

Do Courts Matter for Firm Value? Evidence from the U.S. Court System*

Stefano Colonnello[†] Christoph Herpfer[‡]

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Abstract

We estimate the impact of U.S. state court characteristics on firm value by exploiting a U.S. Supreme Court ruling that exogenously changed firms' exposure to different courts. We find that increased exposure to more business-friendly courts is associated with positive announcement returns. We find no such association for objective court quality. We confirm that this U.S. Supreme Court ruling impacted firm value through the legal environment channel. We show that this ruling reduced the ability of affected firms to remove cases from certain state courts, and we show that announcement returns are stronger for firms that have high litigation exposure.

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[†]Otto-von-Guericke University and Halle Institute for Economic Research (IWH). E-mail: stefano.colonnello@iwh-halle.de.

[‡]Corresponding author. Emory University, Goizueta Business School, 1300 Clifton Road, Atlanta, GA 30322. E-mail: christoph.herpfer@emory.edu.

The legal system allows parties to settle disputes in nonviolent ways, so it plays an important role in economic development. Research on the intersection of law and economics has identified property rights (La Porta, López de Silanes, Shleifer, and Vishny, 1998) and legal formalism (Djankov, La Porta, López de Silanes, and Shleifer, 2003) as drivers of the differences in legal system quality between countries. We analyze another key link between the legal system and economic development: the impact of different court characteristics on individual firms. The legal system consists of two parts, the law and the courts, which are the institutions responsible for applying those laws. It is well-documented that countries in different stages of development feature legal institutions of varying quality (Hay and Shleifer, 1998) and that specific court proceedings pose significant risk to individual firms (Bhagat, Brickley, and Coles, 1994; Hutton, Jiang, and Kumar, 2015; Karpoff and Lott, 1999). However, less research has examined to what extent the court system creates value for specific firms.

We quantify the effect of the court system on firm value in the U.S. context, and we investigate the specific channel through which this occurs. Efficient and competent resolution of cases reduces legal uncertainty and litigation costs; therefore, the court system's conflict resolution function is one potential channel through which courts impact firm value. Alternatively, courts can have a more business-friendly attitude and increase firm value by moving resources from plaintiffs to shareholders.

Our paper makes two key contributions. First, we disentangle the effect of courts from confounding factors both at the state level (e.g., variation between laws) and at the case level (e.g., details specific to a single case). We exploit a U.S. Supreme Court ruling as an exogenous shock to firms' exposure to different state courts to quantify the link between court characteristics and shareholder value. We find strong evidence that differences between courts matter for firm value, even in the highly developed legal system of the U.S. Using a ranking of state courts produced by the U.S. Chamber of Commerce, we find that firms that are exposed to state courts in the top tercile of this ranking experience a positive abnormal return of 0.47% on the day of the ruling compared to firms exposed to courts in the bottom tercile.¹

¹The U.S. Chamber of Commerce is a vocal business lobbying organization that advocates for legal reform. Since 2002, the U.S. Chamber of Commerce has published rankings of U.S. state courts based on surveys of large U.S. corporations. According to the *2015 Lawsuit Climate Survey*, 75% of surveyed corporate lawyers responded that the quality of a state's court system was likely to impact specific business decisions, such as where to conduct business. In its pursuit of legal reform, the U.S. Chamber of Commerce does not merely rank courts; it actively supports judges friendly to its agenda through donations and advertisements (Champagne, 2001, 2004). Candidates supported by the U.S. Chamber of Commerce in this manner prevailed in 36 out of 40 judicial elections between 2000 and 2004 (Shepherd,

Our second contribution is to identify the economic channel that links court characteristics to firm value. We present evidence that, in the U.S., courts' attitude toward businesses, rather than their efficiency, drives their impact on firm value. We further show that courts have the strongest effect on firm value when firms have more exposure to litigation because they operate in industries with high legal/operational risk or high firm-specific litigation intensity.

Previous studies of the impact of the legal system have mainly taken two forms: cross-country comparisons of aggregate data (see, e.g., [Glaeser and Shleifer, 2002](#)) or event studies around the announcement (or resolution) of lawsuits (see, e.g., [Bhagat, Bizjak, and Coles, 1998](#)). Both approaches are not suitable for our goal of estimating the impact of courts on firm value. The key challenge is to isolate the impact of courts from the impact of the law itself. On the one hand, any study that compares measures of court quality across countries also inadvertently compares different laws, firms, or sociopolitical environments. On the other hand, event studies that focus on the impact of lawsuits on firm value cannot distinguish between the role of courts as a whole and the merits of each specific case.

We provide a solution to the above identification challenges. We link the impact of the court system across various U.S. states to firm value while keeping the applied laws constant. We obtain identification from the two-tiered structure of the U.S. court system (i.e., state and federal courts) combined with an event study around a U.S. Supreme Court ruling.

To understand our proposed solution, consider an ideal experimental setting in which identical firms (thus facing identical lawsuits) are randomly assigned to different courts. These different courts then apply the exact same laws. Since firms, lawsuits, and laws are identical in such a setting, the only difference between these two firms lies in their respective courts. A simple event study of stock price reactions could then identify the impact of courts on firm value. Such an ideal setting must meet two key requirements: i) firms, laws, and lawsuits must be identical; and ii) firms are randomly assigned to their respective courts.

A special feature of the U.S. court system creates a setting that fulfills the first requirement, namely that firms and laws are the same across two different court systems. The U.S. judiciary is composed of two tiers: federal courts and state courts. Most cases are handled by state courts, but there are important exceptions. If a civil lawsuit features

[2009](#)). [Choi, Gulati, and Posner \(2009\)](#) provide further examples of the ranking's impact on both politicians and judges. See *2015 Lawsuit Climate Survey*, available at <http://www.instituteforlegalreform.com>.

parties from different U.S. states, their so-called “diversity of citizenship” grants federal courts jurisdiction over the case. *Diversity jurisdiction* has been a long-standing feature of U.S. law, rooted in the Constitution and practiced in various forms since the Judiciary Act of 1789.

Importantly, in diversity cases, federal courts are bound to apply exactly the same laws as the state court in the state the original case was based in. As a result, the difference between trying a case in federal or state courts does not lie in different laws, but solely in differences among courts. Diversity jurisdiction therefore allows us to evaluate the impact of courts on firm value while controlling for applicable laws.

However, a simple comparison of stock market reactions to cases assigned to either state or federal courts would not fulfill the second requirement of the idealized setting. The assignment to courts must be random. Whether a case is tried in a federal or a state court is endogenously determined, and both the plaintiff and the defendant aim to steer cases into favorable courts. Lawyers refer to a party’s attempt to move cases between courts as *forum shopping*. Forum shopping is pervasive, and the outcome of a trial can be heavily influenced by whether one party is successful in moving the trial to the party’s preferred forum (Clermont and Eisenberg, 1994). Therefore, the endogeneity of court choice is a challenge for any attempt to examine the effect of courts on legal outcomes based on actual cases.

To resolve the endogenous matching of firms and courts, we exploit a U.S. Supreme Court ruling that caused an exogenous variation in the ability of parties to move cases between forums. In the case of *Hertz Corp. v. Melinda Friend (Hertz)*, the U.S. Supreme Court ruled on the conditions under which a corporation is deemed a citizen of a state for the purpose of diversity jurisdiction.² On February 23, 2010, the Court decided that a corporation should be deemed a citizen of at most two states: the state of incorporation and the state in which its officers guide the firm’s daily activities (i.e., its “nerve center”). The justices ruled that this corporate nerve center is at the firm’s headquarters. After the ruling, firms could reliably claim diversity of citizenship in state courts that were neither in their headquarters state nor in their state of incorporation. At the same time, firms that were only marginally active in their headquarters state lost the ability to claim diversity of citizenship when sued in that state. Those firms were therefore “pinned” into their headquarters’ state courts through the ruling. We exploit this setting as an exogenous shock to those firms’ court environments.

We begin our analysis by performing a stock price event study around the ruling in

²Case 559 U.S. 77 (2010).

Hertz.

We focus on firms that lost the ability to move cases into a federal court in their headquarters state, (i.e., firms with small operations in their headquarters state). To identify such firms, we rely on a text-based measure of geographic dispersion of corporate activities across states, as in [Garcia and Norli \(2012\)](#). Using both an academic ranking of courts by [Choi et al. \(2009\)](#) and a ranking produced by the U.S. Chamber of Commerce, we find that exposure to different courts is systematically connected to firm value. A firm exposed to a state court in the top tercile of the U.S. Chamber of Commerce ranking experiences a positive abnormal return of 0.47% compared to a firm exposed to a court in the bottom tercile. This translates into a \$21.39 million increase in equity value for the median firm in our sample.³

Due to its construction, the U.S. Chamber of Commerce ranking reflects courts' business attitudes more than their objective quality. Therefore, we interpret the differing effects of these two rankings as evidence that a business-friendly court increases firm value, while a merely efficient court does not. We further document that our findings are stronger for firms that have higher litigation exposure, which underscores the causal nature of the link between court exposure and firm value.

We then provide evidence that the ruling in *Hertz* actually had a substantial effect on the number of lawsuits faced by firms in specific courts. We collect data on the lawsuits filed against firms in federal courts. These data allow us to identify cases that were filed in federal court due to diversity of citizenship jurisdiction. As firms were pinned into their headquarters states, their ability to claim diversity of citizenship, and subsequently remove the case into federal court, should decrease. We find that the ruling in *Hertz* had the predicted effect of reducing the number of diversity lawsuits faced by firms in their headquarters states.

As a final step, we investigate whether companies reacted to the ruling by shifting their physical operations. We find that firms with high exposure to the legal system indeed initiated operations in states that had less business-friendly courts after the ruling in *Hertz* allowed them to reliably move cases into federal court.

Our paper contributes to the literature on law and finance, in particular to the research on the link between court quality and firm value. Particularly relevant to our study is the work by [Brown, Cookson, and Heimer \(2017\)](#), who investigate whether jurisdiction through U.S. state courts as opposed to native American tribal courts impacted economic development in Native American reservations. They find that counties with

³Measured in 2010 dollars.

externally imposed legislation through state courts, rather than tribal courts, exhibit stronger per capita income and more developed credit markets. We add to their findings by investigating the differential impact of court characteristics inside the U.S. state court system, rather than between state courts and tribal courts, and by distinguishing between the role of business friendliness and efficiency of courts. While differences between state courts are likely to be smaller than those between state courts and tribal courts, state courts litigate a larger number of cases. Given the larger role of state courts compared to tribal courts, the aggregate effect of small differences between them are still having a sizable impact on the aggregate despite the smaller between-court differences. Second, by exploiting a time-series shock to court exposure, our setup allows us to control for firm-level differences.

A number of authors examine the role of courts for specific types of litigation. [Wilson \(2016\)](#) focuses on shareholder lawsuits, showing that exclusive forum provisions adopted by the board of directors, which define the venue for shareholder litigation, positively affect the value of corporations.⁴ [Carletti, Colla, Gulati, and Ongena \(2017\)](#) use the mandatory introduction of collective action clauses for eurozone sovereign bonds and find that investors value these clauses the most in countries with strong legal systems that allow them to enforce those clauses. Other papers have used important case-law decisions to show that the interpretation of existing laws and the creation of new legal standards impacts firm value and policies ([Johnson, Nelson, and Pritchard, 1999](#); [Becker and Strömberg, 2012](#); [Grinstein and Rossi, 2016](#); [Vig, 2013](#); [Karpoff and Wittry, 2018](#)). Our study contributes to this literature by going beyond a change to specific laws or the enforcement of one type of contractual provision, and instead looking at the impact of the legal system more broadly.

Another strand of the literature has focused on the impact of specific aspects of individual courts rather than broad systematic differences across court systems. A set of authors investigate the role of judges as part of the litigation process both theoretically ([Gennaioli and Rossi, 2010](#)) and empirically ([Chang and Schoar, 2013](#)). Specific frictions such as high case loads can lead to congestion of courts and lower efficiency ([Chemin, 2010](#); [Iverson, 2017](#); [Ponticelli and Alencar, 2016](#)), whereas courts that specialize in business law can lead to increases in firm value ([Dammann, 2017](#)). While judges and specific court attributes are an important part of the litigation process, we add to this literature

⁴Importantly, forum provisions in specific contracts do not rule out removal of cases under diversity of citizenship. See the U.S. Supreme court's decision in *Stewart Organization, Inc. v. Ricoh Corp.*, 487 U.S. 22.

by investigating a broader set of court characteristics and, in effect, comparing pools of judges across different courts rather than individual judges within a single court. [Bennett, Milbourn, and Wang \(2018\)](#) use a text based measure of corporations’ general legal risk and find that higher legal risk impairs investments, a finding that is consistent with our result that firms with higher litigation risk react more sensitively to changes in court exposure.

A very prominent example of the interplay between legal institutions and corporate behavior is that the small state of Delaware is the incorporation state of more than 50% of U.S. corporations ([Daines, 2001](#)). The causes and consequences of this phenomenon, the so-called Delaware effect, are widely debated. Some authors argue that Delaware’s sophisticated case law on corporate issues and its specialized business courts are key explanations for its popularity ([Romano, 1985](#); [Daines, 2001](#)). Other factors that received significant attention are the effect of different regulations regarding merger and acquisition activity or liability of directors ([Dodd and Leftwich, 1980](#); [Bebchuk and Cohen, 2003](#); [Heron and Lewellen, 1998](#)). The evidence, however, is mixed due to the endogenous nature of the decision whether to incorporate in Delaware. Our finding that court characteristics have a significant impact on firm value helps explain the Delaware effect.⁵

I. Methodology and Data

A. Institutional Background and Identification Strategy

Our identification strategy consists of three components. First, we exploit a feature of the U.S. court system which allows cases to be tried under institutionally distinct court systems but considering the exact same laws. Second, since selection into either of those two court systems is endogenous, we use a decision by the U.S. Supreme Court in *Hertz* that changed the rules under which cases are assigned to the different courts. Finally, we exploit cross-sectional variation in firm’s litigation exposure to underscore the causal channel between changes in the court environment and firm value.

⁵From a methodological perspective, the effect of court decisions has been the subject of various studies in economics and finance. For instance, court decisions have been used as exogenous shocks to study the role of takeover provisions ([Cohen and Wang, 2013](#); [Cheng, Nagar, and Rajan, 2005](#)), alternative corporate governance mechanisms ([Grinstein and Rossi, 2016](#)), conflicts between shareholders and creditors ([Becker and Strömberg, 2012](#)), and shareholder litigation risk ([Chu, 2017](#)). Moreover, many studies have used variation across state laws as an identification strategy (e.g., [Bailey, 2006](#); [Bertrand and Mullainathan, 2003](#); [Jayaratne and Strahan, 1996](#)). Our approach differs since it does not rely on state-level differences in laws but on the differential interpretation and application of laws across states as a result of differing court characteristics.

The questions of which laws are to be applied and which courts are in charge of the case are inherently intertwined. The outcome of a case brought, for example, in Austin under Texas law will be determined by different laws, legal precedents, and courts than that of a similar case brought in Seattle under Washington law. There is, however, a peculiarity of the U.S. judicial framework that allows for the separation of court systems and laws. The U.S. constitution (Article III, Section 2) grants federal courts the right to try civil cases as long as the involved parties are from different states: “The judicial power [of federal courts] shall extend to all cases [...] between citizens of different states”.⁶ In practice, this rule (diversity of citizenship) means that civil cases in which the two parties are from different states can be heard by federal courts. The Founding Fathers were afraid that a state court trying an out-of-state citizen lacked objectivity. The federal courts were to act as a neutral forum to settle interstate disputes.

This dual court system allows us to disentangle the role of court characteristics from that of laws because federal courts are forced to apply exactly the same laws as the state court in which the case would otherwise have been brought. This so-called “Erie Doctrine” is a longstanding legal rule, first established in the case of *Erie Railroad Co. v. Tompkins* in 1938.⁷ As a result of the Erie Doctrine, the difference between a case tried in state or federal court lies solely in the court system itself. There are different judges, juries and procedural rules between the two systems, but the same laws.

We exploit this dual court structure to isolate the impact of court attributes from that of laws. Since the choice of federal or state courts is endogenous, a simple ex-post comparison of cases tried in the two court systems cannot identify the causal impact of courts. We resolve this challenge by exploiting the U.S. Supreme Court’s decision in *Hertz* as an exogenous shock to the ex-ante ability of firms to move cases between federal and state courts. In the case of *Hertz*, the U.S. Supreme Court had to decide under which circumstances a corporation is allowed to remove cases from state into federal court.

At the time, there were two competing schools of thought about corporate citizenship. Most judges subscribed to the so-called “major operations” idea. Under this interpretation, a corporation was a citizen of those states where it conducted a large fraction of its operations (*operations test*). Different courts had adopted varying definition of “major operations”, considering various combinations and cutoffs for employees, revenues or assets to determine whether operations were “major”. The competing view, held by the

⁶Implemented as “28 U.S. Code 1441 - Removal of civil actions”. The rule was later interpreted as requiring “complete diversity”: no member of the defendants can be a citizen of the same state as any of the plaintiffs (546 U.S. 81). See http://www.archives.gov/exhibits/charters/constitution_transcript.html

⁷Case 304 U.S. 64 (1938).

federal courts of the 7th Circuit, was that a firm was a citizen only of the state in which its “nerve center” is located: the place where managers direct the corporation’s day to day operations, generally considered to be the headquarters state (*nerve center test*).⁸

In *Hertz*, the car rental firm Hertz, headquartered in New Jersey, was being sued by its former employee Melinda Friend in California state court over an alleged breach of employment laws. Hertz petitioned to remove the case into federal court, claiming diversity of citizenship. The petition was rejected on the grounds that between 16% and 20% of Hertz’s employees, revenues, and transactions were stemming from its California operations. Hertz appealed this decision and on February 23, 2010, the U.S. Supreme Court ruled in favor of Hertz. In particular, the U.S. Supreme Court ruled that a corporation should only be considered a citizen in its state of incorporation and the state in which its day to day business activities are guided, namely following the nerve center doctrine. As a result of the ruling in *Hertz*, firms with small operations in their headquarters state that had the ability to claim diversity of citizenship based on the operations test in the past lost their ability to do so, and hence experienced an increase in their exposure to their headquarters state courts. We describe the ruling and its effect on the accessibility of federal and state courts in more detail in [Appendix A](#).

Figure 1 visualizes our identification strategy. Assume there are two identical firms, A and B , located in two different states i and j with corresponding court qualities X and Y . Before the ruling in *Hertz*, they were both able to claim diversity of citizenship in their respective headquarters states because they have a small fraction of their operations located there (for instance, less than 15% of operations). After the ruling in *Hertz*, both firms lost their ability to claim diversity of citizenship and hence experienced an increase in exposure to their respective state courts i and j . Whereas the ruling in *Hertz* affected the ability of corporations to move cases in a vertical fashion between state and federal courts, we can compare the relative impact it had on different firms to learn about horizontal differences between different state court systems. The relative impact on court quality between the two firms in this example is $X - Y$, or the difference in court quality between the two states and is independent of the quality of the federal court system.

The main hypothesis we test concerns the way in which court characteristics, such as court quality, impact corporate valuations. The court system can impact firm value through two main channels. The first channel is court efficiency. If courts predictably apply the law and arrive at decisions quickly, legal uncertainty and transaction costs are lower and equity value should be higher. We will refer to this channel as objective

⁸A corporation is always a citizen of its incorporation state independently of its operations.

court quality. The second channel is court and jury attitude towards business. If judges have a pro- or anti-business stance, this can significantly alter the course of a trial. The same logic holds even more strongly for juries. We will refer to this channel as business attitude. Our main goal is to test to which extent it is (objective) court quality and to which it is (subjective) business attitude that drives the impact of courts on firm value.

Hypothesis 1: *Firm valuations are lower for firms with exposure to worse courts.*

Moreover, some firms might be involved more often in lawsuits than others. Firms that rarely face lawsuits will be less sensitive to court characteristics than those who regularly do. We therefore test the following cross-sectional hypothesis.

Hypothesis 2: *Firms with a bigger exposure to the legal system should react more to court quality.*

B. Data

We obtain accounting and daily stock return data for all public, non-financial U.S. firms from the CRSP-Compustat merged database over the period 2006 through 2014.⁹ We obtain data about nonfatal occupational injury and illness rates from the Injuries, Illnesses, and Fatalities (IIF) program of the Bureau of Labor Statistics (BLS) and industry-level information regarding labor intensity from the NBER-CES Manufacturing Database. We winsorize all variables at 1% and 99% percentile. All dollar amounts are expressed in 1990 dollars.

We focus on firms with available stock price data on the day of the ruling in *Hertz*, February 23, 2010 and use Fama-French four factor adjusted abnormal returns (ARs) for the event study.¹⁰ The U.S. Supreme Court released its decision at around 10 a.m. EST,

⁹Since Compustat backfills information on headquarter and incorporation states, we add information on each firm’s historic headquarters and incorporation states starting from 2007 from CRSP’s COMPHIST table. We include firm-years with non-missing sales, total assets, common shares outstanding, share price, and calendar date. In addition, we require firms to report total assets in excess of \$10 million in 1990 dollars. We use daily return risk factors from Kenneth French’s data library. These risk factors are available on Kenneth French’s website: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

¹⁰More precisely, we estimate regressions of each firm’s daily stock returns—excluding dividends as in Larcker, Ormazabal, and Taylor (2011)—over the estimation window on the three Fama and French (1993) risk factors (excess market return, small minus big, high minus low) and a momentum factor. We use a one-year estimation window from July 1, 2008 to June 30, 2009. We winsorize estimated factor loadings at 5% and 95% as in Becker, Bergstresser, and Subramanian (2013). We then compute event-day ARs, $CAR[0, 0]$, and cumulative abnormal returns (CARs) between day $n1$ and day $n2$, $CAR[n1, n2]$, as residuals of the estimated factor model for each firm in our sample.

so in our baseline tests we use event-day ARs because they should already incorporate the effect of the ruling in *Hertz*.¹¹

As an important step in our identification strategy we need to determine the exposure of firms to lawsuits in the various states. A firm with significant operations in a given state is likely to also experience exposure to this state’s courts, either through employees, business partners or customers. To compute our measure of corporate geographic dispersion, we collect data from annual reports filed with the SEC’s EDGAR database. Similar to Garcia and Norli (2012) and Bernile, Kumar, and Sulaeman (2015), we construct our measure of geographic dispersion by searching certain segments of 10-K annual report filings for the names of U.S. states and rank them according to their frequency of occurrence.¹² We use these state counts to compute our baseline measure of corporate geographic dispersion: *Out of HQ state operations (%)* captures the fraction of non-headquarters state counts to total state counts.¹³ A detailed description of the construction of the measure can be found in Appendix B.

The other key component of our analysis is a measure of various state court characteristics. We employ three measures of court characteristics. First, we use the 2010 ranking of state courts published by the U.S. Chamber of Commerce. The ranking is constructed by surveying legal counsels of large U.S. corporations about various aspects of state court systems. The data for the 2010 ranking were collected between October 23, 2009 and January 21, 2010, meaning that the ranking is well suited to describe the state of the legal system on the day of the ruling in February 2010.

Despite the ranking’s large public profile, corporate lawyers are not an unbiased audience to survey and the ranking has been criticized in the past as reflecting not objective court quality, but rather a pro-business stance (see, e.g., Eisenberg, 2009). We therefore contrast the U.S. Chamber of Commerce ranking with the academic ranking by Choi et al. (2009) that uses measures of objective court efficiency, such as the rulings per judge or citation counts of rulings in other states.¹⁴

¹¹See B. Kendall, “High court rules for Hertz in California case”, Wall Street Journal, February 23, 2010 (<http://www.wsj.com/articles/SB10001424052748704188104575083371529042754>).

¹²If the number of state mentions is missing for a given firm-year, we interpolate it by taking the average between the counts in the year before and after the year with missing data. We do not interpolate iteratively and remove all the observations for which state counts are still missing after interpolation.

¹³The ruling reduced firms’ exposure to states in which they were not headquartered. An exception is a firm’s state of incorporation: the ruling had no effect on exposure to incorporation state courts. For the sake of brevity, we will use the expression “non-headquarters state” to mean states that are neither headquarter nor incorporation states.

¹⁴We make the two rankings comparable by removing U.S. territories and the District of Columbia from the academic ranking, as they are not featured in the U.S. Chamber of Commerce ranking. In addition,

In our analysis, we measure each state’s court quality by its ordinal rank, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (see [Appendix C](#)).

One potential concern might be that the U.S. Chamber of Commerce ranking could capture both business attitude as well as objective court efficiency to varying degrees. We therefore orthogonalize the two rankings to extract a pure measure of courts’ business attitude.¹⁵ A key aspect of our setup is that we *do not* assume that the U.S. Chamber of Commerce ranking captures objective court quality. On the contrary, our setup exploits the fact that the U.S. Chamber of Commerce ranking is a measure of business attitude that is different from objective court efficiency.

Finally, since the ruling in *Hertz* affects corporations through changes in the likelihood with which they can claim diversity of citizenship, the ruling should affect the number of diversity of citizenship cases faced by corporations. We verify this validity condition for our quasi-natural experiment using machine-collected data. More specifically, we obtain data on the number of lawsuits in which the name of each sample firm is among the defendants from a website which publishes data on all lawsuits filed in federal courts. This website obtains its data directly from the Public Access to Court Electronic Records (PACER) platform. We use the location of the federal court in which the case is filed to determine the state in which the case is located.¹⁶ This procedure leaves a total of 109,677 lawsuits. We classify cases as driven by diversity of citizenship if the word “diversity” appears in the description of the cases’ cause of action. While there is no comprehensive data source of all state court cases, this setup allows us to obtain a measure of the

the academic ranking assesses the civil and criminal courts of Texas separately. We only consider the civil court’s ranking, since it is more relevant for the kind of lawsuits usually faced by corporations.

¹⁵We decompose the U.S. Chamber of Commerce ranking into one part that is explained by the academic ranking, and an unexplained part. Econometrically, the residuals of a regression of the U.S. Chamber of Commerce ranking on the academic ranking are the unexplained variation in the U.S. Chamber of Commerce ranking and orthogonal to the part of the ranking that can be explained through the academic ranking. We refer to these residuals as “business attitude”. By orthogonalizing the two rankings, we obtain a measure of court business friendliness that is unrelated to court efficiency. The U.S. Chamber of Commerce and academic rankings are only weakly (negatively) correlated, and the resulting measure is therefore highly correlated with the U.S. Chamber of Commerce ranking.

¹⁶For example, a case in the Illinois Northern district Court would be assigned to the state of Illinois, since under U.S. Code 28:1441 cases that get removed from state court are assigned to “the district and division embracing the place where the action is pending”. We also only consider lawsuits in which the firm acts as the defendant since claiming diversity of citizenship is the sole right of the defendant. We exclude appeals cases since diversity can only be claimed at the initial lawsuit. We also exclude asbestos-related cases to avoid that this one frequent type of lawsuit drives our results. Our analysis is robust to including those two case types.

number of cases that end up being removed from state courts based on diversity.¹⁷ We then consider the intersection between the lawsuit database and the CRSP-Compustat merged database between 2007 and 2014.

Table I presents summary statistics. Panels A through C focus on the data used in our event study on the ruling in *Hertz*. Panel A reports the mean of two measures of geographic dispersion by sample-quartile. We observe non-trivial variation in these measures between the bottom and top quartiles. For instance, the average firm in the bottom quartile by *Out of HQ state operations (%)* has 18.5% of operations outside its headquarters state. By contrast, the average firm in the top quartile has 85.4% of operations outside its headquarters state. Similarly, the lowest quartile of firms mention three different states in their annual report compared to 23 states for the highest quartile of firms. Panel B reports the mean of February 23, 2010 ARs by quartile of the same geographic dispersion measures. Event-day ARs ($CAR[0,0]$) are generally decreasing in these measures of geographic dispersion. Panel C provides unconditional summary statistics for all the main variables used in our analysis.

Panel D of Table I reports summary statistics about the number of lawsuits before and after the ruling in *Hertz* of 2010, both at the firm-state and state level. For each firm, we only consider those states in which the firm faces at least one lawsuit between 2007 and 2009, i.e., before the ruling in *Hertz*. Diversity lawsuits constitute 18.9% of all lawsuits brought against these corporations. Diversity cases are thus slightly less common in our sample compared to the population of all federal lawsuits, where diversity of citizenship is at roughly 30% of all civil cases filed in the federal circuit as of 2016.¹⁸ The fraction of diversity cases drops significantly during our sample, with the average going from 28% before 2010 to 7% in 2010 and after. This coincides with a decline in the fraction of diversity lawsuits on the aggregate level that was, however, less severe.

Detailed definitions of the variables are given in Appendix Table D.1.

¹⁷Panel B of Table IX shows that our results are the same with a more broad classification of diversity that flags cases as driven by diversity of citizenship if the corresponding law number from the U.S. federal code appears in the cause of action field. We winsorize the number of lawsuits at the 99% level.

¹⁸The comparison between our dataset and the aggregate statistics is, however, not straightforward. Our sample only constitutes less than 10% of all civil cases filed in federal courts. There are a number of potential explanations why diversity cases are underrepresented in our sample compared to the aggregate statistics. First, our analysis excludes financial firms, yet a large number of diversity cases is related to insurance firms. In addition, all cases involving federal law are tried at the federal court level and can therefore by definition not be subject to diversity of citizenship jurisdiction. Federal laws include areas such as environmental law that are arguably more important to corporations than to the general population and, hence, contribute a larger fraction of lawsuits for those corporations. See aggregate statistics at <http://www.uscourts.gov/statistics-reports>.

II. The Effect of *Hertz* on Firm Valuation

Since the ruling in *Hertz* made it more difficult to claim diversity of citizenship in headquarters states for firms with small operations in them, the ruling effectively increased the exposure of those firms to their headquarters state court system. To test Hypothesis 1, i.e., whether court characteristics impact firm value, we carry out an event study on the day of the ruling in *Hertz*. We focus on firms which were previously able to move cases to the federal system because they had very small operations in their headquarters state. These firms were unable to do so after the ruling in *Hertz* and were therefore “pinned” into their headquarters state’s court system. If the quality of state courts matters for firm value, firms pinned into states with worse courts will lose value compared to firms which get pinned into better courts. Importantly, this setup allows us to compare different state courts while controlling for potential differences between state and federal courts.

We limit the sample to 267 firms for which the headquarters state comprises less than 15% of their overall operations and those headquartered outside the 7th Circuit (Illinois, Indiana, and Wisconsin). We choose the 15% cutoff as it is between the threshold of 18% at which *Hertz* was found a California citizen and the 13% at which Best Buy was found not to be a citizen. The reason for eliminating firms headquartered in the 7th Circuit is that courts there had applied the nerve center test even before *Hertz*. Those firms are not suitable for our test since we estimate cross-sectional variations in treatment based on differences in court quality. By contrast, all firms with their headquarters in a state outside the 7th Circuit were able to claim diversity of citizenship when sued in their headquarters state pre-*Hertz* as long as their operations in that state were small enough.

To compare the differential impact of court characteristics on firm value, we need to quantify the characteristics of the court system in each firm’s headquarters state. We do so using two rankings of state courts: the 2010 U.S. Chamber of Commerce ranking and the academic ranking by [Choi et al. \(2009\)](#). In our main specification, we estimate different variations of the following model:

$$CAR[0, 0]_i = \alpha + \beta \cdot \text{Ranking} (HQ \text{ state})_{i,s} + \epsilon_i, \quad (1)$$

where $CAR[0, 0]_i$ is the Fama-French four factor adjusted cumulative abnormal event day return of firm i , and $\text{Ranking} (HQ \text{ state})_{i,s}$ is the position of firm i ’s headquarters state s in the different rankings. The ruling in *Hertz* denied firms the ability to escape state courts in their headquarters state. If state court quality matters, β should be negative

for all ranking variables, since a higher ranking number corresponds to a worse court system.¹⁹

Table II reports estimated coefficients from these regressions. Column 1 shows that unconditional ARs on February 23, 2010 are negative at -0.64% and statistically significant. The market wide negative event day returns are driven by disappointing consumer confidence numbers released that day.²⁰ Column 2 tests whether event day returns are systematically related to increased exposure to certain courts. The main explanatory variable for abnormal returns in this specification is the ranking of each firm's headquarters state in the U.S. Chamber of Commerce ranking. The estimated coefficient is -0.029 and statistically significant at the 1% level. As predicted by Hypothesis 1, firms that experience increased exposure to state courts ranked poorly by the U.S. Chamber of Commerce experience significantly worse event day returns than those with increased exposure to courts ranked favorably. Column 3 repeats the exercise using the academic ranking as a measure of court quality. The estimated coefficient is both statistically and economically insignificant. The coefficients remain qualitatively unchanged once we include both rankings in the specification in column 4. This result suggests that there is no systematic relationship between increased exposure to courts ranked positively in the academic ranking and firm value.

The value of courts for firms can stem from two economic channels: objective efficiency and business attitude. To distinguish between the two channels, we exploit differences between the two rankings. The academic ranking, which is based on citation counts and productivity per judge, captures mainly the efficiency aspects of courts. On the other hand, the U.S. Chamber of Commerce ranking could reflect both efficiency and attitude considerations. To separate the two channels, we orthogonalize the two rankings. Column 5 reports estimates of a regression of ARs on the resulting measure of business attitude. We find a negative point estimate of -0.028, suggesting that increased exposure to courts with an unfavorable business attitude significantly lowers firm value. The observed low degree of commonality between the two rankings, reflected by the high correlation between *Chamber of Commerce ranking (HQ state)* and *Business attitude (HQ state)*, suggests that the U.S. Chamber of Commerce ranking mainly proxies mainly for court business friendliness. Thus, in the remainder of the paper, we only report results for the U.S. Chamber of Commerce ranking and the academic ranking.²¹

¹⁹We code the best ranked state as having rank 1 and the worst ranked state as 50.

²⁰To rule out that this consumer confidence shock drives our results we run a placebo test, the result of which are displayed in Table V.

²¹Appendix Table E.1 presents robustness about the cutoff of non-headquarters state operations used

Our tests exploit a single event and, as a consequence, we need to address cross-correlation of stock returns (Schwert, 1981; Fama and French, 2000). We account for cross-correlation in the systematic component of stock returns by adjusting returns for the three Fama and French (1993) risk factors and a momentum factor. Unless otherwise noted, we also correct for cross-correlation in the idiosyncratic component by clustering standard errors at the industry-level to allow for arbitrary correlation among errors inside industries (Cohen and Wang, 2013). Another standard technique to account for cross-sectional correlation is to form portfolios of securities. Accordingly, in Table III, we repeat the analysis in Table II using equally-weighted portfolios of stocks in states with at least two observations. Although the number of observations shrinks to just 28 state portfolios, all results retain their economic and statistical significance. This result also alleviates the concern that our result may be driven by a contemporaneous shock to the regional economy in a state with many corporations headquartered there.

Our results show that the stock price reaction to *Hertz* reflects the characteristics of the headquarters state courts. Firms pinned into state courts which are highly ranked by the U.S. Chamber of Commerce experience higher event-day ARs. We find no evidence that differences in efficiency between state courts impact firm value. Instead, it seems that state courts' attitude towards business explains the different stock price reactions to *Hertz*.²²

A. *Economic Significance*

To illustrate the economic significance of the baseline result on firm value, consider a positive shock to the state court ranking from the 32nd (bottom tercile) to the 16th (top tercile) position of the U.S. Chamber of Commerce ranking. Based on the estimates

to define the sample of firms, the inclusion of industry fixed effects, and the length of the event window. Appendix Table E.2 evaluates to sensitivity of our baseline results to winsorizing event-day ARs at different levels. Our results remain robust in each case. Appendix Table E.3 investigates the role of the different subrankings used to build the U.S. Chamber of Commerce and the academic ranking. If the Chamber of Commerce ranking was capturing both business attitude and objective court quality, event day ARs should load differently on different subrankings. In the data, we do however find a very strong correlation across all subrankings, which reinforces the idea that the U.S. Chamber of Commerce ranking is largely a measure of court business friendliness.

²²These results do not imply that objective court quality is irrelevant for firm value. There are two potential alternative explanations. The first explanation is that objective court quality inside the U.S. may not vary much. After all, state court systems have a very similar legal and administrative tradition, the same legal origins, and recruit their judges from a pool of similarly qualified candidates. A second potential explanation why court efficiency does not affect firm value is that it aids both plaintiffs and their counterparts symmetrically. A more efficient court system does not help the firm win more cases, unlike court attitude which is a zero sum game.

in column 2 of Table II, such a change is associated with a 0.47% increase in equity value. For the average (median) firm in our estimation sample, this effect corresponds to an increase in market capitalization of \$12.54 (\$2.32) million.²³ The 0.47% increase in equity can be represented as a perpetuity equal to the the expected annual savings from facing lawsuits in a more favorable forum:

$$\Delta Equity = \frac{1}{r} \times \Delta No. \text{ state lawsuits} \times E[\Delta Equity|Lawsuit] \times \Theta, \quad (2)$$

where r is the required rate of return on equity, $\Delta No. \text{ state lawsuits}$ is the number of additional lawsuits per year that are litigated in state court as a result of *Hertz*, $E[\Delta Equity|Lawsuit]$ is the expected loss of equity value conditional on facing a lawsuit, and Θ is the difference in the impact between the two courts.

By using empirically observed values of the quantities in this equation we can obtain a rough, back-of-the-envelope estimate of Θ , the differential impact on equity value from court characteristics. We set r equal to the market wide average cost of equity for non-financial firms at 8.57%.²⁴ Below, we estimate $\Delta No. \text{ HQ state. lawsuits}$ at 0.031 cases annually for firms with less than 15% of headquarters state operations (see column 6 of Table IX). Finally, [Prince and Rubin \(2002\)](#) find that the loss of equity value $[\Delta Equity|Lawsuit]$ equals 2% for product liability lawsuit announcements, a frequent category of diversity lawsuits. Using these numbers in equation (2), we obtain an implied value of -64.97% for Θ . Facing a lawsuit in a state court ranked as number 16 by the U.S. Chamber of Commerce as opposed to a court ranked as number 32 mitigates the negative effect on equity value by about two thirds. Although this is only a very rough approximation, we find the magnitude of the effect plausible.

B. *The Role of Litigation Risk*

In this section, we investigate cross-sectional differences in the impact of courts on firm value. As formalized in Hypothesis 2, court quality should be more relevant for firms that face larger litigation risk either due to industry or firm specific characteristics.

Table IV reports estimates of regressions of CARs around the ruling in *Hertz* on *Chamber of Commerce ranking (HQ state)* and its interaction with measures of litigation

²³These figures are based on stock prices at the end of 2009 and are expressed in 1990 dollars. This amounts to \$21.39 (\$3.96) million in 2010 dollars for the average (median) firm.

²⁴The estimate stems from Aswath Damodaran's website, available at http://people.stern.nyu.edu/adamodar/New_Home_Page/datafile/wacc.htm.

risk. The sample is the same as in the baseline event study.²⁵ We start with firm-level measures of litigation risk. In column 1, we use the past number of lawsuits a firm was involved in as a defendant between 2007 and 2009, scaled by total assets (*Litigation intensity*). In column 2, as a second measure, in the spirit of [Jagannathan and Wang \(1996\)](#) and [Luo and Balvers \(2017\)](#), we proxy for firm level litigation risk using after-tax settlements reported in annual reports (*Settlements*). As before, we scale *Settlements* by total assets. Consistent with Hypothesis 2, we find that firms with higher litigation risk react more strongly to changes in the court environment: the coefficient on both interaction terms is negative and statistically significant at the 5% level. In terms of economic magnitude, we find that the effect of a change in state court quality from rank 32 to rank 16 increases equity value for high litigation firms by 0.61%, about 1.5 times as much as for a firm with median litigation risk.²⁶

Our firm-level measures of litigation risk are built on realized litigation exposure in the past. One potential concern is that they capture idiosyncratic firm-level events, rather than the prospective future exposure to legal issues driven by actual operations. Hence, we also use two industry-level measures of litigation risk that are less sensitive to specific firm events. As a first proxy, we employ each industry’s occupational nonfatal injuries and illnesses rate since these are likely to lead to lawsuits by affected employees. Second, we look at labor intensity in manufacturing industries, as measured by the total payroll to total value added ratio. This measure captures the exposure to employment-related lawsuits, such as the lawsuit that triggered *Hertz* and labor-related lawsuits are a particularly prominent category of diversity lawsuits ([Kramer, 1990](#)).

We repeat the analysis with our industry-level measures. In columns 3 and 4, we find that the coefficients on the interaction of the U.S. Chamber of Commerce ranking and the measures of litigation intensity are negative and statistically significant at the 5 and 1% level, respectively. Exposure to courts ranked poorly by the U.S. Chamber of Commerce impacts stock prices more negatively for firms operating in industries with above-median occupational risk or labor intensity.²⁷ We therefore find a pattern consistent with Hy-

²⁵Past work on the effect of legal risk on firms has generally focused on securities lawsuits, either in the context of IPO underpricing or auditor liability (e.g., [Lowry and Shu, 2002](#); [Kim and Skinner, 2012](#)). These financial lawsuits are not subject to diversity jurisdiction and hence proxies for legal risk from this literature are not suitable for our investigation.

²⁶This example is for high *Litigation intensity*= 0.013, i.e., at the 90th percentile, and low *Litigation intensity*= 0.000, i.e., at the median, over the estimation sample.

²⁷There is a reduced number of observations in labor intensity specifications, as we are limited to manufacturing industries from the NBER-CES Manufacturing Database. When using the industry occupational nonfatal injuries and illnesses rate, we have less observations than in Table II, because some SIC groups cannot be directly mapped to NAICS industries from BLS data.

pothesis 2 across all four measures of litigation risk in Table IV: the valuation of firms with higher litigation risk is more sensitive to changes in their court environment.

These results do not only provide evidence that the channel through which the ruling in *Hertz* impacted firm value was through changes in the legal environment. These cross-sectional differences in treatment effects based on litigation intensity also alleviate the concern that our results are driven by geographic co-movement of stock prices that happens to be correlated with our measures of court characteristics (Pirinsky and Wang, 2006). While we cannot rule out such a coincidental shock in the geographic dimension, it is unlikely that there is a shock that is both positively correlated with the state level measure of court characteristics and, at the same time, with various firm- and industry-level measures of litigation risk.

C. Event Study Validity

As with all regulatory event studies, we need to verify that *Hertz* indeed constitutes a valid event. The ruling in *Hertz* was unanimous, which might prompt fears that the ruling was anticipated. U.S. Supreme Court cases proceed in multiple steps, with an oral argument as the last stage before the actual ruling is made and announced. The oral argument is the last time the U.S. Supreme Court’s judges publicly discuss a case before the actual ruling. If the oral argument had revealed that the court was leaning towards the nerve center test, the actual announcement of the ruling might have been anticipated. We therefore conduct a news search in the week following the day of the oral argument, namely November 10, 2009. Reassuringly, we find little to no coverage of the case.²⁸

As a more rigorous test, in columns 1 through 3 of Table V, we repeat the event study on the argument day for the same sample of firms as in our main specification. We find that the mean AR on that day is statistically indistinguishable from zero (column 1). Moreover, ARs are not significantly associated with court characteristics as measured by the U.S. Chamber of Commerce ranking (column 2) and the academic ranking (column 3). These results make it seem unlikely that the ruling in *Hertz* was anticipated by market participants.

Next, we repeat the news search for the day of the announcement to verify that the

²⁸We carry out the news search on Google News and Factiva on the day of the argument, November 10, 2009, and the following week. In particular, we search for the following expressions: “Hertz”, and “Hertz v. Friend”. We find one article in Dow Jones Business News in Factiva covering the argument and one blog post using Google. The lack of media mentions does not rule out that the decision was at least somewhat anticipated, but, to the degree that this attenuates the market reaction on the announcement day, it will bias us against finding an effect on the day of the actual ruling in *Hertz*.

ruling was picked up by the market. We limit the search to the day of the announcement and the following day. We find 209 (33) news stories on Google News (Factiva) mentioning the decision in *Hertz*, including reports on Reuters and in the Wall Street Journal. To further rule out that there was an anticipation effect, we re-estimate the effect of the U.S. Chamber of Commerce ranking on excess returns every day over a five-day window before and after the ruling. Figure 2 plots the resulting coefficients. Abnormal returns associated with the U.S. Chamber of Commerce ranking were statistically and economically insignificant on the four days preceding the ruling in *Hertz*, and turn negative and statistically significant only on the event day and the following day. It is important to note that the plotted coefficients represent returns, i.e., *changes* in stock prices, not levels. The statistically insignificant coefficients two and more days following the ruling therefore do not constitute a reversal of the effect. Instead, these coefficients mean that stock prices of firms headquartered in states with worse courts according to the U.S. Chamber of Commerce fall in value relative to other stocks upon the announcement of the ruling, while neither over- nor under-performing them at another time. The significant recognition in the financial press on announcement of the ruling in *Hertz*, together with the absence of anticipation effects and delayed responses in excess returns, therefore, corroborate the validity of our experiment.

Finally, we check whether there were any other big announcements on the day of the ruling that might drive our findings. Contemporaneously with the ruling in *Hertz*, there was a report of a drop in the U.S. Conference Board Consumer Confidence Index.²⁹ As a result of that drop, the aggregate stock market declined by roughly 1% on the event day. While our analysis exploits cross-sectional variation in court quality, it might still be affected by the contemporaneous shock if court quality was systematically correlated with exposure to consumer spending. We address this point by means of a placebo test. We perform a placebo test on June 29, 2010, when there was a similarly sized negative shock to the U.S. Conference Board Consumer Confidence Index, but no U.S. Supreme Court ruling on corporate citizenship.³⁰ Columns 4 through 6 of Table V present regression estimates for this placebo event study. The average event-day ARs is -0.9% and statistically significant (column 5), as on the event day. Reassuringly, however, ARs are not significantly associated with any of our measures of court characteristics (columns 5 and 6). Moreover, in Appendix Table E.1, we introduce industry fixed effects in the

²⁹See, e.g., A. Twin, “Stocks hit as confidence slips”, CNN Money, February 23, 2010 (http://money.cnn.com/2010/02/23/markets/markets_newyork/).

³⁰See, e.g., B. Rooney, “Consumer confidence craters in June”, CNN Money, June 29, 2010 (http://money.cnn.com/2010/06/29/news/economy/consumer_confidence/).

baseline event study, as different industries arguably have heterogeneous exposure to consumer confidence shocks. Our results again carry through after adding this additional control. These tests show no evidence that the adverse consumer confidence shock is driving our results.³¹

Of course, as in any regulatory event study, an unobservable shock that coincided with the ruling in *Hertz* may be driving our results. Such a shock would have to happen contemporaneously with the ruling and would have to affect firms with small headquarters state operations differently than those with large headquarters state operations. In addition, such a shock would have to correlate in its impact geographically with the U.S. Chamber of Commerce ranking, as well as having to be correlated with firms' sensitivity to litigation risk. While we cannot rule out the existence of such a shock, we fail to find a plausible story that fits all the above criteria.

D. Additional Tests

Our first additional test repeats the main analysis with a different estimation strategy. Whereas any event study can be interpreted as a difference-in-differences estimation in which the whole market serves as the control group (see [Atanasov and Black, 2016](#)), one may be concerned that our subsample of firms is a special one. Hence, in [Table VI](#) we perform a battery of tests that more closely resemble a standard difference-in-differences estimation, which both confirms our main results and allows us to account for further alternative explanations using fixed effects.

In line with our main analysis, we define the firms with small headquarters state operations and headquartered outside the 7th Circuit as the *treated* group, and other firms as the *control* group. We then estimate regressions of event-day ARs on the treatment indicator (*Treated*) and its interaction with our measures of court quality. In columns 1 to 3, we use the U.S. Chamber of Commerce ranking. In column 1, the interaction between *Treated* and *Chamber of Commerce ranking (HQ state)* is -0.02%. This estimated coefficient is not just statistically significant at the 5% level but also economically very similar to the one estimated in the main specification (-0.029%). The non-interacted

³¹There were some other announcements on the event day, none of which should affect our findings. One such event was the FDIC publishing a press release containing information on an increased number of financial institutions on its so called "problem list". This particular event should not affect our findings since we exclude financial firms. In addition, a Google News (Factiva) search for "problem list" on the event day returns 1 (12) hits, in contrast to 209 (32) hits for the case in *Hertz*. In addition, there were a number of earnings announcements. No single firm drives our results, but it could be that a earnings shock impacts both a firm and its competitors. Such an effect, however, would be captured by the industry fixed effects.

coefficient on *Chamber of Commerce ranking (HQ state)* is 0.009% and similarly statistically significant. These results suggest that there was a weak correlation between the U.S. Chamber of Commerce ranking and event day ARs, but the effect is concentrated among treated firms.

One implicit underlying assumption in our main specification is that federal courts are all equally business-friendly. In column 2, we control for federal circuit fixed effects, which absorb variation in the quality of federal courts, as well as geographic correlations in stock returns. We also include industry fixed effects. The estimated coefficient on the interaction between *Treatment* and *Chamber of Commerce ranking (HQ state)* in this specification is -0.04% and statistically significant at the 5% level.

Another concern might be that firms with small headquarters state operations are fundamentally different from the control sample, and that those differences are somehow correlated with state court quality. Column 3 therefore estimates the regression over a synthetic sample obtained through the *overlap weighting* method of [Li, Morgan, and Zaslavsky \(2017\)](#), to improve the covariate balance between treatment and control group.³² The estimated coefficient for the interaction terms stays negative and significant in this specification.

Column 4 to 6 repeat these estimations using the academic ranking rather the U.S. Chamber of Commerce ranking as a measure of court characteristics. The estimated coefficients on the interaction of *Treatment* and *Academic ranking (HQ state)* is statistically insignificant in all three cases. These results confirm the finding in the main analysis that the effect of courts on firm value seems to be driven by business attitude rather than objective efficiency.

Our next set of tests makes further use of the geographic spread of firms. Our main test investigates firms that get pinned into their headquarters state court, and effectively assumes that this is the only change in court quality. In reality, however, the ruling in *Hertz* did not just increase firms' exposure to their headquarters state's courts, but also reduced their exposure to non-headquarters states' courts, since post-*Hertz* firms were

³²The overlap weighting method was first applied in finance by [Bartram, Conrad, Lee, and Subrahmanyam \(2018\)](#). The method builds on a logit model providing a firm's treatment probability (propensity score). Then, the synthetic sample is obtained by re-weighting observations in such a way that higher weights are given to firms with intermediate propensity scores as opposed to those with scores close to 0 or 1. Our logit model includes the following covariates: the court ranking, size, book leverage, market-to-book, tangibility, cash holdings, interest expenses, capital expenditures, net debt issuance, Z-score, rating status indicator variables, and industry and federal circuit fixed effects. Figure 4 shows how the covariate balance for selected variables in the logit model improves after applying the overlap weighting method to our setting.

able to escape any court that was not their headquarters state's. Firms therefore experienced both increased exposure to headquarters state's courts and decreased exposure to non-headquarters states' courts. We thus investigate the effect of this aggregate change in court exposure.

Figure 3 visualizes our empirical design. Consider two firms, A and C , which operate in the same headquarters state i , but have operations in two different non-headquarters states: state j with court ranking Y and state k with ranking Z , respectively. After *Hertz*, both firms experience an increased exposure to state i 's courts and a decreased exposure to state j and k . The difference in aggregate effects between A and C in this simplified example is therefore $X - Y - X + Z = Z - Y$. The differential stock price reaction between firms A and C therefore corresponds to the different court characteristics in their non-headquarters states.

To test this hypothesis, we calculate for each firm the difference between the firm's headquarters state's court ranking and the average non-headquarters states' court ranking.³³ We call this variable $\Delta Court$, where $Court$ is any of our measures of court characteristics. For example, if a firm's headquarters state is given the rank of 5 and the weighted average ranking of all of its non-headquarters states is 10, $\Delta Court$ would take the value of $5 - 10 = -5$. $\Delta Court$ therefore increases as headquarters state's courts get worse or as non-headquarters states' courts get better. Since *Hertz* increased exposure to the former and decreased exposure to the latter, $\Delta Court$ captures the effect on aggregate court quality: the higher $\Delta Court$, the more negative the shock to a firm's court environment post-*Hertz*. If exposure to worse courts is associated with negative announcement returns, the effect of $\Delta Court$ should be negative.

Table VII repeats the baseline event study using $\Delta Court$ as explanatory variable. We find that the coefficient estimate on $\Delta Court$ is negative at -0.023 and statistically significant at the 5% level for the U.S. Chamber of Commerce ranking (column 1), and positive and significant at the 10% level for the academic ranking at 0.019 (column 2). These estimates confirm for firms' aggregate change in exposure to courts what we found earlier for exposure to their headquarters state courts: firm value declines either from increased exposure to unfriendly courts or reduced exposure to friendly courts.

In Table VIII, we conduct additional tests on the effect of firms' geographic dispersion of operations on their reaction to *Hertz*. In columns 1 through 3, we conduct a placebo

³³Since exposure to courts increases with a firm's operations in the state, we weight each state courts' ranking by the firm's size of operations. For a detailed description of our measure of operations, see the definition of *Chamber of Commerce ranking (out of HQ state)* in Appendix Table D.1. The same applies to the academic ranking.

event study focusing on firms whose fraction of headquarters state operations is between 15% and 30%. These firms are geographically similarly dispersed compared to our main sample, but arguably not enough to claim diversity of citizenship in their headquarters state. Consistently, we do not find significant results for any of the measures of court characteristics. In columns 4 and 5, we estimate the effect of increased treatment intensity conditional on treatment. A firm’s exposure to lawsuits likely increases with the size of its operations because more employees, business partners and customers create more potential conflicts. Conditional on being considered a citizen in a certain state, a firm’s reaction to the ruling in *Hertz* should therefore increase in its operations in that state, as long as total operations remain below 15%. We test this hypothesis by interacting our measures of court characteristic with the firm’s fraction of operations located in its headquarters state. If larger operations lead to more potential lawsuits, sensitivity to changes in court exposure should be larger for firms with larger operations. We find that among our main sample, the effect of court characteristics is indeed increasing in the magnitude of operations in the headquarters state for the U.S. Chamber of Commerce ranking (column 4), whereas the interaction with the academic ranking is insignificant (column 5). Finally, in columns 6 and 7, we show that our baseline results are robust to controlling for the fraction of non-headquarters state operations and the average non-headquarters state U.S. Chamber of Commerce ranking. The coefficients retain their economic and statistical significance compared to the baseline specification without those controls.

III. Change in the Number of Diversity Cases

The next step in our analysis is to document that the ruling in *Hertz* did actually impact the number of diversity lawsuits faced by corporations. Before *Hertz*, firms could argue for diversity of citizenship in their headquarters state on the grounds of having relatively small operations there. Firms with small operations in their headquarters state should have had an easier time claiming diversity of citizenship pre-*Hertz* and hence should have had more diversity lawsuits in federal court. After the ruling, those firms should have seen a particularly large drop in the number of cases they were able to remove from state-to federal court. Hence, after the ruling, we expect the number of diversity lawsuits to decrease in headquarters states relative to non-headquarters states. The effect should be stronger for states where firms have small operations, i.e., the firms from our main analysis.

To test this conjecture, we estimate the following regression specification:

$$\begin{aligned} \text{No. diversity lawsuits}_{i,s,t} = & \theta \cdot \text{Post}_t \times \text{HQ state}_{i,s,t} \times \text{Treated}_{i,s,t} \\ & + \gamma \cdot X_{i,s,t} + FE_{i,s,t} + \epsilon_{i,s,t}, \end{aligned} \quad (3)$$

where the subscripts i , s , and t indicate firm, state, and year, respectively. *No. diversity lawsuits* $_{i,s,t}$ is the total number of diversity lawsuits in a given firm-state-year. *Post* $_t$ is an indicator variable equal to one from 2010 onwards, i.e., including 2010, given that the ruling in *Hertz* took place in February 2010. *HQ state* $_{i,s,t}$ is an indicator variable equal to one if a given state is the headquarters state of the firm. In line with the previous section, *Treated* $_{i,s,t}$ is an indicator variable equal to one if a firm-state hosts less than 15% of the overall operations of the firm in a given year, as measured by the number of times the firm mentions such a state in its annual report. We include different levels of fixed effects ($FE_{i,s,t}$) and control variables ($X_{i,s,t}$).³⁴

We are interested in θ , the coefficient on the interaction term. Based on the discussion above, we expect to find $\theta < 0$: since it became harder for firms with small operations in their headquarters state to claim diversity of citizenship there, the number of diversity cases in those states should fall post-*Hertz* for those firm-states.

In Panel A of Table IX, we present estimates of regression (3), adding additional levels of fixed effects and control variables progressively. Column 1 includes year fixed effects to account for fluctuations in the number diversity lawsuits over time. Column 2 adds firm fixed effects to control for the fact that some firms might be exposed to diversity lawsuits more frequently than others. The specification in column 3 controls for state-by-year fixed effects to rule out that our findings are driven by some federal circuits that are simply more or less likely to recognize diversity of citizenship. Column 4 adds state-by-firm fixed effects to absorb time-invariant heterogeneity in firm exposure to litigation risk across states. Column 5 presents the results from the most saturated specification including state-by-year, state-by-firm, and firm-by-year fixed effects, where the latter set of interactions is included to mitigate the concern that specific firm-level events, such as a large number of lawsuits related to a single incidence category, drive our results. As a final test, we control for the total number of lawsuits filed for a given firm-state-year in column 7, to rule out that a large number of lawsuits faced by individual firms in specific states drives our result.

³⁴We do not report non-interacted variables in equation (3) for ease of exposition, but they are included in our actual estimation unless they are subsumed in fixed effects.

We find that, pre-*Hertz*, firms in the treatment group face about 0.3 more diversity of citizenship lawsuits per year in their headquarters state than in other states. As predicted, the number of diversity of citizenship lawsuits for these firms in their headquarters states drops by about 0.4 cases per year after *Hertz*, an effect that is statistically significant across all specifications.³⁵ Firms with small operations in their headquarters state have larger operations in other states and are, conversely, more likely to be considered a citizen of other states prior to *Hertz*. Consistent with those firms being able to more easily claim diversity of citizenship in non-headquarters states, we find that the number of diversity lawsuits in those states goes up for treated firms after the ruling. These highly saturated specifications suggest that the ruling in *Hertz* indeed reduced the ability of firms to claim diversity of citizenship in their headquarters states.

Whereas the absolute coefficient magnitude is low, it is important to keep in mind that we do not observe settlements, a prominent way to resolve legal cases. [Eisenberg and Lanvers \(2009\)](#), for example, report a settlement rate of 66.9% for federal cases in the Eastern District of Pennsylvania and the Northern District of Georgia. The effect of changes in exposure to different courts affects firm value through all cases, including all settled cases. For instance, a more business-friendly court will, in equilibrium, lead to more frequent settlements, and lower settlement amounts.

In Panel B of Table IX, we conduct several robustness tests. Columns 1 to 3 re-estimate equation (3) with different measures of diversity lawsuits. In column 1, we identify diversity cases according to an alternative rule, i.e., counting case descriptions that mention the expression “28:1332”, which is the number of the U.S. law governing diversity of citizenship jurisdiction (U.S. Code 28:1332). In column 2, we use the fraction of diversity lawsuits relative to the total number of lawsuits, rather than their absolute number. Given that only about 10% of firm-state-years exhibit non-zero diversity lawsuits (see Panel D of Table I), we then define an indicator variable equal to one if at least one diversity lawsuit is observed in a given firm-state-year. Column 3 estimates a logit model using this indicator as dependent variable, which also alleviates potential concerns regarding the estimation of a linear panel model with a large number of zeros as dependent variables.³⁶ Our result remains robust in each case. This allows us to rule out that outliers or our specific measurement definition or measurement of diversity lawsuits drive our results.

³⁵The number of diversity lawsuits in headquarters states does not drop all the way to zero since the U.S. Supreme Court explicitly stated that the headquarters state should be generally, but not always be considered the corporation’s “nerve center”.

³⁶The logit specification includes only year and state fixed effects.

Columns 4 to 6 of Panel B repeat the tests for different subsamples of firms. In column 4, we consider only firms in Panel A-C of Table I, i.e., those with available data on the day of the ruling. In column 5, we exclude the few firms that change headquarters state at least once between 2007 and 2014. In column 6, to deal with the serial correlation problem of standard errors in difference-in-differences estimation, we follow [Bertrand, Duflo, and Mullainathan \(2004\)](#) and collapse data into a two-period panel (pre- vs. post-*Hertz*). For each firm-state, we compute the average number of diversity lawsuits before and after *Hertz*, and define *HQ state (pre)* and *Small operations (pre)* based only on information before *Hertz*. Our result that the number of diversity lawsuits decreased for treated firms in their headquarters state after the ruling in *Hertz* carries through in each case.

IV. Relocation of Operations

Our main analysis shows that the ruling in *Hertz* affected firms' exposure to certain state courts and that exposure to more business-friendly courts increased firm value. According to the *2015 Lawsuit Climate Survey*, which forms the basis of the U.S. Chamber of Commerce ranking, 75% of surveyed corporate lawyers claimed that the quality of a state's court system impacted their decision to conduct business in a state. In this final section, we therefore explore whether the ruling had an effect on firms' choice to start operations outside of their headquarters state.

Pre-*Hertz*, firms might have chosen to avoid states with courts that were unfriendly towards businesses. After *Hertz*, firms were able to escape bad courts, possibly leading to an increase in operations in states with less business-friendly state courts. We estimate the following linear probability model:

$$Active\ in\ state_{i,s,t} = \phi \cdot Post_t \times Ranking_s + FE_{i,s,t} + \epsilon_{i,s,t}. \quad (4)$$

Our outcome variable, *Active in state*, is an indicator variable equal to one if firm i has any operation in state s in year t , as captured by our text-based measure from annual reports. $Post_t$ is an indicator variable equal to one from 2010 onwards. $Ranking_s$ is either the state's U.S. Chamber of Commerce ranking, or an indicator variable equal to one if the state's courts are outside the top ten of such a ranking. We control for state and firm-by-year fixed effects ($FE_{i,s,t}$). We estimate equation (4) over a sample comprising all possible firm-state pairs, as long as they do not exhibit operations in the first year

they enter the sample (*Active in state* = 0) and up to the year in which the firm begins operating there (*Active in state* = 1). We are interested in ϕ , the coefficient on the interaction term.

Table X reports estimates of regression (4). For ease of exposition, we multiply *Active in State* by 100. In column 1, we interact *Post* with *Chamber of Commerce ranking*, effectively testing for a linear effect of business friendliness on the relocation of operations. The estimated coefficient is both statistically and economically indistinguishable from zero. Not all firms will react similarly to changes in legal environment. Firms with high litigation risk may exhibit more pronounced event day ARs and also adjust their operations more strongly. In column 2, we therefore include a further interaction with the firm’s litigation risk, as measured by *Litigation intensity*. We find that firms with higher litigation risk are less likely to be active in states ranked more poorly by the Chamber of Commerce before *Hertz*. The triple interaction term between *Post*, *Chamber of Commerce ranking*, and *Litigation intensity* exhibits a positive and statistically significant coefficient, which means that firms with high litigation risk start operations in states with unfriendly courts at a higher rate after *Hertz*. Since the legal environment is just one of many factors in firms’ decision where to operate, it is not surprising that the effect’s economic magnitude is relatively small.

In column 3, we replace our linear measure of the court ranking with a simple indicator for non-top ten ranked states. This exercise serves two purposes: first, it allows for nonlinear effects of business attitude between top ranked states (i.e., top ten states) and others, and, second, it makes sure our results are not driven by a single outlier state. We interact an indicator of non-top-ten states with our *Post* indicator and find that firms indeed start operating in such states at a 0.15% higher rate after *Hertz*. The estimates in column 4 show that the increase is stronger for firms with high litigation intensity.

These results point towards firms with high exposure to the legal system increasing operations in states with less business-friendly courts after the ruling in *Hertz*.

V. Conclusion

We examine the relation between U.S. state court characteristics and firm value with particular focus on the channel through which courts impact firm value. Our identification strategy makes use of the parallel, two-layer court structure of federal and state courts in the U.S., which allows the same cases to be tried in different courts, but under the exact same laws. Since selection into those court systems is endogenous, we further exploit a

U.S. Supreme Court's decision in *Hertz* as a shock to the ability of firms to move between the two court systems.

We confirm that the ruling changed the number of lawsuits firms were able to move from state into federal court and therefore increased firms' exposure to certain state courts. This increased exposure to courts systematically affected firm value. In an event study around the ruling, firms with increased exposure to courts that are more highly ranked by the U.S. Chamber of Commerce experienced positive abnormal returns compared to firms with increased exposure to poorly ranked courts. By contrasting this subjective ranking with an objective academic ranking of court efficiency, we find that the economic channel through which U.S. state courts impact firm value appears to be courts' business attitude rather than their objective efficiency. Firms with higher litigation risk either due to industry-wide characteristics or to firm-specific, high past litigation activity seem to react more sensitively to changes in their legal environment.

This result is novel as it derives from differences between different state courts inside the U.S. rather than differences of courts between different countries. Since the U.S. are a country with a well-developed court system, it is interesting that there exist such stark differences between courts. For countries with overall poorer legal institutions, the effect of court characteristics on firm value are likely to be significantly more pronounced than what we found in this study.

Our results point to a large impact of court characteristics on firm value. As business attitude impacts firm value only indirectly by reducing existing legal uncertainty and expenses, this suggests that the original legal component of firm value is significant. Quantifying the absolute contribution of legal risk, and in particular legal operational risk, to firm value is an open question.

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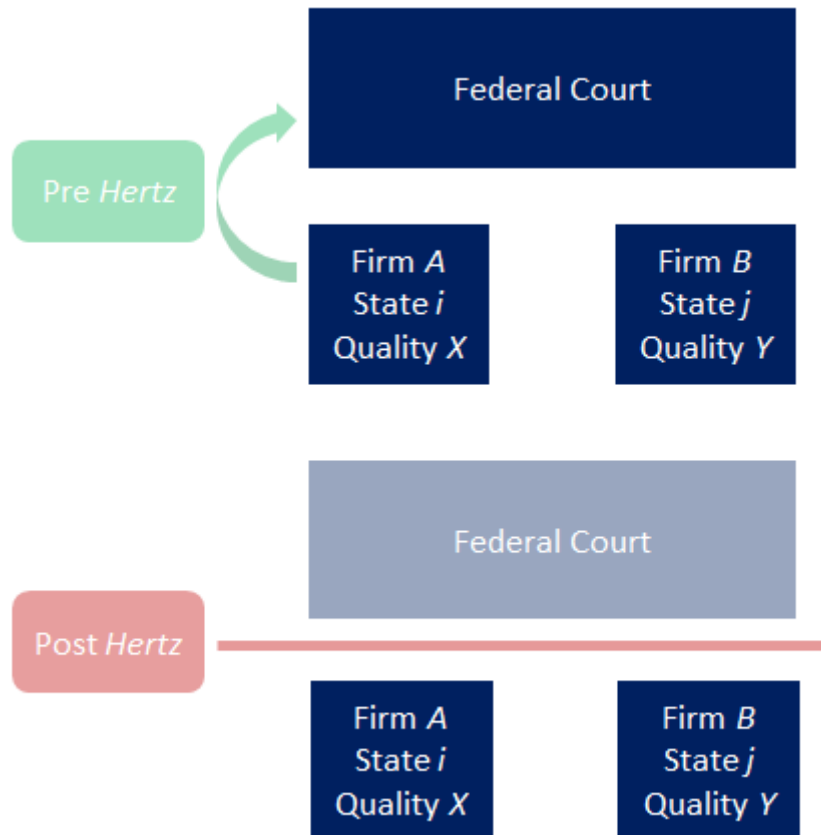


Figure 1: This figure illustrates our identification strategy. Two identical firms, *A* and *B*, are headquartered in states *i* and *j*, respectively. Before the ruling in *Hertz*, both firms are able to remove a case brought against them in state court to the federal court system under diversity of citizenship jurisdiction. After the ruling in *Hertz*, they lose this ability and therefore face increased exposure to their headquarters state’s court system. The two firms will experience differential changes in court quality, $X - Y$, based on the characteristics of their respective headquarters states.

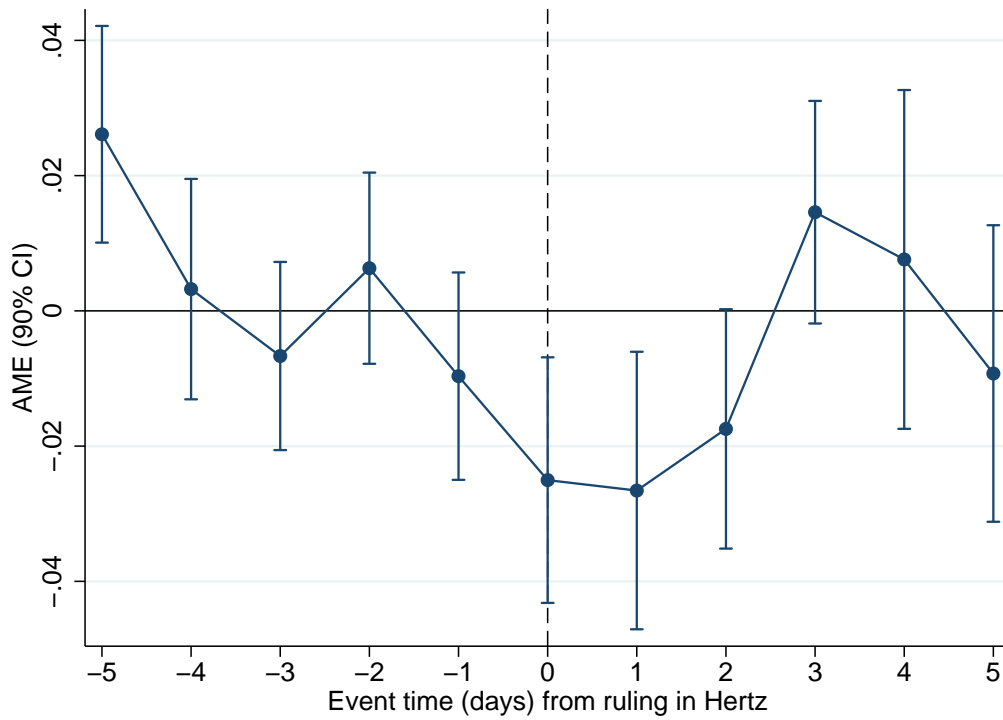


Figure 2: This figure depicts the average marginal effect (AME) of the business attitude of courts in the firm's headquarters state on daily excess stock returns over the window $[-5, 5]$ around the ruling in *Hertz* of February 23, 2010. The AMEs are computed from a panel regression of excess stock returns on day indicator variables and their interactions with the U.S. Chamber of Commerce ranking of courts in the firm's headquarters state. The estimation window includes the days in the interval $[-60, 60]$.

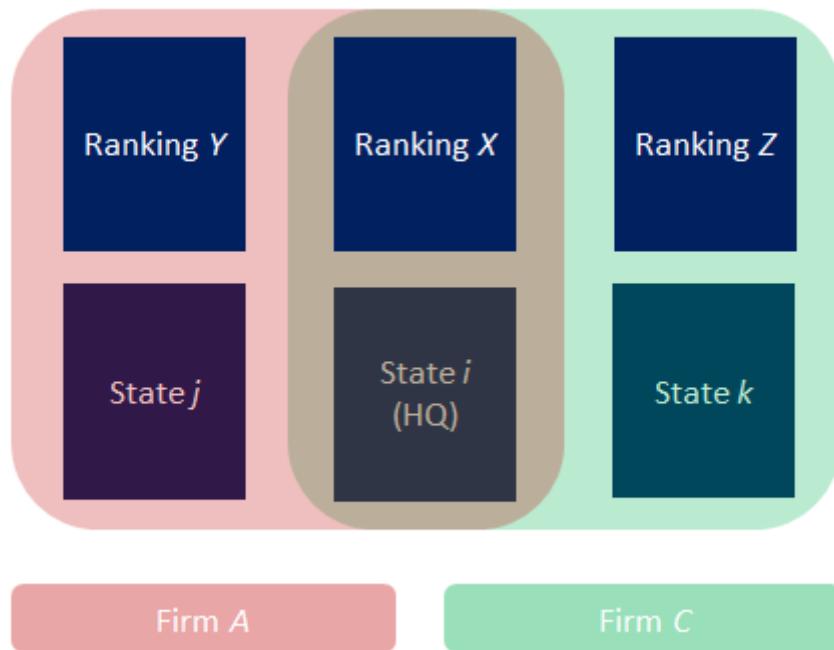


Figure 3: This figure illustrates the differences between aggregate court quality and headquarters state court characteristics. Two firms, *A* and *C*, are both headquartered in state *i*, whose courts are assigned a rank of *X*. Both of them have operations in a different, second state: state *j* with court ranking *Y* and state *k* with court ranking *Z* for firms *A* and *C*, respectively.

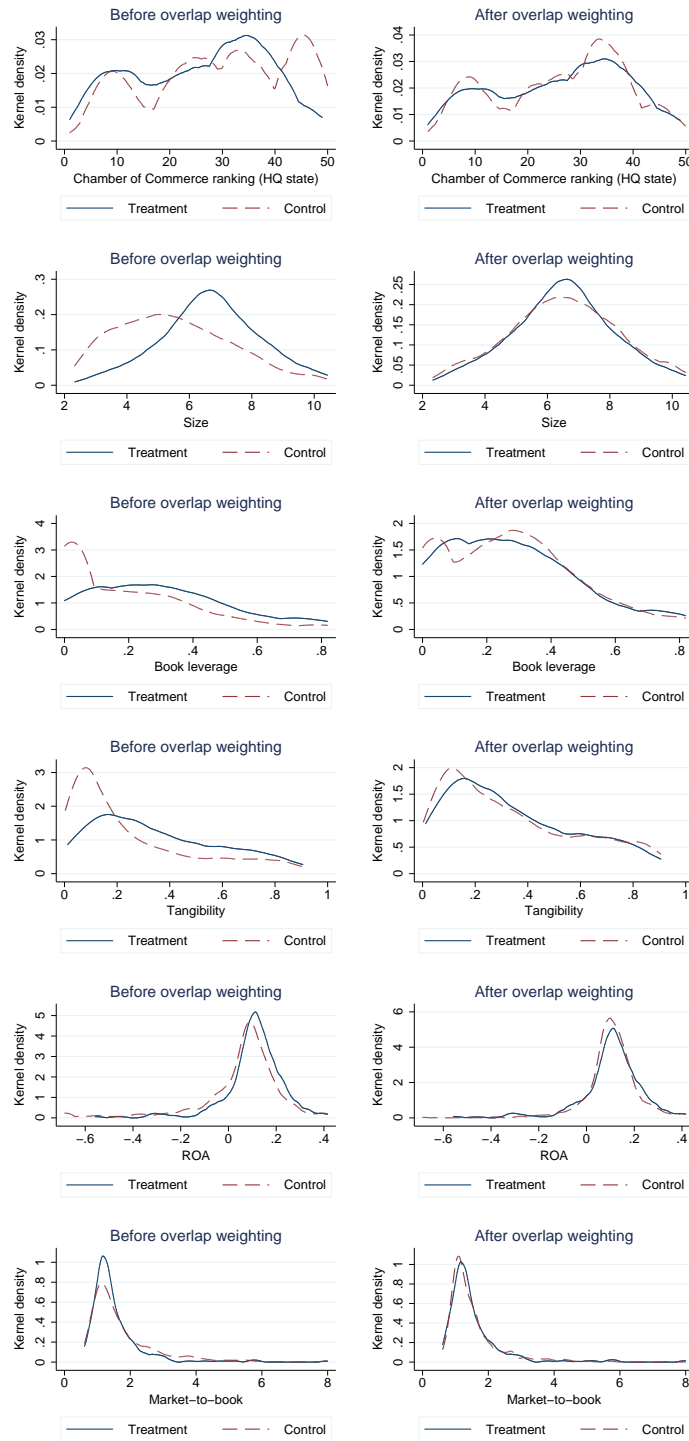


Figure 4: This figure illustrates the empirical distribution of selected variables across the treatment and control group before and after applying the overlap weighting method of Li et al. (2017). The treatment group includes all firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85%. The plotted variables are among those included in the logit model used for column 3 of Table VI.

Table I: Summary Statistics

This table reports summary statistics of the variables employed in the analysis of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* on February 23, 2010. The sample includes U.S. public firms with available stock price data on the event-day, excluding financial institutions and utilities. Panel A presents the mean of several corporate geographic dispersion measures (*No. states* and *Out of HQ state operations*) by quartile (further details in [Appendix B](#)). Panel B presents the mean of event-day ARs by quartile of the same corporate geographic dispersion measures as in Panel A. Panel C presents the summary statistics of the main variables used in our analysis as of the event-day. Panel D presents the summary statistics of the number of lawsuits in federal courts faced by firms (all lawsuits vs. diversity lawsuits) before and after the event, both at the firm-state-year and at the state-year level. All variables are defined in [Appendix Table D.1](#).

Panel A: Geographic dispersion measures				
	No. states		Non-HQ st. op.	
	Mean	Obs.	Mean	Obs.
Low	3.080	615	0.185	576
2	5.911	628	0.471	580
3	9.673	539	0.674	572
High	23.128	522	0.854	576

Panel B: Abnormal returns (event day)				
	No. states		Non-HQ st. op.	
	Mean	Obs.	Mean	Obs.
Low	-0.124	615	-0.308	576
2	-0.266	628	-0.119	580
3	-0.515	539	-0.388	572
High	-0.627	522	-0.660	576

(Continued)

Table I: – *Continued*

Panel C: Main variables						
	Mean	St. dev.	Q1	Med.	Q3	Obs.
<i>Stock market reaction</i>						
CAR[0, 0]	-0.368	3.091	-1.689	-0.216	0.932	2304
CAR[0, 1]	-0.387	4.463	-2.011	-0.232	1.054	2304
CAR[-10, -5]	1.038	8.027	-3.426	0.461	5.219	2304
CAR[-2, -1]	0.303	6.243	-1.485	0.024	1.577	2304
CAR[-1, 1]	-0.137	6.900	-1.939	-0.229	1.286	2304
CAR[-2, 2]	-0.084	8.044	-2.504	-0.203	1.774	2304
<i>Geographic location and litigation risk</i>						
No. states	9.936	9.111	4.000	7.000	12.000	2304
Non-HQ state operations	0.546	0.260	0.337	0.579	0.761	2304
Headquarter 7th Circuit	0.070	0.256	0.000	0.000	0.000	2304
<i>Court rankings and litigation risk</i>						
Chamber of Commerce ranking (HQ state)	29.229	13.605	20.000	30.500	42.000	2304
Academic ranking (HQ state)	20.142	15.887	6.000	14.000	35.000	2304
Business attitude (HQ state)	3.200	13.116	-6.280	6.747	16.181	2304
Litigation intensity	0.012	0.094	0.000	0.000	0.000	2304
Settlements	0.057	0.294	0.000	0.000	0.000	2304
High occupational risk	0.168	0.374	0.000	0.000	0.000	1801
High labor intensity	0.255	0.436	0.000	0.000	1.000	859
<i>Other firm characteristics</i>						
Size	5.674	1.875	4.220	5.545	6.942	2304
Book leverage	0.200	0.206	0.004	0.152	0.328	2304
Market-to-book	1.720	1.099	1.068	1.353	1.970	2304
Tangibility	0.258	0.239	0.073	0.168	0.382	2304
Cash holdings	0.225	0.228	0.048	0.146	0.331	2304
ROA	0.061	0.174	0.019	0.089	0.147	2261
Interest expenses	0.015	0.017	0.002	0.010	0.022	2138
Capital expenditures	0.040	0.047	0.012	0.025	0.051	2260
Net debt issuance	-0.026	0.084	-0.055	-0.007	0.000	2261
Z-score	-3.344	5.064	-4.806	-2.733	-1.164	2258
Rated	0.284	0.451	0.000	0.000	1.000	2304
Investment grade	0.128	0.335	0.000	0.000	0.000	2304
Panel D: Lawsuits						
	Mean	St. Dev.	Q1	Med.	Q3	Obs.
<i>Firm-state-year level (2007-2009)</i>						
No. lawsuits	1.880	3.149	0.000	1.000	2.000	10674
No. diversity lawsuits	0.530	1.211	0.000	0.000	0.000	10674
No. diversity lawsuits > 0	0.205	0.403	0.000	0.000	0.000	10674
No. diversity lawsuits (alt.)	0.479	1.156	0.000	0.000	0.000	10674
<i>Firm-state-year level (2010-2014)</i>						
No. lawsuits	1.408	3.063	0.000	0.000	2.000	11467
No. diversity lawsuits	0.104	0.555	0.000	0.000	0.000	11467
No. diversity lawsuits > 0	0.042	0.200	0.000	0.000	0.000	11467
No. diversity lawsuits (alt.)	0.303	0.973	0.000	0.000	0.000	11467
<i>State-year level (2007-2014)</i>						
No. lawsuits	93.806	148.743	10.000	46.500	96.000	386
No. diversity lawsuits	17.751	34.156	0.000	4.000	22.000	386
No. diversity lawsuits (alt.)	22.238	34.298	2.000	9.500	24.000	386

Table II: Firm Value and Court Characteristics

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day ARs, $CAR[0,0]$, on measures of court characteristics in the firm's headquarters state. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). $CAR[0,0]$ is computed in an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm's headquarters state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in the firm's headquarters state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). *Business attitude (HQ state)* is the business attitude of the court system in the firm's headquarters state measured by the component of *Chamber of Commerce ranking (HQ state)* that is orthogonal to *Academic ranking (HQ state)*. t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to Appendix Table [D.1](#) for variable definitions.

	CAR[0, 0]				
	(1)	(2)	(3)	(4)	(5)
Chamber of Commerce ranking (HQ state)		-0.029*** (-2.90)		-0.029*** (-2.91)	
Academic ranking (HQ state)			0.011 (0.98)	0.009 (0.87)	
Business attitude (HQ state)					-0.028*** (-2.91)
Constant	-0.642*** (-2.89)	0.127 (0.43)	-0.931** (-2.54)	-0.143 (-0.43)	-0.616*** (-2.81)
Observations	267	267	267	267	267
R^2	0.00	0.02	0.00	0.03	0.02

Table III: Firm Value and Court Characteristics – Portfolio Approach

This table analyzes the relation between firm value and state court characteristics by estimating cross-sectional regressions of event-day ARs aggregated at the headquarters state-level, $\overline{CAR}[0, 0]$. Firm-level ARs are computed in an event study of the U.S. Supreme Court’s ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. These firm-level ARs are then grouped into equally-weighted portfolios at the headquarters state-level, which comprise only firms with a fraction of non-headquarters state operations above 85% as measured by different states’ mentions in 10-K annual report filings (further details in [Appendix B](#)). Portfolios for states that host the headquarters of less than two of such firms or that are part of the 7th Circuit are not included in the regression sample. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in a given state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in a given state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). *t*-statistics calculated with robust standard errors. Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to Appendix Table [D.1](#) for variable definitions.

	$\overline{CAR}[0, 0]$		
	(1)	(2)	(3)
Chamber of Commerce ranking (HQ state)		-0.032* (-1.98)	
Academic ranking (HQ state)			0.006 (0.43)
Constant	-0.689*** (-2.99)	0.156 (0.30)	-0.847* (-1.99)
Observations	28	28	28
R^2	0.00	0.12	0.01

Table IV: Firm Value and Court Characteristics – The Role of Litigation Risk

This table analyzes the relation between firm value, court characteristics, and litigation risk by estimating cross-sectional regressions of event-day ARs, $CAR[0, 0]$. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). CARs stem from an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* on February 23, 2010. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm's headquarters state published by the U.S. Chamber of Commerce. This measure is ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). Columns 1 and 2 interact court characteristics with firm-level measures of litigation risk. *Litigation intensity* is the average number of lawsuits the firm is involved in as a defendant between 2007 and 2009 scaled by total assets. *Settlements* is computed as the firm's after-tax settlements scaled by total assets. Columns 3 and 4 interact court characteristics with industry-level measures of litigation risk. *High labor intensity* is an indicator variable equal to one if a firm belongs to an industry with above-median labor intensity, defined as the 2009 total payroll to total-value-added ratio from the NBER-CES Manufacturing Industry Database. *High occupational risk* is an indicator variable equal to one if a firm belongs to an industry with an above-median nonfatal occupational injuries and illnesses rate. Nonfatal occupational injuries and illnesses rate data are from the 2008 issue of the Injuries, Illnesses, and Fatalities (IIF) program of the Bureau of Labor Statistics (BLS). *t*-statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0, 0]			
	Firm measures		Industry measures	
	(1)	(2)	(3)	(4)
Ch. of Comm. rank. (HQ state) \times Lit. int.	-1.027** (-2.38)			
Litigation intensity	20.421** (2.46)			
Ch. of Comm. rank. (HQ state) \times Settl.		-0.059** (-2.58)		
Settlements		1.280*** (3.54)		
Ch. of Comm. rank. (HQ state) \times High occup. risk			-0.064** (-2.48)	
High occupational risk			2.093*** (3.64)	
Ch. of Comm. rank. (HQ state) \times High lab. int.				-0.224*** (-3.43)
High labor intensity				3.687 (1.58)
Chamber of Commerce ranking (HQ state)	-0.025** (-2.39)	-0.025** (-2.70)	-0.009 (-0.79)	0.005 (0.23)
Constant	0.013 (0.04)	0.051 (0.18)	-0.489 (-1.54)	-0.762* (-1.96)
Observations	267	267	192	44
R^2	0.03	0.03	0.03	0.25

Table V: Firm Value and Court Characteristics – Falsification Tests

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day ARs, $CAR[0,0]$, on several measures of court system characteristics in the firm's headquarters state. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). $CAR[0,0]$ is computed from event studies on two different event dates. Columns 1 through 3 present the results from a placebo regression on November 10, 2009, the day of the oral argument at the U.S. Supreme Court for *Hertz*. Columns 4 through 6 present the results from a placebo regression on June 29, 2010, a day on which there was a drop the U.S. Conference Board Consumer Confidence Index of similar magnitude to that on the day of the ruling in *Hertz*. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm's headquarters state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in the firm's headquarters state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0,0]					
	Argument day			Consumer confidence shock		
	(1)	(2)	(3)	(4)	(5)	(6)
Chamber of Commerce ranking (HQ state)		0.013 (1.20)			-0.015 (-0.92)	
Academic ranking (HQ state)			-0.004 (-0.32)			0.009 (0.65)
Constant	-0.047 (-0.27)	-0.373 (-1.66)	0.045 (0.11)	-0.895*** (-5.39)	-0.519 (-1.08)	-1.130*** (-3.13)
Observations	264	264	264	259	259	259
R^2	0.00	0.00	0.00	0.00	0.00	0.00

Table VI: Firm Value and Court Characteristics – Alternative Estimation

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day ARs, $CAR[0, 0]$, on measures of court characteristics in the firm’s headquarters state and their interaction with a treatment indicator. $CAR[0, 0]$ is computed in an event study of the U.S. Supreme Court’s ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. *Treated* is an indicator variable equal to one for firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states’ mentions in 10-K annual report filings (further details in [Appendix B](#)). *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm’s headquarters state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in the firm’s headquarters state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). All specifications, except those in column 1 and 4, include Fama-French 30-industry and federal circuit fixed effects. Columns 3 and 6 estimate specifications over a synthetic sample obtained by means of the overlap weighting model of [Li et al. \(2017\)](#). The propensity scores are estimated through a logit model that includes the following covariates: The court ranking, size, book leverage, market-to-book, tangibility, cash holdings, interest expenses, capital expenditures, net debt issuance, Z-score, rating status indicator variables, and industry and federal circuit fixed effects. *t*-statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to Appendix Table [D.1](#) for variable definitions.

	CAR[0, 0]					
	(1)	(2)	(3)	(4)	(5)	(6)
Ch. of Comm. rank. (HQ state) × Treated	-0.020** (-2.06)	-0.031** (-2.67)	-0.039** (-2.66)			
Chamber of Commerce ranking (HQ state)	-0.009** (-2.13)	-0.005 (-1.17)	0.017* (1.75)			
Acad. rank. (HQ state) × Treated				0.017 (1.51)	0.016 (1.57)	0.010 (0.59)
Academic ranking (HQ state)				-0.007 (-1.05)	-0.008 (-1.46)	-0.004 (-0.42)
Treated	0.192 (0.71)	0.468* (1.89)	0.863** (2.56)	-0.722* (-1.98)	-0.753** (-2.34)	-0.420 (-0.86)
Industry FE	No	Yes	Yes	No	Yes	Yes
Circuit FE	No	Yes	Yes	No	Yes	Yes
Weighting	No	No	Yes	No	No	Yes
Observations	2273	2273	1914	2273	2273	1914
R^2	0.00	0.06	0.11	0.00	0.05	0.10

Table VII: Firm Value and Court Characteristics – Changes in Aggregate Court Characteristics

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day ARs, $CAR[0, 0]$, on the aggregate change in court characteristics measured as the difference between the firm’s headquarters state and other states, $\Delta Court$. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states’ mentions in 10-K annual report filings (further details in [Appendix B](#)). $CAR[0, 0]$ is computed in an event study of the U.S. Supreme Court’s ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. $\Delta Court$ is the difference between firms’ headquarters state’s court characteristics and the average non-headquarters states’ court characteristics (weighted by the size of operations), where $Court$ is one of these two measures of court characteristics: *Chamber of Commerce ranking* and *Academic ranking*. *Chamber of Commerce ranking* is the 2010 ranking of the court system in a given state published by the U.S. Chamber of Commerce. *Academic ranking* is the ranking of the court system in a given state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0, 0]	
	(1)	(2)
Δ Chamber of Commerce ranking	-0.023** (-2.15)	
Δ Academic ranking		0.019* (1.85)
Constant	-0.624*** (-2.80)	-0.757*** (-3.26)
Observations	267	267
R^2	0.02	0.02

Table VIII: Firm Value and Court Characteristics – The Role of Geographic Dispersion of Operations

This table analyzes the relation between firm value, court characteristics, and geographic dispersion of operations by estimating cross-sectional regressions of event-day ARs, $CAR[0,0]$. Columns 1 through 3 focus on a sample of firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations between 70% and 85%. Columns 4 through 7 focus on the baseline sample of firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85%. The measures of geographic dispersion of operations are based on different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). CARs are from an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* on February 23, 2010. *Chamber of Commerce ranking* is the 2010 ranking of the court system in a given state published by the U.S. Chamber of Commerce. *Academic ranking* is the ranking of the court system in a given state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). *HQ state operations* is the firm's fraction of headquarters state operations. *Out of HQ state operations* is the firm's fraction of non-headquarters state operations. *Chamber of Commerce ranking (out of HQ state)* is the average U.S. Chamber of Commerce ranking of the states where the firm operates (weighted by the size of operations). All the measures of geographic dispersion of operations are based on different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). *t*-statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0, 0]						
	Non-HQ st. op. \in (70%, 85%]			(4)	(5)	(6)	(7)
	(1)	(2)	(3)				
Chamber of Commerce ranking (HQ state)		0.001 (0.12)		0.034 (0.82)		-0.029*** (-2.89)	
Ch. of Comm. rank. (HQ st.) \times HQ st. op.				-0.612* (-1.77)			
Academic ranking (HQ state)			-0.015 (-1.53)		-0.035 (-1.29)		0.010 (0.92)
Acad. rank. (HQ st.) \times HQ st. op.					0.457 (1.54)		
HQ state operations				14.530* (1.84)	-14.697 (-1.69)		
Out of HQ state operations						1.388 (0.34)	2.640 (0.63)
Ch. of Comm. rank. (non-HQ state)						-0.035 (-1.08)	-0.036 (-1.16)
Constant	-0.514** (-2.52)	-0.546** (-2.55)	-0.172 (-0.58)	-1.350 (-1.39)	0.538 (0.73)	-0.249 (-0.07)	-2.379 (-0.61)
Observations	430	430	430	267	267	267	267
R^2	0.00	0.00	0.01	0.03	0.01	0.03	0.01

Table IX: The Impact of the Ruling in *Hertz* on Diversity Lawsuits

This table analyzes the impact of the U.S. Supreme Court’s ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010 on the diversity lawsuits faced by firms. The unit of observation is the firm-state-year. The sample covers the period from 2007 to 2014. Panel A presents baseline tests over the whole sample. *No. diversity lawsuits* is the number of diversity lawsuits in federal courts for a given firm-state-year. *Post* is an indicator variable equal to one from 2010 onwards. *HQ state* is an indicator equal to one if a state is the firm’s headquarters state in a given year. *Treated* is an indicator equal to one if the firm’s fraction of operations in a given state-year is below 15%. *No. lawsuits* is the total number of lawsuits in federal courts for a given firm-state-year. Panel B considers different measures of diversity lawsuits as dependent variable (columns 1 to 3), and different samples (columns 4 to 6). Column 1 uses an alternative classification of diversity lawsuits based on case descriptions containing the expression “28:1332”, i.e., the federal code’s section governing diversity of citizenship jurisdiction. Column 2 uses the fraction of diversity lawsuits relative to the total lawsuit activity. Column 3 reports average marginal effects from a logit model, where the dependent variable is an indicator variable if at least one diversity lawsuit is reported in a given firm-state-year. Column 4 focuses on firms in Panel A-C of Table I, i.e., those with available data on the day of the ruling in *Hertz*. Column 5 excludes firms that change headquarters state at least once between 2007 and 2014. Column 6 considers a collapsed two-period panel (before and after *Hertz*), where the dependent variable is average *No. diversity lawsuits* across the two period. *HQ state (pre)* and *Treated (pre)* are computed based on information up to 2009. All the measures of geographic dispersion of operations are based on different states’ mentions in 10-K annual report filings (further details in Appendix B). The reported regressions include different sets of fixed effects. All the specifications in Panel B include interaction terms and *No. lawsuits* as a control variable. *t*-statistics calculated with robust standard errors clustered by firm are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to Appendix Table D.1 for variable definitions.

Panel A: Baseline tests						
	No. diversity lawsuits					
	(1)	(2)	(3)	(4)	(5)	(6)
HQ state × Treated	0.397** (2.55)	0.373** (2.36)	0.310** (1.98)	0.341** (2.20)	0.262* (1.65)	0.266* (1.80)
Post × HQ state	0.195** (2.09)	0.222** (2.37)	0.199** (2.16)	0.224** (2.31)	0.171 (1.58)	0.177* (1.84)
Post × Treated	0.174** (2.37)	0.209*** (2.88)	0.177** (2.44)	0.173** (2.26)	0.252*** (2.97)	0.286*** (3.76)
Post × HQ state × Treated	-0.372** (-2.13)	-0.464*** (-2.61)	-0.412** (-2.34)	-0.495*** (-2.72)	-0.448** (-2.27)	-0.494*** (-2.63)
No. lawsuits						0.607*** (23.68)
Year FE	Yes	Yes	No	No	No	No
Firm FE	No	Yes	Yes	No	No	No
State × Year FE	No	No	Yes	Yes	Yes	Yes
State × Firm FE	No	No	No	Yes	Yes	Yes
Firm × Year FE	No	No	No	No	Yes	Yes
Observations	22141	22127	22124	21994	21145	21145
<i>R</i> ²	0.06	0.13	0.18	0.36	0.46	0.56

(Continued)

Table IX: – *Continued*

Panel B: Additional tests						
	Diversity lawsuits measured as			No. diversity lawsuits		
	No. (alt.)	Fraction	Indicator	Table I (Panel A-C)	Never HQ state change	Collapsed
	(1)	(2)	(3)	(4)	(5)	(6)
Post × HQ state × Treated	-0.476** (-2.56)	-0.198*** (-2.75)	-0.089** (-2.06)	-0.464** (-2.39)	-0.517** (-2.55)	
Post × HQ state (pre) × Treated (pre)						-0.370** (-2.17)
Interactions & controls	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Firm FE	Yes	Yes	No	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	No	Yes	No	No	No
State FE	No	No	Yes	No	No	No
Observations	21145	6685	22141	20363	20047	6360
R^2	0.60	0.64		0.55	0.57	0.77
Pseudo R^2			0.37			

Table X: Relocation of Operations

This table analyzes the impact of the U.S. Supreme Court’s ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010 on the firm’s decision to start operating in a given state, depending on local state court characteristics and the firm’s litigation risk. The unit of observation is the firm-state-year. Estimates from linear probability models for the indicator variable *Active in state*, which is equal to one if a firm has operations in a given state, are reported. For ease of exposition, we multiply *Active in state* by 100. The sample covers the period from 2007 to 2014 and does not include the firm’s headquarters state. All possible firm-state combinations are included, provided that they do not have operations as of the year they enter the sample, and up to to the year in which the firm begins operating there. *Post*, an indicator variable equal to one from 2010 onwards, is interacted with different measures of court characteristics and with *Litigation intensity*. *Chamber of Commerce ranking* is the 2010 ranking of the court system in a given state published by the U.S. Chamber of Commerce. *Chamber of Commerce ranking (non-top 10)* is an indicator equal to one if *Chamber of Commerce ranking* is larger than 10 for a given state. *Litigation intensity* is the average number of lawsuits the firm is involved in as a defendant between 2007 and 2009 scaled by total assets. *Active in state* is based on different states’ mentions in 10-K annual report filings (further details in [Appendix B](#)). The reported regressions include state and firm-by-year fixed effects. *t*-statistics calculated with robust standard errors clustered by firm are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	Active in state			
	(1)	(2)	(3)	(4)
Post × Ch. of Comm. rank.	-0.003 (-1.42)	-0.003 (-1.63)		
Ch. of Comm. rank. × Lit. int.		-0.017* (-1.87)		
Post × Ch. of Comm. rank. × Lit. int.		0.036*** (2.64)		
Post × Ch. of Comm. rank. (non-top 10)			0.149** (2.23)	0.140** (2.07)
Ch. of Comm. rank. (non-top 10) × Lit. int.				-0.485* (-1.92)
Post × Ch. of Comm. rank. (non-top 10) × Lit. int.				0.773*** (2.75)
State FE	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes
Observations	610909	610909	610909	610909
R^2	0.18	0.18	0.18	0.18

Appendix for “Do Courts Matter for Firm Value? Evidence from the U.S. Court System”

Appendix A Background on the U.S. Supreme Court Ruling

In the ruling of the case *Hertz Corp. v. Friend* (*Hertz*) on February 23, 2010, the U.S. Supreme Court ruled on how to determine the citizenship of a corporation for the purpose of establishing diversity of citizenship. Whereas citizenship is easily determined for a “natural person” (i.e., a human being), it is less clear for a legal person such as a corporation.³⁷ The law reads that a firm is a citizen in both the state it is incorporated in and the state where its “principal place of business” lies.³⁸ Before the ruling in *Hertz* there was no unanimous interpretation as to what “principal place of business” meant. Some courts interpreted the principal place of business to mean a corporation’s nerve center, the (physical) place in which the firm’s executives steer its day to day activities. Proponents of this interpretation understood the nerve center to generally be located at the firm’s headquarters, and hence corporations were found to be citizens of the state in which their headquarter was located (*nerve center test*). The nerve center test was adopted by the courts of the 7th Circuit of the federal court system.

Other courts interpreted “principal place of business” to mean the state in which a firm conducts a significant fraction of its operations (*operations test*). Among the courts which applied the operations test there was again no consensus as to how to measure the size of “operations”. Different courts used different indicators such as assets, sales or employees. Both the choice of indicators and their relative weighting varied between courts. Finally, there was no consensus as to what fraction of either indicator would cross the threshold of making a state a corporation’s “principal” place of business.

In *Hertz v. Friend*, the car rental operator Hertz Corporation was sued by employees in California state court over an alleged breach of labor law. Hertz tried to claim diversity of jurisdiction and move the case to the federal level. But the 9th circuit court found Hertz to be a citizen of California. The decision was based on the fact that between 16% and 20% of Hertz’s locations, employees, revenue and transactions were located in

³⁷ The details for the case are filed as Case 559 U.S. 77.

³⁸ “A corporation shall be deemed to be a citizen of every State and foreign state by which it has been incorporated and of the State or foreign state where it has its principal place of business” (see 28 U.S. Code 1332).

California.³⁹ The court therefore denied federal jurisdiction to Hertz.

Shortly thereafter, in a similar case, the same court ruled that the retailer Best Buy was not a citizen of California, despite the state contributing 11% of its locations and 13% of both its revenues and employees.⁴⁰ These differing interpretations of the law caused significant uncertainty among corporations as to the applicability of federal jurisdiction. Hertz appealed the decision to deny its removal to the federal court. The U.S. Supreme Court accepted the case and, on February 23, 2010, decided in favor of Hertz: since the corporation's headquarter was located in New Jersey rather than California, Hertz was found not to be a citizen of California and therefore had the right to remove the case to federal court.

With the ruling in *Hertz*, the U.S. Supreme Court came down on the side of the nerve center interpretation and ruled that, in most cases, a corporation's principal place of business was to be found in its headquarters state.⁴¹ The U.S. Supreme Court ruling established binding precedent for all lower courts. The decision opened the doors to federal courts for corporations under some circumstances and closed it under others. After the ruling, firms would (almost) always be able to claim diversity of citizenship and move cases to federal court when sued in a state which was neither their headquarters nor their incorporation state. At the same time, firms with few assets or revenues in their headquarters state lost the ability to move cases into federal court. Before *Hertz*, those firms were able to claim diversity when sued in their headquarters state under the majority of operations rule.

One aspect that makes the decision in *Hertz* a useful natural experiment to assess the relevancy of court quality is that it affected firms differently based on their geographic footprint. The reason is that the nerve center test and the operations test were consistently favored by different U.S. circuits. The issue of corporate citizenship constituted a so-called "circuit split" in which different circuits interpret the law consistently in different ways. Importantly, a corporation sued in a state can only remove the case to the circuit of that state. Hence for any lawsuit brought in a specific state, only a single circuit's interpretation was binding. The 9th Circuit, covering the West Coast of the U.S., was most extreme in applying the operations test. Its interpretation allowed a firm to be found a citizen of not just one but multiple states each contained significant opera-

³⁹<https://www.law.cornell.edu/supct/pdf/04-1186P.ZS>, retrieved on April 19, 2015.

⁴⁰See http://www.jonesday.com/hertz_v_friend/, retrieved on April 19, 2015

⁴¹"The phrase 'principal place of business' in 1332(c)(1) refers to the place where a corporation's high level officers direct, control, and coordinate the corporation's activities, i.e., its 'nerve center', which will typically be found at its corporate headquarters."

tions. On the opposite side of the split stood the 7th Circuit containing Illinois, Indiana and Wisconsin. The 7th Circuit's courts were proponents of the nerve center rule. The remaining circuits fell somewhere between those two extremes. Most applied either the nerve center or the operations test on a case by case basis, although none would consider a firm a citizen of more than two states like the 9th Circuit did. Because of this split, the U.S. Supreme Court ruling had no effect for lawsuits brought in the 7th Circuit, a large effect for lawsuits in the 9th Circuit, and an intermediate effect for all other circuits. Throughout our analysis we exploit this geographic variation in treatment to sharpen our inference.

Appendix B Measures of Corporate Geographic Dispersion

Our measure of geographic dispersion is built on corporate filings with the Securities and Exchange Commission (SEC). The Securities Exchange Act of 1934 mandates U.S. corporations with publicly traded securities to file an annual form containing information on both the business itself and its financial situation.⁴² We use this information to determine the geographical scope of the firm. As [Garcia and Norli \(2012\)](#) and [Bernile et al. \(2015\)](#), we use text search to count the number of occurrences of each U.S. state's name in critical items of discussion in the report. In our analysis we focus on the information provided in Items 1,2,3,6 and 7, which detail general information on the firm's business, property and financial situation.

Item 1 contains reports on the corporation's business activities as well as those of any subsidiaries. Item 2 contains information on the location of the corporations most important physical properties, such as plants. In an important deviation from [Garcia and Norli \(2012\)](#) and [Bernile et al. \(2015\)](#), we also use information from Item 3, which details legal proceedings. Since our paper deals specifically with legal risk and exposure to different court systems, this section is relevant for our measure. Item 3 provides information on legal proceedings which exceed the firm's normal scope, including the name of the court in which the action is pending. Item 6 covers financial information of the firm. Item 7 contains the management's discussion and analysis of the company's performance.⁴³

A firm can file more than one type of annual report. Small businesses submit Form 10-KSB which is a reduced version of Form 10-K. Otherwise firms can submit amended versions of their annual reports. We use only a single filing for each firm and year. As

⁴²Available at <https://www.sec.gov>.

⁴³For a detailed description of each item, see Regulation S-K <http://www.ecfr.gov>.

Garcia and Norli (2012) we use the standard form 10-K whenever it is available. When no such filing is available, we search for an amended filing 10-K/A. Only if neither a normal nor an amended form is present we search for small business reports 10-KSB and 10-KSB/A.

As an example for the mention of states in Item 3, consider the following excerpt from Ford Motor Co.'s 10-K filing for the year 2014. State names are underlined.

Excerpt from Item 3 of Ford Motor Co's 10-K 2014

“[...] Medium/Heavy Truck Sales Procedure Class Action. This action pending in the Ohio state court system alleges that Ford breached its Sales and Service Agreement with Ford truck dealers by failing to publish to all Ford dealers all price concessions that were approved for any dealer. The trial court certified a nationwide class consisting of all Ford dealers who purchased from Ford any 600-series or higher truck from 1987 to 1997, and granted plaintiffs' motion for summary judgment on liability. During 2011, a jury awarded \$4.5 million in damages to the named plaintiff dealer and the trial court applied the jury's findings with regard to the named plaintiff to all dealers in the class, entering a judgment of approximately \$2 billion in damages. We appealed, and on May 3, 2012, the Ohio Court of Appeals reversed the trial court's grant of summary judgment to plaintiffs, vacated the damages award, and remanded the matter for a new trial. The retrial in September 2013 resulted in a verdict in Ford's favor. On February 7, 2014, the trial court granted plaintiffs' motion for a new trial, but on December 11, 2014, the Ohio Court of Appeals reversed the order granting a new trial and reinstated the verdict in Ford's favor. Plaintiffs have sought further review in the Ohio Supreme Court. [...]”

We verify our measure of state exposure in several ways. First, we find that on average, the corporation's headquarters state accounts for 45% of all states mentioned in the report. This makes it the most mentioned state for almost all corporations. The second most mentioned state on average accounts for 41% of all state mentions. Second, we find a strong positive and statistically significant correlation between a state's population and the firm-level mentions of that state in form 10-Ks. Finally, we obtain data on geographic dispersion of commercial banks' branches from the Federal Deposit Insurance Corporation (FDIC). Although, our sample does not include banks, comparing our measure of state exposure with high-quality FDIC data provides a useful test. In Appendix Table B.1, we validate our text-based measures against 2005 FDIC data on bank branches. We find that both the fraction of non-headquarters state offices and deposits of banks is highly

correlated with our *Out of HQ state operations* measure based on banks' annual reports.

Appendix C Court Rankings

Appendix Table C.1 reports ordinal rankings of state court systems according to the 2010 edition of the U.S. Chamber of Commerce ranking and the academic ranking proposed by Choi et al. (2009) in their Table 8.

Appendix D Variable Definitions

See Appendix Table D.1.

Appendix E Further Results

We conduct a battery of additional tests about firm value. One potential concern is that our proxy of geographic dispersion is noisy, thus making our event study results sensitive to the precise cutoff point. In Appendix Table E.1, we therefore repeat the analysis of Table II with varying cutoffs for non-headquarters state operations. Columns 1 and 2 report results when we loosen the restriction from 15% headquarters state operations to 20%. All coefficients retain both their economic and statistical significance. Columns 3 and 4 add Fama-French 30-industry fixed effects to control for any potential industry shocks on the days surrounding the event. Columns 5 and 6 extend our event window by one additional day following the ruling. Again, both the economic and statistical significance of all coefficients remain unchanged.

In Appendix Table E.2, we also show that our results are not sensitive to different levels of winsorization for CARs: at 2% and 98% (columns 1 and 2), and at 5% and 95% (columns 3 and 4).

Finally, Appendix Table E.3 reports univariate regressions of event-day ARs on the different dimensions that are taken into account to construct the U.S. Chamber of Commerce ranking and the academic ranking. We estimate univariate regressions because the various dimensions that constitute these rankings tend to be highly correlated. Panel A presents estimates for the U.S. Chamber of Commerce ranking's dimensions. We find that all dimensions exhibit a negative and statistically significant correlation with event-day ARs. Interestingly, this holds true even for dimensions that are arguably supposed to capture objective court efficiency, such as timeliness and juries' fairness. Panel B presents estimates for the academic ranking's sub-dimensions. None of these dimensions exhibits a statistically significant association with event-day ARs.

Table B.1: Validation of Measures of Corporate Geographic Dispersion

This table validates the text-based measures of corporate geographic dispersion obtained from Form 10-Ks filed with the SEC's EDGAR database against similar measures based on data from the 2005 FDIC's Summary of Deposits. The bank sample includes U.S. banks with available stock price data, based on the CRSP-FRB link file made available by the Federal Reserve Bank of New York. Pairwise correlations among the different measures of a bank's fraction of operations outside its main office state are estimated. Refer to Appendix Table D.1 for variable definitions.

	(1)	(2)	(3)
(1) Non-HQ state operations	1		
(2) Non-HQ state offices (FDIC)	0.5129	1	
(3) Non-HQ state deposits (FDIC)	0.4754	0.9745	1

Table C.1: State Court System Rankings

This table reports ordinal rankings of state court systems based on the 2010 Chamber of Commerce ranking (see <http://www.instituteforlegalreform.com/states>) and the academic ranking in Table 8 of [Choi et al. \(2009\)](#).

U.S. state	Abbreviation	Chamber of Commerce ranking (2010)	Academic ranking
Alabama	AL	47	17
Alaska	AK	33	29
Arizona	AZ	13	36
Arkansas	AR	44	2
California	CA	46	1
Colorado	CO	8	34
Connecticut	CT	24	31
Delaware	DE	1	32
Florida	FL	42	20
Georgia	GA	27	6
Hawaii	HI	35	46
Idaho	ID	18	42
Illinois	IL	45	11
Indiana	IN	4	30
Iowa	IA	5	23
Kansas	KS	14	16
Kentucky	KY	40	44
Louisiana	LA	49	40
Maine	ME	12	37
Maryland	MD	20	14
Massachusetts	MA	9	9
Michigan	MI	30	50
Minnesota	MN	11	35
Mississippi	MS	48	7
Missouri	MO	37	49
Montana	MT	43	4
Nebraska	NE	3	10
Nevada	NV	28	45
New Hampshire	NH	16	18
New Jersey	NJ	32	28
New Mexico	NM	41	43
New York	NY	23	12
North Carolina	NC	17	48
North Dakota	ND	2	3
Ohio	OH	29	5
Oklahoma	OK	31	38
Oregon	OR	21	33
Pennsylvania	PA	34	8
Rhode Island	RI	38	15
South Carolina	SC	39	24
South Dakota	SD	10	26
Tennessee	TN	19	19
Texas	TX	36	39
Utah	UT	7	22
Vermont	VT	25	27
Virginia	VA	6	41
Washington	WA	26	13
West Virginia	WV	50	21
Wisconsin	WI	22	47
Wyoming	WY	15	25

Table D.1: Definition of Main Variables

Variable	Definition
CAR[$n1, n2$]	Cumulative abnormal returns $n1$ days to $n2$ days around the event-day. Abnormal returns are obtained by computing the residuals of separate regressions of individual CRSP daily stock returns on the three Fama and French (1993) risk factors and a momentum factor, with the factor loadings estimated in the pre-event period. For the bank sample, the market model is used to obtain such residuals.
Chamber of Commerce ranking (HQ state)	U.S. Chamber of Commerce ranking of a firm's headquarters state.
Academic ranking (HQ state)	Academic ranking of a firm's headquarters state based on Choi et al. (2009) .
Business attitude (HQ state)	State court system's business friendliness of a firm's headquarters state. This is the residual from a regression of the U.S. Chamber of Commerce ranking on the academic ranking of Choi et al. (2009) .
No. states	The number of unique U.S. states for which state count is at least one for a firm.
Non-HQ state operations (%)	A firm's sum of non-headquarters state counts divided by the total sum of state counts.
Chamber of Commerce ranking (non-HQ state)	Average Chamber of Commerce ranking of states a firm operates in (excluding the headquarters state), weighted by state counts. Formally, let I be the set of states mentioned in the annual report different from the firm's headquarters state, and J be the set of all states. Let $rank_i$ denote the ranking and $count_i$ the number of mentions of $state_i$, then: $\frac{\sum_{i \in I} count_i rank_i}{\sum_{i \in I} count_i} \frac{\sum_{j \in J} count_j}{\sum_{j \in J} count_j} = \frac{\sum_{i \in I} count_i rank_i}{\sum_{j \in J} count_j}$
No. lawsuits	Number of lawsuits brought against a firm-state (or the firm as a whole) in federal courts in a given year. This is based on machine-collected data from a website collecting information from PACER. Only lawsuits in which the firm acts as defendant are considered. We exclude appeals and asbestos-related cases.
No. diversity lawsuits	Number of lawsuits driven by diversity of citizenship. A lawsuit is classified as driven by diversity of citizenship if the word "diversity" appears in the description of the cases' cause of action.
No. diversity lawsuits (alt.)	Number of lawsuits driven by diversity of citizenship according to an alternative classification. In this case, a lawsuit is classified as driven by diversity of citizenship if the expression "28:1332" (the number of the U.S. law governing diversity of citizenship jurisdiction) appears in the description of the cases' cause of action.
Litigation intensity	Average <i>No. lawsuits</i> the firm is involved in as a defendant between 2007 and 2009 scaled by total assets (item at in Compustat).
Settlements	After-tax settlements (item <i>seta</i> in Compustat) scaled by total assets (item at in Compustat).
High labor intensity	Indicator variable equal to one if a firm belongs to an industry with above-median labor intensity. We define industry labor intensity as the 2009 total payroll to total value added ratio from the NBER-CES Manufacturing Industry Database.
High occupational risk	Indicator variable equal to one if a firm belongs to an industry with an above-median nonfatal occupational injuries and illnesses rate. Nonfatal occupational injuries and illnesses rate data are from the 2008 issue of the Injuries, Illnesses, and Fatalities (IIF) program of the Bureau of Labor Statistics (BLS). 2008 BLS data follow the 2002 North American Industry Classification System NAICS, so we use the 2002 NAICS to 1987 SIC concordance table from the U.S. Census Bureau, and the concordance table for SIC groups from the BLS.

Table E.1: Firm Value and Court Characteristics – Other Tests

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day CARs, $CAR[0,0]$ or $CAR[0,1]$, on several measures of court system characteristics in the firm's headquarters state. Columns 1 and 2 focus on a sample of firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 80%. Columns 3 through 6 focus on the baseline sample of firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85%. The measures of geographic dispersion of operations are based on different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). $CAR[0,0]$ and $CAR[0,1]$ are computed in an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm's headquarters state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in the firm's headquarters state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). Columns 3 and 4 include Fama-French 30-industry fixed effects. t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0,0]				CAR[0,1]	
	(1)	(2)	(3)	(4)	(5)	(6)
Chamber of Commerce ranking (HQ state)	-0.023** (-2.59)		-0.030** (-2.42)		-0.056** (-2.72)	
Academic ranking (HQ state)		0.001 (0.17)		0.009 (0.77)		-0.003 (-0.18)
Constant	-0.034 (-0.14)	-0.681*** (-2.85)	0.075 (0.20)	-1.085*** (-3.27)	0.442 (0.80)	-0.937** (-2.10)
Industry FE	No	No	Yes	Yes	No	No
Observations	430	430	267	267	267	267
R^2	0.01	0.00	0.18	0.16	0.04	0.00

Table E.2: Firm Value and Court Characteristics – Winsorization Level

This table analyzes the relation between firm value and measures of court efficiency and business attitude by estimating cross-sectional regressions of event-day ARs, $CAR[0,0]$, on several measures of court system characteristics in the firm's headquarters state. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states' mentions in 10-K annual report filings (further details in [Appendix B](#)). $CAR[0,0]$ is computed in an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. In columns 1 and 2, $CAR[0,0]$ is winsorized at 2% and 98%. In columns 3 and 4, $CAR[0,0]$ is winsorized at 5% and 95%. *Chamber of Commerce ranking (HQ state)* is the 2010 ranking of the court system in the firm's headquarters state published by the U.S. Chamber of Commerce. *Academic ranking (HQ state)* is the ranking of the court system in the firm's headquarters state by [Choi et al. \(2009\)](#). Both measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state (further details in [Appendix C](#)). t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to [Appendix Table D.1](#) for variable definitions.

	CAR[0,0]			
	Wins. at 2% and 98%		Wins. at 5% and 95%	
	(1)	(2)	(3)	(4)
Chamber of Commerce ranking (HQ state)	-0.027*** (-2.87)		-0.025*** (-3.03)	
Academic ranking (HQ state)		0.008 (0.80)		0.005 (0.51)
Constant	0.088 (0.31)	-0.831** (-2.55)	0.025 (0.09)	-0.750** (-2.45)
Observations	267	267	267	267
R^2	0.02	0.00	0.02	0.00

Table E.3: Firm Value and Court Characteristics – Different Dimensions of Court Rankings

This table analyzes the relation between firm value and court characteristics by estimating cross-sectional univariate regressions of event-day ARs, $CAR[0, 0]$, on the different dimensions that determine the ranking of the court system in the firm's headquarters state. The sample is restricted to firms headquartered outside the 7th Circuit and with a fraction of non-headquarters state operations above 85% as measured by different states' mentions in 10-K annual report filings (further details in Appendix B). $CAR[0, 0]$ is computed in an event study of the U.S. Supreme Court's ruling in *Hertz Corp. v. Melinda Friend* of February 23, 2010. Panel A considers the different dimensions taken into account by the U.S. Chamber of Commerce (see <http://www.instituteforlegalreform.com/states>). Panel B considers the different dimensions taken into account by the academic ranking of by Choi et al. (2009). All measures are ordinal, such that the value of 1 corresponds to the best ranked state, and 50 to the worst ranked state. t -statistics calculated with robust standard errors clustered by Fama-French 30-industry are reported in (). Significance at the 10%, 5%, and 1% level is indicated by *, **, ***, respectively. Refer to Appendix Table D.1 for variable definitions.

Panel A: Chamber of Commerce subrankings		CAR[0, 0]									
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tort litigation	-0.027** (-2.51)										
Class action			-0.027** (-2.31)								
Damages				-0.031** (-2.47)							
Venue requirements					-0.027** (-2.39)						
Timeliness						-0.030*** (-2.81)					
Discovery							-0.026** (-2.09)				
Evidence								-0.018* (-1.92)			
Impartiality									-0.033*** (-2.95)		
Competence										-0.035*** (-3.34)	
Fairness											-0.033*** (-3.49)
Constant	0.045 (0.14)	-0.010 (-0.04)	0.199 (0.51)	-0.022 (-0.08)	0.142 (0.45)	0.058 (0.15)	-0.241 (-1.01)	0.233 (0.90)	0.285 (1.21)	0.256 (0.95)	
Observations	267	267	267	267	267	267	267	267	267	267	267
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.03	0.03	0.03	0.03

(Continued)

Table E.3: – *Continued*

	CAR[0, 0]		
	(1)	(2)	(3)
Opinion	0.011 (0.98)		
Outside citations		-0.003 (-0.31)	
Independence			0.015 (1.63)
Constant	-0.944*** (-2.89)	-0.562* (-1.74)	-1.027*** (-3.01)
Observations	267	267	267
R^2	0.00	0.00	0.01