Funding and Incentives of Regulators: Evidence from Banking

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Abstract

Regulation is often funded with fees paid by regulated firms, potentially creating incentive problems. We use this feature to study the incentives of regulators and their ability to affect firm behavior. Theoretically, we show that firms that pay higher fees may face more lenient regulation, when leniency increases regulatory budgets in the short term. Our identification approach uses multiple kinks in fee schedules of federal bank regulators as a source of exogenous variation. Using a novel dataset on fees and regulatory actions, we find that firms that pay higher fees face more lenient regulation, which leads to a buildup of risk. Higher fee-paying banks are allowed higher leverage and asset risk, and in the longer term have more loan defaults and a higher likelihood of regulatory actions, which tend to follow banking crises.

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1 Introduction

Incentives and abilities of regulators are frequently called into question, especially following periods of economic turmoil and high-profile accidents. Recent examples of industries where regulatory agencies became subjects of controversies and allegations of failure include pharmaceuticals (withdrawal of painkiller Vioxx), offshore drilling (BP oil spill), and finance (financial crisis of 2008). But empirical work on the incentive schemes of regulators and their effectiveness is scarce, leaving such policy debates largely unaided by evidence.[1]

To study the effects of regulators' incentives, we use the fact that in many industries (including those mentioned above) regulation is funded with fees paid by regulated firms. We show that variation in fees from individual firms can be used to study the incentives of regulators, their ability to alter enforcement in response to monetary incentives, and, generally, shed light on the regulatory process. We test this idea using a novel dataset on regulatory assessment fees and regulatory actions in the US banking sector. Using exogenous variation in these fees, generated by kinks in the fee schedules, we provide new evidence on the interaction between regulators and firms, as well as on the consequences of the user fee model of regulation.

Banking provides a useful laboratory. First, banking data includes measures of risk—a primarily concern for regulators—and measures of potential outcomes of risk-taking, such as bank failures, enforcement actions, and loan defaults. Second, as we discuss below, regulators have leeway in determining bank risk. Finally, kinks in fee schedules faced by banks provide a previously unexplored source of exogenous variation in the revenues of regulatory agencies.

Our data covers all banks supervised by the two primary regulators of national banks and thrifts: The Office of Thrift Supervision (OTS) and The Office of the Comptroller of the Currency (OCC). During our sample period, which spans almost three decades, these agencies oversaw 65% of banking assets. About 95% of their funding comes from supervised banks, and the remainder from interest on their past budget surpluses. They received no appropriations from Congress.

[1] Following withdrawal of Vioxx, an FDA epidemiologist testified that Vioxx was a "profound regulatory failure," and that the FDA "is incapable of protecting America against another Vioxx." (Senate Committee on Finance, 2004). The BP Oil Spill Commission Report (2011) calls the incident an "inexcusable, shortfall in supervision." The offshore oil drilling regulator (Minerals Management Service) was reorganized following the Deepwater Horizon spill. Following the 2008 financial crisis, the Office of Thrift Supervision—regulator of such institutions as WaMu, IndyMac, and AIG—was accused of failure and abolished. OTS itself was preceded by the Federal Home Loan Bank Board, which also closed following the Savings and Loans Crisis (Senate Permanent Subcommittee on Investigations, 2010).

To clarify the theoretical implications of regulation funded with user fees, we present a stylized model of a regulator who negotiates the choice of risk with a firm. The firm has bargaining power because it can deprive the regulator of its fees: it could forgo the project, scale down, or even decide to exit. In finance, such a threat is far from a hypothetical possibility—many academics and policy makers are concerned with the ability of banks to avoid regulation by moving assets off balance sheet (Acharya, Schnabl, and Suarez, 2013; Kisin and Manela, 2015), or, more generally, shift operations into the unregulated "shadow banking" sector (e.g., Gorton, 1994; Adrian and Shin, 2009; Gorton and Metrick, 2010; Gorton, 2010). We show that under plausible conditions, regulators accept higher risk in higher fee-paying firms. Intuitively, higher fee income alleviates the negative effect of higher risk. The model, therefore, shows how exogenous variation in fees may help identify the effects of monetary incentives on the regulatory process. This "leniency-for-fees" channel applies more broadly than the banking context by broadening the definition of risk consequences (prescription drug-related fatalities, oil spills, etc.).

Empirically isolating the effect of fees is hard, because they are determined by bank size, and therefore correlated with unobserved bank characteristics. Moreover, fees are a deterministic piecewise-linear function of size, which precludes the use of conventional instrumental techniques. To address this, we use the "kinked" structure of the fee schedule. Bank size affects fees differently depending on which side of a kink a bank happens to be located. This discontinuity in slopes allows us to identify a treatment effect of fees, applying the sharp regression kink design ("RKD," Card, Lee, Pei, and Weber, 2012). Intuitively, we look for kinks in the relationship between an outcome variable (e.g., bank risk) and bank size, which coincide with kinks in the fee schedule.

Importantly, the kinks significantly affect regulatory revenues; a one percent increase in assets has a 0.17 percentage point higher effect on fees on the left side of the average kink than on its right. Because the average elasticity on the left is 0.76, the "first-stage" effect is a 22 percent decrease in the elasticity of fees to bank size. Another way to gauge the importance of kinks for regulatory budgets, is to ask, how much would the budget change if instead of one average-sized bank the regulator would be in charge of two banks half its size. Intuitively, the change would be zero if there were no kinks in the fee schedule.[2] For the mean bank in our sample, such an exercise would raise the fee revenue by sixty thousand dollars, or 17 percent. For the median bank it would raise

[2]We thank Luigi Zingales for suggesting this exercise.

the revenue by sixteen thousand dollars, or 29 percent.

Another attractive feature of our empirical setup is that the fee schedules feature multiple kinks along the distribution of bank sizes. This ensures that our results are not driven by a small group of banks from a particular part of the size distribution. The existence of multiple kinks, however, together with changes in kinks across regulators and time, also complicates a straightforward application of the RKD in our setting. Therefore, we extend the basic framework to account for multiple kinks and time variation in kink positions and slopes.

We find that banks that pay higher fees take more risks, as measured by leverage and the riskiness of assets. A 1% increase in regulatory fees paid by a bank decreases its regulatory capital ratios by 2.3%, and increases its expected loan losses by 3.5%. These findings suggest that the user fee system has a significant effects on the regulatory process. As predicted by our theoretical model, banks that pay higher fees face more lenient regulation. We find that the effect of fees is similar across regulators, and report a single treatment effect of fees for all kinks in our sample.

We further extend the RKD framework to study possible dynamic effects of fees on bank outcomes. Intuitively, while regulation may affect some outcomes immediately (e.g., capital ratios), other outcomes (e.g., bank failures, regulatory actions, loan defaults) are likely to respond slowly to changes in regulatory strictness. Moreover, fees may affect banks' future assignment around the kinks, which could bias estimates from a regression of future outcomes on current fees. We explore the delayed effects of fees by adapting the dynamic regression discontinuity (RD) design developed by Cellini, Ferreira, and Rothstein (2010). This approach allows estimating both the effect of current fees on future outcomes that includes their indirect effect through interim fees (intent-to-treat), and the pure effect of current fees on future outcomes (treatment-on-treated).

We find that fees have significant effects on future regulatory enforcement actions, loan default rates, and bank failures. Higher fees increase enforcement actions after 3 quarters, and increase noncurrent loans after 7–8 quarters. We document that regulators tend to initiate corrective actions mostly following banking crises, which implies that higher fees lead to a buildup of risk in regulated banks. The results on regulatory actions and loan defaults capture delayed consequences of risks taken in response to lax regulatory treatment. Interestingly, higher fees *decrease* the probability of failure after 5–7 quarters. This result is consistent with anecdotal evidence that regulators delay the closure of banks that are important for their budget, or try to avoid revelations of past leniency.[3]

Our paper contributes to several strands of literature on the economics of regulation. To the best of our knowledge, we provide the first causal evidence—in finance and elsewhere—on the effect of regulatory fees on the outcomes of regulation. Besides providing rare evidence on the effects of regulators' incentives, we directly relate regulatory outcomes to the user-fee model of regulation. This model is used in many industries, such as aviation, oil and gas, pharmaceuticals, and antitrust, and is commonly motivated by efficiency improvements and a reduced burden on taxpayers. It is often feared, however, that the user-fee model can lead to regulatory capture and favoritism. Our paper is the first causal evidence on this issue.[4] More broadly, our findings expose a similarity between user fee-funded government regulators and private auditors, such as credit rating agencies and self-regulatory organizations (see, e.g., Bolton, Freixas, and Shapiro, 2012, for a theoretical treatment). Duflo, Greenstone, Pande, and Ryan (2013) study private pollution auditors in India, who are chosen and paid by firms. They find that the existing system is corrupted, but audit quality can be improved by altering regulators' incentives.

In the context of financial regulation, our study is related to Agarwal, Lucca, Seru, and Trebbi (2014) and Kroszner and Strahan (1996; 1999). Agarwal, Lucca, Seru, and Trebbi exploit exogenous rotations between federal and state supervisors of state-chartered banks to show inconsistency in regulatory outcomes. In addition, they document that larger bank size is associated with regulatory leniency, but do not identify the effect of fees. Kroszner and Strahan (1996) show that during the S&L crisis in the 80s, regulators kept insolvent thrifts alive by influencing the allocation of private capital. Kroszner and Strahan (1999) find that pressure from interest groups affected the implementation of the interstate branching regulation.[5]

[3]The US Senate Permanent Subcommittee on Investigations report dated April 16, 2010 on the failure of Washington Mutual Bank, a nationally-chartered thrift, concluded that "OTS repeatedly identified significant problems with Washington Mutual's lending practices, risk management, and asset quality, but failed to force adequate corrective action, resulting in the largest bank failure in U.S. history." Washington Mutual provided 12–15% of OTS revenue from 2003 to 2008, which can explain the OTS's reluctance to place it in FDIC receivership.

[4]For the benefits, see President's FY 2014 Budget Analytical Perspectives. Philipson, Berndt, Gottschalk, and Sun (2008) provide evidence of efficiency gains. They document shorter drug approval times by the FDA after user fees were introduced, but point out that other events may have led to this result. See BP Oil Spill Commission Report (2011) for a critique of the user-fee model of regulation.

[5]See also Barth, Caprio, and Levine (2004), Lucca, Seru, and Trebbi (2014) and Shive and Forster (2013). Rosen (2003), Calomiris (2006) and White (2011) discuss the effects of competition between regulators. We examine the role of competitive regulation in Section (5.3). Theoretically, the effect of fees does not rely on competition across regulators, but could be exacerbated by it. We do not find significant difference in the effect of fees across agencies, and our tests are consistent with Agarwal, Lucca, Seru, and Trebbi (2014) who find that bank and time effects effectively deal with the issue of charter shopping.

This literature convincingly shows that regulation depends on the identities and incentives of regulators, and not just driven by laws and rules. We complement and extend this literature by measuring the effects of the structure and sources of regulatory revenues. The sensitivity of regulatory conduct to monetary incentives implies that effective design of regulatory agencies should take this issue into account. Our evidence is particularly informative for studies on the design of regulatory agencies, since we examine the variation in incentives *within* agencies. This allows isolating the effect, holding constant other characteristics of regulators and firms. Moreover, our focus on revenues is particularly advantageous from a policy perspective. Revenue structure is a generally available policy tool with a clear interpretation. Its power stems from general economic tradeoffs, and does not hinge on particular features of a given sector. Therefore the insights gained in one sector are likely to be relevant in other settings.

Methodologically, we contribute to a growing literature that applies RKD to important economic questions, following the seminal contributions of Nielsen, Sørensen, and Taber (2010) and Card, Lee, Pei, and Weber (2012).[6] Often, the method is applied in settings with multiple and time-varying kinks. While this feature may provide an important advantage—in our setting, it increases statistical power and allows estimating the effects from across the size distribution—it introduces heterogeneity across kinks and over time. We show that pooling data across kinks, while ignoring such heterogeneity may lead to severe bias. We provide a parsimonious and easily implementable adjustment of the RKD that accommodates kink heterogeneity, while still allowing researchers to pool observations across multiple kinks in estimating treatment effects. Our extensions of RKD to multiple kinks and dynamic effects could prove useful in future applications.

The paper proceeds as follows. Section 2 describes our theoretical framework and derives the estimating equations. Section 3 describes banking regulation in the United States, the roles of OCC and OTS, and our data on regulatory fees. Section 4 describes our empirical approach and the application of the RKD in our setting. Section 5 reports our empirical results. Section 6 examines their robustness. Section 7 concludes.

[6] Ganong and Jäger (2014) survey more than 20 RKD applications in the last 5 years.

2 Model

Our goal in this section is to provide a simple economic framework for the empirical analysis of the effects of user-fee models on regulatory incentives. We start by modeling the objective of a regulator with respect to firm risk. There is little theoretical or empirical work to guide us in the choice of the regulator's objective function.[7] Since we want to analyze incentives generated by the fee structure, the regulator in our model has a preference over net fee income, $u(q,x) = f(q) - c(q,x)$, where $f(q)$ is fee revenue. As in our institutional setting, the fees are determined by firm size. The cost or regulation $c(q,x)$ may include the regulator's private cost of supervision, and the social costs net of social benefits from a firm of size q bearing risk x.

This objective could raise two immediate questions. First, why would regulators care about fee income? This assumption is motivated by the fact that for the banking regulators we study labor costs take up 70–80% of the fee income, as we show below in Figure 1. Therefore, in our stylized model, the fees in the objective function play the role of labor income and job security. Second, this objective ignores other factors that could be valued by the regulator, such as social welfare from bank lending. Such preferences may introduce additional tradeoffs, potentially mitigating the effect of fees. We abstract from these factors since our goal in this section is to highlight a potential mechanism for the effect of fees on incentives in the simplest possible setting. Importantly, our empirical application does not impose specific preferences and we estimate the equilibrium net effect of fees non-parametrically. Specifically, we use a nonparametric identification framework developed by Card, Lee, Pei, and Weber (2012), which allows for non-separability between f, q, and the error term, and for nonlinearities in the dependency of risk on size.

The regulator and the firm negotiate the choice of risk x. The firm is assumed to maximize its profits net of regulatory fees $\pi(q,x) - f(q)$. We assume that q is set by the time of the negotiation and it is known to both parties. This assumption reflects the regulatory practice in our setting.[8]

[7] The theoretical literature on optimal regulation often considers a benevolent planner maximizing social welfare. See, e.g, Baron and Myerson (1982) and Laffont and Tirole (1986). Boot and Thakor (1993) consider a self-interested bank regulator concerned with its reputation. Dewatripont and Tirole (1994) allow banking regulators that care more about the value of deposits than social welfare. A separate literature relates prevailing forms of regulation to political bargains and institutions. See for example, Stigler (1971); Peltzman (1976); Shleifer and Vishny (2002), and more recently Calomiris and Haber (2014).

[8] End-of-period $t-1$ book assets q_{t-1} determine regulatory fees f_t paid at time t, in the case of the banking regulators we study. Therefore, future fees are mostly known before period t actions take place. Importantly, a bank can leave its federal regulator in favor of a state charter up until the last day of period $t-1$. Whether this simple bargaining channel applies in other settings depends on the ability of the firm to walk away from the negotiating

Assume a Nash bargaining solution, so that firm risk x maximizes the regulator and the firm's bilateral Nash product with bargaining power parameter $\beta \in (0,1)$

$$\max_x \left[f(q) - c(q,x)\right]^{\beta} \left[\pi(q,x) - f(q)\right]^{1-\beta}. \tag{1}$$

This setup is widely used in empirical bargaining models (e.g., Crawford and Yurukoglu, 2012), and nests the special cases where choice of risk is made solely by the regulator ($\beta = 1$) or the firm ($\beta = 0$). While the objective of both the regulator and the firm may be aligned over some range of firm size q and risk x, we assume that around the optimal choice of risk, they pull in different directions, so that the optimal risk is an interior solution (i.e. the second-order conditions hold). We normalize the outside options to zero, without loss of generality, since both regulatory costs and firm profits can be thought of as net of each party's disagreement payoff.

The optimal choice of x equates the bilateral marginal benefit and marginal cost, weighted by each party's bargaining power:

$$\beta \frac{c_x(q,x)}{f(q) - c(q,x)} = (1-\beta) \frac{\pi_x(q,x)}{\pi(q,x) - f(q)}. \tag{2}$$

The marginal cost of an increase in risk x is the percent decrease in the regulator's net fees times its bargaining power, while the marginal benefit is the percent increase in the bank's net profit.

The effect of an increase in fees on the equilibrium risk is apparent from (2). Higher fees increase the regulator's payoff, therefore diminishing the effect of higher costs in percentage terms. Moreover, higher fees increase the marginal benefit because the percent increase in firm profits is relative to profits net of fees. Both effects work to increase the resulting risk, although we expect the first to be much stronger than the latter in our setting because fees are a major source of regulator revenue but only a minor cost for banks.

Assuming that higher risk increases both firm profits and the cost of regulation, the effect of an exogenous increase in fees on risk is positive:

$$\frac{d\log x}{d\log f} = \frac{\frac{f(q)}{f(q)-c(q,x)} + \frac{f(q)}{\pi(q,x)-f(q)}}{\frac{1}{1-\beta}\frac{c(q,x)}{f(q)-c(q,x)}\epsilon_{cx} + \sigma_{cx} - \sigma_{\pi x}} > 0, \tag{3}$$

table, either directly by leaving for a competing regulator, or by reducing the size of a project.

where $\sigma_{\pi x} \equiv \frac{\pi_{xx}(q,x)x}{\pi_x(q,x)}$, $\sigma_{cx} \equiv \frac{c_{xx}(q,x)x}{c_x(q,x)}$, and $\epsilon_{cx} \equiv \frac{c_x(q,x)x}{c(q,x)}$. Equation (3) shows that the treatment effect of fees is determined by the importance of fees in the payoffs of the regulator and the firm, and by the effect of risk on these payoffs. The elasticity of risk with respect to fees is larger when fees are large relative to the payoffs of the regulator or the firm, when the regulator has little bargaining power β, when the elasticity of costs to risk ϵ_{cx} is small, or when the relative convexity of the cost function σ_{cx} is not much larger than that of the profit function $\sigma_{\pi x}$.

Equation (3) also highlights identification challenges in estimating the effect of regulatory fees. Since fees are determined by size q, any variable that affects fees f, will also affect firm size q, thereby violating the independence assumptions required for an instrument. Moreover, unobserved factors can be correlated with the sensitivity of profits to risk, firm size, and fees. Our implementation of the RKD addresses these issues.

3 Institutional Background and Data

3.1 Regulation of Banks and Thrifts in the United States

A depository institution can choose between a bank or a thrift charter, and whether it is a federal or state charter. This choice determines their primary regulator. Federally-chartered banks are regulated by the Office of the Comptroller of the Currency (OCC). Federally-chartered thrifts were regulated by the Office of Thrift Supervision (OTS), until 2011 when it was closed and subsumed by the OCC. State-chartered banks and thrifts are regulated jointly by each state's chartering authority and by either the Federal Deposit Insurance Corporation (FDIC) or the federal reserve system (Fed).[9]

We focus on the regulators of nationally chartered banks and thrifts—the OCC and the OTS. Unlike other federal banking regulators (Fed and FDIC), these agencies are almost entirely funded with assessment fees paid by the regulated banks and receive no appropriations from congress. Figure 1 shows the breakdown of revenues and costs of these agencies over time. On average, assessment fees accounted for 96 percent of the OCC's revenues. A small addition to its revenue comes from interest income on its accumulated savings from historical budget surpluses. The bulk

[9] Moreover, the Fed supervises bank-holding companies and the FDIC has backup authority over all depository institutions. Blair and Kushmeider (2006) review of the history and challenges of this "dual banking system."

of its supervisory costs, 67 percent on average, cover labor costs (personnel compensation and benefits). The data from OTS show similar patterns.[10]

3.2 Data

We collected a novel dataset of all fee schedules for OCC from 1985 to 2014, and for OTS from 1990 to 2012. A Notice of OCC or OTS Fees for each year is usually published towards December of the previous year, though in some years fees are keep constant, or change midyear. These regulatory bulletins specify semi-annual assessment fees due January 31 and July 31 based on call report information as of December 31 and June 30, respectively. Older bulletins were retrieved from the Westlaw legal research database and recent ones online.[11]

We merged this information with the Research Information System database maintained by the FDIC, which contains bank-level quarterly call/thrift reports for the entire period. Importantly, the FDIC records the identity of the primary regulator of each insured depository institution. Over our sample, on average, the OCC regulated 2,800 banks holding 50 percent of US bank assets. The OTS regulated 1,600 banks holding 15% of the assets. Our analysis omits, on average, about 7,000 state-chartered banks holding 35% of the market, because their fee structures are somewhat heterogeneous, increasing substantially the data collection and classification effort.

An example fee schedule for OCC appears in Table 1. Fees are a deterministic function of total balance-sheet assets of the regulated bank. Specifically, fees are a non-decreasing piece-wise linear function of bank size, with mostly decreasing slopes. This regressive fee schedule implies that the marginal cost of regulation per-dollar of assets decreases in bank size. Such a fee structure makes sense if, for example, the costs of regulating banks are increasing but concave in bank size. OTS fee schedules follow the same structure, but use different cutoffs and marginal rates. The summary statistics in Table 2 show that from a regulated bank's perspective, regulatory fees account for 1.3 percent of its noninterest expense, or 0.7 percent of its operating expense.[12]

[10] State-chartered depository institutions pay assessment fees to state regulators, which are often cheaper because a portion of the costs of the supervision are borne by the FDIC and Fed. The FDIC is funded by deposit insurance premiums, and the Fed is funded by interest earned on its securities holdings.

[11] Recinded bulletins were accessed at http://www.occ.gov/news-issuances/bulletins/rescinded/occ-rescinded.html and http://www.occ.gov/news-issuances/bulletins/rescinded/ots-thrift-bulletins-rescinded.html.

[12] The OCC and OTS also include a surcharge for banks with high (bad) CAMELS ratings above 2. The OCC gives a 12 percent reduction in fees to non-lead banks belonging to a multiple national bank organization. These proportional fee changes preserve both kink locations and fee elasticities and therefore do not affect our analysis.

Our identification strategy exploits kinks in the regulatory fee functions. The lower panel of Table 2 reports the average differences in the elasticity of fees to balance-sheet assets moving from the left to the right of each kink. The table shows that kinks 1, 3 and 9 exhibit the largest slope changes while the remaining kinks are rather small. It turns out that our results are mostly due to these larger kinks, though our main analysis pools information from all kinks in the fee functions of the OCC and OTS over the entire sample. Both agencies mostly keep the kink points fixed in nominal terms, but change the marginal rates at times, mostly to index them to inflation.

Figure 2 shows that the average kink is substantial. A one percent increase in assets on the left of the average kink yields a 0.17 percentage point higher fees than on its right. Because the average elasticity on the left is 0.76, the "first-stage" effect is a 22 percent decrease in elasticity.

The theoretical model of Section 2 predicts a positive relationship between bank risk and regulatory fees. Since there is no unified definition for bank risk-taking, we examine two separate sets of risk measures: measures of bank capital and measures of asset risk.

The first set of risk measures are regulatory capital ratios. A bank with a higher capital ratio is less risky in that it is less likely to default on its debt. It is well known that the raw relationship between the regulatory capital ratios and bank size is decreasing (e.g., Kisin and Manela, 2015). This correlation, however, does not uncover the effect of fees on capital ratios since size is potentially correlated with other bank characteristics, such as profitability. Bank regulators supervise leverage to make sure that capital ratios match the risks taken by banks. As a result, regulators have a substantial leeway in determining the appropriate capital ratios.[13]

Our second set of risk measures focuses on the riskiness of banks' assets. We examine three commonly-used measures of realized or expected loan losses. The first is the noncurrent loans to loans ratio, which would be high if many of the loans held by the bank are seriously past due (over 90 days) or not accruing interest. The second is the loss-reserve to loans ratio, which is higher if banks that set aside large allowances for expected losses. The third is the loss-reserve to noncurrent loans ratio, which increases as a bank recognizes realized and impeding losses on its loans.

Previous literature has documented a tension between the accounting rules and regulatory treatment of loan loss reserves (see, e.g., Balla and Rose, 2011, for a review). Loan loss reserves,

[13]The relevant rules state, "banks should maintain capital commensurate with the level and nature of risks, including the volume and severity of adversely classified assets, to which they are exposed" (12 CFR Part 325, Appendix B).

according to accounting standards, should account for *imminent and probable* losses, making them a good measure of expected losses. Bank supervisors, however, might wish to increase them further to enhance bank stability. This has interesting implications for our setting. On the one hand, banks with riskier loans would expect higher losses and therefore would have to maintain higher reserves. This would predict a positive effect of fees on loss reserves to loans ratios, but no effect on the loss reserves to noncurrent loans ratio. On the other hand, if lax regulation leads to smaller reserves, we may see a negative effect of fees on the loss reserves to loans ratio, and a negative effect on the loss reserves to noncurrent loans ratio.

Bank regulators can also take enforcement actions against banks or individuals employed by banks, and impose monetary penalties. Figure 3 plots the fraction of banks under corrective actions imposed by OCC and OTS.[14] A striking pattern that emerges from these data is the clustering of regulatory actions following banking crises. To take a closer look at this relationship, we add the fraction of failed/assisted banks to Figure 3. Corrective actions appear to follow the spikes in bank failures, with a delay of approximately four quarters. Echoing this stylized fact, in our analysis below we document a strong dynamic pattern in the effect of fees on corrective actions: prior risk taking increases the probability that regulators will have to take an action against a bank during the crisis.

4 Empirical Methodology: Regression Kink Design with Multiple Time-Varying Kinks and Dynamic Effects

Our identification strategy uses kinks in the fee function to exogenously vary fees to identify their treatment effect on risk. Importantly, the estimates reflect the effects of higher fees on the dependent variable and not the effects kinks themselves.

We build on a nonparametric identification framework of Card, Lee, Pei, and Weber (2012), which allows non-separability of the error term, and for nonlinear effects of firm size. We do so because even in our simplified model, the effects of higher size q on risk x are potentially nonlinear.

[14] Our definition of "corrective actions" includes prompt corrective actions, cease and desist orders, safety and soundness orders, decision/opinion orders, capital directives, securities enforcement actions, and formal supervisory agreements. Monetary penalties and personal actions are not included in this definition. Proceeds from monetary penalties are deposited into the Treasury general fund. See 12 U.S.C. 1818(i)(2) and 12 U.S.C. 1467a(i)(2).

Moreover, this specification allows for unobservables to enter the risk equation in a flexible way.

We begin by describing the single-kink regression kink design (RKD). We then show how to apply this estimator in our setting, which has two distinctive features. First, there are multiple kinks in the assessment fee schedules, and their location and magnitude vary over time and across regulators. Second, we adjust the estimation strategy for potential dynamic effects of regulation on the outcome variables. Our extension gives us more power to estimate the treatment effects, and highlights some pitfalls that could occur if one ignored cross-sectional or time-series heterogeneity in kink slopes.

Card, Lee, Pei, and Weber (2012) study a general single kink model, specifying that for each observation i, the outcome y_i conditional on the regressor of interest $b(v_i)$, the assignment variable v_i, and an unobservable shock ε_i

$$y_i = y\left(b\left(v_i\right), v_i, \varepsilon_i\right),$$

where the outcome can be a non-separable function of $b(v)$, v, and ε. The key to identification is a "smooth density assumption," stating that, the density of v conditional on ε is continuously differentiable in v for all v and ε. That is, the assignment variable v (bank size in our case) cannot have kinks of its own. In these models, one can identify the treatment-on-the-treated (TOT) parameter as

$$\beta^{\text{TOT}} = \frac{\lim_{v_0 \downarrow k^+} \frac{dE[y|v]}{dv}|_{v=v_0} - \lim_{v_0 \uparrow k^-} \frac{dE[y|v]}{dv}|_{v=v_0}}{\lim_{v_0 \downarrow k^+} b'\left(v_0\right) - \lim_{v_0 \uparrow k^-} b'\left(v_0\right)}. \tag{4}$$

In our setting, (4) says that the effect of fees $(b(v_i))$ on y (e.g., capital ratio) is identified from the discontinuous change in the slope of y as a function of v (bank size). In other words, the treatment effect is identified when the kink in the relationship between size and capital coincides with the kink in the fee schedule.

Card, Lee, Pei, and Weber (2012) suggest using a local polynomial regression for estimation:

$$y_i = \sum_{p=1}^{P} \beta_p \left(v_i - k\right)^p D\left(v_i > k\right) + \sum_{p=0}^{P} \alpha_p \left(v_i - k\right)^p + \varepsilon_i, \tag{5}$$

where observations are weighted by a kernel $K\left(\frac{v-k}{h}\right)$ over a bandwidth h, giving relatively more weight to observations closer to the kink. The treatment-on-treated is identified by the scaled regression coefficient

$$\hat{\beta}^{\mathrm{TOT}} \equiv E\left[\frac{\partial y\left(b,k,\varepsilon\right)}{\partial b}\right] = \frac{\hat{\beta}_1}{\Delta},$$

where $\Delta \equiv b'\left(k^+\right) - b'\left(k^-\right)$ is the change in slopes at the kink. Intuitively, if the regression finds a large change in slopes of y as a function of v at the kink (β_1 is large), when the change in slope in the assignment function $b\left(v\right)$ at the kink is small, then the effect of b on y must be larger than in the case when Δ is large. Note that in this single kink setting Δ is constant across observations.

4.1 RKD With Multiple Time-Varying Kinks

In many applications of RKD, the kinked function $b\left(v\right)$ is not constant. In our setting, while the fee schedules are deterministic and known for each observation, they change over time and across regulators. In addition, there are multiple kinks in each fee function. This is highly advantageous, as it allows us to estimate the treatment effect using firms across the size distribution. Another important advantage is that this feature can be used to increase statistical power—a common problem with discontinuity designs—by pooling observations across kinks. In order to do this, however, the basic RKD methodology needs to be adjusted to account for the heterogeneity across kinks. As we show in Section 6.3, ignoring such heterogeneity, may lead to severe misspecification and biased estimates.[15]

We therefore extend the single-kink nonparametric specification (5) to allow for multiple kinks:

$$y_{ij} = \sum_{p=1}^{P} \beta_p \Delta_j^p \left(v_{ij} - k_j\right)^p D\left(v_{ij} > k_j\right) + \sum_{p=0}^{P} \alpha_{jp} \left(v_{ij} - k_j\right)^p + \varepsilon_{ij}, \tag{6}$$

where j indexes observations in the neighborhood of the same kink j of a unique regulatory fee schedule. Since bank regulatory fee schedules change about once a year and differ across regulators, observations with the same j subscript have the same regulator, and roughly the same year and size. The model assumes a constant treatment effect but properly allows the controlling polynomial coefficients α_{jp} to vary across kinks. Intuitively, a P-th order Taylor expansion around each kink k_j involves different coefficients for every kink j. Moreover, we include $\Delta_j \equiv b'\left(k_j^+\right) - b'\left(k_j^-\right)$ in the "instrument" and recover the treatment-on-treated effect directly as $\hat{\beta}^{\mathrm{TOT}} = \hat{\beta}_1$.

[15] The problem is similar to what happens when the true model depends on real dollar values, but the econometrician uses nominal explanatory variables unadjusted for inflation. A trend in inflation can overwhelm the true effect and lead one to draw the opposite conclusions from the data.

We specify our empirical model in logs because the bank size distribution is highly skewed. The assignment variable is therefore $v_{ij} = \log q_{ij} = \log Assets_{ij}$ of bank i, which belongs to kink $k_j = \log q_j = \log Assets_j$. The changes in slope multipliers in the log specifications are the changes in elasticities $\Delta_j \equiv \left[f'\left(q_j^+\right) - f'\left(q_j^-\right)\right]\frac{q_j}{f(q_j)}$ reported in the lower panel of Table 2. Based on the recommendations of Calonico, Cattaneo, and Titiunik (forthcoming) and Ganong and Jäger (2014) we focus on polynomial degrees of $P = 2$ or 3, and use a triangular kernel throughout.[16]

4.2 RKD with Dynamic Effects

Regulation may have dynamic effects on banks for two reasons. First, bank outcomes may respond slowly to regulatory actions. This issue is most likely to affect the estimates of the effect of fees on bank failures, regulatory actions, and loan defaults. Intuitively, while regulatory leniency may affect contemporaneous risk taking, it may take time for this to affect the probability that the bank fails or deteriorates enough to warrant a corrective enforcement action. Similarly, riskier loans may be reflected in the contemporaneous loan loss reserves. But a corresponding increase in defaults is likely to happen in the future. Therefore, fees may affect these variables with (unknown) lags, which may bias the static estimates. Second, contemporaneous fees may affect the treatment assignment of a bank *in future periods*. For example, lax regulation could affect the future size of the bank (by allowing riskier behavior today), which also complicates the identification of the treatment-on-the-treated (TOT) effect.

To address these issues, we follow Cellini, Ferreira, and Rothstein (2010), who extend the regression discontinuity design (RD) and show how to identify both the intent-to-treat (ITT) and the TOT effects in a dynamic setting. The ITT is the *total effect* of exogenous variation in fees on outcomes over multiple quarters. This effect is comprised of the direct treatment effect of the lagged fee on current outcomes, and the indirect effect of the lagged fee through its impact on the assignment of the banks to treatments in the interim periods. The ITT effect of the τ-th lag on the outcome at time t is

$$\beta_\tau^{\mathrm{ITT}} \equiv \frac{\partial y_{ijt}}{\partial b_{ijt-\tau}} + \sum_{s=1}^{\tau}\left(\frac{\partial y_{ijt}}{\partial b_{ijt-\tau+s}} \times \frac{\mathrm{d}b_{ijt-\tau+s}}{\mathrm{d}b_{ijt-\tau}}\right) = \beta_\tau^{\mathrm{TOT}} + \sum_{s=1}^{\tau}\beta_{\tau-s}^{\mathrm{TOT}}\pi_s, \tag{7}$$

[16] The triangular kernel is $K(u/h) = (1-|u|) \times 1_{|u|\leq 1}$ widely used in recent RD applications. The choice of kernel turns out to be less important than the choice of bandwidth h.

where π_s is the ITT effect of $b_{ijt-\tau}$ on $b_{ijt-\tau+s}$. Equation (7) can be used to recursively extract τ-specific TOT estimates β_τ^{TOT}:

$$\beta_\tau^{\text{TOT}} = \beta_\tau^{\text{ITT}} - \sum_{s=1}^{\tau} \beta_{\tau-s}^{\text{TOT}} \pi_s. \tag{8}$$

The estimation is done in two steps. First, we estimate β_τ^{ITT} and π_s simultaneously from

$$y_{ijt+\tau} = \sum_{p=1}^{P} \beta_{p\tau} \Delta_j^p \left(v_{ijt} - k_{jt}\right)^p D_{ijt} + \sum_{p=0}^{P} \alpha_{jp\tau} \left(v_{ijt} - k_{jt}\right)^p + \theta_\tau + \psi_t + \varepsilon_{ijt+\tau}, \tag{9}$$

where $y_{ijt+\tau}$ is an outcome variable at time $t+\tau$ of an observation in the neighborhood of kink j at time t, D_{ijt} is a shorthand for $D\left(v_{ijt} > k_{jt}\right)$—an indicator of the position of the bank relative to the kink at time t, θ_τ is a fixed effect for the number of quarters relative to t, and ψ_t is a year fixed effect. All coefficients, including those on the controlling polynomial are τ-specific. In the second step, we use the ITT estimates (β_τ^{ITT} and π_s) and their covariance matrix estimated via equation (9) to recover β_τ^{TOT} for each lag τ via equation (8). Standard errors for these estimates are computed using the delta method.[17]

4.3 Smooth Density Tests

Since the smooth density assumption is a key identifying assumption in the RKD setting, we test whether it holds in the data. We follow McCrary (2008), which provides a smooth density test for the regression discontinuity design (RD), and adapt this test for an RKD setting with multiple time-varying kinks. We test for a kink in the histogram of the assignment variable using a local polynomial regression similar to the one used to estimate our main effect, which explains the height of the bins using the bin midpoints. Further details are provided in Section A, which show that the smooth density hypothesis is not rejected in our data.

[17]Standard errors are clustered at the bank level to account for serial correlation and the fact that each bank-quarter observation (i, t) may be used multiple times. As before, each observation is weighted by the kernel.

5 Results

We start with a graphical presentation of our regression kink design in several raw outcome variables: capital ratios and loan loss reserves (our measures of risk), followed by statistical tests based on the multiple kink design developed above. We then report estimates of the dynamic model, which additionally include delayed outcome variables such as bank failures and enforcement actions.

5.1 Effects of Regulatory Fees on Risk-Taking: Capital and Loan-Loss Reserves

Before applying the RKD framework, we would like to see if the outcome variables exhibit kinks at the same points where kinks exist in the fee schedules. Since there are multiple kinks that cover a wide range of bank sizes and vary over time, we take residuals from a regression of risk measures on a *smooth* flexible polynomial function of bank size interacted with kink-year effects. Specifically, we take residuals from the regression $y_{ij} = \sum_{p=0}^{2} \alpha_{jp} \left(v_{ij} - k_j\right)^p + \varepsilon_{ij}$, group the residuals in bins by $v - k$, and plot the average outcome of each bin around the kink. Intuitively, these residuals capture the variation in the outcome variables unexplained by bank size and the fixed effects.

Figure 4 shows the results of this exercise. There is a visible discontinuity in slopes at the kink for all three capital ratios. These liability-based measures of bank safety decrease before the kink, and increase after the kink. The loan loss reserves ratio—a measure of asset risk—exhibits the opposite behavior: loss reserves increase on the left and decrease the right. This change in slopes suggests that higher fees increase risk. These figures may be less intuitive than the ones commonly used in regression discontinuity designs. To see the intuition, note that the difference in slopes in these figures is comprised of the treatment effect, multiplied by the difference in slopes of the fee function, whose slopes are decreasing. This means that if the effect of fees on capital ratio is negative, we should see an *increase* in the slope of the outcome variable in $v - k$ at the kink, while for loan loss reserves, we should see the opposite.

Table 3 reports the estimates from the static RKD model for capital ratios. We estimate a statistically significant negative elasticity of about 2 for all three regulatory capital ratios. Increasing the polynomial degree from 2 to 3 does not substantially change our inferences, but results in elasticities of larger magnitude.

The effects on the riskiness of banks' assets are presented in Table 4. We find that a 1 percent increase in regulatory fees increases loan loss reserves ratio by about 3.5 percent. By contrast, we find no effect on the loan loss reserve to noncurrent loans ratio. Both results are consistent with the earlier discussed interpretation that higher loss reserves are driven by higher expected losses from riskier loans—an indirect consequence of softer regulation. In other words, this means that lax regulation allows banks to invest in riskier assets, which leads to an increase in these measures of risk.

Table 4 shows a weak contemporaneous effect of fees on the non-current loans ratio. Note, however, that the static model is not well-suited to analyze this effect—intuitively, the potential effect of regulation on the realized loan performance is likely to be delayed, if only because these are loans that are over 90 days past due. In this sense, this measure is similar to regulatory actions and bank failures in that it captures the delayed consequences of risk-taking. Not surprisingly, we find no contemporaneous effect of fees on failures and regulatory actions. We omit these estimates here, since the dynamic model results will include the static estimator as a special case for $\tau = 0$. We now proceed to analyze these variables with a dynamic extension of our framework.

5.2 Dynamic Effects: Risk-Taking, Regulatory Actions and Bank Failures

We next present estimation results of the dynamic RKD model of Section 4.2. We start with the outcome variables that showed strong contemporaneous effects of fees—the three capital ratios and loan loss reserves.

Table 5 reports the intent-to-treat and treatment-on-treated effects of fees on these ratios for lags 0 through 11.[18] The ITT effects are highly stable for all lags and remain highly significant for the leverage ratio and loan loss reserves. As shown in equation (7), ITT measures the effect of a contemporaneous exogenous variation in fees τ quarters from now, without controlling for fees in the interim periods. The TOT, on the other hand, isolates the impact of each τ'th lag, which evidently, diminishes rapidly over time. For example, for the leverage ratio, the point estimate drops from -1.95 to -0.4 within one year. Figure 5 shows these results graphically. We learn that the effects of fees on these ratios are most pronounced for short periods and tend to diminish rapidly over time.

[18] In our empirical specification we allowed for a full set of possible lags for each bank. We report the first 12 lags in the table and 16 in the figures to simplify the exposition.

Next, we turn to the dynamic effects on bank failures, regulatory actions and the non-current loans ratio reported in Table 6 and Figure 6. The results for $\tau = 0$ mirror the (lack of) evidence on contemporaneous effects for these variables that we saw in Section 5.1. At longer horizons, however, the fees start having significant effects. In particular, banks that pay higher fees have higher non-current loan ratios and are more likely to find themselves subject to regulatory enforcement actions. The effect of fees on regulatory actions appears after 3 quarters, and on non-current loans after 7–8 quarters.

The results on bank failures are statistically weaker, likely due to the scarcity of failures in the data. The estimates show a *negative* effect of fees on the probability of bank closure, which shows up after 5–7 quarters. While the results on non-current loans and actions are intuitive—past risk taking catches up with the banks—the result on the bank failure may appear puzzling. This finding, however, is in line with the anecdotal evidence that regulators are reluctant to close banks that are important for their budget (see footnote 3), or try to avoid revelations of past leniency.

5.3 Competitive Regulation and the Race to the Bottom

The model in Section 2 features a single regulator. The firm's bargaining power stems from its ability to deprive the regulator of fees, for example by scaling down or moving assets off balance sheet. Banks, however, can also *choose* their regulator by selecting a charter, which could result in competition among regulators (Calomiris, 2006; White, 2011), thereby potentially exacerbating the effects of fees.

Empirically, we find no significant difference in the effect of fees across agencies or a significant effect of fees on the probability of changing a regulator. This is not surprising, since in our regressions each kink-regulator combination is assigned a separate controlling polynomial, which includes a fixed effect. Therefore, our estimates can be viewed as the effect of fees controlling for the effects of regulatory competition, which is consistent with Agarwal, Lucca, Seru, and Trebbi (2014) who find that bank and time effects effectively deal with the issue of charter shopping. This is an attractive feature of our empirical approach, since it allows us to focus on a less sector-specific effect of user fees. On the other hand, this leaves unanswered the interesting question of a potential effect of regulatory competition in banking. While a full treatment of competition is outside of the scope of this paper, a simple extension of our basic model can be used to analyze its effects.

With multiple regulators, each regulator and firm bargain over firm risk x

$$\max_x \left[f(q) - c(q,x)\right]^{\beta} \left[\pi(q,x) - f(q) - o(q)\right]^{1-\beta}, \tag{10}$$

where $o(q) \equiv \pi(q,\tilde{x}) - \tilde{f}(q) - \kappa$ is the firm's outside option—the alternative net profit it would earn with the next best regulator who allows risk $\tilde{x}$, and κ is a switching cost.[19] The optimal choice of risk again equates the bilateral marginal benefit and marginal cost

$$\beta \frac{c_x(q,x)}{f(q) - c(q,x)} = (1-\beta) \frac{\pi_x(q,x)}{\pi(q,x) - f(q) - o(q)}, \tag{11}$$

where $o(q)$ is the the main difference relative to equation (2).

Comparing this model with that of Section 2, we see that competition among regulators could improve the firm's outside option, thereby increasing its effective bargaining power and tilting the scale in favor of profits π_x. Furthermore, only under fairly strong assumptions, such as linearity of costs and profits in risk, the effect of fees on risk does not depend on the outside option.[20] Under more general specifications, competition could further strengthen the effect of fees. Our empirical approach flexibly accounts for such effects, but as we see from this extension, it is plausible that regulatory competition in banking has further aggravated the effects of fees beyond the results reported in this paper.

6 Robustness

6.1 Bandwidth Sensitivity

A common feature of local polynomial regression is that the choice of bandwidth is important. Our choice of bandwidth $h = 0.3$ log points is somewhat arbitrarily chosen to maximize sample size while avoiding overlapping observations across kinks. We investigate the sensitivity of our estimates to this choice by gradually shrinking the bandwidth from 0.3 down to 0.03 fixing the polynomial degree at 2. Figure 7 shows that point estimates of the elasticity of capital ratios to fees increase

[19] If the best alternative for the firm is exit, $o(q) = 0$.

[20] To see this, specify the costs and profits as $c(q,x) = xc_x(q)$ and $\pi(q,x) = x\pi_x(q)$. Then $x^*(f,o,q) = \left[\frac{\beta}{\pi_x(q)} + \frac{1-\beta}{c_x(q)}\right] f(q) + \frac{\beta}{\pi_x(q)} o(q)$ and therefore $\frac{\partial \log x}{\partial \log f} = \beta \frac{f(q)}{\pi(q,x)} + (1-\beta) \frac{f(q)}{c(q,x)}$. In other words, in this specification regulatory competition affects risk taking, but it does not alter the effect of fees on risk.

in magnitude and mostly remain significant as we shrink the bandwidth. Of course, in the limit as the bandwidth shrinks to zero, no observations are left and the confidence interval blows up. Similarly, the estimated effect of fees on loan loss reserves is robust and remains significant as we shrink the bandwidth.

6.2 Placebo Tests

We investigate whether our results are spurious by shifting all kink points from k_j to $k_j + h$ for all fee schedules g and kinks j. This procedure focuses on fake kinks where no kink is known to exist. If our methodology is biased toward rejecting the null, we would expect the placebo tests to identify significant treatment effects.

Placebo tests using capital ratios as dependent variables are reported in Table 7. As expected, none of the effects is statistically significant. Similar placebo tests using loan loss ratios as dependent variables are reported in Table 8. Here, the second degree polynomial gives some unexpected statistically significant coefficients, but increasing the polynomial degree to 3 eliminates the significance of these placebo results suggesting they are indeed spurious.

6.3 Monte Carlo Simulations

A more structured way to test whether our methodology is biased, is by Monte Carlo simulation. Specifically, we simulate the dependent variable as $\log y_i = 2 + TT_0 \times \log Fees_i - 0.7 \times \log Assets_i + 0.01 \times (\log Assets_i)^2 + w_i$. We generate random samples of the same size as our real sample, by sampling from the assets and kinks distribution and drawing independent shocks (w_i) from the distribution of residuals from a preliminary OLS regression of log leverage ratios on log fees and log assets. We then apply the exact same multiple kink regression specification as before. Table 9 shows that when the true effect is zero, the mean estimates reported in Panel (a) are zero as well. Moreover, Panel (b) shows that just as expected, a test with $p = 0.05$ significance falsely rejects the null in a 0.04 to 0.05 fraction of the simulated samples. Panels (c) and (d) repeat the exercise, but this time when the true effect is -2, roughly corresponding to our leverage ratio effects. The mean estimates and the rejection rates are about right when we use a second or third degree polynomial together with our benchmark bandwidth of $h = 0.3$. As the bandwidth shrinks, the power to reject the false null hypothesis is reduced and point estimates seem to be attenuated toward zero. We conclude therefore, that our estimates based on the kinks in our data are likely to be consistent.

6.3.1 Necessity of Multiple Time-varying Kinks RKD Specification

In Section 4, we extended the local polynomial regression specification of Card, Lee, Pei, and Weber (2012) to include regulator-time, and kink-specific controlling polynomials. As we discussed, this allows us to increase statistical power, provided that the estimating equations are properly adjusted to account for the heterogeneity across kinks. In this section, we study the importance of this adjustment and show that a failure to account for kink heterogeneity would result in severely biased estimates, and false rejections of the null hypothesis.

Table 10 shows the results of an estimation that does not properly account for heterogeneity across kinks. The simulated samples are identical to those of Table 9, but pool all kinks without properly allowing for kink-specific controlling polynomials as we advocate in Section 4. Panels (a) and (b) show that even when the true effect is zero, this specification rejects the null frequently with the linear specification, and practically always with higher degree polynomials. Panels (c) and (d) show that when the true effect is -2, single-kink RKD estimates are large and *positive*.

7 Conclusion

We provide the first causal evidence that funding of regulatory agencies affects the implementation of regulatory policies. In many sectors regulation is funded by regulated firms. We show how variation in user-fees can be used to study regulatory incentives and the effects of incentives on regulatory outcomes. Using a simple model where stricter regulation reduces regulators' income by pushing firms to forgo projects, scale down, or shift activity to the unregulated sector, we show that firms that pay higher fees face more lenient regulation. Intuitively, higher fee income alleviates the negative effect of higher risk.

Empirical tests of this channel, however, face significant identification challenges, which we address using kinks in the fee schedules of federal banking regulators. We find strong evidence that banks that pay higher fees get more lenient regulatory treatment. Higher fees increase bank leverage and the riskiness of assets. Moreover, banks that pay higher fees are less likely to be closed by their regulators, and more likely to experience future enforcement actions and loan defaults. We conclude that regulators have the ability to regulate firm behavior, but financial incentives of regulatory agencies matter for the implementation of regulation. Our findings imply that this issue should be taken into account in an effective regulatory design.

The "leniency-for-fees" channel identified in this paper applies broadly, outside of the banking sector. Given the pervasiveness of this model of regulation and the availability of data on regulatory budgeting policies, it should be possible to quantify these effects in other industries. Our framework and extensions of the regression kink design to allow multiple kinks and dynamic effects could be useful in such future research, and other applications of the regression kink design.

Appendix

A Smooth Density Tests

We follow McCrary (2008), which provides a smooth density test for the regression discontinuity design (RD), and adapt this test for an RKD setting with multiple time-varying kinks.

We construct frequency "observations" of equally-spaced bins of length $w \ll h$ around each kink j of group g. We assign to each such bin region the discretized version of the assignment variable v_{ij} around kink k_j[21]

$$G_j\left(v_{ij}\right) = \left\lfloor \frac{v_{ij} - k_j}{w} \right\rfloor w + \frac{w}{2} \in \left\{ \dots, -5\frac{w}{2}, -3\frac{w}{2}, -\frac{w}{2}, \frac{w}{2}, 3\frac{w}{2}, 5\frac{w}{2}, \dots \right\}$$

Define the (normalized) cell size for the sth bin of kink j,

$$Y_{js} = \frac{1}{nw} \sum_{i=1}^{n} 1\left(G_j\left(v_{ij}\right) = X_{js}\right) \times 1\left(\left|v_{ij} - k_j\right| \leq h\right),$$

where X_{js} are the equi-spaced grid points of the support of $G_j\left(v_{ij}\right)$.

The first-histogram is the scatterplot $\left(X_{js}, Y_{js}\right)$. The second step smooths the histogram using a local polynomial regression similar to the one used to estimate our main effect, which explains the height of the bins using the bin midpoints:

$$Y_{js} = \sum_{p=1}^{P} \beta_p X_{js}^p D\left(X_{js} \geq 0\right) + \sum_{p=0}^{P} \alpha_{jp} X_{js}^p + \upsilon_{js} \tag{12}$$

weighted by a kernel $K\left(X_{js}/h\right)$ and tests whether β_1 is different from zero to identify a kink in the density $f\left(v\right)$ at $v = k$.

The smooth density hypothesis is not rejected in our data. Figure A.1 shows that the histogram of log assets is quite smooth around the kinks. The regression results do not reject the null of a smooth density with t-statistics 0.37 and 1.05 for polynomial degrees of 2 and 3 respectively.

[21] McCrary adds the discontinuity point (c in his notation) back to the normalized grid points, but since we would like to center various kinks around zero we do not.

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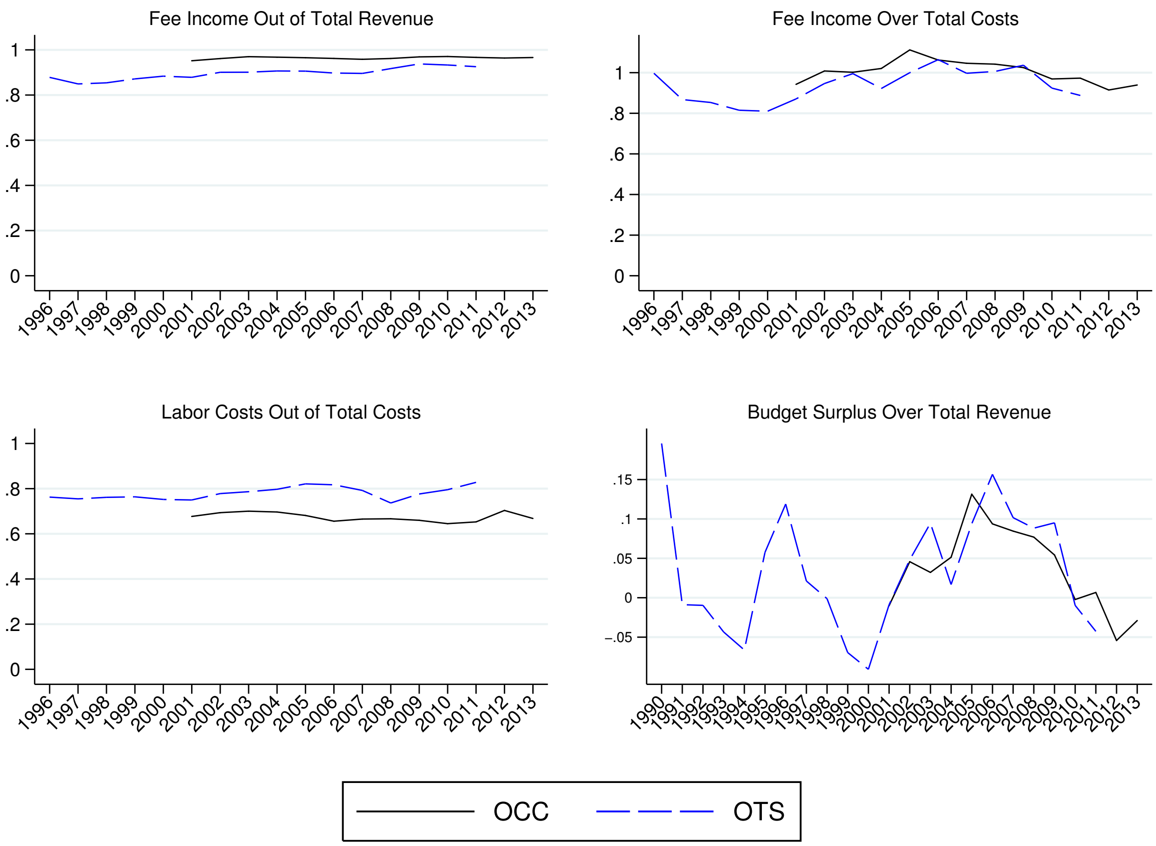


Figure 1: **Revenues, Costs and Budget Surplus of Bank Regulators**
Source: The Office of the Comptroller of the Currency (OCC) annual reports for fiscal years 2001-2013 and The Office of Thrift Supervision (OTS) financial statements for 1990-2011.

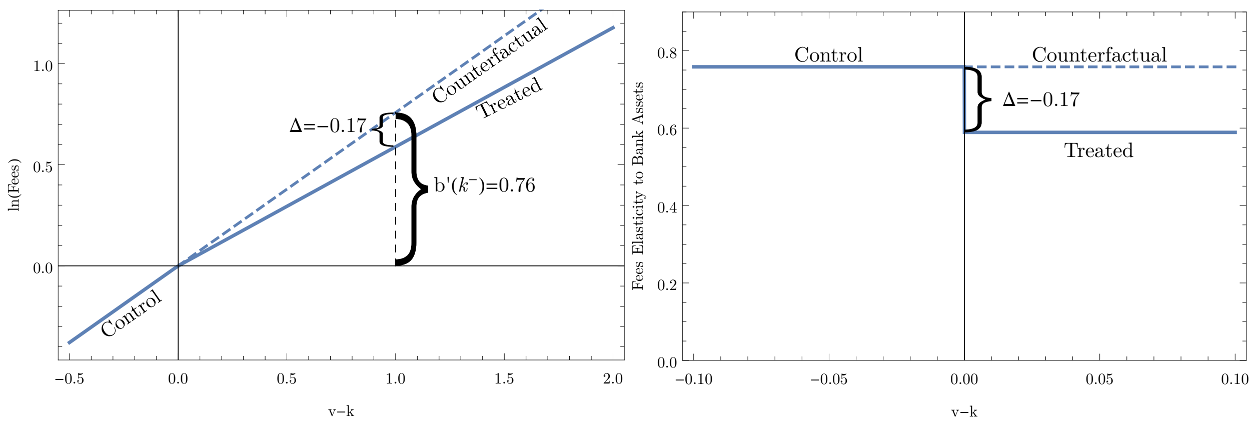


Figure 2: **Kinks in Regulatory Fee Schedules**

The figures show the mean difference in elasticities estimated with a local linear regression $b(v_{ij}) \equiv \log Fees_{ij} = \alpha_j + \beta(v_{ij} - k_j) + \Delta(v_{ij} - k_j) D(v_{ij} > k_j) + \gamma D(v_{ij} > k_j) + \varepsilon_{ij}$, where v_{ij} is the log assets of bank i in kink-year j, and k_j is log assets at the kink. The figure on the left illustrates the mean slopes ignoring the levels α_j. The figure on the right illustrates our "first-stage" effect, which is the mean difference in elasticities. The interpretation of $\Delta = -0.17$ is that a one percent increase in assets has a 0.17 percentage point higher effect on fees on the left side of the average kink than on its right. Because the average elasticity on the left is 0.76, the "first-stage" estimated effect represents a 22 percent decrease in elasticity. Our dataset includes all fee schedules for national banks and thrifts regulated by OCC (1985–2014) and OTS (1990–2012). An example fee schedule appears in Table 1.

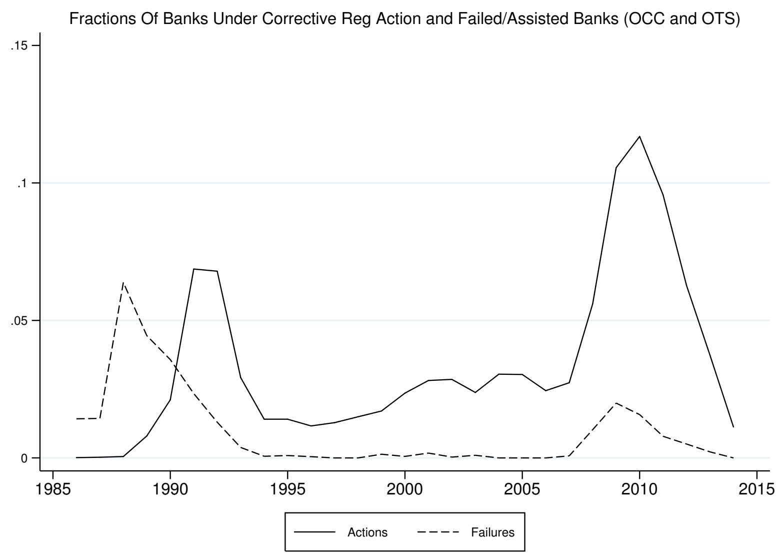


Figure 3: **Fraction of Failing/Assisted Banks and Banks Under Corrective Actions**
The solid line is the fraction of banks under corrective enforcement actions initiated by their primary regulator. The dashed line is the fraction of banks that failed or received assistance from the FDIC. The sample includes all banks regulated by OTS and OCC.

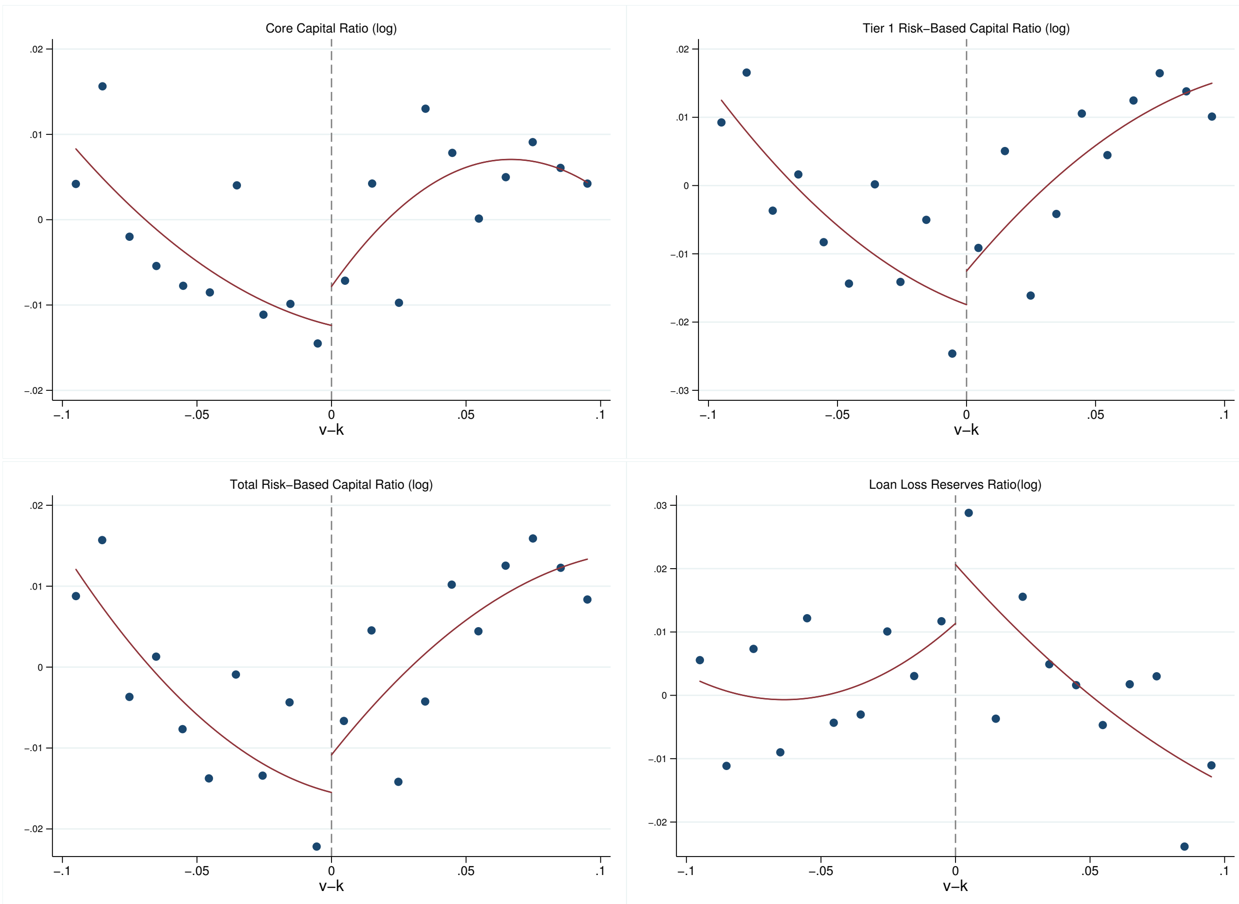


Figure 4: **Fees and Risk—Kinks in the Outcome Data**

The figures show that the four risk measures exhibit kinks at the same points where kinks exist in the fee schedules. The risk measures are core capital (leverage) ratio, tier 1 risk based ratio, total risk-based ratio, and loan loss reserves. On the vertical axis are average residuals from a regression of the risk measures on a *smooth* flexible function of the assignment variable, interacted with kink-year fixed effects: $y_{ij} = \sum_{p=0}^{2} \alpha_{jp} (v_{ij} - k_j)^p + \varepsilon_{ij}$, where v_{ij} is the log assets of bank i in kink-year j, k_j is log assets at a kink. The residuals are grouped into 20 bins and the average for each bin is plotted. The solid lines are quadratic fitted lines of the residuals in $v - k$ on both sides of the kink. The sample is national banks and thrifts regulated by OCC (1985–2014) and OTS (1990–2012).

Intent-to-Treat (ITT) **Treatment On Treated (TOT)**

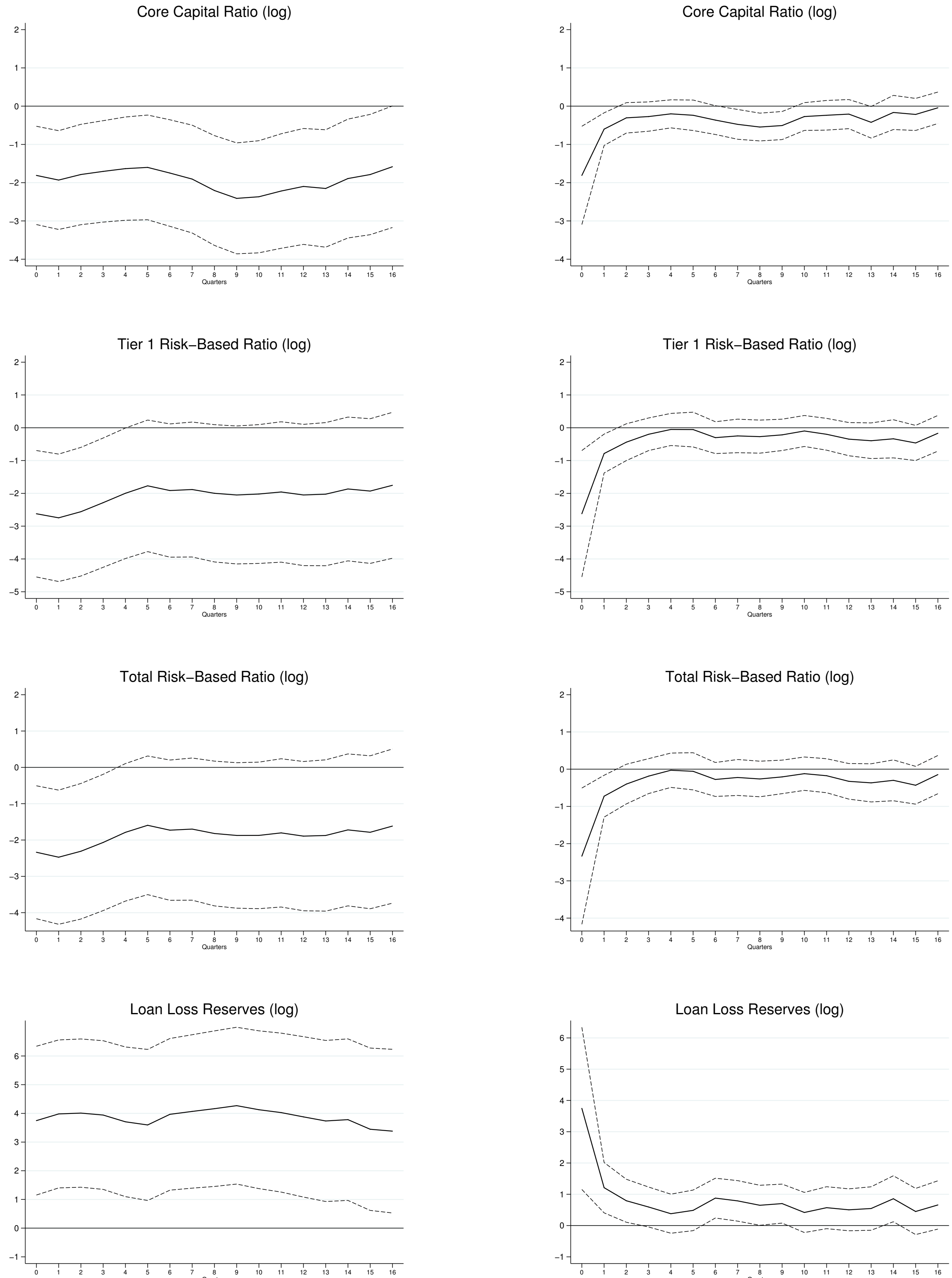
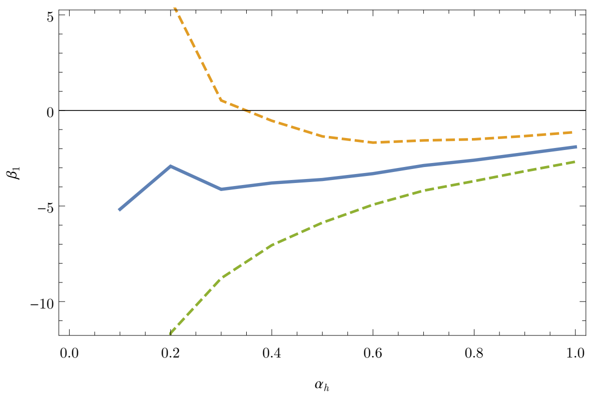
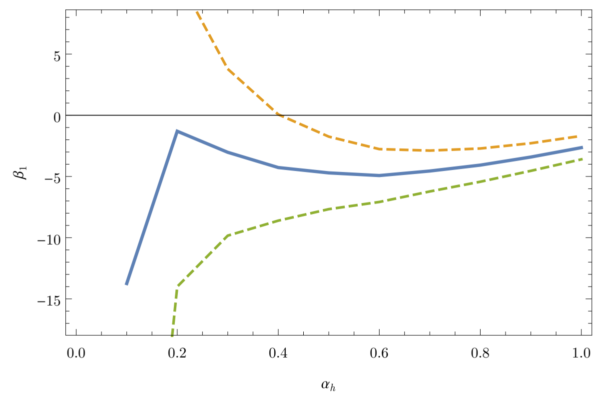


Figure 5: **Dynamic RKD: Capital Ratios and Loan-Loss Reserves**
ITT and TOT estimates from Table 5 (with 95% confidence intervals) of the effect of the exogenous variation in lagged assessment fees paid by national banks and thrifts to OCC and OTS. Quarter 0 is the contemporaneous effect of fees on the outcomes. ITT is estimated with equation (9) and TOT with equation (8).

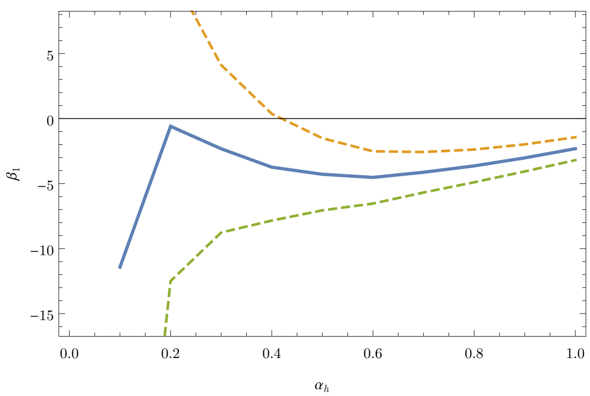
Figure 6: **Dynamic RKD: Failures, Regulatory Actions, and Noncurrent Loans**
ITT and TOT estimates from Table 6 (with 95% confidence intervals) of the effect of the exogenous variation in lagged assessment fees paid by national banks and thrifts to OCC and OTS. Quarter 0 is the contemporaneous effect of fees on the outcomes. ITT is estimated with equation (9) and TOT with equation (8).



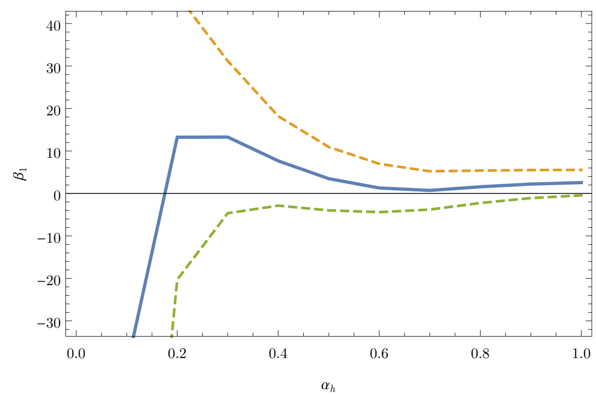
(a) Tier 1 Leverage Ratio



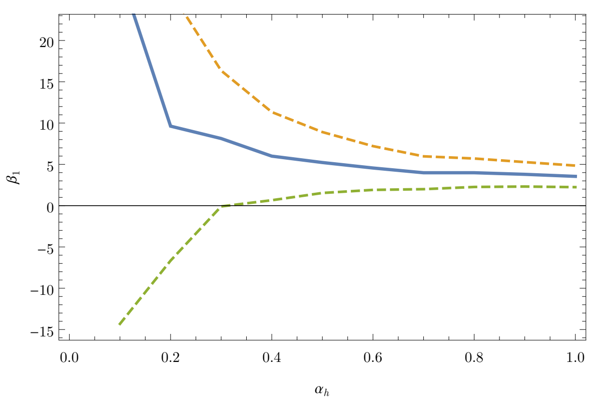
(b) Tier 1 Risk-based Capital Ratio



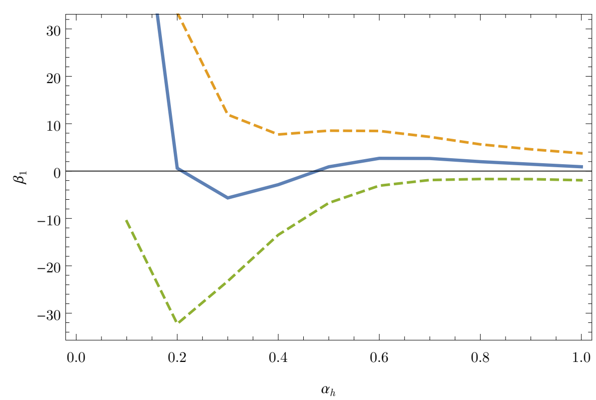
(c) Total Risk-based Capital Ratio



(d) Noncurrent Loans / Loans



(e) Loan Loss Reserve / Loans



(f) Loan Loss Reserve / Noncurrent

Figure 7: **Bandwidth Sensitivity - Effects of Fees on Bank Capital and Loan Losses**
Solid lines show how point estimates ($\hat{\beta}_1(\alpha_h \times h)$) of the elasticity of each of the dependent variables to regulatory fees change as we shrink the bandwidth from its benchmark value of $h = 0.3$ log points ($\alpha_h = 1$) toward zero. Dashed lines are the 95% confidence interval corrected for time (quarter) clusters.

If the amount of total balance-sheet assets is (millions)		the semiannual assessment is		
Over	But Not Over	This Amount	Plus	Of Excess Over
0	2	0	0.001574233	0
2	20	3148	0.000196781	2
20	100	6691	0.000157424	20
100	200	19284	0.000102325	100
200	1000	29517	0.000086582	200
1000	2000	98783	0.000070840	1000
2000	6000	169623	0.000062971	2000
6000	20000	421507	0.000053579	6000
20000	40000	1171613	0.000050403	20000
40000		2179673	0.000033005	40000

Table 1: **OCC 1999 Fees Schedule**
Source: OCC Bulletin 98-54, *Office of the Comptroller of the Currency Fees for 1999*, dated December 1, 1998.

	Mean	Stdev	p1	p50	p99	Obs
Assets, $M	1731.80	27771.00	5.26	132.35	20607.00	433028
Fees, Annualized $M	0.19	1.90	0.01	0.05	2.40	433028
Operating Expense, Ann $M	99.80	1250.70	0.69	8.65	1381.70	432484
Noninterest Expense, Ann $M	54.75	804.60	0.39	4.01	676.56	432484
Fees / Operating Expense	0.65	3.70	0.09	0.49	2.21	432327
Fees / Noninterest Expense	1.32	4.68	0.11	1.11	3.62	432303
Tier 1 Leverage Ratio	11.05	10.15	2.50	8.73	68.54	432483
Tier 1 Risk-based Capital Ratio	21.26	23.99	4.87	14.65	151.56	339059
Total Risk-based Capital Ratio	22.32	23.86	6.17	15.71	152.11	339059
Noncurrent Loans / Loans	1.83	2.44	0.00	0.98	13.55	421966
Loss Reserve / Loans	1.51	1.11	0.07	1.25	6.52	421430
Loss Reserve / Noncurrent	409.26	1252.80	9.68	109.56	8475.00	394837

(a) Summary Statistics

Kink	Fees Elasticity Diff.		Assets ($M)		Fees, Annualized ($M)	
	OCC	OTS	OCC	OTS	OCC	OTS
1	-0.61	-0.25	3	94	0.01	0.03
2	-0.11	-0.24	28	302	0.02	0.09
3	-0.28	-0.23	147	1406	0.05	0.29
4	-0.11	-0.10	297	8481	0.08	1.23
5	-0.16	-0.16	1480	25315	0.26	3.25
6	-0.09	-0.17	2957	49224	0.44	5.66
7	-0.13		8872		1.10	
8	-0.08		29577		3.07	
9	-0.31		59157		5.62	

(b) Average Kinks in Fee Schedules

Table 2: **Nationally-chartered Banks and Thrifts, 1985–2014**
Reported are summary statistics for nationally chartered banks and thrifts regulated by OCC (1985–2014) and OTS (1990–2012). The unit of observation is bank-quarter. Dollar amounts are real 2012 dollars. Ratios are in percentage terms and winsorized at 0.01 level. Panel (b) reports the average differences in the elasticity of fees to balance-sheet assets moving from the left to the right of each kink, as well as the levels of assets and fees at the kink point.

Dependent Variable:	ln(LR)		ln(T1RB)		ln(TotRB)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fees Elasticity	-1.91***	-2.18***	-2.64***	-3.40***	-2.32***	-3.01***
	(-4.83)	(-4.91)	(-5.41)	(-5.58)	(-5.15)	(-5.32)
Polynomial Degree	2	3	2	3	2	3
R-squared	0.33	0.33	0.29	0.29	0.29	0.29
Obs	198621	198621	158182	158182	158182	158182

Table 3: **Effects of Regulatory Fees on Bank Capital**
Reported are regression kink results for nationally chartered banks and thrifts using log regulatory capital ratios as the dependent variables. t-statistics corrected for time (quarter) clusters are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	$\ln\left(\frac{\text{Noncurrent Loans}}{\text{Loans}}\right)$		$\ln\left(\frac{\text{Loss Reserve}}{\text{Loans}}\right)$		$\ln\left(\frac{\text{Loss Reserve}}{\text{Noncurrent Loans}}\right)$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fees Elasticity	2.55*	2.84	3.55***	3.92***	0.91	0.62
	(1.67)	(1.09)	(5.32)	(3.11)	(0.63)	(0.23)
Polynomial Degree	2	3	2	3	2	3
R-squared	0.15	0.15	0.27	0.27	0.16	0.16
Obs	184751	184751	194472	194472	185197	185197

Table 4: **Effects of Regulatory Fees on Loan Losses**
Reported are regression kink results for nationally chartered banks and thrifts using log loan loss ratios as the dependent variables. t-statistics corrected for 117 time (quarter) clusters are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

	Ln(LR)		Ln(T1RB)		Ln(TotRB)		Ln(LLR)	
	(1) ITT	(2) TOT	(3) ITT	(4) TOT	(5) ITT	(6) TOT	(7) ITT	(8) TOT
Lag0	-1.949***	-1.949***	-2.657**	-2.657**	-2.331**	-2.331**	3.609***	3.609***
	(-2.64)	(-2.64)	(-2.36)	(-2.36)	(-2.18)	(-2.18)	(2.72)	(2.72)
Lag1	-2.101***	-0.816***	-2.790**	-0.915**	-2.474**	-0.833**	3.785***	0.978**
	(-2.85)	(-2.83)	(-2.47)	(-2.40)	(-2.30)	(-2.30)	(2.87)	(2.53)
Lag2	-1.923**	-0.412	-2.658**	-0.573*	-2.365**	-0.519	3.807***	0.672**
	(-2.57)	(-1.62)	(-2.33)	(-1.66)	(-2.19)	(-1.59)	(2.88)	(1.98)
Lag3	-1.897**	-0.390	-2.432**	-0.268	-2.176**	-0.255	3.796***	0.541*
	(-2.51)	(-1.64)	(-2.12)	(-0.90)	(-2.00)	(-0.91)	(2.86)	(1.73)
Lag4	-1.797**	-0.265	-2.103*	-0.0649	-1.855*	-0.0344	3.581***	0.311
	(-2.35)	(-1.15)	(-1.83)	(-0.22)	(-1.70)	(-0.12)	(2.68)	(1.02)
Lag5	-1.709**	-0.313	-1.751	-0.0334	-1.545	-0.0279	3.468**	0.394
	(-2.21)	(-1.20)	(-1.52)	(-0.10)	(-1.41)	(-0.09)	(2.57)	(1.25)
Lag6	-1.878**	-0.504**	-1.977*	-0.463	-1.757	-0.419	3.794***	0.736**
	(-2.39)	(-2.07)	(-1.70)	(-1.55)	(-1.59)	(-1.49)	(2.81)	(2.38)
Lag7	-2.110***	-0.675***	-1.984*	-0.373	-1.762	-0.333	3.879***	0.659**
	(-2.66)	(-2.69)	(-1.69)	(-1.19)	(-1.58)	(-1.12)	(2.84)	(2.10)
Lag8	-2.486***	-0.810***	-2.281*	-0.592*	-2.063*	-0.565**	3.978***	0.533*
	(-3.09)	(-3.41)	(-1.93)	(-1.96)	(-1.83)	(-1.97)	(2.87)	(1.71)
Lag9	-2.704***	-0.740***	-2.310*	-0.375	-2.094*	-0.351	4.092***	0.592**
	(-3.32)	(-3.03)	(-1.94)	(-1.27)	(-1.85)	(-1.26)	(2.93)	(1.97)
Lag10	-2.636***	-0.435*	-2.314*	-0.237	-2.127*	-0.255	4.007***	0.343
	(-3.21)	(-1.79)	(-1.94)	(-0.84)	(-1.87)	(-0.96)	(2.85)	(1.10)
Lag11	-2.494***	-0.426	-2.254*	-0.352	-2.064*	-0.319	3.930***	0.473
	(-2.98)	(-1.60)	(-1.86)	(-1.12)	(-1.79)	(-1.08)	(2.78)	(1.48)
R^2	0.319		0.273		0.266		0.288	
Observations	2134889		1799616		1799616		2093032	

Table 5: **Dynamic RKD: Capital Ratios and Loan-Loss Reserves**

Intent-to-treat (ITT) and treatment-on-treated (TOT) estimates for the effect of the exogenous variation in lagged regulatory assessment fees. Dependent variables are logs of leverage ratio (LR), tier 1 risk-based ratio (T1RB), total risk-based ratio (TotRB), and loan loss reserves (LLR). Lag0 corresponds to the contemporaneous effect of fees on the outcomes. ITT is estimated with equation (9) and TOT is recursively recovered through equation (8). The sample is national banks and thrifts regulated by OCC (1985–2014) and OTS (1990–2012). All regressions include kink-group, year, and lag-level fixed effects. t-statistics are in parentheses, and the standard errors are clustered at the bank level. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

	Failure		Any Action		Ln(Noncur)	
	(1) ITT	(2) TOT	(3) ITT	(4) TOT	(5) ITT	(6) TOT
Lag0	0.0182	0.0182	0.0884	0.0884	2.661	2.661
	(0.39)	(0.39)	(0.71)	(0.71)	(1.03)	(1.03)
Lag1	0.0564	0.0434	0.0860	0.0233	2.224	-0.0625
	(1.19)	(0.87)	(0.67)	(0.21)	(0.85)	(-0.07)
Lag2	0.00989	-0.0308	0.278^{**}	0.213^{*}	1.215	-0.664
	(0.20)	(-0.64)	(2.13)	(1.94)	(0.46)	(-0.78)
Lag3	0.0358	0.0253	0.528^{***}	0.322^{***}	1.879	0.747
	(0.77)	(0.54)	(3.72)	(2.65)	(0.71)	(0.91)
Lag4	-0.00347	-0.0328	0.429^{***}	0.0352	2.104	0.390
	(-0.08)	(-0.78)	(3.02)	(0.30)	(0.78)	(0.47)
Lag5	-0.0967^{***}	-0.0971^{**}	0.203	-0.138	2.574	0.727
	(-2.67)	(-2.35)	(1.55)	(-1.25)	(0.95)	(0.87)
Lag6	-0.0798^{*}	-0.0123	0.247^{*}	0.0479	4.226	1.916^{**}
	(-1.92)	(-0.29)	(1.79)	(0.40)	(1.55)	(2.31)
Lag7	-0.0910^{**}	-0.0319	0.0271	-0.185	6.349^{**}	2.690^{***}
	(-2.49)	(-0.89)	(0.21)	(-1.60)	(2.30)	(3.08)
Lag8	-0.00563	0.0690^{*}	-0.00907	-0.0551	5.677^{**}	0.0564
	(-0.13)	(1.76)	(-0.07)	(-0.49)	(2.06)	(0.06)
Lag9	0.0569	0.0676	-0.255^{*}	-0.270^{**}	3.747	-1.339
	(1.24)	(1.57)	(-1.90)	(-2.37)	(1.33)	(-1.56)
Lag10	0.0763	0.0400	-0.130	0.0325	3.419	-0.119
	(1.63)	(0.95)	(-0.98)	(0.28)	(1.20)	(-0.14)
Lag11	0.0925^{*}	0.0363	-0.117	-0.0264	3.329	0.174
	(1.87)	(0.74)	(-0.88)	(-0.23)	(1.16)	(0.20)
R^2	0.0120		0.0672		0.148	
Observations	2138247		2138247		1999462	

Table 6: **Dynamic RKD: Failures, Regulatory Actions and Non-current Loans**
Intent-to-treat (ITT) and treatment-on-treated (TOT) estimates for the effect of the exogenous variation in lagged regulatory assessment fees. Dependent variables are bank failures, regulatory action, and log of non-current loans to loans ratio. Lag0 corresponds to the contemporaneous effect of fees on the outcomes. ITT is estimated with equation (9) and TOT is recursively recovered through equation (8), using a linear probability model for the discrete outcomes. The sample is national banks and thrifts regulated by OCC (1985–2014) and OTS (1990–2012). All regressions include kink-group, year, and lag-level fixed effects. t-statistics are in parentheses, and the standard errors are clustered at the bank level. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Dependent Variable:	ln(LR)		ln(T1RB)		ln(TotRB)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fees Elasticity	-0.22	-0.21	-0.49	-0.02	-0.30	0.20
	(-0.39)	(-0.35)	(-0.63)	(-0.03)	(-0.41)	(0.24)
Polynomial Degree	2	3	2	3	2	3
R-squared	0.37	0.37	0.32	0.33	0.32	0.32
Obs	187365	187365	149340	149340	149340	149340

Table 7: ***Placebo Tests*****: Effects of Regulatory Fees on Bank Capital**
Reported are regression kink results for nationally chartered banks and thrifts using log regulatory capital ratios as the dependent variables. We construct a placebo test for the instrument by adding to each kink point in the fee function 0.3 log points. t-statistics corrected for time (quarter) clusters are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	$\ln\left(\frac{\text{Noncurrent Loans}}{\text{Loans}}\right)$		$\ln\left(\frac{\text{Loss Reserve}}{\text{Loans}}\right)$		$\ln\left(\frac{\text{Loss Reserve}}{\text{Noncurrent Loans}}\right)$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fees Elasticity	4.39**	6.13*	2.21**	1.59	-2.27	-5.30*
	(2.39)	(1.79)	(2.54)	(1.26)	(-1.41)	(-1.68)
Polynomial Degree	2	3	2	3	2	3
R-squared	0.14	0.14	0.28	0.28	0.15	0.15
Obs	174453	174453	183017	183017	174766	174766

Table 8: ***Placebo Tests*****: Effects of Regulatory Fees on Loan Losses**
Reported are regression kink results for nationally chartered banks and thrifts using log loan loss ratios as the dependent variables. We construct a placebo test for the instrument by adding to each kink point in the fee function $h = 0.3$ log points. t-statistics corrected for 117 time (quarter) clusters are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	-0.11	0.04	0.06	0.06	0.04	0.02	0.01	-0.00	-0.01	-0.02	-0.02
2	0.03	-0.22	0.01	0.07	0.12	0.12	0.10	0.09	0.07	0.06	0.04
3	0.00	-0.09	-0.07	-0.00	0.09	0.11	0.09	0.08	0.06	0.05	0.03

(a) Mean TOT Estimate. True Regulatory Fees Elasticity = 0

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	0.05	0.04	0.04	0.04	0.04	0.05	0.04	0.04	0.05	0.05	0.05
2	0.05	0.05	0.04	0.04	0.04	0.03	0.04	0.04	0.05	0.04	0.05
3	0.05	0.05	0.04	0.05	0.05	0.04	0.05	0.04	0.04	0.04	0.04

(b) Rejection Rate $p < 0.05$. True Regulatory Fees Elasticity = 0

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	-2.07	-1.31	-1.47	-1.53	-1.63	-1.65	-1.66	-1.64	-1.62	-1.60	-1.58
2	0.39	-1.76	-1.10	-1.22	-1.23	-1.41	-1.54	-1.67	-1.75	-1.78	-1.79
3	0.43	-1.40	-1.36	-1.24	-1.16	-1.20	-1.35	-1.49	-1.65	-1.73	-1.77

(c) Mean TOT Estimate. True Regulatory Fees Elasticity = -2

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	0.08	0.14	0.43	0.84	0.99	1.00	1.00	1.00	1.00	1.00	1.00
2	0.04	0.06	0.06	0.08	0.14	0.25	0.42	0.65	0.85	0.94	0.99
3	0.04	0.05	0.06	0.08	0.10	0.17	0.26	0.45	0.69	0.86	0.95

(d) Rejection Rate $p < 0.05$. True Regulatory Fees Elasticity = -2

Table 9: **Simulated Samples**

Reported are mean treatment-on-treated estimates and the fraction of the 1000 simulated random samples where the regression kink design rejects the null hypothesis of zero effect. Each entry corresponds to a different RKD regression specification with a polynomial degree P and bandwidth h. We simulate the dependent variable as $\log y_i = 2 + TOT_0 \times \log Fees_i - 0.7 \times \log Assets_i + 0.01 \times (\log Assets_i)^2 + w_i$. We generate the random samples by sampling from the assets and kinks distribution and drawing independent shocks (w_i) from the distribution of residuals from a preliminary OLS regression of log leverage ratios on log fees and log assets.

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	9.25	7.03	3.40	1.76	0.98	0.64	0.65	0.59	0.53	0.38	0.28
2	232.03	109.99	73.66	54.89	42.47	35.41	30.24	26.25	23.51	20.90	18.95
3	263.99	170.69	112.25	87.34	71.02	59.45	50.96	44.93	40.27	36.53	33.32

(a) Mean TOT Estimate. True Regulatory Fees Elasticity = 0

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	0.17	0.63	0.54	0.40	0.24	0.19	0.27	0.35	0.40	0.30	0.22
2	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
3	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

(b) Rejection Rate $p < 0.05$. True Regulatory Fees Elasticity = 0

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	57.02	37.44	20.68	12.81	8.71	6.42	5.46	4.67	4.05	3.20	2.52
2	701.05	344.13	233.76	172.63	133.46	110.85	94.02	81.65	73.16	65.26	58.95
3	789.81	473.57	321.71	250.22	201.69	167.66	143.16	126.00	113.13	102.85	93.50

(c) Mean TOT Estimate. True Regulatory Fees Elasticity = -2

P \ h	0.03	0.06	0.09	0.12	0.15	0.18	0.21	0.24	0.27	0.30	0.33
1	0.77	1.00	1.00	1.00	1.00	0.99	1.00	1.00	1.00	1.00	0.99
2	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
3	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

(d) Rejection Rate $p < 0.05$. True Regulatory Fees Elasticity = -2

Table 10: **Ignoring Kink Heterogeneity Leads to Severe Biases**

Reported are mean treatment-on-treated estimates and the fraction of the 1000 simulated random samples where the regression kink design rejects the null hypothesis of zero effect. The simulated samples are identical to those of Table 9, but pool all kinks without properly allowing for kink-specific controlling polynomials as we advocate in Section 4. Each entry corresponds to a different RKD regression specification with a polynomial degree P and bandwidth h. We simulate the dependent variable as $\log y_i = 2 + TOT_0 \times \log Fees_i - 0.7 \times \log Assets_i + 0.01 \times (\log Assets_i)^2 + w_i$. We generate the random samples by sampling from the assets and kinks distribution and drawing independent shocks (w_i) from the distribution of residuals from a preliminary OLS regression of log leverage ratios on log fees and log assets.

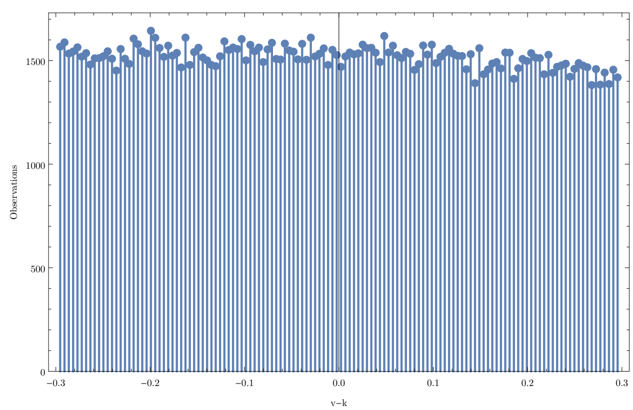


Figure A.1: **Histogram of Assignment Variable** ($\ln Assets$) **Around Kinks**
The histogram shows that the density of the assignment variable, log assets, is continuous and smooth around the kinks. We aggregate observations around all kinks j belonging to the fee schedule effective at time t by reporting the histogram of $v_{itj} - k_{tj}$. We test for a kink in the histogram of the assignment variable using a local polynomial regression similar to the one used to estimate our main effect, which explains the height of the bins using the bin midpoints. Further details are provided in Section A. The regression results do not reject the null of a smooth density with t-statistics 0.37 and 1.05 for polynomial degrees of 2 and 3 respectively.