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DO OSHA INSPECTIONS REDUCE INJURIES? A PANEL ANALYSIS

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ABSTRACT

Using data on injuries and OSHA inspections for 6,842 large manufacturing plants between 1979 and 1985, we find evidence that OSHA inspections significantly reduce injuries. This effect comes exclusively from inspections that impose penalties; inspections which do not impose penalties appear to have no effect on injuries. Plants which are inspected (and penalized) in a given year experience a 22 percent decline in their injuries during the following few years. In our sample, total OSHA enforcement is predicted to have reduced injuries by about 2 percent.

We take advantage of the panel nature of our data to test for a number of potential biases: autocorrelated injuries, plant-specific fixed-effects which are correlated with both inspections and injuries, and endogeneity of inspections (injuries causing inspections). These biases lead us to use the percentage change in injuries, rather than injury levels, as the dependent variable for our estimation. Our analysis shows that the estimated effect of inspections on the percentage change in injuries is not significantly affected by these biases, and thus seems to reflect a 'deterrence' effect of OSHA inspections on injuries.

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DO OSHA INSPECTIONS REDUCE INJURIES? A PANEL ANALYSIS*

1. Introduction

Since the Occupational Safety and Health Administration (OSHA) was founded in 1970, there has been considerable doubt about its ability to improve workplace safety. OSHA inspects only a small fraction of all plants in a given year, and imposes relatively small fines, so that the expected penalty for a violation is too low to induce firms to comply with OSHA regulations (Smith, 1976). In addition, OSHA's safety regulations cover few of the potential causes of injuries, so that even perfect compliance would have a limited effect on injuries (Mendeloff, 1979). Empirical studies of OSHA's impact on injuries have had mixed results, some finding no effect while others find significant (usually small) effects.

This study extends the literature by analyzing a unique database with panel data techniques. We have data for 1979 to 1985 on injuries and OSHA inspections at 6,842 manufacturing plants. We use a method from Chamberlain (1982, 1984) to control for econometric problems which affect plant-level enforcement studies, especially the endogenous effect of injuries on inspections from OSHA's targeting its inspections towards high-injury plants. We also have data allowing us to identify those inspections which impose penalties, unlike earlier studies which treat all inspections alike.

We find that plants experience significantly declining injuries after being inspected. This decline is almost entirely due to inspections that impose penalties; inspections without an associated penalty have little effect on injuries. The average plant in our sample that is penalized in a given year reduces its injuries by 22 percent over the following three years. These results probably overstate the impact of a typical OSHA inspection on a typical manufacturing plant: the plants in our sample are inspected relatively frequently, so they may be more sensitive to OSHA enforcement, and only about one-third of inspections impose penalties. Still, the existing level of OSHA enforcement in our sample is predicted to reduce injuries by about 2 percent.

In section 2 we survey prior empirical studies on the impact of OSHA enforcement on injuries. We describe our dataset in detail in section 3 and compare our sample with all of U.S. manufacturing. We consider the connection between inspections and injuries in Section 4, using an ordinary regression. Section 5 discusses potential sources of bias, and a method proposed by Chamberlain to correct them. The results of this method are reported in section 6; section 7 presents a summary and areas for future research.

2. Survey of Past Research

Studies of OSHA's impact on injuries have been done using both industry and plant-level data. Industry studies have regressed injury rates on inspection rates, controlling for a variety of industry characteristics. Some plant-level studies compare injury rates of plants inspected early in the year with injury rates of plants inspected later in the year, others simply regress injury rates or changes in injury rates on recent inspections of the plant.

All of these studies are based on the idea of deterrence: OSHA inspections make plants less likely to violate OSHA standards because violations are detected and penalized. We should distinguish between two types of deterrence:

'specific deterrence', where plants make an effort to comply after they have been inspected and 'general deterrence', where those plants that are especially likely to be inspected will make an effort to comply even before they are inspected. The increase in compliance from deterrence could reduce injury rates, although both the enforcement-compliance and compliance-injury links have been questioned (Mendeloff, 1979; Bartel and Thomas, 1985).

The relative usefulness of industry and plant-level data depends in part on the relative magnitudes of specific and general deterrence. If specific deterrence is more important, industry data which includes both inspected and uninspected plants may fail to observe deterrence in the industry average. If general deterrence is more important, industry data may be better at capturing deterrence effects, to the extent that plants base their subjective probability of being inspected on the industry average probability.

Among existing industry studies, Viscusi (1986) uses two-digit industry data for 1973-1983 and finds that OSHA inspections significantly reduce injuries, although the reduction is small: only about 2 to 3 percent of all injuries are prevented by OSHA enforcement. Bartel and Thomas (1985) use three-digit industry data for 1974-1978. They find that OSHA enforcement does significantly increase compliance [1] (a total of 26 percent, relative to no enforcement), but that there is only a weak link between compliance and injury rates (perfect compliance would reduce injuries by only 10 percent). Note that a point estimate of OSHA's impact based on the Bartel and Thomas results (reducing injuries by 2.6 percent) is similar to Viscusi's.

A variety of plant-level studies have also found that OSHA enforcement affects injuries. Cooke and Gautschi (1981), in a study of plants in Maine, regress the change in lost workdays between 1970 and 1976 on the number of OSHA citations issued to that plant during the same period. They find that injuries tend to decline in plants with more citations, although this result is only significant for large plants (200+ workers). Robertson and Keeve (1983) use detailed data on the injury experience of individual workers in three plants for 1973-1980 to construct expected injury rates, finding that actual injury rates are significantly lower than expected in the year following an OSHA inspection of the plant. In earlier work with our data (Scholz and Gray, 1990), we find significant (and large) general deterrence effects, calculating the expected enforcement effort faced by each plant and regressing changes in injuries at the plant on changes in expected enforcement. These studies suggest OSHA substantially reduces injuries, by as much as 10 percent.

Alternative results, that OSHA inspections have no effect on injuries, come from a set of plant-level studies. These follow Smith (1979) in constructing a 'natural experiment', comparing injury rates at plants inspected in March or April with injury rates at plants inspected in November or December. If OSHA inspections reduce injuries, plants inspected earlier in the year should have lower injury rates, ceteris paribus, because the inspection has had a longer time to affect that year's injuries. Smith (1979) finds that 1973 inspections significantly reduced injuries, by as much as 16 percent, but that 1974 inspections did not have a significant impact. Later studies using the same method find little or no impact: McCaffrey (1983) with data for 1976-1978, and Ruser and Smith (1991) with data for 1981-1985.

Why does the Smith methodology give different results? One possible explanation lies in the 'month inspected' variable used in these studies, taken from an injury survey. Our dataset also includes OSHA's own inspection data, which disagrees with the 'month inspected' value more than 20% of the time.

These errors in measuring the inspection date will tend to bias OSHA's estimated impact towards zero. Our data also show that earlier-inspected plants are more likely to have multiple inspections during the year. Since multiple inspections tend to happen at high-hazard plants, any tendency for early-inspected plants to be high-hazard could offset a tendency for OSHA inspections to reduce injuries. Much of the motivation for the present paper is the attempt to control for the endogeneity of inspections in a large, plant-level dataset without relying on the Smith method.

3. Data Description

The dataset used in this project consists of 6,842 manufacturing plants with annual data from 1979 to 1985. The sample comes from the Bureau of Labor Statistics (BLS) annual injury survey, and contains only those plants which were in the BLS sample for each year from 1979 to 1985 (Ruser and Smith, 1991). Those plants which were located in states with federal OSHA enforcement were then matched with data from OSHA's Management Information System database (MIS).[2] The BLS data includes the number of employees, the total hours worked in the year, the number of lost workday injuries and the total number of lost workdays. The MIS data includes all inspections of these plants, with information about the type of inspection, citations issued and penalties assessed during the inspection. The plants in our sample averaged 2.7 inspections, although 1,566 plants (23 percent) were never inspected.

Table 1 compares our plants to manufacturing averages. The BLS survey oversamples large plants and rotates the sample, so that small plants are much less likely to be sampled for the seven consecutive years needed to be in our data (since the BLS sampling is based solely on plant size, not on injury rates or OSHA inspections it should not induce any 'sample selection' problem in our analysis). Our plants are almost ten times as large as the average manufacturing plant (523 to 54 workers). Furthermore, sample plants are six times more likely to be inspected (.51 to .08 inspections per plant) because OSHA inspects large plants more frequently than small ones. While not representative of all plants, our sample includes a large part of the manufacturing sector that is important for OSHA: only 2% of all plants, but 20% of employees and 12% of all OSHA inspections in manufacturing.

Our data has both advantages and disadvantages relative to datasets used before. The results are most applicable to large plants and may not generalize to smaller, less frequently inspected plants. Also, the data is restricted: to guard the anonymity of plants in the BLS sample, the BLS and OSHA datasets were merged at BLS under our direction, and all identifying variables removed before the data was released to us. Thus, we can use only employment, hours worked, and OSHA inspection data to explain injuries. On the other hand, the panel nature of the data allows us to control for any consistent differences across plants in demographics, technology or management practices. We also have more information on the inspections and a longer time-span of data.

Injury Measures

A standard measure of the frequency of injuries at a plant is the lost workday incidence rate, defined by the Bureau of Labor Statistics as the annual number of injuries (involving lost workdays) per 100 'full-time equivalent' workers (200,000 hours worked). Graph 1 presents this measure for the manufacturing sector from 1975 to 1985, and for our sample from 1979 to 1985.

A second standard measure, which reflects the severity of injuries rather than just their frequency, is provided by the number of workdays lost due to injury per 100 full-time workers, also shown in Graph 1.

Both injury rates move in tandem during the period, declining sharply from 1979 to 1982, then holding steady and rising slightly at the end of the period. This pro-cyclical movement of injury rates (also seen in the rising injury rate before 1979 in the manufacturing data) could be attributed to changes in the quality of workers and the number of hours per worker. In recessions, only experienced workers remain on the job, and more safety-related maintenance can be done; in expansions, less experienced workers are hired, overtime work leads to exhaustion, production lines run faster, and safety gets less attention.

Plants in the sample appear safer than the manufacturing average, with 13 percent fewer injuries and 16 percent fewer lost workdays per 100 employees. This is probably due to the larger size of plants in the sample, since larger plants generally have lower injury rates. There is a strong plant-specific component to injury levels throughout the period, with a correlation of .68 between the injuries at a plant in 1979 and the injuries at the same plant in 1985. For our analysis, we transform the injury measures into percentage changes.[3] The percentage change in injuries shows much less long-run persistence than the levels of injuries, with correlations of less than .06 between distant years, although there is a strong first-order negative correlation of -.34, induced by the construction of percentage changes.

OSHA Inspections

The inspection experience of the sample plants from 1979 to 1985 is shown in Graph 2, and compared to the experience for the entire manufacturing sector from 1975 to 1985.[4] The number of inspections done on sample plants declines substantially, with a smaller decline for manufacturing as a whole. This suggests that OSHA has used its enforcement resources more broadly in recent years, since plants in our sample are much more heavily inspected than the average plant. The decline occurs primarily in multiple inspections of the same plant during a year, as the fraction of plants with any inspection during the year remains relatively stable.

The fractions of inspections which had penalties imposed, which found serious violations, and which found any violations, are also shown in Graph 2. All of these measures were stable in the sample, but declined for manufacturing as a whole. This indicates that plants outside our sample, which accounted for a growing share of OSHA inspections, either had fewer problems or received less rigorous inspections than plants in our sample (perhaps just because our plants were larger). For inspections which had some penalty imposed, there was a sharp decline in the average penalty for both the sample and for all of manufacturing between 1979 and 1981, with some increase after 1984.

4. Inspections and Injuries - A First Look

A simple cross-section test of the connection between inspections and injuries is to examine the correlation between the injury experience at a plant and its inspection experience throughout the sample period. These correlations are presented in Table 2, looking at four measures of enforcement and both levels and changes in injuries. The different enforcement measures distinguish between all inspections and only those which impose penalties, and between the total number of inspections and the number of years in which an inspection occurred.[5] The top section of the table shows that the enforcement measures are strongly correlated with each other. In the middle of the table, we see a strong positive correlation between injury levels and OSHA enforcement. This comes as no surprise, as OSHA's policy of programmed inspections is designed to target high-hazard plants.[6] The lower section, looking at the percentage change in injuries, shows that plants facing more enforcement during the sample period had a greater decline in injuries. In our analysis we use the percentage change in injuries, rather than injury levels, because of the endogeneity of inspections and injury levels (this issue is examined in more detail in section 5).

Consider an ordinary regression designed to explain the annual percentage change in lost workday injuries, PCHNUM. We control for changes in the plant's workforce with the percentage change in the number of workers at the plant (PCHEMP) and the percentage change in the number of hours worked at the plant (PCHHOUR), and also include year dummies. Our primary interest is in the amount of OSHA enforcement (ENF) directed at the plant in this year and the past two years. The regression equation is

$$(1)\quad PCHNUM_{it} = \alpha_t + \beta^E\, PCHEMP_{it} + \beta^H\, PCHHOUR_{it} + \sum_{j=0}^{2} \beta^I{}_j\, ENF_{it-j} + u_{it},$$

estimated for 6,842 plants on 1981-1985 data (34,210 observations in all).

Table 3 presents the results from estimating equation (1) using four measures of annual enforcement: NINSP is the number of inspections during the year; INSP is a dummy variable for having any inspection during the year; NIPEN and IPEN are similar, but count only those inspections that impose a penalty. In lines 1-4, we find that current and past enforcement is associated with declining injuries, with little difference in the performance of the four enforcement measures. However, when we separate penalty and non-penalty inspections in line 5, we find that the impact of inspections on injuries comes exclusively from penalty inspections. In line 6, when we separate penalty inspections into the first penalty inspection during the year (1PEN) and subsequent inspections (2+PEN), we find the impact is concentrated on the first inspection. As a result, we will use 1PEN to measure OSHA enforcement.

5. Econometric Issues and the Chamberlain Method

The major econometric issue in any plant-level study of the impact of OSHA inspections on injuries is the endogeneity of inspections. OSHA tries to target its inspections on high-hazard plants, and there is every evidence that they succeed: there is a strong positive correlation between injury levels and inspections (as seen in Table 2). This correlation switches sign (and becomes much weaker) when we consider injury changes rather than injury levels, suggesting that the endogeneity problem could be reduced (or eliminated) by switching the analysis to injury changes.

Consider three factors which could lead inspections and injuries to be correlated. The first ('deterrence') is that inspections (current and past) cause plants to reduce hazards, thereby reducing injuries. The second ('endogeneity') is that injuries (current and past) cause plants to get more inspections. The third ('fixed-effect') is that plants which are high-hazard (for reasons of technology, management style, worker quality, or whatever) tend over a number of years to have both more inspections and more injuries. In order to get a good estimate of the 'deterrence' factor (which is likely to be negative), we must also account for the other two factors (which are likely to be positive). Distinguishing between 'deterrence' and 'fixed-effect' is based on the timing of inspections and injuries. For example, a tendency for an inspection to occur exactly one year after a high-injury period suggests 'endogeneity'. A tendency for high-injury plants to have more inspections on average throughout the period, with the inspections sometimes occuring before high-injury years and sometimes occuring after high-injury years, suggests a 'fixed-effect' interpretation.

We used the Chamberlain procedure (described below) to test for these three factors, comparing the results for the percentage change in injuries with the results for injury levels (actually log(injuries)). For the percentage change in injuries, we found a small fixed-effect but no evidence of endogeneity (plants with increasing injuries have slightly more inspections, but the inspections are as likely to come before a big increase in injuries as after). For injury levels, we found a very large fixed-effect term, and evidence of significant endogeneity (high-injury plants have many more inspections, and the inspections are more likely to follow high-injury years). For both levels and changes we found a negative and significant deterrence effect after controlling for fixed-effects. However, the estimated deterrence effects in the injury levels model were much more sensitive to the specification of the rest of the model, particularly the fixed-effect term. This coefficient instability for the injury levels model, combined with the evidence of endogeneity, persuaded us to concentrate on the percentage change form of injuries presented here.[7]

We consider three sources of bias that could affect the estimated impact of OSHA enforcement on injuries in the models of Table 3. These biases could arise from (1) serial correlation in the dependent variable (injury changes),

(2) endogeneity of inspections (with respect to injury changes), and (3) omitted plant-specific effects (correlated with both injury changes and inspections). The Chamberlain method allows us to test for these biases, and we consider each bias in turn, using an abbreviated form of equation (1):

$$(1') \quad y_{it} = \alpha_t + \beta^z z_{it} + \sum_{j=0}^{2} \beta^I{}_j x_{it-j} + u_{it}$$

(y refers to injury changes, x to inspections, and z to the other controls).

The significant negative impact of lagged inspections on injury changes could be biased due to the negative autocorrelation of injury changes. If injury changes follow a partial adjustment model,

$$(2) \quad y_{it} = \beta x_{it} + \delta y_{it-1} + e_{it}, \quad \text{with } \delta<0,$$

we could find a spurious negative coefficient for past inspections when regressing current injury changes on current and past inspections. In effect, we would misinterpret the actual effect of past injury changes on current injury changes as an effect of past inspections on current injury changes.

Table 2 shows that OSHA inspects high-injury plants more frequently. If inspections are also more common for plants with increasing injuries, it could bias our results. Suppose that we believe inspections cause injury changes, but in fact current inspections are caused by past injury changes:

$$(3) \quad x_{it} = \Sigma\, \beta_j y_{i,t-j} + e_{it}.$$

If we regressed current injury changes on current and lagged inspections, we could get spurious negative coefficients on current inspections, due to the negative autocorrelation of injury changes. In this case, we would be misinterpreting the actual effect of past injury changes on current inspections as an effect of current inspections on current injury changes.

Finally, other characteristics of the plant not measured in our data could cause both injury changes and inspections. We model this with a fixed plant-specific effect, c_i, driving both inspections and injury changes:

(4) $y_{it} = \beta c_i + e_{it}$ and $x_{it} = \alpha c_i + \epsilon_{it}$.

We would get a bias (equal to $\beta * \alpha$) in the estimated effect of inspections on injury changes. A negative bias means the measured effect of inspections on injury changes could be spurious; a positive bias means the measured effect understates the impact of inspections on injury changes.

The estimation method used here, developed by Chamberlain (1982, 1984), is able to test and correct for these three sources of bias. Instead of estimating a single-equation, panel dataset (as we did in Table 3) with several years of data for each plant, we estimate a six-equation model, regressing each year of the dependent variable on the values of the independent variables for all the years (past, present and future). The τ-th equation would be:

$$(5)\ y_{i\tau} = \gamma_\tau + \sum_{t=1}^{6} (\pi_{\tau,t}) x_{it} + u_{i\tau}\ .$$

This gives us a 6x6 matrix of coefficients, π, on each independent variable in the regression, with each row consisting of the coefficients from one of the equations (for ease of notation we will concentrate on the case with one independent variable; with several independent variables, we get a π matrix for each of the variables). Thus $\pi_{1,3}$ is the coefficient on x at time 3 in the equation for y at time 1.

Chamberlain shows that restrictions on the π matrix can be imposed using a minimum distance estimation method. First, express the restricted π matrix as $g(\theta)$, where θ is a vector of 'underlying' parameters. θ can have between 0 and T^2 elements, where T^2 allows a completely unrestricted π matrix and 0 is a 'completely restricted' matrix (no free parameters). Choose θ to minimize

(6) $L = (N*T) [\pi - g(\theta)]' A^{-1} [\pi - g(\theta)]$,

where A is an estimate of the variance-covariance matrix of the unrestricted parameters. Chamberlain suggests a general form of A which allows for both heteroskedasticity and autocorrelated errors:

(7) $A = (1/NT) \Sigma_i [((y_i - \pi x_i)(y_i - \pi x_i)') \times (S_x^{-1} (x_i x_i') S_x^{-1})]$

where $S_x = \Sigma_i (x_i x_i')/N$. Chamberlain also shows that L in equation (6) follows a χ^2 distribution, with degrees of freedom equal to the number of restrictions being imposed. This provides a test for the willingness of the data to accept particular constraints, and can be used to compare sets of nested constraints.

Consider the problem of a fixed effect correlated with the x variables, as described in equation 4. We could begin with a model in which only current-year values of x mattered in determining y:

(8) $y_{it} = \beta x_{it} + u_{it}$.

This corresponds to a diagonal π matrix:

(9) $\pi = \beta I$,

with only one free parameter, β. Adding a plant-specific effect, c_i, correlated with x, gives:

(8') $y_{it} = \beta x_{it} + c_i + u_{it}$.

The corresponding π matrix is:

(9') $\pi = \beta I + e\alpha'$,

where e is a vector of ones and α is a vector $(\alpha_1,\ldots,\alpha_T)$ of the coefficients from a regression of the fixed effects c_i on all the years of x_{it}. Under (9) the diagonal elements of π are equal and the off-diagonal elements are zero, while (9') allows non-zero off-diagonal elements, with equal values in the same column of the π matrix and corresponding differences along the diagonal.

Table 4 indicates how to compare the restrictions implied by (9) and (9'), with models I and II. Model II (equation 9′) is more general, since it includes T+1 parameters (β plus the T-element vector α), while model I (equation 9) includes only one (β). Another way of putting this is that model I imposes T restrictions on model II ($\alpha_1 = \ldots = \alpha_T = 0$). We can compare the L-statistics (from equation 6) for the two models. The difference between them ($L_I - L_{II}$) is distributed as χ^2, with T degrees of freedom. If $L_I - L_{II}$ is sufficiently large, we will reject the T restrictions implied by Model I ($\alpha_1 = \ldots = \alpha_T = 0$), and find a fixed-effect significantly correlated with x.

To test for any biases induced by a partial adjustment process determining injury changes, we need to consider other explanatory variables (changes in employment and hours) that are strongly related to current injury changes. If equation 2 holds, we should see significant coefficients on the lagged values of these other variables (π_{ij} for i>j). A failure to find significant lagged effects (model I vs. model III in Table 4), will be taken as evidence that the significant effects of lagged inspections are not the spurious result of a partial adjustment process determining injury changes.

To test for reverse causality between the y and x variables, we use a variation on the usual tests for exogeneity that was suggested by Chamberlain. This revised version, called "conditional strict exogeneity", allows for the presence of a plant-specific effect that is correlated with x, giving a test for "y does not cause x, conditional on c". As in the usual exogeneity tests, we regress y (injury changes) on past and future values of x (inspections). If the coefficients on future inspections are significant, we reject exogeneity and find reverse causality (injury changes cause inspections). In terms of the π matrix, we allow a plant-specific effect

(10) $\pi = B + e\alpha'$,

and test to see whether the upper-triangular part of the matrix B, showing the effect of future inspections, is zero (testing model V vs. IV in Table 4).

The restrictions imposed on the π matrix for these tests can be combined with other assumptions. One restrictive assumption that is usually made implicitly, and will here be made explicitly, is that of model stability: the parameters of the underlying model do not change during the data period. In equation (10), this amounts to B(i,j)=B(i+n,j+n) for all n (e.g., aside from possibly different contributions to the plant-specific effect through the α vector, the effect on current injury changes of last year's inspections is the same for all years). We can test the model stability assumption, since it imposes restrictions on the π matrix, but generally impose it, as it greatly reduces the number of coefficients to interpret.

6. Results

Since the effect of enforcement on both the frequency and seriousness of injuries is of interest, we use two dependent variables in our analysis: the percentage change in lost workday injuries (PCHNUM) and the percentage change in lost workdays (PCHDAYS). To control for changes in the plant's workforce we use the percentage change in the number of workers at the plant (PCHEMP) and the percentage change in the number of hours worked at the plant (PCHHOUR). Our primary interest is in the effect of OSHA enforcement, measured by IPEN.

The coefficients from the regressions of each year's dependent variable on all years of the independent variables are presented in Table 5 (for PCHNUM) and Table 6 (for PCHDAYS). The coefficients in each table make up the three π matrices, one each for PCHEMP, PCHHOUR and IPEN. The workforce measures, PCHEMP and PCHHOUR, have significant positive coefficients on the (underlined) diagonal in both tables, showing strong effects of current workforce changes on the changes in injuries. Their off-diagonal coefficients are smaller, usually insignificant, with no particular pattern of signs. The IPEN measure is much less powerful. Its coefficients are similar in magnitude on and off the diagonal, but there is a pattern of signs, with current and past years' inspections being negatively related to injuries, and future inspections being positively related to injuries.

We calculate the L-statistics for eight different restrictions on the π matrix of coefficients, using the tests outlined in Table 4. These results are presented in Table 7, where the same test is conducted separately for the two dependent variables and each of the three independent variables, while the coefficients on the other two independent variables are allowed to be completely unrestricted. Each model can be compared with the completely unrestricted π matrix, using the L-statistic for that line. For example, model I imposes 35 restrictions on the π matrix, allowing only one free coefficient (the effect of current x on current y). Since the critical value for $\chi^2(35)$ at the 5% level is 49.8, we can reject model I for the IPEN variable, but not for PCHEMP or PCHHOUR. We can also compare two restricted models where one model is a less restricted version of the other. For example, model II allows an additional 6 parameters beyond model II, through the correlation between each year's x value with the fixed-effect $(\alpha_1,\ldots,\alpha_6)$. If going from model II to model I (imposing 6 more restrictions) raises the L-statistic by more than the critical value for $\chi^2(6)$ at the 5% level (12.59), we reject the additional restrictions of model I (thus we reject the restrictions for IPEN, and accept them for PCHEMP and PCHHOUR).

We begin by considering the effect of the control variables, PCHEMP and PCHHOUR. We recall from Table 3 that changes in injuries are very strongly related to current changes in employment and hours. Table 7 shows that we can restrict our attention to the contemporaneous impact of PCHEMP or PCHHOUR: model I, which forces all other impacts to be zero, is not a significant restriction. In contrast, the relationship between inspections and injury changes is more complex, with significant off-diagonal elements of π (model I), a significant plant-specific effect (model I vs. model II), and significant off-diagonal terms after the plant-specific effect is accounted for (model II).

We now turn to the three potential sources of bias discussed earlier: a partial adjustment process for injury changes, a fixed-effect correlated with both inspections and injury changes, and endogenous inspections. There is no evidence for a partial adjustment process, since neither past PCHEMP nor past PCHHOUR is related to current injury changes. Since both PCHEMP and PCHHOUR are more strongly related to injury changes than IPEN is, we would expect any partial adjustment process to appear more strongly there than for IPEN. There is strong support for a fixed-effect correlated with inspections (model I vs. II for IPEN). Given a fixed-effect, the test for conditional strict exogeneity (model V vs model IV), is not significant. In other words, the positive connection between future inspections and current injury changes (seen in Tables 5 and 6) seems to be due to fixed characteristics which lead certain plants to have both many inspections and growing injuries over the period.

To establish a final form for the model we first impose the model stability assumption discussed earlier.[8] We then test restricting the effect of lagged inspections to last for three years (model VII vs. model V), and forcing an equal contribution of being inspected to the plant-specific effect for each of the years (forcing $\alpha_1 = \ldots = \alpha_6$, model VIII vs. model VII). Both of these restrictions are supported by the data.

Table 8 presents the estimated effects of inspections on injury changes for various versions of the final model. Some versions restrict PCHEMP and PCHHOUR to have only a current-year effect on injury changes; others allow completely unrestricted effects. Still other versions allow the plant-specific effect to be differently related to the IPEN values from different years. The coefficients on the IPEN variables are not much affected by these changes in specifications, and always show significant negative effects over the few years after an inspection with penalty. The IPEN coefficients have somewhat larger coefficients on the one- and two-year-lagged values than on the current or three-year-lagged values; the data reject the imposition of a simple distributed lag pattern on the coefficients.[9]

The cumulative effect of an inspection, obtained by summing the IPEN coefficients, is about -.22 on PCHNUM and -.20 on PCHDAYS across all of the specifications. These effects are nearly twice as large as the original regression results in Table 3, due to the fixed-effect terms. The positive coefficients in α indicate that inspected plants tend to have rising injuries relative to non-inspected plants, averaged across all the years of data. For inspected plants to have declining injuries after an inspection (as they do in Tables 5 and 6), the 'true' impact of past inspections (matrix B in equation 10) must be even larger (more negative) to offset the positive effects of α.[10]

7. Summary

We find a significant negative relationship between OSHA enforcement activity at a particular plant and the change in injuries at that plant over subsequent years. We find no evidence that this relationship is the result of endogeneity of inspections, the negative autocorrelation of injury changes, or plant-specific fixed-effects. Based on the coefficients in Table 8, a plant that is inspected (and penalized) in a given year experiences a 22 percent decline in injuries over the following three years, and a 20 percent decline in lost workdays.

Since the average plant in our sample experiences 25 lost workday injuries and 363 lost workdays (Table 1, for 1979), these reductions amount to 5 or 6 fewer injuries and 73 fewer lost workdays. Stating these results somewhat differently, the overall level of enforcement in our sample (9.3% of all firms penalized in a given year) reduces total injuries in the sample by about 2 percent (0.093*0.22). This estimate is similar to the 1.5 to 3.6 percent effectiveness estimated by Viscusi (1986) with industry-level data, and contrasts with the finding of no impact from a number of plant-level studies.

The Chamberlain technique has allowed a rigorous test of a variety of potential biases to our results. However, one should use caution in extrapolating from our results to the total impact of OSHA on injuries. First, results based on our sample of large, intensively inspected firms may overstate the effect of inspections on moderate and smaller sized firms subjected to less vigilant monitoring. Second, the impact on inspections are expressed in terms of inspections with penalties. Since only about one inspection in three results in a penalty, an additional inspection on average reduces injuries by only about 7%. Third, our results are based on OSHA's existing enforcement policy: a new policy of imposing a small penalty on every inspection would not triple OSHA's effectiveness, since presumably it is the penalization of specific kinds of serious violations that enhances safety, not just any penalization policy.

On the other hand, our results may underestimate the total impact of OSHA. We consider only specific deterrence, but OSHA may also have a general deterrence effect (Scholz and Gray, 1990).[11] OSHA-induced changes in technology, management, and safety education could contribute to injury reduction in ways that are difficult to measure (Mendeloff, 1979). Finally, OSHA's impact in earlier years may have been considerable greater, with the cumulative effects of inspections over time leading to lower marginal effects on injuries during our period of study.[12]

To answer the broader question of the potential effectiveness of OSHA enforcement, future research needs to go beyond the effects of inspections on injuries. We find that inspections which impose a penalty have a much greater impact than those which don't. Additional testing is needed to see whether different types of inspections have different impacts, and whether inspectors differ in their ability to detect and prosecute violations (as Feinstein, 1989, found for the Nuclear Regulatory Commission). More generally, we need to examine the impact of different enforcement strategies, comparing cooperative with deterrence-oriented approaches, to test the existing theoretical work in this area (Scholz, 1984; Braithwaite, 1985).

Finally, although we find that OSHA enforcement has an effect on injuries, this does not mean that such enforcement is cost-effective, or more efficient than alternative mechanisms (such as an injury tax) for reducing injuries. A true benefit-cost analysis of OSHA enforcement would need to incorporate estimates of the benefits from reducing injuries, the cost of OSHA's enforcement activities, and the expenditures by companies on compliance with OSHA regulations. Evidence that the compliance costs for many health standards exceed the health benefits to workers, as presented in Mendeloff (1990), would suggest that no enforcement program could be cost-effective (in the broadest sense), because compliance itself is not cost-effective. Our results (and future work examining the effectiveness of different types of enforcement) take the current standards and regulatory structure as given, and focus on how OSHA could increase the effectiveness of its enforcement, thus promoting compliance with those standards and reducing injuries.

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FOOTNOTES

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1. I have also found a significant effect of OSHA enforcement on compliance, using plant-level data (Gray and Jones (1989)).

2. The matching procedure was based on various characteristics identified on both the BLS and MIS files: firm name, address, zip code, city, state, number of employees and industry. A record-matching technique developed by Fellegi and Sunter (1969) was used to identify matches across the files, based on the probability of agreement on each of the potentially matching variables (see Gray, 1987). Those cases where it was not clear whether the records were properly matched were hand-checked. Hand-checking used to examine all matches for two states indicated that the error rates for false matches and missed matches were below one percent. To ensure that all plants in the final dataset contained no ambiguous matches, 198 plants were dropped from our original file.

3. Percentage changes, rather than just changes, are used to avoid giving undue influence in the regression to the largest plants, which tend to have much larger absolute changes in injuries than other plants.

4. For studies that examine the determinants of OSHA's enforcement activity see Bartel and Thomas (1985), Scholz and Wei (1986) and Scholz (1991).

5. One might think of looking at inspections which issued serious citations as an alternative to those with penalties, but these two measures are virtually identical in our data. Of the 5,613 inspections with serious violations, 97 percent had penalties; 99 percent of the 5,529 inspections with penalties had serious violations.

6. Gray (1988) finds that the inspection targeting is more closely related to the injury rate of the industry the plant is in, while the problems observed during the inspection (citations and penalties) are more closely related to the injury rate of the plant itself.

7. The results of the estimation using injury levels are available from the authors.

8. This restriction of model stability is rejected for INSP and (less strongly) for PCHHOUR, as seen by the significant test statistics for models IV, V and VI. We impose it to simplify the interpretation of the coefficients in the final model. Doing the same analysis by allowing the coefficients to vary across equations and then averaging those coefficients across all equations of the model gives similar results.

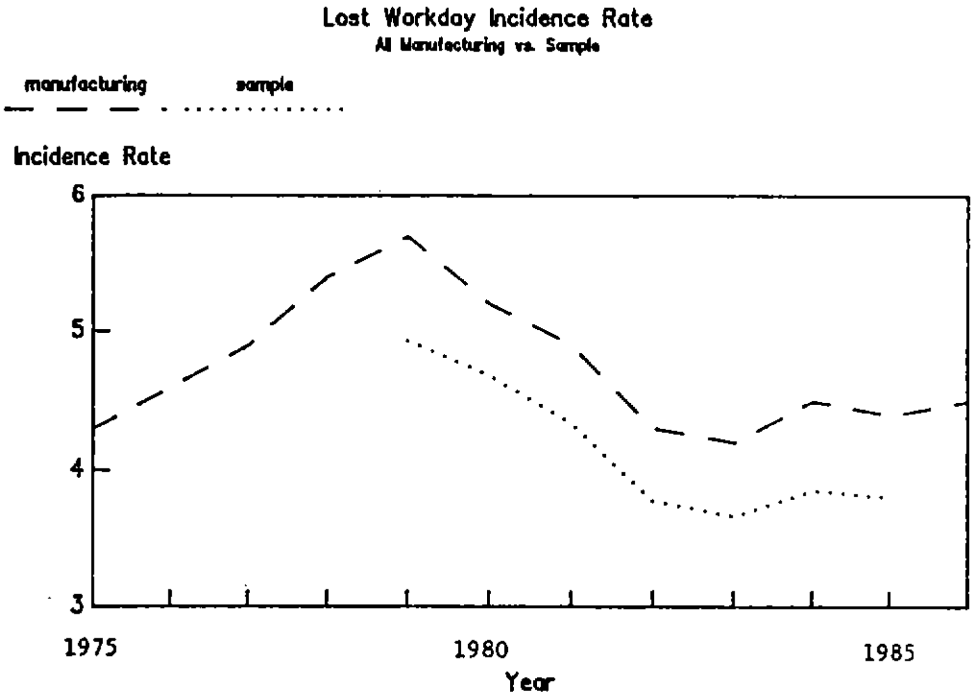
9. The somewhat smaller impact of current inspections on injury changes may indicate a contemporaneous endogeneity of inspections. If plants with rising injuries are sometimes inspected immediately, this would tend to offset some of the negative impact of current inspections on injury changes (and could not be tested for here).

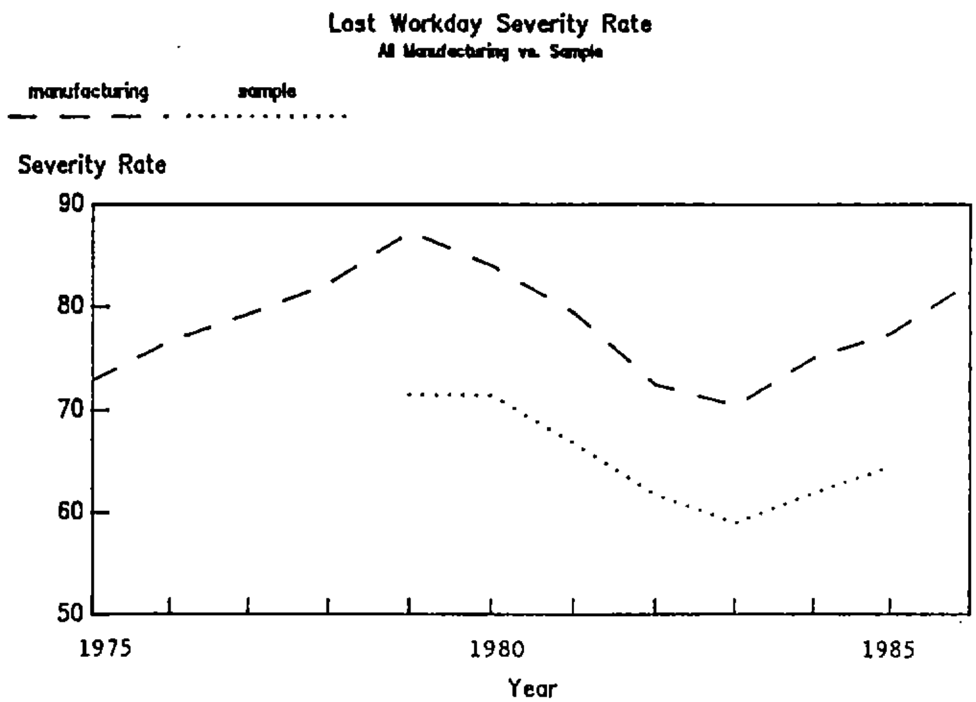
10. If we wished to treat the fixed-effect as reflecting another impact of inspections, rather than the impact of unobserved factors that happen to be correlated with inspections, we could combine the positive (α) and negative (B) effects, giving a total impact of -.13 on PCHNUM and -.09 on PCHDAYS.

11. Distinguishing between specific and general deterrence may be less critical for the heavily inspected plants in our sample, for whom an actual inspection is less surprising than it might be for a typical manufacturing plant.

12. Smith (1979) finds that 1973 inspections were more effective than 1974 inspections; Gray and Jones (1989) find that the first inspection of a plant is more effective than subsequent inspections in reducing violations.

GRAPH 1


Lost Workday Incidence Rate
All Manufacturing vs. Sample
manufacturing
sample
Incidence Rate
6
5
4
3
1975
1980
1985
Year


Lost Workday Severity Rate
All Manufacturing vs. Sample
manufacturing
sample
Severity Rate
90
80
70
60
50
1975
1980
1985
Year

GRAPH 2

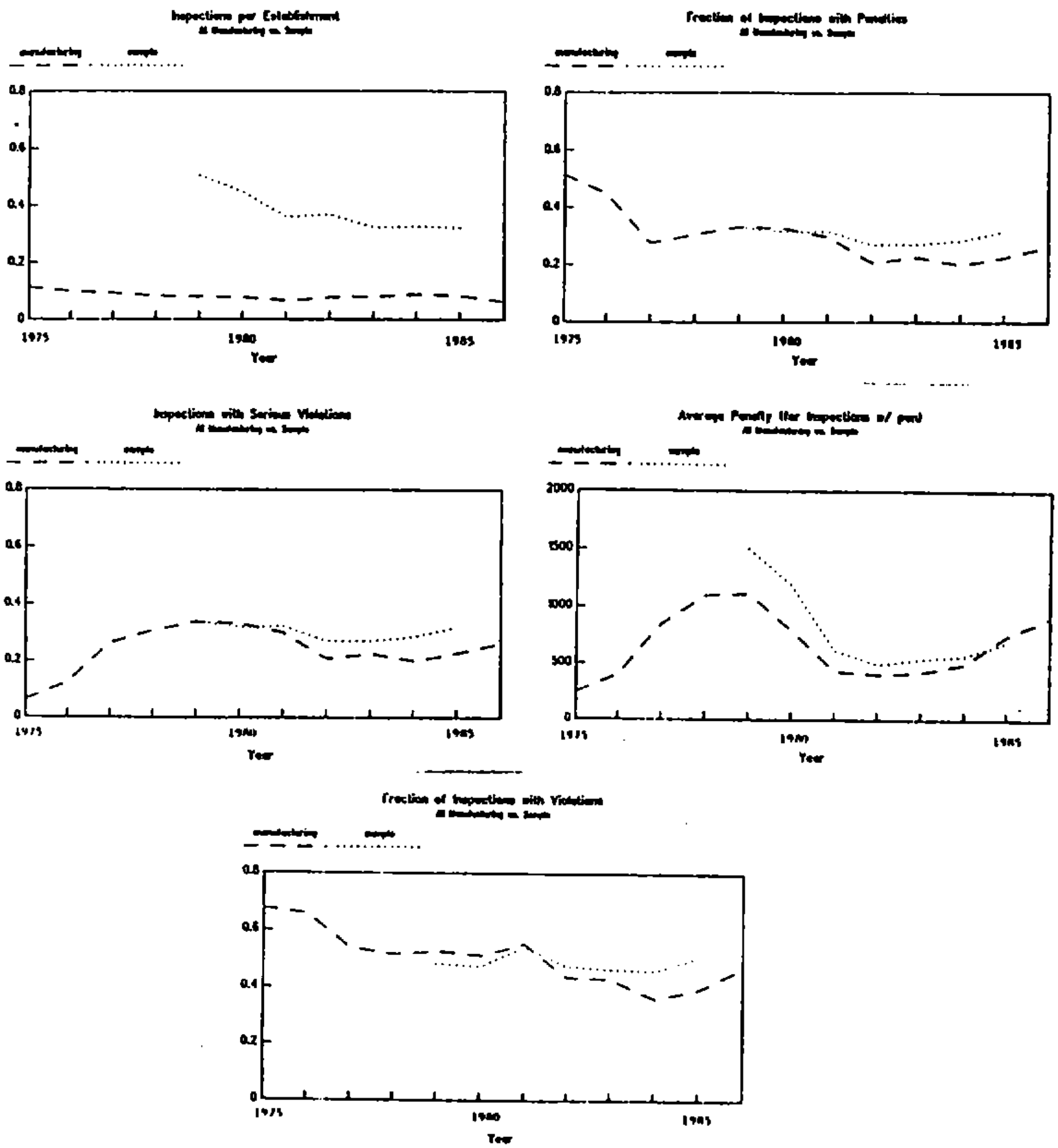

Inspections per Establishment
manufacturing
sample
0.8
0.6
0.4
0.2
0
1975
1980
1985
Year
Fraction of Inspections with Penalties
manufacturing
sample
0.8
0.6
0.4
0.2
0
1975
1980
1985
Year
Inspections with Serious Violations
manufacturing
sample
0.8
0.6
0.4
0.2
0
1975
1980
1985
Year
Average Penalty (for Inspections w/ pen)
manufacturing
sample
2000
1500
1000
500
0
1975
1980
1985
Year
Fraction of Inspections with Violations
manufacturing
sample
0.8
0.6
0.4
0.2
0
1975
1980
1985
Year

Table 1

Comparison of Sample with National Manufacturing Sector

	Sample 1979	National Manufacturing Sector 1979
Number of plants	6,842	349,913 [a]
Number of employees	3,575,394	18,510,498 [a]
Employees per plant	523	54
Lost workday incidence rate	4.93	5.9 [b]
Lost workday injuries	171,333	1,243,000 [b]
Total lost workdays	2,484,704	18,998,000 [b]
Inspections	3,458	28,293 [c]
Inspections w/ penalty	1,145	9,453 [c]
Total penalties	$1,722,973	$10,543,990 [c]
Inspections per plant	.505	.081
Insp w/ penalty per plant	.167	.027
Penalty per inspection	$498	$373
Penalty per insp w/ penalty	$1,505	$1,115

Sources:

a. Census of Manufacturers, 1977.

b. Occupational Injuries and Illnesses in 1979: Summary (BLS: April, 1981)

c. OSHA Management Information System.

Table 2

Correlations between Enforcement and Injuries
(Full Sample of 6,842 plants)

	TNINSP	TNIPEN	YRINSP	YRIPEN
TNINSP	1			
TNIPEN	0.71	1		
YRINSP	0.96	0.66	1	
YRIPEN	0.70	0.99	0.66	1
AVGNUM	0.41	0.40	0.39	0.40
AVGDAYS	0.40	0.39	0.38	0.39
AVGLWDI	0.28	0.37	0.28	0.38
PCHNUM	-0.10	-0.07	-0.10	-0.06
PCHDAYS	-0.05	-0.03	-0.04	-0.02
PCHLWDI	-0.07	-0.05	-0.07	-0.04

Enforcement measures:
- TNINSP - total number of inspections, 1979-85
- TNIPEN - total number of inspections w/ penalty, 1979-85
- YRINSP - number of years with inspection, 1979-85
- YRIPEN - number of years with inspection w/ penalty, 1979-85

Injury measures: (AVG - average, 1979-85; PCH - percentage change, 1979-85)
- NUM - number of lost workday injuries
- DAYS - number of lost workdays
- LWDI - lost workday injury rate

Table 3

Regressions Testing Alternative Enforcement Measures
Dependent Variable: PCHNUM
(34,210 observations: 6,842 plants, 1981-1985)
(standard errors in parentheses)

Model	Enforcement Measure	Enforcement t	t-1	t-2	PCHEMP	PCHHOUR	R^2
1	NINSP	-.0056	-.0168	-.0071	.497	.458	.0785
		(.006)	(.006)	(.005)	(.033)	(.028)	
2	INSP	-.0171	-.0252	-.0227	.498	.458	.0785
		(.010)	(.010)	(.010)	(.033)	(.028)	
3	NIPEN	-.0254	-.0290	-.0285	.496	.459	.0786
		(.012)	(.012)	(.011)	(.033)	(.028)	
4	IPEN	-.0408	-.0366	-.0419	.497	.459	.0787
		(.015)	(.014)	(.014)	(.033)	(.028)	
5	NIPEN	-.0255	-.0255	-.0252			
	and	(.013)	(.012)	(.011)			
					.496	.459	.0787
					(.033)	(.028)	
	NOPEN	.0036	-.0132	.0001			
		(.008)	(.007)	(.007)			
6	IPEN	-.0460	-.0326	-.0413			
	and	(.018)	(.019)	(.017)			
					.496	.460	.0788
					(.033)	(.028)	
	2+PEN	.0523	-.0301	-.0032			
		(.037)	(.029)	(.024)			

Regressions include current PCHEMP and PCHHOUR, year dummies, and current and past 2 years enforcement.

$$PCHNUM_{it} = \beta_t + \beta^E PCHEMP_{it} + \beta^H PCHHOUR_{it} + \sum_{j=0}^{2} \beta^I{}_j Enforcement_{it-j} + u_{it}.$$

Enforcement measures:

- NINSP = number of inspections during year
- INSP = 0-1 dummy for any inspections during year
- NIPEN = number of inspections that imposed penalties during year
- IPEN = 0-1 dummy for any inspections that imposed penalty during year
- NOPEN = number of inspections that didn't impose penalties during year
- 2+PEN = number of penalty inspections, excluding first one during year

Table 4
Testing Restrictions on π Matrix of Coefficients
(T years of data)

Model Number	Description of Model (Test Statistic)	equation for π (parameters in θ)	example of π (for $T=3$)
0	Completely unrestricted ($L_0 = 0$)	unrestricted $(\pi_{11},\ldots,\pi_{TT})$	
I	current year, no fixed effect ($L_I - L_0 \sim \chi^2(T^2-1)$)	$\pi = \beta I$ (β)	$\begin{matrix} \beta & 0 & 0 \\ 0 & \beta & 0 \\ 0 & 0 & \beta \end{matrix}$
II	current year, fixed effect ($L_{II} - L_0 \sim \chi^2(T^2-T-1)$)	$\pi = \beta I + e\alpha'$ $(\beta,\alpha_1,\ldots,\alpha_T)$	$\begin{matrix} \alpha_1+\beta & \alpha_1 & \alpha_1 \\ \alpha_2 & \alpha_2+\beta & \alpha_2 \\ \alpha_3 & \alpha_3 & \alpha_3+\beta \end{matrix}$
I vs. II	Assuming current year only, test for fixed effect ($L_I - L_{II} \sim \chi^2(T)$)		
III	current and past, no f.e. ($L_{III} - L_0 \sim \chi^2((T^2+T-2)/2)$)	$\pi = \beta I + B_{past}$ $(\beta,\beta_1,\ldots,\beta_{(T^2-T/2)})$	$\begin{matrix} \beta & 0 & 0 \\ \beta_1 & \beta & 0 \\ \beta_2 & \beta_3 & \beta \end{matrix}$
I vs. III	Assuming no fixed-effect, test whether past matters ($L_I - L_{III} \sim \chi^2((T^2-T)/2)$)		

Models IV - VI assume both stable coefficients (in B) and a fixed-effect.

Model Number	Description of Model (Test Statistic)	equation for π (parameters in θ)	example of π (for $T=3$)
IV	current, past, future ($L_{IV} - L_0 \sim \chi^2(T^2-3T+1)$)	$\pi = B + e'\alpha$ $(\beta_1,\ldots,\beta_{2T-1},\alpha_1,\ldots,\alpha_T)$	$\begin{matrix} \alpha_1+\beta_1 & \alpha_1+\beta_4 & \alpha_1+\beta_5 \\ \alpha_2+\beta_2 & \alpha_2+\beta_1 & \alpha_2+\beta_4 \\ \alpha_3+\beta_3 & \alpha_3+\beta_2 & \alpha_3+\beta_1 \end{matrix}$
V	current and past ($L_V - L_0 \sim \chi^2(T^2-2T)$)	$\pi = B_{lower} + e'\alpha$ $(\beta_1,\ldots,\beta_T,\alpha_1,\ldots,\alpha_T)$	like model IV, but $\beta_4 = \beta_5 = 0$
V vs. IV	Assuming current and past matter, test whether future matters ($L_V - L_{IV} \sim \chi^2(T-1)$)		
VI	current year, no f.e. ($L_{VI} - L_0 \sim \chi^2(T^2-2T)$)	$\pi = B_{upper} + e'\alpha$ $(\beta_1,\beta_T,\ldots,\beta_{2T-1},\alpha_1,\ldots,\alpha_T)$	like model IV, but $\beta_2 = \beta_3 = 0$
VI vs. IV	Assuming current and future matter, test whether past matters ($L_{VI} - L_{IV} \sim \chi^2(T-1)$)		

Note: The test statistic to be used is the L-statistic given in equation (6), distributed as χ^2 with degrees of freedom equalling the number of restrictions. Each model can be tested against a completely unrestricted π matrix with T^2 parameters (as in line I), or against other restrictive models (I vs. II).

Table 5

Unrestricted Regression of PCHNUM on IPEN, PCHEMP, PCHHOUR (Full Sample of 6,842 plants)

	Year of Dependent Variable					
	1980	1981	1982	1983	1984	1985
INTERCEPT	-0.059	-0.050	-0.097	-0.006	0.029	0.017
IPEN						
1980	0.040	-0.035	-0.015	-0.051	-0.006	0.024
1981	0.011	-0.003	-0.058	-0.067	0.025	-0.002
1982	-0.005	0.052	-0.083	-0.060	-0.022	0.033
1983	0.036	0.055	-0.030	-0.052	0.005	-0.073
1984	-0.015	0.033	0.077	0.009	-0.055	-0.074
1985	-0.014	-0.008	0.059	0.066	0.048	-0.052
PCHEMP						
1980	0.615	0.032	-0.018	-0.131	0.141	-0.083
1981	0.098	0.405	0.126	-0.178	0.190	0.026
1982	0.068	-0.093	0.654	-0.133	0.174	-0.018
1983	0.005	-0.209	0.283	0.467	0.216	-0.054
1984	-0.079	-0.115	0.102	0.109	0.578	-0.063
1985	-0.181	0.012	0.059	-0.008	0.220	0.421
PCHHOUR						
1980	0.365	0.045	0.092	0.015	-0.178	0.114
1981	0.034	0.523	0.004	0.098	-0.194	0.119
1982	0.005	0.118	0.334	0.254	-0.328	0.058
1983	-0.054	0.206	-0.166	0.540	-0.227	0.050
1984	0.119	0.022	-0.154	0.135	0.252	0.166
1985	0.080	-0.005	-0.069	0.024	-0.184	0.591
R^2	.0749	.0562	.0750	.0874	.0540	.0963

Standard errors were .01 for intercept terms, and ranged from .03 to .04 on IPEN, from .07 to .10 on PCHEMP and from .07 to .09 on PCHHOUR.
(note: each equation is a column, not a row, so this table shows π', not π)

Table 6

Unrestricted Regression of PCHDAYS on IPEN, PCHEMP, PCHHOUR
(Full Sample of 6,842 plants)

	Year of Dependent Variable					
	1980	1981	1982	1983	1984	1985
INTERCEPT	-0.051	-0.058	-0.048	-0.008	0.03	0.019
IPEN						
1980	0.049	-0.046	0.014	-0.074	0.026	0.029
1981	0.007	0.020	-0.093	-0.079	0.040	0.032
1982	-0.002	0.077	-0.044	-0.022	-0.093	0.080
1983	0.066	0.053	-0.040	-0.027	0.014	-0.058
1984	0.021	0.008	0.093	-0.026	0.007	-0.095
1985	0.054	-0.051	0.011	0.080	0.054	-0.026
PCHEMP						
1980	0.688	0.097	-0.096	-0.068	0.143	-0.152
1981	0.117	0.360	0.180	0.052	0.043	0.059
1982	0.107	-0.069	0.459	0.058	0.217	0.049
1983	-0.057	-0.113	0.096	0.683	0.062	-0.007
1984	-0.110	0.005	-0.118	0.277	0.449	-0.094
1985	-0.105	-0.005	-0.024	0.013	0.171	0.382
PCHHOUR						
1980	0.220	0.028	0.192	-0.059	-0.082	0.109
1981	-0.083	0.496	0.029	-0.087	0.019	-0.019
1982	-0.069	0.143	0.433	0.035	-0.328	-0.060
1983	0.020	0.145	-0.037	0.242	-0.076	0.067
1984	0.110	-0.033	0.010	-0.051	0.341	0.205
1985	0.018	0.023	-0.031	0.019	-0.153	0.567
R^2	.0376	.0281	.0370	.0431	.0308	.0491

Standard errors ranged from .01 to .02 on the intercept, from .04 to .05 on IPEN, from .10 to .14 on PCHEMP and from .09 to .13 on PCHHOUR.
(note: each equation is a column, not a row, so this table shows π', not π)

Table 7

L-Statistics from Testing Restrictions on π Matrix of Coefficients
(see Table 5 for more complete explanation of tests)

Model Number	Restriction: $L - \chi^2$(df)		PCHNUM IPEN	PCHNUM PCHEMP	PCHNUM PCHHOUR	PCHDAYS IPEN	PCHDAYS PCHEMP	PCHDAYS PCHHOUR
I.	current only	(35)	182.35[a]	44.49	46.08	103.10[a]	41.39	45.28
II.	current and fixed-effect	(29)	98.74[a]	38.87	41.14[c]	80.02[a]	33.99	39.03
I vs II.	Any fixed-effects?	(6)	83.61[a]	5.62	4.94	23.08[a]	7.40	6.25
III.	current and past	(20)	91.69[a]	30.56	24.22	52.74[a]	20.61	15.87
I vs III.	Does past matter?	(15)	90.66[a]	13.91	21.86	50.36[a]	20.78	29.41[b]
	Models IV-VIII assume stable coefficients over time and allow a fixed-effect.							
IV.	current, past, and future	(19)	54.71[a]	20.45	36.74[a]	39.40[a]	24.23	36.93[a]
V.	current and past	(24)	63.59[a]	35.41	38.82[b]	43.35[b]	30.31	37.92[b]
V vs IV.	Does future matter?	(5)	8.88	14.96[b]	2.08	3.95	6.08	0.99
VI.	current and future	(24)	69.49[a]	24.38	39.35[b]	57.42[a]	29.44	39.28[b]
VI vs IV.	Does past matter?	(5)	14.78[b]	3.93	2.61	18.02[a]	5.21	2.35
	Final models (IPEN only): test minor variations on model V.							
VII.	current and past 3 years	(26)	65.24[a]			43.42[b]		
VII vs V.	Do lags>3 matter?	(2)	1.65			0.07		
VIII.	equal ϵ_j terms	(31)	68.13[a]			45.09		
VIII vs VII.	Do all years of INSP contribute equally to ϵ_j?	(5)	2.89			1.67		

Each L-statistic test (distributed as χ^2) restricts the coefficients of one of the three sets of independent variables (IPEN, PCHEMP or PCHHOUR), with the other two sets completely unrestricted. There are 36 coefficients in each set, 6 equations * 6 years of data for each variable. Thus line 1, which only allows one free coefficient, β (diagonal coefficients equal β, with zeros off-diagonal) imposes 35 restrictions on π.

(a - significant at 1% level, b - significant at 5%, c - significant at 10%)

Table 8

Final Estimates
(Full Sample of 6,842 plants)
(standard errors in parentheses)
(Using Chamberlain method to calculate coefficients, standard errors)

Dep Var	IPEN Coefficients t	t-1	t-2	t-3	equal[1] α_j	mean[1] α_j	PCHEMP[2]	PCHHOUR[2]	L
PCHNUM	-.0518	-.0611	-.0585	-.0372	-	.0220	UNR	UNR	65.24
	(.012)	(.013)	(.015)	(.016)		(.007)			
PCHNUM	-.0548	-.0670	-.0623	-.0396	.0242	-	UNR	UNR	68.13
	(.012)	(.012)	(.014)	(.015)	(.004)				
PCHNUM	-.0555	-.0610	-.0627	-.0331	-	.0226	.584	.549	220.12
	(.012)	(.013)	(.015)	(.016)		(.007)	(.032)	(.041)	
PCHNUM	-.0583	-.0664	-.0656	-.0361	.0246	-	.584	.549	222.87
	(.012)	(.012)	(.014)	(.014)	(.004)		(.032)	(.041)	
PCHDAYS	-.0266	-.0765	-.0799	-.0208	-	.0268	UNR	UNR	43.42
	(.017)	(.018)	(.020)	(.023)		(.009)			
PCHDAYS	-.0272	-.0778	-.0778	-.0206	.0271	-	UNR	UNR	45.09
	(.017)	(.017)	(.019)	(.020)	(.005)				
PCHDAYS	-.0236	-.0769	-.0846	-.0109	-	.0263	.458	.413	152.69
	(.017)	(.018)	(.020)	(.022)		(.009)	(.029)	(.036)	
PCHDAYS	-.0243	-.0788	-.0828	-.0113	.0267	-	.459	.414	154.36
	(.017)	(.017)	(.018)	(.020)	(.005)		(.029)	(.036)	

1. In some models, the 6 α_j coefficients in the α vector ($IPEN_j$'s contribution to the plant-specific effect) are restricted to be equal to each other, and their common value is given in the 'equal α_j' column. In the others, the α_j coefficients are allowed to differ, and their mean value is given in the 'mean α_j' column.

2. In some models, the π matrices of PCHEMP and PCHHOUR coefficients are allowed to be completely unrestricted, indicated by 'UNR'. In the others, they are restricted to a single coefficient, ρ, along the diagonal ($\pi = \rho I$), and that coefficient is reported.