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The Impact of Price Controls in Two-sided Markets: Evidence from US Debit Card Interchange Fee Regulation

Mark D. Manuszak and Krzysztof Wozniak*

Abstract

We study the pricing of deposit accounts following a regulation that capped debit card interchange fees in the United States and provide the first empirical investigation of the link between interchange fees and granular deposit account prices. This link is broadly predicted by the theoretical literature on two-sided markets, but the nature and magnitude of price changes are key empirical issues. To examine the ways that banks adjusted their account prices in response to the regulatory cap on interchange fees, we exploit the cap's differential applicability across banks and account types, while accounting for equilibrium spillover effects on banks exempt from the cap. Our results show that banks subject to the cap raised checking account prices by decreasing the availability of free accounts, raising monthly fees, and increasing minimum balance requirements, with different adjustment across account types. We also find that banks exempt from the cap adjusted prices as a competitive response to price changes made by regulated banks. Not accounting for such competitive responses underestimates the policy's impact on the market, for both banks subject to the cap and those exempt from it.

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I. Introduction

In a two-sided market, two groups of agents interact on a "platform," and each group's welfare depends on the size of the other group. The theoretical literature predicts that successful platforms may require transfers between the two groups in order to promote participation on both sides of the market. However, the literature is not able to provide a definite answer as to the optimal size of these payments, or even their optimal direction in some cases, which makes the practical implementation of such payments a potential source of controversy.

Such is the case for interchange fees and payment cards, which are often cited as a canonical example of a two-sided market (Rochet and Tirole, 2006). Interchange fees, paid by a merchant's bank to a cardholder's bank for each payment card transaction, have long been controversial, with merchants arguing that their high level was inflating their card transaction costs. A provision of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010, known as the Durbin Amendment, addressed this controversy by instructing the Federal Reserve Board to draft regulations related to debit card interchange fees. The resulting regulation, known as Regulation II or Reg II, went into effect on October 1, 2011, and imposed a cap that effectively halved the interchange fee for domestic debit card transactions involving banks that account for the vast majority of transactions.[1]

During the rule-writing process, much of the debate over Reg II among interested parties concerned the potential impact of interchange fee regulation on prices faced by cardholders, which were less well-understood than the link between interchange fees and merchant transaction costs that motivated the Durbin Amendment.

[1] All types of depository institutions, including banks, savings institutions, and credit unions, are potentially subject to Reg II's interchange fee cap. We use the term "bank" to refer to any type of depository institution.

In the months preceding the regulation's effective date, many market participants and industry observers predicted that card-issuing banks would look to mitigate lost interchange revenue through higher fees to accountholders, prompting headlines such as "Bankers Should Call Every New Charge a 'Durbin Fee'" in trade publications.[2] Such a possibility is emphasized in the theoretical literature on payment cards as a two-sided market (Baxter, 1983; Rochet and Tirole, 2002), which stresses that interchange fees alter revenue streams for card-issuing banks, who could adjust cardholder terms and fees accordingly.[3]

Notwithstanding this theoretical link, the magnitude and form of adjustments to cardholder fees, if any, are key empirical questions. Recent papers, including Carbo Valverde, Chakravorti and Rodriguez Fernandez (2016), Kay, Manuszak and Vojtech (2014), Wang, Schwartz and Mitchell (2014), and Evans, Chang and Joyce (2015), provide empirical evidence of some effects of interchange fees in payment card markets. However, none of these studies consider changes in specific account terms and fees in response to interchange fee declines. As the debate over Reg II suggests, these end-user fees are arguably a crucial item of interest when considering regulation of payment cards or, more generally, two-sided markets.[4]

In this paper, we examine changes in account terms and fees in response to the interchange fee caps under Reg II.[5] We take advantage of a notable aspect of the statute and resulting regulation, namely that banks with less than $10 billion in

[2] *American Banker*, June 30, 2011.

[3] See Rysman and Wright (2014) for a summary.

[4] In recent years, a nascent empirical literature (for example, Kaiser and Wright, 2006; Jeziorski, 2014; Gentzkow, Shapiro and Sinkinson, 2014) has looked to estimate models of two-sided markets and examine the theory's predictions. This empirical work has complemented the continued growth of the theoretical literature on two-sided markets since the early summary of Rochet and Tirole (2006).

[5] Our analysis of the granular implications of interchange fee restrictions for cardholder fees is similar to Agarwal, Chomsisengphet and Mahoney (2015), who consider whether restrictions on credit card fees under the CARD Act altered other terms for credit cards, and to papers testing the "waterbed effect" in mobile telephony (for example, Genakos and Valletti, 2011).

consolidated assets are exempt from the interchange fee cap. This exogenous variation allows us to compare the pricing behavior of banks subject to the cap ("covered banks") to that of banks not subject to the cap ("exempt banks") in order to separate the response to Reg II from other contemporaneous factors.

A further key feature of this paper is that we account for equilibrium effects under which exempt banks may adjust their pricing in response to price changes by covered rivals.[6] The possibility of equilibrium effects introduces an important consideration for predicting and evaluating a regulation's effect, namely that, in a competitive market, an intervention may have an impact that goes beyond the targeted institutions. Moreover, failure to account for these effects would not only disregard the impact of the treatment on the non-treated, but would also yield biased estimates of the treatment effect on the treated, as our empirical results ultimately show. To account for these effects, we exploit variation across exempt banks in their exposure to competition from covered banks. This approach builds on that used in papers such as Miguel and Kremer (2004) and Angelucci and De Giorgi (2009), but offers more flexibility when the program design prevents clean separation of untreated units into those exposed and unexposed to treated units.

We use a uniquely detailed panel data set of bank-level pricing that captures multiple terms and fees across various account types, and we estimate how those terms and fees responded to the regulation. The richness of our data allows us to consider price adjustment in multiple dimensions, as well as to examine heterogeneity in responses across account types. In addition, we ensure that other shocks correlated with bank size are not biasing our results by comparing price changes for checking

[6] This concern reflects the broader possibility of spillover effects of treatment. Such effects can take many forms, from externalities to equilibrium pricing effects, and have been consider in many applications in the economics literature on program evaluation (Angelucci and Di Maro, 2015).

accounts with changes for savings accounts, the latter of which typically do not include debit cards that would be affected by a cap on interchange fees. Finally, we account for the possibility that bank responses may have anticipated or lagged the regulation's effective date.

Our empirical results broadly indicate that, in response to Reg II, covered banks changed various account terms and fees in ways that increased prices for accountholders. In our most general specification, we find that covered banks were 35.2 percent less likely after the regulation to offer noninterest checking accounts that did not involve a monthly fee (that is, "free accounts"). This result suggests that, absent Reg II, 65.2 percent of noninterest checking accounts offered by covered banks would have been "free," compared with the actual post-implementation figure of 30.0 percent. Next, we find that, on average, covered banks raised monthly fees on noninterest and interest checking accounts by $1.34 (or 20 percent) and $2.02 (or 17 percent), respectively. These increases were large enough to fully recoup lost interchange fee revenues for noninterest and interest checking accounts with five and eight $40 debit card transactions per month, respectively.

Finally, we find that covered banks made it more difficult for accountholders to avoid paying monthly fees; the average minimum balance to avoid a monthly fee increased by over $400 (or 50 percent) for noninterest checking accounts, and by nearly $1,700 (or 55 percent) for interest checking accounts. The nonlinear pricing response suggests that consumer price sensitivity differs substantially between terms and fees, as well as across different account types.

Our results further show that, depending on their competitive exposure to covered banks, exempt banks also raised their prices, although the magnitude of their responses was generally lower than those of covered banks. For example, our results suggest that exempt banks fully exposed to competition from covered banks

would reduce the availability of free noninterest checking accounts 15.5 percent, compared with a reduction of 35.2 percent for covered banks. We also underestimate the responses of covered banks when we do not account for equilibrium pricing effects. These findings provide direct evidence of equilibrium pricing effects in an oligopolistic market and underscore the importance of these effects when considering the impact of a policy such as Reg II.

II. Background

Debit cards facilitate the electronic transfer of funds from a consumer's deposit account at a bank to a merchant's deposit account in exchange for goods and services.[7] Card-issuing banks provide deposit accounts, debit cards, and other account services to consumers, while acquiring banks provide accounts and transaction-processing services to merchants. Card networks, such as Visa, MasterCard, Star, or NYCE, provide infrastructure and services to exchange information and funds between issuing and acquiring banks. Networks also establish various rules and terms associated with card acceptance and the conduct of transactions, including certain fees for cardholders' banks (issuers) and merchants' banks (acquirers).

The largest network-determined fees by aggregate value are interchange fees, which acquirers pay to issuers on a per-transaction basis. In 2009, before Reg II, acquirers paid issuers more than $16 billion in debit card interchange fees on 37.6 billion card transactions, or roughly 43 cents per transaction (Federal Reserve Board, 2011). Because acquirers generally pass these fees to merchants, they serve to increase merchants' costs of payment card transactions, causing interchange fees to be highly controversial and leading to substantial acrimony and litigation between merchants

[7] Prager et al. (2009) discuss the structure and operation of payment card networks. Hayashi, Sullivan and Weiner (2003) provide a detailed description of the debit card industry.

on one side and payment card networks and banks on the other.[8]

The Durbin Amendment to the Dodd-Frank Act proposed a regulatory solution to merchant concerns about their costs of debit card transactions. Among other things, the amendment instructed the Federal Reserve Board to write regulations to establish "standards for assessing" whether debit card interchange fees are "reasonable and proportional to the cost incurred by the issuer with respect to the transaction."[9] To implement the amendment, the Board issued Reg II in June 2011, after receiving more than 10,000 public comments on a proposed regulation. Under the regulation, debit card interchange fees paid to certain issuers cannot exceed 21 cents per transaction plus 0.05 percent of the transaction value with an effective date of October 1, 2011. For an average $40 debit card transaction, this results in a maximum interchange fee of 23 cents, a substantial cut from the pre-regulation fee.[10]

Reg II contains various exemptions from the interchange fee cap. For our purposes, the relevant exemption is based on bank size. Per explicit instructions in the Durbin Amendment, the cap does not apply to banks with consolidated assets below $10 billion. This exemption was introduced to prevent declines in interchange revenue for small banks, but the rationale behind the specific threshold of $10 billion is unclear.[11] For our analysis, this exemption creates one group of banks that is subject to a

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[8] Wildfang and Marth (2006) summarize historical litigation related to payment cards, much of which concerns credit cards. As Hayashi and Maniff (2014) describe, central banks and competition authorities in various countries have intervened in payment card markets, often through interchange fee caps.

[9] Hayashi (2012) and Hayashi (2013) discuss the Durbin Amendment and Reg II in more detail.

[10] The regulation includes a one cent increase in the base cap for fraud-prevention costs incurred by issuers who meet certain fraud-prevention standards.

[11] In a letter to other lawmakers (Durbin, 2010), the amendment's sponsor stated that the exemption would cover "99% of all banks and credit unions" who "would not lose any interchange revenues that they currently receive," thereby "preserving the ability of small banks and credit unions to compete with big banks in issuing cards." Before Reg II, the same interchange fee schedules applied to all banks, and one issue was whether networks would apply the cap to exempt banks. All networks have since established tiered fee schedules with no material change in average interchange fees for exempt banks (Federal Reserve Board, 2011).

limitation on interchange fees and another group that is not.

Account types also vary in the extent to which they involve debit cards and, thus, are affected by Reg II's interchange fee cap. For checking accounts, debit cards are a core service; the ability to access funds easily for purchases and withdrawals is an important feature of such accounts. In contrast, debit cards are typically not issued for savings accounts, and interchange income is generally a negligible component of revenue associated with these accounts. Although a savings account may include a card that permits ATM access and point-of-sale transactions, this practice is uncommon because of regulations that limit transactions and withdrawals for nontransaction accounts.[12] Consequently, through its regulation of a fee associated primarily with checking accounts, Reg II also has differential applicability across account types for covered banks.

Interchange fees play a central role in theoretical models of payment card networks, which emphasize the card market's two-sided nature (for example, Rochet and Tirole (2002)). On one side of the market, interchange fees alter acquirers' costs, influencing the transaction fees they charge merchants. On the other side of the market, interchange fees provide a source of revenue that defrays issuers' costs of card services for accountholders, and, thus, influence fees that banks charge accountholders. As a result, these theoretical models broadly predict that a reduction in interchange fees will induce issuers to increase prices for accountholders.[13]

However, theoretical models of two-sided markets rely on an overly simple char-

[12] Regulation D requires that a "savings deposit" account for purposes of reserve requirements permit no more than six "convenient" transfers or withdrawals per month, whether by check, debit card, or similar order.

[13] Much of the theoretical literature concerns the implications of interchange fees for the efficiency of merchant and consumer decisions about card acceptance and usage. This literature generally concludes that interchange fees can internalize externalities across the two sets of end users, but that privately set interchange fees may diverge from the social optimum. See Verdier (2011) or Rysman and Wright (2014).

acterization of issuers, which diverges from reality in three important ways. First, issuers use nonlinear, account-based pricing rather than per-transaction fees typically assumed by the theory but rarely observed in reality. The theoretical literature on nonlinear pricing emphasizes the sensitivity of consumer demand to different price components. For the debit card industry, it predicts that higher costs will result in increases in prices for which consumers' demand is less sensitive, and lower or no rises in prices to which the demand is more sensitive.

Second, issuers are multiproduct firms, cross-selling a variety of products in addition to card transactions. The theoretical literature on multiproduct pricing predicts that a firm's price for one good will internalize its impact on the demand for the firm's other products. In the debit card industry, this implies that, since a bank is best positioned to offer additional services to consumers who are already its accountholders, the price for such an account is less likely to reflect higher costs than it would otherwise.

Finally, issuers are heterogeneous firms, subject to idiosyncratic cost shocks based on their status under the regulation, and compete for customers in the market for banking services. An issuer's prices are not determined in isolation by its costs and the market demand, but rather jointly with other issuers' prices. In a simple Cournot setting, there is a single market price determined jointly by the market demand function, as well as all firms' cost functions. In more general models of oligopolistic competition, each firm can set its own price, but such a price is still dependent on those set by competitors. The implication for our analysis is that, for the debit card industry where issuers compete in an oligopolistic fashion, we need to recognize that prices set by covered issuers influence those set by their exempt competitors and, hence, the regulatory shock to covered issuers and their prices may influence the pricing of exempt banks.

III. Data

Our primary data come from RateWatch, a consultancy that specializes in the retail banking industry. To provide information about pricing conditions, RateWatch conducts periodic surveys of terms, fees, and interest rates for various loan and deposit account products at individual branches of commercial banks, savings institutions, and credit unions across the country. For account terms and fees, RateWatch typically surveys a sample of branches for each bank twice a year, with variation in the precise timing of data collection across branches.[14] The resulting data set is one of the most detailed data sources on terms, fees, and rates for individual products, banks, and geographical markets.

RateWatch bases its branch sample on client requests for information and analysis about certain geographic markets. As such, the surveys are neither a random sample of branches nor a comprehensive census. However, selection bias associated with inclusion in the sample is mitigated by the fact that, once RateWatch surveys a branch, it continues to collect information from that branch even absent data requests.

III.A. Sample and variable construction

We aggregate the data to the bank-quarter level by averaging each bank's branch-level observations in a given quarter.[15] Aggregation to the bank level reflects an assumption that strategic pricing responses to a broadly applicable regulation, such as Reg II, occur at the bank level. In turn, aggregation by quarters smooths variation in data collection times to yield a balanced panel data set, while providing observations at

[14] This observation frequency differs from the weekly frequency of other data that RateWatch tracks, such as loan rates. This difference reflects the infrequency of changes in account terms and fees compared with changes in rates.

[15] More precisely, we aggregate the branch-level data to the level of bank holding companies.

intervals around the effective date.

In our analysis, we look at pricing information for three deposit account products: noninterest checking, interest checking, and savings. We focus on these account types for a number of reasons. First, most banks offer these accounts, allowing RateWatch to achieve extensive coverage of them. Second, each product type is largely standardized, which permits comparison of a core set of terms and fees across banks. Finally, as discussed in the previous section, Reg II affects checking and savings accounts differently, which provides a source of identification in our model.

For each product, we use information on four basic terms and fees that RateWatch tracks.[16] First, we consider whether an account is free of a monthly fee (*FreeAccount*). Next, for banks that charge a monthly fee, we use information about the fee size (*MonthlyFee*). We further consider whether an accountholder can avoid the fee by maintaining a minimum account balance (*AvoidFee*).[17] Finally, for banks that allow an accountholder to avoid the fee through a minimum balance, we include the balance required to do so (*MinBalance*).

We do not observe per-transaction fees that banks could use to directly offset decreased interchange revenue for accounts that involve debit cards; however, anecdotal evidence suggests these fees are rarely used. In addition, while we do not observe debit card rewards that banks could also cut, data collected by the Board suggest covered banks spent only a small portion of what they collected in interchange fees on reward programs pre-Reg II.[18]

[16] RateWatch requests information for accounts that a bank offers at the time of the survey. These terms and fees apply to new accounts and can apply to existing accounts if the bank provides customers with 30 days advance notice about changes in terms and fees.

[17] We do not have information about alternative ways that some banks may provide to avoid a monthly fee, such as having at least one direct deposit (for example, a paycheck). Although some banks report this information in comment fields, RateWatch does not collect it as a regular data element. If banks removed these account features after Reg II, our estimates will be a lower bound on the regulation's effect on the ability of accountholders to avoid fees.

[18] In 2011, the average per-transaction cost of debit card reward programs for covered banks was

We combine the data on account terms and fees with information about a bank's status under Reg II.[19] The vast majority of variation comes from the regulation's initial implementation. A small number of banks experience a status change in subsequent years, and we account for the timing of those changes under the regulation.[20]

Our analysis of equilibrium pricing effects requires a measure of how strongly an exempt bank competes with covered banks for retail customers. We construct such a metric, called *Exposure*, using data on all banks, their branches, and the deposits held at those branches from the FDIC's Summary of Deposits (SOD).[21] We combine this information with geographic retail banking market definitions established by the Federal Reserve for purposes of evaluating bank mergers.[22] For every exempt bank, we consider each retail banking market in which that bank operates and calculate the percentage of total deposits in the market held at branches owned by covered banks. We then average this percentage across all markets in which the exempt bank is present, weighted by the fraction of its total deposits in each market. The *Exposure* variable thus calculated is then merged with our main data set. In the Appendix, we consider an alternative exposure measure based on number of branches rather than deposits.

The final data set in our analysis is a panel of quarterly bank-level observations from Q1 2009 through Q2 2014. This time frame corresponds to 11 quarters before

three cents (Federal Reserve Board, 2013).

19 The Board releases lists of bank status under Reg II at http://www.federalreserve.gov/paymentsystems/regii-interchange-fee-standards.htm.

20 For the over 5,500 banks in our data, we observe six changes from exempt to covered status and three changes from covered to exempt status over the years 2012 – 2014.

21 The SOD reports branch-level deposits for all FDIC-insured institutions, but excludes credit unions. Because we cannot compute *Exposure* for credit unions, we drop a small number of credit union observations from the regressions that include *Exposure*. We further do not include credit unions in the *Exposure* variable for banks, which would tend to overstate *Exposure* given that nearly all credit unions are exempt from Reg II.

22 Banking market definitions are available at $https://www.federalreserve.gov/bankinforeg/afi/market\_info.htm$.

and 11 quarters after Reg II's effective date.

III.B. Summary Statistics

Figure I illustrates the data's coverage in terms of banks and total deposits for the U.S. retail banking market as reported in the SOD. The data capture 50 percent of covered banks and 66 percent of exempt banks, accounting for 83 percent of total deposits for both categories. The figure also illustrates that, because of the skewed asset size distribution across banks, our data include far fewer covered than exempt banks, although the covered banks account for a large fraction of total deposits.

Table I provides sample means for each outcome variable, broken out by issuer status under the regulation, account type, and pre/post-Reg II period.[23] Figure II further depicts average values of the variables by quarter, with Reg II's effective date noted by the vertical line at date 0.

Table I and Figure II illustrate several notable features of the data. First, different account types exhibit substantial differences in terms and fees. Second, in many cases, terms and fees for a given account type differ substantially between covered and exempt banks. Third, while most of the data series are relatively stable, a few exhibit pronounced changes over the sample period.

In addition, Figure II previews some main findings from our subsequent analysis. In particular, Figure II.A shows that the percentage of covered banks offering free noninterest checking accounts decreased substantially around the time Reg II was introduced, with a possible leading effect in anticipation of the rule change, while the corresponding percentage for exempt banks did not change substantially. Figure II.C indicates that, compared with exempt banks, the percentage of covered banks offering

[23] To categorize banks before the regulation's effective date, we use each bank's status at the effective date.

their customers the chance to avoid a monthly fee on their noninterest checking accounts decreased following Reg II's introduction. Finally, Figure II.D illustrates that the average balance to avoid a monthly fee on interest checking accounts increased substantially for covered banks compared with exempt banks after the regulation.

Table II provides summary statistics for the *Exposure* variable, our measure of how strongly an exempt bank competes with covered banks for retail customers. By construction, this variable is bounded by [0,1), and Table II shows that the data contain exempt banks that are highly exposed to covered banks, as well as exempt banks that are completely insulated from them. In other analyses of indirect treatment effects (for example, Miguel and Kremer, 2004), extreme cases of exposure would serve to identify indirect effects. Indeed, measures of indirect exposure are typically limited to those two cases. However, reflecting heterogeneity in local competitive conditions for retail banking, the vast majority of observations in our data fall between these extremes with a great deal of variation in the *Exposure* variable.

IV. Empirical Analysis

IV.A. Modeling approach

To investigate deposit account pricing, it is useful to consider how the different account terms and fees jointly determine Fee^E, the monthly fee that a bank can expect to collect from an accountholder, conditional on its fee structure:

$$Fee^E = \begin{cases} 0 & \text{if } FreeAccount = 1 \\ MonthlyFee & \text{if } FreeAccount = 0 \text{ and } AvoidFee = 0 \\ MonthlyFee \cdot P(Balance < MinBalance) & \text{if } FreeAccount = 0 \text{ and } AvoidFee = 1 \end{cases} \quad (1)$$

where P($Balance < MinBalance$) is the probability that a customer's average monthly balance is below the minimum balance to avoid the monthly fee. In our analysis, we focus on each of the four term and fee variables directly, an approach which allows us to examine specific mechanisms by which banks adjust their pricing. A bank could alter the fee income from its accounts by adjusting any of the four variables (or some combination of them) for which we have data. In particular, a bank could impose a fee on a previously free account or raise the monthly fee on accounts with a pre-existing fee. Alternatively, a bank could increase the minimum balance to avoid a fee, thereby increasing the likelihood that an accountholder would be subject to the fee. The extent to which banks utilize some channels over others will provide information about consumer sensitivity to the different dimensions of account pricing.

An alternative approach could be to model and analyze Fee^E. However, that approach would require knowledge of *Balance* across consumers, which we do not observe. Such an approach would not necessarily provide more insight into banks' pricing reactions to Reg II, but could shed light on the response of account balances to changes in minimum balance requirements, as well as the revenue effects of the price changes for the banks. While these various issues are important, and the variation in terms and fees and subsequent accountholder behavior induced by Reg II may be a useful environment to examine them, they are beyond this paper's scope.

IV.B. Econometric specification and identification strategy

In our empirical analysis, we estimate separate fixed effects regressions for each of the four outcome variables: *FreeAccount*, *MonthlyFee*, *AvoidFee*, and *MinBalance*. We exploit the panel structure of our data set and the existence of multiple account

types to separate the regulation's effects from other forms of pricing heterogeneity. Specifically, for account types $a \in \{$*noninterest checking*, *interest checking*, *savings*$\}$, bank i, and time period t, we consider a regression of the form

$$y_{ait} = \gamma_{ai} + \theta_{at} + \lambda_{it} + \beta_a w_{ait} + \delta_a f(c_{ait}) + \boldsymbol{\beta}_a^{lag} \boldsymbol{w}_{ait}^{lag} + \boldsymbol{\delta}_a^{lag} f(\boldsymbol{c}_{ait}^{lag}) + \epsilon_{ait} \qquad (2)$$

where y_{ait} is an outcome variable for product a at bank i in time t, w_{ait} is an account type-specific indicator for the two checking account products at covered banks in the post-Reg II period, c_{ait} is the measure of competitive exposure to covered banks for checking accounts at exempt banks in the post-Reg II period, $f(\cdot)$ is a monotonic, well-behaved function (described later), $\boldsymbol{w}_{ait}^{lag}$ and $\boldsymbol{c}_{ait}^{lag}$ are leading and lagging terms that correspond to w_{ait} and c_{ait}, respectively, and ϵ_{ait} are unobserved factors that influence the pricing of product a at bank i in time t.[24] The focus of our analysis will be on the coefficient β_a, which measures the long-run pricing response of type a checking accounts at covered banks to Reg II, as well as the coefficient δ_a, which measures the exempt banks' equilibrium pricing response for type a checking accounts.[25]

This econometric specification allows identification of Reg II's impact on bank pricing to come from two sources. First, we compare banks covered by the regulation with banks exempt from it, which allows us to separate the regulation's effect from general time-specific factors that may have coincided with the regulation and affected

[24] In other words, recalling that bank status is based on the \$10 billion asset threshold and letting T^* be the effective date of the regulation, $w_{ait} = 1(a = \text{checking}) \cdot 1(Assets_{it} \geq \$10\text{billion}) \cdot 1(t \geq T^*)$, $c_{ait} = 1(a = \text{checking}) \cdot 1(Assets_{it} < \$10\text{billion}) \cdot 1(t \geq T^*) \cdot Exposure_{it}$, $w_{ait}^{lag} = 1(a = \text{checking}) \cdot 1(Assets_{it} \geq \$10\text{billion}) \cdot 1(|t - T^*| \leq N^{lag})$ and $c_{ait}^{lag} = 1(a = \text{checking}) \cdot 1(Assets_{it} < \$10\text{billion}) \cdot 1(|t - T^*| \leq N^{lag}) \cdot Exposure_{it}$, where N^{lag} is the number of periods forward/back in time for which we include lagging/leading terms, $Assets_{it}$ are bank i's consolidated assets at time t, and $Exposure_{it}$ is the exposure to competition from covered banks faced by bank i in time t. In our estimation, we allow the responses to differ across the two types of checking accounts, and the actual variables reflect this distinction.

[25] Any additional bank-specific characteristics would be perfectly collinear with the fixed effects in our regressions.

pricing of all banks, as captured by θ_{at}. Our ability to perform this comparison based on an exogenous feature of the regulatory environment contrasts with evaluation of interchange fee regulation in other jurisdictions, such as Australia or the European Union. In those cases, broad applicability of interchange fee regulation prevents separation of the regulation's effect from other contemporaneous factors (Chang, Evans and Garcia Swartz, 2005; Hayes, 2007).

Second, we compare checking accounts, which involve debit cards and were directly affected by the regulation, with savings accounts, which typically do not involve debit cards and we assume were not affected. Doing so allows us to introduce bank-time period fixed effects, λ_{it}, that account for any time-specific shocks that may be correlated with bank size and, thus, status under the regulation. For example, it addresses the concern that the financial crisis or regulatory pressures may have affected large banks differently than small banks, influencing their pricing irrespective of Reg II.[26]

We could alternatively focus on covered banks and exploit the regulation's different applicability across checking and savings accounts at those banks. Although we provide estimates based on this identification strategy, it does not allow us to account for all of the heterogeneity in our full model. In particular, it leaves open the possibility that certain account types were affected by time-specific shocks unrelated to Reg II.[27]

In addition to capturing the direct response of covered banks, the econometric

[26] We could address this concern by considering banks with assets in a window around the $10 billion threshold. However, such a window would need to be fairly wide in order to have enough observations to avoid losing too much power.

[27] For example, in 2009, the Board amended Regulation E to prohibit all financial institutions from charging fees for overdrafts unless a consumer opts in to an overdraft service. This change, which became effective on July 1, 2010, would have largely affected overdraft revenue for checking accounts and could have influenced checking account pricing around the same time as Reg II.

specification also captures its impact on pricing at exempt banks through equilibrium pricing effects. Ignoring such effects would not only fail to capture the regulation's impact on exempt banks but would also bias our estimates related to covered banks. As noted in our discussion of the data, following Miguel and Kremer (2004), among others, we could envision dividing exempt banks into those that compete with covered banks and those that do not. The regulation's impact on the former would reflect equilibrium pricing effects, while the latter would become the "clean" control group. However, competition between banks does not lend itself to such a clear division, because most exempt banks compete with covered banks to some degree. Instead, the covariate $f(c_{ait})$ builds on the variable *Exposure*, which captures how intensely a given exempt bank competes with covered banks on a continuous [0,1) scale, with 0 signifying complete isolation from covered banks and a value close to 1 representing a situation in which all of a bank's competitors are covered by Reg II. Initial analysis indicated that it is important to allow for nonlinearity in the effect of *Exposure*, with much of the effect arising at relatively low levels of exposure and diminishing effects at higher levels. Various functional forms of the $f(\cdot)$ function can incorporate such nonlinearity, and our primary results employ the cubed root function. In the Appendix, we consider a number of alternative functional forms for the $f(\cdot)$ function.

A number of assumptions, in addition to functional form assumptions, underlie identification of the regulation's effects. First, we require that exempt banks not be directly influenced by the regulation, but only experience equilibrium pricing responses because of their competitive exposure to covered banks. Second, the analysis assumes that the Reg II "treatment" is exogenous, which requires that banks not adjust their assets in order to avoid being covered by the regulation. As noted in our discussion of the data, we see very few banks make any transition around the threshold, and more move above it than shrink below. In addition, Kay, Manuszak

and Vojtech (2014) find no evidence that banks systematically altered their assets in the period around Reg II. Finally, we require that Reg II not affect the pricing of savings accounts, which experience the same unrelated shocks to pricing, λ_{it}, as checking accounts. If re-optimization of pricing occurred across a bank's entire product line in response to Reg II, or if competition in savings led to different responses to non-Reg II shocks for large banks, then estimates based on the comparison between checking and savings accounts could be biased.

We analyze the binary variables *FreeAccount* and *AvoidFee* using a linear probability model, rather than a nonlinear logit or probit model.[28] We use this approach because, unlike in a nonlinear binary outcome model, a linear probability model allows us to easily incorporate a rich set of fixed effects. Not only would a nonlinear binary outcome model with fixed effects be difficult to estimate computationally, we would also face the "incidental parameters problem" (Lancaster, 2000) that could result in biased estimates. Importantly, our application avoids the main drawback of linear probability models, namely that they can generate predicted outcomes outside of [0,1] for some values of the explanatory variable. Our main regressor – status under the regulation – is binary, so we are effectively comparing averages for observations in cells corresponding to different status under the regulation (and different time periods). The regressions with the continuous *Exposure* metric do not involve simple cell averages, but the regressor based on that variable is bounded between 0 and 1 by construction, so that the predicted outcomes would be as well.[29]

[28] In theory, because these variables are averages across a bank's branches, they could take values other than zero or one. In practice, this is exceedingly rare, but a linear probability model allows us to incorporate these unusual observations.

[29] The linear probability model also generates heteroskedastic errors. We address this issue by using standard errors, clustered at the bank level, that are robust to arbitrary heteroscedasticity.

V. Regression Results

Our results are presented in Table III. In these regressions, an observation is a bank-quarter, with each dependent variable reflecting the average value across all branches sampled in a quarter for a given bank. We perform the analysis separately for each outcome variable, with the dependent variable capturing the term or fee for each account type. The results for each variable comprise the four panels in Table III.

Turning to the columns in Table III, the specifications differ in their underlying identification strategies and whether they account for equilibrium pricing effects or leading and lagged responses. Columns (1) and (2) represent simple "difference-in-difference" specifications with column (1) including only information on checking accounts at covered and exempt banks and column (2) considering checking and savings accounts only at covered banks. Column (3) is a "triple-difference" specification that combines these identification strategies, and column (4) adds two-period leading and lagging terms. Column (5) then builds on column (3), adding covariates that capture equilibrium pricing effects. The final two columns further add two-period leading and lagging terms for only covered banks (in column (6)) and for both covered and exempt banks (in column (7)). This last column corresponds to the full regression model in the previous section.

V.A. Did banks covered by Reg II raise prices?

The estimates from all specifications broadly provide evidence that Reg II resulted in covered banks increasing prices on affected accounts in an attempt to recover revenue lost due to lower interchange fees. Specifically, the signs of almost all statistically significant coefficients are consistent with higher expected fees for accountholders:

lower availability of free accounts, higher monthly fees, lower likelihood that the monthly fee could be avoided, and a higher minimum balance to avoid the fee. The exact manner in which these different terms and fees changed across account types is analyzed in the following section.

Analyzing columns (1) and (2), we find that even the simple "difference-in-difference" approach allows for meaningful identification of Reg II's impact on the dependent variables. The estimated coefficients have broadly similar values for the two columns, which suggests that each identification strategy is reasonably successful at identifying the true value of the coefficients. Comparing columns (1) and (2) with column (3), we find that employing both identification strategies generates coefficient estimates which are mostly closer to those in column (1) than in column (2). Under the assumption that a bank's coverage status did not influence its pricing on savings accounts following Reg II, this suggests that any shocks to covered banks that were contemporaneous with the regulation were relatively less important compared with the effect of the regulation itself. In other words, checking account pricing at covered banks appears primarily driven by the interchange fee restriction rather than other factors related to the financial crisis or subsequent regulatory initiatives.

Comparing columns (3) and (4), we find that the addition of leading and lagging terms increases the coefficient estimates for all statistically-significant coefficients. This finding suggests that the adjustment of terms and fees following the introduction of Reg II was not always instantaneous, but rather occurred gradually over multiple quarters. For example, we find that the availability of "free" noninterest checking accounts fell for covered issuers 9.2 percent and 16.9 percent, respectively, two quarters and one quarter ahead of Reg II's effective date. Intuitively, the coefficients on these leading effects have the same sign, but smaller magnitude, than the estimated long-term decline of 26.3 percent. The implication of this finding is twofold. First,

from an econometric perspective, not accounting for leading/lagging effects can result in the underestimation of coefficients of interest. Second, from a policy perspective, it is important to recognize that market reactions to regulations may not be fully observed immediately when the regulation goes into effect.

V.B. Analysis of nonlinear pricing employed by banks

In addition to providing evidence that banks increased prices in response to Reg II, Table III also documents the specific mechanisms that banks used. With respect to the provision of free accounts, we find a large and highly significant drop in the propensity of covered banks to offer free noninterest checking accounts. In contrast, we find only a small and statistically insignificant decline in the provision of free interest checking accounts. These findings reflect that, before Reg II, only 6.2 percent of covered banks offered free interest checking accounts, compared with 51.7 percent for noninterest checking accounts. Thus, banks imposed new monthly fees where they were able to do so (that is, for noninterest checking accounts), but were constrained in their ability to introduce fees where most accounts already had a monthly fee (that is, for interest checking accounts).

Beyond the estimated decline in the provision of free noninterest checking accounts at covered banks, our most significant results, both economically and statistically, relate to the continuous monetary variables, *MinBalance* and *MonthlyFee*. First, covered banks increased the likelihood that consumers would be subject to a monthly account fee by raising the minimum balance to avoid a fee for interest checking accounts. Focusing on column (7), the increase of $1,694.1 for interest checking accounts is especially economically significant given that the average checking account balance (across all types of accounts) was around $2,000 in 2013 (Bricker et al, 2014). Cov-

ered banks further found it optimal to increase monthly fees; we find large and highly significant increases in *MonthlyFee* for both types of accounts. To put the estimated values in context, a monthly fee that is $1 higher would offset the lost interchange revenue on an account used for around four debit card transactions per month given the $0.27 decline per transaction in average interchange fees for covered banks when the regulation took effect.

At the same time, we do not find that covered banks chose to significantly decrease the percentage of checking accounts on which monthly fees can be avoided. According to our estimates, covered banks were less likely to allow accountholders to avoid fees, but the decline is statistically significant only for interest checking accounts, and economically insignificant for both account types. These findings suggest that consumers may be particularly averse to being forced to pay account fees, as evidenced by the fact that, before the regulation, fees were mandatory on only around 11.8 percent and 1.2 percent of noninterest and interest checking accounts, respectively, at covered banks. Alternatively, it is possible that banks value the ability to price discriminate across consumers by allowing consumers with high enough balances to avoid fees.[30]

Finally, the factors underlying the nonlinear price responses of covered banks to the fall in interchange fee revenues are of interest, especially given the estimated differences between the two types of checking accounts. Specifically, we find that, for noninterest checking accounts, the greatest response seems to have come from a large fall in the proportion of banks that offer free accounts, while for interest checking accounts, the response involved a larger increase in monthly fees and the minimum balance to avoid the monthly fee. A possible explanation is that the nonlinear price

[30] Other examples of second-price price discrimination in checking accounts are described by Stavins (1999).

responses were driven by the varying degrees to which consumer demand is sensitive to the different terms and fees on the two account types. While answering this question is beyond the scope of this paper, it may be an interesting research avenue to consider in the future given the prevalence of nonlinear pricing in the retail banking industry.

V.C. Analysis of equilibrium pricing effects

The estimates from columns (5) – (7) provide evidence that banks exempt from Reg II also raised their prices, depending on their exposure to competition from covered banks. Focusing on column (7), the signs on all coefficients indicate that, as at covered banks, prices increased at exempt banks facing more significant competition from covered banks. However, the magnitude of the estimated competitive response is generally smaller than the pricing response estimated for the covered banks. For example, our results suggest that exempt banks fully exposed to competition from covered banks would reduce the availability of free noninterest checking accounts 15.5 percent, compared with a reduction of 35.2 percent for covered banks. This difference is expected, given that covered banks' response represents both the first-order price change as a result of lower revenues (higher costs), as well as the second-order competitive response to price increases by exempt competitors.

From an econometric perspective, the estimates from columns (5) – (7) emphasize the importance of accounting for equilibrium effects when analyzing the impact of a policy that only applies to a portion of the population. First, as described above, the policy may have an impact on the portion of the population that is not directly affected by the policy. Second, not accounting for equilibrium effects can lead to underestimation of the direct impact of the policy on the affected portion of the population. This possibility is illustrated by comparing the coefficients capturing

Reg II's impact on covered banks in columns (3) and (5), which are uniformly higher in the latter specification.

These findings are also notable more broadly. From an economic perspective, they illustrate that competition among firms may generate an industrywide response even for a policy that only targets a specific set of firms. Such spillover effects thus serve to magnify the pricing response by banks that was, for many, an unintended consequence of the regulation. Their presence also suggests that policymakers need to take a broad view of an entire industry, even when evaluating policies that directly target only a subset of firms.

VI. Conclusion

Legislation and other actions that followed the recent financial crisis have generated new regulations of banks and other financial institutions. Often, these regulations have a direct impact on certain activities or firms, but may also involve consequences for prices, firms, or markets that are not direct subjects of a regulation. These consequences may be particularly complex in an industry, such as the industry for payment cards and other financial services, where two-sided market considerations are important. Moreover, in an industry where heterogeneous firms compete, as in the banking industry, a regulation that addresses a narrow activity or set of firms may alter the entire market equilibrium, thereby indirectly affecting firms outside the regulation's immediate scope.

In this paper, we focus on a particular regulation which illustrates the challenges associated with empirically evaluating a regulation's effect, namely the interchange fee cap for certain debit card transactions that resulted from the Durbin Amendment to the Dodd-Frank Act. The economic theory of interchange fees and two-sided

markets broadly predicts that cardholder fees may change as banks respond to a cut in interchange fee revenue. Correspondingly, we examine terms and fees for deposit accounts around the regulation's effective date and find clear evidence that, in an indirect consequence of the regulation, those variables changed in ways that increased deposit account prices for accountholders. By focusing on individual terms and fees we are able to accurately capture the complex ways in which financial institutions may adjust their prices in response to a regulatory cut in revenue.

Our analysis exploits variation in the cap's applicability across account types and banks, including an exogenous statutory provision that clearly delineated banks into a group subject to the interchange fee cap and another group exempt from it. Unlike for many other regulations, including interchange fee regulations elsewhere in the world, such variation allows us to clearly disentangle the regulation's impact from other factors affecting account terms and fees.

In addition, we characterize exempt banks' competitive exposure to covered banks and find evidence that exempt banks with greater exposure mimicked the price responses of covered banks, albeit to a lesser degree. Such equilibrium effects have important and broad implications for empirically evaluating a regulation's impact. First, not accounting for equilibrium effects can lead to biased estimates of the regulation's impact on institutions it covers. Second, when evaluating the impact on the industry as a whole, it is important to not just look at the institutions covered by the regulation, but also at those in competition with the regulated institutions, whose actions may be indirectly affected by the regulation.

Finally, this paper suggests avenues for future research. Bank services tend to involve a bundle of products with complex nonlinear pricing, but analyzing the way in which prices are set is complicated by the co-determination of all price terms at the same time. Our analysis demonstrates how account pricing may change in response

to regulations that target one aspect of account services, and provides an opportunity to investigate the reasons underlying the exact form such a price response may take. Such research could be combined with analyzing how accountholders responded to the changes in account terms and fees that we document. The degree to which accountholders switched banks following these price changes may provide information about switching costs for banking services, which are often cited as an important feature of the market. Other data sets may provide information about this consideration, and Reg II may provide a useful exogenous shock in which to examine it. A final interesting avenue of future research is to examine the pass-through of the cut in interchange fees from acquiring banks to transaction fees for merchants (and, ultimately, retail prices).

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Table I: Sample means over time by account type and issuer status

		Covered Issuers		Exempt Issuers	
Variable	Account type	Pre-Reg II	Post-Reg II	Pre-Reg II	Post-Reg II
FreeAccount	Noninterest checking	51.7	30.0	54.0	55.3
(%)	Interest checking	6.2	8.3	6.5	7.2
	Savings	11.8	15.3	25.5	22.8
MonthlyFee	Noninterest checking	6.74	7.68	6.02	6.03
($)	Interest checking	11.89	13.18	8.67	8.89
	Savings	5.05	5.40	3.86	4.05
AvoidFee	Noninterest checking	82.6	86.7	88.2	89.8
(%)	Interest checking	100.0	97.1	98.8	98.8
	Savings	100.0	100.0	99.9	100.0
MinBalance	Noninterest checking	848.34	1098.98	592.58	620.68
($)	Interest checking	3062.55	4570.28	1402.43	1713.12
	Savings	292.76	387.36	177.77	189.05

Source: RateWatch and Federal Reserve Board

Table II: Exposure variable summary statistics for exempt issuers

Base	Mean	Std. Dev.	Min	Median	Max
Deposits	0.19	0.15	0.00	0.16	0.68
Branches	0.23	0.19	0.00	0.19	0.98

Notes: Exposure measures the degree to which an exempt banks faces competition from covered banks, calculated as the share of market-level branches owned or deposits held by covered banks averaged across all local banking markets in which an exempt bank operates. Time period captured in summary statistics is Q1, 2009 – Q2, 2014.
Source: RateWatch, Summary of Deposits, and Federal Reserve Board

Table III: Regression Results

Specifications summary

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Data included	Savings accounts		X	X	X	X	X	X
	Exempt banks	X		X	X	X	X	X
Fixed-effects	γ_{ai}	X	X	X	X	X	X	X
	θ_{at}	X		X	X	X	X	X
	λ_{it}		X	X	X	X	X	X
Leads/lags	w_{ait}^{lag}				X		X	X
	c_{ait}^{lag}							X

A. FreeAccount (%)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\beta_{chk\_non\_int}$	−23.7	−24.4	−24.4	−26.3	−32.7	−34.6	−35.2
	(2.1)	(1.9)	(2.4)	(2.6)	(3.0)	(3.2)	(3.4)
$\delta_{chk\_non\_int}$					−14.6	−14.5	−15.5
					(3.3)	(3.3)	(3.7)
β_{chk_int}	0.9	3.6	0.6	0.9	−3.5	−3.2	−4.8
	(0.7)	(1.4)	(1.8)	(2.0)	(2.4)	(2.5)	(2.7)
δ_{chk_int}					−7.2	−7.2	−10.0
					(2.7)	(2.7)	(3.0)
R^2	0.839	0.839	0.914	0.914	0.914	0.914	0.914
No. obs	32,299	3,300	49,192	49,192	49,171	49,171	49,159
No. banks	3,865	102	3,877	3,877	3,877	3,877	3,877

B. MonthlyFee ($)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\beta_{chk\_non\_int}$	0.58	0.73	0.96	1.03	1.37	1.44	1.34
	(0.21)	(0.20)	(0.30)	(0.34)	(0.46)	(0.47)	(0.42)
$\delta_{chk\_non\_int}$					0.72	0.72	0.54
					(0.54)	(0.54)	(0.44)
β_{chk_int}	1.17	1.53	1.48	1.69	1.79	2.01	2.02
	(0.18)	(0.17)	(0.25)	(0.28)	(0.30)	(0.32)	(0.32)
δ_{chk_int}					0.54	0.54	0.56
					(0.28)	(0.28)	(0.27)
R^2	0.884	0.941	0.941	0.940	0.941	0.941	0.941
No. obs	23,900	2,952	37,809	37,809	37,270	37,270	37,270
No. banks	4,168	98	4,284	4,284	4,219	4,219	4,219

C. AvoidFee (%)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\beta_{chk\_non\_int}$	−1.7	−0.3	−1.7	−1.7	−2.6	−2.6	−2.9
	(2.2)	(1.8)	(2.5)	(2.9)	(3.0)	(3.3)	(3.4)
$\delta_{chk\_non\_int}$					−1.4	−1.4	−2.0
					(3.1)	(3.1)	(3.6)
β_{chk_int}	−1.2	−1.8	−1.3	−1.3	−3.0	−3.0	−3.2
	(0.6)	(0.9)	(1.2)	(1.4)	(1.4)	(1.6)	(1.6)
δ_{chk_int}					−2.9	−2.9	−3.2
					(1.3)	(1.3)	(1.5)
R^2	0.800	0.778	0.885	0.885	0.885	0.885	0.885
No. obs	21,947	2,853	34,557	34,557	34,539	34,539	34,529
No. banks	3,394	96	3,516	3,516	3,516	3,516	3,516

D. MinBalance ($)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\beta_{chk\_non\_int}$	236.1	435.9	297.4	255.4	403.9	364.7	423.7
	(113.6)	(146.2)	(202.4)	(219.1)	(217.7)	(230.7)	(232.1)
$\delta_{chk\_non\_int}$					165.5	169.8	265.2
					(185.8)	(185.9)	(209.4)
β_{chk_int}	1,025.4	1,387.3	947.0	1,163.3	1,344.6	1,555.9	1,694.1
	(184.9)	(168.3)	(235.0)	(257.9)	(266.3)	(281.4)	(280.7)
δ_{chk_int}					686.2	679.3	916.4
					(242.9)	(242.9)	(258.1)
R^2	0.738	0.910	0.877	0.877	0.877	0.877	0.877
No. obs	20,541	2,728	33,152	33,152	33,134	33,134	33,124
No. banks	3,321	95	3,472	3,472	3,472	3,472	3,472

Notes: The numbers of observations and banks vary across columns for a given variable based on whether data on savings accounts or exempt banks are included in a given specification. Imperfect coverage of the data set used to calculate *Exposure* also causes a small number of observations to be dropped. The numbers of observations and banks differ across outcome variables because certain outcomes are only observed in some circumstances (for example, *MinBalance* is only observed when a bank applies that term to its accounts.) All dummy variables and leading/lagging effects are suppressed. Robust standard errors in parentheses clustered at the bank level.

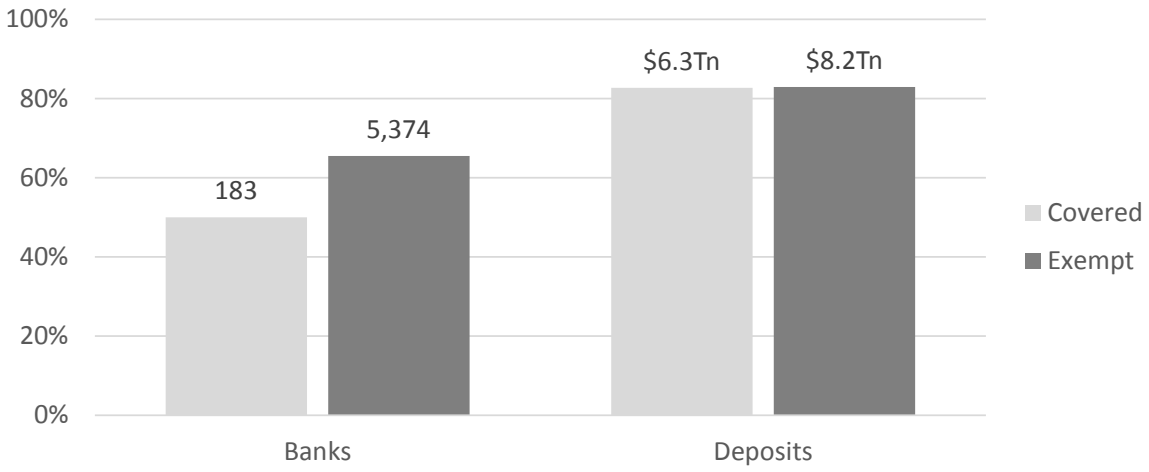
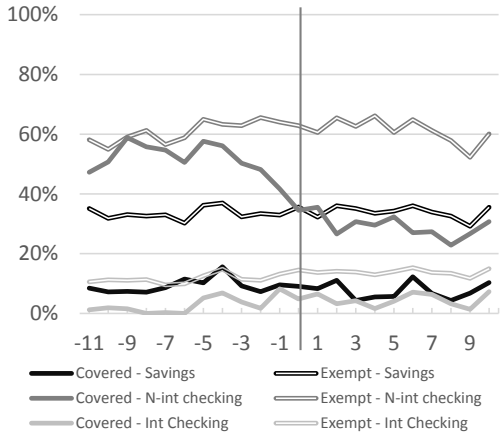


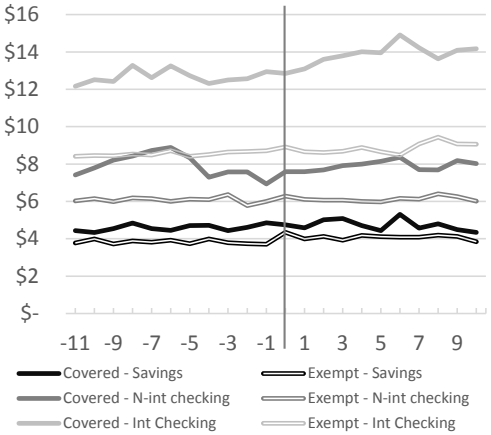
FIGURE I
Data coverage of banks and total deposits by exemption status

Bars represent coverage of data set in terms of banks and total deposits for banks categorized as covered and exempt under Reg II. Values displayed above the bars represent, respectively, total number of banks and average value of total deposits held by banks observed in the data set for period Q1 2009 – Q2 2014. For purposes of this figure, banks are counted at the charter level.
Source: RateWatch, Summary of Deposits, and Federal Reserve Board

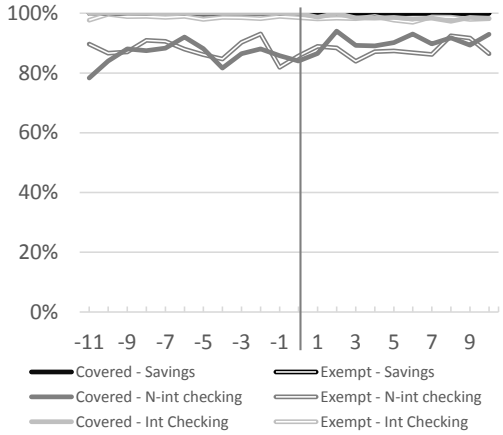
Panel A: FreeAccount



Panel B: MonthlyFee



Panel C: AvoidFee



Panel D: MinBalance

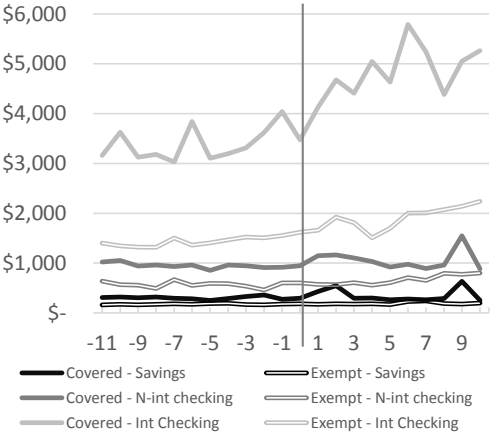


Figure II
Sample means over time by account type and issuer status

Horizontal axes represent quarters, labelled with respect to the Reg II's effective date, with the first period in which the regulation was effective (Q4 2011) labelled '0', the first period in the data set (Q1 2009) labelled '-11' and the last period (Q2 2014) labelled '10'.
Source: RateWatch

Appendix

In this appendix, we examine the sensitivity of our main results to changes in the composition of the data set, ways in which we calculate variables, and the functional form in which those variables enter our model. Our results are presented in Appendix Table I. The econometric specification for each column matches the full regression model, and thus should be compared alongside results in column (7) of Table III in the main text; we have reproduced these results in column (1) in Appendix Table I.

First, we consider the effect of aggregation to the bank-quarter level. As noted in section 3, RateWatch samples account fees and terms for each branch on average once every half a year. When viewed on a quarterly basis, the panel of branch-level observations is thus unbalanced. An implicit assumption when averaging across branches up to the bank-quarter level is that this unbalanced nature of the underlying data does not bias the resulting average fees. Column (2) in Appendix Table I presents estimation results based on data aggregated over a half-year period. The primary takeaway from comparing columns (1) and (2) is that most of the primary results hold, although higher standard errors driven by a smaller sample result in a loss of statistical significance for the increase in *MonthlyFee* for interest checking accounts and the increase in *MinBalance* for noninterest checking accounts. In addition, the absolute values of the statistically significant coefficients in column (2) are higher than in column (1), in particular for *MonthlyFee* and *MinBalance*. As a result, the indirect impact of Reg II on *MinBalance* for noninterest checking accounts at exempt banks is statistically significant in column (2).

Second, we examine whether our main results are affected by the unbalanced nature of the panel data set arising from the nature of the variables *MonthlyFee*, *AvoidFee*, and *MinBalance*. Consider a covered issuer that offered a free account

before Reg II, but afterwards started charging a monthly fee. While the resulting panel of *MonthlyFee* will be unbalanced (it is only observed if *FreeAccount* = 0), the estimates of Reg II's impact on the corresponding coefficient will be unaffected as long as the fee introduced by the covered issuer is comparable to that charged by other covered issuers. However, if the issuer starts charging a fee that is lower than those charged by other issuers after Reg II, this may lead to biased estimates of Reg II's impact on *MonthlyFee* (and, by similar argument, *AvoidFee*). The same problem may arise for *MinBalance*, which is only observed when *FreeAccount* = 0 and *AvoidFee* = 1.

To test whether this issue affects our estimates, column (3) in Appendix Table I presents results based on a panel data set that excludes issuers for which the value of *FreeAccount* or *AvoidFee* changed within three periods of Reg II's implementation. Comparing this column with column (1), we find that the coefficient estimates on *MonthlyFee* for noninterest checking accounts, and *MinBalance* for account types increase in magnitude, confirming the hypothesis that issuers who began charging a monthly fee and offering their customers the possibility to avoid the fee by maintaining a minimum balance on their accounts only following the introduction of Reg II set their fees and thresholds at a lower level than those who had been charging it all along. On the other hand, the magnitude of Reg II's impact on *MonthlyFee* for interest checking accounts falls, suggesting the opposite effect. Nonetheless, the majority of the primary results hold, leading us to conclude the unbalanced nature of the panel does not introduce significant bias in our results.

Third, we consider the effect of an alternative specification of *Exposure*, in which the variable is constructed based on the count of branches of covered/exempt banks, rather than on the deposits held at branches. Comparing summary statistics of the alternative variable with the original variable (see Table II), we find that the

mean of the alternative specification is higher, reflecting the fact that covered banks have a lower average level of deposits per branch than exempt banks. Turning to the estimation results in Appendix Table I, comparing column (4), which presents estimation results using the alternative *Exposure* variable, to column (1), we find that most of the primary results hold.

Finally, we consider alternative specifications for the $f(\cdot)$ function which captures the nonlinear impact on exempt banks of competitive exposure to covered banks. The results are presented in columns (1), (5) and (6), which present the results for cubed root, linear and square root specifications of $f(\cdot)$, respectively. First, we find strong evidence of nonlinearity in the impact on exempt banks of competitive exposure to covered banks - more coefficients are statistically significant, and absolute values of all statistically significant coefficients are larger in column (7) than in column (6). This latter finding is consistent with the effect of exposure arising at relatively low levels of exposure and diminishing at higher levels. Second, comparing columns (1) and (7) we find that the effect of exposure appears to be strongly nonlinear, favoring the cubed root specification of the $f(\cdot)$ function, though the difference is less pronounced than between the linear and square specifications.

Appendix Table I: Regression results – sensitivity analysis

Specifications summary – sensitivity analysis

		(1)	(2)	(3)	(4)	(5)	(6)
Level of data aggregation	Bank-quarter	X		X	X	X	X
	Bank-half-year		X				
Banks which switch *FreeAccount* or *AvoidFee* around Reg II	Included	X	X		X	X	X
	Excluded			X			
Exposure base variable	Deposits	X	X	X		X	X
	Branches				X		
$f(\cdot)$	Cubed root	X	X	X	X		
	Linear					X	
	Square root						X

A. FreeAccount (%)

	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{chk\_non\_int}$	−35.2	−38.7	−35.2	−36.6	−28.9	−32.2
	(3.4)	(4.7)	(3.4)	(3.4)	(2.8)	(3.1)
$\delta_{chk\_non\_int}$	−15.5	−16.7	−15.5	−19.1	−10.3	−12.9
	(3.7)	(4.8)	(3.7)	(4.1)	(4.3)	(3.8)
β_{chk_int}	−4.8	−7.2	−4.8	−3.9	−1.5	−3.5
	(2.7)	(3.7)	(2.7)	(2.7)	(2.2)	(2.5)
δ_{chk_int}	−10.0	−10.8	−10.0	−9.0	−9.9	−9.7
	(3.0)	(4.0)	(3.0)	(3.3)	(3.4)	(3.0)
R^2	0.914	0.918	0.914	0.914	0.914	0.914
No. obs	49,159	39,298	49,159	49,159	49,159	49,159
No. banks	3,877	3,579	3,877	3,877	3,877	3,877

B. MonthlyFee ($)

	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{chk\_non\_int}$	1.34	1.63	0.97	1.44	1.13	1.25
	(0.42)	(0.63)	(0.52)	(0.45)	(0.37)	(0.40)
$\delta_{chk\_non\_int}$	0.54	0.71	0.47	0.77	0.40	0.48
	(0.44)	(0.59)	(0.45)	(0.49)	(0.55)	(0.46)
β_{chk_int}	2.02	2.35	2.10	2.04	1.82	1.94
	(0.32)	(0.47)	(0.33)	(0.34)	(0.29)	(0.30)
δ_{chk_int}	0.56	1.07	0.57	0.63	0.49	0.55
	(0.27)	(0.38)	(0.28)	(0.33)	(0.33)	(0.27)
R^2	0.939	0.934	0.942	0.939	0.939	0.939
No. obs	34,544	27,975	33,327	34,544	34,544	34,544
No. banks	3,513	3,278	3,510	3,513	3,513	3,513

C. AvoidFee (%)

	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{chk\_non\_int}$	−2.9	−0.8	4.5	−2.6	−3.6	−3.5
	(3.4)	(5.1)	(3.8)	(3.5)	(3.1)	(3.3)
$\delta_{chk\_non\_int}$	−2.0	0.7	−0.7	−1.5	−7.9	−3.8
	(3.6)	(4.8)	(3.6)	(3.9)	(4.7)	(3.8)
β_{chk_int}	−3.2	−3.7	−3.2	−2.9	−2.0	−2.7
	(1.6)	(2.4)	(1.5)	(1.6)	(1.4)	(1.5)
δ_{chk_int}	−3.2	−4.0	−3.0	−2.9	−2.7	−3.0
	(1.5)	(2.1)	(1.5)	(1.7)	(1.8)	(1.5)
R^2	0.885	0.885	0.898	0.885	0.885	0.885
No. obs	34,529	27,953	33,322	34,529	34,529	34,529
No. banks	3,516	3,278	3,513	3,516	3,516	3,516

D. MinBalance ($)

	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{chk\_non\_int}$	423.7	453.0	482.8	357.2	316.5	372.3
	(232.1)	(320.7)	(323.6)	(236.7)	(222.5)	(228.2)
$\delta_{chk\_non\_int}$	265.2	552.6	447.3	163.5	207.4	222.9
	(209.4)	(309.3)	(210.2)	(239.6)	(292.3)	(229.3)
β_{chk_int}	1,694.1	1,894.3	1,963.3	1,612.3	1,337.3	1,548.3
	(280.7)	(392.2)	(323.9)	(286.8)	(264.8)	(274.4)
δ_{chk_int}	916.4	1,164.8	998.8	828.1	692.3	835.0
	(258.1)	(356.6)	(269.9)	(289.7)	(353.5)	(279.4)
R^2	0.877	0.871	0.877	0.877	0.877	0.877
No. obs	33,124	26,854	31,686	33,124	33,124	33,124
No. banks	3,472	3,240	3,462	3,472	3,472	3,472

Notes: Column (1) corresponds to column (7) from Table III and presents the primary results from the full model. The numbers of observations and banks vary across columns for a given variable based on aggregation over time (for column (2)) and the criteria imposed on inclusion in the sample (for column (3)). The numbers of observations and banks differ across outcome variables because certain outcomes are only observed in some circumstances (for example, *MinBalance* is only observed when a bank applies that term to its accounts). All dummy variables and leading/lagging effects are suppressed. Robust standard errors in parentheses clustered at the bank level.