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THE EFFECT OF BLIND-BIDDING LAWS ON MOVIE THEATERS**

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## **Abstract**

In the 1970s motion picture studios increased their use of blind bidding and non-refundable guarantees in an attempt to reduce the risks associated with producing a small number of large budget films. However, theater owners claimed that blind bidding and guarantees shifted risk to them and increased the likelihood of bankruptcy, since they were required to bid for the right to exhibit a movie without seeing it first. In response to the lobbying of theater owners, twenty-four states passed laws between 1978 and 1984 that banned blind bidding, while seven states also banned non-refundable guarantees. This paper provides the first empirical analysis of the conflicting claims made by theater owners and movie studios about the impact of these laws on the survival rates of independent theaters, admission prices, and delays in the release of movies. We find that the laws were not only ineffective in keeping theater owners at risk of bankruptcy from exiting the market; they may have been even detrimental to those theater owners converting theaters to multiplexes at that time.

## 1. INTRODUCTION

The recent bailouts of the U.S. automotive and banking industries have generated renewed concern about the incentives that government intervention give failing firms in need of significant restructuring. Government intervention to help failing firms has a long and varied history. For example, the Robinson-Patman Act (1936) has been widely criticized for diminishing the cost advantages of larger, more efficient stores at a time when the retail industry was experiencing an increase in average store size in response to changing demographics and lower transportation costs (Posner, 1976, and Ross, 1984).<sup>1</sup> A large literature also suggests a loss in efficiency resulting from governmental restrictions on hostile takeovers, such as the Williams Act, given that the threat of a hostile takeover can be a mechanism for encouraging existing management to restructure failing firms during periods of rapid technological change (Holmstrom and Kaplan, 2001).

While identifying the impact of a specific government intervention empirically is often difficult, the motion picture industry presents a unique opportunity for a natural experiment of government intervention to save failing firms. As a result of the Supreme Court's 1948 landmark decision, *United States vs. Paramount et al.*, the five major, vertically integrated studios/distributors were no longer allowed to own theaters. The major studios eventually adjusted to this decision and to the changes in post-war demographics by producing a smaller number of large budget films and changing their distribution strategies. An important change in their distribution strategy was an increased use of non-refundable guarantees and blind bidding, which required theater owners to bid six months to one year in advance of a film's release. Since bids were submitted prior to the completion of the movie, theater owners bid without seeing a finished copy.<sup>2</sup>

In response to complaints from theater owners that blind bidding and non-refundable guarantees were shifting excessive risk to them and would drive them out of business, the federal government initially limited the use of blind bidding but then removed all restrictions in the mid-1970s. Theater-owner trade associations soon convinced 24 states to pass laws banning blind bidding. Seven of the blind bidding laws also limited the use of non-refundable guarantees. In this study we find that while the strictest of the state laws (that is, the ones that also limited the use of non-refundable guarantees) helped some theater owners remain in business longer, this

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<sup>1</sup> For a comprehensive debate on the merits of U.S. antitrust enforcement in general, see Crandall and Winston (2003) and Baker (2003).

<sup>2</sup> They received only a bid letter summarizing the plot and listing the names of the actors.

effect was short-lived. We show empirically that the laws, whether they limited guarantees or not, could not offset the impact of significant structural changes in the market. Prior to the passage of the first law, some theater owners were already beginning to increase the number of screens per theater in order to diversify risk and exploit economies of scale. We find that the number of screens per theater was the main factor influencing the survival of theater chains during this time period. While our main focus is the impact of the laws on theater survival, for completeness we also offer empirical evidence concerning the conflicting claims of the theater owners and studios about the effect of laws banning blind-bidding and guarantees on ticket prices and release dates of new movies. We find no support for theater-owner assertions about higher ticket prices in states that permitted blind bidding and the studios' assertions about longer delays in release dates in states that banned blind bidding.

In the next section we review the historical evolution of the blind-bidding laws and the literature on contractual relations between the studios and theater owners. In Section 3 we present the empirical models and results concerning theater survival. Sections 4 and 5 present the evidence of the laws' impact on prices and delays in release dates. Section 6 provides a conclusion.

## **2. HISTORICAL BACKGROUND AND ECONOMIC LITERATURE ON CONTRACTUAL ARRANGEMENTS IN THE MOVIE INDUSTRY**

In this section we outline the history of blind bidding leading to the passage of the laws banning blind bidding in 24 states between 1978 and 1984 and review the theoretical and empirical literature on contractual arrangements in the movie industry. This section highlights the significant difference of opinion about the merits of the laws at the time of their passage. We also establish our theoretical motivation for the empirical model of the next section. We hypothesize that the impact of the laws on theater survival would be at best short-lived given the dominating changes in market structure due to the transition from one and two-screen theaters to multiplexes.

### **2.1 An Historical Overview of Blind Bidding Laws**

Tension between the studios and independent theater owners dates to the 1930's and 1940's when the five largest studios (Paramount, Loew's/MGM, Warner Brothers, Twentieth Century Fox and RKO) were vertically integrated companies. The studios controlled a majority of theaters showing new releases in major cities. Vertical integration allegedly minimized competition from independent producers, movie companies, and theater owners.

The *Paramount* decision in 1948 coincided with the start of a downward industry trend that persisted into the early 1970's. As argued by *Harvard Law Review* (1979) and Sedgwick (2002) among others, the timing of the theater divestiture did not benefit the studios, as the selling coincided with the baby boom phenomenon after the end of World War II. When the war ended, consumers bought homes and durable goods for their newly formed families. While those with higher education and income generally frequented movie theaters more often, this segment of the population was having children at an historic pace and moving farther away from the centrally located theaters which the studios owned. The studios had to divest their theaters at a time when the value of their holdings was declining due to lower box office revenues.

Television accelerated the decline in theater attendance. By 1954 half of U.S. households had a television set. By 1959, eighty-five percent of households had one (Waterman, 2005, p. 34). Television quickly became the substitute for going to theaters. As a result, film output dropped, as the studios concentrated on producing a smaller number of large budget films. Figure 1 shows the total number of tickets sold between 1970 and 1985, while Figure 2 shows new releases for this time period. By 1970 the number of new films had reached a low point.

Box office returns also became more volatile than in the pre-television years. At the time *Variety* defined a successful film as one where the domestic rentals covered most of the cost of producing the film. By this definition of success, about 33 percent of big budget films were successful between 1976 through 1981 (*Variety*, 1986, p. 9). In addition, the most important new releases were concentrated in three predictable periods: Christmas, pre-July 4th, and Easter. As a result, some theater owners even resorted to closing their doors temporarily for a few weeks or months until a peak season would arrive. Joseph Alterman, executive director of NATO (that is, the National Association of Theater Owners), stated in 1977, "This is the first time in history that so many large, economically successful theaters closed because there were no movies" (Gottschalk, p. 29). The short supply of films allegedly escalated

rental terms to a point where even larger theater chains claimed it was difficult to compete for them.<sup>3</sup>

The Department of Justice was initially sympathetic to the theater owners' argument that blind bidding was responsible for increasing the bankruptcy rate of theaters. In response to the increased use of blind bidding, the Department of Justice concluded an agreement with the movie studios limiting the number of films which could be blind bid from January 1, 1969 to January 1, 1971 to three per studio per year.<sup>4</sup> The two-year agreement was renewed twice.<sup>5</sup> However, the Department of Justice revoked all restrictions limiting blind bidding in 1975 and the practice accelerated rapidly. Once the Department of Justice removed the restrictions on blind bidding, theater owners actively lobbied state governments to ban this practice. In

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<sup>3</sup> Theater owners were mostly concerned about the payment of non-refundable guarantees that could exceed what they would otherwise have paid through the contractual sharing rule. The sharing rule was a sliding scale basis with a higher percentage allocated for the movie studios. Theater owners recovered their house expenses plus received a lower percentage of the film rental. In the first few weeks of a new release the studios often received the highest of 90 percent of the box office in excess of a minimum negotiated house expense (which was called 90/10 percentage terms) or 70 percent of the total box office. The percentage the studios received dropped as the film progressed into later runs. See Hanssen (2002) and Filson et al. (2005) for a more detailed discussion of the evolution of contractual terms over time.

<sup>4</sup> Blind bidding was made illegal by a 1940 Consent Degree, however, a 1944 District Court opinion permitted it as long as exhibitors could reject films which were blind bid.

<sup>5</sup> The agreement also allowed theater owners to cancel the bidding arrangement up to forty-eight hours before the play-date, but theater owners alleged that it was difficult to replace the scheduled movie with anything comparable on short notice.



response to NATO's efforts, twenty-four states banned blind bidding between 1978 and 1984. Table 1 lists the 24 states that passed a law.

## **2.2 Literature**

A study of blind bidding laws also has implications for the vast literature on vertical relationships dating back to Coase's seminal work (1937) on the nature of the firm. When complete vertical integration is too costly or not allowed by law, firms often resort to alternative contractual arrangements. Despite a literature suggesting economic efficiency arguments in favor of many of these vertical relationships, several have been the subject of government intervention, such as resale price maintenance, exclusive dealing, exclusive territories, and tied sales. In light of the efficiency arguments, Easterbrook (2003) has argued that the courts should look more favorably on such vertical arrangements. However, as Carlton and Perloff (2004) indicate, despite the progress in theoretical modeling of various vertical arrangements, there is still a need for more convincing empirical work that identifies both their desirable and undesirable effects.

The Paramount decision banned complete vertical integration and required bidding on a film by film basis. *Harvard Law Review* (1979), DeVany and Eckert (1991), Devany and Winston and DeVany and Walls (1997), among several others, argue that the Paramount decision was ill-advised. For example, according to DeVany and Walls (2007), "the Supreme Court has stood in the way of ownership, franchising, or other forms of long-term contracting between exhibitors and distributors," DeVany and Eckert and Sedgwick add that the studio system and block booking were adaptations to added uncertainty associated with the decision. In addition, Filson (2005) shows in a stationary dynamic common agency model with two distributors

and one exhibitor that complete vertical integration can be privately profitable and socially beneficial due a more favorable pattern of release dates and run lengths.

While there is an extensive literature documenting and analyzing all aspects of the resultant vertical contractual relationships in the movie industry, a primary focus of empirical work has been the uncertainty of predicting success of individual movies and the impact of this uncertainty on contractual sharing arrangements due to risk aversion of both the studios and theater owners. For example, in a series of articles DeVany and Walls attempt to identify the predictors of such outcomes as run length and domestic box office receipts. DeVany and Walls (1997) provide empirical evidence that the number of theaters showing the movie in its first run was the only economically meaningful predictor of run length. Blumenthal (1989) finds empirically that this factor is also an important determinant of the level of winning bids by theater owners. DeVany and Walls (1999) add that their empirical results imply that anything can happen and that film makers “don’t know anything” about the potential success of a given movie. They suggest that “the difficulties of predicting outcomes for individual movies are so severe that a strategy of choosing portfolios of movies may be preferred to the current practice of ‘greenlighting’ individual movie projects” (p. 315).

Despite the more recent empirical evidence in the literature concerning the difficulty of predicting success, the informational value associated with seeing the movie prior to bidding on it was the subject of a heated debate at the time of the passage of the laws banning blind bidding. For example, DeVany and Eckert echo a common complaint from studios and distributors that trade screenings were poorly attended and that trade screening did little to affect the sharing rules or predict success.

On the other hand, while theater owners admitted to attending only some of the previews, they complained of the lack of the option of attending a trade screening in order to avoid the worst of the movies, “pigs in a poke,” that might result in having to pay a binding guarantee or to miss an otherwise unexpected blockbuster. For example, in a memo to the Massachusetts legislature in support of banning blind bidding in that state, John Shenefield, an assistant U.S. attorney, summarized this view by stating that “[Blind Bidding] denies exhibs the right to make an informed judgment as to artistic or box-office merit, tying up playing time, forcing huge guarantees, and resulting in ‘examples that can be cited of exhibitors having been driven into bankruptcy by inferior films which were blind bid’ (Jacobson, 1978). Even Jack Valenti, the President of the Motion Picture Association of America (MPAA) at the time, in his public arguments in favor of blind bidding implied that theater owners might be able to determine which movies would be blockbusters. “With the intense competition between big theater chains, when a movie is shown at a screening and it’s an obvious

smash, the bidding will go right through the ceiling. Exhibitors will bid more and will charge more at the box office” (Gottschalk, Jr., 1979, p. 1).

Studios argued that blind bidding provided a timely return on investment and advanced coordination of the film’s release date with promotional efforts and booking of television advertisements, which were secured six months to one year in advance. They claimed that there would be lengthy delays in release dates without blind bidding. Since films were completed close to their release date, a requirement to show the movies in advance of the bidding process (that is, trade screening) may not have allowed enough time for films to arrive by the anticipated release date. Guarantees allowed the studios a minimum expected return on their investments and offset the allegedly common practice of slow payment of film rentals by theater owners.

On the other hand, theater owners alleged that overbidding due to blind bidding and guarantees resulted in many of them going out of business and raised admission prices due to the theater owners’ need to cover extensive losses from overbid films. They also claimed that higher rental terms would make it more difficult for some theater owners to obtain blockbuster films, since they would not be able to outbid the larger theater chains.

The exhibition market was undergoing important structural change at the time the states were passing laws banning blind bidding. The box office success of *Jaws* in 1975 marked a change in release strategy at the same time as theater owners were lobbying for legal intervention. Universal released the film simultaneously in 500 theaters (Paul, 1994). This strategy was intended to take advantage of the mass advertising campaign which touted the film. Previously, movie companies commonly kept the number of prints below 400 in order to keep down costs (Gottschalk, Jr., p. 46). This number increased to between 1,000 and 1,500 by the early 1980’s (Myers, 1983, p. 278).

While DeVany and Walls (1999) suggested that studios could reduce risk by diversification of their portfolio of movies at any point in time, some risk-averse theater owners were beginning to accomplish the same result by increasing the number of screens at existing theaters and building new multiplex theaters in shopping centers and malls. Figure 3 displays the large overall growth in the number of screens from 1970 to 1985.<sup>6</sup> Theater owners had several reasons to switch to multiple screens. First, theater owners were adding screens to reduce risk. Second, multiplex theaters, especially new ones, were designed to best exploit concession sales, because the

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<sup>6</sup> In 1978, only 10 percent of indoor theaters were multiplexes; while 80 percent were twin theaters (International Motion Picture Almanac, 1978, p. 31A.).

design of the buildings focused on a centrally located concession stand. Third, theater owners were expanding to shopping centers. Since shopping center developers constructed the theaters, theater owners limited their investment to the furniture, fixtures, and equipment.<sup>7</sup>

Our contribution to the literature is empirical evidence that supports blind bidding and guarantees as a logical efficiency-enhancing alternative to complete vertical integration during this period of significant structural change in the industry. As early as 1979 *The Harvard Law Review* questioned the impact of laws banning blind-bidding on market-efficiency and legal grounds. However, in addition to Carlton and Perloff's call for more empirical evidence of the benefits and consequences of vertical arrangements in general, Orbach and Einav (2007) note that while the practice of blind bidding is as controversial as block booking, empirical evidence of its consequences is lacking.

For example, despite the criticism of the state laws in the literature and the questions about the informational value of trade screening, the only empirical study of the impact of blind bidding, Blumenthal (1989), concluded that theater owners were justified in opposing blind bidding and initiating requests for legislative relief.<sup>8</sup> In a natural experiment using contractual data on successful bids in 1982 by one theater chain with theaters in both blind bid and preview states, Blumenthal found that winning bids were lower in states that allowed blind bidding. She argued, however, that in blind-bid states the unfavorable impact on theater owners' risk due to increased variance in quality of the theater owner's successful bids was relatively greater than the favorable impact of the decrease in guarantees necessary to win the bidding.

Blumenthal defined the bid as the guarantee, claiming that other contractual terms, such as percentage share or guaranteed length of run, generally did not vary

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<sup>7</sup> "The exhibition company that leases property may spend only \$100,000 to \$150,000 to equip and modestly furnish each auditorium, and it does not need to tie up capital in cinder and cement" (Guback, 2001, p. 129).

<sup>8</sup> Forsythe, Isaac, and Palfrey (1989) used blind bidding as motivation for laboratory experiments with  $n$  buyers and one seller in a sealed-bid, first-price auction. They concluded that their results implied that the anti-blind bidding laws were unnecessary as buyers would learn that a seller withholds information when it is unfavorable. In their model a seller would abandon blind bidding once all buyers learn that withholding information was in the seller's best interest and not theirs. However, the studios generally did just the opposite by trade screening relatively less desirable films and blind bidding highly anticipated films.

much across bid letters. Filson et al. mention “in practice, the floor is the relevant payment the vast majority of times (p. 356)”. In Blumenthal’s sample the guarantee was binding twenty-four percent of the time.<sup>9</sup> Blumenthal’s empirical results of a lower average winning guarantee and higher variance in blind-bid states lend further support to those arguing that trade screening did provide information to theater owners beyond that available in the bid letter. Although a lack of information about the quality of the movie at the time of the bidding may have no effect on the percent share of revenues received by exhibitors, it could increase the probability of having to exhibit a movie during the peak season even though the guarantee significantly exceeded the value of the movie to the theater owner. Given the important role of the non-refundable guarantee in the bidding process and the uncertainty associated with not having the opportunity of seeing the movie in advance, theater owners argued that there was a greater likelihood of having several peak seasons of showing movies at a loss with blind bidding and could lead to bankruptcy. As Filson et al. conclude, “Operating a theater involves many fixed costs that cannot be avoided without exiting... These differences (relative to distributors) may make exhibitors more reluctant to bear downside revenue risk than distributors (p. 368).

While the debate over the magnitude of the informational value of trade screening is interesting in its own right, studio heads argued that any benefit due to an increase in its informational value came at an even higher loss of efficiency. For example, Mr. Valenti argued that “the end of blind bidding would mean fewer big budget, blockbuster movies .... movie companies just won’t be able to put so much money into a film if they don’t know for sure they have play-dates in theaters at one of the prime times” (Gottschalk, Jr., 1979). According to Valenti, “a substantial slowdown –let alone outright cutoff – of such revenue [from guarantees] would have a serious impact on production” (Variety, 1977, p. 7).

Of particular importance to our empirical approach is the role of diversification at the theater level on theater survival and market efficiency. Blumenthal’s conclusion about the impact of blind bidding ignored the structural changes taking place in the market at that time. In arriving at her policy conclusion, Blumenthal implicitly assumed that theater owners showed only one movie at a time. As a result, she did not mention or account for the fact that winning bids during this time period could have been heavily influenced by the increased competition from larger, more diversified chains that benefited from the lower winning bids per movie relative to the less diversified theater owners, while being less affected by the increased variance in box

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<sup>9</sup> For example, DeVany and Eckert argue that theater owners complained primarily about having to bid for movies and that the binding guarantee, not the sharing rules, were the most onerous condition of the blind-bidding process.

office receipts per movie associated with blind bidding. As Marvin Goldman, president of NATO during the time of the passage of the laws, explained that “if you have one theater with one thousand seats and you have a film not doing well, you’re in trouble”, but “if you have a six one hundred sixty seat theaters, you might have three or four winners and two or three losers. You’re better off,” (Gottschalk, p. 29).

As a result, sufficiently diversified theater owners in blind bid states would be less concerned about overbidding on any one movie and could benefit from the lower average guarantee. Multiplex owners, therefore, lost the benefit of being able to offer lower guarantees on average in states that required trade screening. In other words, blind bidding laws actually could have harmed theater owners that had adapted to the changing market conditions during the transition period while in the end not helping those who resisted the change.

In addition to our hypothesized effect of the laws on theater survival, one would thus not be surprised by our empirical finding in the next section that the likelihood of survival at this time increased with the number of screens per theater both in states allowing and banning blind bidding. In fact, we find that the magnitude of the marginal effect of the number of screens per theater is essentially identical across our data samples and empirical models.

### **3. BLIND BIDDING, GUARANTEES AND SURVIVAL**

As noted in the previous section, while there has been no lack of assertions about the expected consequences of the blind bidding laws and the informational value of trade screening, there has been no empirical study isolating the impact of the laws on the survival of theater owners. In this section we analyze the impact of the laws on the survival of theaters in two ways. Initially, we use a natural experiment to compare a sample of theaters in three states that passed similar laws banning blind bidding but not guarantees in 1979 (Georgia, Tennessee, and North Carolina) with a sample of theaters in Florida that never had such a law. We choose these states because of their similarity in size, regional location, and the timing of the passage of their laws.

As mentioned in section 2, Blumenthal also used a natural experiment in her study of contractual arrangements across blind-bid and trade-screen states. A natural experiment is especially suited for a study of blind bidding, given that the passage of the laws appears to be idiosyncratic and unrelated to exit rates prior to passage of the law.<sup>10</sup> For example, NATO believed it was easier to pass the law in states with year-

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<sup>10</sup> One cannot assume exogeneity of the treatment samples arbitrarily. For example, several authors, such as Kroszner and Strahan (1999), Zhang (2007), and Rosston et

long legislatures, since it was easier for opponents of the laws to table the measure in states with part-time legislatures. If a bill were not passed during the legislative year, it would have to be reintroduced the following year (Robbins, 1981).

The only consistent difference is state size. One explanation mentioned to us by trade association executives at the time and suggested by the *Harvard Law Review* article (footnote 90) was that the MPAA concentrated its lobbying efforts in the largest states for fear that passage by a critical mass of the largest states would by itself mean the end of blind bidding. However, efforts to pass a law in New Jersey were unsuccessful despite a legislature that worked year round and a state Senate vote in favor of prohibiting blind bidding of 29-0 in 1979 (*Variety*, October 31, 1979). In the years which followed NATO continued its unsuccessful efforts to pass the law in New Jersey knowing that the law did not need to be voted on by the Senate.

Finally, we do not find evidence that the theaters in our sample had significantly higher exit rates prior to the passage of the legislation. For example, between 1977 and 1981 Tennessee did have a much higher average rate of 48.65%, but the exit rates in Georgia (30.43%) and North Carolina (33.33%) were close to that of Florida (30.51%). Together, the weighted average for the three states that eventually banned blind bidding was slightly higher than that of Florida at 37.27%.

We use a probit analysis to model the probability of survival controlling for theater-specific, market-specific, and theater-chain-specific factors. Probit models have been used often to examine firm survival.<sup>11</sup> The probit model assumes that the likelihood that an event occurs depends on an unobservable index function. In this case, each theater has a critical index of profitability above which the theater exits the market. The critical values are assumed to follow a normal distribution.

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al. (2008) have shown that certain political economy variables can be important in explaining a state's decision to deregulate banking, electricity and local telephone markets, respectively.

<sup>11</sup> For example, Dunne, Klimek, and Roberts (2005) recently examine the survival rates of seven regional manufacturing industries in the U.S. using plant level data.

We find that the laws of Georgia, North Carolina, and Tennessee did not have a statistically significant impact on the survival of theaters. Given the lack of an impact of the law in these states, we use a second natural experiment with more detailed panel data for a sample of theaters in the two largest states with the strictest laws: Ohio and Pennsylvania. The Ohio and Pennsylvania blind-bidding laws were considered the most restrictive, because they also banned the requirement of including non-refundable guarantees in the bid letter.

We compare the survival rates of theaters in Ohio and Pennsylvania with those in the similarly sized states of Michigan and New York. Neither Michigan nor New York had laws against blind bidding or guarantees. Table 2 shows that exit rates are not systemically higher in Ohio or Pennsylvania prior to the passage of the laws. In fact, the weighted average exit rate is higher in the two blind-bid states relative to that in the preview states both before and after passage of the laws. We find that the laws of Ohio and Pennsylvania had an initial impact on survival times. However, the laws' effect diminished rapidly over time and was completely reversed within a few years. As mentioned in the previous section, we provide empirical evidence that the number of screens, not laws banning blind bidding and guarantees, was the most important factor in predicting a theater's survival. In fact, the number of screens per theater had essentially identical effects on the probability of survival in all the states.

### **3.1 Probit Analysis of Banning Blind Bidding**



Georgia, North Carolina, and Tennessee all passed similar legislation in 1979 banning blind bidding but permitting guarantees. We treat the passage of the anti-blind bidding laws as a natural experiment and use theaters in Florida, which did not pass a law, as a control group.

### **3.1.1 Data**

To estimate the immediate impact of the laws we use data for the two years before and after the passage of the laws (that is, 1977 and 1981). The data consist of 229 theaters from the largest thirteen cities in the four states: Florida (119), Georgia (46), North Carolina (27), and Tennessee (37).<sup>12</sup> We compiled the data set in the following manner. For the first week of June 1977 and June 1981, we recorded the name of all first-run theaters from local newspapers, the number of screens at each theater, and the name of the parent theater chain if relevant. Since we could not always identify the theater chain which operated a theater, we used the *International Motion Picture Almanac* (IMPA) to obtain additional information. The newspapers listed theaters in the city and in the nearby suburbs as well, therefore the data include the entire metropolitan area.

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<sup>12</sup> Data come from local newspapers in the following cities Fort Lauderdale, Jacksonville, Miami, Orlando, and Tampa, Florida; Atlanta, Columbus, and Savannah, Georgia; Charlotte and Raleigh, North Carolina; and Knoxville, Memphis, and Nashville, Tennessee.

### 3.1.2 Empirical Model and Results

We include controls for theater-specific, market-specific, and theater-chain-specific factors.<sup>13</sup> The number of screens per theater is a control for economies of scale, quality, and the potential for risk reduction through diversification.<sup>14</sup> Theaters with more screens can diversify the risk of committing to any one film, are more cost efficient, and offer more choices to consumers (a measure of quality). We include the number of screens per theater in 1977 and expect this variable to be inversely related to probability of exiting the market.<sup>15</sup>

We included a Herfindahl-Hirschman Index (HHI) of the total screens per theater chain in the local market in order to control for the degree of market concentration. In industry-specific studies, high concentration ratios and HHIs are associated with lower entry rates. For this reason, we expect this variable to be negatively related to exit. We include the total number of screens a theater chain operated in the U.S. as a control for firm size. We are unable to measure consistently the initial size of theater chains, so we include the total number of screens in the U.S.

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<sup>13</sup> While studies of firm survival sometimes include additional controls for the age of the firm, we were unable to find useable data on the age of movie theaters in our sample. For example, attempts to do so by direct contact of theaters and industry insiders were not fruitful. We have no reason, however, to expect that there was a sufficiently large enough systematic differences in theater age across our samples given similarities in regional location and city size.

<sup>14</sup> We also estimated the models with a quadratic term for the number of screens per theater. Even in the few cases when the coefficient for this variable was statistically significant, the estimated coefficients for the other variables were essentially unchanged. We find this same result for all the estimations reported in this paper.

<sup>15</sup> We assume that a theater exits the market if any one of the following occurs: (1) the theater owner leaves the market through the sale of the property to a theater chain or property developer, (2) a theater is no longer listed in the newspaper, or (3) a theater no longer exhibits first-run films.

as of 1975. A priori, we anticipate that larger theater chains are less likely to exit. We include a dichotomous variable with a value of one if the theater was in Georgia, Tennessee, or North Carolina and zero otherwise.

Table 3 provides the results along with summary statistics for each variable. The HHI and the total number of screens a theater operates in the U.S. do not have a statistically significant effect. As hypothesized in the previous section, the number of screens at the theater is the only variable with a statistically significant coefficient at the 5 percent level. An increase in the number of screens at a theater decreases the probability of the theater exiting during this time period on the margin by approximately fifteen percent. Most importantly, controlling for the number of screens at the theater and other industry characteristics, we find that theaters in Georgia, North Carolina, and Tennessee did not have statistically different exit rates than those in Florida.

### **3.2 The Impact of Banning Both Blind Bidding and Required Guarantees**

It is possible that the laws in Georgia, North Carolina, and Tennessee did not have an impact, because the laws did not also ban the requirement of paying guarantees. As a result, we next considered the effects of the presumably most restrictive laws of Ohio, passed in October 1978, and Pennsylvania, passed in May 1980. Both laws banned blind bidding and the requirement of including non-refundable guarantees in the bid letter. While these laws were generally regarded as

the most restrictive because of the ban on the requirement of non-refundable guarantees, there is anecdotal evidence that at least some theater owners in Ohio continued to offer them (*Harvard Law Review*, footnote 56). For this reason, one might conclude that the additional ban on the requirement of a guarantee would have had limited impact, because theater owners continued to offer them anyway despite the risk that the guarantee could be binding. We again use a natural experiment this time using theaters in two similarly sized nearby states, Michigan and New York, as the control group.

### **3.2.1. Data**

While we collected data comparable to the first sample in order to conduct a probit analysis, we also expanded the data set for a more detailed survival analysis. Our data for the survival analysis include annual data starting with the year 1977 and extending to the year 1985 for 724 movie theaters from the twenty-two largest cities in Ohio (204), Michigan (135), Pennsylvania (178), and New York (207).<sup>16</sup> We recorded all theaters listed in the local newspaper for each city every year from 1977 to 1985. Each year was a snapshot of the market on the final Friday before the 4<sup>th</sup> of July, one of the peak periods in the year.

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<sup>16</sup> Akron, Cincinnati, Cleveland, Columbus, Dayton, Toledo, and Youngstown, Ohio; Detroit, Grand Rapids, and Lansing, Michigan; Erie, Harrisburg, Philadelphia, Pittsburgh, and Scranton, Pennsylvania; and Bronx, Brooklyn, Buffalo, Manhattan, Queens, Staten Island, and Rochester, New York.

We used several other references to fill gaps in the data. The IMPA is an annual publication that listed large, medium, and small exhibition chains. However, it did not provide a complete listing of all theater chains. In 1975, the almanac accounted for about 53 percent of total screens. The remaining information came from *Variety*, two internet sources (cinematreasures.org and cinematour.com), and *Motor City Marquees*, a thorough reference about theaters in the Detroit Area from 1906-1992. We also contacted several theater owners from the blind bidding years for additional clarifications: Joel Resnick, former President of NATO; Bruce Olson, Senior Vice President of Marcus Theaters; Dick Fox, former President of Fox Theaters of Reading, Pennsylvania; and Jerome Gordon, President of Mid-Atlantic NATO.

### **3.2.2. Probit Results**

While the laws for the three states in our first sample did not have a statistically significant effect on the exit rate, the laws in Ohio and Pennsylvania do. Table 4 shows that after passage of the laws, *ceteris paribus*, theater owners in Ohio and Pennsylvania were more likely to stay in business longer than those in the other states. Interestingly, the coefficient for the number of screens per theater is essentially

identical to that for the first sample, while as before none of the other control variables had a statistically significant effect on the exit rate.<sup>17</sup>

### 3.3 Survival Analysis

While a probit analysis provides an approximation of the effect of the laws over an extended period of time, a theater's propensity to survive may change as time passes. Survival analysis permits the use of time-varying covariates, while the probit analysis does not. The hazard function,  $h(t)$ , is defined as

$$h(t) = \lim_{\Delta t \rightarrow 0} \frac{\Pr\{t \leq T < t + \Delta t \mid T \geq t\}}{\Delta t} \quad (1)$$

The numerator represents the probability a theater survives between  $t$  and  $\Delta t$ , given that it has survived up to that point in time. The inclusion of the conditional statement is necessary, because some theaters may have already exited the market by period  $t$ . Since the numerator alone is a non-decreasing function of  $t$ , it is divided by  $\Delta t$ . In addition, the hazard includes the limit of  $\Delta t$  representing increasingly smaller intervals of  $t$  and  $\Delta t$  until a limiting value is reached.

In this model  $T_i$  is a random variable representing time for the  $i$ th individual so that

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<sup>17</sup> Re-estimation of the model after dropping all variables with statistically insignificant coefficients did not change the results. We found this same result for all the remaining estimations reported in this paper.

$$\log T_i = \beta_0 + \beta_1 x_{i1} + \dots + \beta_k x_{ik} + \sigma \varepsilon_i, \quad (3)$$

where  $x_{ik}$  are predictor variables,  $\varepsilon_i$  is a random error term, and  $\beta_0, \dots, \beta_k$  and  $\sigma$  are parameters to be estimated. We assume that  $\varepsilon_i$  follows a Weibull distribution.<sup>18</sup> The Weibull model assumes  $\varepsilon_i$  follows the extreme value distribution with the probability density function,  $f(\varepsilon_i) = \exp\{\varepsilon_i - \exp(\varepsilon_i)\}$ . For  $\sigma$  equal to 1, the Weibull model simplifies to the exponential function which has a constant hazard function. For  $\sigma$  greater than 1, the hazard decreases over time. When  $0.5 < \sigma < 1$ , the hazard increases at a decreasing rate. When  $0 < \sigma < 0.5$ , the hazard increases at an increasing rate. When  $\sigma$  equals 0.5, the hazard function is an increasing straight line from the origin.

### 3.3.1 Explanatory Variables

In addition to controlling for the number of screens per theater, we also include controls for prior entry and exit of competitors, and industry level measures of the number of new releases and total tickets sold.<sup>19</sup> Numerous empirical studies find that

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<sup>18</sup> For a detailed discussion of the properties and applicability of this model, see Cameron and Trivedi (2005). Parametric survival models are common in studies of firm survival. For example, McCloughan and Stone (1998) used this model to investigate the shape of the hazard function for foreign manufacturing plants and to determine whether greenfield investments survive longer than acquisition plants. In addition, Chen (2002) utilized a Weibull model to examine the lifetimes of petroleum refining plants after the deregulation of crude oil markets.

<sup>19</sup> As previously, we also estimated the model using a quadratic term for screens per theater with no change in the coefficients of the other variables. An increase in the

entry and exit rates are positively correlated phenomena: higher (lower) entry rate industries are also higher (lower) exit rate industries (Dunne, Roberts, and Samuelson, 1988). As a result, we use the gross entry rate in the local market as a control variable. We define the gross entry rate as the number of new theaters divided by the total number of theaters in the current year. Since the dependent variable is time, higher entry rates imply shorter survival times for exiting theaters, *ceteris paribus*.

Through the early 1980's theater owners frequently complained about the shortage of films. We expect more releases in a given year to extend how long a theater owner will survive. However, the movie studios' focus on large-budget films in the early 1970's also affected theater attendance. We include the aggregate number of tickets sold per year as a covariate and expect that stronger demand correlates with theater owners remaining in operation longer.

As before, we include a dichotomous variable to capture the effect of the laws. However, a dichotomous variable is only one way for capturing the effect of the law in a duration model. It is possible that the relationship between a theater owners' survival and the laws may be more complex. For this reason, we also test for the length of the impact of the law by interacting this variable with a dichotomous variable for the years 1982 to 1985.

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number of screens per theater increases the probability of survival at a decreasing rate as the number of screens increases.



We measure the number of tickets sold and the entry rate as a one-period lag and the number of new releases as a one-period lead. We assume theater owners used the previous year's box office success as an indication of how successful the present year would be. However, theater owners are generally aware in time  $t$  of the number of new releases planned for time  $t + 1$ .

### **3.3.2. Results**

Our estimates of the parameters of the duration model for Ohio, Michigan, Pennsylvania and New York are shown in Table 5. Since the laws in Ohio and Pennsylvania were passed a year and a half apart, we also present results separately for these two states in comparison to both New York and Michigan both with and without the interaction term controlling for the longer term effect of the laws.

The results are similar in all cases. The coefficient for the number of screens per theater is positive and statistically significant, indicating that a theater with more screens stayed in business longer. The coefficients for the number of new releases and number of tickets sold are positive and statistically significant. We find that the coefficient for local rate of entry is not statistically different from zero.

As was the case in the probit analysis, the laws result in longer survival times when measured only by the dichotomous variable. Controlling for the possibility that the impact varied over time, we find that the impact of the laws was significantly and positively correlated with survival times from 1978 to 1981. However, theaters left the market at a faster rate in these states starting in 1982. As a result, while the laws initially encouraged some theater owners to stay in business longer, theater owners eventually realized that the law was not going to sustain them. As in the case of the probit results, theater owners that survived had adjusted by increasing the number of screens per theater.

The estimated Weibull parameter,  $\sigma$ , ranges between 0.25 and 0.33 and is significantly greater than zero but less than one. Since the null hypothesis that  $\sigma$  equals 1 is rejected, the theater data are not consistent with an exponential model. More specifically, the function follows the pattern of positive duration dependence: the hazard rate of exit remains lower for earlier years but it increases at an increasing rate in later years.<sup>20</sup>

### **3.4 Summary of Results**

Overall, we find that the laws banning only blind bidding had no effect on the likelihood of a theater owner staying in business, while the stricter laws that banned

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<sup>20</sup> We also estimated a Cox Proportional Hazards model with the same variables as a robustness check with essentially identical results (that is, only changes in the third significant digit).

both blind bidding and obligatory guarantees had a limited and short-lived effect that was soon reversed. In our samples we found that the one consistent factor in theater survival was the number of screens at the theater. We also found that there were forces that resulted in a rising probability that traditional theaters would exit the market. As a result, even though the strictest laws delayed the switchover to multiplexes for a short time, the inevitable structural change in the exhibition market soon offset the impact of these laws.

#### **4. ADMISSION PRICES**

While the main focus of our empirical work is the impact of the laws on the survival of theaters, we also investigate the opposing claims made by theater owners and studios concerning the impact of the laws banning bidding laws on admission prices. As noted earlier, the studios and distributors argued that ticket prices were lower in states allowing blind bidding, while theater owners argued that prices were higher. We test these claims using data from the two trade-screen states of Ohio and Pennsylvania. We use a natural experiment of average admission prices in Cleveland, Philadelphia and Pittsburgh before and after the passage of the laws compared to those in the comparably sized city of Detroit and find evidence that the ticket prices were somewhat lower after passage of the laws in states allowing blind bidding. We provide a possible explanation for why one may not be surprised by this result. Data come from the weekly survey in *Variety* of the average ticket prices for specific movies in

the relatively larger market cities. These cities were chosen primarily because of their consistent listing in *Variety's* survey of large metropolitan markets.

#### **4.1. Background**

There is no consistent empirical evidence in the literature on the impact of blind bidding on ticket prices. For example, the impact of blind bidding on ticket prices was not a part of Blumenthal's study, since she only focused on guarantees. DeVany and Eckert suggest that contractual arrangements focused on the guarantee and the sharing rule did not differ across trade-screen and blind-bid states. Given that the sharing rule is more closely associated with marginal cost and guarantees, when binding, are fixed costs, there should be no difference in ticket prices across blind-bid and trade-screen states.

In addition, a recent literature documents the general industry practice of constant admission prices across motion pictures at a given theater and over time for a specific motion picture regardless of demand (Filson et al. and Orbach and Einav, 2007). There appears, however, at least more recently to be some variation in prices due to the degree of market competition. For example, using data from 1993 to 1997, Davis finds that admission prices are slightly lower in more competitive markets. While there is price discrimination across consumer groups (adults, children under twelve years of age, seniors over sixty years of age, and veterans), there appears to be

little use of price discrimination by time of day or day of the week until more recently (Davis, 2005).

While there is no empirical evidence in the literature that addresses the impact of blind bidding on ticket prices, theater owners argued that blind bidding would result in higher prices. For example, A. Alan Friedberg, the NATO president, asserted that losses associated with overbidding on poor quality movies due to blind bidding added about 10 percent to film rentals each year, and without it, that savings could be passed along to the general public (Jacobson, 1979). It is not clear, however, given a mutually beneficial goal of revenue maximization by both the distributors and exhibitors in both trade-screen and blind-bid states why theater owners have an incentive to seek a different price schedule in blind-bid states following an unexpected series of losses on earlier movies.

On the other hand, while the distributors' main marginal cost per film is due to advertising expenditures, they apparently incurred additional expenses in trade-screened states due to the costs of sales prints especially made for the purpose of trade screening. According to Barry Reardon, distributional president at Warner Brothers, the additional expense to trade screen amounted to approximately \$50,000 per film (Robbins, 1985, p. 80). However, given that this additional cost is a fixed cost and the distributors' desire to maximize revenue for any given movie; it is also not clear why this would lead to a higher price in only trade-screen states.

Taken as a whole, it is therefore not clear from these arguments why ticket prices would vary across blind-bid and trade-screen states. However, admission pricing in the movie industry appears to be at odds with the predictions of traditional

pricing models. For example, theater owners and distributors have potentially conflicting incentives concerning admission prices. While they both share box office receipts, theater owners keep all of concessions sales. As a result, the impact of a price decrease on a theater owner's marginal revenue may be more favorable relative to that of the distributor, at least at times of excess theater capacity. While theater owners could have specified lower price schedules in their bids to the distributors, they might have lost the right to show the movie given the available information about its prospects at the time. As a result, prices were generally determined a priori implicitly by the distributors in order to maximize sales revenue with the most significant variation across contracts coming from the guarantee (Filson et al).<sup>21</sup>

As mentioned earlier, while Blumenthal did not consider the admission price schedule as a part of the bids, the lower winning guarantees and greater variance in box office revenues did imply the possibility of informational value of trade screening. To the extent there was greater uncertainty about movie quality in blind bid states at least as it pertains to the optimal price schedule, it would not be surprising to find relatively more downward flexibility in pricing schedules in these states at least ex post. For example, Filson et al. state that contractual terms pertaining to the price schedule, along with requirements concerning number of shows per week and theater size, were suggestive and rarely enforced. (p. 356). As a result, while we see no reason to expect ticket prices to be higher in states allowing blind bidding, we cannot rule out the possibility that they could be lower.

## **4.2. Data and Methods**

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<sup>21</sup> Recently Moul (2007) uses a structural model to measure the degree of collusion among distributors. In order to estimate his model he assumes that admission prices are exogenous to both distributors and exhibitors on a weekly basis.

We obtained the data from *Variety*, which sampled between ten and twenty theaters weekly from fifteen cities in the U.S. We compare prices in the three cities of Cleveland, Philadelphia and Pittsburgh with those of Detroit. We sampled each city quarterly. Each sample included cities in the entire metropolitan area. For example, Detroit included theaters from surrounding areas of Wayne, Oakland, and Macomb counties.

Since theater owners bid on films six months to one year in advance of the release date, we cannot be completely sure which films were blind bid during the first year that the anti-blind bidding laws were in effect. For example, Ohio enacted the law in October 1978, but theater owners may have bid for films to be released in April 1979 or as far away as October 1979. We address the possible lagged effect of the law on films by examining admission prices using two different time periods: (1) two years before and after a law, and (2) three years before and after a law. As before, we consider the passage of the Ohio and Pennsylvania laws a natural experiment and compare the change in the population means. We assume that relative changes in price approximate the effect of the law.<sup>22</sup>

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<sup>22</sup> We were unable to find suitable data on possible explanatory variables, such as proxies for consumer preferences, for a detailed panel study of the price changes. However, given the similarity of the states and the short time period, we are not optimistic that there would be sufficient variation over time studied to affect our results.

Figure 4 displays average admission prices for Cleveland and Detroit from 1975 to 1981. Detroit's average prices remain consistently above Cleveland's throughout. Average admission prices for Cleveland and Detroit remain relatively steady before the implementation of the law, implying the assumption of a common trend appears valid. Table 6 shows that Detroit's prices increase by seven cents and Cleveland's rise by 16 cents after passage of the law. Since the seven cent increase in average prices is assumed to control for how Cleveland's prices would have changed in the absence of the law, the Ohio law significantly increases Cleveland's average prices by nine cents. Cleveland's relative average prices are lower by only one cent, however, when we compare prices starting three years prior to the passage of the law.

Figure 5 shows average prices in Philadelphia and Pittsburgh versus those in Detroit from 1977 to 1983. For the first two years, prices are nearly identical. In 1979 and 1980, the difference in average prices remains relatively steady at 10 and 15 cents, respectively. Beyond 1980, the difference in average prices increases, ranging from 36 to 41 cents. Table 7 shows that average prices for Philadelphia and Pittsburgh rise after the passage of Pennsylvania's law by 43 cents while Detroit's increases by 11 cents for a statistically significant 32-cent increase in admission prices. When we extend the time period to a total of six years, the law causes higher average admission prices for Philadelphia and Pittsburgh by 53 cents.



## 5. DELAYS

For completeness we also examined the claim made by movie studios that the laws would significantly delay the release of films. The studios warned of delays of three to six months, while even some theater owners expected delays of at most one month (Brill, 1978). We add to our investigation of the impact of the anti-blind bidding laws of Ohio and Pennsylvania by considering the possibility of delays in these two states. Because of consistent inclusion in *Variety* of the largest metropolitan markets in these two states, we paired Cleveland with Detroit and Philadelphia with New York City. We do not find evidence that the two trade-screen cities of Cleveland and Philadelphia exhibit films relatively later after the passage of the laws.

Despite the studios' statements to the contrary, a lack of significant systematic delays in trade screen states should not be surprising. To this day release dates for large budget films follow strong seasonal trends. In addition, Einav (2007) has shown recently that ticket price rigidity has created an amplification effect making the seasonal release pattern even more pronounced. Given the relatively small number of films during the time period of our study and the potentially large loss of revenues from delays during the critical peak demand seasons, studios had a strong incentive to release films in a timely manner in all states. As a result, we did not expect to find systematic differences in release dates between blind-bid and trade-screen states.

## 5.1. Data and Methods

We were unable to determine the full list of movies blind bid during this time period. As a result, we selected our data set to minimize the possibility of including those movies not blind-bid, while also assuring a large enough sample. There is at least anecdotal evidence that the highest grossing movies were blind bid, while the worst were trade screened (Bratman, 1980). As a result, we used data from *Variety* which reported the number of weeks a film was exhibited in a city for the top twenty films in each year from 1975 to 1985. *Variety* sampled theaters from downtown areas and surrounding suburban areas in the cities of Cleveland, Detroit, and Philadelphia. For New York City, *Variety* consistently sampled theaters in Manhattan.<sup>23</sup>

## 5.2 Results

Table 8 summarizes the impact of Ohio's law on the relative release dates in Cleveland and Detroit. Before passage of the Ohio law, Cleveland exhibited 24 of the 99 films later than Detroit with an average delay of 3.75 weeks. After passage of the law, only eight films were delayed with the average delay of 1.75 weeks. In addition, before passage of the law Cleveland exhibited eight films before Detroit did and 67 films were at the same time. After passage of the law, 60 films were exhibited first in

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<sup>23</sup>In some cases, we were unable to obtain information for all of the top twenty films each year. For example, *Benji* (1974) was the third highest grossing film in 1975, but *Variety* did not furnish information about its film release pattern for Cleveland, Detroit, Philadelphia, and New York City. In circumstances like this, we included the next highest grossing film.

Cleveland, and 53 were released at the same time. The overall average number of weeks of delay was only 0.81 before passage of the law and -0.44 after it. Therefore, after passage of the Ohio law, films were even exhibited *sooner* on average in Cleveland than in Detroit.

Table 9 summarizes similar results for the impact of the Pennsylvania law. Philadelphia received films after New York City 47 times with the average delay of 2.78 weeks prior to the passage of the law. Afterwards there were only six delays with an average length of 3.5 weeks. Prior to passage of the law, there were two instances when Philadelphia exhibited a film before New York City and 77 times films were exhibited at the same time. After passage of the law, Philadelphia exhibited a film before New York City one time, and 87 times films had the same release date. For all films the average weeks of delay before the passage of the law were 0.52 weeks and 0.36 afterwards.

## **6. CONCLUSION**

Laws banning blind bidding and guarantees were intended to help theater owners remain in business. However, the decline of one and two-screen theaters was a trend that the laws could not reverse. While all theater owners were initially affected by the scarcity of new releases and the concentration of blockbuster films into a few peak periods, the industry was experiencing important structural changes during the time when the laws were being passed.

The blockbuster films increased aggregate demand. In addition, studios changed their philosophy on the release pattern of films. Prior to the passage of the laws, films followed a slow release pattern where only a handful of theaters exhibited a new release in a limited number of cities during their first-run. However, over time studios changed to a fast release pattern, making first-run films more widely available. Through the development of the ancillary markets, cable television and VCRs, the decades long product shortage came to an end. By the early 1980's new releases were consistently double their previous levels of the early blind bidding years.

Theater owners that remained competitive restructured by converting their theaters to the multiplex concept, which increased their likelihood of exhibiting blockbuster films and diversified risk. The econometric results that we present in this paper suggest that even the strictest state laws had at most a limited and short-lived impact on theater survival. In addition, we find evidence that admission prices for the relatively more successful films were actually somewhat higher in trade-screen states.

Our results indicate that blind bidding helped those theater owners who had diversified, because they benefited from paying lower guarantees on average. In other words, trade screening may have actually resulted in higher bid prices for the most promising films that these theater owners would have shown anyways. As a result, the laws were an unnecessary attempt to protect less efficient theaters from going out of business while penalizing those theater owners who had modernized by improving the quality of their theaters.

Taken as a whole, our results have implications for the literature on market concentration, competition, and endogenous investment in quality. For example, U.S.

Justice Department's Assistant Attorney Shenefield in his letter to the Massachusetts State Legislature stated a common concern about the changes in market structure at the time, "It is not surprising that ...only the large chains are still operating profitably. Blind bidding appears to contribute to this trend toward concentration of the exhibition market" However, Sutton (1991) has shown that industries can remain both competitive and highly concentrated as market size increases over time as long as the increase in firm size is due to endogenous investment in quality.<sup>24</sup> More recently, Ellickson (2007) in an empirical study of supermarket quality using data for the 1990s shows that consumers experienced a higher level of quality as supermarkets offered a wider selection of products in larger stores, while local markets remained competitive despite increasing concentration levels.

As in the case of the movie industry, some states initially attempted to impede endogenous quality-enhancing investment in supermarket quality. Using data from 1974 to 1985, Das, Falaris, and Mulligan (2009) show that as supermarkets made endogenous quality-enhancing investments in the first vintages of optical scanner technology in the late 1970s and early 1980s, six states passed laws requiring that prices be placed on each individual item sold in grocery stores. While these laws were intended to save jobs, they slowed down the diffusion of scanner technology at a time when stores were starting to install computers linked to the scanner technology.

Levy et al. (1997) provide additional evidence of the potential harmful effects of the laws by comparing the costs of changing prices at supermarkets located in Connecticut with those in states without such a law using data for the years 1991 and 1992. They determined that stores subject to item pricing laws at that time faced costs that were higher than those in states not subject to the law. Using data for 1991, Bergen et al. (2004) provide evidence that consumer prices were, on average, ten percent higher in states with these laws.

As in the grocery market, the exhibition sector of the motion picture industry in local markets became more concentrated when theater owners began investing endogenously in quality (that is, number of screens per theater). Despite the increased level of concentration, our results imply that moviegoers were actually better off in terms of quality due to a wider selection of films in one location. These results should give policy makers second thoughts prior to intervening to prevent bankruptcy in markets undergoing important structural change.

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<sup>24</sup> Our results are also relevant to a growing related literature on how quality and product differentiation decisions can affect exit and entry (Toivanen and Waterson, 2005).

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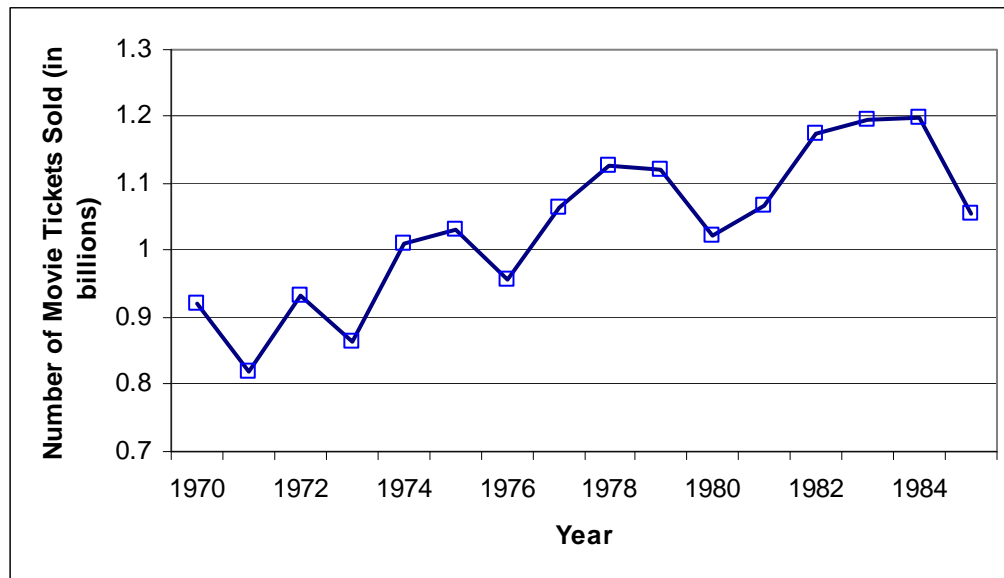
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**Figure 1**

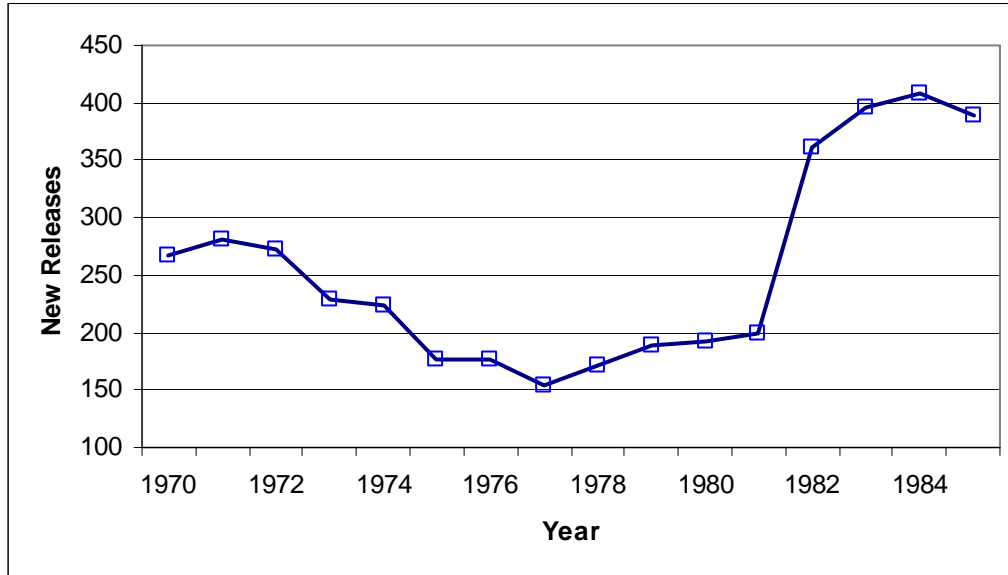
**Number of Movie Tickets Sold, 1970-1985**



Source: Encyclopedia of Exhibition (National Association of Theater Owners: annual)

**Figure 2**

**Number of New Releases from U.S. Movie Studios, 1970-1985**



Source: Encyclopedia of Exhibition (National Association of Theater Owners: annual)

**Table 1**

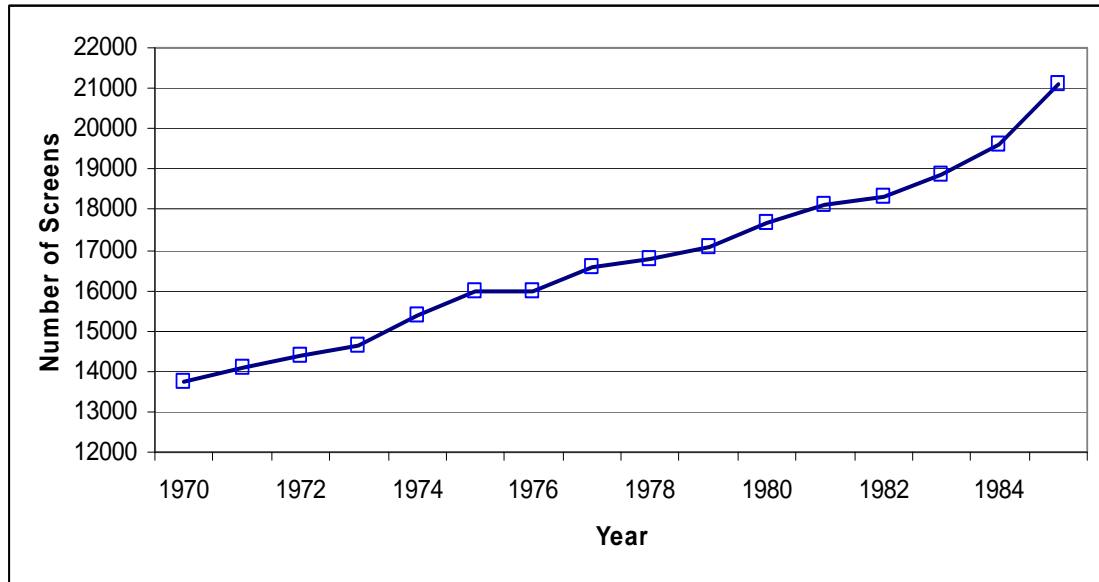
**States that Enacted Laws Banning Blind Bidding**

State	Year Enacted	Guarantees Permitted
Alabama	1978	Yes
Arkansas	1981	Yes
Georgia	1979	Yes
Idaho	1979	No
Indiana	1980	Yes
Kansas	1981	Yes
Kentucky	1980	No
Louisiana	1978	Yes
Maine	1979	Yes
Massachusetts	1979	Yes
Missouri	1982	Yes
Montana	1981	No
New Mexico	1979	Yes
North Carolina	1979	Yes
Ohio	1978	No
Oregon	1979	Yes
Pennsylvania	1980	No
South Carolina	1978	Yes
Tennessee	1979	Yes
Utah	1979	No
Virginia	1978	Yes
Washington	1979	Yes
West Virginia	1979	Yes
Wisconsin	1984	No

Source: Encyclopedia of Exhibition (National Association of Theater Owners: annual)

**Figure 3**

**Aggregate Number of U.S. Theater Screens from 1970 to 1985**



**Table 2**

**Theater Exit Rates for Pennsylvania, Ohio, New York, and Michigan**

Period 1973 to 1976 (Pre-blind bidding laws)						
	blind (MI,NY)	preview (OH,PA)	OH	MI	PA	NY
exit rate	19.59%	17.80%	16.67%	18.52%	19.10%	20.29%

Period: 1977 to 1985						
	blind (MI,NY)	preview (OH,PA)	OH	MI	PA	NY
exit rate	57.49%	52.72%	58.37%	50.00%	46.53%	62.19%

Period: 1977 to 1981						
	blind (MI,NY)	preview (OH,PA)	OH	MI	PA	NY
exit rate	32.42%	23.40%	26.24%	24.60%	20.30%	37.31%

**Table 3****Probit Estimates of the Probability of Exit Between 1977 and 1981  
(Florida, Georgia, North Carolina and Tennessee)**

Variable	Coefficient (standard errors) [marginal effects]	Mean Standard deviation Minimum Maximum
Constant Term	0.23 (0.24) [0.08]	
Number of Screens	-0.43* (0.10) [-0.15]	2.03 1.26 1.0 9.0
Herfindahl-Hirschman Index	-0.13 E-4 (0.10 E-3) [-0.48 E-5]	1484.7 925.5 724.0 5612.2
Number of Screens a Theater Owner Operates	0.31 E-3 (0.10 E-3) [0.11 E-3]	157.8 169.9 1.0 25
Preview State	0.27 (0.19) [0.09]	0.48 0.50 0.0 1.0
Log L	-133.41	
Cases	228	

\* Significant at the 5 percent level.

\*\* Significant at the 10 percent level.



**Table 4****Probit Estimates of the Probability of Exit between 1977 and 1981  
(Michigan, New York, Ohio, and Pennsylvania)**

Variable	Coefficient (standard errors) [marginal effects]	Mean Standard deviation Minimum Maximum
Constant Term	0.22 (0.14) [0.07]	
Number of Screens	-0.43* (0.07) [-0.13]	1.54 0.99 1.00 8.00
Herfindahl-Hirschman Index	-0.20 E-4 (0.53 E-4) [-0.66 E-5]	1929.1 1081.4 895.6 5410.2
Number of Screens a Theater Owner Operates	-0.67 E-3 (0.42 E-3) [-0.21 E-3]	93.3 140.7 1.0 525
Preview State	-0.23* (0.11) [-0.07]	0.56 0.49 0.00 1.00
Log L	-411.46	
Cases	750	

\* Significant at the 5 percent level.

\*\* Significant at the 10 percent level.

**Table 5**

**Duration Model Estimates of Movie Theater Survival  
(Ohio, Michigan, Pennsylvania and New York)  
1977-1985**

	Ohio, Michigan, and New York		Pennsylvania, Michigan, and New York		All Four States
Constant	-3.0* (0.30)	-3.7* (0.25)	-3.1* (0.27)	-3.5* (0.25)	-3.9* (0.19)
Number of Screens	0.17* (0.027)	0.15* (0.023)	0.19* (0.027)	0.17* (0.025)	0.15* (0.019)
Entry Rate	-0.32 (0.35)	-0.46 (0.31)	0.15 (0.35)	0.18 (0.32)	-0.27 (0.25)
Number of New Releases	0.0029* (0.0002)	0.0038* (0.0002)	0.0030* (0.0002)	0.0034* (0.0002)	0.0038* (0.0001)
Number of Tickets Sold	3.47* (0.29)	3.94* (0.25)	3.49* (0.26)	3.81* (0.24)	4.12* (0.19)
Preview State	0.11* (0.045)	0.2* (0.051)	0.15* (0.05)	0.25* (0.057)	0.25* (0.036)
Preview x Years 1982-1985		-0.49* (0.072)		-0.29* (0.072)	-0.40* (0.048)
Sigma	0.33* (0.032)	0.29* (0.026)	0.29* (0.025)	0.26* (0.023)	0.26* (0.019)
Log L	-975	-951	-877	-870	-1254
Cases	3453	3453	3447	3447	4860

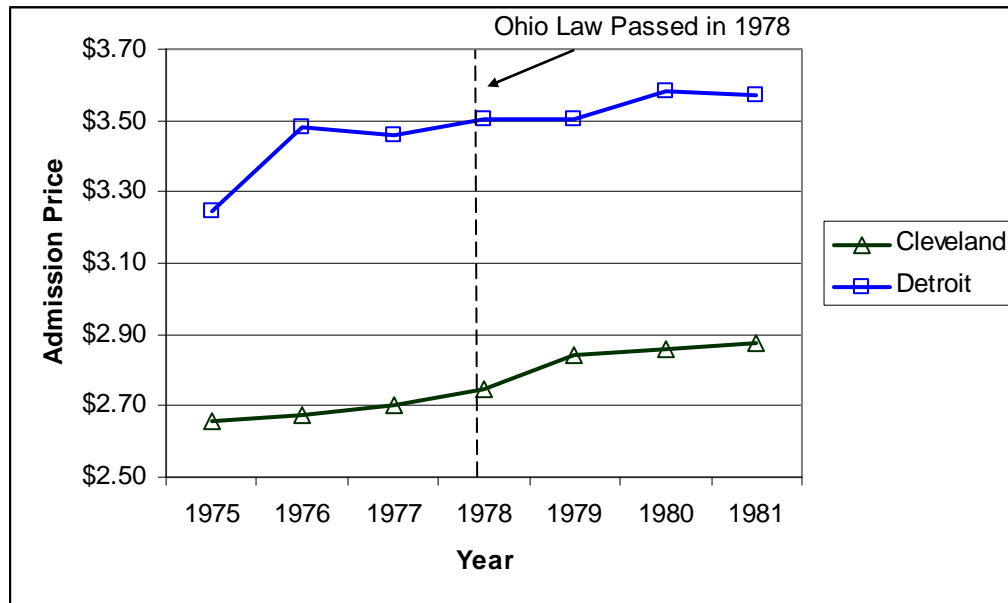
Standard Errors are in parentheses.

\* Significant at the 5 percent level.

\*\* Significant at the 10 percent level.

**Figure 4**

**Average Movie Theater Admission Prices, Cleveland and Detroit  
1975-1981**



**Table 6****Average Admission Price Before and After Passage of the Ohio Law**

Variable	Cleveland (1)	Detroit (2)	(1) – (2)
Mean Price before (1976-77)	2.69 (0.364)	3.47 (0.187)	
Mean Price after (1979-80)	2.85 (0.451)	3.54 (0.101)	
Change in Mean Price	0.16* (0.048)	0.07* (0.019)	0.09* (0.004)
Mean Price before (1975-76)	2.67 (0.345)	3.36 (0.251)	
Mean Price after (1980-81)	2.87 (0.428)	3.57 (0.330)	
Change in Mean Price	0.20* (0.046)	0.21* (0.040)	-0.01** (0.005)

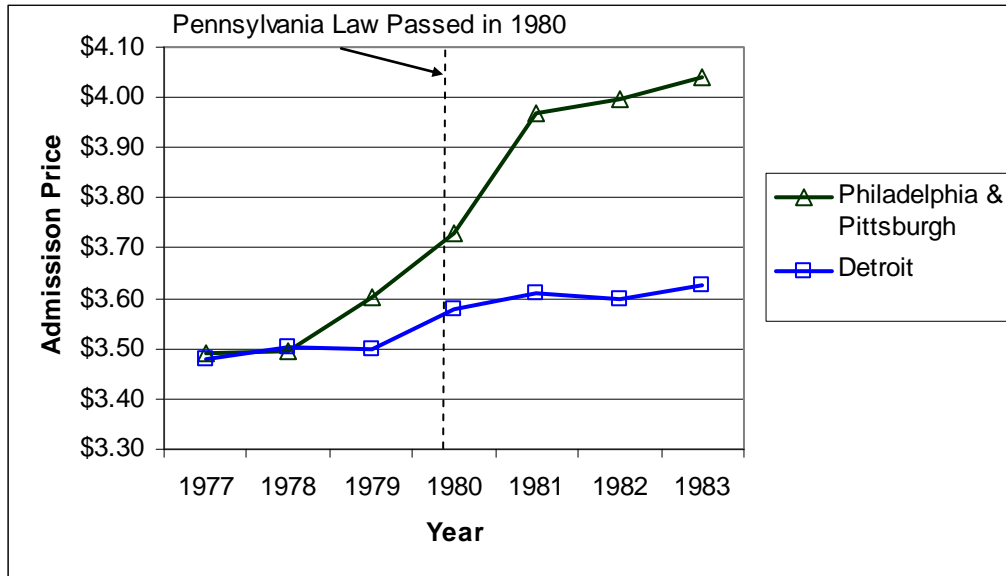
Standard Errors are in parentheses.

\* Significant at the 5 percent level.

\*\* Significant at the 10 percent level

**Figure 5**

**Average Movie Theater Admission Prices, Philadelphia & Pittsburgh and Detroit, 1978-1983**



**Table 7****Average Admission Price  
Before and After Passage of the Pennsylvania Law**

Variable	Philadelphia and Pittsburgh (1)	Detroit (2)	(1) – (2)
Mean Price before (1978-79)	3.55 (0.109)	3.50 (0.024)	
Mean Price after (1981-82)	3.98 (0.073)	3.61 (0.202)	
Change in Mean Price	0.43* (0.008)	0.11* (0.021)	0.32* (0.002)
Mean Price before (1977-78)	3.49 (0.068)	3.48 (0.171)	
Mean Price after (1982-1983)	4.02 (0.092)	3.61 (0.152)	
Change in Mean Price	0.53* (0.008)	0.13* (0.023)	0.53* (0.002)

Standard Errors are in parentheses.

\* Significant at the 5 percent level.

\*\* Significant at the 10 percent level

**Table 8**

**Average Relative Delays in Release Dates in Cleveland Prior to and After Passage of the Ohio Law**

	Movies first released in Detroit  (number of movies and average relative delay in weeks)	Movies first released in Cleveland  (number of movies and average relative delay in weeks)	Same release date  (number of movies)	All movies  (number of movies and average relative delay in weeks)
Prior to passage of the law in Ohio	24 3.75	7 -1.29	68	99 0.81
After passage of the law	8 1.75	60 -1.12	53	121 -0.44

**Table 9****Average Relative Delays in Release Dates in Philadelphia Prior to and After Passage of the Pennsylvania Law**

	Movies first released in Manhattan  (number of movies and average relative delay in weeks)	Movies first released in Philadelphia  (number of movies and average relative delay in weeks)	Same release date  (number of movies)	All movies  (number of movies and average relative delay in weeks)
Prior to passage of the law in Pennsylvania	47 2.79	2 -1.00	77	126 1.03
After passage of the law	6 3.50	1 -2.00	87	94 0.20