

Three Essays on Urban Policies

Meiping (Aggie) Sun

SUBMITTED IN PARTIAL FULFILLMENT OF THE
REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY
IN THE GRADUATE SCHOOL OF ARTS AND SCIENCES

COLUMBIA UNIVERSITY

2017

© 2017 - MEIPING (AGGIE) SUN
ALL RIGHTS RESERVED.

Three Essays on Urban Policies

ABSTRACT

This dissertation contains three chapters that examine urban policies. The first chapter considers the impacts of a new card fee for prepaid transit cards in New York City. Since 1998, the New York City Metropolitan Transportation Authority (MTA) system has used prepaid cards (MetroCards) to collect subway and bus fares. In 2013, the MTA imposed a \$1 card fee (surcharge) on new MetroCard purchases. Using a novel dataset with transaction-level deposit and card use information, I show that the fee caused riders to put more money on new MetroCard purchases, particularly those in low-income neighborhoods and those who used cash or debit (rather than credit) cards. As a result, the net monthly outstanding balance from transit card deposits increased dramatically, with riders lending an extra \$150 million, on an annual basis, to the MTA. Moreover, over \$20 million of the increased balances in the first year were never redeemed and escheated to the MTA when these cards expired. The leading explanation highlights the importance of the cost of effort to remember to carry the same card. I pose a structural model to calibrate the effect of a new card fee. Counterfactual simulation predicts that a new card fee of \$4.35 will maximize the MTA's profit. These findings have implications for fiscal policy designs and fee structures of prepaid card industry.

The second chapter examines the causal effects of local access to alcohol on birth outcomes. After the repeal of National Prohibition in 1933, 30 states gave

counties and municipalities the local option to continue alcohol restrictions. Citizens set alcohol control policies in their communities through jurisdiction-wide elections (i.e., local option elections). Currently, 10% of U.S. communities maintain a ban on some or all alcohol sales. Assessing the impact of local access to alcohol on alcohol-related outcomes such as birth weight, drinking under the influence, alcohol-related crimes, and so on is complicated by the potential non-random selection of liquor laws. I examine the causal effects of local access to alcohol on birth outcomes by comparing municipalities where referenda on legalizing liquor sales passed and failed by narrow margins. My results indicate that municipalities which were studied experienced higher incidence of low birth weight after legalizing the local sale of alcohol to the general public. The incidence of low birth weight rose by 4.5% for babies born within two years after the elections.

The third chapter measures the deleterious effect of institutional discrimination on health. Interest in the impact of institutional discrimination on health outcomes has increased dramatically. Since research has mostly been done in the western context where social segregation has already been established, it is difficult to isolate the effect of initial social segregation on health outcomes. In this chapter, I examine the causal effect of institutional discrimination on health by exploiting a 1964 change in household registration system (*hukou*) in China, which caused a nationwide discrimination against rural dwellers. The 1964 change in the *hukou* system started to put tight control on domestic migration. Thereafter, movement from rural to urban areas became virtually impossible.

Following the 1964 change in hukou policy, the fraction of urban hukou residents suddenly fell from over 50% to about 40%. I use this discontinuity in the proportion of urban hukou residents to identify the causal effect of institutional discrimination anchored in the hukou system on health. The regression-discontinuity (RD) design estimates suggest that urban hukou citizens have much better chances of being in good health. The deleterious effect of rural hukou on health possibly works through mechanisms of labor disparity, limited access to healthcare, and deprivation of quality education.

Acknowledgments

I would like to thank Brendan O’Flaherty, Pietro Ortoleva, Douglas Almond, Suresh Naidu, and Jonah Rockoff for invaluable guidance, assistance and advice. I thank Jushan Bai, Alessandra Casella, Donald Davis, Mark Dean, Francois Gerard, Wojciech Kopczuk, and Miikka Rokkanen for discussions and comments that shaped the content of this paper. I thank Laxman Gurung, Sun Kyoung Lee, Xuan Li, Janis Priede, Tuo Chen, and Danyan Zha for their help. I thank Tuo Chen for helping with Matlab Codes. I thank Sun Kyoung Lee for providing subway-census-tract data. I am particularly indebted to the staff at the New York City Metropolitan Transportation Authority (MTA), the Carolina Population Center at the University of North Carolina at Chapel Hill, and Texas Department of State Health Services who have provided data for my research. All errors are my own.

Contents

LISTING OF FIGURES	iii
1 MONEY FOR METROCARDS: HOW A NEW CARD FEE MADE TRANSIT RIDERS INVEST MORE AND LOSE MORE	1
1.1 Introduction	1
1.2 Background	6
1.3 Data	9
1.4 Main Findings	11
1.5 Model	17
1.6 Alternative Mechanisms	24
1.7 Robustness tests	26
1.8 Implications of the Model	28
1.9 Discussions and Conclusions	29
1.10 Appendices	67
2 HOW LOCAL ALCOHOL ACCESS AFFECTS BIRTH OUTCOMES: EVIDENCE FROM TEXAS	71
2.1 Introduction	71
2.2 Data	74
2.3 Model	76
2.4 Results	78
2.5 Discussions	83

2.6	Conclusion	85
3	THE CAUSAL EFFECT OF INSTITUTIONAL DISCRIMINATION ON HEALTH: EVIDENCE FROM HUKOU SYSTEM IN CHINA	86
	REFERENCES	105

Listing of figures

1.9.1	MetroCard Purchase Venues	38
	(a) Vending Machine	38
	(b) Manned Booth	38
1.9.2	First Screen Snapshot on Vending Machine Purchases Before and After the New Card Fee	39
	(a) Before	39
	(b) After	39
1.9.3	Monthly New MetroCard Sales From January 2009 to June 2015	40
	(a) Total	40
	(b) Pay-per-ride	40
	(c) 30-day-unlimited	40
	(d) 7-day-unlimited	40
1.9.4	Daily Sales of New MetroCards (All Types) from January 1, 2013 to April 30, 2013	41
	(a) Raw	41
	(b) Adjusted	41
1.9.5	Daily Sales of New Pay-per-ride MetroCards from January 1, 2013 to April 30, 2013	41
	(a) Raw	41
	(b) Adjusted	41

1.9.6	Daily Sales of New 7-day-unlimited MetroCards from January 1, 2013 to April 30, 2013	42
	(a) Raw	42
	(b) Adjusted	42
1.9.7	Daily Sales of New 30-day-unlimited MetroCards from January 1, 2013 to April 30, 2013	42
	(a) Raw	42
	(b) Adjusted	42
1.9.8	Monthly Outstanding balance from Pay-per-ride Deposits from January 2008 to April 2015	43
1.9.9	Aggregate Forgone Balances on Pay-per-ride MetroCards Initially Purchased Between January 1, 2013 and April 30, 2013	44
1.9.10	Weekly Total Number of MetroCard Swipes from January 2011 to May 2015	45
1.9.11	Monthly Revenue from Pay-per-ride Deposits from January 1, 2011 to April 30, 2015	46
	(a)	46
	(b)	46
1.9.12	Deposits on New Pay-per-ride MetroCard Purchases Before versus After the New Card Fee (From January 1, 2013 To April 30, 2013): Cumulative Distribution Function	46
	(a) Total	46
	(b) One-week	46
1.9.13	Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013): Histogram	47
	(a) Total	47
	(b) One-week	47
1.9.14	Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013): Cumulative Distribution Function	48

(a)	No Refills	48
(b)	With Refills	48
1.9.15	Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013):	
	Histogram	49
(a)	No Refills	49
(b)	With refills	49
1.9.16	Changes in Deposits and Forgone Balances on New Pay-per-ride MetroCards By Different Payment Methods (January 1, 2013 To April 30, 2013): Cumulative Distribution Function	50
(a)	Deposits (Cash)	50
(b)	Forgone Balances (Cash)	50
(c)	Deposits (Debit)	50
(d)	Forgone Balances (Debit)	50
(e)	Deposits (Credit)	50
(f)	Forgone Balances (Credit)	50
1.9.17	Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee by Payment Methods (From January 1, 2013 To April 30, 2013): Histogram	51
(a)	Cash	51
(b)	Debit	51
(c)	Credit	51
1.9.18	Percent of Cash Payments Across Different Subway Stations From January 2013 To April 2013	52
1.9.19	Threshold of Prepaying for Rides in the Second Day For $n = 2$ Case	53
(a)	No Card Fee	53
(b)	With Card Fee	53
(c)	With Card Fee (Changes)	53
1.9.20	Threshold of Prepaying for Rides in the Second and Third Day For $n = 3$ Case	54

(a)	No Card Fee	54
(b)	With Card Fee	54
1.9.21	Threshold of Prepaying for Rides in Future Days For $n = 30$ Case (No Card Fee)	55
1.9.22	Default Choices of Deposit Amounts on Vending Machine Screen Before and After the New Card Fee	56
(a)	Screen Before	56
(b)	Cumulative Distribution Before	56
(c)	Screen After	56
(d)	Cumulative Distribution After	56
1.9.23	Default Choices of Deposit Amounts For $n = 30$ Case	57
(a)	No Card Fee	57
(b)	With Card Fee	57
(c)	With Card Fee (Changes)	57
1.9.24	Daily Total Number of Pay-per-ride MetroCard Purchases From January 1, 2013 To April 30 2013 By Deposit Amounts	58
(a)	$\sim \$5$	58
(b)	$\sim \$10$	58
(c)	$\sim \$20$	58
(d)	$\$40 \sim \50	58
1.9.25	Changes in Average Deposit Amounts For Pay-per-ride Metro- Cards Linked to the Same Rider Using MetroCard Trade-in and Trade-out Data (January 1, 2013 To December 31, 2013)	59
(a)	$\sim \$5$ Deposit Amounts Before the New Card Fee	59
(b)	$\sim \$10$ Deposit Amounts Before the New Card Fee	59
1.9.26	Changes in Deposit Amounts After the New Card Fee For Pay- per-ride MetroCards Linked to the Same Rider Using MetroCard Trade-in and Trade-out Data (January 1, 2013 To December 31, 2013): Cumulative Distribution Function	60
(a)	$\sim \$5$ Deposit Amounts Before the New Card Fee	60
(b)	$\sim \$10$ Deposit Amounts Before the New Card Fee	60

1.9.27	Proposed Default Choices of Deposit Amounts on Touchscreen	60
1.9.28	Monthly Sales of Single-ride Tickets From January 2011 To June 2015	61
1.9.29	Daily Total Number of MetroCard Purchases From January 1, 2013 To April 30, 2013: New and Refills	62
	(a) 7-day-unlimited Raw	62
	(b) 7-day-unlimited Adjusted	62
	(c) 30-day-unlimited Raw	62
	(d) 30-day-unlimited Adjusted	62
1.9.30	Deposits on Pay-per-ride MetroCard Purchases Before and After the 2009 Fare Hike (From May 1, 2009 To August 30, 2009): Cumulative Distribution Function	63
	(a) New	63
	(b) Refill	63
1.9.31	Deposits on Pay-per-ride MetroCard Purchases Before and After the 2009 Fare Hike (From May 1, 2009 to August 30, 2009): Histogram	64
	(a) New	64
	(b) Refill	64
1.9.32	Deposits on Pay-per-ride MetroCard Purchases Before and After the 2015 Fare Hike (From January 1, 2015 To May 31, 2015): Cumulative Distribution Function	65
	(a) New	65
	(b) Refill	65
1.9.33	Implication of the Models	66
	(a) Skype	66
	(b) Reloadable Debit Card	66
1.10.1	Inverse Demand Curve and Riding Threshold	68
2.2.1	Number of Referenda By Year, 1979-2003 (Total 310)	75

2.4.1	Histogram of Voting Shares in Local Option Elections, 1979-2003	
		79
2.4.2	Percent of Low Birth Weight Newborns, by the Share of Voters	
	For Alcohol Sales, One Year and Two Years Before Election	80
	(a) 13-24 Months Before	80
	(b) 1-12 Months Before	80
2.4.3	Percent of Low Birth Weight Newborns, by the Share of Voters	
	For Alcohol Sales, One Year and Two Years After Election	83
	(a) 1-12 Months After	83
	(b) 13-24 Months After	83
2.5.1	Percent of Low Birth Weight Newborns, by the Share of Voters	
	For Alcohol Sales, Within Ten Months After Election	84
3.0.1	Percent of Urban Hukou Residents in Cohorts From 1940-1990	98
	(a) CHNS	98
	(b) 1990 Census	98
3.0.2	Self-reported health status and birth year from 1940 to 1990 . .	99

1

Money for MetroCards: How a New Card Fee Made Transit Riders Invest More and Lose More

1.1 INTRODUCTION

Prepaid cards have become an increasingly prominent form of payment for many industries and public services providers. For instance, currently over 23 million US adults, mostly "unbanked" consumers from low-income households, use general purpose reloadable cards such as Green-Dot Card every month (Urahn et al. 38). Some controversy has emerged because merchants are critical of the card fees, challenging both structure and level, and heated debates among researchers, practitioners, and policymakers have ensued. While the academic literature has

so far focused on fees that are proportional to the transaction values or fixed per-transaction fees, the effect of a new card fee is not clear, especially in monopolistic markets.

In this paper, I show how a new card fee for prepaid transit cards induced riders to put more money on cards and lose more when these cards expired. I present a novel transaction-level data set from the public transit system in New York City that allows me to analyze changes in deposit amounts and the forgone balance on expired cards. The data set contains detailed information on all the deposits and card uses from January 2013 to May 2015, with more than 100 million observations.

In March 2013, the New York City Metropolitan Transportation Authority (MTA) imposed a \$1 "green" card fee on new MetroCard purchases to motivate riders to refill and keep using their existing cards rather than purchasing new ones, thereby reducing litter. The Authority's stated goal behind the card fee was achieved as the number of new MetroCards sold dropped immediately and stayed low after the card fee was imposed. Before 2013, the Authority, on average, sold about 7 million cards per month. After the card fee, this number dropped to about 2 million per month. Meanwhile, there was only a minor decrease in ridership since the imposition of the card fee.

Surprisingly, riders started to make much larger deposits on new MetroCard purchases after the \$1 new card fee (surcharge); this translates into riders lending the transit authority \$150 million more annually. The monthly outstanding balance that riders carry on their MetroCards (defined as the difference between the total amount loaded on the cards in that month and the reductions caused by swipes at turnstiles in the same month¹) jumped from less than \$35 million to more than \$45 million. Currently, the MTA is paying 0.37% interest on funds raised from short-term notes. This additional free lending potentially saved the MTA hundreds of thousands of dollars in interest payments.

Moreover, over \$20 million of the increased balances in the first year were never

¹Mathematically, the net outstanding balance for a specific month is calculated as $Balance = \sum deposits * (1 + bonus(\%)) - \sum rides * basefare$

redeemed and escheated to the MTA when these cards expired. Each MetroCard is valid for 18 months after the initial purchase; inactive balances on cards become assets of MTA under the category "expired fare revenue" after the expiration date. The aggregate forgone balance (i.e., expired fare revenue) in 2015, from cards initially purchased in late 2013 and 2014, the first year after the MTA implemented the card fee, increased to \$75 million from \$52 million, the aggregate forgone balance in 2014.

There are five main empirical findings regarding changes in deposit amounts and forgone balances on expired cards. First, the changes largely came from new cards that would not have subsequent refill activities, not from cards that showed subsequent refill activities. Second, the changes mainly came from cash or debit card payments rather than from credit card payments. Third, among cash payments, the changes were mostly from payments made at vending machines rather than from payments made at manned booths (tellers). Fourth, the response to the new card fee was larger in low-income neighborhoods than in high-income neighborhoods. Fifth, the response to the new card fee was not primarily from tourists. Although part of the changes could have come from tourists and short-term visitors, deposits and forgone balances increased dramatically in neighborhoods with few tourists such as South Bronx and Sunset Park in Brooklyn.

The increase in deposit amounts and leftover balances on expired cards was unanticipated: the MTA never said that the goal of this card fee was to attract more deposits; also, the card fee on a new MetroCard purchase is a one-time fee, which should have no impact on deposit amounts. Now the question is: why did riders make larger deposits to their MetroCards and lose more money after the new card fee was introduced? I explore potential explanations, including avoidance of coins, persuasion by vending machine messages, and commitment device.

In my view, these findings are consistent with a rational model that highlights the importance of the cost of effort to remember to bring the same MetroCard for future rides, the fixed cost of making deposits to MetroCards, and consumer uncertainty about future rides. When MetroCards were free, riders with a low fixed cost of making deposits to MetroCards chose not to incur the cost of effort to remem-

ber to bring the same card for future days. They deposited only a small amount of money on cards and purchased a new MetroCard each period if needed. After the new card fee was imposed, many riders switched to refilling existing cards since their cost of effort to remember to carry the same card was smaller than the new card fee. They started making larger deposits to save on the fixed cost of making deposits to cards. Because consumers are uncertain about future rides, these riders on average had higher leftover balances after the card expiration dates.

To calibrate the effect of a new card fee, I develop and estimate a dynamic model of MetroCard deposits and usage that makes use of detailed MetroCard data from the years 2013 to 2015. Given my parameter estimates, counterfactual simulations predict the effect of a \$1 new card fee had it been implemented with all the default choices of deposit amounts giving an exact number of rides. Holding prices fixed, my simulations predict an increase in expired fare revenue of \$19.76 million (39.52%) after the \$1 card fee was imposed on new MetroCard purchases, as opposed to \$25 million. I then simulate the amount of new card fee that maximizes the MTA's profit while holding the payment prompts on Touchscreen and base fare for every subway ride fixed. The model predicts that a new card fee of \$4.35 will .

The importance of the cost of effort to remember to carry the same card and the fixed cost to make deposits to cards may explain the prevalence of required minimum deposit amounts in the online or mobile prepaid services such as E-ZPass and Skype. When authorities or firms adopt online or mobile payments with an automatic deduction from bank accounts or credit cards, consumers' fixed cost to make payments converges to zero. As a result, cash flow from prepaid services will drop significantly as consumers switch from prepaying for future consumption to pay only for consumption this period (pay-as-you-go). To maintain the benefit from unused account balances, most online or mobile prepaid services providers have required minimum deposit amounts and use suggested deposit amounts to attract even more deposits.

The results of this study are likely to generalize to 8.3 million Americans who use public transit to go to work. 10.65 billion passenger trips were taken on transit

systems in 2013 (American Public Transportation Association 2014). Therefore, both in terms of monetary magnitude and in terms of population involved, the new card fee on transit card has a significant economic impact. Empirical analysis on optimal pricing for public transit system has been limited, with most studies focusing on the demand elasticity of rides in response to fare increases ([39]; Vickrey 40; De Palma and Lindsey 12; Small and Verhoef 36; Tirachinia and Henshera 37; De Jong and Gunn 11; Litman 26; Chen et al. 8; Miao and Gao 27). This paper is the first to examine in detail the effects of a new transit card fee. I show that a new card fee induced riders, especially low-income riders, to put more money on cards and lose more when these cards expired.

These findings also have implications for the fee structure of payment cards, especially reloadable prepaid debit cards. The academic literature has so far focused on fees that are proportional to the transaction values or fixed per-transaction fees (Shy and Wang 35; Schwartz and Vincent 33; Schmalensee 32). [35] showed that, when card networks and merchants both have market power, card networks earn higher profits by charging proportional fees. [33] showed that, when a card company faces local monopolist merchants, the No Surcharge Rule which prohibits merchants from charging higher prices to consumers who pay by card instead of other means ('cash') raises card company profit and harms cash users and merchants. Complementing prior studies, I show that, when a prepaid card issuer has market power, a new card fee (or card activation fee) could push consumers to prepay more for future consumption.

Finally, these findings have implication about regressive ways to raise money. Mainly due to data limitations, studies on regressive fees focus on the portion of fee revenue collected from low-income people (Dorfman 13; Gertler et al. 15; Grainger and Kolstad 16 and Leape 24). Here I provide evidence that whether or not the card fee itself is regressive, it may push low-income consumers to behave in ways that cost them money, especially in monopolistic market. The authorities should take into consideration the possible additional responses from low-income people when imposing a fee.

The remainder of the paper is organized as follows. Section 2 gives a brief in-

roduction to MetroCards and the new card fee. Section 3 describes main features of MetroCard data sets used in the empirical analysis. Section 4 presents the main findings about MetroCard sales, deposit patterns, and leftover balances on expired cards. Section 5 describes my model and identification in a simplified setting. Section 6 discusses estimation and counterfactual analysis. Section 7 considers other potential mechanisms that might explain the results. Section 8 performs robustness tests. Section 9 concludes the paper.

1.2 BACKGROUND

1.2.1 METROCARD

The MetroCard is a stored ride fare card for the New York City public transit system. It is a thin plastic card on which a rider electronically loads fares. Various types of MetroCards are available for purchase. There are two types of value-based cards: pay-per-ride MetroCards and single-ride tickets. Also, there are two types of time-based cards: 7-day-unlimited MetroCards and 30-day-unlimited MetroCards. The minimum purchase on a new pay-per-ride MetroCard is the fare of a round trip (currently \$5.50). No minimum purchase is required for refill transactions. Riders can put as much money on the card as they want.

A rider can purchase new or refill existing MetroCards at a subway station MetroCard vending machine (MVM) (Figure 1.9.1a) or at a station's manned booth (teller) (Figure 1.9.1b)². Upon the imposition of the card fee in March 2013, there was no major change in the user interface of vending machine screens, except for the added message about the card fee (Figure 1.9.2). More information is available on MTA's website: www.mta.info.

²MetroCards can also be purchased out-of-system through the MTA extended sales network (including merchants and tax-benefit providers), which now accounts for the majority of MetroCards sold. Approximately 2.8 million MetroCards are sold out-of-system each month, and this level has not changed noticeably since the introduction of the \$1 new card fee (out-of-system sales are not subject to the \$1 fee).

BONUS FREE RIDE FOR PAY-PER-RIDE PURCHASES

Since January 1, 1998, the MTA has given a "bonus" for pay-per-ride purchases that are at or above a certain threshold amount. For instance, from June 28, 2009, to December 29, 2010, the bonus value for pay-per-ride purchases was 15% of the purchase amount for purchases of \$8 or more. For example, when a rider made a deposit of \$10 to a pay-per-ride MetroCard, the card balance increased by \$11.50 ($\$10 + \1.50).

This is not a typical bonus since it is always a certain percent of the purchase amount (i.e., linear) while a usual bonus is an increasing percent of the purchase amount (i.e., non-linear). Currently, the bonus value for pay-per-ride MetroCards is 11% of the purchase amount for purchases of \$5.50 or more (Table 1.9.1 row 6).

1.2.2 POLICY CHANGES

Table 1 shows the recent history of MTA policy changes. Column 1 presents the fare hike in 2009. On June 28, 2009, the base subway and bus fare rose from \$2 to \$2.25. The monthly MetroCard rose from \$81 to \$89. The weekly MetroCard rose from \$25 to \$27. The pay-per-ride MetroCard bonus remained at 15%, but the threshold for the bonus increased from \$7 to \$8.

Column 2 lists the fare hike at the end of 2010. On December 30, 2010, the 30-day-unlimited card increased to \$104 and the 7-day-unlimited card increased to \$29. The bonus value for pay-per-ride cards decreased to 7% for every \$10. There was no change in base subway and bus fares, but the cost of a single-ride ticket went from \$2.25 to \$2.50.

Column 3 shows the fare hike in 2013. On March 3, 2013, the base subway and bus fare increased from \$2.25 to \$2.50. The cost of a 30-day-unlimited card increased to \$112. The cost of a 7-day-unlimited card increased to \$30. The bonus for the pay-per-ride MetroCard decreased from 7% to 5%, but the threshold for the bonus decreased from \$10 to \$5. The price of a single-ride ticket increased from \$2.50 to \$2.75. The MTA also imposed a \$1 fee on new card purchases, the impact of which forms the basis of this study.

Column 4 depicts the fare hike in 2015. On March 22, 2015, the base fare of subway and bus rides rose from \$2.50 to \$2.75. The cost of a 7-day-unlimited card rose from \$30 to \$31 and the cost of a 30-day-unlimited card increased from \$112 to \$116.50. Pay-per-ride bonuses increased from 5% to 11% for purchases greater than or equal to \$5.50.

CARD FEE ON NEW PURCHASES

The main policy change that concerns this paper is the imposition of a card fee on new MetroCard purchases. A new MetroCard itself used to be cost-free. A \$1 new card fee, tacked on when someone buys a new MetroCard, went into effect with the fare hikes on March 3, 2013. The fee applies to each new MetroCard purchased at a MetroCard Vending Machine, station booth, or commuter rail station. Riders can avoid this fee by refilling their MetroCards. The MTA will issue a new MetroCard at no charge if a card is expired or damaged. The new \$1 charge did not apply to single-ride tickets or to MetroCards bought by reduced fare customers (seniors and customers with disabilities)³.

1.2.3 ENVIRONMENTAL IMPACT

The transportation authority justified the new policy of the \$1 card fee on new MetroCards purchases in environmental terms, arguing that the policy would lead to cleaner subway stations by discouraging people from littering subway stations with their discarded, empty MetroCards. MTA officials mentioned this fee as an environmentally friendly initiative in numerous news reports⁴. On average, it

³Also, the card fee does not apply to MetroCards purchased out-of-system through MTA extended sales merchants, users of EasyPayXpress cards, transit benefit organization customers who get their MetroCards directly from employers or their benefit providers, or customers who purchase a combination railroad/MetroCard ticket. Out-of-system MetroCard sales now account for the majority of MetroCards sold. Approximately 2.8 million MetroCards are sold out-of-system each month, and this level has not changed noticeably when comparing MetroCard numbers sold before and after the card fee went into effect.

⁴Some news reports where MTA talked about the \$1 new card fee: [NY Times](#), [NY Daily News](#), [NBC News](#)

costs the agency \$20 million a year to print and clean up discarded cards from subway stations. According to MTA, after the imposition of the new card fee, printing fewer MetroCards and trimming cleanup costs was expected to save about \$2 million a year ⁵.

1.3 DATA

In this section, I present the main features of the datasets used in this study. This paper documents changes in deposit amounts on MetroCard purchases and increases in forgone balances on expired MetroCards using three data sets: MetroCard deposit data, swipe data, and trade-in and trade-out data.

1.3.1 METROCARD DEPOSIT DATA

MONTHLY-AGGREGATE DEPOSIT DATA

The MetroCard monthly revenue data from January 2009 to June 2015 includes information on the number of deposit transactions (new sales versus refills) as well as total in-system MetroCard purchase amounts, broken out for various types of MetroCards ⁶.

TRANSACTION-LEVEL DEPOSIT DATA

The transaction-level MetroCard deposit data⁷, covers all deposit transactions for the following periods: 1) May 1, 2009 - September 30, 2009; 2) January 1, 2013 -

⁵Some news reports where MTA mentioned the potential savings from the imposition of the new card fee: [NY Daily News-1](#), [NY Daily News-2](#), [NBC New York](#)

⁶Pay-per-ride, 7-day-unlimited, 30-day-unlimited, 7-day-unlimited Express, single-ride, reduced-fare seniors and disabled, etc.

⁷This dataset included deposit transactions from the New York City Subway rapid transit system; New York City Transit buses, including routes operated by Atlantic Express under contract to the Metropolitan Transportation Authority (MTA); MTA Bus, and Nassau Inter-County Express systems; the PATH subway system; the Roosevelt Island Tram; AirTrain JFK; and Westchester County's Bee-Line Bus System

May 1, 2013; and 3) September 1, 2014 - May 31, 2015. Each observation corresponds to a MetroCard deposit transaction and includes information on the amount of money added to the card, the station at which the card was purchased, the date and time of purchase, the type of deposit, the method of payment, and the balance of the MetroCard before the transaction. The data also include information on whether the transaction took place at a booth station or at a vending machine.

1.3.2 METROCARD SWIPE DATA

TRANSACTION-LEVEL SWIPE DATA

The transaction-level swipe data⁸, covers all MetroCard swipe transactions for the time period from January 1, 2013 to May 31, 2015. Each observation corresponds to a MetroCard swipe transaction and includes information on the amount of money deducted from the card, the station or bus route at which the card was swiped, the date and time of card swipe, and the balance of the MetroCard before the transaction.

WEEKLY-AGGREGATE SWIPE DATA

This data set includes the total number of MetroCard swipes riders made each week as they entered each station of the New York City Subway, PATH, AirTrain JFK and Roosevelt Island Tram from January 2011 to June 2015, broken out for various types of MetroCards.

1.3.3 METROCARD TRADE-IN AND TRADE-OUT DATA

Riders can transfer money in (trade-in) and out (trade-out) across different MetroCards they have. Also, riders can trade in their old cards that expired within the

⁸This dataset included MetroCard swipe transactions from the New York City Subway rapid transit system; New York City Transit buses, including routes operated by Atlantic Express under contract to the Metropolitan Transportation Authority (MTA); MTA Bus, and Nassau Inter-County Express systems; the PATH subway system; the Roosevelt Island Tram; AirTrain JFK; and Westchester County's Bee-Line Bus System

past two years and transfer any remaining money to a new card. This data set allows me to link multiple cards to the same rider. I can then compare changes in deposit amounts and foregone balances on cards initially purchased before and after the implementation of the new card fee by the same rider.

1.4 MAIN FINDINGS

The main empirical findings are summarized in Table 1.9.2. In particular, riders purchased MetroCards with much larger deposits after the imposition of the new card fee. Consequently, the monthly outstanding balance of deposits and foregone balances on expired cards jumped up significantly.

1.4.1 PURCHASES OF NEW METROCARDS DROPPED AFTER THE NEW CARD FEE WAS INTRODUCED

Table 1.9.3 shows the monthly purchase of new MetroCards from January 2009 to June 2015. After the implementation of the new card fee, the total monthly new MetroCard sales dropped from over 7 million to about 2 million and stayed low, which is very robust across different subgroups: the monthly sales of new pay-per-ride cards decreased from 5.8 million to 1.8 million; the monthly sales of new 30-day-unlimited cards decreased by about 75% to about 0.15 million; and the monthly sales of new 7-day-unlimited cards decreased by over 1.2 million to 0.34 million.

Figure 1.9.3 plots the monthly new MetroCard sales from January 2009 to June 2015. This figure shows that new MetroCard sales dropped immediately and stayed low after the imposition of the new card fee, confirming the summary statistics in Table 1.9.3. I replicated this analysis using transaction-level deposit data in Figure 3.0.1. This figure plots the daily new MetroCard sales from January 1, 2013 to April 30, 2013. The MTA sold about 0.2 million new MetroCards daily before the new card fee was introduced. This estimate dropped immediately to about 0.13 million on the first day the new card fee was implemented and further decreased gradually

over the next two months. Similar results are observed for all three MetroCard subgroups (Figure 1.9.5-1.9.7).

REGRESSION-DISCONTINUITY APPROACH

Since there was no significant sorting of MetroCard purchases around the date when the MTA implemented the new card fee, I estimated the effect of the new card fee with a regression discontinuity (RD) design. Under some mild regularity conditions, the average causal effect of the new card fee on MetroCard sales just before and just after the new card fee could be identified. There was no discontinuity in ridership or other covariates around the implementation date of the new card fee.

Assuming a homogeneous effect of the new card fee on MetroCard sales with one cutoff date:

$$Y_t = \beta + \gamma 1\{t \geq t_0\} + a(t) + X_t + u_t, \quad (1.1)$$

where t is the indexed date, Y_t denotes the new MetroCard sales on day t , t_0 was the distinct cutoff point (i.e., March 3, 2013), $a(\cdot)$ is a flexible function of date, X is a set of controls including day-of-week and month-of-year fixed effects. The coefficient of interest is γ which measures the effect of the new card fee on changes in daily new MetroCard sales.

I present estimates using the analog of the [5] bandwidth selectors for sharp RD. Similar estimates are observed under alternative bandwidth selectors based on the [20]. In the baseline specifications, I used local quadratic regression (a local polynomial of order two) for $a(\cdot)$. Across specifications, the estimated effect of the new card fee from both local linear and local quadratic regressions corroborate the visual evidence.

Table 1.9.4 presents the results for the effects of the new card fee on changes in daily new MetroCard sales from January 1, 2013 to April 30, 2013, using different control variables in each specification. The model in column 2 controls for day of week. The results show that the implementation of the new card fee caused a sig-

nificant decrease of 125,000 (70%) in daily new MetroCard purchases. To account for the possibility of variations in MetroCard sales across different months, my preferred specification in column 3 included month-of-year fixed effects. As with the other controls, the addition of month-of-year fixed effects has little impact on the estimated effects of the new card fee.

Using this preferred specification, Table 1.9.5 includes measures of changes in daily new card sales for 7-day-unlimited and 30-day-unlimited cards as well as pay-per-ride cards in response to the implementation of the new card fee. The imposition of the new card fee led to a decrease of 120,000 (65%) in daily new pay-per-ride MetroCard sales, a decrease of 23,000 (73%) in daily new 7-day-unlimited MetroCard sales, and a decrease of 24,000 (75%) in daily new 30-day-unlimited MetroCard sales.

1.4.2 DEPOSITS TO METROCARDS INCREASED SIGNIFICANTLY

After the MTA imposed the new card fee, riders who purchased new MetroCards, on average, made larger deposits. The imposition of the new card fee led to a decrease in the percentage of riders who made deposits of approximately \$5 by about 40% and an increase in the percentage of riders who made deposits of approximately \$10 and \$20 by about 35% (Figure 1.9.12a and Figure 1.9.13a). I checked the robustness of the findings by limiting my focus to deposit transactions within one week before and after the imposition of the new card fee. The same changes in deposit amounts are observable (Figure 1.9.12b and 1.9.13b). As a result, monthly revenue jumps up by 9.4%, from around \$160 million to \$175 million after the implementation of the new card fee (Figure 1.9.11a). This increase in monthly revenue becomes more noticeable when looking at year-on-year monthly revenue (Figure 1.9.11b).

I then used equation (3.1) to evaluate the effects of the new card fee on the amounts of deposits. Table 1.9.6 presents the results for the effects of the new card fee on changes in deposit amounts on new pay-per-ride purchases from January 1, 2013, to April 30, 2013, using different control variables in each specification.

The model in column 3 controls for day-of-week fixed effects. The results show that implementation of the new card fee caused a significant increase of \$1.64 in deposit amounts on new pay-per-ride purchases. Riders, on average, made larger deposits during morning and evening rush hours. To account for the possibility of variations in deposit amounts across different neighborhoods, my preferred specification in column 4 included station fixed effects. The addition of station fixed effects causes the estimated effects of the new card fee to be smaller, which indicates that there is heterogeneity in deposit amounts across different stations.

1.4.3 CHANGES IN THE OUTSTANDING BALANCE OF DEPOSITS

Because of the minor decrease in ridership and the significant increases in deposit amounts, the monthly outstanding balance of deposits made to pay-per-ride MetroCards unexpectedly jumped by about one-third, from around \$35 million to over \$45 million after the imposition of the new card fee (Figure 1.9.8). The aggregate monthly outstanding balance that riders carried on their MetroCard is defined as the difference between the total amount loaded on the cards and the reductions caused by swipes at turnstiles⁹. This additional outstanding balance translates to riders lending, on an annual basis, an extra \$150 million to the MTA. In contrast, the net outstanding balance showed no significant increase after the

⁹Mathematically, the net outstanding balance for month i is calculated as:

$$Balance_{mi} = \sum deposits * (1 + bonus(\%)) - \sum rides * basefare \quad (1.2)$$

where base fare is \$2.0 for months before February 2008, \$2.25 for months from March 2008 to February 2013; \$2.5 for months from March 2013 to February 2015.

Theoretically, the net outstanding balance for month i should be calculated as:

$$Balance_{mi} = \sum deposits + \sum bonus - \sum rides * basefare \quad (1.3)$$

However, I only have aggregate monthly deposit amounts data and cannot observe bonus amount for each deposit transaction. Hence, the outstanding balance calculated using equation (1) is the upper bound of the outstanding balance for each month. Since the threshold for bonus free rides was much higher (\$10) before the new card fee was imposed, the jump in the outstanding balance from the imposition of the new card fee should be even larger.

fare hikes in 2009 or 2010, nor after the fare hike in 2015. Hence, the observed changes in deposit patterns after the new card fee was not likely driven primarily by the \$0.25 increase of base fare.

1.4.4 INCREASES IN FORGONE BALANCES ON EXPIRED METROCARDS

Moreover, over \$20 million of the increased balances on MetroCards that were purchased in the first year after the new card fee was imposed were never redeemed and escheated to the MTA when these cards expired. Each MetroCard is valid for 18 months after the initial purchase; inactive balances on cards become assets of MTA under the category "expired fare revenue" after the expiration date. The aggregate forgone balance (i.e., expired fare revenue) in 2015, from cards initially purchased in late 2013 and 2014, the first year after the MTA implemented the card fee, increased to \$75 million from \$52 million, the aggregate forgone balance in 2014.

Figure 1.9.9 plots the aggregate leftover balances on MetroCards initially purchased between January 1, 2013 and April 30, 2013. This figure shows that the leftover balances on expired MetroCards jumped by about 50%, from around \$150,000 to over \$250,000 after the imposition of the new card fee, confirming the observed increase in aggregate leftover balances on expired MetroCards.

1.4.5 DEPOSITS TO METROCARDS SHOW DIFFERENT CHANGES: NEW METRO-CARDS THAT WOULD BE HELD FOR DIFFERENT LENGTHS OF TIME

In order to explore the change in deposit patterns further, I examined the deposit pattern in MetroCards held for different lengths of time. Deposits on new pay-per-ride purchases increased tremendously for MetroCards without subsequent refill activities¹⁰ (Figure 1.9.14a). Before the new card fee, about 60% of deposits were \$5 or less for pay-per-ride MetroCards that had no subsequent refill activities. After the new card fee, this percentage dropped by half to about 30%, while the per-

¹⁰To minimize measurement errors, I only focused on cards purchased before April 1, 2013 and give each card at least one month to demonstrate refill activities.

centage of \$10 or \$20 deposits almost doubled. In contrast, there was only a minor change in the initial deposit amount for MetroCards that showed subsequent refill activities (Figure 1.9.14b).

1.4.6 HETEROGENEITY IN DEPOSITS TO NEW PAY-PER-RIDE CARDS AND FORGONE BALANCES ON EXPIRED CARDS: BY PAYMENT METHODS

With transaction-level MetroCard information, I examined the changes in deposit amounts and forgone balances on cards purchased using different payment methods (cash, debit card, versus credit card). The main changes came from cash or debit card payments rather than from credit card payments. Consistent with the pattern of deposit changes, there was higher forgone fare on MetroCards initially purchased by cash or debit cards rather than by credit cards (Figure 1.9.16b, 1.9.16d, and 1.9.16f). For credit card payments, there was only a slight increase in deposit amounts and forgone balances after riders are charged a fee for purchasing a new MetroCard (Figure 1.9.16e and 1.9.17c). In contrast, the percentage of riders who made cash deposits of approximately \$10 or \$20 increased significantly, from 33% to 63%. The percentage of riders who made cash deposits of approximately \$5 dropped by about one-third (Figure 1.9.16a and 1.9.17a). For debit card payments, the percentage of riders who made deposits of approximately \$10 or \$20 increased significantly from 46% to 63%. The percentage of riders who made deposits of approximately \$5 dropped by about half (Figure 1.9.16c and 1.9.17b).

CHANGES IN DEPOSIT AMOUNTS ACROSS DIFFERENT NEIGHBORHOODS

The Survey of Consumer Finances (SCF) indicates that it is primarily the poor who use cash in the US ([2]; [21]). To test whether this is true in the case of MetroCards, I linked the deposit amounts in different subway stations to local census tract income data. As shown in Figure 1.9.18, low-income neighborhoods have a much higher percentage of cash payments compared to high-income neighborhoods. This finding is in line with other studies on cash usage across different socioeconomic groups ([22]; [14]).

Since the increase in the outstanding balance mainly came from cash or debit-card payments, the new card fee may cause a stronger tendency to load more onto their cards among low-income communities. Controlling for station-level covariates, I used a regression framework to evaluate the effects of new card fee on deposit amounts across different neighborhoods. The empirical model takes the following forms:

$$Y = \theta_0 + \theta_1 \text{Surcharge} + \theta_2 \text{Income} + \theta_3 \text{Surcharge} * \text{Income} + \lambda X + \varepsilon$$

where Y is the deposit amount for pay-per-ride MetroCards, and Surcharge is an indicator for observations after the implementation of the new card fee. Income represents log-income at the census-tract level. X is a set of controls such as day-of-week fixed effects. The coefficient of interest is θ_3 , the coefficient on the interaction of Surcharge and Income , which measures the effect of income level on changes in deposit amounts after the new card fee relative to changes in deposit amounts before the new card fee.

Table 1.9.7 presents the results for the effect of the new card fee on deposit amounts using MetroCard transactions from January 1, 2013 to April 30, 2013. After the implementation of the new card fee, the change in deposit amounts was larger in poorer neighborhoods. On the other hand, there are no significant changes in ridership across different neighborhoods before and after the new card fee (Table 1.9.8). These results indicate that the new card fee induces poor riders to make larger deposits on new MetroCard purchases and lose more balances on expired cards, which is not surprising because riders from richer neighborhoods already made large deposits on their MetroCards before the new card fee was imposed.

1.5 MODEL

I present a simple model that highlights card carrying cost and estimate this model using my observational data. A rider is assumed to take two rides (a round-trip) or no rides each day. The base fare for a ride is p . Each rider is assumed to live in New York City for the next n -day time period ($n \in (0, \text{inf})$). The card fee for

purchasing a new MetroCard is T . The discount factor is ignored since the time horizon is small.

For rider i , the fixed cost of each deposit (e.g., the opportunity cost of waiting in line to add money to the card, the expected opportunity cost of missing a train in station, etc) is c_i . Among riders, latent c_i is normally distributed censored at zero: $N_c \sim (\mu_c, \sigma_c)$. Rider i incurs a utility cost e_i to remember to carry the same MetroCard for the next day. Among riders, latent e_i is normally distributed censored at zero among riders: $N_e \sim (\mu_e, \sigma_e)$. (Throughout the paper, I will use the parameters of the pre-censoring distributions to describe the censored distributions.) The correlation, $\rho_{c,e}$, between e_i and c_i is assumed to be zero since c_i depends on rider i 's opportunity cost of time while e_i depends on rider i 's mental cost to be well-organized.

We start observing riders on day 1 when they are about to take a round-trip. On each day but the first, q_i is the probability of taking a round-trip for rider i . Among riders, latent q_i is normally distributed censored at zero and one: $N_q \sim (\mu_q, \sigma_q)$. Given that realization of q_i , demand for trips is inelastic. For now, I assume that q_i is independent of the price of a subway ride. This is a reasonable approximation for the changes I study. In the appendix, I show how the results can be extended to the general case where q_i depends on the price of a subway ride. N_i denotes the number of rides that rider i actually takes. Hence, N_i is distributed binomially with parameters $2n$ and $q_i \sim B(2n, q_i)$.

On day t , rider i makes choice of deposit amount $D_{it} \in [0, 2np]$ to her MetroCard. The delivery of fares is immediate. Also, rider i decides whether or not to exert effort $E_{it} \in \{0, 1\}$ to remember to carry the same card to the next day on day t . Rider i incurs utility cost e_i if she exerts effort (i.e., $E_{it} = 1$). Every day t , rider i decides whether or not to incur cost C_{it} to make a deposit on her card. Let $C_{it} = 1$ if she decides to do so (i.e., if $D_{it} > 0$) and $C_{it} = 0$ otherwise (i.e., if $D_{it} = 0$)

The marginal utility of a subway or bus ride for rider i on day t is r_{it} . Assume riders are risk neutral and they have quasilinear utility. The marginal value of a dollar is normalized to one. Rider i 's money-metric utility from riding subways

Keep the same card for next day			Condition to Carry the Same Card When Card Balance is Zero
Save on card fee	Loss on Carrying cost		
No Card Fee	0	e_i	$e_i = 0$
With Card Fee	$q_i T$	e_i	$e_i < q_i T$

and buses on day $t \geq 2$ is:

$$U_{it} = q_i(r_{it} - D_{it} - E_{it}e_i - C_{it}c_i - T(1 - E_{i,t-1})) \quad (1.4)$$

When purchasing a new MetroCard with card fee T and take two rides in day 1, rider i 's objective is to choose a fare deposit policy $\{D_{it}, E_{it}, C_{it}\}$ that minimizes expected total cost:

$$E\left[\min_{\{D_{it}, E_{it}, C_{it}\}} (D_{i,1} + E_{i,1}e_i + C_{i,1}c_i + T + \sum_{t=2}^n q_i(D_{it} + E_{it}e_i + C_{it}c_i + T(1 - E_{i,t-1})))\right] \quad (1.5)$$

1.5.1 MODELING RESPONSE OF NEW METROCARD PURCHASES TO THE NEW CARD FEE

When will riders choose to carry the same MetroCard to the next day when the card balance is zero (i.e., when will riders choose to refill the same card)? The table below outlines the conditions under which a rider would choose to bring the same MetroCard under different policies when the card balance is zero. If no card fee is imposed, riders will discard their MetroCards when the card balance is zero if $e_i > 0$, i.e., if they have to incur a utility cost to remember to carry the same card to the next day. If riders are charged a fee for purchasing a new MetroCard, they will keep the same card for the next period when the card balance is zero if the decrease in utility they suffer from having to pay the new card fee is larger than the cost to remember to carry the same card for the next period.

1.5.2 OPTIMAL STRATEGY: PREPAY VERSUS NO PREPAY

To begin the analysis, I look at the case of $n = 2$. When $n = 2$, the rider's only decision is whether to prepay for rides in the second day. The table below outlines the conditions under which a rider would choose to prepay for rides. When no card fee is imposed, riders will prepay for future rides only if the fixed cost to make deposits is larger than the cost to remember to carry the same card for the next day and the risk of losing the deposit. If riders are charged a fee for purchasing a new MetroCard, riders will prepay for future rides only if the fixed cost to make deposits and pay the new card fee is larger than the cost to remember to carry the same card for the next day and the risk of losing the deposit.

Figure 1.9.19 shows the threshold of prepaying for rides in the second day when no card fee is imposed (Figure 1.9.19a) and when a card fee is imposed (Figure 1.9.19b), respectively. Before the new card fee was imposed, riders in region C only deposited the fare of the first day's rides since the decrease in utility they suffer from having to incur the fixed cost to make deposits in the second day is smaller than the cost to remember to carry the same card to the second day and the risk of losing the deposit. After the new card fee was imposed, these riders switched to prepaying for rides in the second day as the fixed cost to make deposits and pay the new card fee is larger than the cost to remember to carry the same card for the next period and the risk of losing the deposit. As a result, they risk losing $2p$ on expired MetroCards if they do not actually take rides in the second day (Table 1.9.9).

TOURISTS VERSUS LOCAL RESIDENTS

The model developed above supports the observation that major changes in deposit amounts and forgone balances came from MetroCards purchased by local residents rather than by tourists. Visitors are likely to have low probability of taking a round-trip in the second day (i.e., small q_i). Also, visitors may have low fixed cost to make deposits to a MetroCard (i.e., small c_i) since they may not understand they are missing a train or they are on vacation so their time is not very valuable.

	3*Cost Function	3*Decision Variables	Condition to Prepay for Rides in the Second Day
6*With Card Fee	$2*4p + e_i + c_i$	$E_{i,1} = C_{i,1} = 1 \text{ and } C_{i,2} = 1$ $D_{i,1} = 4p \text{ and } D_{i,2} = 0$	$e_i + (1 - q_i)2p$ $6^* < q_i c_i$
	$2*(1 + q_i)2p + e_i + (1 + q_i)c_i$	$E_{i,1} = C_{i,1} = C_{i,2} = 1$ $D_{i,1} = D_{i,2} = 2p$	
	$2*(1 + q_i)2p + (1 + q_i)c_i$	$E_{i,1} = 0 \text{ and } C_{i,1} = C_{i,2} = 1$ $D_{i,1} = D_{i,2} = 2p$	
6*With Card Fee	$2*4p + e_i + c_i + T$	$E_{i,1} = C_{i,1} = 1 \text{ and } C_{i,2} = 1$ $D_{i,1} = 4p \text{ and } D_{i,2} = 0$	If $e_i < q_i T$ $3*(1 - q_i)2p < q_i c_i$
	$2*(1 + q_i)2p + e_i + (1 + q_i)c_i + T$	$E_{i,1} = C_{i,1} = C_{i,2} = 1$ $D_{i,1} = D_{i,2} = 2p$	If $e_i \geq q_i T$ $e_i + (1 - q_i)2p$ $3^* < q_i(c_i + T)$
	$2*(1 + q_i)2p + (1 + q_i)c_i + (1 + q_i)T$	$E_{i,1} = 0 \text{ and } C_{i,1} = C_{i,2} = 1$ $D_{i,1} = D_{i,2} = 2p$	

The fixed cost to make deposits and pay the new card fee is smaller than the cost to remember to carry the same card for the next period and the risk of losing the deposit. As a results, visitors were not likely to increase their deposits after the new card fee was imposed.

Local residents, in contrast, are likely to have high probability of taking a round-trip in the second day (i.e., large q_i). Also, their fixed cost to make deposits to their MetroCards are likely to be high (i.e., high c_i) since their opportunity cost of missing a train and being late for work is high. The fixed cost to make deposits and pay the new card fee is larger than the cost to remember to carry the same card for the next period and the risk to lose rides fare if they do not actually take rides in the second day. As a results, local residents are more likely to increase their deposits after the new card fee was imposed.

N=3 CASE

In line with the $n = 2$ case, when $n = 3$, riders in region D switched from only depositing the fare of the first day's rides to prepaying for rides in the second day after the new card fee T was introduced. Riders in region E switched from prepaying for rides in the second day to prepaying for rides in the second and third day after the new card fee was imposed (Figure 1.9.2ob). Leftover balances on expired

cards will increase accordingly.

1.5.3 N=30 CASE

When $n = 30$, according to the model, there should be thirty-one regions ranging from no prepay for future rides to prepay for rides in the next 30 days (Figure 1.9.21). In reality, riders either choose the "other amount" option and manually enter \$4.5 (or \$5) as the desired deposit amount, or choose one of the payment prompts (i.e., suggested deposit amounts) on Touchscreen (Figure 1.9.22a and 1.9.22c). Therefore, there should be four regions as in Figure 1.9.23a and Figure 1.9.23b, corresponding to the "other amount" choice, "the first default choice", "the second default choice", and "the third default choice", respectively.

Therefore, instead of $D_{it} \in [0, 2np]$, rider i makes choice of deposit amount $D_{it} \in \{5, 10, 20, 40\}$ to her new MetroCard. When purchasing a new MetroCard with card fee T and take two rides in day 1, rider i 's objective is to choose a fare deposit policy $\{D_{it}, E_{it}, C_{it}\}$ that minimizes expected total cost:

$$E\left[\min_{D_{it} \in \{5, 10, 20, 40\}, E_{it}, C_{it}} (D_{i,1} + E_{i,1}e_i + C_{i,1}c_i + T + \sum_{t=2}^n q_t (D_{it} + E_{it}e_i + C_{it}c_i + T(1 - E_{i,t-1})))\right] \quad (1.6)$$

When riders are charged a fee for purchasing a new MetroCard, riders in region A' switched from choosing "other amount" choice to the first suggested deposit amount (\$10); riders in region B' switched from the first suggested deposit amount to the second suggested deposit amount (\$20); while riders in region C' switched from the second suggested deposit amount (\$20) to the third suggested deposit amount (\$40) (Figure 1.9.23c). Consistent with the predictions of the model, the number of deposits in suggested amounts jumped up significantly after the new card fee was imposed (Figure 1.9.24)

1.5.4 PARAMETER ESTIMATES

This subsection summarizes the construction of the likelihood function and the estimation procedure; complete details are in Appendix D. I use changes of deposits

in suggested amounts to calibrate the values of three groups of unobservables that must be integrated out: individual specific unobserved heterogeneity, including e_i , c_i , and q_i . Computational difficulties in estimation mainly come from the model's high dimensional unobserved heterogeneity which requires many evaluations of the likelihood function.

The estimates for the 7 parameters discussed in the main text are shown in Table 1.9.1. Recall that the parameters of the pre-censoring distributions are used to describe the censored distributions. The price coefficient β is 0.12, which indicates that a price increase from \$0.00 to \$2.50 per ride decreases usage by 25 percent. The next six parameters characterize the normal distribution of riders' riding probability q_i , fixed cost to purchase MetroCards c_i , and card carrying cost e_i . The average rider's riding probability q_i is estimated to be 0.62. The average riders' fixed cost c_i is estimated to be \$1.45, while the average rider's carrying cost e_i is \$0.83. The population standard deviations of q_i , c_i , and e_i (σ_{q_i} , σ_{c_i} , and σ_{e_i}) are 0.34, \$0.56, and \$0.53, respectively.

1.5.5 COUNTERFACTUAL ANALYSIS

FIXED-PRICE COUNTERFACTUAL: IMPACT OF DEFAULT CHOICES

The MTA has been criticized for having payment prompts (i.e., default choices of deposit amounts) on TouchScreen of MetroCard vending machines that do not give an exact number of rides: If a rider chooses one of the payment prompts on the Touchscreen, she will end up with a card that has leftover change because none of those suggested amounts (a \$9.00 MetroCard with a \$.45 bonus, a \$19.00 card with a \$.95 bonus, or a \$39.00 card with a \$1.95 bonus) are divisible by \$2.50, the base fare for every subway ride.

Some people have proposed a software change as shown in Figure 1.9.27. Now all the payment prompts on Touchscreen give an exact number of rides: a \$9.55 MetroCard with a \$.48 bonus gives exactly 4 rides, a \$19.05 card with a \$.95 bonus gives 8 rides, while a \$38.10 card with a \$1.91 bonus gives 16 rides.

I simulate the change in expired fare revenue that results from the introduction

of the \$1 new card fee with this new payment prompts on Touchscreen while holding base fare for every subway ride fixed. I construct this counterfactual simulation in the sense that I hold fixed the number of riders and their riding patterns. The model predicts that, even with all the payment prompts give an exact number of rides, expired fare revenue will still increase by 39.52% (\$19.76 million)

OPTIMAL NEW CARD FEE

I simulate the optimal amount of new card fee that maximizes the MTA's profit while holding the payment prompts on Touchscreen and base fare for every subway ride fixed. I construct this counterfactual simulation in the sense that I hold fixed the number of riders and their riding patterns. The model predicts that the optimal amount of new card fee is \$4.35.

1.6 ALTERNATIVE MECHANISMS

This paper provides evidence of the impacts of the new card fee that is consistent with a rational model highlighting the importance of the cost of effort to remember to bring the same MetroCard for future rides, the fixed cost of making deposits to MetroCards, and consumer uncertainty about future rides. However, transaction cost is not the only possible explanation for the observed increase in deposit amounts and the forgone balances on expired cards. This section investigates other potential theories or mechanisms that might explain the results described above.

1.6.1 PERSUASION

The screens of vending machines show three suggested payment amounts (\$10, \$20, \$40), along with bonuses. This could potentially push riders to make higher deposits. However, the screen displayed the same \$10 and \$20 suggested amounts before and after the card fee went into effect; only the third suggested amount changed from \$50 to \$40 (Figure 1.9.22a and 1.9.22c). Since the main changes in deposit amounts were switching from \$5 to \$10 and \$20 with no changes of the

\$10 and \$20 suggested amounts, persuasion, solely, is not likely to account for the observed changes in new purchases.

1.6.2 AVOIDANCE OF COINS

When riders make deposits to MetroCards using cash at vending machines, they may receive as many as 20 quarters (i.e., \$4) as change if they purchase new MetroCards with a \$10 bill and only make a deposit of round-trip fare to the card (i.e. \$10 - \$5 fare cost - \$1 new card fee). If some riders prefer not to have a lot of coins as change, they may start making \$10 or \$20 cash deposits during new pay-per-ride card purchases at vending machines.

However, this explanation, solely, cannot explain the changes observed in debit card payments. Before the new card fee, more than 30% of riders made only \$5 deposits (round-trip fare) when they purchased new pay-per-ride MetroCards (Figure 1.9.16c). Since \$5 has never been one of the suggested deposit amounts on the screen, this means that many riders used to hit the "other amount" option and manually enter \$5 as the desired deposit amount. These riders can still choose the "other amount" option and manually enter \$6 (\$5 fare + \$1 new card fee) after the new card fee. But the percentage of \$5 deposits dropped to below 20% after the new card fee (Figure 1.9.16c).

1.6.3 QUICK FIX

Some riders may want to make larger deposits when purchasing new cards because larger deposits could minimize (or alleviate) the perceived cost of the new card fee. For instance, riders may want to compensate for the absolute monetary cost of the new card fee by bonuses on pay-per-ride deposits. The existence of a quick fix can largely explain why larger increases on new card purchases were mainly on cards that had no subsequent refill activities: Before the new card fee, more than 60% of the deposits on new cards that had no subsequent refill activities were \$4.50 (the fare of a round-trip) while more than 70% of the deposits on new cards with refill transactions were already at least \$10 or \$20.

However, this mechanism is not likely to explain the persistent increase in deposit amounts on new cards and forgone balances on expired cards. If riders only increased their deposit amounts to alleviate the perceived cost of the new card fee but did not incur utility cost to keep the same card, their forgone balances on lost or expired cards would increase. Over time, they should decrease their deposit amounts back to the pre-fee level. It has been three years since the introduction of the new card fee. I did not observe decrease in deposit amounts or forgone balances over time.

1.6.4 COMMITMENT DEVICE

Many riders may use larger deposits as a means to push themselves to remember to carry the same card and avoid paying the new card fee. As the forgone balances on expired cards increased by 50%, the larger deposits is clearly a failed commitment device. Literature shows that people stop using a commitment device that does not work (Brocas and Carrillo 3; Carrillo and Mariotti 6; Vigna and Malmendier 41). If this explanation is the primary one, we should see deposits bounce back to lower level. It has been three years since the the introduction of the new card fee. No decrease in deposit amounts or forgone balances was observed.

1.7 ROBUSTNESS TESTS

In this section, I perform several robustness tests to further validate the explanations for the findings.

SAMPLE

The same results remain no matter whether I use the whole sample or sub-samples (e.g., one week before and after the implementation of the new card fee, or randomly picked stations).

MINOR DECREASE IN RIDERSHIP

There was only a minor decrease in ridership after the imposition of the new card fee (Figure 1.9.10).

CROSS-TYPE SWITCH IN PURCHASES OF METROCARDS

One concern with interpreting changes in deposit patterns as a response to the new card fee is that there may be cross-type switches from pay-per-ride to unlimited-ride cards or single-ride tickets due to the variations in fare hike across different types of MetroCards.

Table 1.9.13 lists monthly revenue before and after the new card fee. There was no significant change in the percent of revenue from pay-per-ride versus that from time-based cards. Table 1.9.11 and Table 1.9.12 show that the threshold for switching from pay-per-ride to unlimited-ride cards barely changed after the fare hikes in March 2013. This suggests that differences in fare hikes across different types of MetroCards are unlikely to be driving these results. These findings confirm that there is minimal cross-type switching.

CROSS-TYPE SWITCH FROM PAY-PER-RIDE TO SINGLE-RIDE TICKETS

A seemingly reasonable implication from the imposition of the new card fee is that the sales of single-ride tickets are likely to rise since some riders may switch from pay-per-ride to single-ride tickets, especially when they forget their regular MetroCards but need to take a round trip. However, monthly sales of single-ride tickets actually decreased after the new card fee went into effect.

CROSS-TYPE SWITCH FROM PAY-PER-RIDE TO TIME-BASED CARDS

Table 1.9.11 compares the costs of trips using pay-per-ride versus 7-day-unlimited cards. The red oval circles mark the threshold number of trips needed to switch from pay-per-ride to 7-day-unlimited before versus after the new card fee implementation. Before the new card fee, a rider would only save more money buying

a 7-day-unlimited than buying a pay-per-ride card if he takes more than 14 trips within one week. After the new card fee, this threshold was 13, only decreasing by one trip.

Table 1.9.12 compares the costs of trips using pay-per-ride versus 30-day-unlimited cards. The red oval circles mark the threshold number of trips needed to switch from pay-per-ride to 30-day-unlimited before versus after the new card fee implementation. Again, the threshold for switching from pay-per-ride to 30-day-unlimited cards only slightly changed after the fare hikes in March 2013.

NO SIGNIFICANT INCREASES IN DEPOSIT AMOUNTS BEFORE VERSUS AFTER THE FARE HIKE IN 2009 OR 2015

There may be some concern that changes in deposit patterns are solely due to the fare hike in 2013. Figure 1.9.31 and 1.9.32 plots the histogram for deposits on pay-per-ride MetroCard purchases before and after the fare hike in 2009 and 2015, respectively. For both new and refill purchases, there were barely any changes in deposit amounts. Hence, the observed changes in deposit patterns after the new card fee was not likely driven primarily by the \$0.25 increase of base fare.

1.8 IMPLICATIONS OF THE MODEL

In this section, I discuss implications of the model developed above. My evaluation of new card fee on MetroCard purchases could be insightful in other contexts, such as mobile and online prepaid services as well as general purpose reloadable prepaid cards.

1.8.1 MOBILE AND ONLINE PREPAID SERVICES

The importance of the cost of effort to remember to carry the same card (e_i) and the fixed cost to make deposits to cards (c_i) highlighted in this model may explain the prevalence of required minimum deposit amounts in the online or mobile prepaid services such as E-ZPass and Skype. When authorities or firms adopt

online or mobile payments with an automatic deduction from bank accounts or credit cards, consumers' fixed cost to make payments c_i converges to zero. As a result, consumers switch from prepaying for future consumption to pay only for consumption in current period (pay-as-you-go). To maintain the benefit from unused account balances, most online or mobile prepaid services providers have required minimum deposit amounts and use suggested deposit amounts to attract even more deposits. For skype credit, the minimum purchase amount is \$10 (Figure 1.9.33a) and consumers can only select from \$10 or \$25 to buy.

1.8.2 GENERAL PURPOSE RELOADABLE CARDS

Findings in this model are consistent with findings in the prepaid card industry. Currently, over 23 million U.S. adults use general purpose reloadable cards such as green dot cards each month. Consumers loaded \$672 billion on these cards in 2013. Since most general purpose reloadable card users are low-income and excluded from the financial mainstream, they do not have other banking options (Urahn et al. 38). Most card companies charge a new card fee (or card activation fee) as high as \$9.95. When a prepaid card issuer has market power, a new card fee (or card activation fee) could push consumers to prepay more for future consumption.

1.9 DISCUSSIONS AND CONCLUSIONS

How do consumers respond to the imposition of a new card fee? In this paper, I show how a new card fee for prepaid transit cards induced riders to put more money on cards and lose more when these cards expired. I present a novel transaction-level data set from the public transit system in New York City that allows me to analyze changes in deposit amounts and the forgone balance on expired cards. After the introduction of a \$1 new card fee, the net monthly outstanding balance from transit card deposits increased dramatically, with riders lending an extra \$150 million, on an annual basis, to the MTA. Moreover, over \$20 million of the increased

balances in the first year were never redeemed and escheated to the MTA when these cards expired.

There are five main empirical findings regarding changes in deposit amounts and forgone balances on expired cards. First, the changes largely came from new card purchases, not from refills. Second, the changes were mainly from new cards that would not have subsequent refill activities, not from cards that showed subsequent refill activities. Third, the changes mainly came from cash or debit card payments rather than from credit card payments. Fourth, among cash payments, the changes were mostly from payments made at vending machines rather than from payments made at manned booths (tellers). Fifth, the response to the new card fee was larger in low-income neighborhoods than in high-income neighborhoods.

One leading explanation highlights the importance of the cost of effort to remember to bring the same MetroCard for future rides, the fixed cost of making deposits to MetroCards, and consumer uncertainty about future rides. When MetroCards were free, riders with a low fixed cost of making deposits to MetroCards chose not to incur the cost of effort to remember to bring the same card for future days. They deposited only a small amount of money on cards and purchased a new MetroCard each period if needed. After the new card fee was imposed, many riders switched to refilling existing cards since their cost of effort to remember to carry the same card was smaller than the new card fee. They started making larger deposits to save on the fixed cost of making deposits to cards. Because consumers are uncertain about future rides, these riders on average had higher leftover balances after the card expiration dates.

The results of this study are likely to generalize to 8.3 million Americans who use public transit to go to work. 10.65 billion passenger trips were taken on transit systems in 2013 (American Public Transportation Association 2014). Therefore, both in terms of monetary magnitude and in terms of population involved, the new card fee on transit card has a significant economic impact. These findings are also consistent with findings in the prepaid card industry. Since most general purpose reloadable card users are low-income and excluded from the financial mainstream, they do not have other banking options (Urahn et al. 38).

These findings also have implication about regressive ways to raise money. Mainly due to data limitations, studies on regressive fees focus on the portion of fee revenue collected from low-income people. Here I provide evidence that whether or not the card fee itself is regressive, it may push low-income consumers to behave in ways that cost them money, especially in monopolistic market. The authorities should take into consideration the possible additional responses from low-income people when imposing a fee.

Findings in this paper predict that riders' fixed cost to make payments converge to zero with mobile payments and automatic deductions from bank accounts or credit cards. As a result, riders will switch from prepaying for future consumption to pay only for consumption this period (pay-as-you-go). A natural follow-up works is to conduct field experiments to examine the effects of mobile payments on riders' deposit amounts.

Table 1.9.1: Recent History of MTA Policy Changes

Date	Jun 28, 2009 - Dec 29, 2010	Dec 30, 2010 - March 2, 2013	March 3, 2013 - March 21, 2015	March 22, 2015 -
Base fare (\$)	2.25	2.25	2.50	2.75
7-day-unlimited (\$)	27	29	30	31
30-day-unlimited (\$)	89	104	112	116.50
Single-ride tickets (\$)	2.25	2.50	2.75	3.00
\$1 Card Fee on new MetroCard purchase	No	No	c2 Yes* c2	Yes
Bonus for Pay-Per-Ride, %(threshold)	15%(\$8)	7%(\$10)	5%(\$5)	11%(\$5.50)

* The main policy change for this paper is the imposition of \$1 card fee on new MetroCard purchases.

Table 1.9.2: Empirical Features And Possible Explanations

After the new card fee	Persuasion of Machine Screen Info	Avoidance of Coins	Quick Fix	Commitment Device	Transaction Cost
Finding 1 A large drop in number of new MetroCard sold	Yes	Yes	Yes	Yes	Yes
Finding 2 Monthly outstanding balance from pay-per-ride deposits jumped by 1/3	No	Yes	Yes	Yes	Yes
Finding 3 Increased expired fare revenue	No	Yes	Yes	Yes	Yes
Finding 4 No decrease in deposit amounts or forgone balances over time	Yes	No	No	No	Yes
Finding 5 Increase in deposit amount on new purchases	No	Yes	Yes	Yes	Yes
Finding 6 Larger increase in deposits on cash or debit card payments	No	No	Yes	Yes	Yes
Finding 7 Larger increase in deposits on cards with no subsequent refill activities	No	Yes	Yes	Yes	Yes
Trivial change in deposit amount on cards with refill activities	No	No	Yes	Yes	Yes

Table 1.9.3: Monthly Demand for New Cards Before And After the new card fee
(In millions)

	Before (Jan 2009 to Feb 2013)		After (Mar 2013 to Jun 2015)		<i>p-value</i> (5)
	Number (1)	Percent (2)	Number (3)	Percent (4)	
Total	7.70 (0.55)	1	2.32 (0.39)	1	
Pay-per-ride	5.84 (0.47)	0.758 (0.011)	1.826 (0.28)	0.791 (0.01)	<0.0001*
30-day-unlimited	0.58 (0.055)	0.076 (0.01)	0.15 (0.042)	0.063 (0.01)	<0.0001*
7-day-unlimited	1.26 (0.079)	0.163 (0.005)	0.339 (0.076)	0.144 (0.01)	<0.0001*
<i>N</i>	104	104	112	112	

Standard deviations in parentheses

Table reports mean values of each variable

* *p*-value of mean difference in percent sales before and after the new card fee was implemented.

Table 1.9.4: Effect of New Card Fee on Daily Total New MetroCard Sales

sale₁₃S[table-format=1.2,table-column-width=25mm] Outcome variable: total new card sales on daily basis from January 1, 2013 to April 30, 2013 Robust standard errors in parentheses.

+ *p* < 0.1 * *p* < 0.05, ** *p* < 0.01, *** *p* < 0.001

Table 1.9.5: Effect of New Card Fee on Daily New MetroCard Sales By Different Types of Cards

sale₂₃S[table-format=1.2,table-column-width=25mm] Outcome variable: new card sales on daily basis from January 1, 2013 to April 30, 2013 Robust standard errors in parentheses.

+ *p* < 0.1 * *p* < 0.05, ** *p* < 0.01, *** *p* < 0.001

Table 1.9.6: Effect of New Card Fee on Deposit Amounts: RD Regressions

table10.tex3S[table-format=1.2,table-column-width=20mm] Dependent variable: deposit amount at new pay-per-ride purchases. Standard Deviation in parentheses and clustered at station level. The sample is limited to all deposit transactions made to new pay-per-ride MetroCards from January 1, 2013 to April 30, 2013. The coefficients reported here are based on default bandwidth with local quadratic described in Calonico et. al(2014). + $p < 0.1$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.9.7: Median Neighborhood Income and Pay-per-ride Deposit Amounts

mta_patial_3.tex4S[table-format=1.2,table-column-width=15mm]Dependentvariable : transaction –

Table 1.9.8: Median Neighborhood Income and Ridership At Station-level

mta_patial_2.tex6S[table-format=1.2,table-column-width=10mm]Dependentvariable : weeklyridesfrom

Table 1.9.9: Welfare Analysis: Cost of Rides for $n = 2$ Case

	No Card Fee	With Card Fee	Changes in Utility
A	$-(c_i + 2p) - q_i(c_i + 2p)$	$-(c_i + 2p) - e_i - q_i(c_i + 2p)$	$-e_i$
B	$-(c_i + 2p) - q_i(c_i + 2p)$	$-(c_i + 2p) - q_i(c_i + 2p + T)$	$-q_i T$
C	$-(c_i + 2p) - q_i(c_i + 2p)$	$-(c_i + 4p) - e_i$	$-e_i - c_2(1 - q_i)2pc_2 + q_i c_i$
D	$-(c_i + 4p) - e_i$	$-(c_i + 4p) - e_i$	No change

Table 1.9.10: Parameter Estimates

Parameter	Estimate
β	Price Sensitivity 0.20
$E[q_i]$	Mean of Probability 0.62
$\sigma[q_i]$	Standard Error of Probability 0.34
$E[c_i]$	Mean of Fixed Cost 1.45
$\sigma[c_i]$	Standard Error of Fixed Cost 0.56
$E[e_i]$	Mean of Carrying Cost 0.83
$\sigma[e_i]$	Standard Error of Carrying Cost 0.53

Table 1.9.11: Comparison of ridership cost Pay-per-ride versus 7-day-unlimited

Rides	Dec 30, 2010 to Mar 2, 2013 7% bonus for every \$10		Mar 3, 2013 to Mar 21, 2015 5% bonus for every \$5	
	Pay-per-ride	7-day-unlimited	Pay-per-ride	7-day-Unlimited
1	2.25	29	2.5	30
2	4.5	29	5.0	30
...
12	25.23	29	28.57	30
13	27.34	29	30.95	30
14	29.44	29	33.33	30

Table 1.9.11 compares the costs of trips using pay-per-ride versus 7-day-unlimited cards. The red oval circles mark the threshold number of trips needed to switch from pay-per-ride to 7-day-unlimited before versus after the new card fee was implemented.

Table 1.9.12: Comparison of ridership cost Pay-per-ride versus 30-day-unlimited

Rides	Dec 30, 2010 to Mar 2, 2013 7% bonus for every \$10		Mar 3, 2013 to Mar 21, 2015 5% bonus for every \$5	
	Pay-per-ride	30-day-unlimited	Pay-per-ride	30-day-Unlimited
1	2.25	104	2.5	112
2	4.5	104	5.0	112
...
47	98.83	104	111.90	112
48	100.93	104	114.29	112
49	103.04	104	116.67	112
50	105.14	104	119.05	112

Table 1.9.12 compares the costs of trips using pay-per-ride versus 30-day-unlimited cards. The red oval circles mark the threshold number of trips needed to switch from pay-per-ride to 30-day-unlimited before versus after the new card fee was implemented.

Table 1.9.13: Monthly Revenue Before and After the New Card Fee (In Millions \$)

	Before (Jan 2011 to Feb 2013)		After (Mar 2013 to Jun 2015)	
	Value (1)	Percent (2)	Value (3)	Percent (4)
Total	269.47 (8.82)	1	310.00 (1.36)	1
Pay-per-ride	159.02 (6.92)	0.59 (0.012)	179.97 (9.39)	0.58 (0.013)
New	53.98 (4.04)		20.68 (2.94)	
Refill	105.04 (4.39)		159.28 (7.63)	
30-day-unlimited	64.81 (3.78)	0.241 (0.015)	69.56 (4.40)	0.225 (0.014)
New	60.56 (5.67)		16.68 (4.64)	
Refill	4.25 (3.94)		52.88 (5.90)	
7-day-unlimited	38.61 (2.18)	0.143 (0.005)	51.96 (4.15)	0.168 (0.009)
New	36.46 (2.28)		10.17 (2.26)	
Refill	2.15 (2.01)		41.79 (4.71)	
Reduced fares	5.63 (0.25)	0.022 (0.001)	6.91 (0.40)	0.021 (0.001)
<i>N</i>	104	104	112	112

Standard deviations in parentheses

Table reports mean values of each variable

Table 1.9.13 shows the monthly revenue from sales of different types of MetroCards before and after the new card fee. There was no significant change in the percent of revenue from pay-per-ride versus that from time-based cards.

Figure 1.9.1: MetroCard Purchase Venues

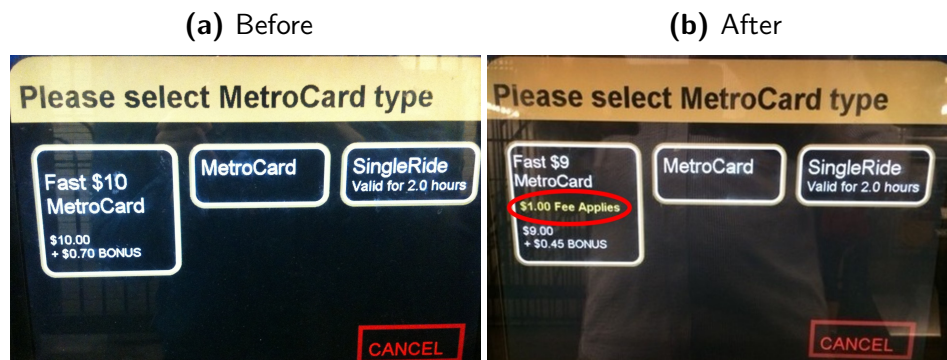
(a) Vending Machine

(b) Manned Booth



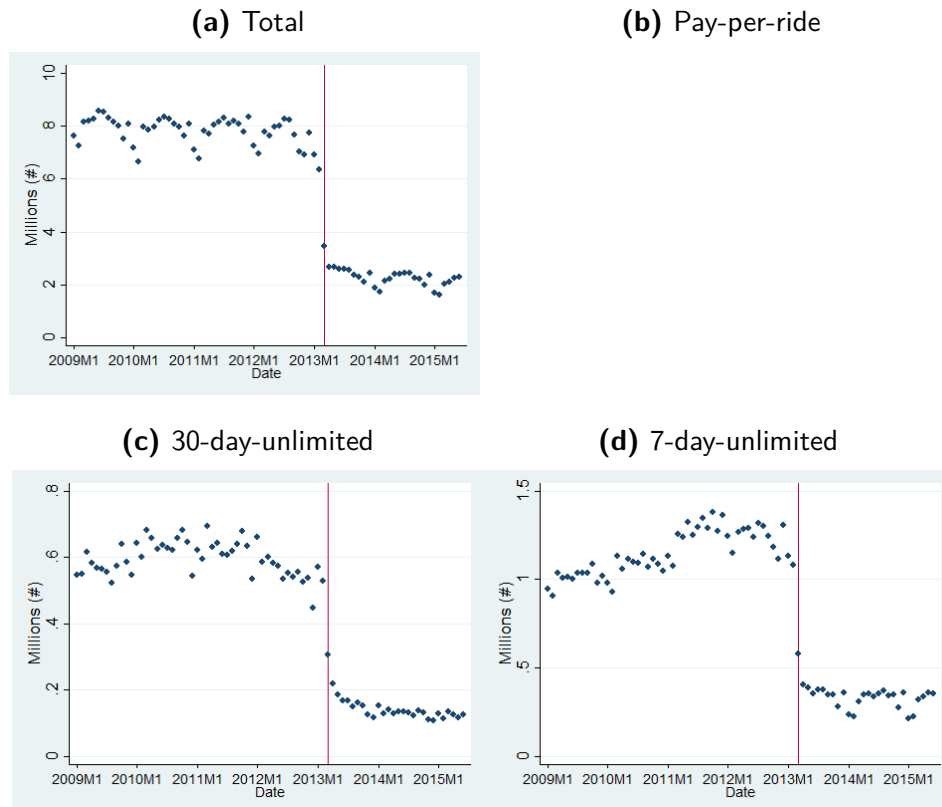
Source: Figure 1.9.1a www.fastcompany.com; Figure 1.9.1b Benjamin Kabak on Flickr

Figure 1.9.2: First Screen Snapshot on Vending Machine Purchases Before and After the New Card Fee



Note: Figure 1.9.2 shows the first screen snapshot of the vending machines before (1.9.2a) and after (1.9.2b) the new card fee went into effect, respectively.

Figure 1.9.3: Monthly New MetroCard Sales From January 2009 to June 2015



Note: Figure 1.9.3 plots monthly sales of new MetroCards from January 2009 to June 2015, broken out for different types of MetroCards. The vertical line marks the month when the new card fee was implemented.

Figure 1.9.4: Daily Sales of New MetroCards (All Types) from January 1, 2013 to April 30, 2013

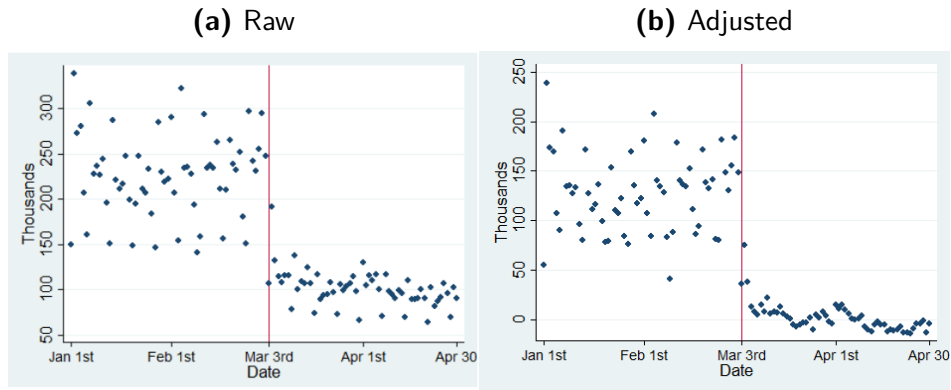
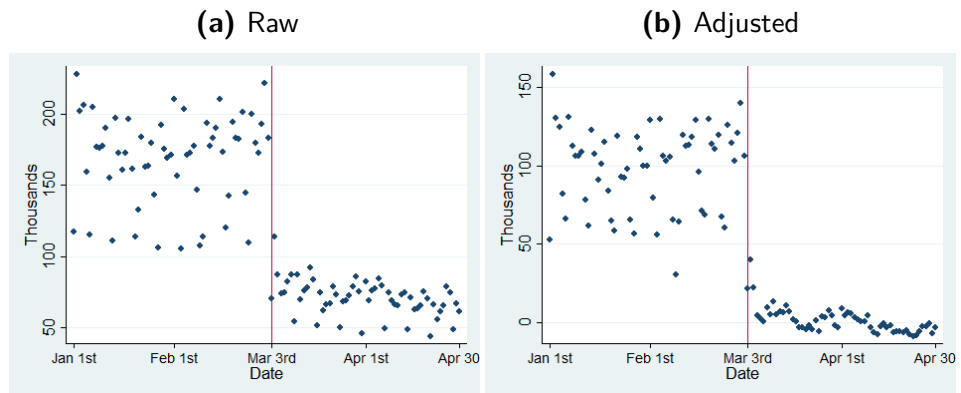


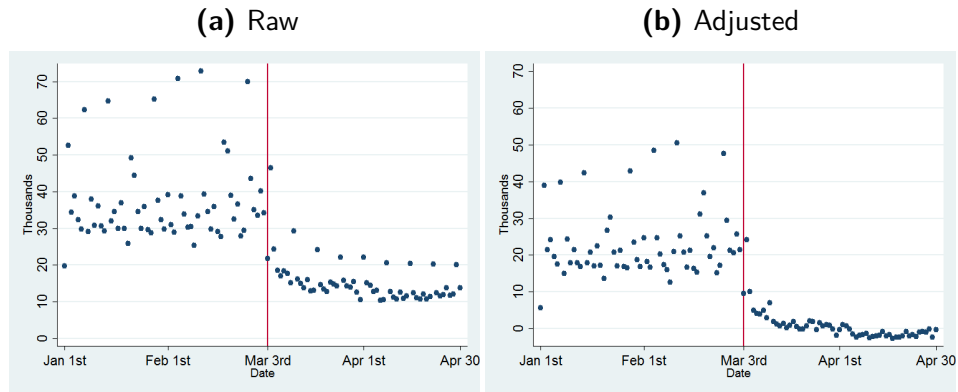
Figure 3.0.1a plots daily sales of new MetroCards (all types) from January 1, 2013 to April 30, 2013. Figure 3.0.1b plots residual of daily sales from day of week fixed effect. The vertical line marks the day when the new card fee was implemented.

Figure 1.9.5: Daily Sales of New Pay-per-ride MetroCards from January 1, 2013 to April 30, 2013



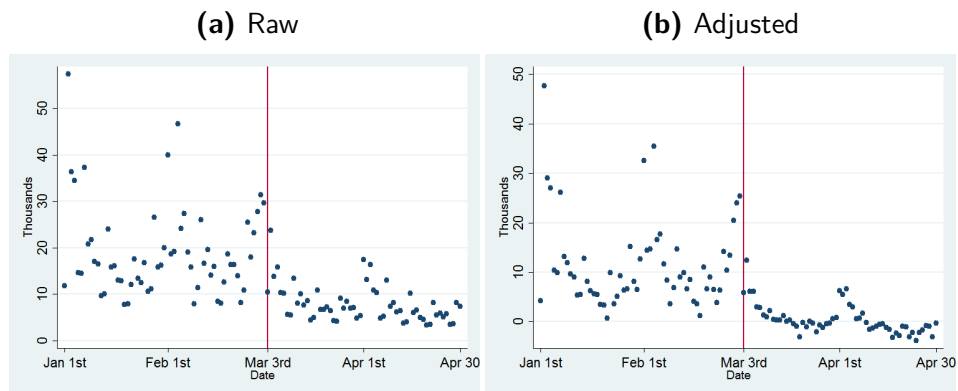
Note: Figure 1.9.5a plots daily sales of new pay-per-ride MetroCards from January 1, 2013 to April 30, 2013. Figure 1.9.5b plots residual of daily sales from day of week fixed effect. The vertical line marks the day when the new card fee was first imposed

Figure 1.9.6: Daily Sales of New 7-day-unlimited MetroCards from January 1, 2013 to April 30, 2013



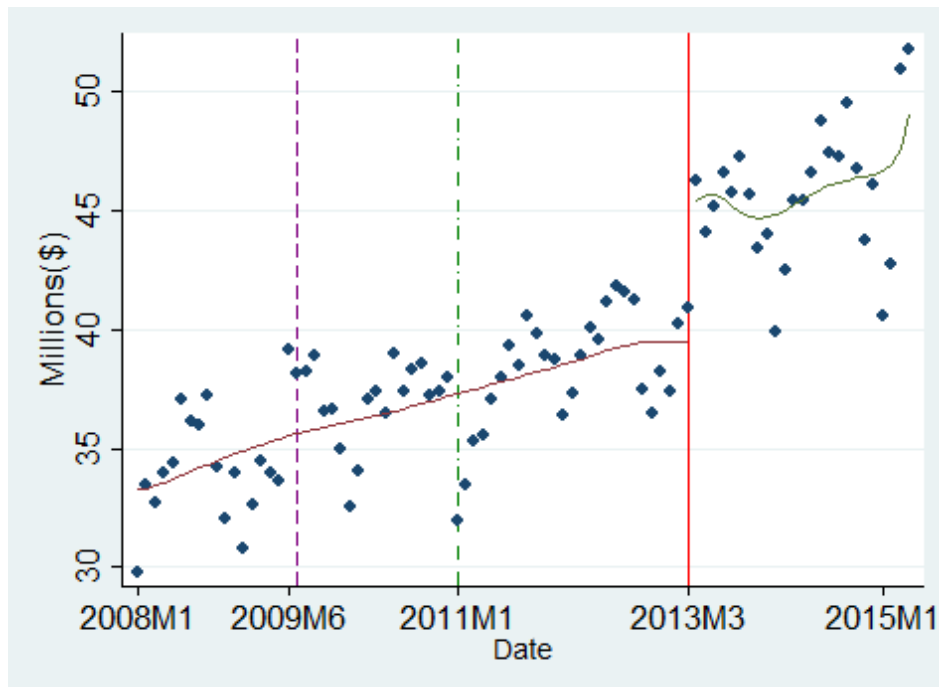
Note: Figure 1.9.6a plots daily sales of new 7-day-unlimited MetroCards from January 1, 2013 to April 30, 2013. Figure 1.9.6b plots residual of daily sales from day of week fixed effect. The vertical line marks the day when the new card fee was first imposed

Figure 1.9.7: Daily Sales of New 30-day-unlimited MetroCards from January 1, 2013 to April 30, 2013



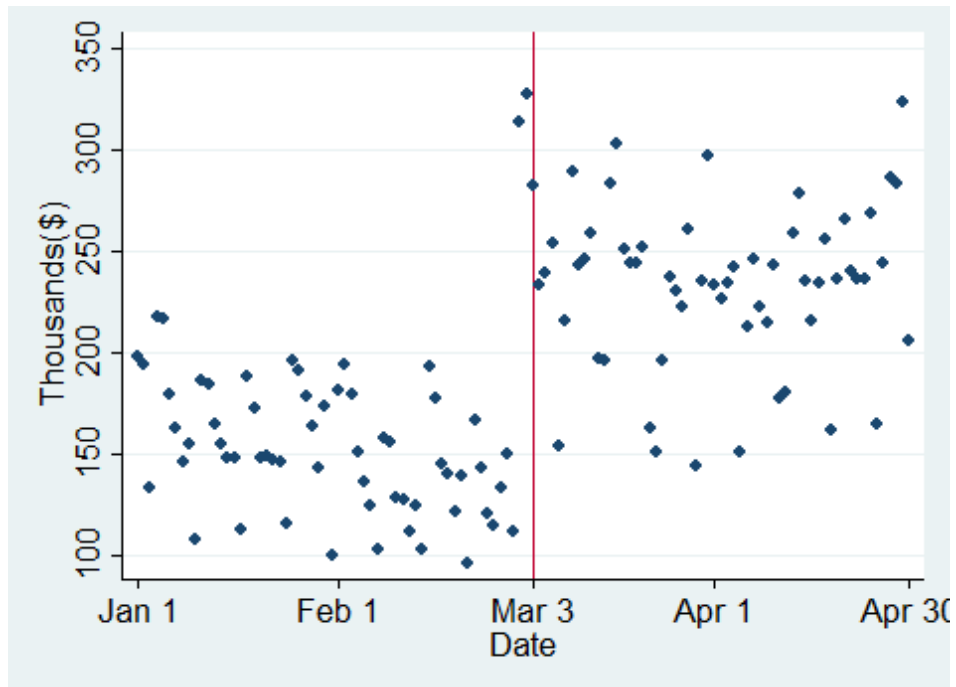
Note: Figure 1.9.7a plots daily sales of new 30-day-unlimited MetroCards from January 1, 2013 to April 30, 2013. Figure 1.9.7b plots residual of daily sales from day of week fixed effect. The vertical line marks the day when the new card fee was first imposed

Figure 1.9.8: Monthly Outstanding balance from Pay-per-ride Deposits from January 2008 to April 2015



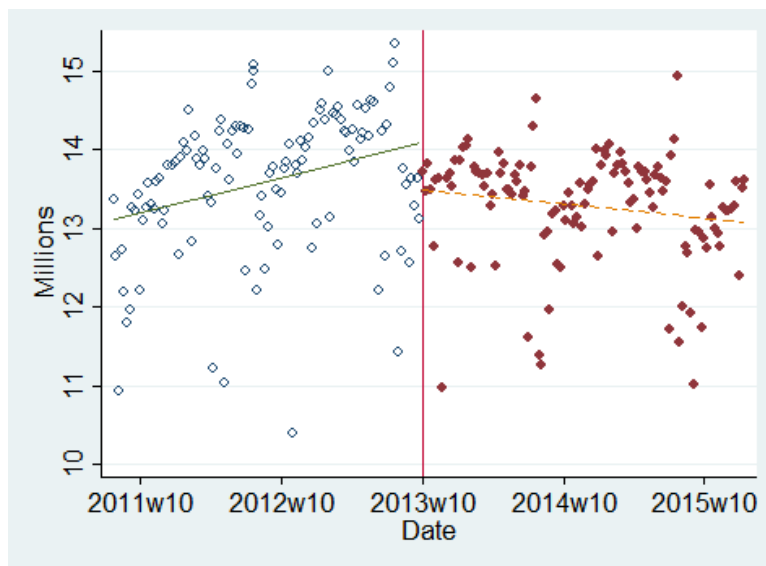
Note: Figure 1.9.8 plots monthly outstanding balance from pay-per-ride deposits from January 2008 to April 2015. The aggregate monthly outstanding balance that riders carried on their MetroCard is defined as the difference between the total amount they loaded on the cards and reductions caused by swipes at turnstiles. The first vertical line (purple) marks the month when the 2009 fare hike went into effect, the second vertical line (green) marks the month when the 2010 fare hike went into effect, and the third vertical line (red) marks the month when the new card fee was first imposed (also the month when the 2013 fare hike went into effect). The lines plot fitted values of locally weighted regressions (using Stata's `lowess` command) of outstanding balance on time.

Figure 1.9.9: Aggregate Forgone Balances on Pay-per-ride MetroCards Initially Purchased Between January 1, 2013 and April 30, 2013



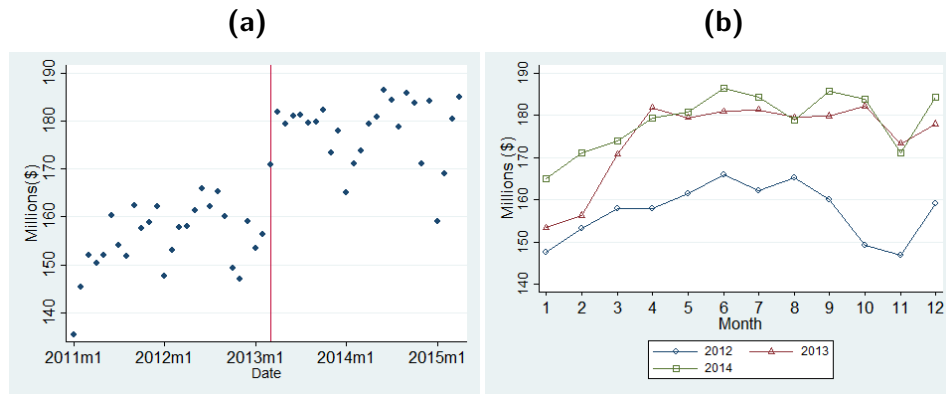
Note: Figure 1.9.9 plots the aggregate forgone balances on pay-per-ride MetroCards initially purchased between January 1, 2013 and April 30, 2013. The forgone balances on pay-per-ride MetroCards is defined as the unspent balances on expired MetroCards. The vertical line marks the day when the new card fee was first imposed.

Figure 1.9.10: Weekly Total Number of MetroCard Swipes from January 2011 to May 2015



Note: Figure 1.9.10 plots weekly total number of MetroCard swipes from January 2011 to May 2015. The vertical line (red) marks the week when the new card fee went into effect.

Figure 1.9.11: Monthly Revenue from Pay-per-ride Deposits from January 1, 2011 to April 30, 2015



Note: Figure 1.9.11a plots monthly revenue of pay-per-ride deposits from January 2011 to April 2015. The vertical line (red) marks the month when the new card fee went into effect. Figure 1.9.11b plots year-to-year monthly revenue of pay-per-ride deposits from January 2012 to December 2014.

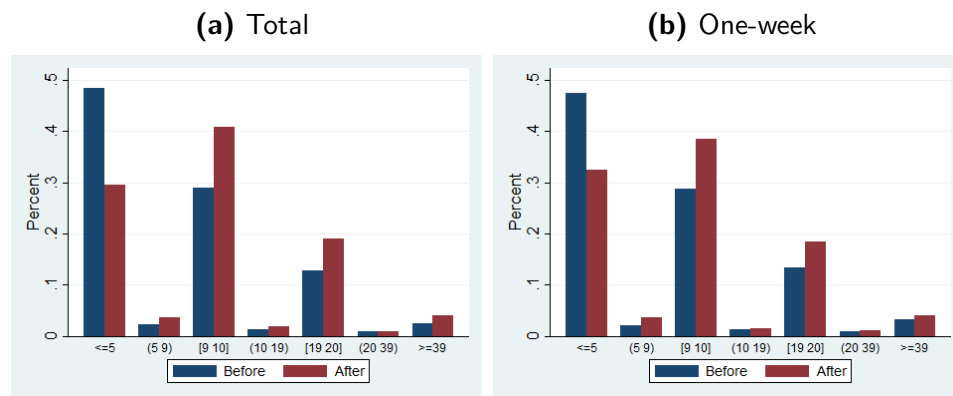
Figure 1.9.12: Deposits on New Pay-per-ride MetroCard Purchases Before versus After the New Card Fee (From January 1, 2013 To April 30, 2013): Cumulative Distribution Function

(a) Total

(b) One-week

Note: Figure 1.9.12 plots the cumulative distribution function (CDF) for deposit amounts on new pay-per-ride MetroCard purchases before versus after the new card fee. Figure 1.9.12a used all the deposit transactions from January 1, 2013 to April 30, 2013. Figure 1.9.12b plots deposits of purchases within one week before and after the new card fee.

Figure 1.9.13: Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013): Histogram

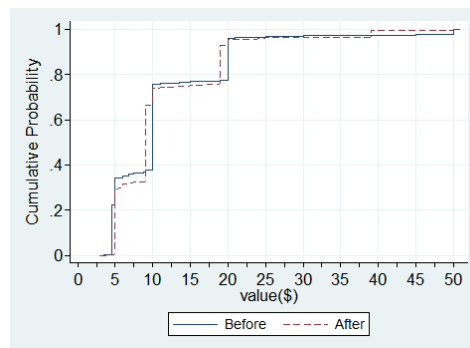


Note: Figure 1.9.13 plots the histogram of deposit amounts on new pay-per-ride MetroCard purchases before versus after the new card fee. Figure 1.9.13a used all the deposit transactions from January 1, 2013 to April 30, 2013. Figure 1.9.13b plots deposits of purchases within one week before and after the new card fee.

Figure 1.9.14: Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013): Cumulative Distribution Function

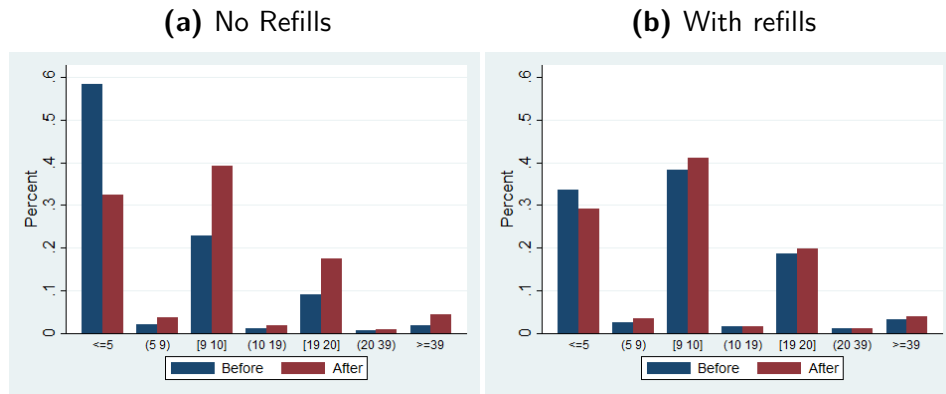
(a) No Refills

(b) With Refills



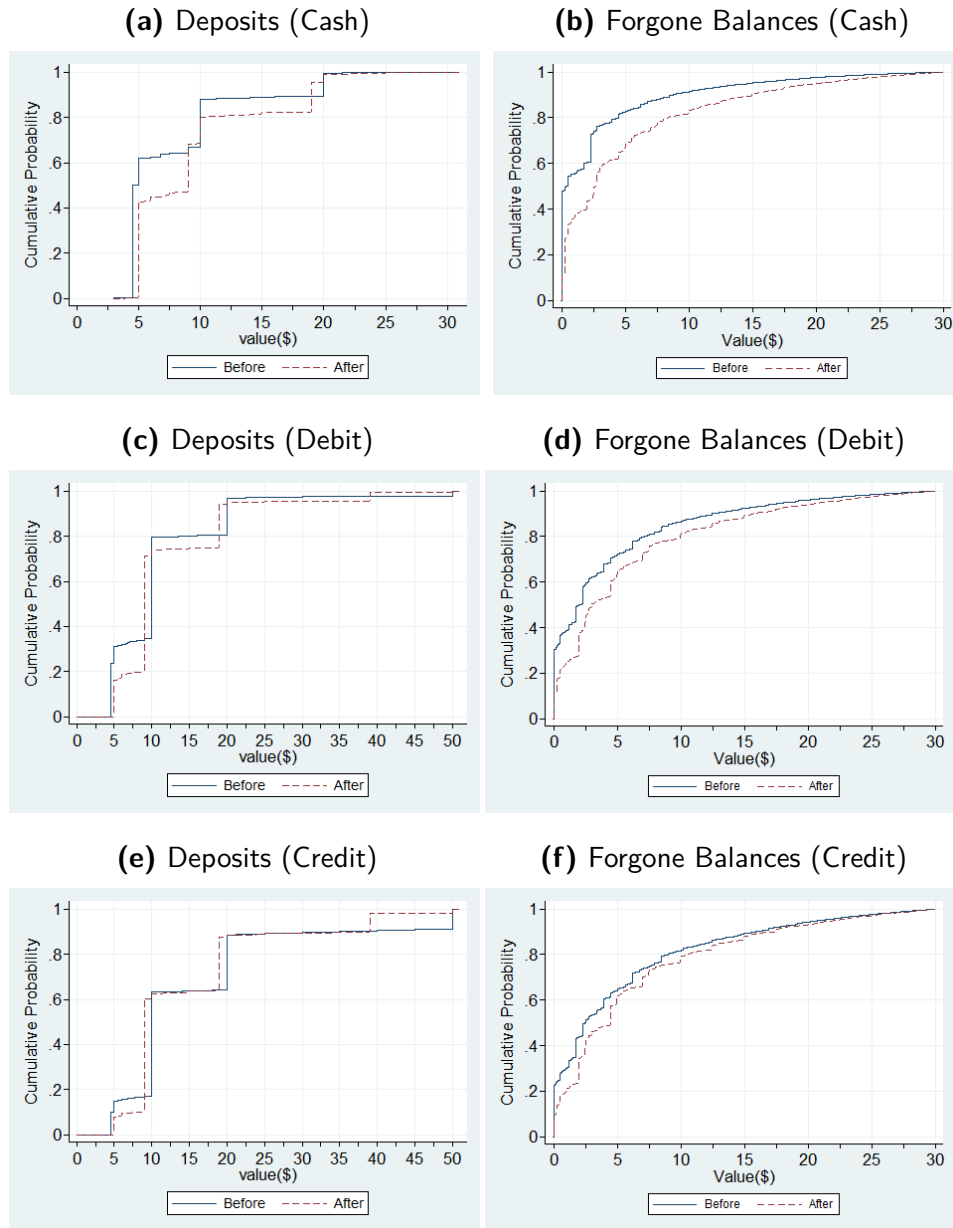
Note: Figure 1.9.14 plots the cumulative distribution function for deposits on new pay-per-ride purchases before and after the new card fee. Figure 1.9.14a plots the cumulative distribution function for deposits on new pay-per-ride purchases without subsequent refill activities. Figure 1.9.14b plots the cumulative distribution function for deposits on new pay-per-ride purchases with subsequent refill activities.

Figure 1.9.15: Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee (From January 1, 2013 To April 30, 2013): Histogram



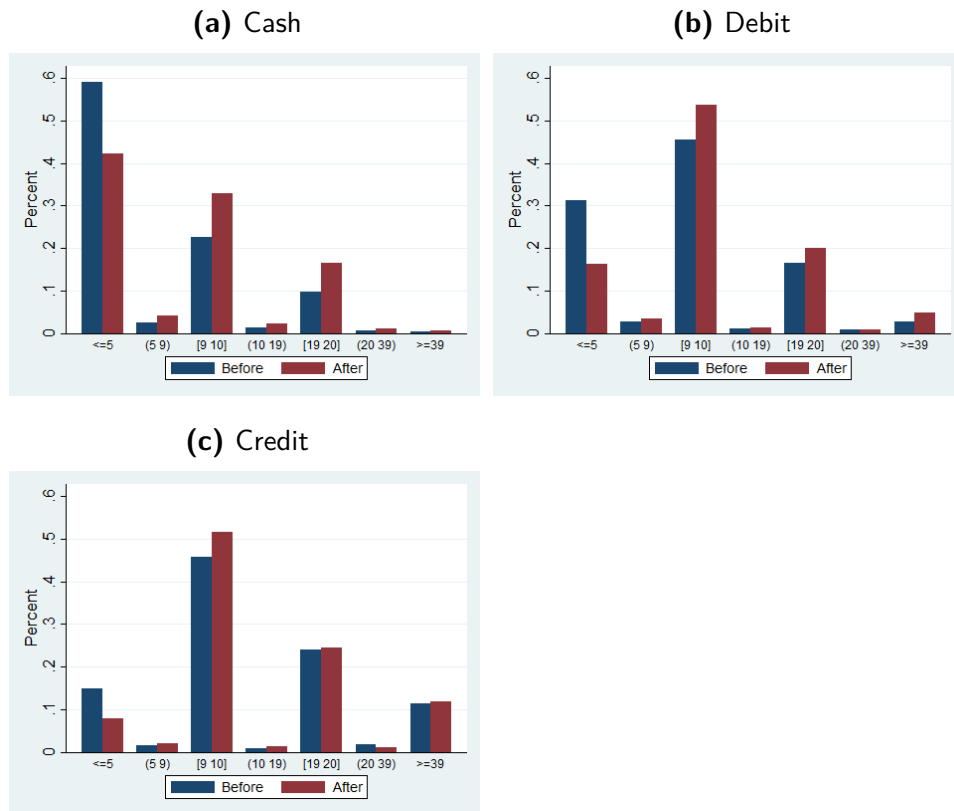
Note: Figure 1.9.15 plots the histogram for deposits on new pay-per-ride purchases before and after the new card fee (from January 1, 2013 to April 30, 2013). Figure 1.9.15a plots the histogram for deposits on new pay-per-ride purchases without subsequent refill activities. Figure 1.9.15b plots the histogram for deposits on new pay-per-ride purchases with subsequent refill activities.

Figure 1.9.16: Changes in Deposits and Forgone Balances on New Pay-per-ride MetroCards By Different Payment Methods (January 1, 2013 To April 30, 2013): Cumulative Distribution Function



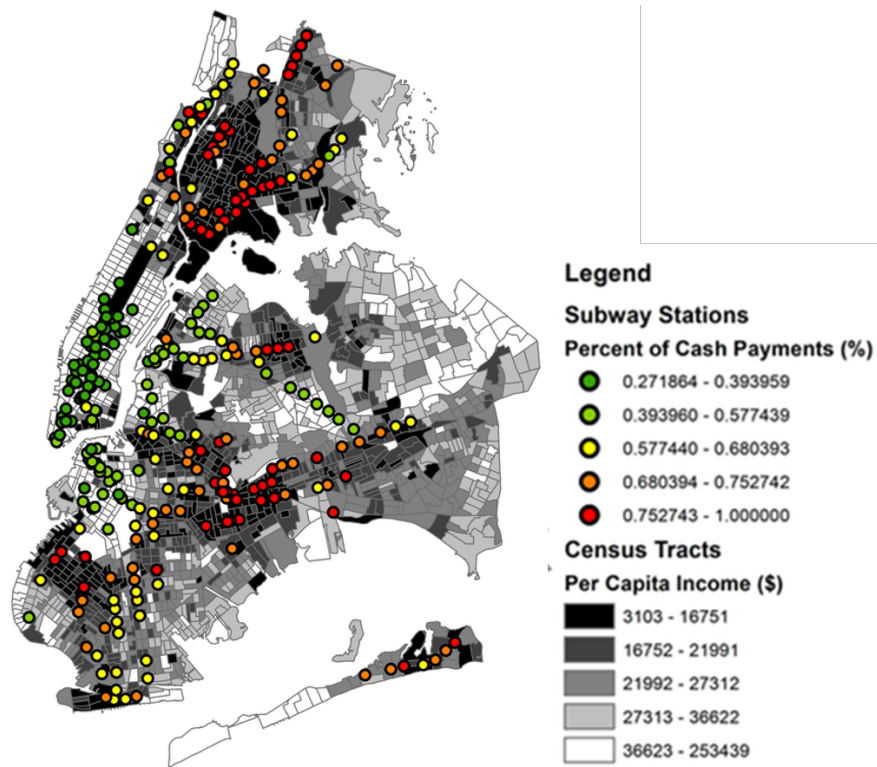
Note: Figure 1.9.16 plots the cumulative distribution function for deposits and forgone balances on new pay-per-ride MetroCards before and after the new card fee, broken out for different payment methods. Figure 1.9.16a-1.9.16b plot the charts for cash purchases. Figure 1.9.16c-1.9.16d plot the charts for debit card purchases. Figure 1.9.16e-1.9.16f plot the charts for credit card purchases.

Figure 1.9.17: Deposits on New Pay-per-ride MetroCard Purchases Before and After the New Card Fee by Payment Methods (From January 1, 2013 To April 30, 2013): Histogram



Note: Figure 1.9.17 plots the histogram for deposits on new pay-per-ride purchases before and after the new card fee by different payment methods. Figure 1.9.17a plots the cumulative distribution function for cash deposits on new pay-per-ride purchases. Figure 1.9.17b plots the histogram for debit-card payments on new pay-per-ride purchases. Figure 1.9.17c plots the histogram for credit-card payments on new pay-per-ride purchases.

Figure 1.9.18: Percent of Cash Payments Across Different Subway Stations From January 2013 To April 2013



Note: Figure 1.9.18 plots the spatial differences in percent of cash payments for MetroCard purchases across different census tracts. The census-tract level per-capita income data is from American Community Survey (ACS) 2009–2013 (5-Year Estimates)

Figure 1.9.19: Threshold of Prepaying for Rides in the Second Day For $n = 2$ Case

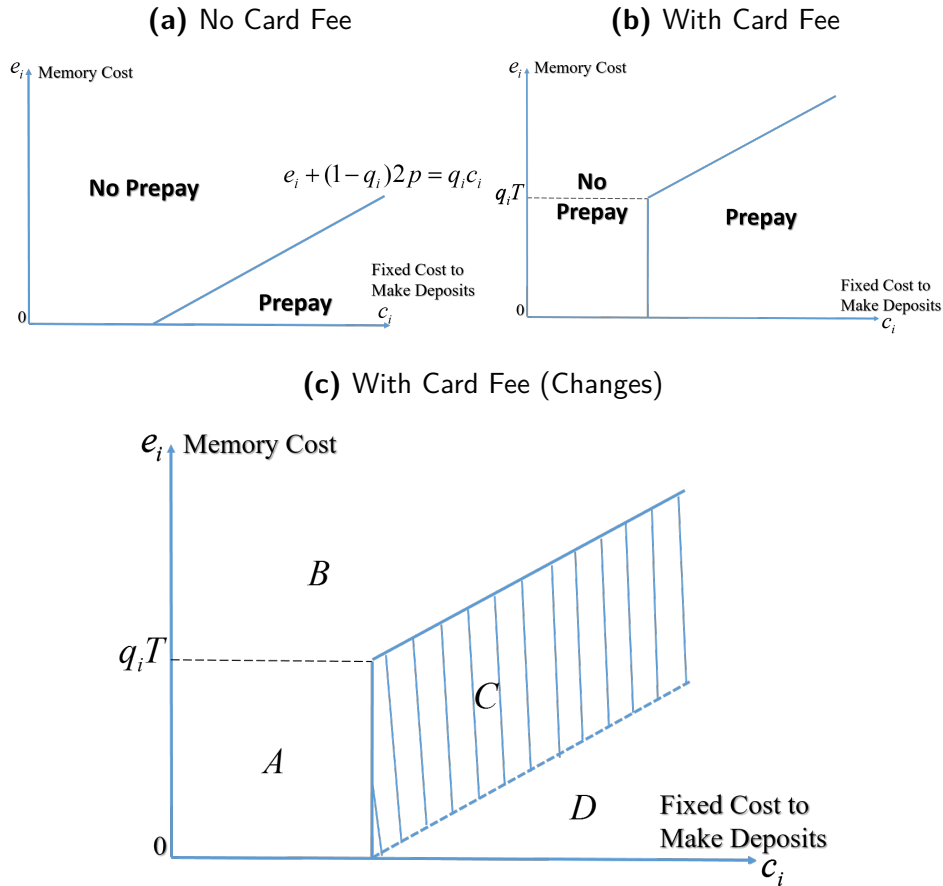


Figure 1.9.19 shows the threshold of prepaying for rides in the second day for $n = 2$ case when no card fee is imposed (1.9.19a) and when a card fee is imposed (1.9.19c), respectively. Before the new card fee was imposed, riders in region C only deposited the fare of the first day's rides since the fixed cost to make deposits in the second day is smaller than the cost to remember to carry the same card to the second day and the risk of losing the deposit. After the new card fee was imposed, these riders switched to prepaying for rides in the second day as the fixed cost to make deposits and pay the new card fee is larger than the cost to remember to carry the same card for the next period and the risk of losing the deposit. As a result, they risk losing $2p$ on expired MetroCards if they do not actually take rides in the second day (Table 1.9.9).

Figure 1.9.20: Threshold of Prepaying for Rides in the Second and Third Day For $n = 3$ Case

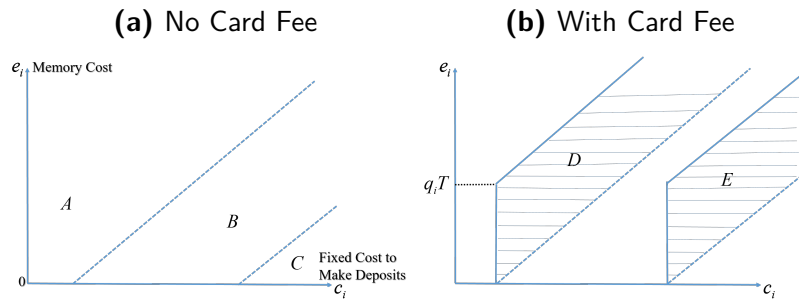
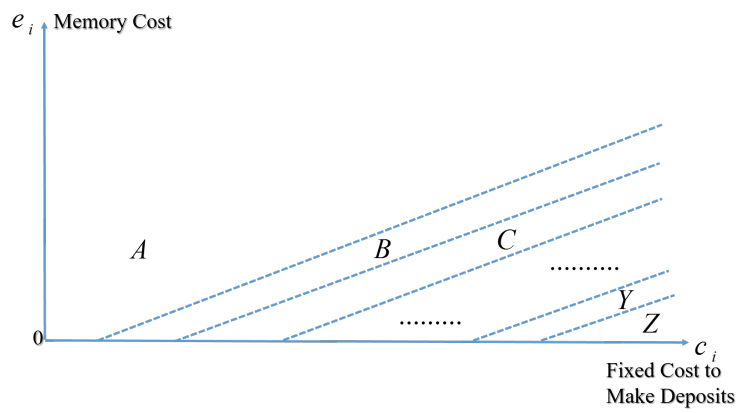


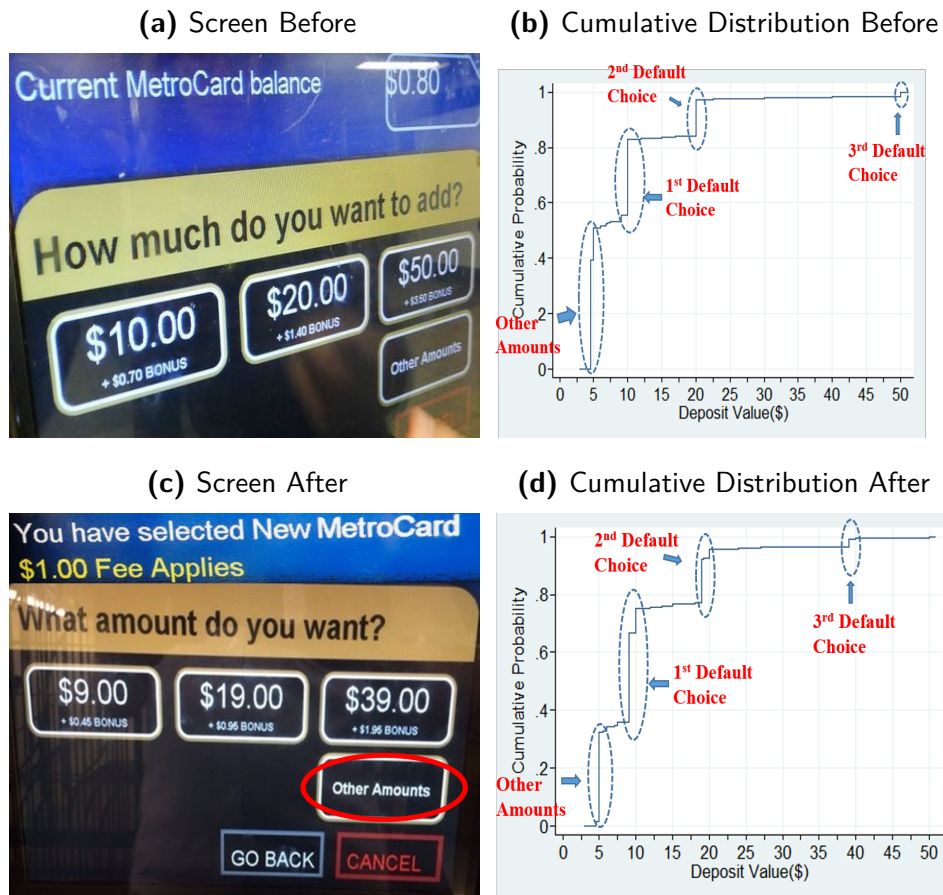
Figure 1.9.20 shows the threshold of prepaying for rides in the second and third day for $n = 3$ case when no card fee is imposed (1.9.20a) and when a card fee is imposed (1.9.20b), respectively. Before the new card fee was imposed, riders in region D only deposited the fare of the first day's rides since the fixed cost to make deposits in the second day is smaller than the cost to remember to carry the same card to the second day and the risk of losing the deposit. After the new card fee was imposed, these riders switched to prepaying for rides in the second day as the fixed cost to make deposits and pay the new card fee is larger than the cost to remember to carry the same card for the next period and the risk of losing the deposit. As a result, they risk losing $2p$ on expired MetroCards if they do not actually take rides in the second day. Analogously, riders in region E switched from prepaying for rides in the second day to prepaying for rides in the second and third day after the new card fee was imposed.

Figure 1.9.21: Threshold of Prepaying for Rides in Future Days For $n = 30$ Case (No Card Fee)



Note: For the $n = 30$ case, according to the model, there will be thirty-one regions ranging from no prepay for future rides (region A) to prepay for rides in the next 30 days (region Z) when no card fee is charged for new MetroCard purchases.

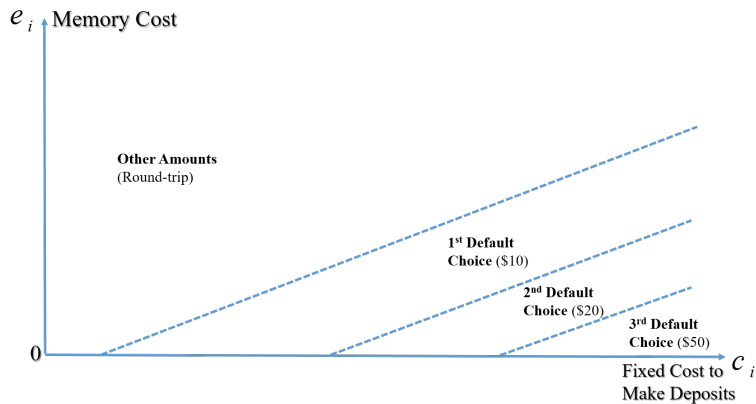
Figure 1.9.22: Default Choices of Deposit Amounts on Vending Machine Screen Before and After the New Card Fee



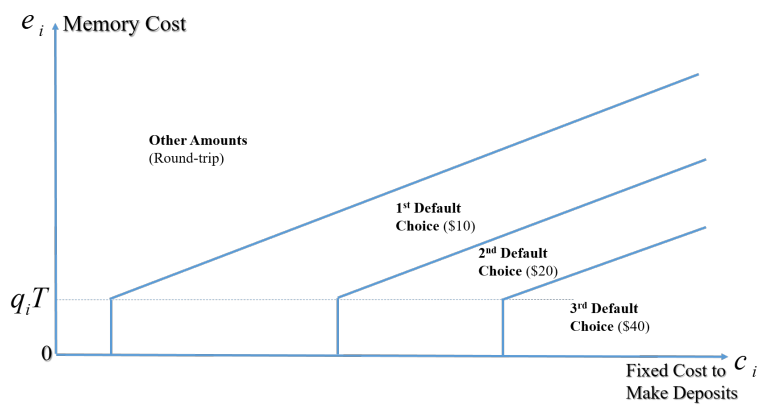
Note: Figure 1.9.22a and 1.9.22c show the suggested deposit amounts on vending machine screen before and after the new card fee went into effect, respectively. Figure 1.9.22b and 1.9.22d plot the cumulative distribution function (CDF) for deposit amounts on new pay-per-ride MetroCards before and after the new card fee was imposed, respectively.

Figure 1.9.23: Default Choices of Deposit Amounts For $n = 30$ Case

(a) No Card Fee



(b) With Card Fee



(c) With Card Fee (Changes)

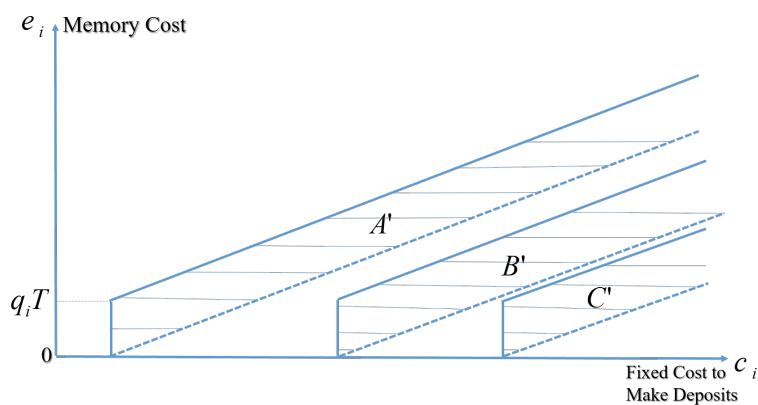
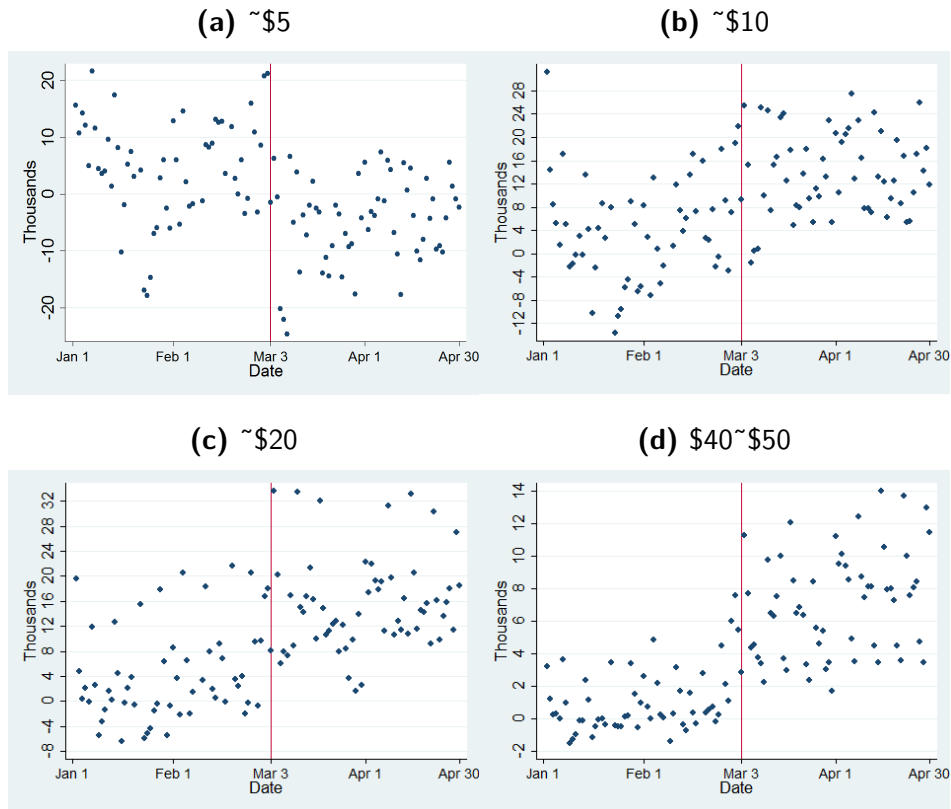


Figure 1.9.23 shows the threshold of choosing different deposit amounts for $n = 30$ case when no card fee is imposed (1.9.23a) and when a card fee is imposed (1.9.23b), respectively. Riders in region A' switched from choosing "Other Amounts" to choosing "1st default choice (\$10)" after the new card fee was imposed. Riders in region B' switched from choosing "1st default choice (\$10)" to choosing "2nd default choice (\$20)" after the new card fee was imposed. Riders in region C' switched from choosing "2nd default choice (\$20)" to choosing "3rd default choice (\$40)" after the new card fee was imposed.

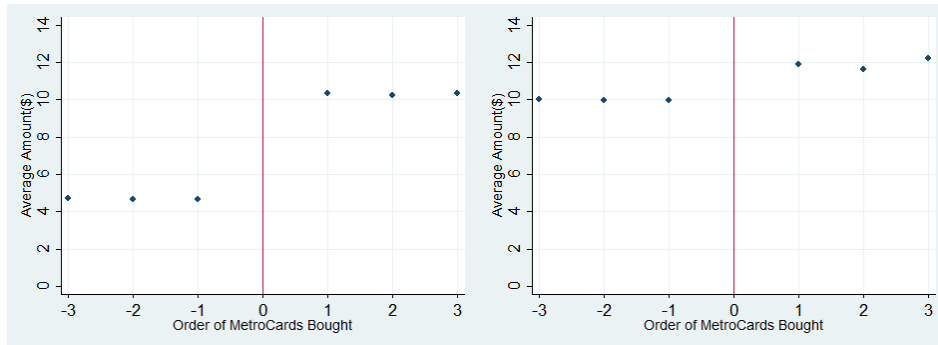
Figure 1.9.24: Daily Total Number of Pay-per-ride MetroCard Purchases From January 1, 2013 To April 30 2013 By Deposit Amounts



Note: Figure 1.9.24a plots daily total number of MetroCard purchases (new and refills) from January 1, 2013 to April 30, 2013. Figure 1.9.24b plots residual of daily total number of MetroCard purchases (new and refills) from day of week fixed effect. The vertical line marks the day when the new card fee was implemented.

Figure 1.9.25: Changes in Average Deposit Amounts For Pay-per-ride MetroCards Linked to the Same Rider Using MetroCard Trade-in and Trade-out Data (January 1, 2013 To December 31, 2013)

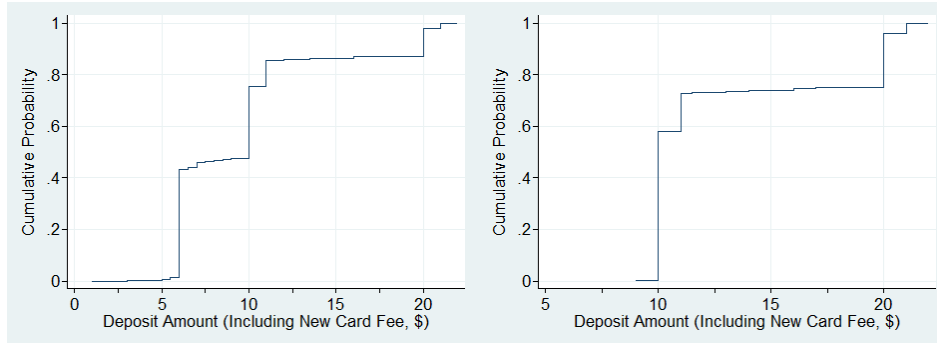
(a) ~\$5 Deposit Amounts Before the New Card Fee **(b)** ~\$10 Deposit Amounts Before the New Card Fee



Note: Figure 1.9.25a and 1.9.25b show average deposit amounts for pay-per-ride MetroCards linked to the same rider using MetroCard trade-in and trade-out data, by ~\$5 and ~\$10 average deposit amounts before the new card fee was implemented, respectively. X-axis shows the order of MetroCards purchased: the number 1 represents the first MetroCard purchased by a rider after the new card fee was introduced, the number -1 represents the last MetroCard purchased by the same rider before the new card fee was implemented, and so on.

Figure 1.9.26: Changes in Deposit Amounts After the New Card Fee For Pay-per-ride MetroCards Linked to the Same Rider Using MetroCard Trade-in and Trade-out Data (January 1, 2013 To December 31, 2013): Cumulative Distribution Function

(a) ~\$5 Deposit Amounts Before the New Card Fee (b) ~\$10 Deposit Amounts Before the New Card Fee



Note: Figure 1.9.26a and 1.9.26b show cumulative distribution function for deposit amounts after the new card fee, by ~\$5 and ~\$10 average deposit amounts before the new card fee was implemented, respectively.

Figure 1.9.27: Proposed Default Choices of Deposit Amounts on Touch-screen

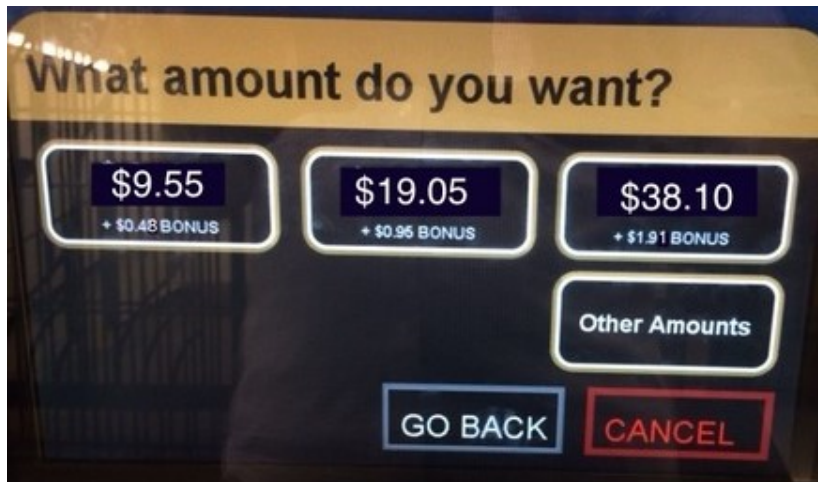


Figure 1.9.28: Monthly Sales of Single-ride Tickets From January 2011 To June 2015

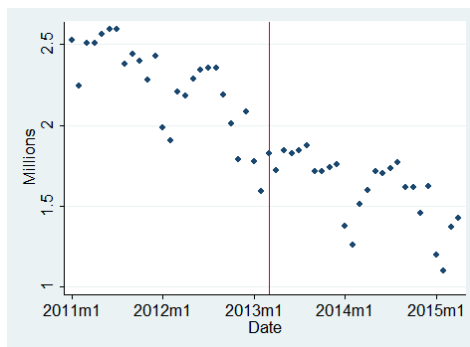
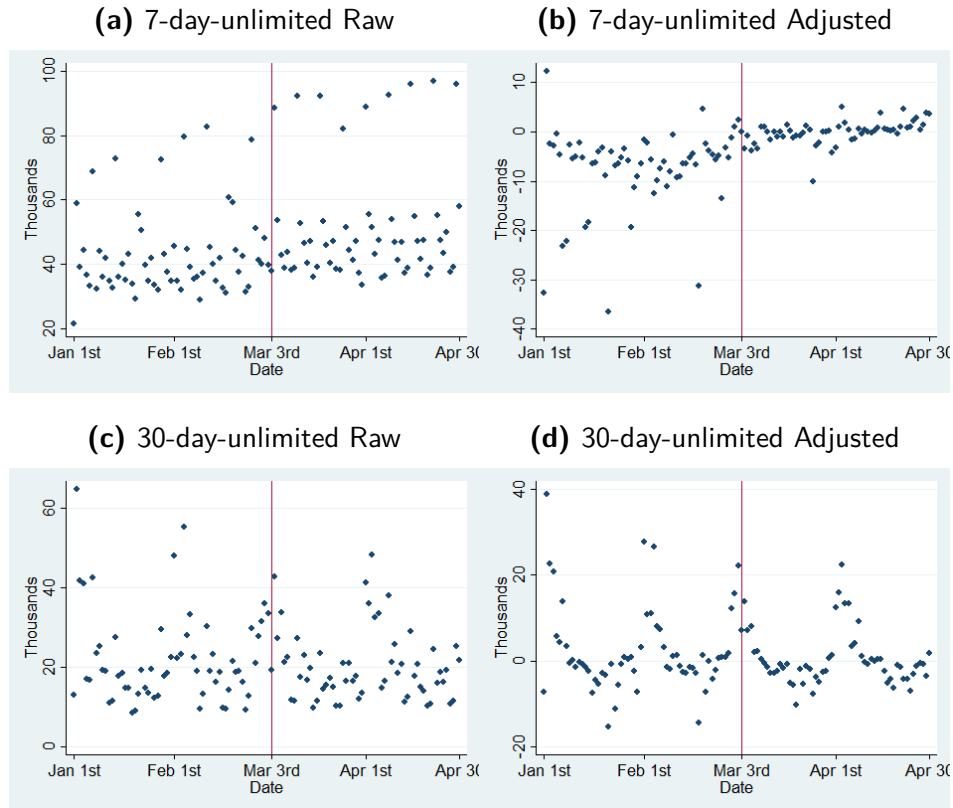
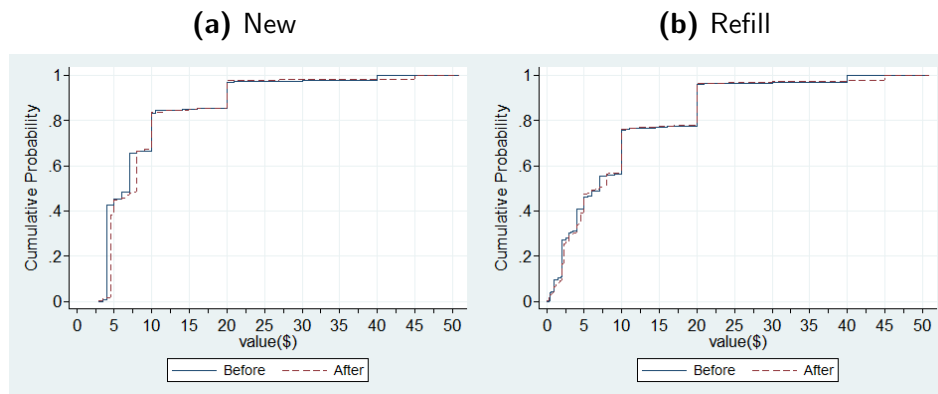


Figure 1.9.29: Daily Total Number of MetroCard Purchases From January 1, 2013 To April 30, 2013: New and Refills



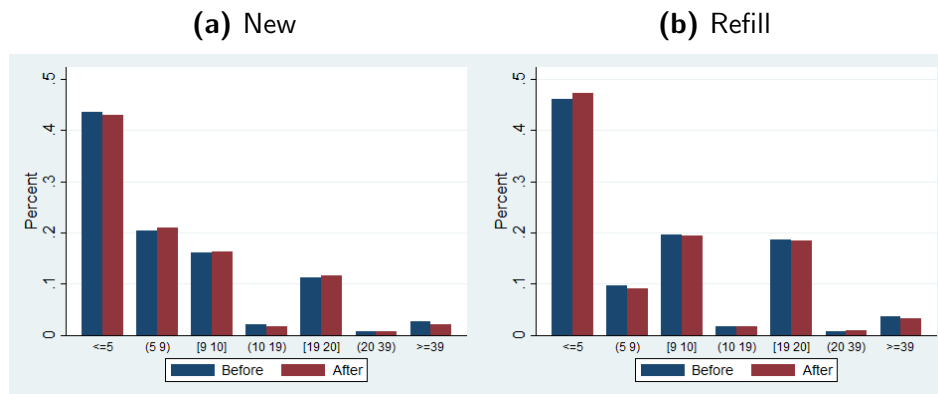
Note: Figure 1.9.29a plots daily total number of 7-day-unlimited MetroCard purchases (new and refills) from January 1, 2013 to April 30, 2013. Figure 1.9.29b plots residual of daily total number of 7-day-unlimited MetroCard purchases (new and refills) from day of week fixed effect. Figure 1.9.29c plots daily total number of 30-day-unlimited MetroCard purchases (new and refills) from January 1, 2013 to April 30, 2013. Figure 1.9.29d plots residual of daily total number of 30-day-unlimited MetroCard purchases (new and refills) from day of week fixed effect. The vertical line marks the day when the new card fee was implemented

Figure 1.9.30: Deposits on Pay-per-ride MetroCard Purchases Before and After the 2009 Fare Hike (From May 1, 2009 To August 30, 2009): Cumulative Distribution Function



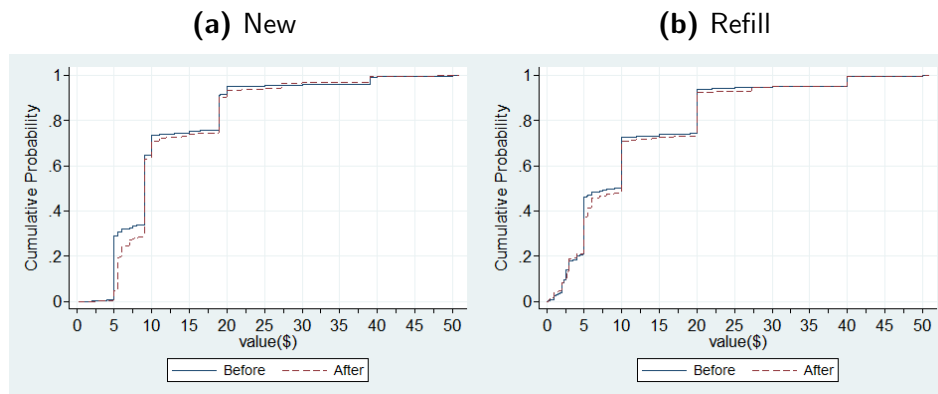
Note: Figure 1.9.30 plots the cumulative distribution function for deposits on pay-per-ride MetroCard purchases before and after the 2009 fare hike. Figure 1.9.30a plots cumulative distribution function for deposits on new pay-per-ride MetroCard purchases from May 1, 2009 to August 30, 2009. Figure 1.9.30b plots the cumulative distribution function for deposits on pay-per-ride refills from May 1, 2009 to August 30, 2009.

Figure 1.9.31: Deposits on Pay-per-ride MetroCard Purchases Before and After the 2009 Fare Hike (From May 1, 2009 to August 30, 2009): Histogram



Note: Figure 1.9.31 plots the histogram for deposits on pay-per-ride MetroCard purchases before and after the 2009 fare hike. Figure 1.9.31a plots the histogram for deposits on new pay-per-ride purchases from May 1, 2009 to August 30, 2009. Figure 1.9.31b plots the histogram for deposits on pay-per-ride refills from May 1, 2009 to August 30, 2009.

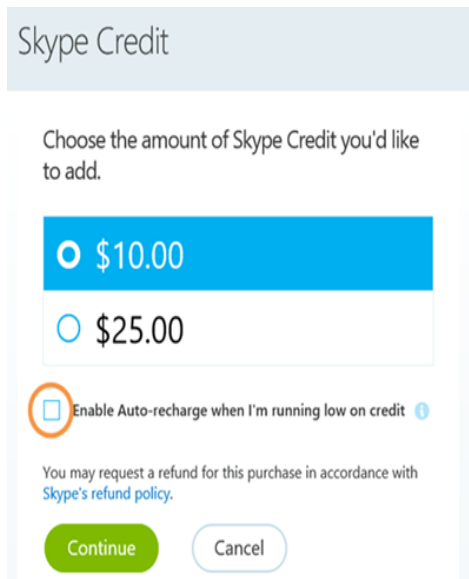
Figure 1.9.32: Deposits on Pay-per-ride MetroCard Purchases Before and After the 2015 Fare Hike (From January 1, 2015 To May 31, 2015): Cumulative Distribution Function



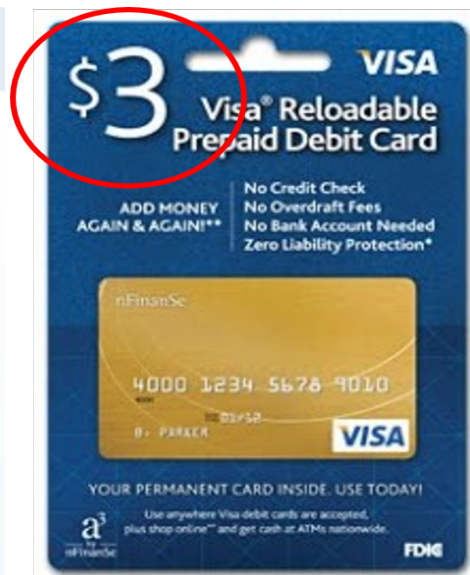
Note: Figure 1.9.32 plots the cumulative distribution function for deposits on pay-per-ride MetroCard purchases before and after the 2015 fare hike. Figure 1.9.32a plots cumulative distribution function for deposits on new pay-per-ride MetroCard purchases from January 1, 2015 to May 31, 2015. Figure 1.9.32b plots the cumulative distribution function for deposits on pay-per-ride refills from January 1, 2015 to May 31, 2015.

Figure 1.9.33: Implication of the Models

(a) Skype



(b) Reloadable Debit Card



1.10 APPENDICES

1.10.1 A GENERAL MODEL WHERE q_i DEPENDS ON THE BASE FARE OF A RIDE

The marginal utility of a subway or bus ride for rider i on day t is r_{it} . β is the price sensitivity parameter that determines how sensitive riding choices are to the marginal price of an additional ride. Here marginal price is v_i for rider i . For each rider i , r_{it} is uniformly distributed from 0 to $\frac{1}{\beta} \sim U(0, \frac{1}{\beta})$. On day t , the probability of actually taking a round-trip for rider i is:

$$\bar{q}_i = q_i \hat{q}(v_i) \quad (1.7)$$

where $\hat{q}(v) = 1 - \beta v$ is the fraction of riding opportunities worth more than v_i per ride. Over the course of an n -day period, actual number of rides taken by rider i is:

$$\hat{N}_i = \hat{N}(v_i, q_i, N_i) = N_i \hat{q}(v_i) \quad (1.8)$$

The expected value from riding subways and buses over a n -day time period is:

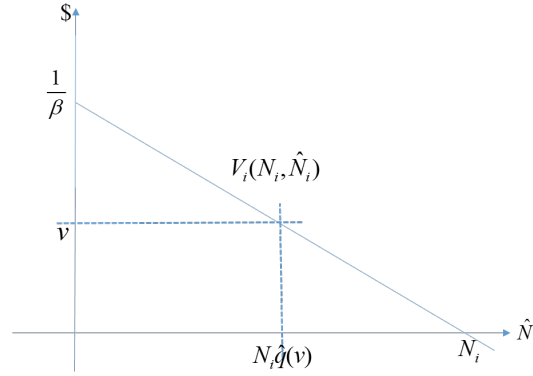
$$V(\hat{N}_i, N_i) = \frac{1}{\beta} \hat{N}_i (1 - \frac{1}{2} \hat{q}(v_i)) \quad (1.9)$$

Figure 1.10.1 shows the riding threshold v and resulting riding choice in relation to a rider's realized inverse demand curve. Assume riders are risk neutral and they have quasilinear utility. The marginal value of a dollar is normalized to one. Rider i 's money-metric utility from riding subways and buses on day $t \geq 2$ is:

$$U_{it} = \bar{q}_i (r_{it} - D_{it} - E_{it}e_i - C_{it}c_i - T(1 - E_{i,t-1})) \quad (1.10)$$

When purchasing a new MetroCard with card fee T and take two rides in day 1, rider i 's objective is to choose a fare deposit policy $\{D_{it}, E_{it}, C_{it}\}$ that maximizes

Figure 1.10.1: Inverse Demand Curve and Riding Threshold



expected total utility:

$$U_i = E\left[\max_{\{D_{it}, E_{it}, C_{it}\}} (V(\hat{N}_i, N_i) - (D_{i,1} + E_{i,1}e_i + C_{i,1}c_i + T + \sum_{t=2}^n q_i(D_{it} + E_{it}e_i))) \right] \quad (1.11)$$

Alternatively, to minimize total cost:

$$E\left[\min_{\{D_{it}, E_{it}, C_{it}\}} (D_{i,1} + E_{i,1}e_i + C_{i,1}c_i + T + \sum_{t=2}^n q_i(D_{it} + E_{it}e_i + C_{it}c_i + T(1 - E_{i,t-1}))) \right] \quad (1.12)$$

STYLIZED FACTS RELEVANT TO MODELING RIDES CHOICES

Three features of the data are important to accurately model rides choices by riders. First, riders' usage choices are price sensitive. Second, riders' usage choices are made while riders are uncertain about the ex post marginal price. Third, riders are inattentive to the remaining balance of their MetroCards. These three stylized facts motivate my assumption that, rather than choosing a precise quantity, riders choose riding thresholds and proceed to take all rides valued above the threshold.

Rider price sensitivity is clearly illustrated by a sharp decrease in single-ride ticket riding volume when the base fare for a ride increased (Figure 1.9.28). Two pieces of evidence demonstrate rider uncertainty about ex post marginal price. First, given clear sensitivity to marginal price, we would expect to see most rid-

ers using all the money in their MetroCards. Figure 1.9.16b shows more than 82 percent MetroCards had leftover balances at expiration, which is consistent with similar findings in the contexts of cellular phone service (Grubb and Osborne 19), electricity consumption (Borenstein 1), and labor supply (Saez 31). Hence the standard model (Cardon and Hendel 2001; Reiss and White 2005), which assumes perfect consumer foresight, fits the MetroCard data poorly.

Evidence for inattention comes from in-person surveys of riders I conducted as they exited subway stations. These surveys were conducted at forty-two subway stations in Manhattan, Bronx, Brooklyn, and Queens. The survey yielded a response rate of 91 percent for a total of 8,346 respondents. Riders were asked whether they knew the current balances of their MetroCards. Out of the 8,346 respondents, 99.7% riders did not give the accurate balances of their MetroCards.

1.10.2 RIDES CHOICES

In line with the approach taken by [19] and [1], I assume that riders are uncertain about the ex post marginal price when making riding choices. She chooses a riding threshold v_i based on her beliefs about q_i . During the course of the month, rider i is inattentive and does not track usage but takes all rides valued above v_i . Taking all rides above the constant threshold v_i is the optimal strategy of an inattentive rider who does not track usage and hence cannot update her beliefs about the marginal price of the next ride. (It is analogous to an electricity consumer setting a thermostat rather than choosing a quantity of kilowatt hours.)

Conditional on choosing pay-per-ride MetroCards, rider i chooses her riding threshold v_i to maximize her expected utility conditional on her information about c_i , e_i , and q_i . Given new card fee T , the base fare per ride p , and demand in equation (1.9), the optimal threshold derived in Appendix B.1 is uniquely characterized by equation (1.13)

$$v_i = pPr(N_i \geq 0 | e_i; c_i; q_i) \frac{E[N_i | N_i \geq 0; e_i; c_i; q_i]}{E[N_i | e_i; c_i; q_i]} \quad (1.13)$$

Here the calling threshold $v_i = v = p$. Note that choosing threshold v_i is equiv-

alent to choosing a target riding quantity $\hat{N}_i^T = E[N_i] \hat{q}(v_i)$, which is implemented with endogenous error $(N_i - E[N_i]) \hat{q}(v_i)$. Importantly, riders are aware of their inability to hit the target precisely and take this limitation into account when making their threshold/target choice.

2

How Local Alcohol Access Affects Birth Outcomes: Evidence from Texas

2.1 INTRODUCTION

In the past four decades, one of the major goals of US public policy has been to reduce the health, safety, and criminal risks associated with alcohol abuse. A number of policies are designed to restrict the alcohol consumption. Some, such as the minimum legal drinking age (MLDA), and preliminary-breath-test laws, have become noticeably more stringent in the past 30 years. An assumption underlying these social policies is that alcohol-related problems and behaviour can be controlled by restricting the availability of alcoholic beverages. An extensive empirical literature focuses on the impact of these policies on alcohol-related outcomes such as motor vehicle accidents, Driving Under the Influence, and violent crimes. For

example, studies suggest that increases in federal or state excise taxes on alcohol discourage heavy drinking and reduce motor vehicle fatalities (Coate and Grossman 10, Grossman 18, Laixuthai and Chaloupka 23, Mullahy and Sindelar 28). Research also concludes that changes in minimum drinking age law significantly reduced alcohol-related accident fatalities. Ironically, while these laws were being tightened, local alcohol control policies at city and county levels were relaxed in many parts of the country. After the repeal of Prohibition in 1933, 34 states passed local option laws, where county and municipal governments govern alcohol policies in terms of alcohol availability and distribution, mostly by popular vote.

Restricting alcohol availability at the local level may be a plausible prevention strategy for several reasons. Light to moderate drinkers, rather than alcoholics, are believed to contribute disproportionately to a community's alcohol-related problems (Bruun et al. 4, Room 29). Curbing all community members' alcohol consumption, not just consumption by the heaviest alcohol abusers, may decrease alcohol-related problems (Rose et al. 30). However, as local alcohol restrictions generally limit the availability of alcohol, these law changes also significantly increase the travel distance required to obtain the alcohol, change where the alcohol is consumed and change the type of alcohol consumed. Hence, the overall effect of local alcohol access policies is ambiguous.

In this paper, I examine the causal effects of local access to alcohol on birth outcomes by comparing municipalities where referenda on legalizing liquor sales passed and failed by narrow margins. My results indicate that municipalities which were studied experienced higher incidence of low birth weight after legalizing the local sale of alcohol to the general public.

In 26 states, alcohol restrictions differ across counties and municipalities (Alabama, Alaska, Arkansas, Florida, Georgia, Illinois, Kansas, Kentucky, Massachusetts, Michigan, Minnesota, Mississippi, Nevada, New Hampshire, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, South Dakota, Tennessee, Texas, Virginia, Washington, and Wisconsin). A dry community is a community whose government forbids the sale of alcoholic beverages. A wet community is a community with no restrictions on local liquor sales. A moist community is a community

on the “middle ground” between a dry community and a wet community. A moist community allows alcohol to be sold in certain situations, but has limitations on alcohol sales that a normal “wet” community would not have. Some prohibit on-premises sale, some prohibit off-premises sale, and some prohibit both.

The only option for residents of dry areas is to go to areas without alcohol restrictions, which can entail large distances and associated time costs, or risk incarceration through the illegal production of alcohol. Many dry communities do not prohibit the mere consumption of alcohol, which could potentially cause a loss of profits and taxes from the sale of alcohol to their residents in “wet” areas. Their main argument against alcohol sales is that unrestricted alcohol consumption in wet status may cause social and moral degradation, which in turn would lead to an increase in crime.

Local alcohol control policies have received much less attention than other alcohol-related policies from the mainstream media and have been examined less frequently in the literature. Currently, there are thousands of dry communities across the United States with about 18,000,000 people in the 10% of the area of the US that is dry (Hanson 2000). Almost one-half of the counties in Mississippi are dry with their own prohibition against the production, advertising, sale, distribution, or transportation of alcoholic beverages within their boundaries (Hanson 2000). As local prohibition affects a large portion of population living in dry areas, most of which with disadvantaged economic status, study of this policy is critical for local social outcomes such as alcohol-related accidents, alcohol-involved violent crimes, as well as maternal drinking during pregnancy.

This paper makes two contributions to the existing literature. The primary innovation of this paper is to examine the causal effect of local alcohol access (i.e., “wet” communities) on alcohol-related outcomes. Local option elections determine the types of alcoholic beverages which may be sold and how they can be sold by counties, cities, or individual justice of the peace precincts. With universal 50% winning voting share threshold, close losers and close winners of local option elections provides quasi-random variation in winner status that can be used to overcome the endogeneity of local alcohol sales restrictions, since for narrowly decided

ances the outcome is unlikely to be correlated with other distinct characteristics as long as there is some unpredictable component of the ultimate vote.

Second, while the existing literature of local prohibition focuses on outcomes such as alcohol-related crimes and vehicle crashes, this paper examines the effect on birth outcomes, especially birth weight. Birth weight is the single most important indicator of infant health. It is a significant predictor of infant mortality and morbidity and of health and learning disabilities in later life. (see Currie and Cole [1991] for references). Babies born with low birth weight are more likely than babies born at a normal weight to have health conditions, including respiratory distress syndrome and bleeding in the brain as newborns and diabetes and obesity later in life. When a community goes from “dry” to “wet”, there may be a higher risk of maternal drinking during pregnancy since buying liquor locally becomes easier. Drinking alcohol during pregnancy can result in embryonic developmental abnormalities such as low birth weight.

2.2 DATA

In this paper, I study the link between local restrictions on liquor sales and birth outcomes among local residents. I rely on two broad sources of data to identify: (1) date and location of local option elections as well as the number of vote for and against the issues voted on; (2) newborns’ characteristics such as birth weight and mother’s characteristics.

2.2.1 LOCAL OPTION ELECTIONS

The first source of data (and the reason I focus on Texas) is annual local option election data drawn from Texas Alcoholic Beverage Commission (TABC) Annual reports. The data include date of elections, alcoholic issues that were voted for, city and/or county of the election, number of vote for and against the issues, required vote shares for passage of the issues, and dry/wet status before and after the election. Our sample includes local option elections in counties and municipalities in Texas between 1979 and 2003.

Table 1 describes the number of local option elections observed between 1979 and 2003. Over this 24-year-period, there were 310 referenda, all trying to go from dry to wet. In total, 87 of the 254 counties in Texas were dry at the start of the period in 1979, and 34 of these legalized some type of alcohol sales by 2003. There were 32 other changes in which already non-dry counties further relaxed alcohol control policies. Figure 2.2.1 shows the yearly number of local option elections between 1979 and 2003. Although there were elections about alcohol access laws over the entire period, the majority of elections took place before 1990 and after 2000.

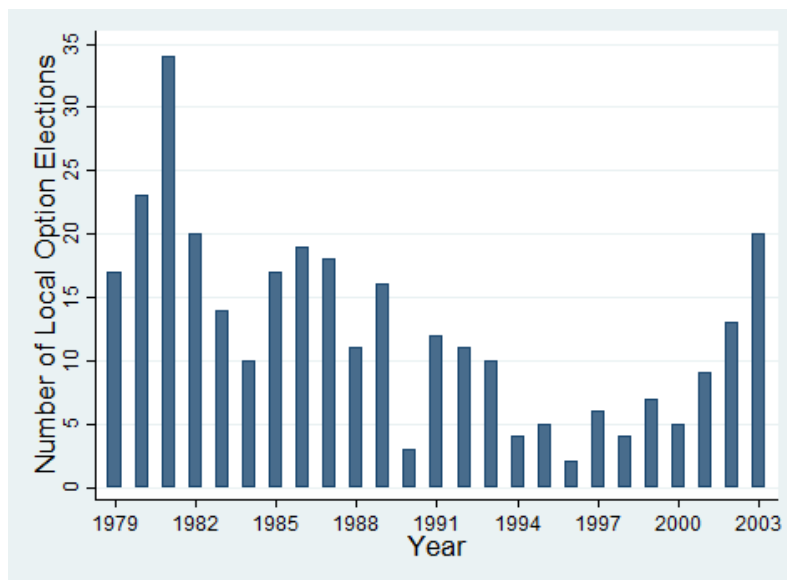


Figure 2.2.1: Number of Referenda By Year, 1979-2003 (Total 310)

2.2.2 VITAL STATISTICS RECORDS

The Vital Statistics Records, collected by Texas Department of State Health Services, correspond to 9.6 million babies born in hospitals within the 5,513 municipalities in Texas from 1979 to 2003. I only include newborns to mothers who

reside in Texas¹. The final analysis of this paper will use a panel of newborns appearing within two years before to six years after each local option election, reducing the sample to 279,270 birth records. The average birth weight in Texas for the period of study was 3,324 grams, and 7.04 percent of births were low birth weight.

The working dataset merges Vital Statistics Records with local option election data by mother's municipality of residence. Further details are given in a data appendix available from the authors on request. Data on birth weight are comparatively accurate.

2.3 MODEL

In this section I describe my regression discontinuity design. I show in a cross-sectional framework how a RD design approximates a randomized experiment. Suppose that municipality j considers a local option election to legalize the sale of alcoholic beverages and that this proposal receives vote share v_j (relative to the required threshold v^*). Let $b_j = \mathbf{1}(v_j \geq v^*)$ be a dummy indicator for local alcohol access. Suppressing time-related considerations, we can write some outcome y_j (incidence of low birth weight, for example) as

$$y_j = \alpha + b_j\beta + \mu_j,$$

where β is the causal effect of local alcohol access and μ_j represents all other determinants of the outcome (with $E[\mu_j]=0$).

In general, the election outcome may be correlated with other municipal characteristics that influence local alcohol consumption and alcohol-related outcomes (i.e., $E[\mu_j b_j] \neq 0$). If so, a simple regression of y_j on b_j will yield a biased estimate of β . However, as Lee (2008) points out, as long as there is some unpredictable random component of the vote, a narrowly decided election approximates a randomized experiment. In other words, the correlation between the election outcome and unobserved municipal characteristics can be kept arbitrarily close to zero by

¹I do not have data on newborns outside Texas to mothers who reside in Texas

focusing on sufficiently close elections.

Therefore, one can identify the causal effect of measure passage by comparing municipalities that barely won an election (the “treatment group”) with others that barely failed an election (the “control group”). That is, if v_j is the vote share and v^* is the threshold required for passage, unobserved local characteristics μ_j may vary with v_j but should be similar for municipalities with $v_j = v^* + \varepsilon$ and $v_j = v^* - \varepsilon$ (for small ε). Formally, the required assumption is that $E[\mu_j|v_j]$ is continuous at $v_j = v^*$. Now we can write

$$E[y_j|v_j] = a + E[b_j|v_j]\beta + E[\mu_j|v_j] = a + b_j\beta + E[\mu_j|v_j],$$

The assumed continuity of $E[\mu_j|v_j]$ at v^* implies that:

$$\lim_{x \uparrow v^*} E[y_j|v_j] - \lim_{x \downarrow v^*} E[y_j|v_j] = \beta$$

I focus on an implementation of the RD strategy that involves approximating the regression functions above and below the cutoff by means of weighted polynomial regressions with weights computed by applying a kernel function on the distance of each observation’s score to the cutoff. I present these kernel-based estimator using alternative bandwidth selectors and polynomial orders, as well as bias-corrected estimates per Calonico et al (Forthcoming) (henceforth CCT).

To implement this, I begin by identifying each (j, t) combination with an election. I then select observations from municipality j in months $t-24$ through $t + 72$. Where a municipality has multiple elections in the same month, the same calendar month observation is used more than once. Specifically, assuming a homogeneous effect of the local option election passage on birth outcomes with universal 50% voting share for winning:

$$Y_{ijt} = a + \gamma 1\{v_j \geq v^*\} + a(t) + X_{ij} + \mu_{ij} \quad (2.1)$$

where i indexed individuals and j indexed municipalities, Y_{ij} denoted the outcome of interest (for example, low birth weight) for individual i in city j , v_j was

the voting share for local option election in city j , v^* was the distinct voting share for winning a local option election (i.e., 50%), $a(\cdot)$ is a flexible function of voting shares, X is a set of controls including gender and month-of-year fixed effects, and $E(\mu_{ij}|v_j) = 0$.

2.4 RESULTS

2.4.1 EVALUATING THE LOCAL OPTION REFERENDUM QUASI-EXPERIMENT: BALANCE OF TREATMENT AND CONTROL GROUPS

My empirical strategy is to use close elections to approximate a true experiment. This requires that legalization of alcohol be as good as randomly assigned, conditional on having a close election. In this section, I consider tests of this assumption. I examine three diagnostics for the validity of the RD quasi-experiment, based on the distribution of the share of voters for alcohol sales, preelection differences in mean characteristics, and differences in preelection trends. Tests of the balance of outcome variable means and trends before the election are possible only because of the panel structure of our data and provide particularly convincing evidence regarding the approximate randomness of measure passage.

Figure 2.4.1 shows a histogram of vote shares for local option elections among municipalities in Texas from 1979 to 2003. Discontinuous changes in density around the threshold can be an indication of endogenous sorting around this threshold, which would violate the RD assumptions (McCrary 2008). I see no evidence of such changes. I then follow McCrary's test and show that the density just to the left of the cutoff is statistically indistinguishable from the density just to the right of the cutoff (McCrary 2008).

Figure 2.4.2 presents graphical analyses of mean municipal incidence (probability) of low birth weight by the margin of victory or defeat, for births in 1-12 months (left panel) and in 13-24 months (right panel) before the election. I show average outcomes in one-percentage-point bins defined by the vote share relative to the threshold. Thus, the leftmost point represents measures that failed by be-

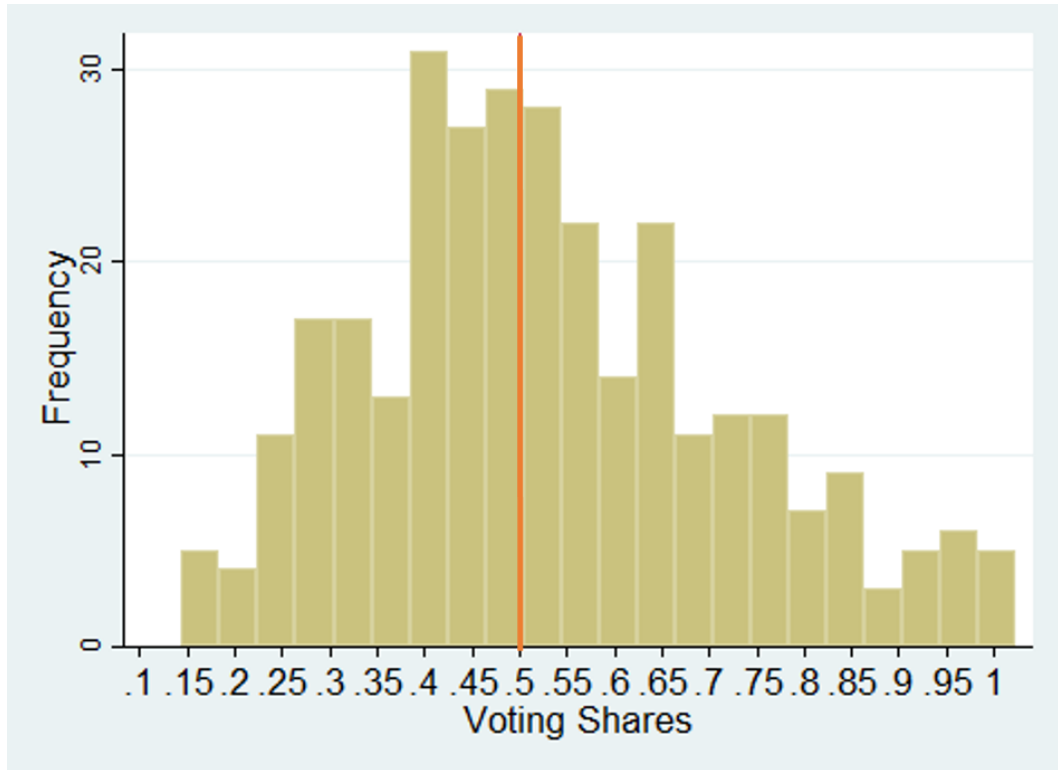
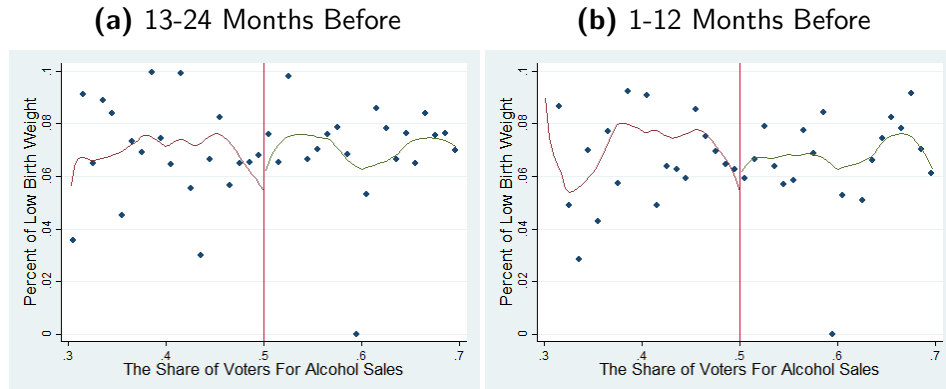


Figure 2.4.1: Histogram of Voting Shares in Local Option Elections, 1979-2003

tween nine and ten percentage points, the next measures that failed by eight to nine points, and so on. As expected, there is no sign of a discontinuity in probability of low birth weight for newborns before the election.

Figure 2.4.2: Percent of Low Birth Weight Newborns, by the Share of Voters For Alcohol Sales, One Year and Two Years Before Election



Notes: Graph shows percent of low birth weight for births in 1-12 months (left panel) and in 13-24 months (right panel) before the local option election, by the share of voters for alcohol sales. Local Option elections are grouped into bins one percentage point wide: measures that passed by between 0.001% and 1% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin. The solid dots plot “raw” percent of low birth weight, along with the fitted values of a locally weighted regression calculated within each bin.

Columns (1)–(2) of Table 2.4.1 present regressions of birth weight, mother’s marriage status, and percentage of children born by mothers below age 21 variables measured in the year before alcohol referendum, on an indicator for whether the local option election was passed. The specifications are estimated from a sample that includes only observations from the year before the election. The first column controls for month of year effects and the required threshold. It reveals large pre-measure differences in some outcomes. The second column adds a quadratic polynomial in the measure vote share. Comparing communities that barely passed an election with communities that barely failed eliminates the significant estimates,

shrinking two of the point estimates substantially.

Columns (3)–(4) in Table 2.4.1 repeat my two first specifications, taking as the dependent variable the value in each outcome in the year $t-2$ (i.e., in two years before alcohol referendum). Although the model without controls shows some differences in trends between communities that pass and fail measures, these are eliminated when I include controls for the vote share. Overall, there seems to be little cause for concern about the approximate randomness of the measure passage indicator in our RD framework. Once I control for a quadratic in the measure vote share, measure passage is not significantly correlated with pretreatment trends of any of the outcomes I examine.

Table 2.4.1: RD Estimates of the Local Alcohol Access on Birth Weight: Pre-elections

Each entry comes from a separate regression. Columns (1)–(2) report estimated alcohol effects on outcome levels the year before the election; columns (3)–(4) report estimated effects on outcome levels two years before the election. Samples in columns (1) and (3) include observations from the year before each local option election. Samples in columns (2) and (4) consist of observations from two years before each local option election. The specification in these columns is equation 3.1, with indicators for each month of year, and indicator of gender, an indicator for measure passage, and a quadratic in the share of votes for alcohol sales. Sample sizes vary with availability of dependent variable. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the school district level. - 0.1 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.4.2 INTENT-TO-TREAT EFFECTS ON BIRTH WEIGHT

Figure 2.4.3 presents graphical analyses of mean municipal incidence (probability) of low birth weight by the margin of victory or defeat, for births in 1-12 months (left panel) and in 13-24 months (right panel) after the election. For municipalities where the measure just failed, there was no significant changes in the incidence of low birth weight. By contrast, after the election, municipalities where the measure

just passed saw a significant increase in the incidence of low birth weight. It is important to note that this result is obtained without adopting any controls. That is, I am only analyzing a simple difference between pre- and post-election. Notice, however, that as the election becomes less random (i.e. the local option election wins by a margin of more than 5 percent), the change in the incidence of low birth weight returns to zero. This highlights the importance of using quasi-experimental methodology to deal with endogeneity.

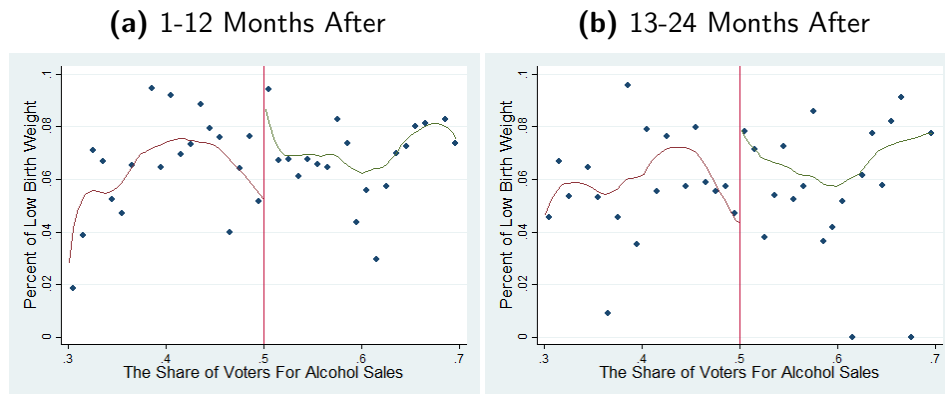
Table 2.4.2 reports the results of my regression discontinuity estimates, which are consistent with the information displayed in Figure 2.4.3: For municipalities where the measure just passed, the changes in local restriction on local sales (i.e., going from “dry” to “wet”) highered the incidence of low birth weight by 4.54% for babies born within 12 months after the elections. The incidence of low birth weight rose by 4.45% for babies born 13-24 months after the elections. In contrast, there is no significant change in mother’s marriage status or percentage of children born by mothers below age 21 after the elections.

Table 2.4.2: RD Estimates of the Local Alcohol Access on Birth Weight: Post-elections

Posttrend.tex9S[table-format=1,table-column-width=20mm] Each entry comes from a separate regression. Columns (1)-(2) report estimated alcohol effects on outcome levels the year after the election; columns (3)-(4) report estimated effects on outcome levels two years after the election. Samples in columns (1) and (3) include observations from the year after each local option election. Samples in columns (2) and (4) consist of observations from two years after each local option election. The specification in these columns is equation 3.1, with indicators for each month of year, and indicator of gender, an indicator for measure passage, and a quadratic in the share of votes for alcohol sales. Sample sizes vary with availability of dependent variable. Standard errors (in parentheses) are robust to heteroscedasticity and clustered at the school district

level. + 0.1 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.4.3: Percent of Low Birth Weight Newborns, by the Share of Voters For Alcohol Sales, One Year and Two Years After Election



Notes: Graph shows percent of low birth weight for births in 1-12 months (left panel) and in 13-24 months (right panel) after the local option election, by the share of voters for alcohol sales. Local Option elections are grouped into bins one percentage point wide: measures that passed by between 0.001% and 1% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin. The solid dots plot “raw” percent of low birth weight, along with the fitted values of a locally weighted regression calculated within each bin.

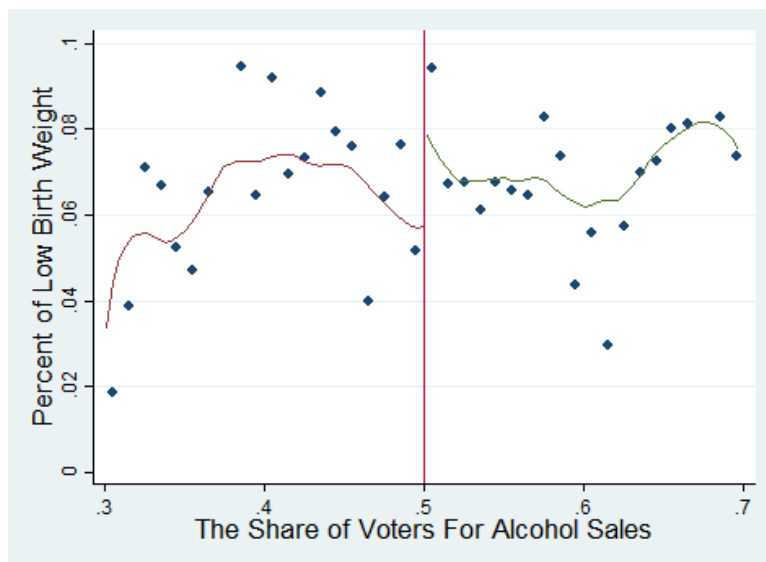
2.5 DISCUSSIONS

From the results, it is clear that municipalities which were studied experienced higher incidence of low birth weight after legalizing the local sale of alcohol to the general public. The incidence of low birth weight rose by 4.5% for babies born within two years after the elections. Among all possible explanations, the leading candidate is maternal drinking: When a community goes from “dry” to “wet”, there may be a higher risk of maternal drinking during pregnancy since it is easier to purchase liquor locally. This mechanism is strongly supported when we look at births within 10 months (i.e., babies conceived just before an election) after a community became wet.

Figure 2.5.1 presents graphical analyses of mean municipal incidence (probabil-

ity) of low birth weight by the margin of victory or defeat, for births in 1-9 months after the election. For municipalities where the measure just failed, there was no significant changes in the incidence of low birth weight. By contrast, after the election, municipalities where the measure just passed saw a significant increase in the incidence of low birth weight. This result is most likely driven by maternal drinking during pregnancy. Meanwhile, there is no significant change in mother's marriage status or percentage of children born by mothers below age 21 after the elections. These findings provide additional evidence indicating that the estimated impact of local prohibition is not driven by unobserved factors coinciding with the policy or by general family-specific factors.

Figure 2.5.1: Percent of Low Birth Weight Newborns, by the Share of Voters For Alcohol Sales, Within Ten Months After Election



Notes: Graph shows percent of low birth weight for births in 1-9 months after the local option election, by the share of voters for alcohol sales. Local Option elections are grouped into bins one percentage point wide: measures that passed by between 0.001% and 1% are assigned to the 1 bin; those that failed by similar margins are assigned to the -1 bin. The solid dots plot “raw” percent of low birth weight, along with the fitted values of a locally weighted regression calculated within each bin.

2.6 CONCLUSION

My empirical analysis differs from the existing empirical literature by exploiting a quasi experimental design to consider the causal effect of local alcohol sales (i.e., “wet” communities) on birth outcomes, especially birth weight. To my knowledge, no previous study has estimated the effects of local prohibition on birth outcomes and the vast majority of previous studies are correlational. This study provides results that are particularly relevant to an effort to combat Fetal Alcohol Spectrum Disorder (FASD). This paper also contributes to the broader and rapidly growing literature interested in the prenatal determinants of medium and long-term outcomes in several ways.

3

The Causal Effect of Institutional Discrimination on Health: Evidence from Hukou System in China

Interest in the impact of institutional discrimination on health outcomes has increased dramatically. Literature suggests that institutional discrimination produces societal-level conditions that constrain the opportunities, resources, and well-being of disadvantaged groups (Chan and Zhang 7; Grey 17; Li et al. 25). For instance, one study showed that LGB respondents living in states that passed antigay marriage amendments in 2006 had higher psychological distress than did LGB individuals in states without such an amendment on the ballot (Li et al. 25). Another study has shown that segregation, as one measure of institutional discrimination, is positively associated with mortality among adult African Americans while ac-

counting for other possible confounding variables (Shen and Huang 34). Since research has mostly been done in the western context where social segregation has already been established, it is difficult to isolate the effect of initial social segregation on health outcomes.

In this paper, I examine the causal effect of institutional discrimination on health by exploiting a 1964 change in household registration system (*hukou*) in China, which caused a nationwide discrimination against rural dwellers. In 1964, the central government enforced the *hukou* system, which enables institutional discrimination against rural dwellers in many aspects. *Hukou* was (and is) ascribed at birth based upon one's mother's *hukou* status, and could not be altered easily. Urban dwellers were given a non-agricultural (urban) *hukou* status, with which they are allocated food, housing, and other social benefits accordingly. In contrast, rural residents were given an agricultural (rural) *hukou* status and were excluded from these services and welfare entitlements. The 1964 change in the *hukou* system started to put tight control on domestic migration. Thereafter, movement from rural to urban areas became virtually impossible.

The 1964 change in the *hukou* system provided a particularly credible source of exogenous variation in administrative discrimination. I explore the effects of this change using a large, on-going national dataset from the China Health and Nutrition Survey (CHNS). Figure 3.0.1a illustrates the remarkable effect that this legislation change had on lowering the proportion of urban *hukou* residents. Among people born before 1965, there were more than 50% of urban *hukou* residents. Following the 1964 change in *hukou* policy, the fraction of urban *hukou* residents suddenly fell to about 40%. The same sharp drop in proportion of urban *hukou* residents was confirmed in the National Population Census of 1990 (Figure 3.0.1b).

I use this discontinuity in the proportion of urban *hukou* residents to identify the causal effect of institutional discrimination anchored in the *hukou* system on health. This is done by comparing the health of people born just before and after the legislation change in the *hukou* system. Identification comes from the assumption that potential outcomes are smooth around the cutoff (i.e., birth year 1965). The regression-discontinuity (RD) design estimates suggest that urban *hukou* cit-

izens have much better chances of being in good health. The deleterious effect of rural hukou on health possibly works through mechanisms of labor disparity, limited access to healthcare, and deprivation of quality education.

The structure for this paper is as follows. The next section gives relevant background information on the origin and changes of the hukou system in China, explaining the regression-discontinuity design. There will then be a description of the data used in this analysis. The section of method and results will provide the empirical method and results, which are carefully interpreted in the section of discussions. The last section is the conclusion.

BACKGROUND INFORMATION ON *HUKOU* SYSTEM

The *hukou* system requires individuals to register with local authorities to gain residency, thereby determining where people can live and work. It was first started in cities in 1951 and extended to the rural areas in 1955 (Yang and Zhou 42). In the early years of the system, it served largely as a monitoring, not a control, mechanism of population movements.

In the 1950s, there were several stimuli for rural-to-urban migration. The most influential stimulus lay in the "pull" of the cities, with the appeal of urban employment that offered workers security, a series of benefits, and prestige. Yet, there were also "push" features. These included escape from poorer regions, unhappiness with co-operatives, and the loss of income-earning opportunities, which were associated with the market as the state truncated private commerce and nationalized enterprises (Cheng and Selden 9). According to historical data, China's urban population increased from 10.6% of total population in 1949 to 14.6% in 1956, with a net gain of 34.6 million. Rural migrants accounted for 19.8 million of the total increase.

Due to the unexpected dramatic inflow of rural migrants to urban regions, the authorities issued a number of documents to control the huge domestic movement. When measures prompted by these state guidelines failed to stanch the population flow to major cities, the hukou system was promulgated as a perma-

ment system in 1958 (Chan and Zhang 7).

Ironically, as the regulations were put into effect, the whole country was swept by the radical campaign of the Great Leap Forward. As the top priority of the state shifted to accelerating industrial growth, this new legislation was simply brushed aside as urban enterprises increased recruitment of labor, prompting some super-high rates of rural-urban migration in 1958-1959.

Then, in 1964, the State Council approved the Regulation of the Ministry of Public Security on Hukou Change. It put tight control on migration to towns or cities from the countryside or to cities from towns. Consequently, from 1965, movement from rural to urban areas became virtually impossible because hukou was (and is) ascribed at birth based upon one's mother's hukou status, and could not be altered.

Through the strict food rationing system and state-control of all industries, the unreachable gap between rural and urban household registration populations was formed after 1964. The rural agricultural hukou people could no longer freely migrate into cities to seek better living conditions. Only several groups were permitted to receive a change from rural to urban hukou under the stringent system (known as *nongzhuanfei*)—recruitment by a state-owned enterprise (*zhaogong*), enrollment in an institution of higher education (*zhaosheng*), promotion to senior administrative jobs (*zhaogan*) (Yang and Zhou 42). However, the groups mentioned above are basically a very small percentage of the whole population. Furthermore, if one chose to migrate without going through state channels, that person was not permitted access to resources in the destination area. Denial of food, housing, education, and any other social services rendered illegal migration impossible to maintain (Grey 17).

In short, the hukou system acted as a domestic passport system to draw a chasm in the Chinese society. It served to produce and reproduce social segregation and social disparity, especially during the planned economy from 1965 to 1978. While economic dualism (rural/urban) is characteristic of most developing countries and is also existent in China, the hukou system has reproduced a much stronger social dualism through economic, and more importantly, institutional means.

DATA

Data were derived from the China Health and Nutrition Survey (CHNS), an ongoing longitudinal survey initiated in eight provinces in 1989. While the survey was not nationally representative, the provinces did vary substantially in geography, stage of economic development and health status. At present, there are about 4,400 households in the overall survey, covering some 19,000 individuals. Follow-up levels were high, but families that migrated from one community to a new one were not followed. The first round of CHNS data was collected in 1989. Eight additional waves were collected in 1991, 1993, 1997, 2000, 2004, 2006, 2009, and 2011.

The key explanatory variable in the study was the type of hukou (urban or rural) a respondent held. Respondents were asked: "What type of household registration do you belong to?" The survey recorded an individual's ranking with one if he or she had urban *hukou*, and zero otherwise.

The sample used was of men and women obtained from longitudinal sections between 1989 and 2011, for whom there were complete sets of data. Only individuals born after 1940 were included to minimize selection bias from the aged group. The survey was used as cross-sectional instead of panel, since there were little within-group variations of individual *hukou* status. Table 1 provides descriptive statistics of the principal variables used in regressions for the overall survey population, as well as different subgroups.

Multiple measures of overall health were used as dependent variables. There were three questions in the CHNS, which provide general information about the respondent's assessment of his or her state of health. Taking these questions in order, respondents are asked: "Right now, how would you describe your health compared to that of other people your age? Excellent, good, fair, poor, or unknown?" I recoded an individual's ranking as one if they are in good or excellent health, and zero otherwise. This variable is labeled Self-Reported Good Health. The mean values presented in Table 1 indicated that approximately 71% of respondents describe themselves as being in good overall health. Only about 70% of respondents

in the subgroup, with rural *hukou*, identified themselves as in good overall health compared to 72.5% in the subgroup with urban *hukou*.

Next, in relation to illness or infirmity, each respondent was asked: “During the past 4 weeks, have you been sick or injured? Have you suffered from a chronic or acute disease?” This was coded as a single dichotomous variable labeled: No illness. The mean values, presented in Table 1, suggested that the proportion of respondents who suffered from some illness was approximately 9.4%. About 11.5% of respondents in the rural *hukou* subgroup suffered from some illness while only about 8% of urban *hukou* respondents reported some illness in the past four weeks.

A subsequent question asked: “Over the past three months, have you had any difficulty in carrying out your daily activities and work due to illness?” (Daily activities are thought to include activities such as walking upstairs without assistance or feeding oneself.) This question offered more precise detail on the implications of the illness, as it measured the individual’s level of independence. This variable was labeled: No activity-limiting illness. From Table 1, it was apparent that approximately 6.22% of individuals in the data suffered from an activity-limiting illness. About 6% of individuals in the subgroup with rural *hukou* suffered from an activity-limiting illness, while around 6.9% of urban *hukou* respondents reported some activity-limiting illness in the past three months. All three health variables can be interpreted as measurements of the individual’s perceptions of his or her overall stock of health capital.

In Table 3.0.1, only 30% of the respondents had health insurance, with approximately 49.3% in the urban *hukou* subgroup, and only about 19.5% in the rural *hukou* subgroup. Also, over 60% of the whole sample held rural *hukou* and only 32.3% of households were located in urban areas. There was a huge income gap related to *hukou* status, no matter how the income was calculated. The average income of rural *hukou* individuals were only about half that of urban *hukou* individuals.

MODEL

REGRESSION-DISCONTINUITY APPROACH

From the background information on the hukou system, it was apparent that individuals born before 1964 had the opportunity to obtain a non-agricultural hukou type. They would have had to migrate to urban areas before the imposition of tight control on the hukou system, in 1964. This change in hukou policy allowed us to apply a fuzzy regression discontinuity (RD) design. Under some mild regularity conditions, the average causal effect of hukou status on health for cohorts born just before and just after the cutoff could be identified. There was no discontinuity in income or other covariants among residents born around 1964.

Assuming one hukou-status cutoff and a homogeneous effect of hukou status on health outcomes (Van der Klaauw 2002):

$$Y_i = \gamma E(K_i|X_i) + a(X_i) + u_i, \quad (3.1)$$

$$E(K_i|X_i) = \beta 1(X_i \geq X_o) + b(X_i), \quad (3.2)$$

where i indexed individuals, Y_i denoted the outcome of interest (for example, self-reported health state) for individual i , K_i was the hukou status, X_i was the birth year, and X_o was the value of hukou-status cutoff (i.e., 1965), $a(\cdot)$ and $b(\cdot)$ were flexible functions of birth year, and $E(u_i|X_i) = 0$. In the present setting this model corresponded to a "fuzzy" (as opposed to "sharp") RD design. As Figure 3.0.1 indicated, birth year affected, but did not perfectly explain, hukou status. In the baseline specifications, I used local quadratic regression (a local polynomial of order two) for $a(\cdot)$ and $b(\cdot)$.

Table 3.0.2 reported the results of the OLS regressions. A positive coefficient implied a positive connection between urban hukou and better health. OLS regressions for ease of interpretation were displayed. Logit models produced similar results. The OLS estimates reflected a highly statistically significant correlation

between better health outcomes and urban hukou. A change from rural to urban hukou improved the chances of being in good health by 2.1 percentage points. The urban hukou people had 1.1% more chance to report a risk of suffering from an illness. There was no significant difference between urban and rural hukou in experiencing an activity-limiting illness.

Table 3.0.3 reported the results of the RD analysis. Column 1 presented the first-stage regression of urban *hukou* on indicators for whether an individual was born after 1964, along with the quadratic spline for birth year. The coefficients on the cutoff indicator was an estimate of the average decline, in probability, of urban *hukou* at this break. Consistent with the visual evidence in Figure 3.0.1a, the probability of having urban *hukou* dropped by about 6.7% at the threshold.

Figure 3.0.2 presented “raw” means of self-reported health status, along with the fitted values of a locally weighted regression, calculated within each birth year cohort. Around the cutoff, the change in *hukou* legislation was accompanied by a decrease in the average self-reported health status. This observation was also borne out by the regression results. Columns 2–4 of Table 3.0.3 presented reduced-form regressions of self-reported health status, infirmity, and activity-limiting illness, showing positive and significant increases in health outcomes at the cutoff.

After exploring the possible causal relationship between hukou status and health outcomes, I examined channels through which hukou status influenced later life health outcomes. As mentioned earlier, I hypothesized that the effects of hukou work through mechanisms such as occupational segregation, wage differentials, and social isolation. Just as it seemed implausible to argue that hukou reforms could directly manipulate health outcomes, it seemed reasonable to assume that a change in hukou legislation only affected financial earnings, years of schooling, or access to healthcare through hukou status, shaping these dependent variables indirectly. Specifically, I applied the same RD model to investigate the impact of hukou status on the availability of healthcare.

The regressions were presented in Table 3.0.4. The dependent variable represented the availability of healthcare. The OLS estimates reflected a highly statistically significant correlation between healthcare accessibility and urban hukou. A

change from rural to urban hukou improved the chances of having health insurance by 51.6%.

After controlling for income, availability of health insurance, and educational fulfillment, the effects of hukou on health outcomes were no longer significant. These results provided support that hukou status acted on health mainly through discrimination against rural migrants in financial income, access to health coverage, and educational accomplishment.

DISCUSSIONS

From the results, it is clear that urban hukou dwellers have a much better chance for self-evaluated good health compared to rural hukou dwellers. The possible explanations for this result are that urban hukou status grants people with better access to health allowance, public hospitalization, and higher economic income. Those with urban hukou are less likely to have to work in places with bad working conditions or poor sanitary circumstances.

The urbanites with urban hukou, however, reported more occurrence of difficulty in carrying out daily activities and work due to illness. There are two possible explanations. First, as they have much broader access to public health facilities, urban hukou inhabitants may be more concerned about infirmity, taking a break from daily activities and work when they are ill. Whereas, rural hukou people usually still carry out their daily-life activities and work, even when they are sick, in order to keep their income steady.

Second, as urban hukou households are, on average, smaller than that of rural hukou households, and most urban hukou citizens hold full-time employment outside of the household, urban hukou individuals probably encounter more difficulties in getting care by other family members when they are sick. They will, therefore, easily recognize themselves as not carrying out daily activities and work. In contrast, most rural hukou people do not have permanent full-time jobs, but have larger families with extended family members around the household. The individuals will be looked after when they are sick and will not identify themselves

as activity-limited by poor health.

The interpretation of the results of this article involves an exploration of the mechanisms that lay behind the observed causal relationship. The hukou effects estimated here may pick up indirect effects of hukou on health as well as direct effects. I emphasize the role of healthcare accessibility in this case. A change from rural to urban hukou significantly improves the chances of having health insurance.

The Ministry of Labor and Social Security provides health insurance plans only to urban hukou residents. Rural people have limited access to sanitation and other healthcare facilities. This limited access to healthcare is likely to have detrimental impact on their health.

CONCLUSIONS

Prior studies suggested that institutional discrimination has deleterious effects on health outcomes (Chan and Zhang 7; Grey 17; Li et al. 25). Since research has mostly been done in the western context where social segregation has already been established, it is difficult to isolate the effect of initial social segregation on health outcomes.

In this paper, I provide plausible evidence on a causal relationship between institutional discrimination and health outcomes by exploiting a 1964 change in household registration system (hukou) in China, which caused a nationwide discrimination against rural dwellers. RD estimates show that changing from a rural to urban household registration type increases the probability of being in good health by more than 10%. In particular, rural hukou people have a significantly worse self-evaluation of health than urban hukou people.

Since the causal effects of hukou disappeared after controlling for income disparity, healthcare variations, and years of schooling, hukou status potentially works on health through mechanisms such as access to quality education, availability of health facilities, and wage differentials. I focused on the role of healthcare accessibility. The Ministry of Labor and Social Security provides health insurance plans only to urban hukou residents. Rural people have limited access to sanitation and

other healthcare facilities. This limited access to healthcare is likely to have detrimental impact on their health.

The findings suggest that rural hukou people, as a group, face more access barriers than urbanites. In particular, health insurance is very important in reducing access barriers. Our results could aid policy makers in assessing critical needs among rural population and set policy priorities accordingly. Obviously, providing healthcare coverage for every citizen is costly, and China may not be at the stage of economic development where such policy is a possibility or a priority. Some nationwide or localized policies could potentially alleviate access barriers faced by rural citizens and help to improve their health outcomes. For instance, community-based free or subsidized clinics can be very beneficial. Also, subsidized services for gynecological and maternal-child healthcare can provide good safety measures for female rural dwellers and their children.

My work thus far suggests that rural hukou status has a deleterious effect on health outcomes. Due to data limitation, I cannot explore, in greater depth, channels through which hukou status influenced later life health outcomes in this study. Also, the CHNS data does not allow me to check the variation in health among children born in the urban household, versus children that moved to urban settings with their parents. The next steps would be to build a stronger overall evidence base. A natural follow-up work is to carry out a well-developed and detailed survey, which will better confirm the causal relationship between health and hukou status. It will also help investigate the channels through which hukou status affects later-life health outputs.

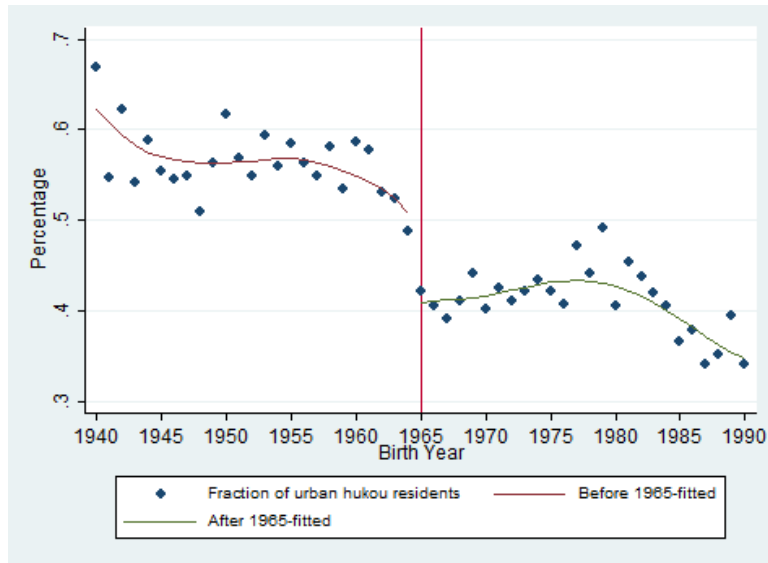
Table 3.0.1: Summary Statistics of Important Variables for Different Sub-samples

Variable	Total Survey Population Mean[std]	All individuals with Rural <i>hukou</i> Mean[std]	All individuals with Urban <i>hukou</i> Mean[std]
Self-reported Good Health	0.712[0.453]	0.699[0.459]	0.725[0.464]
No Illness	0.906[0.291]	0.885[0.271]	0.920[0.319]
No Activity-limiting Illness	0.938 [0.242]	0.939[0.239]	0.932[0.251]
Health insurance	0.301[0.657]	0.195[0.627]	0.493[0.791]
Hukou (1=rural, 0=urban)	0.618[0.486]		
Urban household (1=yes, 0=no)	0.323[0.494]	0.170[0.375]	0.549[0.498]
Female	0.500[0.500]	0.504[0.500]	0.500[0.500]
Age	35.107[20.406]	37.240[19.604]	42.429[19.721]
Age Squared	1648.944[1591.684]	1760.671[1556.800]	2163.760[1723.600]
Years of Schooling	4.700[3.390]	4.600[3.306]	4.745[3.530]
Total Net Household Income	6027.719[9203.700]	4709.970[7185.800]	9952.430[12986.000]
Total Net Individual Income	4085.650[7130.500]	3838.910[5665.880]	7939.400[10682.030]
<i>N</i>	47171	29152	18019

Standard errors in brackets.

Figure 3.0.1: Percent of Urban Hukou Residents in Cohorts From 1940-1990

(a) CHNS



(b) 1990 Census

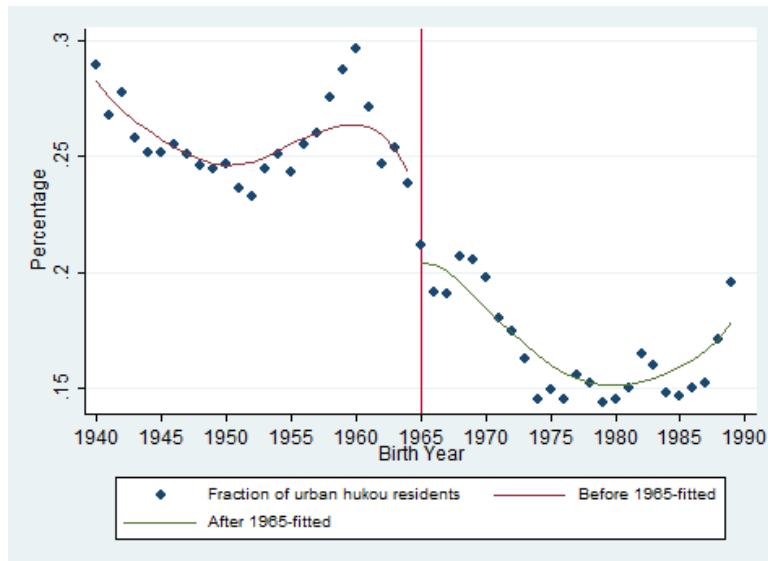


Table 3.0.2: OLS Specifications: *hukou* effects

	Self-reported good health (1)	No illness (2)	No activity-limiting illness (3)
<i>hukou</i>	0.021*** (0.005)	0.011*** (0.002)	-0.006 (0.006)
age	0.000 (0.001)	0.006** (0.002)	-0.004 (0.002)
age ²	-0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
birthyear	-0.006*** (0.001)	0.012*** (0.002)	0.008*** (0.001)
Survey year FE	Yes	Yes	Yes
Observations	42654	71115	30131
R ²	0.057	0.034	0.008

Robust standard errors in parentheses.

- 0.1 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 3.0.2: Self-reported health status and birth year from 1940 to 1990

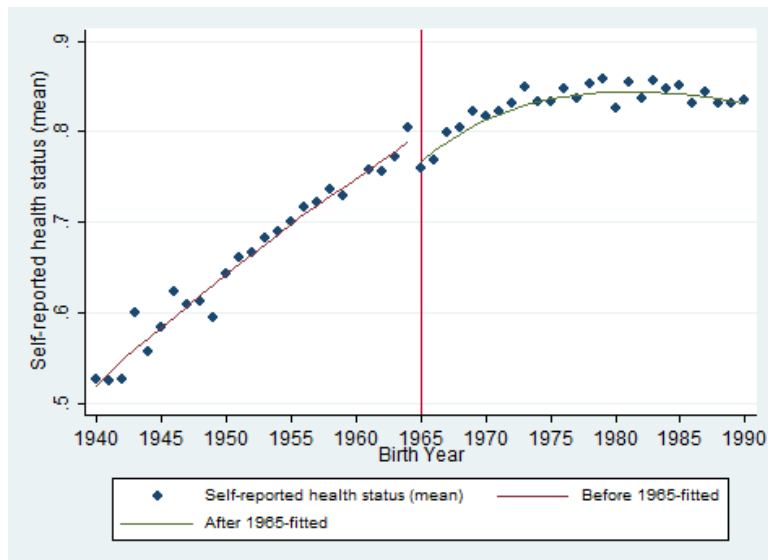


Table 3.0.3: First-Stage and Reduced-Form Specifications: hukou effects

	First Stage	Reduced form		
	Urban <i>hukou</i> (1)	Self-reported good health (2)	No illness (3)	No activity -limiting illness (4)
Born After 1964	-0.067*** (0.014)	-0.127*** (0.037)	0.066* (0.032)	-0.063 (0.083)
birthyear	0.05*** (0.004)	0.000 (.)	0.057** (0.019)	0.000 (.)
birthyear*year65	-0.004 (0.005)	0.014 (0.011)	-0.049* (0.022)	-0.061* (0.028)
birthyear ² *year65	-0.001- (0.000)	-0.003*** (0.001)	0.004 (0.002)	-0.004- (0.002)
birthyear ²	0.000*** (0.000)	0.001 (0.001)	0.000 (0.002)	0.004*** (0.001)
age	.0421 (0.0421)	-0.012 (0.010)	0.099*** (0.011)	-0.034- (0.020)
agesqu	0.00 0.00	-0.000*** (0.000)	-0.000*** (0.000)	-0.000 (0.000)
Survey year FE	Yes	Yes	Yes	Yes
Observations	54401	40408	39167	25100
R ²	0.009	0.0452	0.0442	0.0175

Robust standard errors in parentheses.

- 0.1 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.0.4: Healthcare accessibility: *hukou* effects

	OLS (1)	Reduced form (2)
<i>hukou</i> (1=urban; 0=rural)	0.516*** (0.013)	
Born After 1964		0.117*** (0.030)
age	0.136*** (0.015)	0.162*** (0.015)
age ²	0.000* (0.000)	0.000 (0.000)
birthyear	0.130*** (0.015)	0.147*** (0.016)
birthyear*year65		-0.001* (0.001)
birthyear ² *year65		0.003*** (0.001)
Observations	60324	71337
R ²	0.352	0.304

Robust standard errors in parentheses.

- 0.1 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

References

- [1] Severin Borenstein. To what electricity price do consumers respond? residential demand elasticity under increasing-block pricing. *unpublished manuscript*, 2009.
- [2] Jesse Bricker, Lisa J. Dettling, Alice Henriques, Joanne W. Hsu, Kevin B. Moore, John Sabelhaus, Jeffrey Thompson, and Richard A. Windle. Changes in u.s. family finances from 2010 to 2013: Evidence from the survey of consumer finances. *Federal Reserve Bulletin*, 10, 2014.
- [3] Isabelle Brocas and Juan D Carrillo. Rush and procrastination under hyperbolic discounting and interdependent activities. *Journal of Risk and Uncertainty*, 22(2):141–164, 2001.
- [4] Kjetil Bruun, Lynn Pan, and Ingemar Rexed. *The gentlemen’s club: international control of drugs and alcohol*, volume 9. University of Chicago Press Chicago, 1975.
- [5] Sebastian Calonico, Matias Cattaneo, and Rocio Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82:2295–2326, 6 2014.
- [6] Juan D Carrillo and Thomas Mariotti. Strategic ignorance as a self-disciplining device. *The Review of Economic Studies*, 67(3):529–544, 2000.
- [7] Kam Wing Chan and Li Zhang. The hukou system and rural-urban migration in china: Processes and changes. *The China Quarterly*, 160:818–855, 1999. doi:[10.1017/S0305741000001351](https://doi.org/10.1017/S0305741000001351).
- [8] Cynthia Chen, Don Varley, and Jason Chen. What affects transit ridership? a dynamic analysis involving multiple factors, lags and asymmetric behavior. *Urban Studies*, 48:1893–1908, 7 2011.

- [9] Tiejun Cheng and Mark Selden. The origins and social consequences of china's hukou system. *The China Quarterly*, 139:644–668, 1994. doi:[10.1017/S0305741000043083](https://doi.org/10.1017/S0305741000043083).
- [10] Douglas Coate and Michael Grossman. Effects of alcoholic beverage prices and legal drinking ages on youth alcohol use. *The Journal of Law and Economics*, 31(1):145–171, 1988.
- [11] Gerard De Jong and Hugh Gunn. Recent evidence on car cost and time elasticities of travel demand in europe. *Journal of Transport Economics and Policy*, 35:137–160, 5 2001.
- [12] André De Palma and Robin Lindsey. Transport user charges and cost recovery. *Research in Transportation Economics*, 19:29–57, 2007.
- [13] Robert Dorfman. Incidence of the benefits and costs of environmental programs. *The American Economic Review*, 67:333–340, 1977.
- [14] Luke E. G. Feudner. The poverty of wealth and income in the united states. *College of Professional Studies Professional Projects*, 2011.
- [15] Paul Gertler, Luis Locay, and Warren Sanderson. Are user fees regressive?: The welfare implications of health care financing proposals in peru. *Journal of Econometrics*, 36:67–88, 1987.
- [16] Corbett A. Grainger and Charles D. Kolstad. Who pays a price on carbon? *Environmental and Resource Economics*, 46:359–376, 2010.
- [17] Whitney Grey. Migrant education in beijing: Hukou and the future of human capital development. *Western Political Science Association Annual Meeting*, 2008.
- [18] Michael Grossman. The economic analysis of addictive behavior. *Economics and the prevention of alcohol-related problems*, pages 91–123, 1993.
- [19] Michael D. Grubb and Matthew Osborne. Cellular service demand: Biased beliefs, learning, and bill shock. *American Economic Review*, 105(1):234–71, January 2015. doi: [10.1257/aer.20120283](https://doi.org/10.1257/aer.20120283). URL <http://www.aeaweb.org/articles?id=10.1257/aer.20120283>.
- [20] Guido Imbens and Karthik Kalyanaraman. Optimal bandwidth choice for the regression discontinuity estimator. *Review of Economic Studies*, 20:1–27, 2012.

- [21] Arthur B Kennickell and Myron L Kwast. *Who Uses Electronic Banking?: Results from the 1995 Survey of Consumer Finances*. Division of Research and Statistics, Division of Monetary Affairs, Federal Reserve Board, 1997.
- [22] Elizabeth Klee. Paper or plastic? the effect of time on the use of check and debit cards at grocery stores. 2006.
- [23] Adit Laixuthai and Frank J Chaloupka. Youth alcohol use and public policy. *Contemporary Economic Policy*, 11(4):70–81, 1993.
- [24] Jonathan Leape. The london congestion charge. *Journal of Economic Perspectives*, 20:157–176, 2006.
- [25] Xiaoming Li, Bonita Stanton, Xiaoyi Fang, and Danhua Lin. Social stigma and mental health among rural-to-urban migrants in china: A conceptual framework and future research needs. *World Health and Population*, 8:14–31, 2006. doi:[10.12927/whp.2006.18282](https://doi.org/10.12927/whp.2006.18282).
- [26] Todd Litman. Transit price elasticities and cross-elasticities. *Journal of Public Transportation*, 7:37–58, 2 2004.
- [27] Yanan Miao and Liang Gao. Study on the price elasticity of demand of beijing subway. *Journal of Traffic and Logistics Engineering*, 1(1):25–29, 2013.
- [28] John Mullahy and Jody L Sindelar. Health, income, and risk aversion: assessing some welfare costs of alcoholism and poor health. Technical report, National Bureau of Economic Research, 1994.
- [29] Robin Room. Alcohol control and public health. *Annual review of public health*, 5(1):293–317, 1984.
- [30] Randall L Rose, William O Bearden, and Jesse E Teel. An attributional analysis of resistance to group pressure regarding illicit drug and alcohol consumption. *Journal of Consumer Research*, 19(1):1–13, 1992.
- [31] Emmanuel Saez. Do taxpayers bunch at kink points? *American Economic Journal: Economic Policy*, 2(3):180–212, 2010.
- [32] Richard Schmalensee. Payment systems and interchange fees. *The Journal of Industrial Economics*, 50(2):103–122, 2002. ISSN 1467-6451. doi: [10.1111/1467-6451.00170](https://doi.org/10.1111/1467-6451.00170). URL <http://dx.doi.org/10.1111/1467-6451.00170>.

- [33] Marius Schwartz and Daniel R. Vincent. The no surcharge rule and card user rebates: Vertical control by a payment network. *Review of Network Economics*, 5:1446–9022, 2006.
- [34] Jianfa Shen and Yefang Huang. The working and living space of the “floating population” in china. *Asia Pacific Viewpoint*, 44:51–62, 2003. doi:[10.1111/1467-8373.t01-1-00183](https://doi.org/10.1111/1467-8373.t01-1-00183).
- [35] Oz Shy and Zhu Wang. Why do payment card networks charge proportional fees? *The American Economic Review*, 101:1575–1590, 2011.
- [36] Kenneth A. Small and Erik T. Verhoef. *The Economics of Urban Transportation*. Routledge, 2007.
- [37] Alejandro Tirachinia and David Hensher. Multimodal transport pricing: First best, second best and extensions to non-motorized transport. *Transport Review*, 32:181–202, 2 2012.
- [38] Susan K. Urahn, Travis Plunkett, Nick Bourke, Alex Horowitz, and Walter Lake. Why americans use prepaid cards: A survey of cardholders’ motivations and views. *The Pew Charitable Trusts*, 2014.
- [39] William Vickrey. A proposal for revising new york’s subway fare structure. *Operations Research*, 3(1):38–68, 2 1955.
- [40] William Vickrey. Pricing in urban and suburban transport. *American Economic Review*, 53:452–465, 1963.
- [41] Stefano Della Vigna and Ulrike Malmendier. Paying not to go to the gym. *The American Economic Review*, 96(3):694–719, 2006.
- [42] Dennis Tao Yang and Hao Zhou. Rural-urban disparity and sectoral labor allocation in china. *Journal of Development Studies*, 35:105–133, 1999. doi:[10.1080/00220389908422575](https://doi.org/10.1080/00220389908422575).