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A Fallacy of Division: The Failure of Market Concentration as a Measure of Competition in U.S. Banking

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Abstract
Empirical literature and related legal practice using concentration as a proxy for competition measurement are prone to a fallacy of division, as concentration measures are appropriate for perfect competition and perfect collusion but not intermediate levels of competition. Extending the classic Cournot-type competition model of Cowling and Waterson (1976) and Cowling (1976) used to derive the Hirschman-Herfindahl Index (HHI) of market concentration, we propose an adaptation of this model that allows collusive rents for all, none, or some of the firms in a market. Application of our model to data for U.S. commercial banks in the period 1984-2004 confirms that concentration measures are unreliable competition metrics. While collusion is prevalent in the banking industry at the state level, the critical market shares at which market power is achieved, rents earned from collusion, and collusive concentration levels vary widely across states. These and other results lead us to conclude that a fallacy of division exists in concentration-based competition tests.

Keywords: SCP hypothesis, competition, Cournot, conjectural variation, efficiency hypothesis

JEL classification: G21, L11, L22

Fallacy of division: The error of assuming that what is true about something must also be true of all or some of its parts.

1. Introduction

Empirical literature and related legal practice using concentration as a proxy for competition measurement are prone to a fallacy of division.[1] This problem arises from the fact that market concentration measures of competition are appropriate only in the extreme situations of perfect competition or perfect collusion. But what about markets on the continuum between these two competition endpoints? Are concentration measures reliable competition metrics in intermediate situations that likely exist many times in the real world? Extending the classic Cournot-type competition model of Cowling and Waterson (1976) and Cowling (1976) used to derive the Hirschman-Herfindahl Index (HHI) of market concentration, we propose an adaptation of this model that allows collusive rents for all, none, or some of the firms in a market. A variety of novel competition measures are derived from the proposed model, such as the critical market share at which market power is achieved, percentage of firms in a market with market power, timing of firms reaching market power, and the markup due to market power. Application of our model to data for U.S. commercial banks in the period 1984-2004 confirms that concentration measures are unreliable competition metrics. While collusion is prevalent in the banking industry at the state level, the critical market shares, rents earned from collusion, and collusive concentration levels vary widely across states. These and other results lead us to conclude that a fallacy of division exists in concentration-based competition tests.

Forthcoming sections discuss the use of concentration as a measure of competition, develop our proposed model, and present empirical results for the U.S. banking industry. An important policy implication of our results is that government merger policies based on concentration measures should be broadened in scope to encompass critical market share and related information.

2. Background discussion

Concentration measures, such as the Hirschman-Herfindahl Index (HHI), are commonly used in tests of the structure-conduct-performance (SCP) hypothesis (Hirschman, 1964). In this regard, market concentration measures are frequently employed as a proxy for competition. Increases in market concentration are believed to increase the potential for collusion, as a negative causal relationship between market concentration and competition is typically assumed (e.g., Cournot-type models). However, both the direction and the validity of the relationship between competition and market concentration can be challenged. A prime example in the industrial organization (IO) literature is the two player Bertrand competitive market model which demonstrates that the number of firms in the market as well as market concentration can be poor proxies for competition (Tirole, 1988).[2] Recent work by Boone (2008) further illustrates the advantages of alternative competition models. Also, a related branch of literature (e.g., Goppelsroeder et al., 2008) has documented aggregation problems in constructing market concentration measures and sought to develop improved measures.

Others studies consider various aspects of concentration measures as proxies for competition by taking into account the notion of strategic behavior among a segment of firms in a market (e.g., Salop and Scheffman, 1983; Evans and Kessides, 1993; Riordan, 1998) and the existence of large, dominant firms (e.g., Borenstein, 1990; Buschena and Perloff, 1991; Kahai et al., 1996; Klein, 2001).[3] In describing the relation between industry profit rates and concentration, Bain (1951) noted that "... major reliance will have to be

[1]We thank Claire Economidou, Ryan van Lamoen, Timothy Riddiough and seminar participants at Maastricht University and Utrecht School of Economics for helpful comments. The usual disclaimer applies.

[2]To some extent the assumptions underlying different IO models can be tested. For example, there is extensive literature on the effects of price versus non-price competition on market contestability and entry barriers as well as (tacit) collusion (e.g., see Hausman and Sidak (2007), Salop and Scheffman (1983), Kahai et al. (1996), Borenstein (1990), Riordan (1998), Shepherd (1972), Klein (2001), Evans and Kessides (1993), and Martin (1988)).

[3]An excellent review is provided in Scherer and Ross (1990).

placed upon group averaging and upon comparison of group average profit rates at different levels of concentration ..." (p. 309), and "... it is essential in this setting to know whether intragroup variance is of such magnitude as to obliterate the significance of any difference discovered between group averages." (p. 310) As observed by Martin (1988), Bain (1956) also posited that small firms would not benefit from market concentration and entry barriers sufficient to support effective collusion, which engenders the possibility of a leading-firm group.

Despite controversy surrounding HHI's usage in competition analyses (White, 2008; Elhauge, 2007), it is routinely applied to mergers by the U.S. Department of Justice. If a merger increases HHI by 100 points, it likely will be under scrutiny by the Antitrust Division.[4] According to horizontal merger guidelines by the Department of Justice and Federal Trade Commission effective since 1992, any market with a post-merger HHI between 1,000 and 1,800 is moderately concentrated, and markets with HHI above 1,800 are concentrated. Recognizing the limitations of market concentration measures, HHI figures are interpreted by authorities in the context of a variety of market conditions, including (for example) the introduction of a new technology through a merger, rise of demand substitutes, growth rate of the market, and entry barriers.

A natural laboratory for examining the relationship between concentration and competition is the U.S. banking industry. Over the last thirty years, historic consolidation has dramatically changed the structure of the banking industry. Studies by Kane (2000), Stiroh and Strahan (2003), Berger et al. (1999), and others document this process, its causes, and some of its potential consequences. There were 15,084 U.S. banking and thrift institutions at year-end 1984 (Jones and Critchfield, 2004, p. 3), but by the end of 2003 the number of institutions had shrank by 48% to 7,842. Numerous studies have sought to determine whether higher bank concentration is detrimental to competition with mixed results. For example, Berger and Hannan (1989) found a positive relationship between profitability and market concentration in retail banking markets in the late 1980s. By contrast, Cole et al. (2004) reported no evidence that differences in loan approval procedures of large versus small banks had a negative effect on pricing and volume in the market for small business lending. However, in tests of how competition in local banking markets affects the market structure of nonfinancial sectors, Cetorelli and Strahan (2006) showed that potential entrants faced greater difficulty gaining access to credit in concentrated markets than in more competitive markets. Not surprisingly, in most markets characterized by bank consolidation or high market concentration, fears of anti-competitive behavior persist (i.e., more concentrated markets are expected to increase the likelihood of collusion).[5]

In this paper we seek to contribute to theory and practice on the usefulness of market concentration measures, in particular HHI, for competition measurement and policy making. Our methodology builds upon the seminal work of Stigler (1964) popularized by Cowling and Waterson (1976) and Cowling (1976)(henceforth CW and CL).[6] Extending these studies, we assume that an increase in market share and coincident increase in market concentration *may* result in market power but not necessarily for *all* firms in the market. Hence, although we similarly model and test for Cournot-type competition, we do so allowing for the possibility that collusion is not necessarily either absent or omnipresent. Put differently, collusive rents may be earned by all, none, or some of the firms in the market. Following Stigler (1964), we assume that collusion becomes more attractive as firms increase in size due to rising potential losses from not colluding. Modifying the CW and CL model allows estimation of the critical market share – that is, the market share beyond which firms have market power. Our proposed model incorporates the three classic outcomes of the SCP hypothesis as formulated by CW, CL, and others: (1) for monopoly the critical market share is 100 percent and only one firm has this market share, (2) for perfect competition the critical market share is higher than the highest market share in the market, and (3) for Cournot oligopoly the critical market share is lower than the lowest market share in the market.

[4]Throughout this paper, we measure HHI as the sum of squared market shares, where the latter are between 0 and a 100, which yields a maximum HHI of 10,000.

[5]See Berger et al., 2004 for an excellent survey of empirical research on bank market concentration and competition.

[6]For further discussion of HHI, see Adelman (1969), Acar and Sankaran (1999), Kwoka (1998), and others.

3. Methodology

CW and CL employed a Cournot model to derive the relation between *industry* markup and HHI. The model's intuitive appeal and straightforward applicability have contributed to its widespread usage. Importantly, these authors assume that the relationship between each firm's markup and market share is the same for all firms in a market, which allows aggregation of these variables. In so doing, empirical estimation of the model is effectively reduced to tests of perfect collusion and perfect competition. Only in these two extreme cases is each firm representative of all other firms in the market, and the estimated coefficient for HHI in the test of the resulting SCP relation is either insignificantly different from zero (perfect competition) or unity (perfect collusion). For these two cases there is no fallacy of division. Relevant to the present study, what happens in intermediate cases when the coefficient for HHI lies between zero and unity? Their model implies that there is *some* collusion. But how much? And by whom? Here empirical tests of the model are inconclusive, as the coefficient on HHI (as well as many other concentration measures) can only be compared on an ordinal scale (Kwoka, 1985, 1998; Bikker and Haaf, 2002).

3.1. The Cournot model revisited

Revisiting the original Cournot model by CW and CL, each firm chooses its output X_i based on rivals' output levels and seeks to maximize profits Π_i:[7]

$$\begin{aligned} &\Pi_i = pX_i - c_i(X_i) \quad s.t. \\ &p = f(X) = f(\textstyle\sum_{i=1}^{N} X_i), i = 1, 2, 3, ..., N \end{aligned} \tag{1}$$

where $f(X)$ is the inverse demand function. The first order maximization condition of equation (1) is:

$$\frac{d\Pi_i}{dX_i} = p + X_i f'(X)\frac{dX}{dX_i} - c_i'(X_i) = 0, i = 1, 2, 3, ..., N \tag{2}$$

where

$$\frac{dX}{dX_i} = 1 + \frac{d\sum_{j=1}^{N} X_j}{dX_i} = 1 + \lambda_i, j \neq i \tag{3}$$

and λ_i is the conjectural variation of firm i, which measures the output reaction of its rivals to a change in its own output with $-1 \geq \lambda_{i,t} \geq 1$. Whereas a myopic Cournot oligopoly implies $\lambda_{i,t} = 0$, a collusive oligopolist and perfect competition imply $\lambda_{i,t} > 0$ and $\lambda_{i,t} = -1$, respectively. Equation (2) can therefore be rewritten by multiplying the left-hand-side by $\frac{X_i}{X_i}$ and right-hand-side by $\frac{X}{X}$ and then dividing both sides by p to obtain:

$$\frac{pX_i - c_i'(X_i)X_i}{pX_i} = \frac{X_i}{X}\frac{f'(X)X}{p}(1 + \lambda_i), \tag{4}$$

where $c_i'(X_i)X_i$ is the total cost of firm i's output, and $p - c_i'(X_i)X_i$ is firm i's net profit. Thus, the left-hand-side of equation (4) is firm i's markup, also known as the Lerner index L_i (Lerner, 1934). Firm i's market share θ_i is given by $\frac{X_i}{X}$, and the inverse price elasticity of demand $\frac{1}{\eta}$, which is assumed to be the same for all firms, is given by $\frac{f'(X)X}{p}$. Adding time subscripts t, we can now write:

$$L_{i,t} = (-\frac{1}{\eta_t})\theta_{i,t}(1 + \lambda_{i,t}). \tag{5}$$

Thus, each firm's Lerner index depends on the (market) price elasticity of demand, firm market share, and its conjectural variation.

[7] Without loss of generality, we only consider the total costs of a firm (c_i), rather than its fixed and variable costs.

3.2. *Proposed model and critical market share*

CW and CL make two key assumptions.[8] First, by aggregating equation (5) over all N firms and either assuming that the price elasticity of demand is constant over time or can be captured by control variables, they specify the relationship between industry mark-up and HHI, which has been tested in many empirical studies.[9] Second, and relatedly, they crucially assume (as well as many empirical tests of the SCP hypothesis) that conjectural variation $\lambda_{i,t}$ is an omitted variable. In this respect CW and CL rely upon Stigler (1964), who argued that the (pricing) behavior of firms must be inferred from the way their customers react. From Stigler's rule we know that "[T]here is no competitive price-cutting if there are no shifts of buyers among sellers ..." (Stigler, 1964, p. 48).

Stigler proved that, given the de facto existence of collusive behavior, the extent to which firms will engage in collusive behavior is directly related to their market share. To see why, following Stigler (1964), assume that a firm targets three groups of customers: new customers, its own old customers, and other firms' old customers.[10] The firm wants to garner its share of the growth of each group.[11] For each group the cost of cheating (i.e., not behaving collusively) is given by the variance of the expected number of customers.[12] The higher this variance, the more likely a firm is to exhibit collusive behavior. Since the three groups are disjoint subsets of the whole customer population, we can simply add up their variances.[13] Consequently, if an increase in market share (θ_i) makes cheating more costly, it will lead to an increase in awareness ($\lambda_{i,t}$) and thereby facilitate collusive behavior. This outcome is supported by the fact that the variance of a firm's expected number of customers increases with an increase in its market share.[14]

Stigler's rule is used by CW, CL, and others to treat a firm's conjectural variation $\lambda_{i,t}$ as an implicit function of its market share. Importantly, the resultant empirical specification only leads to conclusive results if all firms have the same conjectural variation. From equation (5) we can see that this condition holds for two extreme scenarios. First, if all firms behave as myopic Cournot oligopolists, then $\lambda_{i,t} = 0$ for all firms, such that, for a given price elasticity of demand, an increase in θ_i leads to an exactly proportional increase in the Lerner index. Second, in the case of perfect competition, an increase in market share has no impact on performance, as $\lambda_{i,t} = -1$ for all firms. However, returning to our earlier questions, what happens for other intermediate values of $\lambda_{i,t}$? And how much does the impact of an increase in market share on the Lerner index depend on the value of $\lambda_{i,t}$?

To answer these questions, we propose the following Lerner index specification:

$$L_{i,t} = \beta_i + \beta_\theta \cdot \theta_{i,t} + \beta_\lambda \cdot \lambda_{i,t} + \beta_{\theta\cdot\lambda} \cdot (\theta_{i,t} \cdot \lambda_{i,t}) + Controls + \varepsilon_{i,t}. \tag{6}$$

Equation (6) differs from previous specifications in two ways. First, we do not aggregate but instead relate the firm's markup to its market share. Second, rather than treating $\lambda_{i,t}$ as an omitted variable, we include it in our empirical specification. In Section 3.3 we elaborate on the measurement and importance of $\lambda_{i,t}$. For

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[8]See Bikker and Bos (2008).

[9]To be precise CW include two specifications of their model. In the second specification they allow for the existence of unequal size firms as determined by their different marginal cost functions. This specification leads to an equality between a slightly modified conjectural variation and HHI. An intuitive way of deriving this result from equation (4) is by multiplying the right-hand-side by $\frac{X_i}{X_i}$, where the denominator of the latter term finds its way into the remainder of the equation, and the numerator (after aggregating the entire equation over N firms) yields HHI instead of firms' market share.

[10]Let: Q_n = number of new customers; Q_o = the total number of old buyers in the market; and q_o^i = the number of old customers for firm i.

[11]First, it wants a share of the new customers (D_n). Second, it wants to retain as many old customers as possible (D_r). And, third, it wants to win over other firms' old customers (D_o).

[12]With the probability of repeat purchases denoted p, the expected number of firm j's customers for each group is given by: $E(D_n^i) = \theta_i * Q_n$; $E(D_r^i) = p * \theta_i * Q_o$; and $E(D_o^i) = (1-p) * \theta_i * (Q_o - q_o^i)$.

[13]A firm expects a consumer to either become a customer (with expectations dependent on its current market share) or not. Thus, for the binomial mean $\mu = n * p$, variance is $n * p(1-p)$. Hence, variances for each group are given by: $var(D_n^i) = [Q_n * \theta_i * (1-\theta_i)]$; $var(D_r^i) = [Q_o * p * \theta_i * ((1-p)\theta_i)]$; and $var(D_n^i) = [(Q_o - q_0^i) * (((1-p)\theta_i) * (1-(1-p)\theta_i))]$.

[14]In fact: $\frac{\partial var(C_n^i)}{\partial \theta_i} = Q_n - (2 * Q_n * \theta_i) > 0$; $\frac{\partial var(D_r^i)}{\partial \theta_i} = pQ_o - (2 * Q_o * p^2 * \theta_i) > 0$; and $\frac{\partial var(D_o^i)}{\partial \theta_i} = ((1-p)(Q_o - q_0^i)) - (2(1-p) * (Q_o - q_0^i) * \theta_i) > 0$. The first and last equations hold iff $\theta_i < 0.5$. The remaining equation holds iff$p > 2p^2 * M\theta_i$. If $\theta_i < 0.5$, this condition is satisfied also.

now, to see how equation (6) can be used to reduce the fallacy of division, in line with Stigler's rule, we utilize it to determine how large a firm needs to be to act as a collusive oligopolist. From equation (6) note that:

$$\frac{\delta L_{i,t}}{\delta \lambda_{i,t}} = \beta_{\lambda} + \beta_{\theta \cdot \lambda} \cdot (\theta_{i,t}). \tag{7}$$

Recall that for the oligopolist $\lambda_{i,t} \geq 0$. Therefore, we are interested in knowing at what point $\frac{\delta L_{i,t}}{\delta \lambda_{i,t}} = 0$. Setting the derivative in equation (7) equal to zero and rewriting yields:

$$\theta^* = -\frac{\beta_{\lambda}}{\beta_{\theta \cdot \lambda}}, \tag{8}$$

where θ^* is the critical market share defined as the market share at or beyond which firms collude. Denoting the lowest (highest) market share in a market by θ^{min} (θ^{max}), we can relate this result to CW and CL by observing that in the case of perfect competition $\theta^* > \theta^{max}$, whereas in the case of an oligopoly $\theta^* < \theta^{min}$. Moreover, the notion that the likelihood of collusion increases with market share is consistent with Stigler (1964), and $\theta_{i,t} \geq \theta^*$ nicely identifies dominant firms (Scherer and Ross, 1990).

As discussed in Section 2, for policy purposes a more in-depth knowledge of the competitive conditions in a market is often required. When deciding whether or not to approve a merger, the current level of competition is only one piece of required information. Our model provides potentially valuable information in this connection. For example, we can estimate the number of firms with market power in the relevant market, the effect of an increase in market share on market power, and the actual rents earned by firms with market power. Additionally, such information may be useful to researchers (for example) seeking to explain an inverted-U relationship between competition and innovation (Aghion et al., 2005; Aghion and Griffith, 2005; Aghion et al., 2001). In this case the optimal level of innovation may require a (slightly) positive markup (e.g., see recent U.S. commercial bank evidence by Bos, Kolari, and van Lamoen (2009)). Moreover, for industries that are considered to play a key role in the economy, such as the banking sector, there may also be a trade-off between competition and (financial) stability that warrants a (slightly) positive markup (Allen and Gale, 2003).

Table 1: Competition measures

Abbreviation	Description	Calculation
$L_{i,t}$	*Lerner index*	$\frac{\text{profits} + \text{fixed costs}}{\text{total revenue}(pX_{i,t})}$
θ^*	*critical market share*	$-\frac{\beta_{\lambda}}{\beta_{\theta \cdot \lambda}}$
$\%^*$	*percentage of firms with market power*	$\left(\frac{\sum n \vert \theta_{i,t} \geq \theta^*}{\sum n}\right) * 100$
n^*	*annual average number of firms with market power*	$\frac{\sum n \vert \theta_{i,t} \geq \theta^*}{\text{number of years that year} \geq year^*}$
$year^*$	*first year in which at least one firm had market power*	$\text{year} \vert \theta_{i,t} \geq \theta^*, \text{year}_{-t} \neq year^*, \text{for } t = 1, ..., T$
mfx^*	*marginal effect of an increase in market share*	$\beta_{\theta} + \beta_{\theta \cdot \lambda} \cdot \lambda_{i,t}$
rents	*percentage of the markup attributable to market power*	$\frac{\left(\frac{\sum L_{i,t} \vert \theta_{i,t} \geq \theta^*}{\sum n \vert \theta_{i,t} \geq \theta^*}\right) - \left(\frac{\sum L_{i,t} \vert \theta_{i,t} < \theta^*}{\sum n \vert \theta_{i,t} < \theta^*}\right)}{\left(\frac{\sum L_{i,t} \vert \theta_{i,t} < \theta^*}{\sum n \vert \theta_{i,t} < \theta^*}\right)} * 100$
dollar bonus	*dollar value of the profits (in thousands) attributable to rents*	$\frac{\text{rents}}{100} * pX_{i,t} - \text{fixed costs}$

For each market the number of firm-year observations included in an estimation is $n = 1, ..., N$, and the number of years is $t = 1, ..., T$.

As shown in Table 1, equation (6) enables new insights into the level of competition in a market. Not only can the critical market share, θ^*, be identified, but the percentage of firms which currently operate at or beyond that critical market share, $\%^*$, can be estimated. Also, we can determine the first year in which there was market power, *year**. From estimations of equation (6), we can further calculate the number of firms with market power, n^*.[15] Moreover, although the Lucas critique (Lucas, 1976) applies, we may be interested in evaluating the marginal effect of an increase in market share on the Lerner index, mfx^*, to assess the competitive effects of a merger or takeover, or more generally, an increase in relative size. Finally, when there is market power, we can calculate both the rents of colluding firms and the dollar amount of additional profits they derive from their collusive behavior. We do so by assuming that the difference between the average markup of the firms with and without market power is the direct result of collusion.[16]

3.3. *Collusion and critical market share*

In the model of CW and CL, competition is measured at the firm level by each firm's conjectural variation $\lambda_{i,t}$, or the way it expects other firms to react to a change in the size of its operations. Our derivation so far has relied on the interaction between the firm's current market share $\theta_{i,t}$ and $\lambda_{i,t}$. Hence, given the theoretical setting of this model, we have shown that the assessment of competition brings together both a static market view (as reflected in market shares) and a dynamic market view (as reflected in each firm's conjectural variation). Since these two views are intertwined, as larger firm's are expected to perceive different reactions to a change in their output than smaller firms, equation (6) must include the interaction between market share and conjectural variation.

In our interpretation of the marginal effect of an increase in conjectural variation (conditional on market share) on the Lerner index, we relied upon Stigler's (1964) analysis and argued that the likelihood of collusion increases with market share. The latter argument allows us to define critical market share, which represents a dividing line between those that collude and those that do not. In an empirical specification it is desirable to test whether firms with (without) critical market share indeed (do not) collude. As such, we next propose: (1) an estimation strategy, and (2) a way of deriving each firm's conjectural variation $\lambda_{i,t}$ that allows us to distinguish between firms with and without market power.

3.3.1. *Estimation strategy*

In equation (3) we defined each firm's conjectural variation $\lambda_{i,t}$ as $\frac{d\sum_{j=1}^{N} X_j}{dX_i}, j \neq i$. When estimating equation (6), a correction may be necessary to control for endogeneity, as this equation includes market share measured as $\frac{X_i}{X}$. Without this correction, the estimated coefficients may be biased. In turn, the conditional marginal effect of $\lambda_{i,t}$ on the Lerner index may be inconsistent and therefore unreliable for the purpose of policy making. Also, including the interaction between market share and conjectural variation increases multicollinearity, thereby increasing the standard errors and reducing the likelihood that the estimated coefficient on the interaction term will be significant.

It is obvious that equation (6) may suffer from potential endogeneity due to including both the level of each firm's output (i.e., its market share) and change in output (i.e., its conjectural variation) plus the output levels and changes of all other firms combined. Hence, the treatment of conjectural variation as an exogenous regressor is an empirical issue. To address this issue we estimate equation (6) in three alternative ways:

$$L_{i,t} = \beta_i + \beta_1\theta_{i,t} + \beta_2\lambda_{i,t} + \beta_3(\theta_{i,t}\lambda_{i,t}) + \beta_x(Controls_{i,t}) + \epsilon_{i,t} \tag{9a}$$

$$\Delta L_{i,t} = \beta_1\Delta\theta_{i,t} + \beta_2\Delta\lambda_{i,t} + \beta_3\Delta(\theta_{i,t}\lambda_{i,t}) + \beta_x\Delta(Controls_{i,t}) + \Delta\epsilon_{i,t} \tag{9b}$$

$$\Delta L_{i,t} = \beta_1\Delta\theta_{i,t-1} + \beta_2\Delta\lambda_{i,t-1} + \beta_3\Delta(\theta_{i,t-1}\lambda_{i,t-1}) + \beta_x\Delta(Controls_{i,t}) + \Delta\epsilon_{i,t}. \tag{9c}$$

[15] This measure is logically similar to the simple concentration ratio obtained by summing the market shares of a subset of the largest firms, C_n, where n is the number of the largest firms.

[16] Of course, this approach to calculating both the rents and the dollar bonus is subject to criticism. The most obvious problem is that this comparison does not take into account other ways in which the two groups of firms differ, most notably related to size such as scale economies. However, note that, even in the extreme case in which there are constantly increasing economies of scale, large firms are not guaranteed a higher markup in the case of perfect competition. Rather, large firms would be expected to drive small firms out of the market, in part by undercutting them, thus effectively operating with a *lower* markup.

The basic specification (9a) is a fixed effect panel estimation that ignores possible endogeneity issues. Specifications (9b) and (9c) instrument for the three key variables in our model: market share, conjectural variation, and the interaction between these two variables. Equation (9b) includes an instrument based on the third and fourth lags of $\theta_{i,t}$, $\lambda_{i,t}$ and $\theta_{i,t}\lambda_{i,t}$. And, equation (9c) instruments the lags of $\theta_{i,t}$, $\lambda_{i,t}$ and $\theta_{i,t}\lambda_{i,t}$ with the fourth and fifth lags of each variable.

Endogeneity tests for one or more endogenous regressors involve testing the difference between the Sargan-Hansen statistic for the equation with the smaller set of instruments (i.e., equation (9a) in our case) and the equation with the larger set of instruments (i.e., equations (9b) and (9c)). Under the null hypothesis that the specified endogenous regressors are exogenous, this test statistic has a chi-squared distribution.[17] For our purposes, because homoskedasticity is not required, we use the Durbin-Wu-Hausman test.[18]

We begin by estimating both equations (9a) and (9b). If our tests confirm that we cannot reject at least one of our regressors as endogenous, we proceed to test equations (9b) and (9c). Unless otherwise noted, reported results are always based on the first specification that produces unbiased results. Of course, we are not only concerned about the unbiasedness of the estimated coefficients, but potential multicollinearity associated with the inclusion of the interaction between market share and conjectural variation.[19] According to Brambor et al. (2006, p. 70), multicollinearity raises suspicion when the estimated coefficients in a linear-additive model change due to including an interaction term.[20] However, in our analysis, rather than being interested in the average effect of a variable, we focus on the sign and significance of the conditional marginal effect of conjectural variation on the Lerner index. That is, the significance of the expression in equation (7) is our focal point, not the significance of β_λ and $\beta_{\theta\cdot\lambda}$. From Brambor et al. (2006) we know that the variance of the conditional marginal effect in equation (7) is:

$$\hat{\sigma}^2_{\frac{\delta L_{i,t}}{\delta\lambda_{i,t}}} = var(\hat{\beta}_\lambda) + \theta^2_{i,t} var(\hat{\beta}_{\theta\cdot\lambda}) + 2\theta_{i,t} cov(\hat{\beta}_\lambda \hat{\beta}_{\theta\cdot\lambda}). \tag{10}$$

Using equations (7) and (10), marginal effects and their significance can be evaluated at different levels of market share $\theta_{i,t}$. In our empirical analysis, unless otherwise noted, we evaluate the marginal effect at the critical market share $\theta^*_{i,t}$.

3.3.2. *Collusion among those that have market power*

Do firms with a market share at or above the critical market share behave more collusively than other firms? Following Stigler (1964), we know that collusion is (more) feasible if each colluding firm expects other firms *not* to react to changes in its own output. In other words, among colluding firms, changes in profits (or the Lerner index) result from either changes in its own output or changes in its marginal cost but not from stealing away (losing) earnings from (to) its colluding competitors. Hence, if industry profits change, it is not because profits are reallocated among oligopolists.

We next utilize this view of collusion to derive and interpret $\lambda_{i,t}$. We start by considering changes in the industry markup. Letting $\Pi_{i,t}$ ($R_{i,t}$) be profits (total revenues) of firm i at time t, we can write the industry markup at time t as:

$$L_t = \frac{\Pi_t}{R_t} = \frac{\sum_i \Pi_{i,t}}{\sum_i R_{i,t}} = \sum\nolimits_i L_{i,t}\theta_{i,t} = \frac{\sum_i \Pi_{i,t}}{R_{i,t}} \frac{R_{i,t}}{\sum_i R_{i,t}}, \tag{11}$$

where $\Pi_{i,t}$ again denotes firm profits ($pX_i - c_i(X_i)$), and $R_{i,t}$ denotes firm revenues ($pX_{i,t}$). From equation (5) we can write:

$$L_t = \sum\nolimits_i \left[\left(-\frac{1}{\eta_t} \right) \theta_{i,t}(1 + \lambda_{i,t}) \right]. \tag{12}$$

[17] The degrees of freedom are equal to the number of regressors tested.

[18] As shown by Hayashi (2000, pp. 233-234), the standard test statistic is numerically equal to a Hausman test statistic only under conditional homoskedasticity, whereas the Durbin-Wu-Hausman test is not.

[19] Centering has been suggested as a way of mitigating multicollinearity issues. As pointed out by Brambor et al. (2006) and Kam and Franzese Jr. (2007), centering does not provide us with more accurate data, and "... although the algebraic transformation that results from centering the variables will result in different coefficients and standard errors in the centered model compared to those in the uncentered model, ... this is because they measure different substantive quantities in each model and not because one model produces better estimates than the other." (Brambor et al., 2006, p. 71)

[20] See also Friedrich (1982).

Also, the change in industry L is:

$$\Delta L_t = \sum_i \left[\left(-\frac{1}{\eta_t} \right) \theta_{i,t}(1+\lambda_{i,t}) \right] - \sum_i \left[\left(-\frac{1}{\eta_{t-1}} \right) \theta_{i,t-1}(1+\lambda_{i,t-1}) \right]. \tag{13}$$

At this point we make two additional assumptions. First, we assume that the market price elasticity of demand η is constant. Constant price elasticity is commonly used in empirical demand analyses due to the success of log-linear demand functions (Iwata, 1974, p. 949). Also, empirical studies have found that the market price elasticity of demand is relatively constant (Teles and Zhou, 2005, p. 57).[21] Second, we assume that the conjectural variation of an individual firm is constant in the short run. In the short run firms use historical data to predict their rivals' production and decide on their own output levels. Now we can combine equation (12) with equation (13) to write:[22]

$$\sum_i -\frac{1}{\eta}(1+\lambda_i)\Delta\theta_{i,t} = \underbrace{\underbrace{\sum_i [\Delta L_{i,t} \cdot \theta_{i,t-1}]}_{\text{operate in t and t-1}}}_{\text{within effect}} + \underbrace{\underbrace{\sum_i [\Delta\theta_{i,t} \cdot (L_{i,t-1} - L_{t-1})]}_{\text{operate in t and t-1}} + \underbrace{\sum_i [\Delta L_{i,t} \cdot \Delta\theta_{i,t}]}_{\text{operate in t and t-1}} + \underbrace{\sum_i [\Delta\theta_{i,t} \cdot L_{t-1}]}_{\text{operate only in t}}}_{\text{reallocation effect}}. \tag{14}$$

Equation (14) clearly captures the effect of collusion on the industry markup. For the myopic Cournot oligolopolist, changes in the industry markup are purely from within each firm. Since firms do not capture market share at the expense of others, changes in the industry markup arise from the within effect (Stiroh, 2000; Stiroh and Strahan, 2003). However, increasing competition leads to increasing reallocation effects, as firms expropriate each others' market shares.[23]

Therefore, in order to derive an expression for $\lambda_{i,t}$, we write equation (14) at the firm level. Removing summations, dividing both sides by $\Delta\theta_{i,t}$, and simplifying further, we can write:[24]

$$-\frac{1}{\eta}(1+\lambda_i) = \Delta L_{i,t} \cdot \frac{\theta_{i,t-1}}{\Delta\theta_{i,t}} + L_{i,t}. \tag{15}$$

where $\frac{\theta_{i,t-1}}{\Delta\theta_{i,t}}$ is the inverse of the percentage change in firm i's market share. Using $\alpha_{i,t}$ to represent the percentage change in firm i's market share at time t, and dividing both sides of equation (15) by $\frac{1}{\eta}$, we can rearrange to write:[25]

$$\lambda_{i,t} = -\left(\frac{\Delta L_{i,t}}{\alpha_{i,t}} \cdot \eta \right) - (L_{i,t} \cdot \eta) - 1. \tag{16}$$

This expression for $\lambda_{i,t}$ and its lags can be used in estimating equations (9a)-(9c). Also, it can be used to test whether there is more collusion among firms with a market share at or above the critical market share. To do so, we perform three types of tests. First, we test whether reallocation is zero for banks with market power compared to banks without market power. Second, we test whether reallocation equals zero in states with banks that have market power versus states that do not have banks with market power. Third, we test whether reallocation is zero for banks with market power before and after they gained market power.[26]

In sum, we derived a Cournot model that enables identification of firms with market power and construction of related market power measures.

[21] In fact, our analysis rests on the assumption that the price elasticity of demand is relatively constant over time. Alternatively, the demand function may also be nonlinear. In that case, the marginal benefits from behaving competitively, undercutting competitors and thereby causing a reallocation of profits, vary depending on the shift along the demand curve that results. However, although this would alter the amount of reallocation, it will not affect our analysis otherwise.

[22] A similar type of decomposition is given in Stiroh (2000) and Stiroh and Strahan (2003).

[23] This line of thinking is similar to Caballero and Engel (1993).

[24] The complete derivation is available upon request from the authors.

[25] As should be expected, rewriting and rearranging equation (5) gives the same result.

[26] Test results are reported in Section 4.3.

4. Empirical results

This section applies our competition model to the U.S. banking industry. We describe the data, estimate our competition indicators, and investigate a number of questions concerning relationships between critical market share and collusion, concentration, rents, and bank deregulation. In general, our results agree with studies that find market concentration constitutes a poor measure of competition (Gilbert and Zaretsky, 2003; Claessens and Laeven, 2004) and, therefore, suffers from a fallacy of division.

4.1. Data

Our data include all insured U.S. commercial banks in the period 1984-2004. We collect year-end Call Report balance sheet and income statement data for individual banks in each state for the period 1984-2004. Empirical tests are conducted on the state level, as most policy decisions involving market concentration measures occur on the state level, evidence supports state-wide pricing (Radecki, 1998; Heitfield, 1999), and state-level competition matters more than local competition (Hannan and Prager, 2004; Heitfield and Prager, 2004).[27] As shown by the solid line in Figure 1, state level market concentration measured by HHI rose considerably during the period under consideration. At the same time the dashed line shows that the number of banks was almost halved.

Figure 1: Consolidation in U.S. banking

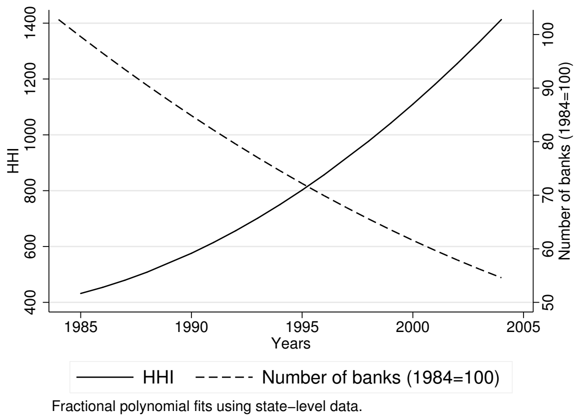


Table 2 contains the descriptive statistics for our variables. Markup is calculated as the sum of profit before tax and fixed asset expenditures over total revenues. Market share is based on total assets, and conjectural variation is calculated via equation (16) using the markup, market share, and market price elasticity of demand. Following empirical methods in Teles and Zhou (2005), we estimated the latter elasticity to be -0.15.[28] In the sample period 1984-2004 the average markup $L_{i,t}$ was close to 16%, the average market share was 0.5%, and average conjectural variation was -0.943.

When estimating equations (9a)-(9c), consistent with previous literature (Bikker and Bos, 2008), we utilize control variables. To take into account differences in risk-taking, we include the ratio of total loans and

[27] Of course, banks can compete on local, state, and national levels to various degrees (e.g., see Gilbert and Zaretsky, 2003).

[28] Teles and Zhou (2005) proxy the market price elasticity of demand with the interest elasticity of money demand. The real money demand function is estimated for our sample period. The dependent variable is the deflated MZM money aggregate (i.e., M2 less small time deposits plus institutional money market funds), and the independent variables are GDP and interest rate (i.e., the federal funds rate minus the MZM own rate). The first derivative of this specification and one-period lagged dependent variable is used to adjust for serial correlation. Based on this model, we estimate the interest elasticity of money to be -0.15, which is close to the -0.20 estimate obtained by Teles and Zhou (2005, p. 57) in the similar sample period 1980-2003.

Table 2: Descriptive statistics for variables

Variable		Mean	Std. Dev.
markup	($L_{i,t}$)	0.159	0.225
market share	($\theta_{i,t}$)	0.005	0.026
conjectural variation	($\lambda_{i,t}$)	-0.943	27.187
risk	(loans and leases/total assets)	0.557	0.152
earning assets	(total assets - fixed assets)/total assets)	0.983	0.013
cost	(noninterest expenses/noninterest income)	5.778	9.795
cost efficiency	(translog cost frontier estimates)	0.791	0.096

The total number of observations based on the specification in equation (9a) is 200,488. The p-value for the one-sided t-test that $\lambda_{i,t} > 1$ ($<$) equals 0.828 (1.000). Cost efficiency is estimated on an annual basis from the translog cost frontier in Bos and Kolari (2005) available upon request from the authors.

leases over total assets as a proxy for credit risk exposure. We also include earning assets measured as the ratio of total assets minus fixed assets over total assets.[29] The ratio of noninterest expenses over noninterest income is used to control for costs, rather than the ratio of total expenses over total income, as year-to-year changes in interest earnings and expenses may reflect yield curve changes instead of banks' cost management.[30] Finally, to control for the fact that market shares as well as markups may reflect differences in efficiency (i.e., the so-called efficiency hypothesis (Goldberg and Rai, 1996)), we include cost efficiency estimates based on standard translog cost frontier methods (see Bos and Kolari (2005) and citations therein).

With the exception of the cost ratio, the standard deviations of the control variables are relatively low. This low variability may (in part) explain why later empirical results suggest that control variables play a minor role. Another reason for this result is that our model is grounded in an identity and controls for firm-level heterogeneity either through fixed effects using equation (9a) or dynamic panel estimators using equations (9b) and (9c).

4.2. Main empirical results

Table 3 summarizes our competition measures for states in which there is evidence of collusion. Endogeneity appears to be less of an issue than was originally believed. Although we find evidence of collusion in 30 states, tests indicate that instrumenting is required in only 12 states, and estimating specification (9c) was never warranted. For illustrative purposes, Figure 2 provides a graphical representation of the estimation results for California. The graph shows the marginal effect of a change in conjectural variation on the markup conditional on market share. Since our model states that collusion results in a marginal effect equal or greater than zero, we can infer from the results in Figure 2 that collusion exists in California. As shown in Table 3, we estimate California's critical market share $\theta^*_{i,t}$ to gain market power at slightly more than 6 percent. Other results in the table indicate that on average less than 1 percent of California banks has market power. Note that collusion first occurs in 1989 when state-level HHI was a modest 1,074. Also, approximately three banks had market power and earned rents averaging almost 58 percent of the total markup.

Our results for California aptly demonstrate the fallacy of division. Although the market is moderately concentrated, there is collusion among a very small number of banks. In effect, according to our results, this market's relevant concentration measure is a C_3 ratio. Even though rents for these top 3 banks are sizeable, our results suggest that the label 'collusive' does not fit most banks operating in the California market.

The estimated competition metrics in Table 3 reveal that, among the states in which there is evidence of collusion, the HHI at which collusion begins differs widely. In Arkansas, Iowa, Montana, Oklahoma, and

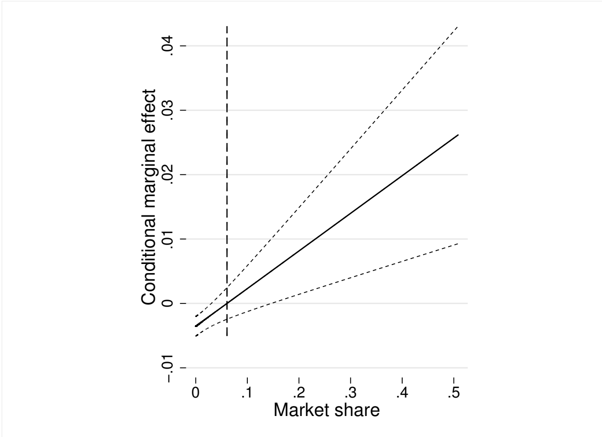
[29] Additionally, we tested the leverage ratio equal to total equity over total assets with little change in results.
[30] In unreported results the inclusion of total expenses over total revenues did not alter our conclusions.

Table 3: Empirical results for states with market power

State	*Critical market share* θ^*	*Banks with market power (%)* $\%^*$	*First year with market power* $year^*$	*HHI in first year with market power* HHI^*	*Marginal effect of change in market share* mfx^*	*Markup due to market power (%)* rents	*Profits due to market power* bonus	*Number of banks with* θ^* n^*	*Instrument test (p-value)* endog.	*Specification* spec.
Alabama	1.73***	2.60	1984	571.79	0.14	24.97	227141.30	5.13		eq. (9a)
Alaska	44.21***	11.01	1989	3325.73	0.09	13.74	14936.35	0.44		eq. (9a)
Arkansas	1.86***	2.51	1985	131.06	13.42	100.38	71028.99	5.52	0.000***	eq. (9b)
California	6.06***	0.72	1989	1074.26	0.97	57.80	3088269.00	3.06		eq. (9a)
Colorado	0.47***	15.15	1984	234.77	1.26	9.34	1225.39	43.98		eq. (9a)
Dist. of Columbia	0.00***	100.00	1984	2403.24	0.30			17.76		eq. (9a)
Florida	0.25***	19.35	1984	364.69	8.45	86.62	85973.55	62.90	0.049**	eq. (9b)
Georgia	12.89***	0.41	1984	723.39	0.79	81.33	1654141.00	1.50	0.000***	eq. (9b)
Hawaii	0.00***	100.00	1984	2767.40	0.31			17.30		eq. (9a)
Illinois	1.99***	0.83	1998	888.39	0.31	20.94	304911.40	5.41		eq. (9a)
Indiana	1.19***	5.49	1984	209.35	0.35	21.04	42393.09	13.36		eq. (9a)
Iowa	0.18***	28.64	1984	51.57	1.93	9.33	683.41	144.67		eq. (9a)
Kansas	7.70***	0.17	1997	227.03	2.69	102.81	358141.60	0.43	0.004***	eq. (9b)
Kentucky	0.04***	97.76	2000	538.68	11.39	11.82	962.62	269.50	0.001***	eq. (9b)
Mississippi	0.29***	51.99	1984	520.06	0.53	7.49	691.96	60.50		eq. (9a)
Missouri	0.46***	8.98	2000	395.28	0.69	16.18	12531.79	27.48		eq. (9a)
Montana	1.82***	8.40	1984	149.95	1.21	32.13	9531.43	9.93		eq. (9a)
New Mexico	7.63***	8.16	2004	801.04	0.33	-252.03	-432071.60	2.11	0.007***	eq. (9b)
New York	0.02***	59.41	1990	1021.53	0.48	7.83	18723.18	96.02		eq. (9a)
Oklahoma	0.97***	3.91	1991	149.36	5.55	98.04	95339.06	10.71	0.000***	eq. (9b)
Oregon	0.00***	100.00	1984	2751.76	1.12			47.67	0.006***	eq. (9b)
Pennsylvania	0.00***	100.00	1984	620.41	3.66			254.98	0.001***	eq. (9b)
South Carolina	5.96***	5.22	1995	1372.66	0.25	30.63	82510.62	3.48		eq. (9a)
South Dakota	29.60***	1.01	1989	1045.58	0.14	73.36	1279430.00	0.71	0.001***	eq. (9b)
Tennessee	1.07***	3.07	1994	627.30	0.37	19.58	62170.32	9.63		eq. (9a)
Utah	21.05***	3.31	1984	1589.84	0.06	23.06	158612.20	1.62		eq. (9a)
Virginia	0.06***	88.42	1984	1162.51	0.17	28.03	7560.32	138.11		eq. (9a)
Washington	0.58***	27.27	1984	1710.37	0.52	19.19	10362.43	22.58		eq. (9a)
West Virginia	0.00***	100.00	1984	101.41	2.09			160.01	0.011**	eq. (9b)
Wisconsin	5.62***	0.82	1996	438.84	4.80	100.73	631544.60	1.77	0.000***	eq. (9b)

Results are based on the preferred specification denoted in the last column. Significance is indicated at the following levels: 1/5/10% (***/**/*, respectively).

Figure 2: Conditional marginal effect of conjectural variation on the markup in California



West Virginia, this minimum HHI is always less than 150. By contrast, in Alaska, the District of Columbia, Hawaii, and Oregon, the minimum HHI is always above 2,500. Consequently, HHI as a measure of competition appears to be seriously flawed. This problem is further illustrated by the critical market share required to have market power, which ranges from 44.21 percent in Alaska to 0 percent in Pennsylvania. The use of other market concentration measures, such as a C_5 or C_{10} ratio, appear to be subject to question also, as the average number of banks with market power ranges (for example) from less than 1 in South Dakota to more than 269 in Kentucky. Likewise, rents widely range from a modest 7.49 percent in Mississippi to slightly more than 100 percent in Arkansas, Kansas, and Wisconsin.[31] The first year in which collusion started has a fair range too. In 20 out of 30 states, collusion already existed in the 1980s, often from the beginning of our sample period. The last year in which collusion started is 2004, when banks in New Mexico reached their critical market share.

In an effort to verify these results, we estimated specifications (9a)-(9c) for four separate periods: 1984-1988, 1989-1993, 1994-1998 and 1999-2004. Table A.1 in the Appendix reports the results for states and periods in which there is evidence of collusion. For the states included in Table 3, the results are generally robust when compared to Table A.1. However, the latter table also includes a number of other states in which there is evidence of collusion in one or more of the four periods. In the remainder of this section, we focus on the main results in Table 3, except when the distinction between periods is crucial, such as in the case of the Interstate Banking and Branching Efficiency Act of 1994.

In sum, although there is substantial evidence of collusion at the state level in U.S. banking, the extent to which banks colluded, the rents they earned from colluding and, importantly, the level of concentration at which collusion started for (a subset of) banks vary widely. These results confirm the dangers of using market concentration as a singular measure of competition.

4.3. Does collusion increase above the critical market share?

In section 3.3.2 we distinguished between banks that operate with and without market power and proposed three related hypotheses. Table 4 contains the test results for these hypotheses.[32]

The first and most direct market power test compares banks with market power to banks without market power. Our results confirm that the reallocation effect in equation (14) equals zero for banks with

[31] Results for New Mexico are suspect due to estimated rents of -252.03 percent, or a negative dollar bonus.

[32] In addition to the tests reported in Table 4, we performed unreported tests. First, non-parametric rank tests yielded qualitatively identical results. Second, we scaled the reallocation effects by the size of the banks, which also did not change the results. In our opinion the reported results constitute a strong test of our hypothesis, as scaling especially reduces the reallocation effects of large banks.

Table 4: More collusion results in less reallocation

Tests	Null hypothesis	p-value	Result
Banks with market power versus banks without market power	$reallocation_{(\theta_{i,t} \geq \theta^*_{i,t})} = 0$	0.506	accept
	$reallocation_{(\theta_{i,t} \geq \theta^*_{i,t}} = 0$	0.002	reject
States with market power versus states without market power	$reallocation_{(year \geq year^*)} = 0$	0.436	accept
	$reallocation_{(year < year^*)} = 0$	0.000	reject
Banks with market power before year and after year**	$reallocation_{(year \geq year^*)\|year^* \neq .} = 0$	0.334	accept
	$reallocation_{(year < year^*)\|year^* \neq} = 0$	0.017	reject

All p-values are based on two-sided t-tests with a critical value of 5%.

market power but not for banks without market power. The second test of whether the reallocation effect equals zero in states where banks have market power versus states where no banks have market power yields similar results. The third and last test of whether the reallocation effect equals zero in states where banks have market power before and after the first year in which at least one bank gained market power again yields similar results. Together, these test results suggest that collusion does increase above the critical market share. In this regard, collusion can take place among a select number of firms in a market. Also, banks that are too small and do not collude experience a markup change due to reallocation effects (as banks gain market share at the expense of other firms with higher markups).[33]

4.4. Additional empirical results

In this section we further investigate relationships between concentration measures and market power (competition) metrics by means of graphical and empirical analyses.

4.4.1. Is there more collusion when the market is more concentrated?

Implicit in HHI as a measure of competition is the idea that there is a higher probability of collusion when a market is more concentrated. Our model makes clear that this assertion may be flawed. Although the probability of collusion increases in our model with higher HHI, it does not necessarily imply that the number (or percentage) of banks colluding increases. Additionally, because a higher HHI often implies fewer banks, endogeneity may be an issue. As the results in Table 4 show, this does not necessarily mean that all remaining banks in a market collude. By way of illustration, Figure 3a compares the percentage of banks with market power ($\%^*$) with the HHI in the first year in which there was market power (HHI^*). To simplify the graphical analysis the sample is restricted to states in which $\%^*$ was at least 5 percent.

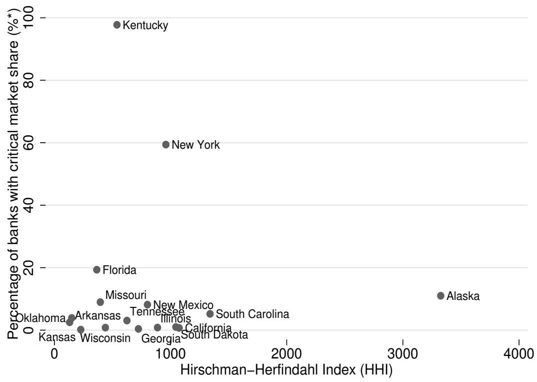
If the intuition for HHI as a measure of competition is correct, we expect to see a higher percentage of banks with market power in states with higher HHIs. However, the results shown in Figure 3a do not support this relation, as casual inspection suggests a slightly negative association between market power and HHI.

Certainly the number of observations in Figure 3a is low, and the results may be affected by the fact that we only consider the first year in which there is collusion. For these reasons a more formal analysis is provided in the first part of Table 5. Using the results in Table A.1, we regress the percentage of banks with market power on HHI. Again, we find no positive relationship between these variables, which contradicts the intuition underlying HHI as a competition measure. Consequently, an important potential pitfall is Type I error in terms of ignoring collusion in markets with low concentration.

[33] The implication here is a negative reallocation effect.

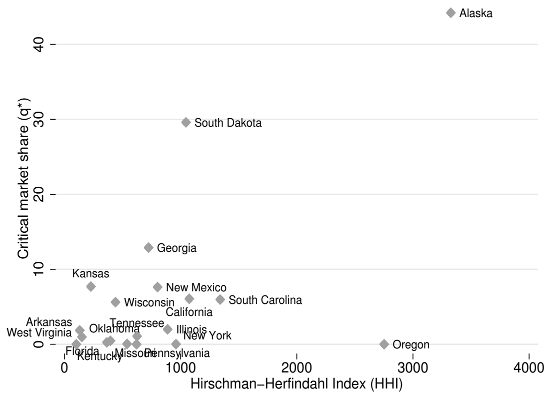
Figure 3: The fallacy of division

(a) Is there more collusion in more concentrated markets?

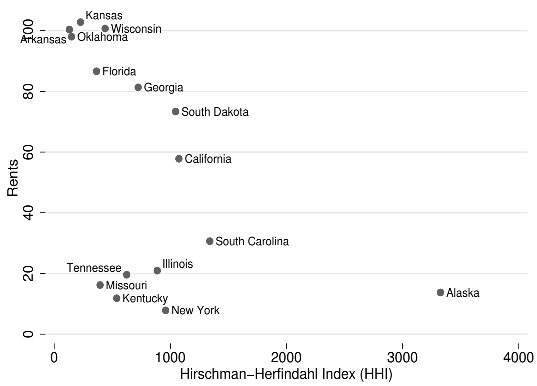


HHI in the first year in which there was market power, %* <100.

(b) Is the critical market share lower in more concentrated markets?

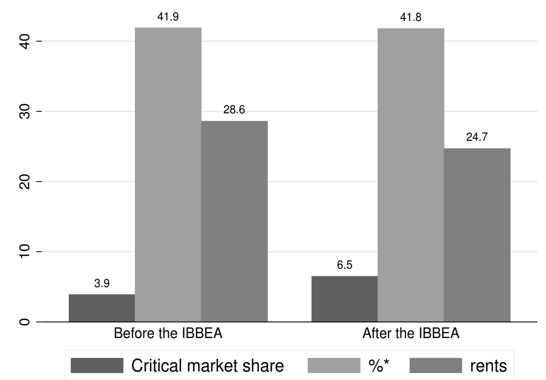


(c) Are there (more) rents for banks in more concentrated markets?



Average rents; New Mexico excluded.

(d) Has competition increased after the IBBEA of 1994?



Based on estimates per period.

4.4.2. Is the critical market share lower in more concentrated markets?

As noted in the introduction, mergers are more likely to be approved in markets with lower concentration. Our results so far raise serious questions about this practice, as many states have low concentration levels but exhibit low critical market shares. As such, there is the possibility of a Type II error by authorities in terms of approving a merger even though it leads to (or reaffirms) market power.

An important caveat here is that, consistent with the Lucas critique, a merger may alter the market dynamics. To partially address this concern, consider the relationship in Figure 3b between critical market share and HHI in the first year in which there is market power. This relationship appears to be positive. For the states shown there, based on casual observation of the marginal effects of increases in market share on markup, an increase in market share (e.g., due to a merger) tended to have a much larger effect in less concentrated markets. This relation is confirmed by the regression results in Table 5, in which a positive estimated coefficient is found when critical market shares are regressed on HHI. Again, there is a risk of Type II errors, as authorities may be inclined to wrongly approve mergers in states with low concentration, even though the merger may result in significant market power.

Table 5: Regressions of market power measures on HHI in states

Is there more collusion when the market is more concentrated?					
	HHI		Constant		N
%*	0.004	(0.83)	43.853	(6.17)**	95
Is the critical market share lower in more concentrated markets?					
	HHI		Constant		N
$\theta^*_{i,t}$	0.002	(2.54)*	-0.268	(0.19)	95
Do banks in more concentrated markets earn higher rents?					
	HHI		Constant		N
rents	0.014	(1.53)	9.738	(0.62)	67

Estimations for the first and second question are based on a Tobit regression model with upper (100) and lower (0) limits. Estimations for the third question are based on a standard regression model. All estimations utilize estimated results from Table A.1.

4.4.3. Do banks in more concentrated markets earn higher rents?

The extent to which regulatory authorities contemplate Type I and II errors in assessing the level of competition depends on how market power affects consumers. As noted earlier, some market power may foster financial innovation and stability. Therefore, it is important to know the extent to which market power enables firms to capture rents. Figure 3c shows the relationship between our rough measure of rents earned by banks with market power and HHI. The overall pattern of the data is disconcerting, as banks with high rents appear to operate in markets with low concentration.[34] However, results from regressing rents on HHI in Table 5 do not suggest a significant relationship between these two variables. We infer that market concentration likely has little effect in terms of enabling those with market power to earn rents.

4.4.4. Has competition increased after the IBBEA of 1994?

In order to stimulate competition, legal and regulatory changes aimed at lowering entry barriers, removing geographic barriers, lowering costs, and enabling innovation have been periodically implemented in the U.S. banking industry. A major legislative change in our sample period took place in 1994 when the Interstate Banking and Branching Efficiency Act (IBBEA) was passed by the U.S. Congress. This Act sought to significantly enhance competition by allowing banks to compete across state borders. In view of major restructuring in the banking industry due to this legislation, as a final assessment of the level of competition in U.S. banking, we briefly examine the impact of the IBBEA.

Table 6: Competition before and after the 1994 IBBEA

States with collusion before	37
States with collusion after	36
States with collusion before and after	26
States with collusion only *before*	11
States with collusion only *after*	10

Based on estimates per period provided in Table A.1.

Dividing our sample data into the subperiods 1984-1993 and 1994-2004, Figure 3d summarizes different aspects of collusion before and after the IBBEA. The critical market share required to have market power increased from an average of about 4 percent to about 6.5 percent. However, due to increases in concentration, the percentage of banks with market power (in states where there is collusion) did not change. Also, average rents earned decreased by almost 4 percentage points. Table 6 provides further information (based on Table A.1.). The number of states with collusion stayed almost constant before and after IBBEA.

[34]Based on Table 4, these results may reflect the low number of observations for banks with market power to some degree.

Although there were 11 states with collusion only before interstate banking deregulation, 10 states experienced collusion only after the IBBEA was implemented. Thus, we infer that, while the IBBEA had mixed effects on bank competition across states, on average competition was not generally affected on the state level.

5. Conclusion

Competition tests based on market concentration measures, such as the popular Hirschman-Herfindahl Index (HHI), assume either perfect competition or perfect collusion and, therefore, are prone to a fallacy of division. In the real world there is a continuum between these extreme endpoints that encompasses many competitive market conditions. Extending previous work by Cowling and Waterson (1976) and Cowling (1976), we relax this restrictive assumption by assuming that Cournot-type collusive rents can be earned by all, none, or some of the firms in a market. We propose that a firm's markup (or Lerner index) is a function of its market share, conjectural variation (or reaction of rivals to a change in a firm's output), and their interaction. The proposed model allows estimation of how large a firm must be to achieve critical market share as a collusive oligopolist. Subsequently, the percentage of firms with market power can be estimated. Other potentially valuable market power metrics are the first year in which at least one firm gained market power, the marginal effect on markup of an increase in market share, percentage of the markup due to market power, and the dollar value of profits due to rents of colluding firms.

Empirical specifications of the model are developed that take into account the possibility of endogeneity as well as a new measure of conjectural variation. We applied the model to data for U.S. commercial banks at the state level in the period 1984-2004. In general, our results do not support concentration as a singular measure of competition. While we found considerable evidence of collusion at the state level, the extent of collusion, rents earned from collusion, and collusive concentration levels varied widely across states. Even when some banks have market power in a state, it is rarely the case that collusion is perfect among all banks in the state. Instead, large banks in some states collude. Moreover, reliance on concentration as a measure of competition results in both Type I and II errors, as there are both states with high-concentration/high-competition levels and states with low-concentration/low-competition levels. Consistent with our model, we found more collusion among banks with market shares beyond the critical market share required in a state. However, there was no clear time pattern as to when a state reaches the critical market share. Compared to less concentrated markets, more concentrated markets do not exhibit more collusion, tend to have larger critical market shares, and are not more likely to earn collusive rents. Lastly, interstate banking deregulation did not have major competitive effects on the banking industry.

We conclude that empirical evidence supports a fallacy of division with respect to market concentration as a measure of competition in the U.S. banking industry. An important policy implication is that the U.S. Department of Justice, Federal Reserve, Comptroller of the Currency, and similar authorities in other countries should supplement HHI concentration guidelines with information on critical market shares and related market power metrics for commercial banks provided herein. In this way a more complete picture of market competition and collusion can be obtained in bank merger analyses and decisions. We have made a start in terms of identifying banks with market power. Also, in view of the Lucas critique, we have shown that our method can be informative about the current level of market competition.

Our new competition measures open up a number of new avenues for possible future study. For example, further research is recommended with respect to other industries, the definition of the relevant market, and validity of Cournot-type competition models. In addition, due to reputation risk, bank supervisory authorities may be interested in the certainty equivalent of critical market shares (i.e., minimizing type II errors associated with wrongly accusing a bank of market power). Also, supervisory authorities may be concerned with the potential effects of market power on the safety and soundness of financial institutions (e.g., systemic spillover effects of large bank failures on other banks and financial markets).

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A. Robustness results for the subperiods 1984-1988, 1989-1993, 1994-1998, and 1999-2004

Table 7: 5-year estimates

State	θ^*	%*	*year**	*HHI**	mfx^*	rents	dollar bonus	n^*	period
Alabama	1.34***	3.13	1984	571.79	0.38	-70.08	-166164.10	7.49	1984-1988
Alabama	1.34***	3.13	1984	571.79	0.38	-70.08	-166164.10	7.49	1984-1988
Alabama	0.73***	6.80	1989	918.37	3.43	-50.34	-101180.90	14.80	1989-1993
Alabama	0.25***	9.39	1999	1890.19	0.95	2.87	-22525.88	14.49	1999-2004
Alaska	27.66***	19.44	1994	2911.01	0.26	9.22	3937.16	1.33	1994-1998
Arizona	0.41***	41.60	1999	4598.03	0.99	16.84	14575.73	17.38	1999-2004
Arkansas	0.57***	18.54	1994	135.43	10.74	-6.45	-3051.66	42.46	1994-1998
California	0.00***	100.00	1989	1074.26	4.78			458.10	1989-1993
California	49.28***	0.17	1994	2676.25		177.82	29400000.00	0.59	1994-1998
California	0.12***	36.64	2004	585.56	1.18	11.04	14990.16	77.73	1999-2004
Colorado	0.46***	10.17	1984	234.77	10.60	57.59	11643.13	43.42	1984-1988
Colorado	0.25***	24.95	1989	276.37	3.82	15.06	1521.59	93.89	1989-1993
Colorado	0.00***	100.00	1994	318.50	0.53			224.53	1994-1998
Connecticut	6.52***	8.22	1984	1828.37	0.36	-27.16	-154322.40	4.80	1984-1988
Connecticut	14.53***	3.03	2001	722.22	0.00	347.37	1368986.00	0.50	1999-2004
Delaware	6.24***	40.48	1984	1564.34	0.19	31.59	52180.79	6.77	1984-1988
Dist of Columbia	3.31***	27.45	1984	2403.24		571.73	1304418.00	5.63	1984-1988
Dist of Columbia	0.00***	100.00	1989	2140.34	0.15			21.50	1989-1993
Florida	0.00***	100.00	1984	364.69	1.48			402.00	1984-1988
Florida	0.02***	86.95	1989	372.59	0.94			345.05	1989-1993
Florida	0.06***	88.70	1999	295.62	2.14			231.43	1999-2004
Georgia	0.13***	48.91	1997	916.31	0.12	1.97	-1201.20	113.99	1994-1998
Georgia	3.46***	0.41	1999	1219.29	0.24	41.45	1856724.00	1.34	1999-2004
Hawaii	0.00***	100.00	1989	3166.74	2.34			19.38	1989-1993
Hawaii	25.72***	26.67	1999	4038.03		-169.20	-1266789.00	2.00	1999-2004
Idaho	4.40***	16.67	1984	2326.83	0.70	96.38	142776.40	4.00	1984-1988
Idaho	52.37***	0.00						0.00	1989-1993
Illinois	0.00***	100.00	1984	678.95	1.53			1204.26	1984-1988
Illinois	0.00***	100.00	1989	674.91	0.57			1036.87	1989-1993
Indiana	0.23***	26.86	1984	209.35	3.92	-2.51	-2285.17	95.06	1984-1988
Indiana	0.00***	100.00	1989	196.98	3.80			279.54	1989-1993
Indiana	0.43***	20.91	1994	504.18	0.32	12.97	7316.90	40.90	1994-1998
Indiana	0.00***	100.00	1999	921.53	0.39			150.72	1999-2004
Iowa	0.05***	88.37	1994	53.82	4.86			414.85	1994-1998
Iowa	0.02***	99.23	1999	279.35	6.48			408.83	1999-2004
Kansas	0.19***	26.83	1991	259.36	1.19	-0.11	-505.10	138.39	1989-1993
Kansas	0.18***	31.62	1994	381.22	1.56	-1.48	-847.35	131.71	1994-1998
Kansas	0.50***	10.08	1999	242.84	0.41	6.43	320.40	36.90	1999-2004
Kentucky	0.54***	14.15	2003	462.81	3.91	35.17	25937.81	25.14	1999-2004
Louisiana	0.53***	10.92	1984	175.18	0.66	-64.98	-41847.15	30.61	1984-1988
Louisiana	0.00***	100.00	1989	669.93	1.36			222.55	1989-1993
Louisiana	0.65***	9.75	1994	640.56	1.61	-2.74	-12214.61	16.96	1994-1998
Maine	19.66***	6.98	1985	1177.67		-0.45	-10027.47	1.09	1984-1988
Maine	87.09***	0.00						0.00	1999-2004
Massachusetts	0.00***	100.00	1984	1230.37				110.78	1984-1988
Michigan	0.00***	100.00	1985	629.95	0.52			325.59	1984-1988
Michigan	0.00***	100.00	1999	1285.63	0.53			162.22	1999-2004
Minnesota	0.00***	100.00	1999	2029.77	2.18			476.11	1999-2004
Mississippi	0.30***	48.36	1989	748.07	10530.88	-26.05	-9504.81	58.79	1989-1993
Missouri	3.25***	0.92	1986	236.08	0.26	4.89	-1164.75	4.28	1984-1988
Missouri	0.15***	19.85	1994	423.85	2.07	1.16	-1880.96	84.34	1994-1998
Missouri	0.18***	26.96	2001	434.45	1.94	10.70	2398.52	89.53	1999-2004

Continued on next page ...

Table 7 (Continued from previous page)

State	θ^*	%*	$year^*$	HHI^*	mfx^*	rents	dollar bonus	n^*	period
Montana	0.44***	52.25	1991	171.42	1.33	12.92	655.94	63.07	1989-1993
Montana	2.80***	4.63	1994	301.88	0.29	-13.53	-12786.45	4.60	1994-1998
Montana	22.15***	1.27	2001	847.07	0.96	57.89	127116.50	0.65	1999-2004
Nebraska	3.27***	0.70	1989	252.53		-25.58	-38974.81	2.60	1989-1993
Nebraska	0.80***	7.65	1999	207.77	0.81	7.10	708.94	20.15	1999-2004
Nevada	2.19***	23.75	1999	2600.93	0.43	2.22	-1841.76	6.51	1999-2004
New Hampshire	0.00***	100.00	1984	391.69	3.36			54.84	1984-1988
New Hampshire	4.04***	38.24	2000	4312.38	0.08	20.40	11615.03	4.65	1999-2004
New Jersey	0.00***	100.00	1994	766.48	0.35			76.26	1994-1998
New Jersey	0.00***	100.00	1999	1587.46	1.68			78.95	1999-2004
New Mexico	0.49***	59.52	1985	466.91	4.59	43.91	5351.26	56.01	1984-1988
New Mexico	2.34***	7.94	1989	538.51		-67.74	-47523.94	6.79	1989-1993
New Mexico	0.52***	60.00	1997	879.30	1.15	28.28	5799.13	39.30	1994-1998
New York	0.00***	100.00	1984	1082.75	2.10			192.82	1984-1988
North Carolina	1.00***	11.78	1989	1717.66	0.55	35.39	238038.30	9.00	1989-1993
North Dakota	1.02***	12.22	1984	122.08	1.23	-0.81	-763.03	21.12	1984-1988
North Dakota	0.16***	86.42	1999	394.81	0.16	5.36	46.02	90.42	1999-2004
Ohio	0.06***	47.25	1994	701.10	0.18			114.54	1994-1998
Oklahoma	0.35***	13.01	1992	181.69	3.31	18.11	3395.75	50.25	1989-1993
Oklahoma	8.92***	0.52	1996	374.18	0.50	16.17	43037.21	1.39	1994-1998
Oregon	0.00***	100.00	1989	3062.33	2.47			45.99	1989-1993
Oregon	0.87***	43.18	1999	716.36	0.45	22.94	12667.50	16.22	1999-2004
Pennsylvania	1.80***	3.29	1989	472.97	1.78	43.36	378414.80	9.38	1989-1993
Pennsylvania	0.06***	61.10	1994	1115.12	0.35			134.19	1994-1998
Rhode Island	12.58***	15.00	1984	4660.74	0.17	32.89	147546.80	1.78	1984-1988
Rhode Island	0.00***	100.00	1989	4098.08	0.60			10.93	1989-1993
Rhode Island	0.16***	91.43	1994	5857.59	0.09	165.84	518777.20	6.49	1994-1998
South Dakota	2.15***	3.33	1987	838.17	0.15	23.33	14299.17	4.00	1984-1988
South Dakota	5.86***	0.88	1989	1045.58		117.40	248054.00	1.00	1989-1993
South Dakota	0.40***	41.18	1994	1700.23	1.65	-7.88	-2392.76	42.26	1994-1998
South Dakota	0.00***	100.00	1999	2326.59				83.28	1999-2004
Tennessee	11.22***	0.35	1984	400.97	0.57	37.26	167580.90	1.00	1984-1988
Tennessee	0.17***	27.97	1999	1766.69	0.34	5.33	-466.06	53.88	1999-2004
Texas	0.03***	40.69	1994	692.22	0.67			359.87	1994-1998
Texas	0.16***	15.28	2001	312.37	3.99	12.96	5518.47	89.81	1999-2004
Utah	26.16***	2.24	1985	1659.08	1.95	83.34	224403.50	1.20	1984-1988
Utah	5.44***	7.43	1994	2009.21	1.17	-15.80	-94304.94	2.96	1994-1998
Utah	39.44***	1.95	1999	2513.30	0.33	41.20	883669.10	1.00	1999-2004
Vermont	8.06***	19.38	1984	1038.70	0.20	17.44	7657.46	5.00	1984-1988
Vermont	1.01***	86.54	1994	1149.80	10.76	5.12	-158.48	18.02	1994-1998
Virginia	0.32***	16.77	1984	1162.51	0.86	0.33	-5245.91	28.38	1984-1988
Virginia	0.45***	11.43	1989	1292.42	0.51	-10.80	-31761.63	19.36	1989-1993
Washington	7.05***	4.09	1984	1710.37	0.23	35.20	101090.60	3.81	1984-1988
Washington	15.14***	2.22	1994	1514.19	0.11	19.76	101803.90	1.84	1994-1998
Washington	0.58***	44.80	1999	433.39	3.86	-4.20	-4059.43	35.17	1999-2004
West Virginia	0.44***	31.14	1984	101.41	3.32	-16.45	-2939.14	66.21	1984-1988

Results are based on the preferred specification among equations (9a)(9c) as discussed in the text. Also, results are reported for only states and subperiods in which evidence of collusion is obtained. Significance is indicated at the following levels: 1/5/10% (***/**/*, respectively).