THE EFFECTS OF OPTIONS MARKETS ON THE UNDERLYING MARKETS: QUASI-EXPERIMENTAL EVIDENCE

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ABSTRACT

This dissertation consists of three essays in applied financial economics. The unifying theme is the use of financial regulation as quasi-experiments to understand the interrelationship between derivatives and the underlying assets. The first two essays use different quasi-experimental econometric techniques to answer the same research question: how does option listing affect the return volatility of the underlying stock? This question is difficult to answer empirically because being listed on an options exchange is not random. Volatility is one of the dimensions along which the options exchanges make their listing decisions. This selection bias confounds any causal effect that option listing may have. What is more, the options exchanges may list along unobservable dimensions. Such omitted variable bias can also confound any causal effect of option listing.

My first essay overcomes these two biases by exploiting the exogenous variation in option listing that is created by the SEC-imposed option listing standards. Specifically, the SEC mandates that a stock must meet certain criteria in the underlying market before it can trade on an options exchange. For example, a stock needs to trade a total of 2.4 million shares over the previous 12 months before it can be listed. Since 2.4 million is an arbitrary number, stocks that are “just above” the 2.4 million threshold will be identical to stocks that are “just below” it, the sole difference being their probability of option listing. Accordingly, I use the 2.4 million threshold as an instrument for option listing in a fuzzy regression discontinuity design. I find that option listing causes a modest decrease in underlying volatility, a result that corroborates many previous empirical studies.
My second essay attempts to estimate the effect of option listing for stocks that are “far away from” the 2.4 million threshold. I overcome the aforementioned omitted variable bias by fully exploiting the panel nature of the data. I control for the unobserved heterogeneity across stocks by implementing a two-way fixed effects model. Unlike most previous studies, I control for individual-level fixed effects at the firm level rather than at the industry level. My results show that option listing is associated with a decrease in volatility. Importantly, these results are only statistically significant in a model with firm-level fixed effects; they are insignificant with industry-level fixed effects.

My third essay is a policy evaluation of the SEC’s Penny Pilot Program, a mandated decrease of the option tick size for various equity options classes. Several financial professionals claimed that this decrease would drive institutional investors out of the exchange-traded options market, channeling them into the opaque, over-the-counter (OTC) options market. I empirically test an implication of this hypothesis: if institutional investors have fled the exchange-traded options market for the OTC market, then it may take longer for information to be impounded into a stock’s price. Using the ‘price delay’ measure of Hou and Moskowitz (2005), I test whether stocks become less price efficient as a result of being included in the Penny Pilot Program. I perform this test using firm-level fixed effects on all classes that were included in the program. I confirm these results with synthetic control experiments for the classes included in Phase I of the Penny Pilot Program. Generally, I find no change in price efficiency of the underlying stocks, which suggests that the decrease in option tick size did not materially erode the price discovery that takes place in the exchange-traded equity options market. I also find evidence that the decrease in option tick size caused an increase in short selling for the piloted stocks.
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TABLE OF CONTENTS

ABSTRACT ....................................................................................................................... iii

ACKNOWLEDGMENTS .................................................................................................... v

LIST OF TABLES ............................................................................................................. viii

LIST OF FIGURES .......................................................................................................... ix

CHAPTER

1. THE EFFECT OF EQUITY OPTION LISTING ON UNDERLYING VOLATILITY: REGRESSION DISCONTINUITY EVIDENCE FROM OPTION LISTING STANDARDS
   1.1 Introduction ............................................................................................................. 1
   1.2 Previous Literature ............................................................................................ 6
   1.3 Data ....................................................................................................................... 14
   1.4 Methodology and Identification Strategy .......................................................... 18
   1.5 Results .................................................................................................................. 23
   1.6 Supplemental Analysis and Robustness .............................................................. 27
   1.7 Conclusion .......................................................................................................... 36

2. THE EFFECT OF EQUITY OPTION LISTING ON UNDERLYING RETURN VOLATILITY: EVIDENCE FROM FIRM FIXED EFFECTS
   2.1 Motivation ............................................................................................................ 40
   2.2 Data ....................................................................................................................... 42
   2.3 Methodology ........................................................................................................ 45
# LIST OF TABLES

<table>
<thead>
<tr>
<th>Table</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.1 Causal Estimates of Option Listing on Volatility</td>
<td>25</td>
</tr>
<tr>
<td>1.2 2SLS Estimates of Listing on Alternative Volatility Measures</td>
<td>33</td>
</tr>
<tr>
<td>1.3 Local Linear 2SLS Regression Results for Increasingly-Narrow Bandwidths</td>
<td>35</td>
</tr>
<tr>
<td>2.1 Baseline Estimates of Listing on Volatility</td>
<td>50</td>
</tr>
<tr>
<td>2.2 Random Trends Model</td>
<td>57</td>
</tr>
<tr>
<td>2.3 Anticipation and Post-Treatment Tests</td>
<td>62</td>
</tr>
<tr>
<td>3.1 Regression Estimates of the Effect of Option Penny Tick Size on Option Limit Orders</td>
<td>76</td>
</tr>
<tr>
<td>3.2 Regression Estimates of the Effect of Option Penny Tick Size on the Underlying Market</td>
<td>94</td>
</tr>
<tr>
<td>3.3 Regression Estimates of the Effect of Option Penny Tick Size on Underlying Price Efficiency</td>
<td>96</td>
</tr>
<tr>
<td>3.4 Institutional Ownership and Short-Selling Weights for Synthetic AMD</td>
<td>108</td>
</tr>
<tr>
<td>3.5 Price Delay Weights for Synthetic AMD</td>
<td>112</td>
</tr>
</tbody>
</table>
## LIST OF FIGURES

<table>
<thead>
<tr>
<th>Figure</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.1 Visual RD for 2000-2002</td>
<td>24</td>
</tr>
<tr>
<td>1.2 Density Plots of the Running Variable</td>
<td>29</td>
</tr>
<tr>
<td>1.3 Cutoff at Pre-Treatment Covariates</td>
<td>31</td>
</tr>
<tr>
<td>2.1 Graphs of Anticipation and Post-Treatment Effects</td>
<td>63</td>
</tr>
<tr>
<td>2.2 Histograms of Placebo Effects</td>
<td>66</td>
</tr>
<tr>
<td>3.1 AMD Constrained-Regression Synthetic Control: Institutional Ownership and Short Selling</td>
<td>110</td>
</tr>
<tr>
<td>3.2 AMD Constrained-Regression Synthetic Control: Price Delay</td>
<td>114</td>
</tr>
<tr>
<td>A.2 Agilent Synthetic Control Analysis</td>
<td>132</td>
</tr>
<tr>
<td>A.3 Caterpillar Synthetic Control Analysis</td>
<td>133</td>
</tr>
<tr>
<td>A.4 Intel Synthetic Control Analysis</td>
<td>134</td>
</tr>
<tr>
<td>A.5 Microsoft Synthetic Control Analysis</td>
<td>135</td>
</tr>
<tr>
<td>A.6 Texas Instruments Synthetic Control Analysis</td>
<td>136</td>
</tr>
</tbody>
</table>
CHAPTER 1

THE EFFECT OF EQUITY OPTION LISTING ON UNDERLYING VOLATILITY: REGRESSION DISCONTINUITY EVIDENCE FROM OPTION LISTING STANDARDS

1.1 Introduction

Many policymakers are apprehensive about derivatives because of the fear of destabilization or market manipulation. For instance, in the 1950s American onion farmers successfully lobbied Congress to ban trading in futures contracts on onions, a ban that’s still in place to this day (Onion Futures Act). The Securities and Exchange Commission (SEC) placed a moratorium on option listing in the late 1970s citing fears of destabilization. More recently, India’s finance minister has instituted a Tobin-type tax on commodity futures and options in an attempt to curb the perceived-to-be-rampant speculation in those markets, which is seen as a net negative on the Indian economy.¹

¹www.financialexpress.com/archive/futures-caused-the-market-manipulation/296336
Is such an apprehension about the destabilizing ability of options—and derivatives more generally—justified? To some extent, yes. There is some reason to believe that the introduction of equity options can cause an increase in the volatility of the underlying stock. One channel for such destabilization is dynamic delta hedging. Dynamic hedging is a risk-minimization strategy employed by option writers. For example, since the writer of a call option faces upside price risk (of the underlying stock), he hedges his option position by purchasing the underlying stock. This hedge is destabilizing because it requires the option writer to buy when price is increasing, which further increases the price, pulling it away from its fundamental value.²

Despite the possibility for destabilization, there are some reasons to think that the introduction of an options market for a stock will stabilize the underlying price (and return). The general idea is along the lines of Friedman (1953): speculators stabilize markets by selling when an asset is overvalued, and buying when an asset is undervalued. To the extent that a new options market brings in more speculation, price (and return) volatility of a stock should decrease upon being listed on an options market.

The formal theoretical models don’t bring us much closer to answering the question of whether a new options market stabilizes or destabilizes the underlying market. Ross (1976) argues that options span more of the payoff space, thereby helping to complete the market. But Hart (1975) points out that ‘partial completion’ of the market is not necessarily welfare-enhancing. Diamond and Verrecchia (1987) argue that options allow traders with negative information to circumvent short-selling restrictions. But Stein (1987) notes that if informed traders are working with inaccurate

²Somanathan and Nageswaran (2015) have a concise numerical example demonstrating the destabilizing nature of dynamic delta hedging (pgs. 78-80).
information, the option may be destabilizing. Biais and Hillion (1994) show that the introduction of an options market can either worsen or ameliorate the degree of asymmetric information in the underlying market, which, consequently, has an ambiguous effect on volatility. All of this is to say that the stabilizing/destabilizing nature of a new options market is an empirical question.

Empirically, researchers must contend with the inherent selection bias in trying to assess the volatility effect of option listing. That is, options exchanges choose to list options on stocks because of the volatility of these stocks. Some recent studies have attempted to correct for this selection bias. For instance, Mayhew and Mihov (2004) perform a control-sample event study where the control group (non-optioned stocks) is matched on propensity score. They find an increase in volatility. Using a more sophisticated propensity score matching function, Danielsen, Van-Ness, and Warr (2007) find that measures of market quality, including return volatility, are already declining when the option is listed. These authors find that any meaningful break in volatility can happen as early as 35 days before the actual listing date. Simply put, there is no option listing date effect.

I believe that there are at least several reasons to revisit these empirical studies. The main reason is methodological. The empirical studies mentioned above rely on a matching function to choose their control sample. But even the best matching function will not match optioned stocks and non-optioned stocks along unobservable dimensions. It is at least conceivable that the option-listing committees at the options exchanges select stocks along unmeasurable dimensions, e.g., name recognition of the stock. Moreover, matching functions flirt with the possibility of violating the assumption of ignorability of treatment (Wooldridge (2005)).

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3 Figlewski and Webb (1993) show empirically that short-sellers do indeed use options to implement their trading strategy.
Furthermore, there have been a number of profound changes to the industry’s market structure. The options exchanges were colluding throughout all of the 1990s. Specifically, the options exchanges were not cross-listing each other’s options, meaning that the options market was effectively monopolized throughout the 1990s. Most previous studies have data going up to 2002 at the latest and thus their samples are from a time when the options market was monopolized by the collusion among the options exchanges. Furthermore, there have been a few new entrants into the options market. Most notably, International Securities Exchange (ISE)–the first fully electronic exchange in the world–began selling options in 2001. The rise of electronic trading has had a profound effect on the options market, as it has in all markets (Gorham and Singh (2009)).

I address the potential shortcomings of previous studies by revisiting the option listing question in a time period when the options exchanges were competing (the collusive agreement was broken in 1999). I address the other principal potential shortcoming of previous studies—the possibility that unobservable variables confound the effect of option listing—by using option listing standards as an instrument for option listing in a fuzzy regression discontinuity design framework. Option listing standards are minimal necessary criteria that a stock must meet before it can trade on an options exchange.

Specifically, the SEC requires a stock to have at least a 12-month aggregate trading volume of 2.4 million shares before it can be traded on an options exchange. The threshold level of 2.4 million shares is exogenous since it is set and enforced by the SEC. Moreover, this volume threshold was set 27 years ago (in 1991), lending further evidence that the threshold is exogenous to return volatility. Since the volume threshold is correlated with option listing, but exogenous to return volatility, it can
be used as an instrument for option listing.\(^4\) The continuous nature of the volume
threshold also lends itself to an analysis within the regression discontinuity design
framework. The “fuzzy” regression discontinuity design is based on the idea that ob-
servations of the dependent variable (daily GARCH return volatility) on one side of
the threshold are very similar to observations on the other side of the threshold, even
along unobservable dimensions—the only difference between the two observations is
that one has a higher probability of being listed on an options exchange.

Thinking about this question within the quasi-experimental paradigm of modern
econometrics, the treatment is option listing; however, it is not randomly assigned.
The volume threshold of 2.4 million shares, however, \textit{induces} random assignment of
option listing: being above the threshold serves as a proxy for receiving treatment,
while being below it is a proxy for not receiving treatment.

The regression discontinuity design (RDD) can be implemented parametrically
via two-stage least squares on the full sample, controlling for varying degrees of poly-
nomials of the assignment variable (12-month aggregate trading volume). Or the
RDD can be implemented semi-parametrically via local linear regression: two-stage
least squares regression on an increasingly-narrow bandwidth of the threshold. Of
course, for the sake of robustness the results from both estimation procedures should
be approximately equal. Parametrically, I find that option listing causes a 0.7%
decline in option listing, a result that is statistically significant and robust to the
inclusion of leverage effects (gjr-GARCH), but not robust to the assumption about
the assumption of underlying probability distribution in the maximum likelihood es-
timation procedure of GARCH estimation: assuming a normal distribution leads to

\(^4\)To be sure, 2.4 million cumulative trade volume is a small number. To get an idea, in my sample
of 2000-2002, Microsoft’s average \textit{daily} trading volume is 39.6 million shares. The global average of
the 12-month aggregate trade volume for the sample is 113.1 million shares, and the median is 16.6
million shares.
statistically significant results, while assuming a Student’s t distribution leads to statistically insignificant results at the typical conventional levels of significance. In any case, the magnitudes of the results are economically insignificant. The decrease—and it’s always a decrease—in (annualized) volatility ranges from 0.04% to 1.1%, which is in the 0.01 percentile of the sample (the global sample average is 78% with a standard deviation of 57% and a median of 64%). Non-parametric results generally confirm these estimates.

The paper proceeds as follows. Section 1.2 reviews the previous literature. Section 1.3 describes the data. Section 1.4 describes the methodology. Section 1.5 discusses the results. Section 1.6 contains robustness checks. Section 1.7 concludes and presents avenues of future research.

1.2 Previous Literature

1.2.1 Theoretical Literature

The classical view of options is that they are redundant financial instruments. The returns generated through the purchase/sale of options can be replicated through dynamic trading strategies, that is, different combinations of purchase/short-sale of stocks and risk-free bonds. In complete markets with no imperfections and no transactions costs, options serve no purpose.

However, if the equity market is not complete, options can fill the gap. Ross (1976) seems to be the first to note the market-completing role of options. Hence, a good starting point for the theoretical models of the introduction of a derivatives market is Ross, later expounded upon by Arditti and John (1980). Ross (1976) notes that real-world markets are incomplete, and options can make them complete—or
at least more complete. He states “options written on existing assets can improve efficiency by permitting an expansion of the contingencies that are covered by the market.” In other words, options create new spanning possibilities. The introduction of an options market allows for a more efficient allocation of risk, thereby boosting aggregate welfare. Hodges (1992) has a numerical illustration of Ross’s model.

To be sure, the introduction of an option market will not necessarily increase welfare. Ross says “[t]he possibility of inefficiency arises whenever the feasible set of pure contingent claims...fails to span all the state space.” Hart (1975) builds a model showing just that: “if we start off in a situation where markets are incomplete, opening new markets may make things worse rather than better.” He goes on to say “[i]n this respect, an economy with incomplete markets is like a typical second best situation.” Thus, although introducing new options markets may inch us closer to market completeness, this may not be welfare enhancing unless the market becomes fully complete. Elul (1995) shows that Hart’s model is a general phenomenon, and cannot be brushed aside as a one-off counterexample.

Allen and Gale (1990) endogeneize the creation of an options exchange and find that the exchanges don’t have an incentive to introduce option classes that assist in completing the market. The exchanges may introduce options that are completely financially redundant, i.e., create no new spanning opportunities. Furthermore, these authors note that previous models do not consider the cost and resources that are used in creating an options exchange.

What’s more, completing the market—or inching towards completion—has the effect of altering the transmission of information in the market on which the option is written. Biais and Hillion (1994) point this out in their model. Options complete the underlying market, which has an ambiguous effect on the informational efficiency of the underlying market. The authors invoke the typical distinction among investors
from the asymmetric information model of Glosten and Milgrom (1985): informed traders and liquidity traders. Informed traders trade on private, superior knowledge. Liquidity traders trade as a result of their own idiosyncratic liquidity shocks, i.e., for non-speculative reasons.

Biais and Hillion (1994) go on to note that in an incomplete market, some liquidity traders cannot completely hedge their liquidity shocks. But in a more complete market liquidity traders can better hedge their shocks, i.e., engage in a greater degree of risk-sharing. Hence, on the margin, options allow more trades among liquidity traders. And since informed traders like to “hide” among the liquidity traders (so as not to reveal their type), the increase in liquidity trading means that there will be a corresponding increase of informed trading.\(^5\) This makes prices more informative. More informative prices are less sensitive to news shocks than less informative prices—with more informed prices, news shocks aren’t as “shocking,” as it were. Therefore, the introduction of options should be associated with a decrease in underlying volatility.

However, there is a countervailing force through which the introduction of options can be associated with an increase in underlying volatility. Biais and Hillion (1994) point out that since options complete the market, there are more trading strategies available to the informed trader. In other words, options enable the informed trader to better “hide” himself, i.e., with the availability of options, the informed trader does not have to rely as much on “hiding” among the liquidity traders. Hence, the option market draws some informed traders away from the underlying market. The private information that these informed traders possess is not impounded into the price of

\(^5\)Informed traders prefer to trade in more liquid markets. Why? Not only is it easier to find a trading partner in more liquid markets, but it is easier to “hide” among the liquidity traders. Many asset markets have dealers. Dealers buy and sell assets. If an informed trader reveals himself, then prices will surely move against him, e.g., the dealer will quote the informed trader a higher price than he otherwise would have.
the underlying asset. The underlying asset’s price becomes more sensitive to news shocks. Underlying volatility increases.

Another channel through which the introduction of options can affect the underlying market is through the reduction of transactions costs: options reduce the cost of gaining exposure to a stock. Within the realm of transactions costs that are associated with trading, John, Koticha, Narayanan, and Subrahmanyam (2003) focus on one specific type of transaction cost—margin requirements, which the authors claim has been overlooked by most of the previous studies on the question of how options affect the underlying market.

John et al. (2003) note that the underlying stock market is deeper than the option market in the sense that it can better accommodate the bulk selling that is so characteristic of informed traders. However, if margin requirements are introduced in both the underlying and the options markets, and if these margin requirements are both binding, then the informed trader is faced with a trade-off: sell his private information in the underlying stock market where prices will not move against him as much as they would in the options market or sell his private information in the options market, where he can exploit greater leverage. More concretely, suppose that the margin requirement in the underlying stock market was very large relative to the margin requirement in the options market. The ‘margin advantage’ of the options market could, in principle, be large enough that the informed trader sells all of his private information in the options market. This scenario could translate into a decrease in the informational efficiency of the underlying market, which, in turn, would translate into a decrease in the volatility of the underlying asset.6

6In reality it is probably the case that the options market does indeed have a ‘margin advantage’ over the underlying stock market. How so? In the underlying stock market, the broker sets the margin requirement, while in the options market, it is the exchange itself that sets the margin requirement, e.g., an exchange can better withstand clearing the wrong end of a short sale than a
Other theoretical models are less ambiguous about the effect of the introduction of an options market. Some models highlight a channel through which introducing an option market can increase underlying volatility, while others focus on channels through which there is a decrease. For example, Stein (1987) builds a model that focuses on the channel that derivatives can draw in informed traders from the underlying market. But if these informed traders are misinformed about the fundamental value of a stock, then the volatility of the underlying asset (or market) can increase.

A more recent model is that of Brock and Wagener (2009). Their model shows that the introduction of more hedging instruments may increase underlying volatility. The channel through which this happens is that traders are heterogeneous with regard to rationality: there are non-rational traders who follow the herd, trading according to some performance measure. This performance measure then overshoots the fundamental value of the security only to readjust at a later time. Volatility increases. In the Brock and Wagener (2009) model, rational traders can prevent this destabilization but only if information-gathering costs are not too large. Simsek (2013) has a similar model but focuses on portfolio risk rather than price instability.

Several models demonstrate a decrease in underlying volatility as the result of an introduction of a derivatives market. Grossman (1988) argues that exchange-traded options convey information about the underlying asset, namely the number of traders following a portfolio insurance trading strategy. Grossman argues that this information makes the market less susceptible to some types of market herding. Volatility decreases. Gennottee and Leland (1990) has a similar argument.

Other models showing a decrease in volatility are Detemple and Selden (1991) and H. Cao (1999). Detemple and Selden model a general equilibrium where investors have different levels of beliefs about the downward prospects of an asset, which leads to op-
tions being a complement to the underlying asset in aggregate. The complementarity of the option to the underlying asset works to decrease the latter’s return volatility. H. Cao (1999) develops a noisy rational expectations model where the introduction of an option increases the market’s incentive to collect information about the underlying asset, thereby making price more informative and reducing volatility.\(^7\)

### 1.2.2 Empirical Literature

The first formal empirical studies of whether option introduction affects the underlying market are Nathan Associates (1974) of the Chicago Board Options Exchange, Trennenpohl and Dukes (1979), and Hayes and Tennenbaum (1979). These studies compare the volatility of optioned stocks to the volatility of a random sample of non-optioned stocks over the same time period before and after the existence of the Chicago Board Options Exchange (CBOE). These two studies find that option listing is associated with a decrease in underlying volatility. Hayes and Tennenbaum (1979) find that option listing is associated with an increase in the trading volume of the underlying stock.

Nabar and Park (1988) is a noteworthy study because it is the first study to use a relatively large sample of stocks, reducing any concern of a small sample bias. Using an event study, Nabar and Park (1988) examine option introduction on 390 stocks and compare the volatility of these stocks before and after listing to a control sample of 340 stocks. They find a small decline in volatility as a result of being optioned.

Conrad (1989) finds a decrease in return volatility associated with option listing. Furthermore, Conrad appears to have been the first study to examine the possibility that there is an ‘announcement effect’, i.e., that it is the announcement date of op-

\(^7\)An implication is that the prices of optioned stocks should be less sensitive to earnings announcements, a result that has been empirically confirmed by Hu (2017).
tion listing that will affect the underlying stock, not the listing date. Conrad finds no evidence of an announcement effect. Detemple and Jorion (1990) revisit Conrad’s findings. They find that the introduction of an options market increases prices in the underlying spot market, and it also leads to a decrease in the volatility in the underlying asset. Detemple and Jorion further find that there is, in fact, an announcement effect: there is a 2% price increase in the price of the underlying stock around a three-day band of the announcement date.\textsuperscript{8}

Skinner (1989) is a noteworthy study on the question of the effects of option introduction on the underlying market because it is the first study to mention the potential for selection bias in the findings. The selection bias that he mentions is the one mentioned in Section 1.1 above: the options exchanges do not randomly choose stocks to option. The options exchanges choose to list stocks partly because of the volatility properties of these stocks. Skinner (1989) finds that stock return volatility decreases after option introduction. He attributes this decrease in volatility to the possibility that the options exchanges choose to offer options on a stock when the stock is at its peak volatility. Hence, volatility decreases after option listing because it would have decreased anyway (i.e., the stock’s volatility would have reverted to its mean level).

Mayhew and Mihov (2004) find that underlying stock return volatility increases with option introduction. This finding is in stark contrast to nearly every other empirical study. The authors explain this counter-consensus finding by noting two points: first, volatile stocks earn more money for the options exchanges; and second, options exchanges are forward-looking. Thus, according to Mayhew and Mihov (2004), options exchanges will choose to list options on stocks that are inherently volatile.

\textsuperscript{8}Danielsen et al. (2007) is the most recent study on announcement date effects. These authors find that the announcement date effect on volatility is greater than the effect of option listing date, but nonetheless, both are small in magnitude.
Options exchanges prefer to list options on relatively volatile stocks since one of the principal reasons that investors purchase options is for hedging. A stock with a volatile return translates into uncertainty for the investor, which, in turn, translates into the purchase of option contracts. Mayhew and Mihov (2004) argue that options exchanges likely have the ability to forecast underlying stock volatility. Hence, once options are listed, the stock eventually becomes more volatile and more options are sold, i.e., the options exchanges’ forecast comes to fruition. Essentially, Mayhew and Mihov (2004) argue that options exchanges are forward-looking with respect to listing choice, while Skinner (1989) argues that exchanges are backward-looking with respect to listing choice.

A key study that finds no volatility effect is Mazouz (2004). Mazouz couples a GARCH (1,1) estimation process with the control sample methodology employed by Mayhew and Mihov (2004). He employs a matching function to obtain a control sample in an attempt to correct for selection bias. He employs a GARCH estimation procedure in order to account for the clustering of volatility that is so prevalent in stock returns; there is no volatility effect of option introduction.\(^9\)

The increase-in-volatility finding of Mayhew and Mihov (2004) is corroborated by Faff and Hillier (2005). Faff and Hillier employ a GARCH estimation procedure using UK data. These authors find that there is a statistically discernible increase in volatility ten days after initial option listing. It is worth noting that the options market in the UK is monopolized (the same of Mayhew and Mihov (2004) ends in 1996, a time when the US options market was effectively monopolized).

Danielsen et al. (2007) revamp the Mayhew and Mihov (2004) approach using microstructure data and a greatly-improved matching function to control for the se-

lection bias in option listing. Danielsen et al. (2007) confirm the Mayhew and Mihov (2004) findings: options exchanges select stocks to option based on stock characteristics and volatility but especially liquidity, which they measure as the closing bid-ask spread. These authors argue that the importance of liquidity makes sense because market-makers engage in dynamic delta hedging. A full hedge requires the underlying stock to be very liquid.\textsuperscript{10}

Jubinski and Tomljanovich (2007) analyze the option listing question using a GJR-GARCH (TGARCH) specification of volatility, apparently being the first study to account for the “leverage effects” in return volatility. These authors find that option listing has no discernable effect on volatility except for small firms with high trading volume. It is worth noting that trading volume is a measure of liquidity since a stock that trades a lot in the underlying market is one whose option class can be delta-hedged by market makers in the options market.

1.3 Data

The period of study is all trading days from 2000 to 2002. I chose the year 2000 as the start of the sample for a couple of reasons. First, prior to August of 1999 the options exchanges were illegally colluding. The options exchanges were implicitly refusing to cross-list each other’s options.\textsuperscript{11} Hence, any analysis prior to 2000 is examining an options market that is effectively monopolized. Second, option

\textsuperscript{10}In a personal correspondence, two CBOE traders—who together have over 50 years of trading experience—confirmed that liquidity of the underlying market is by far and away the most important factor in making a market for an option class.

\textsuperscript{11}For example, the PHLX options exchange had the monopoly on Dell options. In August of 1999, the CBOE broke the (illegal) tacit agreement and cross-listed options for Dell on its exchange.
listing dates are difficult to find prior to 2000.\textsuperscript{12} I chose the year 2002 as the end of the sample because as of January 22, 2003, the SEC dramatically eased the median share price option listing standard: it went from $7.50 for a majority of the previous 90 days to a share price of $3.00 for the previous five days. This easing could be a confounder. In addition to the easing of the share-price listing standard, in 2003 the CBOE began electronic screen trading, presumably lowering the cost of trading options, which could also serve as a confounder.

The frequency of data is daily, for a total number of observations of 4,609,735. I use daily data rather than aggregating up to a lower frequency because the option listing standards are set at the daily level; that is, a stock becomes eligible on a particular day. Furthermore, regression discontinuity designs are data hungry on account of their reliance on local linear regressions as a key robustness check. Thus, aggregating to a lower frequency would dramatically cut the sample size, which, in turn, reduces the credibility of the results.

The data structure is panel data (CUSIPs over time), which, in an RDD framework is treated as a pooled cross section (see Lee and Lemieux (2010)). From CRSP, via WRDS, I collect firm-identifying information such as company name, ticker, share type, CUSIP, PERMCO, exchange code, and industry. CUSIP is the unit of study. I filter the data to retain only shares of stock, that is, share codes 10 and 11. I also filter the data by exchange code, keeping only those stocks that trade on the NYSE, NASDAQ, or Amex.

Also from CRSP, I collect trading data such as the intraday low, intraday high, final closing price, adjusted closing return (adjusted for stock splits and dividends), and trading volume. I collect data from 1999 to 2002. Going back to 1999 allows

\textsuperscript{12}Of course, the first day of option trading could be used (see Danielsen et al. (2007)), but that is an imperfect measure since an option could, in principle, be listed and yet not traded.
me to create the assignment variable, which is a rolling 12-month aggregate of trade volume. Using this measure, I create a dummy variable that is set to 1 if the value is 2.4 million or greater, and 0 otherwise. This dummy variable is the RDD threshold; it is the instrumental variable. Once the threshold variable has been calculated, I eliminate all observations prior to 2000.

To calculate volatility, I use the adjusted return series to estimate a GARCH (1,1) process for each stock in the sample. This estimation necessitates that I eliminate any CUSIP that has fewer than five observations since there are five parameters in the most general GARCH (1,1) estimation.\footnote{I also estimate a GJR-GARCH (1,1) process, for which at least six observations are necessary for estimation.} Why a GARCH (1,1) process, rather than, say, a GARCH (1,2) process? Alexander (2001)[pg.72] has the answer: “it is rarely necessary to use more than a GARCH(1,1) model.” Since I estimate GARCH(1,1) for every stock in the sample—of which there are 7,728—I want the most general model. To that end, the distributional assumption that I make for the maximum likelihood estimation is the normal distribution. Formally

\[
y_{it} = \mu + u_{it} , \quad u_{it} \sim N(0, \sigma_{it}^2);
\]

\[
\sigma_{it}^2 = \alpha_0 + \alpha_1 u_{it-1}^2 + \beta \sigma_{t-1}^2
\]

Since it is common knowledge that many financial time series exhibit “fat tails” I also re-run the main results assuming a Student’s $t$ distribution. As a robustness check, I also estimate a GJR-GARCH (TGARCH) model with the normal distribution as well as the Student’s $t$ distribution. I annualize all GARCH series (and multiply by 100) for ease of interpretation (percentage).
For the sake of the robustness regarding the estimation of volatility, I also use the intraday range, measured as the log of the ratio of intraday high to intraday low as an additional measure of volatility (Brooks (2008)). The idea here is that the spread of a series—dispersion about the mean—is a measure of volatility and the range is a measure of spread. Hence, the range is a measure of volatility. Parkinson (1980) argues that if stock prices are a random walk, then the intraday range is a “superior estimate” of a stock’s volatility than the “traditional” measure of volatility of Merton (1980), which squares the difference of the log of the daily close price and then averages this measure over some time period such as a month (Poterba and Summers (1986)) or year (Pindyck (1988)). Bollerslev, Chou, and Kroner (1992) say that Merton’s method does not make efficient use of all the data. However, Corwin and Schultz (2012) argue that the intraday range is composed of two parts, spread and volatility. In their paper, they decompose the intraday range into its spread component and its volatility component. Leaving the interpretation of the intraday range aside, I estimate the effect and let the reader decide the interpretation.\footnote{As the results show (see below), the intraday range has a substantially higher magnitude than the GARCH measure. More importantly, the sign is reversed: using GARCH, I find a decrease in volatility from option listing, but with intraday range, I find an increase.}

I collect the option listing dates from the Options Clearing Corporation’s (OCC) website. The OCC is the central clearinghouse for all of the equity options exchanges in the US (options are fungible across the exchanges as a result of sharing the OCC as the central clearinghouse). These dates allow me to create a dummy variable for each time period that an option was listed for a stock. This listing dummy is the treatment.
The OCC website only denotes new listings as of February 14, 2001. Since the time period of the study is January 3, 2000 - December 31, 2002, I collect the exhaustive list of all listed options up to December 31, 1999 from the 1999 OCC Annual Report so that I know which stocks were listed prior to the start of the study. I collect the set of new option listing dates for the year 2000 and for the first six weeks of 2001 from the archived web pages of the options exchanges using the Wayback Machine. 

1.4 Methodology and Identification Strategy

1.4.1 Option Listing Standards

To identify the causal effect of option listing, I use option eligibility requirements as instruments for option listing in a fuzzy regression discontinuity design (RDD) framework. The SEC imposes constraints on which stocks can be listed in the options market. For example, from the time period of 2000 to 2002, to be listed on an options exchange a stock must meet all of the following criteria: trade in the underlying market on a national market exchange (NMS); have 2,000 unique shareholders; have at least seven million free-float shares; have a trading volume in the underlying market of at least 2.4 million shares in the twelve months prior to listing; trade at $7.50 per share for the majority of the previous 90 days preceding an option listing.

Within the fuzzy regression discontinuity framework, I compare stocks that are “just above” the thresholds to stocks that are “just below” them, first globally and then in an increasingly narrow bandwidth around the cutoff. The causal power of the

---

15As of 2018, this is no longer the case: the OCC has recently deleted all option listing dates prior to 2012

16https://archive.org/web/
estimates comes from the exogenous nature of the thresholds: eligibility is correlated with listing but not with market quality except through listing. Since the ‘national market exchange’ criterion is not a continuous variable (required by regression discontinuity designs), I filter out all stocks that do not trade on the NYSE, Nasdaq, or Amex. I ignore the public float criterion for a lack of data access (Form 144 - insider trading from the SEC). Following Mayhew and Mihov (2004) I ignore the ‘unique shareholder’ criterion because many investors hold their shares in street name, which makes it nearly impossible to determine the exact number of unique shareholders. The ‘minimum share price’ requirement shows no discontinuity in option listing (see Figure 1.1). Therefore, the option listing standard that I focus on is the ‘underlying trade volume’ requirement, which does show a distinct discontinuity and kink at the threshold value.\footnote{Actually, the minimum share price requirement is highly significant in the data, but the discontinuity in option listing is nearly completely absent in the Visual RD, as shown in Figure 1.1. Details from the share-price analysis are available upon request.}

### 1.4.2 Regression Discontinuity Design

Meeting the option listing requirements is necessary for being listed on an options market, but it is not sufficient: the options exchanges have discretion with regard to which stocks they choose to list. As mentioned above, listing is endogenous to return volatility, and being eligible for option listing is exogenous to return volatility but is correlated with option listing. Hence, it follows that option eligibility can be used as an instrument for option listing. And in a fuzzy regression discontinuity design, that is indeed the interpretation.
Formally, let $D_i$ be the treatment variable (option listing). Let $x_i$ be the running variable, i.e., the variable that determines whether a stock is eligible for treatment, e.g., aggregate underlying trade volume 12-months back. The treatment is not completely determined by the running variable, only the probability of receiving treatment is determined by the running variable. That is,

$$P(D_i = 1|x_i) = \begin{cases} 1 & \text{if } g_1(x_i) \geq x_0 \\ 0 & \text{if } g_0(x_i) < x_0 \end{cases}$$

(1.4.1)

where $P(\cdot)$ is probability; $g_1(x_i)$ is some function—any function—of the running variable $x_i$; $g_0(x_i)$ is also some function—any function—of the running variable; and $x_0$ is the cutoff value of the running variable where probability of receiving treatment “jumps.” For my purposes, the value of $x_0$ is 2.4 million shares for the volume eligibility criterion.

It is necessary to make two assumptions in a fuzzy regression discontinuity design. First, assume that $g_1(x_0) \neq g_0(x_0)$, which says that the function that determines probability of treatment cannot be the same at the cutoff. Second, assume that $g_1(x_0) > g_0(x_0)$, which says that at the cutoff, the probability of receiving treatment is greater than not receiving it.

Since the treatment variable, $D_i$, is binary, we can write its conditional expectation as

$$E[D_i|x_i] = \left(D_i = 1\right)P(D_i = 1|x_i) + \left(D_i = 0\right)P(D_i = 0|x_i)$$

(1.4.2)

$$E[D_i|x_i] = P(D_i = 1|x_i)$$

(1.4.3)

---

The formal treatment in this section is loosely based on Angrist and Pischke (2009)
The right-hand side of equation 1.4.3 is the left-hand side of equation 1.4.1, which we can write as a function of a binary indicator variable, \( \tau_i \), which “switches on,” i.e., take a value of 1, when \( x_i \geq x_0 \), and zero otherwise. That is

\[
E[D_i|x_i] = g_0(x_i) + [g_1(x_i) - g_0(x_i)]\tau_i \tag{1.4.4}
\]

so that when \( \tau_i = 1 \), equation 1.4.4 collapses to \( g_1(x_i) \); and when \( \tau_i = 0 \), equation 1.4.4 collapses to \( g_0(x_i) \). In a two-stage least squares framework, equation 1.4.4 is the first stage, regressing the treatment variable (option listing) on a function of the running variable, interacted with the cutoff indicator.

The \( g_0(x_i) \) and \( g_1(x_i) \) functions can be polynomials of any order. The most general functional form of 1.4.4 is

\[
E[D_i|x_i] = \gamma_{00} + \gamma_{01}x_i + \gamma_{02}x_i^2 + \ldots + \gamma_{0p}x_i^p
+ [\pi + (\gamma_{11} - \gamma_{01})x_i + (\gamma_{12} - \gamma_{02})x_i^2 + \ldots (\gamma_{1p} - \gamma_{0p})x_i^p]\tau_i \tag{1.4.5}
\]

where higher-order polynomials are included to distinguish between discontinuities and non-linearities, cf. Gelman and Imbens (2016). As explained by Hahn, Todd, and Klaauw (2001), fuzzy regression discontinuity designs can be implemented via two-stage least squares (2SLS). Equation 1.4.5 is the first stage. The second stage is a regression of the outcome variable (stock return volatility) on the fitted values from equation 1.4.5, along with a constant term and any of the polynomial-interaction terms that were used in the estimation of equation 1.4.5.
Applying this general two-stage least squares setup to the particulars of this paper, we have

\[ L_{it} = \gamma_1 + \phi_1 \tilde{V}_{it} + \phi_2 \bar{V}_{it} \tau_{it} + \pi \tau_{it} + \varepsilon_{1it} \]  \hspace{1cm} (1.4.6)

\[ \hat{\sigma}_{it}^2 = \gamma_2 + \phi_3 \tilde{V}_{it} + \phi_4 \bar{V}_{it} \tau_{it} + \rho \hat{L}_{it} + \varepsilon_{2it} \]  \hspace{1cm} (1.4.7)

where equation 1.4.6 is the first-stage regression, with \( L_{it} \) being a dummy variable for option listing for stock \( i \) and time \( t \). The \( \gamma \) term is a constant term. The variable \( \tilde{V}_{it} \) is the centered (at zero) running variable. The variable \( \tau_{it} \) is a dummy variable that equals 1 if the 2.4 million threshold is met, and zero otherwise. The \( \varepsilon \) term is the classic error term. Equation 1.4.7 is the second-stage regression, where \( \hat{\sigma}_{it} \) is the GARCH volatility estimate for stock \( i \) at time \( t \). The variable \( \hat{L}_{it} \) is the fitted values from the first stage regression (equation 1.4.6). As a robustness check, I include varying polynomials of the centered running variable as well as interactions of the threshold variable with the running variable. The causal estimate of option listing is \( \rho \).

Regression discontinuity designs are typically associated with cross-sectional data. With panel data, Lee and Lemieux (2010) recommend treating the data as a pooled cross section since fixed effects are “unnecessary for identification.” The panel nature of the data is nevertheless useful since it will allow for the possibility of analyzing two-way clustered standard errors. The panel nature of the data is also useful—indeed necessary—to create the running variable since it requires looking back 12 months; it is also necessary to estimate GARCH volatility. Additionally, the panel nature of the data allows me to run robustness checks that would otherwise be unavailable. We should not expect to see, for example, a discontinuity in stock return volatility on lagged price, for instance.
1.5 Results

1.5.1 Visual RD and 2SLS Estimates

Since a fuzzy regression discontinuity design can be thought of as a manifestation of instrumental variables (IV), the effect of option listing on underlying volatility can be estimated through a two-stage least squares (2SLS) regression. The first-stage is a linear probability model (LPM) regression of option listing on a constant term, the threshold that corresponds to eligibility, a set of various polynomials, and a set of interaction terms. The second-stage is a regression of return volatility on a constant term, the fitted values from the first stage, and the same set of various polynomials and interaction terms from the first stage.

As is custom in a regression discontinuity design study, I plot the “visual” first stage for the volume threshold requirement, which can be seen in Figure 1.1 below. The left panel plots the probability of option listing against the 12-month cumulative aggregate volume threshold. The right panel plots the probability of option listing against the 90-day $7.50 median share price threshold. The visual plots strongly suggest that there is a jump (and a kink) at 2.4 million shares, but not much of a jump in the median-share-price threshold. To be sure, the jump that is evident in the left panel is only about 0.05 percentage points. Nonetheless, the jump may be enough to bring about a statistically-significant effect on volatility.

There are two running variables in this setup, each creating its own eligibility threshold. Khanna (2015) lists four ways of estimating treatment effects in an RD framework with multiple thresholds. First, estimate the treatment effect for each threshold separately, ignoring the other. Second, limit the sample to the treatment group that meets the first threshold in order to estimate the second, i.e., to estimate the local average treatment effect using the volume threshold, limit the sample to those stocks that meet the median price threshold. Third, estimate one specification with running variables and polynomials for each threshold. Fourth, estimate one specification by using some distance measure with respect to both thresholds. Khanna finds that the third and fourth methods perform the best with respect to biasedness in a monte carlo study.
Figure 1.1: Visual RD for 2000-2002

Notes: Both panels plot the “first-stage” visual results for daily data on years 2000-2002. To be sure, the vertical axis of each panel is different with regard to its range. The left panel plots the SEC underlying trade volume eligibility threshold, which states that a stock must have a cumulative total of 2.4 million shares in the underlying market over the 12 months prior to option listing. The right panel plots the SEC underlying share price eligibility requirement, which states that a stock must trade at $7.50 per share for the majority of the previous 90 days prior to option listing (coded as a 90-day rolling median). For visual clarity I plot the respective arithmetic centers of the running variables. Both panels have 50 bins on each side of the cutoff, a number that was chosen arbitrarily, again for visual clarity. It is evident that there is a break at 2.4 million. The visual plots strongly suggest that there is a discontinuity (and a kink) at 2.4 million. Though to be sure, it is worth noting that the break is only about 0.05 of a percentage point. The share-price eligibility threshold does not seem to show any meaningful jump. In either case, it is worth cautioning against inferring too much from visual evidence alone.

To estimate this causal effect, I run two-stage least squares regressions for polynomial and interactions of degrees 1 through 4. Table 1.1 below has the main results of the study: regression coefficients of the reduced-form, first-stage, second-stage, and baseline OLS.
Table 1.1: Causal Estimates of Option Listing on Volatility

<table>
<thead>
<tr>
<th>Model</th>
<th>OLS</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reduced-Form</td>
<td>-</td>
<td>-0.28**</td>
<td>-0.30**</td>
<td>-0.24*</td>
<td>-0.11</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>First-Stage</td>
<td>-</td>
<td>0.46***</td>
<td>0.43***</td>
<td>0.40***</td>
<td>0.38***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Second-Stage</td>
<td>-</td>
<td>-0.62**</td>
<td>-0.71**</td>
<td>-0.59*</td>
<td>-0.29</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.30)</td>
<td>(0.33)</td>
<td>(0.35)</td>
<td>(0.37)</td>
</tr>
<tr>
<td>Baseline OLS</td>
<td>1.27***</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%
b The results in the table are the global sample: 4,609,735 observations.
c The numbers in the table are coefficient estimates from various models. The OLS model estimates volatility on a constant term and the listing dummy. The reduced form is a regression of volatility on a constant term, the volume threshold dummy and polynomial-interaction terms. The first stage is a regression of the listing dummy on a constant term, the volume threshold dummy, and polynomial-interaction term. The second stage is a regression of volatility on a constant, the fitted values of the first stage, and polynomial-interaction terms. Assignment variable centered at the cutoff
d The interpretation of the reduced form, second stage, and baseline OLS is percentage of volatility (annualized GARCH). The interpretation of the first stage is probability of option listing.
e The columns labeled (1), (2), etc., represent the order of polynomial as well as the corresponding interaction term. So (2) is the specification of equations 1.4.6 and 1.4.7, with the inclusion of a second-order polynomial as well as two interaction terms, that is, one for each polynomial term.
f Robust standard errors are in parentheses
1.5.2 Discussion

The first row of Table 1.1 shows the estimates of the reduced-form regression for varying degrees of polynomials and interaction terms.\(^{20}\) The reduced-form regression is the dependent variable (conditional volatility) regressed directly on the IV (2.4 million threshold) and covariates. The importance of the reduced-form estimates is that it allows the IV to affect the dependent variable through any channel—through the endogenous variable or any other way. Loosely put, the reduced-form specification relaxes the exclusion restriction. Of course, if this assumption is relaxed and there is no statistically-discernable effect, then there is likely no effect at all. As Angrist and Pischke (2009) quip: ‘if it ain’t in the reduced form, it ain’t there’.

The reduced-form results from Table 1.1 show that the 2.4 million threshold does indeed have a statistically-discernable effect on the conditional volatility. The effect is robust to the inclusion of the third-degree polynomial and interaction term. These are promising results; they give license to study the issue further, e.g., by imposing the exclusion restriction to estimate the causal effect of option listing.

The second row of Table 1.1 shows the estimates of the first-stage regression. The first-stage results can be thought of as an informal test of the relevance of the IV. The results show that meeting the 2.4 million threshold leads to a 0.4% increase in the probability of option listing, a finding that corroborates the Visual RD plot in the left panel of Figure 1.1. The F-statistics on the first-stage regressions are all well above 3,000, which shows that the first-stage regression results are meaningful—the IV is relevant, even if the magnitude is small.

\(^{20}\) The columns labeled (1), (2), etc., represent the order of polynomial as well as the corresponding interaction term.
The third row shows the causal estimates of option listing—the $\rho$ from equation 1.4.7, while the fourth row shows the baseline OLS estimate. The sign on the OLS estimate is positive while the sign on the IV estimates are all negative. This sign reversal is evidence that there is indeed selection bias: options exchanges choose to list high-volatility stocks. The results show strong evidence that option listing causes a statistically-meaningful decrease in annualized conditional return volatility of approximately 0.7%. To be sure, the effect is quite small: the mean volatility for the entire sample is approximately 55%. With over 4.6 million observations, it’s likely that even small patterns are picked up by the regression estimates.\footnote{The standard errors in Table 1.1 are all heteroskedasticity-corrected robust. I did not cluster the standard errors. Abadie, Athey, Imbens, and Wooldridge (2017) argue “[t]he researcher should assess whether the sampling process is clustered or not, and whether the assignment mechanism is clustered. If the answer to both is no, one should not adjust the standard errors for clustering” (emphasis theirs). The answer to both situations is indeed “no” for the present study and thus I do not report clustered standard errors. For the sake of candor and continuing with the still-orthodox view with regard to clustering, I cluster the standard errors with two-way clustering based on industry (two-digit SIC code) and month-year. I also cluster the standard errors with one-way clustering based on SIC-month-year. To be sure, the standard errors increase significantly—so much so that much of the statistical significance disappears. These results are available upon request.}

1.6 Supplemental Analysis and Robustness

1.6.1 Threshold Exogeneity

Even though the 12-month cumulative trade volume option listing standard was created by and enforced by the SEC, it is nevertheless possible that the threshold is not exogenous if firms can manipulate their 12-month cumulative trade volume in order to become listed in the options market. In principle, this would be difficult to do. Nonetheless, it is at least conceivable that firms want to be listed in the options market because it sends a signal about the viability of the company; being listed in the options market gives the company’s stock some legitimacy, as it were. Additionally,
it could be that firms desire to have their stock listed in the options market because management can use option prices to infer investor sentiment regarding management’s performance. This is more than a mere hypothetical: Blanco and Wehrheim (2017) find that the opening of an options market leads to a more efficient allocation of a firm’s resources.

Conversely, it is also conceivable that a firm may not want to be listed, if, say, management fears that listing leads to an increase in volatility of their firm’s stock. Moreover, there is some evidence that options trading leads to an increase in a firm’s borrowing cost as measured by its bond spread (see Blanco and Garcia (2017)).

One way to check if firms are manipulating their way into- or out of treatment is to check for “bunching” around the 2.4 million threshold. If, for example, the density increases around the 2.4 million cutoff, then there is strong evidence that firms are manipulating their 12-month cumulative trade volume in order to be listed in the options market. Of course, if bunching is happening, this itself would be an interesting economic phenomenon.

Figure 1.2 below contains a visual check for bunching. The top panel of Figure 1.2 plots the unconditional density of the running variable, while the bottom panel plots the conditional density (conditional on being listed). The vertical line denotes the 2.4 million threshold.
Figure 1.2: Density Plots of the Running Variable

a. Unconditional Density Plot of 12-Month Cumulative Trade Volume

b. Conditional Density Plot of 12-Month Cumulative Trade Volume
If firms are manipulating this running variable, then we should expect density to be highest around the threshold. This plot is evidence that bunching is not happening and therefore, the threshold value of 2.4 million shares is exogenous. Firms that are not listed (solid line) may want to avoid listing. If so, we would expect to see density peak just to the left of the threshold. This does not appear to be the case. On the other hand, unlisted firms may want to be listed. If so, we would expect to see density peak just to the right of the threshold. This, too, does not appear to be the case. Furthermore, the density of the already-listed firms (dashed line) does not peak near the 2.4 million threshold. To summarize, there does not seem to be manipulation of the threshold value, which means that we can treat the threshold as exogenous.

Another check of threshold exogeneity is to examine whether there are discontinuities in pre-treatment variables. For instance, suppose that the manager of a non-listed firm wanted to be listed in the options market. In order to meet the volume threshold, the firm may issue new shares in an attempt to push down the stock price, which would induce new trades, thereby boosting—or eventually boosting—the 12-month cumulative underlying trade volume.

The top panel of Figure 1.3 plots the contemporaneous shares outstanding against the running variable, with the vertical line denoting the 2.4 million threshold. The bottom panel of Figure 1.3 plots the one-day lagged share price against the running variable, with the vertical line denoting the 2.4 million threshold. In neither panel of Figure 1.3 does there appear to be any discontinuity at the 2.4 million threshold, a finding that supports Figure 1.2, and is further evidence that firms are not manipulating the running variable in an attempt to become listed (or avoid listing).
Figure 1.3: Cutoff at Pre-Treatment Covariates

a. Shares Outstanding Plotted Against Running Variable

b. Lagged Stock Price Plotted Against Running Variable
1.6.2 Alternative Measures of Volatility

In the above analysis I assumed a GARCH (1,1) process with a normal distribution. However, a well known empirical fact about asset returns is that they have “fat tails.” In order to control for this possibility, I also run the analysis assuming a GARCH (1,1) process assuming a Student’s t distribution, which has “fatter tails” than the normal distribution. Furthermore, another empirical regularity with stock volatility is the presence of asymmetry with respect to information—volatility changes more with negative “news” than it does with positive news. If we take positive news to be a positive residual, and negative news to be a negative residual, then I can include a dummy in the GARCH estimation procedure that captures this asymmetric effect (also known as “leverage effects”). This estimation of volatility is known as GJR-GARCH or TGARCH (for “threshold” GARCH). I loop through all of the CUSIPs in order to estimate GJR-GARCH (1,1) volatility assuming a normal distribution and then again assuming a Student’s t distribution. As yet another measure of volatility, I also estimate the effects on the intraday range. The results are in Table 1.2.

The results from Table 1.2 corroborate some of the findings from the main results of Table 1.1. For example, the OLS result from Table 1.2 shows a positive effect of option listing, which, like the OLS result from Table 1.1, is evidence of selection bias. The IV results in Table 1.2 demonstrate this selection bias since since the IV regression results all show a negative coefficient. Moreover, the magnitudes from the various GARCH estimations are somewhat consistent in that they show a small, negative effect. The overarching conclusion, then, is that option listing causes a slight decrease in return volatility.
### Table 1.2: 2SLS Estimates of Listing on Alternative Volatility Measures

<table>
<thead>
<tr>
<th>Volatility</th>
<th>Mean (SD)</th>
<th>OLS</th>
<th>Highest Polynomial &amp; Interaction Term</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>GARCH-t</td>
<td>76.78</td>
<td>1.21***</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>(58.87)</td>
<td>(0.06)</td>
<td>(0.31)</td>
</tr>
<tr>
<td>GJR-GARCH-n</td>
<td>75.97</td>
<td>1.18***</td>
<td>-1.06***</td>
</tr>
<tr>
<td></td>
<td>(52.92)</td>
<td>(0.05)</td>
<td>(0.28)</td>
</tr>
<tr>
<td>GJR-GARCH-t</td>
<td>74.15</td>
<td>1.22***</td>
<td>-0.26</td>
</tr>
<tr>
<td></td>
<td>(52.84)</td>
<td>(0.05)</td>
<td>(0.28)</td>
</tr>
<tr>
<td>Range</td>
<td>21.86</td>
<td>1.47***</td>
<td>13.19***</td>
</tr>
<tr>
<td></td>
<td>(12.17)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%
b The results in the table are the global sample: 4,609,735 observations.
c The numbers in the table are 2SLS coefficient estimates of the effect of option listing on alternative measures of volatility. Assignment variable centered at the cutoff.
d GARCH-t is a GARCH(1,1) process with the assumption of Student’s t distribution. GJR-GARCH-n is a GARCH(1,1) process with the assumption of a normal distribution; GJR-GARCH-t is the same, but assumes a t-distribution. The intraday range is the log of the ratio of intraday high to intraday low. Parkinson (1980) argues that this is a measure of volatility; and financial economics textbooks concur, see e.g., Brooks (2008), but some argue it is a hybrid measure of both volatility and spread (see Corwin and Schultz (2012)).
e Robust standard errors are in parentheses.
To be sure, though, there are some key differences between the results of Tables 1.1 and 1.2. For instance, in Table 1.2 none of the results that assume a t-distribution are statistically different from zero. Additionally, the intraday range estimate of volatility is completely out of sync with any of the GARCH measures: the range estimates are all positive, statistically significant, and are of a relatively large magnitude. Of course, this could be an indication that the intraday range is more of a measure of bid-ask spread rather than it is of volatility (see Corwin and Schultz (2012) for a discussion on the intraday range being a hybrid measure of volatility and spread). The standard errors in Table 1.2 are the same as they are in Table 1.2.

1.6.3 Local Linear Regression

Table 1.3 shows the estimates of the treatment effect of option listing for an ever-narrow bandwidth around the 2.4m cutoff. As stocks move away from the 2.4m cutoff, the treatment effect gets weaker and weaker. The inclusion of the polynomial terms can help control for this weakening away from the cutoff, but the best way to examine the effect of threshold-induced option listing is to focus on those stocks for which the threshold has bite—that is, the stocks that are very close to the cutoff. The estimation procedure is to estimate the treatment effect (via 2SLS) for an increasingly-narrow window about the cutoff. One potential problem with this bandwidth estimation procedure is that the sample size shrinks dramatically, which leads to wider confidence intervals—a manifestation of the bias-variance trade-off.

The results in Table 1.3 show that in the interval of 0 to 4.8m shares of 12-month cumulative trade volume, option listing causes a statistically-significant decrease in volatility, regardless of how volatility is measured; the sole exception is the intraday range measure, which, as mentioned above, is a hybrid measure of volatility and spread (see Corwin and Schultz (2012)). As a promising sign for the robustness of
Table 1.3: Local Linear 2SLS Regression Results for Increasingly-Narrow Bandwidths

<table>
<thead>
<tr>
<th>Bandwidth: Shares (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2,400,000 (100%)</td>
</tr>
<tr>
<td>300,000 (12.5%)</td>
</tr>
<tr>
<td>150,000 (6.25%)</td>
</tr>
<tr>
<td>100,000 (4.17%)</td>
</tr>
<tr>
<td>75,000 (3.13%)</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>1,333,479</td>
</tr>
<tr>
<td>148,341</td>
</tr>
<tr>
<td>74,480</td>
</tr>
<tr>
<td>50,028</td>
</tr>
<tr>
<td>37,731</td>
</tr>
</tbody>
</table>

Volatility Measure

<table>
<thead>
<tr>
<th>Volatility Measure</th>
<th>GARCH-n</th>
<th>GARCH-t</th>
<th>GJR-GARCH-n</th>
<th>GJR-GARCH-t</th>
<th>Range</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-30.34***</td>
<td>1016.6***</td>
<td>134.57</td>
<td>77.45</td>
<td>-226.31</td>
</tr>
<tr>
<td></td>
<td>(11.43)</td>
<td>(271.4)</td>
<td>(125.04)</td>
<td>(142.27)</td>
<td>(275.31)</td>
</tr>
<tr>
<td></td>
<td>-38.54***</td>
<td>1056.2***</td>
<td>83.029</td>
<td>-28.95</td>
<td>-377.44</td>
</tr>
<tr>
<td></td>
<td>(11.79)</td>
<td>(282.1)</td>
<td>(124.97)</td>
<td>(143.39)</td>
<td>(303.98)</td>
</tr>
<tr>
<td></td>
<td>-47.15***</td>
<td>940.46***</td>
<td>47.90</td>
<td>-40.96</td>
<td>-371.96</td>
</tr>
<tr>
<td></td>
<td>(10.73)</td>
<td>(252.67)</td>
<td>(111.92)</td>
<td>(130.44)</td>
<td>(290.83)</td>
</tr>
<tr>
<td></td>
<td>-40.69***</td>
<td>915.27***</td>
<td>62.12</td>
<td>-104.07</td>
<td>-227.92</td>
</tr>
<tr>
<td></td>
<td>(10.80)</td>
<td>(248.90)</td>
<td>(113.79)</td>
<td>(132.91)</td>
<td>(246.59)</td>
</tr>
<tr>
<td>Range</td>
<td>5.79**</td>
<td>35.53</td>
<td>-115.78***</td>
<td>-34.04</td>
<td>0.34</td>
</tr>
<tr>
<td></td>
<td>(2.81)</td>
<td>(36.88)</td>
<td>(37.52)</td>
<td>(35.23)</td>
<td>(0.64)</td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%

b The bandwidth is the number of shares on either side of the 2.4m cutoff. So a bandwidth of 2,400,000 limits the sample to those stocks that have a 12-month cumulative trade volume between 0 and 4,800,000; a bandwidth of 300,000 shares limits the sample to the interval (2.1m, 2.7m), and so on. The percentage (%) is the percentage of the cutoff amount: 150,000 is 6.25% of 2.4m, 75,000 is 3.13% of 2.4m, etc.

c The numbers in the table are 2SLS coefficient estimates of the effect of option listing on all of the measures of volatility. These results come from the specification of a polynomial of order 1 with one interaction term (plus a constant term and the running variable). Following Gelman and Imbens (2016) I do not include higher-order polynomials for the results in this table.

d GARCH-n is a GARCH (1,1) process with the assumption of a normal distribution. GARCH-t is a GARCH (1,1) process with the assumption of Student’s t distribution. GJR-GARCH-n is a GARCH (1,1) process with the assumption of a normal distribution; GJR-GARCH-t is the same, but assumes a t-distribution. ‘Range’ is intraday range, which is the log of the ratio of intraday high to intraday low. Parkinson (1980) argues that this is a measure of volatility, and financial economics textbooks concur, see e.g., Brooks (2008), but some argue it is a hybrid measure of both volatility and spread (see Corwin and Schultz (2012)).

e Robust standard errors are in parentheses.
these estimates, the magnitudes are all roughly close, ranging from a 30% decrease in volatility to a 47% decrease, which, in addition to being statistically significant, is economically significant—a 30% drop in volatility is certainly economically meaningful. Moreover, in this (0, 4.8m) window, the treatment effects are more reliable than the treatment effects from the global estimates since the outliers with respect to 12-month cumulative trade volume are not included in this bandwidth.

The robustness of the findings disappears, however, as we move rightward in Table 1.3: going from the 12.5% bandwidth to the 0.42% bandwidth, the estimates vacillate wildly, ranging from a very large statistically-significant increase in volatility (12.5% bandwidth) to a very large statistically-insignificant decrease in volatility (0.42% bandwidth). The variation in these results could be attributable to the dramatic decrease in the number of observations. As the sample size shrinks, each observation left in the sample carries more weight. Thus, it may be worthwhile to eliminate any potential outliers. However, the elimination of outliers inevitably introduces subjectivity regarding the definition of ‘outlier’. Therefore, an alternative comparison would be to compare the median of stock volatility of those stocks that are just to the left of the cutoff with those that are just to the right.

1.7 Conclusion

In this paper I estimate the causal effect of option listing on underlying stock return volatility. Since volatility is one of the dimensions along which option listing happens, there is a problem of selection bias. I overcome this selection bias by exploiting the exogenous variation in option listing that is created by an SEC-imposed option listing standard, which states that a stock must have a minimum 12-month cumulative underlying trade volume of 2.4 million shares prior to option listing. I use
this eligibility requirement as an instrument for option listing: eligibility is related
to listing, but is unrelated to volatility. Furthermore, the 2.4 million threshold is
unlikely to affect volatility except through option listing. Therefore, the threshold
randomly assigns stocks into the treatment group (option listing). The exclusion re-
striction is met since it is unlikely that the arbitrary threshold could increase volatility
through a channel other than option listing. Results show that option listing causes a
statistically-significant decrease in return volatility, a finding that corroborates many
of the empirical papers on this question, as well as confirming many of the formal
theoretical models of the volatility effects of option listing.

Although the findings above are statistically significant, they do not seem to be
economically significant—or at least they are not consistently economically signifi-
cant. It may be the case that GARCH-measured volatility at the daily frequency is
too noisy to accurately estimate with any consistency. One potential avenue of future
research, then, is to eliminate the year 2000 from the sample since this year was the
year of the bursting of the tech bubble.

A related strand of research would be to estimate the causal effect of option listing
in a market that is incomplete. Mayhew and Mihov (2004) argue that an important
economic role for equity options is completing the market (Ross (1976)). But it may
be that by the year 2000, the market was already “fully” complete, as it were. Indeed,
Mayhew and Mihov (2004) find that the volatility effect of option listing is weaker in
the 1990s than it was in the 1980s, a finding that they attribute to the idea that by
the 1990s, the market was much more complete than it was in the 1980s. It could be,
then, that if option listing does indeed cause a decrease in option listing, this should
be even more evident in a time period when the market was relatively incomplete,
e.g., the 1980s—and especially the early 1980s, before the introduction of index op-
tions.
Yet another avenue of future research is to focus on the effect that option listing has on bid-ask spread rather than volatility. Danielsen et al. (2007) find that while volatility is indeed an important determinant of option listing, the underlying bid-ask spread is an even more important determinant. Spread is important because it is a measure of liquidity, which, according to anecdotal evidence from options traders, is by far the most important aspect of making a market. The wider the spread, the more costly it is to move into and out of the stock. Since market makers engage in delta hedging to cover their options position, they want a stock with a lower spread because trading costs are lower. In addition to being a measure of liquidity (and therefore trading cost), the bid-ask spread can also be thought of as a measure of informed trading. All else equal, a wider spread could mean that the underlying market makers fear trading against an informed trader, e.g., a hedge fund manager, and thus the market-maker widens the spread to compensate for that risk (Glosten and Milgrom (1985)). The theoretical model of H. Cao (1999) formalizes this possibility, and Hu (2017) confirms it empirically by measuring the probability of informed trading (PIN) before and after option listing compared to a before-and-after comparison of control stocks (non-option listed stocks).

As a final avenue of future research, it would be interesting to study the effect of option listing on volatility in a setting with a higher degree of external validity. One criticism of regression discontinuity designs is that while they have a strong case for internal validity, they often fail along the lines of external validity. Specifically, as in any instrumental variables-based study, the average treatment effects are local: on average, option listing causes a decrease in volatility for those stocks affected by the threshold. Hence, my findings above do not necessarily hold for stocks like Apple, Boeing, and Wal-Mart since their volatility is unaffected by the 2.4m threshold. One way to achieve a greater degree of external validity—and thus a greater degree
of generalizability—is to fully exploit the time dimension of the data in a staggered difference-in-differences (two-way fixed effects) framework. That is, compare the underlying return volatility of all option-listed stocks before and after option listing compared to all non-option-listed stocks before and after option listing, controlling for share-effects as well as time-effects. In a current working paper, I study this exact question and I find that option listing causes a decreases in volatility, a result that corroborates the findings in the present paper.
CHAPTER 2

THE EFFECT OF EQUITY OPTION LISTING ON UNDERLYING VOLATILITY: EVIDENCE FROM FIRM FIXED EFFECTS

2.1 Motivation

In Section 1.1 above, I noted the need for an empirical examination of the stabilizing/destabilizing nature of a new options market. I further noted that previous empirical studies were beset by the problem of selection bias: options listing is determined, in part, by the volatility properties of the underlying stock. In the present chapter, I employ an alternative identification strategy: firm fixed effects.

The need for an alternative—or rather, an additional—identification strategy stems from the interpretation of the estimates in a fuzzy regression discontinuity design. In essence, the fuzzy RD framework uses the discontinuity in the running variable as an instrument for the endogenous variable. Accordingly, any causal interpretation of the estimates is most persuasive for the treated units for which the discontinuity changes their treatment status. In other words, the estimates from Chapter 1 are causal, but only for those stocks that have a 12-month cumulative
underlying trade volume “close to” 2.4 million shares.\footnote{Actually, Lee and Lemieux (2010) argue that the treatment effect in an RD framework is a weighted average treatment effect across all units in the sample. That is, we can think of the fuzzy RD estimation procedure as weighting those observations close the 2.4 million cutoff more heavily. In any case, it is clear that RD results are most convincing for those observations near the threshold.} Put another way, RD has very strong internal validity, but much weaker external validity—the “strength” of the causality diminishes as we move “away from the threshold.” To fix ideas, Steinway (the musical instruments manufacturer) is near the 2.4 million threshold, and hence has the potential to be affected by the option listing standards. A firm like Coca-Cola, on the other hand, would be virtually unaffected by the option listing standards since it has a 12-month cumulative underlying trade volume of more than one billion shares.

Therefore, in order to be able to speak more broadly about the effect of option listing on underlying volatility, I estimate a two-way fixed effects model.\footnote{This model is sometimes known as difference-in-differences (Baltagi (2013)) with staggered policy implementation. In the context of the present paper, the “policy” is option listing. Also, to be sure, I am using individual, firm-level data rather than data that is aggregated at some higher level, e.g., country, state, etc.} The benefit of this increase in external validity comes at the cost of stronger assumptions, e.g., fixed unobservable heterogeneity, no simultaneity between the treatment- and dependent variable.

As mentioned in Section 1.1, previous empirical studies use a matching function in an attempt to estimate the effect of option listing on underlying volatility. There, I noted that matching is a selection-on-observables identification strategy. I argued that options exchanges may list along unobservable dimensions such as name recognition. The two-way fixed effects model, conversely, is a selection-on-unobservables identification strategy. It models the unobservable heterogeneity of stocks by including of a dummy for each firm in the sample (as well as a dummy for each time period). A key assumption of this strategy is that the unobserved heterogeneity is constant
over time. This assumption can be checked by including firm-specific time trends, which serves as a way to allow for time-varying heterogeneity.

To be sure, many previous empirical studies do estimate a two-way fixed effects model (along with a matching function), but their individual-level fixed effect is at the industry level, typically the two-digit SIC code. In this chapter I show that controlling for industry fixed effects is insufficiently granular to estimate the effect of option listing on underlying volatility. I estimate a two-way industry fixed effects model and find no effect of option listing. However, when the individual fixed effect is at the firm level, there is a statistically meaningful change in volatility. In particular, I find that option listing is associated with a 0.10 standard deviation decrease in underlying return volatility. This finding of a decrease corroborates many of the previous empirical studies, but overturns the results of those studies that find an increase or no effect (see Section 1.2.2).

The paper proceeds as follows. Section 2.2 describes the data. Section 2.3 describes the methodology. Section 2.4 discusses the results. Section 2.5 contains robustness checks. Section 2.6 concludes and presents avenues of future research.

### 2.2 Data

I collect several data sets from several sources. From CRSP (via WRDS) I collect firm-identifying information such as company name, ticker, share type, CUSIP, PERMCO, exchange code, and industry. The second type of data that I collect (also from CRSP) is trading data such as the intraday low, intraday high, final closing price, and trading volume.

I collect options listing dates from the Options Clearing Corporation’s (OCC) website. The OCC is the central clearinghouse for all of the equity options exchanges.
in the US (options are fungible across the exchanges as a result of sharing the OCC as the central clearinghouse). These dates allow me to create a dummy variable for each time period that an option was listed for a stock.

To be sure, the OCC only denotes new listings as of February 14, 2001. Since the time period of the study is January 3, 2000 - December 29, 2006, I collect the exhaustive list of all options up to December 31, 1999 from the 1999 OCC Annual Report. I collect the set of new option listing dates for the year 2000 and for the first six weeks of 2001 from the archived web pages of the options exchanges using the Wayback Machine.\footnote{https://archive.org/web/}

The frequency of this study is monthly, but option listing happens at the daily frequency. The option listing dates are the principal variable of interest. I aggregate the daily data to the monthly level and count any option listing in a month as a listing for the entire month. For example, if an option listing happens on April 30, then I count all of April as an option listing month. The necessity of such data construction could pose problems since I am aggregating volatility for the entire month. However, with a sample size as large as mine, the end-of-the-month listing dates are likely to be “averaged out” by the listing dates that happen on the first of the month. In any case, if this data construction poses a problem, then it should show up in the “event study” analysis in the robustness section below (Section 2.6) since I will be controlling for previous month.

Regarding the dependent variables, underlying stock return volatility, I collect data from CRSP. Specifically, I retrieve daily data on adjusted return (adjusted for stock splits and dividends). From Kenneth French’s website, I collect the adjusted risk-free rate of return as well as the market return.\footnote{mba.tuck.dartmouth.edu/pages/faculty/ken.french/index.html} I subtract the risk-free rate from
the daily return, yielding a measure of excess return. The result of these calculations is a measure of daily excess stock return volatility. Then, I scale up this measure by 10,000. I rescale for two reasons. First, I want to avoid the cumbersome scientific notation. Second, and more importantly, since daily returns are so small, I run the risk of the computer truncating decimal points. I then take the standard deviation of this measure for each month. As a final step I standardize the measure for a cleaner interpretation of the results.

For the sake of robustness, I use the intraday range (daily high minus daily low) as an additional measure of volatility (Brooks (2008)). The idea is that the spread of a series (dispersion about the mean) is a measure of volatility and the range is a measure of spread. Hence, the range is a measure of volatility. Parkinson (1980) argues that if a stock prices are a random walk, then the intraday range is a “superior estimate” of a stock’s volatility than the “traditional” measure of volatility of Merton (1980), which squares the difference of the log of the daily close price and then averages this measure over some time period such as a month (Poterba and Summers (1986)) or year (Pindyck (1988)). Bollerslev et al. (1992) say that Merton’s method does not make efficient use of all the data (at least not at frequencies lower than the daily level). For interpretability, I express the intraday range as a percentage of the daily high.

As a final dependent variable, I examine the effect of option listing on trading volume. I examine the effect on volume for two reasons. First, many previous studies examine volume. Second, volume is likely the channel through which option listing would affect volatility. Option listing may decrease transactions costs for short-sellers

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5 There may seem to be a conspicuous absence of GARCH estimation of volatility. I avoid GARCH estimation because the frequency under study is monthly and Alexander (2001) says that monthly frequency will smooth out the clustering of returns that GARCH is meant to fix. I limit the frequency of my study to the monthly level because daily stock data is noisy.
(Diamond and Verrecchia (1987)), which results in a change in trading volume. Option listing may incentivize investors to collect more information on the underlying security (H. Cao (1999)), which results in a change in trading volume. And so on. Therefore, a working hypothesis is that if there is an effect of option listing on volatility, there should also be a corresponding effect on volume.\(^6\) For interpretability I take the log of trading volume.

I merge the trade data and options listing data by stock ticker. Tickers are recycled over time, which creates the problem that a stock ticker does not uniquely pick out a stock. To overcome this problem I manually go through the merged file to ensure that the company names match.\(^7\) The definition of the firm is the PERMCO identifier from CRSP.\(^8\) I eliminate all REITs, ETFs, etc., and only keep regular shares (SHRCD 10 and 11 from CRSP).

### 2.3 Methodology

Selection into the treatment group (optioned stocks) potentially happens along unobservable dimensions, e.g., name-recognition of the underlying stock. If these

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\(^6\)My sample of stocks includes both NYSE and Nasdaq stocks. This is important because Nasdaq is a decentralized dealer market and thus their reported volume is double that of the NYSE stocks. Hence, strictly speaking, a proper adjustment to account for this difference in market structure is to divide all Nasdaq trading volume by two to account for Nasdaq’s ‘double counting’ as Atkins and Dyl (1997) argue. However, since I want to get an overall measure of the impact of option listing on volatility, I do not make such an adjustment in this paper.

\(^7\)The OCC data is very “dirty” so this name-ticker matching process needed to be done manually.

\(^8\)Of course, options are listed on shares of stock, e.g., CUSIP, not company, e.g., PERMCO. There are two reasons that I use PERMCO as the definition of firm. First, the OCC sometimes does not properly specify if an option is listed on a class A share of a stock or a class B share. PERMCO avoids this ambiguity since PERMCO identifies the underlying firm, regardless of share class. Second, in a fixed-effects specification, PERMCO is a superior measure of firm because it does not change due to, say, a bankruptcy. For example, General Motors had a vast debt restructuring and issued new shares of stock in 2008. This resulted in a new CUSIP number. However, the PERMCO identifier did not change. Thus, PERMCO ensures greater continuity.
unobservable dimensions are fixed over time, I can control for them by estimating
fixed-effect regressions. A brief, heuristically-formal exposition is as follows: Suppose
that we have a panel data regression model where we decompose the traditional error
term into three components: an individual-specific component, $\mu_i$; a time-specific
compONENT, $\lambda_t$; and a random component, $u_{it}$.

$$y_{it} = \alpha + x_{it}' \beta + \epsilon_{it}$$

$$\epsilon_{it} = \mu_i + \lambda_t + u_{it}$$

$$y_{it} = \alpha + x_{it}' \beta + \mu_i + \lambda_t + u_{it}$$  \hfill (2.3.1)

The intuition of this econometric model is that each individual in the sample has
an idiosyncratic effect on the dependent variable. The time-specific effect controls
common shocks experienced by all individuals in the sample. The implication of the
time-specific effect is that a temporal effect affects each individual in a similar way.

The coefficient $\beta$ in equation (2.3.1) can be consistently estimated by including
a dummy variable for each individual (e.g., firm) in the sample as well as each time
period (e.g., month-year) in the sample and then running OLS, a procedure often
known as ‘least-squares dummy variables’ estimation (LSDV). Of course, if the sam-
ple has lots of individuals, the LSDV approach may not be feasible. In my sample, for
instance, there are over 8,000 firms. Hence, LSDV estimation would require inverting
a matrix with hundreds of thousands of rows and thousands of columns, a task that
is not easily done on even the largest cluster of computers.

To be sure, there is another estimation procedure that is feasible. It is possible
to de-mean the data, i.e., take the within- and between-transformations of the data
and run OLS on the de-meaned sample. The intuition of this procedure is that the
unobserved, idiosyncratic effects are constant and thus each observation is equal to
its mean. Therefore, the de-meaning “wipes out” the constant term, the individual fixed effects, and the time effects.\textsuperscript{9}

I run two baseline regressions that serve as benchmark comparisons for the two-way fixed effects (2FE) estimates: pooled OLS and industry fixed effects. These baseline regressions do not control for firm fixed effects. Therefore, if there is any unobserved heterogeneity that confounds the option listing effect, it should manifest itself as a difference in the baseline estimates and 2FE estimates.

For the first baseline specification, I run a pooled OLS regression. By ‘pooled’ I mean that even though the data are panels, I “pool” them together to treat them as one cross-section, i.e., \( Y_i = \alpha + \beta L_i + \varepsilon_i \), where \( L \) is the listing dummy that denotes whether an observation is listed on an options exchange. The lack of a time subscript on the variables demonstrates the pooled nature of the specification. Because the pooled OLS is effectively a cross-section, I two-way cluster the standard errors (based on two-digit SIC codes and year).

The second baseline specification is an estimation of the option listing effect controlling for industry (2-digit SIC code dummies) and month-year dummies. In total there are 102 industry dummies and 84 month-year dummies. The estimations for this specification are run via OLS (LSDV), that is, a dummy for each industry along with a dummy for each month-year. This industry fixed effects model is common in empirical financial economics. For instance, Hu (2017) employs this strategy (along with a few covariates) in estimating the matching function that he uses to choose the control group of (non-optioned) stocks.\textsuperscript{10} The most granular industry definition is a

\textsuperscript{9}The de-meaning procedure assumes that panels are balanced, i.e., each individual has the same number observations per time period. I discuss the implications of having an unbalanced panel in the robustness section.

\textsuperscript{10}To be sure, Hu (2017) examines the probability of informed trading as his dependent variable rather than volatility, but his methodology and findings are relevant overall.
four-digit SIC code. In my sample, which ranges from the years 2000-2006, there are over 8,000 firms across 102 2-digit industries and about 1,000 4-digit industries. Thus, 2-digit SIC codes as a control for unobservable heterogeneity is likely not sufficiently granular, which prompts the need for firm fixed effects.

The main specification is a two-way fixed effects estimator (2FE), which estimates the coefficient of interest, controlling for individual-level (as opposed to group-level) unobservable heterogeneity as well as unobservable time-specific effects that are common to all firms in the sample. There is no need to control for other covariates in this specification since other covariates only serve to reduce the standard errors (Angrist and Pischke (2009)). Because I have such a relatively large data set, statistical precision is not much of a concern.

The 2FE model that I estimate is as follows:

\[ Y_{pt} = \alpha + \sum_{i=1}^{n} \beta_i PERMCO_{ip} + \sum_{j=1}^{J} \beta_j DATE_{jt} + \delta LIST_{pt} + \nu_{pt} \]  

where \( Y_{pt} \) is the dependent variable for firm \( p \) in time \( t \), \( \beta_i \) is the coefficient on each firm’s fixed effect, and \( PERMCO \) is the company. Thus, \( PERMCO_{7p} \) would be 1 for Apple’s stock (AAPL) since Apple’s PERMCO number is 7; Apple’s column in the data matrix will have a zero otherwise. Similarly, for the time effects, \( DATE \) is a month-year dummy that “switches on” when \( j = t \) and zero otherwise. The variable \( LIST \) is the variable of interest. It is a dummy variable that equals one when firm \( p \) is listed on an options exchange in time \( t \) and is zero otherwise. The variable \( \nu_{pt} \) is the error term.

\(^{11}\)An application of ‘two-way fixed effects’ is difference-in-differences (DID) with staggered implementation of the policy, which in this case is option listing. DID is also known as the “two-way error component model,” as described in Baltagi (2013, Chapter 2). The name of the latter comes from the notion that the error term can be decomposed into an individual-specific component as well as a time-specific component.
Since there are over 8,000 unique firms in my sample, an LSDV specification is not feasible. Thus, I estimate equation (2.6.2) using the lfe package in R by Gaure (2013), which iteratively runs the Fisch-Waugh-Lovell theorem to “sweep out the fixed effects.”

2.4 Results and Discussion

Table 2.1 has the results of the baseline benchmarks and the Two-Way (2FE) estimates for the LIST variable, all of which are run on a monthly sample of 457,166 observations at the monthly frequency from 2000 - 2006. Since the Pooled OLS estimates are treated as cross-sectional data, I two-way cluster the results by two-digit SIC industry code and year. The Industry and Two-Way standard errors are both clustered at the two-digit SIC industry code. Clustering at the higher level (industry), rather than at a lower level (firm), is the more conservative way to cluster standard errors (Cameron and Miller (2015)). Cluster-robust standard errors are robust to heteroskedasticity and also allow for serial correlation for units within the cluster (Angrist and Pischke (2009)). The measure of excess return volatility is standardized, i.e., in units of standard deviations. Since trading volume is logged, the estimates represent a percentage change in trade volume. And the intraday range in percentage points since it is the intraday range expressed as a percent of the intraday highest intraday price.

The results show that the effect of option listing on volatility is statistically less than zero. Nonetheless, it’s not clear that a movement of 0.10 standard deviations is economically significant. The intraday range measure—also a measure of volatility—declines with option listing by about 0.64 percentage points. The negative coeffi-

\[12\] Together, these two findings support Mendelson (1985)’s model, which shows that increasing
Table 2.1: Baseline Estimates of Listing on Volatility

<table>
<thead>
<tr>
<th>Dependent Variables</th>
<th>Pooled OLS</th>
<th>Industry Fixed Effects</th>
<th>Firm Fixed Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td>Volatility</td>
<td>-0.05</td>
<td>-0.006</td>
<td>-0.10***</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.005)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Volume (log)</td>
<td>3.11***</td>
<td>2.80***</td>
<td>0.68***</td>
</tr>
<tr>
<td></td>
<td>(0.26)</td>
<td>(0.14)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Intraday Range</td>
<td>-0.53</td>
<td>-0.93***</td>
<td>-0.64***</td>
</tr>
<tr>
<td></td>
<td>(0.44)</td>
<td>(0.10)</td>
<td>(0.08)</td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%
b Volatility is the monthly standardized historical return volatility of the underlying stocks, i.e., the standard deviation of the monthly series of daily excess returns, standardized for interpretation. Volume is underlying trade volume, logged for interpretation. Intraday range—as a measure of spread—is another measure of volatility. It is the standard deviation of the monthly series of daily price range.
c Pooled OLS treats the data as a pooled cross-section. For industry fixed effects, industry is defined as the two-digit SIC code. For firm fixed effects, the firm is defined as the PERMCO identifier from the CRSP database. Fixed effects estimates include time fixed effects (month-year).
d Standard errors are cluster-robust at the industry level (two-digit SIC)
cients from the two-way estimation confirm most of the previous empirical studies on option listing effects, which show that option listing is associated with a decrease in return volatility (Skinner (1989), Conrad (1989), Detemple and Jorion (1990), Damodaran and Lim (1991)) as well as many of the theoretical models (Grossman (1988), Gennette and Leland (1990), Detemple and Selden (1991), H. Cao (1999)). Trade volume increases 0.68 percentage points as a result of option listing. Despite being an economically small number, this finding answers an important question: Are equity options a complement or a substitute for the underlying security? There are several reasons why it is possible to view options as a complement for the underlying asset, i.e., increases the trading in the underlying asset. First, an options market for a stock allows investors to purchase insurance on that stock, or partial out the risk of just one stock from a portfolio. Second, options can allow investors to take advantage of differing capital gains tax rates (see L. Harris (2003, pg. 26)). Third, equity options are physically settled (as opposed to cash settled), meaning that options positions are settled by delivery of the underlying shares of stock. Thus, the existence of market makers in the options markets translates into higher trading volume in the underlying market. Finally, options allow the possibility of manipulation across the two markets, which increases trading volume in the underlying market as investors exploit these manipulation strategies (Jarrow (1994), Ni, Pearson, and Poteshman (2005)). As a specific example, on January 18, 2003—an expiration date for options contracts—Apple’s stock (AAPL) closed at exactly $500, which, coincidentally (or not), was the strike price of many expiring options contracts.13

13 The “or not” part was (is?) up for debate. Some in the popular press, including academics, argued that such a round closing price on an options expiration date is normal, while others argued that it was market manipulation. In any case, it is clear that this activity in Apple’s underlying market would not have happened if the market of Apple’s options did not
On the other hand, options can also be thought to be a substitute for the underlying security. An options market can pull short sellers out of the underlying market (Diamond and Verrecchia (1987)). Options offer traders more leverage and also have lower margin requirements, lowering the cost of options vis-a-vis the underlying market (M. O. Easley David and Srinivas (1998); John et al. (2003)), both of which work to pull traders out of the underlying market. Put simply, options compete with the underlying stock since both give the exposure to the asset’s volatility.

The findings in Table 2.1 offer evidence on which of the above competing effects “win out” on net. Option listing increases trading volume in the underlying asset by an average of 0.68 percentage points, showing that options are a net complement to the underlying stock. Trading volume holds a special place within the theoretical literature on the effects of option listing. If option listing is to have any effect on liquidity, volatility, probability of informed trading, share price, etc., it is likely to come by way of an increase or decrease in trading volume. In other words, volume is the “first stop,” as it were, on the channel through which option listing affects volatility, liquidity, etc. Put another way, it would be somewhat of a puzzle if option listing affected volatility, but did not have an effect on trading volume.

It is worth comparing the results across specifications. Specifically, the comparison of the pooled OLS results with the two-way results shows the importance of controlling for firm heterogeneity, including unobservable heterogeneity. The pooled OLS specification shows that, all else equal, option listing is associated with lower volatility and higher trade volume. After controlling for firm-specific effects, each of these effects is reinforced—volatility drops even more and volume increases even more.

The omitted variable bias formula can shed some light on this comparison

\[ E[\hat{\delta}] = \delta + \frac{\text{Cov}(\text{LIST, UNOBS})}{\text{Var}(\text{LIST})} \]

where \( E[\hat{\delta}] \) is the estimated effect of option listing on volatility from the pooled OLS specification, \( \delta \) is the true effect of option listing, and the second term on the right-hand side is the bias term. The bias term is composed of \( \gamma \), which is the effect of the unobservable variable on the dependent variable and the covariance of option listing and the unobservable variable. We can plug in some of the estimates from Table 2.1:

\[-0.5 = -0.10 + \text{Bias}\]

Thus, the omitted variable bias must be positive. Suppose that a relevant unobservable variable is name recognition of the stock. It is likely that options exchanges would want to list options on stocks that have high name recognition, independent of the stock’s volatility properties, trading volume, etc. If so, then \( \text{Cov}(\text{LIST, UNOBS}) > 0 \), which, in turn, must mean that \( \gamma > 0 \), i.e., name recognition must be positively related to volatility, volume, etc., which bodes well with intuition.

Table 2.1 also shows a relatively dramatic change in magnitudes when controlling for firm-specific effects as opposed to using industry-specific effects. Intuitively, industry specific effects are much too broad to capture firm-level heterogeneity, at least with respect to option listing. For example, the OSHA 2-digit SIC code for communications has cell phone carriers in the same class as radio broadcasting.
2.5 Supplemental Analysis and Robustness

2.5.1 Unbalanced Panels

With unbalanced panel data (i.e., some cross-sectional units are not observed for the entire sample period), the demeaning process that is so key to controlling for firm fixed effects is problematic, as noted in Baltagi (2013): “[i]f the $\mu_i$ and $\lambda_t$ [in model 2.4.1 above] are fixed, one has to run [the LSDV model]. Most likely, this will be infeasible for large panels” (pg. 198). Since the lfe package from Gaure (2013) only mentions unbalanced panels in passing, I check that the two-way estimates in Table 2.1 are robust by randomly sampling 260 unique firms (3% of the total number of firms) and running the LSDV specification of the two-way model. I repeat this process 10,000 times, each time recording the coefficient on listing. I then take the average of these estimates. I perform these steps for each of the dependent variables in Table 2.1. The results of this re-sampling procedure are nearly identical to the estimates from the two-way within estimation procedure. 14

2.5.2 Firm-Specific Linear Time Trends

The two-way fixed effects estimator controls for fixed unobservable heterogeneity. If option listing is a function of time-invariant unobservable dimensions, then the main results above need no further analysis. Of course, it’s possible that the unobserved heterogeneity is time-varying. Wooldridge (2002) says “the individual-specific trend is an additional source of heterogeneity.” Thus, it is important to control for the possibility of time-varying unobservable heterogeneity.

14The estimates from the sampling procedure are as follows: excess return standard deviation is -0.0994; log volume is 0.6741; the intraday range is -0.6411.
If the coefficient on listing is no longer significant after controlling for firm-specific trends, then one interpretation would be that the options exchanges choose to list options on stocks that are already have a rising/falling volatility. Skinner (1989) finds that option listing is associated with a decrease in volatility but intuits that the fall in volatility is possibly mean-reversion at work: stocks peak at a high volatility, the options exchanges see the peak, list those stocks, and then volatility falls; but, argues Skinner, it was going to fall anyway. In other words, mean reversion confounds the listing effect.

Mayhew and Mihov (2004) contest Skinner's interpretation. They find that option listing is associated with an increase in volatility, which they reason to be the options exchanges' volatility forecast coming to fruition: options exchanges forecast volatility for a host of stocks and list options on those that have a high volatility forecast. The volatility of those listed stocks increases just as forecast. Mayhew and Mihov (2004) argue that volatility is able to be forecast and that options exchanges should be as good as anyone at forecasting it.

Danielsen et al. (2007) empirically show that the bid-ask spread—and especially its trend—is the most important selection criterion of the options exchange, as demonstrated in their matching function; the authors find no option listing effect on volatility or liquidity. It is worth noting that the matching function of Danielsen et al. (2007) includes contemporaneous measures of bid-ask spread, price, and volatility. The relevance of these specific right-hand side variables and their contemporaneous nature could work as a ‘bad control’ as elaborated in Wooldridge (2005). By controlling for key contemporaneous variables, it is possible that Danielsen et al. (2007) “controlled away” any true effect. In other words, their matching function may have “blocked” the channels through which option listing affects volatility.
I control for firm-specific trends by estimating the following:

\[ Y_{pt} = \alpha + \sum_{i=1}^{n} \beta_i PERMCO_{ip} + \sum_{j=1}^{J} \beta_j DATE_{jt} + \delta LIST_{pt} \]

\[ + \sum_{i=1}^{n} \theta_i (PERMCO_{ip} \times t) + \nu_{pt} \]

(2.5.1)

where \( t \) is a linear time trend. For monthly data it is a number running from 1 to 84 (since there are 84 months from 2000-2006) for each firm in the sample. The results are in Table 2.2.

Table 2.2 shows that the estimates of the industry fixed effects model do not change much with the inclusion of industry trends. One conclusion of this finding is that to the extent that the options exchanges select stocks based on industry (2-digit SIC code), they don’t consider industry-wide trends. Contrast that finding with the firm-level estimates with the inclusion of firm-specific trends, where the options exchanges at least partly consider volatility trends when deciding to list a stock, which corroborates Danielsen et al. (2007).

The two-way specifications were run via the within transformation (lfe package) and with resampling via the LSDV specification. Unfortunately, the volume and intraday range measures timed out when run via the within estimation. As a remedy, I randomly sampled 87 unique firms from the sample (1% of all firms) and ran equation 2.5.1 above. I repeated the process 10,000 times and took the average of the coefficient on \( LIST \).

The excess return volatility measure falls by about half, but remains statistically less than zero. The coefficients on volume and intraday range also fall, the latter

\[ ^{15} \text{All standard errors are cluster-robust, with clusters being at the industry level (2-digit SIC code).} \]
Table 2.2: Random Trends Model

<table>
<thead>
<tr>
<th>Dependent Variables</th>
<th>Industry Fixed Effects</th>
<th>Firm Fixed Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Within Estimator</td>
</tr>
<tr>
<td></td>
<td></td>
<td>LSDV</td>
</tr>
<tr>
<td>Volatility</td>
<td>-0.005</td>
<td>-0.054**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Volume (log)</td>
<td>2.804***</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>(0.141)</td>
<td>0.408</td>
</tr>
<tr>
<td>Intraday Range</td>
<td>-0.892***</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>(0.110)</td>
<td>-0.195</td>
</tr>
</tbody>
</table>

\(^a\) Significance: *** is 1%; ** is 5%; * is 10%

Volatility is the monthly standardized historical return volatility of the underlying stocks, i.e., the standard deviation of the monthly series of daily excess returns, standardized for interpretation. Volume is underlying trade volume, logged for interpretation. Intraday range—as a measure of spread—is another measure of volatility. It is the standard deviation of the monthly series of daily price range.

For industry fixed effects, industry is defined as the two-digit SIC code. For firm fixed effects, the firm is defined as the PERMCO identifier from the CRSP database. Fixed effects estimates include time fixed effects (month-year). The within estimates for volume and range timed out.

The within estimator is the de-meaning process as implemented in the lfe package in R. The LSDV estimates are based on a 1% sample of firms (87) averaged over 10,000 samples.

Standard errors are cluster-robust at the industry level (two-digit SIC)

falling substantially compared to the fixed-effects model with no trends, as can be seen by comparing Tables 2.1 and 2.2. This drop in magnitude of the estimates suggests that the firm fixed effects were picking up some of the effect of the firm-specific trends.

Overall, the results from Table 2.2 show that even after allowing for time-varying unobservables, option listing still has an effect on underlying return volatility, which leaves open the possibility that option listing indeed has a causal effect on underlying return volatility, or there is a simultaneity bias in the option listing. Firm fixed effects does a nice job of controlling for unobservables, but not if the bias is simultaneous in
nature. A dynamic panel model could be used to pick up any potential simultaneity bias, a possibility that I leave for future research.

2.5.3 Anticipation and Post-Treatment Effects

As a check on the anticipatory effects of option listing, I include leads of the listing variable in models 2.3.2 and 2.5.1 above. That is, I check to see if past values of option listing influences return volatility and volume. Furthermore, I include lags of the listing variable in models 2.3.2 and 2.5.1 above. The lags are values of the listing dummy iterated forward to account for the possibility that option listing has persistent effects or potentially takes a while to take effect. The inclusion of leads and lags is often known informally as an “event study.”¹⁶ A key study in this vein is Autor (2003) who examines whether labor market rigidity increases temp employment. Autor (2003) includes leads in order to examine any “anticipatory response.” Autor also includes lags to get a ‘sense of the dynamics’ of the policy impacts, e.g., whether the impact of the labor policy accelerates, stabilizes, or reverts back to its mean level, i.e., exhibits a transitory effect as opposed to a permanent effect. From the macroeconomic literature, Blanchard and Perotti (2002) want to pin down the timing of government spending using a mixed structural VAR/event study approach. Timing is important because it’s possible that the market reacts to announcements of government expenditures rather than the actual expenditure itself. Blanchard and

¹⁶Note that the use of the term “event study” in this context is related to- but distinct from the traditional use in empirical finance, where event studies refer to testing, say, an event like a stock split on the average return of a stock. For instance, traditional event studies examine whether there is a change in the cumulative abnormal mean return around some window of an event, e.g., a stock split (see Kothari and Warner (2007)). The key difference between a difference-in-differences/two-way study and a traditional event study from finance is that in the latter the average return is compared to a counterfactual benchmark that comes from some model, e.g., a CAPM regression (see Campbell, Lo, and MacKinlay (1997)), whereas in a difference-in-differences/two-way fixed effects study the counterfactual benchmark is the (average) behavior of a group of control stocks.
Perotti call the leads “implementation lags,” i.e., it takes time for fiscal policy to be enacted; and they call the lags “decision lags,” i.e., it takes time for the changes to take effect.

As mentioned in the literature review section above (Section 2.1), Conrad (1989) finds no evidence of an announcement effect, while Detemple and Jorion (1990) find a small decrease in volatility associated with the announcement dates of listing rather than the actual listing date. I have announcement dates for the years 2000 and 2001, and the gap between the announcement date and actual listing date is anywhere from one day to four days, with a few exceptions where there is a gap of about three weeks. Since the sample for my analysis is at the monthly frequency the distinction between announcement date and listing date shouldn’t have much bearing. Nonetheless, the inclusion of leads will allow me to check for the “policy endogeneity” while the lags will allow me to check whether there is a permanent effect on return volatility or whether the effect is merely transitory.

In performing the anticipation-post-treatment test, I run the following model:

\[
Y_{pt} = \alpha + \sum_{i=1}^{n} \beta_i \text{PERMCO}_{ip} + \sum_{j=1}^{J} \beta_j \text{DATE}_{jt} + \sum_{i=1}^{n} \theta_i (\text{PERMCO}_{ip} \times t) \\
+ \sum_{\tau=0}^{m} \delta_{-\tau} \text{LIST}_{p,t-\tau} + \sum_{\tau=1}^{q} \delta_{+\tau} \text{LIST}_{p,t+\tau} + \nu_{pt} \tag{2.5.2}
\]

The lags correspond to “post-treatment effects” while the leads correspond to “anticipatory effects” (Angrist and Pischke (2009)). I run model 2.6.2 both with- and without the firm-specific trends for the standard deviation of excess returns. Model 2.5.2 times out when run with trends for the intraday range measure of volatility. It also times out with the volume measure. I consider six leads, i.e., \(q = 6\) and I consider three lags, i.e., \(m = 3\). The results are in Table 2.3 (all results have cluster-robust
standard errors, clustered at the industry level). Figure 2.1 depicts these results graphically.

With regard to the lead variables, i.e., the anticipatory effects, it is clear from Table 2.3 that one month before there is a statistically significant effect, which suggests some anticipatory effects. To be sure, since the frequency is at the monthly level, this effect could be the result of the variable construction since, for example, option listing happens on a particular day within a month. In any case, a surprising result is that the sign of the coefficient changes from positive to negative (on average) as a stock is listed on an options exchange. Furthermore, the magnitude of the swing is also noteworthy. These findings are robust across both measures of volatility (monthly standard deviation of excess returns and intraday range percentage) and also robust to the inclusion of firm-specific time trends.

It seems that, on average, one month before option listing a stock becomes relatively more volatile, and then, once the stock is actually listed in the options market, volatility declines. To my knowledge, there is no theoretical model that predicts such dynamic behavior. Conrad (1989) empirically finds that stock price declines several days before option listing, which engenders a permanent decrease in volatility as well as an increase in volume. She intuits that it could be the case that traders build up inventory for hedging purposes in anticipation of the option listing. Her story could explain why volatility increases while at the same time that trading volume increases.

It’s possible that during the time period of the study, there was in increase in trading from uninformed traders who trade with the sole intention of hedging (rather than, say, profiting from speculation or trading on information). An interesting extension, therefore, would be to collect microstructure data (e.g., the TAQ database) and estimate the probability of informed trading (PIN) from this time period.
Then, compare it to the probability of uninformed trading to see if the trading of the uninformed traders “outweighs” the trading from the informed traders.

With regard to the lag variables, i.e., the posttreatment effects, only the range measure of volatility seems to have any type of persistence, showing a statistically significant effect one month later, as well as five and six months later. To be sure, the intraday range has long been used as a measure of volatility, but recently Corwin and Schultz (2012) have argued that the intraday range also has components of the bid-ask spread since the intraday high price is often an asking price, while the intraday low price is often a bid price. In their paper, they decompose the intraday range into its “true” variance component and its bid-ask component. A further extension of my paper would be to use their decomposition to estimate the effect of option listing on the bid-ask spread, which is often thought to be the best measure of a stock’s liquidity (see Section 2.6 below for details).
<table>
<thead>
<tr>
<th>lead</th>
<th>StDev</th>
<th>StDev</th>
<th>Range</th>
<th>Volume</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Trends</td>
<td>No Trends</td>
<td>No Trends</td>
<td>No Trends</td>
</tr>
<tr>
<td>Lead 3</td>
<td>0.044</td>
<td>0.037</td>
<td>-0.059</td>
<td>0.721***</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.039)</td>
<td>(0.077)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Lead 2</td>
<td>0.015</td>
<td>0.016</td>
<td>0.080</td>
<td>0.117***</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.029)</td>
<td>(0.052)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Lead 1</td>
<td>0.228***</td>
<td>0.236***</td>
<td>0.345***</td>
<td>0.288***</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.047)</td>
<td>(0.059)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>LIST</td>
<td>-0.188***</td>
<td>-0.195***</td>
<td>-0.152***</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
<td>(0.054)</td>
<td>(0.054)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Lag 1</td>
<td>-0.043</td>
<td>-0.044</td>
<td>-0.174***</td>
<td>-0.071***</td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.056)</td>
<td>(0.063)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Lag 2</td>
<td>0.020</td>
<td>0.021</td>
<td>0.033</td>
<td>0.008</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.043)</td>
<td>(0.053)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Lag 3</td>
<td>-0.090*</td>
<td>-0.093*</td>
<td>-0.082</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.049)</td>
<td>(0.095)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Lag 4</td>
<td>0.066</td>
<td>0.065</td>
<td>-0.070</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.045)</td>
<td>(0.049)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Lag 5</td>
<td>-0.085</td>
<td>-0.089*</td>
<td>-0.071**</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.050)</td>
<td>(0.035)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Lag 6</td>
<td>0.010</td>
<td>-0.027</td>
<td>-0.348***</td>
<td>-0.224***</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.037)</td>
<td>(0.107)</td>
<td>(0.032)</td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%
b ‘StDev’ is the measure of historical volatility. ‘Range’ is the intraday range. ‘Volume’ is logged underlying trade volume.
‘Trend’ signifies whether the model is estimated with a trend or not.
c Standard errors are cluster-robust at the industry level (two-digit SIC)
Figure 2.1: Graphs of Anticipation and Post-Treatment Effects

a. Anticipation-Post-Treatment: Volatility

b. Anticipation-Post-Treatment: Intraday Range

c. Anticipation-Post-Treatment: Volume (log)
2.5.4 Simulated Placebo Testing

With a treatment that “switches on” and then stays on, there is typically the possibility that serial correlation contaminates statistical inference. I control for this possibility by ensuring that the standard errors are cluster-robust, being clustered at a very broad level, i.e., 2-digit SIC industry code, which yields 78 clusters, typically sufficient to alleviate concerns about serial correlation (see Angrist and Pischke (2009)).

Nevertheless, the level of clustering is arbitrary. Therefore, an alternative (or complement) is to perform so-called placebo tests. The idea is to generate “fake” a treatment variable and run the two-way fixed effects regression. This process is then repeated a large number of times, each time saving the estimate of the coefficient of interest. Since the treatment is “fake” we would expect to reject the null hypothesis of ‘there is a statistically significant effect that is different from zero’ approximately 95% of the time. This simulated placebo testing idea comes from Bertrand, Duflo, and Mullainathan (2004) from the microeconometrics literature, and Brown and Warner (1985) from the event study literature in applied finance.

To implement the simulated placebo testing I eliminate all firms that were listed on an options exchange before the study began, i.e., were listed on or before January 3, 2000, e.g., IBM. I eliminate these firms because they add nothing to the estimation with regard to identification (Wooldridge (2002)). For these firms’ treatment is “switched on” for the entirety of the sample. Thus, they will be wiped out in the demeaning process since their mean is 1.

That said, I need to eliminate these firms from the sample when doing the simulated placebo tests because placebo testing generates fake treatment effects so the zeros and ones are assigned randomly. If I did not eliminate the ‘always-treated’
firms, then they could be assigned zeros and thus I would be inadvertently including them in the testing.

Once these firms are eliminated, I convert \textit{LIST}, which is the causal variable of interest, to a random variable using the \texttt{discreteRV} package in R by Hare, Buja, and Hofmann (2015). This transformation allows me to iteratively sample “fake” treatment variables from the empirical distribution of the true variable. Then, using the \texttt{lfe} package in R, I iteratively regress the main dependent variable of interest, monthly standard deviation of excess returns, on a constant term, firm fixed effects, time fixed effects, and the fake treatment. In other words, take model 2.4.2 but replace \textit{LIST} with random draws from its own empirical distribution. I take 5,000 random draws and calculate beta-hat on ‘fake \textit{LIST}’ each time. I plot the histogram in Figure 1. We should expect the mean of these beta hats to be zero. Why? Because they are generated from “fake” data and thus we would not expect to see an effect. Furthermore, we should expect to see the estimated beta hat using the \textit{actual \textit{LIST}} variable in the rejection region of the histogram.

The histograms in Figure 2.2 show that the mean is indeed zero. Specifically, the mean is 0.00000941; the 2.5\% quantile is -0.0071; and the 97.5\% quantile is 0.0069, thus confirming that the estimated effect from Table 2.1, -0.10, and the estimated effect from Table 2.2, -0.054, are well inside the tails of the histogram—a good sign for the robustness of my estimates. I further confirmed the findings by assuming that the distribution of the fake \textit{LIST} came from a Bernoulli distribution with a probability of success of 0.15, which is the fraction of listings in the data. The findings confirm those in the body as well as panel a of Figure 2.2, i.e., that the effect of the fake treatment is zero. Specifically, with the Bernoulli distribution assumptions, the average beta hat of the fake treatment is -0.0000463; the 2.5\% quantile is -0.00978; the 97.5\% quantile is 0.00916.
Figure 2.2: Histograms of Placebo Effects

a. Placebo Results: Treatment as Discrete Random Variable

b. Placebo Results: Treatment as Bernoulli Random Variable (15% Success)
2.6 Conclusion and Future Research

In a two-way fixed effects framework, I estimate the effect of option listing and find that option listing is associated with a decrease in volatility at the monthly frequency, a finding that confirms the implications of several theoretical models, but overturns the findings of the two most recent empirical studies that examine the effect of option listing on volatility (Mayhew and Mihov (2004) and Danielsen et al. (2007)). My results are robust to two measures of volatility: standard deviation of excess returns and intraday range (aggregated to the monthly frequency). These findings are robust to the inclusion of firm-specific time trends. The analysis of volatility show compelling evidence that option listing is a function of unobservable dimensions, an important finding because previous studies only control for observable aspects of option listing, and only include industry-fixed effects, which, as shown above, is insufficient in discerning the true relationship between option listing and volatility. Furthermore, I find trading volume of the underlying stock increases as a result of being listed in an options market, which shows that options, on net, complement the underlying security rather than substitute for it.

Despite the evidence presented in this paper that option listing causes a decrease in volatility it is nevertheless true that a two-fixed effects econometric model cannot ascertain causal effects in the face of simultaneity. Therefore, a promising avenue of future research is to reexamine the ‘option listing’ question with a dynamic panel data model, thereby allowing a feedback between option listing and underlying volatility.

Volatility is only one measure of market quality. As Brogaard (2010) states “market quality refers to liquidity, price discovery, and volatility.” Along these lines, another area of potential future research is to empirically examine the effect of option listing on the liquidity of the underlying stock as well as how option listing affects
the price efficiency of the underlying market after listing, i.e., does the opening of an options market make the underlying stock’s price “bounce back” more quickly? The formal models mentioned in Section 2.2.1 are well-suited to these two future avenues of research.

What precisely is liquidity? Foucault, Pagano, and Roell (2013) state that liquidity (when in reference to assets rather than, say, banking) is characterized by the following: breadth, depth, and resiliency. Breadth is a low transaction cost of buying, i.e., the buying price and selling price are close. In other words, there is a narrow bid-ask spread. Depth of a market is the ability to buy/sell a large order without having price move against the buyer/seller, i.e., there are many orders from multiple market makers. Resiliency is the notion that if some liquidity loss does occur, it is replenished quickly; relatedly, the asset quickly incorporates new information into its price and quote amount (price efficient).

Regarding the change in resiliency and price efficiency of the underlying market, Hu (2017) finds that earnings announcements have a smaller effect for stocks that are listed in the options market, implying that the information contained in an earnings announcement is “old” since the information contained therein was likely already conveyed in the options market. An alternative to examining earnings announcements would be to measure the degree of serial correlation in prices before and after listing for optioned and non-optioned stocks in the spirit of Fama, Fisher, Jensen, and Roll (1969). Yet another measure of price efficiency and resiliency is the “delay” measure of Hou and Moskowitz (2005), which measures delay as the F-statistic of a Ramsey’s RESET test on two CAPM regressions, where the restriction(s) are on various lags of the stock’s return.

Regarding liquidity, the theoretical models from Section 1.2.1 offer guidance into the effects of option listing on underlying liquidity, say, as measured by the bid-ask
spread, where a narrower spread translates to a more liquid stock. Suppose that the opening of an options market draws in more informed trading as well as more uninformed trading, what happens to the bid-ask spread? Within the Glosten and Milgrom (1985) framework a higher preponderance of informed trading means that the market-maker will widen the spread for protection. Therefore, if the bid-ask spread widens as a result of option listing, this is evidence that, on net, there is an increase in informed trading relative to uninformed trading, which would make the underlying market less liquid.

Empirically, Fedenia and Grammatikos (1991) find that NYSE bid-ask spreads decrease on average, while OTC (Nasdaq) stocks see a spread increase on average. Kumar and Shastri (1998) find that the adverse selection component of the bid-ask spread declines with option introduction, and the spread as a whole narrows, thereby making the market for the underlying asset more liquid on average. Danielsen et al. (2007) find that option listing and bid-ask spread are negatively related, but these authors interpret this correlation to mean that options exchanges select firms to option based in part on that firm’s liquidity properties, i.e., a narrow spread. Hu (2017) tests whether option listing increases informed trading directly. Hu finds that, all else equal, option listing increases informed trading by 12.4% while it increases uninformed trading by 23.9%, which shows that on net, option listing improves the liquidity of the underlying stock.

Other than the closing bid and ask price, spread data are not available for equities at the daily frequency. Data on the spread—and liquidity generally—come from intraday data or inferring it from daily data. Roll (1984) developed a widely-used method of estimating the spread from daily-level data. Hasbrouck (2009) improves the Roll estimator using Bayesian techniques. Corwin and Schultz (2012) develop a method of estimating bid-ask spread from the daily high and the daily
low. Other low-frequency liquidity-related measures include a measure of a stock’s liquidity risk (Pastor and Stambaugh (2003)) and an approximation of Kyle (1985)’s lambda (Amihud (2002)), i.e., the price impact of a trade.

The extent to which the options markets affect the liquidity of the underlying stock would be especially relevant given the recent rise of the role of liquidity in asset pricing models (LAPM), e.g., Pastor and Stambaugh (2003), Acharya and Pedersen (2005), and Holmstrom and Tirole (2011).
CHAPTER 3

THE EFFECT OF OPTION TICK SIZE ON THE UNDERLYING MARKET: SYNTHETIC CONTROL EVIDENCE FROM THE PENNY PILOT PROGRAM

3.1 Introduction

In the first quarter of 2007, the SEC lowered the tick size for 13 options classes: Whole Foods, Agilent, Microsoft, AMD, Intel, General Electric, Caterpillar, Texas Instruments, SemiConductors Holders, Flextronics, and three ETFs. For these classes the SEC changed the tick size from $0.10 to $0.05 for options contracts trading at or above $3.00 and from $0.05 to $0.01 for options trading below $3.00. The SEC called the lowering of the tick size the “Penny Pilot Program” and it gradually phased in options on other stocks and ETFs. The SEC continues to phase in options to this day.

Why did the SEC lower the tick size? There are several reasons, but two stand out in particular: to stop the practice of ‘payment for order flow’ and to lower spreads in an attempt to make options more appealing to retail investors. As explained below, the SEC was successful in both of these goals.
A large tick size creates a gap in prices, a wedge, as it were. This wedge leaves open a profitable channel to trade “inside of the spread.” In other words, the gap between one price and the next can be exploited by market makers and some of it given to brokers in exchange for order flow. The brokers subsequently route orders to wherever payment for order flow is highest rather than routing the order to the exchange that has the best price. With a reduced tick size, there is less of a wedge, making payment for order flow less profitable. DeCovny (2007) says that the SEC was successful in this regard: after the implementation of the Penny Pilot Program, payment for order flow dropped. One of the options exchanges, NYSE-Arca, completely eliminated the practice.\footnote{For more details see: \textit{Penny Pilot Moves Into High Gear} from Futures Industry Magazine, available at https://secure.fia.org/downloads/PennyPilot.pdf}

In addition to wanting to stop the practice of payment for order flow, the SEC wanted to make the options market more accommodating to retail investors. These investors didn’t like the staggered price increments because it led to confusion. They also didn’t like the ten-cent price increment because it fostered a lack of transparency. Specifically, if an investor purchases an option contract for $4.60, he does not know if he could have purchased the contract for $4.51 if the tick size were not artificially set to $0.10. The SEC lowered the tick size with the goal making options contracts cheaper, less confusing, and more price-transparent.

Research has shown that the SEC was successful in its goal: after the decrease in option tick size, spreads did indeed decrease as the SEC intended (Stone (2009), Saraoglu, Louton, and Holowczak (2014)). These findings suggest that the tick size was a binding constraint on option prices. The lower tick size also incentivized the options exchanges to create user-friendly technology, making it easier for investors to place an order (Stone (2009)).
Although the SEC met its goal of making the options market more appealing to retail investors, there is a downside: Stone (2009) and Saraoglu et al. (2014) find that the depth of quotes (“quote depth”) decreases for the option classes in the pilot program. That is, in response to the decrease in tick size, market makers responded by lowering the size of the lot of options that they were willing to trade at given price. Quote depth is important for institutional investors since they typically buy in bulk. The proverbial ‘dentist in Des Moines’ may trade 5-10 contracts at a time, while a hedge fund trades hundreds of contracts at a time. The empirical finding of a decrease in quote depth is backed up by anecdotal evidence from traders and market makers.  

Stone (2009) argues that it is possible that the decrease in tick size—through its consequential reduction in quote depth—pushed institutional investors out of the options market and into the over-the-counter (OTC) market, which could lead to a hinderance of price discovery since prices that are negotiated in the OTC options market are not shown to the public. Hence, the information that institutional investors collect is not impounded into options prices, at least not immediately.

While there is an abundance of empirical evidence that price discovery takes place in the exchange-traded equity options market, the empirical evidence that institutional investors fled the exchange-traded equity options market for the OTC market is scant; evidence is limited to anecdotes from industry reports and exchange professionals. For example, the Aite Group wrote in a 2008 report that the penny pilot program [and potential full decimalization] may push liquidity into the over-

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2See Winners and Losers in the ‘Penny Pilot’ from the Financial Times, available at https://www.ft.com/content/8bfd4e52-e276-11df-9ea3-00144feabdc0


73
the-counter market.”⁴ As another example, in a 2009 letter to then-Secretary of the SEC, Elizabeth Murphy, the Equity Options Trading Committee of the Securities Industry and Financial Markets Association (SIFMA) wrote “[t]hese negative effects [of penny quoting] include a tendency to drive institutional customers into the OTC market, resulting in a less transparent market.”⁵

Exchange professionals have also expressed concern about the Penny Pilot Program channeling institutional investors into OTC markets. The managing director of the ISE options exchange states that the change in option tick size has “inhibited growth in the use of options by institutional investors, it has also driven them to over-the-counter markets.”⁶ And the CBOE stated the following: “CBOE members and institutional investors continue to advise that executing large size orders is difficult in Penny Pilot classes, and that trading is moving to non-listed markets.”⁷

Are these exchange professionals correct? Did institutional investors leave the exchange-traded options market? In principle, these questions are easy to answer: the SEC requires that institutional investors report their option holdings (13-f filings). In practice, however, this data is difficult to obtain.⁸ Thomson-Reuters collects, cleans,

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⁶ See Winners and Losers in the ‘Penny Pilot’ from the Financial Times available at https://www.ft.com/content/8bfd4e52-e276-11df-9ea3-00144feabdc0?mhq5j=e1


⁸ EDGAR has option holdings by institutional in text files (.txt) for some institutions and some time periods, but .html files for other institutions and other time periods. Furthermore, some institutions change CIK codes. There does not seem to be any pattern to the change in CIK codes or which filings are .txt and which are .html. This lack of uniformity in the filings makes it difficult to crawl through the filings and collect the data. I have tried on several occasions to write a program to do just that, but to no avail.
and centralizes the 13-f filings data, but it does not collect 13-f option data. I am unaware of any third-party vendors that have 13-f options data.

Even though institutional options holdings are hard to come by, we can examine other data to answer the question of whether institutional investors fled the exchange-traded options market. Theory shows that informed traders optimally choose to use limit orders rather than market orders (Kaniel and Liu (2006)). Empirically, informed trading is proxied by institutional investors (Anad, Chakravarty, and Martell (2005)). The implication is that institutional investors prefer limit orders. Hence, a change in limit orders could signal a change in the preferences of institutional investors. I collected limit order data from two of the options exchanges: Boston Options Exchange (BOX) and International Securities Exchange (ISE). I ran two regressions—simple OLS and fixed effects—of percent of option trades that were limit orders on a Penny Pilot dummy. The results are in Table 3.1 below.

The fixed-effects estimates in Table 3.1 support the hypothesis that the Penny Pilot Program altered the trading strategy of institutional investors. This support comes from the sign of the estimates: the coefficient estimate on the Penny Pilot variable is negative for both exchanges. The limit orders on large (101-500 contracts) BOX trades fell by 9%, a number that is both statistically- and economically significant. To be sure, the estimates from Table 1 are not definitive; perhaps institutional investors migrated to a different options exchange, e.g., the CBOE. Perhaps they simply switched to market orders. Nevertheless, the anecdotal evidence coupled with the evidence in Table 3.1 shows that further analysis is warranted.

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9 These are the only options exchanges that make their market execution data available to the public.

10 I focus on limit orders rather than, say, number of orders because I am trying to tease apart institutional trades from retail trades. The total number of orders could increase or decrease and this alone would tell us nothing about institutional trading.
Table 3.1: Regression Estimates of the Effect of Option Penny Tick Size on Option Limit Orders

<table>
<thead>
<tr>
<th>Options Exchange</th>
<th>Mean (SD)</th>
<th>OLS</th>
<th>2FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>BOX</td>
<td>90.48</td>
<td>-3.96***</td>
<td>-9.23***</td>
</tr>
<tr>
<td></td>
<td>(23.56)</td>
<td>(0.22)</td>
<td>(0.94)</td>
</tr>
<tr>
<td>ISE</td>
<td>89.42</td>
<td>0.41***</td>
<td>-0.95***</td>
</tr>
<tr>
<td></td>
<td>(13.13)</td>
<td>(0.09)</td>
<td>(0.34)</td>
</tr>
</tbody>
</table>

Significance: *** is 1%; ** is 5%; * is 10%

b The BOX limit order data is narrow in scope: I filtered on order size, keeping only those orders that were between 101-500 option contracts. I apply this filter because it is in this larger interval of contracts that institutional investors are most likely to trade.

c The frequency of the data for both exchanges is monthly. For BOX the sample period is 2009.3-2015.6 for a total of 220,230 observations. The ISE sample period is 2009.6-2016.12 for a total of 51,942 observations. These sample periods are all that’s available.

d Cluster-robust (CUSIP) standard errors in parentheses.

Suppose—for the sake of argument—that we knew with certainty that institutional investors did, in fact, flee the exchange-traded option market, in favor of, say, the OTC options market. What is the harm? Why should anyone care?

We should care because institutional investors trade on the basis of information. Institutional investors bear the cost of collecting information on firms, and they trade on this information. Through prices, this information is impounded into the price. So the more-relevant question is this: has the options market price discovery been eroded as a result of the decrease in option-market tick size? If institutional investors fled to the OTC market to get options exposure, then it may take longer for information to be impounded into price since the OTC market is opaque. I test an implication of this hypothesis: if price discovery has indeed been eroded, then the underlying price should be less price efficient as a result. Using daily returns I calculate the monthly
‘price delay’ measure of Hou and Moskowitz (2005) to test whether the speed at which information is impounded into underlying stock price has changed.

As an alternative hypothesis, it is possible some institutional investors went into the underlying market rather than going into the OTC market to gain exposure to a stock. For example, on the margin, an institutional investor may be indifferent between selling his negative information about a company in the options market (purchasing a put option) or in the underlying market (short-selling). With a decreased tick size and its subsequent decrease in quote depth, the institutional investor has to search for ways to break up his orders (D. Easley and O’Hara (1987)) in an attempt to lower execution costs, since trading in bulk will move prices against him. In other words, a decrease in quote depth in the options market erodes the initial value of going into the options market in the first place, e.g., exploiting greater leverage, tax benefits. Phillips (2011) finds that put options and short-sales are highly substitutable. Dai and Massoud (2012) find that there was in an increase in the short-interest in the stocks that were included in Phase I (original 13 option classes) and Phase II (an additional 22 option classes) of the Penny Pilot Program, which the authors attribute as being caused by the decrease in option market tick size.

I test this hypothesis; I test the effect of the Penny Pilot Program on the short-interest in the underlying market as well as the effect on institutional ownership of the underlying shares. If it’s true that a decrease in option tick size leads to increased costs for institutional investors and at least some of these institutional investors went into the underlying market, then we would expect to see increases in short-interest at the time of inclusion into the Penny Pilot Program. We would also expect to see an increase in institutional ownership of those stocks included in the program.

I collect data on stock returns from CRSP. I collect the Penny Pilot enrollment dates from the NYSE-Arca website. Using the stock return data, I create the price
delay measure for each stock for each quarter in the sample from 2000q1-2015q4. I
collect data on institutional ownership from the SEC’s 13f quarterly filings, available
through Thomson Reuters. I collect data on short interest from Compustat.

I run a simple OLS regression to test the effect that the Penny Pilot Program—a
proxy for an exogenous increase in option tick size—on price delay, short interest,
and institutional ownership of the underlying shares. Since the SEC does not enroll
stocks at random into the program, I attempt to control for this selection bias by
running a two-way fixed effects regression.

There is considerable heterogeneity of the stocks that are included in the Penny
Pilot Program. For instance, Microsoft and Whole Foods are included in Phase I of
the program. Microsoft is included in many indices, which translates to an abnor-
mally high trade volume; it is a market leader; it has a large number of institutional
owners. Whole Foods meets none of these requirements, at least not when it was
initially included in the Penny Pilot Program (2007q1).

A two-way fixed effects analysis cannot handle such heterogeneity—it lumps all of
the “treated” stocks into one group and compares them to an equally-weighted “con-
trol group” of stocks (those not included in the Penny Pilot Program). Therefore, in
an attempt to account for such heterogeneous treatment effects, I perform a synthetic
control analysis of several of the stocks that were included in Phase I (2007q1) of the
program.

Specifically, for each stock included in Phase I of the Penny Pilot Program I cre-
ate a synthetic control unit.\footnote{I only perform the synthetic control analysis on Phase I stocks since later phases incorporate many stocks. Also, Phase I is especially novel since it is the first phase, i.e., the first time that options investors are seeing the lowered tick size, regardless of options class.} I compare price delay, short interest, and institutional
ownership of the stock (the “treatment”) to its synthetic counterpart (the “control”)
before and after inclusion in the Penny Pilot Program. The synthetic control method
is especially well-suited for this scenario because there are only 10 single-stock equity option classes affected by the policy, while there are thousands of potential control stocks available for comparison. Hence, in a two-way fixed effects (“staggered difference-in-differences”) setup the selection of a control stock would be arbitrary. Furthermore, since all 10 classes were selected into the program on identical dates, I cannot exploit staggered implementation to help identify the causal effect of the policy.

To be sure, the type of synthetic control analysis that I employ is called “constrained regression,” as explained in Doudchenko and Imbens (2016). The constrained regression analysis “matches” the treatment unit with the potential control units only on “lagged” dependent variables. By “lagged” I mean pre-treatment observations of the dependent variable, e.g., short interest. In other words, the constrained regression version of synthetic control does not require covariates.

The question of the effect of a change in tick size has current policy relevance. On April 5, 2012 the Jumpstart Our Business Startups (JOBS) Act was signed into law. The Act was passed in hopes of getting small firms to participate in capital markets. The decimalization of the equity market in 2001 led to a substantial decrease in initial public offerings. Hence, the goal of the JOBS Act is to increase the tick size on small firms in order to study its effects on market quality in those stocks. On October 3, 2016 the increase in tick size was increased on a sample of 400 randomly-chosen securities to study the effects of alterations in tick size. Studying the effect of changes to option market tick size on underlying market quality can shed light on investor behavior as it pertains to the interrelationships across markets, specifically with regard to intermarket spillovers.

12See http://www.finra.org/investors/tick-size-pilot-program
The rest of the paper proceeds as follows. Section 3.2 describes the previous literature. Section 3.3 describes the data, Section 3.4 has a two-way fixed effects analysis of the Penny Pilot Program, including basic methodology and results. Section 3.5 has a constrained-regression synthetic control analysis of Phase I of the Penny Pilot Program, with a specific focus on Advanced Micro Devices (AMD). Section 3.6 concludes and offers future avenues of research. The Appendix has additional analyses.

3.2 Previous Literature

3.2.1 Theoretical Models

To my knowledge there are only two formal studies that link tick size to large (block) trading, which is generally associated with institutional investors. Seppi (1997) builds a model of a market-making specialist who competes with a public limit order book, a model that captures the essentials of the NYSE as well as all of the options exchanges. Seppi finds that institutional investors prefer a larger (wider) tick size, while retail investors, i.e., small-quantity traders, prefer a small tick size.

Portniaguina, Bernhardt, and Hughson (2006) extend Seppi’s model to include price-sensitive market-order traders. These authors find that lowering the tick size to too small of a level could lead to a market failure. How so? The smaller the tick size, the easier it is to step in front of publicly-displayed limit orders. Limit-order traders stop posting limit-orders and liquidity dries up. A direct implication of their model is that “a reduction in tick size increases the cost of submitting a large order.” Traders can break up their trades as a result (see D. Easley and O’Hara (1987)). But breaking up a large order into many small orders can be costly. Furthermore, if the limit order book becomes too thin, the trader may opt to place a limit order on the

80
underlying market instead. See Werner, Wen, Rindi, Consonni, and Buti (2015) for a recent review of the theoretical models of the tick size and how it affects market quality.

3.2.2 Tick Changes in the Underlying Market

Much of the previous empirical literature on the effects of changes in tick size comes from the underlying stock market. On September 3, 1992 AMEX lowered the tick size from $0.125 to $0.0625 for stocks selling below $5. On June 2, 1997 (NASDAQ) and June 24, 1997 (NYSE), the tick size decreased from $1/8 to $1/16. And on January 29, 2001 (NYSE) and April 9, 2001 (NASDAQ) the tick size in the underlying market was once again reduced from $1/16 to one penny. This latter reduction in tick size is also known as “decimalization” since stocks went from being quoted in fractions to being quoted in decimals.\(^\text{13}\)

L. E. Harris (1994) uses an empirical model to predict what would happen if stocks switched to penny increments. Harris finds that spreads would be tighter, but quote depth would decrease. On net, trade volume would increase, at least for low-priced stocks, i.e., those trading at less than $10. Harris notes that the combination of a narrower spread and lower quote depth has an ambiguous effect on market liquidity: small traders are unequivocally better off, but large liquidity-demanding traders, e.g., institutional investors, are possibly worse off.

One channel through which large traders could be made worse off is that a penny increment would reduce the value of time precedence. Time precedence is one of many order precedence rules. When multiple orders for a stock arrive at an exchange, the exchange needs some way to sort the orders. One way, of course, is price: whichever

\(^{13}\)These changes in tick sizes were brought about by regulatory pressure. See the Common Cents Stock Pricing Act of 1997 (H.R. 1053).
order offers the best price gets the asset. But what about when prices are equal? With a wide tick size, it is more common to have multiple orders at the same price point. If prices are equal, then the next sensible order-precedence rule is time priority: conditional on submitting an offer of the same price, the offer that arrived first takes precedence in the queue. With a relatively larger tick size, if a trader wants to “step in front” of another trader, he must improve price by a larger amount; if the tick size is relatively small, however, then in order to get price precedence he must only improve price by a small amount. Harris states “[t]ime precedence and a large minimum price variation [tick size] protect traders who display size by forcing quote matches to improve price significantly if they wish to acquire precedence.”

Ahn, Cao, and Choe (1996) use the AMEX tick size change to test the L. E. Harris (1994) model. The authors find that spreads do indeed narrow, suggesting that the artificially-wide AMEX tick size was a binding constraint on narrower spreads. The authors find that trading volume is unchanged; market depth (measured as quotation quantity) is also unchanged. Interestingly, the authors find that stocks that face competition on another exchange, say, a regional exchange, are affected more by the AMEX tick size change: spreads narrow even more than they otherwise would, i.e., if they didn’t face competition from a regional exchange. This finding suggests that competition across exchanges is an important factor with regard to tick size, but perhaps not internationally, as these same authors (Ahn, Cao, and Choe (1998)) find that the spreads of NYSE stocks that are cross-listed on the Toronto Stock Exchange (TSE) are unaffected by a lowering of tick size on the TSE.

Goldstein and Kavajecz (2000) point out that many previous studies—along with many non-economist commentators—argued that liquidity would increase after the reduction in tick size since it would lower spreads, which, in turn, would increase demand. Goldstein and Kavajecz point out, however, that such arguments are only
focused on the demand side of the market. To get a full picture of the effect of tick reduction on liquidity, it is necessary to examine the supply side of liquidity as well. As spreads narrow it becomes less profitable to make a market in a given stock. As market makers leave the market, they take their liquidity with them.

Furthermore, Goldstein and Kavajecz note that it is important to look at the entire limit order book before and after decimalization. The authors calculate cumulative liquidity. They find that spreads do indeed narrow, confirming both intuition and previous studies. But the authors also find that specialist depth (at the NYSE) as well as the depth of the limit order book decrease. On the whole, the decrease in tick size led to a decrease in transactions costs for small market orders, but not for larger market orders, which now face higher transactions costs.

Regarding the effects of a lowering of the tick size on institutional investors, Bacidore and Farkas (2001) find that the NYSE limit order book does get thinner, but this decrease in liquidity does not negatively affect execution quality. Institutional investors, who are informed traders (Anad et al. (2005)) in the Glosten and Milgrom (1985) framework, submit limit orders (Kaniel and Liu (2006)), rather than market orders. Using order data from Nasdaq, Werner (2003) confirms the finding that a decrease in tick size does not raise trade-execution costs for institutional investors.

These findings conflict with survey evidence from traders. In August 2001, Midwood Securities, Inc., conducted a survey of institutional investors. The results of the survey showed that institutional investors reported a lower level of liquidity and higher trade-execution costs. Furthermore, the survey found that traders altered their trading strategies. Hence, if nothing else, institutional investors bore a fixed cost of switching trading strategies, e.g., how to best break up a block trade in order

to minimize market impact. Additionally, Jones and Lipson (2001) find that execution costs for institutional investors—mutual funds specifically—increase, despite a decrease in spreads. Indeed, the authors state “[t]he switch to pennies appears to have levied a burden in the form of lower mutual fund returns.”

S. Chakravarty, Ness, and Ness (2005) estimate the effect of a change in tick size on the adverse-selection component of the bid-ask spread and find that it decreases across all trade sizes. The authors interpret this finding as implying that institutional trading—often used as proxy for informed trading—declined after decimalization. The authors note, however, that the institutional traders could simply be breaking up their trades as a result of the decrease in tick size. S. Chakravarty, Panchapagesan, and Wood (2005) find mixed results for the effect of decimalization on institutional trading costs.

3.2.3 Tick Size Changes in the Options Market

There are only a few academic studies that examine the effects of the Penny Pilot Program.\textsuperscript{15} Stone (2009) appears to be the first to do a systematic analysis of the Penny Pilot Program. Stone chooses a control option class for each of the piloted classes, i.e., options on a similar firm’s stock, and then employs a difference-in-differences regression to estimate the effects of the pilot on the average bid-ask spread in the options market. Stone finds that, on average, spread declines, option trading volume increases, and size decreases. Stone’s findings echo the empirical findings of the effects of decimalization in the equity markets (see e.g., Bessembinder (2003)). Stone’s findings show no transition dynamics, which suggests that the market’s re-

\textsuperscript{15}In addition to the few academic studies, the options exchanges have done their own studies. As part of the Penny Pilot Program, each of the options exchanges is required to release two reports per year, detailing the effects of the decrease in tick size. The reports contain descriptive statistics, basic graphs, and some commentary.
response to the change in tick size was immediate and was quickly stabilized. Stone points out that the decline in trading size is sure to have an adverse effect on institutional investors since this class of investors is much more likely to trade in large quantities compared to retail investors. A decrease in quoted size means that trading size is now spread across multiple price points, i.e., the decrease in tick size leads to the fragmentation of liquidity. Stone also notes that front running is easier with a small tick size, which can incentivize institutional investors to take their liquidity to the OTC market, which, in turn, leads to a less transparent market overall. Furthermore, Stone explains that the lowering of the tick size creates an incentive for traders to create computer trading algorithms in an attempt to break up the bulk order into “child orders” in an attempt to save on execution costs. Widespread algorithmic trading could lead to a more volatile market.

Stone highlights a trade-off that regulators face with regards to the optimal tick size: a lower tick size narrows the bid-ask spread, but it also makes public prices less transparent. Taken to the extreme, institutional investors, as a group, may flock to OTC markets, which would ultimately lead to two distinct markets: retail investors trade on one of the options exchanges, while institutional investors trade options in the OTC market. Price discovery could seize up completely. Or, at the very least, it would likely take longer for information to be impounded into prices, ultimately making prices relatively inefficient.

Dai and Massoud (2012) examine the extent to which the Penny Pilot Program pulls investors away from the underlying equity market. Specifically, these authors argue that since short-selling and put options are highly substitutable, the lower spreads that result from the Penny Pilot Program should entice traders to substitute their short selling for put options. The authors test whether underlying trading volume decreases and also whether option trading volume increases—the idea being
that if underlying volume decreases around the time of the introduction of the Penny Pilot Program and if there is a simultaneous increase in option trading volume, it is reasonable to suppose that the increase in option trading volume came at the expense of underlying trading volume. The authors choose a control firm for each firm of the first three phases of the Penny Pilot Program, for a total of 63 comparisons. The authors employ a difference-in-differences specification using daily data one quarter before and one quarter after. They find that on average option volume increases, while underlying trading volume decreases. They also find that put option volume increases, while underlying short interest decreases. Furthermore, the authors find that the bid-ask spread in the underlying market decreases around the dates of the implementation of the Penny Pilot Program. I emphasize this finding because it seems to conflict with the story that institutional investors have fled the options market. If Glosten and Milgrom (1985) is an adequate model of the underlying equity market, then a decrease in spread would mean that informed trading—empirically manifested as trading by institutional investors—left the underlying market. I address this concern in the results and discussion section below.

Saraoglu et al. (2014) augment the reports that were done by the options exchanges by giving a rigorous econometric treatment to the question of whether option spreads, option trading volume, and option quote size changed as a result of the Penny Pilot Program. The authors find that option spreads show a significant decline. Option trading volume shows no measurable change except for the ETFs that are included in the Penny Pilot, which generally show an increase. It is worth keeping in mind that this finding is an average, which masks the considerable heterogeneity in the change in trading volume. For instance, option volume on the CBOE increased dramatically for both of the ETFs (QQQ and IWM), General Electric (GE), and Microsoft (MSFT); but fall almost as dramatically for the options on Advanced Micro Devices (AMD),
Sun Micro Systems (SUNW), Texas Instruments (TXN), and Whole Foods (WFMI). The report from the ISE options exchange shows similar heterogeneity in the effect of the Penny Pilot Program on option trading volume.

Saraoglu et al. (2014) find that quote size—a measure of liquidity—decreases. They probe this result further. Specifically, the authors collect data on trade sizes by order from the ISE and CBOE options exchanges. The authors classify sizes as “large” and then examine whether large trades decreased compared to a control class before and after the introduction of the Penny Pilot Program. The only consistent result across the large trade sizes from these two options exchanges is Microsoft (MSFT): large trades in Microsoft options declined relative to a control class after the introduction of the Penny Pilot Program. The authors interpret the lack of consistency in the changes in large trades to mean that institutional investors were not “negatively affected as a result of the [Penny Pilot Program].”

It could be the case, however, that institutional investors are negatively affected insofar as they “broke up” their large trades in order to mitigate movements against them (D. Easley and O’Hara (1987)). That the authors find such inconsistent results across just two of the six exchanges means that there is more going on. For example, the options volume for Advanced Micro Devices (AMD) decreases on the ISE options exchange, while it increases on the CBOE options exchange. Additionally, the authors’ finding that institutional investors are unaffected by the Penny Pilot Program conflicts with anecdotal evidence from some of the options exchanges and indeed institutional traders themselves. One industry expert says “[i]nstitutional traders looking to execute large orders, as well as retail traders looking to execute complicated multi-leg transactions now have a difficult time executing their orders.” (see footnote 5 above). And the head of a prominent research firm states in reference to the expansion of the Penny Pilot Program: “[i]nstitutional traders need to
use additional market resources to find the liquidity, making it more challenging to complete an order.”

3.3 Data

I retrieve daily stock return data from CRSP (via WRDS). For each CUSIP and for each quarter I estimate the price delay measure of Hou and Moskowitz (2005). The price delay measure is the ratio of two R-squared measures: one from a regression of individual stock return on contemporaneous market return; and the other from a regression of individual stock return on contemporaneous market return and lagged own returns. The lagged stock returns proxy for information. Formally

\[
\begin{align*}
    r_{jt} &= \alpha_j + \beta_j^0 R_{mt} + \varepsilon_{jt} \quad \text{(base)} \\
    r_{jt} &= \alpha_j + \beta_j^0 R_{mt} + \sum_{n=1}^{5} \beta_j^n R_{j,t-n} + \varepsilon_{jt} \quad \text{(extended)}
\end{align*}
\]

The price delay measure is one minus the ratio of the R-squared measures, that is,

\[
D_1 = 1 - \frac{R^2_{\text{base}}}{R^2_{\text{extended}}}
\]

This is a measure of delay because the closer that the base model is to the extended model in terms of R-squared, the less the lagged values matter, which, in turn, means

---


17The measure of market return that I use is the equally-weighted portfolio from CRSP.

18The formal layout follows Phillips (2011)
that information is incorporated quickly and delay is small. Conversely, delay becomes larger as the base specification and the extended specification differ.

Following Hou and Moskowitz (2005), I also include related price delay measures, $D_2$ and $D_3$, which are defined as follows

$$D_2 = \frac{\sum_{n=1}^{5} n|\beta^n_j|}{|\beta^0_j| + \sum_{n=1}^{5} |\beta^n_j|}$$

(3.3.4)

$$D_3 = \frac{\sum_{n=1}^{5} n|\beta^n_j|/\text{se}(\beta^n_j)}{\sum_{n=1}^{5} |\beta^n_j|/\text{se}(\beta^n_j)}$$

(3.3.5)

The price delay measure $D_2$ accounts for the magnitude of each lag, while $D_3$ accounts for both the magnitude and the statistical significance of each lag. These two delay measures have the same interpretation as $D_1$: the higher the delay measure, the longer it takes the stock to incorporate information.\footnote{Hou and Moskowitz (2005) use Wednesda-to-Wednesday weekly returns, but Phillips (2011) uses daily returns. I opt to follow Phillips (2011) and use daily returns rather than weekly returns for several reasons. First, weekly returns could be sensitive to the day chosen, i.e., Monday vs. Wednesday, etc. Second, Hou and Moskowitz (2005) is the first paper to use the price delay measure, and accordingly, their paper attempts to see how the market prices the delay. Their sample starts from 1976. My sample, on the other hand, is much more recent. As such, information is impounded into price at a much faster rate than it was in, say, the 1980s, e.g., electronic trading. What’s more, the delay measure requires lags. Using weekly returns means that four lags is looking one month back. Using daily returns with five lags means that the measure incorporates information from five trading days back, e.g., a full trading week.}

Following Phillips (2011) I allow for the price delay measures to account for whether information, e.g., news, is positive or negative by including an interaction dummy into equation 3.3.2. That is,

$$r_{jt} = \alpha_j + \beta^0_j R_{mt} + \beta^{d0}_j D^0 R_{mt} + \sum_{n=1}^{5} \beta^m_j R_{j,t-n} + \sum_{n=1}^{5} \beta^{dm}_j D^n_j R_{j,t-n} + \varepsilon_{jt} \text{ (extended-neg)}$$

(3.3.6)
The dummies are set to 1 when the observed return for that time period is negative and zero otherwise. These dummies are important because negative news may affect stock returns in a way that is different from positive news. Furthermore, given the substitutability of put options and short selling, it is important that the price delay measures account for negative news. I then calculate the negative-news-augmented measures of price delay. The formal details can be found in the Appendix.\textsuperscript{20}

To test whether short selling changed as a result of the Penny Pilot Program, I collect short interest from the Compustat short-interest file. The short-interest data is bimonthly (two observations per month), but I aggregate up to the quarterly level in order to match the lower-frequency institutional holdings data. I also collect quarterly trade volume from Compustat. And since neither the Thomson Reuters 13f data nor the short-interest file have industry codes, I also collect SIC and NAICS codes from Compustat. I use industry codes to limit the list of potential control units (“donor pool” in synthetic-control jargon). The institutional holdings, short interest, and underlying trading volume are all expressed in logs.

To test whether institutional investors retreated to the underlying market, I collect institutional ownership data from the Thomson Reuters s34 file (via WRDS) on 13f disclosures. The 13f filings are SEC-mandated quarterly disclosure statements for all US institutional investors (defined to be any firm that manages at least $100 million in assets). These investment companies must publicly disclose all of their holdings of “13f securities.” The “13f securities” are any exchange-traded stocks, shares of closed-end investment companies, shares of exchange-traded funds (ETF), certain convertible debt securities, and equity options.\textsuperscript{21} Not included are sovereign bonds, corporate

\textsuperscript{20}Since the results of the negative-news-augmented price delay measures are similar to the results of the baseline price delay measures, I do not report them in this paper. They are available upon request.

\textsuperscript{21}See https://www.sec.gov/divisions/investment/13ffaq.htm
bonds, foreign currency, real assets, commodity futures, and other derivative products. OTC products of any kind are not included in the disclosure files. Thomson Reuters classifies institutions as banks, insurance companies, investment companies and their managers, investment advisors, and others such as pension funds, university endowments, and foundations.22

It is important to note that 13f filings only include the long interest; short interest is not reported. I say that this is important because many investors use put options as a substitute for short selling (Diamond and Verrecchia (1987), Figlewski and Webb (1993)). As mentioned above, Dai and Massoud (2012) find that short interest declines as a result of the implementation of the Penny Pilot Program. Although institutions are required to disclose any exchange-traded equity option positions, the Thomson Reuters s34 file does not contain options holdings.

I collect the exhaustive list of stocks that are included in the Penny Pilot Program from an Excel file on the NYSE-Arca website.23 The file includes the company name, ticker, and the date that it was selected into the program. Given the company name and ticker I manually match each stock with its CUSIP. Even though I am only analyzing first phase in this paper I nevertheless need all of the stocks that are ever included in the program–again because the “donor pool” of potential controls cannot contain any units that receive treatment.

The sample for the analysis of price delay is 2000q1-2015q4. This sample size puts the implementation of the Penny Pilot Program right at about the middle of the sample. It also avoids most of the tech bubble. The sample for the analysis of short selling and institutional ownership is 2003q3-2015q4. I limit the series to begin

22Although Thomson Reuters disaggregates institution by type, I use the aggregation of all institution types. I leave the question of how institutional heterogeneity was affected by the Penny Pilot Program to future research.

23See: https://www.nyse.com/markets/arca-options/reports
in 2003q3 because for many of the Penny Pilot classes, the short interest series begins in 2003q3.

3.4 Two-Way Fixed Effects Analysis

3.4.1 Methodology

The SEC doesn’t state how stocks are selected for enrollment into the Penny Pilot Program. If selection into the Penny Pilot Program happens along unobservable dimensions and if these unobservable dimensions are fixed over time, I can control for them by estimating a fixed-effect regression. A brief, heuristically-formal exposition is as follows: Suppose that we have a panel data regression model where we decompose the traditional error term into three components: an individual-specific component, $\mu_i$; a time-specific component, $\lambda_t$; and a random component, $u_{it}$.

\[
y_{it} = \alpha + x_{it}'\beta + \epsilon_{it} \\
\epsilon_{it} = \mu_i + \lambda_t + u_{it} \\
y_{it} = \alpha + x_{it}'\beta + \mu_i + \lambda_t + u_{it} \tag{3.4.1}
\]

The intuition of this econometric model is that each individual in the sample has an idiosyncratic effect on the dependent variable. The time-specific effect controls for common shocks experienced by all individuals in the sample. The implication of including time-specific effects is that any temporal effect affects each individual in a similar way.

The coefficient $\beta$ in equation (3.4.1) can be consistently estimated by including a dummy variable for each individual in the sample as well as each time period in the sample and then running OLS, a procedure often known as ‘least-squares dummy
variables’ estimation (LSDV). Of course, if the sample has lots of individuals, the LSDV approach may not be feasible because this would require inverting a matrix that has thousands of columns.

To be sure, there is another estimation procedure that is feasible. It is possible to de-mean the data, i.e., take the within- and between-transformations of the data and run OLS on the de-meaned sample. The intuition of this procedure is that the unobserved, idiosyncratic effects are constant and thus each observation is equal to its mean. Therefore, the de-meaning “wipes out” the constant term, the individual fixed effects, and the time effects.\textsuperscript{24} The specific model that I estimate is as follows:

\[
Y_{pt} = \alpha + \sum_{i=1}^{n} \beta_i CUSIP_{ip} + \sum_{j=1}^{J} \beta_j DATE_{jt} + \delta PENNY_{pt} + v_{pt} \tag{3.4.2}
\]

where \(Y_{pt}\) is the dependent variable for stock \(p\) in time \(t\), \(\beta_i\) is the coefficient on each stock’s fixed effect, and \(CUSIP\) is each specific stock. \(DATE\) is a quarter-year dummy that “switches on” when \(j = t\) and zero otherwise. The variable \(PENNY\) is the variable of interest. It is a dummy variable that equals one when stock \(p\) is included in the Penny Pilot Program, and zero otherwise. The variable \(v_{pt}\) is the error term.

### 3.4.2 Regression Results

I estimate the effects of the Penny Pilot Program for all enrolled stocks using OLS as a baseline case, and then with the two-way fixed effects specification from equation 3.4.2. The two-way fixed effects regression model controls for unobservable factors.\textsuperscript{25}

\textsuperscript{24}The de-meaning procedure is run using the lfe package in R by Gaure (2013), which iteratively runs the Fisch-Waugh-Lovell theorem to “sweep out the fixed effects.”

\textsuperscript{25}In a two-way fixed effects model there is no real need to include covariates since they only serve to reduce the standard errors (see the heuristic explanation in Angrist and Pischke (2015)).
### Table 3.2: Regression Estimates of the Effect of Option Penny Tick Size on the Underlying Market

<table>
<thead>
<tr>
<th>Underlying Variable</th>
<th>Mean (SD)</th>
<th>OLS</th>
<th>2FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Trade Volume (log)</td>
<td>16.14</td>
<td>3.81***</td>
<td>0.04</td>
</tr>
<tr>
<td></td>
<td>(2.36)</td>
<td>(0.03)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Short Interest (log)</td>
<td>14.76</td>
<td>0.08***</td>
<td>0.03***</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Institutional Ownership (log)</td>
<td>16.52</td>
<td>3.52***</td>
<td>-0.08**</td>
</tr>
<tr>
<td></td>
<td>(2.28)</td>
<td>(0.03)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Institutional Ownership (%)</td>
<td>0.55</td>
<td>0.12**</td>
<td>-0.03</td>
</tr>
<tr>
<td></td>
<td>(0.28)</td>
<td>(0.05)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Max Institutional Ownership (log)</td>
<td>14.81</td>
<td>2.95***</td>
<td>-0.02</td>
</tr>
<tr>
<td></td>
<td>(1.84)</td>
<td>(0.03)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Institutional Ownership HHI</td>
<td>0.13</td>
<td>-0.09***</td>
<td>0.04***</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
</tbody>
</table>

a Significance: *** is 1%; ** is 5%; * is 10%

b Trade volume is underlying trade volume. Max institutional ownership is the number of shares owned by the largest owner. Institutional ownership HHI is the Herfindahl-Hirschman Index, which measures the concentration of ownership of shares for a given stock. A high HHI value means that fewer institutional owners own shares of the stock, while a low HHI value means that the amount of shares held by institutions is more distributed.

c Cluster-robust (four-digit SIC level) standard errors in parentheses.
The results are in Table 3.2. All of the OLS results are significant. But there is likely selection bias. The stocks are almost certainly not chosen randomly since the tech titans AMD, Intel, and Microsoft were chosen together in Phase I of the Program—a highly coincidental event if selection were random.

The two-way fixed effects model shows that underlying trade volume is unaffected. Of course, it could be that the decrease in option tick size has drawn hedgers into the underlying market, thereby increasing volume. At the same time, it could be that informed traders have left the stock altogether. Thus, on the whole underlying trade volume appears unaffected, but in reality, there was a change in the composition.

Institutional ownership falls by approximately 8%. But when measured as a percentage of shares outstanding, it only falls by approximately 3%. The latter result is not statistically discernable form zero at conventional confidence levels. The concentration of institutional ownership for a “piloted stock” saw a statistical increase. This finding indicates that the number of institutional investors for a piloted stock has decreased. In other words, the institutional market for a piloted stock has become more monopolized as a result of the Penny Pilot Program.

Short selling increases by approximately 3%. This finding corroborates the notion that put options are highly substitutable with short selling (Diamond and Verrecchia (1987); Figlewski and Webb (1993); Phillips (2011)). It also corroborates the finding of Dai and Massoud (2012), who find that Phases I and II of the Penny Pilot Program led to an increase in short selling. If it’s true that short-selling is largely done by investors who are trading on the basis of some information, then the increase in short selling is also evidence that institutional investors have fled the exchange-traded options market.

Table 3.2 is evidence that the decrease in tick size led to an increase in short selling as well as a decrease in institutional ownership. These findings corroborate
Table 3.3: Regression Estimates of the Effect of Option Penny Tick Size on Underlying Price Efficiency

<table>
<thead>
<tr>
<th>Price Delay Measure</th>
<th>Mean (SD)</th>
<th>OLS</th>
<th>2FE</th>
</tr>
</thead>
<tbody>
<tr>
<td>D1</td>
<td>-0.13</td>
<td>-0.33***</td>
<td>0.08</td>
</tr>
<tr>
<td></td>
<td>(1.91)</td>
<td>(0.03)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>D2</td>
<td>1.11</td>
<td>-0.06***</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.61)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>D3</td>
<td>1.61</td>
<td>-0.14***</td>
<td>0.02</td>
</tr>
<tr>
<td></td>
<td>(0.62)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
</tbody>
</table>

\[ a \text{ Significance: *** is } 1\%; \text{ ** is } 5\%; \text{ * is } 10\% \]

\[ b \text{ The measures D1, D2, and D3 are measures of “price delay” (Hou and Moskowitz (2005)). D1 is the baseline measure of price efficiency, as explained in Section 3.3 above (ratio of two coefficients of determination). D2 accounts for the magnitude of the coefficients. D3 accounts for the magnitude and the statistical significance of each lagged term (see Section 3.3 above for details).} \]

\[ c \text{ Cluster-robust (four-digit SIC level) standard errors in parentheses.} \]

the anecdotes that institutional investors fled the options market. But from a policy perspective does this matter? In other words, why should the SEC care if institutional investors are now shorting stocks rather than purchasing put options? The answer is that the exchange-traded options market is a source of price discovery: the information that option traders collect is impounded into price. If these traders are no longer purchasing exchange-traded options, then this information may be lost. If that’s true, then we would expect to see a longer delay in the speed to which underlying market prices respond to firm-specific information. Table 3.3 has estimates of the effect of the Penny Pilot Program on price delay, which, as mentioned above, is a measure of price efficiency.
The OLS estimates show that Penny Piloted stocks have an associated decrease in price delay. In other words, stocks that were included in the Penny Pilot Program are more price efficient than those stocks that are not included in the program. Of course, OLS regression estimates do not account for the possibility that price efficiency is one of the dimensions along which the SEC chooses stocks for inclusion into the program. The third column in Table 3.3 has the estimates from a two-way fixed effects ("staggered difference-in-differences") regression.

The results show that the Penny Pilot Program did not materially affect the price efficiency of the underlying stocks—stocks respond to ‘information shocks’ just as quickly as they did before they were included in the Penny Pilot Program. When these results are coupled with the results from Table 3.2, we see that the Penny Pilot Program does not seem to have the adverse consequences that some people feared. Even if institutional investors fled the exchange-traded options market for, say, the OTC options market, this fleeing did not have a statistically significant effect on underlying price efficiency; information is still impounded into prices, and at approximately the same rate that it was prior to the change in option tick size.

On the whole, when the results from Tables 3.2 and 3.3 are considered with the fact that option trading volume for many Penny-Piloted stocks increased (Saraoglu et al. (2014)), we see that it’s possible that trading costs for institutional investors increased, but this cost manifests as a benefit to retail investors, who now have better price transparency on their option trades. Simply put, it’s possible that the Penny Pilot Program did nothing more than transfer wealth from institutional investors to previously-underserved retail investors, all while ensuring that underlying price efficiency remained unaffected.
3.5 Synthetic Control Analysis

3.5.1 Introduction and Motivation

The fixed-effects analysis above examines the Penny Pilot Program in its entirety—an overall average effect. But such aggregation masks the considerable heterogeneity of the stocks included in the Penny Pilot Program. For example, in the three months after the initial phase of the program, average daily option volume rose 49% in AMD and 20% in Agilent, but fell 24% in Whole Foods and 20% in Caterpillar.\textsuperscript{26} As somewhat of an industry leader, AMD may be more informationally efficient than, say, Whole Foods (in 2007), since AMD is included in various indices, etc. Hence, AMD’s price efficiency may react quite differently to a change in option tick size than that of a stock like Whole Foods.

Heterogeneity is not the only reason to probe the fixed-effects results further. The fixed-effects analysis compares piloted stocks—of which there are 362—to several thousand non-piloted stocks. A fixed-effects regression puts equal weight on each of these control units. Other than computational ease, there is no justification for such a weighting scheme. The fixed-effects analysis could be augmented by running a weighted least squares (WLS) regression, but any weighting scheme will be arbitrarily chosen.

The problem of ‘which weighting scheme to use’ and the problem of ‘treatment-effect heterogeneity’ is addressed by the synthetic control method of Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010), and recently generalized by Doudchenko and Imbens (2016). The main idea behind the synthetic

\textsuperscript{26}Understanding Economic and Capacity Impacts of the Options Penny Pilot, Appendix D, pg. 20, available at: https://www.nyse.com/markets/arca-options/reports
control method is to let the data choose a control unit—or a weighted average of control units—to compare to the treated unit, typically when only one or a few units receive treatment. Doudchenko and Imbens (2016) show that the synthetic control method is a generalization of difference-in-differences when the policy is a one-time event, i.e., when the policy is not implemented in a staggered fashion.

Phase I of the Penny Pilot Program meets these criteria: there are relatively few stocks that receive treatment, and the treatment took place in one time period (2007q1). Since only a few stocks receive treatment in Phase I of the program, I perform a synthetic control analysis for each of them subject to feasibility. Such a focused analysis overcomes the aforementioned problem of ‘treatment heterogeneity’ because I am examining one stock at a time. The synthetic control method overcomes the ‘equal weights’ problem because weights are chosen based on pre-treatment fit of the treated unit and a donor pool of control units along some covariate or set of covariates.

Here’s a way of thinking about the synthetic control method and why it is optimal for answering this research question. Suppose I wanted to test the effects of the lowering of the option market tick size on institutional ownership of Microsoft. The choice of a control stock will be arbitrary. I could choose Apple since it’s Microsoft’s foremost competitor. But what if Apple has a wildly-different pre-treatment trend (of institutional ownership) vis-a-vis Microsoft? If that were the case, then Apple would be a bad control. The synthetic control method chooses the optimal control stock—a synthetic Microsoft—based on a weighted average of whichever stocks minimize the distance between the pre-treatment trend of Microsoft to the pre-treatment trend of the weighted average of the control units.  

---

27 If the sample is long enough, it is possible to use a hold-out sample for training in order to assess fit. For many stocks, the short interest series begins in 2003q3, but the first phase of the
Athey and Imbens (2016) say that synthetic control is “[a]rguably the most important innovation in the [policy] evaluation literature in the last fifteen years.” The method has been used in many various subfields of economics: international trade (Billmeier and Nannicini (2013)); economic growth (Cavallo, Galiani, Noy, and Pantano (2013)); labor (Sabia, Burkhauser, and Hansen (2012)); health (Clemens (2013)); immigration (Bohn, Lofstrom, and Raphael (2014)); economic inequality (Tanndal and Waldenstrom (2016)); international macroeconomics (Chamon, Garcia, and Souza (2017)); finance (Acemoglu, Johnson, Kermani, Kwak, and Mitton (2016)); and banking (Berger, Butler, Hu, and Zekhnini (2017)).

The synthetic control method exploits both the time-series nature of the data as well as cross-sectional correlations. Gobillon and Magnac (2016) point out a fundamental trade-off when using the synthetic control method: the control group cannot be affected—either directly or indirectly—by the treatment, which means that units that are similar are likely to be affected in some way by the treatment; but these are exactly the units that give the method its cross-sectional power.

It is necessary, therefore, to make sure that any results are robust to control-group selection. For each treated stock, I purge the donor pool of any stocks that are included in any subsequent phases of the Penny Pilot Program. I limit the donor pool to those stocks that are in the same industry as the treated stock, defined as two-digit SIC code. Admittedly, this choice is arbitrary. Nonetheless, a choice must be made. Not narrowing the donor pool at all increases the chances of stumbling upon spurious correlations.

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program starts in 2007q1, which only leaves 14 time periods, likely insufficient for a holdout period. However, the short interest series is semi-monthly and this relative high frequency means that it may be possible to have a hold-out sample to assess fit.
On the other hand, narrowing the donor pool to the most granular level—four-digit SIC code—leaves too small of a donor pool for most stocks.\textsuperscript{28}

3.5.2 Synthetic Control Method: A General Framework

Consider a panel data setting in which there are \( N + 1 \) cross-sectional units observed in time periods \( t = 1, \ldots, T \).\textsuperscript{29,30} Further consider the potential outcomes framework \( Y_{i,t}(0) \) and \( Y_{i,t}(1) \), which respectively corresponds to the outcome given the control and active treatment for each of the cross-sectional units in each of the time periods.\textsuperscript{31} Let the causal effects at the unit and time level be \( \tau_{i,t} = Y_{i,t}(1) - Y_{i,t}(0) \), for all \( i = 0, 1, \ldots, N \) and for all \( t = 1, \ldots, T \).

Let unit 0 be the treated unit. It receives the control treatment in periods \( 1, \ldots, T_0 \), where \( T_0 \) is time period in which the treatment switches from a control-treatment to an active-treatment (for unit 0). Unit 0 receives the active treatment—the policy being studied—in periods \( t = T_0 + 1, \ldots, T_0 + T_1 \), where \( T = T_0 + T_1 \). The control units, \( i = 1, \ldots, N \), never receive active treatment in any of the time periods.

Note that the nature of the synthetic control method is to consider one treated unit at a time. Applying this method to evaluate the effects of Penny Pilot Program, I iteratively apply the method to each of the equity options classes in the pilot, of which there are ten in total for Phase I of the pilot. It’s possible to aggregate the treated units and consider this aggregated unit as the treated unit as in Acemoglu et

\textsuperscript{28}I perform a four-digit SIC code analysis for Microsoft. For brevity’s sake I omit the results in this paper, but they are available upon request.

\textsuperscript{29}This section is an adaptation from the formal treatment from Doudchenko and Imbens (2016).

\textsuperscript{30}There are \( N + 1 \) units rather than simply \( N \) because the treated unit is unit 0.

\textsuperscript{31}In a medical clinical trial the active treatment would be, say, some drug that is being tested, while the control treatment would be some placebo drug, e.g., a water capsule.
al. (2016). But Gobillon and Magnac (2016) argue that the synthetic control method with multiple treated units yields a less-biased treatment effect when the treated units are considered separately than when aggregated as a single unit. Further note that I am considering a policy that has a one-time implementation rather than a widespread policy that is implemented in a staggered manner.

Denote the received treatment (active treatment) as $W_{i,t}$, which satisfies

$$W_{i,t} = \begin{cases} 1 & \text{if } i = 0, \text{ and } t \in \{T_0 + 1, \ldots, T\} \\ 0 & \text{otherwise} \end{cases}$$

The causal effect that I want to examine is the treatment effect on the treated unit (unit 0), during those time periods for which it receives (active) treatment, i.e., $\tau_{0,t}$ for $t = T_0 + 1, \ldots, T$.

For unit $i$ in period $t$, the researcher observes the treatment $W_{i,t}$ as well as the realized outcome, $Y_{i,t}^{\text{obs}}$, where “obs” stands for ‘observed’. Thus

$$Y_{i,t}^{\text{obs}} = Y_{i,t}(W_{i,t}) = \begin{cases} Y_{i,t}(0) & \text{if } W_{i,t} = 0 \\ Y_{i,t}(1) & \text{if } W_{i,t} = 1 \end{cases}$$

Let $Y_{\text{c, pre}}$ denote the $N \times T_0$ matrix with the $(i,t)$-th entry equal to $Y_{i,T_0-t+1}^{\text{obs}}$. The treatment unit is excluded from this matrix. Let $Y_{\text{t, pre}}$ denote a $T_0$-vector with the $t$-th entry equal to $Y_{0,t}^{\text{obs}}$. The same notation is used for the post-treatment period as well, namely $Y_{\text{c, post}}$ and $Y_{\text{t, post}}$. Note that the ‘c’ in the subscript refers to ‘control’ while the ‘t’ in the subscript refers to ‘treated’.
Putting these together in a block matrix we arrive at the following:

\[
\mathbf{Y}_{\text{obs}} = \begin{pmatrix}
\mathbf{Y}_{\text{obs}}^{t,\text{post}} & \mathbf{Y}_{\text{obs}}^{c,\text{post}} \\
\mathbf{Y}_{\text{obs}}^{t,\text{pre}} & \mathbf{Y}_{\text{obs}}^{c,\text{pre}}
\end{pmatrix} = \begin{pmatrix}
\mathbf{Y}_{t,\text{post}}(1) & \mathbf{Y}_{c,\text{post}}(0) \\
\mathbf{Y}_{t,\text{pre}}(0) & \mathbf{Y}_{c,\text{pre}}(0)
\end{pmatrix}
\]

The causal effect of treatment is the difference between \(\mathbf{Y}_{t,\text{post}}(1)\), which is observable, and \(\mathbf{Y}_{t,\text{post}}(0)\), which is unobservable. The goal, then, is to find a way to impute the unobservable \(\mathbf{Y}_{t,\text{post}}(0)\) from the observable \(\mathbf{Y}_{c,\text{post}}(0)\), \(\mathbf{Y}_{c,\text{pre}}(0)\), and \(\mathbf{Y}_{t,\text{pre}}(0)\). As a block matrix we have the following

\[
\mathbf{Y}(0) = \begin{pmatrix}
? & \mathbf{Y}_{c,\text{post}}(0) \\
\mathbf{Y}_{c,\text{pre}}(0) & \mathbf{Y}_{c,\text{pre}}(0)
\end{pmatrix} = \begin{pmatrix}
? & Y_{1,t}(0) & Y_{2,t}(0) & \ldots & Y_{N,t}(0) \\
\vdots & \vdots & \vdots & \ddots & \vdots \\
? & Y_{1,T_{0+2}}(0) & Y_{2,T_{0+2}}(0) & \ldots & Y_{N,T_{0+2}}(0) \\
? & Y_{1,T_{0+1}}(0) & Y_{2,T_{0+1}}(0) & \ldots & Y_{N,T_{0+1}}(0) \\
Y_{0,T_{0}}(0) & Y_{1,T_{0}}(0) & Y_{2,T_{0}}(0) & \ldots & Y_{N,T_{0}}(0) \\
\vdots & \vdots & \vdots & \ddots & \vdots \\
Y_{0,2}(0) & Y_{1,2}(0) & Y_{2,2}(0) & \ldots & Y_{N,2}(0) \\
Y_{0,1}(0) & Y_{1,1}(0) & Y_{2,1}(0) & \ldots & Y_{N,1}(0)
\end{pmatrix}
\]

where the question mark stands for \(\mathbf{Y}_{t,\text{post}}(0)\). That is, we cannot observe what would have happened to the treated unit had it not received the active treatment. And so we have to impute its value from data on the pre-treatment outcomes.

The imputed values of \(Y_{0,t}\), i.e., the vector of question marks in the block matrix, are estimated solely from the pre-treatment outcome variable, i.e., lagged values of \(Y\). “Lags” here is with respect to the intervention period \(T_{0}\). In other words, there are no covariates to control for. This lack of covariates contrasts with the original implementation of the synthetic control method as in Abadie and Gardeazabal (2003),
as well as most subsequent studies that use the synthetic control method. Abadie and Gardeazabal estimate the impact of ETA-led terrorism on Basque Country real GDP per capita throughout the 1970s. The authors create a synthetic Basque Country using a weighted average of pre-1969 values of the following: GDP (i.e., “lagged” GDP), investment ratio, sectoral share percentage, and human capital (see Table 3 in their paper), from both Madrid and Catalonia (regions unaffected by the terrorism).

But covariates are not necessary for creating a reliable synthetic control. Doudchenko and Imbens state “[i]n terms of predictive power the lagged outcomes tend to be substantially more important, and as a result the decision how to treat these other pre-treatment variables need not be a very important one.” since, ‘practically, they play a minor role’. Doudchenko and Imbens confirm that covariates are unimportant by revisiting the results of Abadie et al. (2010) and Abadie, Diamond, and Hainmueller (2015). Doudchenko and Imbens replicate the original findings. Then, for each study they create the respective synthetic controls matching only on lagged outcome values. The authors find that the inclusion of controls makes virtually no difference to the analyses. Accordingly, I ignore covariates in the main analysis. I create the synthetic control for each stock in the Penny Pilot Program by “matching” on lagged, i.e., pre-treatment values of price delay, institutional ownership, and short interest.\footnote{Gompers and Metrick (2001) study the demand of equity holdings by institutional investors and find that following covariates are important for institutional investors: book-to-market, size, turnover, price, and age of the firm. The authors find that return volatility is somewhat important, while momentum factors are not at all important. I perform the “traditional” synthetic control analysis, i.e., using covariates, for Microsoft. Omitted for the sake of brevity, the results are available upon request.}

Doudchenko and Imbens (2016) harmonize several of the econometric policy evaluation methods into a single framework. They argue that commonly-employed methods (e.g., difference-in-differences, synthetic control method) have a common structure in-
so far as these methods estimate the imputed value of the unobservable $Y_{o,T}(0)$:

$$\hat{Y}_{0,T}(0) = \mu + \sum_{i=1}^{N} \omega_i \cdot Y_{i,T}^{obs}$$

We need some way to choose the intercept, $\mu$, and the weights, $\omega$. The simplest way is the minimization of the residual sum of squares via an OLS regression. That is,

$$(\hat{\mu}_{OLS}, \hat{\omega}_{OLS}) = \arg \min_{\{\mu, \omega\}} \sum_{s=1}^{T_0} \left( Y_{0,T_0-s+1}^{obs} - \mu - \sum_{i=1}^{N} \omega_i \cdot Y_{0,T_0-s+1}^{obs} \right)^2$$

OLS may not be feasible, however, if the number of control units is larger than the number of pre-treatment time periods, a situation that is analogous to having too many unknowns relative to the number of equations.

The way to proceed, then, is to make the problem feasible by imposing some type of restriction on the intercept or weights, i.e., make assumptions. The general formulation of the constrained minimization problem is

$$(\hat{\mu}, \hat{\omega}) = \arg \min_{\{\mu, \omega\}} \left\{ \left( Y_{t, pre}^{obs} - \mu - \omega^T Y_{c, pre}^{obs} \right)^T \left( Y_{t, pre}^{obs} - \mu - \omega^T Y_{c, pre}^{obs} \right) \right\}$$  (3.5.1)

The difference-in-differences (DID) method imposes a constant term, $\mu$. The constant term represents a constant difference between the treated and control units in the pre-treatment time periods, i.e., the assumption of parallel trends. The DID method further assumes that the weights are non-negative, $\omega_i \geq 0, i = 1, ..., N$; that weights sum to 1, $\sum \omega_1 = 1$; and that weights are constant, $\omega_i = \bar{\omega}, i = 1, ..., N$. With these assumptions, along with the definition $\frac{1}{N} \sum \omega_i = \bar{\omega}$, we arrive at the familiar equally-weighted controls in the DID method.

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33See Angrist and Pischke (2015) for an exposition of OLS as an ‘automated matchmaker’, i.e., an optimal weighting scheme.
In contrast to the DID method, the synthetic control method (SCM) as described by Abadie et al. (2010) relaxes the assumption of a non-zero constant since the goal is to not have a constant gap between the treatment group and the control group. Similar to the DID method, SCM imposes the assumptions that weights add up to 1, \( \sum \omega_i = 1 \), and that weights are non-negative, \( \omega_i \geq 0 \), \( i = 1, ..., N \). As noted above, Abadie et al. (2010) minimize the distance between the treated unit and the weighted combination of control units with respect to covariates, yielding a diagonal matrix of the relative importance of each of the different covariates. Then they optimize an objective function that is similar to equation 3.5.1 above

\[
\hat{V} = \arg \min_{V=\text{diag}(v_1, ..., v_M)} \left\{ (Y_{\text{obs}} - \hat{\omega}(V)^T Y_{\text{obs}}^c)^T (Y_{\text{obs}} - \hat{\omega}(V)^T Y_{\text{obs}}^c) \right\}
\]

subject to \( \text{tr}(V) = 1, v_m \geq 0, m = 1, ..., M \), where, again, \( M \) is the number of covariates. As mentioned above, covariates are not necessary if the treated unit is matched to the weighted combination of the control units on all of the pre-treatment outcomes.

Ignoring covariates is tantamount to setting the \( V \) matrix to the identity matrix. In this paper I do just that: drop the covariates but keep the rest of Abadie et al. (2010)’s SCM intact. Doudchenko and Imbens (2016) call this method “constrained regression.” The reason that I drop covariates is that by using all previous lagged (i.e., pre-treatment) outcomes, any effect of a covariate will be embedded into the lagged dependent variable anyway. Plus, the inclusion of covariates necessarily entails searching for a set of covariates, which is subjective. Covariates play a “minor role” according to Doudchenko and Imbens (2016), as can be seen in the results of the replications in their paper.

That said, I optimize equation 3.5.1 subject to \( \mu = 0; \sum_{i=1}^N \omega_i = 1; \text{and } \omega_i \geq 0, i = 1, ..., N \). The zero constant ensures no permanent, constant gap between the
treated unit and the synthetic control unit; the weights summing to one ensures a unique weighting scheme; and the non-zero weights ensure pure interpolation, i.e., no extrapolation beyond the support of the treated unit’s series.

### 3.5.3 Synthetic Control Results

As mentioned above, I perform the “constrained regression” version of the synthetic control method. The constrained regression method is formally analyzed in detail in Doudchenko and Imbens (2016). Concisely put, constrained regression is a type of synthetic control method, but instead of fitting the treatment and synthetic control unit on arbitrarily-chosen covariates, the synthetic control unit is chosen based on pre-treatment fit of the dependent variable. Fitting the treatment and control units along the pre-treatment dependent variables is arguably superior to fitting on covariates because the pre-treatment dependent variables incorporate all covariates.

The first phase of the Penny Pilot Program had 13 options classes: 10 single-equity stocks, and 3 exchange-traded funds (ETFs). I exclude all ETFs from the analysis because finding a suitable control would be difficult, if not impossible. I also exclude from the analysis Flextronics and SemiConductors Holders due to a lack of complete data. I eliminate Sun Micro Systems Inc., from the analysis because the company was acquired before 2015q4. Finally, I eliminate General Electric (GE) from the analysis because the company is too unique; GE’s two-digit SIC code is 99, which is categorized by OSHA as a “nonclassifiable establishment.”[^1] I eliminate Whole Foods (WFMI) because of a lack of control units in its donor pool. The final analysis has a total of six treatment units (ticker in parentheses): Agilent Technologies (A), Advanced Micro Devices (AMD), Caterpillar (CAT), Intel (INTC), Microsoft (MSFT),

Table 3.4: Institutional Ownership and Short-Selling Weights for Synthetic AMD

<table>
<thead>
<tr>
<th>Company Name</th>
<th>Institutional Ownership</th>
<th>Short Interest</th>
</tr>
</thead>
<tbody>
<tr>
<td>Digital Pwr Corp</td>
<td>0.00</td>
<td>0.65</td>
</tr>
<tr>
<td>Ixys Corp</td>
<td>0.27</td>
<td>0.00</td>
</tr>
<tr>
<td>Mercury Computer Sys</td>
<td>0.20</td>
<td>0.00</td>
</tr>
<tr>
<td>Nve Corp</td>
<td>0.00</td>
<td>0.20</td>
</tr>
<tr>
<td>Napco Sec Sys Inc</td>
<td>0.00</td>
<td>0.15</td>
</tr>
<tr>
<td>On Semiconductor Corp</td>
<td>0.10</td>
<td>0.00</td>
</tr>
<tr>
<td>Plantronics Inc New</td>
<td>0.12</td>
<td>0.00</td>
</tr>
<tr>
<td>Regal Beloit Corp</td>
<td>0.26</td>
<td>0.00</td>
</tr>
<tr>
<td>S L Inds Inc</td>
<td>0.03</td>
<td>0.00</td>
</tr>
<tr>
<td>Vasco Data Sec Intl Inc</td>
<td>0.02</td>
<td>0.00</td>
</tr>
</tbody>
</table>

Notes: The weights are estimated from the constrained regression - synthetic control, i.e., minimizing the distance between the treated series and the synthetic control series in the pre-treatment time period. The stocks (CUSIP) in the table come from AMD’s donor pool, which is the set of stocks that are in the same industry (four-digit SIC) and have never received treatment, i.e., have never been included in the Penny Pilot Program (hence Intel is not in AMD’s donor pool). The donor pool is 168 stocks. The weights in the respective columns sum to one.

On the whole, the results show no discernable effect of the Penny Pilot Program on institutional ownership or short interest for the stocks that were enlisted in phase I of the program (2007q1). For each stock in the analysis the treated stock does not significantly differ from its synthetic control despite having a good pre-treatment fit. The graphs and the corresponding placebo tests can be found in the Appendix.

3.5.4 Analysis of Advanced Micro Devices

The lone exception to the synthetic control results is Advanced Micro Devices (AMD). It shows a clear deviation from its synthetic control for both series (see next page). The weights that are used to create synthetic AMD are in Table 3.4 above.
The weights are all positive and sum to one (these are constraints that are imposed on the estimation procedure, see the Methodology section above). One benefit of the synthetic control method is that the weighting scheme is both data-driven and transparent. For each variable, the weights are distributed across about a half-dozen stocks, which boosts confidence in the “match” of the synthetic control unit; Doudchenko and Imbens (2016) argue that small weights distributed across a large number of control units from the donor pool is a sign of an unreliable synthetic control unit.

The weights from Table 3.4 are used to create the synthetic AMD. The trajectories for AMD and synthetic AMD are in Figure 3.1 below. There is an increase in short interest relative to the synthetic control at the time of the enrollment into the Penny Pilot Program, which is consistent with investors substituting short selling for put options as a result of the decrease in option tick size. But while short interest increased, institutional ownership decreased dramatically, a finding that seems inconsistent with the hypothesis that institutional investors left the options market for the underlying market.

One explanation for the decrease in the share of institutional ownership is that there may have been an influx into AMD’s underlying market, but this influx was driven by non-institutional investors in AMD’s stock. There is some preliminary evidence for this explanation in that AMD’s underlying trade volume does increase after enrollment into the Penny Pilot Program, but, to be sure, it was already increasing for about one year prior. Other evidence corroborates this hypothesis: after the introduction of the Penny Pilot Program, AMD saw an overall increase of 49% in option volume. To the extent that options and their underlying stocks are complements, we would expect underlying volume to rise as options volume rises. And if these volume

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35I am unaware of any trading strategy that uses put options and short selling as complements.
Figure 3.1: AMD Constrained-Regression Synthetic Control: Institutional Ownership and Short Selling

Notes: Panels b and d plot the gap between the actual path and the synthetic path from Panels a and c, respectively, for all stocks in the donor pool. The black line represents AMD’s gap, while each gray line represents the gap between each stock in the donor pool and its synthetic control.
increases are driven by retail investors, then the Penny Pilot Program has achieved its stated goal.

Of course, there is always the possibility that there was some idiosyncratic shock to AMD. In any case, more research is needed, specifically whether institutional ownership of AMD options decreased as a result of the Penny Pilot Program (see footnotes 3 and 13).

The decrease in institutional ownership could also mean that institutional investors left the market for AMD altogether. Suppose that a hedge fund likes to trade AMD options and AMD underlying shares together to achieve some investment goal. With the narrower option tick size, it’s possible that the hedge fund changes its investment strategy, no longer buying any shares (or derivatives) of AMD stock. It is worth noting that exchange-traded option contracts are physically delivered, but OTC options contracts can be either physically delivered or cash-settled. This distinction is important because it could be the case that some institutional investors, e.g., hedge funds, have switched to OTC option contracts, and yet never owning shares of the stock. On the other hand, banks are also institutional owners. And banks are the ones supplying the OTC contracts. In any case, a future avenue of research would be disentangling institutional ownership by institution type, e.g., bank, hedge fund, etc.

As with the findings from the fixed-effects analysis, we can ask: suppose that institutional investors have fled the exchange-traded options market, what’s the harm? One harm would be that the information that these institutional investors possess takes longer to be impounded into the underlying stock price. This raises concerns about the price efficiency of the market.

I run the constrained regression-synthetic control for the price delay measures of AMD. The weights used to create the synthetic AMD price delay measures are in Table 3.5 below.
<table>
<thead>
<tr>
<th>Company Name</th>
<th>D1</th>
<th>D2</th>
<th>D3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Microsemi Corp</td>
<td>0.09</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Bel Fuse Inc</td>
<td>0.05</td>
<td>0.08</td>
<td>0.00</td>
</tr>
<tr>
<td>Cirrus Logic Inc</td>
<td>0.00</td>
<td>0.27</td>
<td>0.00</td>
</tr>
<tr>
<td>Lattice Semiconductor Corp</td>
<td>0.00</td>
<td>0.21</td>
<td>0.15</td>
</tr>
<tr>
<td>Atmel Corp</td>
<td>0.12</td>
<td>0.01</td>
<td>0.00</td>
</tr>
<tr>
<td>Vtel Corp</td>
<td>0.00</td>
<td>0.00</td>
<td>0.07</td>
</tr>
<tr>
<td>Forgent Networks Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.07</td>
</tr>
<tr>
<td>Asure Software Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.07</td>
</tr>
<tr>
<td>Kopin Corp</td>
<td>0.10</td>
<td>0.11</td>
<td>0.00</td>
</tr>
<tr>
<td>Jabil Circuit Inc</td>
<td>0.27</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Viasat Inc</td>
<td>0.00</td>
<td>0.04</td>
<td>0.25</td>
</tr>
<tr>
<td>Digital Power Corp</td>
<td>0.04</td>
<td>0.00</td>
<td>0.04</td>
</tr>
<tr>
<td>Emcore Corp</td>
<td>0.00</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>Power Integrations Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.05</td>
</tr>
<tr>
<td>American Xtal Technology Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>A X T Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>I X Y S Corp Del</td>
<td>0.05</td>
<td>0.05</td>
<td>0.09</td>
</tr>
<tr>
<td>Alpha Industries Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>Skyworks Solutions Inc</td>
<td>0.00</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>Hubbell Inc</td>
<td>0.27</td>
<td>0.22</td>
<td>0.28</td>
</tr>
<tr>
<td>Plantronics Inc New</td>
<td>0.00</td>
<td>0.02</td>
<td>0.00</td>
</tr>
</tbody>
</table>

Notes: The weights are estimated from the constrained regression - synthetic control, i.e., minimizing the distance between the treated series and the synthetic control series in the pre-treatment time period. The stocks (CUSIP) in the table come from AMD’s donor pool, which is the set of stocks that are in the same industry (four-digit SIC) and have never received treatment, i.e., have never been included in the Penny Pilot Program (hence Intel is not in AMD’s donor pool). The weights in the respective columns sum to one.
Price delay measure D3 has a a little bit of weight, e.g., 3%, distributed to a larger number of firms when compared to price delay measures D1 and D2. This suggests that the D3 synthetic AMD is less reliable. Hubbell, Inc. receives anywhere from 22%-28% of the weight, which suggests that it is the most similar to AMD when compared to the other stocks in the donor pool.

The synthetic control graphs, along with their respective placebo tests, can be found in Figure 3.2 below. The plots generally corroborate the results from the fixed-effects analysis: although short interest and institutional ownership changed in statistically meaningful ways, price efficiency was largely unaffected.

Nonetheless, the price delay measures—especially D1 and D2—do indeed show an increase in price delay in the quarter after inclusion into the Penny Pilot Program. This increase in price delay means that the underlying return was less price efficient. These results give at least some plausibility to the anecdotal evidence that institutional investors have fled the exchange-traded options market and have taken their information with them.
Figure 3.2: AMD Constrained-Regression Synthetic Control: Price Delay

a. Price Delay D1: Synthetic Control  

b. Price Delay D1: Placebo Test  

c. Price Delay D2: Synthetic Control  

d. Price Delay D2: Placebo Test  

e. Price Delay D3: Synthetic Control  

f. Price Delay D3: Placebo Test
3.6 Conclusion and Future Research

With the passage of the Jumpstart Our Business Startups (JOBS) Act in 2012, along with its implementation in October of 2016, research into the effects of a change in tick size has once again become relevant. To that end, it is worth studying history to discover insights useful for the future. In the first quarter of 2007 the SEC mandated a decrease in tick size for 13 option classes. The option bid-ask spreads for all of these classes decreased (narrowed). A consequence of the narrowing of option spreads was that the time precedence order rule became less meaningful, which made limit orders less appealing to option traders. With narrower spreads, front-runners generally find it easier to front-run large orders. As a result, many options exchanges and options-market participants complained that the decrease in tick size caused institutional investors to abandon the exchange-traded options market, choosing to get options exposure in the OTC options market instead.

The fear is that because OTC markets are opaque, the price discovery that happens in the exchange-traded market will seize up. Is this hypothesis true? OTC markets have no price transparency, so there is no way to directly test this hypothesis. However, we can test an implication of this hypothesis: since OTC markets are opaque regarding prices, price discovery may take longer to happen. Hou and Moskowitz (2005) developed a measure of price delay, a measure of how quickly a firm’s stock price reacts to information shocks. A two-way fixed effects regression shows that, on average, price delay was not significantly altered by the change in tick size. The sole exception is AMD, where a synthetic control analysis shows that price delay increases immediately after enrollment into the Penny Pilot Program, but this effect is shortlived.
To be sure, the OTC options market is not the only substitute for exchange-traded options: the underlying market can be used to create options returns. Specifically, through the dynamic buying and (short-)selling of stocks (along with a risk-free asset), options returns can be perfectly replicated (Ross (1976)). Many investors trade in the exchange-traded options market because transactions costs are lower. If the SEC-mandated decrease in tick size caused transactions costs for investors—institutional investors specifically—to increase, then some of these investors may have chosen to gain exposure to the ‘Penny Pilot stocks’ by going into the underlying market, e.g., by selling them short, instead of, say, going long put options.

I indirectly test this hypothesis. On average, the Penny Pilot Program led to a 3% increase in short interest and an 8% decrease in shares held by institutional investors. Furthermore, the HHI of institutional ownership also increases, meaning that the ‘institutional market’ has become more concentrated—fewer institutional investors owning piloted shares. These findings are confirmed using a synthetic control experiment on Advanced Micro Devices (AMD). But synthetic control evidence on several other stocks included in Phase I of the Penny Pilot Program shows mixed results.

Putting all of the results together, it’s not clear that policy prescription is necessary. Anecdotal evidence suggests that institutional investors have fled the exchange-traded options market; they have instead chosen to use the OTC market (and possibly the underlying market) to implement their trades. Presumably the OTC markets are undesirable because they are opaque with respect to settled price, which, in turn, is undesirable from a policy perspective because markets become less efficient. The results in the present paper demonstrate that this undesirability has not come to fruition: the Penny Pilot Program has not materially affected how long it takes stocks to incorporate information. The information that institutional investors have
is still being impounded into prices at the same rate that it was before the change in option tick size.

Nevertheless, there are a few promising avenues of future research. One such avenue is to examine institutional holdings at a more disaggregated level. Thomson Reuters makes several distinctions regarding the type of institutional investor: banks, mutual funds, hedge funds, and “other,” e.g., university endowments. These institutions are not equally informed; and they do not trade with the same frequency. Hedge funds, for example, are generally thought to trade on the basis of specialized information. It would be insightful to see how different institutions react to a change in option tick size.

Another avenue of future research is to estimate pricing inefficiency in a completely different way. In this paper, I estimated inefficiency using the price delay measure of Hou and Moskowitz (2005), which is entirely data driven. Alternatively, one could estimate pricing inefficiency using a structural time series model. For instance, Chelley-Steeley (2008) estimates the change in price efficiency of the London Stock Market after a change in the market’s microstructure. She invokes the model of Amihud and Mendelson (1987) and estimates price inefficiency using time series techniques from Harvey (1992).
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123


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APPENDIX A

CHAPTER 3 APPENDIX

A.0.1 Price Delay Extensions

In addition to using own-lagged stock returns as a proxy for information, I also use lagged market returns. Formally\(^1\)

\[
\begin{align*}
    r_{jt} &= \alpha_j + \beta_j^0 R_{mt} + \varepsilon_{jt} \quad \text{(base)} \quad \text{(A.0.1)} \\
    r_{jt} &= \alpha_j + \beta_j^0 R_{mt} + \sum_{n=1}^{5} \beta_j^n R_{m,t-n} + \varepsilon_{jt} \quad \text{(extended)} \quad \text{(A.0.2)}
\end{align*}
\]

The price delay measure is one minus the ratio of the R-square measures, that is,

\[
D_1 = 1 - \frac{R^2_{\text{base}}}{R^2_{\text{extended}}} \quad \text{(A.0.3)}
\]

This is a measure of delay because the closer that the base model is to the extended model in terms of R-squared, the less the lagged values matter, which, in turn, means that information is incorporated quickly and delay is small. Conversely, delay becomes larger as the base specification and the extended specification differ.

\(^1\)The formal layout follows Phillips (2011), and it is nearly identical to the layout in Section 3 above.
Following Hou and Moskowitz (2005), I also include related price delay measures, \( D_2 \) and \( D_3 \), which are defined as follows

\[
D_2 = \frac{\sum_{n=1}^{5} n|\beta_j^n|}{|\beta_j^0| + \sum_{n=1}^{5} |\beta_j^n|} \quad \text{(A.0.4)}
\]

\[
D_3 = \frac{\sum_{n=1}^{5} n|\beta_j^n|}{se(\beta_j^n)} + \frac{\sum_{n=1}^{5} |\beta_j^n|}{se(\beta_j^n)} \quad \text{(A.0.5)}
\]

The price delay measure \( D_2 \) accounts for the magnitude of each lag, while \( D_3 \) accounts for both the magnitude and the statistical significance of each lag. These two delay measures have the same interpretation as \( D_1 \): the higher the delay measure, the longer it takes the stock to incorporate information.

Following Phillips (2011) I allow for the price delay measures to account for whether information, e.g., news, is positive or negative by including an interaction dummy. That is

\[
r_{jt} = \alpha_j + \beta_j^0 R_{mt} + \beta_j^{dn} D^n R_{mt} + \sum_{n=1}^{5} \beta_j^n R_{m,t-n} + \sum_{n=1}^{5} \beta_j^{dn} D^n R_{m,t-n} + \varepsilon_{jt} \quad \text{(extended-neg)} \quad \text{(A.0.6)}
\]

The dummies are set to 1 when the observed return for that time period is negative and zero otherwise. These dummies are important because negative news may affect stock returns in a way that is different from positive news. Furthermore, given the substitutability of put options and short sales, it is important that the price delay measures account for negative news.
The negative-news-augmented delay measures are as follows

\[ D^n_{\text{neg}} = 1 - \frac{R^2_{\text{extended}}}{R^2_{\text{extended-neg}}} \]  
(A.0.7)

\[ D^2_{\text{neg}} = \frac{\sum_{n=1}^{5} n |\beta^d_n|}{|\beta^0_j| + |\beta^0_j| + \sum_{n=1}^{5} |\beta^n_j| + \sum_{n=1}^{5} |\beta^d_j|} \]  
(A.0.8)

\[ D^3_{\text{neg}} = \frac{\sum_{n=1}^{5} n |\beta^d_n|}{|\beta^0_j| + |\beta^0_j| + \sum_{n=1}^{5} |\beta^n_j| + \sum_{n=1}^{5} |\beta^d_j|} \]  
(A.0.9)

Finally, I calculate the delay measures when the news is positive, i.e., when the dummies are “switched off,” that is, set to zero. In those cases, the price delay measures become the following

\[ D^4_{\text{pos}} = \frac{\sum_{n=1}^{5} n |\beta^n_j|}{|\beta^0_j| + \sum_{n=1}^{5} |\beta^n_j|} \]  
(A.0.10)

\[ D^5_{\text{pos}} = \frac{\sum_{n=1}^{5} n |\beta^n_j|}{|\beta^0_j| + \sum_{n=1}^{5} |\beta^n_j|} \]  
(A.0.11)

I calculate these five delay measures using the extended model with market returns as well as using the extended-firm model, which uses the stock’s own lagged return. The former measures a stock’s delay regarding common information, while the latter measures a stock’s delay regarding firm-specific information.\(^2\)

\(^2\)For brevity’s sake I omit these results, but they are available upon request. In addition to these price delay measures, I also ran the analysis using the “traditional” measure of price efficiency: the coefficient on an AR(1) model. That is, the \( \theta \) coefficient from \( r_t = \alpha + \theta r_{t-1} + e_t \). The autoregression coefficient, \( \theta \), serves as a measure of price efficiency (Fama (1965), Amihud and Mendelson (1987), Campbell et al. (1997, Chapter 2)). Again for brevity’s sake I do not report these results in this paper, but they are available upon request.
A.0.2 Constrained Regression Plots for Phase I Stocks

Pictured below are the plots for four of the stocks that were included in Phase I of the Penny Pilot Program (ticker): Agilent (A), Caterpillar (CAT), Intel (INTC), and Texas Instruments (TXN). Whole Foods (WFMI) has too small of a donor pool, having only four stocks as potential controls. The plots show the comparison of the dependent variable for each stock with its respective synthetic control, run using the “constrained regression” analysis of Doudchenko and Imbens (2016). The dependent variables are institutional ownership, short interest, and the D1 measure of price delay. The donor pool is the group of stocks in the same industry (two-digit SIC code) that was never included in the Penny Pilot Program, as well as the imposition that the panel be balanced. Each of the subsequent pages contains six plots: three for each dependent variable; three placebo tests; for each of the four aforementioned stocks plus Microsoft (AMD, of course, is in the body of the paper).

In the figures on the next page, panels b and d plot the gap between the actual path and the synthetic path from panels a and c, respectively, for all stocks in the donor pool. In the left panels (a, c, e), the solid line is the actual series, while the dotted line is the synthetic control series. The vertical line represents the time period of inclusion into the Penny Pilot Program (2007q1). In the right panels (b, d, f), the black line represents the treated unit’s gap, i.e., the difference between the treated series and the synthetic control series, while each gray line represents the gap between each stock in the donor pool and its synthetic control.
Figure A.1: Agilent Synthetic Control Analysis

a. Institutional Ownership  
b. Institutional Ownership Placebo  
c. Short Interest  
d. Short Interest Placebo  
e. Price Delay  
f. Price Delay Placebo
Figure A.2: Caterpillar Synthetic Control Analysis

a. Institutional Ownership

b. Institutional Ownership Placebo

c. Short Interest

d. Short Interest Placebo

e. Price Delay

f. Price Delay Placebo
Figure A.3: Intel Synthetic Control Analysis

- a. Institutional Ownership
- b. Institutional Ownership Placebo
- c. Short Interest
- d. Short Interest Placebo
- e. Price Delay
- f. Price Delay Placebo
Figure A.4: Microsoft Synthetic Control Analysis

a. Institutional Ownership
b. Institutional Ownership Placebo
c. Short Interest
d. Short Interest Placebo
e. Price Delay
f. Price Delay Placebo
Figure A.5: Texas Instruments Synthetic Control Analysis

a. Institutional Ownership
b. Institutional Ownership Placebo
c. Short Interest
d. Short Interest Placebo
e. Price Delay
f. Price Delay Placebo