

THE EFFECTIVENESS OF STATE LEGISLATION IN MITIGATING MORAL HAZARD: EVIDENCE FROM AUTOMOBILE INSURANCE*

ROBERT E. HOYT,
University of Georgia

DAVID B. MUSTARD,
University of Georgia

and

LAWRENCE S. POWELL
University of Arkansas, Little Rock

ABSTRACT

Insurance fraud, which adds an estimated \$85 billion per year to the total insurance bill in the United States, is an extremely serious problem for consumers, regulators, and insurance companies. This paper analyzes the effects of state legislation and market conditions on automobile insurance fraud from 1988 to 1999, a period exhibiting a substantial increase in the enactment of antifraud legislation. Our empirical results show that the laws have mixed effects: two laws have no statistically significant effect on fraud. The strongest evidence of fraud mitigation effects is associated with mandatory special investigation units, classification of insurance fraud as a felony, and mandatory reporting of professionals to licensing authorities. However, laws requiring insurers to report potentially fraudulent claims to law enforcement authorities actually increase fraud, which may reflect some substitution from more efficacious private efforts to less productive state activity. Many underlying characteristics of the market also affect fraud.

I. INTRODUCTION

INSURANCE fraud is one of the most serious problems facing insurers, insurance consumers, and regulators. Fraud increases the cost of insurance, threatens the financial strength of insurers, and negatively affects the availability of insurance. The Coalition against Insurance Fraud (CAIF) estimates that the cost of insurance fraud in the United States is as high as \$85 billion per year (CAIF 1996). For example, it is estimated that more than one-third of claimants hurt in automobile accidents exaggerate their injuries, fraudulent

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activity that in 1993 alone added \$13–\$18 billion to America's insurance bill (Carroll, Abrahamse, and Vaiana 1995).

In an attempt to reduce significant costs associated with insurance fraud, many states have enacted a wide variety of laws and regulations. Between 1988 and 1999, 43 states enacted 124 new antifraud statutes, representing substantial variation from state to state. These include increased penalties, allocation of more funds to detect and prosecute violators, and implementation of regulations requiring insurers and insurance regulators to devote more resources to reduce insurance fraud. Although antifraud and tort reforms have been promoted as important elements in the fight against insurance fraud, the actual effect of these laws on moral hazard is unclear: these laws may have no effect, could achieve the desired effect of reducing fraud, or could even generate unintended consequences that increase insurance fraud.

Despite the significant change in the regulatory environment over the 12-year period 1988–99, no academic study has assessed the effectiveness of these statutory efforts to reduce insurance fraud. This study measures the efficacy of these reforms on the level of automobile insurance fraud.

Our study uses regression analysis to evaluate the relationship between the level of insurance claim fraud and the existence of antifraud fraud statutes. Our measure of insurance fraud is the ratio of auto injury losses to auto property damage losses. Using state-level panel data between 1988 and 1999, we regress this fraud proxy variable on a set of insurance fraud and tort reform statutes, market structure characteristics, and other variables to assess their effects on fraudulent auto insurance claims. The results of our analysis should prove valuable to regulators, insurers, legislators, and consumers in mitigating insurance fraud through regulatory and statutory changes at the state level.

This analysis is presented as follows. First, Section II describes the variables used in the study and develops hypotheses about how the legislative and market structure variables may affect automobile insurance fraud. Sections III and IV present the empirical model and results of our empirical estimation, respectively. Finally, Section V concludes the analysis.

II. DATA AND HYPOTHESES

We use state-level data for 1988–99 for two reasons. First, we start in 1988 because the Insurance Research Council (IRC), which provides data for our dependent variable, did not provide the data before that year. Second and more important, this period is the most relevant for analyzing this issue because it covers a significant change in the legal environment as states aggressively implemented antifraud statutes. Figure 1 plots the number of

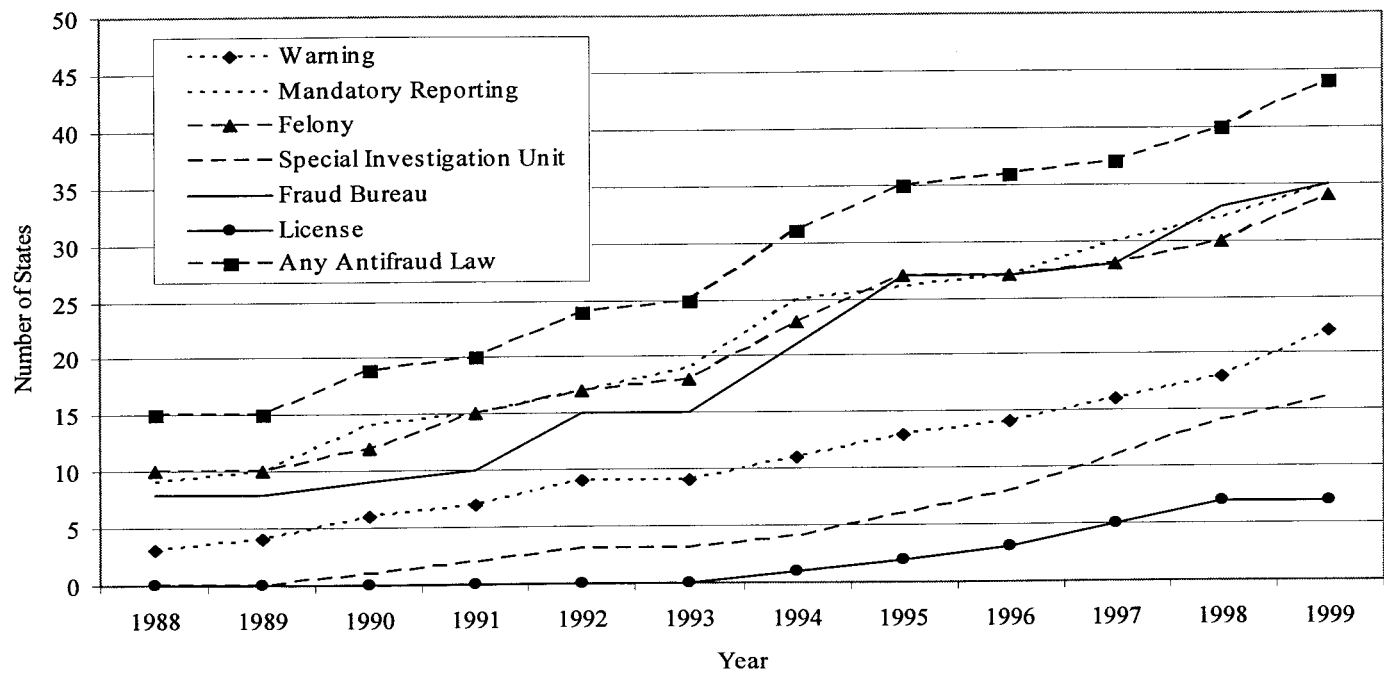


FIGURE 1.—Antifraud law enforcement

states that adopted six antifraud laws during the sample period.¹ In 1988, fewer than 10 states had enacted any of these laws. During the next decade, the number of states adopting these laws increased quickly and substantially. Two laws that existed in no state in 1988—laws that establish special investigation units and require states to revoke professional licenses in the event of insurance fraud—were used in 16 and seven states, respectively, by 1999. In 1988, the three most frequently used laws—requiring insurance companies to report potential fraud, instituting a fraud bureau, and instituting a felony for insurance fraud—were each used in 10 or fewer states. By the end of our sample, each law was used in about 35 states.

A. *Insurance Fraud*

Moral hazard is present when claimants' behavior affects the amount of insured losses and when claimants have information, unavailable to insurers, about the value of losses. Insurance fraud occurs when moral hazard entices a claimant to inflate the value of a loss by deceiving an insurer.

Insurance fraud is often divided into two categories based on when the intent to defraud an insurer arose. The most egregious type of fraud is planned before the occurrence of a loss. For example, several instances of "fraud mills" have been exposed by law enforcement in recent years. A fraud mill is a network of attorneys and physicians that stages low-speed automobile collisions to bill insurance companies for unnecessary and fictitious medical procedures. Many other elaborate schemes have been designed and implemented to defraud insurers.²

The more common form of insurance fraud occurs after a legitimate accident. This *ex post* fraud involves unnecessary build-up or overstatement of claim costs (Dionne and St-Michel 1991; Dionne, St-Michel, and Vanasse 1993). The IRC surveys of 1991 and 1995 uncovered a surprising public tolerance for dishonesty in insurance claiming (Insurance Research Council 1991, 1995). According to the surveys, many consumers believe it is acceptable to exaggerate the cost of an accident to offset a deductible or to make up for premiums paid in past periods when no claims were filed.

Being difficult to detect, insurance fraud is hard to measure, and because no direct measure of insurance fraud is available, we must use a proxy that we expect to be highly correlated with insurance fraud. The proxy is the ratio of the average loss cost per insured car (for all injury coverages) combined to the average loss cost per insured car for property damage coverage.

Moral hazard can arise only when the claimant has information about the

¹ The list of laws is from Coalition against Insurance Fraud, "State Anti-fraud Regulations" (http://www.insurancefraud.org/regulations_search.html); dates of enactment were researched individually via LexisNexis.

² Examples of insurance fraud schemes are described by Coalition against Insurance Fraud, "By the Numbers: Fraud Stats" (http://www.insurancefraud.org/stats_set.html).

distribution of damages that is not available to the insurer. If the insurer can objectively verify all damages, it will pay only the verified amount. When the insurer cannot observe the actual value of damages, the claimant can inflate damages by deceiving the insurer. Therefore, moral hazard in insurance claims is most likely to affect bodily injury losses because some portion of these losses is difficult to verify objectively. For example, whiplash and other soft-tissue injuries cannot be detected by objective procedures such as X-rays. The existence and severity of soft-tissue injuries can be evaluated only on the basis of statements made by the claimant. Therefore, once we control for other factors that could affect claiming behavior, differences across states and periods in injury losses should be highly correlated with insurance fraud. Property damage loss is the appropriate denominator because it approximates the universe of potential bodily injury losses. It is very rare for bodily injury to occur in the absence of property damage.³

This fraud proxy is most similar to the dependent variable used by Cummins and Tennyson (1996), who find that the ratio of the frequency of bodily injury claims to the frequency of property damage claims is positively related to opinions regarding the morality of fraudulent insurance acts in a state. Carroll and Abrahamse (2001) also employ a fraud proxy similar to our proxy. They use closed-claims data to compare claiming patterns involving hard injuries with those involving soft injuries. Hard injuries (bone fractures and lost limbs) can be objectively verified. Soft injuries (whiplash, sprains, and strains), however, are harder to verify because the diagnosis is subjective and relies on information provided by the injured party. Carroll and Abrahamse compare the ratio of hard-injury claims to soft-injury claims across states with various regulatory regimes. They find evidence consistent with fewer soft-injury claims in states where regulation is less conducive to bringing a liability suit. Dionne and St-Michel (1991) find that injured workers miss more days of work owing to injuries that are difficult to verify objectively such as lower back pain than workers with verifiable injuries such as bone fractures. They attribute this result to moral hazard.

Following the reasoning described above, we assume that the ratio of potentially fraudulent bodily injury losses to the universe of potential losses (estimated by property damage losses, which are generally more conducive to objective verification) will be highly correlated with the level of insurance fraud in a state.

Other studies have used different measures of fraudulent insurance claims. Some have used conviction rates and other outcome-based measures to proxy for the fraud (Derrig and Zicko 2002). While appropriate for evaluating prosecutorial efforts relative to allegations of fraud, this measure is less appropriate for this study, which also aims to measure fraud that goes un-

³ Automobile collisions with pedestrians are exceptions to this argument. However, pedestrian injuries constitute a small fraction of bodily injury losses.

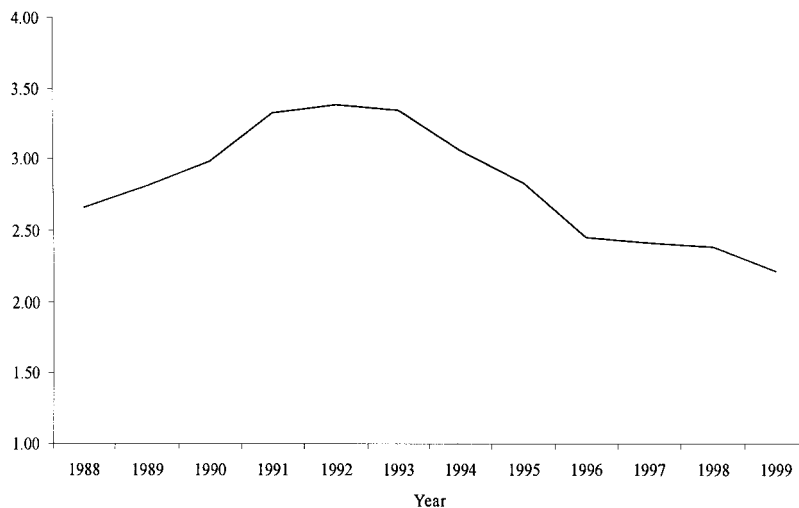


FIGURE 2.—Insurance fraud proxy, 1988–99: the ratio of average loss cost per insured car for all injury coverages combined to the average loss cost per insured car for property damage coverage (Insurance Research Council 2002).

punished. If all perpetrators of insurance fraud were subsequently convicted, moral hazard in insurance claiming would not be a problem worthy of great attention. Thus, such measures are likely biased downward from the actual fraud rate as defined in this study.

The IRC provides the state-level claims information that we used to calculate our fraud proxy. Figure 2 displays the trend of our fraud proxy during the sample period: it increases about 27 percent from 1988 until 1992 and then decreases rapidly thereafter. The value in 1999 is over 36 percent less than it was in the peak year. This pattern is very similar to the overall trend in crime rates, which grew significantly in the late 1980s, peaked in 1991, and dropped precipitously since then.

B. Antifraud Legislation

In Figure 2, we estimate the effect of six laws that are designed to mitigate moral hazard. Although the wording of each law differs slightly across states, the general form and meaning of the law is consistent with the Model Insurance Fraud Act proposed by the CAIF. Information about insurance fraud statutes for each state is obtained from the CAIF and a search of LexisNexis. For each law, we record the year it was first enacted. The first three statute variables described below—Warning, Mandatory Reporting, and Special Investigation Unit—require private-sector action by insurance companies. The

last three—Felony, Fraud Bureau, and License—require action by public authorities including law enforcement and insurance regulators.⁴

The first antifraud variable, Warning, requires insurers to print a warning on application and claim submission forms defining insurance fraud and stating that it is a crime. In 1999, 22 states had such warning laws. The standard language of the warning is, “It is a crime to knowingly provide false, incomplete or misleading information to an insurance company for the purpose of defrauding the company. Penalties include imprisonment, fines and denial of insurance benefits” (CAIF 1999). If the claimants are not aware of the penalty for committing fraud or are unaware that it is a crime to inflate insurance claims, this warning legislation might mitigate moral hazard by increasing the social and psychic costs of fraud and might therefore affect the claimants’ subjective estimate. Obviously, though, this legislation will not affect the behavior of claimants who have no moral scruples.

The second law, indicated by the variable Mandatory Reporting, had been adopted in 35 states by 1999. It requires insurance companies to report any suspected fraudulent claims to the proper authority. In most states, fraudulent claims are reported either to the state insurance fraud bureau or to the district attorney. If insurers already report potentially fraudulent activities to the proper authorities, this law may merely ratify current behavior. In such a case, this statute would not affect moral hazard. If, however, prior to the enactment of this statute, insurers did not report suspected fraudulent behavior and the public agencies have a comparative advantage in dealing with insurance fraud, the law could mitigate moral hazard by increasing the probability that fraud is detected and punished. Alternatively, if insurers are better able to fight fraud privately, reporting suspected fraud to law enforcement officials may increase moral hazard by shifting the responsibility to the inferior party.

The third antifraud law, denoted by Special Investigation Unit, was adopted in 16 states by 1999. It requires insurers in the state to take specific actions in investigating and reducing insurance fraud. Many of these statutes require that insurers create a special investigation unit to discover and prosecute fraudulent claims. Others require insurers to perform similar antifraud activities using existing claims staff or by subcontracting outside investigators. This law should mitigate moral hazard by increasing the probability of detecting fraud. However, the effect on moral hazard will be small to the extent that insurers already have special investigation units in place or have performed similar fraud detection activities prior to enactment of this law.

The fourth type of antifraud legislation, indicated by Felony, classifies insurance fraud as a felonious crime. By 1999, 35 states had adopted this

⁴ Some states have enacted antifraud laws that apply only to workers’ compensation insurance. These laws are not included in our sample. Otherwise, antifraud laws apply to all types of insurance.

designation, operating on the assumption that severity of penalties for committing insurance fraud serves as an effective deterrent. Although insurance fraud is a crime in all states, some states classify it as a misdemeanor carrying only a small fine for first-time offenders. Therefore, classifying insurance fraud as a felony with greater fines and incarceration should mitigate moral hazard by increasing the cost of committing fraud.

The fifth law, denoted by *Fraud Bureau*, requires the state to form an insurance fraud bureau to detect, investigate, and prosecute insurance fraud. Therefore, creating a bureau is intended to affect the expected utility of committing fraud by increasing the probability of detection and prosecution for fraud. Bureaus existed in 35 states by the end of our sample period (1999).

Because insurers often fund fraud bureaus, the creation of a fraud bureau may redistribute antifraud resources from private to public entities. If bureaus have a comparative advantage in reducing fraud, the creation of a bureau should reduce moral hazard. Anecdotal evidence also suggests a complementary relationship between public and private antifraud efforts. Insurance company representatives complain that state prosecutors are often unwilling to devote resources to prosecuting insurance fraud if the fraudulent claim was denied (IRC-ISO 2001). Further, the survey results in the Insurance Research Council Insurance Services Office (IRC-ISO 2001) study suggest that prosecutors are often unwilling to prosecute individual instances of insurance fraud. Establishing a fraud bureau may increase antifraud efforts by prosecutors and law enforcement officials, especially when prosecutors and investigators are assigned to the bureau. This would increase an insurer's incentive to deny a fraudulent claim and to devote resources to the prosecution of fraud. If public and private antifraud efforts are complements, the creation of a bureau should reduce moral hazard.

Incomplete cross-sectional data suggest that fraud bureaus in some states may be less effective than those in other states. If a fraud bureau does not have adequate resources, it may be unable to affect moral hazard. The CAIF (2001) study shows that fraud bureau resources and activity varied substantially across states in 2000. Fraud bureau budgets per capita ranged from \$3.07 in New Jersey to \$.06 in South Carolina; the number of claims referred to fraud bureaus ranged from 22,598 in California to 105 in Connecticut. Given this large variation in resources and activity, the ability of fraud bureaus to decrease fraud may vary significantly across states. Unfortunately, available data are inadequate to facilitate the inclusion of resources and activity of fraud bureaus in our empirical analysis.

The sixth law, denoted by the variable *License*, requires prosecutors to report to the appropriate licensing authority any licensed professional who is convicted of or who pleads no contest to insurance fraud. For example, attorneys are reported to the state's Bar Association, physicians are reported to the Board of Medical Examiners, and insurance agents are reported to the

state's Department of Insurance. This law can mitigate moral hazard by increasing transaction costs of filing a fraudulent claim. The increased punishment for professionals involved in fraud should decrease the pool of physicians and attorneys willing to participate in such claims while increasing the claimant's search costs. It should also increase the compensation demanded by professionals when a crime has been committed. License is the least commonly used of our antifraud laws and was enacted in only seven states in 1999.

Statutory immunity is frequently mentioned as important for fraud control. However, because many states had immunity statutes that existed well in advance of our sample period and few states changed this law during this period, we do not test the efficacy of immunity statutes.

C. Tort Reform Legislation

In addition to antifraud laws, we investigate the effects of tort reforms on our dependent variable. We test four tort reforms: limits on noneconomic damages, limits on punitive damages, limits on joint and several liability, and modification of the collateral source rule.⁵ These reforms can affect severity directly by altering the outcome of bodily injury liability litigation and can affect frequency indirectly by changing the expected value of filing a claim. (Born and Viscusi [1994] and Viscusi et al. [1993] examine the response of the insurance market to various liability reforms.)⁶ While most of these laws were enacted in response to the liability insurance crisis of the 1980s, some reforms were enacted during our sample period. The number of states with such laws increased from 29 to 35 for joint and several liability, from 21 to 24 for collateral source rules, from 24 to 30 for punitive damage limits, and from 10 to 14 for limits on noneconomic damages. The tort reform data are obtained from the American Tort Reform Association (2002).

The first tort reform is legislation that limits damages awarded for noneconomic losses. This includes compensation for pain and suffering, emotional distress, loss of consortium or companionship, and other intangible injuries. These damages involve no direct economic loss and have no precise value. It is very difficult for juries to assign a dollar value to these losses, given the minimal guidance they customarily receive from the court. Several states have modified the rules for awarding noneconomic damages by limiting the amount of damages or the circumstances under which they are available.

⁵ We test these four tort reform laws because they are most likely to affect our dependent variable: they are able to affect the frequency and severity of bodily injury losses. The American Tort Reform Association also tracks changes in prejudgment reform, product liability reform, class action reform, attorney retention sunshine act, appeal bond reform, and jury service reform. The only one of these that could significantly affect automobile insurance claims is jury service. However, none of these reforms were passed before 2003.

⁶ These authors find that tort reform laws generally decrease insured losses, decrease insurance premiums, and increase insurance market profitability.

Capping noneconomic damages should decrease bodily injury liability losses in a state. Because caps are often set at \$250,000 or higher, this law will not directly affect the severity of most automobile insurance losses. However, a cap on noneconomic damages decreases a plaintiff's incentive to file a lawsuit and increases an insurer's incentive to defend, rather than settle, a suit.

Punitive damages are awarded to punish the defendant, not to compensate the plaintiff. The possibility of collecting punitive damages increases the plaintiff's expected value of civil litigation. Many states have passed laws that limit the opportunities for awarding punitive damages and restrict the amount of punitive damage awards. Although one might anticipate that punitive damage caps would have a large effect, punitive damages are not legally insurable in many states.⁷ In these states, a limitation on punitive damages would only deter filing a claim; it would not directly affect the amount of an insured loss. Further, in contrast to products or general liability litigation, in automobile liability litigation, the defendant is more likely to be an individual than a corporation or business. As a result, the likelihood of punitive damages being awarded in automobile liability litigation would be lower, and punitive damage caps would be expected to be less important.

Joint and several liability permits the plaintiff to recover damages from multiple defendants collectively or from each defendant individually, regardless of degree of culpability. For example, if a plaintiff sues three defendants, two of whom are 95 percent responsible for the defendant's injuries but are insolvent and one who is solvent but is only 5 percent responsible, the plaintiff may recover 100 percent of his damages from the latter (American Tort Reform Association 2002). Thus, all else being equal, a plaintiff in a state that recognizes joint and several liability is likely to receive a larger damage award through civil litigation. Many states have enacted laws limiting the application of joint and several liability. Several studies find evidence that joint and several liability reforms reduce the frequency and severity of liability losses (Viscusi et al. 1993; Lee, Browne, and Schmit 1994).

Finally, the collateral source rule of common law says that the defendant cannot present evidence at trial to show that the plaintiff's losses have been compensated from other sources such as health insurance or workers' compensation insurance. Thus, a large percentage of economic damages awarded by juries may already have been paid to the plaintiff by another source. Many states have abolished or modified the collateral source rule. By allowing defendants to present evidence that plaintiffs have already received some compensation for medical expenses, legislation that abolishes the collateral source rule of common law may reduce the damages awarded for bodily injury. All else being equal, this would decrease our dependent variable.

⁷ Examples of such are California, Colorado, Florida, Illinois, Indiana, Kansas, Massachusetts, Minnesota, Missouri, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Dakota, and Utah.

Unlike the two types of damage caps, this law can directly affect the outcome of large and small claims.

Tort reforms can affect our dependent variable by affecting both fraudulent and legitimate bodily injury claims. Our empirical analysis cannot distinguish between the effects of tort reforms on fraud and on other claims. However, these laws could mitigate moral hazard, and it is necessary to control for tort reforms when assessing the effects of antifraud laws.

D. Demographic and Market Structure Variables

The last set of explanatory variables measures the structure of the automobile insurance market and the demographic characteristics in a state. The first market structure variable is a measure of personal injury protection (PIP). Under a no-fault insurance system, each party's insurer pays for his or her own injuries, and bodily injury lawsuits are not allowed. The purpose of this system is to reduce the litigation costs inherent in a tort system. While pure no-fault systems do not exist in the United States, several states have enacted modified no-fault laws for automobile insurance. There are three observed variations of the modified no-fault system. The most restrictive form of modified no-fault insurance limits drivers' rights to sue for bodily injury liability damages unless the damages exceed some threshold and requires the purchase of first-party coverage, or PIP. Two types of thresholds are used to restrict an injured party's ability to sue. If a state uses a verbal threshold, injuries must exceed a definition of severity included in the law—for example, loss of a limb—before an injured party can sue for bodily injury. If a state uses a dollar threshold, medical cost resulting from the accident must exceed a dollar limit before the injured party can sue for bodily injury. In an add-on no-fault system, the state requires drivers to carry PIP coverage but does not limit the ability to sue for bodily injury. Finally, in an elective PIP system, motorists can purchase PIP coverage, even though it is not mandated by statute (for a detailed description of no-fault automobile insurance laws, see Harrington 1994).

A recurring theme in the insurance fraud literature is that PIP may affect the level of fraud (Cummins and Tennyson 1996; Carroll and Abrahamse 2001; Tennyson and Salsas-Forn 2002). Cummins and Tennyson expect modified no-fault laws with verbal or dollar thresholds to reduce the ratio of bodily injury claims to property damage claims, but they make no firm prediction regarding the effects of add-on and elective PIP laws. In states where the ability to sue for bodily injury is limited by no-fault laws, the frequency of bodily injury claims should decrease because PIP claims and bodily injury claims are mutually exclusive. The PIP variable in our model, however, compares the relative expected net benefit of submitting a fraudulent PIP claim with that of submitting a fraudulent third-party claim for bodily

injury.⁸ Our dependent variable measures the amount paid for both bodily injury and PIP claims in the numerator.

A decrease in the insurers' ability to detect and deny fraud in PIP claims may increase the expected utility from submitting a fraudulent PIP claim. Insurance regulators in New York and other states have expressed concern that PIP coverage presents additional opportunities to submit fraudulent insurance claims (Hillman 2002). In many states, insurers must pay or deny a first-party insurance claim within 30 days of the claim's submission to avoid a penalty under Unfair Claims Practices statutes. Moreover, to deny a claim on the basis of fraud, the insurer must be able to prove the fraud at the time that it denies the claim. This effect is exacerbated by the insured's option to wait up to 6 months before submitting a PIP claim in some states. After 6 months, evidence of common injuries such as soft-tissue damage may have disappeared, with evidence of injury being limited to a physician's statement. For these reasons, we expect modified no-fault coverage to increase moral hazard by decreasing the probability that fraud will be detected and punished. This is consistent with a positive relationship between the percentage of PIP losses and our fraud proxy. To our knowledge, extant literature does not empirically test this hypothesis.

There are two reasons why we measure the effect of modified no-fault coverage with the ratio of PIP losses to total bodily injury losses (rather than with a dummy variable that captures the existence of the law). First, there is very little change in the existence of no-fault laws during our sample period, whereas PIP losses vary considerably across states and years. Second, the ratio of PIP losses to total bodily injury losses reveals changes in the use of PIP coverage over time. This information is important because insurance regulators have expressed concern about a rising trend of fraud in PIP claims starting near the middle of our sample period.

The next market structure variable is the market concentration by state. The concentration of a state's automobile insurance market should reduce fraud. It is common for insurers to allocate significant resources to detecting and denying fraudulent claims, but antifraud efforts across insurers are not uniform. Evidence suggests that large insurers are more likely than small insurers to participate in aggressive antifraud measures (IRC-ISO 2001). Cummins and Tennyson (1996) contend that externalities of claims resistance by insurers in a given state, such as reductions in consumers' expectations regarding the probability that a fraudulent claim will be paid, may be more beneficial to insurers with a larger market share in that state. Thus, there is potential for a free-rider problem, where primarily the largest insurers expend

⁸ The expected net benefit of submitting a fraudulent claim equals the expected value of the claim in excess of the fair value of damages multiplied by the probability that the fraudulent act will go unpunished, minus any fine or other decrease in utility from prosecution times the probability that the claimant will be found guilty of fraud.

resources to fight fraud. It is also possible that large insurers can achieve economies of scale in fraud detection and develop a reputation for fighting fraud. In a concentrated market, claimants are more likely to encounter a large insurer than they would be in a less concentrated market. Following Cummins and Tennyson (1996), we do not expect market concentration to affect legitimate claims because denying legitimate claims exposes insurers to bad-faith penalties and other punitive damages. To estimate these effects, our model includes a measure of concentration, Market Concentration, in each state's market for automobile insurance; this variable is a Herfindahl index of direct automobile insurance premiums written by insurers in each state calculated by using data purchased from the National Association of Insurance Commissioners.⁹ Larger observations of this measure indicate a greater degree of market concentration in the state. Therefore, if moral hazard is less problematic in states with more concentrated markets, the market concentration will reduce the fraud proxy.

The last three variables in our model control for demographic factors that could affect a claimant's expected utility from submitting a bodily injury claim. We include the natural logarithm of real per capita income as a control variable, even though its theoretical relationship with fraud is ambiguous. Competing hypotheses suggest that per capita income could increase or decrease the expected utility from submitting a claim. First, the transaction costs of submitting a claim may be greater for people with higher incomes because the opportunity cost of time is increased income. Furthermore, prior research suggests that people with higher incomes face larger opportunity costs of time in committing crimes and higher penalties in the form of lost future wages if convicted (Lott 1992). Similarly, wealthier individuals should be less likely to engage in fraudulent insurance claiming behavior because marginal utility is decreasing in wealth (Cummins and Tennyson 1996). However, Danzon (1984) suggests that the transaction costs of filing a claim could be decreasing in wealth if it is assumed that people with higher incomes are more familiar with the legal system than are those with lower incomes. If the transaction costs of filing a claim are decreasing in wealth, the expected relation between fraudulent claiming and wealth would be positive. In addition, since one component of compensable damages is the lost wages of an injured claimant, individuals with greater income should naturally therefore be awarded greater damages than claimants with lower incomes. Because these effects work in different directions, the effect of per capita income is unclear.

Finally, we control for the unemployment rate and the percentage of a state's population residing in metropolitan areas. In addition to possible wealth effects, we expect the transaction costs of filing a claim to be smaller

⁹ NAIC InfoPro Property and Casualty database (National Association of Insurance Commissioners, 2301 McGee Street, Suite 800, Kansas City, Mo. 64108).

TABLE 1
DESCRIPTIVE STATISTICS

Variable	Mean	SD	Minimum	Maximum
Dependent variable: fraud	2.8219	.9825	1.18	7.44
Antifraud laws:				
Warning	.2200	.4146	0	1
Mandatory Reporting	.4300	.4955	0	1
Special Investigation Unit	.1117	.3152	0	1
Felony	.4183	.4937	0	1
Fraud Bureau	.3950	.4893	0	1
License	.0433	.2038	0	1
Tort reform laws:				
Cap on noneconomic damages	.2333	.4233	0	1
Cap on punitive damages	.5583	.4970	0	1
Modification of joint and several liability	.6667	.4718	0	1
Elimination of collateral source rule	.4650	.4992	0	1
Market structure variables:				
Personal injury protection ratio	.0935	.1400	.0000	.7417
Market concentration	.0844	.0239	.0417	.1914
Per capita income (%)	21,331	4,371	11,695	38,560
Unemployment	5.4031	1.5554	2.2300	11.3900
Metropolitan	.6744	.2079	.2664	1.0000

for unemployed people and for those living in metropolitan areas (Danzon 1984, 1985). First, the opportunity costs of engaging in illegal activity are lower for unemployed people (Gould, Weinberg, and Mustard 2002). Second, Cummins and Tennyson (1996) assert that observed automobile insurance claiming behavior in large metropolitan areas is different from that in other areas of a state. For example, the ratio of bodily injury claim frequency to property damage claim frequency in Philadelphia is more than three times higher than in the rest of Pennsylvania. Similarly, this ratio is more than twice as high in New York City and Los Angeles than their individual state averages. This may be due in part to reduced search costs for the claimant in locating an attorney or physician. Furthermore, large-scale insurance fraud is often perpetrated by organized crime rings that are more likely to be found in large metropolitan areas (CAIF 2004).

Table 1 contains the summary statistics for the primary variables used in this study based on U.S. Census Bureau data.¹⁰

¹⁰ Data for unemployment, real per capita income, and the percentage of state population residing in metropolitan areas are from U.S. Bureau of the Census (1990–2001).

III. EMPIRICAL MODEL

We use two empirical strategies to determine the impact of laws on our fraud proxy—a base specification and a more sophisticated specification that includes time trends before and after the various laws are implemented.

A. Base Specification

Model 1 uses dummy variables to indicate whether each law existed in a given state (i) and year (t):

$$\text{FRAUD}_{it} = \alpha + \sum_{j=1}^{10} \beta_j \text{LAWS}_{ijt} + \gamma' \mathbf{F}_{it} + \sum_{t=1988}^{1999} \tau_t T_t + \sum_{i=1}^{50} \delta_i S_i + \varepsilon_{it}. \quad (1)$$

The variable FRAUD_{it} proxies for insurance fraud in state i at time t . The variable LAWS_{ijt} contains the set of six antifraud laws and the four tort reform measures discussed in Section II. Coefficients are estimated for each of these 10 laws ($j = 1-10$). The term \mathbf{F}_{it} is a vector of market structure variables in state i at time t that includes logged income, unemployment, and urbanization. Time and state fixed effects, T_t and S_i , control for unobserved time trends that affect all states in common and for unobserved characteristics within states that are constant over time, respectively.

The coefficient estimates on the law variables are interpreted as the average effects of the law after it is in existence. It tests whether the fraud proxy is lower on average after implementation of the law than before. However, this simple test may be biased if the laws were adopted in response to changes in fraud. If states adopted these laws because fraud was increasing and the laws lowered fraud, the estimates underestimate the reduction in insurance fraud—the before and after averages would show little difference. For example, in Connecticut the fraud proxy increased by 48 percent between 1988 and 1991. Connecticut created a fraud bureau in 1992, and the fraud proxy steadily decreased by 45 percent from 1992 until the end of our sample period. In the year before North Dakota passed antifraud legislation, the fraud proxy increased by 28 percent. The following year, it decreased by 24 percent. Likewise, if the laws were adopted when insurance fraud was declining, the bias would be in the opposite direction.

B. Before and After Trends

A common approach to control for this type of endogeneity is to use instrumental variables. Valid instruments must be correlated with the decision to enact a change in the law but uncorrelated with the fraud proxy. Unfortunately, it is not technically feasible to use this approach because we would need at least six valid instruments to estimate the antifraud laws and four more to properly identify effects of the changes in the tort laws. Therefore,

TABLE 2
COMPARISON OF LAW ENACTMENT VARIABLES

Year	LAWS (Model 1)	LAWSBEFORE (Model 2)	LAWSAFTER (Model 2)
1988	0	-6	0
1989	0	-5	0
1990	0	-4	0
1991	0	-3	0
1992	0	-2	0
1993	0	-1	0
1994	1	0	0
1995	1	0	1
1996	1	0	2
1997	1	0	3
1998	1	0	4
1999	1	0	5

NOTE.—The three variables for a law enacted in 1994 are shown.

we use an alternative method that controls for before and after time trends for each law, as shown in equation (2):

$$\begin{aligned}
 \text{FRAUD}_{it} = & \alpha + \sum_{j=1}^{10} \beta_j \text{LAWSBEFORE}_{ijt} + \sum_{j=1}^{10} \gamma_j \text{LAWSAFTER}_{ijt} \\
 & + \eta' \mathbf{F}_{it} + \sum_{t=1988}^{1999} \tau_t T_t + \sum_{i=1}^{50} \delta_i S_i + \varepsilon_{it}.
 \end{aligned} \tag{2}$$

By using this estimation technique, we follow a growing literature that uses before and after time trends for each of the state laws.¹¹ Once we estimate these trends, we can test whether the differences in the before and after trends are statistically significant. In addition to being technically feasible, this strategy has two other advantages. First, the coefficient estimates are easy to interpret—positive coefficient estimates on the before and after trends indicate that our fraud proxy was increasing before and after the laws were enacted. Second, it does not shorten the sample, as would the use of a series of leads and lags. Table 2 depicts the differences in the two estimation strategies.

IV. RESULTS

The results of the by-state regressions of model 1 and model 2 are presented in Tables 3 and 4, respectively. The results associated with the wealth proxies

¹¹ Table 2 allows comparison of the LAWS variables in model 1 to the LAWSBEFORE and LAWSAFTER variables in model 2. Others who have used this empirical technique to evaluate the impact of laws are Lott (1998), Mustard (2001), Plassman and Whitely (2003), and Grinols and Mustard (2006).

TABLE 3
RESULTS FROM MODEL 1 BEFORE AND AFTER AVERAGES

Variables	Coefficient Estimate	SE
Changes in laws—antifraud legislation:		
Warning	.032	.0566
Mandatory Reporting	-.055	.0525
Special Investigation Unit	-.0743	.0556
Felony	-.1134 ⁺	.0462
Fraud Bureau	.0642	.0467
License	-.3341**	.0687
Changes in laws—tort reform:		
Cap on noneconomic damages	.4005**	.0865
Cap on punitive damages	.2665*	.0781
Modification of joint and several liability	-.1997	.0892
Elimination of collateral source rule	-.4071**	.1078
Market structure:		
Personal injury protection ratio	2.525**	.4705
Market concentration	-3.7495	.0176
Per capita income (%)	-.3753	.5782
Unemployment	.0816**	.0156
Metropolitan	7.4615*	2.5496
Intercept	3.735	5.4569
R^2	.890	
Adjusted R^2	.874	

NOTE.—Results for the state and time fixed effects variables, included for both averages, are available from the authors. The dependent variable is the ratio of bodily injury liability losses incurred to property damage liability losses incurred and proxies fraud.

⁺ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

are consistent with the previously stated hypotheses. The coefficient estimates of the urbanization variable in model 1 and the unemployment variable in models 1 and 2 are statistically significant. They are positively related to our insurance fraud proxy and thus are consistent with theory and prior empirical research. The significance of these variables provides indirect evidence of the appropriateness of our fraud proxy.

Table 3 presents the results of the basic regression (model 1) described above. The coefficient estimates for two of the six antifraud law variables—classification of insurance fraud as a felony (Felony) and revocation of licenses of professionals (License)—are negative and statistically significant and indicate that these two laws reduce the insurance fraud proxy by 11.3 and 33.4 percentage points, respectively. The coefficient estimates for the other four law variables—Mandatory Reporting, Warning, Special Investigation Unit, and Fraud Bureau—are not statistically different from zero.

Three of the tort laws—modification of joint and several liability, caps on noneconomic damages, and caps on punitive damages—have statistically significant effects on the fraud proxy. States that cap noneconomic damages and those that cap punitive damages display a higher value of the fraud proxy

TABLE 4
RESULTS FROM MODEL 2 BEFORE AND AFTER TRENDS

VARIABLES	BEFORE TREND		AFTER TREND		DIFFERENCE: <i>F</i> -VALUE
	Coefficient Estimate	SE	Coefficient Estimate	SE	
Changes in laws—antifraud legislation:					
Warning	-.0072	.0165	-.0362 ⁺	.0194	1.36
Mandatory Reporting	-.0705**	.0175	-.0219	.0183	4.05*
Special Investigation Unit	.0483**	.0167	-.074**	.0235	17.91*
Felony	.0117	.0131	-.0181	.0144	3.33 ⁺
Fraud Bureau	-.0078	.0147	.0218	.0187	1.89
License	-.043**	.0145	-.1318**	.0437	3.25 ⁺
Changes in laws—tort reform:					
Cap on noneconomic damages	.0321	.0255	-.0475**	.0118	8.67**
Cap on punitive damages	.0214	.0275	-.0123	.011	1.34
Modification of joint and several liability	-.093**	.0293	-.028*	.0112	4.54*
Elimination of collateral source rule	-.1128*	.0567	.03**	.0109	6.09*
Market structure:					
Personal injury protection ratio	2.9403**	.612			
Market concentration	-2.8608	2.7979			
Per capita income (%)	-.5533	.8226			
Unemployment	.0933**	.0203			
Metropolitan	4.4717	3.874			
Intercept	5.8103	7.7934			
<i>R</i> ²	.907				
Adjusted <i>R</i> ²	.892				

NOTE.—Results for the state and time fixed effects variables, included for both trends, are available from the authors. The dependent variable is the ratio of bodily injury liability losses incurred to property damage liability losses incurred and proxies fraud.

⁺ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

after the law, and those that eliminate the collateral source rule have less fraud after the law. All of these results are statistically significant; however, the results change in the specification that controls for changes in fraud before and after the laws are implemented.

The coefficient estimates for each of the market structure variables have the anticipated signs. However, there is no evidence supporting the hypothesis that firms with a greater share of the market expend more resources to fight fraud.

The positive and highly statistically significant coefficient estimates on the PIP ratio suggest that there is an inflationary effect of no-fault coverage on claim costs. This is the opposite of Cummins and Tennyson's (1996) result regarding the effect of no-fault laws on insurance claiming behavior, but it does not contradict their hypothesis. Cummins and Tennyson expect no-fault coverage to reduce the frequency of bodily injury claims because bodily injury claims and PIP claims are mutually exclusive. However, because our

dependent variable measures the amount paid for bodily injury and PIP claims in the numerator, the PIP variable in our model compares the relative expected net benefit of submitting a fraudulent PIP claim with that of a bodily injury claim. Thus, our finding that the ratio of claims paid under PIP coverage increases the fraud proxy is consistent with suggestions in the trade press. Hillman (2002), for example, believes that regulation restricting the insurers' ability to deny fraudulent PIP claims makes it easier for insureds to inflate PIP claims compared to other bodily injury claims.

The positive and statistically significant coefficient estimates on both the unemployment rate and the fraction of the population in metropolitan areas (Metropolitan) are consistent with the predictions that these groups have lower transaction and opportunity costs from engaging in fraudulent activity. Because of potential positive or negative correlations between income and fraud, the sign of its coefficient estimate is uncertain. The empirical results show a slightly negative coefficient with a very high standard error, which indicates that these opposing effects approximately offset each other.

Table 4 presents the results for the before and after trends (model 2) and reports the results of whether the before and after trends differ. The results for the antifraud and tort reform laws are very different when we use the before and after time trend variables. We now find the strongest evidence that three of the antifraud laws—the establishment of special investigation units, the classification of insurance fraud as a felony, and the revocation of licenses of professionals who engage in fraud—reduce the fraud proxy. We find a statistically significant decrease in our fraud proxy when considering the difference in the before and after time trends for each of these laws. However, laws requiring insurers to report suspected fraudulent claims show decreases before the law and slight decreases (not statistically significant) after the law is implemented. Although the lower after effect is not statistically significant on its own, the difference between the before and after trends is statistically significant and positive.

To better understand the motivation for and interpretation of the before and after trends model (model 2), it is useful to consider the differences in results produced by the two models. Model 1 compares the average fraud proxy before a law is enacted with the average after a law is enacted. Model 2 compares the rate of change in the fraud proxy before a law is enacted with the rate of change after a law is enacted. For example, the coefficient estimate for Special Investigation Unit in the before and after averages model (model 1) is not statistically different from zero; therefore, the average of the fraud proxy before the law is enacted is not significantly different from the average after the law is enacted. This test would be biased if the fraud proxy was increasing before enactment and decreasing after enactment.

The same law produces different results in the before and after trends model (model 2). The before-trend coefficient estimates indicates that, all else equal, the fraud proxy increased by an average of .048 per year before

the law was enacted in states that passed the law during our sample period. The after-trend coefficient estimate indicates that the fraud proxy decreased by an average of .074 per year after the law was enacted. The F -value (17.91) indicates that the difference between the before and after coefficient estimates is statistically significant, and the magnitude of the difference suggests that this law reduced the insurance fraud proxy by .122 annually. Thus, the model 2 results are consistent with the notion that laws requiring special investigation units mitigate moral hazard in automobile bodily injury claims by increasing the probability that insurance fraud will be detected.

Interpretations of the coefficient estimates for the other three laws displaying statistically significant effects on fraud—License, Felony, and Mandatory Reporting—are slightly different from that of Special Investigation Unit. Laws revoking the licenses of professionals involved in insurance fraud have reduced the rate of change in the fraud proxy. Before these laws were enacted, the fraud proxy decreased by an average of .043 annually; after legal enactment, the average rate of change dropped even lower to $-.132$. The difference between the before and after coefficient estimates is significantly different from zero at the 10 percent confidence level (F -value = 3.25), and the magnitude of the effect is equivalent to a .089 annual reduction in the fraud proxy. This evidence is consistent with the effect of the law in decreasing moral hazard in automobile insurance by increasing the cost to the claimant of obtaining a willing participant in insurance fraud, such as a physician or attorney.

The classification of insurance fraud as a felony also decreases the rate of change in the fraud proxy. Although neither the before-trend nor the after-trend coefficient estimates are statistically different from zero, the difference between them is statistically different from zero at the 10 percent confidence level (F -value = 3.33). The magnitude of the effect is equivalent to a .03 reduction in the fraud proxy per year. This evidence is consistent with the law decreasing moral hazard by increasing the cost of committing fraud.

Requiring insurers to report suspected insurance fraud to the proper authorities increases the rate of change in our fraud proxy. The before-trend coefficient estimate indicates that the fraud proxy was decreasing by an average of .071 per year before such laws were enacted. The after-trend coefficient estimate is not significantly different from zero, and the difference between the before and after estimates is significant at the 5 percent level (F -value = 4.05). The magnitude of the effect equates to a .049 annual increase in the fraud proxy. This evidence suggests that when states enact the mandatory reporting law, previously declining fraud rates stop declining. The result is consistent with our hypothesis that requiring insurers to report suspected fraud to the proper authorities may increase moral hazard by decreasing the probability of detecting and prosecuting fraud. One possible explanation for this result is that private antifraud efforts are more effective than public efforts.

The difference between the before- and after-trend variables is not statistically significant for fraud warnings or the creation of a fraud bureau. As noted in Section II, this result for fraud bureaus may indicate that public and private antifraud efforts offset each other. Alternatively, given the differences across bureaus in available resources and activity, some bureaus may decrease fraud while others increase fraud, thus effectively canceling each other out in the empirical analysis. Available data do not facilitate an empirical test of this hypothesis.

We also find interesting results in the before- and after-trend analysis of the tort reform variables. Limiting noneconomic damages has a statistically significant and negative effect on the fraud proxy. The difference between the before- and after-trend variables for limiting noneconomic damages is statistically significant at less than the 1 percent level. There is some evidence that altering the collateral source rule and modifying joint and several liability increases the fraud proxy. Tort reforms involving punitive damages have no statistically significant before or after effects, consistent with the analysis of Eaton, Mustard, and Talarico (2005), who find that the decision to seek punitive damages does not have a significant statistical effect on the majority of phases of the tort litigation process.

The results for our market structure variables are similar to those in Table 3. The positive and statistically significant coefficient estimate on the PIP ratio reinforces the belief that no-fault laws may inflate claim costs. The qualitative results for the other market variables are similar in the two specifications. While the Metropolitan variable is not statistically significant in this specification, states with high unemployment rates display a higher value of our fraud proxy. Per capita income has no effect in either specification.

V. CONCLUSION

In the face of growing concern over the economic and societal implications of insurance fraud, many have proposed a variety of steps to reduce it. A popular approach has been the enactment of antifraud legislation in many states. The number of states with at least one antifraud statute has increased by more than threefold since 1988. In spite of the substantial increase in these laws, there are no thorough empirical analyses of their efficacy.

Not surprisingly, the evidence suggests that while some antifraud laws appear to reduce insurance fraud, the effects of all laws are not equal. An important feature of our study is the unique methodology to control for trends in the fraud proxy before the laws were implemented. These estimates are consistent with laws being enacted while fraud was changing, thus biasing the results of the basic ordinary least squares specifications. Finally, the strongest evidence of fraud mitigation effects is associated with the mandatory special investigation units, the classification of insurance fraud as a felony, and the mandatory reporting of professionals to licensing authorities.

However, laws requiring insurers to report potentially fraudulent claims to law enforcement authorities increase fraud. This result may reflect a tendency for this type of law to substitute less productive state activity for more efficacious private efforts that would have been undertaken in the absence of this mandate.

The tort reform laws also have mixed effects. Modification of joint and several liability and elimination of the collateral source rule are associated with an increase in our fraud proxy. Reforms on punitive damages have little effect on fraud. The most promising tort reform limits noneconomic damages, which reduces the fraud proxy.

The market structure variables have very strong effects. States with a higher ratio of PIP claims have substantially higher values of the fraud proxy. This is consistent with our hypothesis that the expected net benefit from submitting a fraudulent PIP claim is greater than that from a bodily injury claim. The difference is likely due to statutory regulations that limit an insurer's ability to deny PIP claims without facing significant bad-faith penalties. Also, as expected, states with higher unemployment rates display substantially higher values of our fraud proxy.

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