyclical effects are included. Contrary to what was found for males, there is no clear evidence of a reduction in such differential in either of these two specifications, as the interaction between $Post$ and $FixT$ attracts a small and statistically insignificant coefficient in both cases. The Wald tests show that there is some support for the hypothesis that wages of different groups of workers respond differently to the economic cycle, but there is no evidence of different underlying trends in column 8. The coefficients on the TAW dummy reveals clear evidence of a negative wage differential for agency workers in all specifications but the one with time trends. There is however no evidence that this conditional differential changed at all after 2002, as indicated by the interaction between $Post$ and TAW.

In conclusion, for males I find that changes in the conditional wage differentials are broadly consistent with those in the unconditional differentials. While the decomposition of the previous section shows that changes in the unconditional differentials are largely driven by changes in observable and unobservable characteristics, this section shows that even when one hold observable characteristics constant the wage gap between fixed-term and permanent workers appears to have closed after 2002. However, the evidence that male agency workers have experienced a reduction in their wage penalty as well, suggests that the actual effect of the new legislation might have been smaller than the initial estimates indicate. Among women, I find that the (unconditional) wage premium for fixed-term workers of the previous section turns into a wage penalty when observable characteristics are controlled for and that this penalty has not changed appreciably after 2002.

5.1 Comparison with agency workers

Table 7 reports estimates of the wage differential between fixed-term and agency workers. For males, we see that fixed-term workers' wages were higher than that of agency workers before the reform. The differential conditional on job and demographic characteristics is about .13 log points. Statistical precision is lost when contract-specific trends are allowed, for which however there is no support in the data as discussed above. The interaction between $Post$ and $FixT - TAW$ in column 1 shows that the unconditional differential between agency and fixed-term workers increased slightly as one would expect following the introduction of the equal-treatment legislation. This however no longer holds after job and individual controls are included in the regressions. On the contrary, there is some indication that agency workers' wages might have increased more than those of fixed-term workers, but the effect is small and statistically insignificant.

For females, we also see a statistically significant pre-reform wage premium for fixed-term workers vs agency workers of .073 log points, down from .176 log points when individual and job characteristics are not accounted for. Statistical significance is however lost when cyclical effects are introduced in column 7. For women too the regression picks up an increase in the differential (of .039 log points in column 5) which is however lost when accounting for individual characteristics. In particular, the estimates of the post-2002 conditional change in the $FixT - TAW$ differential are both economically and statistically insignificant.

In conclusion, although for both men and women we find that the unconditional differential between fixed-term and agency workers has increased following the reform, there is no evidence of such a change for either gender when we account for observable characteristics. This evidence, therefore, casts doubt on the extent to which the increase in the relative wage of fixed-term to permanent workers (which we only find for male workers) can be ascribed to the equal treatment legislation.

6 The evolution of differentials over time

The new legislation came into effect in October 2002, but it is not obvious when its effects actually started to unfold. It is possible that some employers began to apply the equal-treatment principle to contracts started before October 2002. On the other hand, existing contracts might have been adjusted only to a limited extent or not at all and the new legislation might have began to unfold its effects as new contracts were started after 2002. In light of these concerns, in this section I take a closer look at how differentials evolved over time.

This part of the analysis is also helpful in assessing the empirical relevance of possible general equilibrium effects. The most plausible concern is that the increase in the relative wages of fixed-term workers might have generated upward pressure on agency workers' wages as a result of increased demand and decreased supply. To the extent that such effects take time to unfold, one would expect this to show in the form of a FixT-TAW differential that first increases as a direct result of the new legislation and then decreases when the general equilibrium effects kick in.

I begin by considering post-reform periods of increasing length, varying the end year from 2003 to 2006. These models therefore compare the 1997-2001 average log wage to averages over different time periods depending on when the sample is truncated. Clearly, in restricting the time span considered one is faced with a trade-off. On one hand, a tighter time interval increases the chances of removing confounding general equilibrium effects that take time to unfold. On the other hand, it also limits the amount of information that can be exploited to try and disentangle the effect of the reform from that of underlying trends and cyclical effects. The results are reported in table 8 for the two genders separately.

For males, the table shows that the change in the FixT-Perm differential in the post-reform period is estimated quite consistently across panels. There is therefore no indication that choice of the length of the post-reform period has any substantive bearing on the main conclusions. Similarly, the estimates of the changes in the FixT-TAW differential are consistent with those obtained using the whole post-reform period available - the signs of the coefficients vary across columns and statistical significance is never attained. The stability of these estimates across panels lends no support to the hypothesis that general equilibrium effects might have pushed agency workers' wages up over time. The same conclusions can be reached for females based on the estimates reported in columns 5 to 8 of table 8.

Further insights can be gained from looking at the evolution of the differential over the entire period from 1997 to 2007. The yearly differential in the log wage (conditional on demographic characteristics) between fixed-term and permanent workers is graphed in figure 2 by gender while figure 3 plots the year-on-year changes and their 95% confidence intervals.

For males, figure 2 shows that the differential between fixed-term and permanent workers exhibited some variability even before the reform. The graph does suggest that the differential between fixed-term and permanent workers was moving towards zero even before the reform. The wage of fixed-term workers did increase relative to that of permanent workers after 2002 although it seems to have experienced a relative decrease in 2007. The plots of the year-on-year changes in the differentials in figure 3 show that most of these changes in the FixT-Perm differential can only be estimated imprecisely. Overall, on one hand, these graphs are consistent with the hypothesis that the new legislation did induce a reduction in the FixT-Perm differential particularly between 2003 and 2006. On the other hand, they indicate that it is hard to quantify the effect of the new legislation because even in the pre-reform period the differential did not appear flat over time.

For females, the yearly differential between fixed-term and permanent workers plotted in figure 2 shows no clear change after 2002. If anything, the conditional wage penalty for women on fixed-term contracts seem to exhibit less variability after 2002. In fact, figure 3 shows that year-on-year changes in the differential appear statistically significant before but not after the new legislation was introduced.

Figure 4 and 5 allow us to look at the evolution of the differentials between fixed-term and agency workers. When considering the FixT-TAW differential among male workers, no clear jump in the yearly differential appears in figure 4 after the new legislation was introduced. The FixT-TAW differential appear very unstable both before and after the introduction of the new legislation, resulting in the overall null effect found in the difference-in-difference exercise.

As for the differential between female fixed-term and agency workers, the yearly estimates in figure 4 suggest that the new legislation might have interrupted a negative trend causing the differential first to increase in 2003 and then to stabilise along a slightly decreasing trend. Again, the considerable and statistically significant variation in the FixT-TAW differential over the pre-reform period which appears clearly in figure 5 makes it difficult to quantify the effect of the legislation per se.

In conclusion, for both genders it is reassuring to find that altering the length of the post-reform period considered does not make a substantive difference. It does appear, however, that all the differentials considered exhibit a degree of variation in the pre-reform period that undermines the ability of the difference-in-difference exercise to provide an accurate estimate of the magnitude of the effect of the equal-treatment legislation.

7 Investigating the assumption of no compositional changes within contract groups

If the composition in unobservables of the treated and the comparison groups changes over time, one cannot hope to disentangle the effect of such changes from the pure effect of the new regulations. This would happen, for example, if individuals move across contract groups in response to the policy. Before providing some empirical evidence on this, let us consider why changes in unobservables within contract group might have occurred following the new legislation.

In the first place, the legislation might have changed the way workers select themselves into job contracts and induced some workers to take fixed-term jobs rather than permanent or agency ones. If these workers differ in their attitude to risk or in some other unmeasured characteristics from the ones not altering their choices, this could confound the actual effect of the legislation. In the second place, in response to the increase in their relative cost, firms might have decreased the use of fixed-term contracts, presumably in favour of agency employment. Moreover, firms might also change the way they select workers into fixed-term jobs, possibly by improving the quality of workers employed on such contracts.

LFS data can be used to look for indirect evidence of any of the above. Indirect tests of this sort are often necessary to investigate the validity of identification assumptions in the absence of alternative sources of identification (Imbens and Wooldridge, 2008). Although they cannot provide definite evidence, they do allow a better assessment of the plausibility of such assumptions. In this spirit, I conducted three simple exercises which are briefly described in the next section.

7.1 Empirical evidence

To try and assess the assumption that contract groups remained stable over time, I consider:

1. the evolution over time of the shares of workers working on different contracts
2. the changes in the proportion of workers who voluntarily choose temporary contracts
3. the selection of highly educated workers into fixed-term contracts before and after the reform.

In section 3, I briefly discussed the data on the share of different types of employment reported in table 1. In the context of this section, we need to highlight any changes in the shares after 2002 which might support the hypothesis that either firms or workers changed the selection mechanism. For males we do see that the share of fixed-term employment fell by 1.4 percentage points between 1997 and 2002, but almost 2/3 of this decline had already occurred by the time the new legislation came into effect in 2002. There is no clear sign of a break in 2002 nor of an acceleration of the rate of decline after 2002. For females, the evidence is perhaps more dubious. In fact, we observe a reduction in the share of fixed-term workers by a third (from 3.9 in 1997 to 2.6 in 2007), and while it is clear that the decline began well before the new legislation came into effect (at least from 1998), most of the reduction seems to have taken place after 2002. There is no clear discontinuity since the change of -.3 percentage points between 2001 and 2002 is comparable to those between 1998-1999 and 2004-2005. In addition, it is not obvious that the change between 2001 and 2002 can be attributed to the legislation given that the reform only came into effect in October 2002. Nevertheless, the evidence does warrant caution in interpreting the results for females.

Second, I estimated a linear model for the probability of being a voluntary temporary worker on the subsample of fixed-term and agency workers. Since the new legislation made fixed-term jobs relatively more attractive, evidence that voluntary temporary employment increased more (or decreased less) among fixed-term workers than among agency workers would lend support to the hypothesis that the reform affected the selection of workers into contract types. Table 9 shows that voluntary temporary workers are more likely among agency workers for both genders. For males there is no evidence of any change after 2002 for either type of contract, since both the $Post$ dummy and its interaction with TAW attract coefficients which are statistically and practically equal to zero. For females, there is evidence of a small decrease in voluntary temporary employment after 2002 (-3.2%). However, there are no significant differences between fixed-term and agency workers as indicated by the coefficient on the interaction $Post * TAW$.

Finally, I looked for evidence that, given the increase in their relative cost, firms tried and hire more highly-educated workers on fixed-term contracts. If no evidence is found of a change in selection on education, then it is more credible that the reform induced no change in the selection based on some other unobserved measure of workers' quality. I estimated two linear probability models, one for the probability of being fixed-term vs permanent (columns 1 and 2 of table 10) and the other for fixed-term vs agency (columns 3 and 4). For both genders, there is no evidence that highly educated workers became more likely to be selected into fixed-term rather than permanent contracts after the reform. On the contrary, the relative size of the coefficients implies that, if anything, after 2002 highly educated individuals were less likely to be on fixed-term contracts. Estimates from a multinomial logit (not reported here) lead to the same substantive conclusions.

The last two columns of table 10 show some evidence that highly educated workers became more likely to work on fixed-term rather than agency contracts after 2002, but the coefficient on the relevant interaction is significant only for males. Although this is only a rough test, it does cast doubt on the validity of the assumption that the composition of unobservables within the fixed-term and agency groups remained stable over time, at least for males.

To summarise, in this section we have found limited evidence that the selection of workers into contract types changed following the introduction of the equal-treatment legislation. In particular, there has been a decrease in the share of fixed-term contracts among women and an increase in the likelihood to be employed on a fixed-term contract rather than on an agency one for highly educated men. While the other tests described above provide more reassuring results, this evidence does warrant caution in the causal interpretation of the estimate of the effect of the new legislation on wage differentials.

8 Discussion of other potential limitations

As described in the Introduction, the 2002 Regulations contained a number of provisions that in practice made it possible to limit the scope of applicability of the equal-treatment principle. In particular, the necessity to find a comparable permanent employee within the same firm, the possibility of "objectively" justifying unequal treatment and the possibility of balancing different working conditions could jointly attenuate the impact of the new legislation on the observed contract differentials.

In the second place, even in the presence of negligible effects on the average differentials, the legislation might have produced significant effects in some segments of the wage distribution. Table 11 presents the *unconditional* wage differentials at each decile before (in 2000) and after (in 2004) the equal-treatment legislation. The differentials are decomposed using the methodology proposed by Melly (2005) into three components driven by (i) observable characteristics, (ii) coefficients and (iii) the distribution of the residuals. A negative (positive) figure represents a penalty (premium) for fixed-term workers and bootstrapped t-statistics are reported in parentheses[5]. For females, column 1 shows that the unconditional wage premium which we previously found around the mean is also observed across the wage distribution. However, this premium is statistically significant only in the upper part of the distribution, ranging from 0.07 log-points at the median to 0.17 at the ninth decile. Column 5 shows that the unconditional premium had increased across the distribution by 2004, well exceeding 0.20 log-points at all deciles above the median. The decomposition results show that in both years the best part of the observed differentials is explained by differences in observed characteristics, which is consistent with our earlier finding that the conditional OLS differential is negative.

For males, the picture is less clear. In 2002, we see that the sign of the estimates varies across the wage distribution but statistical precision is never attained (column 1). After the reform (column 5), we continue to observe statistically insignificant unconditional wage penalties at the bottom of the wage distribution, but in the upper part of the distribution we now see larger wage premia which are statistically significant for the 6th, 8th and 9th decile. The decomposition results suggest that the (statistically insignificant) wage penalties found at the bottom of the wage distribution in both years are largely driven by differences in the coefficients, i.e. in the remuneration of observable characteristics. There is no indication that this changed in 2004, but again all these figures are estimated with low precision.

Overall, these results lend some (statistically weak) support to the hypothesis that fixed-term contracts are associated with less of a premium (females) or more of a penalty (males) at the bottom of the wage distribution. In addition, there is no indication of a change following the reform. These results can only be seen as preliminary since a full assessment of the causal effect of the reform across the wage distribution requires careful consideration of a number of underlying assumptions, as highlighted by recent contributions in the literature on decomposition methods (Fortin et al., 2011) and quantile treatment effects (Frolich and Melly, 2010). Such an analysis is beyond the scope of this paper and is left for future research.

9 Concluding remarks

Following a EU directive adopted in 1999, all Member States now have legislation dictating that fixed-term workers should receive the same wage and benefits as permanent workers. This paper has looked at the wage effects of the introduction of such legislation in the UK in 2002.

Using data from the Labour Force Survey, I find that in the five years before the introduction of the equal-treatment principle, male fixed-term workers suffered a 4% wage penalty. This is considerably smaller than the penalty found by Booth et al. (2002) in a slightly earlier period (1991-1997) using data from the BHPS. The evidence of this paper also indicates that the gap between fixed-term and permanent workers closed and possibly turned into a positive difference after 2002. There are however several pieces of evidence that cast doubt on the extent to which this change can be interpreted as a direct consequence of the new legislation. In the first place, the differential between fixed-term and permanent workers appears to have been decreasing even before the new legislation. Secondly, there is evidence that the wages of agency workers increased relative to that of permanent workers over the same time period in spite of the fact that the equal-treatment legislation did not apply to them. The paper has explored the possibility of using agency workers as a counterfactual for what would have happened to fixed-term workers in the absence of the new legislation. The variability of the differential between the two groups before 2002 indicates that agency workers are unlikely to be a suitable control group for fixed-term workers. Finally, there is also some evidence that following the introduction of the equal-treatment legislation the selection of workers into contract types might have changed. Overall, therefore, there are doubts regarding the extent to which the observed closing of the gap between male fixed-term and permanent workers can be ascribed to the new legislation.

For females, in line with the results by Booth et al. (2002), there is no evidence of a wage penalty for fixed-term workers unless demographic characteristics are controlled for. In other words, women on temporary contracts tend to have characteristics which are associated with higher wages, but they are paid less than women with the same characteristics employed on permanent contracts. Booth et al. (2002) suggest that the difference in the role of fixed-term contracts between genders might be explained by women's stronger preference for career flexibility. Since temporary contracts make career changes and breaks easier, women's preference for flexibility makes them more likely to be on temporary contracts. In addition, career breaks and changes are arguably easier for women with higher general human capital or ability. Their preference for flexibility can then explain why we observe women with high human capital on fixed-term contracts, leading to the finding of an unconditional wage premium which is not observed for men.

The analysis of this paper has also shown that there is no clear sign that the conditional wage differential between fixed-term and permanent workers decreased after 2002 among women. This might again be explained by women's preference for the higher degree of career flexibility associated with fixed-term contracts. Given such preferences, women might be more willing to accept lower wages to obtain these contracts, therefore making the wage gap between the two types of contracts more resilient than among men.

The fact that the wages of female agency workers decreased after 2002 suggests an interpretation whereby the equal-treatment legislation prevented fixed-term workers' wages from following the same pattern. However, there is no evidence that the wages of the agency and fixed-term workers behaved similarly before 2002. It also seems unlikely that the changes in agency workers' wages can be explained as an indirect effect of the equal-treatment legislation for fixed-term workers. In fact, simple economic intuition suggests that the new rules would have increased the demand for and decreased the supply of agency workers therefore generating upward rather downward pressure on their wages.

This paper has also presented some tentative evidence that fixed-term contracts bear more of a penalty (for males) or less of a premium (for females) in the lower half of the wage distribution and that no clear change was observed after the new legislation was implemented. Future research should build on the growing literature on decomposition methods and quantile treatment effects (Frolich and Melly, 2010; Fortin et al., 2011) to provide a full assessment of the causal effect of the equal-treatment legislation across the wage distribution.

Figures

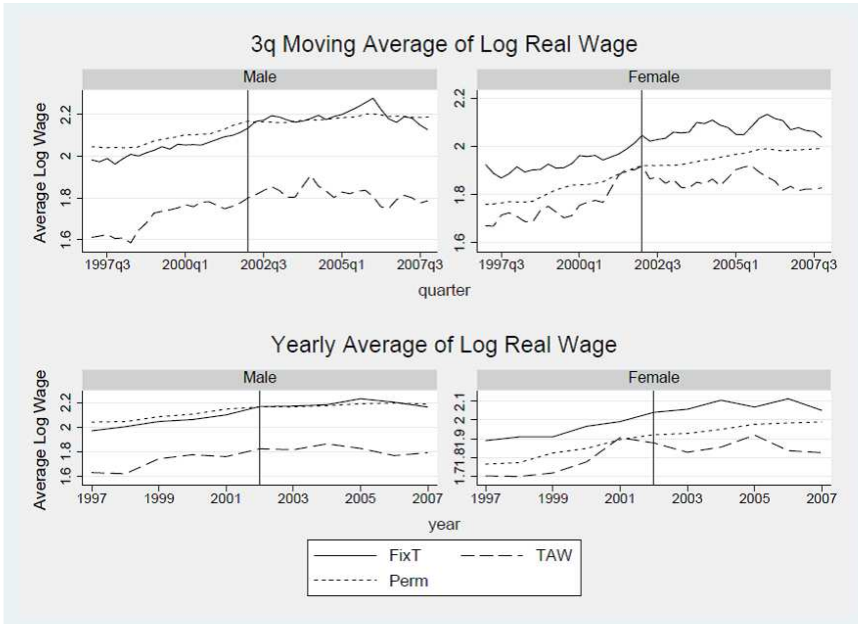

Figure 1: Average log real hourly wage by contract and gender in the UK.

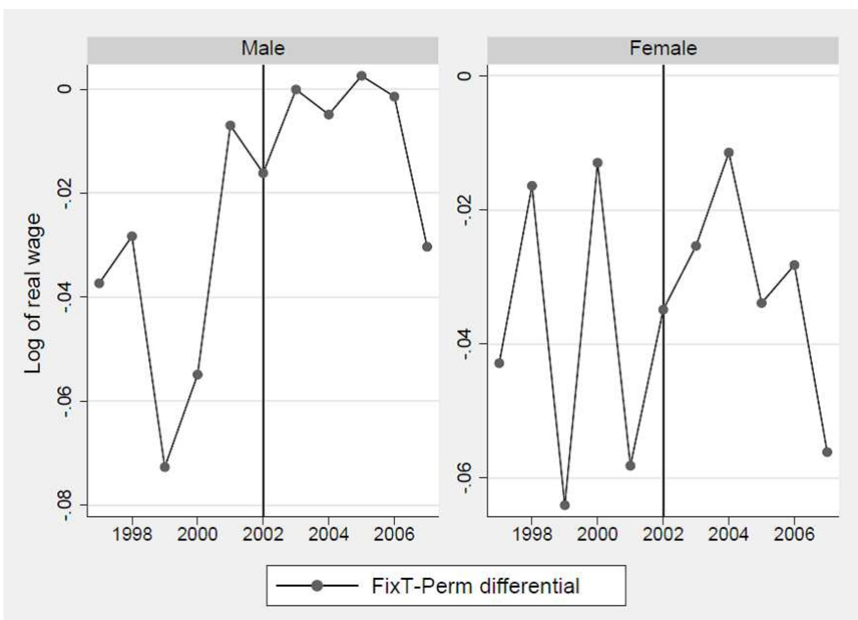

Figure 2: Yearly log of real wage differential between fixed-term and permanent workers.

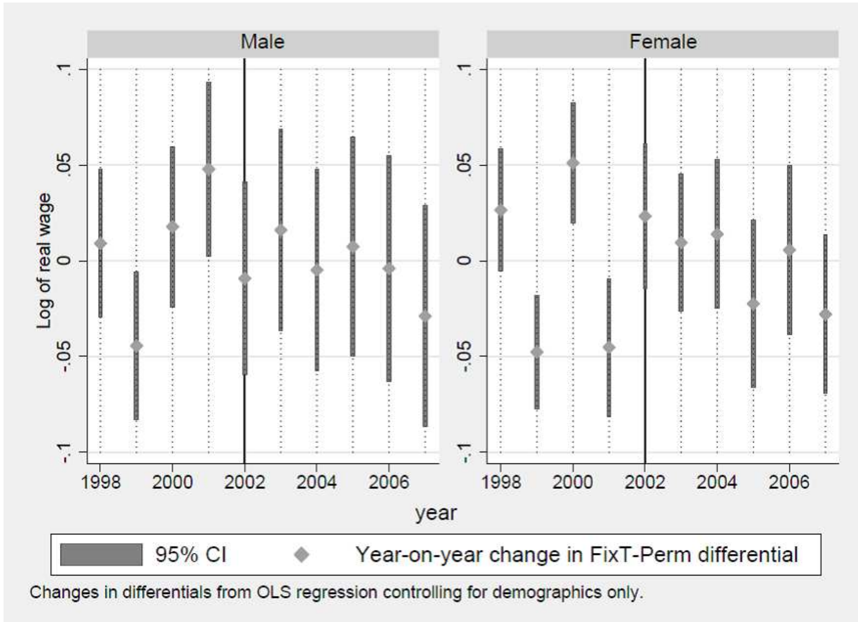

Figure 3: Year-on-year changes in log of wage differential between fixed-term and permanent workers.

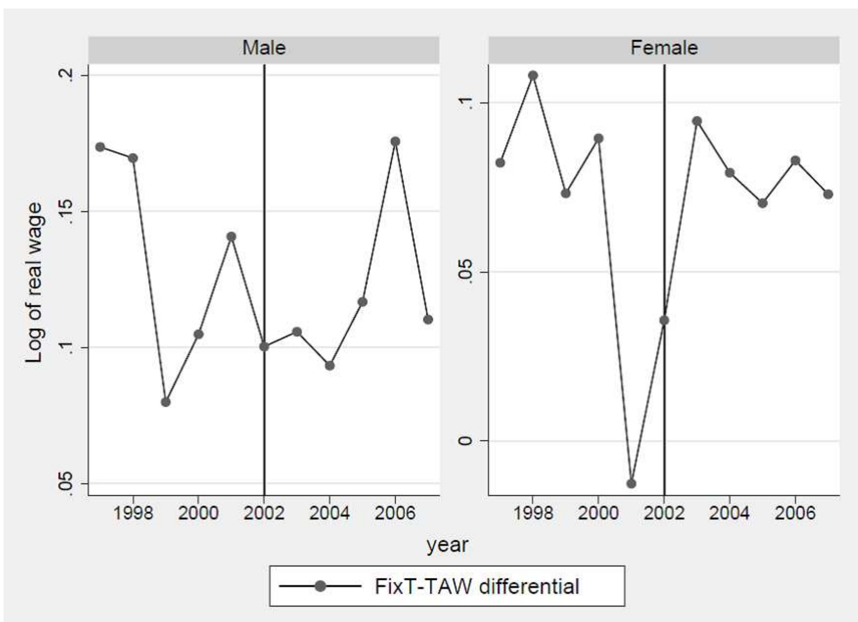

Figure 4: Yearly log of wage differential between fixed-term and agency workers.

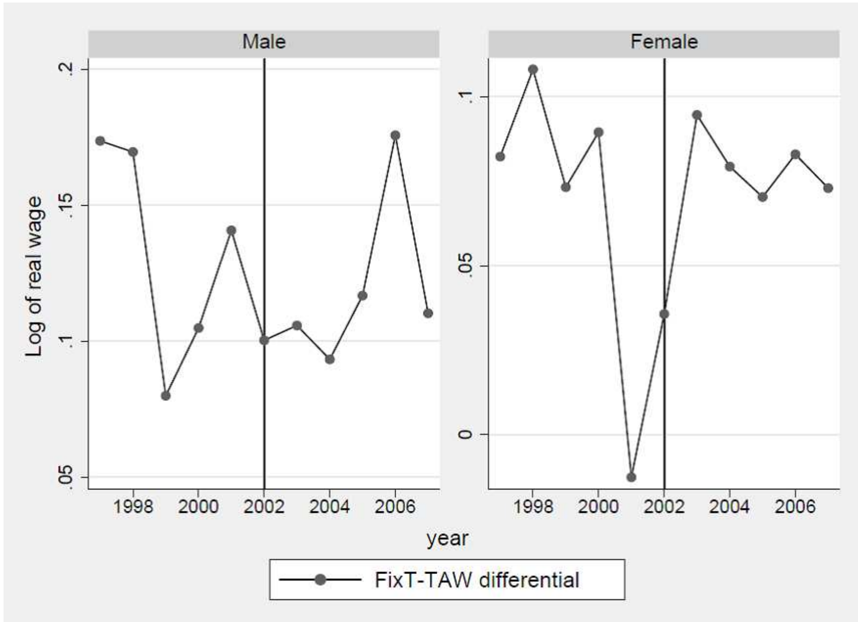

Figure 5: Year-on-year changes in log of wage differential between fixed-term and agency workers.

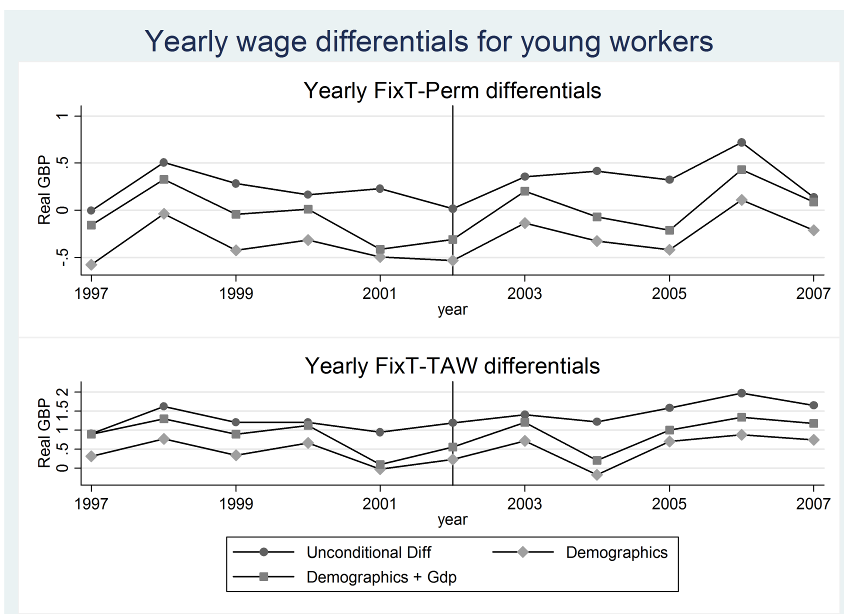

Figure 6: Yearly wage differentials for workers below the age of 30.

Tables

Table 1: Shares of workers in a given labour contract.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Perm	**FixT**	**TAW**	**OtherT**[a]	**Perm**	**FixT**	**TAW**	**OtherT**[a]
1997	93.3	3.4	.9	2.5	91.7	3.9	1.0	3.3
	(.144)	(.104)	(.054)	(.089)	(.160)	(.112)	(.059)	(.104)
1998	93.9	3.2	.9	2.0	91.9	4.0	1.1	3.0
	(.132)	(.096)	(.052)	(.078)	(.150)	(.108)	(.058)	(.094)
1999	93.9	3.1	.9	2.1	92.5	3.7	1.1	2.7
	(.134)	(.097)	(.054)	(.081)	(.148)	(.106)	(.059)	(.091)
2000	94.2	2.7	1.1	2.0	92.5	3.5	1.1	2.9
	(.136)	(.094)	(.061)	(.081)	(.152)	(.107)	(.060)	(.097)
2001	94.4	2.6	1.1	1.9	92.8	3.6	1.1	2.5
	(.156)	(.109)	(.069)	(.093)	(.174)	(.125)	(.072)	(.105)
2002	94.9	2.4	1.0	1.7	93.3	3.3	1.0	2.4
	(.130)	(.091)	(.059)	(.076)	(.147)	(.105)	(.059)	(.090)
2003	95.0	2.3	1.0	1.7	93.6	3.1	1.0	2.2
	(.132)	(.091)	(.060)	(.078)	(.146)	(.104)	(.059)	(.089)
2004	94.9	2.3	.9	1.8	94.0	3.0	.8	2.1
	(.137)	(.094)	(.059)	(.084)	(.147)	(.106)	(.056)	(.089)
2005	95.3	2.2	1.0	1.5	94.5	2.7	.8	2.1
	(.134)	(.093)	(.062)	(.079)	(.143)	(.101)	(.054)	(.089)
2006	95.5	2.1	.9	1.5	94.3	2.7	.9	2.1
	(.136)	(.094)	(.063)	(.079)	(.148)	(.104)	(.062)	(.091)
2007	95.3	2.0	1.1	1.7	94.2	2.6	.8	2.4
	(.136)	(.088)	(.066)	(.083)	(.147)	(.100)	(.057)	(.096)

a: job is seasonal, casual, or temporary for "other reasons".

Standard errors clustered at the individual level.

LFS quarterly data (1997-2007)

Table 2: Average value of some explanatory variables for female fixed-term workers and differences with agency and permanent workers.

	(1) **Age**	(2) **HighEdu**	(3) **Tenure**	(4) **Public**	(5) **>50 employees**	(6) **Professionals**	(7) **Clerical**	(8) **Plant operatives**	(9) **Part-time**
1997-2001									
FixT	36.96***	.14***	2.71***	.64***	.54***	.28***	.21***	.03***	.50***
	(.173)	(.005)	(.066)	(.007)	(.007)	(.007)	(.006)	(.002)	(.007)
Perm-FixT	1.94***	–.03***	4.22***	–.31***	–.05***	–.19***	.05***	.01***	–.06***
	(.177)	(.005)	(.070)	(.007)	(.008)	(.007)	(.006)	(.002)	(.007)
TAW-FixT	–2.98***	–.03**	–1.66***	–.40***	.17***	–.23***	.34***	.04***	–.16***
	(.375)	(.010)	(.089)	(.013)	(.014)	(.009)	(.014)	(.007)	(.014)
Obs.	139705	138901	139599	139441	139489	139681	139681	139681	139699
2002-2007									
FixT	37.80***	.13***	3.03***	.65***	.51***	.30***	.17***	.01***	.46***
	(.196)	(.006)	(.082)	(.008)	(.008)	(.007)	(.006)	(.002)	(.008)
Perm-FixT	2.57***	–.01*	4.39***	–.29***	–.02**	–.20***	.06***	.01***	–.04***
	(.199)	(.006)	(.085)	(.008)	(.008)	(.007)	(.006)	(.002)	(.008)
TAW-FixT	–2.54***	–.03**	–1.66***	–.35***	.17***	–.23***	.28***	.04***	–.14***
	(.421)	(.010)	(.123)	(.015)	(.016)	(.011)	(.015)	(.006)	(.015)
Obs.	152130	143356	151964	151840	144484	152104	152104	152104	152120
Wald tests for changes in differences before and after									
Perm-FixT:									
Wald test	5.718	2.788	2.394	3.104	5.585	.703	2.371	1.521	4.064
P-value	.017	.095	.122	.078	.018	.402	.124	.217	.044
Perm-TAW:									
Wald test	.606	.031	.000	7.324	.015	.010	9.324	.108	1.457
P-value	.436	.860	.999	.007	.901	.919	.002	.742	.227

*Significance levels:** 10% ** 5% *** 1%

Standard errors clustered at the individual level.

LFS quarterly data (1997-2007)

Table 3: Average value of some explanatory variables for male fixed-term workers and differences with agency and permanent workers.

	(1) Age	(2) HighEdu	(3) Tenure	(4) Public	(5) >50 employees	(6) Professionals	(7) Clerical	(8) Plant operatives	(9) Part-time
1997-2001									
FixT	37.17***	.09***	2.42***	.39***	.70***	.28***	.12***	.10***	.17***
	(.221)	(.005)	(.085)	(.008)	(.007)	(.007)	(.005)	(.005)	(.006)
Perm-FixT	1.82***	.00	6.35***	–.20***	–.12***	–.17***	–.04***	.05***	–.11***
	(.223)	(.005)	(.090)	(.008)	(.008)	(.007)	(.005)	(.005)	(.006)
TAW-FixT	–2.94***	.00	–1.73***	–.29***	.03*	–.23***	.15***	.20***	–.04**
	(.450)	(.009)	(.096)	(.011)	(.014)	(.010)	(.013)	(.014)	(.011)
Obs.	139649	138701	139543	139381	139334	139623	139623	139623	139646
2002-2007									
FixT	39.00***	.09***	2.93***	.44***	.66***	.32***	.10***	.07***	.21***
	(.265)	(.006)	(.113)	(.009)	(.009)	(.009)	(.005)	(.005)	(.007)
Perm-FixT	1.48***	.00	5.79***	–.24***	–.10***	–.19***	–.05***	.06***	–.13***
	(.267)	(.006)	(.116)	(.009)	(.009)	(.009)	(.005)	(.005)	(.007)
TAW-FixT	–2.98***	–.02*	–1.84***	–.29***	.02	–.26***	.06***	.19***	–.06***
	(.471)	(.009)	(.138)	(.014)	(.016)	(.011)	(.011)	(.013)	(.012)
Obs.	147068	138393	146874	146776	139425	147026	147026	147026	147059
Wald tests for changes in differences before and after									
Perm-FixT:									
Wald test	.947	.190	14.785	12.864	3.987	4.673	.133	.381	5.892
P-value	.331	.663	.000	.000	.046	.031	.715	.537	.015
Perm-TAW:									
Wald test	.002	3.525	.440	.137	.043	4.051	26.348	.104	1.732
P-value	.961	.060	.507	.712	.836	.044	.000	.748	.188

*Significance levels:** 10% ** 5% *** 1%

Standard errors clustered at the individual level.

LFS quarterly data (1997-2007)

Table 4: Average wage for permanent workers and differentials with temporary workers before and after the introduction of the equal-treatment legislation in 2002.

	Hourly Pay		**Log of Hourly Pay**	
	Before	After	Before	After
	(1)	(2)	(3)	(4)
Males				
Permanent	9.176***	10.087***	2.081***	2.179***
	(.016)	(.017)	(.002)	(.002)
FixT-Perm	–.398***	.203*	–.053***	.007
	(.081)	(.108)	(.009)	(.010)
TAW-Perm	–3.034***	–3.310***	–.374***	–.363***
	(.097)	(.096)	(.012)	(.011)
OtherT-Perm	–3.575***	–4.039***	–.507***	–.512***
	(.074)	(.084)	(.010)	(.010)
Obs.	142364	149204	142364	149204
Females				
Permanent	6.977***	7.994***	1.816***	1.957***
	(.012)	(.013)	(.002)	(.001)
FixT-Perm	.894***	.996***	.112***	.114***
	(.066)	(.077)	(.008)	(.008)
TAW-Perm	–.558***	–.917***	–.065***	–.101***
	(.089)	(.106)	(.012)	(.012)
OtherT-Perm	–1.620***	–1.877***	–.287***	–.283***
	(.060)	(.073)	(.008)	(.009)
Obs.	143463	155145	143463	155145

Hourly pay is constructed y the ONS as pay over usual hours.

Top and ottom 1% excluded, wage deflated using RPI with 2000 ase.

*Significance levels:** 10% ** 5% *** 1%

Standard errors account for clustering at the individual level.

LFS quarterly data (1997-2007)

Table 5: Decomposition of the change in the wage differential between fixed-term workers and permanent workers before and after 2002.

	Overall (1)	**Quantity effect** (2)	**Price effect** (3)	**Interaction** (4)
Males		Δ in log(w): before –.041; after .024		
Change in:				
Overall Δ	.065			
Explained Δ	.042	.028	.023	–.009
Unexplained Δ	.023	.024	.000	–.001
Females		Δ in log(w): before .096; after .104		
Change in:				
Overall Δ	.008			
Explained Δ	.010	.008	.004	–.002
Unexplained Δ	–.002	–.006	.003	.001

Results of Juhn-Murphy-Pierce decomposition (1991).
Wage differential computed as FixT-Perm, permanent workers used as reference group.
LFS quarterly data (1997-2007)

Table 6: Estimates from wage regressions; dependent variale is log of real wage and permanent workers are the excluded contract group.

	Males				**Females**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FixT	–.049***	–.042***	–.013	.137	.113***	–.037***	–.049***	.030
	(.009)	(.007)	(.015)	(.152)	(.008)	(.006)	(.012)	(.114)
PostXFixT	.055***	.034***	.029**	.049**	.001	.006	.007	.018
	(.013)	(.011)	(.011)	(.023)	(.011)	(.008)	(.008)	(.017)
TAW	–.378***	–.174***	–.156***	–.157	–.063***	–.110***	–.066***	–.050
	(.012)	(.011)	(.023)	(.205)	(.012)	(.011)	(.025)	(.209)
PostXTAW	.014	.047***	.045***	.045	–.038**	.004	–.001	.001
	(.017)	(.016)	(.016)	(.032)	(.017)	(.015)	(.016)	(.032)
Gdp			.009***	.009***			.009***	.010***
			(.003)	(.003)			(.003)	(.003)
GdpXFixT			–.036**	–.041**			.015	.012
			(.018)	(.019)			(.014)	(.014)
GdpXTAW			–.023	–.023			–.057**	–.057*
			(.026)	(.027)			(.028)	(.029)
Trend				.003***				.003***

Continued next page...

...table 6 continued

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				(.000)				(.000)
TrendXFixT				–.001				–.000
				(.001)				(.001)
TrendXTAW				.000				–.000
				(.001)				(.001)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Reg. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Job charact.	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Ind. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occ. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Obs.	286717	255589	255589	255589	291835	256038	256038	256038
F equal GDP			2.402	2.696			2.630	2.242
P-value			.090	.068			.072	.106
F equal trends				.504				.247
P-value				.604				.781

Trend is a quarterly linear trend. GdpChange is quarterly real GDP growth.
a: Results not affected by the exclusion of quarterly dummies.
Demographics: Education, Bad Health, NonBritish, Children, Married, Single.
Job characteristics: Tenure, Experience, Public, Employment Size, PartTime.
Significance levels: * 10% ** 5% *** 1%
Standard errors clustered at the individual level.
LFS quarterly data (1997-2007)

Table 7: Estimates from OLS hourly wage regressions using dummies defined to show differentials among different contract groups.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FixT-TAW	.329***	.132***	.142***	.294	.176***	.073***	.017	.081
	(.015)	(.013)	(.028)	(.253)	(.014)	(.012)	(.027)	(.237)
Post*(FixT-TAW)	.041*	–.013	–.016	.005	.039**	.002	.009	.017
	(.021)	(.019)	(.019)	(.039)	(.020)	(.017)	(.018)	(.036)
FixT-Perm	–.049***	–.042***	–.013	.137	.113***	–.037***	–.049***	.030
	(.009)	(.007)	(.015)	(.152)	(.008)	(.006)	(.012)	(.114)
Post*(FixT-Perm)	.055***	.034***	.029**	.049**	.001	.006	.007	.018
	(.013)	(.011)	(.011)	(.023)	(.011)	(.008)	(.008)	(.017)
Gdp			.009***	.009***			.009***	.010***
			(.003)	(.003)			(.003)	(.003)
GdpXFixT			–.036**	–.041**			.015	.012

Continued next page...

...table 7 continued

	Males				**Females**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			(.018)	(.019)			(.014)	(.014)
GdpXTAW			–.023	–.023			–.057**	–.057*
			(.026)	(.027)			(.028)	(.029)
Trend				.003***				.003***
				(.000)				(.000)
TrendXFixT				–.001				–.000
				(.001)				(.001)
TrendXTAW				.000				–.000
				(.001)				(.001)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Reg. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Job charact.	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Ind. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occ. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Obs.	286717	255589	255589	255589	291835	256038	256038	256038

Trend is a quarterly linear trend. GdpChange is quarterly real GDP growth.

a: Results not affected by the exclusion of quarterly dummies.

Demographics: Education, Bad Health, NonBritish, Children, Married, Single.

Job characteristics: Tenure, Experience, Public, Employment Size, PartTime.

*Significance levels:** 10% ** 5% *** 1%

Standard errors clustered at the individual level.

LFS quarterly data (1997-2007)

Table 8: OLS estimates of differentials in log of hourly wage before and after the reform.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1997-2003								
FixT-TAW	.330***	.130***	.147***	1.186***	.176***	.074***	.010	.274
	(.015)	(.013)	(.031)	(.427)	(.014)	(.012)	(.031)	(.389)
Post*(FixT-TAW)	.021	–.028	–.031	.056	.020	–.003	.004	.026
	(.029)	(.026)	(.026)	(.045)	(.026)	(.024)	(.024)	(.042)
FixT-Perm	–.049***	–.039***	–.005	.260	.113***	–.037***	–.052***	–.111
	(.009)	(.007)	(.017)	(.234)	(.008)	(.006)	(.013)	(.183)
Post*(FixT-Perm)	.052***	.033**	.028*	.052**	.010	.007	.009	.004
	(.018)	(.016)	(.016)	(.025)	(.014)	(.011)	(.011)	(.019)
Obs.	192915	174905	174905	174905	194262	172456	172456	172456
1997-2004								
FixT-TAW	.330***	.131***	.157***	1.184***	.176***	.073***	.012	.178
	(.015)	(.013)	(.029)	(.397)	(.014)	(.012)	(.029)	(.370)
Post*(FixT-TAW)	.011	–.030	–.036	.060	.036	–.001	.007	.023
	(.025)	(.023)	(.023)	(.044)	(.023)	(.021)	(.022)	(.042)
FixT-Perm	–.049***	–.040***	–.001	.278	.113***	–.037***	–.047***	–.132
	(.009)	(.007)	(.016)	(.221)	(.008)	(.006)	(.013)	(.170)
Post*(FixT-Perm)	.053***	.035**	.027**	.055**	.019	.013	.014	.006
	(.016)	(.014)	(.014)	(.025)	(.013)	(.010)	(.010)	(.019)
Obs.	217372	192308	192308	192308	219369	190238	190238	190238
1997-2005								
FixT-TAW	.330***	.132***	.157***	.978***	.176***	.073***	.015	.151
	(.015)	(.013)	(.029)	(.367)	(.014)	(.012)	(.029)	(.347)
Post*(FixT-TAW)	.024	–.027	–.033	.051	.022	–.001	.009	.023
	(.023)	(.021)	(.022)	(.044)	(.022)	(.020)	(.020)	(.041)
FixT-Perm	–.049***	–.041***	–.003	.218	.113***	–.037***	–.047***	–.107
	(.009)	(.007)	(.016)	(.206)	(.008)	(.006)	(.013)	(.160)
Post*(FixT-Perm)	.061***	.037***	.028**	.052**	.009	.011	.012	.006
	(.015)	(.013)	(.013)	(.025)	(.012)	(.009)	(.010)	(.019)
Obs.	240857	211445	211445	211445	244094	210275	210275	210275
1997-2006								
FixT-TAW	.329***	.131***	.144***	.347	.176***	.073***	.017	.087
	(.015)	(.013)	(.028)	(.305)	(.014)	(.012)	(.028)	(.283)
Post*(FixT-TAW)	.039*	–.012	–.015	.009	.037*	.002	.010	.018
	(.022)	(.020)	(.020)	(.041)	(.021)	(.018)	(.019)	(.039)
FixT-Perm	–.049***	–.042***	–.013	.084	.113***	–.037***	–.049***	–.076
	(.009)	(.007)	(.016)	(.177)	(.008)	(.006)	(.012)	(.132)
Post*(FixT-Perm)	.060***	.038***	.032***	.044*	.010	.010	.012	.009
	(.014)	(.012)	(.012)	(.024)	(.011)	(.009)	(.009)	(.018)
Obs.	263418	233182	233182	233182	267673	232852	232852	232852
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Continued next page...

...table 8 continued

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reg. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Job charact.	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Ind. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occ. Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Trends	No	No	No	Yes	No	No	No	Yes
GdpChange	No	No	Yes	Yes	No	No	Yes	Yes

Trend is a quarterly linear trend. GdpChange is quarterly real GDP growth.
Demographics: Education, Bad Health, NonBritish, Children, Married, Single.
Job characteristics: Tenure, Experience, Public, Employment Size, PartTime.
*Significance levels:** 10% ** 5% *** 1%
Standard errors clustered at the individual level.
LFS quarterly data (1997-2007)

Table 9: Linear model for the probability of eing a voluntary temporary employee.

	Males	Females
TAW	.063***	.187***
	(.014)	(.015)
Post*TAW	–.002	–.018
	(.018)	(.020)
Post	–.002	–.036***
	(.009)	(.009)
Obs.	9110	11351

Dep var is 1 if "did not want a permanent job".
Sample restricted to FixT and TAW workers only.
*Significance levels:** 10% ** 5% *** 1%
Standard errors clustered at the individual level.
LFS quarterly data (1997-2007)

Table 10: Linear models for the probability of working on a fixed-term contract.

Dep Var:	**1 FixT, 0 Perm**		**1 FixT, 0 TAW**	
	Males	**Females**	**Males**	**Females**
	(1)	(2)	(3)	(4)
Post	–.010***	–.001	–.115***	–.045
	(.001)	(.001)	(.031)	(.029)
HighEdu	.004**	.017***	–.055**	–.022
	(.002)	(.002)	(.027)	(.026)
Post*HighEdu	–.005**	–.019***	.072**	.046
	(.002)	(.002)	(.033)	(.031)
MedEdu	.004***	.005***	.002	.014
	(.001)	(.002)	(.027)	(.027)

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...table 10 continued

	DepVar: 1 FixT, 0 Perm		DepVar: 1 FixT, 0 TAW	
	Males	**Females**	**Males**	**Females**
	(1)	(2)	(3)	(4)
Post*MedEdu	–.003*	–.009***	.021	.000
	(.002)	(.002)	(.036)	(.036)
LowEdu	.003**	.006***	–.000	.007
	(.001)	(.001)	(.026)	(.025)
Post*LowEdu	–.002	–.010***	.022	–.024
	(.002)	(.002)	(.036)	(.033)
Obs.	269593	266446	9399	11710

Demographics: Education, Bad Health, NonBritish, Children, Married, Single.
Job characteristics: Tenure, Experience, Public, Employment Size, PartTime.
*Significance levels:** 10% ** 5% *** 1%
Standard errors clustered at the individual level.
LFS quarterly data (1997-2007)

Table 11: Decomposition of unconditional log wage differentials between fixed-term and permant workers across the wage distribution. Bootstrapped t-statistics in parenthesis.

	2000				2004			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Decile	**Total**	**X**	**b**	**Resid**	**Total**	**X**	**b**	**Resid**
				Females				
1	0.017	0.129	-0.095	-0.017	0.005	0.200	-0.050	-0.145
	(0.54)	(2.04)	(1.34)	(0.31)	(0.07)	(1.70)	(0.41)	(1.34)
2	0.037	0.113	-0.070	-0.006	0.071	0.148	-0.026	-0.052
	(1.48)	(2.07)	(1.21)	(0.17)	(1.12)	(1.50)	(0.25)	(0.81)
3	0.046	0.102	-0.057	0.001	0.126	0.138	-0.004	-0.008
	(2.01)	(2.00)	(1.08)	(0.04)	(2.68)	(1.76)	(0.05)	(0.16)
4	0.050	0.090	-0.048	0.008	0.168	0.142	0.002	0.024
	(1.75)	(1.79)	(0.93)	(0.25)	(2.91)	(1.67)	(0.02)	(0.51)
5	0.068	0.093	-0.034	0.009	0.223	0.169	0.003	0.051
	(2.00)	(1.85)	(0.67)	(0.28)	(3.84)	(1.93)	(0.04)	(1.10)
6	0.090	0.097	-0.019	0.011	0.274	0.198	-0.001	0.078
	(2.47)	(1.92)	(0.36)	(0.32)	(5.32)	(2.36)	(0.01)	(1.69)
7	0.107	0.102	-0.008	0.014	0.297	0.200	-0.010	0.107
	(2.83)	(1.95)	(0.16)	(0.37)	(7.34)	(2.60)	(0.12)	(2.29)
8	0.136	0.109	0.001	0.026	0.264	0.144	-0.014	0.134
	(3.21)	(2.02)	(0.01)	(0.65)	(8.94)	(1.86)	(0.15)	(2.69)
9	0.175	0.096	0.012	0.068	0.239	0.091	-0.022	0.169
	(3.45)	(1.58)	(0.20)	(1.32)	(4.74)	(0.97)	(0.20)	(2.59)
				Males				
1	-0.089	0.160	-0.118	-0.132	-0.117	0.103	-0.113	-0.106

Continued next page...

...table 11 continued

	2000				2004			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Decile	**Total**	**X**	**b**	**Resid**	**Total**	**X**	**b**	**Resid**
	(1.83)	(2.04)	(1.28)	(1.54)	(1.86)	(0.75)	(0.72)	(1.08)
2	-0.053	0.084	-0.091	-0.046	-0.073	0.064	-0.080	-0.057
	(1.15)	(1.35)	(1.25)	(0.89)	(1.71)	(0.56)	(0.59)	(0.82)
3	-0.025	0.054	-0.057	-0.022	-0.025	0.056	-0.047	-0.034
	(0.66)	(0.92)	(0.80)	(0.52)	(0.43)	(0.51)	(0.37)	(0.58)
4	0.010	0.049	-0.028	-0.011	0.025	0.057	-0.014	-0.017
	(0.27)	(0.87)	(0.38)	(0.26)	(0.40)	(0.52)	(0.11)	(0.30)
5	0.021	0.025	-0.005	0.001	0.086	0.075	0.014	-0.003
	(0.54)	(0.45)	(0.06)	(0.02)	(1.54)	(0.69)	(0.11)	(0.06)
6	0.024	0.007	0.000	0.017	0.109	0.061	0.039	0.009
	(0.79)	(0.13)	(0.00)	(0.36)	(2.37)	(0.55)	(0.29)	(0.15)
7	-0.002	-0.030	-0.007	0.035	0.125	0.034	0.062	0.028
	(0.04)	(0.55)	(0.07)	(0.69)	(1.55)	(0.26)	(0.43)	(0.43)
8	-0.017	-0.061	-0.009	0.053	0.183	0.043	0.087	0.053
	(0.40)	(1.04)	(0.09)	(0.97)	(2.47)	(0.31)	(0.53)	(0.68)
9	0.010	-0.074	-0.015	0.099	0.207	-0.007	0.118	0.096
	(0.19)	(1.12)	(0.14)	(1.30)	(2.85)	(0.05)	(0.58)	(0.78)

Positive (negative) figures are wage premia (penalties) for fixed-term workers.
LFS sample: all employees in wave 1 from all quarters of each year.
Decomposition obtained in STATA 12 using the command cdeco_jmp
by Chernozhukov et al. (2012).

Notes

[1]What constitutes an objective justification is not specified in the Regulations, but it is stipulated that unequal treatment can be justified when it is a necessary and proportionate way to achieve a legitimate objective, such as a genuine business objective.

[2]The new regulations also abolished the possibility of waiving the right to redundancy payment for fixed-term workers and dictate that such workers cannot be selected for redundancy purely because they are on fixed-term contracts. Finally, the maximum cumulative length of consecutive temporary contracts is set at four years unless further extensions can be objectively justified.

[3]Even if the legislation made the cost to the firm of permanent and fixed-term contracts equal, there are still features of fixed-term contracts that can make them appealing to firms, most prominently the absence of “firing costs” at the end of the contract itself. On the other hand, the Regulations prevent fixed-term contracts from being renewed indefinitely, so one would not expect a complete substitution of permanent contracts with fixed-term contracts.

[4]For reasons that remain unclear, information on union membership and coverage is missing in the LFS quarterly datasets for most of the years considered in the current analysis. Since permanent workers are more likely to be unionised and unionised workers generally have higher wages this might result in a positive bias in the estimate of the wage differential. Note that Booth et al. (2002) find statistically and economically significant differentials even after controlling for union coverage.

[5]These estimates are obtained in STATA 12 using the cdeco_jmp command written by Victor Chernozhukov, Ivan Fernandez-Val and Blaise Melly. To minimise computing time, these estimates are obtained by restricting the sample to employees in wave 1 in both years (i.e. employees in wave 5 are excluded - all other waves have no earning information).

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