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Insider Trading Enforcement and the Private Information Environment: Evidence from the Newman Ruling

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Insider Trading Enforcement and the Private Information Environment:
Evidence from the Newman Ruling

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Abstract

I exploit a shock to U.S. insider trading law to investigate whether a reduction in the enforceability of tipper-tippee insider trading restrictions leads to changes in information parity among investors and the efficiency of price discovery. The December 2014 Federal Second Circuit Court of Appeals ruling in *US v. Newman* constrained enforcement by restricting the types of exchanges between managers and investors that trigger tipper-tippee insider trading liability. Following *Newman*, I find that Second Circuit hedge funds experienced a significant increase in their stock picking ability of Second Circuit stocks in terms of preempting future earnings announcement returns and future earnings surprises; this is consistent with *Newman* having a differential effect on market participants as the ruling represented binding precedent only within the jurisdictional boundaries of the Second Circuit. I also find evidence that the suspect trading activity of Second Circuit hedge funds led to improved market efficiency in the Second Circuit, as evidenced by tests that approximate the speed of price discovery between quarterly earnings announcement cycles of portfolio firms. This study extends prior work by documenting the relation between important aspects of capital market activity and tipper-tippee insider trading law.

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1. INTRODUCTION

In December 2014, the U.S. Federal Circuit Court of Appeals for the Second Circuit issued a landmark decision in *US v. Newman* (hereafter *Newman*) where the criminal convictions of two prominent hedge fund managers were overturned on appeal. The Department of Justice (DOJ) referred to the ruling as "one of the most significant developments in insider trading law in a generation" that would "limit the Government's ability to prosecute some of the most common, culpable, and market-threatening forms of insider trading" (DOJ 2015). Others in the investment community raised similar concern, noting that *Newman* could create avenues for executives "to play fast and loose with private information" (Stockman 2015). Indeed, to an extent *Newman* represented a deregulation of insider trading law since the ruling restricted the types of exchanges between managers and investors that trigger *tipper-tippee* insider trading liability.[1]

In this article, I exploit the *Newman* shock to further our understanding of the role of tipper-tippee insider trading law in the U.S. capital market. Specifically, my empirical tests address two questions regarding how information parity and price informativeness relate to insider trading: Do investors capitalize on private information when regulators are constrained in enforcing traditional tipper-tippee insider trading restrictions? Is price efficiency impacted when the enforceability of insider trading law is reduced? These questions are relevant to lawmakers, regulators, investors, and academics for at least two reasons. First, prior work has well examined the relationship between insider trading prohibitions and the profitability and informativeness of trades of *classical insiders* (i.e., executives, directors, and beneficial shareholders; e.g., Jaffe 1974; Seyhun 1992;

[1] Tipper-tippee insider trading relates to the "tipping" or selective disclosure of material nonpublic information by an individual who holds a fiduciary responsibility to the firm. The disclosure is a violation of insider trading law if the recipient (tippee) actually trades on the information and the tipper receives a *personal benefit* in exchange for the disclosure. As will be discussed in this article, what constitutes a personal benefit is the fundamental issue underlying the *Newman* ruling and subsequent rulings which provide empirical motivation for this study.

Jagolinzer 2009; Brochet 2010, Ali and Hirschleifer 2017; among others), but there is little to no empirical evidence in the area of tipper-tippee insider trading. It is therefore unclear whether the capital market effects related to insider trading law, as documented by prior work, are mainly attributable to restricting the ability of insiders to capitalize on inside information or reducing the private information advantages of outside investors. The *Newman* shock is novel in that allows for an analysis that isolates the capital market effects related to potential insider trading by investors from that of insiders, since the ruling is irrelevant to corporate insiders.

Second, prior work generally finds that the implementation and enforcement of insider trading law is beneficial for markets. For example, Bhattacharya and Daouk (2002), Fernandez and Ferreira (2009), and Christensen et al. (2016) find that the benefits (i.e., improved cost of equity and liquidity) of insider trading law are more apparent in markets where regulators carry out enforcement of insider trading restrictions. However, because these studies are conducted in international settings, the empirical findings related to insider trading enforcement may not necessarily generalize to the U.S. market. This is due to the fact that the U.S. has a longer history of insider trading law and leads the world in maintaining a strict enforcement regime in terms of imposing severe insider trading sanctions and regularly bringing actions against insider trading defendants (Bromberg et al. 2017); whereas the international studies examine the effects of insider trading enforcement with little to no prior history of insider trading restriction.[2] Thus, differential

[2] Bromberg et al. (2017) examine insider trading court case data of countries with common law judicial systems (Australia, Hong Kong, Canada, Singapore, United Kingdom, and the United States). Among these countries, there were 682 total criminal and civil defendants sanctioned for insider trading violations over the 2009-2015 period. The U.S. sanctioned 535 defendants (78% of the international sample). The next highest regime was the U.K. with 53 insider trading defendants over the same period. Further, comparing insider trading enforcement in the U.K. with that of the U.S., the U.K. is considered to take a "softer" stance on insider trading (Gapper 2012)–the Financial Services Authority (FSA) saw its first insider trading criminal conviction in 2009 (*R. v. McQuoid*). In contrast, the first criminal conviction in the U.S. occurred in 1978 (*US v. Chiarella*). Moreoever, the U.S. is capable of imposing a 20-year prison sentence per insider trading violation, whereas the other countries considered in Bromberg et al. (2017) impose up to a 10-year sentence; the law in the U.K. only imposes up to a 7-year sentence (Coffee 2007).

enforcement coupled with significant complexity in the judicial processes that define insider trading law make it unclear whether the U.S. insider trading enforcement regime produces similar capital market benefits (see Coffee 2007 and Park 2020 for a discussion).[3] As a structural change, *Newman* provides a unique opportunity to examine the consequences of a reduction in the enforceability of traditional U.S. tipper-tippee insider trading restrictions.

The *Newman* ruling provides two distinct empirical features that allow strong inferences to be drawn regarding how insider trading restrictions relate to investor information parity and the efficiency of stock prices. First, *Newman* differentially impacted market participants due to the jurisdictional processes that underlie insider trading litigation in criminal and civil enforcement. General venue protocols of federal securities enforcement stipulate that the geographic location of a criminal or civil action be determined mainly on the basis of a tipper's and/or tippee's physical location (Vestal 1977); court venue is a critical component of securities litigation because of the U.S. legal doctrine of stare decisis, which dictates that federal district courts must follow the decisions of the Supreme Court and the court of appeals encompassing the district. Because *Newman* was controversial, legal experts questioned whether it would affect judicial decisions outside of the Second Circuit (Morrison and Foerster 2015). This likely led market participants to vary in their propensity to engage in insider trading since *Newman* would mainly reduce enforcement risk in scenarios where both the tipper and tippee were based in the Second Circuit (i.e., *Newman* represented binding precedent for federal district courts in New York, Connecticut, and Vermont).[4] Second, *Newman*'s constraint on insider trading enforcement was temporary as

[3] As discussed in Coffee (2007), some members of the investment community have raised concern that the competitiveness of the U.S. capital markets are hindered by government overregulation.
[4] An insider trading strategy involving a non-Second Circuit tipper or tippee would be susceptible to "venue shopping" by regulators to a non-Second Circuit court that would not be bound to follow the precedent established in *Newman*. This concept of securities litigation is discussed in greater detail in Section 2.

two subsequent rulings–the Supreme Court's 2016 ruling in *Salman v. US* (hereafter *Salman*) and the Second Circuit Court's 2017 ruling in *US v. Martoma* (hereafter *Martoma*)–reversed the effects of *Newman*. Together, these features approximate a quasi-experimental research setting since market participants received "as if" random treatment assignment due to the arbitrary division of U.S. circuit court jurisdictional boundaries (Dunning 2012), making it unlikely that shocks unrelated to my test design line up both in time and locale with *Newman*.[5]

The main tests focus on the trading activity of 683 hedge fund managers reporting 13F filings to the SEC over the 2013-2018 time period. Hedge funds are natural candidates to examine as potential tippees in the case of insider trading since they possess strong incentives to outperform passive benchmarks (Ackermann et al. 1999; Agarwal et al. 2009), they meet more frequently with company managers in private venues (e.g., one-on-one meetings) relative to other institutional investors (Johnson 2013; Solomon and Soltes 2015), and they likely maintain a sophisticated understanding of securities laws. To investigate whether hedge funds likely benefited from increased private information following *Newman*, I use a measure of informed trading developed in Baker et al. (2010), which captures an investor's stock picking ability in terms of being able to execute profitable trades on the basis of future earnings announcement returns. Investigating the ability of hedge funds to profitably trade in relation to future earnings is relevant to suspected insider trading since the phenomenon of insiders tipping investors with future earnings news is common in tipper-tippee insider trading prosecution (Ahern 2017).

To the extent hedge funds were able to obtain more private information regarding future earnings following *Newman*, then their trading advantages should be evident in tests that compare

[5] Huang et al. (2019) similarly exploit the exogenous features of federal circuit court jurisdictional boundaries to examine the disclosure behavior of firms, as they show that federal judge political ideology affects the ex ante risk of securities class action lawsuits.

their trade performance in the *Newman* treatment period (i.e., the period of time in which *Newman* constrained enforcement) with the pre- and posttreatment periods. Further, if *Newman* resulted in differential trading advantages among market participants because the ruling reduced enforcement risk mainly for market participants residing within the Second Circuit, then I expect to observe evidence of Second Circuit hedge funds outperforming non-Second Circuit hedge funds in their trades of Second Circuit stocks over the *Newman* treatment period.

My main findings are consistent with *Newman* having a significant effect on private information-based trading by hedge funds in the Second Circuit. Specifically, tests between the pretreatment and treatment periods show that treated hedge funds' stock-picking ability of Second Circuit firms increased significantly (approximately 61 basis points measured over three-day earnings announcement windows) following the *Newman* decision, and this treatment period abnormal performance fell to near pretreatment levels in the posttreatment period, which is consistent with the *Salman* and *Martoma* rulings reversing the effects of *Newman* in the Second Circuit. Further, the treated funds significantly outperformed nontreated funds (non-Second Circuit funds) in trading Second Circuit stocks only over the treatment period, as there was no significant difference between the two groups of hedge funds in picking Second Circuit stocks in the pretreatment and posttreatment periods. Consistent with *Newman* only affecting private information flows in the Second Circuit, neither the treated nor nontreated funds exhibit any significant changes in their stock-picking ability of non-Second Circuit firms over the sample period. In total, these results are consistent with Second Circuit hedge funds receiving information advantages in stocks when insider trading enforcement was constrained. The main finding holds through a number of robustness tests and additional tests that examine whether Second Circuit hedge funds were able to actually buy and sell stocks on the basis of future earnings surprises, which provides corroborative evidence to the inference of increased private information-based trading.

As a final analysis, I examine the capital market consequences of *Newman* in terms of market efficiency. The argument surrounding the necessity of prohibiting insider trading remains an unsettled debate, and the stance held by regulators is that less informed investors will reduce their trading activity and/or exit the market when insider trading is unchecked, which would *a priori* impair market efficiency (Fishman and Hagerty 1992; Khanna et al. 1994). On the other hand, proponents of insider trading argue that all market participants benefit from insider trading as informed traders impound private information into the market and reduce the need for investors to engage in redundant information gathering tasks (Manne 1966; Carlton and Fischel 1983). There is further mixed evidence in the empirical literature. For example, Meulbroek (1992) finds evidence of greater price efficiency after examining illegal trades by corporate insiders prior to material information announcements (i.e., increased price efficiency following insider trading activity). Sidhu et al. (2008) document higher adverse selection costs following the passage of Regulation Fair Disclosure (Reg FD; i.e., increased information asymmetry following disclosure regulation related to insider trading restrictions). Moreover, the international studies referenced previously report improvements in market efficiency in international markets following stronger enforcement of insider trading laws.

I provide evidence on the link between market efficiency and insider trading by examining whether *Newman* affected price efficiency as evidenced by intra-period timeliness tests that measure the speed of stock price formation leading up to quarterly earnings announcements, similar to Bushman et al. (2010). The results of these analyses suggest that the informed trading by Second Circuit hedge funds significantly improved price efficiency in Second Circuit firms over the *Newman* treatment period. In contrast, firms outside of the Second Circuit showed no evidence of greater price discovery following *Newman*.

This study is among the first to examine the capital market effects of tipper-tippee insider trading law.[6] My findings suggest that insider trading law plays a pivotal role in facilitating a level playing field for investors by mitigating trading advantages related to private information. However, an important implication of my findings is that while reduced tipper-tippee insider trading enforcement appears to lead to pronounced information disparity between investors, the related trading activity may produce positive externalities in terms of improved price discovery; these findings are thus relevant to lawmakers and regulators who face the challenge of seeking to balance the severity of insider trading penalties in relation to desired market outcomes. This paper extends the insider trading literature by documenting the relation between significant components of capital market activity and tipper-tippee insider trading restrictions. This study is also related to the literature that examines the trading performance of hedge funds (e.g., Brunnermeier and Nagel 2004; Griffin and Xu 2009; Gao and Huang 2016) and suggests that inside information may be a significant determinant of hedge fund outperformance.

Lastly, this article is relevant to current legislative processes surrounding the ongoing insider trading debate. The investment community has long called for congressional action in providing statutory definition of insider trading activity. These efforts have historically been impeded by the SEC, which has challenged legislation attempts based on the need for case-by-case flexibility in enforcement (Swanson 1997). However, my findings demonstrate that to the extent

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[6] An exception is Patel (2018), who finds broad evidence of increased insider trading across the U.S. between the *Newman* ruling and Ninth Circuit Court's ruling in *Salman* as evidenced by greater price run-ups before M&A announcements. In contrast to Patel (2018), I examine suspected insider trading activity in the hedge fund setting and exploit the quasi-experimental features of *Newman*. My findings suggest *Newman* had a more isolated effect on insider trading.

insider trading remains a "judge-made" law (Park 2020), the judicial processes that underlie insider trading enforcement can in and of themselves create information disparity among investors when federal courts disagree on the interpretation of tipper-tippee insider trading restrictions–a paradox given that insider trading law is designed to facilitate information parity among investors. Hence, this study adds credence to the call by the investment community for lawmakers to provide a clear definition of tipper-tippee insider trading activity rather than repeated attempts to create a unified standard via the courts (Martoma 2019; Bharara et al. 2020).

2. INSTITUTIONAL SETTING AND PREDICTIONS

2.1 Overview of Tipper-Tippee Insider Trading Law

The federal prohibitions on insider trading are derived from the broad antifraud statutes contained in Section 10(b) of the Securities and Exchange Act of 1934. Due to the ambiguous nature of these statutes, the scope of illegal insider trading activity is further articulated through the process of common law, which leads insider trading law to be in a continual state of evolution.[7] In 1983, the Supreme Court established tipper-tippee liability in the seminal *Dirks v. SEC* (hereafter *Dirks*) insider trading case. Per *Dirks*, outsiders face insider trading liability when an insider breaches a fiduciary duty by divulging confidential nonpublic information for a personal benefit and the outsider trades on the information. *Dirks* requires the assessment of a personal benefit to be based on objective criteria, and may include (i) a pecuniary gain, (ii) a reputational benefit that will translate into future earnings, or (iii) a gift of nonpublic information to a relative or friend. The Supreme Court established in *Dirks* that tippee liability is conditional on tipper liability. Thus, if an insider receives no personal benefit for divulging material information to an

[7] For a detailed overview of tipper-tippee insider trading law, see Park (2020).

outside recipient, there is no breach of insider trading law and neither the tipper nor the tippee have violated insider trading law even if the recipient trades on the information.

The December 2014 *Newman* ruling sought to add clarity to decades of federal court cases following *Dirks* by requiring more objective tests of an insider's personal benefit in two important ways. First, the court held that evidence of friendship between a tipper and tippee alone is not sufficient to obtain insider trading liability. Rather, even in the presence of a "meaningfully close personal relationship," the court stated that there must be evidence of an "exchange that is objective, consequential, and represents at least a potential gain of a pecuniary or similarly valuable nature [for the insider]" (Newman 2014). Second, the panel also held that in cases where the tippee is multiple levels removed from the tipper, there must be proof "that the tippee knew that an insider disclosed confidential information in breach of a fiduciary duty and that [they] did so in exchange for a personal benefit" (Newman 2014). The DOJ criticized the court's opinion, stating that *Newman*'s "exchange-based pecuniary limitation on what constitutes a personal benefit, and its resulting absolution of deliberate, corrupt, and formerly criminal insider trading that fails this new test… will dramatically limit the Government's ability to prosecute some of the most common, culpable, and market-threatening forms of insider trading" (DOJ 2015). Following the decision, the SEC similarly referred to *Newman* as "an issue of exceptional importance, because the [ruling] could affect the SEC's ability to protect investors and the markets through meritorious insider trading enforcement actions" (SEC 2015). However, despite concerns that *Newman* was inconsistent with Supreme Court precedent established in *Dirks* and conflicted with prior Second Circuit rulings (DOJ 2015; SEC 2015), the court denied the DOJ's petition to rehear the case en ban*c*, and the Supreme Court subsequently declined the DOJ's request for writ of certiorari.

Consistent with legal experts questioning the overall longevity of *Newman* and its judicial impact outside of the Second Circuit (Morrison and Foerster 2015; Miller et al. 2015), in July 2015 the Ninth Circuit parted from *Newman* in upholding the criminal conviction of the tippee Bassam Salman, which led the Supreme Court to hear *Salman* due to the resulting circuit split. In December 2016, the Supreme Court held with the Ninth Circuit, overturning much of *Newman*. In June 2017, the Second Circuit reinforced the Supreme Court's *Salman* decision in *Martoma*.[8] In combination the *Salman* and *Martoma* rulings effectively eliminated the effects of *Newman* in the Second Circuit.

2.2 Insider Trading Enforcement

The enforcement of insider trading occurs in three forums: (i) criminal actions by the DOJ in federal district courts, (ii) civil actions by the SEC in federal district courts, and (iii) and follow-on administrative actions by the SEC in courts of administrative law.[9] The U.S. federal court system is composed of three tiers: the U.S. Supreme Court, the circuit courts of appeals, and district courts. Insider trading enforcement originates in federal district courts and may continue vertically through the court system hierarchy to the extent there is basis for an appeal by either the prosecution or defense.

[8] The *Martoma* decision constitutes a novel expansion to the scope of insider trading liability in the Second Circuit that includes any selective disclosure provided by an insider that is intended to benefit the recipient irrespective of a personal benefit. *Martoma* was interpreted by the investment community as a ruling that would impair manager-investor communications that have long been permissible under disclosure regulation (Martoma 2019) and has been criticized as a standard that creates significant uncertainty as to how insider trading liability will be approached in future court decisions. See Coffee (2018) and Park (2020) for greater detail regarding *Martoma*.

[9] As noted in Perino (2019), a significant portion of insider trading enforcement occurs in SEC administrative courts in addition to federal courts, which are separate from the federal court system. However, SEC administrative actions are mostly "follow-on actions to bar defendants from the securities industry or to bar them from appearing or practicing before the [SEC]" (p. 26). Insider trading defendants can ultimately see enforcement occur in all three forums: criminal court, civil court, and SEC administrative courts (Perino 2019). For further information on the role of administrative courts in SEC enforcement actions, see Mathews (1980) and Glassman (2016).

The U.S. common law principle of *stare decisis* dictates that lower courts must follow the decisions of higher courts. Thus, all courts in the U.S. must follow precedent set by the Supreme Court, and district courts must follow precedent set by the court of appeals associated with the district court. Further, each court of appeals follows the precedent set by the Supreme Court and the precedent established by itself, but importantly, district courts and courts of appeals do not necessarily need to follow the decisions of courts in other circuits (Hansford 2011; Walker 2016). This is an important feature of securities regulation as the partitioning of the U.S. federal court system into jurisdictional boundaries can create cross-sectional differences across market participants with respect to the outcomes of securities enforcement if federal judges are expected to differ in their interpretation of securities laws and/or disagree with decisions made by courts in other jurisdictions. On this point, Huang et al. (2019) find that federal circuit court judge political ideology creates cross-sectional differences in litigation risk for firms and their related willingness to provide long-term earnings guidance due to the risk of shareholder class actions related to disclosure.

2.3 Predictions

These intricacies of enforcement have important implications for insider trading enforcement in the wake of *Newman*. Specifically, *Newman* plausibly allowed for sophisticated insider trading to occur in the Second Circuit prior to the *Salman* and *Martoma* rulings since *Newman* represented binding precedent for all federal courts in the Second Circuit. However, the risk of insider trading enforcement was likely preserved for market participants outside of the Second Circuit to the extent that the DOJ and/or SEC could strategically litigate insider trading activity in more favorable non-Second Circuit courts, where *Newman* would not dictate the decision of a lower district court.[10] Therefore, to the extent *Newman* impacted information asymmetry between investors and, relatedly, the efficiency of stock prices, then I expect this impact to be concentrated within Second Circuit firms. That is, to the extent hedge funds exploit private information following *Newman*, then I expect to observe a significant increase in informed trading by Second Circuit hedge funds within Second Circuit stocks. Moreover, to the extent this trading more or less impacts the efficiency of stock prices, I expect to evidence of significant changes in price discovery within Second Circuit firms.

3. SAMPLE DEVELOPMENT AND DESCRIPTIVE STATISTICS

3.1 Sample Development

The sample period begins in the first quarter of 2013 and ends in the final quarter of 2018, which allows me to examine hedge fund trading activity over three, two-year testing periods: the pretreatment period spanning 2013-2014, the treatment period spanning 2015-2016 (beginning immediately following the Second Circuit's December 2014 *Newman* decision), and the posttreatment period spanning 2017-2018 (beginning immediately following the Supreme Court's *Newman*-reversing December 2016 decision in *Salman*). Figure 1 outlines the sample testing periods, as well as the timing of notable judicial events related to *Newman* and the subsequent court rulings.

[10] Stare decisis would have provided Second Circuit market participants with assurance of the shift in insider trading law. As noted in Walker (2016), stare decisis "confers many benefits on the American judicial system. At its core, the doctrine protects… the legitimate expectations of those who live under the law. [It] promotes stability, represents an element of continuity in law, and is rooted in the psychological need to satisfy reasonable expectations… [It] furthers a system of justice based on fairness to the individual… and ensures that legal changes move in an incremental fashion, facilitating the gradual assimilation of new rules into the overarching legal framework" (p. 3).

I construct a sample of hedge fund holdings data by identifying hedge fund managers via Form ADV filings.[11] Following prior work (Brunnermeier and Nagel 2004; Griffin and Xu 2009; Gao and Huang 2016), I identify hedge fund managers as investment advisors charging performance-based fees (Form ADV item 5-E(6)) that have 50% or more of investment listed as "other pooled investment vehicles" or 50% or more client base listed as "high net worth individuals" (Form ADV items 5-D(B) and 5-D(F), respectively). Only funds with $100 million or more in assets under management (AUM) at the end of 2014 are considered in the screening process, since hedge funds with less than $100 million in AUM are less likely to file a 13F with the SEC. I then obtain hedge funds' long-equity positions reported on 13F filings by manually merging the Form ADV data with the Thomson Reuters S34 Institutional Holdings database.[12]

The location of each fund is based on the primary business address reported on Form ADV, and I use this address to identify funds headquartered in the Second Circuit, which includes the U.S. states of New York, Connecticut, and Vermont. I require each hedge fund to file a 13F in each of the three testing periods and to not switch their primary business address to-from the Second Circuit during the sample period. This latter condition is to help facilitate empirical inference by ensuring "as if" random treatment assignment in the *Newman* setting. As noted in Dunning (2012), "as if" treatment assignment is violated if observational units "self-select into treatment conditions" (p. 236). Self-selection could be a plausible issue in this context as investors

[11] The Dodd-Frank Act mandated that the majority of hedge funds begin registering with the SEC in July 2011 via Form ADV, although some hedge funds remain exempt from this reporting requirement (assets of less than $150 million). For more detail regarding hedge fund public disclosure requirements, see Honigsberg (2019).

[12] Identifying hedge funds solely via Form ADV filings is an imprecise process, which is mainly due to the fact that a single 13F will contain the equity positions of numerous funds reporting underneath a single fund family. I do not expect this to bias my analysis, however, since I am mainly interested in identifying institutional investors with high demands for private information and incentives to capitalize on *Newman*, which is achieved through the Form ADV screening process as detailed in Brunnermeier and Nagel (2004).

may have shifted their corporate headquarters if they perceived a more favorable regulatory environment in the Second Circuit.[13]

The final sample includes 683 distinct U.S.-based hedge fund managers. At the end of 2014, the mean (median) hedge fund manages 13F portfolio values of $3.027 billion ($475 million), and in aggregate hold approximately 6.9% of CRSP equity, which is similar to the magnitude of hedge fund holdings reported by prior work (e.g., Griffin and Xu 2009; Gao and Huang 2016).

3.2 Descriptive Statistics

In Panel A of Table 1, a map of the U.S. federal circuit courts of appeals is provided for reference. Panel B of Table 1 reports the geographic dispersion of the sample hedge funds across the U.S. circuit courts. Notably, 327 (roughly 48%) of the sample funds are based in the Second Circuit, and together they hold approximately 38.9 percent of all hedge fund equity at the end of 2014. I obtain data on the characteristics of hedge funds' U.S.-based portfolio firms by measuring accounting and stock price fundamentals from the intersection of the Compustat Quarterly and CRSP databases. The primary business address listed in Compustat is used to identify firm location. Panel B of Table 1 also reports the geographic dispersion of the portfolio firms held by the sample hedge funds. Notably, approximately 11.6% of the portfolio firms are based in the Second Circuit, which includes approximately 13.8% of total U.S. market capitalization per Compustat.

Panel A of Table 2 reports descriptive statistics for the sample hedge funds. Bolded values in Panel A indicate significant differences (p-value < 10%) in the characteristics of the two groups of hedge funds. The average Second Circuit (non-Second Circuit) hedge fund manages

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[13] In developing the sample, only two hedge funds shifted their primary business address to/from the Second Circuit over the sample period. These funds were excluded from the analysis.

approximately \$2.577 (\$3.747) billion in public equity (*PortSize*) over the sample period. Relative to non-Second Circuit hedge funds, Second Circuit hedge funds have higher portfolio concentration (*PortConcentration*), lower turnover (*PortTurnover*), and hold stocks with lower market capitalization (*CompMV*), higher prior returns (*CompMOM*, based on median difference), and higher volatility (*CompVOL*). Panel B of Table 2 reports the portfolio concentration of the sample funds in the Fama-French 30 industry classifications. For the most part, Second Circuit and non-Second Circuit funds are relatively similar in apportioning capital in each respective industry, yet there are significant differences in 13 industries (see bolded categories).

4. RESEARCH DESIGN AND MAIN RESULTS

4.1 Research Design

Using the methodology developed in Baker et al. (2010), I examine the ability of hedge funds to make informed trading decisions on the basis of future earnings announcement returns. Intuitively, the methodology measures trading skill as the ability of an institutional investor to buy (sell) stocks in quarter q which are about to realize high (low) earnings announcement returns in quarter $q+1$. Following Baker et al. (2010), I measure three-day raw earnings announcement returns as the cumulative return over the [–1,+1] interval surrounding earnings announcement dates for each firm-quarter at the intersection of Compustat and CRSP. Each calendar quarter, I then compute a benchmark earnings announcement return for each portfolio firm, which is the firm's three-day earnings announcement return less the value-weighted three-day earnings announcement return of stocks in the same portfolio formed on quintile sorts of size, book-to-market, and momentum (5 x 5 x 5) in the same calendar quarter (i.e., *DGTW* portfolio).[14] A hedge

[14] I follow Daniel et al. (1997) in sorting firms into 125 characteristics-based portfolios.

fund's trades in quarter q are then sorted into weight-increasing "buys" and weight-decreasing "sales" based on changes in portfolio weight over a 13F trading quarter. A hedge fund's stock picking ability (*BAR*) is then measured as follows:[15]

$$BAR = 1 / I \bullet \sum^{I}_{i=1} (r_{j,q+1} - r_{DGTW,i,q+1}) - 1 / D \bullet \sum^{D}_{d=1} (r_{j,q+1} - r_{DGTW,d,q+1}) \qquad [1]$$

where I indexes stocks with portfolio-weight increases over quarter q, and D indexes stocks with portfolio-weight decreases over quarter q; $r_{j,q+1}$ is the three-day raw earnings announcement return of stock j in quarter $q+1$; and $r_{DGTW,j,q+1}$ is the value-weighted benchmark-adjusted earnings announcement return in quarter $q+1$ of stock j's *DGTW* benchmark portfolio. I measure *BAR* separately for Second Circuit portfolio firms (BAR^{2C}) and non-Second Circuit portfolio firms (BAR^{N2C}). In general, a positive and significant *BAR* provides evidence that a fund is able to either forecast or acquire private information regarding future fundamentals and/or other important information reported in the forthcoming earnings announcements of its portfolio firms.

As discussed in Baker et al. (2010), there are several empirical benefits associated with *BAR*. First, because *BAR* is based on trades, Fama's (1970) joint-hypothesis problem is mitigated since unobserved risk premiums are differenced away by comparing the performance of stocks that are bought against those that are sold. Second, as opposed to holdings-based tests, trade-based tests provide greater detection of information-driven trading activity (Chen et al. 2000; Kacperczyk et al. 2005). Lastly, *BAR* controls for the documented association between earnings announcement returns and known firm characteristics, namely low market value and high book-to-market (Baker et al. 2010).

[15] Inferences are similar if I calculate *BAR* with a value-weighting approach. The results of value-weighted *BAR* are reported in Appendix C.

To the extent that a hedge fund's *BAR* performance is primarily derived from an investor's ability to select stocks independent of inside information, then *BAR* should not significantly vary in relation to the *Newman* ruling. While I interpret fluctuations in *BAR* that are directionally consistent with *Newman* to be evidence of increased private information-based trading, an important caveat relates to my inability to determine whether improved stock picking ability is a direct result of insider trading. However, in addition to the advantageous empirical features of the *Newman* setting, several other factors may partially alleviate this concern. First, as noted previously, the selective disclosure of future earnings news is common in tipper-tippee insider trading cases (Ahern 2017). Second, prior work that examines *BAR* in connection with informed trading by institutional investors document a positive relation between *BAR* and private disclosure (Baker et al. 2010; Bhojraj et al. 2012). Lastly, changes in the enforceability of insider trading law is unlikely to impact a hedge fund's independent stock picking ability. The tactics deployed by regulators to detect illegal insider trading over the past decade have involved sophisticated investigation techniques, such as wiretaps, the use of cooperating witnesses, and broad subpoena power to seize corporate communication records (Loewenson and Smithline 2017). Sophisticated market participants (hedge funds) were likely aware of these monitoring efforts, and to the extent that a hedge fund manager's trading information had no connection to corporate insiders or other individuals holding a fiduciary duty to withhold disclosing nonpublic material information, there is little reason to suspect that a hedge fund would forgo trading profits related to their independent trading abilities.

4.2 Main Results–Univariate Analysis

I first present univariate tests of hedge fund stock picking ability.[16] Table 3 reports the mean BAR^{2C} and BAR^{N2C} for the sample hedge funds over the sample period. To better understand which trade types more or less contribute to *BAR*, the component equal-weighted buy (*Buys*) and sale (*Sales*) returns are also reported. The arrangement of Table 3 is organized as follows: Rows 1–3 (4–6) of Panel A report the stock picking ability of Second Circuit hedge funds in Second Circuit (non-Second Circuit stocks) over the testing periods; and Rows 7–9 report univariate tests of difference between the components of BAR^{2C} and BAR^{N2C} within each testing period. Columns 1–3 contain the *BAR* measures for the pretreatment, treatment, and posttreatment periods, respectively; and columns 4–5 report the tests of differences within the components of BAR^{2C} and BAR^{N2C} (separately) across time. Differences-in-differences (DiD) tests, which indicate the outperformance of BAR^{2C} between the treatment period and control periods relative to BAR^{N2C}, are reported in Rows 7–9 of Columns 4 and 5. Panel B reports similar statistics for the control group of non-Second Circuit hedge funds. And Panel C reports the same values differenced between Second Circuit hedge funds and non-Second Circuit hedge funds.

Panel A shows that BAR^{2C} increased significantly over the treatment period for Second Circuit hedge funds; specifically, BAR^{2C} increased by a significant 61 basis points (see Row 3 of Column 4) relative to the pretreatment period, and subsequently fell a significant 55 basis points (see Row 3 of Column 5) in the posttreatment period. Notably, it appears that the increase in BAR^{2C} for Second Circuit funds in the treatment period was largely attributable to their ability to preempt negative earnings returns, which contrasts starkly with the pretreatment and posttreatment periods where their sale decisions are uninformed to the extent that an informed hedge fund would not sell

[16] This approach follows Dunning (2012), who recommends simple analyses when a credible natural experiment is identified.

stocks with future earnings announcement returns that outperform the earnings announcement returns of stocks that are purchased.[17] Importantly, Second Circuit funds' trading of non-Second Circuit firms (BAR^{N2C}) reveals no evidence of significant variance over the sample period, which suggests that *Newman* likely affected the profitability of Second Circuit hedge fund trading only within the Second Circuit.

Panel B reports the stock picking ability of non-Second Circuit funds. Importantly, these funds exhibit no variance in BAR^{2C} and BAR^{N2C}, which is consistent with *Newman* providing no advantage to these funds. Panel C reports the differences in the *BAR* performance between Second Circuit and non-Second Circuit hedge funds, with the most pronounced differences in *BAR* attributable to informed trading between the two groups of hedge funds being observable in the treatment period for Second Circuit stocks (BAR^{2C}), with Second Circuit funds outperforming non-Second Circuit funds by a significant 37 basis points over the treatment period (see Row 3 of Column 2).[18] The bottom-right corner of Panel C reports univariate difference-in-difference-in-differences (DiDiD) tests, which show a positive and significant 63 (–50) basis point DiDiD relative to the pretreatment (posttreatment) period.

Overall, the stock picking ability of Second Circuit hedge funds in the *Newman* treatment period is economically significant. Consider the *BAR* performance of the mutual funds documented in Baker et al. (2010), who examine mutual fund *BAR* over the 1980-2005 period. Baker et al. (2010) and Bhojraj et al. (2012) both document a significant attenuation of mutual fund *BAR* following the passage of Reg FD, consistent with *BAR* being associated with private information.

[17] These results are consistent with recent findings that institutional investors do well in buying stocks but tend to do poorly in sell decisions (Akepanidtaworn et al. 2019).

[18] While Column 1 of Panel C suggests that Second Circuit hedge fund BAR^{2C} was significantly lower than non-Second Circuit hedge fund BAR^{2C} in the pretreatment period, it is important to note that this difference does not arise from non-Second Circuit hedge funds necessarily being informed (see Panel B); rather, the difference is driven by Second Circuit hedge fund BAR^{2C} being negative and significant in the pretreatment period (see Panel A) due to their buy trades significantly underperforming their sale trades.

Baker et al. (2010) report a pre-Reg FD equal-weighted mean *BAR* value of 0.49 annualized basis points (see Table 5 in Baker et al. 2010; p. 1125). As a comparison, if I similarly annualize the BAR^{2C} estimates in Table 3 (untabulated), then Second Circuit hedge fund's equal-weighted BAR^{2C} in the *Newman* treatment period is a significant 1.15 annualized basis points (*t*-stat = 2.72). Furthermore, the largest mutual funds examined in Bhojraj et al. (2012) are reported to have non-annualized equal-weighted *BAR* performance of 25 basis points in the pre-Reg FD years of 1994-1999 (see the fund size quintile 5 *BAR2* estimate reported in Table 3 of Bhojraj et al. 2012), which is 4 basis points lower than the treatment period Second Circuit hedge fund BAR^{2C} of 29 basis points (see Panel A of Table 3). This is notable as the mutual funds examined in Baker et al. (2010) and Bhojraj et al. (2012) had significant private information advantages in the pre-Reg FD period, which suggests *Newman* had a significant impact on private information in the Second Circuit.

4.3 Main Results–Multivariate Analysis

I next examine *BAR* in a multivariate DiD framework using the following OLS regression:

$$
\begin{aligned}
BAR^{T/C} = {} & \alpha + \delta + \beta_1 Treatment + \beta_2 (HF^{2C} \text{ x } Treatment) \\
& + \beta_3 Posttreatment + \beta_4 (HF^{2C} \text{ x } Posttreatment) \\
& + \textstyle\sum^{7}_{k=1} \beta_k X + \varepsilon \qquad [\,2\,]
\end{aligned}
$$

Fund and year-quarter (e.g., 2015Q1) fixed effects are denoted by α and δ, respectively. $BAR^{T/C}$ is either BAR^{2C} (treated trades) or BAR^{N2C} (control trades). *Treatment* (*Posttreatment*) is an indicator equal to one for fund-quarters in the 2015-2016 (2017-2018) period, and zero otherwise. HF^{2C} is an indicator equal to one for hedge funds headquartered in the Second Circuit, and zero otherwise. X is a vector of time-varying controls that include the hedge fund characteristics reported in Table 1. Because the HF^{2C} dummy variable is time invariant, its base effect is omitted from Equation (2). In estimating Equation (2), standard errors are computed by using two-dimensional clustering by fund and year-quarter so as to correct for residual correlation. The variable of interest is the DiD coefficient on *HF^{2C} x Treatment*, which will be positive and significant in estimations of Equation (2) with *BAR^{2C}* as the outcome variable if Second Circuit hedge funds significantly outperform non-Second Circuit hedge funds after controlling for time period effects and time varying and time invariant hedge fund characteristics.

Table 4 reports the estimations of Equation (2). Columns 1 and 2 (3 and 4) report the results for *BAR^{2C}* (*BAR^{N2C}*); Columns 1 and 3 (2 and 4) report the estimations without (with) control variables. The coefficient on *HF^{2C} x Treatment* in Columns 1 and 2 are positive and significant (*p*-values < 0.01), which suggests outperformance by Second Circuit hedge funds in trading Second Circuit stocks relative to non-Second Circuit hedge funds in the treatment period. The coefficients on *HF^{2C} x Treatment* in Columns 1 and 2 are also significantly larger than the coefficients on *HF^{2C} x Posttreatment* (see F-tests reported at the bottom of Table 3), which suggests a loss of information advantage for Second Circuit hedge funds following the *Salman* ruling. The coefficients on *HF^{2C} x Treatment* in Columns 3 and 4 are not significant, which indicate that *Newman* had no significant effect on Second Circuit hedge stock-picking ability in non-Second Circuit firms, relative to non-Second Circuit hedge funds. The cross-equation differences in the coefficients on *HF^{2C} x Treatment* between Columns 1 and 3, and Columns 2 and 4, are significant ($\chi^2 = 10.29$ and 11.13, respectively). The results on the *HF^{2C} x Treatment* coefficients in Columns 1 and 2 are also similar in magnitude, which suggests that the relationship between *BAR^{2C}* and the DiD coefficient is largely independent of the observable hedge fund characteristics.

5. ROBUSTNESS TESTS

In this section, I conduct three tests to examine the empirical robustness of the main finding. These include tests of the parallel trends assumption in the *Newman* setting, bootstrapping placebo tests that consider whether the main inference is due to random chance, and a fund-level changes analysis following the recommendation of Bertrand et al. (2004).

5.1 Test of Parallel Trends Assumption

As addressed in Roberts and Whited (2013), DiD estimation requires a parallel trend in the outcome variable (in this case, BAR^{2C}) among treated and nontreated groups. Therefore, I next test whether the parallel trends assumption is reasonable in the *Newman* setting. To do so, I modify Equation (2) by expanding the pretreatment period to include trading activity in 2011 and 2012 and examine stock-picking ability by the sample funds over the 2011–2016 period. Thus, this analysis considers the BAR^{2C} performance of Second Circuit hedge funds relative to non-Second Circuit hedge funds over a four-year pretreatment period (2011–2014) and the two-year *Newman* treatment period (2015–2016). To assess parallel trends, I replace the *Treatment* dummy variable in Equation (2) with indicator variables that identify each calendar quarter spanning 2014–2016, with each of these quarter indicators interacted with the dummy for Second Circuit hedge funds (HF^{2C}). If the parallel trends assumption is reasonable, then there will be little to no evidence of significant variance between the two groups of hedge funds in the year leading up to the *Newman* ruling–thus, the coefficients on HF^{2C} interacted with the four calendar quarter indicators for the year 2014 should be insignificant.

Table 5 reports the result of this estimation. The results indicate that the pretreatment interactions of interest (HF^{2C} interacted with the four 2014 quarter indicators) are insignificant, which suggests that Second Circuit and non-Second Circuit hedge funds did not significantly differ in their BAR^{2C} performance prior to *Newman*. However, following *Newman*, Second Circuit hedge funds exhibit significantly greater performance in trading Second Circuit firms, evidenced by the positive and significant coefficients on HF^{2C} interacted with the quarter-time indicators for 2015 and 2016 years. Specifically, all of the HF^{2C} and 2015-quarter interactions are positive and significant, and two of the HF^{2C} and 2016-quarter interactions are positive and significant. These results suggest that the parallel trends assumption is reasonable in the *Newman* setting with respect to stock picking ability of Second Circuit stocks, as BAR^{2C} does not significantly differ between the treated and nontreated hedge funds in the year preceding the *Newman* treatment effect.

5.2 Bootstrapping Placebo Test

I next conduct a bootstrapping placebo analysis, which considers whether the effects documented in the main analysis are due to random chance.[19] Note that in the full estimations of Equation (2) where BAR^{2C} is the outcome variable (see Table 4), the coefficient on the variable of interest (HF^{2C} x *Treatment*) is 0.66 (t-statistic = 3.46). I conduct a bootstrapping placebo test which creates a random distribution of coefficients for Equation (2) with BAR^{2C} as the outcome variable. In the first placebo test, I examine whether the differences observed across the dimension of treatment classification of hedge funds is due to chance by maintaining the actual *Treatment* and *Posttreatment* time indicator variable classification, and randomly assigning HF^{2C} to the sample hedge funds in proportion to the actual rate of treatment assignment, and re-estimate the full specification of Equation (2), including time and hedge fund fixed effects, over 1,000 iterations.[20] I then compute a p-value by comparing the actual coefficient estimate of 0.66 against the random

[19] This analysis is similar to that performed in Dyreng et al. (2016), who conduct DiD tests of the effects of tax scrutiny on corporate tax avoidance in the U.K.
[20] In this procedure, I begin with the vector of 683 distinct hedge funds and randomly assign 327 of the hedge funds as "treated" funds irrespective of their actual primary business address. These data are then joined to the main sample data whereupon I interact the placebo HF^{2C} dummy with the *Treatment* and *Posttreatment* indicators, and re-estimate Equation (2). The placebo test coefficient on HF^{2C} x *Treatment* is retained, and this process is repeated 1,000 times.

distribution of placebo coefficients and counting the number of instances in which the placebo coefficient on HF^{2C} x *Treatment* exceeds 0.66. In 1,000 randomly generated samples, the placebo coefficient on HF^{2C} x *Treatment* never exceeds 0.66; this corresponds to a *p*-value equal to 0.000.

Next, I conduct a bootstrapping placebo analysis along the dimension of time, where *Treatment* and *Posttreatment* are randomly assigned in proportion to the actual frequency of the fund-quarter sample. The actual measure of HF^{2C} is preserved in the analysis.[21] I then construct a random distribution of coefficients related to Equation (2) by re-estimating the full specification of Equation (2), including time and hedge fund fixed effects. The coefficient of interest (HF^{2C} x *Treatment*) in the actual sample data is 0.66. In the 1,000 iterations where the treatment and posttreatment periods are randomly classified, I observe a placebo coefficient on HF^{2C} x *Treatment* that is greater than 0.66 in only one instance; this corresponds to a *p*-value equal to 0.001. Together, these tests provide strong evidence that my main finding is not due to random chance, or that there are structural issues in the underlying data which produce understated standard errors in the OLS analysis. In the next set of robustness tests, I conduct a series of changes analysis which more directly relates to the potential of serially correlated errors in DiD estimation.

5.3 Changes Analysis

Bertrand et al. (2004) demonstrate that DiD estimations can lead to over-rejection of the null hypothesis of no treatment effect in the presence of positive serial correlation. Following their recommendation, I collapse the sample data into three observations per sample hedge fund, where

[21] The random assignment of *Treatment* and *Posttreatment* here is done as follows. Note that the sample period spans 2013–2018, which includes 24 13F-reporting calendar quarters. Thus, in each bootstrap iteration the *Treatment* indicator is randomly assigned to eight of the sample quarters, and the *Posttreatment* indicator is randomly assigned to eight of the sample quarters not indicated as *Treatment*; the remaining eight quarters are thus randomly associated with the pretreatment period, and represent the omitted time category in the re-estimation of Equation (2). In each iteration, the random time period assignment is then joined to the actual sample data, and the actual HF^{2C} dummy is then interacted with the *Treatment* and *Posttreatment* indicators, after which I then re-estimate Equation (2). The placebo test coefficient on HF^{2C} x *Treatment* is retained, and this process is repeated 1,000 times.

the new observations represent the mean testing period (pretreatment, treatment, and posttreatment) values. This process produces consistent standard errors even in the presence of a small geographic treatment effect (Bertrand et al. 2004), which is relevant in the *Newman* context. Table 6 reports the re-estimation of Equation (2), with time and hedge fund fixed effects omitted and standard errors computed via bootstrapping over 1,000 iterations.[22] The change variables are computed between the pretreatment and treatment periods, and treatment and posttreatment periods. Columns 1 and 2 (3 and 4) report the change analysis from the pretreatment to the treatment period (treatment to the posttreatment period), with ΔBAR^{2C} and ΔBAR^{N2C} as the outcome variable, respectively. The results here are consistent with my prior findings. That is, the change coefficient on HF^{2C} is positive and significant (p-value < 0.001) in Column 1, and negative and significant (p-value < 0.01) in Column 3, which together suggest an increase in BAR^{2C} over the *Newman* treatment period and subsequent decrease following the *Salman* ruling for Second Circuit hedge funds. The coefficients on HF^{2C} in Columns 2 and 4 are not significant, suggesting no significant differences between the sample hedge funds over the sample testing periods.

6. ALTERNATIVE MEASURE OF STOCK PICKING ABILITY

The analyses thus far demonstrate that Second Circuit hedge funds showed an increase in their informed trading ability post-*Newman* based on their ability to preempt earnings announcement returns. A related question is whether these hedge funds' increased stock picking ability derived from an ability to actually preempt earnings news. To investigate this possibility, I follow Baker et al. (2010) and measure stock picking ability (*ueSPA*) on the basis of future earnings surprises as follows:

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[22] Inferences are similar if the changes analysis are estimated using OLS.

$$ueSPA = 1 / I \bullet \sum^{I}_{i=1} (ae_{j,q+1} - exp_{i,q+1}) - 1 / D \bullet \sum^{D}_{d=1} (ae_{j,q+1} - exp_{i,q+1}) \qquad [3]$$

where *I* indexes stocks with portfolio-weight increases over quarter *q*, and *D* indexes stocks with portfolio-weight decreases over quarter *q*; $ae_{j,q+1}$ is the actual earnings of stock *j* in quarter *q+1*; and $exp_{i,q+1}$ is the consensus (median) earnings per share of stock *j* in quarter *q+1* as reported in the I/B/E/S summary database, scaled by stock price at the beginning of the forecast period. *ueSPA* is multiplied by 100 at the firm-quarter level for exposition. *ueSPA* calculated with respect to trades of Second Circuit (non-Second Circuit) stocks is denoted as $ueSPA^{2C}$ ($ueSPA^{N2C}$). To the extent $UE\ Spread^{2C}$ significantly varies with *Newman*, this would provide evidence that Second Circuit hedge funds' trading advantage plausibly stemmed from private information, as it is unlikely that buying and selling stocks in relation to future surprise earnings, in the absence of private information, would vary in time and locale with *Newman*.

Table 7 reports the univariate statistics of *ueSPA* in a similar manner as Table 3. For brevity, *t*-statistics are omitted, and the univariate statistics of non-Second Circuit hedge funds are also omitted so as to reduce the dimensionality of the tabulated results. Panel A reports *ueSPA* for Second Circuit hedge funds and Panel B reports *ueSPA* for non-Second Circuit hedge funds. Panel C reports the tests of differences between the treated and nontreated hedge funds. Panel A shows that the Second Circuit hedge funds experienced a significant increase in their stock picking ability of Second Circuit stocks in the *Newman* treatment period, as evidenced by the significant $ueSPA^{2C}$ reported in Row 3 of column 2. In Column 3, $ueSPA^{2C}$ is positive but not significant, which would suggest that the Second Circuit hedge funds private information advantages were reduced in the post-*Salman* period. This analysis provides further corroborative evidence to the main inference of this paper as $ueSPA^{N2C}$ is insignificant in each testing period and does not significantly vary across time for Second Circuit hedge funds.

Notably, non-Second Circuit hedge funds similarly showed improved stock picking ability on the basis of future earnings surprises from the pretreatment to the treatment period in Second Circuit stocks (see Row 3 of Panel B). However, this increase was not as large as the increase in $ueSPA^{2C}$ for Second Circuit hedge funds, as evidenced by the positive and significant difference reported in Row 3 of Panel C. I also examine *ueSPA* with regression analysis via re-estimation of Equation (2). Table 8 reports these analyses, where *BAR* is substituted with *ueSPA*.[23] In Column 1, the coefficient on the variable of interest (HF^{2C} x *Treatment*) is marginally significant (p-value < 0.10 based on a one-sided test). The inference of a pronounced increase in stock picking ability in the Second Circuit is weaker in this test, as the coefficient on HF^{2C} x *Treatment* is not significantly different between Columns 1 and 2. However, Table 8 overall provides marginal evidence of increased stock picking ability by Second Circuit hedge funds in Second Circuit stocks based on future earnings news.

7. TESTS OF MARKET EFFICIENCY

I next investigate an important consequence of the *Newman* ruling–whether suspect trading activity by hedge fund managers following *Newman* led to more or less market efficiency. As previously discussed, there is both theoretical and empirical ambiguity on the effects of insider trading activity on market efficiency (Manne 1966; Carlton and Fischel 1982; Fishman and Hagerty 1992; Khanna et al. 1994; Meulbroek 1992; Bhattacharya and Daouk 2002; Fernandes and Ferreira 2009; Christensen et al. 2016). *Newman* received significant media attention, which

[23] Here, the number of firm-quarter observations is reduced as *ueSPA* is calculated based on hedge fund stock holdings that are covered by analysts in the I/B/E/S database, which reduces the number of data at the institution-firm-quarter level.

likely led to market participants being broadly aware of the constraints placed on regulators.[24] The *Newman* ruling therefore provides a rich empirical setting to test the capital market effects of a structural change in insider trading law, as news of *Newman* was likely ubiquitous in the market, and my prior test results suggest that the ruling plausibly led to increased (traditional) insider trading activity.

7.1 Research Design

To investigate this issue, I examine the speed of price formation between quarterly earnings announcement cycles via tests of intraperiod timeliness (IPT), similar to Bushman et al. (2010) who examine the effect of syndicated loans on equity price discovery. IPT is an area-under-the curve measure that approximates market efficiency by capturing the speed at which private information is impounded into publicly available stock price, and is measured as:

$$IPT = \tfrac{1}{2} \sum^{+M}_{m=-60} (BH_{m-1} + BH_{m}) / BH_{M} = \sum^{+M-1}_{m=-60} (BH_{m} / BH_{M}) + \tfrac{1}{2} \qquad [\ 4\ ]$$

where the measurement interval [*m*, M] is the [–60, +1] interval surrounding earnings announcement dates [0]. Higher *IPT* values are associated with greater price efficiency, as a higher area-under-the-curve would indicate more timely incorporation of information into price prior to quarterly earnings news. Examining *IPT* over the [–60,+1] interval with respect to earnings announcements (which is similar to Bushman et al. 2010) is complementary to the prior tests of hedge fund stock picking ability, as informed trading by hedge funds in the quarter preceding earnings announcements would likely correspond to price discovery activity within much of the 62-day *IPT* measurement interval.

[24] *Newman* received immediate coverage by large national media outlets such as the Wall Street Journal, Financial Times, New York Times, CNBC, Bloomberg, and Forbes, among others.

7.2 Sample Development and Descriptive Statistics

Tests of price efficiency necessitate a shift in the observational unit of analysis from hedge fund-level analysis to firm-level analysis. Accordingly, I develop a firm-quarter sample of public issuers that were held by Second Circuit hedge funds in the treatment period. I retain firm-quarters with data necessary for estimating *IPT*, as well as basic accounting fundamental control variables which include firm market value (*Firm Size*), book-to-market (*Book-to-Market*), return on equity (*Return on Equity*), long-term debt to total assets (*Leverage*), and intangible assets (*Intangibles*); I also include controls for price fundamentals–prior year abnormal returns (*Returns*), and stock liquidity (*Liquidity*). To control for factors that affect information production in the market as well as the demand for information, I control for the number of analysts covering the firm (*Analysts*) and the fraction of common shares held by institutional investors (*Inst. Ownership*). I also control for the number of Wall Street Journal articles and Dow Jones newswires produced for each firm over the [–60,–1] interval preceding earnings announcement dates (*Press Coverage*), as prior work shows that news wire has a significant effect on price discovery processes (Bushee et al. 2010; Rees et al. 2015; Twedt 2016; Drake et al. 2017). These variables are formally defined in Appendix A.

Panel A of Table 9 reports the steps included in forming the firm-quarter level for testing price discovery. Specifically, I eliminate extreme *IPT* observations by truncating the sample at the 0.5% and 99.5% levels of *IPT* and then winsorizing all continuous variables at 1% and 99%.[25] I

[25] I manually examine the characteristics of the extreme *IPT* observations prior to truncation. Notably, these firm-quarters do not significantly differ from the preserved sample in terms of firm size, book-to-market, or prior return activity. However, the firms significantly differ in terms of the 62-day total cumulative abnormal return used to estimate *IPT*. Specifically, the truncated firms have extremely low absolute 62-day cumulative returns on average (0.16% as opposed to 13.53% for the preserved sample firms; *t*-test statistic = 19.43), which is consistent with area-under-the-curve measures being unsuitable for instances of small cumulative price response (Blankespoor et al. 2020). Inferences are similar if I retain the extreme *IPT* observations and winsorize all variables at 1% and 99%. Inferences are also similar if, after truncating the sample, I preserve the raw *IPT* values in the analysis.

also require each firm to have data coverage in each of the sample testing periods to accommodate firm fixed effects DiD estimations. These criteria produce a sample of 81,949 firm-quarter observations (4,008 unique firms) spanning the 2013-2018 period. Panel B reports descriptive statistics for the sample firms.

7.3 Univariate Test Results

In Panel A of Table 10, plots 1 and 2 present the 62-day *IPT* curves for the sample firms for Second Circuit firms ($Firm^{2C} = 1$) and non-Second Circuit firms ($Firm^{2C} = 0$), respectively. Each figure provides the *IPT* curves associated with the pretreatment, treatment, and posttreatment periods. A visual examination of Plot 1 provides evidence of increased price efficiency in the treatment period. Specifically, nearly 45% of the cumulative 62-day abnormal return is realized approximately 40 days prior to earnings announcements of the Second Circuit firms in the *Newman* treatment period. In contrast, cumulative price discovery is approximately 25–35% realized on average for the same firms in the control periods. The treatment period curve trends above the curves for the control periods, indicating timelier price incorporation leading up to earnings news, and the curves appear to converge in the five days preceding earnings announcements. A visual examination of Plot 2 suggests that there is no difference in pre-earnings announcement price discovery for non-Second Circuit firms between the testing periods.

Panel B reports the actual *IPT* values for the sample firms in each sample testing period. The mean *IPT* of Second Circuit stocks in the treatment period is 31.99, and this value significantly exceeds the pretreatment and posttreatment periods by 6.24 and 5.51, respectively. Univariate *t*-tests indicate that these differences are significant at traditional levels (p-value < 0.05).

In addition to *t*-tests, I test the statistical difference between these values by using nonparemetric permutation analysis. This latter approach is ideal for area-under-the-curve measures, which are subject to considerable noise in measurement, though truncating extreme *IPT* observations reduces much of this noise. The permutation tests are akin to bootstrapping methods in that they involve random shuffling of treatment and nontreatment assignment along the two dimensions of (i) treatment classification among the sample firms, and (ii) across testing periods in terms of treatment and posttreatment classification. These tests are conducted as follows. First, I randomly shuffle treatment assignment for each set of paired testing groups in direct proportion to the true rate of treatment assignment in the subgroup population. Second, *IPT* values are then constructed and differenced between the placebo treated and nontreated groups. After iteratively conducting the previous two steps 1,000 times, I count the number of instances that the placebo difference is as or more extreme than the actual difference. *P*-values (reported in brackets in Panel A) are calculated based on the number of times that the difference in *IPT* values between the placebo treated and nontreated groups is equal to or larger than the true difference, divided by 1,000. As evidenced in Panel B, the *p*-values associated with the permutation analyses are significant according to traditional levels and suggest that the treatment period differences are not likely to be due to random chance.

7.4 Multivariate Test Results

I next test the statistical significance of differences in *IPT* with the following OLS regression:

$$IPT = \rho + \delta + \theta_1 Treatment + \theta_2(Firm^{2C} \text{ x } Treatment)$$
$$+ \theta_3 Posttreatment + \theta_4(Firm^{2C} \text{ x } Posttreatment)$$
$$+ \sum^{10}_{k=1}\theta_k X + \varepsilon \qquad [\ 5\ ]$$

where *IPT* is as previously described; *Treatment* is an indicator variable that indicates *IPT* intervals measured following the *Newman* ruling; *Posttreatment* is an indicator variable that indicates *IPT* intervals following the *Salman* ruling; $Firm^{2C}$ is as previously described, and X is a vector of time-varying control variables that include *ln*(*Market Value*), *Book-to-Market*, *RETQ1*, and *RETQ24*, as previously described. ρ and δ denote firm and year-quarter (e.g., 2015Q1) fixed effects, respectively. I estimate Equation (3) using two-dimensional clustering by firm and year-quarter to correct for residual correlation in the error terms (Petersen 2009). The base effect of $Firm^{2C}$ is omitted due to the inclusion of firm fixed effects.

Table 11 reports the results of estimating Equation (5). The coefficient of interest is $Firm^{2C}$ *x Treatment*, which is positive and significant (p-value < 0.05) in both Columns 1 and 2, which report the estimations without and with the inclusion of control variables, respectively, indicating that pre-earnings announcement price discovery was significantly higher for Second Circuit firms relative to non-Second Circuit firms in the *Newman* treatment period. An F-test (reported at the bottom of Panel B) also indicates that the coefficient on $Firm^{2C}$ *x Treatment* is significantly greater than the coefficient on $Firm^{2C}$ *x Posttreatment* (F-stat = 4.03, p-value = 0.045).

The concern of potential serial correlation in the DiD estimation here is also relevant (Bertrand et al. 2004). To mitigate concern that the *IPT* inferences are due to structural complications in the data that produce understated standard errors, I conduct a changes analysis similar to the results provided in Table 6. Table 12 reports the multivariate changes analysis where *IPT* variables and controls are collapsed into mean values, by firm, by testing period. The results here corroborate the evidence in Table 11, with the coefficient on $Firm^{2C}$ being positive (negative) and significant in Column 1 (2), where the changes between the pretreatment and treatment (treatment and posttreatment) periods are examined.[26]

[26] Standard errors in the estimations reported in Table 12 are based on bootstrapped estimations over 1,000 iterations. Inferences are quantitatively similar if standard errors are calculated using traditional OLS regression.

In total, the results of my analyses suggest that *Newman* affected the flow of private information in the Second Circuit, which allowed Second Circuit hedge funds to engage in greater informed trading activity in Second Circuit stocks. Relatedly, the results in Table 7 indicate that improved hedge fund stock picking ability was associated with greater market efficiency. This finding is notable as the *Newman* ruling was highly publicized and some market participants expressed concern that *Newman* would allow insider trading to occur with impunity. Thus, a reasonable prediction is that *Newman* would have impaired market efficiency due to uninformed investors reducing their trading activity and/or exiting the market. However, my findings suggest that *Newman* may have produced some positive externalities in terms of improved price discovery.

8. CONCLUSION

This study exploits the empirical features of the landmark December 2014 insider trading court ruling by the U.S. Federal Circuit Court of Appeals for the Second Circuit (Second Circuit) in *US v. Newman* (hereafter *Newman*) to further our understanding of the effects of tipper-tippee insider trading restriction and enforcement in the U.S. capital market. *Newman* represented a structural change in insider trading law by restricting the type of exchanges between managers and investors that trigger *tipper-tippee* insider trading liability and provides a plausible quasi-experimental setting to examine the capital market effects of a change in insider trading restrictions that is exogenous to capital market outcomes. I find that Second Circuit hedge funds capitalized on the *Newman* ruling as evidenced by their significantly improved stock picking ability of Second Circuit stocks during an approximate two-year time period where *Newman* constrained insider trading enforcement. This finding is consistent with market participants engaging in suspect trading activity where *Newman* impaired the enforceability of traditional insider trading restrictions. I also find that Second Circuit hedge funds showed a marked improvement in their abilities to trade in advance of future earnings surprises in Second Circuit stocks, which is further consistent with *Newman* allowing these funds to receive and trade on valuable private information. Lastly, I find that this increased suspect trading activity relates to improvements in price efficiency, which is notable given the competing arguments surrounding the costs and benefits of insider trading regulation (i.e., arguments related to Manne 1966; Carlton and Fischel 1983; and Fishman and Hagerty 1992; Khanna et al. 1994).

A maintained assumption throughout my empirical analyses is that an increase in hedge fund trading abilities and price discovery in relation to *Newman* is likely to be due to insider trading. An important caveat to this inference relates to my inability to observe actual insider trading activity. However, *Newman* provides several empirical features that challenge alternative explanations to the inference of increased private information flows, mainly that the *Newman* "treatment" effect was temporary (allowing for measurable time series variation), impacted market participants differentially (allowing for measurable cross-section variation among investors and issuers), and was relatively isolated from confounding market events.

In sum, this study highlights the potentially paramount importance of tipper-tippee insider trading law for protecting the integrity of U.S. capital markets in terms of reducing private information advantages stemming from privileged access to inside information. My findings should be useful for lawmakers and policy influencers as my results point to a potential weakness that underlies current insider trading regulatory processes in the U.S.: to the extent that tipper-tippee liability lacks statutory definition and evolves through common law, judicial decisions can create differential information advantages among investors when federal courts disagree on the interpretation of tipper-tippee insider trading law.

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10. APPENDICES

10.1 APPENDIX A: Variable Definitions

Hedge Fund Trade Variables:

BAR = Hedge fund stock-picking ability on the basis of future earnings announcement returns, calculated as:

$$= 1 / I \bullet \sum^{I}_{i=1} (r_{j,q+1} - r_{DGTW,i,q+1}) - 1 / D \bullet \sum^{D}_{d=1} (r_{j,q+1} - r_{DGTW,d,q+1})$$

where I indexes stocks with portfolio-weight increases over quarter q, and D indexes stocks with portfolio-weight decreases over quarter q; $r_{j,q+1}$ is the three-day raw earnings announcement return of stock j in quarter $q+1$; and $r_{DGTW,j,q+1}$ is the value-weighted benchmark-adjusted earnings announcement return in quarter $q+1$ of stock j's *DGTW* benchmark portfolio. I measure *BAR* separately for Second Circuit portfolio firms (BAR^{2C}) and non-Second Circuit portfolio firms (BAR^{N2C}).

ueSPA = Hedge fund stock picking ability on the basis of future earnings surprises, calculated as:

$$= 1 / I \bullet \sum^{I}_{i=1} (ae_{j,q+1} - exp_{i,q+1}) - 1 / D \bullet \sum^{D}_{d=1} (ae_{j,q+1} - exp_{i,q+1})$$

where I indexes stocks with portfolio-weight increases over quarter q, and D indexes stocks with portfolio-weight decreases over quarter q; $ae_{j,q+1}$ is the actual earnings of stock j in quarter $q+1$; and $exp_{i,q+1}$ is the consensus (median) earnings per share of stock j in quarter $q+1$ as reported in the I/B/E/S summary database, scaled by stock price at the beginning of the forecast period. *ueSPA* is multiplied by 100 at the firm-quarter level for exposition. *ueSPA* calculated with respect to trades of Second Circuit (non-Second Circuit) stocks is denoted as $ueSPA^{2C}$ ($ueSPA^{N2C}$).

Hedge Fund Characteristics:

HF^{2C} = An indicator variable equal to one for hedge funds with primary business addresses listed in the U.S. Federal Second Circuit Court of Appeals, which includes the states of New York, Connecticut, and Vermont; zero otherwise. Hedge fund primary business addresses are obtained from Form ADV filings.

PortSize = The market value of a hedge fund's 13F long-equity portfolio.

PortConcentration = The inverse of the number of stocks held by a hedge fund.

PortTurnover = The sum of buys and sells divided by 0.5 times the sum of the portfolio size at quarter *q-1* and that at quarter *q*, multiplied by 2 (annualized).

CompMV = Portfolio-weighted average of the log market value of each portfolio firm.

CompBM = Portfolio-weighted average of the book-to-market ratio of each portfolio firm.

CompMOM = Portfolio-weighted average of the 12-month prior return of each portfolio firm.

CompVOL = Portfolio-weighted average of the 12-month standard deviation of daily raw returns of reach portfolio firm.

Variables for Firm-Level Price Discovery Tests:

IPT = IPT is an area-under-the curve measure that approximates market efficiency by capturing the speed at which private information is impounded into publicly available stock price, and is measured as:

$$= ½ \sum^{+M}_{m=-60} (BH_{m-1} + BH_m) / BH_M,$$

which is equivalent to:

$$= \sum^{+M-1}_{m=-60} (BH_m / BH_M) + ½$$

The measurement interval [*m*, M] is the [–60, +1] interval surrounding earnings announcement dates [0]. *BH* is the cumulative return in excess of the CRSP value-weighted index return.

$Firm^{2C}$ = An indicator variable equal to one if the state of the firm's primary business address reported in Compustat is either New York, Connecticut, or Vermont; and zero otherwise.

Firm Size = Market value of equity measured at the end of the fiscal-quarter-end date immediately preceding the earnings announcement date.

Book-to-Market = The book value of common equity scaled by the market value of common equity measured at the end of the fiscal-quarter-end date immediately preceding the earnings announcement date.

Return on Equity = Income before extraordinary items scaled by the book value of common equity measured at the end of the fiscal-quarter-end date immediately preceding the earnings announcement date.

Leverage = Total debt scaled by total assets, measured at the end of the fiscal-quarter-end date immediately preceding the earnings announcement date.

Intangibles = Total intangibles scaled by total assets, measured at the end of the fiscal-quarter-end date immediately preceding the earnings announcement date.

Returns = 12-month cumulative market-adjusted returns (in excess of the CRSP value-weighted index return), measured at the end of the month immediately preceding the earnings announcement date.

Liquidity = The natural log of –1 times 1 + Amihud illiquidity, where Amihud illiquidity is calculated as:

$= 1 / 252_{i,t} \text{ x } \sum^{252}_{d=1} | RET_{i,d} | / | VOL_{i,d} |$

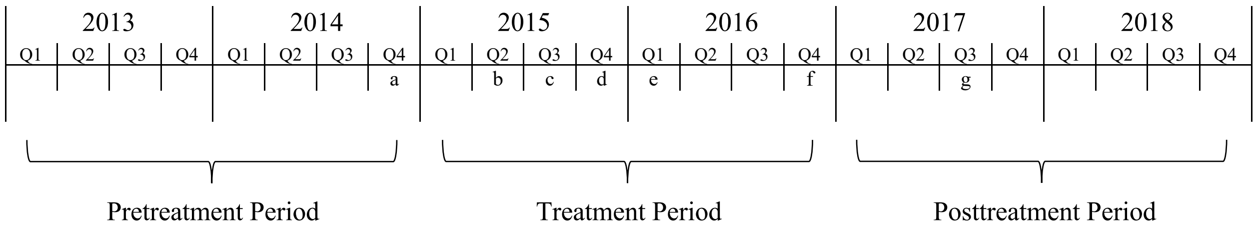
Where $RET_{i,d}$ and $VOL_{i,d}$ are, respectively, the absolute returns and dollar trading volume on day *d* for firm *i*. Amihud illiquidity is calculated using daily CRSP data over the 252 days ending on the month immediately preceding the earnings announcement date.

Analysts = The number of I/B/E/S analysts providing EPS forecasts for the firm.

Press Coverage = The total number of full Dow Jones and Wall Street Journal articles issued for the firm in the 60 days preceding the earnings announcement date from RavenPack.

Inst. Ownership = The fraction of total common shares held by institutional investors as reported in the Thomson Reuters S34 Institutional Holdings database, measured at the end of the most recent calendar quarter preceding the earnings announcement date.

10.2 APPENDIX B: Figures and Tables



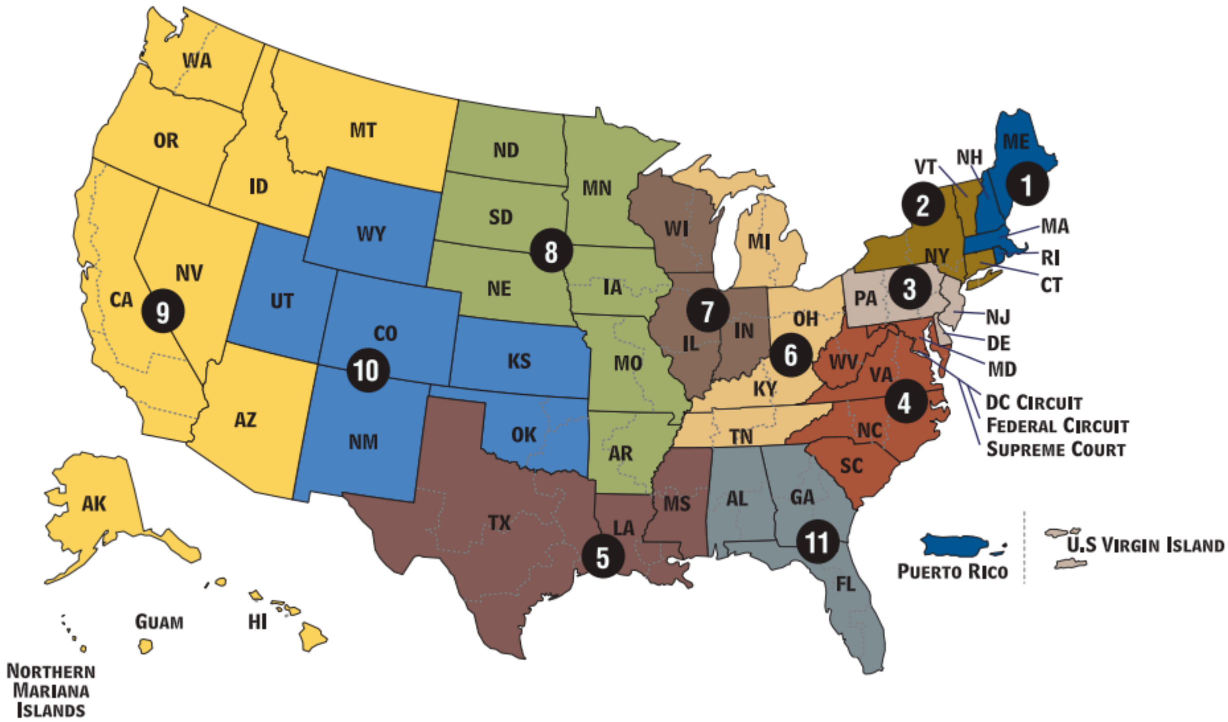
Notable Judicial Events:

a) Second Circuit issues *Newman* ruling on 10-Dec-2014
b) Second Circuit denies DOJ petition for en banc rehearing on 03-Apr-2015
c) Ninth Circuit upholds Salman tippee conviction and departs from *Newman* on 06-Jul-2015
d) Supreme Court denies granting certiorari to the *Newman* case on 02-Oct-2015
e) Supreme Court grants certiorari to the *Salman* case on 19-Jan-2016
f) Supreme Court issues *Salman* ruling on 06-Dec-2016
g) Second Circuit issues *Martoma* ruling on 06-Jun-2018

Figure 1

Table 1–Geographic Summary Statistics

Panel A: Map of U.S. Circuit Court Jurisdictional Boundaries



Source: www.uscourts.gov

Panel B: Geographic Dispersion of Sample Hedge Funds and Portfolio Firms

	Hedge Funds			Portfolio Firms		
	# of Funds	Freq.	% of Total HF Holdings	# of Firms	Freq.	% of Total PF MVE
1st Circuit	43	0.063	0.128	288	0.064	0.042
2nd Circuit	327	0.479	0.389	520	0.116	0.138
3rd Circuit	45	0.066	0.033	387	0.086	0.065
4th Circuit	21	0.031	0.061	362	0.081	0.062
5th Circuit	31	0.045	0.100	556	0.124	0.077
6th Circuit	19	0.028	0.013	299	0.067	0.054
7th Circuit	47	0.069	0.097	320	0.071	0.071
8th Circuit	21	0.031	0.053	224	0.050	0.069
9th Circuit	89	0.130	0.093	985	0.220	0.342
10th Circuit	13	0.019	0.009	234	0.052	0.021
11th Circuit	27	0.040	0.024	302	0.067	0.054
	683			4,493		

This table reports summary statistics regarding the geographic dispersion of the sample hedge funds and the public companies held by the sample hedge funds during the sample period. Panel A provides a map that details the jurisdictional boundaries of the U.S. federal circuit courts of appeals. Panel B reports the number of distinct hedge funds and public companies with corporate headquarters in each federal circuit court boundary.

Table 2–Descriptive Statistics

Panel A: Hedge Fund Descriptive Statistics

	HF^{2C} = 1		HF^{2C} = 0		Test of Difference	
	Mean	Median	Mean	Median	Mean	Median
PortSize	2,577.163	592.277	3,747.427	511.495	[0.197]	[0.247]
PortConcentration	0.031	0.024	0.019	0.012	**[<0.001]**	**[<0.001]**
PortTurnover	0.958	0.963	1.142	1.176	**[<0.001]**	**[<0.001]**
CompMV	9.406	9.549	9.627	9.926	**[0.032]**	**[0.004]**
CompBM	0.410	0.385	0.408	0.386	[0.870]	[0.821]
CompMOM	0.217	0.202	0.203	0.180	[0.109]	**[0.007]**
CompVOL	0.084	0.079	0.077	0.071	**[<0.001]**	[**[<0.001]**]
Unique Funds	327		346			
Fund Quarters	6,894		7,857			

Panel B: Hedge Fund Portfolio Concentration in FF30 Industries

	Portfolio Concentration			Portfolio Concentration	
Industry	HF^{2C} = 1	HF^{2C} = 0	Industry	HF^{2C} = 1	HF^{2C} = 0
Food Products	2.09%	1.93%	Aircraft, Ships, and Railroad Equipment	1.24%	1.15%
Beer & Liquor	1.03%	0.80%	Metals	0.78%	0.95%
Tobacco	**0.32%**	**0.60%**	Coal	0.05%	0.06%
Recreation	**1.85%**	**1.08%**	Petroleum & Natural Gas	5.32%	5.84%
Printing and Publishing	0.44%	0.28%	Utilities	2.56%	2.83%
Consumer Goods	**1.04%**	**1.53%**	Communication	**5.77%**	**4.06%**
Apparel	0.79%	0.76%	Personal & Business Services	**14.50%**	**13.13%**
Healthcare, Medical Equipment, and Pharmaceuticals	10.13%	9.91%	Business Equipment	**7.96%**	**9.08%**
Chemicals	**2.17%**	**1.70%**	Business Supplies & Shipping Containers	**0.90%**	**1.19%**
Textiles	**0.17%**	**0.08%**	Transportation	3.51%	3.66%
Construction & Construction Materials	**2.25%**	**1.74%**	Wholesale	1.84%	2.13%
Steel Works	0.38%	0.32%	Retail	6.19%	5.68%
Fabricated Products & Machinery	**1.67%**	**1.99%**	Hospitality	**1.93%**	**1.51%**
Electrical Equipment	0.45%	0.46%	Financial	**19.00%**	**21.40%**
Automobiles & Trucks	1.31%	1.43%	Miscellaneous	2.36%	2.72%

This table presents descriptive statistics for the sample hedge funds. Panel A reports the mean and median values of the above variables for Second Circuit hedge funds (HF^{2C} = 1) and non-Second Circuit hedge funds (HF^{2C} = 0). Panel B reports hedge funds' average portfolio concentration in the Fama-French 30 industry classifications. Bolded values in Panels A and B indicate significant differences between the two groups of hedge funds (p-value < 10%), based on t-tests for means and Wilcoxon rank-sum tests for medians. Variable definitions are provided in Appendix A.

TABLE 3–Univariate *BAR* Analysis

Panel A: Stock Picking Ability of Second Circuit (2C) Hedge Funds

		(1)		(2)		(3)		(4)		(5)	
		Equal-weighted Earnings Announcement Returns to Buy and Sale Trades						Differences			
		Pretreatment		Treatment		Posttreatment		Treat–Pre		Post–Treat	
		Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	0.05	(0.77)	0.15**	(2.08)	0.04	(0.44)	0.10	(0.95)	–0.11	(–0.94)
2	$Sales^{2C}$	0.38***	(5.38)	–0.14*	(–1.79)	0.30***	(3.28)	–0.52***	(–4.93)	0.44***	(3.66)
3	BAR^{2C}	–0.32***	(–3.26)	0.29***	(2.72)	–0.26**	(–2.00)	0.61***	(4.21)	–0.55***	(–3.28)
	N2C Stocks										
4	$Buys^{N2C}$	0.20***	(5.58)	0.19***	(4.75)	0.00	(0.06)	–0.01	(–0.16)	–0.19***	(–3.26)
5	$Sales^{N2C}$	0.09**	(2.54)	0.16***	(4.07)	–0.09**	(–2.14)	0.07	(1.32)	–0.25***	(–4.36)
6	BAR^{N2C}	0.11**	(2.21)	0.03	(0.53)	0.09	(1.55)	–0.08	(–1.07)	0.06	(0.77)
	2C – N2C										
7	*Buys*	–0.15*	(–1.87)	–0.04	(–0.53)	0.04	(0.38)	0.10	(0.91)	0.08	(0.63)
8	*Sales*	0.29***	(3.62)	–0.30***	(–3.45)	0.39***	(3.87)	–0.59***	(–4.98)	0.69***	(5.20)
9	*BAR*	–0.43***	(–3.91)	0.26**	(2.16)	–0.35**	(–2.46)	0.69***	(4.24)	–0.61***	(–3.29)

Panel B: Stock Picking Ability of Non-Second Circuit (N2C) Hedge Funds

		Pretreatment Mean	(*t*-stat)	Treatment Mean	(*t*-stat)	Posttreatment Mean	(*t*-stat)	Treat–Pre Mean	(*t*-stat)	Post–Treat Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	–0.02	(–0.36)	–0.11*	(–1.92)	–0.04	(–0.61)	–0.09	(–1.12)	0.07	(0.81)
2	$Sales^{2C}$	0.06	(1.09)	–0.02	(–0.41)	0.08	(1.16)	–0.08	(–1.03)	0.10	(1.14)
3	BAR^{2C}	–0.08	(–1.01)	–0.09	(–1.04)	–0.12	(–1.31)	–0.01	(–0.05)	–0.03	(–0.25)
	N2C Stocks										
4	$Buys^{N2C}$	0.08***	(2.84)	0.08***	(2.75)	–0.02	(–0.51)	0.00	(0.01)	–0.09**	(–2.23)
5	$Sales^{N2C}$	0.03	(1.28)	0.10***	(3.38)	–0.07**	(–2.20)	0.07*	(1.68)	–0.17***	(–3.92)
6	BAR^{N2C}	0.04	(1.14)	–0.02	(–0.57)	0.05	(1.23)	–0.07	(–1.20)	0.08	(1.29)
	2C – N2C										
7	*Buys*	–0.10	(–1.54)	–0.19***	(–2.92)	–0.02	(–0.33)	–0.09	(–1.01)	0.16*	(1.69)
8	*Sales*	0.03	(0.44)	–0.12*	(–1.85)	0.15**	(1.99)	–0.15*	(–1.66)	0.27***	(2.72)
9	*BAR*	–0.12	(–1.41)	–0.06	(–0.69)	–0.17*	(–1.71)	0.06	(0.47)	–0.11	(–0.79)

(Continued on next page)

Table 3 (Continued)

Panel C: 2C Hedge Fund *BAR* – N2C Hedge Fund *BAR*

		(1)		(2)		(3)		(4)		(5)	
		Equal-weighted Earnings Announcement Returns to Buy and Sale Trades						Differences			
		Pretreatment		Treatment		Posttreatment		Treat–Pre		Post–Treat	
		Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	0.07	(0.83)	0.26***	(2.86)	0.08	(0.73)	0.19	(1.46)	–0.18	(–1.26)
2	$Sales^{2C}$	0.32***	(3.61)	–0.11	(–1.18)	0.22**	(2.00)	–0.43***	(–3.29)	0.34**	(2.29)
3	BAR^{2C}	–0.24*	(–1.94)	0.37***	(2.81)	–0.14	(–0.92)	0.62***	(3.38)	–0.52**	(–2.54)
	N2C Stocks										
4	$Buys^{N2C}$	0.12***	(2.82)	0.12**	(2.41)	0.02	(0.36)	–0.01	(–0.14)	–0.10	(–1.39)
5	$Sales^{N2C}$	0.06	(1.34)	0.06	(1.31)	–0.02	(–0.40)	0.01	(0.08)	–0.08	(–1.19)
6	BAR^{N2C}	0.07	(1.08)	0.05	(0.77)	0.04	(0.54)	–0.01	(–0.16)	–0.01	(–0.13)
	2C – N2C										
7	*Buys*	–0.05	(–0.51)	0.14	(1.40)	0.06	(0.51)	0.19	(1.36)	–0.08	(–0.51)
8	*Sales*	0.26***	(2.63)	–0.18*	(–1.65)	0.24**	(1.98)	–0.44***	(–2.98)	0.42***	(2.58)
9	*BAR*	–0.31**	(–2.22)	0.32**	(2.16)	–0.18	(–1.06)	0.63***	(3.09)	–0.50**	(–2.23)

This table reports univariate tests of hedge fund *BAR* over the sample period. *BAR* measured with respect to trades of Second Circuit stocks (non-Second Circuit stocks) is denoted as BAR^{2C} (BAR^{N2C}). Panel A reports *BAR* for Second Circuit hedge funds, including the component returns derived from buy and sale trades (*Buys* and *Sales*, respectively). Columns 1, 2, and 3 report *BAR* and the component return measures for the pretreatment, treatment, and posttreatment periods, respectively. Tests between the treatment and control periods are reported in Columns 4 and 5. Panel B reports the *BAR* statistics for non-Second Circuit hedge funds. Panel C reports tests of differences between Panels A and B. Variable definitions are provided in Appendix A.

***, **, and * indicate significance at the 1%, 5%, and 10% levels. Returns are expressed as a percent, and winsorized at 1% and 99%.

Table 4–Multivariate *BAR* Analysis

		(1)	(2)	(3)	(4)
		DV = BAR^{2C}		DV = BAR^{N2C}	
		Coef. (*t*-stat.)	Coef. (*t*-stat.)	Coef. (*t*-stat.)	Coef. (*t*-stat.)
Treatment	β_1	–0.899*** (–3.34)	–0.859*** (–3.06)	–0.135 (–1.08)	–0.141 (–1.09)
HF^{2C} x Treatment	$\boldsymbol{\beta_2}$	**0.640*** (3.46)**	**0.657*** (3.55)**	**–0.003 (–0.03)**	**–0.021 (–0.22)**
Posttreatment	β_3	–0.866*** (–2.59)	–0.810** (–2.33)	–0.350** (–2.57)	–0.410*** (–2.83)
HF^{2C} x Posttreatment	β_4	0.084 (0.41)	0.106 (0.52)	–0.014 (–0.14)	–0.030 (–0.31)
ln(PortSize)			0.181 (1.56)		–0.051 (–0.90)
ln(PortConcentration)			–0.043 (–0.26)		0.063 (0.76)
PortTurnover			–0.048 (–0.41)		–0.046 (–0.74)
CompMV			–0.000 (–0.00)		0.089 (1.01)
CompBM			1.082 (1.38)		0.049 (0.13)
CompMOM			0.393 (0.87)		0.095 (0.40)
CompVOL			0.000 (0.30)		–0.000 (–1.03)
Observations		14,751	14,751	14,751	14,751
R^2		6.04%	6.11%	5.92%	6.00%
Fixed Effects		Fund, YQ	Fund, YQ	Fund, YQ	Fund, YQ
SE Clustering		Fund x YQ	Fund x YQ	Fund x YQ	Fund x YQ
F-test: $\beta_2 = \beta_4$		**7.32*** [0.007]**	**7.20*** [0.007]**	0.01 [0.913]	0.01 [0.0.927]
Cross-Equation Test: β_2		Column 1 = Column 3		Column 2 = Column 4	
χ^2 Statistic [*p*-value]		**10.29*** [0.001]**		**11.13*** [<0.001]**	

This table reports the estimations of Equation (1). The sample period includes hedge fund quarters spanning 2013-2018. BAR^{2C} (BAR^{N2C}) is hedge fund stock-picking ability based on trades of Second Circuit (non-Second Circuit) portfolio firms. HF^{2C} is an indicator variable equal to one for hedge funds based in the Second Circuit; and zero otherwise. *Treatment* is an indicator variable equal to one for fund-quarters spanning the 2015-2016 period, in conjunction with the effect of the *Newman* ruling; zero otherwise. *Posttreatment* is an indicator variable equal to one for fund-quarters spanning the 2017-2018 period, in conjunction with the *Salman* ruling; zero otherwise. The variable of interest is the interaction of HF^{2C} and *Treatment*. The estimations include year-quarter fixed effects (e.g., 2015Q1) and hedge fund fixed effects. Standard errors are two-way clustered by fund and year-quarter. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5–Test of Parallel Trends Assumption

		DV = BAR^{2C}	
	Pred.	Coef.	(*t*-stat.)
2014Q1		–0.408*	(–1.90)
2014Q2		–0.704***	(–2.82)
2014Q3		0.058	(0.25)
2014Q4		0.098	(0.42)
2015Q1		–0.719***	(–3.05)
2015Q2		–0.032	(–0.13)
2015Q3		0.031	(0.13)
2015Q4		0.230	(0.91)
2016Q1		–0.535**	(–2.45)
2016Q2		0.284	(1.30)
2016Q3		–0.115	(–0.57)
2016Q4		–0.530**	(–2.45)
HF^{2C} x 2014Q1		–0.197	(–1.17)
HF^{2C} x 2014Q2		0.281	(1.66)
HF^{2C} x 2014Q3		–0.236	(–1.29)
HF^{2C} x 2014Q4		–0.175	(–1.14)
HF^{2C} x 2015Q1	+	**1.045*** **	**(6.36)**
HF^{2C} x 2015Q2	+	**0.988*** **	**(5.00)**
HF^{2C} x 2015Q3	+	**0.570*** **	**(3.00)**
HF^{2C} x 2015Q4	+	**0.958*** **	**(4.38)**
HF^{2C} x 2016Q1	+	**0.366** **	**(2.13)**
HF^{2C} x 2016Q2	+	**0.500*** **	**(3.14)**
HF^{2C} x 2016Q3	+	–0.422**	(–2.62)
HF^{2C} x 2016Q4	+	0.155	(0.80)
ln(PortSize)		0.001	(0.00)
ln(PortConcentration)		0.069	(0.44)
PortTurnover		0.103	(0.69)
CompMV		0.012	(0.10)
CompBM		0.851	(1.10)
CompMOM		0.511	(1.05)
CompVOL		0.001	(0.92)
Observations		13,904	
R^2		6.67%	
Fixed Effects		Fund, YQ	
SE Clustering		Fund x YQ	

This table reports the re-estimation Equation (2) to examine whether treated and nontreated hedge funds exhibit a parallel trend in the year preceding the *Newman* ruling (2014) in their stock picking ability of Second Circuit stocks (BAR^{2C}). The estimation includes an extended pretreatment period and covers trading quarters spanning 2011-2016. Equation (2) is adjusted to include year-quarter indicators for the 12 quarters spanning the 2014-2016 period which are individually specified and interacted with the Second Circuit hedge fund dummy (HF^{2C}). The estimation includes year-quarter and hedge fund fixed effects, with the year-quarter dummies for 2011-2013 omitted for brevity. Standard errors are two-way clustered by fund and year-quarter. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A.
***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 6–Changes Analysis across Testing Periods

	(1)	(2)	(3)	(4)
DV =	ΔBAR^{2C}	ΔBAR^{N2C}	ΔBAR^{2C}	ΔBAR^{N2C}
	Pretreatment → Treatment		Treatment → Posttreatment	
	Coef. [*p*-val.]	Coef. [*p*-val.]	Coef. [*p*-val.]	Coef. [*p*-val.]
HF^{2C}	**0.931*****	**–0.052**	**–0.663*****	**–0.002**
	[0.000]	**[0.663]**	**[0.007]**	**[0.990]**
Δ*PortSize*	0.138	0.031	0.716**	–0.332*
	[0.621]	[0.849]	[0.032]	[0.093]
Δ*PortConcentration*	–0.478	0.218	0.952*	0.167
	[0.278]	[0.333]	[0.071]	[0.643]
Δ*PortTurnover*	–0.463	–0.293	0.921	0.171
	[0.495]	[0.357]	[0.155]	[0.638]
Δ*CompMV*	0.301	0.296	0.275	0.053
	[0.398]	[0.138]	[0.524]	[0.824]
Δ*CompBM*	–0.117	0.079	1.435	–0.390
	[0.949]	[0.928]	[0.543]	[0.709]
Δ*CompMOM*	–1.094	0.864	1.639	0.605
	[0.551]	[0.167]	[0.350]	[0.522]
Δ*CompVOL*	0.003	0.000	0.002	0.001
	[0.128]	[0.702]	[0.287]	[0.337]
Intercept	–0.286	0.070	–0.279	0.055
	[0.418]	[0.580]	[0.243]	[0.688]
Observations	683	683	683	683
R^2	3.53%	2.15%	4.37%	2.99%
Bootstrapped S.E.	Yes	Yes	Yes	Yes

This table reports a changes analysis where all of the regressors in Equation (2) are collapsed into pretreatment, treatment, and posttreatment mean values for each of the sample hedge funds. Change in stock picking ability (Δ*BAR*) is then regressed on the indicator variable for Second Circuit hedge funds (HF^{2C}) and the remaining variables included in Equation (2) that are likewise transformed into fund-level mean values by testing period. Columns 1 and 2 (3 and 4) report the change in stock picking ability of Second Circuit and non-Second Circuit stocks, respectively, between the pretreatment and treatment (treatment and posttreatment) periods. To correct for potentially understated standard errors, *p*-values (reported in brackets) are calculated based on bootstrapped standard errors, based on 1,000 iterations for each estimation. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 7–Stock Picking Ability based on Future Earnings News

Panel A: HF^{2C} *ueSPA*

		(1)	(2)	(3)	(4)	(5)
		EW-Future Earnings Surprises			Differences	
		Pretreatment	Treatment	Posttreatment	Treat–Pre	Post–Treat
Row		Mean	Mean	Mean	Mean	Mean
	2C Stocks:					
1	$ueBuys^{2C}$	0.10***	0.13***	–0.01	0.02	–0.14***
2	$ueSales^{2C}$	0.14***	0.03**	–0.02	–0.12***	–0.05**
3	$ueSPA^{2C}$	–0.04	0.10***	0.01	0.14***	–0.09***
	N2C Stocks					
4	$ueBuys^{N2C}$	0.01*	0.01**	0.04***	0.00	0.02**
5	$ueSales^{N2C}$	0.01**	0.00	0.04***	–0.01	0.04***
6	$ueSPA^{N2C}$	–0.00	0.01	–0.00	0.01	–0.02
	2C–N2C					
7	*ueBuys*	0.09***	0.11***	–0.05***	0.02	–0.17***
8	*ueSales*	0.13***	0.02*	–0.07***	–0.11***	–0.09***
9	*ueSPA*	–0.04	0.09***	0.02	0.13***	–0.07**

Panel B: HF^{N2C} *ueSPA*

Row		Pretreatment	Treatment	Posttreatment	Treat–Pre	Post–Treat
	2C Stocks:					
1	$ueBuys^{2C}$	0.06***	0.09***	0.04***	0.02	0.05***
2	$ueSales^{2C}$	0.10***	0.04***	0.02*	–0.06***	0.02
3	$ueSPA^{2C}$	–0.04***	0.05***	0.02	0.09***	0.03
	N2C Stocks					
4	$ueBuys^{N2C}$	0.03***	0.03***	0.04***	–0.00	–0.01*
5	$ueSales^{N2C}$	0.02***	0.03***	0.05***	0.00	–0.02***
6	$ueSPA^{N2C}$	0.01**	0.01	–0.00	–0.00	0.01
	2C–N2C					
7	*ueBuys*	0.03***	0.05***	–0.01	0.02	0.06***
8	*ueSales*	0.08***	0.01	–0.03**	–0.07***	0.04**
9	*ueSPA*	–0.05***	0.04***	0.02	0.09***	0.02

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Table 7 (Continued)

Panel C: HF^{2C} *ueSPA* – HF^{N2C} *ueSPA*

		(1)	(2)	(3)	(4)	(5)
		EW-Future Earnings Surprises			Differences	
		Pretreatment	Treatment	Posttreatment	Treat–Pre	Post–Treat
Row		Mean	Mean	Mean	Mean	Mean
	2C Stocks:					
1	$ueBuys^{2C}$	0.04**	0.04**	–0.05***	–0.00	–0.09***
2	$ueSales^{2C}$	0.04**	–0.01	–0.04**	–0.05**	–0.03
3	$ueSPA^{2C}$	0.00	0.05**	–0.01	0.05	–0.06*
	N2C Stocks					
4	$ueBuys^{N2C}$	–0.02***	–0.02**	–0.01	0.00	0.01
5	$ueSales^{N2C}$	–0.01	–0.03***	–0.00	–0.02	0.02*
6	$ueSPA^{N2C}$	–0.01	0.01	–0.00	0.02	–0.01
	2C–N2C					
7	*ueBuys*	0.06***	0.06***	–0.05**	–0.00	–0.10***
8	*ueSales*	0.05**	0.01	–0.04*	–0.04	–0.05**
9	*ueSPA*	0.01	0.05*	–0.00	0.03	–0.05

This table reports univariate tests of an alternative measure of hedge fund stock picking ability based on the ability of hedge funds to trade in advance of surprise earnings (*ueSPA*). Panel A reports *ueSPA*, measured with respect to trades of Second Circuit ($ueSPA^{2C}$) and non-Second Circuit ($ueSPA^{N2C}$) stocks, for Second Circuit hedge funds; Panel B reports the same information for non-Second Circuit hedge funds. Panel C reports the differences between Second Circuit hedge fund *ueSPA* and non-Second Circuit hedge fund *ueSPA*. Variable definitions are provided in Appendix A.

***, **, and * indicate significance at the 1%, 5%, and 10% levels. Unexpected earnings are multiplied by 100 and winsorized at 1% and 99%.

Table 8–Multivariate Analysis of Earning Surprise Stock Picking Ability

		(1)	(2)
		DV = $ueSPA^{2C}$	DV = $ueSPA^{N2C}$
		Coef. (*t*-stat.)	Coef. (*t*-stat.)
Treatment	β_1	–0.047	0.003
		(–0.91)	(0.11)
HF^{2C} x *Treatment*	$\boldsymbol{\beta_2}$	**0.055†**	**0.008**
		(1.50)	**(0.48)**
Posttreatment	β_3	0.033	–0.006
		(0.55)	(–0.19)
HF^{2C} x *Posttreatment*	β_4	–0.022	0.000
		(–0.56)	(0.03)
Controls		Included	Included
Observations		14,074	14,074
R^2		8.89%	7.84%
Fixed Effects		Fund, YQ	Fund, YQ
SE Clustering		Fund x YQ	Fund x YQ
F-test: $\beta_2 = \beta_4$		**4.34****	0.18
		[0.037]	[0.673]
Cross-Equation Test: β_2		Column 1 = Column 2	
χ^2 Statistic [*p*-value]		1.55 [0.107]	

This table reports the estimations of Equation (1) where the outcome variable is either $ueSPA^{2C}$ or $ueSPA^{N2C}$. The sample period includes hedge fund quarters spanning 2013-2018. HF^{2C} is an indicator variable equal to one for hedge funds based in the Second Circuit; and zero otherwise. *Treatment* is an indicator variable equal to one for fund-quarters spanning the 2015-2016 period, in conjunction with the effect of the *Newman* ruling; zero otherwise. *Posttreatment* is an indicator variable equal to one for fund-quarters spanning the 2017-2018 period, in conjunction with the *Salman* ruling; zero otherwise. The variable of interest is the interaction of HF^{2C} and *Treatment*. The estimations include year-quarter fixed effects (e.g., 2015Q1) and hedge fund fixed effects. Standard errors are two-way clustered by fund and year-quarter. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. † indicates significance at the 10% level using a one-sided test when there is a signed prediction and the sign of the coefficient is consistent with the prediction.

Table 9–Sample Development and Descriptive Statistics

Panel A: Sample Development

	Firm Quarters	Unique 2C Firms	Unique N2C Firms
Intersection of Compustat, CRSP, I/B/E/S, Thomson Reuters S34, and RavenPack databases	99,878	612	5,628
Less: Extreme *IPT* observations (top and bottom 0.5%)	(999)	(0)	(3)
Less: Firms without variable coverage in the pretreatment, treatment, and posttreatment periods	(16,930)	(211)	(2,018)
Final sample	**81,949**	**401**	**3,607**

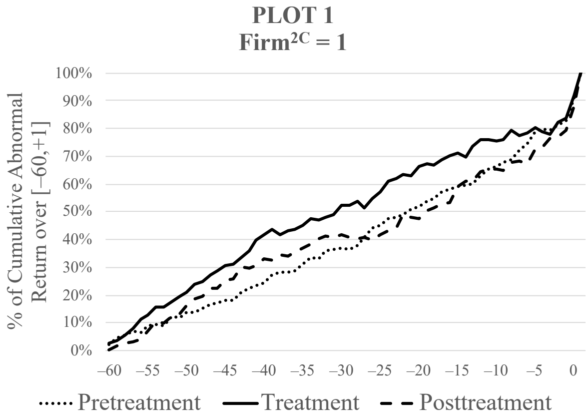
Panel B: Descriptive Statistics

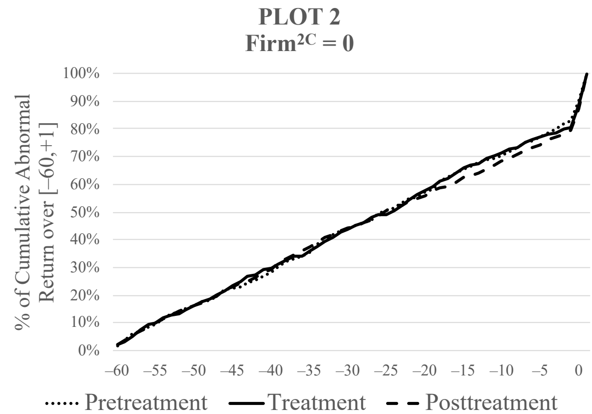
	$Firm^{2C} = 1$		$Firm^{2C} = 0$		Test of Difference	
	Mean	Median	Mean	Median	Mean	Median
IPT	28.136	28.339	28.096	28.267	[0.969]	[0.868]
Firm Size	7,763.514	747.121	6,687.098	940.246	**[<0.001]**	**[<0.001]**
Book-to-Market	0.615	0.519	0.587	0.470	**[<0.001]**	**[<0.001]**
Return on Equity	0.010	0.020	0.000	0.018	**[<0.001]**	**[<0.001]**
Leverage	0.216	0.165	0.203	0.144	**[<0.001]**	**[<0.001]**
Intangibles	0.166	0.051	0.152	0.047	**[<0.001]**	**[0.028]**
Returns	–0.004	–0.030	–0.005	–0.036	[0.758]	**[0.004]**
Liquidity	–0.878	–0.006	–0.689	–0.007	**[<0.001]**	**[0.014]**
Analysts	5.461	3.000	5.829	3.000	**[<0.001]**	**[<0.001]**
Press Coverage	857.689	75.000	395.724	60.000	**[<0.001]**	**[<0.001]**
Inst. Ownership	0.483	0.526	0.490	0.552	**[0.063]**	**[0.020]**
Fund Quarters	8,373		73,576			

This table reports the sample development process and descriptive statistics for the price discovery tests conducted at the firm-level. Panel A describes the sample development, and Panel B reports comparative summary statistics for the sample firms. *Firm2C* is an indicator variable equal to one for firms with primary business addresses in the Second Circuit (New York, Connecticut, or Vermont) as reported in Compustat; and zero otherwise. Bolded values in Panel B indicate significant differences between the two groups of firms (*p*-value < 10%), based on *t*-tests for means and Wilcoxon rank-sum tests for medians. Variable definitions are provided in Appendix A.

Table 10–Univariate *IPT* Analysis

Panel A: IPT Curves





Panel B: Univariate Tests of Differences

	$Firm^{2C} = 1$	$Firm^{2C} = 0$	Test of Differences	
Period:	Mean IPT	Mean IPT	*t*-test	PA
Pretreatment	25.75	28.25	–2.50 [0.177]	**–2.50*** **[0.081]**
Treatment	31.99	28.23	**3.76**** **[0.035]**	**3.76**** **[0.020]**
Posttreatment	26.48	27.80	–1.32 [0.466]	–1.32 [0.229]

Difference:	*t*-test	PA	*t*-test	PA
Treat – Pre	**6.24**** **[0.012]**	**6.24***** **[0.009]**	–0.02 [0.977]	–0.02 [0.513]
Postt – Treat	**–5.51**** **[0.025]**	**–5.51**** **[0.017]**	–0.43 [0.599]	–0.43 [0.300]

This table reports univariate analyses of the speed of price discovery between quarterly earnings cycles based on measures of intraperiod timeliness (*IPT*). Panel A reports the cumulative *IPT* curves for Second Circuit firms (Plot 1) and non-Second Circuit firms (Plot 2) for each testing period, where the *IPT* curves plot for each day *m* the cumulative abnormal return realization through day *m* scaled by the total 62-day cumulative abnormal return measured over the [–60,+1] window with respect to earnings announcement dates. Panel B reports mean *IPT* values for the sample firms in the pretreatment, treatment, and posttreatment periods. Univariate tests of differences are based on *t*-tests, as well as permutation analyses (PA) that randomly shuffle treatment assignment along the two dimensions of treatment in time (see bottom rows), and treatment by firm (see two far-right columns). Variable definitions are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 11–Multivariate *IPT* Analysis

		(1)		(2)	
		DV = IPT			
		Coef.	(*t*-stat.)	Coef.	(*t*-stat.)
Treatment	θ_1	0.332	(0.05)	0.360	(0.06)
Firm^{2C} x *Treatment*	$\boldsymbol{\theta_2}$	**6.362****	**(2.42)**	**6.470****	**(2.46)**
Posttreatment	θ_3	3.607	(0.38)	3.285	(0.35)
Firm^{2C} x *Posttreatment*	θ_4	1.046	(0.40)	1.115	(0.43)
ln(Firm Size)				–2.153***	(–2.58)
Book-to-Market				–0.247	(–0.21)
Return on Equity				–0.545	(–0.33)
Leverage				–4.312	(–1.14)
Intangibles				2.595	(0.50)
Returns				1.700*	(1.65)
Liquidity				–0.084	(–0.51)
Analysts				–0.125	(–0.68)
ln(Press Coverage)				1.078**	(2.32)
Inst. Ownership				–0.323	(–0.10)
Controls Included		No		Yes	
Observations		81,949		81,949	
R^2		4.97%		5.00%	
Fixed Effects		Firm, YQ		Firm, YQ	
SE Clustering		Firm x YQ		Firm x YQ	
F-test: $\theta_2 = \theta_4$		**4.24****		**4.43****	
		[0.040]		**[0.039]**	

This table reports the estimations of Equation (5). The sample period includes firm quarters spanning 2013-2018. *Firm^{2C}* is an indicator variable equal to one for firms based in the Second Circuit; and zero otherwise. *Treatment* is an indicator variable equal to one for firm-quarters corresponding to earnings announcements reported in 2015-2016 period; zero otherwise. *Posttreatment* is an indicator variable equal to one for firm-quarters corresponding to earnings announcements reported in the 2017-2018 period; zero otherwise. The variable of interest is the interaction of *Firm^{2C}* and *Treatment*. The estimations include year-quarter fixed effects (e.g., 2015Q1) and firm fixed effects. Standard errors are two-way clustered by firm and year-quarter. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A.
***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 12–*IPT* Changes Analysis

	(1)		(2)	
	DV = ΔIPT			
	Pretreatment → Treatment		Treatment → Posttreatment	
	Coef.	[*p*-val]	Coef.	[*p*-val]
$Firm^{2C}$	**6.827*** **	**[0.008]**	**–5.982** **	**[0.029]**
ΔFirm Size	–1.038	[0.543]	–2.235	[0.209]
ΔBook-to-Market	3.813	[0.111]	–2.012	[0.450]
ΔReturn on Equity	1.446	[0.817]	3.356	[0.520]
ΔLeverage	–5.129	[0.554]	–3.209	[0.691]
ΔIntangibles	14.364	[0.180]	–1.972	[0.852]
ΔReturns	0.279	[0.904]	–2.373	[0.366]
ΔLiquidity	0.193	[0.491]	–0.322	[0.525]
ΔAnalysts	0.242	[0.575]	0.002	[0.997]
ΔPress Coverage	0.021	[0.982]	–0.703	[0.558]
ΔInst. Ownership	3.528	[0.579]	0.121	[0.985]
Observations	4,008		4,008	
R^2	0.34%		0.27%	
Bootstrapped SE	Yes		Yes	

This table reports a changes analysis where all of the regressors in Equation (5) are collapsed into pretreatment, treatment, and posttreatment mean values for each sample firm. Change in the speed of price discovery (Δ*IPT*) is then regressed on the indicator variable for Second Circuit firms (*$Firm^{2C}$*) and the remaining variables included in Equation (5) that are likewise transformed into changes variables based on firm-level mean values by testing period. To correct for potentially understated standard errors, *p*-values (reported in brackets) are calculated based on bootstrapped standard errors, based on 1,000 iterations for each estimation. Lines related to variables of interest are bolded and highlighted. Variable definitions are provided in Appendix A.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

10.3 APPENDIX C: Alternative *BAR* Measurement

In the main test, I follow Baker et al. (2010) in computing *BAR* for each hedge fund quarter in the sample period. Baker et al. (2010) present the majority of their analyses with *BAR* calculated on an equal-weighted basis at the fund level, and Bhojraj et al. (2012) similarly compute *BAR* on an equal-weighted basis in their examination of mutual fund trading following the passage of Regulation Fair Disclosure. To allow greater comparison with these two studies, the *BAR* analyses in my main analysis were similarly based on an equal-weighting approach. However, *BAR* measured in the hedge fund context may differ from the mutual fund setting (see Griffin and Xu 2009). Thus, measuring *BAR* via a value-weighting approach may produce different inferences to the extent that value-weighting *BAR* better captures private information on a trade-by-trade basis. Accordingly, I measure *BAR* in a value-weighted fashion as follows:

$$BAR = \sum w_{j,q} > w_{j,q-1} \left([w_{j,q} - w_{j,q-1}] \bullet 2\right)(r_{j,q+1} - r_{DGTW,j,q+1})$$

$$- \sum w_{j,q} < w_{j,q-1} \left([w_{j,q-1} - w_{j,q}] \bullet 2\right)(r_{j,q+1} - r_{DGTW,j,q+1}),$$

where $w_{j,q}$ is the weight of stock *j* in a hedge fund's equity portfolio at the end of quarter *q*; $w_{j,q-1}$ is the weight of stock *j* in a hedge fund's equity portfolio at the end of quarter *q-1*; $r_{j,q+1}$ is the three-day raw earnings announcement return of stock *j* in quarter *q+1*; and $r_{DGTW,j,q+1}$ is the value-weighted benchmark-adjusted earnings announcement return in quarter *q+1* of stock *j*'s *DGTW* benchmark portfolio, where portfolios are formed following Daniel et al. (1997).

The univariate tests of hedge fund value-weighted *BAR* are reported in the below table, and is arranged similar to Table 3. As evidenced in this analysis, the inference of Second Circuit hedge funds experiencing a significant improvement in their trades of Second Circuit stocks over the *Newman* treatment period is similar here, as is the general trading patterns of both groups of hedge funds in their trades of non-Second Circuit stocks.

Table AC1–Univariate Tests of *BAR* based on Value-weighted Measurement

Panel A: Stock Picking Ability of Second Circuit (2C) Hedge Funds

		(1)		(2)		(3)		(4)		(5)	
		Value-weighted Earnings Announcement Returns to Buy and Sale Trades						Differences			
		Pretreatment		Treatment		Posttreatment		Treat–Pre		Post–Treat	
		Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	0.05	(0.90)	0.15***	(2.74)	0.03	(0.54)	0.10	(1.36)	–0.12	(–1.40)
2	$Sales^{2C}$	0.20***	(3.91)	–0.10*	(–1.71)	0.18***	(2.77)	–0.30***	(–3.88)	0.28***	(3.23)
3	BAR^{2C}	–0.15**	(–2.05)	**0.25*** **	**(3.15)**	–0.15*	(–1.67)	**0.40*** **	**(3.69)**	**–0.40*** **	**(–3.33)**
	N2C Stocks										
4	$Buys^{N2C}$	0.16***	(0.90)	0.16***	(0.90)	0.01	(0.90)	0.01	(0.17)	–0.15***	(–3.22)
5	$Sales^{N2C}$	0.07**	(4.61)	0.15***	(1.15)	–0.04	(–2.05)	0.08*	(1.74)	–0.19***	(–3.91)
6	BAR^{N2C}	0.09**	(2.05)	0.02	(0.40)	0.05	(1.02)	–0.07	(–1.10)	0.03	(0.49)
	2C – N2C										
7	Buys	–0.11*	(–1.80)	–0.01	(–0.20)	0.02	(0.33)	0.10	(1.09)	0.04	(0.38)
8	Sales	0.13**	(2.19)	–0.24***	(–3.73)	0.23***	(2.99)	–0.37***	(–4.23)	0.47***	(4.71)
9	BAR	–0.24***	(–2.79)	0.23**	(2.52)	–0.20*	(–1.95)	**0.47*** **	**(3.75)**	**–0.43*** **	**(–3.14)**

Panel B: Stock Picking Ability of Non-Second Circuit (N2C) Hedge Funds

		Pretreatment Mean	(*t*-stat)	Treatment Mean	(*t*-stat)	Posttreatment Mean	(*t*-stat)	Treat–Pre Mean	(*t*-stat)	Post–Treat Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	0.01	(0.21)	–0.09**	(–2.11)	–0.04	(–0.83)	–0.10*	(–1.65)	0.05	(0.84)
2	$Sales^{2C}$	0.02	(0.46)	–0.02	(–0.48)	0.04	(0.88)	–0.04	(–0.66)	0.06	(0.97)
3	BAR^{2C}	–0.01	(–0.19)	–0.07	(–1.11)	–0.08	(–1.24)	–0.06	(–0.66)	–0.01	(–0.12)
	N2C Stocks										
4	$Buys^{N2C}$	0.06***	(2.83)	0.07***	(3.02)	–0.01	(–0.57)	0.01	(0.29)	–0.08**	(–2.51)
5	$Sales^{N2C}$	0.04*	(1.92)	0.05**	(2.19)	–0.02	(–0.73)	0.01	(0.30)	–0.07**	(–2.05)
6	BAR^{N2C}	0.02	(0.57)	0.02	(0.52)	0.00	(0.14)	–0.00	(–0.02)	–0.01	(–0.26)
	2C – N2C										
7	Buys	–0.05	(–1.13)	–0.16***	(–3.31)	–0.02	(–0.45)	–0.10	(–1.59)	0.13*	(1.95)
8	Sales	–0.02	(–0.49)	–0.07	(–1.48)	0.06	(1.13)	–0.05	(–0.73)	0.13*	(1.84)
9	BAR	–0.03	(–0.43)	–0.08	(–1.22)	–0.08	(–1.15)	–0.05	(–0.57)	0.00	(0.03)

(*Continued on next page*)

Table AC1 (Continued)

Panel C: 2C Hedge Fund *BAR* – N2C Hedge Fund *BAR*

		(1)		(2)		(3)		(4)		(5)	
		Value-weighted Earnings Announcement Returns to Buy and Sale Trades						Differences			
		Pretreatment		Treatment		Posttreatment		Treat–Pre		Post–Treat	
		Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)	Mean	(*t*-stat)
	2C Stocks										
1	$Buys^{2C}$	0.04	(0.59)	0.24***	(3.51)	0.07	(0.93)	0.20**	(2.10)	–0.17*	(–1.66)
2	$Sales^{2C}$	0.18***	(2.75)	–0.08	(–1.08)	0.14*	(1.84)	–0.26***	(–2.66	0.22**	(2.09)
3	BAR^{2C}	–0.14	(–1.51)	0.32***	(3.22)	–0.07	(–0.69)	0.46***	(3.37)	–0.39***	(–2.69)
	N2C Stocks										
4	$Buys^{N2C}$	0.10***	(2.65)	0.09**	(2.40)	0.02	(0.57)	–0.00	(–0.03)	–0.07	(–1.24)
5	$Sales^{N2C}$	0.03	(0.76)	0.09**	(2.38)	–0.02	(–0.54)	0.07	(1.22)	–0.12**	(–2.01)
6	BAR^{N2C}	0.07	(1.33)	0.00	(0.02)	0.05	(0.79)	–0.07	(–0.89)	0.05	(0.56)
	2C – N2C										
7	Buys	–0.06	(–0.76)	0.14*	(1.83)	0.05	(0.54)	0.20*	(1.85)	–0.10	(–0.83)
8	Sales	0.15**	(2.03)	–0.17**	(–2.10)	0.17*	(1.87)	–0.32***	(–2.92)	0.34***	(2.80)
9	BAR	–0.21**	(–1.97)	0.31***	(2.79)	–0.12	(–0.99)	0.52***	(3.37)	–0.44***	(–2.62)

***, **, and * indicate significance at the 1%, 5%, and 10% levels. Returns are expressed as a percent, and winsorized at 1% and 99%.