Turning Pink Slips into Red Tape: The Unintended Effects of Employment Protection Legislation

Harlan Holt* Joshua R. Hendrickson†

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Abstract

This paper presents evidence on the link between employment protection legislation (EPL), such as mandated severance packages for fired workers, and the rate of unemployment in a cross-country panel data set of OECD countries from 1990-2013. We use both a traditional fixed effects panel specification with lags of the policy variable, and also a unique structural panel vector autoregression (PVAR) method to determine the long-run dynamic interaction between employment protection legislation and unemployment. We confirm that a tightening of EPL for permanently employed workers causes a significant and persistent increase in unemployment, but the effect is only apparent at long lag lengths, some 2-5 years after the law has been implemented. We find weaker evidence that employment protection legislation specific to temporary worker contracts also increases unemployment.

*University of Mississippi - Department of Economics, 341 Holman Hall, University, MS, 38677, hmholt@go.olemiss.edu
†University of Mississippi - Department of Economics, 229 North Hall, University, MS, 38677, jrhendr1@olemiss.edu

1 Introduction

Over the past five years, there has been a considerable effort on the part of many developed countries including Spain, Estonia, the UK, Italy and Portugal, to reduce the costs imposed on employers associated with hiring workers. While labor legislation debates typically revolve around the use of unemployment benefits and their distortionary effects on workers' labor-leisure allocation, relatively less attention is paid to the effects of Employment Protection Legislation (EPL), which is designed to protect the worker from losing his or her job by imposing additional costs on employers who fire their workers. These policies include state-mandated severance packages for firing workers, lengthy prior notice to terminated employees, and other administrative costs associated with dismissal.

The purpose of this paper is to determine whether the passage of additional EPL increases the rate of unemployment in a country, an effect that the prior literature has had difficulty confirming. To address this question, we present a fixed-effects panel model with lags of the policy variable and a panel vector autoregression (PVAR) specification which, to our knowledge, has not been used to address this question. The reason for the policy lag is that the choice of policy is not independent of the unemployment rate. Policy makers will base their legislative decisions in part on the current unemployment rate, thus any model that estimates the contemporaneous relationship between EPL and unemployment will have estimates that are biased and inconsisent due to simultaneity bias. The choice of policy however will be unrelated to the future unemployment rate because it is unobserved at the time the choice of policy is made. The policy lags also allow us to construct the basis for a long-run adjustment mechanism.

Even though we could conceivably identify a contemporaneous effect through the use of instrumental variables, focusing on the contemporaneous effect obscures much of the story. For example, consider the passage of an employment protecton law. In the short run, it is improbable that firms would immediately begin firing workers because of the costs of immediately changing output decisions, in addition to the additional penalties resulting from firing workers under the new legislation. Thus in the short run, we should expect very little increase in unemployment, even if the effect is correctly identified. Instead, we expect that any interesting structural change in the unemployment rate will occur in the long run, and perhaps very far off into the future. This is because firms will much more likely wait for workers to leave their positions voluntarily, and then simply close the vacancy if they do not want to incur the cost of potentially firing the new worker. This means that previous research that focuses on the contemporaneous effect, underreports the true impact on unemployment, *even if* the effect is correctly identified and estimated. In fact, if the effect is strong enough, we may even observe a temporary drop in the unemployment rate if businesses find it too costly to fire workers in the short run leading to an incorrect assessment of the policy's effects.

We believe this is a subtle point that deserves special emphasis. Many prior studies have found that increased employment protection have no effect on the unemployment rate (See: Addison and Grosso (1996), Nickell (1997), OECD (1999), Addison et al. (2000), and many others).[1] Our goal in this paper is not to refute or overturn prior studies that show that increased EPL has no effect on unemployment in the short-run. Rather, we heartily agree with these findings, and believe them to be very plausible. Our broader goal is to show that failure to investigate the long-run effects of this policy can lead to an incorrect policy recommendation. There are a number of reasons why the long-run effects may have escaped the prior literature. Foremost among them is that available data on EPL has only been available since the mid 1990's. Long-run investigation of policy requires a sufficiently large number of observations on the time dimension for the data to show the effects.[2]

We revisit the hypothesis that EPL increases unemployment, however we focus on the long-run impact of these types of policies.[3] We follow up the traditional panel model with a time series approach to identify the long-run adjustment path with a panel vector autoregression (PVAR). The use of the PVAR model is important for two reasons. First, the PVAR specification allows for the assumption that unemployment and EPL are endogenously related, but allows us to identify the causal effect without the problems of instrument selection. Second, we are able to use the PVAR model to compute the dynamic response of unemployment to a change in EPL rather than just the initial marginal effect. This allows us to observe how the unemployment rate evolves over time after an exogenous change in EPL.

In both the fixed effects panel model and the PVAR model, we find evidence that an increase in the degree of employment protection leads to significantly higher unemployment, but the effect is only apparent

[1] It is arguable that this is the general conclusion of the literature.

[2] A simple panel fixed-effects regression can estimate the impact of EPL on unemployment. However if we want to examine a long-run relationship on a data set with time dimension $t = 1, ..., T$ by including the policy variable from the year prior, we must cannot analyze the first and last time observations (the policy lag for year 0 is unobserved, and data in response to a policy change in year $T - 1$ does not exist to be analyzed yet). Each subsequent lag we include in the regression consumes two additional degrees of freedom. Thus if you wished to examine the long-run effects of policy 4-5 years into the future, the researcher must throw away 8-10 years of observations. Because the EPL indicies on OECD countries used by most studies on EPL only became available after Grubb and Wells (1993), this study in particular would not have been possible as recently as 5-10 years ago, much less 15-20 years ago when many of the referenced papers in this field were published.

[3] The impact on the unemployment rate in the long-run should not be confused with the long-run unemployment rate which is defined as the percentage of the labor force that has been unemployed for longer than one year. This study predominantly concerns the former, not the latter.

at long lag lengths, some 2-5 years after the adoption of new legislation. This implies that while these policies produce a persistently higher unemployment rate, the effects may not become apparent until well after the policy has been implemented, and potentially may even appear counter to the political cycle since many legislatures in developed countries are turned over only once every two to six years.

2 Employee Protection Legislation

2.1 The Effects of EPL on Unemployment

Historically the direct policy implementation of EPL across most developed countries has concerned the length and generosity of severance payments and the amount of notice or administrative effort required by firms to terminate the employment of a worker. While these are the two largest components of EPL, labor market rigidity produced through legal protections for collective bargaining as well as an array of other policies can also be interpreted as an employment protection law.

The implementation of EPL can be thought of as having a similar goal as unemployment insurance (UI), but targeted instead at those who have work rather than those who are out of work. To the extent that it allows workers to smooth consumption by reducing the uncertainty of their permanent income, it does function somewhat like UI or other types of unemployment benefits. However, as stated by Blanchard, Jaumotte, and Loungani (2013), the intended policy goals are somewhat different: ”The purpose of *unemployment insurance* is to reduce the *pain* of unemployment. The purpose of *employment protection* is to reduce the *incidence* of unemployment.”[4] Thus, in accordance with the stated policy goals, we can generate a direct testable hypothesis of whether or not EPL actually achieves its stated purpose of reducing the incidence of unemployment.

The desire to have the state enforce a level playing field between employers and employees has been historically popular in political circles, especially with regards to policies that do not bear an explicit pecuniary cost to the government. Pro-labor policies are often aimed at limiting the supposed advantages that a firm enjoys when bargaining over wages and employment status. For example, Section 1 of the U.S. National Labor Relations Act of 1935, which outlines the right for workers to organize, explicitly states that “The inequality of bargaining power between employees who do not possess full freedom of association or actual liberty of contract and employers who are organized in the corporate or other forms of ownership associa-

[4] Emphasis from the original authors.

tion substantially burdens and affects the flow of commerce ... by depressing wage rates and the purchasing power of wage earners in industry." Similarly, researchers have argued monopsony buying power of firms that arises from either specialization or switching costs provides the basis for pro-labor type policies such as the minimum wage and the right to unionize. The implementation of EPL has a similar political motive because these policies are intended to create better outcomes for laborers by making firms pay an extra cost to fire their workers. This paper takes no position on whether or not these employer advantages exist or to what degree they affect labor market freedom. However, we do wish to examine whether the use of legislative force to overcome these perceived disadvantages can actually lead to adverse outcomes for workers.

As stated, the intended goal of these policies is to reduce the incidence of unemployment. However, there is strong reason to believe that while EPL certainly does disincentivize the firm from firing or laying off its existing labor force, the policy may not work as intended. Increasing the cost to firing workers means that firms will likely be more reluctant to hire them in the first place. Since it doesn't directly impact the firm's productivity, it is unlikely that firms will react instantly to a tightening of EPL by firing workers. Instead they will likely be more inclined to simply close positions that workers leave voluntarily, leaving fewer positions for new workers to compete over. This means that the most important effects of the policy exist in the *long run*. The short run effect is ambiguous and perhaps not all that meaningful. Unemployment may rise, fall, or remain the same immediately after employment protection legislation is passed. Understanding this is crucial to correctly assessing the policy's effectiveness. Econometrically, this means that any attempt to estimate the effect of employment protection laws on unemployment must necessarily focus on the long run effect of the policy, including examining the dynamic adjustment path from one level of unemployment to the next.

This logic is consistent with labor market search models, such as that of Pissarides (2000), which imply that increases in the ex ante cost of hiring workers leads to an increase in the unemployment rate and a reduction in the vacancy rate in the steady state. In addition, EPL might increase unemployment if it impedes labor market reallocation as others such as Bertola and Boeri (2002) have suggested. Thus, in the long run, we expect to see fewer vacancies and a higher unemployment rate directly contradicting the stated purpose of the law.

Since the number of vacancies is expected to fall, we should expect the additional unemployment generated by these policies to be persistent. That is, we do not expect to see a spike of unemployment which gradually returns to the steady state as we might with an ordinary aggregate demand or supply shock. Thus, the type of unemployment created by these incentives is likely to be structural and long-term in nature.

2.2 Recent Work

Lazear (1990) uses a panel model of 20 developed nations with a quadratic time trend to show that increases in mandatory severance packages to terminated employees have a positive effect on the unemployment rate, and a negative effect on the employment-to-population ratio, the labor force participation rate, and average hours worked. Lazear also estimates the regression with country fixed effects, and is able to confirm his results for all of the dependent variables *with the exception of* the unemployment rate. Lazear justifies this by stating that if employment protection causes structural unemployment, then over the long run we would expect more discouraged workers and the unemployment rate might indeed *fall*. This, however is unlikely given that his panel results reflect the instantaneous change in unemployment due to an increase in severance pay, whereas a discouraged worker problem is likely only to arise after a considerable time has passed.

Lazear (1990), and most of the accompanying literature says little about how long it can take for the effects of changes in the structure of employee protection legislation to appear in the aggregate employment statistics for an economy. Most studies focus on the contemporaneous impact of employment protection legislation on unemployment. The results have generally suggested that EPL has little to no effect on the unemplyoment rate. For instance, Addison and Grosso (1996), Nickell (1997), OECD (1999), Addison et al. (2000), Nickell et al. (2005), Sarkar (2013), and Avgadic (2013) all fail to find evidence that the unemployment rate is increased after additional employment protection is legislated. In addition to Lazear (1990), Scarpetta (1996), and Heckman and Páges (2000, 2003), find evidence of an increase in unemployment after an increase in the degree of employment protection.[5]

A more uncontroversial finding from the literature is that flows into and out of unemployment fall, suggesting that unemployment becomes more stagnant as a result of stricter EPL.[6] Once again, this is not surprising given the likely transmission mechanism as described above. If businesses do not immediately react to additional EPL by firing workers, but by reducing the amount of vacancies they post in the future, then this is consistent with both a small, nonexistent, or possibly even negative short run effect of EPL on unemployment and reduced employment flows.

[5] For an excellent survey of the literature, see Addison and Teixiera (2003).

[6] For examples, see OECD (1999), Kugler and St. Paul (2000), and Autor et al. (2007).

2.2.1 Estimation Issues

Despite the considerable depth of the literature, there is really no careful identification of the long-run trend of employment after a change in EPL. This is likely because of the availability of the data. Reliable EPL indicators as explored by Grubb and Wells (1993) have only been established since Lazear's (1990) seminal work. The most widely used data set, the OECD's EPL indicators, only report data back to 1990 for most nations, which means we have less than 25 years of data for this particular measure. It is relatively easy to establish contemporaneous effects in a panel model, however when trying to identify the long-run effect, the researcher inevitably consumes degrees of freedom quickly. What's more, as Nickell (1981) shows, dynamic panel models which attempt to incorporate a partial adjustment mechanism such as an AR(1) process for unemployment, like those suggested by Arellano and Bond (1991), can produce biased estimates in samples with a relatively small time dimension. It is no coincidence then that most of the previous studies on this topic do not even attempt to identify the long run marginal effects of EPL on unemployment, instead focusing on the contemporaneous impact of those laws. Sarkar (2013) summarizes the need for more long-run tests by stating, "A panel regression based on a short-term time series has the constraint of studying only the instantaneous relationship, which may not be meaningful; rather it may be spurious."

The focus on the contemporaneous effect of EPL is potentially problematic from an empirical perspective since the unemployment rate and EPL might be simultaneously determined. Many examples indicate why simultaneity might be a problem. Politicians may choose EPL to combat spells of high or rising unemployment. It is also possible that implementation lags may cause the policy to actually be enacted after the recessionary spell has passed leading to a suprious correlation between low or falling unemployment and EPL. On the contrary, since EPL policies represent a form of unemployment insurance for currently employed workers, it may be that EPL policies have greater political support when unemployment is low or falling as the economy exits a recession. If we wish to investigate how EPL affects unemployment, we must take into account the fact that unemployment almost certainly affects EPL as well.

In addition, even if these problems are corrected for the contemporaneous relationship between EPL and unemployment, there may still be reason to be skeptical about the policy implications of not finding a statistically significant relationship. This is because the long run *structural* relationship between EPL and unemployment may be very different than the instantaneous one. This would very likely be the case if the transmission mechanism described above is indeed the case. If employers react to a sudden tightening of

EPL not by immediately firing workers, but by closing future vacancies, then we should very likely see the detrimental effects of the policy only in the future, not in the immediate aftermath of the policy.

A recent attempt to reconcile these issues is Sarkar (2013), who uses an error correction panel model to examine the effects of EPL on unemployment. Sarkar fails to find a long run relationship between general unemployment and EPL, though he does find evidence that additional EPL can increase the proportion of the long-term unemployed population, which can reduce production which then further aggravates long-term unemployment.[7] Nonetheless, there is reason for skepticism about these results. First, Sarkar's model requires that the variables are non-stationary. In samples with a small number of observations across time, however, unit root tests are likely to over-accept the hypothesis of a unit root. Examination of the autocorrelation functions (ACFs) of the variables used in this paper cast doubt on the presence of non-stationarity. In Figure 1 we plot the ACFs for the G-7 countries plus Australia for the sample period 1990-2013. Visual inspection of the plots show that the autocorrelation of unemployment is statistically greater than zero for no more than a two year lag. Thus, we find the hypothesis of a unit root in unemployment doubtful. Second, Sarkar's failure to find a long-run relationship between EPL and unemployment is based on an absence of evidence of cointegration between the two variables. However, Sarkar finds evidence that EPL is cointegrated with GDP. Thus, the relevant test within the context of a vector error correction framework when there is cointegration is to test the hypothesis that the coefficient on EPL is equal to zero in the cointegrating vector rather than test for bivariate cointegration.

Whatever the mechanism, it is clear that the choice of EPL is partly determined by the present unemployment rate, thus any estimates of a contemporaneous effect of EPL on unemployment that do not take this into account are likely to be biased and invalid for policy inference. We address this in two ways. First, we use a fixed effects model to examine the effect of EPL on unemployment using various policy lags for employment protection. Second, we use a panel vector autoregression (PVAR) model to examine the effect of EPL on unemployment. Given the possibility of endogeneity, this second approach is important because tools like vector autoregression models treat all variables as endogenous. To our knowledge we are the first authors to pursue this latter approach.

[7] Long-term unemployment here refers to the percentage of the labor force unemployed for longer than a year, not the long-run trend of unemployment.

3 Data

We measure the degree of employment protection using indexes of employment protection for permanent workers and for temporary workers constructed by the OECD. The OECD's EPL measures are compiled using 21 different individual indicators on mandatory severance packages and administrative costs of dismissal tiered by employee tenure and the pervasiveness of unions and collective bargaining at the industry and national level. The OECD organizes these measures separately for permanent and temporary workers. The data spans the 34 OECD member countries over the period from 1990 to 2012. Summary statistics for these variables and the unemployment rates by country are provided in Table 1. Each indicator is assigned a score from 0 to 6 based on answers from a questionnaire on the strictness of the labor code with regards to that particular indicator with 0 being the least strict, and 6 being the most. Each item in the survey is then assigned a weight and then added up into component scores for procedural inconveniences, length of notice and severance pay, difficulty of dismissal, and additional provisions for collective dismissal. These component scores are again weighted and then combined into a final employment protection score from 0-6.[8]

Shortly after Lazear (1990), the need for better measures of employee protection became apparent. Grubb and Wells (1993) suggested that new measures incorporate not only the level of mandatory severance packages, but the length of term of prior notice needed to be given to individuals, tiered measures for the level of tenure among employees, and the differences arising from permanent versus temporary work positions. The modern measures of employee protection are largely based on the Grubb and Wells methodology.

The remaining variables in our model are taken from the World Bank's World Development Indicators. These variables include the unemployment rate, the percentage of unemployed who are identified as long-term unemployed (over one year), government expenditures as a percentage of GDP, inflation as determined by the CPI for each country, and income per capita growth defined as the log difference of real per capita income in constant 2005 US dollars to serve as control variables. A brief summary of these data is presented in Table 2.

[8] Since the scores for each category are weighted, and then aggregated, the final EPL measure assigned to each nation is not, in general, an integer value. This is beneficial in terms of the data since it increases the variation of EPL for the sample along both the cross sectional and time dimensions.

4 Fixed Effects Model

4.1 Model Specification

To examine the relationship between employment protection and unemployment, we begin with the following fixed effects model:

$$unemp_{i,t} = \beta_1 Perm_{i,t-k} + \beta_2 Temp_{i,t-k} + \beta_X' \mathbf{X}_{i,t} + a_i + a_t + u_{i,t} \tag{1}$$

where *unemp* is the unemployment rate in country i in year t, *Perm* is the value of the strictness of employment protection indicator for permanent work positions, and *Temp* is the value of the strictness of employment protection for temporary positions. $\mathbf{X}_{i,t}$ is a vector of basic control variables for unemployment in which we include the inflation rate to control for Phillips' curve effects, the growth rate of real per-capita GDP to control for Okun's law, and real government expenditures. We also include year fixed effects, a_t, to control for any global factors such as the global financial crisis that began in 2008.[9]

The choice of the fixed effects model is due to the fact that unobserved heterogeneity between countries is likely correlated with the explanatory variables. For example, Venn (2009) finds that EPL is correlated with the countries' legal origins. EPL is also likely to be correlated with other factors, many of which will be unobservable concerning the connection between labor market structure and the legal environment in which it operates. Examples might include the favorability of judgements in labor proceedings by the country's judicial system, the degree of monopoly or monopsony power of firms in key industries, or other effects produced by unrelated and unobserved government policies.

4.2 Results

A drawback of any specification of this type is that the contemporaneous effect of the EPL measure will be determined simultaneously with the unemployment rate. This is because lawmakers may find it more difficult to pass EPL during periods of high unemployment or may be more determined to do so while the unemployment rate is low. Any estimate of the contemporaneous effect of EPL on employment will be endogenously determined by reverse causality and will be unsuitable for policy analysis. Because of the high degree of serial correlation with unemployment rates, it is doubtful that even the first or second lags

[9] We also tested a specification with time trend instead of time dummies. The results were not significantly affected.

escape this endogeneity problem since the unemployment rate strongly depends on its previous value.

While the potential for simultaneity bias prevents us from saying anything about the instantaneous effect of the policy change, we can determine the long run effects of the policy by using lags of the policy since the future unemployment rate should not affect any policy change in the present. We use single, discrete lags of the policy variable for each regression, not a distributed lag. The choice of a single lag rather than a distributed lag is because the changes in EPL for each country are generally infrequent, at most occurring once every 5-10 years and sometimes not at all. Legislative overhauls of most sensitive national policy positions such as labor law often take considerable political effort, and the majority of votes needed to do so only occurs every so-often. Because of this, inclusion of a distributed lag of the policy variable is improper, because we would likely be introducing multicollinearity among the regressors which would reduce the precision of our coefficient estimates. We present the estimates of the specification using differing lag lengths of the policy variables in Table 3.[10]

As shown in Table 3, the EPL measure for permanent workers has a positive and statistically significant for lags of 2 - 5 years. The contemporaneous EPL measure for temporary workers is negative and statistically significant. However, we suspect that this is due to the potential for simultaneity bias. At the bottom of Table 3 we include the Akaike and Schwarz-Bayes Information Criteria (AIC and SBC) . The information criteria both uniformly decline as the lag gets longer indicating that the model fits better the further out from the policy change we get.

It is reasonable to suppose that if labor market rigidity were tightened through increases in EPL, we should also expect more of the workforce to become structurally unemployed as a result. This is because EPL may have uniform effects that differ across industries. For instance, we would likely expect firms that operate in industries that experience historically high turnover rates such as dining or retail to be affected more than historically low turnover industries such as finance. In this case, we would expect the workers displaced by this policy to have more difficulty finding suitable employment elsewhere, implying structural employment should rise. This type of unemployment is generally more painful for an economy because it tends to be persistent and longer lasting than other types. We use long-term unemployment as a proxy since direct measures of structural unemployment are not widely available. Using the same specification as equation (1), we present the results using long-term unemployment as a percentage total unemployment as

[10]We also estimated equation (1) with a regional fixed effect to control for regional supply shocks such as a natural disaster, changes in regional trading patterns, or localized systemically important asset market conditions. We also included regionally clustered standard errors. These changes did not significantly alter the regression results.

the dependent variable. The results are presented in Table 4.

As shown in Table 4, the measure of employment protection for permanent workers is positive and statistically significant for both the model with a four year policy lag and the model with a five year policy lag. The long time lags should not be surprising. Structural unemployment takes time to appear in the data since it is, by nature, a long-term phenomenon. The increase in structural unemployment is problematic for governments. Structural unemployment is generally stubborn and can take time to dissipate, leading to more pressure over time on social safety nets to provide for the unemployed and the loss of taxable income from the newly created structurally unemployed.

5 Panel Vector Autoregression Model

In this section we provide evidence of the effect of employment protection on the unemployment rate using an estimation method that has not previously been used in the literature. Specifically, we use a panel vector autoregression (PVAR) specification similar to Holtz-Eakin, et al. (1988) to capture the long run effects of changes in EPL on the unemployment rate by focusing on the long run dynamic transmission of the policy impact on unemployment. The PVAR model has two key advantages. First, it allows us to preserve the effects of unobserved heterogeneity between countries while assuming that the variables are endogenous. Second, the PVAR allows us to recover the orthogonal policy innovations at the cost of imposing a sufficient number of identification restrictions on the data. We use the recovered policy innovations to plot the dynamic response path of the unemployment rate to a change in either of the EPL policy variables with an impulse response function (IRF). Intuitively, once the model is appropriately identified, the IRFs plot the dynamic path of the unemployment rate in response to an unexpected increase in EPL.

The PVAR model is given as

$$AY_{i,t} = B_0 + B_1 Y_{i,t-1} + a_i + \varepsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ is a vector of endogenous variables, a_i are country fixed effects included to account for any non-time varying, unobserved heterogeneity among the countries, A, B_0, and B_1 are coefficient matrices, and $\varepsilon_{i,t}$ is a vector of structural shocks.[11] Here, the term structural shock refers to an unanticipated change in a

[11] To estimate the regression equation in (2), we first need to transform the variables to remove the fixed effects by applying a Forward Orthogonal Difference (FOD) procedure as in Arellano and Bover (1995), also commonly known as a Helmert transformation. We then estimate a reduced form representation of (2) and use a Choleski decomposition to uniquely identify the structural shocks of the system. For those unfamiliar with VAR methods, the econometric details on the transformation and estimation procedure are

particular variable.

The vector, Y, includes the endogenous policy variables $Perm$ and $Temp$ from the panel regression earlier and the unemployment rate. It is important to note that equation (2) cannot be estimated directly because the model implies that $Y_{i,t}$ is correlated with $\varepsilon_{i,t}$, which violates standard estimation assumptions. As a result, we first have to estimate a reduced form representation of (2). After the reduced form estimation, we can recover the structural shocks by imposing a sufficient number of identifying restrictions on the matrix A in equation (2), which captures the contemporaneous relationships between the variables.

We use a Choleski decomposition to identify the model, which amounts to forcing A to be lower triangular with zero entries below the principle diagonal, and free parameters elsewhere. Intuitively, the Choleski decomposition is consistent with the assumption that the variables in $Y_{i,t}$ have no contemporaneous effects on the variables ordered above them. This carries the potential drawback that the ordering of the variables in the regression equation matters for the estimates because the identifiying assumptions change. As is common in the estimation of multiple time series using a Choleski decomposition, we test the model using different orderings of the variables to show that the estimates are robust to the identifying assumptions. The first identification strategy orders the unemployment rate first and the policy variables last. This is consistent with the assumption that the policy variables do not have a contemporaneous effect on unemployment. The second identification strategy order the policy variables first and the unemployment rate last. This is consistent with the assumption that the policy variables do have a contemporaneous effect on unemployment. Once we have recovered the structural shocks, we can then plot impulse response functions that capture the dynamic response of the unemployment rate to unanticipated changes in the measure of EPL.

The path of the unemployment rate in response to a shock to employment protection for permanent workers is shown by the impulse response function in Figure 2. The number of years after the shock is plotted on the horizontal axis and the magnitude of the response of unemployment is plotted on the vertical axis. The solid line plots the estimated response of unemployment and the dotted lines represent the 95% confidence interval calculated from bootstrap simulations of the model. As shown in Figure 2, a change in employment protection for permanent workers leads to a positive and statistically significant change in the unemployment rate for each of the five periods following the shock. In addition, the estimates imply that the unemployment rate continues to rise for five years after the policy change. The estimates imply that a one point increase in employment protection (e.g. an increase from 0 to 1 in the EPL statistic) would result in a

located in Appendix A.

0.16 percentage point increase in the unemployment rate after five years.

To put this effect in the context of policy choice consider the comparison of the United States and Sweden. In the sample used in this paper, the average unemployment rate in Sweden was 6.97% whereas the average unemployment rate for the U.S. was 5.97%. The average value of the index of employment protection legislation for Sweden over the sample was 2.68 where the average value of the index was 0.26 for the U.S. Our estimates imply that the difference in employment protection legislation can explain 38.7% of the difference in unemployment between Sweden and the U.S.

Similar results are shown in Table 5 for the G7 countries. The second column in Table 5 lists the difference in the average unemployment rate over the sample period for the country listed and the United States. The last column of Table 5 shows the fraction of the difference in unemployment that can be explained by differences in employment protection legislation, given our estimates. As shown, five of the six remaining G7 countries had higher unemployment rates that the United States over the sample period. Our estimates suggest that the difference in the degree of employment protection can explain 8.4% to 17% of the difference in unemployment for these five countries, or 12.94% on average.

The path of the unemployment rate in response to a shock to employment protection for temporary workers is shown in Figure 3. The estimates in this figure assume that policy has no contemporaneous effect on unemployment. As shown, employment protection for temporary workers has a positive effect on the unemployment rate for each of the five periods after the shock. However, the change is not statistically significant.

Figures 4 and 5 plot the estimated path of the unemployment rate following a shock to employment protection for permanent and temporary workers, respectively, under the assumption that policy has a contemporaneous impact on unemployment. As shown in Figure 4, the initial effect is negative, but not statistically different from zero. Nevertheless, the effect of employment protection on the unemployment rate is positive and statistically different from zero beginning three years after the shock and the point estimates are of a similar magnitude as shown in Figure 2. This is also identical to the pattern found in our original simple panel estimates in which policy lags of 3 years or more showed a positive and statistically significant effect on unemployment. This also shows that the the earlier results are robust to alternative causal ordering of the vector of endogenous variables since the effect of EPL on unemployment remains positive despite different assumptions about the contemporaneous effects. Figure 5, however, shows that the effect of employment protection for temporary workers on the unemployment rate is not statistically different from zero for any period after the shock.

These results imply that EPL for permanent workers has a positive and statistically significant effect on unemployment. As hypothesized in the introduction, this effect tends to appear with a lag. Thus, policymakers and researchers should be concerned with the impact that EPL has on the unemployment rate far into the future, and should not just with the immediate impact of the legislation.

6 Conclusion

Employment protection legislation is used in a number of countries to specify the length and generosity of severance packages as well as the legally required amount of advanced notice and administrative costs associated with the termination of workers. For politicians these policies are appealing because they purport to reduce unemployment without a direct, explicit cost to the government. Our paper examines the impact of additional EPL on unemployment and tells a much different story. The initial effect of additional employee protection on unemployment is low or even negative. This may give policymakers the false impression that these policies can be enacted to protect workers from being fired without imposing additional costs on society. However, our results indicate that an increase in EPL does increase unemployment in the long run, and may not even be noticeable until after several years have passed. Presumably, this is because employers do not immediately fire workers after additional EPL is enacted, but close vacant positions opened up by workers who leave voluntarily. This finding potentially reconciles the two arguments in the literature that additional EPL does not immediately increase unemployment, but does reduce employment flows by making pools of unemployed more stagnant.

While the policy does not cost the state anything explicitly, the societal costs of maintaining a persistently larger population of unemployed are potentially quite large. Thus, the policy recommendation outlined by this paper is that OECD governments would be wise to lower the mandated costs associated with firing workers in order to permanently reduce the level of unemployment. In light of our findings, the efforts of nations in the EU to reduce EPL as part of their labor market reforms are likely to encourage labor market health in those countries over the next five years.

Appendix A: PVAR Estimation Details

The Panel VAR is shown in equation (2) as

$$AY_{i,t} = B_0 + B_1 Y_{i,t-1} + a_i + \varepsilon_{i,t}$$

Direct estimation of this equation is not possible for two reasons. First, the country-specific fixed effect, a_i, is correlated with the lagged dependent variables, so any estimated coefficients would be biased (but not inconsistent), as demonstrated by Nickell (1981). To eliminate this fixed effect from the regression, we use the Forward Orthogonal Difference (FOD) technique proposed by Arellano and Bover (1995), also commonly referred to as the Helmert transformation. This procedure transforms each of the variables into deviations from the means of all of the future instances of that variable. This removes the fixed effect, but does not produce a correlation between the transformed variables and the error term, and Nickell bias is no longer problematic.

Let the forward mean of any single variable in Y be given by

$$\bar{y}_{i,t} = \sum_{t+1}^{T} \frac{y_{i,t+1}}{(T-t)} \tag{3}$$

The transformed variables are then,

$$\tilde{y}_{i,t} = c_{i,t}(y_{i,t} - \bar{y}_{i,t}) \tag{4}$$

where $c_{i,t} = \sqrt{(T-t)/(T-t+1)}$ is a constant included to equalize the variances.[12] Denote the vector of transformed variables as $\tilde{Y}_{i,t}$. The transformed structural VAR can be written as

$$A\tilde{Y}_{i,t} = B_0 + B_1 \tilde{Y}_{i,t-1} + \tilde{\varepsilon}_{i,t} \tag{5}$$

Second, the model cannot be estimated in this form since it implies that all of the variables in Y_{it} have contemporaneous effects on the others. Thus, to estimate the equation above, we need to re-write the VAR as

$$\tilde{Y}_{i,t} = \Gamma_0 + \Gamma_1 \tilde{Y}_{i,t-1} + e_{i,t} \tag{6}$$

[12] For details, see Arellano and Bover (1995).

where $\Gamma_0 = A^{-1}B_0$, $\Gamma_1 = A^{-1}B_1$ and $e_{i,t} = A^{-1}\tilde{\varepsilon}_{i,t}$.

It should be noted that (6) is a reduced form equation. It can be estimated by applying equation-by-equation OLS. The resulting $e_{i,t}$'s, however, are not orthogonal. To identify the structural shocks, $\tilde{\varepsilon}_{i,t}$, we must make assumptions about the structural model in (5) by placing restrictions on the matrix A. As is common in analysis of multiple time series, we use a Choleski decomposition, which forces A to be lower triangular with zero entries below the principal diagonal. This amounts to assuming that the variable ordered first in $\tilde{Y}$ is not contemporaneously affected by the variables ordered below it. The Choleski decomposition is popular with multiple time series analysis because it guarantees enough restrictions to just identify the structural shocks, so under or over-identification is not an issue. As is common in this literature, we provide a robustness check by re-ordering the variables in the VAR to show that the results are not sensitive to ordering of the variables.

Just identification through the Choleski decomposition ensures that we can recover the $\tilde{\varepsilon}_{i,t}$ from the data. This allows us to generate causal inference. Transforming (6) into the moving average representation and substituting for $e_{i,t}$ yields

$$\tilde{Y}_{i,t} = \bar{\tilde{Y}} + \sum_{j=0}^{\infty} \Gamma_1^j A^{-1} \tilde{\varepsilon}_{i,t-j}, \tag{7}$$

The impulse response is therefore given as

$$\frac{\partial \tilde{Y}_{i,t}}{\partial \tilde{\varepsilon}_{i,t-j}} = \Gamma_1^j A^{-1}$$

These impulse responses represent the marginal effect of a shock to $\varepsilon_{i,t-j}$ on the variables contained in $\tilde{Y}_{i,t}$ at time $t-j$. By collecting and plotting the impulse responses for $j = 0, 1, 2, \ldots$ we can observe the dynamic response of a variable in $\tilde{Y}_{i,t}$ to an orthogonal innovation in $\tilde{\varepsilon}_{i,t-j}$.

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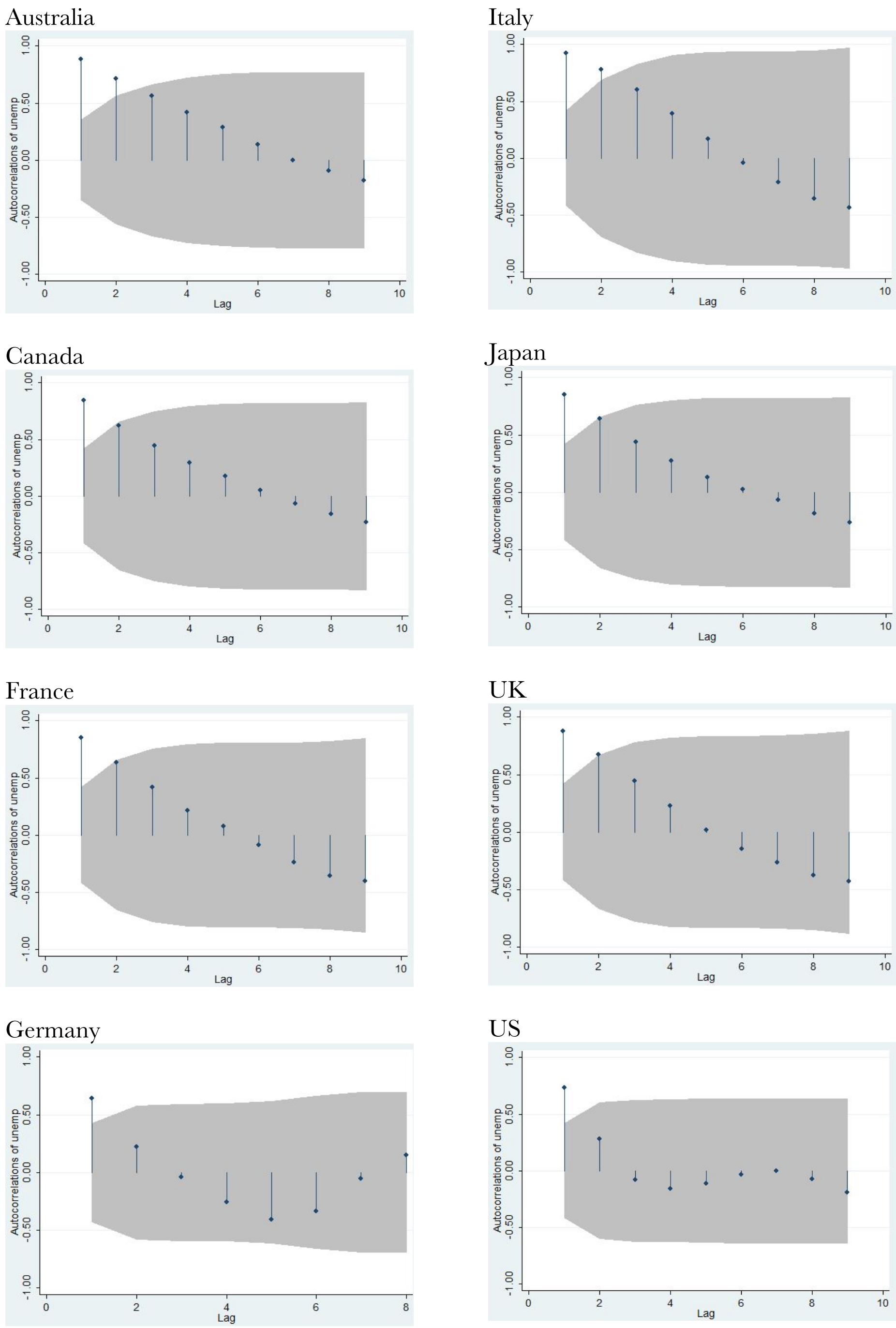

Figure 1: **Autocorrelation functions**. Autocorrelation functions for the unemployment rate in the G-7 countries and Australia.

Table 1: Time averages of unemployment and EPL protection indicies by country

Country	Unemployment	Perm	Temp
Australia	6.96	1.35	0.88
Austria	4.07	2.58	1.31
Belgium	7.98	1.80	3.13
Canada	8.22	1.80	0.25
Chile	7.23	2.62	3.00
Czech Republic	6.23	3.21	0.82
Denmark	6.16	2.15	1.74
Estonia	8.81	2.12	2.06
Finland	9.85	2.31	1.46
France	9.99	2.38	3.60
Germany	8.32	2.74	1.94
Greece	9.89	2.74	1.94
Hungary	8.41	1.99	0.84
Iceland	3.90	1.73	0.625
Ireland	9.22	1.39	0.40
Israel	8.54	2.04	0.88
Italy	9.46	2.75	3.18
Japan	3.94	1.61	1.19
South Korea	3.46	2.59	2.50
Luxembourg	3.32	2.25	3.75
Mexico	3.80	2.19	3.92
Netherlands	4.71	2.89	1.10
New Zealand	6.45	1.40	0.71
Norway	4.12	2.33	3.01
Poland	13.51	2.23	1.15
Portugal	6.63	4.42	2.71
Slovakia	14.40	2.30	1.32
Slovenia	6.72	2.63	1.81
Spain	15.96	2.58	3.23
Sweden	6.97	2.68	1.65
Switzerland	3.47	1.60	1.13
Turkey	9.20	2.36	4.88
United Kingdom	6.78	1.12	0.31
United States	5.97	0.26	0.25
Countries	34		
Observations	651		

Time means and standard deviations for the unemployment and strictness of protection index measures for the 34 countries in the sample. Data covers 23 years from 1990 to 2012. Data for unemployment is collected from the World Bank's World Development Indicators. The strictness of protection measures are obtained from the OECD. The data comes after German reunification in 1990, so we do not need special treatment for Germany.

Table 2: Summary statistics for control variables

	Mean	Standard Deviation	Max	Min
Government expenditures as a percentage of GDP	19.20	4.16	26.89	10.79
Inflation	5.94	8.03	44.72	-0.52
Income per capita growth rate	1.71	1.00	5.05	0.47

Table 3: Simple panel fixed effects estimates for total unemployment

	Policy lag					
	$t = 0$	1	2	3	4	5
Perm	-1.087	0.752	2.173***	3.011***	3.312***	3.035***
	(0.537)	(0.564)	(0.590)	(0.584)	(0.600)	(0.612)
Temp	-0.470***	-0.328	-0.201	0.003	0.006	0.064
	(0.175)	(0.178)	(0.181)	(0.180)	(0.189)	(0.195)
R^2	0.152	0.193	0.190	0.186	0.192	0.206
$Prob > F$	0.000	0.000	0.000	0.000	0.000	0.000
AIC	2516.64	2390.24	2248.72	2106.76	1958.72	1836.41†
SBC	2636.12	2504.04	2356.78	2209.03	2055.24	1927.49†

The results from a simple panel fixed effects model of EPL on the unemployment rate. All regressions include both country and year fixed effects. The independent variable is the unemployment rate, *Perm* is the value of the strictness of employee protection index for permanent workers, and *Temp* is the value for temporary workers. The coefficients for the control variables are suppressed for brevity. The strictness of protection index is lagged between one and five years to show the long-run effects of policy.

* - Estimate is significant at the 10% level; ** - Estimate is significant at the 5% level; *** - Estimate is significant at the 1% level; † - Indicates the best fit as implied by the Akaike Information Criterion (AIC) and the Schwarz-Bayes Information Criteron (SBC).

Table 4: Simple panel fixed effects estimates for long-term unemployment as a percentage of total unemployment

	Policy lag					
	$t = 0$	1	2	3	4	5
Perm	-2.830	-1.679	1.055	1.796	3.390*	5.194***
	(0.1.85)	(1.927)	(1.971)	(1.940)	(1.889)	(1.825)
Temp	0.075	0.0703	-0.022	0.213	0.104	1.495**
	(0.660)	(0.610)	(0.602)	(0.601)	(0.185)	(0.583)
R^2	0.022	0.007	0.020	0.051	0.102	0.128
$Prob > F$	0.000	0.000	0.000	0.000	0.000	0.000

The results from a simple panel fixed effects model of EPL on the long-term unemployment rate. All regressions include both country and year fixed effects. The independent variable is the unemployment rate, *Perm* is the value of the strictness of employee protection index for permanent workers, and *Temp* is the value for temporary workers. The coefficients for the control variables are suppressed for brevity. The strictness of protection index is lagged between one and five years to show the long-run effects of policy.

* - Estimate is significant at the 10% level; ** - Estimate is significant at the 5% level; *** - Estimate is significant at the 1% level.

Table 5: Unemployment Differences and Employment Protection – G-7

Country	Difference in Avg. Unemployment	Fraction Explained by EPL
Canada	2.25	11.0%
France	4.02	8.5%
Germany	2.35	16.9%
Italy	3.49	11.4%
Japan	-2.03	–
United Kingdom	0.81	17.0%
Average		12.94%

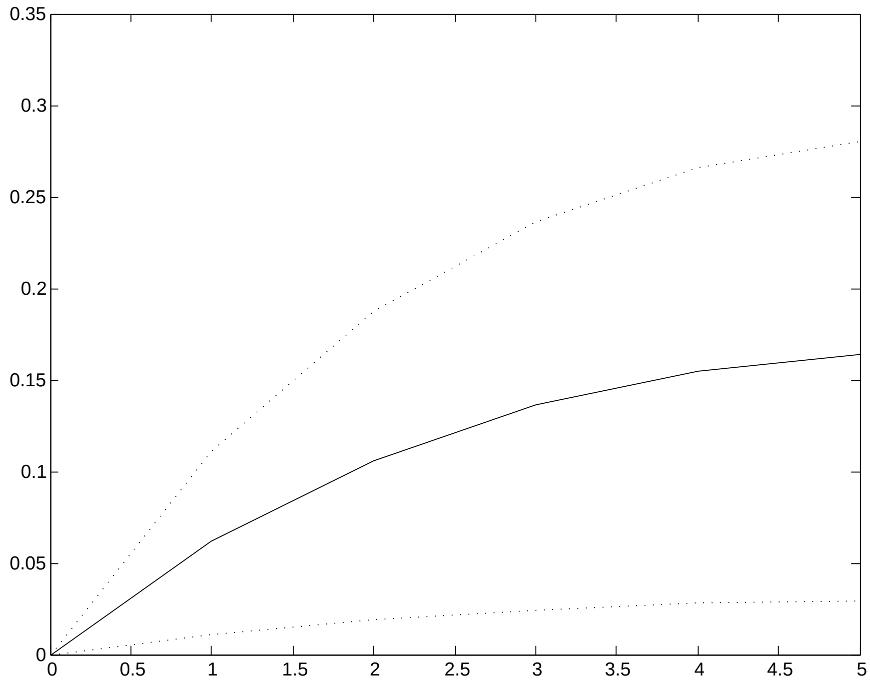

Figure 2: **Response of the Unemployment Rate to a One Unit Shock in Employment Protection for Permanent Workers (No Contemporaneous Policy Effects)**. This figure shows the response of the unemployment rate (vertical axis) to an unanticipated increase in employment protection legislation of one point for permanent workers over time (horizontal axis) in years. This estimate assumes that changes in employment protection legislation have no contemporaneous effect on unemployment. The solid line shows the estimated response. The dotted lines represent the 95% confidence interval.

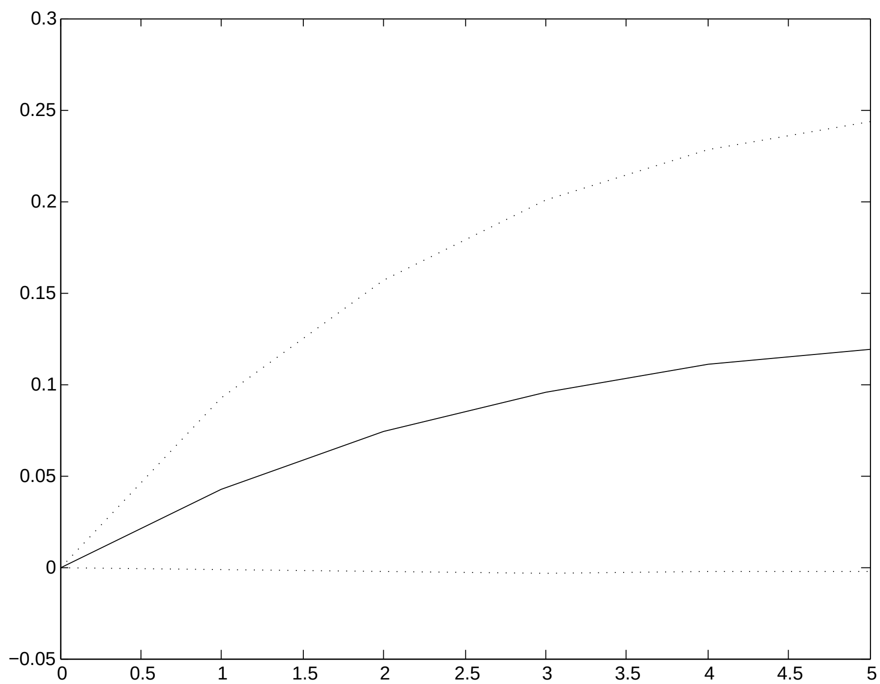

Figure 3: **Response of the Unemployment Rate to a One Unit Shock in Employment Protection for Temporary Workers (No Contemporaneous Policy Effects)**. This figure shows the response of the unemployment rate (vertical axis) to an unanticipated increase in employment protection legislation of one point for temporary workers over time (horizontal axis) in years. This estimate assumes that changes in employment protection legislation have no contemporaneous effect on unemployment. The solid line shows the estimated response. The dotted lines represent the 95% confidence interval.

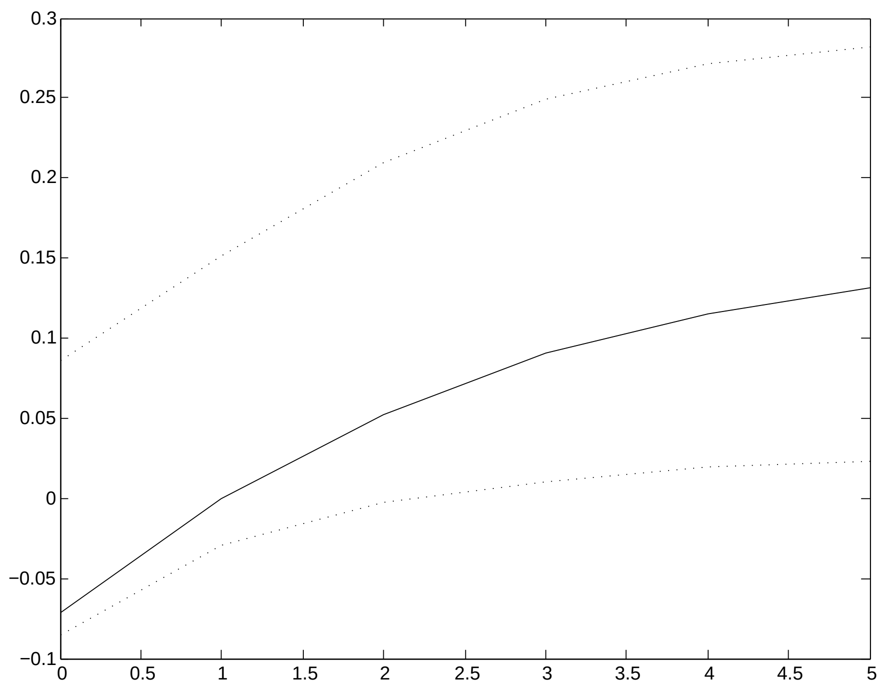

Figure 4: **Response of the Unemployment Rate to a One Unit Shock in Employment Protection for Permanent Workers (Contemporaneous Policy Effects)**. This figure shows the response of the unemployment rate (vertical axis) to an unanticipated increase in employment protection legislation of one point for permanent workers over time (horizontal axis) in years. This estimate assumes that changes in employment protection legislation has a contemporaneous effect on unemployment. The solid line shows the estimated response. The dotted lines represent the 95% confidence interval.

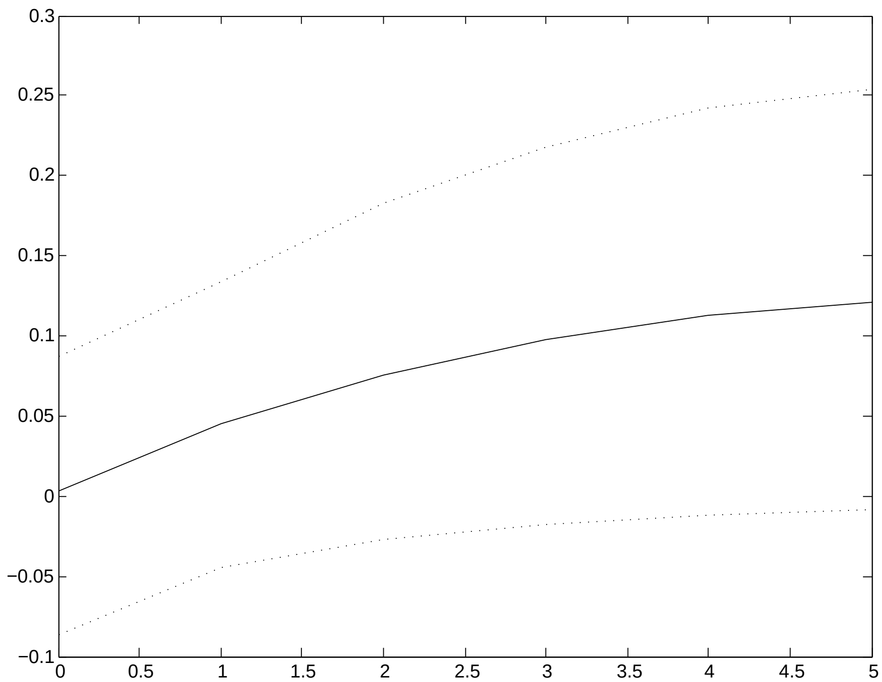

Figure 5: **Response of the Unemployment Rate to a One Unit Shock in Employment Protection for Temporary Workers (Contemporaneous Policy Effects)**. This figure shows the response of the unemployment rate (vertical axis) to an unanticipated increase in employment protection legislation of one point for temporary workers over time (horizontal axis) in years. This estimate assumes that changes in employment protection legislation has a contemporaneous effect on unemployment. The solid line shows the estimated response. The dotted lines represent the 95% confidence interval.