Working Paper 95-48
Statistics and Econometrics Series 16
Economics Series 27
November 1995

Departamento de Estadística y Econometría
Universidad Carlos III de Madrid
Calle Madrid, 126
28903 Getafe (Spain)
Fax (341) 624-9849

DO STRIKE VARIABLES AFFECT WAGE INCREASE SETTLEMENTS IN SPAIN?

Sergi Jiménez-Martín*

Abstract

This paper analyzes the wage increases in a bargaining context using an unbalanced panel from the Spanish Collective Bargaining in Large Firms. Central to the analysis are the joint determination of strike and wage outcomes and the role of strike threats that are not always realized. We find a positive relationship between strike threats and the size of the settlement. We also find that a key characteristic of settlements conditional on strikes taking place is a negative relationship between the length of the strike and the size of the wage increase. We also find that wage increases respond positively to the level and the uncertainty of profits. Finally, we do not reject the possibility of wage determination processes being different for the strike and non-strike samples. These findings suggests that the behavior of employers is either to early "concede" when observing a credible threat or to "resist" a realized conflict. The crucial determinant of the decision is the expected profitability level.

Key Words
Collective Bargaining; Wage Increase; Strike; Unions; Panel Data.

*Departamento de Economía y Departamento de Estadística y Econometría, Universidad Carlos III de Madrid. I am very grateful to Miguel A. Delgado, Jaume García, Larry Kranich, Praveen Kujal, José M. Labeaga and Víctor Ríos-Rull for helpful comments. Financial help from FIES and DGICYT PB92-1036-C02-01 is acknowledged.

I. Introduction.

The relationship between strike and wage outcomes has been stressed in many theoretical studies.[1] One strand of the literature argues that strikes are accidents, or mistakes, that occur during bargaining.[2] The other, more dominant line of research, based on the seminal work of Hicks (1932) stresses a negative relationship between the length of a work stoppage and the wage settlement (also called the "concession schedule"). Among the different theories that predict this relationship one-sided asymmetric information (OSAI) models are the most popular.[3] OSAI models provide a rational explanation for the existence of strikes during negotiations suggesting that a strike is used as a revealing mechanism, in the presence of asymmetric information, about the level of profitability of the firm.

Using microeconomic data the empirical literature in the field has found ambiguous results regarding the slope of the concession schedule. One line of the literature consider a wage equation controlling for both the ocurrence and the observed length of a strike.[4] Card (1990b) and McConnell (1989) have used panel data to control for the presence of time-persistent unobserved heterogeneity. Only McConnell finds evidence of a negative relationship. The other considers two equations, one for each strike regime, at most accounting for self-selection.[5] Stengos and Swidinsky (1990) assume a joint determination process and detect selection in the wage equations induced by the strike outcome. However, none of the papers fully considers the set of potential issues that the joint determination of strike and wage outcomes and the dynamic nature of the negotiation process raise, such as, the endogeneity of the strike variables, relevance of strike's threats, selection and, possible dynamics.

Concerning endogeneity and threats, Hayes (1984) states, in the context of an OSAI

---

[1] See Kennan (1986) for a review of earlier work and Card (1990a) for a review of recent microeconometric work.

[2] See, for example, Siebert and Addison (1981).

[3] Other theories to mention are the Union-Political Model of Ashenfelter and Johnson (1969) and the Joint Cost Theory of Reder and Neumann (1980) and Kennan (1980). For a review of these and OSAI models see Kennan and Wilson (1993).

[4] See for Canada Ridell (1980), Lacroix (1986) and Card (1990b). For the US, see Vroman (1984) and McConnell (1989).

[5] See Fisher (1989) and Stengos and Swidinsky (1990), both using Canadian data.

model, that a work stoppage is an endogenous outcome of the rational behavior of employers and unions. Moreover, agents negotiate a wage settlement but with a strike threat which the union can carry out or not. In such a case threats should play an important role in the determination of the settlement. Regarding selection, Cramton and Tracy (1992, 1994) stress that the employees could use other actions besides strikes. These actions can lead to different wage equations. Finally, a problem that has attracted little attention is concerning the dynamics of settlements in the presence of strikes (realized or threatened). Bargaining is a dynamic process as it depends on what happened in the past and the expectations in the future.

This paper, using an Spanish data set, extends the empirical literature further by emphasizing specification and testing issues. That is, we raise the following questions. First, should the strike variables be endogenous, or exogenous, to the wage setting process? Second, which variables capture best the effect of a strike, observed outcomes, or underline threats? Third, should we consider one, or two different, wage equations (one for each strike regime)? Finally, after controlling for unobserved heterogeneity, does it remain a self selection problem?

In regard to the econometric methods, the relevance of the endogeneity problem can be assessed, using panel data, by means of a Hausman test (Griliches and Hausman, 1986). This is done by comparing the obtained estimates under alternative set of instruments. To answer the second question, we estimate the model by replacing strike outcomes by using the corresponding predicted threats. A Wald test comparing the estimates of each equation detects any difference in the behavior of firms in each strike regime. Finally, to detect selection, we perform a test for selection bias.[6]

We use "La Negociación Colectiva en las Grandes Empresas" (Collective Bargaining in Large Firms, NCGE), a yearly survey on bargaining issues. It provides data on initial bargaining positions, negotiation timing, strike activity, wage increases, and other variables (see the Appendix).

[6] This type of test has been proposed by Wooldridge (1995).

The paper is organized as follows. In section II, we summarize the theoretical foundations and the applied work. The empirical framework, the econometric and testing methods used in this paper are outlined in section III. The empirical findings and the sequence of tests are discussed in section IV. Section V summarizes.

II. The theoretical foundations and the empirical literature.

There have been several attempts to circumvent the well-known *Hicks Paradox*[7] on the optimality of strikes, which implies that strikes result from faulty negotiations. Early studies focused on finding alternative explanations for the rationality of strikes appealing to asymmetries between union leaders' and members' expectations. Ross (1948) postulated a *Union Political Model* by recognizing that union leaders are motivated by personal advancement and the growth of the union. Ashenfelter and Johnson (1969, p.39) noted: "*The basic function of the strike is as an equilibrating mechanism to square up the union membership's wage expectations with what the firm is willing to pay.*" Thus, the lack of information by union members about what the firm is willing to pay causes strikes. Kennan (1980) and Reder and Neumann (1980) developed a simple theory in which the existence of bargaining costs for both agents explains both strike incidence and duration. They stated that both the likelihood of a strike and its expected duration are lower the higher the *joint cost* of a strike to the firm and its employees. Although these theories yield valuable predictions for empirical testing they lack a rational foundation for the process that determines a work stoppage.

Recently, one of the important developments in non-cooperative bargaining has been a private-information theory of disputes (Morton, 1983, and Hayes, 1984) that argues that the rationality of strikes comes from asymmetries in the information set that each bargainer faces while negotiating.[8] The union uses the strike mechanism towards

[7] The Hicks paradox is implicit in Hicks' (1932) discussion of strikes: "If there is any theory which predicts when a strike will occur and what the outcome will be, the parties can agree to this outcome in advance, and so avoid the cost of a strike. If they do this, the theory ceases to hold." (quoted from Kennan, 1986).

[8] However, Fernández and Glazer (1991), among others, show that rational lengthy strikes are still possible in symmetric information games.

gaining more information about the level of profitability of the firm. The contribution of these models is twofold. On theoretical grounds, they provide a rational explanation for the occurrence of strikes, as strikes can be Pareto optimal ex-ante. From the empirical point of view, they preserve all of the implications of earlier models. Recently, Cramton and Tracy (1992, 1994) have considered alternative union actions, such as the delay in negotiation. Their model predicts that the union substitute work stoppages for delays if, for instance, the alternative wage have worsened of the expected profitability of the firm has declined.

Unfortunately, the empirical implementation of these models is constrained by the inadequacy of the available data. In fact, a significant amount of applied work has only been done for Canada and the US.[9] Using Canadian data, in a single equation context, Ridell (1980) and Lacroix (1986) find that strikes imply significantly higher wage increases. However Lacroix points out that the finding does not survive to an unrestricted treatment of the year effects in the wage equation. In the same context, using panel data, Card (1990b) finds virtually no relationship between strike duration and expected wages. In a two equation framework, Fisher (1989) considers an adverse selection model of wages and strikes and Stengos and Swindinsky (1990) find evidence of selection induced by the strike outcome. Microeconometric research using US data is more recent. Striking examples are Vroman (1984) and McConnell (1989). Whereas Vroman finds no relationship between strike duration and wages, McConnell finds a negative relationship between strike duration and the unpredictable component of the wage.

III. Economic, econometric framework and methods.

a. Spanish negotiation framework.

Bargaining procedures in Spain, similarly to other European countries, are quite different from those in the US or Canada.[10] Bargaining occurs at the industry-wide and

[9] In Europe research has focused on strike determination (see Franzosi, 1989, for a summary of early research). Recently van Ours and van de Wijngaert (1993), using data from the Netherlands, analyze a combined wage and holdout model.
[10] See Jimeno and Toharia (1994) for a description of the Spanish industrial relations system.

firm level simultaneously. The terms of industry-wide agreements being a binding floor for all the firms in the sector, i.e., the so-called "mandatory extension" principle. Union's affiliation is low but its power is high because unions carry negotiations at industry-wide level. The coverage of the system is notably high. In 1984-91 almost 82 per cent of all employees were covered by collective agreements, 20 per cent of these correspond to firm-level agreements in large firms.

Most employees and the employer have an indefinite contract. Current working and pay conditions are settled in an additional protocol called "convenio" that stipulates wages, hours of work and covers a number of years. However, wage increases are negotiated, or renegotiated, almost yearly. For the sake of simplicity, we assume a yearly renegotiation. Elected work councils substitute unions in firm-level negotiations and, as a major difference to other European countries, they can call for a strike. In our sample, above 80 per cent of the bargaining units carry over some formal or informal negotiation process. The rest apply a sectorial agreement. It seems that the main motivation of the decision to negotiate at the firm-level bargaining in Spain is to distribute firm-specific quasi-rents (Palenzuela and Jimeno, 1995).

The negotiation at the firm-level proceeds as follows. It starts when the council makes a wage increase *Claim*. The institutional setting is such that the firm must counteroffer immediately. If this *Offer* equals the above *Claim*, there is an immediate agreement. If not, they alternate offers until they reach an agreement. In the meantime, the council uses a latent strike threat. There are several institutional features which condition this threat during negotiation. First, it is unusual to call a strike before both the *Claim* and the *Offer* have been announced. Second, the councils must compulsorily announce in advance to the firm the starting date of the strike. Moreover, they announce the length of the work stoppage or, alternatively, they announce an indefinite threat. Third, it is forbidden for the firm to hire temporary replacement workers. Finally, workers must compulsorily guarantee a minimum service level in some key industries. Undoubtedly, these institutional features greatly affect what is observed. For instance, the announcement could lower observed strike activity (and increase the relevance of credible threats) because the firm could react to a formal threat avoiding a costly work stoppage.

b. A framework for analysis.

Most OSAI models postulate that strike (or delay) duration is determined by the time required to credibly establish that the employer's demand price of labor is no higher than the truth. Then, the wage settlement splits the difference between the demand and supply prices. Card (1990b) and Cramton and Tracy (1992, 1994) consider alternative models in a non-dynamic context. While the first concentrates on the relationship between strikes and wages, the second considers also the incidence of delaying the agreement. Nonetheless, both papers agree with the general idea that longer strikes should produce lower observed wages. Thus, the test of this implication seems to be independent of the framework.

In our benchmark model, which generalizes an stylized econometric model described in Kennan and Wilson (1989), the wage increase settlement (Δw) depends on a set of strike variables (*SV*), observed variables (x) and an unobserved component (e). The unobserved part is composed of a term (m) which randomly varies across observations, which is known by the parties but unknown to the econometrician and that can be trough as an individual heterogeneity effect and a second unobservable component which shift firms valuations (h). Summarizing,

$$\Delta w_i = f(SV) + x_i\gamma + e_i \tag{1}$$

$$e_i = m_i + h_i \tag{2}$$

where the function f captures "the resistance curve." Depending on the exact set of assumptions about what f is and the variables it includes we are in front of one type of empirical model or another. McConnell (1989) and Card (1990b), both using panel data, consider a linear version of the resistance curve like:

$$\Delta w_i = \delta_1 s_i + \delta_2 d_i + x_i\gamma + e_i \tag{3}$$

where s takes the value one if a strike is observed and zero otherwise, and d is the realized length of a work stoppage. Note that $\Delta w(d=0)$ can be through as the maximum wage increase available for workers, i.e. a corner solution of the wage with a strike. In such a context, Card (1990b) finds no evidence of any systematic relationship between wage outcomes and strike variables, at least for a vast majority of strikes; whereas McConnell (1989) finds some evidence to support a negative relationship. In both cases a control for the individual heterogeneity is introduced. However, in both cases the strike variables are treated as exogenous, ignoring the fact that strike and wage outcomes are determined in a joint process. As far as strike realizations could be correlated with the error term in (3) a negative simultaneity bias[11] is expected to arise when estimating without instrumenting the strike variables.

Apart from this, notice that agents negotiate a wage increase but with a latent strike threat which the union could carry out or not. In such case, there is a serious doubt over which variables better capture the effect of a strike: outcomes or threats. In particular, reputation may explain why threats instead of outcomes are important in the model. Consequently, we should consider the alternative specification,

$$\Delta w_i = \delta_1^* s_i^* + \delta_2^* d_i^* + x_i \gamma + e_i \qquad (4)$$

where s_i^* and d_i^* are the underlining strike and strike's length threats, respectively. Moreover we should also consider further specifications. For instance, the one that nests (3) and (4) which implies combined dependence of threats and outcomes. Alternatively, Cramton and Tracy's model with multiple threats (i.e., strike and delay) suggests that each threat leads to a different wage equation which, simplifying, we consider that are related to the strike indicator as follows:

$$\Delta w_i^0 = x_i \gamma^0 + e_i^0 \qquad \text{if } s_i=0 \qquad (5.0)$$
$$\Delta w_i^1 = \delta_2^1 d_i + x_i \gamma^1 + e_i^1 \qquad \text{if } s_i=1 \qquad (5.1)$$

[11] Naturally we have uncertainty about the sign of such a correlation. However it is reasonable to observe that positive errors of the wage are associated with negative ones of the strike incidence or duration.

where s_i takes the value one if $s_i^*>0$. Thus, the strike indicator determines whether we observe the wage with strike (Δw_i^1) or without it (Δw_i^0). The latter can be through as the wage under an alternative threat. In such a context, Stengos and Swidinsky (1990), using cross-section data, find evidence of selection induced by strike outcomes.

c. Econometric specification and methods.

Before elaborating the econometric treatment, notice that there are at least two potential sources of dynamics in the model. On the one hand, learning or reputation may influence the current outcome of the negotiation process. On the other hand, a single negotiation is embedded in an indefinite negotiation process. Consequently, there is no reason to expect that current negotiation can be isolated from past (or future) negotiation rounds. Thus, consider, in a panel data context, a dynamic version of equation (3), which is also valid for (4):

$$\Delta w_{it}= \alpha\Delta w_{it-1}+ \delta_1 s_{it}+ \delta_2 d_{it}+ x_{it}\gamma + \mu_i+ u_{it}; \qquad t=1,..,T_i;\ i=1,..,N \quad (6)$$

According to (1) we have decomposed the error term into a firm specific effect, μ_i, and a mixed error, u_i. Firm specific effects stand for persistent unobserved information and/or variables with very few time series variation. Notice that, by construction, at least lagged outcomes are correlated with the effects. Moreover, x could include some variables potentially correlated with both μ_i and u. Consequently, the model in levels does not easily allow us to obtain consistent estimates of the relevant parameters. Alternatively, they might be obtained by applying an instrumental variables method to the first differenced version of (6):

$$\Delta\Delta w_{it} = \alpha\Delta\Delta w_{it-1} + \delta_{11}\Delta s_{it} + \delta_{12}\Delta d_{it} + \Delta x_{it}\gamma + \Delta u_{it} \qquad (7)$$

Provided n is large and t fixed, we consistently estimate (7) using a general method of moment instrumental variables (GMM-IV) estimation procedure by Arellano and Bond (1991). Under the assumption that the error term in (6) is serially uncorrelated, all variables dated t-2 and earlier are, as a rule, valid instruments for (7).

As far as first differencing implies in our case the renounce to an important share of the sample for estimation it is important to test whether the model in levels produces consistent estimates, which is equivalent to test the importance of firm specific effects. We use a Sargan-difference test proposed by Holtz-Eakin (1988) and Arellano (1993) for autoregressive models and extended here to account for the presence of endogenous regressors. Under the null that the specific effects are negligible, both the model in levels and in differences provide consistent estimates. This test accounts for the lack of orthogonality between the errors in levels and a set of additional instruments. The test is distributed as a χ^2_r, where r is the number of additional orthogonality restrictions implied by the model in levels.

Regarding other testing procedures, we assess the endogeneity of the strike outcomes by comparing the IV estimates of the wage equation (6) obtained using the set of instruments valid under the null of endogeneity (current and lagged values) and under the alternative of exogeneity (only lagged twice values).[12]

The above approaches produce consistent estimates provided that each strike regime does not lead to a different wage equation. In such an alternative case we should consider the estimation of the following dynamic extensions of (5.0) and (5.1):

$$\Delta w^0_{it} = \alpha^0 \Delta w_{it-1} + x_{it}\gamma^0 + e^0_{it} \quad \text{if } s_{it}=0 \qquad (8.0)$$

$$\Delta w^1_{it} = \alpha^1 \Delta w_{it-1} + \delta^1_2 d_{it} + x_{it}\gamma^1 + e^1_{it} \quad \text{if } s_{it}=1 \qquad (8.1)$$

notice that as a simplifying assumption we have considered the lagged wage increase outcome to be the relevant dynamic term. The same considerations we have made for estimating equation (3) still apply, excepting that we should take into account that,

$$E(\Delta w^0_{it}/s_{it}=0) = E(\Delta w^0_{it}/s^*_{it}\leq 0) = x_{it}\gamma^0 + E(e^0_{it}/s^*_{it}\leq 0) \qquad (9.0)$$

$$E(\Delta w^1_{it}/s_{it}=1) = E(\Delta w^1_{it}/s^*_{it}\geq 0) = x_{it}\gamma^1 + E(e^1_{it}/s^*_{it}\geq 0) \qquad (9.1)$$

where in general neither $E(e^0_{it}/s^*\leq 0)$ nor $E(e^1_{it}/s^*\geq 0)$ are expected to be zero. Assuming that the errors in the wage increase and an underline strike decision equations are

12 This kind of testing procedure have been proposed by Griliches and Hausman (1986) for an error in variables models. Recently, Keane and Runkle (1992) have proposed a similar test devoted to detect the presence of correlated effects.

jointly normal we undertake this problem, following Heckman (1976), by estimating a model for the decision to strike and then correct the wage equations with the corresponding inverse Mill's ratio. Thus, adding some consistent estimates of the inverse Mill's ratio, say $\hat{\lambda}_{w0}$ and $\hat{\lambda}_{w1}$, to (8.1) and (8.2), respectively, we have:

$$\Delta w^0_{it} = \alpha^0 \Delta w_{it-1} + x_{it}\gamma^0 + \sigma_{w0}\hat{\lambda}_{w0} + u^0_{it} \qquad \text{if } s_{it}=0 \qquad (10.0)$$
$$\Delta w^1_{it} = \alpha^1 \Delta w_{it-1} + \delta^1_2 d_{it} + x_{it}\gamma^1 + \sigma_{w1}\hat{\lambda}_{w1} + u^1_{it} \qquad \text{if } s_{it}=1 \qquad (10.1)$$

Concerning test procedures, we compare the estimates (excluding the intercept and the strike variables) of (8.0) and (8.1) using a Wald test to account for the possibility of different structural parameters. Finally, we perform a variable addition test for selection bias. The procedure, following Wooldridge (1995), may be stated as follows. First, we estimate T decision equations using standard discrete choice models. Then, we compute the inverse Mill's ratio for each observation in each time period. In a second stage, we estimate by IV (8.0) and (8.1) adding the selection term. Finally, we test the null that the effect of the selection term is zero. Given that some of the explanatory variables are predetermined or endogenous and given the possible existence of relevant specific effects, the test is conducted in both level and differenced models. Unfortunately, given the small sample size of the strike subsample, differenced estimates are only obtainable using the non-strike sample. However, the test of selection is still valid as noted by Wooldridge (1995), for selection, if present, can be detected in any subsample.

IV. Empirical results.

a. Data and variables.

The data source we use in this study is the NGCE which collects information about collective bargaining of firms with more than two hundreds workers. The available sample covers a time span of 6 years, from 1985 to 1990. From the row data set, we select those observations which contain information about claim, offer, agreement and the length of the negotiation process. It is necessary to select these data in order to be able to use some controls concerning price expectations and wage signals, which we consider as key variables in our framework. The Data Appendix provides the definition of the variables and some descriptive statistics.

The first group of variables we consider are proxying the change in firm's demand price for labor. The change in the level of added value per employee stands for the change in the level of productivity and, in some sense, firm's demand. We include the lagged level of profits per employee in order to control not only the change but the level of profitability. Both are expected to add upward pressure to settlements. In addition, we include the percentage of sales in the local market as an indirect measure of competitive pressure. Although we cannot construct a direct measure of profits or productivity volatility, we use the difference between the union initial claim and the firm initial offer as a proxy of the level of uncertainty that agents have at the beginning of the negotiation process. We use the share of capital in hands of foreign agents, of the public sector and of national agents in order to account for differences in the bargaining power of the firms.

The difference among work councils bargaining power as well as the characteristics of the bargaining unit, its pay structure, the timing of negotiations and the incidence of the market conditions have also been considered. We accounted for any potential difference among the effect of any union within the work council by including the percentage of the members of the council that belong to the nationwide union "comisiones obreras" (workers commissions, CCOO), any regional union and those that do not belong to any union. We also consider a dummy which accounts for the presence of a single union in the work council. A single union has no coordination problems and, as a result, could have greater negotiating power. In order to capture the effect of the negotiations' timing we have included a dummy which takes the value one if the negotiation process starts after the expiration of the last agreement. Bargaining unit *status quo* may be well-represented by the lagged relative wage. The size of the bargaining unit is controlled by the lagged level of employment. We also include the concession of a cost of living allowance clause, which is expected to lower non-contingent settlements.

In regard to the incidence of the market conditions, the industry average of days lost by strike per employee acts as a proxy for the aggregate bargaining pressure and offers an excellent source of instruments. An increase in the regional unemployment rate or a decrease in the change of industry employment produce a drop in the alternative wage, and thus should decrease settlements. The higher the expected price level, the higher is expected to be the settlement. Moreover, the industry average settlement, in the month preceding the signing, stands for the information that agents have about other bargaining units actions and it could capture the wage spillover (McConnell, 1989). It should contribute to the improvement of our specification in at least two directions. First, it offers some demand information not directly observable to the econometrician at industry and regional levels. Second, other firms' wage settlements may enter directly into wage negotiations through the reservation wage and/or the profit function. Finally, we also consider carefully the possibility of wage dynamics as well as a set of calendar (year and seasonal) and industry dummies.

b. Results and testing.

We present in Table 1 and 2 some GMM-IV estimates of the level and first difference wage increase equation, respectively. In column (1) of both tables, neither decision nor strike duration are instrumented. In column (2) both variables are instrumented using lagged values dated t-2 and earlier, while in (3) these instruments are replaced by threat' predictions.[13] Estimates restricting the sample to non-strike and strike observations are reported in Table 1(4) and 2(4) and Table 1(4), respectively. Finally, Table (3) reports several specifications including threat predictions. The first three columns are estimates of the levels model and the rest of the differenced model. In columns (1) and (4), we replace strike outcomes by threat

[13] The predictions of the strike indicator are obtained using a year-by-year Probit and the (unconditional) predictions of the strike duration in a selectivity model (see Jiménez (1995)).

predictions. In columns (2) and (5), we jointly consider threats and outcomes. And a combined specification using the realized duration and the strike decision threat is reported in columns (3) and (6).

[Table 1 here]

Regarding the results of Table 1, the first thing to note is the importance of dynamics in the determination of the wage increase in those equations where we do not control for the heterogeneous effects. When we drop out the lagged wage increase of the equation, all the tests show severe misspecification problems.[14] Second order serial correlation test detect that heterogeneous effects are not fully removed. The presence of this kind of serial correlation invalidates level estimates.

In regard to the estimates of the differenced model the statistical results are as follows. First, the existence of effects test,[15] significant across all the columns, and the non-significance of the coefficient of the lag of the endogenous variable confirm the importance of the heterogeneity of the bargaining units in the negotiation process. Second, we find first order serial correlation as well as absence of second order correlation. Thus, the error in levels is white noise (Arellano and Bond (1991)). Finally, the test for over-identifying restrictions shows the adequacy of the set of instruments used. Thus, the differenced model is preferable.

[Table 2 here]

Concerning the set of questions formulated, first, the Hausman test which compares the set of estimates of columns (1) and (2) of Table 2 (also of Table 1) shows an endogeneity problem of the strike variable and, moreover, a downward bias of both coefficients. Second, we reject the null of equal coefficients between the coefficients obtained using the overall sample and those obtained using the non-strike observations.[16] Third, we detect endogenous selection in the wage equation induced by

[14] Although we do not present these results they are available on request.

[15] Due to the large number of firm's variables, all of them potentially correlated with the effect and/or the error term, there are a large number of overidentifying restrictions to test. However given the small sample size relative to the number of overidentifying restrictions we combine the test of restrictions like $E(x_{t-1}\varepsilon_t)$ for the important variables (lagged wage increase and strike outcomes) with restrictions like $E(\Sigma x_{t-1}\varepsilon_t)$ for the rest of the firm variables.

[16] The statistic is 205.8 which is distributed as a χ^2_{24}. A similar test, but comparing the strike and non-strike subsamples, is rejected in the model in levels.

the strike outcome. Column (4) of Table 2 reports the results corresponding to the non-strike subsample. A variable addition test over the augmented model with the Mill's ratio rejects the null of the absence of sample selection bias.[17] In fact, there are important differences between strike and non-strike regimes in the wage increase as a comparison between any of columns (1) to (3) and (4) shows.

The previous comments imply that strike outcomes are endogenous. However, it does not affect dramatically the results obtained in this work, as a comparison of columns (1) and (2) of Table 2 shows. This could be due to the low quality of the instruments used for the strike variables, which are zero in a great proportion. Column (3) of Table 2 (also of Table 1), has been estimated using strike's threats instead of realizations as instruments. This experiment increases both the significance and magnitude of the coefficients of the strike variables.

All the specifications that include strike threats show, in general, satisfactory results. The effect of the decision threat is positive and, as a rule, significant across all the columns. On the contrary, the effect of the duration threat is less clear. This can be due to the fact that the prediction of the duration threat are much more inaccurate that those of the strike decision threat. In our opinion, column (5) reports the most striking results. They imply that threats put upward pressure on settlements and, simultaneously, the observed length implies a decrease of settlements. Thus, it suggest that firm "concede" in front of credible threats and, conditionally, "resist" to realized work stoppages.

[Table 3 here]

From an economic perspective, we mention the robustness of the duration effects. This variable, which is instrumented with a subset of the available moments (dated t-2 to t-4), shows the expected negative sign in all the cases considered. Thus, there is evidence in favor of a downward sloping wage-concession schedule. The coefficients of

[17] The non-strike subsample includes those firms which have suffer no strike for at least two consecutive years. However, as Wooldridge (1995) shows, the presence of endogenous selection can be tested without worrying about the adequate specification of the equation and considering any of the subsample. We cannot conduct a test for sample selection with the firms in the strike regime because there are not a sufficiently large number of observations.

Table 2(2) imply that strikes lasting more than 2 weeks reduce the wage increase, for instance. Moreover, after a long strike, say one month, the settlement is reduced by 5.0 per cent. However, short strikes still produce higher wage increases because of the positive coefficient of the strike indicator. Notice that this coefficient may be hiding structural differences between the strike and non-strike settlements equations.

The results confirm the relevance of the difference between the initial claim and offer, which is translated in a greater proportion into wage increases for the non-strike sample. This implies greater concession on the part of strikers. There is also a significant difference in the settlement achieved by the proportion of regional representatives and the omitted group. The concession of a cost of living allowance clause, significant in most columns, decreases the settlement. The size of the bargaining unit seems to reduce the settlements in the case of the non-strike sample, because of the fact that larger negotiation units are able to diversify negotiation issues (by considering bonuses and/or productivity payments). It also seems that those employees with lower relative wages achieve higher wage increases, in accordance with most of the previous work (c.f. Card (1990a)). However, this evidence does not survive to the substitution of either strike outcomes or instruments by threats.

Regarding the firm variables, both the level and dummy of positive lagged profits are both, as expected in the context of the OSAI theory, been found positive. Thus, more profitable firms tend to grant workers with a relatively higher wage increase. In regard to the change in the added value per employee we obtain contradictory result, through in general appears to be positive, overall in specification in which threats including threats. Estimates of the differenced model indicate that the greater the proportion of sales in the internal market, the lower the settlement. Thus, exporting firms suffer stronger wage increase pressure from strikers. Finally public-owned firms pay a wage increase premium with respect to foreign-owned firms. This may be due to the fact that public-owned firms in Spain in those years used not to worry excessively about performance and competitiveness and, in addition, they had very powerful unions.

The aggregate and industry indicators show contradictory results. While price and conflicting variables have a highly significant coefficient, both the coefficient of the unemployment and employment variables, despite showing correct signs, are insignificant. Industry average strike incidence levels are positively correlated with settlements, evidencing a spillover effect. The industry average wage increase and the expected inflation level strongly influence the agreement. Notice also that the expected inflation level is much more significant for strikers than non-strikers. This reflects the fact that strikers try to link their settlement to the relevant conditions of the market, evidencing the fact that the current system of wage bargaining leads to an inflationary bias.[18] This constitutes clear evidence in favor of the existence of some sort of wage rigidity in Spain caused, amongst other reasons, by the combination of the structure of the collective bargaining system, and high firing costs for part of the labor force.

c. Strike/non-strike wage differential and the wage decline

The results we have obtained about the wage increase setting permit us to obtain some conclusions about the implicit wage increase differential among both strike regimes and the magnitude of the decline of the wage increase. Both sets of results are drawn in tables 4 and 5, respectively. As in Stengos and Swidinsky (1990), we use the set of parameter estimates, including the selection terms, reported in Table 1(4) and 1(5) in order to calculate the wage increase differential.

[Table 4 here]

Our sample mean corrected differential is 0.33 percentage points. The uncorrected estimate is roughly the same, because of the small effect of the selection terms. Our result is very close to Stengos and Swidinsky's estimate, 0.36, for a set of Canadian contracts. By industries, the largest differences appear in the Minerals and Chemical

[18] In words of Blanchard, Jimeno et al. (1995): "In the current system, each level of bargaining establishes a floor on the wages which can be set at the lower level. Sectorial-level bargaining in effect sets a wage floor on firm-level agreements, which can either set wages at the floor, or at a higher level. Thus, firms which are doing well can pay higher wages, but firms which are not doing so well are prevented from paying lower wages. The result is a wage setting system with an inflationary bias. The problem is likely to be particularly acute in times when more re-allocation is needed, as has been the case in Spain with the rapid increase in openness and foreign trade".

and the Retail Services, while the lowest appear in the Energy and Utilities industry. Finally, note that the estimated differential sharply decreases with the length of a work stoppage. In this sense, after a conflict of two weeks, it falls by two thirds.

[Table 4 here]

The evidence about the decline of the wage settlement is consistent across models. All of them identify a negatively sloped concession schedule. Despite the negative slope, the joint effect of the strike decision and duration is positive for short strikes and negative for long ones. The decline of the wage increase is set between a low of 2.6 per cent and a high of 10.0 per cent, after a month. However, in terms of wage levels this decline is rather small. The estimated range for a strike of a month is 0.3 to 0.7 per cent. In a previous study for the US, McConnell (1989) found a wage level decline of 3.0 per cent after a conflict of 100 days, which is slightly above the upper bound of our preferred estimate for a strike of 100 days, 2.28 per cent. We would stress that a strike of 100 days is rarely observed (less than two per cent of the cases). Some exploratory results by sector suggest that the wage decline is sharpest for the manufacturing sector, although the difference between both sectors is not significant.[19]

V. Summary of findings and main conclusions.

This study focuses on the analysis of the empirical relationship between wage settlements, strike outcomes and threats using Spanish data from the NCGE. The analysis has emphasized specification, estimation and testing procedures.

With regard to specification issues we reject the exogeneity of strike outcomes and the relevance of dynamics and confirm the presence of self-selection induced by strike outcomes. We detect endogeneity of the strike decision although its incidence is not extremely important. The importance of the dynamic term diminishes when we consider a correct setup (first differenced model controlling for unobserved

[19] We have replicated Table 2(2) interacting a services dummy with the strike decision, duration and the set of time dummies. Both strike coefficients are lower (in absolute value) for services. The full set of results of such an exercise are not reported but are available on request.

heterogeneity). In fact, we obtain some evidence that a dynamic specification may hide the presence of heterogeneity (at least in levels models). Although, the existence of effects test detects relevant individual heterogeneity, the power of the test is very low and sensitive to the set of instruments. Finally, we found that there is self-selection and/or differences in some coefficients among strike regimes. Thus, a two-equation framework seems to be preferable over a single-equation framework, but impedes a proper identification of the strike wage increase equation because of the small sample size.

The set of estimates about the wage increase equation suggests that uncertainty and profitability measures are important determinants of the settlement. Specially interesting is the result we have obtained on the effect of the difference between the initial claim and offer. Regarding aggregate factors, the proxies for the available information, price expectations and the industry average wage increase, have a greater influence than market factors, like suggesting some sort of wage rigidity in Spain.

Although, we have not been able to consistently estimate a strike equation, we have found a lot of evidence in favor of the most important prediction of many strike models. We have found that the wage increase settlement declines with the strike duration. However the effect is not very important. Moreover it does not compensate, except for very long strikes, the positive effect on wage increase settlements that the strike indicator an also the work stoppage threats have.

To conclude, we stress that our findings suggest a duality of strikes: short and long. Short strikes produce a boost on wage settlements. Thus, short strikes act as an enforcement mechanism (strikes as accidents?). Whereas, long strikes yield a wage agreement concession on the part of the workers. Thus, they act as a revelation mechanism. Finally, we would to stress that strike threats matter. The relevance of threat could explain, among other well-known reason, why we observe so few work stoppage during wage negotiations.

Table 1. Wage increase determination and strike outcomes. Model in Levels.

	(1) coef. t-st.	(2) coef. t-st.	(3) coef. t-st.	(4) coef. t-st.	(5) coef. t-st.
CONSTANT	-0.74 (1.25)	-0.91 (1.53)	-0.72 (1.10)	-0.17 (0.24)	-0.69 (0.55)
Δw(-1)†	0.36 (16.0)	0.36 (15.6)	0.37 (15.1)	0.38 (15.0)	0.36 (8.06)
DCO†	0.02 (6.05)	0.03 (5.61)	0.02 (3.38)	0.01 (0.66)	0.04 (3.38)
s (†in 2)†	0.331(6.47)	0.230(1.96)	0.544(4.17)	--	--
d(† in 2-3)†	-0.012(2.74)	-0.032(5.69)	-0.051(6.23)	--	-0.018(1.96)
$\hat{\lambda}$¥	--	--	--	0.03 (1.83)	-0.39 (3.66)
SINGLEUN	-0.20 (2.63)	-0.18 (2.31)	-0.22 (2.78)	-0.13 (1.53)	-0.22 (1.16)
CCOO	0.24 (3.05)	0.17 (1.98)	0.19 (2.16)	0.17 (1.75)	0.51 (2.48)
REG	0.26 (1.41)	0.29 (1.57)	0.24 (1.16)	0.28 (1.13)	0.38 (1.13)
OTHER	-0.32 (2.01)	-0.42 (2.46)	-0.40 (2.31)	-0.36 (1.97)	0.72 (1.80)
RETARD	0.12 (2.61)	0.14 (2.64)	0.09 (1.72)	0.14 (2.34)	-0.33 (2.75)
COLA‡	-0.04 (0.57)	-0.10 (1.35)	-0.21 (3.08)	-0.05 (0.56)	-0.09 (0.47)
n(-1)†	-0.04 (2.25)	-0.02 (1.01)	-0.04 (1.92)	-0.05 (1.93)	-0.02 (0.47)
{w-wj}(-1)‡	0.11 (1.71)	0.10 (1.53)	0.06 (0.89)	0.12 (1.04)	-0.12 (0.73)
ΔAV†	-0.01 (0.07)	0.05 (0.39)	0.39 (3.33)	0.14 (1.04)	0.32 (1.22)
B(-1)†	0.13 (0.92)	0.08 (0.59)	0.27 (1.82)	0.06 (0.42)	0.75 (1.97)
DB(-1)	0.31 (5.03)	0.33 (5.16)	0.31 (4.59)	0.27 (3.44)	0.42 (2.45)
LSALES	0.01 (0.05)	-0.03 (0.24)	-0.04 (0.36)	-0.10 (0.80)	-0.05 (0.24)
CAPEXT	0.04 (0.73)	0.08 (1.44)	0.02 (0.34)	0.00 (0.01)	0.05 (0.40)
CAPPUB	-0.25 (3.72)	-0.26 (3.89)	-0.23 (3.30)	-0.24 (2.92)	-0.07 (0.43)
Sj†	-0.10 (3.92)	-0.11 (3.76)	-0.02 (0.94)	-0.06 (1.78)	0.19 (3.57)
ur	-0.20 (1.83)	0.23 (2.01)	-0.24 (2.08)	-0.04 (0.29)	-0.64 (2.25)
Δej	-0.32 (2.06)	-0.35 (2.13)	-0.25 (1.51)	0.29 (1.76)	-0.63 (0.82)
EXPECT	0.16 (3.82)	0.16 (3.60)	0.15 (3.56)	0.17 (3.24)	0.51 (6.20)
SIGNALj	0.49 (6.91)	0.50 (7.05)	0.49 (6.78)	0.46 (5.67)	0.05 (0.39)
Sample/obs	all/1131	all/1131	all/1131	no-st./969	strike/162
Wald (df)	861.4 (26)	720.6 (26)	713.6 (26)	596.6 (26)	821.0 (26)
Sargan (df)	133.5 (111)	115.9 (101)	112.7 (93)	84.3 (80)	38.8 (37)
fosc (obs)	-0.582	-0.86	-0.78	-0.32	--
sosc (obs)	1.98	1.84	1.77	1.10	--

Exogeneity of strike outcomes (Hausman test comparing estimates of (1) and (2)): 21.53 (χ^2_3)

All the columns consider a full set of time dummies (5), quarterly and industry dummies (8).

†: Variables used as GMM instruments (-1 and earlier lags)

¥: Variables used as GMM instruments (current and earlier lags).

‡: Instrumented by using its lagged value.

Wald (df): Wald test of the null that the vector of coefficients (excluding time and industry dummies) is zero.

Sargan (df): Test of the validity of the set of instruments. Under the null of adequacy, the test is distributed as a χ^2_r, where r is the number of overidentifying restrictions.

fosc(sosc): Test of the absence of first (second) order serial correlation in the error term (Arellano and Bond (1991)).

Table 2. Wage increase determination and strike outcomes. First differenced models.

	(1) coef. t-st.	(2) coef. t-st.	(3) coef. t-st.	(4) coef. t-st.
CONSTANT	-0.70 (5.75)	-0.75 (5.85)	-0.46 (2.94)	-0.97 (6.08)
Δw(-1)†¶	-0.02 (0.58)	-0.03 (0.76)	0.01 (0.26)	-0.24 (4.64)
DCO†ƒ	0.02 (3.51)	0.03 (4.25)	0.02 (3.93)	0.04 (4.42)
s†¶	0.232(3.53)	0.560(4.78)	0.560(4.07)	--
d†¶	-0.012(3.06)	-0.030(5.58)	-0.030(4.41)	--
$\hat{\lambda}$¥	--	--	--	0.046(2.31)
SINGLEUN	-0.05 (0.49)	-0.03 (0.25)	-0.08 (0.83)	-0.00 (0.02)
CCOO	0.36 (1.89)	0.18 (0.89)	0.56 (2.93)	0.27 (1.44)
REG	-1.54 (3.35)	-1.72 (3.30)	-1.54 (2.96)	-1.26 (2.67)
OTHER	0.06 (0.16)	0.11 (0.32)	0.21 (0.57)	0.73 (1.67)
RETARD	0.16 (2.66)	0.13 (1.77)	-0.03 (0.43)	0.16 (1.60)
COLA‡ƒ	-0.25 (2.99)	-0.41 (4.73)	-0.12 (0.73)	-0.28 (1.70)
n(-1)‡ƒ	-0.78 (1.41)	-0.86 (1.34)	-1.67 (2.63)	-1.98 (2.65)
{w-wj}-1‡ƒ	-0.27 (0.90)	-0.98 (2.58)	1.04 (2.54)	0.39 (0.92)
SALES†ƒ	0.01 (0.09)	0.11 (1.02)	0.20 (1.68)	-0.45 (2.51)
B(-1)†ƒ	1.10 (2.42)	1.21 (2.75)	0.89 (2.05)	0.94 (2.40)
DB(-1)	0.42 (3.92)	0.43 (3.80)	0.19 (1.40)	0.08 (0.62)
LSALES	-0.36 (1.47)	-0.43 (1.63)	-0.62 (2.39)	-0.24 (0.67)
CAPEXT	-0.30 (0.95)	-0.18 (0.54)	-0.39 (1.12)	-0.02 (0.06)
CAPPUB	0.88 (2.85)	0.89 (2.67)	0.68 (2.56)	0.39 (1.20)
Sj†	0.11 (6.32)	0.12 (6.34)	0.16 (8.57)	0.17 (4.60)
ur	-0.30 (1.52)	-0.32 (1.49)	-0.31 (1.44)	-0.06 (0.30)
Δej(-1)	0.02 (0.10)	0.01 (0.08)	0.29 (1.65)	0.18 (0.91)
EXPECT	0.16 (3.15)	0.13 (2.45)	0.23 (3.92)	0.06 (0.93)
SIGNALj	0.39 (4.84)	0.41 (4.93)	0.41 (4.45)	0.38 (3.74)
Sample/Obs	all/521	all/521	all/521	no-st./370
Wald (df)	1426.6 (26)	1185.2 (26)	560.8 (26)	911.3 (25)
Sar (df)	80.74 (82)	79.1 (74)	71.6 (71)	68.8 (60)
fosc	-3.98	-4.03	-3.75	-2.11
sosc	0.60	0.76	0.55	0.05
Effects(df)	39.57(17)	47.63(27)	19.87(12)	22.80(12)

Exogeneity of strike outcomes (Hausman test comparing estimates of (1) and (2)): 33.13 (χ^2_2)

All the columns consider a full set of time (5 or 4) and quarterly dummies and industry dummies (only in levels columns).

†: Variables used as GMM instruments (-1 and earlier lags -levels- or -2 and earlier -differ.)

¥: Variables used as GMM instruments (current and earlier lags).

‡: Instrumented by using their lagged value (levels) or twice value (differ.).

Effects(df): This is a Sargan difference test (Arellano (1993)), under the null that both the level and differenced models provide consistent estimates it is distributed as a χ^2_r, where r is the number of overidentyfying restrictions implied by the levels model.

For the set of variables marked with a ¶ we have test overidentifying restrictions like $E(x_{it-1}\varepsilon_t)=0$ and for those marked with a f we have tested $E(\Sigma x_{it-1}\varepsilon_t)=0$, being ε_t a levels error.

Other notes: See below Table 1.

Table 3. Wage increase determination. Relevance of strike's threats.

	(1) LEVELS coef. t-st.	(2) LEVELS coef. t-st.	(3) LEVELS coef. t-st.	(4) DIFFER coef. t-st.	(5) DIFFER coef. t-st.	(6) DIFFER coef. t-st.
CONSTANT	-0.74 (1.09)	-0.62 (1.06)	-0.32 (3.75)	-0.53 (3.12)	-0.79 (6.19)	-0.68 (4.31)
Δw(-1)†¶	0.38 (15.2)	0.36 (17.5)	0.35 (15.0)	-0.01 (0.21)	-0.04 (1.15)	-0.00 (0.10)
DCO†ƒ	0.02 (4.21)	0.03 (5.87)	0.02 (3.64)	0.03 (4.69)	0.01 (3.26)	0.02 (2.41)
s†¶	--	0.255(2.56)	--	--	0.377(4.14)	--
d†¶	--	-0.040(9.42)	-0.030(6.28)	--	-0.033(8.73)	-0.009(1.93)
$\hat{s}^*$¥	0.029(3.11)	0.023(2.81)	0.045(3.42)	0.038(2.40)	0.027(2.18)	0.024(1.46)
$\hat{d}^*$¥	-0.005(1.96)	-0.005(2.79)	--	-0.045(0.54)	0.078(1.56)	--
SINGLEUN	-0.20 (2.52)	-0.18 (2.36)	-0.17 (2.30)	0.01 (0.06)	-0.09 (0.99)	-0.21 (1.71)
CCOO	0.21 (2.31)	0.19 (2.35)	0.13 (1.51)	0.62 (3.22)	0.31 (1.82)	0.31 (1.37)
REG	0.33 (1.60)	0.24 (1.32)	0.17 (0.88)	-1.55 (3.21)	-1.66 (3.66)	-1.97 (4.00)
OTHER	-0.41 (2.35)	-0.39 (2.52)	-0.37 (2.15)	0.47 (1.23)	0.28 (0.88)	-0.53 (1.48)
RETARD	0.11 (2.11)	0.13 (2.80)	0.11 (2.31)	-0.03 (0.39)	0.05 (0.96)	0.10 (1.45)
COLA‡ƒ	-0.15 (2.47)	-0.13 (2.03)	-0.07 (0.99)	-0.13 (0.83)	-0.10 (1.20)	-0.05 (0.31)
n(-1)‡ƒ	-0.02 (1.03)	-0.02 (1.12)	-0.02 (1.42)	-1.82 (2.73)	-0.90 (1.99)	1.00 (1.40)
{w-wj}-1‡ƒ	0.04 (0.63)	0.06 (0.95)	0.06 (0.84)	1.00 (2.29)	-0.08 (0.28)	0.71 (1.54)
ΔSALES†ƒ	0.24 (1.88)	0.38 (3.90)	0.25 (2.08)	0.03 (0.24)	0.23 (2.29)	0.36 (2.98)
B(-1)†ƒ	0.26 (1.80)	0.20 (1.42)	0.16 (1.11)	0.85 (1.90)	1.17 (3.16)	2.58 (6.11)
DB(-1)	0.32 (4.53)	0.36 (6.05)	0.31 (4.72)	0.16 (1.14)	0.36 (4.20)	0.49 (4.05)
LSALES	-0.07 (0.59)	0.01 (0.10)	-0.02 (0.15)	-0.46 (1.68)	-0.48 (2.10)	-0.56 (2.01)
CAPEXT	0.03 (0.46)	0.08 (1.54)	0.07 (1.34)	-0.42 (1.23)	-0.45 (1.50)	-0.45 (1.79)
CAPPUB	-0.24 (3.46)	-0.26 (4.19)	-0.27 (1.09)	0.64 (2.33)	0.65 (2.33)	0.35 (1.25)
Sj¥	-0.03 (1.13)	-0.01 (0.64)	-0.08 (2.89)	0.15 (7.84)	0.17 (11.2)	0.20 (10.9)
ur	-0.25 (2.09)	-0.24 (2.25)	-0.20 (1.76)	-0.27 (1.24)	-0.17 (0.90)	-0.10 (0.43)
Δej(-1)	-0.37 (2.20)	-0.29 (1.89)	-0.50 (2.98)	0.21 (1.08)	0.12 (0.82)	0.21 (1.00)
EXPECT	0.15 (3.49)	0.14 (3.64)	0.13 (3.06)	0.21 (3.53)	0.13 (2.70)	0.15 (3.05)
SIGNALj	0.50 (6.49)	0.48 (7.52)	0.50 (6.68)	0.39 (4.11)	0.39 (5.26)	0.56 (6.56)
Sample/Obs	all/1131	all/1131	all/1131	all/521	all/521	all/521
Wald (df)	811.4 (26)	1425.7 (28)	673.7 (26)	673.7 (26)	5986.4(28)	562.8 (26)
Sar (df)	116.0 (93)	141.0 (127)	104.8 (103)	76.0 (71)	102.9 (96)	79.9 (77)
fosc	-0.77	-0.53	-0.72	-3.67	-3.93	-3.65
sosc	1.77	1.78	1.69	0.56	0.46	0.54
Effects(df)	--	--	--	23.23(12)	35.56(27)	32.30(22)

Exogeneity of strike outcomes (Hausman test comparing estimates of (1) and (2)): 33.13 (χ^2_2)

All the columns consider a full set of time (4) and quarterly dummies.
†: Variables used as GMM instruments (-2 and earlier lags).
‡: Instrumented by using their lagged twice value.
¥: Variables used as GMM instruments (current value and lags).
¶,ƒ: See below Table 2.
Other notes: See below Table 1.

Table 4. Implicit wage increase differential.

	CORRECTED(a)	NON CORRECTED(b)	OBSERVED(c) $\Delta w_s - \Delta w_{ns}$
ALL (strike length=4.93)	0.329	0.355	0.252
Energy	-0.051	-0.016	0.202
Minerals and Chemical	0.409	0.416	0.353
Metal Processing	0.245	0.353	0.255
Other Manufacturing	0.203	0.211	0.107
Building	0.378	0.497	0.146
Retail Services	0.978	0.898	0.658
Transportation	0.565	0.640	0.284
Others Services	0.456	0.394	0.471
ALL; d=15	0.149	0.174	---
ALL; d=30	-0.119	-0.095	---
ALL; d=100	-1.370	-1.350	---

(a): The corrected differential can be expressed as:

$$CD = (1/M).\Sigma_{i=1}^{N}\Sigma_{t=1}^{T} \{E\{\Delta\hat{w}_{it}^{1}/s_{it}=1\} - E\{\Delta\hat{w}_{it}^{0}/s_{it}=0\}\}$$

where M is the number of observations, $\Delta\hat{w}_{it}^{1}$ is the prediction of the strike model (Table 1(4)) and $\Delta\hat{w}_{it}^{0}$ is the prediction of the non-strike model (Table 1(5)), both considering the selection terms.

(b): the uncorrected differential is defined as:

$$UD = CD - \{\sigma_{w1}\hat{\lambda}_{w1} - \sigma_{w0}\hat{\lambda}_{w0}\}.$$

(c): residuals controlled for year and sector.

Table 5. A summary on results about strike variables and wage decline.

		strike's coefficients		% effect wage increases (d)				%effect wage levels(e)	
sample	table	s	d	d=1	d=5	d=15	d=30	d=30	d=100
all	1(1)	0.331	-0.012	4.64	3.94	2.19	-0.42	-0.03	-0.81
	1(2)	0.227	-0.032	2.84	0.98	-3.64	-10.7	-0.69	-1.55
	1(3)	0.544	-0.051	7.17	4.20	-3.17	-14.4	-0.92	
non-strike	1(4)	--	-0.018	-0.26	-1.30	-3.90	-7.80	-0.51	-1.68
all	2(1)	0.232	-0.012	3.20	2.50	0.76	-1.86	-0.11	-0.91
	2(2)	0.560	-0.030	7.71	5.97	1.60	-4.95	-0.32	-2.28
	2(3)	0.560	-0.030	7.71	5.97	1.60	-4.95	-0.32	-2.28

Keys:
s: Strike indicator.
d: length of a strike (in days).
(d): $100*(\delta_1 s + \delta_2 d)/\bar{w}$; $\bar{w}=6.87$
(e): $100*(100 + \bar{w} + \delta_1 s + \delta_2 d)/(100 + \bar{w})$

Appendix. Data and variables.

The data used in this paper comes from the NCGE, an annual survey about bargaining in Spanish large firms (more than 200 employees). Each wave provides information about firm variables (sales, profits), employment and negotiation issues by bargaining unit. Despite the fact that the survey runs from 1978, we only have information for the period 1985-1990. Although it is not a typical panel of data, we are able to use some coded information in order to extract an unbalanced panel of bargaining units. From the original sample, we have excluded firms which did not report information about some key variables such as wages or employment. There is also an important share of the records which has missing values for some key pieces of information (settlements, negotiation length and initial positions). There are 581 negotiation units in the final sample. The sample is highly unbalanced as far as there are 335 of those units which have two time series observations (see Table A.1). Hence, they do not contribute to the estimation of dynamic models in differences. Thus, testing correlated effects becomes of crucial importance.

Table A.1. Structure of the Sample

# of annual observations				2	3	4	5	6
number of units				335	127	78	42	28
years			85	86	87	88	89	90
observations			132	247	316	327	411	308
strike incidence %			18.2	9.7	20.6	13.5	17.3	11.0
sectors	1	2	3	4	5	6	7	8
observations	123	294	402	418	39	82	115	268
strike incidence %	15.4	13.3	24.4	12.2	28.2	8.5	20.9	4.9

a. Evidence on the correlation among wage outcomes and conflicting variables.

A key prediction in most of the theoretical background is that wage settlements and time-related threats are correlated. In particular, a negative correlation between wage outcome and strike duration has been emphasized. In Table A.2 we examine the relationship between wage outcomes and strike duration, negotiations' length and delay, respectively. With respect to the first, strike duration, *there is little descriptive evidence of a negative relationship*. On the contrary, there is some evidence supporting a positive association, given the fact that longer strikes are associated with higher wage outcomes in most of the years. The finding is robust to the control of wage outcomes by time and industry dummies. With respect to the two other variables considered, there is some evidence to support a negative correlation between wage increases and length of negotiation and also with respect to delay. In both cases and for all the years analyzed, the mean wage increase of the upper quartile is lower than the overall mean, from a minimum of 1% to a maximum of 7%.

Table A.2. Evidence about the average wage settlement by quartiles of the length of the threats.

		Q1	Q2	Q3	Q4
Strike(hours)		0-8h	8h-16h	16h-40h	+40h
1985		7.02	7.51	6.82	7.24
1986		7.99	7.33	8.00	8.32
1987		6.61	6.64	6.62	6.65
1988		5.69	6.28	5.54	5.89
1989		6.44	6.68	6.72	7.03
1990		7.65	7.60	8.30	8.70
Spell of neg. (days)		-37d	37-70d	70-120d	+120
1985		7.36	7.26	7.26	7.26
1986		8.13	8.00	8.28	8.09
1987		6.76	6.63	6.66	6.35
1988		5.69	5.82	5.65	5.42
1989		6.47	6.63	6.57	6.23
1990		7.98	7.80	7.79	7.69
delay (days)	No.del	0-68	68-112	112-153	+153
1985	7.68	7.32	7.27	7.24	7.14
1986	7.70	8.31	8.18	8.26	8.27
1987	6.52	6.91	6.70	6.71	6.32
1988	5.53	5.81	5.79	5.67	5.39
1989	6.07	6.31	6.87	6.67	6.43
1990	6.95	8.04	7.91	8.15	7.97

SOURCE: Own calculations using the NCGE.

Variables. Definition and main source.

Bargaining unit and firm variables.[Source:NCGE]

ΔW: Agreement about wages increases
DCO: Work council initial wage increase claim - firm initial offer (%).
DELY: 1 if the negotiation finishes after the starting date of the agreement.
RETARD: 1 if the negotiation starts after the starting date of the agreement.
n: Employment in the BU.
s: 1 if there is a work stoppage.
d: Strike hours divided by n*8 (which is a proxy of the to length in days).
%part: % of the employees that actively participate in the strike.
Qi: 1 if the negotiation finishes during the i quarter.
w: Wage bill by employee (in logs).
COLA: Cost of living allowance clause (1 agreed, 0 otherwise).
%CCOO: % work council representatives that belongs to the nationwide union CCOO (workers comissions).
%REG: % work council representatives that belongs any REGIONAL union.
%OTHER: % work council representatives that do not belong to any union.
SINGLEUN: 1 if the work council is formed by a single group.
AV: Gross added value per employee (in logs).
B: 100*(Gross real profit per employee).
DB: 1 if there are positive profits.
LSALES: Percentage of sales in the domestic market.

CAPEXT: Percentage of foreign agents ownership.
CAPPUB: Percentage of public ownership.

Industry, regional or aggregate variables.

SIGNALj: Industry average of the settlements signed in the month preceding the signing (%). (Ministerio de Trabajo, *Estadística de Convenios Colectivos*)
wj: Industry wage level (1 digit SIC) (in logs). (Instituto Nacional de Estadística: *Encuesta de Salarios* (ES))
Sj: Industry average working days lost by strike per employee. (Ministerio de Trabajo: *Boletín de Estadísticas Laborales*)
Ej: Employment in the j industry (44). (INE: *Encuesta de Población Activa* (EPA)).
ur: Regional market unemployment ratio (in logs). (EPA)
EXPECT: ARIMA price increase forecast at the date of signing the contract.

Table A.3. Variables. Descriptive statistics.

	non-strike 1479 obs.		**strike** 262 obs.	
	mean	**st.dev.**	**mean**	**st.dev.**
Δw	6.8573	1.3814	6.9401	1.3416
dco	4.2377	4.5680	5.9298	6.4335
d	0.0000	0.0000	4.9318	11.993
$\hat{d}$	0.2232	1.7017	0.2823	0.9110
$\hat{s}$	-2.4542	2.1427	-0.1173	0.9380
$\hat{\lambda}$	-2.7886	1.7123	0.8163	0.6010
singleun	0.1244	0.3301	0.0496	0.2175
ccoo	0.3353	0.2651	0.4126	0.2375
reg	0.0517	0.1437	0.0665	0.1524
other	0.0546	0.1422	0.0637	0.1298
retard	0.6700	0.4703	0.7519	0.4327
COLA	0.7484	0.4340	0.7709	0.4210
n(-1)	6.2979	1.0154	6.8977	1.3417
$w-w_j(-1)$	3.3986	0.3180	3.3229	0.2580
ΔAV	0.0825	0.3005	0.1143	0.3077
B(-1)	0.1032	0.1812	0.0539	0.1568
DB(-1)	0.8302	0.3755	0.7022	0.4581
lsales	0.8742	0.2016	0.8171	0.2349
capext	0.2357	0.3846	0.2788	0.4058
cappub	0.1621	0.3479	0.2653	0.4270
S_j	0.3730	0.8307	0.6936	1.2534
u_r	-1.6354	0.2028	-1.6319	0.1854
Δe_j	0.0340	0.1163	0.0142	0.0753
expect	5.3862	1.9856	5.4878	1.8254
$signal_j$	7.0847	0.8954	6.9815	0.8524
Spring	0.5037	0.5001	0.5572	0.4976
Summer	0.0885	0.2842	0.1946	0.3966
Autumn	0.0439	0.2050	0.0305	0.1723
Energy	0.0703	0.2557	0.0725	0.2598
Minerals	0.1724	0.3778	0.1488	0.3566
Metal Proc.	0.2055	0.4042	0.3740	0.4848
Other Manuf.	0.2481	0.4320	0.1946	0.3966
Building	0.0189	0.1363	0.0419	0.2009
Retail	0.0507	0.2194	0.0267	0.1615
Transportation	0.0615	0.2403	0.0916	0.2890
Other Services	0.1724	0.3778	0.0496	0.2175

References.

Arellano, M. (1993), "On the Testing of Correlated Effects with Panel Data," *Journal of Econometrics*, 59, 87-97.

_______ and Bond, S. (1991), "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations," *Review of Economic Studies*, 58, 277-297.

Ashenfelter, O. and Johnson, G.E. (1969), "Bargaining Theory, Trade Unions and Industrial Strike Activity," *American Economic Review*, 59, 39-49.

Blanchard, O. , Jimeno, J.F. (coor.) et al. (1995), *Spanish Unemployment: Is There a Solution?* CEPR Report, London.

Card, D. (1990a), "Strikes and Bargaining: A Survey of the Recent Empirical Literature," *American Economic Review* (AEA papers and proceed.), 80(2), 410-415.

_______ (1990b), "Strikes and Wages: A Test of an Asymmetric Information Model," *Quarterly Journal of Economics*, 105(3), 625-659.

Cramton, P. and Tracy, J. (1992), "Strikes and Holdouts in Wage Bargaining: Theory and Data," *American Economic Review*, 82(1), 100-121.

_________ and _______ (1994), "Wage Bargaining with Time-varying Threats" *Journal of Labor Economics*, 12, 594-617.

Fernández, R. and Glazer, J. (1991), "Striking for a Bargain between Two Completely Informed Agents," *American Economic Review*, 81, 240-252.

Fisher, T. (1989), "A Theoretical and Empirical Analysis of an Adverse Selection Model of Wages and Strikes," Unpublished PhD dissertation, University of British Columbia.

Franzoni, R. (1989), "One Hundred Years of Strike Statistics: Methodological and Theoretical Issues in Quantitative Strike Research," *Industrial and Labor Relations Review*, 42(3), 348-362.

Griliches, Z. and Hausman, J.A. (1986), "Errors in Variables in Panel Data," *Journal of Econometrics*, 31, 93-118.

Hayes, B. (1984), "Unions and Strikes with Asymmetric Information," *Journal of Labor Economics,* 2, 57-83.

Heckman, J. (1976), "The Common Structure of Statistical Models of Truncation, Sample Selection and Limited Dependent Variables as a Simple Estimator for such Models," *Annals of Economic and Social Measurement*, 5, 475-92.

Hicks, J.R. (1932), *The Theory of Strikes*, London: MacMillan Press.

Holtz-Eakin, D. (1988), "Testing for Individuals Effects in Autoregressive Models," *Journal of Econometrics*, 39, 297-307.

Jimeno, J.F. and Toharia, L. (1994), *Unemployment and Labour Market Flexibility: Spain*, International Labour Office, Geneva.

Jiménez-Martín, S. (1995), "The incidence, the Duration and the Wage increase Effect of a Strike: Evidence from the Spanish NCGE survey," D.T. 113/1995, FIES, Madrid.

Keane, M.P. and Runkle, D.E. (1992), "On the Estimation of Panel-Data Models with Serial Correlation when Instruments Are not Strictly Exogenous," *Journal of Business and Economics Statistics*, 10, 1-9.

Kennan, J. (1980), "Pareto Optimality and the Economics of Strike Duration," *Journal of Labor Research*, 1, 77-94.

Kennan, J. (1986), "The Economic of Strikes" in O. Ashenfelter and R. Layard (eds) *Handbook of Labor Economics*, vol. 2, Ch. 19, Amsterdam: North-Holland. 1091-1137.

_________ and Wilson, R. (1989), "Strategic Bargaining Models and Interpretation of Strike Data," *Journal of Applied Econometrics*, 4, s87-s130.

_________ and _______ (1993), "Bargaining with Private Information," *Journal of Economic Literature*, 31, 45-104.

Lacroix, R. (1986), "A Microeconometric Analysis of the Effects of Strikes on Wages," *Relations Industrielles*, 41, 111-126.

McConnell, S. (1989), "Strikes, Wages and Private Information," *American Economic Review*, 79, 801-815.

Morton, S. (1983), "The Optimality of Strikes in Labor Negotiations," Discussion Paper No 83-7, Murphy Institute for Economics. Tulane Univ.

Palenzuela, D.R. and Jimeno, J.F. (1995), "Wage Drift in Collective Bargaining at the Firm Level: Evidence from Spain," FEDEA, Madrid, mimeo.

van Ours, J.C. and van de Wijngaert, R.F. (1993), "Holdouts and Wage Negotiations in the Netherlands," mimeo, Free University, Amsterdam.

Reder, M.W. and Neumann, G.R. (1980), "Conflict and Contract: The Case of Strikes," *Journal of Political Economy*, 88, 867-886.

Ridell, C.W. (1980), "The Effects of Strikes and Strike Length on Negotiated Wage Settlements," *Relations Industrielles*, 35, 115-120.

Ross, A.M. (1948), *Trade Union Wage Policy*, Berkeley: University of California Press.

Siebert, W.S. and Addison, J.T. (1981), "Are Strikes Accidental?," *The Economic Journal*, 91, 389-404.

Stengos, T. and Swidinsky, R. (1990), "The Wage Effects of a Strike: A Selectivity Bias Approach," *Applied Economics*, 22, 375-385.

Vroman, W. (1984), "Wage Contracts Settlements in U.S. Manufacturing," *Review of Economics and Statistics*, 66, 661-665.

Wooldrigde, J.M. (1995), "Selection Corrections for Panel Data under Conditional Mean Independence Assumptions," *Journal of Econometrics*, 68(1), 115-132.