The Value of an Attorney: Collateral Source Rule Changes as an Invalid Instrument*

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Abstract

The value of lawyers to their clients is notoriously difficult to estimate due to endogeneity. We utilize modifications to the collateral source (CS) rule that require reducing trial awards by the amount of payments from first-party insurance as an instrument for hiring a lawyer. The problem with our instrument is that modifications to the CS rule have a direct effect on recovery, and hence violate the exogeneity requirement for a valid instrument. We develop a new identification and estimation method that uses CS rule changes as an invalid instrument and bounds the impact of lawyers. We find that the upper and lower bounds of our estimated impact are lower than estimates that do not correct for endogeneity. Our estimates of the impact of lawyers on total payment are uniformly negative. The upper bound of the effect of hiring a lawyer on total payment received is -$26,000 after fees in our preferred specification, which suggests that even in the most optimistic scenario lawyers appear to reduce total recovery.

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1 Introduction

One of the more contentious questions in law is the value of lawyers to their clients.[1] There are reasons to believe that lawyers can reduce a client's expected recovery; most obviously lawyers could charge more in fees than the increase in payments from third-party source their services generate for their clients. Moreover, because claimants typically lack the experience with—or expertise about—the civil justice system, they are unable to judge whether they would be better off without an attorney.[2] 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.[3]

Compensation for auto accidents, like most other personal injuries, can take two broad forms. The first is that the injured party recovers from their own insurance—known as the first-party insurance. First-party insurance includes their own auto insurance, health insurance, workers' compensation, and government-provided insurance, such as Medicare and Medicaid. The second option is that the injured party recovers from a third-party's insurance because another party to the accident was in some way at fault for the victim's injury. In about a third of these cases, according to the Insurance Research Council (IRC) data used in this paper, the injured party retains an attorney to help them recover their losses from a third party.[4] The attorneys are almost always retained on a contingent fee basis, according to which they are not paid unless the client recovers. Given that the typical contingent fee is a third of the recovery, a long-standing question is whether lawyers are

[1]For an extensive discussion of the literature on civil cases, see Greiner and Pattanayak (2012) and Greiner, Pattanayak, and Hennessy (2012). There is also literature on the role of lawyers in criminal cases. The estimates of the value of lawyers in indigent defense are better identified since lawyers in these case are often randomly assigned. See, for example, Agan, Freedman, and Owens (2021), who examine moral hazard and adverse selection effects, and Roach (2014), who examines the impact of outside labor markets on attorney's performance.

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

[3]In 2018, there are more than six million car accidents in the United States. Of these accidents, almost two million involved a bodily injury and thirty-three thousand involved a fatality. See the Insurance Information Institute (https://www.iii.org/fact-statistic/facts-statistics-highway-safety).

[4]It is, of course, possible that attorneys represent clients in recovering from their own insurance as well. Our data does not seem to contain any instances in which the attorney negotiated exclusively with the victims insurance and did not attempt to recover from third-party sources.

worth the cost.[5] It is certainly possible to recover from a third-party without retaining a lawyer. Insurance companies regularly make offers to injured third parties on behalf of their policy holders. Yet the commute into any major American city is covered with billboards asking, "Have you been injured in an accident?" and advertising the services of an attorney who can help the injured party recover.[6]

Given the importance of lawyers in accident victims' recovery for personal injuries of all types, one would correctly assume that the value of lawyers in recovering from third-party sources has been extensively studied. 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 can be endogenous. Greiner and Pattanayak (2012) survey the empirical literature, which consists of hundreds of studies. They conclude that beyond the three randomized controlled trials (RCT), inclusive of their own study, the rest of the literature lacks credibility. The challenge is that most of the literature surveyed by Greiner and Pattanayak compares 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. Moreover, the randomized control trials have been used to estimate the value of an attorney but RCTs are typically used in very specific contexts.[7]

The standard alternative to a randomized control trial is an instrument that affects the decision to hire a lawyer but does not affect claim value. Our candidate instrument is changes to the collateral source (CS) rule (or collateral source doctrine). 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 re-

[5]While there are reasons to believe that contingent fee arrangements between lawyers and clients may be more efficient than hourly fees, law and economics have long recognized that under any fee arrangement the lawyer has distinct interests from the client. 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. See Rubinfeld and Scotchmer (1993) and Dana and Spier (1993).

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

[7]One limitation, acknowledged by those conducting the RCTs, is that the stakes of the case must be fairly small. Because 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, must take place in claims in which retaining a lawyer on contingent fees is not possible and in which the potential plaintiffs are liquidity constrained (i.e., eviction cases or debt collection). Thus, by construction, a lawyer's value is potentially very different in the types of cases amenable to an RCT than in other cases. 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)

sulting 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. 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 supported by empirical evidence, allowing us to conclude that our candidate instrument satisfies the relevance requirement of being a valid instrument.[8] However, one serious concern with our candidate instrument remains: modifications to the CS rule require first-party payments to be deducted from any trial awards, and hence they have a direct effect on recovery. This direct effect on recovery violates the exogeneity requirement for a valid instrument. In the terminology of the instrumental variable (IV) estimation method, our candidate instrument has a direct effect on outcome variables, making it an invalid IV.

We solve this invalid IV problem by considering a potential outcome variable that blocks the direct effect of the instrument on outcome but allows the indirect effect of the instrument through treatment. Specifically, we ask what would be the total payment if offsets under the modified CS rule impact the decision to hire a lawyer but then do not materialize in payments? By using this potential payment instead of the observed payment as our outcome measure, we define a local average indirect effect.[9] If the direct effect were blocked, the modification to the CS rule impacts payment only through the decision to hire a lawyer. This indirect effect has a causal interpretation. This potential outcome variable, however, is not directly observed, so the local average indirect effect may not be identified. Fortunately, we can link the potential outcome to observed quantities because offsets reduce payments in mechanical ways; offsets literally reduce the award at trial dollar for dollar with the payments from relevant collateral sources. By calculating the maximum offsets allowed by

[8]However, the modification of the CS rule itself may not be random. To address this concern, we use a variation in law changes. When states introduced modifications to the CS rule, some applied it to all cases, while others restricted it to medical malpractice. The scope of the modification appears to be random and can be used as IV because the law change in the latter has no impact on auto accidents. See Section 2.2 for further discussion.

[9]A similar approach has been used in the treatment effect literature (mainly outside of economics) 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. However, our identification strategy and resulting bound estimator are substantially different from the prior works.

the modification enacted in the relevant state, we are able to bound the potential payment.[10] In this way, we can identify a sharp bound on the local average indirect effect.

Consistent with previous research, we find that lawyers increase the amount recovered from third-party sources by $10,000 to $21,000 when the model is estimated using the ordinary least squares estimator, depending on samples and model specifications. If the results are to be believed, hiring an attorney substantially increases overall recovery by those injured in auto accidents. We then estimate an upper and lower bound on the causal impact of hiring an attorney using our invalid instrument. Consistent with Greiner and Pattanayak (2012)—and in contrast to earlier evaluations of auto accidents—we find that the upper and lower bounds of our estimated impact are lower than our naive estimates. Moreover, our estimates of the impact of lawyers on total payment are uniformly negative. The upper bound of the effect of hiring a lawyer on total payment received is -$26,000 in our preferred specification.

The local average indirect effect identifies the effect of hiring lawyers on claims in which the decision to retain lawyers is affected by the CS rule status.[11] Our bound estimate can be viewed as evidence against the positive naive estimate if the treatment effect is homogeneous throughout the population. However, if claims affected by the CS rule change, the so-called compliers, are different from the rest of the population, a direct comparison between the IV and the least-squares estimates is potentially misleading. The theoretical model in Section A.2 predicts that claims affected by the CS rule are the group with the lowest claim values. If this is the case, the negative bound estimate suggests that the marginal group of accident victims overuse lawyers. Since they have the least valuable cases, they are less likely to benefit from lawyers and resulting court actions.[12]

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 by

[10]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 although factors such as contributory negligence by the victim might result in an actual recovery lower than this maximum even without offsets.

[11]Observations are called compliers in the literature if their decisions to take treatment are impacted by changes in instrument. We will use this terminology throughout.

[12]For a similar result in a different context, see Chandra and Staiger (2020), who found evidence of treatment overuse in the healthcare industry. Specifically, Chandra and Staiger (2020) documented that aggressive hospitals are more likely to treat patients with negative treatment effects. It suggests the overuse of treatment in these hospitals.

exerting less effort on the client's behalf. Given that modifications to the CS rule in all but two states base the lawyer's contingent fee on pre-deduction recovery, the lawyer's payments for a given case should not change, and hence effort should not be affected. Consistent with the expectation, we do not find any evidence of significant changes in total fees after a CS rule change. Further, we also find no post-CS rule modification changes in several variables reflecting the lawyers' effort level in the case. We also exploit variations in state-level rules that determine lawyer's contingent fees. We repeat the analysis under different scenarios and obtain consistent results. The results also remain consistent using different samples and alternative constructions of our IV. We further find evidence that the effect of hiring a lawyer is heterogeneous across institutional arrangements. Overall, the effect of hiring a lawyer on third-party and total recovery is likely to be negative.

The negative impact of lawyers at the margin on total recovery is partly explained by the fact that the first-party payments are lower for victims who hired lawyers, which could be explained by either selection or substitution. The selection channel refers to the possibility that claimants who felt their insurance companies underpaid them for their claim can hire a lawyer to pursue the other party to the accident's insurance. The substitution channel, by contrast, exists because the first-party insurance is a secondary payer. Most first-party insurers, such as health insurance, reimburse clients only for expenses not covered by another source. Thus, claimants who hired lawyers and had the other party's insurance cover their medical expenses may not be paid again from the first-party insurance once they recover. The contingent fee arrangements may also lead to substitution between payment sources. Lawyers in our sample are not paid for recovery from first-party sources and hence may advise their client not to claim damages from first-party sources so as to increase the possible recovery from third-party sources. While untangling these two explanations would be intriguing, our data only contain the final payment from different sources, not the timing or sequence of payments.

The lawyer's negative effect is also partially driven by the fact that third-party payments are lower when victims hire lawyers independent of first-party sources. Because the main role of the lawyer is to negotiate with another party to the accident, this negative effect on the third-party insurance is more substantive than similar finding on the first-party insurance. Because this effect cannot be due to selection, we conjecture that the negative impact of lawyers on recovery from third-party sources is driven by third-party insurers to making offers to victims to reduce the likelihood that they hire a lawyer in the first place. As such, individuals who do hire a lawyer, either because they expect to recover more than

the third-party insurer thinks the case is worth, or simply by mistake, can receive less compensation after attorneys' fee. If correct, this means that hiring a lawyer would be a public good. Absent someone hiring a lawyer, third-party insurers would not make offers to avoid litigation. Yet for any individual who hires a lawyer, the expected recovery is lower. Essentially individuals who hire lawyers are paying to make the third-party insurers' expected loss creditable.

This paper contributes to the invalid IV literature that studies the properties of the instrumental variables methods without assuming the exogeneity condition. Nevo and Rosen (2012) study the implications for identification 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, and 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. Our proposed 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 then to use it to obtain an upper and lower bounds of the potential outcome. This procedure provides a sharp bound for the treatment effect and can be useful in other contexts.

The rest of the paper is organized as follows. Section 2 provides additional background on compensation in auto accidents and the CS rule. It then presents a simple model of the decision to hire an attorney and analyzes the impact of the CS rule modification on the decision. Section 3 describes the data; Section 4 details our estimation method and explains how we utilize the CS rule modifications as an invalid IV. Section 5 discusses the main findings of the paper, and Section 6 explores several threats to the valid inference and effects heterogeneity. Section 7 discusses potential causes and the implications of our findings and then offers a conclusion.

2 Background

2.1 Compensation in Auto Accidents

Accident victims can generally seek compensation from two sources: first- and third-party insurance. First-party insurance compensates an accident victim based solely on the existence of an injury. Thus, someone with health insurance who is involved in an auto accident would receive compensation for the costs of their medical care regardless of whether they were at fault. Other first-party sources of insurance such as medical coverage through the victim's auto insurer, Medicare and Medicaid, workers' compensation, and VA benefits are similar. Third-party insurance, by contrast, compensates victims of the policy holder's negligent actions.

In the data used in this study, about 40% of the accident victims receives payments from their health or auto insurance, with 3% receiving first-party insurance payments from other types of insurers. About 33% receives payment from third-party sources, of which 51% is from litigation broadly defined. Of the 17% of the sample that receives payments from litigation about 68% filed a case. In the other cases, the lawyer negotiated with the third-party insurance, typically the auto insurance of the other party to the accident. Roughly, the breakdown of compensation is that first-party insurance sources cover around 50% of victim's accident costs, 33% of cases receives compensation from the third-party source; and in about half of the cases, the third-party insurer makes an offer to the victim that closes the claim without a lawyer representing the victim.

2.2 Modifications to Collateral Source Rule

The collateral source rule in common law prohibits juries or judges from considering payments made to the plaintiff when calculating the damages owed by the defendant. Thus, under the CS rule, the fact that a plaintiff had received first-party insurance payments to compensate them for their injuries could not be introduced as evidence in a civil lawsuit. In practice, if the plaintiff's injuries were covered by medical insurance, they could receive compensation for the same injury from both the defendant and their medical insurance.[13] Beginning in the 1970s, a number of states modified the CS rule and required that any payments from

[13]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).

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—as noted above—is that while it does reduce the likelihood of an accident victim hiring 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 which occurred from 1982 to 2002, also contain information on compensation from other sources, such as health insurance, allowing us to link the specific offset required under the state's modification of the CS rule to whether the individual received compensation for a source covered by the offset (i.e., are offsets required for payments from Medicare, and did the individual receive compensation for the accident from Medicare?).

One concern is that a state's decision to modify the CS rule 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 are sufficiently rich in detail on the history of law changes that they give us an additional source of random variation. When states introduced modifications to the CS rule, some legislatures applied it to all cases, while other states restricted it to cases involving allegations of medical malpractice. Because we study only auto accidents, the law change in the latter should have no impact on our cases. Sloan and Chepke (2008) suggest that the 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. A subset of these reform efforts 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 that the decision to apply it more broadly or narrowly is random.

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. The data on the modifications to the CS rule 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 apply to medical malpractice cases and "All" means that they apply 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" has both starting and ending dates.[14] The table also lists what sources are exempted in the modifications.

2.3 The CS Rule and the Decision to Hire an Attorney

This section presents a 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 reduces the likelihood of a claimant retaining an attorney, thus demonstrating the relevance of the CS rule modification as an instrument. Empirical evidence also supports the prediction of the model.

Suppose that a claimant is legally entitled to an amount M in compensation from the defendant, assuming the defendant is either found liable or willing to settle (in order to avoid a trial).[15] Suppose that the claimant will receive M from the (potential) defendant or his insurer, with probability P_0 if no lawyer is hired. If the claimant hires an attorney, they face a probability P_1 of recovering M. We assume that $P_1 > P_0$. We also assume that the attorney, if they agree to take the case, faces a cost of C to pursue the claim —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.[16] 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. Consider first the case in which this amount is irrelevant because the amount owed by a liable defendant is not offset 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 client receives $P_1(1-\beta)M$

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

[15]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.

[16]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 Section A.1 in the supplemental appendix and show that cost sharing does not change the conclusion.

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 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 is 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 the claimant, it does not reduce demand for attorneys because the decision rule remains the same.[17] 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.

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

Using the data discussed in the next section we evaluate the model's predictions. Table 2 presents the probability of an accident victim hiring an attorney.[18] 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 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

[17] 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.

[18] We use the "collisions-only" sample in Table 3. The overall results we present are substantively identical if we use other samples.

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 -5.01 (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. Other differences are not statistically significant, which is what we predict from our theoretical model.

3 Data

3.1 Survey of Auto Injury Claims

The data for this study come from the Insurance Research Council (IRC)'s Consumer Panel Study of Auto Injury Claims. The IRC consumer panel study contains a 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 cover accidents from 1982 to 2002 in all fifty states and the District of Columbia. Tables A.1 and A.2 in the supplemental appendix show the distribution of accidents by year and by state, respectively. Because the data survey accident victims, they offer 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.[19]

Table 3 shows summary statistics of the samples considered in the paper. The IRC consumer survey has 18,451 cases. The IRC Consumer Panel asked what the lawyer did for the client so we are confident that, for our final sample of cases, if a lawyer is hired, then the lawyer in question is attempting to recover from third-party sources. We make several sample restrictions to attempt to remove cases in which a lawyer might not be attempting to recover from third-party sources; because, our measure might be biased toward zero in

[19]Under a first-party insurance contract, the injured party is paid by their 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.

those cases.[20] For this reason, we exclude 931 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 performed any other service identified in the survey. We conjecture that, in these cases, the respondent is mistaken either that the lawyer took the case or that 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 3 display summary statistics of this sample under the heading "Whole Sample."

Fifteen states did not change their CS laws until the end of our sample period. Out of the remaining thirty-six states that changed their CS laws, nineteen chose to apply it to all cases, while seventeen states applied it only to medical malpractice cases. As Section 2.2 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 3, and it includes 13,260 cases.

The majority of accidents are collisions with another moving vehicle, constituting about 81% 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. We restrict our analysis to accidents involving at least two vehicle collisions, as they are the cleanest test of the value of a lawyer. Single vehicle accidents rarely involve liability for a third-party, and hence lawyers are unlikely to be particularly valuable in these cases.[21] 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 the 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 pedestrian accidents. The "Collisions Only" sample in the last set of columns in Table 2 has 10,729 collision cases.

The treatment variable is the decision to hire a lawyer. Overall, about 32% of claimants

[20]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, which might make lawyers look less valuable because we are incorrectly measuring the service they provide.

[21]In most single vehicle accidents, victims in our sample do not retain attorneys nor do they receive payments from third parties. However, in a small subset of single vehicle accidents, liability payments are quite large. Our theory is that these are products liability cases in which the victim sues the manufacturer of the car for a design defect that led to the accident. Because our data do not identify which victims are suing the other driver and which are suing the vehicle's manufacturer, we exclude single vehicle accidents from the analysis.

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 in the analysis to proxy both for potential differences in bargaining strategy across age and gender but also as a control for the potential scope of damage.[22] We also include data on the number of injured parties in the accident in order to capture the scope of the accident. The data 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) damages including medical bills.

We also include whether the state has no fault insurance laws and/or a mandatory insurance policy law. A number of states, starting in the 1970s, passed no fault insurance laws (see Anderson et al. 2010). The literature on the impact of no fault insurance on recovery is vast, 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 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 which 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 to medical malpractice.[23] 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[24], and, finally, an indicator if the state requires

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

[23]During our sample period, thirty-one states have caps on non-economic damages for medical malpractice cases while only thirteen states have non-economic damage caps that apply to auto accidents (see Seabury and Lakdawalla [2012]).

[24]Joint and several liability is a common law doctrine under which a defendant can be held liable for the

structured settlements rather than lump sum payments for particularly large settlements.

We construct three measures of the claimant's total payment received from third-party sources as a result of their injury. Our first measure captures 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 an attorney's 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 third-party payment measure consists of payments from litigation—either judgments or settlements—and payments from the injurer's insurance that did not result from litigation[25]

The broadest measure is the 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.

As noted above, 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 may, at least indirectly, impact payments from first-party insurance over the course of the patient's long term treatment in claims that involve litigation. It is also possible, that lawyers directly influence first-party compensation in order to increase third-party compensation.

As mentioned, the data also contain information on whether 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 A.3 from the supplemental appendix, which breaks the sample down by whether the victim hired a

full amount of the plaintiff's damages, regardless of their personal degree of fault, if the other defendants in the case are bankrupt (See Currie and MacLeod (2008)).

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

lawyer, talked to a lawyer or did not talk to a lawyer, and 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.[26] This is not surprising as attorney are paid on a contingent fee in these cases, and hence are more likely to take larger cases. Because both victims and lawyers are selective, however, simply regressing the presence of an attorney on compensation will be biased. Table A.4 in the supplemental appendix breaks the collision sample down by whether the state modified the CS rule before 2002 and the scope of those modifications.

4 Research Design

4.1 Identification and Estimation with Invalid IV

The outcome variable Y is the payment a claimant receives, and the treatment variable D is their decision to hire an attorney. The instrument $Z = 1$ indicates that the claimant's accident occurred in a state and a year in which a modified CS rule requiring offsets was in effect, and $Z = 0$ indicates that they can recover from all 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 that occurred in states that did not modify the CS rule by 2002 are excluded. What differentiates two groups in terms of Z is whether the state applied the modification to all insurance claims or only to medical malpractice claims: $Z = 1$ means the claim was in a state that applied the modification to all insurance claims; and $Z = 0$ indicates the claim was in a state that applied the CS modification only to medical malpractice claims. The instrument is not modification itself but its scope of coverage.

As the collateral source rule can affect the decision to hire an attorney, combine D and Z and define potential treatment variables $D(Z)$; $D(1)$ stands for the claimant's decision to hire an attorney when $Z = 1$ and $D(0)$ is their decision when $Z = 0$. Thus, if $D(1) = 0$ and $D(0) = 1$, the claimant would hire an attorney only when $Z = 0$.

The value of outcome depends on the treatment and instrument, so we define the potential outcome variable $Y(Z, D)$. Depending on Z and D, one has four potential outcome variables; $Y(1, 1)$ is payments the claimant would receive if they decided to hire an attorney while their

[26] This profile may be because such cases are inherently more complex. Shavell (2009) argues that the more complex the case, the more likely an individual is to pursue litigation.

case was in a modified state; $Y_i(1,0)$ is the payment if they did not hire an attorney in a modified state. $Y(0,1)$ and $Y(0,0)$ are defined similarly if the case was in a non-modified state. The observables and potential variables are related by $D = ZD(1) + (1-Z)D(0)$ and $Y = DY(Z,1) + (1-D)Y(Z,0)$.

4.2 Average Indirect Effect

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

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

Given D, the value of Z is irrelevant to outcomes. That is, the only possible effect of Z on Y is through its effect on D. Except for this indirect channel, the exclusion restriction (1) says that there is no other channel through which the instrument can affect the outcome. If so, potential outcomes can be simplified to $Y(D)$.

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, as these are constructed in expectation of the award at trial. The instrument has both direct and indirect effects, and the direct effect violates the exclusion restriction; hence, the term invalid instrument. We solve this invalid IV problem by combining two facts inherent in the CS rule modification; (i) it is natural to introduce a potential outcome variable that blocks the direct effect of the instrument on outcome, and (ii) this potential outcome variable can be linked to observed quantities, and therefore its effect can be learned from data.

Consider a potential outcome $Y(0, D(1))$. It is payments a claimant would receive if the value of the instrument was allowed to affect their decision to hire an attorney (through $D(1)$) but not allowed to have a direct effect as the value of the first argument is fixed at 0. The direct effect of Z on Y is suppressed by fixing the first argument. Consider two scenarios. If one could assume the exclusion restriction, the effect of Z on Y for an individual i is

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

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

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

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 because 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 part (i) means 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 part (ii) requires the existence of a randomly assigned instrument but also has a larger meaning. A random assignment of Z only guarantees that $(D(1), D(0))$ is independent of Z but 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 the instrument.

To illustrate, consider a linear latent index model

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

$$D_i = I(D_i^* \geq 0) \quad \text{where} \quad D_i^* = \alpha_0 + \alpha_1 Z_i + v_i, \tag{3}$$

where $I(A)$ is an indicator function taking 1 if an event A is true and 0 if A is false. The results of this section do not depend on the linear functional form and/or the effect being constant. We use this simplified model for expositional convenience.

In this model, D^* is a continuous latent variable and can be interpreted as the net utility of hiring an attorney, and D is the observed decision variable. The causal effect of hiring an attorney is captured by β_1. Assumption 1(i) means that $\alpha_1 \neq 0$. 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 a direct effect as well, a violation of the exclusion restriction (1) and the cause of the invalid IV. The potential variable $Y(0, D(1))$ turns off this direct effect by requiring that $Z = 0$ in the outcome equation (2), while allowing

$Z = 1$ in the selection equation (3). If this potential variable were observed, the direct effect would not be an issue, and Z would be a valid IV.[27]

As such, what Assumption 1(ii) requires is that $E[Z\varepsilon] = 0$ and $E[Zv] = 0$. The no correlation condition summarizes the idea that the instrument Z can affect Y only through two channels: direct effect through β_2 and indirect effect through D. Because ε and v can be correlated, the zero correlation between Z and v guarantees that there is no other indirect channel through the selection equation and the zero correlation between Z and ε guarantees that there is no other direct channel besides β_2. In summary, Assumption 1(ii) allows only 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 the potential outcome $Y(0, D(1))$ takes care of the direct effect, only the indirect effect remains.

This assumption can be violated if there is a second indirect or direct channel through which our 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 clients, essentially increasing the fees rather than simply refusing more cases. These changes could potentially impact clients' recovery.

For the lawyer effort channel, we are essentially assuming that our instrument affects the quantity (likelihood) of a lawyer's service but not its quality. Section 6 examines several variables that measure a 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 the lawyer's effort levels. For the fee structure channel, we find no evidence that the CS rule changes affected the amount lawyers changed on their clients. Section 6 has more information on these channels.

The next assumption is the individual level monotonicity.

Assumption 2 (Monotonicity)

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

[27]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.

This is not a testable condition. In our case, however, the simple supply-demand analysis in Section 2.3 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" are those whose decision is affected by the law change in the sense that they would hire only when they were under a non-modified state ($D_i(1) = 0, D_i(0) = 1$); and "always-takers," a group that always hires 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 that would hire attorneys only when they are in a modified state ($D_i(1) = 1, D_i(0) = 0$).

We can identify the effect on compliers, as the proposition below makes clear.

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

$$E\left[Y(0,1) - Y(0,0) | D(0) - D(1) = 1\right] = \frac{E[Y(0, D(0))|Z = 0] - E[Y(0, D(1))|Z = 1]}{E[D|Z = 0] - E[D|Z = 1]}. \quad (4)$$

Proof can be found in Section A.3 of the supplemental appendix.[28] Flores and Flores-Lagunes (2013) obtained a similar result. However, as is clear in Proposition 4.2, the bound we derive from the expression (4), and assumptions we use are quite different from theirs.

To understand the expression (4), compare it to the usual LATE under the exclusion restriction (1) (Imbens and Angrist (1994)):

$$\begin{aligned} E[Y(1) - Y(0)|D(0) - D(1) = 1] &= \frac{E[Y|Z = 0] - E[Y|Z = 1]}{E[D|Z = 0] - E[D|Z = 1]} \\ &= \frac{E[Y(0, D(0))|Z = 0] - E[Y(1, D(1))|Z = 1]}{E[D|Z = 0] - E[D|Z = 1]}. \end{aligned} \quad (5)$$

The difference is that (4) uses the indirect effect (IE) but (5) uses the total effect (TE).[29] The average indirect effect (4) is what we can estimate even if the exclusion restriction is not guaranteed.

[28] The right-hand side in (4) uses $D(0) - D(1)$ instead of the more conventional form $D(1) - D(0)$ because compliers in our setup take treatment when $Z = 0$; so, for them $D(0)$ means treatment while $D(1)$ implies non-treatment.

[29] By the independence assumption, the numerator from the right side of (4) is $E[Y(0, D(0)) - Y(0, D(1))] = -E[\text{IE}]$. By the same reasoning, the numerator for (5) is equal to $-E[\text{TE}]$.

One outstanding issue is that $Y_i(0, D_i(1))$ is not observed. It is necessary to link this potential outcome to some observed quantities. For this purpose, let W_i be the maximum that can be deducted from a 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 that what types of insurance payments claimants have received (or will receive) should be deducted from settlements or awards. In our sample, of the 23 states that have modified the collateral 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.[30] For more information on state specific rules, see Table 1. The maximum offsets can include all sorts of payments from all first-party insurance sources available to the victim. As noted above, this is first-party insurance and may include payments from disability insurance, health insurance including Medicare, Medicaid, and workers' compensation plans. Depending on what a state's CS rule modification specifies, the actual offsets can differ from this maximum amount.

The amount W depends on Z, so we may write it as $W(Z)$. Note that $W(0) = 0$ simply because of how the offsets work. What is non-trivial is $W(1)$. Hence, we keep the simpler notation W but intend to use it for $W(Z)$. Between the unobserved quantity, $Y(0, D(1))$, and observed quantities, $Y(1, D(1))$ and W, 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 modification to the collateral source rule works. The first inequality, $Y(1, D(1)) \leq Y(0, D(1))$, 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 the modification of the CS rule (captured by $Y(1, d)$). So, the former must be always greater than or equal to the latter. The second inequality $Y(0, D(1)) \leq Y(1, D(1)) + W$ simply says that the maximum that can be deducted from awards or settlements is W.

With Assumption 3, one can obtain an interval identification result.

[30] 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)

Proposition 4.2 *Let Assumptions 1, 2, 3 hold. Then, a sharp bound for the local average indirect effect in (4) is given by* $[LAIE^L, LAIE^U]$ *where*

$$LAIE^U = \frac{E[Y|Z=0] - E[Y|Z=1]}{E[D|Z=0] - E[D|Z=1]} \quad \text{and} \quad LAIE^L = \frac{E[Y|Z=0] - E[Y+W|Z=1]}{E[D|Z=0] - E[D|Z=1]}.$$

Proof can be found in Section A.3 of the supplemental appendix. This bound can be uniquely determined from data.

4.3 Value of Case

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 timing of the state law change, the nature of the offsets mandated by the law change, and the sources of compensation received by the victim outside of the civil justice system. Our second instrument takes advantage of the fact that states differed in what sources of compensation required an offset. For example, when the Illinois legislature modified the state's CS rule, legislators decided 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 that 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 second 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 contain the victim's estimate of the total damage from the accident in terms of property loss, medical expenses, and lost wages. Constructed from this information, the value of the case (VC) is a continuous measure that takes into account what has already been compensated by first-party sources:

$$\text{VC}^0_{ijt} = 1 - \frac{A_{ijt} \cdot Z_{jt}}{B_{ijt}}.$$

Here, A_{ijt} denotes "offset compensation" for the claim i in state j and year t, and B_{ijt} and Z_{jt} denote damages and the CS rule modification indicator, respectively. The "offset compensation" is defined as the first-party insurance payments minus "non-offset payments," where the latter reflects the state-specific modifications to the CS rule. This value of the case captures the proportion of the damages not subject to offsets. The offset compensation is the total payment that would be deducted, if $Z_{jt} = 1$. When $Z_{jt} = 0$, the offset compensation is, of course, zero. For example, if an accident victim suffered \$50,000 in damages of which \$25,000 was paid for by their health insurance but \$25,000 in lost wages was uncompensated, the VC would be 1 without a modification to the CS rule and 0.5 with a modified CS rule, as state modifications deduct payments from private health insurance. Suppose, however, the victim lived in Illinois and received \$25,000 in compensation from the state's workers' compensation program. The VC would now be 1 because workers' compensation payments are not deducted from awards at trial under Illinois law. More generally, 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. In this sense, VC is designed to capture the degree to which the CS modification would actually reduce any payments in the civil justice system.

One potential issue of the VC variable is that its numerator, payments from various sources of the first-party insurance, can be correlated with the decision to retain a lawyer. As discussed above, lawyers might advise a client not to submit damages to first-party insurers to increase the proportion of damages that would then be compensated by third parties. To alleviate this concern, we use a predictive regression. Using claims characteristics, we estimate models for each component of the offset compensation and replace observed payments with predicted payments. For this purpose, let $A_{ijt} = A^1_{ijt} - A^2_{ijt}$ where A^1_{ijt} denotes the first-party payment and A^2_{ijt} denote the non-offset payment. Consider a linear model $A^l_{ijt} = X'_{ijt}\delta^l + \alpha^l_j + \lambda^l_t + u^l_{ijt}$ for $l = 1, 2$, where X_{ijt} is a vector of covariates and α^l_j and λ^l_t are state and year fixed effects, respectively. The covariates includes medical cost, amount of lost wages and other cost, types of accidents, age and gender of claimants, whether the other party involved has no insurance and state laws. We estimate the model, obtain the predicted value $\widehat{A}^l_{ijt} = X'_{ijt}\hat{\delta}^l + \hat{\alpha}^l_j + \hat{\lambda}^l_t$, for $l = 1, 2$, and replace A^l_{ijt} with $\tilde{A}^l_{ijt} = \max(\widehat{A}^l_{ijt}, 0)$. The max operator in $\tilde{A}^l_{ijt}$ guarantees that the predicted payment will be non-negative. Finally, in the

definition of the value of case, substitute $\tilde{A}_{ijt} = \max(\tilde{A}^1_{ijt} - \tilde{A}^2_{ijt}, 0)$ for A_{ijt}:

$$\text{VC}_{ijt} = 1 - \left(\tilde{A}_{ijt} \cdot Z_{it}\right) / B_{ijt}.$$

We use this estimated VC variable as a second instrument for the analysis.

5 Results

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

$$Y_{ijt} = \beta_0 + \beta_1 D_{ijt} + X'_{ijt}\gamma + \alpha_j + \lambda_t + \varepsilon_{ijt} \tag{6}$$

where Y_{ijt} is an outcome measure for the claim i in state j and year t, D_{ijt} is a binary treatment, X_{ijt} is a vector of control variables, and α_j and λ_t denote state and year fixed effects, respectively. Unlike the setup in Section 4, we include covariates in the regression models to capture the observed differences across cases. The inclusion of covariates also allows us to refine the identification assumption by making the independence assumption, Assumption 1(ii), conditional on covariates, which is clearly a more plausible assumption in our context.

5.1 The Naive Regressions

The typical approach to determining the value of a lawyer is to estimate the model (6) using ordinary least squares. We call it a naive regression. Table 4 shows results. The main sample of interest is the collisions-only sample that excludes single vehicle and pedestrian accidents and states that did not modified the CS rule. But we also report results for two broader samples—the whole samples and the CS rule modified samples—to show that results are not sensitive to the inclusion of non-collision cases or non-modified states. Total payment and the third-party payment are net of attorney's fees. For the first-party payment, fees are not deducted from payments because there is little evidence that lawyers in our sample helped their clients to recover expenses from their own insurances. The list of the control variables used in the analysis can be found at the bottom of Table 4.

Without including control variables, the impact of hiring an attorney on total payment is \$23,855 for collisions in modified states. That number is \$26,667 when all states are

included and $29,776 when all accidents in modified states are used. In all samples, the value of a lawyer falls dramatically when control variables are included (to $11,485, $15,120 and $16,244, respectively). When state and year fixed effects are added, the estimated coefficients of hiring a lawyer are smaller but the impact is trivial compared to the inclusion of controls. This outcome strongly suggest that lawyers are not randomly assigned to claims, and at a minimum observed claim characteristics are highly correlated with the decision to hire a lawyer. This finding should generate some concern about the naive regression approach because if unobservable characteristics are also correlated with the decision to hire a lawyer, we have a classic endogeneity problem.

Column three and four break the impact of hiring a lawyer down by payments from third-party sources and first-party sources. In the case of third-party payments, the coefficients are over twice as large as the impact on first-party payments even without control variables. Although the estimated impact of lawyers on third-party payments shrinks when we include controls and fixed effects, the impact remains statistically significant at the 1% level in all samples. By contrast, the impact of lawyers on first-party payments is not significant after including control variables, although in both cases the point estimate is very near zero.

There are two takeaways from Table 4. The first is that for total payments and payments from third-party sources, lawyers appear to generate considerable value—a conclusion reached by almost all of the previous literature on the topic. This conclusion assumes that the presence of a lawyer in a case is random beyond the factors controlled for in the regression. The second takeaway is that given the dramatic decrease in the estimated value of a lawyer when controls are included, there is considerable selection on observables. Given that unobservables are also likely correlated with the decision to hire a lawyer, this potential endogeneity can be an issue.

5.2 First-Stage Estimates

Table 5 presents the first-stage regression using instruments on the decision to hire a lawyer and the F statistic. Columns (1) and (2) present the impact of changes in the CS rule on the probability of hiring a lawyer. In this case, the CS rule is simply a dichotomous variable equal to one in state-years in which the CS rule had been modified. Column (1) presents the results with control variables but without state fixed effects. The results indicate that modifying the CS rule reduces the probability of hiring a lawyer by 3.7 percentage points. The F statistics is 9.981, just below 10, the rule of thumb for a weak instrument. Column

2 provides the estimates including state and year fixed effects. The impact falls to 2.3 percentage points and is no longer significant, suggesting that there is not enough within state variation in the dichotomous law change variable to provide a strong instrument.

In columns (3) and (4), we present the results of our value of the case VC instrument. VC is the proportion of the damages not subject to offsets. We find that when none of the damages is subject to offsets (i.e., the victim can theoretically recover their full damages in litigation), the probability of hiring a lawyer is 7.7 percentage points higher than when all of the damages are subject to offsets. The impact is close when we include state fixed effects and the F statistics are 40.486 and 34.483 respectively, far in excess of the weak instrument threshold. In columns (5) and (6) we include the dichotomous CS rule change and the VC measure. The impact of the value of the case remains significant, and the joint F test is highly significant.[31] In the next section, we use the joint instruments to estimate the bounds discussed.

5.3 Attorney Values from IV Regressions

The results in Table 4 strongly suggest the endogeneity of hiring a lawyer. In Table 6, we present the results of our alternative approach to estimate the model (6) for each of our three payment: total payments, third-party payments, and first-party payments. All estimates contain the same set of control variables as in naive regression. We estimate the bounds of the local average effects of hiring attorneys using two stage least squares regressions on the collisions-only sample. The 95% confidence intervals are in parentheses.

The bounds represent the best and worst case scenario for the value of lawyers. The upper bound in column (1) is estimated by the two stage least squares estimate, with Y as the dependent variable, and D and X as endogenous and exogenous independent variables, respectively, and (Z, X) as instruments. With fixed effects in column (2), dummy variables for state and year fixed effects were added to the set of exogenous independent variables and to the instruments. The point estimate for the upper bound is given by the two stage least squares estimate $\hat{\beta}_1$ in equation (6). For the lower bound, recalling that $W = 0$ when $Z = 0$, the numerator for the lower bound AIE^L is equal to $E[Y + W|Z = 0] - E[Y + W|Z = 1]$, while the denominator remains the same. This outcome means that the lower bound can be estimated by the two stage least squares estimate, with $Y + W$ as the dependent variable.

[31] Because there are now two instruments, the F statistic of 10 is no longer a good rule of thumb for weak instruments. See Cragg and Donald (1993).

Focusing on total payments (after fees), our estimate of an attorney's local average indirect effect is between -$106,361 and -$46,369 without fixed effects and between -$66,429 and -$26,207 with fixed effects. In contrast to the naive regressions, the bounds consistently show a negative effect. Using IV, the sign of the estimated effect reverses. If the population is homogeneous, this finding can be viewed as evidence that the uniformly positive results in Table 4 are partially driven by unobservable effects biasing the effect of lawyers on recovery upward. If compliers are quite different from the rest of the population, a direct comparison of the IV and the least-squares estimates can be misleading. The former identifies the effect for the complier while the latter concerns the whole population. One cannot identify who compliers are, so whether the population is homogeneous cannot be answered empirically. However, theoretical prediction in Section A.2 suggests that compliers (whose treatment decision is affected by the CS modification status) are the marginal group with the smallest case values (among the cases that would retain lawyers if the CS rule is not modified). Because accident victims with low case values are less likely to benefit from hiring lawyers and the resulting court actions, the negative estimates suggest those marginal cases overuse lawyers.

The bounds are not estimated precisely enough to conclude that lawyers have significantly negative effects. Although the bounds are consistently negative, the 95% confidence interval does contain zero. The upper end of our confidence interval includes zero lawyer effect, meaning that in the best-case scenario we cannot reject the null that the lawyer effect can be zero. However, for example, if we are willing to assume the worse-case scenario (the lower bound), the impact of lawyers would be both negative and significant at the 5% level.

We then decompose the impact of attorneys on first- versus third-party payments. For third-party payments (those from third-party insurance sources or litigation), the bounds are -$87,234 to -$27,242 and- $50,319 to -$10,097 without and with fixed effects, respectively. These negative estimates of a lawyer's effect on the third-party payment are in contrast with those from the naive regression. In the case of first-party payments, the impact is negative but not significant, although the point estimate is now further from zero. Because there are no offsets involved in first-party payments, we can obtain a point estimate for the impact of lawyers on first-party payments without needing to resort to bounds. Unlike naive regression results, even after including control variables the point estimate is consistently negative.

The overall picture is that the impact of lawyers seems to be negative, or, at best, ambiguous (Table 6) for the marginal cases with low case values. And the strongly positive estimates from the naive analysis (Table 4) are not credible. The bounds estimates are

consistently informative in that the upper and lower bound are both negative. This tendency is highlighted in Figure 1, where we compare point estimates from the naive regressions and interval estimates from the IV regressions. We show all three outcome measures and specifications with and without state and year fixed effects. The figure clearly shows that the naive regressions tend to overestimate the value of attorneys.

In the next section, we turn to a series of alternative specifications and sample restrictions to see if the negative bounds are robust.

6 Robustness Checks

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 the lawyer's effort or caused them to change their fee structure, perhaps shifting to hourly fees or lump sum payments. This outcome seems unlikely, as modifications to the CS rule outside of Florida and Minnesota stipulate that the lawyer 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 a lawyer's value in a claim might be tied to particular institutional arrangements.

6.1 Changes in Fees and Attorney Efforts Levels

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 addressed in Table 7. In the first two rows of Table 7, we estimate a regression with the total payment to the lawyer as the dependent variable. The regression includes only cases in which the victim hired a lawyer and includes the same control variables 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 but insignificant decrease after the CS rule changes. 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 should have no impact on auto accidents claims because the law does not apply to these cases. So, we should find a null result for this group. 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.

A second possibility is that lawyers changed the nature of their compensation in states that modified the CS rule. Most lawyers in the sample are paid on a contingent fee basis, which is a percentage of any eventual recovery but nothing if the client does not recover from third-party sources. The concern is that if lawyers switch from contingent fees, which theoretically induce more effort on the part of lawyers, to fixed fees, we might see a change in attorneys' effort levels in states that modified their CS rules. In the next two rows in Table 7, we examine the probability that a lawyer retained was paid only via 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.

The other assumption required to retain the validity of an instrument even after our offsets correction is that the CS rule modification affected the quantity of lawyer services but not their 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, a lawyer's efforts in a case are not directly observable. Instead, we construct seven proxy variables that reflect a dimension of the lawyer's effort level. The second part of Table 7 examines proxies for attorney effort. The first six variables measure whether 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 II of Table 7 shows the regression outcomes of each effort variable on the dummy variable for the CS rule change. When we use the "All Cases" sample, we find no evidence that any of those effort measures were meaningfully different before and after the rule changes.

This finding is consistent with the prediction of the model in Appendix A.2. The CS rule modification reduces the number of cases retaining lawyers, but the average quality of those cases improves because it is the marginal cases that are dropped. Thus, the surplus to lawyers—and therefore their effort level—can go up or down depending on how their fees are calculated. See Appendix A.2 for details.

6.2 Without Florida and Minnesota

We also use state-specific variations in collateral source rules to see any potential impacts from changes in a lawyer's effort. Most states allow attorneys to receive contingent payments based on the total recovered before any CS offsets, so in these states the value of the claim to the attorney is unchanged even when the CS rule is modified. Important exceptions are Florida and Minnesota, whose state laws require that attorneys base their payment on the total recovered after offsets. Outside of Florida and Minnesota, a lawyer may have no reason to change their effort levels in response to the CS rule change because it did not affect how much compensation they received for a given case.

The first two columns of Table 8 estimate the bounds using all CS modifying states except Florida and Minnesota. In columns (1) and (2) of Table 8, we find similar results to those in Table 6, which include Florida and Minnesota, suggesting that the differences in fee calculation methods under the CS rule are not driving our results.

6.3 Heterogeneity: No-Fault States

Conceptually, no-fault insurance is a contract in which each person 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 plaintiffs' lawyers groups opposed state no-fault laws, suggesting that they expected these laws to reduce the need for attorneys. However, by the 1990s, Anderson et al. (2010) report, while the use of attorneys was constant for first-party insurance claims, the use of attorneys for third-party claims in no-fault states saw substantial growth. The authors identify several causes for the increase but primarily the argument was that no-fault was no longer effective at preventing litigation.

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 negligence 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 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 get clients out of the no-fault

system when their claims are more valuable in the fault system (see Anderson et al. 2010).

In columns (3) and (4) of Table 8, 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 of the no-fault variable, we cannot include fixed effects. For total payments, the lawyer value is clearly negative for no-fault states, while the impact can be positive for fault states. This difference stems from the difference in third-party payment. In fault states, lawyers appear to be helpful in recovering higher payments from the injurer's insurance, under the most optimistic scenario, consistent with Anderson et al.'s (2010) view that while lawyers in no-fault states have options to increase their client's recovery, the scope of these options is more limited than in fault states.

6.4 Including Non-Modified States

In the estimates discussed above we restrict the sample to collisions in states that modified the collateral source rule during the sample period. The group with $Z = 0$ is states that modified the CS rule but applied it only to medical malpractices cases. The aim was to estimate the model using random variation of the scope of the reform not the enactment of the reform itself. Some states, however, had not modified their CS rule by the end of the sample period.

In Table 9 columns (1) and (2), we include those fifteen states (the non-modified states) in the analysis. The results are similar to Table 6. In all cases, the upper and lower bounds are negative, in contrast to the naive regression. Including the non-modified states makes the bounds slightly less negative; for example, the bound on total payment becomes $[-44,947, -14,111]$ when all states are included versus the bound of $[-66,429, -26,207]$ for the CS modification sample. This finding suggests some heterogeneity in the impact of lawyers in claims value.

6.5 States that have unclear provisions for the CS rule

In columns (3) and (4) of Table 9 we explore a different potential source of heterogeneity. 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 both "All

cases" and "Medical only" states; for this reason, we exclude them from our sample this time.

The findings are consistent with our previous findings. The negative estimates for a lawyer's value are larger without those states, suggesting 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. Thus retaining a lawyer may be less beneficial to the client in the presence of mandatory rather discretionary offsets.

7 Conclusion

In this paper, we propose a new identification and estimation method using invalid instruments and examine the impact of hiring an attorney on accident victims' recovery. The results of our bounds analysis suggest that 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 requires courts to deduct payments from first-party insurance sources 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 effect of attorneys on claim value.

The evidence in the naive regression suggests that there is considerable selection on observable claim characteristics and that claims with larger damages and lower recovery from first-party sources are more likely to retain lawyers. The bounds analysis suggests that the selection on unobservables is large enough to reverse the sign of the estimated effect of hiring a lawyer on total recovery and recovery from third-party sources. Put differently, under relatively weak assumptions, the bounds of the estimated effects are consistently negative for both overall recovery and recovery from third-party sources. In addition, we find a negative impact of lawyers on recovery from first-party sources. Finally, these negative bounds are robust to different institutional arrangements and sample restrictions.

The first question raised by our results is why our estimated impact of lawyers on first-party recovery would become negative when we control for endogeneity. The first possibility is selection, as the claims in which a victim seeks out a lawyer are unlikely to be a random sample of all claims. Accident victims are more likely to seek legal representation when the damages are more significant. Our data shows that every dollar in first-party payments is associated with an increase in third-party payments of \$0.52. However, even if we control

for the level of damages, there are likely to be unobservable differences related to first-party recovery that also drive the decision to hire a lawyer. For example, accident victims might decide to retain lawyers and seek compensation from third-party sources if their insurer's offer is lower than anticipated or does not cover their full damages. If this is the case, conditional on damages, we might find that claims involving lawyers have lower compensation from first-party sources.

Lawyers might also have a direct effect on first-party compensation. Clients typically pay lawyers using a contingent fee of 33% of the eventual recovery from third-party sources. Thus, the lawyer has every incentive to advise the client not to claim damages from first-party sources to increase the possible recovery. Of course, if the client hires the lawyer after the victim has recovered some of their losses from first-party insurers, the scope for reducing first-party compensation to increase eventual third-party compensation is far more limited. Since our data do not contain information on the timing of payments, we cannot resolve which of these explanations drives the lower first-party payments. It is clear, however, that the reduction in first-party payments is not offset by an increase from third-party sources.

The second question is how the value of an attorney in third-party recovery can be consistently negative. Because clients in our sample hire lawyers to negotiate with the other party's insurance, this negative effect on the third-party payment is more substantive than the finding on the first-party payment. The negative impact on total and third-party recovery could be due to fees. The average recovery for cases with a lawyer is \$39,544 in total before fees, of which \$28,942 is from third-party sources before fees. The typical contingent fee is 33% in our sample, suggesting that a client who recovered \$28,942 paid \$9,647 in attorney's fees. Given our bounds estimates in Table 6, it is certainly possible that the difference between cases hiring a lawyer and those which do not is mostly attorney's fees, which would imply that third-party insurers are offering accident victims an amount greater than the expected value of the claim to the victims, net of attorney's fees.

Given that the third-party insurer pays the total third-party claim plus their own lawyer's fees in the event of litigation, this strategy would be reasonable. This possibility is significant in understanding the value of an attorney. If a victim hires a lawyer to recover damages they have suffered in an accident or defend them in litigation, we would expect third-party payments to be higher. However, as repeat players in litigation, third-party insurers almost certainly have a reasonably accurate prediction of what the victim's case against them will be worth, as well as the court costs the insurer will incur if the victim hires a lawyer. The third-party insurer is making offers in the shadow of litigation.

If the third-party insurer's offer were accurate, at least on average, then the lawyer would simply be collecting fees on the offer the client would have received had they not pursued litigation. Given the consistently negative bounds, erroneously high estimates by clients or their attorneys leading to victims hiring lawyers seems a plausible explanation. This explanation, however, suggests that lawyers are socially valuable. Absent a threat of legal action—or, more accurately, in the face of less frequent legal action—third-party insurers would lower their initial offers to victims. In effect, lawyers are a public good. Those victims who overestimate the value of their claim and hire an attorney provide other accident victims with the creditable threat that makes their recovery possible.

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Table 1: Collateral source rule modifications requiring offsets of first-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 the "Types of Claims" column, "Medical only" means that the modifications were applied to medical malpractices only, while "All" means that the modifications were applied to all cases. "CS1–CS3" indicate what sources are exempted in the modifications; "CS 1" means that a modification exempts worker's compensation; "CS 2" means that a modification exempts Medicaid and Medicare; and "CS 3" means the CS reduction cannot be bigger than 50% of the total judgement.

Table 2: 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 the corresponding p-values (in parentheses) of the differences in hiring 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 3: Summary statistics.

Variables	**Whole Sample**			**CS Rule Modified**			**Collisions Only**		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
I. Accident Characteristics									
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 Damages	10,922	1,421	58,064	11,231	1,444	61,420	10,097	1,458	51,335
State per-capita Income	32,825	32,786	5,250	33,534	33,534	5,157	33,677	33,534	5,109
II. Other Tort Reforms									
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 joint/several liability reform	0.47	0.00	0.50	0.51	1.00	0.50	0.52	1.00	0.50
III. Outcome Measures									
Total Payment	15,673	1,122	194,973	16,579	1,015	221,699	14,711	1,233	160,436
Third party insurance Payment	10,175	0.00	185,965	10,948	0.00	212,375	9,842	0.00	150,023
First party insurance Payment	5,385	0.00	41,942	5,512	0.00	45,460	4,788	0.00	34,927
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 4: Naive regression.

Samples	**Total payment**	**Third party payment**	**First party payment**
I. **Whole Sample**			
no controls	26,667***	19,141***	7,260***
	(2,301)	(2,132)	(676)
controls	15,120***	15,091***	-164
	(2,397)	(2,294)	(566)
add fixed effects	14,600***	14,703***	-293
	(2,437)	(2,334)	(574)
II. **Non-modified Sample**			
no controls	29,776***	21,368***	8,099***
	(3,014)	(2,812)	(846)
controls	16,244***	16,671***	-660
	(3,140)	(3,021)	(695)
add fixed effects	15,679***	16,241***	-781
	(3,190)	(3,070)	(704)
III. **Collisions-only Sample**			
no controls	23,855***	16,534***	7,130***
	(2,290)	(2,047)	(704)
controls	11,485***	10,872***	488
	(2,363)	(2,191)	(624)
add fixed effects	11,344***	10,760***	458
	(2,403)	(2,229)	(633)

Estimated effects of lawyers from the "wrong" regression. Standard errors are in parentheses. Definitions of outcome measures and samples can be found in the main text. All effects are measured in 2002 constant dollars. Control variables (when used) include age of claimant, claimant is male, number of injured, number of work days lost, alleged total damages, 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. Their estimates are not reported here to save space but are available upon request. Some specifications include state and year fixed effects. Here, '*', '**', '***' indicate significance at 10%, 5%, 1%, respectively.

Table 5: First-stage regression.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
CS rule change	-0.037***	-0.023			-0.017	-0.001
	(0.012)	(0.021)			(0.013)	(0.022)
Value of Case			0.077***	0.073***	0.072***	0.074***
			(0.012)	(0.013)	(0.013)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	Yes	No	Yes	No	Yes
R-squared	0.074	0.103	0.077	0.107	0.077	0.107
F statistic	9.981	1.214	40.486	34.483	21.130	17.240

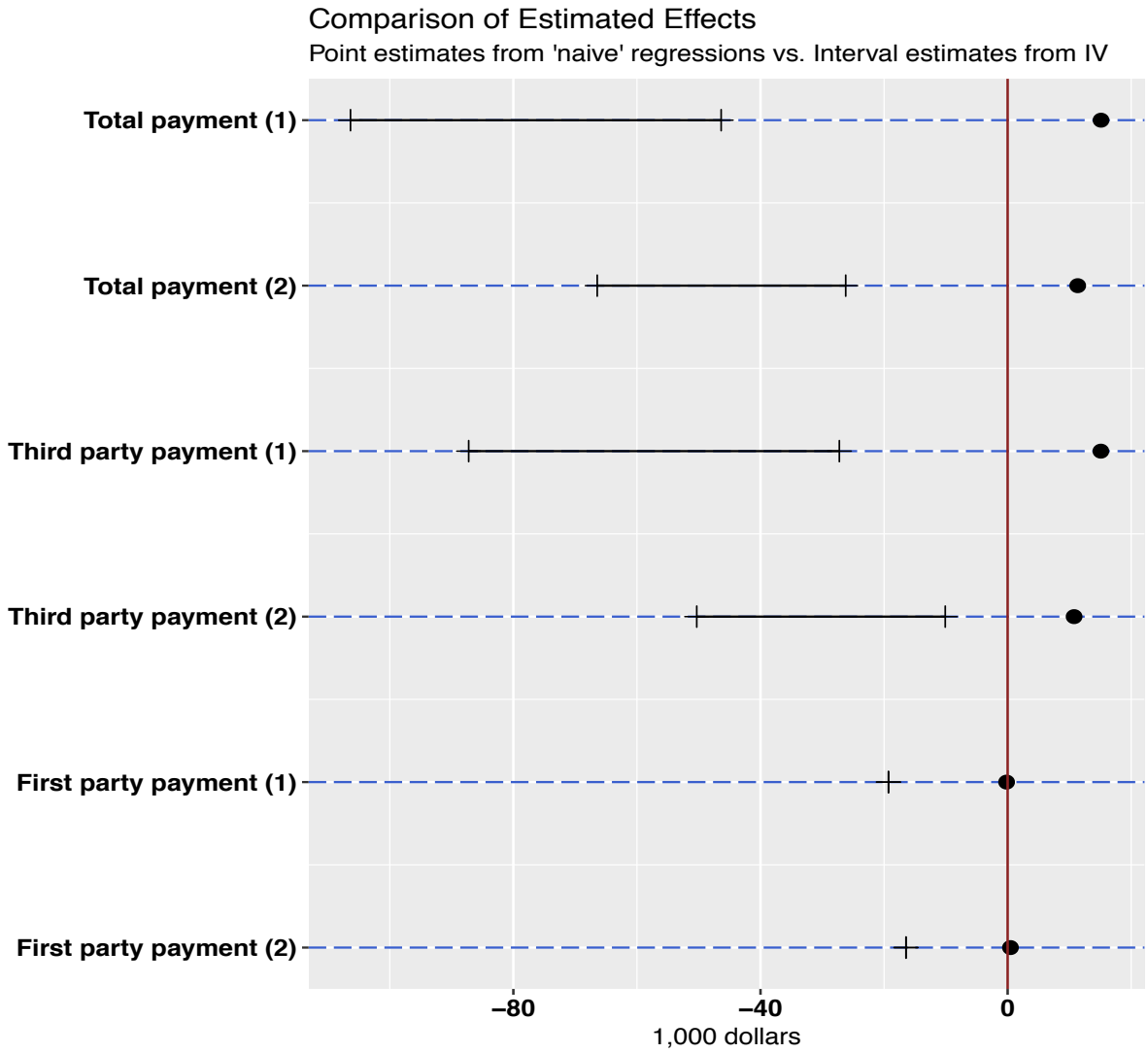
The first-stage regression. The dependent variable is "Hired lawyers," and the main independent variables are the CS rule change and the value of the case (VC). The list of other control variables can be found in Table 4. Columns (2), (4), and (6) include state and year fixed effects as additional control variables. The F statistic tests the null hypothesis that coefficients on instruments are all equal to zero. Here, '*', '**', '***' indicate significance at 10%, 5%, 1%, respectively.

Table 6: Average indirect effects of attorneys.

Outcomes	(1)	(2)
Total payment	[-106,361, -46,369]	[-66,429, -26,207]
	(-178,291, 16,088)	(-143,050, 42,749)
Third party payment	[-87,234, -27,242]	[-50,319, -10,097]
	(-151,300, 29,879)	(-118,812, 53,401)
First party payment	-19,286	-16,451
	(-39,319, 746)	(-38,589, 5,686)
Controls	Yes	Yes
Fixed effects	No	Yes

Bounds of the average indirect effects of hiring attorneys from IV regressions with 95% confidence intervals in parentheses. We use the collisions-only sample, and all effects are measured in 2002 constant dollars. Columns (1) and (2) show results without and with fixed effects, respectively. The list of control variables can be found in Table 4. The first party payment is not affected by the CS rule, so it is point-identified.

Figure 1: Naive vs. IV regressions.



Comparison of point estimates (dots) from naive regressions and bound estimates (thick, solid horizontal lines) from IV regressions. The sample includes collisions only. Naive estimates are from Part III of Table 4, while IV estimates are from Table 6. Each horizontal line represents a regression model. The first line, "Total payment (1)," uses total payment as the outcome variable, and the model includes control variables but no fixed effects. The model for "Total payment (2)" includes both control variables and fixed effects. The same convention applies to other cases. For the first-party insurance payment, interval estimates reduce to point estimates.

Table 7: Changes in fees and lawyer efforts.

Outcomes	All cases	Medical only
I. Lawyer's Fees:		
total fees charged	-795.0	-1648.9
	(13080.2)	(3358.4)
paid only a contingent fee	0.025	-0.033
	(0.045)	(0.068)
II. Lawyer's Efforts:		
defended family	-0.008	0.063**
	(0.015)	(0.028)
helped financial planning	-0.026	0.043*
	(0.015)	(0.023)
advised which MD to use	0.033	-0.038
	(0.037)	(0.060)
filed a lawsuit	-0.018	-0.031
	(0.042)	(0.061)
negotiated with other driver's insurer	0.042	-0.035
	(0.044)	(0.070)
gave general advice	0.028	0.018
	(0.019)	(0.026)
did nothing	0.000	-0.020*
	(0.008)	(0.011)

Part I reports regression outcomes of the total fees charged by attorneys on the CS rule change dummy variable. Part II shows the results of the lawyer's efforts variables on the CS rule change. Analysis for two groups are conducted 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. Because the CS rule changes did not affect auto accidents, effects in the second group (if there are any) are spurious. All regression models include covariates listed in Table 4 and state and year fixed effects. Standard errors are in parentheses.

Table 8: Effects heterogeneity and robustness.

Outcomes	**Without FL and MN**		**No Fault**	
	(1)	(2)	(3) No-fault states	(4) Fault states
Total payment	[-93,335, -48,336]	[-70,336, -28,808]	[-46,650, -11,584]	[-31,065, 3,267]
	(-162,688, 14,554)	(-156,817, 50,680)	(-171,283, 107,175)	(-72,632, 35,233)
Third party payment	[-81,284, -36,285]	[-52,357, -10,829]	[-52,314, -17,248]	[-21,549, 12,782]
	(-144,986, 22,528)	(-131,269, 63,504)	(-172,455, 98,801)	(-49,910, 33,096)
First party payment	-12,391	-18,104	4,922	-9,579
	(-30,293, 5,511)	(-41,550, 5,341)	(-17,251, 27,096)	(-30,491, 11,332)
Controls	Yes	Yes	Yes	Yes
Fixed effects	No	Yes	No	No

Bounds of the average indirect effects using collisions in 2002 constant dollars. The 95% confidence intervals are in parentheses. Columns (1) and (2) show results excluding Florida and Minnesota. Columns (3) and (4) compare no-fault vs. fault states.

Table 9: Effects heterogeneity and robustness (continued).

Outcomes	**Including non-modified states**		**Excluding ambiguous states**	
	(1)	(2)	(3)	(4)
Total payment	[-90,988, -37,992]	[-44,947, -14,111]	[-133,040, -63,480]	[-85,271, -38,056]
	(-142,019, 6,405)	(-101,516, 37,288)	(-222,630, 13,000)	(-182,021, 48,071)
Third party payment	[-76,610, -23,615]	[-32,989, -2,154]	[-109,585, -40,025]	[-64,176, -16,961]
	(-121,941, 16,738)	(-83,306, 44,880)	(-189,684, 30,265)	(-150,971, 62,849)
First party payment	-14,525	-12,490	-23,661	-21,540
	(-29,681, 629)	(-30,216, 5,235)	(-47,210, -112)	(-48,327, 5,245)
Controls	Yes	Yes	Yes	Yes
Fixed effects	No	Yes	No	Yes

Bounds of the average effects using collisions measured in 2002 constant dollars. Columns (1) and (2) expand the scope of the sample by including 15 states that had never modified the CS rule during the sample period. Columns (3) and (4) restrict the sample by 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 are not required to.

Supplemental Appendix: The Value of an Attorney, Evidence from Changes to the Collateral Source Rule (Not for publication)

Eric Helland and Jungmo Yoon

A.1. Demand-Supply Analysis Under Cost Sharing

In this section, we present a different version of our model in Section 2.3, 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 2.3. Recall that the cost for a claimant of pursuing a claim without an attorney is normalized to be 0. But arguments here as well as those in Section 2.3 continue to hold as long as the cost without an attorney is lower than the cost with an 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

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

The attorney will take the case if

$$P_1\left((1-\beta)C+\beta M\right) > C. \tag{A.2}$$

Now consider states that have modified the CS rule. As discussed, some states allow their attorneys to receive payments based on M, while other states require that their attorneys 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. Thus 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 (A.1), 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 (A.2), 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

$$P_1(1-\beta)(M-C) - \left(P_1(1-\beta) - P_0\right)L > P_0 M. \tag{A.3}$$

Consider a potential client who is willing to hire an attorney without the CS modification. For them, the condition (A.1) 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 (A.3), we conclude that the demand for an attorney will be negatively affected for such a client.

Could a potential client who did not want a lawyer without the CS modification change their mind and want an attorney with the modification? In other words, is it possible to have a defier? For such a client to exist, a necessary condition is that (A.1) does not hold while (A.3) 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 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 (A.2), 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 2.3 remains true if the cost is shared between the client and the 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 the lawyer's effort level. The simpler model in Section 2.3 leads to the same conclusion. The working assumption in this section is that the lawyer's effort is proportional to the expected net benefit (surplus) to lawyers from claims they take. A lawyer's efforts can be modeled in

a different way, but this approach is consistent with the literature.

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 will be a reasonable assumption if the CS rule change is not a determinant of damages from auto accidents.

Consider first states that did not modify the CS rule at all. The demand condition (A.1) 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 (A.2) 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

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

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

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

Clearly, $\mathrm{AS}_1 \geq \mathrm{AS}_0$ means 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. These marginal cases are "compliers" in the terminology of the IV regression. The remaining cases that retain lawyers tend to have higher M on average. Because a lawyer's surplus for a given case does not change, the surplus will go up.

Finally, consider cases in which attorneys are required to receive fees based on $M - L$. From the supply-and-demand, 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

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

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

In sum, assuming that lawyers base their efforts on expected surplus, the analysis in this section predicts that the average effort level depends on how the fees are determined. If the state law requires lawyers to get paid after offsets, the effort will be lower. If lawyers can be paid before offsets, they will make more effort, on average. In addition, the theory predicts that the CS rule modification causes the least valuable cases to be dropped either by clients or by lawyers.

A.3. Proof of Main Results

By the independence assumption, the numerator of the left hand side of the equation (4) 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

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

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 (4). This proves Proposition 4.1.

Next, focus on the numerator of the right-hand side of (4). The first term $E[Y(0, D(0))|Z = 0] = E[Y|Z = 0]$ by the definition of the potential outcome. On the other hand, the second term is bounded by $E[Y(1, D(1))|Z = 1] \leq E[Y(0, D(1))|Z = 1] \leq E[Y(1, D(1))+W|Z = 1]$ using Assumption 3, which, in turn, becomes $E[Y|Z = 1] \leq E[Y(0, D(1))|Z = 1] \leq E[Y + W|Z = 1]$. This proves Proposition 4.2.

Table A.1: 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 A.2: 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

Table A.3: 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
I. Accident Characteristics									
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 Damages	20,083	5,431	90,105	9,363	1,672	48,572	5,626	767	32,131
II. Other Tort Reforms									
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 joint/several liability reform	0.44	0.00	0.50	0.48	0.00	0.50	0.48	0.00	0.50
III. Outcome Measures									
Total Payment	39,544	7,972	341,348	7,628	1,851	31,020	4,346	396	19,305
Third party insurance payment	28,942	4,101	326,964	3,168	0.00	9,532	1,270	0.00	5,061
First party insurance payment	10,309	0.00	68,622	4,439	0.00	29,194	3,048	0.00	18,513

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 A.4: 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
I. Accident Characteristics									
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	14.99	0.00	60.52	14.08	0.00	54.01
Number Injured	1.36	1.00	0.69	1.36	1.00	0.73	1.36	1.00	0.68
Amount Claimed in Damages	14,031	1,402	85,431	9,637	1,446	41,399	9,636	1,383	44,558
II. Other Tort Reforms									
No Fault	0.46	0.00	0.50	0.25	0.00	0.43	0.00	0.00	0.05
Compulsory Insurance	0.72	1.00	0.45	0.72	1.00	0.45	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	0.28	0.00	0.45	0.01	0.00	0.08
State awards prejudgement interest	0.33	0.00	0.47	0.38	0.00	0.48	0.44	0.00	0.50
State has joint/several liability reform	0.74	1.00	0.44	0.46	0.00	0.50	0.34	0.00	0.47
III. Outcome Measures									
Total Payment	17,679	313	252,162	13,949	1,045	120,893	12,294	1,523	55,741
Third party insurance payment	10,891	0.00	233,963	9,191	0.00	113,568	7,428	0.00	41,243
First party insurance payment	6,744	0.00	70,758	4,597	0.00	26,133	4,770	0.00	27,284

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.