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THE DETERRENT EFFECT OF ANTITRUST ENFORCEMENT: A THEORETICAL AND EMPIRICAL ANALYSIS

M.K. Block, F.C. Nold and J.G. Sidak

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CENTER FOR ECONOMETRIC STUDIES OF THE JUSTICE SYSTEM

Hoover Institution, Stanford University

EXECUTIVE SUMMARY

This paper formulates and tests a simple model of collusive pricing behavior. It shows that the price-fixing decision is, in general, not a discrete choice, but instead a continuous choice of the optimal degree of collusive price markup. The optimal markup is shown to depend on enforcement efforts and penalties. Empirical tests, using data on the bread industry, reveal that Department of Justice prosecutions for price-fixing have deterred collusion insofar as they have reduced the optimal markup. But the deterrent effect of government cases appears to depend on the prospect of subsequent private treble damage suits, especially class actions. More tentative results suggest that the consumer class action is particularly important in deterring price-fixing.

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I. INTRODUCTION

Soon after the passage of the Sherman Act, the Supreme Court determined that horizontal price-fixing was so inherently injurious to consumer welfare that it should be illegal per se, regardless of its apparent "reasonableness." Horizontal collusion has since become a major focus of federal antitrust enforcement.[1] Through use of its criminal and civil sanctions, the government has sought not only to remedy specific instances of price-fixing, but also to achieve general deterrence of potential price-fixing.[2]

Recently several legal scholars have recommended that Congress amend the antitrust laws to clarify and emphasize the dominant position that the prohibition of horizontal collusion should occupy in an antitrust policy dedicated to consumer protection.[3] Former Solicitor General Bork, for example, has argued: "Price-fixing cases deliver more consumer welfare for the enforcement dollar than any other kind of prosecution."[4]

These recent recommendations make especially relevant to legislators as well as to economists and lawyers the question of how well antitrust enforcement has succeeded in deterring horizontal price-fixing. Although several economists have studied the effects and determinants of government antitrust activity,[5] none has tested the extent to which antitrust enforcement actually deters price-fixing. The Stigler Report suggested in 1969 that sanctions existing at that time posed little deterrent threat to concealable anticompetitive agreements between competitors. Accordingly, the authors of the Report specifically recommended higher fines for price-fixing. Congress responded in 1974 by raising fines and making price-fixing a felony punishable by three years' imprisonment.

But simply enacting stiffer penalties has not resolved the fundamentally empirical question of how well the penalties have actually worked; there remains the question of how far antitrust enforcement has reduced collusive markups. This paper is a first attempt to answer that question. To do so, we construct a theoretical model of the collusive pricing decision and assess empirically the deterrent effect of public and private antitrust enforcement on the decision to collude.

II. A SIMPLE MODEL OF COLLUSIVE PRICING

A formal, albeit simple, model of the collusive pricing decision is developed below. Our objective in developing this model is to provide a framework for explicitly considering the effect of antitrust enforcement on the decision of firms within an industry to fix prices collusively. While there has been considerable discussion in the literature of the probable effects of antitrust enforcement on price-fixing activity, there has been very little theoretical and virtually no empirical analysis.[1] As the following study will show, the price-fixing decision is one area where even a modest modeling effort provides useful insights.

A. Variables and Definitions

We begin by presenting the variables and definitions that are used in this analysis:

$p \equiv$ price of output

$z \equiv$ non-price factors affecting demand

$Q \equiv$ industry output level $= Q(p, z)$
demand for the industry's output

$c \equiv$ average cost of output, including a normal rate of return

$C \equiv C(Q) =$ total cost of industry output

$mc \equiv$ marginal cost of output

$\lambda \equiv [p - mc]/mc \equiv$ markup over marginal cost

$\pi \equiv$ total profits of colluders

$\gamma \equiv$ level of public and/or private enforcement efforts

$U \equiv U(\pi) \equiv$ the colluders' Von Neumann-Morgenstern utility or objective function

$F \equiv t[p - mc]Q \equiv$ the penalty (criminal and/or civil) for price-fixing, where t is the damage multiple

$d \equiv d(\lambda, \gamma) \equiv$ the probability that a collusive pricing scheme will be detected

The last two definitions, $d \equiv d(\lambda, \gamma)$ and $F \equiv t[p - mc]Q$, play an important role in our analysis and warrant further discussion.

B. <u>The Penalty Function</u>

In considering the appropriateness of our specification of the penalty function ($F \equiv t[p - mc]Q$), the reader should note that under current statutes a price-fixing conspiracy is subject to both criminal and civil sanctions.[2] Civil sanctions, especially the well-known treble damage provisions of the antitrust laws, are clearly related to markups, and our specification of the penalty function fits this sanction perfectly. At first glance, however, our specification appears to be somewhat more problematical for the criminal sanction. While our penalty specification may imperfectly account for criminal sanctions, it should be pointed out that until very recently both potential and actual criminal sanctions for price-fixing were quite modest.[3] It is true that a substantial increase in potential criminal sanctions was legislated late in 1974, under the Antitrust Procedures and Penalty Act, but there is little evidence, at present, on how effective this has been in increasing realized sanctions.[4] To the extent that criminal sanctions are small the controlling or important sanctions will be civil, and our specification of the damage function will quite literally describe the actual penalty structure.[5]

On somewhat more speculative grounds, we maintain that even if criminal penalties become a significant factor in antitrust enforcement our penalty function will still approximate the actual penalty structure. The durability of our specification results from our supposition that there is likely to be some positive relationship between criminal sanctions meted out and the extent or "seriousness" of the collusion. If the "seriousness" of the collusive scheme, as perceived by the judicial system, is strongly related to the magnitude of the markup, then our penalty specification will be reasonably descriptive even in the cases where criminal sanctions are or are likely to become significant. To the extent that setting and maintaining high markups requires that colluders take quite bold actions, there is at least reason to suspect a strong correlation between judicial concepts of seriousness and the degree to which prices diverge from marginal costs.[6]

C. The Probability of Detection and the Markup Level

As important in our analysis of the price-fixing decision as the penalty specification is our assumption concerning the probability of detection. We have assumed that the probability of being detected at price-fixing is, all things equal, an increasing function of the markup over marginal cost, $\frac{\partial d(\lambda, \gamma)}{\partial \lambda} > 0$. In other words, the more conspirators mark up or the closer they come to the price that the cartel would charge if there were no antitrust enforcement, the greater their chances of being discovered. There are two reasons for this phenomenon. The first is the most obvious, and it is simply that higher markups per se make customer complaints to the Department of Justice much more likely.[7,8] Second, as Stigler [39] has noted in a related context, there is probably a positive relationship between the effectiveness of a collusive device and its detectability.[9] Efficiency or effectiveness here is defined in terms of a device's ability to prevent cheating on the part of individual colluders. For example, a set of colluders could virtually eliminate cheating and assure themselves a monopoly price if they formed a joint sales agency, but so visible a collusive device would be nearly assured of detection.[10] In general, we hypothesize that, the more efficiently a collusive device produces markups, the more likely will that device result in the detection, and conviction or civil liability of the colluders. Hence, the very technology of collusion makes it likely that the probability of detection, d, will be, as we have assumed, an increasing function of the markup level.

In addition to assuming that the probability of detection increases with the markup level, we will also maintain that the Department of Justice never charges non-colluding firms, i.e., $d(0, \gamma) = 0$, and that a pure cartel price will not necessarily be detected with probability 1, i.e., $d(\lambda^c, \gamma) \leq 1$, where λ^c is the markup that a set of colluders would obtain if there were no antitrust enforcement.[11]

D. Optimal Collusion

Now in order to model the price-fixing or collusive pricing decision in a straightforward manner, we impose several additional restrictions: First, we formally assume that output is produced under conditions of constant cost, $mc = c$, and $C = cQ$. Second, we presume that all firms take part in the collusion. Finally, we assume that 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 assumed to be its visibility and not its resource costs. Within our simple cartel model this last assumption is tantamount to assuming that firms within the industry under consideration could, at least in the short run, perfectly collude if there were no antitrust enforcement.[12] That is, they could, in the absence of antitrust enforcement, actually maintain the cartel price at the level that would prevail if there were only one seller. This simplification of the cartel problem enables us to concentrate our analytical efforts on assessing the impact of antitrust enforcement on collusion. Considering the intra-industry cheating in the absence of antitrust enforcement and the non-optimality of perfect collusion complicates the analysis without significantly adding to our understanding of the present problem.

The objective of any collusion is straightforward and is simply to set a price, p, that will maximize the colluders' objective function, $U(\pi)$. Formally, the colluders,

$$(1)\quad \max_{p} EU(\pi) = (1 - d)U(\pi_1) + dU(\pi_2) = Z$$

where $\pi_1 = (p - c)Q(p, z)$ is the colluders' profits if they are successful, $\pi_2 = [p - c(1 + \lambda t)]Q(p, z)$ is the profit level if the collusive scheme is detected, and $\lambda = (p - c)/c$ is the markup under constant costs. It should be noted from the formulation of π_2 that detection, sanctions and civil liability are assumed to be limited to the current episode of collusion. That is, the penalty for price-fixing is assessed only on the results of the current pricing decision.[13]

Returning to the formal maximization problem, the necessary condition for a relative and internal maximum is:

$$(2)\quad Z_p = \frac{\partial d}{\partial \lambda}\frac{1}{c}\,[U(\pi_2) - U(\pi_1)] + [Q(p, z) + (p - c)\,\frac{\partial Q(p, z)}{\partial p}]$$
$$\times\ [(1 - d)\,\frac{\partial U(\pi_1)}{\partial \pi_1} + d(1 - t)\,\frac{\partial U(\pi_2)}{\partial \pi_2}] = 0.$$

Equation 2 has an interesting implication: If, contrary to our previous assumption, the probability of detection is actually unaffected by the markup ($\partial d/\partial \lambda = 0$), then the optimal markup is likely to be either zero or the full or pure cartel markup, λ^c.[14] For risk neutral colluders the optimal markup will be zero--or, equivalently, the price will be set at the competitive level ($p = c$)--if the gamble is unfair [$(1-dt) < 0$].[15] If, however, the gamble is favorable, $(1 - dt) > 0$, then such colluders will set the markup at precisely the level they would have in the absence of antitrust laws and enforcement. Specifically, they will set the price, p, and the markup, λ, such that:

$$(3) \quad Q(p, t) + c\lambda\frac{\partial Q(p, z)}{\partial p} = 0.$$

The markup satisfying this condition is the pure or full cartel markup λ^c, i.e., the markup that colluding firms would have chosen if there were no antitrust laws. Hence, if the probability of detection is unaffected by the markup decision, then risk neutral colluders will always treat the price-fixing problem as a dichotomous choice between a competitive price (no markup) and a pure cartel price.[16] Risk averse colluders will also choose between zero and the cartel markup (λ^c) as long as $(1 - d)\partial U(\pi_1)/\partial\pi_1 + d(1 - t)\partial U(\pi_2)/\partial\pi_2 \neq 0$, for $\lambda \leq \lambda^c$. If it pays to mark up at all, then in these cases it pays to mark up all the way to the cartel level prevailing in the absence of antitrust laws. Apparently is it the dichotomous model that some authors may have in mind when they discuss the deterrence potential of antitrust enforcement.[17]

However, the special case of dichotomous choice probably does not describe the decision problem facing most colluders. As argued above, the probability of detection would appear to be significantly affected by the collusive devices employed and, hence, by the markup level chosen by the colluders.[18] To the extent that this is the case, colluders will not only face the decision of whether to collude, but also the more complex and, from our perspective, more interesting, decision of how high to set the markup. Again, Equation 2 has a straightforward but intriguing implication: If the probability of detection is increasing in the markup ($\partial d/\partial\lambda > 0$), Equation 2 cannot be satisfied as an equality at the pure cartel markup, λ^c, given by Equation 3. Likewise, Equation 2 cannot be satisfied at the competitive price, that is, a markup equal to zero. Consequently, the optimal markup, λ^*, in a world that imposes increases in detection risk for increases in markup, will be greater than zero but less than the full or pure cartel markup, λ^c (i.e., $0 < \lambda^* < \lambda^c$).

This result raises the general proposition that the deterrent effect of antitrust laws turns not on the question of whether or not enforcement has prevented price-fixing, but rather on the question of how enforcement has effected the magnitude of the markup. It is unlikely that even the most stringent antitrust enforcement could reduce the optimal markup to zero, or completely deter all price-fixing.[19] However, even modest antitrust penalties and enforcement efforts are likely to deter colluders from choosing the pure or full cartel markup. These important implications are clearly seen in the case of risk neutrality. If we rewrite Equation 2 for a risk neutral collusion we obtain:

$$\text{(2A)}\quad Z_p = -\frac{\partial d}{\partial\lambda}\lambda tQ + [Q(p, z) + (p - c)\frac{\partial Q(p, z)}{\partial p}](1 - dt) = 0.$$

As long as price-fixing is a favorable gamble for some markup, (1 - dt)

> 0, and our assumptions assure it will be, risk neutral colluders will obviously not choose the competitive price. Also, while "unfairness" of the price-fixing gamble at the pure cartel markup $[1 - d(\lambda^c, \lambda)t] < 0$, is sufficient to dissuade the colluders from adopting this strategy, it is not necessary. In general, colluders will stop marking up when both $Q(p, z) + (p - c)\frac{\partial Q(p, t)}{\partial p}$ and $(1 - dt)$ are positive. Hence they will stop marking up short of the full cartel price and while the gamble is still favorable $[(1 - dt) > 0]$.

As we have seen, deterring a set of colluders from setting the pure or full cartel markup is a rather easy task. Deterring all collusion, however, is a nearly impossible task. Therefore, antitrust policy toward horizontal price-fixing must simply be concerned with cost effective strategies that minimize collusion and markups.

E. Assessing the Effect of Enforcement Efforts on Markups

To assess formally the specific effects on markups of increases in enforcement effort, we now 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:[20]

$$(4)\quad \frac{\partial\lambda}{\partial\gamma} = \left(\frac{-1}{cZ_{pp}}\right)\left(\frac{\partial^2 d}{\partial\lambda\partial\gamma}\frac{1}{c}[U(\pi_2) - U(\pi_1)] + \frac{\partial d}{\partial\gamma}[Q(p, t) + (p - c)\frac{\partial Q(p, t)}{\partial p}] \times [\frac{\partial U(\pi_2)}{\partial\pi}(1 - t) - \frac{\partial U(\pi_1)}{\partial\pi}]\right)$$

Essentially, we obtain the predictable result in Equation 4 that, if increases in enforcement efforts do not decrease the effect of markups on the detection probability, $\left(\frac{\partial^2 d}{\partial\lambda\partial\gamma}\right) \not< 0$, then as long as $t > 1$, increases in such efforts will unambiguously decrease markups. In other words, efforts by the Department of Justice to increase potential and actual price-fixers' subjective probabilities of detection will generally have the expected effect of reducing markups.[21]

Increasing the penalty for price-fixing is often advocated as an efficient method of reducing the incidence of such behavior.[22] In fact, Elzinga and Breit [19] argue on the basis of risk aversion for large antitrust penalties and relatively low enforcement levels.[23] In the present model, the effect of changes in the penalty level, in general, will be given by:[24]

$$(5)\quad \frac{\partial\lambda}{\partial t} = \left(\frac{\partial U(\pi_2)/\partial\pi}{cZ_{pp}}\right)\left(\frac{\partial d}{\partial\lambda}\lambda Q + d[Q(p,\ z) + (p - c)\frac{\partial Q(p,\ z)}{\partial p}]\right)$$

Unlike the enforcement result, this requires absolutely no qualification.[25] Increases in the penalty for price-fixing violations will always unambiguously decrease markups.[26]

F. Uncertain Penalties and the Markup Decision

In the work above, we have presented the penalty or damage factor, t, as a simple multiple of the profits from collusion that is known with certainty. This is a useful starting point, but it is a simplification that is not necessary for a number of the major results reported above.

We could lift the restriction that the damage function is known with certainty by assuming that the colluders consider t a random variable, and that they have a subjective probability density, $g(t)$, that indicates their beliefs as to the intervals in which the damage factor or multiple is likely to lie. This appears much closer to the situation in which actual colluders find themselves, since they are subject to criminal and civil sanctions of basically uncertain sizes.

The results derived above for simple movements in t will now be applicable to pure mean changes in the distribution of t: An increase in the mean of t, μ_t, (all other moments constant) will unambiguously decrease markups, $\partial\lambda/\partial\mu_t < 0$.[27] While we have not done so here, formally investigating the response of markups to changes in the degree of certainty surrounding the colluders' beliefs concerning the costs of detection is straightforward. For example, it can be shown that increasing the dispersion of the penalty factor will, if the mean is held constant, decrease markups.[28] This result has the rather interesting implication that a policy designed to spread the range of penalty outcomes by encouraging consumer class actions will, even if it does not increase the average penalty, induce cartels to lower markup levels.[29]

III. EMPIRICAL FINDINGS

The theoretical model developed above has simple yet powerful implications: Increases in enforcement levels or penalties for price-fixing will reduce markups. Though straightforward to derive, these implications are 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 problematical environment. Although a small set of industries may produce a virtually identical product for a local market, input costs and demand conditions are likely to vary across markets. Moreover, all firms nominally face the same antitrust sanctions. This empirical environment requires developing indicators of markups in one or more industries, along with inter-regional and intertemporal measures of the actual, rather than the nominal, threat of antitrust enforcement. We have adopted precisely this procedure to examine the effectiveness of antitrust enforcement.

A. Choice of Industry

We chose to analyze the market for white pan bread, a homogeneous commodity not only regionally produced and consumed, but also well-represented among Department of Justice price-fixing cases. During our sample period, 1974-76, bread price-fixing cases were the most numerous type of food price-fixing case[1] which the Department of Justice brought. One Federal Trade Commission staff report characterized the bread baking and distribution industry as having "the essential characteristic of a conspiracy-prone industry--relatively few sellers in individual markets."[2] Furthermore, the homogeneous nature of white bread frustrates nonprice competition.

In addition to the nature of the product market and its history of price-fixing conspiracies, the bread industry is attractive because it has well recorded output and input prices which facilitate empirical research. Bureau of Labor Statistics publications contained comparable price data for white bread for 20 major cities. For twelve of these cities the data went back as far as 1955, although for the remaining eight they extended only to 1968.[3] Input price data, though not as complete, proved adequate to construct a sample of 288 observations: twelve major cities for 1964-76, and eight major cities for 1968-76.[4] Hence we were able to construct a sample that enabled us to use both cross-sectional and temporal variations in product prices, costs and antitrust enforcement.

B. Estimating City-wide Bread Markups

Developing an indicator for price markups in the bread industry involved two steps. First, we used the known fixed coefficient nature of part of the production process, since a standard recipe specifies the amounts of ingredients necessary for making a loaf of white bread. Using this recipe, presented in Table 1, and the ingredient prices obtained from Bureau of Labor Statistics published sources, we subtracted from each bread price observation the recipe cost (RC), that is, the component of that price attributable to the cost of ingredients.[5] Formally, we defined the recipe-adjusted bread price as:

$$(6) \quad BADJ_{iT} \equiv BREAD_{iT} - RC_{iT}$$

where i is the city, T the time index, and BREAD the price of bread.

BADJ varies across cities and over time because of differences in profits and other non-ingredient input costs. As a second step in developing a measure of markups, we estimated the amount of variation in BADJ attributable to variations in energy and labor costs. Table II contains the results of this analysis. ELEC and GAS are measures of electricity and natural gas prices; TRUCK is a measure of wage rates for truck drivers. We assume this wage to be a proxy for general labor costs.[6] The results in Table II suggest that variations in energy and labor input costs account for a reasonable amount of the variation in adjusted bread prices. Of course, this procedure substantially understates the importance of variations in input prices since the dependent variable, BADJ, has already netted out the recipe or ingredient costs.[7]

To construct a measure of the collusive price markup on bread we then used the recipe costs and the results of this regression to estimate PRED1, the markup indicator:

TABLE I

RECIPE COST FOR A ONE-POUND
LOAF OF WHITE BREAD

Ingredient	Coefficients for Price per Pound of Ingredient
Flour	.6350
Sugar	.0571
Cooking Oil	.0026
Dry Milk	.0350

All prices are retail, except dry milk, which is wholesale. Cooking oil is used as a proxy for shortening. Source: A Study of Bread Prices, Council on Wage and Price Stability, p. 7 (April 1977).

TABLE II

EFFECT OF NON-RECIPE INPUT COSTS
ON ADJUSTED BREAD PRICE (BADJ)

DEPENDENT VARIABLE:	BADJ
INDEPENDENT VARIABLES:	
ELEC	.256[1] (4.62)[2]
GAS	2.492 (3.15)
TRUCK	1.068 (7.14)
Constant	7.05
Number of Observations:	228
R-Squared:	.49
F-Statistic (3,224):	72.93

[1] The estimated coefficient.

[2] The value of the coefficient divided by its standard error.

(7) PRED1 ≡ [BREAD - (FIT1 + RC)]/BREAD,

where FIT1 refers to the fitted values generated by the equation in Table II.[8] Essentially, Equation 7 estimates the markup by first subtracting from the market price the sum of the known ingredient costs (RC) and an estimate of other major non-ingredient costs (FIT1), and then dividing this difference by the market price.[9] The higher the residual as a proportion of the market price, the higher the markup.[10] The residual is, in theory, a measure of the omitted input prices and the pure unit profit; if pure unit profit or loss dominates the residual, then Equation 7 will reliably estimate actual markups.[11,12]

C. Estimating the Effect on Markups of Department of Justice Antitrust Enforcement

1. Controlling for Non-enforcement Markup Effects

Antitrust enforcement and penalties are only two of the many factors that actually determine markup levels. Certainly all factors influencing the price elasticity of demand will affect the markup level. Moreover, although we have assumed to this point that the only cost of collusion is the penalty cost, non-penalty costs of collusion are also important in determining both the full, or pure, cartel markup and the actual markup chosen in the face of antitrust enforcement. Factors other than antitrust enforcement will determine in major part the absolute level of markup. Assessing the effect of antitrust enforcement, therefore, requires a workable method of controlling for such outside influences. This study controls for influences basically generated by market structure variables and considers those influences to be relatively stable. That is, we assume that the market structure variables do not change rapidly, or at the very least, that they change significantly less rapidly than the antitrust variables. Under this specification we can control for non-antitrust effects by considering not the markup level, but <u>changes</u> in the markup level.[13] We therefore use first differences in markup levels, or:

(8) $DEP1_T = (PRED1)_T - (PRED1)_{T-1}$

in testing the effectiveness of antitrust enforcement.[14]

2. <u>Measuring Department of Justice Enforcement Efforts</u>

One difficulty in assessing the effectiveness of antitrust enforcement is measuring the actual enforcement efforts. We have assumed in our model of collusion that for any given markup level the probability of detection is a function of Department of Justice enforcement efforts. Unfortunately this straightforward theoretical proposition does not suggest a unique empirical counterpart for measuring enforcement efforts. This formulation does suggest, however, that for any markup level the probability of detection probably relates to the capacity of the Antitrust Division to bring 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 its enforcement capability. Similarly, changes in the Division's real, or price-adjusted, budget (BUDG1) should suggest changes in the Division's enforcement capability.[15] Although changes in the Division's budget level are unlikely to concern colluding firms directly, such changes do provide an indirect indicator of changes in the level of antitrust enforcement which colluders actually perceive.[16]

A more direct indicator of enforcement efforts is simply current Justice Department activity in the price-fixing area. Lawyers and economists have well recognized the importance of actual activity as an indicator of enforcement efforts and hence deterrence. The Stigler Report, for example, contained the following recommendation:

> We emphasize the great economic and social importance of continued vigilant, aggresive seeking-out and conviction of conventional price-fixers. Every victory weakens the efficiency of undetected collusion in that area of economic life. We strongly recommend the bringing of a series of strategic cases against regional conspiracies, which we believe to be numerous and economically important.[17]

As this statement implies, each prosecution for price-fixing is rare enough that colluding firms in the affected industry consider it a significant event.[18] We have assumed, therefore, that each price-fixing case which the Department of Justice brings against a bread producer increases, for each markup level, the perceived probability of prosecution for other bread producers operating in the same Department of Justice region.[19,20]

Our variable for regional antitrust enforcement (DOJREG) was constructed from Appendix Table IV,[21] which lists all Department of Justice price-fixing cases in the bread industry during 1954-76. After selecting the years for which we had sufficient price data to estimate markups, 1965-76, we constructed DOJREG 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. Appendix Table V contains the actual values of DOJREG for each city and year. In other words, DOJREG is a shift variable designed to capture the changes in the perceived probabilities of cartel failure. It estimates that the probability that Justice will detect a collusive arrangement increases in the wake of a Department of Justice action within the same region.[22]

3. Estimated Remedial and Deterrent Effects

Variable DOJB measures the effect of the antitrust action in the city where the Antitrust Division has actually begun prosecuting bread producers. In constructing DOJB, we assumed that the impact of the antitrust action would be significantly different for the firms specifically prosecuted than for the firms in other cities in the same region. Basically, we assumed that for prosecuted firms a specific remedial reduction in their markup would dominate the general deterrent effect, and that for strategic reasons, the timing of that remedial reduction might not coincide with the reduction made by firms in non-affected cities. To capture this effect, we set DOJB equal to one in a city one year after the Antitrust Division had filed an action there.[23]

In Table III we present our estimates of the effect of Department of Justice enforcement variables on markups in the bread industry. The dependent variable, DEP1, is again the annual change in the markup on white bread for the 20 large cities listed in Appendix Table I. The results in Table III strongly suggest a deterrent effect of Department of Justice enforcement efforts. First, the coefficient on our general measure of changes in enforcement capacity--the change in the real value of the Antitrust Division's budget (BUDJ1)--is negative and significant. In other works, an increase in the enforcement capacity of the Antitrust Division appears to reduce markups on white bread. Second, and more important, the coefficient on our direct measure of the Department's enforcement activity (DOJREG) is also negative and significant. In other words, our results suggest that a price-fixing case against bakers in one city induces bakers in neighboring cities to reduce markups. This result comports with both the formal theoretical results developed above and the Stigler Report's related hypothesis that "every victory" in seeking out price-fixing "weakens the efficiency of undetected collusion." The filing of a bread price-fixing case, because of its rarity, does appear to induce groups of firms in neighboring cities to revise their estimates of failure probabilities for collusion, and hence, reduce their optimal markups on bread. Finally, the coefficient on DOJB, the variable meas-

TABLE III

ESTIMATED EFFECTS OF DEPARTMENT OF JUSTICE ENFORCEMENT ON MARKUPS IN THE BREAD INDUSTRY, 1965-76

Dependent Variable:	DEP1	DEP1
Number of Observations:	208	208
R-square:	.064	.089
F-statistic:	6.97 (2, 205)	6.66 (3, 204)
Independent Variables:		
BUDG1	-.017[1] (-3.154)[2]	-.018 (-3.330)
DOJREG	-.024 (-1.963)	-.025 (-2.122)
DOJB		-.046 (-2.391)
Constant	.013	.016

[1]Coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

uring the remedial effort of a price-fixing case, is negative and significant. Once discovered and prosecuted, price-fixers appear to take "remedial" action by actually reducing their markups in the following year. This remedial effect appears stronger than the general deterrent effect inasmuch as the point estimate of the effect of DOJB is larger than the point estimate for DOJREG. Also, the introduction of DOJB into the equation does not appreciably alter the coefficients on BUDG1 or DOJREG.

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

The results presented in Table III are surprising because they comport exactly with the implications of our simple model of collusive behavior. The measures of deterrence, and in particular the direct measure of enforcement--actual Department of Justice price-fixing actions--perform just as predicted. An increase in the perceived probability of detection engendered by the filing of a price-fixing case in a proximate city appears to reduce the degree of effective collusion as measured by markups over marginal cost. These results are surprising because the penalties meted out in horizontal price-fixing cases appear insufficient to justify the degree of sensitivity to enforcement that we actually observe.[24, 25]

1. Criminal and Civil Penalties for Antitrust Violations

Historically trial judges have punished price-fixing leniently;[26] the cases collected for this study were no exception. All but two of the seventeen price-fixing cases involved nolo pleas.[27] As Table IV shows below,

TABLE IV

CRIMINAL FINES FOR PRICE-FIXING:
BREAD INDUSTRY, 1957-76

Individuals	
Average	$ 4,025
Minimum	1,031
Maximum	9,966
Firms	
Average	$20,690
Minimum	2,930
Maximum	50,638
Fines/Defendants' Annual Sales	
Average	0.31%
Mimimum	0.09%
Maximum	1.0%

All fines are expressed in 1976 dollars.

for the eighteen criminal cases involving either a nolo plea or a conviction, the average fine in 1976 dollars was $20,690 per firm and $4,025 per individual; total fines as a percentage of the annual sales of the colluding firms averaged 0.31%.[28] These modest monetary penalties hardly pose an ominous deterrent to managers of successfully colluding firms.

Although even modest monetary penalties will, in theory, affect markups, our empirical tests probably are unresponsive to such subtle effects. That our simple tests nonetheless produce results consistent with the deterrence hypothesis suggests that being charged with price-fixing still confronts colluding firms with a large potential cost. Since price-fixing is a felony punishable by either a fine or imprisonment, or both, one might argue that the threat of imprisonment is the potentially significant penalty that colluding firms associate with price-fixing. But as Posner [34] has observed in his survey of antitrust enforcement, imprisonment historically has been an extremely rare punishment.[29] In fact, in the seventeen price-fixing cases analyzed for this study no defendants ever served actual prison sentences. In short, for the years covered by this study, imprisonment does not appear to have been a serious concern for participants in regional price-fixing conspiracies.

Rather than imprisonment, the recovery of treble damages by bread consumers or distributors appears to be the potentially significant cost of being prosecuted for price-fixing. It is our conjecture that this private civil remedy was responsible for the observed deterrent effects of the

Department's enforcement efforts against price-fixing. In other words, government enforcement became important not because prosecution subjected colluding firms to substantial fines or risk of imprisonment, but because it substantially increased the risk of a private action for treble damages.[30]

Since bread price-fixing causes a relatively small injury to many individual consumers, 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. These actions, brought under Rule 23 of the Federal Rules of Civil Procedure, have become far more frequent since that rule was amended in 1966.[31] 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' cause of action; the amended rule instead presumed persons to be class members unless they affirmatively "opted out" of the class.[32]

To determine when class actions alleging price-fixing started to influence collusive pricing decisions in the bread industry, we searched both recorded and unrecorded cases and found seven class actions involving horizontal price-fixing of bakery products.[33] Details of these cases appear in Appendix Table VI. None of these cases was filed early

TABLE V

CLASS ACTION DAMAGES/SETTLEMENTS
FOR PRICE-FIXING:
BREAD INDUSTRY, 1957-76[1,2]

Cases	
Average	1,998,646.
Minimum	1,197,810.
Maximum	6,100,000.
Firms	
Average	293,919.
Minimum	39,562.
Maximum	1,220,000.
Damages/Defendants' Annual Sales	
Average	2.87%
Minimum	0.41%
Maximum	19.68%

[1]All fines are expressed in 1976 dollars.

[2]Based on data for five of the seven recorded cases.

TABLE VI

INTERACTION OF DEPARTMENT OF JUSTICE ENFORCEMENT AND CLASS ACTIONS IN THE BREAD INDUSTRY: 1965-76

Dependent Variable				
Number of observations	208	208	208	208
R-square	.0722	.0974	.0662	.0917
F-statistic	5.29 (3, 204)	5.48 (4, 203)	4.824 (3, 204)	5.124 (4, 203)
Independent Variables				
BUDG1	-.016[1] (-2.940)[2]	-.017 (-3.114)	-.016 (-3.086)	-.017 (-3.261)
DOJREG1	.003 (0.119)	.001 (0.029)		
DOJREG2	-.033 (-2.385)	-.035 (-2.520)		
DOJREG12			-.014 (-0.750)	-.016 (-0.859)
DOJREG22			-.031 (-1.990)	-.033 (-2.112)
DOJB		-.046 (-2.385)		-.046 (-2.385)
Constant	.013	.015	.013	.015

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

in our sample years; we found no class action filing before 1968 and no settlement before 1970.[34] We have posited from this admittedly limited information that the class action became a credible threat in the bread industry only after 1968-69.[35]

Information on the amount of the damages or settlements for these class actions came from federal district court documents for five of the six relevant cases.[36] These documents revealed the important datum that settlements as a percentage of defendants' sales were almost ten times greater than fines as a percentage of sales.[37] A comparison with the information in Tables IV and V shows that by most measures class action settlements substantially exceed the fines actually imposed in criminal cases. Moreover, the absolute magnitudes of the settlements are not trivial. The average settlement per case is almost two million dollars, the average settlement per firm nearly three hundred thousand dollars. The settlement on the average represents almost three percent of the defendants' annual sales.[38] The maximum settlement is also particularly striking. For example, in the recent Arizona bread case the settlement for the various class action suits represented nearly twenty percent of the defendants' combined annual sales in the relevant market.

2. Estimating the Deterrent Effect of Civil Actions

To test the hypothesis that civil actions, not criminal penalties, provided the deterrent effect of antitrust enforcement in our sample, we

TABLE VII

CRIMINAL FINES FOR PRICE-FIXING IN THE BREAD INDUSTRY: ESTIMATED TIME TRENDS, 1957-76

Dependent Variable	FIRMFINE	CASEFINE	PCTX
Number of observations	13	13	13
R-square	.06	.13	.10
F-statistic	.33 (2, 10)	.73 (2, 10)	.57 (2, 10)
Independent Variables			
G2	4135 (.414)[1]	-84022.9 (-1.07)	.18 (0.55)
TIME	28.90 (0.036)	7515.75 (1.20)	-.026 (-0.95)
Constant	10926.6	27268.9	.630

[1] The value of the coefficient divided by its standard error.

partitioned the sample into periods before and after the advent of class actions in the bread industry. Since all but one class action in this industry followed a Department of Justice case, we assumed in partitioning the sample that class actions affected the cost of detection, not the probability of detection.[39] Two partition dates were considered: the filing date for the first bread class action, and the date of the first class action settlement. To accomplish the actual partitioning of the sample, we concentrated on this study's most significant variable, DOJREG. Specifically, we split this enforcement variable around these two critical partition dates. In Table VI, DOJREG1 is the Department of Justice enforcement variable (DOJREG) for 1965-70, and DOJREG2 is the same variable for 1971-76. DOJREG1 ends in 1970 instead of 1968 simply because no class actions occurred in 1969 and 1970. Hence DOJREG is the same variable from 1965 to 1968 as DOJREG1. Moreover, 1970 was the year of the first class action certification in the bread industry. DOJREG12 and DOJREG22 are similar variables with the split occuring around 1971 instead of 1970.[40] As is apparent from the estimates in Table VI, the data is consistent with our hypothesis concerning class actions. Whether we partition the enforcement variable around either the first filing date or the settlement date does not seem to matter. In either instance the deterrent effect of a horizontal price-fixing case, one of the Department's major enforcement efforts, is significant only for the later period. That is, only when class actions represent a credible threat does the

Department of Justice produce a significant deterrent effect by prosecuting a horizontal price-fixing conspiracy.

The choice of partition dates--either the date of initial filing or of settlement--is not central to our results, especially since only one year separates the two dates. Still, it would appear from the estimates in Table VI that splitting on the filing date is marginally superior. For the remainder of this paper we will use the DOJREG1-DOJREG2 partition; however, all the reported results are almost identical for the partition around the initial settlement date. Returning to the major result in Table VI, the estimates of the coefficients on DOJREG1 and DOJREG2 clearly indiciate that the Justice Department's prosecutions for price-fixing in the bread industry produced a significant deterrent effect only after private plaintiffs filed the first class action alleging horizontal price-fixing of bakery products.[41] Although the use of class action suits clearly increased the expected cost of price-fixing in the later years of our study, several factors confound analysis of the deterrent effect. First, the early 1970s included a period of price controls, and DOJREG2 possibly acted as a proxy for the effect of these controls. We tested whether DOJREG2 was performing 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 were a significant determinant of markups, DOJREG2 was not simply a proxy for price controls. This result is apparent in Appendix Table VII for a slightly modified form of enforcement variable.

Second, and somewhat more important, is the possibility that criminal fines were increasing over the period, or were simply larger when class actions became a relevant concern. If criminal fines were significantly larger in the "class action period," the effect of class actions and criminal fines would be confounded. In this case, the results in Table VI would not provide very convincing evidence on the independent deterrent effect of class action suits.

To check for this possible confounding influence, we estimated time trends for several measures of criminal fines. Table VII contains our estimates of time trends for FIRMFINE (fine per firm), CASEFINE (fines per case), and PCTX (the ratio of fines to annual sales of defendants). Using a time trend, TIME, for the time trend between 1957 and 1976, and a shift variable G2 for our class action period, we were unable to find any significant time trend in the fine data. Taking as an example PCTX, probably the best criminal penalty measure since it normalizes the fine by the size of the violation, we see in Table VII that neither time trend is significant, nor is the simple shift variable significant. If the results from PCTX indicate anything, it is that a shift downward occurred in criminal penalties during the class action period. However, as we noted above, none of these results is statistically significant. Overall, the evidence from Table VII suggests that an increase in criminal penalties did not confound our results on the deterrent effect of class action suits. Judging from the evidence in the bread industry, the deterrent effect of a criminal prosecution for price-fixing appears almost entirely dependent on the existence of a substantial civil remedy.

Government antitrust prosecution is not unimportant because it has relied on private enforcement for its deterrent effect. A successful government suit still serves as a signal both to private parties and to local governments that a civil suit for treble damages has become feasible. A plea of nolo contendere appears to be all that is necessary to accomplish this signalling. A guilty verdict, of course, is prima facie evidence in a treble damage suit, and should induce private enforcement. However, guilty verdicts in price-fixing cases are quite rare. Of the seventeen price-fixing cases used in this sample and listed in Appendix Table IV, one ended in an acquittal and another in a conviction; all the other fifteen cases involved nolo pleas. Even if a government case ends in a nolo plea, it still unambiguously signals to potential private plaintiffs that they would be likely to prevail in a private damage suit against the alleged price-fixing conspiracy.[42] In fact, all the class actions in our sample that followed a government action and received class certification eventually obtained a settlement award. The pattern in the bread industry is clear: the federal government signals the conspiracy by bringing a price-fixing case, and subsequent private plaintiffs provide the effective penalty in the form of a class action for treble damages. Hence, for price-fixing in this industry--and, we suspect, for price-fixing in general--deterrence has been a product of both public and private enforcement efforts.

3. The Eisen Decision: Some Preliminary Findings

As the empirical results in the preceeding section suggest, class action suits provide a crucial penalty component in antitrust enforcement. As class actions become either easier or harder to bring, government prosecution will change in its ability to control price-fixing effectively. The amendment of Rule 23 in 1966, of course, was the factor which facilitated class actions, thereby enhancing the power of the federal government to deter price-fixing. Landmark Supreme Court decisions regarding class action suits are likely to have similarly far-reaching effects.

Most antitrust class actions come under sections (b)(3) of Rule 23, which requires that the class representative provide the "best notice practicable under the circumstances, including individual notice to all members who can be identified through reasonable effort."[43] Courts interpreted this provision liberally in the early post-amendment period. In its formal report in 1972, the Special Committee on Rule 23 of the American College of Trial Lawyers reported: "the rule [23] appears to permit a notice to class members which though mandatory may be informal."[44] However, in 1974 the Supreme Court in <u>Eisen IV</u> read the notification language in (c)(2) of Rule 23 as requiring the class representative to notify by mail over two million identifiable class members. Moreover, the class representative must pay for the cost of notice as part of the ordinary burden of financing his own suit.[45] The identifiability proviso in the decision led legal experts to speculate that <u>Eisen</u>'s effect on antitrust suits would be slight because the large class sizes and small individual claims would make individual identification of class members impracticalbe, thereby obviating compliance with the "best notice practicable" requirement of (c)(3).[46] Nonetheless, in the Arizona Bakery Products Litigation the Arizona attorney general individually mailed 772,318 bilingual class action notices to Arizona households,[47] which suggests that Eisen still bound price-fixing cases. At best, the evidence is mixed on the specific relevance of Eisen IV to price-fixing cases. Yet even when the class representative need not individually notify class members, the cost of mass notification still is high enough relative to the size of individual claims to discourage the potential class member from serving as representative for a class of consumers.

Although no consumer classes actually were certified in bread cases until 1974, private plaintiffs made four serious attempts at certification involving classes as large as 20 million consumers.[48] At the very least, the possibility that a large consumer class would gain certification presented a credible threat to any group of colluding firms. Eisen IV appears to have reduced the probability of certification for a consumer class, and thereby to have reduced the defendants' potential costs of a class action settlement.[49,50] This reduction in the colluders' expected cost should have reduced the deterrent effect of government enforcement and raised markups in the bread industry.

In Table VIII appear the results of two crude attempts to isolate the effect of Eisen IV on markups in the bread industry.[51] The variable EISEN is a dummy variable equal to one in 1974-76; EISEN1 equals one in 1974 only. These two variables use 1974 because the Supreme Court de- diced Eisen IV in May of that year; the importance of the case suggests that colluders quickly observed the new precedent. Since DEP1 measures changes in markup levels, EISEN corresponds to a trend in markups induced by the decision, while EISEN1 corresponds to a single, discrete shift in markups due to the Eisen decision. Both variables perform as expected and have significant, positive coefficients--results consistent with our hypothesis that Eisen IV reduced the effective deterrence of contemporaneous antitrust enforcement.

The results for EISEN1 emphasize one problem with our approach. Since for the purposes of BUDGD1 there are effectively only thirteen observations, a date-specific dummy variable may interact with BUDGD1. The results in Table IX suggest that this is the case in the estimation using EISEN1. The results for EISEN, though somewhat more speculative, do not suggest the same problem. This evidence on the Eisen decision is surprising because most of the cases in our small sample of price-fixing cases are not consumer class actions involving numerous households. Rather, most of our class actions involve local governments, school boards, institutions, and "middlemen" or distributors.[52] We suspect that this is a general phenomenon.[53] Why then should a

TABLE VIII

ESTIMATED EFFECTS OF DEPARTMENT OF JUSTICE ENFORCEMENT, CLASS ACTIONS, AND THE EISEN DECISION ON MARKUPS IN THE BREAD INDUSTRY, 1965-76

Dependent Variable	DEP1	DEP1	DEP1	DEP1
Number of Observations	208	208	208	208
R-square	.0951	.118	.1161	.1355
F-statistic	5.33 (4, 203)	5.44 (5, 202)	6.67 (4, 203)	6.33 (5, 202)
Independent Variable				
BUDGD1	-.022[1]	-.023	-.008	-.009
	(-3.684)[2]	(-3.810)	(-1.337)	(-1.549)
DOJREG1	.005	.003	.010	.008
	(0.234)	(0.143)	(0.464)	(0.364)
DOJREG2	-.035	-.036	-.037	-.038
	(-2.514)	(-2.643)	(-2.692)	(-2.795)
DOJB		-.044		-.040
		(-2.328)		(-2.131)
EISEN	.020	.019		
	(2.269)	(2.210)		
EISEN1			.041	.038
			(3.177)	(2.984)
Constant	.001	.013	.005	.007

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

Supreme Court decision that affects predominantly consumer class actions influence the level of deterrence significantly?

The importance of events like Eisen rests in the distribution of settlements. A consumer class action, once certified, is likely to receive a substantial settlement, which provides an extreme observation in the settlement distribution.[54] Such a case is an outlier both in frequency and in magnitude of damage awards or settlements.[55] Successful consumer class actions are rare, but expensive. Consequently, even if the expected value of the settlement distribution remains unchanged, as long as colluders are risk averse, legislative or judicial decisions that make consumer class actions more or less probable are likely to have an independent and significant effect on the price-fixing decision.[56] Illinois Brick v. Illinois may then reduce deterrence not because, as many of its critics allege, it will significantly reduce the numbers of class actions in price-fixing cases, but rather because it removes entirely the possibility of a consumer class action. That is, it may reduce deterrence simply by making what has been an unlikely, but costly, event an impossibility.[57]

E. Consumer Class Actions: Some Additional Speculation

A number of important developments have affected consumer class actions during the past several years. In 1976 Congress passed the Hart-Scott-Rodino Antitrust Improvements Act which empowered state attorneys general to sue for treble damages as parens patriae on behalf of consumers.[58]

TABLE IX

ESTIMATED EFFECTS OF ENFORCEMENT AND SELECTED RECENT DEVELOPMENTS IN CONSUMER CLASS ACTIONS ON MARKUPS IN THE BREAD INDUSTRY, 1965-76

Dependent Variable	DEP1	DEP1	DEP1	DEP1
Number of observations	208	208	208	208
R-square	.1284	.1074	.1227	.1404
F-statistic	4.93 (6, 201)	4.86 (5, 202)	5.650 (5, 202)	5.473 (6, 201)
Independent Variables				
BUDGD1	-.019[1] (-2.980)[2]	-.018[1] (-2.812)[2]	-.003[1] (-0.477)[2]	-.005[1] (-0.733)[2]
DOJREG1	.004 (0.186)	.006 (0.276)	.011 (0.491)	.009 (0.391)
DOJREG2	-.040 (-2.876)	-.039 (-2.787)	-.040 (-2.863)	-.040 (-2.934)
DOJB	-.042 (-2.201)			-.039 (-2.036)
EISEN	.024 (2.614)	.025 (2.734)		
EISEN1			.043 (3.334)	.040 (3.120)
YR76	-.022 (-1.495)	-.025 (-1.671)	-.017 (-1.232)	-.015 (-1.070)
Constant	.012	.010	.004	.006

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

1. Hart-Scott-Rodino Antitrust Improvements Act

The statute's parens patriae provision specifically allows the use of "aggregate or statistical sampling methods" to prove damages and drops all requirements of proof of individual injury.

The potential effect of the Hart-Scott-Rodino Act is clear. Given the difficulties of consumer class certification and the added costs which Eisen imposes, parens patriae actions would appear to be a powerful method of increasing the likelihood of large civil awards or settlements in price-fixing cases. The Hart-Scott-Rodino Act passed in the middle of 1976 and did not apply to any injury prior to enactment. Still, the statute's potentially drastic effect on the probability distribution of settlements suggests that well-informed colluders would immediately modify their behavior. Specifically, in our sample we would expect a downward shift in bread margins as colluders in various cities recognized the implications of this new and costly penalty for price-fixing.[59]

2. Arizona Bakery Products Litigation

The Arizona Bakery Products Litigation also was an important development likely affecting the collusive pricing decision among bread manufacturers. Five classes were certified by the court: restaurants, grocery stores, governmental bodies, private health care institutions, and consumers. Two elements were unique about this case: first, the settlement was extremely large by historical standards and, second, the class involved the first consumer class certified in the bread industry. The consumer class, comprising nearly 800,000 households, was certified, and its settlement plan approved, in 1976. Hence, in 1976 well-informed bread manufacturers in various cities had to recognize when making their price-fixing decisions not only the potential liabilities posed by the Hart-Scott-Rodino Act but also the information on possible outcomes of consumer actions which the Arizona Bakery Products Litigation provided.[60]

3. Testing the Effect of Recent Antitrust Developments

We would expect that both of these events would tend to increase the amount of antitrust deterrence. Accordingly, we would expect lower markups on bread in our sample cities. Unfortunately we could not devise any subtle method to isolate the separate effects of these developments; nor did we have sufficient data to study the time paths of their effects.

We were only able to consider the date-specific effects of both actions combined.[61] That is, we assumed that optimal markups in the bread industry fell in 1976 because of the Hart-Scott-Rodino Act and the Arizona case. This technique, of course, is quite crude and can offer tentative results at best.

In Table IX we present the results of introducing a dummy variable (YR76) for 1976 into the equations for DEP1. For all cases the coefficient is of the hypothesized sign, but is statistically significant ($\alpha = .05$) only for a single case, and there only marginally so. There is some evidence of the hypothesized deterrence effect, but the coefficients are too imprecisely estimated to suggest any strong conclusions.[62,63] It is, however, reassuring that with this crude technique the effect of these liberalizing influences on consumer class actions were not inconsistent with our previous findings concerning the deterrent effect of class actions.[64]

IV. SUMMARY

In this paper we have formulated and tested a simple model of collusive pricing behavior. We showed that the price-fixing decision is, under reasonable assumptions, not a dichotomous, either/or choice, but rather a traditional decision on the optimal amount of price-fixing. That is, under the plausible, if not necessary, condition that the failure rate of a collusive enterprise increases with the markup, colluders will make a marginal decision on how far to price above cost, not a global decision to charge either the cartel price or the competitive price.

The optimal markup was shown to depend, as expected, on antitrust enforcement efforts and penalties. Under plausible assumptions, the optimal markup decreases as enforcement efforts increase. For penalties the results are unambiguous: an increase in the penalties for price-fixing always reduces the optimal markup.

In order to test the implications of our simple pricing model, we first developed a measure of the markups on white bread in twenty major cities for 1965-76. Next we developed several measures of enforcement and found that both a global indicator, BUDG1, and a regional and specific indicator of antitrust enforcement, DOJREG, performed as expected. Most dramatically, it was shown that the Department of Justice does create a deterrent effect by bringing a price-fixing case. Markups in neighboring cities fall in the wake of a Department action.

We were further able to show that, at least until the end of our sample in 1976, the deterrence generated by Department actions was in major part due to the existence of subsequent private civil litigation. Only after the advent of class action suits as a private remedy did the Department's bringing of a bread price-fixing case act as a deterrent to other colluders in the industry. In other words, class action suits were shown to be the effective penalty for price-fixing, at least in the bread industry.

Having provided some evidence on the importance of class actions in general, we then presented somewhat more speculative findings on the importance of consumer class actions. A simple test of the Eisen IV decision performed as predicted and indicated that the decision caused a statistically significant reduction in deterrence. A crude test of the combined effect of the Hart-Scott-Rodino Act and the Arizona Bakery Products Litigation indicated an increase in deterrence but, unlike the test of Eisen IV, the results proved statistically insignificant. Overall, the results on consumer class actions, though not conclusive, do suggest that such actions have an independent deterrent effect. This is likely due to the large settlements involved in consumer class actions and a degree of risk aversion among colluders that was perhaps unexpected.

The results of this research are encouraging and demonstrate the feasibility of empirical research on the deterrent effects of antitrust activity. We hope that our preliminary findings encourage others to increase the stock of knowledge in this important area. If, "antitrust shows signs of becoming the economic battlefield of the late 1970s and 1980s,"[1] perhaps knowledge of the effects of various antitrust weapons will prove decisive.

THE DETERRENT EFFECT OF ANTITRUST ENFORCEMENT: A THEORETICAL AND EMPIRICAL ANALYSIS

ADDENDUM

This section contains the notes, the glossary and the appendix tables.

NOTES

Section I

[1]Posner [34], p. 398, Table 23, reports that 989 of the 1551 Department of Justice antitrust cases between 1890 and 1969 contained charges of horizontal conspiracy.

[2]One federal district court judge has justified imposing prison sentences on price-fixers on the grounds of general deterrence. See Renfrew [35].

[3]Bork [13]; Posner [33].

[4]Bork [13], p. 406.

[5]Stigler [39], pp. 259-95; Asch [7]; Asch and Seneca [8]; Siegfried [37].

NOTES
Section II

[1]Recent contributions by Elzinga and Breit [19] and Blair [10] involve the application of simple deterrence models to the decision to violate the antitrust laws but neither involves empirical analysis or a specific theoretical development of the price-fixing decision.

[2]Criminal sanctions for price-fixing now include imprisonment for a term up to three years, a maximum fine of $100,000 for an individual and a maximum fine of $1,000,000 for a corporation.

[3]See our discussion of criminal and civil penalties in Section III below.

[4]For an interesting account of some recent changes in sentencing practices for antitrust offenders see D. Burnham [14].

[5]Because, until quite recently, firms did operate in an environment where both potential and actual criminal sanctions were modest, we would argue quite strongly that the penalty specification above is, at a minimum, fully adequate for modeling past decisions concerning the violation of antitrust law.

[6]While not undertaken here, it does appear feasible, although time-consuming, to actually test this hypothesis. By analyzing past price-fixing cases in some detail it should be possible to determine whether the sanctions imposed were related to alleged markups.

[7]Hay and Kelley [25] find that complaints by customers are the third most numerous method of detection in their sample of Department of Justice price-fixing cases.

[8]It is also possible that higher markups will increase the likelihood of private litigation. The situation here is somewhat more complex than the complaint case. Private litigation will only be undertaken if the expected award or settlement exceeds relatively significant costs of the litigation. Higher markups will always increase the "damage" done to potential plaintiffs but usually will not increase the likelihood of a private legal action until the expected award or settlement exceeds the expected litigation expenses. Hence all that can strictly be said for markup levels and private actions is that an increase in markups will never decrease the likelihood of a private action.

[9]See Stigler [39], pp. 268-270.

[10]Stigler [39] presents some indirect evidence on this point. In a sample of Department of Justice cases he divides the types of collusion between what he considers efficient and inefficient collusion. The average time to capture or period from inception of alleged collusion to complaint is significantly shorter for the efficient than for the inefficient forms of collusion.

[11]Obviously if there are significant cheating problems this will be substantially below the markup level, λ^m, that might be set by a simple monopolist.

[12]Formally, Equation 1 would become $\underset{P}{\text{MAX}}\ EU(\pi) = U(\pi_1)$.

[13]See footnote for a discussion of some of the implications of lifting this restriction.

[14]It is sufficient that $d = \bar{d}$ for all $\lambda > 0$.

[15]The optimal markup will also be zero if the colluders are risk averse and the gamble is simply fair, $(1 - dt) = 0$.

[16]Clearly, if $(1 - dt) = 0$, Equation 2 will not imply a unique markup for risk neutral colluders.

[17]See Posner [34], pp. 223-227, and Elzinga and Breit [19].

[18]Prior studies have used rate of return analyses rather than markups. See [7], [8], and [37].

[19]This, of course, is strictly true only as long as we neglect the non-penalty costs of collusion.

[20]The comparative static derivative for the special case of risk neutrality (Equation 2A) is:

$$\frac{\partial\lambda}{\partial\gamma} = \left(\frac{t}{cZ_{pp}}\right)\left(\frac{\partial^2 d}{\partial\lambda\partial\gamma}\lambda Q + \frac{\partial d}{\partial\gamma}\left[Q(p, z) + (p - c)\frac{\partial Q(p, z)}{\partial p}\right]\right)$$

[21]In the general case, only when $t < 1$ does establishing the sign of $\partial\lambda/\partial t$ take specific preference information.

[22]For a particularly well-written advocacy of harsh punishment for antitrust violation, see Section III, President Nixon's Task Force on Productivity and Competition [65].

[23]It should be mentioned at this point that there are logical limits to the effectiveness of harsh penalties. It is possible to show that there does not exist an antitrust penalty severe enough to deter all collusion for all non-zero probabilities of detection; see Block and Lind [12].

[24]The expression for $\partial\lambda/\partial t$ in the risk neutral case is simply:

$$\frac{\partial\lambda}{\partial t} = \left(\frac{1}{cZ_{pp}}\right)\left(\frac{\partial d}{\partial\lambda}\lambda Q + d[Q(p, z) + (p - c)\frac{\partial Q(p, z)}{\partial p}]\right)$$

[25]As we noted above when setting up the maximization problem, only contemporaneous sanctions or penalty effects were to be considered. At this point, let us modify the model and assume, as is theoretically possible today, that there is effectively no statute of limitations in price-fixing cases. Under this assumption and in the absence of any interest charges, the profit level of the colluders at time T if detected would be:

$$\pi_{2T} = \pi_{1T} - t\sum_{i=0}^{B}\frac{\lambda_{T-i}}{C_{T-i}}Q_{T-i}$$

where B denotes the number of time periods that the collusion has been successful and T the current time period. It is quite straightforward to show that as B increases--even if the probability of detection depends only upon this year's markup--the optimal markup, at time T, decreases. Removing the statute of limitations is simply another method of increasing the penalty for price-fixing during the current year. This obviously suggests that under almost any set of antitrust laws that do not involve a statute of limitations, price-fixing, if it is renegotiated on a regular basis, tends to decrease over time among firms that have not been detected.

[26] In very simple models, the relative magnitude of the elasticities with respect to enforcement and penalties is implied by behavior toward risk. Note than these implications will hold in the present model if $\partial^2 d/\partial\lambda\partial\gamma = 0$.

[27] It should be noted that establishing this result will take slightly more preference information than is required to sign Equation 5. See Block and Heineke [11].

[28] In order to establish the results with respect to μ_t and the dispersion of t, rewrite EU in Equation 1 as:

$$EU = (1 - d)U(\pi_1) + d \int_{\underline{t}}^{\bar{t}} [U(\pi_1 - t\frac{\lambda}{c} Q(p, z)]g(t)dt$$

The "pure" mean and dispersion change employed by Block and Heineke [11] may be used to analyze this case and obtain the results for the mean and dispersion discussed above.

[29] Readers familiar with the small literature in this area will recognize this as simply a generalization of the results presented by Elzinga and Breit [19] and Blair [10].

NOTES

Section III

[1]Milk was the second most numerous, with eight cases over the period 1964-76.

[2]Federal Trade Commission [51], p. 135.

[3]U.S. Department of Labor, Bureau of Labor Statistics, [43] and [44].

[4]Actual sample is displayed in Appendix Table I.

[5]The sources of price data were: U.S. Department of Labor, Bureau of Labor Statistics, [43], [44] and [45].

[6]A specification using food and kindred workers' wages (WAGE) as well as an alternative functional form is given in Appendix Table II.

[7]The entire procedure, taking account of both RC and the estimated cost function, accounts for approximately 82% of the variation in the BREAD series.

[8]In general, prices for significant inputs are included in cost functions. Our asymmetric treatment of input prices amounts to using a priori information, the recipe for bread, to supplant estimation of some of the coefficients. It can be argued that this increases the efficiency of the estimation but, as implemented, it does preclude substitution among raw materials and other inputs in production.

[9]Obviously, because it is generated from a simple regression, FIT1 will, in many instances, exceed BADJ and thus generate a negative PRED1. For our purposes, these negative values pose no problem.

[10]While the qualitative properties of this markup are the same as the traditional definition, it is quantitatively dissimilar in that the residual is divided by the price instead of cost. PRED3, which is defined in the Glossary, is the traditional concept of markup and when used in the place of PRED1 it yields similar results.

[11]Available evidence suggests that ingredient and labor costs are the predominant costs in bread production. See, Council on Wage and Price Stability [49].

[12]We structured our test of antitrust enforcement around markups, rather than the traditional and simpler--but more indirect--test of overall profitability or rate of return. One basic reason for using the markups is the very directness of the test. It circumvents some of the problems associated with measuring the impact of price-fixing on total profitability or rate of return. Also of major importance was the availability of regional or city price data which enables us to conduct somewhat more interesting and perhaps more powerful tests of effectiveness than could have been performed with national data.

[13]Obviously, an alternative--and in this case not very inexpensive--approach would involve collecting data on these "other influences." Particularly burdensome here would be collecting information on market structure at a city or SMSA level.

[14]DEP3 is defined in an analogous manner for PRED3.

[15]Formally BUDG1 is the first difference in the real value of the annual budget levels of the Antitrust Division. Data on budget levels was obtained from the federal budget [64].

[16]Some evidence of the relationship between changes in budget levels of the Antitrust Division and changes in specific enforcement activities appears in Appendix Table III. Annual changes in the constant dollar budget of the Antitrust Division show a positive, though weak, relationship to annual changes in the number of bread price-fixing investigations brought by the Division. Aggregate data on investigations was provided to us by the Office of Economic Policy, Antitrust Division, U.S. Department of Justice.

[17]President Nixon's Task Force on Productivity and Competition [65].

[18]We can produce a formal rationale for this statement if we assume that colluders are estimating the probability that they will be apprehended in a period using Bayesian methods. Assuming a binomial structure for the apprehension process and imputing a B prior distribution to the colluders, the mean of the posterior distribution after one period is:

$$E(P|r, n) = \frac{\alpha + r + 1}{\alpha + \beta + 3},$$

where r is 1 if a bread case is brought by the Department of Justice and zero otherwise, and α and β are the parameters of the prior distribution. (See Good, [22].)

Consequently, whenever colluders are apprehended, colluders' estimate of the probability of apprehension increases dramatically. For example, in this simple model suppose that $\alpha = 1$ and $\beta = 17$ so that the a priori probability of apprehension was .1. An apprehension of colluders increases the posterior probability to .143. If no colluders are apprehended in succeeding periods the estimated probability declines first to .136, then .125 and finally to .1 after nine periods without an apprehension. That is, after an initial impact, the effect of the case on the apprehension probability estimated by colluders deteriorates if no new cases are brought. The way we have defined the variables captures the effects of actual apprehension on colluders' estimates of the probability of apprehension in this simple model.

[19]The Antitrust Division operated seven field offices during the years of our sample. "These field offices [were] located in the cities indicated below and their operations, subject to such specific assignments as may be made to the Washington staff, [covered] the geographical areas listed:

New York (New York City): Connecticut, Maine, Massachusetts, New Hampshire, Northern New Jersey, New York, Rhode Island, and Vermont.

Middle Atlantic (Philadelphia): Delaware, Maryland, Southern New Jersey, Pennsylvania, and Virginia.

Great Lakes (Cleveland): Kentucky, Eastern District of Michigan, Ohio, and West Virginia.

Midwest (Chicago): Denver, Colorado metropolitan areas, Illinois, Indiana, Iowa, Kansas, Western District of Michigan, Minnesota, Missouri, Nebraska, North Dakota, Oklahoma, South Dakota, Texas (except Western District.), and Wisconsin.

San Francisco (San Francisco): Alaska, Northern and Eastern Districts of California, Colorado (except Denver area), Hawaii, Idaho, Montana, Nevada (except Las Vegas area), Oregon, Utah, Washington, and Wyoming.

Los Angeles (Los Angeles): Southern and Central Districts of California, Arizona, New Mexico, Western District of Texas, and Las Vegas, Nevada metropolitan areas.

Atlanta (Atlanta): Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee."

("Memorandum to all personnel. Re: Organization and Operation of the Antitrust Division." Department of Justice, Antitrust Division, Directive 10-73, announced September 30, 1973, quoted in CCH Trade Regulation Reporter ¶8530.10.)

The Antitrust Division established a Dallas office in 1976 and realigned the areas covered by the regional offices. See CCH Trade Regulation Reporter, ¶8542. This study used the earlier seven regions for 1976, rather than re-aligning all regions for that single year.

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[21]This table was constructed using data from the Commerce Clearing House [15] and [16]. Filing dates and other summary information was checked against a special listing of bread price-fixing cases prepared for us by the Economic Policy Office, Antitrust Division, U.S. Department of Justice, for a subperiod (1963 to 1976).

[22]An alternative hypothesis is that, since Department of Justice resources are limited, a prosecution in a region is a signal that an

additional prosecution in the same region is unlikely. Hence firms within an area would actually decrease their estimates of capture probabilities in that area after a Department of Justice case. This does, however, have much in common with the suggestion that drivers on a highway speed up when they see a car being ticketed.

[23]For an estimate of the contemporaneous relationship between a Department of Justice action and the markup in a given city see Appendix Table IX.

[24]Note that the point estimate of the coefficient on DOJREG, -.025, is, in absolute value, approximately an order of magnitude larger than the average value of DEP1, .00249. Hence, the bringing of a bread price-fixing case by the Department of Justice leads to a change in the regional markup level that is not only of opposite sign from the overall average year-to-year change but also is substantially larger.

[25]Formally, the magnitude of Equation 4, or the responsiveness of markups to changes in enforcement efforts, will be an increasing function of the magnitude of the penalty.

[26]But the attitude of trial judges appears to be changing. See Renfrew [35].

[27]See Posner [34] for a more general discussion of the magnitude of antitrust penalties.

[28]Average criminal fines imposed amounted to seven percent of pre-tax profits (13% of after-tax profits). This specific figure was derived by dividing the ratio of total fines to defendants' sales by the ratio rate of net profit to sales. Data on the first ratio came from court documents relating to thirteen cases (76 firms) during 1957-75. The ratio of net profit to sales, 4.65% (2.42% after tax) was taken from a study by the Executive Office of the President's Council on Wage and Price Stability [49].

[29]See table below.

Criminal Sentences for Price-Fixing, 1966-76

	1966-76	1975-76
Number of persons charged:	249	122
Number of defendants convicted or pleading nolo contendere / Number of persons charged :	.82	.93
Number of defendants serving prison sentences / Number of defendants convicted or pleading nolo contendere :	.15	.06
Total number of days served / Total number of defendants serving prison sentences :	26.23	29.00

Source: Compiled from Commerce Clearing House [15].

Note: Sentences imposed in November 1976 for defendants in the Paper Label Case, Case No. 2505-2506, were substantially reduced in February 1977; the figures above reflect the data from 1977.

[30] While civil actions are possible without a preceding criminal case, this does not appear very common in price-fixing cases.

[31] Although surprisingly little historical data exists on class actions, the data that does 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, suggests that the 1966 amendments to Rule 23 made the class action a more attractive legal device. See Appendix Table XI.

"The Committee [on Rule 23] in an effort to measure the statistical effect of the (b)(3) action [amendment of Rule 23] upon the litigation process, sought to discover the number of class actions brought under the rule, the number still pending and their impact on court dockets. Federal courts in major cities and the Administrative Office of the Federal Courts in Washington, D.C. were contacted. No statistics were available. Accordingly the Committee was compelled to limit its study to one district and the Southern District of New York was selected. Since the Southern District kept no records on class suits until July 1971, and since examination of all complaints filed since 1966 was not feasible, the only alternative was to examine docket entries in all 29,673 cases brought since July 1966." American College of Trial Lawyers [4], p. 13.

[32] The Federal Rules of Civil Procedure were introduced by the Supreme Court in 1937. Until Rule 23 was amended in 1966, the antitrust class action fell under the "spurious" class rubric of old subsection (a)(3).

which amounted to a permissive joinder device "operated merely as an invitation to other members of the class to come forward and join the action." (American Bar Association [3], pp. 302-03.) Class members, therefore, could "opt into" the class, but res judicata did not bind them to the judgment in the action if they failed to join the class. The amendment of Rule 23 in 1966 discarded the earlier rubrics and, more important, required class members affirmatively to "opt out" to avoid being bound by the class action judgment. Understandably, the amended rule also established provisions for fair and adequate representation of absentee class members.

[33]We compiled these class actions from Newberg [3] ; McLaughlin [29]; Trade Cases [16]; Trade Reg. Rep. [15]; and a LEXIS computer search [27].

[34]See Appendix Table VI.

[35]Because of the filing date of the first case and the time required to communicate the event, 1969 is assumed to be the first year that class actions became a relevant concern in collusive decisions.

[36]One class action, Hackett v. General Host Corp., had no settlement or award since the only class at issue was denied certification.

[37]Average settlements/sales and fines/sales are important ratios because they normalize settlements or fines by a measure of the magnitude of the violation. It is interesting to note that average damages per firm is almost fifteen times average fines per firm

while average damages as a percentage of defendants' sales is only ten times average fines as a percentage of defendants' sales. This occurs because the average sales per defendant is significantly different in the two samples: $5,795,271 for the fine calculations and $10,254,205 in the class action calculations.

[38]Damages or settlements amounted to 62% of pre-tax profits (11.8% of after-tax profits). Please see footnote for the method used. The ratio of damages or fines to total sales is from district court records on five cases (21 firms) during 1975-76.

[39]In our sample only Kramer v. Gold Medal Baking Co., which appears to be a minor action, was initiated without a preceeding government case. One area that requires further empirical investigation is the general relationship between class actions and government price-fixing cases.

[40]DOJREG12 = DOJREG, 1966-71; DOJREG22 = DOJREG, 1972-76.

[41]Since data on class action suits is not well organized, the statements concerning the first class action must always be qualified as the first class we were able to locate. However, the fact that it could be located suggests that, at the very least, it was likely the first significant class action suit in this industry.

[42]It is interesting to note in this respect that a bill currently under consideration, H.R. 7647, would repeal Section 5(a) of the Clayton Act in order to extend the common law of collateral estoppel to litigated antitrust judgments. Given the prevalence of nolo pleas in the price-fixing area, it would not appear that passage of this bill would, as is the intention of its authors, significantly aid in the private enforcement of such offenses.

[43]Antitrust actions certified under Rule 23 (b)93) must satisfy the mandatory notice provisions of (c)(2) of Rule 23. For a discussion of this point see Newberg [31].

[44]American College of Trial Lawyers [4], p. 6.

[45]Eisen v. Carlisle & Jacquelin [69].

[46]Newberg [31] at §7598

[47]See Committee on the Office of Attorney General [30].

[48]The 20 million consumers were part of the class action that followed U.S. v. American Bakeries et al.

[49]The Arizona case indicates the relative magnitudes likely to be involved in a consumer class action. The settlement was approximately 20% of annual sales, or more than seven times the average ratio of settlements to sales for all class action settlements in the industry.

[50]That the first consumer class class was certified after this decision should not alter this proposition, since the state of Arizona acted as the class representative in that case and underwrote the notification costs. We doubt that an unsubsidized class representative would have had the incentive to underwrite the cost of mailing and processing almost 800,000 notices. Hence Eisen IV significantly reduced the probability of a consumer class certification by removing any incentive to be private class representative.

[51]An alternative, and again imperfect, test involving a split of DOJREG both on class action activity and on the Eisen decision appears in Appendix Table VII. Unfortunately, there were no Department of Justice actions in 1976, so DOJREG3 is simply a dummy variable for 1974 and 1975. All of the Department of Justice action variables perform more or less as expected. These actions appear not to have a deterrent effect before the first class action; this also seems to be the case immediately after the Eisen decision. The latter result is somewhat perplexing since we expected a decrease in the size of the coefficient, but not of the magnitude which these results suggest.

[52]No one has yet conducted a comprehensive empirical study of the relative importance of various types of class actions in antitrust enforcement. Such a study might illuminate a number of important policy questions. For example, both the House and Senate are currently proposing bills, H.R. 8359 and S. 1874, to restore consumer class actions in the antitrust area. In neither case does the relevant committee appear to have access to the type of information necessary to judge the absolute and relative importance of consumer class actions in antitrust enforcement.

[53]See McLaughlin [29].

[54]See: National Association of Attorneys General [30].

[55]The reason for this is not obvious. Since Hanover Shoe Co. v.

United Shoe Machinery Corp., the Supreme Court has recognized the right of distributors or "middlemen" to sue for the full extent of any overcharge. Part of the reason may be the more frequent use of the "opt out" by distributors or perhaps the multiple legal "classes" involved in the distribution process.

[56]Encouraging consumer class actions while simultaneously providing disincentives for distributor class actions may, on balance, actually increase effective deterrence. Legislation that encourages consumer class actions significantly increases potential settlements; even if this increase is offset by institutional changes that discourage class actions, deterrence will, on balance, unambiguously increase if the mean of the settlement distribution is unaltered and colluders are risk averse.

[57]Since Illinois Brick prohibits an indirect consumer from recovering damages it also will have a significant effect on the number of settlements (actions) in industries where governments and institutions purchase from a distributor.

[58]For a good discussion of the parens patriae provision, see National Association of Attorneys General [30], pp. 7-8.

[59]Technically the penalty is not new; it is simply a new mechanism for collecting damages in price-fixing cases with a large class of affected individuals. It is, from our perspective, simply a method of

increasing the probability of a very large damage award or settlement.

[60]In addition to these two class action events, colluders may have been concerned also with the possible effects of the Antitrust Procedures and Penalty Act (PL93-528). This statute amended existing antitrust legislation to 1) make price-fixing (actually any violation of Sections 1, 2 and 3 of Sherman) a felony instead of a misdemeanor; 2) increase the maximum prison term to three years from one year; and 3) change the maximum fine from $50,000 to $100,000 for individuals and $1,000,000 for corporations. Unfortunately none of our bread cases were brought under the amended act, and we have no other direct evidence of whether colluders faced more serious penalties in 1976. However, based on the information compiled from Department of Justice sources by New York Times reporter David Burnham [14], it does not appear that stiffer criminal penalties became a relevant concern until 1977 and 1978. In fact, Donald I. Baker, former head of the Antitrust Division, made the point in 1976 that: "[A] higher percentage of persons convicted of violating the migratory bird laws were sentenced to prison, for longer terms than those who violated the antitrust laws."

[61]Even here there is of course the outside possibility of some confounding due to the provisions of the Antitrust Procedures and Penalty Act.

[62]Again, using a dummy variable of this form appears to cause some

problems by interacting with BUDG1. As in the EISEN1 form of the results in Table IX, YR76 reduces the coefficient on BUDG1.

[63]The coefficient on YR76 is also of the expected sign in the version of the regression equation that splits DOJREG both in class actions and EISEN and introduces price controls (see Appendix Table VII).

[64]When analyzing recent developments in consumer class actions, it would be inappropriate to neglect formally what is probably the most important Supreme Court decision regarding these actions: Illinois Brick Co. v. Illinois (1977). In this decision the Court ruled that indirect purchasers (ultimate consumers who buy from distributors) do not have a right to sue under current antitrust law. This ruling essentially eliminates both the traditional consumer class action in price-fixing cases and the yet untried parens patriae actions. The effect of this ruling on deterrence should be simple to predict. All things equal, this ruling, as noted in a previous section, will significantly reduce deterrence by effectively eliminating the outliers in the upper end of the settlement distirubtion. Our data in the bread industry, of course, stops prior to this action, and even if it were extended another year the best we could do would be to test for an annual markup effect. The decision appears to be important enough to warrant a separate research effort. This might involve both an extension of the current single industry time series approach as well as a cross section study that utilizes

inter-industry effects. Certainly the consumer class action must be a differentially important event across industries prone to price-fixing, and this fact might be profitably utilized in testing the deterrent effect of Illinois Brick.

NOTE

Section IV

[1]See [68].

GLOSSARY

BADJ	Adjusted bread price. [Unit price for bread minus estimated direct material costs per unit.]
BREAD	Retail price of a one-pound loaf of white bread; annual average, by city. Source: Estimated Retail Food Prices by City, Bureau of Labor Statistics, various issues, 1965-1976.
BUDGD	Fiscal year budget for the Antitrust Division of the Department of Justice (DOJ), expressed in real terms. Source: The Budget of the U.S. Government, Appendix (U.S. Government Printing Office), fiscal years 1966-1977; Consumer Price Index, Bureau of Labor Statistics, various issues, 1965-1976.
BUDG1	Annual change in BUDGD.
CASEFINE	Amount of fines imposed on defendants in the 16 price-fixing cases in the bread industry that involve fines, 1957-1975.
CITY DUMMIES	A dummy variable for every city equal to 1 for every year in the sample period for that city; zero otherwise.
CONTROLS	Dummy variable equal to 1 during the years of the Phases I and II of price and wage controls (most of the 1971-1972 period).
DEP1	Change in markup levels.
DMILK	Wholesale price of dry milk per pound; annual average, by city. Source: Wholesale Prices and Price Indexes, Bureau of Labor Statistics, various issues, 1965-1976.
DOJ	Dummy variable equal to 1 in a city in the year of a DOJ action is filed in that city; zero otherwise.
DOJ1	Dummy variable equal to 1 in a city one year before a DOJ action is filed; zero otherwise.
DOJB	Dummy variable equal to 1 in a city one year after a DOJ action is filed; zero otherwise. [Used to test the immediate remedial effect of a DOJ filing in the offending city.]
DOJCASES	Annual number of DOJ bread investigations.
DOJCASE1	Annual changes in DOJCASES. [Used in Appendix Table III.]

DOJREG	Dummy variable equal to 1 in a region where a DOJ action is filed for that year, except in the city of the action; zero otherwise. [Used to test the deterrent effect of DOJ filings.]
DOJREG1	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1964-1969, except in the city of the action; zero otherwise. [Used to test the deterrent effect of DOJ filings during 1964-1969, the period preceding the first distributor class actions against bread conspiracies in 1970. Splits DOJREG on 1971.]
DOJREG2	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1970-1976, except in the city of the action; zero otherwise. [Used to test the deterrent effect of DOJ filings during 1971-1976, the period following the advent of bread class actions by distributors in 1971. Splits DOJREG on 1971.]
DOJREG2A	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1970-73 except in the city of the action; zero otherwise.
DOJREG3	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1974-75 except in the city of the action; zero otherwise. There were no actions in 1976.
DOJREG12	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1964-1971, except in the city of the action; zero otherwise. [Analogous to DOJREG1, but splits DOJREG on 1972.]
DOJREG22	Dummy variable equal to 1 in a region where a DOJ action is filed for a year during 1972-1976, except in the city of the action; zero otherwise. [Analogous to DOJREG1, but instead splits DOJREG on 1972.]
EISEN	Dummy variable equal to 1 for all cities in 1974-1976; zero otherwise. [Used to test the erosion of antitrust deterrence during 1975-1976 because of increased costs of bringing consumer class actions after the Eisen decision in 1974.]
EISEN1	Dummy variable equal to 1 for all cities in 1974; zero otherwise. [Used to test the erosion of antitrust deterrence in 1974 because of the increased costs of bringing consumer class actions after the Eisen decision that year.]
ELEC	Monthly residential electricity rate for 500 kilowatt-hours; annual, by city. Source: Typical Electrical Bills, Federal Power Commission, various issues, 1965-1976.

FIRMFINE	Average fine per defendant firm in each of the 16 relevant price-fixing cases in the bread industry, 1957-1975.
FIT1	Total labor and energy costs in a loaf of bread. $FIT1 = 7.05464 + (.25616 \times ELEC) + (2.49165 \times GAS) + (1.0679 \times TRUCK)$
FLOUR	Retail price of flour per five-pound bag; annual average, by city. Source: Estimated Retail Food Prices by City, Bureau of Labor Statistics, various issues, 1965-1976.
G2	Dummy variable equal to 1 in 1969-1975.
GAS	Price of gas to industrial users, in dollars per therm, by city by year. Source: Data Services Department, American Gas Association.
OIL	Retail price per 24 fluid ounce bottle of cooking oil; annual average, by city. Source: Estimated Retail Food Prices by City, Bureau of Labor Statistics, various issues, 1965-1976. [Note: Bread recipes use mass, rather than volume, measures of shortening; consequently, cooking oil volume was converted to weight (via the .92 specific gravity of corn oil) so that the input price could be expressed per pound. See E. Eckey, Vegetable Fats and Oils, at 286-87 (1954).]
PCTX	Fines as a percentage of total defendants' sales pertaining to the 16 relevant price-fixing cases in the bread industry, 1957-1975.
PRED1	[BREAD - (RECIPE COST + FIL1)] ÷ BREAD.
PRED3	[BREAD - (RECIPE COST + FIT1)] ÷ (RECIPE COST + FIT1).
RECIPE COST	Total cost of ingredients in white bread: RC: .6350 × retail price per pound of flour + .0571 × retail price per pound of sugar + .0026 × retail price per pound of cooking oil* + .0350 × wholesale price per pound of dry milk *Cooking oil is used as a proxy for shortening. Source: A Study of Bread Prices, Council on Wage and Price Stability, p. 7 (April 1977).

SUGAR — Retail sugar price per five pounds; annual average, by city. Source: Estimated Retail Food Prices by City, Bureau of Labor Statistics, various issues, 1965-1976.

TIME — Number of years since 1956 (i.e., 1957 = 1). Used in testing for a trend in the amounts of fines imposed in the price-fixing cases.

TRUCK — Mean hourly wage of truck drivers (light truck, non-manufacturing); annual wage, by city; used as a proxy for bakers' wages, and used instead of food and kindred workers' wages. Source: Area Wage Survey, Bureau of Labor Statistics, various issues, 1965-1976.

WAGE — Mean hourly wage of food and kindred workers; annual average, by city. Source: Employment and Earnings, States and Areas 1934-1974, Bureau of Labor Statistics, 1975.

YR76 — Dummy variable equal to 1 for all cities in 1976; zero otherwise. [Used to test the deterrent effect of the first antitrust consumer class action to be certified against bakery products producers.]

YR7173 — Dummy variable equal to 1 for the years 1971-1973.

APPENDIX TABLE I

SAMPLE BY CITY AND YEAR

	65	66	67	68	69	70	71	72	73	74	75	76
Atlanta					X	X	X	X	X	X	X	X
Baltimore	X	X	X	X	X	X	X	X	X	X	X	X
Boston	X	X	X	X	X	X	X	X	X	X	X	X
Chicago	X	X	X	X	X	X	X	X	X	X	X	X
Cincinnati					X	X	X	X	X	X	X	X
Cleveland	X	X	X	X	X	X	X	X	X	X	X	X
Dallas					X	X	X	X	X	X	X	X
Detroit	X	X	X	X	X	X	X	X	X	X	X	X
Houston					X	X	X	X	X	X	X	X
Kansas City					X	X	X	X	X	X	X	X
Los Angeles	X	X	X	X	X	X	X	X	X	X	X	X
Minneapolis					X	X	X	X	X	X	X	X
New York	X	X	X	X	X	X	X	X	X	X	X	X
Philadelphia	X	X	X	X	X	X	X	X	X	X	X	X
Pittsburgh	X	X	X	X	X	X	X	X	X	X	X	X
St. Louis	X	X	X	X	X	X	X	X	X	X	X	X
San Diego					X	X	X	X	X	X	X	X
San Francisco	X	X	X	X	X	X	X	X	X	X	X	X
Seattle					X	X	X	X	X	X	X	X
Washington, D.C.	X	X	X	X	X	X	X	X	X	X	X	X

X: included in sample

APPENDIX TABLE II

EFFECT OF NON-RECIPE INPUT COSTS
ON ADJUSTED BREAD PRICE:
ALTERNATIVE SPECIFICATIONS

DEPENDENT VARIABLE:	LN(BADJ)[1]	BADJ	LN(BADJ)
INDEPENDENT VARIABLES:			
ELEC		.272 (4.64)[2]	
LN(ELEC)	.217 (4.89)		.225 (4.88)
GAS		1.726 (2.05)	
LN(GAS)	.084 (2.65)		.053 (1.59)
LN(TRUCK)	.261 (7.28)		
WAGE		1.054 (5.23)	
LN(WAGE)			.301 (5.98)
Constant	1.916	7.035	1.798
Number of Observations	228	228	228
R-Square	.46	.45	.42
F-Statistic (3,224)	63.63	60.21	55.01

[1] LN(·) indicates that the variable is a logarithm.

[2] Estimated coefficient divided by its standard error.

APPENDIX TABLE III

RELATIONSHIP BETWEEN BUDGET CHANGES AND CHANGES IN THE NUMBER OF BREAD INVESTIGATIONS

Dependent Variable:	DOJCASE1
Number of observations	15
R-square	.1958
F-statistic	3.16 (1, 13)
Independent Variable:	
BUDG1	.002 (1.779)[1]
Constant	-.696

[1] The estimated coefficient divided by its standard error.

APPENDIX TABLE IV

U.S. DEPARTMENT OF JUSTICE CASES
INVOLVING HORIZONTAL PRICE-FIXING
OF BAKERY PRODUCTS, 1954-1976

CASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES SOUGHT BY GOVERNMENT	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
U.S. v. General Baking Co., et al.	2/20/57	1319 crim.	Oklahoma, North Texas, South Kansas, Southwest Missouri	4 firms 1 individual	NA	NA; at least from 9/56 - 2/57	None	Nolo 3/11/57	Individual: $500; firms: $2,500 - $10,000
U.S. v. Continental Baking Co., et al.	2/20/57	1320 crim.	Las Vegas	5 firms 4 individuals	$1.5	1953 - 2/57	None	Nolo 3/5/58 (5 defendants); acquittal (4 defendants)	Individuals: $1,000; firms: $1,000 - $3,000
U.S. V. Continental Baking Co., et al.	5/7/58	1385 crim.	Memphis	4 firms	9	NA	None	Criminal conviction 12/18/58; reversed and remanded for new trial 7/18/60; criminal acquital 12/20/60	Firms: $3,500 - $42,500 (remanded)

Source: Trade Cases (CCH), 1955-75; Trade Reg. Rep (CCH).

APPENDIX TABLE IV
(continued)

CASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES SOUGHT BY GOVERNMENT	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
U.S. v. American Bakeries Co., et al.	3/6/61	1592 crim.	Florida, Southeast	6 firms	$23	4/60 - 3/61	None	Nolo 4/11/61	Firms: $3,000 - $5,000
U.S. v. Ward Baking Co., et al. (bid rigging)	3/6/61; 7/21/61 civil	1593 crim. 1618 civil	Naval installations in North Florida and Southeast Georgia	5 firms	11	9/57 - 3/61	Double damages under False Claims Act	Nolo 4/11/61 False Claims suit dismissed on payment of $44,000 5/8/62; consent 12/10/62; consent vacated and remanded by U.S. Supreme Ct. 3/9/64; new consent 9/1/65	Firms: $2,500 - $6,000
U.S. v. Greater New York Roll Bakers Ass'n, Inc. et al.	6/5/62	1662 crim. 1665 civil	Metropolitan New York City	9 firms 10 individuals 1 trade ass'n	NA	1956 - 6/62	Dissolution of trade ass'n sought; no damages sought	Nolo 12/27/62 - 3/19/64; consent 11/19/64	Individuals: $1,000 - $2,500; firms: $1,000 - $10,000

APPENDIX TABLE IV

(continued)

ASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES SOUGHT BY GOVERNMENT	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
.S. v. ard Baking o., et al	6/27/62 6/29/62 civil	1676 crim. 1687 civil	Philadelphia, Trenton, N.J.	6 firms 7 individuals	$4.5	4/60 - Spring 1961	None	Criminal conviction 5/17/63; 5/20/63; Nolo 8/29/ 8/29/63; consent 3/24/64 civil acquittal (Ward) 6/21/65	Individuals: $1,000 - $5,000; firms: $3,000 - $12,000
.S. v. ıerican ..eries .., et al.*†	10/4/67	1963A crim.	Michigan (statewide)	13 firms 13 individuals 1 trade ass'n	Over 200 for the 3 years of conspiracy	1/64 - 10/66	None	Nolo 3/15/68; 9 nolo pleas rejected at that time and subsequently allowed 6/13/ 68	Individuals: $0 - $2,000, suspended sentences and probation; firms: $1,250 - $45,000; trade ass'n: $250
S. v. Laub ..ing Co, al.*†§	11/14/67	1970 crim. 1971 civ.	Northern Ohio	5 firms 3 individuals	31	Unspecified time before 1964 - 11/67	None	Nolo 9/4/68 12/18/68; consent 9/8/69	Individuals: $500 - $1,000; firms: $3,000 - $35,000

'ase used in both DOJ and DOJREG samples
'ubsequent consumer class action denied
ubsequent non-consumer class action certified

APPENDIX TABLE IV
(continued)

CASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES SOUGHT BY GOVERNMENT	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
U.S. v. General Host Corp., et al.*	3/13/68	1997 crim. 1998 civil	Philadelphia, South New Jersey, North Delaware	7 firms 8 individuals	$40	1/64 - 3/68	None	Nolo 12/8/69; consent 3/1/71	Individuals: $1,000 - $4,500, suspended sentences and probation; firms: $2,500 - $7,500
U.S. v. Sweetheart Bakers, Inc., et al.; U.S. V. E.H. Koester, et al.*	7/29/71	2182 - 2185 crim./civil	Baltimore, parts of Maryland, Delaware and Virginia	4 firms 2 individuals	17 (Baltimore) 5 (3 partial states)	12/65 - 4/69	None	Nolo 9/23/71 1/13/72; consent 11/27/72 (2183, 2185)	Individuals: $2,000; firms: $5,000 - $25,000
U.S. v. American Bakeries Co., et al.*†§	9/12/27	2277 crim.	Metropolitan New York City	4 firms	75	1966-69	None	Nolo 11/20/72	Firms: $25,000 - $50,000
U.S. v. Gonella Baking Co., et al.*	10/4/72	2280 2281	Chicago	2 firms 3 individuals	NA	Early 1930s - 11/72	None	Nolo 8/21/73; consent 9/19/74	Individuals: $7,400 - $10,000, 1 year probation; firms: $30,000 - $45,000

*Case used in both DOJ and DOJREG samples
†Subsequent consumer class action denied
§Subsequent non-consumer class action certified

APPENDIX TABLE IV
(continued)

CASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
U.S. v. Rainbo Baking Co. of Phoenix et al.#¶§	2/14/74	2368 crim. 2369 civil	Phoenix	5 firms 6 individuals	$31	Since before 1963	Single damages under Clayton Act § 4 or double damages and forfeitures under False Claims Act (31 U.S.C. § 231-32	Nolo 3/20/74 civil settlement 6/10/76	Individuals: $8,00 and 1 month (suspended); firms: $7,500 - $30,000
U.S. v. Ideal Baking Co. of Paris, Inc., et al.#	2/28/75	2433A-2440 crim./civil	Louisiana, Arkansas, Mississippi, and Texas	9 firms 15 individuals	49	From mid-1950s, 7/71, 8/71, 12/72, until 2/75 (4 cases)	None	Nolo 5/16/75 - 9/26/75 Civil pending	Individuals: $0 - $7,500, probation; firms: $10,000 - $25,00

#Case used only in DOJREG sample
¶Subsequent consumer class action certified
§Subsequent non-consumer class action certified

APPENDIX TABLE IV

(continued)

CASE	DATE FILED	DOJ CASE NUMBER	GEOGRAPHIC MARKET	NUMBER OF DEFENDANTS	DEFENDANTS' ANNUAL SALES (in millions)	LENGTH OF VIOLATION	CIVIL DAMAGES	OUTCOME OF CASE	CRIMINAL FINES AND SENTENCES
U.S. v. Kahn's Bakery, et al.#	6/3/75	2455 2456 crim./ civil	El Paso	3 firms	$9	1954 - 1/74	Single damages or False Claims fine (31 U.S.C. § 231-32 (double damages)	Nolo 9/25/75	Firms: $20,000 - $25,000
U.S. v. ITT Continental Baking, et al.*	6/24/75	2465 crim. 2466 civil	San Diego	6 firms 5 individuals	25	1966 - at least 7/74	Single damages under Clayton Act § 4 or double damages and forfeitures under False Claims Act (31 U.S.C. § 231-32)	Criminal acquittal 1/28/76; civil dismissal 4/19/76	None

#Case used only in DOJREG sample
*Case used in both DOJ and DOJREG samples

APPENDIX TABLE V

DOJREG CASES

	65	66	67	68	69	70	71	72	73	74	[illegible]	76
Atlanta	-	-	-	-	-	-	-	-	-	-	1	-
Baltimore	-	-	-	1	-	-	-	-	-	-	-	-
Boston	-	-	-	-	-	-	-	1	-	-	-	-
Chicago	-	-	1	-	-	-	-	-	-	-	-	-
Cincinnati	-	-	-	-	-	-	-	-	-	-	-	-
Cleveland	-	-	-	-	-	-	-	-	-	-	-	-
Dallas	-	-	-	-	-	-	-	-	-	-	1	-
Detroit	-	-	-	-	-	-	-	1	-	-	-	-
Houston	-	-	-	-	-	-	-	-	-	-	1	-
Kansas City	-	-	-	-	-	-	-	1	-	-	-	-
Los Angeles	-	-	-	-	-	-	-	-	-	1	1	-
Minneapolis	-	-	1*	-	-	-	-	1	-	-	-	-
New York	[illegible]	-	-	-	-	-	-	-	-	-	-	-
Philadelphia	-	-	-	-	-	-	1	-	-	-	-	-
Pittsburgh	-	-	-	1	-	-	1	-	-	-	-	-
St. Louis	-	-	1	-	-	-	-	1	-	-	-	-
San Diego	-	-	-	-	-	-	-	-	-	1	-	-
San Francisco	-	-	-	-	-	-	-	-	-	-	-	-
Seattle	-	-	-	-	-	-	-	-	-	-	-	-
Washington, D.C.	-	-	-	1	-	-	1	-	-	-	-	-

1: There was a DOJ action in that region during that year, but not in that specific city.

*: This year was not included in the sample for Minneapolis.

APPENDIX TABLE VI

CLASS ACTIONS INVOLVING
HORIZONTAL PRICE-FIXING OF BAKERY PRODUCTS†
1966-1976

Case	Date of Class Certification or Denial*	Geographic Market	Preceding DOJ Case and Case Number	Consumer Class	Other Classes	Damages/ Settlement
Akron v. Laub Baking Co., 1972 Trade Cases (CCH) ¶ 73,188 (N.D. Oh. 1970)	N.A.; filed 4/18/70	Akron, Ohio	U.S. v. Laub Baking Co., et al. (1970, 1971)	Denied (1.3 million households)	Upheld (228 governmental and quasi-governmental agencies)	$188,458.07 (1975)
Donson Stores, Inc. v. American Bakeries Co., 58 F.R.D. 481 (S.D.N.Y. 1972)	3/20/73 (Consumer class denied) (Date of distributor class certification unknown)	New York City	U.S. v. American Bakeries Co. et al. (2277)	Denied (20 million consumers)	Upheld (Distributor class; number of members unknown)	$1,225,000 (1973-1975)
Kramer v. Gold Medal Baking Co. 1973 Trade Cases (CCH) ¶75,543, 17 F.R.Serv.2d 488 (E.D. Pa. 1973).	4/27/73	Philadelphia	None	None	Upheld (Distributor class; number of members unknown)	N.A.

†Compiled from Newberg, Class Actions (1977); McLaughlin, Federal Class Action Digests 1976 (1976); Trade Cases (CCH) 1966-1976; Trade Reg. Rep. (CCH); a LEXIS computer search in February 1978 of antitrust class actions involving price-fixing; and unpublished District Court records.

*Latest applicable date, e.g., date that certification is approved or denied on appeal, or date that notice method is approved after class certification, etc.

APPENDIX TABLE VI (continued)

CLASS ACTIONS INVOLVING HORIZONTAL PRICE-FIXING OF BAKERY PRODUCTS†

1966-1976

Case	Date of Class Certification or Denial*	Geographic Market	Preceding DOJ Case and Case Number	Consumer Class	Other Classes	Damages/ Settlement
Philadelphia v. General Host Corp., C.A. No's. 68-704, 68-708 (E.D. Pa. 1968).	1970	Philadelphia	U.S. v. General Host Corp. et al. (1997, 1998)	N.A.	Upheld (Five governmental entities and 49 educational institutions)	$517,862.88 (1971)
Detroit v. American Bakeries Co., C.A. No. 33046 (E.D. Mi. 1970)	N.A.	Detroit	U.S. v. American Bakeries Co, et al. (1963A)	Denied (2,540,000 households)	Upheld	$1,015,000 plus accrued interest (1971)
Hackett v. General Host Corp., 1972 Trade Cases (CCH) ¶ 73,879 (E.D. Pa. 1970), aff'd 455 F.2d 618 (3d Cir. 1972), cert. den. 407 U.S. 925, 92 S.Ct. 2460, 32 L.Ed.2d 812 (1972).	1/14/72	Philadelphia	U.S. v. General Host Corp. et al. (1997, 1998)	Denied (Six million consumers)	None	None

†Compiled from Newberg, Class Actions (1977); McLaughlin, Federal Class Action Digests 1976 (1976); Trade Cases (CCH) 1966-1976; Trade Reg. Rep. (CCH); a LEXIS computer search in February 1978 of antitrust class actions involving price-fixing; and unpublished District Court records.

*Latest applicable date, e.g., date that certification is approved or denied on appeal, or data that notice method is approved after class certification, etc.

APPENDIX TABLE VI (continued)

CLASS ACTIONS INVOLVING

HORIZONTAL PRICE-FIXING OF BAKERY PRODUCTS†

1966-1976

Case	Date of Class Certification or Denial*	Geographic Market	Preceding DOJ Case and Case Number	Consumer Class	Other Classes	Damages/ Settlements
In re Arizona Bakery Products Litigation, No. Civ. 74-208-A PHX CAM Pretrial Order No. 4, 1975-2 Trade Cases (CCH) ¶ 60,556 (D. Az. 1975).	10/20/75 (Distributor class) 5/10/76 (Consumer class)	Arizona	U.S. v. Rainbo Baking Co. of Phoenix, et al. (2368, 2369)	Upheld (800,000 consumers)	Upheld (Restaurant, grocery store, governmental body and private health care classes)	$6,100,000 (1976)

†Compiled from Newberg, Class Actions (1977); McLaughlin, Federal Class Action Digests 1976 (1976); Trade Cases (CCH), 1966-1976; Trade Reg. Rep. (CCH); a LEXIS computer search in February 1978 of antitrust class actions involving price-fixing; and unpublished District Court records.

*Latest applicable date, e.g., date that certification is approved or denied on appeal, or date that notice method is approved after class certification, etc.

APPENDIX TABLE VII

ESTIMATES OF THE EFFECT OF ENFORCEMENT, CLASS ACTIONS AND PRICE CONTROLS ON MARKUPS IN THE BREAD INDUSTRY, 1965-76

Dependent Variable	DEP1	DEP1
Number of observations	208	208
R-square	.2447	.2501
F-statistic	9.26 (7, 200)	9.53 (7, 200)
Independent Variables		
BUDG1	-.014[1] (-2.341)[2]	-.015 (-2.568)
DOJB	-.046 (-2.581)	-.038 (-2.154)
DOJREG1	-.008 (-0.370)	-.012 (-0.553)
DOJREG2A	-.035 (-1.921)	-.044 (-2.546)
DORJEG3	.003 (0.164)	.000 (0.034)
YR76	-.018 (-1.353)	-.020 (-1.530)
YR7173		-.041 (-5.301)
CONTROLS (price controls)	-.047 (-5.145)	
Constant	.023	.027

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

APPENDIX TABLE VIII

ESTIMATED EFFECTS OF ENFORCEMENT

USING ALTERNATIVE MEASURE OF MARKUP (DEP3): 1965-76

Dependent Variable	DEP3	DEP3	DEP3	DEP3
Number of observations	208	208	208	208
R-square	.0547	.0810	.0660	.0925
F-statistic	5.928 (2, 205)	6.038 (3, 204)	4.801 (3, 204)	5.176 (4, 203)
Independent Variables				
BUDGL	-.014 (-2.745)	-.015 (-2.921)	-.013 (-2.508)	-.014 (-2.683)[2]
DOJB		-.046 (-2.444)		-.046 (-2.439)
DOJREG	-.024 (-2.047)	-.026 (-2.211)		
DOJREG1			.006 (0.249)	.004 (0.158)
DOJREG2			-.035 (-2.564)	-.037 (-2.705)
Constant	.011	.013	.011	.013

[1] This coefficient is estimated per million dollars.

[2] The value of the coefficient divided by its standard error.

APPENDIX TABLE IX

ESTIMATES OF DETERRENT AND REMEDIAL EFFECTS IN THE BREAD INDUSTRY, 1965-76

Dependent Variable	DEP1	DEP1
Number of observations	208	208
R-square	.0715	.0972
F-statistic	3.905 (4, 203)	4.349 (5, 202)
Independent Variables		
BUDG1	-.017[1] (-3.099)[2]	-.017 (-3.280)
DOJB		-.046 (-2.399)
DOJREG	-.025 (-2.042)	-.026 (-2.206)
DOJ	-.018 (-.955)	-.020 (-1.061)
DOJ1	.017 (0.852)	.015 (0.771)
Constant	.014	.015

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

APPENDIX TABLE X

INTERACTION OF DEPARTMENT OF JUSTICE ENFORCEMENT ON CLASS ACTIONS IN THE BREAD INDUSTRY: CITY FORMULATION, 1965-76

DEPENDENT VARIABLE	DEP1	DEP1
Number of Observations	208	208
R-square	.1616	.1813
F-statistic	1.620 (22, 185)	1.77 (23, 184)
INDEPENDENT VARIABLES		
Enforcement/Class Action Variables		
BUDG1	-.016[1] (-2.994)[2]	-.107[1] (-3.135)[2]
DOJREG1	.005 (0.200)	.003 (0.123)
DOJREG2	-.033 (-2.315)	-.034 (-2.398)
DOJB		-.042 (-2.106)
City Variables		
Baltimore, Md.	-.037 (-1.609)	-.034 (-1.472)
Boston, Ms.	.012 (0.511)	.012 (0.509)
Chicago, Il.	.002 (0.098)	.005 (0.247)
Cincinnati, Oh.	.018 (0.733)	.018 (0.735)
Cleveland, Oh.	-.011 (-0.488)	-.008 (-0.350)
Dallas, Tx.	-.002 (-0.488)	-.002 (-0.350)

APPENDIX TABLE X

(continued)

Detroit, Mc.	.001 (0.480)	.014 (0.628)
Houston, Tx.	.003 (-.115)	.003 (0.116)
Kansas City, Mo.	-.023 (-0.907)	-.023 (-0.915)
Los Angeles, Ca.	-.024 (-1.030)	-.024 (-1.043)
Minneapolis, Mn.	.009 (0.375)	.009 (0.378)
New York, NY	-.010 (-0.452)	-.007 (-0.314)
Philadelphia, Pa.	.001 (0.046)	.004 (0.192)
Pittsburgh, Pa.	.005 (0.204)	.005 (0.206)
St. Louis, Mo.	.006 (0.276)	.006 (0.279)
San Diego, Ca.	-.034 (-1.357)	-.029 (-1.157)
San Francisco, Ca.	-.020 (-0.862)	-.020 (-0.880)
Seattle, Ws.	.014 (0.562)	.014 (0.563)
Washington, D.C.	-.012 (-0.508)	-.012 (-0.512)
Constant	.018	.018

[1]This coefficient is estimated per million dollars.

[2]The value of the coefficient divided by its standard error.

APPENDIX TABLE XI

CLASS ACTIONS FILED:
SOUTHERN DISTRICT OF NEW YORK:
1967-71[1]

Year	Number of Class Actions Filed	Class Actions/ All Civil Actions
1967	118	2.21%
1968	214	4.01%
1969	226	4.15%
1970	281	4.82%
1971	410	6.82%

[1]Sources: American College of Trial Lawyers, "Report and Recommendations of the Special Committee on Rule 23 of the Federal Rules of Civil Procedure," 1972, p. 13; and U.S. Government Printing Office: "Annual Report of the Director of the Administrative Office of the U.S. Courts."

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