

Measuring the Effects of Employment Protection on Job Flows: Evidence from Seasonal Cycles

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Abstract

Theory implies that employment protection will unambiguously decrease job flows. However, cross-country comparisons of annual rates of job reallocation seem to show that employment protection has no discernible effect on job flows. This paper presents a model that shows that employment protection does not significantly alter a firm's response to highly persistent shocks – such as those present in annual data. By contrast, quarterly job flows will reflect highly transitory shocks – such as those associated with the seasonal cycle. It is here that employment protection should reduce job flows.

Testing this hypothesis requires a consistent set of cross-country set of quarterly job flows. In the absence of such data, a novel approach is used, manipulating available household survey data. Specifically, a measure of job flows caused by the seasonal cycle is constructed. Analyzing these flows across 14 OECD countries, employment protection is shown to have significant and economically meaningful effects on job flows. Indeed, the size of the effect is sufficient to confirm Blanchard and Portugal's hypothesis that it is employment protection that explains the different pattern of labor turnover between Portugal and the USA.

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1. Introduction

This paper is concerned with identifying the effects of employment protection on job flows.¹ Despite the fact that theory predicts that employment protection will reduce job flows, empirical studies using annual cross-country flow data have not been able to document such a relationship. In this paper I argue that this lack of empirical results may be partially due to the frequency of the data examined (combined with the lack of consistent cross-country job flow data). By arguing that the reduction in job flows stemming from a temporary shock should be much higher than that which results from a permanent shock, I am able to use variation in seasonal employment to get a more plausible measure of the effects of employment protection on job flows.

As such this paper uses quarterly data, rather than the more commonly examined annual data. A measure of the number of job flows mandated by the seasonal employment cycle is constructed by examining the seasonal cycle in household employment survey data. This method permits me to examine the effects of employment protection on job flows across a range of countries on both sides of the Atlantic, and both sides of the equator.

2. Background

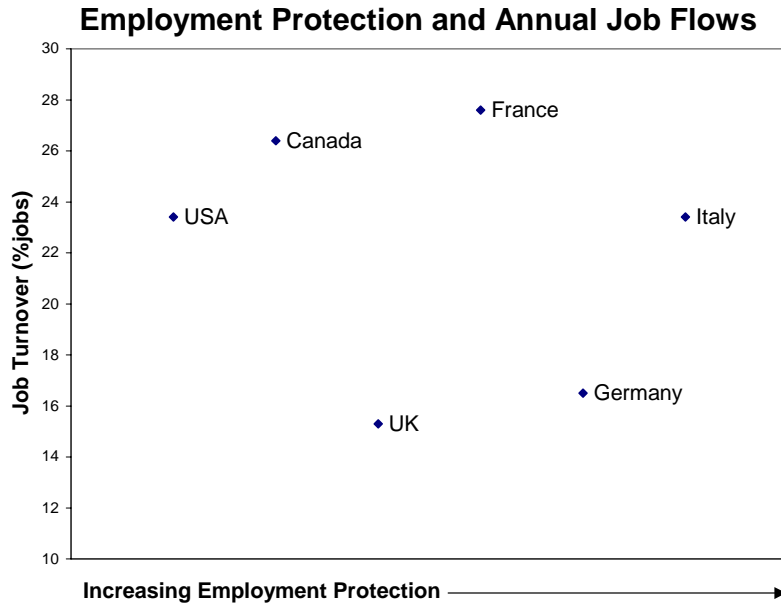
Employment protection has both transfer and cost components. Theory predicts that inefficient employment protection in the form of firm/worker transfers should have little effect as a reverse worker/firm transfer can offset it. As a result Lazear (1990) argues that employment protection may not matter in a world of efficient contracting. However, he notes that imperfect (or absent) bonding is probably a better characterization of current employment practice.

Employment protection also often has a real cost component (e.g. administrative costs). Reverse transfers cannot eliminate these costs. Therefore even in the face of perfect bonding employment protection will be binding in many situations. As a result employment protection should have a significant effect on job flows.

The theoretical framework set out below shows that employment protection should reduce job creation and destruction. However, looking at international comparisons of *annual* rates of job reallocation across countries, Bertola and Rogerson (1997) find that “despite stringent dismissal restriction in most European countries, rates of job creation and destruction are remarkably similar across European and North American labor markets.” Indeed, the most striking regularity found in

¹ The OECD Jobs Study, defines “employment protection” as follows: “Employment protection legislation relates to ‘hiring and firing’ rules governing unfair dismissal, lay-offs for economics reasons, severance payments, minimum notice periods, administrative authorization for dismissals and prior discussion with labor representatives.”

these data is that *annual* rates of job destruction and creation are approximately equal across countries. Figure 1 from Bertola and Rogerson, reproduced below illustrates this:²



* For further details, see p.1154 of Bertola and Rogerson (1997)

In a more detailed study, Blanchard and Portugal (1998) find that annual rates of job reallocation are approximately equal in the USA and Portugal, despite significantly higher firing costs in Portugal.

The failure to find an effect of employment protection on job flows is a significant puzzle. So far the literature has offered two interpretations of this result. Firstly, Buechtemann (1993) argues that that employment protection legislation does not “bite”, and has only a negligible impact on outcomes. Second, Bertola and Rogerson argue that while European job flows should be dampened by job protection, the rigid wage structure has an (equal) offsetting effect.

This paper rejects both of these interpretations, arguing that job protection does significantly reduce job flows, but that this will be most evident in short-frequency data. This hypothesis has the merit of being consistent with evidence on *monthly worker* flows. Mosley and Kruppe (1993) and later Blanchard (1998) find that employment protection is highly correlated with the exit rate from unemployment. To date, the literature does not contain a reconciliation of different stories told by the job flow and worker flow data. This is a second important element of the puzzle.

² For supporting evidence, see also Davis, Haltiwanger and Schuh’s table 2.2.

This paper provides a first step in reconciling these results. We know from Barsky and Miron (1989) that much of the variation in aggregate employment in the US is due to seasonal variation.³ Hence it seems likely that much of the “action” in hiring and firing will occur within the seasonal cycle.

Indeed, a central result of my model is that employment protection will have a larger effect on shocks that are less persistent, and when shocks hit the economy more regularly. Consequently, job flows due to the (regular and transitory) seasonal cycle will be dampened by employment protection, while longer frequency and more persistent shocks – those present in annual data – will be largely unaffected.

Blanchard and Portugal (1998) hint that these theoretical results might be present in the data. Specifically, they find that despite similarities in *annual* job flows in Portugal and the USA, the *quarterly* rate of reallocation is relatively smaller in Portugal. They conjecture that this might reflect the influence of employment protection. The open question from their paper is whether this explanation will generalize to a broader cross-section of countries.

In this paper I attempt to provide an answer to this question. However, given the problems in constructing a consistent set of cross-country quarterly reallocation rates, available data is manipulated to yield insights. Rather than focus on all short-frequency reallocations, this paper only examines those job flows due to the seasonal cycle. By limiting the focus to this (potentially important) source of employment reallocation, standard household survey data can be used to provide an indicator of that subset of job flows mandated by the seasonal cycle.

The maintained hypothesis is that short-frequency reallocation is substantially impaired by employment protection, but that this is not the case when looking at more persistent shocks.

3. A Model of Employment Protection and Job Destruction

This section adapts the model presented in Blanchard (1998) to allow variation in the duration of a shock. By allowing the shock to be finitely lived, the effect of the persistence of shocks on the sensitivity of job flows to firing costs can be examined.

The unit of analysis here is a job, which is subject to productivity shocks of varying duration. Adverse productivity shocks may result in job destruction, depending upon the level of employment protection (a firing cost), and the persistence of the shock. The model works out the sensitivity of job destruction to these parameters.

³ Barsky and Miron note on p.513 that “Seasonal dummies also explain a quantitatively important percentage of the fluctuations in the labor market variables (approximately two-thirds of the log growth

Assumptions

(A1) The productivity of a job is denoted y . New jobs have productivity y .

(A2) Each period there is a $\lambda\%$ probability that the job will be subject to a productivity shock. This shock will change the productivity of the job to a level drawn from $F(y)$. The shock persists for n periods, and in period $t+n+1$, productivity reverts to its previous level. When a shock arrives, the firm knows how long it will last. (Translated to my purposes, this assumption says that the firm can discern seasonal shocks from business-cycle shocks.)

(A3) When a productivity shock has rendered a job sufficiently unprofitable, the firm will destroy the job, incurring a firing cost G . Either because of incomplete contracting, or the nature of the firing cost, G is not offset by a reverse firm/worker transfer.

(A4) The wage is set once at the beginning of the job, and is not renegotiated following productivity shocks. Because all jobs start with the same level of productivity, all wages are equal.

(A5) If a firm decides to close a job, it can use the capital in the old job to open a new one.

Equilibrium Condition

The firm will keep a job open as long as its productivity does not become too low. That is, the firm will destroy the job if the cost of doing so, G , is less than the opportunity cost of keeping the job open, $V(y) - V(\bar{y})$ (noting that the firm can re-employ its capital in a new job). In equilibrium, there will be a cut-off level of productivity, y^* which will leave the firm indifferent:

$$V(y^*) = V(\bar{y}) - G$$

Turning now to deriving y^* , I focus on the value function.

rates)".

Value function

Working in discrete time, the present value of a job depends upon both its current productivity, and the expected persistence of that level. Specifically for a job that has just undergone a productivity shock, resulting in a productivity level of y which will last n periods, the discounted stream of future output (in the absence of further shocks) can be expressed as:

$$PV(y, n) = \sum_{i=1}^n (y - w)(1+r)^{i-1} + \sum_{i=n+1}^{\infty} (\bar{y} - w)(1+r)^{i-1}$$

$$= \frac{1+r}{r} \left[(y - w) - \frac{y - \bar{y}}{(1+r)^n} \right]$$

And hence the value function can be expressed:

$$\frac{r}{1+r} V(y) = \left[(y - w) - \frac{y - \bar{y}}{(1+r)^n} \right] + \lambda F(y^*) [V(\bar{y}) - V(y) - G] + \lambda \int_{y^*}^{\infty} [V(y') - V(y)] dF(y')$$

Solving for the equilibrium condition yields the “cutoff” level of productivity:

$$y^* = \bar{y} - G \left[\frac{\lambda + r/(1+r)}{1 - (1+r)^{-n}} \right]$$

That is, if the productivity of a job falls below y^* then the firm will destroy the job. The above equation tells us firstly that positive productivity shocks will never lead to job destruction. Beyond this, higher firing costs, less persistent shocks, and a higher probability of a further shock, all unambiguously reduce this cutoff level.

Job Destruction

If firms destroy jobs whenever their productivity falls below y^* , then the rate of job destruction will simply reflect the probability that productivity falls below this level. As such, the equilibrium rate of job destruction will be $\lambda F(y^*)$, and hence will depend on both the distribution of shocks hitting the economy, and those factors that affect y^* , particularly firing costs, and the frequency and persistence of shocks.

Turning now to the sensitivity of job destruction to firing costs:

$$d(\text{Job Destruction}) = d(\lambda F(y^*)) = -f(y^*) \left[\lambda \frac{\lambda + r/(1+r)}{1 - (1+r)^{-n}} \right] dG$$

Note that responsiveness of job destruction to firing costs depends on the persistence of shocks n , and the frequency of shocks, λ - both directly (through the term in square brackets) and indirectly through the determination of y^* . It now remains to sort out the signs of these different effects.

If the distribution of shocks is drawn from a uniform distribution, then the probability distribution term, $f(\bullet)$, is constant and the indirect effect can be ignored. These effects are now easier to disentangle.

To get a sense of the measure of responsiveness assume a quarterly real interest rate of 1% and compare a shock that lasts for one quarter ($n=1$) with one that is permanent ($n=\infty$). The responsiveness of job destruction to firing costs is larger in response to the temporary shock by a factor of $(1+r)/r$, which is close to a hundred-fold higher responsiveness.⁴

Highlighting the finding that job destruction is more sensitive to firing costs when shocks are less persistent, these derivatives can be expressed as ratios to their values when assessed at parameter values for both permanent and transitory shocks:

$$\frac{dJD(n)}{dG} = \left[\frac{1}{1 - (1+r)^{-n}} \right] \frac{dJD(Permanent\ shock)}{dG} = \left[\frac{1+r}{r} (1 - (1+r)^{-n}) \right] \frac{dJD(Seasonal\ shock)}{dG}$$

Business cycle-shocks might be characterized as lasting say, four years. Adjustment to these shocks is unlikely to be significantly hampered by the presence of firing costs. Indeed, the above equation suggests that the sensitivity of job destruction to firing costs is likely to be around one-fifteenth that of seasonal shocks.

The simple conclusion is that if job protection retards labor reallocation, it does so most when the shock is not persistent. Further, the responsiveness of job destruction to firing costs is increasing in λ , the frequency of shocks. Because seasonal shocks occur every quarter, this gives a further reason to suspect that employment protection will retard seasonal reallocation, but not less frequent shocks. Hence the model suggests that while Bertola and Rogerson find no discernible effect of employment protection on job flows in annual data, an effect might be found in quarterly data.

⁴ Note that the assumption that the shocks are drawn from a uniform distribution makes the results look a little too strong. Clearly y^* is decreasing in n , and hence for bell-shaped distributions, the “indirect” effect, operating through $f(y^*)$ slightly offsets the direct effect.

4. Data

a) Description of Data

The measure of employment protection used is the summary measure compiled by the OECD Jobs Study. This measure is an analysis are shown below.

Table 1: Employment Protection and Seasonal Re-allocation Rates

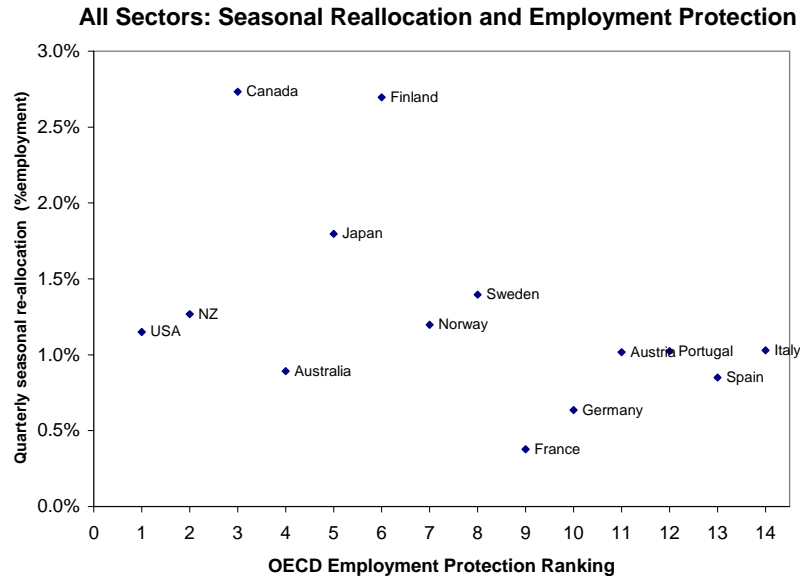
	Employment Protection Ranking	Quarterly re-allocation rate due to the seasonal cycle (%employment)			
		Total ¹	Agriculture	Industrial	Services
USA	1	1.15%	10.45%	2.22%	0.40%
NZ	2	1.27%	4.15%	1.16%	0.84%
Canada	3	2.73%	10.71%	5.94%	1.28%
Australia	4	0.89%	2.35%	0.59%	0.89%
Japan	5	1.80%	14.97%	1.02%	0.74%
Finland	6	2.70%	5.39%	4.48%	1.48%
Norway	7	1.20%	4.55%	2.47%	0.47%
Sweden	8	1.40%	3.62%	1.83%	1.10%
France	9	0.38%	3.90%	0.36%	0.31%
Germany	10	0.64%	2.81%	0.59%	0.52%
Austria	11	1.02%	1.42%	1.47%	0.67%
Portugal	12	1.02%	1.24%	1.22%	0.83%
Spain	13	0.85%	1.37%	0.68%	0.84%
Italy	14	1.03%	3.67%	0.70%	0.82%
AVERAGE (Unweighted)		1.29%	5.04%	1.77%	0.80%

¹ Total is an employment-weighted average of the three sectoral reallocation rates.

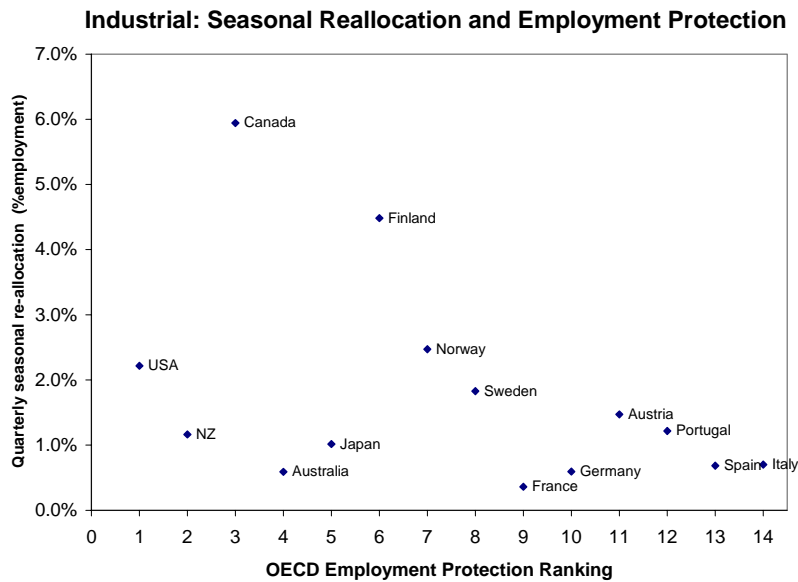
5. Results

The correlation between employment protection and aggregate seasonal reallocation is shown in the following chart.

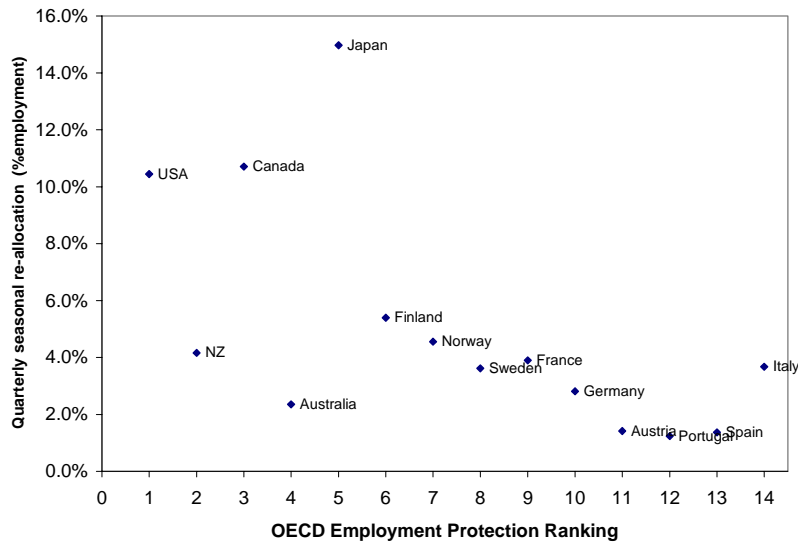
Chart 1



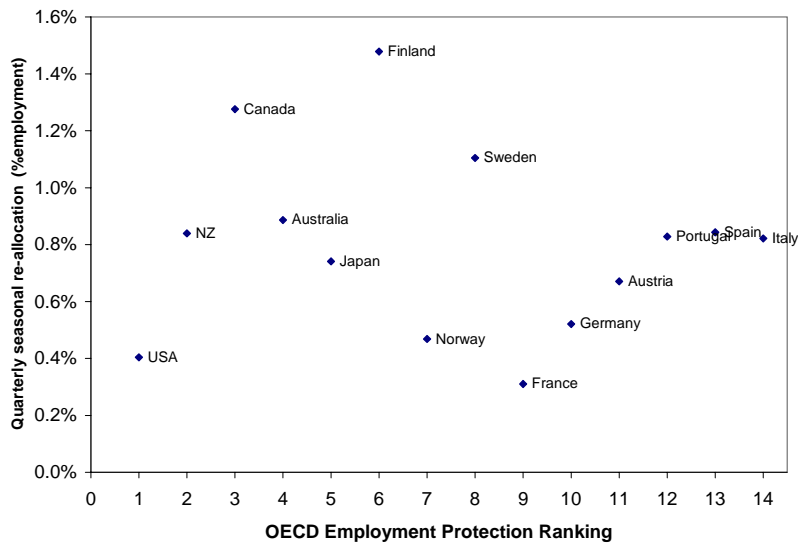
Averaging across the three sectors the expected negative relationship appears to be present, but weak in the data. The correlation is -0.44 , which returns a p-value tantalizingly close to 10%. Looking at the same plot for the individual industry groupings is more revealing.



Agriculture: Seasonal Reallocation and Employment Protection



Services: Seasonal Reallocation and Employment Protection



Clearly, the strongest relationship exists in agriculture, with a less pronounced, but still clear relationship in the industrial sector, and a barely observable (but still negative) correlation in the service sector. Encouragingly, all three correlations have the expected sign, although, individually, only the correlation in the agricultural sector is significant at the 10% level.

With only 3 industries and 14 countries, combining these data to obtain statistical significance will be a difficult hurdle. Reflecting the fact that different industries will experience seasonal shocks of different magnitudes, the regression equation uses all 42 industry-country

observations,⁵ but allows for different slope and intercept terms by industry. (The services sector is the suppressed industry).

Table 2: Regression Results

Dependent Variable: Rate of Seasonal Re-allocation	
Emp Protection * Agriculture	-0.61 (0.14)
Emp Protection * Industrial	-0.17 (0.14)
Emp Protection * Services	-0.01 (0.14)
Dummy: Agriculture	8.74 (1.71)
Dummy: Industrial	2.16 (1.71)
Constant	0.87 (1.21)
Adjusted R² 0.54	
(Standard errors are shown in brackets)	

Testing for the joint significance of the three employment protection terms yields an F-statistic of 6.62, which is significant at the 1% level of significance (p-value of 0.11%). In the context of the US versus Portugal puzzle, I will show that coefficients are also economically significant.

Table 2 suggests that the statistical significance of employment protection is driven largely by the agricultural sector. This result seems at least a little puzzling: exceptions to standard “employment protection” clauses are reasonably common for agricultural seasonal workers, and hence we might expect the aggregate OECD-ranking to be less informative in this sector.

An index describing employment protection specifically in the agriculture sector could not be found in the literature. The next best check is to exclude observations on agriculture and test the joint significance of employment protection in only the industrial and service sectors. That is, excluding my most favorable 14 observations, an F-statistic of 2.68 is found to be significant at the 10% level (p=6.1%), providing further confirmation that the hypothesized effect is in fact present in the data.

⁵ More fully, there are 42 industry-country observations of rates of seasonal reallocation. Implicitly I assume that the OECD aggregate employment protection rankings are equally applicable in each sector. Whether this is a fair characterization of agriculture is discussed further below.

6. Interpretation

This section will discuss four main issues: the relationship between these results and job flows, reasons for different industry effects, reconciling the evidence on Portugal v. USA, and endogeneity bias.

Reconciling my results with job flows

A natural question is whether my proxy measure of job flows mandated by the seasonal cycle yields any interesting insights into what sort of relationship to expect between employment protection and a Davis-Haltiwanger measure of quarterly job flows. Again, I restrict attention to those job flows that are caused by the seasonal cycle.

If the “right” Davis-Haltiwanger measure were available, then the regression co-efficient would be:

$$\beta^{D-H} = \frac{\text{cov}(\text{job flows}, \text{employment protection})}{\sigma_{\text{job flows}} \sigma_{\text{employment protection}}}$$

Note that $\alpha\%$ of my firms follow a standard seasonal hiring pattern, and $1-\alpha\%$ follow a “counter-seasonal” pattern.⁶ Hence:

$$\beta^{D-H} = \frac{\text{cov}(\alpha(\text{job flows}) + (1-\alpha)(\text{job flows}), \text{employment protection})}{\sigma_{[\alpha(\text{job flows}) + (1-\alpha)(\text{job flows})]} \sigma_{\text{employment protection}}}$$

By contrast, my regression co-efficient measures:

$$\beta^{\text{estimated}} = \frac{\text{cov}(\alpha(\text{job flows}) - (1-\alpha)(\text{job flows}), \text{employment protection})}{\sigma_{[\alpha(\text{job flows}) - (1-\alpha)(\text{job flows})]} \sigma_{\text{employment protection}}}$$

Under the assumption that employment protection affects both seasonal and counter-seasonal firms equally, and both types of firms are drawn from equally disperse populations, the following simple relation appears:

$$\beta^{\text{estimated}} = (2\alpha - 1) \cdot \beta^{D-H}$$

That is, the estimated regression has the same sign as one of job flows on employment protection, but it is biased down (noting that $1/2 \leq \alpha \leq 1$). This allows me to conclude that employment protection does significantly decrease intra-year job flows.

⁶ To be more precise, $\alpha\%$ of job flows in each quarter are job creation (destruction) when that quarter is one in which creation is greater than destruction (destruction > creation).

Size of the co-efficients across sectors

Recall that the effects of employment protection in the agriculture sector were large, with noticeable effects in the industrial sector, and barely observable effects in the services sector. Two explanations for this pattern are offered.

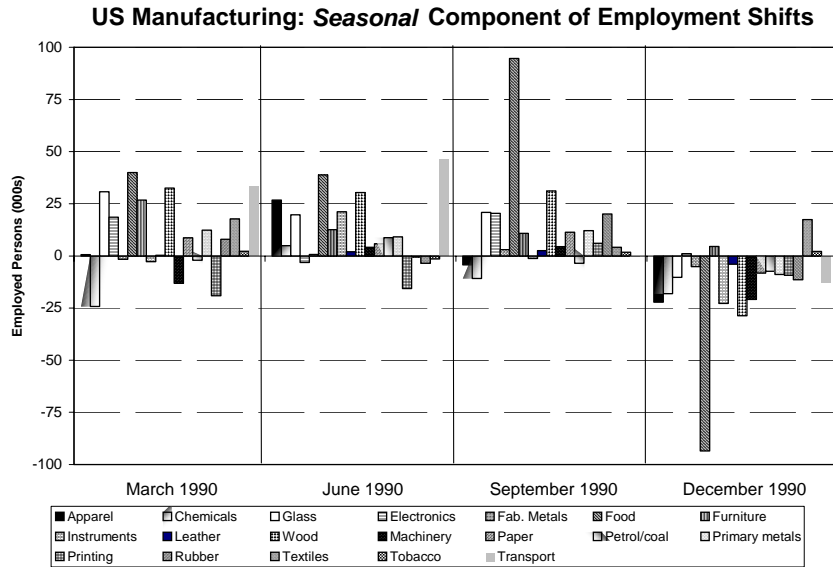
The first and most obvious reason for different effects is that each sector will have a seasonal cycle of a different magnitude, and different technology available to smooth these fluctuations. The second reason is a little less obvious. Recall that the regression co-efficient is biased down by a factor of $2\alpha-1$, where α is a measure of the extent to which the seasonal cycles of firms within an industry coincide. It seems likely that a larger and more heterogeneous sector, like the “services sector” contains a great number of firms and sub-sectors with offsetting seasonal cycles. Thus, employment protection may significantly dampen each of these seasonal cycles, but the effect that my proxy measure picks up is further biased down.

Portugal v. USA: An attempted reconciliation

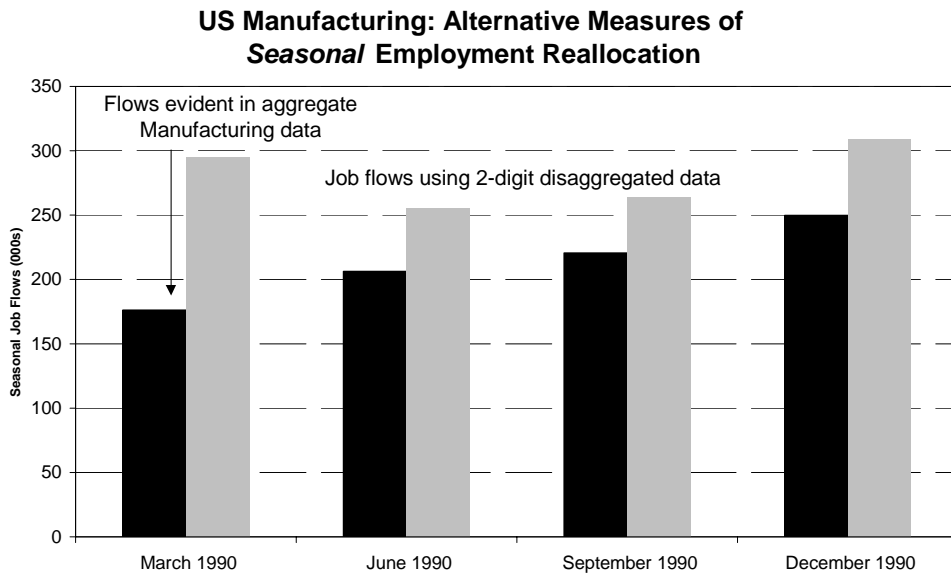
Blanchard and Portugal (1998) show that Portugal and the USA have vastly different levels of employment protection (their rankings in my sample are 12th and 1st respectively), but they nonetheless record comparable rates of *annual* job flows. Digging deeper, they find that *quarterly* job flows are much lower in Portugal than the US (6.8% compared with 11.1%). They suggest that employment protection might explain this differential, but suggest that the argument needs to be strengthened by looking at careful cross-country work. The results of this paper are an ideal test of their hypothesis.

Taking the co-efficient estimates in table 1 seriously, I find that if Portugal were to adopt a US-style system of employment protection, then quarterly rates of seasonal reallocation would rise from their current level of around 1% to 3%. That is, the different employment protection measures lead me to expect a 2 percentage point difference in quarterly reallocation rates. However, as suggested above, this estimate does not translate directly into an impact on Davis-Haltiwanger-style job flows because my co-efficient is biased downwards. I now turn to estimating the size of this bias, asking the following question: By using aggregate data, what proportion of quarterly seasonal job flows am I capturing?

As a simple case study (dictated by data availability), I focus on the US manufacturing sector. My “representative” seasonal cycle assumption can be tested by checking how uniform the “manufacturing seasonal cycle” is when I disaggregate to the level of 2-digit SIC sub-industries. The results of some simple manipulations are shown below.



Focussing on the first panel (the March quarter), it can be seen that employment rises in twelve sub-sectors, and falls in six. These offsetting changes would be missed by a measure of reallocation based on aggregate manufacturing data (as mine is). The next chart sums the (absolute value of) the seasonal job flows identified at this level of disaggregation, and compares it with the job flows that would be identified at a higher level of aggregation.



Averaging across all four quarters, it can be seen that measuring seasonal job flows at a 1-digit, rather than 2-digit level will lead me to “find” only three-quarters of all flows. Given the possibility that there are still offsetting seasonal cycles within 2-digit industries, this number serves as an upper bound on the proportion of seasonal job flows that my measure picks up. Indeed,

my work probably misses even more flows given that it is not even based upon 1-digit industry data, but 3 sectors across the whole economy.

Taking the 75% number from this example as a guide, the rough orders of magnitude of US job flows can be characterized in a new and novel way. Annual job flows are around 20%, with quarterly rates closer to 10%. This suggests that intra-year job flows average around 5% per quarter. My measured rate of seasonal reallocation within the industrial sector is 2¼% per quarter, but if my measure has missed a quarter of seasonal flows, then the right number is closer to 3%. That is, quarterly job flows are 10%, of which 5% are due to persistent shocks (lasting more than a year), 3% are due to employment adjustments over the seasonal cycle, and 2% are due to other transitory flows (such as when a job goes unfilled for a quarter following a quit).

Returning to Portugal, my measure finds seasonal rates of reallocation in Portugal are 1%. Counting in the flows missed by my measure leads to a number closer to 1¼-1½%. If I took the co-efficient from Table 1 at face value, then the effects of Portugal having a higher level of employment protection than the US retards quarterly rates of seasonal reallocation by nearly 2% (12th ranking – 1st ranking * 0.17). However, as suggested above, these co-efficients are biased down. Now, if my measure catches 75% of flows this suggests that one-eighth of flows occur in “counter-seasonal firms”, and hence that $\alpha = 7/8$. Correcting my coefficient by a factor of 4/3, employment protection reduces quarterly job flows in Portugal by 2½-3 per cent. Further, it is likely that other (non-seasonal) intra-year job flows are reduced by employment protection.

Consequently, most and possibly *all* of the gap between measured quarterly job flows in Portugal and the USA can be attributed to the presence of higher employment protection in Portugal.

Reverse Causation?

Employment protection is a policy variable, which is presumably set according to some political reaction function. Consequently, all studies of the effects of employment protection on the economy are subject to a charge of reverse causation. Indeed, Lazear (1990) notes that causality is “the most troublesome part” of his analysis. While there are stories that might point to reverse causation driving my results, it is worth noting that the “political story” told in such an argument will be different than the usual one.

Lazear (1990) summarizes the usual story: high unemployment causes worker anxiety, leading to political demands for employment protection. However, given that there is no obvious relation between the amplitude of the seasonal cycle and the unemployment rate, this story poses no problems for my analysis. In the present case, there is a negative correlation between the rate

of labor reallocation and employment protection across countries. For causation to go the “wrong” way would require voters in those countries with large employment fluctuations to demand less employment protection. At first glance, this seems to be an unlikely story. However, as noted above, exceptions to employment protection legislation are often granted in agriculture – an industry in which there are large seasonal job flows.

7. Conclusion

When thinking about job flows, seasonal cycles clearly matter. Consequently, when looking for the effects of labor market institutions on job flows, one must burrow beneath annual flows and look at intra-year reallocation. Institutions such as employment protection impose a significant cost to firing and therefore should reduce firms’ willingness to both create and destroy jobs in response to shocks. However it seems intuitively plausible that firms will be more worried about these costs when the benefit of adjusting to the shock is smaller – such as when the shock is less persistent.

In the model presented it is shown that the duration of a shock does matter for job flows. Specifically it is shown that the effect of employment protection on job flows is much smaller in the face of a permanent shocks compared with that which occurs from a temporary shock.

In this paper household employment survey data is used to construct a measure of the number of job flows mandated by the seasonal employment cycle. This data is then manipulated in order to examine the effects of employment protection on the seasonal cycle. The approach used finds a significant effect of employment protection on seasonal job flows.

That is, job flows are significantly affected by employment protection. Existing findings to the contrary are reconciled by arguing that this effect will be clearer at shorter frequencies. This conclusion has the merit of being consistent with both micro-studies, which document significant effects, and worker-flow data, which finds large effects at very short frequencies.

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