The Value of an Attorney: Evidence from Changes to the Collateral Source Rule*

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Abstract

One of the more contentious questions in law is the value of lawyers to their clients. Yet a simple comparison of recovery in cases with and without lawyers will not yield a satisfactory estimate of the value of an attorney since hiring a lawyer is endogenous. We utilize modifications to the collateral source rule that require reducing trial awards at trial by the amount of payments from first party insurance as an instrument. Theoretically, these modifications to the collateral source rule reduce the likelihood a claimant will hire a lawyer and reduce the likelihood that the lawyer accepts the case. Consistent with our model the modification of the CS rule has a non-trivial effect on the probability of hiring attorneys. There is one problem with our candidate instrument; modifications to the CS rule that require offsets have a direct effect on recovery and hence violate the exogeneity requirement for a valid instrument. We propose an alternative estimator that uses CS rule changes as an invalid instrument and bounds the impact of lawyers. We find that even the upper bounds of our estimated impact of hiring a lawyer are negative for our measures of accident victim's total recovery. The upper bound of the reduction in total payment received from hiring a lawyer range from over -$10,407 to approximately -$20,320 in 2002 constant dollars depending on model specifications and sample restrictions. This is in contrast to a gain from hiring a lawyer of $11,042 to $16,796 when estimated using the conventional approach. We also estimate the quantile effect of hiring an attorney. Here the effect for all but the largest claims is close to zero with the estimated bounds typically straddling zero. However, for the top 3% of claims we find very large negative impacts suggesting that lawyers actually reduce the client's recovery relative to the amount they would have received without representation.

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1 Introduction

One of the more contentious questions in law is the value of lawyers to their clients.[1] Unlike other services, in which the fact that people are willing to pay for the service strongly suggest that the service creates value, there are theoretical reasons to believe that lawyers could reduce a client's expected recovery.[2] The most obvious problem is that lawyers could charge more in fees than the increase in payments from third party source their services generate for their clients. Beyond their fees lawyer's presence may generate additional costs to claimants, which would be passed on to claimants as compensation absent the litigation. This could be the case if, for example, expert witness' testimony generates less on average than its additional cost in recovery. Moreover, since claimants typically lack the experience with or, expertise about, the civil justice system, they are unable to estimate whether they would be better off without an attorney. Much like doctors, realtors, mutual fund managers and funeral directors, the client's lack of expertise necessitates hiring someone to represent their interests but this also means the potential client is uncertain as to whether the service is worth the cost.[3] In the context of this paper, tort litigation resulting from auto accidents, major accidents are, thankfully, rare, and therefore the typical person has very little experience with the complex process of recovery from a third party who may be liable for their injuries.

Even if lawyers could produce an increase in the value of the claim there are reasons to be concerned that they lack the incentives to do so once retained. Lawyefrs in tort cases are paid a percentage of the proceeds of the litigation when the client recovers. Moreover, in the typical contract the client does not pay fees for experts and court costs if there is no recovery but such fees are deducted from the total recovery before the lawyer's contingent fee is calculated. While there are reasons to believe that contingent fee arrangements between lawyers and clients may be more efficient than hourly fees, law and economics has long recognized that under any fee arrangement

[1] For an extensive discussion of the literature see Greiner and Pattanayak (2012).

[2] See Ashenfelter, Bloom and Dahl (2013) and Ashenfelter and Dahl (2012) for an example in which hiring a lawyer is modeled as a prisoners dilemma game.

[3] For a discussion of each of these professions see Fuchs (1978), Gruber and Owing (1996), Chavalier and Ellison (1997), Harrington and Krynski (2002), and Levitt and Syverson (2008).

the lawyer has distinct interests from the client.[4] These divergent interests generally cover three dimensions: the willingness of lawyers to take a case and bring suits on behalf of a prospective client; the lawyer's willingness to settle a case before trial and the lawyer's effort in pursuing the case.

We use the modification of the common law collateral source rule (CS rule) as an (invalid) instrument to estimate the value of attorneys in auto accident claims. Under the CS rule, the fact that a plaintiff had received payments to compensate them for their injuries could not be introduced as evidence in a civil lawsuit. In practice, this means that if the plaintiff's injuries were covered by medical insurance, the plaintiff could receive compensation for the same injury from both the defendant and the plaintiff's own medical insurance.[5] Beginning in the 1970s a number of states modified the CS rule and required that any payments from third party sources be deducted from the plaintiff's recovery from the defendant. The stated aim was to prevent duplicate recoveries but tort reform advocates also stressed that these reforms would reduce the incentives to sue in the first place.

We use modifications to the CS rule as an instrument for the decision to hire a lawyer. The problem with our instrument is that while it does reduce the likelihood an accident victim hires a lawyer; it also reduces the possible recovery. That is, our instrument violates the exclusion restriction required of a valid instrument. Our solution to this violation comes from the fact that our data, which cover accidents that occurred in the years 1982 to 2002, also contain information on compensation from other sources such as health insurance. Individuals in our sample can choose either to hire a lawyer in their case against a potentially liable third party or simply accept whatever payments they would receive from third party insurance plus their own insurance.[6] Since payments from other sources are mechanically deducted from recovery at trial, we are therefore able to construct a maximum possible recovery for the case if the CS rule had not been modified. Because

[4]See Rubinfeld and Scotchmer (1993) and Dana and Spier (1993).

[5]It is, of course, possible for the medical insurance provider to subrogate the claim and recover their costs from any recovery the plaintiff received. That is, the medical insurer essentially joins the case and is reimbursed out the amount that the plaintiff would have received in compensation from the defendant. In practice, it appears such subrogation was rarely used in auto accidents (see Spurr, 2017).

[6]For a more extensive discussion of the compensation of auto accident victims see Hammitt (1985), Rolph et al. (1985) and Hensler et all. (1991).

the plaintiff might not recover the full amount of this offset at trial our measure can only produce a bound rather than a point estimate. Nonetheless, our measure allows us to use modifications to the CS rule as an "invalid" instrument; i.e. one that has impacts on the likelihood of an attorney being retained while violating the exogeneity condition.[7]

Consistent with Greiner and Pattanayak (2012), and in contrast to Hammitt's (1985) earlier evaluation of auto accidents, we find that the upper and lower bounds of our estimated impact of hiring a lawyer on total payment are mostly negative. The upper bound of the reduction in total payment received from hiring a lawyer range from over $18,407 to approximately $10,407 in our preferred specification. This effect is quite large for an average payment, in 2002 dollars, of approximately $15,673. To find out why this is the case, we estimate the quantile effect of hiring an attorney. Here the effect for all but the largest claims is close to zero with the estimated bounds typically straddling zero. However, for the top 3% of claims we find very large negative impacts suggesting that lawyers actually reduce the client's recovery relative to the amount they would have received without representation.[8]

We find evidence of a negative relationship between the value of a lawyer and the relative fault of the victim. We assume that victims are, on average, the most likely to be at fault, i.e. have no third party that is liable for their injuries, in single vehicle accidents. We assume victims are the least likely to be at fault, i.e. the most likely to have a liable party who is responsible for their injuries, in accidents in which the victim was a pedestrian struck by a car. Collisions involving more than one vehicle are intermediate cases in which fault is more likely to be ambiguous or shared

[7]Our solution is to define a potential outcome that is not affected by the direct effect, to link the potential outcome to the observed quantities, and use it to obtain an upper and lower bounds of the potential outcome. This estimation provides us a sharp bound for the treatment effect. This paper contributes to the invalid IV literature that studies properties of the instrumental variables methods without assuming the exogeneity condition. Nevo and Rosen (2012) study the identification power of assuming that the correlation between the instrument and the error term has the same sign as the correlation between the endogenous regressor and the error term. Using such sign restrictions, they derive bounds for the parameters of interest. Manski and Pepper (2000) obtain the identification region when the mean response conditional on an instrument is assumed to be weakly monotone for any given value of the endogenous covariate. Flores and Flores-Lagunes (2013) also derive bounds under related but different assumptions. Kolesar, Chetty, Friedman, Glaeser, Imbens (2015) show that point identification is possible if one assumes that the direct effects of the instrument on the outcome are orthogonal to the direct effects of the instrument on the endogenous regressor. Other related studies include Hahn and Hausman (2005) who compare biases for OLS and 2SLS in the presence of direct effects, and Conley, Hansen and Rossi (2012), who propose sensitivity analyses in the presence of possibly invalid instruments.

[8]This is also consistent with Hammitt (1985) who also finds that for very large claims cases with lawyers actually receive less compensation than cases with similar damages where the claimant does not hire a lawyer.

between drivers. In our bounds analysis this is what we find. The bounds for lawyer's value added is the most negative in a sample including single vehicle accidents, and the least negative for the sample including pedestrian accidents, with the collisions only sample the intermediate case.

We find that lawyer's value may be positive in no-fault states, but strictly negative in fault states. Although it is perhaps counterintuitive, given that the aim of no-fault was to reduce the need for lawyers by removing fault, there is evidence that lawyers in no-fault states help their clients get out of the no-fault system if their claims are worth more in the fault system. Because it is possible to manipulate no-fault system's thresholds by claiming more or less in terms of damages, expert advice on which part of the system will maximize recovery maybe particularly valuable in no fault states.

One potential threat to identification is that modifications to the CS rule impact recovery in some way other than the decision to hire a lawyer or reductions in payments (which our estimation method controls for). The biggest concern is that lawyers will respond to modifications in the CS rule by increasing the fees they charge their clients or simply exert less effort on the client's behalf. Given that modifications to the CS rule in all but two states base the lawyers contingent fee on pre-deduction recovery, the lawyers payments for a given case should not change and hence effort should be affected. Consistent with the expectation we do not find any evidence that there were significant changes in total fees after a CS rule change. We also find no post CS rule modification changes in several variables reflecting lawyer's effort level in the case. We also exploit variations in outcome measures (payments before or after deducting fees) and in state level rules that determine lawyer's contingent fees. We repeat the analysis under different scenarios and obtain consistent results.

Although we find that the lawyer's value added in terms of recovery is heterogeneous, one consistent finding is that lawyer's effect on the total recovery is negative. Our quantile bounds analysis finds that this negative impact of lawyers on recovery is due to the large negative impacts at the right tail of the value of claims distribution. The evidence also suggest a potential substitution effect. Our results are consistent with a possibility that lawyers generate gains in recovery from the injurer's insurance or from litigation. However, accident victims without lawyers tend to utilize their

own insurance to a greater extent. The reduction in the use of personal insurance to compensate for accident losses is less than the increase in recovery from third parties when an attorney is hired. Overall, after deducting lawyer's fees, the presence of lawyers tends to reduce their client's total recovery.

In the next section we provide additional background on the the incentives facing lawyers and clients under contingent fees and provide additional detail our approach to estimating the value of a lawyer. In section 3 we provide a simple model of the impact of modifying the CS-rule on the decision to hire an attorney. Section 4 details our estimation method how we utilize the modifications to the CS-rule as an invalid IV. Section 5 provides a description of the data. Section 6 presents the results of our bounds analysis and section 7 explores several threats to inference and effects heterogeneity. Section 8 concludes.

2 Background

2.1 Aligning Lawyer and Client Interests

To understand the divergence of interests between lawyer and client, the literature has focused on the incentive effects of the contingent fee contract. These contracts typically give the lawyer one third of the total recovery (often net of costs other than the lawyer's time) but typically provide no additional payment for time worked. In this context, it is clear that a contingent fee contract provides better incentives for the lawyer to reveal the value of a claim to a client than an hourly fee contract. A lawyer on an hourly fee contract has the incentive to take any case regardless of expected outcome. However, a contingent fee contract is not optimal since, in most models, it is efficient to bring cases worth more than legal costs meaning that a lawyer on a contingent fee accepts too few cases.[9] A similar logic applies to settlement. Lawyers under contingent fee arrangement want to settle too frequently since they receive only a portion of the proceeds of a trial and yet in the models the lawyers typically pay all of the litigation costs. Finally, in most models assume that the client cannot directly observe the lawyer's effort meaning that under an hourly fee, the lawyer

[9]Since the lawyer receives only one third of case value to cover his or her investment in the case, the lawyer will only want to accept a case in which a third of the expected value of the case is greater than these costs.

would expend no effort but even under a contingent fee the lawyer will expend less effort than the plaintiff would find optimal in a world where effort was observable.[10] All of these incentive effects tend to diminish the value of hiring a lawyer.

One dimension that has not been extensively studied is the lawyer's non-labor investment in the case. The plaintiff's lawyer typically pays court costs and expert witness expenses up front and is then reimbursed out of any recovery. Contingent fees are often based on recovery net of expenses. This introduces a more complex dynamic in which lawyers are sharing the cost of the expenses with the client unless they do not recover. This would cause lawyers to underinvest in value creating expenses since they bear all the risk in the event of a loss and pay one third of the expenses in the event of recovery but only recover a third of the benefit of the investment.[11]

The second factor diminishing the value of a lawyer is that contingent fees allow lawyers to finance the claims of their capital-constrained clients. Since the lawyer is essentially working for free until the claim is resolved he or she is issuing the client a contingent loan paid back only in the event the client recovers. As with all investments financed by borrowed money the value of payoff must be net of the borrowing costs. In a world of perfect information and repeated dealings with the civil justice system, we might expect clients to, at least on average, borrow from plaintiff's attorneys only when the expected payoff is greater than the lending costs but in the realm of personal injury litigation, these conditions need not be met.[12]

Models in law and economics typically assume a zero recovery is the alternative to the value of a lawyer. That is, absent bringing a claim the client would recover nothing. Yet this is often not the case. In the cases examined here, auto accidents, the value of a lawyer needs to be evaluated

[10] Or in which the client could legally sell the case to the attorney: a contingent fee of 100%.

[11] Engstrom (2009, 2011) has argued that this incentive has produced "settlement mills"; law firms specializing in settling auto cases with a minimum investment in the case. To the extent that Engstrom is correct and settlement mill firms dominate auto accident litigation, we would expect to see lawyers adding very little value on average as the incentive to invest would be pushed toward the opportunity cost of lawyer's time. Engstrom also points out that while settlement mills on average may add very little to the amount clients recover, net of fees, beyond what they would recover without the lawyer, these firms may add value in faster recovery, reduced court congestion and other non-recovery dimensions.

[12] As in most instances of agency problems, the most important check on plaintiff's lawyers expropriating the proceeds of litigation is a competitive market. There is considerable dispute about whether the market for plaintiff's lawyer is competitive. First, there are the above-mentioned information problems. In addition, there is the fact that law school admissions and bar exams represent at least a partial barrier to entry. Of course, the lack of a competitive market is not required for lawyers to grab a larger share of the litigation surplus.

relative to what the client would have received in compensation without the lawyer. For example, it is common in auto accidents for victims to get an offer from the other party's insurance company even if the injured party does not retain a lawyer.

2.2 Estimating the Value of an Attorney in a Case

The theoretical literature offers no consensus on the value of an attorney suggesting the importance of empirical estimates. Yet the simple comparison of recovery with and without lawyers will not yield a satisfactory estimate of the value of an attorney since hiring a lawyer is endogenous. Greiner and Pattanayak (2012) survey the empirical literature, which consists of hundreds of studies. They conclude that beyond the three randomized controlled trials, inclusive of their own study, the rest of the literature is not worthy of credence.[13] The difficulty is that all of the studies compare the outcome of cases with and without lawyers despite the fact that the decision to hire a lawyer and the lawyer's decision to accept the case are endogenous.[14]

Randomized control trials have been used to estimate the value of an attorney. One limitation, acknowledged by those conducting the RCTs, is that the stakes of the case must be fairly small. Since lawyers are paid on a contingent fee valuable cases with plaintiffs who are liquidity constrained are regularly financed. RCTs of the value of a lawyer, because of the existence of a contingent fee based market for lawyers, have to take place in claims in which it is not possible to retain a lawyer on contingent fees and in which the potential plaintiffs are liquidity constrained (i.e. eviction cases or debt collection). Thus by construction the value of a lawyer is potentially very different in the types of cases amenable to a RCT than in other cases.

The alternative to a randomized control trial when there are endogenous variables is an instrument that affects the decision to hire a lawyer but does not affect claim value. Yet contingent fees present a different problem for natural experiments, in which some exogenous change is as good as random in assigning individuals to a treatment or control group. Our instrument is changes to the collateral source rule. The issue is that, like modifications to the collateral source rule, almost

[13] In fact, Greiner and Pattanayak (2012) argue that the problems are so severe as to render their conclusions untrustworthy.

[14] For an example of an alternative approach to the selection issues inherent in litigation data see Ashenfelter and Dahl (2012) and Ashenfelter, Bloom and Dahl (2013).

any measure of the value of an attorney would need to utilize the recovery received by the plaintiff. Because the lawyer is paid out of the recovery and chooses how much effort to put into the case based on the expected recovery, it is difficult to see how a law change could affect the likelihood of hiring an attorney without impacting eventual recovery, making the natural experiment by our definition an "invalid" IV.

Our candidate instrument is changes to the collateral source (CS) rule (or collateral source doctrine). As noted above, this evidentiary rule prohibits the admission of evidence of compensation the victim has received from other sources. For example, the defendant would still be liable for the full amount of the victim's injury even if the victim had received payment from their health insurance to cover injuries resulting from the defendant's negligence. Beginning in the mid-1970s several states modified this rule to prevent compensating the plaintiff twice for the same injury and in many cases states mandated that any award be offset by the amount paid to plaintiffs from collateral sources. In Section 3, we present a simple demand and supply analysis of the decision to hire an attorney and demonstrate that when the collateral source rule is modified, claimants are less likely to hire an attorney. The modification of the CS rule, our instrument, has a non-trivial effect on the probability of hiring attorneys, our treatment. This theoretical prediction is also supported by empirical evidence which we discuss below. This leads us to conclude that our candidate instrument satisfies the relevance requirement of being a valid instrument.

One concern is that the decision by a state to modify the CS rule itself may not be random. For example, if the state was expecting a dramatic rise in litigation, which might affect the demand for lawyers, it might adopt a CS rule modification requiring offsets. Fortunately, our data is sufficiently rich in detail on the history of law changes that it give us an additional source of random variation. When they introduced modifications to the CS rule, some states applied it to all cases, while other states restricted it to cases involving allegations of medical malpractice. Since we study only auto accidents, the law change in the latter should have no impact on our cases. Sloan and Chepke (2008) suggest that scope of the modification, including by implication the decision to include all cases or just medical malpractice cases, was essentially random and depended on the specific political coalitions in the state that led to passage of the reform. Tort reform primarily arises

out of insurance crises affecting doctors, but in certain states the coalition supporting reform also included insurers more broadly. In a subset of these, the reform effort was successful. Also in a subset of those successful states the reform was not overturned by the courts. The result is that the comprehensiveness of modifications to the collateral source rule appears to be random. Thus while the modification itself may not be random, we argue the decision to apply it more broadly or narrowly is random.

There is a more serious concern with our candidate instrument, however. Specifically modifications to the CS rule have a direct effect on recovery and hence violate the exogeneity requirement for a valid instrument. Lawyers are paid a fraction of the eventual recovery, and modifications to the CS rule typically have a direct effect on recovery by requiring that collateral source payments must be offset. In the terminology of instrumental variable (IV) estimation method, our candidate instrument has a direct effect on outcome variables, which makes it a 'invalid' instrumental variable.

We solve this invalid instrumental variable problem by considering a potential outcome variable that blocks the direct effect of instrument on outcome but allows the indirect effect of instrument through treatment.[15] To be specific, we ask what would be the total payment if offsets under the modified CS rule are to impact the decision to hire a lawyer but then did not materialized at trial. By using this potential payment instead of the observed payment as our outcome measure, we can define a local average indirect effect that has a causal interpretation. If the direct effect were blocked, the modification to the CS rule impacts payment only through the decision to hire a lawyer or not. This would have a causal interpretation.

This potential outcome variable, however, is not directly observed, so the local average indirect effect may not be identified from data.[16] Fortunately, we can link the potential outcome to observed quantities because offsets reduce payments in mechanical ways; it literally reduces the award at trial dollar for dollar with the payments form relevant collateral sources. By calculating the maximum

[15] A quantity like this has been defined and used in the treatment effect literature, mainly outside of economics. It was called the pure indirect effect (Robins and Greenland (1992), Robins (2003)), or the natural indirect effect (Pearl (2001)). In economics, Flores and Flores-Lagunes (2013) use the concept to define the mechanism treatment effect.

[16] Intuitively the existence of the offset does not mean that the plaintiff would have recovered the entire amount paid by the plaintiff's insurance. The total of payments from first and third party insurance simply represents the maximum the plaintiff could have recovered. For example, the degree of the defendants negligence, the ease of proving a link to the defendants conduct or other factors could reduce the actual award the would have been received absent the collateral source offset from the maximum value of the plaintiff's injury as captured by our measure.

offsets allowed by the modification enacted in the relevant state, we are able to bound the potential payment. In this way, we can identify a sharp bound on the local average indirect effect. The details of the estimation strategy are in Section 4.

3 Impact of Modifying the CS Rule on Hiring an Attorney

In this section, we examine a simple theoretical model of the decision to hire an attorney. Using a simple demand and supply analysis for attorney's service, we demonstrate that a modification of the CS rule that requires offsets of any judgment at trial by the amount the plaintiff received from collateral sources, such as their personal health or auto insurance, reduces the likelihood a claimant retains an attorney. This shows the relevance of the modification of the CS rule as an instrument and hence justifies our estimation strategy.

Suppose that the claimant is legally entitled to an amount M in compensation from the defendant assuming the defendant is either found liable or willing to pay (in order to avoid at trial).[17] Suppose that the claimant will receive M from the (potential) defendant or his/her insurer with probability P_0 if no lawyer is hired. If the claimant hires an attorney, he/she faces a probability P_1 of recovering M. We assume that $P_1 > P_0$. Assume that the attorney, if he/she agrees to take the case, faces a cost of C to pursue the claim. This is generally assumed to be the opportunity cost of the attorney's time plus expenses, such as, court costs and expert witness fees. In this section, we follow the convention in the literature and assume that the client pays none of the expenses. In reality, however, expenses are often divided between the attorney and client in the event of a successful claim. We consider such an extension in Appendix A.1 and show that the cost sharing does not change conclusions. We normalize the cost to the claimant of pursing the claim without the attorney to be 0. The attorney is paid a fraction β of the recovery M but only if the claim is successful.

Let L be the amount that the claimant receives from collateral sources. We first consider the case where this amount is irrelevant because the amount owed by a liable defendant is not offset

[17] This setup abstracts away from the decision to settle or litigate the claim to trial and hence M can be thought of as the expected outcome of successful litigation.

by payments from collateral sources. This applies to cases or accidents that occurred in states that did not modify the CS rule. In such cases, the potential client receives $P_1(1-\beta)M$ if they hire an attorney and P_0M if they do not. Therefore they will hire an attorney if $P_1(1-\beta)M > P_0M$ or if $(1-\beta)P_1 > P_0$. The attorney will take the case if $\beta P_1 M > C$.

Suppose that the state has modified the CS rule such that offsets are required. There are two cases to consider. In most states, the law allows attorneys to receive contingent payments based on M. In Florida and Minnesota, however, state law requires attorneys to receive contingent payments based on $M - L$.

In the first case, under the modified CS rule, the claimant receives $P_1(M - L - \beta M)$ if they hire an attorney and $P_0(M - L)$ if they do not (assuming that the potential defendant's insurer will reduce any settlement offer by the expected offsets at trial). So the condition to hire an attorney is $(1-\beta)P_1 - P_1\beta\frac{L}{M-L} > P_0$. There is a negative demand effect since the client would recover less after fees under a modified CS rule. The attorney will take the case if $\beta P_1 M > C$, so there would be no supply effect.

In the second case, under the modified CS rule, the claimant receives $P_1(1-\beta)(M-L)$ if they hire an attorney and $P_0(M-L)$ if they do not. So the condition to hire an attorney is still $(1-\beta)P_1 > P_0$. While the modification reduces the payment for claimants, it does not reduce their demand for an attorney since their decision rule remains the same.[18] The attorney, however, is less willing to take the claim. Their decision rule shifts to $\beta P_1(M-L) > C$, which means they are less likely to take the case for any given expected recovery and cost.

In summary, when contingent fees are based on M, the modification of the CS rule would lower the demand but has no effect on supply. When lawyer's contingent payments are based on $M - L$, the modification has no demand effect for attorneys but reduce their supply. In both cases claimants are less likely to hire an attorney when the collateral source rule is modified.

[18]This is because they do not bear any cost of litigation under a contingent fee arrangement. If they did, they would be less likely to pursue litigation.

4 Identification and Estimation with an Invalid IV

Let the subscript i denote the i-th claimant (or case) in our sample. We observe n iid samples of triples (Y_i, D_i, Z_i). The outcome variable Y_i is the payment he/she receives. The treatment variable D_i is his/her decision to hire an attorney: $D_1 = 1$ if the claimant hired an attorney and $D_i = 0$ otherwise. The instrumental variable $Z_i = 1$ indicates that a claimant's accident occurred in a state and a year in which a modified CS rule requiring offsets was in effect and and $Z_i = 0$ indicates that the claimant can recover from both sources.

For reasons discussed in Section 2, we restrict our attention to claims that occurred in states that modified the CS rule before the end of our sample period. Any claims occurred in states that did not modify the CS rule by 2002 are excluded. What differentiates the two groups in terms of the instrument is whether the state applied the modification to all insurance claims or only medical malpractice claims: $Z_i = 1$ indicates the claim i was in a state that applied the modification to all insurance claims and $Z_i = 0$ indicates the claim was in a state that applied the CS modification only to medical malpractice claims (i.e. the law was modified but did not affect our sample of auto accident claims or a state that has not yet modified its CS rule for all claims.). The instrument is not modification itself, but its scope of coverage.

Since the collateral source rule can affect the decision to hire an attorney, we combine D_i and Z_i and define potential treatment variables $D_i(Z_i)$; $D_i(1)$ stands for the subject's decision to hire an attorney when $Z_i = 1$ and $D_i(0)$ is the decision when $Z_i = 0$. Thus, if $D_i(1) = 0$ and $D_i(0) = 1$, the claimant i would hire an attorney only when $Z_i = 0$.

The value of the outcome depends on the treatment and the instrument, so we define potential outcome variables $Y_i(Z_i, D_i)$. Depending on Z_i and D_i, we have four potential outcome variables; $Y_i(1,1)$ is payments the i-th claimant would receive if he decided to hire an attorney while his case was under a modified state, and $Y_i(1,0)$ is the payment if he did not hire an attorney in a modified state. $Y(0,1)$ and $Y(0,0)$ are defined similarly if his case was in one of non-modified states. The observed variables and potential variables are related by $D_i = Z_i D_i(1) + (1 - Z_i)D_i(0)$ and $Y_i = D_i Y_i(Z_i, 1) + (1 - D_i)Y_i(Z_i, 0)$.

One crucial assumption that makes the conventional instrumental variable method work is the exclusion restriction. Following Angrist, Imbens and Rubin (1996) we write it as

$$Y_i(1, D_i) = Y_i(0, D_i) \quad \text{for } D_i \in \{0, 1\}. \tag{1}$$

This equation states that, given D_i, the value of Z_i is irrelevant to outcomes. The only possible effect of Z_i on Y_i is through its effect on D_i. Except for this indirect channel, the exclusion restriction says that there is no other channel through which the instrument can affect the outcome. If this was the case, potential outcomes could be simplified to $Y_i(D_i)$.

4.1 Average Indirect Effect

In our setup, however, the IV (modification of the CS rule) has a clear direct effect on the outcome (payments). In modified-states, the law change reduces the award at trial by the amount the claimant has received from first party insurance sources prior to the judgment. This impact is also expected to reduce the amount at stake in settlement negotiations since these are constructed in expectation of the award at trial. The instrument has both direct and indirect effects, and the direct effect causes a problem since it violates the exclusion restriction; hence the term invalid instrument.[19]

We solve this invalid instrumental variable problem by combing two facts that are inherent in modifications to the CS rule. First, we introduce a potential outcome variable that blocks the direct effect of the instrument on outcome. Second, this potential outcome variable can be linked to observed quantities and therefore its effect can be learned from data. The potential variable has by design an upper and lower bounds and these bounds can be constructed from observed quantities. Consider a potential outcome $Y_i(0, D_i(1))$. It means payments the i-th claimant would receive if the value of the instrument was allowed to affect his decision to hire an attorney (through $D_i(1)$) but not allowed to have a direct effect since the value of the first argument is fixed at 0. We suppress the direct effect of Z on Y since we fix the first argument.

[19] It may appear to be oxymoronic to call an instrument that violates a crucial requirement of IV but we follow the literature's terminology and call it an invalid instrument.

Consider two scenarios. If one could assume an exclusion restriction, the individual effect of Z_i on Y_i can be written as

$$\text{TE}_i = Y_i(1, D_i(1)) - Y_i(0, D_i(0))$$

We call this the total effect of Z for the individual i. If the exclusion restriction is violated, the expression $Y_i(1, D_i(1))$ is problematic because it includes both direct and indirect effects of Z_i on Y_i. To remove this direct effect, use $Y_i(0, D_i(1))$ and define an indirect effect of Z on Y as[20]

$$\text{IE}_i = Y_i(0, D_i(1)) - Y_i(0, D_i(0)).$$

An average effect defined by TE_i does not have a casual interpretation because of the presence of direct effect. But, an average effect by IE_i has a causal interpretation, since the direct effect is now blocked. To formalize this idea, we make the following assumptions.

Assumption 1 (Existence of instruments) *Let Z_i be a binary random variable such that*

(i) $P(z) = E[D_i|Z_i = z]$ *is a non-trivial function of* z,

(ii) $\{Y_i(1,1), Y_i(1,0), Y_i(0,1), Y_i(0,0), D_i(1), D_i(0)\}$ *are independent of* Z_i.

The first part implies that the instrument has a non-zero effect on the treatment D, i.e. $P(1) - P(0) = E[D(1) - D(0)] \neq 0$. The second part requires the existence of a randomly assigned instrument but it also has a larger meaning. A random assignment of Z does not guarantee (ii); if Z is randomly assigned, it implies that $(D_i(1), D_i(0))$ are independent of Z, but it does not imply that the potential outcomes are independent of Z. In fact, the assumption (ii) means a particular type of exclusion-like restriction in addition to the independence of instrument.

To illustrate, consider a linear latent index model

[20] To understand the indirect effect consider the following thought experiment. Imagine that we allow the treatment variable to be changed from the value that would occur if the claimant's case was in a modified state, $D_i(1)$, to the value that would occur if his case was in a non-modified state, $D_i(0)$. But we hold the direct effect of the modification at 0. What would be the change in outcomes? IE_i captures this counterfactual change.

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 Z_i + \varepsilon_i, \tag{2}$$

$$D_i = I(D_i^* \geq 0), \tag{3}$$

$$D_i^* = \alpha_0 + \alpha_1 Z_i + v_i, \tag{4}$$

where $I(A)$ is an indicator function taking 1 if an event A is true and 0 if A is false.[21]

In this simple model, D_i^* is a continuous latent variable and can be interpreted as the net utility from hiring an attorney, and D_i is the observed decision variable. The causal effect of hiring an attorney is captured by β_1. Assumption 1(i) means that $Cov(Z_i, D_i) \neq 0$, or, $\alpha_1 \neq 0$. If $\beta_2 = 0$, one has a conventional instrumental variables model: the instrument Z has no direct effect on Y but can have an indirect effect through D. If $\beta_2 \neq 0$, Z has the direct effect as well, a violation of the exclusion restriction (1). We have an invalid instrument problem. The potential variable $Y_i(0, D_i(1))$ turns off this direct effect by requiring that $Z = 0$ in the outcome equation (2), while allowing $Z = 1$ in the selection equation (4). If the potential variable were observed, the direct effect would not be an issue and Z would be a valid IV.[22]

Given this, what Assumption 1(ii) requires is that $E[Z_i \varepsilon_i] = 0$ and $E[Z_i v_i] = 0$. The zero correlation conditions summarize the idea that the instrument Z can affect Y only through two channels; the direct effect and the indirect effect through D_i. Since ε_i and v_i can be correlated, the zero correlation between Z_i and v_i guarantees that there is no other indirect channel through the selection equation and the zero correlation between Z_i and ε_i guarantees that there is no other direct channel besides $\beta_2 Z_i$. In sum, Assumption 2(ii) says that we only allow two channels for Z to impact Y, a direct effect as captured by a possibly non-zero β_2 and an indirect effect through D, but nothing else. As $Y_i(0, D_i(1))$ takes care of the first channel of direct effect, what is left out is the indirect effect only.

This assumption can be violated if there is a second direct or indirect channel through which our

[21] The results of this section do not depend on the linear functional form of the model and/or the effect being constant. We use this simplified model only for expositional convenience.

[22] This potential outcome is not directly observed, of course, so its role here is purely conceptual. To make it operational, we have to link the potential outcome to observed quantities.

instrumental variable may influence outcomes. One potential channel is that the modification of the CS rule may change market environments for attorneys and change their approach to litigating the case. For example, a modified CS rule may make attorneys exert more or less effort in each case regardless of the terms of the contract. Another possibility is that given new market conditions, attorneys may change the fee structures in contracts with their client essentially increasing the fees rather than simply refusing more cases. This will affect net benefits of their customers.

For the lawyer effort channel, we are essentially assuming that our instrument affects the quantity (likelihood) of lawyer's service but not its quality. In section 7, we examine several variables that measure lawyer's efforts and find that they were not systematically affected by the CS law changes. We also utilize state-wise variations in rules that determine the lawyer's contingent fees. In light of these robustness checks, the main results are not likely to be driven by changing lawyer's effort levels. For the fee structure channel, we find that there is no evidence that the CS rule changes affected the amount lawyers changed on their clients. We also obtain consistent findings even when we use 'before fees' payments as the outcome variables. This implies that out findings are not likely to be derived from the changing fee structure.

The next assumption is the individual level monotonicity.

Assumption 2 (Monotonicity)

$$D_i(1) \leq D_i(0) \quad \textit{for all } i.$$

This is not a testable condition. In our case, however, the simple supply-demand analysis in Section 3 predicts that the condition is likely to hold. Under the monotonicity, there are only three distinct groups: 'never-takers' is a group of subjects who never hire lawyers ($D_i(0) = D_i(1) = 0$), 'compliers' is those whose decision is affected by the law change in the sense that he would hire only when he was under a non-modified state ($D_i(1) = 0, D_i(0) = 1$), and 'always-takers' is who always hire lawyers ($D_i(0) = D_i(1) = 1$). What is not allowed under the monotonicity assumption is the existence of the so-called 'defiers', a set of subjects who would hire attorneys only when he was in a modified state ($D_i(1) = 1, D_i(0) = 0$).

The effect we can identify under the given assumptions and data is the effect on compliers. The proposition below defines the effect we aim to estimate.[23]

Proposition 4.1 *Let Assumptions 1 and 2 hold. The local average indirect effect can be represented as*

$$E\left[Y(0,1) - Y(0,0)|D(0) - D(1) = 1\right] = \frac{E[Y(0,D(0))|Z=0] - E[Y(0,D(1))|Z=1]}{E[D|Z=0] - E[D|Z=1]}. \quad (5)$$

Proof can be found in Appendix A.[24] To understand the expression (5), compare it to the usual LATE expression under the exclusion restriction (1) (Imbens and Angrist (1994)):

$$\begin{aligned} E[Y(1) - Y(0)|D(0) - D(1) = 1] &= \frac{E[Y|Z=0] - E[Y|Z=1]}{E[D|Z=0] - E[D|Z=1]} \\ &= \frac{E[Y(0,D(0))|Z=0] - E[Y(1,D(1))|Z=1]}{E[D|Z=0] - E[D|Z=1]}. \end{aligned} \quad (6)$$

The difference is that (5) uses the indirect effect (IE) but (6) uses the total effect (TE).[25] The average indirect effect in (5) is what we can estimate when we cannot assume the typical exclusion restriction.

One outstanding issue is that $Y_i(0, D_i(1))$ is not observed in (5). We need to link this potential outcome to an observable quality. For this purpose, let W_i be the maximum that can be deducted from claimant's settlement or trial awards under the modified CS rule. Note that the exact nature of W_i depends on the year and state in which the accident occurred, because state laws specify what types of insurance payments claimants have received (or will receive) should be deducted from settlements or awards. In our sample the 23 states that have modified the collateral

[23] Flores and Flores-Lagunes (2013) obtained a closely related result. However, we use it quite differently in our applications and since the result is the key to derive our bound, we restate the result here.

[24] The notations in (5) may look unconventional in that it uses a reversed order in $D(0) - D(1)$. This is because compliers here take the treatment when $Z_i = 0$, so $D(0)$ means the treatment while $D(1)$ implies the absence of the treatment.

[25] The independence assumption implies that the numerator in the left hand side of (5) is $E[Y(0, D(0)) - Y(0, D(1))] = -E[\text{IE}]$. The same reasoning holds for (6).

source rule to require the trial court to deduct payments from the claimant's judgment all require offsets for first party health and auto insurance and the majority for workers compensation and government provided health insurance.[26] For more information on state specific rules, see Table 1. The maximum W_i can take is when the possible judgment includes all sorts of payments from all first party insurance sources available to the victim. We call this value the claimant's 'own' insurance for simplicity, but it may includes payments from disability insurance, health insurance including Medicare, Medicaid, and worker's compensation plans. W_i includes all sources of insurance payments that are not from injurer's insurance.[27] Again depending on what a state's CS rule modification specifies, the actual offsets can differ from this maximum amount.

The amount W_i depends on Z_i, so we may write it as $W_i(Z_i)$. Note that $W_i(0) = 0$ simply because of how the offsets inherent in the collateral source rule work. What is non-trivial is $W_i(1)$. Hence we keep the simpler notation W_i but intend to use it for $W_i(Z_i)$. Can W_i be dependent on D_i? We assume that it is not because payments from 'own' insurance are mostly mechanically determined by the reimbursements of medical bills or other damages. These payments depend on characteristics of accidents and not on the decision to retain a lawyer or the modification of the CS rule.

For the relationship between the unobserved quantity, $Y_i(0, D_i(1))$, and observed quantities, $Y_i(1, D_i(1))$ and W_i, we have the following relationship.

Assumption 3 (Bounds on potential outcomes)

$$Y_i(1, D_i(1)) \leq Y_i(0, D_i(1)) \leq Y_i(1, D_i(1)) + W_i \quad \textit{for all } \ i.$$

In many ways this is not an assumption. It simply describes the mechanical way that the modifications to the collateral source rule requiring offsets work. The first inequality, $Y_i(1, D_i(1)) \leq Y_i(0, D_i(1))$ follows from how offsets work under the modified collateral source rule. The plaintiff may be paid twice for the same damage under the CS rule (captured by $Y(0, d)$) but may not under

[26] Our sample predates strict enforcement of the Medicare Secondary Payer Act, which requires repayment of all expenses paid by Medicare. See Helland and Kipperman (2011)

[27] In contrast, recall that the observed outcome Y_i includes the payment from injurer's insurance.

the modification of the CS rule (as captured by $Y(1,d)$). So the former must be always greater than or equal to the latter. The second inequality $Y_i(0, D_i(1)) \leq Y_i(1, D_i(1)) + W_i$ simply says that the maximum that can be deducted from awards or settlements is W_i, the sum of all payments from other collateral sources.

When the assumption holds, we have an interval identification of the average indirect effect. We state it as a proposition.

Proposition 4.2 *Let Assumptions 1, 2, 3 hold. Then, a sharp bound for the average indirect effect in (5) is given by* $[AIE^L, AIE^U]$ *where*

$$AIE^U = \frac{E[Y_i|Z_i = 0] - E[Y_i|Z_i = 1]}{E[D_i|Z_i = 0] - E[D_i|Z_i = 1]},$$
$$AIE^L = \frac{E[Y_i|Z_i = 0] - E[Y_i + W_i|Z_i = 1]}{E[D_i|Z_i = 0] - E[D_i|Z_i = 1]}.$$

This bound can be uniquely determined from observed quantities. Proof is trivial and hence we omit it to conserve on space.

4.2 Quantile Treatment Effect

Thus far, we have focused exclusively on the average effect. However, as is common in this type of analysis, the data has a non-trivial right tail caused by several large payments. Most claims in our dataset are relatively minor and therefore lead to small (or even no) insurance payments and settlements/awards. There are, however, occasional but infrequent large outliers in the sample. These outliers have a significant influence on the average treatment effect. Common approaches to outliers, such as winsorized means or trimmed means, are not desirable considering that much of the policy debate about lawyer's value in the recovery process arises in the context of cases with serious stakes and potentially large payments. These infrequent large payments may have valuable information so we are reluctant to drop them. For this reason, we extend our analysis and consider quantile effects.

To estimate quantile indirect effects, we propose a method using a distribution regression. The distribution regression proves to be useful in Firpo, Fortin, and Lemieux (2009), although they do not allow endogenous variables. For methods that deal with endogeneity problem in quantile regression, Abadie, Angrist, and Imbens (2002) is applicable when both treatment indicator and instrument are binary variables. The potential outcome distribution approach in Abadie (2003), who assumes that there are no control variables. Our approach allows multiple instrumental variables, control variables, and a continuous endogenous variable and continuous instrument.

Let $\mathcal{Y}$ be the support of Y_i and let y be a constant in $\mathcal{Y}$. Define $T_i = I(Y_i(0, D_i(1)) \leq y)$. The conditional mean of T_i given D_i is

$$\mathrm{E}\left[T_i | D_i = d\right] = \mathrm{E}\left[I(Y_i(0, D_i(1)) \leq y) | D_i = d\right] = F(y|d).$$

The new variable T_i is not observed since it is based on a potential outcome. Suppose, for a moment, T_i is directly observed. If this is the case, the above conditional mean model could be estimated by a 2SLS using T_i as the dependent variable, D_i as endogenous covariate, and Z_i as instrumental variable. We take a linear and additive model for convenience:

$$\mathrm{E}\left[T_i | D_i\right] = \beta_0(y) + \alpha(y) D_i. \tag{7}$$

Let $F_{(1)}(y)$ and $F_{(0)}(y)$ be the conditional distribution functions of Y_i given $D_i = 1$ and $D_i = 0$, respectively. From (7), $F_{(1)}(y) = \beta_0(y) + \alpha(y)$ and $F_{(0)}(y) = \beta_0(y)$. This method can be viewed a distribution function equivalent of the 2SLS estimator.

Let $F_Y(\cdot)$ be the marginal distribution function of Y. For a fixed value of y, define τ such that $F_Y(y) = \tau$. Define the τ-th conditional quantiles such that $Q_{(1)}(\tau) = \inf\left\{y | F_{(1)}(y) \geq \tau\right\}$ and $Q_{(0)}(\tau) = \inf\left\{y | F_{(0)}(y) \geq \tau\right\}$. The τ-th quantile indirect effect is then given by

$$\Pi(\tau) = Q_{(1)}(\tau) - Q_{(0)}(\tau).$$

Since T_i is unobserved, this quantile effect is not point identified. However, as before, upper

and lower bounds for $\Pi(\tau)$ can be constructed. To be specific, the upper bound for $\Pi(\tau)$ is $\Pi(\tau)$ itself when we use Y_i in place of $Y_i(0, D_i(1))$ in the definition of T_i. Likewise, the lower bound can be found by defining $T_i^L = I(Y_i + W_i \leq y)$. Apply an analogous 2SLS using T_i^L as the dependent variable. This procedure will estimate $\mathrm{E}\left[T_i^L | D_i = d\right] = \mathrm{E}\left[I(Y_i + W_i \leq y) | D_i = d\right] = F^L(y|d)$. We can obtain $F_{(1)}^L(y)$ and $F_{(0)}^L(y)$, and from them, quantile functions $Q_{(1)}^L(\tau)$ and $Q_{(0)}^L(\tau)$. The lower bound for the quantile effect is obtained by

$$\Pi^L(\tau) = Q_{(1)}(\tau) - Q_{(0)}^L(\tau).$$

The bounds of the quantile effects, depends only on observable quantities, and therefore, can be consistently estimated.

5 Data and Variable Construction

5.1 Survey of Auto Injury Claims

The data for this study comes from the Insurance Research Council (IRC)'s Consumer Panel Study of Auto Injury Claims. The IRC consumer panel study contains series of nationally representative and audited surveys sponsored by the IRC in which individuals injured in auto accidents are asked detailed questions about medical losses and sources of compensation, if any, for those losses. We utilize data from surveys conducted in 1987, 1992, 1997, and 2002 (See IRC, 2004 and Crocker and Tennyson, 2002). The data covers accidents in the years 1982 to 2002, and all 50 states and the District of Columbia. Tables 2 and 3 show the distribution of accidents by year and by state, respectively.

Because the data surveys accident victims, it has several advantages for our research over more traditional closed claim data. Most importantly for our purposes, the Consumer Panel contains data on payments from both first party insurance and third party sources. Under a first party insurance contract, the injured party is paid by his or her insurer in the event of an injury regardless of whether the injury was caused by a third party or whether that third party was at fault. Health

and auto insurance are the most common first party insurance in the data with Medicare and Medicaid being the third and fourth most common insurance. Under third party (or liability) insurance, the insured is protected against claims by a third party who alleges a negligent action. Thus, in our context third party payments are payments either from a liable or potentially liable driver of another vehicle involved in the accident.

In our data, a "claimant" who was involved in an accident and does not retain an attorney might receive compensation from their own insurance (private health or auto insurance, Medicare, Medicaid, worker's compensation, or other sources), a third party insurer (even if they did not hire an attorney or file a legal claim) or no compensation for the accident. This is also true of a claimant who retained an attorney.[28]

Table 4 shows summary statistics of the samples considered in the paper. The IRC consumer survey has 18,451 cases. The IRC Consumer Panel asked about the use of a lawyer in a third party claim so we are confident that for the vast majority of cases if a lawyer is hired, then the lawyer in question is attempting to recover from third party sources. As discussed below, we make several sample restrictions to attempt to remove cases in which a lawyer might not be attempting to recover from third party sources and, hence, our measure might be biased toward zero in those cases.[29] For this reason, we exclude 1,021 cases in which the survey respondent claims a lawyer was involved in the case but in which there is no record of a contingent fee or other agreed upon payment method and the lawyer has not negotiated with any insurer or preformed any other service identified in the survey. Our assumption is that in these cases the respondent is mistaken either that the lawyer took the case or the lawyer was hired for some other purpose such as defending the claimant against traffic violations associated with the accident but not in securing a recovery. This sample restriction leaves us 17,520 cases. The first three columns in Table 4 display summary

[28]The terminology for the accident victim becomes somewhat problematic in that we would typically refer to an accident victim who sues another party as the plaintiff. However, since we are comparing the accident victim in the state in which they retain a lawyer to assist them in receiving a payment from a third party to states in which they do not, we choose to refer to the accident victims as claimants in both situations and askew the use of the word plaintiff.

[29]The concern is that if a lawyer is hired to defend the respondent against traffic violations resulting from the accident and not attempt to recover from a third party we would only observe a zero third party recovery and no contingent fees. This might make lawyers look less valuable because we are incorrectly measuring the service provided by the lawyer.

statistics of this sample under the title 'Whole sample'.

Fifteen states did not change their CS laws until after the end of our sample period. Out of the remaining thirty-six states that changed their CS laws, nineteen states chose to apply it to all cases, while seventeen states applied it only to medical malpractice cases. As Section 3 explains, we keep cases from the modified states (i.e., the last two types of states) and drop cases from the non-modified states. This is the 'CS Rule Modified' sample in Table 4 and it includes 13,260 cases.

The majority of accidents are collisions with another moving vehicle, constituting about 82% cases of the sample. Other types of accidents include single vehicle crashes and accidents involving a pedestrian or cyclist, each of which comprises 16% and 3% of the sample, respectively. While we include all types of accidents in the subsequent analysis, a vehicle collision provides the cleanest test of a lawyer's value because which party is at fault is not inherently obvious. Single vehicle accidents rarely involve liability for a third party and hence lawyers are unlikely to be particularly valuable in these cases. By contrast, pedestrian accidents in which the claimant is the pedestrian are very likely to involve fault by the other driver. One issue is that our survey does not identify whether the survey respondent was the pedestrian or the driver of the car in the accident. To correct for this omission in the survey we exclude the small number of pedestrian accidents in which the survey respondent did not suffer a physical injury under the assumption that the respondent might have been the driver of the car rather than the pedestrian. In the remaining cases, the survey respondent was likely to be the pedestrian hit by car and hence very unlikely to be at fault. The 'Collisions Only' sample in the last set of columns in Table 4 has 10,729 collision cases. This is our main sample for analysis.

The treatment variable is the decision to hire a lawyer. Overall, about 32% of claimants hired lawyers. By accidents type, the hiring probabilities are 35%, 9%, 50% for collisions, single vehicle accidents, and pedestrian accidents. The data contains the age and gender of the claimant. We include these variables to proxy both for potential differences in bargaining strategy across age and gender but also as a control for the potential scope of damage.[30] We also include data on the number of injured parties in the accident in order to capture the scope of the accident. The data

[30]Retired individuals, for example, cannot claim lost wages making their claims somewhat less lucrative to attorneys regardless of whether the collateral source role has been modified.

also contain information on the size of the claimant's alleged injuries such as the number of days of work lost and the amount of the claimants (alleged) medical bills.

We also include whether the state has no fault insurance laws and/or mandatory insurance policy. A number of states, starting in the 1970s, passed no fault insurance laws (see Anderson et al. 2010). There is a vast literature on the impact of no fault insurance on recovery and we include it here largely to control for the possibility that no fault laws limit potential recovery. The compulsory insurance variable is included because during our sample period a number of states adopted compulsory insurance laws that require drivers to have some minimum level of auto insurance with liability coverage. Given that the limited personal assets of the typical uninsured driver almost certainly make it unprofitable to sue them, we would expect this the presence of these laws to increase the size of payments. In addition, we include an indicator variable equal to one if the victim did not have any personal insurance (health, auto and was not eligible for workers compensation.)

Finally, we include several indicator variables to capture changes in state tort law. The most important of these is whether the state has capped noneconomic damages at the time of the accident. Given that these caps limit recovery for noneconomic damages to some specified amount (e.g. $250,000), we would expect the presence of these laws to reduce plaintiff recovery. The noneconomic damages indicator is derived from Ronen Avraham's Database of State Tort Law Reforms (DSTLR 5.1), however, we examined state statutes to determine if the cap applied to auto injury cases as well as medical malpractice.[31] We also include whether the state caps the contingent fees that plaintiff's attorneys can receive in a case, whether the state requires prejudgment interest to be paid when the case is resolved, whether the state caps total damages that can be received in a case, whether the state has limited the scope of joint and several liability[32], and, finally, an indicator if the state requires structured settlements rather than lump sum payments for particularly large settlements.

[31] During our sample period 31 states have caps on non-economic damages for medical malpractice cases while only 13 states have non-economic damage caps that apply to auto accidents.

[32] Joint and several liability is a common law doctrine under which a defendant can be held liable for the full amount of the plaintiff's damages, regardless of their personal degree of fault, if the other defendant's in the case are bankrupt.

We construct five measures of the claimant's total payment received as a result of their injury. The broadest measure is 'Total payment' and it captures compensation from all sources inclusive of both first party health, government provided insurance, private auto insurance and workers compensation and third party payments from the defendant's auto insurance or personal assets. Given the nature of the survey, these are final payments and thus in states that have modified their collateral source rule to require offsets for payments from first party insurers, the payments at trial would reflect the required reductions. Our assumption is that payment in settlements are negotiated based on the expected outcome at trial and thus would represent a bargain struck in the shadow of the offset. One reason for estimating attorney's impact on total payments is that the proceeds of litigation, either once they are secured through litigation, or the threat of litigation, must be used to pay for treatment of injuries resulting from the accident. In this way payments from litigation preempt payments, at least in part, from first party insurance after the litigation is complete. This means that lawyers, at least indirectly, impact payments from first party insurance over the course of the patient's long term treatment in claims that involve litigation.

Our second measure is 'Direct payment,' which represents only payments from third parties. It is common to receive compensation for injuries from a third party insurer even when the claimant has not hired a lawyer so this measure is a more direct measure of attorneys value to their clients relative to what the client would have been able to secure from third parties absent the presence of the attorney. The direct payment measure consists of payments from litigation, either judgments or settlements, and payments from the injurer's insurance that did not result from litigation.

We also consider three other measures of compensation received by the victim. The first is 'Litigation payment,' which captures both trial awards and, more commonly, settlements.[33] The second is 'Own Insurance payment', which is payment from first party insurers. These payments are primarily auto and health insurance and often include government-funded insurance such as VA benefits, Medicare and Medicaid. Finally, we include 'Total Insurance payment', which is payments from first and third party insurers, that is, the individuals insurance plus payments from other drivers involved in the accident's insurance. Payments from litigation is not included in this

[33]It is worth noting that you do not have to hire a lawyer to file a lawsuit. A substantial portion of plaintiffs are pro se litigants representing themselves in court.

measure. We include this measure to see if lawyers are beneficial in those cases in which the victim finds himself or herself negotiating with a third party insurer or when the victims insurance company itself negotiates with another party's insurer. We find that the average payments are much larger than the median payments. In addition, the standard deviations are quite larger suggesting that while most accidents in the sample are minor and do not lead to large payments between injured parties; there is a right tail to the distribution that consists of expensive and serious accidents involving much larger payments.

As mentioned above, the data also contains information on whether or not the victim hired a lawyer. In addition, the survey recorded whether the victim talked to a lawyer but did not hire them. Our treatment variable, given our interest in the value of a lawyer, is whether the lawyer is ultimately hired to represent the victim. Some justification for limiting the treatment variable to cases in which the lawyer was ultimately hired can be found in Table 5, which breaks the sample down by whether the victim hired a lawyer, talked to a lawyer or did not talk to a lawyer, and hence did not hire one. Consistent with our expectations cases in which a lawyer is ultimately hired involve more extensive accidents with a larger number of victims, more days of work lost and greater alleged medical expenses.[34] This is not surprising since attorney are paid on a contingent fee in these cases and are hence more likely to take larger cases. Since both victims and lawyers are selective, however, simply regressing the presence of an attorney on compensation will be biased. Table 6 breaks the collision sample down by whether or not the state modified the CS rule before 2002 and the scope of those modifications.

Table 7 shows accident characteristics broken by quantiles of total recovery. Accidents that associated with bigger recovery also claimed more in medical cost and damages. Those cases also tend to hire lawyers with higher probability.

5.2 Construction of the Instrument

In Table 1, we provide a state-by-state breakdown of our instrument. The status of the collateral source rule, the year it was modified, and the statute modifying the rule are provided. The data on

[34]This maybe because these cases are inherently more complex. Shavell (2004) finds that the more complex the case, the more likely an individual is to pursue litigation.

the modifications to the CS rule also come from Ronen Avraham's DSTLR 5.1, but we examine the specific state statute to determine if it applies only to medical malpractice cases or also includes auto cases. The entry 'None' in the 'Statute' column means that the state has never modified the CS rule. In the 'Types of Claims' column, 'Medical only' means that the modifications only applies to Medical Malpractice cases and 'All' means that it applies to any cases. In some cases, the modification of the CS rule has been overturned by the state supreme court (e.g. Georgia and Illinois). In those cases, the 'Effective Date' have both starting and ending dates.[35] The table also lists what sources are exempted in the modifications.

In Table 6 we provide the breakdown of the sample by whether the state has modified its collateral source rule. Table 6 uses 'Whole Sample'. Results with 'Collisions Only' sample are not substantively different. The table provides summary statistics of three subgroups. Column (1) includes claims in state that modified the CS rule and applied it to all types of claims, after the modification. Column (2) includes claims in states that modified the rule but restricted its application to medical malpractice claims. Column (3) includes claims in states that did not modify the CS rule. Our identification strategy regards the claims in column (2), plus claims in the states in (1) but occurred outside years in which the CS modification was in effect, as the group with $Z = 0$. Claims in column (1) during the periods in which the CS modification was in effect are regarded as the group with $Z = 1$. Claims in column (3) are discarded.

The averages of the claimant demographics are similar across both modified and unmodified states. Claims in modified states appear to be slightly more severe when judged from medical costs or damages. This is interesting because modified states have slightly lower number of work days lost. The probability of hiring lawyers is lower in modified states, which is consistent with the theoretical prediction in Section 3. The next subsection provides further evidence supporting the prediction. The direct payment, payments from the third party insurance and trial awards, is slightly lower in modified states, while the total payment, including victim's own auto and health insurance, is slightly higher. One possible explanation of the overall pattern is that victims eventually get more from their own insurance when they not to hire a lawyer. Put differently if a victim is dissuaded

[35]Note that if the law was enjoined, we use only the years for which it was in effect.

from hiring a lawyer by the modified CS rule, then he may claim more of his injuries on his own insurance.

One issue with simply using law changes as our instrument is that changes in the CS rule are statewide and binary but the actual offsets caused by these law changes vary depending on both the state law change and the sources of compensation received by victim outside of civil justice system. Our instrument takes advantage of fact that states differed in what sources of compensation required an offset. For example, when the Illinois legislature modified the states' CS rule they choose to exempt payments from the workers compensation system. Thus, an accident victim in Illinois who had received payments from this system would not have to deduct them from any payment in the civil justice system. By contrast, when Florida modified its CS rule it required payments from the workers compensation to be deducted. Similar differences exist across states over payments from federal insurance programs, such as Medicare and the VA, joint state and federal programs, such as Medicaid, and state insurance programs. In all states, the modified the CS rule direct payments from first party private insurance are deducted but only a subset of states exempted workers compensation programs or government insurance. Finally, some states limit the amount of the offset to 50% of the value of insurance payments.

To capture this variation our instrument utilizes the fact that our data identifies payments from different insurance sources that are paid to cover the cost of the accident. Thus payments from government insurance, workers compensation and private first party insurance are itemized separately from payments from private third party insurance or the civil justice system (i.e. settlements or trial awards). The data also contains the victims estimate of the total damage from the accident in terms of property loss, medical expenses and lost wages. From this information we can construct a (continuous) measure of the value of the case that has already been compensated:

$$\text{VC} = 1 - \frac{\text{(insurance payments - non-offset payments)}}{\text{total damages}},$$

where "insurance payments" is the total payments from collateral sources and "damages" is the total amount that could be compensated in the civil justice system without any modifications to

the CS rule. In addition, "non-offset payments" reflects the state specific modifications to the CS rule, so the numerator is the total payment that would be deducted. So if VC is equal to zero, all damages were already compensated by various insurance sources (except the third-party insurance) and all those compensations can be deducted from the third-party insurance payment even if plaintiffs win. So VC is designed to capture the degree to which the CS modification would actually reduce any payments in the civil justice system.[36]

6 Results

6.1 The Naive Regressions

In the theoretical literature, the value of an attorney is ambiguous. There is no a priori reason to believe that lawyers will not systematically charge more than the surplus they generate net of fees or that their presence will not systematically result in a lower payment than the client would have received without them due to expenses. Yet there is also no reason to suppose the reverse. The typical approach to this problem is to run the following regression,

$$Y_i = \beta_0 + D_i\beta_1 + X_i'\beta_2 + \epsilon_i \tag{8}$$

where Y_i is one of our outcome measures of the claimants recovery, D_i is a binary treatment variable, equal to one if the claimant hires an attorney, and zero, otherwise. In addition, X_i is a vector of control variables, and ϵ_i is the error terms.

The results of this regression are shown in Table 8. We report results of the three samples in Table 4: 'collisions-only', 'collisions and pedestrian accidents', and 'collisions and pedestrian and single accidents'. Each cell in the table represents the results of a different version of the above regression. Payment measures, outcomes of the regressions, are net of attorney's fees. We use the control variables control variables discussed above and we include state and year fixed effects in a subset of specifications. Below we discuss results for collisions-only sample. Other samples lead to

[36] We also include VC directly in the first stage regression since clients maybe less likely to seek out a lawyer at all if most or all of their damages are covered from other insurance sources.

similar results.

The impact of hiring an attorney on the total payment (after fees) is $15,846 or $11,042 depending on whether the state and year fixed effects are included in the regression model. The estimates of a lawyer's value is higher, $16,391 or $11,018, when we use our direct payments (after fees) measure which captures payments only from third parties. Other payment measures also yield similar results. The estimated effects are always significantly different from zero. For most of our measures of recovery, lawyers appear to generate considerable value.[37]

Only for recovery from the individuals own insurance and total insurance payments is the impact of hiring an attorney negative (and only significant in the fixed effect model of total insurance payments). The result, combined with the overall increase in payments found in the other regressions, suggests that the presence of attorneys may in fact cause victims to utilize their own insurance less in favor of recovering from the third party sources the retention of a lawyer makes available. The regressions suggest, however, that this is a good deal for accident victims. Even after forgoing perhaps five thousand dollars from their own insurance, if we believe the regressions, their lawyers are worth about $11,000 even after we deduct fees.

Overall the results in Table 8, assuming that the presence of a lawyer in a case is random beyond the factors controlled for in the regression, is very large; a conclusion reached by almost all of the previous literature on the topic. The difficulty, as discussed above, is that the decision to retain an attorney is likely to be endogenous.

6.2 Predictive Power of Instruments

In Table 9 we present the probability of an accident victim hiring an attorney. We use collisions-only sample. The overall results we present in this section are substantively identical if we use other samples. Group 1 stands for the states that modified the collateral source rule for all case types. Before the modification, a claim had a 35.30% chance of being represented by an attorney. The likelihood of being represented by an attorney falls to 31.12% after these states modify the collateral

[37]The estimated value of lawyers tend to be smaller than those obtained from Table 5 by simply comparing the average recoveries of two groups 'hired lawyer' and 'not hired lawyer'. This means that the control variables are important in explaining the variation of payment measures.

source rule. This difference is statistically significant with a t-value of 3.019 (p-value of 0.0025). Group 2 are the states that modified the collateral source rule only for medical malpractice cases, and hence had the same political pressures to modify the collateral source rule but decided not to modify it for all cases. In this group, 37.35% of cases retained an attorney before modification and 36.59% of cases after modification. This change is not significant. Between two groups, the difference after the modification is statistically significant with a t-value of -5.010 (p-value of 0.0001). These two cases (Before vs. After in Group 1 and Group 1 vs. Group 2 after the modification) are the only significant changes in hiring probabilities and the difference is due to the prevalence of the CS rule change. Any other differences are not statistically significant. This is what we should expect if the theoretical prediction is correct.

We also test the differences between the probability of hiring a lawyer in states that modified the collateral source rule before the modification was enacted against the same probability in states that either modified the rule only for medical malpractice cases or for states that did not modify the rule at all. As would be expected if the collateral source rule is causal in change the probability of hiring a lawyer in auto claims, none of these groups show a statistically significant difference because none of three groups were affected by the CS rule change.[38] This is also consistent with the theory.

Table 10 presents the first stage regression using our continuous instrument on the decision to hire a lawyer and the F statistic. The point of this exercise is to show the predictive power of instrumental variables, while including the continuous version of the instrument in regression. The value of the case is highest when none of the damages are covered by collateral sources, i.e. $VC = 1$. Consistent with past studies, as less of the victim's damages from the accident are compensated by collateral sources, the more likely the victim is to hire a lawyer. The first stage results are very similar across specifications without and with fixed effects. In fact, moving from the damages being completely compensated to no compensation from collateral sources increases the likelihood of hiring a lawyer by 22.3% in states that the CS rule change was not in effect. In states that have modified the CS rule to mandate offsets, having no offsets, $VC = 1$, increases the likelihood of an

[38]The t-statistic when comparing each pair of those three groups are -1.189, 0.275, and 1.188, respectively.

attorney by 13.0%. The first stage F statistics are 34.697 and 32.305, respectively. We conclude that the instruments have strong first-stage predictive power.

6.3 Attorney Values from IV Regressions

Taking the results of the naive regression in Table 8 as our starting point, ideally we would now estimate a regression that takes into account the endogeneity of hiring a lawyer and produce point estimates from an instrumental variables regression in which we approximate the random assignment of lawyers by a natural experiment. The issue is that our natural experiment has both an effect on hiring an attorney and on recovery. Our solution, discussed above, is to construct the upper and lower bounds of the plaintiff's possible recovery. We construct the bounds by adding medical expenses that must be deducted under the states modification of the collateral source rule back into the outcome measure. This allows us to construct an upper and lower bound of the value of a lawyer. In Appendix A.4 we discuss the methodology of extending the IV regression approach to allow the inclusion of control variables..

In Tables 11 and Figure 1 we present the results of our alternative approach for each of our five measures net of attorney's fees. We estimate the bounds of the average effects of hiring attorneys using two stage least squares regressions with and without state and year fixed effects. The 95% confidence intervals are in parentheses. Focusing on total payments, our estimate of the bounds on the average indirect effect of an attorney on claim value finds the causal impact is between -$71,985 and -$20,320 after fees without fixed effects and -$64,369 and -$14,585 when state and year fixed effects are included. Although the bounds are consistently negative, the 95% confidence interval does contain zero. For direct payments (those from third party insurance sources or litigation) and litigation payments, the bounds include zero. For direct payments from third party sources the bounds include zero as do the bounds of the litigation measure of payments. Although not significant at the 95 % level, the result is consistent with the hypothesis that lawyers potentially generate a greater recovery from these sources although we cannot distinguish this from lawyers having no effect. This begs the question why are the upper and lower bounds negative for total recovery.

A possible answer can be found in payments from the individuals own insurance sources and total insurance payments. For payments from the individuals own insurance, that is auto or health insurance, the impact is consistently negative and the 95% confidence interval is negative. This is consistent with the impression from the naive regression; hiring attorneys results in a substitution from the individuals own insurance to third party sources. However, once we correct for the endogeneity of hiring an attorney it appears that the victim does not come out ahead. Finally, in the case of total payments from all insurance sources, third party and first party but not those payments received from litigation, the bounds are uniformly negative as is the 95% confidence interval. This suggests that the value of attorneys is limited in negotiating with insurers outside of the context of litigation.

The overall picture here is that the strongly positive estimates from the naive analysis (Table 8) are not credible and the real impact of lawyers seem to be negative or at best ambiguous (Table 11). This tendency is highlighted in Figure 1. There we compare point estimates from the naive regressions and interval estimates from the IV regressions. We show all five outcome measures and specifications with and without state and year fixed effects. It is very clear from the figure that the naive regression tends to overestimate the value of attorneys. Only for direct payments with fixed effects and litigation payments with fixed effects are the estimated bounds inclusive of the naive regression estimates.

In column (1) and (2) of Table 11 we restrict the sample only to collisions involving at least two-cars. The logic is that single vehicle accidents and accidents in which the victim is a pedestrian are less likely to involve ambiguity over who is at fault and their liability. Put simply there are potentially fewer people to sue when you run your car into a tree relative to a two car accident. In column (3) and (4) we add pedestrian accidents into the sample. Given our removal of zero damage pedestrian accident, our sample the pedestrian-victim is overwhelmingly likely be the survey respondent which means that this group is far more likely to not be a fault and hence is more likely to have a valid claim against a third party, namely the driver that hit them.[39] Consistent with this we find that for collisions and pedestrian accidents the upper and lower bounds on the

[39]Ideally we would like to estimate pedestrian accidents and single vehicle accidents independently. However, the sample sizes are simply too small.

causal effects of hiring a lawyer are slightly more positive. For example, for total recovery with fixed effects we find estimate that the impact of a lawyer is bounded by $-64,369 and $-14,585 for the collusion sample. When we add in pedestrian accidents the bound shifts up to $-59,004 and $-10,407. We find similarly positive impacts on direct payments and litigation. For payments from the victims own insurance the bounds on the impact of hiring a lawyer continue to be negative and for the total payment from all insurance sources the bounds include zero.

In column (5) and (6) we add in accidents with only a single vehicle. Since these are far less likely than pedestrian accidents to be the fault of another driver, we would expect the bounds to become more negative. Consistent with our expectations all of the value of a lawyer bounds are smaller by several thousand dollars. This is consistent with our hypothesis that the greatest value of a lawyer comes in cases in which fault maybe contentious (collisions) or the other party is more likely to be at fault (pedestrian accidents) and that the value of a lawyer is lower if the driver is more likely to be at fault (single vehicle). Nonetheless, in each sample the picture is similar even if the levels vary across samples. Hiring a lawyer reduces overall recovery unlike the strongly positive estimates from the naive analysis.

6.4 Quantile Effects of Hiring an Attorney

These numbers (bound estimates from the IV regressions) are potentially driven by some large outliers in the claim's payments. Although the average payment is $15,673 the median payment is $1,122 suggesting a large right tail. It is possible, as Hammitt (1985) finds using the 1986 sample of the data, that for very large claims the damages are so large and injuries so obvious that hiring an attorney reduces the total payments received by the claimant. The reduction could be driven by the lawyer's fees or because the adversarial system's expenses actually consume more of the payment than the surplus generated by the attorney.

If expenses, which are not observable in our data, drive the negative value of attorneys in auto claims, then larger cases should drive the negative impact of attorneys since expenses in smaller claims are quite low and typically only cover court fees. In Table 12 we examine the impact broken down by quantile. At the median ($\tau = .5$) the bounds on the impact of an attorney on total

payments from all sources are positive and reflect the very small recoveries involved. This continues to be true until we reach $\tau = .95$ when the bound becomes inclusive of zero. After $\tau = .97$ the bounds on lawyer's impact on total payment becomes strongly negative. The pattern is similar with direct payments although we are unable to estimate the lawyer's impact for direct payments for $\tau = .5$ or lower simply because small claims almost never have payments from third party sources.

We are similarly unable to estimate the impact of lawyers on litigation payments below $\tau = .5$. Interestingly, the bounds on lawyer value is consistently negative until we reach $\tau = .98$ when the bound includes zero. For own insurance payments the bounds are consistently negative and for total insurance payments from all sources, including third party insurers, the bounds are negative after $\tau = .95$.

The results are consistent with the evidence, presented above, that hiring a lawyer results in a substitution away from first party insurance payments and to third party insurance payments. It is further consistent with the evidence above that on net the trade-off does not result in larger overall payoffs for the client. Finally, the results from our quantile bounds suggest that the right tail of the distribution drives the net negative effect of lawyers. Larger claims appear, paradoxically, to be the cases in which lawyers add the least value in terms of total payments, although the most in terms of litigation, and in which their clients appear to systematically receive less than they would have had they not retained a lawyer. In the next section, we turn to several potential threats to identification and the possibility of effects heterogeneity.

7 Validity of Instrument and Effects Heterogeneity

As noted in Section 4, our identification strategy requires that there is not a second direct or indirect channel through which our invalid instrument might influence outcomes. As we suggested the most likely threat to identification is that modifications to the CS rule changed lawyer's effort or caused them to change their fee structure, perhaps shifting to hourly fees or lump sum payments. This seems unlikely since modifications to the CS rule outside of Florida and Minnesota stipulated that lawyer would be paid before mandated deductions. However, it remains a possibility that in

the face of a demand shift, lawyers in those states modified their effort or fees.

The heterogeneity question arises from the possibility that lawyer's value in a claim might be tied to particular institutional arrangements. As noted above the likely fault of the client does appear to change the value of attorney although the finding that lawyers continue to reduce total recover remains. We examine the role of no fault insurance in determining the value of a lawyer to a claim. Conceptually no-fault insurance is a contract in which each person pays is indemnified only for their own losses regardless of fault and the right to recover from a third party is restricted. In the 1970s, a number of states passed no-fault laws in an effort to reduce the need for attorneys and litigation and hence lower insurance costs (Anderson et al. 2010). During the 1970s a number of plaintiff's lawyers groups opposed state no-fault laws suggesting that they expected these laws to reduce the need for attorneys. However, by the 1990 Anderson et. al. (2010) report that while the use of attorneys was constant for first party insurance claims there was substantial growth in the use of attorneys for third party claims in no fault states. The authors identify several causes for the increase but primarily the argument was that no-fault was no longer effective at preventing litigation.

7.1 Changes in Fees

The concern that lawyers might have responded to changes in the CS rule, either because of changes in market conditions or some other factor, and changed the fee structures that charged on their clients is address in Table 13. In the first two rows of Table 13 we estimate a regression with the total payment to lawyer as the dependent variable. The regression includes only cases in which the victim hired a lawyer and includes the control variables included in IV regressions. The coefficient is the impact of modifying the CS rule.

The first column under the heading 'All cases' shows results for states that applied CS rule changes to all cases. We find a modest decrease after the CS rule changes but it is insignificant. The second column under the heading 'Medical only' repeats the analysis for states that applied CS rule changes only to medical malpractice. Here the CS rule change is supposed to have no impact on cases of auto accidents. We find a small and insignificant decrease in total fees. Overall, the

evidence indicates that it is unlikely that lawyers systematically charged their clients more (or less) in response to the CS rule change.

Did lawyers change how they got paid after CS rule changes? In the next two rows in Table 13, we examine proportions of cases in which lawyers got paid only a contingent fee. In those cases, lawyers got no fixed or hourly fees. The results indicate no significant change in the proportion due to the CS rule change.

To further study the issue, we repeat the IV regressions in Table 11 using 'before fees' measures, i.e., payments before deducting attorney's fees. The first part of Table 14 shows results using collisions only. Other samples including pedestrian accidents or single vehicle accidents show consistent overall patterns. Compared to Table 11, we find that lawyer's values went up significantly. This is natural as we are not deducting the fees from payments and this will obviously inflate the value of lawyer's services. Overall, however, our main findings in Table 11 remain valid. The value of lawyers is negative for total payment. And this is mainly due to the negative contributions from the own insurance payments.

7.2 Attorney Efforts Levels

The other assumption required to retain the validity of instrument even after our offsets correction is that the CS rule modification affected the quantity of lawyer services but not its quality. If lawyers change their effort level in CS modified cases regardless of the terms of the contract, this assumption will be violated. In such a case, lawyers reducing the quality of their services may explain a negative value of lawyers.

Unfortunately, unlike lawyer's fees, lawyer's efforts in a case is not directly observable. Instead, we construct seven proxy variables that reflect a dimension of the lawyer's effort level. The first six variables measure whether or not lawyers made meaningful efforts to help the victims in their efforts to recover from a third party. They are binary indicators that equal one if lawyers 'defended family', 'helped financial planning', 'advised which doctors to use', 'filed a lawsuit', 'negotiated with other driver's insurer', and/or 'gave general advice'. The last variable is the opposite. It is a binary indicator if the survey respondent indicated that the lawyer 'did nothing.'

Part 2 of Table 13 show the regression outcomes of each effort variable on the dummy variable for the CS rule changes. When applied to 'All cases' sample, we find no evidence that any of those effort measures were meaningfully different before and after the rule changes.

We also use state-wise variations in rules to determine lawyer's fees to examine the validity of our instrument. Most states allow attorneys to receive contingent payments based on the total recovered before any CS offsets. An important exception are Florida and Minnesota whose state law requires that attorneys to base their payment on the total recovered after offsets. Theoretically, outside of Florida and Minnesota, there is no reason for a lawyer to change their effort levels in response to the CS rule change because it did not affect how they much compensation they received for a given case.

The second two columns of Table 14 breaks the sample down by the state's method of fee calculation (before or after offsets). Results for states that applied offsets after fees are calculated are found column (3). This sample includes all CS modifying states except Florida and Minnesota. Column (4) includes only Florida and Minnesota. States that applied the CS modification to 'medical cases only' are included as a control group in both (3) and (4). One complication is that the law change in Florida had been in effect since 1976, and therefore, Florida has no within state variation. Since 80% observations from 'all cases' states in (4) are from Florida, using state fixed effects would base the estimates entirely on Minnesota. For this reason, we estimate the models without fixed effects for this comparison.

We find negative effects of hiring a lawyer on total recovery in both samples. Overall, however, the lawyer's values are relatively higher when we exclude Florida and Minnesota. This suggests that negative lawyer values we report in Section 6 are partly driven by claims from Florida and Minnesota. However, one must be careful with this interpretation. Our theoretical analysis in Section A.2 suggests that simply dropping the two states does not guarantee that the lawyer's efforts will stay constant. If the lawyer's effect is proportional to the average net benefits of cases they tend to take, the lawyer's effort will go up in most states besides Florida and Minnesota. The intuition is that in those states the CS rule changes induce lawyers not to take marginal cases with lower claim values. The remaining cases that lawyers still take therefore have higher values for the

lawyers. Since their compensation is still based on payments before CS offsets, the average benefits of the remaining cases to lawyers is actually higher. Hence, column (3) in Table 14 represents the most optimistic estimates of lawyer's values.

7.3 No-fault states

The effects of lawyer's service could be heterogeneous depending on institutions of the state. In particular, we focus on no-fault systems as these systems were specifically designed to remove auto claims from the tort system and were actively opposed at the time of passage by plaintiff's attorneys (see Anderson et. al. 2010). No-fault systems essentially require people to pay their own accident costs if the damages are below a certain threshold. Thus it seems reasonable to assume that the value of a lawyer is more limited in no-fault states. Fault states require proof of fault in order to get a recovery from a third party, which means that the threat of a suit, i.e. more effort by the lawyer, is often required to receive a larger recovery. Thus in no-fault states, clients simply may not have as much need of the services of attorneys or similarly when they do hire lawyers, the lawyers ability to generate additional recovery is much more limited. There is, however, a counter argument, namely that in no-fault states lawyers are more valuable as they help clients get clients out of the no-fault system when their claims are more valuable in the fault system (see Anderson et. al. 2010).

In the first part of Table 15, we estimate the value of lawyers separately for no-fault states and fault states. Most states did not change the no-fault status during our sample period. Because there is little within-state variation in no-fault variable, we cannot include fixed effects. For total payment, the lawyer value is clearly negative for fault states, while can be positive for no-fault states. This difference stems from the difference in direct payments. In no-fault states, lawyers appear to be helpful in recovering higher payments from the injurer's insurance, consistent with the Anderson et. al. (2010) view that lawyers in no-fault states have options to increase their client's recovery.

7.4 States that have unclear provisions for the CS rule

Several states, including Georgia, Kansas, Kentucky, Ohio, Oklahoma, Oregon and Wisconsin, have ambiguous provisions in their CS rules regarding offsets. The law in these states says *may* rather than *shall* deduct as it does in other offset states. Put differently, in these states the courts can deduct collateral sources but do not have to. They belong to 'All cases' states and 'Medical only' states. For this reason, we exclude them from our samples. The findings, which can be found in Second part of Table 15, are consistent with what we have found before. The negative estimates for lawyer's value get a little bigger without those states. This suggests the possibility that in the "may states" judges are exercising their option not to reduce awards by the full amount of potential offsets or that the threat of this possibility results in higher settlement payments.

8 Conclusion

In this paper we examine the value added by hiring an attorney. A simple comparison of recoveries in cases with and without attorneys will be uninformative given the endogenous nature of retaining a lawyer. We utilize modifications to the collateral source rule that require payments from first party insurance sources to be deducted from any award as an instrument for retaining an attorney. The instrument is problematic in the sense that it impacts both the likelihood of retaining an attorney and the eventual recovery. We exploit the mechanical nature of modification to the CS's offsets to produce bounds on the affect of attorneys on claim value.

Overall, the results are consistent with the difference between hiring an attorney and accepting the payments offered from first party and third party insurers without representation being negative. That is, for the average case lawyers appear to reduce total recovery from the case. This effect appears to be driven by reductions in payments from the injured party's own insurance being larger than the increases in recovery from third parties. All the effect is heterogeneous, lawyers appear to generate more value in no-fault states and the likelihood of negligence appears to impact the value of attorney's the overall picture is that lawyer's affect on total recovery is negative.

The second finding is that the negative value of an attorney in the average case appears to be

driven by the very top of the distribution. Kritzer (2004) argues that the reputational constraints deter self-dealing by lawyers. Reputational penalties keep attorneys from pushing the returns into negative territory and simply taking a one third cut of the payment the client would have received without representation. The combination of market pressures to settle cases quickly and with minimum investment combined with reputational penalties for settling cases too cheaply would produce exactly the phenomenon we observer for the bottom 97% of our cases. Specifically, an average return over the amount offered by insurers in the absence of legal representation of zero.

The question remains why these two limiting factors pushing the return above opportunity cost to zero break down for larger cases. Engstrom (2011) argues that larger claims involving injuries that are more serious simply require greater investment of attorney time and greater expenses. Assuming that the value of the claimants injury is fairly consistent it is perhaps not surprising that on average once expert witness fees and other expenses are deducted from any settlement amount the premium earned by a lawyer over the amount paid to claimants is negative.

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A Appendix

A.1. Demand-Supply Analysis Under Cost Sharing

We re-do the analysis in Section 3 assuming that the attorney and the client divide the legal cost C in case they win. We continue to assume that, if they lose, the attorney is solely responsible for all expenses. We keep the modeling assumptions in Section 3. Recall that there the cost of claimant of pursuing a claim without an attorney is normalized to be 0. But arguments here as well as those in Section 3 continue to hold as long as the cost without attorney is lower than the cost with attorney.

Consider states that did not modify the CS rule at all. The clients receive $P_1(1-\beta)(M-C)$ if they hire an attorney and P_0M if they do not. So they will hire an attorney if

$$P_1(1-\beta)(M-C) > P_0M. \tag{9}$$

The attorney will take the case if

$$P_1\left((1-\beta)C+\beta M\right) > C. \tag{10}$$

Now consider states that have modified the CS rule. As discussed, some states allow their attorney to receive payments based on M, while other states require that their attorneys to receive contingent payment based on $M-L$.

Consider the first case. The claimant receives $P_1(M-C-L)-P_1\beta(M-C)$ if they hire an attorney and $P_0(M-L)$ if they do not. So the condition to hire an attorney is $P_1(1-\beta)(M-C)-(P_1-P_0)L > P_0M$. Since $(P_1-P_0)L > 0$, when compared to (9), we conclude that the CS modification reduces the demand for an attorney. The attorney will take the case if $P_1\left((1-\beta)C+\beta M\right) > C$. When compared to (10), the CS modification has no effect on supply.

For the second case, claimants receive $P_1(1-\beta)(M-C-L)$ if they hire an attorney and $P_0(M-L)$ if they do not. So the condition to hire an attorney is

$$P_1(1-\beta)(M-C)-\left(P_1(1-\beta)-P_0\right)L > P_0M. \tag{11}$$

Consider a potential client who was willing to hire an attorney without the CS modification.

For them, the condition (9) implies that

$$\frac{P_1(1-\beta)}{P_0} \cdot \frac{M-C}{M} > 1.$$

Since $\frac{M-C}{M} < 1$, this leads to $P_1(1-\beta) > P_0$. From (11), we conclude that the demand for an attorney will be negatively affected for such a client.

Is it possible that there is a potential client who did not want a lawyer without the CS modification but changes his decision and wants an attorney with the modification? In other words, is it possible to have a potential defier? For such a client to exist, a necessary condition is that (9) does not hold while (11) does hold. This will be the case if

$$\frac{M-L}{M-C-L} < \frac{P_1(1-\beta)}{P_0} < \frac{M}{M-C}.$$

This will be be true only if $M-C > M$ or $C < 0$, which is a contradiction. So the CS modification can only have a negative or no effect on the demand.

The attorney will take the case if $P_1\left((1-\beta)C + \beta(M-L)\right) > C$. When compared to (10), there is an extra minus term $-P_1\beta L$ on the left side of the inequality. So the CS modification has a negative effect on supply.

In summary, the conclusion in Section 3 remains to be true if the cost is shared between the client and attorney.

A.2. Analysis for Lawyer's Efforts

The purpose of this section is to use the Model in Section A.1 and derive a prediction for lawyer's effort level. In turns out that the simpler model in Section 3 will lead to the same conclusion. The working assumption in this section is that the lawyer's effort level is proportional to the expected net benefit (or surplus) to lawyers from claims they take. Of course, there can be other ways to model lawyer's efforts. But we believe that it is a reasonable way to think about lawyer's effort.

Suppose that claim value of a case M is randomly drawn from a fixed distribution $F(\cdot)$. The distribution function does not change over the CS rule modification. It is a reasonable assumption if the CS rule changes were not a determinant of damages from auto accidents.

Consider first states that did not modify the CS rule at all. The demand condition (9) means that only cases with value M satisfying $M \geq \frac{P_1(1-\beta)}{P_1(1-\beta)-P_0}C$ are willing to retain attorneys. The supply condition (10) means that lawyers will accept cases with M such that $M \geq \frac{1-P_1(1-\beta)}{P_1\beta}C$. Therefore, lawyers will be hired if $M \geq M^*$ where

$$M^* = \max\left(\frac{P_1(1-\beta)}{P_1(1-\beta)-P_0}\,,\, \frac{1-P_1(1-\beta)}{P_1\beta}\right) \times C.$$

For an attorney who takes a case with value M, the expected net benefit is $S(M) = P_1\beta M - (1 - P_1(1-\beta))C$. Note that for given P_1, P_0, β, C, the surplus is a linear function of M with the slope $P_1\beta$. Also note that $0 < P_1\beta < 1$. The average surplus to lawyers, and therefore, lawyer's effort level, is given by

$$\text{AS}_0 = \int_{M^*}^{\infty} S(M) dF(M).$$

Next, consider states that modified the CS rule. First consider the case where lawyers were allowed to receive payments based on M. From supply and demand conditions, it is straightforward to calculate that lawyers will be hired if $M \geq M_1^*$ where

$$M_1^* = \max\left(\frac{P_1(1-\beta) + P_1 - P_0}{P_1(1-\beta) - P_0}, \frac{1 - P_1(1-\beta)}{P_1\beta}\right) \times C.$$

Since $P_1 > P_0$, it is clear that $M_1^* \geq M^*$. The expected surplus for lawyers is still $S(M) = P_1\beta M - (1 - P_1(1-\beta))C$. This leads to the average surplus to lawyers

$$\text{AS}_1 = \int_{M_1^*}^{\infty} S(M) dF(M).$$

Clearly $\text{AS}_1 \geq \text{AS}_0$ meaning that lawyers exert *more* efforts in this case. The result is intuitive because marginal cases, $M \in [M^*, M_1^*)$, will cease to demand lawyers. So the remaining cases that retain lawyers tend to have higher M on average. Since lawyer's surplus for a given case does not change, the surplus will go up.

Finally, consider cases where attorneys are required to receive fees based on $M - L$. From the supply and demand conditions, lawyers will be hired if $M \geq M_2^*$ where

$$M_2^* = \max\left(\frac{P_1(1-\beta)}{P_1(1-\beta) - P_0}, \frac{1 - P_1(1-\beta)}{P_1\beta}\right) \times C + L = M^* + L.$$

Clearly $M_2^* > M^*$. The expected surplus to lawyers is $\tilde{S}(M) = P_1\beta(M - L) - (1 - P_1(1-\beta))C$, which is proportional to $M - L$ not M. The average surplus to lawyers becomes

$$\text{AS}_2 = \int_{M_2^*}^{\infty} \tilde{S}(M) dF(M).$$

It is straightforward to show that $\text{AS}_2 \leq \text{AS}_0$. So in the second case, lawyers exert *less* efforts. Lawyers will drop marginal cases, so the remaining cases tend to have higher M. But surplus to lawyers get reduced by the same proportion, so overall the average surplus to lawyers (and their efforts) gets smaller.

In sum, if we assume that lawyers base their effort on the expected surplus, we predict that lawyers in states like Florida and Minnesota would undertake a lower effort level. This is because their law required lawyers to get paid after offsets (i.e., based on $M - L$). In contrast, in other states which allowed their lawyers to get paid before offsets (i.e., based on M), we predict that

lawyers will make more efforts on average.

A.3. Proof of Proposition 5.1.

By the independence assumption, the numerator of the right hand side of (5) is equal to $E[Y(0, D(0)) - Y(0, D(1))]$. By the definition of potential variables and the law of iterated expectation, we have

$$\begin{aligned}
&E[Y(0, D(0)) - Y(0, D(1)) \\
&= E\left[\{D(0)Y(0,1) + (1 - D(0)Y(0,0)\} - \{D(1)Y(0,1) + (1 - D(1)Y(0,0)\}\right] \\
&= E\left[(D(0) - D(1))\left(Y(0,1) - Y(0,0)\right)\right] \\
&= \mathrm{P}\left(D(0) - D(1) = 1\right)\mathrm{E}\left[Y(0,1) - Y(0,0)|D(0) - D(1) = 1\right] \\
&\quad - \mathrm{P}\left(D(0) - D(1) = -1\right)\mathrm{E}\left[Y(0,1) - Y(0,0)|D(0) - D(1) = -1\right].
\end{aligned}$$

The second term in the last quantity is zero because of the non-existence of defiers. Finally, $\Pr(D(0) - D(1) = 1)$ can be identified by $E[D|Z = 0] - E[D|Z = 1]$, so we get the expression (5).

A.4. Including Covariates in the Bounds Analysis

This section discusses a simple method to control for the effects of covariates. Allowing covariates in the regression models is helpful in many ways. It helps refine the identification assumption by making the independence assumption (Assumption 1(ii)) conditional on covariates. This expands the plausibility of the assumption. If it necessary, we can allow the size of treatment effects to be different for subgroups defined by covariates (Abadie (2005)). Let X_i be the collection of covariates, such as, the passage of other tort reforms and characteristics of accidents.

To incorporate the covariate into the analysis, we take the traditional two-stage least squares (2SLS) setup where the covariates enter the outcome equation linearly and additively:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 Z_i + \gamma X_i + \varepsilon_i,$$

and the covariates added to the set of instruments. For the average indirect effect, the upper bound in Proposition 5.2 can be estimated by a 2SLS estimate with Y_i as the dependent variable, and D_i and X_i as endogenous and exogenous independent variables, respectively, and (Z_i, X_i) as instruments. For the lower bound, recall that $W_i = 0$ when $Z_i = 0$. So the lower bound can be estimated by a 2SLS estimate with $Y_i + W_i$ as the dependent variable.

Including covariates in quantile effects can be done as follows. Define the conditional mean of T_i given D_i and X_i:

$$\mathrm{E}\left[T_i|D_i=d,X_i=x\right]=\mathrm{E}\left[I(Y_i(0,D_i(1))\leq y)|D_i=d,X_i=x\right]=F(y|d,x).$$

Use an additive, linear model

$$\mathrm{E}\left[T_i|D_i,X_i\right]=\beta_0(y)+\alpha(y)D_i+X_i'\beta_1(y).$$

Then $F_{(1)}(y|x)=\beta_0(y)+\alpha(y)+x'\beta_1(y)$ and $F_{(0)}(y|x)=\beta_0(y)+x'\beta_1(y)$. From the conditional distribution function $F_{(1)}(y|x)$, one can obtain the marginal distribution $F_{(1)}(y)=\int F_{(1)}(y|x)f_X(x)dx\approx n^{-1}\sum_{i=1}^n F_{(1)}(y|x_i)$. Obtain $F_{(0)}(y)$ similarly. Invert them and obtain $Q_{(1)}\left(\tau\right)$ and $Q_{(0)}\left(\tau\right)$, and define the τ-th quantile effect

$$\Pi(\tau)=Q_{(1)}\left(\tau\right)-Q_{(0)}\left(\tau\right).$$

One can obtain the lower bound $\Pi^L(\tau|x)$ similarly.

Table 1: Collateral Source Rule Modifications Requiring Offsets of 1st Party Insurance Payments.

State	Statute	Year	Types of Claims	Effective Date	CS 1	CS 2	CS 3
Alabama	§12-21-45	1987	All	June, 1987	Yes	Yes	
Alaska	§09.17.070	1986	All	1986, amended Apr, 2008	Yes	Yes	
Arizona	§12-565	1976	Medical only	1976			
Arkansas	None						
California	§3333.1	1975	Medical only	Dec, 1975			
Colorado	§13-21-111.6	1986	All	July, 1986			
Connecticut	§52-225a	1987	All	Oct, 1987	Yes	Yes	
Delaware	§6862	1976	Medical only	Apr, 1976			
DC	None						
Florida	§768.76	1976	All	July, 1976		Yes	
Georgia	§51-12-1	1987-1991	All	July, 1987-Mar, 1991	Yes	Yes	
Hawaii	§663-10	1986	All	Aug, 1986			
Idaho	§6-1606	1990	All	Mar, 1990	Yes	Yes	
Illinois	5/2-1201 and 5/2-1205.1	1986-1997	All	Nov, 1986 - Dec, 1997	Yes	Yes	Yes
Indiana	§34-44-1-2, now 34-4-36-1	1986	All	Sep, 1986	Yes	Yes	
Iowa	§668.14	1987	All	July, 1987		Yes	
Kansas	§60-3802	1988-1993	All	July, 1988-Apr, 1993			
Kentucky	§411.188	1988-1995	All	July, 1988-Jan, 1995			
Louisiana	None						
Maine	§2906	1990	Medical only	Apr, 1990			
Maryland	None						
Massachusetts	231 §60G	1986	Medical only	Nov, 1986			
Michigan	§600.6303	1986	All	Oct, 1986	Yes	Yes	
Minnesota	§548.251	1986	All	Mar, 1986	Yes	Yes	
Mississippi	None						
Missouri	None						
Montana	§27-1-308	1987	All	Oct, 1987	Yes	Yes	

Nebraska	§44-2819	1976	Medical only	Apr, 1976		
Nevada	§42.021	2004	Medical only	Nov, 2004		
New Hampshire	§507-C:7	1977-1980	All	Sep, 1977-Dec, 1980		
New Jersey	§2A:15-97	1987	All	Dec, 1987		
New Mexico	None					
New York	§4545	1984	Medical only	Aug, 1984		
North Carolina	None					
North Dakota	§32-03.2-02	1987	All	July, 1987	Yes	Yes
Ohio	§2317.45	1997-1998	All	Jan, 1997-Feb, 1998	Yes	Yes
Oklahoma	§1-1708.1D	2003	Medical only	July, 2003		
Oregon	§31.580	1987	All	July, 1987	Yes	Yes
Pennsylvania	§1303.508	2002	Medical only	Mar, 2002		
Rhode Island	§9-19-34.1	1976	Medical only	1976		
South Carolina	None					
South Dakota	§21-3-12	1977	Medical only	Apr, 1977		
Tennessee	§29-26-119	1975	Medical only	July, 1975		
Texas	None					
Utah	§78-14-4.5	1986	Medical only	July, 1986		
Vermont	None					
Virginia	None					
Washington	§7.70.080	1975	Medical only	June, 1975		
West Virginia	§55-7b-9a	2003	Medical only	Mar, 2003		
Wisconsin	§893.55(7)	1995	Medical only	May, 1995		
Wyoming	None					

Source: Database of State Tort Law Reforms (5th edition) and State Statutes. In 'Types of Claims' column, 'Medical only' means that the modifications were applied to medical malpractices only, while 'All' means the modifications were applied to all cases. CS1 – CS3 indicate what sources are exempted in the modifications; 'CS 1' means that modification exempts worker's compensation and 'CS 2' means that modification exempts Medicaid & Medicare, and 'CS 3' means CS Reduction cannot be bigger than 50% of total judgement.

Table 2: Distribution of Accidents, by year.

Year	Frequency	Percentage
1982	159	0.9
1983	851	4.9
1984	1,246	7.1
1985	1,347	7.7
1986	197	1.1
1989	1,381	7.9
1990	1,596	9.1
1991	1,888	10.8
1992	142	0.8
1995	1,352	7.7
1996	1,745	10.0
1997	2,098	12.0
1998	208	1.2
1999	984	5.6
2000	946	5.4
2001	1,197	6.8
2002	183	1.0
Total	17,520	100.0

Number of accidents by year.

Table 3: Distribution of Accidents, by state

State	Frequency	Percentage	State	Frequency	Percentage
AK	4	0.02	MT	58	0.33
AL	266	1.52	NC	537	3.07
AR	242	1.38	ND	29	0.17
AZ	315	1.8	NE	117	0.67
CA	2040	11.64	NH	104	0.59
CO	239	1.36	NJ	558	3.18
CT	180	1.03	NM	93	0.53
DC	39	0.22	NV	103	0.59
DE	64	0.37	NY	1,058	6.04
FL	882	5.03	OH	758	4.33
GA	467	2.67	OK	283	1.62
HI	2	0.01	OR	292	1.67
IA	209	1.19	PA	810	4.62
ID	80	0.46	RI	89	0.51
IL	713	4.07	SC	303	1.73
IN	430	2.45	SD	46	0.26
KS	130	0.74	TN	362	2.07
KY	320	1.83	TX	1,166	6.66
LA	338	1.93	UT	146	0.83
MA	378	2.16	VA	374	2.13
MD	410	2.34	VT	33	0.19
ME	92	0.53	WA	471	2.69
MI	560	3.2	WI	348	1.99
MN	213	1.22	WV	150	0.86
MO	410	2.34	WY	32	0.18
MS	177	1.01	Total	17,520	100.0

Number of accidents by state.

Table 4: Summary Statistics

Variables	**Whole Sample**			**CS Rule Modified**			**Collisions Only**		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Accident was a Collision	0.82	1.00	0.39	0.81	1.00	0.39	1.00	1.00	0.00
Single-vehicle crash	0.14	0.00	0.35	0.14	0.00	0.35	0.00	0.00	0.00
Pedestrian Accident	0.03	0.00	0.16	0.03	0.00	0.17	0.00	0.00	0.00
Hired Lawyer	0.32	0.00	0.47	0.32	0.00	0.47	0.35	0.00	0.48
Age of Claimant	36.73	35.00	17.60	37.00	35.00	17.71	37.81	36.00	17.48
Claimant was Male	0.40	0.00	0.49	0.40	0.00	0.49	0.38	0.00	0.49
Number Injured	1.35	1.00	0.70	1.35	1.00	0.71	1.37	1.00	0.72
Number of Work Days Lost	15.10	0.00	60.19	15.25	0.00	61.52	14.69	0.00	60.91
Amount Claimed in Medical Cost	10,596	1,730	56,539	11,057	1,799	60,643	9,635	1,805	48,728
No Fault	0.24	0.00	0.43	0.32	0.00	0.47	0.32	0.00	0.47
Compulsory Insurance	0.77	1.00	0.42	0.74	1.00	0.44	0.74	1.00	0.44
Non-economic Damages Cap	0.08	0.00	0.26	0.08	0.00	0.27	0.08	0.00	0.26
State limits contingent fees	0.19	0.00	0.39	0.24	0.00	0.43	0.25	0.00	0.44
State awards prejudgement interest	0.37	0.00	0.48	0.34	0.00	0.47	0.33	0.00	0.47
State has js liability reform	0.47	0.00	0.50	0.51	1.00	0.50	0.52	1.00	0.50
State per-capita Income	32,825	32,786	5,250	33,534	33,534	5,157	33,677	33,534	5,109
Total Payment	15,673	1,122	194,973	16,579	1,015	221,699	14,711	1,233	160,436
Direct Payment	10,175	0.00	185,965	10,948	0.00	212,375	9,842	0.00	150,023
Litigation Payment	7,219	0.00	183,491	8,127	0.00	210,153	6,694	0.00	146,771
Own Insurance Payment	5,497	0.00	42,167	5,631	0.00	45,709	4,869	0.00	35,026
Total Insurance payment	8,453	673	49,760	8,452	561	53,203	8,017	708	45,386
No. of observations	17,520			13,260			10,729		

Mean, median, and standard deviation of variables. All dollar amounts are measured in 2002 constant dollars.

Table 5: Characteristics of Claims, by decision to hire an attorney

Variables	**Hired Lawyer**			**Talk to Lawyer**			**Not Hired Lawyer**		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Accident was a Collision	0.90	1.00	0.30	0.88	1.00	0.32	0.78	1.00	0.42
Accident was a Single Vehicle	0.04	0.00	0.20	0.07	0.00	0.25	0.19	0.00	0.39
Pedestrian Accident	0.04	0.00	0.20	0.04	0.00	0.18	0.02	0.00	0.14
Age of Claimant	37.18	35.00	16.23	36.05	34.00	16.47	36.51	34.00	18.21
Claimant was Male	0.41	0.00	0.49	0.40	0.00	0.49	0.40	0.00	0.49
Number of Work Days Lost	31.53	2.00	89.92	10.87	0.00	42.69	7.30	0.00	36.28
Number Injured	1.40	1.00	0.75	1.30	1.00	0.65	1.33	1.00	0.68
Amount Claimed in Medical Cost	20,594	5,282	87,122	8,613	1,704	47,923	5,573	933	30,237
No Fault	0.25	0.00	0.43	0.22	0.00	0.42	0.24	0.00	0.43
Compulsory Insurance	0.78	1.00	0.42	0.75	1.00	0.43	0.76	1.00	0.43
Non-economic Damages Cap	0.07	0.00	0.25	0.06	0.00	0.25	0.08	0.00	0.27
State limits contingent fees	0.21	0.00	0.41	0.20	0.00	0.40	0.18	0.00	0.38
State awards prejudgement interest	0.35	0.00	0.48	0.36	0.00	0.48	0.38	0.00	0.49
State has js liability reform	0.44	0.00	0.50	0.48	0.00	0.50	0.48	0.00	0.50
State per-capita Income	33,156	33,016	5,336	32,658	32,611	5,024	32,668	32,524	5,201
Total Payment	39,544	7,972	341,348	7,628	1,851	31,020	4,346	396	19,305
Direct Payment	28,942	4,101	326,964	3,168	0.00	9,532	1,270	0.00	5,061
Litigation Payment	22,188	1,080	322,955	585	0.00	4,525	116	0.00	1,998
Own Insurance Payment	10,601	0.00	69,014	4,460	0.00	29,196	3,076	0.00	18,542
Total Insurance Payment	17,355	2,753	82502.10	7,042	1,704	30,522	4,229	370	19,136

Mean, median, and standard deviations of variables in three groups defined by the decision to hire an attorney. All dollar amounts are measured in 2002 constant dollars. Outcomes from 'Whole Sample'.

Table 6: Characteristics of Claims, by modifications of the CS rule.

Variables	**(1) CS Rule Modified (applied to any)**			**(2) CS Rule Modified (medical only)**			**(3) CS Rule Not Modified**		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Hired Lawyer	0.28	0.00	0.45	0.34	0.00	0.47	0.34	0.00	0.47
Accident was a Collision	0.83	1.00	0.38	0.84	1.00	0.37	0.82	1.00	0.38
Age of Claimant	38.14	36.00	18.41	36.77	35.00	17.42	35.74	34.00	17.15
Claimant was Male	0.40	0.00	0.49	0.40	0.00	0.49	0.41	0.00	0.49
Number of Work Days Lost	13.39	0.00	56.40	14.14	0.00	59.07	14.08	0.00	54.01
Number Injured	1.36	1.00	0.69	1.37	1.00	0.75	1.36	1.00	0.68
Amount Claimed in Medical Cost	13,645	2,019	82,386	9,612	1,946	46,206	8,908	1,652	40,336
No Fault	0.46	0.00	0.50	0.26	0.00	0.44	0.00	0.00	0.05
Compulsory Insurance	0.72	1.00	0.45	0.66	1.00	0.47	0.79	1.00	0.41
Non-economic Damages Cap	0.23	0.00	0.42	0.00	0.00	0.05	0.06	0.00	0.24
State limits contingent fees	0.26	0.00	0.44	0.33	0.00	0.47	0.01	0.00	0.08
State awards prejudgement interest	0.33	0.00	0.47	0.23	0.00	0.42	0.44	0.00	0.50
State has js liability reform	0.74	1.00	0.44	0.55	1.00	0.50	0.34	0.00	0.47
State per-capita Income	34,523	33,836	5,320	35,360	35,410	4,675	30,730	30,278	4,837
Total Payment	17,679	313	252,162	12,345	999	63,235	12,294	1,523	55,741
Direct Payment	10,891	0.00	233,963	7,774	0.00	47,223	7,428	0.00	41,243
Litigation Payment	8,464	0.00	231,485	4,760	0.00	36,901	4,172	0.00	30,342
Own Insurance Payment	6,787	0.00	70,824	4,570	0.00	28,953	4,866	0.00	27,411
Total Insurance Payment	9,214	22	78291.75	7,585	465	40,767	8,122	1,015	35,727

Mean, median, and standard deviations of variables in three groups. All dollar amounts are measured in 2002 constant dollars. The group (1) includes claims in state that modified the CS rule and applied it to all types of claims, after modified. The group (2) includes claims in states that modified the rule but restricted its application to medical malpractice only. The group (3) includes claims in states that did not modify the CS rule.

Table 7: Characteristics of Claims, by quantiles of total recovery.

Variables	Q4	Q5	Q6	Q7	Q8	Q9	Q10
Amount Claimed in Medical Cost	460	1,432	2,433	3,566	5,468	9,170	47,752
Amount Claimed in Damages	548	1,709	2,708	4,016	6,385	10,656	52,084
Total Compensation all source	95	608	2,028	4,364	8,046	15,993	125,761
Direct Payment	22	213	900	2,078	4,058	9,093	85,463
Litigation Payment	0	25	151	494	1,365	4,612	65,589
Own Insurance Payment	73	395	1,127	2,286	3,987	6,900	40,298
Total Insurance Payment	94	583	1,876	3,870	6,680	11,381	60,171
Hired Lawyer	0.05	0.09	0.19	0.27	0.39	0.59	0.80
Vehicle Collision	0.79	0.79	0.86	0.85	0.86	0.84	0.83
Single Vehicle Crash	0.17	0.16	0.11	0.12	0.11	0.12	0.10
Pedestrian Accident	0.02	0.02	0.02	0.02	0.01	0.03	0.05
Number of Work Days Lost	1.99	3.69	5.76	7.32	12.61	19.92	59.79
Number Injured	1.40	1.38	1.33	1.26	1.26	1.28	1.30
No Fault Indicator	0.24	0.24	0.20	0.21	0.23	0.24	0.27
Compulsory Insurance	0.85	0.80	0.78	0.78	0.72	0.76	0.76
Non-economic Damages Cap	0.12	0.10	0.08	0.06	0.06	0.07	0.06

Average values of variables in groups defined by deciles of the total recovery. All dollar amounts are measured in 2002 constant dollars.

Table 8: Naive Regression Results.

Dependent Variables	**Collisions only**		**Collisions + Pedestrian accidents**		**Collisions + Pedestrian & Single accidents**	
	(1)	(2)	(3)	(4)	(5)	(6)
Total payment	15,846***	11,042***	15,712***	12,649***	16,296***	16,796***
	(2,801)	(2,823)	(2,815)	(3,086)	(2,873)	(3,954)
Direct payment	16,391***	11,018***	16,121***	12,325***	16,635***	18,105***
	(2,697)	(2,640)	(2,710)	(2,917)	(2,766)	(3,829)
Litigation payment	11,538***	6,863***	11,335***	8,206***	12,261***	13,852***
	(2,215)	(2,588)	(2,679)	(2,874)	(2,734)	(3,795)
Own insurance	-8,256***	-5,906***	-7,992***	-6,193***	-8,121***	-9,839***
	(1,247)	(1,402)	(1,252)	(1,519)	(1,278)	(1,731)
Total insurance	-3,403***	-1,750	-3,206**	-2,074	-3,747***	-5,586***
	(1,274)	(1,473)	(1,280)	(1,587)	(1,306)	(1,767)
State & year fixed effects	No	Yes	No	Yes	No	Yes

The estimated effects of lawyers from the "wrong" regression. Standard errors are in parentheses. All effects are measured in 2002 constant dollars. Read the main text for definitions of outcome measures and samples used. All payment measures are after attorney's fees, i.e., after deducting attorney's fees. Columns (1), (3), and (5) do not use fixed effects, while columns (2), (4), and (6) include state and year fixed effects. The following control variables were included in the regression (but their results are not reported here to save space): age of claimant, claimant is male, number of injured, number of work days lost, alleged medical cost, dummy for no insurance, dummy for non-economic damage cap in place, dummy indicating no fault state, dummy for state that has fee limit, dummy for state having compulsory insurance, dummy for state having total cap, dummy for per pay, dummy for pre-judge interest (the interest payed on a judgement or settlement based on the time it takes to resolve the case), dummy for joint and several liability reform, and dummy for structure recovery.

Table 9: Probabilities of Hiring Attorneys.

	Group 1	Group 2	t-value
Before	0.3530	0.3735	-1.1885 (0.2347)
After	0.3112	0.3659	-5.0105 (0.0001)
t-value	3.0196	0.5062	
	(0.0025)	(0.6127)	

Probabilities of hiring attorneys. At the bottom and in the left, we have t statistics and p-values (in parentheses) of the differences in probabilities. 'Before' means before the modification and 'After' means after the modification. Group 1 is states that modified the CS rule and applied it to all cases. Group 2 is states that modified the CS rule but applied it only to medical malpractice.

Table 10: First Stage F statistics.

	(1)	(2)
Variables	Hired Lawyers	Hired Lawyers
CS rule change	0.113***	0.112***
	(0.034)	(0.039)
Value of case	0.234***	0.223***
	(0.028)	(0.028)
CS*VC	-0.113***	-0.093**
	(0.037)	(0.037)
Fixed effects	No	Yes
R-squared	0.090	0.122
F statistic	34.697	32.305

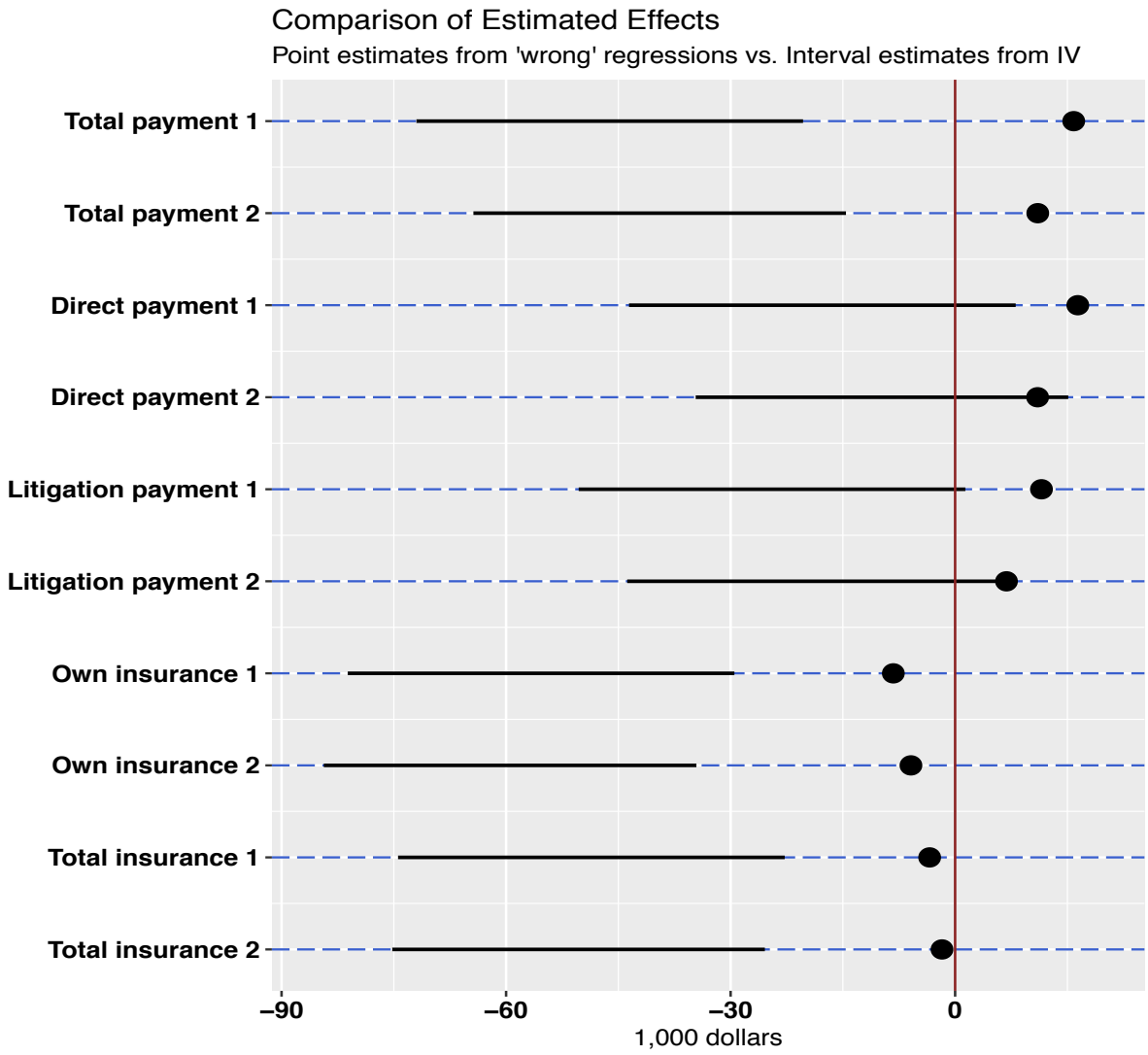
The first stage regression. The dependent variable is "Hired lawyers" and the main independent variables are CS rule change, value of the case (VC), and their interaction term. The list of other control variables can be found in Table 8. The F statistic tests the relevance condition of the IVs, i.e, it tests the null hypothesis that coefficient vectors of three instruments are all equal to zero. Column (2) includes state and year fixed effects as additional control variables.

Table 11: Average Indirect Effects of Hiring Attorneys from IV regressions.

Outcomes	**Collisions only**		**Collisions + Pedestrian accidents**		**Collisions + Pedestrian & Single accidents**	
	(1)	(2)	(3)	(4)	(5)	(6)
Total payment	[-71,985, -20,320]	[-64,369, -14,585]	[-66,731, -15,800]	[-59,004, -10,407]	[-87,370, -18,965]	[-85,386, -18,407]
	(-118,864, 21,887)	(-113,050, 29,510)	(-117,699 , 31,050)	(-111,862, 38,440)	(-154,791, 43,768)	(-155,487, 46,955)
Direct payment	[-43,574, 8,090]	[-34,671, 15,113]	[-39,942, 10,989]	[-31,452, 17,146]	[-49,919, 18,484]	[-45,538, 21,440]
	(-85,423, 47,252)	(-78,206, 56,167)	(-86,279, 55,022)	(-79,608, 63,179)	(-112,604, 78,962)	(-110,735, 84,501)
Litigation payment	[-50,300, 1,365]	[-43,816, 5,968]	[-46,664, 4,267]	[-40,300, 8,298]	[-57,947, 10,456]	[-55,234, 11,743]
	(-91,499, 39,753)	(-86,758, 46,212)	(-92,448, 47,636)	(-87,948, 53,638)	(-120,265, 70,386)	(-120,101, 74,233)
Own insurance	[-81,162, -29,497]	[-84,357, -34,573]	[-80,117,-29,186]	[-81,901, -33,304]	[-111,181, -42,777]	[-114,295, -47,317]
	(-108,383, -8,342)	(-113,033, -12,242)	(-108,940, -5,951)	(-112,063, -8,906)	(-147,606, -14,929)	(-152,369, -18,157)
Total insurance	[-74,437, -22,772]	[-75,212, -25,428]	[-73,396, -22,465]	[-73,053, -24,456]	[-103,153, -34,749]	[-104,599, -37,620]
	(-102,329, -623)	(-104,364, -2,175)	(-102,887, 1,745)	(-103,728, 866)	(-139,672, -6,421)	(-142,612, -8,050)
Fixed effects	No	Yes	No	Yes	No	Yes

Bounds of the average effects of hiring attorneys using instrumental variables. The 95% confidence intervals are in parentheses. All effects are measured in 2002 constant dollars. See the main text for explanations of the outcome measures and definitions of samples used. The control variables at the bottom of Table 8 are also used. Columns (1), (3) and (5) do not have any fixed effects, while columns (2), (4) and (6) include state and year fixed effects. For 'collisions + pedestrian accidents' sample, we included a dummy for the pedestrian accident as an additional control variable. When we added single accidents in the final sample, we included a dummy for single vehicle accidents.

Figure 1: Point estimates from "wrong" regressions vs. Bound estimates from IV regressions.



Comparison of point estimates (dots) from the wrong regressions and bound estimates (thick solid horizontal lines) from the IV regressions. The wrong regression tends to overestimate the value of attorneys. The sample includes collisions only. The point estimates are from columns (1) and (2) of Table 8, while the bound (interval) estimates are from columns (1) and (2) of Table 11. Each horizontal line corresponds to a regression model. The first line, 'Total payment 1', uses total payment as the outcome variable but the regression specification does not include fixed effects. The second line, 'Total payment 2', uses the same outcome variable but the regression specification includes fixed effects. The same naming convention applies to other cases.

Table 12: Quantile Effects of Hiring an Attorney.

	$\tau = 0.5$	$\tau = 0.75$	$\tau = 0.9$	$\tau = 0.95$	$\tau = 0.97$	$\tau = 0.98$
Total payment	[3,630, 7,594]	[7,009, 14,906]	[131, 22,457]	[-56,347, 25,000]	[-161,562, -33,424]	[-158,217, -104,291]
Direct payment	NA	[7,892, 14,218]	[10,382, 24,158]	[-35,122, 30,886]	[-89,036, 14,493]	[-93,588, 56,796]
Litigation payment	NA	NA	[-13,979, -13,730]	[-49,070, -7,343]	[-87,570, -2,726]	[-89,570, 12,316]
Own Insurance	NA	[-10,334, -3,284]	[-24,189, -9,453]	[-72,432, -24,064]	[-95,550, -52,563]	[-95,501, -49,054]
Total Insurance	[-685, 660]	[-4,290, 5,016]	[-14,788, 9,141]	[-76,089, -1,303]	[-95,171, -40,988]	[-91,777, -45,793]

Bounds of the τ-th quantile effects of hiring attorneys, measured in 2002 constant dollars. The sample includes collisions only. All outcome measures are after fees, i.e., after deducting attorney's fees. Control variables appearing in Table 8 were also used in regression. 'Total payment' is the sum of all payments: direct payment plus payments from the first party (claimant's own) insurance. 'Direct payment' includes payments from injurer's insurance plus settlements or trial awards (if there is any). 'Litigation payment' means amounts in settlements or trial awards. 'Own insurance' includes all the first party insurance payments. 'Total insurance' is the sum of own insurance payment and injurer's insurance payment. In many cases, payments are zero. So 'NA' in 'Direct payment' at $\tau = 0.5$ indicates that the median of the marginal distribution of direct payment is zero, and therefore, the median effect cannot be meaningfully measured.

Table 13: Changes in Fees and Lawyer Efforts

Outcomes	All cases	Medical only
I. Lawyer's Fees:		
total fees charged	-1040.5	-33.9
	(14687.4)	(3684.0)
paid only a contingent fee	0.052	0.003
	(0.047)	(0.070)
II. Lawyer's Efforts:		
defended family	-0.023	0.058**
	(0.015)	(0.029)
helped financial planning	-0.020	0.035
	(0.016)	(0.022)
advised which MD to use	0.055	-0.050
	(0.040)	(0.061)
filed a lawsuit	-0.002	-0.040
	(0.045)	(0.065)
negotiated with other driver's insurer	0.088*	-0.058
	(0.046)	(0.070)
gave general advice	0.018	0.026
	(0.019)	(0.029)
did nothing	0.001	-0.023*
	(0.009)	(0.013)

Part I reports regression outcomes of the total fees charged by attorneys on the CS rule change dummy variable. Other control variables include covariates listed in Table 8 and state & year fixed effects. Standard errors are in parentheses. Part II shows results of lawyer's efforts variables on the CS rule change. Analysis for two groups are done separately: 'All cases' is the sample that includes cases when the CS modification is applied to all cases, while 'Medical only' is the sample that includes cases when the CS modification is applied to medical malpractice only. Since the CS rule changes did not affect auto accidents, effects in the second group (if there are any) are spurious.

Table 14: Average Indirect Effects from Further IV Regressions.

Outcomes	**Before Fees**		**FL and MN**	
	(1)	(2)	(3) Without	(4) Only
Total payment	[-70,897, -19,232]	[-59,494, -9,710]	[-48,834, -11,744]	[-55,912, -23,245]
	(-135,260, 41,403)	(-126,479, 53,685)	(-97,126, 33,358)	(-87,714, 1,680)
Direct payment	[-42,487, 9,177]	[-29,796, 19,987]	[-30,396, 6,693]	[-29,398, 3,268]
	(-102,846, 67,280)	(-92,734, 80,887)	(-74,782, 49,169)	(-52,061, 21,009)
Litigation payment	[-49,212, 2,452)	[-38,941, 10,842]	[-40,199, -3,110]	[-33,490, -823]
	(-108,660, 59,555)	(-100,985, 70,696)	(-84,197, 38,751)	(-51,932, 12,025)
Own Insurance	[-80,075, -28,410]	[-79,482, -29,698]	[-57,051, -19,962]	[-58,442, -25,774]
	(-101,924, -16,815)	(-102,160, -17,529)	(-83,581, 2,453)	(-79,003, -12,616)
Total Insurance	[-73,349, -21,684]	[-70,337, -20,553]	[-47,248, -10,158]	[-54,349, -21,682]
	(-97,216, -6,430)	(-94,877, -4,724)	(-74,027, 13,034)	(-78,197, -4,501)
Fixed effects	No	Yes	No	No

Bounds of the average effects using collisions only in 2002 constant dollars. The first and second columns use payment measures before attorney's fees. The third and fourth columns show results using subsets of states that have different rules to determine lawyer's fees. For states that applied the CS modification to 'all cases', column (3) includes those states excluding Florida and Minnesota, while column (4) only includes Florida and Minnesota. For states that applied the CS modification to 'medical cases only', both (3) and (4) include them all. The same control variables appearing in Table 8 were used in the IV regressions. The 95% confidence intervals are in parentheses.

Table 15: Average Indirect Effects from Further IV Regressions (continued).

Outcomes	**No Fault**		**Excluding ambiguous states**	
	(1) no-fault states	(2) fault states	(3)	(4)
Total payment	[-40,868, 11,606]	[-56,435, -22,320]	[-76,924, -22,867]	[-69,513, -17,086]
	(-87,747, 53,813)	(-103,314, 19,887)	(-130,942, 25,684)	(-126,438, 34,363)
Direct payment	[-17,914, 34,560]	[-35,368, -1,253]	[-46,657, 7,399]	[-37,081, 15,345]
	(-59,762, 73,721)	(-77,217, 37,908)	(-95,286, 52,901)	(-88,378, 63,702)
Litigation payment	[-31,794, 20,680)	[-44,176, -10,061]	[-52,963, 1,094]	[-45,864, 6,562]
	(-72,993, 59,068)	(-85,375, 28,326)	(-100,810, 45,698)	(-96,434, 53,968)
Own Insurance	[-88,218, -35,743]	[-53,521, -19,406]	[-84,899, -30,842]	[-89,797, -37,370]
	(-115,439, -14,587)	(-80,742, 1,749)	(-116,274, -6,371)	(-123,411, -11,142)
Total Insurance	[-74,338, -21,863]	[-44,713, -10,598]	[-78,594, -24,537]	[-81,014, -28,587]
	(-102,230, 285)	(-72,605, 11,551)	(-110,824, 1,129)	(-115,271, -1,226)
Fixed effects	No	No	No	Yes

Bounds of the average effects using collisions only. The first and second columns compares no-fault vs. fault states. The 95% confidence intervals are in parentheses. The same control variables appearing in Table 8 were used in the IV regressions. The third and fourth columns repeat the analysis in Table 11 excluding several states (Georgia, Kansas, Kentucky, Ohio, Oklahoma, Oregon and Wisconsin) whose law says "may" rather than "shall" deduct. In those states the courts can deduct collateral sources but do not have to.