Did the JOBS Act Benefit Community Banks? A Regression Discontinuity Study

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Abstract

This study examines the effect of section 601(a)(2) of the Jumpstart Our Small Business (JOBS) Act of 2012, which modified the threshold for unlisted banks and bank holding companies (BHCs) to deregister under the Securities Exchange Act of 1934 from 300 to 1,200 shareholders of record. This change in the cutoff permits utilizing the quasi-experimental technique of regression discontinuity to identify the causal effect of Exchange Act deregistration on the performance of banks and BHCs that took advantage of the statutory change. Using an original dataset consisting of 187 community banks and a novel application of comparative interrupted time series analysis to regression discontinuity, I estimate the local average treatment effect of deregistration on compliers. Consistent with theory and qualitative evidence that the JOBS Act was beneficial for smaller banks, deregistration caused $1.27 higher net income and $3.38 lower pretax expenses per $1 of average assets, and $1.24 million greater assets per employee. However, deregistered banks also had $2.35 lower pretax income and $1.95 lower equity capital per $1 of assets.

Keywords: community banks, deregistration, JOBS Act, regression discontinuity

JEL Codes: G21, G28, M41, M48, K22

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I. Introduction

This study examines the effect on community banks of the reporting, disclosure, and corporate governance requirements of registration under the Exchange Act. Banks and BHCs are required to periodically report public financial statements to prudential regulators such as the OCC, FDIC, and Federal Reserve, and investors may obtain financial data for these institutions from these regulatory agencies. However, registration under the Securities Exchange Act of 1934 (Exchange Act) imposes additional duties beyond the periodic reporting of financial statements. For example, the requirements of the Sarbanes-Oxley Act regarding board composition, director independence, and auditor independence only apply to firms reporting under the Exchange Act.

An interesting question is whether these duties provide a net cost or benefit to reporting firms. One way to test this would be to examine the effects of deregistration under the Exchange Act on firms that continue to report financial data to prudential regulators. Cost savings or increased profitability would imply that Exchange Act registration is associated with higher net costs for reporting firms. But merely examining firms that deregistered over a certain period of time would suffer from a critical problem of selection: firms may deregister for unobserved reasons that also affect financial results. If so, the cause of differing financial results between registered and deregistered firms may be entirely unrelated to the Exchange Act.

However, in April 2012, Congress passed the Jumpstart Our Small Business (JOBS) Act of 2012, Pub. L. 112–106, 126 Stat. 306 (2012), which modified the threshold for Exchange Act reporting for unlisted banks and BHCs from 300 to 1,200 shareholders of record. As explained in this study, the imposition of an arbitrary threshold permits utilizing the quasi-experimental technique of regression discontinuity because the location of banks and BHCs around the threshold is "as good as randomly assigned" (Lee and Lemieux, 2010). Banks and BHCs that deregister in response to the threshold change essentially constitute a treatment group in a natural experiment where treatment is assigned in a quasi-random manner according to the number of shareholders of record. Differing financial results may then be causally attributed to the sole difference between firms just below and just above the threshold: deregistration under the Exchange Act as a result of the JOBS Act.

While the results are not necessarily generalizable to non-bank firms that do not publicly report periodic financial data, this study identifies the causal effect of Exchange Act deregistration for unlisted banks and BHCs. This effect is directly relevant to evaluating the impact of the threshold change under the JOBS Act. It may also inform regulatory policy more generally regarding the effect of registration under the Exchange Act for firms that simultaneously report periodic financial data to the public through other means.

II. Background and summary of the literature

A. Legal framework

In general, while 12 U.S.C. § 78l(i) delegates the administrative function of ensuring compliance with the Securities Exchange Act of 1934 to banks' prudential regulators (Malloy, 1990), banks and BHCs are subject to the same substantive securities laws that apply to other firms. Specifically, they are required to register a class of securities pursuant to section 12(b) of the Exchange Act if those shares would be traded on a national securities exchange, which as of 1999 included not only the NYSE and NASDAQ but also the Over-the-Counter Bulletin Board (Bushee and Leuz, 2005).

Banks and BHCs are also required to register a class of securities pursuant to section 12(g) of the Exchange Act if they have total assets exceeding \$10 million and the shares are held "of record by 2,000 or more persons," which is nearly identical to the threshold for nonbank firms. 15 U.S.C. § 78l(g)(1)(B), 17 C.F.R. § 240.12g-1. Unless a firm undertakes an issuer self-tender offer or reverse stock split (Leuz et al., 2008, p. 206), it has little control over the precise number of shareholders of record of its common stock because these shares are typically freely exchangeable.[1] Accordingly, section 12(g) makes the registration obligation dependent on a factor largely out of the firm's control rather than a voluntary decision to list shares on an exchange under section 12(b).

Until the enactment of the JOBS Act, banks and BHCs were permitted to deregister a class of securities that were registered under section 12(g) when the number of shareholders fell below three hundred. 15 U.S.C. 78l(g)(1)(B)(4). However, section 601(a)(2) of the JOBS Act amended section 12(g) by replacing "three hundred" with "300 persons, or, in the case of a bank or a bank holding company, as such term is defined in section 2 of the Bank Holding Company Act of 1956 (12 U.S.C. 1841), 1,200 persons." P.L. 112-106, 126 Stat. 306, § 601(a)(2). Accordingly, as of April 5, 2012, the effective date of the JOBS Act, banks and BHCs may deregister a class of securities when the number of shareholders of record falls below 1,200. This change in the threshold led a number of unlisted banks and BHCs between 300 to 1,200 shareholders to deregister their securities.

B. Costs and benefits of SEC disclosure and governance obligations

A substantial body of literature discusses the costs and benefits of compliance with the disclosure and governance obligations that apply to firms having a registered class of securities. In general, investors will prefer that a firm go public if the marginal gains from obtaining disclosure and governance beyond what may be compelled contractually exceed the losses from the costs of compliance (Engel et al., 2007). For a comprehensive survey of the theoretical and empirical literature on the economic implications of public company reporting and disclosure regulation, see Leuz and Wysocki (2008).

The 2002 passage of the Sarbanes-Oxley Act of 2002 (SOX) led to a substantial increase in compliance costs, particularly for firms with over $75 million in assets, as they are subject to extensive internal control obligations under Section 404 of SOX (Zhang, 2007). These higher costs led many researchers to examine firms' deregistration in the wake of SOX's passage. Zhang (2007) find negative abnormal returns associated with SOX compliance, suggesting that SOX imposes net costs on public companies. Engel et al. (2007) examine a sample of 470 going-private transactions from 1998 to 2005 and find a greater frequency of these transactions after SOX's passage and positive abnormal returns associated with SOX for smaller firms with a high level of inside ownership. Leuz (2007) critique Zhang (2007) for high sensitivity to date specifications and Engel et al. (2007) for mixing going-private and "going dark" transactions, where the former refers to firms that cease trading entirely whereas the latter refers to firms that remain publicly traded but cease to provide financial statements as a result of deregistration under the Exchange Act. Chhaochharia and Grinstein (2007) conclude that reduced compliance with corporate governance rules is correlated with higher abnormal returns. Hochberg et al. (2009) take an innovative approach by suggesting that the firms most likely to be affected by SOX are those which lobbied most heavily against its implementation, and conclude that these firms experienced abnormal positive returns.

Two post-SOX studies are particularly relevant to the present paper. Bushee and Leuz (2005) evaluate the consequences of the SEC "eligibility rule," which subjected firms listed on the Over-the-Counter Bulletin Board (OTCBB) to the public reporting, disclosure and governance obligations of the Exchange Act. They find that over 75% of OTCBB firms which were not reporting moved to the Pink Sheets, and suggest this indicates that the "costs of SEC disclosure regulation outweigh the benefits" (p. 261). Moreover, they find these firms are smaller and less leveraged than firms which continued to report voluntarily, and the latter experience increases in liquidity.

Leuz et al. (2008) examine only "going dark" deregistrations by firms with fewer than 300 shareholders of record. Leuz et al. find that going-dark firms are "smaller and have poorer stock market performance, higher leverage, and fewer growth opportunities than the population of firms that could but choose not to go dark" (p. 204). Unlike going-private transactions, going-dark announcements lead to negative abnormal returns on average. Leuz et al. suggest that even if the cost savings from deregistration are substantial, the going-dark announcement has an adverse signaling effect that explains the negative market reaction. This important study shows the association between firm characteristics and the going-dark decision, but it does not permit identifying the causal effect of deregistration because of the presence of the going-dark selection effect.

Moreover, while the costs of SOX compliance gives every firm an incentive to deregister its securities, banks and BHCs that deregistered in response to the JOBS Act threshold change differ fundamentally from the firms in Leuz et al.'s sample in two ways. The imposition of an arbitrary threshold eliminates selection effects, as discussed below. But more significantly, banks and BHCs that deregister are not "going dark" in the sense that Leuz et al. use the term. Unlike nonbank firms, they are subject to continued public financial reporting to prudential regulators such as the Federal Reserve, Federal Deposit Insurance Corporation (FDIC), and Office of the Comptroller of the Currency (OCC). Accordingly, the consequences of Exchange Act deregistration are much more limited for banks and BHCs, and consist primarily of non-applicability of SOX corporate governance and internal control duties as well as additional disclosure under the Exchange Act such as Form 10-K, 10-Q and 8-K filing obligations.

In another recent paper, Bakke et al. (2012) examine the effects of involuntary delisting from the NASDAQ using a fuzzy regression discontinuity design, i.e., where eligibility is an instrument for treatment receipt. Their threshold variable is an arbitrary index constructed from NASDAQ listing requirements such as stockholders equity, market value of listed securities, and net income. However, this appears to violate the "excludability" requirement for fuzzy RD, namely that crossing the cutoff cannot affect outcomes other than by influencing the receipt of treatment, i.e., delisting (Hahn et al., 2001; Lee and Lemieux, 2010). Bakke et al. (2012)'s delisting index consists of the same financial characteristics that are measured as "treatment" effects resulting from delisting. Crossing the delisting cutoff virtually determines the value of the outcome variable, whether or not delisting occurred, invalidating the fuzzy RD design and making it impossible to untangle selection and treatment effects. As I explain further in section III.B, the regression discontinuity design in the present study relies on the external imposition of a numerical cutoff–shareholders of record–and crossing this cutoff does not independently affect financial performance.

Finally, in a very recent paper, Frankel et al. (2013) examine factors associated with bank deregistration under the JOBS Act. They find that pre-JOBS Act size, more than 300 shareholders of record, and asset growth are significant predictors of the deregistration decision. The size effect is expected since the cutoff change only affected smaller firms and those with 300-1200 shareholders. They also find that the stock price reaction to JOBS Act deregistration announcements is insignificant. Most relevant to this paper, however, is their examination of the accounting performance of deregistered banks. They find that "banks perform better after deregistration in the pre-Act period but the effect vanishes for banks deregistering after the Act" and conclude that "these results again reinforce prior findings that deregistration before the Act is more likely motived by maximizing shareholder value than deregistration after the JOBS Act" (p. 20). However, they acknowledge that these results may be "possibly due to low power to detect any change since fewer observations fall in the post-Act period."

Unlike Frankel et al. (2013), this study finds that the JOBS Act had a statistically significant effect on the performance of deregistered banks that was largely beneficial. There are several possible reasons for this discrepancy. It seems that Frankel et al. (2013) use a different sample than the banks and BHCs which were registered under section 12(g) and therefore eligible to deregister under the JOBS Act statutory change. They obtain a "sample of banks that deregistered over the period January 2002 to October 2012 from SNL Financial." However, deregistration prior to the JOBS Act is fundamentally qualitatively different from deregistration under the JOBS Act statutory change: the former is purely due to self-selection whereas the latter is driven by the external imposition of a cutoff change, leading to local randomization in the vicinity of the cutoff. Similarly, including banks which are registered under section 12(b) would be inappropriate because that section mandates Exchange Act registration for firms which are listed on a national exchange. These firms were unaffected by the JOBS Act cutoff change–they are obligated to remain registered regardless of the number of shareholders of record–and therefore cannot constitute part of the population within which the assumption of local randomization of treatment in the vicinity of the cutoff applies. Selection bias therefore confounds all of Frankel et al. (2013)'s estimations of the effect of the JOBS Act. As explained below, this study controls for selection bias by instrumenting the deregistration decision with eligibility which is as-good-as-randomly assigned in the vicinity of the cutoff.

Finally, Frankel et al. (2013) measure return on assets (ROA), which only indirectly reflects expense savings from the JOBS Act. Moreover, while they state that they "do not distinguish bank holding companies from commercial banks in this study and refer them to as banks as a whole," (p. 11 n.7), the ROA measurement fundamentally differs between the two types of entities. Parent BHC assets consist largely of the equity capital in subsidiary banks, whereas bank assets are those of depositors. Similarly, the net income of parent BHCs reflects intra-firm dividends and changes in equity capital rather than the direct interest and noninterest income and expenses which would appear on the balance sheet of subsidiary banks. By referring to SNL obtaining financial data from "the Y-9C" (p. 11), it seems that Frankel et al. (2013) are calculating net income on a consolidated basis. But the filing threshold for form Y-9C is $500 million in assets, suggesting that Frankel et al. (2013) are omitting a large group of smaller BHCs which only report on form Y-9SP. To ensure that these firms are included, in this study I compare both directly registered banks as well as the bank subsidiaries of publicly listed BHCs, regardless of total assets. Frankel et al. (2013) take this approach with respect to the lending information variable (p. 11 n.8) but not for ROA.

C. Hypothesis and Qualitative Evidence

Particularly for banks and BHCs, the benefits of the additional reporting obligations from Exchange Act registration seem limited. Investors already have access to financial statements filed publicly with prudential regulators, and it is not clear that the additional governance and disclosure obligations of the Sarbanes-Oxley Act and Exchange Act provide much of a net benefit to smaller firms such as these, as the literature suggests. Deregistration, therefore, should be associated with cost savings and improved profitability.

H_1 : Deregistered firms will have lower expenses, higher net income, and greater efficiency their counterparts on the other side of the cutoff as well as their own prior performance.

To verify my hypothesis is qualitatively plausible, I contacted the accounting and finance departments of several anonymous BHCs which deregistered following the enactment of the JOBS Act and asked whether deregistration led to cost savings. One individual responsible for accounting and finance answered in the affirmative: "Community banks have had significant regulatory burden hoisted upon them over them past few years and even going back 10 years with SOX, with no end in sight. Regulatory burden raises costs, which in a competitive market gets passed on to consumers in one way or another. The JOBS Act did lessen that burden for us." When asked specifically about cost savings from deregistration, the individual replied affirmatively: "Yes, legal, accounting, consulting, printing, XBRL (we hire a firm to assist us in this significant reporting burden), internal audit, other – including various soft costs related to preparing and submitting the SEC filings (i.e, my, our assistan[t] controller, our chief credit officer, etc. time in that process – here we are able to better use that time to work on productivity)."

This interview is consistent with media reports of cost savings as well. A March 12, 2013 article in CFO Magazine noted that Coastal Banking Company deregistered in May 2012 and is "saving $150,000 to $200,000 a year on such costs as converting filings to XBRL, paying attorneys to review them, filing a Section 16 form every time an insider trades stock, and meeting some of the provisions of Sarbanes-Oxley. 'The cost savings keep compounding,' says Paul Garrigues, Coastal Banking's CFO." (Ryan, 2013) In short, there is ample qualitative evidence to support the hypothesis that deregistered firms should have lower expenses, greater profitability and more efficiency, on average, than their counterparts on the other side of the cutoff.

III. Research design

A. Fuzzy regression discontinuity: Identification of the local average treatment effect for compliers

Imbens and Lemieux (2008) summarize causal inference in regression discontinuity design using the potential-outcome terminology of the Rubin Causal Model. If $Y_i(0)$ and $Y_i(1)$ are the pair of potential outcomes for unit i, $W_i \in \{0,1\}$ denotes whether treatment was received, and X_i is a scalar predictor that determines whether treatment is received depending on whether the predictor lies on either side of a fixed cutoff c, then observed outcomes can be written:

$$Y_i = (1-W_i)Y_i(0) + W_iY_i(1) = \begin{cases} Y_i(0) \text{ if } W_i = 0, \\ Y_i(1) \text{ if } W_i = 1 \end{cases} \tag{1}$$

For a regression discontinuity design to identify a treatment effect, the following continuity assumption must be valid (p. 618):

$$F_{Y(0)|X}(y|x) \text{ and } F_{Y(1)|X}(\mathrm{y}|\mathrm{x}) \text{ are continuous in } x \text{ for all } y. \tag{2}$$

which implies that $E\left[Y(0)|X=c\right] = \lim_{x\uparrow c} E[Y|X=x]$ and $E\left[Y(1)|X=c\right] = \lim_{x\downarrow c} E[Y|X=x]$. If treatment compliance were perfect, the local average treatment effect may be estimated by the difference of those conditional expectations at point c.[2] However, in the present study, the threshold change merely makes firms eligible to deregister their class of shares, but they are not required to do so. Indeed, not every bank or BHC with less than 1,200 shareholders deregistered after the enactment of the JOBS Act. Such imperfect compliance renders it necessary to utilize the technique of "fuzzy" regression discontinuity (p. 619). A comprehensive discussion of fuzzy regression discontinuity design is given in Hahn et al. (2001). Following Imbens and Lemieux (2008), the possibility of imperfect compliance is stated formally as:

$$\lim_{x \downarrow c} \Pr(W_i = 1 | X_i = x) \neq \lim_{x \uparrow c} \Pr(W_i = 1 | X_i = x) \tag{3}$$

This implies that the local average treatment effect with imperfect compliance is (p. 619):

$$t_{FRD} = \frac{\lim_{x \downarrow c} E[Y|X = x] - \lim_{x \uparrow c} E[Y|X = x]}{\lim_{x \downarrow c} E[W|X = x] - \lim_{x \uparrow c} E[W|X = x]} \tag{4}$$

This ratio may be interpreted as the local average treatment effect for "compliers," which are units that receive treatment if and only if they are eligible pursuant to the cutoff c (Hahn et al., 2001). As Hahn et al. (2001); Imbens and Lemieux (2008); Lee and Lemieux (2010) explain, fuzzy regression discontinuity is an application of instrumental variables estimation, with the randomized treatment eligibility as an instrument for treatment receipt. Accordingly, as with instrumental variables estimation more generally (Imbens and Angrist, 1994), the assumptions of both monotonicity and excludability are necessary for fuzzy regression discontinuity to estimate a valid treatment effect (Hahn et al., 2001).

B. Inability to precisely manipulate the assignment variable

My study satisfies the local randomization requirement that banks and BHCs cannot precisely manipulate the assignment variable (Lee and Lemieux, 2010, p. 283). As noted previously, unless a firm undertakes an issuer self-tender offer or reverse stock split, the number of shareholders of record lies largely outside its control because shares of its common stock are freely exchangeable among investors. Leuz et al. (2008) note the possibility that companies may have a low number of shareholders of record but "have thousands of beneficial shareholders, most of whom have their shares held in street name by financial institutions" (p. 182). While this suggests that the number of shareholders of record is likely to systematically fall short of the number of beneficial owners, it does not imply that the former lies under an issuer's control unless it colludes with financial institutions or conducts a formal share repurchase effort.

Precise manipulation is also unlikely because the legislative history of the JOBS Act suggests that the 1,200-shareholder cutoff was rather arbitrary. The efforts to raise the deregistration cutoff that culminated in the JOBS Act began in 2008 when the American Bankers Association lobbied for an increase to "between 900 and 1,800 shareholders of record" (Miller, 2008). The 1,200 cutoff was introduced in the Himes-Womack Act, 112 H.R. 1965 (May 24, 2011), and while a few banks might have been aware of the planned cutoff, it is unlikely that many could have anticipated the precise number prior to the Act's introduction.

To empirically verify the lack of manipulation of the assignment variable, I perform the sorting test specified in McCrary (2008). Figure 1 shows a density graph of the number of shareholders as of December 31, 2011[3] using the bandwidth calculation algorithm of McCrary (2008), estimated at estimated at 451.39 with the dataset limited to the +/-900 shareholder window. The y-axis is the fractional density of observations within the default bin size calculated by McCrary (2008). The McCrary (2008) test evaluates whether there is sorting at the point of threshold, i.e., whether there are many more observations on one immediate side of the threshold. The below graph demonstrates that the density on either side of the threshold lies within the 95% confidence intervals, indicating a failure to reject the hypothesis of no sorting. This is confirmed by the McCrary (2008) t-statistic of -0.2741468. To ensure that the result is not driven by choice of bandwidth, I performed the sorting test repeatedly with arbitrary bandwidths. The results are given in the Appendix. The tests fail to indicate any sorting up to the maximum bandwidth of 900.

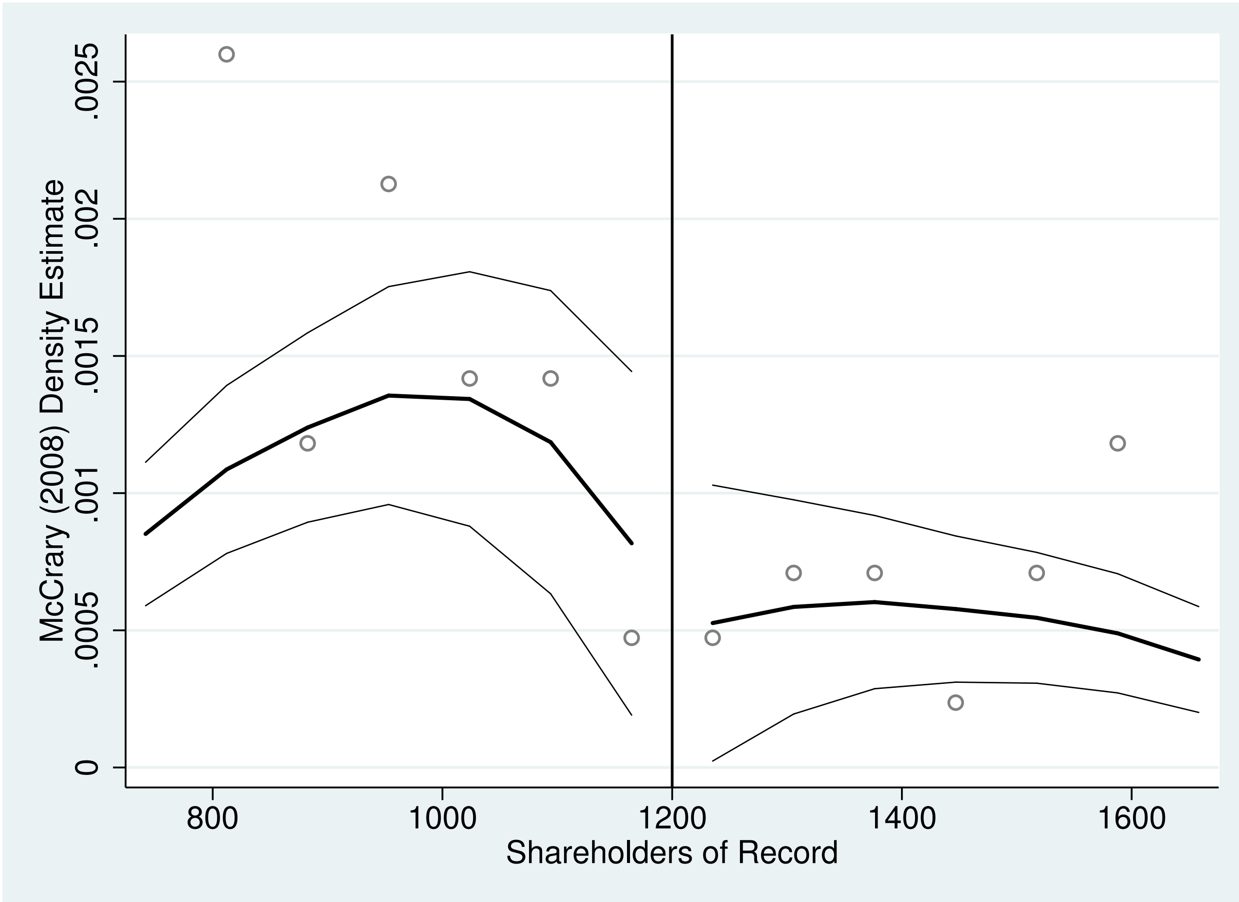

Figure 1: McCrary (2008)Density Plot

As further empirical support for the inability of firms to precisely manipulate the assignment variable, I examine the change in each deregistering firm's number of shareholders of record from December 31, 2010 to the date of deregistration. As the JOBS Act was enacted on April 5, 2012, an ability to manipulate the assignment variable should lead to significant differences in the number of shareholders and movement across the threshold between these dates. No bank or BHC crossed the 1,200 shareholder threshold in either direction during this period other than three cases of a formal share repurchase or other restructuring program, which I exclude from the dataset.[4] Moreover, a t-test of difference in means between the number of shareholders of deregistered banks and BHCs as of December 31, 2012 and both December 31, 2010 and December 31, 2011 yields a t-statistic of -1.1250 and -0.3037, respectively. Accordingly, not only did no deregistering firm cross the threshold after the enactment of the JOBS Act, but the total difference in the number of shareholders between the two periods was statistically insignificant.

C. Monotonicity and Excludability

My study also satisfies the two assumptions of monotonicity and excludability which are necessary for fuzzy RD and instrumental variables estimation more generally (Lee and Lemieux, 2010; Hahn et al., 2001; Imbens and Angrist, 1994). Specifically, crossing the cutoff "cannot simultaneously *cause* some units to take up and others to reject the treatment" (Lee and Lemieux 2010, p. 300). This is self-evident: it is difficult to imagine how the presence of any bank or BHCs on either side of the 1,200 shareholder of record cutoff could have any causal impact on whether other banks or BHCs choose to deregister.

The excludability condition merits additional discussion. Lee and Lemieux describe this condition as: "X crossing the cutoff cannot impact Y except through impacting receipt of treatment" (p. 300). It might seem that extreme differences in the overall number of shareholders would affect firms' financial performance, if nothing else than simply because of economies of scale–large differences in number of shareholders is likely a proxy for size.[5] Moreover, the excludability restriction is that *crossing the cutoff* cannot impact Y, not that the running variable is uncorrelated with the outcome. The use of local linear regression to measure discontinuity simply assumes that the counterfactual is continuous (Imbens and Lemieux, 2008). The RD assumption is *local* randomization: crossing the cutoff–moving from 1,199 to 1,201 shareholders–should have no effect on outcomes other than influencing the receipt of treatment. Interpreting the LATE as a weighted average treatment effect based on the ex ante probability of proximity to the cutoff reflects the inherent limitation that any RD design provides relatively little information regarding observations that are far away from the cutoff. Indeed, it seems highly unlikely that moving from 1,199 to 1,201 shareholders would have any effect on financial performance other than influencing the receipt of treatment. The continuity of observables test below is consistent with this assumption, as none of the pretreatment observables affect outcomes in close proximity to the threshold.

D. Comparative interrupted time series analysis

A novel approach in this paper is to utilize not only outcome variables at one point in time but repeated observations on firms' financial performance. Most regression discontinuity studies analyze post-treatment outcomes at a single or small number of times. Financial results, however, are reported on a quarterly basis. Accordingly, my dataset incorporates all post-treatment observations as well as pre-treatment observations dating back to 2003 to evaluate the performance of treated firms compared to otherwise identical firms on the other side of the cutoff as well as treated and control firms' prior performance.

This provides greater precision, reducing residual variance and strengthening the plausibility of the counterfactual inference. The interpretation of the local average treatment effect as the causal effect of deregistration under the JOBS Act reflects not only differences at a "snapshot" in time, i.e., one quarter financial results, but the average difference in financial results between treatment and control where the control group includes the average pre-treatment performance of treated firms as well as control firms. The logic of this approach is similar to a fixed effects or difference-in-differences model, but rather than observing post-treatment outcomes at a arbitrary single unit of time and simply differencing between another arbitrary pre-treatment unit of time, the entirety of pre-treatment and post-treatment performance is compared. This gives the LATE a more compelling substantive interpretation as the effect of deregistration under the JOBS Act over time rather than a single quarter difference.

Indeed, the comparative interrupted time series method has been used in a variety of settings to provide a plausible counterfactual inference over time. Interrupted time series examines the effect of a policy change on a single unit's performance over time, whereas comparative interrupted time series includes the addition of an otherwise identical control group to strengthen the counterfactual inference in the post-treatment period (McCleary and

Hay, 1980). Recent applications in the literature include community intervention studies (Biglan et al., 2000), medicine use research (Wagner et al., 2002), and the effect of pay for performance on hypertension (Serumaga et al., 2011). More recently, Somers et al. received a grant from the U.S. Department of Education to study the combination of regression discontinuity analysis and interrupted time series analysis (Somers et al., 2009) and have authored a working paper on related issues (Somers et al., 2012).

My study is one of the first applied papers to combine regression discontinuity with comparative interrupted time series analysis and obtain the causal identification benefits of both. Regression discontinuity permits identifying the local average treatment effect of deregistration at the 1,200-shareholder mark, and the time series observations for both the treatment and control groups allow identifying the average treatment effect between treatment and control firms under the assumption that the control group is a valid counterfactual to treatment. This assumption is significantly strengthened by the same property that enables the use of regression discontinuity: the imposition of an arbitrary cutoff, which leads to as-good-as-random assignment in the vicinity of the cutoff. I control for serial correlation across time within firms and across firms within each quarter by clustering standard errors by firm and quarter using the method of Cameron et al. (2011).

In summary, it is highly likely that treatment and control observations in my study will be balanced on unobservables for three reasons: (1) firms on either side of the 1,200-shareholder cutoff are likely to be identical in every way–treatment assignment in the vicinity of the cutoff is as good as random; (2) pre-treatment observations for firms with less than 1,200 shareholders provide a plausible counterfactual for post-treatment performance; (3) pre- and post-treatment observations for firms with more than 1,200 shareholders provide an additional counterfactual for post-treatment performance of treated firms, thereby controlling for any history effect, i.e., the potential for an event simultaneously occurring with the JOBS Act to cause the difference in post-treatment outcomes. The validity of the counterfactual inference from control firms' performance is significantly strengthened by the as-good-as-random assignment of treatment between treatment and control in the vicinity of the cutoff.

IV. Results

A. Data and summary statistics

My primary dataset is composed of 187 banks and BHCs which were (1) registered under section 12(g) of the Exchange Act as of December 31, 2011 with over 300 shareholders of record and (2) either (a) remained registered or (b) deregistered and continued to report financial performance to prudential regulators as of June 30, 2013. I exclude firms which are registered under section 12(b), as they were unaffected by the statutory change, as well as those which were acquired, dissolved, or otherwise ceased reporting financial data. I also exclude three cases where a bank or BHC openly manipulated the number of shareholders of record by engaging in a restructuring or share repurchase program. The initial list of banks and BHCs was hand-collected from SEC EDGAR filings according to SIC classification code and filing statute, e.g., section 12(g) or section 12(b). A list of bank subsidiaries for each BHC was downloaded from the FDIC to generate the final list of banks in the dataset. Quarterly financial data were downloaded from the FDIC and linked to banks on their unique bank certificate number.

The outcome variables in this study are ratios from the Uniform Bank Performance Report (UBPR), which provides each income statement and balance sheet item as a fraction of average net assets for the reported quarter. These ratios are provided by the FDIC for the purpose of evaluating bank performance independent of the size of the total assets on the institution's balance sheet.[6] The outcome variables utilized are (1) personnel & other noninterest expenses, which is the sum of (a) other noninterest expenses, i.e., "retainer fees, legal fees, audit fees, and other fees and expenses paid to attorneys, accountants, management consultants, investment counselors, and other professionals who are not bank officers or employees"[7]; and (b) personnel expenses, i.e., "salaries and employee benefits." (FDIC, 2013); (2) total noninterest expenses; (3) total pretax expenses; (4) total income; (5) net income; (6) efficiency ratio; (7) assets per employee; and (8) capital ratio. A complete codebook is provided in the Appendix with the official UBPR field codes.

In addition, the estimations use Shareholders of Record as the running variable, which consists of the number of shareholders at the time of deregistration for firms which deregistered or as reported on the annual report immediately preceding the enactment of the JOBS Act (typically December 31, 2011) for the other observations. As the number of shareholders of record is only reported on firms' annual 10-K filings, this reflects the best possible approximate division of firms into eligible for treatment and control at the time of treatment, i.e., the enactment of the JOBS Act on April 5, 2012.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
BHC ID	N/A	N/A	N/A	N/A	5849
Bank Certificate Num.	N/A	N/A	N/A	N/A	6709
Deregistration (1/0)	0.39	0.49	0	1	6709
Eligible for Deregistration (1/0)	0.1	0.29	0	1	6709
Shareholders of Record	1251.27	1186.79	145	9180	6709
Shareholders of Record at Deregistration	712.81	199.92	355	1150	2618
Personnel & Other Noninterest Expenses	3.14	9.72	0	535.93	6709
Total Noninterest Expenses	3.62	10.31	0	573.22	6709
Total Expenses	5.64	10.45	1.38	573.22	6709
Total Income	6.05	1.63	0.34	70.38	6709
Net Income	0.11	9.69	-529.61	6.37	6709
Efficiency Ratio	86	356.11	-1466.02	25750	6709
Assets per Employee	3.84	1.54	0.65	18.92	6709
Capital Ratio	10.3	5.72	-3.4	96.83	6709

B. Continuity of pretreatment outcomes at the threshold

As suggested by Lee and Lemieux (2010), I verify that the distribution of *pretreatment* measurements of the outcome variable remains continuous at the threshold of 1,200 shareholders of record by running a regression discontinuity estimation limited to pretreatment observations and verifying an insignificant “treatment” effect. While it is impossible to prove that the population on either side of the threshold is balanced on unobserved characteristics, showing that “the data ’failed to reject’ the assumption of randomization” for observed characteristics is recommended (p. 296). If treatment assignment in the vicinity of the cutoff is as-good-as-random, there should be no significant difference between pretreatment observations of the outcome variables on either side of the cutoff.

The pretreatment estimations are a “strict” RD design with the following functional form:

$$Y_{it} = \alpha + \tau D_{it} + f(X - c) + \epsilon_{it} \tag{5}$$

where $D = 1[X \geq c]$, i.e., whether the running variable exceeds the 1,200 shareholder cutoff and the observation is after April 5, 2012, when the JOBS Act was enacted. Pretreatment observations are drawn from financial reports through March 31, 2012, i.e., immediately prior to the enactment of the JOBS Act. I use a quartic polynomial function for the running variable (i.e., controlling for s, s^2, s^3, and s^4, where s is the number of shareholders at the time of deregistration) and control for serial correlation by clustering standard errors by firm and quarter using the method given by Cameron et al. (2011).

Table 2: Continuity of Pretreatment Outcomes at the Threshold

	Personnel & Other Noninterest Expenses	Total Noninterest Expenses	Total Pretax Expenses	Total Income	Net Income	Efficiency Ratio	Assets per Employee	Capital Ratio
100	-0.85	-0.59	-1.36	-0.018	0.99	-10.8	-6.19***	-6.67
$n = 314$	(0.79)	(0.83)	(1.34)	(1.16)	(1.61)	(20.81)	(2.16)	(4.80)
200	0.011	0.0037	-0.13	0.045	0.21	-6.79	-5.08	-2.98
$n = 891$	(0.64)	(0.65)	(0.89)	(3.41)	(1.20)	(58.64)	(3.24)	(3.34)
300	1.43*	1.60**	1.67*	0.44	-1.12	11.1	-2.15	-2.43
$n = 1,671$	(0.74)	(0.73)	(0.89)	(0.54)	(1.03)	(25.01)	(1.82)	(3.61)
400	-0.21	-0.17	-0.42	-0.045	0.25	-5.09	-1.30	-2.88
$n = 2,432$	(0.81)	(0.84)	(0.97)	(0.43)	(0.94)	(15.97)	(1.42)	(2.92)
500	-0.21	-0.30	-0.47	-0.11	0.20	-5.11	-0.91	-2.45
$n = 3,096$	(0.59)	(0.64)	(0.73)	(0.40)	(0.70)	(12.63)	(1.31)	(2.68)
600	0.27	0.18	0.51	-0.018	-0.55	13.5	-0.30	-0.99
$n = 4,093$	(0.53)	(0.59)	(0.64)	(0.33)	(0.61)	(13.13)	(1.03)	(2.20)
700	-0.11	-0.22	-0.042	0.073	-0.026	2.11	-0.40	-1.50
$n = 4,876$	(0.45)	(0.52)	(0.58)	(0.30)	(0.53)	(12.52)	(0.96)	(2.04)
800	0.17	0.044	0.31	-0.28	-0.59	24.1	-0.22	0.13
$n = 5,325$	(0.45)	(0.51)	(0.57)	(0.27)	(0.51)	(19.59)	(0.83)	(1.98)
900	0.31	0.22	0.55	-0.16	-0.66	20.1	-0.28	0.59
$n = 5,664$	(0.49)	(0.54)	(0.60)	(0.26)	(0.53)	(14.97)	(0.76)	(1.89)

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2: Pretreatment Outcomes - Continuity Verification (1 of 2)

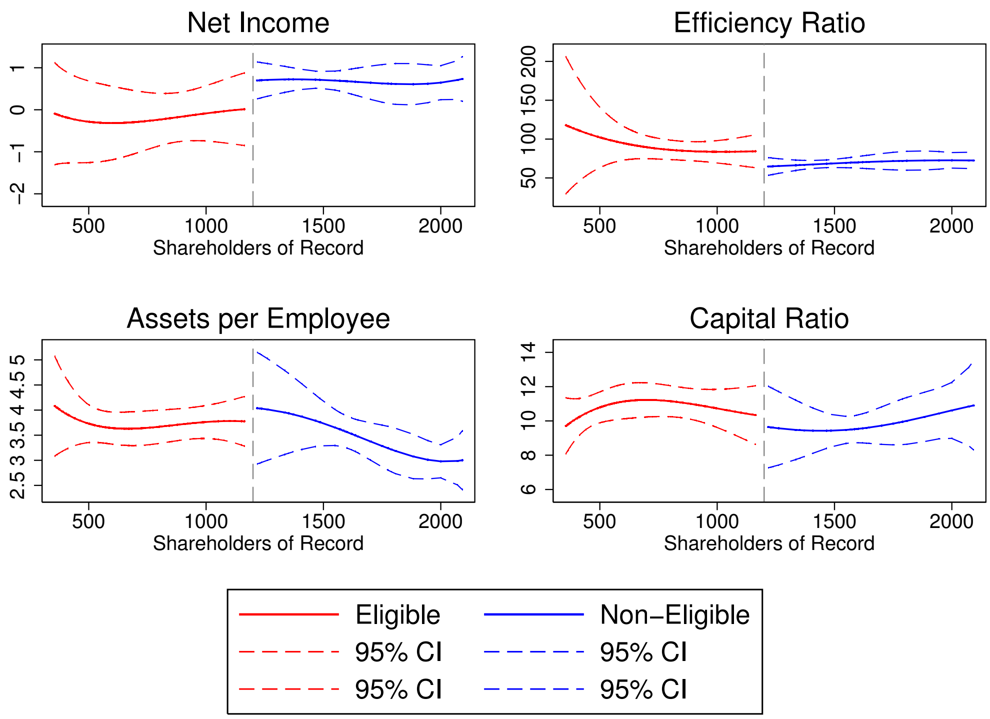

Figure 3: Pretreatment Outcomes - Continuity Verification (2 of 2)

As these tables and figures demonstrate, there is no systematic discontinuity on the pretreatment observables at the 1,200-shareholder mark at $\alpha = .05$ at data windows up to 900 shareholders. While the figures show a minor difference at the threshold, the absence of statistical significance suggests any such difference is likely a result of random chance.

C. Treatment Effect Estimates

My primary estimations measure the effect of JOBS Act deregistration on banks and BHC bank subsidiaries' financial performance with quarterly data from January 1, 2003 to December 31, 2013, using polynomial regression with standard errors clustered by bank and quarter to control for serial correlation across firms and by quarter using the method of Cameron et al. (2011). The two-stage least squares functional form of these fuzzy RD primary estimations with interrupted time series is given by:

$$Y_{it} = \alpha + \tau D_{it} + f(X - c) + \epsilon_{it}, \tag{6}$$

$$D_{it} = \gamma + \delta T_{it} + g(X - c) + \nu_{it} \tag{7}$$

where $T = 1[X \geq c]$, i.e., whether the running variable exceeds the 1,200 shareholder cutoff and the observation is after April 5, 2012, when the JOBS Act was enacted. This reduces to:

$$Y_{it} = \alpha_r + \tau_r T_{it} + f_r(X - c) + e_r \tag{8}$$

where $\tau_r = \tau\delta$, i.e., the intention-to-treat effect (Lee and Lemieux, 2010, p. 328). I use a quartic polynomial function for the running variable (i.e., controlling for s, s^2, s^3, and s^4, where s is the number of shareholders at the time of deregistration) and control for serial correlation by clustering standard errors by firm and quarter using the method of Cameron et al. (2011).

While the outcome variables are implicitly adjusted for the size of each institution's total assets, including additional covariates should be unnecessary if assignment to treatment is as-good-as-random in the vicinity of the cutoff. As noted by Lee and Lemieux (2010), including covariates in a regression discontinuity design may "lead to inconsistent estimates of [the treatment effect], and may cause the asymptotic variance to increase" (p. 333 n. 44). In general, the quasi-experimental design renders it unnecessary to impose functional form assumptions, i.e., by including a linear combination of covariates in the regression analysis. However, as a robustness check, I control for portfolio composition in an additional set of estimations with covariates for (1) short-term non-core funding, (2) domestic and foreign deposits of banks in foreign countries as a percent of total deposits, (3) demand, now, ATS, MMDA and deposits below insurance limit less fully insured brokered deposits, and (4) other borrowing with a maturity greater than one year. As shown in the Appendix, including these covariates does not substantially alter the results, which is to be expected if assignment to treatment is as-good-as-random in the vicinity of the cutoff.

Figures 4 and 5 present cross-sectional results across time from directly registered banks as well as the bank subsidiaries of parent BHCs which were registered as of December 31, 2011. The figures use a window of 900 shareholders, encompassing the minimum lower bound of 300 shareholders to a symmetric upper bound of 2,100. The first graph in the upper-left corner of Figure 4 shows the effect of deregistration on other noninterest expenses, which is most likely to reflect the cost savings from deregistration, and the remaining graphs show the treatment effect on the other outcome variables. The blue line and confidence intervals reflect the "baseline" from which the treatment effect is measured, i.e., the average of the outcome variable among pre-treatment quarters for all banks in the dataset and among post-treatment observations for those banks that were ineligible to deregister. The red line and confidence intervals reflect the outcome of the treatment group, i.e., the average of the outcome variable for those banks that deregistered subsequent to the JOBS Act statutory change.

Figure 4: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs (1 of 2)

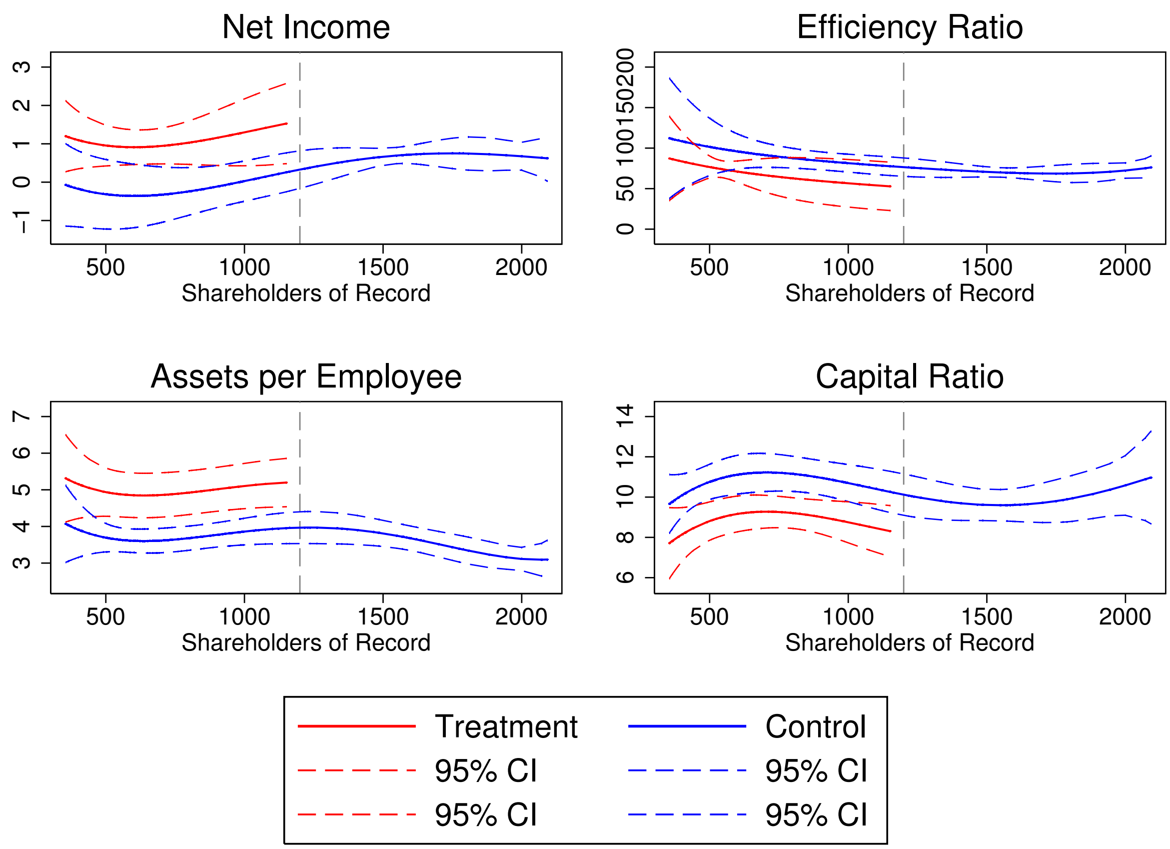

Figure 5: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs (2 of 2)

These figures indicate several discontinuities. Table 3 shows sensitivity of the results to choice of data window and lists point estimates of the treatment effect for individual regressions on the outcome variable at varying window lengths, omitting the running variable polynomial control. All of these regressions have standard errors clustered by firm and quarter to control for serial correlation over time and across firms within each quarter.

Table 3: Effect of JOBS Act Deregistration on Unlisted Banks and BHCs

	Personnel & Other Noninterest Expenses	Total Noninterest Expenses	Total Pretax Expenses	Total Income	Net Income	Efficiency Ratio	Assets per Employee	Capital Ratio
100	-0.059	0.19	-6.93	-4.22	2.96	6.10	-5.57	-3.78
$n = 314$	(1.60)	(1.66)	(5.66)	(2.79)	(3.38)	(35.50)	(4.69)	(8.14)
200	-1.14	-1.35	-6.86**	-4.75***	2.45	-30.9	1.25	-2.41
$n = 891$	(1.36)	(1.47)	(3.14)	(1.77)	(1.77)	(40.51)	(1.09)	(3.42)
300	-1.54	-1.74	-4.93***	-3.05***	2.17**	-36.6*	1.62***	-1.81
$n = 1,671$	(1.11)	(1.13)	(1.41)	(0.65)	(1.07)	(21.01)	(0.57)	(1.25)
400	-1.13	-1.35	-4.19***	-2.60***	1.80**	-29.1*	1.35***	-1.70*
$n = 2,432$	(0.76)	(0.83)	(1.05)	(0.46)	(0.81)	(15.97)	(0.44)	(0.99)
500	-0.86*	-1.05*	-3.49***	-2.44***	1.29**	-16.1	1.50***	-1.22
$n = 3,096$	(0.52)	(0.57)	(0.72)	(0.35)	(0.55)	(10.60)	(0.49)	(0.75)
600	-1.03*	-1.25*	-3.65***	-2.43***	1.46**	-18.6*	1.41***	-2.21***
$n = 4,093$	(0.59)	(0.64)	(0.77)	(0.37)	(0.60)	(11.23)	(0.41)	(0.79)
700	-0.92*	-1.13**	-3.50***	-2.45***	1.30***	-15.6*	1.34***	-1.93***
$n = 4,876$	(0.48)	(0.52)	(0.65)	(0.35)	(0.49)	(9.30)	(0.36)	(0.70)
800	-0.99*	-1.17**	-3.44***	-2.40***	1.28**	-25.4	1.21***	-2.01***
$n = 5,325$	(0.56)	(0.59)	(0.67)	(0.32)	(0.56)	(15.95)	(0.32)	(0.63)
900	-0.91*	-1.09*	-3.38***	-2.35***	1.27**	-24.8	1.24***	-1.95***
$n = 5,664$	(0.53)	(0.57)	(0.64)	(0.32)	(0.54)	(15.71)	(0.31)	(0.60)

Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As the UBPR income statement ratios are defined as percentages of average quarterly assets, the point estimates for income statement figures may be interpreted as the dollar effect of deregistration per $1 of average quarterly assets. To summarize, at a window of 900 shareholders, deregistration had a positive effect on treated firms, leading to a reduction of $0.91 in personnel & other noninterest expenses and $1.09 in total noninterest expenses per $1 of assets at the $\alpha = .10$ level, and a reduction of $3.38 in total pretax expenses and an increase of $1.27 in net income per $1 of assets at the $\alpha = .05$ level. The total noninterest expenses coefficient is significant at the $\alpha = .05$ level with data windows of 700 to 800 shareholders. Nonetheless, as this coefficient remains relatively stable throughout differing windows, it seems likely that there is a real effect but insufficient data to overcome excess variance. Deregistration also led to an increase of $1.24 million in assets per employee, which is a measure of efficiency.[8] However, deregistration caused a decrease of $2.35 in total pretax income and a decrease of $1.95 in total equity capital per $1 of assets. The efficiency ratio–total overhead expense as a percent of net interest and noninterest income–remained unchanged as a result of deregistration.

A 900 shareholder window reduces variance by using the maximum available data, while partially controlling for omitted variables bias at the ends (where treatment assignment is not necessarily as-good-as-random) through the number of shareholder running variable. Nonetheless, these results are largely consistent with smaller data windows through 300 shareholders, below which there are few distinct cross-sectional observations.

D. Comparative interrupted time series analysis

The cross-sectional regression discontinuity estimates constitute the primary outcomes of this study. But as explained previously, I also incorporate observations over time. The following comparative interrupted time series graphs plot median outcomes by treatment and control firms against time, using a smaller window of 600 shareholders to ensure greater comparability. These figures are useful as a demonstration of the treatment effect over time, but their utility is limited because they are plotting outcomes across firms at varying points in time rather than showing the treatment effect vs. control observations across time to estimate the treatment effect. This also makes the confidence intervals less useful because the predicted values vary across time rather than averaging across time and varying on the cross-sectional dimension. Nonetheless, these figures largely comport with the cross-sectional regression discontinuity results shown previously.

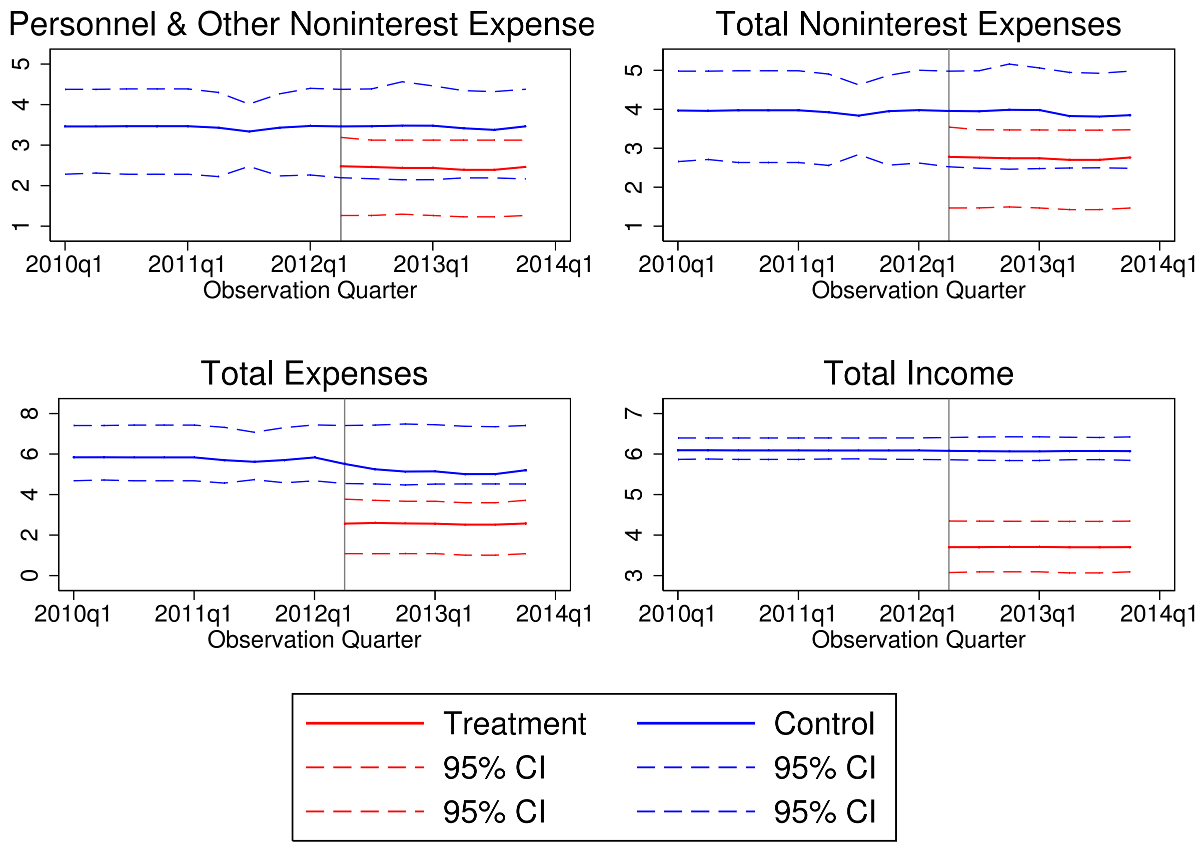

Figure 6: Multiple Time Series - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs (1 of 2)

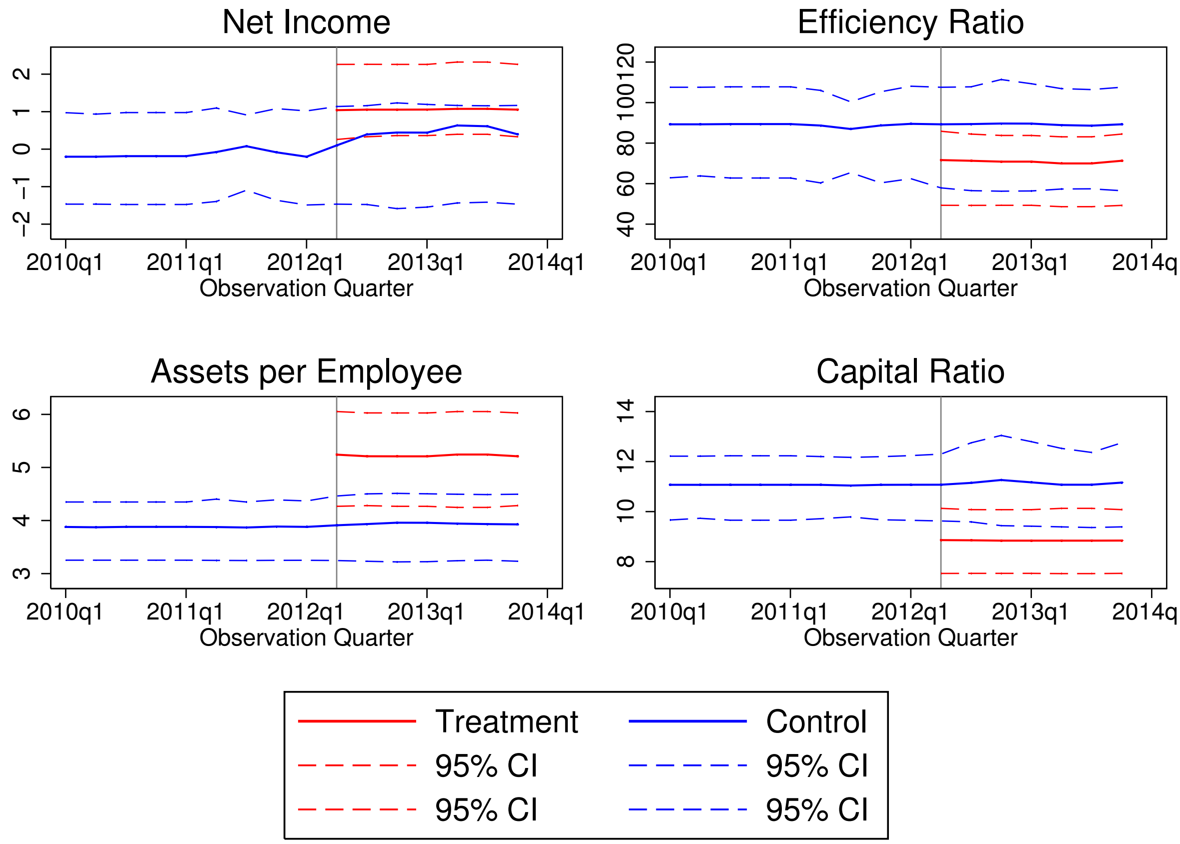

Figure 7: Multiple Time Series - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs (2 of 2)

IV. Conclusion and policy recommendations

The consistency of the estimated treatment effect across data windows from 300 to 900 shareholders suggests that the JOBS Act had a largely beneficial effect on community banks which deregistered in response to the cutoff change. As suggested by the literature and qualitative evidence, the benefits of Exchange Act registration are exceedingly limited for banks that already report financial data to prudential regulators. While the estimated effect in this study is a locally averaged at the point of discontinuity–and thus applies to the entire dataset only under an assumption of homogeneity–there is little reason to suspect substantial heterogeneity among banks within a certain band of shareholders. While this study cannot directly prove homogeneity, it seems intuitively reasonable to suppose that a bank with 1,500 shareholders is not significantly different from a bank with 1,100 with respect to the cost savings of deregistration, since these savings derive from the preexisting parallel reporting to prudential regulators that applies to all banks and is orthogonal to number of shareholders. Accordingly, the results of these study suggest that Congress should consider raising the cutoff further to permit banks and BHCs with more than 1,200 shareholders of record to deregister.

An interesting question for further research is whether the cost savings from deregistration persist with non-bank firms. Those with over $10 million in assets are permitted to deregister under section 12(g) when the number of shareholders falls below 300. Unlike banks and BHCs, non-bank firms are not generally required to report periodic financial data to prudential regulators, suggesting that the net benefits of deregistration for these firms is less clear. Nonetheless, the results of this study do suggest that Congress should pass the Holding Company Registration Threshold Equalization Act of 2013, which extends the JOBS Act deregistration cutoff change to savings and loan holding companies, as these do report to prudential regulators in a manner identical to BHCs.

Two puzzling aspects of these results are the fall in total pretax income and overall equity capital as a result of deregistration. Pretax income is particularly perplexing, as one would imagine that Exchange Act registration would affect expenses alone. The fall in total capital per $1 of assets might result from investors fearing potential reduced liquidity from non-reporting under the Exchange Act. However, as the decrease in total capital is not robust to smaller data windows, it may simply be correlating with an unobserved confounder at the larger data window which is correlated with firm size and not entirely captured by the running variable polynomial.

Finally, it is worth noting that many of the cost savings are likely to be greater in the future. In an interview with an anonymous bank, an individual noted that some of the

Exchange Act-related expenses "are definitely and distinctly realized in 2012 and some are yet to come (e.g., the effects on our insurance) – for instance our full year of audit as a deregistered company has not been experienced, our XBRL assistance contract runs through the year end but we will not have any XBRL costs next year." Accordingly, the long-term effect of deregistration would be an interesting topic for future research.

Appendix

Bandwidth Selection Tests

Below are the results of the McCrary (2008) test with arbitrary bandwidths for the number of shareholders as of December 31, 2011.

Bandwidth	T-statistic
100	N/A
200	-.6777339
300	-.5652987
400	-.55364361
500	-.19802725
600	-.50684398
700	-.31490446
800	-.788729
900	-.98731778

Treatment Effect Graphs - 300 and 600 Shareholder Windows

The following figures present regression discontinuity results at windows of 300 and 600 shareholders.

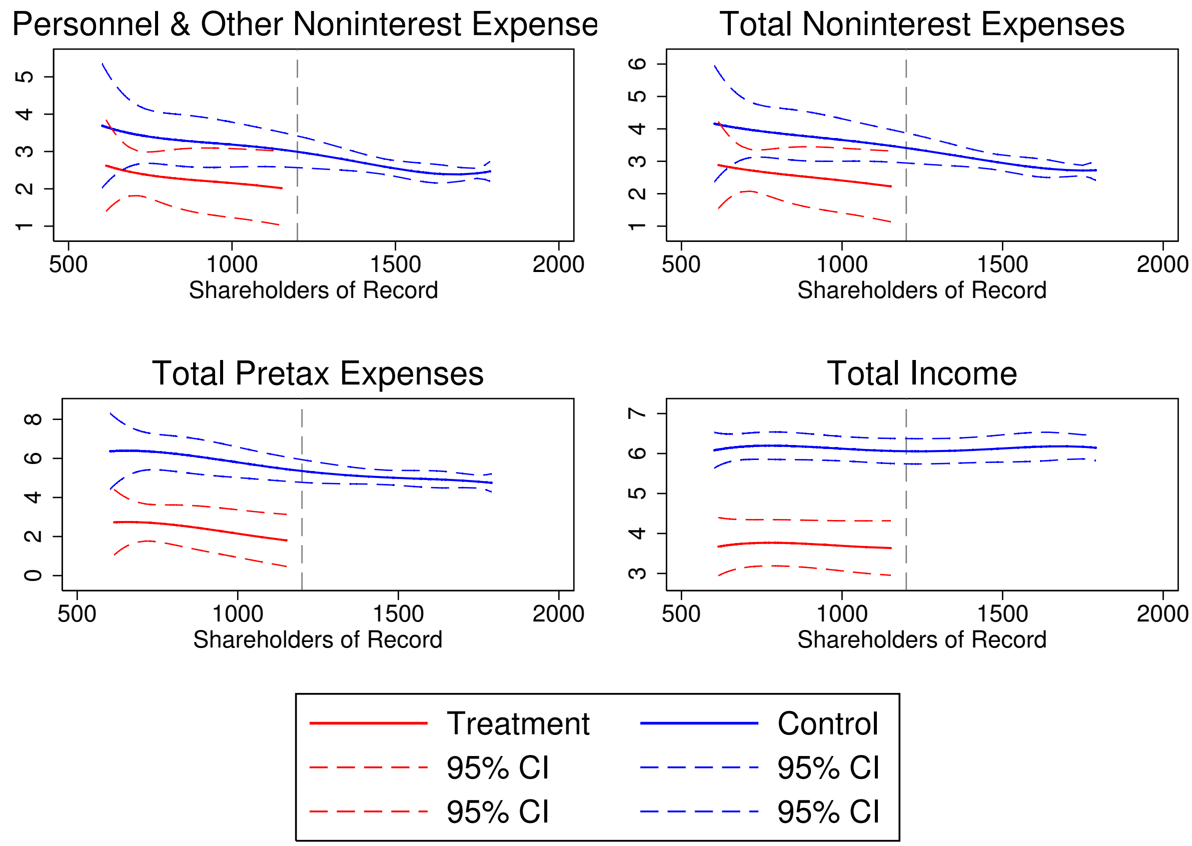

Figure 8: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs - 600 Shareholder Window (1 of 2)

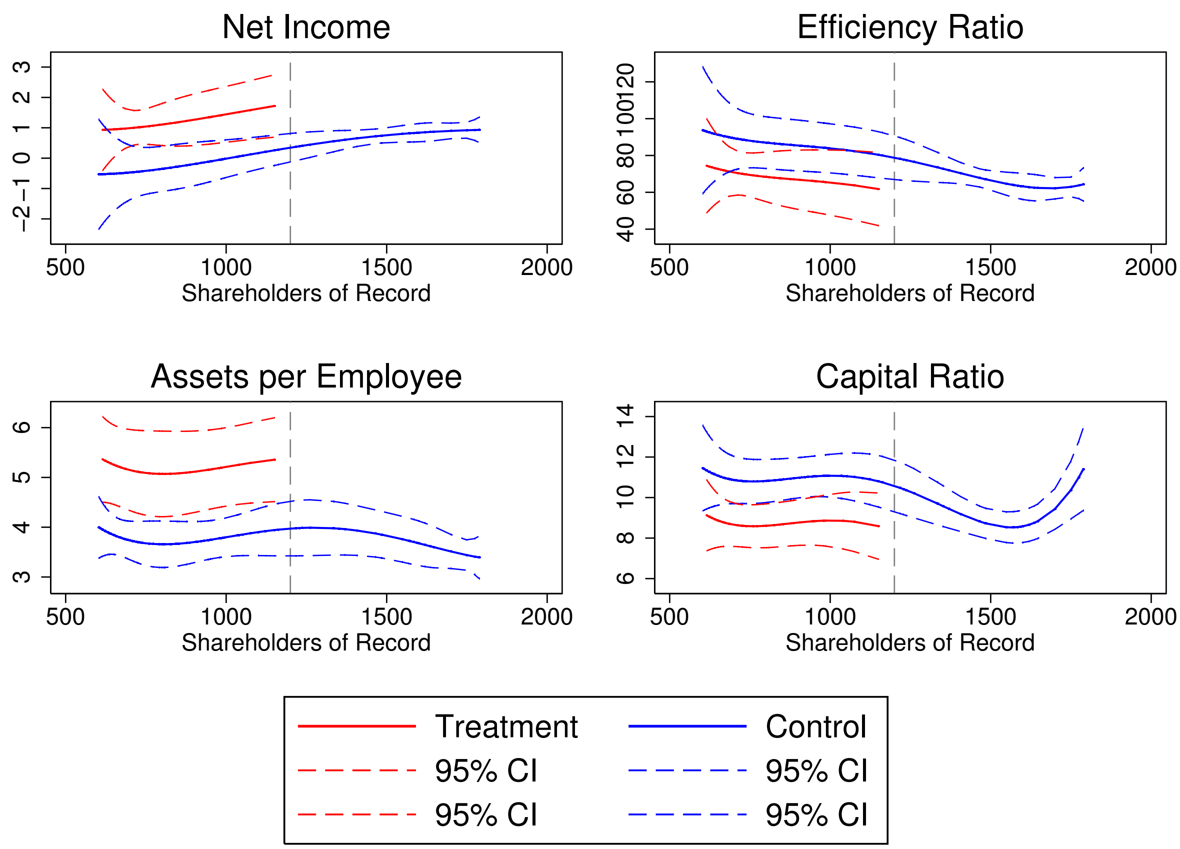

Figure 9: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs- 600 Shareholder Window (2 of 2)

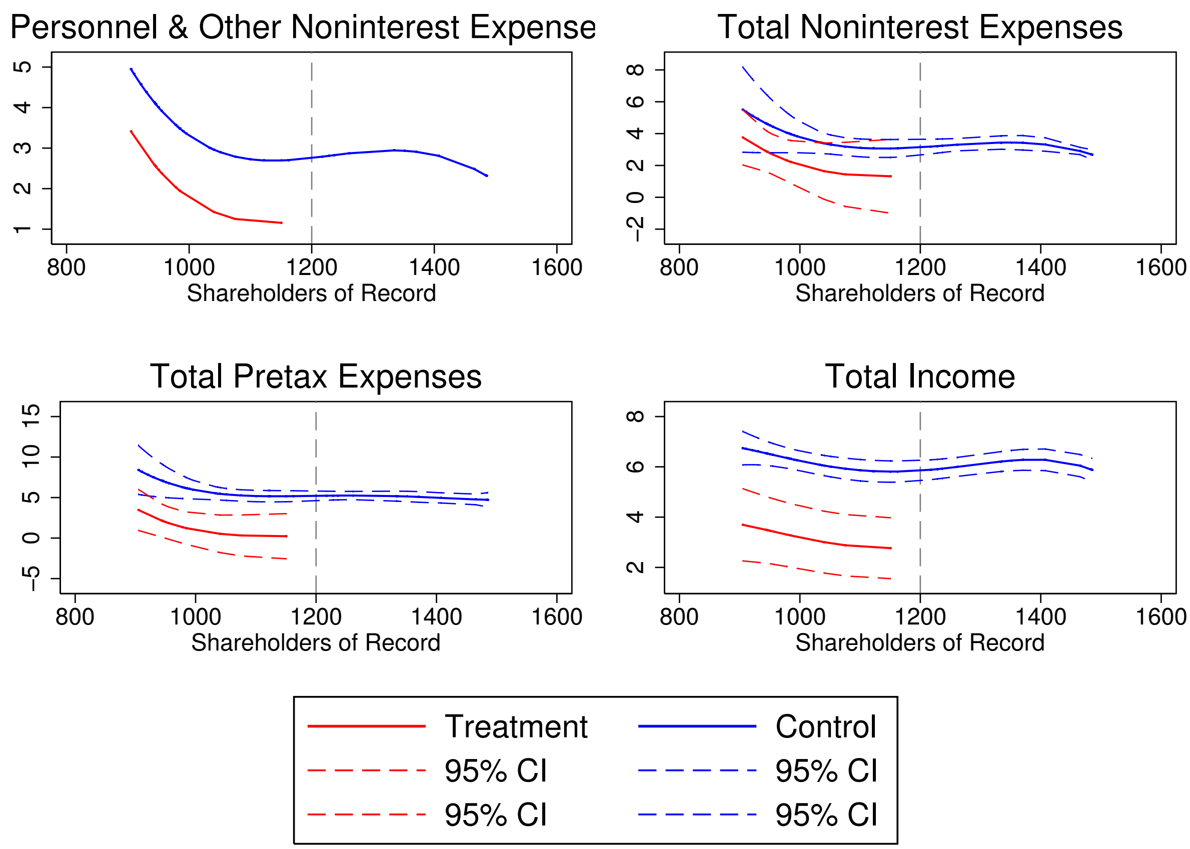

Figure 10: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs - 300 Shareholder Window (1 of 2)

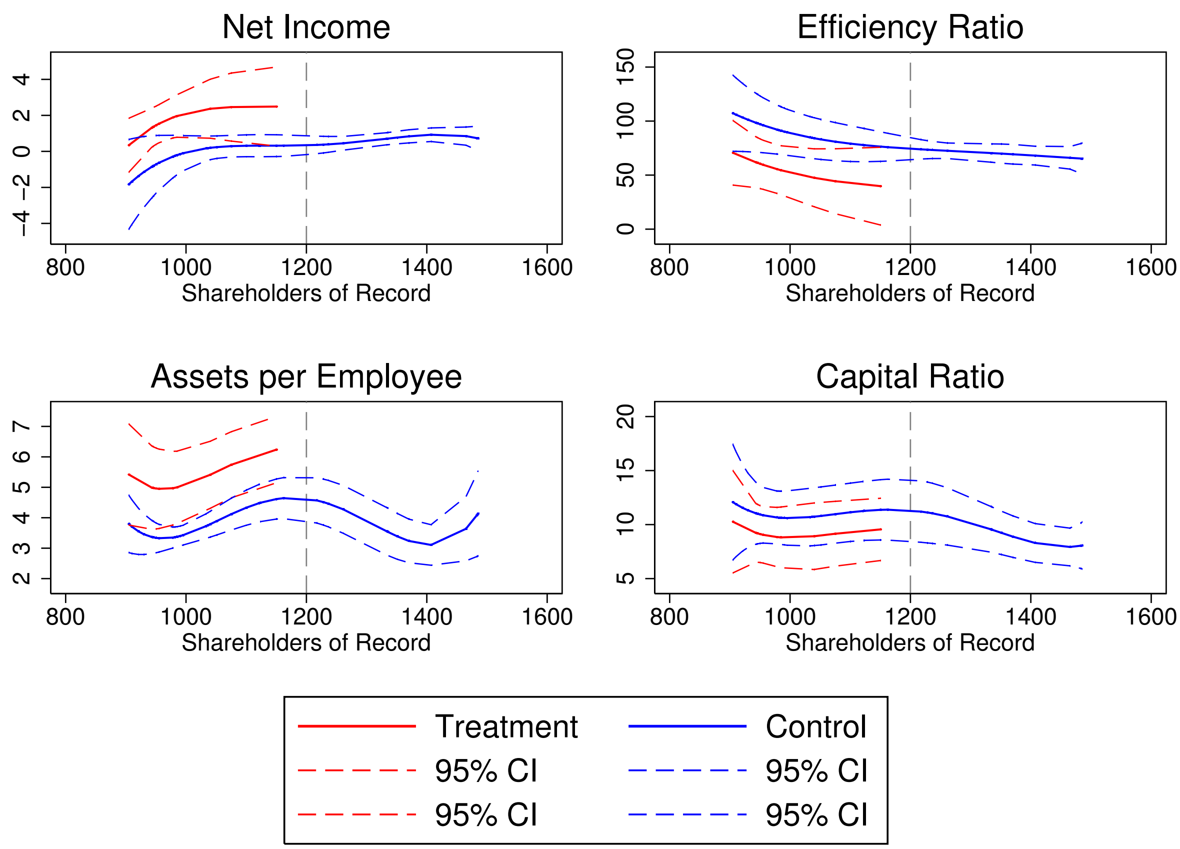

Figure 11: Regression Discontinuity - Effect of JOBS Act Deregistration on Unlisted Banks and BHCs - 300 Shareholder Window (2 of 2)

Robustness Check - Treatment Effect Estimations

The following table presents estimates of the treatment effect controlling for additional co-variates for portfolio composition: (1) short-term non-core funding, (2) domestic and foreign deposits of banks in foreign countries as a percent of total deposits, (3) demand, now, ATS, MMDA and deposits below insurance limit less fully insured brokered deposits, and (4) other borrowing with a maturity greater than one year.

Table 4: Treatment Effect Estimates – Robustness Check with Covariates

	Personnel & Other Noninterest Expenses	Total Noninterest Expenses	Total Pretax Expenses	Total Income	Net Income	Efficiency Ratio	Assets per Employee	Capital Ratio
100	-1.68	-1.46	-6.23	-3.92	2.73	6.72	-5.86	-11.5
$n = 314$	(2.36)	(2.54)	(6.09)	(2.62)	(4.25)	(46.64)	(5.40)	(12.47)
200	-0.85	-0.99	-5.06*	-3.59***	2.13	-21.6	-0.86	-3.30
$n = 891$	(1.31)	(1.45)	(2.66)	(1.38)	(1.84)	(43.56)	(0.73)	(4.07)
300	-0.97	-1.13	-3.94***	-2.62***	1.67*	-24.5	1.02*	-0.90
$n = 1,671$	(0.90)	(0.96)	(1.15)	(0.62)	(0.89)	(17.96)	(0.53)	(1.17)
400	-0.76	-0.94	-3.43***	-2.23***	1.45**	-20.3	0.99**	-1.03
$n = 2,432$	(0.65)	(0.72)	(0.88)	(0.43)	(0.70)	(13.80)	(0.44)	(0.90)
500	-0.63	-0.80	-2.89***	-2.08***	1.07**	-10.9	1.32***	-0.72
$n = 3,096$	(0.46)	(0.50)	(0.62)	(0.33)	(0.48)	(9.34)	(0.51)	(0.70)
600	-0.76	-0.95*	-2.86***	-2.04***	1.11**	-11.1	1.35***	-1.53**
$n = 4,093$	(0.51)	(0.56)	(0.64)	(0.35)	(0.50)	(10.18)	(0.43)	(0.73)
700	-0.67	-0.84*	-2.74***	-2.12***	0.93**	-7.58	1.30***	-1.16*
$n = 4,876$	(0.41)	(0.44)	(0.53)	(0.32)	(0.41)	(8.18)	(0.38)	(0.62)
800	-0.75	-0.91*	-2.72***	-2.08***	0.94**	-17.0	1.22***	-1.30**
$n = 5,325$	(0.49)	(0.51)	(0.56)	(0.30)	(0.48)	(14.27)	(0.35)	(0.57)
900	-0.68	-0.84*	-2.67***	-2.02***	0.95**	-17.2	1.28***	-1.26**
$n = 5,664$	(0.47)	(0.50)	(0.54)	(0.29)	(0.47)	(14.37)	(0.34)	(0.55)

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results are largely consistent with the results in Table 3, albeit with some minor variations and loss of statistical significance on the personnel & other noninterest expenses result. As noted by Lee and Lemieux (2010), including covariates in a regression discontinuity design may "lead to inconsistent estimates of [the treatment effect], and may cause the asymptotic variance to increase" (p. 333 n.44).

UBPR Descriptions

The following codebook provides the official UBPR description for each outcome variable from the Federal Reserve Board Micro Data Reference Manual.

Variable	Description - UBPR Code
Personnel & Other Noninterest Expenses	UBPRE085 (“OTHER OPER EXP (INCL INTANGIBLES)/PERCENT OF AVERAGE ASSETS”) + UBPR7400 (“SALARY EXPENSE AS PERCENT OF AVERAGE ASSETS”)
Total Noninterest Expenses	UBPRE005 (“NON-INTEREST OVERHEAD EXPENSE/PERCENT OF AVERAGE ASSETS”)
Total Pretax Expenses	UBPRE002 (“INTEREST EXPENSE/PERCENT OF AVERAGE ASSETS”) + UBPRE005 (“NON-INTEREST OVERHEAD EXPENSE/PERCENT OF AVERAGE ASSETS”) + UBPRE006 (“PROVISION: LOAN&LEASE LOSSES/PERCENT OF AVERAGE ASSETS”)
Total Income	UBPRE001 (“INTEREST INCOME (TE)/PERCENT OF AVERAGE ASSETS”) + UBPRE004 (“NONINTEREST INCOME/PERCENT OF AVERAGE ASSETS”) +
Net Income	UBPRE013 (“NET INCOME/PERCENT OF AVERAGE ASSETS YTD”)
Efficiency Ratio	UBPRE088 (“EFFICIENCY RATIO”)
Assets per Employee	UBPRE090 (“ASSETS PER EMPLOYEE ($MILLION)”)
Capital Ratio	UBPRJ245 (“EQUITY CAPITAL PLUS MINORITY INTERESTS AS A PERCENT OT TOTAL ASSETS”)

Table 5: Codebook - UBPR Descriptions of Outcome Variables

Notes

[1]As Bushee and Leuz (2005) explain, the number of shareholders of record is often much lower than the number of “actual shareholders” or beneficial owners because “shares are often held in ’street name’ by a brokerage firm or clearinghouse, which counts only as one owner” (p. 238-39). But the number of shareholders of record still increases as shares are exchanged among brokerage firms, clearinghouses, or individual holders.

[2]The local average treatment effect is generalizable to the entire population only under a strong assumption of homogeneity. But even with heterogeneous treatment effects the discontinuity constitutes “a weighted average treatment effect where the weights are directly proportional to the ex ante likelihood that an individual’s realization of X will be close to the threshold” (Lee and Lemieux, 2010, p. 298).

[3]The number of shareholders is reported annually on Form 10-K. Accordingly, this date provides the best approximation of the number of shareholders at the time of deregistration throughout 2012.

[4]In light of the possibility that the treatment effect may have begun in 2011, I also examined whether any BHC or national bank crossed the 1,200-shareholder threshold between December 31, 2010 and December 31, 2011. (The 2010 10-K filings for the 19 FDIC-insured state banks in my dataset were not available in electronic form.) Two BHCs crossed from above to below 1,200 but did not deregister. As non-compliers are excluded from the regression analysis under the fuzzy RD design, this has no effect on my results. Two firms crossed from below to above 1,200. These occasional cases are consistent with the possibility of random movement of the number of shareholders and do not demonstrate that firms can precisely manipulate the assignment variable.

[5]However, in many of the estimations conducted in this study, the number of shareholders polynomial running variables were individually and jointly insignificant, suggesting that the number of shareholders does not correlate with all of the outcomes.

[6]The income statement items are also annualized to permit comparing results across quarters.

[7]While some banks report separate income statement items for "legal" and "accounting/audit" expenses, these cannot be utilized in the study because banks are required to report these legal and accounting/audit expenses separately only if they exceed $25,000 and 3% of the total category of "other noninterest expenses" (FDIC, 2013). Variation between banks in crossing this threshold is non-random and thus highly problematic for inference.

[8]The magnitude of this figure results from bank assets consisting primarily of cash deposited by depositors.

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