Price Ceilings as Focal Points for Tacit Collusion: Evidence from the Credit Card Market

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Abstract

We test whether a non-binding price ceiling may serve as a focal point for tacit collusion. We use data from the credit card market during the 1980s; in our sample, most credit card issuers face state-level usury ceilings, and well over half match their ceilings. Our empirical model explicitly controls for the possibility that ceilings may have been binding. We find evidence in favor of tacit collusion: a statistically significant proportion of issuers match their ceiling even though it is not binding. The issuer-level probability of tacit collusion is lower in states with higher ceilings, higher in states with higher concentration, and higher for larger issuers. It appears that tacit collusion was less prevalent in the late 1980s than in the early 1980s, possibly because of intensified competition at the national level. The results highlight a potentially perverse effect of price caps.

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

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

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

1 Introduction

It is well-known that firms may tacitly collude to achieve cooperative outcomes. In many cases, the most important factor determining firms' ability to tacitly collude is their ability to coordinate price-setting. This coordination is typically easier when the number of firms is small; when firms are not too different in terms of costs; when the product is fairly homogeneous; when the quality of information is very high; and when a stable set of firms interacts over a long period of time. Generally speaking, the empirical literature on tacit collusion shows that collusion succeeds most often in environments that facilitate sucessful coordination of price-setting.

In this paper, we consider the possibility that a non-binding price ceiling may serve as a focal point for tacitly collusive price-setting. The notion that focal points may facilitate tacit collusion revolves around the idea that the focal point resolves the coordination problems that plague games of tacit collusion. While this idea has received considerable anecdotal support, it has been difficult in practice to formally test the focal point hypothesis. Such testing requires cross-sectional or time-series variation in both the focal point and firm behavior, in order to identify the facilitative power of the focal point.

We test the focal point hypothesis in a setting that provides a near-ideal natural experiment: the credit card market during the 1980s. During this sample period, credit card interest rates are extremely "sticky;" issuers change their rates roughly once every five years. Moreover, interest rates display significant clustering at particular interest rates. For example, in many years of our sample over eighty percent of issuers maintain an interest rate of eighteen percent. The stickiness and clustering of interest rates attracted considerable attention during the 1980s from lawmakers, antitrust authorities and academics; allegations of tacit or explicit collusion (similar to those in the quote above) were not uncommon. The setting is therefore one in which we might admit the possibility of tacit collusion.

The empirical work begins with the observation that interest rate clustering occurs overwhelmingly at state-level credit card usury ceilings. In our sample, nearly all issuers face these ceilings, which are legally binding based on the issuer's state of incorporation. The most common interest rate ceiling is eighteen percent, and in states with this ceiling over ninety percent of issuers set their interest rate at the ceiling. There is similar clustering at ceilings other than eighteen percent. The extent of this clustering varies across states and over time, but remains significant even at the end of the sample period. For the sample as a whole, over sixty percent of observed interest rates match their ceiling. Even more important, because ceilings and clustering vary across states and time, we can conduct tests of the focal point hypothesis. We also have data for a number of issuers in states with no ceilings. These observations represent a useful control group, as issuers in these states do not face a focal point.

The novelty of our empirical approach is that it can separately identify the instance in which an issuer matches the ceiling of its home state because it is binding, and the instance in which it matches the ceiling even though it is not binding – a result that we interpret as tacitly collusive. The likelihood function for the data explicitly allows ceilings to be binding, by incorporating features of a standard censored regression model of pricing. It then extends the model to admit tacit collusion by introducing an independent probability that issuers price at the ceiling even when 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. We find that a statistically significant percentage of issuers match their ceiling even though it is not binding. In the early years of the sample, we estimate that tacit collusion is quite common; over half of the issuers for which the ceiling is not binding match it nonetheless. This proportion falls dramatically by the end of the sample. We also find that the issuer-level probability of tacit collusion is a decreasing function of its ceiling, an increasing function of concentration in its home state, and an increasing function of its auto loan rate. These results suggest that the facilitative power of the ceiling dissipates at higher levels. They also corroborate the general intuitions that tacit collusion is easier in concentrated markets, and that higher-cost firms find tacit collusion more attractive than lower-cost firms.

2 Focal Points and Tests of Collusion

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.[1] 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.[2] The Folk Theorem is powerful, in the sense that it provides quite general conditions under which tacit (or explicit) collusion may be sustainable. However, this generality leads to difficulty in conducting empirical tests for collusion or tacit collusion; we discuss this is more detail below.

In practical terms, the problem of tacit collusion often reduces to one of coordination. Because firms can sustain nearly any set of strategies, the most important factor is often the construction of a mechanism through which firms can coordinate their pricing. In many documented instances of tacit collusion, firms maintain successful collusive arrangements because they build institutions that make coordination easier. In the early 1990s, for example, airlines used their computerized reservation system to communicate price infomation and pre-announce price increases.[3] In a similar case involving the Ethyl Corporation, firms allegedly used publicly listed prices and price pre-announcements to achieve tacit collusion.[4] In a well-known case involving General Electric and Westinghouse, the two large manufacturers of turbine engines developed a standardized method of pricing that resolved the coordination problem. Other examples of industries in which tacit collusion may have emerged include cigarettes and steel.

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

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

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

[3] See "Rapid Price Communication and Coordination: The Airline Tariff Publishing Company Case," in Kwoka and White eds. (1999).

[4] See "Facilitating Practices: The Ethyl Case (1984)," in Kwoka and White eds. (1999).

2.1 Empirical Implications of Tacit Collusion at a Focal Point

Because firms may sustain tacit collusion under a variety of observationally equivalent mechanisms, we do not attempt to explicitly model the process by which a focal point facilitates tacit collusion. Rather, we develop empirical implications of the focal point hypothesis by making observations regarding the patterns of pricing that we would observe if a focal point were facilitating tacit collusion. In our case, the focal point is a price ceiling; this complicates matters by introducing the possibility that the ceiling is binding. We therefore discuss our tests of the focal point hypothesis in the context of this alternative hypothesis.

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 econometrics of this test below.

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

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

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[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 Appendix A 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.

In addition to the above implications of pricing at focal points, we note two general empirical implications of tacitly collusive pricing. The first is that tacit collusion is generally viewed as easier to maintain among a low number of firms. Thus, we will be more likely to observe successful tacit collusion when market concentration is high. The second is that we might expect larger firms to be more likely to cooperate than smaller firms. Given that cheating is attractive because it steals business from other firms, a small firm will find the gains from cheating proportionately larger than a larger firm. This implies that the probability that a firm matches the ceiling even though it is not binding should be an increasing function of firm size.

2.2 Testing for Collusion and Tacit Collusion

Most empirical tests for collusion or tacit collusion use one of two approaches. The first approach involves testing whether the distribution of prices thought to reflect collusion or tacit collusion is different from a control distribution thought to reflect non-collusive behavior. In such tests, a central question is whether the candidate distribution of collusive prices can be identified *a priori* – for example, because it comes from a group of firms accused or convicted of collusion. When the candidate and control distributions can be identified in this way, it is relatively straightforward to test for equality of the distributions. Rejection of equality is taken as evidence of tacit or explicit collusion; more finely characterized tests can often rule out plausible alternative hypotheses. Examples in this line of work include Porter and Zona (1993, 1999).[8]

When the candidate set of collusive prices can not be identified *a priori*, it may be possible to endogenously identify the collusive and non-collusive distributions, often using some form of mixture modeling. Porter (1983), for example, uses a switching regression to endogenously classify periods of pricing into collusive and non-collusive regimes. Ellison (1994) uses a similar approach that defines the transition probabilities between collusive and non-collusive periods using a Markov process. In each of these cases, the data clearly identify periods of collusive and non-cooperative behavior.

A second approach to testing for tacit collusion is less direct. This line of work typically tests theoretical predictions regarding the sustainability of tacit collusion as a function of exogenous factors - such as demand, costs, or the quality of information. For example, Borenstein and Shepard (1999) find that current retail gasoline margins fall when expected future profits fall. This result is

[8]Porter and Zona corroborate their tests in various ways. In Porter and Zona (1999), for example, they show that bids for non-cartel firms are correlated with firm-specific costs in an intuitive manner, while bids from cartel firms are not.

consistent with the Rotemberg and Saloner (1986) and Haltiwanger and Harrington (1991) models of tacit collusion. Porter (1987) and Ellison (1994) both test the prediction that unanticipated demand shocks should trigger price wars, as in the model of Green and Porter (1984). Albaek et al. (1997) show that prices for two grades of concrete in Denmark rose by 15-20 percent following the institution of a regularly published list of industry prices. They argue that the list facilitated tacit collusion by improving the quality of price information.

Our empirical approach possesses features of each approach mentioned above. While we do not fully identify the observations thought to reflect non-cooperative behavior, we do maintain the assumption that tacit collusion can only occur at the focal point. Thus, we can rule out tacit collusion for observations with prices below the ceiling (or issuers that face no ceiling).[9] This yields a control group of observations that 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 admit the possibility that some firms match the ceiling because it is binding. The presence of these observations requires a means of endogenously separating the observations that reflect tacitly collusive behavior from those that do not. This problem is similar in spirit to that faced by Porter (1983) and Ellison (1994). However, in their case the problem is simplified by the fact that tacitly collusive and non-cooperative regimes lead to different observations of the dependent (price) variable. This permits a switching regression to endogenously identify collusive and non-collusive regimes.

In our case, a given observation at the ceiling may reflect either collusive or non-collusive behavior. We resolve this complication by using an econometric specification that expands upon traditional models for censored data. The specification introduces an independent probability that we observe a limit observation (that matches the ceiling) even for observations for which the true value of the dependent variable should be below the limit. This independent probability is the probability of tacit collusion for firms whose non-collusive price would lie below the ceiling.

The final component of our approach parallels Borenstein and Shepard's work. Because our full specification allows the probability of tacit collusion to vary by observation based on issuer- and state-specific factors, we can test hypotheses regarding the relationship between the sustainability of tacit collusion and exogenous factors thought to affect the viability of tacit collusion: the level of the ceiling, firm-level costs, market concentration, and firm size.

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

[9]Another interpretation of our tests is that we are estimating the differential impact of the focal point on firms' ability to tacitly collude.

discuss the relevant institutional detail.

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

3.1 Interest Rate Ceilings in the Credit Card Market

In 1979, most credit card issuers faced state-level ceilings on credit card interest rates.[10] 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.[11] 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.[12] The pattern for the state-level data is nearly identical to that for the issuer-level data.[13]

In 1979 ceilings existed in over ninety percent of states in our sample. The ceilings varied across states, but the most common ceiling was eighteen percent, which prevailed in nearly eighty percent of states. In 1979 a few states had ceilings below eighteen percent; usually these ceilings were imposed at fifteen or twelve percent.[14] In response to high inflation, and also as part of a general trend toward deregulation, in the early 1980s many states chose to remove or raise their interest rate ceilings. From 1981 to 1984, the percentage of states with no ceiling or a ceiling above eighteen

[10]The ceilings bound the behavior of credit card issuers based on their state of incorporation. However, they did not restrict interstate marketing of credit cards. For example, in 1984 Citibank was incorporated in New York and therefore faced a ceiling of 25%. It could offer a credit card to a customer in Maryland at 19.8%, despite the fact that Maryland's own issuers faced a ceiling of 18%. In practice, these nuances of regulation were unimportant in the early to mid-1980s, because nearly all issuers restricted their marketing efforts to their home states. We discuss the advent of heightened national competition (and its effect on state-level tacit collusion) later in the paper.

[11]During the sample period, ceilings applied for other types of debt as well. 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.

[12]The number of firm-level observations falls significantly over the sample period, from a high of 183 in 1980 to a low of 106 in 1989. This is primarily due to the fact that the number of banks participating in the survey from which the data are taken falls from 236 to 167 over the same period (not all banks report credit card rates).

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

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

percent rose dramatically. By 1983 no state in the sample had a ceiling below eighteen percent. After 1984, the cross-sectional pattern of ceilings remains fairly static.

3.2 Pricing in the Credit Card Market, 1979-1989

There are two striking aspects of credit card pricing during the 1980s.[15] The first is the extent of "clustering" at certain interest rates; we discuss this in detail below. The second is the infrequency with which card issuers adjusted their interest rates. For example, in our sample the average "spell" during which a given issuer's credit card rate remains unchanged is more than five years. These two factors seem puzzling, because they seem to defy conventional notions of pricing in competitive markets.[16] It is certainly true that no other loan market displays similar pricing patterns during the same time period.

An examination of interest rate ceilings and pricing reveals that both clustering and stickiness are explained by the fact that throughout the sample period, most issuers charged interest rates equal to the ceiling of their home state. Table 2 describes this broad pattern of interest rate clustering at ceilings. Our interest rate data are taken from the "*Quarterly Report of Rates of Selected Direct Consumer Installment Loans*," a survey collected quarterly by the Federal Reserve Board. Banks voluntarily participate in the survey.[17]

In states with ceilings, well over eighty percent of issuers matched their ceiling in the early years of the sample. The clustering is most pronounced in states with ceilings at 18 percent (the most common ceiling). It is interesting to note that in the early years of the sample, clustering is more

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

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

[16]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.

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

pronounced in states with ceilings at 18 percent than in states with ceilings below 18 percent. Even in states with ceilings above eighteen percent, the majority of issuers match the ceiling in the early part of the sample. The extent of clustering falls over time, but still remains over forty percent at the end of the sample period.

A corollary of the relationship between clustering and ceilings is the extent to which state-specific factors explain cross-sectional variation in interest rates. The bottom two rows of the table show the R-squared figures from a series of year-by-year cross-sectional regressions with the issuer-level interest rate as the dependent variable. In the first row, the explanatory variables are a set of fixed state effects. In the second, the only explanatory variable is the level of the interest rate ceiling faced by the issuer.[18] 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.[19] The R-squared from the regression including state effects is significantly higher; in most years it is greater than 0.70 and reaches a high of 0.85. This indicates that while interest rates ceilings are an important determinant of state-level variation in interest rates, there are other important state-level determinants of interest rates. We will attempt to control for these state-specific factors in the empirical work.

We should note that the clustering and stickiness of rates did attract the attention of lawmakers, academics and antitrust authorities concerned about the level of competition among card issuers. Members of Congress at various times implied that issuers were engaging in tacit or explicit collusion.[20] Ausubel (1991) noted that the stickiness of interest rates might imply a "failure of competition." In California, the state Attorney General brought price-fixing charges against three of the state's largest credit card issuers. The suit alleged explicit collusion on interest rates by First Interstate Bancorp, Wells Fargo and Bank of America between 1982 and 1986. First Interstate and Wells Fargo settled and agreed to pay $55 million in damages, while Bank of America was acquitted at trial. Another suit in Chicago, again alleging direct price-fixing, was dismissed in the early 1980s.

In summary, the fact that issuers were setting interest rates equal to their state-level interest

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

[19]Excluding a constant term from these regressions leaves the r-squared figures essentially unchanged. The coefficients on the level of ceiling variable are over 0.90 in every year, when the constant is excluded. With the constant included, the coefficients on the level of ceiling variable range from 0.82 in 1979 to 0.40 in 1989, and fall continuously throughout the sample period.

[20]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%." In an ironic response given our findings, Schumer advocated the imposition of a national interest rate cap in response to what he perceived as insufficient competition in the credit card market.

rate ceilings explains both the clustering of credit card interest rates and the relative inflexibility of rates during the 1980s. Moreover, the extent to which state-specific factors explain cross-sectional variation in pricing suggests that the relevant level for our analysis is the state. Focusing on the state is also suggested because interest rate ceilings - the potential focal point for tacit collusion - vary at the state level. In addition, much of the regulatory and antitrust activity regarding credit card issuers during this period (such as the suits mentioned above) took place at the state level. Finally, we should note that in contrast to the situation in the 1990s, during the 1980s the vast majority of credit card customers held cards issued by a bank in their home state.[21] This more regional orientation reflected the residual impact of interstate banking restrictions, which still applied to most banking services.[22] While a group of larger issuers did market their cards nationally, their aggregate market share during the early part of the decade was still fairly small.

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

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

While the summary data presented in this section can not conclusively establish or rule out tacit collusion, they do shed light on patterns of pricing across states and firms. In particular, we can note that the most plausible alternative explanation for "clustering" at ceilings 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 rows show average credit card interest rate data for the banks in our sample. There are two broad patterns. First, the average interest rate rises from 1979 to 1983, the gradually falls over the remainder of the sample period. The increase in the early part of the decade is consistent with the pattern of deregulation

[21] A 1984 Survey by Synergistics Research Corp. quoted in the American Banker (September 10, 1984) notes that only 8-9 percent of customers with incomes above $15,000 held a card from an out of state bank.

[22] Interstate banking law restricted banks from offering banking services (other than credit cards) to out-of-state customers. Because banks used their existing customer bases to target credit card customers, they rarely attracted customers from other states.

during that time. The fall later in the sample may be due to falling interest rates in the economy as a whole.

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

The second set of rows shows auto loan rates for the banks in our sample.[23] Auto loans track other interest rates in the economy quite closely, including the cost of funds for banks. It is therefore likely that this auto loan rate captures state- and issuer-specific components of marginal cost. This allows us to use the credit card/auto loan margin as a proxy for the issuer's interest margin on credit cards. It is important to note that there is little difference in auto loan rates across states with different ceilings. Thus, it seems unlikely that credit card rates are higher in states with high ceilings simply because issuers in these states have higher costs.

The next rows report the gap between credit card interest rates and auto loan rates. Again, if the auto loan rank captures issuer-specific costs, this gap should be correlated with the interest rate margin on credit cards. The primary rows show the average gap in states with ceilings of eighteen percent, in states with ceilings greater than eighteen percent, and in states with no ceiling.[24] In nearly every year, this gap is highest in states with ceilings greater than eighteen percent. The gap is lower in states with no ceiling, and still lower in states with an eighteen percent ceiling. This contradicts the notion that the margin should be lower in states with ceilings than in states without ceilings.

The sub-headings in the last set of rows provide more information, by comparing the credit card-auto loan gap for issuers at the ceiling ($p = C$) to the gap for issuers pricing below the ceiling ($p < C$), within states with a given ceiling. In both cases ($C = 18\%$ or $C > 18\%$), the gap is significantly higher for banks at the ceiling than for banks below the ceiling. Again, this is surprising. We would imagine that if interest rate ceilings imposed a constraint, and the auto loan rate captured variations in marginal cost over time and across issuers, that the gap would be greater for issuers below the ceiling than for issuers at the ceiling.

As a concluding point, while it is likely that variation in the auto loan rate captures a component

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

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

of variation in marginal cost at the issuer level, it is certainly possible that there are unobserved components of marginal cost that are driving issuers to the ceiling. We address this issue more fully later in the paper, but we note at this point that for it to explain the observed differences in margins, the unobserved component of costs would have to be large.

4 Specification of the Model

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

4.1 Baseline Specifications

Consider a reduced-form pricing equation describing an issuer's non-cooperative interest rate, p^*_{it}:

$$p^*_{it} = X_{it}\beta + \varepsilon_{it}, \quad \text{with} \quad \varepsilon \sim N(0, \sigma_1). \tag{1}$$

This non-cooperative price is that which the issuer would set in the absence of tacit collusion at the focal point. The vector X_{it} includes issuer- and state-specific cost and demand variables, market structure variables, and a set of fixed state and year effects.[25] For issuers in states without ceilings, our observation of the data will simply be $p_{it} = p^*_{it}$.[26]

[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 four 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.

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

[26]Our specification does not incorporate 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.

For issuers in states with ceilings, the ceiling may be binding. This will censor the data, although it should not change anything else about the pricing relationship:

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

.

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

$$L = \prod_{I^{ceil}_{it}=1} \left[\prod_{p_{it}=C_{it}} \Phi\left(\frac{C_{it} - X_{it}\beta}{\sigma_1}\right) \prod_{p_{it}<C_{it}} \sigma_1^{-1}\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right)\right] \cdot \prod_{I^{ceil}_{it}=0} \sigma_1^{-1}\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right). \tag{3}$$

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

It is worth noting that the standard Tobit model in the specification above implicitly assumes that the limit observations (those for which $p^*_{it} = C_{it}$) are drawn from the same distribution as the non-limit observations. If in fact some issuers at the limit are tacitly colluding (meaning that their non-cooperative interest rate $p^*_{it} < C_{it}$, but that they are setting $p_{it} = C_{it}$), the coefficients in the Tobit specification will be biased. A simple alternative that may be less vulnerable to this problem is a truncated regression model of pricing in states, rather than a Tobit.[27] Combining the truncated model with OLS for issuers in states without ceilings yields:

$$L = \prod_{I^{ceil}_{it}=1} \left[\prod_{p_{it}<C_{it}} \sigma_1^{-1}\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right) \left[\Phi\left(\frac{C_{it} - X_{it}\beta}{\sigma_1}\right)\right]^{-1}\right] \cdot \prod_{I^{ceil}_{it}=0} \sigma_1^{-1}\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right). \tag{4}$$

This model does not use any limit observations in estimation. It will yield unbiased estimates of the coefficients β even if some issuers are tacitly colluding, as long as the probability of tacit collusion is uncorrelated with the right-hand side variables, and the fact that other issuers are tacitly colluding leaves the non-cooperative pricing relationship unchanged.

A final point regarding our specification of the pricing equation is that we allow for the pricing relationship to differ across issuers in states with and without ceilings, by including in X_{it} a dummy

[27] The truncated regression uses only non-limit observations in estimation, but accounts for the selection bias that may result from the truncation.

variable equal to one if the issuer faces an interest rate ceiling.[28] Allowing the relationship to differ will capture the effects of ceilings on issuers that price below their ceiling. The predicted sign of the dummy variable coefficient is indeterminate; it depends on the extent to which ceilings bind and/or facilitate tacit collusion.[29] If the effects of tacit collusion dominate, the coefficient will be positive, while if ceilings are prdominantly binding, the coefficient will be negative.

4.2 Modeling Tacit Collusion

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

We begin by defining an indicator of tacit collusion:

$$w_{it} = \begin{cases} 1 \text{ if issuer } i \text{ is tacitly colluding at time } t, \\ 0 \text{ otherwise.} \end{cases} \tag{5}$$

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 non-cooperative price, $p_{it} = p^*_{it} = X_{it}\beta_1 + \varepsilon_{1it}$. In states with ceilings, issuers for which the ceiling is binding ($p^*_{it} \geq C_{it}$) match the ceiling. Among the issuers for which the ceiling is not binding, some issuers may tacitly collude ($w_{it} = 1$), and set $p_{it} = C_{it}$. The remaining issuers do not tacitly collude, and set a price equal to their non-cooperative price. More formally,

$$p_{it} = \begin{cases} p^*_{it} \text{ if } I^{ceil}_{it} = 1,\ p^*_{it} < C_{it}, \text{ and } w_{it} = 0; \\ p^*_{it} \text{ if } I^{ceil}_{it} = 0 \\ C_{it} \text{ otherwise.} \end{cases} \tag{6}$$

A simple way to allow for tacit collusion is to model w_{it} as the outcome of a latent process determining the attractiveness of tacit collusion. If the 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,

[28] In an earlier version of the paper, we allowed all of the β coefficients to differ across issuers in states with and without ceilings. The only statistically significant difference in the coefficients was that the coefficient on the HHI was more positive in states with ceilings.

[29] This assumes that the underlying best response functions in a given state are upward sloping and prices are strategic complements.

$$L = \prod_{I_{it}^{ceil}=1} \left[\prod_{p_{it}=C_{it}} \left\{ \Phi\left(\frac{C_{it} - X_{it}\beta}{\sigma_1}\right) + \alpha\Phi\left(\frac{X_{it}\beta - C_{it}}{\sigma_1}\right) \right\} \right. \tag{7}$$
$$\left. \cdot \prod_{p_{it}<C_{it}} \sigma_1^{-1}(1-\alpha)\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right) \right] \cdot \prod_{I_{it}^{ceil}=0} \sigma_1^{-1}\phi\left(\frac{p_{it} - X_{it}\beta}{\sigma_1}\right).$$

The model yields an estimate of α that is constant across all issuers and time periods. This model falls within the class of "bivariate" approaches to Tobit modeling, in which the probability of observing a limit observation is determined distinctly from the level of the dependent variable (p_{it}^* in our case). The particular specification above is known as the "p-Tobit" because it estimates a single probability that an observation that should be below the limit is in fact observed at the limit. The model was first presented by Deaton and Irish (1984) in an attempt to explain the underreporting of British tobacco expenditures, and has typically been applied in similar contexts in labor economics.[30]

Because we do possess observables that should be correlated with the sustainability of tacit collusion (e.g., concentration), we also allow α to be a function of covariates that affect the sustainability of tacit collusion. This involves estimating a set of specifications using the "double-hurdle" model proposed by Cragg (1971), in which the indicator w_{it} is the binomial observation of a latent variable w_{it}^*, where $w_{it}^* = Z_{it}\gamma + \upsilon_{it}$ and $\upsilon_{it} \sim N(0,1)$. The probability that a given issuer tacitly colludes is then $\alpha_{it} = \Phi(-Z_{it}\gamma)$. Incorporating this into the likelihood function above yields:

$$L = \prod_{I_{it}^{ceil}=1} \left[\prod_{p_{it}=C_{it}} \left\{ \Phi\left(\frac{C_{it} - X_{it}\beta}{\sigma_1}\right) + \Phi(-Z_{it}\gamma)\Phi\left(\frac{X_{it}\beta - C_{it}}{\sigma_1}\right) \right\} \right. \tag{8}$$

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

$$\cdot \prod_{p_{it}<C_{it}} \sigma_1^{-1}\Phi\left(Z_{it}\gamma\right)\phi\left(\frac{p_{it}-X_{it}\beta}{\sigma_1}\right)\Bigg] \cdot \prod_{I_{it}^{ceil}=0} \sigma_1^{-1}\phi\left(\frac{p_{it}-X_{it}\beta}{\sigma_1}\right)$$

Note that we can view the p-Tobit model as just a special case of the double-hurdle, in which $\alpha = \Phi(\lambda)$, where λ is a constant.[31]

To summarize, our model begins with a simple formulation of an issuer's non-cooperative price. It allows this pricing equation to differ for issuers in states with ceilings and without ceilings. We then allow for the possibility that the data may be censored because issuers face ceilings that are binding. Estimating this model using a Tobit specification maintains the assumption that tacit collusion is not occurring, while estimating it using the truncated regression model does not maintain this assumption. Finally, we broaden the model to explicitly allow for tacit collusion, and to estimate the probability that a given issuer is tacitly colluding. We now describe the variables included in the regressions, and discuss some econometric issues.

4.3 Variables

In this section, we outline the variables included in the pricing equation, X_{it}, and also the variables included in the vector Z_{it}. Table 4 contains summary statistics for these variables. It presents the data stratified based on whether the observation comes from a state with or without a ceiling.[32]

The vector X_{it} in the pricing equation include variables that capture demand, cost, and market structure. The primary issuer-specific cost proxy is the issuer's auto loan rate. We also include the state-level default rate on credit card debt as an additional control for state-level costs. The X_{it} vector also includes two state-level demographic variables. The first is average weekly income per capita, adjusted for inflation. The second is the state-level unemployment rate. These variables are generally intended to capture state-specific variations in demand. Finally, the X_{it} vector also includes a set of annual time dummy variables, and a set of fixed state effects. These variables are intended to pick up the effects of unobserved variables that affect prices identically for all issuers within a year, or all issuers within a state during the entire sample period.

[31] 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.

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

We also include in X_{it} the state-level Herfindahl index in credit cards.[33] Given the more regional nature of competition during this time, this variable may capture cross-state variation in the competitive environment.[34] Another variable included in X_{it} is the outstanding credit card loans measured at the issuer level, adjusted for inflation. This variable is somewhat difficult to interpret because loans may be positively or ngatively correlated with issuer-level costs; larger issuers tend to have higher default rates, but there are also significant scale economies in credit card operations. It is also possible that larger issuers may have market power.[35] These conflicting effects imply that we should exercise caution in the interpretation of the coefficient on loans. Because both the HHI and the credit card loans variables are highly skewed, we include the logarithm of each in the X_{it} vector.

The Z_{it} vector includes variables that should affect the sustainability of tacit collusion. It includes the level of the interest rate ceiling faced by the issuer. Under the focal point hypothesis, tacit collusion is more difficult to sustain under higher ceilings. The Z_{it} vector also includes the issuer's auto loan rate. The auto loan rate is a proxy for issuer-specific costs; the higher are these costs, the easier is tacit collusion at the ceiling. The Z_{it} vector also includes the state-level HHI in credit cards. This tests whether tacit collusion is easier to sustain in markets with greater concentration. We also include the issuer's credit card loans in Z_{it}, to test the hypothesis that larger issuers are more likely to tacitly collude. Finally, we also include in Z_{it} a vector of time dummies, to capture any systematic influences on the sustainability of tacit collusion (based on competition from the national market, for example).[36]

4.3.1 Econometric Issues

Of the variables in X_{it} and Z_{it}, we would expect that the issuer's interest rate might affect both the issuer's credit card loans and the Herfindahl Index; this raises endogeneity concerns. To deal 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 assets.[37]

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

[34] A number of studies have found that state-level concentration in banking is correlated with state-level price-cost margins.

[35] Stango (2000) finds that larger issuers have higher interest rates, even controlling for issuer-level default.

[36] We do not include state effects in the Z vector.

[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).

Where the observation is missing a value for credit card loans, we fill the missing value for the variable using the first-stage regression containing the total assets instrument.[38]

In unreported results, we also consider the possibility that the price ceiling itself is endogenous. This would be true if ceilings were imposed in reaction to the state-level competitive environment in credit cards. While this seems unlikely, if it were true we might observe a correlation between prices and price ceilings even absent tacit collusion. To account for this, we instrument for the level of the price ceiling using a vector of banking regulation variables.[39] Results from these specifications are nearly identical to those reported below.

4.4 Results

Table 5 presents the results of regressions using our baseline specifications – the Tobit and truncated regression models. We present results from specifications including and excluding the dummy variable I_{it}^{ceil}. The coefficients on the other variables do not change when I_{it}^{ceil} is included.

The Tobit and truncated regression coefficients are generally similar; the exception is that the cost variables (auto rate and default) are not significant in the Tobits, but positive and significant (consistent with expectations) in the truncated regresssions. This suggests that the Tobit may be poorly specified relative to the truncated regressions. This is worth noting, because if the limit observations are drawn from the same distribution as the non-limit observations the Tobit and truncated regressions would yield identical results. This is preliminary evidence that the limit observations are drawn from a different distribution.

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. The size of the coefficient implies that a doubling of issuer size is associated with an interest rate roughly 14 basis points higher; this may seem small, but in our sample larger issuers may be more than

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However, in our sample credit cards comprise less than 5% of total bank assets on average, so the endogeneity concern is greatly reduced using the instruments.

[38] Note that because of these missing values, we can not measure total assets net of credit card loans - which would be a more appropriate instrument.

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

one hundred times larger than smaller issuers. The coefficients on the demographic (unemployment and income) variables suggest that rates are higher in states with weaker economic environments.[40]

In both the Tobit and truncated regressions, the coefficient on I_{it}^{ceil} is positive and statistically significant. This suggests that prices are higher in states with ceilings than in states without ceilings. Recall that this coefficient will capture the net effect of tacit collusion and binding ceilings. The positive coefficient implies that the effects of tacit collusion offset the effects of binding ceilings.

Table 6 shows results from the p-Tobit and double-hurdle specifications. We estimate the p-Tobit model by specifying a double-hurdle model in which $\alpha = \phi(\lambda)$, where λ is a constant; the regression results show the estimate of λ.[41] The second column shows results from 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; thus, the "year dummies" pertain to 1980-1, 1982-3, etc.[42] In general, the pattern of coefficients in the pricing equation is similar to that in the Tobit and truncated regression models. Interestingly, default is no longer statistically significant in these specifications. We also note that in the fuller specifications the coefficient on I_{it}^{ceil} is no longer significant (although its t-statistic is greater than one); thus, we can not reject the hypothesis of equality in the pricing equation across states with and without ceilings.

In the first column, we estimate that the sample-wide probability α of tacit collusion is roughly nine percent.[43] 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 (such as the double-hurdle) that allow α to vary by observation.[44] This is borne

[40] The magnitudes of the coefficients (in Model 2b) imply that one standard deviation changes in unemployment and income lead to changes in interest rates of roughly 65 and 45 basis points.

[41] Note that the regression estimates λ, and that $\alpha = \Phi(\lambda)$. The estimate of λ is in units of standard deviations away from zero in the standard normal distribution. Thus, if $\lambda = 0$, $\alpha = 0.50$; thus, a standard t-statistic is inappropriate for assessing the significance of the coefficient. 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 identical to those using $\alpha = \Phi(\lambda)$.

[42] This restriction is not rejected.

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

[44] Maki and Nishiyama (1996), for example, find that the p-tobit estimate of α is less than zero, but that the

out by the results from the year-dummy double-hurdle model in the next column. In 1979, the year-dummy double hurdle model yields an estimate of $\alpha = 0.82$. By 1982-3 the probability falls to twenty-seven percent, and by 1986-7 to just over eight percent. There is a clear downward trend in the probability of collusion over the sample period.

The last two columns of Table 6 show results from the full double hurdle models. These specifications include the vector of variables in Z_{it} that should influence the sustainability of tacit collusion, as well as the time dummies. The Z_{it} vector coefficients show a pattern that is generally consistent with our discussion of factors affecting the sustainability of tacit collusion. In every column, the coefficient on the level of the price ceiling is negative and significant, suggesting that the facilitative power of the ceiling dissipates as 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. And, the coefficient on the auto loan rate is positive and significant (in Model 5), suggesting that high-cost issuers are more likely to tacitly collude. This is also consistent with our expectations. The coefficient on loans is not significant in either column.

An interesting feature of the results in this table is that when the covariates in Z_{it} are included, the coefficients on the time dummies change. There are essentially no changes in the level of the intercept between 1979 and 1985, and a sudden downward shift in 1986-7 that continues until the end of the sample. This downward shift reduces α at the means from fifty percent to under twenty percent.

In summary, the results provide fiarly 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 pattern of matching is consistent with our expectations: it is less likely in states with high ceilings, more likely in concentrated states, and more likely for issuers with high costs. We also find that instances of tacit collusion become less likely throughout the sample period - markedly so after 1986. In the next section we expand upon these points.

4.5 Some Further Detail on the Economic Implications of the Results

To highlight the economic significance of the variables in Z, we present in Table 7 the predicted probability α by year, ceiling, concentration, and auto loan rate. The categories "high" and "low" refer to figures one standard deviation above the mean.[45] As the table indicates, concentration

average value of α is roughly 0.35 in the double hurdle specification that allows α to vary by observation.

[45]The means and standard deviations are measured within the two-year period indicated by the column entries. Note that both HHI and loans enter the regressions in logs; thus, the standard deviations are in logs as well. The

and issuer size have a fairly small impact on the probability of tacit collusion; moving from "High ALR, High HHI" to "Low ALR, Low HHI" reduces the probability of tacit collusion by fewer than ten basis points in every case. The year and interest rate ceiling effects are much larger. Within a given year, moving from a ceiling of eighteen percent to a ceiling of twenty-four percent reduces α by a factor of five or ten. Across years, the overall level of α falls from over ninety percent to under twenty percent; most of this decline occurs between 1985 and 1986.

To highlight the economic impact of ceilings, in Table 8 we provide some measures of the their overall effects. To this point, we have discussed only the extent to which ceilings facilitated tacit collusion. But the ceilings clearly were binding for many issuers as well. The overall effects of ceilings on prices might be positive or negative, based on the relative magnitudes of these opposing influences. To construct the table, we use the coefficients from the pricing equation in Table 6 to construct fitted values $\hat{p}$ for issuer interest rates.[46] For all issuers with observed rates at ceilings ($p = C$), we classify those with predicted rates below the ceiling ($\hat{p} < C$) as having $w = 1$, which indicates that the issuer is tacitly colluding. Issuers for which ($\hat{p} \geq C$) are classified as having $w = 0$. The table shows the average positive price effect for issuers with $w = 1$, the average negative price effect for issuers with $w = 0$, and the number of issuers in each category. As the table indicates, in the early years of the sample the effects of tacit collusion dominate. Ceilings lead to higher prices overall from 1979-81, and at the end of the sample period. The overall effect is negative in the middle years of the sample period. This pattern is driven in large part by the extent to which ceilings became binding (and therefore led to lower prices) in the early 1980s; before and after this period, the effects of tacit collusion offset the effects of binding ceilings. As would be expected, lower ceilings are more likely to be binding than higher ceilings.[47] Thus, the overall price effect of ceilings is quite mixed, as would be the effect on consumer welfare. We can say with fair certainty, however, that the consumer welfare effects of high ceilings ($C > 18\%$) are negative.

As a final point, it is worth remarking on our finding of a marked downward shift in the probability of tacit collusion after 1985. This result is in fact consistent with anecdotal evidence

marginal impacts of HHI and loans are roughly equal. Thus, we show results only for "high HHI, large issuer" and "low HHI, small issuer." Results for "high HHI, small issuer" and "low HHI, large issuer" are roughly equal to those at the means.

[46] In constructing the fitted values, we set $I^{ceil} = 0$ for all issuers. This predicts the counterfactual interest rate that an issuer would set in the absence of a ceiling.

[47] It is possible to compare these data, and the fitted values of α from Table 9, in order to assess their concordance. In most cases, the fractions of issuers in each category predicted in Table 9 are consistent with those "observed" in Table 10. (We use quotes because the proportions of issuers tacitly colluding and bound in Table 10 are in fact based on fitted values from the pricing equation.)

regarding changes in the competitive environment in credit cards during this time.[48] In 1986, many of the largest nationally marketed issuers cut their rates, moves that signaled heightened competition at the national level. This may have reduced the state-level incentives to tacitly collude.[49] Many larger issuers intensified their mailout campaigns as well, which would have had similar effects. Accounting data from this time period also show a sharp drop in return on assets from 1985 to 1986, due to higher default rates and lower interest rates. 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.[50]

5 Discussion

In this section we discuss some alternative explanations for the results. In essence, our results identify two distinct price distributions for issuers - based on our specification of the pricing equation. We hypothesize that the distribution with a lower mean reflects non-collusive behavior, while the distribution with a higher mean reflects tacitly collusive behavior. The results of the double hurdle model corroborate this hypothesis, because we find that "switching" across these distributions is consistent with theories of tacit collusion at focal points.

Despite these findings, one might think that a plausible alternative explanation for the results would be that our pricing relationship is subject to measurement error or omits important variables. This might lead to the spurious identification of separate price distributions. In practice, however, this is unlikely. Let us first consider the possibility that the dependent variable - price - is measured with error. This might occur in two ways. First, our measured interest rate for each issuer may not completely measure the "price" of carrying a credit card because it does not include the issuer's annual fee. If issuers traded higher interest rates for lower annual fees, we would mis-identify issuers with low annual fees as engaging in tacit collusion.[51] However, this is unlikely. Other data from

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

[49]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.

[50]It also seems plausible that if ceilings were facilitating tacit collusion and leading to supernormal profits, these profits would attract entry. This entry would ultimately lead to lower profits and increased competition. Unfortunately we do not possess adequate data to fully address this issue; it is certainly true, however, that there was widespread entry at the national level throughout our sample period.

[51]On average, annual fees made up roughly 15% of total revenue for issuers in our sample. Other components of

the credit card market generally show a positive relationship across issuers between interest rates and annual fees. Thus, it is likely that those issuers that we identify as engaging in tacit collusion (because their interest rates are drawn from a higher distributiuon) also charge higher annual fees. We also note that for this to explain the results of the double hurdle regressions, issuers' propensity to offer annual fees (conditional on their interest rate) would have to be correlated with their auto loan rate, their ceiling, and the state-level HHI. As the latter two variables are exogenous at the issuer level, this seems unlikely.

A second way in which prices may be measured with error is through intra-firm price variations. Some issuers charge different interest rates to different customers, primarily based on credit worthiness. We might mis-classify issuers with observed rates below their "true" rates as engaging in tacit collusion. First, we note as a practical matter that this sort of intra-firm price variation was little-used in the 1980s, particularly in the earlier part of the decade (when we observe tacit collusion most often).Second, we reiterate that this issuer-level measurement error would have to be correlated with loan rates, ceilings, and HHI in order to account for the results of the double hurdle model.

We might also consider the possibility that there are unobserved components of costs that are driving the results. This argument would be that we mis-classify firms with higher unobserved costs as tacitly colluding. This also inconsistent with the results, for three reasons. The first was discussed earlier in the paper; any unobserved component of costs would have to be large relative to the observable components of costs to account for the results. A second reason is more fundamental; if in fact the true underlying cost distribution were normal, the fact that high-cost issuers price at the ceiling would be captured within the standard Tobit model. The true underlying distribution of costs would have to be non-normal in order to drive the observed results. There is no evidence that this sort of cost structure exists in the credit card market. Finally, we note again that unobserved costs would have to be correlated with the variables in Z_{it}, in order to explain the results in Table 6. Given that HHI and ceiling are measured at the state rather than the issuer level, it is unlikely that they would be correlated with issuers' costs in a manner consistent with the double hurdle results. For example, it is difficult to see that a given issuer's unobserved costs would be negatively correlated with the interest rate ceiling within that issuer's home state.[52]

"price" such as late fees, and over-limit fees were a trivially small component of issuer revenue during this period.

[52]In fact, the correlation that would have to exist in order to yield the observed empirical result would be *opposite* to the correlation that we might expect if interest rates and ceilings were jointly determined (i.e. in the story that justifies instrumenting for the ceiling). This is corroborated by the fact that in the double hurdle model, the coefficient on the predicted interest rate ceiling is slightly more negative than the coefficient when the actual ceiling is included.

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

6 Conclusions

The notion that a non-binding price ceiling may facilitate tacit collusion has important policy implications.[53] The possibility that such caps might facilitate tacit collusion is largely ignored in most debates regarding their welfare effects. Our results suggest that caps can indeed facilitate tacit collusion. More generally, our results caution against the imposition of regulations that may allow firms to coordinate their behavior. As in the "concrete case" in Denmark discussed earlier in the paper, it appears that well-intentioned government intervention may facilitate coordination in a wider set of circumstances than previously thought.

The particular relevance of our results to the credit card market is that they explain a long-standing puzzle in credit card pricing - the stickiness of interest rates during the 1980s, and clustering at particular rates.[54] It is interesting to note that this stickiness has all but disappeared since the early 1990s. Most cards now use "variable rate" pricing under which the rate is pegged to the prime rate. The market is also characterized by widespread price discrimination and price dispersion, and non-price card characteristics are much more complex. This regime change in pricing is fairly discrete; it can be traced to 1991 and 1992. A host of factors appear to have contributed to it, among them threatened imposition of national rate cap, changes in the technology of credit scroing, and the entry by large nonbank issuers such as AT&T. Given the stability of pricing up

[53] For example, the current electricity policy debate includes discussions of the imposition (or re-imposition) of price caps; these caps would be non-binding during off-peak periods.

[54] While most nationally marketed issuers faced no ceiling (because they incorporated in Delaware), their interest rates also displayed significant clustering and stickiness. As late as 1991, eight of the largest ten issuers charged a rate of 19.8% to the majority of their customers.

to that point, the suddenness of the change evokes the idea that a tacitly collusive equilibrium was broken.

Finally, we should note that our paper in some sense only examined half of the focal point issue. We largely ignore the process by which firms coordinate to move from one tacitly collusive equilibrium to another, or achieve such an equlibrium in the first place. In our case, we simply seek to establish the existence of tacit collusion, as opposed to the acheivement of tacit collusion. We note some suggestive evidence on this point, however. In preliminary work we have examined changes in credit card rates, in both states that eliminated ceilings and states that simply raised their ceilings. Controlling for changes in auto loan rates, we find that rates rise by more in states that raise their ceilings than in states that eliminate them entirely.[55] This suggests that future work may be able to capture some dynamics of tacit collusion at focal points.

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

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A The Sustainability of Tacit Collusion at a Focal Point

In this section, we show some general results regarding the sustainability of tacit collusion at a focal point. Our example simplifies matters by considering competition between two firms. We also assume that $MC = 0$ for both firms. The results should easily generalize to settings with multiple firms and non-zero marginal cost.

We begin with some notation. First, define the general profit function

$$\Pi_1(p_1, p_2)$$

that indicates that firm 1's profits are a function of its own price and the price charged by others. We can also write

$$\Pi_1(p_1, p_2) = p_1 D_1(p_1, p_2)$$

We impose three conditions on profits and demand:

(1) Concavity of the profit function:

$$\frac{\partial^2 \Pi(p_1, p_2)}{\partial p_1^2} < 0$$

(2) The two goods are substitutes:

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

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

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

The sustainability of tacit collusion will depend on:

a. The profits under tacit collusion at the price ceiling,

$$\Pi_1(C, C)$$

where the arguments indicate that the function is evaluated where both firms charge prices equal to the focal point.

b. The profits from unilateral defection

$$\Pi(p_1^*(C), C)$$

where

$$p_1^*(C) = \arg\max_{p_1} \Pi(p_1, C)$$

the argument simply indicates that firm 1's optimal price is a function of firm 2's price.

c. The noncooperative profits (obtained when both firms play their short-run best responses). For simplicity we assume that these are zero.

A.1 Changes in the Sustainability of Tacit Collusion

We know that if collusion is sustainable at the current discount rate, it will become "less sustainable" if at any C,

$$\frac{d\Pi(p_1^*(C), C)}{dp_2} > \frac{d\Pi(C, C)}{dp_2}$$

We proceed in two steps.

I. We know that

$$\frac{d\Pi(p_1^*(NC), NC)}{dp_2} > \frac{d\Pi(NC, NC)}{dp_2}$$

where NC is the non-cooperative price. This must be true because the LHS allows re-optimization.

II. Then, we know that

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

for any $C > NC$.

We can see that this is true by writing the second derivatives of the profit functions under collusion and cheating. The first is:

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

where we note that some terms have dropped out because at the short-run best response,

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

The other second derivative is:

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

Here we make the simplifying assumption that the second derivatives of demand are small (zero) relative to the first derivatives. This is true, for example, when demand is linear.

If the second derivatives of demand are zero, $\frac{d^2\Pi(p_1^*(C),C)}{dp_2^2}$ is positive because $\frac{\partial p_1^*}{\partial p_2}$ and $\frac{\partial D_1(.)}{\partial p_2}$ are both positive. The second expression $\frac{d^2\Pi(C,C)}{dp_2^2}$ is strictly negative, because $\frac{\partial D_1(.)}{\partial p_2} + \frac{\partial D_1(.)}{\partial p_1} < 0$. (This has to be true if the own-price elasticity of demand is greater than the cross-price elasticity of demand). Thus,

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

Combining the two conditions implies that

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

for any $C > NC$.

B Tables

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

Year	**1979**	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
N (States):	36	40	39	35	33	34	35	37	35	32	33
Share of States:											
No Ceiling	6%	5%	10%	23%	33%	38%	37%	38%	37%	34%	33%
C > 18%	3%	5%	15%	23%	30%	29%	37%	38%	34%	34%	33%
C = 18%	81%	83%	69%	51%	36%	32%	26%	24%	29%	31%	33%
C < 18%	11%	7%	5%	3%	–	–	–	–	–	–	–
N (Issuers):	175	176	165	106	137	136	125	127	116	114	101
Share of Issuers:											
No Ceiling	2%	2%	7%	16%	26%	33%	33%	35%	34%	33%	35%
C > 18%	3%	6%	19%	25%	30%	31%	33%	32%	32%	30%	30%
C = 18%	82%	80%	67%	56%	44%	36%	34%	33%	34%	37%	35%
C < 18%	13%	13%	8%	4%	–	–	–	–	–	–	–

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

Table 2: Credit Card Pricing and Interest Rate Ceilings

	1979	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
Share of Issuers at Ceiling:											
All Facing Ceiling	83%	83%	85%	85%	73%	67%	65%	60%	48%	40%	43%
$C = 18\%$	88%	91%	92%	95%	90%	84%	86%	74%	62%	45%	47%
$C > 18\%$	0%	9%	13%	19%	24%	26%	29%	22%	9%	9%	7%
$C < 18\%$	68%	64%	86%	50%	–	–	–	–	–	–	–
R-Squared:											
Fixed State Effects	0.61	0.69	0.72	0.85	0.79	0.83	0.84	0.76	0.76	0.69	0.63
Level of Ceiling	0.37	0.45	0.41	0.65	0.41	0.46	0.46	0.28	0.31	0.25	0.21

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

Table 3: Average Interest Rates by Ceiling

	1979	**80**	**81**	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**
Avg CC Rate (%):											
C = 18%	17.4	17.6	17.7	17.9	17.7	17.7	17.7	17.4	17.1	17.0	17.1
C > 18%	17.5	18.6	18.7	19.3	19.7	19.7	19.7	19.2	18.6	18.4	18.3
No Ceiling	16.0	16.0	18.7	18.9	19.5	19.0	19.1	19.1	18.5	18.1	18.1
Avg Auto Rate (%):											
C = 18%	11.8	13.6	16.0	16.9	14.5	13.1	13.2	12.2	10.6	10.7	11.8
C > 18%	11.2	12.8	16.3	17.5	15.3	13.8	13.7	12.6	10.4	10.8	11.9
No Ceiling	11.8	13.9	16.9	18.2	14.6	13.1	13.4	12.2	10.3	10.8	11.9
Avg Gap (%):											
C = 18%	5.6	4.0	1.7	1.0	3.2	4.6	4.5	5.2	6.6	6.3	5.3
$p = C$	6.1	4.4	2.0	1.1	3.5	4.9	4.8	5.8	7.8	7.5	6.4
$p < C$	1.6	0.3	-1.0	-1.4	0.2	2.8	3.2	3.6	4.7	5.3	4.3
C > 18%	6.3	5.7	2.4	1.8	4.4	5.9	6.0	6.7	8.2	7.6	6.4
$p = C$	–	–	7.8	5.1	6.9	8.1	8.2	8.9	11.0	10.8	8.3
$p < C$	6.3	5.7	1.5	1.0	3.6	5.2	5.1	6.0	7.9	7.3	6.2
No Ceiling	4.2	2.1	1.8	0.7	4.9	5.9	5.7	6.9	8.2	7.3	6.2

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

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

	Entire Sample				$I^{ceil} = 0$		$I^{ceil} = 1$	
	Mean	**Std Dev**	**Min**	**Max**	**Mean**	**St Dev**	**Mean**	**St Dev**
Credit Card Rate	17.98	1.80	11.50	22.00	18.74	1.64	17.78	1.78
Price Ceiling†	19.13	2.51	12.00	25.00	—	—	19.13	2.51
Auto Rate	13.25	2.29	7.00	23.00	12.82	2.24	13.37	2.29
HHI in Credit Cards	1758	1495	203	9949	1845	1869	1735	1378
CC Loans‡(1000s$)	464	1534	0	28184	576	2603	512	1390
Default Rate	2.21	0.84	0.53	5.35	1.89	0.68	2.29	0.86
Unemployment Rate	7.31	2.40	2.40	18.00	6.81	2.23	7.43	2.43
Weekly Income‡ ($100s)	5.89	0.15	5.50	6.31	5.92	0.13	5.89	0.15
Sample Size	1478				312		1166	

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

Table 5: Tobit and Truncated Regression Models of Credit Card Rates

	Tobit		**Truncated**	
	Model 1a	**Model 1b**	**Model 2a**	**Model 2b**
Auto Rate	0.037	0.067	0.183***	0.236***
	(0.061)	(0.065)	(0.071)	(0.071)
HHI	0.271**	0.281*	0.108	-0.144
	(0.138)	(0.168)	(0.086)	(0.110)
CC Loans	0.205***	0.204***	0.186***	0.138***
	(0.041)	(0.044)	(0.037)	(0.047)
Default Rate	-0.090	-0.087	0.287***	0.370***
	(0.108)	(0.139)	(0.096)	(0.134)
Unemployment	-0.332***	-0.316***	0.161***	0.279***
	(0.077)	(0.072)	(0.062)	(0.072)
Income	-10.61***	-10.50***	-1.887***	-2.946***
	(1.47)	(2.24)	(0.719)	(0.849)
I^{ceil}		0.659**		0.243***
		(0.313)		(0.064)
σ	1.86***	1.86***	1.60***	1.60***
-N-	1478		739	

Specifications include fixed year and state effects (not shown).
* denotes significant at the 0.10 level, ** significant at the 0.05 level and *** significant at the 0.01 level.

Table 6: Double Hurdle Models of Credit Card Rates and Tacit Collusion

	Model 3	**Model 4**	**Model 5**	**Model 6**
Pricing Equation:				
Auto Rate	0.076*	0.166***	0.102*	0.060***
	(0.046)	(0.075)	(0.064)	(0.024)
HHI	0.233*	0.141*	0.092	0.105
	(0.139)	(0.079)	(0.078)	(0.124)
CC Loans	0.207***	0.160***	0.118***	0.161***
	(0.043)	(0.033)	(0.033)	(0.035)
Default Rate	−0.055	−0.035	0.130	−0.064
	(0.136)	(0.147)	(0.097)	(0.098)
Unemployment	−0.327***	−0.312***	0.139***	−0.294***
	(0.070)	(0.046)	(0.052)	(0.075)
Income	−8.077	0.980*	−1.839***	1.145
	(2.320)	(0.605)	(0.572)	(1.803)
I^{ceil}	0.861***	0.479	1.009	0.812
	(0.322)	(0.262)	(0.857)	(0.776)
Probability of Tacit Collusion:				
Constant	−1.307***	0.900***	6.905***	6.247***
	(0.259)	(0.087)	(0.670)	(0.811)
Price Ceiling			−0.432***	−0.344***
			(0.032)	(0.038)
Auto Rate			0.114***	0.056
			(0.029)	(0.044)
HHI			0.133*	0.078*
			(0.068)	(0.042)
CC Loans			−0.026	−0.001
			(0.035)	(0.004)
1980,1981		−0.991***		−0.337
		(0.173)		(0.256)
1982,1983		−1.501***		−0.522
		(0.178)		(0.413)
1984,1985		−1.628***		−0.637
		(0.187)		(0.607)
1986,1987		−2.287***		−1.443***
		(0.216)		(0.243)
1988,1989		−2.212***		−1.519***
		(0.286)		(0.304)
σ	1.44***	1.29***	1.23***	1.82***
-N-		1478		

Pricing equation includes fixed year and state effects.
* denotes significant at the 0.10 level, ** significant at the 0.05 level and
*** significant at the 0.01 level.

Table 7: Estimates of α by Ceiling, Auto Loan Rate, and Concentration

	1979	**1980-81**	**1982-83**	**1984-85**	**1986-87**	**1988-89**
Overall	0.92	0.86	0.70	0.50	0.19	0.16
C=18	0.90	0.88	0.86	0.77	0.45	0.38
High ALR, High HHI	0.92	0.91	0.88	0.80	0.52	0.42
Low ALR, Low HHI	0.87	0.85	0.83	0.73	0.41	0.35
C=20	0.72	0.69	0.65	0.52	0.21	0.16
High ALR, High HHI	0.76	0.73	0.69	0.56	0.26	0.19
Low ALR, Low HHI	0.68	0.63	0.60	0.47	0.18	0.14
C=22	0.46	0.42	0.38	0.26	0.07	0.05
High ALR, High HHI	0.50	0.47	0.43	0.30	0.09	0.06
Low ALR, Low HHI	0.41	0.37	0.33	0.22	0.05	0.04
C=24	0.21	0.19	0.16	0.09	0.01	0.01
High ALR, High HHI	0.25	0.23	0.19	0.11	0.02	0.01
Low ALR, Low HHI	0.18	0.15	0.13	0.07	0.01	0.01

Notes: Estimates of α are fitted values from Model 6. *Overall* row values are fitted using within-column means of variables in Z. Other row values are fitted at the mean level of loans. "High" and "Low" are one standard deviation from the mean, measured within columns.

Table 8: Average Price Effects of Ceilings

	1979	**1980-81**	**1982-83**	**1984-85**	**1986-87**	**1988-89**
All Issuers	2.04 (142)	0.38 (258)	-1.58 (128)	-0.41 (101)	-0.35 (67)	0.34 (41)
w=0	-1.79 (9)	-1.25 (102)	-1.85 (117)	-1.52 (68)	-1.21 (49)	-0.57 (30)
w=1	2.29 (133)	1.46 (156)	1.36 (11)	1.89 (33)	1.98 (18)	2.83 (11)
C=18	2.11 (127)	0.51 (227)	-0.90 (111)	-0.66 (78)	-0.58 (55)	0.16 (36)
w=0	-2.49 (5)	-1.20 (82)	-1.01 (106)	-1.42 (60)	-1.17 (45)	-0.59 (28)
w=1	2.30 (122)	1.47 (145)	1.52 (5)	1.86 (18)	2.10 (10)	2.80 (8)
C>18	–	-1.45 (5)	-0.94 (15)	0.46 (23)	0.69 (12)	1.62 (5)
w=0	–	-1.94 (4)	-2.38 (9)	-2.27 (8)	-1.61 (4)	-0.29 (2)
w=1	–	0.51 (1)	1.21 (6)	1.92 (15)	1.84 (8)	2.89 (3)
C<18	1.43 (15)	-0.35 (26)	-1.37 (2)	–	–	–
w=0	-0.91 (4)	-1.37 (16)	-1.37 (2)	–	–	–
w=1	2.29 (11)	1.29 (10)	–	–	–	–

Notes: Table includes only observations for which $p_{it} = C_{it}$. Issuers with $\hat{p}_{it} < C_{it}$ are assigned $w = 1$, and issuers with $\hat{p}_{it} \geq C_{it}$ are assigned $w = 0$. Numbers in parentheses are number of issuers in category. Units are hundreds of basis points.