Halliburton II and its Impact on Litigation Trends of Securities Fraud Class Actions

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Abstract

This paper attempts to empirically measure the impact of the Supreme Court's *Halliburton II* decision on litigation trends in securities class actions. By applying a differences-in-differences approach, we examine the ruling's effect on dismissal rates, case lengths, number of annual filings, and filing activity in the Southern District of New York. We find that the ruling had little to no impact on these outcome variables with the exception of number of annual filings. Our regressions indicate that *Halliburton II* led to more annual filings. This result is difficult to understand and seriously casts doubt on our methodology.

Keywords: Halliburton II, Securities Fraud, Differences-in-Differences, Section10b, Seciton11

I pledge my honor that this paper represents my own work in accordance with University regulations

/s/ Leland Domaratzky

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1 Introduction

1.1 Background

On June 25th, 2014, the Supreme Court delivered its highly anticipated opinion in *Halliburton Co. v. Erica P. John Fund*, more commonly know in the legal community as the *Halliburton II* ruling. *Halliburton II* was the first opportunity to overturn *Basic, Inc. v. Levinson* and restructure how the legal system approaches securities fraud lawsuits. While the Justices ultimately did not overturn *Basic*, they did offer a slight technical update in precedent to streamline the litigation process. In particular, *Halliburton II* ruled that defendants in a securities fraud lawsuit can get their case dismissed *at the class certification stage* if they can show that the fraud in question had no price impact. This would presumably be accomplished by a quantitative event-study. The main justification or spirit of *Halliburton II* was that the precedent change was supposed to streamline the litigation process. In theory, the ruling should have given trial judges more discretion to throw out cases that lacked any legitimate claim .

Now nearly seven years after *Halliburton II* this paper uses differences-in-differences estimation to empirically measure the impact of the ruling on litigation trends. The four primary outcome variables we investigate are case dismissal rates, case lengths, number of annual cases, and filing activity in the Southern District of New York. We will formalize this later on, but we seek to understand to what extent *Halliburton II* as ruling successfully made securities fraud litigation more *efficient*.

This paper presents a unique contribution to the existing literature in two principal ways. One, it offers the first empirical assessment of the impacts of *Halliburton II*. And two, by employing differences-in-differences techniques, we offer an alternative methodology to evaluate litigation trends other than just summary statistics.

1.2 Securities Fraud Litigation in the United States

Canonically, a securities fraud suit involves a publicly-traded firm that made a material misrepresentation on its financial statements; typically, when the misrepresentation is revealed

(i.e. *the truth gets out*), the firm's investors can sue management and the firm to recover damages from the false information.

The Supreme Court's endorsement of *Fraud on the Market Theory* in their 1988 decision in *Basic Inc. v. Levinson* radically transformed the landscape of securities fraud litigation. Before *Basic*, securities fraud cases were rare, difficult to win, and undesirable work for an enterprising attorney. By reorienting securities fraud claims as massive class action suits, Basic enabled plaintiff attorneys to credibly demand billions of dollars in damages and strong-arm settlements. Securities fraud litigation quickly became one of the most active, and lucrative, fields in the U.S. legal sector. In aggregate, securities class action suits netted over one-hundred trillion dollars in settlements over the past twenty-five years. In 2020, around 4.4% of SP 500 firms were a defendant in an active securities fraud lawsuit. In 2018, almost one-in-ten SP 500 companies were dealing with a securities fraud suit (Cornerstone 2020). [1]

By their nature, securities fraud filings are primarily "event-driven". In other words, there is a specific time that fraudulent activity becomes public, the stock price drops, and within days lawyers file a class action complaint. As a result of Basic, securities litigation encourages lawyers to bring so-called "strike suits". These are cases that are weakly anchored in fraudulent activity but claim damages sufficiently high to strong-arm the defendant into a settlement [2]. At its worst, the status-quo incentives a law firm to file a class action complaint anytime they see a company's stock drop by a material amount. Pritchard writes "The FOTM presumption generates too many suits because defendants' incentive to settle these cases only has an tenuous connection with the merits…Even supremely confident defendants will settle meritless cases rather than risk the very real possibility of a jury verdict that threatens bankruptcy" (Pritcahard 2015). As a final note, the plaintiff lawyers are the real party that benefit. Approximately, 40% of settlement funds in these types of cases go towards attorney fees while the rest is distributed across thounds or even millions of shareholders (Yingling 2021).For context, the

[1]The statistic for 2019 is 9.4%. Note that these statistics from Cornerstone Research do not include MA related filings. The actual numbers are therefore probably slightly higher.

[2]Common examples of strike suits might claim that, after observing the CEO have a heart attack, the CEO's poor health was not properly disclosed. Another example might be that the company's facilities were damaged in a storm and the suit could claim that the potential for severe weather was properly disclosed.

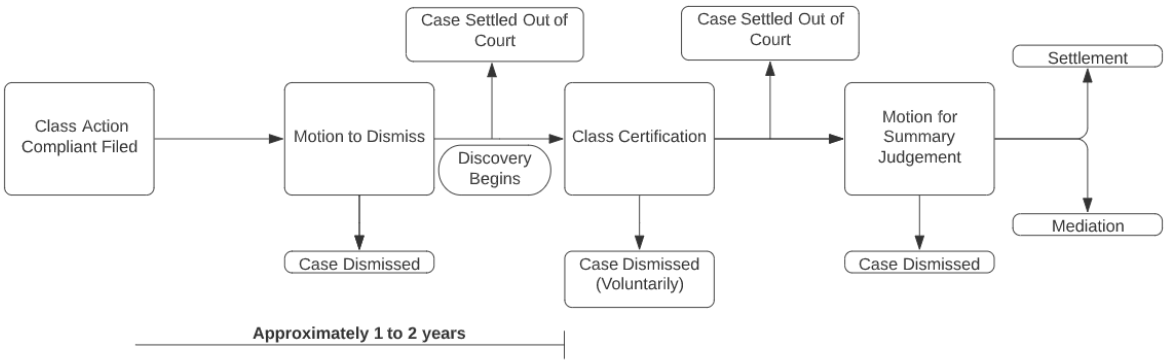
median settlement value in 2020 was 10.1 million dollars (Cornerstone 2020).

In response to the surge in litigation activity after *Basic*, Congress passed the Private Securities Litigation Reform Act of 1995 (PSLRA). One of the act's main goals was to deter strike-suits but the final legislation was watered down and did very little to change the status quo (Pritchard 2015).

The next big opportunity to alter the system came with *Halliburton II* and to many observer's disappointment they did not choose to dismantle *Basic*. That said, the Supreme Court did in a way attempt to improve the system by approving of "no price impact" as reasonable grounds to dismiss during class certification. To give the reader more context, we now offer a brief digression on how these cases work procedurally.

1.3 Background on Litigation Process

Figure 1: Basic Progression of a Securities Class Action through the Legal System



Speaking broadly, a case begins when a law firm files a class action complaint in a Federal District Court. While the defendant firm is not requited to, they almost always then file a motion to dismiss. The motion to dismiss in theory should be the first opportunity for the system to weed out meritless claim. Conditional on the case surviving a motion to dismiss, the next stage is class certification. At this point, the case is already one to two years old. This is when the relevant lawyers solidify the set of shareholders who are capable of claiming damages. The judge must then verify that all class members have similar claims; then the judge must approve or appoint an attorney who they feel can fairly represent all members of

the class (Yingling 2021).

Conditional on the class being certified the case then there can finally be an argument of case merits which a judge will preside over. Also note that at any point in the process, the two parties can halt any further litigation and just choose to settle out of court.

For our purposes, *Halliburton II* is relevant because it shifted Class Certification from a mainly procedural step and gave the Defendant firm an additional opportunity to get their case dismissed. Specifically, the Supreme Court stated that if a defendant brought sufficient evidence, in the form of a case study, that the fraud in question did not impact their stock price.

Taken at face value, *Halliburton II* should have added an additional level of rigor to the system. In other words, all else being equal cases with less robust merits should now have a more difficult time achieving a settlement. We later attempt to test this idea empirically.

2 Literature Review

2.1 Theory of Class Action Lawsuits

Implicit in our discussion so far is this overarching idea that the current status-quo is inefficient or at least not ideal. We now review some literature about why class actions are useful and what they can achieve given they are in equilibrium [3].

In *The Problem of Social Cost*, Ronald Coase wrote "Judges have to decide on legal liability but this should not confuse economists about the nature of the economic problem involved" (Coase 1960) . If we understand securities fraud lawsuits as (perhaps morally justified) transfers of wealth between two parties there really is no interesting economic question.

Typically speaking, economists view class actions as a quasi-public good. By organizing as a class, plaintiffs share the cost of an attorney who would have been too expensive had they operated alone. There is also the positive externality that by bringing the case to court, it is a deterrent to future undesirable behavior. Rubinstein writes, “the class form represents a

[3] We are not formally or rigorously defining an equilibrium here. We just loosely use the term for what a sustainable/ideal system

government intervention in the individual litigation market aimed at producing small claims cases so as to generate the positive externalities of such lawsuits" (Rubinstein 2006). In the case of securities fraud class actions, authors like Strauss acknowledge that these cases are an an extension of the role of the (chronically underfunded) SEC Enforcement Division (Strauss 2021).

This is all to say that, securities class actions are socially valuable, beyond just restoring damages to defrauded investors, because they penalize misbehaving firms and deter other firms from future fraudulent activity [4]

But this is not to say that securities class actions, even if they appear like a semi-public good, have strictly positive marginal returns. In fact, one might claim that the status-quo leads to *too many* claims being filed. Kessler and Rubinfield, writing about medical malpractice insurance, observe that at a certain point too many lawsutis leads to a market failure that "create[s] incentives for too much precaution or 'defensive medicine' " (Kessler and Rubinfield 2007). This is when doctors prescribe treatment not because its warranted but as a prophylactic measure against a potential lawsuit. Securities fraud lawsuits distort firm behavior in the exact same way. The "defensive medicine" takes the form of over-investing in compliance, accounting, and legal departments rather than organic growth.

This all suggests that there might an efficient or optimum level of securities fraud litigation that maximizes fraud deterrence while not significantly distorting firm behavior.

In theory, if we start with the premise that there was an over-saturation of securities fraud cases before *Halliburton II* , one might say that the ruling made the legal system more *economically efficient* if it reduced the perceived threat of securities fraud suits.

2.2 Legal Literature Following Halliburton II

The most salient body of literature to this paper is the group of legal scholars who opined on the effects of *Halliburton II* when the decision was handed down back in 2014. To our knowledge there has not been a formal study of *Halliburton II* on litigation outcomes. In this

[4]This requires we assume that corporate fraud is a general social ill.

light, this paper serves as an empirical test/validation of the legal academics, in particular Murdock and Pritchard, who are writing at the time of the decision.

Both legal academics claim that typically class certification is a purely procedural step that takes place over a three-day window. Given the complexity of event studies, they doubt a judge can be convinced to dismiss a suit in that time period especially because the plaintiff will inevitably produce his own event study. Pritchard writes, "Halliburton II's price impact defense will encourage defendants to put on economists to testify that the alleged misstatements did not affect the market price. Plaintiffs will respond with their own economists who will testify that it did…[Trail judges], faced with conflicting economic evidence that they are scarcely equipped to evaluate, will opt to certify a class" (Pritchard 2015). Likewise, Murdock, stressing that class certification is a purely procedural step, doubts that a judge would be a willing to opine on a "fact-intensive question" like a case study that typically belongs in the next step of the trial. At least some early evidence confirms these predictions, Murdock, observing in the end 2014, found that lower court interpretations of Halliburton II "[had] generally been favorable to plaintiffs" (Murdock 2015). The law firm Paul Hastings LLP even claims that it was not until April 2016 that an appellate court[5] would use Halliburton II to dismiss a case at the class certification phase. The actual language of Halliburton II stated that a defendant could get the case dismissed at class certification given they showed the fraud in question had no impact on the security's price. While event studies were popular in securities fraud cases before Halliburton, the Supreme Court's ruling officially labeled them as a legitimate piece of evidence (Fisch et al 2018). Fisch et al (2018) writes that the ruling made event studies an essential piece of evidence for plaintiffs and defendants. However, the authors do not opine whether this helps or hurts defendants.

A great deal of the literature following Halliburton II, revolves around establishing the proper statistical methodology and accuracy to prove/disprove whether a certain fraudulent behavior impacted a stock's price. See for example Fox (2015), Murdock (2015), and Fisch et al (2018). Even today, there is no strong judicial or academic consensus on the proper role of

[5]Note that this finding only applied to an Appellate Courts (Antonelli et al w016).. The majority of Securities Fraud litigation happens at the District Court level.

event studies in these cases. For example, Fish and Gelbach (2020) argue that a 95% Confidence Level, while commonplace among studies in the social sciences, is an inappropriate threshold when considering legal liability. This all suggests that *Halliburton II* was not feasible precedent to go out and apply. Even if it was ruling designed to improve the securities litigation, it might be ineffective because judges struggle with case studies.

2.3 Empirical Studies of Securities Fraud Class Actions

As stated, there is a gap in the literature in terms of an empirical assessment of *Halliburton II*. However, in 1995 Congress passed the Private Securities Litigation Reform Act (PSLRA); one of the goals of this legislation, like *Halliburton II*, was to discourage meritless securities fraud suits. By examining the literature on the efficacy of the PSLRA, we can draw some inferences on methodology.

Choi (2004) aggregates a number of studies that empirically investigated the PSLRA, the two most relevant for my paper being Gilberston and Avila (1999) and Bajaj et al (2003). Because "underlying merits of a suit" cannot be directly observed, they suggest several ways to proxy it. Gilbertson and Avila focus on the time between the end of the class period and the suit filing [6]. They assume that a legitimate case takes time for attorneys to dissect and understand the parameters of the case before filing. Meanwhile, attorneys have to rush to file frivolous case due to competition. Bajaj et al looks at the fraction of cases dismissed within one year, two years, and so on, before and after the PSLRA. We will follow a partially similar approach in our investigation of dismissal rates.

Many papers referenced in Choi, along with most papers about litigation outcomes, do not really go outside of using changes descriptive statistics to show causal effects. One of the goals of this paper is to offer a more rigorous approach through differences-in-differences estimation.

[6] Unfortunately, we cannot replicate this approach because our data set does not include class period

3 Data

3.1 Data from the Securities Class Action Clearinghouse

The primary dataset was provided by Cornerstone Research; it is partial copy of the Securities Class Action Clearinghouse, a database maintained by Cornerstone and Stanford Law School. The data includes observable information about 2,368 federal securities class action suits filed from 1/3/2008 to 12/24/2020. The observations are updated as of the end of 2020. The key variables of interest are the filing date, industry sector[7], district court, date of case's last stage, category of the last stage, the case status as of 12/31/2020, and whether the case is a Section 10b or Section 11 claim.

Date of case's last stage, given the case is not ongoing, is the last point in the case's life. By subtracting case's last stage from the filing date we can obtain the case length.

Twenty-three observations have their Case Outcome as "Remanded". This means that an Appeals Court ordered the case to be reheard; typically, this happens when the original District Court judge made some form of error. We drop these observations with the rationale that they are probably structurally different than cases that had the opportunity to go through their natural life cycle in the court system. For instance a remanded case, all else being equal, will have a longer case length simply because the original judge mishandled the case.

Whether the suit revolved around a Section10b or a Section11 claim will be important for our methodology; we plan to use Section11 claims as the control group in our diff-in-diff approximation. Section10b claims are typically what people refer to when they talk about securities fraud actions. Section 11 claims are a separate type of securities fraud suits that are specific to damages accruing in an initial public offering [8]

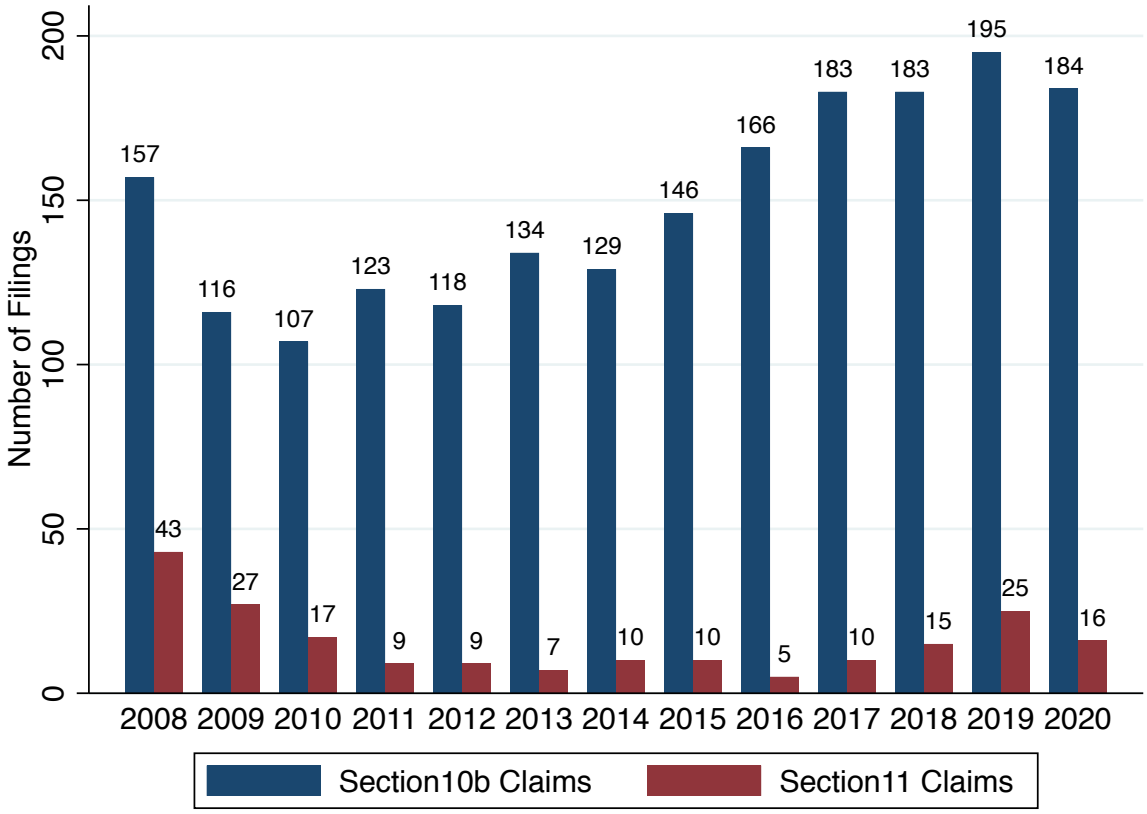
Eighty-three of the observations include neither a Section10b or a Section11 Claim and are dropped. Its possible that these filings relate to Section20 which is a rarely cited provision. Finally, 133 observations contain both Section10b and Section11 Claims; trivially, an observation

[7]Sectors include: Basic Materials, Capital Goods, Conglomerates, Consumer Cyclical, Consumer Non-Cyclical, Energy, Financial Services, Healthcare, Services, Technology, Transportation, Utilities, and Unknown

[8]Section10b and Section11 Claims actually refer to seperate laws. Section11 is found in the 1933 Act while Section10b is part of the 19 Act.

cannot be in both the treatment and control group so we drop these. In the end, we are left with 2,144 observations made of up of 1,941 Section10b cases and 203 Section11 claims.

Figure 2: Number of Filed Cases by Year

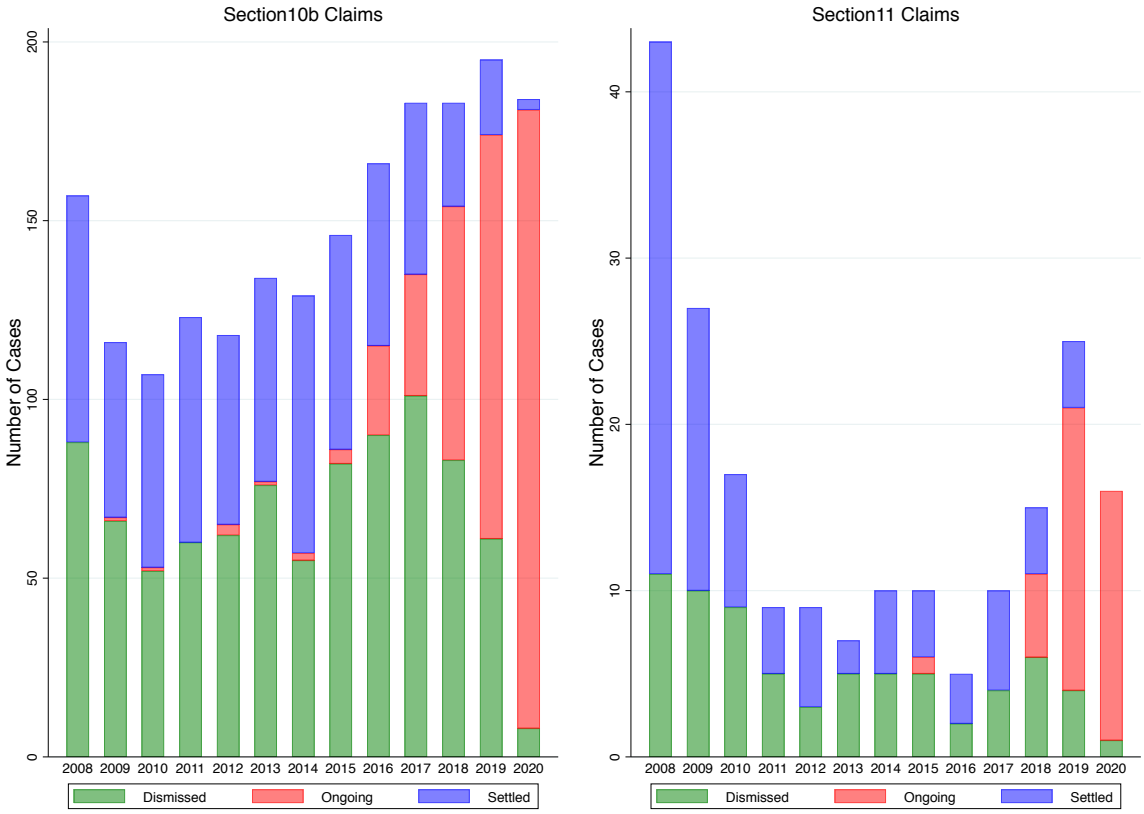


3.2 Digression on Selection Bias: Ongoing Cases

A significant hurdle in our dataset is that many recent court cases are still ongoing. As of December 31, 2020, 428 Section10b cases and 38 Section11 cases were still being litigated. As seen in Figure 2, a significant portion of cases filed in 2018 onward are still in the court system. These presents a problem because it could introduce selection bias.

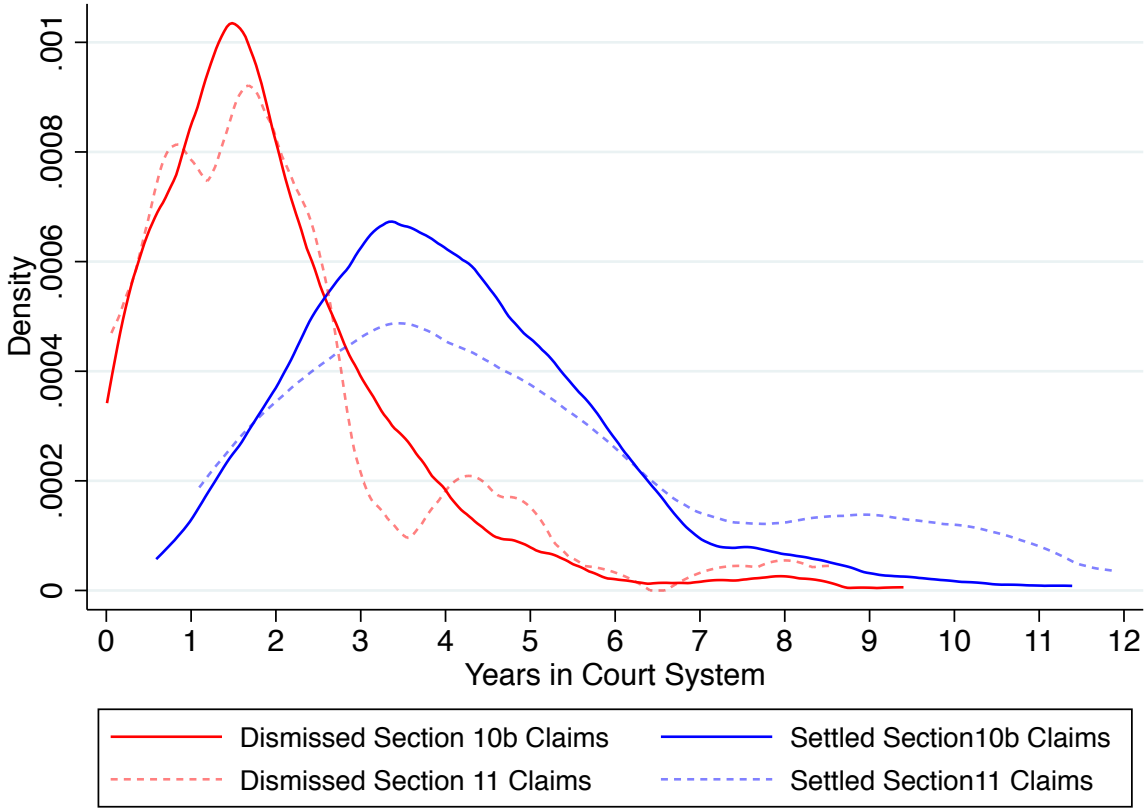
In particular, we might guess that the longer a case is ongoing carries information about whether its likely to be dismissed or not. To examine this hypothesis, we plot the kernel densities of case length for settled and dismissed Figure 4, however we restrict to cases filed between 2008 and 2015. Relatively few cases within this time period are still ongoing so we can be pretty confident we are looking at a range where (almost) all cases had the opportunity

Figure 3: Case Outcomes by Filing Year



to complete their lifecycle through the Court System. Figure 4 illustrates that dismissed cases follow a very different time dynamic than settled case. The kernal density of the length of dismissed cases shows a really high mass around one to two years, and then displays a long tail. Settled cases meanwhile are rarely completed within one-year and follow a more bell-shaped distribution. This dynamic ought to be expected. Recall (see Figure 1), that the first stage of the legal procedure is a formal motion to dismiss which might take up to two years to overcome. Figure 4 suggests that if a case survives the first two years, the likelihood it gets dismissed dramatically declines.

Figure 4: Kernel Density of Case Lengths: Restricted to Cases Filed from 2008-2015



To summarize, we should absolutely be worried about selection bias caused if we dropped ongoing cases. For example, if we were looking at the probability of the case being dismissed, the observations filed in 2019 will be structurally different. (If we are only picking up cases completed within two years we will over-represent the likelihood a case gets dismissed)

3.3 Proxying Firm Size with Total Revenue

As a control for a measure of firm size we attempted to gather the Total Revenue of defendant firms in the year that they were sued. Using stock tickers, we queried the Wharton Research Data Services site searching for a firm's Total Revenue on December 31st of the year it was sued.

Of the 2,144 different lawsuits, we were only able to recover the Total Revenue of the Defendant Firm in 1,370 observations (1,257 Section10b claims and 113 Section11 claims). There are a variety of reasons that might explain why we were unable to obtain Total Revenue.

For example, the firm may no longer exist, have gone private, or was delisted following the fraudulent event in question. Furthermore, some defendants in the dataset are not publically traded companies but different types of mutual funds.

We use the data on Total Revenue cautiously and fully aware that we could very easily be opening ourselves to selection bias by including it in our regressions. Given that our observations occur from 2008 to 2020, we normalize total revenue to 2008 dollars using the U.S. CPI found on the U.S. Bureau of Labor Statistics website.

Finally, a simple inspection of our observations for Total Revenue reveals the the data is very right skewed. To reduce the weight of extreme outliers we therefore take the log transformation of Total Revenue (see Appendix B Figure 12 and Figure 13).

3.4 U.S. IPO Volume

When we investigate number of annual filings one of the controls we add is the annual volume of initial public offerings in the United States. We pull this series from PwC's 2020 Annual Capital Markets Watch Report.

3.5 Digression on Cases Filed in 2008 and 2009

Throughout 2008 and 2009 the world economy was in the midst of a massive financial crisis and severe economic downturn. Given the severe and atypical conditions from the Great Recession, its very feasible that our observations from 2008 and 2009 might be distorted and not representative of typical securities fraud claims. As already claimed, the current legal procedure makes enables lawyers to entertain any material negative price impact as evidence of securities fraud. Given this was a period where equity prices were systematically weak, we should expect there to be more claims of securities fraud during the Great Recession.

Returning to Figure 2, 2008 and 2009 were extreme outliers for Section 11 claims. More Section11 cases were filed during those two years than the next seven years combined.

Please see Figure 16 in Appendix B for a brief illustration of other irregularities we see in the data during 2008 and 2009.

4 Methodology

4.1 Motivation for Differences-in-Differences Estimation

As stated, the majority of the literature that studied the PSLRA looked at summary statistics before and after the legislation. The problem with this approach is that there is no way to isolate the *causal effect* of the policy. In this spirit, we use a differences-in-differences model to measure the effect of *Halliburton II* on litigation trends. The utility of a diff-in-diff analysis is that we can now control for factors in the post-treatment period that we can not observe which might impact our outcome variables. In the context of securities fraud litigation, these factors might include developments in the legal sector, changes in government decisions/policies around securities fraud (e.g. enforcement actions by the SEC), changes in court procedures, administrative problems in the court system, etc. By for these factors, our diff-in-diff method will allow us to make a stronger claim about the treatment effect of *Halliburton II* on litigation trends.

4.2 Section11 as a Valid Control Group

A robust diff-in-diff estimation requires a valid control group. We claim that a valid control group should satisfy two properties:

1. The control group cannot receive the treatment. In other words, our control group cannot be impacted by *Halliburton II*
2. We expect unobserved factors to influence the treatment and control group in the same way

We claim that our proposal to use Section11 claims satisfies both properties. Recall that *Halliburton II* established a "no price impact' defense at class certification. More specifically, if a defendant could produce enough evidence, through a highly quantitative case study, that the fraud in question did not impact the security's price then the case could be dismissed. We argue that this type of evidence would be irrelevant to a Section11 case; econometric case studies are much less applicable to these types of cases. The fraud in a Section11 claim usually

contained in the IPO Prospectus–*before* the security becomes public. There is simply no price history to establish whether the security was trading at inflated/deflated values. So while *Halliburton II* became part of the legal precedent, it just not the kind of decision that would be relevant to a Section11 case.

Regarding the second condition, Section11 claims *are still securities fraud class actions.* So for example, general rulings or SEC guidance on what kind of behavior constitutes securities fraud would impact Section10b and Section11 litigation similarly. Both Section10b and Section11 claims are filed and argued in federal courts. If a judge (or the Court system in general) becomes backlogged with outstanding cases, this should impact a Section10b and a Section11 case in a parallel manner. Note that our diff-in-diff model slightly changes depending on the outcome variable we are studying:

4.3 Our Base Regression

When we study Dismissal Rates, Number of Cases, and Probability that a Filing is in the Southern District of New York, we use the following base regression:

$$Y_i = \beta_0 + \beta_1 section10b + \beta_2 afterDecision + \beta_3 (section10b \cdot afterDecision) + \beta_4 \gamma_i + \beta_5 \kappa + \beta_6 \alpha_i + \varepsilon_i$$

Y is the particular outcome variable we are interested in (e.g. dummy for if the case was dismissed , case length in days, dummy for if the case is filed in the SDNY). γ, κ , and α are fixed effects for filing year, industry sector, and the district court. We account for district court; the case's jurisdiction carries significant weight on how a claim progresses. Some jurisdictions might be more backed up with cases. It also accounts for differences in the qualifications/types of judges that oversee the case. For example, a judge in the Eastern District of Oklahoma probably has less experience than a judge in the Southern District of New York. For these reasons, we opt to cluster out standard errors by district court where feasible.

section10b is a dummy variable. In terms of our diff-in-diff approach, this is a dummy for whether the observation *is in the treatment group.* Similarly, *afterDecision* is a dummy for it the case was impacted by the *Halliburton II* ruling; i.e, if an observation was in the *post-treatment*

period.

In standard diff-in-diff analysis, the coefficient on the interaction term, in this case β_3, recovers the *treatment effect* of the policy which in our context is the impact of the *Halliburton II* ruling.

4.4 Additional Specifications to the Regression

In addition to our base regression, we also try adding Total Revenue as a control variable for firm size. We think this is an important control because the larger the firm, we might infer it has greater resources to fight an expensive court battle. Small firms on the hand might have less cash on hand to pay lawyers; furthermore, they probably have higher incentives to settle early on; there is a higher probability that the lawsuit could bankrupt the firm if they risk a jury trial (and a much higher settlement amount). This is all to say that we think that a defendant's size should carry significant information on how the case progresses. That said, when we attempted to gather data on Total Revenue we could only confidently obtain it in 64% of cases. As discussed, we have to acknowledge that there probably is some selection bias going on and we might be dealing with a non-representative sub-sample. We proceed accepting this as a limitation of our study. As already claimed, we worry that observations from 2008 and 2009 were drawing from non-representative years, therefore we rerun the model dropping these years. So for example, if one looks at Appendix A Table 1 there will be four regressions:

1. The base regression
2. The base regression excluding and 2009
3. The base regression controlling for Total Revenue; note that this regression is restricted to the subsample of observations where we could measure Total Revenue
4. The base regression controlling for Total Revenue excluding 2008 and 2009

A priori, we believe that the second specification is the most robust and demonstrative.

4.5 Digression on Assigning "Treated by Halliburton II"

Recall that the unit of observation in our dataset is an individual case. Some of the variables such as District Court and Sector are observed at the start when thee case is filed. Other varibales such as Case Length and Case Outcome are not observed until the case is decided. This presents a challenge for how we should assign cases as *treated by Halliburton II*. If the outcome variable is observed when the case is originally filed (eg District Court) this is not a problem. We can assign cases filed before *Halliburton II* was argued as in the pre treatment group and every thing else we assing as treated.

Note that we use the date *Halliburton II was argued* and not the date the decsiion was released. We make this distinction to avoid distortions from anticipation effects. In particular, its quite common place for attorneys to parse through a Supreme Court Case's oral arguments and guess their decision in advance. In practice, this should not make a significnat difference; there is only a three-month gap between the argument and decsion date.

However, if the case was ongoing when *Halliburton II* was decided and we are observing Case Outcome or Case Length; its less trivial how to decide which cases actually should be *treated* by *Halliburton II*. After toying with different specifications we employ the following methodology:

- If the case concluded before *Halliburton II* was argued then trivially it was *not treated by the decision*
- If a case was ongoing when Halliburton was argued, and the case is younger than two years,*is treated by Halliburton II*

Our assumption is that cases younger than two years still have the capacity to reach the class certification phase and be influenced by the ruling. We make a blunt assumption that any case older than two years is already past class certification and would not be impacted by the ruling.

5 Hypothesis

Recall that the original spirit of *Halliburton II* was to streamline securities fraud litigation. In particular, the decision gave judges more discretion to discard cases that had dubious underlying merits. Given this, we form the following hypothesis about out outcome variables:

1. **Dismissal Rates** - Given that the ruling gave firms an additional opportunity to get their cases dismissed, all else being equal we should expect dismissal rates to increase following *Halliburton II*

2. **Case Lengths** - There are two intuitions for how *Halliburton II* impacted case duration. One, the cases that were eventually going to get dismissed during Summary Judgement anyway can now be dismissed earlier in the legal process. And two, the case that have stronger underlying merits now have another potential barrier before they can reach a settlement. Plaintiffs now have to go and rebut the defendant's claim of *no price impact* meaning the case will last longer. In summary, cases that tend to be dismissed should tae shorter, and more robust cases, ie those more likely to be settled, spend more time in the Court system. By looing at the average case length we can see which effect dominates.

3. **Number of Annual Cases**- Recall that the plaintiff attorneys are primarily compensated from the settlement fund. A ruling like *Halliburton II* that increases the opportunity for a case to be dismissed should, at least in theory, make securities fraud less lucrative for an enterprising attorney. More specifically, will be more selective in the cases they take up and stick to the cases that have a legitimate chance of being settled.

4. **Filing Activity in the Southern District of New York** - Recall that Murdock and Pritchard argued that *Halliburton II*, and its affirmation of econometric case studies, probably would not have much of an impact because would be reluctant to apply to the precedent. One way we can invetigate this claim is by looking the probabilty a case is filed in the Southern District of New York. Historically, the SDNY is where most of the nation's (federal) financial crimes take place. By extension, the judges at the SDNY are thought to be "experts" on the intersection of law and financial markets. If anyone

would be comfortable appliyng the *Halliburton II* decision it would be SDNY. Knowing this, we investigate whether plaintiff attorney's become less likely to file in the SDNY after the ruling in orderr to get an easier judge.

The hypotheses above are just our surface level intuition of how *Halliburton II* should effect litigation trends. In reality, there might be some interaction between these outcomes that will complicate our analysis and our results for each outcome should not be discussed in isolation. For example, if in a post *Halliburton II* regime lawyers file less often because now they are choosing cases with stronger underlying claims then we should see dismissal rates decrease and case lengths actually go up simply because the composition of cases that get filed have stronger underlying merits. [9]

6 Empirical Results

6.1 Case Dismissal Rates

6.1.1 Examination of Pre-Trends

A primary assumption of the differences-in-differences methods is that the control group and treatment group follow parallel trends in the pre-period. The word parallel trends might be misleading. More precisely, we would ideally want a consistent difference in the outcome variable between the two groups in the pre-period. This is evidence that our control and treatment group are generally affected by unobserved variables in a comparable way.

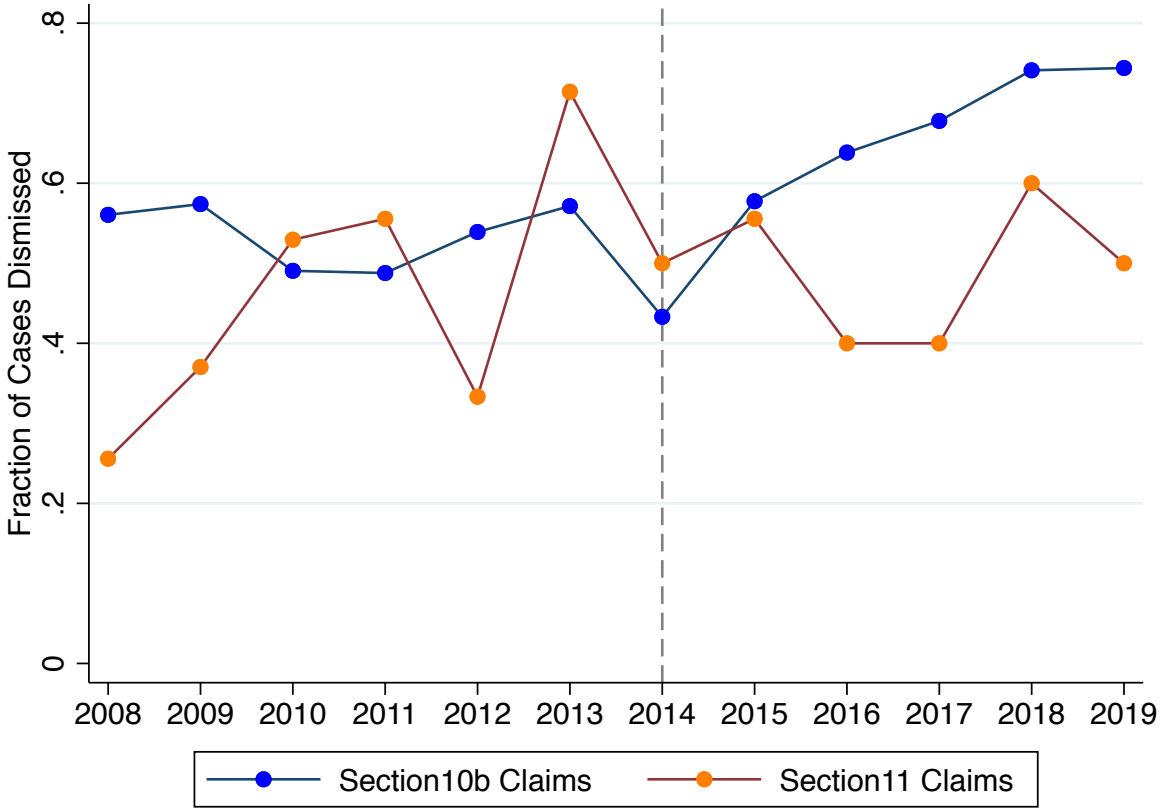
In Figure 5, we collapse our observations by filing year to get the fraction of cases that were ultimately ended up being dismissed. Right now, we start by simply omitting the ongoing case. [10]

Unfortunately, a major limitation in our analysis is that the dismissal rate of Section11 claims is simply very volatile at the year level. This is partially a reality that we simply do not have as much data on Section11 claims because they are less popular. For example, between

[9] Trivially, if more cases are legitimate grievances of securities fraud then the dismissal rate should go down. Furthermore, we saw in Figure 4 that dismissed cases tend to be shorter so average case length should increase.

[10] In other words, if a case was not dismissed, then it had to have been settled.

Figure 5: Fraction of Cases Dismissed versus Filing Year: Dropping Ongoing Cases



2011 and 2013 there were less than 10 claims filed each year.

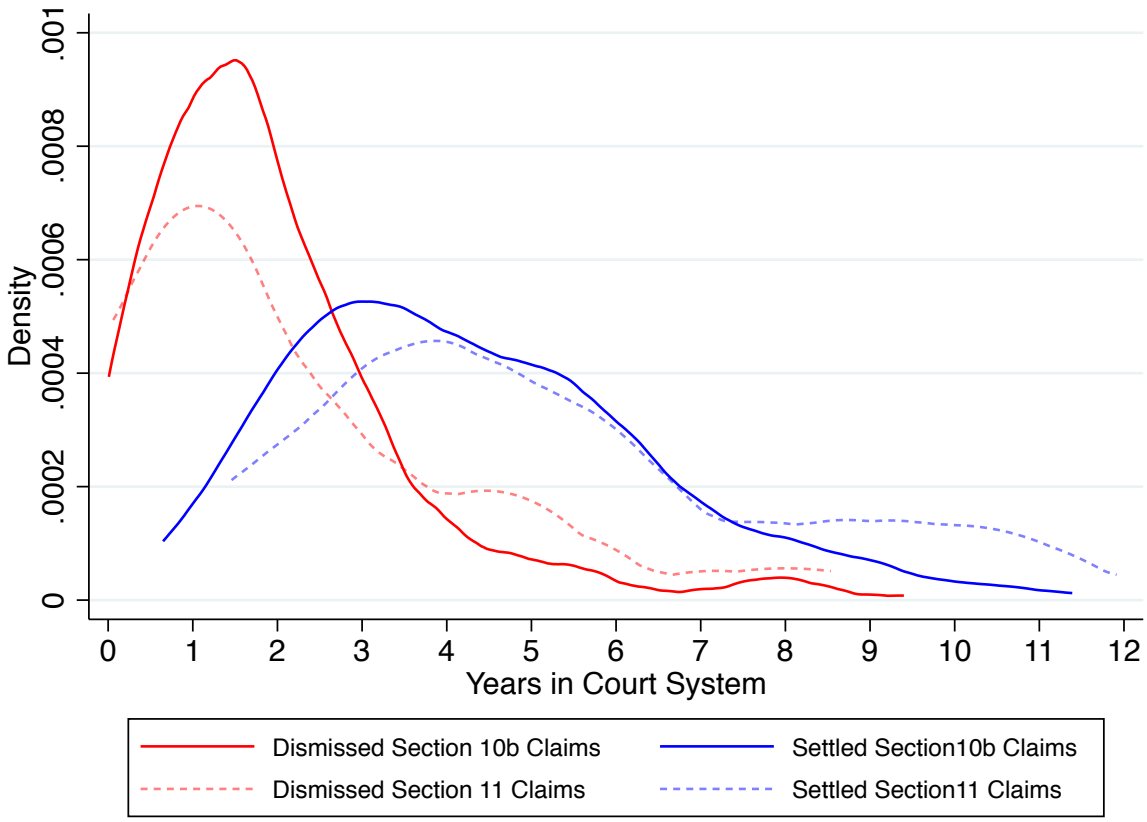
In a perfect diff-in-diff model, there should be a relatively consistent gap between the control and treatment group in the pre-period. Unfortunately, the Section11 data is just too noisy and there is no real discernible relationship. That said, our intuition that Section11 is a valid control group still holds, and we proceed noting that there might be some flaws in our diff-in-diff estimates because of a failure of a parallel trends.

Digressing, we already mentioned that there might be some selection bias when we drop ongoing cases. As previously written, the observations from 2016 onwards all contain a significant number of ongoing cases. By omitting them, we are selectively sampling cases with shorter judicial lifeycles which tend to be dismissed cases. This might explain why we see an uptick in Section10b dismissal rates from 2015 to 2019.

Now in theory, our control group should account for this if Section10b and Section11 cases follow the same time dynamics at least in the pre-period (its possible that *Halliburton II* had

an effect on Section10b case lengths). [11]

Figure 6: Kernal Density of Case Lengths: Cases Identified as Not Treated by Halliburton II



Looking at Figure 6, Section10b and Section11 seem to exhibit the same time patterns. [12] Mainly that there is a high mass of dismissed cases in the first two years; settled cases seem to take longer and follow something closer to a bell curve. That said, the distributions are not identical. Compared to the Section10b claims, both settled and dismissed Section11 claims have fatter tails and more damped.

To be rigorous, we now proceed with two separate sets of regressions. In the first, we assume that Section10b and Section11 cases follow the same time patterns. In other words, the diff-in-diff estimator "can handle" the selection bias and we can feel okay dropping ongoing cases. We then relax this assumption, and try to use fraction of cases dismissed within one year, two years, three years, etc. to get around this problem of ongoing cases.

[11] e.g. If we are oversampling dismissed Section11 claims and oversampling dismissed Section10b cases, the diff-in-diff estimator should control for this.

[12] Note all these cases were filed way back in 2014 or before. With a few exceptions almost all the observations in the pre-period were able their lifecycle through the court system.

6.1.2 Dropping Ongoing Cases

First, we drop ongoing cases assuming this will not lead to any selection bias. This gives 1,678 total observations, 1,513 Section10b claims and 165 Section11 Claims. Given that our dependent variable is a dummy that equals one if a case is dismissed and zero if its settled, the coefficient on our differences-in-differences estimator can be loosely interpreted as the effect of the *Halliburton II* decision on the probability that a case will be eventually dismissed [13]. Please see Table 1 in Appendix A for our regression output. The diff-in-diff estimator, the coefficient of interest in this context, is labeled as "Treatment Effect" in our tables.

Across our four regressions, none of the estimates of the treatment effect are statistically significant at the 10% significance. Our base regression estimates a treatment effect of -0.118 with t-stat of -1.17. Taken at face value we can say with an 75% confidence level that *Halliburton II* reduced the probability a case is dismissed by 11.8%. This is more consistent with the idea expressed earlier that post *Halliburton II* the cross-section of cases were stronger in terms of merits. That said, we are cautious to make any kind of strong claim. After dropping the financial crisis the estimated coefficient *becomes positive* at 0.0916 with a p-value of 0.477. The most statistically robust estimate of the treatment effect comes from the specification that controls for total revenue and drops 2008 and 2009. The specification estimates a treatment effect of -0.228 with a t-stat of -1.44; this corresponds to an 85% confidence level. While this seems more robust, we already mentioned that this regression might be based on a non-representative sample. In addition, a treatment effect of -22.8% seems suspiciously large in magnitude. Recall that most legal commentators writing at the time of *Halliburton II* thought the case's impact would be minimal.

Given the wide variety of results, we cannot easily conclude anything about the *Halliburton II* decision on dismissal rates. Perhaps, part of this stems from our decision to omit ongoing cases.

[13] Assuming that the only two final case outcome are settled and dismissed. Recall we dropped remanded cases

6.1.3 Assume that Ongoing Cases cause Selection Bias

To avoid the problem of ongoing cases, we attempt to control for the amount of time was exposed to the legal system. In particular, we define four new outcome variables: the probability that a case is dismissed in one years, two year, three year, and finally four years [14]. The idea is that we are now comparing dismissals of cases which had comparable levels of exposure to the legal system. To that end, when we define the dummy variable for a case being dismissed in two years, we drop cases that were filed in 2019. Our justification is that some of those cases did not two full years of exposure to the legal process and hence are non-representative. (Recall that our dataset is updated as of 12/31/2020). We likewise drop observations from 2017 onward and from 2016 on wards when we define "Dismissed in Three Years" and "Dismissed in Four Years". Note that while we lose our most recent observations, we also "gain" some observations back because we no longer are dropping ongoing cases.

A priori, we think that the coefficient for the treatment effect for "Dismissed in Four Years" should be a decent proxy for the effect of *Halliburton II* on dismissal rates. Only around 6% of dismissed cases (Section10b and Section11) in the entire dataset last longer than four years. Given that *Halliburton II* is directly relevant to cases in the class certification phase around, we hypothesize that the treatment effect for "Dismissed in One Year" and "Dismissed in Two Years" should be innocuous. These cases usually get tossed out because of a Motion to Dismiss which the *Halliburton II* ruling has noting to do with.

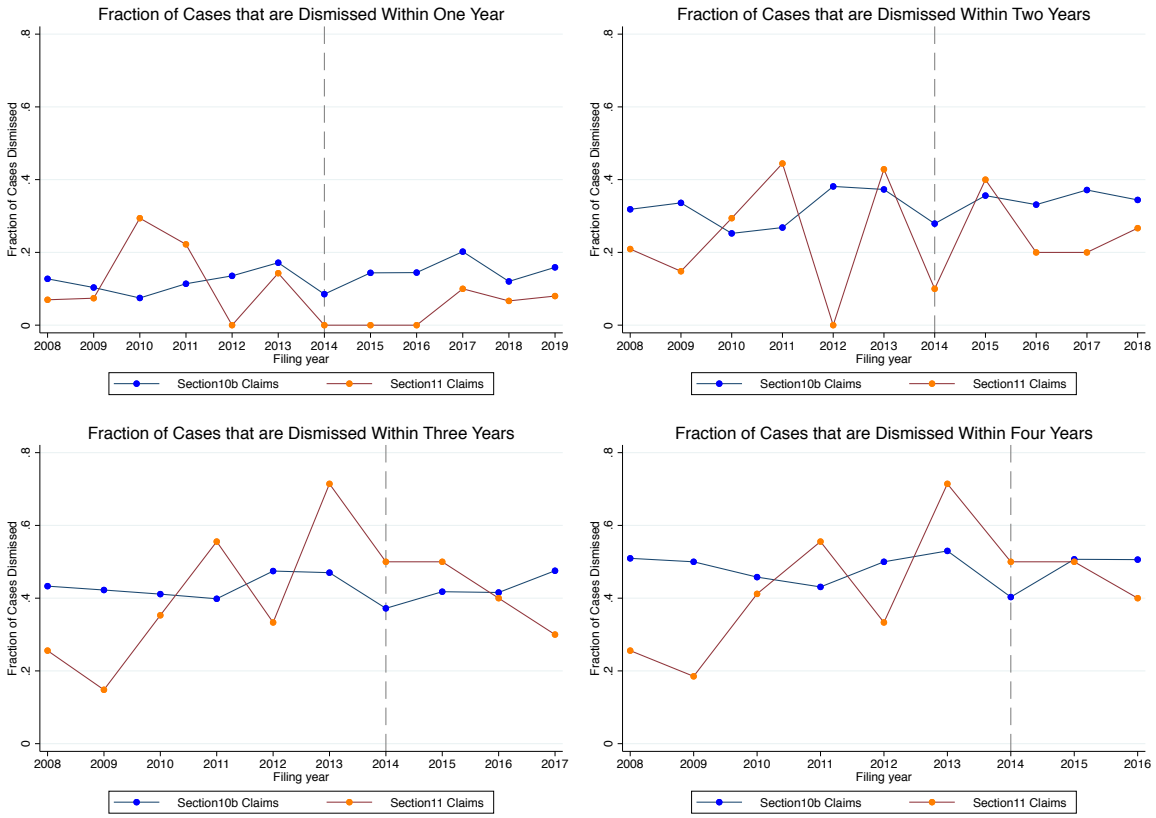
Unless of course we believe that the *Halliburton II* ruling significantly incentives lawyers to pursue stronger claims thereafter. Than of course we might see the fraction of cases dismissed in one year go down. Also note that if *Halliburton II* significantly altered the duration of cases than our analysis would be flawed. In that case, "X years in the legal system" before the ruling would carry different weight than "X years" in a post *Halliburton II* regime

As before, we do a cursory inspection of pre-trends for each of the four different outcome variables. To our dissatisfaction, the data for Section11 claims is just too noisy at the year level that we can feel confident in the parallel trends assumption. As before we will have to accept

[14] Bajaj et al

this as a limitation to our analysis.

Figure 7: Pre-Trend Inspection



The results are partially consistent with our expectations. In all four regression specifications,[15] the estimated coefficient on the Treatment Effect when the outcome variable is "Dismissed in Four Years" *is negative*. In three of the four specifications, these coefficients are at statistically significant at least at the 5% level. (See Table 2, Table 3, Table 4, and Table 5 in Appendix A).However, their magnitudes seem improbably high. For the base regression, the estimated treatment effect on the probability of dismissed in four years was -0.265. The estimated coefficients that include a control for (log) total revenue were -0.353 and -0.484 if we drop observations from the financial crisis. Taken at face value, this last coefficients states that *Halliburton II* was responsible for a 48.4% reduction in the probability a case is dimissed in four years. As already claimed, *Halliburton II* was a small technical change in the type of evidence that a judge might consider only during class certification. These estimated treatment effects

[15]Recall: Base Regression, Base Regression excluding 2008 and 2009, Base Regression controlling for Total Revenue, and Controlling for Total Revenue excluding 2008 and 2009

seem suspiciously high in magnitude.

The only regression that *did not* show have a statistically significant estimated treatment effect on the probability a case was dismissed in four years was the base regression excluding 2008 and 2009. Recall that this is the specification we feel the most confident in. It produces an estimated treatment effect of -0.0774 with a t-statistic of -0.74. With a p-value of 0.460 this result is not statistically significant *at any reasonable* significance level. Given that the specification gave dubiously large treatment effects, we feel confident that, at least for the probability that a case is dismissed in one year, *Halliburton II* probably had a minimal, if any, impact. This result would be consistent with Murdock and Pritchard's assessment that *Halliburton II* should have a limited impact on cases in general.

One non-trivial result (seen Table 2 and Table 3 of Appendix A) is that for both the base regression and the base regression excluding the financial crisis, the treatment effect is positive and statistically significant at the 1% level when our outcome variable is "Dismissed in One Year" (We estimate coefficients of 0.0899 and 0.236). Meanwhile, the treatment effect for "Dismissed in Two Years" and "Dismissed in Three Years" is not *not* statistically significant at the 10% level Recall that during the first year of a case's life, the claim is typically wrapped up in a motion to dismiss which in theory should be the first opportunity for the system to discard meritless suits.

The fact that "Dismissed in One Year" saw positive and statistically significant treatment effect (but not for "Dismissed in Two" and "Dismissed in Three") lends itself to the idea that there was a temporal shift. In particular, among cases that were dismissed in two or three years, a higher portion of those dismissals happen within the first year. This would lend itself to the idea that in a post *Halliburton II*, all else being equal, there are a greater portion of dispelled due to a motion to dismiss. By extension, the composition of cases after the *Halliburton II* decision contained a higher portion of frivolous suits. Its not obvious why *Halliburton II* would encourage lawyers to file frivolous lawsuits. This is exactly the opposite of our intuition. More likely, this puzzling result is a reflection of flaws in our analysis. Recall that we were not even confident in the parallel trends assumption.

If anything, our analysis of dismissal rates suggests that perhaps in aggregate the probability a given case *was not* dismissed was not impacted by Halliburton II.

Add note in appendix in how we do the methodology change

6.2 Case Length

6.2.1 Pre-Trends and Limitations

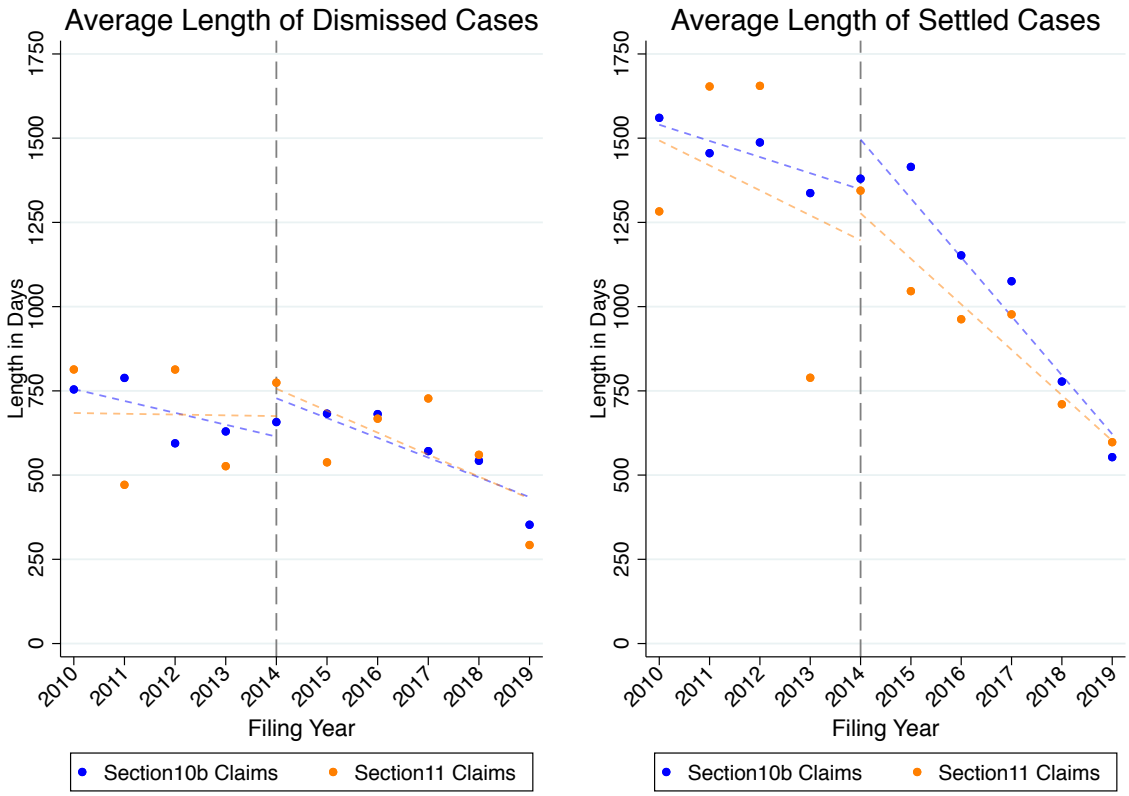
As we turn to examining case length, we remind the reader that we still might have to worry about selection bias in more recent years due to ongoing cases. Similar to Case Outcome, we only observe Case Length once a case concludes. So our observations in recent years (which have a lot morre ongoing cases) will be downward biased because we only observe the cases that ended rather quickly. In the previous subsection we tried to get around this obstacle by controlling for "Years in the Legal System". Now that we want to use case length as our dependent variable, this is no longer an option. We have to move forward assuming that the control group, Section11 cases, also is victim to selection biasing. Put differently, that Section10b and Section11 cases follow similar time dynamics. By looking at Figure 6, we already discussed that this is not a particularly expensive assumption.

We first divide the regressions by whether the case was settled or dismissed. Recall that we hypothesized that the case length of the type of cases that are robust enough to get settled will be higher after *Halliburton II*. Meanwhile, we predicted that dismissed cases should exit the court system sooner.

We begin by doing an inspection of pre-trends in Figure 8. Similar to what we saw in dismissal rates, the data for Section11 claims tends to be particularly noisy at the year level. Note that we did not include 2008 and 2009 in Figure 8 as those years contain significant outliers. See Figure 20 in Appendix B for a version of the plot with 2008 and 2010 included. To overcome the noisiness of the Section11 we add in a few linear fits to ascertain a better sense of how the data is trending. After we add this the length of Settled Cases shows decent parallel trends. More formally, there is consistent and predictable gap between Section10b and Section11 claims prior to 2014. Again, this inspection is critical to diff-in-diff because if the

control and treatment group behave consistently (in relation to one-another) in the pre-period that suggests that they are impacted in a similar manner to outside factors.

Figure 8: Average Case Duration: Settled vs Dismissed Cases



6.2.2 Average Length of Dismissed Cases

As discussed, 2008 and 2009 were significant outliers in terms of average case length (see Figure 20 in Appendix B). This is true whether or not one separates the observations between dismissed anzd settled cases. For these reasons, we focus on the specification that exclude observations from the financial crisis. For the case length of dismissed cases we we estimate a treatment effect in the base regression (excluding 2008 and 2009) of -92.57 days with a meager t-statistic of -0.52 (see Appenidx A Table 6). This value is not statistical significant at any feasible significance level (its p-value is over 0.6) and we cannot reject the null hypothesis that the treatment effect is zero. The specification that controls for log total revenue (and restricts to observations where total revenue was observed) yields a estimate treatment effect of positive to 243.6 days but possesses a t-stat of just 0.34. Even the regressions that include

2008 and 2009 do not yield statistically significant treatment effects and also have massive standard errors. For instance, the estimated treatment effect in the base regression (including 2008 and 2009) was -24.31 days but with a standard error of 120.1 days. In all, we are confident that there is not sufficient evidence to claim that the Halliburton II affected case length of cases (at least for settled cases)

6.2.3 Average Length of Settled Cases

Similar to Dismissed cases, there is limited evidence that *Halliburton II* caused the length of settled claims to change. When we run our base regression excluding 2008 and 2009 we estimate a treatment effect of -126.9 days with a t-stat of -0.71 (see Appendix A Table 7). If we not exclude the observations from the financial crisis, the base regression estimates a treatment effect of positive 292.7 days; with a t-stat of 2.40 this estimate is statistically significant at the 5% level. Furthermore, the positive sign does is consistent with our conjecture that *Halliburton II* introduced another hurdle for cases *with legitimate underlying claims* and would hence in aggregate make cases longer. Curiously, when we attempt to control for total revenue (and not exclude 2008 and 2009) we estimate a treatment effect of -256 days and its statistically significant at the 5% level. If we exclude observations from the financial crisis, goes all the way to -411.5 days; it also significant a the 5% level. This estimate is not consistent with our expectations. Its against our beleif that the length of settled cases would go *down* after the ruling, not to mention by over a year.

All said, we are most confident in the base regression not including observations from the financial crisis. We cannot conclude that the case length *settled* securities fraud claims were impacted by *Halliburton II*

6.2.4 Average Case Length

For completeness, we now quickly aggregate settled and dismissed cases and ask how *Halliburton II* impacted average case length. This section had more motivation back when hypothesized that the ruling impacted cases that tend to be settled and cases that tend to be dismissed differently. For an inspection of parallel trends please see Figure 21 in Appendix A. One would note

that if we exclude 2008 and 2009 the parallel trends assumption seems to hold. Our estimate of the Treatment Effect excluding observations from teh financial crisis, is -126.9 with a t-stat of -0.66 and a p-value of 0.51 (see Appendix A Table 8). We fail to reject the null at any reasonable confidence level.

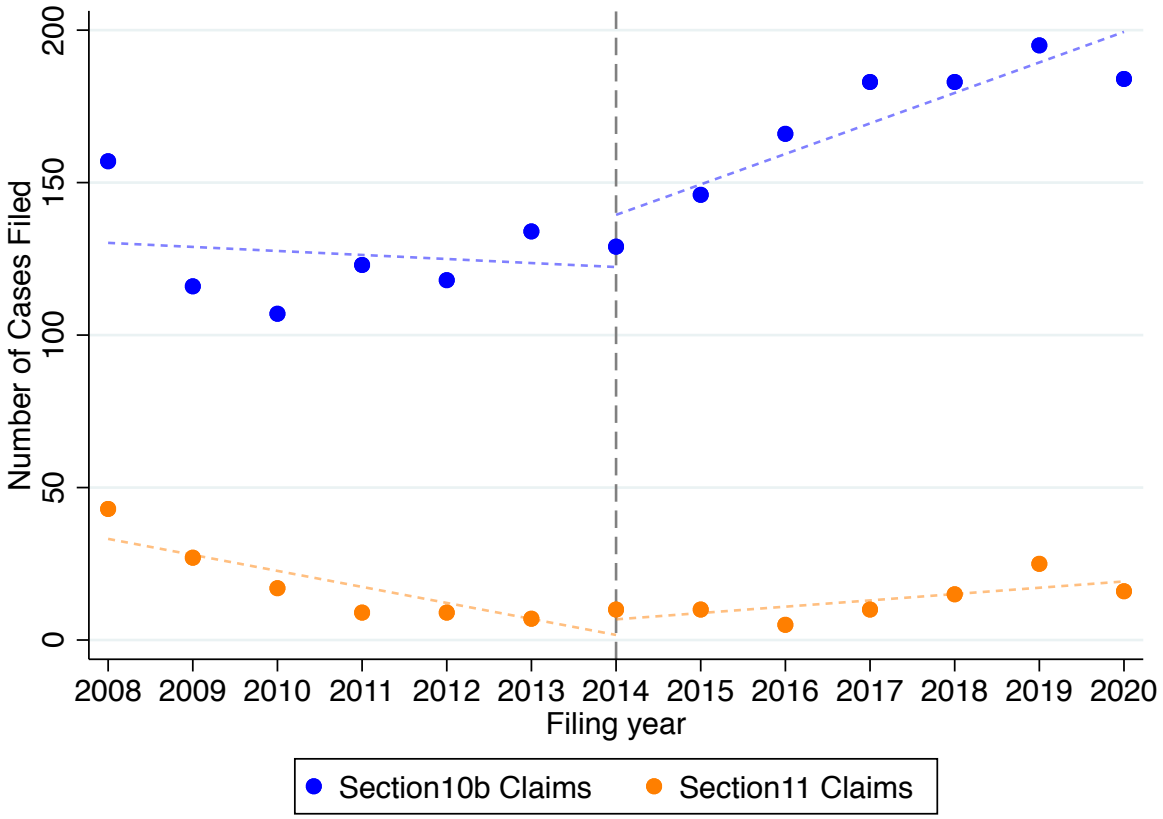
Using this result, we fail to reject the null hypothesis that *Halliburton II* had zero effect on the length of the average case.

6.3 Number of Cases Filed

After studying Case Outcomes and Case Length we now turn the Annual Filings. First we make a few technical notes. To begin with, we now include 2020 observations. There is no reason to *not* include them; there is no selection bias going on. Whether a case is completed or is ongoing is immaterial as of now, we are just concerned with the volume of yearly filings. We collapse our data to obtain the number of Section10b and Section11 claims filed in a particular year. In our diff-in-diff estimation, we simply drop 2014–the year *Halliburton II was argued and dedided* as a filing year. [16] Any filing year after 2014 is considered treated by *Halliburton II*.

[16] Therre was no obviosu way to count that 2014 observation as treated or not treated.

Figure 9: Average Number of Cases Filed by Year



The parallel trends assumption is not perfect but its pretty robust, both series look to be ticking down or flat in the pre-perriod; it appears that 2008 is a large outlier for both Section10b and Section11 Claims. See Appendix A Table 9 for the regression results. Our diff-in-diff model estimates a treatment effect of 55.5 with a t-statistic of 4.63; this is statistically signfiiniat a the 1% level (see Appendix A Table 9). In other words, our findings suggest that *Halliburton II* has increased the annual volume of Section10b filings by 55.5 cases. This is definerly a non-negligible amount; for instance, in the year *Halliburton II* was argued there only 129 cases field. We add a control variable for the amount of IPOs in the U.S. in a given year. We might assume that the volume of Section11 cases is a reflection of IPOs in a given year. We also add year fixed effects. This does not change our results, it turns out these controls had very little explanatory power. When we replicate the regression but drop 2008 and 2009, we estimate a treatment effect of 52.67 cases with a t-stat of 5.63 (see Appendix A Table 10), again our results are statistically significant at the 1% level.

This result on its puzzling if taken at face value. *Halliburton II* gave defendant's an additional opprotunty to get their cases dismissed. Our intution tells us that this should have made secruites fraud class actions more difficult to win and less attractive to an enterprising attorney looking to file a class action complaint. Or at the very least, it would constrain attorneys to file more cases with better underlying merits. Perhaps there is a flaw in the model or we are not accounting for something. In particular, maybe our control group is not as robust as we thought. Its possible that, since 2014, Section10b filings have gotten more popular in a manner that is not represnted in our data for Section11 claims. This would represent a significant flaw in our methodology; we suspect that this is what's driving the results in this section. We hesitate to conclude that *Halliburton II* actually led to significnatly more Section10b filings.

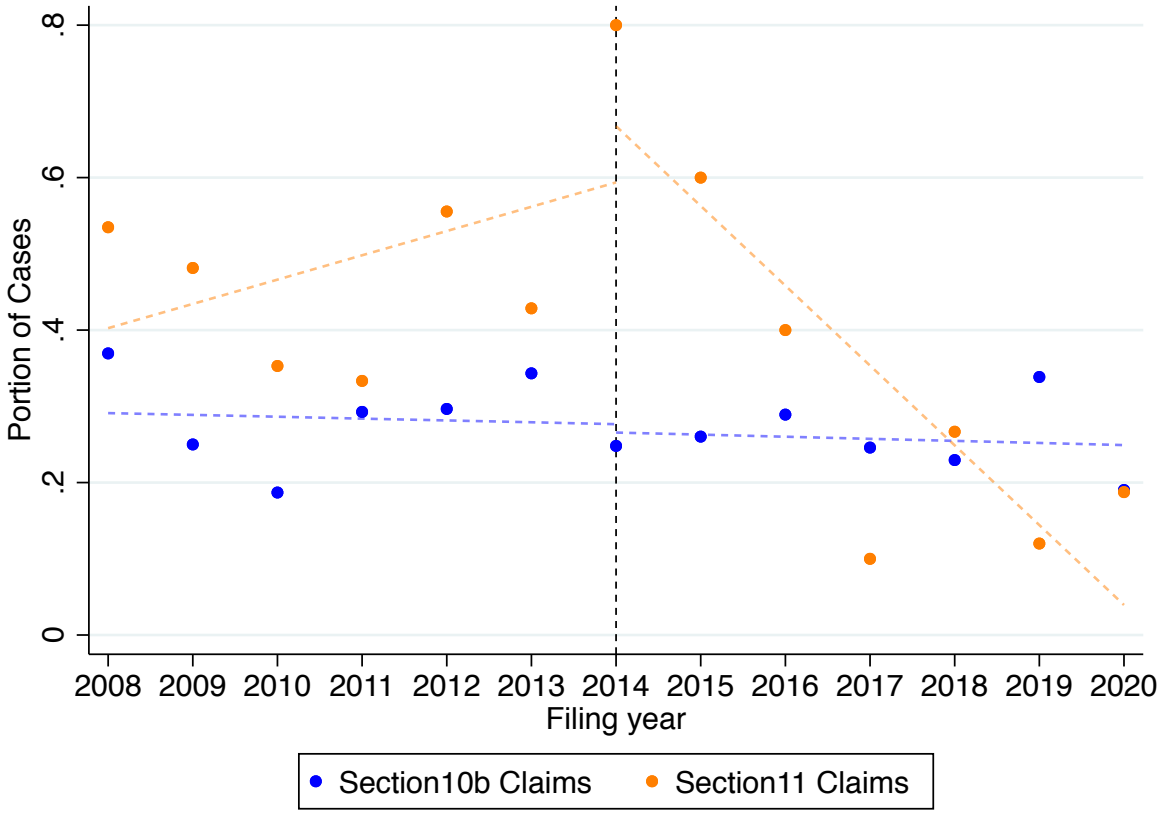
6.4 Changes in Filing Activity Across District Courts

To conclude our emperical analysis we make a creative extension from Murdock's arguments in 2015 about why *Halliburton II* was supposedly an underwhelming case. They argued (independently) that judges simply were not the type of experts positioned to analyze case studies and they would be reluctant to invoke/follow the *Halliburton II* decision.

It is well-documented that most financial-related court cases take place in the Southern District of New York. Judges in the SDNY are thought to be, at least within the legal community, the so-called experts on the intersection of law and finance. In other words, we claim its well-known that judges in the SDNY would be more appreciative of financial case-studies and more confident in applying the *Halliburton II* decision. Given this, we investigate if the probability that a case was filed in the Southern District of New York fell as a result of *Halliburton II*. (Assuming that attorneys redistribute their claims to more accomodative jurisdictions). We plan to rerun our standard diff-in-diff model before but use a dummy variable for being filed in the SDNY as the outcome variable. Figure 10 and Figure 11 shows the distribution of filings across the seventy-seven District Courts. By inspection, we see that the portion of Section10b cases filed in the SDNY slightly declines in the observations after the ruling.

6.4.1 Filing Activity in the Southern District of New York

Figure 10: Portion of Cases Filed in the Southern District of New York

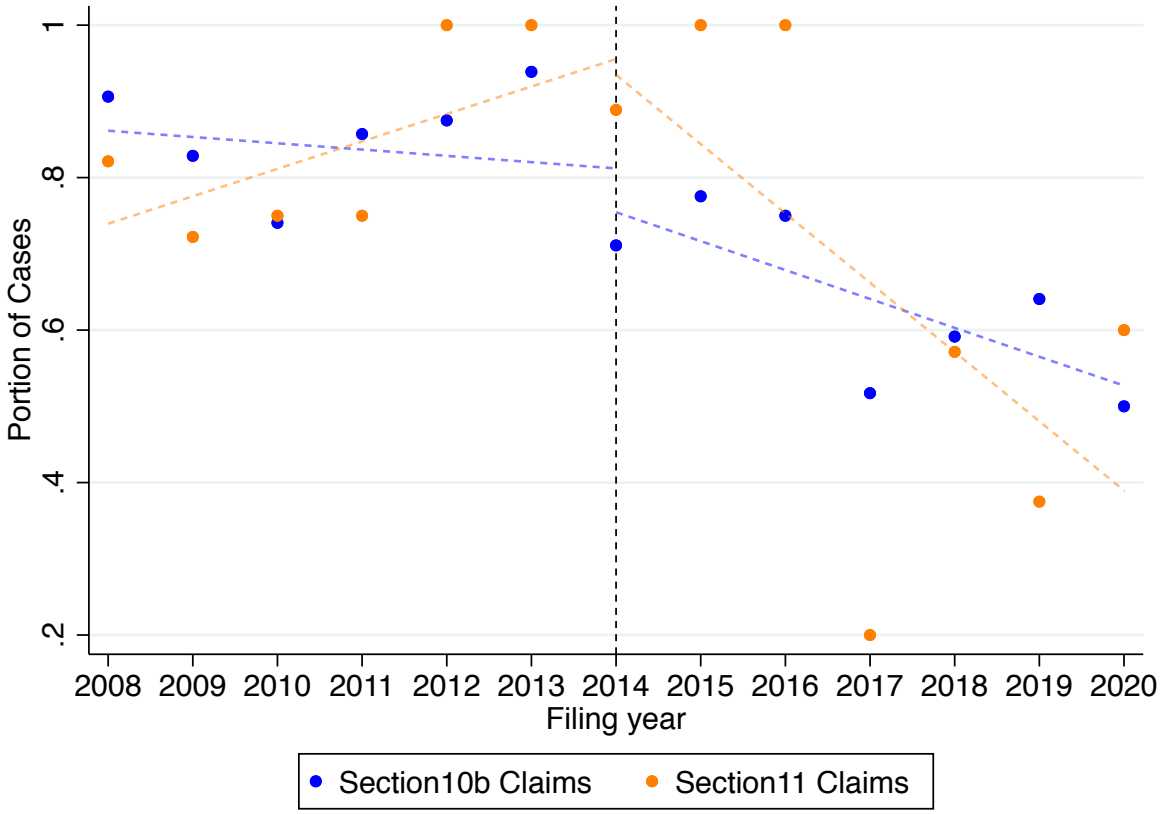


When we run our diff-in-diff model (see Appendix A Table 11) the base regression estimates treatment effect of 0.112; with a p-value of 0.118 the estimate is close to being statistically significant at least at the 10% level. That said, we. should have expected the treatment effect to be negative representing a slight from the SDNY. When we omit observations from the financial crisis the p-value drops to 0.234.

We might worry that our current specification is an imperfect way of understanding how attorneys choose to file claims. Perhaps it makes sense to restrict our sample based on geography. In particular we want to zone in on the New York Metropolitan Area. Ideally, we want to investigate the atorneys/lawfirms who would typically file in the SDNY but because of *Halliburton II* filed in an alternative court.

Now we run the our diff-in-diff estimation but restrict to observations in the Southern District of New York, Eastern District of New York, and District of N.J. The latter two being the natural subsitutes for where an attorney working out of New York would file their claim.

Figure 11: Portion of Cases Filed in the SDNY: Restricted to New York Metropolitan Area



As seen above, because we are restricting the number of observations, the data becomes much more volatile. As a result the parrralla trends assumption clearly breaks down. Either way, this regression does not return any statisitcally signiciant Treatment Effects across our four regression types; each estimate has. a very large standard error.

In conclusion, our results suggest that a claim about "flight from the Southern District of New York" is dubious.

7 Discussion and Conclusion

We set out to determine if *Halliburton II* was a successful intervention by the Supreme Court to reform, or at least improve, securities fraud litigation. We start with the premise that securities fraud lawsuits are frequently based on meritless or dubious claims of fraud. Plaintiff attorney are rent-seekers in this environment who frequently strong-arm settlements anytime a company's stock has a negative price event. *Halliburton II* was supposed to be a step in the

right direction by giving defendants an additional opportunity to get theirr case thrown out. Summarizing our events, we found that at best *Halliburton II* had no impact on case dismissal rates. If anything, our results indicate that as an effect *Halliburton II* a higher proportion of cases were dismissed within one year of the case being filed. This suggests that a higher proportion of cases were discarded *during the motion to dismiss*. If anything, this suggests that the cross-section of cases after *Halliburton II* are actually weaker in terms of merits.This result seems puzzling given *Halliburton II* made it easier to dismiss cases at the class certification phase. We found no evidence that *Halliburton II* had an affect on case lengths. We also did not find that *Halliburton II* distorted if a plaintiff attorney court choose to file in the Southern District of New York. And perhaps most surprisingly, our diff-in-diff model estimated that *Halliburton II* had a significant increase in the annual volume of section10b filings.

At best, our results about case length and filings in the SDNY provide empirical support the legal literature (like scholars such as Murdock and Pritchard) that *Halliburton II* was a minor technical ruling that would unlikely have any major impacts on the system. Our other results, particularly about the quantity of yearly filings, seem inconsistent with our basic intuition surrounding *Halliburton II*.

Its likely that our differences-in-differences model was an imperfect approach to measuring the *causal effect* of *Halliburton II*. In particular, our parallel trends assumptions broke down at many points and at some points our estimated treatment effects were egregiously by any standarrd of common sense. A potential fruitful area for further research might be to establish a better control group or even methodology to study the causal effect of rulings *Halliburton II* on litigation trends. In the case of securities class action, a future paper might try to to use securities fraud suits revolving around mergers & acquisitions as a control for section10b claims.

If anything, this paper adds to the literature claiming that *Halliburton II* was probably not a status quo-changing ruling. And in addition, the current level of securities fraud litigation is non-optimal. A future paper might explore what is the best mechanism or policy tool to alleviate this problem. For example, will it require a robust new precedent from the Supreme

Court or will it take ambitious legislative acttion comparable to the Private Securities Litigation Reform Act of 1995.

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8 Appendix A: Regression Results

Table 1: Dismissal Rate as the Dependent Variable: Excluding Ongoing Cases

	(1)	(2)	(3)	(4)
	Base Regression	Excluding 08-09	Controlling for Firm Size	Excluding 08-09
=1 if section10b	0.204***	-0.0118	0.208***	0.339***
	(0.033)	(0.111)	(0.078)	(0.108)
=1 if treated by H2	-0.330***	-0.534***	-0.388**	-0.241
	(0.114)	(0.135)	(0.183)	(0.193)
Treatment Effect	-0.118	0.0916	-0.0858	-0.229
	(0.101)	(0.128)	(0.137)	(0.158)
Log Real Total Rev			0.00520	0.00673
			(0.005)	(0.005)
Constant	0.203***	0.442***	0.167	-0.0465
	(0.065)	(0.103)	(0.172)	(0.121)
Observations	1678	1336	1021	856
R^2	0.117	0.140	0.146	0.183

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 2: Dismissal Rates Controlling for Case Progress

	(1)	(2)	(3)	(4)
	Dismissed in 1Yr	Dismissed in 2Yrs	Dismissed in 3Yrs	Dismissed in 4Yrs
=1 if section10b	-0.0205	0.0699$^{\cdot}$	0.150$^{\cdot\cdot\cdot}$	0.209$^{\cdot\cdot\cdot}$
	(0.022)	(0.039)	(0.041)	(0.034)
=1 if treated by H2	-0.551$^{\cdot\cdot\cdot}$	-0.629$^{\cdot\cdot\cdot}$	-0.317$^{\cdot\cdot}$	-0.217
	(0.090)	(0.079)	(0.138)	(0.134)
Treatment Effect	0.0899$^{\cdot\cdot\cdot}$	0.00923	-0.207	-0.265$^{\cdot\cdot}$
	(0.033)	(0.079)	(0.126)	(0.120)
Constant	0.114$^{\cdot\cdot\cdot}$	0.162$^{\cdot\cdot}$	0.166$^{\cdot\cdot}$	0.137$^{\cdot\cdot}$
	(0.039)	(0.078)	(0.065)	(0.064)
Observations	1944	1724	1526	1333
R^2	0.113	0.108	0.098	0.116

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdot\cdot\cdot}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 3: Dismissal Rates Controlling for Case Progress:Excluding 2008 and 2009

	(1)	(2)	(3)	(4)
	Dismissed in 1Yr	Dismissed in 2Yrs	Dismissed in 3Yrs	Dismissed in 4Yrs
=1 if section10b	-0.166$^{\cdot}$	-0.0729	-0.00252	0.0209
	(0.084)	(0.092)	(0.105)	(0.103)
=1 if treated by H2	-0.697$^{\cdot\cdot\cdot}$	-0.779$^{\cdot\cdot\cdot}$	-0.477$^{\cdot\cdot\cdot}$	-0.414$^{\cdot\cdot\cdot}$
	(0.083)	(0.099)	(0.125)	(0.117)
Treatment Effect	0.236$^{\cdot\cdot\cdot}$	0.153	-0.0554	-0.0774
	(0.085)	(0.094)	(0.111)	(0.104)
Constant	0.196$^{\cdot\cdot\cdot}$	0.263$^{\cdot\cdot}$	0.316$^{\cdot\cdot\cdot}$	0.339$^{\cdot\cdot\cdot}$
	(0.073)	(0.105)	(0.094)	(0.083)
Observations	1601	1381	1183	990
R^2	0.128	0.135	0.117	0.141

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdot\cdot\cdot}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 4: Dismissal Rates Controlling for Case Progress and Total Revenue of Defendant Firm

	(1) Dismissed in 1Yr	(2) Dismissed in 2Yrs	(3) Dismissed in 3Yrs	(4) Dismissed in 4Yrs
=1 if section10b	-0.0354	0.0704	0.195$^{\cdot}$	0.274$^{\cdot\cdot}$
	(0.049)	(0.091)	(0.112)	(0.114)
=1 if treated by H2	-0.670$^{\cdot\cdot\cdot}$	-0.690$^{\cdot\cdot\cdot}$	-0.323	-0.155
	(0.144)	(0.156)	(0.207)	(0.189)
Treatment Effect	0.127$^{\cdot\cdot}$	0.0103	-0.236	-0.353$^{\cdot\cdot}$
	(0.053)	(0.152)	(0.188)	(0.157)
Log Real Total Rev	-0.00609$^{\cdot}$	-0.0107$^{\cdot\cdot}$	-0.00815$^{\cdot}$	-0.00214
	(0.003)	(0.005)	(0.004)	(0.005)
Constant	0.211$^{\cdot\cdot\cdot}$	0.348$^{\cdot\cdot}$	0.224	0.118
	(0.077)	(0.164)	(0.180)	(0.230)
Observations	1231	1056	895	763
R^2	0.135	0.139	0.123	0.134

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdot\cdot\cdot}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 5: Dismissal Rates Controlling for Case Progress and Total Revenue of Defendant Firm: Excluding 2008 and 2009

	(1) Dismissed in 1Yr	(2) Dismissed in 2Yrs	(3) Dismissed in 3Yrs	(4) Dismissed in 4Yrs
=1 if section10b	0.0119	0.243$^{\cdot\cdot}$	0.362$^{\cdot\cdot\cdot}$	0.376$^{\cdot\cdot\cdot}$
	(0.098)	(0.111)	(0.111)	(0.114)
=1 if treated by H2	-0.620$^{\cdot\cdot\cdot}$	-0.507$^{\cdot\cdot\cdot}$	-0.141	-0.0215
	(0.174)	(0.171)	(0.200)	(0.191)
Treatment Effect	0.0812	-0.166	-0.412$^{\cdot\cdot}$	-0.484$^{\cdot\cdot\cdot}$
	(0.114)	(0.170)	(0.177)	(0.164)
Log Real Total Rev	-0.00515	-0.00831	-0.00605	-0.00158
	(0.004)	(0.005)	(0.004)	(0.005)
Constant	0.0698	0.107	-0.0308	-0.0998
	(0.144)	(0.187)	(0.180)	(0.219)
Observations	1066	891	730	598
R^2	0.149	0.170	0.160	0.180

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdot\cdot\cdot}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 6: Case Length Conditional on Case Being Dismissed

	(1) Base Regression	(2) Excluding 08-09	(3) Controlling for Firm Size	(4) Excluding 08-09
=1 if section10b	40.95	118.8	-32.75	-221.1
	(83.137)	(198.314)	(215.649)	(691.870)
=1 if treated by H2	498.4$^{\cdots}$	589.4$^{\cdots}$	462.2$^{\cdot}$	273.1
	(145.959)	(156.315)	(267.879)	(752.739)
Treatment Effect	-24.31	-92.57	43.07	243.6
	(120.101)	(178.560)	(255.017)	(725.953)
Log Real Total Rev			16.77$^{\cdots}$	10.48
			(4.951)	(8.405)
Constant	655.0$^{\cdots}$	752.3$^{\cdots}$	472.6$^{\cdot\cdot}$	838.0
	(135.150)	(259.746)	(203.338)	(786.068)
Observations	945	770	606	514
R^2	0.177	0.192	0.222	0.234

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdots}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court.
Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 7: Case Length Conditional on Case Being Settled

	(1) Base Regression	(2) Excluding 08-09	(3) Controlling for Firm Size	(4) Excluding 08-09
=1 if section10b	-155.0$^{\cdot}$	221.3	292.0$^{\cdot}$	430.5$^{\cdot\cdot}$
	(91.317)	(190.553)	(161.946)	(181.922)
=1 if treated by H2	-401.9$^{\cdot\cdot}$	9.231	322.1	510.7
	(184.057)	(243.237)	(274.377)	(344.983)
Treatment Effect	292.7$^{\cdot\cdot}$	-126.9	-256.0$^{\cdot\cdot}$	-411.5$^{\cdot\cdot}$
	(121.766)	(178.422)	(114.111)	(164.420)
Log Real Total Rev			46.04$^{\cdots}$	29.56$^{\cdot\cdot}$
			(10.897)	(12.771)
Constant	1614.1$^{\cdots}$	1407.4$^{\cdots}$	816.3$^{\cdots}$	1054.2$^{\cdots}$
	(118.643)	(213.019)	(189.460)	(246.573)
Observations	721	554	405	332
R^2	0.334	0.350	0.424	0.453

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdots}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court.
Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 8: Case Length: Using all Completed Cases

	(1) Base Regression	(2) Excluding 08-09	(3) Controlling for Firm Size	(4) Excluding 08-09
=1 if section10b	-277.1$^{\cdots}$	131.7	-19.72	-27.36
	(58.795)	(149.318)	(211.581)	(254.599)
=1 if treated by H2	322.6$^{\cdots}$	720.8$^{\cdots}$	760.0$^{\cdots}$	742.4$^{\cdot\cdot}$
	(104.264)	(139.825)	(244.194)	(279.494)
Treatment Effect	315.5$^{\cdots}$	-93.30	-1.546	22.85
	(65.924)	(141.125)	(232.191)	(267.486)
Log Real Total Rev			28.22$^{\cdots}$	17.20$^{\cdot\cdot}$
			(5.857)	(7.702)
Constant	1463.6$^{\cdots}$	1188.7$^{\cdots}$	908.2$^{\cdots}$	1232.7$^{\cdots}$
	(99.862)	(163.453)	(247.291)	(372.395)
Observations	1666	1324	1011	846
R^2	0.220	0.239	0.262	0.303

Standard errors in parentheses

$^{\cdot}$ $p < .1$, $^{\cdot\cdot}$ $p < .05$, $^{\cdots}$ $p < .01$

NB: The regression above includes fixed effects for industry, year of filing, and district court. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 9: Number of Annual Cases

	(1)	(2)	(3)
	Base Regression	Adding Annual IPO Volume	Adding Year FEs
=1 if section10b	107.2***	107.2***	107.2***
	(9.213)	(9.473)	(6.096)
=1 if after 2014	-5.167	-5.260	-11.24
	(6.390)	(6.647)	(8.868)
Treatment Effect	55.50***	55.50***	55.50***
	(11.981)	(12.292)	(8.259)
IPOvolume		0.00115	-0.0472*
		(0.032)	(0.023)
Filing year=2009			-27.93**
			(10.635)
Filing year=2010			-32.76**
			(10.449)
Filing year=2011			-30.89***
			(5.514)
Filing year=2012			-32.96***
			(4.255)
Filing year=2013			-21.81
			(12.108)
Filing year=2015			-33.56*
			(15.333)
Filing year=2016			-28.98***
			(6.031)
Filing year=2017			-14.40*
			(7.159)
Filing year=2018			-10.10*
			(4.688)
Constant	18.67***	18.52**	49.11***
	(5.737)	(8.022)	(5.008)
Observations	24	24	24
R^2	0.964	0.964	0.992

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

NB: Explain the high $R^2 and why there is no estimate for 2018 and 2019 (no new delta)$

Table 10: Number of Annual Cases: Excluding 2008 and 2009

	(1) Base Regression	(2) Adding Annual IPO Volume	(3) Adding Year FEs
=1 if section10b	110.0***	110.0***	110.0***
	(5.836)	(5.798)	(7.427)
=1 if after 2014	3.000	0.688	22.94
	(3.586)	(3.886)	(12.976)
Treatment Effect	52.67***	52.67***	52.67***
	(9.756)	(9.474)	(9.354)
Annual U.S. IPO Volume		0.0479**	-0.0472*
		(0.021)	(0.024)
Filing year=2011			1.877
			(11.452)
Filing year=2012			-0.198
			(11.227)
Filing year=2013			10.95
			(14.726)
Filing year=2015			-33.56*
			(15.649)
Filing year=2016			-28.98***
			(6.156)
Filing year=2017			-14.40*
			(7.307)
Filing year=2018			-10.10*
			(4.784)
Constant	10.50***	2.799	14.92
	(2.147)	(3.974)	(12.447)
Observations	20	20	20
R^2	0.980	0.983	0.993

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

NB: Explain the high R^2 $and why there is no estimate for 2018 and 2019 (no new delta)$

Table 11: Probability a Case is Filed in the Southern District of New York

	(1) Base Regression	(2) Excluding 08-09	(3) Controlling for Firm Size	(4) Excluding 08-09
=1 if section10b	-0.129$^{¨}$	-0.129	-0.297$^{¨¨}$	-0.267$^{¨}$
	(0.052)	(0.079)	(0.085)	(0.132)
=1 if treated by H2	-0.129	-0.125	-0.273$^{·}$	-0.239
	(0.121)	(0.137)	(0.146)	(0.178)
Treatment Effect	0.112	0.111	0.243$^{¨}$	0.214
	(0.072)	(0.093)	(0.102)	(0.143)
Log Real Total Rev			0.00706	0.00661
			(0.004)	(0.005)
Constant	0.598$^{¨¨}$	0.452$^{¨¨}$	0.732$^{¨¨}$	0.542$^{¨¨}$
	(0.073)	(0.096)	(0.115)	(0.150)
Observations	2144	1801	1370	1205
R^2	0.046	0.034	0.072	0.049

Standard errors in parentheses

$^{·}$ $p < .1$, $^{¨}$ $p < .05$, $^{¨¨}$ $p < .01$

NB: The regression above includes fixed effects for industry and year of filing. Those estimated coefficients are omitted from our regression table for sake of brevity.

Table 12: Probability a Case is Filed in the Southern District of New York: Restricted to Cases in the Southern District of New York, Eastern District of New York, and District of New Jersey

	(1) Base Regression	(2) Excluding 08-09	(3) Controlling for Firm Size	(4) Excluding 08-09
=1 if section10b	0.0807	0.0217	-0.0917	-0.121$^{¨}$
	(0.055)	(0.066)	(0.078)	(0.055)
=1 if treated by H2	-0.0259	-0.0857	-0.173	-0.197
	(0.175)	(0.178)	(0.216)	(0.211)
Treatment Effect	-0.0346	0.0289	0.111	0.142
	(0.091)	(0.098)	(0.114)	(0.099)
Log Real Total Rev			0.0124$^{·}$	0.0139$^{¨}$
			(0.007)	(0.007)
Constant	0.885$^{¨¨}$	0.0409	0.922$^{¨¨}$	1.065$^{¨¨}$
	(0.079)	(0.074)	(0.119)	(0.096)
Observations	854	731	522	515
R^2	0.151	0.204	0.161	0.186

Standard errors in parentheses

$^{·}$ $p < .1$, $^{¨}$ $p < .05$, $^{¨¨}$ $p < .01$

NB: The regression above includes fixed effects for industry and year of filing. Those estimated coefficients are omitted from our regression table for sake of brevity.

9 Appendix B: Figures

Figure 12: Total Revenue of Defendant Firms: Section10b Claims

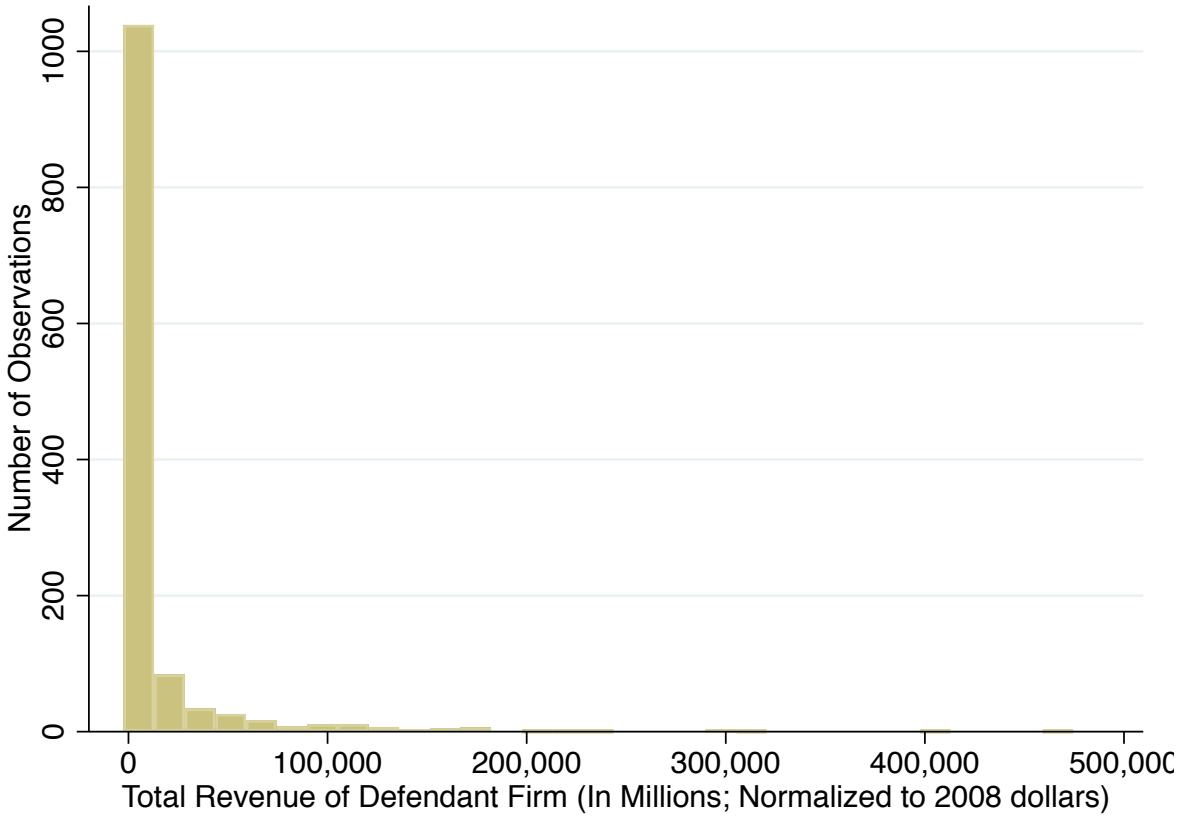


Figure 13: Log Total Revenue of Defendant Firms: Section10b Claims

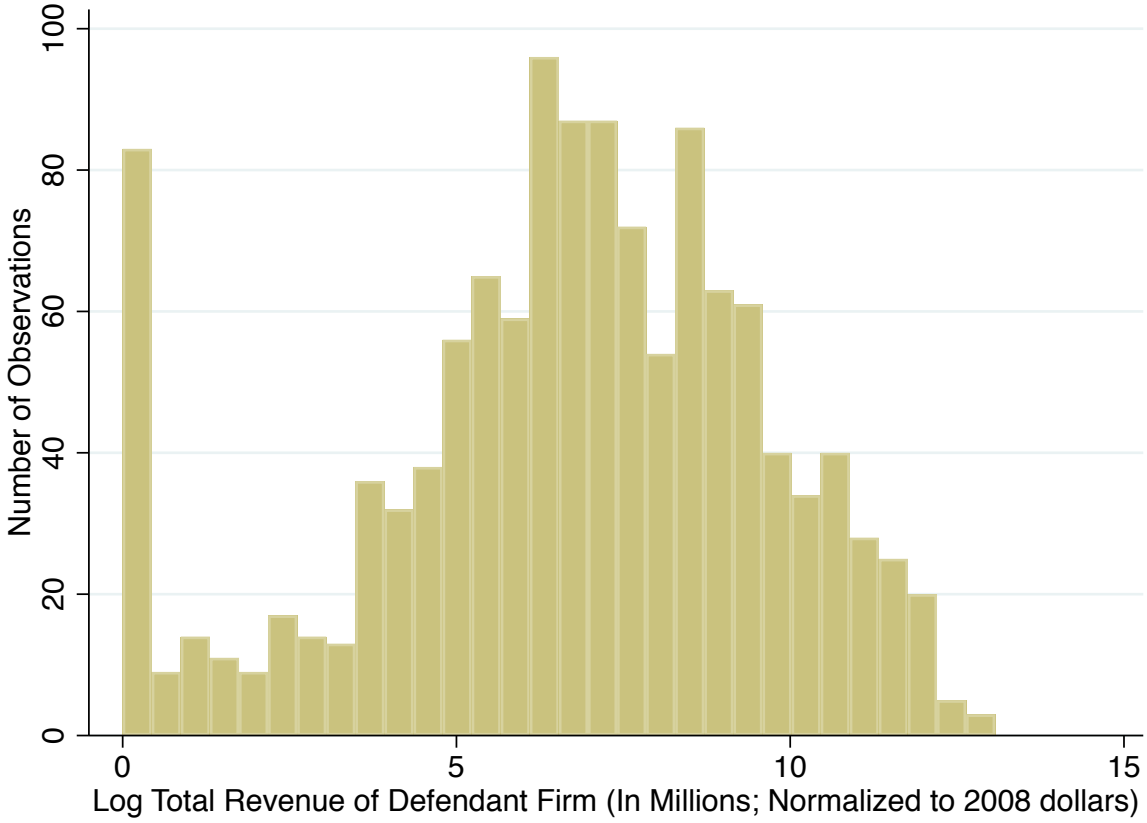


Figure 14: Total Revenue of Defendant Firms Pre vs Post Halliburton II: Section10b Claims

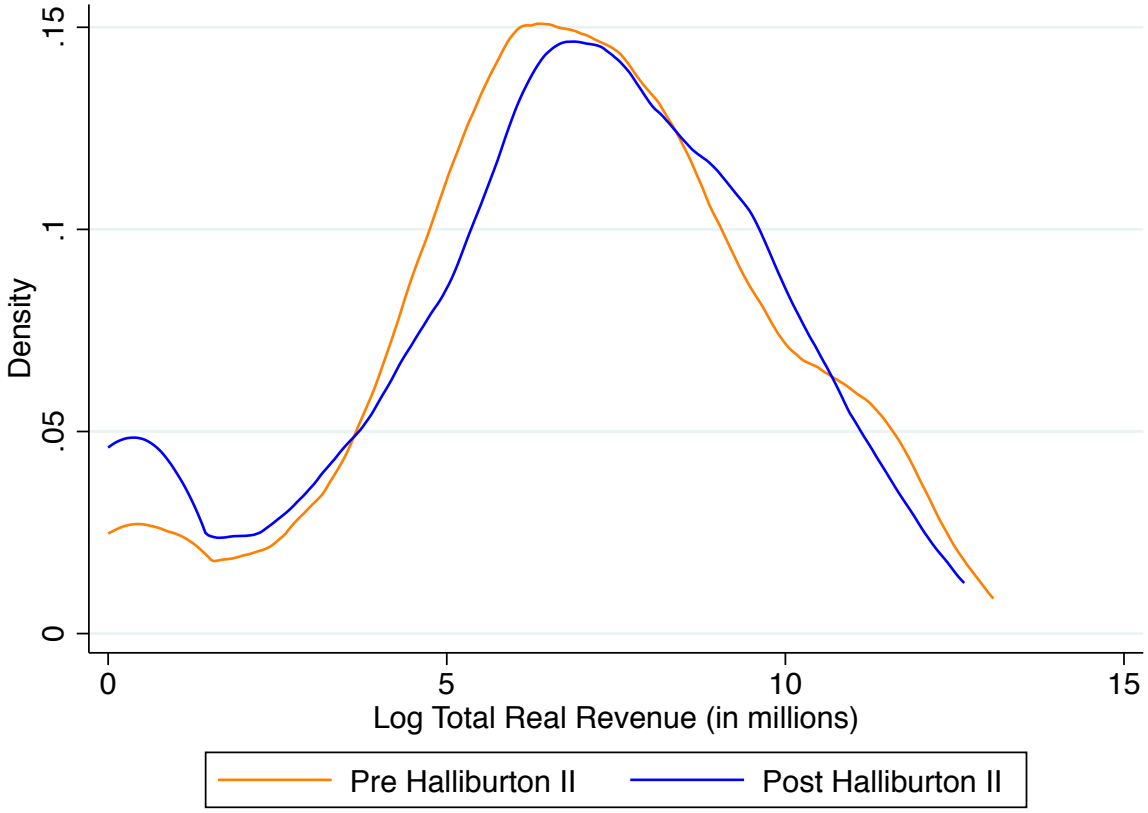


Figure 15: Total Revenue of Defendant Firms : Pre vs Post Halliburton II: Section11 Claims

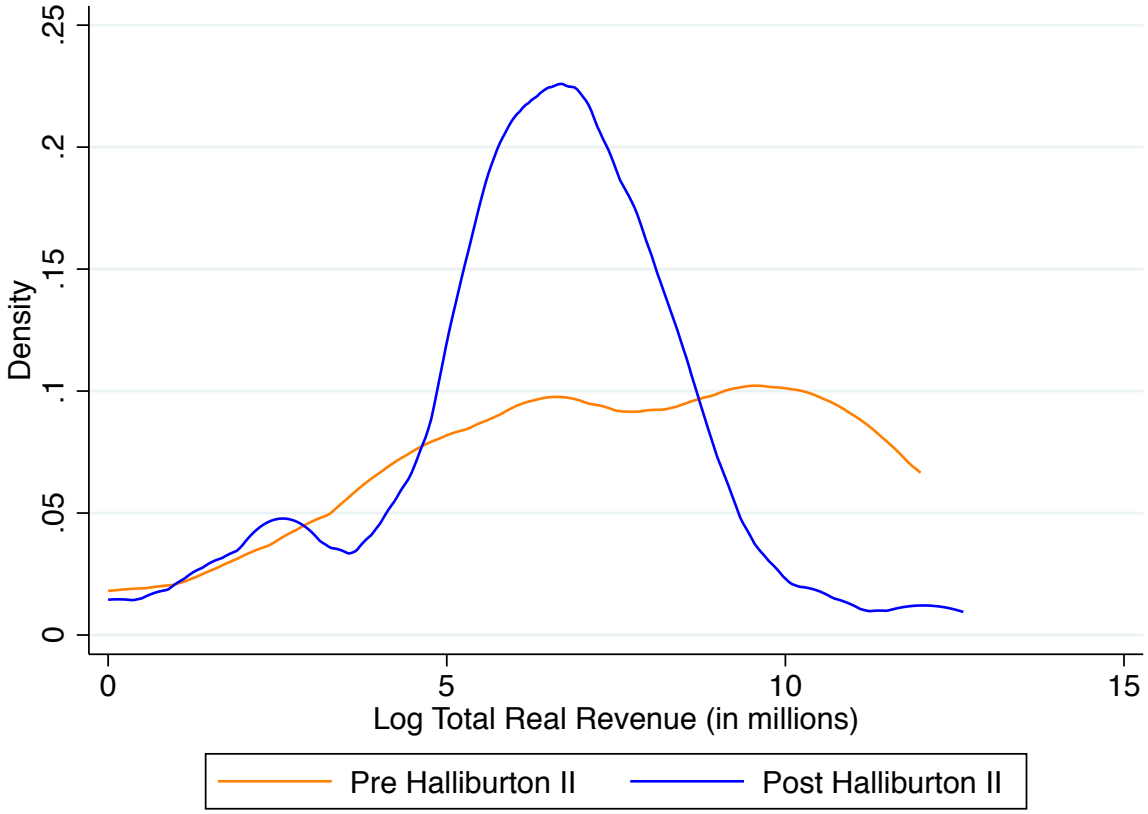


Figure 16: Atypical Filing Activity in 2008 and 2009

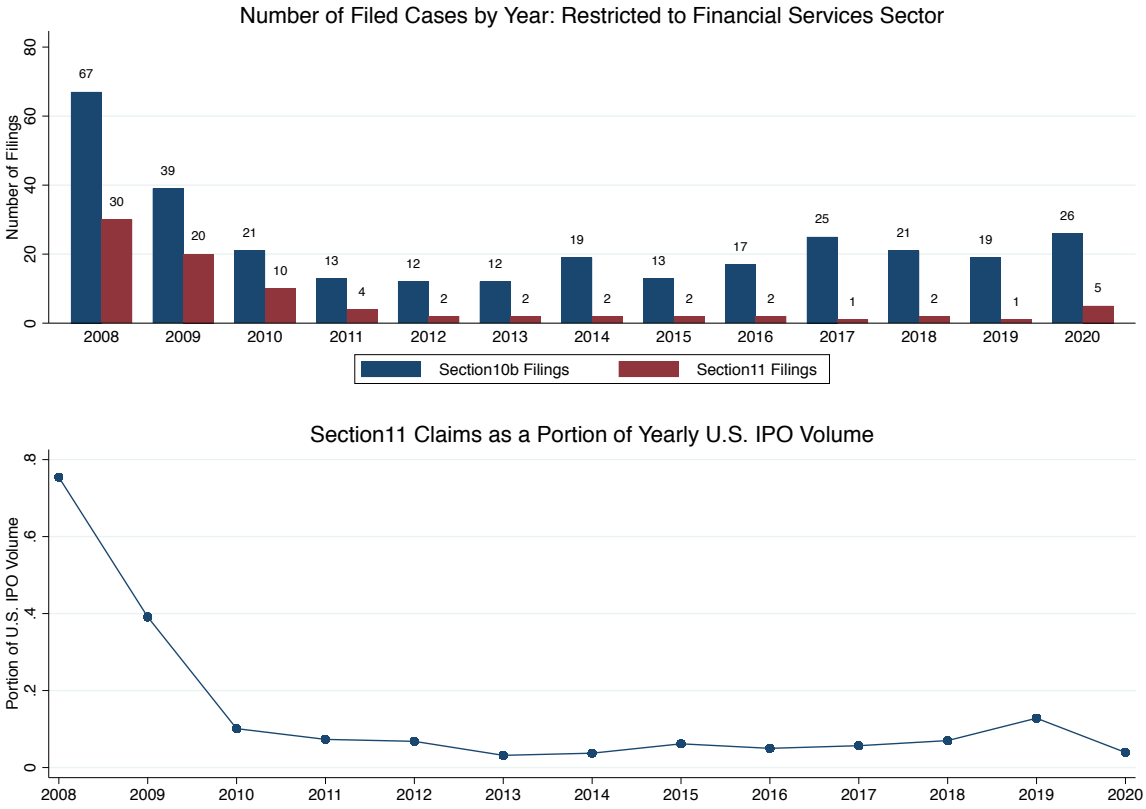


Figure 17: Case Length Distributions Pre and Post Halliburton II

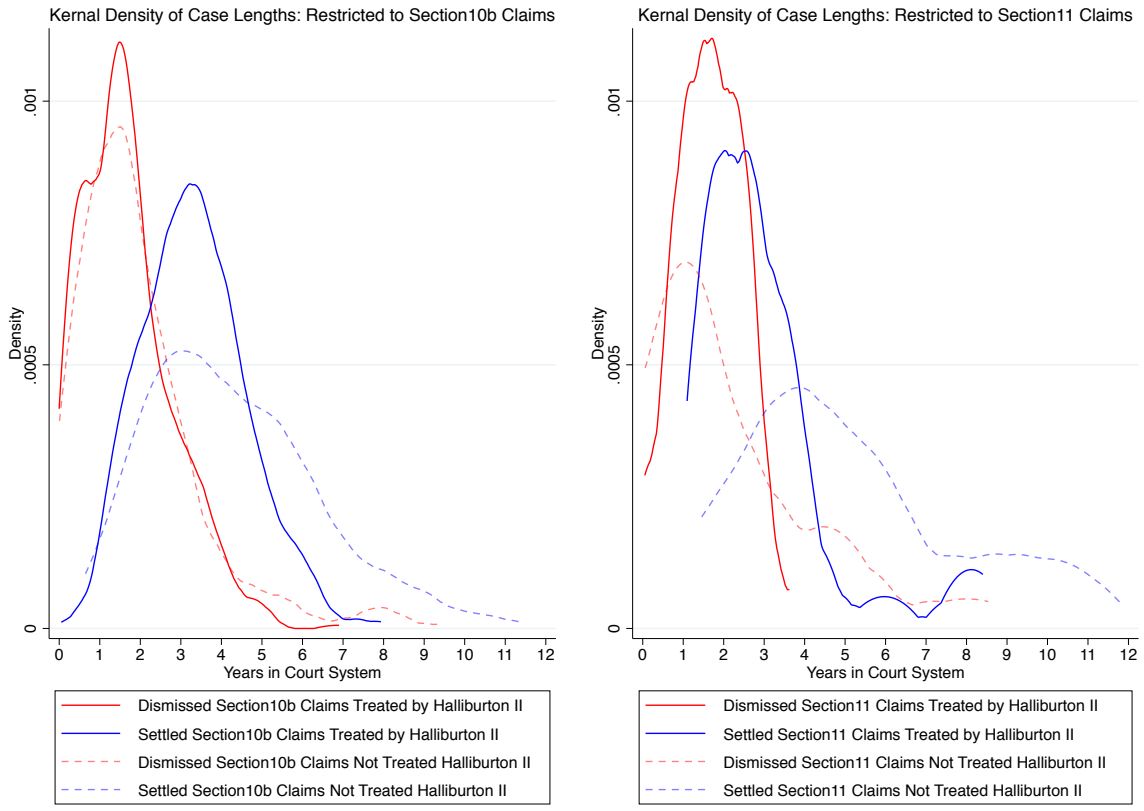


Figure 18: Average Case Duration: Settled vs Dismissed Cases | Including Financial Crisis

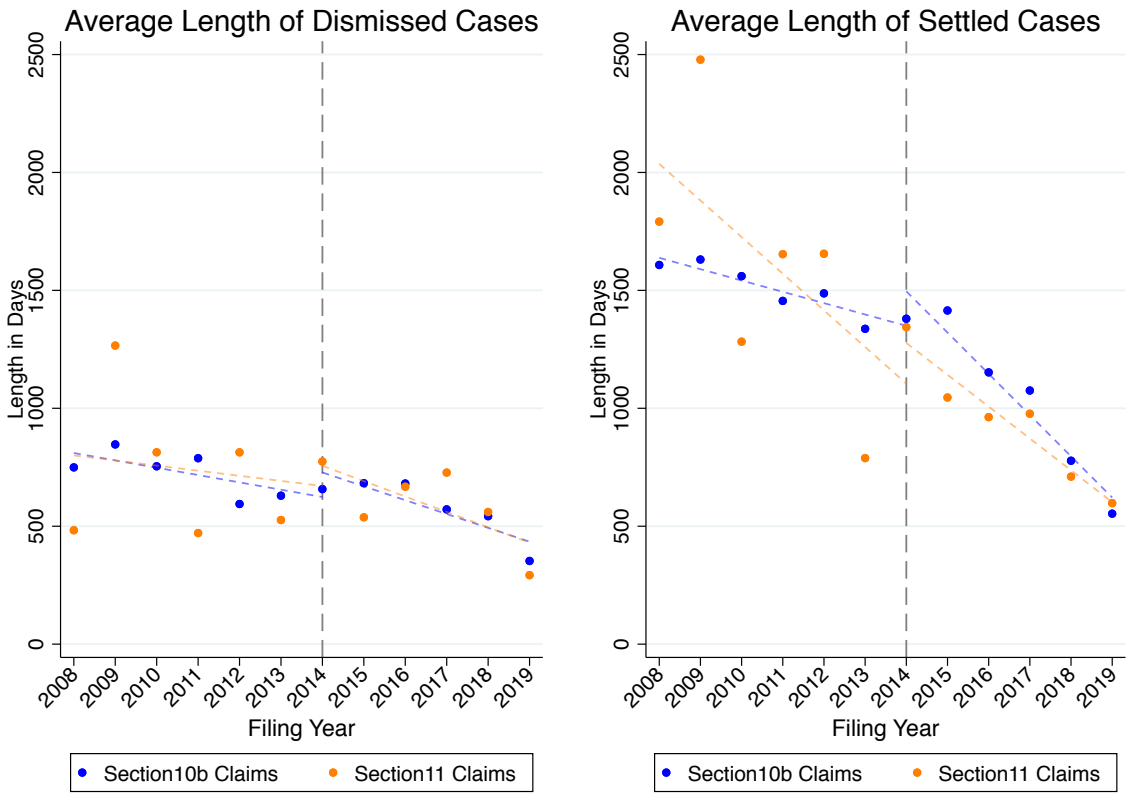


Figure 19: Average Case Duration: Combining Dismissed and Settled Cases

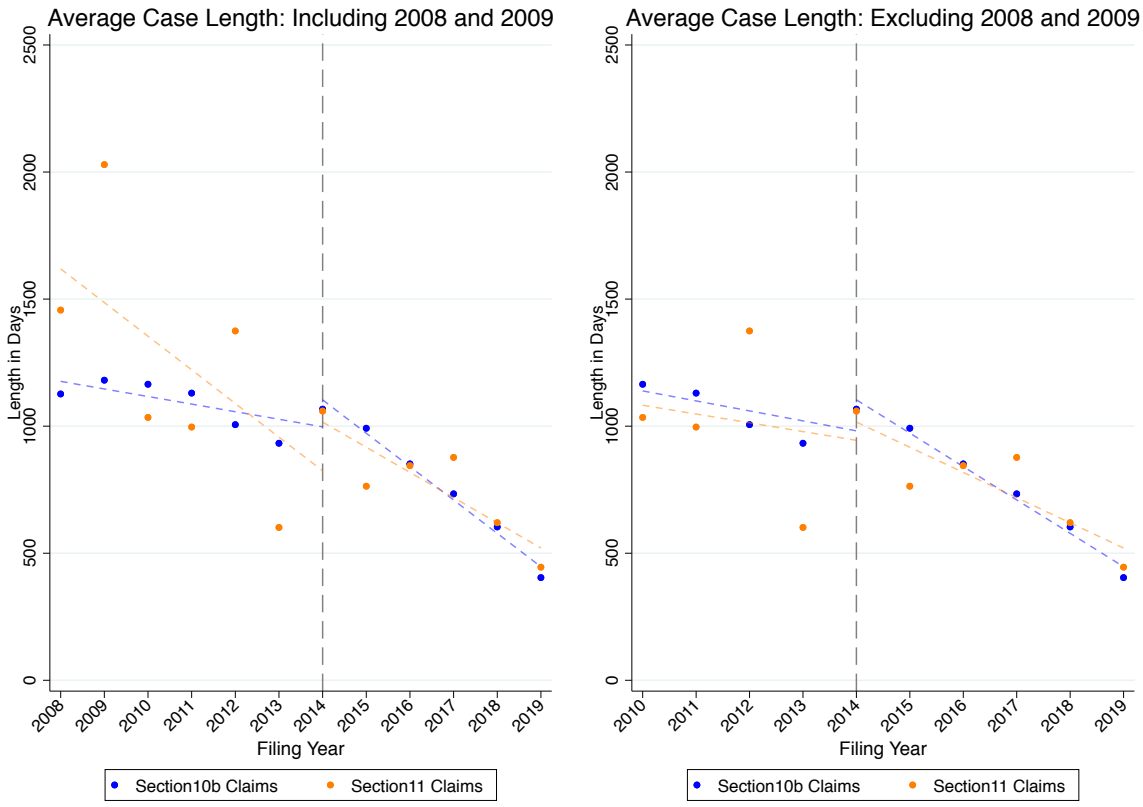


Figure 20: Average Number of Cases Filed: Log Transformed

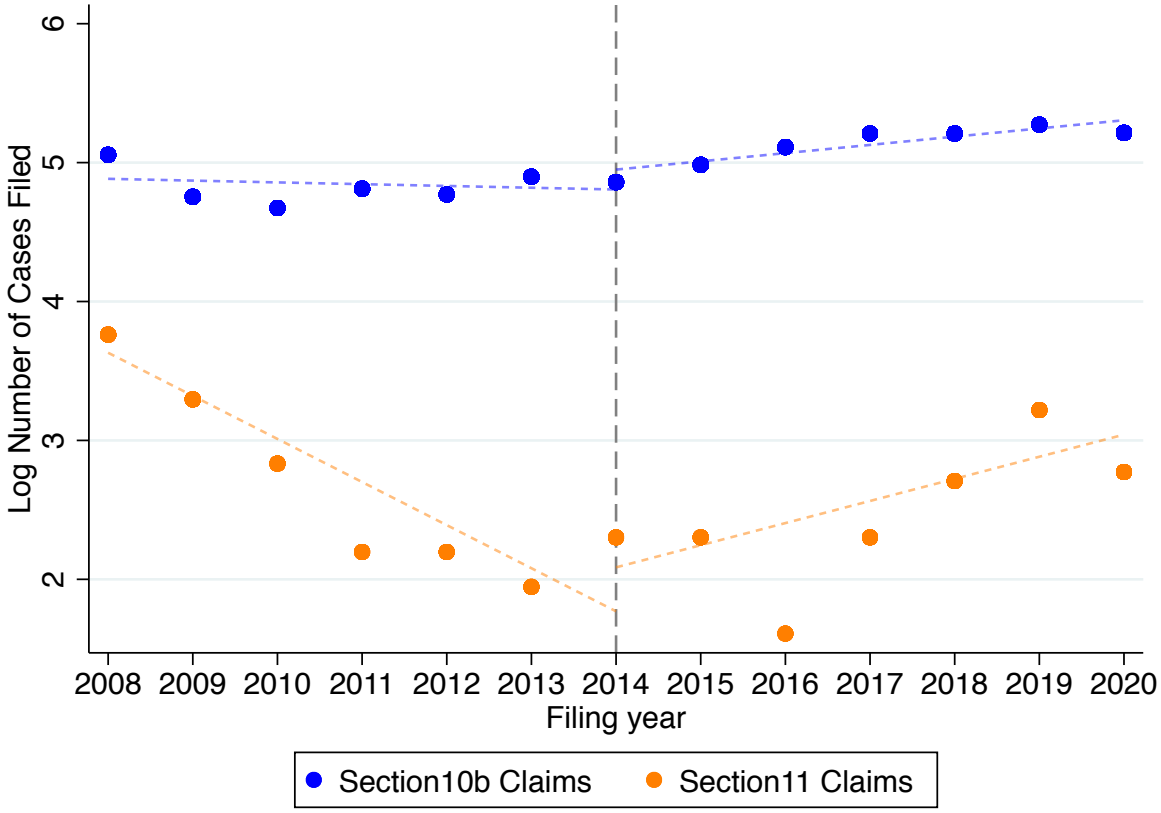


Figure 21: Filings Across District Courts: Section10b Claims

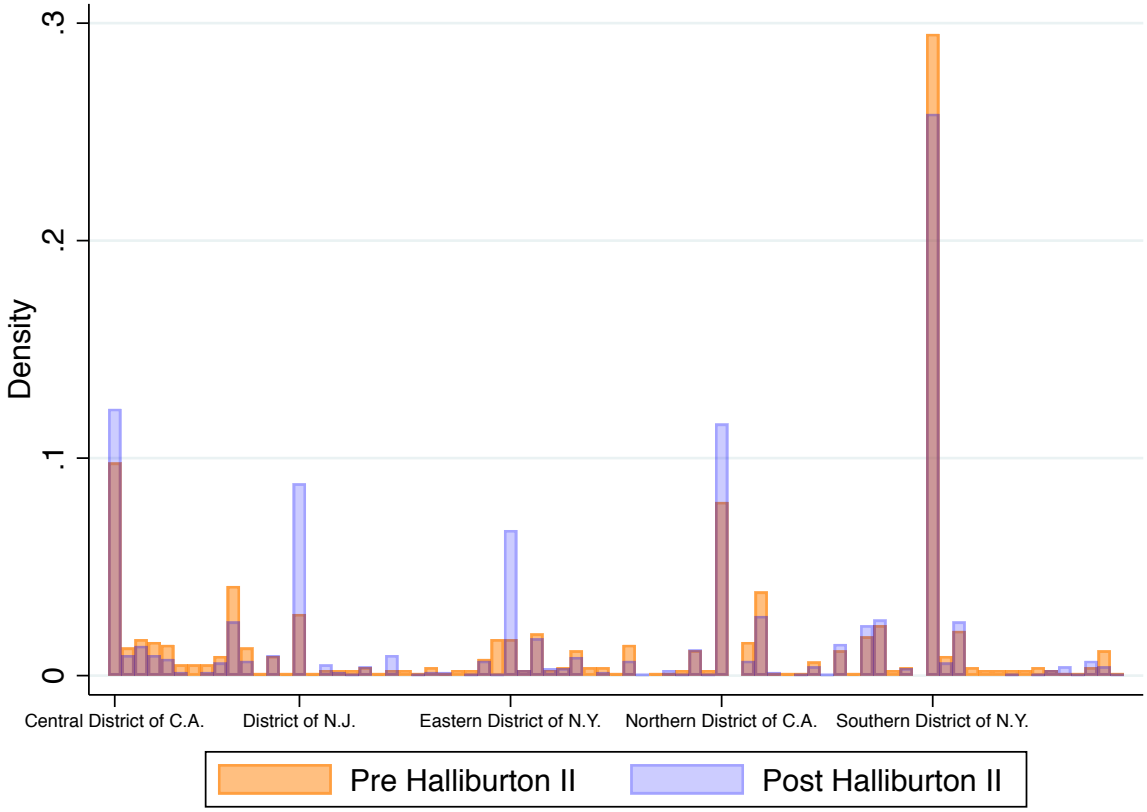
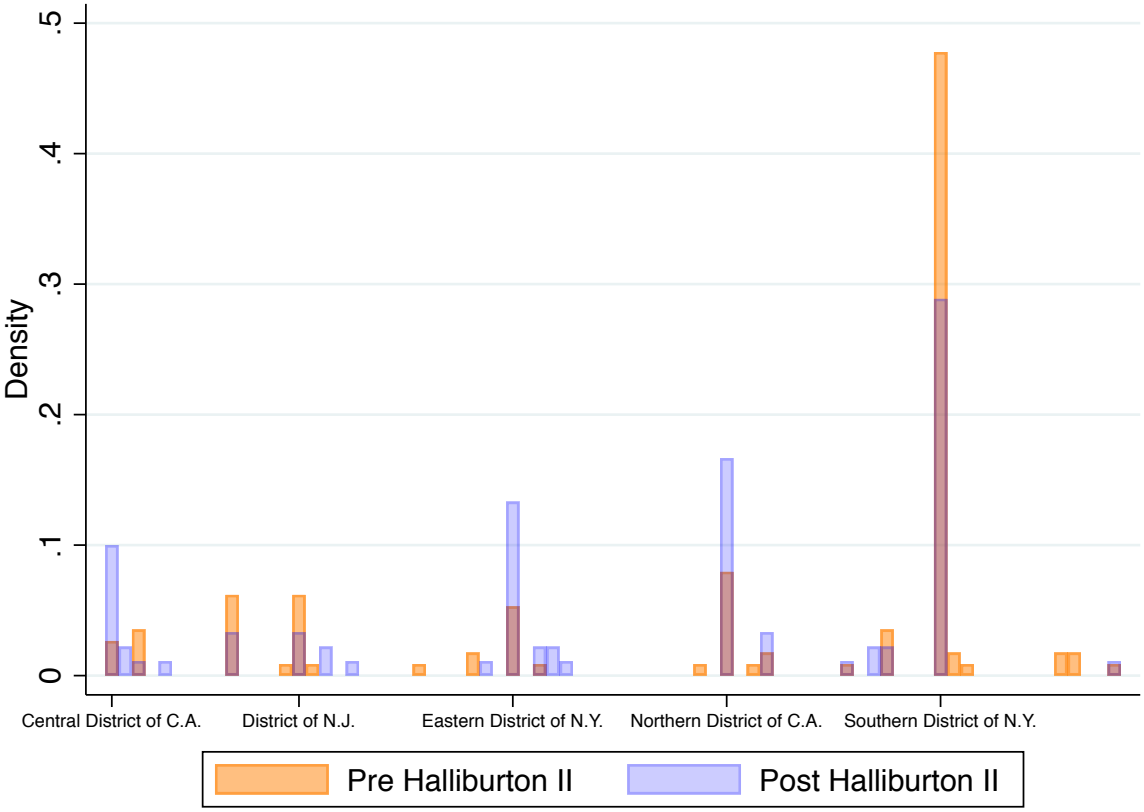


Figure 22: Filings Across District Courts: Section11 Claims

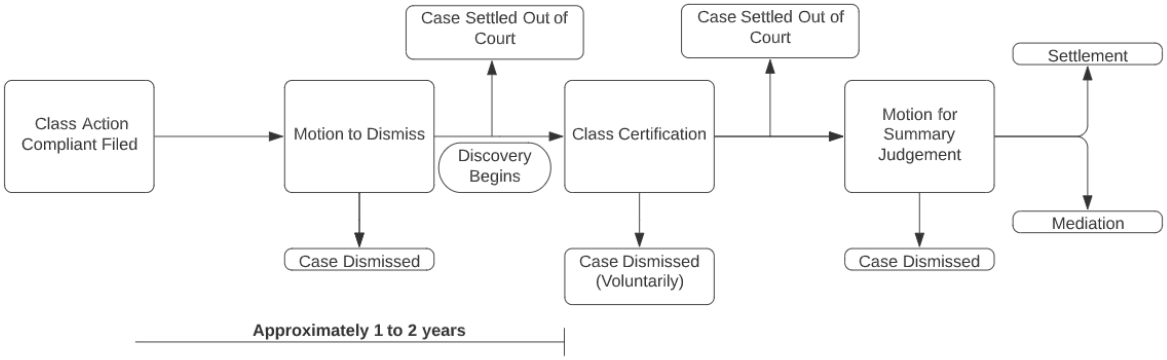


10 Appendix C: Other

1. Fraud on the Market Theory

2. Litigation Process Revisited

Figure 23: Basic Progression of a Securities Class Action



3.

$$NumberAnnualCases_i = \beta_0 + \beta_1 section10b + \beta_2 afterDecision + \beta_3 (section10b \cdot afterDecision)$$

$$\beta_4 IPOvolume + \beta_5 \gamma_i + \varepsilon_i$$

4. Discussion of *Treated by Halliburton II*