Price Ceilings as Focal Points for Tacit Collusion: Evidence from Credit Cards

Christopher R. Knittel and Victor Stango*

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Abstract

We test whether a non-binding price ceiling may serve as a focal point for tacit collusion, using data from the credit card market during the 1980s. In our sample, most credit card issuers face a state-level interest rate ceiling, and well over half match their ceiling. We develop an empirical model that can separately identify the instance in which an issuer matches its ceiling because it is binding, and the instance in which an issuer matches its ceiling even though it is not binding. The model yields evidence in favor of tacit collusion: a statistically significant proportion of issuers match their ceiling even though it is not binding. Within a state, tacit collusion is less likely as the ceiling rises, more likely as concentration or costs rise, and less likely in periods of high demand. We also find that entry into credit cards is higher where we find evidence of tacit collusion, and lower where we find evidence that a ceiling is binding. It appears that tacit collusion became less prevalent over the 1980s, as entry into credit cards surged nationwide. The results highlight a perverse effect of price cap regulation.

Keywords: Focal Points, Tacit Collusion, Price Ceilings, Double Hurdle.

* *Knittel:* Department of Finance and Economics, Boston University, Boston MA 02215. *Email:* knittel@bu.edu. *Stango:* Emerging Payments Department, Federal Reserve Bank of Chicago, 230 S. LaSalle St., Chicago IL 60604. *Email:* victor.stango@chi.frb.org. Thanks to Larry Ausubel, Ron Johnson, Sharon Oster, Ariel Pakes, Rob Porter, Peter Reiss, Alan Sorensen, and seminar participants at the University of Maryland, Northwestern (Kellogg), the Federal Reserve Bank of Chicago and the 2001 NBER Summer Institute in Industrial Organization for constructive comments and suggestions.

...The Michigan Citizens Lobby asserted that the failure of virtually all VISA and Mastercard issuers in the state, including the 10 largest, to reduce their rates from the maximum 18% allowed by law may indicate "potentially illegal activities." "Since smaller banks have assured us that they are making profits charging interest rates of 15% and below, it is clear that this uniformity is not justified by actual costs. We fear the alternative may be tacit or explicit collusion," said the Citizens Lobby director.

- from *The American Banker*, **March 26, 1987**

1 Introduction

Price ceilings are a common form of economic regulation. While debates over their welfare and distributional effects are far-ranging, one commonly held conception is that their effect on prices can only be negative. At the heart of this conception is the assumption that a price ceiling has no price or output effects when it is not binding. While a small body of work exists challenging this view, it has been to this point anecdotal or experimental; empirical evidence suggesting that non-binding price ceilings affect prices is largely non-existent.[1]

In this paper, we empirically test the hypothesis that a non-binding price ceiling may lead to *higher* prices – by serving as a focal point for tacitly collusive price-setting. We test the focal point hypothesis using data from credit card issuers during the 1980s. During our sample period, most credit card issuers face state-level price ceilings that could plausibly serve as focal points. These price ceilings vary across and within states; there is also a group of states with no ceiling. More importantly for our purposes, many issuers match their ceiling – particularly in the early years of the sample. Finally, the states themselves vary in market characteristics thought to affect the sustainability of tacit collusion. The data therefore display a great deal of heterogeneity in firm behavior, focal points and market characteristics. This allows us to conduct a variety of tests of the focal point hypothesis.

The novelty of our empirical approach is that it separately identifies the instance in which an issuer matches the ceiling of its home state because it is binding, and the instance in which an issuer

[1] See Isaac and Plott (1981) and Smith and Williams (1981) for experimental evidence suggesting that nonbinding price ceilings affect prices.

There is also a case precedent supporting the view that horizontal agreements fixing maximum prices can facilitate tacit collusion. In *Arizona v. Maricopa County Medical Soc., 457 U.S. 332 (1982)*, the maximum-fee schedule used by a medical association was found "to have the effect of stabilizing and enhancing the level of actual charges by physicians."

matches the ceiling even though it is *not* binding. The likelihood function for the data explicitly allows ceilings to be binding, by incorporating features of a standard censored model of pricing. It then extends the model to allow for tacit collusion by introducing an independent probability that an issuer matches its ceiling even though it is not binding. Our full specification uses issuer-, state- and time-specific covariates to allow the probability of tacit collusion to vary across issuers and time.

The results support the focal point hypothesis. Our model estimates a statistically and economically significant probability of tacit collusion. In the early years of the sample, we estimate that tacit collusion is quite common; a large fraction of issuers match their ceiling even though it is not binding. We find that the facilitative power of the ceiling dissipates as the ceiling rises. We also find that tacit collusion is more likely as concentration, issuer-level costs rise and the size of the firm increases, and that tacit collusion is less likely in periods of high demand. Near the end of the sample period, we identify a regime change after which tacit collusion is much less likely. We attribute this to a surge in entry into credit cards during 1985-86, and aggressive competition at the national level by a set of large issuers.

In the final section of the paper, we show that our estimates of state-level tacit collusion are directly related to state-level entry rates in credit cards. Entry rates are significantly higher than average when we estimate that issuers within a state are tacitly colluding, and significantly lower than average when we estimate that issuers face a binding ceiling. The link between state-level tacit collusion and entry is quite strong from 1979-84, then disappears. This corroborates our finding of a regime change in credit card competition in 1985-86.

2 Price Ceilings, Focal Points and Tacit Collusion: Theory and Empirical Implications

In this section, we discuss the empirical implications of the hypothesis that firms are tacitly colluding at a focal point. We also discuss some general empirical implications of models of tacit collusion. Because the natural alternative hypothesis explaining pricing at price ceilings is simply that they are binding, we discuss the empirical implications of the focal point hypothesis in the context of this alternative. We then relate our empirical approach to previous work testing for collusion and tacit collusion.

2.1 Tacit Collusion at a Focal Point

Under quite general conditions, firms may sustain supercompetitive prices by interacting repeatedly and constructing strategies under which they use the threat of future punishment to sustain current cooperation.[2] In this context, the "Folk Theorem" asserts that for sufficiently low discount rates nearly any set of payoffs may be sustained as the outcome of a repeated game.[3] The Folk Theorem is powerful, in the sense that it provides quite general conditions under which tacit (or explicit) collusion may be sustainable. However, this generality leads to difficulty in conducting empirical tests for collusion or tacit collusion.

In practical terms, the problem of tacit collusion often reduces to one of successful coordination. Firms can resolve the coordination problem in many ways; one such way is through the use of a focal point. The theory of focal points dates at least to Schelling (1960), who noted that in simple games with many equilibria, agents can quite often recognize a focal point and use it to coordinate. In one of his more well-known examples, Schelling discusses the problem of two people simultaneously choosing a common location (in which to meet) in New York City. Given that the game possesses an infinite number of equilibrium location-pairs, we might expect the odds of successful coordination to be quite low. However, in practice most people who play the game choose a well-known spot – such as Times Square or the Statue of Liberty – and can successfully coordinate. In situations where firms set prices, it is often suggested that the "clustering" of prices occurs at certain natural focal points (*e.g.*, \$9.99).

Because firms may sustain tacit collusion under a variety of observationally equivalent mechanisms, we do not attempt to explicitly model the process by which a focal point facilitates tacit collusion. Rather, we develop empirical implications of the focal point hypothesis by making observations regarding the patterns of pricing that we would observe if a focal point were facilitating tacit collusion.

2.1.1 Empirical Implications of Tacit Collusion at a Focal Point

The first empirical implication of the focal point hypothesis is that if the focal point facilitates tacit collusion, we should observe greater clustering at the focal point than would otherwise be

[2]Simple forms of these models are described in Tirole (1992), Chapter 6. Some well-known supergame-theoretic models of tacit collusion can be found in Green and Porter (1984), Rotemberg and Saloner (1986), Haltiwanger and Harrington (1991), and Abreu, Pearce and Stacchetti (1986).

[3]See, e.g., Fudenberg and Tirole (1991) and others for discussion of the Folk Theorem.

expected.[4] Because the focal point is a price ceiling, we might expect a certain degree of clustering even absent tacit collusion. The relevant empirical test, then, is an estimate of the extent to which firms match the ceiling even when it is not binding. We outline the econometrics of this test below.

A second implication of the focal point hypothesis is that all else equal, it becomes more difficult to sustain tacit collusion as the focal point rises.[5] To see the intuition behind this claim, consider first the limiting case in which the focal point is equal to a firm's one-shot non-cooperative price. In this instance, it is trivially easy for a firm to maintain cooperation at the focal point. As the focal point rises, profits from cheating rise faster than profits from cooperation; this must be true because profits from cheating reflect unconstrained re-optimization, while profits from cooperation reflect constrained behavior.[6] Because cheating becomes relatively more attractive as the ceiling rises, we should be less likely to observe tacit collusion in markets with higher focal points for pricing. More precisely, the probability that a given firm matches a nonbinding price ceiling should be a decreasing function of the ceiling.

A related issue is that as costs rise, cooperation at the focal point becomes easier to maintain. Again, note that cooperation is trivially easy when costs are such that a firm's non-cooperative price equals the ceiling. As costs fall below this level, profits from cheating rise more quickly than profits from cooperation, because the former reflect re-optimization. Thus, high-cost firms will find cooperation more attractive.

In addition to the above implications of pricing at focal points, there are other general empirical implications of tacitly collusive pricing that should be testable in our setting. A first implication is that tacit collusion is generally viewed as easier to maintain among fewer firms. Thus, we should be more likely to observe successful tacit collusion when market concentration is high. A second is that we might expect larger firms to be more likely to cooperate than smaller firms. Given that cheating is attractive because it steals business from other firms, a small firm will find the gains from cheating proportionately larger than a larger firm. This implies that the probability that a firm tacitly colludes at the ceiling should be an increasing function of firm size. A final implication is that tacit collusion becomes more difficult to sustain in periods of high demand, because high current demand increases the current gains from cheating.[7]

[4]We need not observe unanimous clustering at the focal point in order to infer tacit collusion. The Folk Theorem readily admits instances of "partial" tacit collusion, in which some firms tacitly collude at the focal point and others play their short-run best responses given other firms' prices.

[5]In Appendix A, we show a general set of conditions under which this is true.

[6]The constraint under cooperation is that the firm's price must match the focal point.

[7]This follows the intuition in Rotemberg and Saloner (1986). In their model, the key assumption underlying the

2.2 Testing for Collusion and Tacit Collusion

Most empirical tests for collusion or tacit collusion involve testing whether the distribution of prices thought to reflect collusion or tacit collusion is different from a control distribution thought to reflect non-collusive behavior. In such tests, a central question is whether a candidate distribution of collusive prices can be identified *a priori* – for example, because it comes from a group of firms accused or convicted of collusion. When the candidate and control distributions can be identified in this way, it is relatively straightforward to test for equality of the distributions. Rejection of equality is taken as evidence of tacit or explicit collusion. Examples in this line of work include Porter and Zona (1993, 1999).

When the candidate set of collusive prices can not be identified *a priori*, it may be possible to endogenously identify the collusive and non-collusive distributions, often using some form of mixture modeling. Porter (1983), for example, uses a switching regression to endogenously classify periods of pricing into collusive and non-collusive regimes. Ellison (1994) uses a similar approach that defines the transition probabilities between collusive and non-collusive periods using a Markov process. In each of these cases, the data clearly identify periods of collusive and non-cooperative behavior. Baldwin, Marshall and Richard (1997) employ a similar model within an auction-bidding framework. They estimate a model in which winning bids may be drawn from a collusive bid distribution with probability p, or from a noncooperative bid distribution with probability $1-p$. This model outperforms a model that maintains the hypothesis of noncooperative behavior.

A criticism of the approaches above is that results consistent with collusion may simply reflect specification error. For example, an omitted variable might lead to the spurious identification of separate price distributions, or rejection of equality of two distributions. Mis-specification of functional form may also lead to spurious evidence in favor of collusion. For these reasons, most of the work above uses more finely characterized tests to narrow the set of explanations for the empirical results. For example, Porter and Zona (1999) condition prices on observable costs, and find that prices for control firms are correlated with costs in an intuitive manner while prices from candidate firms are not. Porter (1985) and Ellison (1994) allow the transition probabilities between collusive and non-collusive regimes to depend on unanticipated demand shocks, as in the model of Green and Porter (1984).[8] Baldwin, Marshall and Richard (1997) use a dummy variable thought to capture "neighbor" effects to pick up differences in the likelihood that firms collude. These

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prediction that high current demand reduces the maximum sustainable price is that demand shocks are i.i.d. Other specifications of demand (see, e.g., the Haltiwanger and Harrington [1991] model in which demand is cyclical) may yield different predictions.

[8]The primary test in Borenstein and Shepard (1999) is similar in spirit, in that they examine the effects of changes

refinements can not conclusively rule out specification error, but they increase the burden faced by a specification error-based explanation for the results. For example, it seems highly unlikely that in Porter and Zona's data there are unobserved costs that are correlated with prices and negatively correlated with observable costs only for those firms in the candidate group.

Our empirical approach parallels those mentioned above. Because we are interested in estimating the facilitative power conferred by the focal point, we can use observations of firms that are not pricing at their focal point (or do not face a focal point) as a control group. In our sample this control group is composed both of observations in states without a price ceiling, and of observations with prices below their ceiling (in states that have ceilings). This control group can be exploited in a manner similar to that in Porter and Zona's work.

For firms at the ceiling the issue is more complex. We must recognize the possibility that a firm may match the ceiling because it is binding. This requires a means of endogenously separating the observations that reflect tacitly collusive behavior from those that do not. While this problem is similar in spirit to that faced by Porter (1983), Ellison (1994), and Baldwin, Marshall and Richard (1997), in these other cases the problem is simplified by the fact that collusive and non-cooperative regimes lead to different observations of the dependent (price) variable. In our case, a given observation at the ceiling may reflect either collusive or non-collusive behavior. We resolve this complication by using an econometric specification that expands upon traditional models for censored data. The specification introduces an independent probability that an issuer matches the ceiling even though it is not binding. In essence, we allow the data to endogenously identify the collusive and non-collusive price distributions; the difference between previous approaches and ours is that the collusive observations in our case are drawn from a point distribution.

To address the standard omitted variable concern and strengthen our claim that the identification of a second price distribution reflects tacit collusion at the focal point, we extend our empirical work in two ways. First, we test whether our identification of tacit collusion is consistent with factors thought to affect the sustainability of tacit collusion. This involves allowing the probability of tacit collusion to vary as a function of issuer- and state-specific factors thought to affect the viability of tacit collusion: the level of the ceiling, costs and demand, market concentration and firm size.

Our second extension involves using our empirical results to construct a state-level variable measuring the extent to which issuers are tacitly colluding or facing a binding ceiling. We would

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in demand and costs. However, their dependent variable is price rather than the probability of collusion. They also focus on anticipated rather than unanticipated changes in demand and costs, in order to test the Rotemberg and Saloner (1986) and Haltiwanger and Harrington (1991) models of tacit collusion.

expect this variable to be correlated with state-level entry into credit cards; entry should be higher in states where issuers are tacitly colluding, and lower where issuers face a binding ceiling. This test involves regressing state-level entry in credit cards on a set of explanatory variables and our estimated measure of tacit collusion.

Before outlining the empirical approach in more detail, we present a summary of our data, and discuss the relevant institutional detail.

3 Pricing and Interest Rate Ceilings in the Credit Card Market, 1979-1989

3.1 Interest Rate Ceilings in the Credit Card Market

In 1979, most credit card issuers faced state-level ceilings on credit card interest rates. The ceilings bound the behavior of credit card issuers based on their state of incorporation.[9] Table 1 presents data describing the incidence of ceilings between 1979 and 1989. The information on interest rate ceilings is from *The Cost of Personal Borrowing in the United States*, an annual compendium of state-level usury law.[10] The top rows of the table show data averaged over all states in the sample, while the bottom rows show data averaged over issuers for which we have interest rate data.[11] The pattern for the state-level data is nearly identical to that for the issuer-level data.[12]

In 1979, ceilings existed in over ninety percent of states in our sample. The ceilings varied across states, but the most common ceiling was eighteen percent, which prevailed in nearly eighty percent of states. In 1979, a few states had ceilings below eighteen percent; usually these ceilings

[9]Neither ceilings nor other regulation restricted interstate marketing of credit cards. For example, in 1984 Citibank was incorporated in New York and therefore faced a ceiling of 25%. It could offer a credit card to a customer in Maryland at 19.8%, despite the fact that Maryland's own issuers faced a ceiling of 18%. In practice, this nuance of regulation was unimportant in the early to mid-1980s, because nearly all issuers restricted their marketing efforts to their home states.

[10]During the sample period, states also maintained interest rate ceilings on other types of debt. However, there is little or no "clustering" at ceilings for other types of debt. This suggests that the ceilings were not binding, and also precludes the possibility that they acted as focal points for tacit collusion.

[11]The number of firm-level observations falls significantly over the sample period, from a high of 173 in 1980 to a low of 100 in 1989. This is primarily due to the fact that the number of banks participating in the survey from which the data are taken falls from 236 to 167 over the same period (not all banks report credit card rates).

[12]In the latter years of the sample, the population distribution of ceilings (across issuers) is surely weighted more heavily toward the "no ceiling" category, as many larger issuers relocated their credit card operations to ceiling-free states during the late 1980s.

were imposed at fifteen or twelve percent.[13] In response to high inflation, and also as part of a general trend toward deregulation, in the early 1980s many states chose to remove or raise their interest rate ceilings (in the empirical work below, we discuss the possible endogeneity of these changes). From 1981 to 1984, the percentage of states with no ceiling or a ceiling above eighteen percent rose dramatically. By 1983, no state in the sample had a ceiling below eighteen percent. After 1984, the cross-sectional pattern of ceilings remains fairly static.

3.2 Pricing in the Credit Card Market, 1979-1989

During the sample period, between four and six thousand banks issued credit cards. For the purposes of this study, we consider the term "credit card" to apply only to those credit cards issued by commercial banks on the VISA and Mastercard networks. Cards issued by other networks (such as Discover) are excluded from the discussion, as are charge cards such as that issued by American Express.

Each bank had discretion over the interest rate it charged, as well as any fees. In contrast to the situation that arose during the 1990s, during our sample period nearly every card issuer charged a "fixed rate" that was not pegged to any market rate. Moreover, during the 1980s the functional characteristics of credit cards themselves were still fairly homogeneous. Frequent flyer plans, rebates and cash back plans, affinity (co-branding) and other loyalty inducements were uncommon.

The most striking aspect of credit card pricing during the 1980s is the extent of "clustering" at certain interest rates; we discuss this in detail below. A corollary of this clustering is stickiness of rates, which stemmed from the infrequency with which card issuers adjusted their interest rates. For example, in our sample the average "spell" during which a given issuer's credit card rate remains unchanged is more than five years. These two factors seem puzzling, because they seem to defy conventional notions of pricing in competitive markets.[14] It is certainly true that no other loan market displays similar pricing patterns during the same time period.

The clustering and stickiness of rates attracted the attention of lawmakers, academics and antitrust authorities concerned about the level of competition among card issuers. Members of Congress at various times implied that issuers were engaging in tacit or explicit collusion.[15] Con-

[13]The only exception to this pattern is Arkansas, which capped credit card rates at 5% above the discount rate.

[14]Of course, "clustering" of prices at particular levels can indicate either competition or collusion. The argument that clustering in the credit card market is suspicious is based on the fact that similar clustering is not observed in other loan markets.

[15]The American Banker (October 10, 1991, p.2) quotes Rep. Charles Schumer (D-NY) as saying "It is virtually impossible, if a free market was working, that [interest rates for] the five largest would be exactly 19.8%."

sumer groups (such as that quoted in the introduction) accused issuers of exploiting their customers. Ausubel (1991) noted that the stickiness of interest rates might imply a "failure of competition." In California, the state Attorney General brought price-fixing charges against three of the state's largest credit card issuers. The suit alleged explicit collusion on interest rates by First Interstate Bancorp, Wells Fargo and Bank of America between 1982 and 1986. First Interstate and Wells Fargo settled and agreed to pay $55 million in damages, while Bank of America was acquitted at trial. Another suit in Chicago, again alleging direct price-fixing, was dismissed in the early 1980s.

An examination of interest rate ceilings and pricing in our sample reveals that both clustering and stickiness are explained by the fact that throughout the sample period, most issuers set rates that matched the price cap of their home state. Table 2 describes this broad pattern of interest rate clustering at caps. Our interest rate data are taken from the *Quarterly Report of Rates of Selected Direct Consumer Installment Loans*, a survey collected quarterly by the Federal Reserve Board. Banks voluntarily participate in the survey.[16]

In states with ceilings, well over eighty percent of issuers match their ceiling in the early years of the sample. The clustering is most pronounced in states with ceilings at 18 percent (the most common ceiling). It is interesting to note that in the early years of the sample, clustering is more pronounced in states with ceilings at 18 percent than in states with ceilings below 18 percent. The extent of clustering falls over time, but still remains significant at the end of the sample period.

The extent of clustering naturally implies a strong relationship between state-level variation in ceilings and cross-sectional variation in interest rates. To illustrate this, in the second-to-last row of the table we present the R-squared figures from a series of year-by-year cross-sectional regressions with the issuer-level interest rate as the dependent variable, and the level of the interest rate ceiling faced by the issuer as the only independent variable.[17] The R-squared measure including only the interest rate ceiling is roughly 0.40 in the early years of the sample, and falls by the end.

Our other primary finding in this section is that while interest rates ceilings are an important state-level determinant of variation in interest rates, other state-level characteristics play a primary role in explaining cross-sectional variation in interest rates. To illustrate this, in the last row of the table we show the R-squared figures from a series of year-by-year regressions including only a set of fixed state effects. The R-squared figures from these regressions are significantly higher than those from the regressions including only the ceiling; in most cases they are greater than 0.70.

[16]Most of the banks in our sample are smaller issuers of credit cards. Thus, the sample and our results are not necessarily representative of the behavior of the large, nationally marketed issuers (although we do discuss their pricing in the conclusion).

[17]These regressions exclude observations for issuers in states without ceilings.

This point is also illustrated in Figure 1, which summarizes within-state patterns of clustering at ceilings. The figure presents histograms of the state-level share of issuers at the ceiling in each year, by categories based on the level of the ceiling.[18] The histograms show a significant fraction of states in which all issuers match the ceiling. Another striking aspect of the figure is that in the majority of states and years, the fraction at the ceiling is either zero or one; thus, in most states clustering is either completely absent or unanimous.[19] This suggests that sample-wide variation in clustering is driven more by parallel changes in the clustering of issuers in a given state over time rather than by within state cross-sectional variation in issuer behavior in a given state-year. Thus, it appears that within-state variation in issuer costs is not the factor driving issuers to match the ceiling (or not match the ceiling).

Our finding that state-level factors explain most cross-sectional variation in credit card interest rates is not particularly surprising given the nature of competition in credit cards during the 1980s. Particularly during the early part of the decade, competition between credit card issuers occurred primarily at the state level. While issuers were able to export credit cards across state lines, very few did so. Evidence of this more regional orientation is indicated by the fact that during the 1980s the vast majority of credit card customers held cards issued by a bank in their home state. For example, a 1984 Survey by Synergistics Research Corp. quoted in the *American Banker* notes that only 8-9 percent of customers with incomes above $15,000 held a card from an out of state bank. In concert with the fact that much of the regulatory and antitrust activity regarding credit card issuers during this period took place at the state level, this evidence suggests that the relevant level for testing for tacit collusion is the state.

In order to provide some preliminary insight into the relationship between pricing and interest rate ceilings, in the next section we discuss some summary data on credit card interest rates, price ceilings and other loan rates.

3.3 Credit Card Rates, Auto Loan Rates and Interest Rate Ceilings

In this section, we present summary data that shed light on patterns of pricing across states and firms. While they are not conclusive, these data address the most plausible alternative explanation for "clustering" at ceilings, which is simply that ceilings are binding. All else equal, in our sample

[18] The unit of observation is the state-year. Thus, each data point describes the fraction of issuers pricing at the ceiling, within a particular state in a given year. There are 401 observations (state-years) in the data.

[19] In constructing the figure, we omit any state-years with only one observed issuer (for which the fraction must be zero or one). This avoids biasing the histograms toward the tails.

this would imply two patterns in prices. First, if clustering represents a constraint on issuer behavior, rates should be lower in states with ceilings than in states without ceilings. Second, if high-cost issuers are more likely to price at ceilings than low-cost issuers because ceilings bind the high-cost issuers, interest margins should be lower for issuers at ceilings (who have higher costs and are constrained) than for issuers below ceilings.

Table 3 presents average interest rate data for the sample period. The first row shows average credit card interest rate data for the banks in our sample. The average interest rate rises from 1979 to 1983, then gradually falls over the remainder of the sample period. Below, we show the average auto loan rate, which follows a similar pattern but is much less sticky than the credit card series.

In order to assess the claim that rates should be lower in states with ceilings, we present the credit card rate data stratified by level of ceiling (C). Not surprisingly, rates are lowest in the states with the most restrictive ceilings ($C < 18\%$). What is somewhat surprising is that for much of the sample period, rates are somewhat higher in states with relatively high ceilings ($C > 18\%$) than in states without any ceiling. We would expect that if ceilings' only effect were to place a constraint on pricing, that all else equal prices would be lower in states with ceilings than without.

The second set of rows shows auto loan rates for the banks in our sample.[20] Auto loans track other interest rates in the economy quite closely, including the cost of funds for banks. It is therefore likely that this auto loan rate is correlated with marginal cost at the issuer level. As the table reveals, there is little difference in auto loan rates across states with different ceilings. Thus, it seems unlikely that credit card rates are higher in states with high ceilings simply because issuers in these states have higher costs.

The next rows report the margin between credit card interest rates and auto loan rates. Because the auto loan rank captures issuer-specific costs, this gap should be correlated with the interest rate margin on credit cards. The primary rows show the average gap in states with ceilings of eighteen percent, in states with ceilings greater than eighteen percent, and in states with no ceiling.[21] In nearly every year, this gap is highest in states with ceilings greater than eighteen percent. The gap is lower in states with no ceiling, and still lower in states with an eighteen percent ceiling. This contradicts the notion that the margin should be lower in states with ceilings than in states without ceilings.

The sub-headings in the last set of rows provide more information, by comparing the credit card-auto loan gap for issuers at the ceiling ($p = C$) to the gap for issuers pricing below the

[20]The auto loan rate is that on a 36-month new car loan.

[21]For this part of the table, we omit ($C < 18\%$) because there are so few observations in that category.

ceiling ($p < C$), within states with a given ceiling. In both cases ($C = 18\%$ or $C > 18\%$), the gap is significantly higher for banks at the ceiling than for banks below the ceiling. Again, this is surprising. We would imagine that if interest rate ceilings imposed a constraint, and the auto loan rate captured variations in marginal cost over time and across issuers, then the gap would be greater for issuers below the ceiling than for issuers at the ceiling.

As a concluding point, while it is likely that variation in the auto loan rate captures a component of variation in marginal cost at the issuer level, it is certainly possible that there are unobserved components of marginal cost that are driving issuers to the ceiling. However, this seems unlikely given our discussion above of Figure 1, which suggests that very little variation in pricing is driven by cross-issuer differences within particular states.

4 Specification of the Model

In this section, we develop the empirical framework for our empirical tests. We begin by specifying a simple reduced-form equation describing a card issuer's interest rate in the absence of a ceiling. We then incorporate the possibility that an issuer may face a binding ceiling, by allowing interest rate observations to be censored at the ceiling. In order to test for tacit collusion, we then extend the model to allow an issuer to match the ceiling even when it is not binding. Our full specification estimates the issuer-level probability of tacit collusion at the ceiling, by allowing the probability of matching although the ceiling is not binding to vary based on issuer- and state-specific factors. This also allows us to estimate the relationship between these factors and the facilitative power of the ceiling. After presenting the empirical framework, we describe the variables included in the pricing equations, and discuss some econometric issues.

4.1 Baseline Specifications

Consider a reduced-form pricing equation describing an issuer's interest rate in the absence of a price ceiling, p^*_{it}:

$$p^*_{it} = X_{it}\beta + \mu_j + \delta_t + \varepsilon_{it}, \quad \text{with} \quad \varepsilon \sim N(0, \sigma). \tag{1}$$

The vector X_{it} includes issuer- and state-specific cost, demand and market structure variables; μ_j and δ_t are sets of fixed effects varying by state j and year t.[22] For issuers in states without

[22] Including fixed state effects restricts the sample to include only states for which some observations of p_{it} are below the ceiling. This eliminates two states from the analysis (the descriptive statistics presented earlier reflect this

ceilings, our observation of the data will simply be $p_{it} = p^*_{it}$.[23]

For issuers facing a ceiling, the ceiling may be binding. This will censor observations of the price variable. If it does not change anything else about the pricing relationship, we will observe:

$$p_{it} = \begin{cases} p^*_{it} \text{ if } p^*_{it} = X_{it}\beta + \mu_j + \delta_t + \varepsilon_{it} < C_{jt} \\ C_{jt} \text{ otherwise.} \end{cases} \tag{2}$$

Combining observations for issuers in states with ceilings and without ceilings yields the following likelihood function:

$$\begin{aligned} L \;=\; & \prod_{I^{ceil}_{jt}=1} \left[\prod_{p_{it}=C_{jt}} \Phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) \prod_{p_{it}<C_{jt}} \sigma^{-1}\phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right)\right] \\ & \cdot \prod_{I^{ceil}_{jt}=0} \sigma^{-1}\phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right). \end{aligned} \tag{3}$$

This baseline specification combines a Tobit model for the observations in states with ceilings, and ordinary least squares for the observations in states without ceilings. The indicator I^{ceil}_{jt} takes on a value of one for issuers that face price ceilings.

It is worth noting the assumptions underlying this specification. The standard Tobit model in the specification above implicitly assumes that the limit observations (those for which $p^*_{it} = C_{jt}$) are drawn from the same distribution as the non-limit observations. If in fact some issuers at the limit are tacitly colluding (meaning that $p^*_{it} < C_{jt}$, but that $p_{it} = C_{jt}$), the coefficients in the Tobit specification will be biased.[24]

omission). The results without fixed state effects (that use the larger sample) show slightly stronger support for the focal point hypothesis.

As an additional point, we can not use fixed issuer effects because doing so would eliminate from consideration any issuer that matched its ceiling for the entire sample. This would raise profound sample selection concerns.

[23] Our specification does not consider the possibility that tacit collusion may persist in states that have eliminated their ceilings. This biases our results against a finding of tacit collusion in states with ceilings.

[24] One alternative to this specificiation is a truncated regression model, which uses only the nonlimit observations (but still accounts for the selection bias due to censoring). The truncated model may be less vulnerable to bias if issuers at price caps are tacitly colluding, because it ignores these observations. However, the truncated model only will yield unbiased estimates of the coefficients β if the probability of tacit collusion is uncorrelated with the right-hand side variables in the pricing relationship, and if tacit collusion by some issuers leaves the pricing relationship for non-limit observations unchanged. Because we later find that the probability of tacit collusion is correlated with several right-hand side variables, we see no reason to prefer the truncated model to the Tobit.

The specification above also imposes the restriction that the pricing relationship for an issuer below the ceiling is unaffected by the presence of a price ceiling. In previous versions of the paper, we allowed the pricing relationship to differ for issuers in states with and without ceilings, and also allowed the pricing relationship to vary based on the level of the ceiling.[25] None of these other specifications yielded results that differed materially from those presented here, regarding either the existence of tacit collusion or its net effect on prices.

4.2 Modeling Tacit Collusion

In order to allow for the possibility that issuers may tacitly collude at the ceiling, we extend the model. Our approach allows for the possibility that an issuer sets its rate at the ceiling ($p_{it} = C_{jt}$) even though its desired price from the pricing equation is less than the ceiling ($p^*_{it} < C_{jt}$).

We begin by defining an indicator of tacit collusion:

$$w_{it} = \begin{cases} 1 \text{ if issuer } i \text{ is tacitly colluding at time } t, \\ 0 \text{ otherwise.} \end{cases} \tag{4}$$

We can describe the data-generating process for prices in the following way: In states without ceilings, each issuer sets a price equal to its desired price, $p_{it} = p^*_{it} = X_{it}\beta + \mu_j + \delta_t + \varepsilon_{it}$. In states with ceilings, issuers for which the ceiling is binding ($p^*_{it} \geq C_{jt}$) match the ceiling. Among the issuers for which the ceiling is not binding, some issuers may tacitly collude ($w_{it} = 1$), and set $p_{it} = C_{jt}$. The remaining issuers do not tacitly collude, and set a price equal to their desired price. More formally,

$$p_{it} = \begin{cases} p^*_{it} \text{ if } I^{ceil}_{jt} = 1,\ p^*_{it} < C_{jt}, \text{ and } w_{it} = 0; \\ p^*_{it} \text{ if } I^{ceil}_{jt} = 0 \\ C_{jt} \text{ otherwise.} \end{cases} \tag{5}$$

A simple way to allow for tacit collusion is to model w_{it} as the outcome of a latent process determining the viability of tacit collusion. If the latent process is completely unobservable and uncorrelated with the observable variables, we can model w_{it} as a random variable taking on the value 1 with probability α and 0 with probability $(1 - \alpha)$. Doing so yields the combined likelihood function for the data,

[25]Our simplest alternative specification involved including the dummy variable I^{ceil}_{it} in the pricing relationship. We also estimated the model with interation terms $I^{ceil}_{it} \cdot C_{jt}$ and $I^{ceil}_{it} \cdot (C_{jt})^2$ in the pricing relationship. In other specifications, we allowed the β coefficients to differ across issuers in states with and without ceilings.

$$
\begin{aligned}
L \;=\; & \prod_{I_{jt}^{ceil}=1} \prod_{p_{it}=C_{jt}} \left\{ \Phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) + \alpha\Phi\left(\frac{C_{jt} - X_{it}\beta - \mu_j - \delta_t}{\sigma}\right)\right\} \\
& \cdot \prod_{p_{it}<C_{jt}} \sigma^{-1}(1-\alpha)\phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right)\Bigg] \cdot \prod_{I_{jt}^{ceil}=0} \sigma^{-1}\phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right).
\end{aligned} \tag{6}
$$

The model yields an estimate of α that is constant across all issuers and time periods. This model falls within the class of "bivariate" approaches to Tobit modeling, in which the probability of observing a limit observation is determined distinctly from the level of the dependent variable (p_{it}^* in our case). The particular specification above is known as the "p-Tobit" because it estimates a single probability that an observation that should be below the limit is in fact observed at the limit. The model was first presented by Deaton and Irish (1984) in an attempt to explain the underreporting of British tobacco expenditures, and has typically been applied in similar contexts in labor economics.[26]

The p-Tobit specification above is a general distribution-based test for tacit collusion. It is therefore vulnerable to the criticism mentioned in the literature review that a result in favor of tacit collusion may simply reflect specification error. In order to deal with this possibility, we allow α to be a function of covariates that should be correlated with the sustainability of tacit collusion. This not only increases the burden upon any alternative explanation based on specification error, but also sheds further light on the factors influencing tacit collusion.

Introducing covariates involves estimating a set of specifications using the "double-hurdle" model proposed by Cragg (1971), in which the indicator w_{it} for issuer i in state j in time t is determined as the binary outcome of a latent variable w_{it}^*, where $w_{it}^* = Z_{it}\gamma + \eta_j + \lambda_t + \upsilon_{it}$ and

[26] The development and application of p-Tobit and double hurdle models in labor economics stem from two problems with consumer expenditure survey data. The first is that respondents consistently under-report consumption of some goods (alcohol and tobacco being notable examples). In this case, the "p-Tobit" model estimates the probability that a respondent did in fact consume some positive (non-limit) quantity of the good, but reported a zero (limit) quantity. A second application of the "p-Tobit" model is to estimation of consumption functions with durable goods. For these goods, purchases are infrequent and the data will contain zero expenditures for many households with positive consumption of the good in question (say, an automobile). In this instance, one can model the probability in the "p-tobit" model as the frequency of purchase (and scale the other coeffiicients in the consumption function by α). In essence, both applications of the model are designed to handle situations in which there are "too many" limit observations in the data. Our situation is most analogous to the first mentioned above; if issuers tacitly collude, there will be "too many" issuers matching their ceilings. See Maki and Nishiyama (1996) and Blundell and Meghir (1987) for applications of the "p-Tobit" model.

$\upsilon_{it} \sim N(0,1)$. The probability that a given issuer tacitly colludes is then $\alpha_{it} = \Phi(-Z_{it}\gamma - \eta_j - \lambda_t)$. Incorporating this into the likelihood function above yields:

$$
\begin{aligned}
L \;=\; & \prod_{I_{jt}^{ceil}=1} \left[\prod_{p_{it}=C_{jt}} \left\{ \Phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) \right. \right. \\
& \left. + \Phi(-Z_{it}\gamma - \eta_j - \lambda_t)\, \Phi\left(\frac{C_{jt} - X_{it}\beta - \mu_j - \delta_t}{\sigma}\right) \right\} \\
& \left. \cdot \prod_{p_{it}<C_{jt}} \sigma^{-1} \Phi(Z_{it}\gamma + \eta_j + \lambda_t)\, \phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right) \right] \\
& \cdot \prod_{I_{jt}^{ceil}=0} \sigma^{-1} \phi\left(\frac{X_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right)
\end{aligned}
\tag{7}
$$

Note that we can view the p-Tobit model as a special case of the double-hurdle, in which $\alpha = \Phi(-\lambda)$, where λ is a constant.[27]

To summarize, our model begins with a simple formulation of an issuer's non-cooperative price. It allows this pricing equation to differ for issuers in states with ceilings and without ceilings. We then allow for the possibility that the data may be censored because issuers face ceilings that are binding. Finally, we broaden the model to explicitly allow for tacit collusion and to estimate the probability that a given issuer is tacitly colluding. We now describe the variables included in the model and discuss some econometric issues.

4.3 Variables

In this section, we outline the X_{it} variables included in the pricing equation, and also the variables included in the vector Z_{it}. Table 4 contains summary statistics for these variables. It presents the data stratified based on whether the observation comes from a state with or without a ceiling.[28]

The vector X_{it} in the pricing equation include variables that capture demand, costs and market structure. It includes the issuer's auto loan rate as a measure of issuer-specific costs. We also include

---

[27]Standard estimation of the p-Tobit does not restrict α to fall between zero and one, while using $\alpha = \Phi(\lambda)$ does. This is notable given that some previous studies (e.g., Deaton and Irish [1984], Maki and Nishiyama [1996]) estimate values for α that lie outside the [0,1] interval.

[28]Note that differences in descriptive statistics for observations with and without $I^{ceil} = 1$ reflect both cross-sectional differences, and differences in the general environment over time (because most instances where $I^{ceil} = 1$ occur later in the sample period).

the state-level default rate on credit card debt as an additional control for state-level costs.[29] The X_{it} vector also includes two state-level demographic variables. The first is average weekly income per capita, adjusted for inflation. The second is the state-level unemployment rate. These are included to capture variation in demand.

We also include in X_{it} the state-level Herfindahl index in credit cards.[30] Given the more regional nature of competition during this time, this variable may capture variation in the competitive environment, or measure firms' ability to tacitly collude in the absence of a focal point. Another variable included in X_{it} is the outstanding credit card loans measured at the issuer level, adjusted for inflation. This variable is somewhat difficult to interpret because loans may be positively or negatively correlated with issuer-level costs; larger issuers tend to have higher default rates, but there are also significant scale economies in credit card operations. It is also possible that larger issuers may have market power. Because both the HHI and the credit card loans variables are highly skewed, we include the logarithm of each in the X_{it} vector. The final components of the pricing equation, μ_j and δ_t, are captured by a set of fixed state and year effects.

The Z_{it} vector includes variables that should affect the sustainability of tacit collusion. It includes the level of the interest rate ceiling faced by the issuer. Under the focal point hypothesis, tacit collusion is more difficult to sustain under higher ceilings. The Z_{it} vector also includes the issuer's auto loan rate. The auto loan rate is correlated with issuer-specific costs; the higher are these costs, the easier is tacit collusion at the ceiling. The Z_{it} vector also includes the state-level HHI in credit cards. This tests whether tacit collusion is easier to sustain in markets with greater concentration. We also include the issuer's credit card loans in Z_{it}, to test the hypothesis that larger issuers are more likely to tacitly collude. The last component in Z_{it} is income; this tests the Rotemberg-Saloner prediction that tacit collusion is more difficult to sustain in periods of high demand.[31] Finally, the inclusion of fixed state and year effects in this equation captures any systematic year- or state-specific influences on the sustainability of tacit collusion.

4.3.1 Econometric Issues

Of the variables in X_{it} and Z_{it}, we would expect that the issuer's interest rate might affect both the issuer's credit card loans and the Herfindahl index; this raises endogeneity concerns. To deal

[29] Issuer-level default data are unavailable during our sample period.

[30] The HHI is measured using market shares by outstanding balances. It is constructed using the population of credit card issuers from the FDIC Call Reports.

[31] In unreported results, we also include the unemployment variable in Z_{it}. It is not statistically significant.

with this issue, we instrument for credit card loans using the issuer's total assets. Similarly, we instrument for the state-level HHI in credit card loans using the state-level HHI in total bank assets.[32]

In unreported results, we also consider the possibility that the price ceiling itself is endogenous. This would be true if ceilings were imposed in reaction to the state-level competitive environment in credit cards. While this seems unlikely, if it were true we might observe a correlation between prices and price ceilings even absent tacit collusion. To account for this, we instrument for the level of the price ceiling using a vector of banking regulation variables.[33] Results from these unreported specifications are nearly identical to those reported below.

Finally, we noted earlier in the paper that functional form is of some concern given our empirical tests. Using results from our fullest specification (model 5 below), we test our assumption of a normally distributed error using the residuals from observations in states without ceilings.[34] While we reject normality using all of the residuals, this is due to the presence of negative outliers; when we omit the six most negative residuals from the test, we do not reject the hypothesis of normality.[35]

4.4 Results

Table 5 presents the results of the above models – the Tobit, p-Tobit and double-hurdle models. We estimate the p-Tobit model by specifying a double-hurdle model in which $\alpha = \phi(-\lambda)$, where λ is a constant; the results show the estimate of λ.[36] The third column shows results from a double-hurdle specification that includes only a vector of time dummies, *i.e.* $\alpha_t = \phi(-\lambda_t)$. This

[32]It is possible that the instruments themselves are weakly endogenous. Credit card balances are a part of total assets for each bank (and by extension, concentration in credit cards is a component of concentration in banking). However, in our sample credit cards comprise less than 5% of total bank assets on average, so the endogeneity concern is greatly reduced using the instruments.

[33]There are four such variables, each of which is a dummy. The first indicates whether the state allows *de novo* bank branching. The second indicates whether the state allows bank branching through merger. The third indicates whether the state allows interstate banking restrictions. The fourth indicates whether the state allows multi-bank holding companies to operate in the state. These variables are taken from Amel and Keane (1986), and updated with information from Kroszner and Strahan (1999).

[34]Conducting a normality test on the residuals from observations with $(p < C)$ in states with ceilings is considerably more difficult. The observed distribution of residuals is truncated normal rather than normal.

[35]Our test statistic is the Shapiro-Wilk "W" statistic. The p-value associated with the statistic is 16%.

[36]Note that the regression estimates λ, and that $\alpha = \Phi(\lambda)$. The estimate of λ is in units of standard deviations away from zero in the standard normal distribution. Thus, if $\lambda = 0$, $\alpha = 0.50$; thus, a standard t-statistic is inappropriate for assessing whether tacit collusion is occurring. We can use the standard error of the coefficient to form confidence intervals, however.

allows the probability of tacit collusion to vary over the sample period. To make this latter model more parsimonious, we restrict each consecutive pair of year dummies to have equal coefficients; thus, the "year dummies" pertain to 1979-80, 1981-2, etc. The coefficients in the pricing equation for the Tobit and the models that allow for collusion are generally similar; the exception is that the auto loan rate which is not significant in the Tobit, but positive and significant in each of the other models. This suggests that the Tobit is poorly specified relative to the other models, perhaps because it does not allow for the possibility of tacit collusion.

In the pricing relationship, the coefficient on credit card loans is positive and significant in every specification; this is consistent with other work examining the relationship between issuer size and interest rates.[37] The sizes of the coefficients imply that a doubling of issuer size is associated with an interest rate ranging from 9 basis points higher (Model 3) to 20 basis points higher (Model 2); this is a fairly small effect. The coefficients on the unemployment variable suggest that rates are positively associated with demand. It is interesting to note that the coefficient on income is negative and significant in the first two specifications, but becomes positive and significant once the specification includes the Z_{it} variables (which also include income). This appears to be because high demand makes tacit collusion more difficult, which biases the coefficient in the simpler pricing relationships downward when the model does not allow income to affect the probability of tacit collusion.

Moving to the specifications that allow for tacit collusion, in Model 2 we estimate that the sample-wide probability α of tacit collusion is roughly 11.5 percent.[38] We do not place much emphasis on the economic interpretation of this coefficient. This is because in cases where the true value of α is thought to vary by observation, estimates that restrict α to be equal across all observations are typically much lower than the average value of α in specifications that allow α to vary by observation.[39] This is borne out by the results from the year-dummy double-hurdle model in the next column. In 1979 and 1980, the year-dummy double-hurdle model yields an estimate of $\alpha = 0.81$. By 1981-2 the probability falls to forty-four percent, and by 1987-8 to just over seven percent. There is a clear downward trend in the probability of collusion over the sample period.

We should also note that we estimated a standard p-Tobit specification in which α entered directly; the estimates were nearly identical to those using $\alpha = \Phi(\lambda)$.

[37]See Stango (2000) for evidence on this point.

[38]We should stress that this coefficient is interpreted as the probability of tacit collusion conditional on $(p^*_{it} < C_{it})$. Thus, if the proportion of issuers for whom $(p^*_{it} < C_{it})$ is γ, the unconditional sample probability of tacit collusion is $\alpha\gamma$. Of issuers at the ceiling, the fraction that are tacitly colluding is $\alpha\gamma/(1-\gamma+\alpha\gamma)$.

[39]Maki and Nishiyama (1996), for example, find that the p-tobit estimate of α is less than zero, but that the average value of α is roughly 0.35 in the double hurdle specification that allows α to vary by observation.

The last columns of Table 5 show results from the full double-hurdle models. These specifications include the vector of variables in Z_{it} that should influence the sustainability of tacit collusion (Models 4 and 5), as well as the time dummies (Model 5). The Z_{it} vector coefficients show a pattern that is generally consistent with our discussion of factors affecting the sustainability of tacit collusion. In every column, the coefficient on the level of the price ceiling is negative and significant, suggesting that the facilitative power of the ceiling dissipates at higher levels. The coefficient on the HHI is positive and significant, suggesting that sustaining tacit collusion at the ceiling is easier in states with higher concentration. The coefficient on the auto loan rate is positive and significant, suggesting that high-cost issuers are more likely to tacitly collude. And, the coefficient on income is negative and significant. This is consistent with the Rotemberg-Saloner prediction that tacit collusion becomes more difficult to sustain in periods of high demand. Finally, there is weak evidence that larger firms are more likely to collude.

An interesting feature of the double-hurdle results in this table is that when the covariates in Z_{it} are included, the coefficients on the time dummies change. There are no statistically significant changes in the level of the intercept between 1979 and 1984, and a downward shift beginning in 1985 that continues until the end of the sample. This suggests that there was a regime change beginning in 1985 that made tacit collusion more difficult.

In summary, the results provide fairly strong support for the focal point hypothesis. A statistically and economically significant proportion of issuers for which ceilings are not binding match them nonetheless. Furthermore, the probability of tacit collusion is negatively related to the level of the ceilings, positively related to concentration, positively related to issuer-level costs, and negatively related to demand. We also find that the probability of tacit collusion falls throughout the sample period – markedly so after 1986. In the next section we expand upon these points.

4.5 Some Further Detail on the Results

To illustrate the economic significance of the variables in Z_{it}, Figures 2 and 3 plot the predicted probability α for an average firm as a function of the price ceiling across years and also by different levels of HHI, CC loans, auto rate and income. Figure 2 illustrates the shift in issuers' propensity to collude over time. This effect is fairly dramatic. For example, during the years of 1979 through 1984, an issuer facing a price ceiling of 18 percent was twice as likely to collude (80% vs. 39%) than an issuer facing the same ceiling in 1989.[40] Figure 3 plots the predicted probability for an issuer

[40] We calculate these probabilities using means of the variables in Z_{it} measured within each two-year period. Thus, changes in the probabilities over time reflect movement both because of the year dummies, and because of changes

at the mean of each right-hand side variable. It then shows similar plots for issuers one standard deviation away from the mean in each of four right-hand side variables: HHI, credit card loans, auto rate and income.[41] In concert, these variables have an economically significant effect on the probability of tacit collusion. For example at a price ceiling of 18 percent, moving from "High HHI, High CC Loans, High Auto Rate, Low Income" to "Low HHI, Low CC Loans, Low Auto Rate, High Income" reduces the probability of tacit collusion by over sixty basis points.

To highlight the economic impact of ceilings, in Table 6 we provide some measures of their overall effects. To this point, we have discussed only the extent to which ceilings facilitated tacit collusion. But the ceilings clearly were binding for many issuers as well. The overall effects of ceilings on prices might be positive or negative, based on the relative magnitudes of these opposing influences. To construct the table, we use the coefficients from the pricing equation in Table 6 to construct fitted values $\hat{p}$ for issuer interest rates.[42] For all issuers with observed rates at ceilings ($p = C$), we classify those with predicted rates below the ceiling ($\hat{p} < C$) as having $\hat{w} = 1$, which indicates that the issuer is tacitly colluding. Issuers for which ($\hat{p} \geq C$) are classified as having $\hat{w} = 0$. The table shows the average positive price effect for issuers with $\hat{w} = 1$, the average negative price effect for issuers with $\hat{w} = 0$, and the number of issuers in each category.

The sample-wide average effect of ceilings is positive, meaning that the effect of tacit collusion outweighs the effect of binding ceilings. The magnitude of the average suggests that for issuers at ceilings, prices are roughly 100 basis points higher than they would be in the absence of tacit collusion at the focal point.[43] The average effect is positive at the beginning and end of the sample, and negative in the middle years (during which market rates were high and ceilings became binding). As would be expected, lower ceilings are more likely to be binding than higher ceilings, and issuers who match high ceilings are more likely to be identified as tacitly colluding. In general, these results are statistically significant; we can reject the hypothesis that the price effect is zero for over fifty

in the means of the variables in Z_{it}.

[41]Probabilities are measured at the mean "year effect." Note that both HHI and loans enter the regressions in logs; thus, the standard deviations are in logs as well.

Because the coefficients from this regression are best interpreted as reflecting the impact of within-state changes from variables in Z_{it}, we use within-state standard deviations in the calculations.

The marginal effects of the income and ceiling variables are largest, followed by the auto rate, loans, and HHI. The largest marginal effect (income) is roughly four times bigger than the smallest marginal effect (HHI).

[42]The fitted values include our estimates of the fixed state effects, $\hat{\mu}_j$.

[43]Recall that our comparison does not rule out the possibility that issuers might tacitly collude at a price other than the ceiling. This estimated price effect is the incremental effect of tacit collusion facilitated by non-binding price ceilings.

percent of issuers with $\hat{w} = 1$.[44]

As a final point, it is worth remarking on our finding of a marked downward shift in the probability of tacit collusion after 1985. This result is consistent with evidence regarding changes in the competitive environment in credit cards.[45] The primary change was a sharp increase in entry. From 1979-84, the average annual entry rate in credit cards was 1.3%; the average jumped to 7.7% in 1985-86.[46] Furthermore, in 1986 many of the largest nationally marketed issuers cut their rates and/or launched aggressive national marketing campaigns. These moves certainly might have reduced the state-level incentives to tacitly collude.[47] Accounting data from this time period show a sharp drop in return on credit card assets from 1985 to 1986. Finally, card managers reported during 1986 that increased publicity about high credit card interest rates had increased consumer sensitivity to high rates. In concert, all of these factors would have placed increased competitive pressure on issuers at the state level.

In the next subsection, we expand upon this point, by directly relating a state-level measure of tacit collusion to state-level entry in credit cards.

4.6 Tacit Collusion, Binding Ceilings and Entry Rates

In Table 6, we presented estimates of $C_{jt} - \hat{p}_{it}$, the "price effect" of interest rate ceilings. In essence, this price effect is our estimate of the extent to which a ceiling binds or facilitates tacit collusion; a positive price effect indicates tacit collusion, while a negative price effect indicates that the ceiling is binding. In this section, we use the issuer-level estimates to construct an annual average price effect at the state level. In state j at time t, this effect is

$$Effect_{jt} = \sum_{i \in j, p=C} \frac{C_{jt} - \hat{p}_{it}}{n_{jt}}$$

[44]The significance level of this test is 5% (one-tailed).

[45]See for example, "Credit Card Wars: Profits are Taking a Direct Hit," *Business Week* 11/17/86, p.166.

[46]We measure the entry rate as the year-to-year percentage change in number of banks with positive credit card receivables. It is therefore most properly viewed as a net entry measure that includes the effect of entry, exit, and mergers. We take data for this calculation from the Federal Reserve's Call Reports, which contain the population of commercial banks in the United States.

It is not the case that the jump in credit card entry is due to general entry into the banking sector. From 1979-84 the entry rate for banks overall averaged 0.9%. It averaged -0.1% from 1985-86.

[47]The effect of current intensified competition at the national level would reduce current profits from tacit collusion. The overall effect on the incentive to tacitly collude would depend on whether the reduction were perceived as transitory or permanent.

where n_{jt} is the number of issuers in state j at time t. While the numerator sums only over those issuers at the ceiling, the denominator averages over all issuers in the state. Effectively, this means that any issuer with $p_{it} < C_{jt}$ (or in a state without a ceiling) has a price effect of zero. Thus, the average price effect in a state will be affected by both the share of issuers that match the ceiling and the level of $C_{jt} - \hat{p}_{it}$ for those issuers that match the ceiling. The price effect is zero by construction in state-years without a ceiling.[48]

If $Effect_{jt}$ measures the degree to which a ceiling binds issuers or facilitates tacit collusion at the state level, we would expect it to be correlated with subsequent entry into credit cards. In order to test this hypothesis, we construct a state-level measure of entry, $CCEntry_{j,t+1}$. We measure entry as the net percentage change in the number of banks with positive credit card loans within a specific state. Because we think that current price effects should affect future entry, we measure our entry variable for the year following our measurement of the price effect variable. Thus, if $Effect_{jt}$ is measured in March 1981, $CCEntry_{j,t+1}$ is the percentage change in banks offering credit cards between March 1981 and March 1982.

Our empirical specification also includes two control variables. The first, $BkEntry_{j,t+1}$, measures the percentage change in the number of commercial banks in state j. This controls for changes in the number of credit card banks that are simply due to entry, exit or mergers by banks. The second variable, $BankswithCC_{jt}$, is the percentage of banks in state j that have positive credit card loans at time t. In our sample, entry into credit cards occurs because an existing bank begins to offer a credit card; it is very rare that we observe *de novo* entry by a credit card "pure-play." Thus, we expect that the percentage of banks already offering credit cards would be a measure of the potential for further entry.

Our baseline specification, which includes fixed state and year effects in addition to the variables discussed above, is

$$CCEntry_{jt} = \beta_1 Effect_{jt-1} + \beta_2 BkEntry_{jt} + \beta_3 BankswithCC_{jt} + \mu_j + \delta_t + \varepsilon_{jt} \qquad (8)$$

If our "price effect" variable accurately captures the extent to which a ceiling binds or facilitates tacit collusion, we would expect to observe a positive relationship between price effects and entry rates.

[48]The regressions presented below include both observations from states with no ceiling, and observations from states in which no issuer matches the ceiling. We see no reason to exclude these observations; in any case, doing so leaves the results unchanged.

We make two modifications to our baseline specification. The first is to allow for asymmetry in the impact of the price effect variable based on whether it is positive or negative. This allows different effects on entry of ceilings that are either binding or facilitate tacit collusion. The other modification we make is to allow the impact of our price effect variable to vary over the sample period. Because we observe a regime change in nationwide entry rates and the probability of tacit collusion in 1985, we interact the price effect variable with a dummy variable equal to one from 1985-89. Our fullest specification is:

$$\begin{aligned} CCEntry_{jt} &= \beta_1 Effect^{+}_{jt-1} + \beta_2 Effect^{-}_{jt-1} + \beta_3 Effect^{+}_{jt-1} \cdot D8589_t + \\ &\quad \beta_4 Effect^{-}_{jt-1} \cdot D8589_t + \beta_5 BkEntry_{jt} + \beta_6 BankswithCC_{jt} + \mu_j + \delta_t + \varepsilon_{jt} \end{aligned} \tag{9}$$

Table 7 reports the results of these regressions, proceeding from the simplest specification to the fullest. In every specification, the coefficient on $BkEntry_{jt}$ is positive and significant and the coefficient on $BankswithCC_{jt}$ is negative and significant; these results are consistent with our expectations. Based on likelihood ratio tests comparing columns 1-2 and 3-4, we find that we can not reject the restriction of symmetric positive and negative price effects.[49] However, tests comparing columns 1 and 3 show that we do reject the hypothesis that the price effect variable has equal effects from 1979-84 and 1985-1989. Thus, the specification in the third column is preferred.

In every specification, the coefficients on the price effect variables show a direct link between our measure of price effects and entry rates. The positive coefficients indicate that a positive price effect leads to greater net entry in credit cards, while a negative price effect leads to lower net entry. In our preferred specification, we estimate that from 1979-84, a 100 basis point price effect is associated with an entry rate 2.3% higher. Given that the sample mean entry rate is 3.2%, this is quite a large effect. The coefficient on the interaction term is negative, significant and almost exactly equal to the coefficient on the price effect variable. This suggests that after 1984, there is no relationship between our state-level measure of price effects and state-level entry patterns. Given our discussion in the previous section, we think this might reflect the growing national integration of the market. In such an instance, tacit collusion within a state would simply attract entry by a nationally marketed issuer (who would not show up in our entry measure). The wave of entry in 1985 and 1986 would then have weakened both the incentives for within-state tacit collusion and the link between tacit collusion and entry.

[49]The p-value for the test of Model 2 against Model 1 is 0.16, and the p-value for the test of Model 4 against Model 3 is 0.64.

5 Discussion

In this section, we discuss some alternative explanations for the results. To clarify the discussion, we point out that our primary empirical result is that the data identify two distinct price distributions for issuers. Features of the first distribution are captured by the pricing relationship. We maintain the assumption that this primary distribution is one in which price ceilings are not facilitating tacit collusion. We then conclude that the second (point) distribution identified by the data is the distribution of observations for which the focal point facilitates tacit collusion.

Corroborative evidence in support of this conclusion comes from the fact that our set of covariates thought to affect the sustainability of tacit collusion – the variables in Z_{it} – plausibly affect the likelihood that an observation is drawn from the second distribution. The entry regressions also support this interpretation of our results.

Despite these findings, an alternative explanation for the results might be that our pricing relationship is subject to measurement error or omits important variables, leading to the spurious identification of separate price distributions. One plausible candidate for this measurement error is that using the issuer's interest rate to measure its "price" is inaccurate because it excludes the issuer's annual fee. If issuers traded higher interest rates for lower annual fees, we would misidentify issuers with low annual fees as engaging in tacit collusion.[50] However, this is unlikely because issuer-level data from the credit card market generally show a positive relationship across issuers between interest rates and annual fees.[51]

Another way in which measurement error could affect the results is through unobserved issuer-level differences in costs. In such an instance, we would mistakenly identify high-cost issuers as tacitly colluding. While we acknowledge that this possibility should lead to a cautious interpretation of our summary evidence in Table 3, we find it much less likely that it could explain the results from our full specifications. First, we should note that if unobservable issuer-level costs are normally distributed and uncorrelated with the variables in X_{it}, they will be captured in the error term of the Tobit model. Second, to explain the results from our fullest specifications, costs that are unobserved in the pricing equation would have to be correlated with each variable in Z_{it} in a way consistent with our empirical results. This seems particularly unlikely given that nearly all of the

[50] On average, annual fees made up roughly 15% of total revenue for issuers in our sample. Other components of "price" such as late fees, and over-limit fees were a trivially small component of issuer revenue during this period.

[51] Unfortunately, issuer-level data on interest rates and fees are only available after 1989. However, data taken from the Card Industry Directory from 1989-1994 show a raw correlation of 0.25 between interest rates and annual fees for the 250 largest issuers in the country.

variables in Z_{it} are also in X_{it}. There is one variable in Z_{it} that is not in X_{it} (the price ceiling), but it is difficult to think of an explanation for why the state-level interest rate ceiling would be negatively correlated with the unobservable issuer-level component of costs.

A somewhat different alternative explanation for the results might be that issuers face menu costs in changing their rates. If issuers were forced to the ceiling when it was binding, they might maintain rates at their ceiling when their one-shot rate had fallen below the ceiling. This seems plausible given that rates in the economy were generally falling during our sample period. The source of menu costs also seems plausible. During our sample, issuers applied rate cuts to all current outstanding balances, as well as any future balances. Thus, the cost of cutting rates would be the forgone interest income on current outstanding balances. However, the menu cost argument seems implausible based on the results from the double-hurdle regressions. There is no reason to believe that issuers would face higher adjustment costs in more concentrated states, because they face higher ceilings, or in states with higher income.

6 Conclusions

The finding that a non-binding price ceiling may facilitate tacit collusion has important policy implications. For example, price caps recently have been imposed in the electricity industry to curb market power during peak demand periods.[52] However, the high day-to-day variance of electricity demand implies that these price caps frequently will be non-binding. Our results imply that any welfare analysis of the caps should consider the possibility that firms might use them to facilitate tacit collusion. This is particularly important given the degree of distortion in market outcomes that we observe as a consequence of ceilings; they affect not only pricing, but also patterns of entry.

Our results have particular relevance to researchers interested in the credit card market, because they explain a long-standing puzzle in credit card pricing - the stickiness of interest rates during the 1980s, and clustering at particular rates. We must point out, however, that while our results go far in explaining credit card pricing during the 1980s, they have little relevance in explaining credit card pricing in the 1990s - a decade that saw a vast increase in inter- and intra-issuer variance in pricing.

As a concluding point, we note that our paper in some sense only examines half of the focal point issue. We primarily seek to establish the existence of tacit collusion; we do not focus on the processes by which firms move from one tacitly collusive equilibrium to another, or achieve such

[52]They have been extensively used in California and the Pennsylvania, New Jersey and Maryland market.

an equilibrium in the first place. We have some suggestive evidence on this point, however. In preliminary work we have examined changes in credit card rates, in both states that eliminated ceilings and states that simply raised their ceilings. Controlling for changes in auto loan rates, we find that rates rise by more in states that raise their ceilings than in states that eliminate them entirely.[53] This suggests that future work may be able to capture some dynamics of tacit collusion at focal points.

[53]We also estimate logit models that show that the probability that an issuer raises its rate is greater in a state that has raised its ceiling than in one that has eliminated its ceiling.

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A The Sustainability of Tacit Collusion at a Focal Point

In this section, we provide an illustrative example regarding the sustainability of tacit collusion at a focal point. Our example simplifies matters by considering competition between two firms. We also assume that costs are zero for both firms. The results should easily generalize to settings with multiple firms and non-zero costs.

A.1 Notation

Define the general profit function

$$\Pi_1(p_1, p_2) \tag{A1}$$

indicating that firm 1's profits are a function of its own price and the price charged by the other firm. We will be interested in evaluating this function and its derivatives, both for general values of p_1 and p_2 and also particular values of each price. For example, we can represent firm 1's profits when both firms match the ceiling as

$$\Pi_1(C, C) \tag{A2}$$

where C is the level of the price ceiling.

Define

$$p_1^*(p_2) = \arg\max_{p_1} \Pi_1(p_1, p_2) \tag{A3}$$

as firm 1's short-run best response to firm 2's price. Thus, we can represent firm 1's profits when it plays its best response to firm 2's price at the ceiling by

$$\Pi_1(p_1^*(C), C) \tag{A4}$$

and the noncooperative profits as

$$\Pi_1(NC, NC) \tag{A5}$$

where

$$NC = p_1^*(NC),\ NC = p_2^*(NC) \tag{A6}$$

For simplicity we assume that

$$\Pi_1(NC, NC) = 0 \tag{A7}$$

Note that all of the above can also be represented using equivalent notation in terms of prices and demand, where

$$\Pi_1(p_1, p_2) = p_1 D_1(p_1, p_2) \tag{A8}$$

A.2 Assumptions

We maintain three assumptions regarding competition:

(1) The two goods are substitutes:

$$\frac{\partial D_1(p_1, p_2)}{\partial p_2} > 0 \tag{A9}$$

(2) Prices are strategic complements:

$$\frac{\partial p_1^*}{\partial p_2} > 0 \tag{A10}$$

(3) The own-price elasticity of demand is greater in absolute value than the cross-price elasticity of demand:

$$\frac{\partial D_1(p_1, p_2)}{\partial p_2} < -\frac{\partial D_1(p_1, p_2)}{\partial p_1} \tag{A11}$$

A.3 Changes in the Sustainability of Tacit Collusion

We are interested in examining changes in the sustainability of tacit collusion as the ceiling changes. This sustainability will depend on the profits from cooperation and the profits from cheating. We can represent changes in the ceiling as being equivalent to change in p_2, under the assumption that firm 2 tacitly colludes by matching the ceiling.

Consider first the simple case in which $C = NC$ - that is, the ceiling is set at the noncooperative price. Obviously, sustaining tacit collusion is trivially easy in this situation. We can note, however, that when $C = NC$:

$$\frac{d\Pi_1(p_1^*(NC), NC)}{dp_2} > \frac{d\Pi_1(NC, NC)}{dp_2} \tag{A13}$$

That is, for small changes in the ceiling, the change in profits from cheating is more positive than the change in profits from cooperation. This means that cheating becomes incrementally more attractive than cooperation, as the ceiling rises slightly above the noncooperative price. The intuition behind this is fairly clear: the left-hand side of the above expression allows firm 1 to re-optimize based on the change in p_2, while the right-hand side does not because it constrains firm 1 to maintain $p_1 = C$.

Knowing that cheating becomes more attractive as the ceiling rises around $C = NC$, we then address the issue of whta happens beyond that point. To look at this more closely, we examine the second derivatives of profits under cooperation and cheating.

The second derivative of the profit function under cheating is simpler, so we write it first:

$$\frac{d^2\Pi(p_1^*(C), C)}{dp_2^2} = \frac{\partial p_1^*}{\partial p_2}\frac{\partial D_1(.)}{\partial p_2} + p_1^*\frac{\partial^2 D_1(.)}{\partial p_2^2}$$

We note that some terms have dropped out because of the envelope theorem results that at the short-run best response,

$$\frac{\partial \Pi(p_1^*(C), C)}{\partial p_1} = 0$$

Here we make the simplifying assumption that the second derivatives of demand are zero. This means that

$$\frac{d^2\Pi(p_1^*(C), C)}{dp_2^2} = \frac{\partial p_1^*}{\partial p_2}\frac{\partial D_1(.)}{\partial p_2}$$

This expression is strictly positive by assumptions (1) and (2) - that the goods are substitutes and prices are strategic complements.

The second derivative of profits under cooperation with respect to the ceiling is:

$$\frac{d^2\Pi(C, C)}{dp_2^2} = C\left[\frac{\partial^2 D_1(.)}{\partial p_1^2} + 2\frac{\partial^2 D_1(.)}{\partial p_1 \partial p_2} + \frac{\partial^2 D_1(.)}{\partial p_2^2}\right] + 2\left(\frac{\partial D_1(.)}{\partial p_2} + \frac{\partial D_1(.)}{\partial p_1}\right)$$

Again, if the second derivatives of demand are zero the expression simplifies to:

$$\frac{d^2\Pi(C, C)}{dp_2^2} = 2\left(\frac{\partial D_1(.)}{\partial p_2} + \frac{\partial D_1(.)}{\partial p_1}\right)$$

This is strictly negative by assumption (3), that the own-price elasticity of demand is greater than the cross-price elasticity of demand. Thus,

$$\frac{d^2\Pi(p_i^*(C), C)}{dp_2^2} > 0 \text{ and } \frac{d^2\Pi(C, C)}{dp_2^2} < 0$$

Along with the first result, that

$$\frac{d\Pi_1(p_1^*(NC), NC)}{dp_2} > \frac{d\Pi_1(NC, NC)}{dp_2} \tag{10}$$

this implies that

$$\frac{d\Pi(p_i^*(C), C)}{dp_2} > \frac{d\Pi(C, C)}{dp_2}$$

for any $C > NC$. Thus, the profits from cheating rise faster than the profits from cooperation.

B Tables

Table 1: Credit Card Interest Rate Ceilings, 1979-1989

Year	**1979**	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
N (States):	37	41	40	37	35	36	36	40	37	35	31
Share of States:											
No Ceiling	6%	5%	10%	23%	33%	38%	37%	38%	37%	34%	33%
$C > 18\%$	3%	5%	15%	23%	30%	29%	37%	38%	34%	34%	33%
$C = 18\%$	81%	83%	69%	51%	36%	32%	26%	24%	26%	28%	33%
$C < 18\%$	11%	7%	5%	3%	–	–	–	–	3%	3%	–
N (Issuers):	171	173	164	147	135	133	124	129	116	115	100
Share of Issuers:											
No Ceiling	2%	2%	7%	14%	26%	34%	33%	34%	34%	33%	35%
$C > 18\%$	4%	6%	19%	22%	30%	31%	32%	31%	31%	30%	30%
$C = 18\%$	85%	84%	70%	63%	44%	35%	35%	35%	34%	37%	35%
$C < 18\%$	9%	8%	5%	1%	–	–	–	–	1%	1%	–

Sources: *The Cost of Personal Borrowing in the United States* and *Quarterly Report of Rates of Selected Direct Consumer Installment Loans*, various issues.

Table 2: Credit Card Pricing and Interest Rate Ceilings

	1979	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
Share of Issuers at Ceiling:											
All Facing Ceiling	85%	85%	76%	76%	65%	58%	59%	51%	38%	31%	28%
$C > 18\%$	0%	9%	13%	19%	25%	27%	30%	23%	8%	9%	7%
$C = 18\%$	89%	91%	92%	97%	92%	85%	86%	76%	64%	48%	46%
$C < 18\%$	75%	79%	100%	50%	–	–	–	–	100%	100%	–
R-Squared:											
Level of Ceiling	0.32	0.39	0.33	0.61	0.42	0.49	0.47	0.30	0.31	0.28	0.24
Fixed State Effects	0.58	0.66	0.69	0.77	0.72	0.70	0.76	0.69	0.73	0.57	0.57

Notes: Table excludes observations from states without ceilings. R-squared figures are from year-by-year regressions using issuer interest rate as the dependent variable. *Fixed state effects* specification includes a dummy variable for each state in the sample. *Level of ceiling* specification includes a constant term and the level of the interest rate ceiling in the issuer's home state.

Table 3: Average Interest Rates by Ceiling

	1979	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
Avg CC Rate (%):	17.0	17.3	17.8	18.3	18.8	18.8	18.8	18.6	18.1	17.8	17.8
No Ceiling	16.0	16.0	18.7	18.8	19.5	19.0	19.1	19.1	18.5	18.1	18.1
$C > 18\%$	17.5	18.6	18.7	19.4	19.7	19.7	19.7	19.3	18.6	18.4	18.3
$C = 18\%$	17.5	17.6	17.7	17.9	17.7	17.7	17.7	17.4	17.2	17.1	17.1
$C < 18\%$	13.2	13.3	14.7	14.0					15.0	15.0	
Avg Auto Rate (%):	11.8	13.4	16.0	17.1	14.7	13.4	13.4	12.3	10.4	10.8	11.9
No Ceiling	11.8	13.9	16.9	18.1	14.6	13.2	13.4	12.3	10.3	10.8	11.9
$C > 18\%$	11.2	12.8	16.3	17.4	15.2	13.8	13.7	12.6	10.4	10.8	12.0
$C = 18\%$	11.8	13.6	15.9	16.8	14.5	13.2	13.2	12.2	10.5	10.7	11.8
$C < 18\%$	11.5	12.5	13.9	14.0					11.0	11.0	
Avg Gap (%):	5.3	3.9	1.8	1.2	4.0	5.4	5.4	6.2	7.7	7.0	5.9
No Ceiling	4.2	2.1	1.8	0.7	4.9	5.9	5.7	6.8	8.2	7.3	6.2
$C > 18\%$	6.3	5.8	2.3	2.0	4.5	5.9	6.1	6.7	8.2	7.6	6.4
$p = C$	–	7.5	7.8	5.2	6.9	8.1	8.2	8.9	11.2	10.7	8.3
$p < C$	6.3	5.6	1.5	1.2	3.7	5.2	5.2	6.0	7.9	7.2	6.2
$C = 18\%$	5.6	4.0	1.8	1.1	3.2	4.5	4.5	5.2	6.7	6.3	5.3
$p = C$	6.2	4.4	2.0	1.2	3.5	4.8	4.7	5.8	7.8	7.5	6.4
$p < C$	1.5	0.3	-1.0	-1.4	0.1	2.7	3.2	3.6	4.7	5.3	4.3

Sources: *The Cost of Personal Borrowing in the United States* and *Quarterly Report of Rates of Selected Direct Consumer Installment Loans*, various issues.

Table 4: Descriptive statistics for the variables used in the regressions

	Entire Sample				$I^{ceil} = 0$		$I^{ceil} = 1$	
	Mean	**Std Dev**	**Min**	**Max**	**Mean**	**St Dev**	**Mean**	**St Dev**
Credit Card Rate	17.98	1.80	11.50	22.00	18.74	1.64	17.78	1.78
Price Ceiling†	19.13	2.51	12.00	25.00	—	—	19.13	2.51
Auto Rate	13.25	2.29	7.00	23.00	12.82	2.24	13.37	2.29
HHI in Credit Cards	1758	1495	203	9949	1845	1869	1735	1378
CC Loans‡($1000*s*)	464	1534	0	28184	576	2603	512	1390
Default Rate	2.21	0.84	0.53	5.35	1.89	0.68	2.29	0.86
Unemployment Rate	7.31	2.40	2.40	18.00	6.81	2.23	7.43	2.43
Weekly Income‡ ($100s)	5.89	0.15	5.50	6.31	5.92	0.13	5.89	0.15
Sample Size	1507				316		1191	

† Mean of observations for which $I^{ceil} = 1$. ‡ Reported in 1983-1984 dollars.

Table 5: Models of Pricing and Tacit Collusion

	Model 1	Model 2	Model 3	Model 4	Model 5
Pricing Equation:					
Auto Rate	0.102	0.106**	0.184***	0.090**	0.074**
	(0.077)	(0.051)	(0.059)	(0.044)	(0.040)
HHI	0.182	0.167***	0.180***	0.125**	0.104*
	(0.121)	(0.049)	(0.075)	(0.061)	(0.060)
CC Loans	0.194***	0.200***	0.088**	0.171***	0.161***
	(0.039)	(0.039)	(0.040)	(0.027)	(0.027)
Default Rate	−0.098	−0.101	−0.130	−0.094	−0.083
	(.108)	(0.105)	(0.085)	(0.054)	(0.053)
Unemployment	−0.324***	−0.332***	−0.330***	−0.374***	−0.372***
	(0.067)	(0.066)	(0.041)	(0.036)	(0.039)
Income	−0.016***	−0.013***	0.001	0.004***	0.004***
	(0.005)	(.003)	(0.002)	(0.001)	(0.001)
Probability of Tacit Collusion:					
Constant		−1.201***	0.886***	19.03***	10.19***
		(0.240)	(0.090)	(2.278)	(2.398)
Price Ceiling				−1.020***	−0.355***
				(0.098)	(0.129)
Auto Rate				0.073**	0.118***
				(0.033)	(0.052)
HHI				0.118***	0.117***
				(0.012)	(0.013)
CC Loans				0.055	0.095**
				(0.043)	(0.043)
Income				−0.010***	−0.015***
				(0.003)	(0.003)
1981,1982			−1.030***		−0.422
			(0.182)		(0.311)
1983,1984			−1.579***		−0.619
			(0.202)		(0.421)
1985,1986			−1.758***		−1.248***
			(0.223)		(0.339)
1987,1988			−2.439***		−2.248***
			(0.275)		(0.364)
1989			−2.678***		−2.418***
			(0.485)		(0.411)
σ	1.89***	1.43***	1.33***	1.31***	1.32***
State Effects in Collusion Eqn?	N/A	No	No	Yes	Yes
-N-	1507	1507	1507	1507	1507

Pricing equation includes fixed year and state effects.
* ** and *** denote significance at the 0.10, 0.05 and 0.01 levels.

Table 6: Price Effects of Ceilings

	1979-80	**1981-82**	**1983-84**	**1985-86**	**1987-88**	**1989**	**Total**
At Ceiling	2.77 (286)	-0.44 (213)	-0.08 (116)	0.55 (92)	0.81 (53)	0.94 (18)	1.03 (778)
w=0	-1.72 (7)	-0.86 (154)	-0.86 (76)	-0.44 (53)	-0.79 (7)	-0.11 (2)	-0.80 (299)
w=1	2.89 (279)	0.66 (59)	1.40 (40)	1.89 (39)	1.06 (46)	1.07 (16)	2.17 (479)
C>18	2.35 (1)	-0.24 (10)	1.65 (21)	2.50 (21)	2.96 (6)	3.05 (2)	1.82 (61)
w=0	–	-0.74 (8)	-0.57 (8)	-0.27 (6)	–	–	-0.55 (22)
w=1	2.35 (1)	1.79 (2)	3.01 (13)	3.61 (15)	2.96 (6)	3.05 (2)	3.15 (39)
C=18	2.96 (262)	-0.36 (194)	-0.47 (95)	-0.02 (71)	0.68 (45)	0.67 (16)	1.03 (683)
w=0	-1.44 (4)	-0.77 (137)	-0.90 (68)	-0.46 (47)	-0.06 (5)	-0.11 (2)	-0.74 (263)
w=1	3.03 (258)	0.62 (57)	0.62 (27)	0.82 (24)	0.77 (40)	0.78 (14)	2.13 (420)
C<18	0.63 (23)	-2.38 (9)	–	–	-2.62 (2)	–	-0.36 (34)
w=0	-2.08 (3)	-2.38 (9)	–	–	-2.62 (2)	–	-2.35 (14)
w=1	1.04 (20)	–	–	–	–	–	1.04 (20)
N	344	311	268	253	231	100	1507

Notes: "At Ceiling" row includes all issuers for which $p_{it} = C_{it}$. Issuers with $\hat{p}_{it} < C_{it}$ are assigned $\hat{w} = 1$, and issuers with $\hat{p}_{it} \geq C_{it}$ are assigned $\hat{w} = 0$. Numbers in parentheses are number of issuers in category. Units are hundreds of basis points. "N" row shows total number of issuers in sample.

Table 7: Entry Regressions

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Price Effect	0.011**		0.023***	
	(0.006)		(0.007)	
Price Effect·D8589			-0.028***	
			(0.010)	
Price Effect$^+$		0.007		0.019**
		(0.007)		(0.009)
Price Effect$^+$·D8589				-0.023*
				(0.012)
Price Effect$^-$		0.026**		0.030**
		(0.013)		(0.013)
Price Effect$^-$·D8589				-0.046
				(0.030)
Bank Entry	0.695***	0.693***	0.713***	0.708***
	(0.066)	(0.066)	(0.066)	(0.066)
% Banks with CC Loans	-0.818***	-0.793***	-0.876***	-0.873***
	(0.112)	(0.114)	(0.113)	(0.117)

Notes: Dependent variable in all regressions is state-level percentage change in number of commercial banks with positive credit card loans, in the year following the measurement of the price effect. "Price Effect" is state-level average of a variable equal to $C_{it} - \hat{p}_{it}$ for issuers with $p_{it} = C_{jt}$, and 0 for issuers with $p_{it} < C_{jt}$ (or not facing a ceiling). "D8589" is a dummy variable equal to one from 1985-89. All specifications include fixed state and year effects.

C Figures

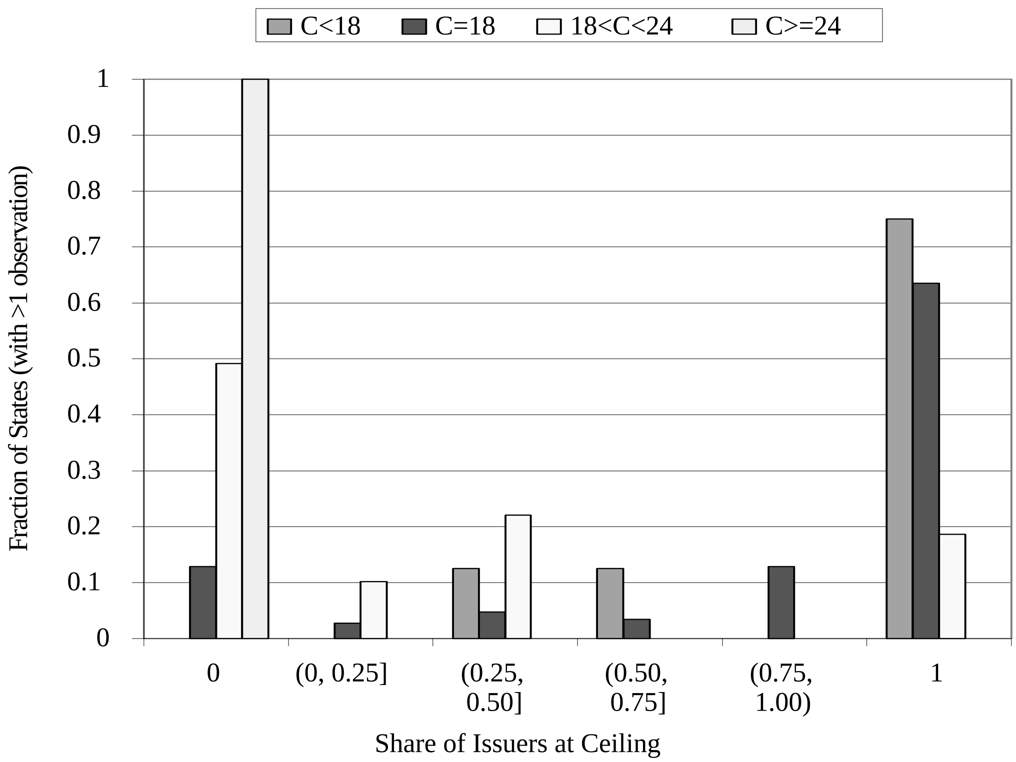

Figure 1: Histograms of Within-State Share at Ceiling, by Level of Ceiling

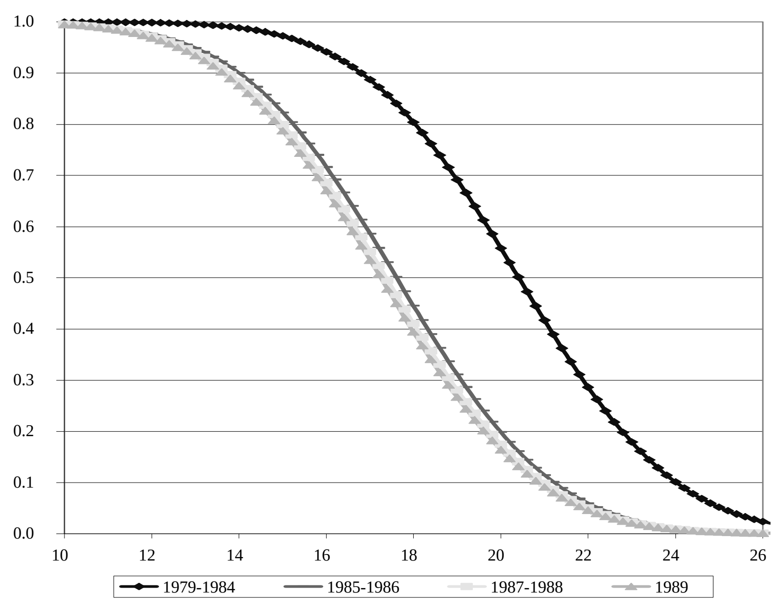

Figure 2: Probability of Tacit Collusion vs. Price Ceiling, by Years

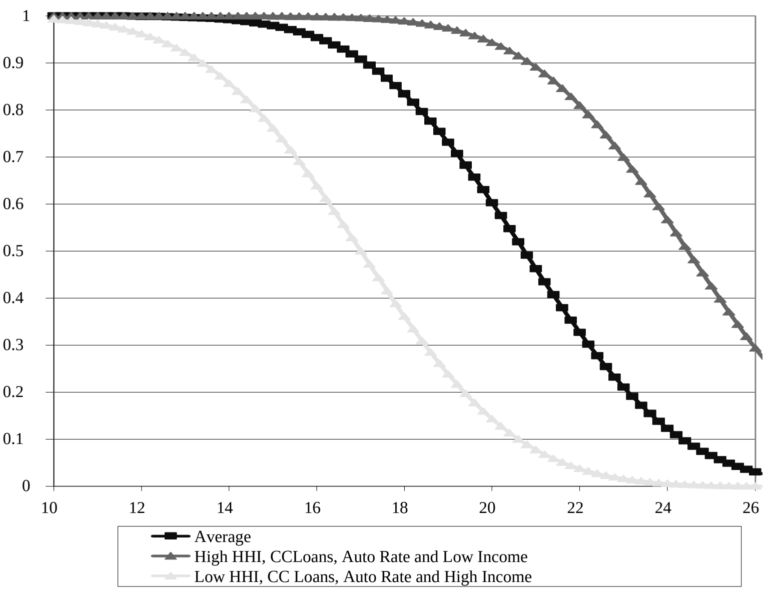

Figure 3: Probability of Tacit Collusion by HHI, CC Loans, Auto Rates and Income.