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The Essays on Unintended Consequences of Public Policy

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ABSTRACT

This dissertation studies three examples of public policies having consequences other than those intended when the policy was passed. They demonstrate that due to the interconnectedness of the economy, the intended effect of a policy is rarely the sole effect.

The first essay examines the Texas Top 10% Plan. This policy guarantees automatic admission to their state university of choice for all high school seniors who graduate in the top 10% of their high school class. The essay shows evidence that households reacted strategically to this policy by moving to neighborhoods with lower-performing schools, increasing property values by 4.9 percent in those areas relative to areas with slightly better performing schools. The effect is strongest among schools that were the lowest performing before the change in policy; and weakens as the previous performance of the school district increases. These strategic reactions were influenced by the number of local schooling options available: areas that had fewer school choices showed no reaction to the Top 10% Plan.

The second essay examines individual differences in the effects of medical malpractice tort reforms on pre-trial settlement speed and settlement amounts by age and likely settlement size. I focus on changes in the value of settlements for those trying to receive quick compensation – an understudied but very important population. Findings of note include that, unlike previously assumed, losses from tort reform among infants are small in an asset value sense and that the prime-aged working population that are the most negatively affected by tort reform, losing over 50 percent of the value of their mean settlements post reform. Maximum entropy quantile results show that the median expected

settlement losses are often the most informative for policy evaluation and differ greatly from mean policy effects.

The third essay uses the implementation of medical malpractice damage caps in several states, and a panel of private insurance claims to identify the effect of damage caps on the amount physicians charge to insurance companies and the amount that insurance companies reimburse physicians for medical services. In most cases the amount that physicians charge insurers does not change, but the amount that insurers reimburse physicians (which is the price seen in the market) decreases. I estimate price reductions as large as 14.5 percent for specific procedures.

THREE ESSAYS ON UNINTENDED CONSEQUENCES OF PUBLIC POLICY

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DISSERTATION

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

1. Unintended Consequences of Public Policy

The formation of public policy through legislation is a difficult process. Not only do policy makers need to come to agreements with a large number of actors who have power in the legislative arena, but understanding the exact effects of a given policy is a complicated task. Legislation that is aimed at addressing one issue in society may have effects elsewhere that dampen or even reverse the gains the policy sought to acquire in the first place. Markets are connected in an incredibly complicated web, and an intervention in one market can create incentives in a different market, changing the economic landscape in powerful ways. Often these unintended consequences are not even considered a possibility at the time the legislation is written. The possibility of unintended consequences makes careful and thorough cost benefit analysis a must in any public policy intervention. But, even the most careful analyst is unlikely to think of every possible incentive that could emanate from a law, so it is also of the utmost importance to analyze policies in retrospect, so that the impact of unforeseen consequences can be factored into future analysis.

The identification of unintended effects of policy is a classic topic in economics and one that is at the center of this body of work. I use the term "unintended" as a catch all for policies that have unforeseen impacts, as well as for policies that impact underappreciated populations or that have understudied results. The three essays presented here are examples that help illustrate specific instances of policies carrying effects that are further in scope than what was originally intended by the legislation. Though the essays range in the nature of the policies, they all point to the same conclusion: legislative intervention in the market is a blunt instrument, and one that must be handled with care.

The first essay examines an instance where a law aimed at correcting an inequality in the market for higher education creates a price distortion in the housing market. In 1996, the *Hopwood v. University of Texas Law School* case judicially banned Texas from using race as a criterion in admissions decisions. Once this ruling came into effect, minority enrollment at Texas state universities plummeted. In an attempt to grant increased access to universities to minorities Texas passed what came to be known as the Texas Top 10% Plan, which guaranteed automatic admission to any state university of choice to high school seniors who placed in the top 10% of their graduating high school class. The automatic admission guarantee included the two most competitive schools in the system, University of Texas at Austin, and Texas A&M. The efficacy of the Top 10% Plan depended on the segregated nature of Texas School districts: though explicit segregation was and still is illegal, many districts had largely minority populations. Granting instant access to universities to the top 10% of all districts would give greater access to minorities without explicitly using affirmative action in admissions, which the *Hopwood* case ruled to be illegal.

Policy makers did not consider that students and their parents could act strategically in response to the Top 10% Percent Plan, specifically in their choice of location. If a student was close to the top decile of his class but did not have a strong enough application to get into a competitive school such as Texas A&M, then he would have a lot to gain by moving to a less competitive school district where his level of performance would place him in the top decile. The Top 10% Plan created an unintended amenity of increased access to top universities in poorly performing school districts, which changed the willingness to pay for, and thus price of housing in those school districts. Specifically, properties in the most poorly performing school districts grew on average by 4.9 percent relative to properties in the second most poorly performing districts. The change in prices in the housing market was far outside the intended scope of the Top 10% Plan, which was aimed at addressing admissions inequalities.

The second essay studies a group that suffers as a result of medical malpractice tort reform. Usually medical malpractice tort reforms are passed in response to what are termed malpractice crises, steep increases in the cost of malpractice insurance due to tightness in the medical malpractice insurance market. Tort reforms such as caps on damages and early offer rules are passed in attempts to alleviate pressure on physicians by limiting the size of large claims with blockbuster payouts. The decrease in claims severity (and often frequency) in turn lowers malpractice insurance premiums and helps to bring costs to physicians under control.

The medical malpractice tort system has many dimensions. Besides being a structure that incents doctors to practice with the appropriate amount of care through the negligence rule, it also provides patients with compensation for injuries. Medical malpractice torts serve as an insurance policy against negligent adverse events. Laws which are passed in an attempt to control costs for physicians also have the effect of lowering the value of these insurance policies. Those who are seeking to acquire quick compensation through speedy settlement, or who in other words are trying to collect their insurance policy in an expedient manner suffer a large percentage decrease in the value of their insurance.

On average, the value of an implicit insurance policy drops by 36 percent after the implementation of reforms. However, this value reduction is not uniform across age cohorts. For example, people who are in their prime working years take even larger reductions to the value of their implicit insurance, an average reduction of over 58 percent. Even more conservative estimates of the value of the implicit insurance at the median and maximum entropy quintile show reductions of over 25 percent. This consequence of medical malpractice reform is rarely discussed when medical malpractice laws are being evaluated.

The final essay looks at an unintended consequence of policy: the pass through of medical malpractice costs. As noted before, the malpractice system is meant to create incentives for physicians to practice appropriately through the negligence rule. It does this by putting costs on physicians via the risk of suit. But, if physicians can pass through the costs of malpractice risk to patients, then the system may not work very well at promoting the appropriate level of care.

Historically, cost pass through has been difficult to study due to a lack of data covering a large number of procedures over a sizable length of time. In the third essay I use one of the first such databases to study the effect of state caps on damages on the price of medical procedures. I am able to show sizable price reductions in response to damage caps (which lower costs on physicians), and present the first evidence of cost pass through of medical malpractice tort reforms. Caps on non-economic damages lower the prices of procedures performed by Obstetricians and Gynecologists (a high risk specialty) significantly. For example, the price of vaginal delivery drops by approximately 7 percent in the presence of a cap.

The most interesting part of the analysis in this essay may not be the evidence of price reductions, but the evidence on how the price reductions come about. Specifically, I show that price reductions in response to damage caps are mainly the result of private insurance companies lowering the amount that they are willing to reimburse providers, rather than the result of providers willingly lowering the amount that they charge insurers.

This understanding of how prices adjust in the market will provide valuable information to future policy makers when dealing with the issue of medical malpractice tort reform.

The three essays help illustrate the importance of unintended effects in policy making. The nature of the economy is complex, and no matter how finely tuned a policy the intended effect is rarely the sole effect. If the government is going to assert itself in the marketplace, then great care must be taken into understanding the full set of consequences to an action.

2. Ranking Up by Moving Out: The Effect of the Texas Top 10% Plan on Property Values

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2.1 Introduction

Texas engaged in an unforeseen large-scale experiment when it replaced the use of affirmative action policies in its college admissions with the Top 10% Plan admissions policy. The Top 10% Plan guarantees admission into any of Texas' public universities to all high school seniors who finish within the top decile of their graduating class. This includes the most selective state universities: The University of Texas at Austin and Texas A&M at College Station. For school districts that had poor acceptance rates to postsecondary institutions this admissions policy suddenly provided a valuable local amenity: improved access.

In this study, we analyze the effect of the Top 10% Plan on property values. More specifically, we analyze whether the change in admissions policies led to an increase of the value of residential homes in school districts with low-performing high schools relative to school districts with higher-performing high schools. School districts with low-performing high schools are expected to be the areas where property values are most responsive to the policy change because it is at these schools where *access* to selective public colleges was improved the most. We expect to find less reaction to the Top 10% Plan in areas with high-quality schools: because these high schools are more likely to place their top 10% of graduates in highly ranked postsecondary institutions, the Top 10% Plan would do much less to increase access.

Using a difference-in-differences methodology, we find that, as a consequence of the change in admissions policy, residential property values in the areas served by schools in the bottom quintile of school quality grew more rapidly relative to areas served by schools in the 2^{nd} quintile (second from the bottom). We also find that the bottom quintile grew relative to other quintiles in the school quality distribution, although the effect attenuates further away from the bottom of the distribution. We also compare the 4th quintile with the top quintile and find that the growth in home values did not occur in the top end of the school quality distribution.

Furthermore, we observe that changes in property values are sensitive to the number of schooling options locally available. If a household is going to react strategically to the Top 10% Plan by moving, then moves would be easier in areas with a large number of local schooling options (e.g., a shorter distance to find a new school would not require finding a new job). Specifically, counties with a relatively high Herfindahl-Hirschman Index (HHI) for schooling would show little to no reaction to the change in policy, whereas counties with a relatively low HHI for schooling would show the greatest reaction to the policy change. This is precisely the case: we find that the disproportionate growth of property values in the bottom quintile of school quality relative to the 2nd quintile did not occur in counties that were more monopolistic, but did occur in counties that were more competitive.

Lastly, our analysis estimates that the Top 10% Plan had a rate of return of 4.9 percent in relative average property value gains of the lowest quintile of school quality compared to the 2nd quintile of school quality. As property values vary greatly from district to district before the policy shift and property tax rates also vary greatly it is easy to see how the Top 10% Plan had a powerful impact not only on admissions decisions, but also on school finance and local taxation decisions.

2.2 Background of the Top 10% Plan and Literature Review

2.2.1. The Top 10% Plan

The 5th Circuit Court's decision in Hopwood v. University of Texas Law School judicially banned the use of race as a criterion in admissions decisions in all public postsecondary institutions in Texas.[1] The end of affirmative action admissions policies was overwhelmingly felt, especially at the two most selective public institutions, The University of Texas at Austin and Texas A&M University at College Station, where the number of minority enrollees plummeted (Tienda et al. 2003; Bucks 2004; Walker and Lavergne 2001). In response to this ruling, Texas passed the H.B.588 Law on May 20, 1997–more commonly known as the Top 10% Plan. The Top 10% Plan guarantees automatic admission to any public university of choice to all seniors who graduate in the top decile of their graduating high school class.[2,3] This is similar to other states' percent plans (e.g., California and Florida), but is unique in the sense that it gives students the choice of which public institution they would like to attend rather than assigning the institution outright.[4]

Proponents of the plan believed that it would restore campus diversity because of the high degree of segregation among high schools in Texas. Their logic was that the number of minority students who would be rank-eligible under the Top 10% Plan would be sufficient to restore campus diversity in the university system. Even though the goal of the Top 10% Plan was to improve access for disadvantaged and minority students, the use of a school-specific standard to determine eligibility may have led to some unintended effects if

[1] See *Hopwood v. University of Texas Law School* 78 F.3d 932, 944 (5th Cir. 1996).
[2] In 2009, Texas placed some limits on student choice: the University of Texas at Austin is now allowed to cut off the proportion of Top 10% Plan students in a given freshman class at 75 percent.
[3] Although private universities are duty-bound by the *Hopwood* ruling, they are not subject to the automatic admissions guarantee (Tienda et al. 2003).
[4] In both California and Florida students are accepted into the state university system by rank eligibility but are not given a choice of which institution they would like to attend.

households responded strategically. In a recent study, Cullen, Long, and Reback (2011) find that a large number of students increased their chances of being in the top 10% by choosing a high school with lower-achieving peers. They analyze student mobility patterns between the 8th and 10th grades before and after the policy change, and conclude that the change in admissions policies in Texas did indeed influence the high school choices of students. This evidence of students changing districts strategically goes a long way towards explaining the changes in enrollment probabilities for minority and non-minority students found in Tienda et al. (2003), Bucks (2003), Walker and Lavergne (2001), Niu et al. (2006), and Cortes (2010).

If households are moving strategically between schools then their valuation of those schools must have changed due to the policy. Our analysis pushes this idea further by looking for evidence of this change through households' maximum willingness to pay for housing services. This is reflected in changes in property values in school districts whose desirability changed when the Top 10% Plan was implemented.

2.2.2. Related Literature

The Top 10% Plan changed how much certain households are willing to pay for school district quality through their housing prices. This sort of reaction is best illustrated with bidding and sorting models, which are a part of the local public finance literature. This branch of the literature is widely seen as starting with Tiebout (1956) who put forth the idea that households shop for property tax and public service packages through their choice of location, and compete for entry into communities with more desirable packages by bidding on housing. This forms the cornerstone of bidding and sorting models in which different income and taste classes of households sort themselves based on their maximum willingness to pay for a quality adjusted unit of housing in communities with different tax and service packages.[5] Ross and Yinger (1999) provide a discussion of this class of model as well as a review of the capitalization literature that analyzes how differing property tax or public service levels are reflected in housing prices.

The part of this literature that is germane to our analysis deals with estimating the capitalization of school district characteristics. The main empirical hurdle with these studies is disentangling the capitalization of school district characteristics from the capitalization of neighborhood characteristics and taxes because these attributes are also spatially linked. A popular solution to this empirical hurdle is to use school districts that have more than one school in them and identify capitalization effects using variation across boundaries inside of the school district. Variations on this strategy have been used by Bogart and Cromwell (1997), Black (1999), as well as Weimer and Wolkoff (2001).

Another possibility is to use panel data and difference out the undesired effects; this allows analysis of the capitalization of school district characteristics that vary over time. Barrow and Rouse (2004) use school district fixed effects to see how differences in state aid to schools are capitalized into property values. Their identification strategy is similar to Clapp, Nanda and Ross (2008) who use census tract fixed effects to study the capitalization of differences in state standardized test scores and school district demographics over time. Also, a study by Figlio and Lucas (2004) uses repeat sales data, which allows for property level fixed effects, to look at the effect of school report card grades on property values.

Our identification strategy is closer to the second set of papers: we tackle neighborhood and tax effects by differencing over time as part of our difference-in-

[5] Households sort along income for both property taxes and public services, but they only sort along preferences for public services. This is because regardless of tastes any household is willing to pay a maximum of one dollar to avoid one dollar of taxes.

differences estimator. However, our analysis is different in that we are not interested in the level of public service capitalization into property values as much as we are interested in how property values change in response to a policy shift. There are not a lot of studies that take such an approach, the only paper that we are aware of is by Reback (2005), who analyzes how property values respond to the introduction of a school choice program in Minnesota.

2.3 Theoretical Framework: The Effect of the Top 10% Plan on Property Values

This section presents the conceptual model that sheds light on our identification strategy. Our hypothesis is that after the implementation of the Top 10% Plan property values will increase in lower-quality school districts relative to higher-quality school districts. To explain why we expect this to be the case we will briefly introduce a model of bidding and sorting. Following Ross and Yinger (1999), we make the following assumptions:[6]

(A.1) Household utility depends on consumption of housing, public services (in our case school district quality), and a composite good. Furthermore we will assume that the households utility function takes on a Cobb-Douglas functional form, this will make the specific effect of the Top 10% Plan easier to see algebraically.

(A.2) Every household falls into a distinct income and taste class of which there are a finite number.

(A.3) Households are perfectly mobile homeowners.

(A.4) All households in the same school district receive the same level of school district quality, and the only way to gain access to a school district is to reside within its borders.

[6] For a complete treatment of this and similar types of bidding models as well as a review of the relevant bidding and sorting literature refer to Ross and Yinger (1999).

(A.5) There are many school districts with varying levels of quality that finance themselves through a local property tax.[7]

We will use the following notation: S is the level of local public services (school district quality), H is housing, measured in quality adjusted units of housing services with a price of P per unit. Z is the composite good, with a price normalized to one. The effective property tax rate is t, the total tax payment is T, which equals t times V, and the value of a property is given by $V = \frac{PH}{r}$, where r is the discount rate. T can be simplified by noticing that $T = t \cdot V = \frac{t}{r} \cdot PH = \tilde{t} \cdot PH$. This yields a household budget constraint of: $Y = Z + PH \cdot \left(1 + \tilde{t}\right)$.

To capture competition for entry into desirable communities, the household utility maximization can be viewed as a bidding problem: How much is a household willing to bid for a unit of housing in a more desirable community? This is shown by rearranging the budget constraint to solve for a household's maximum bid:

$$\underset{\{H,Z\}}{Max} \quad P = \frac{Y - Z}{H \cdot \left(1 + \tilde{t}\right)} \tag{1}$$

$$\textit{Subject to} \;\; U\left(Z, H; S\right) = U^{0}\left(Y\right)$$

Setting up the Lagrange function, the household's optimization problem becomes the following:

$$\underset{\{H,Z\}}{Max} \quad \mathrm{L} = \frac{Y - Z}{H \cdot \left(1 + \tilde{t}\right)} + \lambda \cdot \left\{U\left(Z, H; S\right) - U^{0}\left(Y\right)\right\} \tag{2}$$

[7] An alternate to this assumption is to assume a proportional tax on housing services consumed. This is essentially a property tax, but does allow for the possibility of renters, allowing (A.3) to be slightly relaxed. An implementation of this assumption can be found in Epple, Filimon, and Romer (1993).

The household's maximization problem has the following first order conditions for an interior solution:

$$\frac{\partial L}{\partial H}:-\frac{Y-Z}{H^{2}\cdot\left(1+\tilde{t}\right)}+\lambda\cdot U_{H}=0 \tag{3}$$

$$\frac{\partial L}{\partial Z}:-\frac{1}{H\cdot\left(1+\tilde{t}\right)}+\lambda\cdot U_{Z}=0 \tag{4}$$

These results allow us to solve for the Lagrange multiplier, which will be needed later to get comparative statics via the envelope theorem. There are two possible solutions for the Lagrange multiplier. Using the first order condition with respect to housing, H, the solution is:

$$\lambda=\frac{Y-Z}{H^{2}\cdot\left(1+\tilde{t}\right)\cdot U_{H}} \tag{5}$$

And using the first order condition with respect to the composite good, Z, the solution is:

$$\lambda=\frac{1}{H\cdot\left(1+\tilde{t}\right)\cdot U_{Z}} \tag{6}$$

These are both apt expressions for the Lagrange multiplier, λ, however, the second expression lends itself to ease of interpretation in the next step. If we recognize that school district quality, S, is a parameter in this setup, then we can solve for the impact of S on the bid P by applying the envelope theorem to equation (1):

$$P_{S}=\lambda\cdot U_{S} \tag{7}$$

We can then substitute in equation (6) for λ to get,

$$P_S = {U_S}/{U_Z} \cdot \frac{1}{H^* \cdot (1+\tilde{t})} = \frac{MB}{H^* \cdot (1+\tilde{t})} \tag{8}$$

This is greatly simplified by our use of the second expression for λ, since ${U_S}/{U_Z}$ is the marginal benefit of a unit of S (as the price of a unit of Z has been normalized to one). P_S is an expression for the slope of a bid-function (i.e., maximum willingness to pay for a quality adjusted unit of housing) with respect to S for an arbitrary income and taste class. If we notice that the value of this slope will be different for different income and taste classes then we can display a group of bid-functions B1, B2, and B3 as shown in Figure 2.1.

B1, B2, and B3 represent bid-functions for three different income and taste classes. Since housing is purchased by the highest bidder, the market bid-function is the upper envelope of the bid-functions of all income and taste classes. To look at the theoretical impact of the Top 10% Plan, consider a Cobb-Douglas utility function:

$$U(Z,H;S) = \alpha \cdot \ln(S) + \beta \cdot \ln(Z) + (1-\alpha-\beta) \cdot \ln(H)$$

$$\text{Where } 0 < \alpha, \beta < 1, \alpha + \beta < 1 \tag{9}$$

The Top 10% Plan makes school district quality (i.e., ACT test scores) less valuable to a specific income and taste class, namely households whose children would now benefit from having peers who perform more poorly. This can be viewed as a decrease in the parameter α, which captures the household's taste for school district quality. Hence, we can find the effect of the Top 10% Plan on housing prices through a change in the parameter α by substituting equation (9) into equation (1) and then applying the envelope theorem:

$$P_\alpha = \lambda \cdot \left[\ln(S) - \ln(H^*)\right] = \frac{\ln(S) - \ln(H^*)}{H^* \cdot (1+\tilde{t}) \cdot U_Z} \tag{10}$$

Equation (10) is positive if S > H, negative if S < H, and zero when the two are equivalent. Suppose B2 is the bid-function for the income and taste class that will be affected by the Top 10% Plan, then as shown in Figure 2.1, B2' is the income and taste class bid function after the Top 10% Plan is enacted.

Since S and P are both in per quality adjusted unit of housing terms, there exists some S^* such that there is one unit of school district quality per unit of housing. For school districts with higher quality than S^* the affected income and taste class will have a smaller bid after the policy is enacted, and for school districts with lower quality than S^* the affected income and taste class will have a larger bid after the policy is enacted. If we compare the upper envelope of B1, B2, and B3 to the upper envelope of B1, B2', and B3 the impact of the Top 10% Plan is clear. The two wedges to either side of S^* show the potential distortion in housing prices caused by the policy change. It should be noted that the part of the B2' bid function that is mapped to S^* will not necessarily be part of the market bid-function envelope. This means that the part of the post-policy market bid-function that comes from the affected income and taste class could be either greater or less than it was prior to the policy change. That is, housing prices will solely increase on the affected portion of the bid-function if S^* is to the right of or equal to the point where B2' and B3 intersect, whereas housing prices will solely decrease on the affected portion if S^* is to the left of or equal to the point where B2' and B1 intersect. Which case prevails does not change the qualitative result of the policy change. The Top 10% Plan makes school districts of lower quality than S^* increase in value relative to those school districts of higher quality than S^*. Whether the relative gain is because of an increase in value for low-quality school districts, a decrease in value for high-quality school districts, or some amalgam of the two is uncertain.

Realistically the Top 10% Plan will influence multiple household types all at the same time. This can be visualized as an overall flattening of the distribution of bid functions. Households that have more to gain by improved access will flatten their bid functions to a larger extent. There is also some uncertainty as to the specific mechanism by which the property values change, but whether prices adjust because of moves within a district, across districts or even from out of state is immaterial to our analysis. All that matters is that households change residence for some reason (possibly unrelated to the Top 10% Plan), and in the course of the move their new willingness to pay for housing services (that factors in the Top 10% Plan) will be capitalized into housing values.

2.4 Empirical Strategies and Model Specification

2.4.1. Difference-in-Differences Analysis

We use a difference-in-differences analytic approach to study the effect of the Top 10% Plan on property values. We compare changes in home values before and after the Top 10% Plan was enacted by differencing property values in the pre-policy period (1994-95 school year through 1996-97 school year) from property values in the post-policy period (1997-98 school year through 2005-06 school year). This removes any effects that are constant between the pre and post-periods such as omitted neighborhood effects. The second difference is between the 1st and 2nd quintiles of school quality. This should yield the net effect of the Top 10% Plan on home values in the 1st (bottom) quintile relative to the 2nd quintile. Our identification strategy hinges on the assumption that there were no other exogenous factors that could have caused these differences in this time frame.

Several models of the following form are estimated by ordinary least squares (OLS) with interest on the parameter δ, the difference-in-differences estimator,

$$\ln\left(Y\right)_{jt} = \alpha + \gamma \cdot Post_t + \beta \cdot Treatment_i + \delta \cdot Post_t \cdot Treatment_i + \tau \cdot Ltrend_t$$

$$+X_{it} \cdot \theta + C_{kt} \cdot \lambda + \varphi + \varepsilon_{jt} \tag{11}$$

where the dependent variable $\ln\left(Y\right)_{jt}$ indicates the log of the average price of a single family home in school district *j* in year *t*. $Post_t$ is a binary variable indicating the period after the law was passed (i.e., equal to 1 for the 1997-98 through 2005-06 school years or equal to 0 for the 1994-95 through 1996-97 school years). $Treatment_i$ is a binary variable indicating low-performing high school campuses (i.e. campuses with poor pre-policy access to universities), these campuses are identified by their median American College Test (ACT) scores (i.e., equal to 1 for the 1st ACT quintile or equal to 0 for the 2nd ACT quintile).[8] $Post_t$ multiplied by $Treatment_i$ is the interaction of these two indicator variables. $Ltrend_t$ is a linear time trend. X_{it} is a vector of time varying characteristics associated with high school *i* in year *t*. C_{kt} is a vector of time varying characteristics associated with county *k* in year *t*, and φ is a vector of Metropolitan Statistical Area (MSA) fixed effects. Lastly, ε_{jt} is a normally distributed random error term.

More specifically, the vectors described in equation (11) contain the following variables: X_{it} is comprised of the high school demographic controls and variables for the degree of urbanization at the high school's location. The high school demographics include: the percentage of minority students, the percentage of economically disadvantaged students,

[8] We can observe ACT scores on the individual high school level, but our dependent variable is measured at the district level. This introduces an aggregation bias towards finding no response from the policy change.

the percentage of gifted students, average teacher experience, and the teacher-to-student ratio. The urbanization controls are dummy variables for the school campus being located in a large or small city, a large or small urban fringe, or in a town. Rural campuses are the omitted category. C_{kt} is a vector of time varying county characteristics and has controls for the percentage of the population that is black, the percentage of the population that is Hispanic, the average number of persons per housing unit, the percentage of housing units that are owner-occupied, violent crimes per 1,000 people, and the percentage of county residents with a college degree.

Our theoretical model from the previous section cannot tell us whether the relative price change is driven by low or high-quality school districts, and neither can the difference-in-differences estimator. However, the difference-in-differences estimator has some nice properties when faced with some highly probable types of misspecification. Incorrect specification of S^* the border between the treatment and control groups will bias the difference-in-differences estimator towards zero. Moreover, incorrectly specifying the bottom edge of the treatment group or the top edge of the control group will also bias the difference-in-differences estimator towards zero.

Also, high school switching could realistically happen between any two schools of differential quality in the lower end of the school quality distribution. Not all switches will be from the 2nd ACT quintile of school quality to the bottom ACT quintile of school quality – there is a possibility for intra-quintile switches. However, if we assume that all switches inspired by the policy change are from higher to lower-quality schools, then failing to capture price changes coming from these intra-quintile switches will bias the difference-in-differences estimation towards finding no effect from the legislative change.

Our estimation strategy allows us to identify effects from the part of the distribution of school quality that should be most responsive to the policy shift. The astute reader will notice the opportunity to check other parts of the distribution for policy effects. Specifically we can estimate the effect of the Top 10% Plan on *all* quintiles relative to the bottom quintile. We would expect to see the quintiles closest to the bottom of the distribution to have the largest effect, and to see the effects attenuate as we look further and further away from the bottom quintile. This can be done by estimating the following model specification:

$$\ln(Y)_{jt} = \alpha + \gamma \cdot Post_t + \beta \cdot Qtile_i + \delta \cdot Post_t \cdot Qtile_i + \tau \cdot Ltrend_t$$

$$+ X_{it} \cdot \theta + C_{kt} \cdot \lambda + \varphi + \varepsilon_{jt} \qquad (12)$$

where *Qtile* is a vector of dummy variables for the 2nd, 3rd, 4th and top quintiles (the bottom quintile is the omitted category). The different realizations of δ, the coefficient on the interactions between the dummy variables and the post period indicator will give the effect of the policy on the different quintiles relative to the bottom quintile.

We can also run the difference-in-differences analysis for the top two ACT quintiles of the school quality distribution. High schools with top levels of academic performance should be placing much more than their top 10% of graduates into institutions of quality and as such should be largely unaffected by the implementation of the Top 10% Plan. If in the top end of the school quality distribution, relatively "poor" performing school districts (4th ACT quintile) are gaining in property value relative to better performing school districts (5th ACT quintile), then our proposed mechanism for property value changes in the bottom end of the school quality distribution would be called into serious doubt. Such a result would show that migration from higher to relatively "lower" quality school districts occurred in a part of the school quality distribution where the Top 10% Plan should have little to no effect, making it likely that any changes observed in the bottom part of the school quality distribution were caused by some other phenomenon all together. Our hypothesis will be greatly strengthened if there are noticeable difference-in-differences between the bottom two (2nd and 1st) ACT quintiles but not between the top two (5th and 4th) ACT quintiles.

2.4.2. Herfindahl-Hirschman Index Analysis

Our second estimation strategy investigates if the number of schooling options available influenced the effect of the Top 10% Plan on property values. If it is costly to change school districts, which is the proposed mechanism for the property value changes, then it is less likely that households will react to the policy change. Therefore, if there are more local schooling options then it should be less costly to change school districts and there should be a larger reaction. For example, a move across the state to find a more strategic school seems unlikely because of the costs of finding new employment for the parents. However, a move of a smaller distance such as a couple blocks seems much more reasonable.[9]

One approach is to measure how concentrated the schooling industry is at the county level. This can be done by calculating the Herfindahl-Hirschman Index (HHI) for each county,

$$HHI_k = \sum_{i \in k} s_i^2 = \sum_{i \in k} \left(\frac{\text{Total \# of students in each high school}_i}{\text{Total \# of students in the county}_k} \right)^2 \tag{13}$$

where s_i is the market share of each high school *i* in county *k*. For schooling, a measure of the market share is the number of students at the high school divided by the total number of

[9] It is not necessary for the household to move because of the policy change to get a resulting change in property values. A change in values may be driven by households that were already planning to move and simply found lower-performing schools to suddenly be more desirable.

students in the county. A HHI_k value close to 1 indicates a more *monopolistic* county, whereas a HHI_k value close to 0 indicates a more *competitive* county.

To analyze whether the number of schooling options available influenced the effect of the Top 10% Plan on property values, we interact the pre-policy county level HHI_k measure with our difference-in-differences estimator, yielding the following triple-difference specification,

$$\ln(Y)_{jt} = \alpha + \gamma \cdot Post_t + \beta \cdot Treatment_i + \delta \cdot Post_t \cdot Treatment_i + \tau \cdot Ltrend_t$$

$$+ \psi \cdot HHI_k + \phi \cdot Post_t \cdot HHI_k + \rho \cdot Treatment_i \cdot HHI_k + \pi \cdot Post_t \cdot Treatment_i \cdot HHI_k$$

$$+ X_{it} \cdot \theta + C_{kt} \cdot \lambda + \varphi + \eta_{jt} \qquad (14)$$

where π is now the parameter of interest, estimating the effect of the county HHI_k on the relative impact of the Top 10% Plan on property values.

A negative value for the coefficient π would imply that counties with less school choice showed a smaller reaction to the Top 10% Plan. This coefficient will tell us if school choice matters, but does not give us any information as to which part of the school competition distribution could be driving the result. To get at this point we split counties into quintiles based on their pre-policy years' HHI_k value. We then estimate a different-in-differences regression (equation 11) for each HHI_k quintile separately. This allows us to show how the effect of the Top 10% Plan differed for areas with different amounts of local schooling options in greater distributional detail by comparing difference-in-differences estimates for the HHI_k quintile subsamples.

2.5 Data Sources and Sample Characteristics

2.5.1. Data Sources

The data for this study was compiled from five sources: the Texas Comptroller Property Tax Division (TCPTD); the Academic Excellence Indicator System (AEIS) from the Student Assessment Divisions of the Texas Education Agency; the National Center for Education Statistics (NCES); the U.S. Census Bureau; and lastly, the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) database. The TCPTD, AEIS, and NCES all utilize Independent School District unique identification numbers that are identical across datasets and enable the linkage of variables in each of these datasets to their specific high school campuses.

The TCPTD database, contains information on total appraised home values for all school districts from 1994-95 to 2005-06, which covers both pre and post-policy years. This value is an aggregation of all residential homes that are served by a specific school district. Our analysis uses property values for single family homes only. We exclude multiple family dwellings and condominiums as well as all non-residential properties from our analysis. The TCPTD data also has information on the number of residential housing units in each school district. We use this information to construct our dependent variable by dividing the aggregate value of all residential homes in a school district by the number of housing units in that district. All home values are normalized to 1990 dollars.

Property appraisals in Texas follow a specific procedure. A property must be reappraised by its appraisal district at least once every three years, but this can be done more frequently. If a property is sold in a given year, then the sale price of the property is automatically used as the new appraised value of the property. For properties that do not sell, they are assigned a value based on how their characteristics compare to the characteristics of properties that were sold recently. The tax assessors generate a model based on recent sales and then use that model to predict what the assessment should be for the unsold properties. There are also limits on how much an appraisal can increase over the previous year's appraisal.[10] Given how Texas calculates its home appraisals our data accounts fairly well for property value changes as reflected by housing transactions.

We use the AEIS data in the pre-policy years (i.e., 1994-95 through 1996-97) to identify low-performing high school campuses using the median American College Test (ACT) scores of the graduating class. The mean of the median ACT scores in the pre-policy years is then used to sort campuses into quintiles. This allows for the identification of poor-performing schools that are most likely to be targeted by parents who chose to change districts in order to increase the chances of their children being rank-eligible for automatic admission. While some states use the ACT as their assessment measure for the No Child Left Behind Act (NCLB) to hold schools accountable, this is not the case in Texas. Texas has its own state assessment test, the Texas Assessment of Academic Skills (TAAS). Thus, using the ACT scores allows us to more reliably identify low-performing schools relative to higher-performing schools.[11,12]

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[10] An appraisal may not increase to more than the lesser of:

a) The sale price of the property if it sold that year, or
b) 110 percent of the previous year's appraisal plus the market value of any new improvements on the property.

[11] Our analysis was also conducted using the Scholastic Aptitude Test (SAT) scores and found similar results to that of the ACT analysis.

[12] For purposes of our analysis ACT scores are superior to TAAS scores because the TAAS unlike the ACT (or SAT) is not used in college admissions decisions and is not necessarily a good indicator of a school's access to universities.

The AEIS data also contains detailed information on student and teacher demographic variables; this allows us to calculate the percentage of minority students, the percentage of economically disadvantaged students (i.e., those who qualify for reduced price school lunch), the percentage of students that participate in a gifted program, average teacher experience, and the teacher-to-student ratio at a given high school. Our analysis is restricted to "regular" high schools; any alternative or magnet high schools as well as any juvenile delinquency centers are dropped from the analytic sample.

The NCES data link high school campuses to the urbanization level of their surrounding area. For the purposes of this study, campuses are considered to be located in a large city if they are in the central city of a Consolidated Metropolitan Statistical Area (CMSA) with a population greater than 250,000. Campuses are considered to be located in a small city if they are in the central city of a CMSA with a population less than 250,000. Campuses located in large and small fringes refer to addresses that are within the CMSAs for large and small cities respectively, but are not located in the central city of that CMSA. Campuses located in towns are in areas that are not incorporated into the above definitions and also have a population greater than or equal to 2,500. All other campuses are considered to be located in a rural setting, which is the omitted category in our analysis.

In addition, we use the U.S. Decennial Census and UCR data to merge in additional controls needed in the analysis. We use the 1990 and 2000 U.S. Decennial Censuses to create county-level variables to capture the trends in the percentage of the population that is black, the percentage of the population that is Hispanic, the average persons per housing unit, and the percentage of housing units that are owner-occupied. Lastly, the UCR database provides us with county-level variables on violent crimes (i.e., murder, rape, robbery, and assault).[13] Combining the UCR data with the Census data allows us to use estimates of the county-level violent crime rate for the school years of interest.

2.5.2. Sample Characteristics

Table 2.1 reports means and standard deviations for the variables used in our analysis. It also reports the data for the relevant subsamples. For our main specifications the subsample of interest is the bottom two quintiles of school quality with regards to the ACT score distribution. The 1st quintile (bottom) serves as the treatment group and the 2nd quintile as the control group. The 1st quintile of schools represents schools that are most likely to be targeted by parents seeking to take advantage of the Top 10% Plan. The 2nd quintile is a good approximation for schools that a strategic parent would want to move their child from in order to gain the benefits available in the bottom quintile. This is because the 2nd quintile is most similar to the bottom quintile in terms of academic performance and pre-policy access to selective state colleges and universities.

It is immediately noticeable that the 1st and 2nd quintiles are actually quite different in many of their other characteristics. One such characteristic is that property values are far greater in the bottom quintile than in the 2nd quintile. This is largely because the bottom quintile contains many more large urbanized areas (34.8 percent versus 11.5 percent). Further evidence of this is found in Figure 2.2 that shows the time trends for the property values of the treatment and control groups. The 1st and 2nd quintiles appear as if they may be on different growth paths in the post period. This provides us with reason to control for trends in property values in our analysis. But even without these controls, it appears at first glance that the 1st quintile does have a jump in property values after the Top 10% Plan is

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[13] There are several measures of crime available in the UCR database. We use violent crimes because they are largely not financially motivated and thus exogenous with respect to local property values, as opposed to an alternate measure of property crimes (grand theft auto, larceny, etc.), which are highly endogenous.

enacted on May 20th, 1997, however, less of a discernible jump in property values is observed for the 2nd quintile. Figure 2.2 also indicates that prior to the implementation of the Top 10% Plan the slopes of the treatment and control groups trend lines seem to be quite close.

Additionally, Table 2.2 reports the differences without a linear time trend (or controls) for levels and logs of property values. As seen in panel B of Table 2.2, we observe a 2.9 percent increase in residential home values for low-performing school districts relative to the second quintile after the policy change.

2.6 Discussion of Results

2.6.1. Overall Results: Difference-in-Differences Analysis

The results for the regression adjusted difference-in-differences analysis are summarized in Table 2.3. This table only reports the estimated coefficients on the post indicator variable interacted with the treatment indicator variable, treatment indicator, post indicator, and the linear time trend. The layout of Table 2.3 is as follows: column (1) presents the unadjusted baseline effects, column (2) controls for high school demographics and urbanization characteristics, column (3) is the fully controlled regression specification (i.e., high school characteristics, urbanization characteristics, and county level controls), and lastly, column (4) is the fully controlled regression specification with the addition of MSA fixed effects.

There is a positive and statistically significant difference-in-differences estimate for all model specifications. The point estimate on the difference-in-differences estimator ranges between 0.032 and 0.051.[14] Our preferred specification (shown in column (4)), estimates a 4.9 percent increase of housing prices in low-performing school districts. This

[14] Similar results were found using median pre-policy ACT scores (as opposed to the mean).

lends credence to our hypothesis of the Top 10% Plan influencing property values in the lower end of the school quality distribution. Specifically, this suggests that the benefit offered by the increased likelihood of college admissions from attending a lower-quality school has caused property values in the bottom quintile to increase in value relative to those in the 2nd quintile. Though the magnitudes of the point estimates do vary, the directions of these estimates are not sensitive and are fairly robust to the addition of controls and MSA fixed effects. Our point estimates are comparable in size to effects found in other studies looking at the capitalization of schooling attributes. Our results are larger than the estimated effect on property values of a one standard deviation increase in test scores of around 1 percent as found in studies such as Clapp, Nanda and Ross (2008) and Black (1999); but smaller than the 7 percent effect found by Figlio and Lucas (2004) for top marks on school report card grades. Reback (2005), whose methodology is closest to our own, finds around a 2 percent effect for gaining access to a high school choice program.

As for the point estimates on the control variables for property values, the point estimate on percent of minority students is positive. This is not surprising as this variable is negatively correlated with the variable for percent of economically disadvantaged students. Property values also appear to be positively related to schools with more students in gifted programs and a higher teacher-to-student ratio. The urbanization controls all have positive point estimates that increase in magnitude as the school's location increase in population size. This is consistent with the standard urban economics result of higher land prices in more urbanized areas. Lastly, county education level has a positive and significant effect on property values.[15]

[15] Full regression results are available upon request.

Analysis of Other Parts of the School Quality Distribution

The results for the entire distribution of school quality relative to the bottom of the distribution are presented in Table 2.4. Table 2.4 has the same table layout as Table 2.3. The negative point estimates for the interaction terms indicate that the quintile in question is losing value relative to the bottom quintile. Or, the bottom quintile is gaining relative to the quintile in question. Table 2.4 shows a clear story for the distribution of school quality: the effect of the Top 10% Plan is strongest in the quintiles closer to the bottom of the distribution and attenuates with distance.

Lastly, the results from the placebo difference-in-differences regression analysis are summarized in Table 2.5. The placebo analysis uses the top two quintiles of school quality instead of the bottom two quintiles. The placebo treatment group is thus the 4^{th} quintile and the placebo control group is the 5^{th} quintile. The most important result in Table 2.5 is that all of the difference-in-differences point estimates are either negative or statistically insignificant. This is not the effect that one would expect to see if the Top 10% Plan had caused strategic high school switching in the top of the school quality distribution.

Taken all together, our results suggests that the benefit offered by the increased likelihood of college admissions from attending a lower-quality school caused property values in the bottom quintile to increase in value relative to those in the 2^{nd} quintile. Thus, the Top 10% Plan makes school district quality less valuable to a specific income and taste class, namely households whose children would now benefit from having peers who perform more poorly.

2.6.2. Robustness Analysis

It is possible that our findings could be the result of events other than the implementation of the Top 10% Plan. In fact, the time period around the implementation of the Top 10% Plan contains many policy changes in Texas that also affect schooling. These changes could serve as alternative explanations that would invalidate the interpretation of our difference-in-differences estimates. In this section, we present additional analyses that rule out these policy changes as alternatives to our interpretation.

2.6.2.1. Pre-existing Trends

A concern that arises when conducting a difference-in-differences analysis is that the treatment and control groups are on different growth paths before the policy is enacted. In order for our previous analysis to provide unbiased estimates of the effect of the Top 10% Plan, it must be the case that the treatment and control groups exhibit common trends in the pre-policy period. This assumption in the difference-in-differences framework is commonly known as the parallel-trends assumption. Even though we use a linear time trend in our analysis it is still a possibility that the treatment and control groups are on different growth paths even after this inclusion. Figure 2.2 suggests that this assumption holds for our analytic sample. We can also formally test the parallel-trends assumption. To do so, we drop all post-policy observations (i.e., 1997-98 to 2005-06) and redefine the "post" variable to a "fake year" (i.e., 1995-96), choosing a year when the Top 10% Plan was not in effect. The results of this analysis are reported in Table 2.6. None of the regressions show any significant difference-in-differences point estimates, that is, there are no statistically significant differences between our treatment (1^{st} ACT quintile) and the control (2^{nd} ACT quintile) groups prior to the implantation of the Top 10% Plan.

2.6.2.2. Open Enrollment, No Child Left Behind, and Texas School Accountability

<u>*Open Enrollment*</u>

In 1995 Texas enacted open enrollment laws that gave students in poorly performing school districts the option to enroll in higher-quality schools without changing residence. This could have potentially increased property values in low-performing school districts making the effects we are attributing to the Top 10% Plan simply a residual change from the enactment of open enrollment. However, it is very unlikely that the open enrollment laws had any effect on property values at the school district level. This is because though school districts were required to accept transfer requests from within the district they were not required to accept out of district transfer requests. This made across district switches extremely rare and unlikely to influence property values. To verify this, the above test of the parallel trends assumption also coincides with the enactment of open enrollment laws. Since none of the regressions reported in Table 2.6 show any significant difference-in-differences point estimates, this helps to rule out open enrollment as an alternative explanation of our results.

<u>*No Child Left Behind*</u>

It is also possible that the passing of the No Child Left Behind (NCLB) Act on January 8, 2002 causes our results. The first school year affected by the NCLB was 2002-03. To check against such a possibility and gauge the stability of the point estimates shown in Table 2.3, we re-run our difference-in-differences analysis using different sized post-period windows. Table 2.7 reports alternative regression results using three different sized post-period windows. Column (1) reports results using the full twelve year sample, which are the results from Table 2.3. Column (2) reports results using an eight-year period subsample, this analysis drops all of the school years in which NCLB was in effect: school years 2002-03, 2003-04, 2004-05 and 2005-06. Column (3) further restricts the sample to a six-year period window, three years in the pre-policy period, and an equal number in the post-policy period. The difference-in-differences point estimates are positive and significant in all of the alternative subsample analyses. Thus, the results shown in Table 2.3 are robust to considering smaller windows around the implementation of the Top 10% Plan, and most importantly the results from column (3) also helps us rule out the passing of the NCLB Act as driving our results.

Texas School Accountability

Another important policy change is the Texas school accountability requirements, which were introduced in 1993. The school accountability measure likely affects low-performing schools more than high-performing schools. If the school accountability requirements became more stringent around 1997, then we may also observe larger performance improvement of low-performing than high-performing schools. This would manifest itself in the housing market. We address this concern by analyzing the Texas Assessment of Academic Skills (TAAS) pass rates for 3rd to 8th grades as the outcome. These results are reported in Table 2.8.[16] For 3rd to 6th grades, the sample does not include schools in the top quintile, so the estimates are relative to the second highest quintile (4th quintile). Most of the point estimates shown in Table 2.8 are insignificant; in particular, for 8th graders, all estimates are negative. Estimates are negative and significant for the 2nd and 3rd quintiles, suggesting that low-performing schools actually performed worse relative to the top schools

[16] The model estimated in Table 8 is the same as presented in equation (12), except that the top, rather than the bottom quintile is omitted.

in the post-Top10 years. Overall, there is no consistent evidence of larger improvement in academic achievement for low-performing schools following the Top 10% policy; therefore, the estimates in Table 2.3 are unlikely to be driven by changes in school accountability in Texas.

2.6.2.3. Robin Hood Plan

Another schooling policy that likely did influence property values in Texas was the "Robin Hood Plan."[17] The Robin Hood Plan, true to its name was a scheme that redistributed recaptured tax revenues of school districts with a lot of property wealth per adjusted pupil to districts with little property wealth per adjusted pupil. It is very possible that the Robin Hood Plan lowered property values in property rich places relative to values in property poor places.

We can rule out Robin Hood on two counts. The first is that Robin Hood was implemented in the 1993-94 school year. This coincides with the beginning of our sample, so any time invariant effects of the Robin Hood Plan will difference out in our difference-in-differences estimator. The only way remaining that the Robin Hood Plan could serve as an alternative explanation for our results would be if the plan had time varying effects that intensified over time and if property poor districts coincide with poorly performing districts. If this is the case, then we would expect the lowest quality school districts to receive a larger amount of funding as the effect of the Robin Hood Plan intensifies over time. We can test for this by running our difference-in-differences estimator with the amount of spending per pupil as a dependent variable. Table 2.9 presents the results of this analysis. All estimates

[17] A thoughtful analysis of the Robin Hood Plan can be found in Hoxby and Kuziemko (2004).

are both negative and insignificant, which further discredits the Robin Hood Plan as an alternative explanation of our results.

2.6.2.4. Longhorn Opportunity Scholarships

One further policy of note is the introduction of Longhorn Opportunity Scholarships in 2001. Longhorn Scholarships are offered through the University of Texas at Austin (UT-Austin) and are tied to the Top 10% Plan. These scholarships are aimed at helping students from schools that did not historically place many students at UT-Austin. To be eligible you must attend a school identified by UT-Austin as historically under or non-represented at UT-Austin and be rank eligible under the Top 10% Plan. These students get a scholarship of $5,000 per year for four years. It is possible that our results are inflated by the effect of the Longhorn scholarships.

To eliminate the effect of these scholarships we re-estimate our difference-in-differences estimator after dropping all schools listed by UT-Austin as eligible for Longhorn Opportunity Scholarships from the sample.[18] The results of this estimation are reported in Table 2.10. The results are nearly identical to those presented in Table 2.3, which rules out the effect of these scholarships as an alternative explanation for our results.

2.6.3. School Competition: Herfindahl-Hirschman Index Analysis

The results for the number of schooling options are presented in Tables 2.11 and 2.12. Table 2.11 shows results from estimating equation (14). All controls used in column (3) of Table 2.3 are used in the regressions for Table 2.11. The coefficient of interest is the interaction between the difference-in-differences estimator and the county level HHI_k. The

[18] 44 schools are listed as eligible for Longhorn Opportunity Scholarships.

interaction is negative and significant, implying that the more monopolistic the county, the less the school districts in that county reacted to the implementation of the Top 10% Plan.

Table 2.12 shows the difference-in-differences estimates from subsamples of counties that are the most monopolistic (i.e., have a higher HHI_k value for schooling) at the right of the table, and the least monopolistic (i.e., having a lower HHI_k value for schooling) at the left of the table. Only the difference-in-differences estimators are reported, and each coefficient represents a separate regression. Again, all controls used in column (3) of Table 2.3 are used in the regressions for Table 2.12.[19] The difference-in-differences point estimates only measure positive and significant in the locations with the largest amount of school choice. Specifically, counties that were more monopolistic in nature were unresponsive to the policy shift. In other words, areas where there are not a lot of local high school options to switch to did not respond to the Top 10% Plan. In contrast, the responsive areas were counties with the lowest fifth of HHI_k measures: the difference-in-differences point estimates are only positive and significant for the least monopolistic school districts. Our results show that for counties with the lowest fifth of HHI_k measures, the average price grew by 3.4 percent in low-performing school districts.

The HHI analysis suggests that if the changes in property values are due to households moving strategically, then these moves are likely short distance. Furthermore, the HHI_k analysis reinforces the results presented in the previous section, as these results help to rule out alternative explanations. For instance, it is possible that the growth in property values in low-quality school districts was due to the housing bubble and rapid

[19] MSA fixed effects were not included because they are too closely related the quintile of HHI values to be used reliably given the sample size of the subsamples.

growth of subprime mortgages in the early years of the 2000s. However, any growth in property values due to this housing bubble should be orthogonal to the schooling option variation used in the HHI_k analysis.

Lastly, Figures 2.3 and 2.4 show the growth in property values from the pre-policy period to the post-policy period. Figure 2.4 overlays the location of the least monopolistic counties from Table 2.12 onto the map in Figure 2.3. This overlay illustrates that the growth in home values is spread across the state, and is not just relegated to specific areas. These figures along with the HHI analysis provide a strong backing to our proposed mechanism of families moving to drive changes in property values. It also helps to reinforce our earlier findings of a change in property values in response to the Top 10% Plan.

2.7 Conclusion

Since its implementation over 10 years ago, the Top 10% Plan has received only mixed reviews. One of the main criticisms of this policy is that it is unfair to high-achieving students who attend elite high schools. Because the Top 10% Plan is solely based on class rank and this criterion is applied to all high schools that use grade point averages to rank students, there is redistribution in the university system from students who graduate from high-performing high schools to automatically admitted students who graduate from low-performing high schools. On the other hand, while the goal of the Top 10% Plan was to improve access for disadvantaged and minority students, the use of a school-specific standard to determine eligibility has led to some other unintended effects.

The estimate from our preferred specification implies that the implementation of the Top 10% Plan raised property values by 4.9 percent. We can get a rough sense of the total effect on the tax base by running our main estimation strategy on the dependent variable of total appraised property value in a school district. The results of such a regression show a 16.6 percent increase in the total property tax base. If we arbitrarily divide the 16.6 percent evenly (i.e., assuming an 8.3 percent gain in aggregate property values in the bottom quintile and an 8.3 percent loss in aggregate property values in the second quintile) then one can see that the effect of the Top 10% Plan on the property tax base was potentially quite large. The average district in the bottom quintile would have gained $344.9 million in their tax base and the average district in the 2nd quintile would have lost $129.9 million in their tax base. If we apply an arbitrary property tax rate of 0.4796 percent (i.e., the property tax rate in the city of Austin, Texas in 2008) then there would be an additional $1.65 million in property taxes for the average district in the bottom quintile and $0.6 million less in property taxes for the average district in 2nd quintile. These property tax estimates are by no means exact, especially since we do not know how the relative value shift is distributed between 2nd quintile losses and bottom quintile gains, and because these are only changes in single family homes and do not include other taxable properties that could have been affected. However, these tax estimates do illustrate the type of effect that the Top 10% Plan had on the property tax landscape in Texas.

The results from the HHI analysis reinforce this point even further. The effects of the Top 10% Plan appear to be both spatially concentrated and of larger magnitude in places with many schooling options. This implies that these places were likely hit with particularly large distortions to their property tax bases. Any future implementations of or modifications to *top x-percent* plan admissions policies should bear in mind that the redistribution of educational resources will not be the only effect of such a policy change.

3. Losers and Losers: Some Demographics of Medical Malpractice Tort Reforms

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The medical malpractice tort system in the United States has several purposes. Most notorious is the goal of incenting doctors to practice so-called appropriate medicine through the negligence rule of liability. The negligence aspect of the malpractice system has been widely studied for its implications on physician behavior, most notably the outcomes of practicing defensive medicine (Kessler and McClellan 1996, Kim 2007) or physician labor supply (Kessler, Sage, and Becker 2005; Matsa 2007). Another purpose of the malpractice tort system is to provide compensation to injured patients. The compensation is intended to offset economic damages from lost wages and the psychic costs of pain and suffering. The medical malpractice tort system therefore also serves as an insurance against adverse outcomes that covers patients implicitly when consuming medical services. Here we examine an under-appreciated dimension of the insurance aspect of the medical malpractice tort system, which is how tort reforms have affected the interpersonal distribution of patients' implicit insurance.[20]

To elaborate, we use closed claims from the state of Texas to examine econometrically how a reform package impacts people seeking recompense under their implicit insurance – people who have been negligently injured and are trying to get quick compensation. The particular reform package of interest was part of the Texas 2003 HB 4 law, which introduced two changes to the Texas malpractice liability system: (1) a cap on non-economic damages and (2) an early offer system.

The most widespread policy reform of medical malpractice has been a cap on non-economic damages. Caps have been implemented in about half the states and their effects widely studied in terms of their total cost implications for the medical care system

[20] A parallel line of research examines differences in damage cap effects across insurance providers (Viscusi and Born 2005).

(Danzon 1985, Donohue and Ho 2007, Lakdawalla and Seabury 2009, and Mello et al. 2010 to name a few). Damage caps put a maximum on how much can be paid out and, as such, lower the likelihood of a so-called blockbuster case.[21] A consequence is a lower average payout per case (Avraham 2007) plus a shorter length of time to settlement as caps reduce the variance between the plaintiff's and defendant's expected values of the case (Abraham 2001).

Early offer schemes create incentives for plaintiffs and defendants to settle early and punish them for passing up so-called good deals. In Texas, the early offer scheme forces the side that turned down the early offer to pay the other side's legal fees if it can be shown after the case that the party in question would have been better off accepting the offer. Not only do early offer reforms save considerable time in the litigation process (Hersch, O'Connell, and Viscusi 2007) but they also lower the payouts in malpractice litigation (Black, Hyman, and Silver 2009).

Both components of the Texas reforms, damage caps and early offer schemes, have similar effects on the insurance that is implicit in the medical malpractice liability system. The implicit insurance claims here have smaller, quicker payouts after the reforms. Whether or not the reforms improve the economic well-being of the holder of the policy depends on two factors. The first is the cost of the insurance paid implicitly through changes in patients' costs of medical services. Evidence is far from plentiful, but research suggests that physicians respond to changes in malpractice liability mainly via services quantities and not

[21] Although not a perfect match, damage caps parallel bankruptcy law. One has an asset with uncertain value (the right to sue here/the right to declare bankruptcy). It could be worth zero (you lose the case/cannot declare for legal reasons or benefits could be totally offset by a lowered credit score). It could also be worth a lot (you win the case/you are able to declare bankruptcy and protect your assets). The outcome is ambiguous as risk abounds (juries/uncertainty as to the law or how severely your credit score will be affected). In both tort cases and bankruptcy there is an intermediate way out (settlement/ debt restructuring that is less protection of assets or less of a disruption to one's credit score).

prices (Danzon 1990, Lakdawalla and Seabury 2009, Kessler 2011), although the final chapter of this dissertation does show price sensitivity for procedures performed by risky specialties. Second, the ultimate welfare effect of the reforms depends on the implicit policy holders' time preference. We therefore calculate the change in the value of a settlement by considering both the size of the settlement and the time it takes to reach the settlement.

Specifically, we look at how the value of a settlement changes across different age demographics after the reform was enacted. The change in settlement value comes from three channels: a direct effect of the reform lowering the amount of the average settlement, an indirect effect of the reform lowering the average amount that a claimant asks for, and a timing effect of the reform speeding up the time until settlement. We find that claimants in their prime working years suffer the largest economic loss in settlement value. The age pattern is true for the mean, median and maximum entropy quantile of settlement amounts across age groups although the most informative location in the distribution is most often the median. Our results differ from the common belief that medical malpractice reforms have the largest negative impact on the settlements of the very young and the elderly.[22]

3.1 Theoretical Considerations

To understand the fundamental economics of the decision to settle and why there may be age and other interpersonal differences in malpractice insurance damage caps' effects consider two actors A and B. Here both have been negligently injured and now have the right to sue. The right to sue is a risky asset S that takes on two values. An actor can go to court and will win with probability p, in which case S takes on the value $S^* > 0$, or may lose

[22] Medical malpractice damage caps supposedly reduce settlements for the young and the elderly most because they do not have large earnings and, as such, do not have large economic damages to claim (Finley 2004; Rubin and Shepherd 2008).

with probability $1-p,$ in which case S takes on the value zero. For simplicity, assume that $p = 1 - p = 0.5,$ although the exercise that follows does not depend on this assumption.

A and B have different risk preferences: A is risk neutral and B is risk averse. More formally, the actors have respective utility functions $U_A(S)$ and $U_B(S)$ such that $U_A(S)', \; U_B(S)' > 0$ and $U_A(S)'' = 0, \; U_B(S)'' < 0$. We also assume that $U_A(0) = U_B(0) = 0$ and that the utility functions do not cross. This gives the two utility functions shown in Figure 3.1.

Let $E[S^*] = S^{**}$. Each actor receives utility from the asset, person A receives $U_A(S^{**}) = E[U_A(S)]$, which can be seen in Figure 3.1 by tracing up from S^{**} to $U_A(S)$ and over to the vertical axis. B receives expected utility $E[U_B(S)]$, which can be seen in Figure 3.1 by tracing up from S^{**} to the ray connecting the origin to $U_B(S^*)$ and over to the vertical axis. Both actors are indifferent between going to court and a settlement that gives them their expected utility of the risky asset, and will settle for that amount or any greater amount. Person B is willing to accept a settlement of less than S^{**} due to risk aversion.[23] There will then be age differences in settlement willingness to the extent that risk aversion varies by age (Halek and Eisenhauer 2001, Anderson et al. 2008).

Now consider a cap on the amount that can be recovered in damages in a court award. This will change the maximum amount of the risky asset. The new asset S' can now either take on the value zero or S^{**} with equal probability. Let $E[S'] = S^{***}$. We can find each actor's utility from the new asset in a similar fashion as before. Person A receives $U_A(S^{***}) = E[U_A(S')]$, which can be seen in Figure 3.1 by tracing up from S^{***} to $U_A(S)$

[23] The more risk averse actor accepting a smaller settlement appears in a more general case of two risk averse bargainers who will go to an uncertain arbitrator if they cannot reach a settlement by Crawford (1984). In Crawford's model, an increase of an actor's risk aversion leads to a decrease in their settlement all else equal.

and over to the vertical axis. B receives expected utility $E[U_B(S')]$, which can be seen in Figure 3.1 by tracing up from S^{**} to the ray connecting the origin to $U_B(S^{**})$ and over to the vertical axis.

If we take the difference between the utility from the original asset S, and the capped asset S' we get L_A for actor A, and L_B for actor B. It is immediately noticeable that $L_A > L_B$, or that the less risk averse actor has a larger reduction in utility from the implementation of a cap on damages. The implication is that risk aversion differences by age or predicted settlement size can lead to age and other differences in the welfare loss from damage caps.

There are a few remarks that should be made about the above exercise. The first is that the behavioral implications do not depend on one of the actors being risk neutral. If actor A is also risk averse the result that the less risk averse party suffers a larger utility loss is maintained as long as the other assumptions are still met. It is also important to note that actors' changes in minimum acceptable settlements do not follow as clean a rule as their changes in utility. A careful inspection of Figure 3.1 may make it look as if there is a clear association between changes in minimum acceptable settlement and the relative risk aversion of the actors, but that is an artifice of A being risk neutral. Any systematic effects of possible interpersonal differences in relative risk aversion and their attendant implications for how damage caps affect the size (asset value) of the settlement needs to be discovered empirically.

3.2 Data

The data we use to estimate the distributional consequences of malpractice reforms come from the Texas Department of Insurance Closed Claims Database (CCD), which include every insurance claim over $10,000 closed in Texas during 1988-2007. The data include indications of the type of insurance and the party purchasing the insurance so that one can identify cases that deal specifically with medical malpractice. The subset of the data we use includes 21,733 claims on medical malpractice insurance policies of health care providers including physicians, dentists, hospitals, and nursing homes.

Each of our data points is a closed claim. Although there are data for 2007, there can be cases originating prior to 2007 that closed after 2007 and so are not represented. Each claim provides information on the time, location, and type of injury (the closed claims report uses broad definitions such as brain damage or back injury rather than diagnosis codes). For the injured party the data include age, employment status, and availability of compensation other than torts. The CCD also has comprehensive information concerning any and all legal action that took place including all settlement amounts and jury awards. Finally there is limited information on the defendants, including the type of entity the defendant (or defendants) is (are) plus information about the payout limits associated with its policies, and the estimates of litigation and indemnity costs by its insurance providers.

To ensure that we are not looking at people who are deliberately trying for a so-called blockbuster jury award we limit our sample to cases settled in three years or less (the average length of a case that reaches a verdict is 5.5 years).[24] The result is a sample of 6,130 observations. Figure 3.2 shows the density of claims by year for both settled claims and claims that go to verdict. By limiting our sample to three years we exclude most cases that would have been settled close to verdict.

The main outcomes we examine are the total amount of a settlement conditional on settlement before a verdict, the amount of compensation demanded by the claimant

[24] Later we examine the robustness of our results to the length of the settlement window.

conditional on settlement before a verdict and the time until settlement. Because it is a claims database, the CCD contains plentiful information on the relevant insurer and its behavior during the claims process . Of greatest importance is the indemnity reserve, which is the amount of money that the insurance company has set aside to pay for damages. The indemnity reserve is the insurance company's best estimate of the risk associated with a possible jury award or settlement, and effectively controls for many characteristics of the injury. Last, the claims database that we use also contains information on the specific policies' per accident maximum payout limits.

Table 3.1 contains the summary statistics for the data we use in the econometric estimation to follow. The first row documents the substantial reduction (about 55 percent) in the settlement amount after the reform, the second row documents a similar (50 percent) reduction in cash demanded, and the third row documents the notable reduction (33-45 percent) in case duration. There is clear evidence that the Texas reforms affected the ceiling of damages and encouraged quicker settlements on average. Our subsequent econometric models clarify the distributional consequences and the channels at work in the tort reforms producing the outcomes summarized in Table 3.1.

3.3 Empirical Methods and Results

Estimating the component effects of the tort reform can be done with a multi-step procedure. First we estimate the amount that average settlement compensation decreased directly. Then we estimate the indirect effect in settlement amount via changes in cash demanded. We then estimate the reduction in time to settlement after the reform. In all cases we consider distributional issues such as heterogeneity by age, settlement amounts or time to settlement.

3.3.1. Settlement Amounts and Initial Cash Demanded

To begin to disentangle the two effects from other variables that are also related to the size and speed of compensation, we first estimate two multivariate OLS regressions of the following algebraic forms:

(1) $Y_{it} = \alpha_{01} + \beta_{11}X_{1it} + \gamma_1 C_{it} + \delta_1 R_t$ and

(2) $C_{it} = \alpha_{02} + \beta_{12} X_{2it} + \delta_2 R_t$.

Here Y is a claim settlement amount, X is a vector of time varying control variables whose effect we wish to remove from our estimate of the effect of the reform, C is initial cash demanded, and R is an indicator variable equal to one in the time period after the reform has been enacted, and zero otherwise. Thus, δ_k (k = 1, 2) is the estimated effect of the reform on either the amount of the settlement or the amount initially demanded by the claimant.

The OLS results in Table 3.2 illustrate the post-reform settlement amount holding constant other factors, including cash demanded, which we view as an indicator of an initial signal of how likely the claimant is willing to settle. The results for the pooled ages in the last column indicate a $59,000 reduction in the settlement amount post-reform, which is about 13 percent of the pre-reform mean. The disaggregated results show that the groups most affected by the reform are people in the 20s and 30s, and that the reform is non-neutral by age.

A final result of note in Table 3.2 is that for all the age groups there is a significant effect of initial cash demanded on settlements, with the largest impact on babies, where settlements rise by about $0.74 for every $1 of cash demanded initially. The consequence is that one also need examine the effect of damage caps on the initial demands which, as noted, may indicate bargaining rigidity of the claimant.

There is a substantial change in the post-reform period in initial cash demanded. For the pooled (N = 6,130) regression in Table 3.3 there is about a 40 percent reduction in initial cash demanded. So, when paired with the results of Table 3.2, the percentage total effect of the reform, $100(\delta_1 + \gamma_1\delta_2)/\mu_{Y(\text{pre-reform})}$, is to reduce settlements by an average of about 38 percent of the pre-reform average settlement, or by a total of $177,000. Once again the results are heterogeneous by age, so that the largest dollar effects in Table 3.3 are in the prime working years. This may indicate that working age people care about getting back to work quickly compared to those close to retirement who may be more willing to endure a protracted settlement period.

3.3.2. Time to Settlement

To examine the issue of how the reform affected time to payment we also estimated Cox (1972) proportional hazard models

$$(3) \qquad h_i(t) = h_0(t)\exp(\beta_{13}X_{it} + \gamma_3 C_{it} + \delta_3 R_t),$$

with standard errors calculated using the robust method in Lin and Wei (1979). Here the antilog of the coefficient of the reform dummy implies the hazard ratios in Table 3.4, which are revealed in the survival functions illustrated in Figure 3.3. Note, for example, that pre-reform virtually no case had settled by the 500 day mark, while post-reform about one-third had settled. Similarly, it took about 50 percent longer for half the cases to have settled pre reform versus post reform.

From the estimated hazard ratios in Table 3.4 we see that, on average, people settle about 50 percent faster with the largest effect (–60 percent) on cases involving infants. Again there is substantial heterogeneity in the estimated effect of the reform on time until settlement, as cases involving the elderly are settled 40 percent more quickly. Finally, we note that unlike the level of settlements in no case is the time to settlement affected by initial cash demanded so that there is no influence of the policy reform on time to settlement via a moderation of cash demanded channel.

3.3.3. Effect of the Reform on the Economic Value of Settlements

Using the procedure described in the Appendix we display in Table 3.5 the economic effects of the reform in the terms of its impact on the asset value of a malpractice settlement and its heterogeneity by age. Table 3.5 breaks the effect of the policy out by channels, the direct effect on the settlement amount, the indirect effect via decreased cash demands, and then the change in timing from speedier settlements.

For all ages, while speeding up the time to payment by about 420 days, the effect of reform on settlements is to reduce the present value by 36 percent.[25] Once again there is substantial heterogeneity by age. Persons in their 30s demand about $175,000 less and then have an average settlement that is about $103,000 lower that is paid only about 421 days (50 percent) faster so that the implicit asset value of the settlement is about 60 percent lower. The tort reforms are not welfare improving in a basic economic sense. One possible explanation for the heterogeneity by age is that claimants in their prime working age have a different level of relative risk aversion than those with injured children or the elderly. It is

[25] Present value calculations use the average of the interest rate on a 3-month T-bill over the time period of our sample.

also possible that working age claimants settle for less in an attempt to expedite the settlement process and return to work as quickly as possible.

3.3.4. Additional Dimensions of the Distributional Consequences of the Reform

There is much research demonstrating the usefulness of quantile regression in examining the distributional consequences of economic interventions in the labor market (Kniesner, Viscusi, and Ziliak 2010) and in the case of medical malpractice insurance (Viscusi and Born 2005). The standard quantile regression model has an expression for the fitted residual that in our case is

(4) $r_{it} = Y_{it} - \sum_j \beta_j x_{jit} + \gamma_4 C_{it} + \delta_4 R_t$ or

(5) $r_{it} = C_{it} - \sum_j \beta_j x_{jit} + \delta_5 R_t$.

Next there is a multiplier h_i where

(6) $$h_i = \begin{cases} 2q, & if\ \ r_i > 0 \\ 2(1-q), & otherwise \end{cases}$$

with q the quantile of interest. The quantile regression is then

(7) $$min_{\beta_j} \sum_i |r_i| h_i,$$

which is solved via linear programming (Armstrong, et al. 1975).

Recent research adds a parameter (τ) that, when minimized in conjunction with (4)-(7), reveals the most probable or maximum entropy quantile (Golan 2006, Bera et al. 2010).[26] In

[26] One can also intuit τ as a penalty for deviating from the median as the most likely quantile.

terms of policy interventions one should be particularly interested in the most likely effect size, which comes from the most likely quantile.

Table 3.6 presents the estimated maximum entropy quantile for the various age groups. The point of the exercise is to reveal more of the policy heterogeneity. Note that the estimated maximum entropy quantile is lower for older people. Although it is close to the median for ages 50-69 in no other age group is the median outcome the place in the fitted settlement distribution that is most likely.

There is a great deal of heterogeneity in the impact of the reform across conditional quantiles of cash demanded and settlement amounts. The differing effects of the policy are presented in Figure 3.4 for conditional quantiles of settlement amount and in Figure 3.5 for conditional quantiles of cash demanded for the pooled sample. The negative effect of the policy on settlement amounts peaks at the 30th conditional quantile and then drops off at the quantiles increase. For cash demanded the effect of the policy is monotonically increasing in magnitude with the conditional quantile. Because of the differing effects, if a part of the distribution other than the mean is most likely, then using that quantile rather than the mean will make a sizable difference in the value of the settlement to the most likely claimant.

The heterogeneity in policy effects and the difference it makes in focusing on the most likely place in the distribution of potential outcomes are highlighted in Table 3.7 where we compare estimated mean, median, and maximum entropy quantile malpractice reform effects on asset value lost. Note that for people in their 30s the most likely effect is less than half the mean effect. Alternatively, the most likely effect is much larger (−28 percent) than the mean effect (0) in the case of young people 3-19. It is also the case that (1) there is little heterogeneity in effect by age group for the vast majority of the groups and (2) the most likely quantile estimates are fairly similar to the estimates one would get from a median regression. In practice, a simple least absolute deviation regression trimming the outliers is an important improvement over OLS when estimating medical malpractice reform effects. The conclusion again emerging is that on pure economic asset returns grounds the policy is welfare reducing in that claimants would have benefitted economically from a slower larger settlement typical of the pre-reform period. It is also the case that infants and the elderly are not the hardest hit. In addition to infants having the smallest expected effects from damage caps the largest percentage asset loss is among people in their 50s.

3.3.5. Robustness Check

The final econometric issue we confront is whether our results are sensitive to small changes in the assumed settlement period window of three years. Table 3.8 presents settlement results for a 3.5 year time frame compared to a 3 year window, which enlarges the sample size by 50 percent. Note the similarity of results of interest, the estimated values of γ and δ, with those in Table 3.2.[27] Table 3.9 repeats the robustness checking exercise for the dependent variable of cash demanded by the claimant. Again, the results are similar to those found in the three year window.

3.4 Conclusion

Because of its many perceived benefits state legislatures have found tort reform attractive. Reforms such as damage caps and early offer systems speed up cases and help reduce caseloads in the courts. They also lower the size of claims, which possibly decreases so-called wasteful defensive medicine and decreases the related stress costs on physicians. Another touted benefit of tort reforms are that they cut down on claims that lack merit and

[27] Results not tabulated are similar for settlement windows of 3.25 or 3.75 years.

help prevent blockbuster jury awards that are perceived to increase the overall cost of health care. The benefits we have mentioned are not without a downside. Our evidence suggests that although injured parties who may desire quicker payment are indeed compensated more quickly after the reforms the cost of doing so is large, perhaps to the point that given the choice specified in clear economic terms they would prefer the previous system, particularly persons of prime working ages.

4. Medical Malpractice Damage Caps and the Price of Medical Procedures

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4.1 Introduction

The costs of the entire medical malpractice system are estimated to be around $55.6 billion in 2008 dollars (Mello et al 2010), a relatively small number when compared to all U.S. health care costs. However, medical malpractice tort reform is a contentious issue that garners a lot of public attention and is continually placed at the forefront of many states' legislative agendas. Since 2000, thirteen states have in some way changed their laws pertaining to medical malpractice liability. Most of these states enacted or modified a cap on non-economic damages awardable in medical malpractice tort cases.[28] The argument for non-economic damage caps is that they decrease the malpractice risk on physicians, and as such should decrease malpractice insurance premiums. To the extent that this cost reduction is passed through to consumers, the price of health care should also be decreased by caps on damages.

It has been loosely established that increases in malpractice risk to physicians increase the market price of health services. Baicker and Chandra (2004) find a slight effect of malpractice risk on the price of mammography, but no evidence of effects on the price of cesarean section (c-section) or angioplasty. Lakdawalla and Seabury's 2009 study shows that per-bed-day hospital expenditures are responsive to jury awards in medical malpractice cases. The most general evidence is a study by Danzon, Pauly, and Kington (1990) that demonstrates a direct link between the risk of being sued for medical malpractice and the fees charged by physicians.[29] There has been no prior empirical work linking tort reforms such as damage caps to changes in market prices. This study is the first to link the implementation of damage caps directly to changes in market prices, and serves as new

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[28] 31 states have some sort of cap on medical malpractice damages in place as of 2011.

[29] The Danzon, Pauly and Kington study is limited by its use of repeated cross section data and a small number of cross sections.

evidence on the relationship between the market price of medical services and malpractice risk on physicians in general.

I use a large data set of health insurance claims to private insurers along with variation in the implementation of state malpractice tort laws to estimate the effect of non-economic damage caps on the amount physicians charge to insurance companies for specific procedures and the amount that insurance companies agree to reimburse doctors for those procedures.[30] I pay special attention to procedures in the field of Obstetrics and Gynecology (Ob-Gyn) because of its status as a high malpractice risk specialty.[31] I also look at several procedures in other fields. My estimates of the effect of non-economic damage caps are obtained using two variants of a difference in differences method. The first variant uses within state variation to identify the effect of damage caps on the price of services. The second variant uses within metropolitan statistical area (MSA) variation, gaining identification from MSAs that straddle state borders.

Of particular interest are any differences between changes in the allowed amount (the price paid out by an insurance company and the price observed in the market) and changes in the amount that physicians bill insurance companies for the same specific procedures. Differences in these two values show whether physicians lowered the price they asked for in response to a cap on damages, or if insurance companies lowered the amount that they paid out. These estimated changes in tandem provide an intriguing look into how shocks in physician costs are translated into the market for physician services.

[30] Data Source: FAIR Health, Inc., an independent, New York nonprofit corporation.
[31] Phelps (2003) shows that physicians in the field of Obstetrics and Gynecology face a much higher ratio of malpractice insurance premiums to the rate of negligent events than other specialties.

I find that damage caps have a sizable impact on the allowed amount of some, but not all services. Estimated reductions in the allowed amount in response to the implementation of damage caps are as large as 14.5 percent. I find much smaller and often no changes in the amount that physicians charge insurance companies, suggesting that the price changes are driven by insurers bargaining down the price of care. The greatest part of the price reduction occurs in urban areas, which is unsurprising as most procedures are performed in urban areas and as urban areas have more inelastic demand for medical services.

4.2 Background

Suits for medical malpractice are filed under a state's tort law. An individual can bring a case against a physician that treated her by claiming that the physician acted negligently and that the negligence led to an adverse outcome. Three types of damages are awardable: economic damages for lost income and medical expenses, non-economic damages for pain and suffering, and on rare occasion punitive damages if it can be shown that the physician acted in a criminal manner.[32] Because being sued is a rare event that is potentially quite costly, physicians are required to carry insurance against medical malpractice. Medical malpractice insurance is not experience rated on the individual level, and as such premiums for insurance are reflective of the risks inherent to all members of a physician's specialty in their specific state.

Despite the presence of malpractice insurance, the risk of suit is still something that physicians will take great pains to reduce, as the experience of being sued is both time-

[32] Unlike economic damages which measure lost wages, and punitive damages whose values are dictated by law, there is no clear standard for valuing non-economic damages. As such, there is the greatest variation in non-economic damages.

consuming, stressful, and carries large additional costs if the physician hires his own lawyer. The literature has identified three margins of response by which doctors adjust their behavior in response to the risk of suit: doctors change how they practice, where they practice, and they try to pass on the cost of the risk to consumers. Though all these outcomes are of importance to understanding the effect malpractice risk and tort reform on the market, this study will focus primarily on the last of the three.

The literature on malpractice risk and the price of medical services is small, largely because comprehensive data on physician pricing, and the interaction between physicians and health insurance companies is difficult to come by. Baicker and Chandra (2004) find a slight effect of malpractice risk on the price of mammography, but no evidence of effects on the price of c-section or angioplasty. Also, the work by Lakdawalla and Seabury (2009) suggests that per-bed-day hospital expenditures are responsive to jury awards in medical malpractice cases. The most general evidence is in work by Danzon, Pauly, and Kington (1990). They use three survey cross sections to demonstrate a direct link between the risk of being sued for medical malpractice and the fees charged by physicians.

4.2.1. Damage Caps and the Price of Medical Services

Though there has been no previous empirical work linking damage caps to price reductions for medical services, there is much theoretical evidence that would suggest that this would be the case. Danzon, Pauly, and Kington (1990) present some simple models in which the price of medical services is linked to the "generosity" of a legal system in a positive way (i.e. larger awards would cause higher prices). More recently, King (2010) develops a model in which the cost of malpractice insurance, which is tied to malpractice risk, is passed through in full to consumers.

Thaler and Sunstein (2008) suggest that malpractice liability should be thought of in a hedonic framework. Patients pay for medical services; which are a bundle of attributes that includes the right to sue for negligence. To the extent that the right to sue is curtailed their willingness to pay for medical services should decrease. Likewise doctors would be willing to charge less if they do not face as much malpractice risk – this could be viewed as either a shift in supply or as a compensating wage differential. The size of the shift in prices would depend on the relative elasticities of supply and demand, but would be unambiguous in direction: increases in the generosity of the tort system would increase prices, decreases would decrease prices.

Missing in the above models is the inclusion of private health insurers in the market. Observed prices in the marketplace are the result of a bargaining game between physicians and private health insurers. Thus, decreases in prices may be magnified if insurance companies have bargaining power.

To illustrate, consider a Cournot model of oligopoly as a representation of physicians in the marketplace.[33] Physicians certainly are not monopolists, but at the same time do not face pure competition either. The markup in a Cournot model is,

$$m = \frac{p - mc}{p} = -\frac{1}{N\eta_D} \tag{1}$$

where *m* is the markup, *N* is the number of physicians in the market and η_D is the price elasticity of demand. The markup is the percentage of the price that is over and above the marginal cost of production. The markup is a surplus that physicians receive, and is divided in a bargaining game with health insurance companies. Hence, the amount insurers reimburse physicians is marginal cost plus whatever percentage of the markup that

[33] A thorough treatment of the Cournot model can be found in Mas-Colell, Whinston and Green (1995).

physicians receive in the bargaining game. When insurance companies have more bargaining power, they receive a larger piece of the surplus. It should also be noted that markups are bigger in places with more inelastic demand, such as urban areas.[34] This is seen easily, by taking the derivative of the markup with respect to η_D.

$$\frac{\partial m}{\partial \eta_D} = \frac{1}{N\eta_D^2} \qquad (2)$$

The result is positive, indicating that as η_D increases (recall that demand elasticity is negative), or as demand becomes more inelastic, the markup increases.

There are two channels by which prices could decrease from a cap on noneconomic damages. First, the cap will decrease the cost of malpractice insurance and the risk of being sued, which will lower marginal cost and by construction, the price of care.[35] The second channel is that insurers may gain bargaining power from the implementation of a cap. For example, doctors could no longer claim at the bargaining table that their cost of malpractice insurance is quite as high and that they need a higher price to survive.

An increase in insurer bargaining power would result in greater price decreases in places with inelastic demand (cities) relative to those with more elastic demand (rural areas). Cities would have greater markups than rural areas, and thus an equivalent increase in insurer bargaining power in cities and rural areas would have a greater price decrease in urban areas simply because there is more surplus to be divided.

For the above mechanism to work it is necessary for caps on non-economic damages to curtail patients right to sue (which they do by definition), and for caps on non-economic

[34] Urban areas have a much larger percentage of the population which has health insurance (Ormond, Zuckerman, and Lhila 2000) which makes the demand for medical services more inelastic (Manning et al 1987).
[35] Premiums for malpractice insurance scale with the size of the practice, so medical malpractice insurance premiums are indeed a marginal cost.

damages to lower the risk imposed on physicians. Sloan and Chepke (2008) provide a review of the literature on the effects of tort reforms on the legal system and malpractice insurance premiums. Though there is no evidence that damage caps decrease claim frequency significantly, it has been shown that damage caps do indeed decrease claim payments and malpractice insurance premiums in a significant fashion.[36] Therefore, the cost of malpractice risk to physicians should in some part be passed on to consumers.

4.3 Data

The data I use come from several sources. Price data comes from the Medical/Surgical module of FAIR Health database of private health insurance claims; tort law data was created by merging information from the McCullough, Campbell & Lane LLP records with the records of the American Tort Reform Association (ATRA). Additional time varying state level characteristics come from the Health Resources and Services Administration's (HRSA) Area Resource File (ARF). These data sources were combined to create several data sets spanning the years 2003 to 2007.

The Medical/Surgical module of the FAIR Health database reports private health insurance claims for individual patients and accounts for roughly 28 percent of the total number of private insurer claims in the United States in a given year. Individual procedure types can be identified via the American Medical Association's Current Procedure Terminology codes. Each claim's date is known, and they can be matched to states and MSAs via three digit zip codes. The three digit zip codes are flagged as urban or rural based

[36] Damage caps and early offer reforms have also been shown to decrease the amount that plaintiffs ask for by Hyman et al (2009), as well as in the previous chapter.

on Census Bureau definitions.[37] The FAIR Health database is very large, containing several billion observations in totality.

For analysis I use data on eight procedures that vary in their risk. The first four procedures are all commonly performed by Ob-Gyns: c-sections, vaginal deliveries, transvaginal ultrasounds on non-pregnant women, and abdominal ultrasounds on women in the first trimester of pregnancy. I put extra focus on Ob-Gyns because of their place in the medical malpractice literature as a specialty of particular interest. The second four procedures are performed commonly by non-Ob-Gyns: coronary artery bypass graft (CABG) surgery, chest x-rays, fifteen minute office visits and tetanus shots.

Delivery is by far the riskiest procedure performed by Ob-Gyns. Vaginal delivery can often have complications, and if they occur, then Ob-Gyns are often sued on the grounds that they waited too long to deliver and did not perform a c-section, which is a far less risky procedure. For non-Ob-Gyns CABG surgery is also quite common, usually performed by cardiothoracic and vascular surgeons, and also carries with it a fair amount of risk for the patients.

Transvaginal ultrasounds and abdominal ultrasounds are both very common diagnostic tests performed by Ob-Gyns. Transvaginal ultrasounds test the health of the reproductive system before conception and abdominal ultrasounds test the health of the early fetus. Similarly, chest x-rays are incredibly common diagnostic tests used by non-Ob-Gyns, and are used to diagnose a multitude of problems in the chest cavity. Fifteen minute office visits and tetanus shots are two very common general procedures performed by all

[37] Each zip code is flagged by the Census Bureau as either urban or rural based on population density and other criteria (for example airports or city parks which have no population density are still tagged as urban). I consider a three digit zip code to be urban if over fifty percent of that thee digit zip code's population resides in urban five digit zip codes.

specialties and general practitioners respectively. Tetanus shots do carry a small amount of risk: there has been a history of malpractice cases involving children suffering adverse side effects from the vaccine.

For all of the above procedures, I can observe two outcomes of interest. The main outcome of interest is the amount the insurance company pays the physician (henceforth the allowed amount). The allowed amount is the outcome of a bargaining process between the insurer and health care provider and is the price actually observed in the market. The second outcome is the amount the health care provider submits to the health insurer (henceforth the charged amount). The charged amount is the physician's initial offer in the price bargaining game, and is also what the market price would be for uninsured individuals.

For both variables any negative values (errors in the data) or zero amounts (situations in which the insurer denied payment) were dropped, resulting in a subset of the data that only looks at situations where services were paid for. I am also able to observe if the insurer is a health maintenance organization (HMO), preferred provider organization (PPO) or point of service plan (POS). In many cases the insurer type is not reported, these cases are grouped together into a plan type unknown category. The top half of Table 4.1 reports summary statistics for the eight procedures. All prices are expressed in constant 2002 dollars using the consumer price index.

McCullough, Campbell & Lane LLP is a law firm in Chicago that specializes in insurance law. It maintains an online database of current state laws pertaining to medical malpractice tort law. Further, the ATRA keeps an online record of all state legal changes that impact medical malpractice tort law. Using McCullough Campbell & Lane LLP and ATRA records, I constructed a database of all state laws regarding medical malpractice torts from 2003 to 2007. I then merged the database of tort laws with subsets of the FAIR health data. Table 4.2 reports all changes in tort law during between 2003 and 2007.[38]

HRSA publishes the ARF annually. It includes information on the health services market as well as general demographic information taken from the Census Bureau. I use yearly measures at the state level of the number of physicians, the general population, and median household income from the ARF. These variables are summarized at the bottom of Table 4.1.

4.4 Descriptive Results

A general sense of the effect of implementing a damage cap can be gained by a comparison of two similar states, one that implements a cap, and one that does not. Figure 4.1 shows the average allowed amount for an abdominal ultrasound during the first trimester of pregnancy for North and South Carolina. South Carolina implemented a cap on non-economic damages which came into effect in 2005, North Carolina had no cap. South Carolina appears to experience a drop in the average allowed amount for an ultrasound after the cap on damages is enacted, whereas no such drop is observed in North Carolina. In fact, prices in North Carolina were rising during this time.

The average price of an ultrasound was \$155.19 in South Carolina before the cap was enacted and \$127.15 after the cap was enacted. In North Carolina the average price was \$113.31 before the cap was enacted and \$132.48 after the cap was enacted. This yields a rough difference in differences of -\$47.20 or a 30.5% decrease in the price in South Carolina

[38] The shaded out reforms are those enacted for 2003 or those reforms that are not caps on non-economic damages, the variation from the shaded reforms cannot be used for identification of the effect of a damage cap.

relative to North Carolina. This is by no means a well identified result, but it does give a sense of how prices move in response to a cap on non-economic damages.

4.5 Methods

I identify the effect of non-economic damage caps on the price of medical services using within state variation. This is done by estimating the following fixed effects model.

$$\ln(P_{ist}) = \alpha + \beta X_i + \delta C_{st} + \gamma K_{st} + \rho_s + \phi_t + \varepsilon_{ist} \qquad (3)$$

The dependent variables used are the log allowed amount for a medical service or the log charged amount, $\ln(P_{ist})$, reported by the individual claim *i*, in state *s*, in year *t*. $\ln(P_{ist})$ is a function of the a vector of claim characteristics X_i, which includes the type of insurance plan as well as if the procedure was performed in an urban area, a dummy variable for if the state has a cap on non-economic damages C_{st}, a vector of state characteristics including other tort laws K_{st}, a state fixed effect ρ_s, a year fixed effect ϕ_t, and a random error term ε_{ist}. The coefficient δ that is associated with the implementation of a damage cap will give the effect of the cap's implementation on the price of the service. It is possible for an omitted variable to bias the estimate of δ, if it was correlated with the price of medical services and with the implementation of each individual state cap.

The above identification strategy also relies on the assumption that there were no pre-existing state specific trends that could explain changes in price attributed to the implementation of caps on non-economic damages. This parallel trends assumption proves difficult to test with a maximum of two years of data before a cap was implemented. Evidence from Malani and Reif (2010) shows that many medical malpractice tort reforms have sizable anticipation effects, so a test of the parallel trends assumption would likely falsely reject the null of parallel trends in the period before the cap was implemented.[39] It should be noted that the existence of differential trends where the state enacting the damage cap has falling prices is unlikely, as caps tend to be passed in response to prolonged increases in the price of medical services.

The sample sizes used for analysis can be quite large, in some cases over 250 million observations. When sample sizes are so large, the usual critical values may not be entirely appropriate, as the chance of finding statistical significance grows disproportionately to the chance of rejecting a null that is indeed true. To aid in interpretation I also report statistical significance based on the Leamer-Schwarz critical value as suggested by Deaton (1997). These critical values are set at $\sqrt{\ln(n)}$, where n is the sample size, and are more appropriate for interpreting the significance of enormous sample sizes. However, in smaller samples, the Leamer-Schwarz critical value is extremely conservative, and the usual critical values are more appropriate for interpretation.

4.6 Results

At the state level, many allowed amounts decrease when caps on noneconomic damages are enacted. State level estimates of the effect of implementing a damage cap on the allowed amount for Ob-Gyn procedures are reported in Table 4.3A. Each column of Table 4.3A reports results from a separate regression for the allowed amount for a specific procedure. Left to right the procedures reported are: c-section, vaginal birth, transvaginal ultrasound, and abdominal ultrasound in the first trimester. The table only reports the effect of the cap and the constant term, estimates of the effects of control variables and fixed

[39] The above identification strategies are incapable of identifying anticipation effects, and as such I underestimate the effect of a cap on the price of medical services.

effects are suppressed.[40] The first row gives the estimated effect of the cap in log points, followed by the robust standard error clustered at the state level in the second row. The third row gives the percent change in the price as a result of the cap. The allowed amount for a vaginal birth, transvaginal ultrasound and abdominal ultrasound in the first trimester all show a negative and significant reaction to the implementation of a cap on non-economic damages. There was not a significant effect on the allowed amount for a c-section, but the estimate is suggestively negative.

Table 4.3B reports estimates of the effect of implementing a damage cap on several non-Ob-Gyn procedures. The layout of Table 4.3B is identical to the layout of Table 4.3A, with the exception of the procedures reported. From left to right the procedures are: CABG, chest x-ray, 15 minute office visit, and tetanus shot. Only the estimates for the allowed amount for a CABG surgery and a tetanus shot show a negative and significant reaction to the implementation of a cap on non-economic damages. The effect on the allowed amount of a CABG surgery is much larger in magnitude than those estimated for Ob-Gyn procedures.

The decreases found at the state level in the allowed amounts are not found in the charged amounts. The effect of a damage cap on the price charged by physicians to the insurance company is reported in Tables 4.4A and 4.4B. Tables 4.4A and 4.4B are identical in layout to 4.3A and 4.3B. The only result that shows up as statistically significant is that for 15 minute office visits. This estimate is positive and should be disregarded, at the regression is run on a sample of over 250 million observations and while significant based on usual measures is not statistically significant based on the Leamer-Schwarz critical value. All

[40] Full regression results are available upon request.

other estimates show no effect of damage caps on the amount doctors submit to insurance companies.

4.6.1. Urban vs Rural Differences

To further explore how price changes vary between urban and rural areas, I also estimate the following equation.

$$\ln(P_{ist}) = \alpha + \beta X_i + \delta_1 UC_{ist} + \delta_2 RC_{ist} + \gamma K_{st} + \rho_s + \phi_t + \varepsilon_{ist} \qquad (4)$$

Equation (4) is similar to equation (3), but has two variables of interest: UC_{ist}, a dummy variable for if the state has a damage cap *and* the procedure was performed in an urban area, and RC_{ist}, a dummy variable for if the state has a damage cap *and* the procedure was performed in a non-urban area. The coefficients of interest δ_1 and δ_2 give the effect of a damage cap on procedures in urban and rural areas respectively. In the above specification, the vector X_i does not include a dummy for whether the procedure was performed in an urban or rural location.

Tables 4.5A and 4.5B report results from equation (4). The first three rows show the effect of a cap on noneconomic damages on the allowed amounts in urban areas, and the next three rows show the effect in non-urban areas.[41] The urban area estimates mirror the estimates for states as a whole reported in Tables 4.3A and 4.3B, but with slightly larger magnitudes. Estimates for non-urban areas show no reaction to the implementation of damage caps. This suggests that the effects found in Tables 4.3A and 4.3B are almost entirely driven by price changes in cities.

[41] Results for charged amounts are available upon request.

4.6.2. City Level Estimates

An alternative approach to identification of the effect of the damage cap is to use variation within a given city. I do this by restricting my sample to the 11 MSAs that cross the borders of the states that enacted caps after 2003 and estimating the following model.

$$\ln(P_{imst}) = \alpha + \beta X_i + \delta C_{st} + \gamma K_{st} + \psi_{mt} + \varepsilon_{imst} \qquad (5)$$

Equation (5) is similar to equation (3) with two differences, the new subscript m, denotes MSA, and the state and year fixed effects have been replaced by ψ_{mt} a vector of MSA by year fixed effects. This specification gains identification by comparing prices within the same MSA but on different sides of the state border. This removes a great deal of variation that may be omitted in the previous state level analysis. Because equation (5) controls for MSA specific trends, an omitted variable would need to change mid-year within an MSA while at the same time being correlated with both the price of medical services and the implementation of a damage cap to create bias. Such omitted variables are far less likely to occur that the type of omitted variables that would bias equation (3). The border model in equation (5) does have a major drawback: since the model is dependent on variation found exclusively in urban areas, the results may not generalize. However, as shown in Tables 4.4A and 4.4B changes in the price are driven primarily by urban areas, so external validity (from the city to the state level) is not a big concern.

The cross border MSA analysis shows a much larger effect of implementing a damage cap. Estimates of the effect of a damage cap on the allowed amount for procedures using MSAs that cross state borders are reported in Tables 4.6A and 4.6B. Tables 4.6A and 4.6B follow the same layout as Tables 4.3A and 4.3B, but specification (5) is now used and standard errors are now clustered at the MSA level. All of the effects are negative and much greater in magnitude than those estimated using state level variation. Only the estimate for the price of a CABG surgery is statistically insignificant, which may be an artifice of the sample size being severely reduced when transitioning from the state level to the MSA level for this procedure.

Tables 4.7A and 4.7B report results for regressions where the charged amount is the dependent variable. The estimates of the effect of damage caps on the amount charged by physicians are insignificant for procedures in the Ob-Gyn specialty. The estimates for procedures not in the Ob-Gyn specialty are very different. The non-Ob-Gyn procedures show negative and significant reductions in the amount charged, ranging from approximately one third the magnitude of the reduction in the allowed amount to approximately two thirds the magnitude of the reduction in the allowed amount.

4.6.3. Heterogeneous Effects on the Allowed Amount

It is possible that the effect of a damage cap on the allowed amount of a medical procedure may vary across its price distribution. Physicians who have a higher price for their services may realize a greater (or smaller) percentage price reduction after a cap is enacted. To allow for this possibility I re-estimate equation (5) using quantile regression. I am then able to observe what the effect of a damage cap is on each conditional quantile in the procedure allowed amount distribution. The results of the quantile regressions are reported in Figures 4.2A for Ob-Gyn procedures and 4.2B for non-Ob-Gyn procedures.

For Ob-Gyn procedures there is a dip in the magnitude of the effect of a damage cap around the 60th percentile in the price distribution. This suggests that Ob-Gyns who have prices at the high and low end of the distribution have larger effects on prices than those in the middle. The exception to this is c-sections, which show increasing price reductions as the quantile increases. General procedures do not show any strong pattern, although there does appear to be a slight positive relationship between the size of the price reduction and quantile for chest x-rays.

4.6.4. Falsification Test

One way to test the robustness of these estimates is to see if I can find an effect of caps that never happened. If there are significant effects of nonexistent caps on the price of medical services, then the above results would be called into serious doubt. To do this I omit the states that enacted caps from the analysis and test the effect of the enacting of three false caps in states that did not border states that enacted true caps on the price of services. I placed false caps in New York and Washington in 2005, and Nevada in 2004. These caps correspond to true caps enacted in Georgia and South Carolina in 2005, and in Illinois in 2004.

The effect of the false caps is reported in Table 4.8A for Ob-Gyn procedures, and Table 4.8B for non-Ob-Gyn procedures. Tables 4.8A and 4.8B have the same layout and specification as Tables 4.3A and 4.3B, but use false caps instead of real caps, and do not report percent changes in their allowed amounts. Not only is there is no significant effect for any of the procedures, but with the exception of 15 minute office visits, all of the standard errors are larger than the estimates.

4.7 Discussion and Conclusion

My analysis shows a relationship between caps on non-economic damages and the pricing of treatments by physicians, providing strong evidence that malpractice risk is passed through at least to insurance companies and possibly to consumers. My estimates are in some cases even larger than the effects of defensive medicine on expenditures found by Kessler and McClellan (1996). This raises the question of whether quantity effects (which may be defensive medicine) of medical malpractice risk are as large in magnitude as price effects. Further work would need to estimate both effects in tandem to satisfactorily answer this question.

Damage caps have a much larger effect on the allowed amount of a procedure than on the amount that a physician charges to the insurance company. This suggests that the mechanism by which prices are reduced is through the bargaining game between physicians and insurers. Non-Ob-Gyns are willing to decrease their charged amounts slightly in response to a cap, and the full extent of the market price reduction is felt after the insurance company decides how much it is willing to reimburse. This is exacerbated with Ob-Gyns, who appear to be unwilling (or possibly unable) to decide to charge less after a damage cap is implemented, and all of the market price reduction comes from the insurance company.

There does not appear to be a consistent link between the risk inherent in a particular procedure, and that procedure's price sensitivity to damage caps. For Ob-Gyns, birthing is the most risky procedure performed, but the price of ultrasounds is far more sensitive to the implementation of damage caps than the price of birthing. It is possible that Ob-Gyns spread the cost of the risk inherent in practice in general across the pricing of all procedures.

Overall, this work shows that the prices of medical services respond to changes in the strength of the malpractice liability system. The implementation of damage caps yields sizable reductions in the cost of some procedures, especially for those procedures in the Ob-Gyn field or for risky procedures such as CABG surgery. The costs savings from damage caps may in fact be quite sizable. For example, based on my results the cost savings from a caps' price reduction for office visits alone in the state of Illinois in a single year comes to just over $7.5 million. For all 8 procedures estimated that number grows to almost $11.7 million. Further study of all medical procedures in the United States would be needed to know which procedures are price responsive, and which are not.

Finally, the estimated effect of caps does not mean that the same result would necessarily be felt if such caps were implemented in uncapped states or at a national level, as uncapped states may have smaller shares of malpractice risk being passed on to consumers. That is to say that states which pass caps may be self-selected on the proportion of prices which are attributed to malpractice risk pass through.

Appendices

Appendix 3: Asset Value Calculations

Asset Value (Table 3.5) Generation

Table 3.5, Column 2 – Average of Settlements within 3 years for given age group

Table 3.5, Column 3 – Estimated policy effect from Table 3.2, set equal to zero if not significant

Table 3.5, Column 4 – Estimated policy effect from Table 3.3, set equal to zero if insignificant, multiplied by estimated effect of cash demanded in Table 3.3

Table 3.5, Column 6 – Column 4 multiplied by estimate from Table 3.4

Table 3.5, Column 7 – Sum of columns 2, 3 and 4, adjusted for change in timing of payment in Table 3.6

$$\frac{(col2 + col3 + col4)}{1.0141^{(\frac{Col6}{365})}}$$

Table 3.5, Column 8 – divide column 7 by column 1. Multiply by negative 1

Alternate Asset Value (Table 3.7) generation

Table 3.7, Column 1 – Same as Table 3.5, Column 8

Table 3.7, Column 2 – Uses median techniques, generated using same logic as Table 3.5, the differences are:

- Column 2 of Table 3.5 uses the median settlement amount

Table 7, Column 2 – Uses MEQ techniques, generated using same logic as Table 3.5, the differences are:

- Column 2 of Table 3.5 uses the MEQ settlement amount
- Columns 3 and 4 of Table 3.5 come from MEQ regressions (MEQ's vary based on age group)

Figures and Tables

Figure 2.1: Bid-Functions for Several Income and Taste Classes – Before and After the Top 10% Plan

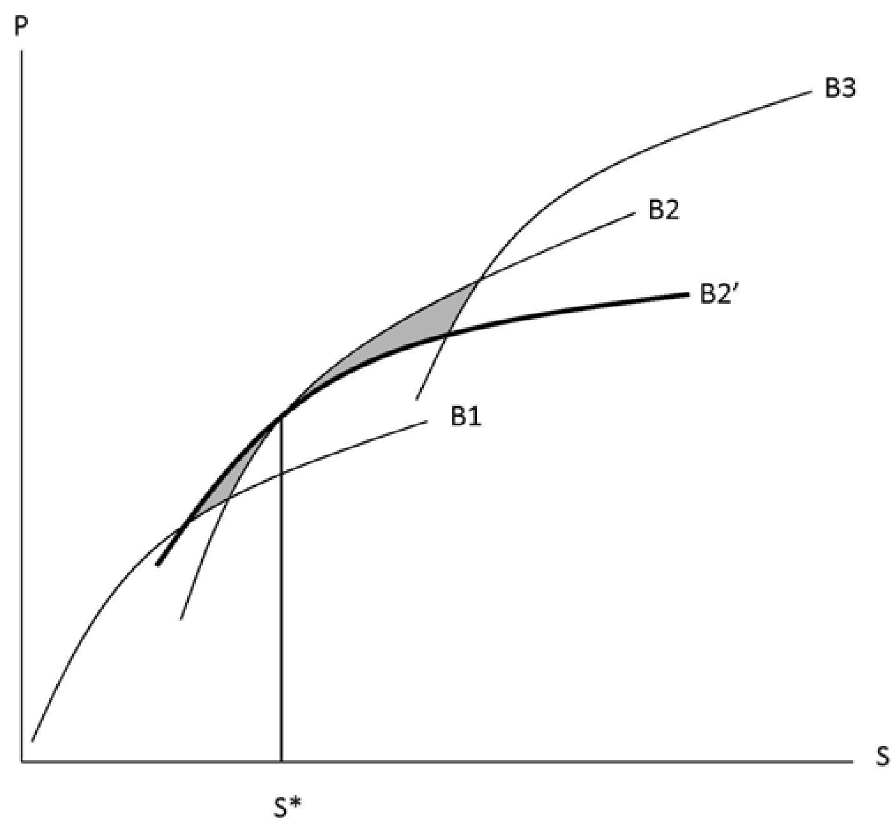
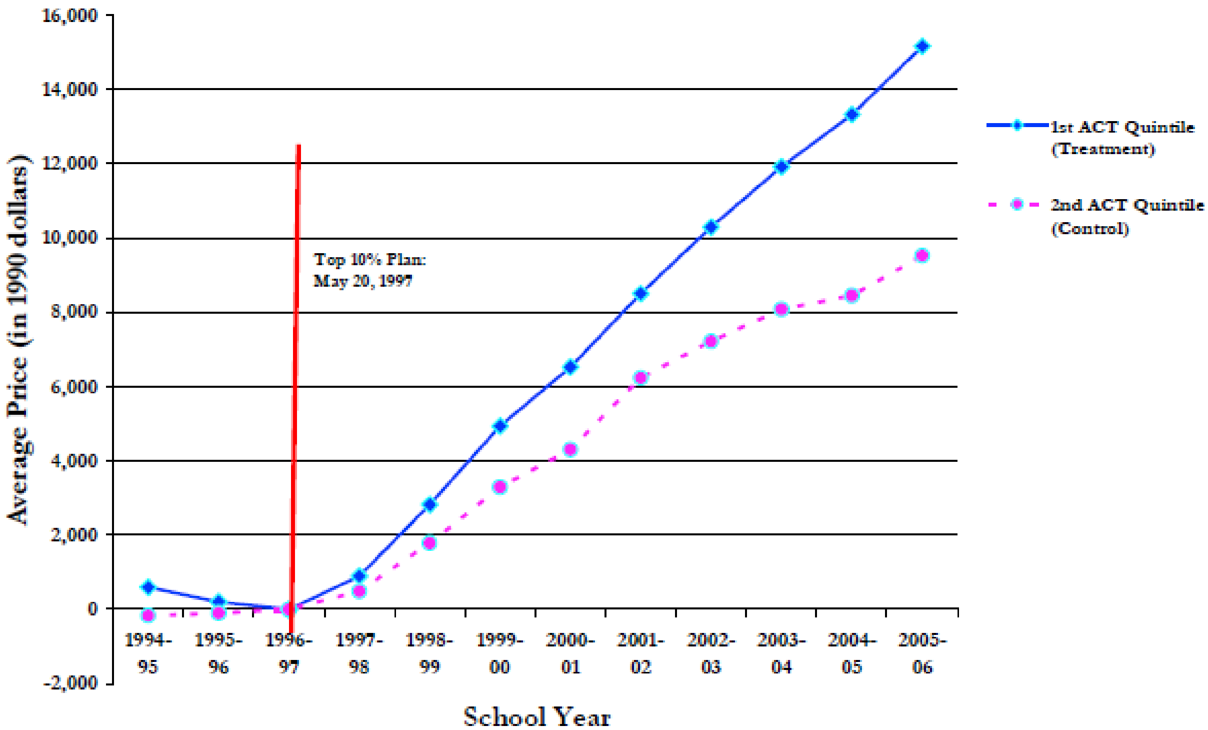


Figure 2.2: Average Housing Price by School Quality (ACT Quintiles)



Notes: 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores.
Source: Texas Comptroller Property Tax Division and the Academic Excellence Indicator System from the Student Assessment Divisions of the Texas Education Agency, 1994-95 to 2005-06. Tabulations by authors.

Figure 2.3: Bottom Two Quintiles of School District Quality –Percent Growth in Home Values

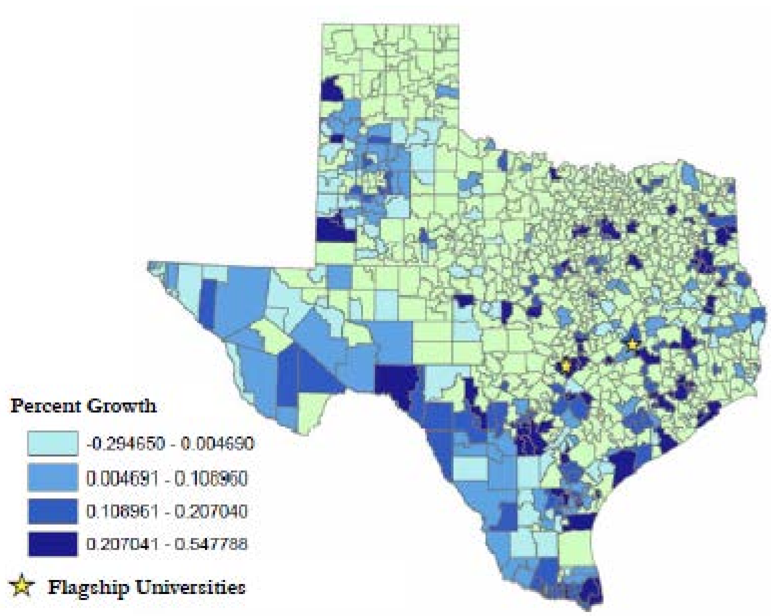


Figure 2.4: Bottom Two Quintiles of School District Quality –Percent Growth in Home Values for Non-monopolistic Counties

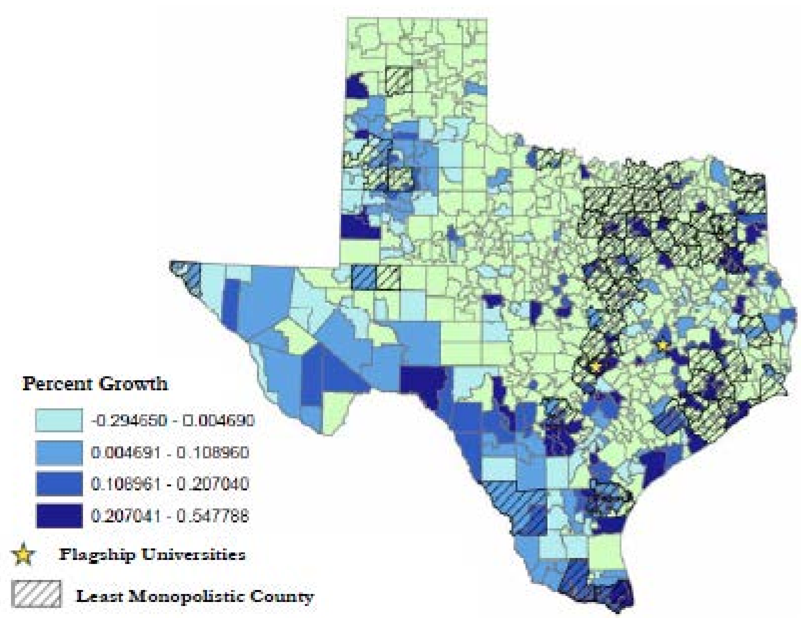


Figure 3.1: Minimum Acceptable Settlements for Two Risk Preferences

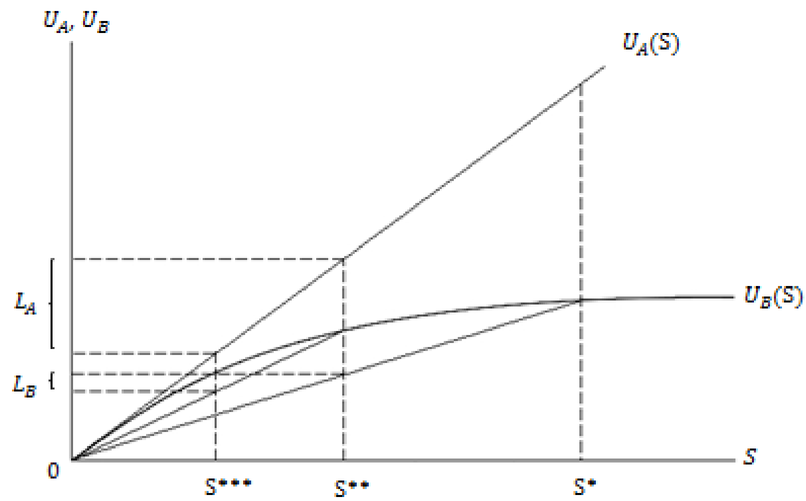


Figure 3.2: Density Plot of Years to Settlement and Years to Verdict

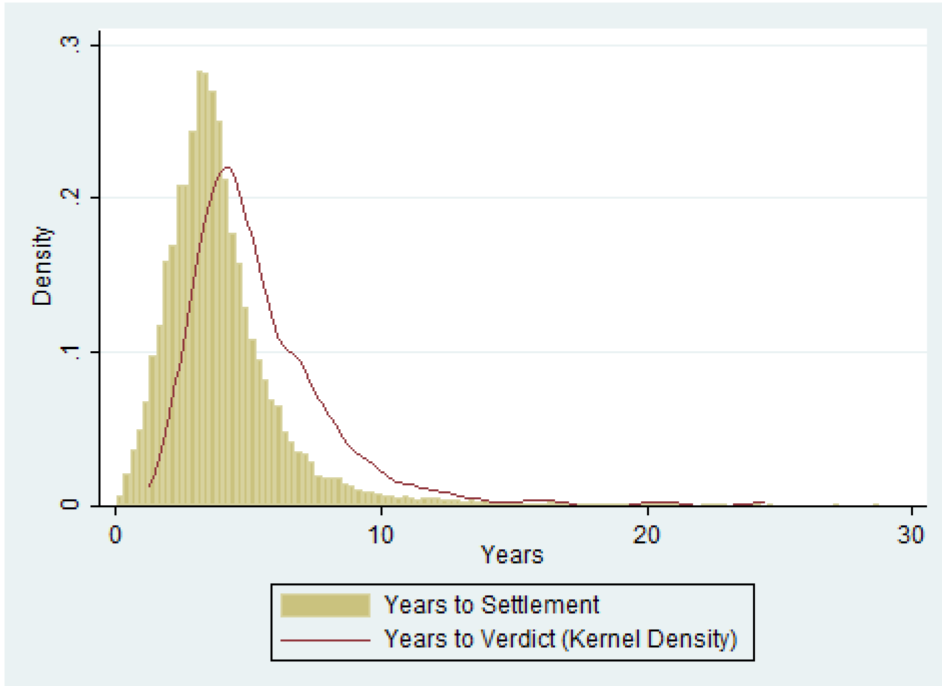


Figure 3.3: Survival Function of Medical Malpractice Cases

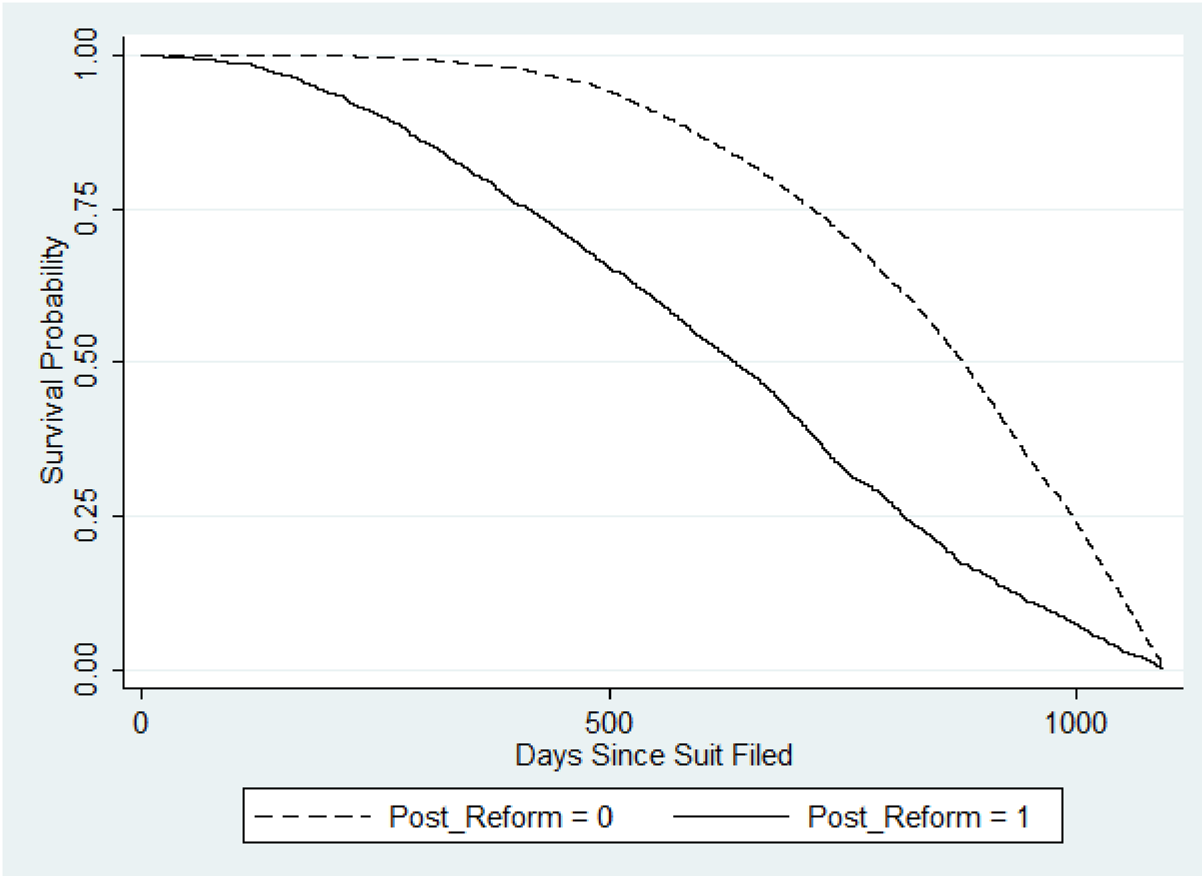


Figure 3.4: Effect of Policy on Settlement Amount Conditional Quantiles

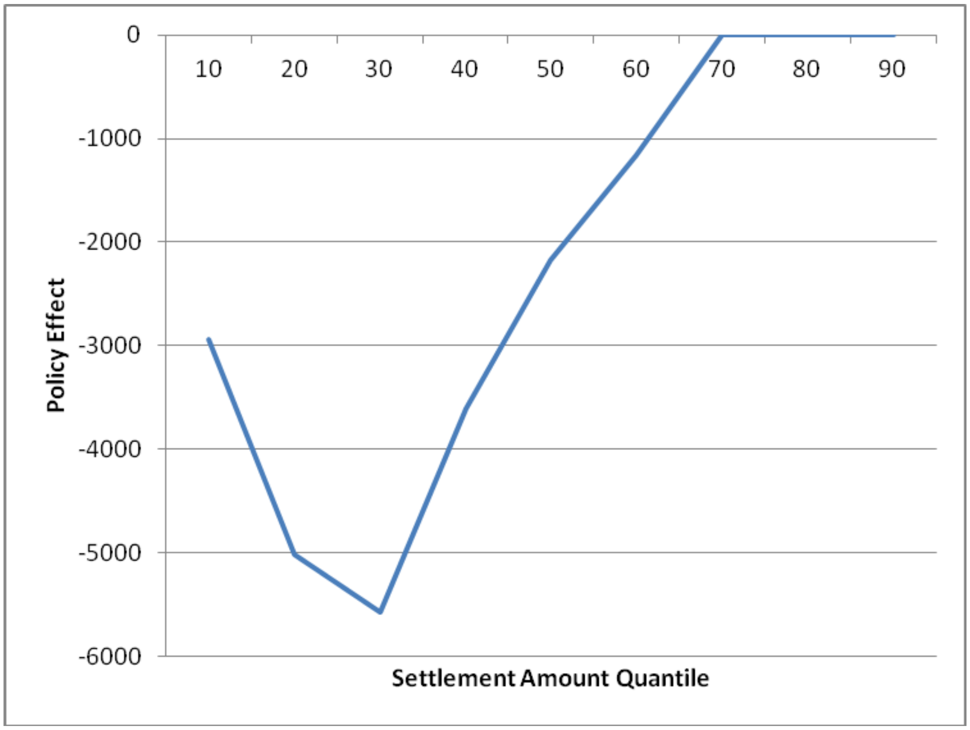


Figure 3.5: Effect of Policy on Cash Demanded Conditional Quantiles

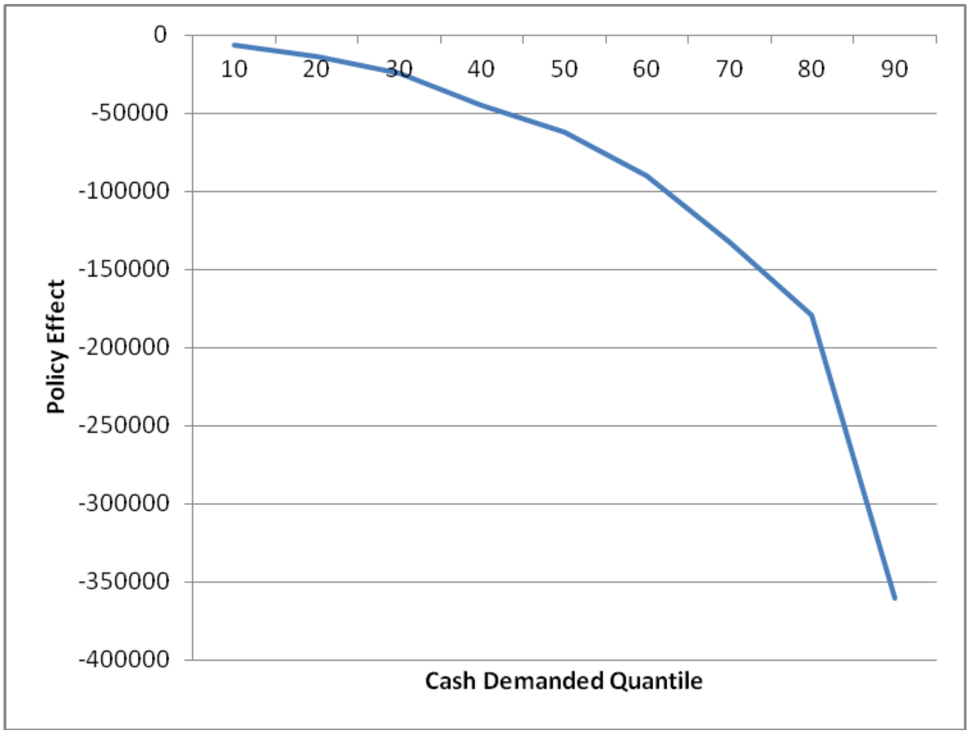


Figure 4.1: Average Allowed Amount for an Abdominal Ultrasound (1st Trimester)

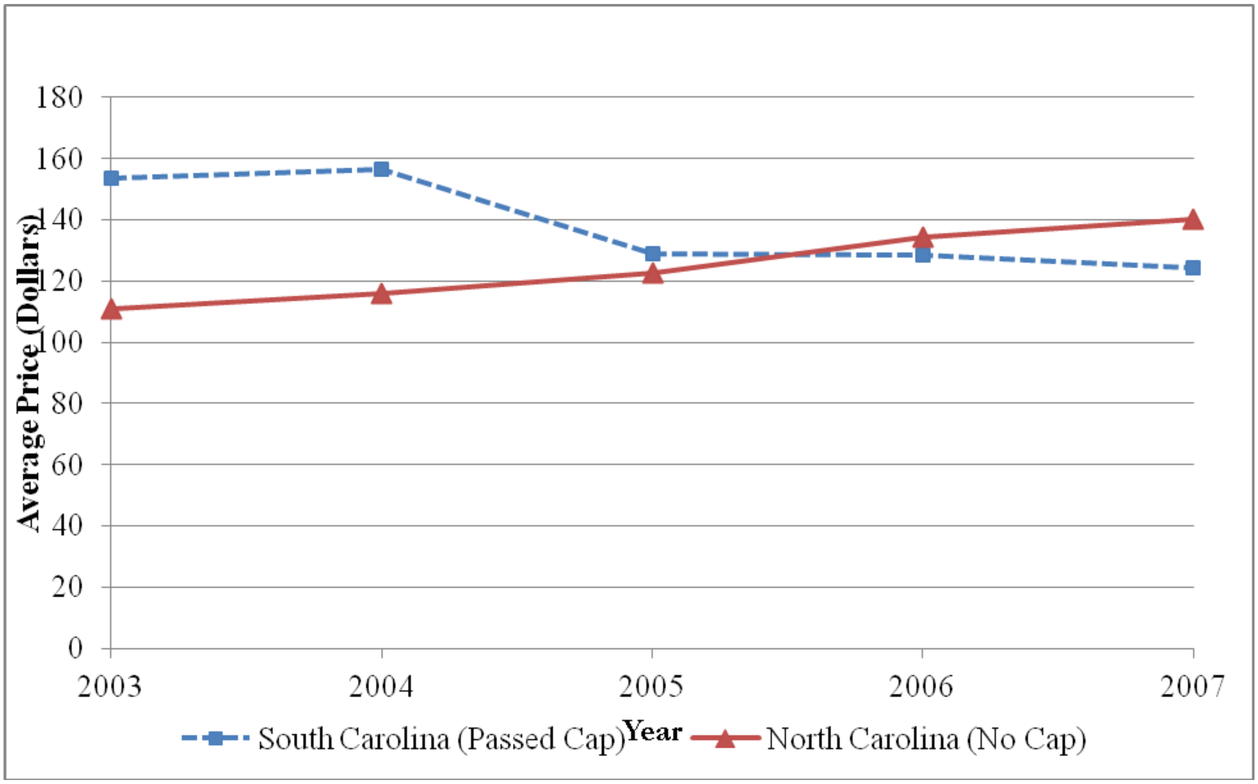


Figure 4.2A: Allowed Quantile Regression Results Ob-Gyn Procedures - Cross Border MSAs

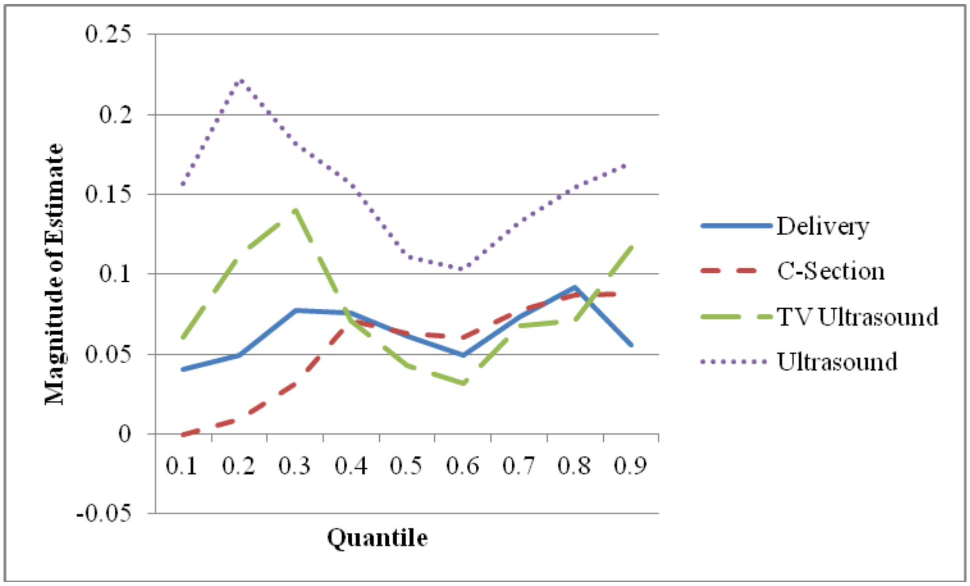


Figure 4.2B: Market Price Quantile Regression Results Ob-Gyn Procedures - Cross Border MSAs

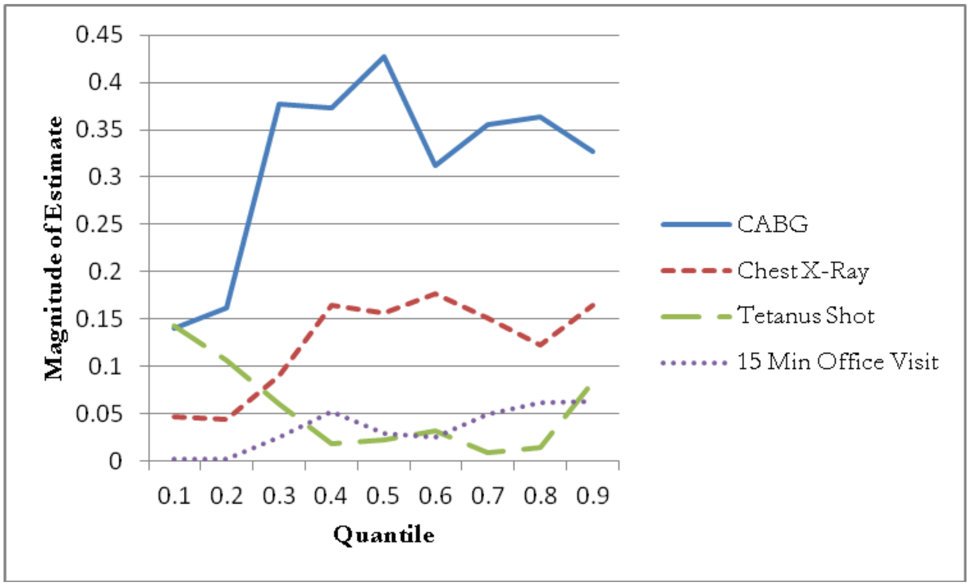


Table 2.1: Descriptive Statistics - Means and Standard Deviations

	School Quality Quintiles Based on ACT Scores		
	Both	**2nd Quintile**	**1st Quintile**
	Subsample	*(Control)*	*(Treatment)*
Dependent Variables			
Average Home Value (in thousands)	38.47	35.18	41.96
	(26.49)	(24.93)	(27.63)
High School Demographics			
Percent Minority Students	0.690	0.514	0.876
	(0.240)	(0.177)	(0.136)
Percent Disadvantaged Students	0.571	0.466	0.682
	(0.187)	(0.137)	(0.167)
Percent Gifted Students	0.094	0.094	0.093
	(0.067)	(0.069)	(0.065)
Average Teacher Experience	12.641	12.659	12.623
	(2.515)	(2.467)	(2.565)
Teacher Student Ratio	13.046	12.300	13.835
	(3.291)	(3.249)	(3.148)
Urbanization Characteristics			
Percent in a Town	0.222	0.228	0.215
	(0.415)	(0.420)	(0.411)
Percent in a Small Fringe	0.061	0.066	0.056
	(0.239)	(0.248)	(0.229)
Percent in a Large Fringe	0.041	0.057	0.025
	(0.199)	(0.232)	(0.155)
Percent in a Small City	0.120	0.080	0.162
	(0.325)	(0.271)	(0.369)
Percent in a Large City	0.228	0.115	0.348
	(0.420)	(0.320)	(0.476)
Percent in a Rural Area	0.328	0.454	0.195
	(0.469)	(0.498)	(0.396)

Table 2.1: Descriptive Statistics - Means and Standard Deviations (continued)

	School Quality Quintiles Based on ACT Scores		
	Both Subsample	**2nd Quintile** *(Control)*	**1st Quintile** *(Treatment)*
County Level Characteristics			
Percent Black	0.092	0.100	0.084
	(0.420)	(0.079)	(0.085)
Percent Hispanic	0.427	0.299	0.563
	(0.269)	(0.189)	(0.274)
Persons per Housing Unit	2.848	2.730	2.972
	(0.321)	(0.246)	(0.343)
Percent Owner Occupied	0.682	0.703	0.660
	(0.092)	(0.092)	(0.085)
Violent Crimes (per 1,000 People)	0.017	0.017	0.018
	(0.008)	(0.009)	(0.007)
Percent with College Degree	0.172	0.170	0.174
	(0.075)	(0.077)	(0.072)
Observations (school-by-year)	5,650	2,910	2,740

Notes: Numbers in parentheses are standard deviations. Average value per unit is reported in real terms of 1990 dollars. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores.

Sources: Texas Comptroller Property Tax Division (TCPTD), 1995 to 2006; Academic Excellence Indicator System (AEIS), Texas Education Agency (TEA), 1994-95 to 2005-06; National Center for Education Statistics (NCES), 1994-95 to 2005-06; U.S. Census Bureau Decennial Census, 1990 and 2000; and the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) database, 1995 to 2006.

Table 2.2: Difference-in-Differences School Quality Quintiles Based on ACT Scores

	Panel A: Average Price of Residential Homes (in thousands)		
	2nd ACT Quintile	**1st ACT Quintile**	
	(Control)	*(Treatment)*	*Difference*
Pre Policy (1994/95 - 1996/97)	31.26	35.91	4.65
Post Policy (1997/98 - 2005/06)	36.93	43.76	6.83
Difference	5.66	7.84	**2.18**
	Panel B: Log Average Price of Residential Homes		
	2nd ACT Quintile	**1st ACT Quintile**	
	(Control)	*(Treatment)*	*Difference*
Pre Policy (1994/95 - 1996/97)	10.184	10.329	0.145
Post Policy (1997/98 - 2005/06)	10.310	10.484	0.174
Difference	0.125	0.155	**0.029**

Notes: Average price of residential homes is reported in real terms of 1990 dollars. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores.

Table 2.3: Difference-in-Differences Regressions - Log Average Price of Residential Homes (Bottom Two ACT Quintiles of School Quality)

	Log Average Price (1990 Dollars)			
	(1)	**(2)**	**(3)**	**(4)**
Post x Treatment	0.032**	0.047***	0.051***	0.049***
	(0.015)	(0.017)	(0.016)	(0.016)
Treatment (1st ACT quintile)	0.153***	-0.087*	-0.044	-0.060
	(0.052)	(0.047)	(0.044)	(0.046)
Post (year after 1996-97)	-0.036***	-0.040***	-0.031***	-0.031***
	(0.009)	(0.011)	(0.011)	(0.010)
Linear Trend	0.027***	0.038***	0.035***	0.034***
	(0.001)	(0.002)	(0.002)	(0.002)
Constant	10.122***	9.616***	8.545***	8.439***
	(0.035)	(0.129)	(0.257)	(0.297)
Controls:				
High School Demographics	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County Level	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	5,650	5,650	5,650	5,650
R^2	0.04	0.71	0.77	0.78

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.4: Difference-in-Differences Regressions - All ACT Quintiles of School Quality

	Log Average Price (1990 Dollars)			
	(1)	(2)	(3)	(4)
Post x 2nd ACT quintile	-0.102***	-0.098***	-0.078***	-0.077***
	(0.020)	(0.023)	(0.019)	(0.019)
Post x 3rd ACT quintile	-0.079***	-0.085***	-0.073***	-0.073***
	(0.020)	(0.023)	(0.019)	(0.019)
Post x 4th ACT quintile	-0.034*	-0.039*	-0.025	-0.024
	(0.019)	(0.023)	(0.019)	(0.019)
Post x 5th ACT quintile	-0.005	-0.032	-0.024	-0.023
	(0.019)	(0.021)	(0.018)	(0.017)
2nd ACT quintile (20-40%)	-0.173***	-0.040	-0.052	-0.043
	(0.050)	(0.040)	(0.034)	(0.034)
3rd ACT quintile (40-60%)	-0.272***	-0.057	-0.071*	-0.067*
	(0.046)	(0.049)	(0.041)	(0.040)
4th ACT quintile (60-80%)	-0.023	0.005	-0.012	-0.006
	(0.047)	(0.056)	(0.046)	(0.045)
5th ACT quintile (80-100%)	0.420***	0.211***	0.147***	0.156***
	(0.050)	(0.064)	(0.053)	(0.051)
Post (year after 1996-97)	0.029*	0.031*	0.026*	0.026*
	(0.015)	(0.017)	(0.014)	(0.014)
Linear Trend	0.033***	0.046***	0.036***	0.037***
	(0.001)	(0.001)	(0.002)	(0.002)
Constant	10.283***	10.159***	9.024***	9.054***
	(0.035)	(0.129)	(0.189)	(0.202)
Controls:				
High School Demographics	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County Level	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	13,943	13,943	13,943	13,943
R^2	0.19	0.67	0.74	0.75

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 1st quintile is the omitted category and is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.5: Placebo Difference-in-Differences Regressions - Top Two ACT Quintiles of School Quality

	Log Average Price (1990 Dollars)			
	(1)	**(2)**	**(3)**	**(4)**
Post x Placebo Treatment	-0.028**	0.005	0.007	0.005
	(0.013)	(0.015)	(0.014)	(0.014)
Placebo Treatment (4th ACT quintile)	-0.443***	-0.134***	-0.087***	-0.095***
	(0.047)	(0.036)	(0.032)	(0.032)
Post (year after 1996-97)	0.001	-0.057***	-0.051***	-0.050***
	(0.009)	(0.014)	(0.013)	(0.013)
Linear Trend	0.037***	0.061***	0.049***	0.050***
	(0.001)	(0.003)	(0.003)	(0.003)
Constant	10.696***	10.471***	9.838***	9.945***
	(0.035)	(0.117)	(0.325)	(0.325)
Controls:				
High School Demographics	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County Level	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	5,491	5,491	5,491	5,491
R^2	0.19	0.66	0.73	0.74

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 4th quintile (placebo treatment) is defined as the upper middle fifth (60-80%) of school quality based on pre-policy ACT Scores. 5th quintile (placebo control) is defined as the top (80-100%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.6: Pre-policy Difference-in-Differences Regressions - Parallel Trends Assumption Test

	Log Average Price (1990 Dollars)			
	(1)	(2)	(3)	(4)
Fake Post x Treatment	-0.002	0.003	0.002	0.002
	(0.005)	(0.007)	(0.007)	(0.007)
Treatment (1st ACT quintile)	0.154***	-0.021	0.013	-0.006
	(0.052)	(0.050)	(0.048)	(0.050)
Fake Post (year is 1995-96)	-0.004	-0.006	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)
Linear Trend	-0.003	0.006**	0.005	0.003
	(0.002)	(0.003)	(0.003)	(0.004)
Constant	10.184***	9.691***	8.585***	8.596***
	(0.036)	(0.155)	(0.319)	(0.371)
Controls:				
High School Demographics	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County Level	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	1,416	1,416	1,416	1,416
R^2	0.02	0.72	0.76	0.77

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. Years of analysis are 1994-95, 1995-96, and 1996-97 (pre-policy data). 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.7: Alternative Difference-in-Differences Regressions - Excluding No Child Left Behind (NCLB) School Years

	Log Average Price (1990 Dollars)		
	(1)	**(2)**	**(3)**
	Full Sample:	**8-Year Window:**	**6-Year Window:**
	1994-95 to 2005-06	1994-95 to 2001-02	1994-95 to 1999-00
	(3 Yrs Pre, 9 Yrs Post)	(3 Yrs Pre, 5 Yrs Post)	(3 Yrs Pre, 3 Yrs Post)
Post x Treatment	0.049***	0.032**	0.025**
	(0.016)	(0.013)	(0.011)
Treatment (1st ACT quintile)	-0.060	-0.034	-0.019
	(0.046)	(0.046)	(0.047)
Post (year after 1996-97)	-0.031***	-0.023**	0.004
	(0.010)	(0.009)	(0.008)
Linear Trend	0.034***	0.032***	0.023***
	(0.002)	(0.003)	(0.003)
Constant	8.439***	8.389***	8.486***
	(0.297)	(0.311)	(0.325)
Controls:			
High School Demographics	Yes	Yes	Yes
Urbanization	Yes	Yes	Yes
County Level	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes
Obs (school-by-year)	5,650	3,782	2,837
R^2	0.78	0.77	0.77

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.8: Difference-in-Differences Regressions by Grade Spans 3rd to 8th Texas Assessment of Academic Skills (TAAS) in Reading, Math, & Writing (RM&W) Pass Rate

	3rd Grade:		4th Grade:		5th Grade:		6th Grade:		7th Grade:		8th Grade:	
	TAAS RM&W		TAAS RM&W		TAAS RM&W		TAAS RM&W		TAAS RM&W		TAAS RM&W	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Post x 4th ACT quintile									-3.08	-1.94	-3.35	-4.41
									[2.241]	[2.423]	[2.556]	[2.757]
Post x 3rd ACT quintile	2.40	0.23	12.86	11.58	3.17	2.54	6.97	5.42	-1.88	-2.62	-2.47	-6.05
	[4.245]	[5.858]	[6.102]*	[6.942]+	[2.741]	[3.757]	[2.462]**	[3.207]+	[2.174]	[2.452]	[2.402]	[2.716]*
Post x 2nd ACT quintile	-2.16	-3.25	8.40	5.24	5.91	5.51	7.57	5.06	-0.77	-1.03	-3.30	-5.98
	[4.139]	[5.655]	[5.984]	[6.715]	[2.770]*	[3.566]	[2.493]**	[3.207]	[1.972]	[2.206]	[2.444]	[2.690]*
Post x 1st ACT quintile	8.70	6.48	11.45	6.35	16.23	14.76	13.22	10.22	6.47	4.18	3.13	-2.35
	[4.798]+	[6.126]	[6.680]+	[7.072]	[3.880]**	[4.526]**	[3.382]**	[4.098]*	[2.505]*	[2.690]	[2.957]	[3.101]
Constant	73.26	61.84	68.23	76.50	76.23	70.30	77.49	75.19	85.11	92.31	67.64	62.92
	[5.591]**	[16.481]**	[6.233]**	[14.046]**	[3.042]**	[11.417]**	[6.300]**	[11.036]**	[1.631]**	[7.697]**	[2.520]**	[8.317]**
Controls:												
Quintile Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
High School	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
School District Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Obs (school-by-year)	936	936	952	952	965	965	1,189	1,189	2,245	2,245	2,263	2,263
R^2	0.14	0.49	0.16	0.57	0.26	0.58	0.27	0.58	0.32	0.60	0.20	0.56

Notes: Numbers in brakets are robust standard errors clustered by high school campus ID. 1st quintile is defined as the bottom fifth (0-20%), 2nd quintile is defined as the lower middle (20-40%), 3rd quintile is defined as the middle (40-60%), 4th quintile is defined as the upper middle (60-80%), and 5th quintile (omitted category) is defined as the top fifth (80-100%). **, *, + indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.9: Difference-in-Differences Regressions - Robin Hood Plan

	Dependent Variable - Log Spending Per Pupil			
	(1)	(2)	(3)	(4)
Post x Treatment	-0.005	-0.008	-0.007	-0.007
	(0.011)	(0.009)	(0.009)	(0.010)
Treatment (1st ACT quintile)	-0.051**	-0.016	-0.013	-0.005
	(0.022)	(0.020)	(0.020)	(0.021)
Post (year after 1996-97)	0.002	0.004	0.001	0.001
	(0.008)	(0.007)	(0.007)	(0.007)
Linear Trend	0.05***	0.04***	0.038***	0.038***
	(0.002)	(0.001)	(0.002)	(0.002)
Constant	8.326***	9.017***	8.782***	8.753***
	(0.017)	(0.044)	(0.118)	(0.141)
Controls:				
School	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	4,257	4,257	4,257	4,257
R^2	0.22	0.69	0.70	0.71

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.10: Difference-in-Differences Regressions - Excluding Longhorn Scholarship Eligible High Schools

	Log Average Price (1990 Dollars)			
	(1)	**(2)**	**(3)**	**(4)**
Post x Treatment	0.029*	0.037***	0.040**	0.039**
	(0.016)	(0.018)	(0.017)	(0.017)
Treatment (1st ACT quintile)	0.075	-0.084*	-0.036	-0.054
	(0.054)	(0.049)	(0.046)	(0.047)
Post (year after 1996-97)	-0.027***	-0.025**	-0.020*	-0.018*
	(0.009)	(0.010)	(0.010)	(0.010)
Linear Trend	0.024***	0.035***	0.032***	0.032***
	(0.001)	(0.002)	(0.002)	(0.002)
Constant	10.115***	9.620***	8.571***	8.417***
	(0.036)	(0.136)	(0.264)	(0.302)
Controls:				
High School Demographics	No	Yes	Yes	Yes
Urbanization	No	Yes	Yes	Yes
County	No	No	Yes	Yes
MSA Fixed Effects	No	No	No	Yes
Obs (school-by-year)	5,164	5,164	5,164	5,164
R^2	0.02	0.70	0.76	0.77

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.11: Difference-in-Differences Regressions - Schooling Market Power

	Log Avg. Price (1990 Dollars)
Post x Treatment x HHI	-0.171**
	(0.070)
Post x Treatment	0.058***
	(0.022)
Post x HHI	-0.218***
	(0.047)
Treatment x HHI	0.226*
	(0.131)
Post (year after 1996-97)	0.028*
	(0.016)
Treatment (1st ACT quintile)	-0.073
	(0.055)
Herfindahl-Hirschman Index (HHI)	-0.073
	(0.084)
Linear Trend	0.035***
	(0.002)
Constant	8.719***
	(0.263)
Controls:	
High School Demographics	Yes
Urbanization	Yes
County Level	Yes
MSA Fixed Effects	Yes
Obs (school-by-year)	5,650
R^2	0.77

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. Schooling market power is mesured by Herfindahl-Hirschman Index (HHI) per pupils. 1st quintile (treatment group) is defined as the bottom fifth (0-20%) of school quality based on pre-policy ACT Scores. 2nd quintile (control group) is defined as the lower middle (20-40%) of school quality based on pre-policy ACT Scores. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.12: Diff-in-Diff Regression Subsamples by County Schooling Market Power

	Log Average Price (1990 Dollars)				
	1st Quintile HHI:	**2nd Quintile HHI:**	**3rd Quintile HHI:**	**4th Quintile HHI:**	**5th Quintile HHI:**
	(Least Monopolistic)				*(Most Monopolistic)*
Post x Treatment	0.034*	-0.012	-0.043	-0.079	-0.009
	(0.019)	(0.048)	(0.039)	(0.058)	(0.070)
Controls	Yes	Yes	Yes	Yes	Yes
Obs (school-by-year)	3,133	818	923	532	244
R^2	0.72	0.57	0.58	0.51	0.61

Notes: Numbers in parentheses are robust standard errors clustered by high school campus ID. Schooling market power is mesured by Herfindahl-Hirschman Index (HHI) per pupils. Each coefficient represents a separate regression of the log average price (in 1990 Dollars) or log number of housing units on a constant, post indicator, treatment indicator, post*treatment indicator, and a linear time trend, controlling for high school demographics, urbanization, and county level characteristics. ***, **, * indicates statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.1: Summary Statistics

Variable Name	Settled Within 3 Years		Universe of Settled Claims	
	Before Reform	After Reform	Before Reform	After Reform
Settlement Amount (Thousands)	471.64	212.79	420.33	228.63
	(1,152.18)	*(577.29)*	*(958.01)*	*(530.99)*
Cash Demanded (Thousands)	530.05	249.74	516.69	259.07
	(1,310.77)	*(756.15)*	*(1,322.53)*	*(703.95)*
Duration of Case (Days)	837.81	632.96	1,597.17	896.61
	(188.78)	*(257.39)*	*(897.89)*	*(618.44)*
Initial Indemnity Reserve (Thousands)	91.37	74.89	79.19	81.69
	(151.64)	*(134.13)*	*(160.45)*	*(135.21)*
Per Accident Policy Limit (Thousands)	1,223.17	1,602.66	999.16	1,390.35
	(2,101.66)	*(2,087.11)*	*(2,241.30)*	*(1,989.05)*
Age of Injured Party (Years)	42.62	41.02	38.09	41.17
	(24.85)	*(26.61)*	*(25.27)*	*(26.12)*
Injured Party was a Baby (Binary)	0.11	0.17	0.17	0.16
	(0.32)	*(0.37)*	*(0.38)*	*(0.37)*
Other Physicians Defending (Binary)	0.63	0.27	0.76	0.33
	(1.07)	*(0.72)*	*(1.32)*	*(0.77)*
Other Health Care Providers Defending (Binary)	0.27	0.13	0.37	0.16
	(0.91)	*(0.51)*	*(1.56)*	*(0.58)*
Observations	4,358	1,772	17,660	2,702

Note: All dollar values are scaled to year 2000 dollars

Table 3.2: OLS Regression Results - Settlment Amount for Cases Settled Within 3 Years (Thousands)

Age Group	**0 to 2**	**3 to 19**	**20 to 29**	**30 to 39**	**40 to 49**	**50 to 59**	**60 to 69**	**All Ages**
After Policy Change (Binary)	-105.68	-4.95	-111.11***	-103.40***	-47.59	-25.00	-23.16	-59.10***
	(69.63)	*(63.84)*	*(40.19)*	*(35.54)*	*(39.92)*	*(18.20)*	*(31.77)*	*(16.15)*
Cash Demanded (Thousands)	0.74***	0.47***	0.38***	0.56***	0.61***	0.55***	0.64***	0.61***
	(0.09)	*(0.07)*	*(0.09)*	*(0.14)*	*(0.15)*	*(0.10)*	*(0.16)*	*(0.06)*
Initial Indemnity Reserve (Thousands)	0.46	0.29	0.60***	0.61**	0.84**	0.24	0.41**	0.46***
	(0.32)	*(0.19)*	*(0.16)*	*(0.29)*	*(0.41)*	*(0.16)*	*(0.18)*	*(0.12)*
Per Accident Policy Limit (Thousands)	0.00	0.01	-0.01	0.02	0.04	0.00	-0.01	0.01
	(0.03)	*(0.02)*	*(0.01)*	*(0.02)*	*(0.03)*	*(0.00)*	*(0.01)*	*(0.01)*
Other Physicians Defending (Binary)	87.03*	99.57**	147.49***	*35.14*	108.11***	76.17***	18.63	72.85***
	(49.68)	*(43.46)*	*(53.48)*	*(31.59)*	*(33.25)*	*(19.04)*	*(21.55)*	*(13.87)*
Other Health Care Providers Defending (Binary)	150.56	57.72	-4.48	103.25**	-42.00	125.59**	35.08	39.21**
	(110.26)	*(48.23)*	*(10.69)*	*(50.77)*	*(37.70)*	*(52.24)*	*(23.87)*	*(16.93)*
Constant	143.63**	60.86	98.84***	34.58	-43.35	*43.99***	31.92	175.75***
	(71.57)	*(98.45)*	*(29.17)*	*(30.85)*	*(71.07)*	*(17.93)*	*(54.54)*	*(42.88)*
Age								*(5.63)* ***
								(1.53)
Age Squared								*0.04****
								(0.01)
Observations	840	373	598	939	871	852	691	6,130
R-squared	0.470	0.647	0.377	0.640	0.660	0.823	0.696	0.575

Note: * denotes P < 0.1, ** denotes P < 0.5, *** denotes P < 0.01, effect of binary variables and constant reported in thousands, robust standard errors in parenthesis

Table 3.3: OLS Regression Results - Cash Demanded for Cases Settled Within 3 Years (Thousands)

Age Group	**0 to 2**	**3 to 19**	**20 to 29**	**30 to 39**	**40 to 49**	**50 to 59**	**60 to 69**	**All Ages**
After Policy Change (Binary)	-195.69**	-138.65	35.44	-312.90***	-341.75***	-119.37**	-205.40***	-193.82***
	(85.52)	*(168.80)*	*(114.81)*	*65.40*	*(152.61)*	*(45.94)*	*(36.41)*	*(27.29)*
Initial Indemnity Reserve (Thousands)	2.70***	1.59	1.27***	2.19***	2.58**	0.78*	1.70*	1.97***
	(0.73)	*(0.96)*	*(0.30)*	*0.75*	*(1.00)*	*(0.44)*	*(0.98)*	*(0.34)*
Per Accident Policy Limit (Thousands)	0.20***	0.04	-0.01	0.07**	0.09	0.01	0.01	0.04**
	(0.08)	*(0.04)*	*(0.02)*	*(0.03)*	*(0.07)*	*(0.01)*	*(0.02)*	*(0.01)*
Other Physicians Defending (Binary)	87.42	223.61**	187.75***	159.07**	227.33***	188.13***	93.46***	162.49***
	(53.55)	*(99.83)*	*(498.34)*	*(69.99)*	*(68.18)*	*(45.13)*	*(33.12)*	*(20.78)*
Other Health Care Providers Defending (Binary)	293.74***	-22.54	13.39	49.14	-22.17	505.83***	-34.71	113.07**
	(111.73)	*(91.88)*	*(21.29)*	*(67.38)*	*(68.51)*	*(156.80)*	*(43.21)*	*(43.85)*
Constant	103.33	184.62	184.90***	187.90	149.63	148.58**	220.89***	300.06***
	(97.46)	*(112.33)*	*(37.72)*	*(58.36)*	*(152.61)*	*(60.27)*	*(66.99)*	*(47.63)*
Age								*(3.78)* **
								1.89
Age Squared								*0.01*
								(0.02)
Observations	840	373	598	939	871	852	691	6,130
R-squared	0.209	0.138	0.100	0.122	0.136	0.245	0.113	0.123

Note: * denotes P < 0.1, ** denotes P < 0.5, *** denotes P < 0.01, effect of binary variables and constant reported in thousands, robust standard errors in parenthesis

Table 3.4: Duration Results - Cox Proportional Hazard, Cases Settled Within 3 Years

Hazard Ratio	
Age Group	**After Policy Change**
0 to 2	2.450***
	(0.213)
3 to 19	2.253***
	(0.284)
20 to 29	1.971***
	(0.244)
30 to 39	1.994***
	(0.192)
40 to 49	2.051***
	(0.199)
50 to 59	1.902***
	(0.169)
60 to 69	1.777***
	(0.196)
All Ages	2.006***
	(0.072)

Note: * denotes P < 0.1, ** denotes P < 0.5, *** denotes P < 0.01, other regression coeffecients supressed (available upon request), Cash Demanded does not statistically influence duration

Table 3.5: Effect of Reform on Quick Settlements, Cases Settled Within 3 Years (All Dollar Values in Thousands)

Age Group	Average Settlement Pre-Reform	Estimated Effect of Reform on Settlement Amount	Estimated Effect of Reform via Cash Demanded	Pre-Reform Average Time to Payment (Days)	Estimated Change in Time to Payment (Days)	Difference in Settlement's Asset Value	Percent of Original Value Lost
0 to 2	784.29	0.00	-144.81	832.18	-492.51	-132.61	16.91
3 to 19	436.84	0.00	0.00	818.47	-455.19	7.69	-1.76
20 to 29	409.37	-111.11	0.00	840.21	-413.92	-106.34	25.98
30 to 39	469.34	-103.40	-175.22	844.61	-421.03	-275.52	58.70
40 to 49	532.87	0.00	-208.47	843.21	-432.09	-203.05	38.10
50 to 59	433.06	0.00	-65.65	854.88	-405.42	-59.89	13.83
60 to 69	381.37	0.00	-131.46	836.17	-365.62	-127.93	33.54
All Ages	471.64	-59.10	-118.23	837.81	-420.16	-172.54	36.58

Note: All dollar values are scaled to year 2000 dollars, calculations assume a real interest rate of 0.0141. Estimated effect amounts generated using Tables 3.2, 3.3, and 3.4, statistically insignificant results reported as zeroes. Estimated effect on settlement amount is the after policy effect from Table 3.2. Estimated effect via Cash Demanded is the effect of Cash Demanded from Table 3.2 multiplied by the after policy effect from Table 3.3. Estimated change in time to payment is the inverse of the hazard from Table 3.4 multiplied by average pre-reform time to payment.

Table 3.6: Maximum Entropy Quantiles

Age Group	MEQ
0 to 2	62
3 to 19	62
20 to 29	45
30 to 39	35
40 to 49	43
50 to 59	52
60 to 69	49
All Ages	43

Table 3.7: Effect of Reform on Asset Value, Cases Settled Within 3 Years

Percent of Asset Value Lost			
Age Group	**OLS**	**Median**	**MEQ**
0 to 2	16.91	5.76	5.34
3 to 19	-1.76	23.08	26.96
20 to 29	25.98	29.14	25.73
30 to 39	58.70	27.59	22.34
40 to 49	38.10	35.69	26.79
50 to 59	13.83	30.29	28.73
60 to 69	33.54	25.37	23.47
All Ages	36.58	29.76	22.94

Note: All dollar values are scaled to year 2000 dollars, calculations assume a real interest rate of 0.0141. OLS results taken from Table 3.5. Median and MEQ columns replicate Table 3.5 using median or MEQ settlement amounts and Median or MEQ regression.

Table 3.8: OLS Regression Results - Settlement Amount (Thousands) - Maximum Time to Settlement Sensitivity

Age Group	3.5 Years	3 years
After Policy Change (Binary)	-68.11***	-59.10***
	(14.19)	*(16.15)*
Cash Demanded (Thousands)	0.58***	0.61***
	(0.04)	*(0.06)*
Initial Indemnity Reserve (Thousands)	0.35**	0.46***
	(0.14)	*(0.12)*
Per Accident Policy Limit (Thousands)	0.02*	0.01
	(0.01)	*(0.01)*
Other Physicians Defending (Binary)	57.85***	72.85***
	(10.40)	*(13.87)*
Other Health Care Providers Defending (Binary)	39.34***	39.21**
	(12.22)	*(16.93)*
Constant	164.07***	175.75***
	(31.68)	*(42.88)*
Age	-4.30***	-5.63***
	(1.18)	*(1.53)*
Age Squared	*0.03***	*0.04****
	(0.01)	*(0.01)*
Observations	9120	6,130
R-squared	0.549	0.575

Note: * denotes P < 0.1, ** denotes P < 0.5, *** denotes P < 0.01, effect of binary variables and constant reported in thousands, robust standard errors in parenthesis

Table 3.9: OLS Regression Results - Cash Demanded (Thousands) - Maximum Time to Settlement Sensitivity

Age Group	**3.5 Years**	**3 Years**
After Policy Change (Binary)	-204.98***	-193.82***
	(43.81)	*(27.29)*
Initial Indemnity Reserve (Thousands)	2.43***	1.97***
	(0.39)	*(0.34)*
Per Accident Policy Limit (Thousands)	0.05***	0.04**
	(0.01)	*(0.01)*
Other Physicians Defending (Binary)	137.72***	162.49***
	(18.00)	*(20.78)*
Other Health Care Providers Defending (Binary)	67.93**	113.07**
	(29.01)	*(43.85)*
Constant	285.95***	300.06***
	(43.81)	*(47.63)*
Age	-4.32***	-3.78**
	(1.56)	*1.89*
Age Squared	*0.02*	*0.01*
	(0.02)	*(0.02)*
Observations	9120	6,130
R-squared	0.153	0.123

Note: * denotes P < 0.1, ** denotes P < 0.5, *** denotes P < 0.01, effect of binary variables and constant reported in thousands, robust standard errors in parenthesis

Table 4.1. Summary Statistics

	Mean Allowed	Std. Deviation	Mean Charge	Std. Deviation	Observations	HMO	PPO	POS	Urban
Ob-Gyn Procedures									
Cesarean Section	2180.44	674.07	3334.18	1238.18	552,113	0.099	0.279	0.256	0.938
Vaginal Birth	2008.83	573.35	2871.10	1039.31	1,112,030	0.113	0.281	0.238	0.928
Transvaginal Ultrasound (Not Pregnant)	99.33	64.86	207.28	121.37	4,713,055	0.086	0.286	0.236	0.948
Abdominal Ultrasound (1st Trimester)	115.55	67.47	210.72	117.10	922,141	0.081	0.263	0.260	0.951
Non Ob-Gyn Procedures									
Single Coronary Artery Bypass Graft	1756.19	2120.38	4139.88	2899.56	6,919	0.085	0.287	0.117	0.927
Chest X-Ray	29.47	29.11	55.48	59.69	20,573,408	0.091	0.300	0.383	0.900
15 Minute Office Visit	52.92	27.21	75.07	433.58	257,882,996	0.093	0.299	0.200	0.897
Tetanus Shot	24.69	21.80	38.73	68.53	3,987,883	0.151	0.312	0.211	0.927

State Characteristics	**Mean**	**Std. Deviation**
Number of Doctors (Thousands)	14.38	17.44
Population (Thousands)	5538.99	6388.02
Median Household Income (Thousands)	38.08	11.98

Table 4.2. State Malpractice Tort Law Changes 2003 - 2007

State	Year Implemented	Change in Tort System
Florida	2003	Cap on Noneconomic Damages
Georgia	2005	Cap on Noneconomic Damages
Georgia	2005	Early Offer
Illinois	2004	Cap on Noneconomic Damages
Montana	2003	Cap on Punitive Damages
Ohio	2003	Cap on Noneconomic Damages
Oklahoma	2003	Cap on Noneconomic Damages
South Carolina	2005	Cap on Noneconomic Damages
Texas	2003	Cap on Noneconomic Damages
Washington	2006	Collateral Source Offset
Wisconsin	2005	Cap Ruled Unconstituional
Wisconsin	2006	Cap on Noneconomic Damages

Note: shaded laws have no bearing on estimates of effect of a cap on noneconomic damages

Table 4.3A. Effect of Damage Cap on Log Allowed (Ob-Gyn Procedures)

	Cesarean Section	**Vaginal Birth**	**Transvaginal Ultrasound (Not Pregnant)**	**Abdominal Ultrasound (First Trimester)**
Effect of Cap	-0.017	-0.021**	-0.021*	-0.072***°
	(0.014)	(0.009)	(0.011)	(0.017)
Percent Change	**-1.669**	**-2.078**	**-2.095**	**-6.909**
Constant	6.850***°	7.418***°	5.230***°	5.425***°
	(0.183)	(0.221)	(0.259)	(0.461)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.087	0.109	0.255	0.188
N	552,113	1,112,030	4,713,055	922,141

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.3B. Effect of Damage Cap on Log Allowed (Non-Ob-Gyn Procedures)

	Coronary Artery Bypass Graft	**Chest X-Ray**	**15 Minute Office Visit**	**Tetanus Shot**
Effect of Cap	-0.129**	-0.014	0.002	-0.068*
	(0.056)	(0.013)	(0.006)	(0.035)
Percent Change	**-12.103**	**-1.415**	**0.183**	**-6.570**
Constant	10.698***°	3.337***°	4.303***°	2.357***°
	(1.446)	(0.518)	(0.235)	(0.409)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.058	0.164	0.162	0.346
N	6,919	20,573,408	257,882,996	3,987,883

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.4A. Effect of Damage Cap on Log Charges (Ob-Gyn Procedures)

	Cesarean Section	**Vaginal Birth**	**Transvaginal Ultrasound (Not Pregnant)**	**Abdominal Ultrasound (First Trimester)**
Effect of Cap	0.000	0.008	0.022	0.007
	(0.013)	(0.013)	(0.020)	(0.015)
Percent Change	**0.023**	**0.852**	**2.235**	**0.683**
Constant	7.001***°	7.057***°	5.172***°	5.253***°
	(0.144)	(0.158)	(0.202)	(0.297)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.167	0.211	0.161	0.156
N	552,113	1,112,030	4,713,055	922,141

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.4B: Effect of Damage Cap on Log Charges (Non-Ob-Gyn Procedures)

	Coronary Artery Bypass Graft	**Chest X-Ray**	**15 Minute Office Visit**	**Tetanus Shot**
Effect of Cap	0.013	0.021	0.009*	-0.007
	(0.074)	(0.032)	(0.005)	(0.007)
Percent Change	**1.313**	**2.143**	**0.904**	**-0.698**
Constant	10.878***°	4.000***°	4.219***°	2.379***°
	(0.823)	(0.313)	(0.168)	(0.305)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.126	0.080	0.142	0.402
N	6,919	20,573,408	257,882,996	3,987,883

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.5A: Effect of Damage Cap on Urban/Rural Log Allowed (Ob-Gyn Procedures)

	Cesarean Section	**Vaginal Birth**	**Transvaginal Ultrasound (Not Pregnant)**	**Abdominal Ultrasound (First Trimester)**
Urban Effect of Cap	-0.023	-0.026**	-0.030*	-0.077***°
	(0.015)	(0.011)	(0.015)	(0.015)
Percent Change	**-2.274**	**-2.566**	**-2.955**	**-7.411**
Rural Effect of Cap	0.053	0.032	0.085	-0.021
	(0.030)	(0.024)	(0.052)	(0.055)
Percent Change	**5.443**	**2.429**	**8.872**	**-2.078**
Constant	6.820***°	7.391***°	5.176***°	5.402***°
	(0.183)	(0.219)	(0.262)	(0.466)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.088	0.109	0.255	0.188
N	552,113	1,112,030	4,713,055	922,141

Notes: Rejection of a single tail test $p<0.1$ denoted *, $p<0.05$ denoted **, $p<0.01$ denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.5B: Effect of Damage Cap on Urban/Rural Log Allowed (Non-Ob-Gyn Procedures)

	Coronary Artery Bypass Graft	**Chest X-Ray**	**15 Minute Office Visit**	**Tetanus Shot**
Urban Effect of Cap	-0.133**	-0.023	0.000	-0.069*
	(0.055)	(0.014)	(0.006)	(0.038)
Percent Change	**-12.453**	**-2.274**	**0.000**	**-6.667**
Rural Effect of Cap	-0.044	0.061	0.014	-0.054***
	(0.106)	(0.047)	(0.015)	(0.019)
Percent Change	**-4.305**	**-5.918**	**1.410**	**-5.257**
Constant	10.650***°	3.345***°	4.315***°	2.360***°
	(1.415)	(0.510)	(0.235)	(0.395)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.058	0.164	0.162	0.346
N	6,919	20,573,408	257,882,996	3,987,883

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level

Table 4.6A: Effect of Damage Cap on Log Allowed (Ob-Gyn Procedures) - Cross Border MSAs

	Cesarean Section	**Vaginal Birth**	**Transvaginal Ultrasound (Not Pregnant)**	**Abdominal Ultrasound (First Trimester)**
Effect of Cap	-0.085**	-0.074***°	-0.097**	-0.147**
	(0.035)	(0.021)	(0.042)	(0.061)
Percent Change	**-8.193**	**-7.136**	**-9.256**	**-13.687**
Constant	6.815***°	7.073***°	5.054***°	4.204***°
	(0.216)	(0.169)	(0.211)	(0.181)
Controls	YES	YES	YES	YES
MSA by Year Fixed Effects	YES	YES	YES	YES
R Squared	0.062	0.068	0.108	0.163
N	62,068	137,628	473,517	98,443

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the MSA level.

Table 4.6B: Effect of Damage Cap on Log Allowed (Non-Ob-Gyn Procedures) - Cross Border MSAs

	Coronary Artery Bypass Graft	**Chest X-Ray**	**15 Minute Office Visit**	**Tetanus Shot**
Effect of Cap	-0.217	-0.157***°	-0.043**	-0.142***
	(0.201)	(0.038)	(0.014)	(0.043)
Percent Change	**-19.508**	**-14.551**	**-4.164**	**-13.206**
Constant	6.484***°	1.484***°	3.042***°	4.141***°
	(1.451)	(0.388)	(0.097)	(0.158)
Controls	YES	YES	YES	YES
MSA by Year Fixed Effects	YES	YES	YES	YES
R Squared	0.121	0.159	0.194	0.136
N	706	2,472,397	29,688,127	540,547

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the MSA level.

Table 4.7A: Effect of Damage Cap on Log Charges (Ob-Gyn Procedures) - Cross Border MSAs

	Cesarean Section	**Vaginal Birth**	**Transvaginal Ultrasound (Not Pregnant)**	**Abdominal Ultrasound (First Trimester)**
Effect of Cap	-0.061	-0.033	-0.046	-0.051
	(0.037)	(0.029)	(0.032)	(0.034)
Percent Change	**-5.872**	**-3.276**	**-4.538**	**-4.949**
Constant	7.559***°	7.619***°	5.696***°	5.139***°
	(0.237)	(0.147)	(0.289)	(0.184)
Controls	YES	YES	YES	YES
MSA by Year Fixed Effects	YES	YES	YES	YES
R Squared	0.072	0.095	0.121	0.177
N	62,068	137,628	473,517	98,443

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the MSA level

Table 4.7B: Effect of Damage Cap on Log Charges (Non-Ob-Gyn Procedures) - Cross Border MSAs

	Coronary Artery Bypass Graft	**Chest X-Ray**	**15 Minute Office Visit**	**Tetanus Shot**
Effect of Cap	-0.150	-0.124***	-0.038*	-0.049***
	(0.095)	(0.033)	(0.021)	(0.015)
Percent Change	**-13.952**	**-11.662**	**-3.729**	**-4.782**
Constant	5.842***°	2.851***°	3.925***°	4.655***°
	(0.974)	(0.178)	(0.068)	(0.123)
Controls	YES	YES	YES	YES
MSA by Year Fixed Effects	YES	YES	YES	YES
R Squared	0.200	0.110	0.206	0.167
N	706	2,472,397	29,688,127	540,547

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the MSA level.

Table 4.8A: Effect of False (Incorrect State) Damage Cap on Log Allowed (Ob-Gyn Procedures)

	Cesarean Section	Vaginal Birth	Transvaginal Ultrasound (Not Pregnant)	Abdominal Ultrasound (First Trimester)
Effect of False Cap	0.016	0.011	0.015	-0.019
	(0.160)	(0.013)	(0.018)	(0.023)
Constant	6.814***°	7.351***°	5.054***°	5.283***°
	(0.183)	(0.221)	(0.224)	(0.468)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.084	0.111	0.256	0.165
N	488,529	965,451	4,231,626	652,608

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level.

Table 4.8B: Effect of False (Incorrect State) Damage Cap on Log Allowed (Non-Ob-Gyn Procedures)

	Coronary Artery Bypass Graft	Chest X-Ray	15 Minute Office Visit	Tetanus Shot
Effect of False Cap	0.012	0.026	-0.022	-0.003
	(0.786)	(0.021)	(0.018)	(0.021)
Constant	10.492***°	3.207***°	4.314***°	2.089***°
	(1.533)	(0.500)	(0.234)	(0.318)
Controls	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
R Squared	0.048	0.150	0.136	0.394
N	6,089	18,057,457	226,717,775	3,463,383

Notes: Rejection of a single tail test p<0.1 denoted *, p<0.05 denoted **, p<0.01 denoted ***. Rejection of null based on Leamer-Schwarz critical value denoted °. All standard errors are robust and clustered at the state level

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