

WORKING P A P E R

Judicial Expenditures and Litigation Access

Evidence from Auto Injuries

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Judicial Expenditures and Litigation Access: Evidence from Auto Injuries

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Abstract: Despite claims of a judicial funding crisis, there exists little direct evidence linking judicial budgets to court utilization. Using data on thousands of auto injuries covering a 15-year period, we measure the relationship between state-level court expenditures and the propensity of injured parties to pursue litigation. Controlling for state and plaintiff characteristics and accounting for the potential endogeneity of expenditures, we show that expenditures increase litigation access, with our preferred estimates indicating that a 10% budget increase increases litigation rates by 3%. Consistent with litigation models in which high litigation costs undermine the threat posture of plaintiffs, increases in court resources also augment payments to injured parties.

JEL Classification: K13, H72, K41

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I. Introduction

Studies examining state court funding in the United States consistently argue that the civil justice system is in the midst of a funding crisis and highlight the negative consequences of insufficient funding. However, the quality of evidence linking funding to the utilization of the courts by ordinary citizens is limited. For example, the American Bar Association's (ABA) State Court Funding Crisis Toolkit (2002) cites reductions in court hours in selected jurisdictions as the primary evidence that budget cuts reduce access to justice, despite the fact that little is known regarding how court hours affect access. In its report on court funding in Washington state, the Board of Judicial Administration's (2004) claims that inadequate civil court funding had harmed the public rested largely on anecdotes from a handful of individual cases, including a case in which a plaintiff was unable to collect a judgment because the defendant corporation went bankrupt during a long court delay. Recent calls for court funding reform by the Florida Supreme Court (2009) argue that access is threatened by budget reductions, but provide no data to substantiate this claim. There is a dearth of systematic evidence regarding the extent to which budgetary decisions affect the ability of individuals to utilize the civil courts.¹

In this paper we provide the first direct evidence of a relationship between the decision to litigate and the availability of court resources. Drawing from a database of tens of thousands of auto injuries covering a fifteen-year period, we demonstrate that increases in judicial resources are associated with an enhanced probability of pursuing litigation, with 10% growth in resources generating a 3% increase in case filings. These results survive after controlling for a wide variety of state and injury characteristics and instrumenting to account for potential endogeneity

¹ Constitution Project (2006) provides a summary of research on state court budgeting, noting, "Despite the clear effects the state budget process has on state court systems' ability to function, there is surprisingly little scholarly research or literature on the subject." (p. 14).

of judicial resources. In a placebo analysis using our statistical framework we show that other forms of government spending do not predict litigation. We also present suggestive evidence that attorney representation increases with judicial expenditure, consistent with the notion that attorneys are less willing to represent marginal clients when court resources are low, although our estimates of this relationship are imprecise. Given that the threat of litigation can provide an important impetus for tortfeasors to compensate injured parties, we might expect factors that facilitate litigation to also increase compensation. Changes in expenditure do appear to affect payouts, with a 10% increase court expenditures augmenting claimant compensation by approximately 2-3%. Simple calculations suggest that changes in court budgets of plausible magnitude could impact tens of thousands of case filings each year and shift millions of dollars of compensation between defendants and plaintiffs.

Our paper departs from much of the existing literature relating availability of resources to performance of the legal system. Numerous survey studies attempt to gauge the extent to which individuals forgo use of the legal system due to indigence, but these studies do not directly address the role of judicial budgets.² Existing empirical studies on access to justice in the civil arena focus primarily attorney representation and attempt to measure the effects of programs that subsidize representation for indigent clients. Farmer and Tiefenthaler (2003) for example, demonstrate that women in counties with more expansive availability of civil representation for victims of domestic violence are less likely to report victimization, while Norrbin, Rasmussen, and von Frank (2005) examine the relationship between civil representation of at-risk youth and later criminal participation. Although not directly addressing access, a related strain of research attempts to correlate judicial salaries with measures of the quality of judicial decision-making,

² The American Bar Association (ABA) Comprehensive Legal Needs Study (1994) and the Legal Services Corporation's Justice Gap Report (2007) provide examples of a studies in this vein that have been widely replicated in individual states.

including case volume (Baker 2008; Choi, Gulati, and Posner 2008). Implicit in these comparisons is the notion that increased resources may attract more able individuals as judges, who improve the handling of cases. In contrast to these studies, we focus on overall state and local expenditures for the justice system, which impact not only resources for indigent counsel and judicial salaries, but also factors such as availability of judges, quality of court personnel, court technology, and fees, all of which influence the administrative burden associated with pursuing a case.

The present study is limited in scope in a number of important ways. First, we focus on a particular class of cases, auto torts, whereas court funding issues may have larger practical implications in other domains of the civil justice system, such as family and employment law. This focus is driven primarily by data availability--our auto data allow us to identify potential litigants and provide substantial detail about the nature of the injury, whereas we lack such systematic data for other types of civil disputes. Second, we take evidence of a decrease in litigation filing rates as indicative of reduced access. One possibility not inconsistent with our findings is that lowering resources deters what are otherwise frivolous lawsuits, and therefore does not impact access of the type that is valued by policymakers. Given that we have no method of objectively assessing the merits of particular cases³, we cannot directly examine whether lawsuit quality has been affected by changes in resources. However, if access represents the ability of all who believe they have a legitimate claim to attempt redress through the civil justice system, our focus on entry into litigation seems appropriate. Third, we have little detail on the nature of expenditures on the judiciary, and it seems reasonable to expect that both the aggregate amount and the allocation of expenditures is important. Our data on resources are

³ Adjudications seem like an obvious possibility here, but given the rarity of adjudications (despite our large sample, we observe only 821 cases decided at trial), we cannot draw strong conclusions from our data.

insufficiently fine to permit such analyses. Despite these important limitations of our study, given that policy discussions of the negative impacts of an underfunded judiciary rely almost entirely on anecdotal evidence, we believe that providing systematic quantitative evidence linking resources to actual behavior of potential litigants represents an important step in understanding how resources may affect utilization of the civil justice system.

Section II of the paper describes the data sources that we use in our analysis. Section III describes our empirical approach and presents estimates of the relationship between filing probability and state court expenditures along with numerous robustness checks. Section IV examines whether budget-induced changes in access affect payments to injured parties, and section V concludes.

II. Data Description

Our data on judicial expenditures are drawn from the Finance Statistics component of the Annual Survey of Governments conducted by the Bureau of the Census. Each year the Census Bureau collects finance data from each state government and a probability sample of local governments, including counties, municipalities, school districts, and other government entities such as water districts. A complete census of governments occurs every five years. Finance data include information about revenues, expenditures, and intergovernmental transfers across a number of different spending categories, including health, education, and administration. In intercensal years the Census Bureau estimates total statewide expenditures by state and local governments across a variety of expenditure categories from survey responses.⁴ We measure judicial

⁴ This is true for all years except 2001, in which the Census Bureau experimented with an alternative sampling scheme which did not produce state-level estimates of total expenditures. For 2001, we estimated total statewide judicial expenditures by adding the actual state government expenditures to imputed local expenditures. We imputed local expenditures by regressing 2001 expenditures on 2002 expenditures for the local governments in a

resources using statewide aggregate expenditures across all levels of government on current operations for judicial and legal services. Data on judicial expenditures are available starting in 1987 through the end of our sample in 2002.⁵ A notable limitation of these data are that they do not separately identify expenditures related to the civil component of the justice system, nor do they provide more precise detail regarding the nature of expenditures. However, a key advantage of these data relative to other sources such as individual state budget documents is that they are collected for all state and local governments according to a uniform procedure designed to facilitate comparisons of expenditures both across states and over time.

Appendix Figure 1 depicts per capita expenditures on judicial operations over our sample period for each state. Several patterns are apparent from the figure. Average real per capital expenditures on the state judiciary increased by roughly 3% per year, and most individual states saw increases in resources over this period as well. However, the levels of expenditure vary substantially across states--for example, on a per capita basis Indiana's judicial expenditures were only about half as large as those of its neighbor Ohio. Some states saw large increases in court expenditures due to legislative changes. For example, expenditures in California jumped dramatically after the passage of the Trial Court Funding Act of 1997, which enacted major changes the court funding system in the state, along with the 1998 passage of Proposition 220 permitting the consolidation of superior and municipal courts. Finally, despite the general upward trend in resources over this period, there were episodes of real declines in resources that spanned multiple years in several states. For example, real judicial expenditures fell in Oregon

particular state that reported in both years, and then multiplying the 2002 local government total (which should have been highly accurate because 2002 was a census year) by the adjustment factor estimated from this regression. We calculated a separate adjustment factor for each category of expenditures in each state.

⁵ Although the survey has existed since the 1970's, previous waves included judicial expenditures in a general administrative category. Later years of the survey also report construction, capital, and equipment outlays related to the judicial system, but these are not a focus of the present study.

between 1995 and 1999, partly due to the passage of a series of ballot initiatives that reduced tax rates and constrained the state budget.

We consider access to justice as representing the ability of individuals who have suffered an injury to pursue redress through the court system if they or their attorney believe it is in their interests to do so. Under this definition, to examine access we must identify a set of potential cases, some of which are not ultimately pursued, and then evaluate the factors that impact whether cases are litigated. Automobile personal injury cases appear well-suited to such an analysis relative to other types of civil cases. First, for these cases there is an easily identifiable pool of potential litigants, namely, those who have suffered injuries in accidents. Second, there is relatively little scope for forum shopping in auto cases, given that auto cases are generally tied to the location of the accident. Forum shopping might dilute any effects of resources given that plaintiffs could avoid courts that are congested due to lack of resources. Third, auto torts represent an important component of the overall civil caseload, comprising more than half of all tort cases handled in state courts (Lafountain et al. 2008). Finally, as we discuss below, an important effect of access barriers may be to alter threat points in settlement negotiations in favor of defendants. Unlike many other classes of civil disputes, auto claims data allow us to readily observe payments made to injured parties even when litigation does not occur.

We merge the expenditure data with data on closed automobile injury claims collected by the Insurance Research Council (IRC). In 1987, 1992, 1997, and 2002 the IRC compiled data on individual bodily injury claims closed by insurers during a two-week sample window; participating insurers represented the majority of the U.S. private passenger automobile market in each year of the study.^{6,7} The resultant claim samples provide a snapshot of auto injuries

⁶ The somewhat unconventional sampling scheme of the IRC data, which captures claims when they terminate rather than when they are initiated, means that observed claims from a particular year are not necessarily a representative

across the country. Our primary dependent variable is an indicator for whether a third-party claimant pursued a lawsuit. We also observe a significant amount of detail regarding each injury, including demographic characteristics of the claimant and nature and severity of the injury, which allows us to control for factors which may affect the desirability of pursuing litigation in a particular case. In the analysis that follows, we limit our sample to third-party claims involving claimants who resided in the same state in which an accident occurred and the insured vehicle was registered, leaving a sample of 94,912 observations. Table 1 presents summary statistics describing selected variables used in our analysis.

III. Effects of Resources on Litigation

As in Shavell (1982) or Spier (2007), we imagine a potential plaintiff balancing the costs of potential litigation with the expected benefits in terms of a settlement or award in deciding whether to pursue litigation over a particular injury. Judicial resources might affect plaintiff costs through several channels. Resource constraints may induce courts to increase civil filing fees, directly increasing the financial costs of pursuing cases. Limited resources may lead to delay in the processing of cases (Goerdt et al. 1991), which may raise attorney fees and decrease

sample of claims filed in that year. For example, claims from 1988, most of which show up in the 1992 closed claim data, tend to be larger and more complex, since few simple claims would survive for the 4-year period between 1988 and 1992. However, we account for this in our empirical work by including a full set of interactions between year of data and year of claim, meaning that the effect of expenditures is identified by comparing litigation patterns across states with differing levels of judicial expenditures among claims that had the same duration. An alternative way to estimate the effects of expenditures on access would be to estimate the relationship between expenditures and claim duration, but the sampling structure of the IRC data means that this data set is not well suited for such an analysis. Moreover, the extent to which claim duration would properly capture duration of court cases, which is the true quantity of interest, is unclear given that a court slowdown could induce faster settlement of claims under some models of defendant behavior.

⁷ The IRC also published similar data in 2007, but unfortunately these data no longer identify the home state of the claimant, making it impossible to exclude cases involving interstate drivers for which venue selection may be a factor.

the value of awards due to discounting.⁸ Judiciaries with limited resources may also attract less qualified employees, which might increase the variance of case outcomes which would discourage case filing when potential plaintiffs are risk-averse.

We begin by estimating regressions relating the probability that an individual claimant files a lawsuit to the level of judicial resources available in a state at the time of the accident. Our regression model takes the form:

$$Y_{ist} = \alpha \cdot Spend_{st} + \beta X_{st} + \delta P_i + \gamma_s + \eta_t + \varepsilon_{ist} \quad (1)$$

where Y_{ist} represents an indicator variable for whether individual i residing in state s who was injured in year t files a lawsuit, $Spend_{st}$ reflects total state and local spending on the judiciary in year t , X_{st} is a set of state-level control covariates, P_i represents a vector of characteristics of the injured party, and γ and η respectively denote state and accident year fixed effects. Here α measures the relationship between state spending and willingness to utilize the court system to resolve claims.

In order to measure the impact of judicial resources on access, the controls in equation (1) must capture factors that affect the likelihood of a suit and that are also correlated with state spending. For example, if wealthier states spend more on both the judiciary and on road safety, leading drivers in these states to have systematically different injuries, failure to control for such differences will generate invalid inference regarding the effects of judicial resources. By including state fixed effects in (1), we remove the influence of all factors specific to a state that are constant over our sample period, which likely includes such factors as the basic structure of

⁸ However, delay may lower costs for plaintiffs if they are able to collect pre-judgment interest at rates above market interest rates (Priest 1989).

the judiciary, major aspects of case law governing torts within the state, and many characteristics of the auto insurance system. Our year-of-accident fixed effects remove variation due to inflation, general safety improvements, and other macroeconomic factors.

In most specifications we also control for time-varying covariates at the state level that may impact both resources and the value of litigation. We include controls for demographic factors such as total population and the gender, racial, and age structure of the population. Given that past research has demonstrated links between civil caseloads and economic conditions within a state (Jacobi 2009), we also control for poverty rates, the unemployment rate, and per capita income. Because we cannot separately identify expenditures for civil versus criminal courts, we also control for factors related to the volume of criminal cases, including crime rates and police expenditures.⁹ Finally, we include a series of controls designed to capture the legal environment in each state, including an indicator for no-fault auto insurance availability and indicators for the presence of damage caps, contingency fee caps, limitations to joint-and-several liability, and collateral source admissibility. These controls draw upon the database produced by Avraham (2006), but we have also examined the relevant statutes in each state to ensure that the reforms are coded correctly as they apply specifically to auto cases.¹⁰ Given Kessler's (1996) finding that pre-judgment interest rules may affect the degree of delay in settling cases, we also control for the provision of pre-judgment interest and rules governing such interest, including

⁹ Population data are taken from Census and NCHS annual population estimates by age, race, and sex. Unemployment data come from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) database. Per capita income data are taken from the Bureau of Economic Analysis Regional Economic Accounts. Data on crime rates come from the FBI's Uniform Crime Reports, and police expenditure data are taken from the Annual Survey of Governments. No-fault data come from Joost (2002).

¹⁰ For example, Florida's 1986 Tort Reform and Insurance Act (Fla. Stat. §768.80) provided for cap on non-economic damages that applied to a wide variety of tort cases, including auto cases. However, this law was ruled unconstitutional by the Florida Supreme Court in 1987, and although non-economic damage caps were re-instated in 1988 for medical malpractice cases (Fla. Stat. §766.207, §766.209), auto cases were no longer subject to caps.

whether judges have discretion over interest and whether interest is applied to non-liquidated damages or cases with rejected settlement offers.

We also exploit the rich detail in our insurance claim data to control for many individual factors that might affect the desire of individuals to pursue litigation. Our claim level controls include variables capturing the age, gender, and marital status of the claimant, seat positioning and seatbelt use, and insurer estimates of the degree of fault of the insured. We also include over 30 covariates capturing the nature of injuries sustained in the accident, treatment received, and limits of available coverage. We expect different individuals residing in the same state who have similar demographic and injury characteristics to have comparable tastes for litigation. Thus, once we condition on these factors, differences in litigation rates we observe that are correlated with judicial spending seem likely to reflect resource barriers to access.¹¹

Table 2 reports our coefficient estimates from estimated of (1) for judicial expenditures and selected other covariates. Given the dichotomous nature of our dependent variable, we estimate (1) using probit regression and reported coefficients represent marginal effects. Specification 1 includes a minimal set of controls, while column 2 adds controls for time-varying state-level variables. In the most basic specification, the estimated relationship between expenditures and litigation is positive and marginally significant. After adding state-level controls (column II), the estimated coefficient of .058 suggests a statistically significant and practically important effect of expenditures on access, with a 10% increase in judicial expenditures increasing the probability of filing a suit by 4%.

¹¹ Because our sample includes only claims filed with auto insurers, if expenditures affect individuals' willingness to file a claim at all, a selection issue might arise despite our substantial set of controls. In a separate unreported analysis we compiled nationally-representative IRC survey data covering 14017 individuals who were injured in auto accidents between 1989 and 2002. These data record information regardless of whether the individual filed an insurance claim. In estimates of versions of equation (1) in which the outcome was whether or not an individual filed a claim with an auto insurer, there was no relationship between expenditures and claim filing. We thus view selection into claim filing as unlikely to be problematic.

The coefficients on the state-level covariates generally accord with intuition. As expected, both non-economic damage caps and limitations of joint and several liability are negatively related to the probability of filing a suit, while the coefficients on other measures of the legal environment are small and not statistically significant. State-level poverty rates are negatively and significantly related to litigation, possibly reflecting a willingness of indigent plaintiffs to more quickly settle cases. Given that our judicial resources measure captures combined resources for both criminal and civil proceedings, we also expect that increases in crime or state expenditures on police to be negatively related to access conditional on expenditures. Conceptually, for a fixed level of resources, a larger criminal caseload implies fewer resources available for civil proceedings. Indeed, the coefficients on police expenditures are negative and statistically significant.

Column 3 introduces claim level covariates as additional controls. Controlling for claim-level covariates lessens the predicted impact of a 10% increase in expenditures on litigation to 3.4%, but the relationship remains statistically significant. Included in these specifications are controls for accident time; impact and injury severity; reported economic losses; available amounts of coverage; and claimant gender, age, marital status, employment status, seat positioning, seatbelt use, degree of fault, medical utilization, and injury. Most of the signs and magnitudes of the control coefficients accord with intuition. For example, the probability of a lawsuit is increasing in the dollar value of claimed losses, and is substantially higher when the insured driver has high policy limits. Lawsuits are less likely when the insurer estimates a higher degree of fault for their insured, presumably because there is less disagreement between the plaintiff and insurer regarding culpability. Claimants reporting particular types of injuries, such as neck injuries, are also more likely to sue. Individuals who are not in the labor force are 6%

less likely to pursue litigation, which may result from liquidity constraints or may reflect easier resolution of claims that do not involve wage loss.

The rich set of controls included in the regressions in Table 2 account for most factors likely to be important in explaining litigation patterns. Nevertheless, it remains possible that the reported coefficients on judicial spending do not capture the causal effect of spending due to omitted variables or other forms of endogeneity. Table 3 reports results from a series of checks designed to test the interpretation of these coefficients as capturing a causal effect of resource availability on court utilization. Although it is reasonable to expect that individuals take the current characteristics of the judicial system as given when making decisions regarding whether to pursue a civil action following an accident, this does not preclude omitted variable bias in the estimation of (1). A particular concern arises regarding shocks to litigiousness, such as changes in civil procedure or state supreme court decisions, that might induce the appropriations authority to allocate more funds to the judiciary, anticipating changes in the nature of the civil caseload. Failure to control for such shocks would generate estimates of the effect of judicial resources on case filing that overstate the true causal effect of judicial resources. Moreover, if individuals respond to court conditions with a substantial lag, an estimating equation relating litigation decisions to contemporaneous levels of resources would be misspecified.

We can directly test for a proper temporal relationship between resources and litigation by including lagged and future levels of judicial spending as additional explanatory variables in equation (1). If spending levels are increased in response to changes in litigiousness, we should observe a strong correlation between current litigation decisions and future spending, which would be manifest by a positive regression coefficient on future spending. However, as demonstrated in Table 3, the point estimate on future spending is negative and insignificant. A

negative coefficient could be consistent with an environment in which policymakers reduce court funding in response to shock to litigiousness, possibly in an effort to deter “frivolous” lawsuits. Such behavior, if present, would lead us to understate the effects of resources on access and would imply that the estimates in Table 2 represent a lower bound on the true effects of resources. Although the substantial collinearity between current, past, and future levels of spending renders the estimates of the contemporaneous effect of expenditures imprecise, overall the pattern across coefficients in specification 1 is supportive of a causal effect of resources on litigation. In particular, relative to the coefficient on contemporaneous spending, the magnitudes of the coefficients on lagged and future spending levels are modest.

Our next specification checks to test for the possibility that the positive relationship we observe between judicial resources and access reflects the effects of omitted factors other than resources. We replace our judicial resources measure with other similar types of spending which should exert no independent causal effect on access to the civil justice system. If omitted factors such as the wage structure of the state economy are correlated with general spending and litigation behavior, and it is this correlation that is driving the observed relationship between resources and access, we might expect to observe positive and significant coefficients on these alternative spending measures. However, replacing judicial expenditures with per capita expenditures on public buildings (specification 2), per capita prison expenditures (specification 3) or per capita legislative operations expenditures (specification 4) yields coefficients that are not statistically significant and of small magnitude. This placebo analysis suggests that our empirical approach is sufficient for isolating the effects of judicial expenditures from other more general changes in spending.

Beyond the causality checks reported in Table 3, a more general method to account for reverse causality or omitted variable bias is to identify an instrumental variable that affects judicial expenditures but does not directly affect the willingness of individuals to pursue litigation. We instrument for judicial expenditures using annual state average tax rates, which we calculate as the share of total personal income collected paid to each state based upon the information reported in the State Annual Personal Income series collected by the Bureau of Economic Analysis. After controlling for general economic conditions in the state, we expect that fluctuations in average tax receipts largely represent idiosyncratic changes in the tax code. Because policies governing general tax rates are unlikely to arise as a result of changes in the latent willingness of individuals to pursue litigation, taxes seem likely to be exogenous from a policy standpoint. However, given that most states have balanced budget requirements, fluctuations in tax collections can have immediate impacts on the availability of funds for state government, including the judicial system.

In order for average tax rates to provide a valid instrument, we must assume that tax rates affect litigation behavior only through changes in judicial spending rather than other uncontrolled channels. Several potential concerns with this assumption merit discussion. One concern is that tax rate changes might be directly correlated with driving behavior and therefore litigation patterns. For example, changes in gasoline taxes might alter normal driving patterns, which could in turn affect the desirability of pursuing suits in accidents. In our context, because we condition on the occurrence of an accident and observe a wide array of information regarding each claim we are likely able to control for relevant confounding factors. For example, our regressions include detailed controls for injuries sustained in each accident, so any behavioral

shifts that affect litigation patterns through injuries (such as substitution from safer to less safe vehicles due to tax changes) would be correctly accounted for in our analysis.

Another potential concern is that tax rates potentially affect income, which is often thought to influence a variety of economic decisions. However, given that the variation in tax rates at the state level is relatively narrow, ranging between 0% and 5%, taxes alone can explain only a small degree of personal income variation. It seems less likely that small changes in income affect litigation behavior. Moreover, in auto cases income does not appear to be a strong predictor of the decision to pursue litigation.¹² Although we believe that tax rates provide a plausibly exogenous source of variation in judicial expenditures, we note that, as with all instrumental variables analysis, the validity of this instrument rests upon assumptions that are not directly testable.

Table 4 reports estimates instrumental variables estimates using our data on tax rates. The top row reports estimates from a reduced-form specification in which we re-estimate equation (1) replacing expenditures with tax rates. As expected, tax rates are positively related to the probability of filing a suit, which is consistent with the interpretation of the instrument given above. The mean and standard deviation of the tax rate in our sample are .02 and .01, indicating that a one standard deviation increase in the tax rate is associated with an increase in the probability of filing a suit of 16%.

The remaining rows of Table 3 report IV estimates analogous to the estimates presented in Table 2. Changes in tax rates generate increases in expenditures that are statistically

¹² The IRC Closed Claim data we use for this analysis do not include income information (aside from weekly wages, which are only reported for individuals claiming lost work). A separate survey of 3478 auto injury victims conducted by the IRC in 2002 does include income data reported in intervals along with litigation and other accident information. In probit regressions of an indicator for whether a claimant filed a suit on indicators for 12 income intervals, the income indicators are jointly insignificant both without controls ($p=.24$) and after controlling for accident severity, accident location, claimant age, gender, marital status, injuries, seat position, and level of economic loss ($p=.12$).

significant and practically important. For example, the first-stage regression estimates in Column II indicate that shifting from a tax rate of 2% to 2.5% generates an increase in per-capita judicial expenditures of roughly 5%. The F-statistics on the first stage instruments are sufficiently large to alleviate concerns related to weak instruments.

The final rows of Table 3 report our IV estimates of the effect of expenditures on access. Consistent with our findings using OLS, judicial expenditures are positively and significantly related to access. The magnitudes of the IV point coefficients are larger than their OLS counterparts, albeit considerably less precise, and suggest that increasing expenditures by 10% generates a roughly 14% increase in the number of lawsuits filed. Although the fact that the OLS and IV effects estimates are not statistically different precludes strong conclusions, larger IV coefficients would be expected if judicial expenditures are negatively correlated with unobserved shocks that affect the probability of filing lawsuits, a pattern hinted at in Table 3. Moreover, classical measurement error arising due to the estimation of expenditures in some years would also downward bias OLS coefficients relative to IV.

In a probit framework, examining the coefficient on the residuals from the first-stage regression when included in the second stage also provides a simple means of testing for the exogeneity of expenditures (Rivers and Vuong 1988). The p-values for our exogeneity tests are marginally significant and range between .10 and .15, providing some justification for the use of tax rates as instruments. Overall, the IV analysis provides evidence consistent with our previous conclusion that resource changes affect access, although effect sizes are somewhat larger.

Table 5 reports results using alternative specifications. We first examine several logical modifications to the sample to ensure that our results are robust. Specification 1 limits the analysis to claimants who were drivers and passengers from vehicles other than the insured

vehicle, eliminating from consideration passengers in the insured vehicle, pedestrians, and motorcyclists, all of whom may have unconventional claiming patterns. Specification 2 eliminates states with no-fault laws from the analysis, as these states may operate differently due to threshold rules that preclude injured parties for suing for less serious accidents. Neither of these sample modifications appreciably affects the results. Specification 3 ensures that our results do not simply reflect the unconventional structure of our data (with claims from different years sampled at different rates) by limiting the analysis to accidents occurring in the year prior to the year of claim resolution. Although we expect the full set of accident year/claim year interactions that are included in all specifications to capture any effects arising due to the sampling scheme, this specification provides an additional verification check. Point estimates of similar magnitude are obtained under this sample restriction, although the smaller sample renders the estimates only marginally significant. In specification 4 we include a full-set of state-specific time trends as additional explanatory variables. For this specification we also obtain results that are very similar to the original results reported in Tables 2 and 3.

We next consider several subsamples of particular interest in their own right. Specification 5 limits the analysis to the 10 most populous states in the country in order to ascertain whether effects differ by population. Effect estimates for the largest states are not appreciably different from the full set of states. Specification 6 limits the analysis to the 8 states that spent an average of \$70 or more per person per year on the court system over the sample period. Overall average per capita yearly expenditures in these states were \$97 as compared to an average of \$54 for the remaining 42 states, an 80% difference. If the judicial production process has diminishing returns to resources, so that additional resources yield small effectiveness gains once a certain basic level of resources has been attained, one might expect a

more modest effect of resources for these states. However, although somewhat imprecise, the point estimates for these states are actually slightly above those for the full set of states, suggesting that resources can expand access even at higher levels of expenditure.

The final specification limits the analysis to individuals who report full-time employment. A priori, it is difficult to know whether effects of expenditures are likely to be larger or smaller for those who are employed. On the one hand, inasmuch as employment proxies for wealth, we might expect wealthier individuals to be less constrained in their ability to purchase representation and pay court costs, in which case low judicial expenditures would be less binding for this group and the effect sizes would be smaller. However, if expenditures primarily affect litigation costs by increasing time costs for potential plaintiffs, then unemployed individuals, who face lower opportunity costs of time, may be less sensitive to such changes. As Table 5 demonstrates, the point estimates of the effects of expenditures increase slightly after limiting the sample to the employed. However, we can not statistically reject equality between these estimates and those for the overall sample.

Given that potential plaintiffs are unlikely to be aware of prevailing conditions in local courts, the most logical mechanism linking expenditures and litigation behavior is through legal representation. In particular, we expect attorneys to be less willing to accept marginal clients when underfunded courts increase the costs of pursuing injury cases. Because our data include information about attorney representation, we can directly examine this possibility by using an indicator for attorney representation as the dependent variable. Results from this analysis are reported in Table 6. We expect to observe a greater likelihood of attorney representation in areas with more generous court funding.

The top row of Table 6 reports estimates obtained using standard probit regression approaches with varying sets of covariates, as in Table 1, and the bottom row reports IV estimates. These results are consistent with the hypothesized mechanism but do not provide strong support due to the imprecision of the estimates. In particular, across all specifications we observe positive point estimates, but only one specification (the OLS estimates in column II) reaches statistical significance at conventional confidence levels. The IV estimates are particularly imprecise. Overall, the evidence presented in Table 6 suggests that more generously funded courts encourage attorneys to represent a wider range of clients, but the evidence on this point is not fully conclusive.

IV. Resources and Insurance Payments

Up to this point our analysis has demonstrated the potential plaintiffs are more likely to file suit and may be likely to obtain attorney representation when judicial resources are plentiful. This evidence is consistent with anecdotal accounts suggesting that underfunding court systems generates congestion and otherwise increases the cost of litigation. Standard models of the litigation process, such as Bebchuk (1984) and Cooter and Rubinfeld (1989), predict that increases in plaintiff costs will decrease the amount of settlement because such cost increases undermine the plaintiff's threat to litigate.¹³ Thus, beyond the primary effect of enhancing access to the courts, increases in court funding may generally raise the amount of compensation provided to injured parties. Given that the number of injury claimants who directly pursue litigation is relatively small, from a policy standpoint any such indirect effects may be an important component of the overall effect of expenditures.

¹³ See Nalebuff (1987) for a model generating alternative predictions.

Our reliance on auto claims data is particularly fortuitous inasmuch as these data allow us to observe payments to injured parties whether or not litigation occurs. We are thus able to directly test whether payments adjust to changes in access in a manner predicted by standard models of litigation. To examine the relationship between judicial expenditures and compensation amounts, we re-estimate equation 1 and its IV analog with the log of the final payment at claim settlement as the dependent variable.

Results of this analysis are reported in Table 7. Across all OLS specifications the effects of expenditures on payments are positive and highly statistically significant. OLS point estimates range between .25 and .32, implying that a 10% increase in per-capital court expenditures increases payment to plaintiffs by 2-3%. The unreported coefficients on the other control variables in these regressions largely accord with intuition in sign and significance. For example, payments are larger for claims involving more serious injuries, more intensive medical treatment, or higher perceived levels of fault among the insured party. As in the analysis of attorney representation, the IV estimates are positive but have large standard errors, achieving statistical significance in only one of the three specifications. However, for these regressions none of the IV exogeneity tests approach statistical significance, suggesting that the more precise OLS estimates may be preferred. Consistent with theory, the data suggest that when courts are better funded and provide wider access to injured parties, potential plaintiffs are able to obtain more generous compensation for their injuries.

V. Conclusions

Although organizations such as the American Bar Association and National Center for State Courts describe a crisis in judicial funding, evidence to date regarding the impacts of judicial

funding on court access has been largely anecdotal. In this paper we provide systematic evidence linking the availability of resources in the judicial system to litigation behavior by injured parties. Our preferred estimates indicate that a 10% increase in judicial budgets at the state level generates a roughly 3% increase in the probability of filing a lawsuit following an auto injury. Consistent with models in which the threat of litigation induces more generous settlement, increases in judicial resources also appear linked to higher average payouts by third-party insurers, with payments increasing by 2-3% for a 10% increase in judicial spending.

Although the effect of judicial expenditures on access to justice at first glance may appear modest, rough calculations suggest that resource constraints may impact a sizeable number of potential plaintiffs. Approximately 2.7 million individuals are injured in auto accidents in the U.S. each year (National Safety Council 2007). Assuming that the IRC data are roughly representative, we expect roughly 15%, or 400,000 of these accidents to result in lawsuits. Based upon our OLS estimates, a 5% change in judicial expenditures nationwide¹⁴ would change the litigation behavior of 6,000 auto claimants each year. If the effects we observe for these auto cases are comparable to the effects for broader classes of civil cases, the count of individuals affected by such resource reductions would be appreciably higher. For example, data from the 2005 Civil Justice Survey of State Courts suggest that auto cases represent about 20% of general civil filings; extrapolating our estimates to the broader set of tort, contract, and real property cases would suggest that a 5% change in judicial expenditures would lead an additional 30,000 plaintiffs to access the courts.

Our analysis in Section IV further indicates that changes in funding may have a broader effect on payments to injury claimants, a phenomenon not previously documented in the

¹⁴ A nationwide 5% increase of would cost roughly \$2 billion dollars, and a 5% shift in expenditures seems well within the realm of plausibility. For example, the National Center for State Courts reported that 12 states reduced judicial expenditures by 5% or more in 2009 (Hall 2009).

literature on judicial access. Recent estimates by the Insurance Information Institute (2009) indicate that that auto insurers paid approximately \$50 billion in compensation for injuries in 2007. Our estimates suggest that, holding other factors constant, a 5% increase in state judicial funding would augment payments to plaintiffs by about \$500 million each year for this single category of tort cases. Given that court expenditures affect not only auto cases but the entire spectrum of civil cases, effects on the balance of payments between potential plaintiffs and defendants are appear potentially large relative to the actual size of budget outlays.

Although this paper empirically demonstrates a link between access to the courts and judicial expenditures for a broad and important class of civil cases, many questions remain. The fact that higher expenditures increase court utilization tells us little about whether current levels of judicial funding are optimal—indeed, many might view increased litigation as a negative by-product of additional court spending. Moreover, although our discussion notes reasons why resources might affect litigation, these data do not provide clear guidance on the specific mechanisms underlying the resource/litigation link. For example, although case delay likely represents an important barrier to access and past research has noted substantial differences across jurisdictions in the speed of cases (Goerdts et al. 1991), whether and how budgets affect delay remains poorly understood. Additionally, the extent to which expenditures affect access for other types of civil cases, in particular those related to family law, remains an open question. Finally, given the wide variation in court expenditures across states, with the most generous states providing more than three times as much funding to their courts on a per-capita basis relative to the least generous states, a natural question arises as to the optimal scale of state court expenditures. At a minimum, for auto torts it appears that the widespread perception that providing more resources to the courts will enhance court utilization is largely accurate.

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Table 1: Summary Statistics

Measure	N	Mean	SD	Min	Max
<i>Outcomes</i>					
Filed lawsuit	94912	0.146	0.353	0	1
Represented by attorney	97755	0.500	0.500	0	1
Compensation received (\$)	98620	7628	19003	1	1050000
<i>State-Level Measures</i>					
Per capita court expenditures	98620	0.072	0.034	0.018	0.245
Tax rate	98620	0.023	0.013	0.002	0.049
<i>State Legal Environment</i>					
Caps on noneconomic damages	98620	0.086	0.280	0	1
Joint and several liability reform	98620	0.681	0.466	0	1
Contingency fee caps	98620	0.276	0.447	0	1
No-fault auto insurance	98620	0.158	0.365	0	1
Pre-judgment interest					
Generally available	98620	0.520	0.500	0	1
On liquidated damages	98620	0.070	0.255	0	1
Judge's discretion	98620	0.008	0.089	0	1
Dependent on settlement offer	98620	0.058	0.233	0	1
<i>State-Level Demographics</i>					
Population	98620	1.26E+07	1.02E+07	453690	3.50E+07
% white	98620	0.818	0.079	0.296	0.989
% black	98620	0.133	0.084	0.003	0.367
% male	98620	0.491	0.007	0.478	0.527
% male aged 15-24	98620	0.073	0.006	0.060	0.100
% female aged 15-24	98620	0.070	0.005	0.057	0.099
<i>State Economy</i>					
Per capita income (thousands of \$)	98620	23.741	5.152	10.802	42.964
Unemployment rate	98620	0.059	0.014	0.023	0.118
Poverty rate	98620	0.138	0.034	0.029	0.272
<i>State Criminal Justice Characteristics</i>					
Violent crime rate per 100000	98620	663.3	244.7	56.8	1244.3
Property crime rate per 100000	98620	4629.6	1043.0	2053.4	7819.9
Per capita police expenditures	98620	0.154	0.052	0.042	0.339
<i>Accident Circumstances</i>					
Occurred in central city	98140	0.328	0.469	0	1
Occurred in suburb	98140	0.217	0.412	0	1
Major damage	60800	0.199	0.399	0	1
Moderate damage	60800	0.540	0.498	0	1
# vehicles involved	97411	2.26	2.64	0	97
<i>Claimant Demographics</i>					
Age	85150	33.5	16.5	0	99
Male	97612	0.453	0.498	0	1
Married	76634	0.396	0.489	0	1

Employed	75869	0.656	0.475	0	1
Degree of fault (%)	97948	5.2	16.5	0	100
Claimed loss (\$)	87335	6452	59164	1	1.09E+07
<i>Claimant Medical Treatment</i>					
None	98620	0.080	0.272	0	1
Hospital	95394	0.542	0.498	0	1
ER physican	98620	0.451	0.498	0	1
Chiropractor	98620	0.298	0.457	0	1
Psychiatrist	98620	0.013	0.115	0	1
<i>Claimant Injury</i>					
None	98620	0.013	0.115	0	1
Burns/scarring	98620	0.022	0.147	0	1
Neck injury	98620	0.656	0.475	0	1
Back injury	98620	0.541	0.498	0	1
Limb fracture	98620	0.031	0.172	0	1
Other fracture	98620	0.036	0.186	0	1
Concussion	98620	0.032	0.176	0	1
Fatality	98620	0.005	0.073	0	1

Table 2: Relationship Between Judicial Expenditures and the Probability of Filing a Suit

Explanatory Variable	I	II	III
Log(Judicial Expenditures/Pop)	.0658 (.0350)	.0575* (.0233)	.0492* (.0206)
<i>Selected State-Level Covariates</i>			
Noneconomic damage caps		-.0157* (.00707)	-.0137* (.00618)
Joint-several liability reform		-.0178* (.00829)	-.0160* (.00763)
Contingency fee caps		-.0152 (.0141)	.00347 (.0170)
Pre-judgment interest generally available		-.0109 (.0151)	-.00878 (.0130)
Log(Unemployment rate)		.00182 (.0246)	.00134 (.0219)
Log(Poverty rate)		-.0241* (.0109)	-.0212* (.00938)
Per capita income (thousands)		7.65E-4 (.00271)	8.82E-4 (.00243)
Violent crime rate per 100000 residents		-4.04E-6 (3.17E-5)	4.84E-6 (2.80E-5)
Property crime rate per 100000 residents		-6.24E-7 (6.60E-6)	-5.15E-7 (5.46E-6)
Log(Police expenditures/Pop)		-.173* (.0741)	-.168* (.0632)
<i>Selected Claim-Level Covariates</i>			
Treated by psychiatrist			.0177** (.00461)
Neck injury			.00599** (.00224)
Paralysis			.00561 (.0186)
Fatality			.00533 (.0170)
Log(Economic loss in dollars)			.0155** (.00108)
Estimated fault of insured party (%)			-5.96E-4** (1.02E-4)
Insured coverage limit above \$500K			.0371* (.0242)
Claimant not in labor force			-.00854** (.00280)
Include state and accident year fixed effects?	Yes	Yes	Yes
Control for state-level covariates?	No	Yes	Yes
Control for claim-level covariates?	No	No	Yes

Notes: The table reports coefficient estimates from probit regressions of an indicator for whether an auto injury claimant filed a lawsuit on the log of state-level per capita expenditures on the court system and additional controls.

The total sample size is 94912 and each column reports results from a unique regression. The mean of the dependent variable is .146. Reported coefficients are marginal effects. All regressions include state and year fixed effects. In addition to the listed covariates, the state-level controls also include log state population; black, white, and male population share; the male and female share of population aged 15-24; indicators for pre-judgment interest availability at the judge's discretion, only in cases with rejected settlements, and only on liquidated damages; an indicator for no-fault insurance availability, and year of claim/data year interactions. The claim-level covariates also include indicators for no treatment and treatment by an ER physician, chiropractor, physical therapist, dentist, and hospital; indicators for no injury, burns, lacerations, scarring, back injuries, sprains, limb and other fractures, concussion, injuries to internal organs, brain, TMJ, or senses; loss of body part, and unknown injury; age and age squared, insured maximum coverage level, and categorical indicators for accident time, impact and injury severity, location, and number of vehicles and claimant sex, marital status, seat position, seatbelt use, employment status. Standard errors clustered on state are reported in parentheses. * denotes statistical significance at the two-tailed 5% level and ** at the 1% level.

Table 3: Additional Specification Checks

Specification	Explanatory Variable	(I)	(II)
1	Log(Judicial Expenditures/Pop) _{t-1}	.0289 (.0874)	.0305 (.0798)
	Log(Judicial Expenditures/Pop) _t	.109 (.0689)	.0971 (.0658)
	Log(Judicial Expenditures/Pop) _{t+1}	-.0455 (.0632)	-.0502 (.0606)
2	Log(Public building expenditures/Pop)	-.0155 (.0145)	-.0131 (.0123)
3	Log(Prison expenditures/Pop)	-.00529 (.0250)	-.00338 (.0238)
4	Log(Legislative expenditures/Pop)	-.00628 (.0178)	-.00436 (.0161)
Include state and year fixed effects and state-level controls?		Yes	Yes
Include claim-level controls?		No	Yes

Notes: This table reports results from three specification checks. Each column and specification reports results from a distinct regression. The first check includes future and past judicial expenditures as additional explanatory variables and otherwise replicates the specifications reported in columns II and III of Table 2. The latter three checks substitute alternative types of expenditures in place of court expenditures as the primary explanatory variable as a placebo test of whether the basic regression specification yields evidence of an effect when none should be present. Sample size for specification 1 is 76249 and for the other specifications is 94914. See notes for Table 2 for more detail regarding the control variables.

Table 4: Instrumental Variables Estimates of the Effect of Judicial Expenditures on the Likelihood of Filing a Suit

	(I)	(II)	(III)
<i>Reduced Form</i>			
Effect of tax rate on Pr(Suit)	2.36* (.985)	2.59* (1.04)	2.14* (.914)
<i>Instrumental Variables</i>			
First Stage: Effect of tax rate on expenditures	12.7** (3.26)	10.5** (2.45)	10.5** (2.44)
F-statistic on instrument:	15.03	18.37	18.37
Second Stage: Effect of expenditures on Pr(Suit)	.189** (.0632)	.243* (.112)	.201* (.0987)
Exogeneity test χ^2 statistic:	2.68	2.55	2.11
P-value:	0.102	0.110	0.147
Include state and year fixed effects?	Yes	Yes	Yes
Include state-level controls?	No	Yes	Yes
Include claim-level controls?	No	No	Yes

Notes: This table reports instrumental variables estimates of the effect of judicial expenditures on the probability of filing suit where the average state tax rate serves as an instrument for judicial expenditures. Each column represents a separate specification. The top row of the table reports coefficient estimates from a reduced-form probit regression of whether a claimant filed suit on the state tax rate. The bottom rows report estimates from the IV first stage and IV probit regressions. See notes for Table 2 for more detail regarding the control variables.

Table 5: Effect Estimates for Alternative Samples

Sample	OLS		IV	
1. Exclude in-vehicle claimants and non-auto claimants (N=82675)	.060*	.052*	.244*	.211*
	(.022)	(.020)	(.102)	(.092)
2. Exclude no-fault states (N=79689)	.075*	.059	.317**	.244*
	(.030)	(.025)	(.135)	(.117)
3. Limit sample to prior year claims (N=34610)	.081*	.059	.251	.179
	(.035)	(.035)	(.151)	(.139)
4. Include state-specific time trends (N=94912)	.050	.039	.358	.292
	(.025)	(.022)	(.211)	(.182)
5. Limit sample to ten largest states (N=49745)	.058	.063	.502	.512
	(.053)	(.048)	(.421)	(.396)
6. Limit sample to states with high expenditures (N=39572)	.066	.055	.603	.482
	(.061)	(.055)	(.392)	(.365)
7. Limit sample to employed plaintiffs (N=42679)	.075*	.069*	.350*	.299*
	(.033)	(.030)	(.142)	(.134)
Include state and year fixed effects and state-level controls?	Yes	Yes	Yes	Yes
Include claim-level controls?	No	Yes	No	Yes

Notes: This table replicates the standard and IV estimates of the effects of court expenditures on litigation for alternative samples. Estimation methods and controls are the same as those used in columns III and IV of Table 2 and columns II and III of Table 3; see notes for those tables.

Table 6: Effect of Judicial Expenditures on Attorney Representation

Estimation Approach	I	II	III
Standard	.0946 (.0601)	.200* (.0789)	.111 (.0696)
IV	.0178 (.187)	.423 (.239)	.324 (.233)
P-value from exogeneity test: H ₀ =Expenditures are exogenous	0.617	0.370	0.341
Include state and year fixed effects?	Yes	Yes	Yes
Include state-level controls?	No	Yes	Yes
Include claim-level controls?	No	No	Yes

Notes: The table reports marginal effect coefficient estimates from probit regressions of an indicator for whether an auto injury claimant utilized an attorney on the log of state-level per capita expenditures on the court system and additional controls. The total sample size is 97755 and each entry reports results from a unique regression. The mean of the dependent variable is .500. Exogeneity tests based on clustered standard errors use the approach described in Wooldridge (1995). See notes for Tables 2 and 3 for additional details regarding the control variables and IV methods.

Table 7: Effect of Judicial Expenditures on Injury Claimant Compensation

Estimation Approach	I	II	III
Standard	.323** (.0995)	.247* (.101)	.261** (.0942)
IV	.581* (.253)	.123 (.330)	.258 (.319)
P-value from exogeneity test: H ₀ =Expenditures are exogenous	0.365	0.698	0.992
Include state and year fixed effects?	Yes	Yes	Yes
Include state-level controls?	No	Yes	Yes
Include claim-level controls?	No	No	Yes

Notes: The table reports coefficient estimates from OLS and IV regressions of the log dollar amount of injury claimant compensation on the log of state-level per capita expenditures on the court system and additional controls. The total sample size is 87335 and each entry reports results from a unique regression. Exogeneity tests based on clustered standard errors use the approach described in Wooldridge (1995). See notes for Tables 2 and 3 for additional details regarding the control variables and IV methods.

Appendix 1: Patterns of Per-Capita Judicial Expenditures by State

