THE DETERRENT EFFECT OF ANTITRUST ENFORCEMENT

MICHAEL KENT BLOCK, FREDERICK CARL NOLD, AND
JOSEPH GREGORY SIDAK

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The Deterrent Effect of Antitrust Enforcement

Michael Kent Block and Frederick Carl Nold

Hoover Institution

Joseph Gregory Sidak

Stanford Law School

In this paper we formulate and test a model of collusive pricing in the presence of antitrust enforcement. We show that a cartel's optimal price is likely to be neither the competitive price nor the price that the cartel would set in the absence of antitrust enforcement but rather an intermediate price that depends on the levels of antitrust enforcement efforts and penalties. Our empirical results reveal that increasing antitrust enforcement in the presence of a credible threat of large damage awards has the deterrent effect of reducing markups in the bread industry.

Soon after the passage of the Sherman Act, the Supreme Court determined that horizontal minimum price fixing was so inherently injurious to consumer welfare that it should be illegal per se. Horizontal collusion has since become a major focus of federal antitrust

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enforcement.[1] Through the use of criminal and civil sanctions the Department of Justice (DOJ) has sought not only to remedy specific instances of price fixing, but also to achieve general deterrence of potential price fixing.

This paper is the first systematic attempt to estimate the impact of antitrust enforcement on horizontal minimum price fixing. We develop a simple theoretical model of the collusive pricing decision and then, using data on the bread industry, assess empirically the deterrent effect of public and private antitrust enforcement on the decision to collude.

I. A Simple Model of Collusive Pricing in the Presence of Antitrust Enforcement

A. *Definitions and Assumptions*

We construct a relatively simple model to consider explicitly the effect of antitrust enforcement on the decision of firms within an industry to fix prices collusively. The model uses the following variables and definitions:

$p =$ price of output;
$Q \equiv Q(p) =$ demand for the industry's output;
$C \equiv C(Q) =$ total cost of industry output, including a normal rate of return;
$c \equiv C(Q)/Q =$ average cost;
$mc \equiv \partial C/\partial Q =$ marginal cost;
$\lambda \equiv (p - mc)/mc =$ markup over marginal cost;
$\pi =$ total profits of colluders;
$\gamma =$ level of enforcement efforts directed toward detecting horizontal collusion;
$F \equiv t(p - mc)Q =$ the combined civil and criminal penalty for price fixing, where t is the anticipated damage multiple;
$d \equiv d(\lambda, \gamma) =$ the probability that a collusive pricing scheme will be detected.

The definitions of F and d are essential to our analysis and warrant further discussion.

Our specification of the penalty function, $F \equiv t(p - mc)Q$, reflects that under current statutes a price-fixing conspiracy is subject to both criminal and civil sanctions.[2] The Clayton Act's private treble dam-

[1] Posner (1970, p. 398, table 23) reports that 989 of the 1,551 Department of Justice antitrust cases between 1890 and 1969 contained charges of horizontal conspiracy.

[2] The maximum criminal sanctions for price fixing are imprisonment for 3 years, a fine of $100,000 per individual, and a fine of $1 million per corporation. We do not consider injunctive remedies in this model.

age remedy is the most formidable civil sanction and clearly relates to markups, as our specification of F reflects. If criminal sanctions are small—as they usually are—then civil sanctions provide most of the sting of antitrust enforcement, and our specification of F accurately describes the true penalty for price fixing.[3] Moreover, if, when sentencing price fixers, a district court judge considers the size of the markup to be an index of the cartel's perniciousness, then F is still reasonably descriptive on the rare occasions when criminal fines and sentences are significant relative to private treble damage awards.

In the presence of uncertain antitrust enforcement the decision to collude depends not only on the magnitude of the penalty but also on the probability of detection, which we assume increases with the markup, $[\partial d(\lambda, \gamma)]/(\partial\gamma) > 0$. The closer colluders come to the price that they would charge if no antitrust enforcement existed, the greater their chances of being discovered. Two observations support this hypothesis. First and most obvious, higher markups make customer complaints to the DOJ more likely.[4] Second, as Stigler (1968, pp. 268–70) has noted, a positive relationship probably exists between a conspiracy's detectability and its ability to prevent cheating by individual colluders. For example, a joint-sales agency would assure a set of colluders strict adherence to a monopoly price, but so visible a collusive device would be nearly assured of detection. In general, we hypothesize that the more efficiently a collusive device produces markups, the more likely it will be detected and the colluders convicted.[5] Hence, as we have assumed, the very technology of collusion makes it likely that the probability of detection increases with the markup.[6]

We also assume that the DOJ never charges noncolluding firms:

[3] A comparison of the magnitude of criminal and civil penalties appears in Sec. II. Though a substantial increase in potential criminal sanctions was legislated late in 1974 under the Antitrust Procedures and Penalties Act, there is little evidence of its effectiveness in increasing realized sanctions (see Burnham 1978).

[4] Hay and Kelley (1974) find that complaints by customers are the third most numerous method of detection among DOJ price-fixing cases.

[5] Stigler (1968) presents some indirect evidence on this point. In a sample of DOJ cases he divides the types of collusion between what he considers *efficient* and *inefficient* collusion. The average time from inception of alleged collusion to complaint is significantly shorter for the efficient forms of collusion.

[6] Our study of the bread industry revealed, as tentative empirical evidence of the relationship between the probability of detection and the markup level, that the probability of being investigated for price fixing was positively related to the markup level. We assumed that a lag exists between the price-fixing decision and its detection; then, using a one-period lagged value of our markup indicator as an explanatory variable, we estimated a logit model of the probability that DOJ would initiate an investigation for bread price fixing. The estimated intercept for our 208-observation sample was −2.49, and the estimated coefficient on the lagged-markup indicator was 5.69 with a standard error of 2.43. When the logit function is evaluated using the sample mean values, this coefficient implies that a 1-percentage-point increase in the markup increases the probability of an investigation by 5 percent. Sec. II elaborates on our specification of the markup and the nature of our sample.

$d(0, \gamma) = 0$. We assume further that DOJ does not necessarily detect a pure cartel price with certainty—that is, $d(\lambda^c, \gamma) < 1$, where λ^c is the markup that colluders would choose if no antitrust enforcement existed.

B. *Antitrust Enforcement and Optimal Collusion*

The objective of collusion is to set a price that maximizes expected cartel profits.[7] To model this price-fixing decision simply, we impose three additional assumptions. First, firms produce output under conditions of constant marginal and average cost so that $mc = c$. Second, all firms take part in the joint-pricing decision. Third, the most significant cost of any collusive device is its impact on the probability of detection. For example, the major cost of a joint-sales agency is its visibility, not its resource costs. This third assumption implies that, in the absence of antitrust enforcement, firms could duplicate perfectly the price and output levels of a monopoly.[8]

Formally, the colluders set the price p by maximizing the expected value of their objective function, $E\pi$. That is, the colluders

$$\max_p E\pi = (1 - d)\pi_1 + d\pi_2 \equiv Z, \tag{1}$$

where $\pi_1 \equiv (p - c)Q(p)$ is the profit level if the colluders avoid detection, $\pi_2 \equiv [p - c(1 + \lambda t)]Q(p)$ is the profit level if the colluders are detected, and $\lambda = (p - c)/c$ is the markup under constant costs. The necessary condition for an internal maximum is

$$Z_p = -\frac{\partial d}{\partial \lambda} \lambda t Q(p) + [Q(p) + (p - c)Q'(p)](1 - dt) = 0. \tag{2}$$

Equation (2) has a simple yet intriguing implication: If, as we have assumed, the markup significantly affects the probability of detection, then neither the competitive price ($\lambda = 0$) nor the pure cartel price markup ($\lambda = \lambda^c$) can satisfy equation (2).[9] Antitrust penalties, therefore, do not necessarily eliminate price fixing. They are, however, likely to reduce the optimal markup.[10]

[7] This assumes that the colluders are risk neutral. An analysis that allows for risk aversion is presented in Block, Nold, and Sidak (1978). The results of that analysis do not differ substantially from those presented here.

[8] As long as we restrict our attention to industries with a history of conspiracy, considering intraindustry cheating would complicate the model without significantly adding to our understanding of collusive pricing in the presence of antitrust enforcement.

[9] The pure cartel markup is the markup λ^c satisfying the condition $Q(p) + (p - c)Q'(p) = 0$.

[10] If the detection probability were not sensitive to the markup level, then the decision to price fix would be a simple either/or proposition. In this case, as long as $(1 - dt) > 0$, the colluders would set the price at the pure cartel level. Clearly no price fixing would

As long as price fixing is a favorable gamble for some markup, $(1 - dt) > 0$—and our assumptions assure it will be—then risk-neutral colluders obviously will not choose the competitive price. Also, although the "unfairness" of the price-fixing gamble at the pure cartel markup, $[1 - d(\lambda^c, \gamma)t] < 0$, is sufficient to dissuade the colluders from choosing $\lambda = \lambda^c$, it is not necessary. In general, colluders will stop marking up when both $[Q(p) + (p - c)Q'(p)]$ and $(1 - dt)$ are positive. To deter colluders from choosing the full cartel markup is rather easy; to deter all collusive pricing in a conspiracy-prone industry is nearly impossible.

C. *The Effects of Changes in Enforcement Efforts and Penalties on Collusive Markups*

To assess the effect on markups of increases in enforcement efforts, we consider the effect of changes in γ on the optimal λ, or $\partial\lambda/\partial\gamma$. Totally differentiating equation (2) with respect to λ, we obtain

$$\frac{\partial\lambda}{\partial\gamma} = \left(\frac{t}{cZ_{pp}}\right)\left\{\frac{\partial^2 d}{\partial\lambda\partial\gamma}\lambda Q + \frac{\partial d}{\partial\gamma}[Q(p) + (p - c)Q'(p)]\right\}, \qquad (3)$$

where Z_{pp} is the second-order derivative. Only in the rather perverse case where an increase in enforcement actually reduces the impact of the markup on the detection probability—that is, where $(\partial^2 d)/(\partial\lambda\partial\gamma) < 0$—does an increase in enforcement have an ambiguous effect on the markup. In most cases of practical importance we expect that DOJ efforts designed to increase the probability of detection also reduce the markup.

Increasing price-fixing penalties is often advocated as an efficient means to achieve deterrence.[11] The effect of such an increase is expressed by the comparative static derivative:

$$\frac{\partial\lambda}{\partial t} = \left(\frac{1}{cZ_{pp}}\right)\left\{\frac{\partial d}{\partial\lambda}\lambda Q + d[Q(p) + (p - c)Q'(p)]\right\}, \qquad (4)$$

which indicates that a higher penalty unambiguously decreases the optimal markup. In summary, an increase in either the probability of

occur if $(1 - dt) < 0$. Enforcement efforts would determine simply whether or not it paid for a cartel to fix prices. If it did, colluders would set markups at the full cartel level; if it did not, they would not collude. This model, in which the probability does not depend on changes in λ, would make the empirical analysis somewhat more straightforward. However, such a formulation would be less descriptive of the actual situation confronting colluders. Formally, our assumptions do not preclude a corner solution at $\lambda = \lambda^c$. If the detection probability displays a limited responsiveness to markups, then eq. (2) may not be satisfied as an equality. However, it is unlikely, given the technology of collusion and the existence of punitive sanctions for price fixing, i.e., $t > 1$, for $(1 - dt)$ to be positive at $\lambda = \lambda^c$.

[11] See Sec. III; see also U.S. Office of the President 1969, and Elzinga and Breit 1976.

detection or the penalty for price fixing can be expected in most cases of interest to reduce the collusive markup.

II. Empirical Findings

Our theoretical model suggests that increases in enforcement levels or penalties for price fixing generally reduce collusive markups. Though straightforward to derive, this implication is hardly trivial to test. Unfortunately, we do not have a set of national, or even regional, industries having identical products, costs, and demand conditions but varying levels of antitrust enforcement. We must assess the impact of antitrust enforcement in a more problematic environment.

A. Choice of Industry

To test the implications of our deterrence model, we analyzed the market for white pan bread, a homogeneous commodity not only regionally produced and consumed but also well represented among DOJ price-fixing cases. During our sample period, bread cases were the most common among DOJ's food price-fixing cases.[12] In addition, the bread industry has well-recorded annual input and output prices compiled for selected cities by the Bureau of Labor Statistics (BLS) (see U.S. Department of Labor, Bureau of Labor Statistics 1964–76, 1965–76).[13] Hence, we constructed a sample that enabled us to use both cross-sectional and temporal variations in product prices, costs, and antitrust enforcement.

B. Estimating City-specific Markups

Developing an indicator for price markups in the bread industry involved two steps. First, we examined a standard recipe for a loaf of white bread.[14] We subtracted from each bread price observation the

[12] The FTC observed that the bread-baking and distribution industry has "the essential characteristic of a conspiracy-prone industry—relatively few sellers in individual markets" (U.S. Federal Trade Commission 1967, p. 135).

[13] The BLS publications contained comparable price data for white bread for 20 major cities. For 12 of these cities the data went back as far as 1955, although for the remaining eight they extended only to 1968. Input price data, though not so complete, proved adequate to construct a sample of 228 observations: 12 major cities for 1964–76, and eight additional major cities for 1968–76. Cities included throughout the entire period (1964–76) were Baltimore, Boston, Chicago, Cleveland, Detroit, Los Angeles, New York, Philadelphia, Pittsburgh, St. Louis, San Francisco, and Washington, D.C. For 1966–76, we were able to add Atlanta, Cincinnati, Dallas, Houston, Kansas City, Minneapolis, San Diego, and Seattle to the sample.

[14] The cost of ingredients (IC) for a 1-pound loaf of white bread is $\mathrm{IC} = .6350P_F + .0571P_S + .0026P_O + .035P_M$, where P_F is price/pound of flour, P_S is price/pound of

TABLE 1

EFFECT OF NONINGREDIENT INPUT COSTS ON ADJUSTED BREAD PRICE (PADJ)

Independent Variables	
PELEC	.256*
	(4.62)†
PGAS	2.49
	(3.15)
LABOR	1.07
	(7.14)
Constant	7.05
Observations (N)	228
R^2	.49
F-statistic (3,224)	72.93

* The estimated coefficient.
† The value of the estimated coefficient divided by its estimated standard error.

component attributable to the cost of the ingredients, IC, calculated from the recipe and from BLS ingredient prices. Formally, we defined the recipe-adjusted bread price, PADJ, as:

$$\mathrm{PADJ}_{it} \equiv p_{it} - \mathrm{IC}_{it}, \tag{5}$$

where i is the city and t the time index.[15] Differences in profits and other noningredient input costs cause PADJ to vary across cities and over time.

As a second step in developing a measure of markups, we estimated the amount of variation in PADJ attributable to variations in energy and labor costs. Table 1 contains the results of this analysis; PELEC and PGAS are measures of electricity and natural gas prices, and LABOR is a measure of wage rates for truck drivers. We assume this wage to be a proxy for general labor costs.[16] The results in table 1 suggest that variations in energy and labor input costs account for a reasonable amount of the variation in adjusted bread prices.[17]

sugar, P_O the price/pound of cooking oil, and P_M is the price/pound of dry milk. All prices are retail, except dry milk, which is wholesale. Cooking oil is used as a proxy for shortening (U.S. Executive Office of the President 1977, p. 7).

[15] The sources of price data were U.S. Department of Labor, Bureau of Labor Statistics (1965–76; 1969).

[16] Specifications using food and kindred workers' wages as well as alternative functional forms were also estimated. These different specifications yielded similar results (see Block et al. 1978).

[17] Of course, measures of goodness of fit for this regression substantially understate the importance of variations in input prices since the dependent variable, PADJ, is already net of the recipe or ingredient. Our entire procedure, taking account of both IC and the estimated cost function, accounts for approximately 82 percent of the variation in the price of bread (p). In general, prices for all significant inputs are included in cost functions. Our asymmetric treatment of input prices amounts to using

To construct a measure of the markup on bread, we then used the recipe costs and the results of this regression to estimate M, the markup indicator:

$$M \equiv [p - (\text{NIC} + \text{IC})]/(\text{NIC} + \text{IC}), \tag{6}$$

where all subscripts are suppressed and NIC refers to the fitted values generated by the equation in table 1.[18] Equation (6) estimates the markup by first subtracting from the market price the sum of the known ingredient costs (IC) and an estimate of other noningredient costs (NIC), and then dividing this difference by estimated unit costs. The higher the residual as a proportion of unit costs, the higher the markup.

C. *Estimating the Effect of DOJ Antitrust Enforcement on Collusive Markups*

Antitrust enforcement and penalties are only two of the many factors that actually determine markup levels.[19] Certainly, all factors that influence the price elasticity of demand also influence the markup level. Moreover, although we have assumed antitrust penalties to be the only cost of collusion, the resource cost of managing and policing a cartel also influences the optimal markup in the presence of antitrust enforcement. Assessing the effect of antitrust enforcement, therefore, requires a workable method of controlling for such outside influences. Influences generated by aspects of market structure that do not change rapidly—or at least that change significantly less rapidly than the antitrust variables—can be controlled by considering not the markup level but *changes* in the markup level.[20] We therefore used first differences in the markup level, or

$$\Delta M_{it} = M_{it} - M_{it-1}, \tag{7}$$

in testing the effectiveness of antitrust enforcement.[21]

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a priori information—the recipe for bread—to supplant estimation of some coefficients. It may be argued that this procedure increases the efficiency of the estimation, but it precludes substitution among raw materials and other inputs.

[18] Because a simple regression generates NIC, approximately half the observations for NIC exceed PADJ and thus generate a negative M. For our purposes, these negative values pose no problems.

[19] We structured our test of antitrust enforcement around markups rather than the traditional—but indirect—test of overall profitability or rate of return. One reason for using the markup was the very directness of the test. Also, the availability of regional and city data enabled us to conduct more powerful tests of the effectiveness of antitrust enforcement than could have been performed with available data on rates of return.

[20] Controlling for these factors by actually assembling the relevant information on market structure for this industry at the city or SMSA level was not feasible.

[21] This procedure also facilitated our use below of a straightforward measure of regional antitrust enforcement.

1. Measuring Detection Probabilities, DOJ Enforcement Efforts, and Remedial Effects

We assume in our formal model of collusion that for any markup level the probability of detection is a function of DOJ enforcement efforts. Unfortunately, this straightforward theoretical proposition does not suggest a unique empirical counterpart for measuring either the probability of detection or DOJ's enforcement efforts. This formulation does suggest, however, that for any markup level the probability of detection is related to the capacity of the Antitrust Division to initiate cases. To the extent that the Division's litigation capacity relates to its expenditure level, the Division's annual budget should provide an indirect measure of this enforcement capability.[22]

A more direct measure of enforcement is simply DOJ price-fixing prosecutions. A price-fixing prosecution is rare enough for colluding firms in the affected industry to take special notice.[23] We have assumed, therefore, that each price-fixing case that the DOJ brings against a bread producer increases, for any markup level, the probability of prosecution perceived by other bread producers operating in the same DOJ region.[24]

We had sufficient price data to estimate changes in markups for 1965–76. We constructed a regional antitrust enforcement variable, DOJREG, for this period by setting the variable equal to one for each city within a region where the Antitrust Division filed an action that year—except for the city incurring the action—and by setting the variable equal to zero otherwise.[25] In other words, DOJREG is a shift

[22] A positive and significant relationship exists between the Antitrust Division's annual budget and the number of price-fixing cases brought by the Division. Using data on price-fixing cases during 1964–76 supplied by the Antitrust Division's Economic Policy Office, we estimated the relationship:

$$\text{DOJPF} = -11.39 + \underset{(2.42)}{.003\text{BUDGET}},$$

between the number of price-fixing cases brought annually by the Division (DOJPF) and its budget (BUDGET) measured in thousands of 1967 dollars (2.42 is the t-statistic).

[23] We can provide a formal rationale for this observation by assuming that colluders use Bayesian methods to estimate the probability that they will be apprehended in a particular period. In this formulation, whenever colluders are apprehended, colluders' estimate of the probability of apprehension increases, and that increase is dramatic if their a priori distribution is diffuse and has a small mean. Also, after an initial impact, the effect of the case on the apprehension probability estimated by the colluders deteriorates if no new cases are brought.

[24] The Antitrust Division operated seven regional offices during the years of our sample: New York, Philadelphia, Cleveland, Chicago, San Francisco, Los Angeles, and Atlanta. The Antitrust Division established a Dallas office in 1976 and realigned the areas covered by the regional offices. This study used the earlier seven regions for 1976 rather than realigning all regions for that single year. For coverage of regions, see U.S. Department of Justice, Antitrust Division (1973).

[25] This variable was constructed using data from the Commerce Clearing House (1955–75; 1966–76). A summary of those data appears in Block et al. (1978). Filing dates and other summary information were checked against a special listing of bread

variable designed to capture the changes in the perceived probabilities of cartel failure. Its form presumes that the colluders' estimate of the probability that DOJ will initiate a case for collusion increases after a DOJ price-fixing action within the same region.[26]

The variable DOJREM measures the effect of the antitrust action in the city where the Antitrust Division actually prosecuted a bread producer. In constructing DOJREM, we assumed that the impact of the antitrust action on the firms specifically prosecuted would differ significantly from the impact on firms in other cities in the same region. Basically we assumed that, for strategic reasons, the timing of the reduction in markups by prosecuted firms would not coincide with the reduction made by firms in nonaffected cities. To capture this effect, we set DOJREM equal to one in a city 1 year after the Antitrust Division had filed an action there.

2. Estimated Deterrent Effects

In table 2 we present our estimates of the effect of DOJ enforcement variables on markups in the bread industry. Again, the dependent variable, ΔM, is the annual change in the markup on white bread for the cities in our sample. The results in table 2 strongly suggest a deterrent effect of DOJ enforcement efforts.[27] First, the coefficient on

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price-fixing cases prepared for us by the Economic Policy Office, Antitrust Division, U.S. Department of Justice, for the years 1963–76.

[26] An alternative hypothesis would be that, since DOJ has limited resources, a prosecution in a region signals that an additional prosecution in the same region is unlikely. Hence a firm actually would reduce its estimated probability of detection after a DOJ case in its own region. This hypothesis is equivalent to predicting that drivers on a highway accelerate when they see a car already being ticketed.

[27] Our formulation, which posits that increases in the markup increase the detection probability, generates the structural equations: $\Delta d = \xi + \beta\Delta\lambda + \tau\Delta\text{BUDGET} + \eta\text{DOJREG}$ and $\Delta\lambda = \psi + \alpha\Delta d + \theta\text{DOJREM}$. Therefore, the reduced forms are

$$\Delta d = \omega + \frac{\tau}{1-\alpha\beta}\Delta\text{BUDGET} + \frac{\eta}{1-\alpha\beta}\text{DOJREG} + \frac{\beta\theta}{1-\alpha\beta}\text{DOJREM}$$

and

$$\Delta\lambda = \rho + \frac{\alpha\tau}{1-\alpha\beta}\Delta\text{BUDGET} + \frac{\alpha\eta}{1-\alpha\beta}\text{DOJREG} + \frac{\theta}{1-\alpha\beta}\text{DOJREM}.$$

Our estimated reduced-form coefficients in table 2 are of the form $(\alpha\tau)/(1-\alpha\beta)$, $(\alpha\eta)/(1-\alpha\beta)$, and $(\theta)/(1-\alpha\beta)$. We are mainly interested in deducing the sign of α given the sign of the reduced-form coefficients. The usual approach would be to estimate the other reduced-form equation in the system which takes Δd to be a function of the exogenous variable. Unfortunately this approach cannot be applied directly to this problem. We do not have a proxy for Δd or measures of the detection probability at a regional level. Even at a national level measuring d is problematic. However, given our formulation of the determinants of detection, if we assume that DOJ expenditures have some efficacy—i.e., $\tau > 0$—then the condition necessary to deduce the nonpositivity of α from the reduced-form coefficients is that $(1-\alpha\beta) > 0$. This condition is required for Walrasian stability in the collusion market and in conjunction with our reduced-form

TABLE 2

ESTIMATED EFFECTS OF CHANGES IN DOJ ENFORCEMENT ON CHANGES IN MARKUPS IN THE BREAD INDUSTRY, 1965–76

Independent Variables				
ΔBUDGET	−.015*	−.015	−.024	−.020
	(−2.74)†	(−2.68)	(−4.06)	(−3.65)
DOJREG	−.025	−.026	−.025	−.027
	(−2.05)	(−2.21)	(−2.09)	(−2.26)
DOJREM		−.046	−.046	−.044
		(−2.32)	(−2.41)	(−2.32)
ΔFOODM			+.058	...
			(2.33)	
ΔGENM				−.010
				(−1.60)
Constant	.011	.013	.014	.017
R^2	.055	.082	.113	.101
F-statistic	5.93 (2,205)	6.04 (3,204)	6.47 (4,203)	5.68 (4,203)

NOTE.—Each regression is based on 208 observations.
* This coefficient is estimated per million dollars.
† The value of the estimated coefficient divided by its estimated standard error.

our general measure of changes in enforcement capacity—the change in the real value of the Antitrust Division's budget, ΔBUDGET—is negative and significant. In other words, an increase in the enforcement capacity of the Antitrust Division appears to reduce markups on white bread. Second, the coefficient on our direct measure of DOJ's enforcement activity (DOJREG) is negative and significant, suggesting that a price-fixing case against bakers in one city induces bakers in neighboring cities to reduce markups. This result comports both with our formal theoretical results and with the related conjecture in the Stigler report that "every victory" in seeking out price fixing "weakens the efficiency of undetected collusion."[28] Finally, the coefficient on DOJREM, the variable measuring the remedial effect of a price-fixing case, is negative and significant. Once discovered and prosecuted, colluders apparently "remedy" their price fixing by reducing their markups in the following year.[29]

results implies that there is a deterrent effect, i.e., $\alpha < 0$. In addition to this theoretical restriction on the sign of $(1 - \alpha\beta)$, we noted previously that there is a positive relationship between the total number of price-fixing cases brought by the Antitrust Division and the Division's budget level. If we can assume that other collusion-prone industries made decisions about collusion in the same way as bread producers, then this aggregate regression gives information about the second set of reduced-form coefficients. Again, if $\tau > 0$, the sign of the reduced-form coefficient on ΔBUDGET indicates that $(1 - \alpha\beta)$ is positive.

[28] See U.S. Office of the President, President Nixon's Task Force on Productivity and Competition 1969.

[29] It is only necessary to assume that DOJ capacity is productive ($\tau > 0$) to infer from our empirical results that $\eta > 0$, or that filing of a DOJ case in the region increases d.

The ΔFOODM and ΔGENM variables in table 2 control for general year-to-year variations in manufacturing markups that might not be adequately controlled by the first-difference procedure. The variable ΔFOODM is an annual series of the first differences in markups of food-and-kindred-product manufacturers; ΔGENM is an analogous series for all manufacturing firms.[30] The coefficient on ΔFOODM is of the expected sign, suggesting that, holding enforcement constant, markups in the bread industry move in the same direction as markups in other food-related industries. Although using the general manufacturing markup (ΔGENM) in the equation does not alter the estimates of the coefficients on the enforcement variables, the sign of the coefficient on ΔGENM is curious.

D. *Class Actions and the Effect of Antitrust Enforcement on Markups*

Historically, trial judges have punished price fixers leniently.[31] The cases collected for this study were no exception. All but two of the 17 bread price-fixing cases between 1957 and 1976 involved *nolo contendere* pleas, and in only one case did a defendant serve an actual prison sentence; moreover, total fines as a percentage of the pretax profits of the colluding firms averaged only 7 percent.[32] Neither imprisonment nor monetary penalties posed a credible threat to colluding firms. We hypothesized, therefore, that the deterrent effect of DOJ's enforcement efforts came not from the threat of publicly imposed fines or imprisonment, but from the increased likelihood of an award of private treble damages to bread consumers or distributors.[33]

[30] Both series were computed from data reported by the Federal Trade Commission in *Quarterly Financial Report for Manufacturing, Mining, and Trade Corporations*, in U.S. Council of Economic Advisers, *Economic Report of the President* (annual). Of course, both markup measures are influenced by antitrust enforcement efforts to the extent that collusion is important in these manufacturing industries.

[31] E.g., during 1966–76 over 85 percent of all price fixers who were convicted or who pleaded *nolo contendere* did not serve prison terms (see Block et al. 1978).

[32] For the 16 criminal cases involving either a *nolo* plea or a conviction the actual criminal fines for price fixing were:

	Minimum	Maximum	Average
Individuals ($)	1,031	9,966	4,025
Firms ($)	2,930	50,638	20,690
Fines, as % of defendants' annual sales	.09	1.0	.31

All fines are in 1976 dollars. Data on the ratio of fines to sales were derived from court documents relating to 13 cases (76 firms) during 1957–75. The overall ratio of net profits to sales was obtained from a study by the U.S. Executive Office of the President, Council on Wage and Price Stability (1977).

[33] Civil actions alleging horizontal price fixing are possible but, certainly in this industry, uncommon without a preceding criminal case.

Since bread price fixing generally causes a small injury to many individual distributors and consumers, private damage recovery is usually feasible only through a class-action suit. Class actions enable plaintiffs who are numerous, and whose independent damage claims are too insignificant to justify litigation, to maintain a single action for their aggregate damages.[34] District court documents revealed that settlements in class actions for price fixing in the bread industry were almost 10 times greater than government-imposed fines.[35]

To test the hypothesis that private class actions actually provided the effective penalty in price-fixing cases, we partitioned our sample into the periods before and after class actions became a credible threat in the bread industry. Since only one class action in our sample did not follow a DOJ case, we assumed in partitioning the sample that

[34] These actions, brought under Rule 23 of the Federal Rules of Civil Procedure, have become far more frequent since the Supreme Court amended that rule in 1966. The amended Rule 23 contributed to this greater frequency by changing the procedure for becoming a member of the class. Originally, Rule 23 required persons to "opt into" the class before they could benefit from the adjudication of the class's cause of action; the amended rule instead presumed persons to be class members unless they affirmatively "opted out" of the class. Although surprisingly few historical data exist on class actions, the data that do exist—particularly the information on docket entries for the Southern District of New York collected by the American College of Trial Lawyers' Special Committee on Rule 23—suggest that the 1966 amendments to Rule 23 made the class action a much more attractive legal device (American College of Trial Lawyers 1972). A recent alternative to the antitrust consumer class action is the *parens patriae* device by which a state attorney general may sue on behalf of the consumers in his state. For the purposes of deterrence, *parens patriae* actions are virtually identical to class actions (see Block et al. 1978).

[35] A search of Commerce Clearing House (1955–75; 1966–76), McLaughlin (1976), Newberg (1977), and the LEXIS computer file (1978) revealed seven major class actions for price fixing in the bread industry since 1966. According to district court documents, class-action settlements or damages during 1971–76 obtained for price fixing in the bread industry were:

	Minimum	Maximum	Average
Cases ($)	1,197,810	6,100,000	1,998,646
Firms ($)	39,562	1,220,000	293,919
Damages as % of defendants' annual sales	.41	19.68	2.87

All fines are in 1976 dollars. These figures are based on data for five of the seven recorded cases. One of the seven original class actions had no settlement or award since the district court refused to certify the class. No data at all were available on another case. All five cases were settled rather than litigated to judgment. Details of these class actions appear in Block et al. (1978, appendix table VI). Whereas average damages as a percentage of defendants' sales were only 10 times average fines as a percentage of defendants' sales, average damages per firm were almost 15 times average fines per firm. This occurred because the average sales per defendant differed significantly between the two samples: $5,795,271 for the fine calculations and $10,254,205 in the class-action calculations.

TABLE 3

ESTIMATED EFFECT OF DOJ ENFORCEMENT AND CLASS ACTIONS ON MARKUPS IN THE BREAD INDUSTRY, 1965–76

Independent Variables		
ΔBUDGET11	−.018* (−.622)†	. . .
ΔBUDGET12	−.014 (−2.68)	. . .
ΔBUDGET21	. . .	−.002 (−.158)
ΔBUDGET22	. . .	−.019 (−3.53)
DOJREG11	+.004 (.160)	. . .
DOJREG12	−.037 (−2.70)	. . .
DOJREG21	. . .	−.019 (−1.03)
DOJREG22	. . .	−.029 (−1.87)
DOJREM	−.046 (−2.43)	−.042 (−2.19)
Constant	.013	.013
Observations (N)	208	208
R^2	.0926	.1024
F-statistic	4.13 (5,202)	4.60 (5,202)

* This coefficient is estimated per million dollars.
† The value of the estimated coefficient divided by its estimated standard error.

class actions affected primarily the penalty cost of detection, not the probability of detection.[36] We considered as partition dates the year that a district court first certified a class in a bread case (1970) and the year that the Administrative Office of the Courts began reporting class-action activity (1972). Before 1970 class actions probably were not a credible threat in the bread industry; by 1972 no doubt remained that class actions had become prevalent and important. To accomplish the actual partitioning of the sample, we split the enforcement variables ΔBUDGET and DOJREG around 1970 and 1972. In table 3, ΔBUDGET11 and DOJREG11 are ΔBUDGET and DOJREG for 1965–69, while ΔBUDGET12 and DOJREG12 are the variables for 1970–76. The comparable variables partitioned around

[36] To determine when, after the 1966 amendments to the class-action provisions in the Federal Rules of Civil Procedure, the class action became a credible threat in the bread industry, we searched for both recorded and unrecorded class actions involving horizontal price fixing of bread products. None of the seven class actions we found was filed before 1968, actually certified before 1970, or settled before 1971.

1972 instead of 1970 are ΔBUDGET21, ΔBUDGET22, DOJREG21, DOJREG22.[37]

The estimates in table 3 are consistent with our hypothesis that class actions represent the effective penalty in price-fixing cases. This result does not seem to depend upon whether we partition the enforcement variables around the date of the first class certification or around the date of the first reporting of class-action activity by the Administrative Office of the Courts. For either partition, only in the latter period, when class actions represented a credible threat, did a significant deterrent effect result from either an increase in the Antitrust Division's resources or from the actual prosecution of a horizontal price-fixing conspiracy.[38]

The pattern in the bread industry is clear. A successful federal prosecution signals to consumers that a treble damage suit has become feasible. Private plaintiffs subsequently provide the effective penalty in the form of class actions for treble damages.[39] For price fixing in this conspiracy-prone industry—and, we suspect, for price fixing in general—deterrence has been a product of both public and private enforcement efforts.[40]

[37] Formally, the definitions of the variables are: ΔBUDGET21 = ΔBUDGET, 1966–71, zero otherwise; ΔBUDGET22 = ΔBUDGET, 1972–76, zero otherwise; DOJREG21 = DOJREG, 1966–71, zero otherwise; DOJREG22 = DOJREG, 1972–76, zero otherwise.

[38] While the estimates in table 3 do not include the markup controls, ΔFOODM and ΔGENM, regressions with these controls yielded similar results. Several factors possibly confound this analysis of the deterrent effect of class actions. First, the average criminal fine imposed by district courts might have increased over the period, or might simply have been larger when class actions became a relevant concern. To check for this possibly confounding influence, we estimated time trends for several measures of criminal fines. Overall, the evidence suggests that an increase in criminal penalties did not confound our results on the deterrent effect of class-action suits. Second, the early 1970s included a period of price controls, and DOJREG12 and/or ΔBUDGET12 possibly proxied for the effect of these controls. We tested whether the enforcement variables performed this role by including directly in the markup regression a dummy variable for price controls. The results of this procedure suggest that, although price controls had a significant depressing effect on markups, the enforcement variables were not merely a proxy for price controls. Finally, we controlled for pure time trends associated with changes in demand or market structure within each city. This was accomplished by regressing the dependent variable (ΔM) against city dummies along with the deterrent variable. The effect of the enforcement variables remained significant and unchanged and nearly all of the city dummies were statistically insignificant (see Block et al. 1978).

[39] A *nolo* plea apparently is a sufficient signal. Although guilty verdicts are prima facie evidence in a treble damage suit and should induce private enforcement, they are rare in price-fixing cases. In our sample of 17 price-fixing cases, one case ended in an acquittal and another in a conviction; the other 15 involved *nolo* pleas. Yet even a government case that ends in a *nolo* plea signals to potential private plaintiffs that they probably would prevail in a damage suit against the alleged price-fixing conspiracy. In fact, all the reported class actions in our sample that followed a government action and received class certification eventually obtained a settlement award.

[40] The amendment of Rule 23 in 1966, of course, was the event which facilitated class actions, thereby increasing the power of the federal government to deter price fixing.

III. Conclusion

In this paper we formulated and tested a simple model of collusive pricing in the presence of antitrust enforcement. We showed that if a cartel's probability of detection increases with its markup, then the cartel's optimal price is neither the competitive price nor, in most cases, the price that a cartel would charge in the absence of antitrust enforcement, but rather an intermediate price that depends on the levels of antitrust enforcement efforts and penalties.

Our empirical results revealed that increasing DOJ's enforcement capacity or filing a DOJ price-fixing complaint had the deterrent effect of reducing markups in the bread industry. We noted that government-imposed price-fixing penalties were trivial and found support for the proposition that the effective deterrent to price fixing was the credible threat of large damage awards to private class actions that followed DOJ's case against the same conspiracy. Consequently, only after class actions became a credible private remedy did the Antitrust Division's enforcement capacity or its filing of a bread price-fixing case deter collusion in the conspiracy-prone bread industry.

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Landmark Supreme Court decisions regarding class-action suits such as Eisen v. Carlisle and Jacquelin (417 U.S. 156 [1974]) and legislation such as the Hart-Scott-Rodino Antitrust Improvement Acts are likely to have similarly far-reaching effects. Some very preliminary attempts to assess the impact of these events on collusion appear in Block et al. (1978).

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