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Strategic Responses to Regulatory Threat in the Credit Card Market*

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Abstract

In November 1991, federal lawmakers threatened to place a binding cap on credit card interest rates. I find that credit card rates declined following the regulatory threat, more so for larger and more politically visible credit card issuers. A set of stock market event studies reveals that interest rate cuts announced after the threat led to positive abnormal returns, both for announcing issuers and their rivals. This pattern does not exist for similar rate cuts made outside the period of regulatory threat. The results suggest that firms may experience private benefits to price-cutting when doing so mitigates regulatory threat, and spillover benefits when another firm cuts prices in order to ease regulatory threat.

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I. Introduction

In November 1991, President Bush and members of Congress threatened to place a binding cap on credit card interest rates unless they saw significant rate cuts by major credit card issuers. In this paper, I document the events surrounding this episode of regulatory threat, focusing both on the threat itself and subsequent rate-cutting by credit card issuers. I examine both the price and stock market effects of these events, in order to assess the empirical relevance of models in which firms may react strategically to regulatory threat.

The broad issue at hand is an investigation of the relationship between firm conduct and the threat of government regulation. Most economic models of firm conduct assume away the influence of regulatory threat, or assume that it is exogenous. This is too restrictive if we think that regulatory threat might constrain prices, particularly when there is a direct link between prices and the probability of regulation.[1]

Because quantifying the level of threat is so difficult, most empirical tests of the regulatory threat hypothesis exploit cross-sectional differences in exposure to or influence on regulatory threat. During an episode of threat, these differences should be correlated with movements in prices. For example, Erfle and McMillan [1990] posit that large domestic firms have more impact on the probability of regulation than smaller or foreign firms, and find that domestic firms restrained prices more during the 1979 oil crisis (during which, they argue, there was regulatory threat). They also examine two different

[1] Theoretical work by Glazer and McMillan [1992] and Erfle and McMillan [1990] formalizes the intuitive proposition that when regulators tie the probability of regulation to prices, firms will constrain their prices to mitigate regulatory threat. This line of work derives broadly from the political economy literature by Becker [1983], Stigler [1971], and Peltzman [1973], among others.

types of oil, one of which was more "visible" than the other to American consumers, and find that producers restrained price increases to a larger degree on the more visible type.

More recently, Ellison and Wolfram [2000] examine pharmaceutical prices during an episode of regulatory threat. Ellison and Wolfram also use cross-sectional patterns to test the regulatory threat hypothesis, but they focus on differences in the cost of regulation, were it to be imposed. They suggest that firms with longer-lived patents are more vulnerable to the imposition of regulation because they would lose monopoly rents. The results show that these firms indeed constrained their prices more than those with little patent protection during public discussion of health care reform (which proposed regulation of pharmaceutical prices) in the early 1990s. They also find that firms with high recent PAC contributions constrained their prices more. Maxwell, Lyon, and Hackett [1999] take a similar approach in examining environmental "self-regulation," showing that firms in states with higher membership in conservation groups exhibit higher levels of self-regulation. Other more anecdotal evidence in support of the regulatory threat hypothesis can be found in Glazer and McMillan [1992] and Olmstead and Rhode [1985].

In a series of tests that evoke the work discussed above, I conduct in the first part of this paper an examination of credit card prices. An event study of national average credit card interest rates shows that rates fell significantly following the threat. The cross-sectional pattern of rate changes during this period is also consistent with the regulatory threat hypothesis: larger, more politically visible issuers cut their rates by more than smaller issuers. However, because there are other plausible explanations for falling

credit card rates during the 1990s, it is difficult to draw a firm conclusion regarding the precise impact of regulatory threat on rates.

The main body of the paper extends the literature on regulatory threat by using stock market event studies to examine both the threat itself and subsequent rate-cutting announcements by the largest credit card issuers.[2] Observing stock market effects allows me to test a wider set of implications of models of regulatory threat. To conduct the analysis, I construct a portfolio of the largest (and most politically visible) credit card issuers.

I test first the straightforward prediction that regulatory threat should lead to negative cumulative abnormal returns. Previous studies of regulatory threat have been unable to identify this effect, primarily because the incidence of regulatory threat has rarely been so discrete. The results of the event study show that over a ten-day event window, the Bush/Senate regulatory threat led to a cumulative negative return of over ten percent for the portfolio of credit card issuers.

A second prediction of the regulatory threat model is that for politically visible issuers, price changes should affect firm value by changing the probability of regulation. To test this prediction, I use newspaper accounts of legislative activity and card issuer behavior to identify the period during which lawmakers tied the probability of regulation to the level of credit card interest rates. I then construct a list of rate cuts announced during the threat period by the set of politically visible credit card issuers. Because rate cuts also affect returns in ways unrelated to regulatory threat, I construct a control group of announcements for the ten-year period surrounding the episode of threat. This permits

a comparison of the effects of rate-cutting announcements during the episode of the threat to the effects of rate-cutting announcements when regulatory threat was weak or nonexistent. Under the regulatory threat hypothesis, rate-cutting announcements should be associated with more positive returns during episodes of regulatory threat.

The results confirm the regulatory threat hypothesis. In the absence of threat, firms announcing rate cuts experience only small positive abnormal returns. During the period of regulatory threat, however, firms announcing rate cuts experience large positive abnormal returns. This suggests that there are private benefits to rate-cutting under regulatory threat.

The final empirical test examines the spillover effects that price-cutting may have in a multi-firm setting when a price change by any one firm affects the probability of regulation faced by all firms. In such a setting, a price cut by one firm may yield positive returns for all firms because it reduces the probability of regulation. To test this prediction, I examine the returns of the announcing issuers' rivals under the two regimes. During the control period, rivals experienced weakly negative abnormal returns following rate-cutting announcements. During the episode of regulatory threat, rivals experienced large, statistically significant, and positive abnormal returns following rate-cutting announcements by their competitors. The results confirm the existence of spillover effects.

As corroborative evidence, I regress the issuer-level abnormal returns following rate-cutting announcements on a set of event- and issuer-specific explanatory variables. These results are also consistent with the regulatory threat hypothesis. Absent threat,

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[2] Event studies have been used extensively to analyze actual regulation. Rose [1985] and Prager [1989]

larger and more widely applicable rate cuts lead to more negative returns for rivals of the announcing issuer. During threat, larger and more widely applicable rate cuts lead to more positive returns for rivals. Additionally, rivals' gains following these rate-cutting announcement are directly proportional to the losses they sustained when the threat was imposed.

Taken together, these results make a convincing case that regulatory threat has affected credit card issuers, and that issuers have responded strategically to the threat. In the conclusion of the paper, I highlight the implications of these findings in the credit card market and other markets, note some limitations of the analysis, and offer suggestions for future work.

II. Theories of Regulatory Threat

Theories of regulatory threat typically analyze a firm facing a stochastic threat of regulation. This is the approach taken by Erfle and McMillan [1990], who model firm value as an average of regulated and unregulated profits, weighted by the probability of regulation.

The probability of regulation may be exogenous or endogenous. While exogenous regulatory threat reduces the value of the firm as long as its regulated value is below its unregulated value, it does not cause the firm to adjust its price. Endogenous regulatory threat includes both a positive level of regulatory threat and a positive relationship between prices and the probability of regulation.[3] Under endogenous regulatory threat the firm will constrain its price to stave off regulation.

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are examples of this line of work.

[3] A deterministic threshold that induced regulation would yield a limit pricing equilibrium, in which firms constrained prices just enough to avoid regulation.

This simple formulation of regulatory threat contains two predictions for empirical work. The first is that firm values should fall when regulatory threat is imposed; this will be true under exogenous or endogenous regulatory threat. This prediction, while straightforward, has not been tested in previous work. The second prediction, which will only be true under endogenous regulatory threat, is that firms should cut prices to stave off regulation.[4] A corollary of this prediction is that firms with a greater exposure to or impact on the probability of regulation will cut their prices by more than those with a lesser exposure to or impact on the probability of regulation. These latter predictions form the basis for the work by Erfle and McMillan [1992], Ellison and Wolfram [2000], and Maxwell et al. [1999].

Price Changes and Firm Value

In an oligopolistic environment without endogenous regulatory threat, we would expect that price changes would directly affect profits, for firms initiating the price changes and their rivals. These changes in profits would no doubt vary based on the reason for the price change, but *a priori* it is difficult to determine the sign of these direct profit effects.

Under endogenous regulatory threat, there will be an additional effect of any price change – it will affect the industry-wide probability of regulation. This implies that price changes under regulatory threat should affect firm value differently than equivalent price changes absent regulatory threat. Furthermore, if the threat of regulation is industry-wide,

[4] Glazer and McMillan [1990] note that when an increase in the probability of regulation is accompanied by a reduction in the marginal effect of price on the probability of regulation, firms may raise prices. Here I assume that an increase in the probability of regulation is associated with an increase in the marginal effect of price on the probability of regulation. (Media accounts of the credit card market suggest that outside of the period I identify below, there was essentially no relationship between credit card rates and the probability of regulation).

this difference should be apparent not only for the firm announcing the price but for its rivals as well.

This provides two final tests of the regulatory threat hypothesis. One such test is that under regulatory threat, firms announcing price cuts should experience returns that are *more positive* than returns absent regulatory threat.[5] A second and perhaps more important test is that in a multi-firm setting, the actions of one politically visible issuer may spill over to all issuers in the market. Thus under endogenous regulatory threat, the rivals of an issuer announcing a price cut will experience returns *more positive* than those absent regulatory threat.

There are two important points to make about this empirical approach. First, while the empirical work below makes every attempt to compare the effects of "equivalent" price cuts made under threat and absent threat, this is difficult. Price cuts made under threat, and indeed motivated by regulatory threat, will be different *per se* from price cuts made absent threat – certainly in their motivation, and possibly in their effects on firm value.[6] This means that while rejecting equality of returns across the two regimes is certainly evidence in favor of the regulatory threat hypothesis, we should be cautious in interpreting the magnitudes of the observed differences in returns.

A second point is that while the empirical predictions discussed above relate to firm value, the empirical work below uses stock market returns to infer changes in firm value. While the regulatory threat hypothesis has fairly clear implications for changes in

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[5] As stated above, this does not say anything about the *absolute level* of returns following rate-cutting announcements.

[6] The difficulty stems (as an example) from the fact that if price changes affect the probability of regulation, then firms' marginal revenue and best response functions will also include the probability of

firm value following price cuts, stock market returns following price-cutting announcements will reflect changes in *expectations* about firm value. If investors fully and rationally anticipate price-cutting following the initial imposition of regulatory threat, subsequent price cuts might have no effect on the stock market valuation of firms. Thus, we will only observe a link between the above pattern of changes in value and stock market returns if investors only partially update their expectations following each announcement or a price cut.

III. Regulatory Threat in the Credit Card Market

Although it has been virtually free of formal regulation since the early 1980s, the credit card industry has faced scrutiny and criticism from Congress, academics, and consumer groups.[7] Congressional attention has waxed and waned, although certain members of Congress have devoted consistent efforts toward correcting what they see as a "failure of competition" in the market.[8] An influential article by Ausubel [1991] identified several features of credit card pricing that seemed to defy theories of pricing in competitive markets. Consumer advocacy groups also have periodically accused credit card issuers of exploiting consumers.

These pressures have induced members of Congress to propose various forms of regulation. The least stringent of these are consumer protection bills such as the Credit Card Disclosure Act of 1988; these bills typically attempt to improve the quality of

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regulation. Thus, even identical price cuts made under or absent threat will induce different responses among the rivals of the firm cutting its price, and will lead to different *ex post* equilibria.

[7] State-level usury ceilings existed in most states in the early 1980s, but the deregulation of interstate banking in 1982 allowed issuers to incorporate their credit card operations in ceiling-free states such as Delaware and South Dakota.

information available to consumers.[9] More stringent bills proposing explicit rate caps on credit card interest rates have been introduced at various times as well.[10] It seems safe to say that until 1991, the industry viewed these bills as unlikely to pass; media and congressional discussions of the rate cap proposals typically acknowledged that they had little support.[11]

In 1991 a much stronger threat emerged. The gap between credit card rates and short-term market rates reached an all-time high, while rates for the largest credit card issuers remained nearly identical to their 1982 levels. Rep. Charles Schumer, who during 1985-1986 had opposed a rate cap while supporting the disclosure bill, reversed his position and began advocating a cap of 800 basis points above the 6-month Treasury bill rate. Ausubel's article was influential enough to gain mention in the national media, further provoking anti-credit card sentiment.

On November 11, 1991, President Bush commented at a state dinner that he would "like to see credit card rates down." One business day later, the Senate voted 74-19 to pass a bill capping credit card interest rates at 14%, well below the 19.8% rate

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8 The term is taken from Ausubel [1991]. Allegations against the industry have been wide-ranging. Reps. Charles Schumer (D-NY) and Alfonse D'Amato (R-NY) have at various times accused the largest issuers of explicit or tacit collusion. They also mention periodically the need for consumer protection laws.

9 The support for disclosure requirements arose from the idea that imperfect price information allowed card issuers to earn supercompetitive rents. Shaffer [1998] finds that the passage of disclosure requirements had no effect on pricing, which argues against the imperfect price information theory.

10 *LEXIS/NEXIS' Congressional Universe* files show that there were five such bills introduced in 1986 and six introduced in 1987. However, these bills all died in committee, most with only one sponsor.

11 In September 1986, an American Banker articled discussing a rate cap proposal had this to say: "Efforts to put a cap on credit card interest rates suffered a serious defeat when the House Banking Committee's consumer affairs subcommittee -- a panel that traditionally champions consumer protection -- defeated a bill by subcommittee chairman Frank Annunzio, D-Ill., to cap rates at eight percentage points above the one-year Treasury bill rate…Credit card rate limits are not even being considered in the Senate." -- *The American Banker*, September 22, 1986.

A later article read: "Bankers concerned about moves to impose a federal ceiling on credit card interest rates heard from members of Congress that rate caps likely will be rejected in favor of expanded disclosure requirements." -- *The American Banker*, April 23, 1987.

charged by the largest credit card issuers. There had been little discussion of the rate cap prior to its passage.[12] Indeed, the cap was an amendment that had been attached to a more general banking bill at the last minute.

The Bush/Senate actions sparked a furious debate in the national media and on Capitol Hill. The banking industry rapidly began lobbying against House passage of the interest rate cap. By November 19th the efforts of the banking lobby and a White House retraction of Bush's comments had combined to make House passage of any rate cap unlikely. Nonetheless, the rate cap remained a serious issue. Many members of Congress supported legislation that explicitly tied future congressional activity to rates, and introduced proposals calling for studies of the market in order to determine its competitiveness. Most proposals set a 12- or 18-month deadline, following which the rate cap issue would be revisited. Initial support for these proposals was strong.

Over the next few months, the threat of regulation was mentioned periodically in major newspapers and banking newsletters; the consensus seemed to be that it was abating. Congress did commission a General Accounting Office report on the competitiveness of the credit card market, but did not explicitly tie the imposition of a rate cap to the outcome of the study. On April 27, 1992, an article entitled "Threat of Credit Card Cap Legislation Easing" noted that the probability of regulation was small. This period seems to mark the end of the threat.

IV. Regulatory Threat and Interest Rates

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[12] As an indication of how unexpected Bush's comments were, one can consider the following: A *NEXIS* major newspaper search of "Bush and credit card" yields no matches between November 1, 1991 and November 11. It yields 140 matches between November 12 and the end of the month.

In this section I present two complementary pieces of evidence regarding the influence of regulatory threat on prices. The first is an examination of national average interest rates following the incidence of regulatory threat. The second is a cross-sectional examination of the rate-cutting of issuers with different political visibility.

Average Interest Rates Following the Imposition of Threat

Figure 1 shows the national average credit card rate and a benchmark rate, the 6-month Treasury Bill rate, from 1972-1999.[13] One notices two breaks in the series. The first occurs between 1980 and 1982, a period during which the average rate rose from under 18% to nearly 19%. This increase reflects both the elimination of state-level usury ceilings, and the effects of deregulation that allowed credit card issuers to incorporate in usury ceiling-free states and export credit card rates across state lines. The second break occurs in late 1991 and early 1992, during the episode of regulatory threat.

In order to test the proposition that credit card rates fell significantly following the episode of threat, I conduct a series of event studies using the quarterly change in the average interest rate series as the dependent variable (for a description of the specifications used in the event studies, and the method of calculating cumulative abnormal returns, see Appendix A). The results of these event studies are shown in Table 1. The columns show results for three different estimation window specifications, while the rows show results for various event windows. One can see that credit card rates fell sharply and significantly in the years following the threat. Within one year, rates had fallen by over fifty basis points percent, and within three years they had fallen by well

over two hundred basis points. These changes are particularly substantive given the stickiness of rates prior to 1991.

Cross-Sectional Differences in Rate-Cutting Behavior

As noted by Erfle and McMillan [1990], Ellison and Wolfram [2000], and Maxwell et al. [1999], one can use cross-sectional differences in exposure to or influence on regulatory threat to test the hypothesis. In the credit card market, the primary such difference is that only the largest, most politically visible issuers would realistically affect the probability of regulation.[14] This suggests that during the episode of threat, rate-cutting would be more pronounced for the largest issuers than for smaller issuers.

I test this prediction using a panel of year-end data for nearly 200 credit card issuers from 1989-1994. The issuers vary greatly in size and presumed effect on regulatory threat. The data are compiled from the *Card Industry Directory*, an annual trade publication that lists a variety of data for the largest credit card issuers. For each issuer/year observation, I construct *VISIBLE*, a dummy variable equal to one if the issuer is politically visible. I define as visible the largest 20 credit card issuers in 1991.[15]

The episode of regulatory threat extended from November 1991 to April 1992, and would therefore affect observations at year-end 1991, and also year-end 1992. Under the regulatory threat hypothesis, politically visible issuers would cut rates more than

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[13] Ausubel [1991] notes that this benchmark rate tracks credit card issuers' cost of funds fairly closely.

[14] Comments by members of Congress during the episode of regulatory threat focused almost exclusively on the largest issuers. The sentiment seemed to be that if the largest issuers could be persuaded to cut their rates, smaller issuers would follow.

[15] The results are robust to alternate size cutoffs such as ten or fifteen. I use a discrete measure of visibility rather than a continuous measure (such as market share) because medium-sized issuers attract no more attention from members of Congress than do issuers one-hundredth their size.

smaller issuers during these two years.[16] The regressions therefore specify the annual change in each issuer's interest rate as a function of a set of explanatory variables, which includes *VISIBLE* interacted with a set of annual dummy variables. Under the regulatory threat hypothesis, the coefficient on the interaction between *VISIBLE* and the 1991 and 1992 dummy variables should be negative. The set of explanatory variables also includes fixed year effects; dummy variables for issuer rate type and charter type; and the change in the issuer's default rate. [17] The dummy variables for issuer rate type and charter type are interacted with the year effects, so that they may vary by year.[18]

Because the default rate may be endogenous, I consider two specifications. The first uses the issuer's listed interest rate as the dependent variable:

$$\Delta RATE_{it} = \alpha_t + \beta \cdot VISIBLE_{it} \cdot YEAR_t + \delta \cdot \Delta DEFAULT_{it} + \gamma \cdot RATETYPE_{it} \cdot YEAR_t + \omega \cdot CHARTER_i \cdot YEAR_t + \varepsilon_{it}$$

The second set of regressions uses the issuer's default-adjusted interest rate as the dependent variable and drops the change in default:[19]

$$\Delta RATEDEF_{it} = \alpha_t + \beta \cdot VISIBLE_{it} \cdot YEAR_t + \gamma \cdot RATETYPE_{it} \cdot YEAR_t + \omega \cdot CHARTER_i \cdot YEAR_t + \varepsilon_{it}$$

Table 2 shows results of these regressions.[20] The 1992 coefficient on *VISIBLE* is negative in every column, and significant in three of the four columns. The coefficients

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[16] It is possible that smaller issuers would also cut their rates, in response to the rate cuts by larger issuers. The movements need not be equal, however; there are persistent and significant differences in rates/margins between larger nationally marketed and smaller regionally marketed issuers. These differences might be due to product differentiation conferred by product bundling at the regional level, or to consumer switching costs.

[17] Rate type is either fixed or variable, and charter type is either credit union or non-credit union. Issuers with variable rates and credit unions have been found to charge lower rates than issuers with fixed rates or those who are not credit unions. See Stango [2000] for a closer examination of these issues.

[18] A set of fixed issuer effects is not statistically significant. It is likely that first-differencing the data eliminates any issuer-specific patterns in rate changes.

suggest that in the time period surrounding the episode of regulatory threat, visible issuers may have reduced their rates by more than 100 basis points relative to less visible issuers. The 1991 and 1993 coefficients are also negative, although they generally are not statistically significant. Based on these results, it seems generally true that visible issuers cut their rates by more than less visible issuers for the period 1991-1993. One can also note from the coefficients on the year dummies that 1991-93 was a period of falling rates for less visible issuers. The statistically significant rate cuts for these issuers generally continued into 1993, suggesting that their rate-cutting lagged that of the visible issuers.

Some Remarks on the Price Evidence

While the above evidence is suggestive, before moving on it is worthwhile to discuss some alternative explanations for the changes in pricing that occurred in the early 1990s.

First, as credit scoring technology became more sophisticated and cheaper, issuers increasingly began to offer different rates to different consumers, and also engaged in price discrimination by offering low introductory "teaser rates" to potential new customers.[21] While the price effects of price discrimination are typically ambiguous, it is possible that it led in this instance to lower average rates. It is even possible that these changes might have been more relevant for larger issuers.[22]

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[19] The default-adjusted interest rate subtracts annual dollars of default per $100 of outstanding balances from the interest rate (which measures annual dollars of revenue per $100 of outstanding balances).

[20] The table suppresses the coefficients on the rate and charter type variables, as they are of little interest.

[21] The practice of offering different rates to different groups of consumers is not price discrimination *per se*, as much of the motivation for these differences derives from differences in default risk. However, differences in creditworthiness, and the existence of consumer switching costs (for which "teaser rates" are *prima facie* evidence) lead to incentives to engage in price discrimination based on differences in cross-elasticities of demand. Overall it is difficult to say how much intra-firm price dispersion is cost-based, and how much is due to price discrimination.

[22] Larger issuers are more likely to offer teaser rates than smaller issuers. They are also more likely to offer different rates to different customers.

The second watershed event of the early 1990s was the entry by AT&T and other large nonbank credit card issuers. These issuers launched aggressive marketing campaigns, engaged in more intense price competition than had ever been seen in the market, and in many cases had an installed base in another market from which to draw customers.[23] The consensus in trade and media publications at the time was that these actions were a serious threat to industry profitability. It is certainly plausible that the competitive pressure introduced by nonbank entry would have caused rates to fall. It might also explain why rates fell by more for larger issuers, because the nonbank entrants competed more directly with that segment of the market.

In concert, these other factors certainly put downward pressure on credit card rates. They could certainly explain a significant share of the downward movement in rates during the episode of threat. They may also be amore plausible explanation for the continued downward trend in rates after the threat had dissipated. Thus, while the evidence presented above is certainly consistent with the regulatory threat hypothesis, it would be inappropriate to ascribe the entire fall in rates to the imposition of regulatory threat.

That having been said, there is substantial anecdotal evidence from major media that the cuts in the six months following the announcement were responses to the threat of regulation. Most articles mentioned the threat directly (although issuers rarely admitted that they were cutting rates in response to regulatory threat), and *The American*

[23] This price competition took the form of AT&T's "no fee for life offer" to charter members. Both AT&T and GM also offered rebate plans on purchases.

Banker noted that more that 25% of issuers were prompted to seriously consider cutting rates shortly after the episode of threat.[24]

In sum, the interest rate evidence is broadly consistent with the regulatory threat hypothesis, but separating the effects of regulatory threat from these other influences requires cleaner tests. With this in mind, we turn to the stock market evidence.

V. Regulatory Threat, Rate-Cutting, and Returns

In this section I outline the event study approach used to measure changes in firm value. I then estimate the effects of the imposition of regulatory threat – the Bush statement and Senate vote - on abnormal returns for a set of the largest credit card issuers. Finally, I identify the set of rate-cutting announcements both within and outside the episode of regulatory threat, and compare the abnormal returns surrounding each set of announcements. The baseline is identical to that used in the cross-sectional regressions: the twenty largest credit card issuers in the country at year-end 1991. Due to missing data, not all of these issuers enter the regressions below. The Data Appendix describes this issue in more detail.

Issuer Returns Following the Threat

In this section and those that follow, I use event studies to estimate stock market returns for the portfolio of credit card issuers. An event study uses stock market prices to measure changes in firm value surrounding an event date, under the assumption that efficient asset markets will immediately incorporate new information revealed on that date. Mackinlay [1997] provides a survey of event study techniques and applications. Event studies commonly use the market model to specify the equation:

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[24] "Ruckus Spurring Many Banks to Weigh a Cut in Rates," *The American Banker*, December 6, 1991.

$$R_{it} = \alpha_i + \beta_i \cdot R_{mt} + \sum_s \delta_{is} \cdot D_{st} + \varepsilon_{it}$$

where R_{it} = the return on shares in issuer i at time t

R_{mt} = the return on the CRSP equally-weighted portfolio at time t

δ_{is} = the abnormal return for issuer i from event s

D_{st} = a dummy variable equal to one during event s

ε_{it} = an error term

This equation yields estimates of the abnormal return during each event period. Here, I use daily return data, and choose as the event date November 12,1991 – the first business day following Bush's comments.[25] I use the window [t-270, t-21] to estimate the model, allowing the estimation window to end twenty days before the event date to mitigate the possibility that pre-event information changes the model parameters.[26] I examine cumulative abnormal returns (CARs) over two event windows: the ten-day period preceding Bush's comments, and the eleven-day period following them. I estimate separate equations for each issuer.[27]

One difficulty with using the market model to analyze the imposition of regulatory threat is that during the days immediately following the Bush/Senate actions, the market index fell substantially. Most newspapers attributed these movements to the regulatory threat; the movements reflected fears that an interest rate cap would lead to substantial credit rationing, with economy-wide repercussions. Thus, the market model may be

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25 Bush's comments occurred after the close of trading on the 11th.

26 See the discussion in Salinger [1991] regarding inference when the event may affect the model parameters.

27 Although AT&T is in the group of issuers for which I record rate-cutting announcements, I do not include it in the portfolio of stock prices. AT&T's holdings of credit card receivables are too small relative to its assets to yield any discernible effect of events on its stock price.

inappropriate because the market index was itself affected by the event.[28] With this in mind, I also use the constant mean return model:

$$R_{it} = \alpha_i + \sum_s \delta_{is} \cdot D_{st} + \varepsilon_{it}$$

This specification omits the CRSP portfolio return. It uses less information than the market model, but as noted in Mackinlay [1997], it performs quite well in most circumstances.

Table 3 presents summary data and the results of event studies for the events surrounding the Bush announcement and Senate passage of the rate cap. The top two rows show raw cumulative returns for both the CRSP equally-weighted index and an equally-weighted index of the credit card issuers in the sample. There was no large pre-event effect for either index, but both fell in the ten days following the regulatory threat. The CRSP index fell nearly four percent, while the credit card issuer portfolio fell by over ten percent.

The third and fourth rows use the individual abnormal returns to calculate CARs over the ten-day periods prior to and following the regulatory threat.[29] The third row uses the market model, while the fourth uses the constant mean return model. The specification restricts the CARs to be identical across issuers, and allows the error terms to be contemporaneously correlated across issuers.[30]

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[28] It is not the case here that the market index fell simply because it contains returns for the issuers in the sample; it fell broadly, although the financial sector was hurt more severely than others.

[29] As noted by Salinger [1991], there is a simple transformation of the data matrix that allows direct estimates of CARs that correct for the forecast error implicit in their calculation. All CARs in the paper use this correction.

[30] It is standard to allow the errors to be correlated across firms when examining the effects of a single event on multiple firms in the same industry. In this instance it does not make much difference whether one allows for this correlation.

The results here are quite similar to those in the raw data. There is no statistically significant CAR in the pre-event period, and a negative and significant CAR in the post-event period. These effects are quite large when one consider that the corporations in the portfolio do not specialize in credit card offerings; most hold roughly 10% of their assets in credit card receivables. Thus, to see a net drop of ten percent in their values implies a larger reduction in the future profit stream from credit card operations.[31]

The Stock Market Effects of Rate-Cutting

The next step in the analysis is the analysis of stock market effects for rate-cutting by the largest credit card issuers. To conduct this analysis, I identified any rate-cutting announcements for the group of credit card issuers for the period 1986-1996, using the *LEXIS/NEXIS* database of major newspapers and banking trade publications (the exact search terms are described in the Data Appendix).[32]

Table A1 lists the set of rate-cutting announcements in chronological order. There are a total of eighteen cuts during the eleven year sample period. Two of the cuts occurred on the days immediately following Bush's comments, seven occurred within the six months following the regulatory threat, and 11 occurred within one year of the threat. This represents more than half of the total rate cuts by this set of issuers during the eleven-year sample period.[33] The rate cuts varied in size and in the percentage of an issuer's customer base to which they applied. It does not appear, however, that the size or

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[31] One should take care in interpreting these percentage figures. Credit card operations probably comprised much more than 10% of the discounted future profit stream for these issuers. Thus, we should use the 10% value as a lower bound on the change in value of issuers' credit card operations.

[32] Note that these are the dates on which the cuts were announced, rather than the dates upon which they took effect.

[33] For the sample of issuers, there are two rate increases during the eleven-year sample period. Neither increase applied to all of the issuer's customers, and neither occurred during the episode of threat.

applicability of the rate cuts was appreciably different following the regulatory threat. There were simply more of them.

I use the market model to estimate cumulative abnormal returns for the periods surrounding rate-cutting announcements.[34] There are 18 announcements in the sample. Two occurred on the two days following Bush's announcement; the effects of these announcements can not be separately identified from the effects of the regulatory threat. This leaves sixteen announcements. Based on the evidence discussed in section 3, media accounts noted the dissipation of regulatory threat by April 27, 1992. I therefore categorize the three announcements between November 11, 1991 and April 27, 1992 as occurring under regulatory threat. The remainder is in the control group, during which there was essentially no regulatory threat.[35]

I estimate separate equations for each issuer and announcement, and allow the effects of the announcement to be different for the announcing issuer and its rivals. I impose the restriction that all rival issuer CARs are identical for a given level of threat, and that all announcing issuer CARs are identical for a given level of threat.[36] I also report sign test results for the unrestricted estimates of issuer-event coefficients; the null hypothesis in these tests is that fifty percent of the CARs are below zero.

Returns for Announcing Issuers

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[34] In some cases the estimation window contains another price-cutting announcement. When this occurs, I move the estimation window back in time to the closest 250-day period that does not contain a price-cutting announcement.

[35] Classifying the cuts by Banc One on April 28 as occurring under threat rather than in the control group reduces the size and significance of the positive CARs experienced under regulatory threat in Table 4. The regulatory threat dummy in Table 5 remains positive and strongly significant.

[36] As noted in Rose [1985] and Prager [1991], these cross-equation equality restrictions on event coefficients will yield more efficient estimates, but might mask interesting differences across firms.

Table 4 shows results of these event studies for three sets of event windows. The first two event dates include either pre- and post-event dates, or only post-event dates; under the regulatory threat hypothesis, these will reflect returns based on the announcements.[37] The last event window includes only pre-event dates, to allay any concerns that the event studies are picking up spurious results.

The top set of rows shows results for the announcing issuers. There is no statistically significant effect of announcements on returns during the control period. However, for the ten days surrounding the announcements, 73 percent of CARs for announcing issuers are positive. The second two columns show the results of event studies for the announcements that occurred under regulatory threat. The results show positive abnormal returns for all event windows, and the CARs are statistically significant for the shortest event window, regardless of the error structure used to estimate the returns. The results are fairly large in economic terms - the average positive CAR is over ten percent for the announcing issuer. The sign test results are significant as well; all of the announcing issuer CARs are positive.

Returns for Rivals

The bottom set of rows shows returns for rivals of the announcing issuers – the remaining twelve issuers in the portfolio. Again, none of the returns during the control period are significant, but the general pattern is for weakly negative returns – the average returns are negative in all event windows that include post-announcement dates, and more than half (58-59 percent) of the CARS are negative.

[37] The post-event window is [t+1, t+5] rather than a more standard choice of [t, t+5] to show that nearly all of the CARs occurred after the event date. Compare the [t+1, t+5] CARs to the [t-5, t+5] CARs.

The returns during the episode of threat are starkly different. A statistically significant majority (84%) of the returns are positive, and the average CARs are positive and significant in nearly every specification. The magnitude of the results shows positive returns between four and six percent for the rivals. These are economically significant as well, although not as large as the CARs for the announcing issuers.

VI. Event and Issuer-Specific Influences on CARs

The results in the preceding section treat all announcements as identical, but the announcements might differ in ways that might be empirically relevant.[38] In this section I extend the analysis by regressing the announcing and rival issuer CARs on a set of explanatory variables. The data are arrayed in a panel of sixteen events, with between ten and thirteen issuer-level CARs for each event. Due to missing data for some of the explanatory variables, the total number of observations is 166. I include the following event- and issuer-specific variables in the regressions:[39]

(1) Magnitude of the rate cut = the size of the rate cut measured in hundreds of basis points. Larger rate cuts should have stronger direct profit effects (whatever their direction), and also might have a greater effect on the probability of regulation.

(2) Affects all customers dummy = a dummy variable equal to one if the rate cut applies to all customers of the announcing issuer. Again, we might expect this to strengthen the effects on returns.

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[38] See Table A1 for a list of the announcements and details regarding each announcement.

[39] Two other variables were included in initial specifications and dropped because they were not significant. These are the market share of the announcing issuer, and a dummy variable indicating whether the rate cut was accompanied by a switch to variable rate pricing.

(3) *Announcing issuer dummy* = a dummy variable equal to one for the issuer announcing the rate cut. Given the results in Table 4, we would expect announcing issuers to experience more positive returns than rivals.

(4) *Market share* = the market share of the issuer for which the observation is constructed. Issuers with higher market share will have greater exposure to changes in industry profitability.

(5) *Percent of assets held in credit cards* = the percent of a corporation's assets held in credit card operations. This also measures exposure to changes in industry profitability.

(6) *Losses from regulatory threat* = the (negative) return experienced by the issuer following the Bush remarks and Senate actions, measured as the (0, +10) CAR.[40] This variable is interacted with a dummy variable equal to one during the episode of regulatory threat. This captures vulnerability to the regulatory threat that is not quantifiable using the other explanatory variables.

I measure the effects of regulatory threat using two alternative specifications. The first regresses the CARs on the set of independent variables and a dummy equal to one if the announcement occurred under regulatory threat. This specification effectively assumes that the independent variables have the same effect on returns under and absent threat, and that any benefits of reduced threat are invariant to factors such as the size of the rate cut. This implictly assumes that the explanatory variables above will capture the direct profit effects of rate cuts on rivals. We would expect that larger rate cuts, and those that apply to all customers, would lead to more negative returns for rivals. We would

also expect that the two variables measuring increased exposure to changes in profitability would be negatively correlated with returns.

While the simple dummy variable approach useful for estimating the average difference in returns across the two regimes (threat vs. no threat), it is limited. We might think that the marginal effect of a given independent variable (say, the magnitude of the rate cut) might be different under regulatory threat, particularly if the independent variable affects the probability of regulation. I therefore also estimate a second, more flexible specification that interacts the regulatory threat dummy with each of the other independent variables. This allows the marginal effect of each variable to differ under regulatory threat and absent regulatory threat.[41]

There are two dependent variables in the regressions. The first is the raw [+1, +5] event window CAR. This should be viewed as estimating a lower bound on the true change in profitability for the issuer's credit card operations. The second dependent variable is a transformed CAR, equal to the raw CAR divided by the percent of assets in credit cards for the issuer in question.[42] This can be viewed as an upper bound estimate of the change in profitability for the issuer's credit card operations, as it assumes that the profit stream is evenly distributed across credit card and non-credit card operations.[43]

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[40] Essentially, Table 3 presents the averages of these issuer-specific returns. The individual returns are all negative, and range from –3.4 percent to –23.2 percent.

[41] Ideally, we would like to interact the announcing issuer dummy with these variables as well, because the marginal effect of the independent variables may differ for the announcing issuer. This creates collinearity problems that leave the model inestimable. Excluding the observations for announcing issuers leaves the results virtually unchanged, suggesting that their inclusion is not biasing the coefficients.

[42] This transformation is similar in spirit to that in Rose [1985]. Rose implements the transformation directly in the first stage event studies.

[43] During the early 1990s, all evidence suggests that credit card operations were two to three times more profitable than other sectors of the banking industry.

The specifications that use the transformed CAR omit the percent of assets in credit cards from the set of independent variables. All specifications include fixed issuer effects.[44]

Table 5 shows results of these regressions. The first two columns show results for the specifications that omit the interaction terms. In both columns the coefficient on the regulatory threat dummy variable is positive and strongly significant. Thus, both rivals and announcing issuers experience significantly positive returns following rate-cutting announcements under regulatory threat.[45] The coefficient on the announcing issuer dummy is positive, suggesting more positive returns on average for announcing issuers, but it is significant only in the raw CAR specification. For the most part, the coefficients on the other independent variables are negative. This is consistent with the idea that rate cuts have negative direct profit effects; larger and more widely applicable rate cuts lead to more negative returns, and greater exposure to changes in industry profitability is associated with more negative returns.

The next two columns show results from the specifications that include interaction terms. While the regulatory threat dummy is no longer positive, the pattern of coefficients on the interaction terms strongly supports the regulatory threat hypothesis. Larger rates cuts, and cuts that apply to all customers lead to higher returns; both of these effects are statistically significant. Moreover, the coefficient on the losses from threat interaction variable is negative, and significant at 10% in the raw CAR specification. This suggests that issuers with larger negative returns following Bush's remarks and the

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[44] Fixed event effects are not identified because some of the independent variables vary only across events. Estimates using random event and issuer effects yielded nearly identical results.

[45] If we interpret the coefficients from the two columns as lower and upper bounds, then the average positive return for all firms following a rate cut under threat ranges from six percent to nearly fifty percent.

Senate actions experienced larger positive returns following rate cuts during threat. In concert, the positive effect of these coefficients offsets the negative coefficient on the regulatory threat dummy.[46] In addition, while there is no significantly positive effect for announcing issuers absent threat, there is a fairly large positive and significant effect for announcing issuers under threat.

Some Remarks on the Results

The results presented in this section yield a coherent picture. The threat of regulation led to negative returns for the issuers in the sample, as we would expect under either exogenous or endogenous regulatory threat. While there had been murmurs in Congress and the press regarding the likelihood of price cap regulation, it seems clear that the Bush/Senate actions were regarded as indicating a significantly higher probability of regulation.

The regressions using the CARs as the dependent variable show that the average return for both announcing issuers and rivals is significantly more positive under regulatory threat. The specifications using the interaction terms are also consistent with the regulatory threat hypothesis. Absent threat, larger and more widely applicable rate cuts lead to more negative returns for rivals, and issuers that have greater exposure to changes in profitability experience more negative returns. The pattern is largely reversed during the episode of regulatory threat. In concert, the results provide support for the regulatory threat hypothesis.

While a positive return of fifty percent seems implausibly high, even the much weaker threat mentioned in the conclusion below led to single-day negative returns greater than ten percent for credit card pure-plays.

[46] At the means of the independent variables, these coefficients sum to 1.553, which is greater than the negative coefficient of –1.114 on the regulatory threat dummy.

VII. Conclusion

We do not typically think of the credit card market as one in which government intervention has substantively changed firm behavior. The evidence presented here suggests otherwise. Following the regulatory threat, prices fell significantly, and those issuers most directly targeted cut their rates the most. In concert with this direct price evidence, the stock price effects of both the threat and the rate-cutting announcements following it are strongly consistent with the regulatory threat hypothesis. When lawmakers linked the probability of regulation to issuer rates, rate-cutting led to significantly more positive stock market returns for issuers announcing rate cuts. More importantly, the data identify strong spillover effects to other issuers following rate-cutting announcements; this result is extremely difficult to explain without an appeal to the regulatory threat hypothesis.

One pattern in the results that deserves further discussion. In most specifications, the CAR regressions identify a significant positive return to being the announcing issuer. This raises the question of why this positive "kick" exists. It does not seem to be the case that any issuer announcing a rate cut during this period would have experienced such a kick; there seems to have been no race to announce first.[47] This suggests that those issuers who announced cuts first had the greatest incentives to do so. These incentives might have derived from cross-sectional differences in issuers' vulnerability to regulation. For example, issuers with high default rates would face greater incentives to ration credit if a rate cap were imposed, and might therefore have a greater incentive to cut rates. On the other hand, issuers with very low default rates might also have a greater incentive to

move first, because they would be better able to absorb the rate cut. Unfortunately the set of observations is too small to formally test these hypotheses.

A final point worth discussing is whether we should view this instance of regulatory threat as a transitory event. While this is possible, it seems more likely that the events of 1991 marked a new and permanent introduction of (or upward shift in) endogenous regulatory threat. One reason to believe this is that the dissipation of a transitory threat would lead to higher rates. In contrast, credit card rates remained low well after 1991. Further evidence in support of this idea is provided by events that occurred over two years later. On February 21, 1993, a rumor circulated that President Clinton was reconsidering the imposition of a rate cap. Stocks in the credit card pure-plays (Advanta, First USA, and MBNA) fell an average of 11% that day. They rebounded fully the next day after the White House denied the rumor, but this seems to suggest that while the threat had been staved off in 1991, it might become viable at some point in the future.[48]

The relevance of these events in the credit card market suggests that regulatory threat may be an overlooked influence on pricing in other industries. Evidence is accumulating that similar behavior occurs in markets threatened with environmental regulation.[49] While the situations are not exactly analogous, the findings presented here

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[47] Recall that of the five cuts during the period of threat, two occurred in the days immediately following its imposition, but the remaining three were spread over the next six months.

[48] Ironically, an article examining the reasons for investor jitters regarding card stocks cited the administration's actions toward the pharmaceutical market (the subject of Ellison and Wolfram's work) as a reason for uncertainty.

[49] Arora and Cason [1995], Maxwell et al. [1999], Hemphill [1994], Konar and Cohen [1997], and Pargal and Wheeler [1996] all examine different aspects of this issue. Pirrong [1995] examines similar behavior in commodity exchanges.

might illuminate empirical approaches to measuring the effects of regulatory threat in these and other markets.

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Data Appendix

A. Price Evidence

Average Interest Rates and Treasury Bill Rates

Average credit card and Treasury bill notes taken from the Federal Reserve Board Statistical Release E.5, *Terms of Consumer Installment Credit*, and publicly available lists of Treasury bill rates.

Cross-sectional (Panel) Data

The data for the cross-sectional comparison of visible and non-visible firms are from the *Card Industry Directory*, an annual trade directory. The directory contains data for the largest 250 issuers, although missing values limit the usability of many data points. The values for data in the directory are measured at year-end.

B. Profit Evidence

The Set of Issuers in the Portfolio

The issuers chosen for initial inclusion in the sample were the top 20 (ranked by accounts) at year-end 1991, the period closest to the regulatory threat. Due to missing data, some issuers were dropped. The issuers for which data exist are: Citibank, Bank of America, Chase Manhattan, Chemical Bank, Bank of New York, Banc One, First Chicago, Corestates, Norwest, Manufacturers Hanover, AT&T Universal, Wells Fargo, and Household Financial. By 1996 only ten of these issuers remain - three having been acquired by other issuers in the group.

Rate-Cutting Announcements

Rate-cutting announcements were compiled from the Lexis/Nexis database. Articles from major newspapers and industry publications were included in the search. The first-pass search consisted of a keyword search for "rate" and "cut" and "credit card." Once the set of issuers had been identified, an additional issuer-specific search was conducted, e.g., "rate" and "cut" and "Citibank." The list in Table A1 includes all announcements found in this manner, during the period 1986-1996 inclusive. Nearly all of the announcements appear in *The American Banker*, a daily banking newspaper.

Legislation Relating to the Credit Card Market

The set of proposed credit card legislation was compiled from Lexis/Nexis' *Congressional Universe* data file, which tracks proposed legislation.

Stock Prices

Daily stock returns for the major credit card issuers and the CSRP index were obtained from Wharton Research Data Services' CRSP database.

Appendix A: Event Studies of Credit Card Interest Rates

The baseline specification for these event studies is:

$$\Delta CCR_t = \beta \cdot \Delta X_t + \delta \cdot D_t + \varepsilon_t ,$$

where ΔCCR is the quarterly change in the national average credit card interest rate series, X is a vector of explanatory variables and D is a vector of dummy variables equal to one during the event window [t-4, t+16]. The model is estimated from 1972.1 to 1995.4 (which is t+16). The coefficients δ measure abnormal changes in the credit card series. For each event window shown in Table 1, the cumulative abnormal return is in theory the sum of the individual δ's for that window. In practice, the sums and their standard errors were calculated by using the dummy variable approach in Salinger (1992); this approach implicitly corrects for the forecasting error implicit in the estimation of the model.

The three specifications shown in Table 1 vary the regressors in X. The first specification includes a only constant term (this is analogous to the constant mean return model discussed in Section 5):

$$\Delta CCR_t = \beta_1 + \delta \cdot D_t + \varepsilon_t$$

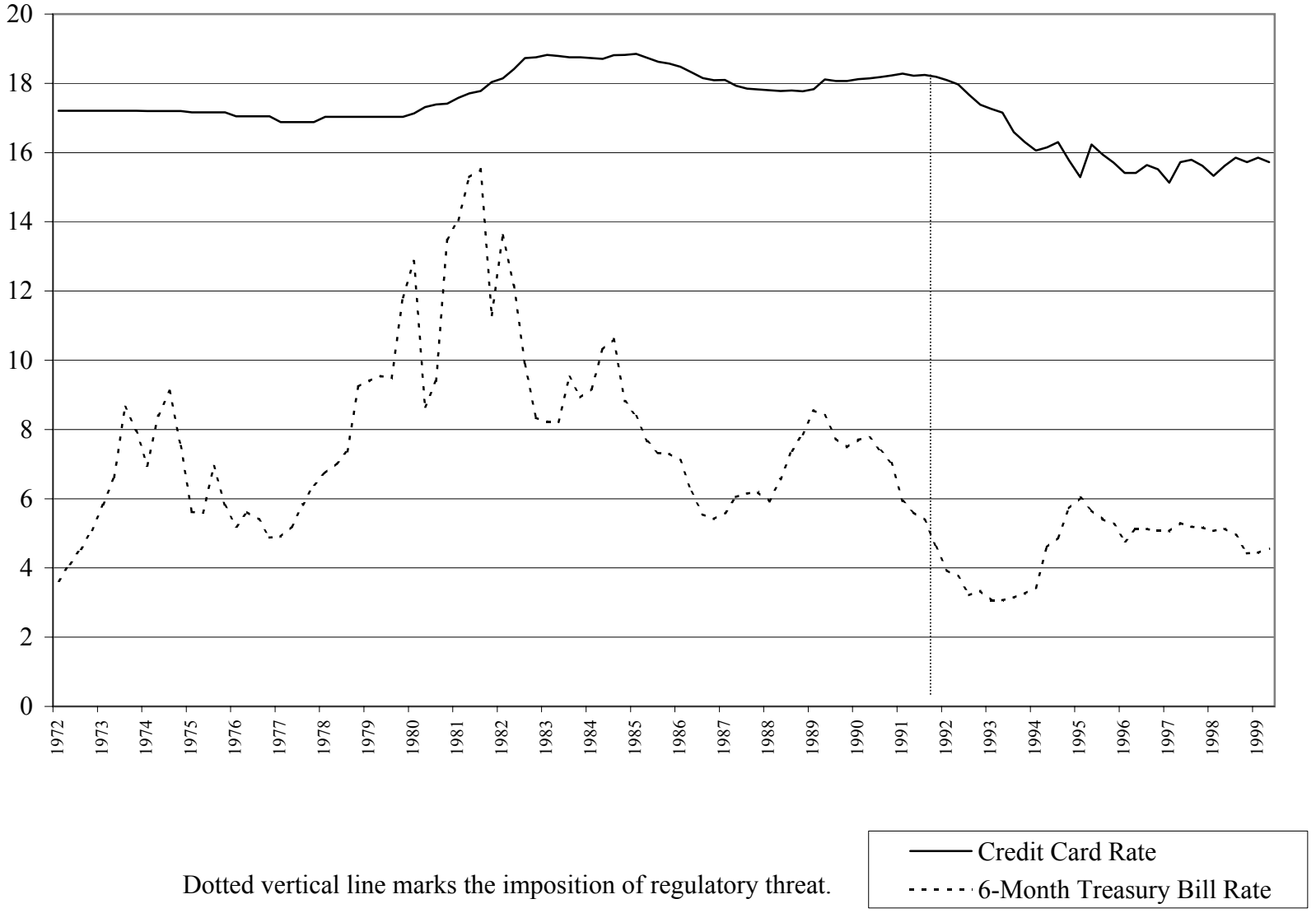
The second specification includes the change in the 6-Month Treasury bill rate, and the lagged change:

$$\Delta CCR_t = \alpha + \beta_1 \cdot \Delta TBILL_t + \beta_2 \cdot \Delta TBILL_{t-1} + \delta \cdot D_t + \varepsilon_t$$

The third specification allows for asymmetric responses of the credit card rates to increases and decreases in the T-bill rate. It also defines a dummy variable equal to one during the period of more stringent credit card usury laws (1972-1981), and interacts this dummy with the (asymmetric) T-bill variables:

$$\begin{aligned}\Delta CCR_t = {} & \alpha + \beta_1 \cdot \Delta TBILL_t^+ + \beta_2 \cdot \Delta TBILL_t^- + \beta_3 \cdot \Delta TBILL_{t-1}^+ + \beta_4 \cdot \Delta TBILL_{t-1}^- + \\ & \beta_5 \cdot D7281 \cdot \Delta TBILL_t^+ + \beta_6 \cdot D7281 \cdot \Delta TBILL_t^- + \beta_7 \cdot D7281 \cdot \Delta TBILL_{t-1}^+ + \\ & \beta_8 \cdot D7281 \cdot \Delta TBILL_{t-1}^- + \delta \cdot D_t + \varepsilon_t\end{aligned}$$

Figure 1
Credit Card and Treasury Bill Rates, 1972-1999



Dotted vertical line marks the imposition of regulatory threat.

Table 1
Cumulative Abnormal Changes in Credit Card Interest Rates Following Regulatory Threat

Event Window:	Specification (1)	(2)	(3)
[-4, -1]	-0.029	-0.033	0.165
	(0.166)	(0.164)	(0.175)
[0, +3]	**-0.638****	**-0.652****	**-0.481***
	(0.254)	**(0.253)**	**(0.214)**
[0, +7]	**-1.761****	**-1.769****	**-1.500****
	(0.413)	**(0.419)**	**(0.329)**
[0, +11]	**-2.104****	**-2.103****	**-1.798****
	(0.537)	**(0.549)**	**(0.421)**
[0, +15]	**-2.516****	**-2.520****	**-2.158****
	(0.645)	**(0.661)**	**(0.510)**
Adj. R2 of Estimating Equation	0.72	0.74	0.81
n	95	94	94

* - significant at 5% ** - significant at 1%

Notes:

(1) All results are calculated based on an estimating equation using quarterly data from 1972-1991.
(2) Dependent variable in the estimating equation is the quarter-to-quarter change in the average most common credit card interest rate, as reported by the Federal Reserve.
(3) Event date 0 is the 4th quarter of 1991.
(4) Specification (1) includes only a constant term in the estimating equation.
(5) Specification (2) includes a constant term, the change in the 6-Month Treasury bill rate, and the lagged change in the 6-Month Treasury bill rate in the estimating equation.
(6) Specification (3) includes the current and lagged changes in the 6-Month Treasury bill rate, interactions allowing for asymmetric effects of positive and negative T-bill rate changes, and interactions allowing for different (and asymmetric) effects of changes in T-bill rates from 1972-1981.
(7) Standard errors calculated using the method in Salinger (1992) are shown below CARs.

Table 2
Political Visibility and Changes in Interest Rates

Variable	Dependent Variable: Change in Rate		Dependent Variable: Change in Default-Adjusted Rate	
Visibility	0.209	0.186	0.520	0.523
	(0.264)	(0.287)	(0.390)	(0.405)
Visibility*1991	-0.567	-0.533	-0.702	-0.716
	(0.480)	(0.450)	(0.584)	(0.574)
Visibility*1992	**-0.757***	-0.536	**-1.285****	**-1.007****
	(0.396)	(0.385)	**(0.494)**	**(0.490)**
Visibility*1993	-0.590	-0.108	**-1.420***	-1.001
	(0.709)	(0.656)	**(0.797)**	(0.768)
Visibility*1994	0.540	-0.122	0.865	0.024
	(0.683)	(0.640)	(0.816)	(0.750)
1991 Dummy	0.002	0.146	-0.213	-0.028
	(0.154)	(0.124)	(0.187)	(0.177)
1992 Dummy	**-0.274***	-0.188	**-0.862****	**-0.864****
	(0.145)	(0.130)	**(0.176)**	**(0.171)**
1993 Dummy	**-0.668****	**-0.473****	**-0.609****	**-0.392***
	(0.173)	**(0.186)**	**(0.200)**	**(0.221)**
1994 Dummy	0.237	-0.244	**0.410***	-0.033
	(0.182)	(0.187)	**(0.207)**	(0.221)
Change(Default)	**0.097****	**0.091***		
	(0.050)	**(0.049)**		
Constant	-0.171	-0.084	-0.155	-0.109
	(0.116)	(0.098)	(0.138)	(0.130)
Effects		Rate Type, Charter Type, Year*Rate Type, Year*Charter Type		Rate Type, Charter Type, Year*Rate Type, Year*Charter Type
Adj. R2	0.05	0.17	0.09	0.18
n	842	842	842	842

* - significant at 5% ** - significant at 1%

Notes: (1) All standard errors are heteroscedasticity-consistent.
(2) "Visible" issuers are those in the top 20 (ranked by accounts) at year-end 1991.
(3) Data covers the period 1990-1994. Source: *Card Industry Directory* , various years.
(4) "Charter Type" is a dummy equal to one if the issuer is a credit union, zero otherwise.
(5) "Rate Type" is a dummy equal to one if the issuer charges a variable rate, zero otherwise.

Table 3
Stock Market Returns Surrounding the Threat of Regulation, 1991

	Event Window: (-10, -1)	(0, +10)
Credit Card Portfolio Return	0.94%	-10.61%
Cumulative Market Return	1.22%	-3.89%
CAR, Market Model	0.51%	**-5.42%**
	(2.34)	**(2.48)**
CAR, Mean Return Model	1.93%	**-10.49%**
	(2.64)	**(2.80)**

Notes:

(1) "Credit Card Portfolio Return" is the cumulative return on an equally-weighted portfolio of credit card issuers.
(2) "Cumulative Market Return" is the cumulative return on the CRSP equally-weighted index.
(3) CARs are calculated using the method in Salinger (1991).
(4) Event date 0 is November 12, 1991 - the day after Bush's statement, and one day before the Senate passage of the rate cap.
(5) Bold type indicates statistical significance at 5% or better.

Table 4
Stock Market Returns Surrounding Rate-Cutting Announcements

	Period			
	No Threat		**Threat**	
Event Window	(1)	(2)	(1)	(2)
CAR, Announcing Firm				
(-5, +5)	0.006	0.008	**0.116****	**0.137****
	(0.039)	(0.043)	**(0.050)**	**(0.050)**
(+1, +5)	0.001	0.002	**0.111****	**0.115****
	(0.023)	(0.025)	**(0.034)**	**(0.033)**
(-10, -1)	-0.005	0.002	-0.039	-0.029
	(0.038)	(0.041)	(0.047)	(0.047)
Percent Negative CAR, Announcing Firm				
(-5, +5)	27%		0%	
(+1, +5)	36%		0%	
CAR, Rivals				
(-5, +5)	-0.01	-0.009	0.044	**0.064****
	(0.029)	(0.020)	(0.037)	**(0.024)**
(+1, +5)	-0.008	-0.005	**0.041***	**0.051****
	(0.020)	(0.013)	**(0.025)**	**(0.016)**
(-10, -1)	0.001	-0.002	-0.01	-0.005
	(0.029)	(0.019)	(0.035)	(0.023)
Percent Negative CAR, Rivals				
(-5, +5)	58%		**16%**	
(+1, +5)	59%		**16%**	

* - significant at 10% ** - significant at 1%

Notes:

(1) Column (1) allows contemporaneous correlation of errors across firms.
(2) Column (2) assumes independent errors across firms.
(3) Event date 0 is date the announcement appeared in the news.
(4) CARs are cumulative abnormal returns over the event window in percent.
(5) Standard Errors are shown below CARs.

Table 5
Cumulative Abnormal Return Regressions

Variable	CAR:			
	Raw	Transformed	Raw	Transformed
Regulatory Threat Dummy	**0.060****	**0.499****	-0.059	**-1.114****
	(0.008)	**(0.079)**	(0.058)	**(0.501)**
Announcing Issuer Dummy	**0.017***	0.249	0.008	0.013
	(0.009)	(0.162)	(0.006)	(0.113)
Market Share	-0.117	-0.292	-0.210	**-0.999***
	(0.198)	(0.618)	(0.184)	**(0.594)**
Percent of Issuer's Assets in Credit Cards	-0.215		-0.140	
	(0.132)		(0.142)	
Magnitude of Rate Cut	0.001	-0.013	-0.002	**-0.042***
	(0.003)	(0.023)	(0.002)	**(0.022)**
Affects all Customers Dummy	**-0.014****	**-0.193****	**-0.010***	**-0.116***
	(0.006)	**(0.064)**	**(0.006)**	**(0.066)**
Announcing Issuer*Regulatory			**0.041****	**0.662****
Threat Dummy			**(0.019)**	**(0.281)**
Market Share*Regulatory			**0.301****	0.694
Threat Dummy			**(0.130)**	(1.083)
Percent of Assets*Regulatory			-0.076	
Threat Dummy			(0.148)	
Magnitude of Rate Cut*Regulatory			**0.027****	**0.375****
Threat Dummy			**(0.013)**	**(0.103)**
Affects All Customer Dummy*Regulatory			0.032	**0.700****
Threat Dummy			(0.034)	**(0.339)**
CAR Following Threat*Regulatory			**-0.186***	-1.996
Threat Dummy			**(0.095)**	(1.957)
n	166	166	166	166
adj. R2	0.34	0.28	0.45	0.33

* - significant at 10% ** - significant at 5%

Notes: (1) CARs used as dependent variable are [+1, +5].
(2) Transformed CAR = (Raw CAR)/(% of Assets in Credit Cards)
(3) Heteroscedasticity-consistent standard errors are shown below coefficient estimates.

Table A1
Chronology of Rate-Cutting Announcements

Date	Announcing Firm	Old Rate	New Rate	Applied to All Customers?
November 28, 1986	Citibank	19.8	16.8	N
December 19, 1986	Chase	19.8	17.5	N
January 19, 1987	Chemical	19.5	16.8	N
March 1, 1991	AT&T	18.9	17.9	Y
June 28, 1991	AT&T	17.9	17.4	Y
November 12, 1991	First Chicago	17.4	14.4	N
November 13, 1991	AT&T	17.4	16.4	Y
January 31, 1992	Chemical	19.8	17.8	Y
March 17, 1992	AT&T	16.4	15.4	Y
April 16, 1992	Citibank	19.8	15.8	N
April 28, 1992	Bank One	various	various	Y
May 21, 1992	Norwest	19.8	15.5	Y
July 6, 1992	Norwest	15.5	15	Y
August 5, 1992	Chase	19.8	16.4	Y
September 17, 1992	AT&T	15.4	14.9	Y
September 17, 1992	Bank of America	19.8	16.9	N
September 19, 1996	Citibank	17.65	15.65/13.65	N
November 6, 1996	AT&T	14.9	13.9	Y

Notes:
AT&T's listed rate is that for "charter members." New member rates are 100 basis points higher.
Bank One's rate cuts were for 300-400 basis points for all customers.
Cuts within the dotted lines are denoted as occurring under regulatory threat.

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