Economics Working Paper Series

2012/007

New Estimates of the Effect of Temporary Employment on Absenteeism

Inmaculada Garcia, Colin. P. Green and Maria Navarro

The Department of Economics
Lancaster University Management School
Lancaster LA1 4YX
UK

New Estimates of the Effect of Temporary Employment on Absenteeism

Inmaculada García*, Colin P. Green** and María Navarro**

* Department of Economic Analysis, University of Zaragoza

** Department of Economics, Lancaster University

Abstract

Restrictive employment protection legislation has been highlighted as one of the key reasons for lower labour productivity in Europe compared to the US. A variety of channels have been suggested, including recently a focus on the direct effect of employment protection on worker effort decisions. This paper uses longitudinal data from Spain and legislative changes aimed at reducing the incidence of temporary employment to estimate the causal effect of employment protection on one aspect of effort, absenteeism. We demonstrate a large effect of employment protection on absenteeism, one that is markedly larger than that identified in previous studies. We then use this information to estimate the cost of this absence in terms of lost productivity. Our results suggest that cross-countries differences in employment protection can, through its effect on worker effort, have a substantial impact on labour productivity.

JEL codes: J22, J38
Keywords: Absenteeism, Temporary Employment, Labour Productivity

Acknowledgements: The authors would like to thank Paul Devereux, Juan Ramon Garcia, John Heywood, Owen O'Donnell and seminar participants at WPEG and EALE 2012 for helpful comments. Maria Navarro gratefully acknowledges financial support from the LUMS Pump-priming Grant ECA6364. Correspondence to: Colin Green, Lancaster University Management School, Lancaster, LA1 4YX, UK. E-mail: c.p.green@lancs.ac.uk.

I. INTRODUCTION

Differences in labour productivity between Europe and the US are an issue of long-standing concern (Prescott, 2004; Timmer and van Ark, 2005 and van Ark *et al*., 2008). A range of explanations have been posited to explain these differences. For instance, it has been suggested that the `knowledge economy' emerged much more slowly in Europe when compared to the US (Aghion *et al*., 2004; van Ark *et al*., 2008). Another explanation, and a highly contentious one, is that markedly more strict employment protection legislation (EPL) in Europe leads to lower labour productivity (Bassanini *et al*., 2009; Dolado and Stucchi, 2008; Capellari *et al*., 2012).

EPL has the potential to affect productivity through its influence on labour market transitions and unemployment; this has been the source of a large and contentious literature (Lazear, 1990; Addison *et al*., 2000; Blanchard and Portugal, 2001; Acemoglu and Angrist, 2001 and Kugler and Pica, 2008). EPL could also influence worker productivity more directly through its effects on worker effort decisions (Dolado and Stucchi, 2008 and Dolado *et al*., 2011). Recently, a literature has developed that seeks to estimate the effect of employment protection legislation on worker effort, focusing primarily on absenteeism (Riphahn and Thalmaier, 2001; Ichino and Riphahn, 2004 and 2005; Engellandt and Riphahn, 2005; Olsson, 2009, Scoppa, 2010 and Bradley *et al*., 2012). This literature can be summarised as showing statistically significant effects of employment protection on absenteeism; more employment protection increases absenteeism. While these papers compare worker behaviour across large variations in employment protection, it must be recognised that the economic magnitude of these estimated effects are quite small. For instance, Ichino and Riphahn (2005) show that Italian bank workers who pass a 12 week probation period increase absence by 0.04 of a day per week, while Bradley *et al*., 2012 show that public sector workers who move from temporary to permanent contracts increase absence by 0.017 of a day per week. These effects appear too small to lead to large cross-country differences in labour productivity. Other studies find larger effects but in quite specific small firm settings. Olsson (2009) finds a decrease in sickness absence of 13% among firms of less than 10 workers in Sweden who experienced a reduction in employment protection strength. While, Scoppa (2010) examine the effect of an increase in dismissal costs in small Italian firms and find an increase in sickness absence of 18%. In this paper we revisit this issue and provide new evidence on EPL effects on worker absence across an entire labour market relying on legislative changes in Spain.

In the European context the key variation in employment protection relates to differences between temporary / fixed term contracts and permanent contracts. Temporary contracted workers are relatively easy to fire or face non-renewal of contract while the dismissal of permanent workers is extremely difficult and in the case of success may still impose large costs on the employer. Spain is a particularly interesting case as it is known both for very stringent employment protection legislation for permanent employees and at the same time has the highest incidence of temporary employment contracts in the EU. More than 30% of the Spanish workforce over the 1990-2007 period was employed on a fixed-term contract (Bentolila *et al*., 2008). Moreover, this can be viewed as essentially a segmented labour market as transitions from temporary to permanent contracts are quite uncommon. For instance, there is no evidence that these temporary contracts are associated with a higher likelihood of transition into permanent employment when compared to being unemployed (Güell and Petrongolo, 2007).

The temptation in this context is to compare permanent and temporary workers to provide an estimate of the effect of employment protection on absenteeism. However, the identification of the effect of temporary employment on workers' effort is complicated by a number of issues related to non-random selection of workers into employment contracts. First, temporary jobs are not randomly distributed across the labour market and are, for instance, concentrated amongst the young, women, immigrants and the less skilled (Kahn, 2007). Moreover temporary jobs are characterised by features that may make them on average less desirable, such as lower levels of work-related training (Arulampalam and Booth, 1998; Draca and Green, 2004), lower wages (Booth *et al*., 2002), poorer working conditions, and increased risk of work-related accidents (Guadalupe, 2003). A further complication is that these contracts could be used for specific reasons that may condition effort choices. For instance, they may be used by employers as a buffer against cyclical fluctuations and/or for screening purposes. In the latter case, under imperfect information regarding their productivity, this may lead temporary workers to increase their effort so as to augment their probability of being retained by the firm. Along these lines, Booth *et al*., (2002) find evidence that temporary jobs are stepping stones to permanent work for female workers in the UK. While, Green and Leeves (2004) provide evidence that some Australian employers use temporary workers as part of a screening process.

Together this suggests a variety of common unobserved factors that influence both the probability of being hired on a temporary contract and effort choices by workers. In particular, there may exist important omitted individual characteristics, such as motivation and ability, which lower the opportunity cost of being hired on a temporary basis. This gives rise to an endogeneity problem that leads to biased and inconsistent estimates of the causal effect of temporary employment on absenteeism. Fixed effects models which take into account worker level unobserved heterogeneity and/or instrumental variable approaches are two potential solutions to this problem. Each in isolation is open to criticism. Fixed effect approaches control for time-invariant characteristics associated with both effort and employment contract but cannot control for time-varying factors that cause worker assignment to permanent contracts. IV estimation relies on plausible exogenous assignment to contract type that is a function of a suitable instrument. Our paper uses a longitudinal representative data for Spain and recent policy changes that allow us to use both of these approaches in combination to estimate the effect of temporary employment, and through this employment protection, on worker absence.

Specifically, this paper investigates the effect of temporary contracts on workers' effort by adopting an IV Fixed Effects framework that exploits two employment protection reforms as a source of exogenous variation in temporary employment and then estimates within worker variation in absence. Previous research has already shown that these types of reforms affect the likelihood of being employed on a temporary contract. Kahn (2010) finds an effect of employment protection reforms on the incidence of temporary jobs for the countries of the EU-15 from 1996 to 2001. Dolado *et al*., (2011) use the 1994, 1997 and 2002 reforms to identify the exogenous variation in the share of temporary workers within firms so as to evaluate the impact of the extended use of temporary contracts on the productivity of Spanish manufacturing firms. Kugler *et al*., (2005) demonstrates an increase in permanent employment for workers under 30 and over 45 years old with a difference in difference analysis for a 1997 reform in Spain. Spain presents the ideal scenario to analyse the effect of temporary employment on absence. This is due to the large scale of temporary work in Spain and the sequence of reforms that have been introduced in the last decade to reduce the use of these contracts. Using this setting, we extend earlier research (Ichino and Riphahn, 2005;

Olsson 2009; Scoppa 2011) to cover general reforms that affect all workers and as a result can be viewed as providing more readily generalisable estimates of employment protection effects.

We find an effect of temporary employment on absence that is in the range of 0.3 of a day per week, which we argue is causal. This estimate is substantially larger than that reported in the previous literature (Ichino and Riphahn, 2005; Olsson, 2009; Scoppa, 2011; Bradley *et al*., 2012) and we argue that it is of an economically significant magnitude. Our institutional setting covers an entire labour market, where temporary workers may be employed in a variety of occupational and industry settings. We demonstrate that our primary source of variation, reforms to employment protection, influence the assignment of workers to types of employment contracts throughout the labour market. Our results add to previous research by providing causal evidence of employment protection effect on absence in a broader economic and labour market setting. Finally, using a particular subset of workers, young unskilled construction workers, we provide the first estimates of productivity effects associated with absenteeism and contract re-assignment.

II. INSTITUTIONAL BACKGROUND AND DATA

Institutional background

Spain’s labour market is characterised by high unemployment and high rates of temporary employment; the latter is the highest in Europe (14% and 33% in the EU-27 and Spain in 2005, respectively). Figure 1 demonstrates the evolution of the rate of temporary employment in Spain and the countries of the EU-15 and EU-27 (See Dolado *et al*., (2002) for a survey on temporary employment in Spain).

INSERT FIGURE 1

Temporary contracts in their modern form were introduced in Spain in 1984 as an attempt to combat high unemployment rates. Specifically, a contract (*Contrato temporal de fomento de empleo*) was created under which individuals could be hired for a maximum duration of three years, with no constraints. Temporary contracts did exist prior to this (from 1976 on) but were only allowed in jobs that were temporary in nature, such as seasonal work. The 1984 legislative change removed this link between temporary work and job type. This reform led to a large and rapid increase in the use of temporary contracts. While there was no necessary link between job and contract type, in reality the growth in temporary contracts was concentrated in what could be considered "lower quality" jobs. Dismissal of temporary contract workers was relatively easy with no dismissal compensation. In contrast, permanent workers remained very hard to dismiss. Permanent workers must receive 30 days notice of dismissal. If the dismissal is due to objectively bad conduct of the worker there is no further compensation. However, if the dismissal is due to poor demand conditions of the firm, dismissed workers receive 20 days full pay per year worked with the firm up to a maximum of 12 months full pay. If the dismissal is for any other reasons, compensation rises to 45 days per year worked with a maximum of 42 months full pay. In practice, most dismissals are not for the first two reasons and maximum compensation is paid. Moreover, if a dismissal was contested by the worker for being unfair and subsequently judged against the firm (i.e. unfair dismissal) they had to pay full wages for the period between dismissal and trial. This coupled with the relatively slow time to trial in Spain implies a large additional dismissal cost. Together, this helps to explain why temporary contracts proliferated once they were introduced.

The scale of the temporary employment sector generated by the original reform was eventually viewed as undesirable and has led to a number of subsequent reforms aimed at reducing the level of temporary employment, and reducing the size of the disparity between permanent and temporary contract conditions. Of specific interest is the series of reforms that began in 1997. In 1997 a new type of permanent contract was created (*Contrato de Fomento de la Contratacion Indefinida*, CFCI). The key characteristics were pay tax breaks for 2 years for firms to hire a worker on this type of contract, and lower potential dismissal costs than standard permanent contracts. The lower dismissal costs of CFCI workers primarily took the form of a lower level of compensation of 33 days full pay per year worked (as compared to 45 for other permanent workers) and a reduction in the maximum period of full pay from 42 to 24 months. Only certain groups of workers could be offered these contracts, 18-29 year olds, workers older than 45 years, long-term unemployed (at least one year), disabled workers and workers currently on temporary contracts within the firm. The eligible groups were subsequently expanded in 2001 to also include 16 and 17 year olds, unemployed women between 16 and 45 years old, those unemployed for at least 6 months and women in sectors where they were under-represented. Another key change that reduced the `distance' between contract types was that temporary contract workers were now eligible for dismissal compensation of 8 days in some cases.

Further reforms on the 14th of December 2002, extended this contract to women who gave birth in the last 24 months, but also introduced `express dismissal' (*despido express*). This lowered dismissal costs of all permanent workers markedly as the employer did not have to pay processing wages if they accepted that the dismissal was unfair within 48 hours, but still had to pay dismissal compensation. Finally in 2006 (active from the 31st December 2006) these CFCIs were extended to cover essentially all workers. In addition, the tax breaks for hiring these workers were changed from a proportion of the wage to a specific amount, which favours the hiring of lower wage workers on these contracts. The period of payment of these breaks was also extended from 2 to 4 years.

In terms of the impact of these reforms on the Spanish labour market, previous research demonstrates that the reforms of the 1990s had a very limited effect on the use of employment contracts in Spain (see for instance Kugler *et al.*, 2005; García Perez and Rebollo, 2009a and 2009b and Mendez, 2012). By the mid-2000s, the share of temporary jobs remained very high and the conversion rate into open-ended contracts remained low and stable at around 4% of the total number of contracts (Bentolila *et al.*, 2008). Since the approval of the 2006 reform, there has been a substantial reduction in the temporary employment rate but part of the reduction can be attributed to the large destruction of temporary jobs in the construction industry. However, we will demonstrate that the 2002 and 2006 reforms have an effect on the temporary mechanism in Spain and this constitutes a key part of our identification strategy.[1]

Data

The data we use in this paper are drawn from the Longitudinal Spanish Labour Force Survey (herein LSLFS). The LSLFS is a quarterly representative survey that provides a range of information on individual and work characteristics. It is a rotating panel data set that follows individuals for 6 consecutive quarters. It contains 1,430,599 observations for 506,252 different individuals in the period spanning 1st quarter of 2002 to 4th quarter of 2008.[2] The chosen period is crucial for identification purposes since it contains several labour market reforms and both the period and the reforms are not associated with any major economic

[1] In unreported estimates we found no effect of the 2001 reform on the probability of being hired on a permanent contract. Previous research has considered the 2001 and 2002 reforms jointly (Bentolila *et al.*, 2008).

[2] The Spanish Labour Force Survey has been demonstrated to have an internationally consistent definition of absence (Barmby *et al*., 2002).

crisis.[3] This ensures that the decrease in temporary employment is not due to a general drop in labour demand and employment levels.

INSERT FIGURE 2a and 2b

Information on usual and actual hours of work per week forms the basis of our measures of both the extensive and intensive margins of absence. The extensive margin is a variable that takes value 1 when usual hours are greater than actual hours and value 0 if they are the same.[4] The intensive margins of absence are calculated as deviations from contractual hours either in absolute terms or as an absence rate. We calculate the hours a worker is absent per week as the difference between usual and actual hours, $A_{it} = H_{it}^{u} - H_{it}^{e}$.[5] For ease of interpretation we multiply this number by 60 so that the estimated coefficients are in terms of minutes of absence. The absence rate is defined as the ratio of the hours reported absent to contractual hours in the reference week $AR_{it} = A_{it} / H_{it}^{u}$. This approach has previously been used by Barmby *et al*., (1999 and 2002) for the UK, eight European Countries and Canada, Lozano (2011) for the US and Green and Navarro (2012) for Spain.

Figure 2 uses this definition of minutes difference due to absence to provide preliminary evidence on the association between absence and contract type. To aid presentation we plot these as 5 year moving averages. Two things are notable. First there is a marked and stable difference over time in the absence levels of temporary and permanent contract employees. For instance, temporary workers are, on average, absent from work 90 minutes less a week than permanent workers. Second, overall absence levels for both types of workers are high. This in part reflects the choice to differences in usual and actual hours due to all types of absence such as personal/family responsibilities, paternity leave, bad weather, summer schedule/flexible hours, holidays, local or national bank holidays, sickness, training, employment change, strike, union representation and "other reasons". That is, we use information on the difference between usual hours and actual hours for those that report the reason of any difference between them as due to sickness to generate our narrower dependent

[3] We stop our sample just after the fall of Lehman Brothers which could be considered as the start of the financial crisis.

[4] In the results we examine the case of overtime work which may also be influenced by employment contracts.

[5] Where usual hours is taken as synonymous to contractual hours.

variables. However, we stress that our main results are robust to using narrower definitions of absence.

We focus on employees and Table A1 presents summary statistics on the variables used in the empirical analysis. These include dummies for each of the age brackets, gender, marital status and education level. Also included are controls for the worker's type of contract and whether the individual works in the public sector, along with tenure, tenure squared, industry and occupation dummies. Finally, in all the empirical modelling we use year, quarter and regional fixed effects so as to take regional, seasonal, and time variations into account.

III. METHODOLOGY AND IDENTIFICATION

Our identification approach draws upon Kahn (2010) who demonstrates the effect of European employment protection reforms on the incidence of temporary work contracts. Specifically, we use the 2002 and 2006 employment protection reforms as a source of exogenous variation in the assignment of contract type.[6] The standard identifying assumption is that the chosen instrumental variables, employment protection legislation (EPL) reforms, are relevant and validly excluded. The relevance condition requires that there is a correlation between these reforms Z and the likelihood of temporary employment $E[Z, TC_{it}] \neq 0$. With respect to the validity condition, our assumption is that reforms affect workers absence behaviour only through its effect on temporary contracts but not directly $E[Z, \varepsilon_{it}] = 0$. An argument in support of this assumption is that government policy aimed at introducing temporary contracts and reducing dismissal costs has had the primary objective of fighting high unemployment. Although workers absence leads to significant costs for firms, and in some countries like Spain absence levels are very high, governments have generally ignored this aspect when designing employment protection reforms.[7] Furthermore, it has been shown that there is no effect of these EPL reforms on the overall employment level in Spain (Kahn, 2010). In the results section we further discuss the statistical validity of our instruments.

[6] Similarly, Dolado *et al*., (2011) use the 1994, 1997 and 2001/2002 Spanish labour market reforms to identify the exogenous variation in the shares of temporary workers when they evaluate the impact of the extended use of temporary contracts on the productivity of Spanish manufacturing firms.

[7] The Labour Reform that occurred in February 2012 is the first reform in Spain that explicitly includes measures that aim to reduce work absenteeism.

We present OLS, Fixed Effects and IV estimates of the effects of temporary contracts on worker's absence behaviour based on variants of the following specification:

$$A_{it} = \alpha_i + \gamma TC_{it} + \eta X_{it} + \varepsilon_{it}, \qquad (1)$$

$$TC_{it} = \mu_i + \theta R2002 + \pi R2006 + \lambda X_{it} + v_{it} \qquad (2)$$

Equation (1) provides the effect of being employed on a temporary contract, TC_{it}, on workers' absence behaviour, A_{it}. TC_{it} is a dummy variable which takes value 0 if the worker is hired under a permanent contract and 1 under a temporary one. Our dependent variable, A_{it} is measured either at the extensive or the intensive margin of absence (absence incidence *versus* minutes difference or absence rate).

Equation (2) provides the relationship between the probability of being employed on a temporary contract to the reforms of 2002 and 2006. R2002 and R2006 are indicators that take the value of unity if the worker is observed during the reform period and zero otherwise (from the 14th of December 2002 and 31st of December 2006, respectively). θ and γ provide the percentage effect of those reforms on reductions in the probability of temporary employment in Spain. X is a set of standard control variables such as age dummies, gender, marital status, education, sector of employment, tenure and tenure squared, year, industry, occupation, regional and quarter dummies.

We then further extend our analysis to utilise the longitudinal nature of our data by estimating a variant of the IV model above including worker fixed effects (FE-IV). This model identifies the within worker change in absence behaviour due to re-assignment of contract type that results from the legislative change.

IV. RESULTS

Table 1 reports regression estimates of the effect of being employed on a temporary contract on absence. We use a simple Linear Probability Model (LPM) for the extensive margins of absence and OLS estimates for the intensive margins, that is, both for the minutes difference and the absence rate variables/models.[8] Our findings provide evidence that temporary workers exert more effort as they are absent 43 minutes less on average than

[8] The sign, magnitude and significance of our estimates were unaffected when we estimated the absence rate with a Tobit model that takes censoring into account.

permanent workers (-0.016 at the extensive margin and -0.016 for the absence rate). These differences are substantially lower than that apparent in the raw data. This suggests that permanent contract workers have 'unfavourable' observable characteristics in terms of absence propensity. Once these characteristics are held constant the contract absence gap narrows substantially. This is perhaps not surprising as permanent workers are on average older, more likely to be in the public sector and have longer tenure: all known positive correlates of absenteeism. This is supported by the fact that excluding these controls leads to a markedly larger estimate temporary contract effect on absence. For instance, the estimate of temporary contract effects on minutes difference increases to 63.

INSERT TABLE 1

These marked observable differences lead naturally to a concern regarding the comparability of the treatment and control group. Permanent and temporary contracts are not randomly distributed across the Spanish labour market and this leads to concerns about the comparability of these two groups absences. We seek to address this primarily by examining the robustness of our results to the exclusion of groups where temporary work is likely to be very uncommon. For instance, temporary contracts are heavily concentrated in short tenure jobs. Hence, we remove workers with 5 or more years' tenure. They are also heavily concentrated in the private sector. This is not surprising as in the public sector the selection criteria for new hires does not include tax breaks for hiring temporary workers. As a result, we also exclude public sector workers from the following estimates. [9] The absence behaviour of the permanent and temporary workers in this short tenure private sector subsample are closer than in the overall sample. Temporary workers are absent 177.05 minutes, while permanent workers are absent 219.49 minutes. The corresponding figures for the absence rate are 8.38% and 10.06%, respectively.

Table 2 reports the OLS and FE estimates for this selected sample. When compared to the earlier results, these exclusions lead to a marked decrease in our contract estimate from 43 to 19 minutes of general absence, and -0.016 to -0.008 and -0.016 to -0.005 in the extensive margin and absence rate, respectively. Nonetheless, even in this more homogenous group, we observe a statistically significant conditional relationship between contract type and absence.

[9] In unreported estimates we also use the approach set out by Caliendo and Kopeinig (2008) to remove all observations not in the common support region after estimating a matching model of contract type. While this necessarily leads to a reduction in the sample size, the OLS and FE estimates of contract effects on absence are essentially unchanged following this procedure.

In the next column we exploit the longitudinal nature of the data and introduce worker level fixed effects. When we take into account time invariant worker heterogeneity the coefficient falls further to 15 minutes. This could be viewed as a lower bound estimate if we believed there was measurement error in the contract type variable and hence attenuation bias (Angrist and Pischke, 2009, pp. 225-226). However, given the stark differences in employment protection between contract types in Spain it seems quite unlikely that workers are unaware of which contract they are employed under.

A concern with our measures of absence is that for workers who experience variations in their contractual hours, such as part time workers, there may be substantial measurement error in our absence measures.[10] Furthermore, part-time workers are more likely to be employed on temporary contracts. As a result, in the next two columns we report OLS and FE estimates for full-time workers only. While, this leads to some change in point estimates, these are not particularly marked. In unreported estimates we take this a step further and limit our sample to those workers who were observed in all 6 periods in a full time job. Re-estimating our FE models on this balanced panel of full-time workers revealed an essentially unchanged set of contract estimates.

INSERT TABLE 2

To accept the current results as causal would require an assumption that contract status was randomly assigned in the OLS conditional on observed characteristics; and in the FE context that they are randomly assigned conditional on observed characteristics and time invariant unobserved characteristics. For a number of reasons discussed earlier this is unlikely to hold. As a result we seek to address this problem in a number of ways. First we seek to take advantage of the labour reforms of 2002 and 2006 as a source of exogenous variation in the likelihood of being hired on a temporary contract. Panel A of Table 3 provides LPM estimates of the probability of being on a temporary employment contract with the key focus being the estimates of the effects of the reforms.[11] These demonstrate that these reforms appear to have had a substantial effect in decreasing the likelihood of being employed on a temporary basis. In particular, the 2002 reform that came into force the 14th of

[10] It is worth noting however that our estimates based on absence rate implicitly controls for variation in contractual hours.

[11] In unreported estimates, we ran the absence incidence model as a probit and the marginal effects were essentially identical in terms of magnitude and statistical significance to those obtained via LPM.

December 2002 decreased the probability of being on a fixed-term contract by 6 percent. The corresponding figure for the 2006 reform is a decrease by 2 percent of the probability of being on a fixed-term contract. The reforms appear to be suitable instruments as they are statistically strong with an F-test value for the exclusion restriction of 518.04. In addition, we cannot reject the assumption that our reforms are unrelated to our dependent variable, that is, absenteeism at the 1% level. Together this suggests that the reforms provide strong and valid instruments.

INSERT TABLE 3

The IV estimates indicate that temporary workers are absent 144 minutes less than permanent workers. This is markedly higher than the 19 minutes for the OLS estimates, but closer to the magnitude of contract variation in absence seen in the raw data of 90 minutes. This suggests that the OLS and FE estimates substantially understate the effect of contract status, and hence employment protection, on worker absence behaviour.

We then further extend this to incorporate worker time invariant unobservables that may be correlated with both contract re-assignment and absence behaviour. We do this by introducing the IV estimation in a worker fixed effects context (FE-IV). Estimates of these models are reported in the final two columns of Table 3. The first thing to note is that while both the policy reforms remain strong predictors of contract type in a fixed effects context, the relative magnitude of the two reforms has changed. Specifically, the 2002 reform is now associated with a lower, but still substantial, decrease in the likelihood of being on a temporary contract of 3.8 percentage points. The 2006 reform is now associated with a much larger decrease in temporary contract probability of 7.2 percentage points. These estimates, especially the 2006 effect seems more in line with what the raw data suggested as presented in Figure 1. The bottom panel provides the resultant FE-IV estimate. While this approach leads to a large loss of precision, evidenced by the near tripling of the standard errors compared to the IV model, we still find a statistically significant effect of contract type on absenteeism. This is roughly 3 hours a week and statistically significant at the 10% level. While imprecise this indicates that re-assignment to permanent contract by these policies had a marked increase in within worker absence behaviour.

Taken together, our results suggest that the previous OLS and FE estimates of temporary contract effects on absence dramatically understated the effect of employment protection.[12]

The Economic Magnitude of Contract Effects on Absence – An Illustrative Example

To this point, we have demonstrated that a robust contract effect on worker absence; and one that is markedly larger than previously reported in the literature. What is missing here, and from the literature in general, is an idea of the economic magnitude of these effects. As a result, how much these differences in estimated absence effects matters. Specifically, we are interested in the effect of the re-assignment of workers from temporary to permanent contracts on worker productivity. One way of doing this would be to examine the effects of absence on wages as below:

$$\log W_{it} = \tau_i + \beta A_{it} + \delta X_{it} + \upsilon_{it} \qquad (3)$$

$$A_{it} = \alpha_i + \gamma TC_{it} + \eta X_{it} + \varepsilon_{it} \qquad (4)$$

where the key parameter of interest is β, which provides the effect of absence on wages. This model is identified by an exclusion restriction, here that temporary contracts have no, conditional on observables, direct effect on wages/productivity.

One initial complication is that there is no wage information in the LSLFS, hence there is no way of directly linking contract re-assignment, absence and wages. To get around this problem we exploit a further representative data set for Spain, the Wage Structure Survey (WSS) of 2002 and 2006. This provides information on, amongst other things, individual wages and type of contract (but not absence). Provided that the underlying populations are equivalent and the sample moments are independent these two data sets can be combined and the absence effect of temporary workers reassignment to permanent contracts on wages can be estimated via two samples two stage least squares (Angrist and Krueger, 1992) such that:

[12] The focus on absence may seem to miss one key component of worker effort choices, overtime work (Engellandt and Riphahn, 2005). In unreported estimates we also examined the effect of permanent work on all variations in working hours (i.e. including positive variations from usual hours as well as absence). As might be expected this increases our permanent contract effects (i.e. temporary workers are also more likely to work overtime), but only slightly.

$$\log W_{it} = \tau_i + \rho TC_{it} + \delta X_{it} + \upsilon_{it} \quad (5)$$

$$A_{it} = \alpha_i + \gamma TC_{it} + \eta X_{it} + \varepsilon_{it} \quad (6)$$

To implement this approach we first estimate the effect of being employed on a temporary contract on wages (5) using the WSS data, where W_{it} is the logarithm of the wage per hour of individual i at time t. TC_{it} is a dummy that takes value 1 if the individual is employed on a temporary contract and 0 if on a permanent contract. X_{it} represents a set of standard control variables.

Second, we estimate the effect of temporary employment on absenteeism (6) where, for ease of estimation and interpretation, we use the minutes difference absence indicator (A_{it}) which provides the number of minutes that individual *i* is absent at time *t*. TC_{it} is a dummy that takes value 1 if the individual is employed on a temporary contract and 0 if on a permanent one. X_{it} represents age, education, gender and labour market characteristics such as whether the individual works in the public sector, tenure, occupation, sectoral, regional and year dummies.

The ratio of the two estimated coefficients $\frac{\hat{\rho}}{\hat{\gamma}}$ is identical to the 2SLS β coefficient in Equation (3) for the exactly identified case where we have as many instruments (temporary contract) as potential endogenous variables (absenteeism). This will give us a LATE estimate of absenteeism on wages for those workers that change employment contract (are reassigned from a temporary to a permanent job). Given that our estimate is a non linear combination of estimators we apply the delta method to compute the standard errors (Van Kippersluis et al., 2011 and Devereux and Hart, 2010).

In equilibrium temporary workers, who work harder, should earn more. However, in practice because contract assignment is non-random (i.e. more able workers are likely to be offered permanent contracts) and may have a dynamic influence on productivity (for instance temporary workers are offered less training, see Arulampalam and Booth, 1998 and Draca and Green, 2004) temporary workers are generally observed to earn less (Booth et al, 2002).[13] This is not surprising as it is these particular types of issues which have motivated our

[13] In the WSS full sample permanent workers earn on average 13.5 Euros an hour, while temporary workers earn only 9.42 Euros per hour.

identification strategy in the earlier parts of the paper. However, due to the lack of longitudinal element to the WSS we do not have the same identification options available. As a result, we adopt a different strategy which is to look for a group of workers where training opportunities are relatively unimportant and where there should not be marked variation in underlying individual productivity. Specifically, we focus on young (<30 years) unskilled (no post-compulsory education) workers in the construction industry. This is a large group of workers in the time frame of our data which was characterised by quite extensive use of temporary contracts. For instance we have 89,484 person-time observations of these workers in the LSLFS and 62% are temporary contracted workers. The absence differentials between contract types for this group is similar to our private sector, short tenure sample; minutes difference equals 234.52 for permanent contract workers and 197.84 for temporary contract workers.

INSERT TABLE 4

The resultant estimates of (5) and (6) are reported in Table 4. The first panel shows a conditional wage premium to be on a temporary contract of roughly 2.87% for our construction sample. In the lower panel we report temporary contract effects on absence. The first are those (OLS and IV) from our full sample as per Tables 2 and 3. The second are corresponding estimates for the young, unskilled construction worker sample. These latter estimates again reveal a relationship between contract type and absence in the order of 13 minutes (OLS) and 211 minutes (IV), although the subsequent computed $\hat{\beta}$ suffer from a loss of precision.[14] With this information we can generate a range of estimates of absence effects on wages. These are reported as $\hat{\beta}$ with corresponding standard errors. These point estimates provide the effect of one minute's absence on wages. For instance, the subsample OLS estimates would suggest productivity losses of 0.22% for a minute's absence, or scaled up to 13.44% for an hour's absence. If instead, we use the IV estimates of contract effects on absence this provide a substantially lower productivity loss from re-assignment from temporary to permanent contract of 0.014% for a minutes absence, or 0.84% for an hour's absence. The magnitudes of these estimates are similar if we use the more precise full-sample temporary contract on absence coefficients.

[14] We do not report IV-FE estimates as it proves impossible to gain precise estimates with our smaller sub-sample.

V. CONCLUSION

The strong employment protection found in many areas of Europe has the potential to dramatically influence labour market performance and outcomes. For instance, a large literature has demonstrated the effects of these regimes on labour productivity and unemployment. Less well understood is employment protections' behavioural effect on workers. A recent literature has developed examining how employment protection conditions worker effort. This literature has shown an effect of employment protection on absenteeism, but one that appears too small in magnitude to generate large differences in labour productivity. In practice, identifying causal effects of employment protection on effort is difficult, especially in a broad labour market setting. Our paper used a combination of within worker estimation and legislative changes in Spain as a source of exogenous variation in contract type to identify this causal effect.

We demonstrate large effects of temporary contract on worker absence. For instance, our fixed effects IV estimates reveal that workers whose contracts are reassigned from temporary to permanent take 0.3 day more absence per week. This finding is robust to a range of sub-samples, and attempts to address issues related to the appropriate control group. This evidence suggests that variations in employment protection can have a marked effect on worker effort, which in turn will influence cross-country labour productivity. For instance, Dolado *et al*., (2011) attribute 20% of the slowdown of TFP growth in Spanish manufacturing firms to the reduction in conversion rates. Finally, we provide estimates of the productivity cost of absence associated with different employment contracts for a specific subsample of workers. Future work, with more advantageous data, could expand this analysis of productivity losses to a more general setting.

References

Acemoglu, D. and Angrist, J. (2001). "Consequences of employment protection? The case of the Americans with Disabilities Act" *Journal of Political Economy*, 109, 915–57.

Addison, J., P. Teixera and J-L, Grosso (2000). "The Effect of Dismissals Protection on Employment: More on a Vexed Theme" *Southern Economic Journal*, 67(1), 105-122.

Aghion, P., Blundell, R., Griffith, R., Howitt, P., and S. Prantl (2004). "Entry and Productivity Growth: Evidence from Microlevel Panel Data" *Journal of the European Economic Association* 2:2–3, 265–276.

Angrist Joshua and Alan Krueger (1992). "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples," *Journal of the American Statistical Association*, 87 (418), 328-336.

Angrist Joshua and Jörn-Steffen Pischke (2009). Mostly Harmless Econometrics, Princeton University Press.

Arulampalam, Wiji and Alison L. Booth (1998). "Training and Labour Market Flexibility: Is There a Trade-off?" *British Journal of Industrial Relations*, 36 (4), 521–536.

Barmby, T., Ercolani, M. and J. Treble (1999). "Sickness absence in Great Britain: new quarterly and annual series from the GHS and LFS, 1971-1997" *Labour Market Trend*s, August, 405-415.

Barmby, T., Ercolani, M. and J. Treble (2002). "Sickness absence: An international comparison" *Economic Journal*, 112, F315-F331.

Bassanini, A., L. Nunziata and D. Venn (2009). "Job protection legislation and productivity growth in OECD countries", *Economic Policy*, 24, 349-402.

Bentolila, S., Dolado, J.J. and Jimeno, J.F. (2008). "Two-tier employment protection reforms: The Spanish experience" CESifo DICE Report 4/2008, 49-56. www.cesifo.de

Blanchard, Olivier and Pedro Portugal (2001). "What Hides behind an Unemployment Rate: Comparing Portuguese and U.S. Labor Markets" *American Economic Review* 91(1), 187-207.

Booth, Alison L., Francesconi, Marco and Jeff Frank (2002). "Temporary Jobs: Stepping Stones or Dead Ends?" *Economic Journal*, 112(480), F189-F213.

Bradley, S., Green, Colin., and Gareth Leeves (2012). "Employment Protection, Threat and Incentive Effects on Worker Absence," *forthcoming, British Journal of Industrial Relations.*

Caliendo, Marco., and Sabine Kopeinig (2008). “Some practical guidance for the implementation of propensity score matching” *Journal of Economic Surveys* 22(1), 31-72.

Cappellari, Lorenzo, Carlo Dell’Aringa and Marco Leonardi (2012). “Temporary Employment, Job Flows and Productivity: A Tale of two Reforms,” *Economic Journal* 122, F188-F215.

Devereux, Paul. J., and Robert A. Hart (2010). “Forced to be Rich? Returns to Compulsory Schooling in Britain” *Economic Journal*, 120(549), 1345–1364

Dolado, J.J. and Stucchi, R. (2008). “Do temporary contracts affect total factor productivity? Evidence from Spanish manufacturing firms” CEPR Discussion Papers, 7055.

Dolado, J.J., Garcia-Serrano, C., Jimeno, J.F., (2002). “Drawing lessons from the boom of temporary jobs in Spain” *Economic Journal* 112 (480), F270–F295.

Dolado, J.J., Ortigueira, S., and Stucchi, R. (2011). “Does dual employment protection affect TFP? Evidence from Spanish manufacturing firms,” Economics Working Papers 11-37 Universidad Carlos III.

Draca, Mirko and Colin Green (2004). “The Incidence and Intensity of Employer Funded Training: Australian Evidence on the Impact of Flexible Work,” *Scottish Journal of Political Economy*, 51(5), 609-625.

Engellandt, Axel and Regina T. Riphahn (2005). “Temporary Contracts and Employee Effort”, *Labour Economics*, 12, 281-299.

García Pérez José Ignacio and Yolanda Rebollo (2009a): "The use of permanent contracts across Spanish regions: do regional wage subsides work?, *Investigaciones Económicas*, XXXIII(1), 97-130.

García Pérez José Ignacio and Yolanda Rebollo (2009b): “Do wage subsidies affect the subsequent employment stability of permanent workers?: the case of Spain”, *Moneda y Crédito* 228, 65-103.

Green, Colin and Gareth Leeves (2004). “Casual Jobs and Internal Labour Markets,” *Manchester School*, 72 (5), 658-676.

Green, Colin and Navarro, Maria. (2012). “Does Raising the School Leaving Age Reduce Teacher Effort? Evidence from a Policy Experiment”, *Economic Inquiry*, 50 (4), 1018-1030.

Guadalupe, Maria (2003) “The hidden costs of fixed term contracts: the impact on work accidents” *Labour Economics*, 10 (3), 339– 357.

Güell, Maia and Petrongolo, Barbara (2007). “How binding are legal limits? Transitions from temporary to permanent work in Spain," *Labour Economics*, 14, 153-183.

Ichino, Andrea and Regina Riphahn (2004). “Absenteeism and Employment Protection: Three Case Studies.” *Swedish Economic Policy Review* 11. 95-114.

Ichino, Andrea and Regina Riphahn (2005). “The Effect of Employment Protection on Worker Effort: A Comparison of Worker Absenteeism During and After Probation,” *Journal of European Economic Association* 3, 120-143.

Kahn, Lawrence M. (2007). “The impact of employment protection mandates on demographic temporary employment patterns: international microeconomic evidence,” *Economic Journal*, 117, F333-F356.

Kahn, Lawrence M. (2010). “Employment protection reforms, employment and the incidence of temporary jobs in Europe: 1996-2001,” *Labour Economics*, 17, 1-15.

Kugler Adriana and Giovanni Pica (2008). “Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform,” *Labour Economics*, 15, 78–95.

Kugler, Adriana., Jimeno, Juan F., and Hernanz Virginia (2005). “Employment Consequences of Restrictive Permanent Contracts: Evidence from Spanish Labor Market Reforms,” Working Paper (JEEA).

Lazear, Edward P. (1990). “Job Security Provisions and Employment,” *The Quarterly Journal of Economics*, 105 (3), 699-726.

Lozano, Fernando A. (2011) “The Flexibility of the Workweek in the United States: Evidence from the FIFA World Cup.” *Economic Inquiry*, 49(2), 512-529.

Mendez, Ildefonso (2012) "Promoting Permanent Employment: Lessons from Spain," forthcoming, *Series*.

Olsson, Martin (2009). “Employment Protection and Absence”, *Labour Economics*, 16, 208-214.

Ortega, B. and A. J. Marchante (2010) “Temporary contracts and labour productivity in Spain: a pectoral analysis” *Journal of Productivity Analysis*, 34, 199-212.

Prescott, Edward, C (2004). “Why Do Americans Work So Much More than Europeans?” *Federal Reserve Bank of Minneapolis Quarterly Review*, 28 (1), 2-13.

Riphahn, Regina and Anja Thalmaier (2001) "Behavioral Effects of Probation Periods: An Analysis of Worker Absenteeism," *Journal of Economics and Statistics*, vol. 221(2), pages 179-201.

Scoppa, V. (2010). Shirking and Employment Protection Legislation: Evidence from a Natural Experiment. *Economics Letters*, 107: 276-280.

Timmer, Marcel P., and Bart van Ark (2005). “Does Information and Communication Technology Drive EU-US Productivity Growth Differentials?” *Oxford Economic Papers*, 57 (4), 693-716.

Van Ark, Bart., Mary O’Mahony., Marcel P. Timmer (2008). “The Productivity Gap between Europe and the United States: Trends and Causes,” *The Journal of Economic Perspectives*, 22(1), 25-44.

Van Kippersluis, Hans., O’Donnell, Owen., and Eddy van Doorslaer (2011). “Long-Run Returns to Education: Does Schooling Lead to an Extended Old Age?” *Journal of Human Resources*, 46(4), 695-721.

Figure 1. Temporary employees as percentage of the total number of employees in Spain, the EU-15 and EU-27 (SOURCE: EUROSTAT and Spanish Labour Force Survey)

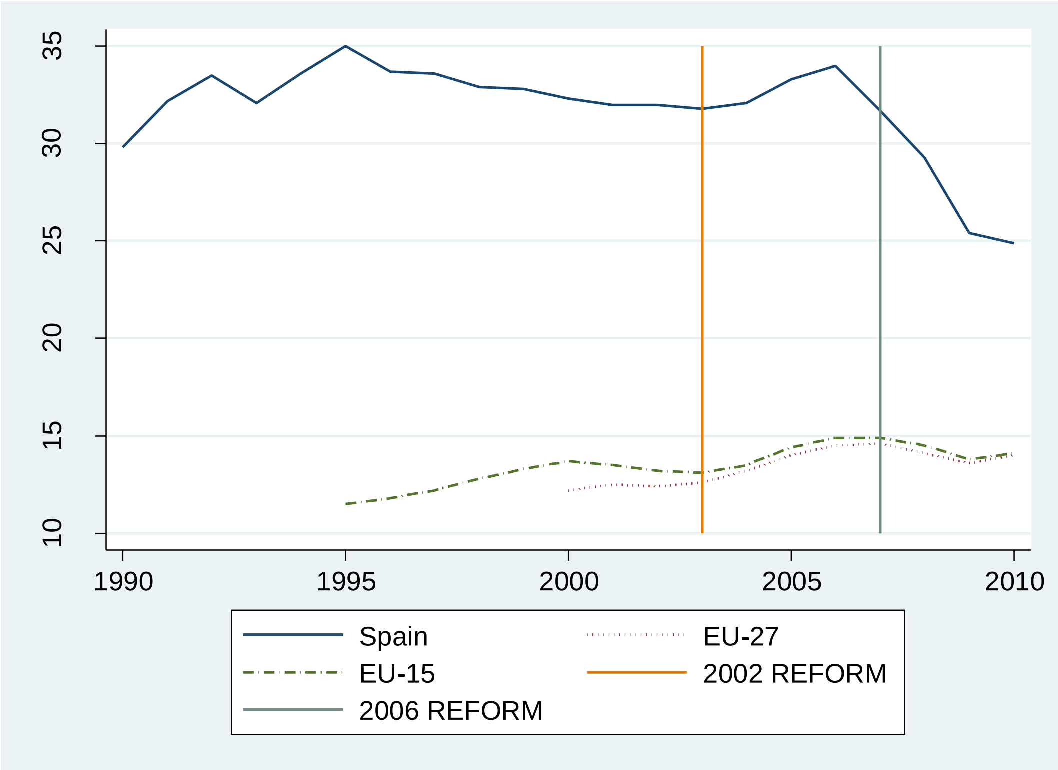

Figure 2a. Minutes Absence, Temporary and Permanent Workers in Spain, 1994 – 2010 (SOURCE: Spanish Labour Force Survey)

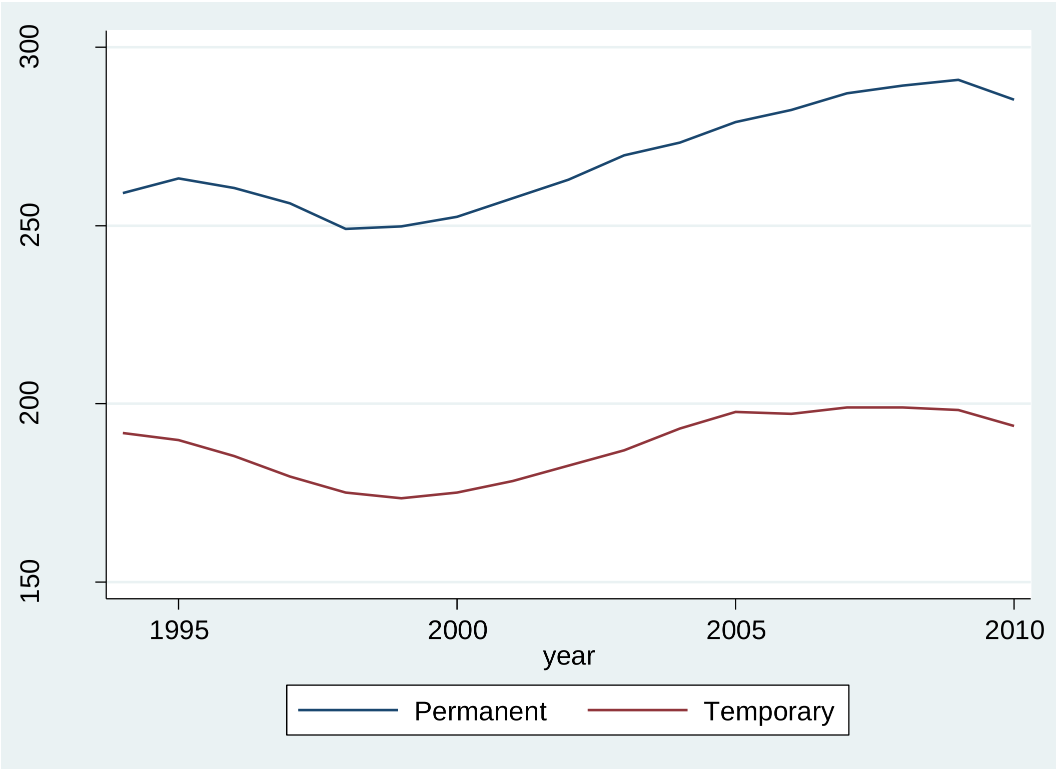

Figure 2b. Absence Rate, Temporary and Permanent Workers in Spain, 1994 – 2010 (SOURCE: Spanish Labour Force Survey)

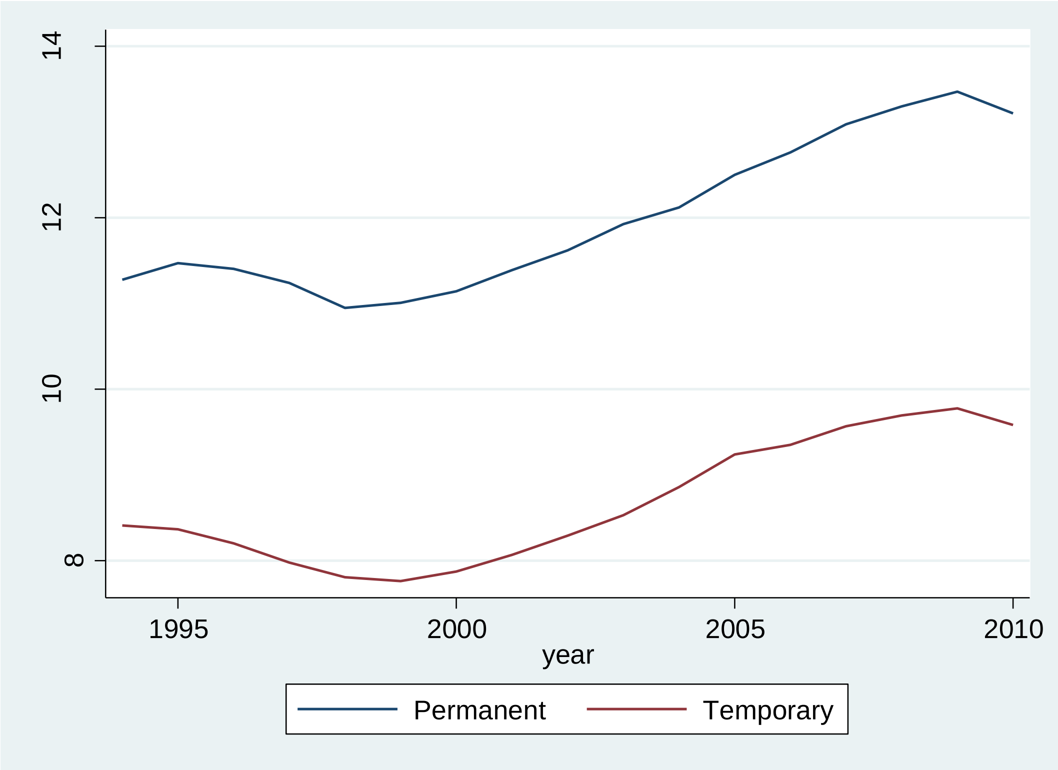

Table 1. Effect of temporary contracts on absence behaviour in Spain, 2002-2008

	General Absence		
	Ext margin	Intensive margin	
		Minutes difference	Absence rate
Temporary contract	-0.016	-43.218	-0.016
	(0.001)***	(1.377)***	(0.001)***
Female	0.031	49.560	0.031
	(0.001)***	(1.295)***	(0.001)***
Married	0.015	20.951	0.012
	(0.001)***	(1.380)***	(0.001)***
Second Education	0.000	-1.117	-0.001
	(0.001)	(1.482)	(0.001)
Higher Education	0.007	9.103	0.002
	(0.001)***	(1.703)***	(0.001)***
Age 16-19	0.015	14.829	-0.005
	(0.006)**	(9.491)	(0.005)
Age 20-24	0.014	20.363	-0.004
	(0.006)**	(9.219)**	(0.004)
Age 25-29	0.016	31.608	-0.002
	(0.006)***	(9.192)***	(0.004)
Age 30-34	0.025	50.441	0.006
	(0.006)***	(9.195)***	(0.004)
Age 35-39	0.021	39.843	0.003
	(0.006)***	(9.192)***	(0.004)
Age 40-44	0.012	20.321	-0.006
	(0.006)**	(9.175)**	(0.004)
Age 45-49	0.007	14.636	-0.009
	(0.006)	(9.158)	(0.004)**
Age 50-54	0.012	27.487	-0.002
	(0.006)**	(9.163)***	(0.004)
Age 55-59	0.025	54.196	0.012
	(0.006)***	(9.228)***	(0.004)***
Age 60-64	0.040	82.811	0.029
	(0.006)***	(9.593)***	(0.005)***
Public Sector	0.028	58.006	0.023
	(0.001)***	(2.365)***	(0.001)***
Tenure	0.000	0.498	0.000
	(0.000)***	(0.017)***	(0.000)***
$Tenure^2$	-0.000	-0.001	-0.000
	(0.000)***	(0.000)***	(0.000)***
Observations	1399824	1399824	1430511

Note: Controls for industry, workers' occupation, quarter, year and region are included but not reported. Robust standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, the 5%, and the 1% levels, respectively.

Table 2. Effect of temporary contracts on absence behaviour in Spain, OLS and Fixed Effects Estimates 2002-2008, private sector workers with $\leq$ 5years tenure.

	All Workers		Full Time Only	
	OLS	FE	OLS	FE
Extensive Margin	-0.008 (0.001)***	-0.013 (0.002)***	-0.007 (0.001)***	-0.017 (0.003)***
Minutes difference	-18.926 (1.691)***	-14.966 (3.200)***	-13.034 (1.982)***	-16.074 (3.845)***
Absence rate	-0.005 (0.001)***	-0.006 (0.001)***	-0.005 (0.001)***	-0.007 (0.002)***
Observations	625295	625295	527985	527985

Note: All controls as per Table 1. Robust standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, the 5%, and the 1% levels, respectively.

Table 3. OLS, Fixed effects and IV estimates of the effect of temporary contracts on absence behaviour in Spain, 2002-2008. Private sector workers with ≤ 5years tenure.

Panel A. First Stage: Probability of Temporary Contract				
	Pool		Fixed effects	
	OLS	IV	FE	FE-IV
2002 Reform	-0.060***		-0.038***	
	(0.002)		(0.002)	
2006 Reform	-0.020***		-0.072**	
	(0.002)		(0.037)	
Panel B. Second Stage: Effect of Temporary Contract on absence				
	Pool		Fixed effects	
	OLS	IV	FE	FE-IV
Extensive Margin	-0.007	-0.011	-0.011	-0.436
	(0.001)***	(0.030)	(0.002)***	(0.084)***
Minutes difference	-19.798	-144.343	-12.152	-187.524
	(1.620)***	(41.760)***	(3.048)***	(113.965)*
Absence rate	-0.006	-0.074	-0.004	-0.098
	(0.001)***	(0.018)***	(0.001)***	(0.051)*
Observations	726655	726655	726655	726655

Note: All controls as per Table 1. Robust standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, the 5%, and the 1% levels, respectively.

Table 4. Estimates of the effect of absence on productivity

SAMPLE 1 – EES	$\hat{\rho}$	$V(\hat{\rho})$		
$\log W_{it} = \tau_i + \rho TC_{it} + \delta X_{it} + \upsilon_{it}$	0.028728	0.000254		
SAMPLE 2 – LSLFS	$\hat{\gamma}$	$V(\hat{\gamma})$	$\hat{\beta}$	S.E.
$A_{it} = \alpha_i + \gamma TC_{it} + \eta X_{it} + \varepsilon_{it}$				
OLS	-19.798	2.625	-0.00145	0.000814
IV	-144.343	1744.043	-0.0002	0.000125
OLS (subsample)	-12.814	22.642	-0.00224	0.001497
IV (subsample)	-211.900	15571.983	-0.00014	0.00011

Standard Errors are calculated using the Delta Method $V(\hat{\beta}) \cong \frac{\hat{\rho}^2}{\hat{\gamma}^4} V(\hat{\gamma}) + \frac{1}{\hat{\gamma}^2} V(\hat{\rho})$

Appendix. Table A1. Descriptive Statistics

Variables	All workers	Temporary	Permanent
Ext. margin	0.214	0.187	0.226
	(0.410)	(0.390)	(0.418)
Minutes difference	270.175	206.833	296.905
	(647.250)	(551.191)	(681.986)
Absence rate	0.122	0.097	0.133
	(0.290)	(0.253)	(0.304)
Temporary contract	0.297	1.000	0.000
	(0.457)	(0.000)	(0.000)
Female	0.431	0.468	0.415
	(0.495)	(0.499)	(0.493)
Married	0.566	0.394	0.639
	(0.496)	(0.489)	(0.480)
Primary Educ	0.451	0.423	0.520
	(0.497)	(0.494)	(0.500)
Second Educ	0.217	0.208	0.221
	(0.412)	(0.406)	(0.415)
Higher Educ	0.332	0.272	0.357
	(0.471)	(0.445)	(0.479)
Age 16-19	0.024	0.063	0.007
	(0.153)	(0.244)	(0.086)
Age 20-24	0.098	0.194	0.057
	(0.297)	(0.396)	(0.232)
Age 25-29	0.139	0.201	0.113
	(0.346)	(0.401)	(0.317)
Age 30-34	0.135	0.144	0.131
	(0.341)	(0.351)	(0.337)
Age 35-39	0.137	0.121	0.143
	(0.344)	(0.327)	(0.351)
Age 40-44	0.139	0.104	0.154
	(0.346)	(0.305)	(0.361)
Age 45-49	0.125	0.076	0.146
	(0.331)	(0.265)	(0.353)
Age 50-54	0.098	0.050	0.119
	(0.298)	(0.219)	(0.323)
Age 55-59	0.070	0.031	0.086
	(0.255)	(0.173)	(0.281)
Age 60-64	0.031	0.013	0.039
	(0.174)	(0.112)	(0.194)
Public Sector	0.222	0.172	0.243

	(0.415)	(0.377)	(0.429)
Tenure	111.608	24.397	148.409
	(122.301)	(45.693)	(125.795)
Observations	1399824	415407	984417