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THE EFFECTS OF FTC ADVERTISING REGULATION

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Working Paper No. 019

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## THE EFFECTS OF FTC ADVERTISING REGULATION

This study inquires into the effects of the FTC's regulation of false and misleading advertising. Skepticism about the effectiveness of this regulation is widespread. For example, a Congressional committee has described the regulation as "impotent."<sup>1</sup> A team of lawyers under the aegis of Ralph Nader has echoed that sentiment.<sup>2</sup> The best that an American Bar Association panel could say was that "occasional successes" could not outweigh the "recurrent flaws of FTC enforcement."<sup>3</sup> A noted legal scholar, who has cast doubt on the only "success" cited,<sup>4</sup> wonders if a system of regulation without penalties can be effective, but argues that this is probably for the best since there is no substantial problem worth regulating anyway.<sup>5</sup> The authors of an industrial organization textbook think there is a problem, but damn

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<sup>1</sup>U.S. House of Representatives, Committee on Government Operations, False and Misleading Advertising, 85th Congress, 2nd Session, 1958, p. 2668.

<sup>2</sup>E. Cox, R. Fellmeth, and J. Schulz, The Consumer and the Federal Trade Commission (1969).

<sup>3</sup>American Bar Association, Committee to Study the Federal Trade Commission, Report (1969), p. 41.

<sup>4</sup>The alleged success does not, strictly speaking, involve false advertising. It is an FTC rule requiring disclosure of nicotine content of cigarettes. Richard Posner argues that this rule did not affect market shares of cigarettes with high nicotine content. See R. Posner, "The FTC's Mandated Disclosure Program," in H. Goldschmid, ed., Business Disclosure: The Government's Need to Know (1979), pp. 331-359.

<sup>5</sup>R. Posner, Regulation of Advertising by the FTC, American Enterprise Institute (1973).

by faint praise an enforcement mechanism that "is not to be described as rigorous."<sup>6</sup> But these critiques are based entirely on procedural and statutory exegesis, rather than evidence on the actual effects of the regulation. This study seeks to provide such evidence. It adds to a lengthening list of empirical studies of the effects of regulation, but is perhaps the first in which the prior consensus belief is that the regulation is ineffective. It shares with much of this literature the conclusion that this consensus is wrong, or at least exaggerated.

The paper begins by outlining the relevant regulatory institutions and the reasons for the prevailing skepticism about their effectiveness. This is followed by some theoretical considerations designed to motivate the empirical work. Here, I draw on and extend some of the literature on the economics of advertising to derive some implications for the behavior of markets--prices, outputs, and advertising expenses--in which one or more firms advertise "falsely" and then are legally prevented from continuing to do so. The main part of the paper confronts these implications with data from product and advertising markets which were subject to FTC regulation and collateral data from the capital market.

A couple of caveats deserve immediate statement:

(1) It is far easier to take at face value an FTC charge that an advertisement is "false" and then elaborate the marketplace effects of the ensuing regulation than it is to evaluate normatively these effects. This is partly because there is no obvious definition of "false" advertising.

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<sup>6</sup>C. Wilcox and W. G. Shepherd, Public Policies Toward Business (1975), p. 654.

Substitute "aggressively competitive" for "false and misleading" and the normative connotation of successful regulation will change, but the measurable effects will be hard to distinguish. I will try to indicate how the effects of perversely successful regulation might be distinguished from those where genuine deception is eliminated; but, as I will indicate, the difficulty of doing this remains too formidable. Consequently, normative issues are treated lightly here.

A specific case may illustrate the difficulty. Blue Bonnet margarine advertised that "moisture buds" in its product made it taste more like butter than its competitors. The FTC succeeded in having the ad removed on the ground that Blue Bonnet did not in fact contain any unique flavor-enhancing ingredient. Suppose the FTC charge is true; indeed, suppose Blue Bonnet is physically identical to some of its competitors. To the common intuition the advertising is "false," and a showing that, e.g., Blue Bonnet's sales declined after it stopped showing the ad might imply that margarine buyers were saved paying for more than they received. But the actual content of the ad may not be the relevant message. This point has been made by Nelson.<sup>7</sup> For a good like margarine, an "experience" good in Nelson's lingo, the ad cannot convey any interesting objective information since the good has to be consumed before its important characteristics are revealed. Nelson then argues that what is important about such ads is their size, frequency, cost, etc., not their content. Even if the content is relevant, the message it is supposed to convey is not, especially for an experience good, obvious from the script. Suppose about all a Blue Bonnet ad could convey is that many consumers will find it better than or equal to other brands, so that, price the same, buying Blue Bonnet is superior to random selection of brands. Efficient provision of such

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<sup>7</sup> Phillip Nelson, Advertising as Information, 82 J. Pol. Econ. 729 (1974).

a message requires overcoming a "public goods" problem (the message will be underproduced if Blue Bonnet cannot exclude Mazola from sharing the benefits), and singing Blue Bonnet's praises to the exclusion of other brands does this. Efficiency is also enhanced if the message is quickly assimilated and remembered, and the palaver about "moisture buds" may accomplish this better than alternative messages; surely better than, say, a chemical analysis of the product and its competitors. On either of these views, pure Nelson or Nelson modified, the FTC action would involve net consumer losses; the ad would mislead few and its absence would serve mainly to reduce the stock of valuable relevant information.

Much of the empirical work here consists of things like before/after comparisons of Blue Bonnet sales. When terms like "false advertising" or "guilty advertiser" are used in conjunction with such comparisons, they should be understood as my equivalent of avoiding the chemical analysis--i.e., as useful shorthand.

(2) The usual disclaimer about data inadequacy deserves special emphasis here. In a typical year, there will be literally hundreds of FTC advertising proceedings concluded (and many more settled prior to a formal proceeding). The vast majority concern sellers too small or localized to generate relevant data that we might even have hope of accessing. A very few cases, maybe four or five a year, will be of the Blue Bonnet type, involving a large seller of a product with substantial enough sales to have the potential for generating useful data. The empirical work is limited to this small and size-biased subsample of all FTC cases. This focus on so skewed a sample does have advantages. In terms of the potential impact on consumers and of the FTC's commitment of resources, our coverage is much greater than the small numbers suggest.

Also, these are the sort of cases that tend to garner media publicity. Hence, we can, in principle, distinguish "announcement" effects of an FTC proceeding from those of its resolution. However, any claim of comprehensiveness for the empirical work would be false and misleading.

The more debilitating problem is that even where useful data exist, they are usually proprietary--to the advertisers and/or suppliers of market research services to them--and not easily accessible to an outsider. As a result, we are confined to a motley collection of cases and data which are a small subsample of a small subsample. In the period 1960-75, I was able to find around 80 FTC cases where the potential for relevant data seemed sufficiently promising (i.e., the products had sufficiently widespread distribution and the brands involved were sufficiently well-known) to warrant further inquiry. In my analysis of the product market effects of FTC regulation, only thirteen cases involving seven products are treated, and the quality of the data varies considerably among these cases. This paucity of data has to temper the conclusions that can be drawn from my results. The analysis is best viewed as an initial exercise whose underlying methodology might usefully be expanded upon.

#### I. Institutional Background

The statutory basis of the FTC's current regulation of advertising is a series of 1938 amendments to the 1914 FTC Act. These empower the FTC to prevent "deceptive" acts or practices in the sale of goods in general, and add some stronger strictures against false advertising of foods, drugs, and

cosmetics in particular.<sup>8</sup> The stricture on "deceptive" ads was added to one on "unfair methods of competition" in the 1914 Act. The courts had interpreted "unfair" to mean methods which injured competitors rather than consumers, and the 1938 wording was designed to overcome this limitation. Beyond this statement of Congressional intent, there is little in the law to explicitly guide or limit the mechanics of enforcement.

The mechanics have by now become fairly routinized. A salient characteristic is that each instance of false advertising is treated as a unique case. In principle, the FTC could prohibit (or require) mention of, say, flavor enhancers, or even of flavor, in any ad for any brand of margarine. In practice, it has rarely tried to regulate across a whole industry or type of ad, though it has moved further in this direction after passage of the 1975 Magnuson-Moss Act.<sup>9</sup> Instead, it will issue a complaint against, say, Blue Bonnet's particular ad. If it successfully prosecutes the complaint,<sup>10</sup> and Mazola then produces

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<sup>8</sup>For example, the FTC can ask a court to enjoin a false food or drug ad, but has rarely done so. This special statutory treatment of foods and drugs probably reflects the same Congressional concerns that led to important amendments to the Food, Drug, and Cosmetic Act in the same year. Even though actual FTC procedure in food and drug advertising cases is not usually different from other cases, it is notable that most of the major cases have involved food and drug ads.

<sup>9</sup>For example, it has sought to require claims for nonprescription drugs be stated in language used in drug labels approved by the Food & Drug Administration.

<sup>10</sup>In fact, the majority of nascent complaints are settled prior to any formal proceeding by an assurance from the seller that it will discontinue the ad.



a substantially similar ad, that would entail a separate complaint, during whose prosecution the ad could continue to be run. The formal complaint procedure is supposed to involve a hearing before an FTC administrative law judge, who recommends either that the complaint be dismissed or that the advertiser be ordered to cease and desist. The Commission then reviews the recommendation and issues its decision. Few cases run this whole procedural gamut. Most formal complaints signal negotiations with the FTC staff, which end in a consent decree whereby the advertiser agrees to terminate or modify the ad. Less frequently, the complaint may simply be dropped. Sometimes negotiations begin prior to formal complaint, and both formal complaint and cease-and-desist are entered simultaneously, thereby sparing the advertiser some unfavorable publicity. Whether ordered or consented to, the cease-and-desist carries no criminal penalties nor does it form the basis for consumer damage claims. The guilty advertiser is thus allowed a free "bite" of the forbidden fruit. Penalties are enforced only if the cease-and-desist is subsequently violated.

This description makes it easy to understand all the skepticism about the effectiveness of this regulation. It appears to try to prevent crime without any punishment that could not be avoided by a reasonably clever copywriter. The skepticism is compounded when consideration is given to some of the inadequacies of the FTC detection or enforcement procedure.<sup>11</sup>

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<sup>11</sup>For example, the ABA (supra n. 3, p. 42) found that some of the detection burden had been shifted to employees, who had been asked to report questionable ads they had personally seen, and to undergraduates hired to clip ads for perusal by FTC lawyers. The ABA (1969, p. 44) also found that no systematic survey of compliance with cease-and-desist orders had taken place for 15 years. Posner (supra n. 5, p. 15) points out that the lack of a damage remedy weakens incentives for consumers to aid detection by complaining of deception to the FTC.

Perhaps as a result of all these perceived institutional defects, Congress broadened the FTC's enforcement powers in 1975. Since then, the FTC has been able to go beyond a case-by-response by, e.g., obtaining injunctions, promulgating industry-wide advertising rules, and punishing violations of cease-and-desists by nonrespondents. These changes are too new to be evaluated here, so this paper will, in effect, try to see if the empirical judgment about pre-1975 enforcement underlying the changes was justified.

Since the main purpose of this paper is to examine empirically the effects of FTC cease-and-desist orders, there will be no extended discussion of these alleged weaknesses here. I will permit the data to tell us whether and to what extent a cease-and-desist constrains the effectiveness of a firm's advertising activities. In the next section, I discuss the effects of false advertising and of legal constraints on it.

## II. Theoretical Considerations

In an oft-cited article, Stigler<sup>12</sup> distinguished two roles of advertising: reaching potential new customers and reminding old customers. Since the latter are presumably already aware of the product's attributes, a false or misleading ad could not affect their behavior. These existing customers will need to be reminded of the product's name, where it can be bought, etc., none of which the advertiser would wish to falsify. Among the likely targets of a false ad—the potential new buyers—it is useful to recall Nelson's<sup>13</sup> distinction between "search" and "experience" goods. He argues that any incentive for false

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<sup>12</sup> George Stigler, *The Economics of Information*, 69 J. Pol. Econ. 213 (1961).

<sup>13</sup> See Nelson, supra n. 7.

advertising are likely to be greater for sellers of experience goods, because search-good buyers will be able to detect the exaggeration prior to purchase. Seen in this light, the previously noted dominance of experience goods among major FTC cases makes some sense.<sup>14</sup> Nelson, however, argues that any gains from false ads are likely to be temporary, because the experience of new buyers ultimately reveals any exaggeration. For the strategy to work, enough first-time buyers have to be persuaded to try the product for long enough to make up for the loss of repeat business as these buyers' experience accumulates.

But this says too little to limit the domain of false advertising. Let there be  $N$  potential customers who will become repeat purchasers of  $X$  if they try it once. Suppose, for illustrative purposes, that all  $N$  will respond to a truthful ad. If a false ad garners these  $N$  and another  $M$  besides, the advertiser of  $X$  ought to prefer it to the truthful ad. After the first trial only  $N/(N + M)$  percent, will repeat, but total sales are higher by  $M$ .<sup>15</sup> To make the choice of ad content an interesting problem, it must be either that the false ad costs more to produce or, perhaps more relevant, that it leads to a worse match with potential repeat customers. That is, some of the  $N$  won't respond

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<sup>14</sup> Of the 83 major 1960-75 cases from which I later draw subsamples, only six involved what appear to be search goods. Of these, four involve toys. Another five cases appear to fit Darby and Karni's category of "credence goods," goods whose distinct qualities may be difficult to ascertain even after much experience. (See Michael Darby and Edi Karni, Free Competition and the Optimal Amount of Fraud 16 J. Law & Econ. 67 (1973).) Three of these involve automotive products (gasoline, motor oil additive), where technically unsophisticated buyers' experience may yield little information, and two involve dog food, where the consumer may have difficulty articulating the experience.

<sup>15</sup> See Richard Schmalensee, A Model of Advertising and Quality 86 J. Pol. Econ. 485 (1978) for a similar argument.

to the false ad. For example, consider a product with two attributes—say, taste and texture—of varying importance to different  $N$ . To exaggerate taste in a given ad space or time, texture will have to be deemphasized. Then, those of the  $N$  for whom texture is more important will be less likely to respond to the misleading ad.

To put some formal structure on the tradeoff between first-time and repeat purchasers, which can be used to confront data, I adopt a model introduced by Telser.<sup>16</sup> He classifies buyers of a brand in any period into those who bought the brand last period and those who bought another brand last period and those who bought another brand last period but have switched to this one. For simplicity, assume there are only two brands: A, the brand of primary interest, and B. Purchasers always buy 1 unit. Then sales of A in period 1 can be expressed:

$$A_1 = tB_0 + rA_0 \quad (1)$$

where:

$t$  = the fraction of purchasers of B last period who now switch to A (transition probability), and

$r$  = the fraction of  $A_0$  who also buy A now (repeat purchase probability).

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<sup>16</sup> Lester Telser, The Demand for Branded Goods as Estimated from Consumer Panel Data 44 Rev. of Econ. and Stat. 300 (1962).

In a nongrowing market with no sales leakages to or from other products ( $A_1 + B_1 = A_0 + B_0$ ), a little manipulation of (1) yields:

$$\frac{m_1}{(1 - m_0)} = t + r \frac{m_0}{(1 - m_0)} \quad , \quad (2)$$

where  $m_1$  = market share of A in period 1. With the usual stochastic assumptions appended, (2) becomes a statistical model which promises information on transition and repeat purchase behavior from the parameters of a simple autoregression of the (transformed) market shares—i.e., the constant term of the autoregression is an estimate of the transition probability and the coefficient of the lagged market share variable is an estimate of the repeat-purchase probability. Intuitively, if new buyers arrive and depart randomly over time and never repeat, observed market shares will also behave randomly: A's sales today will simply be today's ration of new buyers and be uncorrelated with last period's sales. If, on the other extreme, the random arrivals all repeat (until a random departure), the best estimate of today's sales is yesterday's sales ( $r = 1$ ). The model is easily expanded to incorporate behavioral assumptions about the transition and repeat-purchase probabilities. For example, both ought to respond negatively to the price of a brand relative to competitors. In that case, Telser shows that a tractable linear approximation to the resulting model would be:

$$\frac{m_1}{(1 - m_0)} = a - bp + r \frac{m_0}{(1 - m_0)} \quad , \quad (3)$$

where a regression estimate of  $r$  continues to estimate the repeat-purchase

probability, and the estimate of  $a - bp$  at the sample mean of  $p$  is an estimate of  $t$ .<sup>17</sup>

This simple model has some obvious implications for advertising regulation. The first concerns the difficult issue of how one can infer that an ad is substantively false. From the preceding discussion, if buyers are misled we would observe a shift in the composition of buyers from repeaters to transitors ( $r$  would fall and  $t$  would rise). Unfortunately, such a shift is necessary, but probably not sufficient to permit an inference of false advertising. Since the reminding function of ads--i.e., the one that would affect  $r$ --cannot be much affected by product claims, any unusual stress on product claims, truthful or not, is likely to aim at increasing the number of transitors. If there is a difference in the behavior of customers moved by false ads as opposed to merely "aggressive" ads, it would be one of degree: one expects fewer subsequent drop-outs in the latter case. This caveat understood, in the empirical work I will test for the presence of a shift from repeaters to transitors in the clientele of a firm accused of false advertising in the period before the firm must cease and desist from displaying it. This will serve as a crude check on how sensible the FTC's enforcement effort is. If the targets of allegedly false ads for experience goods enthusiastically return for more of the same product, it would be hard to know how they had been misled, how they could be helped by cessation of the ad, or what worthwhile outcome could be expected if similar ads are deterred.

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<sup>17</sup> This holds only if the covariance of  $p$  and  $(m_0/1 - m_0)$  is sufficiently small, a conclusion which typically holds for the sort of data to which the model is applied. See Telser, op. cit. supra n. 16, p. 311.

I will also test for the operational success or failure of allegedly false ads by estimating the pre-case change in the height of demand for the advertised product. Since any false ad is a gamble that more transitors will be garnered than potential repeaters will be lost, there should be no presumption that false ads which happen to be detected by the FTC are typically successful. The ad itself may be an unskillful example of the genre if it is caught in the FTC's net, or the detection itself could cut off some of the intended gains. But a normative evaluation of FTC enforcement ought to credit any unusual selectivity of successful false ads--those which increase brand demand--since the market will select out the other type. The credit would be withheld if we see targets of FTC cases experiencing unusual demand growth prior to the case without any unusual reduction of customer loyalty.

The same evidence--shifts in demand and in the customer loyalty parameters of (2) or (3)--will be examined for the post-case period. If FTC regulation is effective, we ought to find that any unusual bulge in demand is reduced and that buyer loyalty is increased, since the presumed lure for disappointed transitors has been removed. Post-case data also provides some opportunity for distinguishing among explanations of FTC behavior. If the

regulation merely taxes worthwhile advertising, any post-case effects should be temporary. The difficulty here is that even a tax on false advertising would not have the same effects forever, since the advertiser can adjust by reformulating the ad content. There are also a few cases which the FTC loses or drops. If the regulatory process effectively culls out the false ads, these "mistakes" should (a) have no unusually low customer loyalty before the case and (b) no permanent increase in loyalty (loss of transitors) after the case is dropped.

Another implication of effective regulation, which we have only a scrap of data to test, concerns demand elasticities. Treat a false ad as distorting a potential transitor's estimate of the gains from search among brands. It raises the expected gain of including the advertiser's brand and reduces the marginal gain of including additional brands in the search for the one that yields the best value. In that case, false advertising ought to reduce a brand's cross-elasticity of demand, and this would be offset once the ad is detected and removed. To see this most simply, write:

$$t = 1 - g ; \quad (4)$$

where

$t$  = transition probability for brand  $i$ , and

$g$  = probability that a searcher buys some other brand.

In general,

$$\begin{aligned} g &= g(P), & g' &> 0 \\ p &= p^i/p^0 ; \end{aligned} \quad (5)$$



where

$P^i$  = price of  $i$ , and

$P^0$  = the average price of other brands searched, which declines with the number of brands searched,

Thus,

$$P^0 = P^0(S), \quad P^0_S < 0 ; \quad (6)$$

where  $S$  = number of non- $i$  searched.

Define the absolute value of the relevant elasticity ( $E$ ):

$$E = -t_p \cdot \frac{P}{t} = g_p \cdot \frac{P}{1 - g} . \quad (7)$$

Now, the  $S$  in (6) is the result of an optimizing process whose only interesting characteristic for us is that less occurs with false advertising. To see the effects of false advertising on  $E$ , let  $X$  denote "more false advertising of  $i$ " and derive:

$$\frac{dE}{dX} = -E(1 + E) \frac{P^0_S}{P^0} \cdot \frac{dS}{dX} , \quad (8)$$

which is negative, since  $\frac{dS}{dX} < 0$ .

Finally, I derive some implications of false advertising for the advertising, rather than the goods, market: Generally, firms which advertise falsely will advertise more than others. But this needs to be interpreted carefully, since the obverse does not hold. To sort the issues out, a simple model is helpful. Let there be  $N$  potential customers for a firm's brand. Each buys 1 unit today at a \$1 price if he buys this brand. If the customer stays with this brand, he buys  $m$  units over all future periods. There are no costs of production; the only cost to the firm is for advertising to reach potential customers. So, ignoring discounting, the value of the firm is:

sales today + sales tomorrow - advertising .

To introduce the content of the ad message, let us write sales today (Q):

$$Q = g(A) \cdot p(c) ; \quad (9)$$

where

$g$  = contact function; the number of potential customers who see the ad.

This is assumed to depend only on the amount spent for advertising (A), and we'll assume diminishing returns to A; i.e.,  $g_A > 0$ ,  $g_{AA} < 0$ .

$p$  = probability that a contacted potential customer buys.

Here I will assume short-run gullibility: Let  $c$  stand for the extent to which the claims made for the product exaggerate the truth, and assume  $p_c > 0$ . While we await a suitable metric, it will do to think of assigning  $c = 0$  to something like "Blue Bonnet is margarine" and  $c =$  a very high number to "Blue Bonnet: your key to immortality."

In line with the previous discussion, the major cost of exaggeration is loss of future sales, so we write sales tomorrow (F) as:

$$F = mQ \cdot h(c) ; \quad (10)$$

where

$mQ$  = future sales if all of today's buyers remain loyal,

$h$  = probability that a buyer remains loyal:  $h_c < 0$   
because exaggeration brings ultimate disappointment to some and a worse fit of first-time buyers to the attributes desired (too many immortality seekers and not enough bread eaters).

The value of the firm (V) is then:

$$V = Q(1 + mh) - A \quad , \quad (11)$$

which the firm maximizes by choosing  $A$  and  $C$ . The first-order conditions are:

$$V_C = gP_C(1 + mh) + gP \cdot mh_C = 0 \quad ; \quad (12)$$

$$V_A = (1 + mh)g_A \cdot P - 1 = 0 \quad . \quad (13)$$

In each equation, the first r.h.s. term is the relevant marginal revenue and the second is marginal cost. (We are, fortunately, spared from having to say much about second derivatives of  $p$  and  $h$ , except that an interior maximum is guaranteed if they are not positive.)

In this model, firms will dissemble more the greater the short-run gullibility or the less long-run disappointment among their customers. In either case, the equilibrium level of  $A$  will also increase. For example, let some factor  $X$  increase  $p_C$  over the whole range, so  $p_{CX} = +1$  and  $P_X = +C$ . The sign of the resulting change in the equilibrium levels of  $C$  and  $A$ , respectively, are those of:

$$V_{CX} = g(1 + mh) \left(1 - \frac{P_C}{P/C}\right) \quad ; \quad (14)$$

$$V_{AX} = C/P \quad . \quad (15)$$

(15) is obviously positive, and (14) is positive for "typical" functions.<sup>18</sup>

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<sup>18</sup> For example, if we assume that  $P(0) > 0$ --i.e., no exaggeration produces at least some sales--the marginal gain to exaggeration ( $P_C$ ) will be below the average ( $P/C$ ) and (13) will be positive.

The results for  $h_{CX} > 0$  are the same. The "catch" here is that even in this skeletal model, there are potential stimuli to A that do not involve more C. For example, an increase in the productivity of A ( $g_{AX} > 0$ ) increases A but leaves C unchanged. Moreover, consistent with Nelson, an increase in the importance of future business ( $m_X > 0$ ) increases A and decreases C. There are also long-run competitive equilibrium considerations: if firms have access to the same technology and there is a change in, e.g.,  $p_C$ , there will be "entry" into the false-claim (as well as the counter-claim) business, and this will eventually reduce the short-run incentive to increase A.

It is obvious that to get anywhere here requires some strong assumptions. Mine will be: (1) the "technology" ( $g(A), m$ ) is the same for competing brands, so that, c.p., all brands have the same A (per N); (2) firms caught by the FTC are early entrants into the "false claim" business. From these, it follows that such firms should have been increasing their share (relative to competitors) of all advertising for the product prior to a case. The implications of effective regulation are less clear, because of distinct "stock" and "flow" effects. In our model, the whole stock of advertising capital is acquired at once. In reality, this takes time. Effective regulation implies destruction of an existing stock as well as a reduced rate of return on future investment in advertising. This means that, in equilibrium, investment will be reduced by effective regulation. But, unless any equilibrium is usually attained very rapidly, effective regulation could induce a temporary increase in investment (and, hence, the expenditures which we measure) for stock replacement purposes.

All this leads cautiously to the following advertising expenditure implications of effective regulation: the growth rate of the firm's advertising

expenditures (relative to competitors) will rise prior to a case and begin to decline at some point after it loses the case. This will ultimately lead to a smaller advertising share than the firm held just prior to the case. The decline need not, however, begin right after the case is lost, because of temporarily high stock replacement expenses.

Subsequent sections test both the product market and advertising market implications derived here. But we begin with an analysis of the stock market's judgment about the sum of these effects on the profitability of the firms subject to FTC regulation. All three types of data are consistent with effective regulation.

### III. Empirical Evidence on the Effects of Regulation

#### 1. Evidence from the Capital Market

The strongest evidence in this paper that FTC regulation has some non-trivial effects on the regulated firms is provided by the stock market. This market's independent appraisal seems clearly to be that FTC enforcement reduces the capital value of a firm's advertising, either past or prospective. Given the relevant magnitudes involved, stock prices would appear to be an unpromising source of information about the effects of FTC regulation. Ad expenditures on a particular campaign or product usually play a negligible role in the overall activities of large companies with actively traded stocks. For such companies, it is rare to find a product involved in an FTC case whose advertising expense accounts for over 1 percent of the company's total sales or assets. Even if the case wipes out the capital value of all past and potential advertising on the product, the capital loss would seem to be on the order of the standard deviation of a single day's return on the stock.

In spite of these potential problems, I examined the behavior of stocks in the period surrounding FTC complaints and decisions. From the 80 or so major FTC cases from 1960-75, a sample of 23 which met the following criteria was drawn:

1. The case began after July, 1962, which is the earliest available date for the stock market data.

2. Any company in a case involving a product which accounted for over half of company advertising is included, no matter how small the company's advertising expense. However, companies producing primarily nonconsumer goods are excluded.

3. Where a product accounts for at least 5 percent of the company advertising budget, the company is included if it spends at least 2 percent of sales on advertising.

This seems like an overly lax standard, but it was required by the data: most of the companies included in the sample are too diverse to meet (2) above, and a much stricter standard than (3) would have left too small a sample.

For each stock in the sample, I calculate cumulative excess returns (CER) for various periods surrounding the dates of FTC complaints and decisions. The CER is simply the sum of daily differences between the return on a stock and the return on a portfolio of stocks with similar systematic (beta) risk; the excess returns are from data files of the University of Chicago Center for Research in Security Prices (CRSP). Three stages in a case are distinguished, any of which can effectively terminate the case: (1) the complaint (which can be accompanied by a cease-and-desist); (2) an initial decision (which can be the recommendation of a hearing examiner, a dismissal of the case, an agreement to cease and desist, etc.); (3) in cases involving hearing-examiner

recommendations, a further decision to accept or reverse the recommendation is made by the full commission. Initial and further decisions were classified as either favorable or unfavorable to the company.

The data in Table 1 imply that involvement in an FTC case can be expensive, almost unbelievably so. Mean values of CERs (MCER) are shown for periods of up to a month on either side of an "event"--a complaint or decision. The strongest results are for complaints (lines 1-4). For 70 percent of the sample (line 3), the market appears to discount part of the likelihood of a complaint before the event, and then there is some further negative response following the complaint.<sup>19</sup> The general thrust of the results is that, on average, a 1 to 2 percent capital loss is suffered sometime in the month before a complaint and a further 2 percent or so is lost in the month after. Column (7) pins these dates down further, and leaves little doubt that the complaint is a critical event in this two-month period. Consider, for example, the loss over the whole two-month period surrounding the complaint (column (1) + (6)); it amounts to about  $-3\frac{1}{4}$  percent. Any other subperiod ( $\pm 2$  weeks or  $\pm 1$  week) has a loss of the same magnitude. Column (7) zeroes in on the week's trading from three days before through the day after the complaint. In these five days, the loss is essentially everything that is experienced over any longer surrounding period. And almost all of the stocks (21 of 23, or 91 percent) decline in this five-day period. In other words, except for a few days around the complaint, the MCER for the rest of the two months is essentially zero.

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<sup>19</sup>In a few cases, there is a lag of over a week between the date of complaint or decision and its public disclosure. This is because public announcement requires an official copy of the complaint or decision, which may take time to prepare. There were too few such cases to change any overall results if the "event" date is redefined to be the date of public disclosure.

TABLE 1  
 MEAN CUMULATIVE EXCESS RETURNS FOR COMPANIES INVOLVED IN FTC  
 CASES FOR VARIOUS PERIODS AROUND EVENT DATES. PERCENT

Type of Event (number of cases) and Statistic	Periods Ending Day Before Event and Beginning:			Periods Beginning the Day of Event and Ending:			1 Week Period: 3 Days Before Through 1 Day After Event
	1 Month Before (1)	2 Weeks Before (2)	1 Week Before (3)	1 Week After (4)	2 Weeks After (5)	1 Month After (6)	
<u>Complaints (23)</u>							
1. MCER	-1.57	-2.10	-1.29	-2.42	-2.25	-1.76	-3.12
2. $t_M$	1.06	2.01	1.74	3.27	2.15	1.19	4.22
3. F(-)	.70	.70	.70	.65	.61	.43	.91
4. $t_{F-.5}$	2.04	2.04	2.04	1.53	1.07	.63	7.03
<u>Initial Decision (18)</u>							
a) Favorable (5)							
5. MCER	1.72	.85	.21	-.02	.18	-.83	.97
6. $t_M$	.60	.42	.15	.01	.09	.29	.68
7. F(+)	.60	.40	.40	.60	.60	.40	.60
8. $t_{F-.5}$	.46	.46	.46	.46	.46	.46	.46
b) Unfavorable (13)							
9. MCER	.06	-1.66	-.34	-2.17	-.91	-1.82	-.58
10. $t_M$	.03	1.13	.33	2.09	.62	.88	.55
11. F(-)	.54	.54	.69	.69	.62	.46	.69
12. $t_{F-.5}$	.28	.28	1.50	1.50	.86	.28	1.50
<u>Further Decisions (7)</u>							
a) Favorable (4)							
13. MCER	-9.24	-6.15	-4.42	-1.08	-1.00	-2.04	-4.27
14. $t_M$	2.59	2.44	2.47	.61	.39	.57	2.39
15. F(+)	0	0	0	.25	.50	.25	.25
16. $t_{F-.5}$	—	—	—	1.15	0	1.15	1.15
b) Unfavorable (3)							
17. MCER	-3.18	1.50	.33	-.96	1.72	-.82	-1.24
18. $t_M$	1.24	.60	.18	.54	.68	.23	.70
19. F(-)	1.0	.33	.67	.67	.33	.67	1.0
20. $t_{F-.5}$	—	.61	.61	.61	.61	.61	—



The virtual unanimity of declines in these few days implies that some stocks anticipate the complaint by a few days, others react to it, but almost all are adversely affected by it. Since all of this stock market action is packed into the few days near the complaint, any notion that, e.g., the FTC is for some reason reacting to a stock market decline rather than creating one is scarcely credible.

While it is somewhat amazing that an adverse effect of complaints could show up so clearly in these data, the size of the effect is even more astounding. Even at two standard errors below the mean loss of 3.12 percent, the loss would still be around 1-1/2 percent. Recall that for the typical product involved in these cases, total advertising is (generously) on the order of 1 percent of company sales. The story the stock market appears to be telling is that an FTC complaint implies essentially a wiping out of the brand's advertising capital. Since most of these brands survive the ultimate disposition of the case, we have to suspect that the adverse effects on a company go beyond those on the market for the specific product.<sup>20</sup> At this point, these additional adverse effects are a mystery.<sup>21</sup>

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<sup>20</sup> But additional data argue against this. My selection criteria, based on the importance of a brand's advertising expense, excluded 16 companies from the sample. For these, the MCER in the critical week around a complaint was essentially zero.

<sup>21</sup> At least some of the large average effect of an FTC case can be explained by the skewed size distribution of the firms in the sample: the mean market value deflated by the Standard & Poor's 500 stock index of the 23 firms in the sample exceeds the median. One expects that larger companies will be affected less by a single FTC case than smaller companies for whom sales of a single product will be a large share of total sales. This is indeed so. The weighted (by deflated market value) MCER for the week surrounding complaints is -2.38 percent ( $t = 2.82$ ) v. the simple average of -3.12 in col. 7, line 1 of Table 1. When the company CER is regressed on the log of (company weight/sample-average-weight), the intercept of the regression (an estimate of the average-sized-company's CER) is -2.46 and the coefficient of the regressor is +1.21 ( $t = 2.32$ ). These are still substantial "average-company" effects, but the regression

Table 1 continued:

NOTE: Definition of symbols.

MCER = mean cumulative excess return for stocks in sample over period indicated.

$t_M$  = ratio of MCER to its standard error, absolute value. The standard error of MCER is computed from standard deviations of daily excess returns for each stock for the 100 trading days, terminating two months before the complaint (or decision, if more than six months elapse from complaint to decision or decision to decision). This is done to eliminate any effects of the case on the variability of returns. Serial independence of excess returns is assumed in calculating multi-day standard deviations.

F(-, +) = frequency of negative (-) or positive (+) excess returns in the sample over the period.

$t_{F-.5}$  = ratio of (F( ) - .5) to standard error of F( ), absolute value. The periods covered either terminate on the day before a complaint or decision, or begin the day of the complaint or decision. Thus, column (1) shows MCERs for the 20 trading days prior to a complaint or decision, column (6) shows MCERs for the 20 trading days beginning the day of the decision, and so on. In column (7), MCERs are for the five trading days, terminating on the day following a decision or complaint, so it contains three of the five days in column (3) and two of the five in column (4).

The companies in the sample (year of complaint) are:

Sterling Drug (3 cases: '63, '72, '74)	Standard Brands ('73)
Plough, Inc. ('63)	STP ('73)
Bristol Myers (3 cases: '67, '71, '72)	Colgate ('70)
Campbell Soups ('69)	Procter & Gamble ('70)
Carnation ('70)	Coca Cola ('70)
Standard Oil of California ('70)	Borden ('70)
Amstar ('71)	Mattel ('70)
Warner Lambert ('71)	Morton Norwich ('75)
Sun Co. ('71)	General Foods ('75)
American Home Products ('72)	

Data for two event dates are added for the STP case. The case was stimulated by a 1971 consumer magazine article charging misrepresentation by STP of its motor oil additive. The first public hint of FTC involvement I found was in press reports (Wall Street Journal, May 25, 1973) of an attack by Ralph Nader on the FTC's laxity in pursuing STP; I take this as the first "event date." The first disclosure that an FTC investigation was underway was in STP's 10-K report to the SEC dated September 10, 1973 (my second "event date"). It was disclosed that the FTC had been investigating STP since 1971. For all other stocks, the event date is the official date of FTC action, which can differ from the dates of public disclosure.

The remainder of the table contains no similarly strong results, and one additional mystery. This is the significant decline (line 13) preceding a favorable final decision. But we usually have too small samples to expect any strong results from the various types of decisions which occur in contested cases, and, given the fairly high unconditional probability of losing such a contest, it is no mystery that there is not much of an adverse effect from unfavorable rulings (lines 9 and 17). The overall message of the results is that the salary of the copywriter or lawyer who avoids entanglement with the FTC in the first place is a bargain.

The rest of the article explores the mechanisms by which FTC regulation could have exerted such profound negative effects on the regulated firms.

## 2. Advertising Expenditures

If an unusually successful advertiser faces an above-average marginal return to advertising investment, and if FTC regulation reduces this advantage, some counterpart to the capital market data might be found in advertising expenditures. My "false advertising" model implied higher expenditure a conventional strategy. The immediate impact of an effective long-run constraint on false advertising was ambiguous, due to counteracting "desired

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implies a substantial firm-size effect. Over the range of our sample, the estimated CER for the largest firm is only around -1% v. -7% for the smallest.

It is also worth noting some fairly weak evidence that firms involved in FTC cases do unusually well prior to a complaint. In the ten months up to the month before a complaint, the MCER for the sample was +5.05 percent. Of this, +3.38 percent is realized in the second half of this 10 month period. However, both these MCERs are only roughly equal to their standard errors. If these data are interpreted as the market's estimate of the value of an unusually successful advertising campaign, the Table implies that an FTC complaint just about wipes this value out.

stock" and "stock replacement" effects, but the model predicted a long-run reduction of the firm's advertising expenditures from the false advertising equilibrium.

I test these implications with advertising data for 18 FTC cases which were decided after 1969.<sup>22</sup> For each case, I computed the brand's share of total advertising expenditures in a product category over several years on either side of the cease-and-desist. The deflation is meant to adjust for nonregulatory factors affecting a brand's advertising expenses. The hypotheses to be tested are that (a) this ratio increases sometime preceding the cease-and-desist and (b) declines from the pre-cess-and-desist peak, but (c) the decline need not commence immediately following the cease-and-desist.

The relevant data are summarized in Table 2. I found that two of the 18 cases had such vastly different histories than the rest that any useful summary had to treat them separately. Except for these two outliers, the data tend to support all three implications of the false-advertising-cum-effective-regulation model. For most of the brands, their advertising share grows prior to the cease-and-desist (line 1). However (line 2), this growth is completed two years prior to the cease-and-desist. In the year following the cease-and-desist, the mean change in advertising share is essentially zero, with growing and declining shares about equally represented (line 3). However,

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<sup>22</sup>The data are from Leading National Advertisers (various years), which estimates total advertising expenditures on behalf of major advertised brands in various media. The pre-1969 data exclude spot TV and radio advertised expenditures. There was so much year-to-year variation in the pre-1969 expenditure series for particular brands that those data were unusable; apparent shifts from, e.g., network to spot TV would sometimes result in a virtually zero total expenditure for some brands in some pre-1969 years.

TABLE 2

GROWTH RATES OF ADVERTISING SHARES FOR 18 BRANDS IN FTC CASES:  
VARIOUS PERIODS BEFORE AND AFTER CEASE-AND-DESIST

Period	Growth Rates ( $\Delta \ln \times 100$ )				Frequency of Growth Rates with Same Sign as Mean, 16 Brands	
	16 Brands		2 Outliers		F	$t_{F-.5}$
	Mean	$t_M$	Mean	$t_M$		
	(1)	(2)	(3)	(4)	(5)	(6)
TO the year before cease-and-desist FROM:						
1. 3 years before	18.6	1.90	-94.5	12.60	.80	2.31
2. 2 years before	5.8	0.84	-24.5	1.69	.54	0.29
FROM the year before cease-and-desist TO:						
3. 1 year after	-4.3	.61	+43.0	1.34	.56	0.50
4. 2 years after	-11.0	1.06	+75.0	4.41	.69	1.58
5. 4 years after	-42.3	3.95	+126.30	2.09	.81	3.10

SOURCE: Leading National Advertisers, various issues. This gives quarterly advertising expenditures for brands and product categories. I aggregated these over annual (to eliminate seasonal effects) periods, ending in the quarter closest to the month of the cease-and-desist. The basic advertising share variable is then brand advertising expenses/aggregate product category and expenditures for the annual period so defined. (Leading National Advertisers product category definitions are used.) The table shows mean values of the change in logs of these shares for the specified interval. For example, if a cease-and-desist is entered in March 1972, that case's contribution to line 2 is log share (1971-II to 1972-I) - log share (1970-II to 1971-I); the contribution to line 1 is log share (1971-II to 1972-I) - log share (1969-II to 1970-I); and so forth.  $t_M$  is ratio of mean to its standard error (absolute value). For some cases, pre-cess-and-desist data are unavailable. Line 1, columns (1) and (2), is based on 10 cases, line 2 on 13 cases, and lines 3-5 on all 16 cases. The 16 cases (year of cease-and-desist) are:

J. B. Williams-Vivarin ('72)	STP Oil Treatment ('75)
Dupont-Zerax ('72)	Sterling Drug-Lysol ('74)
Mattel Toys ('71)	General Foods-Gaines Burgers ('74)
Borden-Kava ('71)	General Foods-Post Grape Nuts ('75)
Union Carbide-Prestone ('71)	Warner Lambert-Listerine ('74)
Procter & Gamble-Crisco ('72)	Ocean Spray Cranberry Juice ('72)
R. J. Reynolds-Hawaiian Punch ('73)	American Home Products-Easy Off ('72)
Carnation Instant Breakfast ('70)	Standard Brands-Fleischmann's ('73)

The two outliers (see text) are Bristol Myers-Dry Ban ('73) and IIT Continental Baking ('73). Column (5) shows fraction of all cases with same sign for log change share as the mean in column (1).  $t_{F-.5}$  is ratio of this frequency, less .5, to its standard error.

in the subsequent year most of the "normal" brands' shares begin a decline that subsequently accelerates. By the fourth year after the cease-and-desist, a substantial majority of brands' shares have fallen below their pre-cess-and-desist levels, and the average decline is substantial--around one-half (line 5). This last result seems consistent with a substantial regulatory constraint on the overall returns to advertising the affected brand, not merely those for pursuit of the pre-case strategy.

### 3. Effects on the Demand for Goods

In this section I compare consumer purchasing patterns before an FTC case is brought with those obtaining in the aftermath to see if they change in a manner consistent with our model of effective regulation. Because the data are so sparse and diverse, I will treat the cases separately in rough chronological order and then see what general conclusions this body of data seems to imply. A first triad of cases is discussed most extensively to provide background for the subsequent cases. The data involve thirteen of the more than 80 "major" 1960-75 cases.

(A) The Toothpaste Cases. The FTC sometimes opens roughly simultaneous cases against several firms in the same product category after, e.g., its staff completes a study of industry practices. This occurred in the toothpaste market in the early 1960s. This market was then in the midst of adjustment to a major technological change (the advent of fluoridated toothpaste), which brought about a substantial realignment of market shares. This competitive turmoil also entailed advertising practices that caught the eye of Congress. In 1958, a House Committee<sup>23</sup> held hearings on

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<sup>22</sup>See n. 1 supra.

allegedly deceptive dentrifice ads, which terminated in the usual criticism of the FTC's ineffectiveness and a call for stronger action.<sup>23</sup> It took a year for the FTC to respond. In November, 1959, it charged Colgate, the traditional market leader, with deception in its ads promoting the ingredient "gardol." The ads showed a giant tooth shrouded in a plastic guard which shielded the tooth from various great dangers and asserted that gardol provided a similar shield for ordinary teeth against decay. Not so, charged the FTC. Colgate denied guilt and its president counter-attacked that its competitors were promulgating deceptive ads.<sup>24</sup> In the same month, the FTC brought a second case against Pepsodent, which had been advertising the cosmetic (the ability to remove tobacco stains) rather than health virtues of its product. This, too, led to a denial, and both cases moved to the hearing stage. Colgate lost at both the hearing-examiner and Commission stage and agreed to cease and desist in March, 1961. Almost a year later, Pepsodent was exonerated. Within a week, in March, 1962, there was a simultaneous announcement of complaint and consent to cease and desist by Procter and Gamble, manufacturer of Crest. Crest had been the innovator in fluoridated toothpaste, and actively promoted its decay-resistant properties. It had, by 1962, risen to virtual market-share parity with Colgate. The offending Crest ad showed how many fewer cavities occurred among Crest users than among a control group using "regular" toothpaste. On investigation, it turned out that "regular" meant Crest without fluoride. The burden of the FTC case was that the term "regular" misled consumers to infer superiority of Crest over other brands.

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<sup>23</sup>Wall Street Journal, August 18, 1958.

<sup>24</sup>Wall Street Journal, January 28, 1960.

The three cases then cover a range from a widely publicized victory for the FTC (Colgate), to quiet victory (Crest), to a well-publicized loss (Pepso-dent).

The data we use to examine the effects of the cases come from published surveys by newspapers of the buying habits of consumers in their metropolitan markets.<sup>25</sup> The availability of the data is their major virtue. Among their more serious deficiencies are:

1. We have only brand market shares, not prices. This biases any test against showing effects of regulation. If the regulation does lower demand for a brand, the seller has an incentive to cut price, and this would help offset any loss of market share. If, as I argued, effective regulation also increases the elasticity of demand, the post-case market share may even exceed that prior to the case. But we are limited to inferring changes in demand from observed changes in market share.

2. The market shares are self-reported by consumers. The lack of independent verification or of any apparently large incentive of the supplier to check their accuracy implies measurement error.

3. The data are annual. This prevents examination of very short-run effects of regulation and creates problems for empirical implementation of the Telser model in (2) or (3). The relevant probabilities there apply over a

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<sup>25</sup> These were done as part of a loosely cooperative effort by newspapers in various cities and typically bear the title Consumer Analysis of (name of city). Some of the survey data are collected in Consolidated Consumer Analysis (various years). Both sources provided usable data for toothpaste and for the margarine and aluminum foil cases discussed later. The specific cities-newspapers whose Consumer Analysis data were used are: Denver Post, Milwaukee Journal, Indianapolis Star & News, Long Beach Independent & Press Telegram, Phoenix Republic and Gazette, Salt Lake Tribune and Deseret News, St. Paul Dispatch & Pioneer Press, Chicago Sun-Times, Omaha World-Herald(\*), Wichita Eagle(\*). \* = toothpaste and aluminum foil only.



representative consumer's decision period, and it would be reasonable to suppose that, for something like toothpaste, decisions to switch or stay with brands are made several times in a year.

The data consist of brand market shares for ten metropolitan-area markets; the pre-case data run from 1958-61 and the post-case data from 1961-67. The brands chosen are those which retained measurable market shares in a city for the entire 1958-67 period. (The competitive pressures initiated by Crest led some 1958 brands to disappear entirely or become submerged into an "all other" category by the 1960s.) All three of the brands involved in FTC cases met this test for all ten city markets. In total, there are 48 "brand-city" pairs in our sample. Of these, 30 contain the brands involved in the cases (3 brands  $\times$  10 cities); the remaining 18 are referred to as "other brands."

We look first at simple trends in market shares--our crude measure of shifts in demand--in the pre- and post-case periods. If any "deception" was effective, the "case brands," especially the "guilty" Colgate and Crest, should have been growing more rapidly than the "others" prior to the cases. If the regulation was effective, this excess growth would be smaller after the case for the two guilty brands but not for Pepsodent. The evidence, in Table 3, is mixed, but not very supportive of effective regulation.

It is clear that all three case brands, most especially Crest, were doing better than the others prior to 1961 (compare lines 2, 3, and 4 to line 1, col. (1)). But effective regulation should have halted this. There is indeed a marked reduction in Crest's growth, but this is essentially delayed to the post-1963 period when competitive fluoride products emerge in large numbers (see below). Colgate manages to halt its modest pre-1961 decline,

TABLE 3

## ANNUAL RATES OF CHANGE: TOOTH PASTE MARKET SHARES, 1958-61 AND 1961-67

Brands	Before Case (1958-61)		After Case (1961-67)		Change from Before Mean
	Growth of Market Share, %/year (1)	N (2)	Growth of Market Share, %/year (3)	N (4)	
1. Other Brands	-7.98% (1.10)	72	-10.29% (.61)	114	-2.31% (1.26)
2. Colgate	-2.40 (.84)	40	-0.25 (.53)	63	+2.15 (.99)
3. Crest	+28.15 (4.61)	40	+2.78 (1.19)	63	-25.37 (4.76)
4. Pepsodent	-3.33 (1.59)	40	-4.40 (1.34)	63	-1.07 (2.08)
Deviations from After Case Trend Lines			<u>1961</u>	<u>1962</u>	
5. Colgate			+21.51% (5.73)		
6. Crest			-46.20 (5.54)	-23.90 (3.37)	
7. Pepsodent			+26.15 (3.68)	-0.70 (.12)	

NOTE: For description of data and source, see text. Standard errors are shown below means. The growth rates are from simple regressions of market shares on time for brand-city pairs converted to percent per annum. To remove "between city" effects, the city mean for each brand in a period is subtracted from each observation. In the After Case regressions, 1961 dummy variables were included for the three case brands as well as 1962 dummies for Crest and Pepsodent. (These equal +1 for the given brand-year combination, zero otherwise.) Therefore, the after-case trends on lines 2, 3, 4 apply to years other than those singled out with dummy variables. The entries on lines 5, 6, and 7 are percentage deviations from the after-case trend lines summarized on lines 2, 3, and 4. N = sample size for the period. This is the number of brand-city pairs x 4 years in the pre-case period. Since there are 18 "other" brand-city pairs and 30 "case" brand-city pairs, the maximum N for 1961-67 would be 126 (7 years x 18) for line 1 and 70 for lines 2, 3, and 4. The actual N reflects some missing 1966 and 1967 observations.

and Pepsodent's decline is not alleviated by its exoneration. Surely the "other" brands do not appear to gain customers from the two large brands found guilty by the FTC; their plunge toward oblivion accelerates in the wake of these cases.

Of course, these long trends may hide the arguably more plausible short-run effects of regulation. Those to which these crude data can speak are not, however, much more encouraging. Recall that the Colgate case terminates in 1960. To see if its loss of the case affected its immediate growth, I reestimated the regressions underlying column (1) with a dummy variable, allowing Colgate to deviate from its 1958-61 trend in 1961. The result (not shown): no significant deviation. The same procedure underlies the post-case results in column (3). In estimating the trends on lines 2, 3, and 4, deviations for the early years of the period for case brands were permitted. These are shown on lines 5, 6, and 7. A positive (negative) deviation implies subsequently smaller (faster) growth than the brand's post-1961 average. Colgate's share, for example, falls by over 20 percent, to its essentially flat post-case trend, from 1961 to 1962. This could be read as a delayed response to its cease-and-desist of the prior year. However, the same fate befalls Pepsodent in the very year after it is exonerated. One then has to argue that consumers respond to the charge, not the outcome, to draw a link to FTC regulation. The data on line 6 for Crest's 1961-63 experience does not resolve matters--they imply that its prolific, over-20-percent annual market share growth continues, uninterrupted by its 1962 cease-and-desist order. The growth then halts abruptly after 1963. This halt would be more plausibly credited to the FTC than the response of competitors if it had occurred earlier.

If the data in Table 3 are examined less narrowly, a plausible role for the FTC does emerge. The fact is that after the rash of cases, the market settles down considerably. Twenty-percent-per-year market share changes disappear; Colgate and Crest essentially split 60 to 70 percent of the market for the post-1963 data we have; and the FTC has not brought a dentifrice case since 1962. All this is consistent with the FTC triad of cases acting as a deterrent to aggressive marketing strategies that might help create the sort of instability graphically reflected in the 1961 and 1962 data on lines 5, 6, and 7.

In Table 4, we examine the effects of the FTC cases on share stability more directly. They are, again, consistent at best with only a marginal role of regulation. Table 4 attempts to implement the Telser model (equation (2)), with a view to answering the following questions:

1. Were the allegedly false ads luring "triers" who were later disappointed? If so, the "case" brands would have below-average customer loyalty prior to the case.
  2. Did FTC regulation effectively constrain this behavior? If so, customer loyalty for the case brands would increase after the case.
- On both tests, Pepsodent ought to look like a "non-case" brand.

Since we have very short time series for each brand-city pair, direct application of equation (2) to each pair is not going to be very illuminating. Since there is a wide range of market shares across brands and cities and considerable persistence over time, simply combining brand-city pairs into one sample would lead to biased estimates of the parameters of (2).<sup>26</sup> Accordingly,

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<sup>26</sup> Specifically, the autoregressive parameter would be biased toward 1 if brand shares tend to cluster around widely different means.

TABLE 4  
ESTIMATED REPEAT PURCHASE SHARES: TOOTHPASTE BRANDS  
BEFORE AND AFTER 1961

Estimate	Before 1961			After 1961	
	Coefficient/t Ratio			Coefficient/t Ratio	
	(1)	(2)	(3)	(4)	(5)
1. Repeat purchase share (r) "other brands"	.3865 2.44	.3939 2.55	.3865 2.49	.7421 17.62	.7707 18.19
Deviation from line 1 for:					
2. Colgate	.0821 .31	.1988 .76	-.2472 .88	-.3607 5.11	-.3541 4.27
3. Crest	-.0302 .04	.0825 .62	-.0303 .04	-.1454 2.06	-.1996 2.45
4. Pepsodent	-.4496 1.69	-.7596 2.01	-.4496 1.73	-.3304 4.68	-.3403 4.07
5. Colgate in 1961			+1.6211 3.09		
6. Trend in r, "other brands," per year		.3608 2.34			.0842 3.60
Deviation from line 6 for:					
7. Colgate		.1941 .79			-.521 1.12
8. Crest		.4393 .64			-.1090 2.34
9. Pepsodent		-.3111 1.26			-.691 1.59
$R^2$	.06	.12	.10	.64	.65
S.E. × 100	116	114	114	41	41

NOTE: Dependent variable is  $\frac{(y_i - \bar{y}_i)}{S.D.(X_i)}$  and independent variables is  $\frac{X_i - \bar{X}_i}{S.D.(X_i)}$ ; where X and y are as defined in equation (16); S.D. = standard deviation. For lines 2, 3, and 4, the line-1 variable is multiplied by a dummy variable = +1 for the specific brand, 0 otherwise; for line 5 the dummy variable = +1 for Colgate in 1961 only. For line 6, variable on line 1 is multiplied by trend (mean = 0); for 7, 8, and 9, the line 2, 3, and 4 variables are multiplied by trend. See text and note to Table 3 for further explanation.

we proceed as follows: for any brand-city pair (1), the regression model implied by (2) is:

$$y_i = t_i + r_i X_i + u_i, \quad y = \frac{m_1}{1 - m_0}, \quad X = \frac{m_0}{1 - m_0} \quad (16)$$

$u$  = random error

For our purposes, we can focus on  $r_i$ . Its estimator is an expected value of the equilibrium share of a brand's sales accounted for by repeat buyers. Note further that a regression estimate of  $t_i$  is constrained by:

$$\hat{t}_i = \bar{y}_i - \hat{r}_i \bar{X}_i ; \quad (17)$$

so an estimate of the model,

$$(y_i - \bar{y}_i) = r_i (X_i - \bar{X}_i) + u \quad (18)$$

implies a consistent estimate of  $r_i$ . If we estimate (18) across all markets, the resulting estimator,  $\hat{r}$ , is an estimate of the average loyalty share for the brand across markets. This is what is done in Table 4, with one modification: the variables are divided by the standard deviation of  $X_i$ . This gives each city market equal weight in the calculation of  $\hat{r}$ .<sup>27</sup> For example, one observation of the dependent variable would be the value of  $y$  for Crest in 1959 in New York less the 1958-61 mean of  $y$  for Crest in New York and divided by the standard deviation of  $X$  for Crest in New York for '58-61.

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<sup>27</sup> And corrects for heteroskedasticity evident in the unnormalized data. The estimate in Table 4 should not be taken literally, since consumers do not plausibly make one purchase decision per year. It can, however, be shown that the true  $r$  ( $r^*$ ) is related to the estimate from annual data by:

$$r^* = r^{1/n} ;$$

where  $n$  = number of decisions per year. So the estimates in the table are at least monotonically related to those we would like.

The evidence on pre-1961 behavior (column (1)) seems inconsistent with the FTC's concerns, except possibly for Pepsodent customers. That brand does show a substantially below average loyalty share (almost implausibly so, since, theoretically,  $0 < r < 1$ ). However, the "guilty brands are essentially indistinguishable from the rest. In column (2), we allow for trends in  $r$  (by adding an interaction term between  $X$  and Time, and allowing the case brands' trends in  $r$  to deviate from other brands). The motive here is to see whether, as the case dates approached, the case brands' loyalty shares were declining. The results are on lines 6-9. The other brands loyalty shares were increasing over the period, but only Pepsodent gives any evidence of lagging behind.

If the data imply no problem, what do they say about the FTC's solution? The first thing to note about the post-case regressions in columns (4) and (5) is the marked increase in customer loyalty--the relevant parameter on line (1) doubles--and overall stability of market shares (the regression's standard error) falls by over one-half. This corroborates both the cruder evidence of Table 3 and the pre-1961 trend toward greater loyalty just noted. But two brands do not share fully in this shift; they are precisely the two found guilty by the FTC. Colgate's loyalty share essentially stays where it was pre-1961 and, thus (line 2, column (4)), is significantly below the post-1961 norm. Crest does experience an increase in loyalty, but since this is not as great as for non-case brands, the change is not plausibly related to the cease-and-desist order. Surely, none of this very clearly implies that the FTC cases changed the type of customers buying these brands. Instead,

it appears that by the early 1960s, the less aggressive brands had been stripped of their less loyal customers, and the emerging market leaders were left to take their new customers from each other.

In column (5), we again allow for trends in loyalty shares to see if there is any gradual effect of the FTC cases. The general trend (line 6) is toward increasing loyalty, but no more so--even less so--for the case brands (lines 7-9).

The one datum that seems consistent with effective regulation is in column (3), where (line 5) we permit the autoregressive process for Colgate to deviate from its pre-1961 norm for 1961, the year just after it agreed to cease and desist from its gardol ads. There is a significant, though implausibly large, increase in its loyalty share for 1961. Since this is not maintained beyond 1961, the hint is that the effects of FTC regulation are too short-lived to be caught by the data we have been examining. I followed up the hint by allowing similar deviations from the column (4) values for Crest and Pepsodent for 1962. In neither case was the deviation as much as a standard error, though both were positive.

In sum, the most notable difference between the pre- and post-case behavior of toothpaste markets is the considerable increase in their stability. However, our necessarily crude attempt to link this change directly to the FTC's regulation has been mainly unavailing.

(B) Blue Bonnet margarine and Alcoa Wrap. The previously described Blue Bonnet case resulted in a June 1960 agreement by the advertiser to cease and desist from claiming that its margarine contained a unique flavor enhancer. Alcoa advertised the superior properties of its Alcoa-wrap foil by comparing the appetizing results of a baked ham swathed in Alcoa-



wrap with the disaster befalling a similar ham clad in competitive foil. In January, 1960 the FTC charged that the comparison had been falsified by less careful preparation and more aggressive treatment of the latter ham. Over a year later, Alcoa agreed to terminate the ad. The relevant data for both cases are summarized in Table 5.<sup>28</sup>

For both products, a simple comparison of pre- and post-case trends in market share (lines 1 and 2) shows a sharp post-case deterioration for the case brands relative to their competitors. Both case brands had growing market shares in the pre-case period, while their competitors' shares were declining. That superior performance essentially vanishes for both of them in the post-case period, and this relative deterioration (line 2 minus line 1, cols. (5) and (6)) is significant for both. However, any link between this post-case change and the case itself seems more plausible for margarine. Blue Bonnet's immediate post-case shares fall below-trend (lines 3 and 4, cols. (1) and (3). Recall that plus values in col. 3 imply declining market shares in the early part of the post-case period). By contrast, Alcoawrap's share goes above-trend right after the cease and desist (lines 3 and 4, cols. (2) and (4)), and it takes two years for the post-case deterioration in its growth to set in.<sup>29</sup>

The repeat purchase share data on lines 5-7 are also consistent with some effects of the cases for both products. But here, the effects seem better defined for the Alcoa case. Alcoa, unlike Blue Bonnet, did appear to be attracting a disproportionate share of transitors prior to the case (compare col. (2) to col. (1) on line 6). For both case brands, the loyal-customer share rises

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<sup>28</sup>There were no discernible trends in repeat purchase probabilities for either product.

<sup>29</sup>Note, however, the large standard errors on the values in col. (4), lines 3 and 4, which imply the superior growth for Alcoa immediately after the cease and desist.

MARKET SHARE GROWTH RATES AND REPEAT PURCHASE PROBABILITIES  
BEFORE AND AFTER FTC CASES MARGARINE AND ALUMINUM FOIL.

VARIABLE	BEFORE CASE		AFTER CASE		CHANGE FROM BEFORE	
	MARG. (1)	ALUM. (2)	MARG. (3)	ALUM. (4)	MARG. (5)	ALUM. (6)
I. Annual Growth Rate of Market Share						
1. Other Brands	-6.9%	-3.9%	-2.9%	-1.9	+4.0%	+2.0
	1.1	1.9	.8	.5	1.3	1.9
2. Case Brand	+3.1	+4.5	+0.3	-2.7	-2.8	-7.3
	1.7	3.0	1.8	2.4	2.5	3.8
Deviation from Trend, Case Brand in:						
3. Year of Case						
(1960 for Margarine,	-7.3	+21.4	+17.8	-9.9		
1961 for Aluminum)	5.4	7.7	7.5	8.3		
4. Year After Case			+7.4	-7.9		
			6.2	6.2		
II. Repeat Purchase Shares						
5. Other Brands	.426	.573	.376	.492	-.050	-.081
	.080	.059	.065	.083	.103	.101
6. Deviation from Line 5 for Case Brand	-.048	-.440	-.201	-.248	-.154	+.192
	.196	.117	.147	.159	.245	.198
7. Deviation from Line 5 for Case Brand in Year of Case	+4.70	+6.33	+2.72	+2.02		
	.310	.275	.271	.330		

NOTE: See notes in Tables 1 and 2 for definitions of variables and see text for sources. Standard errors are shown below means. The means on lines 3 and 4 are derived from coefficients of dummy variables (= +1 in the indicated year) which were included in regressions of market share on trend. These coefficients, which measure deviations from the underlying trend, are expressed as percentages of the mean market share in the period. The growth rates on line 2 therefore apply to years other than those singled out on lines 3 and 4. The means on line 7 are coefficients of a variable equal to the lagged market share variable in equation (2) for the year of the case and equal to zero for other years. Therefore, the value on line 6 is the mean deviation for the case brands in years other than the year of the case. For margarine, there are 33 "brand-city" pairs, of which 8 are the case brand. The BEFORE-case period is the 5 years, 1956-60, so the maximum number of observations for this period is 165 (= 33 x 5). The actual number of observations is 160 (some 1956 data are missing). The AFTER-case period is 1960-65, and the actual number of observations is the maximum of 198. For aluminum foil there are 30 "brand-city" pairs (3 brands, 10 cities). The BEFORE-case period is 1956-61, and contains the maximum of 120 observations; the AFTER-case period is 1961-65.

right after the case, as would be implied by effective regulation. Again, the effect seems strongest for Alcoa (in cols. (1) and (2) of line 7, but not in cols. (3) and (4)). In the subsequent years of the post-case period, both case brands seem able to establish some (insignificantly) above average attraction of transitors (line 6, cols. (3) and (4)). But, in this respect, this is a change for the better for Blue Bonnet and for the worse for Alcoa (line 6, compare cols. (5) and (6), and note the large standard errors).

The details aside, the general pattern of post-case loyalty shares here seems similar to Colgate. That brand, like these two, temporarily lost some ability to attract transitors in the year after its cease and desist. Colgate, like Blue Bonnet, but not Alcoa or Crest, also lost substantial market share in that same period. So the 5 cases and 4 cease and desists examined so far seem to hint most clearly that FTC regulation does, perhaps temporarily, reduce the proportion of a brand's clientele which has switched from other brands. The consequences of this reduction for a brand's market share are, however, so far unclear. The cases subsequently discussed pursue the hint and the anomaly with better data.

(C) Two Gasoline Cases. In two unrelated cases about a year apart, the FTC attacked ads of Standard Oil Co. of California (Chevron) and Sun Oil Co. (Sunoco).<sup>30</sup> The FTC complained against Chevron in September 1970 and against Sunoco in December 1971. Both cases were challenged and took several years

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<sup>30</sup> Standard Oil had been advertising an additive (F310) to its Chevron gasoline which purported to clean auto engines and reduce pollution. Sun had been promoting the benefits of its high-octane gasoline, Sunoco 260. This had the highest octane rating in its marketing area and could be mixed with lower-octane grades at the pump. The Sunoco ads claimed that improved power ("260 action") could be obtained from such a mixture. The FTC charged that these ads misled motorists into thinking that the Sunoco blend was superior to other brands with the same octane rating and that, in any case, octane beyond an engine's minimum requirement did not improve power.

for the Commission to decide.<sup>31</sup> Chevron was exonerated by the FTC examiner in March, 1973, but he was reversed by the Commission in December, 1974. Chevron thereupon appealed to the courts, which reversed the FTC in 1976. Sunoco lost its case and agreed to cease and desist in July, 1974. Before either case was adjudicated, the Arab oil embargo occurred (October, 1973), and all gasoline advertising virtually ceased. Consequently, the FTC's victories did not constrain Chevron's and Sunoco's ads. But analysis of the aftermath of these cases is still interesting. It can reveal if termination of the type of advertising the FTC found offensive has different effects from termination of run-of-the-mill ads.

We have far better data for the gasoline cases than for any so far discussed. They consist of monthly, rather than annual, market shares (but not prices) for a number of states. Thus, we can explore short period effects of the cases, and we have sufficient degrees of freedom to hope for meaningful results.<sup>32</sup> Both Chevron and Sunoco are marketed regionally. Chevron is sold mainly in the West and Sunoco in the East. In industry parlance, both are "major" brands, which means that they are (were) heavily advertised, have non-trivial market shares in most of their regional markets, and sell at a price premium to non- (or less) advertised "independent" brands. For the empirical

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<sup>31</sup>Both companies continued the disputed ad campaigns throughout the litigation, up to the 1973 Arab oil embargo. Sunoco, however, modified its ads after the complaint, employing the slogan "Sunoco 260 Action: to be used, not abused."

<sup>32</sup>The data are from the Lundberg Survey, which is a copyrighted service providing gasoline market-share data to its clients. I obtained the data with the understanding that I would not reveal any of the monthly market shares. Annual market shares from the Lundberg Survey are published in the National Petroleum News.

analysis, I constructed a sample of states in each region in which Chevron or Sunoco were substantial participants. For each sample of states, I compare the market-share behavior of the "case brand" to that of a sample of other majors which also had nontrivial (over 2 percent) market shares in each state in the region.<sup>33</sup> Thus, the basic datum for analysis is market share<sub>ijt</sub>, where i = brand, j = state, t = month. The main comparisons are between the "case brands" and averages of the other brands. The time period over which such comparisons are made runs from January 1969 to July 1978, when retail price controls on gasoline became binding. Another period of binding controls, following the Arab oil embargo (October 1973 to April 1974) is excluded from the analysis.

The effect of the progress of the cases on the defendants' market shares is summarized in Table 7, while Table 6 gives the relevant chronologies. The data imply deleterious effects of hostile FTC actions for both brands, but less so for the ultimately-exonerated Chevron. Neither brand was gaining market share BEFORE the FTC complained. Both brands' shares erode DURING the gestation of the case (cols. (1) and (2)), but Sunoco's decline begins promptly after the complaint (col. (4), DURING I) while the data are ambiguous about

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<sup>33</sup>The states and brands are as follows. For the Chevron case, the competing brands are Shell, Texaco, Exxon, Mobil, and Union; the states are California, Oregon, Washington, Nevada, Arizona, and New Mexico. For the Sunoco case, the competing brands are Exxon, Mobil, Texaco, Gulf, and Arco; the states are New York, New Jersey, Pennsylvania, Delaware, Connecticut, Massachusetts, and New Hampshire.

TABLE 6  
SUBPERIODS FOR THE GASOLINE CASES

MNEMONIC FOR SUBPERIOD	TERMINAL DATES		DESCRIPTION
	CHEVRON	SUNOCO	
BEFORE	9/70	11/71	Period prior to FTC complaints
DURING I	4/73	9/73	FTC examiner rules in <u>favor</u> of Chevron. Sunoco agrees to cease and desist (7/74; post 9/73 date deleted because of embargo 10/73-4/74).
DURING II	11/74	X	FTC orders Chevron to cease and desist (11/74; embargo period data deleted).*
AFTER I	9/76	6/76	Two equal Subperiods after cease and desists
AFTER II	6/78	6/78	

NOTE: BEFORE period starts 2/69. Each subsequent period starts the month after the end of preceding period, except AFTER I for Sunoco, which begins 7/74.

\*Chevron won appeal in 1976.

TABLE 7

COMPARISON OF CASE BRANDS AND OTHER  
BRANDS MARKET SHARE GASOLINE CASES

Subperiod	Index of Market Shares (Before = 100) Case Brand/Average of Other Majors		Intra Period Difference of Growth Rate in Market Share (Std. Error) Case Brand-Average of Other Majors, %/year	
	Chevron (1)	Sunoco (2)	Chevron (3)	Sunoco (4)
BEFORE	100	100	-2.83 (1.14)	-0.36 (.42)
DURING I	96	98	-0.06 (.66)	-11.79 (.83)
DURING II	93	—	+5.91 (1.18)	—
AFTER I	94	89	+3.94 (1.06)	-2.58 (.69)
AFTER II	98	99	+0.07 (1.00)	+5.23 (1.13)

NOTE: Columns (1) and (2) are case brands' average monthly market shares divided by the average share of the other major brands, with the ratio expressed as an index. Columns (3) and (4) are derived by first regressing each brand's monthly market share on a time trend for each state and converting the trend coefficients to annual percentage rates. Then the average growth rate for other majors is deducted from the case brand's average growth rate. See n. 33 supra for the names of other brands and of states. See n. 32 for source.

the role of the Chevron complaint.<sup>34</sup> Chevron's decline is reversed after it is exonerated by the FTC examiner (col. (3), DURING II), and there is no apparent adverse effect of the subsequent negative FTC decision. On the other hand, Sunoco's decline does continue after it agrees to cease and desist (compare cols. (3) and (4) in AFTER I). However, for both brands any adverse effects of the cases seem temporary. About two years after the adverse ruling (i.e., in AFTER II), both brands have regained virtually all of the market share they had previously lost.

A similar, even stronger, story of discernible, but temporary, effects of regulation is told by the customer loyalty data for both brands. They are summarized in Table 8. A glance down either of the first two pairs of columns in that table reveals a remarkably similar series of zigs and zags in customer loyalty for both brands: declines after the complaints are issued, abrupt but temporary increases after the cease and desist orders. These post-complaint declines in customer loyalty seem contrary to our hypothesis that FTC hostility to the ads should have repelled potential transitors. If so, customer loyalty should rise after any unfavorable publicity from the complaint.

That apparent puzzle is resolved by a closer look at the data in the periods surrounding the complaint. Note first (line 1) that both brands have average (Sunoco) or above-average (Chevron) loyal customer shares over the whole

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<sup>34</sup>Two characteristics of the data should be noted: (1) The level of market share (columns (1) and (2) in Table 7) will tend to lag the intra-period growth rates. Positive growth in one period, if it is not reversed, implies that next period's level will be higher than this period's. (2) The data in columns (1) and (2) tend to give a greater weight to brands and states with high market shares, while brand-state pairs are weighted equally for columns (3) and (4). Thus, there is no mechanical connection between the data in the columns. Note the decline in Chevron's relative market share from DURING I to DURING II, while its average relative growth rate is zero or positive; this can be produced by a decline in large Chevron markets together with growth in smaller markets. Because of the different weighting schemes, a comparison of the two columns allows a useful check of the consistency of any effects of the case.



TABLE 8

ESTIMATED PERCENTAGE OF SALES TO REPEAT BUYERS:  
CASE BRAND v. OTHER BRANDS GASOLINE CASES VARIOUS  
SUBPERIODS 1969-1978

Subperiod	Case Brands		Case Brands - Avg. of Other Majors		Number of Observations Per Brand Per State	
	Chevron (1)	Sunoco (2)	Chevron (3)	Sunoco (4)	Chevron (5)	Sunoco (6)
1. BEFORE	61.2% (6.5)	63.1% (4.0)	+25.1% (7.4)	+4.0 (4.5)	20	34
2. DURING I	49.5 (5.5)	47.4 (7.2)	-6.0 (6.1)	-14.4 (7.7)	31	22
3. DURING II	38.0 (9.6)	X	-1.6 (10.8)	X	12	
4. AFTER I	70.7 (5.9)	66.9 (5.6)	+23.8 6.8	+14.9 (6.2)	22	24
5. AFTER II	40.9 (7.0)	43.9 (5.8)	-8.7 (7.7)	-8.1 (6.4)	21	24

Within Subperiod  
Differences: 2nd Half-  
1st Half:

6. BEFORE	-17.1 (17.7)	-30.5 (11.0)	-23.2 (19.7)	-34.8 (12.3)		
7. DURING I	-11.6 (14.0)	-31.9 (19.2)	-14.5 (15.5)	-62.5 (21.3)		
8. AFTER I	+26.4 (13.9)	-11.9 (15.1)	+14.6 (16.4)	+6.7 (16.5)		

NOTE: The estimated repeat-purchase shares are the regression coefficients from estimates of equation (2). This regression is run for each state-brand pair for any subperiod. Then the coefficients are averaged for each brand and these are averaged over the non-case brands. These non-case brand averages are deducted from the case brand average to get columns (3) and (4). For lines 6, 7, and 8, the basic regression is run with slope and intercept dummies, which permit the second half of the period to differ from the first half. The results for the coefficients of the slope dummies are summarized in the table. These coefficients answer: was customer loyalty larger or smaller in the second half of the period? A negative coefficient means "smaller in the second half." Columns (5) and (6) show the number of months for each brand-state regression. A total of 36 such regressions (6 brands × 6 states) is run for Chevron in each subperiod, and 42 (6 brands × 7 states) for Sunoco.

of the BEFORE period. However when we divide this period (line 6), we note that, as the date of complaint approached, these loyal customer shares were declining. Thus, the effects of the two ad campaigns seem to have increased as the FTC prepared to challenge them. The DURING period data (lines 2 and 3) indicate that these pre-complaint reductions in customer loyalty are not permanently reversed prior to the cease and desist orders. But when the DURING period is bifurcated (line 7), a temporary rise in customer loyalty can be discerned: the first part of that period has a higher loyalty share than the second. For both brands, the cease and desist orders are followed by sharp increases in loyal customer shares (line 4). Even though OPEC may deserve more credit than the FTC for the demise of the challenged ad campaigns, this rise in loyal customer share is just what we would expect if these campaigns had been unusually successful in attracting new buyers. However, within two years after the cease and desist orders, and coincident with their recovery of market share, both brands seem able to reassert their attraction for transitors (line 5).

The data for the two brands seem to tell the following common story: brands which had been finding it difficult to lure new customers begin to do so with their new ad campaigns. The FTC attacks and, for a while, some transitors are repelled. The ad campaigns finally end and the brands again experience trouble in luring transitors for a while. The effects of regulation are thus clearly discernible, but last only on the order of a year or two. This may help explain why these effects did not show up as strongly for the cases where we had only annual data.

(D) Widgets and Gimracks. For two cases, I use proprietary data provided me on condition that the products remain anonymous. The products are both food items sold primarily in supermarkets and bought by a substantial fraction of households. Extensively advertised national brands account for

the bulk of sales of both items, and both cases involve such brands. The products and cases are not otherwise related. Both cases occurred in the 1970s, and terminated in cease-and-desist orders.

The data for the products, which I will call Widgets and Gimracks, were provided by National Purchase Diary, Inc. (NPD). NPD employs a panel of consumers who record details of product purchases in a diary. My data are from national summaries of these diary records. For both cases, a trichotomy of brands is useful. In addition to "case" and "other" brands, I will distinguish "related brands." Such a brand has the same name and manufacturer as the case brand, but is advertised separately, and, therefore, not directly affected by the cease-and-desist. For example, think of Mother Jones Frozen Widgets as a case brand and Mother Jones Canned Widgets as a related brand. Since both are widgets, one wants to know whether the market for her canned version is affected adversely if some consumers learn that Mother Jones may have misrepresented the virtues of her frozen widgets.

The widget data have some glaring defects for the present purpose:

- (1) They are annual, which obviates analysis of short period effects.
- (2) We have only one annual observation per brand for any variable of interest, and the data begin in the year of the cease-and-desist. Therefore, we cannot replicate any test used previously. We can use the data only because the cease-and-desist occurred sufficiently late in the year to make that year a plausible pre-case reference period.

However, some of the data focus directly on buyer loyalty, which we have seen to be a potentially interesting dimension of the effect of regulation. Before analyzing these, I summarize the few scraps of data on market shares and relative prices. These are in Table 9, where Year 0 is the mostly pre-case year.

TABLE 9  
 MARKET SHARE AND PRICE INDEXES  
 (Case and Related Brands/Other Brands' Widgets. Year 0 = 100)

	<u>Market Share Indexes</u>		<u>Relative Prices Indexes</u>	
	Case	Related	Case/Other	Related/Other
1	106.5	88.4	102.5	104.9
2	91.3	79.1	100.3	103.2
3	73.9	79.1	97.6	103.1

SOURCE: National Purchase Diary, Inc. See text for description.

It is risky to draw strong conclusions, but the gist of the data is that neither the case nor related brands fared very well in the three years following the case. This is clearest for the case brand, which loses over one-quarter of its pre-case market share without any increase in its relative price.

The more interesting customer loyalty data are not repeat-purchase probabilities, but, rather, describe the propensity of those who buy one brand to buy other brands as well. For each member of the consumer panel, NPD calculates the share, in pounds, of the customer's total widget purchases over a year, accounted for by each brand. For each brand, the following summary measures result.

(1) Nonloyal buyer share: the fraction of all buyers who report buying the brand, but also report that the brand accounts for under 30 percent of their total widget purchases.

(2) Nonloyal volume share: the fraction of the brand's pound sales accounted for by nonloyal buyers.

The notion here is, of course, that "loyal" buyers would always buy the same brand. For Chamberlinian consumers who so value variety that they loyally consume several brands week-in and week-out, this may not be the best loyalty measure for our purpose, but the data do not allow us to account for such refinements.

The loyalty data are summarized in Table 10. The regressions are across brands and the basic dependent variable is the log odds of nonloyal purchases of the brand; i.e., the  $\log\left(\frac{\text{nonloyal buyers}}{\text{loyal buyers}}\right)$  for a brand. This is regressed on two zero-one dummy variables for the case and related brands, respectively. Year 0 is the base against which we measure the cease-and-desist order's effects. The first two regressions imply, but not too forcefully, some pre-case success by the case and related brands in attracting nonloyal customers. The more interesting post-case data take up the remainder of the table. The relevant standard errors are large enough to suggest a cautious interpretation,

TABLE 10

REGRESSIONS OF LOG-ODDS OF NONLOYAL BUYERS AND OF  
VOLUME SOLD TO NONLOYAL BUYERS: WIDGET BRANDS,  
YEAR 0 - YEAR 4

Regression and Year	Coefficient/t Ratio		R <sup>2</sup>	S.E. × 100	N
	Case Brand	Related Brands			
<b>A. <u>Level of log-odds</u> <u>in Year 0</u></b>					
1. Buyers	+ .197 .49	+ .042 .14	.02	38.4	17
2. Volume	+ .508 1.06	+ .558 1.59	.19	46.5	17
<b>B. <u>Change in log-odds</u></b>					
I. Year 0 to Year 1					
3. Buyers	-.402 2.83	+ .028 .27	.37	13.7	17
4. Volume	-.427 1.47	-.302 1.42	.22	28.1	17
II. Year 1 to Year 2					
5. Buyers	+ .195 0.92	-.089 .69	.09	20.4	18
6. Volume	+ .119 .31	-.099 .42	.02	37.1	20
III. Year 2 to Year 3					
7. Buyers	-.084 .32	-.066 .41	.02	25.2	18
8. Volume	-.164 .44	-.040 .18	.01	35.8	18
IV. Year 1 to Year 3					
9. Buyers	+ .131 .47	-.135 .79	.06	26.7	17
10. Volume	-.028 .09	-.122 .66	.03	29.1	18

SOURCE: National Purchase Diary, Inc. See text for description.

but the pattern is fairly clear. In the year after the case, the case brand loses both nonloyal buyers and volume in substantial amounts.<sup>35</sup> There is a similar decline in related-brand nonloyal volume (but not buyer) share. These losses are not made up in the next two years. Except for the longer duration, the effects here are, of course, the same as in both of the gasoline cases, and there is a hint here that the effects spill over to closely related brands.

The main gimrack data are monthly market shares and average-prices-paid by the NPD consumer panel. We have fewer than two years of pre-case monthly data on the case and related brands, and these turn out to be too few observations for most of the before/after case differences to be significant. We are therefore limited to asking about their qualitative consistency with the data from the other cases. Also, we can at least start in a new direction with the price data.

It is useful here to begin with results for customer loyalty. These are summarized in the first six columns of Table 11, and they come from regressions which implement the Telser model in equation (3), which includes a relative-price term. The model was estimated for the case, related, and eight other brands. These are all nationally distributed, well-advertised brands. The price variable in each regression is a time series of the price of the modal package size for the brand divided by the average per-package price for all gimrack brands.<sup>36</sup> The model is estimated for three subperiods: Before: 21

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<sup>35</sup>The coefficients imply a loss on the order of one-third of the 1974 nonloyal buyer and volume shares.

<sup>36</sup>Modal size is the size garnering more pound volume than any other: many brands come in several sizes, but one size is usually dominant. There are two considerations behind my choice of a relative price variable: (1)

LOYALTY AND PRICE ELASTICITY MEASURES: CASE, RELATED, AND  
OTHER GIMRACK BRANDS BY VARIOUS SUBPERIODS

Brand	Percent of Brand Volume Bought by Repeat Buyers				Relative Price Elasticity of Market Share (Short-Run, Absolute Value)							
	Before %	S.E. (2)	After I %	S.E. (4)	After II %	S.E. (6)	Before Mean	S.E. (8)	After I Mean	S.E. (10)	After II Mean	S.E. (12)
1. Case	(1.7%)	28.4	19.8	23.7	41.8	16.5	.86	.49	3.17	1.51	1.62	.79
2. Related	33.3	31.0	35.6	23.9	56.6	15.5	(.17)	1.27	3.58	1.61	2.03	1.84
3. Average of 8 Others	24.2	7.1	32.1	7.5	31.5	5.6	1.45	.26	1.78	.40	1.71	.31
Change from Before:												
4. Case (1)			+21.4	37.0	+43.5	32.8			+2.31	1.59	+7.6	.93
5. Related			+2.3	39.2	+23.3	34.7			+3.75	2.05	+2.21	2.45
Differences from Average of 8 Others:												
6. Case (1-3)	-25.9	29.3	-12.3	24.9	+10.3	17.4	-.59	.55	+1.39	1.57	-.09	.85
7. Related (2-3)	+9.1	31.8	+3.5	25.1	+25.0	16.5	-1.62	1.30	+1.80	2.09	+3.32	1.87
Change in Differ- ences from Before:												
8. Case (6)			+13.6	38.4	+36.1	34.1			+1.98	1.66	+4.9	1.01
9. Related (7)			-5.6	40.5	+16.0	35.8			+3.42	2.46	+1.95	2.28

NOTE: Data supplied by National Purchase Diary, Inc. See text for explanation. Columns (1)-(6) are from regression estimates of the parameter,  $r$ , in equation (3). Columns (7)-(12) are from regression estimates of  $b$  in that equation, converted to elasticities at sample means. ( ) denotes theoretically inadmissible values: a negative estimate of  $r$  or a positive estimate of  $b$ . The figures on lines 4 and 5 (or 8 and 9) show the change in a parameter from the pre-case (Before) period. Thus, for example, the figure on line 4, column (3), is the difference between the column (3) and column (1) values on line 1. The figures on lines 6 and 7 are the differences between the case or related brand value of a parameter and the average for 8 other brands. Thus, for example, line 6 is just line 1 minus line 3. The interperiod changes in the differences is shown on lines 8 and 9.



months prior to the FTC complaint; After I: 19 months following the cease-and-desist; After II: 29 subsequent months. I use two post-case subperiods to see if any case effects were long lived.

If one keeps in mind the typically large standard errors, the direction of any case effects on loyalty shares seems to be similar to those in the gasoline cases. Look first at line 1, which summarizes the experience of the case brand. Its estimated loyal-customer share increases (from zero before the case) in the immediate aftermath of the cease-and-desist (column (3)), and continues to do so thereafter (column (5)). These before/after case differences are summarized on line 4. There is no comparably large change in the average other brands' loyalty share. These shares (line 3) stay between about one-quarter and one-third. Lines 6-9 in the table focus on differences between the case brands and the control group of eight other brands. Given the relative stability of the control group's average loyalty share, lines 6-9 tell the same qualitative story as lines 1-5: the case brand was attracting a disproportionate share of first-time buyers. After the cease-and-desist, it becomes very much like a more typical brand in this respect. The same sort of increase in loyal-customer share occurs for the related brand, but not until the "post-post-case" period (compare line 9 to line 8). Any connection of

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The data permit a computation of price per pound, which is the unit used for the market shares. However, at the individual-brand level, a per-pound price can be sensitive to shifts between package size. A brand's per-pound price increases if customers shift to smaller sizes, because these sizes invariably have the higher per-pound prices. But that sort of shift should not be treated as a "real" price increase, if the customer could have bought any size at the same price as before. The increase here would just reflect the consumers' decision to buy greater convenience in the form of a smaller package. (2) It was not feasible, from the NPD data, to compute an all-gimrack average per-pound price; the mean package price is the only deflator available.

this change to the case is made implausible by this delay, and by the fact that the related brand never has the very low loyal-customer share of its namesake (compare line 6 and line 7 of column (1)).

I had concluded that effective regulation would, by extending the brand-span of search by transitors, increase the affected brand's price elasticity of demand. This prediction is addressed in columns (7)-(12) of Table 11. The thrust of these data is consistent with that prediction, and, hence, with effective regulation. Again, the relevant standard errors, while a bit smaller than for the loyalty-share parameters, are still too wide to permit this conclusion to be more than a goad to future research.

The elasticities in the table are for a brand's market share w.r.t. its relative price. They are labeled "short-run" because feedback effects of this period's change in sales on future sales via repeat purchases are ignored. For the typical brand, this elasticity is on the order of one-and-one-half to two for all periods (line 3). The case brand's elasticity moves from about one-half to about twice this average right after the cease-and-desist order, and subsequently declines only to the average. The interesting and puzzling change is for the related brand; its elasticity also rises and then falls, but the rise is sharper and the longer-run net change larger than for its brand-mate (see lines 8 and 9).

The overall thrust of Table 11, then, is the same as that of the gasoline and widget data. It is consistent with regulation which is effective for at least a while (say, one or two years), and perhaps, on a generous interpretation of the data, longer (three years or so).

Table 12 summarizes the market share and growth histories of the case brands. These are shown as pure shifts in demand; that is, adjusted for effects of price changes on actual market shares. Note first (lines 10 and 11) the absence of any superior growth for either the case or related brand prior to the FTC complaint; this seems by now to be the standard finding. For the case brand, the aftermath of the case seems to bring a temporary decline in demand (compare lines 1 and 3). The fact that the case brand has a positive After I growth rate (line 7 or 10) implies that most of the decline in its demand (line 1) occurred early in that period. Indeed, so strong is the reversal of the decline that by After II, demand for the case brand recovered almost all its lost ground, while the typical established brand (line 3) lags further behind the initial period. The data for the related brand (lines 2, 8, and 11) create the same puzzle as the loyalty data. Any effect of the FTC case seems curiously delayed; the negative After I growth rate (column (3), lines 8 and 11) and the low After II level of demand (line 2, column (6)) imply a decline in demand beginning only in the second half of the After I period.

The behavior of prices provides further insight into the temporary nature of any case-related decline in demand. While the price data in Table 12 (lines 4-6) show no unusual difference in price behavior between the three brand types in the After I period, we have available some potentially more sensitive data. It is common practice in the gimrack industry for manufacturers to offer temporary price reductions ("deals") to retailers. The NPD consumer panel reports the number of pounds bought on such deals. If the case brand's manufacturer expected any After period decline in demand to be temporary and/or confined to particular (separable) customer types, transaction and information cost economies would induce an increase in the frequency of "deals" rather than

TABLE 12

MARKET SHARES AND INTRAPERIOD GROWTH RATES, PRICE  
ADJUSTED, FOR GIMRACK BRANDS BY SUBPERIODS

Variable and Brand	<u>Before</u>		<u>After I</u>		<u>After II</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Equilibrium Market Share at Price of Before Period (Before = 100)						
1. Case		100		87		97
2. Related		100		91		74
3. Average of 8 others		100		97		84
B. Relative Price (Before = 100)						
4. Case		100		96		97
5. Related		100		99		101
6. Average of 8 others		100		95		95
C. Intraperiod Growth Rate of Market Share (%/year) Adjusted for Price Change (S.E. in parentheses)						
7. Case	1.37	(4.81)	12.39	(7.30)	-0.54	(3.03)
8. Related	5.06	(4.30)	-23.42	(9.08)	24.09	(5.24)
9. Average of 8 others	1.08	(1.96)	-1.98	(2.25)	-2.59	(1.03)
Differences from Avg. of 8 others						
10. Case (7-9)	.29	(5.19)	14.37	(7.64)	2.05	(3.20)
11. Related (8-9)	3.98	(4.73)	-21.44	(9.35)	26.68	(5.35)

NOTE: See text for definition of subperiods. All data, except lines 4-6, are adjusted for effects of prices changes as follows: Lines 1-3--the regression estimates of equation (3), which provide the data for Table 11, imply an equilibrium brand market share which is a function of the brand's estimated transition and repeat purchase probabilities, which themselves depend on the brand's relative price. The entries in columns (4) and (6) are, in index form, the brand's equilibrium market share in an After period, given the transition-repeat purchase process in that period, with the price set equal to that in the Before period. Lines 7-11 are derived by solving for "growth of demand": actual growth rate of market share = growth of demand - elasticity  $\times$  growth rate of price, where the elasticity is that shown in Table 11. The data are annualized monthly rates of change.

any across-the-board wholesale price reduction that would later have to be rescinded. To see if such an increase in deal frequency took place, I constructed from the NPD data the variable:

$$\text{DEAL ODDS} = \log \left( \frac{\text{CDEAL}}{1 - \text{CDEAL}} \right) - \log \left( \frac{\text{DEAL}}{1 - \text{DEAL}} \right) ;$$

where:

CDEAL = pounds of case brand sold on deal/total pounds sale of case brand, and

DEAL = the same ratio for the aggregate of gimracks.

I then regressed a monthly DEAL ODDS series on a series of dummy variables:

D1-6 = +1 for each of first 6 months following the complaint, 0 otherwise;

D6-12 = +1 for each of subsequent 6 months, 0 otherwise;

D2YR = +1 for each month of subsequent year, 0 otherwise;

DREST = +1 for the remainder of post-case period, 0 otherwise.

The results was (t-ratios in parentheses):

$$\text{DEAL ODDS} = \text{constant} + .894\text{D1-6} - .053\text{D6-12}$$

(4.89)      (.29)

$$- .075\text{D2YR} + .193\text{DREST}$$

(.51)      (1.63)

$$R^2 = .31, \text{SE} \times 100 = 34.5, \text{DW} = 2.03.^{37}$$

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<sup>37</sup> The regression reported is after correction for first-order serial correlation; the results are virtually identical without such a correction.

This says that there was a six-month period, right after the case was brought, of unusually heavy deal activity for the case brand.<sup>38</sup> A similar regression for the related brand also showed an increase in deal volume after the case, but the relevant coefficients were insignificant.<sup>39</sup>

In summary, the various gimrack market-share and price data seem to agree that regulation is effective, but only for a brief period. And there is no clear spillover of effects to the related brand.

(E.) Another Margarine Case and Three Juice Cases. This final set of cases also exploits proprietary data, made available by Market Research Corp. of America (MRCA). Like NPD, MRCA has a consumer panel, and I have the actual household purchase records for 1972-73 for the relevant products. The cases to be analyzed (date of decision) are:

1. Fleischmann's Margarine (complaint and cease and desist announced 1/73)
2. Hi C (complaint dismissed 9/72, announced 9/70)

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<sup>38</sup> The data imply that deal volume for the case brand in this period was double its average. When  $\log \left( \frac{\text{DEAL}}{1 - \text{DEAL}} \right)$  was entered as an additional independent variable, its coefficient was insignificantly different from zero, and the signs and magnitudes of the other coefficients were substantially the same as shown here.

<sup>39</sup> The coefficients (t ratios) for that regression (also adjusted for serial correlation) were:

$$\begin{array}{l}
 + .383D1-6 + .548D6-12 + .181D2YR \\
 (.81) \quad (1.09) \quad (.42) \\
 \\
 + .004DREST \\
 (.01)
 \end{array}$$

The large (but insignificant) coefficient of D6-12 is another symptom of the "lag" in "response" of the related brand to the case.

3. Ocean Spray Cranberry Juice (cease and desist 3/72, complaint announced 2/71)
4. Hawaiian Punch (complaint and cease and desist announced 5/73).

To conserve space and the reader's patience, I focus on customer loyalty and some related price effects of these cases, and I limit myself to summaries of differences between the case brand and averages of other brands in the same product category. The data yield enormous degrees of freedom (over 10,000 individual purchases for each case were analyzed) and allow us to look at actual household decisions.

To make comparisons with previous customer loyalty data, I found it useful to modify the Telser model in equations (2) and (3). The straightforward application of that model would imply a regression like

$$(2)' \quad y_1 = t(1 - y_0) + ry_0 = t + (r - t)y_0$$

where  $y_i$  = dummy variable = +1 if a household buys brand  $y$  now ( $i = 1$ ) or on its previous purchase ( $i = 0$ ); zero otherwise.

Thus, if  $y_0 = 0$ , the household is a potential switcher and the parameter  $t$  = probability that a household who bought non- $y$  switches to  $y$ . Similarly  $r$  = probability that a household who bought  $y$  ( $y_0 = 1$ ) stays with this brand. However, a preliminary examination of the data indicated that households frequently interspersed purchases of one brand with purchases of others. So, if we stayed with a one-period model where the "period" was the interval between transactions, something like (2)' might not yield a sensible estimate of customer loyalty: For a buyer who regularly alternated purchases of  $y$  and non- $y$ ,  $r = 0$  in (2)' even though this buyer is obviously quite loyal to  $y$ . I found, after some preliminary work with distributed lags, that a reasonable solution to this problem would be to simply add a second lag ( $y_{-1}$ ) to (2)'.

The model underlying this procedure distinguishes "permanent" from "temporary" brand switching and implies that the sum of the coefficients (intercept plus coefficients of the two lagged terms) is the probability of permanent loyalty to the brand.<sup>40</sup>

The regressions actually run had the form

$$(3)' \quad y_1 = a + b_1 p_i + c y_0 + d y_{-1} ,$$

where

$$p_i = \text{price of brand } y \text{ or non-}y ,$$

and  $\hat{a} + \sum \hat{b}_i \bar{p}_i + \hat{c} + \hat{d}$  was the estimate of the fraction of a brand's sales to permanently loyal buyers = 1 - fraction of sales to "true first-time" buyers.

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<sup>40</sup>The model is:  
Let

- (a)  $r = 1 - p - a$   
 $p$  = probability of a "permanent" switch to non- $y$   
 $a$  = probability that a purchase of  $y$  will be followed by a non- $y$  next time, but by a return to  $y$  the time after next

Then express the first r.h.s. term in (2)'

$$(b) \quad t(1 - y_0) = a y_{-1} + t'k(1 - y_{-1}),$$

where  $k$  = probability that a non- $y$  remains loyal for one period  
 $t'$  = probability that this non- $y$  migrates to  $y$  next period.

This says that the transitors to  $y$  come from two sources: those who switched to non- $y$  temporarily two periods ago and return to  $y$  now ( $a y_{-1}$ ), those who were loyal buyers of non- $y$ , but move to  $y$  now ( $t'k(1 - y_{-1})$ ).

Finally, rewrite (2)' to incorporate (a) and (b) as

$$(c) \quad y_1 = [a y_{-1} + t'k(1 - y_{-1})] + (1 - p - a)y_0 \\ = t'k + (1 - p - a)y_0 + (a - t'k)y_{-1}$$

and note that the sum of coefficients is just  $(1 - p)$ , or the probability of "permanent" loyalty. In the preliminary work, it was found that there was often a large difference between the  $r$  as estimated from a single-lag regression like (2)' and the  $(1 - p)$  from a two-lag regression, but that adding more than 2 lags did not substantially change the sum of coefficients.



This regression was run for the case brand and for a control group of other national brands for various periods before and after the FTC decisions.<sup>41</sup>

The results are summarized in Table 13, which describes how the difference between the case and other brands loyalty shares change from the pre-decision period. These are the same sort of data as in lines 8-9 of Table 11, but we can observe shorter period movements; each of the After periods is about a quarter. Effective regulation implies that relative customer loyalty should rise after a cease and desist. This happens for two of the three cease and desist orders covered by the table (Fleischmann is the exception). For one of these two cases (Ocean Spray), the rise in the loyal customer share is delayed by a quarter, but persists at least for a year. For the other (Hawaiian Punch) I had about 1½ years of pre-case data, so I bifurcated that period and discovered (see Before II) a rise in the loyal customer share in the 9 months prior to the cease and desist. No formal complaint was issued in this case until the day

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<sup>41</sup>The regressions were estimated by OLS, instead of the more appropriate logit method, to save computation costs. Two price variables were included in each regression:  $P_y$  = price of  $y$  facing the household,  $P_x$  = price of non- $y$ . These were constructed as follows (all prices were divided by their 1972-73 averages):

- (a)  $P_y$  (1) if  $y = 1$  (i.e., the household is observed to buy  $y$ ),

$P_y$  = the per unit (see below) price actually paid, as reported by the household.

- (2) if  $y = 0$ ,  $P_y$  = average of prices paid by all sample households for  $y$  in the same month.

- (b)  $P_x$  (1) if  $y = 0$  and the household buys one of the control group brands,  $P_x$  = price paid for this brand.

- (2) if  $y = 0$  and the household buys one of the myriad of non-control group brands

$P_x$  = average price paid for all sampled brands (control + case) by all sample households in the same month; if  $y = 1$ , the average is over control brands only.

For margarine, the price is per pound, since this is the standard package size. For juices, there was a wide range of package sizes. For each brand, a regression of price paid on package size (ounces) was run for each brand across all purchases over the two year-period. The ratio of actual to predicted (from the regression) price was then used.

TABLE 13

LOYAL CUSTOMER SHARES, DIFFERENCE BETWEEN CASE AND  
OTHER BRANDS, CHANGE FROM PRE-DECISION PERIOD,  
FOUR FTC CASES 1972-73

SUBPERIOD: FROM BEFORE TO:	CHANGE IN DIFFERENCES/(S.E. OF CHANGE) FOR			
	FLEISCHMANN'S MARGARINE	OCEAN SPRAY	HI C	HAWAIIAN PUNCH
BEFORE II*				+8.5% (2.3)
AFTER I	-5.6% (2.2)	-1.1% (4.4)	+7.6% (2.3)	+7.2 (3.0)
AFTER II	NA	+15.1 (4.7)	+4.4 (2.7)	-0.6 (4.2)
AFTER III	NA	+18.5 (4.5)	-4.4 (3.1)	
AFTER IV	-1.3 (2.7)	+5.8 (4.7)	-1.8 (3.4)	
AFTER V		+16.9 (4.4)	-7.4 (3.3)	

See text for description of data and estimates. There are 5 "other" brands in the control group for Fleischmann's and 7 "other" for the three juice cases. The Before periods, against which the changes are measured are

Fleischmann's: 1-12/72  
Ocean Spray: 1-2/72  
Hi C: 1-9/72  
Hawaiian Punch: 1/72-5/73.\*

\*The period is further divided I = 1-8/72, II = 9/72-5/73.

The After periods, each about a quarter in length, begin in the following months:

Fleischmann's: I = 1/73, IV = 10/73 (data from 5-9/73 are missing).  
Ocean Spray: I = 3/72, II = 7/72, III = 11/72, IV = 3/73,  
V = 7/73.  
Hi C: I = 10/72, II = 1/73, III = 4/73, IV = 7/73,  
V = 10/73.  
Hawaiian Punch: I = 6/73, II = 10/73.

the cease and desist was entered, so that there was a prior period of negotiation between the advertiser and the FTC staff. In such cases, the advertiser will sometimes modify the ad campaign as part of the negotiation process. I have not determined whether this occurred here, but such modification would explain the otherwise peculiar pre-decision rise in the loyal customer share. The fourth case in the table (Hi C) was dismissed, so we expect, if anything, a fall in the loyal customer share. This in fact occurs about a year after the dismissal, but the initial impact (After I) is a rise in customer loyalty which is then gradually reversed.

I had hoped to use these data to test further the implication of a rise in the elasticity of demand following a cease and desist. However, for the three juice cases, I was unable to estimate sensible elasticities, perhaps because of the difficulty in estimating prices.<sup>42</sup>

The margarine data did yield credible price elasticities, and these tend to confirm the previous evidence of a post-cessate and desist rise in elasticity: In the year before, the absolute value of the case brand's price elasticity (standard error) was 0.87 (.75) less than the other-brand average. In the first quarter after the decision, its elasticity is 3.28 (1.47) more than the others. In the fourth quarter after the cease and desist (the intervening data were missing), this difference widens to 4.09 (2.14). So there seems to be a substantial and significant rise in relative price elasticity, about +5, from the pre-case period that lasts for at least a year. This strong result is

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<sup>42</sup> See n. 41 supra on the procedure I had to follow to adjust for the variety of package sizes. The elasticity estimates jumped around both sides of zero, with about as many positive as negative estimates. Since the data in Table 13 came from regressions with price terms included, I ran others deleting prices, but found no important changes in the results reported in Table 13.

perhaps surprising in light of the evidence in Table 13 that customer loyalty was unaffected by the cease and desist in this case.

Finally, Table 14 summarizes data on the effect of the cases on the frequency of purchases on "deal." The entries in the table are coefficients of dummy variables, converted to percentages at the sample means, in a regression with the frequency of brand purchases on deal in a month as the dependent variable<sup>43</sup> and the average frequency for other brands as another independent variable. The dummies are +1 for the first, second and third couplet of months following the cease and desist, so their coefficients measure deviations from the average "propensity to deal" over the two years. Except for the exonerated brand (Hi C), the results strongly confirm the finding for gimracks--a sharp temporary increase in deal frequency after the case. Thus, on the whole, there are few surprises in these household data. They mainly tend to confirm the patterns found in the more aggregated data.

#### IV. Summary and Conclusions

The disparate data seem to agree on one major point: the "toothless tiger" image of FTC advertising regulation is wrong. Visible and sometimes very substantial effects of the regulation show up in the product market, the advertising market, and, especially, the capital markets. As with the original debacle in the Garden of Eden, the seemingly free bite of the forbidden fruit turns out to be very costly. Whether these costs, borne by owners of firms subject to the regulation, are offset by gains to consumers of the affected products is less clear. I argued that successful false advertising ought to

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<sup>43</sup>For two of the brands zero or near-zero values of this variable made use of the "log odds" form of the regression unfeasible.

TABLE 14  
 PERCENT DEVIATION OF DEAL FREQUENCY FOR  
 CASE BRANDS, THREE BI-MONTHLY PERIODS  
 AFTER CEASE AND DESIST

CASE	PERCENT DEVIATION (t-ratio) IN		
	FIRST 2 MOS.	SECOND 2 MOS.	THIRD 2 MOS.
Fleischmann's	63.2% (3.13)	31.6 (1.91)	NA
Ocean Spray	81.3 (2.39)	56.3 (1.58)	6.3 (.11)
Hi C	25.6 (.98)	-4.7 (.15)	-20.9 (.85)
Hawaiian Punch	175.0 (3.33)	-31.3 (.66)	-37.5 (.70)

See text for explanation. Source: MRCA

lure first-time buyers rather than increase purchases of loyal buyers. While the data are not entirely clear on this point, there appears to be some tendency for brands involved in cases to attract an above-average share of first-time buyers before an FTC case. It is more clear that the ability to attract these buyers is reduced if the case is lost. But that sort of evidence cannot by itself be conclusive about the normative issue. It could just as easily be read as a restraint on an especially effective competitor.

If one simply assumes that ads eliminated by FTC regulation were costly to consumers, any beneficial effects of regulation would depend importantly on their permanence. This is due in part to the inevitably temporary nature of the gains from any particular act of deception, at least for frequently purchased experience goods. If the reduction of first-time buying after an FTC case is temporary, we should conclude either that the product did have unusual attractions for such buyers which the seller was able to convey in a later, truthful ad, in which case the earlier ad ought not have been eliminated, or that evading regulation with new false ads is easy. Our finding that the decline in first-time buying after an FTC case is in fact most often temporary is not, therefore, encouraging.

There are some further puzzles in our results. The temporary reduction in first-time buying is not reliably accompanied by a decline in market share. This implies that old customers increase their purchases when the new ones fail to show. Since we usually have no price data, this seemingly strange behavior of old customers may only mean that the firm has reduced its price in response to a decrease in demand. Post-case price behavior is a clear missing link in our data. Without them the substantial decline in advertising expenditure which we find raises another puzzle. This decline persists after

the temporary product-market effects have dissipated. On its face, this seems to mean that the FTC is actually doing a firm a favor by attacking it: the firm gets the same sales in the long-run with lower advertising expenses. However, if it retains sales only as a result of a price decrease in the face of reduced demand, the reduced advertising would be intelligible.

Even if all these puzzles are resolved, one large one remains. This is the size of the loss in the capital value of firms attacked by the FTC. This loss seems even larger than the entire advertising capital of the brand, not just the part which may be due to false advertising. That finding would surely appear to imply more durable product-market effects and, perhaps, more conservative advertising scripts than we find.