Price Regulation in Two-Sided Markets: Empirical Evidence from Debit Cards

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Abstract

This paper studies the impact of price regulation in two-sided markets, where intermediaries must get both sides of the market on board. Since platforms such as debit card networks can only succeed by simultaneously convincing consumers to use cards and merchants to accept them, they often subsidize one side of the market to generate supracompetitive profits from the other side (Rochet and Tirole 2003). Using a novel dataset on card processing fees, we show a regulation restricting banks' ability to charge high processing fees (the Durbin Amendment of the 2010 Dodd-Frank Act) transferred value from the previously subsidized side of the market—consumers—to merchants. Our evidence adds empirical support to the concern that market failures in two-sided markets are hard to identify, and potentially even harder regulate.

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I. Introduction

Fees paid by merchants for processing credit and debit card transactions in the U.S. totaled more than $100 billion in 2018 (Nilson 2019). This is substantially more than the costs card networks and card issuing banks incur when processing these transactions. Do the resulting profits represent economic rents that regulators should rein in? Perhaps surprisingly, a long line of theoretical literature argues that the answer to this question may be: no (Rochet and Tirole 2002, 2003, 2006; Wright 2004; Weyl 2010). This is because two-sided markets involve network externalities. Credit and debit card users benefit when more merchants accept cards; and merchants benefit when more consumers use cards. Platforms that intermediate in these markets must, in the words of Rochet and Tirole (2003), choose price structures that "get both sides of the market on board." In such settings, both consumer surplus and total surplus are often maximized by treating one side of the market as a profit center and the other side as a loss leader (Rochet and Tirole 2003). Consequently, high prices on one side of the market are not obviously anticompetitive. This is in contrast to traditional one-sided markets, where the presence of prices that substantially exceed cost is often indicative of a market failure.

In summary, theory suggests that two-sided markets may not be anticompetitive simply because prices are above cost on one side of the market. But it acknowledges that these prices may be "wrong"—that is, prices set at the privately optimal level by platforms may not equal socially optimal prices (Rochet and Tirole 2003b; Wright 2004b). However, there is little empirical evidence on whether two-sided markets can be improved by regulatory intervention; or if instead they are well-functioning.[1]

Since two-sided markets have grown in importance over time, this is an increasingly significant gap in our understanding. For instance, in 1990, less than 15 percent of U.S. transactions were made with payment cards; in 2018, this share was nearly 70 percent (Eubanks and Smale 2002; Kumar and O'Brien 2019). More generally, the emergence of

[1] The complexity for regulators is that two-sided markets are distinct for two reasons: 1) network effects exist; and 2) these markets are concentrated. These go hand-in-hand: network effects create a barrier to entry for new competitors. Efforts to rein in intermediaries' market power by cost regulation on one side of the market could raise costs on the other side, leading to fewer participants. Accordingly, regulations designed to decrease market power can also decrease network size and render it less valuable. Further, such interventions could merely shift rents away from the platform and toward other market participants.

two-sided platforms across a variety of industries, including ridesharing (Uber and Lyft), retail goods (eBay and Alibaba), and social media (Facebook and Twitter), all raise the same fundamental questions for policymakers: are prices too high, and should regulators intervene?

The contribution of this article is to marshal empirical evidence to advance our understanding of two-sided markets: we assess the theoretical prediction that regulations requiring prices to be set at marginal cost on one side of a two-sided market ("cost-based regulation") will be socially beneficial (Rochet and Tirole 2003; Wright 2004). Specifically, we study cost-based regulation of debit card processing fees ("interchange fees") and show intervention can change price structures in two-sided markets. When financial institutions are no longer able to charge high prices to merchants for processing debit transactions, they shift costs to the previously subsidized side of the market: consumers. Merchants do not fully pass-through the benefits of regulation to their customers. As such, efforts to rein in financial institutions' rents increase merchants' rents; and perversely, regulation aimed at benefitting consumers makes them worse off.

The setting for our study is Section 1075 of the Dodd-Frank Act of 2010, colloquially known as the "Durbin Amendment" for its main sponsor, Senator Richard Durbin of Illinois. The Durbin Amendment ("Durbin") required that the interchange fees paid by merchants be "reasonable and proportional to the cost incurred" for processing those transactions. The objective of this regulation was to merchants' costs so that the savings would be passed along to consumers in the form of lower retail prices.

To assess the incidence of the Durbin amendment, we construct a novel dataset that includes the effective interchange fees that U.S. merchants pay. This proprietary dataset is made available to us by a leading payments industry player, subject to robust privacy and data protection controls. We combine this interchange data with branch-level bank account pricing data, and daily prices set by gas retailers across the ten most populous states in the U.S. Overall, we have branch-level pricing data for nearly 70 percent of bank holding companies, daily prices for over 50 percent of all gas stations, and ZIP-level interchange data for nearly 80 percent of all ZIP codes in the United States.

Using these data, we show that banks, who collectively lost $5.5 billion in annual revenue because of Durbin, passed 42 percent of these losses through to bank consumers by eliminating free checking accounts. We also show that merchants passed through at most 28 percent of Durbin savings to consumers. Thus, as summarized below, Durbin led to a value transfer from banks and consumers to merchants. Prior to regulation, the price structure in this market offered consumers loss-leader prices and generated revenue from merchants. Durbin's cost-based regulation shifted the price structure, benefitting merchants at the expense of consumers, who now pay higher fees.

Estimated annual impact of Durbin in $ billion

Source of gain/loss	Banks	Merchants	Consumers
Interchange fees	–5.5	+5.5	
Checking account fees	+2.3		–2.3
Retail prices		–1.5	+1.5
Net change	–3.2	+4	–0.8

Following the financial crisis, there was an overhaul of financial regulation. Thus, a reasonable concern is that we may mischaracterize bank and merchant responses to this broader shift as a Durbin effect. However, Durbin applied only to banks with more than $10 billion in total assets. To understand Durbin's effect on bank and merchant pricing and, ultimately, the extent to which consumers benefited from its passage, our empirical design compares banks and merchants impacted by Durbin (banks above the $10 billion threshold and merchants with customers who use debit cards from these banks) to those not impacted. Our identifying assumptions are twofold. On the bank side, we assume that, in the absence of Durbin, interchange revenue and account fees of banks covered by Durbin and banks exempt from Durbin would have followed parallel trends. On the merchant side, we assume that prices set by gas retailers who were more and less exposed to Durbin would have followed parallel trends. We provide both graphical evidence and formal tests which suggest that these identifying assumptions are valid.

We show that the Durbin interchange cap successfully drove down interchange revenue earned by covered banks. Covered institutions' annual interchange revenue fell by over 30 percent ($5.5 billion). These revenue declines were persistent and there were no comparable declines for banks who were exempt from the new regulation. If passed through by merchants to consumers and not offset by banks' adjusting prices on other consumer financial products, the result would be annual consumer savings about half as large as the Agarwal et. al (2015) estimates of the Credit Card Accountability Responsibility and Disclosure Act's ("CARD Act") welfare enhancement.

Instead, we find significant evidence that banks offset the resulting loss of interchange revenue by raising checking account fees. We estimate that the share of free basic checking accounts[2] fell from 61 percent to 28 percent as a result of Durbin. Average checking account fees rose from $3.07 per month to $5.92 per month. Monthly minimums to avoid these fees rose by 21 percent, and monthly fees on interest-bearing checking accounts also rose by nearly 14 percent. These higher fees are disproportionately borne by low-income consumers whose account balances did not meet the monthly minimum required to waive these fees.

Placebo tests with alternative asset cutoffs show no change in account fees, confirming that our results capture banks' response to Durbin; rather than other regulatory changes that disproportionately impacted large banks following the Great Recession. Our results are also robust to focusing on banks directly above and below the $10 billion threshold, an approach close in spirit to a regression discontinuity design.

It is not obvious a priori what economic model explains our finding that banks adjusted checking account fees, and why this was the lever banks chose to offset their Durbin losses. We make the simple point that the bank checking account is a bundle of services with several price terms. When Durbin regulates one aspect of the checking account bundle (interchange fees), the other aspect (account fees) adjusts to push the aggregate price above the marginal cost.

We next test for Durbin's impact on merchant prices by focusing on the gasoline industry. We choose gas because it is an industry where interchange expense declines substantially post-Durbin: gas retailers account for around 15 percent of total Durbin savings. Furthermore, gas prices are set locally, and products are standardized, allowing

[2] Accounts with a $0 monthly minimum for all customers, regardless of account balance.

for identification of relatively small price movements. We compute an "impact" variable for each ZIP code based on our proprietary interchange data which captures how much merchants save from Durbin.

High values of Durbin impact correspond to regions where consumers tend to bank with large banks; and where debit usage is common. These regions may well be different from low-impact regions in ways that make simple ordinary-least-squares analysis problematic. Instead, to identify the effect of Durbin, we employ matching methods to ensure selection of an appropriate control group. Three different approaches—matching ZIP codes within narrow geographic distances, propensity-score matching, and synthetic control analysis—all confirm the same result: Prices fall post-Durbin only for gas stations in regions where Durbin savings are large. For all other retailers, in the six months following Durbin, we precisely estimate a negligible pass-through of savings to retail prices. In aggregate, we can rule out gas retailers passing through more than 28 percent of their decreased interchange expense to consumers.

As we describe above, this article contributes to the theoretical literature on two-sided markets (Rochet and Tirole 2002, 2003, 2003b, 2006; Wright 2004; Evans and Schmalensee 2005; Armstrong 2006; Farrell 2008; Rysman 2009; Weyl 2010; Valverde et al. 2016). It provides empirical evidence in support of the theoretical conjecture that cost-based regulation in these markets is unlikely to benefit consumers.

We also contribute to a literature that discusses the need for and analyzes the efficacy of consumer financial regulation (Campbell 2006; Sunstein 2006; Bar-Gill and Warren 2008; Barr et al. 2009; Campbell et al. 2011; Campbell 2016). Additionally, we relate to a small but growing empirical literature on interchange (Santiago et al. 2016; Rysman 2007); and on the Durbin Amendment, including Kay et al. (2018) and Manuszak and Wozniak (2017), who, like us, find evidence that banks pass through Durbin losses;[3]

[3] These authors' results are consistent with ours; however, there are notable differences. For example, Kay et. al (2018) fail to account for contemporaneous deposit growth at large banks, and so overestimate their recovery of Durbin losses. Our approach is also more comprehensive, as we document the consequences of Durbin on both sides of the market.

and Wang et al. (2014) and Evans et al. (2013) who rely on survey data to study how merchants respond to Durbin.

Our paper also complements important work on the CARD Act by Agarwal et al. (2015). These authors show that post-crisis price regulation of consumer credit cards reduced borrowing costs by nearly $12B annually. They find no evidence that other price terms adjusted in response to the CARD Act's restrictions. In recent work, Nelson (2019) confirms that overall consumer surplus is increased by the CARD Act.

On their face, Durbin and the CARD Act are similar: post-crisis price regulations of payment instruments involving the same financial institutions. So, their varied efficacy is surprising. However, the CARD Act and the Durbin Amendment regulate different sides of the two-sided card market. Durbin regulates prices paid by merchants who accept cards; the CARD Act regulates prices paid by consumers who use them.

Regulators can successfully lower costs through price regulation if markets are both imperfectly competitive and regulated prices are nonsalient (Agarwal et al. 2015; Gabaix and Laibson 2006). The CARD Act regulated hidden fees that consumers ignore, such as penalty fees for delinquency. In the absence of regulation, sophisticated firms exploit this ignorance and raise these prices above cost. No third party is being "brought on board" into the card market by above-cost late fees.

The joint lessons of the Durbin Amendment and the CARD Act suggest that price regulations that limit firms' ability to take advantage of consumers' behavioral biases can deliver welfare gains. But in the absence of such exploitation, traditional skepticism that intervention will lead regulated firms to "whac-a-mole" losses rings true (Kahn 1979; Mullainathan et al. 2009).

The rest of the paper proceeds as follows. Section II provides background on credit and debit card interchange, describing the concerns that led to the Durbin Amendment and its key provisions. Section III describes the data sources. Section IV discusses our methodology and presents results on banks' and merchants' price response. Section V considers overall consumer welfare as well as distributional effects. Section VI concludes.

II. Background on the Durbin Amendment

a. An Introduction to Interchange

The card system involves four distinct parties: (1) cardholders who use the cards for purchases; (2) merchants who accept the cards; (3) issuing banks who issue cards to cardholders; and (4) acquiring banks who manage the card accounts of merchant clients. In practice, the acquiring banks and the issuing banks can be the same. Card networks are "two-sided" because the success of their platforms relies on their ability to recruit both cardholders to use their cards and merchants to accept them.

Interchange fees are fees paid by the bank of the merchant ("acquiring bank") to the bank of the customer ("issuing bank"). To simplify a complex series of transactions, the interchange fee can be understood as a cost paid from a merchant to a bank for processing a consumer's debit or credit transaction. Unlike virtually all other bank fees (e.g., credit card late fees, overdraft fees, out-of-network ATM fees), interchange fee schedules are set by the card networks that intermediate transactions, not the banks directly (Ausubel 1991). Card networks receive a portion of the fees merchants pay for processing transactions. Prior to Durbin, the interchange fee schedule was equivalent for all bank participants in a card network, although there were differences both among the networks (e.g., American Express with the highest interchange rates) and within networks (e.g., premium rewards cards had higher interchange rates). Historically, interchange fees ranged between 1 and 3 percent of the value of a customer's transaction.

In the decade leading up to the Great Recession, interchange expense became a significant cost of operating for merchants, in some cases even their second highest cost after labor (Gackle 2009). This growth had two causes. First, the use of payment cards increased substantially: in 1990, less than 15 percent of consumer payments were made by credit or debit cards; in 2018, this share is nearly 70 percent (Eubanks and Smale 2002; Kumar and O'Brien 2019). Second, card networks began introducing premium cards with higher interchange fees and card issuers began incentivizing the use of these cards through attractive rewards programs. By 2008, a merchant was paying $1 in interchange fees on a $40 purchase for a premium card (2.5 percent interchange rate); compared with around $0.60 for a basic card (1.5 percent interchange rate) (GAO 2009).

b. Push for regulation and the Durbin Amendment

Concerns about pricing practices in the interchange market prompted attention from the regulatory community well before the crisis (Federal Reserve 2005; GAO 2009).

Initial regulatory proposals considered fee caps for *credit* interchange (Sarin 2019). This was because historically credit interchange rates were higher than debit rates. Additionally, credit is viewed by some as a riskier payment instrument because, unlike debit, it does not decouple transacting from consumer borrowing (Bar-Gill 2004). In fact, predecessor legislation in Australia capped credit interchange rates to discourage excessive credit use and encourage a shift toward debit (Chang et al. 2005; Farrell 2008).

Despite this context, the Durbin Amendment eventually made debit interchange its target. This was after substantial lobbying by credit card networks and financial firms.[6] Senator Durbin, who sponsored the Amendment celebrated the focus on debit, rather than credit, arguing this would mitigate any offsetting behavior by banks (Durbin 2010).

The Durbin Amendment to Dodd-Frank was passed by a vote of 64-33 on May 13, 2010. As a late addition to Dodd-Frank, it was passed without hearings or debate, prompting significant criticism (Nichols 2016). Durbin called on the Federal Reserve to promulgate a rule to ensure that issuer interchange fees for debit transactions be "reasonable and proportional" to the processing cost incurred. In June 2010, the Board issued Regulation II to implement the Durbin Amendment. The Board's initial rule called for a $0.12 fee cap, which, based on comments received by industry and academic experts, was raised to $0.21 per transaction, plus five basis points times the transaction value and an additional $0.01 for the implementation of anti-fraud measures. The final rule was announced on June 29, 2011 to be enacted in October of that year.

Durbin's dollar (rather than percentage) cap changes the structure of interchange rates from a percentage fee to a flat fee, with only five basis points of the transaction value as the variable component. Post-Durbin, for banks above the $10 billion threshold, interchange fees on an average transaction ($38.00) fall from $0.43 to $0.24 (exactly the

[6] This ability of lobbyists to shape Durbin exemplifies Stigler's concerns about regulatory capture (Stigler 1971, 1983).

maximum Durbin allows: $0.22 + 0.05% x $38.00). Average interchange fees for banks below the $10 billion threshold remain at $0.43 per transaction.

III. Data

We use data from a variety of sources to analyze Durbin's incidence.

a. Bank financials

Our initial bank sample includes all bank holding companies with more than $500 million in assets[7] for whom quarterly data is available on the regulatory Call Reports (FRY-9c). We begin our sample in 2008 because prior to this date, interchange income was not reported as a line item on bank financial statements. We are primarily concerned with data reported on bank assets; credit and debit interchange income; and service charges on deposit accounts (a variable that captures all fees associated with deposit accounts, including monthly maintenance fees but also overdraft, check-cashing fees, ATM fees, and many others.). We exclude from our sample bank holding companies who do not report service charges or interchange income quarterly from 2008–2013 and bank holding companies who experience a significant merger.[8] We are left with 451 bank holding companies, 46 above the $10 billion Durbin threshold and 405 below it.

Relevant summary statistics for both our Durbin treatment and control group are highlighted in Table 1. The table presents averages across a range of balance sheet and income statement variables as of year-end 2010 (pre-Durbin) and 2011 (immediately following Durbin's Q4 2011 enactment).

To capture the impact of local market dynamics on bank behavior, we supplement Call Reports with the FDIC's Summary of Deposits. For each county we compute a Herfindahl-Hirschman competition index (HHI) based on local deposit shares of banks

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[7] We exclude small banks (under $500 million in total assets) because of concerns that these are not comparable to the banks impacted by Durbin above the $10 billion threshold. In appendix specifications, we also exclude "megabanks" with more than $100 billion in assets.

[8] Banks only report interchange income if it is 3 percent or more of total non-interest income. By excluding banks who do not report interchange income throughout our sample, we understate Durbin's impact. 10 percent of banks who reported non-interest income in Q3 2011 (prior to Durbin) no longer report this income in Q4 2011 after Durbin is enacted. A bank experiences a significant merger if its assets change by 20 percent or more in a quarter.

with branches in that county. Intuitively, this index captures the probability that two randomly drawn dollars of bank deposits within a county are held by the same bank.

b. Bank account pricing

RateWatch, a data collection firm, surveys bank branches weekly for information on their fee-setting practices. While a fee-setting branch remains in its sample, RateWatch provides data at a weekly frequency on its monthly maintenance fee and the minimum deposit required in that account to avoid the fee. For non-transactional accounts (savings and money market accounts), RateWatch also provides information on withdrawal fees. We average weekly data for quarterly snapshots of branches' fee-setting practices.

Importantly, RateWatch surveys only fee-setting branches. It provides data on linkages between fee-setting branches and non-fee-setting branches. However, it only contains reliable data on the most recent linkage. To avoid conflating the impact of the Durbin Amendment with unrelated changes in bank-branch relationships, we thus restrict our analysis to all *fee-setting branches* of the bank holding companies in our sample. Our sample thus contains data on 4,127 unique bank branches[9]

Relevant summary statistics for fee-setting practices of branches in the Durbin treatment and control group are highlighted in Table 2. The table considers fees associated with bank checking, interest checking, savings, and deposit accounts in Q4 2010 (pre-Durbin) and Q4 2011 (immediately following Durbin's Q4 2011 enactment). We define a branch as offering a free checking or savings account if it has a $0 monthly maintenance fee associated with this account, regardless of account size.

c. Interchange data

We also obtained access to proprietary data on merchant interchange rates from a leading payments industry player. For 120 retail merchant categories (ranging from grocery stores, to barber shops, to gas stations), we received aggregated and anonymized data at the ZIP-level on the total volume of regulated (card issued by bank above $10

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[9] These totals are for basic checking accounts, which are our main focus. They are very similar for savings accounts (4,108 branches), interest checking accounts (4,119 branches); and money market accounts (4,097 branches).

billion Durbin threshold) and unregulated (card issued by bank below $10 billion threshold) debit, as well as the number of transactions and the interchange fees collected. Table 3 provides a snapshot of the granular data in our sample.

Absent regulation, interchange schedules differ significantly across industries and even merchants within an industry. For example, as publicly available interchange schedules show, transactions made with Mastercard debit cards issued by small banks (below the $10 billion Durbin threshold) have a base interchange rate of 1.05% + $0.15 in 2018, but grocery merchants with significant debit card volume receive a discount: "Tier-1" grocers (with annual debit volume of $400 million or more) pay only 0.70% + $0.15. Credit card pricing has a similar tiered structure—in 2018, Visa credit cards used at grocers with $92.7 million or more in annual volume pay 1.15% + $0.05, but those with less than $14.8 million annually pay seven basis points more: 1.22% + $0.05.

This publicly available tiered pricing schedule does not fully capture differences in unregulated interchange rates: Merchants with significant market power may negotiate even more attractive terms (Digital Transactions 2011). Durbin changes the structure of the market by eliminating much of this dispersion: In 2018, for transactions made with Mastercard debit cards issued by banks above the Durbin threshold, grocers pay $0.22 plus five basis points times the value of the transaction, regardless of their debit volume. Dispersion in the credit interchange market, left unregulated by Durbin, remains, as Appendix Figure A1 shows.

Our detailed dataset allows us to trace out how interchange expense varies across industry and payment instrument. The merchants most helped by Durbin are those without tiered or otherwise low debit interchange rates negotiated ex-ante with card networks. Some merchants actually see their interchange fees rise post-Durbin.[10] This is because small-ticket discounts disappear post-Durbin as the $0.22 debit interchange fee

[10] Interestingly, some merchants had debit interchange rates below the $0.22 cap prior to Durbin: for these merchants, interchange expense *rises* post-regulation. Appendix Figure A1 shows this: for some merchants, regulated rates are higher than unregulated rates.

cap becomes a floor across debit transactions (Digital Transactions 2011, American Banker 2012).[11]

d. Gas price data

Our data on gas prices comes from Oil Price Information Service (OPIS). OPIS contains station-level information on daily pump prices for (1) regular; (2) mid-grade; and (3) premium fuel. It also contains information on retail margins for each category, which it computes based on the difference between the net fuel price (retail price less state, federal, and local taxes and freight) and the wholesale price (the same-day rack price quoted by the nearest wholesale distributor to a particular station).

The gas data is attractive for its granularity: for the ten largest states in the United States (CA, TX, FL, NY, PA, IL, OH, GA, NC, and MI) we have daily data for the six months prior to and six months following the implementation of Durbin (April 2011 – March 2012). In addition to station-level pricing, the data contains station-specific information including: name, street address, ZIP code, latitude/longitude, and brand. Pricing data comes from a mix of sources: exclusive relationships with credit card companies who provide this information directly to OPIS; gas station "fleet card" users; direct feeds from fuel retailers; and a data partnership with GasBuddy, a company that collects user-inputted station pricing information.

OPIS data cover over 65,000 gas stations in our ten-state subsample. However, some stations are available only for a portion of 2011–2012 or have prices that are reported sporadically. As such, we focus on ZIP codes in which OPIS reporting meets the Barrage et al. (2014) "minimum density criteria" and require fuel stations in the sample to have at least three observations per week for the sample period.

e. Other data sources

To study the distributional consequences of Durbin, we also use two surveys conducted by bank regulators: (1) the Survey of Consumer Finances, which contains

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[11] Economically, the disappearance of this discount post-Durbin is surprising. One explanation could be that financial institutions target a certain level of interchange revenue—when they are no longer able to generate it from large merchants, they feel compelled to change pricing structure across merchant categories. Additional theoretical and empirical work on the structure of this market is warranted.

demographic and financial information about consumers, including checking account balances; (2) the FDIC's Survey of Unbanked and Underbanked Households, which contains information on these consumers, including the main reason they are unbanked.

IV. Methodology and results

a. Banks' response to Durbin

i. Intended consequences: bank interchange revenue falls post-Durbin

The summary statistics in Table 1 hint at the regression results to follow. Relative to untreated banks, treated banks experience a significant decrease in interchange revenue. Between Q4 2010 (pre-Durbin) and Q4 2011 (post-Durbin), interchange revenue fell by 26.7 percent for Durbin banks. During this same period, interchange income *increased* by 12.8 percent for banks above the Durbin threshold. This difference is statistically significant at the 1 percent level.

Figure 1 demonstrates the impact of the Durbin Amendment on banks' interchange revenue. The basic univariate results show a significant decline (over $5 billion annually) in interchange revenue for banks above the $10 billion threshold immediately following Durbin's enactment. There is no commensurate decrease for banks below the $10 billion threshold.

In Figure 2, we perform estimate the change in banks' interchange revenue post-Durbin using an event-study approach to observe how this change evolves over time.

Specifically, we estimate:

$$Ln(Y_{i,t}) = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \mathrm{Durbin_i} \times 1[s = t] + \epsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is interchange income, α_i is a bank holding company fixed effect to control for time invariant bank characteristics, and year-quarter fixed effects ϕ_t control for time trends. $Durbin_i$ is an indicator that takes a value of 1 if a bank holding company is above the $10 billion threshold. We cluster our standard errors at the bank holding company level. This is a generalized version of a basic difference-in-differences approach. The log specification allows for us to estimate the percentage change in banks' interchange revenue attributable to Durbin.

The coefficients plotted represent the change in interchange income over our sample period. The omitted category is Q2 2010 (when Durbin is passed). Our identifying assumption is that in the absence of Durbin, interchange revenue of banks above and below the $10 billion threshold would have followed parallel trends. The near-zero and statistically insignificant pre-period estimates suggest that this identifying assumption is valid. Once Durbin is enacted, interchange revenue falls by 40 percentage points.

ii. Unintended consequences: bank account fees

1. Baseline results

In Figure 3, we illustrate the impact of Durbin on free checking ($0 monthly fee, regardless of account size) and monthly minimum fees. Importantly, we see no evidence in these figures of differential trends for large banks (Durbin treatment) relative to small banks (Durbin control) in the pre-Durbin period. These parallel pre-trends give us confidence that the identifying assumption is satisfied, and we can attribute the changes in checking account pricing to Durbin's passage.

We estimate the impact of Durbin and provide more formal support for our parallel trends assumption in Figure 4, using the same event study approach described above. Here, in the quarters prior to and following Durbin's passage we estimate the change in free checking and account fees for branches of banks above, relative to below, the Durbin threshold in a series of quarters relative to Q2 2010 (Durbin's passage). We estimate:

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \text{Durbin}_\text{i} \times 1[s = t] + \epsilon_{i,t} \tag{2}$$

We cluster standard errors at the bank holding company level and include branch (α_i) and time (ϕ_t) fixed effects. We benchmark against Q2 2010 because we hypothesize—and empirically confirm—that at least some banks begin adjusting prices in response to Durbin passage, but before enactment. This anticipation effect explains why in Q3 2011 there is a statistically significant decrease in free checking (or increase in monthly maintenance fee), even though Durbin is not enacted until Q4 2011.

Many more banks respond to Durbin in the immediate aftermath of its enactment: by Q4 2011, Durbin results in a 42 percentage point decrease in free checking. We can precisely rule out an effect on free checking that is smaller than 18 percentage points. Equivalently, monthly maintenance fees, which averaged $3.07 for banks above the Durbin threshold increased by nearly 100 percent because of Durbin's passage.

In Table 4, we also present the results of a basic difference-in-differences approach, where we estimate the following for a broader set of fees and accounts:

$$Y_{i,t} = \alpha_i + \phi_t + \beta_d \times Durbin_i \times Post_t + \epsilon_{i,t} \quad (3)$$

The coefficient of interest β_d can be interpreted as the change in pricing for banks above relative to below the $10 billion threshold attributable to Durbin.

We observe that banks' response to Durbin is concentrated in the basic checking account that prior to regulatory intervention generated more interchange revenue. In addition to a decrease in the availability of free checking and an increase in monthly fees, we also observe a 21% increase in the minimum account balance that must be maintained before banks will waive monthly fees. These results suggest that banks adjust each aspect of the basic checking account's price to recover the revenue that was previously generated by interchange from debit cards attached to this account.

On other accounts, there is some evidence of price adjustment, but it is less pronounced. On interest checking accounts, Durbin banks increase monthly maintenance fees by $1.55 (14 percent). On savings accounts, banks increase withdrawal penalties by $1.40 (39 percent).

2. Impact of competition

We next test whether market power impacts the speed or size of banks' response to Durbin.[12] We again use an event study approach, and in Table 5, we estimate Equation (2) separately for banks located in counties where HHI is above-median (significant market power) and those where HHI is below-median (closer to perfect competition). We fix our

[12] This inquiry is closely related to Drechsler et al. (2018). These authors find that banks exploit their market power to offset the impact of interest rates on profitability. In this paper, we are interested in whether banks' adjustment to a different profitability shock—Durbin—also depends on local market dynamics.

HHI measure in 2008 to avoid any possible endogeneity between Durbin's effect and local market power.

We observe that Durbin banks with pricing power begin to decrease free checking earlier following Durbin's passage (a 25 percentage point decrease by Q1 2011). The decrease by Q1 2011 for banks in more competitive regions is smaller (11 percentage points) and not statistically significant. The difference in fee-setting practices between these groups is significant at the 10 percent level. Monopolistic Durbin banks also adjust more to Durbin—by Q4 2012 free checking decreases by 49 percentage points for Durbin branches in above-median HHI counties, relative to 35 percentage points for their below-median counterparts. This difference is also significant at the 10 percent level.

iii. Bank robustness checks

1. Large vs. small bank trends

One concern with our identification strategy is that it may capture general differences in revenue and pricing for large versus small banks that are independent of Durbin. The passage and enactment of Durbin coincides with a post-crisis overhaul of the financial sector that results in significantly elevated regulatory burdens for all banks, but particularly the largest "too-big-to-fail" financial institutions. If the heightened regulatory burden triggers pricing changes, then we will mistakenly ascribe these to Durbin.

To test for this possibility, we perform a series of robustness checks.

In Table 6, we address this concern by performing our difference-in-differences estimation on different subgroups of banks. In Panel A, we estimate Equation 3 for narrower and narrower subgroups, eventually considering banks with only between $5 and $20 billion in total assets. This final specification is closer in spirit to a regression discontinuity design that would compare banks directly above and below the $10 billion threshold. Although we lose power as our subsample shrinks (there are less than 15 banks in our sample with $5 to $20 billion in assets), the coefficients are nearly identical across all specifications. Unsurprisingly given these results, we can conduct all our bank-level analysis excluding megabanks with more than $100 billion in total assets and find largely similar results.[13]

Next in Table 6, Panel B we conduct a series of placebo tests. We perform our difference-in-differences estimation and compare the availability of free checking at banks above versus below alternate asset thresholds following Durbin's Q2 2010 passage. In many cases, these are regulatory thresholds: for example, $50 billion in total assets was formerly the threshold for Dodd-Frank annual stress-testing; and $5 billion the threshold for longer and more detailed Call Report submission.

If what we label as the "Durbin effect" was instead capturing a general difference in fee-setting practices by large versus small banks in response to a heightened post-crisis regulatory burden for large financial institutions, we would expect to see price differences at alternate asset thresholds. Instead, we observe no statistically significant differences in pricing by banks above and below "placebo Durbin" thresholds, and in many cases the estimated coefficients go in the opposite direction—implying an increase in the availability of free-checking for larger banks at alternative asset cutoffs.

2. Other regulations around $10 billion

Another potential concern with our identification strategy is that Durbin is not the only regulation that kicks in at the $10 billion asset threshold. One may be concerned that we are conflating banks' offsetting Durbin with their offsetting other regulations. This appears unlikely. For example, Dodd-Frank required that banks with more than $10 billion in assets perform annual company-run stress tests and subject these banks to CFPB supervision. Since the first company-run stress tests are not performed until 2013, it seems improbable that the costs associated with them drive the price movements we document in the immediate aftermath of Durbin.

The local nature of the price response to Durbin we observe (primarily in exactly the checking account that pre-Durbin generated significantly more debit interchange revenue) and the fact that banks themselves report plans to respond to Durbin by

[13] Results available in online Appendix.

increasing account fees provide confidence that our results are attributable to debit interchange regulation (Bank of America 2011, TCF 2011).

3. Bunching

Our identification strategy assumes that the Durbin Amendment is a natural experiment which exposes banks to treatment (decrease in interchange revenue) based on an arbitrary asset threshold of $10 billion. If banks strategically avoid this $10 billion threshold during our sample period, this casts doubt on our assumption of exogeneity.

To test for this possibility, we look for strategic manipulation around the $10 billion asset threshold. We implement a variation of the McCrary (2008) test[15] using the local polynomial density estimator of Cattaneo et al. (2019) to estimate the density of the distribution of bank assets around the discontinuity of interest ($10 billion) with quarterly data on bank assets. The goal is to ascertain whether banks are sorting themselves out of treatment to avoid the Durbin hit to their interchange revenue. If they are not systematically sorting, then we expect the density near the cutoff to be continuous. Figure 5 provides the results of our manipulation test using a third-order polynomial. Our empirical results provide no evidence of manipulation in the period surrounding Durbin (2010–2012). The lack of strategic manipulation during our sample period validates our empirical approach.[16]

iv. Quantifying banks' Durbin offset

We next measure the extent to which banks above the $10 billion threshold recover their lost interchange revenue through these higher account fees. To do so, we rely on

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[15] McCrary (2008) proposes a density test to validate regression discontinuity (RD) designs, but as Cattaneo et al. (2017) note, the general principle applies to a wide array of questions regarding self-selection around a boundary point including our setting. We prefer the discontinuity test based on the density estimator in Cattaneo et. al. (2019) over the original approach taken in McCrary (2008) based on the local polynomial density estimator of Cheng et al. (1997) as it does not require the choice of many additional tuning parameters.

[16] Interestingly, when we expand outside of our sample period we find a large and statistically significant gap in bank assets immediately above the $10 billion threshold (t-statistic of -4.22, Panel B of Table 5). This is consistent with work by Ballew et al. (2017) who find that in recent years banks below the $10 billion threshold are less likely to engage in acquisitions.

bank regulatory data. This is because RateWatch does contains only information on prices, and so does not allow for quantification of the overall Durbin offset.

Specifically, in Table 7 we estimate

$$\text{Ln}(Y_{i,t}) = \alpha_i + \phi_t + \beta_d \times Durbin_i \times Post_t + \gamma \times \text{Ln}\,(Deposits_{i,t}) + \epsilon_{i,t} \tag{4}$$

at the bank level for two outcome variables: interchange income and service charges on deposits. The latter variable is broader than just monthly maintenance fees, and captures all fees associated with deposit accounts, such as overdraft fees and check cashing fees. For this reason, much of our prior analysis uses RateWatch's data exclusively on account fees.

Importantly, as the summary statistics reported in Table 1 reflect, Durbin coincides with a shift by depositors towards larger banks: during our sample period, Durbin banks grow faster than their non-Durbin counterparts: between Q4 2010 and Q4 2011 deposits grew by 8.3 percent at Durbin banks relative to 2.8 percent at non-Durbin banks. This difference is significant at the 1 percent level. As such, in Table 7 we control for contemporaneous deposit growth when estimating Durbin's impact on fee revenue. Failing to do so will conflate an increase in deposits—which mechanically increase deposit fees—with Durbin's effect. Our use of a log specification allows us to interpret the coefficients as the percentage change in interchange and deposit fee revenue attributable to Durbin.

Interchange income falls by 31% for banks above the Durbin threshold, and deposit fees increase by 7%. This implies that banks above the $10 billion threshold lose $5.5 billion annually in interchange revenue because of the regulation, and they recover $2.3 billion of these losses (42 percent) through higher account fees.

v. What economic model explains bank response?

In some sense, banks' increasing checking account fees post-Durbin is unsurprising, because they cautioned exactly this response. For example, TCF, who challenged the constitutionality of Durbin to the Supreme Court warned "who is going to pay for this? That customer that gets that debit card for free." Interestingly, the Federal Reserve's argument in response to TCF relied on the ability of large banks to adjust other checking account prices to cover interchange losses. So, the price response to Durbin that we document was contemplated by both banks and regulators ex-ante.

But economically, this price adjustment is a priori puzzling. If profit-maximizing banks could generate additional income from consumers by eliminating free checking, it would have been optimal to do so even in the absence of this price regulation.

One explanation is that the bank checking account is bundle with several prices. What matters from the perspective of the bank is that the aggregate price inclusive of all aspects of the bundle is above marginal cost. In the pre-Durbin era, banks generated revenue from checking account customers either through account fees or interchange fees from debit payments. Because interchange fees were such a significant source of revenue, from the banks' perspective, it was profitable to fully subsidize checking accounts. Durbin restricted banks' ability to generate revenue from interchange fees, so in a competitive market the other bundle price (monthly fees) must adjust.

The bundled nature of checking account prices helps us understand why banks' response to Durbin is local, and fees on savings accounts and on money market accounts are largely unchanged (Table 2). Specifically, post-Durbin, it is no longer profitable for large banks to subsidize free checking for consumers who generate revenue primarily through now paltry debit interchange.[17] Indeed, we find suggestive evidence for a growth in the un- and under-banked individuals who leave the traditional financial sector because of prohibitively high account prices (Table 11).

b. How do merchants adjust to Durbin?

We next study the extent to which merchants pass through their interchange savings from Durbin. Since Durbin-induced savings are related to consumer payment choice, the share of customers who bank at covered entities, and the pre-Durbin interchange rate merchants face; Durbin's impact is unevenly distributed across the ZIP codes in our sample. This variation motivates our empirical analysis.

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[17] Indeed, banks immediate response to Durbin was to levy a $5 monthly fee only on consumers who used their debit cards as a means of transacting to make up interchange losses directly. Only after substantial consumer outcry did banks' abandon this fee: Bank of America debit cards were being burned in the street, and Vice President Joe Biden called the fee "incredibly tone-deaf" (Terkel 2019).

i. Calculating merchants' Durbin impact

To capture the Durbin impact, we compare what interchange fees *would have been* in the absence of Durbin to what interchange fees *are* post-regulation. We calculate a ZIP code-by-industry's Durbin-induced change in interchange fees as:

$$\Delta InterchangeFees_{z,i} = Fees_{z,i}^{PostDurbin} - Fees_{z,i}^{PreDurbin} = Value_{z,i}^{reg} \times \left(IC_{z,i}^{reg} - IC_{z,i}^{unreg}\right)$$

where z is a ZIP code, i is an industry, $IC_{z,i}^{reg}$ and $IC_{z,i}^{unreg}$ are the per dollar interchange rates for regulated debit and unregulated debit, respectively; $Value_{z,i}^{reg}$ is the dollar value of transactions made with regulated debit. [18,19]

We then scale the absolute dollar Durbin-induced reduction in interchange fees by the total level of card transactions in a ZIP code for that industry and define:

$$Impact_{z,i} = \frac{-\Delta InterchangeFees_{z,i}}{Value_{z,i}^{reg} + Value_{z,i}^{unreg} + Value_{z,i}^{cr}}$$

Unsurprisingly, this measure is highly correlated with a more naïve estimate of Durbin exposure, for example by considering the share of bank branches (or the share of bank deposits) in a ZIP code associated with banks above and below the Durbin threshold. We prefer our interchange-based measure of Durbin savings because it allows us to observe exactly how a merchant's cost dynamics evolve post-Durbin. For example, if 50 percent of a ZIP code's bank branches are above-Durbin banks, but none of the customers of these branches use their debit cards to transact, then the more naïve estimate will overstate Durbin's importance. Additionally, our *Impact* measure correctly accounts for the fact that customers may reside and shop in regions outside their banks' ZIP codes.

[18] Total interchange fees include regulated and unregulated debit interchange fees as well as credit interchange fees. However, since credit and unregulated debit interchange fees are identical pre- and post-regulation, the Durbin-induced change in interchange fees is driven entirely by the reduction in regulated debit interchange rates.

[19] Due to limitations on the availability of historical data, our interchange data runs from 2014–2016. However, our data provider made available historical interchange rate bulletins that show that interchange rates between 2009–2018 changed only for regulated debit in response to Durbin. Regulated debit rates adjust to exactly the $0.22 cap (Appendix Figure A1).

ii. How does Durbin change retail gas margins?

To estimate Durbin's impact on retail prices, we focus on the gas industry for several reasons. First, of the 120 industries in our dataset, gas retailers save the most as a result of Durbin—over 15 percent of total savings across all industries. Second, gas stations set prices locally; unlike many retailers, for example grocery and drug stores price uniformly across locations (Gentzkow and DellaVigna 2019). This means that we will be able to test whether interchange savings for gas retailers within a ZIP code translate to lower gas prices for customers in that ZIP code. Additionally, gas offers standardized products, facilitating price comparisons. Our main outcome variable of interest is:

$$\Delta Margin_{f,z} = \overline{Margin_{\mathrm{f,z,post}}} - \overline{Margin_{\mathrm{f,z,pre}}}$$

A station's retail margin is its retail price in excess of all applicable state and local taxes, freight costs, and wholesale price. For each fuel station (f) within a ZIP code (z), the pre- and post-Durbin margins are averaged from the daily data over six months pre- and post- the Durbin Amendment's enactment on October 1, 2011.

Regions with high Durbin impact, where consumers tend to use debit cards issued by large banks, may be different from low-impact regions. For that reason, we employ three different matching techniques to identify the effect of Durbin from comparable treatment and control groups. First, since ZIP codes located within close geographic proximity are more like regions and are vulnerable to similar shocks, we examine if local variation in Durbin impact translates to local variation in retail margins. Second, we use the propensity score matching approach (Rosenbaum and Rubin 1983, 1984) to directly account for the difference in observable characteristics between high- and low-impact ZIP codes. Third, we assemble the control group using the synthetic controls method (Abadie and Gardeazabal 2003). All three approaches yield similar results; we discuss each in turn.

The address of each gas station in our data is geocoded (i.e., contains the latitude and longitude), which allows us to calculate the distance between any two stations. First, we construct all possible pairs of gas stations 1) located within 20 miles from each other

2) in different ZIP codes.[20] Second, we collapse these data at the pair of ZIP codes level. We can now evaluate if the difference in Durbin impact among neighboring ZIP codes affects retail margins by estimating:

$$\Delta Margin_{z1} - \Delta Margin_{z2} = \beta \times (Impact_{z1} - Impact_{z2}) + \gamma \times (\theta_{z1} - \theta_{z2}) + \epsilon \qquad (5)$$

Column 1 of Table 8 shows that nearby ZIP codes with higher Durbin impact, in fact, experience a bigger reduction in margins following Durbin, statistically significant at the 1 percent level. In particular, a one standard deviation increase in the disparity of *Impact* between ZIP codes in a pair lowers the margins in a higher impact ZIP code by an additional 0.1 cents.[21] After controlling for the difference in paired ZIP codes' characteristics, the estimate's absolute value drops only slightly from 0.697 to 0.564 in Column 2. These results are consistent with gas retailers passing through some of Durbin-induced savings to consumers through lower prices.

Columns 5 and 6 of Table 8 illustrate the importance of accounting for the endogenous determination of Durbin impact. Compared to poorer rural areas, richer urban ZIP codes exhibit upward pressure on gas margins. Having more presence by large banks, they have also been subject to stronger Durbin impact. Therefore, Column 5 shows a strong *positive* univariate relation between *Impact* and *Δ Margin* at the ZIP code-level; controlling for the ZIP code characteristics in Column 6 reduces the point estimate's magnitude and renders it statistically insignificant. Drawing inference from pairs of nearby—hence, similar in demographic and macro terms—ZIP codes in Columns 1–4 shields the estimates from the endogeneity bias.

In Columns 3 and 4 of Table 8, we investigate how this effect evolves depending on the proximity of the ZIP codes considered. Specifically, we construct dummy variables that take a value of 1 when ZIP codes are within a one mile radius; within one to two miles; within two to five miles; within five to ten miles; within ten to fifteen miles; and within fifteen to twenty miles. We interact these dummies with changes in impact between the ZIP codes in a pair, thus estimating:

[20] Since our *Impact* measure is defined at the ZIP-level, we can draw inference only by comparing pricing at gas stations located in different ZIP codes.
[21] 0.0012 x 0.697 ≈ 0.001

$$\Delta Margin_{z1} - \Delta Margin_{z2} =$$

$$\beta_{0-1} \times (Impact_{z1} - Impact_{z2}) \times \mathbb{I}_{\text{dis(z1,z2)}\in(0,1]} +$$

$$\beta_{1-2} \times (Impact_{z1} - Impact_{z2}) \times \mathbb{I}_{\text{dis(z1,z2)}\in(1,2]} +$$

$$\ldots +$$

$$\beta_{15-20} \times (Impact_{z1} - Impact_{z2}) \times \mathbb{I}_{\text{dis(z1,z2)}\in(15,20]} + \gamma \times (\theta_{z1} - \theta_{z2}) + \epsilon$$

We find that for ZIPs that are immediate neighbors (i.e., within two miles of each other), no statistically significant relation exists between *Impact* and the change of margins. This is unsurprising, because ZIP codes located that close to each other possess almost identical *Impact* scores. In contrast, further apart ZIP codes feature disparity in *Impact* with variation sufficient to estimate a statistically significant relation. These results hint at the possibility that the documented price effect is concentrated in ZIP code pairs with sizeable differences in *Impact*. We test this hypothesis in Panel B of Table 8.

Specifically, we sort pairs of ZIP codes—within 20 miles from each other—into deciles based on the absolute value of differences in their level of Durbin *Impact*. The top decile includes pairs who are geographically proximate but whose retailers have large differences in Durbin exposure because of their customers' predisposition to pay with debit cards issued by large banks. For ZIP code pairs within this decile, a one standard deviation increase in a pair's *Impact* disparity lowers the margins in a higher impact ZIP code by an additional 0.3 cents,[22] an economically meaningful number representing about 10% of the dependent variable's standard deviation. Outside of the top decile, the impact of Durbin on retailers' margins shrinks and is no longer statistically significant.

Next, in Table 9 we estimate the extent to which retailers pass-through Durbin savings using the propensity score matching (PSM) method (Rosenbaum and Rubin 1983, 1984). The control group comprises ZIP codes in the bottom (1st) decile of the *Impact* distribution, since these ZIP codes experienced a negligible reduction in interchange expense post-Durbin. The treatment group ranges from the top (10th) decile in Column 1 to the 2nd decile in Column 9. First, for each column (i.e., treatment group decile) we run a probit regression of the treatment status on ZIP code characteristics to estimate

[22] 0.0012 x 2.516 ≈ 0.003

the probability of a ZIP code being exposed to Durbin.[23] We then use these predicted probabilities (i.e., propensity scores) to match treatment and control group ZIP codes and ascertain whether the treatment group areas experience a downward pressure on margins vis-à-vis the control group in the aftermath of the interchange fee regulation's enactment.

The PSM results presented in Table 9 show that only the most exposed ZIP codes (i.e., the top decile of the Impact distribution) exhibit a reduction in margins post-Durbin by about 0.6 cents, statistically significant at the 1 percent level. The average value of *Impact* in this decile equals 0.006; in other words, top decile gas retailers experience a reduction in interchange fees equal to 0.6 percent of their total card revenue. Given the average price of gas in this decile equal to $3.6 and assuming cash transactions amount to 15 percent of total sales, full pass-through would have lowered gas prices by 1.8 cents[24] in this decile. As such, our estimates are consistent with the most generously affected by Durbin gas stations passing through only a third of their savings to consumers through lower retail prices. Considering the point estimate's standard error (0.00247), we may rule out a pass-through of more than 60 percent[25] among the top decile of Durbin savers at the 95 percent confidence level.

Repeating the same exercise across all treatment deciles, assuming for simplicity that point estimates in Columns 1–9 are pairwise independent, we can (at the 95 percent confidence level) rule out a pass-through of more than 28 percent from merchant to consumers.

We also study the importance of competitive forces for Durbin's price pass-through. Specifically, for the only subgroup with significant pass-through—the top decile of impacted ZIP codes—we separately consider gas stations facing stronger and weaker competition. We quantify competition by counting the number of other gas stations within concentric circles of varying radiuses around a given station. Fuel stations surrounded by an above-median number of competitors face high competition.

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[23] Average household income, area, population density, and fuel station density.
[24] $3.6 x 0.6% x (1–15%) ≈ 0.018
[25] (0.006 + 1.96 x 0.00247) / 0.018 ≈ 0.60

Table 10 reports the PSM analysis side-by-side for gas station facing high and low competition. The results are consistent with the notion that local market competition exerts pressure on gas stations to pass through Durbin-induced savings to consumers. The point estimates in high-competition subsamples are of greater absolute magnitudes and showcase stronger statistical significance.

iii. Synthetic control analysis to capture merchants' response

For additional robustness, we use the synthetic control method of Abadie and Gardeazabal (2003), an approach regarded as "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey and Imbens 2017). For each gas station exposed to Durbin, we assemble a linear combination of unexposed stations, in such a way that the treated station and the linear combination (i.e., a synthetic match) follow a similar path in the outcome variable of interest prior to Durbin's enactment. In contrast to the PSM method, which matches observations in the treatment and control groups based on the covariates, the synthetic control method requires that observations in the two groups behave similarly in the trajectory of weekly retail gas margins prior to Durbin. The effect of Durbin on retail margins can thus be measured by the difference between retail margins in treatment versus synthetic control stations. This approach controls for the effect of unobservable factors (Abadie, Diamond, and Hainmueller 2010).

The synthetic control method was designed for the case of a single treated unit. However, recent papers, including Acemoglu et al. (2016), extend this approach for situations with multiple treatment units, and we follow their methodology. We designate the gas stations in the top decile of *Impact* as the treatment group; the bottom decile stations form the set of donors from which synthetic matches are constructed. We construct a synthetic match for each gas station in the treatment group by solving the following optimization problem:

$$\forall i \in \text{Treatment group}, \left\{w_j^{i*}\right\}_{j \in \text{Control group}} = \underset{\left\{w_j^i\right\}_{j \in \text{Control group}}}{\arg\min} \sum_{t \in \text{Estimation window}} \left[M_{it} - \sum_{j \in \text{Control group}} w_j^i M_{jt} \right]^2$$

$$\text{s.t.} \sum_{j \in \text{Control group}} w_j^i = 1 \quad \text{and } \forall j \in \text{Control group}, \forall i \in \text{Treatment group} \ \ w_j^i \geq 0$$

where M_{it} is the margin charged by the gas station i on week t and w_j^i is the weight of control gas station *j* in the optimal weighting for gas station *i*. Similar to Acemoglu et al. (2016), we terminate the estimation window at a period prior to the intervention: Four weeks right before Durbin's enactment are excluded from the estimation window.

Next, for each gas station in the treatment group, we obtain the margin time series for the synthetic gas station using the formula:

$$\widehat{M_{\iota t}} = \sum_{j \in \text{Control group}} w_j^i M_{it}$$

and estimate the effect of intervention:

$$\hat{\phi}(k) = \frac{\sum_{i \in \text{Treatment group}} \frac{\sum_{t=1}^{k} M_{it} - \widehat{M_{\iota t}}}{\widehat{\sigma_\iota}}}{\sum_{i \in \text{Treatment group}} 1/\widehat{\sigma_\iota}}$$

where

$$\widehat{\sigma_\iota} = \sqrt{\frac{\sum_{t \in \text{Estimation window}} \left[M_{it} - \widehat{M_{\iota t}}\right]^2}{T}}$$

Our gas data spans 26 weeks before Durbin's enactment on October 1, 2011 and 26 weeks after. Estimation window includes 22 weeks (i.e., $T = 22$), because 4 weeks in the run up to intervention are excluded. As in Acemoglu et al. (2016), the effect of intervention is a weighted average, with greater weights assigned to better matches in the estimation window.

Figure 6 plots $\hat{\phi}(k)$ for each of the 26 weeks following Durbin's enactment. Since it is a cumulative measure (i.e., additive with time), one must divide $\hat{\phi}(k)$ by k to capture the estimated effect on treated stations' gas margins over.k weeks post-intervention. $\hat{\phi}(26) = -0.168$ implies that fuel stations in the top decile of *Impact* exhibit a 0.6 cents reduction in margins. This estimate is virtually identical to the one obtained using the PSM approach.

To apply confidence bounds to this number, we effectively bootstrap standard errors. We randomly assign pseudo-treatment to some stations in the control group and repeat the above procedure for each pseudo-treatment group to estimate a placebo intervention effect $\hat{\phi}(k)$. We do so 100 times and plot the resulting placebo curves in Figure 6 along side the actual intervention effect. As of week 26, only 3 placebo curves lie beyond the actual intervention curve, suggesting that the 0.6 cents margin reduction is statistically significant at an almost 5 percent level. With a 95 percent confidence level, over 6 months after Durbin's enactment, we can reject a pass-through of more than 72 percent[27] by the most affected gas stations; the corresponding PSM upper bound reported above is 60 percent.

Interestingly, Figure 6 also shows that the effect tapers off after approximately 10 weeks post-intervention. As of week 10, treated gas stations have cut margins by 1 cent, statistically significant at the 1% level.

iv. What economic model explains asymmetric retailer pass-through?

Our results suggest that gas retailers who are most heavily impacted by Durbin pass through savings to consumers. However, less impacted retailers do not adjust prices. What explains this asymmetry?

One explanation is that retailers set prices based on "rule-of-thumb" pricing equations. They price based on some standard mark-up over their costs, including wholesale costs, transport costs, taxes, and interchange expense (Amato and Laubach 2003, Zbaracki 2004). Until there is a significant shock to one of these pricing dimensions, the pricing equation is not updated. This possibility is consistent with survey evidence about the Durbin Amendment, which reveals that in the aftermath of its enactment many merchants do not realize that their interchange expense has declined (Wang et al. 2014).

[27] Standard deviation of placebo estimates as of week 26 equals 0.084. As such, the lower bound of a 95% confidence interval is 0.168 + 1.96 x 0.084 = 0.333 which, after scaling by 26, turns into 1.3 cents and accounts for 72% of the full theoretical pass-through of 1.8 cents in the top decile.

However, in ZIP codes where Durbin looms large—where interchange expense falls significantly—retailers do revise prices quickly. In the gasoline sector specifically, empirical evidence suggests that rule-of-thumb pricing exists and helps to explain sluggish price movement in response to cost shocks (Alvarez et al. 2006).

A related possibility is that incomplete and slow price adjustment to Durbin is consistent with the tendency of retail prices to rise faster in response to merchants' costs increasing than they fall in response to merchants' costs decreasing. This is known as the "rockets and feathers" phenomenon and is well-documented across industries (Peltzman 2000). For example, gas prices increase quickly in response to positive wholesale shocks, but they take time to fall in response to wholesale costs decreasing (Borenstein et al. 1997; Deltas 2008; Owyang and Vermann 2014). And although when the cost of funds rises banks quickly increase interest rates for borrowers, they are slow to raise the rates they pay depositors (Hannan and Berger 1991). Durbin decreases interchange costs for merchants, so, consistent with "rockets and feathers" pricing, this decrease is not immediately and fully passed through to consumers.

Theoretically, the asymmetric response of retailers to cost increases versus cost decreases is made possible by imperfectly competitive retail markers. When costs fall, oligopolists exploit market power to earn positive profits—at least in the short-run, before these are competed away. This is why the "rockets and feathers" phenomenon is most pronounced in concentrated markets (Knittel 2017): In more competitive markets, retailers are pressured to immediately pass through cost savings to consumers, as our evidence on Durbin suggests (Table 10).[29]

V. Suggestive evidence on Durbin's distributional consequences

a. Low income consumers bear incidence of new fees

In response to Durbin, basic checking account fees nearly double. However, these fees are borne only by customers who do not maintain a minimum balance high enough

[29] Whereas banks in highly competitive regions pass through Durbin *losses* less and adjust more slowly (Table 5). Overall, our results confirm that firms with market power are more easily able to extract rents from regulatory intervention aimed at increasing consumer surplus.

to avoid them.[30] To try and understand the size and incidence of the bank response to Durbin, we turn to the Federal Reserve Board's 2010 Survey of Consumer Finances, which contains detailed demographic and financial information about individuals, including annual income, checking and savings account balances, and mortgage information.[31]

Unsurprisingly, the data illustrates that wealthier households have higher average account balances. This means that new account fees associated with Durbin are borne primarily by low-income consumers. Specifically, as Appendix Figure A4 illustrates, over 70 percent of consumers in the lowest income quintile (annual household income of $22,500 or less) bear higher account fees, since they fall below the average post-Durbin account minimum required to avoid a monthly maintenance fee ($1,400). In contrast, only 5 percent of consumers in the highest income quintile (household income of $157,000 or more) fall below this threshold.

b. Potential impact on the unbanked

Nearly 8 percent of Americans were unbanked in 2013, with nearly 10 percent of this group becoming unbanked in the last year. Using data from the FDIC National Survey of Unbanked and Underbanked Households, in Table 11 we show that immediately following Durbin there is a significant growth (81 percent increase relative to survey pre-Durbin) in the share of the unbanked population that credits high account fees as the main reason for their not having a bank account. This difference is significant at the 1 percent level.

Respondents in states most impacted by Durbin (those with the highest share of deposits at banks above the $10 billion threshold) are most likely to attribute their unbanked status post-Durbin to high fees (over 15 percent of those surveyed in the highest Durbin tercile). The growth in the recently unbanked (those who had accounts previously but closed them within the last year) is also highest in states with the most Durbin banks, where the increase in account fees is most pronounced. As with the overall sample, these

[30] In some cases, monthly fees can also be avoided by customers who receive regular direct deposits into their checking account, e.g. from an employer.
[31] Unfortunately, this information is not bank-specific, so we are not able to observe, for example, the differences in average checking account size for large bank versus small bank customers.

differences are significant at the 1 percent level. This suggests that at least some bank customers respond to Durbin fee increases by severing their banking relationship and potentially turning to more expensive alternative financial services providers such as payday lenders and check-cashing facilities. Further work on the distributional consequences of Durbin is warranted.

VI. Conclusion

The conventional view is that the existence of prices higher than costs is indicative of a market failure that regulation can usefully address. This belief motivated regulators to adopt the Durbin Amendment, capping debit interchange fees in hopes of increasing consumer welfare.

We provide a quantitative analysis of the incidence of the Durbin Amendment. We find that cost-based regulation on one side of the market is offset by increases in unregulated prices on the other side. Specifically, banks double account fees to recover 42 percent of Durbin losses. Merchants who benefit pass through at most 28 percent of their savings, and perversely regulation increases their economic rents. Overall, Durbin results in nearly a billion dollar annual transfer from consumers to merchants. These findings demonstrate that regulating two-sided markets is not as straightforward as the conventional view would suggest. Our evidence adds empirical support to a long-standing concern in the theoretical literature that market failures in two-sided markets are difficult to regulate successfully (Rochet and Tirole 2003).

Our paper complements important work on the CARD Act, a contemporaneous regulation also in the payments market (Agarwal et al. 2015). Read together, the evidence suggests that price regulation benefits consumers when it targets rents that accrue when firms take advantage of consumers' behavioral limitations. As the Durbin Amendment illustrates, in the absence of such exploitation, traditional whac-a-mole concerns loom large.

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Table 1: Descriptive Statistics for Call Report Data

This table compares banks holding companies above and below the $10 billion Durbin threshold in 2010 Q4 (pre-Durbin) and 2011 Q4 (immediately post-Durbin). Log differences relative to Q4 2010 are reported in two columns on the right. The data are quarterly from the Call Reports and cover bank holding companies with more than $500 million in total assets.

	Treated ($)		Untreated ($)		11Q4 vs 10Q4 (%)	
	2010 Q4	**2011 Q4**	**2010 Q4**	**2011 Q4**	**Treated**	**Untreated**
Interchange income	133,146	109,804	679	719	–26.70	12.78 ***
	(311,483)	(255,289)	(1,446)	(1,458)	(47.12)	(37.85)
Deposit fees	131,559	137,661	1,640	1,608	0.23	–3.63
	(281,183)	(298,955)	(4,099)	(4,112)	(12.56)	(18.45)
Assets	221,950,131	225,954,719	1,678,170	1,723,664	5.57	2.24 **
	(537,365,248)	(537,956,864)	(1,747,534)	(1,810,433)	(14.14)	(7.75)
Deposits	98,754,298	108,567,839	1,335,878	1,381,009	8.26	2.77 ***
	(198,388,144)	(218,426,304)	(1,336,403)	(1,396,394)	(13.80)	(7.85)
Observations					46	405

Table 2: Descriptive Statistics for RateWatch Data

This table compares branches of bank holding companies above and below the $10 billion Durbin threshold in 2010 Q4 (pre-Durbin) and 2011 Q4 (immediately post-Durbin). Differences relative to Q4 2010 are reported in two columns on the right. The data are quarterly from RateWatch and cover bank holding companies with more than $500 million in total assets.

Figures in $ unless otherwise indicated.	Treated		Untreated		11Q4 vs 10Q4	
	2010 Q4	2011 Q4	2010 Q4	2011 Q4	Treated	Untreated
Basic checking account						
Free account (%)	58.09	28.98	62.10	65.78	–28.12	3.25 ***
Monthly fee	3.95	6.15	2.36	2.22	2.33	–0.11 ***
Monthly min to avoid fee	1,308.75	1,383.94	1,251.43	1,175.18	148.37	1.28 ***
Observations					1,561	1,939
Interest checking account						
Free account (%)	1.80	2.29	4.13	4.68	0.76	0.82
Monthly fee	15.12	15.67	8.81	8.83	0.73	–0.05 ***
Monthly min to avoid fee	369.21	248.99	457.91	440.34	–119.03	–4.75 ***
Observations					1,573	1,941
Savings account						
Free account (%)	5.46	5.47	15.62	14.51	0.06	–0.46
Monthly fee	4.43	4.45	3.18	3.22	0.02	0.05
Monthly min to avoid fee	307.54	307.38	193.65	193.37	9.03	0.76
Withdrawal fee	3.70	6.44	2.31	2.56	1.70	–0.33 ***
Observations					1,577	1,937
Money market account						
Free account (%)	8.12	6.16	9.15	8.79	–1.47	–0.10 **
Monthly fee	9.81	11.09	9.15	9.55	1.14	–0.41 ***
Monthly min	2,530.98	3,380.37	2,284.46	2,288.57	854.10	–96.67 ***
Withdrawal fee	9.36	9.99	6.36	6.69	0.73	0.36 ***
Observations					1,566	1,937

Table 3: Sample interchange data

This table shows samples of the interchange provided by a leading payments industry player. The data is aggregated and anonymized at the ZIP Code level and includes information on the total volume of regulated cards (issued by a bank above $10 billion Durbin threshold) and unregulated cards (issued by a bank below the $10 billion threshold), as well as the number of transactions and the interchange fees collected. For gas retailers, data is also provided on credit card volumes and interchange rates. These data cover over 75 percent of ZIP Codes in the United States.

Panel A: Regulated Debit Data

Regulated Debit (Bank Issuer Over $10 billion) ZIP Code XXXXX			
Volume	Transactions	IC Fees	Rate
$50,841,211.40	955,612	$235,231.15	0.46%

Panel B: Unregulated Debit Data

Unregulated Debit (Bank Issuer Under $10 billion) ZIP Code XXXXX			
Volume	Transactions	IC Fees	Rate
$59,346,844.59	1,118,540	$1,124,299.04	1.89%

Panel C: Credit Data

Credit ZIP Code XXXXX			
Volume	Transactions	IC Fees	Rate
$100,365,522.25	1,539,349	$3,081,221.53	3.07%

Table 4: Difference-in-Differences: Impact of Durbin on Bank Fees

This table examines the effect of the Durbin Amendment on bank account fees. The results are from the following difference-in-differences regression that compares fee-setting practices of banks above and below the $10 billion Durbin threshold, reported separately for basic checking, interest checking, savings, and money market accounts.

$$Y_{i,t} = \alpha_i + \phi_t + \beta_d \times Durbin_i \times Post_t + \epsilon_{i,t}.$$

Durbin is an indicator that takes a value of 1 for banks above the $10 billion threshold. *Post* is an indicator that takes a value of 1 in Q4 2011 (Durbin's enactment) and all quarters thereafter. Coefficients on $Durbin \times Post$ indicators are reported, and column name includes dependent variable in each model. The data are quarterly from RateWatch and cover bank holding companies with more than $500 million in total assets. Standard errors are clustered at the bank holding company level. Branch and time fixed effects are included. Estimates are all are dollar values, except for "free" which is binary, with value 1 if branches offer $0 fee accounts to customers, regardless of account size.

	Fee (1)	Free (2)	Minimum (3)	Withdrawal (4)
Basic checking				
Durbin × Post	2.85***	–0.33***	270.60**	
	(0.75)	(0.10)	(130.40)	
Q2 2010 average	3.07	0.61	1,267.30	
Observations	65,366	65,366	32,068	
Interest checking				
Durbin × Post	1.55**	–0.01	–85.95	
	(0.79)	(0.01)	(181.40)	
Q2 2010 average	11.42	0.03	433.40	
Observations	65,514	65,514	65,266	
Savings				
Durbin × Post	1.27	0.01	–4.56	1.40 ***
	(0.91)	(0.03)	(10.33)	(0.47)
Q2 2010 average	3.76	0.106	246.00	3.58
Observations	65,540	65,540	60,806	12,625
Money market				
Durbin × Post	0.36	–0.01	983.00	–0.34
	(0.59)	(0.01)	(1,006.90)	(0.66)
Q2 2010 average	9.76	0.08	2,489.30	7.85
Observations	65,375	65,375	62,047	65,307
Branch FE	X	X	X	X
Year-Quarter FE	X	X	X	X

Table 5: Market Power and Impact of Durbin on Bank Fees

This table examines how market concentration impacts the effect of the Durbin Amendment on the availability of free checking. Columns 1-3 present the results from the following event study regression, reported separately for all bank branches, branches in competitive regions (below-median county Herfindahl index), and branches in less competitive regions (above-median county Herfindahl index).

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \text{Durbin}_\text{i} \times 1[s = t] + \epsilon_{i,t}$$

Durbin is an indicator that takes a value of 1 for banks above the $10 billion threshold. The omitted category is Q2 2010 (Durbin's passage). Coefficients on $Durbin \times Time$ indicators are reported, and column names indicate the relevant samples. To ascertain the magnitude and significance of differences between the above and below median HHI results, Column 4 presents a triple-difference event study regression:

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \text{Durbin}_\text{i} \times 1[s = t] + \sum_{s \neq 10Q2} \beta_h \times HHI_i^{High} \times 1[s = t]$$
$$+ \sum_{s \neq 10Q2} \beta_{s,h} \times \text{Durbin}_\text{i} \times HHI_i^{High} \times 1[s = t] + \epsilon_{i,t}$$

where HHI^{High} is a binary variable that takes a value of 1 for branches with above median HHI. The coefficient on the triple interaction, $\beta_{s,h}$, is reported. The data are quarterly from RateWatch and cover bank holding companies with over $500 million in total assets. Standard errors are clustered at the bank holding company level. Branch and time fixed effects are included.

	All banks		Below median HHI		Above median HHI		Diff. b/w (2) & (3)	
	(1)		(2)		(3)		(4)	
	Pt. Est.	St. Errors	Pt. Est.	St. Errors	Pt. Est.	St. Errors	Pt. Est.	St. Errors
Pre-Passage								
2009 Q1	–0.06	0.05	–0.02	0.05	–0.10	0.06	–0.05	0.05
2009 Q2	–0.04	0.05	–0.03	0.06	–0.05	0.06	–0.06	0.06
2009 Q3	–0.02	0.03	–0.02	0.03	–0.02	0.03	–0.04	0.04
2009 Q4	–0.00	0.02	–0.00	0.02	–0.01	0.03	–0.03	0.03
2010 Q1	–0.06	0.04	–0.04	0.04	–0.07	0.05	–0.06	0.05
Post-Passage								
2010 Q3	–0.03	0.02	–0.00	0.02	–0.05	0.03	–0.01	0.05
2010 Q4	–0.06	0.05	–0.03	0.05	–0.10	0.06	–0.02	0.04
2011 Q1	–0.18**	0.09	–0.11	0.07	–0.25**	0.12	–0.10*	0.06
2011 Q2	–0.21**	0.09	–0.16**	0.08	–0.27**	0.11	–0.07	0.07
2011 Q3	–0.32***	0.11	–0.25**	0.10	–0.38***	0.12	–0.09*	0.05
Post-Enactment								
2011 Q4	–0.40***	0.12	–0.34***	0.11	–0.45***	0.13	–0.06	0.05
2012 Q1	–0.42***	0.13	–0.36***	0.12	–0.47***	0.14	–0.07	0.05
2012 Q2	–0.43***	0.13	–0.37***	0.12	–0.49***	0.14	–0.07	0.05
2012 Q3	–0.43***	0.13	–0.35***	0.13	–0.49***	0.14	–0.09*	0.05
2012 Q4	–0.43***	0.13	–0.35***	0.12	–0.49***	0.14	–0.09*	0.05
Branch FE		X		X		X		X
Time FE		X		X		X		X
Observations		80,889		40,338		40,528		80,889
R-squared		0.658		0.679		0.667		0.659

Table 6: Difference-in-Differences Subgroup and Placebo Results: Impact of Durbin on Bank Fees

This table examines the effect of the Durbin Amendment on bank account fees. Panel A documents the effect of the Durbin Amendment on the availability of free checking in smaller samples that narrow in on the $10 billion cutoff. Panel B documents the effect of "placebo" Durbin Amendments, at hypothetical cutoffs that are unrelated to interchange regulation—but do implicate other regulatory thresholds.

The results are from the following difference-in-differences regression that compares changes in the availability of free checking accounts for banks above and below various thresholds, reported in the column name.

$$Y_{i,t} = \alpha_i + \phi_t + \beta_d \times Threshold_i \times Post_t + \epsilon_{i,t}.$$

Post is an indicator that takes a value of 1 in Q4 2011 (Durbin's enactment) and all quarters thereafter. *Threshold* is an indicator that takes a value of 1 for different asset thresholds, described in the column name. Coefficients on $Threshold \times Post$ indicators are reported, and column name includes dependent variable in each model. The data are quarterly from RateWatch and cover bank holding companies with more than $500 million in total assets. Standard errors are clustered at the bank holding company level. Branch and time fixed effects are included.

Panel A: Durbin subsamples around the $10B threshold

	Banks > $500M	**$5-75B**	**$5-20B**
	(1)	(2)	(3)
Durbin × Post	–0.33***	–0.29*	–0.28
	(0.10)	(0.15)	(0.19)
Branch FE	X	X	X
Time FE	X	X	X
Observations	65,370	12,595	8,623
R-squared	0.664	0.645	0.642

Panel B: Placebo Durbin (at different thresholds)

	Banks > $15B			**Banks < $9B**	
Placebo Cutoff	$25B	$50B	$100B	$5B	$1B
	(1)	(2)	(3)	(4)	(5)
Threshold × Post	–0.02	–0.03	–0.08	0.11	0.03
	(0.15)	(0.13)	(0.15)	(0.12)	(0.05)
Branch FE	X	X	X	X	X
Time FE	X	X	X	X	X
Observations	33,843	33,843	33,843	34,901	34,901
R-squared	0.660	0.660	0.660	0.682	0.681

Table 7: Quantifying Bank Pass-Through

This table quantifies the extent to which banks offset decreases in interchange revenue with increases in account prices. To calculate pass-through, Panel A estimates the change in interchange revenue and deposit fees following Durbin's enactment using the following difference-in-differences regression:

$$\text{Ln}(Y_{i,t}) = \alpha_i + \phi_t + \beta_d \times Durbin_i \times Post_t + \gamma \times \text{Ln}\,(Deposits_{i,t}) + \epsilon_{i,t}$$

Deposits is the dollar total of banks' transaction (non-interest bearing) deposits. *Durbin* is an indicator that takes a value of 1 for banks above the $10 billion threshold. *Post* is an indicator that takes a value of 1 in Q4 2011 (Durbin's enactment) and all quarters thereafter.

Panel B uses these estimates to quantify how much the increases in account fees documented offset banks' losses from the Durbin Amendment. The data are quarterly from Call Reports and cover bank holding companies with more than $500 million in total assets. Standard errors are clustered at the bank holding company level. Bank and time fixed effects are included.

Panel A: Change in Interchange and Deposit Fees post-Durbin

	Log Interchange Income	Log Deposit Fees
Durbin × Post	–0.37***	0.07**
	(0.07)	(0.03)
Log Deposits	0.19***	0.22***
	(0.06)	(0.07)
Bank FE	Yes	Yes
Time FE	Yes	Yes
Observations	22,783	22,783
R-squared	0.95	0.98

Panel B: Durbin Impact Offset by Account Fees

	Interchange income	Deposit fees
(a) Durbin banks total four quarter pre-Durbin	17.8	31.9
(b) Durbin-induced %Δ = exp{Durbin × Post} – 1	–31%	7%
(c) Durbin-induced $Δ = (a) × (b)	–5.5	2.3

Table 8: Estimating Durbin Impact on Retail Gas Margins based on Geographic Proximity

This table examines the effect of the Durbin Amendment on gas station margins by comparing proximate regions. Specifically, in Columns 1 and 2 of Panel A we estimate the following regression:

$$\Delta Margin_{z1} - \Delta Margin_{z2} = \beta \times (Impact_{z1} - Impact_{z2}) + \gamma \times (\theta_{z1} - \theta_{z2}) + \epsilon$$

for ZIP code pairs within a 20-mile radius of one another. *Impact* is a ZIP code's post-Durbin decrease in interchange expense, scaled by total sales. $\Delta Margin_z$ is the difference between average margins in the six months following Durbin's enactment and in the six months prior to it. In Columns 3 and 4, we estimate how this effect evolves over narrower geographic distances by including dummy variable interactions into the regression that take a value of 1 when ZIP pairs are within one mile, one to two miles, and so forth. In Columns 5 and 6, we compare this approach to naïve ordinary least-squares estimation at the ZIP code level. Panel B estimates the Column 2 specification for each decile of ZIP code pairs sorted by Δ *Impact*.

Panel A

Unit of analysis:	Pairs of nearby ZIP codes				ZIP code-level	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Impact	-0.697***	-0.564***				
	[0.082]	[0.081]				
Δ Impact x Distance ∈ (0,1]			-0.804	-0.868		
			[1.273]	[1.272]		
Δ Impact x Distance ∈ (1,2]			0.368	0.153		
			[0.467]	[0.467]		
Δ Impact x Distance ∈ (2,5]			-0.610***	-0.666***		
			[0.176]	[0.174]		
Δ Impact x Distance ∈ (5,10]			-0.967***	-0.840***		
			[0.119]	[0.117]		
Δ Impact x Distance ∈ (10,15]			-0.705***	-0.518***		
			[0.113]	[0.113]		
Δ Impact x Distance ∈ (15,20]			-0.526***	-0.379***		
			[0.122]	[0.121]		
Impact					1.670***	0.360
					[0.236]	[0.243]
ZIP Code Controls	No	Yes	No	Yes	No	Yes
Observations	151,983	151,983	151,983	151,983	8,323	8,323
Adjusted R-squared	0.001	0.014	0.001	0.014	0.006	0.059

Panel B: Decile-by-decile pairs analysis

Decile of Δ Impact:	Pt. Est.	St. Errors	Decile of Δ Impact:	Pt. Est.	St. Errors
10th	-2.516***	0.253	5	-4.509	3.834
9	-0.743	1.088	4	-5.711	4.716
8	-0.513	1.871	3	-7.629	4.947
7	1.311	2.61	2	1.721	5.402
6	-2.793	3.167	1	5.369	5.532

Table 9: Estimating Durbin Impact on Retail Gas Margins based on Propensity Score Matching

This table presents the propensity score matching estimates of the change in gas station margins post-Durbin. ZIP codes in the bottom decile of Durbin-induced change in interchange expense form the control group. Treatment groups range from the top decile in Column 1 to the 2nd decile in Column 9. The dependent variable is the difference between average margins in the six months following Durbin's enactment and in the six months prior to it. Propensity scores are estimated using household income, population density, fuel station density, and size. The gas station data are from OPIS and the interchange data are from a large payments player. Heteroskedasticity-robust standard errors are reported in brackets.

Treatment Decile:	10th (Top)	9th	8th	7th	6th	5th	4th	3rd	2nd
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Impact: Treatment vs. Bottom Decile	-0.0065***	0.0008	-0.0018	0.0020	-0.0032	0.0013	0.0010	0.0017	0.0029
	[0.0025]	[0.0025]	[0.0023]	[0.0026]	[0.0027]	[0.0025]	[0.0023]	[0.0023]	[0.0022]
Observations	2,114	1,938	1,931	1,979	1,979	2,015	2,053	2,075	2,207

Table 10: Market Power and Impact of Durbin on Retail Gas Margins

This table examines how Durbin's impact on retail gas margins varies with local market dynamics. We identify fuel stations in the top (treatment group) and bottom (control group) deciles of the Durbin Impact distribution, split them into two subsamples (high-competition and low-competition), and run the propensity score matching analysis over each subsample. The dependent variable is the difference between average margins in the six months following Durbin's enactment and in the six months prior to it. Propensity scores are estimated using household income, population density, fuel station density, and size. The measure of competition is based on the number of other gas stations within concentric circles of varying radiuses (indicated in Column headers) around a given station. Fuel stations surrounded by an above-median number of competitors face "high competition." The gas station data are from OPIS and the interchange data are from a large payments player. Heteroskedasticity-robust standard errors are reported in brackets.

Competition radius:	2 miles	5 miles	10 miles	15 miles
	(1)	(2)	(3)	(4)
High competition	-0.0104***	-0.0067***	-0.0134***	-0.0107***
	[0.0019]	[0.0024]	[0.0020]	[0.0020]
Observations	5,495	5,214	5,198	5,205
Low competition	-0.0070**	-0.0052*	-0.0052**	-0.0081***
	[0.0030]	[0.0029]	[0.0026]	[0.0028]
Observations	4,891	5,172	5,188	5,181

Table 11: Understanding Causes of Unbanked Status

This table examines the share of individuals who are unbanked (without a bank account); as well as the reason reported for unbanked status. It relies on data from the FDIC Surveys of Unbanked and Underbanked Households collected in the years up to but not including and after 2012, the year of Durbin's enactment. The table reports unbanked status, new unbanked status, and the main reasons consumers are unbanked overall and separately for subgroups of states depending on the share of bank deposits at large (above $10 billion threshold) banks. Differences are reported in the right-most columns.

	Overall			1st tercile Least Durbin			2nd tercile			3rd tercile Most Durbin		
	2011	2013	Diff	2011	2013	Diff	2011	2013	Diff	2011	2013	Diff
Became unbanked in last year	9.3	9.1	-0.3	9.2	6.8	-2.3*	10.6	9.8	-0.8	7.6	9.5	1.9
Main reason for being unbanked												
Insufficient money for account	44.98	35.62	–9.36***	44.9	36.87	–8.03***	45.28	35.46	–9.82***	44.61	35.09	–9.52***
Don't trust banks	10.26	14.93	4.68***	12.31	14.13	1.82	10.63	15.37	4.74***	8.55	14.84	6.29***
Account fees too high/unpredictable	7.38	13.38	6.00***	7.81	12.26	4.44**	7.19	12.33	5.14***	7.39	15.39	8.01***
ID/credit history problems	9.03	6.85	–2.18***	6.91	5.24	–1.67	8.34	7.21	–1.12	11.23	7.32	–3.90***
Banks don't offer needed products	0.57	1.25	0.68**	0.79	2.01	1.22*	0.54	0.69	0.15	0.48	1.52	1.04**
Inconvenient hours/locations	1.97	2.64	0.67	1.84	3.53	1.69*	2.44	2.81	0.37	1.38	1.89	0.51
Other reason	25.82	25.33	–0.48	25.43	25.96	0.53	25.58	26.13	0.55	26.36	23.94	–2.43
Observations			5,011			1,286			1,959			1,766

Figure 1: Impact of Durbin Amendment on Interchange Revenue

This figure plots the evolution of bank interchange fees for banks above and below the $10B Durbin threshold. Panel A reports interchange fees in aggregate for these two groups; Panel B reports total interchange fees scaled by total deposits. The data are quarterly from the Call Reports and cover bank holding companies with more than $500 million in total assets.

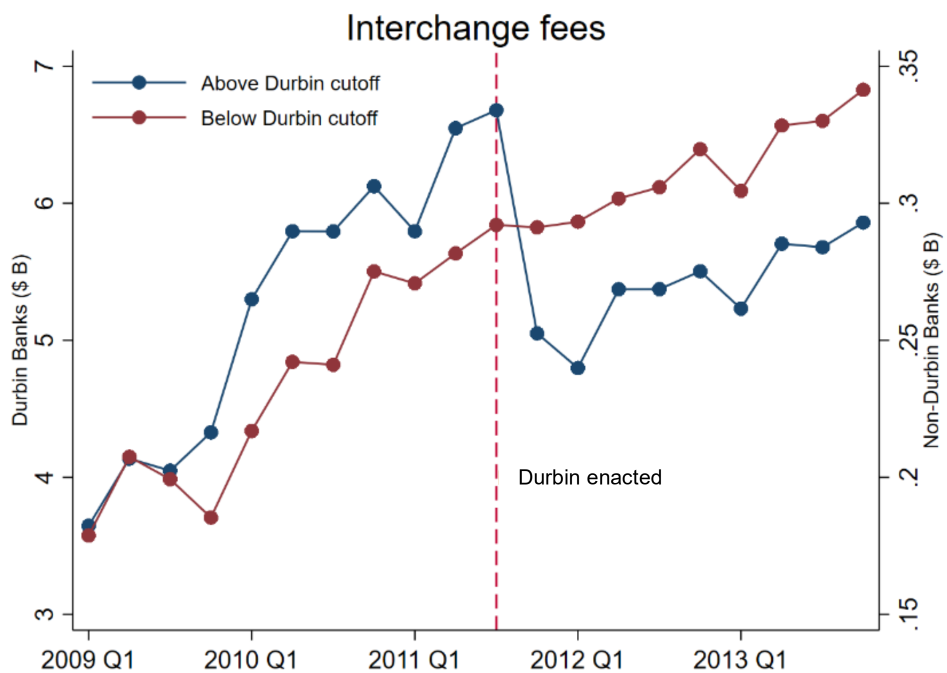


Figure 2: Impact of Durbin on Interchange Revenue, Event Study Approach

This figure examines the effect of the Durbin Amendment on bank interchange revenue. The results are from the following event study regression, reported separately for the evolution of bank-level interchange fees and interchange fees scaled by deposits:

$$\text{Ln}(Y_{i,t}) = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \text{Durbin}_\text{i} \times 1[s = t] + \epsilon_{i,t}$$

Durbin is an indicator that takes a value of 1 for banks above the $10 billion threshold. The omitted category is Q2 2010 (Durbin's passage). Coefficients on $Durbin \times Time$ indicators are reported, along with their 95 percent confidence intervals. The data are quarterly from the Call Reports and cover bank holding companies with more than $500 million in total assets. Standard errors are clustered at the bank holding company level. Bank and time fixed effects are included.

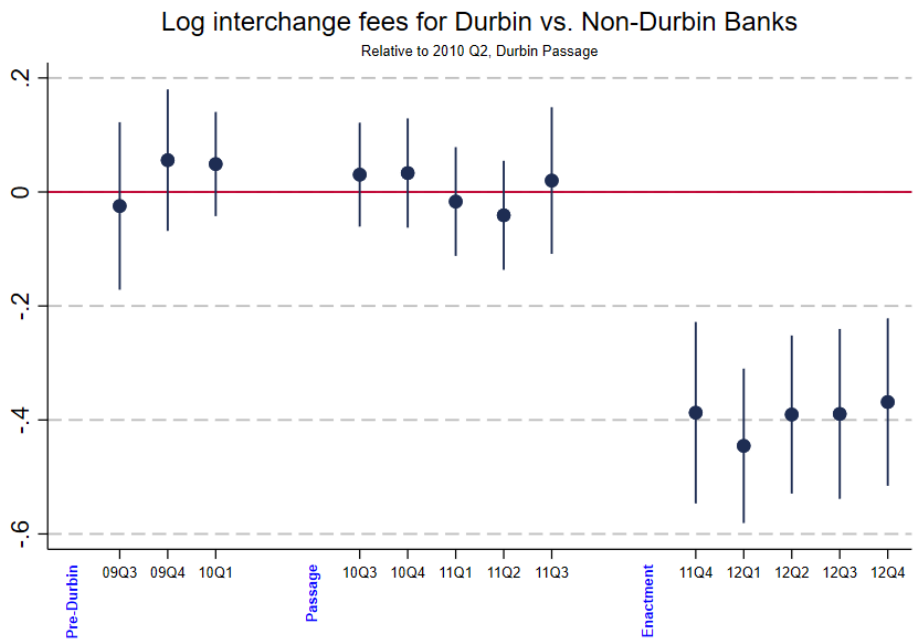
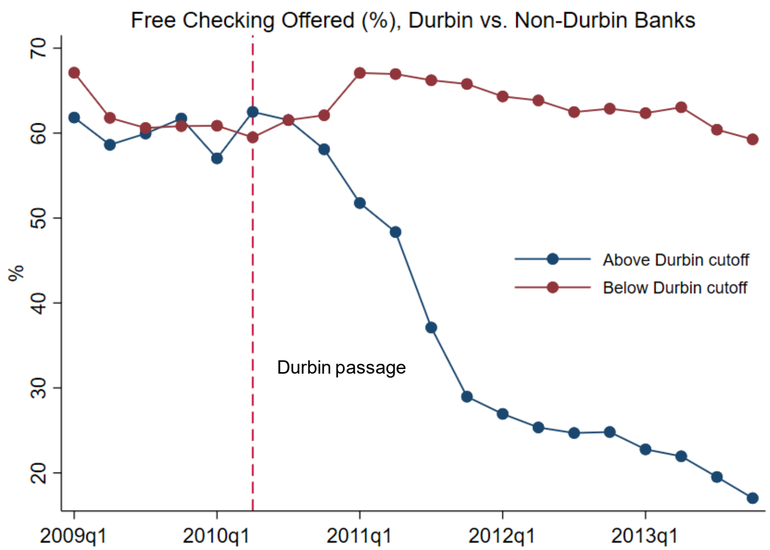


Figure 3: Impact of Durbin on Basic Checking Account Fees

This figure plots the evolution of checking account fees for banks above and below the $10 billion Durbin threshold. Panel A reports the share of banks that offer a free checking account, regardless of account size; Panel B reports average checking account fees. The data are quarterly from RateWatch and cover fee-setting branches of bank holding companies with more than $500 million in total assets.

Panel A: Availability of Free ($0 Monthly Fee) Accounts



Panel B: Account Fees

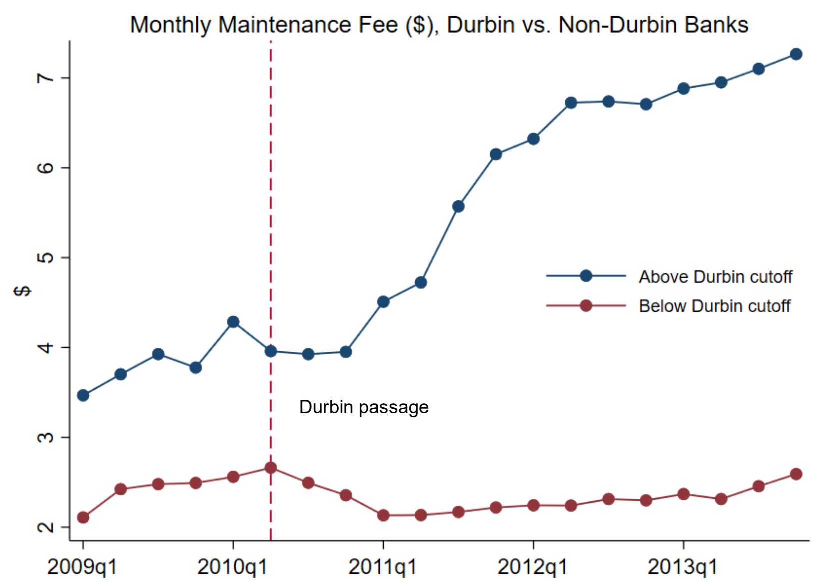


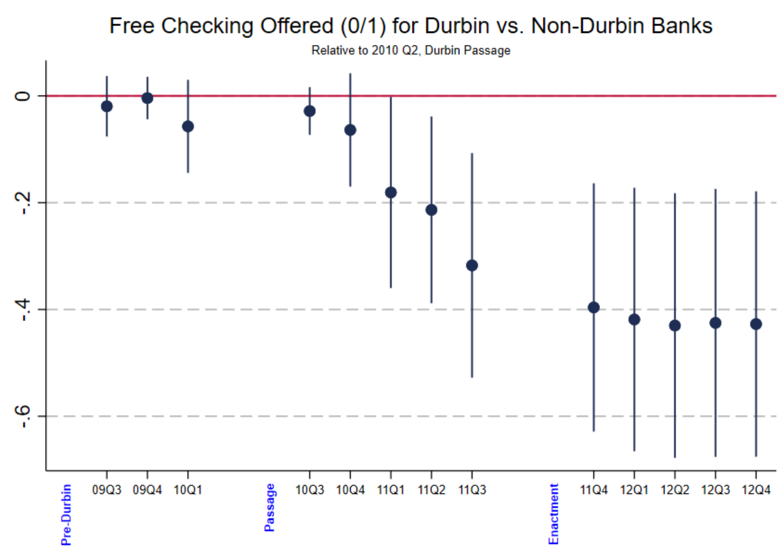
Figure 4: Impact of Durbin on Basic Checking Account Fees, Event Study Approach

This figure examines the effect of the Durbin Amendment on the checking account fees banks charger. The results are from the following event study regression, reported separately for the binary free checking variable (= 1 if banks charge no monthly maintenance fee for a checking account, regardless of account size) and the dollar value of banks' checking account fees.

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{s \neq 10Q2} \beta_s \times \text{Durbin}_\text{i} \times 1[s = t] + \epsilon_{i,t}$$

Durbin is an indicator that takes a value of 1 for banks above the $10 billion threshold. The omitted category is Q2 2010 (Durbin's passage). Coefficients on $Durbin \times Time$ indicators are reported, along with their 95 percent confidence intervals. The data are quarterly from RateWatch and cover fee-setting branches of bank holding companies with more than $500 million in total assets. Standard errors are clustered at the bank holding company level. Branch and time fixed effects are included.

Panel A: Free Checking



Panel B: Account Fees

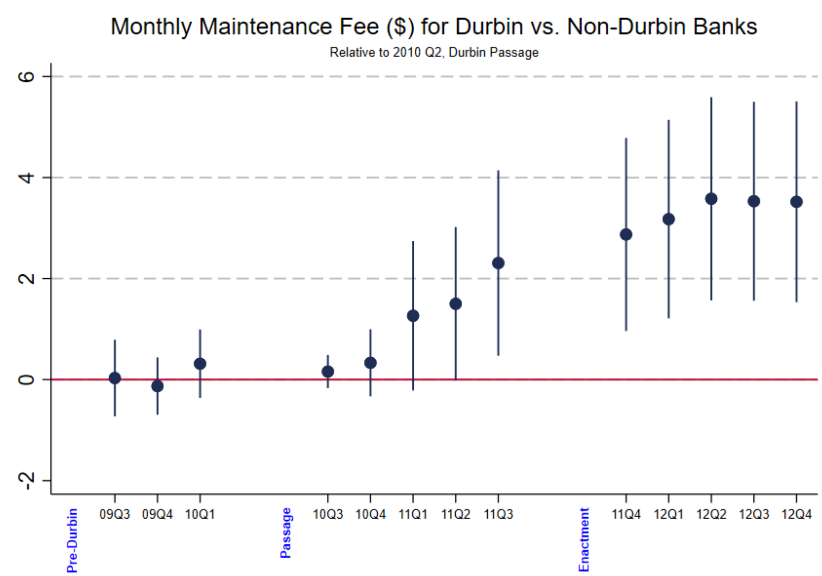
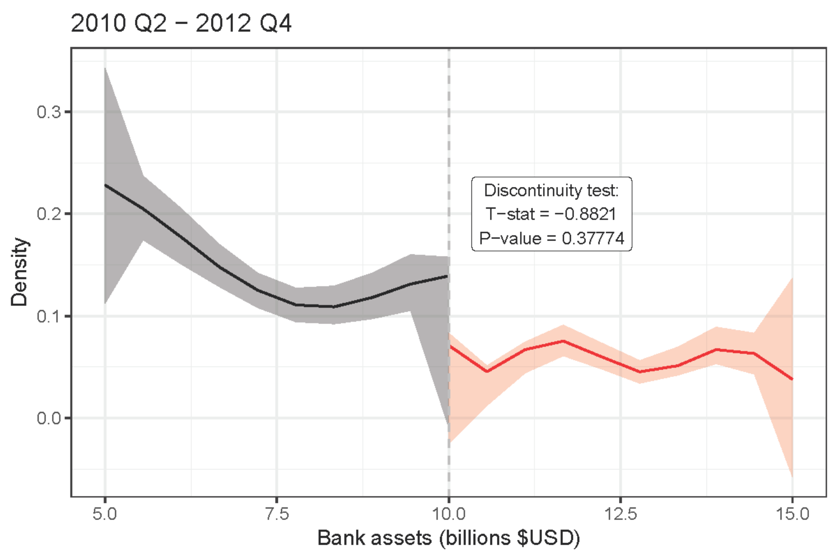


Figure 5: Bunching Tests of Durbin Threshold

This figure explores whether banks strategically avoid the $10 billion Durbin threshold. Panels A and B provide an estimation of the density of the distribution of bank assets around the $10 billion Durbin threshold using Cattaneo et al. (2017) local polynomial density estimator to test for any discontinuities around the $10 billion threshold. This estimation is provided both for the years immediately surrounding Durbin (the sample period); and for the more recent epoch. The data are quarterly from the Call Reports and cover bank holding companies with between $5 billion and $15 billion in total assets.

Panel A: Years Surrounding Durbin (in sample)



Panel B: More Recent Period (not in sample)

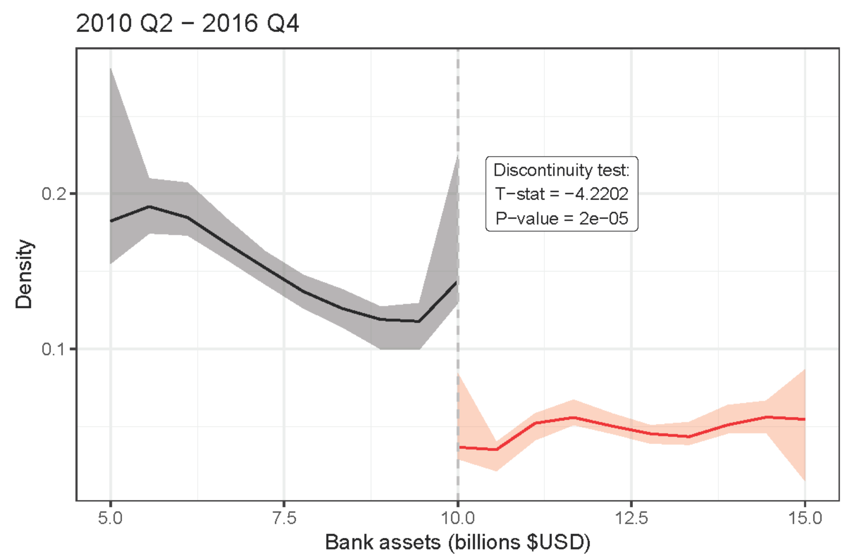


Figure 6: Impact of Durbin on Gas Margins: Synthetic Control Analysis

This figure shows how gas retailers respond to Durbin by comparing those gas stations whose interchange expense decreases most post-Durbin (top decile of Durbin impact) to synthetic control stations constructed to match pre-period margin dynamics, following Abadie and Gardeazabal (2003). Larger red dots plotted below represent the cumulative differences in margins between treatment and synthetic control stations in the weeks post-Durbin. Smaller and in black are the evolution of margins for 100 placebo treatment groups, constructed to apply confidence bounds on our estimate of the effect of intervention.