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

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We test whether a nonbinding price ceiling may serve as a focal point for tacit collusion, using data from the credit card market during the 1980's. Our empirical model can distinguish instances when firms match a binding ceiling from instances when firms tacitly collude at a nonbinding ceiling. The results suggest that tacit collusion at nonbinding state-level ceilings was prevalent during the early 1980's, but that national integration of the market reduced the sustainability of tacit collusion by the end of the decade. The results highlight a perverse effect of price regulation. (*JEL* L0, L12, L5)

> ... 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 percent allowed by law may indicate "potentially illegal activities." "Since smaller banks have assured us that they are making profits charging interest rates of 15 percent 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.
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> —*American Banker,* March 26, 1987, p. 8

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 price or output effects only when it is binding. While a small body of work exists challenging this view, to this point it has been anecdotal or experimental; empirical evidence suggesting that nonbinding price ceilings affect prices is largely nonexistent.[1]

In this paper, we empirically test the hypothesis that a nonbinding 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 1980's. 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, many issuers match their ceiling—particularly in the early years of the sample. Finally, states and issuers vary in characteristics thought to affect the sustainability of tacit collusion. The data therefore display heterogeneity in firm behavior, focal points, and market characteristics. This allows us to conduct a variety of tests related to the focal point hypothesis.

The novelty of our empirical approach is that

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[1] See R. Mark Isaac and Charlies R. Plott (1981) and Vernon L. Smith and Arlington W. 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."

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 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, and the size of the firm increase, 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 1985–1986, 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 1979–1984, then grows weak. This corroborates our finding of a regime change in credit card competition in 1985–1986.

I. 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 a ceiling is simply that it is 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.

A. *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. On the other hand, 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.[4] Firms can resolve the coordination problem in many ways; one such way is through the use of a focal point. The theory of focal

[2] Simple forms of these models are described in Jean Tirole (1992, Ch. 6). Some well-known supergametheoretic models of tacit collusion can be found in Edward J. Green and Robert H. Porter (1984), Julio J. Rotemberg and Garth Saloner (1986), Dilip Abreu et al. (1990), and John Haltiwanger and Joseph E. Harrington, Jr. (1991).

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

[4] Of course, firms attempting to tacitly collude face problems other than coordination. They must deter entry, detect cheating, and punish cheating when it occurs. We deal with entry in our empirical work and find evidence of a regime change in entry in the middle of our sample period. This regime change dramatically reduced firms' ability to tacitly collude. Detecting and punishing cheating is less of a concern. In our sample, credit card issuers have very good information about their competitors' prices. In fact, many card issuers pre-announce price changes through the media. During our sample period, "secret" price cuts (through targeted mailout solicitations, for example) are uncommon. Nor is the industry characterized by frequent and/or idiosyncratic demand shocks.

points dates at least to Thomas 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. Nonetheless, 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 testable implications by making observations regarding the patterns of pricing that we would observe if a focal point were facilitating tacit collusion. We also attempt to be as general as possible regarding pricing and competition in the absence of a focal point. For example, it may be the case that firms are able to tacitly collude for reasons unrelated to the existence of a focal point. Even if this is true, our focus is on the *incremental effect of the focal point on pricing*. As a result, in the discussion and empirical work below we do not explicitly model market structure and competition. One limitation of this approach is that we are unable to make inferences regarding the ability of firms to sustain supercompetitive outcomes in the absence of a focal point; we can only estimate by how much a focal point increases this ability.

B. *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 expected.[5] 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 intuition behind 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.[6] Consider the limiting case in which the focal point is equal to a firm's one-shot noncooperative 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 reoptimization, while profits from cooperation reflect constrained behavior.[7] 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. More precisely, the probability that a given firm matches a nonbinding price ceiling should fall as the ceiling rises.

A related issue is that as costs rise, cooperation at the focal point becomes easier to maintain. Cooperation is trivially easy when costs are such that a firm's noncooperative price equals the ceiling. As costs fall below this level, profits from cheating rise more quickly than profits from cooperation, because the former reflect reoptimization.

In addition, we can test other general empirical implications of tacitly collusive pricing. First, tacit collusion is generally viewed as easier to maintain among fewer firms; we therefore should be more likely to observe tacit collusion when market concentration is high. Second, we expect larger firms to find cooperation more

[5] 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.

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

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

attractive than smaller firms. Given that cheating steals business from other firms, small firms will find the gains from cheating proportionately larger than larger firms. 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.[8]

C. *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 noncollusive 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. Under assumptions regarding the functional form of the distribution and the composition of the candidate and control groups, 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 Douglas J. 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 noncollusive distributions using some form of mixture modeling. Porter (1983), for example, uses a switching regression to endogenously classify prices into collusive and noncollusive regimes. Glenn Ellison (1994) uses a similar approach that defines the transition probabilities between collusive and noncollusive periods as a Markov process. In both of these papers, the data clearly identify periods of collusive and noncooperative behavior. Laura Baldwin et al. (1997) estimate 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 or misspecification of the functional form might lead to the spurious identification of separate distributions. For these reasons, most of the work above uses more finely characterized tests to narrow the set of explanations consistent with the empirical results. For example, Porter and Zona (1999) project 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 noncollusive regimes to depend on unanticipated demand shocks, as in the model of Green and Porter (1984).[9] Baldwin et al. (1997) use a dummy variable thought to capture "neighbor" effects to pick up differences in the likelihood that firms collude. These refinements can not conclusively rule out specification error, but they increase the burden faced by alternative explanations 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 use observations of firms that

[8] This follows the intuition in Rotemberg and Saloner (1986). In their model, the key assumption underlying the prediction that high current demand reduces the maximum sustainable price is that demand shocks are independently and identically distributed (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.

[9] The primary test in Severin Borenstein and Andrea Shepard (1996) is similar in spirit, in that they examine the effects of changes 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.

are not pricing at their focal point (or do not face a focal point) as a control group. 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 matches the ceiling because it is binding. This requires a means of endogenously separating observations that reflect tacit collusion from those that do not. While this problem is similar in spirit to that faced by Porter (1983), Ellison (1994), and Baldwin et al. (1997), in these other cases the problem is simplified by the fact that collusive and noncooperative regimes lead to different observations of the dependent (price) variable. In our case, a given observation at the ceiling may reflect either collusive or noncollusive behavior. We resolve this complication by expanding 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, 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, 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. If our results are not spurious, we would 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.

II. Pricing and Interest Rate Ceilings in the Credit Card Market, 1979–1989

Throughout the 1980's, between four and six thousand banks issued credit cards.[10] Each bank had discretion over the interest rate it charged, as well as any fees. In contrast to the situation that arose during the 1990's, during our sample period nearly every card issuer charged a "fixed rate" that was not pegged to any market rate. Moreover, during the 1980's 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.

In the sample period, most credit card competition took place within states. 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 1980's the vast majority of credit card customers held cards issued by a bank located 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. These state-level markets vary significantly. During our sample period, the state-level Herfindahl index (HHI) ranges from 203 to 9,949; the mean HHI is 1,758. Most state-level markets consist of a relatively small number of larger banks that operate throughout the state, and a large number of very small fringe banks.[11]

Our sample of issuers comes 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; there are a

[10] 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.

[11] Since the 1980's, the market has become more integrated (and concentrated) at the national level.

total of 1,449 usable issuer-level observations in the data set.[12] The issuers in the sample are primarily mid- to large-sized state banks; these banks are among the primary issuers of credit cards in their home states, and fall well within the upper half of the population size distribution. As an example, in 1983 the largest issuer in our data is a bank from California that is the third largest in the country (of 4,825). The median issuer in our sample is the 179th largest in the country, and the tenth smallest issuer in our sample is the 990th largest in the country.

A. *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 home state.[13] 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.[14] 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. The pattern for the state-level data is nearly identical to that for the issuer-level data.[15]

Ceilings existed in over 90 percent of states in the early part of our sample. The ceilings varied across states, but the most common ceiling was 18 percent, which prevailed in nearly 80 percent of states. In 1979, a few states had ceilings below 18 percent; usually these ceilings were imposed at 15 or 12 percent.[16] In response to high inflation, and also as part of a general trend toward deregulation, in the early 1980's 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 18 percent rose dramatically. By 1983, no state in the sample had a ceiling below 18 percent. After 1984, the cross-sectional pattern of ceilings remains fairly static.

B. *Pricing in the Credit Card Market, 1979–1989*

The most striking aspect of credit card pricing during the 1980's is the extent of clustering at certain interest rates; we discuss this in detail below. A corollary of this clustering is rate stickiness. 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.[17] 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

[12] The number of issuer-level observations falls over the sample period, from a high of 164 in 1979 to a low of 94 in 1989. During the same period the number of banks participating in the survey falls from 236 to 167 over the same period. On average, the banks that exit our sample are smaller and more likely to be in states with ceilings than those that remain.

[13] 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 percent. It could offer a credit card to a customer in Maryland at 19.8 percent, despite the fact that Maryland's own issuers faced a ceiling of 18 percent. In practice, this nuance of regulation was unimportant in the early to mid-1980's, because nearly all issuers restricted their marketing efforts to their home states.

[14] 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.

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

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

[17] 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.

TABLE 1—CREDIT CARD INTEREST RATE CEILINGS, 1979–1989

Year	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
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
$C > 18$ percent	3	5	15	23	30	29	37	38	34	34	33
$C = 18$ percent	81	83	69	51	36	32	26	24	26	28	33
$C < 18$ percent	11	7	5	3	—	—	—	—	3	3	—
N (Issuers):	164	164	158	147	134	127	120	121	110	110	94
Share of issuers:											
No ceiling	2	2	7	14	26	34	33	34	34	33	35
$C > 18$ percent	4	6	19	22	30	31	32	31	31	30	30
$C = 18$ percent	85	84	70	63	44	35	35	35	34	37	35
$C < 18$ percent	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.

level of competition among card issuers. Members of Congress at various times implied that issuers were engaging in tacit or explicit collusion.[18] Consumer groups (such as that quoted in the introduction) accused issuers of exploiting their customers. Lawrence M. 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 1980's.

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 ceiling of their home state. Table 2 describes this broad pattern of interest rate clustering at ceilings. In states with ceilings, well over 80 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). In the early years of the sample, it is also 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.[19] 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 subsection 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

[18] 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 percent."

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

TABLE 2—CREDIT CARD PRICING AND INTEREST RATE CEILINGS

	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Share of issuers at ceiling:											
All facing ceiling	85	85	76	76	65	58	59	51	38	31	28
$C > 18$ percent	0	9	13	19	25	27	30	23	8	9	7
$C = 18$ percent	89	91	92	97	92	85	86	76	64	48	46
$C < 18$ percent	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.

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.

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.[20] 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; in most states clustering at the ceiling is either completely absent or unanimous.[21] Because sample-wide variation in clustering is driven more by parallel changes in the clustering of issuers in a given state over time than by within-state cross-sectional variation in issuer behavior in a given state-year, it appears that within-state variation in issuer costs is not the factor driving issuers to match the ceiling.

C. *Credit Card Rates and the Cost of Funds*

In this subsection, 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 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 Fed funds 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 ($C < 18$ percent). What is somewhat surprising is that

[20] The unit of observation is a 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.

[21] 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.

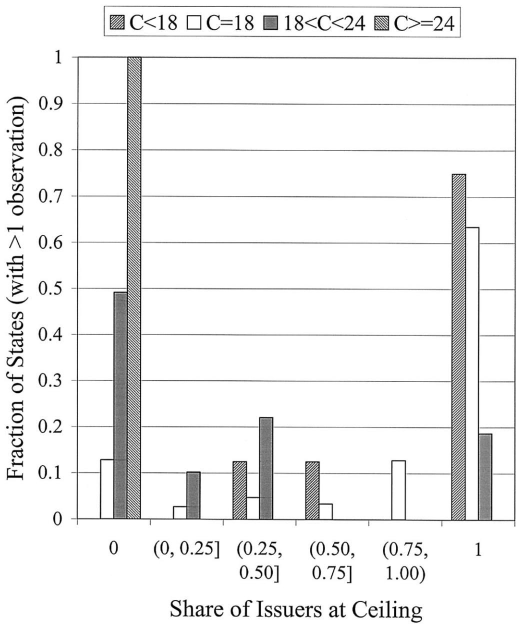

FIGURE 1. HISTOGRAMS OF WITHIN-STATE SHARE AT CEILING, BY LEVEL OF CEILING

for much of the sample period, rates are somewhat higher in states with relatively high ceilings ($C > 18$ percent) than in states without any ceiling.

The second set of rows shows federal funds rates paid by the banks in our sample.[22] The federal funds rate is a proxy for the issuer's cost of funds, which is the largest component of marginal cost for credit card operations.[23] As the table reveals, there is little difference in funds rates across states with different ceilings; it seems unlikely that credit card rates are higher in states with high ceilings simply because issuers in these states have higher costs of funds.

The next rows report the margin between credit card interest rates and the funds rate. Because the funds rate captures issuer-specific marginal cost, this gap should be a close proxy for the interest margin on credit cards. The primary rows show the average gap in states with ceilings of 18 percent, in states with ceilings greater than 18 percent, and in states with no ceiling.[24] In nearly every year, this gap is higher in states with ceilings greater than 18 percent than in states with no ceiling. This contradicts the notion that the margin should be lower in states with ceilings than in states without ceilings.

The subheadings in the last set of rows provide more information, by comparing the credit card funds rate gap for issuers at the ceiling ($p = C$) to the gap for issuers pricing below the ceiling ($p < C$), within states with a given ceiling. In both cases ($C = 18$ percent or $C > 18$ percent), 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, then the gap would be greater for issuers below the ceiling than for issuers at the ceiling.

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

III. Specification of the Model

In this section, we develop the framework for our empirical tests. We begin by specifying a reduced-form equation describing a card issuer's

[22] The federal funds rate is calculated by dividing the issuer's payments on federal funds borrowed by the dollar amount of funds borrowed; we obtain these variables at the issuer level from the Federal Reserve's *Reports of Condition and Income* (or Call Reports). The funds rate is measured for the quarter associated with our observation of the interest rate, and annualized. In the data, we observe a small number of outliers in the funds rate; the outliers may stem from input errors or reflect large-scale balance sheet activity within the quarter (such as merger or divestiture). To deal with these outliers we censor any rate above 25 percent at 25 percent. There are 26 censored observations (of 1,449 total).

[23] Data from the Federal Reserve's *Functional Cost Analysis* (various years) suggest that the cost of funds comprises roughly 75 percent of variable costs for credit card operations. The other primary component of variable cost is default, for which we attempt to control in the empirical work below.

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

TABLE 3—AVERAGE INTEREST RATES BY CEILING

	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Average CC rate (percent):	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
$C > 18$ percent	17.5	18.6	18.7	19.4	19.7	19.7	19.7	19.3	18.6	18.4	18.3
$C = 18$ percent	17.5	17.6	17.7	17.9	17.7	17.7	17.7	17.4	17.2	17.1	17.1
$C < 18$ percent	13.2	13.3	14.7	14.0	—	—	—	—	15.0	15.0	—
Average fed funds rate (percent):	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
$C > 18$ percent	11.2	12.8	16.3	17.4	15.2	13.8	13.7	12.6	10.4	10.8	12.0
$C = 18$ percent	11.8	13.6	15.9	16.8	14.5	13.2	13.2	12.2	10.5	10.7	11.8
$C < 18$ percent	11.5	12.5	13.9	14.0	—	—	—	—	11.0	11.0	—
Average gap (percent):	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
$C > 18$ percent	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
$C = 18$ percent	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.

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 allows the probability of tacit collusion to vary based on issuer- and state-specific factors. After presenting the empirical framework, we describe the variables included in the pricing equations, and discuss some econometric issues.

A. *Baseline Specifications*

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

(1)

$$p^*_{it} = \mathbf{X}_{it}\beta + \mu_j + \delta_t + \varepsilon_{it}, \text{ with } \varepsilon_{it} \sim N(0, \sigma).$$

The vector $\mathbf{X}_{it}$ 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.[25] For issuers in states without ceilings, our observation of the data will simply be $p_{it} = p^*_{it}$.[26]

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:

(2)

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

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

[26] 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.

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

(3)

$$L = \prod_{I_{jt}^{ceil}=1} \left[\prod_{p_{it}=C_{jt}} \Phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) \cdot \prod_{p_{it}<C_{jt}} \sigma^{-1}\phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right)\right] \cdot \prod_{I_{jt}^{ceil}=0} \sigma^{-1}\phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right).$$

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_{jt}^{ceil} 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 nonlimit 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.[27]

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.[28] 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.

B. *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 an issuer to match its ceiling ($p_{it} = C_{jt}$) even though it is not binding ($p_{it}^* < C_{jt}$).

We begin by defining an indicator of tacit collusion:

(4)

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

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, $p_{it} = p_{it}^* = \mathbf{X}_{it}\beta + \mu_j + \delta_t + \varepsilon_{it}$. 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,

(5)

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

[27] One alternative to this specification 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 nonlimit 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.

[28] Our simplest alternative specification involved including the dummy variable I_{it}^{ceil} in the pricing relationship. We also estimated the model with interaction terms $I_{it}^{ceil} \cdot C_{jt}$ and $I_{it}^{ceil} \cdot (C_{jt})^2$ in the pricing relationship. In other specifications, we allowed the β coefficients to differ across issuers in states with and without ceilings. None of these alternatives yielded results that differed in a statistically significant way from those presented here or suggested that the pricing equation was affected by the presence of a ceiling. For example, the coefficient on the dummy variable I_{it}^{ceil} in the pricing relationship is positive with a p-value of 0.41 in the double-hurdle model.

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,

$$L = \prod_{I_{jt}^{ceil}=1} \left[\prod_{p_{it}=C_{jt}} \left\{ \Phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) + \alpha\Phi\left(\frac{C_{jt} - \mathbf{X}_{it}\beta - \mu_j - \delta_t}{\sigma}\right)\right\} \cdot \prod_{p_{it}<C_{jt}} \sigma^{-1}(1-\alpha)\phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right)\right] \cdot \prod_{I_{jt}^{ceil}=0} \sigma^{-1}\phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - p_{it}}{\sigma}\right). \tag{6}$$

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 is known as the "p-Tobit." The model was first presented by Angus Deaton and Margaret Irish (1984) in an attempt to explain the underreporting of British tobacco expenditures, and has typically been applied in similar contexts in labor economics.[29]

The specification above models α as the probability of tacit collusion *conditional on* $p_{it}^* < C_{jt}$. For example, an estimate $\alpha = 0.80$ implies that there is an 80 percent probability that an issuer for which the ceiling is not binding is in fact pricing at the ceiling. More formally, α is the conditional probability:

$$\alpha_{it} = \Pr[w_{it} = 1 | p_{it}^* < C_{jt}]. \tag{7}$$

We stress this because identification of the parameter α comes from its expression as a conditional probability. In order for the model to "label" an observation as being tacitly collusive, it must also "label" the fitted value of p_{it}^* as being below C_{jt}. For observations with high prices relative to their fitted values, the "fit" of the model will be improved by increasing α rather than by changing β.

Because our identification strategy is based on functional form, it is vulnerable to the criticism that a result in favor of tacit collusion may simply reflect specification error. We deal with this possibility through a set of robustness tests presented later in the paper. We also refine our empirical approach by allowing α to be a function of covariates that should be correlated with the sustainability of tacit collusion. This not only increases the burden any alternative explanation based on specification error must meet, 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 John G. 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 $w_{it}^* = \mathbf{Z}_{it}\gamma + \eta_j + \lambda_t + \upsilon_{it}$ and $\upsilon_{it} \sim N(0, 1)$. The probability that

[29] 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 underreport 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 (nonlimit) quantity of the good, but reported a zero (limit) quantity. A second application of the p-Tobit model is 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 coefficients 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 Richard Blundell and Costas Meghir (1987) and Atsushi Maki and Shigeru Nishiyama (1996) for applications of the p-Tobit model.

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	Std Dev	Mean	Std Dev
Credit card rate	18.00	1.78	10.00	22.00	18.76	1.62	17.78	1.78
Price ceiling†	19.11	2.52	12.00	25.00	—	—	19.11	2.52
Fed funds rate	11.60	3.03	0	25.00	9.25	4.48	10.61	4.74
Auto rate	13.40	2.35	7.00	23.00	12.92	2.29	13.53	2.35
HHI in credit cards	1,760	1,482	203	9,949	1,791	1,823	1,759	1,380
CC loans‡ ($1,000s)	464	1,534	0	28,184	576	2,603	512	1,390
Loan loss rate	0.51	0.95	−0.10	9.79	0.89	0.96	0.78	0.95
Unemployment rate	7.40	2.46	2.40	18.00	6.93	2.27	7.53	2.50
Weekly income‡ ($100s)	3.66	0.55	2.44	5.50	3.79	0.47	3.62	0.57
Sample Size	1,466				314		1,152	

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

a given issuer tacitly colludes is then $\alpha_{it} = \Phi(\mathbf{Z}_{it}\gamma + \eta_j + \lambda_t)$. Incorporating this into the likelihood function above yields:

$$(8)\quad L = \prod_{I_{jt}^{ceil}=1}\left[\prod_{p_{it}=C_{jt}}\left\{\Phi\left(\frac{\mathbf{X}_{it}\beta + \mu_j + \delta_t - C_{jt}}{\sigma}\right) + \Phi(\mathbf{Z}_{it}\gamma + \eta_j + \lambda_t)\Phi \cdot \left(\frac{C_{jt} - \mathbf{X}_{it}\beta - \mu_j - \delta_t}{\sigma}\right)\right\} \cdot \prod_{p_{it}<C_{jt}} \sigma^{-1}\Phi(-\mathbf{Z}_{it}\gamma - \eta_j - \lambda_t) \cdot \phi\left(\frac{p_{it} - \mathbf{X}_{it}\beta - \mu_j - \delta_t}{\sigma}\right)\right] \cdot \prod_{I_{jt}^{ceil}=0} \sigma^{-1}\phi\left(\frac{p_{it} - \mathbf{X}_{it}\beta - \mu_j - \delta_t}{\sigma}\right).$$

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.[30]

To summarize, our model begins with a simple formulation of an issuer's price. It allows the data to be censored because issuers may face ceilings that are binding. Finally, we broaden the model to explicitly allow for an observation-specific probability of tacit collusion.

C. *Variables*

In this subsection, we outline the $\mathbf{X}_{it}$ variables included in the pricing equation, and also the variables included in the vector $\mathbf{Z}_{it}$. Table 4 contains summary statistics for these variables. It stratifies the data based on whether the observation comes from a state with or without a ceiling.[31]

The vector $\mathbf{X}_{it}$ includes variables that reflect demand, costs, and market structure. It includes the issuer's average interest rate paid on fed funds as a measure of issuer-specific costs. It also includes the issuer's loan loss ratio. This is intended to capture issuer-specific variation in the riskiness of the issuer's loan portfolio (which includes credit cards).[32] The issuer's

[30] 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.

[31] Note that differences in descriptive statistics based on the value of I^{ceil} reflect both cross-sectional differences and differences in the general environment over time (because most observations of $I^{ceil} = 0$ occur later in the sample period).

[32] The loan loss ratio is measured using data from the Call Reports regarding the issuer's provisions for loan losses and total outstanding loan balances. The loss ratio is calculated by dividing loan losses by outstanding loans. We use the loan loss ratio (which is aggregated over all loans)

36-month auto loan rate is also included; this may also capture issuer-specific costs or demand related to loans.[33] The $\mathbf{X}_{it}$ 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 capture variation in demand.

We also include in $\mathbf{X}_{it}$ the state-level Herfindahl index in credit cards.[34] 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 $\mathbf{X}_{it}$ 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 greater market power. Because both the HHI and the credit card loans variables are highly skewed, we include the logarithm of each in the $\mathbf{X}_{it}$ 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 $\mathbf{Z}_{it}$ 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 $\mathbf{Z}_{it}$ vector also includes the issuer's fed funds rate. The fed funds rate measures issuer-specific costs; the higher are these costs, the easier is tacit collusion at the ceiling.[35] The $\mathbf{Z}_{it}$ 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 $\mathbf{Z}_{it}$, to test the hypothesis that larger issuers are more likely to tacitly collude. The last component in $\mathbf{Z}_{it}$ is income; this tests the Rotemberg-Saloner prediction that tacit collusion is more difficult to sustain in periods of high demand.[36] 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.

Econometric Issues.—Of the variables in $\mathbf{X}_{it}$ and $\mathbf{Z}_{it}$, 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 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.[37]

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, and would imply a spurious correlation between prices and price ceilings. In unreported results, we instrument for the level of the price ceiling

rather than the issuer-level credit card default rate because issuer-level default is only available for the years 1985–1989. During these years, the raw correlation between the two measures of riskiness is 0.50, suggesting that loan losses are a good proxy for credit card default at the issuer level. Additionally, in unreported results we compare specifications for the subsample 1985–1989, and find no qualitative difference between estimates using the issuer-level credit card default rate and estimates using the loan loss ratio.

In rare cases (three observations in our data), provisions for loan losses are negative, reflecting an expectation by the issuer that its recoveries on bad debt will exceed its losses.

[33] The auto loan rate is reported by banks in the *Quarterly Report of Rates of Selected Direct Consumer Installment Loans*.

[34] The HHI is measured using market shares by outstanding balances. It is constructed using the population of commercial banks issuing credit cards from the Call Reports.

[35] We do not include the auto loan rate as a cost measure in $\mathbf{Z}_{it}$ because its interpretation is unclear; it may measure variation in either costs or demand at the issuer level. The loan loss ratio is not significant when it is included in $\mathbf{Z}_{it}$, and leaves the other coefficients unchanged; we therefore omit it from our reported specification.

[36] In unreported results, we also include the unemployment variable in $\mathbf{Z}_{it}$. It is not statistically significant.

[37] 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 percent of total bank assets on average, so the endogeneity concern is greatly reduced using the instruments.

using a vector of banking regulation variables.[38] Results from these unreported specifications are nearly identical to those reported below.

D. *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.[39] 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 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; the "year dummies" pertain to 1979–1980, 1981–1982, 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 fed funds and auto loan rates are not significant in the Tobit and *p*-Tobit models, but positive and significant in each of the other models. This suggests that the Tobit and *p*-Tobit models are poorly specified relative to the other models, perhaps because they do not allow for the possibility of issuer- or time-specific probabilities 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.[40] The coefficients imply that a doubling of issuer size is associated with an interest rate ranging from 11 basis points higher (Models 3 and 4) to 17 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. The coefficient on income is negative and significant in the first two specifications, but becomes positive and marginally significant once the specification includes the $\mathbf{Z}_{it}$ variables (which also include income). This appears to be because high demand makes tacit collusion more difficult, which biases the coefficient in the pricing relationship 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 10 percent.[41] 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.[42] 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 $\alpha = 0.82$, but the probability of collusion falls dramatically over the sample period.

The last columns of Table 5 show results from the full double-hurdle models. These specifications include the vector of variables in $\mathbf{Z}_{it}$ that should influence the sustainability of tacit

[38] 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 multibank holding companies to operate in the state. These variables are taken from Dean Amel and Daniel G. Keane (1986), and updated with information from Randall S. Kroszner and Philip E. Strahan (1999).

[39] 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 $\lambda = 0$, $\alpha = 0.50$; 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. 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 $\alpha = \Phi(\lambda)$.

[40] See Stango (2000) for further discussion of this point.

[41] We should stress that this coefficient is interpreted as the probability of tacit collusion conditional on $(p^*_{it} < C_{it})$. 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 $\alpha\gamma/(1 - \gamma + \alpha\gamma)$.

[42] 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.

TABLE 5—MODELS OF PRICING AND TACIT COLLUSION

	Model 1	Model 2	Model 3	Model 4	Model 5
Pricing equation:					
Fed funds rate	0.024	0.024	0.015***	0.013***	0.015***
	(0.016)	(0.015)	(0.002)	(0.002)	(0.002)
Auto rate	0.061	0.069	0.104**	0.102**	0.100**
	(0.056)	(0.057)	(0.052)	(0.051)	(0.051)
HHI	0.443	0.448***	0.544**	0.544***	0.538***
	(0.262)	(0.172)	(0.195)	(0.195)	(0.245)
CC loans	0.158***	0.165***	0.112***	0.112***	0.114***
	(0.045)	(0.045)	(0.036)	(0.036)	(0.036)
Loan loss rate	0.049	0.049	0.074**	0.074**	0.076*
	(0.062)	(0.061)	(0.036)	(0.036)	(0.046)
Unemployment	−0.308***	−0.321***	−0.274***	−0.274***	−0.249***
	(0.068)	(0.065)	(0.069)	(0.069)	(0.070)
Income	−0.020***	−0.019***	0.005	0.005	0.007*
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Probability of tacit collusion:					
Constant		−1.293***	0.925***	—	—
		(0.164)	(0.085)		
Price ceiling				−1.042***	−0.788***
				(0.113)	(0.166)
Fed funds rate				0.050***	0.038***
				(0.017)	(0.017)
HHI				0.700**	0.925**
				(0.370)	(0.418)
CC loans				0.096*	0.126**
				(0.055)	(0.056)
Income				−0.012**	−0.013***
				(0.007)	(0.004)
1981, 1982			−1.183***		−0.159
			(0.192)		(0.327)
1983, 1984			−1.491***		−0.076
			(0.300)		(0.416)
1985, 1986			−2.240***		−0.896**
			(0.312)		(0.407)
1987, 1988			−3.138***		−1.146***
			(0.312)		(0.391)
1989			−3.833***		−1.470***
			(0.540)		(0.434)
σ	1.76***	1.71***	1.40***	1.13***	1.22***
State effects in collusion equation?	N/A	No	No	Yes	Yes
N	1,466	1,466	1,466	1,466	1,466

Note: Pricing equation includes fixed year and state effects.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

collusion (Models 4 and 5), as well as the time dummies (Model 5). The $\mathbf{Z}_{it}$ 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 fed funds rate is positive and significant, suggesting that high-cost issuers are more likely

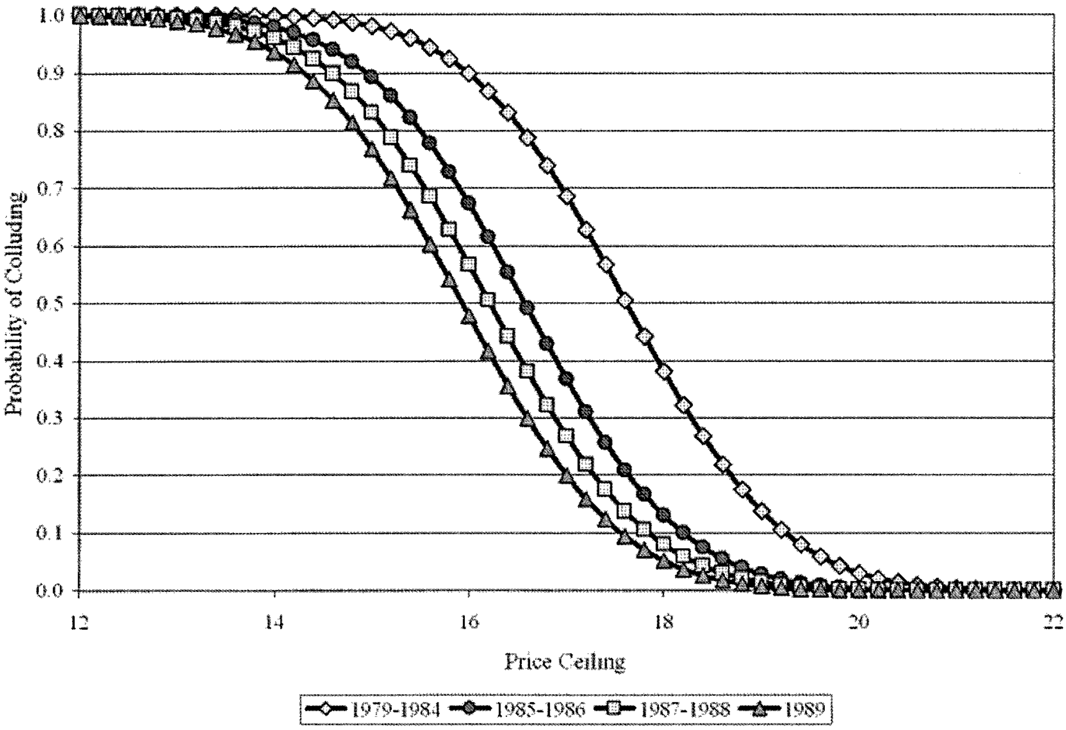

FIGURE 2. PROBABILITY OF TACIT COLLUSION VS. PRICE CEILING, BY YEARS

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 issuers are more likely to collude.

An interesting feature of the double-hurdle results is that when the covariates in $\mathbf{Z}_{it}$ 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 less likely.

In summary, the results provide 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 markedly after 1986. In the next subsection we expand upon these points.

E. *Some Further Detail on the Results*

To illustrate the economic significance of the variables in $\mathbf{Z}_{it}$, 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, fed funds rate, and income. Figure 2 illustrates the shift in issuers' propensity to collude over time; this effect is fairly dramatic. For example, between 1979 and 1984 an issuer facing a price ceiling of 18 percent is almost eight times more likely to collude (39 percent vs. 5 percent) than

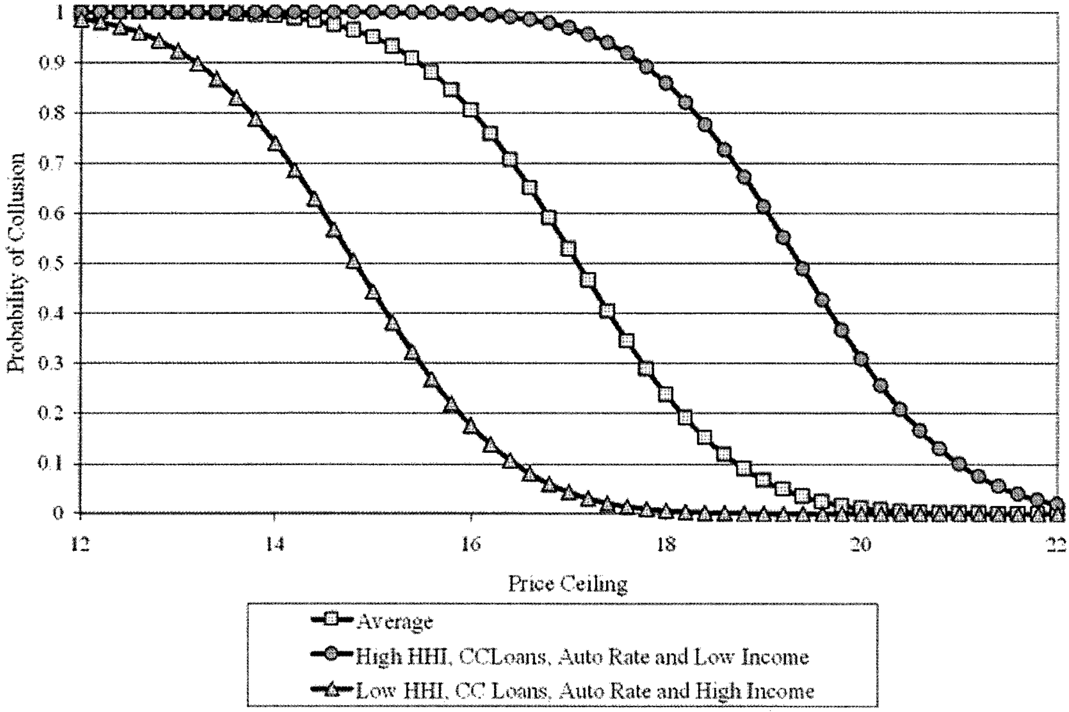

FIGURE 3. PROBABILITY OF TACIT COLLUSION BY HHI, CC LOANS, AUTO RATES AND INCOME

an issuer facing the same ceiling in 1989.[43] Figure 3 plots the predicted probability for an issuer 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 variables in $\mathbf{Z}_{it}$: HHI, credit card loans, fed funds rate, and income.[44] 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 80 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 were binding for some 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.[45] For all issuers with observed

[43] We calculate these probabilities using means of the variables in $\mathbf{Z}_{it}$ measured within each two-year period and the unweighted averages of the fixed state effects. Thus, changes in the probabilities over time reflect movement both because of the year dummies, and because of changes in the means of the variables in $\mathbf{Z}_{it}$.

[44] 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 $\mathbf{Z}_{it}$, we use within-state standard deviations in the calculations. The marginal effects of the ceiling and income variables are largest, followed by the HHI, loans, and fed funds rate. The largest marginal effect (ceiling) is roughly ten times bigger than the smallest marginal effect (fed funds rate).

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

TABLE 6—PRICE EFFECTS OF CEILINGS

	1979–1980	1981–1982	1983–1984	1985–1986	1987–1988	1989	Total
At ceiling	2.20 (276)	0.10 (206)	0.42 (112)	0.85 (80)	1.53 (44)	1.75 (16)	1.14 (734)
$w = 0$	−1.24 (9)	−0.93 (116)	−1.42 (53)	−1.52 (28)	−0.76 (15)	−0.44 (5)	−1.11 (243)
$w = 1$	2.56 (267)	1.44 (90)	2.07 (59)	2.12 (52)	2.71 (29)	2.75 (11)	2.26 (491)
$C > 18$	0.11 (1)	−1.54 (9)	0.59 (21)	1.05 (18)	1.08 (4)	−1.29 (1)	0.38 (54)
$w = 0$	—	−1.96 (8)	−2.41 (8)	−2.36 (5)	−1.38 (2)	−1.29 (1)	−2.12 (24)
$w = 1$	0.11 (1)	1.84 (1)	2.43 (13)	2.36 (13)	3.54 (2)	—	2.38 (30)
$C = 18$	2.42 (244)	0.27 (181)	0.38 (91)	0.76 (62)	1.57 (40)	1.96 (15)	1.29 (633)
$w = 0$	−1.86 (8)	−0.75 (99)	−1.24 (45)	−1.33 (23)	−0.66 (13)	−0.22 (4)	−0.96 (192)
$w = 1$	2.56 (236)	1.50 (82)	1.96 (46)	2.03 (39)	2.65 (27)	2.75 (11)	2.27 (441)
$C < 18$	0.54 (31)	−0.90 (16)	—	—	—	—	0.05 (47)
$w = 0$	−0.96 (18)	−2.01 (9)	—	—	—	—	−1.312 (14)
$w = 1$	2.61 (13)	0.54 (7)	—	—	—	—	1.89 (33)
N	337	309	266	240	219	95	1,466

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.

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.[46]

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.[47] The average effect is positive at the beginning and end of the sample, and close to zero in the early 1980's (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 50 percent of issuers with $\hat{w} = 1$.[48]

While the conditional probability of tacit collusion is much lower at the end of the sample, the net price effect of ceilings grows more positive. This occurs because fewer issuers face a binding ceiling; this offsets the lower incidence of tacit collusion. For example, in 1981–1982 the share of issuers for which the ceiling is binding is 0.38, while the share tacitly colluding is 0.29. By 1989, the share for which the ceiling is binding is 0.05, while the share tacitly colluding is 0.12. This is corroborated by the data in Table 3, which show an increase toward the end of the sample in our proxy for price-cost margin.[49]

As a final point, our finding of a downward

[46] While we allow for tacit collusion to be "partial," our calculations show that in most state-years it is either nonexistent or unanimous. The share of issuers colluding (for which $p^* < C$) is one in nearly 40 percent of state-years, zero in 42 percent of state-years, and between zero and one (indicating partial tacit collusion) in fewer than 20 percent of state-years. In those states where tacit collusion is partial, it is generally the larger issuers that tacitly collude. The within-state median size differential between colluding and noncolluding issuers is roughly $300 million in assets (the sample median is $1.7 billion.). Finally, we find that partial tacit collusion is unstable as well as uncommon; it persists into the following year roughly 25 percent of the time.

[47] 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 nonbinding price ceilings.

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

[49] Independent of tacit collusion, our model predicts higher rates toward the end of the sample period (relative to a benchmark such as the cost of funds). This stems primarily

shift in the probability of tacit collusion after 1985 is consistent with evidence regarding changes in the competitive environment in credit cards.[50] The primary change was a sharp increase in entry. From 1979–1984, the average annual entry rate in credit cards was 1.3 percent; the average jumped to 7.7 percent in 1985–1986.[51] Furthermore, in 1986 many of the largest nationally marketed issuers cut their rates and/or launched aggressive national marketing campaigns.[52] These moves certainly might have reduced the state-level incentives to tacitly collude.[53] 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 our measure of tacit collusion to state-level entry in credit cards.

F. *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 subsection, 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

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

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 $p_{it} < C_{jt}$ has a price effect of zero. 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.[54]

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, $CCEntry_{jt}$. 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 beginning-of-year price effects should affect entry over the following year, we measure our entry variable for the year following our measurement of the price effect variable; if $Effect_{jt-1}$ is measured in March 1981, $CCEntry_{jt}$ is the percentage change in banks offering credit cards between March 1981 and March 1982.

Our specification also includes two control variables. The first, $BkEntry_{jt}$, 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 bank entry, exit, or mergers. The second

from sample-wide increases in concentration and average issuer size throughout the 1980's.

[50] See for example, "Credit Card Wars: Profits are Taking a Direct Hit," *Business Week,* November 17, 1986, p. 166.

[51] 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 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 1979–1984 the entry rate for banks overall averaged 0.9 percent. It averaged −0.1 percent from 1985–1986.

[52] For an interesting account of the competitive threat posed by large national issuers (and the responses of local issuers), see "Credit Card War Erupts in Connecticut: Institutions Slash Rates in Battle with Money Center Invaders for Market Share," *American Banker,* June 5, 1986, p. 1.

[53] 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.

[54] The regressions presented here 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.

TABLE 7—ENTRY REGRESSIONS

Variable	Model 1	Model 2	Model 3	Model 4
Price effect	0.021***		0.022***	
	(0.007)		(0.007)	
Price effect · D8589			−0.005	
			(0.011)	
Price effect$^+$		0.028***		0.034***
		(0.009)		(0.009)
Price effect$^+$ · D8589				−0.021
				(0.013)
Price effect$^-$		0.002		0.007
		(0.015)		(0.020)
Price effect$^-$ · D8589				−0.008
				(0.037)
Bank entry	0.653***	0.665***	0.650***	0.649***
	(0.079)	(0.079)	(0.079)	(0.079)
Percent banks with CC loans	−0.897***	−0.907***	−0.894***	−0.893***
	(0.125)	(0.125)	(0.125)	(0.126)

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 1 from 1985–1989. All specifications include fixed state and year effects.

*** Significant at the 1-percent level.

variable, $BankswithCC_{jt-1}$, is the percentage of banks in state j that have positive credit card loans at time $t - 1$. In our sample, entry into credit cards occurs because an existing bank begins to offer a credit card; it is very rare to observe de novo entry by a credit card "pure-play." We expect that the percentage of banks already offering credit cards is 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

$$CCEntry_{jt} = \beta_1 Effect_{jt-1} + \beta_2 BkEntry_{jt} + \beta_3 BankswithCC_{jt-1} + \mu_j + \delta_t + \varepsilon_{jt}. \tag{9}$$

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.

We make two modifications to our baseline specification. The first allows 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 allows the impact of the price effect variable to vary over time. 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 1 from 1985–1989. Our fullest specification is:

$$\begin{aligned} CCEntry_{jt} = {} & \beta_1 Effect^+_{jt-1} + \beta_2 Effect^-_{jt-1} \\ & + \beta_3 Effect^+_{jt-1} \cdot D8589_t \\ & + \beta_4 Effect^-_{jt-1} \cdot D8589_t \\ & + \beta_5 BkEntry_{jt} \\ & + \beta_6 BankswithCC_{jt-1} \\ & + \mu_j + \delta_t + \varepsilon_{jt}. \end{aligned} \tag{10}$$

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 $BankswithCC_{jt-1}$ is negative and significant; these results are consistent with our expectations.

In every specification, the price effect variables show a link between our measure of price effects and entry/exit rates, although the results are not symmetric.[55] The results suggest that the link between tacit collusion and entry is quite strong; the "positive price effect" coefficient in Models 2 and 4 is economically and statistically significant. In Model 2, we estimate that a 100 basis point increase in the price effect is associated with an entry rate 2.8 percent higher. Given that the sample mean entry rate is 3.2 percent, this is quite large. The results of Model 4 suggest that this relationship is stronger early in the sample and becomes weaker. The interaction term between the positive price effect and the 1985–1989 dummy is negative, and almost completely offsets the pre-1985 effect; the estimated pre-1985 effect is also larger and estimated more precisely in this specification. While the pre-1985 effect is statistically significant, the net 1985–1989 effect is not significant at any reasonable level.

Given our discussion in the previous subsection, we interpret our results as finding that prior to 1985, a strong link existed between our measure of tacit collusion and state-level entry. In contrast, this relationship virtually disappears after 1985. We suspect that this reflects 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.

[55] The p-value for the test of Model 2 against Model 1 is 0.11, and the p-value for the test of Model 4 against Model 3 is 0.02. This appears to reflect the fact that there is a strong link between positive price effects and entry, but no relationship between negative price effects and exit. The latter result is unsurprising if there are sunk costs associated with offering credit cards.

TABLE 8—MARKET DYNAMICS FOLLOWING REGIME CHANGES

	Regime change:		
	Raise 1–400 pts.	Raise 400+ pts.	Eliminate
Mean change in price effect	1.38	−0.38	0.91
Share raising rate	0.68	0.28	0.49
Share matching new ceiling	0.36	0.04	n/a
Share colluding:			
Before	0.15	0.67	0.24
After	0.36	0.04	n/a
Share binding:			
Before	0.77	0.29	0.66
After	0	0	n/a
N	25	25	41

G. *The Effects of Changes in Focal Points*

The focus of our analysis is primarily static, in that we seek to identify whether issuers are tacitly colluding given market conditions at time t. However, the data do contain instances in which we observe issuer behavior following changes in ceilings (or their elimination). Table 8 presents some summary statistics regarding "price effects" and other statistics following a regime change in the ceiling. We stratify the data into three categories: issuers for which the ceiling rises by fewer than 400 basis points, issuers for which the ceiling rises by more than 400 basis points, and issuers for which the ceiling is eliminated. There are 91 issuer/year observations in these categories.

The top row of the table shows the average change in "price effect" for issuers under the three regime changes. In states that raise their ceilings by relatively small amounts (<400), the price effect rises by an average 134 basis points—larger than the increase (93 basis points) in states that eliminate their ceilings, and far more than in states that raise their ceilings by large amounts. In states with small increases in ceilings, issuers also are more likely to raise their rates, to match the new ceiling and to tacitly collude at the new ceiling. This result is confounded slightly by the greater prevalence of binding ceilings for issuers facing small increases, but among issuers facing small increases the majority who match the new ceiling are tacitly colluding. In contrast, large increases in the price ceiling are followed on average by lower price effects, and a lower incidence of tacit collusion. This pattern is consistent with the focal point hypothesis: small increases in ceilings are more likely to sustain tacit collusion at the new ceiling, and therefore lead to relatively higher prices than large increases or elimination of the ceiling.

IV. 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 $\mathbf{Z}_{it}$—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 specified incorrectly, leading to the spurious identification of separate price distributions. This might occur if we have misspecified the functional form of the likelihood function. 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.[56] While we reject normality using all of the residuals, this is due to the presence of five observations with extremely low interest rates (15 percent and lower); when we omit the residuals for these observations, we do not reject the hypothesis of normality.[57]

Another source of measurement error may be 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.[58] 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.[59]

A further potential source of measurement error would be unobserved issuer-level cost differences. This would lead us to mistakenly identify high-cost issuers as tacitly colluding. The most natural candidate to explain issuer-level cost differences is default risk. It is for this reason that we include a measure of default risk in the pricing equation. Furthermore, the results regarding changes in focal points from Table 8 are inconsistent with this alternative. And while unobserved costs such as default should lead to a cautious interpretation of our summary evidence in Table 3, we find it much less likely that they could explain the results from our full specifications. First, our variables measuring issuer-specific costs (the funds rate and the loan loss ratio) should capture most cross-sectional variation in variable costs, as these are the largest components of variable cost for credit card operations. Second, if unobservable issuer-level costs are normally distributed and uncorrelated with the variables in $\mathbf{X}_{it}$, they will be captured in the error term of the Tobit

[56] 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.

[57] Our test statistic is the Shapiro-Wilk "*W*" statistic. The *p*-value associated with the statistic is 0.28. Omitting these observations with very low rates does not qualitatively change any of the estimates presented in Tables 5–8.

[58] On average, annual fees made up roughly 15 percent 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.

[59] 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.

model. Third, unobserved costs from the pricing equation would have to be correlated with each variable in $\mathbf{Z}_{it}$ in a way consistent with our empirical results. This seems particularly unlikely; for example, 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 final 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. The cost of cutting rates therefore 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 lower ceilings, or in states with lower income.

V. Conclusions

The finding that a nonbinding price ceiling may facilitate tacit collusion has important policy implications. For example, price caps recently have been imposed in the electricity industry to curb prices during peak demand periods.[60] However, the high day-to-day variance of electricity demand implies that these price caps frequently will be nonbinding. 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 1980's, and clustering at particular rates. We must point out, however, that while our results go far in explaining credit card pricing during the 1980's, they have little relevance in explaining credit card pricing in the 1990's—a decade that saw a vast increase in inter- and intra-issuer variance in pricing.

As a concluding point, our paper does not focus explicitly on the processes by which firms move from one tacitly collusive equilibrium to another, or achieve such an equilibrium in the first place. While our final table provides some suggestive evidence on this point, our data regarding market dynamics are too limited to learn much about the dynamics of tacit collusion at focal points. We believe that this is a promising area for future research.

[60] They have been extensively used in the California and Pennsylvania/New Jersey/Maryland markets.

Appendix: The Sustainability of Tacit Collusion at a Focal Point

In this Appendix, 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 generalize to settings with multiple firms and nonzero costs.

A. 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

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

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

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

and the noncooperative profits as

$$\text{(A5)} \qquad \Pi_1(NC, NC)^{61}$$

where

$$\text{(A6)} \qquad NC = p_1^*(NC), NC = p_2^*(NC).$$

For simplicity we assume that

$$\text{(A7)} \qquad \Pi_1(NC, NC) = 0.$$

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

$$\text{(A8)} \qquad \Pi_1(p_1, p_2) = p_1 D_1(p_1, p_2).$$

B. Assumptions

We maintain three assumptions regarding competition:

(1) The two goods are substitutes:

$$\text{(A9)} \qquad \frac{\partial D_1(p_1, p_2)}{\partial p_2} > 0.$$

(2) Prices are strategic complements:

$$\text{(A10)} \qquad \frac{\partial p_1^*}{\partial p_2} > 0.$$

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

$$\text{(A11)} \qquad \frac{\partial D_1(p_1, p_2)}{\partial p_2} < -\frac{\partial D_1(p_1, p_2)}{\partial p_1}.$$

C. 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 $C = NC$:

$$\text{(A12)} \qquad \frac{d\Pi_1(p_1^*(NC), NC)}{dp_2} > \frac{d\Pi_1(NC, NC)}{dp_2}.$$

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 reoptimize 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 what happens beyond that point. To look at this more closely, we examine the second derivatives of profits under cooperation and cheating.

[61] This representation of "noncooperative" profits implicitly assumes that defection by one firm is met by a reversion to pure noncooperative behavior by the other firm. In Section II, subsection A, we note that such reversion may not occur—in a multifirm setting, a subset of firms may continue to tacitly collude following the defection of one firm. This will make cheating more attractive. Our intuition here still applies, however, in that *relative to any given pattern of reversionary behavior*, cooperation becomes more difficult as the focal point rises.

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

(A13)

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

Some terms have dropped out because of the Envelope theorem results that at the short-run best response,

$$\text{(A14)} \qquad \frac{\partial\Pi(p_1^*(C), C)}{\partial p_1} = 0.$$

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

$$\text{(A15)} \qquad \frac{d^2\Pi(p_1^*(C), C)}{dp_2^2} = \frac{\partial p_1^*}{\partial p_2}\frac{\partial D_1(\cdot)}{\partial p_2}.$$

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:

$$\text{(A16)} \qquad \frac{d^2\Pi(C, C)}{dp_2^2}$$

$$= C\left[\frac{\partial^2 D_1(\cdot)}{\partial p_1^2} + 2\frac{\partial^2 D_1(\cdot)}{\partial p_1 \partial p_2} + \frac{\partial^2 D_1(\cdot)}{\partial p_2^2}\right]$$

$$+ 2\left(\frac{\partial D_1(\cdot)}{\partial p_2} + \frac{\partial D_1(\cdot)}{\partial p_1}\right).$$

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

$$\text{(A17)} \qquad \frac{d^2\Pi(C, C)}{dp_2^2} = 2\left(\frac{\partial D_1(\cdot)}{\partial p_2} + \frac{\partial D_1(\cdot)}{\partial p_1}\right).$$

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

(A18)

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

Along with the first result, that

$$\text{(A19)} \qquad \frac{d\Pi_1(p_1^*(NC), NC)}{dp_2} > \frac{d\Pi_1(NC, NC)}{dp_2}$$

this implies that

$$\text{(A20)} \qquad \frac{d\Pi(p_i^*(C), C)}{dp_2} > \frac{d\Pi(C, C)}{dp_2}$$

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

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